5. Vagueness, New York style

The theories of vagueness we have considered thus far explicate the concept of a borderline case in either semantic terms or epistemic terms. What is common to the semantic explanations is that they deny one or other aspect of a Tarskian theory of truth for vague languages. Theories based on many-valued logics deny that the only truth values in a Tarskian truth theory are *true* and *false*. Supervaluational theories deny that the extension of a predicate can be identified with a particular set of objects, and rather has to be identified with a function from precisifications to such sets. The epistemic theory keeps the Tarskian theory of truth, and explicates the concept of a borderline case by relating it to a kind of inaccessibility to the extensions of our concepts.

Recently, Stephen Schiffer (2000) and Hartry Field (2000) have suggested that we abandon both the semantic and epistemological approaches to vagueness, and instead adopt a *psychological* theory of vagueness. On each of their theories, *a* is a borderline case of being *F* iff we can properly have a distinctive propositional attitude towards the propositions *Fa* and $\neg$*Fa*. Schiffer suggests that we have a distinctive *kind* of attitude towards them, an attitude that is somewhat, but not exactly, like familiar partial belief. One respect in which it is unlike partial belief is that the logic of these partial beliefs is not the ordinary probability calculus. Field does not suggest that we have a new kind of attitude towards these propositions, we partially believe them just like we might partially believe propositions about tonight's lottery results or unknown matters of history, but the logic of these partial beliefs is not the ordinary probability calculus.

Since both semantic and epistemic approaches to vagueness have a few difficulties, as we have seen, a new approach such as Schiffer and Field promise would be valuable, if it worked. The major reason it will not work is that these psychological approaches need an account of numerical degrees of belief that does not compel those degrees to be bound by the probability calculus. And there is an argument, derived from some late work by Frank Ramsey, that suggests no such account will be forthcoming. I will argue that it follows from the only plausible definition of numerical degrees of belief we have that these degrees must be constrained by the probability calculus. There are some other difficulties we shall raise, including an amusing problem concerning how their account (mis)handles the vagueness of certain attitude ascriptions, but the argument from Ramsey is the main problem these theories face.

While the conclusions this chapter makes concerning vagueness are rather negative, the argument I derive from Ramsey might have some positive interest for philosophers of probability. By the end of the chapter we have an argument that an agent whose degrees of belief do not conform to the probability calculus is, in an extended sense, being inconsistent. Traditional arguments for this conclusion have gone via decision theory, and this detour has aroused some suspicion. So it is valuable to have an argument that does not take the detour.

5.1. Schiffer's Theory

In section 4.5 we noted one surprising fact about boundaries around vague concepts - we don't look for them. As noted there[1], one reason for that is that we do not believe that there is a boundary to be found. Let's play with that idea for a while. Presumably if we think a concept is precise, then we do think there is a boundary to be found, so we do not believe that there is a boundary iff we believe that the concept is vague. Note that we can hold onto this characterisation of what it is to believe a concept to

[1] At the prompting, I should say, of Juan Comesaña.

be vague without thinking that this is what constitutes, or even characterises, vagueness. We might do this because we have a rather strange kind of error theory about vagueness. Or we might do it because we think there is no way to coherently characterise what it is to be vague, but we think that this characterisation of believed vagueness is somehow philosophically informative. [For later drafts, compare this to Sorensen's theory, which is sort of the converse.]

In different ways, this is the idea that Stephen Schiffer and Hartry Field develop. Both of them develop the idea by looking at the different ways our partial beliefs are structured when we recognise that the concepts they involve are vague. Both of them take for granted that when the concepts are involved are precise (which in practice they never are, but set that aside) that our partial beliefs should conform to the probability calculus. And both of them deny that this norm applies when our beliefs involve vague concepts. That is about where the agreements end.

Schiffer stipulates that "*vagueness-related partial beliefs* [are] those partial beliefs that *can't* under any suitable idealisation be identified with subjective probability." (2000: 23) He means a few different things by this. First, it is no constraint on vagueness-related partial beliefs (VRBPs) that they conform to the probability calculus. Secondly, we do not regard the fact that our VRBP in p is in (0, 1) as in itself evidence that we are in an imperfect epistemic situation with respect to p. We can have a VRBP of 0.5 in p, and think that there is nothing that could be done to drive this towards 0 or 1. Thirdly, VRBPs do not measure likelihoods in any sense. That our VRBP in *Louis is bald* is 0.5 does not mean that we think, in any interesting sense, that it is as likely as not that Louis is bald.

If VRBPs need not conform to the probability calculus, then what is their logic? Schiffer suggests, without much as I can see by way of argument, that it is the infinite-valued Łukasiewicz logic, at least for the $\wedge$, $\vee$, $\neg$ fragment. (There are special problems about conditionals, which Schiffer spends a little time over, but we will find enough difficulties before that.) For simplicity, say our ideal agent v-believes p to degree x just in case her VRBP in p is x. And she s-believes p to degree x just in case her ordinary subjective probability in p is x. As noted, Schiffer thinks that her s-beliefs should conform to the probability calculus. But v-beliefs need not. In fact, Schiffer thinks the following laws should hold for our ideal agent who v-believes p_1 to degree x_1, v-believes p_2 to degree x_2, and s-believes p_3 to degree x_3, at least p_1, p_2 and p_3 are unrelated. Let VRBP(p) = x mean she v-believes p to degree x, and SRBP(q) = y mean she s-believes q to degree y.

VRBP($p_1 \wedge p_2$) = Min(x_1, x_2)
VRBP($p_1 \wedge p_3$) = Min(x_1, x_3)
SRBP($p_1 \wedge p_3$) is undefined, since she can only have a VRBP in $p_1 \wedge p_3$ because it is vague
VRBP($p_1 \vee p_2$) = Max(x_1, x_2)
VRBP($p_1 \vee p_3$) = Max(x_1, x_3)
SRBP($p_1 \vee p_3$) is undefined, since she can only have a VRBP in $p_1 \vee p_3$ because it is vague
VRBP($\neg p_1$) = 1 - x_1

This leads to a rather surprising result, indeed a result so surprising that it suggests to me that we should be looking elsewhere for our correct theory. Sally has two friends, Jack and Jill, that she doesn't know that well. She is fairly confident that Jack is a lawyer and Jill is a doctor, but in each case her subjective probability is about 0.9. And she regards these as independent, her degree of belief in *Jack is a lawyer and Jill is a doctor* is 0.81. Note that her uncertainty here is not because she is worried about borderline cases, she is certain that Jack is either determinately a lawyer or determinately not a lawyer, and similarly for Jill and being a doctor. Sally doesn't remember occupations very well, but she remembers their heights perfectly. (She always had that kind of memory.) But even knowing that, she isn't sure whether Jack is short or whether Jill is tall. In both cases she leans strongly towards saying they are, but in the final analysis it is still vague to a degree. So her VRBP in *Jack is short* is 0.9, as is her VRBP in *Jill is tall*. So by Schiffer's rules, her VRBP in *Jack is short and a lawyer*, which we'll shorten to *Jack is a short lawyer*,[2] is 0.9, and her VRBP in *Jill is a tall doctor* is 0.9. Hence her VRBP in *Jack is a short lawyer and Jill is a tall doctor* is 0.9. But this is higher than her degree of belief in *Jack is a lawyer and Jill is a doctor*, which it entails. Even worse, her VRBP in *Jack is a short lawyer and Jack is not a lawyer* is 0.1. But surely she should have degree of belief 0 in this conjunction, since she is certain that one of the conjuncts is determinately false. These results suggest we need a different theory of how to integrate these non-classical degrees of belief in a classical theory of partial beliefs about the determinate. In this light, we should find Field's suggestions very interesting.

5.2. Field's Theory

Field's theory adopts Schiffer's idea of providing a psychological account of vagueness, but embeds it in a quite different theory. First, Field thinks that vagueness should not prompt us to give up classical logic, and hence he is committed to excluded middle. Indeed, he acknowledges a disquotational concept of truth, so he is also committed to bivalence, a point on which Schiffer was explicitly equivocal. As we saw when discussing the epistemicist theory, holding on to classical logic raises some challenges for dealing with the Sorites and the anomalies. As Field notes, classical logic implies that there is some value *v* such that a person with more than $*v* is rich, and a person with $*v* or less is not. (This is Field's example, and it is not entirely a happy one. As Soames notes, whether someone is rich depends, at least, on their wealth and their income. I would say it also depends on their liquidity. So assuming that we can present a numerical measure of how 'rich' someone is seems to be an idealisation. From now on, we make this idealisation.) Also, for any person, they are either rich or they are not. As Field notes, there is something common to the standard solutions to these problems favoured by most defenders of classical logic, including supervaluationists and epistemicists. Our penumbral case of richness might be either rich or not rich, but there is no requirement that she be *determinately* rich or *determinately* not rich. And the borderline *v* need not be such that anyone with more than $*v* is *determinately* rich, and everyone else is *determinately* not rich. Different theorists will have different theories of what *determinately* here amounts to, and Field is about to provide his own theory of this, but the explanations of the Sorites and the anomalies do have this structural similarity. So we then explain the puzzling consequences of keeping classical logic by noting the possible confusion between the true claims that classical logic shows we are committed to, and the false claims, the ones with determinately operators liberally inserted, to which we are not committed.

Field points out, correctly, that this solution is not worth much until and unless we provide some explanation of what 'determinately' means here. He does not require that a reductive analysis of determinateness be provided, but he does say that we should provide enough of a theory to 'fix its meaning close to uniquely' (288). Since Field believes, broadly speaking, in a conceptual role semantics, this means fixing its conceptual role close to uniquely.

As Field notes, saying that *p* is determinately true iff it is true on all acceptable precisifications does not seem to help much here, unless we happen to have an account of what is an acceptable precisification. One thing that might help is that when *p* is indeterminate, it is 'misguided' in some distinctive way to ask whether *p* is true. Because Field rejects epistemic theories of vagueness, he distinguishes between the inappropriateness of this inquiry, and the inappropriateness of inquiries into

[2] Of course, this compressed sentence is not logically equivalent to the conjunction, since predicate modifiers are not logically equivalent to predicates. But this complication is irrelevant here.

matters from which we are physically blocked, such inquiries into how penguins react when approaching the centre of a black hole. A too quick appeal to conceptual role semantics might look helpful here. Why not just say that it is part of the conceptual role of *determinately* that if it is indeterminate whether *p* then it is misguided to enquire into whether *p* is true? See, it isn't that hard to find analytic truths after all! Field rejects this approach because it is unenlightening. We have a theoretical duty to explain why this inquiry would be misguided. To be sure, explanations have to stop somewhere, but it seems a little cheap to have them stop just here.

Although Field rejects this move as 'feeble', he thinks it is on the right track. We can identify the concept of determinateness by considering the propositional attitudes of an ideal agent who regards *p* as indeterminate. One feature of this agent will be that she does not regard further enquiry into whether *p* as misguided. Another feature, Field suggests, will be that her partial beliefs with respect to *p* will not obey the probabilistic calculus. Consider some proposition *p* that I regard as having a determinate, but unknown truth value. Perhaps *p* is the proposition that the Dow is up for the day right now. (I'm writing this about an hour after opening time.) If I am unsure about whether *p*, then I will neither believe *p* nor believe $\neg p$. Even if I have neither belief, I might be much more confident in one direction than the other. I might, for example, regard it as, say, four times more probable that *p* is true than that it is false. (The Dow futures were up when I looked this morning, and they are a pretty good, though hardly perfect, guide to what the market will do in its first hour.) In that case, my degree of belief in *p* will be 0.8, and my degree of belief in $\neg p$ will be 0.2. There are a few reasons why I might not have such a precise degree of belief in *p*. I might not have thought about it in any detail, I might have confused beliefs, or, more theoretically, the relation *Brian is more confident in…than…* might be non-linear. The first two are (among the many) ways in which I am not an ideal epistemic agent. It is somewhat controversial whether the third also is such a way. There is a long and esteemed tradition, highlighted by Ramsey, de Finetti and Savage, of writers who hold that it is. There are also, on this question as every other, a brave army of heretics who hold that it is not, and that non-linearity of one's measures of confidence is no epistemic flaw.[3] Field to some extent sides with tradition here. He holds that when an ideal agent regards *p* as determinate, her strength of belief in both *p* and in $\neg p$ will be measurable, and these measures, her degrees of belief in *p* and in $\neg p$, will sum to one. More generally, if *F* is any field of propositions[4] such that the agent considers every member of the field to be determinate, then the function *Bel*( ) from members of *F* to [0, 1] will be a probability function.

Field departs from orthodoxy in cases where the agent regards *p* as indeterminate, and more generally about cases where the agent is not sure whether *p* is indeterminate. In those cases, Field thinks an agent can have a low degree of belief in both *p* and its negation. If she is certain that *p* is indeterminate, then she will have a degree of belief 0 in both *p* and $\neg p$. This seems to make some sense: if you are certain that *p* is indeterminate, then you think *p*'s eventual truth is as likely as a an event with probability 0 (i.e. a very unlikely event), so you should have degree of belief 0 in it. Field notes there is a simple, and mathematically attractive, way to get to this position from orthodoxy. Assume that in the agent's 'language of thought' there is a determinately operator *D*, which for reasons we shall soon get into is a modal operator whose logic is at least as strong as S4. And assume that there is a probability function *P* such that whenever the agent regards *A* as determinate, i.e. whenever she fully believes $DA \vee \neg DDA$, then $Bel(A) = P(A)$. Field provisionally thinks that for any ideal agent there will be such a

[3] The work of this army is well set out, and advanced, by Walley 1991. It is perhaps misleading to say that on every question there is a *brave* army of heretics, because heresy is often so *easy* in philosophy. Still, we shouldn't let these facts get in the way of a catchy descriptions.

[4] A field of propositions is a set of propositions closed under negation, conjunction and disjunction.

function, though he is prepared to retract this depending on how certain formal questions turns out. This probability function represents her attitudes towards propositions about which she is certain they are determinate. Now define another function Q such that $Q(A) = P(DA)$. Q represents how certain our agent is that A is definitely the case, that is, it represents her confidence that A is a fact, in some thick sense of fact. Field argues that it is Q, not P, that in general represents the agents degrees of belief.

Since the logic for D is at least as strong as S4, every instance of $DA \leftrightarrow DDA$ will be a logical truth, and hence will be believed by our ideal agent. Hence $P(DDA) = P(DA)$, since our agent must assign the same probability to every pair of propositions that she regards as necessarily equivalent. So $Q(DA) = P(DDA) = P(DA) = Q(A)$. This tells us something about the logic of these Q-functions, and Field proceeds to derive a few more results by similar reasoning. The most important of these are the following:

(*) $Q(DB \vee DC) = Q(B) + Q(C) - Q(B \wedge C)$;

(#) $Q(B \vee D) \geq Q(B) + Q(C) - Q(B \wedge C)$;

(**) $Q(DB_1 \vee \ldots DB_n) = \Sigma_i Q(B_i) - \Sigma_{i,\, j \text{ distinct}} Q(B_i \wedge B_j) + \Sigma_{i,\, j,\, k \text{ distinct}} Q(B_i \wedge B_j \wedge B_k) - \ldots \pm Q(B_1 \wedge \ldots \wedge B_n)$;

(##) $Q(B_1 \vee \ldots B_n) \geq \Sigma_i Q(B_i) - \Sigma_{i,\, j \text{ distinct}} Q(B_i \wedge B_j) + \Sigma_{i,\, j,\, k \text{ distinct}} Q(B_i \wedge B_j \wedge B_k) - \ldots \pm Q(B_1 \wedge \ldots \wedge B_n)$,

where the '$\pm$' is + if n is odd and - if n is even. (Field 2000: 298)

Obviously enough, (**) implies all the others, and hence is the most important of these. If we are considering the D-free part of the language, then (##) is the most general restriction on these functions. (In conjunction, of course, with things like $0 \leq Q(A) \leq 1$, and $Q(A) = Q(B)$ if $A \dashv\vdash B$, that mostly go unstated in these contexts.) It turns out there is quite a bit of mathematical work on functions that obey (##), largely because of the influence of Glenn Shafer. Shafer argued that the logic of these Q-functions, which he called belief functions but have subsequently become known as Shafer-functions, is the logic of partial belief. Shafer was more interested in varieties of anti-realism than in semantic indeterminacy, but he still drew the same conclusions as Field. In section four, we shall return to the details of Shafer's approach.

If the agent regards A as certainly determinate, then she fully believes $DA \vee D\neg A$, whether or not she fully believes either disjunct. Since in S4 $DA \vee D\neg A \dashv\vdash D(DA \vee D\neg A)$, we need not worry whether the appropriate way to represent this in Field's system is by saying $P(DA \vee D\neg A) = 1$ or $Q(DA \vee D\neg A) = 1$, since the two are equivalent. By (*), this implies that $Q(DA) + Q(D\neg A) = 1$, and since $Q(DX) = Q(X)$, this implies that $Q(A) + Q(\neg A) = 1$. Conversely, if she is less than certain about the determinateness of A, then $Q(DA \vee D\neg A)$ will be less than 1, and hence $Q(A) + Q(\neg A) < 1$, by the same reasoning as above. So if Q represents her degrees of belief, then we can say just what it is for an ideal agent to regard A as determinate: it is to have degrees of belief in it and its negation that sum to one. So we now not only have a theory about how to reason about the indeterminate, the theory of these Q-functions, or Shafer functions as we shall call them from now on, but a theory of what it is to regard something as determinate. These points form the centrepiece of Field's theory of vagueness.

5.3. What is it to be Determinate?

At the start of *Concepts*, Fodor makes fun of those who take it to be a Very Important Methodological Precept that the question *What is it to have a concept?* is logically prior to the question *What is a concept?* Fodor thinks it is absurd to think that one of these questions could be prior to the other, since we can quickly move from an answer to one to an answer to the other. If we answer the second, by saying for example that concepts are pumpkins, then we can quickly answer the first: to have a concept is to have a pumpkin. And if we answer the first, in a certain natural way, then we can answer the second. If to have a concept is to have a pumpkin, then concepts are, presumably, pumpkins. There is one difficulty for this neat little symmetry. If we don't answer the first question in that natural way, we might not be able to answer the second. If what it is to have the concept FISH is to be on time for work every day, then not a lot follows about the concept FISH is.

A similar worry arises naturally for Schiffer's and Field's theories about determinateness. These theories tell us, at best, what it is to believe that something is determinate. If things go well, then we can derive from this a theory about what it is for a sentence, or proposition, to be determinate. (Since both Field and Schiffer hold that to believe a thing is determinate or indeterminate is to take a certain (kind of) propositional attitude towards that thing, it looks like they hold it is propositions, not sentences, that are properly thought of as being determinate or indeterminate. I will adopt this interpretation in what follows, though I think most of what I say could be altered my quick exegetical argument fails.) That is, if to believe that a proposition is determinate is to believe *it* is F, for some suitable F, then to be determinate is to be F. The problem is that this is not the form of Field's theory. Schiffer's theory seems to be in less difficulty here. On his theory, you have degree of belief 1 in $p \vee \neg p$ iff p is not vague. Remember that on Schiffer's theory if you only have a partial belief in p because of its vagueness, then your degree of belief in $p \vee q$ is the maximum of your degree of belief in p and your degree of belief in q, so if you believe p to some intermediate degree because of vagueness, then since you believe neither p nor $\neg p$ to degree 1, you do not believe $p \vee \neg p$ to degree 1. Assuming that to fully believe something is just to believe it to degree 1, we can, to a first approximation, say that on Schiffer's theory that p is determinate iff $p \vee \neg p$ is true. This is just the analysis of determinateness we get on the many-valued theories whose logic Schiffer incorporates. For Schiffer this logic is a logic of partial belief, rather than a logic of partial truth, but the formalism remains the same.

On Field's theory, to believe p is indeterminate, or determinate, is not to believe, or disbelieve, anything. Field thinks that vagueness does not threaten classical logic, so $p \vee \neg p$ is true, and fully believed, even when p is indeterminate. All that happens when you regard p as indeterminate is that your degrees of belief in p and in $\neg p$ do not sum to 1. Note that even if this is true, you might or might not believe it to be true. If you have imperfect cognitive access, you might have these attitudes without believing you do. Since you can believe p is indeterminate without believing that you believe p to be indeterminate, that is, without believing that your degrees of belief in p and in $\neg p$ do not sum to 1. So we cannot use such a belief to easily read off what it is to be determinate. And, conversely, even if you do not believe that your degrees of belief in p and in $\neg p$ do not sum to 1, you might still regard p as determinate, just because you haven't given the matter of this sum any particular thought. (Quick quiz: what is the sum of your degrees of belief in *The Cubs will win the pennant this year* and *The Cubs will not win the pennant this year*?) So whether or not you believe p is determinate doesn't seem to imply much at all about what else you believe. Hence we cannot turn Field's theory about what it is to believe something to be determinate into a theory of what it is to be determinate with any ease.

Maybe the problem is not with Field's theory, it is with you. If only you had decent levels of cognitive access and reflectance, these problems wouldn't arise. (Some might think that I must mean *ideal* levels of cognitive access and reflectance, not merely *decent*, but they just set their standards too low.) Perhaps, then, Field can adopt the following position, which in fact is very close to the position Schiffer adopts. (See his discussion of VPB*s.) If an ideal agent regards p as determinate, then her degrees of belief in p and $\neg p$ sum to 1. Such an agent is sufficiently reflective that if her partial beliefs sum this way, she believes that they do. And, if p is determinate, then she is disposed at least, in virtue of her idealness, to believe this and hence have degrees of belief in them that sum to 1. I assume that being ideal does not imply having opinions, even partial opinions, on everything, but does imply having correct opinions when one does form them. So it seems p is determinate iff an ideal agent is disposed to have degrees of belief in p and in $\neg p$ that sum to 1.

This still won't do, though not for the reasons we gave above. As we will discuss in great detail in later chapters, there are many objects about which no one ever thinks. If one believes in unbounded mereology and a little set theory, then there are literally boundlessly many of these things. Further, beliefs can be vague in just the same way that sentences, and propositions, can be. (If I were to take a stand on these matters, I would say that the vagueness of beliefs *causes* and *explains* the vagueness of sentences, and probably *constitutes* the vagueness of propositions. But I'm not taking a stand here, just reporting my guesses.) One way they can be vague is that it might be vague just what the subject of my belief is. If I believe *The Thames is more beautiful than the Yarra River*, as I suppose I do, it is indeterminate just which bodies of water are the subjects of my belief.[5] For some things, including some of the bodies of water which have a claim to be *the* Yarra River, it is indeterminate whether anyone has beliefs of which they are a subject. Indeed, for some objects, including those bodies, it is indeterminate whether anyone has any propositional attitude that has that object as a constituent at any level, either by being about that object, or about an attitude about that object, or about that kind of attitude, and so on. Say an object is *cognated* iff someone has a propositional attitude that has that object as a constituent. Now it seems obvious that it can be vague whether a particular object is cognated. I would argue that there are no material objects, except perhaps the universe as a whole, and a few well-placed indivisible particles, such that it is determinate that they are cognated, but we don't need this for the weaker conclusion that for some objects it is indeterminate whether they are cognated. Field's theory cannot, it seems, allow this possibility. If our ideal agent gives any thought to the proposition that x is cognated, she is disposed to believe to degree 1 that it is cognated, since she then believes to degree one that she has a propositional attitude of some kind with respect to the proposition *x is cognated*. So we cannot turn Field's definition of what it is to believe something is determinate into a definition of what it is to be determinate by either considering the beliefs of non-ideal agents, like you and I, or ideal agents. Hence it seems unlikely that we could possibly turn it into a definition of what it is to be determinate, and this seems like a downside of Field's theory.

5.4. Field, Shafer and Supervaluations

Let us return for a while to the nature of Shafer functions, in part to see if we can discover a theory of determinateness in Field's theory, and in part to find out more about the nature of this theory. We shall draw heavily here on Shafer's work in his book *A Mathematical Theory of Evidence* (Shafer 1976) and his paper "Constructive Probability" (Shafer 1981). One of the things that Shafer establishes in the book is

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[5] This is a very hard case for a few reasons. Since I would naturally assent to this sentence, I suppose I do believe the proposition it expresses. But I only have opinions about a very small segment of the Thames, the bits within a short distance of Westminster, they're the only bits I've seen, and while I do have opinions about other bits of the Yarra, I wouldn't normally take the sentence to be about the bits significantly outside the centre of Melbourne. So does this mean that the referents of *Thames* and *Yarra River* are their metropolitan parts, or that the names pick out the entire rivers, but predicate *beautiful* here picks out a relation that holds in virtue of the salient parts of the rivers? I'm not sure there is even a fact of the matter, and it certainly doesn't challenge my view that the names here are indeterminate, and that this is good evidence for the claim that it is indeterminate just which objects are the subjects of my corresponding belief. If anyone has good arguments one way or the other though on the semantics of the sentence, I'd be interested to hear them.

that with respect to most of the formal properties of Shafer functions, we lose little or no generality by only considering functions defined on finite possibility spaces. Of course if we are interested in whether these functions do or should obey principles like countable additivity we must consider infinite possibility spaces, but for our more mundane concerns, finite spaces are sufficient. So we shall work for a while under the (false) hypothesis that the Shafer functions in which we are interested in are defined on finite possibility spaces. (Shafer does not call these Shafer functions, so the terminology here is a little anachronistic, but this is better, I think, than being misleading.)

So imagine we have a finite possibility space U, intuitively a set of possible worlds, and the propositions defined on that space are just the elements of the powerset of U. We all know how to define probability functions on such a space. Assign a measure, or weight, to each world, such that the sum of these is one. The probability of a proposition is the sum of the weights of each of the worlds in that proposition. Any function defined this way will satisfy the axioms of the probability calculus, and any function (on these propositions) satisfying these axioms can be generated by providing a suitable weight to each world.

We can generate Shafer functions in a similar way. Rather than assigning a weight to every world, assign a mass to every non-empty set of worlds, such that the sum of these masses is one. We can now define a Shafer function on the propositions using these masses. For any proposition A, $Q(A)$ is the sum of the masses of sets of worlds that are subsets of A. Any function defined this way will be a Shafer function, in particular it will satisfy (**), and any Shafer function on these propositions can be generated by a suitable mass function. If this is a little abstract, perhaps an example will help clarify matters. Imagine our possibility space U is $\{w_1, w_2, w_3\}$, and the mass function m assigns mass 1/7 to each of the non-empty elements of the powerset of U. Let A be the proposition $\{w_1, w_2\}$. Then $Q(A)$ will be $m(\{w_1, w_2\}) + m(\{w_1\}) + m(\{w_2\}) = 3/7$, and $Q(\neg A)$ will be $m(\{w_3\}) = 1/7$. So here we have a case where $Q(A)$ and $Q(\neg A)$ do not sum to 1, since $Q(A) + Q(\neg A) = 4/7$.

For many purposes, it is easier to work with these mass functions than with the Shafer functions themselves. In the appendix to this chapter I use the fact that there is a unique mass function for every Shafer function to resolve some formal questions about Field's theory that he left open. For now, we can use them to draw some connections between Field's theory and supervaluationism.

For the purposes of this paragraph, assume supervaluationism is true, and indeed assume the version of supervaluationism that identifies truth with supertruth is true. Let U be the normal possibility space, and w the weight function our ideal agent uses to assign probabilities to propositions (precisely) defined on U. Let F be the set of acceptable precisifications of English. We assume, as ever in this section, that U and F are finite, indeed we assume that the cardinality of F is f. Then there are two types of uncertainty relative to the truth of any sentence: uncertainty as to which world we are in (which member of U is actual), and uncertainty as to which language we are speaking (which member of F is English). These two uncertainties must be kept distinct at some level, because the first of them is uncertainty about a question about which it is misguided to try to discover the correct answer. Still, they are both there, so we can represent the possibilities relevant to our agent as members of $U \times F$. Given a member of $U \times F$, we can work out the truth of any sentence, because members of U are complete, so they fix all the facts, and a precisification precisely identifies which sets of facts make sentences true. So members of $U \times F$ are well suited to be the members of a possibility space. These members are ordered pairs $<w, l>$, where w is a world in U, and l a precisification, or language, in F. Now define a mass function on $U \times F$ by setting the mass of each of the sets $\{<w, l>: w = w_i\}$ to be the original weight of w_i. Using this mass function we can generate a Shafer function that satisfies (##). This function will measure the probability our supervalutionist give to the claim that a sentence S is true, given that she knows that all and only members of F are acceptable precisification. Further, any function on the D-free part of the language that satisfies (##) can be generated by this procedure. So, at least as far as the D-free parts of the language go, Field's theory is just probability theory for supervaluationists. If this is right, then Field has an easy analysis of determinateness waiting for him to take - the supervaluationist analysis. I doubt he will accept this little offer, but it is there.

Does this mean Field's theory is just an uninteresting variant of supervaluationism? No, for three reasons; two of them fairly trivial, but one of them potentially important. The first reason is that the Shafer functions generated this way measure something like sentential attitudes, since they measure attitudes concerning the truth of various sentences, whereas Field's theory is a theory of propositional attitudes. I'm not sure how much should be made of this - it all turns on how tight we think the connection is between propositional vagueness and sentential vagueness, which is an issue we shall return to in chapter 10. The second reason is that the connection here only concerns the D-free parts of the language. Since Field explicitly says his is not (yet) a theory of higher-order vagueness, we shouldn't make too much of this either. There are ways to make the supervaluationist theory sketched in the previous paragraph more like Field's. First, we adopt the account of higher order vagueness Williamson suggested (see section 3.6), with a transitive, but not necessarily symmetric, accessibility relation. Secondly, we don't assign mass to the sets that include all ordered pairs of the form $<w_i, l>$, but rather, to sets such that whenever $l_1 R l_2$, and $<w_i, l_1>$ is in the set, then $<w_i, l>$ is too. I won't display the details any further, because this should be enough to show that there may still be a connection between Field's theory and supervaluationism, even once higher order vagueness is considered.

The important reason that Field's theory may be interesting even if it is (merely) a proper part of supervaluationism is that sometimes a part of the theory may be more interesting than the whole. In some sense, ZFC is a proper part of naïve set theory (provided we take choice to be a part of naïve set theory, which I suppose we might not). But in many respects ZFC is a more interesting theory than naïve set theory, because we have good reason to believe that what it leaves out is mistaken. There may be a plausible analogy here with Field's theory. It may well be true that what his theory boils down to is probability theory for supervaluationists. But this does *not* show that his theory is no better than supervaluationism. If we think that the probability theory generated by supervaluationism is (a) correct and (b) sufficient to explain the central phenomena of vagueness, even if the rest of supervaluationism is in some way mistaken or incoherent, then we should think Field's theory is an improvement over supervaluationism. I have my doubts about (b), and as we'll presently see, there's a conclusive argument against (a), but the point here is that a response to Field must rest on arguments against (a) and (b), and not just on the formal parallels between his theory and supervaluationism.

5.5. Ramsey's Analysis and the Problem

Field's theory doesn't explain what it is for a proposition to be determinate. Schiffer's theory does this, but only by relying on his account of partial beliefs about disjunctions which we saw was a major weakness in his theory. So each of them have an explanatory deficit. This deficit is even larger than we have so far noted. The fundamental problem with each of these positions is that they don't explain just what is meant to be measured by these degrees of belief. More generally, neither theory has an account of what it is to believe p to degree x. And, as far as I can tell, the only way to fix this omission implies a commitment to classical probabilistic accounts of degrees of belief.

Rarely does introspection directly reveal to me that my degree of belief in p is 0.37. If it is true that I do believe p to this degree, either this must be a fact that is not accessible to me, or one that I can only discover after some sustained investigation of my mental states, not simple introspection. Ramsey provided two accounts of how to perform this introspection. His first version became the basis for the contemporary orthodoxy in this area, but I think the merits in his second have been too long overlooked.

In the first paper (Ramsey 1926), he argued that there was no way to convert qualitative judgements of greater or smaller credences into quantitative judgements of, say, degree of belief, or credence as we'll call it from now on, equal to 2/3. Hence he thought we had to define numerical credences in terms of betting behaviour. However, there is a relatively simple way to do this, as he pointed out in Ramsey 1929. To say my credence in *A* is 2/3 is to say I have the same credence in it as I have in $p_1 \vee p_2$ when I know that exactly one of p_1, p_2 and p_3 is true and each of the p_i are equally probable. From the various qualitative judgements, *A* has the same credence as $p_1 \vee p_2$ and all p_i have the same credence, we can work out a quantitative judgement.

It is rather trivial to generalise this. My credence in *A* is *x/y* when it is the same as my credence in $p_1 \vee ... \vee p_x$ given that I believe fully that exactly one of $p_1, ..., p_y$ is true and each p_i is equally probable. By equally probable, I just mean that my credence in p_i equals my credence in p_j for all i, j. This can be generalised to real values by using the definition of real numbers as Dedekind cuts. The details will be spelled out below, but we will stick to rational values for now for simplicity. (Note that 'real' and 'rational' here always refer to properties of numbers. Saying that a credence is real doesn't entail that anyone has it, nor does saying it is rational entail that anyone should have it.) I'll call this analysis of credences, that to believe *A* to a certain degree is to believe it to the same degree as a certain disjunction, the *Equivalence Analysis*.

It might seem that the Equivalence Analysis is circular, since we have analysed credences in terms of, *inter alia*, credences. This objection misses the point. The aim of the Equivalence Analysis is to reduce quantitative credences to qualitative credences. If we were to write it as a precise definition (or more exactly a set of definitions) we would find that on one side we have quantitative sentences and on the other we have only qualitative sentences. Indeed the only qualitative relation we have used is of equality of credences; we haven't even used inequalities. This seemed to be Ramsey's philosophical point – we can understand what it is to be more confident in *p* than in *q*, or to be as confident in *p* as in *q*. So if we can explain having credence 0.37 in terms of these comparative concepts, we have explained it. We have to use the 'more confident in' concept to define real degrees of belief, which is why I have brought it up here as being part of the basis in the philosophical explanation. (Ramsey's definition was not totally ignored in the literature. The most interesting appeal to it is in Koopman's theory of comparative probability. Koopman takes a 'as probable or more probable than' relation as primitive, but he notes that sometimes this can be used to produce numerical probability assignments, just in the cases where Ramsey-style definitions are possible.)

To simplify, I'll adopt the notation *Bel*(*A*) for the agent's credence in *A*. The aim of this section is to show that, at least for the special case when *Bel* takes only rational values, Ramsey's definitions imply that an agent is incoherent if *Bel* is not a probability function. Since both Field and Schiffer claim that vagueness creates contexts in which agents can be coherent even though *Bel* is not a probability function, if this argument is successful then Schiffer and Field are mistaken. Obviously, if I appeal to any principles that are obviously dubious in the presence of vagueness, the argument will be question-begging, but I think we will avoid that trap in what follows. Note before we start that I do not say that coherence requires that *Bel* even be defined. I think an agent can be fully coherent even though they have no numerical degree of belief in some propositions. Indeed, I think the best reaction to vagueness is to be in just this position. But this is not the position Schiffer and Field take – they hold that agents do have numerical degree of beliefs, but the agent can be coherent even though these are not probability functions.

By definition, *Pr* is a finitely-additive probability function iff it is a function from propositions to numbers satisfying:

(Pr1) $Pr(A) \geq 0$;
(Pr2) If $\vdash$T then $Pr(\mathrm{T}) = 1$;
(Pr3) If $\vdash \neg(A \,\&\, B)$ then $Pr(A \vee B) = Pr(A) + Pr(B)$.

If we wish *Pr* to be countably additive we need to extend (Pr3) to cases where there are countably many propositions, each pairwise disjoint, such that the probability of their disjunction equals the sum of their probabilities. I will not discuss countable additivity here, though I note that there are reasons for doubting this argument can be generalised to provide an argument for countable additivity, because of considerations concerning the case where we take *Bel* to be a partial function. (This case is discussed in some depth in Walley 1991, especially pages 294-327.)

If an agent believes, in the old-fashioned sense, *A*, then they have the same credence in *A* as they would have in p_1 were they to believe exactly one member of the set $\{p_1\}$ is true. Hence their credence in *A* will be 1/1, that is, 1. If they believe $\neg A$ then there will be no value *y* such that they have the same credence in *A* as in p_1 and exactly one member of $\{p_1, ..., p_y\}$ is true. Hence for all *y*, $Bel(A) < 1/y$. So $Bel(A) = 0$.

It is worthwhile clarifying before we continue what is meant by coherence here. An agent's non-probabilistic beliefs are coherent iff they are closed under entailment and non-trivial. There are familiar doubts about how much of a virtue coherence is, but we will ignore those here. I take the arguments here to show that if *Bel* is not a probability function, then the agent is, for evaluative purposes, comparable to an agent who has inconsistent beliefs. Now, whether that agent's doxastic states are best all things considered in her situation, I think it is clear that being inconsistent is not a good thing, and certainly not an ideal reaction to any evidential situation. For purposes of refuting Schiffer and Field, this is sufficient, for they hold *Bel* need not be a probability function even if the agent is ideal. We will say a little below about what effect these considerations have on the more general philosophical importance of this result.

Above I said that $Bel(A) = x/y$ meant simply $Bel(A) = Bel(p_1 \vee ... \vee p_x)$ where $\{p_1, ..., p_y\}$ is a set of propositions such that the agent knows one of them is true and for any i, j $Bel(p_i) = Bel(p_j)$. Put this another way, we could say that $Bel(A) = x/y$ means that the agent has the same credence in *A* as they would have were they to believe (i.e. believe fully) $A \equiv p_i \vee ... \vee p_j$, where there are *x* disjuncts and $\{p_1, ..., p_y\}$ is defined the same way.

It is clearly no constraint on rationality that if $Bel(A) = x/y$ we can find *y* equiprobable alternatives such that the agent believes *A* is materially equivalent to a disjunction of *x* of these. However it does seem to be a constraint that she should be able to consistently believe something of this sort. If it were inconsistent for her to believe that there was any such set as $\{p_1, ..., p_y\}$ and *A* is materially equivalent to a disjunction of *x* elements of this set, something seems amiss.

Indeed, an even stronger constraint than this seems in order. Assume that there exists a *y* such that for all propositions *A* in a finite field of propositions Γ, $y \cdot Bel(B)$ is an integer. (A field of propositions is just a set of propositions closed under negation, conjunction and disjunction.) Provided the credence in any proposition in Γ is rational this will be possible, so let's make that simplifying assumption, just for now. Again assume $\{p_1, ..., p_y\}$ is a set of propositions such that we know exactly one is true and all are equiprobable, and that no proposition about any p_i is in Γ. Then it seems to be a rationality constraint that it should be possible for any *B* in Γ, where $Bel(B) = x/y$ to find a disjunction of *x* elements of $\{p_1, ..., p_y\}$ such that it is consistent to believe *B* is materially equivalent to that disjunction.

(Of course, we do not hold that this material equivalence claim is *actually* believed; the p_i are 'dummy' propositions about which the agent herself has no propositional attitudes.) That is, it should be possible to model the agent's probabilistic beliefs about propositions in Γ on $\{p_1, ..., p_y\}$. Since what it means to say $Bel(B) = x/y$ is to say B is believed to the same degree as such a disjunction, it would be odd if it were inconsistent to say that B is materially equivalent to any such disjunction.

The above is fairly informal, particularly the requirement that no proposition 'about' the p_i be in Γ. The following is a more formalised statement of it, followed by proofs that these restrictions are sufficient to show why the degrees of belief should follow the probability calculus.

Assume an agent has a certain set of beliefs, say **K**, and certain credences $Bel(\cdot\ )$. Let y be the lowest common denominator of these credences. We want to test for coherence her credences about a certain set Γ of propositions, assuming that all these credences are rational numbers. The agent is coherent iff there is a set $P = \{p_1, ..., p_y\}$, and a possible agent C who has the following properties:

(a) If the agent believes that A, then C also believes that A
(b) If $Bel(A) = x$, then C's credence in A is x
(c) C believes the disjunction $p_1 \vee \ldots \vee p_y$;
(d) For any two distinct members p_i and p_j of P, C believes $\neg(p_i \wedge p_j)$;
(e) For every member p_i of P, C's credence in p_i is $1/y$.
(f) $Bel(A) = x/y$ iff there is a subset S of P such that $|S| = x$ and C believes the material biconditional $A \leftrightarrow (p_i \vee p_j \vee \ldots)$, where the RHS lists all the members of S;
(g) C's beliefs are closed under entailment;
(h) There is some proposition that C does not believe;
(i) C believes A iff C's credence in A is 1.
(j) C believes $\neg A$ iff C's credence in A is 0.

Constraints (a) and (b) are needed to ensure that C is a model for *this* agent, rather than one who has some suitably corrected beliefs. Constraints (c) through (f) are needed to ensure that C provides a model for the agent's credences, in the Ramseyian sense outlined above. And constraints (g) and (j) are needed to ensure that C really is coherent! Constraints (g) and (h) are familiar, and we showed above that given Ramsey's definitions of degrees of belief, any coherent agent will satisfy (i) and (j).

Note that I do not say that it is a requirement of coherence, much less rationality, that the agent herself has *any* of these properties, especially the closure of her beliefs. Well, it is a constraint that she has the last property, but only because this follows from (a) and (h). All I say is that coherence requires is that the agent *could* extend her beliefs so that P models her credences, and her beliefs are closed under entailment. It seems very plausible that this is necessary for coherence. (I think it is necessary for rationality too, but that is another matter.)

The important result is that if such a (possible) agent C exists, then Bel must be a probability function. And we have already argued that the agent is coherent iff C exists. Since Field and Schiffer say that a coherent agent can have credences that do not satisfy the probability calculus, this result directly contradicts their theories. The proof that C's existence entails that Bel is a probability function is not too hard, though it is a little long.

Let $\mathbf{K}^*$ be the set of propositions that C believes, and let Bel^* be C's credence function. Since for each p_i, $Bel(p_i) = 1/y$, and $1/y > 0$, it follows that $\neg p_i \notin \mathbf{K}^*$, for each p_i. To ease the exposition from now on, for any set S of propositions, I will write S^* for the disjunction of all the elements in S. Using this notation, we can put the constraint that $\neg p_i \notin \mathbf{K}^*$ as follows.

(1) $(S \subseteq P \;\&\; S^* \in \mathbf{K}^*) \rightarrow S = P$

Also using this notation, we can rewrite constraint (f) as follows.

(2) If $Bel(A) = x/y$ then $\exists S$: $((S \subseteq P \;\&\; |S| = x) \;\&\; (S^* \leftrightarrow A \in \mathbf{K}^*))$

(The expression $|S|$ in (2), and (f), refers to the cardinality of S). We now prove the following lemmas:

(L1) $\underline{\vee}(p_1, ..., p_y) \in \mathbf{K}^*$
(L2) $(S \subseteq P \;\&\; \neg S^* \in \mathbf{K}^*) \rightarrow S = \varnothing$

The notation $\underline{\vee}$ in (L1) refers to exclusive disjunction. There is a difficulty with representing n-place exclusive disjunction, since if we just took $A \,\underline{\vee}\, B =_{df} (A \vee B) \;\&\; \neg(A \;\&\; B)$, we would have the result that $(p_1 \,\underline{\vee}\, p_2) \,\underline{\vee}\, p_3$ would be true iff one or three of the p_i were true. So I allow exclusive disjunction to be an n-ary connective, written $\underline{\vee}(p_1, ..., p_n)$, which is true provided exactly one of the p_i is true. (This notation is borrowed from James McCawley 1993.)

To prove (L1), we first prove that the inclusive disjunction $p_1 \vee \ldots \vee p_y$, i.e. P^*, is in $\mathbf{K}^*$, and then that the exclusive disjunction is in $\mathbf{K}^*$. Let A be any proposition such that $Bel(A)$ and $Bel(\neg A)$ are defined. Without loss of generality, say $Bel(A) = x_1/y$ and $Bel(\neg A) = x_2/y$. So there are sets S_1 and S_2, both subsets of P, such that $|S_1| = x_1$, and $|S_2| = x_2$, and $S_1{}^* \leftrightarrow A \in \mathbf{K}^*$, and $S_2{}^* \leftrightarrow \neg A \in \mathbf{K}^*$. Since $A \vee \neg A \in \mathbf{K}^*$, and $\mathbf{K}^*$ is closed under entailment, this implies that $S_1{}^* \vee S_2{}^* \in \mathbf{K}^*$. Since $S_1{}^* \vee S_2{}^* \vdash P^*$, since all the disjuncts in $S_1{}^*$ and $S_2{}^*$ are in P^*, this implies that $P^* \in \mathbf{K}^*$. Now, (d) says that for any two distinct members of P, p_i and p_j, $\neg(p_i \wedge p_j) \in \mathbf{K}^*$. The last two results, plus the closure of $\mathbf{K}^*$ implies that $\underline{\vee}(p_1, ..., p_y) \in \mathbf{K}^*$.

(L2) follows fairly directly from (1). If $S \subseteq P$, then $\neg S^*$ will be $(P - S)^*$, which is also a subset of P. So by (1), if $(P - S)^* \in \mathbf{K}^*$, then $P - S = P$, hence $S = \varnothing$, as required. These lemmas are sufficient to prove that the following theorems hold for any propositions A, B in Γ.

(T1) If $\vdash A$ then $Bel(A) = 1$
(T2) If $\vdash \neg A$ then $Bel(A) = 0$
(T3) $Bel(A) + Bel(B) = Bel(A \vee B) + Bel(A \;\&\; B)$
(T4) If $A \vdash B$ then $Bel(A) \leq Bel(B)$
(T5) $0 \leq Bel(A) \leq 1$
(T6) If $Bel(A) = x/y$ and $Bel(A) = z/y$ then $x = z$

If (T1) to (T6) are all true, then it follows that Bel is a probability function defined over the elements of Γ, as required. (T1) follows directly from (g) and (i), and (T2) follows from (g) and (j). In the proofs that follow I will use S (occasionally subscripted) to refer to a subset of P.

Assume $Bel(A) = x/y$ and $Bel(B) = z/y$. It follows that there are S_1 and S_2 such that $S_1{}^* \leftrightarrow A$ and $S_2{}^* \leftrightarrow B$ are in $\mathbf{K}^*$, $|S_1| = x$ and $|S_2| = z$. It follows the way S_1 and S_2 are defined that $(S_1{}^* \vee S_2{}^*) \leftrightarrow (S_1 \cup S_2)^*$ and $(S_1{}^* \wedge S_2{}^*) \leftrightarrow (S_1 \cap S_2)^*$ are logical truths and hence in $\mathbf{K}^*$. We can tell from (L1) that if any two subsets of P, say S_3 and S_4, are such that $S_3 \leftrightarrow S_4$ is in $\mathbf{K}^*$ then $S_3 = S_4$. Finally, if two sets are subsets of P then their intersection and union are also subsets of P. From all these, it follows that $(A \vee B) \leftrightarrow (S_1 \cup S_2)^*$ is in $\mathbf{K}^*$. Hence by the right-to-left direction of (f), it follows that $Bel(A \vee B) = |S_1 \cup S_2|/y$. Similarly $A \wedge B \leftrightarrow (S_1 \cap S_2)^*$ is in $\mathbf{K}^*$, so $Bel(A \wedge B) = |S_1 \cap S_2|/y$. We know from set theory that $|S_1| + |S_2| = |S_1 \cup S_2| + |S_1 \cap S_2|$. Dividing both sides of this equation by y and substituting terms which we have already shown to be identical gives us (T3).

If $A \vdash B$ then $A \rightarrow B$ is a theorem. Assume $Bel(A) = x/y$ and $Bel(B) = z/y$. As above, it follows that there are S_1 and S_2 such that $S_1^* \leftrightarrow A$ and $S_2^* \leftrightarrow B$ are in $\mathbf{K}^*$, $|S_1| = x$ and $|S_2| = z$. Since $\mathbf{K}^*$ is closed it contains $A \rightarrow B$ and hence $S_1^* \rightarrow S_2^*$, or equivalently, $\neg(S_1^* \,\&\, \neg S_2^*)$. Assume p_i is in S_1 and not in S_2. Since $p_i \vdash (S_1^* \,\&\, \neg S_2^*)$, and $\neg(S_1^* \,\&\, \neg S_2^*)$ is in $\mathbf{K}^*$, it follows that $\neg p_i^*$ is in $\mathbf{K}^*$. As we showed earlier, it is inconsistent to have any p_i in $\mathbf{K}^*$. Hence there is no such element p_i, so S_1 is a subset of S_2. Hence $x \leq z$, so $Bel(A) \leq Bel(B)$, as required for (T4).

We have actually proved something more general than (T4). Since all that was used was that $\mathbf{K}^*$ contains $\neg(A^* \,\&\, \neg B^*)$, it follows that whenever $A \rightarrow B \in \mathbf{K}$, then $Bel(A) \leq Bel(B)$.

(T5) follows immediately from (T4). Since $A \vdash T$, where T is a tautology, and $\mathrm{Bel}(T) = 1$, $\mathrm{Bel}(A) \leq 1$. And since $\perp \vdash A$, where $\perp$ is the falsum, and $\mathrm{Bel}(\perp) = 0$, $0 \leq \mathrm{Bel}(A)$.

From our definitions of credences, it would not be contradictory to say that (T6) failed to obtain. The reason is that $Bel(A) = x/y$ just means that there is some set S of cardinality x such that $Bel(S) = Bel(A)$, not that all sets S satisfying $Bel(S) = Bel(A)$ have this cardinality. This might make us question the use of '=' signs when discussing $Bel(\cdot)$. Fortunately, however, we can prove that it is a requirement of coherence that (T6) holds. Assume that it doesn't. So there are sets S_1 and S_2 of different cardinality such that C believes both $A \leftrightarrow S_1^*$ and $A \leftrightarrow S_2^*$ and hence by closure believes $S_1^* \leftrightarrow S_2^*$. Let S_3 be the set $(S_1 \cup S_2) - (S_1 \cap S_2)$. Since S_1 and S_2 are of different cardinality S_3 is non-empty. Since $S_1^* \leftrightarrow S_2^*$ entails $\neg S_3^*$, it follows that C believes $\neg S_3^*$ and hence by (L2) that S_3 is empty. This contradicts our assumption that S_1 and S_2 are of different sizes, so S_1 and S_2 must be of the same cardinality, as required for (T6).

Now it simply falls to us to show that these requirements ensure that *Bel* is a probability relation. By (T6) *Bel* is a function from propositions to numbers (i.e. it is uniquely valued). By (T5) it satisfies (Pr1), by (T1) (and (T6)) it satisfies (Pr2) and by (T2) and (T3) it satisfies (Pr3). Hence it is a probability function.

5.6. Objections

So we have a long argument that the kind of cognitive states endorsed, even recommended, by Field's theory are incoherent, and that this shows something is wrong with Field's theory. Our overall line of reasoning rests on three main premises.

P1. Ramsey's analysis of what it is to believe p to degree x/y is correct.
P2. If Ramsey's analysis is correct, then coherence requires that partial belief conforms to the probability calculus.
P3. If Field's theory requires that we be incoherent to believe that a sentence is indeterminate, then Field's theory is mistaken.

From these, plus the fact that Field's theory quite explicitly does require that we not have our partial beliefs conform to the probability calculus to judge that a sentence is indeterminate, the falsity of Field's theory follows. As with any long philosophical argument, *every* step is vulnerable to challenge. One could reject P3, accepting that to judge that there is indeterminacy is a kind of incoherence, and make Field's theory a descriptive theory of how we actually (incoherently) think about the indeterminate. Such a move is not without precedent, it is fairly close in spirit, if not in detail, to Roy Sorensen's version of epistemicism. But it seems to be a long way from Field's position. After all, he says that he believes, and wants to believe in indeterminacy, but I doubt that he wants to be incoherent.

The argument for P2 is the bulk of the previous section, and there are a few places at which it could be challenged. As I noted a few times, several of the moves I make will look dubious if you take *coherent* to either mean, or imply, or be implied by, *rational*. If you take coherence to be a separate norm to rationality, however, I think the assumptions made in the argument for P2 are fairly sound.

The most important attacks on the argument, I feel, are attacks on P1. Before we look at these attacks, note one thing about what an attack must do to succeed. I said above that Ramsey's theory was part of the best analysis of what it was to believe *p* to degree *x*/*y*. I think that's true. But I don't need anything that strong for the argument to succeed. All I need is that it is *a priori* that Ramsey's analysis is extensionally correct. At no stage in the argument did I appeal to the claim that the analysis could serve as a *reductive* analysis. It can, but that is no part of the argument. So an objection that merely attacks the reductive aspect of Ramsey's theory will be no use here.

The best way to object to P1 then, will be to find some other theory of what it is to believe *p* to degree *x*/*y*, and use that to challenge the extensional adequacy of Ramsey's analysis. I don't hold that one needs a theory of what it is to believe something to a degree in order to ground a challenge because I hold that any philosophical theorising must be grounded in general theories, or any other such absurd claim. Rather, I hold it because the concept of numerical degrees of belief is so technical, and so obscure to ordinary thought, that without such a theory we know nothing about it. Remember just how hard it is to say just what, if anything, we mean by saying *X*'s degree of belief in *p* is 0.5613.

One obvious such theory springs to mind - numerical degrees of belief represent willingness to accept bets at certain odds. Roughly, if you believe *p* to degree *x*, and there is no declining marginal utility of *u*'s, then you are disposed to accept a bet that pays you 1*u* iff *p*, for short, a bet on *p* or a *p*-bet, iff the cost of the bet is no greater than *x u*'s The reference to dispositions here is crucial, since there may be many bets that we are in *some* sense disposed to take, but would not take if offered, because we suspect (often reasonably) that we would only be offered these bets by someone who knew more about the underlying situation than we did, and was trying to exploit our ignorance. So if I am about as confident that *p* as that $\neg p$, but this is more because I have little information one way or the other about whether *p* is true, and I have no reason to think it is indeterminate whether *p* then my degree of belief in *p* should be about 1/2, but I may not accept a *p*-bet for 0.5 *u*'s, because if anyone offered such a bet I would think they knew, or had good reason to believe, that *p* is false. Stating the theory in terms of dispositions removes this particular difficulty.

Nevertheless, there are problems with this general approach. First, there is an issue about whether the relevant units of currency can be found. It is commonly held that certain kinds of lottery tickets have no declining marginal utility, but all that we really know about these tickets is that the marginal *use*-value of each extra ticket is non-declining, given some plausible assumptions. This does *not* imply that their marginal *total*-value is non-declining, since they may have varying *exchange*-values. (In practice, they will have, since it is easier to find suckers to buy 20 lottery tickets than to find suckers to buy 2000 of them.) So the apparently operational character of the analysis is misleading. Secondly, the theory runs into problems with people who have an aversion to, or a like of, gambling. Such people will pay less, or more, for a ticket than its face value. Even if you are not such a person, it is obvious that some people have these desires, so our reduction of numerical degrees of belief to betting dispositions cannot be an *analysis* as it stands, although in *some* cases they may be *evidence* as to which numerical degrees of belief the agent has. Perhaps we can solve this problem by complicating the analysis, but it is hard to see just how this is to be plausibly done. [Some citations here would be useful, and will be added presently.]

Even if we bracket these concerns, it is hard to see just how this analysis can be the ground for an *objection* to P1. Most theorists who think that numerical degrees of belief can be analysed in terms of