2016

In Defense of a Contextualized Suppositional Account of Conditional Credence

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May 23, 2016

ABSTRACT

Conditional credence is an important concept in many areas of philosophy. However, little consensus has been achieved regarding to its semantics and ontology. In this thesis I shall sketch a contextualist suppositional account of conditional credence by drawing insights from two seemingly disjoint debates: the foundational debate on the relationship between conditional and unconditional probabilities, and the semantic debate on the relationship between conditional probability and probabilities of conditionals. I argue that, for a given pair of propositions A and B, the conditional credence one ought to have for B given that A - $P(B|A)$ - may depend on contextual parameters like the way in which we mentally represent the extensions of as well as the stochastic relationships between A and B.

ACKNOWLEDGEMENTS

First and foremost, I wish to express my sincere gratitude to my thesis advisor, Professor Catherine Wearing. This thesis would not have been possible without her invaluable guidance, support and incredible patience and generosity with her time.

Secondly, I would like to thank Professor Jonathan Tannenhauser, Professor Corinne Gartner, Professor Justin Khoo, Professor Vann McGee, Professor Stephen Yablo and Professor Augustin Ryo for their helpful discussions and insights.

I also want to express my special appreciation to Claudia Yau and Beba Cibralic for their much appreciated support, encouragement and company. I am very lucky to have them both as my thesis buddies.

Lastly I want to thank all my friends, my professors and my parents for their support at various stages of my thesis. The conditional probability of me doing what I have done now given that I had to do it alone, is zero in all epistemically possible scenarios.

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Part I

INTRODUCTION

According to the subjective theory of probability, probabilities are degrees of partial credence. For a subject S, the probability that a coin lands on head is $1/2$ just in case her degree of certainty that the coin will land on head is half as much as her degree of certainty that the coin will land on heads or tails.[1] On this account, conditional probabilities measure conditional credence, which plays a vital role in analyses of inferential dispositions, suppositional reasoning and belief revisions. However, while the notion of conditional credence is widely used in many areas of philosophy, very little consensus has been achieved with respect to its meaning and ontology. What is conditional credence? How is conditional credence related to unconditional credence, if the distinction withholds? Are they two intrinsically different epistemic states, or is one supervenient on or reducible to the other? And what are the connections between conditional credence and unconditional credence in conditionals, apart from their orthographic similarity?

In this thesis, I shall sketch a preliminary account of conditional credence by looking into two ongoing debates concerning conditional probability: the foundational debate on whether conditional probabilities are reducible to unconditional probabilities, and the semantic debate on whether conditional probabilities are equivalent to probabilities of conditionals. Traditionally, these two debates have been treated as orthogonal to each other, partly because they effectively start off from two opposite sides of the same sequence of equalities:

$$\frac{P(AB)}{P(A)} = P(A \text{ given that } B) = P(\text{if } A \text{ then } B).$$

For the sake of brevity, I shall refer to the first equality

$$P(A \text{ given that } B) = \frac{P(AB)}{P(A)}$$

as THE RATIO and the second equality

$$P(A \text{ given that } B) = P(\text{if } A \text{ then } B)$$

1 I am being deliberately vague here by using the word "certainty", which could mean at least two epistemic attitudes: belief or acceptance. While the distinction between belief and acceptance is controversial and, to my knowledge, does not feature very much in the literature of Bayesian epistemology, those who do respect the difference generally regard acceptance as requiring more evidential support but involving less absolute certainty than belief. For example, a scientist may accept the quantum field theory without actually believing that there are no particles, only fields. On the other hand a student may believe that her answer to an exam question is right, but would only accept that this is in fact the case she double-checks with the book afterwards. For more discussion on this distinction, see [Schwitzgebel, 2015]. For the rest of my thesis I shall join the mainstream and use belief and acceptance (and correspondingly, believability and acceptability) interchangeably.

as THE THESIS. Those who care about the relative analytical primitiveness of conditional and unconditional probabilities often assume that there is an intimate connection between conditional probability and probabilities of conditionals. On the other hand, those who are invested in explicating the semantics of indicative conditionals would suppose that the concept of conditional probability is well-defined, usually in terms of the ratio formula $P(B|A) = P(AB)/P(A)$. As a result, the point of contention for one debate is taken for granted by the other, and what each side ends up showing is therefore the compatibility of THE RATIO and THE THESIS, rather than the tenability of each equality all by itself.

I contend that, if we constrain ourselves to the subjectivist framework, then we can break out this loop and address both questions simultaneously by a contextualist suppositional account of conditional credence. Specifically, according to this account, conditional probabilities are not reducible to unconditional probabilities, but conditional probabilities are analytically equivalent to unconditional probabilities of conditionals.

The thesis is structured as follows: in §1 I present an "omission" argument against the ratio analysis of conditional probabilities given in [Hájek, 2003]. Hájek argues that THE RATIO leaves conditional probabilities undefined in cases where we seem to have well-defined intuitions about what they should be. This suggests that the notion of conditional probability is primitive and irreducible to unconditional probabilities. One way to counter Hájek's allegation is by extending the range of probability functions to include infinitesimal numbers. However, I argue that this solution would not completely stave off Hájek's objection in light of the argument of [Williamson, 2007]. Instead, I think the "omission" argument is predicated on ungrounded assumptions about conditional probabilities. In §2 I give a rundown of seven claims which Hájek takes to be the "basic facts about conditional probabilities." I argue that none of Hájek's seven claims hold universally across all possible propositions and probability distributions. So the ratio analysis is correct to "omit" such cases because there is insufficient contextual information given to pin down a unique value for the given conditional probability. Moreover, from this analytical perspective, the fact that the ratio analysis is guilty of "the sin of omission" counts *in favor of* rather than *against* Hájek's general thesis, namely that conditional probabilities are not reducible to or supervenient on unconditional probabilities.

Having foreshadowed the context-dependency of conditional probabilities in continuous settings, I shall generalize this observation to discrete cases by giving a semantic account of conditional probability within the subjectivist framework. In §4 I consider and dismiss the counterfactual account of conditional credence as proposed by [Lowe, 1996]. In §5 I propose a modified suppositional account of conditional probabilities that would stave off the objections to the counterfactual account and a more general worry raised in [Chalmers and Hájek, 2007]. Since the suppositional account gives the same semantics for "*B* given that *A*" and "if *A* then *B*," it predicts that THE THESIS is trivially true. So any objection to THE THESIS is therefore a challenge to the suppositional account and in §6 I defend my contextualized suppositional account against these challenges by appealing to a technical result from [Kaufmann, 2004].

Part II

CONDITIONAL PROBABILITY AND THE RATIO

The current orthodoxy in probability theory is Kolmogorov's axiomatization. According to Kolmogorov, the conditional probability of B given that A is given by

$$P(B|A) =_{\text{def}} \frac{P(AB)}{P(A)} \text{ (provided that } P(A) \neq 0)$$

Hájek argues that this definition of conditional probability is inadequate. The argument proceeds as follows:

1. THE RATIO leaves the value of $P(B|A)$ undefined whenever $P(A) = 0$.
2. There are meaningful propositions with zero probabilities.
3. $P(B|A)$ can be well-defined even if $P(A) = 0$.
4. So THE RATIO is inadequate.

Since 1 follows directly from THE RATIO, Hájek proceeds to validate premise 2 and 3. He first proves the necessary existence of "trouble spots" in an uncountable probability space:

(Four Horn Theorem) For any uncountable probability space, there are uncountably many "trouble spots" in the space for which the probability assignment must be zero, infinitesimal, vague or undefined.

Since the probability functions are presumed to be sharp and real-valued, it follows that for any uncountable probability space, there are uncountably many nonempty propositions that have probability zero.

Hájek then posits seven "basic facts" about conditional probabilities that he thinks are necessarily true for any proposition A and probability function P irrespective of the value of $P(A)$.[2] They are

I. $P(A, \text{given that } A) = 1$

II. $P(\neg A, \text{given that } A) = 0$

III. $P(T, \text{given that } A) = 1$, where T is a necessary truth (e.g. it is raining or it is not raining)

IV. $P(F, \text{given that } A) = 0$, where F is a contradiction (e.g. it is raining and it is not raining)

V. $P(\text{a fair coin lands on head, given that } A) = 1/2$

2 For purposes relevant to the particular goal of this thesis, I shall constrain myself to talking about probabilities of propositions. On this interpretation, the probability of a set S is the probability of the proposition that "a randomly selected point from the domain is an element of S," and the probability of an event is the probability of its corresponding linguistic descriptions.

VI. Let x be a point selected from the interval $[0,1]$ with uniform probability distribution. Then

$$P(x = 1/4, \text{ given that } x = 1/4 \text{ or } x = 3/4) = 1/2$$

VII. Let x be a point randomly selected on the Earth's surface. Let W denote the Western hemisphere, and E the Equator. THen

$$P(x \in W, \text{given that } x \in E) = 1/2.$$

In all seven cases, THE RATIO predicts that the conditional probability is undefined when $P(A) = 0$ (or equivalently when A is a "trouble spot"), whereas our intuitions suggest that the conditional probability is in fact well-defined. So Hájek concludes that THE RATIO is guilty of "sins of omissions"- by reducing conditional probabilities to ratios of unconditional probabilities, it fails to capture certain sharp conditional probabilities that should be well-represented in the formal system.

However, there seems to be a quick way out: recall that Hájek's Four Horn Theorem says that for any uncountable probability space, there are uncountably many trouble spots for which the probability assignments are *either* zero, infinitesimal, vague *or* undefined. So what Hájek has really shown is that *if* we restrict ourselves to real-valued probability functions, then conditional probabilities cannot be defined or analyzed in terms of unconditional probabilities using THE RATIO. But there is no *a priori* prescription that says (subjective) probability functions must be real-valued. So the defender of THE RATIO may respond to Hájek's objection by simply enlarging the range of probability functions from $\mathbb{R}$ to a hyperreal field $\mathbb{H}$ such that every purported "trouble spot" receives an infinitesimal but non-zero probability. In this way, THE RATIO can be redeemed as follows:

If A is a trouble spot in our probability space, let $P(A) = \epsilon$ where $0 < \epsilon < \frac{1}{n}$ for all $n \in \mathbb{Z}_{\geq 1}$. Then

I. $P(A, \text{given that } A) = P(A)/P(A) = \epsilon/\epsilon = 1$

II. $P(\neg A, \text{given that } A) = 0/\epsilon = 0$

III. $P(T, \text{given that } A) = \epsilon/\epsilon = 1$, where T is a necessary truth

IV. $P(F, \text{given that } A) = 0/\epsilon = 0$, where F is a contradiction

V. $P(\text{a fair coin lands on head, given that } A) = (1/2)/\epsilon = 1/2$

VI. Let x be a point selected from the interval $[0,1]$ with uniform probability distribution. Then

$$P(x = 1/4, \text{ given that } x = 1/4 \text{ or } x = 3/4) = \epsilon/2\epsilon = 1/2$$

VII. Let x be a point randomly selected on the Earth's surface. Let W denote the Western hemisphere, and E the Equator. THen

$$P(x \in W, \text{given that } x \in E) = (\epsilon/2)/\epsilon = 1/2.$$

So introducing infinitesimal numbers seems sufficient for saving THE RATIO from Hájek's objection.

1

CRITIQUE OF INFINITESIMAL ANALYSIS

1.1 THE ETERNAL COIN

One of the strongest objection to the use of infinitesimal numbers is given in [Williamson, 2007]. Williamson argues that infinitesimal numbers cannot assign non-zero probabilities to *all* non-empty propositions. In particular, consider the following scenario: Suppose a fair coin is tossed infinitely many times. Let

$$
\begin{aligned}
H_1 &: \text{the coin lands on head} \\
H_0 &: \text{the coin lands on tail} \\
H_{1\ldots} &: \text{the coin lands on head on every toss since the first toss} \\
H_{2\ldots} &: \text{the coin lands on head on every toss since the second toss}
\end{aligned}
$$

Clearly $H_{1\ldots}$ is doxastically possible, in the sense that we can imagine a world in which this turns out to be true. However, if the probability measure is real-valued, then $P(H_{1\ldots}) = lim_{n\to\infty}(\frac{1}{2})^n = 0$. Note that $lim_{n\to\infty}(\frac{1}{2})^n = 0$ is true only if the field that we are working with has the Archimedean property - the property that for every $r \in \mathbb{R}_{\geq 0}$ there exists $n \in \mathbb{N}$ such that $\frac{1}{2^n} < r$.[1] Since $lim_{n\to\infty}(\frac{1}{2})^n < \frac{1}{2^n}$ for all $n \in \mathbb{N}$, it follows that in $\mathbb{R}$ (which is an Archemidean field), $\lim_{n\to\infty}(\frac{1}{2})^n = 0$. On the other hand, if we extend $\mathbb{R}$ to a hyperreal field $\mathbb{H}$ by adding hyperreal numbers ϵ such that $\epsilon < \frac{1}{2^n}$ for all $n \in \mathbb{N}$, then the fact that $lim_{n\to\infty}(\frac{1}{2})^n < \frac{1}{2^n}$ for all $n \in \mathbb{N}$ no longer implies that $lim_{n\to\infty}(\frac{1}{2})^n = 0$. So it would appear that we can distinguish between having an infinite sequence of heads from other straightforward impossibilities by stipulating that $P(H_{1\ldots}) = \epsilon > 0$ where ϵ is an infinitesimal number.

However, Williamson contends that even if we introduce the infinitesimals, we still have to assign 0 to $P(H_{1\ldots})$. The argument is given as follows:

1. $P(H_{1\ldots}) = P(H_1)P(H_{2\ldots}) = \frac{1}{2}P(H_{2\ldots})$

2. $P(H_{1\ldots}) = P(H_{2\ldots})$

1 This property is usually formalized as $(\forall r \in \mathbb{R}_{\geq 0})(\exists n \in \mathbb{N})[\frac{1}{n} < r]$. But these two statements are equivalent.

3. Let $P(H_{1\dots}) = x$. Then $x = 2x$. So $x = 0$.[2]

Since 3 follows from 1 and 2, and 1 is warranted by the assumption that the coin is fair and the tosses are pairwise independent of each other, the only way of evading Williamson's conclusion is to reject premise 2.

Williamson's argument for 2 is an argument of isomorphism: suppose that we have two fair coins A and B. At time t_0, we toss coin A. For every second thereafter, we toss A and B simultaneously. So we have two qualitatively indiscernible sequences of coin tosses that differ only by their starting times. Let $H^*_{1\dots}$ be the event that coin B lands on head every single toss since the first toss. Then intuitively $P(H_{1\dots}) = P(H^*_{1\dots})$.[3] On the other hand, note that the sequence of tosses of coin B is physically isomorphic to the sequence of tosses of coin A starting from the second toss. So $P(H^*_{1\dots}) = P(H_{2\dots})$. By transitivity of identity $P(H_{1\dots}) = P(H_{2\dots})$.

1.2 EQUINUMEROSITY VS. EQUIPROBABILITY

Hofweber objects to Williamson's analysis by rejecting the equation $P(H_{1\dots}) = P(H^*_{1\dots})$. He argues that this equation derives from a fundamental confusion between parthood and correspondence. It is a canonical feature of infinity that a proper subset of an infinite set could have the same cardinality as the whole set. For example, consider the set of natural numbers $\mathbb{N}$ and the set of even numbers $2\mathbb{N}$. Every even natural number is a natural number, but not every natural number is even. So $2\mathbb{N}$ is a part of $\mathbb{N}$. But $2\mathbb{N}$ and $\mathbb{N}$ have the same "size" in the sense that we can construct a bijective map between $2\mathbb{N}$ and $\mathbb{N}$ such that every single natural number is mapped to a unique even number.

At first glance, it would appear that the equation $P(H_{1\dots}) = P(H^*_{1\dots})$ comes from a mistaken assumption about the relationship between equinumerosity and equiprobability. It seems that Williamson is arguing that if two sequences of tosses have the same cardinality with each other, then they are equiprobable. Hofweber thinks this reasoning is fallacious. If this is the basis on which Williamson comes to

2 Note that $x = 2x$ implies that $x = 0$ for any $x \in \mathbb{F}$, where $\mathbb{F}$ is a field (including the hyperreal field). In fact this holds insofar as $\mathbb{F}$ is a group, because all we need is for x to have an additive inverse $-x$ such that we can add the additive inverse of x to both sides of the equation to get $0 = x$.

3 An alternative way to think about this is to consider two people A and B tossing coins for infinitely many times. Then from a God's perspective it is intuitive that the probability that A always get heads is the same as the probability that B always gets heads. And it shouldn't matter whether they start tossing their coins at the same time, in the sense that if we the omniscient and impartial spectator were asked to compare the relative probability of A getting an infinite sequence of heads and B getting an infinite sequence of heads, we shouldn't care whether they started tossing their coins at the same time. I thank Haimei for suggesting this way of conceptualizing the thought experiment.

conclude that $P(H_{1\ldots}) = P(H^*_{1\ldots})$, then by similar logic the probability that the coin lands on heads for every million-th toss would be the same as the probability that the coin lands on heads for every single toss, which seems absurd. Yet the probability that a coin lands on head for every milliion-th toss is higher than the probability that a coin lands on head every single time. So equal cardinality does not warrant equiprobability. "Measuring chances and sizes of sets are simply two different things."(Hofwebwe, 12)

I disagree with Hofweber's argument on two grounds. First, I think it has not done justice to Williamson's argument. Second, I argue that while there is a way of resisting Williamson's conclusion, this line of defense is not available to those who want to defend the primitiveness of unconditional probabilities using hyperreal analysis. I shall first redeem Williamson's intuition of $P(H_{1\ldots}) = P(H^*_{1\ldots})$. I then argue that one can block Williamson's argument for $P(H^*_{1\ldots}) = P(H_{2\ldots})$ by appealing to a notion of relative qualitative differences using comparative conditional probabilities.

I think Hofweber's analysis overlooks two important aspects in Williamson's argument. First, recall that Williamson did not conclude $P(H_{1\ldots}) = P(H_{2\ldots})$ right away from the fact that there are equal number of heads in $H_{1\ldots}$ and $H_{2\ldots}$. Rather, he took a detour by inviting us to consider two equalities independently: $P(H_{1\ldots}) = P(H^*_{1\ldots})$ and $P(H^*_{1\ldots}) = P(H_{2\ldots})$. He then reasoned that these two equalities hold in virtue of, not equinumerosity, but physical isomorphism. But equinumerosity and isomorphism are not synonyms of every other. The two notions coincide only in the set-theoretic context where two sets are isomorphic just in case there is a bijective correspondence between them. In general, isomorphism is stronger than mere bijective correspondence. For example, two groups are isomorphic if there is a bijection that preserves their respective group operations; two topological spaces are isomorphic if the bijection is a continuous, and two graphs are isomorphic if the bijection preserves the vertex-edge relationships. In this particular context, one way to make Williamson's idea of physical isomorphism more rigorous from an abstract mathematical standpoint is by stiuplating that two sequences are isomorphic just in case there is a function between the two sequences such that it is 1-1 and onto, and it preserves the pairwise relations between two coin tosses. Schematically, let $\mathcal{H} = (\{H_n\}_{n\in\mathbb{N}}, s)$ where s is the successor relation, i.e. $s(H_n) = H_{n+1}$. Then $\mathcal{H}$ is isomorphic to $\mathcal{H}^*$ iff there exists

$$\pi : \{H_n\}_{n\geq 1} \to \{H^*_n\}_{n\geq 1}$$

such that $\pi(s(H_n)) = s(\pi(H_n))$.

In more concrete terms, let $\mathcal{H}$ be the sequence of heads generated by coin A and $\mathcal{H}^*$ be the sequence generated by coin B. Then

$$\mathcal{H} : H_1, H_2, H_3, \ldots$$
$$\mathcal{H}^* : H_1^*, H_2^*, H_3^*, \ldots$$

and clearly there exists $\pi : \mathcal{H}^* \to \mathcal{H}$ given by $\pi(H_n^*) = H_n$.

Similarly, the subsequence $\mathcal{H}' = \mathcal{H} \setminus H_1$ is also structurally isomorphic to $\mathcal{H}^*$ via the function $\tilde{\pi} : \mathcal{H}^* \to \mathcal{H}$ given by $\tilde{\pi}(H_n^*) = H_{n+1}$. Note that $\tilde{\pi}$ is indeed a structural isomorphism because $\tilde{\pi}(s(H_n^*)) = \tilde{\pi}(H_{n+1}^*) = H_{n+2} = s(H_{n+1}) = s(\tilde{\pi}(H_n^*))$.

Let $H_{10^6, 2\times 10^6, \ldots}$ denote the subsequence of every million-th heads in $\mathcal{H}$. It would appear that there is a similar natural structural isomorphism between $H_{10^6, 2\times 10^6, \ldots}$ and $H_{1\ldots}^*$. However, I argue that no such successor-relation-preserving function exists. To see this, note that $s(H_{n\times 10^6}) = H_{n\times 10^6+1} \notin H_{n\times 10^6}$. As a result, for any function $f : H_{10^6, 2\times 10^6, \ldots} \to H_{1\ldots}^*$, we have $f(s(H_{n\times 10^6})) = f(H_{n\times 10^6+1})$. Since $H_{n\times 10^6+1}$ lies outside of the domain of f. So $f(H_{n\times 10^6+1})$ is undefined.

All I am trying to motivate here is that $H_{2\ldots}$ is not just *any* infinite subsequence of $H_{1\ldots}$. It is intrinsically different from $H_{10^6, 2\times 10^6, \ldots}$ in the sense that $H_{2\ldots}$ inherits not only the ordering but also the successor relation from $H_{1\ldots}$.[4]

It may be objected at this point that my definition of structural isomorphism is *ad hoc*. In particular, there is no reason why we cannot extract the subsequence of $H_{10^6, 2\times 10^6, \ldots}$ from the original sequence $H_{1\ldots}$ and define a new successor function on the sequence, such as $s'(H_{n\times 10^6}) = H_{(n+1)\times 10^6}$. I have no objection to this move, except to point out that the extracted sequence *is not* the same as the original subsequence. I shall motivate this claim by considering the following two cases:

<u>Case 1</u>: Monkey A v. Monkey B

Suppose two monkeys A and B throw two identical coins infinitely many times. How does the probability that A gets head every single time compares to B getting head on every million-th toss?

4 Another way of looking at structural isomorphism is that two sequences are isomorphic to each other if we can "smoothly" embed one sequence into another without tearing it apart. To this extent the isomorphism at stake is in fact more of a topological isomorphism (i.e. homeomorphism): two sets are homeomorphic if there is a continuous transformation from one set to another that preserves their relative properties. Two points are close together iff their images in the homeomorphic sets are also close together. When we map $H_{2\ldots}$ to $H_{1\ldots}^*$, all we need is a simple translation that preserves the relative successor relation between any two coin tosses. However, when we try to map $H_{1\ldots}$ into its subsequence $H_{10^6\ldots}$ *qua subsequence of* $H_{1\ldots}$, we have to tear up the sequence such that the H_n's become scattered across the sequences with huge gaps (99999 heads) in between any two original successors. Alternatively one can think of $f, g : [0,1] \to [0,1]$ where $f(a) = 1$ for all $0 \leq a \leq \frac{1}{2}$ and 0 otherwise, whereas $g(x) = x$. Clearly there is a bijective correspondence between the images of f and g even though the two images are clearly non-homeomorphic (one is connected, the other is not.)

Case 2: Monkey v. Sloth
Suppose a monkey and a sloth toss two identical coins for infinitely many times. The sloth, being a sloth, tosses the coin one million times slower than the monkey. How does the probability that the monkey gets head every single time compares to the probability that the sloth gets head every single time?

Intuitively, it would appear that the $P(\text{Monkey A}) < P(\text{Monkey B})$ whereas $P(\text{Monkey}) = P(\text{Sloth})$, despite that the four events we are asked to consider are loosely speaking "the same", i.e. getting an infinite sequence of heads. On Hofweber's interpretation of Williamson's argument, Williamson is committed to saying that $P(\text{Monkey A}) = P(\text{Monkey B})$. Yet if we understand Williamson's argument as one based on structural isomorphism properly defined, then there is a way of reconciling our discrepant intuitions: $P(\text{Monkey A})$ is indeed smaller than $P(\text{Monkey B})$ because the infinite sequence of heads produced by Monkey A is not structurally isomorphic to the subsequence produced by Monkey B *even if* Monkey B also gets head on every single toss on top of getting heads on every million-th toss.

However, I think structural isomorphism, construed as a successor-relation-preserving bijection, is still insufficient to warrant Williamson's conclusion that $H_{1\ldots}$ and $H_{2\ldots}$ are equiprobable. All it requires is to ask what is the probability that coin A will land on head every single time since the million-th toss? Let $H_{10^6\ldots}$ denote such event. Then even on the structuralist isomorphism-interpretation of Williamson's analysis, $H_{10^6\ldots}$ would be structurally isomorphic to $H_{1\ldots}$. So the probability that a coin lands on head every single time since the first toss is equal to the probability that a coin lands on head every single time since the million-th toss. Yet it seems that $H(10^6\ldots)$ is clearly more probable than $H_{1\ldots}$. So either our intuitions are simply unreliable when it comes to infinity, or there is something wrong with Williamson's original account.

Instead of resorting to an error theory (which could be true given that human are notorious when it comes to grappling with arguments involving infinities), I argue that this is a case in which our intuitions are justified - $H_{1\ldots}$ is more probable than $H_{2\ldots}$ - if we take conditional probabilities to be primitive. The analysis is independently given in [Easwaran, 2014] and [Dorr, 2010]: observe that $H_{1\ldots}$ logically entails $H_{2\ldots}$ whereas the converse if not true. So

$$P(H_{1\ldots}|H_{1\ldots} \vee H_{2\ldots}) = 1$$

whereas

$$P(H_{1\ldots}|H_{1\ldots} \vee H_{2\ldots}) = P(H_1|H_{2\ldots}) = \frac{1}{2}.$$

It is worth pointing out that we arrive at these two numerical values by pure logical analysis rather than appealing to THE RATIO. For if our probability function is real-valued, then $P(H_{1\ldots} \vee H_{2\ldots}) = 0$; if we allow for infinitesimal valued probabilities, then Williamson's analysis suggests that $P(H_{1\ldots} \vee H_{2\ldots}) = P(H_{1\ldots}) = P(H_{2\ldots}) = 0$.[5] Either way the condition might have zero probability, which means THE RATIO would leave both conditional probabilities $P(H_{1\ldots}|H_{1\ldots} \vee H_{2\ldots})$ and $P(H_{1\ldots}|H_{1\ldots} \vee H_{2\ldots})$ undefined. [6]

But note that in order to make that distinction, one has to resort to a prior notion of comparative conditional probability. Arguably this is not a particular problem for Hofweber, whose main project is not so much to defend THE RATIO or the reducibility of conditional probabilities to unconditional probabilities as to argue that all probability functions must be regular - that is, the range of probability functions must be fine-grained enough to capture a modal distinction between strict impossibility and highly improbable possibilities. On the other hand, for those who want to defend THE RATIO using hyperreal analysis, this argument against Williamson actually counts *against* rather than *in favor of* their project. For otherwise one is effectively arguing that conditional probabilities are derivable from unconditional probabilities but the primitiveness of unconditional probabilities is justified partially on the ground that the unconditional probabilities of two events (i.e. $H_{1\ldots}$ and $H_{2\ldots}$) are differentiable when we compare their conditional probabilities conditionalized on their disjunctions. This is not strictly circular, but seems nevertheless an unpalatable position to take. Instead, I think there are better ways to defend THE RATIO against Hájek's argument based on the Four Horn Theorem and seven intuitions, which I shall turn to next.

Before I engage critically with Hájek's argument, there seem to be at least two ways of interpreting Hájek's main thesis: i) conditional probabilities are primitive and irreducible to unconditional probabili-

5 Recall that at this junction our *goal* is to refute Williamson's equiprobability analysis. So we would dismiss his proposal on pain of circuitous reasoning.

6 In [Dorr, 2010], Dorr argues that $P(A|A \vee B) \neq P(B|A \vee B)$ is insufficient for breaking the qualitative symmetry between A and B. He appeals to a distinction between weak and strong senses of equiprobability (Dorr, 190). Two hypotheses are *weakly equally likely* just in case they have equal unconditional probabilities, whereas two hypotheses are *strongly equally likely* just in case they are weakly equally likely *and* they have equal conditional probabilities when conditionalized on their disjunctions. Dorr insists that only the weak sense of equiprobability is necessary for qualitative indiscernibility. The strong sense of likelihood tells us not the individual characteristics of the event *per se* but the *qualitative relations* that they bear with each other. The point is that, when evaluating $P(A|A \vee B)$, we are restricting ourselves to only the *A-world* or *B-world*. As a result we can only extrapolate the relational or structural properties of A (with respect to B), as opposed to the intrinsic properties of A all by itself. A full exposition of this line of defense would go beyond the scope of this thesis. However, I think the distinction between weak and strong equiprobability is tenable only if one can provide a coherent account in support of the more fundamental distinction between purely individual qualities versus relational qualities. Lurking behind is the profound metaphysical question: are singular propositions the primary objects of probabilities, or is probability a feature of a *system* and individual propositions (or more precisely the singular events to which they refer) come to bear probabilities in virtue of them being part of the system?

ties[7], or ii) THE RATIO is an inadequate analysis of conditional probabilities. In many instances, it appears that Hájek is in fact arguing for ii) rather than i). For example, he thinks Kolmogorov's extended analysis of conditional probability using measure-theoretic tools is a different analytical schema than THE RATIO and therefore does not count as an objection to his own thesis. To this extent, it seems that Hájek's sole target is THE RATIO and THE RATIO alone. If this is in fact what Hájek had in mind, then I would agree with Hájek's negative thesis (that the Ratio Analysis is not an adequate analysis of conditional probability in terms of unconditional probabilities) but only trivially so. For, to my knowledge, no one - not even Kolmogorov himself - would think that THE RATIO supplies an adequate analysis of conditional probabilities for all practical purposes.[8] However, at other places Hájek does move to draw a stronger conclusion that we should replace Kolmogorov's system by alternative axiomatizations that take conditional probabilities as primitive. Since ii) is a strawman, I shall take the liberty of reinterpreting Hájek (perhaps to his own disagreement) to be arguing against i), or what I shall call the supervenience thesis or ST[9]:

> **(ST)** Conditional probability is supervenient on unconditional probabilities.[10]

I think there are compelling reasons for this thesis, but I contend that Hájek's argument is defeasible to the extent that all seven of his basic intuitions are not universally true for all propositions across all probability distributions. That is, for each basic intuition about conditional probability on the list, there is a particular situation in which the intuition may fail to hold (or at least there is a putative argument for why it does not have to hold). To this extent, I argue that our pre-theoreitc intuitions about conditional probabiltiies are context-dependent. Therefore, *contra* Hájek, I contend that conditional probabilitiies do *not* have to be well-defined when the contextual parameters are not specified. Since Hájek rejects THE RATIO on the sole ground that it leaves a conditional probability undefined

7 Or to be more precise, any available techniques of reducing conditional probabilities to unconditional probabilities are inadequate. Hence it is incumbent on the detractors to provide a proper reduction of conditional probabilities to unconditional probabilities.

8 And Hájek's fixation with THE RATIO is even more surprising given that Kolmogorov's entire project is to embed probability theory into the more general mathematical framework of measure theory.

9 Since reduction is generally taken to entail supervenience, non-supervenience implies irreducibility (and therefore the negation of i)).

10 There are again two interpretations of ST within the subjectivist framework: the epistemological interpretation (unconditional credence in AB and A are necessary and sufficient for the formation of conditional credence in B given that A) and the analytical interpretation (conditional credence can be analyzed purely in terms of unconditinal credence). Here I shall constrain myself to the analytical interpretation of ST. See Appendix II for a discussion on why I think the epistemological version of ST is untenable.

when it should be well-defined, it follows that Hájek's objection to THE RATIO is inconclusive.

2

THE FALL OF SEVEN INTUITIONS

2.1 INTUITIONS I-IV AND THE PARADOXICAL SET

The first four intuitions on Hájek's list state that upon observing that A, we should have full belief in A and zero credence in its negation $\neg A$, whereas our beliefs in necessary truths or falsities should remain resilient.

However, this turns out to be not universally true for any probability space. Inspired by a construction technique used by [G.A.Sherman, 1991], [Pruss, 2013] proves the following two theorems:

1. Intuitions I-IV are equivalent, i.e. for any given event space Ω and probability function P, where P could be an unconditional probability-first function or a conditional probability-first function,[1] we have

   $$P(A|A) = 1 \text{ iff } P(\neg A|A) = 0 \text{ iff } P(\Omega|A) = 1 \text{ iff } P(\emptyset|A) = 0$$

   By contraposition, we have

   $$P(A|A) = 0 \text{ iff } P(\neg A|A) = 1 \text{ iff } P(\Omega|A) = 0 \text{ iff } P(\emptyset|A) = 1.$$

   It follows that, as long as there exists one proposition A for which one of the four intuitions does not hold, then all four intuitions fail with respect to A.

2. For any probability function P, there exists an event space Ω such that there is a subset $A \subset \Omega$ for which $P(A|A) = 0$.

The fact that A exists is not so surprising in light of the Banach-Tarski paradox, which says that, given a solid ball $\mathcal{B}$ in $\mathbb{R}^3$, we can partition $\mathcal{B}$ into finitely disjoint pieces and, under simple translation and rotation, reassemble the pieces into two identical copies of the same ball. However, the Banach-Tarski paradox is often dismissed by philosophers of probability and especially the subjectivitists because of the

1 I call a probability function "unconditional probability-first" just in case it is a one-place function that takes in one proposition (e.g. A) at a time and outputs the unconditional probability of that proposition (e.g. $P(A)$); a "conditional probability-first" function, on the other hand, is a two-place function that takes in an ordered pair of two propositions (e.g. *(B,A)*) at a time and outputs the conditional probability of one given the other (e.g. $P(B, A) = P(B|A)$).

indispensable role that the Axiom of Choice plays in its construction.[2] Unfortunately, this line of objection is not available to Hájek here, because in [G.A.Sherman, 1991] Sherman constructs a set J in $\mathbb{R}^2$ that satisfies similar paradoxical properties as $\mathcal{B}$ in $\mathbb{R}^3$, i.e. $J = U \cup V$ where $U \cap V = \emptyset$ and $U = J$ and $V = J$, without using the Axiom of Choice. Then for this particular J, we have

$$\begin{aligned} P(J|J) &= P(U|J) + P(V|J) \\ &= P(J|J) + P(J|J) \\ &= 2P(J|J)^3 \end{aligned}$$

which implies that $P(J|J) = 0$.[4] So we have a proposition for which the first basic intuition cannot be true. Since the four intuitions are logically equivalent, it follows that the other three intuitions are not universally true either.

What Pruss' argument really shows that what seem to be "basic" intuitions about conditional probabilities for Hájek are in fact neither basic nor merely about conditional probabilities *per se*. Rather, Intuition I-IV reflect our basic assumptions about identity and necessity: every proposition is self-equivalent and not equivalent to its negation, and if we are absolutely confident that something is true or false, our degrees of confidence should be conditionalization-invariant. As we shall see in 1.7 and more so in part II, identities and necessities are not always well-respected in formal modelling. In particular, in a formal model, events are defined with respect to the particular way in which the model is constructed and represented, and therefore events that are actually identical may come apart when modelled differently (or even when they are represented in the same model, as demonstrated by the foregoing analysis).[5]

2 Rumor has it that Ron Maimon alleges that the Banach-Tarski paradox does not seem right because it is *actually* false - in the sense that it cannot be *actualized* in physical experiments.

4 Note that $P(J|J) = 2P(J|J)$ implies that $P(J|J) = 0$ in virtue of the fact that $P(J|J)$ is a real number. In fact, all we need is that $P(J|J)$ is an element of a group under addition, let say r, and thereby has additive inverse "$-r$", for then we can add "$-r$" to both sides of the equation and get $r - r = 0 = 2r - r = r$.

5 Another way to look at the upshot of this result is that, as [Myrvold, 2015] points out in footnote 2, "in probability theory we can't always get what we want...It is well-known that there are no probability functions satisfying certain symmetry conditions, countable additivity and the *desideratum* of having the probability function defined on arbitrary subsets of our sample space." (Myrvold, 3) That is, no probability function could be well-defined on all subsets of a unit circle and at the same time be countably additive and rotationally invariant. And as [G.A.Sherman, 1991] and Banach-Tarski's paradox suggests, one does not really need countable addivitity to get paradoxical results: translational invariance and universal definability are sufficient for generating unpalatable consequences such as $P(J|J) = 0$. This agrees with [McGee, 1989], which shows that there is a correspondence between conditional-probability-first functions and probability functions that take the hyperreal numbers rather than $\mathbb{R}$ as their ranges (countable additivity does not hold in hyperreal analysis).

2.2 intuition v and the eternal coin

Intuition V says that the conditional probability of a fair coin landing on Head, given *any arbitrary proposition* apart from itself and its own negation, should be $1/2$. In particular, this intuition should hold even if the event on which we conditionalize has probability zero. [Dorr 2010] presents a intriguing case in which it seems that in some hypothetical scenarios, conditionalization on a probability-zero event could increase the probability of the coin landing on Head from $1/2$ to 1. Suppose a fair coin is thrown once a day for infinitely many days, both in the past and in the future (assuming that there are infinitely many days in the past and infinitely many days in the future). Assume in addition that each toss is independent of every other. So the probability of the coin landing on head for each toss is presumably $1/2$. Now let

F : the coin lands on head for every single toss in the future.

P : the coin lands on head for every single toss in the past.

H : the coin lands on head today.

T : the coin lands on tail today.

What is the probability that the coin will land on head today, given that it will land on head for every single day in the future, i.e. what is $P(H|F)$?

According to Hájek, this probability should be $1/2$ despite the fact that $P(F) = 0$. This seems intuitively true by virtue of the setup: we are told in the assumption that the coin is fair and the turnout of each toss is probabilistically independent of every other. So the number of heads that we have gotten in the past or will get in the future shouldn't bias the coin towards landing on head or tail today. However, Dorr argues that the conditional probability should be 1 instead of $1/2$, and he presents two arguments for this counterintuitive conclusion.

2.2.1 *Argument 1: Self-locating indifference*

The principle of self-locating indifference is essentially a requirement of objective temporal impartiality in the absence of discriminating evidences. To put it schematically, the principle says that, if the subject cannot tell what time it is, then she ought to distribute her credence equally between all possible times. Now let

K_0 : today will be the last Tail-day and the coin will land on head every single day starting from tomorrow($i.e.$ $K_0 = TF$)
K_n : today will be the n-day of an infinite run of heads
K_∞ : the coin lands on heads every day, past, present and future

Observe that K_n implies H for all $n \in \mathbb{Z}_{\geq 1}$. Moreover, the event that the coin lands on head every single day in the future can be represented as a disjunction of two centered propositions: the coin lands on head every single day in the future and today is the last tail day, or the coin lands on head every single day in the future and today is the n-th day since the last tail day for some n greater than or equal to 1. So $F \equiv K_0 \vee (K_1 \vee K_2 \vee K_3 \vee \dots) \vee K_\infty$.

But now suppose that we are drugged immediately after each toss. After we wake up the next day, we remember neither the date of today, nor the toss result of the previous day. Presumably this shouldn't affect our conditional credence in the coin landing on head today, given that it lands on head every single day in the future, since that assessment appears to require no information about the past behavior of the coin. However, due to our memory loss, we cannot tell whether we had head or tail yesterday, which makes us incapable of adjudicating, given that the coin will land on head every single day in the future, whether today will be the last tail day or some day in the middle of this infinite sequence of heads. As a result, by principle of self-locating indifference, we should assign deem that all K_i are equally likely to be true. So $P(K_0|K_0 \vee K_1 \vee K_2 \vee \cdots \vee K_n) < \frac{1}{n}$ for all $n \in \mathbb{Z}_{\geq 1}$. By the Archimedian property of the real number, it follows that $P(K_0|F) = 0$.[6] But recall that $K_0 = TF$. So $P(TF|F) = P(T|F) = 0$. By law of total probability, this means that $P(H|F) = 1 - P(T|F) = 1 - 0 = 1$.[7]

6 Note that the particular Archimedian property of the real numbers is not crucial in this argument. After all, even if we introduce hyperreal numbers ϵ such that $\epsilon < \frac{1}{n}$ for all $n \in \mathbb{Z}_{\geq 1}$, we would end up with $P(T|F) = \epsilon$ and $P(H|F) = 1 - \epsilon$ where ϵ is infinitesimal. This is still far off from our original intuition that $P(H|F) = 1/2$.

7 The acute reader might be skeptical at this point about the applicability of the principle of self-locating indifference. After all, K_1 and K_2 do not have the same informational content: K_1 makes assumptions about the coin toss results for yesterday, today and every single day in the future, whereas K_2 makes assumptions about the coin toss results for *the day before yesterday*, yesterday, today and every single day in the future. Pictorially,

$$K_1 : T\mathbf{H}HHH\dots$$
$$K_2 : TH\mathbf{H}HHH\dots$$

where $\mathbb{H}$ stands for the prediction that the coin will land on head today. Given that the coin is fair and each toss is presumably independent of every other, from an objective standpoint, the probability of K_2 should be smaller than the probability of K_1. And similarly $P(K_i) > P(K_j)$ for all $i < j$. This is one reason why I find the

2.2.2 *Argument 2: Conditional Probability as Primitive*

Dorr's second analysis seems harder to resist and presents a special challenge to Hájek, who endorses conditional probability-first probability functions. Dorr proposes the following three principles: Let P_+ be the probability function that you will have tomorrow, conditional on the setup. Then

$$P_+(HF|P \vee HF) = P(F|HP \vee F) \tag{1}$$
$$P_+(P|P \vee HF) = P(HP|HP \vee F) \tag{2}$$
$$P_+(HF|P \vee HF) = P(HF|P \vee HF) \tag{3}$$
$$P_+(P|P \vee HF) = P(P|P \vee HF) \tag{4}$$
$$P(P \vee HF) > 0 \text{ and } P(F|P \vee F) > 0 \tag{5}$$

In addition, assume that the multiplicative axiom for conditional probability holds

$$P(A|C) = P(A|B)P(B|C)$$

whenever AC entails B and B entails C.

Then by pure algebraic manipulation, we get $P(H|F) = \frac{1}{P(H|P)}$. But since $P(H|F) \leq 1$ and $P(H|P) \leq 1$, it follows that the only possible values for $P(H|F)$ and $P(H|P)$ are 1.

Admittedly Dorr's analysis is not irresistable. To name but one objection to Dorr's argument, as we shall see in part II, the axiom of multiplicative axiom is not universally true and its veracity depends on the particular stochastic dependency relationship among A, B and C. However, I think the Eternal Coin presents an interesting case that motivates my general concern for Hájek's argument, namely that our pre-theroetic intuitions about conditional probabilities are largely informed by and therefore highly sensitive to the particular way in which we model and represent the given scenario in our mind. For instance, one can provide a qualitative justification for Dorr's analysis as follows: if we were to actually believe that the coin lands on head *every single day in the future*, it is very hard to hold onto our initial belief that the coin is in fact unbiased and therefore has equal probabilitis of landing on head or tail for today. Rather, it seems tempting to conclude that the probability that the coin will land on head, given that it will land on head every single day in the future, is likely to be 1. In this sense, $P(H|F) = 1$ is a particular instance of abductive reasoning in probability assessment that I shall explore more in details in part II.

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second argument a better argument for Dorr's central thesis, namely that $P(H|F) = 1$. More in-depth analysis would require getting into the debate about the principle of self-locating indifference, which is beyond the scope of this thesis.

2.3 INTUITION VI AND SYMMETRY

Intuition VI says that, let x be a number chosen from the interval $[0,1]$ at random, then the conditional probability that $x = 1/4$, given that $x = 1/4$ or $x = 3/4$ is $1/2$.

Presumably, this intuition comes from our assumption about uniform probability distribution. Since the probability distribution is uniform, each point in the interval $[0,1]$ is no more and no less likely to be selected than every other point. In particular, the probability $P(x = 1/4) = P(x = 3/4)$. In addition, since point is chosen at random, the event that $1/4$ is selected is probabilistically independent of and mutually incompatible to the event that $3/4$ is selected. Hence the probability of their disjunction equals to the sum of their individual probabilities: $P(x = 1/4 \vee x = 3/4) = P(x = 1/4) + P(1 = 3/4)$. Let the probability $P(x = 1/4) = P(1 = 3/4) = r$ where r is some sharp numerical value (could be infinitesimal). Then we have[8]

$$P(x = \frac{1}{4}|x = \frac{1}{4} \text{ or } x = \frac{3}{4}) = \frac{r}{2r} = \frac{1}{2} = P(x = \frac{3}{4}|x = \frac{1}{4} \text{ or } x = \frac{3}{4}).$$

Now let y be another real number chosen from the interval $[0,1]$ with a probability density function of $2y$. Then our intuition about this new skewed probability distribution suggests that it is more likely for y to be greater than $1/2$ than being smaller than $1/2$.[9] So

$$P(y = \frac{1}{2}|y = \frac{1}{2} \text{ or } y = \frac{\sqrt{3}}{2}) < P(y = \frac{\sqrt{3}}{2}|y = \frac{1}{2} \text{ or } y = \frac{\sqrt{3}}{2})$$

Yet observe that x is uniformly distributed just in case $\sqrt{x}$ is distributed with a probability density function of $1/2$. So if we let $y = \sqrt{x}$, then $y = 1/2$ iff $x = 1/4$ and $y = \sqrt{3}/2$ iff $x = 1/4$,

8 This looks very much like the infinitesimal analysis, which is why it seems an appealing solution to Hájek's objections against THE RATIO. However, as I hope to have shown in chapter 1, if the goal of introducing infinitesimal numbers is to save THE RATIO and in general the thesis that unconditinoal probabilities are more fundamental than conditional probabilities, then Williamson's argument seems to suggest that this cannot cannot be done convincingly. In some cases (such as the 1-direction eternal coin, in contrast with Dorr's case of two-direction eternal coin that we shall discuss shortly), rather than deducing equal comparative likelihood from equiprobability, we *derive* the latter from the former. And in general, as Dorr and Easwaran point out independently, equal comparative probability captures an aspect of the relational property between two events that is indescribable in a purely unconditinoal probability-based analysis. Easwaran, in particular, goes further to suggest that likelihood could be a partial ordering rather than total ordering, and this goes against the fundamental assumption of unconditional probability-based analysis, which treats likelihoods and probabilities interchangeably and takes equal likelihoods as a direct fallout of the total ordering of probabilities.

9 One way to visualize this change is to imagine an infinitely thin dart thrown on a line $\overline{AB}$ of length 1, and instead of having the line of uniform width, in the second case the line is infinitely thin at A and becoming uniformly thicker and thicker as one approaches B. Thus, intuitively it is more likely to select points nearer B as opposed to points nearer to A.

i.e. the events that $y = 1/2$ and $x = 1/4$ are equiprobable, and are $y = \sqrt{3}/2$ and $x = 3/4$. Thus, if equiprobability implies equal comparative conditional probabilities,[10] then

$$\begin{aligned} P(y = 1/2 | y = 1/2 \text{ or } y = \sqrt{3}/2) &= P(x = 1/4 | x = 1/4 \text{ or } x = 3/4) \\ &= P(x = 3/4 | x = 1/4 \text{ or } x = 3/4) \\ &= P(y = \sqrt{3}/2 | y = 1/2 \text{ or } y = \sqrt{3}/2) \\ &= 1/2. \end{aligned}$$

So

$$P(y = \frac{1}{2} | y = \frac{1}{2} \text{ or } y = \frac{\sqrt{3}}{2}) = P(y = \frac{\sqrt{3}}{2} | y = \frac{1}{2} \text{ or } y = \frac{\sqrt{3}}{2}).$$

This contradicts our previous intuition that

$$P(y = \frac{1}{2} | y = \frac{1}{2} \text{ or } y = \frac{\sqrt{3}}{2}) < P(y = \frac{\sqrt{3}}{2} | y = \frac{1}{2} \text{ or } y = \frac{\sqrt{3}}{2}).$$

What went wrong? I argue that the discrepancy between our two intuitions come down to the differences in our mental representations of a "point." First, observe that we can in fact redeem our intuitions about the inequality between the two conditional probabilities as follows.

Following the same logic, it would appear that

$$P(y = \frac{1}{2}) = \int_{\frac{1}{2}-\epsilon}^{\frac{1}{2}+\epsilon} 2y dy = 2\epsilon^2$$

whereas

$$P(y = \frac{\sqrt{3}}{2}) = \int_{\frac{\sqrt{3}}{2}-\epsilon}^{\frac{\sqrt{3}}{2}+\epsilon} 2y dy = 2\sqrt{3}\epsilon^2.$$

10 The reader might be slightly confused here since I have also talked about the connection between equiprobability and equal comparative probabilities (or comparative likelihoods in short) in my previous analysis of Williamson's argument against the use of infinitesimal numbers. To clarify things a little bit, recall that what we have argued in the Williamson's section is that one can use asymmetrical likelihood (i.e. $P(p|p \vee q) < P(q|p \vee q)$) to justify asymmetrical unconditional probabilities of p and q (thus assigning different infinitesimal numbers to $P(p)$ and $P(q)$). And this conclusion seems palatable given our intuition that the unconditional probability of getting an infinite sequence of heads starting from today is lower (however more slightly) than the unconditional probability of getting an infinite sequence of heads starting from tomorrow. Thus in my analysis I gestures to the assumption that unequal comparative conditional probabilities *could* imply unequal unconditional probabilities, and by contraposition this is the same as saying that equiprobability implies equal comparative conditional probabilities.

So

$$\begin{aligned} P(y=\frac{1}{2}|y=\frac{1}{2} \text{ or } \frac{\sqrt{3}}{2}) &= \frac{2}{2+2\sqrt{3}} \\ &< \frac{2\sqrt{3}}{2+2\sqrt{3}} \\ &= P(y=\frac{\sqrt{3}}{2}|y=\frac{1}{2} \text{ or } \frac{\sqrt{3}}{2}) \end{aligned}$$

Note that in this analysis, we effectively conceptualize the points $1/2$ and $\sqrt{3}/2$ as limits of two intervals of infinitesimal but equal lengths (i.e. $[1/2-\epsilon, 1/2+\epsilon]$ and $[\sqrt{3}/2-\epsilon, \sqrt{3}/2+\epsilon]$). Since the probability distribution is skewed, intervals of the same length are not equiprobable. So on this conception of the "points" 1/2 and 3/2 we have$P(y=1/2) \neq P(y=\sqrt{3}/2)$. On the other hand, if we take $1/2=\sqrt{1/4}$ and $\sqrt{3}/2=\sqrt{3/4}$, while imagining $1/4$ and $3/4$ as two limits of infinitesimal intervals of equal lengths, then when we translate this imagining back to our conception of $1/2$ and $\sqrt{3}/2$ we no longer have them as limits of intervals of equal length. To put this mathematically, observe that when substituting the variable x by $y=\sqrt{x}$, we have

$$P(x=\frac{1}{4}) = \int_{\frac{1}{4}-\epsilon}^{\frac{1}{4}+\epsilon} 1dx = \int_{\sqrt{1/4-\epsilon}}^{\sqrt{1/4+\epsilon}} 2ydy = P(y=\frac{1}{2}) = 2\epsilon.$$

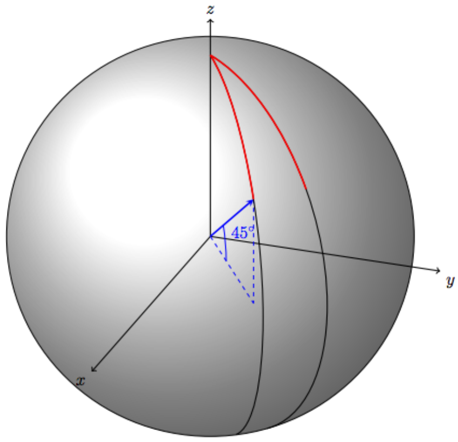
And similarly $P(y=\sqrt{3}/2) = \int_{\sqrt{3/4-\epsilon}}^{\sqrt{3/4+\epsilon}} 2ydy = 2\epsilon$. Therefore $P(y=1/2) = P(y=\sqrt{3}/2)$.[11] The upshot is similar to what we observe from Pruss' paradoxical sets: our pre-theoretic intuitions about conditional probabilities are informed by and thereby contingent on the way in which we represent the events in terms of abstract models and how we subsequently reason about their relative identities or the symmetry/asymmetry in their qualitative relationships. I shall speak more to this point in the next section, where I shall cast doubt on the seventh and last intuition on Hájek's list - the intuition that randomly selected point on a sphere will land in the western hemisphere, given that it lands on the equator, is 1/2.

2.4 INTUITION VII AND THE BOREL-KOLMOGOROV PARADOX

Intuition VII goes as follows: consider a uniform sphere. Let B be a great circle (the black arc in the picture), and R be a subarc of length $1/4$ of B (the red arc in the picture - it is easy to see that if one end of R is the North Pole, then the other end of R is at 45° to the xy-

11 I thank Professor Jonathan Tannenhauser for pointing out this fallacy to me.

plane). Let ω be a point selected at random on the sphere. What is the probability that ω lands on R, given that it lands on B?



As the reader might expect from our discussion in the previous section, there are two solutions to this problem that are nonetheless incompatible with each other. On the one hand, it seems intuitive that

$$P(R|B) = \frac{\text{length of } R}{\text{length of } B} = 1/4.$$

Let's call this the *arc-length solution*. On the other hand, we can reinterpret R as the intersection between the chosen great circle (in this case, B) and the points with latitude of at least 45° north. Then $P(R|B) = P(\text{latitude} \geq 45°|B)$ (i.e. the probability that ω lands on R, given that it lands on B, equals to the probability that ω has a latitude of at least 45°, given that it lands on B). Now let B' be another great circle and R' be the corresponding subarc that starts from the North Pole and ends at the point with a latitude of 45°. It is not hard to see that $P(R'|B') = P(\text{latitude} \geq 45°|B') = P(\text{latitude} \geq 45°|B)$. Since this equation holds for any great circle that passes through the two poles, it follows that $P(\text{latitude} \geq 45°|B)$ is independent of the choice of B, which implies that

$$\begin{aligned} P(\text{latitude} \geq 45°|B) &= P(\text{latitude} \geq 45°) \\ &= \frac{\text{surface area bounded by the circle of latitude } 45°}{\text{surface area of the entire sphere}}. \end{aligned}$$

Let's call this the *surface area solution*. Since in general

$$\frac{\text{surface area bounded by the circle of latitude } 45°}{\text{surface area of the entire sphere}} \neq 1/4,$$

the *arc-length solution* does not generally agree with the *surface area solution*. So like the previous case with $1/2$ and $\sqrt{3}/2$, here we have two seemingly impeccable analyses that nevertheless recommend two competing intuitions about what $P(R|B)$ ought to be.

One quick response to this objection is that the paradox is a red herring. Of course the *arc-length solution* is right. The surface area solution seems rather contrived and counter-intuitive.[12] This is perhaps what makes Easwaran feel unsatisfied with the traditional presentation of the paradox. Instead, he contends "the [surface area solution] is basically correct" based on the following theorem that he proves:

Theorem Let $\mathcal{B} = B_\alpha$ be the collection of all great circles. Let A be the surface area of the sphere that consists of all points with latitude of at least 45°. Assume that there is a probability $P(A|B_\alpha)$ for each $B_\alpha \in \mathcal{B}$. Let

$$S = \{B_\alpha \in \mathcal{B} | P(A|B_\alpha) \neq P(A)\}.$$

Then $P(S) = 0$.

Observe that S is the set of all great circles for which $P(A|B_\alpha) \neq P(A)$, i.e. for which the surface-area solution is incorrect. By showing $P(S) = 0$, Easwaran thereby takes himself to have demonstrated that there are very few if any great circles B for which the surface-area solution is *not* the correct way of computing the conditional probability of $P(A|B)$, where A is an arbitrary surface selected on the sphere. Thus he concludes that the paradoxicality of the Borel-Kolmogorov case lies not in a standoff between two "right" solutions but in the fact that the generalization of the "correct" solution (namely the surface-area solution) leads to a trilemma of itself.

However, note that, as Easwaran himself cautiously admits, the fact that $P(S) = 0$ only means that $P(A|B_\alpha)$ must equal to $P(A)$ *almost everywhere*, i.e. that the set of great circles for which $P(A|B_\alpha) \neq P(A)$ has measure zero in the given probability measure. Easwaran concludes that "[t]his is exactly what [the surface area solution] said". Yet it is not. The surface area solution says that the conditional probability of $P(R|B) = P(A \cup B|B) = P(A|B) = P(A)$ for *all* great circles B and surface area A. And measure theory tells us that the difference between "everywhere" and "almost everywhere" may be very big. For example, if we endow the set of real numbers between 0 and 1 with a uniform (probability) measure, then the probability that a randomly selected point in this interval is a rational number would be zero, despite the fact that the set of rational numbers is countably infinite and dense in the given probability space.

In fact, Easwaran's theorem should hardly take us by surprise in light of the first horn of Hájek's Four Horn Theorem: recall that the Four Horn Theorem says that for any uncountable set (in this case the set $\mathcal{B}$ is clearly uncountable), any (real-valued, sharp) probability measure must fail to assign positive probability to uncountably many subsets of $\mathcal{B}$. To this extent, I argue that Easwaran's proof cannot

12 One may raise the same objection against the second argument in the previous section that reasons by equating y with $\sqrt{x}$.

count as a proper vindication of the surface-area solution of the Borel-Kolmogorov paradox, and to argue otherwise is to be guilty of the fallacious reasoning of equating impossibility with zero-probability possibility.

If the foregoing analysis is correct, then a positive case remains to be made for the surface area solution given its relative unnaturalness. I think [Rescorla, 2015] has in fact provided us with a compelling defense for the surface area solution.

[Rescorla, 2015] observes that the Borel-Kolmogorov paradox takes the following form:

1. $P(R|B) = r$ (the arc-length solution)
2. $P(R|B') = r'$ (the surface-area solution)
3. $B = B'$
4. $P(R|B) = P(R|B')$
5. $r \neq r'$

In response, Rescorla points out that the argument is unsound because 3 is false: the great circle B responsible for $P(R|B) = r$ is not exactly identical to the great circle B' that gives the conditional probability $P(R|B') = r'$. They are two different "representational perspectives" of the same mathematical entity given by two different parametrization metrics of the sphere (i.e. what constitute the collection $\mathcal{B}$). If we partition the sphere into a collection of meridians, then each meridian can be represented as a limit of the area enclosed by two of its neighboring meridians that are infinitely close to each other. In this case, since the surface area is not uniformly distributed across this area, it follows that the probability distribution across the chosen meridian itself cannot be uniform either. On the other hand, if we partition the sphere into a collection of parallel circles (lines of constant latitude), with the chosen great circle B as the equator, then we can represent B as the limit of the strip enclosed by two parallels in its neighbourhood. Since the area of the strip is uniformly distributed across the sphere, it follows that the probability distribution across B should also be uniform. In short, this is consistent with and in fact exactly what I was foreshadowing in my previous analysis of $y = 1/2$ and $y = \sqrt{3}/2$ - in both cases, because the events on which we conditionalize (a point or a point landing on a great circle of a sphere) are microscopic, and the traditional strategy of modelling microscopic events is by defining them as formal limits of some perceivable sets, the conditional probabilities come out differently depending on what sets or parametric we use to conceptualize these microscopic events. In such cases, as Rescorla points out, "the locution '$P(\cdot|A)$' creates an opaque context" that forbids uncritical substitution of entities in virtue of their co-extensiveness.

Arguably, the last two sections are not so much proofs that Hájek's basic intuitions about conditional probabilities are wrong, as evidence suggesting that there is much more going on behind these intuitions. Specifically, intuitions VI and VII - and in fact to a large extent all seven intuitions - are reflective of and derivative from our more fundamental intuitions about the relationship between identity, comparative likelihoods and equiprobability. But, at least in the subjectivist context, these four notions need not go hand in hand. The same physical entity can admit of different and sometimes incompatible abstract formalizations. As a result, two abstract entities that correspond to the same physical entity need not be equally probable across all modelling perspectives.

3

SUMMARY AND SYNTHESIS

I have argued that all seven of Hájek's purported "basic" intuitions about conditional probabilities can be violated or at least fail to be generalized. To this extent, it seems that in these cases, conditional probabilities are not necessarily well-defined, but only well-defined with respect to a specified range of propositions and against a determined contextual parameter. In this sense, it is the conditional-probability-first functions, rather than THE RATIO, that is guilty of "sins of omissions"- by insisting that all conditional probabilities must be sharp, such functions fail to capture the innate dependency of conditional probabilities on context and mental representations. So THE RATIO could be correct after all - when conditionalized on microscopic events, the value of conditional probabilities can be genuinely underdetermined to the extent extraneous contextual information is required for specifying in what way the events are represented and related stochastically.

However, note that this is an argument *in favor of* THE RATIO and yet *against* ST. If the contextualist analysis is correct, then what it highlights is precisely the fact that conditional probabilities *are not* supervenient on unconditional probabilities, in the sense that specification of two well-defined unconditional probabilites may leave the corresponding conditional probabilities genuinely undeterdetermined and therefore undefined. While Hájek takes instances where THE RATIO leaves conditional probabilities undefined as a reason in support of the negation of ST (and subsequently his positive thesis that conditional probabilities are more primitive than unconditional probabilities), I argue that this logic is inherently flawed - not only is THE RATIO consistent with the idea that conditional probabilities are more primitive than unconditional probabilities, but that it literally gestures to the fact that in some cases a full specification of unconditional probabilities may nevertheless leave conditional probabilities "undefined"! To this extent, the output of "undefined" is something worthy of deep reflection rather than a quick rectification.

I have argued that, when computing $P(B|A)$ where A is microscopic (i.e. $P(A) = 0$ if P is real-valued), there could be more than one admissible numerical assignment for $P(B|A)$ depending on the way in which we conceptualize A and the stochastic dependency relation between A and B. One question then arises is whether we can generalize this observation to discrete cases where $P(A) \neq 0$. My answer is yes, and I shall justify my conclusion in part II by sketching a semantic account of conditional probabilities and probabilities of conditionals.

Part III

CONDITIONAL PROBABILITY AND THE THESIS

To my knowledge, there are two dominant semantic accounts of conditional credence: the counterfactual account (proposed by Lowe) and the suppositional account (proposed by Edgington). I argue that the counterfactual account is in general untenable, whereas the suppositional account is basically correct but requires work to unpack the notion of supposition carefully. In particular, I argue that the conditional probability of B given that A varies depending on the specific manner and order in which we make our supposition about A in relation to our other background beliefs. To this extent, conditional probability $P(B|A)$ is a hypothetical probability that we assign to B within the suppositional context centered on A that is itself determined by our background beliefs and other contextual variables not encoded in A and AB. On this account, our previous observation about the context-dependency of $P(B|A)$ when A is microscopic therefore turns out to be a special case of a general phenomenon and reflective of something intrinsic about the notion of conditional credence.

4

THE COUNTERFACTUAL ACCOUNT

One seemingly plausible semantic account of conditional probability is given by [Lowe, 1996], who posits that the conditional probability $P(B|A) = x$ just in case, if $P(A) = 1$, then $P(B) = x$. It is worth pointing out that on one interpretation of Lowe's account of conditional probability, THE THESIS (P(*B* given that *A*)=P(if *A* then *B*)) is analytically true: it seems plausible that a rational agent's subjective credence in the conditional "if *A* then *B*" shares an intimate relationship with her hypothetical credence in B upon adjusting her credence in A to be 1. So P(if A, then B)=x iff $P(\text{if } P(A) = 1, \text{then } P(B) = x) = 1$ iff $P(P(B|A) = x) = 1$ iff $P(B|A) = x$.

Lowe concedes that sometimes the conditional probability of *B* given that *A* does not necessarily correspond to the probability that *B* would have if the probability of *A* were 1. To use Lowe's own example (which he borrowed from [Edgington, 1996]), I may have high degree of conditional belief that I won't know that my office is bugged given that it is the CIA who are bugging my office; yet if I were to believe that the CIA are bugging my office, then my degree of belief in me not knowing that my office is bugged is effectively zero. To put this schematically, let

$$C : \text{The CIA is bugging my office.}$$
$$K : \text{I know that my office is bugged.}$$

Then one may have high value for $P(\neg K|C)$. Yet since knowing that C implies K, one must have low value for $P(\neg K)$ if $P(C) = 1$ (recall that here we take probability to denote the degree of credence and therefore $P(C) = 1$ iff one is certain about C). So $P(\neg K|C)$ and $P(\neg K)$ if $P(C) = 1$ come apart, which contradicts Lowe's proposed definition of conditional probability.

In response, [Lowe, 2008] argues that the force of this counterexample depends on "an irrelevant *indexical* first-person characterization of the belief in question." (Lowe, 610) He notes that the reason why one has high degree of conditional belief in $\neg K$ given that C is because one believes that, "given that the CIA are bugging *any* ordinary citizen's office, that citizen won't know about it" (Lowe, 610, original italics) and I stand in no exceptional epistemic position to know otherwise. In other words, let

C_x : the CIA are bugging x's office
K_x : x knows that her office is bugged

Then C and K are just special cases of C_x and K_x where x is 'I'. So the argument goes as follows:

1. If I believe that the CIA are bugging *someone*'s office (i.e. $P(C_x) = 1$), then I think it is highly probable that *that person* won't know about it (i.e. $P(\neg K_x)$ is high).

2. So $P(\neg K_x|C_x)$ corresponds with the value of $P(\neg K_x)$ if $P(C_x) = 1$. Or we can formalize it using predicate logic as follows:

$$(\forall x)[P(\neg K_x|C_x) = p \text{ iff } P(\neg K_x) = p \text{ if } P(C_x) = 1]$$

3. Since I am no exception in this regard, by universal substitution we have:

$$P(\neg K_i|C_i) = p \text{ iff } P(\neg K_i) = p \text{ if } P(C_i) = 1$$

   where i stands for "I".

In other words, Lowe thinks what is responsible for the discrepancy between the counterfactual probability of $\neg K$ (if $P(C)$ were 1) and the conditional probability of $\neg K$ given that C is true, is a more general disagreement between our high counterfactual probabilities for $\neg K_x$ if $P(C_x)$ were 1 and low counterfactual probabilities for $\neg K$ if $P(C)$ were 1. But this disagreement arises from poor reasoning involving first-person indexicals that has nothing to do with the counterfactual account of conditional credence in particular. Therefore, Lowe thinks the objection does not apply once we figure out the true culprit that is messing up with our intuitions about hypothetical probabilities.

I argue that Lowe's universal-deduction analysis is plausible but cannot be the whole story. Observe that the universal-deduction analysis can only account for sentences that are in fact true for anyone irrespective of her particular identity. That is, it only holds for propositions that are particular instantiations of some general beliefs. Yet not all propositions have universal generalizations. Consider, for example, the following sentence:

> I am the only one on earth who believes that Jo is not a liar, even if Jo lies to me.

Let

$$B : \text{I believes Jo is not a liar.}$$
$$L : \text{Jo lies to me.}$$

In this case I have high degree of conditional belief that I believe Jo is not a liar given that Jo lies to me (i.e. my $P(B|L)$ is high). Yet if we adopt Lowe's definition of conditional probability, then my degree of conditional belief should be given by my hypothetical degree of belief that I believe Jo is not a liar, if I were certain that Jo lies to me. This should be effectively zero, for otherwise I would be certain that she lies to me and yet believes that it is probable that she is not a liar, which seems a *prima facie* contradiction.

And Lowe's universal-deduction analysis won't help in this case either. For let

$$B_x : x \text{ believes Jo is not a liar.}$$
$$L_x : \text{Jo lies to } x.$$

Then note that in this case I am indeed in a special epistemic position different from everyone else: I am by hypothesis the only one on earth for whom $P(B_x|L_x)$ is high (or at least I believe it as such). So I must not have determined my degree of conditional belief in B given that L from my degree of conditional belief in the universal characterization $(\forall x)P(B_x|C_x)$. The universal-deduction story could not avail the counterfactual theory in this particular case, and this - I argue - is indeed the true force of the original CIA-counterexample: the counterfactual characterization of conditionalization rules out *a priori* certain sets of second-order conditional predictions about our epistemic states. By stipulating that an agent's conditional credence $P(B|A)$ is given by her unconditional credence in B if she is fully confident in A, the theory fails to account for cases in which the proposition B stands in tension with the agent having full belief in A. In other words, the counterfactual theory requires that, when evaluating the conditional "if A then B," the subject makes not only 1 but 2 assumptions: a) A is true and b) I believe that A is true. As the previous examples aim to show, the second supposition about our own epistemic stance towards the antecedent is not only superfluous but in fact wrong.

But then the question arises: if assumptions a) and b) are in fact distinct, how then should a) inform or interact with b), i.e. how should assuming that A is true affect our doxastic attitudes towards A (as well as its logical or causal implications)?

Before I turn to this question, I want to raise one last point about Lowe's universal-deduction analysis. While I think it fails to save the counterfactual theory of conditional probability in its generality, it has its own virtue and does point to an interesting discrepancy between third-person and first-person evaluation of counterfactual probabilities, which I won't be able to explore in detail without digressing too much. Recall that Lowe is effectively arguing that, the first-person indexical confuses our judgment of counterfactual probabilities. For example, consider the second sentence with "I" replaced by "A", where "A" stands for the speaker to whom "I" presumably refers, i.e.

> A is the only one on earth who believes that Jo is not a liar, even if Jo lies to A.

Then the speaker's subjective conditional probability in A believing that Jo is not a liar (B_A), given that Jo lies to A (L_A), is high. What about her hypothetical unconditional credence in B_A, if she were to be certain that L_A? In this case my own intuition seems less clear and does in fact pull in both directions. On the one hand, the speaker is conscious of the fact that she is in fact A and therefore for her, to say "Jo lies to A" is the same as saying that "Jo lies to me". On the other hand, suppose the speaker is instead watching a film that features a protagonist who is exactly identical to her - they share the same name, family backgrounds, personal histories, physical traits, etc. In this case, I think the speaker can believe that "Jo lies to A" without believing that "Jo lies to me."While this scenario seems wildly implausible, all I am trying to motivate here is that by substituting the first-person indexical with a name, Jo might be able to step back and evaluate the sentence from an impartial spectator's perspective and therefore dissociate her second-order belief from her first-order belief: she may believe what Jo says *and* disbelieve that what Jo says is believable. In the second case, hypothetically adjusting one's credence in L_A to 1 won't trivialize her unconditional credence in B_A, and Lowe's prediction about the correspondence between $P(B_A|L_A)$ and $P(B_A)$ if $P(L_A)$ were 1 may come out true after all. While I shall not be delving into this question further for the interest of space, I think this is an interesting question and further research on how indexicals are to be treated in evaluations of conditionals and conditional probability may shed interesting light on the nature of supposition and the ontology of the objects of our beliefs.[1]

1 Partly inspired by this first-person/third-person discrepancy, I wonder if Lowe's counterfactual definition could be saved by making the following adjustments:

> **CT'** the conditional probability $P(B|A) = x$ just in case, if $P(ch(A)) = 1$, then $P(B) = x$

where $ch(A)$ stands for the chance of A. This would be equivalent to Lowe's original formulation if we also subscribe to Lewis' Principal Principle, which relates the agent's subjective probability and objective chance as follows

$$P(A|\text{ch}(A) = x) = x.$$

While it goes beyond the scope of this paper to explore this possibility in full details, my intuition is that the Principal Principle won't necessarily hold in the suppositional context for the mere reason that we seem to be able to suppose things that we believe to be impossible (physically or even metaphysically). Consider, for example, the following sentence:

If Jo falls into the Black Hole, then I won't know about it.

If $1 + 1 = 0$, then I'll lose my faith in everything.

In both cases, when evaluating the conditional, it seems that we are adjusting our beliefs in the objective chance of the antecedent without actually modifying our epistemic stance towards the proposition accordingly. If there is still some lingering doubt, I think the hypothetical acceptance-categorical acceptance distinction in the next section might help clarify things a bit more.

5

THE SUPPOSITIONAL ACCOUNT

I have argued that the counterfactual account as proposed by Lowe cannot give a coherent account of conditional probabilities where there is a tension between believing the consequent and having full belief in the antecedent.

Now, according to [Ramsey, 2010],

> "If two people are arguing 'if p will q?' and both are in doubt as to p, they are adding p hypothetically to their stock of knowledge and arguing on that basis about q. We can say that they are fixing their degrees of belief in q given p."

Inspired by the quotation from Ramsey, [Edgington, 1995] proposes an alternative account of conditional probability, which seems to fare better in this respect:

> (**Suppositional Account**) $P(B|A) = x$ just in case $P(B) = x$ on the supposition that A is true.

Note that the advantage of this account is that it does not specify what epistemic attitude one ought to take when supposing that A, and therefore it is invulnerable to counterexamples to the counterfactual account where $P(B) = 0$ whenever one believes A to be true. However, the lack of specificity can also be viewed as a disadvantage of this account to the extent that it is then open to interpretations which may themselves lead to unpalatable consequences. I shall address one such case in order to motivate my own modified suppositional account of conditional credence.

5.1 RAMSEY + MOORE = GOD

One way to interpret the quotation from Ramsey is to take it as specifying an acceptability condition for indicative conditionals as follows:

> **(Acceptability Condition)** "If p then q" is acceptable to a subject S just in case were S to accept p and consider q, S would accept q.[1]

1 I owe this formulation of the acceptability condition to [Chalmers& Hájek, 2007]

This formulation should remind us of Lowe's counterfactual definition of conditional probability. Indeed, since Ramsey seems to think that "if p then q" is acceptable just in case the probability of q given that p is sufficiently high, the acceptability condition can be read as equivalent to Lowe's counterfactual definition of conditional probability. To this extent, one variant of the suppositional account actually coincides with the counterfactual account. The reader may then reasonably expect it to be vulnerable to the same worry as mentioned in the previous section. While I think this is in fact true, I shall present an alternative objection given by [Chalmers and Hájek, 2007] so as to provide enough theoretical resources to motivate my own account.[2]

However, if this acceptability condition of indicative conditionals is the correct interpretation of the quotation from Ramsey, then it would appear that all rational agents must be omniscient and infallible. For let p be any proposition and consider the following sentences:

1. If p, then I accept that p.
2. If I accept that p, then p.

According to the Acceptability Condition, 1 is acceptable to a subject S just in case, were S to accept p, then S would accept that "I accept that p". Similarly 2 is acceptable to a subject S just in case, were S to accept that she accepts that p, then she would accept that p. But by Moorean principles of rationality,

> **(Moore's Principle I)** If an agent accepts that p, then she accepts that she accepts that p.

and

> **(Moore's Principle II)** If an agent accepts that she accepts that p, then she accepts that p.

So the Acceptability Condition, together with the two Moorean principle of rationality, jointly implies that 1 and 2 are true for any rational agents. Yet 1 and 2 are true just in case for every proposition

2 It is not an automatic equivalence because the relationship between probability and acceptability remains controversial. The conventional thought is that a proposition A is acceptable to a subject S just in case S's subjective probability for A is sufficiently high. This position has come under attack recently especially in light of the lottery paradox: let $1 - 1,000,000$ be lottery numbers, one of which is randomly selected as the winning number. When a subject buys a ticket with number n, the probability that n is not the winning number is given by $\frac{999,999}{1,000,000}$, which is very high. Yet it seems that the sentence "this number is not the winning number" could still be unacceptable, in the sense that its modal negation "this number may be the winning number" is acceptable (on the assumption that these two sentences cannot be both acceptable, for otherwise their conjunction "this number is not the winning number and it may be the winning number" should also be acceptable, which seems absurd). Note that no matter what threshold one uses as the cutting edge for (un)acceptability, one can choose a big enough number for the number of lottery tickets such that the probability that each particular number is the winning number is small enough.

p, it is true just in case the agent accepts it to be true. That is, the agent accepts every true proposition, and her mere acceptance of a proposition is sufficient for it to be true, i.e. she is omniscient and infallible. "So the Ramsey test and Moorean reasoning entail that rational subjects should accept that they have the epistemic powers of a god."[Chalmers and Hájek, 2007]

5.2 RAMSEY + MOORE ≠ GOD

The only two ways of blocking this argument seem to be 1) rejecting Chalmer and Hájek's formulation of the Acceptability Condition of indicative conditionals, or 2) rejecting the two Moorean principles of rationality. Since the two Moorean principles appear *prima facie* correct, [3] I shall constrain myself to the evaluation of the Acceptability Condition. [4]

3 This is not to say that they are uncontroversial. For example, given the semblance between acceptance and belief, it seems reasonable to suppose that acceptance could be dispositional and non-transparent. To this extent, the two principles may not hold in general just like knowledge and knowledge of knowledge or belief and belief of belief do not necessarily mutually imply each other. For example, due to ideological indoctrination an agent may actually accept that women are less intellectually competent than men, but, unaware of her own implicit stereotype, declines to accept that she accepts that women are less intellectually competent than men. However, I think the caveat of dispositional acceptance does not quite apply in this particular case because according to the Acceptance Condition of indicative conditionals, which Chalmers and Hájek extracted from Ramsey's quotation, implicitly requires the agent to not only accepts that p but also consciously reflects on her (hypothetical) acceptance of p. Admittedly, an agent may still decline to accept that she accepts what she actually accepts out of bad faith or false consciousness (due to ideological indoctrination), and certain beliefs/acceptances may be so cognitively entrenched in our mindsets that they are irreversible by mere wish or assertion of rejection (in this sense the case of implicit bias is still a good one: an agent may be self-deceived to think that she accepts gender equality while deep down in her heart she is still very much in the grip of self-sabotage based on her own gender.) However, setting these problems aside, the Moorean principles do have their intuitive appeals. After all, as [Chalmers & Hájek, 2003] point out, rejection of Moore's principles in general is "tantamount to accepting the Mooore-pardadoxical sentence 'Not-p and I believe p"' which sounds categorically irrational. Moreover, I think it is worth pointing out that after all, Moore's principles are *indicative conditionals* and therefore an analysis of the acceptability of Moore's principle without a prior analysis of the acceptability conditions for indicative conditionals is like a castle floating in the air.

4 It is also worth pointing out that Chalmers & Hájek themselves do not take their formulation of the Acceptability Condition as the only or most faithful interpretation of the quotation from Ramsey. In their only footnote, they suggest that perhaps the second sentence of the quotation could be read as "an indicative conditional 'if p then q' is acceptable to S iff S's conditional probability $P(q|p)$ is high." However, as [Leitgeb, 2011] points out and I intimated in my fleeting remarks about the equivalence between the Accetpability Condition and Lowe's counterfactual interpretation of conditional probabilities, whether or not the conditional-probability-interpretation of Ramsey's quotation avoids the Ramsey+Moore=God problem depends on how we define the value of a given conditional probability. In particular, on Lowe's account, the conditional probability of I accept that p given that p is x just in case, if I were to have full credence in p, then my degree of credence in me believing that p is x. But since I believe that p iff I have full credence in p, on Lowe's interpretation of conditional probability the "conditional-probability-interpretation"

The entailment of omniscience and infallibility highlights the fact that, despite its apparent plausibility, the Acceptability Condition fails to capture two important qualifications in Ramsey's original proposal. Recall that according to AC, in order to determine whether or not "if p then q" is acceptable S should

1. accept that p
2. adjudicate the acceptability of q

On the other hand, according to Ramsey, in deciding whether or not one should accept the conditional, one should

1. add p *hypothetically* to her stock of knowledge
2. decide *on that basis* the acceptability of q.

The differences are minor but significant. While a full exposition of their distinction requires quite a bit of technicality and terminology, intuitively, the qualification of "hypothetically" and "on that basis" are crucial because they capture two important facts about suppositional reasoning: 1) it is possible to hypothetically accept p without actually accepting that p, and 2) in order for the indicative conditional "if p then q" to be acceptable, there should be some kind of connection between p and q that holds independently of the particular truth-values of p and q. [5] Without these two qualifications, the Acceptability Condition effectively renders it impossible for the subject 1) to perform a second-order evaluation of her actual epistemic attitude towards p when assessing the acceptability of a conditional with antecedent p, and 2) to reject an indicative conditional when she actually believes that q whether or not p is true. In this regard, one does not need to appeal to conditionals involving second-order epistemic evaluations in order to see the problem with the Acceptability Condition. For example, consider the conditional

If I finish my thesis tonight, then it will rain tomorrow.

Suppose that, according to weather broadcast, it is likely to rain tomorrow. Then according to the Acceptability Condition, in order to adjudicate the acceptability of this conditional, one should i) accept that I finish my thesis tonight, and then ii) assess the likelihood of it raining tomorrow, which according to our assumption about the

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of Ramsey's test turns out to be equivalent to the qualitative formulation of the Acceptability Condition and therefore is vulnerable to the same problem.

5 Note that here we are talking about acceptability or assentability of "if p then q", which may be related to but should be distinguished from the truth-value of "if p then q" (if it has any). I take it to be uncontroversial that even for faithful proponents of material conditionals, the acceptability of an indicative conditional cannot be fully determined by the truth-values of its individual components. For otherwise the sentence "If I pray then God will listen" is acceptable just in case I do not pray, which I think is counterintuitive.

setup, is high. So the Acceptability Condition sanctions that the given conditional is acceptable. Yet even if this conditional is in fact acceptable, there seems to be something missing in the adjudication procedure: we accept the conditional solely based on the high likelihood of its consequent, which has nothing to do with the antecedent.

In his response to Chalmers & Hájek's analysis of the Ramsey Test for indicative conditionals, [Barnett, 2008] draws a distinctions between hypothetical acceptance (HA) and categorical acceptance (CA) as follows:

> **(HA)** *S hypothetically accepts* a proposition p just in case S accepts p in virtue of hypothesizing it, supposing it, or holding it on the basis of some hypothesis or supposition.

> **(CA)** *S categorically accepts* a proposition p just in case S accepts p independent of any hypothesis or supposition.

This distinction corresponds well to the widely acknowledged distinction between cognitive imagination and belief.[6] And while it is a well-known fact, it is worth emphasizing that a subject can hypothetically accept that p without categorically accepting that p. For example, sometimes in mathematical demonstration that p is true, one (who is not an intuitionist) may hypothetically accept $\neg p$ is true for the sake of a contradiction. Barnett argues that this distinction between hypothetical and categorical extends to considerations as well:

> **(HC)** *S hypothetically considers* that p just in case she considers p in virtue of hypothesizing it, supposing it, or holding it on the basis of some hypothesis or supposition.

and

> **(CC)** *S categorically considers* a proposition p just in case S considers p independent of any hypothesis or supposition.

Having these two distinctions at hand, Barnett then makes the following observations: when a subject is asked to suppose that p and then asked to evaluate the proposition "I accept that p", the task as presented is ambiguous because there are two sense of the word "acceptance" (i.e. HA or CA) and two ways in which she could be asked to assess the proposition (i.e. HC or CC, or equivalently within or outside of the suppositional context). Depending on the specification of

6 The two distinctions are not necessarily equivalent because, as I briefly mentioned in footnote 1 in my introduction, some may insist that supposition is different from cognitive imagination and belief is different acceptance (Cf. [Maher, 1990], [Arcangeli, 2014]). But I suppose here the main reason why Barnett introduces new terminologies, rather than appealing to the existing notions of supposition vs. acceptance, is that he wants to motivate a much more subtle distinction between hypothetical consideration and categorical consideration, which shall be introduced shortly.

the meaning of "acceptance" used and the evaluational context, the requirements of rationality turn out to be different and are summarized as follows:

"I accept that p"	**HA**	**CA**
CC	CA	indeterminate
HC	✗	✗

In short, the table does nothing more than reiterating the distinction between hypothetical and categorical acceptances: when the subject is merely hypothetically accepting p, her assumption about p only affects her belief about her hypothetical epistemic stance towards p, but not her belief regarding her real (i.e. categorical) epistemic stance towards p. In short, when she supposes that p and is asked to consider whether she actually supposes p, then her answer is yes; if she is asked instead whether she believes p, then her answer is indeterminate.

On the other hand, if the agent is asked to assess whether she accepts p within her suppositional context (i.e. by hypothetically considering the proposition "I accept that p"), is she required by rationality to accept, hypothetically or categorically, that she accepts that p? Barnett says neither. For by supposing that p, the agent does not make any extra hypothetical commitment to her own doxastic stance with respect to p, i.e. she does not suppose in addition that "I suppose that p" or "I believe that p" - all she supposes is that p. If we model the agent's cognitive enterprise in terms of boxes, then the foregoing analysis amounts to the observation that, upon hypothetically accepting that p, the agent

1. does not make any change whatsoever with respect to her *categorical* epistemic stance towards p;
2. forms a box of hypothetical acceptance that includes p and all her *initial* categorical acceptances prior to supposing that p
3. modifies her *posterior* categorical acceptances by adding the proposition "I suppose that p" (or I hypothetically accept that p)

Thus, on this interpretation of the Ramsey Test, upon supposing that p, the proposition "I accept that p" is acceptable if and only if it is read as "(from a non-hypothetical standpoint) I suppose that p". So the only way for "if p then I accept that p" to turn out to be true is for it to mean something like "If p, then (by virtue of evaluating a conditional with antecedent p) I suppose that p." Yet if we unpack the conditional in this way, then it does not imply that the subject is omniscient. So the case is far less controversial if not trivially true. And analogous reasoning shows that the other conditional - "if I accept that p, then p" - can be handled in similar fashion.

Where does this leave us? Recall that Lowe posits that the conditional probability $P(B|A)$ is defined as the probability that B would have if *P(A)* were 1. I have argued that this definition of conditional probability cannot account for cases in which B is inconsistent with the subject having full belief in A, such as "I won't believe that A given that A." However, if we modify the account into a suppositional account, then Barnett's distinction between hypothetical acceptance and categorical acceptance seems to give us enough theoretical resources to handle the counterexamples levied against the counterfactual account of conditional probability. Specifically

> **Suppositional Account of Conditional Probability** $P(B|A) = x$ iff *supposing* that A is true and *on that basis*, $P(B) = x$.

On the other hand, it also seems plausible that

> 'If A then B' is probable just in case supposing that A is probable and on that basis, B is probable.

So if we unpack the meaning and implication of the suppositional account correctly, then the meaning of conditional probability coincides with the meaning of indicative conditionals. Hence THE THESIS is true, if not trivially so: the conditional probability of B given that A is given by the probability of the corresponding conditional "if A then B" because to measure the conditional probability $P(B|A)$ just *is* to evaluate the likelihood of "if A then B."

However, traditionally supporters of the suppositional account of indicative conditionals (and therefore THE THESIS) have been facing two formidable challenges: the triviality argument, and a host of counterexamples whereby our credence in the conditional and our conditional credence given the antecedent seem to come apart. I argue that 1) the triviality argument is a special case of THE THESIS-violating sentence, and 2) both challenges can be met by properly analyzing the content and procedure of supposition that one is asked to perform when assessing the probability of indicative conditionals or its corresponding conditional probability.[7]

7 Surely if we take conditional probability to be a technical term, a fixed numerical value given by the Ratio formula or the extended integral analysis, then I agree with the objector that the conditional probability and the probability of conditionals need not coincide with each other. Yet if we take Hájek's starting point of treating conditional probability as a loaded notion, both semantically and metaphysically, then the inequality results seem much less convincing than they originally do. In particular, as I foreshadowed in my introduction, if we interpret conditional probability within the subjectivist framework and take it to mean something like the degree of conditional credence, then there is nothing *a priori* that should prevent us from at least entertaining the possibility that conditional probability, like its unconditional counterpart, could be relativized with respect to the specific content of the conditionalized event as well as the conditionalization procedure.

6

TWO OBJECTIONS TO THE THESIS

6.1 THE TRIVIALITY OBJECTION

One of the most famous objections to THE THESIS is given by [Lewis, 1976]: suppose that

$$P(A \to B) = P(B|A). \tag{1}$$

In addition, assume that belief functions for rational agents are closed under conditionalization, i.e. $P(\cdot|A)$ is a single-place probability function that also satisfies THE THESIS, namely

$$P(B \to C|A) = P_A(B \to C) = P_A(C|B) \tag{2}$$

for any proposition B and C. Then observe that, for any proposition A, B, C with $P(A\&B) \neq 0$, we have

$$P(A \to (B \to C)) = P_A(B \to C) = P_A(C|B) = \frac{P_A(BC)}{P_A(B)} = \frac{P(ABC)}{P(AB)} = P(C|AB). \tag{3}$$

Lastly, by the total law of probability, for any proposition A, B with $P(B) \neq 0$ and $P(\neg B) \neq 0$, we have

$$P(A) = P(A|B)P(B) + P(A|\neg B)P(\neg B) \tag{4}$$

Now let A, C be two propositions such that $P(AC)$ and $P(\neg A \neg C)$ are both positive, then

$$\begin{aligned} P(A \to C) &= P(A \to C|C)P(C) + P(A \to C|\neg C)P(\neg C) \\ &= P(C|AC)P(C) + P(C|A\neg C)P(\neg C) \\ &= 1 \cdot P(C) + 0 \cdot P(\neg C) \\ &= P(C) \end{aligned} \tag{5}$$

So (1)-(5) jointly imply that for any proposition A, C, $P(C|A) = P(C)$, i.e. A and C are probabilistically independent. Yet this surely seems absurd. To use one of Lewis' own examples, suppose a fair die is thrown. Let A be the event that it lands on an even number and C be the event that it lands on 6. Then $P(AC)$ and $P(\neg A \neg C)$ are both nonzero. But $P(C|A) = 1/2 \neq P(C) = 1/6$. In fact, Lewis observes that, if the foregoing analysis is correct, then the only probabilistic model in which $P(A \to C) = P(C|A)$ is true is one with only four possible numbers for probability assignments, i.e. for any proposition A we have $P(A) \in \{0, a, 1-a, 1\}$. THE THESIS could be true, but only trivially so in models that are too simple to represent the belief system of a rational agent.

Since (1) is THE THESIS, (3) comes from (1) and (2), (4) is a general law about probability and (5) is a direct derivation from (1)-(4), it appears that the only way one can resist THE THESIS is by rejecting (2). But (2) is in line with the general rule of Bayesian reasoning, which states that the posterior probability of B, upon observing that A, is given by

$$P'(B) = P_A(B) = P(B|A).$$

So it would appear that the triviality proof has put a nail in the coffin for THE THESIS. The only way out for those who desperately want to preserve the truth of THE THESIS is by positing that indicative conditionals are not propositions to begin with and hence are not proper objects of subjective probability functions (functions of which the domain is the Boolean algebra of propositions).[1]

However, I think Lewis' triviality argument is not irresistible. But before I expose its falsity, I shall present another objection to THE THESIS which argues that there are "Ramsey-violating" conditionals for which the degree of our posterior belief given the antecedent does not correspond with our degree of belief in the conditionals, i.e. $P(A \to C) \neq P(C|A)$. While this objection has often been discussed separately from the triviality results, following Kaufmann, I argue that they are in fact two sides of the same coin in the sense that both arguments point to the fact that there are two distinct and legitimate ways of computing conditional probabilities. However, unlike Kaufmann, who thinks that the distinction between these two methods lies in their different ways of modelling the situation, I argue that the two methods come apart not in terms of their different model-theoretic parameters, but with respect to the way in which we make our suppositions.

1 In fact (2) sounds very dubious when applied to conditionals given that it implies the following unintuitive consequences:

$$P(B \to C|A) = P(A \to C|B) = P(C|AB) \tag{1}$$

and as a result,

$$P(A \to C|A) = P(A \to C) \tag{2}$$

$$P(A \to C|C) = 1 \tag{3}$$

where 2 and 3 jointly imply the triviality result

$$P(A \to C) = P(C) \tag{4}$$

Yet intuitively, the sentences "if B then C, given that A" is not necessarily equiprobable as the sentence "if A then C given that B."

6.2 *the hospital*-OBJECTION

Suppose there are 72 patients in a hospital, 12 from town X and 60 from town Y. Among the 12 patients from X, 10 exhibit the symptom S for a particular disease D and 9 of them actually have disease D. On the other hand, there are only 10 patients from town Y who have the symptom S and 1 who is diagnosed to have disease D. Suppose you are about to check on Jo, and based on your information about Jo, you are 75% sure that she is from Y rather than X. I invite the reader to stop here and ponder the following question: what is the probability that, if Jo exhibits the symptom S, then she has the disease D?

To summarize the data

	X	Y
S	10	10
D	9	1
total	12	60

It appears that one can reason as follows:

1. It is likely that Jo is from community Y.
2. If Jo is from Y, then only 1 in 10 patients who bear the symptom S actually has the disease D.
3. So it is unlikely that if Jo exhibits the symptom S, then she has the disease D.

On this account, the probability that "if S then D" is low.

On the other hand, the conditional probability $P(D|S)$ is given by

$$\begin{aligned} P(D|S) &= \frac{P(SDX) + P(SDY)}{P(S)} \\ &= \frac{P(D|SX)P(X|S)P(S) + P(D|SY)P(Y|S)P(S)}{P(S)} \\ &= P(D|SX)P(X|S) + P(D|SY)P(Y|S) \\ &= 0.6 \end{aligned}$$

which is high. So our intuited probability of the conditional differs from the conditional probability, which contradicts THE THESIS.[2]

[Kaufmann, 2004] observes that these two observations do not justify a distinction between probability of conditionals and conditional probability. Rather, they reflect two ways in which we can reason about a given conditional. He notes that the computation of conditional probability as presented above can be presented qualitatively as follows

1. Suppose that Jo has the symptom S.

2 The numerical structure of the example first appeared [Kaufmann, 2004]. Here I used the version given in [Khoo, 2016].

2. Since only 10 out of 60 people from town Y exhibits symptom S, it is more likely that Jo is from town X.

3. Given that Jo is from town X, since 9 out of 10 people from town X who have symptom S actually has the disease D, it follows that it is likely that Jo has disease D.

On the other hand, the first observation can be formalized using probability logic as follows:

$$\begin{aligned} P(S \to D) &= P(S \to D|X)P(X) + P(S \to D|Y)P(Y) \\ &= P(D|SX)P(X) + P(D|SY)P(Y) \\ &= 0.3 \end{aligned}$$

Following Kaufmann, I shall call the method that yields low probability value in this particular case the "local probability" denoted as $P_l(S \to D)$, and the other method the "global probability." In general, let $X_1, X_2, \ldots, X_n$ be a partition of the population. Then the local probability of conditional "if A then B" is given by

$$P_l(A \to B) = \sum_{i=1}^{n} P(B|AX_i)P(X_i)$$

whereas the global probability of "if A then B" is given by

$$P_g(A \to B) = \sum_{i=1}^{n} P(B|AX_i)P(X_i|A)$$

As demonstrated in *The Hospital*, the local and global probabilities of a given conditional do not necessarily agree with each other in general. But then we seem to end up in a paradoxical situation in which two apparently sound arguments yield exactly opposite predictions with respect to the likelihood of a single conditional. Which, if any, is the correct probability of "if S, then D"?

[Douven, 2008] argues that the local probability method is inadmissible because depending on the way in which a population is partitioned, a subject may be asked to assign different local probabilities to the same conditional (Douven, 259). For example, suppose that we fine-grain our partitions of the patients based on their residency *and* levels of income. So instead of having two groups X and Y, we have four groups of patients X_1, X_2, Y_1 and Y_2 where X_1 and Y_1 are people who live below the poverty line. The data for the new partition scheme is given as follows:

	X_1	X_2	Y_1	Y_2
S	6	4	3	7
D	6	3	1	0
total	6	6	10	50

Now as before, you are 25% sure that Jo is from town X and 75% sure that she is from town Y. But you have no direct information about Jo's income level. So your subjective probability in Jo being a member of X_1 (i.e. Jo is from X and lives below the poverty line) is given by $P(X_1) = 1/2 \times 1/4 = 1/8$. Similarly $P(X_2) = 1/8, P(Y_1) = 1/8$ and $P(Y_2) = 5/8$. Now according to the rule of computing local probabilities,

$$\begin{aligned}
P_l(S \to D) &= P(S \to D|X_1)P(X_1) + P(S \to D|X_2)P(X_2) + P(S \to D|Y_1)P(Y_1) \\
&\quad + P(S \to D|Y_2)P(Y_2) \\
&= P(S|DX_1)P(X_1) + P(S|DX_2)P(X_2) + P(S|DY_1)P(Y_1) + P(S|DY_2)P(Y_2) \\
&= 1 \cdot 1/8 + 3/4 \cdot 1/8 + 1/3 \cdot 1/8 + 0 \cdot 5/8 \\
&= 25/96 \\
&\approx 0.26
\end{aligned}$$

So we may get different local probabilities merely by fine-graining the partition of our sample population. While I have foreshadowed that such partition-sensitivity might be plausible in the continuous setting where we have ill-defined intuitions about microscopic events, in the discrete case, it seems that partitions have much less bearing on how we conceptualize a given event. Therefore, the way in which we partition an event space in the discrete case should have little if any influence on the probabilities of a given conditional and the corresponding conditional probability.

Before I move on, it is worth pointing out that - as I have foreshadowed at the beginning of this section - given Kaufmann's distinction between local and global probabilities, Lewis' triviality proof is nothing but an extreme case of the local probability of "if A then C" where the event space is coarsely partitioned into C and $\neg C$:

$$P_l(A \to C) = P(A \to C|C)P(C) + P(A \to C|\neg C)P(\neg C)$$

Against this background partition, the corresponding global probability of "if A then C" is given by

$$\begin{aligned}
P_g(A \to C) &= P(C|AC)P(C|A) + P(C|A\neg C)P(\neg C|A) \\
&= 1 \cdot P(C|A) + 0 \cdot P(\neg C|A) \\
&= P(C|A)
\end{aligned}$$

Since the value of local probability is context-dependent, it follows that what Lewis has really shown is not that THE THESIS implies triviality, but that if THE THESIS is true, then the *local* probability of indicative conditionals *against the background partition of* $\{C, \neg C\}$

must be trivial. This is arguably much weaker if not trivially true on its own right. [3]

Now Douven contends that the rule of computing local probability is inconsistent because it yields different values for the same probability distribution under different partitions. This makes the local probability highly context-dependent. And Douven is pessimistic about the prospects of a contextualized theory of Bayesian epistemology. "To mention but one major hurdle for such a project: the possibility of contextually induced shifts in degrees of belief seems in direct conflict with the key Bayesian principle that the only rational shifts in one's degrees of belief are those brought about via the rule of conditionalization." (Douven, 263) Provided that the "rule of conditionalization" accords with the rule of computing global probabilities rather than local probabilities, Douven argues that our first intuition about *The Hospital* - the intuition that squares with the prediction of local probability - must be explained away rather than explained. [4]

In response, I think this objection is founded on the false assumption that ideally, the probabilistic representations of our credence should be partition-invariant. We have seen in the first chapter that, in relatively complex cases (which are nevertheless realizable in physical experiments), such assumptions do not hold in general: the conditional probabilities of certain microscopic events may be heavily dependent on the way in which we parametrize our event space. And I shall demonstrate that the same is true in discrete cases by considering another paradox that is surprisingly rarely discussed in the literature of probabilities: Simpson's Paradox.[5]

3 Douven made a similar point in footnote 5 of [Douven, 2006]. He verifies that "the method of calculating the probabilities of conditionals that respects THE THESIS [is partition-invariant]" by showing that $P(A \to C) = \sum_{i=1}^{n} P(C|AX_i)P(X_i|A) = \sum_{i=1}^{m} P(C|AX'_i)P(X'_i|A)$ for partitions $\{X_1, \ldots, X_n\}$ and $\{X'_1, \ldots, X'_m\}$. To this extent, another way to look at Lewis' triviality argument is that it is not so much an objection to that THE THESIS is false as a demonstration that the order of conditionalization affects the probability one would get. Lewis was onto something, but failed to push all the way through.

4 Note that Douven's argument is consistent with THE THESIS and to this extent he is very much an ally on my side. However, I disagree with Douven that the Ramsey-violating examples should be explained away by means of an error theory. While it is a well-known fact that people are generally bad at probabilistic reasoning, and sometimes their mistakes are systematic and predictable (e.g.conjunction fallacy), I think in this particular case the ambiguity is intrinsic to the notion of conditional probability. In particular, to my knowledge there has been no probability theory that gives well-defined method for computing embedded conditional probability: the probability of "C given that B" given that A. So unlike the conjunction fallacy, where we start off with one well-defined correct answer, in the case of conditionals we have no uncontroversial standard by which we can adjudicate the relative rationality/irrationality of human reasoning.

5 Kaufmann's response to Douven is slightly different: he appeals to a distinction between *model* and *scenario*. A *scenario* is a linguistic description of a particular physical setup and therefore could be under-determined, whereas a model is always well-

6.3 CONDITIONAL PROBABILITY AND SIMPSON'S PARADOX

Suppose that a college is hiring junior faculty members for two departments: Mathematics and Philosophy. Each department has 13 job applicants. For Philosophy, there are 5 male candidates, among whom 1 is hired, and 8 female candidates, among whom 2 are hired. On the Mathematics side, there are 8 male candidates, among whom 6 are hired, and 5 female candidates, among whom 4 are hired. The data is summarized as follows:[6]

	PHILOSOPHY	MATHEMATICS
MALE	1 hired 5 applied	6 hired 8 applied
FEMALE	2 hired 8 applied	4 hired 5 applied

Note that when we restrict ourselves to the employment data of each individual department, it seems that the hiring policy favors female candidates over male candidates (for $\frac{1}{5} < \frac{2}{8}$ and $\frac{6}{8} < \frac{4}{5}$). But when we aggregate the data together, we realize that the inequality is reversed: 7 out of 13 male candidates are hired, whereas 6 out of 13 female candidates are hired.[7]

Now instead of looking at the relative unconditional probability of being hired for male and female candidates, we can instead look at

---

defined. The same event can receive two incompatible probability assignments in two different scenarios. This distinction makes sense relative to Kaufmann's central thesis, which is that different scenarios represent different assumptions about causal in/dependencies. I shall illustrate Kaufmann's idea in *The Hospital*: if the agent supposes that the residency of a patient is causally independent of her residency, then when evaluating the conditional "if Jo has symptom S, then she has disease D", the agent would fix her prior credence about Jo's residency unchanged on the basis of supposing that she has symptom S. On the other hand, if she supposes that there is a causal relation (or stochastic dependence, to be more precise) between Jo's residency and her bearing the symptom S, then upon supposing that S, the agent would subsequently update her degree of belief in X to be $P(X|S)$, and hence her consequent degree of belief in the conditional "if S then D" would be informed by the global probability rather than local probability.

I think this analysis has its merit but cannot accommodate all scenarios. Instead, I argue that the suppositional account provides a simpler explanation for the phenomenon at stake: an agent's probability of conditional is given by her local or global conditional probability depending on the order in which she makes her supposition: if she holds her prior belief about the relative probabilities of a partition variable fixed and then evaluating her conditional probability with respect to this fixed background belief, then her probability in the conditional is given by the local probability or the conditional-conditional probability (conditional probability that is itself conditionalized on an extraneous background distribution). If the agent supposes the antecedent and subsequently adjusts *all* her degree of credence in her prior (partial) acceptances on the basis of her supposition, then her conditional credence in the consequent is given by her global probability.

6 This example is adapted from [Malinas and Bigelow, 2016].

7 The arithmetic is not complicated. Note that

$$\frac{a}{b} < \frac{A}{B} \text{ and } \frac{c}{d} < \frac{C}{D}$$

their relative conditional probabilities by recharacterizing the situation as follows:

suppose your job is to evaluate whether or not there is discrimination against female job candidates in the college hiring process. You come across the file of a job candidate, whose name is Jo. You know that Jo is applying for a job in either Philosophy or Mathematics, but you don't know which. In addition, you are not given any information about Jo's gender. Then you may ask yourself: what is the probability that, if Jo is male, then he is hired? According to Kaufmann, the local probability is given by

$$\begin{aligned} P_l(M \to H) &= P(M \to H|Phil)P(Phil) + P(M \to H|Math)P(Math) \\ &= P(M|H\&Phil)P(Phil) + P(M|H\&Math)P(Math) \\ &= 7/15 < \frac{1}{2}. \end{aligned}$$

On the other hand, the global conditional probability is given by

$$\begin{aligned} P_g(M \to H) &= P(H|M\&Phil)P(Phil|M) + P(H|M\&Math)P(Math|M) \\ &= \frac{7}{13} > \frac{1}{2}. \end{aligned}$$

So the paradoxical relationship between the relative probabilities of being hired as a male or female candidate is reaffirmed by the local-global conditional probability. This is hardly surprising, given that what gives rise to the Simpson's Paradox in the first place is the skewed population distributions across the partitions: despite the fact that there are equal numbers of male and female candidates, there are far more women applying to Philosophy that has a much lower hiring rate. As a result, if we hold fixed our prior credence in whether the candidate is applying for a job in Philosophy or Mathematics, then we are effectively oblivious to the structural feature that is responsible for the Simpson's Paradox. Hence we arrive at a similar conclusion as we would have should we looked at only the data from each sub-population. Once we take into account that the a male candidate is more likely to be applying for jobs in Mathematics, whereas a female candidate is more likely to be applying for jobs in Philosophy, then our conditional probability accords with our "global" observation when aggregating the data.[8]

is consistent with

$$\frac{a+c}{b+d} > \frac{A+C}{B+D}.$$

8 This is not to suggest, however, that one ought to favor global probabilities over local probabilities or *vice versa*. It is still an open question as to what data distribution that fits the Simpson-characteristics ought to be interpreted to make conclusion about causation or correlation. Similarly, I think both local and global probabilities are

In fact, it is not hard to show that there is a tight relationship between the Simpson's Paradox and the discrepancy between local and global probabilities.[9]

Theorem Let the statistics be as follows:

	X	Y
MALE	a hired / b applied	c hired / d applied
FEMALE	A hired / B applied	C hired / D applied

where a, b, c, d, A, B, C, D satisfy the "Simpson-relation", i.e.

$$\frac{a}{b} < \frac{A}{B}; \frac{c}{d} < \frac{C}{D} \text{ and } \frac{a+c}{b+d} > \frac{A+C}{B+D}$$

Assume that $P(X|M) = \frac{b}{b+d}$ and $P(X|F) = \frac{B}{B+D}$.[10] Then

$$P_l(Male \to Hired) < P_l(Female \to Hired)$$

but

$$P_g(Male \to Hired) > P_g(Female \to Hired).$$

Proof. The arithmetic is exactly isomorphic to the particular situation.

$$\begin{aligned} P_l(M \to H) &= P(M \to H|X)P(X) + P(M \to H|Y)P(Y) \\ &= P(M|H\&X)P(X) + P(M|H\&Y)P(Y) \\ &= P(X)(\frac{a}{b}) + P(Y)(\frac{c}{d}) \\ &< P(X)(\frac{A}{B}) + P(Y)(\frac{C}{D}) \\ &= P_l(F \to H) \end{aligned}$$

whereas

$$\begin{aligned} P_g(M \to H) &= P(H|M\&X)P(X|M) + P(H|M\&Y)P(Y|M) \\ &= \frac{a}{b} \cdot \frac{b}{b+d} + \frac{c}{d} \cdot \frac{d}{b+d} \\ &= \frac{a+c}{b+d} \\ &> \frac{A+C}{B+D} \\ &= P_g(F \to H) \end{aligned}$$

□

salient ways of computing the unconditional probability of a conditional. I shall address this point more in the following section.

9 I am using the hiring model for the sake of clarity and brevity, but it is clear that this result holds for any scenarios).

10 See Appendix for a general case in which $P(X) = P(Y) = \frac{1}{2}$

I have shown that there seems to exist some degree of structural similarity between Simpson's paradox and the local-global distinction in conditional probabilities. As Kaufmann points out in his reply to Douven, global probability is after all a special case of local probability with respect to a trivial partition of the entire event space. Thus, insofar as there is no compelling argument to analyze the data by aggregation or subdivision, I argue that neither is there an overriding reason to favor global probability to local probability or vice versa. The property of being partition-dependent is something deeply intrinsic to the notion of conditional probability that we have to embrace rather than dismiss.

Part IV

APPENDIX

6.4 probability primer

6.4.1 *Kolmogorov's axiomatization (unconditional probability-first)*

A probability space is $(\Omega, \mathcal{F}, P)$, where Ω is the sample space, $\mathcal{F}$ is the event space, and $P : \mathcal{F} \to [0, 1]$ is a function that satisfies the following properties

1. $P(E) \geq 0, \forall E \in \mathcal{F}$.
2. $P(\Omega) = 1$.
3. let $E_1, \ldots, E_n$ be mutually exclusive events, then

$$P(E_1 \cup \cdots \cup E_n) = \sum_{i=1}^{n} P(E_i).$$

6.4.2 *Popper's axiomatization (conditional probability-first)*

A probability space is $(\Omega, \mathcal{F}, P)$ where Ω is the sample space, $\mathcal{F}$ is the event space, and $P(\cdot|\cdot) : \mathcal{F} \times \mathcal{F} \to [0, 1]$ is a function that satisfies the following properties:

1. $\forall A, B \in \mathcal{F}, P(A|B) \geq 0$
2. $\forall A \in \mathcal{F}, P(A|A) = 1$.
3. If there exists a $C \in \mathcal{F}$ such that $P(C|B) \neq 1$, then $P(A|B) + P(\neg A|B) = 1$.
4. $\forall A, B, C \in \mathcal{F}, P(A \cap B|C) = P(A|B \cap C)P(B|A)$
5. $\forall A, B, C \in \mathcal{F}, P(A \cap B|C) = P(B \cap A|C)$
6. $\forall A, B, C \in \mathcal{F}, P(A|B \cap C) = P(A|C \cap B)$
7. There exist $A, B \in \mathcal{F}$ such that $P(A|B) \neq 1$.

6.5 the epistemological interpretation of the supervenience thesis

According to the epistemological interpretation of the Supervenience Thesis, the possession of unconditional credence in AB and A is necessary and sufficient for the formation of conditional credence in B given that A. For instance, an agent has a conditional credence in "it will be raining given that it is cloudy" just in case she has well-defined unconditional credence in proposition "it will be raining and it is cloudy" and "it is cloudy."

One of the main objectors to this version of ST is Price. In [Price, 1986], Price argues that unconditional credence in A and $A\&B$ are neither necessary nor sufficient for the possession of conditional credence in B given that A. It is unnecessary because more than often we seem capable of adjudicating our degree of belief in B on the condition that A without having any opinion *whatsoever* regarding A or $A\&B$. For example, I believe that, on the condition that it is cloudy tonight, my astronomy laboratory will be cancelled, while making no judgment on whether it is in fact going to be cloudy tonight. In this case, I have well-defined conditional credence in lab being cancelled given that it is cloudy, which is 1, but the ratio between my absolute credence in "it is cloudy and the lab is cancelled" and "it is cloudy" is undefined, because my credence in it being cloudy tonight is indeterminate.[11] So "the possession of absolute credence is not a necessary condition for the possession of conditional credence".[12]

Price also argued that the possession of unconditional credence is not a sufficient condition for the possession of conditional credence either. For example, I believe $1 = 1$ and the Earth orbits around the Sun are both true. So my subjective probabilities for both propositions are well-defined and have the value of 1. And yet it does not follow that I thereby have full credence that "the Earth orbits around the Sun given that $1 = 1$." Note that it is not Price's contention that my conditional credence does not equal to 1 - in fact, he may very well concede that if I were ever to acquire a conditional opinion regarding the "the Earth orbits around the Sun given that $1 = 1$," then the value of my conditional credence would or at least should agree with THE RATIO. Rather, his point is that the agent *does not have to* adopt any conditional belief regarding the composite sentence "the Earth orbits around the Sun given that $1 = 1$" in the first place. So we have a case in which the agent's absolute credence in A and $A\&B$

11 Some may object to this analysis by arguing that if I suspend my judgment on whether it is cloudy or not, then my subjective probability in "it is cloudy tonight" is not undefined but 0.5. I think Price can respond to this objection by making a distinction between "suspension of judgment" and "absence of judgment". It is plausible that after balancing the possibility of it being cloudy or not cloudy tonight, I find myself unable to give assent to either proposition. Yet this is not the situation that I am in when I assent to the conditional belief that the lab will be cancelled given that it is cloudy. Rather, the thought of whether it is cloudy or not simply has not crossed my mind at all.

12 Hájek made a similar point when defending his analysis of coin-tossing. According to Hájek, the conditional probability of the coin lands on head, given that I (i.e. Hájek) toss it fairly, is $1/2$, and yet the ratio of P(the coin lands on head and I toss it fairly) to P(I toss it fairly) (where P stands for the subjective probability function of the reader) is undefined because the reader knows nothing about Hájek's proclivity to throw the coin fairly. In response to the objection that a rational subject must assign *some* number to P(I toss it fairly) and P(the coin lands on head and I toss it fairly), Hájek pointed out that "you didn't assign any value to 'Toss', did you? But even if you did, for some reason, why must you assign a value to 'Toss'? No one is coercing you to make bets on 'Toss', or to form corresponding preferences; no one is coercing you to make the relevant judgments; no one is coercing you to form the relevant dispositions. And if someone did coerce you, we would get an answer alright, but it is doubtful if it would reveal anything about your state of mind prior to the coercion." (Hájek, 297)

are well-defined and yet her conditional credence in B given that A is not, which seems to suggest that the possession of the former is insufficient for the possession of the latter. Hence the Ratio is inadequate.[13]

However, I think Price's argument would not work for Hájek for three reasons. First, Price's objection is predicated on the assumption that conditional credence is the assent condition for indicative conditionals. This assumption is crucial because it allows Price to infer the reader's epistemic stance with respect to $P(B|A)$ from her doxastic attitude towards the conditional "if A, then B": since conditional credence is necessary and sufficient for assent to conditionals, then conversely a lack of assent to conditionals would indicate an absence of conditional credence and vice versa. To this extent Price's objection to the Ratio is at best *pro tanto*: it holds only if the sentences "B, given that A" and "if A then B" are semantically equivalent or at least intimately connected in terms of their expressive contents. In other words, if what the Ratio prescribes seems to contradict our daily experiences with conditional judgments, then it is plausible that the true culprit is not the Ratio but the assumption that one possesses a conditional credence in B given that A if and only if one possesses an unconditional credence in "if A then B". Thus what Price has shown is not that the Ratio is a false analysis of conditional credence, but that the adequacy of the Ratio as an analysis of conditional credence is inconsistent with THE THESIS that conditional credence represents the assentability of the corresponding conditional. Admittedly, this is not so much of a problem for Price, for the ultimate goal of his project is to vindicate THE THESIS as an analytic truth. Yet this would be a serious problem for Hájek, a devoted opponent to Adams' Thesis, if Price's objection is (partially) what he has in mind.

Secondly, Price argues that the possession of unconditional credence in A and B is insufficient for having conditional credence in $P(B|A)$. Yet the fact that one does not adopt a conditional credence in X does not imply that one *should not* adopt a conditional credence in X. Given that subjectivism is an idealized model of human epistemology that is built upon the (rather unrealistic) presupposition that our degrees of beliefs can be as fine-grained as all real numbers between 0 and 1, the Ratio seems to serve a rather normative role by prescribing how much credence a rational epistemic agent should have, as opposed to describing how much credence a normal human agents actually possesses. While Price may bite the bullet and argue

13 It is easy to object to Price's objection by accusing him of an unfaithful reading of the Ratio: the Ratio specifies what *is* or *constitutes* conditional credence, not a necessary and sufficient condition for possessing conditional credence. I think Price can respond to this objection by appealing to the Leibniz's law, or pointing out the apparent fact that Y is X only if one possesses X if and only if one possesses Y. So the fact that the ratio and conditional credence can be out of sync with respect to their mental possession by an agent is itself a piece of convincing evidence that they are not in fact identical with each other.

that even the idealized agent does not and should not adopt such a conditional credence just in virtue of possessing two well-defined unconditional credence, I think he cannot defend this line of argument without again appealing to the semantic equivalence between conditional credence and credence in conditionals. It may be true that we cannot and should not draw the conclusion "if $1 = 1$ then the Earth orbits around the Sun", even if we know that both the antecedent and the consequent are in fact true; yet it is less clear whether we should or should not infer "the Earth orbits around the Sun, on the condition that $1 = 1$" from the truths of each composite element of the sentence. Again, as I mentioned in the end of previous paragraph, this is not so much of a problem for Price given that he subscribes to THE THESIS, but this path is not open to Hájek in light of his rejection of equating conditional credence with credence in conditionals.

Thirdly, Price argues that we can have well-formed conditional credence without adopting any doxastic attitude towards its antecedent or consequent (assuming the truth of THE THESIS), and this shows that the Ratio is inadequate. Here I agree with Edgington that this is "an over-reaction". This reading of THE RATIO mistakes mathematical exposition as a description or prescription of the right order of human thinking - something that mathematical representation does not even aspire to achieve. In fact, the Ratio was first conceived of as:

$$P(A\&B) = P(B|A)P(A),$$

in which case conditional probability is taken to be primitive and works in conjunction with the unconditional probability of A to derive the joint probability of A and B. To this extent, it seems that the subject of the Ratio is not $P(B|A)$ *per se* but the *equality* between $P(B|A)$ and $P(A)$ as well as $P(A\&B)$. It describes a relationship that should hold in between conditional and unconditional credence without making any loaded assumption about which one has epistemic primacy (Edgington cited Russell's example that the fact that $P \wedge Q \equiv \neg(\neg P \vee \neg Q)$ does not imply that one is epistemically prior to the other, or that one must be able to believe one in order to acquire a full credence in the other). "Conditional degree of belief is an interesting concept to the extent that the ratios are stable fixture of a belief system, which can be settled independently of $P(A)$ and $P(A\&B)$." (Edgington, 267)

To recaptiulate: I have shown that Price's objection to the epistemological interpretation of ST is premised on the truth of THE THESIS that the conditional credence in B given that A determines the assentability of the conditional "if A then B", together with a false understanding of the function and purpose of mathematical exposition. Note that Hájek explicitly rejects THE THESIS, which is why I take him to be arguing against a non-epistemological interpretation of ST.

6.6 A SLIGHT GENERALIZATION OF SIMPSON'S PARADOX AND LOCAL PROBABILITIES

Theorem For Theorem 1, suppose instead that $P(X) = p$ and $P(Y) = 1 - p$ and $b + B = d + D$. Let $x_1 = \frac{a}{b}, x_2 = \frac{c}{d}, y_1 = \frac{A}{B}$ and $y_2 = \frac{C}{D}$. Assume that $x_i - y_j = x_m - y_n$ for all $i, j, m, n \in \{1,2\}$.[14]. Then $P_g(M \to H) > P_g(F \to H)$ iff

$$\frac{Dd}{(b+d)(B+D)} < p < \frac{bD + d(B+D)}{(b+d)(B+D)}$$

6.7 MULTIPLICATIVE AXIOM AND LOCAL PROBABILITY

Corollary 1. *The multiplicative axiom for conditional probability (MA) ($P(A|C) = P(A|B)P(B|C)$ whenever AC entails B and B entails C) is incompatible with the rule for computing local probabilities.*

Proof. consider a modified version of *The Hospital*: let the data be given as follows:

	X	Y
S	10	10
D	8	0
total	10	60

Then all patients who exhibit the symptom and actually have the disease are from twon X. And all patients from town X exhibits the symptom S. So S and D entail X and X entails S. Now according to MA, the conditional probability of $P(D|S)$ is given by

$$P(D|S) = P(D|X)P(X|S) = (\frac{8}{10})(\frac{1}{2}) = \frac{2}{5}.$$

On the other hand,

$$P_l(D|S) = P(D|SX)P(X) + P(D|SY)P(Y) = P(D|SX)P(X) = \frac{4}{35} \neq \frac{2}{5}.$$

□

14 This is hardly a relaxed assumption, since it is not even satisfied by the original example. However a more general case would be much less computationally tractable, and I deem this condition is relaxed enough to give the reader a sense of what are the determining factors for the value of p

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