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THE PREFACE PARADOX REVISITED

ABSTRACT. The Preface Paradox has led many philosophers to believe that, if it is assumed that high probability is necessary for rational acceptability, the principle according to which rational acceptability is closed under conjunction (CP) must be abandoned. In this paper we argue that the paradox is far less damaging to CP than is generally believed. We describe how, given certain plausible assumptions, in a large class of cases in which CP seems to lead to contradiction, it does not do so after all. A restricted version of CP can thus be maintained.

1. INTRODUCTION

Many of us have the intuition that rational acceptability is closed under conjunction, that is, that it obeys the following principle:

Conjunction Principle (CP) If $\varphi_1, \ldots, \varphi_n$ are all rationally acceptable to person S at time t, then $\bigwedge_{i \leqslant n} \varphi_i$ is rationally acceptable to S at t, too.

However, when we combine this principle with a purely probabilistic analysis of rational acceptability according to which a proposition is rationally acceptable to a person just in case the subjective probability she assigns to it (i.e., her degree of belief in that proposition) exceeds some threshold value $\mathbf{t}$ (where $0 < \mathbf{t} < 1$), paradox threatens.[1] The supposition that "sufficiently high" (but non-perfect) probability suffices for rational acceptability has given rise to the well-known Lottery Paradox (Kyburg, 1961; Hempel, 1962); the supposition that it is necessary for rational acceptance gave rise to the Preface Paradox. Whereas the former can be resolved by denying that sufficiently high probability is the *only* (non-trivial) necessary condition for rational acceptance, thus leaving open the possibility that high probability is at least *among* the necessary conditions for rational acceptance, the latter paradox precludes this (at least, as long as we are unwilling to give up CP).

This paper is only concerned with the Preface Paradox. It grants that a sufficiently high degree of belief alone is not enough for rational acceptance, but insists on the following thesis:

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Necessity Thesis (NT) A proposition φ is rationally acceptable to person S at time t only if at t S believes φ to a degree exceeding the threshold value $\mathbf{t}$ ($0 < \mathbf{t} < 1$).

The question to be addressed, then, is whether this insistence must be at the expense of CP. More precisely, the question is whether the assumption of NT necessitates a wholesale rejection of conjunctive closure for rational acceptability. Even if, in the face of NT, the Preface Paradox exhibits CP to be untenable generally, CP might still be unproblematic for specific classes of propositions. To give a trivial example of such a class: it is certainly unproblematic to close rational acceptability under conjunction whenever the propositions are logical and/or mathematical truths. It would be of interest to know more generally in what kinds of cases we can safely close rational acceptability under conjunction, if we want to maintain NT (as we do). The main claim to be argued in this paper is that, given certain plausible assumptions, rational acceptability *can* be consistently closed under conjunction, not just in the aforementioned trivial cases, but also in a class of cases in which, interestingly, the Preface Paradox is alleged to show it *cannot*. This amounts to a defense of a somewhat restricted version of CP.

2. THE PREFACE PARADOX

Here is a typical presentation of the Preface Paradox:

> You write a book, say a history book. In it you make many claims, each of which you can adequately defend. In particular, suppose it is rational for you to have a degree of confidence x in each of these propositions, where x is [above $\mathbf{t}$] but less than 1.0. Even so, you admit in the preface that you are not so naive as to think that your book contains no mistakes. You understand that any book as ambitious as yours is likely to contain at least a few errors. So, it is highly likely that at least one of the propositions you assert in the book, you know not which, is false. Indeed, if you were to add appendices with propositions whose truth is independent of those you have defended previously, the chances of there being an error somewhere in your book becomes [*sic*] greater and greater. Nevertheless, given that rational belief is closed under conjunction, it cannot be rational for you to believe that your book contains any errors. For if ... it is rational for you to believe each of the propositions that make up your book, then, given [CP], it is also rational for you to believe their conjunction. This is so despite the fact that it is rational for you to have a low degree of confidence in this conjunction .... (Foley, 1992, p. 113f)

Thus, given NT and CP, the conjunction of the claims you make in the book both is and is not rationally acceptable.

Foley's presentation of the paradox is very informal. To distill from it a mathematically precise formulation of the paradox is no straightforward matter. The reason for this is that Foley's presentation (as indeed most informal presentations of the paradox) is ambiguous about what exactly gives rise to the paradox. It seems that Foley may at least have two quite different problems in mind. One is what we in this paper shall take to be the Preface Paradox; another is an at least prima facie closely related problem that sometimes also goes under the name of Preface Paradox, but that in our opinion is not in the least pertinent to the tenability of CP, which is generally regarded to be the target of the paradox. Let us start by clearly distinguishing the two problems.

The problems have in common that each involves a conjunction that by the standards for rational acceptability both does and does not qualify as rationally acceptable, and the conjuncts of which all qualify unambiguously as such. What makes the second problem essentially different from the paradox we will be concerned with is that the former, but not the latter (which we take to be the Preface Paradox), crucially assumes the existence of evidence relevant to the evaluation of the conjunction which goes beyond the evidence relevant to the evaluation of each of its conjuncts. For instance, in what is the first presentation in the literature of the Preface Paradox, Makinson (1965) asks us to imagine that, although we have double-checked every single claim made in our book, we have "already written other books, and received corrections from readers and reviewers" and therefore "believe that not everything [we have] written in [our] latest book is true" (p. 205).[2] Foley seems to do something similar when in the above passage he assumes that we "understand that any book as ambitious as [ours] is likely to contain at least a few errors." These passages suggest that the evidence relevant to the evaluation of the conjunction contains data about errors discovered in previous (ambitious) books written by us or by other authors, data which may be assumed to be irrelevant to any of the claims we make in the book (the book is not assumed to be about our making errors). Now, perhaps such references to additional evidence undermining the conjunction are only supposed to serve dramatic purposes. Yet, as we said, if not, this yields a problem different from the one to be described below as the Preface Paradox.

This different problem – one could call it the problem of contrasting evidence bases[3] – is certainly worthy of attention, but if it is taken as militating against CP, then it should also be taken as militating against what is sometimes called probabilism, that is, the view that, on pain of irrationality, one's degrees of belief are to conform to probability theory. The whole setting of the Preface Paradox clearly assumes this view, for else the fact that according to probability theory the conjunction involved in the paradox should be believed to a degree of at most **t** would constitute no impediment to actually believing it to a degree exceeding **t** (so that both NT and CP could be satisfied). Probability theory tells us that a conjunction can never be more probable than any of its conjuncts. Yet it seems arguable that in situations in which the evidence relevant to the evaluation of a conjunction does not coincide with the joint evidence relevant to the evaluation of each of its conjuncts, a conjunction *can* be more probable than (some of) its conjuncts.

To see why, assume that today is the contractually determined deadline for handing over your manuscript to the publisher. If it were not for the contract, you would rather continue working on it, given that you feel your evidence for some of the claims you make in the book is insufficient. As it also happens, however, this is your 100th book, and although each time you delivered a manuscript to a publisher you were skeptical about the result, so far none of your colleagues has been able to spot a single mistake in any of your works, even though they are keen to find one. As you, too, have been unable to detect a mistake in your previous works, you seem to have excellent evidence for the conjunction (or if you don't think the evidence is so excellent, let it be your 1,000th or 10,000th book), even though you also harbor considerable doubts about certain propositions you defend in your book. We do not intend to speculate about how one should proceed in this kind of case. The point we want to make is just that one has to concede that these cases do not provide the right context to discuss the compatibility of CP with NT and that, if it is to pose a threat to CP, the Preface Paradox is better taken to derive from some other problem than from the possible occurrence of conflicting evidence. Thus, we will henceforth assume that the total evidence relevant to the evaluation of a conjunction is the union of the total evidences for each of its conjuncts.

So the problem underlying the Preface Paradox is not that of contrasting evidence bases. Then what is it? When in the same paper we cited from earlier Foley states the Preface Paradox a second time, he writes: "[Y]ou might have enormously strong evidence for each of the propositions in the body of the book, and yet *given their huge number*, you might also have enormously strong evidence for the proposition that at least one of them is false" (p. 117; our italics). Here the only fact that seems to make the conjunction not rationally acceptable to you, even though the conjuncts are, is that it has so many conjuncts; there is no mention of the ambition of the book. Now, it must be noted that, however great the number of claims made in a book is, it does not, by itself, provide any reason not to find the book as a whole (i.e., the conjunction of the claims) rationally acceptable. Mathematically speaking, the probability of the conjunction can be no higher than the probability of the least probable conjunct. But given that all conjuncts are assumed to have a probability exceeding **t**, this means that the probability of the conjunction can still be above **t** as well. Yet this later quote from Foley's (1992) does seem to point in the direction of what we take to be the Preface Paradox.[4]

What is needed to derive a contradiction from CP, given NT, is that, even though each of the claims made in a book separately satisfies all conditions necessary for rational acceptability, including having a probability above the threshold, their conjunction has a probability not above the threshold. Whether the conjunction does have such a probability is dependent not only on the unconditional probabilities of the separate claims and the number of claims, but also on the various mutual conditional probabilities of the propositions. More precisely, what creates difficulties for CP (given NT) are what we shall term *preface cases*. These are defined thus:

DEFINITION 2.1. Let S be a – possible or actual – individual, let $p(\circ)$ be the probability function representing S's degrees of belief at time t, and let $\Phi = \{\varphi_1, \ldots, \varphi_n\}$ be a collection of propositions expressible in S's language. Then $\langle S, p(\circ), \Phi, t\rangle$ is a *preface case* if and only if:

(i) for all $\varphi_i \in \Phi$: the proposition φ_i at t satisfies every condition necessary for rational acceptability for S at t; thus in particular $p(\varphi_i) > \mathbf{t}$, and

(ii) $p(\varphi_1)p(\varphi_2 \mid \varphi_1) \cdots p(\varphi_n \mid \varphi_1 \wedge \cdots \wedge \varphi_{n-1}) \leqslant \mathbf{t}$, that is, the probability of $\bigwedge_{i \leqslant n} \varphi_i$, is, according to S's belief state at t, less than or equal to **t**.

Note that the definition assumes S's degrees of belief to be representable by a probability function, that is, they are assumed to be coherent. Note also that Φ is finite. We shall indeed confine most of our attention to inconsistencies involving finite sets of propositions. The infinite case will be briefly considered towards the end of the paper.

Given a preface case involving an author and her just-completed book, it follows from the first clause of Definition 2.1 and CP that the conjunction of the claims made in the book is rationally acceptable to her, but it also follows from the second clause of the same definition and NT that the conjunction is not rationally acceptable to her. This paradox can be made even more poignant by noting that, under certain circumstances – for example, given "enough" conjuncts that an author believes to a degree exceeding **t** but less than 1 and that are mutually probabilistically independent according to her belief system – the probability of the conjunction may become less than $1 - \mathbf{t}$, so that not only would it fail to be rationally acceptable to her, but its negation might (depending on whether it satisfies the further conditions on rational acceptability) be rationally acceptable.

We shall assume that the class of preface cases is not empty, that is, it is assumed that it is not impossible that (for some person at some time) all the propositions in a given set are rationally acceptable but at the same time the conjunction of those propositions has a probability not exceeding the threshold. Clearly, whether this assumption is correct can be certified only when all necessary conditions for rational acceptability are known. But, equally clearly, if it is not correct, then the Preface Paradox does not arise in the first place.

There seems to be a straightforward solution to the Preface Paradox as we just presented it: simply require that $\mathbf{t} = 1$; in that case, no set of propositions could satisfy both clauses of Definition 2.1, so that the class of preface cases would at once become empty. One should realize, however, that if one opts for this solution, then precious little of what we intuitively regard as rationally acceptable would qualify as such according to our theory of rational acceptability. In fact, several authors have argued that a credal state is rational only if it is representable by a *regular* (or *strictly coherent*) probability function, where a probability function is regular if it assigns to contingent propositions values in the open interval (0, 1) only, that is, it reserves probability 1 for logical and mathematical truths, and probability 0 for logical and mathematical falsehoods.[5] Though not entirely uncontroversial,[6] we shall here go along with this *regularity requirement*, as we shall call it (our solution to the Preface Paradox would only become *easier* without the requirement – cf. note 32 and the closing paragraphs of Section 4). Clearly, when combined with the proposal to require absolute certainty of a proposition for it to be rationally acceptable, the regularity requirement would lead to the absurd conclusion that only logical and mathematical truths can be rationally acceptable. Very likely, then, we will want to have a rule of rational acceptability that countenances propositions as rationally acceptable in the absence of 100% certainty about those propositions. And indeed, so far no one has defended the contrary as a solution to the Preface Paradox and we suspect that no one will ever want to.

A solution that *has* been defended, and that in effect is endorsed by so many that it is no exaggeration to call it the standard solution to the paradox, is to abandon CP.[7] It is plain that if CP is given up, the rational acceptability of a conjunction does not follow from the rational acceptability of its conjuncts, and so, if the probability of the conjunction is equal to or less than $\mathbf{t}$, it unambiguously is *not* rationally acceptable, even if all its conjuncts are. However, a chief problem with this solution is that it asks us to abandon a principle that we rely on in many of our practical deliberations and that seems to have a respectable status in philosophical argumentation, too.[8] We hope it is then found to be good news that a relatively minor restriction of CP (which, as will be explained below, may in actuality not even be a restriction at all) suffices to solve the Preface Paradox. At the outset it was already suggested that CP would be harmless if it were restricted to the class of logical and mathematical truths. But with a thusly restricted version of CP we would hardly be better off than if we had to do without CP altogether. The version of CP we will propose shortly, and defend in the remainder of the paper, is of a much more interesting variety.

What we will do is show that, given certain assumptions to be explained in the next section, CP can be consistently applied to what may be presumed to be a large class of cases in which application prima facie seems to lead to paradox. Specifically, it is consistently applicable to a class of cases that comprises what for a reason to be given shortly we propose to call the *genuine preface cases*.[9] These are defined as follows:

DEFINITION 2.2. Let S, $p(\circ)$, Φ, and t be as in Definition 2.1. Then $\langle S, p(\circ), \Phi, t\rangle$ is a *genuine preface case* if and only if it is a preface case, and moreover

(iii) for all $\varphi_i \in \Phi$ and all $\{\varphi_{j_1}, \ldots, \varphi_{j_m}\} \subseteq \Phi$ such that $1 \leqslant m \leqslant n$: $p\left(\varphi_i \mid \varphi_{j_1} \wedge \cdots \wedge \varphi_{j_m}\right) > \mathbf{t}$, that is, relative to S's belief state at t, the conditional probability of each proposition in Φ given one or more propositions in the same set is also greater than $\mathbf{t}$.

If, as we assume, the class of preface cases is not empty, then it can also plausibly be assumed that there exist genuine preface cases. Firstly, it follows from the simple arithmetical truth that for any real number a strictly between 0 and 1 there are $n \in \mathbb{N}$ and $b \in \mathbb{R}$ such that $a < b < 1$ and $b^n \leqslant a$, that, for a given set $\Phi = \{\varphi_1, \ldots, \varphi_n\}$, the probabilities $p(\varphi_1), \ldots, p(\varphi_n \mid \varphi_1 \wedge \cdots \wedge \varphi_{n-1})$ can all be above $\mathbf{t}$ (whatever value we choose for $\mathbf{t}$, strictly between 0 and 1) and at the same time have a product not exceeding $\mathbf{t}$. So, clause (ii) of Definition 2.1 and clause (iii) above are certainly jointly satisfiable. Secondly, these clauses are clearly compatible with the requirement of clause (i) of Definition 2.1 that $p(\varphi_i) > \mathbf{t}$ for all i with $0 \leqslant i \leqslant n$, and surely it would be absurd to suppose that the other necessary conditions for rational acceptability referred to in the same clause could conflict with clause (iii) of Definition 2.2; the opposite is much more likely, as we shall see below.

It is immediately evident that the class of genuine preface cases includes every preface case $\langle S, p(\circ), \Phi, t \rangle$ for which it holds that the $\varphi_i \in \Phi$ are mutually probabilistically independent relative to $p(\circ)$. Furthermore, it seems arguable that many authors constitute genuine preface cases together with their books at least at the time of the completion of the book (hence the name *genuine* preface cases). Typically authors aim at maximal coherence (Stalnaker, 1984, p. 92). Insofar as the claims made in a book are not independent, they are meant to buttress each other. Claims the author feels might cast aspersion on some of the claims she makes in the book may be discussed but are then argued to be false or implausible; in any case they are not endorsed. At the very least, every author *wants* her book to satisfy clause (iii), and it seems to us that this ideal is attainable (even if it is not always attained). Hence, it seems to us that, if rational acceptability can be safely closed under conjunction in all genuine preface cases, it can safely be closed under conjunction in an interestingly large class of cases, including a reasonable number of real books.

Better still, as was just said, the cases in which CP can be consistently applied *comprise* the genuine preface cases. However, we deem it best to leave to Section 4 the exact definition of the more general class of cases (which will be termed *ε-preface cases*), as that definition presupposes some terminology yet to be introduced. For the nonce, let us proceed on the supposition that CP can be consistently applied in all and *only* genuine preface cases. Given that supposition, how generally applicable is CP? In the introduction we granted that there were conditions necessary for rational acceptability other than high probability. To date, there has been no generally accepted proposal as to what these other conditions might be, nor are we prepared to come up with a proposal to that effect in the present paper. However, the question what they are is of evident relevance to the question whether if CP can be upheld in all genuine preface cases, it is consistently applicable without exception. Given that, as we claim, rational acceptability can indeed be consistently closed under conjunction in all genuine preface cases, putative counterexamples to the claim that CP holds generally can only come from cases where we have a collection of propositions that all have an unconditional probability above the threshold but some of which have a conditional probability, given one or more other propositions from the same collection, below or equal to the threshold. Whether, in such a case, the putative counterexample constitutes a *real* counterexample then entirely depends on whether each of these propositions meets all further necessary conditions for rational acceptability. If not, then the question whether CP can be consistently applied in the given case cannot even arise (for it does not apply at all). To be more precise, whether the fact that conjunctive closure is safe for all genuine preface cases implies that it is safe unexceptionally depends on whether clause (i) of Definition 2.1 implies clause (iii) of Definition 2.2, a question that, again, cannot be answered as long as we are in the dark about the necessary conditions for rational acceptability other than high probability.

It seems likely that the further conditions on rational acceptability will rid us of at least some of those putative counterexamples. Suppose, for instance, that you were to write an n-page book (not likely to become a bestseller) saying, on page k (for each page k with $1 \leqslant k \leqslant n$), that ticket #$k$ will not win, where it is understood that the tickets are tickets in a fair n-ticket lottery with exactly one winner and with $(n-1)/n > \mathbf{t}$. Here each separate claim has a probability exceeding the threshold, but each also is inconsistent with the conjunction of all the claims (for that implicates that there will be no winner). The reason we may hope that further conditions on rational acceptability will rule out cases such as these (e.g., because none of the conjuncts will meet these conditions) is that these cases simply are instances of the Lottery Paradox.[10]

It also seems quite plausible that similar but somewhat less extreme cases will be ruled out. Suppose you write a book containing only two claims, both of which you assign an unconditional probability above $\mathbf{t}$. However, your conditional probability for one given the other is not above the threshold. It will thus seem to you that in your book you have made claims that undermine each other: you have, by your own lights, already provided the reader with a reason *not* to believe both claims made in the book. In this case it seems not just unintuitive to say that the conjunction of the claims is rationally acceptable, but also, that both claims are, and perhaps even, that either is. Here, too, we may hope that further conditions on rational acceptability are in accordance with intuition.[11]

Non-genuine preface cases may not all be of the kinds just mentioned. Thus the above is not to suggest that either something is a *genuine* preface case or it is no preface case at all. As we said, there is presently no way of telling. What can already be shown, however, is that, notwithstanding appearances to the contrary, the genuine preface cases do not militate against CP. Accordingly, we are able to make a case for the following, restricted version of CP:

Restricted Conjunction Principle (RCP) If each proposition in $\Phi = \{\varphi_1, \ldots, \varphi_n\}$ is rationally acceptable to S at t, then so is $\bigwedge_{i \leqslant n} \varphi_i$ – provided $\langle S, p(\circ), \Phi, t\rangle$ does not constitute a non-genuine preface case.

And as will appear toward the end of the paper, the case for RCP readily extends to one for an even less restrictive principle.

Notice that RCP is not obviously correct. Quite the contrary, in fact. A genuine preface is still a preface case, so that the probability of the conjunction of all the propositions involved does not exceed the threshold and hence should, on account of NT, not be rationally acceptable, contrary to what RCP asserts. That is to say, RCP appears to be at war with NT no less than CP is. Yet we will show that, given certain defensible assumptions, RCP *is* reconcilable with NT.

Evidently, RCP is formally weaker than CP in the sense that, if a conjunction is rationally acceptable to someone by virtue of RCP, then it is rationally acceptable to that person by virtue of CP as well, but not vice versa. But our discussion above concerning putative counterexamples against the unrestricted CP amounts to the claim that it is at present not possible to say whether RCP is also *materially* weaker than CP. That is to say, for all we know, CP does not actually apply to more cases than RCP does.

Before we begin with our case, we want to point to an analogy between it and the various attempts that have been made to defend restricted versions of the Principle of Indifference. Very roughly, the latter principle says that, absent any reason to the contrary, one should assign mutually exclusive propositions an equal initial probability. Although intuitively plausible, this principle famously gives rise to several paradoxes.[12] But exactly because it is so plausible from an intuitive viewpoint, and also because the principle has often been successfully applied in practice (Jaynes, 1973; Uffink, 1995), a natural response to the fact that the Principle of Indifference leads to paradox would seem to be to search for restricted versions of the principle that are *not* paradoxical. Such attempts have in fact been made; see, for instance, Keynes (1921) and, for a recent attempt, Castell (1998). In a similar spirit, we will endeavor to salvage as much of CP as is possible without giving the Preface Paradox the chance to rear its ugly head again.

3. Basic Assumptions

We have already pointed to the fact that the Preface Paradox assumes probabilism. The latter is widely embraced as a doctrine about rationality,[13] and in this paper we shall largely stick to it. Yet the first two assumptions to be made in this section deviate from assumptions that commonly go with probabilism. The first of our assumptions is that we do not start out with having degrees of belief in every proposition expressible in our language, but rather derive certain degrees of belief from other degrees of belief if we think it opportune to do so. Secondly, we assume that measuring an agent's degrees of belief is a procedure with limited accuracy. Our third assumption – which does not go counter to anything probabilists have ever said – is that the threshold for rational acceptability is fairly high. How together these assumptions enable us to defend RCP will be shown in the next section. Here we will argue for the plausibility of each of these assumptions, in order.

1. Probabilists commonly assume that we come to have degrees of belief in all propositions expressible in our language while at our mother's knee. These "prior" degrees of belief are then allocated as we obtain information about the world. Thus on the standard probabilist view, an agent's belief system at any moment in her life is representable by a function that is defined on the whole class of propositions expressible in the agent's language $\mathcal{L}$. More specifically, we can, at each moment, associate with each proposition expressible in $\mathcal{L}$ both an unconditional probability and a conditional probability given any finite collection of other propositions expressible in $\mathcal{L}$, where the unconditional probability represents the agent's unconditional degree of belief in the proposition and the conditional probabilities represent the agent's degree of belief in the proposition conditional on the various collections of other propositions.[14] On this account, probability theory need be (and can be) used by the agent only in order to *check* whether she is coherent, that is, whether the function representing her degrees of belief is indeed a probability function; in Garber's (1983, p. 101) words, it functions as a kind of "thought police ... clubbing us into line when we violate certain principles of right reasoning."

To us the foregoing picture seems to be plain false. To be sure, we sometimes do make use of probability theory just to check whether a particular assignment of probabilities to some propositions is coherent, but it should be, and, we surmise, also largely is, uncontroversial that more frequently probability theory is used as a tool for generating *more* probabilities from those we already have.[15] On the account that in our view is much closer to reality than the standard Bayesian account, we are generally able to determine our degrees of belief in some propositions – whether unconditional or conditional on some other propositions – without the help of probability theory (perhaps some "spontaneously", others only after considerable reflection), but this is only so for a – proper – subset of the propositions expressible in our language. Degrees of belief in further propositions are then obtained by means of probability theory – if and when we want or need to obtain these degrees of belief.[16] For example, it seems reasonable to assume that, if authors for some reason want to determine the probability of their book as a whole, they do this by deriving it from the probability they assign to a particular one of the individual claims they make in the book, the probability assigned to a second claim they make in the book conditional on the former claim, the probability assigned to a third claim conditional on the two former claims, and so on.

For obvious reasons, we shall call the probabilities, or degrees of belief, we start out with, *basic* probabilities/degrees of belief, which may be conditional or unconditional. Those that are obtained from them with the help of probability theory, we call the *derived* probabilities/degrees of belief.

The existence of the distinction just described in the status of our degrees of beliefs is only one part of the first assumption. The other part concerns our awareness of the status of our degrees of beliefs. Deriving one probability from one or more other probabilities need not always be, and perhaps mostly is not, an entirely conscious process. And if it is – as, for instance, when we derive $p(\varphi \mid \psi)$ from $p(\varphi \wedge \psi)$ and $p(\psi)$ using pencil and paper and perhaps a pocket calculator – that does not reliably indicate the status of the degrees of belief involved; perhaps we had earlier derived $p(\varphi \wedge \psi)$ from $p(\varphi \mid \psi)$ and $p(\psi)$ but then forgotten the value of $p(\varphi \mid \psi)$. Although it is plausible to assume that we are often unaware of the status of our degrees of belief, that we always are unaware of that status is not quite so evident, yet is part of the assumption we are making here.

2. All methods available for measuring someone's degree of belief in a given proposition come down to proposing bets of one kind or another on this proposition, the basic idea of which was proposed by Ramsey (1926) and (separately) by de Finetti (1937). These methods thereby aim to determine one's fair betting quotient for that proposition, that is, the price at which one is willing to take either side of a bet that pays off a certain positive amount of money if the proposition turns out true, and nothing otherwise. For instance, if an agent is indifferent between buying and selling a bet on φ that pays \$ y if this bet's price is \$ x, then, it is said, she believes φ to a degree of x/y. Thus, if you are willing to take either side of a bet that pays \$ 1,000 if it rains tomorrow (and nothing otherwise) if its price is \$ 750, then it is said that you believe the proposition that it will rain tomorrow to a degree of 0.75.

When probabilism (re-)emerged in the first half of the last century, behaviorism was the established doctrine in the field of psychology. On this view, to say of someone that she believes a proposition to a degree of x/y is not to make a claim about her state of mind, but just to say that under certain circumstances she will behave in a certain way. From this perspective it clearly makes no sense to assume that the device for measuring degrees of beliefs might not always be entirely accurate. After all, that would seem to imply that there is something to degrees of beliefs over and above patterns of betting behavior.

The outlook is quite different from the perspective of modern psychology. It is now widely believed that, although degrees of belief are measurable in the above way, they are not constituted by the outcomes of such measurements.[17] Casual introspection seems sufficient to convince us of their independent reality.[18] And given a realist take on degrees of belief, there does seem to be a danger that a person's measured degrees of belief do not entirely match the degrees of belief that person really has. To suppose that there actually is such a danger is to suppose that the above measuring device for degrees of belief is, like every other measuring device, only accurate up to a certain point. One might object that the fact that our degrees of belief are in some sense internal to us gives us reason to believe measuring them is essentially different from, say, measuring the distance between stars. However, twentieth-century psychology has accustomed us to the idea that, although our mental states (our feelings, motives, emotions, and so on) are clearly internal to us, they are not thereby wholly transparent to ourselves. Thus, that degrees of belief are internal to us is no good reason to suppose the method for measuring degrees of belief is privileged in not having a measurement error. Moreover, it seems that if anyone were to claim that, though we can be wrong about our own motives, etc., we cannot be, or at least usually are not, wrong about the strength of our beliefs, the onus would be on him or her to show that this is so.

We shall assume, then, that with any measured degree of belief is associated a measurement error in the sense that if the measured degree of belief someone has in a proposition φ is a, there are real numbers $\delta \geqslant 0$ and $\eta \geqslant 0$ such that her real degree of belief in φ is no less than $a - \delta$ and no more than $a + \eta$.[19] The exact order of magnitude of δ and η depends on whether the agent's degree of belief in φ is basic or whether it is derived. We shall assume that, for basic degrees of belief, the measurement error is some fixed, small number ε, that is, if $p(\varphi)$ is basic, then the agent's real degree of belief in φ is in $\left[p(\varphi)-\varepsilon,\ p(\varphi)+\varepsilon\right]$. For derived degrees of belief the measurement error depends on the measurement errors associated with the degrees of belief from which they were derived and on the way in which they were derived from those degrees of belief. For example, suppose that agent S has basic degrees of belief of a and b, respectively, in φ and in ψ given φ, and that $ab = c$. Then, although we may assume that she will announce c to be her fair betting quotient for $\varphi \wedge \psi$, her true degree of belief in that proposition is in the interval $\left[c - a\varepsilon - b\varepsilon + \varepsilon^2,\ c + a\varepsilon + b\varepsilon + \varepsilon^2\right]$, which may include, be included in, coincide, or overlap with, the interval

$[c - \varepsilon, c + \varepsilon]$, depending on the values for a, b, and ε. The measurement error for derived degrees of belief can be recursively defined, but since we trust that the reader sees immediately how such a definition is supposed to go, we shall not do so here.

To forestall misunderstanding, we should emphasize that this assumption is *not* motivated by the oft-cited fact that commonly we are only capable of specifying up to a certain point of precision the price we deem fair for a bet on a given proposition.[20] Even if each of us were always able to specify the price he or she deems fair for any given bet as precisely as anyone would like, that would in no way undermine the motivation for our assumption. For the motivation comes from our rejection of operationalism/behaviorism in combination with the fact that modern psychology gives us no reason to believe that the device for measuring degrees of belief is not subject to essentially the same limitations of accuracy as devices for measuring things outside our heads.

As a further comment we note that the assumption that there is a fixed measurement error of ε for all basic degrees of belief may well be a simplification, both as regards the assumed fixedness of the error and as regards the assumed generality. As to the former, it may be arguable that the measurement error is context-sensitive in the sense that we are better able to introspect our true degree of belief in some proposition the more epistemic import that proposition has for us. In addition to this, or alternatively, it may be arguable that some of us have greater introspective gifts than others and that therefore the measurement error varies from one person to another. As to the generality of the assumption, it may be implausible to assume that there is *any* measurement error to be associated with basic degrees of belief in necessary truths/falsehoods (or at least in propositions that are recognizably necessary). As an excuse for the simplifying assumption (if it is one), we may note the following: First, for our purposes it suffices if with the measurement of any basic degree of belief (of any person, in any context) there is associated a small *but positive* measurement error. And second, all that matters for those same purposes is that the assumption holds for degrees of belief in contingent propositions; no assumption need be made about the measurement error for degrees of belief in necessary truths/falsehoods.[21]

3. When (if) authors writing on the Lottery and/or the Preface Paradox give an indication of what the threshold for rational acceptance might be, 0.9 and 0.99 are most frequently mentioned.[22,23] Our third assumption is that the threshold must indeed be in the order of these values. More precisely, for our defense of RCP to work, the threshold for rational ac-

ceptability must at least be $1 - \varepsilon$, which, we may assume, is fairly high. But although high, it is still not 1 and thus allows us to rationally accept a proposition in the absence of *complete* certainty about that proposition – which, as we saw, is an important desideratum for a theory of rational acceptability.

It might be objected that it seems entirely arbitrary to stipulate that the threshold should at least be 1 minus the measurement error for basic degrees of belief. One way to counter this objection would be to say that there is next to nothing that can be said *specifically* about **t** that will not strike one as being arbitrary.[24] And since we eventually will have to commit ourselves to *some* value of **t** if we are to have a fully operational theory of rational acceptability, it seems we may just as well settle on $1-\varepsilon$, or any higher value (except 1), as the value for **t**.[25] But this would seem a rather desperate move, and in any event it is not one we are forced to resort to. For there seems to be a perfectly good and simple pragmatic justification for our proposal regarding the value of **t**, namely that, pending an argument to the contrary, it is only by adopting a value for **t** of at least $1 - \varepsilon$ that we are capable of solving the Preface Paradox without having to entirely sacrifice a plausible principle of everyday reasoning (i.e., CP).[26] To put the point in a slightly different way, if, within a certain range of values, all proposals as to the value of **t** are equally good *but* for the fact that one specific proposal, or rather a specific proposal as to the lower bound for **t**, has the important and distinctive property of allowing us to consistently maintain a principle close to CP, then surely *that* is the proposal we ought to adopt – and that proposal is to set **t** equal to some value in the interval $[1 - \varepsilon, 1)$.

4. The Solution

Remember that the problem in defending RCP was that, though weaker than CP, it still seems to lead to paradox: in a genuine preface case someone finds rationally acceptable each of a set of *n* propositions the conjunction of which she should, according to probability theory, assign a probability equal to or less than **t** (cf. clause (ii) of Definition 2.1), so that it seems that she only can obey RCP and find the conjunction of the *n* propositions rationally acceptable at the expense of either probability theory or NT. In other words, it seems impossible, on pain of inconsistency, to always obey probability theory *and* RCP *and* NT. However, it will now be argued that, with the assumptions made in Section 3 in place, it *is* always possible to apply RCP without this leading to a violation of either NT or probability theory. That is, given the foregoing assumptions, it is possible to solve the Preface Paradox and at the same time maintain a restricted, but still interestingly strong, version of CP.

Our solution to the Preface Paradox turns on the fact that the assumptions together make room for what one might call *corrective reinterpretation*. In this reinterpretation we have, due to assumptions 1 and 2 of Section 3, two degrees of freedom, so to speak. First, we can make certain assumptions about the status (*qua* basic or derived) of our degrees of belief. Second, we can make certain assumptions, on the basis of the measurements of our degrees of belief, about what our *true* degrees of belief are. That given these degrees of freedom any *prima facie* paradoxical genuine preface case can be turned into a non-paradoxical one follows from a mathematical result that we will state further on in this section. However, to introduce the idea underlying that result, and in particular to elucidate the notion of corrective reinterpretation, we first consider, and provide some comments on, an example.

EXAMPLE 4.1. For concreteness, let $\mathbf{t}$ equal 0.99 (and thus ε equals 0.01 – we are not claiming to be psychologically realistic here). Suppose that among your measured degrees of belief are the following:

$$\begin{aligned} p(\varphi) &= 0.992 & \qquad p(\chi \mid \varphi \wedge \psi) &= 0.993 \\ p(\psi \mid \varphi) &= 0.992 & \qquad p(\varphi \wedge \psi \wedge \chi) &\approx 0.977 \end{aligned}$$

Further suppose that $p(\psi)$, $p(\chi)$, $p(\varphi \mid \psi)$, $p(\varphi \mid \psi \wedge \chi)$, $p(\psi \mid \varphi \wedge \chi)$, $p(\chi \mid \varphi)$, and $p(\chi \mid \psi)$, all have some measured value greater than $\mathbf{t}$, and also that for you at this time, φ, ψ, and χ satisfy any other condition necessary for rational acceptability beyond high probability. Then, clearly, $\langle$you, $p(\circ)$, $\{\varphi, \psi, \chi\}$, now$\rangle$ is a genuine preface case.

You happen to be an advocate of RCP, and thus you not only find φ, ψ, and χ rationally acceptable, but you also find their conjunction rationally acceptable. However, in accordance with probability theory, you believe this conjunction only to a degree of (approximately) 0.977. But then, given that this number does not exceed the threshold for rational acceptability, you should *not* find the conjunction rationally acceptable according to NT, to which you also adhere. You are in an inconsistent state of mind. What are you to do?

Here is one possible way out: Assume that your true degree of belief in φ as well as your true conditional degrees of belief in ψ given φ and, respectively, in χ given $\varphi \wedge \psi$ are higher than the measured degrees of belief in these "propositions."[27] More specifically, assume that your true degree of belief in the first equals 0.995 and that your degrees of belief in the second and third both equal 0.998. Then according to probability theory

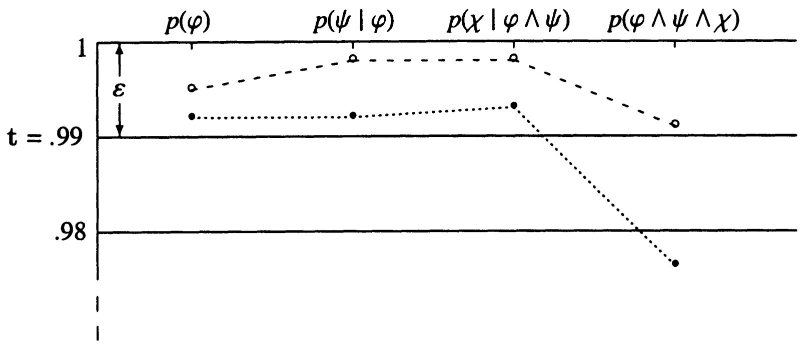

Figure 4.1. •: measured degree of belief; ∘: reinterpreted degree of belief.

you should believe the conjunction of the propositions not to a degree of 0.977 but of (approximately) 0.991, which *is* greater than **t**.

But is this assumption admissible, given that your measured degrees of belief are as previously indicated? Not necessarily. But it is, if you make a further assumption to the effect that your degrees of belief in φ, in ψ given φ, and in χ given $\varphi \wedge \psi$ are all basic and that your degree of belief in $\varphi \wedge \psi \wedge \chi$ is derived from these. For the differences between the measured degrees of belief in φ, ψ given φ, and χ given $\varphi \wedge \psi$ and what (we propose) you should assume to be the true values of those degrees of belief are all within what according to assumption 2 of Section 3 is the measurement error for basic degrees of belief, as can immediately be seen from Figure 4.1 above; and the difference between your measured degree of belief in the conjunction and the assumed true degree of belief in that proposition is, given how the measurement error for derived degrees of belief is to be determined, necessarily within the measurement error associated with your reported degree of belief in this conjunction. That is, given the further assumption about the statuses of your degrees of belief, it is entirely in accordance with our assumption regarding the inaccuracy of the standard device for measuring degrees of belief to assume that your true degrees of belief are the ones we just suggested.

Now a second question is whether this further assumption can be legitimately made. It is clear that if, for instance, for some reason you had to assume that your degree of belief in $\varphi \wedge \psi \wedge \chi$ is basic, the above assumption about what your true degrees of belief are could not be warranted by an appeal to our assumption regarding measurement errors. After all, the difference between your measured and your assumed true degree of belief in the conjunction exceeds ε, the measurement error for basic degrees of belief. Recall, however, that it is part of assumption 1 of Section 3 that the status of degrees of belief is not transparent: calculating one probability on the basis of another does not imply that the former cannot be basic, we said. Hence, even if you have calculated $p(\varphi)$ partly on the basis of $p(\varphi \wedge \psi \wedge \chi)$, that does not prohibit you to assume the former to be basic and the latter to be derived. The answer to the second question thus is that it is legitimate to assume that $p(\varphi)$, $p(\psi \mid \varphi)$, and $p(\chi \mid \varphi \wedge \psi)$ are basic and that $p(\varphi \wedge \psi \wedge \chi)$ is derived from them.

In short, the inconsistency in your state of mind can be avoided by reinterpreting the data of the measurements of your degrees of belief in a way that accords with the assumptions made in Section 3.

A few comments on this example are in order. First, it must be emphasized that it would be wrong to regard reinterpretation of the sort we here recommend as a kind of "throwing away" one's original degrees of belief in order to (perhaps quite opportunistically) substitute others for them. What happens in this sort of reinterpretation is that one possible interpretation of the data concerning one's betting behavior – possible in the sense that it does not violate assumption 2 of Section 3 – that makes one come out inconsistent given one's adherence to RCP and NT is substituted by another possible interpretation of those data that *is* consistent with the combination of RCP and NT.

Second, there may seem to be an obvious objection to the example, which can be put as follows: "So it is *possible*, in the above case, to reinterpret oneself in such a way that an inconsistency is avoided, but why *should* one do so? If reinterpretation is not mandatory, and I, for one, should refuse to do so whenever I find myself in a situation comparable to the one sketched in the example, then RCP *can* lead to paradox after all." To see why this objection fails, recall the venerable tradition in the philosophies of language and action that emphasizes the importance of charity in interpretation.[28] In more precise terms this view says that in interpretation we should be maximally charitable, that is, we should try to make the interpretee come out as favorable as the data permit. Presumably this applies no less when the interpretee is oneself and the data concern one's own (betting) behavior – certainly it is not less important to be able to make sense of oneself than it is to be able to make sense of others. If this is correct, then since (i) in Example 4.1 there is an interpretation on which you are *not* inconsistent (in fact, there are infinitely many such interpretations), (ii) an interpretation of yourself on which you come out as being consistent is undeniably a more favorable interpretation than one on which you come out as being inconsistent, and (iii) there exists a perfectly good explanation for the divergencies between the degrees of belief involved before and after reinterpretation (namely, the inaccuracy of measurements of degrees of belief), charity seems to require that you do interpret yourself as having a degree of belief in φ of 0.995 as well as conditional degrees of belief in, respectively, ψ given φ, and χ given φ and χ, of 0.998 – or, of course, as having any other degrees of belief that will yield a probability greater than **t** for the conjunction and that are consonant with the assumption concerning measurement errors.

Third, while the role assumptions 1 and 2 of the previous section play in the example will be sufficiently clear, it may be worth pointing out why the third assumption, that is, that $\mathbf{t} \geqslant 1 - \varepsilon$, is crucial, too. Suppose that, contrary to this assumption, the threshold were 0.5 (but still $\varepsilon = 0.01$; the following can easily be generalized to other values for **t** and ε such that $\mathbf{t} < 1 - \varepsilon$). Suppose further that your measured degrees of belief in φ, ψ given φ, and χ given $\varphi \wedge \psi$, are not as indicated in the example, but all equal 0.55. Then if your degree of belief in $\varphi \wedge \psi \wedge \chi$ is to be greater than the threshold, you must assume that the true value of at least one of $p(\varphi)$, $p(\psi \mid \varphi)$, and $p(\chi \mid \varphi \wedge \psi)$ exceeds 0.79. But if you assume these degrees of belief to be basic, the assumption that our method for measuring degrees of beliefs is for such degrees of belief only precise up to 0.01 can no longer account for the difference between your measured degrees of belief and the degrees of belief that, after charitable reinterpretation, are taken to be your actual degrees of belief. And there is no guarantee that the assumption that at least two of them are derived can account for a divergence of more than 0.24 (= 0.79 – 0.55) between measured and reinterpreted degrees of belief.[29]

It is easy to see that for the same reason we require that $\mathbf{t} \geqslant 1 - \varepsilon$, our procedure for resolving inconsistency in genuine preface cases does not work for non-genuine preface cases. If, in Example 4.1, φ, ψ, and χ had constituted a non-genuine preface case, there might not be any reinterpretation of your measured probabilities for φ and/or ψ conditional on φ and/or χ conditional on φ and ψ that would assign new conditional probabilities within the measurement error and that yet would result in a new probability for the conjunction that is above **t**. Hence, our case for RCP does not extend to a case for CP.

Fourth, if at time t you have degrees of belief other than those referred to in the example (given the first assumption of Section 3, you *need* not have), you may have to make changes in these degrees of belief, too, in order to maintain probabilistic coherence (e.g., if $p(\varphi)$ is reinterpreted, then $p(\neg\varphi)$ must be changed accordingly – provided you at t have a degree of belief in $\neg\varphi$). Clearly, it will always be possible to explain the divergences along the above lines; they will always be within the measurement error, given suitable assumptions about the (basic/derived) status of the various propositions involved. But one may wonder whether such further reinterpretation may not lead to a change in the status, *qua* rationally acceptable, of certain propositions, that is, whether it could not happen that a proposition that was rationally acceptable before reinterpretation ceases to be so upon reinterpretation.

The answer is that this may indeed happen, and that this is exactly as it should be. When we introduced the Preface Paradox we said that it is possible for the conjunction of propositions that by virtue of CP would count as rationally acceptable to not just have a probability not exceeding $\mathbf{t}$ but to even have a probability below $1-\mathbf{t}$, so that the negation of the conjunction, depending on whether it satisfies the other necessary conditions for rational acceptability, might be rationally acceptable. It goes without saying that, if it was rationally acceptable before reinterpretation, then it should no longer be so after reinterpretation lest we still end up with a contradiction. And of course, after reinterpretation the negation of the conjunction cannot possibly have a probability above $\mathbf{t}$; its probability must even be below $1-\mathbf{t}$ given that reinterpretation will give the conjunction a probability above $\mathbf{t}$.

Our final comment concerns a worry about chances. Suppose φ in Example 4.1 is the proposition that the chance that ticket #i (for some particular i) in a given lottery will not win equals 0.992, and ψ the proposition that ticket #j in the same lottery will not win. Then although it is *possible* to reinterpret your degree of belief in ψ conditional on φ really being equal to 0.998, doing so would seem to be *inappropriate*. For it is reasonable to assume that even in corrective reinterpretation you will want your degrees of belief to accord with Lewis' (1980) Principal Principle, which, very roughly, says that one's degree of belief in φ given that the objective probability or chance of φ equals x, should equal x. Here again the question is relevant what other conditions besides high probability are to be imposed on rational acceptability. It may be that chancy propositions, that is, propositions that are a matter of chance relative to the belief system of the person involved in the preface case, do not meet these conditions (and there are independent reasons to believe that this may be the case; cf. the digression on the Lottery Paradox towards the end of Section 2). Also, the Principal Principle is not beyond every doubt and in any event seems to lack a proper justification (Strevens, 1999). Perhaps it is arguable, then, that it does not really impose any restriction on corrective self-reinterpretation. But if neither of the foregoing, then it may be necessary to restrict RCP further, for instance, to genuine preface cases not involving chancy propositions. For obvious reasons, this is not a matter that can be fully settled here and now.

Example 4.1 only shows that RCP can be consistently applied in *some* genuine preface cases given *some* values for $\mathbf{t}$. However, the example can be generalized to all genuine preface cases and to all values for $\mathbf{t}$:

PROPOSITION 4.1. Let $\langle S, p(\circ), \Phi, t\rangle$ be a genuine preface case, with $\Phi = \{\varphi_1, \ldots, \varphi_n\}$, and let S's measured degrees of belief in $p(\varphi_1)$, $p(\varphi_2 \mid \varphi_1)$, ..., $p(\varphi_n \mid \varphi_1 \wedge \cdots \wedge \varphi_{n-1})$ be, respectively, $\mathbf{p}_1, \ldots, \mathbf{p}_n$. Further suppose that $\mathbf{t} \geqslant 1 - \varepsilon$, with $0 < \varepsilon < 1$. Then S can reassign a value to each of $p(\varphi_1)$, $p(\varphi_2 \mid \varphi_1)$, ..., $p(\varphi_n \mid \varphi_1 \wedge \cdots \wedge \varphi_{n-1})$ so that S comes out as respecting NT and RCP as well as probability theory, that is, S can rationally accept the conjunction of the propositions in Φ without violating probability theory or NT, and S can do so in a way that lets each newly assigned value be within the measurement error that, given assumptions 1 and 2 of Section 3, can be associated with the corresponding measured value.

Proof: It is a simple arithmetical truth that for all $a \in \mathbb{R}$ such that $0 < a < 1$, and for every $n \in \mathbb{N}$, there are $b_1, \ldots, b_n \in \mathbb{R}$ such that: (i) for each b_i: $a < b_i < 1$, and (ii) $\prod_{i=1}^{n} b_i > a$. It follows from this that with every $\mathbf{p}_i$ a real number b_i can be associated that is strictly between $\mathbf{t}$ (whatever its precise value) and 1 so that $\prod_{i=1}^{n} b_i > \mathbf{t}$. Hence S can reinterpret $p(\varphi_1)$, $p(\varphi_2 \mid \varphi_1)$, ..., $p(\varphi_n \mid \varphi_1 \wedge \cdots \wedge \varphi_{n-1})$ in such a way that $p\big(\bigwedge_{i\leqslant n} \varphi_i\big)$ comes to have a value exceeding $\mathbf{t}$. Furthermore, since $\langle S, p(\circ), \Phi, t\rangle$ is a genuine preface case, $|\mathbf{p}_i - b_i| \leqslant \varepsilon$ must hold for each of the numbers b_i. Since, given the second part of assumption 1 of Section 3, nothing precludes S from assuming $p(\varphi_1)$, $p(\varphi_2 \mid \varphi_1)$, ..., $p(\varphi_n \mid \varphi_1 \wedge \cdots \wedge \varphi_{n-1})$ to be all basic and $p\big(\bigwedge_{i\leqslant n} \varphi_i\big)$ to be derived from the former, all the new values are within the measurement errors that, given the latter assumptions, are to be associated with the measurements of the respective degrees of belief.

Proposition 4.1 says that in genuine preface cases there always exists an assignment of probabilities such that (i) it makes the agent come out consistent, and (ii) it diverges no more from the function representing the agent's reported degrees of belief than can be accounted for by making suitable assumptions about the status (*qua* basic/derived) of, and hence the measurement error to be associated with, the various degrees of belief. However, for reinterpretation such as is to be undertaken in these cases to be possible it is insufficient that there *exists* such an assignment; the agent must be able to find it. More than that, there must be an easy-to-use method that enables the agent to find at least one such assignment in a relatively short period of time. It is generally held that a major motivation for maintaining an epistemology of rational acceptability next to a probabilist epistemology is that the former is practically indispensable to such utterly limited beings as we are.[30] If the agent were then only capable of finding an appropriate reinterpretation *in principle* – where for instance in practice even a super-fast computer would need more than the lifetime of our universe to find an assignment of the requisite sort – our attempt to salvage as much as possible of CP would clash badly with the general purpose of the project of developing a consistent theory of rational acceptability.[31] It thus seems that, if the kind of reinterpretation we recommend were not generally possible without a considerable expenditure of effort on the part of the agent, we would still be better off with the standard solution to the Preface Paradox that sacrifices CP unconditionally.

Now for cases such as the one in Example 4.1, in which n is small, it will never be too hard to find a new assignment that fits the bill; generally a little bit of tinkering will do. But – one may wonder – will this remain feasible for exceedingly large n? It will, because there is an extremely easy algorithm for finding a reinterpretation of the sort that is required. Given a genuine preface case involving you and $\varphi_1, \ldots, \varphi_n$, proceed as follows. First, pick an $a \in \mathbb{R}$ such that $\mathbf{t} < a < 1$ (since $\mathbf{t} < 1$, such a's exist) and reinterpret yourself by assigning a probability of $\sqrt[n]{a}$ (or higher) to each of φ_1, φ_2 given φ_1, φ_3 given $\varphi_1 \wedge \varphi_2$, and so on, up to and including φ_n given $\varphi_1 \wedge \cdots \wedge \varphi_{n-1}$.[32] Assume your degrees of belief in these "propositions" to be basic. Any divergence between the measured values and $\sqrt[n]{a}$ (or even any higher value) is necessarily in the measurement error.[33] Finally, take the product of all the reinterpreted degrees of belief and assume that to be your true degree of belief in $\bigwedge_{i \leqslant n} \varphi_i$, where you further assume the latter to be derived from $p(\varphi_1)$, $p(\varphi_2 \mid \varphi_1), \ldots, p(\varphi_n \mid \varphi_1 \wedge \cdots \wedge \varphi_{n-1})$. Then the divergence between your measured degree of belief in the conjunction and the newly assigned degree of belief will be in the measurement error, too, as can easily be verified.

As adumbrated in Section 2, an even less restrictive principle than RCP is defensible. That principle can, in effect, be more or less read off from the proof of Proposition 4.1. Call $\langle S, p(\circ), \Phi = \{\varphi_1, \ldots, \varphi_n\}, t\rangle$ an *ε-preface case* precisely if it is a preface case and in addition it holds that $\prod_{i=1}^{n}\left[p(\varphi_i \mid \varphi_1 \wedge \cdots \wedge \varphi_{i-1}) + \varepsilon\right] > \mathbf{t}$. Evidently, every genuine preface case is an ε-preface case, but not vice versa; if $\langle S, p(\circ), \Phi, t\rangle$ is an ε-preface case, then it may be that, for some or even all $i \leqslant n$, the person S at t has a measured degree of belief in φ_i, conditional on one or more of the other members of Φ, that is equal to or below $\mathbf{t}$ – in which case $\langle S, p(\circ), \Phi, t\rangle$ clearly is not a genuine preface case. It is equally evident that what we called corrective reinterpretation works for prima facie paradoxical ε-preface cases in general, and not merely for the subclass of prima facie paradoxical genuine preface cases; a little reflection will show that everything said in the proof of the previous proposition applies to ε-preface cases as well. Thus, we need only restrict conjunctive closure to ε-preface cases in order to obtain a principle that is consistent with both probability theory and NT. How much of a strengthening of our result this is, is hard to say. As we said earlier, there is currently no way of telling whether there exist any preface cases that are not also genuine preface cases. If there are no such cases, then *a fortiori* all ε-preface cases are also genuine preface cases. And so, a conjunction principle restricted only to ε-preface cases may materially amount to the same as one that is restricted to genuine preface cases.[34]

In closing, we want to say a few words about the infinite analogue of the Preface Paradox. In this paper we have only considered cases in which the sets of propositions found to be rationally acceptable were finite. And, indeed, preface cases were so defined as to involve only finite sets of propositions. But now let there be an infinite book, containing infinitely many claims, all of which the author finds rationally acceptable, and all of which are mutually probabilistically independent relative to the author's credal state. In first-order logic it makes no sense to speak of the conjunction of all these claims, but it does in an infinitary logic such as $\mathcal{L}_{\omega_1\omega}$, which allows us to form infinite conjunctions and disjunctions (Ebbinghaus et al., 1984, pp. 136–141). So, for now, assume such a logic. It is clear enough that the probability of the infinite conjunction of the claims made in the book cannot but equal 0,[35] and also that no amount of corrective reinterpretation could change that. This is not a problem for RCP, which (just like CP) only allows us to conclude to the rational acceptability of certain *finite* sets of rationally acceptable propositions. But it would be in line with our assumption of an infinitary logic to consider a strengthened version of RCP which sanctions the conclusion that the infinite conjunction $\bigwedge_{i<\omega} \varphi_i$ is rationally acceptable to S at t if each proposition in $\Phi = \{\varphi_i \mid i \in \mathbb{N}\}$ is, provided $\langle S, p(\circ), \Phi, t\rangle$ does not constitute a non-genuine preface case for S at t (Definitions 2.1 and 2.2 would have to undergo some slight and obvious changes to make the term "preface case" apply to cases involving infinite sets of propositions). And given such an infinitary principle, we have our paradox again.

We expect that many will respond to this that we should simply refuse to pay serious attention to the infinitary analogue of RCP. Such a principle is not effectively applicable and thus sits badly with what we just said to be a main motivation for having an epistemology of rational acceptability, namely, that given the limitations we happen to be subject to, we just cannot afford to do without it. And there is a clear tension between that purely pragmatic motivation and pretending, even if just for the sake of the argument, that the infinitary version of RCP *is* effectively applicable. This may well be the reason that usually treatments of the Lottery and Preface Paradoxes only deal with the finite versions of these paradoxes.

A tactically better response may be to point to the fact that if the regularity requirement is given up, we can assign probability 1 to all the conjuncts, whereby the conjunction would get probability 1 as well, and to maintain that, *if* the infinite Preface Paradox is found worthy of our attention, it is entirely legitimate to violate the regularity requirement, *as the infinite version of the paradox itself already violates the regularity requirement:* the infinite conjunction in the paradox has probability 0, in spite of the fact that – we can assume without loss of generality – it is not a logical or mathematical falsehood.

But since, as just intimated, the problem broached here leaves RCP itself unscathed, and thus does not conflict with anything argued for in this paper, we shall leave it to the reader to decide which way to take in the infinite version of the Preface Paradox.

5. Conclusion

Authors who in response to the Preface Paradox have proposed to repudiate CP, have overreacted. In this paper it was shown that for an important class of cases in which it prima facie seems that rational acceptability can be closed under conjunction only at the cost of engendering paradox, it can, given three fairly plausible assumptions, be consistently closed under conjunction after all. It thus suffices to impose a rather moderate restriction on CP in order to solve the Preface Paradox. And, as we speculated before, further work on the paradoxes related to the notion of rational acceptability may well show that the moderate restriction actually effects no restriction at all.

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NOTES

[1] Some authors relate rational acceptability not to subjective probability *per se* but to *rational* subjective probability (Foley, 1992, p. 112). According to them, a proposition is rationally acceptable to a person if/only if/iff it is rational for her to assign to it a subjective probability exceeding the threshold. However, none of the claims made in this paper hinges on that distinction (so one should be able to substitute "rational subjective probability" for "(subjective) probability" *salva veritate* throughout the paper).

[2] It is worth noting that, as Makinson (1965) presents it, the Preface Paradox involves the qualitative notion of rational belief only (and not that of degree of belief, that is). Nevertheless, Makinson's paradox can easily and quite naturally be phrased in such a way that it exhibits a problem for the combination of CP and NT, the latter of which of course does involve the notion of degree of belief. This is also how most later authors have understood the Preface Paradox. Clearly, we are joining these later authors. Does the solution to the Preface Paradox we are to offer also solve Makinson's paradox? That depends on how one interprets the notion of rational belief in his paper (which is used only informally there). If it is interpreted as complying with NT, so that in order to qualify as being rationally held a belief must be believed to a degree exceeding **t** (and nothing in Makinson's paper seems to preclude that we do so interpret the notion), then our solution does solve the original paradox, too.

[3] The term is from Moser and Tlumac (1985, p. 135), who explicitly present the Preface Paradox as being due to this problem. Olin (1989) calls it the fallibility argument for inconsistency. She takes it to be a generalized version of the Preface Paradox (p. 101, n. 3). However, at the end of her paper she distinguishes it from "an argument for inconsistency ... based on application of the probability calculus" (p. 100); this latter problem, though dealt with only very briefly in her paper, comes close to what we below shall take to be the Preface Paradox.

[4] The same goes for the remark in the earlier formulation of the paradox about the appendices that might be added to your book. It might be, then, that in that formulation by "ambitious" Foley just meant to express that in the book you make a great number of claims. On the other hand, if it were sufficient for a book to contain a great number of claims to count as ambitious, then your local telephone book would be ambitious, too.

[5] See, e.g., Kemeny (1955), Jeffreys (1961), Stalnaker (1970), Carnap (1971), and Appiah (1985).

[6] See Howson (2000, p. 134f) and Hájek (2003) for some criticisms.

[7] Cf. Makinson (1965), Kyburg (1970, 1990, 1997), Foley (1979, 1992), Klein (1985); Moser and Tlumac's (1985) solution amounts to an indirect repudiation of CP. Kaplan (1981a, b) and Maher (1993), on the other hand, have argued that it is not CP but NT that has to go in the light of the Preface and Lottery Paradoxes. The alternative they offer in its stead appeals to so-called cognitive decision theory. But, first, the concept of cognitive utility involved in this theory seems to be of rather doubtful standing (see, e.g., Goosens (1976); cf. Gillies (2000, p. 56f) for some reasons to be wary of the notion of utility in

general). Secondly, cognitive decision theory is an extension of Bayesian decision theory, a theory that is problematic in its own right; see, e.g., Douven (2002a).

8 For instance, CP seems to be tacitly assumed in the well-known conjunction argument against scientific antirealism proposed in Putnam (1973) and further elaborated in Friedman (1983); cf. Douven (2002b).

9 We are assuming, as is generally done in discussions of the Preface Paradox, that only the fact that "high probability" is not closed under conjunction creates an obstacle to generally closing rational acceptability under conjunction. Without that assumption, the claim we are making in the text might be false: it could then happen that, for some necessary condition for rational acceptability other than high probability, all conjuncts satisfy it but the conjunction does not.

10 If the version of CP we are about to propose is adopted, there is not really a Lottery *Paradox* anymore. This version excludes conjunctive closure of rational acceptability for non-genuine preface cases, and, as is easily seen, given a fair lottery with n tickets, with $(n-1)/n > \mathbf{t}$, the set of propositions saying that ticket #1 will not win, ticket #2 will not win, ..., ticket #n will not win, cannot constitute a genuine preface case relative to the belief system of a person informed about the conditions of the lottery (just consider that the probability of any of the tickets losing conditional on all the others losing equals 0 and thus is not above $\mathbf{t}$). So, given that restricted principle, the conjunction of these propositions is not rationally acceptable. But although thereby the contradiction is evaded, many authors have argued that independently of the paradox it is unintuitive to say that the conjuncts separately can be rationally acceptable (see, among others, Kaplan (1981a, b), Stalnaker (1984), Lehrer (1990), Maher (1993), Ryan (1996), Nelkin (2000), and Douven (2002b)). Hence, if the intuitions of these authors are reliable, the hope is still justified that the further conditions on rational acceptability cull the class of non-genuine preface cases.

11 The two claims made in the book constitute what in Douven (2002b) is called a Probabilistically Self-undermining Set (PSS). If "non-PSS-membership" is adopted as a further necessary condition for rational acceptability – as would seem defensible on the ground of certain arguments given in the paper just mentioned – that indeed precludes that either of the two claims made in the book is rationally acceptable.

12 See van Fraassen (1989, Ch. 12), Uffink (1995), and Gillies (2000, Ch. 3) for surveys of the problems that beset the Principle of Indifference.

13 Which is not to say it is unproblematic. See Foley (1990) for an excellent discussion of some of the more disputable features of the position.

14 In formal terms, probabilists standardly take a rational agent's belief state to be representable by a probability function whose domain is the field, or even the σ-field, generated by the atomic propositions of the agent's language. Fine (1973, p. 63ff), as an exception, considers some weaker conditions on the domain of the probability function, like for instance that it be a Λ-field, i.e., a collection of propositions that includes the necessary proposition and is closed under negation and under countable disjunction of mutually exclusive propositions. On our view, this condition is still too strong; see below in the text.

15 See in the same vein Edgington (1995, p. 266f). Of course even probabilists will acknowledge as much. The assumption that we already "from the beginning" assign a degree of belief to every proposition is mostly regarded as a useful idealization (Sobel, 1987, p. 68). While we do not want to deny the potential usefulness of the assumption, we believe that if the Preface Paradox is due to an otherwise useful idealization, this is worth pointing out.

[16] Note that this implies that the domain of the function representing an agent's degrees of belief need not even be closed under negation. One may assign a (basic or derived) probability to a given proposition, but not have derived from this the probability of the negation of that proposition.

[17] For a (rare) dissenting opinion, see Gillies (2000, Ch. 9), who thinks operationalism is still the correct view of measurement for the social sciences and thus also for psychology. However, this commitment to operationalism seems to be at odds with his comment on Ramsey's (1926, p. 161) remark that "it is ... conceivable that degrees of belief could be measured by a psychogalvanometer or some such instrument". For after citing this remark, Gillies (2000, p. 53f) goes on as follows: "Ramsey's psychogalvanometer would perhaps be a piece of electronic apparatus something like a superior lie detector. We would attach the electrodes to Mr B's skull, and, when he read out a proposition describing the event E in question, the machine would register his degree of belief in that proposition. Needless to say, even if such a psychogalvanometer is possible at all, no such machine exists at present ... ." This passage clearly suggests that Gillies wants to leave open the possibility that degrees of belief are brain states or something else to be found inside our skulls. In any case they are not simply dispositions to engage in certain bets. See Leahey (1980) and Green (1992) for general critical discussions of operationalism in psychology.

[18] Here too not everyone agrees. For instance, Maher (1993) still endorses an instrumentalist view on degrees of belief; according to him degrees of belief form, together with utilities, "a device for interpreting a person's preferences" (p. 9).

[19] It is here assumed that $a - \delta \geqslant 0$ and $a + \eta \leqslant 1$. If not, then the real degree of belief is assumed to be in the interval $[0, 1] \cap [a - \delta, a + \eta]$. This proviso applies to all intervals of degrees of belief mentioned below in the text.

[20] This fact is often taken to signify that many of our degrees of belief are vague, and are therefore better represented by probability intervals than by sharp probabilities (see for instance Hájek (1998, 2003)). For present purposes nothing turns on whether this is correct or not. The reason for associating a measurement error with measuring sharp degrees of belief would also be a reason for associating a measurement error with the measurement of upper and lower bounds on degrees of belief. That means that our solution to the Preface Paradox would work as well on the assumption that our degrees of belief are often vague, assuming that NT is read as saying that a proposition is rationally acceptable only if it is assigned a probability interval the lower bound of which is above **t** – and, as far as we can see, this is the only plausible reading of NT given a version of probabilism that countenances vague probabilities – and provided "probability/ies" in the remainder of the paper is read as "lower bound(s) of probability interval(s)."

[21] Some might also worry that the measurement error for basic degrees of belief is not really constant over the [0, 1] interval. More specifically, the worry might be that agents are better able to home in on their true degrees of belief the closer the values of these degrees of belief are to one of the extremes 0 and 1. After all – it might be said – a degree of belief of, say, 0.9999 (0.0001) in some proposition may commit a person to risk all, or nearly all, of his or her entire fortune in betting on (against) that proposition, just to gain 1 cent. However, this worry can easily be put to rest by following Gillies (2000, p. 55ff) and assuming that, if a bet on (or against) a proposition is at all to give a reliable indication of a person's degree of belief in that proposition, then the size of the stake will have to be so chosen that it is small in relation to the person's total wealth.

[22] For example, Kaplan (1981b, p. 308), Moser and Tlumac (1985, p. 128), and Kyburg (1990, p. 64) mention 0.9; Foley (1992, p. 113) mentions 0.99.

[23] This paper assumes, like virtually all discussions of the Lottery and Preface Paradoxes, that **t** has some fixed value that is the same for every rational person. A noteworthy, different approach is proposed in Hawthorne and Bovens (1999). There it is shown how and under what conditions we can on the basis of a person's qualitatively expressed doxastic states determine bounds on the threshold value for rational acceptability *for that particular person*.

[24] The only specific assertion about **t** that seems to be obviously correct is that **t** must equal at least 0.5, for only that ensures that a proposition and its negation cannot both be rationally acceptable to the same person at the same time. Note that it would seem wrong to go further and claim that **t** *exactly* equals 0.5 as Achinstein (2001, p. 156) does. For 0.5 seems intuitively just to be too low as a threshold for rational acceptability. Suppose that φ and $\neg\varphi$ both satisfy all necessary conditions for rational acceptability except, perhaps, the condition imposed on rational acceptability by NT, and suppose $p(\varphi) = 0.50000001$ and thus $p(\neg\varphi) = 0.49999999$. If it were the case, then, that $\mathbf{t} = 0.5$, φ would be rationally acceptable and $\neg\varphi$ would not be, even though the probability of the latter is only negligibly lower than that of the former. This would already be intuitively odd if measurements of degrees of belief were entirely exact, but it is simply unacceptable if, as we assume, with any measurement of a degree of belief a measurement error must be associated – so that in the example it might hold for the *true* degrees of belief that $p(\neg\varphi) > p(\varphi)$.

[25] Just as well as on any other value in the interval [0.5, 1), that is; see the preceding footnote.

[26] It might seem that an argument to the contrary is readily available, given that, by abandoning NT, Kaplan's and Maher's solutions to the paradox manage to save CP unabridgedly and without having to make any special assumptions concerning the threshold. However, for reasons given in note 7 we find these solutions disputable. And so far no solutions have been put forward that allow us to maintain both NT and CP (or an attenuated version of CP, such as RCP) in the face of the Preface Paradox.

[27] Scare-quotes are used here because it is controversial whether statements of the form "$\varphi \mid \psi$" express propositions. As Lewis (1976) has famously shown, they cannot do so under certain, according to him plausible, conditions. For more on this, see (among many others) van Fraassen (1976), Appiah (1985), McGee (1989), and Edgington (1995, 2001).

[28] Most explicitly in Wilson's (1959) Principle of Charity and in Putnam's (1975) Principle of the Benefit of Doubt.

[29] Note that the claim is *not* that that assumption could not *possibly* account for such a divergence. That would in fact be false. Suppose for instance that $p(\varphi)$ is not basic, and suppose that $\varphi \equiv \bigvee_{i \leqslant 25} \zeta_i$, where $\zeta_1, \ldots, \zeta_{25}$ are propositions expressible in your language and such that $\zeta_i \vdash \neg\zeta_j$ if $i \neq j$. Further suppose that you have a measured degree of belief of, say, 0.023 in each ζ_i. Then if you assume that your degrees of belief in all these propositions are basic and that $p(\varphi)$ is derived from them, the true value of $p(\varphi)$ is between $25 \cdot (0.023 - 0.01)$ and $25 \cdot (0.023 + 0.01)$ (for given the assumption of mutual exclusiveness of the ζ_i's, $p(\varphi) = \sum_{i=1}^{25} p(\zeta_i)$); that is, it is in the interval [0.3, 0.825]. Hence in that case it is possible to reinterpret yourself as having a degree of belief exceeding 0.79 in φ in a way that is justified by the presumed inexactness of measuring degrees of belief. What matters here, however, is that, given assumption 1 of Section 3, you need not have degrees of belief in any of $\zeta_1, \ldots, \zeta_{25}$ at all, nor need you have other degrees of belief from which $p(\varphi)$ can be derived in a way consistent with our second assumption. In brief, there is (as we said) no *guarantee* that in the above case – the case in which it is assumed that $\mathbf{t} < 1 - \varepsilon$ – charitable reinterpretation is possible.

Strictly speaking, the problem, at least in general (and not so much in the rather simple case considered here), is not just that no reinterpretation of the requisite kind is guaranteed to exist if $\mathbf{t} < 1 - \varepsilon$, but also that, if one does exist, there is no guarantee that it can be found by means of some practically applicable method. The importance of this point will become clearer below, where it will also be seen that, if it *is* assumed that $\mathbf{t} \geqslant 1 - \varepsilon$, an easily and generally applicable method for finding a consistent reinterpretation in genuine preface cases is available.

[30] See, for instance, Harman (1986, p. 22), Kyburg (1990, p. 64), Foley (1992, 122), and Weintraub (2001).

[31] More formally, it is insufficient that the problem of finding an assignment of probabilities of the right kind is recursive. It is even insufficient that it is tractable or feasible in the sense in which these words are used in computational complexity theory, for even a tractable problem (in that sense) may be completely unmanageable to us in practice.

[32] If we set aside the regularity requirement, the algorithm becomes even easier: Assume $\varphi_1, \ldots, \varphi_n$ to all have a basic probability and assume that $p(\varphi_i)$ *actually* equals 1 for all i with $1 \leqslant i \leqslant n$. Then the conditional probability of each of these propositions given any other propositions will equal 1 as well, and, hence, so will the probability of their conjunction.

[33] Here of course the assumption that $\mathbf{t} \geqslant 1 - \varepsilon$ is vital – hence our remark in note 29.

[34] We owe the generalization of our result to ε-preface cases to Luc Bovens. He made the further interesting suggestion that, while we have proceeded under the assumption that ε has a fixed value for all of us (see Section 3.2), we could give up that assumption and draw a wholly different moral from the core elements of our solution to the Preface Paradox. Since it seems arguable that it typically is not accessible to us how good we are in assessing our probability assignments, i.e., what our "personal ε value" is, we could, if we find ourselves in a preface situation, calculate our ε value, or at least some bound on it, on the basis of the beliefs we find rationally acceptable. The suggestion certainly merits further exploration.

[35] It should be noticed that this assumes Countable Additivity. This axiom has become standard among mathematicians, who take it to be "justified by its success" (Halmos, 1974, p. 187). Philosophically, however, Countable Additivity has a much more problematic status. Intuition seems not decisive either for or against it; adopting it as an axiom has counter-intuitive consequences – like for instance that there cannot be a fair lottery with a countably infinite number of tickets (this seems to have been de Finetti's main reason for rejecting Countable Additivity (de Finetti (1970, p. 351)) – but not adopting it has counter-intuitive consequences as well, such as that all tickets in an infinite lottery can have a zero probability of winning while at the same time there will with unit probability be a winner (cf. also (Earman, 1992, p. 60f)).

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