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ON DE FINETTI'S PHILOSOPHY OF PROBABILITY

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Colin Elliot
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PROMOTIECOMMISSIE

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Contents

‘Mister Jagielski, will there be good weather?’ they ask.

The raftsman looks around at the sky (he reads the sky, they say) and, pushing hard on his pole until it bends like a bow, pronounces: ‘There are clouds, but they might go away.’

‘An optimist!’ the lecturers marvel.

Ryszard Kapuściński, *A Survivor on a Raft*

Chapter 1

Introduction

Bruno de Finetti (1906-1985) was one of the most prominent writers on probability in the 20th century. He was one of the founders of the subjective school in the philosophy of probability and, in a prolific career in academia, actuarial science and insurance, influenced a variety of fields, including mathematics, economics and philosophy. In this thesis my main focus will be on his philosophy of probability, concentrating in particular on some of its more influential and controversial aspects. My aim is to contribute to the debate in two ways: in the first half, I provide a new insight into how certain aspects of de Finetti's thought, particularly those regarding pragmatism and objectivity, are to be understood, discussed and integrated in contemporary philosophical debates. In particular, I discuss his pragmatism and his objectivity. To do this, I shall take a historically informed view of de Finetti's ideas and apply a version of them to current debates, thus suggesting a potentially productive direction these debates can move in. This will not be done in a primarily historical context; in fact, some of his ideas will be analysed ahistorically (i.e. using arguments not from de Finetti's time) in order to bring to the fore their relevance to current debate. The emphasis in the text, therefore—where useful—will be on the contemporary perspective, but hopefully without distorting any of de Finetti's ideas. In the second part of the thesis, I move on to specific aspects of de Finetti's philosophical programme, with the aim of contributing to the debate on these themes; the first half looks to the past for inspiration, whereas the second aims to be forward-looking. Two main topics are discussed: countable additivity and the betting definition of degrees of belief, taking de Finetti's position as a starting point but moving on to encompass broader questions in their conclusions.

1.1 Introducing de Finetti's probability

De Finetti was mainly a mathematician, but one who had strong and influential philosophical views on the subject of mathematics. The conceptual starting point for his theory of probability is the feeling of uncertainty we humans experience. What de Finetti saw as our good, shared intuitive knowledge of this feeling should, he thought, be the target that a mathematical theory of probability models. In order to do this, we need to be able to record numerically the level of uncertainty, or *degree of belief*, that a given agent has in a given situation. The mathematical theory of probability, then, can operate on these numerical degrees of belief and help us see the full consequences of an uncertainty state. For de Finetti, the theory of probability is a logic of the uncertain. It does a similar job, for example, as the one done by propositional logic: there, we start from a set of propositions, each of which we take to be true or false, and tease out the consequences on the basis of given inference rules. Each all-or-nothing belief can be associated to a 0, if (we believe that) the proposition is false, and 1 if (we believe that) it is true. In probability, on the other hand, we start from a set of degrees of belief, and so we will have numbers between 0 and 1 (including the extremes, because we can still represent certainty about things) instead of 0s and 1s, and the rules of probability will help us tease out the consequences of that set. For example, probability shows us how our degrees of belief about the different possible outcomes of an event, or about the correctness of different hypotheses, should be combined; or it can show us how to update our opinion as the situation evolves and we collect new evidence.[1]

[1] Combining deductive logic and probability is, in fact, no trivial matter; I have included this juxtaposition simply to explain the role that de Finetti saw probability as playing: an equivalent one to deductive logic, in cases where we can't have 0-1 beliefs.

One of the possible bridges between the qualitative feeling of uncertainty, the numerical values needed for mathematics, and the rules of probability is based on the realisation that it is if, and only if, these numerical degrees of belief respect the basic rules of probability that we are not open to systems of bets which lead to a certain loss. This is not a starting point of the theory, but a theorem: the Dutch Book theorem shows that betting quotes must be probabilities; and the Dutch Book argument concludes from this, via the interpretation of degrees of belief as betting prices, that the former must be probabilities as well. This argument is compelling but not obvious, and at various points in my thesis I will discuss de Finetti's requirement that probability be operationalisable and examine and criticise the purported link between betting prices, subjective feeling of uncertainty of agents and the numerical degrees of belief that reflect them. These numerical degrees of belief, which, at least in principle, can reflect an agent's uncertainty, can be called subjective probabilities.

De Finetti accepts *only* these subjective probabilities as a meaningful[2] understanding of the concept of probability. His stance is anti-metaphysical, and he rejects any reference to the presumed existence of chance in a mind-independent world, or to the idea that the long-run frequency in repeated trials of an experiment is an indication of an existing, but unknown, underlying probability of the phenomenon in question. He is against the classical conception of probability of Laplace and others, in which the starting point is equal-probability events: he sees that definition as circular, in that it contains a concept of probability right from the start; he argues against the logical probability that began with Keynes, which saw true rational probability as being dictated by the evidence. And he also railed against measure-theoretic probability (the school of thought that now holds sway, at least in mainstream mathematics).[3] While rejecting the above positions as interpretations of probability, he did however give importance to relative frequency: this is factual evidence that can help us make

[2] *Meaning* here is a technical term connected to a pragmatist theory of meaning: this will be one of the topics dealt with in 2.

[3] I will treat some of these arguments in more detail in what follows; for the positions sketched here see, for example Gillies (2000), de Finetti's arguments against each of these appear in various places, but can be found, for example, in de Finetti (2008).

up our mind as to what our degree of belief is—but frequency is not constitutive of probability itself. The reason why frequency is treated differently to the other concepts is that it is *verifiable* data: this is of crucial importance in de Finetti's view, and it informs his whole approach.

1.2 The impact of subjective probability

The rejection of all non-subjective interpretations of probability is one of the ways de Finetti swam against the current; his rejection of *countable additivity* (more on which below), is another. It could be said that de Finetti eventually lost his main intellectual battles: the focus on verifiable concepts, which, as I will argue in Chapters 2 and 3, implies the exclusive adoption of subjective probability, has somewhat faded from the broader philosophical discourse, while countable additivity has been adopted in the vast majority of today's mathematical texts on probability. On the other hand, however, his influence is great and enduring: subjective probability is seen as one of the leading interpretations of probability (see Hájek (2011); there are many different forms of subjective probability, and there is no need to be as radical as de Finetti in this, of course); and his mathematical results, of which his work on exchangeable probabilities (see de Finetti (1928) and de Finetti (1937/1964)) is the most famous, are an important part of the probability curriculum (Von Plato (1994, p. 240)) and an active area of research beyond mathematics (Von Plato, 1989, p. 264).

Indeed, a notable aspect of de Finetti's writing is that, throughout his career, even as his ideas evolved and his tone and emphasis changed, he largely stuck to the philosophical tenets that he seemed to have developed by the late 1920s. Overall, de Finetti's intellectual project is remarkably consistent, and the mathematical and philosophical aspects of it well integrated. I think this must be one reason for its enduring appeal. At the same time, however, the deep roots of de Finetti's opus in the philosophical ideas of the 1930s, 1920s and earlier are often enigmatic to contemporary philosophers, and tend to clash with their own view of what is important in a philosophical approach to probability. Ideas such as behaviourism and operationalism, seemingly abandoned in broader philosophy after the era of the Vienna Circle, are still discussed today in the philosophy of probability, perhaps due to the enduring influence of de Finetti (Eriksson & Hájek (2007) take this view). In mathematics, de Finetti's opposition, on conceptual grounds, to countable additivity, which makes the theory simpler and more powerful, might puzzle scholars in the field who see adherence to strictly philosophical views as less of a priority. I shall now say a bit more about countable additivity, and how it fits into de Finetti's overall project.

1.3 De Finetti's project

There is an important change in the third edition (2006) of Howson and Urbach's influential book *Scientific reasoning: the Bayesian approach*: the authors no longer accept the mathematical rule of Countable Additivity as an axiom of probability; they have been convinced, they write, by de Finetti's famous arguments against the principle. I deal with this in more detail in Chapter 5, but it is worth mentioning here because it provides a striking example of de Finetti's philosophical and mathematical thought in action, and gives us an initial idea of what a theory of probability is for de Finetti. Countable Additivity is a rule governing the behaviour of the probabilities of countably infinite incompatible events. It is a strengthening of Finite Additivity, which says that if events A and B are incompatible, with probabilities P(A) and P(B), then the probability of their union A or B is P(A) + P(B). Countable additivity imposes that this be true also of a countable sequence of incompatible events. Kolmogorov (1933/1956) included an axiom equivalent to this in 1933, and this made probability into a *measure*, so it could be studied in the well-developed branch of mathematics called *measure theory*. Studying probability as a measure, with the powerful limit theorems that this makes possible, can be said to be the beginning of the modern mathematical theory of probability. Amongst mathematicians, the adoption of Countable Additivity is by now uncontroversial (although a theory using only Finite Additivity does exist). Given the power and the convenience of the rule, a refusal to adopt it might seem rather strange. But this refusal lies at the heart of de Finetti's conception of probability. Although his opposition to Countable Additivity can be found in various writings of his, I see his 1930 response to French mathematician Fréchet (de Finetti (1930)) as the best formulation of the deep-seated reasons underlying this choice.[4] In the 1930 paper, which I discuss at more length in Chapter 2, de Finetti compares probability to weight. While we need an operational definition of weight in order to conduct any sort of application with it (and even just to speak meaningfully about it), weight has certain characteristics which are simply its own, which we cannot modify by changing the axioms of the mathematical rules we use to operate with it formally. The same is true, according to de Finetti, of probability. In this context, it is our intuition that dictates the basic features of probability, and our intuition, de Finetti argues, does not require countable additivity. Therefore, it should not be included when we model our intuitive idea of probability into a mathematical object. The addition of this rule restricts the ways in which degrees of belief can be combined, and it has no right to do this, since it comes from the wrong place: not from our basic idea of what probability is, but from the technical convenience we gain in the model. This kind of attention to the conceptual foundations of his formal theory is something that de Finetti applies throughout his writing. I think it is a valuable method, that can be adapted and applied also to debates that lie beyond what is normally considered de-Finettian territory. I propose such an application of this methodology, as I interpret it, in Chapter 5.

Although I wrote above that probability has certain basic features dictated by our intuition, I have been careful not to argue that it has *real-world* features, bearing in mind the well-known opening phrase, in capital letters, of de Finetti's book (1974/1990): PROBABILITY DOES NOT EXIST. Perhaps, on reflection, that some confusion would have been avoided if he had stated that probability, while not existing as a mind-independent phenomenon, definitely has some sort of existence: it exists as a mental phenomenon, as the feeling we humans have when we are not absolutely certain of the truthfulness of

[4]The more technical reasons are, amongst other places, in de Finetti (1972) and de Finetti (1974/1990).

a proposition, or of whether or not an event will occur. And while the modelling of this concept in mathematics will add some artefacts which do not belong to the intuitive core concept itself (for example, we do not really have real-valued degrees of belief), it should not alter or restrict it (hence the rejection of countable additivity). Furthermore, the rules should not alter the *content* of our beliefs at all: these mathematical rules of probability do not purport to be an attempt at a theory of rationality. If we accept the basic mathematical rules of probability, it follows that - given the probabilistic assessment we start out with - such-and-such will be the consequences; but this, as de Finetti remarks, is a conditional offer (de Finetti (1974/1990)). The usual rules of probability are well motivated, de Finetti thinks, but we are not forced to accept the motivation. I write more on this in Chapter 3.

De Finetti's project in probability, then, is ambitious, but in a way that doesn't chime in well with most of the writing on the subject published after, or indeed during, his career. In his writings on probability, he wanted to capture *the real thing*, by using the only philosophical interpretation of probability that vindicates our intuitive understanding of it, making it a meaningful concept, and modelling it in a properly designed mathematical framework that respects these features. This is exceptional for a mathematician, especially when compared to the ones that came after him, who generally studied probability as a purely formal concept. In this light, his ambition is vast. But in the branch of philosophy where de Finetti often nowadays ends up being located, formal epistemology, his project seems bizarrely unambitious and laissez-fare. De Finetti's degree of belief conception of probability fits very well into some branches of formal epistemology (I explore this in Chapter 3): let us suppose that rational agents have partial beliefs that take the form of probabilities (this is de Finetti's starting point); what, then, should these probabilities be, in a given situation? This seems the logical next step in the theory, and one which, broadly speaking, interests many contemporary writers in the field. But de Finetti pointedly refused to give *any* sort of answer to this, insisting that any combination of degrees of belief that respects the rules of probability is fine by his theory; indeed, countable additivity is rejected precisely on the grounds that it adds content to what should be a content-less theory. By these contemporary standards, then, de Finetti's theory is lacking. Surely a theory of rationality cannot allow people to believe anything they like, and still be called rational! This is the criticism often levelled at the theory of de Finetti.

The simple answer, which I defend in 3, is that his is not a theory of rationality, but a theory of probability in which the mathematical and the philosophical aspects are developed together, at the same time. It is true that, in his earlier writings, de Finetti expressed a strong relativism, famously writing that there are no privileged grounds for defining as crazy the belief that eclipses cause wars (de Finetti, 1931/1989, pp. 179-180). Later, de Finetti takes the more moderate position that while we have arguments to support our belief states, these are usually too subtle to be expressed in mathematics, and should be left out of the theory (see Berkovitz (2018) on this). However, I see this is a change in tone and emphasis rather than a substantial change of message: probability, as seen by de Finetti, is an empty, content-less formal system that can help us draw out the consequences of our probabilistic assessments, or update these assessments according to relevant information. It is simply not in the business of adjudicating which belief states are better than others. Having outlined de Finetti's main ideas on probability, the most important features of his project and aspects of the impact it had, I will say a few words about his philosophical background, which I think helps give a more rounded picture.

1.4 Inspiration and milieu

De Finetti mentions his sources of philosophical inspiration in various places (for excellent discussions of this see Von Plato (1994) and Jeffrey (1989)), the more important of them being David Hume, Ernst Mach, Mauro Calderoni and Giovanni Vailati, Adriano Tilgher, Albert Einstein and Percy W. Bridgman. Tilgher (1921/1923), who is abundantly quoted in de Finetti's essay *Probabilismo*, wrote a booklet describing the various relativist positions in philosophy, the first edition of which was published to mark the occasion of Einstein's visit to Italy in 1921. Amongst those discussed in the booklet and referred to by de Finetti are Hans Vahihinger, Oswald Spengler and Einstein himself. The current of thought I shall concentrate on here, which, I feel, had the most lasting influence on de Finetti, is the idea, originating from Mach, Einstein and then Bridgman, that concepts need an operational definition in order to be meaningful, and his subsequent amalgamation of these ideas into a pragmatist view heavily influenced by Vailati and Calderoni's treatise on pragmatism, which was itself a discussion of Charles S. Peirce. Reading Tilgher (1921/1923) can help us understand the ambitions of the younger de Finetti. Tilgher starts his chapter on Einstein with an accurate sketch of the physicist's results and innovations (1921/1923, pp. 37-47), and makes it clear that Einstein intended his results to be strictly mathematical-physical, and not part of a broader philosophical relativism. Tilgher, however, then proceeds (1921/1923, pp. 47-54) to place Einstein in a broader historical context which includes trends such as philosophical pragmatism, the rise to prominence of finance capitalism, imperialism and *titanism*[5] in art. I do not intend to go too deeply into this aspect of de Finetti's thought, although, on reading his *Probabilismo*, it becomes clear that Tilgher's writing made an impression on him. My point here, quite simply, is that de Finetti too felt part of a broader movement that had rejected the metaphysical fallacies of such things as absolute truth, epistemologically privileged points of view and absolute time and space. Like Tilgher, he found Einstein to be a powerful example of what heights can be reached if we cast aside those old flawed ideas and embrace radical relativism and his own probabilism: he urged that we take "a living, elastic, and psychological logic as the fundamental instrument of scientific thought, instead of the ordinary, categorical, rigid and cold logic. The logical instrument that we need is the subjective theory of probability" (de Finetti, 1931/1989, p. 172).

De Finetti is anti-realist, pragmatist and empiricist (see Galavotti (1989)), but while his ideas sometimes seem close to those of the Vi-

[5] "An attitude of rebelliousness, held even in the knowledge that it will fail, against all the superior forces (divinity, fate, nature, despotic political or socio-economic power, etc.) that dominate man and oppress his vital impulses, his freedom and his very responsibility." (translated by me from the Italian in the online *Vocabolario Treccani*)

enna Circle, he developed them independently.[6] In addition to the authors mentioned above, he considered himself to be inspired by, and continuing with, the work of David Hume. Comparing their basic views on probability is interesting. Here is Hume on probability in *An Enquiry Concerning Human Understanding*: "Though there be no such thing as Chance in the world; our ignorance of the real cause of any event has the same influence on the understanding, and begets a like species of belief or opinion" (Hume, 1748/1993, Section VI - *Of Probability*). And de Finetti: "Probability does not exist. [...] Probabilistic reasoning [...] merely stems from our being uncertain about something" (de Finetti, 1974/1990, p. x). Notwithstanding the similarity between these two passages, I do not mean to suggest that these are the words that actually inspired de Finetti; we need to consider also that the (1974/1990) book is a mature expression of de Finetti's thought. But the affinity is clear. As for continuing with Hume's work, de Finetti thought that his theorem on exchangeable events, together with Bayes' theorem, vindicated and made precise what Hume meant (Galavotti, 1989, p. 250) when he wrote the following:

> A wise man [...] proportions his belief to the evidence. In such conclusions as are founded on an infallible experience, he expects the event with the last degree of assurance [...]. In other cases, he proceeds with more caution: He weighs the opposite experiments: He considers which side is supported by the greater number of experiments: To that side he inclines, without doubt or hesitation; and when at last he fixes his judgement, the evidence exceeds not what we properly call probability." (Hume, 1748/1993, Section VI - *Of Miracles*)

The philosophical success of that theorem, for de Finetti, lay in its showing that we are justified in aligning our degree of belief to the frequency which emerges from repeated trials of a certain experiment, thus expecting the future to resemble the past. This is done without

[6]See Jeffrey (1989, pp. 225-226, 234-235) for differences between de Finetti and Carnap.

invoking objective real-world probabilities or independent events: it is a purely subjectivist justification of Hume.

This concludes my description of de Finetti's philosophy of probability. I shall now conclude this introductory chapter by briefly outlining the themes taken up in the rest of the thesis.

1.5 Outline of the thesis

The main body of the thesis is in four chapters. Each of these can be read as an independent piece of research (for example, a reader can skip to Chapter 4 and understand it without reading the rest). The chapters contain cross-references: a concept mentioned in one may be dealt with more fully in another. Although the chapters can be read independently, however, many of the arguments in the thesis can be seen in the context of the coherent global view of de Finetti's work that I intend to construct - as outlined above and to be returned to in my concluding remarks in Chapter 6. Some strands, on the other hand, grow out of this view and end up in other philosophical fields and debates. The chapters are structured as follows.

Chapter 2 takes a recent article by Eriksson & Hájek (2007) as its starting point. In it, the authors defend a position they call *primitivism* about degrees of belief: this is the idea that the best thing to do is to leave 'degree of belief' as an unanalysed primitive concept, a building block for our conceptual frameworks. A major reason why they propose this is that all the existing analyses of the concept of degree of belief fail, they argue—and so perhaps we should (temporarily) abandon the attempt to analyse the concept. Having it as an unanalysed primitive is not, after all, a bad direction to take. A particularly interesting aspect of their argument is that they depict de Finetti as a strict operationalist with regard to degrees of belief (i.e. as seeing a degree of belief as identical to the result of the operations which, by definition, measure it), but they also wonder why, then, he worried about having *good* operational definitions of degrees of belief. This would suggest that there is something behind mere operations after all, and de Finetti seemingly wanted to measure this 'something' accurately. My view, in fact, is that de Finetti himself is also a sort of *primitivist* about degrees of belief, albeit of a different sort to Eriksson & Hájek (2007). In arguing this conclusion, I trace de Finetti's operationalism and paint a new (to my knowledge) picture of his pragmatism, as it emerges from a comparison with the pragmatism of Peirce.

In **Chapter 3**, I deal with the concept of objectivity in de Finetti's subjective probability. De Finetti's theory is often interpreted as an 'anything goes' approach, because he refuses to add any formal rules that might influence the content of a person's degrees of belief. My main argument will be that this criticism, even though it is widespread in the literature, is misplaced. De Finetti is not interested in constructing a formal theory of rationality, and his theory has ended up being criticised for doing, purportedly badly, something which it was never intended to do. A crucial factor in the vein of criticism I examine is the perceived lack of objectivity in de Finetti's theory. But, on reading his work one realises that objectivity plays a central, motivating role in the whole approach. I think this arguing at cross purposes has led the current debate down a blind alley. I map out the senses of objectivity employed by de Finetti and his critics, to put them on the same page with regards to the usage of the concept, and allow the debate, potentially, to progress in a productive direction.

In **Chapter 4**, I discuss an operational definition of probability given in de Finetti (1974/1990). In this, a degree of belief is measured by proposing a special kind of bet to a person and observing which odds she accepts. In this bet, the agent does not know whether she is betting for or against the event in question in question, which, in theory, forces her to give her honest opinion. I argue, however, that it is in fact a powerful distorting factor. The position that I will defend is that, in order to make the definition work, we have to make assumptions so powerful that they render the betting definition redundant. This chapter moves on from de Finetti in two ways: firstly, it does not attempt to enhance his arguments in the context of contemporary debates, as Chapters 2 and 3 do; secondly, de Finetti himself abandoned bets as an operational device in his later writing. But the link between bets and degrees of belief has lived on in the literature, and so has the debate about it. I propose that my arguments in this chapter can be a good reason for abandoning the betting definition of degrees of belief.

In **Chapter 5**, I apply the de Finettian philosophical methodology outlined in Section 1.3 to the formulation of a proposed way to move the countable additivity debate forward in a broad sense, and attempt a specific contribution in that vein. The specific contribution will be to propose reasons for why two of the major approaches in *objective Bayesianism* (I will look at Edwin T. Jaynes and Jon Williamson) are justified in adopting countable additivity as an axiom; these reasons are different from the ones they give themselves which, I feel, can be improved on. My broader point will be that a general solution to the debate on CA, capable of satisfying everybody, is impossible. And my general methodology here is inspired by de Finetti in this sense: he takes seriously the fundamentals of what he takes probability to be, and what he expects from a formal model of it; this informs his mathematical choices throughout. This method of relating fundamentals to formalisations will be the guide throughout the chapter. My **Conclusion** follows in **Chapter 6**.

Chapter 2

Pragmatism

2.1 Introduction

Bruno de Finetti (1906-1985) gave major contributions to the study of mathematical and philosophical aspects of probability and statistics. He is in one of the founders of the subjectivist school in probability, sometimes also known as subjective Bayesianism, a leading philosophical interpretation of probability. This chapter addressees the foundational aspects that lie at the very basis of his philosophical thought, with particular reference to the role of pragmatism, referred to in the existing literature, but not sufficiently explored; here, I shall attempt to paint a more faithful and detailed picture of it. The reason for this is twofold: firstly, I think that the foundational ideas of an influential thinker such as de Finetti are of historical interest in and of themselves; secondly, an exploration of his pragmatism is the best way to counter a widespread—and, I will argue, mistaken—reading of him in which he is depicted as a strict operationalist. In a somewhat ahistorical way, I will compare de Finetti's position to the recent view on degrees of belief expressed by Eriksson & Hájek (2007), called *primitivism*, and argue that de Finetti is a primitivist of sorts: he thought that probability existed as a psychological phenomenon, but that we needed an operational definition of it in order to speak meaningfully about it and operate mathematically with it. The main features of this fundamental concept must be respected in its operational definition and formal model. This is a relation of target to model.

De Finetti thought that the only meaningful way to understand probability was as a *degree of belief* which could be held by a human agent. A major reason for thinking this, I will argue below, is that the concept thus becomes checkable: we can, conceivably at least, check to what degree an agent believes something to be true. For example, degrees of belief might be reflected in the betting prices we choose in given situations, and this is, in principle, verifiable. What is more, once we represent degrees of belief numerically as betting prices, the celebrated Dutch Book theorem (see de Finetti (1974/1990, p. 87) among many others) states that if (and only if) a set of betting quotients respects the probability axioms, then there does not exist a combination of buying or selling these bets that results in a certain loss (or gain). In the Dutch Book argument for *probabilism*, degrees of belief are interpreted as betting quotients, and the result is then interpreted as saying that if (and only if) an agent has a set of degrees of belief that respect the probability axioms, then she is not open to accepting system of bets that can bring her a certain loss. This means that degrees of belief should be probabilities, if the agent does not wish to be open to certain losses. In Chapter 4 I criticise the purported link between bets and degrees of belief; but *probabilism*, the argument that degrees of belief should be probabilities, does not depend on betting arguments succeeding[1], and nor are bets the only way to operationalise degrees of belief.

Formally, having degrees of belief as probabilities can be expressed as follows. Let *bel* indicate a function which goes from a set which contains the objects of our degree of belief (this can variously be defined as sets, propositions, events, which for de Finetti are mathematical objects which can take only the value 0 or 1) to the real numbers. Then: (1) if we are certain that an event E_1 will occur, our degree

[1] De Finetti proposes a representation theorem, or axioms for qualitative probability which result in quantitative probability when jointly applied (de Finetti (1937/1964)), a Dutch Book argument (de Finetti (1974/1990)) and an accuracy-based argument (de Finetti (1974/1990)); he seems to have stuck only to the latter of these in later writing, and abandoned the betting definition of degrees of belief. For more representation theorems, see Ramsey (1926/1990), Savage (1950/1972), Maher (1993), and for more accuracy based arguments see Joyce (1998), Leitgeb & Pettigrew (2010a) and Leitgeb & Pettigrew (2010b), Pettigrew (2016). For argument by derivation from qualitative axioms see Cox (1961), Paris (1994). There are many more sources for each of these types of argument.

of belief in this occurrence must be $bel(E_1) = 1$; (2) for all events E, it must be $bel(E) \in [0, 1] \subset \mathbf{R}$; (3) if A and B are mutually exclusive, it must be that the degree of belief in A or B occurring, $bel(A \cup B) = bel(A) + bel(B)$.

Some of de Finetti's writings—his emphasis on measurement and checkability, the suggestion that observing behaviour (betting or other) is enough to gain an insight into an agent's mental states—have led to the idea that he was a *strict operationalist*, and that his position inherits the problems of behaviourism. In a recent paper, Eriksson & Hájek (2007) look at the available analyses of the concept of *degree of belief* in the existing literature. *What are degrees of belief?*, they ask, portraying de Finetti's answer to that question as a strictly, or 'actual' in their terminology, operationalist one: degrees of belief are defined by the operations by which we measure them with. They find this problematic: once we deem a procedure's output an acceptable measurement of a given concept, we must, they say, accept any result of such procedure as an exact definition of the concept at hand. The concept just *is* the result of the measurement, and so no ill-calibration of the measuring device is possible ((Eriksson & Hájek, 2007, p. 187)).

Determining whether this is a fair criticism of operationalism is beyond the scope of this work. This characterisation of de Finetti, however, whatever its merits, is quite common (Joyce (1998, pp. 583-584) adopts it too, and see Berkovitz (2018) for further references). The possibility of a leading philosophical interpretation of probability being based, in the eyes of many, on a shaky and outdated (Eriksson & Hájek, 2007, p. 187) principle should be worrying for contemporary supporters of the view. My aim here, however, is to show that this reading is incorrect. The right way to understand de Finetti's operationalism is, I will argue, through his pragmatist philosophy. I approach this by looking at his philosophical inspiration and contrasting his position with that of other authors, some in agreement and others opposed to what they take to be a de Finettian position. What emerges is a clearer picture of de Finetti's distinctive brand of pragmatism, and hence a proper understanding of his operationalism.

The chapter is structured as follows: I start in Section 2.2 by discussing Eriksson and Hájek's characterisation of de Finetti; in Section

2.3 I sketch the verification theory of meaning, as expressed by the pragmatist authors closest to de Finetti, and in Section 2.4 I explore what the consequences of this principle would have been for his theory. In Section 2.5 I trace the differences between Peirce's pragmatism, which indirectly inspired de Finetti, and his own version of the position, and in Section 2.6 I finally return to the position of Eriksson and Hájek to compare it with de Finetti's as constructed in the previous sections. My conclusions are in Section 2.7.

2.2 De Finetti's operationalist puzzle

Eriksson and Hájek, while portraying de Finetti as an actual operationalist, perceive a tension in his writing, in passages of his such as the following:

> In order to give an effective meaning to a notion—and not merely an appearance of such in a metaphysical-verbalistic sense—an operational definition is required. By this we mean a definition based on a criterion which allows us to measure it [. . .]. The criterion, the operative part of the definition which enables us to measure it, consists in this case of testing, through the *decisions* of an individual (which are observable), his *opinions* (previsions, probabilities), which are not directly observable. (de Finetti, 1974/1990, p. 76)

In this passage, a footnote refers the reader to to Bridgman's book (1927/1960), whose message can be conveyed in the following 'slogan-like' quote: "In general, we mean by any concept nothing more than a set of operations [by which it is measured]; *the concept is synonymous with the corresponding set of operations*" (Bridgman, 1927/1960, p. 5). Like Bridgman, de Finetti attributes his operationalism to the shock of Einstein's dismissal of the concepts of absolute time and space. Absolute time, de Finetti (1937/1964, p. 168) writes, used to be seen as an a priori concept but Einstein has taught us that a notion is "only a word without meaning" if we don't know "how to verify practically any statement at all where this notion comes up".

This is a clear statement of intent; but Eriksson & Hájek (2007, p. 190), while depicting de Finetti as an operationalist, doubt whether de Finetti is a true, *strict* operationalist: the fact that he worries about *good* operational definitions means that, for him, probability must exist independently of its measurement. The perceived tension is this: either one believes that there is *nothing more* to a concept than the set of operations by which we measure it; or one believes that a concept exists in its own right, and we can approach its true value with good measurement. De Finetti, Eriksson and Hájek suggest, seems to want both these things at the same time. My argument will be the following: de Finetti, while believing that probability existed in its own right, thought that we needed operations to measure it in order to speak meaningfully about it.

In this, I agree with Galavotti, when she writes that, for de Finetti, "while betting quotients are apt devices for measuring and defining probability in an operational fashion, they are by no means an essential component of the notion of probability, which is in itself a primitive notion, expressing 'an individual's psychological perception'" (Galavotti, 2005, p. 211). I will now flesh out de Finetti's position by studying his pragmatist influences. Galavotti has written much on this, pointing out many crucial aspects, but I think an important element should be added to her picture: the faithful and consistent adoption by de Finetti of the pragmatist verification theory of meaning, which can be seen to motivate much of of his philosophical world-view.

Galavotti, in her earlier work (1989, pp. 241-242), seems to put more emphasis on 'pragmatic' as meaning 'practical', or 'useful'. She argues that, according to de Finetti, probability should be understood not as an abstract concept, but as an "indispensable instrument for reasoning and behaving under uncertainty" (Galavotti, 1989, p. 241). Other aspects of 'pragmatic' in de Finetti's work, according to Galavotti, are the continuity between inductive reasoning and inductive behaviour, and between decisions in everyday life and in science (Galavotti, 1989, p. 240-242). Recently, Galavotti (2011) has examined the broad philosophical perspective that de Finetti borrows from philosophical pragmatism, but the verification theory of meaning is still missing from her list of pragmatist influences in de Finetti's work (Galavotti, 2011, p. 508)

De Finetti writes:

> I had, by and large, adopted the mode of thinking advocated by authors such as Vailati and Calderoni (or perhaps it would be more accurate to say that I found their approach to be close to my own). Papini used to say of Calderoni that 'what he wanted to do was to show what precautions one ought to take, and what procedures one ought to use, in order to arrive at statements which make sense' (de Finetti, 1974/1990, p. 41).

The influence of the Italian pragmatists Vailati and Calderoni on de Finetti, or his agreement with them, has been noted elsewhere (Jeffrey (1989), Parrini (2004), Suppes (2009)), but my aim in this chapter is to improve on this understanding and, in so doing, cast light on the proper role of operationalism. De Finetti's verificationism does not seem to be inspired by Ayers, Wittgenstein, Schlick, Carnap or any of the Vienna Circle authors now mainly associated with the position.[2] Rather, it is Vailati and Calderoni who are mentioned time and time again. Their work on pragmatism, published in 1909, is a critical commentary on Peirce's pragmatism and its misinterpretations by other authors. I look to this next.

2.3 Pragmatist meaning

Vailati and Calderoni, according to Parrini (2004, pp. 35-36), adopt a view very close to that of Peirce. Here I will focus mainly on their approval and adaptation of the Pragmatist conception of meaning, meaning, in which they point out that Peirce endorsed the following methodological rule, attributing it to George Berkeley:

> *The only means to determine and clarify the meaning of an assertion is to indicate which particular experiences,*

[2] There was some correspondence between Carnap and de Finetti, as Parrini (2004, pp. 51-53) points out; but this was on de Finetti's philosophical interpretation of probability, aspects of which puzzled Carnap.

> *according to such an assertion, are going to take place, or would take place under specific given circumstances.* (Vailati & Calderoni, 1909/2010, p. 234)

Further on, they continue as follows:

> Such methodological rule is nothing more than an invitation to translate our assertions into a form that makes it possible to apply [...] those very criteria of true and false which are more "objective", less dependent on individual impressions and preferences. This form would be able to indicate more clearly what kind of experiments or observations can and need to be performed, by us or others, to decide whether, and to what extent, our assertions are true (Vailati & Calderoni, 1909/2010, p. 234).

This is inspired, Parrini points out, by Peirce's classic 1878 paper *How to make our ideas clear*. Parrini notes that neither Vailati nor Calderoni accepted this verificationism uncritically. It is safe to say, however, that they took the principle seriously. seriously. And so did de Finetti, as borne out by his own words: "statements have *objective* meaning if one can say, on the basis of a well-determined observation (which is at least conceptually possible), whether they are either TRUE or FALSE" (de Finetti, 1974/1990, p. 6).

There are several other striking and relevant points of agreement: for de Finetti *opinions* are not subject to criticism or to the criterion for meaning: "it is meaningless to think that my [probabilistic] evaluation is wrong, because it is meaningless apart from me, it has no other function than to express my state of mind" (de Finetti, 1931/1989, p. 193). This idea can be found in Vailati and Calderoni too (Vailati & Calderoni, 1909/2010, p. 236). Vailati and Calderoni saw pragmatism as a logical analysis of our "assertions and beliefs", which proceeds by extracting possible predictions implied in them. Criticising this because it is "bad psychology" has the same worth (i.e. none) as criticising "syllogistic logic, based on the argument that syllogism is not an exact description of our actual ways of reasoning" (Vailati & Calderoni, 1909/2010, pp. 246-247). This too, as I will argue especially in Chapter 3, is in line with with de Finetti's thought. For the purposes of this chapter, however, the most important point of agreement between de Finetti and Vailati and Calderoni is on the concept of *meaning*: I shall now discuss this.

2.4 Meaning and probability

In the passages above by Vailati and Calderoni two things are established: a definition of meaning and a meaningfulness criterion for propositions. The meaning of a sentence is the set of *predicted experiences* that it entails; and the criterion is simply this: does the proposition entail any prediction, checkable by "experiment or observation"? If it does, the proposition has meaning; if it does not, it is meaningless. There is a clear link between this criterion and de Finetti's subjectivism, a link which is perhaps not emphasised enough in the current literature. For de Finetti, the subjectivist account must have been the one that made probabilistic sentences meaningful in a satisfactory way.

Let us take a basic probabilistic sentence, such as this: $S =$ "the probability of event E is a". We want to determine whether this proposition has meaning. In the subjectivist interpretations the probabilistic sentence S is meaningful because, *in principle*, we can verify whether *a human agent* holds probability a as their degree of belief over the occurrence of E, and will act in such a way as to display precisely such belief. Such action could be, for example, accepting a relevant bet, or choosing the number a in a proper scoring rule scenario.[3] Here *in principle* means that, while we realise that there will be practical issues with the measuring process, if we are allowed to make certain simplifying assumptions (de Finetti, 1974/1990, pp. 77-80, 82), we can imagine this definition to be conceivably practical. In the betting definition, the idea is that if we suppose an agent to have utility linear in money, the event to be independent of the betting decisions, and more (much more, perhaps—I deal with this in Chapter 4),

[3]This puts further constraints on the content of the sentence: the event E must be a well-defined, objectively verifiable event—or else the sentence slips away again into meaninglessness. See also Sections 5 and 6 in the Appendix to de Finetti (1974/1990), in which the author takes the possibility of betting over it as a rule of thumb for whether something is a well-defined *event*.

then the agent should declare her sincere degree of belief as her chosen betting price. Note that (it is supposed that) there are no logical or theoretical obstacles for the definition to work in practice, but the practical obstacles are also taken to be such that it is conceivable that they can be overcome, so that this definition could actually work in practice. It seems plausible that in many situations, with small sums of money, a person's utility will be roughly in line with money and the betting decision independent of the state of the world (with regards to the event in question). I return to this below in Section 2.5.

An objection to this readily comes to mind: surely not *all* probabilities are degrees of belief held by someone, or even potentially held by someone. In particular, when we use the mathematical theory of probability, we might have probabilities that are, say, results of deep theorems or of complex calculation, or simply intermediate results of such calculations. It seems unnecessarily cumbersome to re-interpret each and every mathematical probability as someone's degree of belief. This is how de Finetti (1974/1990) saw it. There are two, connected, ways of studying a phenomenon such as probability (here, as elsewhere, the parallel with physical theories is important). There is an "axiomatic approach to the *theory of probability*" and an "axiomatic approach to the *calculus of probability*"; the first emphasizes the "essential meaning", the second "the formal aspect"; this is similar to the division of labour between physics and mathematics: the first works on "the passage from the 'facts' to their mathematical translation", while the mathematician works by building on the latter, putting aside questions of meaning and adherence to the 'facts' (de Finetti, 1974/1990, p. 256).

De Finetti places his work on probability firmly in the first camp, and thinks that many other approaches make the mistake of not applying this strategy strictly enough—even if the meaning they give to probability might be different to his. These other approaches, for example, add rules such as countable additivity, which is not an essential aspect of probability, merely for technical convenience (I discuss countable additivity in Chapter 5). What, then, are these 'facts' and what is the 'meaning' that de Finetti's axioms translate into mathematics? For de Finetti the latter is "the analysis of the condition for coherence for bets (or something similar) on things we called 'events'" (de Finetti, 1974/1990, p. 257). He vows to adopt "nothing more, and nothing less" than what this analysis demands. But remember: de Finetti also writes (as quoted above, (de Finetti, 1974/1990, p. 76)) that the operational definition allows us to measure the unobservable opinions of an individual. So the 'facts' that the axioms translate into mathematics, via the operational procedures, are actually these opinions of people, and this is the primitive concept of probability that de Finetti, like a physicist modelling a natural phenomenon, is seeking to model. This is not put explicitly, but the two passages from de Finetti (1974/1990) I discuss in this section make it clear that the conceptual structure underlying de Finetti's thought goes something like this: the starting point is the opinions of people in situations of uncertainty; this is the phenomenon at hand and the target of the model. de Finetti (1930) compares this to the concept of weight. The next phase is the operational definitions, which quantify this concept, and which should be designed in such a way as to reflect and measure accurately its important features. And finally there are the rules governing the quantified entities that result from the operational definitions; these rules are the axioms of probability. These should be exactly what is required by the analysis of the operational procedure. Once we have the axioms, we can operate mathematically without worrying, in the mathematical practice itself, whether each and every mathematical probability is an actual or potential degree of belief.

An important aspect of the framework I have just described is the acknowledgement that the *fundamental concept* of probability and its *abstract model*, mathematical-probability, are conceptually different. I return to this below, when I contrast de Finetti and Peirce, and this is the idea that underlies the discussion in part 2.6 which follows. So far, I have argued that subjective probability can be put forward as an excellent candidate in the context of a de Finetti-style criterion of meaning.[4] This is especially interesting because the criterion's originator, Peirce, began from this same idea and ended up with a completely different conception of probability. The fine points

[4]It is not the only one: finite frequentism, and perhaps some versions of Lewis's Best System analysis of chance might also be operationally accessible; I leave this discussion for future work.

of this different adoption of the criterion have not, to my knowledge, been discussed in the existing literature, and by contrasting de Finetti with Peirce, the former's approach emerges in sharp relief. I shall deal with this in the next section.

2.5 From Peirce to de Finetti: conceivability and verifiability

Peirce inspired Vailati and Calderoni, who in turn inspired de Finetti. What is striking, then, is that the latter is one of the founders of subjective Bayesianism, while Peirce is considered a precursor of the propensity-interpretation of probability, as it was named by Popper (1957) in a seminal article. For Peirce, probability is the "would-be" of an object, "a property, quite analogous to any *habit* that a man might have" (Peirce, 1910/1978, p. 241). This could be, for example, the 'would-be' of a coin to come up heads when flipped, or the 'would-be' of a tennis player to win her next match, or of a die turning up 6. Note, however, that "in order that the full effect of the die's 'would-be' may find expression, it is necessary that the die should undergo an endless series of throws, [. . .] the throws [being] independent each of every other (Peirce, 1910/1978, p. 242).

How to square this concept with the sort of verificationism which, originating from Peirce, inspired Vailati and Calderoni and, in turn, de Finetti is not clear. We certainly cannot check an infinite sequence of events, not even 'in principle'. Note that Vailati and Calderoni are not interpreting Peirce's as a strict verificationism, nor are they endorsing such a view. They write that we can meaningfully speak of hypotheses which are difficult or even impossible to check directly (Vailati & Calderoni, 1909/2010, pp. 242-243). For such hypotheses, however, "'indirect' verification i.e. that which consists in the verification of *other* affirmations that we can deduce from them" (Vailati & Calderoni, 1909/2010, p. 243) must still be possible. This weak version of verificationism was taken on board by de Finetti[5]: he is happy to measure probability, i.e. the unobservable degrees of belief

[5] Berkovitz (2012), reading de Finetti, reaches a similar conclusion.

of individuals, exclusively by the predicted actions we can deduce from them.

This form of verificationism is permissive, but it still excludes some propositions. If two or more incompatible propositions are unverifiable directly, and the only way of verifying them is through a set of deducible consequences which is identical for all propositions, then any discussion of which proposition is true will be useless (Vailati & Calderoni, 1909/2010, pp. 244-245). But this is exactly what appears to go on in Peirce's definition of probability: regardless of whether the true 'would-be' of flipping heads for a coin is $\frac{1}{10}$ or $\frac{9}{10}$, the deducible consequences are the same. That is, any finite sequence of flips is possible regardless of whether the first or the second 'would-be' is true. This is all the more puzzling, because close reading of Vailati and Calderoni suggests that they have not strayed too far from Peirce on meaning.

The key seems to be in how they interpret something to be 'in principle' or 'conceivably' checkable. Suárez has recently interpreted 'conceivability' in Peirce thus:

> The concept of the object is only exhausted by its full set of conceivable effects. In other words, the pragmatist maxim applies to all objects, whether actual, possible, or merely imaginary. And it defines any such object in terms of all its effects, whether actual, possible or merely imaginary (Suárez, 2013, pp. 13-14).

Suárez, like Vailati and Calderoni, quotes Peirce's *How to make our ideas clear*. But Vailati and Calderoni, I would argue, have a better handle on it: contra Suárez, I think the emphasis is on conceivably *practical*, and not on *conceivable* in itself, understood as something like "anything we can conceive of". To support this, I will now quote some of the relevant passages by Peirce at some length:

> [W]e come down to what is tangible and conceivably practical, as the root of every real distinction of thought, no matter how subtile it may be; and there is no distinction of meaning so fine as to consist in anything but a possible difference of practice. (Peirce, 1878, pp. 293) I only desire

> to point out how impossible it is that we should have an idea in our minds which relates to anything but conceived sensible effects of things. Our idea of anything is our idea of its sensible effects; and if we fancy that we have any other we deceive ourselves, and mistake a mere sensation accompanying the thought for a part of the thought itself. [. . .] Consider what effect, which might conceivably have practical bearings, we conceive the object of our conception to have. Then our conception of these effects is the whole of our conception of the object [. . .] (Peirce, 1878, pp. 294,)

For example, when Peirce says that it is impossible to "have an idea [. . .] which relates to anything but conceived sensible effects of things", I take him to mean, given the context of the paper, that there must be some sensible effects that we imagine, or picture, or predict (or, indeed, conceive) when we entertain an idea. I think it is better, then, to interpret the 'conceivably' in Peirce as doing the same job as the expression 'conceptually possible' adopted by de Finetti. I think it is justified to take the two authors as meaning roughly the same thing when they talk about experiments that are at least conceptually possible, and consequences that are conceivably practical.

If I am right, then, Peirce, Vailati and Calderoni and de Finetti were roughly in agreement on what is conceivably checkable. So where do they diverge? Peirce (1910/1978, p. 242) makes it clear that a definition of probability based on an infinite run of trials is conceivably practical, because its becoming actualised is a logical possibility.[6] At the same time, Peirce, in this paper, shows no sign of having given up on the theory of meaning which inspired the Italian pragmatists: he goes on to say, "I really know no other way of defining a habit other than by describing the kind of behaviour in which the habit becomes actualised" (Peirce, 1910/1978, p. 243). It just so happens that the 'would-be', or 'habit' of a coin is actualised in an infinite sequence of events, which can nonetheless happen in a finite time.

[6] For example, imagine a sequence of coin tosses where the first coin toss occurs at time 0, the second one 30 seconds later, the third 15 seconds after the second, and so on, each interval lasting half of the one preceding it. Then the infinite number of coin tosses would take 1 minute to be performed (see (Peirce, 1910/1978, p. 242)).

This idea of conceivability as logical possibility is not operational. We cannot measure probability with a procedure requiring an infinite number of steps. The emphasis on measurement is a novel aspect here, attributable to de Finetti, and it seems to originate from, or be in agreement with, Vailati and Calderoni, Einstein and Bridgman. The break between Peirce and de Finetti, it appears, might be precisely at this point. A possible problem with this reading, however, is that de Finetti's mathematical theory of probability makes use of real numbers. If a degree of belief is represented by an irrational number, it might take an infinite number of steps to measure it. But the answer to this shows the crucial difference between de Finetti and Peirce, and supports the points made above in part 2.4. Peirce tries to make mathematical probability a conceivably checkable concept, but in order to do so he has to weaken conceivability to mean 'logical possibility'. De Finetti does not attempt this. For him, the mathematical theory is distinct from, and an abstraction of, the practical, pre-theoretical, idea of probability, or rather, the uncertainty held by individuals. It is this latter concept that must be conceivably checkable, and thus meaningful, and we then model it in mathematics.

I think the key to understanding de Finetti's weak version of operationalism lies in a proper exploration of his pragmatism. I now return to the position defended by Eriksson & Hájek (2007), whose characterisation of de Finetti's operationalism inspired this discussion. My conclusions will follow.

2.6 Probability as a primitive concept

Eriksson & Hájek (2007) study the available analyses of the concept 'degree of belief' and find them all lacking; they decide that 'degree of belief' is a *conceptual primitive*, one of the unanalysed "basic building blocks in our thinking" (Eriksson & Hájek, 2007, p. 205). They call this position *primitivism* about degrees of belief. They offer some positive reasons for 'degree of belief' being primitive: it is "well understood [,. . .] natural [. . .]", and, given its role in Bayesian epistemology, forms the basis of a successful and progressive research programme. In particular, bets may sometimes give an indication of underlying degrees of belief, but not always (Eriksson & Hájek, 2007, p. 205-211). Parts of this sound so close to de Finetti's position, as outlined in my discussion above, that it is tempting to read it as a contemporary version of the same idea—a de Finettian position based on readings more palatable to the 21st-century philosopher. But I think de Finetti's project is different. Eriksson & Hájek (2007, p. 205) write that there are *conceptual primitives*, studied by philosophy, and there are *ontological primitives*, studied by science. To argue their point, they use a running parallel with the concept of charge: charge is an unanalysed primitive concept in physics, but the science works perfectly well regardless; surely, they argue, we can do the same with conceptual primitives in philosophy. De Finetti, as is clear from his references to Einstein, also looked to the successful sciences for inspiration on how to treat concepts and meaning. But the sciences were more than just the source of a useful, independent example to be used for proving a point. On this, he writes:

> All concepts, mathematical ones included, are more or less directly and clearly suggested by intuition: however, their definition is totally arbitrary, as long as the consequences that we wish to draw from them are purely formal: as long as, that is, they are propositions in which a concept acts in the way implied by its definition. This is the case for [mathematical] measure; we would have a different case, on the other hand, for weight, because we cannot impose to the scale to work according to our definition; in the same way, it seems to me, probability too is a different case. (de Finetti, 1930, pp. 4-5, my translation)

De Finetti here is arguing against the study of probability as part of the branch of mathematics called *measure theory*.[7] Just like weight, he writes, probability has real-world (if not mind-independent) features, which its formal definition must respect. De Finetti would most likely have agreed with Eriksson and Hájek's view (2007, p. 210) that we have antecedent knowledge of the concept 'degree of belief', independently

[7]This turned out to be a losing battle: since the formalisation of probability theory in Kolmogorov (1933/1956), mathematical probability has largely been studied as a measure.

of its possible measurements. But just as the *ontological primitive* 'charge' must be characterised in such a way that it makes sense of, and creates a coherent framework in which to understand, real-world experiments, so must the concept 'probability' (which, remember, for de Finetti is just 'degree of belief') reflect the features of the pre-existing, intuitive notion of probability. Eriksson and Hájek are happy to use 'degree of belief' as a philosophical instrument to organise our thinking; de Finetti worries that this would be arbitrary, a definition of something, but not of probability as we already know it: he wants to capture 'the real thing'. De Finetti, of course, did not think in the terms and within the conceptual framework adopted by Eriksson and Hájek; he thought in terms of *conventional definitions* and *effective definitions*. Formal, mathematical concepts can be defined in the first way, but for a concept to be meaningful it needs to have conceivably practical consequences: it needs an effective, or operational, definition. But if we wanted to slot de Finetti's probability into Eriksson and Hájek schema, for an idea of where his thinking might be placed today, I think we would say that he took 'degree of belief' as an *ontological primitive*. This is also suggested by the passages I discussed in Section 2.4, where he finds that the method of the physicist of going from facts and meaning to a translation into mathematics is the one he favours in probability too.

While Eriksson & Hájek (2007, p. 207) were not aiming to give a historically accurate depiction of de Finetti, a rebuttal of their portrayal is useful because it allows me to complete the picture of de Finetti's operationalism and pragmatism I have been building. Not only is he not an 'actual operationalist', as they claim, but it is also quite helpful to think of how de Finetti fits into their framework: he could be said to be a *primitivist* about degrees of belief as well, only he thinks they are not a mere convention, or *conceptual primitive*. Rather, they already have a psychological reality, and we should model them in a way which reflects this, via suitable operational definitions.

Before summing up my arguments, a short remark[8] on the sort of *primitivism* about degrees of belief I have in mind is appropriate, to avoid potential confusion with a position of Jeffrey (1984, 1992 and

[8] This was suggested to me by Alberto Mura.

elsewhere) which he calls *radical probabilism.* Jeffrey (1992, p. 203) thinks that "probabilities needn't be based on certainties (e.g. via conditioning): it can be probabilities all the way down, to the roots". Probability is primitive here in the sense that it is not a prevision of an all-or-nothing truth value and so does not depend on another concept (truth) to gain validity. De Finetti may or may not have been a radical probabilist in this sense. His earlier *Probabilism* (1931/1989) seems closer to this position, while by the time he was writing de Finetti (1974/1990) he seemed to have been more distant from it. In the passages above, primitivism was used to indicate that degrees of belief are a basic concept, existing, in the shape of the opinions of people, independently of the context of their measurement. It seems to me that this can be kept distinct from the sort of "primitivism" of Jeffrey just sketched. That is, degrees of belief can be understood as a basic psychological reality whether they are previsions of truth values or whether it is, in fact, 'probabilities all the way down'. Therefore, the above is independent of the question, which I do not address here, of whether de Finetti should be characterised, for a part or the entirety of his career, as a radical probabilist.

2.7 Summary and conclusion

In this chapter I have argued that characterising de Finetti as a strict operationalist, as is often done, is not correct. Even though he seems, at times, to claim this label for himself, in the rest of his writing he emerges at best as a weak operationalist, or or rather as someone who takes the pragmatist verification theory of meaning seriously. Nonetheless, his brand of pragmatism is quite idiosyncratic. I have argued that some of his main ideas, while inspired by Peirce, diverge in subtle but important ways from the American pragmatist. This might be in part thanks to the version of pragmatist philosophy by Vailati and Calderoni. They, together with Bridgman and Einstein, were some of the main influences on de Finetti's own version of pragmatism. By contrasting this to other positions, either similar (Peirce) or critical (Eriksson and Hájek), a good picture emerges. De Finetti considers probability a primitive concept, which has real-world, if not mind-independent, existence. This concept must be meaningful, that is, it must have conceivably practical consequences (when employed, say, in simple, well-formed sentences). Speaking about it in terms of operational definitions ensures this to be the case. Degrees of belief, potentially held by human agents, emerge for de Finetti as the only meaningful interpretation of probability. *This* concept is meaningful; mathematical probability is distinct and must be a model of this, so, differently to what is found in Peirce, mathematical probability is not subject to verifiability.

Chapter 3

Objectivity

3.1 Introduction

In the previous chapter, my approach to de Finetti's pragmatism is based on the feeling that his work, and his operationalism in particular, has been misrepresented in contemporary debates, and that a proper analysis of these aspects could hopefully enhance and render more productive the discussion of his theory. In this chapter, my focus will be on another controversial aspect of de Finetti's theory: its objectivity, or the perceived lack of it. De Finetti says that degrees of belief are subjective, that they should be coherent (i.e. respect the rules of probability)—and that's it. This matter has been much debated and is the main motivation for the current chapter. The problem can be summarised thus: why should coherence be the only requirement for degrees of belief? Many prominent authors believe that if subjective Bayesianism is a theory of rationality—and it does seem to be motivated by a rationality norm—there is something lacking in it. Jon Williamson is one of the authors who shares this widespread, perhaps dominant view in the current literature. To build his theory of *objective Bayesianism*, Williamson starts from de Finetti's subjectivism, finds it lacking, and goes on to add further constraints for rational degrees of belief (see for example (Williamson, 2007, pp.2-5)). Here is Williamson pointing out a crucial difference between different currents of Bayesianism:

> the various Bayesian interpretations chiefly disagree with respect to Objectivity. According to the strictly subjective interpretation [which is de Finetti's, my note], probability is largely a matter of personal choice [...]. Thus, you are perfectly rational if you strongly believe that the moon is made of blue cheese, provided you strongly *disbelieve* that it is not the case that the moon is made of blue cheese. This laxity is often considered to be a stumbling point for the strictly subjective interpretation (Williamson, 2010a, p. 16).

In this chapter I flesh out de Finetti's position and defend it against readings such as the above. I argue that a number of influential and widespread critical readings of de Finetti's subjective Bayesianism are mistaken. What this points to, then, is a new defence of some aspects of subjective Bayesianism. My main aim is to construct a better picture of where a de Finetti-style subjective Bayesianism should be located in contemporary debates, and my strategy, as in Chapter 2, will partly consist in backing up this picture with historically informed arguments. As in the previous chapter, the text by Vailati & Calderoni (1909/2010) will be helpful in constructing or completing a background philosophical picture for my interpretation of de Finetti.

De Finetti's position is often considered the most radical in a spectrum of Bayesian positions that go from 'radically subjective' or 'orthodox' Bayesianism to objective Bayesianism. I elaborate on this below in Section 3.2, and I answer some points of criticism from the objective Bayesian camp. Clearly, intermediate positions do exist, and it might seem disingenuous to pit the two positions at the opposite ends of the spectrum against one another whilst ignoring the rest: after all, a good solution to some of the problems raised in this discussion could of course be to adopt a position somewhere in the middle of the subjective-objective spectrum. However, the reason for embarking on a defence of this sort is two-fold: firstly, I want to show that de Finetti's position does not really belong on the subjective-objective spectrum at all, since his is not a project in formal epistemology like the others it is often compared to. Secondly, by showing that a major class of criticism of de Finetti's position is misplaced, the argument here goes some way towards showing that a de Finetti-style subjective Bayesianism is a tenable and reasonable position. This is independent of other variants of subjective Bayesianism existing in the formal epistemology literature. Readers sceptical of de Finetti's position in general might still not be convinced, but I hope they too might appreciate a discussion in which all positions are properly motivated. Indeed, their criticisms of the position may cut deeper when aimed at a better-defined target.

The rest of the chapter is structured as follows: in Section 3.2 I sketch, and suggest an answer to, a major line of criticism that has been levelled at de Finetti. I give some support for my answer in Section 3.3, where I give an outline of de Finetti's subjective Bayesianism, as I think it should be understood. I back up this position in Sections 3.4, 3.5, 3.6. In Section 3.7 I try to give a proper picture of how objectivity is used in subjective versus objective Bayesianism, not so much to make the disagreement go away, but rather to try and draw a clear conceptual map of the positions. My conclusions are in Section 3.8.

3.2 Degrees of belief and subjectivism

Weisberg, in a review article on the *Varieties of Bayesianism* writes (2011, p. 3) that "a Bayesian theory is any theory of non-deductive reasoning that uses the mathematical theory of probability to formulate its rules." Weisberg is careful to distinguish what he calls the 'degree of belief interpretation', which states only that probabilities are degrees of belief, from the 'subjectivist' interpretation, which says that only the basic three probability axioms should appear in our Bayesian theory. Remember that the basic axioms regulating a probability function P for events A, B, E are the following: (1) $P(A) \in [0,1] \subset \mathbf{R}$, (2) $P(E) = 1$ if E is the certain event, (3) $P(A \cup B) = P(A) + P(B)$ if A, B are mutually exclusive. These three rules alone allow for a wide range of degrees of belief as admissible in a given situation: hence if we accept no further rules we are 'subjectivists', and hence the accusation of advocating an 'anything goes' epistemology. Williamson's reading, quoted above, is far from being idiosyncratic: it is in fact a very common one, perhaps the one which dominates current debates. Hájek (2012) thinks that de Finetti's theory allows for "crazy" belief states, as it only accepts the basic axioms of probability as formal rules. On the subjectivist position, Weisberg (2008, 22) writes the following: suppose an agent has a high degree of belief in a coin landing heads, and the only reason he can give for this is that he has a low degree of belief in it landing tails. Even if the agent's doxastic state were coherent (and thus would pass the subjectivist's requirements) this kind of motivation would not "raise our estimation of his rationality" (2008, 22). This, he finds, makes subjective Bayesianism problematic. He thus suggests we 'fill out' our Bayesian theory with more formal rules.

Although Weisberg does not mention him in his section on subjectivism, de Finetti fits his definition and, as Weisberg's critique is the most detailed one, I shall address myself to that. Firstly, it seems clear to me that the following issues can be kept separate: on the one hand, the issue of which rules are essential to a "theory of non-deductive reasoning that uses the mathematical theory of probability" and whether a belief state is consistent with these rules; on the other, the issue of which degrees of belief an agent *should* have, and whether an agent can give good reasons for assigning the degrees of belief that she does. The first set of questions are fundamental for any project in non-deductive reasoning, while the second do not seem to be: they delve into the content of an agent's credences and their eventual justification. These kinds of requirements suggest that what Weisberg has in mind is a conception of Bayesian theory that goes well beyond merely a class of formal reasoning theories which use probability as its basis, morphing into something like a broad theory of rationality. While de Finetti's theory falls under the first label (a formal theory of reasoning), it definitely does not fall under the second.

Secondly, subscribing to a de Finetti-style subjective Bayesianism does not necessarily mean thinking that coherence is sufficient for rationality, but only that it is a necessary component. Therefore, while it is true that we would doubt the rationality of someone who can give no good reason for their believing something, to use this as a criticism of subjective Bayesianism is misplaced. Weisberg, moreover, writes that since a subjectivist is unable to give such good "epistemological reason for their conviction" (Weisberg 2008, 22), we should add formal rules to our theory. But this suggests that we either have a formal rule, or we have no epistemological reason to have a given degree of belief. This seems to me a false dichotomy. Not only might we have perfectly good knowledge which affects degrees of belief but is not in the form of a general formal rule; but it also, in fact, might be extremely difficult to translate into a formal rule all the information and knowledge that goes into forming a degree of belief. I return to this below in Section 3.6.

All told, I think de Finetti is not attempting a theory of rationality, does not say that a coherent doxastic state is automatically rational and would also reject the notion that the road to more rationality necessarily involves adding more rules. I defend this below in Section 3.3. What is de Finetti doing then? I think his overarching philosophical project is as I depict it in Chapter 2: an attempt to place the mathematical theory of probability on what he thought was sound philosophical ground: that is, a theory which revolves around a *meaningful* concept of probability. De Finetti wishes to study probability as a content-less (non-deductive) logic; considerations of what an agent *should* believe and why are simply outside his intended scope. I shall now discuss this.

3.3 De Finetti's subjective Bayesianism

I argue in Chapter 2 that de Finetti saw *mathematical probability* as a formal model of the pre-theoretical concept of *probability*. This fundamental distinction has a few important consequences. Mathematical probability itself is seen as a content-less logic, an analogue and an expansion of classical deductive logic (de Finetti, 1974/1990, p. 8). However, this is not an arbitrary formal language, but one that must respect, in its fundamental axioms, the features of the target it is modelling: the pre-theoretical idea of probability, as quantified by the operational definitions. Unlike the other authors he is often compared to today, he was not interested in constructing a theory of rationality. In his own words, as quoted by Galavotti in her introduction to de Finetti (2008, pp. xviii-xix): "The subjective theory [...] does not contend that the opinions about probability are uniquely determined and justifiable. Probability does not correspond to a self-proclaimed 'rational' belief but to the effective personal belief of anyone." The concept of probability must conceivably be the degree of belief an agent has in order to make sense, but that is as far as it goes. As mentioned above, de Finetti understands the probability calculus as an analogue of classical deductive logic: a deductive argument can be valid whilst having conclusions which clash with the actual state of the world (imagine a valid argument whose conclusion is that the moon is made of blue cheese), but this is no criticism of the endeavour of first-order logic. De Finetti wishes to avail himself of the same defence and, prima facie, it seems to withstand the criticism of Weisberg, Hájek and Williamson. A few interconnected factors, however, threaten this defence of de Finetti's. I think his theory can survive, although not entirely unscathed. The first issue, which I discuss here in two parts, is that de Finetti attempts to give probability a philosophically acceptable definition starting from a principle which looks a lot like a specific *rationality* norm. De Finetti's critics could well ask why this norm was chosen and why stop at this one alone, if it's rationality norms that we're dealing with. The second is the normative status of the theory. The third is the relativistic position that de Finetti has expressed in some of his writings, which naturally lend themselves to an 'anything goes' reading. I address these issues in this order.

3.4 Rational degrees of belief: why stop at coherence?

A version of the rationality norm that is apparently the driving force behind de Finetti's subjective Bayesianism is that we should not have credences that allow us to make a certain loss in a system of bets: this, remember, is the requirement of coherence. But why choose coherence in particular and base our whole theory around it? There are many other, stronger rules that can be added to reduce the number of belief states considered acceptable by the theory. Williamson adds *Calibration*, the principle that we should set our degree of belief according to relevant frequencies if known, and *Equivocation*, the principle that out of these calibrated degrees of belief we should pick the one having maximum Shannon entropy, here taken as a reverse measure of informativeness (so that the degree of belief that maximises entropy is the one that 'adds the least information' to what emerges from the available data). These, in cases with a finite event space, combine to ensure that there is only one distribution of degrees of belief that is acceptable. Another possible addition is *regularity*, the principle that, unless a proposition is a tautology or a contradiction, degrees of belief about it should always be in $(0, 1)$, and so not at the extremes.

De Finetti was opposed to rigid rules that are supposed to result in only one rational belief attitude. (His criticism was mostly directed at frequentist statistics, so not exactly the angle I discuss here.) For example, he writes that while in many cases it makes sense to adopt a uniform distribution over a set of incompatible and similar events, this should not be elevated to a rigid rule; the evaluation that a given case requires a uniform distribution is always a subjective one (de Finetti (1974/1990, p. 199); see also Berkovitz (2018) on this). But being opposed to something is not enough in itself; what underpins de Finetti's opposition is his assertion that he thought that no further restrictions were demanded by the Dutch Book theorem. He writes (de Finetti, 1974/1990, p. 258), as I quote above, that *the axioms of probability will be nothing more, and nothing less* than what is required from the analysis of the betting scenario. There is no Dutch Book for *Calibration*, *Equivocation* or regularity, and so they should not form part of the basic rules. There are two important challenges to this argument, which I address here.

Regularity and indirect bets

The first is made by Mura (1995), who finds that it is not entirely correct to assert that regularity is not demanded by the Dutch Book theorem. Suppose a bet[1] on event E has stake 1, and we accept to pay 1 to participate in this bet, reflecting our degree of belief 1 in the occurrence of E. Then, we can either lose 1 if E does not occur or gain 1 if it does occur—but this, considering what we spent to participate in the bet, is a gain of 0. Mura (1995, pp. 48-49) thinks that it is

[1] For a more exhaustive discussion of bets see Chapter 4.

consistent with de Finetti's thought to postulate that a belief state that brings us into a situation in which we can only lose or gain 0 is defective. He proposes to substitute the classical bets in de Finetti and Ramsey by what he calls *indirect bets*[2], thanks to which he is able to define a sense of credal inconsistency which works both for all-or-nothing beliefs and for degrees of belief; this, in turn, can give a sense in which probability theory is an extension of classical propositional logic, which, as I explain above, was a tenet of de Finetti's view. Given this framework, Mura (1995, pp.49-50) shows that no violation of rules additional to the basic axioms of probability can bring about credal inconsistency. This is an improved justification for stopping at the basic rules of probability and adding no further rules: your credences are inconsistent if you violate finite additivity, say, but not if you violate regularity or *any* other rule which is not deducible from the basic axioms.

Calibration, equivocation and accuracy

The second challenge appears in Williamson's (2010b) review of de Finetti's (2008). Note that Mura argues that de Finetti cannot claim that regularity is not demanded by Dutch Book arguments; Williamson's *calibration* and *equivocation*, however, are definitely not demanded by them. De Finetti's position is thus immune, it appears, to criticism from this angle. But Williamson (2010b) notes that de Finetti abandoned Dutch Book arguments for probabilism, in favour of accuracy-based ones. In doing this, according to Williamson (2010b, p. 132), de Finetti "shoots himself in the foot", because this sort of reasoning "leads more naturally to objective Bayesianism than to subjective Bayesianism". Bets will not bring about *calibration* and *equivocation*, the argument goes, but accuracy will. He bases his reasoning on the fact that since, in accuracy-based approaches we are "minimising expected loss with respect to a scoring rule [...] *minimising worst-case expected loss* is rather natural" (Williamson (2010b, pp. 132-133), my

[2] An indirect bet is a collection of bets on a composite event (say for example $A \cup B$), with the amount paid for the bets on the 'sub-events' (here A, B) adjusted in such a way as to result in a constant gain or loss if the composite event occurs or not (regardless of which specific combination of sub-events make this true, here A occurring or B occurring).

emphasis). To address this, we must make a brief detour into the kinds of arguments used in accuracy-based formal epistemology, citing a few examples.[3]

Let E be the event over which we want to elicit a degree of belief. Let $\mathbf{1}_E$ be the indicator function of E, such that $\mathbf{1}_E = 1$ if E happens, $\mathbf{1}_E = 0$ (or equivalently $\mathbf{1}_{\bar{E}} = 1$, where $\bar{E}$ is the event complementary to E) if E it does not happen. Although there are other options, in these approaches the Brier score is often used to penalise an agent according to how far from the truth her credences turn out to be. Here,

$$L = \frac{1}{2}((bel(E) - \mathbf{1}_E)^2 + (bel(\bar{E}) - \mathbf{1}_{\bar{E}})^2)$$

so that, for example, if E happens and it was $bel(E) = 1$, the Brier score will be its minimum, 0. If *bel* is incoherent, there exists a set of credences which perform better whatever happens, regardless of whether E or $\bar{E}$ turns out to be the case; they are closer to both points. We say that *bel* is strictly dominated. The Brier scoring rule is in a class of scoring rules called *proper* because their construction makes it advantageous for an agent to declare her sincere degrees of belief.

Now, as an example, let bel_0 be such that $bel_0(E) = 0.1$, $bel_0(\bar{E}) = 0.6$. This is incoherent, as $bel_0(E) + bel_0(\bar{E}) \neq 1$. If E occurs, bel_0 has the Brier penalty

$$L_E(bel_0) = 0.585,$$

while if $\bar{E}$ occurs, the Brier penalty for bel_0 is

$$L_{\bar{E}}(bel_0) = 0.085.$$

What the sources in footnote 3.4 show, is that (1) there exist a set of assignments of degrees of belief to E and $\bar{E}$ that have a lower penalty both if E occurs and if $\bar{E}$ occurs; they are said to strictly dominate bel_0; and (2) that if an assignment is coherent, there exists no other assignment (including other coherent assignments) that strictly dominates it.

[3]See, among others, de Finetti (1974/1990), Joyce (1998), Pettigrew (2016) for full treatments and proofs.

As an example, let bel_1 be such that $bel_1(E) = 0.25$ and $bel_1(\bar{E}) = 0.75$. Then, the Brier penalties will be

$$L_E(bel_1) = 0.5625,$$

if E occurs and

$$L_{\bar{E}}(bel_1) = 0.0625$$

if $\bar{E}$ occurs. This does better than bel_0 in both cases.

The above is an example of why the Brier score makes it convenient for an agent to declare coherent degrees of belief. This needed to be established before treating expected loss, otherwise calculating this latter quantity could involve multiplying Brier scores by numbers which are not probabilities. Attempting to minimise these 'incoherent expectations' would give, for example, the weird result that it is best to choose $bel_{weird}(E) = bel_{weird}(\bar{E}) = 0$, since this makes our 'expected loss' equal to 0.[4] Williamson writes about (1) minimisation of expected loss and (2) minimisation of worst case expected loss. The former idea, (1), comes into play when, having shown that accuracy forces us to declare probabilities, we want to check that it forces us to declare 'our' probabilities, and not just any number. (This is a big problem with betting definitions of degrees of belief, which I discuss next in Chapter 4.) This works as follows (I report the proof by de Finetti (2008)): suppose our sincere degree of belief is given by the coherent degree of belief function bel_s; but suppose we try and declare a different number, say $x \in [0, 1]$. Now, it is simple to see that the expected loss, if I declare bel_s, is

$$\mathbb{E}(L(bel_s)) = bel_s(E) - bel_s^2(E).$$

If, on the other hand, I insincerely quote x and $1 - x$ as my degrees belief in E and $\bar{E}$ respectively, the expected loss will be:

$$\mathbb{E}(L(x)) = x^2 bel_s(E) + bel_s(E) - 2x bel_s(E) + x^2 - x^2 bel_s(E).$$

Now observe that

$$\mathbb{E}(L(bel_s)) - \mathbb{E}(L(x)) = (x - bel_s(E))^2 \geq 0. \tag{3.1}$$

[4] I am grateful to Seamus Bradley for pointing this out.

Any difference between $bel_s(E)$ and x will only add to our expected loss. This is the sense in which we explore the minimisation of expected loss: when we are wondering whether to respond sincerely to the elicitation question or not.

What, then, is (2), the minimisation of worst-case expected loss that Williamson refers to? One notices immediately that the terminology is somewhat curious: if we calculate the expected loss, as above, we include both the 'best case' (in which the events turn out closer to our prevision) and the 'worst case' (the complementary event). But to pick out the minimum worst-case expected loss, we need to have two sets of probability functions to choose from, say M and N, and calculate the expected loss for a belief function in N with respect to a probability function in M. For example, let $ch_1, ch_2 \in M$ and $bel_3, bel_4 \in N$. The expected Brier loss of declaring bel_3, calculated according to ch_1, is:

$$\mathbb{E}(L(bel_3, ch_1)) = ch_1(E)(bel_3(E) - 1)^2 + ch_1(\bar{E})bel_3^2(E). \qquad (3.2)$$

Looking at $\mathbb{E}(L(bel_3, ch_1))$ and $\mathbb{E}(L(bel_3, ch_2))$ on the one hand, and $\mathbb{E}(L(bel_4, ch_1))$ and $\mathbb{E}(L(bel_4, ch_2))$ on the other, we are supposed to choose, between bel_3 and bel_4, the degree of belief assignment whose worse expected Brier score, with respect to ch_1 and ch_2, is lowest (remember that a lower score is better). This is minimisation of worst-case expected loss. A mathematical result (see Pettigrew (2016, Chapter 13 and Appendix IV)), then, shows that the degree of belief assignment, or credence function, that achieves this minimisation is the one, among the available ones in N, which is the most middling. That is, in our example, the one closest to bel_{eq}, where $bel_{eq}(E) = bel_{eq}\left(\bar{E}\right) = 1/2$. This would follow the recommendation of Williamson's principle of *equivocation*.

Furthermore, which credence function is closest will, in general, depend on what notion of distance we adopt; de Finetti adopts the Brier score, but there are many others. If we choose the *logarithmic scoring rule* L_{log} instead[5], we will have the following: the credence function in N which is closest, by the lights of L_{log} to the most middling function, is also the one with maximal Shannon entropy, among

[5]Defined, for credence function bel in case E occurs, as $L_{log}(bel, E) = -\ln(bel(E))$

credence functions in N (Pettigrew (2016, pp. 176-177), Williamson (2010b, pp. 133-134)).

Let us now go back to how this is supposed to be a problem for de Finetti. There are two steps. Firstly, Williamson writes that since de Finetti is minimising *expected loss* (1) with respect to the Brier scoring rule (as in Equation 3.1), it is 'natural' that he would minimise *worst-case expected loss* (2). Secondly, the logarithmic scoring rule is, according to Williamson (2010b, p. 133) better supported and "more typical as the default loss function"; the adoption of this, together with the minimisation of expected loss, leads to choosing the credence function which maximises Shannon entropy, which is the central tenet of Williamson's objective Bayesianism. Hence, de Finetti would inevitably be led into adopting objective Bayesianism because of his adoption of an accuracy-based argument for probabilism.

I think both these moves can be resisted. I treat loss minimisation first. Here we cannot rely on the simple distinction between rules whose violation brings about strict domination and rules that do not have this characteristic, because neither agent sincerity (which results from minimising expected loss (1)) nor equivocation (which results from minimising worst-case expected loss (2)) are required by coherence—but we would like to hold on to (1) but not be forced to accept (2). (Rule (1) is not required by coherence because an agent can be coherent and insincere: the insincere quotation of degrees of belief is only worse off in expectation; it is not strictly dominated.) Nonetheless, a distinction can still be made: (1) and (2) are qualitatively different and applied at different phases. (1) regards the elicitation process; the effort is to show that we should declare our sincere degree of belief, *whatever it is*, or else we add an unnecessary positive number to our expected penalty. (2) is a principle that is meant to help us choose which degree of belief to quote. It requires the introduction of a further class of probability functions.[6] In Williamson (2010b, p. 132), the set M (in my notation) contains "reasonable choice[s] of belief function for the agent". So we are supposed to choose a credence function which minimises worst-case expected loss with respect

[6]Pettigrew (2016, Chapter 13) calls these, which I called $ch_1, ch_2 \in M$, chance functions.

to this class of credence functions. The passage to accepting rule (2) can be resisted for two reasons: firstly, in de Finetti there is no talk of the existence of a separate class of credence functions used in this way; certainly neither its existence nor its use in this way are requirements of coherence. Secondly, de Finetti explicitly wants to avoid rules which interfere with the content of our coherent credence function: requiring sincerity in the elicitation process does not do this; requiring minimisation (2) does. So because adopting (2) requires an additional theoretical framework which is not required by coherence and it goes against the foundational ideas of his theory, de Finetti is not forced to adopt it. Note that de Finetti (1974/1990, p. 188, footnote) briefly considers minimisation of worst-case expected loss and dismisses it as absurd, as its application results in believing all events in a given algebra, say $E, \bar{E}, F, \bar{F}, G, \bar{G}, \ldots$, to degree $1/2$, which is globally incoherent. This is too quick—we should minimise worst-case expected loss while imposing that the overall distribution is coherent. But Williamson's argument is too quick as well. We are allowed to invoke (1) and not (2).

The second part of Williamson's criticism was that the logarithmic scoring rule is better supported than the Brier scoring rule. But while the former might be well supported, however, there are good arguments for the latter too. Williamson writes that the former is more typical, but Pettigrew (2016) and Joyce (1998) are influential proponents of accuracy-based probabilism and they offer extensive support for the latter. It is clear that de Finetti is not forced to pass to logarithmic scoring rules, and hence to objective Bayesianism. All in all, then, de Finetti is not forced to adopt *equivocation* by his adoption of the Brier scoring rule.

Note that Pettigrew (2016) has recently constructed an accuracy-based theory of formal epistemology which vindicates not only probabilism but also versions of *calibration* and *equivocation*. The first, however, requires the introduction of objective *chance functions* (Pettigrew, 2016, Chapters 8-10), which map each chancy event to its objective chance. A follower of de Finetti would simply deny the existence of such functions and of objective chance. If we want to prove that accuracy-based arguments for probabilism by necessity also justify *calibration*, we would first need to demonstrate that objective chances of this kind exist, and that is an open problem. Resolving this vast issue is far beyond the scope of this work; I shall limit myself to noting that it is an arguably tenable position that (we can operate as if) these objective chances do not exist, so a de Finettian position is not forced to accommodate them. The justification of *equivocation* in Pettigrew (2016, p. 162) is formally similar to Williamson's; here it is once more based on chances and on the desirability of the minimisation of worst-case expected loss. This is fine as far as it goes (and is no criticism of Pettigrew), but the adoption of these additional principles is not mandatory.

Summing up the previous two sections, the question was: why does de Finetti stop at the basic rules of probability and go no further? The contention here is that the motivation he adopts for justifying coherence might be powerful enough to justify a host of additional rules as well, which would make the refusal to adopt these quite arbitrary. I think these are serious challenges. Interpreting the 'why' in my question as a question of interpretation of de Finetti, I would say that he wanted to build a theory which would be the equivalent of propositional logic but for uncertain beliefs. The theory would need to interfere as little as possible with the *content* of our beliefs, and only represent formal rules to be adhered to. Regarding the more difficult question of whether this is justified, Mura (1995) shows that from simple Dutch Book arguments it is possible, in fact, to justify regularity, which is additional to, and not deducible from, the basic probability axioms. If we want to avoid this, it seems that Mura's indirect bets would be a good way to do it. In any case, even if de Finetti bites the bullet, keeps simple Dutch Book arguments and accepts regularity, he is safe from the objections arising from the objective Bayesian camp. If he goes for the accuracy-based argument for probabilism, on the other hand, he has to contend with a different set of criticisms. Here too I think de Finetti's theory can survive. Quite simply, the arguments for *calibration* and *equivocation* rely on additional formal structures and principles which are not needed in a theory such as de Finetti's. In an attempted logic of the uncertain, de Finetti is entitled to favour those rules whose violation brings about the possibility of strict domination over others that don't. The proof that the Brier score is proper does not rely on coherence arguments; nonetheless, I argued that de Finetti can make this argument without being forced to argue for the minimisation of worst-case expected loss as well. Next, I consider the normative character that coherence is supposed to have.

3.5 Normativity

Pettigrew (2016) carefully gives his reasons for adopting the principles he does. His aim is to convince the reader of their normative character, which is then inherited by his conclusions on rational reasoning. De Finetti does no such thing in support of the Brier score, nor does he put much effort into backing up coherence itself. One might well wonder what basis the normative status of de Finetti's theory actually rests on—and the theory is definitely supposed to be a normative one, not a descriptive one. He writes that the rule of coherence illustrates how "one must" work with probabilities, but notes the following:

> The 'one must' is to be understood as 'one must if one wishes to avoid these particular objective consequences'. It is not to be taken as an obligation that someone means to impose from the outside, nor as an assertion that our evaluations are automatically coherent. [...] Given any sets of events whatsoever, the conditions of coherence impose no limits on the probabilities that an individual may assign, except that they must not be in contradiction amongst themselves (de Finetti, 1974/1990, p. 72-73)

The previously mentioned consequences of losing money in a system of unfair bets, or of scoring provably less than one could have done in a proper scoring rule scenario, are objective in the inter-subjective sense: anyone is (or should be) able to see them, regardless of their subjective credences. It is to be noted that de Finetti does not put any effort into actually giving reasons why these consequences are *bad*; for his purposes, it is enough that they are recognisable by everyone, and hence objective in this sense. We could even paraphrase the point thus: losing money in an unfair system of bets may or may not be a negative thing (although most people would recognise it to be such), but everyone can realise that this can happen if they have incoherent credences. Hence, *if* you wish to avoid such consequences, your credences should be coherent.

A problem with this 'conditional offer' could be that, while coherence is widely thought to be a positive feature of sets of degrees of belief, it looks as if this conditional approach could be generalised to include whatever we want. So: *if* we value money losses, we should be incoherent[7]; or more generally, whatever practical, objectively recognisable goal we have, there can be an equivalent justification which hinges on an argument identical to the one used by de Finetti. It should be noted that while de Finetti thought that everyone should be able to express in probability whatever beliefs they pleased, he did think that people should be coherent. Now we are bringing this 'freedom' one conceptual level up and achieving a sort of methodological anarchism that de Finetti did not favour. The question then becomes: why should probability, and not some other method that achieves some other practical goal, be the logic of science, as de Finetti (1930) claims?

This issue points to the discussions on pragmatic versus non-pragmatic justifications of probabilism (see Joyce (1998)). A theory such as Pettigrew's (2016) or Joyce's (1998) do not have the problem outlined above, because they explicitly state that accuracy, as measured by a proper scoring rule, is closeness to truth—an idea de Finetti was opposed to (de Finetti (1930), de Finetti (2008, p. 53)). So a scientist, or someone who wants to get closer to the truth, should be coherent because otherwise she is further from the truth than she could be, whatever the truth turns out to be. The scoring rule here has the role of a vehicle for the desired conclusion: given some reasonable assumptions, the Brier score emerges as a good measure of distance from truth, and minimising this expected distance is what, in turn, brings about probabilism. But in de Finetti an agent should be coherent because she would otherwise suffer a higher penalty under the Brier score than necessary, whatever the state of the world. Here I am taking de Finetti's use of scoring rules out of context, since their role in his theory is definitional: the meaningful concept of probability

[7] Although, as Hájek (2005) points out, we should beware of Good Bookies, who want to gift us money.

is given, by definition, as the answer an agent would give if she were subject to Brier score penalties. The worry, however, remains: it is unclear why she should be coherent in general, given that she will not suffer potential penalties on every decision she makes.

I think the intended answer to this question is that incoherence is similar to logical inconsistency, so an incoherent evaluation, regardless of whether it will be punished by higher-than-needed penalties, shows there is something wrong in how we organised our beliefs. This is separate from seeking the truth, and in fact it gives a reason for being coherent, whatever our goals happen to be—including doing scientific research. The problem is that it is notoriously difficult to define exactly in what sense probabilistic incoherence represents a form of inconsistency. With bets, there is a sense of inconsistency if we value exactly the same thing in two different ways. For example, assuming linearity of utility in money (and all the rest, see Chapter 4), betting on $A \cup B$ (with A, B incompatible) or simultaneously betting on A and B separately has exactly the same effect, so accepting to bet at different prices on these two cases shows an inconsistency in evaluation. This idea was originally Ramsey's (1926/1990). Howson (2008) criticises its dependency on the linearity of utility in money, thanks to which you get the principle for free (more on this in Chapter 4). Joyce (1998, p. 586) writes that "it remains unclear why this should be counted an *epistemic* defect given that the inconsistency in question attaches to preferences or value judgements", and answering this problem actually motivates his whole non-pragmatic approach to justifying probabilism. Another approach is that of Mura (1995), who defines *cred-contradiction* in terms of indirect bets.

But how do we characterise coherence as an analogue to consistency in a bet-free, accuracy-based approach like the late-de Finettian one? The problem is that neither Joyce nor Pettigrew, who takes up a similar challenge and writes (2016, p. 9) that "Probabilism [...] is akin to the putative principle of rationality for full beliefs that requires that an agents beliefs be logically consistent ", provide a sense in which coherence is similar to consistency. Being further from the truth than necessary is an epistemic defect, but it does not imply inconsistency (we can have a full all-or-nothing belief that the Moon is made of cheese and this can be part of a consistent set of similarly outlandish ideas). At present, it is not as yet obvious to me what kind of inconsistency inaccuracy represents, how to attach an idea of inconsistency to a subjective Bayesian accuracy-based approach, or how to salvage its normativity in another way. I see a few possible solutions which I list below, but am currently unsure as to their validity and leave their exploration to future work (without ruling out, of course, that there may be good existing solutions to this that have escaped my attention.)

These possible solutions are as follows: (1) Perhaps this is not such a big worry; probabilities will be by definition coherent thanks to their operational definition, and they carry through this coherence as an agent operates with them. Perhaps we eliminate the issue at the initial step and do not to worry more about the problem of understanding the sense in which an incoherent reasoner is also inconsistent. (2) While we define probability thanks to scoring rules to avoid the problem of insincere bettors which I explore in Chapter 4, we could keep the idea of consistency linked to bets, on a Ramsey-like or Mura-like understanding. (3) We find a good link between inaccuracy and incoherence; this would be my preferred solution. Perhaps an equivalent of a Mura-like solution but with accuracy instead of bets. (4) We attach our subjective Bayesian theory to a full-blown theory of how to relate degrees of belief to all-or-nothing beliefs, such as that of Leitgeb (2017), and borrow concepts of inconsistency from there. We otherwise use a concept of probabilistic inconsistency which is not necessarily dependent on accuracy. (5) We substitute "correctness" or "empirical adequacy" for truth and we mimic the arguments by Pettigrew and Joyce. This disregards the search for a consistency-concept, but can give normativity to the approach.

I shall put these questions aside for now, and focus on another popular line of criticism against de Finetti: that he allowed everything and anything to pass as rational by the lights of his theory.

3.6 Relativism and ‘anything goes’

In de Finetti’s 1931 declaration of philosophical intent (de Finetti, 1931/1989, pp. 179-180), he writes that we have no objective reasons to call someone who believes that eclipses cause wars ‘superstitious’. When we “distil from my opinions the objective part, i.e., the part that is purely logical or purely empirical, I will have to recognize that I have no reason to prefer my state of mind to that of a superstitious person [...] I can question nature so that it will give me data as elements of my judgements, but the answer is not in the facts; it lies in my state of mind”. The violently relativistic and exalted tone of the article (he ends with an ode to Fascism) are hard to stomach; the article suffers from having been translated into English for the first time in 1989, thus entering contemporary Anglo-Saxon debate when already a relic. That being said, I think that the central point holds good, and is reinforced by his more mature writings. In a straightforward sense, I read it as saying that raw data, or a given logical structure, cannot tell us what is true and what is false. That is not the same as saying anything goes.

Admittedly, ‘anything goes’ might well have been closer to de Finetti’s position in 1931. I am not attempting to sanitise his work from elements which might be harder to defend, nor claiming that he was completely coherent throughout his career. It does indeed look like de Finetti was more ‘radical’ in his younger years. Nonetheless, it seems to me that the central idea (although it might emerge as such in an ahistorical reading of the article) survived in his later writing, and is clear and arguably a good point. It is the idea that we are not going to find in brute facts, or in the logical rules themselves, a ready-made, knock-down argument for why someone is wrong. Unfortunately, we will have to persuade this person she is wrong by the usual argumentative means; of course these can include an appeal to data, or can be in the shape of a formal rule, but we should not deceive ourselves into thinking that these instruments will, in and of themselves, objectively determine the irrationality of a given belief state. Note that de Finetti did *not* think that, since the formal requirement of coherence has nothing to say on what an individual’s credences should be, that nothing can be said on this at all (de Finetti, 1931/1989, pp. 191-192).

In fact, in his (1974/1990), de Finetti dedicates a chapter (Chapter 5) to the matter. He addresses (1974/1990, p. 179) the "extreme dilemma that a mathematical treatment often poses: that of either saying everything, or of saying nothing". We could place objective Bayesianism in the former class, a theory that 'says everything', or gives formal rules that can select a single credal state as the only rational one to have; and de Finetti's critics accuse his theory of 'saying nothing', or of abstaining completely from recommending a particular credal state. De Finetti claims that his theory explores a third way: he proposes considerations that might help in coming up with a degree of belief, but emphasises that it is ultimately an individual reasoner's responsibility to decide which of these techniques to use and what degree of belief to set on. He advises against "superficiality" in evaluation: this includes thinking that since the judgement is subjective anything goes, and, importantly, thinking that "no mental effort is required, since it can be avoided by the mechanical application of some standardised procedure" (de Finetti, 1974/1990, p. 179). The guidelines de Finetti provides range from practical tips on how get used to putting our uncertainty into numbers (1974/1990, p. 180), how to order one's thoughts and reason clearly (1974/1990, p. 183-185), the application of uniform distributions (1974/1990, p. 199), and the prevision and use of frequency data (1974/1990, p. 202).

All in all, this amounts to the position that considerations on rationality and the content of degrees of belief are, of course, hugely important, but that they should be left *outside* the formal theory of probability.[8] This does not mean that formal rules cannot be used in the process of evaluation of probabilities, but these are not considered to be constitutive of the concept of probability itself, and the decision of which rules to use (for example the determination that we really have no evidence that speaks in favour of, say, heads or tails in a coin toss) is ultimately free and subjective. There is likely much more to be said about this, but it seems to me that de Finetti's position is tenable: the decision to keep some formal rules out of the main axiomatisation of epistemic probability does not necessarily entail slipping into an

[8]For more on this, see (Howson & Urbach, 2006, pp. 265-266) and especially Berkovitz (2018).

anything-goes position. Or, to be more precise, according to the formal theory itself, *anything goes*; this is because the theory is supposed to be neutral with respect to the content of degrees of belief. But this does not mean that if we adopt this type of theory *anything goes* from the point of view of rationality; but we will have to use tools and arguments external to the theory itself to determine which credal state has better support than another. This is in line with the idea that de Finetti's version of subjective Bayesianism gives necessary, but not sufficient, norms for for rationality.

Having thus outlined de Finetti's position in a way that contrasts contemporary readings of him, I think one last major point remains. The main thing the critics of de Finetti share is a worry that his theory is not *objective* enough: some beliefs are *objectively* crazy, but his theory is not able to reject them. De Finetti himself actually thought he was rescuing objectivity, so there must be some crossed wires in the debate. I shall discuss this in the final part of this chapter.

3.7 Senses of objectivity

It is a curious fact that de Finetti, accused of being insufficiently objective, actually felt that his theory finally vindicated objectivity. The concept occupies a particularly central role in Williamson's objective Bayesianism, and Williamson too is concerned about insufficient objectivity in de Finetti, so I will look at his work more closely in this section. My aim in this final section is to try and clarify this debate: the first diagnosis that comes to mind is that the different schools of thought must be talking at cross purposes. I will use Douglas (2004)'s work on the concept of objectivity, and locate the respective uses of the concept by de Finetti, Vailati & Calderoni and Williamson in her classification of the different senses of 'objectivity'. Douglas argues that each different sense of objectivity is not logically reducible to one another; if we could show that Williamson and de Finetti are simply referring to different senses of objectivity, we could hitch ourselves on to Douglas's argument to make a good case for the claim that the two approaches are indeed talking at cross purposes. Unfortunately, and rather surprisingly, I will argue that this is not the case: Williamson, de Finetti and the pragmatists all refer to one and the same sense of objectivity in Douglas's classification, and so this simple line of argument fails. However, the different approaches do make a different use of this sense of objectivity, and so I still think they are talking at cross purposes, albeit in a more subtle way. I hope that showing this can bring the debate out of the confusion in which it is now mired and on to a more productive level.

It is important to note that this does not mean that the two positions are more in agreement than what is commonly thought. The opposite is true: the difference in their respective approaches to objectivity is crucial, and brings further support to my broader point, sketched in Section 3.1. De Finetti's theory does not belong to the objective-subjective spectrum of theories of rationality in formal epistemology. This is a classification made often by contemporary writers, and his theory is then criticised accordingly. But de Finetti was not interested in a formal theory of rationality, so he does not belong there. As I argue in Chapter 2, he wanted to give a mathematical theory of probability in which the central concept is, according to his standards, philosophically well understood, or, in a word, *meaningful*.

Douglas (2004) finds that the concept of 'objectivity' in philosophy of science is invoked in ways which are so varied that there must be a multiplicity of concepts that go under the same name. She sets out to define these different uses in a way which is "operationally accessible" (Douglas, 2004, p. 453): given the term's common use as a persuasive argumentative tool, we should be able to settle what is objective and what is not, and which sorts of calls to objectivity are appropriate for which contexts. Douglas distinguishes three separate kinds of objectivity: objectivity$_1$ has to do with interactions with the world; this is the way in which we hope that experimental data is objective. Objectivity$_2$ has to do with how we use values in our reasoning, independently of real-world data: it pertains to cases in which we try to assess whether our reasoning, say an argumentation or an assessment, is biased or objective. Objectivity$_3$, finally, has to do with "social processes involved in knowledge production" (Douglas, 2004, p. 461). Each mode is divided and classified further, but objectivity$_3$, and a particular kind of objectivity that, according to Douglas, falls under it, will be the focus of my attention. I treat the concept of objectivity used by the pragmatists, de Finetti and Williamson in turn, and argue that they both constitute cases of *procedural objectivity*$_3$. This is how Douglas defines it: "Social processes can be considered "objective" if the same outcome is always produced, regardless of who is performing the process." (Douglas, 2004, p. 461). Procedural objectivity$_3$, Douglas notes, is similar to what Daston & Galison (1992) call mechanical objectivity, a term that characterises the desire for mechanically produced scientific images which arose in the second half of the 19$^{\text{th}}$ century. The idea is that an image is objective if we can produce it mechanically, without the intervention of human bias and interpretation; whoever repeats the procedure should obtain the same result. Douglas highlights the importance of procedural objectivity in the administration of public life and in collective, societal processes; in Daston & Galison (1992), mechanical objectivity is something which applies more narrowly to science. In de Finetti and the pragmatist philosophers Vailati and Calderoni procedural objectivity is applied to the concept of meaning in a general philosophical setting.[9] Williamson, finally, uses the concept in a more specialised problem in formal epistemology. The idea, however, is always the same: the desire for a repeatable mechanical process which gives the same results regardless of who runs it, so that the results of the process will be free of personal whim.

Vailati and Calderoni, the pragmatist inspiration for de Finetti, adopt procedural objectivity in the following sense: they write that for a sentence to have meaning, it should be clear what procedures are possible in order to verify whether the sentence is true or false. They

[9] Both sets of authors also mention, or go close to, other forms of objectivity. In 3.6 I argued that de Finetti considers unfair systems of bets (Dutch Books) an objective consequence in the sense that anyone can see it, regardless of their particular opinion. This is Douglas's *concordant objectivity*$_3$ (Douglas, 2004, pp. 462-463). Nonetheless, I think it is the above sense of *procedural* objectivity that drives de Finetti's general approach. Vailati & Calderoni, on their part, talk about the predicted experiences that a sentence entails, which evokes Douglas's *objectivity*$_1$, which has to do with the interactions with the world (Douglas, 2004, pp. 456-458). Again, however, I note that the overall aim of the Italian pragmatists is to give a procedure that will always give the same results if applied correctly: it is from this freedom from personal whim that objectivity arises. The objective character of the interactions with the world is given for granted; it is assumed that, upon seeing the result of an experiment, any agent would agree on its meaning for the hypothesis tested.

say this will ensure the sentence is *objectively* checkable, the judgement of truth or falsity being thus "less dependent on individual impressions and preferences" (Vailati & Calderoni, 1909/2010, p. 234). Along similar lines, De Finetti writes: "statements have *objective* meaning if one can say, on the basis of a well-determined observation (which is at least conceptually possible), whether they are either TRUE or FALSE" (de Finetti, 1974/1990, p. 6). Objectivity here is given by the existence of a predictable, repeatable procedure that checks whether the statement is true or false. Ideally, it is implied, we would assert every meaningful statement together with the instructions on how to check it. I think this is the best way to understand de Finetti's operationalism, and the two operational definitions of probability that de Finetti offers in his de Finetti (1974/1990) book are in this vein: probability is defined together with the somewhat idealised instructions on how to measure it. For example: we might say Rupert has degree of belief 0.9 that tomorrow it will rain. Then, granting the idealised conditions in which this experiment would take place, anyone could check that Rupert would behave, in a specific betting or scoring-rule scenario, in a way which reflects the fact that his degree belief in rain is 0.9. The objectivity invoked here is not in the sense of objectivity$_1$. The sentence is not, for de Finetti, objectively meaningful because we could base other applications on it—unless we take the special case of the 'application' in which we put Rupert in the specific betting or scoring-rule scenario. Nor is it objective because different experimental methods converge to it as a result (the elicitation methods of bets and scoring rules are equivalent, but in a purely mathematical sense de Finetti (1974/1990). The sentence is objective because, "on the basis of a well-determined observation", anyone can check its truthfulness.

Now, procedural, or mechanical, objectivity$_3$ is clearly what guides Williamson's account too. In objective Bayesianism, "[t]he agent's degrees of belief are objectively determined by her background knowledge and there is no room for subjective choice" (Williamson, 2007, p. 2). In cases modelled by a finite sample space, the application of Williamson's formal rules of *Probability, Calibration* and *Equivocation*, does indeed give a unique numerical degree of belief. (The first principle is what

I have called coherence; details of the other two are unimportant for the current work.)

I would argue that de Finetti and Williamson make use of the very same concept of objectivity. This could be seen as a curious assertion for philosophical positions which are declared opponents on precisely this point—and I think it is correct to consider the positions are diverging on this. The upshot is that it is not possible to run a simple, Douglas-backed argument to show that subjective and objective Bayesianism are talking at cross purposes with regards to objectivity. But the to approaches are, nonetheless, at cross purposes, just in a different sense. The crucial difference lies in how the two currents make use of procedural objectivity. For de Finetti it is the concept of probability itself which must come with a procedure to check its practical consequences. This is intertwined with the criterion for meaning studied in Chapter 2 and his requirement that probability be treated in a similar way to scientific concepts, and he applies procedural objectivity, as he does the criterion, at the level of the sentence $S =$ "the probability of event E is a ". De Finetti requires there to be a procedure by which anyone can check whether this sentence is true or false, or else it will be meaningless. And this procedure must be intelligible and in principle doable by anybody, with the same results: hence the objectivity. Williamson, on the other hand, thinks that the *number* a should be objective; it should be determined in such a way that any rational agent, given the same background information as the agent who came up with it, would have the same degree of belief.

Using the same idea of objectivity in either of these different ways does not exclude the other; but, clearly, neither do they need to be used concomitantly. In a given situation there might be an ideal degree of belief that could be worked out thanks to a given repeatable procedure—but it may or may not be checkable whether an agent holds such degree of belief. Maybe no agent has ever been in that specific situation, or one has but absolutely refuses to take part in any betting or scoring rule game. Vice versa, and more simply, it might be checkable, with one of de Finetti's procedures, whether an agent holds a given degree of belief. But this degree of belief may or may not have been worked out by applying *probability, calibration* and *equivocation.*

In conclusion, it is useless for a supporter of one position to use objectivity as a stick to beat the other. Each position is objective enough, in its own way of applying the term. The discussion, in the terms in which it has happened, is at a dead end. Having cleared that up, we can glimpse a more productive direction in which to go: when is it good to have a procedure by which whoever views a given set of information will come up with the same specific probabilities? And, on the other hand, when would it be useful to have a procedure to determine whether a probabilistic assertion has a practical consequence? It might be possible to devise a full classification of different "operationally accessible" objective probability concepts, in the style of Douglas. I leave this exploration for future work.

3.8 Summary and conclusion

At the beginning of the chapter I promised a new defence of de Finetti's subjective Bayesianism. I have argued that de Finetti is not constructing a theory of rationality at all, so a major line of criticism towards his approach is misguided. He is trying, rather, to situate probability as a type of content-less logic. This is part of his broader attempt, which I treat in Chapter 2, to study probability in a way similar to how he thought the basic phenomena of science should be studied. This results in the focus on a checkable, meaningful concept, and is not concerned with setting out general formal rules for rationality. The aims of subjective and objective Bayesianism are so distinct that I do not think they should be considered as slight variants within the same endeavour, despite the widespread acceptance of this viewpoint. Objectivity is often given as the direction along which these positions differ, and this often goes hand in hand with the idea that more formal rules mean more objectivity. De Finetti disputes this, and I argue that the heart of the disagreement lies in the different uses of objectivity that these two positions make. Douglas's procedural objectivity, I find, is a good description of what kind of objectivity both positions invoke. De Finetti, however, thinks there should be a procedure which tells us if a probabilistic utterance is objectively checkable, while objective

Bayesians think there should be a procedure to tell us which degree of belief is objectively best.

Before this, I treated three other points in which there is a natural clash between objective Bayesians and de Finetti. I argued that de Finetti can safely adopt only the formal rules that he does (perhaps with the addition of the rule of *regularity*), although arguing for this is not trivial; I also argued that the accusation of his approach being an anything-goes position are unfair, as there should be space for deciding which rules are a constitutive part of the theory of probability and which are not, without being accused of such laxness. On the normative status of de Finetti's, or any other, probabilistic rules for reasoning I left more of an open question. If we justify probabilism through accuracy arguments, it is currently not obvious for me how to align this to logical inconsistency, or to another idea from which normativity might be inherited. I leave this as a starting point for future research.

My overall aim is to provide a better picture of subjective Bayesianism in order to suggest new, productive directions for this branch of the debate in Bayesian epistemology and philosophy of science. Some of de Finetti's positions and priorities are locked in debates from which mainstream philosophy has largely moved on. These may be of interest to historians, or may emerge as relevant in the future. But many of his points, when properly understood, can fit quite seamlessly and constructively into contemporary debate. De Finetti's idea of probability as a logic is an area of active research (Howson (2008), Howson (2009), Mura (2009), Sprenger (2018)), as is his tentative suggestion on how to apply this to quantum mechanics (Berkovitz (2012)). His idea that probability is a primitive concept can, as happens in Chapter 2, enter in a productive debate with the position Eriksson & Hájek (2007) call *primitivism* about degrees of belief.

His clear separation between the phenomenon of degrees of belief and its mathematical model of probability can free both the mathematical and the philosophical practices from some foundational worries. While the axioms should be as good a description as possible of degrees of belief, we cannot expect a complex mathematical result to be automatically true of degrees of belief, so mathematicians need not worry about their practice having veered off course and no longer describing the 'real thing'[10]. More saliently, we do not need to worry, in philosophy, about Bayesianism being bad psychology. For example: of course we do not *really* have real-valued degrees of belief; this is an artefact of the model, not of the target (on this, see also the discussion by Jeffrey (1984, pp. 82-84)).

[10]Admittedly, there might be little danger of this: Bingham (2010, p. 11) reports this emblematic quote by Doob, a mathematician who did much seminal work in probability: "I cannot give a mathematically satisfactory definition of non-mathematical probability. For that matter, I cannot give a mathematically satisfactory definition of a non-mathematical chair"

Chapter 4

Betting odds and sincere degrees of belief

4.1 Setting

Probability has a long history of close association with betting and games of chance (see, for example, Hacking (1975/2006)), and in some philosophical schools of thought betting is taken as a *definition* of the concept. This chapter is about one such influential definition and a problem it faces—a problem which I think is fatal. In Bruno de Finetti's 1974 *Theory of Probability*, degrees of belief are meant to be measured by offering a special kind of bet to a person and observing which odds she accepts. In this chapter I address a fundamental problem regarding this deep link between bets of this kind and degrees of belief, namely, that the agent does not know whether she is betting for or against the occurrence of the event in question. This is a powerful distorting factor: supposing the agent has a degree of belief in mind, I will argue that she has good reason not to use this as her declared odds in the betting game. I will examine some possible responses to this, but argue that these responses fail to rescue the betting definition of degrees of belief. The problem I focus on in this work is theoretical and intrinsic to the definition itself, and so, it seems to me, more fundamental than those emerging in the lively debate on the topic. If my argument goes through, bets could still help the agent put her thoughts into numbers, but they would lose their role as a definition or as a measurement device and become mere aides for an agent's intuition. The chapter is structured as follows: in Section 4.2 I introduce the betting definition of degrees of belief; in Section 4.4 I show that, within the usual Bayesian framework, the betting definition often picks up a number which is different from the agent's degree of belief; some additional technical details related to this are to be found in the Appendix, Section 4.7; in Section 4.5 I argue that the assumptions made to save the betting definition are either unconvincing or so powerful that they render the definition itself redundant; I sum up and discuss my arguments in Section 4.6.

4.2 The betting definition

Preliminaries

Let us set the stage with the following, preliminary definition of probability by de Finetti: "The probability a that You attribute to an event E is [...] the certain gain p which You judge equivalent to a unit gain conditional on the occurrence of E" (de Finetti, 1974/1990, p. 75, my notation)

This is a pointer as to what the debate will be about: the idea is to look at how much an agent is willing to bid in a bet on the occurrence of an event, and to infer from that her degree of belief about that event. This helps us to understand the following preliminary observations, after which I will discuss bets in more detail. Throughout this chapter, I will take utility to be identical to money (as de Finetti does). That is, the utility function U for the agent in question is such that, for example, $U(€0.05) = 0.05$ and $U(€100) = 100$. This makes the exposition simpler: I will always talk directly about money with this understanding. It is unrealistic in general but it is a concession to the betting definition, and an abstraction which is generally granted. In fact, any utility function which is linear in money (and not 0 everywhere) gives the same results for the betting definition. The rebuttal of the definition that I propose also depends on utility being linear in money; if the utility function of the agent in question is not linear, in general it will not be true that her betting prices will be identical to her degrees of belief, so the whole betting approach and its rebuttal don't get off the ground. A utility function which is identical to money is also increasing in money; this is not needed for the two-sided betting definition, nor for its rebuttal, but in the latter case some small adjustments need to be made. I say more about this in the Appendix.

Here are the notation and conventions I will use. The details and the way they are used will become clear below. Event E is assumed to be well-defined and verifiable; I will indicate the eventuality of E not occurring by $\bar{E}$. The stake S is a positive number which can be won or lost by the player. I indicate the sums that an agent pays out by a minus sign: for example, 'winning' $-S$ means paying out that sum. Here $S = |s|$, where s is the stake in de Finetti's definition below. The betting definition is a game with two players, which I call agent A and bookie B. Other symbols will be defined as they are used.

One-sided bets

We now go back to bets. Here is an example of the definition above: suppose you consider these two outcomes equivalent: receiving €0.05 for sure, or receiving €1 if it rains tomorrow. This would indicate a low confidence in rain tomorrow, because you are ready to accept a low sum of money and forego the chance to win a considerably larger one. According to this initial definition by de Finetti, the probability you attribute to rain tomorrow is $P(rain) = 0.05$.

One quickly realises that this, in practice, would not work very well. Suppose we were asked what sum we consider equivalent to the gain of €1 conditional on an event E occurring; if this were an actual game, in the knowledge that we would stand to receive this sum for sure, we should definitely declare a number as high as possible. Even if we thought that $P(rain) = 0.05$, we might want to say $P(rain) = 0.9$, or more: nothing said so far would stop us from declaring that we consider receiving, say, €1000 for sure, equivalent to receiving €1 conditional on event E occurring. We would emerge €1000 richer, and the subjective-Bayesian experimenter none the wiser as to how strongly we believe that E will occur. Note that this is not a bet, as usually understood. We could modify the game a little and ask the following: what is the maximum you would be willing to pay, in order to receive €1 if event E occurs? This is now similar to a normal bet. But now we have the opposite problem: even if we thought that $P(rain) = 0.05$, we might declare that €0.01 (or even €0) is the maximum we would pay, because if we know we would win €1 anyway if E occurs, we might as well try and pay as little as possible for the bet.

Two-sided bets

The full betting definition of degrees of belief seeks to avoid distorting factors of this kind by introducing the following device: we measure degrees of belief in a special kind of bet in which the agent doesn't know if they are betting *on* event E happening, or *against* it. Note that a bet against E is equivalent to a bet on $\bar{E}$. Here is de Finetti's definition, again adapted to my notation and treatment: given a well-defined, verifiable event E,

> You are obliged to choose a value x, on the understanding that, after making this choice, You are committed to accepting any bet whatsoever with gain $s(\mathbf{1}_E - x)$, where s is arbitrary (positive or negative) and at the choice of an opponent. $P(E)$, the prevision of E according to your opinion, is by definition the value x which You would choose for this purpose. (de Finetti, 1974/1990, p. 87)[1]

Here is some more assumptions made in the betting scenario: bookie B offers to agent A a bet regarding event E and asks A to declare a betting price x that she finds acceptable. Once A has declared a betting price x, bookie B decides the magnitude of the stake S and whether the bet is going to be on E or on $\bar{E}$. It is then checked whether E has occurred, and A receives or pays out an amount determined by this occurrence and by the direction of the bet. A models the occurrence of the event and the direction of the bet as probabilistically independent (it would hardly make sense to enter a game where an opponent can decide, or have such an influence, that we lose money however we play). Finally, A and B should have similar levels of information; if B has privileged information on E, a loss of money for A whatever hap-

[1] $\mathbf{1}_E$ is the indicator of event E: $\mathbf{1}_E = 1$ if E occurs and $\mathbf{1}_E = 0$ if E does not occur. The notation in this style is mine.

pens cannot be interpreted as a fault in reasoning on her part. Below in Table 4.1 we have the possible gains and losses for the agent A.

	E	$\bar{E}$
Bet on E	$S - xS$	$-xS$
Bet on $\bar{E}$	$xS - S$	xS

Table 4.1: Possible gains and losses for agent A

Most basically, this special two-sided bet ensures that A will declare a number $x \in [0, 1]$. For suppose A tries $x = 1000$; then, the bookie B could make it so that the bet is on E. That is because for any $x > 1$, $xS > S$ and so $S - xS < 0$; this means the bookie could engineer a certain loss of money for A, and this in turn means that to avoid this A must choose $x \leq 1$. Analogous reasoning shows that it must be $x \geq 0$. This style of argument constitutes one direction of the celebrated Dutch Book Theorem, according to which betting odds must be probabilities. Further reasoning along the same lines shows that betting prices must be additive. This needs the assumption that A values the possible prize resulting from a composite bet on incompatible events E, F exactly the same as the *sum* of the prizes arising from the bets on E and F taken individually. This assumption is called *rigidity* by de Finetti, and it is so powerful that the argument becomes (admittedly) circular: betting prices are additive because they are assumed to be so (this becomes important in the discussion Section 5.6).

The betting definition, however, aims at doing much more than producing betting odds which are probabilities. What it aspires to is measuring the *sincere* degree of belief of the agent who plays the game: "the operative part of the definition which enables us to measure it, consists in this case of testing, through the *decisions* of an individual (which are observable), his *opinions* (previsions, probabilities), which are not directly observable."(de Finetti, 1974/1990, p. 76) Not any number will do: we are trying to get a handle on what this agent actually believes.

4.3 Further assumptions, surroundings and the argument

Further assumptions

All this means that many more assumptions are needed in order to reasonably claim that the betting definition measures degrees of belief. These assumptions are and have been the subject of lively debate, which I will allude to here. The agent who plays this game must have utility which is linear with money (see the Appendix 4.7), and so they must not be risk-averse (de Finetti, 1974/1990, p. 78). The independence of the states (in our case above, just $E, \bar{E}$) from the betting actions is also necessary: for example, placing a large bet on whether I will sleep 8 hours tonight might well keep me awake, which will in turn change my confidence in having an 8-hour sleep.[2]

Surroundings

Before going on to discuss this, I think it is worth raising a possible worry concerning the whole venture. De Finetti had already abandoned the betting definition of degrees of belief at least by 1979 (see the collected lectures in de Finetti (2008)). It might seem, therefore, that I am resurrecting a failed project just to try and shoot it down again. Fortunately, although the influence of de Finetti's definition might warrant this anyway, I don't think this is the case: the discussion and adoption of de Finetti's betting definition, or one in this style, outlasted de Finetti's own adoption. The literature on this topic is vast, and I shall now give an indication (necessarily incomplete) of the nature of some of these debates.

Williamson (2010a) and Jeffrey (2004), broadly speaking, take on board de Finetti's understanding of degrees of belief as betting prices, although for Williamson the latter are only an *interpretation* of the former (more on this in Chapter 5).

[2] And it might not be enough: Seidenfeld & Schervish (1990) show that cases which, taken individually, respect state-independence, can be coordinated in such a way as to make it impossible to know which of them measures the degrees of belief of the individual who plays the games.

Maher (1993, pp. 99-102), on the other hand, argues against the de Finetti betting definition. He points out that if our degrees of belief are incoherent, then we cannot choose betting prices identical to them, since this can bring about sure loss. Therefore, betting prices chosen in the two-sided bet are not necessarily identical to our degrees of belief. Hedden (2013) arrives at a similar conclusion via a different route which includes the following interesting observation. Let a and a' be the sincere degrees of belief in the occurrence of events E and $\bar{E}$ respectively. Then, Hedden (2013, p. 486) notes, the betting price x that makes the bets both on E and on $\bar{E}$ (in my notation) have expectation 0 is $\frac{x}{a+a'}$. If, on the other hand, $\bar{E}$ is the event which is the focus of the elicitation procedure, then we see that the betting price x' that makes the bets both on $\bar{E}$ and on E have expectation 0 is $\frac{x'}{a+a'}$, and so $x + x' = 1$. This means that x and x' are the *normalised*[3] versions of our degrees of belief a, a'. Therefore, x and x' are not necessarily identical to our degrees of belief a, a'; they are identical only if it is already the case that $a+a' = 1$, that is, if they are coherent. Hedden sees this as a failure of the elicitation device, as does Maher in his own argument. In the current chapter, I get round the problem by supposing that the agent is coherent, with degree of belief a in event E occurring, and degree of belief $1 - a$ in $\bar{E}$. This concedes more to the betting definition, and so strengthens my argument against it.[4]

Another place where a de Finetti-style betting definition has recently been an important player is in the debate over diachronic Dutch Book arguments (see Briggs (2009), Mahtani (2014)), and, in a particular application of these arguments, in the the literature on the Sleep-

[3]Meaning they are modified in such a way that they, collectively, add up to 1.

[4]In actual fact, I think it is a virtue that a method forces us to change our incoherent degrees of belief and make them coherent (even though here the step is done from (possibly) incoherent degrees of belief only so far as coherent betting prices). In my view, assuming that the incoherent degrees of belief represent our credence state, the new coherent betting prices could still faithfully represent it, but in a manner which is formally improved. That being coherent is better than being incoherent can be supported by one's favourite argument for probabilism: see footnote 2.1. That being said, a strength of Maher and Hedden's arguments is that they show that this particular normalisation is done in a somewhat underhanded way. Perhaps an agent would want to pick their own normalised degrees of belief, or different normalisation techniques might be suited to different incoherent sets of credences: I think this is a very interesting and under-researched area.

ing Beauty problem (see Elga (2000), Hitchcock (2004)). Here, the main focus of attention is not whether or not bets correctly measure or define degrees of belief, although there is an underlying assumption of a somewhat reliable relation between the two. It is not my intention to claim that the arguments put forward in this debate would collapse if this relation were to be proven completely unreliable, but since the papers were written, broadly speaking, within the tradition of the betting definition of degrees of belief, the debates would be, I think, affected. Interestingly for the current work, the contribution of Bradley & Leitgeb (2006) has been to argue that the Sleeping Beauty problem is in fact a case in which degrees of belief can come apart from betting odds. I agree with this conclusion, and my arguments below can lend it some support.

Finally, Eriksson & Rabinowicz (2013) argue that by offering bets we end up measuring not the degree of belief in event E (say), but a series of conditional beliefs related to the bet itself. Part of this is in a similar spirit to what I will do below.

The argument

De Finetti's reasons for abandoning the betting definition (in favour of proper scoring rules) are along the same lines as the one I will explore here. To my knowledge, the argument has not previously been developed fully in the way I do here, neither by de Finetti himself nor others. The problem I discuss in this chapter regards a basic feature of the betting definition: the uncertainty as to the direction of the bet. This is not something we can "abstract away from", as it is a central part of the definition. I do not claim that it is possible to "abstract away from" the other problems listed so far; but, whether or not the criticisms sketched above settle the debate on betting definitions, I think the problem I will discuss has the potential to do so. What has been written on this specific problem so far, by Mura (1995), Walley (1991), de Finetti (1974/1990) and de Finetti (2008) appears, in my argument, as one of the two horns of a dilemma. Walley sees the issue as a reason to move on from the two-sided bet as an elicitation procedure. Mura (1995, p. 23) writes that "there is no serious solution to this difficulty" and proposes a new interpretation of betting arguments. Here I propose to add an important element to the picture and get to the root of what is wrong with the betting definition of degrees of belief. I argue that either the betting definition very often does not work, or the kind of assumptions we need to make to rescue it are either unconvincing or render the betting definition itself redundant. My arguments here are a possible vindication of Mura's statement.

The position I will defend is that there is no reason to think that a Bayesian agent, especially the idealised one around which the theory is constructed, would or should approach the two-sided bet in such a way as to give her sincere degree of belief a as her betting price x. The betting definition seems to be dealt with in two inconsistent ways. On the one hand, it is seemingly designed to force a potentially dishonest agent, who will declare any betting odds that maximise her gain, to actually declare betting odds that are identical to her sincerely held degree of belief. On the other hand, the agent in this game does not seem to be playing to win the largest possible amount, but simply to have an expected gain of 0. De Finetti, more or less explicitly, endorses the latter interpretation. I will argue that both interpretations of the two-sided bet are problematic. In the first one, if the agent is trying to maximise her gains it is reasonable to conclude that the betting odds she will accept will, in most cases, be different from her sincerely held degree of belief. I will discuss this below in Section 4.4, with more details in the Appendix to this chapter, Section 4.7. The second interpretation, on the other hand, rescues the definition but has serious shortcomings: it relies on assumptions which are either unconvincing, or make the betting definition redundant. I discuss this in Section 4.5.

4.4 Anticipating the direction of the bet: why the betting definition is often wrong

As has been pointed out by de Finetti (1974/1990, p. 93), de Finetti (2008, p. 29), Walley (1991, pp. 624-625) and Mura (1995, pp. 22-23), if we were playing this two-sided bet against an opponent, we might try to guess their beliefs and adjust our declared betting odds accordingly, in order to engineer a positive expected gain for us. Walley and Mura describe the problem and accordingly abandon this elicitation method, while de Finetti (1974/1990) offers an unconvincing response to it, which I return to in Section 4.5.

I shall now go through some of the ways in which a Bayesian agent might reason when in the two-sided betting game. My intent is to make the simplest assumptions possible, with generality of the results in mind. The main assumptions I will make are that the agent reasons probabilistically about the direction of the bet, and that she plays to maximise her expected gain. If one accepts these assumptions, the betting definition very often gets it wrong; but, as I will argue in Section 4.5, not accepting them is just as problematic.

In most cases, I will assume the agent has a certain degree of ignorance of the bookie's intentions. There is little agreement in the literature on how to model ignorance, and the more involved a model of the agent needs to be in order to vindicate the betting definition, the less convincing this vindication will be. After all, this definition is supposed to be a starting point for a general treatment of probability; there should be no need, therefore, for advanced modelling assumptions for it to rendered valid. In any case, neither simple-minded nor slightly more involved modelling assumptions are able to vindicate the betting definition.[5] It is important to note that none of these observations are knock-down: it is not normative that an agent should reason in any of the ways which follow. Nonetheless, as I will defend in more detail in Section 4.5, the agent will reason *probabilistically* in some way or another about the problem, and the accumulation of negative examples shows, at the very least, that the two-sided betting definition often, or even in most cases, gets is wrong.

I will now progress from the very simplest to the slightly more involved assumptions an agent could make. All the proofs are in the Appendix. Here is how we can model the situation. Looking at Table 4.1, let x be the betting price A declares in the game, and suppose A has a probabilistic degree of belief a about the occurrence of event E

[5] A significant assumption I make throughout is that the bookie does not alter his degree of belief after seeing what betting price the agent has chosen, perhaps the agent changing her declared betting price with this in mind, and so on. It is beyond the scope of this work to explore the full game-theoretic consequences of dropping that assumption; I would conjecture, however, that the deeper into the game we push our model-agents, the further we get from the original intent of measuring degrees of belief about the event in question.

(so A also believes $\bar{E}$ to degree $1-a$). Now, clearly if the two-sided bet forces agent A to declare $x = a$, then it works: it is designed exactly to pick up this degree of belief; if it does not, it does not work well.

I will assume throughout this section and the Appendix that agent A seeks to maximise her expected gain from the bet. The expectations of gain from each direction of the bet for agent A are

$$\mathbb{E}_A^E = S(a-x)$$
$$\mathbb{E}_A^{\bar{E}} = S(x-a),$$

where, for example, $\mathbb{E}_A^{\bar{E}}$ indicates the expectation for agent A from the bet on $\bar{E}$. If A declares $x = a$, she can be assured that the expectation, according to her degree of belief a, will be 0 in both cases, whether the bookie decides that the bet is on E or on $\bar{E}$. If A chooses to declare an x such that $x \neq a$, the expectation will be positive for one direction of the bet, and negative for the other. The task, for the agent, is estimating in which direction the bet will be.

A degree of belief over the direction of the bet

As a starting point, we can suppose that agent A has a degree of belief q in the bet being on E and degree of belief $1-q$ on the bet being on $\bar{E}$. That the agent would be able to assign a numerical degree of belief to this source of uncertainty is the assumption most in line with de Finetti's general stance (see Section 4.5). For now, we do not introduce the figure of a bookie. We have the following:

Proposition 1. *The expectation for A in the bet as a whole is: $\mathbb{E}_A = S(2q-1)(a-x)$. Hence, if $q > \frac{1}{2}$, the betting price that maximises A's expected gain is $x = 0$; if $q < \frac{1}{2}$, the betting price that maximises A's expected gain is $x = 1$; if $q = \frac{1}{2}$, A's expected gain is $\mathbb{E}_A = 0$ whatever betting price x she chooses.*

Guessing the degree of belief of the bookie

Now suppose agent A does not have a degree of belief directly on the direction of the bet, but that she tries to reason about the beliefs of the bookie B, who she is playing against and who she assumes is also out to maximise his expected gain. The most obvious assumption is that the bookie gains what the agent loses, and vice versa. Here, then, is the table of possible gains and losses of the bookie (Table 4.2), given these assumptions:

	E	$\bar{E}$
Bet on E	$xS - S$	xS
Bet on $\bar{E}$	$S - xS$	$-xS$

Table 4.2: Possible gains and losses for bookie B

Assuming bookie B has degree of belief b on the occurrence of event E, we can compute his expected gain:

$$\mathbb{E}_B^E = S(x - b).$$
$$\mathbb{E}_B^{\bar{E}} = S(b - x).$$

Given the above, B will choose to bet on E if $x > b$, because in this case the expected gain $\mathbb{E}_B^E$ for him will be positive, whereas $\mathbb{E}_B^{\bar{E}}$ will be negative. Conversely, if $x < b$, the bookie will choose to bet on $\bar{E}$.

If A (thinks she) knows the bookie's degree of belief b, she can maximise her expected gain as follows (de Finetti (1974/1990, p. 93), Walley (1991, p. 625) and Mura (1995, pp. 22-23) make the same observation):

Proposition 2. *Any x such that $a < x < b$, or $b < x < a$, will give A positive expected gain. If $a = b$, A should offer betting price $x = a$.*

It is important to note that if A thinks that her and the bookie have identical beliefs with regards to event E, then the betting definition works (de Finetti, 2008, p. 29). If one is satisfied that this is always the case, then, the problem is solved. It could work as follows: suppose that (1) we think bookie and agent must have identical evidence on E or the betting game wouldn't make sense; (2) we think that rational agents with identical evidence will have identical degrees of belief (perhaps because we are a certain brand of objective Bayesians); (3) we suppose that agent A knows that bookie B has identical evidence to her; and (4) A knows that $a = b$ (because of (3) or otherwise); then, agent and bookie will have the same degree of belief, $a = b$, and A

should quote her sincere degree of belief as her betting price. (1) and (2) mean that A and B will have identical degrees of belief; and (3) and (4), or just (4), means that A knows this.

It seems to me that doubts on each of (1) - (4) are legitimate, and this warrants further critical exploration of the betting definition. (4) in particular, namely that the agent would always know the degree of belief of the bookie but not the other way round (or the whole bet would be unnecessary) is puzzling. I think it is reasonable to suppose that a bookie with similar information to us would end up believing something similar, and I model this below. However, even in this idealised scenario, the supposition that we know, as soon as a bookie offers us a bet, that whatever degree of belief we have will match exactly the one of the bookie, seems to me an exceedingly strong one. De Finetti certainly disagrees with (2) (de Finetti, 2008, p. 23). Nonetheless, I will not argue against each one here. For those who do think (1)-(4) or (1), (2), (4) are always met, the betting definition works—but perhaps the burden would be on them to show why these premises are true.

I should note[6] here that the betting scenario is intended as an *idealised* case of elicitation, built in order to fix the definition of the concept of probability and then move on. But some uses of idealised starting points are more successful than others. An example of a potentially good one is de Finetti's choice to treat probability separately from utility for the sake of a simpler treatment (1974/1990, p. 80-81). By doing this we are able to take the monetary value of bets as their actual worth, and start a numerical treatment—without worrying about the complex fluctuations of utility, possibly influenced by things outside of our scope of interest. However, we can imagine that if we had complete information about all the factors that influence an agent's utility deriving from a bet (among others, her attitude towards risk and money) we *could* use the value of the bet as a starting point, and move away from it by adding more detailed and realistic information. This would be a successful use of an idealisation: we start with a simple case and are able to progressively move away from it to make it more and more realistic. The idealisation we are required

[6]This observation was suggested by Alberto Mura.

to make within the betting scenario, however, does not appear to be successful. Firstly, points (1)-(4) are hard to accept, especially, in de Finetti's words, "when formulating that very definition which should provide the connection with reality" (1974/1990, p. 80). And secondly, we cannot use the case of the agent knowing that she and the bookie have the exact same degree of belief as a simple starting point from which we can move away, because as soon as we try to move away from it to make the scenario a bit more realistic, the definition (very often) breaks down.

Note also that if the game is played with discrete currency, for the smallest denomination ε that the specific currency allows (for example, €0.01), if $a < b$, A should offer betting price $x = b - \varepsilon$; if $a > b$, A should offer betting price $x = b + \varepsilon$; here the betting price comes apart nearly completely from the sincere degree of belief.

Bookie's belief as a random variable: continuous uniform distribution

Next, we can drop the idea that A knows B's degree of belief precisely, and suppose that she estimates B's degree of belief b by a continuous random variable $\hat{b}$ that takes values between 0 and 1. Then, we would need to place a reasonable probability distribution over $\hat{b}$, something that would reflect A's total or partial ignorance about the bookie's beliefs. Here are some observations that this starting point allows.

Suppose A knows nothing about B's beliefs. A classic and intuitive probability distribution representing ignorance is the uniform distribution: all values of b are equally likely to A's eyes.

Proposition 3. *If $\hat{b}$ has a uniform distribution, A's expected gain in the bet is $\mathbb{E}_A = S(x-a)(1-2x)$, so to maximise her expected gain A should offer betting price $x = \frac{a}{2} + \frac{1}{4}$.*

The situation is depicted in Figure 4.1. The dotted line results from mapping each sincere degree of belief a to its corresponding expectation-maximising betting price x, given the assumptions above. By way of contrast, the solid represents $x = a$, the case in which the agent declares a betting price x identical to her degree of belief a. The only case in which an agent who is making the assumptions above would declare her sincere degree of belief a as her chosen betting price x, is when $a = \frac{1}{2}$.

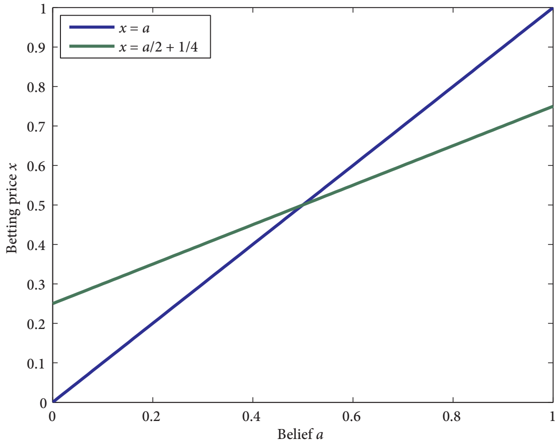

Figure 4.1: Sincere beliefs and maximising betting prices: continuous uniform distribution for $\hat{b}$

Bookie's belief as a random variable: other distributions

It is possible to make a few further general observations on continuous, not necessarily uniform, prior distributions that the agent A might use. For all continuous distributions the expression for the expected gain, which we are assuming A wants to maximise, is the following (see the proof for Proposition 3 in the Appendix),

$$\mathbb{E}_A = (x - a)(1 - 2F_{\hat{b}}(x)),$$

where $F_{\hat{b}}$ is the cumulative distribution function for $\hat{b}$. Let us call $m_{\hat{b}}$ the median of the distribution of $\hat{b}$. The agent has positive expected utility if she chooses an x such that $a < x < m_{\hat{b}}$ or $a > x > m_{\hat{b}}$. So whenever agent A chooses to represent her ignorance of B's beliefs by a symmetric probability distribution for $\hat{b}$, while having a degree of belief $a \neq \frac{1}{2}$, there exists an $x \neq a$ such that $\mathbb{E}_A(x) > 0$. On the other hand, if it is the case that $m_{\hat{b}} = a$, then A should set $x = a$.

I say a little more about this in the Appendix, where I also look at what the adoption of a Beta distribution would do for agent A. In general, however, the exploration of continuous distributions to model the bookie's degree of belief offers little support for the betting definition. There certainly are cases in which agent A would set her betting price $x = a$: an important one is if she models the bookie's belief by a distribution with median equal to a. But, even though it is not possible to quantify this without even further assumptions, the counter-examples pile up: it certainly is not *necessary* to reason in one of the specific ways just listed which result in $x = a$.

Bookie's belief as a random variable: discrete distribution and maximum entropy

As a final example, I look at how agent A might play the two-sided betting game if she is an objective Bayesian, in the style of Jaynes (2003) and Williamson (2010a). To illustrate the special features of this approach, I will model the belief of the bookie by using a discrete distribution.[7] Here I let the stake be €1, and the agent must declare a betting price in Eurocents, that is, it must be $x \in \{0, 1, \ldots, 100\}$.

Objective Bayesians propose that we use Shannon's measure of entropy H to quantify the amount of uncertainty expressed in a probability distribution. H is a function from a distribution to a real number; the larger this number, the less information this distribution is supposed to contain. Given a certain sample space, we can ask which distribution of probabilities has the maximum entropy. Solving this with no further constraints gives back the discrete uniform distribution. Results analogous to those in Proposition 3, and equally discouraging for the betting definition, apply.

To obtain something more interesting we can put in additional constraints. For example, as in the case above, our agent A might find it reasonable that the mean of the distribution modelling the belief of

[7] A discussion of continuous ignorance priors in Jaynes (2003, pp. 376-394) would not add much to the treatment above: Jaynes arrives at a prior distribution which is symmetric about 1/2

her opponent is equal to her degree of belief a. This is an application that Jaynes proposes in his book, and serves as a nice illustration. The results of this are in Figure 4.2. Again, two data sets plotted. The solid diagonal line represents the mapping from A's sincere degrees of belief a to the betting prices x that she would declare if she was perfectly honest, with $x = a$. The small circles represent the following: for each sincere degree of belief a the agent computes the probability distribution for $\hat{b}$ which maximises entropy, with the constraint that the mean of the distribution is $\mu_{\hat{b}} = a$. Each a is mapped to the betting price x which maximises her expected gain given this distribution.

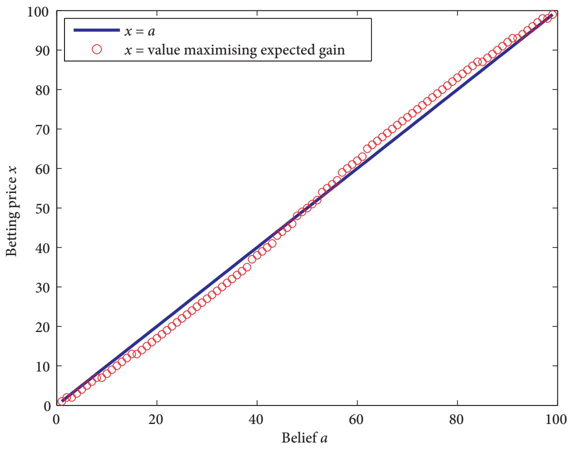

Figure 4.2: Sincere beliefs and betting prices according to maximum entropy distribution.

Note here that, although the two mappings coincide exactly only around $1\%, 50\%$ and 99%, the optimal odds given under this model are not very far from what they would be if they mirrored exactly the sincerely held degrees of belief. This can be both encouraging and not: if we offer the two-sided bet to an objective Bayesian agent who reasons as above, the number we will get back will not be very different from her true degree of belief, so it will be a decent indication of its whereabouts. It falls short, however, of a definition: in the vast majority of cases, it will be $x \neq a$. Of the authors who might endorse such an approach, Williamson (2010a, p. 31) does not *define* degrees of belief as betting prices, but *interprets*, or *explicates* them as such. It is not clear to me whether this requires the two concepts to be numerically identical. If they only need to be 'close enough' for the interpretation to go through, then this is potentially a good result for that school of thought: given a few assumptions which are quite natural within that framework, a Bayesian agent will give responses in the two-sided bet which are quite close to her degree of belief.

Preliminary conclusions

In this section I have shown numerous examples in which a Bayesian agent making reasonable assumptions will declare a betting price x which is different to her degree of belief a in the two-sided bet. In some cases x will be quite close to a, while in others it is almost exclusively tied to what A thinks her opponent believes. In a few special cases is the betting price x identical to a; but remember that this is supposed to be a general definition of degree of belief, not one that works only in special cases.

An objection to this could be that, especially in the latter sections, I have made quite specific, and by no means necessary, modelling choices, and so the results are no counter-example to the betting definition. Of course the betting definition won't work *if* we make these assumptions; but we don't need to make them, so this criticism is invalid, according to the objection. I think this is objection is quite weak, in so far as a definition of degree of belief, a basic concept in subjective Bayesianism, should be able to stand up to the very feeble challenge presented here, when the definition is being applied to an agent who uses some of the most elementary Bayesian tools. One can object to some of the specific modelling choices, but the most powerful assumptions I have made are that the agent A thinks probabilistically about the direction of the bet, and that she maximises her expected gain from the betting game. It is here that the source of the problem lies; and although some of the modelling tools I use here would not be endorsed by de Finetti (but would be by other Bayesians), these two assumptions should be accepted, within his theory, when using the betting definition. Indeed, while we can relax either condition, the resulting rescue of the betting definition is problematic. I discuss this next.

4.5 Playing for a draw: why the betting definition is unnecessary

So far, I have argued that if we apply the betting definition to an agent who thinks probabilistically, in most cases it will fail. I now look at the different ways a supporter of the betting definition get round this. All these responses rely on assumptions additional to, or different from, the ones I made above, and they save the betting definition, but at a cost. They can be summarised as follows: (a) The agent is not supposed to have a degree of belief about the direction of the bet, but only about the event E in question; (b) the agent risks having a *negative* expected gain from the two-sided bet if she tries to engineer a *positive* expected gain, so she should settle for expected gain 0 instead; (c) the betting definition might have practical problems but its strength lies in capturing the price at which an agent is indifferent between bets for and against an event, which is identical to her degree of belief; (d) the agent might be indifferent to gain so she will not try to game the bet for personal gain, or (e) the agent might value honesty more than monetary gain, so she will offer her true neutral betting price; (f) the potential for real gains and losses forces an agent to think carefully about her probabilistic evaluation.

I think these responses fail to secure the two-sided bet as a valid definition of degrees of belief. I discuss them next in order (a) - (f). My own position, which emerges in response to point (c), is that we should abandon bets as a definition of degrees of belief, and also abandon the idea that there is a necessary structural connection between the two (although of course there is some connection), and adopt a primitivism about degrees of belief in the style of Eriksson & Hájek (2007), which I discuss in Chapter 2.

No degree of belief over the direction of the bet

In Section 4.4, an important assumption is that agent A thinks probabilistically about the direction of the bet. But perhaps she is not supposed to think like this: she should focus only on event E. I see this as a weak criticism. There are two sources of uncertainty in the two-sided bet, as is clear from Table 4.1. The event E might happen, or it might not; and the bet might be on E or on $\bar{E}$. Both have influence over our potential gains and losses. It is hard to even see how an agent would know to treat these differently if offered a bet of this sort. One does not need to be a supporter of de Finetti to see this. De Finetti, however, expressly writes that we should treat all sources of uncertainty in the same way, whatever the source: that is, we should reason probabilistically about them (de Finetti, 1974/1990, pp. x-xi). What is more, he writes that it does not make sense, in *any* case, to say we don't know what the probability is, or that it does not exist: "probability [. . .] exists in that it serves to express, in a precise fashion, for each individual, his choice in his given state of ignorance" (de Finetti, 1974/1990, p. 84). Therefore, a good Bayesian agent should treat both sources of uncertainty probabilistically. She might do her modelling differently from the way I do in Section 4.4, but the arguments there make it seem doubtful that her chosen model will end up *always* suggesting that she set $x = a$.

Striving for expectation 0

Another important assumption I make is that the agent tries to maximise her expected gain in the bet. Against this view, de Finetti (1974/1990, p. 93) suggests that in the two-sided bet the agent should eventually choose that unique betting price that makes the expected gains from the bet on E and the bet on $\bar{E}$ equal: that is, $x = a$. This results in both expectations being 0. He reasons as follows: he acknowledges that by trying to estimate the bookie's beliefs, we might arrive at a positive expected gain from the two-sided bet, but he warns us that if we make a wrong estimation we might end up with *negative* expected gain. The matter is left at that, implying we should be satisfied that agents would normally pick $x = a$. In short, the implied recommendation is that, since the agent does not know which way the bet will go, she should avoid trying to maximise her expected gain.

I think this is a problematic response, as highlighted in the following example, which I think is relevantly similar to the two-sided bet. Let us imagine that we can decide to take part in a game of chance or abstain from it. Not playing has expected utility 0 for us, while we calculate that playing has positive expected utility. Let us suppose that it emerges later that our information about the chances involved in the game were erroneous, and so our expected utility in the game was, in fact, negative. In this case, it seems right to say that we should not have played. But the advice from de Finetti is *never* to play, even if we think we would have a positive expected gain from the game, because this judgement *could* be wrong. Note that the two-sided bet is not a rigged casino game: we might legitimately have a positive expected gain, and de Finetti admits as much. This advice, then, is simply too paralysing for it to be a general principle. To repeat: we cannot avoid taking all actions that we think have positive (even maximal) expected utility, just because we might be wrong in our assessment. We will use our best information to make good decisions, and if it turns out that some of the information was incorrect, then we will update it. If we never get to know it in time, then that's just tough luck.

It might be said that this advice by de Finetti is only to be followed in the specific scenario of the elicitation scenario. But my point is precisely that a Bayesian agent, following standard and reasonable rules, would not give her sincere degree of belief in the two-sided bet. Since the advice to be extremely cautious when our expected utility *could* be negative cannot possibly be a general rule, then the point stands. I struggle to see any good reason to simply assume that if the agent knew this was an elicitation bet (albeit with real money involved) she would abandon her ambition to choose the option that gave the highest expected gain and think that her best option would be to strive for expectation 0. I return to this in the following sections.

Capturing the indifference price

A change of emphasis has the potential to improve the response above, but at a certain cost. The strength of the two-sided bet is that it tests the price at which agent A would be neutral between betting on E occurring or against it; I will call this her 'indifference price'. It is, by construction, identical to her sincere degree of belief. The problem, however, is that the betting definition should be at least conceivably practical and general. It should work when applied to a third party; we should be able to say, about an agent A, that she has degree of belief such-and-such because that is what emerges from the bet. But the discussion so far shows that there are theoretical issues which prevent this from being true. The presence of an adversary prevents a Bayesian agent, in most cases, from answering honestly in the betting elicitation scenario.

So the two-sided bet is a private device which theoretically unearths our indifference price. In fact, we could even take the two-sided bet as the procedural definition of the psychological phenomenon of the indifference price. This, in turn, could be the abstract definition of degree of belief. It might be argued, then, that practical measurement problems are separate. But the indifference price is not needed as a definition of degree of belief, even in an abstract, non-operational role. A one-sided bet would work just as well: we know, privately, what the highest price is that we would pay for a gamble. Anyway, in many cases it is absurd to try and think of a bet on the truth of something, and this is a criticism that has often been levelled at the approach. The bets are often just additional, unneeded, baggage. The indifference price is equal to the sincere degree of belief, when betting considerations are appropriate and helpful, but degrees of belief are not necessarily indifference prices.

The two-sided betting definition, then, cannot even be conceived of as public, so it cannot be an operational definition, and is redundant as an abstract definition of degrees of belief. It could be a guide in our introspection, but, again, other guides exist. I think an explicit adoption of primitivism about degrees of belief is a good resolution. I deal with this in Chapter 2. Below, I describe some ways to try and escape this conclusion, but I think they do not work. The requirements are that it needs to be shown that there exists an interpretation of the two-sided betting definition which does not need ad hoc assumptions that are unworkable as general rules, and which makes the definition conceivably practical and public.

Indifference to gain and agent honesty

Given what has already been ruled out above, it seems to me that there two, non-mutually exclusive, reasons for an agent telling an experimenter honestly her indifference price: the agent might not care about gain, or her sense of honesty might override other considerations in her reasoning. I discuss these two reasons in this order.

If an agent does not care about gains, she does not need to be 'tricked' into declaring her true degree of belief by the offering of a bet. Worse, I think that whether or not A already has a numerical degree of belief, her lack of interest in gains or losses would make the two-sided bet a very bad device for her. She is being asked which price would make her indifferent between two bets: but as she is indifferent to gains and losses *all* prices would be her indifference price; and, on the other hand, if she *does* care about gains then we are stuck again with trying to explain why she will sacrifice her gains in this specific game.

We are left with the second assumption: that an agent will answer honestly about her indifference price. An honest answer on the agent's part might mean her sacrificing an expected monetary gain and whatever other advantages she thought keeping her beliefs for herself might bring. She surely realises this, as she sees that her indifference price is $x = a$. Arguably, then, from the point of view of agent A, being honest in declaring her indifference price is the same as being honest if simply asked what her degree of belief is. In the latter case, her expected disadvantage from sharing her beliefs remains the same as when she shares her indifference price (i.e. the possible loss tied to sharing one's information); and the expected gain from the two-sided bet was going to be 0 anyway. This suggests that, if we assume self-defeating agent honesty, the bet is an unnecessary complication.

Preliminary conclusions: thinking carefully about one's evaluation

I have argued that the two-sided bet only really works as a private intuition-boosting device to put a number to our feelings of uncertainty. I have also argued that this diminished role falls short of what a definition of probability is, even in a non-operational facility. There is a link between indifference price and degrees of belief, but there is no need to burden all degrees of belief with the necessity of being the result of reasoning in terms of bets. This is especially true given that the two-sided betting definition has no operational aspect, so we would be tying the definition of degrees of belief to an abstract betting process that brings no particular conceptual advantages. Even if we think that probability is just degrees of belief, primitivism about the fundamental concept of degrees of belief is a viable route. Mathematical probability emerges as the model of this primitive, unanalysed concept.

As a final counter to this, I consider the following. Perhaps the two-sided bet is privileged with respect to other devices that help us discover our degree of belief, and has a public aspect after all, because the money involved forces us to think carefully about our evaluation. What I have in mind here are cases such as the following: a person who agent A does not know, who is in no obvious distress, simply asks her how uncertain she is about the eventuality of rain tomorrow. She might reply she has no idea how uncertain she is, or offer a number without giving it much thought. This would mean the person's elicitation question has failed. But, the argument might go, if agent A now has to place a two-sided bet on (or against!) rain tomorrow, she should think carefully about what betting price she chooses, or she might stand to lose some money. The two-sided bet puts an agent on the spot, and forces her to explore her information and come up with her best estimate. This is more than a private reasoning device.

This is fine as far it goes, but as soon as the agent has made up her mind on what a should be, all the above arguments apply. Even if the bet could be useful for her to put a number to her uncertainty, there is little reason to think that it would also be of any use in getting her to tell us that number—which, remember, was the point of the exercise. And so it gets back to the status of being a private reasoning device. Note, furthermore, that even if the betting definition did work for agents who did not yet have degrees of belief, the fact that it would very often fail for agents that already have a degree of belief a would still be a major problem. This would be a theory saying that rational agents *should* have numerical degrees of belief, while giving a definition of them that often fails when they actually have them.

This concludes the discussion of how to save the betting definition of degree from the problems discussed in Section 4.4. I shall now summarise and discuss my findings.

4.6 Conclusion

In this chapter I have argued for the following position: if a Bayesian agent has a degree of belief over some event or proposition, the classic two-sided bet will not, in the majority of cases, pick up on this degree of belief correctly. This is mainly do the fact that the bet is two-sided. While the fact that there might be a problem with the two-sided betting definition has been noted previously, its full consequences have not yet, to my knowledge, been brought to light. This might explain why the debate surrounding this definition is still ongoing. Bringing to light the full consequences of the problem also calls for the novel task of looking into possible defences of the betting definition. I have argued that these fail to secure the two-sided bet as a valid definition of degrees of belief. I conclude that it should be abandoned: perhaps in favour of Walley's 1991 betting elicitation techniques, or ones based on proper scoring rules (see de Finetti (2008)); but we can also leave degrees of belief as an unanalysed primitive, in the style of Eriksson & Hájek (2007).

4.7 Appendix

This Appendix contains the proofs and observations which, although straightforward, would slow down the reader if included in the main text.

Utility functions. As I mentioned above in Section 4.2, a utility function which is linear in money will give the same results as a utility function which is identical to money, but for a utility function without these attributes, in general the betting definition will not work. Let U be the utility function in question. The expectation for the agent for the bet on E (the case of $\bar{E}$ is entirely similar) is

$$\mathbb{E}_A^E = aU(1-x) + (1-a)U(-x).$$

The two-sided betting definition works because the betting price $x = a$ is the only value that sets the expectation of both sides of the bet to 0. For a general, not necessarily linear, utility function, this fails. For $x = a$ the expression becomes

$$\mathbb{E}_A^E = aU(1-a) + (1-a)U(-a),$$

which need not be identically 0. If we suppose that U is a linear function however, we have, when $x = a$,

$$\begin{aligned}\mathbb{E}_A^E &= aU(1-a) + (1-a)U(-a),\\ &= aU(1) - aU(a) - U(a) + aU(a) = 0.\end{aligned}$$

Because of the symmetric character of the two-sided betting definition, the utility function can be increasing or decreasing. It cannot, however, be identically 0: in that case, $\mathbb{E}_A^E = 0$ for whatever value of x that is chosen, so the definition loses its power. If U is linear and decreasing, things work as follows: suppose, as above in Section 4.2, agent A tries $x = 1000$; then, the bookie B could make it so that the bet is on $\bar{E}$. Then, since for any $x > 1$, $xS > S$, the bookie can engineer a certain gain of money for A, which would represent a decrease in utility; to avoid this A must choose $x \leq 1$.

In my rebuttal to the betting definition some small adjustments are needed if the utility function is linear and decreasing. I give an example of this below, but I will treat it only once as it works analogously in each case.

Proposition 1. *The expectation for A in the bet as a whole is: $\mathbb{E}_A = S(2q-1)(a-x)$. Hence, if $q > \frac{1}{2}$, the betting price that maximises A's expected gain is $x = 0$; if $q < \frac{1}{2}$, the betting price that maximises A's* *expected gain is $x = 1$; if $q = \frac{1}{2}$, A's expected gain is $\mathbb{E}_A = 0$ whatever betting price x she chooses.*

Proof. For calculating the expectation, remember that the direction of the bet and the occurrence of the event are by assumption probabilistically independent. This means that we have $\mathbb{E}_A = S((1-x)aq+(x-1)a(1-q)-x(1-a)q+x(1-a)(1-q))$, hence the result. The properties of this expression are seen by inspection: if $q > \frac{1}{2}$, then $S(2q-1) > 0$ and x should be minimised while $x \in [0,1]$. A symmetrical argument is valid for the case $q < \frac{1}{2}$. If $q = \frac{1}{2}$, then $S(2q-1) = 0$.

For a general linear increasing utility function U, the result is worked out analogously, as we have:

$$\begin{aligned}\mathbb{E}_A &= S\left(U(1-x)aq + U(x-1)a(1-q) - U(x)(1-a)q + x(1-a)(1-q)\right)\\ &= S(2q-1)(U(a) - U(x)),\end{aligned}$$

so x should be minimised while $x \in [0,1]$. If the utility function is linear but decreasing, if $q > \frac{1}{2}$, then x should be maximised while $x \in [0,1]$. A symmetrical argument is valid for the case $q < \frac{1}{2}$. $\square$

Proposition 2. *Any x such that $a < x < b$ or $b < x < a$ will give A positive expected gain. If $a = b$, A should offer betting price $x = a$.*

Proof. Suppose $a < b$. If $x < a < b$, this will trigger the bet on $\bar{E}$ and A's expectation will be $\mathbb{E}_A^{\bar{E}} = S(x-a) < 0$. If $a < x < b$, this will trigger the bet on $\bar{E}$ and A's expectation will be $\mathbb{E}_A^{\bar{E}} = S(x-a) > 0$. If $a < b < x$, this will trigger the bet on E and A's expectation will be $\mathbb{E}_B^E = S(a-x) < 0$. The reasoning for $b < a$ is entirely similar, with only $b < x < a$ resulting in a positive expecation for A.

Now suppose $a = b$. If $x < a = b$, this will trigger the bet on $\bar{E}$ and A's expectation will be $\mathbb{E}_A^{\bar{E}} = S(x-a) < 0$. If $b = a < x$, this will trigger the bet on E and A's expectation will be $\mathbb{E}_B^E = S(a-x) < 0$. So it must be $x = a$.

For the observation under Proposition 2, note that if $x < a < b$, then $\mathbb{E}_A^E = S(b-a-\varepsilon)$, and if $b < x < a$, then $\mathbb{E}_A^{\bar{E}} = S(a-b-\varepsilon)$. $\square$

Proposition 3. *If $\hat{b}$ has a uniform distribution, A's expected gain in the bet is $\mathbb{E}_A = S(x-a)(1-2x)$, so to maximise her expected gain A should offer betting price $x = \frac{a}{2} + \frac{1}{4}$.*

Proof. Let P_A indicate agent A's credence function. From Proposition 2 we have that $P_A(\text{Bet on } \bar{E}) = P_A(\hat{b} > x)$ and $P_A(\text{Bet on } E) = P_A(\hat{b} < x)$. So the global expectation $\mathbb{E}_A$ for the bet is now the following (recall that direction of bet and occurrence of event are independent):

$$\begin{aligned}
\mathbb{E}_A &= S(x-a)(P_A(\hat{b} > x) - P_A(\hat{b} < x)) \\
&= S(x-a)(P_A(\hat{b} > x) - P_A(\hat{b} \leq x)) \text{ since } \hat{b} \text{ has continuous distribution} \\
&= S(x-a)(1 - 2F_{\hat{b}}(x)), \text{ where } F_{\hat{b}} \text{ is the cumulative distribution} \\
&\qquad \text{function for } \hat{b} \\
&= S(x-a)(1-2x) \text{ since } \hat{b} \text{ is uniformly distributed.}
\end{aligned}$$

Given this, it is immediate to calculate the x that maximises this expression. □

Comments on Section 4.4. We can make slightly more specific comments than were made in Section 4.4 if we suppose that A adopts a Beta distribution to model her knowledge of $\hat{b}$. It is common to use a distribution from the Beta family to model an unknown between 0 and 1. The discussion above regarding the median $m_{\hat{b}}$ still applies, and we can add the following observation. It might be reasonable to set one of mode ($M_{\hat{b}}$), median ($m_{\hat{b}}$) or mean ($\mu_{\hat{b}}$) of the distribution over values of $\hat{b}$ equal to a. These constitute different ways of acknowledging that the bookie's beliefs will probably be somehow related to the agent's own. (For example, they might find themselves in similar information environments.) Now, for Beta distributions, either $M_{\hat{b}} = m_{\hat{b}} = \mu_{\hat{b}} = \frac{1}{2}$; or $M_{\hat{b}} < m_{\hat{b}} < \mu_{\hat{b}}$ if $M_{\hat{b}} < \frac{1}{2}$, or $M_{\hat{b}} > m_{\hat{b}} > \mu_{\hat{b}}$ if $M_{\hat{b}} > \frac{1}{2}$ (Groeneveld & Meeden (1977)). So, if $a = \frac{1}{2}$ and agent A wishes to set mode, mean or median equal to it, it will end up being $m_{\hat{b}} = a$, then A should set $x = a$. Note, however, that if $a \neq \frac{1}{2}$ and A sets either $M_{\hat{b}} = a$ or $\mu_{\hat{b}} = a$ (but not both equal to a), then there exists an x such that $a < x < m_{\hat{b}}$ or $a > x > m_{\hat{b}}$, giving A positive expected gain.

Comments on Section 4.4. Here are some additional details on the maximisation problem that led to Figure 4.2. In what follows I will call *events* the numbers from 0 to 100, with the understanding that $\hat{b}$ could turn out to be any one of these. It is a completely different matter from the event E over which the agent and the bookie are betting. In this maximisation problem, the events are 0,1,. . . ,100. We call the probability of each event respectively $p_0, p_1, \dots, p_{100}$. We want to maximise:

$$H(p_0, \dots, p_{100}) = -\sum_{i=0}^{100} p_i \log(p_i),$$

subject to these conditions:

$$\begin{aligned} g(p_0, \dots, p_{100}) &= p_0 + \cdots + p_{100} = 1 \\ h(p_0, \dots, p_{100}) &= 0p_0 + 1p_1 + \cdots + 100p_{100} = a. \end{aligned}$$

By the method of Lagrange multipliers we need to solve this system of equations:

$$\begin{aligned} H_{p_0} &= \lambda g_{p_0} + \mu h_{p_0} \\ H_{p_1} &= \lambda g_{p_1} + \mu h_{p_1} \\ &\vdots \\ H_{p_{100}} &= \lambda g_{p_{100}} + \mu h_{p_{100}} \\ g(p_0, \dots, p_{100}) &= 1 \\ h(p_0, \dots, p_{100}) &= a. \end{aligned}$$

Here λ, μ are the Lagrange multipliers, which we will re-write below and find a solution for; and $H_{p_k}, g_{p_k}, h_{p_k}$ are the derivatives of the functions H, g, h with respect to the variable p_k.

We obtain that, for $k = 0, \dots, 100$,

$$p_k = \exp(-\lambda - \mu k - 1),$$

which we can rewrite simply as

$$p_k = Cr^k,$$

where $C = e^{-\lambda-1}$ and $r = e^{\mu}$. We then solve the system below:

$$\begin{aligned} C\sum_{k=0}^{100} r^k &= 1 \\ C\sum_{k=0}^{100} kr^k &= a. \end{aligned}$$

This reduces to finding the roots of the 100-degree polynomial

$$(a-100)r^{100} + (a-99)r^{99} + \cdots + (a-1)r + a = 0.$$

Note that for $a \in \{1, 2, \ldots, 99\}$, the coefficients of this polynomial, placed in the order of its descending powers as above, have only one sign change, between coefficients $a-(a+1)$ and $a-(a-1)$. Then, by Descartes' rule of signs, the polynomial has exactly one real root. (Note that if $a \in \{0, 100\}$, the polynomial has no positive real roots. This means that this maximisation problem will not give us answers if these are our beliefs and so we cannot apply the model.) Having found r, we find the value of C. We can then calculate the maximum entropy distribution for each value of a between 1 and 99 (or rather: such distribution for $\hat{b}$ having mean equal to a), and having done this, find the value of x that maximises the expected gain from the bet. This is best done numerically. The values obtained are plotted in Figure 4.2. The expectation being maximised here is the following:

$$\begin{aligned}\mathbb{E}_A =& [-x(1-a) + (1-x)a]P(\hat{b} < x) + [x(1-a) + (x-1)a]P(\hat{b} > x) \\ &+ [-x(1-a) + (1-x)a]P(\textit{betting on } E|\hat{b} = x) \\ &+ [x(1-a) + (x-1)a]P(\textit{betting on } \neg E|\hat{b} = x).\end{aligned}$$

If it happens that $b = x$, the bookie could decide to swing the bet either way, since maximising his expected gain cannot guide him (recall that his expected gain was either $b - x$ or $x - b$). Having no information whatsoever about what would happen if $b = x$, an agent might invoke the maximum entropy principle again, to get

$$P(\textit{positive stake}|\hat{b} = x) = P(\textit{negative stake}|\hat{b} = x),$$

therefore reducing the expectation to:

$$\begin{aligned}\mathbb{E}_A &= [-x(1-a) + (1-x)a]P(\hat{b} < x) + [x(1-a) + (x-1)a]P(\hat{b} > x) \\ &= (a-x)(F(x) - p_x) + (x-a)(1-F(x)) \\ &= (x-a)(1 + p_x - 2F(x)).\end{aligned}$$

Chapter 5

Countable additivity and objective Bayesianism

DeFinetti's opposition to the adoption of countable additivity (CA) as an axiom of probability is one of the things he is best known for. Two of his main objections to CA (see Section 5.5) form the basis of the discussion below, but his own position, about which he wrote prolifically and consistently throughout his career, will not be focused on here. Instead, I will employ (what I take to be) a de Finettian philosophical methodology to propose a way to move the debate on CA forward in a broad sense, and attempt to make a specific contribution in that vein.

My specific contribution will be to propose reasons why two of the major approaches in *objective Bayesianism* (I will look at Edwin T. Jaynes and Jon Williamson) are justified in adopting CA as an axiom; these reasons are different from the ones they give themselves, which, I think, can be improved on. The broader point will be that a general solution to the debate on CA, one that satisfies all the participants in it, is impossible. I support this conclusion in Sections 5.3 and 5.5. What is possible, however, is finding case-by-case solutions: we may or may not want CA as an axiom in different approaches and in different uses of probability theory, and we may or may not be justified in adopting it. My discussion of objective Bayesianism below is an application of this idea. And my general methodology here is inspired by de Finetti in this sense: he takes seriously the fundamentals of what he takes probability to be, and what he expects from a formal model of it; this informs his mathematical choices throughout. When we get deeper into discussions of objective Bayesianism we will be well out of de Finetti's territory, and we will be applying this general idea to things he strongly disapproved of, but this method of relating fundamentals to formalisations will be the guide.

5.1 Introducing the debate on countable additivity

Philosophers cannot agree on whether the rule of Countable Additivity (CA) should be an axiom of probability. There is broad consensus on the fact that probabilities should lie between 0 and 1, and that if A and B are two incompatible events, then the probability of their *union* $A \cup B$, should be the sum of their individual probabilities: this rule is Finite Additivity (FA). CA is the extension of this property to countably infinite sequences of events. Although frequently used in much of modern probability theory, it remains philosophically contentious.

E.T. Jaynes, in Chapter 15 of his seminal 2003 book *Probability Theory: The Logic of Science*, attacks the problem in a way which is, to my knowledge, original to him and which has been passed over in the current philosophical debate about the principle.[1] Although his target audience might be mathematicians and other practitioners (as opposed to philosophers), I think a discussion of his position can bring a valuable contribution to the philosophical debate. Jaynes says the debate rests on an erroneous use of mathematical infinity by the authors who participate in it, and the claim is that his approach, in avoiding such mistakes, is not afflicted by the problem. I reconstruct from this the solution to the FA versus CA debate that emerges from the text, which is, quite simply, to avoid the mistaken use of mathematical infinity that Jaynes points out. I argue, though, that this solution fails: these alleged misuses of infinity seem to be independent of the question of additivity in probability. The spirit of the more

[1] The closest it goes to being mentioned are: Hájek (2011) refers the reader to Jaynes's sections 15.3-5 for a discussion of conglomerability; and in a recent paper Myrvold (2015) cites section 15.7 of Jaynes's book on the Borel-Kolmogorov paradox. The discussion on CA is in section 15.6.

general point he is making in the book, however, can be salvaged, and it can inspire a positive contribution to the philosophical debate about the principle. In particular, it can be fruitfully applied to a recent theory of objective Bayesianism, by Jon Williamson: I think Williamson's adoption of CA can be given a different and improved justification thanks to the arguments I put forward below.

I start, in Sections 5.2 and 5.3 by introducing the problem. Then I analyse Jaynes's proposal in Section 5.4, and suggest a new solution to the debate, in the spirit of Jaynes's work in Section 5.5. In the section that follows, 5.6, I focus on Williamson's work on the CA debate. My conclusions are in Section 5.7.

5.2 Mathematicians on Countable Additivity

Let P be a function which assigns, to a certain class of events, real numbers between 0 and 1. Then FA is the following requirement:

Definition 1 (Finite Additivity)**.** *Let A, B be mutually exclusive events, then:*

$$P(A \cup B) = P(A) + P(B). \tag{5.1}$$

Finite Additivity can be iterated to apply to any finite sequence of mutually exclusive events. Importantly, however, this does *not* mean that we could iterate FA a *countably infinite* number of times, with the rule still being valid. This would be a net strengthening of FA: it would be Countable Additivity. Here is a statement of the property:

Definition 2 (Countable Additivity)**.** *Let $\{A_n\}_{n=1}^{\infty}$ be an infinite sequence of mutually exclusive events, all of which have a well-defined probability value; then:*

$$P\left(\bigcup_{n=1}^{\infty} A_n\right) = \sum_{n=1}^{\infty} P(A_n).^2 \tag{5.2}$$

[2] If a measure is countably additive, then it is also finitely additive. Let P be a countably sdditive measure. We want to show that $P\left(\bigcup_{n=1}^{N} A_n\right) = \sum_{n=1}^{N} P(A_n)$, for $A_1, \ldots, A_n$ pairwise disjoint. We can extend this sequence by a countably infinite

CA allows for a powerful mathematical theory of integration and, while a theory of integration which uses only finitely additive measures is also fully developed[3], most mathematicians adopt CA and consider this justified by its highly desirable consequences (Bingham, 2010, pp. 3-4). For classic expositions that adopt the principle of CA see, for example, the following chronological progression, which is also interesting as an indication of the attitude of mathematicians on the matter: Andrey Kolmogorov writes (on the axiom of continuity, which he then immediately shows to be equivalent, together with the other probability axioms, to countable additivity):

> Since [the axiom of continuity] is essential for infinite fields of probability only, it is almost impossible to elucidate its empirical meaning [... as can be done for the finitary axioms]. For, in describing any observable random process we can obtain only finite fields of probability. Infinite fields of probability occur only as idealised models of real random processes. *We limit ourselves, arbitrarily, to only those models which satisfy [continuity].* This limitation as been found expedient in researches of the most diverse sort. (Kolmogorov, 1933/1956, p. 15, emphasis in original).

Paul Halmos writes the following when defining probability measures:

> The general condition of countable additivity is a further restriction on [probability functions]—a restriction without which modern probability theory could not function. It is a tenable point of view that our intuition demands infinite additivity just as much as finite additivity. At any rate, however, infinite additivity does not contradict our intuitive ideas, and the theory built on it is sufficiently far

sequence of empty sets: $A_{n+1}, A_{n+2}, \dots = \emptyset$. We now can write $P\left(\bigcup_{n=1}^{N} A_n\right) = P\left(\bigcup_{n=1}^{\infty} A_n\right) = \sum_{n=1}^{\infty} P(A_n) = P(A_1)+\dots+P(A_N)+P(\emptyset)+P(\emptyset)+\dots = \sum_{n=1}^{N} P(A_n)$. For an example of a probability function which is FA but not CA, see function Q below.

[3]See, for example Dunford & Schwartz (1958, Chapter 3), or de Finetti (1972, Chapter 6)

> developed to assert that the assumption is justified by its success. (Halmos, 1974, p. 187)

And more recently, Donald Cohn dedicates the following passage to the finite versus countable additivity question:

> Finite additivity might at first seem to be a more natural property than countable additivity. However, countably additive measures on the one hand seem to be sufficient for almost all applications and, on the other hand, support a much more powerful theory of integration than do finitely additive measures. Thus we will follow the usual practice and devote almost all of our attention to countably additive measures. (Cohn, 2013, p. 7)

Even though this succession of quotes makes compelling reading, the tone going from tentative (Kolmogorov) through bullish (Halmos) to accepting (Cohn's following of the "usual practice"), I don't mean the progression to be taken too seriously as 'good' history of mathematics—other views have certainly existed and exist. But the above are important texts in mathematical probability, and I take this sample to be representative of mathematicians' general attitude to the matter: countable additivity greatly increases the power of the theory of probability, and the bulk of the theory, in the way it is generally taught today, depends on it.

5.3 The philosophical status of Countable Additivity

Among many philosophers, however, the axiom of CA is contentious. All participants in the debate see the technical advantage of adopting the principle, but many feel this is not justification enough. The main contention, as far as this discussion is concerned, is that CA seems to have significant consequences when we use probability to model epistemic attitudes. These are such that an independent, not merely technical reason for taking CA as an axiom of probability is sought.

De Finetti has been the most famous and vocal critic of CA[4], and he is the main target of Jaynes's critique. The two main issues de Finetti found with CA are specified below in Section 5.5, and responded to in what I take to be a 'Jaynesian' spirit. For now, it will be enough to present the problem in the simplest form in which it arises, that of the *infinite lottery*.

A crucial reason why the matter of CA versus FA is not yet settled to this day is that there are powerful intuitions on either side of the debate, both of which are appealing, but mutually incompatible in the usual mathematical framework. It is in the context of the infinite lottery that Wenmackers & Horsten (2013) make this observation, and it seems to me that this conclusion could be broadened to encompass a lot of the discussion on CA, at least within the realm of epistemic probability. It seems that any number of arguments that support one side of the debate ultimately clash with those intuitions which favour the *other* side of the debate, and the former are unable to overpower the latter, thus leaving the debate open. It might be because these intuitions are more profound ideas on what probability is, or how it should be thought of.

The infinite lottery is a perfect test case for these intuitions.[5] In this thought experiment we imagine a lottery over all the natural numbers, each number representing a ticket. One, and only one, ticket will be picked. Suppose we attach a probability to each number in the infinite lottery. (We assume that probabilities will be between 0 and 1 and finite additivity, but not countable additivity: the point of the thought experiment is to test our intuitions about this very principle.) What should the probability of a given ticket winning be? Should it be possible to consider all numbers equally probable? Suppose we answer yes to the second question. Any positive real number is too big: however small we choose these positive probabilities, by summing them we will eventually, in a finite number of steps, achieve a number greater than 1. So, if we want to preserve the fact that all numbers are equally

[4]For other important contributions to this lively philosophical debate, see, amongst others, the following: Levi (1980), Seidenfeld & Schervish (1983), Kadane *et al.* (1986/1999), Kadane *et al.* (1996), Kelly (1996, Chapter 13), Howson (2009).

[5]I explored this also in my unpublished MSc thesis (Elliot (2014)), and some of the passages in this section are from or based on that work.

probable, they must all be assigned probability 0. This is fine if we apply FA only: nowhere does the principle state that an *infinite* union of exclusive events must have the same probability as the infinite sum of the probabilities of the single events. This assignment respects the following fundamental intuition, which I call I_{FA}:

I_{FA}: *We should be able to assign equal probability to all events, including in a countably infinite setting.*

While this is compelling, it is also clear that the solution of assigning 0 probability to all events, when their union has probability 1, is also counter-intuitive: we lose the idea that the total probability is the sum of its component parts; and we would have a union of probability-0 events making up a certain event. The intuition it contradicts can be expressed thus:

I_{CA}: *The probability of a union of events should be equal to the sum of the probabilities of the events that make up the union, including in a countably infinite setting.*

For a countably infinite number of probabilities to add up to 1, they must form a convergent series. This implies that the probabilities must form a sequence converging to 0. This in turn means that, whatever sequence we choose to adopt, we will always have the vast majority of the probability assigned to a finite set of numbers in the lottery. This contradicts the first intuition above. The two intuitions are incompatible in a countably infinite setting, and we must choose which one to adopt and which one to drop. One way to proceed, adopted by Bartha (2004) and Wenmackers & Horsten (2013) is to abandon the usual mathematical framework in which probability is studied, transfer to an analogous problem in non-standard analysis, and solve that problem. In non-standard analysis intuitions equivalent to the two above can be satisfied at the same time (Wenmackers & Horsten (2013)). Here I shall remain, as Jaynes and Williamson do, within the usual realms of standard analysis. I come back to the standard infinite lottery below. Next, I analyse Jaynes's contribution to the debate, and propose a new one, inspired by him.

5.4 Jaynes on Countable Additivity

Jaynes shows little patience for philosophical arguments of the sort sketched above, as he does for measure theory, which is the branch of contemporary mathematics in which probability is studied (this point is also noted in the reviews by Diaconis (2004) and Faris (2006)). Perhaps because of this, he dismisses the debate, arguing that CA must be adopted in order not to violate a principle of classical mathematics. According to Jaynes, there is a proper way of working with infinite objects, and an improper way. The proper way is this:

> Apply the ordinary processes of arithmetic and analysis only to expressions with a finite number n of terms. Then after the calculation is done, observe how the resulting finite expressions behave as the parameter n increases indefinitely (Jaynes, 2003, p. 452).

Jaynes's example of this methodology is how, by definition, we calculate infinite series: we do all the necessary work on the finite sums of up to n elements of the series first, and only then do we attempt to pass to the limit for $n \to \infty$ (Jaynes, 2003, p. 452). As Jaynes points out, it is easy to make mistakes when attempting to work directly on infinite objects. There is no doubt that this is sound mathematical advice. Unfortunately, however, it is not immediately clear how to apply this principle to the case of FA versus CA, and Jaynes's use of the principle is not always transparent. Simply put, just by using probabilities which are merely finitely additive, we do not seem to generate any mistaken use of infinity.

The following passage by Jaynes is particularly significant:

> it is a trivial remark that our probabilities have 'finite additivity'. As $n \to \infty$ it seems rather innocuous to suppose that the sum rule goes in the limit into a sum over a countable number of terms, forming a convergent series; whereupon our probabilities would be called countably additive. Indeed [...] if this should ever fail to yield a convergent series we would conclude that the infinite limit does not

> make sense, and we would refuse to pass to the limit at all (Jaynes, 2003, p. 464).[6]

Jaynes writes that most accounts of probability theory do not respect the basic rules on how to handle mathematical infinity, which is why they then grapple with issues of finite versus countable additivity. Now, to solve the issue of FA versus CA probability we would need something like the following argument: *only if we do not operate correctly with mathematical infinity, then probability functions which are finitely, but not countably, additive, will arise.* But even in the absence of this claim, which is not explicitly made by Jaynes, a connection between specific uses of infinity and FA-probability would be a valuable insight. Unfortunately, I think this doesn't quite work. But what are these mistaken uses of mathematical infinity? I explore, then respond to, Jaynes's points in the following paragraphs.

The warning in the quoted passage above is an example of Jaynes's "finite-sets policy" on how to handle limits (Jaynes, 2003, p. 44). Note that in what follows, when I talk of infinite sets, I will always be referring to intervals on the real line which have an infinite Lebesgue measure. This seems to me the best understanding of Jaynes when he writes that supporters of finitely additive probability are "concerned with additivity over propositions about *intervals on infinite sets*" (Jaynes, 2003, p. 465, my emphasis). Jaynes then proceeds to consider an example of an interval function, in which the number of intervals and their Lebesgue measure are crucial. This discussion, which I examine below, constitutes the central part of his argument for the adoption of CA. In Section 5.4 below I discuss a different interpretation of what Jaynes could be understood to be arguing.

It seems that Jaynes, in these passages, considers two different kinds of limits. At one point (Jaynes, 2003, p. 464), he mentions the number of events, which becomes infinite in the limit; further on (Jaynes, 2003, p. 465), he mentions single events, which happen to be represented by infinite sets. The first limit regards the number of events, and the other the end-point of an interval on the real line,

[6] n here is the number of propositions treated; while Jaynes speaks of propositions, I will use the terms *proposition* and *event* interchangeably to mean elements of the domain of the probability functions treated.

if we take, as Jaynes does in this section, such intervals to represent our events. It is easy to see that these two limits *need not be* considered together: even just operating on the intervals of the real line, we might have events represented by a finite number of infinite intervals (say $\{(-\infty, 0], (0, +\infty)\}$), or we could have an infinite number of events represented by an infinite number of finite intervals (for example $\{\dots(-2, -1], (-1, 0], \dots\}$). I will thus examine them separately; the mistakes to avoid, on the basis of the above reasoning, are the following: *mistake* M_1: assigning probabilities directly to an infinite number of events, instead of starting from a finite number and observing the limit process; *mistake* M_2: assigning probabilities directly to infinite sets.

Avoiding mistake M_2

M_2 is at the same time both too strong. According to M_2, we should not assign probabilities to infinite sets directly. However, this is commonly done, without incurring any problems with countable additivity. To see this, we can take any cumulative distribution function G; then we consider the σ-algebra of sets generated on the set $\mathbb{R}$ by the sets $(\infty, c]$. We assign to each such left-infinite interval a probability as follows: $P((\infty, c]) = G(c)$. This probability measure is countably additive, and yet it clearly commits mistake M_2. (See also Kolmogorov 1933/1956, 18-19, for this construction.) Note that this is not the only way to define cumulative distribution functions: they can also be defined by a limit process that would not commit M_2. However, all that is important for my purposes is that it is *possible*, and it is commonly done, to work directly with infinite sets, without worrying about which limiting procedure gave us the set, and yet without running into contradiction or encountering a failure of CA. So M_2 is too strong, because it excludes common, unproblematic uses of probability theory. M_2 is also too weak, because avoiding it does not guarantee that we will avoid merely finitely additive probability functions. Here is an example: we define a function Q which avoids mistake M_2 and yet is finitely, but not countably, additive. Let A be the algebra of sets of all finite disjoint unions of intervals $[a, b) \subseteq [0, 1)$, with $a < b$. For $I, J \in A$, we define Q as follows[7]:

$$Q(I) = \begin{cases} 1 & \text{if there is a } \ y \in [0,1] \text{ such that } [y,1) \subset I \\ 0 & \text{otherwise} \end{cases}$$

This function does not assign probabilities to infinite sets, avoiding mistake M_2. Q is finitely additive: take $I, J \in A$, with $I \cap J = \emptyset$. Without loss of generality, suppose $\exists y$ such that $[y, 1) \subseteq I$, so that $Q(I) = 1$. This means that there can be no y' for J such that $[y', 1) \subseteq J$ (since I and J are disjoint), so $Q(J) = 0$. Because $\exists y$ such that $[y, 1) \subseteq I \cup J$, $Q(I \cup J) = 1$. Thus $Q(I \cup J) = Q(I) + Q(J) = 1$. Now suppose $\nexists y$ such that $[y, 1) \subseteq I$ and $\nexists y'$ such that $[y', 1) \subseteq J$. Then $\nexists y''$ such that $[y'', 1) \subseteq I \cup J$, and so $Q(I \cup J) = Q(I) + Q(J) = 0$. However, Q is not countably additive. To see this, take the intervals $\left\{A_0 = \left[0, \frac{1}{2}\right), A_1 = \left[\frac{1}{2}, \frac{3}{4}\right), \ldots, A_k = \left[1 - 2^{-k}, 1 - 2^{-(k+1)}\right), \ldots\right\}$, and note that $Q\left(\bigcup_{i=0}^{\infty} A_i\right) = Q([0, 1)) = 1$, but $\sum_{i=0}^{\infty} Q(A_i) = 0$.

This shows that avoiding the assignment of probability to infinite sets (which I called mistake M_2) is neither necessary nor sufficient for avoiding merely finitely additive probability functions.

Avoiding mistake M_1

According to Jaynes, the other mistake to avoid is M_1: to assign probability directly to an infinite number of events, instead of starting from a finite number of events and then observing the limiting process. This is more interesting, but it is not obvious what constitutes avoiding mistake M_1. Function Q above, for example, gives a rule on how to assign probabilities to intervals in $[0, 1)$, so implicitly it assigns probabilities to an infinite number of intervals. Note that Jaynes himself defines a measure which he describes as correct in the following way: he starts from a continuous, monotonic increasing cumulative density function G, then defines a probability function F (my notation), which is countably additive, in the usual way: $F((a, b)) = G(b) - G(a)$. This is also a *rule*, which applies to an infinite number of intervals at once. However, it seems more charitable to go with a stricter interpretation of

[7] I am grateful to Jan Sprenger for suggesting this example.

the avoidance of M_1 which Jaynes proposes (Jaynes, 2003, p. 44,654), even if Jaynes himself does not always seem to follow it. I use this interpretation because it rules out function Q above as a counterexample to Jaynes's argument. This goes as follows: we always start from a finite number of mutually incompatible events, say $\{A_1, \ldots, A_n\}$, and assign a probability function P_n only to those events, and to those which arise from the Boolean algebra (or algebra of sets, if we take the A_i to be sets) that those events generate. Then, if the problem requires it, we observe how this probability behaves as the number of events increase in the limit $n \to \infty$. Clearly, avoiding this version of M_1 is not necessary in order to avoid merely finitely additive probabilities: F commits this version of mistake M_1 and yet is countably additive. But could it be sufficient? At least it rules out the problematic function Q. As it turns out, it is not sufficient either: the problem is that the limit $\lim_{n\to\infty} P_n$ may not exist, or, worse, it may exist but not be countably additive. As seen above, Jaynes says that if additivity fails in the limit, i.e. if the probability measure, in the limit, is not countably additive, "we would conclude that the infinite limit does not make sense, and we would refuse to pass to the limit at all". But this begs the question. Suppose we start from the events $\{1, 2, \ldots, n\}$, each of which is assigned equal probability $P_n(i) = \frac{1}{n}$, for $i \in \{1, 2, \ldots, n\}$. This is well defined and finitely additive for each finite case, and the limiting procedure, with which we extend this measure to apply to all the natural numbers, is well defined too: $\lim_{n\to\infty} P_n = \lim_{n\to\infty} \frac{1}{n} = 0$. This respects all of Jaynes's warnings, but the resulting limiting measure is not countably additive: each natural number is assigned probability 0, but their countable union must be assigned probability 1.[8] To say that the limit does not make sense begs the question: Jaynes says that merely FA probabilities arise from limits that do not make sense, but the only way in which this limit fails to make sense is that it is not countably additive.

The foregoing discussion shows that Jaynes's warning to avoid the mistakes I labelled M_1 and M_2 are neither necessary nor sufficient for avoiding merely finitely additive probability functions. We can commit

[8]Note, however, that while this function exists, it is not trivial to define, if we require it to be defined for all subsets of $\mathbb{N}$: see Kadane & O'Hagan (1995) and my discussion below.

neither M_1 nor M_2 and end up with a finitely additive probability; and we can commit either M_1 or M_2 (or both) and still have a countably additive probability. There is, however, another way in which Jaynes could be seen to be arguing, which is immune to some of the criticisms levelled above. This has to do with what Jaynes calls the right order of operations: we discuss additivity first, and only then pass to the limit. I discuss this next.

Invoking continuity

Although Jaynes, in his section on countable additivity, considers intervals infinite in number or with infinite Lebesgue measure, it may be that this is merely to illustrate a broader point. In this section I argue that this argument by Jaynes would be an appeal to the rule of *continuity*, which, in the presence of the other probability axioms, is equivalent to countable additivity; in fact, it is in this version that the rule is first presented in Kolmogorov (1933/1956). The fact that the continuity and CA are equivalent is not a problem in itself, since we will need something at least as strong as CA in order to derive it. But Jaynes is wrong in affirming that a function which does not respect continuity represents a mathematical error, and his argument amounts to stating that probability functions *should* respect continuity (and hence CA). Since he offers nothing more, and since there is nothing mathematically wrong with functions that do *not* respect continuity, the door remains open to a position such as de Finetti's, which I describe below in Section 5.5: some probability functions are *continuous*, others not; the first are a special case, but not the only admissible ones. Jaynes (along with many other authors) is opposed to this, and so another motivation for adopting CA, and thus excluding functions which are merely finitely additive, must be given. I propose one such motivation below. Before this, I substantiate my claim that this second interpretation of Jaynes's argument amounts to an appeal to continuity.

The broader point that Jaynes might be orienting towards is that when assigning a probability to a set which is expressible as a countably infinite union of it subsets, we should not be able to do this independently of the probabilities of those subsets. In particular, the probability of the union-set should result from a limit process involving the probabilities of the constituent sets. Failing to do so would be failing to respect the proper order of operations (Jaynes, 2003, pp. 464-466). Here, as I explain above, Jaynes tries to draw a parallel between the correct order of finding the limit of a series and countable additivity. He writes that we should treat the question of additivity first, and only then pass to the limit. How this works in practice, however, is that Jaynes requires that the "sum rule [go] in the limit into a sum over a countable number of terms". For a function P, FA (the sum rule) is the following property, for $A_1, \ldots, A_n$ pairwise disjoint:

$$P\left(\bigcup_{i=0}^{n} A_i\right) = \sum_{i=0}^{n} P(A_n).$$

Taking this rule in the limit means the following:

$$\lim_{n\to\infty} P\left(\bigcup_{i=0}^{n} A_i\right) = \lim_{n\to\infty} \sum_{i=0}^{n} P(A_i).$$

The right hand side of this equation is, by definition, $\sum_{i=0}^{\infty} P(A_i)$. Remember, however, that CA is a property of this expression: $P\left(\bigcup_{i=0}^{\infty} A_i\right)$. So, to obtain CA, we need the following property on the left hand side of Equation 5.4:

$$\lim_{n\to\infty} P\left(\bigcup_{i=0}^{n} A_i\right) = P\left(\bigcup_{i=0}^{\infty} A_i\right).$$

This property is called *continuity*, and it is possible to show that, if taken together with the other Kolmogorov axioms of probability, it is equivalent to CA. So Jaynes is wrong not to see a "substantive issue" here. The issue of whether, for a given probability measure, the rule of FA goes into the limit to become CA, is just as substantive as the issue of whether such probability measure is countably additive: the two matters are equivalent.

To sum up, in this section I have argued that Jaynes's arguments for the adoption of CA as an axiom of probability, seem to fail. Nonetheless, there are some valuable observations to be made on the issue, in what I take to be the spirit of his book. I expand on this below.

5.5 A model for common sense and ‘adequate’ operations

There are two positive contributions on the CA versus FA debate which I wish to take from Jaynes. They do not come from his explicit treatment of the topic (which, I argued, is flawed), but are from, or in the spirit of, other arguments that are put forward in his book. The defence of these contributions is mine, and relies partly on facts about the Axiom of Choice and the constructibility of mathematical objects.

Here are the two main problems with CA that de Finetti highlights: firstly, it is always possible to extend a coherent FA-probability assignment on a collection S, to a coherent assignment on an arbitrary algebra of sets $A \supset S$ (de Finetti, 1972, p. 78). If we do the same operation with a CA probability, there can exist elements (called *non-measurable sets*) for which for such CA probability is not defined.[9] Because in applying CA we can get non-measurable sets, it seems that the rule of CA is forbidding us from assigning probability to some events—perhaps an overreach for what is supposed to be a rule used only for technical convenience (de Finetti, 1972, p. 75). I call this issue A.

Secondly, and as a consequence of the preceding point, there must exist a coherent merely FA probability function which assigns equal probability to all natural numbers, thus respecting the intuition I_{FA} outlined above, *we should be able to assign equal probability to all events, including in an infinite setting*, and which also extends coherently to the power set of $\mathbb{N}$. CA cannot even get off the ground in accommodating intuition I_{FA}, let alone include this in a globally coherent probability function. Again, it might be seen as an overreach, for a merely technical rule such as CA, to rule out seemingly sound epistemological states such as that expressed in this intuition (de Finetti, 1974/1990, pp. 118-128). I call this issue B.

I argue that Jaynes doesn’t need to worry about these issues. As I suggested in Section 5.3, and will expand on below, I don’t think it is possible to make these issues go away, but it is possible to give a

[9] For example, the Vitali sets are members of the power set of $[0, 1] \subset \mathbb{R}$ for which the Lebesgue measure is not defined.

principled justification for adopting CA, if we consider carefully what Jaynes sets out to do in his treatment of probability.

Jaynes need not worry about issue A: he studies probability theory as a "model for common sense", which can hopefully also act as its "powerful extension" (Jaynes, 2003, p. 7). Inspired by Cox (1961), his theory is an attempt at finding quantitative rules for plausible reasoning, of the kind we need to do in practical and scientific problems (Jaynes, 2003, pp. 3-4). Now, because the proof of existence of non-measurable sets requires the Axiom of Choice, an explicit, constructible example of them cannot be given (Fraenkel *et al.*, 1973, p. 70). Given this, it seems reasonable to say that a non-measurable set of this sort will never appear in a practical problem dealt with the way Jaynes deals with it: we will not have a scientific hypothesis, say, which necessitates being represented by a non-measurable set. So a Jaynesian account can escape worry A by saying that it does not apply to what they want to do.

Issue B is harder to avoid, and I think Jaynes, and whoever else wants to apply CA, must simply bite the bullet: given CA (and within standard analysis) it is impossible to model an agent that has an equal credence in a countable infinity of mutually exclusive and exhaustive propositions. There is no way around this. But Jaynes can give a principled reason for deciding to bite this bullet. Of course, it boils down to intuition I_{CA} that *the probability of a union of events should be equal to the sum of the probabilities of the events that make up the union, including in an infinite setting*, but it comes from a different and more informative angle. It comes from a principle which says something like this: I_{CAJ}: *if we know the probabilities of the elementary events, we should be able to know the probability of the compound events they form.* This is close, although perhaps in structure alone, to the principle of *compositionality*: this is the idea that "the meaning of a complex expression is fully determined by its structure and the meanings of its constituents" Szabó (2017). And, closer to home, it is exactly what follows from (Boole, 1854/1958, p. 10), when he points out that the assignment of probabilities to compound propositions, given the probabilities of the elementary propositions that make them up, is one of the problems his theory of probability allows us to solve. I think we should extend this principle to the countable setting.

Before I discuss the application and justification of this principle, I will note four preliminary things.

Firstly[10], the principle I_{CAJ}, that if we know the probabilities of the basic events we should know the probabilities of all the compound events they can form, only works in lotteries or equivalent cases in which the generators (or basic events) of the Boolean algebra are all mutually incompatible. It can be argued, however, that these have no real claim to being more fundamental than other sets of generators. The set of 'lottery tickets' may have the appearance of being a privileged set of generators, and they are by construction mutually independent, but in fact they occupy no special role. A different set of generators can give rise to the same Boolean algebra, with the 'tickets' now appearing as compound events. If these new generators are not all mutually independent, however, I_{CAJ} will fail. Here is an example. Let the generators of Boolean algebra A be $\{\{1\},\{2\},\{3\},\{4\}\}$. These are the classic lottery tickets: mutually incompatible. If we know the probabilities of each lottery ticket, we can compute the probability of any combination of them. However, A can also be generated by the following set: $\{\{1,2,3\},\{2\},\{3,4\}\}$. In this case, knowing the probabilities of *these* basic events, plus the application of the union and intersection rules for probability, does not give us the probabilities of all compound events. For example, $P(\{4\}) = P(\{3,4\} \setminus \{1,2,3\})$, which is not something we are able to calculate. While the latter might seem purposefully muddled up, a simple relabelling changes this perspective. Let $\{A = \{1,2,3\}, B = \{2\}, C = \{3,4\}\}$. Now the second set of generators is $\{A, B, C\}$, while the first is $\{A \setminus (B \cup C),\ B,\ A \cap C,\ C \setminus A\}$. Now, the latter set is supposed to be privileged, because its generators are mutually incompatible. But this relabelling doesn't make clear why this, and not the more 'basic'-looking set of generators should be chosen as fundamental, such that principle I_{CAJ} should apply. This suggests that I_{CAJ} is applied somewhat arbitrarily.

This point helps in delimiting the scope of the principle I_{CAJ}: it will only be applied to sets of incompatible events or propositions, and

[10] I owe this point to Alberto Mura.

the Boolean algebra they go on to generate. These are the cases to which finite and countable additivity apply, where for incompatible events the probability of their union is the sum of their probabilities. These cases have a good claim to being 'basic' in the cases I treat here. It is not a general principle, however, that whichever set of generators of a Boolean algebra appears to be most basic should have this special compositionality property.

Secondly, I_{CAJ} is not explicitly applied by Jaynes to countable additivity. Jaynes, as I specify and criticise above, thinks that rules on finite cases plus a correct application of mathematics will result in CA. Necessarily, then, this will be an extension of his work in this direction. Jaynes sets out a clear and well-reasoned role for formal models, and he positions and delimits it with respect to human reasoning. I think this is valuable, and the idea in this chapter is to work within that framework to try and ground the principle of CA.

Thirdly, here the principle is only applied to finitely or countably many propositions. This is all we encounter in a Jaynes-like approach. The principle does not work in cases with an uncountable infinity of basic events, but that is not a problem here. [11] Jaynes writes that finitely many propositions is "all we ever need in practice" (Jaynes, 2003, p. 107) and that "limiting our basic theory to finite sets of propositions has not in any way hindered our ability to deal with continuous probability distributions" (Jaynes, 2003, p. 108). For a continuous distribution f, for example, he considers events such as $F' \equiv (f \leq q), F'' \equiv (f > q)$. But even when a continuous distribution might be mathematically handy for modelling a certain problem, this is just a convenient intermediate step: the fundamental propositions that give a true description of the problem are finite in number (Jaynes, 2003, pp. 109-110). Further on (Jaynes, 2003, p. 466), Jaynes is happy to consider as properly formed a probability function with a

[11] Principle I_{CAJ} does not work, for example, for $[0,1] \subset \mathbb{R}$ and the Lebesgue measure. Every individual point $x \in [0,1]$ has measure 0, but the union of uncountably many points can, of course, have positive Lebesgue measure: so knowing the probabilities of the fundamental events is not enough to know the probability of the event they make up as a union. If we wanted I_{CAJ} to be true also in uncountable settings we would be imposing what de Finetti calls "perfect additivity" (de Finetti, 1974/1990, p. 118).

countably infinite number of basic propositions, obtained by starting from $n < \infty$ propositions, and then letting $n \to \infty$.

Finally, I_{CAJ} is at least as strong as CA. This is an unavoidable feature for any approach that seeks to derive CA from another principle; when we assume this stronger principle, we are already assuming the seeds of what we want to obtain. All participants in the debate acknowledge this circularity, which is present in de Finetti's camp too. The aim, in this endeavour, is to try and find a principle which might be more intuitively accessible, or has some kind of independent feasibility, or that we might believe for the right reasons, and that will have, as one of its consequences, the desired axiom.

The application of the principle I_{CAJ} works as follows: Jaynes (2003, p. 35) writes that

> just as conjunction and negation are an adequate set of operations for deductive logic, the [...] product and sum rules are an adequate set for plausible inference, in the following sense. Whenever the background information is enough to determine the plausibilities of the basic conjunctions, our rules are adequate to determine the plausibility of every proposition in the Boolean algebra generated by [propositions] $\{A_1, \ldots, A_n\}$.[12]

Strikingly, this is not true for a merely FA probability measure defined on $\mathbb{N}$—the natural extension, in the limit, of a probability measure defined on a Boolean algebra such as the one in the quote. In this case, we can know what the FA measure assigns to each mutually exclusive proposition $\{A_1, A_2, \ldots\}$, but still have no way of knowing what the measure assigns to each combination of such events, if the only rules we can use are what Jaynes calls the *product rule* (the basic property of a probability function P: for events A, B, $P(AB) = P(A)P(B|A) = P(B)P(A|B)$) and the *sum rule* (finite additivity).

[12] The property of operators in deductive logic Jaynes refers to is usually called *functional completeness*: through conjunction and negation all other logical operators can be expressed; it is not obvious that Jaynes's "adequacy" property in probability is a natural parallel, but this is unimportant for the present argument.

Why is this? As Kadane & O'Hagan (1995) write[13], suppose our merely FA probability measure assigns 0 to each natural number; then this tells us, by Finite Additivity, that finite unions of numbers must have probability 0; again by FA, co-finite unions must have probability 1.[14] But, for example, the probability of the set of even numbers is not determined by the sum and product rule of probability: this set is neither finite, nor does it have a finite complement. If we do not have CA, the probability of the set of even numbers does not need to be equal to the sum of the probability of each even number, which would be just 0; in fact, the value of this probability is left undefined by the rules adopted. Therefore, the sum and product rule of probability, in Jaynes's terms, are *no longer adequate* operations for plausible reasoning. If we had allowed countable additivity, on the other hand, the probability of any subset of $\mathbb{N}$ would have been automatically defined by the sums of the probability of its constituent incompatible events (Kadane & O'Hagan, 1995, pp. 626-627).

In a sense, then, by allowing the number of terms of the set $\{A_1, \dots, A_n\}$ to grow to a countable infinity, without strengthening the rule of FA to that of CA, we have radically changed the nature of the problem. The rules that were painstakingly found to be necessary and, in the sense defined above, sufficient, for plausible reasoning are not sufficient any longer. The most obvious cure is to adopt CA: this is the new *adequate* rule for cases in which we have a countable infinity of events. If we do not, and insist on merely FA probability, then there is, in fact, *no rule* that can give explicitly what Jaynes requires: namely, that when we know the probability of all events $\{1, 2, 3, \dots\}$, we have an explicit probability value for every member of the σ-algebra of sets they generate. This, again, involves the Axiom of Choice: because the existence of such FA-functions can only be proved using this axiom, an

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[13] Kadane and O'Hagan treat probability distributions over the natural numbers: events are represented by the sets $\{1\}, \{2\}, \dots,$ instead of propositions $\{A_1, A_2, \dots\}$, but the argument is identical

[14] We can see this easily as follows: if $P(1) = P(2) = \dots = P(n) = 0$, $P\left(\bigcup_{i=1}^{n} i\right) = P(1) + \dots + P(n) = 0$. For the second remark, see the following: $P\left(\bigcup_{i=1}^{n} i\right) = 0$, but $P\left(\bigcup_{i=1}^{n} i\right) + P\left(\bigcup_{i=n+1}^{\infty} i\right) = 1$ by FA, so $P\left(\bigcup_{i=n+1}^{\infty} i\right) = 1$.

explicit example, giving its construction for every subset of the natural numbers, cannot be given (Lauwers (2010)).

I now tackle some further issues that the choice of motivating CA by I_{CAJ} carries with it, and some replies available to a supporter of having only FA as an axiom of probability; answering these will add some depth to the position I defend here. Now, remember that having only FA as an axiom of probability does not rule out countably additive distributions. So a supporter of having only FA as an axiom could point out that the result in Lauwers (2010) is true for distributions which have FA but *not* CA; but if we want a model in which we know the probabilities of all subsets in a distribution over the natural numbers, then we will adopt a CA-distribution. De Finetti's approach would clearly allow this. Countably additive probability distributions, de Finetti (1972, p. 121) thinks, should be considered as constituting a special class, but not be exhaustive of the concept of mathematical probability. But the uniform distribution over the natural numbers that de Finetti allows is exactly of the kind that has FA but not CA, and it is to these functions that Lauwers' result applies. By adopting the weaker FA as an axiom we have a larger set of functions which we call probabilities. But in that added part, the functions which respect FA but not CA, are all those functions that cannot give an explicit value for each combination of the basic elements of the sample spaces to which they are applied. Hence this could be seen as a dubious gain. It is still to be argued why these functions should be excluded, however.

In particular, we might wonder why we need to know the probability of every possible combination of the basic events that we know the probability of. This might be seen as too demanding. By adding certain assumptions we can keep FA and, for example, salvage the intuition that the evens and odds have probability $1/2$ and that the set of the multiples of a number k has probability $1/k$ (see Wenmackers & Horsten (2013, p. 42); these probability functions being merely finitely additive, Lauwers's result applies to them.) So we can have merely-FA probabilities and also explicit results for some important sets of naturals. Further support for the idea that requirement I_{CAJ} might be too demanding seems to come from Jaynes himself: although the sum and product rule for probability are enough to give values for the entire Boolean algebra generated by our basic events, this is “almost always more than we need in a real application”, where a “small part of the Boolean algebra [. . .] is of concern to us” (Jaynes, 2003, p. 35). The pragmatic aspect of Jaynes’s thought was invoked above, and it could be invoked again. I think, however, that this case is subtly, but importantly, different from the above ones; I think it right to insist on the full demands of I_{CAJ}.

As I mention above, Jaynes (2003, p. 7) writes that probability is a “mathematical model for common sense”, and that any such model is successful if it acts as a “powerful extension of common sense in some field of application. Within this field, it enables us to solve problems of inference which are so involved in complicated detail that we would never attempt to solve them without its help.” This is a foundational idea, in Jaynes, of what probability should be. It is given before he starts the derivation of the formal rules of probability, and I think it is very useful to keep it in mind during this discussion. In particular, I think the idea is to be applied as follows: if our theory of probability is to act as an extension of common sense, then it should be able to tell us the probability of all sets of which we can give an explicit description of, which we understand and where we know the probabilities of the elements that make them up. This extends what we know to something that might not have been obvious. This demands the application of CA in the case of the probability distribution over the natural numbers. At the same time, it rules out merely finitely additive probabilities because, instead of extending our common sense, they limit it. We need assumptions additional to the probability axioms up to FA to get any sort of result on sets such as the evens and the odds, and however many assumptions we add, there will be subsets of the natural numbers for which we can give no unique explicit result (for some examples of possible assumptions and results which follow see Kadane & O’Hagan (1995)). Would we encounter these sorts of subsets in practice? We may or we may not, yet these cases are conceptually different from having a hypothesis modelled by a non-constructible set. The latter will never be encountered in practice; the former might, and there is no reason why we should not be able to handle them.

Note also that I am not saying that our formal model should produce values identical to whatever our intuition demands. Intuitions can be informed and modified by a successful formal model (Jaynes says we should "educate" our intuition (Jaynes, 2003, p. 472, 486)), but this case is different: where our intuition needs guiding, the formal model can give no answers at all.

This argument would not persuade all writers on probability: de Finetti, in particular, thought that the formal theory should not inform the content of our intuition in any way; apart from enforcing probabilistic coherence, it should leave an agent maximal freedom in how to distribute her degrees of belief. But Jaynes, as is highlighted by the comments on educating our intuition, has different ends, and allowing every possible coherent assignment of probability need not be a high priority. A formal model for common sense, for Jaynes, must be able to extend it, and so, as a minimum, it should be able to give us results for things we understand and have complete information about. Because of this, we must adopt CA; and if our intuition says we should be able to adopt a uniform distribution over the natural numbers, then, Jaynes might say, we should educate our intuition to be otherwise.

To sum up the above: a follower of Jaynes need not worry about issue A because non-measurable sets will not figure in her use of probability; and she must bite the bullet on issue B, but with good reason: she is guided by the principle I_{CAJ}: *if we know the probabilities of the elementary events, we should be able to know the probability of the compound events they form.* Without CA this is impossible, and so she should adopt CA. In the current setting, this principle does the same work as intuition I_{CA}, but might be more easily acceptable.

Now, issues A and B are well-known arguments in favour of FA, and because they express powerful intuitions directly (namely I_{FA} and related ones), it is not possible to refute them without denying the underlying intuitions. If we try to counter these issues by simply invoking intuition I_{CA} we will most likely be met by simple rejection. This clash of intuitions seems to be at the heart of the deadlock in the existing debates on the principle of CA. This is why I do not attempt to deny either I_{CA} or I_{FA}, or ideas which are related to them. Instead, my strategy is to argue that the motivation behind Jaynes's objective Bayesianism speaks in favour of CA—or at least of not worrying too much about the negative consequences of adopting CA. Note that this might not be true in other approaches: the various consequences of adopting FA or CA will have different weights according to what we take probability to be, or to what we wish to do with it. But given the principles guiding Jaynes's framework and his intended use of probability, I think, given my arguments above, that he is justified in using CA.

I think the arguments in this section improve Jaynes's account and can be useful for other accounts either similar to or inspired by it. In particular, they can be employed in the work of another well-known objective Bayesian, Jon Williamson. The final part of this chapter is dedicated to a brief analysis of his contribution to the FA vs CA debate, and a suggestion to substitute parts of his argument with the ideas defended in this section.

5.6 Williamson on Countable Additivity

Williamson (2010a, pp. 31-38) seeks a good motivating argument for the fact that degrees of belief should be probabilities, the principle known as *probabilism*. He points out that there are two ways of doing this. The first is an argument "by derivation": this kind of argument "proceeds by making some assumptions [...] and then showing that [probabilism] follows by the laws of logic" (Williamson, 2010a, p. 31). This was pioneered by Cox (1961): in a famous derivation, he attempts to show that the usual rules of probability (up to FA but excluding CA) follow from two qualitative rules for plausible reasoning. Jaynes adapts Cox's derivation, as does Paris (1994), who Williamson quotes.

The second kind of argument is by interpretation (Williamson, 2010a, p. 31): we "interpret, or explicate, the terms under consideration in a plausible way, and then show that under such an interpretation the norm must hold". The classic example of the second kind of argument is the interpretation of degrees of beliefs as betting quotients. The Dutch Book theorem is about betting quotients, and it says that if (and only if) a set of betting quotients respect the probability axioms, then there does not exist a combination of buying or selling these bets that results in a certain loss (or gain). In the Dutch Book argument for probabilism, degrees of belief are interpreted as betting quotients, and the result then is interpreted as saying that if (and only if) an agent has a set of degrees of belief that respect the probability axioms, then she is not open to accepting system of bets that can bring her a certain loss. This means that degrees of belief should be probabilities, if the agent does not wish to be open to certain losses. Williamson prefers the second kind of argument, and this is just as well: the argument by derivation he cites only goes as far as motivating FA; there is no extension to it in the literature that successfully motivates CA too, nor is one apparent to this author. He thus turns to the argument by interpretation, which is a Dutch Book argument. This, he writes, "has the means to convince a sceptic", contrary to arguments by derivation, which only have "the potential to convince someone with no prior opinion on the conclusion in question" (Williamson, 2010a, p. 32). In Chapter 4 I argued that the relation between betting prices and degrees of belief is not a necessary one, and should be taken as definitional of the latter concept. I also noted, however, in Section 4.4, that Williamson's objective Bayesianism emerges as more entitled than other approaches, including de Finetti's, to take for granted a close association between betting prices and degrees of belief. This is because for him the relation is an explication, so the former might be allowed to be numerically different from the latter. What is more, with a few additional assumptions, the difference is not very great. A full defence of this idea is beyond the scope of the current work, but I consider the arguments of Section 4.4 grounds enough to grant the relation between betting prices and degrees of belief Williamson claims, at least for the sake of the current argument.

Here is a sketch of one direction of Williamson (1999) Dutch Book argument for CA. We are back in the infinite lottery, and we call event 'n' that event that occurs if ticket number n wins. When we place a bet on n we pay a portion of the stake S on offer, and win the stake S if the event occurs. The portion of the stake that we put down in order to play is $P(n) \cdot S$, where $P(n)$ is our *betting quotient*, or *betting price*, for the bet on event n: the betting quotient is interpreted as our degree of belief in n occurring. The peculiarity of the set-up is that we don't know how large S is, and it could also be negative: in other words, we don't know if we are betting *for* the n winning or *against* this. (This device is meant to force us to declare betting quotients that reflect our sincere credence in the probability of n winning.) In the Dutch Book for CA, we place a bet on each ticket simultaneously, and each bet has the same stake S. Thus, if we pay $S\sum_{n=1}^{\infty} P(n)$ for this combination of bets, the following is true: if (and only if) $\sum_{n=1}^{\infty} P(n) = 1$, it is impossible for us to lose out *whatever happens*, i.e. whatever ticket turns out to be the winning one. An unfair system of bets which leads to a net loss whatever happens is called a Dutch Book; the idea behind the argument is that a rational agent should avoid the *possibility* of being subjected to a Dutch Book. Only countably additive betting prices guarantee this, Williamson points out. Once we interpret degrees of belief as betting prices, we have our result: the former should be countably additive because the latter must be.

This argument works, but note the following. In a passage by de Finetti which unfortunately seems to have been mistranslated in English[15], the Italian author points out that an argument such as the above assumes that we are happy to pay, for a countable collection of bets, the countable sum of their individual prices. Indeed note that we are paying $S\sum_{n=1}^{\infty} P(n)$ for the combined bet on all tickets of the lottery

[15]De Finetti's argument, re-translated from the Italian, is as follows (this is a comment on a proof such as the above, where it is claimed that if our betting quotients do not abide by CA, then we are open to a certain loss):

> But this is a kind of a vicious circle, because only if I knew complete additivity to be valid could I think of extending the notion of 'fair combination of bets' to combinations of infinite bets, and of basing them on the series of the betting odds (de Finetti, n.d., p. 12) [emphasis as in the original, which follows: *Un motivo che tenderebbe ad avvalorare l'additività completa: se le probabilità p_n hanno somma $p < 1$, stipulando tutte le infinite scommesse posso ricevere in ogni caso 1 pagando p, e quindi avrei un'incongruenza. Ma è un po' un circolo vizioso, perchè solo se sapessi valida l'additività completa potrei pensare di estendere la nozione di 'combinazione di scommesse equa' a combinazioni di infinite scommesse, e di basarle sulla serie delle quote di scommessa.*]

Howson (2008) studies this argument but is puzzled by it, as, he writes, are many authors before him. In the English version quoted by Howson and others, the word *serie* (series) is wrongly translated as 'sequence' simultaneously. In order to prove Countable Additivity of degrees of belief, we are assuming Countable Additivity of betting prices. This *argument by interpretation* has elements of an *argument by derivation.*

Now, this is perfectly fine if we are happy with the general principle from which we are deriving our desired result, but compare these two principles: I_{CAJ}: *if we know the probabilities of the elementary events, we should be able to know the probability of the compound events they form.* I_{CAW}: *betting prices should respect the rule of Countable Additivity.* Both principles get you CA for degrees of belief (the second via the interpretation of degrees of belief as betting prices), but I think the first one is better. Principle I_{CAW} invokes CA directly, albeit for betting prices, and therefore seems tailored to showing that degrees of belief are countably additive. Principle I_{CAJ} has an epistemological flavour that rules on bets simply do not have, and while it clearly bears on CA, we can plausibly believe it for reasons which are independent of CA. Williamson aims to persuade a sceptic that there is good reason to adopt CA. Hinging on the countable additivity of bets will not do the job; principle I_{CAJ} just might.

It is worth pausing here to consider if de Finetti is right in stating that the only reason we would postulate the countable additivity of bets is because we already have countable additivity of probabilities in mind. In the de Finetti betting scenario, a Bayesian agent is offering betting odds at which she would be happy to buy or sell a bet on a given event; because the agent sets the prices, she is acting more like a bookie than a bettor. So in the infinite lottery scenario, if CA is not enforced, the agent could set her betting price for each ticket as 0, but a price of 1 (to be multiplied by the size of stake to calculate the final loss or gain) for the event that one ticket will be picked. This means that her opponent could place bets on every single number and pay 0 for each bet, while, since one number will be the winning one, winning the prize: a positive sum of money. This looks like an appealing case for the enforcing of CA. We have to be very careful, however, in noting precisely where the seemingly paradoxical quality of this example creeps in, so we do not have the impression of having gotten CA for free. Of course, the critical step is assuming that because we paid 0 for each bet, we have paid 0 for the overall bet, on all tickets. This is only true if the betting prices are countably additive. I think that here intuitions about finite cases might be guiding us: we can easily see the absurdity of allowing someone to pay $0 + 0$ for a bet on heads and tails, and them winning the prize of 1 (say). But bringing this to bear on countably infinite cases would be, in my view, a sleight of hand. Our intuitions about infinite objects can be unreliable and conflicting—or, more charitably, they might be just fine but are sometimes impossible to accommodate all together in our usual mathematics, this very debate being a case in point. We should also note that a similar reasoning process could be carried out in support of allowing merely finitely additive probability functions. In subjective Bayesianism, the betting procedure is a scheme that lets agents pick which prices they desire for bets, in order to gauge the strengths of their beliefs in the events occurring or not. Given this, it seems clear that whichever non-self-defeating combination of prices best reflects the beliefs of the agent should be available to her. So if the agent thinks each ticket in the infinite lottery has equal an chance of winning, she should be able to express this information in the betting prices she offers. This brings us to accepting merely finitely additive probabilities. But, of course, one could say there was a small sleight of hand here too, since what is obviously always possible in the finite case presents some strange consequences in the countably infinite case. If it were impossible for an agent to express 50/50 probabilities for a regular coin toss, then the betting elicitation method would have deep flaws. But, once again, once we transfer this idea to the countably infinite case, we have to face counter-intuitive facts: now all the bets are priced 0.

Therefore, while de Finetti might have understated the case for countable additivity of bets, I think he is right in highlighting that CA depends on this principle, and questioning the fact that we then might derive CA from it. Of course we *can* derive CA from the countable additivity of bets, but I think we can do better. It seems to me that all our intuitions about bets are necessarily related to finite cases, and when we extend them to countably infinite cases we get shaky and unreliable results. It is not clear to me that we would want to settle this debate based on these shaky intuitions on bets. It might even be better to cut out the middle man and talk of probability on countably infinite spaces directly. This brings us back to the familiar clash of intuitions I outline above, but without the added baggage of ideas on infinite bets. Alternatively, a good way to go in Jaynes's framework is to look to what I called I_{CAJ}.

Now, Williamson would like to have an independent motivation for why the rules of probability are also rules for correct reasoning. But it is important to clarify what sort of 'independence' we have in mind. Principle I_{CAJ} is directly about probabilities: it cannot be taken as an external argument for why degrees of belief should be probabilities. Principle I_{CAW}, on the other hand, is about betting prices, and thus fits the bill. However, I have argued that countably infinite bets are an unfamiliar hypothetical object, and that trusting our intuitions about them in order to solve the debate might not be the best way to go. Perhaps de Finetti is right in wondering whether the countable additivity of bets would be so obvious if we did not have CA in mind. In any case, an independence of this sort, i.e. something which really has nothing to do with probability but can be brought to bear on it, seems both hard to attain and not desirable: the axiom of CA is technical in nature, and it will be an arduous task to find an argument for it which makes no reference to something suspiciously close to (something equivalent to) CA: in some way or another this argument will involve countably infinite sums or countable unions. Given this, the best available option is to give a justification for CA which is internal to the framework itself, and yet more appealing than merely adopting the axiom because it makes certain technical results easier to prove. I think appealing to principle I_{CAJ} can perform this difficult balancing act: it is independent in the sense that it hinges on epistemological intuitions on what we should be able to do with knowledge of all the elementary probabilities.

When applied to Williamson's general framework, the above considerations could be implemented as follows. Williamson could use his preferred argument by derivation or by interpretation in order to justify that degrees of belief should be probabilities, up to FA. In order to justify the adoption of the stronger CA, I think an internal justification will do: he can appeal to principle I_{CAJ} above. It would go something like this: objective Bayesianism is a theory that tells us how much we ought to believe certain propositions; therefore, it ought to be possible to assign beliefs to propositions—not to *all* propositions necessarily, but at least to those which we understand perfectly well as being made up of elementary propositions of which we know the probability. A supporter of FA could retort that we *might* know the probabilities for all the tickets in the infinite lottery, and yet not know the probability of an even-numbered ticket winning: this is simply undetermined, and there is no inconsistency here. They could point out that Williamson too says that sometimes probabilities are undetermined. While this is a fair point, a possible response available to the objective Bayesian who wishes to apply CA is as follows: we understand perfectly well what the event "an even-numbered ticket will win" means, we know it is made up of the tickets $2, 4, 6, \ldots$ and we know the probability for each of those tickets: it is only reasonable that we should also know the probability of the event that an even-numbered ticket will win.

Summing up, in this section I have argued that Williamson wishes to justify the adoption of CA in his objective Bayesianism with an argument by interpretation. In this argument we show certain facts about betting prices, we interpret degrees of belief as betting prices, then we conclude that these facts must be true of degrees of belief as well. But I have pointed out that an argument by interpretation such as this relies on the principle of countable additivity of bets. This means it is also an argument by derivation. Given that Williamson derives CA from a stronger principle, he would do better by deriving it from a principle which we have good reason to adopt. I think that the 'Jaynesian' idea I expose above is better than countable additivity of bets as a motivation to adopt CA.

5.7 Summary and conclusion

Jaynes thought he had an answer to the debate on countable versus merely finite additivity in probability: he argued that the proponents of the latter were making mistakes in the way they treated mathematical infinity. I argue this is not correct: Jaynes's warnings on the use of infinity are neither necessary nor sufficient for avoiding merely

FA probability functions. Nonetheless, there is still a positive, Jaynes-inspired, contribution that can be made to the debate. I argue that there is a sense in which what is seen by some as one of the most undesirable characteristics of the adoption of CA, namely the existence of non-measurable sets, does not concern Jaynes's theory of probability. Secondly, Jaynes has a principled reason for foregoing the possibility to model uniform distributions over countably infinite sets in his framework. This is based on the idea that if we know the probability of each single event of the sample space, then we should be able to have an explicit, unique probability for any combination of those basic events—or at least for those combinations for which we have a clear understanding and description. This motivation for adopting countable additivity works well for Williamson's version of objective Bayesianism too, and I have argued that this would be an improvement on the argument he currently adopts. As I anticipated at the beginning of the chapter, I think this has been an application of what, to my mind, is one of the strongest aspects of de Finetti's approach: a tight and consistently developed relation between foundational aspects of probability (but it could also be applied to other concepts) and its mathematical formulation.

Chapter 6

Conclusion

This chapter concludes the thesis by summarising and discussing my main arguments and sketching out some possible directions for future work.

6.1 Summing up

In this thesis I put forward a unified and, in some aspects, novel, way of placing de Finetti's theory within the contemporary fields of formal epistemology and philosophy of probability; I then develop two specific arguments that take this picture of de Finetti's theory as a starting point. My overall view is that de Finetti created a theory in which the mathematical and philosophical aspects of probability are developed together, and that the former must have certain features which emerge from an understanding of the latter. De Finetti saw probability as a primitive concept: the feeling of uncertainty experienced by human agents. What he proposed was to study this primitive concept something like the way physicists do when they pass "from the 'facts' to their mathematical translation" (de Finetti, 1974/1990, p. 256). This approach results in operational definitions of the concept at hand, so that meaningful statements can be made about it. All—and only—the ones demanded by the intuitive understanding of the concept at hand should be included in the formal/operational definition. Any rules extraneous to this should not be added.

The concept of *meaning* is understood in a pragmatist sense and is weakly verificationist: a meaningful statement is one that we could, in principle, check. The separation of the mathematical theory (which is a model) from the primitive concept of probability (the target of the model) means that the former is exempt from verifiability—this being a crucial difference from Peirce's, otherwise broadly similar, verificationism.

The foundational part of de Finetti's project ends with a definition and proper understanding of the correct axioms of probability. The calculus of probability can now proceed, on these (purportedly) solid philosophical foundations, without worrying about what it all means at each step, but keeping in mind the foundational principles when relevant. In particular, the calculus of probability, the way de Finetti understands it, has nothing to say about which probability distribution is better (in whatever sense) than others. Since probabilities are degrees of belief, the calculus of probability can thus help us to reason correctly; this, though, is no theory of rationality: we can combine our degrees of belief consistently, but there is nothing in the formal theory that favours one degree of belief over another. This does not mean that de Finetti is advocating that any belief whatsoever is rational: that degrees of belief should respect the theory of probability is a necessary, not sufficient, requirement for rationality; and although there is much to say about the quality of an agent's assignment of probability, these arguments are not part of the mathematical theory and need not necessarily appear in mathematical form. This, in a nutshell, is the reading of de Finetti's theory which I defend in this work. Below are my main arguments.

Pragmatism and verificationism

I think the best way to understand de Finetti's pragmatism and operationalism, aspects of his work that have puzzled contemporary writers, is as an evolution and synthesis of the positions coming from Peirce, Vailati and Calderoni on one side, and Einstein and Bridgman on the other. What de Finetti does is to add operationalism to Vailati and Calderoni's reading of Peirce's verificationism. This means that classifying him as a strict operationalist, as is often done—in particular by Eriksson & Hájek (2007)—is mistaken. A better reading is that of seeing de Finetti too as a *primitivist* about degrees of belief. The difference between the two positions, then, is that de Finetti sees probability similarly to an *ontological primitive*, which in the classification by Eriksson & Hájek (2007) is a concept studied by science, as opposed to one used by philosophers to order our reasoning.

Objectivity

De Finetti's theory is often accused of lacking objectivity, in that it purportedly calls all sorts of seemingly crazy beliefs rational. I have argued that de Finetti's is not a theory of rationality at all, so it makes no sense criticising it for doing something badly that it never set out to do in the first place. I see it as something of an accident of history that de Finetti's theory has ended up being studied and criticised in the branch of formal epistemology where these sorts of questions on rationality are addressed. On dissecting the different uses of the concept of objectivity made by objective and subjective Bayesianism, it can be seen how far apart the fields of inquiry of these approaches really are.

In short, one isn't forced to write a theory of rationality if one doesn't intend to. But a harder line of criticism is the following: since de Finetti accepts that *some* formal rules should be respected in reasoning (namely the basic axioms of probability, up to finite additivity), why these, and why only these? I think the theory can be defended also from attacks from this angle; I think it is possible to draw a line between rules that don't affect the content of our degrees of belief and rules that do—if that is what we wish to do. What isn't clear, however, is whether the theory emerges unscathed. In particular, the normative status of the axioms of probability is not obvious. More about this below.

Bets

I propose to abandon the betting definition of degrees of belief once and for all. While there is, in the literature, the feeling that this definition doesn't work very well (de Finetti himself abandoned it), there has as yet been no thorough exploration of its problems. This exploration is what I provide, and my conclusion is that the prospects for the betting definition are not good. It very often gives results that deviate from what would be the sincerely held degree of belief—something which is essential to the whole endeavour, if we are to argue that probability theory is a model of degrees of belief!—and, in order to save it, we need to make assumptions that are so onerous that they take away the need for the betting definition itself.

Countable additivity

Contra de Finetti, I argue that it makes good sense to adopt countable additivity in some philosophical understandings of probability. In particular, I think that the objective Bayesian theories of Jaynes and Williamson are justified in adopting the rule. This, however, is not for the reasons given by the authors in question, which, I have argued, are either flawed (in Jaynes's case) or can be improved (in Williamson's). I propose to apply a principle of *compositionality* to lottery, and similar, cases: if we know the probabilities of the basic events, then we should know the probabilities of all the composite events they can form. I argue that it makes a lot of sense to adopt this principle in a theory, such as Jaynes's, in which probability is used as a powerful aid to common sense. I think it also makes sense to adopt this rule in a theory, such as Williamson's, which seeks to tell us how much we ought to believe certain propositions; it ought to be possible, here, to assign beliefs to those propositions which we understand perfectly well as being made up of elementary propositions whose probability we know.

My broader conclusion is that, within standard mathematics and given the intuitions that lie at the basis of the disagreement on countable additivity, it is impossible to resolve the matter in general; only case-by-case, theory-by-theory solutions will work. If, in the foundational raison d'être of a theory, a good reason can be found for adopting countable additivity, then this should be done; if, on the other hand, the broad motivation of the theory points to a merely finite additivity, then this should be adopted instead. I see this chapter as an application of de-Finettian methodology: that of keeping in mind the foundational motivation for a theory when it comes to deciding on its formal features.

6.2 Dircetions for future work

This work seeks to furthering the discussion on certain issues, whilst attempting to actually close the discussion on others. Nonetheless, I think some questions emerge that deserve to be explored more thoroughly than they have been here. I shall now outline them.

De Finetti's methodology

Above, I outlined the general methodology used by de Finetti in constructing a theory of probability, which he discusses in Part 1 of the Appendix to Volume 2 of (de Finetti, 1974/1990, pp. 256-254) and puts into practice in his book. This, as described above, involves constructing a formal theory which models the primitive, intuitive concept of probability. This model must respect its basic features and add nothing extraneous to them. A comparison between de Finetti's remarks on methodology and those by Carnap on explication in (1950/1962, Chapter 1), however, reveals not only some very interesting divergences, but also that in the former case the methodology is described much less explicitly. I think an explicit, full description of what I have taken to be his methodology, and an exploration of the differences between this and Carnapian explication, would be of interest. De Finetti's project is a sort of explication, in that we create a precise, quantitative concept from a pre-scientific one, and use the former in science. In de Finetti, however, the relation between *explicatum* and *explicandum* is one of model to target. So there is another sense in which this is not an explication at all, since the meaning stays with the primitive notion, and we just create a formal model of it in order to have an object that we can apply our calculus to. Another striking way in which de Finetti's project differs from Carnap's explication is in the importance it gives to *similarity* between explicandum and explicatum. For Carnap, the latter should attempt to be close to the former, but not at the expense of any other positive features, such as simplicity and fruitfulness, that the explicatum might have. For de Finetti this is not true: the formal definition of probability must contain no principles which are extraneous to the ones dictated by the primitive concept at hand. I shall attempt to make a proper exploration of the differences between the two approaches, in such a way as to shed a light on de Finetti's methodology, in future work.

The normative status of the probability rules

The question I raised in Chapter 3 was about how to justify the normative status of the axioms of probability if one refuses to consider accuracy as closeness to the truth. There, I argued that it can be boiled down to the question of what sort of inconsistency incoherence is supposed to represent, in an accuracy-oriented approach to probabilism. It seems to me that there is much to be explored in this direction.

Correcting incoherence

There are many different ways to show that being coherent (i.e. having a set of degrees of belief that collectively respect the rules of probability) is better than being incoherent. It is also often conceded that, as non-idealised human beings, we often make mistakes and are, in fact, incoherent. The adoption of an accuracy-based argument for probabilism allows us to address the following, under-explored question: supposing an agent is incoherent, what is the best way to correct that incoherence? That is, what is the coherent set of degrees of belief *closest* to her original, incoherent, set? The idea behind seeking the closest coherent set is to respect the fact that the original assessment contains some important information, even if the final numerical expression of it was incorrect. The fascinating thing here is that different indications of distance we can apply in an accuracy-based approach bring about different proper scoring rules (I mentioned the Brier and the logarithmic rules above in Chapter 3) and different suggested corrections for the initial incoherent set. On an intuitive level, this is clear: generally, different points will emerge as being the closest according to the different measurement of distance. What is more, different definitions of distance will preserve different mathematical relations between the original incoherent degrees of belief. For example, let $bel_i(E), bel_i(\bar{E})$, be the incoherent degrees of belief in events E and $\bar{E}$, and let bel_c be the coherent belief function. In this case, the coherent assignment that is closest to bel_i according to the Squared Euclidean Distance (SED), preserves the following relation: $bel_c(E) - bel_c(\bar{E}) = bel_i(E) - bel_i(\bar{E})$. Hence, if we think that it was important to preserve the relation $bel_i(E) - bel_i(\bar{E})$, we should adopt SED, which in turn induces the Brier scoring rule. On the other hand, if we wish to preserve the relation $bel_i(E)/bel_i(\bar{E})$, we should adopt the Kullback-Leibler divergence, which in turn induces the logarithmic scoring rule. Examining this from the point of view of Bregman divergences, of which both distances given above are examples, gives us a general formulation of which divergence (and so which scoring rule) preserves which relation between the incoherent degrees of belief. I plan to further expand and explore this result further in future work.

6.3 Final remarks

The aim of this work was twofold: to improve the understanding of de Finetti's theory and of how it fits with some of the current philosophical debates, and to use this understanding to contribute to these very debates. This process resulted, many times, in a defence of de Finetti's theory against what I argued was misguided criticism; but it also resulted in a more effective criticism of the position. The results in this thesis regarding countable additivity and the betting definition of degrees of belief reflect this. I am not arguing that we should all become subjective Bayesians in the style of de Finetti—but I hope, if my aims in this work have been achieved, that even the staunchest opponents of the position will benefit from a more clearly defined target.

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