Normality, Indifference and Induction: Themes in Epistemology and Logic

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Contained in this dissertation are four essays on epistemology and logic. They are self-standing essays and can be read independently from one another. However, I think there are a few common themes that run throughout. Here I will briefly highlight one.

In the first chapter I discuss the principle of indifference: if a body of evidence E supports p no more than it supports q, and likewise E supports q no more than it supports p, then one's credence in p ought to be equal to one's credence in q. One feature of this principle is that it can be employed in the absence of any evidence regarding p and q. If one has no evidence bearing on p and q, then one's evidence supports them equally. Call principles like these 'something-from-nothing principles'. Something-from-nothing principles are evidential principles that govern rational credences and beliefs in cases where has a substantial lack of evidence and background information.

One theme in this dissertation is the idea that something-from-nothing principles are problematic. In the first chapter, I defend an argument that the principle of indifference is inconsistent, and I show that a similar something-from-nothing principle in the imprecise confidence model is problematic. In the second chapter I motivate and defend a no-rules theory of induction. Hume's 'project the past into the future' is a something-from-nothing principle, and, focusing on Roger White's portrayal of this style of induction, I argue that the principle should be rejected.

Something-from-nothing principles principles are usually taken to be knowable a priori. If they are applicable in cases where no evidence or empirical knowledge is relevant to the use of the principle, then one must be in a position to know that the principle is true in such cases. The picture of evidential relations suggested by the no-rules account of induction defended in chapter 2 undermines the idea that evidential principles are knowable a priori. In chapters 3 and 4 I discuss normality theories of knowledge and justification and claim that it is natural for normality theorists to find something-from-nothing principles problematic.

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Preface

To Dad. I think I took your casual interest in philosophy too far.

1.0 The Evidential Principle

1.1 Introduction

In this paper I will investigate the principle of indifference, in various forms. First, I'll describe what I call 'the evidential principle', and show how it entails the principle of indifference. I will then rehearse the standard partition problem argument against indifference. I will then address a recent argument from Roger White to the effect that the partition problem should not lead one to reject the principle of indifference. White's argument hinges on a notion of evidential symmetry. I claim that evidential symmetry is really a mixture of two kinds of symmetry—evidential equivalence and evidential incommensurability. Once we make this distinction, White's argument can be shown to be unsound. I then turn to the principle of indifference reincarnated in the imprecise confidence model. This principle is sometimes called simply 'symmetry'. I show how the partition problem can also be leveraged into an argument against the symmetry principle. I claim that indifference principles, like the principle of indifference and symmetry, will in general run afoul of the distinction between evidential equivalence and evidential incommensurability.

1.2 Principle of Indifference

What we call the Principle of Indifference (PoI) today has its roots in the fomenting of classical probability. Inchoate versions of the principle can be found in Pascal, Leibniz and Jacob Bernoulli.[1] Laplace is taken to be the earliest to articulate the modern idea:

> The theory of chances consists in reducing all events of the same kind to a certain number of equally possible cases, that is to say, to cases whose existence we are

[1] For a history of probabilistic concepts before the 20th century, see [82]

> equally uncertain of, and in determining the number of cases favourable to the event whose probability is sought. The ratio of this number to that of all possible cases is the measure of this probability, which is thus only a fraction whose numerator is the number of favourable cases, and whose denominator is the number of all possible cases. [40]

For Laplace, and others of the classical period, all probabilistic reasoning could be understood as reasoning with a specific kind of ratio–the ratio of a number of equipossible cases to the number of all possible cases. Simple games of chance are paradigmatic cases. Suppose I have a six–sided die, and I want to know the probability of the die landing on an even number. Following Laplace, I consider all the equally possible cases of the die landing on an even number. There are three equally possible cases–the die landing on 2, 4 or 6. They are equally possible because I have no reason to think the die is more biased towards any specific even number. So the probability of the die landing on even is the number of the 'favourable' cases, 3, divided by the number of all possible cases, 6—in other words 3/6 or 1/2.

Laplace's account comes with its criticisms, however. Front and center is the concept of 'equally possible cases', and Laplace glosses this in terms of a 'we' and our 'uncertainty'. It sounds like this leaves open the possibility for one to be certain—albeit mistakenly–that the die will land on 6, and so for those who are so certain, the chance that the die will land on 6 is 1. But clearly Laplace did not want to leave this line open. The whole point of the early development of probability was to correctly represent risk in games of chance, law and other pursuits that involve quantitative uncertainty.[2] With this observation the account looks straightforwardly circular. When ought we be equally uncertain of two events? When they are equally possible. But Laplace gives no indication besides our very own uncertainty for when events are equally possible.

This objection lays the stage for the development of the foundations of probability theory in the 19th and 20th centuries. The frequentist tradition, originating with [88] and in

[2]Anyone today with a basic understanding of probability (and perhaps a modern calculator) would expect to make money at a gambling house in the 17th century.

modern form by [63] and [58], replaces the talk of 'all possible cases' with talk of 'all actual cases'. The subjectivist tradition, originating with [46] and popularized by [62] and [18], gives up the idea that probability measures some objective quantity. Rather, probabilities are just degrees of confidence–'we' and our 'uncertainty' is taken to ground to claims of 'equal possibility'.

With the recent popularization of personalist bayesianism themes in epistemology PoI has attracted interest, not as a grounding principle for the interpretation of probability but rather as a normative principle. It tells you how you ought to structure your confidence in certain evidential situations. Here is [92] in a passing discussion where he appeals to the principle in motivating a premise in an anti-skeptical argument:

> It is notoriously difficult to state what the relevant rule is [i.e. what the principle of indifference is] in a way that is informative enough but doesn't lead to contradictions (van Fraassen 1989; White 2010). Nevertheless, it is hard to deny that something along these lines is correct. In a state of ignorance concerning a class of exclusive possibilities, where we can see no reason to expect one to be true rather than another, we should put about equal credence in each. When we consider the vast array of ways that stars can be arranged, it would be unreasonably arbitrary to put a lot of confidence in some possible arrangements over others.

I think the most perspicuous way to state PoI is via the concept of evidential symmetry:

> **Principle of Indifference (PoI)**: If a body of evidence E supports p no more than it supports q, and likewise E supports q no more than it supports p, then one's credence in p ought to be equal one's credence in q.

In this paper I will be focusing in on the dialectical status of PoI. The front-and-center objection against PoI in the epistemological lore is that it is inconsistent. [91] defends PoI against this charge. I will argue that White's argument fails and that it is inconsistent.[3]

[3]I find this dismantling of White's argument for PoI important. White uses PoI in his argument against

But I will also show that the focus on PoI is misplaced. I claim that the idea behind PoI is encapsulated in what I call the evidential principle (EP). In the precise model of confidence, EP entails PoI. In the imprecise model of belief, EP entails a similar indifference-like principle I call Symmetry. Both of these indifference principles are suspect. The inconsistency of PoI should then be seen in the larger context that it is surprisingly difficult to make EP work within the two most popular models of confidence.

In section 2 I will introduce the terminology required to state EP and PoI, and show that White's argument against the latter's inconsistency fails. In section 3 I will show how EP motivates a popular indifference principle in the imprecise credence framework. Like PoI, however, it too has problems. In particular, I will argue, this principle leads to extremal confidence. The rationality of extremal confidence is suspect, and moreover it undermines the motivation for EP. In section 4 I conclude by reflecting what this discussion shows about the epistemological status of PoI and EP.

1.3 The Evidential Principle and Evidential Symmetry

Evidence comes in many shapes and sizes. Sometimes we are lucky enough to possess evidence that justifies more confidence in one proposition over another. Other times our evidence might support a range of alternatives equally. Still other times, our evidence might be too poor to justify putting more confidence in one proposition over another, simply because we have nothing to go on.

Let us formalize some of this. I will take a notion of evidential support as basic. Define the relation $p \succ_E q$ to mean that one's evidence E supports p more than it supports q. Throughout this paper I will drop the E subscript, but it is important to remember that it is there, and that the total evidence is supplied by the context. Relations of evidential support form the basis of rational confidence. A principle that codifies this basis is what I will call the evidential principle (EP):

inductive skepticism in [92], which I criticize in chapter 2 of this dissertation. There I show how the argument relies on PoI.

Evidential Principle: One ought to be more confident in p than q iff $p \succ q$.

The principle itself does not take a stand on what it means for one to be more confident in p than q. For now let's suppose that our confidence is best modeled by a standard probability function.[4] This means that 'more confident in p than q' should be understood as '$C(p) > C(q)$', a strict total order among probabilities, i.e. real numbers.

Consider what it would take for EP to be false. If my evidence supported p and q equally, but my confidence in p was greater than q, it seems my confidence has no grounding in the nature of the evidence. Likewise, if I was equally confident in p and q, but my evidence supported one over the other, it seems like I would be ignoring relevant features of my evidential situation. Let us assume until section 4 that EP is true. Until then I will be attempting to articulate the relationship between EP and indifference principles.

A few preliminary clarifications are in order. First, we are concerned with rational constraints on our degrees of confidence, and not about justification. Justification, I take it, is a property of beliefs. It is consistent with EP that one might have more justification to believe p than q even though one ought to be more confident in q than p. This is just to say that I do not presume evidentialism, roughly understood as the view that the justification of one's beliefs is solely determined by one's evidence. Second, it is also consistent with EP that even though p is only slightly more supported than q, you are much more confident in p than q, e.g. $C(p) = 0.99$ and $C(q) = 0.01$. EP only rules out situations where the comparisons do not match and says nothing about the magnitude of those comparisons.

Lastly, in order for EP to ultimately be plausible, more needs to be said about the notion of evidential support. Otherwise the principle is vulnerable to counterexamples. Here is such an example: Suppose you see a weather report that there's an 80% chance that it will snow in southern Minnesota tomorrow and that there's a 70% chance that it will snow

[4] That is, C is a function that obeys the axioms of probability:

(1) $C(p) \geq O$
(2) $C(T) = 1$, where T is a tautology
(3) If p and q are disjoint, then $C(p \vee q) = C(p) + C(q)$

in Wisconsin. Now, suppose you tune into another weather report, which you know has completely independent reporting. They report that there will be a 70% chance of snow in Wisconsin but you miss their forecast for Minnesota. Basing your confidence on on weather reports, you are 80% confidence that it will snow in Minnesota and around 72.5% confident that it will snow in Wisconsin. However, the counterexample goes, your evidence supports snow in Wisconsin more than it supports snow in Minnesota. There are two independent predictions for the former, but only one for the latter. In other words, evidential support takes into account evidential balance, but also weight.[5] One way to skirt the objection is to rephrase EP in terms of the evidential balance. Another would be to be more precise about how to understand $p \succ q$ in terms of balance, weight, specificity, etc. At any rate, let us put initial worries about EP aside.

Next, let us can define a notion of evidential symmetry.[6] Propositions p and q are evidentially symmetric ($p \approx q$) if and only if one's evidence neither supports p over q, nor supports q over p. Or, in symbols:

$$p \approx q \text{ iff } \neg(p \succ q) \wedge \neg(q \succ p)$$

Evidential symmetry can be glossed like this: p and q are evidentially symmetric for some total evidence E just in case E gives you no more reason to think that p is true than that q is true, and no more reason to think q is true than that p is true. Therefore, the overall character of the evidence and how it bears on what to believe is symmetric for both p and q. With these definitions in hand, we can formulate the principle of indifference:

Principle of Indifference (PoI): One's confidence ought to satisfy $C(p) = C(q)$

[5]If evidential support is understood in terms of probability, evidential balance in favor of p is the evidential probability $P(p)$, while the evidential weight can be modeled by properties of the conditional probabilities $P(p|-)$. If p has a lot of evidential weight, then new evidence will not effect the balance of evidence as much—the conditional probabilities are in a sense stable. Consider the case of estimating the bias of a coin: if we have observed 50/100 heads, then there is a substantial amount of weight, and a string of 5 tails in a row will not greatly affect our estimate of the coin being unbiased, while if we have only observed 2/4 heads, then a string of 5 tails will substantially affect our confidence that the coin is biased. For more about these distinctions, see [33].

[6]Not to be confused with the symmetry principle I discuss in section 3.

if $p \approx q$.

EP entails PoI under our current assumption that confidence is modeled as a probability function: if $\neg(C(p) > C(q))$ and $\neg(C(q) > C(p))$, it follows that $C(p) = C(q)$. This observation should strike one as a bit surprising, given the controversial nature of PoI and the platitudinal appearance of EP.

Evidential symmetry casts quite a broad net. One can be in a symmetric evidential situation in a number of ways. For example, if you know a coin is unbiased, then you have no more reason to believe the coin will turn up heads than that it will turn up tails, and vice versa. You'd then be in a situation of evidential symmetry. Now suppose you had no reason to believe that the coin has a specific bias. In this scenario too you would have no more reason to believe the coin will turn up heads than tails, and this is not because of something specific the evidence tells you about the objective chances. It's just that the evidence is too sparse to recommend p over q or q over p.

Here's an illustrative example:

> **Two Urns:** There are two urns in front of you. You are told that these urns contain red, blue and yellow balls. For the first urn, you were able to see that the urn contains 10 balls of each color. For the second urn, you have no idea how many balls of each color there are inside the urn. A random ball is then chosen from each urn.

It follows from PoI that if $p_1, ..., p_n$ is a partition of the space of possibilities, and $p_1 \approx ... \approx p_n$, then for all i, $C(p_i) = 1/n$. Let p_r , p_b , and p_y be the propositions that the first ball drawn will be red, blue and yellow respectively. In the case of first urn, we know that there is an equal chance of a red, blue and yellow ball being chosen, and so $p_r \approx p_b \approx p_y$, and hence by PoI one's confidence ought to be such that $C(p_r) = C(p_b) = C(p_y) = 1/3$. What about the second urn? You do not know that there is an equal chance of a red, blue and yellow ball chosen. But your evidence is still symmetric, this time just for a different reason—insofar as you have no reason to suspect any colored ball over another, you have no more reason to believe, say p_r over p_b, nor any more reason to believe p_b over p_r, etc. So in the case of the second urn your evidential state is such that $p_r \approx p_b \approx p_y$, and by PoI your confidence ought to be such that $C(p_r) = C(p_b) = C(p_y) = 1/3$.

1.3.1 Objections to PoI

It is tempting to immediately strike an objection against PoI on the basis of the asymmetry of the two urn cases—the principle applies equally to the first and second urn, but, the objection goes, one's evidential states are importantly different. For both urns, $C(p_i) = 1/3$, but with the first urn you know the objective chances, while for the second you do not. If you adopt the confidences required by PoI for the second urn, you are acting as if you knew something about the objective chances, but you really don't.

I think there is something right about this objection (and I'll say more about why later in this section), but there is also something wrong. To be clear, PoI does not uniquely determine a confidence distribution. All it requires is that one gives equal confidence to propositions equally treated by one's evidence. If you look at the overall confidence distribution for the two urns, you will see two very different belief states. For the first urn, if you observe 5 red balls in a row (with replacement), your confidence in the distribution of balls does not change—i.e. $C(p_r) = C(p_r|$ 5 red balls). This is because you know the distribution of balls, and if you know the distribution, your confidence of drawing any color of ball is independent of any information regarding the outcome of the draws. In other words, observing outcomes does not break the evidential symmetry, since the source of the symmetry is the objective chances. But in the case of the second urn, observing outcomes does break the evidential symmetry. If you observe 5 red balls (with replacement), then you will be more confident that the next ball drawn will be red—i.e. $C(p_r) > C(p_r|$ 5 red balls). The symmetry is broken because you've gained a reason to think there is a higher proportion of red balls. (You gain no such reason in the case of the first urn, since you know that the chances are independent of observed draws.) If one's confidences were such that $C(p_r) = C(pr|$ 5 red balls) for the second urn, then one would be acting like one has more information than one does. But PoI does not require you to adopt or not adopt the independence condition. All it says, again which is not much at all, is that one's confidence match the symmetry. How exactly one's confidence does this is determined by other relevant aspects of the evidential situation.

A more pressing objection is that PoI is inconsistent. The main idea is that the space of possibilities can be partitioned in various ways, and PoI will give contradicting answers if those partitions are non-linearly related. Here is an example based on [87] and [91]:

> **Mystery Square:** The last steel mill in Pittsburgh produces square metal plates. All that is known about the factory is that it produces plates that are no more than two feet wide. Being shrouded in secrecy, nothing else is known about the factory.

Suppose a square plate comes off the line in the factory. How confident are you that, say, the plate is less than 1 square foot? Define the following partitions:

$A1$: $0 \leq$ area < 1 square foot,
$A2$: $1 \leq$ area < 2 square feet,
$A3$: $2 \leq$ area < 3 square feet,
$A4$: $3 \leq$ area < 4 square feet,

You have no more reason to think the plate is less than 1 square foot than between 1 and 2 square feet, and similarly for any partition. So $A1 \approx A2 \approx A3 \approx A4$, and by PoI one's confidence ought to be such that $C(A1) = C(A2) = C(A3) = C(A4) = 1/4$. Now, consider another partition:

$L1$: $0 \leq$ length < 1 square foot,
$L2$: $1 \leq$ length < 2 square feet,

Again, you have no more reason to think the width of the plates is less than 1 foot than that the width is between 1 and 2 feet. So $L1 \approx L2$, and by PoI one's confidence ought to be such that $C(L1) = C(L2) = 1/2$. This straightforwardly entails a contradiction. Here is the argument from [91]:

(1) $L1 \approx L2$	Premise
(2) $A1 \approx A2 \approx A3 \approx A4$	Premise
(3) $C(L1) = 1/2$	PoI, (1)
(4) $C(A1) = 1/4$	PoI, (2)
(5) $C(L1) = C(A1)$	($L1$ and $A1$ are equivalent),
(6) $Contradiction$	3,4 and 5

PoI leads to a contradiction, and so, the argument goes, it must be false.[7] Premises (1)

[7]This might be a bit too quick. To adequately model the **Mystery Square** scenario we really need to talk about probability density functions, since we are working with continuous quantities. In the context of probability densities, our discussion seems to entail that PoI requires that we have a uniform confidence distribution over possible width and a uniform confidence distribution over possible areas. We might think this is a mistake—PoI requires that we treat each partition the same, and all the above shows is that a uniform distribution is not *translation invariant* over width and area. Rather, we need a probability density function that is translation invariant, i.e. if $[a, b]$ is an interval in units of width, and $[c, d]$ is an interval in units of area, and $[a, b]$ and $[b, c]$ pick out the same range of square plates, then $C(a \leq width \leq b) = C(c \leq area \leq d)$. It turns out there is such a distribution. The distribution is generated by a distance metric, m. I will spare the full derivation, but the idea is that $C(a \leq unit \leq b) = m(a, b)/m(lower, upper)$, where *lower* is the lower bound on the unit and upper is the upper bound on the unit. (Since our probability density function is unitless, we must divide by $m(lower, upper)$ to factor in units.) To avoid zeros, we assume that all square plates are at least one foot in width. We then need a metric m such that:

$$m(1, 2)/m(1, 3) = m(1, 4)/m(1, 9)$$
$$m(2, 3)/m(1, 3) = m(4, 9)/m(1, 9)$$
$$\ldots \text{ etc } \ldots$$

The trick is that area is width^2, and so the equations can be written with the form $m(2, 3)/m(1, 3) = m(22, 32)/m(12, 32)$. If $m(x, y) = log(y) - log(x)$, then $m(x2, y2) = 2 * m(x, y)$. The factor of 2 can then be canceled:

$$m(2^2, 3^2)/m(1^2, 3^2) = 2m(2, 3)/2m(1, 3) = m(2, 3)/m(1, 3).$$

This shows that the joint probability density function such that $C(a \leq x \leq b; lower; upper) = log(b/a)/log(upper/lower)$ is translation invariant over width and area. (This derivation is inspired by [87]). This seems like a candidate resolution to the inconsistency. However, it can be shown that this is the only such translation invariant distribution (up to scalar multiplication—this follows from van Fraasen (1989, fn 14). We can construct another partition, for example and (2) seem to be true based on our definition of evidential symmetry. The motivation behind premise (5) is that if p and q are logically equivalent, then it must be the case that $C(p) = C(q)$ by probabilistic coherence. However, there might be some room to maneuver a response to the argument by rejecting (5). For example, [57] writes:

> "However, far from being paradoxical or inconsistent, this [namely, that (5) is false] in fact seems exactly right. What rationality demands of an agent is determined by the resources that are available to her. If her conceptual scheme is impoverished to the extent that she distinguishes only blue/purple from its complement [—length in our example—], then rationality requires one thing. As her conceptual scheme expands to permit more possibilities, however, so that she can now distinguish red/orange from yellow/green [—length from area in our example—], rationality requires something else." (170)

The idea is that one's confidence is rationally constrained by the makeup of one's conceptual scheme. The argument above then should be written as

(1) $L1 \approx L2$	Premise
(2) $A1 \approx A2 \approx A3 \approx A4$	Premise
(3*) $C_1(L1) = 1/2$	PoI, 1
(4*) $C_2(A1) = 1/4$	PoI, 2
(5*) $C_1(L1) = C_2(A1)$	($L1$ and $A1$ are equivalent),
(6) *Contradiction*	3,4 and 5

C_1 is the conceptual scheme where the agent only considers the widths of the square

$$0 \leq (width + area)/2 < 1,$$
$$1 \leq (width + area)/2 < 3,$$

which is not also invariant via measure m. Because of uniqueness, there is no other measure m^* such that m^* generates an invariant distribution over width, area and $(width + area)/2$. We could then run the argument in the same way as before with these three partitions, and arrive at a contradiction. So appeal to translation invariance will ultimately not help.

plates, and $C2$ is the conceptual scheme where the agent only considers square area. Premise (5*) would lose all motivation—it is not a requirement of probabilistic coherence that two distinct probability functions assign the same probability to two logically equivalent possibilities. Hence, according to Pettigrew's line, there is no inconsistency.

I find this a very tough sell. It needs the claim that it is *impossible* for an agent to simultaneously consider in her conceptual scheme (whatever one might mean by that) lengths and areas. If it was possible, then there would be no reason why we could not model the agent's confidence over both, in which case the contradiction would go through. For some conceptual schemes S_1 and S_2 it's reasonable to think that an agent cannot occupy S_1 and S_2 simultaneously. Pettigrew mentions that this may come about because S_1 might be too impoverished and without the resources to make the distinctions necessary in S_2. But in our case, in order to even understand the concept of area one also must understand the concept of width, since area is just width squared. So, even if Pettigrew's line can get him out of some paradoxical applications of PoI like the one he considers in the quote, it won't help for the Mystery Square scenario.

[91] takes a different approach and argues that (1) and (2) are not as innocent as they appear. If you take them, along with a few other premises, you can derive a contradiction without the help of PoI. And if you can derive a contradiction from (1) and (2), then the original reductio is not a reductio of PoI. Here's his argument:

> *Transitivity*: If $p \approx q$, and $q \approx r$, then $p \approx r$.
>
> *Equivalence*: If p and q are known to be equivalent, then $p \approx q$.
>
> *Symmetry Preservation*: If $p \approx q$ and r is known to be inconsistent with both p and with q, then $(p \vee r) \approx (q \vee r)$

(1) $L1 \approx L2$	(Premise)
(2) $A1 \approx A2 \approx A3 \approx A4$	(Premise)
(3) $L1 \approx A1$	(Equivalence)
(4) $L2 \approx (A1 \vee A3 \vee A4)$	(Equivalence)
(5) $A2 \approx (A2 \vee A3 \vee A4)$	(Transitivity)

(6) $(A1 \vee A2) \approx (A1 \vee A2 \vee A3 \vee A4)$ (Symmetry Preservation)

(7) $(A1 \vee A2 \vee A3 \vee A4) \succ (A1 \vee A2)$ (specifics of the case)

(8) *Contradiction* (6, 7 and definition of $\approx$)

White thinks we should not suspect that anything is wrong with transitivity or equivalence, given that we are thinking of symmetry as a kind of equivalence relation (though more on this later). As for symmetry preservation, it's hard to see how it could be false. It's false just in case r is inconsistent with p and q, and you have no more reason to believe p over q, but you have more reason to believe $p \vee r$ over $q \vee r$. But any reason to believe $p \vee r$ over $q \vee r$ must come down to reasons to believe r, since $p \approx q$. Any reason to believe r though would give you equal reason to believe $p \vee r$ and $q \vee r$. So you can't have more reason to believe $p \vee r$ over $q \vee r$. Lastly, premise (7) simply follows from the specifics of the case—you have more reason to believe $(A1 \vee A2 \vee A3 \vee A4)$ over $(A1 \vee A2)$ as you know $(A1 \vee A2 \vee A3 \vee A4)$ but do not know $(A1 \vee A2)$. So, White argues, the multiple partitions argument is not a reductio of PoI. Rather, it's a reductio of either (1) or (2). Since (1) and (2) both appear in the original reductio of PoI, we should not blame PoI for the contradiction.

White doesn't say which of (1) or (2) is false specifically. He just says that one's reasons need not be transparent, and acknowledges that this conclusion is a bit puzzling. But it's no surprise that rejecting (1) or (2) is puzzling—White's argument is invalid. To see why, consider again the urn example at the beginning of this section:

> **Two Urns:** There are two urns in front of you. You are told that these urns contain red, blue and yellow balls. For the first urn, you were able to see that the urn contains 10 balls of each color. For the second urn, you have no idea how many balls of each color there are inside the urn. A random ball is then chosen from each urn.

In both cases our evidence is symmetric, but clearly there is a difference between the cases. In the first urn you have *equal evidence* supporting p_r, p_b, and p_y. You have no more reason to believe pr than p_b because you have equal reason to believe both. In the second case, however, you have no more reason to believe p_r than p_b because you have no evidence bearing on either. Let us get a bit more precise about this difference. Let the relation $p = q$ mean that the evidence supports (or refutes) p and q to some equivalent equal amount. I call this *evidential equivalence.* For the second urn, $(p_r = p_b)$. Your evidence does not support (or refute) p_r and p_b to some equivalent amount, because it does not lend any support or refutation at all towards p_r and p_b. I will call propositions in this position *evidentially incommensurable*: propositions p and q are *evidentially incommensurable* $(p \asymp q)$ if and only if one's evidence has no specific bearing on the difference between p and q. I take evidential incommensurability to be stipulated by this definition:

$$p \asymp q \text{ iff } \neg(p \succ q) \wedge \neg(q \succ p) \wedge \neg(p = q).$$

We can then rewrite our definition of evidential symmetry as

$$p \approx q \text{ iff } \neg(p \succ q) \wedge \neg(q \succ p) \text{ iff } (p = q) \vee (p \asymp q).$$

All we have done so far is just analyze evidential symmetry into the two ways such symmetry could be instantiated. The urn case is a situation where the symmetry is instantiated in the two different ways. For the first urn $p_r = p_b = p_y$, while for the second, $pr \asymp pb \asymp py$.

With this distinction in hand, we can see that White's argument equivocates on the two instantiations.[8] Because '=' and '$\asymp$' are mutually exclusive, we can replace each '$\approx$' with the relevant instantiated disjunct. Clearly $L1 \asymp L2$ and $A1 \asymp A2 \asymp A3 \asymp A4$, since we do not have any evidence bearing on the width or area of the factory's plates. The way our evidence bears on the width and area of the plates is similar to the way our evidence bears on the composition of the second urn. Note however that the premises which appeal to *Equivalence* must involve evidential equivalence, not incommensurability. You know that $L1$ and $A1$ are

[8] [After working out the details of this argument I came across a paper by [75] which essentially makes the same critique of White's argument. For the purpose of the dissertation I won't respond to Smith's paper, but if I prepare this paper for publication I will incorporate it.]

equivalent propositions, and so there *is* a specific bearing on the difference between $L1$ and $A1$—namely, any reason to think that a plate will have a side length between 0 and 1 is also an equivalent reason to think that a plate will have an area between 0 and 1.

The argument is then the following, making explicit the three uses of transitivity:

(1) $L1 \asymp L2$	(Premise)
(2) $A1 \asymp A2 \asymp A3 \asymp A4$	(Premise)
(3) $L1 = A1$	(Equivalence)
(4) $L2 = (A2 \vee A3 \vee A4)$	(Equivalence)
(5_1) $A1 \asymp L2$	(Transitivity, 1, 3)
(5_2) $A2 \asymp L2$	(Transitivity, 2, 5_1)
(5_3) $A2 \asymp (A2 \vee A3 \vee A4)$	(Transitivity, 4, 5_2)
(6) $(A1 \vee A2) \asymp (A1 \vee A2 \vee A3 \vee A4)$	(Symmetry Preservation)
(7) $(A1 \vee A2 \vee A3 \vee A4) \succ (A1 \vee A2)$	(specifics of the case)
(8) *Contradiction*	(6, 7 and definition of $\approx$)

The first and third uses of transitivity are fine. The first takes the form:

$L2 \asymp L1$

$L1 = A1$,

Therefore $L2 \asymp A1$.

To see that this is valid, suppose your evidence did have some bearing on $L2$ versus $A1$ (including the possibility that they are supported equally), i.e. that $\neg(L2 \asymp A1)$. Then since $L1 = A1$, any support (or refutation) your evidence has for $A1$ must also be support (or refutation) for $L1$. But then your evidence can't have no bearing on $L2$ versus $L1$—your evidence has bearing on $L2$ versus $A1$, and the bearing for $A1$ must also be bearing for $L1$, and so your evidence has bearing on $L2$ versus $L1$. The third use of transitivity takes a similar form. Generally, $A \asymp B$, $B = C$, therefore $A \asymp C$ is valid.

The second use of transitivity is spurious, however. The inference is:

$A2 \asymp A1$,

$A1 \asymp L2$,

therefore $A2 \asymp L2$

Your evidence has no bearing on $A1$ versus $A2$, and $A1$ versus $L2$. But it does have a bearing on $A2$ and $L2$. Namely, you know that there will be at least as many plates with an area between 1 and 2 as there are plates with a width between 1 and 2, since $A2$ entails $L2$. In the context of our relations defined so far, if p logically entails q, then $p \leq q$, i.e. $(p \succeq q \vee p = q)$, and hence $\neg(p \asymp q)$. Returning to the urns, in the second urn, the proposition that there is at least 10 balls in the urn is incommensurable with the proposition that there is one green ball, since again we know nothing about the composition of the urn. Likewise, the proposition that there is one green ball is incommensurable with the proposition that there are at least eight balls in the urn. But it does not follow that the proposition that there are at least 10 balls in the urn is incommensurable with the proposition that there are at least 8 balls in the urn. Generally, our relation of evidential incommensurability is not transitive.

1.3.2 PoI and EP

Let us take stock. White's argument relies on the notion of evidential symmetry. I hope to have made plausible, with the case of the two urns as a motivating example, that evidential symmetry can be instantiated in two mutually exclusive ways—one way is where one's evidence bears on the propositions in question, and bears on them equally. The other way is where one's evidence does not bear on the propositions in question, and in so doing bears on them equally (i.e. not at all). This second way that evidential symmetry can be instantiated is not transitive, and White tacitly assumes this in his argument.

So, I claim, despite White's argument PoI is inconsistent. But EP entails PoI. Does this then mean we should reject EP? Perhaps, but a rejection at this point would be too quick. Recall that EP is a principle that relates two relations; '$\succ$', a support relation between propositions; and '$>$', an ordering on confidence. In responding to White's argument, I've argued that we should not think of '$\succ$' as totally ordered. In the second urn, your evidence does not support equally, e.g., the propositions the the next ball is green and the next ball is red, nor does your evidence support one over the other. I have, though, been assuming that the ordering '$>$' over confidence is totally ordered, and I relied on this when deriving PoI from EP. But we might think that this mismatch between '$>$' and '$\succ$' is problematic. Instead of rejecting EP, we ought to model confidence in such a way that makes confidence not totally ordered. A popular model that achieves this is imprecise Bayesianism, which I will turn to now.

1.4 Imprecise Confidence and Symmetry

What I will call imprecise bayesianism is characterized by three theses:[9]

> **Representors:** An agent's confidence is represented by a set C of probability functions
>
> **Conditionalization:** Upon acquiring some bit of evidence E, a rational agent updates her confidence by conditionalizing each probability function $c \in C$ on E: $C_E = \{c(-|E) : \text{for all } c \in C \text{ where } c(E) > 0\}$.
>
> **Supervaluationism:** Epistemic features of a doxastic state are represented by structural features in the representor. In other words: P is a property of an agent's doxastic state iff $\forall c \in C$, c has P. For example, the agent's confidence in p is greater than her confidence in q, written $C(p) > C(q)$ iff for all $c \in C, c(p) > c(q)$.

It's easiest to think of representers as a collection of committee members, corresponding

[9]For articulations of this view see [36], [56], [65], and [44], in addition to its original defense in [34]

to each $c \in C$. Each committee member forms a precise confidence on matters, and the agent is represented by what all members agree on. As [34, 288] says, it's "unanimity or ambiguity".

Some examples might help. Suppose that for every $x \in [0.6, 0.8]$ there is some $c \in C$ such that $c(p) = x$. We then write $C(p) = [0.6, 0.8]$. By supervaluationism, an agent with this representor determinately has a confidence greater than 0.5 in p. Similarly, if $C(q) = [0.1, 0.2]$ and $C(r) = [0.4, 0.5]$ then the agent is more confident in r than q.

Defenders of the imprecise view must equip the formalism with an evidential constraint governing which committee members to include in the representor. The one commonly endorsed is:

> **Evidence Grounding Thesis (EGT):** At any point in time, a rational agent's representor includes all and only all those precise probability functions that are compatible with the total evidence she possesses at that time.[10]

The idea is that if an agent did not satisfy EGT, then their representor would not be correctly representing the evidence—either it includes some committee member's opinion which is ruled out by the evidence, or it fails to include some committee member's opinion which is consistent with the evidence. In either case the representor would not correctly represent the evidence.

Suppose $C(p) = [0.2, 0.6]$ and $C(q) = [0.4, 0.8]$. Then $\neg(C(p) > C(q))$ and $\neg(C(q) > C(p))$, but $C(p) \neq C(q)$. So, on the imprecise model of confidence, EP does not entail PoI because confidence is no longer totally ordered. However, there is an indifference-like principle similar to PoI in this context:

> **Symmetry (SYM):** If $\{P_1, ..., P_n\}$ is a partition of events such that $P1 \approx ... \approx P_n$, then one ought to treat the P_i symmetrically in her credal state by including committee members who take all possible contrary views on their relative probabilities, so that for any c in C and any permutation σ of $\{1, 2, ..., n\}$ there is

[10] For a discussion of EGT, see [83]

always a c' in C with $c'(P_n) = c(P_\sigma(n_\sigma))$.

SYM is supposed to be a specific way that one's representor will satisfy EGT. For any committee member who takes a particular stance on, e.g. P_1 there must be another committee member who takes the same stance on some other P_i, for any P_i. In effect, SYM says that each P_i is balanced by the committee. This does not mean that each proposition is treated equally by every committee member, but that, across the committee, no P_i is favored. In the literature this principle has been tacitly ([35] and [33, 290][11]) or explicitly ([33, 171]) endorsed

EP does not logically entail SYM, but I take it that part of the justification of SYM will appeal to a principle like EP. Consider:

> **Evidential Principle***: If $\neg(p \succ q)$, then there cannot be a rogue committee member $c \in C$ who favors p over q.

By a 'rogue committee member' I mean one who is not balanced out by other committee members. Suppose that $P_1 \approx ... \approx P_n$, and one's representor violated Symmetry. Such a representor would violate EP*. So EP* entails Symmetry. Moreover, we can think of EGT as the constraint that one's representor must contain all the committee members who can consistently abide by EP.

However, I'll show that, while not out-right inconsistent, SYM has problems.

1.4.1 SYM and Extremal Credal States

There are quite a number of ways a representor might satisfy SYM. One way is for the representor to be 'maximally ignorant' in each of $\{P_1, ..., P_n\}$, in the sense that $C(P_1) = ... = C(P_n) = [0, 1]$. A credal state that takes on the interval values $[0, 1]$ or $(0, 1)$ in some

[11] There Joyce says that SYM is a plausible principle, and endorses one way to satisfy it in an example he considers—have C be the set of all credence functions. I talk about different ways one can satisfy SYM below. My claim is that, contrary to how weak the principle may seem, this way is the only way to satisfy it in certain situations.

proposition are called extremal representors. There are also finite, non-extremal representors as well that may satisfy SYM. For example:

$$c_1(P_1) = 1/6, c_1(P_2) = 2/6, c_1(P_3) = 3/6$$
$$c_2(P_1) = 3/6, c_2(P_2) = 1/6, c_2(P_3) = 2/6$$
$$c_3(P_1) = 2/6, c_3(P_2) = 3/6, c_3(P_3) = 1/6.$$

However, I will argue that in the cases of evidential symmetry we have been considering the only way to satisfy SYM is if the representor is extremal.

Let us look at the general structure of the representors that satisfy SYM. SYM entails that for any i and j, $min\{c(P_i) : c \in C\} = min\{c(P_j) : c \in C\}$ and $max\{c(P_i) : c \in C\} = max\{c(P_j) : c \in C\}$, since if m is the minimum credence given to any P_i by some committee member, then for any j, there must be some committee member c' such that $c'(P_j) = m$.[12] It must then be the case that m is a minimum of P_i for c' too, since if there were a $m' < m$ such that m' was a minimum for c', SYM would imply that m' is also a minimum for c, contra our assumption. In other words, the maximum and minimum values in the three columns in the previous example must be the same. For readability, let min and max be the minimum and maximum credence given to any of $\{P_1, ..., P_n\}$ by committee members in C.

Let us now think about the constraints on min and max. Surely it cannot be the case that $min + max = 1$. For example, consider $min = 0.3$ and $max = 0.7$. SYM requires that $c(P_1) = 0.7$, for some c. Then $c(\neg P_1) = c(P_2 \vee ... \vee P_n) = 0.3$. But the minimum of credence of c is 0.3, which must be given to one of $P_2, ..., P_n$. This is impossible, however, since the P_i's are mutually exclusive, and so $c(\neg P_1) = c(P_2 \vee ... \vee P_n) = c(P_2) + ... + c(P_n)$. In general, the constraint on min and max is, for $n > 2$ and $min \neq max$:[13]

(1) $max > 1/n$

[12]... if these minimums and maximums exist. They won't if, e.g., $C(P) = (0, 1)$. There's no loss of generality in what follows; in those cases similar equalities will hold for least upper bounds and greatest lower bounds. When I say below that, e.g. $min = 0$, I will mean that the greatest lower bound will be 0 if the minimum does not exist.

[13]See appendix for derivation of this.

(2) $(-n * max + max + 1) \leq min \leq (1 - max)/(n-1)$

Given the simple appearance of SYM, it's perhaps surprising that the conditions for satisfying it are so specific. (1) says that the minimum must at least be bigger than the inverse of the number of propositions in the partition under consideration. This is not too restrictive, though, as $1/n$ gets fairly small. Here is a more significant consequence of (1) and (2). Let $\{P'_1, .., P'_{n-1}\}$ be a coarse-graining of $\{P_1, ..., P_n\}$ in the sense that $\{P_1, ..., P_n\}$ and $\{P'_1, ..., P'_{n-1}\}$ are partitions over the same space of possibilities. Suppose that both partitions are evidentially symmetric. In such a situation, there can be a credal state C over $\{P'_1, ..., P'_{n-1}\}$ which satisfies (1) but which does not satisfy (1) over $\{P_1, ..., P_n\}$, if $C(P_i) = C(P'_i)$ for some i. In other words, symmetric credal states are not always guaranteed to satisfy (1) if the number of propositions one is considering decreases and $1/n < max < 1/(n-1)$. The same can be said about fine-graining the space of possibilities. Fix some C, min and max. If n goes up, the right side of (2) goes down, and hence shrinks the upper bound on min. So it's possible to have some C which satisfies (2) over n propositions but does not satisfy (2) over $n+1$ propositions.

In particular, consider Mystery Square again, with the following partitions:

$A1$: $0 <$ area < 1 square foot,
$A2$: $1 \leq$ area < 2 square feet,
$A3$: $2 \leq$ area < 3 square feet,
$A4$: $3 \leq$ area ≤ 4 square feet.

$L1$: $0 <$ width < 1 foot,
$L2$: $1 \leq$ width ≤ 2 feet.

Let us see what happens to a representor C which satisfies SYM. Let min_L and max_L be the minimum and maximum confidence committee members take in $\{L1, L2\}$. Similarly define min_A and max_A. Now, every c in C must be such that $c(A1) = c(L1)$. It then follows immediately from our discussion about the minimums and maximums above that $min_L = min_A$ and $max_L = max_A$. So we can then just talk of $min_{A,L}$ and $max_{A,L}$. If C satisfies SYM, then the following four inequalities must hold:

(1') $max_{A,L} > 1/4$

(2') $(-4 * max_{A,L} + max_{A,L} + 1) \leq min_{A,L} \leq (1 - max_{A,L})/(4 - 1)$

(1') $max_{A,L} > 1/2$

(2') $(-2 * max_{A,L} + max_{A,L} + 1) \leq min_{A,L} \leq (1 - max_{A,L})/(2 - 1)$

The only way to satisfy all four is if $min_{A,L} = 0$ and $max_{A,L} = 1$.[14,15]

This result does not yet entail that the representor satisfying SYM must be extremal. All it shows is that the minimum and maximum values the representor assigns to each of $A1$ - $A4$ and $L1$ and $L2$ must be 0 and 1 respectively. Finite representors may satisfy this condition, including the quite trivial representor where each committee member is only completely certain of one of the propositions or completely certain of its negation. The claim that SYM entails extremal representors follows when we adopt the following:

> **Convexity**: For every P and any $a < b$, if $c, c' \in C$ are such that $c(P) = a$ and $c'(P) = b$, then for any x such that $a \leq x \leq b$, there is some $c'' \in C$ such that $c''(P) = x$.

From SYM we know that there must be some $c, c' \in C$ such that, for example, $c(A1) = 0$ and $c'(A1) = 1$. Convexity then entails $C(A1) = [0, 1]$. Convexity is almost universally assumed

[14] To see this, note that $(1 - max_{A,L}) \leq min_{A,L} \leq (1 - max_{A,L})/3$ follows from (2') and (2"). Since each c is a probability function, $max_{A,L} > min_{A,L}$ and both must be between 0 and 1.

[15] This result depends on the size of the partitions. For example, let X be a parametrization of a square such that:

$X1$: $0 < X < 1$ unit,
$X2$: $1 \leq X < 2$ units,
$X3$: $2 \leq X < 3$ units,

and $X1 = A1$. A little bit of algebra will reveal that the possible values of $min_{A,X} and max_{A,X}$ are

$min_{A,X} \in [0, 1/5]$
$max_{A,X} \in [(1 - min_{A,X})/2, 1 - 3 * min_{A,X}]$.

For the purposes of the arguments in this paper, all I need is one situation of evidential symmetry where SYM requires an extremal representor. It turns out that the simple case of area and width suffices.

in discussions of imprecise bayesianism. In effect, it forces representors to have only interval values. The idea is that if Convexity is false, then a representor could be 'spotty', and it is hard to make epistemological sense of spotty representors. I will proceed under the assumption that it is true, or perhaps more accurately, built into the model.

1.4.2 Rationality of Extremal Credal States

We saw in the last section that EP motivates SYM, an indifference-like principle, and utilizing the same case we used to show that PoI was inconsistent, we can show that there are some evidential situations where the only way to satisfy SYM is to adopt extremal credences. I now want to show that there are reasons to think that extremal credences are irrational, and moreover adopting extremal credences in certain evidential situations is in tension with EP.

Problem 1: Maximal states of confidence are 'sticky'. Suppose you have a coin of unknown bias, and let Hi be the proposition that the ith flip will be heads. Further, suppose you have an extremal credal state, where committee members have all possible estimations of the bias $\beta \in (0, 1)$.[16] A state would be such that $C(H_1) = (0, 1)$. You are about to flip the coin 25 times. Because you are uncertain of the bias, $C(H_{25}) = (0, 1)$. Now, suppose you observe H_1, H_2 and H_3. How does this affect your confidence in H_{25}? If you update by Conditionalization, your confidence in H_{25} will not budge, i.e. $C(H_{25}) = (0, 1)$. Why? Suppose some committee member whose confidence in H_{25} is 0.0000001 learns H_1 & H_2 & H_3. Their confidence might go up to, say, 0.0000002. But there is some other committee member whose confidence started out even lower, say 0.00000009, and their confidence will rise to 0.0000001 upon learning H_1 & H_2 & H_3. So for every $x \in (0, 1)$ and for some $c \in C$ such that $c(H_{25}|H_1\&H_2\&H_3) = x$, there will be some $c' \in C$ such that $c'(H_{25}|H_1\&H_2\&H_3) = c(H_{25})$. If one committee member gets more confident in H_{25}, a more skeptical colleague will take her place. This problem of course is fully generalizable, and one can prove that $C(H_{next}|O) = (0, 1)$ where O is any set of observations about the coin.

I am of course not the first to point out this problem, and the phenomenon plays a cen-

[16] In other words, for each $x \in (0, 1)$ there is a committee member with credal state $c(\beta = x) = 1$ and $c(H_1) = c(H_1|\beta = x) = x$.

tral role in debates about the imprecise model.[17,18] It is unclear whether the problem can be resolved. One might think that the problem here is Conditionalization. Perhaps some other updating rule allows for inductive learning. However, things are not that straightforward. [14] provide a careful exploration of possible rules to replace Conditionalization. In the end they find no ad-hoc way of circumventing the problem. There's a sense in which the problem is not located in Conditionalization, but rather that there are just 'too many' probability functions in C. If one takes the problem of learning sketched here seriously, then one will want to forbid principles that require agents to adopt these sticky states.

Problem 2: More interestingly, there is a tension between extremal confidence and EP. Consider a third urn case:

> **The Third Urn**: In the third urn there are balls of two colors, black and white. You know that there are 100 balls in the urn, and that 99 of them are white. You are also told that the balls will have numbers printed on them. You are not told what kind of numbers will appear, i.e. integers, rationals, transcendentals, etc. You also have no idea whether they will all have the same number or whether they will be different. They draw a ball randomly from the urn.

Consider your confidence that the ball will be white. Clearly it should be 99%. What about

[17] [34], the *locus classicus* for the imprecise model, takes this phenomenon to be a feature not a bug:

> "if you really know nothing about the [...] coin's bias, then you also really know nothing about how your opinions about [e.g. H_{25}] should change in light of frequency data. [...] You cannot learn anything in cases of pronounced ignorance simply because a prerequisite for learning is to have prior views about how potential data should alter your beliefs, but you have no determinate views on these matters at all."

On the one hand, you clearly do know how your opinions ought to change in light of frequency data for the coin of unknown bias. You know that the bias of a coin can be approximated via the frequency of heads in independent, standardized trials. This is simply a fact about coins and their behavior, normally understood. On the other hand, if Joyce is not just simply wrong he must be imagining the scenario in some other way, where perhaps the coin is so foreign to you that it's unclear whether frequency data can approximate the bias. But then why call them coins? And how can we even apply the concept of bias? Perhaps the coin is an alien artifact governed by unknown laws. In this case it is clear that you cannot learn anything from frequency data and indeed this is a feature of the view, not a bug. But a simple coin of unknown bias is a less extreme case.

[18] For more on belief inertia, see [89], [45], [38], [71] and [83]

your confidence that the ball will have a 0 printed on it? It follows from the discussion in section 3.1 that one's confidence ought to be $(0, 1)$. Let B_0 be the proposition that the ball has a 0 printed on it, and W the proposition that the ball is white. The following is an inconsistent triad:

(1) $C(W) = 0.99$

(2) $C(B_0) = (0, 1)$

(3) You are more confident in W than B_0

They are inconsistent because of Supervaluationism. From (3) and Supervaluationism, every $c \in C$ must be such that $c(W) > c(B_0)$. But from (2) there is some $c' \in C$ such that $c'(B_0) > 0.99$, and from (1) all $c \in C$ are such that $c(W) = 0.99$, and so in particular $c'(B_0) > c'(W)$.

There are two ways to respond to the inconsistency, while holding on to EP:

(i) $W \succ B_0$, in which case, by EP, (3) is true, and hence we must give up (2).

(ii) $\neg(W \succ B_0)$, in which case, by EP, (3) is false.

If we take route (i), then we have a reason to reject SYM, since this third urn case is a case of evidential symmetry where SYM requires an extremal credal state. But then we are in a position of some dissonance. I've claimed that EP was the kind of motivation we had for adopting SYM in the first place. But then if we hold on to EP in this case we must reject SYM. This is similar to the situation that arose in section 2.2 with EP and PoI. In the context of precise confidence, EP entails PoI, but PoI is inconsistent. Here EP does not strictly entail SYM, but route (i) reveals that we cannot consistently hold on to both.

Why not take route (ii)? One might argue that you ought to be neither more confident in W than B_0 nor more confident in B_0 than W. Suppose you have an empirical theory T which you have tested extensively and you're extremely confident that T is true, and there's another theory $T*$ about some other empirical domain that you have not yet tested, and you have no idea whether it is true. Would you be more confident in T over $T*$? One reasonable response could be: 'well no, since I've yet to test $T*$!'

I do feel the pull of this line of thought. However, I do not think it is ultimately defensible for the third urn. The case is engineered so that one's confidence in W can be arbitrarily high. Suppose that there are 10^{100} balls in the urn and only one is white. In such a case, would it really be rational to be no more confident in W than in B_0? Throughout this paper I have been neutral about the character of the evidential support relation, but a proponent of the imprecise model taking route (ii) we must deny that, in cases like the third urn, having massive evidence for p but no evidence in favor or against q is consistent with $p \approx q$. This strikes me as quite implausible. A *mere lack* of evidence could, on this line of thought, defeat arbitrarily high evidential support–I don't see a way of making sense of 'having massive evidence' if such evidence was not strong enough to support p over q.

In addition, I think a defender of imprecise credences ought to find reasons for not taking route (ii) based on the way imprecise credences hook up with decision theory. [35] gives a sustained defense of what he calls "Modest Probabilism", which is a very close sibling to the imprecise model I sketch above.[19] He derives his version of imprecise probabilism from a few very plausible decision theoretic principles along with a bridge principle linking betting preferences with doxastic states:

> **Confidence**: You are more confident in p than q iff you prefer a bet that pays \$1 if p and \$0 if $\neg p$ over a bet that pays \$1 if q and \$0 if $\neg q$.[20]

This principle plays a crucial role in Kaplan's decision theoretic foundation. Personally I would have no reservations taking a bet on W over B_0 when there are $10^{100}-1$ white balls in the urn. One who denies this must say that there is no number of white balls, however large, such that they would prefer a bet on W over B_0. This just strikes me as wildly implausible.

For these reasons I think that taking route (ii) is implausible. A defender of the imprecise model must then take route (i) and thereby reject SYM.

[19] One of the differences in particular is that for Kaplan, S is more confident in p than q iff for every $c \in C$, $c(p) \geq c(q)$ and there's some c such that $c(p) > c(q)$. This doesn't affect what follows.

[20] [35, 8]

1.5 Taking Stock

Let us take stock. Here is the principle that I have been focusing on throughout this paper:

Evidential Principle: One ought to be more confident in p than q iff $p \succ q$.

In section 2 I showed that EP entails PoI under the assumption that confidence is modeled by a precise probability function. But, as I argue, PoI is false. In section 3 I showed that in the imprecise model, EP motivates SYM, an indifference-like principle. But, as I argue, there are reasons for rejecting SYM, one of them stemming from EP itself.

The moral I would like to draw from these observations is that it is surprisingly difficult to make EP work with the two most popular models of belief. I attribute this to the fact that the precise and imprecise models do not have a plausible way of understanding the negation of the antecedent of EP. Cases of evidential symmetry are situations where one ought not to be more confident in p than q and vice versa, and in such situations I have made trouble for these models.

Why not just reject EP? I imagine that on an extreme permissivist view, EP could be false. Such a view is one where there sometimes exists evidential situations where it is rational to be more confident in p than q, and it is rational to be more confident in q than p, where p and q are contraries. Perhaps the Mystery Square case is such an evidential situation. On this view PoI is expected to be false...there is no rational constraint on the formation of one's confidences besides, e.g. probabilistic coherence. Putting aside extreme permissivism, however, I think EP is a principle that most would want to accept, or at least would need to give some explanation for why it is false.

I do not want to suggest that one cannot supplement or amend the precise or imprecise models to circumvent the trouble I have made for them in this paper. All I want to argue is that if one accepts EP one must supply some such amendment. I will, though, suggest a very simple fix. I've been discussing models which treat ignorance *quantitatively.* In this paper I've exploited some of the qualitative properties of ignorance states to make trouble for the models. A *qualitative* model of ignorance will will not be subject to the same trouble. Consider again the case of the coin of unknown bias. Let $I(p)$ mean that one is totally ignorant about p. One's doxastic state can then just be described as $I(\beta = x)$ for any $x \in (0, 1)$. This ignorance state will have some logical properties, e.g. $I(p) \wedge I(q)$ entails $I(p \wedge q)$, and a minimal theory can be given.[21] If we really have no idea how flips of the coin bear on the bias (see fn 17), then we can model this with $I(\beta = x|T) = I(\beta = x)$ for any trial T. This is functionally equivalent to the extremal state discussed in section 3.1. But if our ignorance is not sticky, we can build ignorance bridging principles that take the form $I(\beta = x|T) = c(\beta = x)$. In the case of the coin of unknown bias, these ignorance bridging principles could be standard statistical treatments of estimating bias, e.g. maximum likelihood estimations, conditional expectation estimates, etc. A simple non-quantitive ignorance state suited with bridge principles could fit the bill for a plausible way of understanding the negation of the antecedent of EP.

I conclude with a diagnosis of White's fallacious use of PoI:

> "In a state of ignorance concerning a class of exclusive possibilities, where we can see no reason to expect one to be true rather than another, we should put about equal credence in each. When we consider the vast array of ways that stars can be arranged, it would be unreasonably arbitrary to put a lot of confidence in some possible arrangements over others." [92]

White is correct that it would be unreasonably arbitrary to put a lot of confidence in some possible arrangements of the stars over others. You have no reason to think one arrangement is more likely than another. But it does not follow that we should put 'about equal credence in each'. This is an example of a problematic use of EP that I hope I have warned against. The only thing that follows is that we ought to treat each possibility equally.

[21]See e.g. [51]

1.6 Proofs

Let $\{E_1, E_2, ..., E_n\}$ be a set of n propositions which form a partition, and let $< min, max >$ be the minimum and maximum credence any c gives to any E_i. What general conditions must be in place on $< min, max >$ and n? Consider $c(E_1) = max$. It follows that $c(\neg E_i) = c(E_2 \vee ... \vee E_n) = 1 - max$. The lowest each $c(E_i)$ could be is min, and so $c(E_2 \vee ... \vee E_n)$ will overshoot $1 - max$ unless $min * (n-1) \leq 1 - max$—if $min * (n-1) > 1 - max$, then there could be no way for each of the $c(E_2)$ through $c(E_n)$ to add up to $1 - max$, since it follows from SYM that min is the minimum credence for any E_i. Likewise, $c(E_2 \vee ... \vee E_n)$ will undershoot $1 - max$ unless $max * (n-1) \geq 1 - max$, since the highest each $c(E_i)$ could be is max. Putting these two conditions together we have:

(a) $min * (n-1) \leq 1 - max \leq max * (n-1)$.

Now, consider $c(E_1) = min$. Again, $c(\neg E_1) = c(E_2 \neg ... \neg E_n) = 1 - min$. Following the same line of reasoning as before, the lowest each $c(E_i)$ could be is max, and so $c(E_2 \vee ... \vee E_n)$ will overshoot $1 - min$ unless $min * (n-1) \leq 1 - min$. Likewise, it will undershoot $1 - min$ unless $max * (n-1) \geq 1 - min$. Hence we have the condition:

(b) $min * (n-1) \leq 1 - min \leq max * (n-1)$.

I've only considered cases where $c(E_i) = min$ or $c(E_i) = min$, but the result holds generally. The conditions each c must satisfy to not overshoot or undershoot when $c(E_i) = min$ or $c(E_i) = min$ are conditions on the maximum and minimum values c could give to E_i, as long as c satisfies these it will not over or undershoot if it assigns E_i some value between min and max.

Now, (a) and (b) are a system of four inequalities with three unknowns. Assuming that $min \neq max$ and that $n > 2$, a bit of algebra will reveal that (a) and (b) are equivalent to:

(c) $min > 1/n$ (b) $(-n * max + max + 1) \leq min \leq (1 - max)/(n-1)$.

2.0 Is Traditional Induction a Myth?

2.1 Introduction

Hume's problem of induction is one of the most long lasting and important problems in philosophy. There have been many attempts at a solution, but none have seemed to succeed. It has preoccupied philosophers since Hume's time, despite the fact that our understanding of our actual inductive methods has improved significantly. One of the reasons is that Hume's critique is not tied to the specific content of the inductive principles that we in fact follow. Hume's critique, rather, only depends on our following any principles at all. This is why the problem appears very deep. However, there have been recent attempts at undermining Hume's problem by denying that we in fact do use inductive principles. In this paper, I will defend this view and bring out what's so interesting about it in light of Hume's problem, and argue that recent objections to the view do not succeed.

2.2 Inductive Inference

Understanding inductive inference is among the central goals in the philosophy of science and epistemology. Broadly speaking, there are two main projects contributing to that understanding, one descriptive and one justificatory. The descriptive project sets out to accurately describe and understand the structure of the paradigmatic inductive inferences made in science and everyday reasoning. Questions in the descriptive project might look something like: How does Newton's deduction from the phenomenon work? How do agents form beliefs on the basis of testimony? This is often not a minor task, especially when reconstructing inferences in the mature sciences. The justificatory project, on the other hand, sets out to determine what kinds of inductive inferences are good and why. There are local and global flavors of this project. We can defend or critique the legitimacy of specific inference patterns or principles, e.g. favoring prediction over accommodation, inferring unobservables on the basis of predictive success, using thought experiments in empirical enquiry, etc. But there are also the long standing skeptical challenges that purport to undermine the legitimacy of inductive inference in general.

These two projects are not always pursued together, and for good reasons. In order to make sense of Darwin's ingenious inferences in the *Origin of Species*, one does not need to first confront inductive skepticism. The descriptive project does not depend on the justificatory project. But does the justificatory project depend on the descriptive? Clearly the success of the local flavor of the justificatory project depends on accurately identifying the principles to be justified—undermining the legitimacy of thought experiments in empirical inquiry does nothing if such reasoning is not used by scientists. But the global flavor seems like it does not depend on the descriptive project. This looks to follow just from the 'global' adjective. Skeptical challenges latch on to logical features in our inductive reasoning. The force of the challenge, and its unending source of vexation, depends on our following any general rules or principles at all.

In this paper I want to examine this source of vexation. In particular, I'll focus on Hume's challenge, mostly because it is the perhaps most well worn form of inductive skepticism (or at least has the richest history). The justificatory project, I think, *does* depend on the descriptive—if it turns out that our inductive practices do not use any general rules or principles at all, then, insofar as the source of the vexation derives from general rules and principles, we ought not be vexed. The idea that we use rules in inductive reasoning has recently come into question. According to [54], Hume's challenge does crucially depend on the assumption that our inductive practices are governed by some set of rules. A subjective Bayesian view of rationality, he thinks, is a kind of picture of ampliative reasoning without rules. [52, 49] argues that all induction is local and that there is no need to appeal to domain inspecific rules. Both argue that Hume's challenge is dissolved once we realize that our ampliative inferences are not rule-governed. [37], [97], [55], [73] and others argue with varying details that Norton trades one problem for another, and hence no dissolution is effected.

I want to investigate no-rules theories and determine whether or not they are in a better position than traditional theories with respect to Hume's problem of induction. I don't think the traditionalists have completely understood the position of the no-rules theorist. I want to rectify this by laying out the underlying motivations for the view. To do this, I need to clearly outline the problem of induction and how it gets a grip on rule-based theories. I'll start with a gloss on Hume's original formulation, and show how it can be transformed into an argument that affects any rule-based theory of inductive inference. In particular, it's important to see that the kind circularity charged in Hume's problem is *rule-based circularity*. I will then say a bit about what a no-rules inductive theory might look like, and present two arguments in favor. Though I don't think any traditionalist has explicitly addressed these arguments in print, I'll present what I think the traditionalists have to say in response to these arguments. Lastly, I'll address the charge that an analogous regress to Hume's problem arises in the no-rules theory. I argue that the shape of the new regress has a much different and much less problematic character than the old. Ultimately, I argue that no-rules theories are in a better position with respect to Hume's problem than traditional theories. Part of the problem with rule-based theories is that they presume the intelligibility of a theoretically barren context in which our inductive rules apply. There is no such context, and the no-rules theory of induction allows us to make sense of our inductive practice without it.

2.3 Hume's Argument

I won't attempt any substantial Hume exegesis here, and I will unabashedly translate Hume's terms into more contemporary language, putting aside the question whether this is faithful to Hume. The goal will be to see how Hume's argument can be generalized to any rule-based theory of induction.

Hume begins his discussion of induction (what he calls probabilistic arguments) with a description of how we arrive at opinions about the unobserved: "In reality, all arguments from experience are founded on the similarity, which we discover among objects, and by which we are induced to expect effects similar to those, which we have found to follow from such objects." ([32, 23]) Or, put much more simply: we observe past regularities and project them into the future. Hume then says that our inductions 'proceed upon the assumption' that the future will resemble the past. What he means by this is that if we have no reason to think that nature is uniform, then we have no reason to believe that the regularities we observe will project into the future:

> "For all inferences from experience suppose, as their foundation, that the future will resemble the past, and that similar powers will be conjoined with similar sensible qualities. If there be any suspicion that the course of nature may change, and that the past may be no rule for the future, all experience becomes useless, and can give rise to no inference or conclusion. It is impossible, therefore, that any arguments from experience can prove this resemblance of the past to the future, since all these arguments are founded on the supposition of that resemblance." ([32, 24])

In short, Hume makes three basic moves. First, he isolates what he thinks is the principle or, what I'll call rule, that underlies any inductive inference (project the past onto the future). Second, he claims that any justified use of this rule requires justification that the rule is truth-conducive (nature is uniform). Lastly, he argues that we can only be justified in thinking that the rule is truth-conducive on inductive grounds, grounds which themselves require justification that the rule is truth-conducive.

Here is the streamlined argument:

(1) Inductive arguments proceed from the supposition that nature is uniform.

(2) If we have no reason to believe that nature is uniform, then we have no reason to believe the conclusion of inductive arguments.

(3) There are only two kinds of arguments, deductive and inductive.

(4) We have no reason to believe that nature is uniform based on a deductive argument.

(5) Any inductive argument that nature is uniform proceeds from the supposition that nature is uniform. [1]

(6) An argument for X that supposes X can give no reason to believe X.

(7) Therefore, we have no reason to believe that nature is uniform based on an inductive argument. [5, 6]

(8) Therefore, we have no reason to believe that nature is uniform. [3, 4, 7]

(9) Therefore, we have no reason to believe the conclusion of inductive arguments. [2, 8]

This argument is valid, but the problem is with P1. I won't rehearse the many discussions of the uniformity principle here, but the main idea is that we clearly do not project all past regularities into the future, and because of this it's not very clear what the content of the principle is.[1] For example, the only recorded major earthquakes in California with a magnitude greater than 7.3 have all happened in the winter season. Should we infer that all future major earthquakes will similarly be in the winter? Clearly this depends on geological facts about fault instability, and not merely on the observed regularities. It turns out this is a coincidence, and there is no reason to think fault instability changes with the season. Hume's uniformity principle is then at best a promissory note for an adequate categorization of the projectable regularities. However, [29] is taken to have shown that such a categorization is impossible without supplement from other inductive rules or empirical generalizations.

I do not want to cast judgment on Hume, and the uniformity of nature principle plays an important and interesting role in his theoretical philosophy. But it's clear that Hume had an incomplete understanding of our inductive practices. Now, if Hume got our inductive practices wrong, does his argument still pose a challenge? The received view is that it does. For example, here's [70]:

> "Hume's arguments are not peculiar to induction by enumeration or any other special kind of inductive inference; they apply with equal force to any inference whose conclusion might be false when it has true premises."

By a rule of inductive inference, I will mean a proposition of this kind: "In circumstances C, do X." The action X is supposed to be epistemic, like accepting, believing, suspending

[1] See [69]

judgment, etc. For example, a principle of simplicity would look something like: "If theory X is simpler than theory Y, and X and Y are otherwise equally theoretically virtuous, accept X." I will specifically think of rules in terms of inference schemas. For example, the argument

> X is simpler than Y
> X and Y are otherwise equally theoretically virtuous
> Therefore, X

is licensed by the simplicity rule.[2] I take it that the propositional understanding of rules is roughly equivalent to the schematic understanding due to premise/rule duality.[3] When an argument is supposed to be licensed by some rule r, I'll call the argument r-valid.

An important feature of inductive rules is that they are contingent. What I mean by this is that for any inductive rule we may employ, there are some possible worlds where the rule is appropriate and generally leads to true beliefs, and some possible worlds where the rule is not appropriate and generally leads to false beliefs. The former I will call the r-friendly worlds, for some rule r. In the r-friendly worlds, reasoning inductively via r will lead one to epistemically good beliefs, however one wants to understand the epistemic good—e.g. in terms of reliability, accuracy, and so on. In the r-unfriendly worlds, one will be led astray by reasoning inductively via r. Contrast this with deductive rules of inference. When one reasons deductively via modus ponens, if your premises are true then you will necessarily arrive at a true belief. In other words, there are no unfriendly worlds for deductive rules, since the reliability of deductive rules does not depend on the character of the world. Note that an r-*un*friendly world may share an identical past history with an r-friendly world. There's nothing in principle stopping a world from favoring simple hypotheses up until some time t but not afterwards.

With this picture of inductive rules in mind, we can generalize Hume's argument. Let R be the set of inductive rules we wish to justify. I'll say that a world is R-friendly just in case every rule in R is r-friendly. Hume's argument is then as follows:

[2] In other words, it is valid with respect to the set of inductive rules under examination, which includes the simplicity rule.

[3] From now on I will ignore the action aspect of the rule.

(1) Inductive arguments generally are truth conducive only if we live in an R-friendly world.

(2) If we have no reason to believe that we live in an R-friendly world, then we have no reason to believe inductive arguments are truth conducive.

(3) There are only two kinds of arguments, deductive and inductive.

(4) We have no reason to believe that we live in an R-friendly world based on a deductive argument.

(5) Any inductive argument that we live in an R-friendly world is truth conducive only if we live in an R-friendly world. [1]

(6) An argument for X that relies on prior justification for X can give no reason to believe X.

(7) Therefore, we have no reason to believe that we live in an R-friendly world based on an inductive argument. [5, 6]

(8) Therefore, we have no reason to believe that we live in an R-friendly world. [3, 4, 7]

(9) Therefore, we have no reason to believe that inductive arguments are truth conducive. [2, 8]

(10) Therefore, we have no reason to believe the conclusions of inductive arguments. [9]

This argument is just a generalization of Hume's three-step process I mentioned above. First, we identify a set of rules R that underlies any inductive inference. Second, we claim that any justified use of these rules requires justification that the rules are truth-conducive, i.e. that we live in an R-friendly world. Lastly, we argue that, because being in an R-friendly world is a contingent matter, we can only be justified in thinking we live in an R-friendly world via an inductive argument, an argument that itself will require justification that we live in an R-friendly world.

The last move which is encapsulated in premise 6 is the most interesting, which I now want to highlight, as discussions of inductive skepticism tend to overlook its importance.

What's at issue is a kind of *rule circularity*, which is a distinct kind of circular reasoning. One kind of circular reasoning we are all familiar with is the following:

> A
>
> Therefore, A

The premise entails the conclusion, so the argument is valid. But you only have reason to believe the conclusion on the basis of the argument if you have reason to believe A, and so one cannot obtain a reason to believe A on the basis of the argument. This is *premise circularity.* In the case of rule circularity, the conclusion does not explicitly contain a premise. For an illustration, suppose r is a simple kind of enumerative induction:

> Most observed Fs so far are Gs
>
> Therefore, Most Fs are Gs.

Following Hume's argument, we must establish that the world is r-friendly via some inductive argument. Consider this argument:

> Most inferences following rule r have been truth-conducive
>
> Therefore, most inferences following rule r are truth-conducive.
>
> (Therefore, the world is r-friendly)

This argument is r-valid. But using this argument to establish that the world is r-friendly is circular, since the appropriateness of the argument depends on whether r is truth conducive, which depends on whether we live in an r-friendly world. In other words, if this argument encapsulates our only inductive grounds for believing that the world is r-friendly, then we cannot obtain a reason to believe that the world is r-friendly based on this argument, since we must first have some reason to use r.

Because of premise/rule duality, we can always add r as a premise:

Most inferences following rule r have been truth-conducive

If most observed Fs so far are Gs, then most Fs are Gs

Therefore, most inferences following rule r are truth-conducive.

This argument is then deductively valid. It is still circular in the sense that in order to obtain a reason to believe the conclusion on the basis of the premises, one must first have a reason to believe the conclusion.

It might be thought that rule circularity is less problematic than premise circularity, since rules can be self-supporting. Perhaps we can tentatively take on a rule r, and then build up confidence that we live in an r-friendly world via arguments like the one above. The trouble with this line of thought is that even counter-inductive arguments are self-supporting. Let $r*$ be some rule like

Most observed Fs so far are Gs

Therefore, it's not the case that Most Fs are Gs.

We can use this rule in a similar way to conclude that the world is $r*$-friendly:

Most inferences following rule $r*$ have not been truth-conducive

Therefore, it's not the case that most inferences following rule $r*$ have not been truth-conducive.

(Therefore, the world is $r*$-friendly)

It's hard to see what is in principle different between the self-supporting nature of r and $r*$.

In sum, inductive inferences are fallible because there is a logical gap between premise and conclusion. The traditional picture bridges this gap with inductive rules. Hume's challenge purports to demonstrate that the gap is unsurmountable.

2.4 No Rules

I now want to turn to a way of addressing Hume's challenge that denies that we use inductive rules at all. The basic idea is that, even if Hume's challenge does not depend on our following any specific inductive rules, it does depend on our employment of some inductive rules. If we are able to safely ditch the assumption that we reason in accordance with general rules, then the skeptical challenge I have just sketched loses its force. The challenge hinged on the problem that inductive rules must justify themselves and they can only do this on pain of vicious circularity. But if our inductive practice does not use any general rules, then there is no longer any need to justify that the world is R-friendly, and so nothing for Hume's challenge to get a grip on.

I realize this proposal may sound odd for those encountering it for the first time. *Surely we must use some kind of general inductive rules, otherwise our inductive reasoning would be lawless, irrational, unscientific, etc.* I will partially address this thought and other objections later in the paper. The task of this section will not be to convince you that this view is correct, but to at least get on the table what exactly the view is. I think the no-rules theories are often misunderstood, partly because the different interests and questions between authors.[4] Here I will lay out the underlying motivation for no-rules theories.

It will be useful to separate the no-rules theories into two groups. On the one hand there are the subjective bayesians (e.g. Okasha, van Fraassen). They will think that empirical inquiry proceeds along the lines of the bayesian calculus, and that there exists no "viable inductive method, inductive logic, or set canons of induction." (van Fraassen 1989, pg 279) Each agent has an allocation of subjective probabilities (or credence) to a set of considered propositions, which are rationally constrained to satisfy the probability axioms. To learn from experience is to conditionalize one's prior credence on what one has learned. This is a no-rules kind of view because, one, there is no substantial constraint on the space of possible prior credences—any probabilistic credence function is permissible, and so no

[4]For example, [73] and [37] criticize Norton for equivocating between two kinds of theories, though he really is not. Norton is primarily interested in inductive logic and only secondarily interested in epistemology, while Skeels and Kelly are primarily interested in epistemology. This leads them to read too much epistemological theory in Norton.

general rules guide the formation of the priors. In contrast, a more objectivist bayesian might claim that there are substantial constraints on the rational priors, and such constraints have inductive content.[5] For example, a rational prior may give more probabilistic weight to simpler hypotheses.[6] And two, conditionalization is not an inductive rule. Why not? Recall that rules, as I have been understanding them, must be contingent in the sense that there are some worlds where the rule is truth conducive and some worlds where the rule is not. In other words, the epistemic status of the output of a rule must depend on the epistemic status of the input, plus how well the world fits the rule. But the epistemic status of one's credences at the output of conditionalization *only* depends on the epistemic status of the input, i.e. one's priors. There are no conditionalization-unfriendly worlds, since conditionalization adds nothing to the content of the priors.

I will call the *non-bayesian* no-rules theorists material theorists (e.g. Sober, Norton). Following [72], we will understand a material inference as an inference that is licensed not in virtue of its form. Rather, the license depends on the material or content of the inference. Material theorists will think that inductive inferences are licensed by 'material postulates' or 'background facts'. I find the phrase 'background fact' quite cumbersome, especially when talking about counterfactual inductive inferences, and so what licenses an inductive inference I will just call 'the background'. In what follows I will talk about no-rules theorists in general, including both bayesian and material theorists, but my explanations will focus on the material theories.

Let us now try to understand what it means for a background to license an inductive inference. To start, consider the following. Suppose a chemistry student is undertaking a laboratory assignment. She's tasked to measure the melting point of a number of substances, one of them being a sample of wax. She makes the following inference:

This sample of wax melts at 90C

Therefore, all samples of wax melt at 90C.

[5]For example, Williamson claims that evidence comes in probabilities, and that the ur-prior for these probabilities "measures something like the intrinsic plausibility of hypotheses prior to investigation." ([94, 211]) I count these kinds of views as traditionalist.

[6]Though bayesians don't normally talk much of 'inductive inference', I take it that this is the way they would adopt the simplicity rule mentioned above.

If she wrote this down on her lab report she would not receive full marks. Why not? One might think that what explains the impropriety of the inference is that it has the invalid form:

> This F is a G
> Therefore, all Fs are Gs.

Perhaps the chemistry TA grading the lab report will write, in bold red ink, "Logical Fallacy!". But the more precise explanation is that wax is not a uniform substance—standard candle wax (usually refined from petroleum nowadays) melts at a different temperature than beeswax or beef tallow. The melting temperature depends on the structure of the hydrocarbon bonds, which is different in each kind of wax.

In the next question, the student is asked to measure the melting point of a sample of bismuth. She writes down the following inference:

> This sample of bismuth melts at 271C
> Therefore, all samples of bismuth melt at 271C.

The TA fortunately catches themself before they write "Logical Fallacy!" again. While the inference has the same form, she deserves full marks. All samples of pure bismuth will melt at the same temperature, since the melting point is generally a constant property of elemental substances. I say generally because of course some elements may combine with themselves to form structures which differ in melting point, e.g. compare pure carbon substances like diamond and graphite. But the TA knows that the student knows the fact that bismuth has no stable allotropes.

Here is the general lesson I want to draw. The traditionalist will think that the bismuth argument is an inductive enthymeme. There is some additional information that must be added to the inference which will make it an instance of a valid inductive schema. The non-enthymatic argument may turn out to be complex and non-obvious, which shouldn't surprise us since it's the output of a philosophical analysis. To take a simplified example, the traditionalist might reconstruct the bismuth argument along the following lines:

> This sample of bismuth melts at 271C
>
> The hypothesis that this sample of bismuth has no allotropes is simpler than the hypothesis that this sample does have allotropes
>
> Therefore, this sample of bismuth has no allotropes (simplicity)
>
> ...
>
> All samples of non-allotropic substances have the same melting point (simplicity)
>
> ...
>
> Therefore, all samples of bismuth melt at 271C (simplicity)

The appropriateness of the inference is grounded in the simplicity rule—in order to understand why 'all samples of bismuth melt at 271C' one needs to understand the simplicity rule.

A no-rules theorist, on the other hand, thinks that all there is to understand the appropriateness of the inference is just for one to understand the background that bismuth has no allotropes and that non-allotropic substances all have uniform melting points. The arguments are not, upon analysis, enthymemes. To illustrate this further, consider the two inferences

> The sun has risen every day so far
>
> Therefore, the sun will rise tomorrow,

> Bread has nourished every day so far
>
> Therefore, bread will nourish tomorrow.

Suppose that the traditionalist says that these are both good inductive arguments, and for the same reason, i.e. they have the same form, so they have the same inductive license.[7] The no-rules theorist holds that there is no inductive license shared between the two inferences. For the first, we know the orbits of the planets around the sun are stable, and will be stable for a very long time. This does not mean that the inference is deductive, however. There are possibilities consistent with our best theories of physics where the sun does not rise tomorrow. For the second, we know that there is a physiological reason why carbohydrates nourish the body and that metabolic processes do not change day-by-day. Again, for the no-rules theorist, all there is to understand the appropriateness of these inferences is to understand these background facts. There is no need to appeal to any inductive rules. Once one understands how these inferences work, one understands induction.

2.5 No Rules?

I now want to sketch what I think are the two strongest arguments for the no-rules picture. The first argument is an argument from disagreement: there are no recognized universal rules of induction in the literature.[8] Disagreement may be a distinctive feature of a philosophical debate, but the literature has yet to agree on any, even very basic rules that could properly be grist for Hume's mill.[9]

There is a point in [94] which I think illustrates the general feeling of things in this debate. For Williamson, evidence comes in probabilities. If one's total evidence is e, one's updated beliefs are a function of the conditional evidential probability distribution $P(-|e)$. This evidential probability distribution, what's called the ur-prior, "measures something like the intrinsic plausibility of hypotheses prior to investigation." (211) For example, if h is simpler hypothesis than h', then $P(h) > P(h')$. I consider Williamson as a traditionalist. Rules for inductive inference, as I have been calling them, will correspond to constraints on

[7] Put aside the fact that no traditionalist would be committed to the naive rule 'If X has occurred every day so far, then infer X will happen tomorrow'.

[8] This is a point Norton makes in many places, and is what in part motivates his account; see [52] specifically.

[9] For an overview of the induction literature, see [50]. Also see [64] for an early discussion of inductive rules.

the rational evidential probability distributions. Williamson says nothing substantive about his ur-prior. He defends this by arguing that the concept of intrinsic plausibility is vague, and this vagueness ought to be reflected in our description of the ur-prior. Any precisification, Williamson thinks, would betray this vagueness.

I think one ought to be very puzzled by this. Williamson can't be saying that P is vague. There are many ways of making sense of vague probabilities by adjusting the formal definitions in some way—e.g. in sets of precise probability functions, ranking functions, Dempster-Schafer functions, etc. It's important to Williamson that P is a precise probability function. He's saying, rather, that our *understanding* of what P could be is vague, and yet P must be *so rich* as to generate a precise probability function over any proposition one could consider. How could something vague be so incredibly precise and also play such a foundational role in our epistemic theories? I don't think this is a quirk of Williamson either. I think it's built into a lot of epistemological theorizing. The disagreement about what the rational inductive rules are is kind of feature rather than a bug. But the vagueness in our concept of intrinsic plausibility should make us doubt both *how* to determine the ur-prior (which Williamson is happy to accept), but also *that* it even intelligibly pins down a single probability function.

Brute appeal to puzzlement aside, the main argument I am driving at is this. If we take seriously the understanding of inductive inferences sketched in section 3, we see we do not need to appeal to any kind of vague intrinsic plausibility. All there is to fully understand the appropriateness of the bismuth inference is to understand the relevant background. What explains the disagreement in the literature is not vagueness, but that there are simply no interesting constraints on intrinsic plausibility, no inductive rules.

The second argument leans into the thought that background beliefs play an important role in inductive inference. Let's start with an analogy. Suppose your friend claims they are trying to avoid red meat. They've read a few articles claiming that red meat can increase your risk for colorectal cancer, high blood pressure, heart attacks, and a bunch of nasty things. They also think it's better for the environment. They self-consciously follow the rule, 'Don't eat red meat'. Unknowingly, you invite your friend to Pittsburgh Blue, a local Minnesota steakhouse chain. Your friend, hearing so many good things about this steakhouse, orders a filet mignon. During dinner, your friend brings up that they're avoiding red meat. You look down at their medium rare steak, back up at your friend, and then back down to the steak.

There is something inconsistent about your friend. If her rule 'Don't eat red meat' is defeated every time they are invited to a fancy steakhouse, then there is reason to believe they are not really following the rule at all. There is a similar kind of irrationality in the traditional picture. Recall the simplicity rule s, 'If X is simpler than Y, etc, infer X'. Again, it's important for Hume's challenge to get off the ground that this rule is contingent, i.e. there are some worlds that are s-friendly and some that are not. So it seems like you could come across some evidence that, in some certain domain, more complex theories are more likely to be true than simpler theories. It's possible that you could learn something about the world—in the analogy, that Pittsburgh Blue has really, really good steaks—which causes you to disregard the rule. But if that's so, then there's reason to think you are not actually following the rule at all.

The idea that I want to bring out is that following inductive rules is in tension with a kind of openness to experience. One should be open to the possibility that one's evidential standards are mistaken—after all, one could always gain some evidence against them. There is no tension, however, on the no-rules theory: the background information, i.e. what you know about the world, places direct constraints on what inductive inferences are appropriate to draw.

I now want to think about what a traditionalist would say in response. I think they would claim that I am missing a crucial feature of their accounts. Precisely because such examples like the bismuth inference involve appeal to domain specific background information, they fail to touch the deep philosophical issues regarding induction. Issues about inductive inference are better raised by contexts where one lacks all domain-specific background information, but yet some inductive conclusions are better supported than others. I will call such contexts *theoretically barren contexts*. One theoretically barren context is the start of inquiry when an agent has no empirical information. Although clearly a fiction, this context is useful in interpreting priors in bayesian epistemology.

For example, take Roger White's argument in his "The Problem of the Problem of Induction" [91], where he attempts to justify something like Hume's uniformity principle in a theoretically barren context. We are asked to imagine an evil demon who has taken up the task of tricking inductive reasoners. How would the demon accomplish this? Consider just a binary sequence of 1s and 0s. Each point in the sequence represents a day, and if *p*—for example, 'the sun rises'—is true on a day then there will be a 1 in the sequence, otherwise there will be a 0. The demon can meddle with the world to bring about any possible binary sequence, and the task of the inductive reasoner is to, on each day, predict what will happen on the next day. In other words, an inductive reasoner seeks to minimize the error rate: (#of errors of prediction)/(#of predictions). The demon should clearly avoid exceptionless regularities like

11111111111...

The inductive reasoner will quickly catch on and keep predicting 1s correctly. Similarly, repeating sequences like

101101011010110...

will also be ripe for induction. Perhaps the demon should try random sequences like

1011110010010100001...

White claims that, while this is a sequence which will pose a problem for the rule, it will not lead an inductive reasoner into error. A reasoner simply won't form any predictions about the sequence if there is no detectable regularity. The error rate is normalized by the the number of predictions, and so if the reasoner suspends judgment, the error rate will not be affected. The demon then must somehow trick the reasoner into thinking there is some regularity, and only then lead her into error. So maybe the most troublesome sequences will be

111111111110000000000011111111111000000000...

When the demon switches from a 1 to a 0 and a 0 to a 1 the reasoner will be tricked. But note that the error rate will actually be fairly low. For every transition from 1 to 0 or 0 to 1 there must be a string of regularities. The string of regularities will be a region of low error rate, while the only errors will occur at transitions. So overall, White argues, the errors at transitions will be outweighed by the regions of regularity.

In the absence of any information about these sequences or what they represent, White claims on the basis of the principle of indifference that we are to suppose that each possible sequence is equally likely.[10] White argues that there will be only three classes of sequences—regular, disordered, and mixed. Regular sequences will have a very low error rate. Disordered sequences will have a low error rate because the reasoner will tend to suspend judgment. Mixed sequences cannot have a very high error rate, since the demon needs to intersperse some amount of regularity to get the reasoner to commit to errors. Even if some mixed sequences have high error, they will only be a fraction of the total number of sequences, White thinks. Therefore, over all of the possible sequences, we ought to expect that induction is accurate. White thinks this shows something *very deep* about induction. Even when we lack all domain-specific background information—i.e. we do not even need to know what state of affairs 0 and 1 represent—we have reason to believe that induction is accurate. If White's argument is cogent, then it shows that a local, no-rules theory of induction must miss the mark—induction is appropriate even in the absence of any background.

But nothing deep has been shown, and White's argument is not intelligible. There are two problems. The first problem is that the inductive rule under discussion is incoherent. The second problem is that White's argument conflicts with the no free lunch theorem in statistical inference.[11] This theorem, I claim, undermines the intelligibility of induction in theoretically barren contexts.

To see the first problem, notice that in order for White's argument to have any rel-

[10]In chapter 1 (The Evidential Principle) of this dissertation I discuss the principle of indifference in detail and give some reasons for why its general use is suspect. Also, note that we are restricting ourselves to sets of finite sequences. There is an uncountable set of infinite sequences, and defining probabilities over this set is tricky business and not important here.

[11]This is pointed out in a response paper by [7]. My aim here is to develop this idea in more detail.

evance to real induction, the 1s and 0s must represent empirical propositions. But which sequences turn out to be induction friendly will depend on these propositions. For example, the sequence

0001010001000100010110101011011011101111111011111...

looks like it falls under White's category of the sequences that would fool an inductive reasoner—it makes you think there's a higher distribution of 0s in the beginning, but then switches to a higher distribution of 1s. White thinks these would be among the sequences with the highest error rate. But if each place in the sequence represents a year and a 1 represents the proposition 'the average global temperature is higher than the year before', then the sequence is not counter-inductive, and the error rate would be low, since we know that the average global temperature is rising. The intelligibility of White's argument requires that 1 and 0 represent propositions capable of *pure regularity*—i.e. those regularities that would be applicable to Hume's claim that we project past regularities into the future. But this naive kind of picture of our inductive practices, as I said earlier, has been debunked. Even if 1 represented the proposition 'the sun will rise', there is some string length n for which an inductive reasoner would start to predict 0 after n consecutive 1s—we know that at some point the sun will not rise.

Even if we accept that White's picture of induction here is simply a naive toy model, or that the inductive rule under consideration needs to be complicated or amended, we run into a more general problem. Let f be a prediction rule, a function from sequences of length $n \geq 2$ to the set $\{0, 1, ?\}$, where ? represents the reasoner's suspension of judgment. The accuracy of f is measured by the quantity (# of correct predictions)/(# of 0 or 1 predictions). Suppose f is the rule 'predict what came before'. On the sequence 1111, f would score a total accuracy of 1, whereas on the sequence 0101, f would score a total accuracy of 0. If each sequence of length $n \geq 2$ is equally likely, as White requires in his argument, then the expected accuracy of any prediction rule that makes at least one prediction is 0.5.[12] In other words, no prediction rule should expect to beat merely flipping a coin. This fact

[12]The proof of this is fairly straightforward—see appendix for details.

follows directly from the more general no free lunch theorem (NFL) in statistical inference (c.f. [96]), which state that, for any arbitrary optimization or prediction problem, any two algorithms will perform equivalently if their performance is averaged across all possible problems. The assumption that each of our sequences is equally likely turns our notion of expected accuracy into an average accuracy across all sequences. So it follows from the NFL theorem that White must be mistaken—over all possible sequences, we should not expect induction to be more accurate than any other prediction method. Crucially, this result is applicable precisely because we are entertaining the context where we have no information about what these sequences represent. If we did have such information, the background would provide distinctions between the sequences. But the NFL theorem shows that, if we cannot make any relevant distinctions between sequences, then all prediction methods are on par.

There are of course ways one might attempt to side step the NFL theorem. For example, a traditionalist may deny that each sequence is equally likely. For example, one might claim that we ought to have a higher prior confidence in simpler sequences. Sequences like '1111' and '0000' will be weighted higher than '1010' and '0101' in the calculation of expected accuracy, and so our rule 'predict what came before' now has higher expected accuracy than 0.5—cases where its accuracy are low are less likely than cases where the accuracy is high.

But this kind of move would not be open to White, whose main point was to show that, without any assumptions at all, we have reason to expect induction to be accurate. Moreover, this move is somewhat complicated by the more general theorems for which NFL is a special case. The more general result (c.f [95]) says that, translated into our special case here, the number of sequences where f_1 out performs f_2 must equal the number of sequences where f_2 out performs f_1.[13] Another way to put this is that there are just as many prior

[13] At the end of his paper, White acquiesces that the principle of indifference is controversial. But he thinks that his main point still stands if he gives it up. He weakens his argument with P3*:

> **P3*** Let P1, P2, ..., Pn be a partition of the region of logical space not ruled out by our evidence and such that we have no more reason to suppose that any one element of the partition obtains over another. If P is true in a large proportion of these possibilities then we are not justified in having little confidence in P.

Here P is the proposition that induction is reliable. White takes himself to have shown that P is true in a large portion of the possible sequences. According to P3* then, we are not justified in having little confidence distributions over sequences where f_1 out performs f_2 and f_2 out performs f_1. So, for every prior distribution that favors simpler sequences, there is a prior distribution that favors complex sequences, and for every prior distribution where induction out performs random guessing, there is a prior distribution where random guessing out performs induction. We must then, have some reason to favor one prior distribution where induction performs well over others where it does not. This brings us back to the problem I raised in the beginning of this section—no one has yet given an adequate defense or even a detailed articulation of these distributions.

Let us contrast the position the traditionalist is in with the position of the material theorist. Privileging a single or set of prior distributions over sequences is a kind of global workaround to NFL and related theorems: sequences like '1111', regardless of what empirical proposition '1' represents, are taken to be intrinsically more probable than sequences like '1010'. The materialist, on the other hand, does not need to come up with a workaround to such theorems—rather, they have a kind of local understanding of the plausibility of sequences like '1111'. If '1' represents 'the average global temperature is higher than the year before', sequences like '1111' will be more plausible than '0000'. For the material theorist, the prediction of the $n+1$ digit does not only depend on the first n digits—it also depends on the background. Because of this, the NFL theorems do not apply.

I do not think this is a complete knock-down argument for the intelligibility of induction in theoretically barren contexts. But I do think that NFL and related theorems show that in absence of a background there are no privileged prediction methods. If the traditionalist thinks that the material theorist cannot have an adequate account of induction because there are some philosophically interesting and important inferences one can make in contexts where one lacks all domain-specific background information, I take the NFL and related theorems to indicate that the traditionalist's claim is mistaken.

in induction. However, the more general result discussed here shows that if the logical space not ruled out by our evidence is the whole of logical space (i.e. we are in a theoretically barren context), then P cannot be true of a large portion of the possible sequences. Induction will out perform counter-induction on the same number of sequences where counter-induction out performs induction.

2.6 Is the Materiality of the Material Theory Immaterial?

I now want to discuss a kind of objection that is often raised in critiques of no-rules theories, especially Norton's material theory (c.f. [97, 37, 73]). Consider again the well worn example of the melting point of bismuth:

> This sample of bismuth melts at 271C
> Therefore, all samples of bismuth melt at 271C.

The material theorist claims that this inference is warranted by the background that, generally, all pure elemental samples will have the same melting temperature. The 'generally' here is to account for allotropic substances that have different melting points depending on bonding structure. As we discussed in section 3, what distinguishes this kind of theory of inductive inference is that its inductive licenses are local. All there is to fully understand the appropriateness of the inference—all our laboratory student would need to express to fully demonstrate the appropriateness of the inference—is some local, domain specific fact about certain kinds of properties of chemical elements. What is not needed is some kind of general inductive rule, e.g. 'from some As being Bs one can infer all As are Bs', (although we all know this rule is too simplistic).

The spirit of the objection is that this locality can vanish quite easily. There are two ways this might be articulated. Here is the first way: suppose the traditionalist will appeal to the inductive rule 'from some As being Bs one can infer that all As are Bs' in licensing our bismuth inference—what stops the traditionalist in also claiming that the validity of this rule derives from some fact? Here's [73] discussing this line:

> The key point here is that there is nothing crucially connecting the materiality of a given fact with the locality of a given inference. This is to say that the exact same explanation is available to formal and informal theories alike. There is nothing stopping the formal theorist from claiming that facts can justify formal inductive inferences in precisely the same way. [... continued in footnote] One

> option might be that "All A's are B's" is a fact that could justify the inference from some A's are B's to all A's are B's, in a way similar to Norton's theory. [...] This would mean that the inference was good in the All A's are B's worlds, and bad in the rest. (10 and fn 9)

Here is the inductive argument Skeels has in mind:

> Some As are Bs
> Therefore, all As are Bs.

Skeels takes "All A's are B's" as a possible fact that licenses the use of the traditionalist's simple enumerative induction.[14] If the locality of no-rules theories is located in the claim that inductive inferences are licensed by facts, then the locality would seem to vanish if the traditionalist's inductive rules could also be, in some way, valid in virtue of facts.

I think this first way of articulating the objection misunderstands the distinction between traditional and no-rules theories of induction. First I want to point out that there seems to be an equivocation in Skeels' discussion. On the one hand, we can understand Skeels' "All A's are B's" as it occurs the the propositional translation of the rule of enumerative induction: "If some As are Bs then all As are Bs". Here "A" and "B" are variables ranging over all predicates. The argument "Some ravens are black. Therefore all ravens are black" would then be valid according to this inductive rule. On the other hand, we can understand Skeels' fact where "A" and "B" are not variables. Rather, Skeels would be a context where the reference of "A" and "B" are fixed. Suppose it is fixed so that in this reading "All A's are B's" just means, e.g., "All ravens are black". However, on the first understanding, "All A's are B's" cannot be a fact, just as "x + y = 5" is not a fact. So we must understand Skeels according to the second reading, under which the fact "All ravens are black" justifies the inference

[14] Again, put aside the fact that simple enumerative induction is not a plausible inductive rule. Whatever is said here applies to whatever rules the traditionalists claim are valid.

Some ravens are black

Therefore, all ravens are black.

The trouble is that we could not be justifying an instance of enumerative induction—the proposition "All ravens are black" does not support the rule of simple enumerative induction "For any A, B: If some As are Bs, then all As are Bs". So, it is unclear what Skeels means by "… 'All A's are B's' is a fact that could justify the inference from some A's are B's to all A's are B's, in a way similar to Norton's theory" if Skeels wants this inference to be an instance of enumerative induction. This is why I think there's some kind of equivocation going on—only if we understand "All A's are B's" in the first way could Skeels' claim that enumerative induction is justified by a fact make any sense—but then it is not clear what "All A's are B's" being a fact could mean on the first reading. If we understand it in the second way, then "All ravens are black" would license the inference in the same way that "all a non-allotropic substances all have the same melting point" licenses the bismuth inference. But this just is the material theory.

I think this is a general point. I don't know what it means for a fact to justify a formal rule of inductive inference. What distinguishes no-rules theories from traditional theories, as I have been arguing, is that a traditionalist's rule of inductive inference can be applicable and appropriate in the absence of any background information. But then a fact, a contingent proposition, cannot justify a traditionalist's rule of inductive inference—there must be appropriate uses of the rule where the contingent proposition is false. I don't think Skeels' way of articulating this first way is merely an abortive attempt. I don't see any way of making this objection precise that still recognizes the distinctions I have been trying to get clear on in this paper.

So much for the first articulation of the objection that the locality of the theory vanishes. Here is the second. The locality of the inference derives from the feature that the specific and relevant background licenses the inference, rather than some background invariant schema. But what happens when we add the background as a premise? If the supplemented inference is valid, it could not be in virtue of the background, as it is now playing a role as a premise. Must then a background invariant schema license the inference? If so, it seems to follow from premise/rule duality that the background cannot play the kind of licensing role that no-rules theories require. Here is [97] discussing the bismuth inference:

> ”... once we articulate this additional ‘fact’ and add it as an extra premise, then the local nature of the inference seems not to be underlined but rather to evaporate. It then, arguably, becomes an inference of something like the following form:
>
> 1. It is highly probable that a chemical element, one (pure) sample of which turns out to have some particular value of a well-defined parameter, such as a melting point, is such that all (pure) samples have that same value.
> 2. This pure sample of bismuth has a melting point of 271C.
> 3. The melting point is a well-defined parameter.
> 4. So, it is highly probable that all pure samples of bismuth have a melting point of 271C.
>
> [...] the inference now seems to be revealed as an arguably generally valid probabilistic inference.” (742)

Worrall's inference has the following form:

> 1. It is highly probable that if A is a B then all As are Bs.
> 2. A is a B
> 3. Therefore, it is highly probable that all As are Bs.

This is a valid probabilistic inference, assuming that the second premise is taken as the claim that A is a B is certain (i.e. has probability 1)—if $P(A \supset B) \geq r$ and $P(A) = 1$ then $P(B) \geq r$.[15]

Worrall's reconstruction here is, however, misleading. One should always take a mo-

[15]If $P(A) = 1$ then $P(\neg A) = 0$, and so $P(A \supset B) = P(\neg A \vee B) = P(B)$.

ment to pause and reflect when probabilities enter the discussion unannounced. Nowhere in the discussion here or in the literature is the conclusion of the bismuth argument 'it is highly probable that all pure samples of bismuth have a melting point of 271C', nor is this the kind of proposition our laboratory student would write down in her report. Worrall's claim is indeed correct: if the background information is that it is highly probable that all pure elements share the same melting point and that one concludes from the fact that bismuth melts at 271C that it is highly probable that all samples melt similarly, then this would clearly be a probabilistic syllogism. But probabilistic logics are themselves a kind of inductive logic. No-rules theorists, however, deny that every good inductive inference is probabilistic. A feature of the view is that there is no general characterization of inductive inference. The straightforward response to Worrall then, is that we ought to simply reject his characterization of the inference.

Putting aside Worrall's presentation, I still think this objection is worth thinking through. In particular, let us examine how the objection would apply to the material theory of induction. The bismuth inference is warranted by the background that, generally, all pure elemental samples will have the same melting temperature. The 'generally' here is to signal that the use of this background induces some inductive risk. According to our best understanding of molecular properties, bismuth does not form allotropes. However, it is possible that this is mistaken. Now, how should we add this background to the inference as a premise? Here is the most straightforward answer:

> This sample of bismuth melts at 271C
> Generally, all pure elemental samples will have the same melting temperature
> Therefore, all samples of bismuth melt at 271C.

Has the locality evaporated, as Worrall thinks it does? In order for the locality to evaporate, either (1) the form of this inference is a plausible inductive schema and so its appropriateness is explained by the schema rather than the background (this is what Worrall was getting at), or (2) the inference does not take the form of a plausible schema but the background plays no more role in explaining why the inference is acceptable. Unlike the probabilistic syllogism previously discussed, however, the form of this argument is not an accepted nor very plausible inductive schema. For example, here is another inference that has the same form:

> This sample of carbon melts at 3600C
> Generally, all pure elemental samples will have the same melting temperature
> Therefore, all samples of carbon melt at 3600C.

Clearly this is not an acceptable inductive inference. We know every well that while graphite melts at 3600C, diamond melts at a much higher temperature. So the locality of the inference does not evaporate because of (1). Moreover, the relevant background explains why the carbon inference is not acceptable while the bismuth inference is. All pure elemental samples will have the same melting temperature only generally because of the existence of allotropic substances like carbon. In the second argument, carbon is one of those abnormal substances—most do not form allotropes—which is not caught by the 'generally' clause, while bismuth is. So while the background is included as a premise, the appropriateness of the inference is still explained by the background. Thus the locality does not evaporate via (2) either.

In this section I've argued that, despite what has been claimed in the literature, the locality of material inferences does not dissolve once we add the background to the premises. This is not to say that it is impossible for material inductions to turn out to take the form of traditionalist's inductive schema. The claim is that the background provides the license. When the background includes explicit facts about probabilities, an induction may take the form of a generally recognized probabilistic syllogism. For a no-rules theorist, the application of the probabilistic syllogism is only warranted because the background itself is probabilistic.

2.7 The Regress Problem

Let us take stock. I've shown so far that Hume's challenge relies on a rule-based picture of our inductive practices. I have gestured at an alternative picture where there are no inductive rules. Rather, inductive inferences are licensed by the background. The background is often domain specific and relevant only to the inference at hand and others like it. I explained how Hume's challenge arises because the vicious circularity of rules. There is no vicious circularity when it comes to the license of material inferences. The background that all non-allotropic substances all have the same melting point need not be used in order to justify the license of the bismuth inference, that all non-allotropic substances have the same melting point. On the rules-based picture, however, the rule that from some As being Bs we can infer that all As are Bs must be used in order to justify the rule that from some As being Bs we can infer that all As are Bs. The pervasiveness of Hume's skeptical challenge derives from this circularity.

I will end this paper by briefly addressing the concern that no real advance has been made with respect to Hume's challenge.[16] The worry is something like this. In order to be justified in believing that all samples of bismuth melt at 217C on the grounds that this sample of bismuth melts at 271C, one must first have the relevant background knowledge (or at least be justified in believing the background knowledge). Kelly ([37], pg 760) calls this

> **Prior Knowledge:** in order to learn a fact by induction, one must have prior knowledge of the material fact that licenses the induction.

If **Prior Knowledge** is true, Kelly thinks, then Hume's challenge arises again:

> "For consider that time immediately before we acquired our first piece of inductive knowledge. Let E represent the totality of our knowledge at that moment. Perhaps E is extremely meager, consisting of a small number of propositions. But, if the principle Prior Knowledge is true, E is nonempty. [...] In this context, we

[16]See [37, 97, 55, 73] for various articulations of this point.

> can view the inductive skeptic as someone who argues that however the various pieces of E are combined, arranged, or ordered with respect to one another, we will never be justified in moving beyond it by reasoning inductively."

In this situation where we have no inductive knowledge, E must contain only direct observations and their deductive consequences. The worry is then that nothing in E will be general enough to license any inductive inferences. The claim that non-allotropic substances all have the same melting point is itself a piece of inductive knowledge. But nothing like that could be in E. So it's hard to see how induction could even be possible. Inductive knowledge, the challenge goes, could never get off the ground in the first place. Note that an analogous problem does not affect a traditional picture, since there is no reason why an inductive rule could not license the inference from direct observations to generalities (simple enumerative induction is an example).

I think this way of putting the problem is spurious, but there is a problem in the neighborhood. Kelly tacitly assumes:

> **First Induction**: There exists some first inductive inference.

It's not clear what this could even mean. Surely no human being has ever performed a first inductive inference, and neither is there any first inductive inference for this 'we' Kelly refers to. The no-rules theorist will just say that the epistemic state described in the quote is conceptually impossible.

I take it that instead Kelly is gesturing at a regress problem. If **Prior Knowledge** is true, then for any inductive generalization we can always question how one arrived at knowledge of the background that licenses the generalization. This background is itself a generalization, and we can ask... etc. This is similar to the well worn and classic regress problem for justification, reformulated for the no-rules setting. Either the justification bottoms out, extends to infinity, or circles back.

I will take no stance on the regress problem. [53] defends a coherentist account of the regress from the point of view of the material theory. The more modest point I want to make here is just that Kelly (and others) mistakenly think that the no-rules theory trades one skeptical problem for another. This is to underestimate Hume's challenge. Hume's challenge is a charge of vicious circularity. It cannot be solved with coherentism, foundationalism or infinitism. The regress problem, on the other hand, is a problem about the traceback of justification. The problems are related but distinct. The regress problem for the no-rules materialist is just the ordinary task of textbook science: we know that bismuth does not form allotropic substances because bismuth complexes are not energetically favored. Bismuth complexes are not energetically favored because of bismuth's electron configuration. Bismuth has the electron configuration it does because of the Schrödinger equation, etc... There is nothing particularly puzzling about this traceback. It will terminate in whatever manner according to the intricate web of scientific claims. Understanding this web is important and interesting philosophical work. If [53] is correct, mature science will have a self-supporting justificatory structure. Hume's challenge, on the other hand, charges a fundamental kind of circularity, which, if unaddressed renders inductive justification impossible. The regress problem concerns the structure of justification, not its possibility. The understanding of the later is a precondition for an understanding of the former. Traditional rule-based theories, I think, do not give us an adequate understanding of the possibility of inductive inference.

2.8 Proofs

We will prove by induction that any prediction function has an expected accuracy of 0.5 for all sequences of length n, for any n. Let $E(s, f)$ be the error rate as defined in the text, where s is some sequence and f a prediction function. We show that sequences of length 2 will have expected accuracy of 0.5, and if a sequence of length n has expected accuracy of 0.5 then a sequence of length $n+1$ also has an expected accuracy of 0.5.

Proof. Suppose that f makes a prediction for sequences of length 2. We make this without loss of generality because otherwise the base case will begin at the first prediction. If f predicts '1' for the initial sequence '0', then f will either score 0 or 1 depending on whether the sequence is completed as '01' or '00'. Because each completion is equally likely, f's expected accuracy for the initial sequence '0' is 0.5. The same can be said if f predicts '0' for the initial sequence '0'. Likewise, the same can be said if f predicts '1' for the initial sequence '1'—f will either score 0 or 1 depending on whether the sequence terminates as '11' or '10', and since they are both equally likely, the expected accuracy of f is 0.5. This establishes the base case. Now, suppose f has expected accuracy 0.5 for sequences of length n. Let s_n be a sequence of length n, and suppose f predicts '1' for the digit for $n + 1$. The expected accuracy of s_{n+1} is just the average of the expected accuracy of s_n plus the expected accuracy for the prediction of the $n + 1$ digit. The $n + 1$ digit is either '0' or '1'. Both completions are sequences of length $n + 1$ which are equally likely. So the expected accuracy of the prediction is $0.5 * 0 + 0.5 * 1 = 0.5$. The expected accuracy of s_{n+1} is thus 0.5. The same can be said when f predicts '0'. When f suspends judgment the expected accuracy of s_n equals the expected accuracy of s_{n+1}.

3.0 Normality and the Modal Landscape

3.1 Introduction

There has been a marked interest in normality theories in epistemology over the last decade. [74, 75] defend a normality theory of epistemic justification and contrasts it with a broadly held risk minimization picture of epistemic rationality. [27] and [28] appeal to normality in order to solve puzzles with improbable knowledge, and [30] appeals to it to defend the KK principle. [8] propose a normality infused virtue epistemology. However, these appeals to normality often occur without much substantial discussion of what normality is or why we ought to incorporate it into our epistemology. In this paper I will attempt to rectify this. I will discuss three motivations for normality theories, two already in the literature and one new. I will then dissect two objections from [43]. The discussion in this chapter of the dissertation paves the way for the work to be done in the fourth chapter, where I propose and discuss a logic of normal justification.

3.2 Normic Support

There has been a marked interest in normality theories in epistemology in the last decade. By a normality theory, I mean any theory where normic support plays some kind of role:[1]

> **Normic Support:** A body of evidence E provides normic support for p iff p is true in the most normal worlds where E is true.

[1] This can take a number of forms; e.g. a condition on justification, where p is justified in the basis of E iff E normically supports p, or a condition knowledge, where p is known iff p is true in worlds at least as normal as the actual world.

The idea is that some worlds consistent with your evidence are more normal than others. Consider a basic case of perceptual knowledge. You look at a wall that appears red. The room is well lit and you have no reason to think that anything tricky is going on. You know that the wall is red—it would be abnormal if, despite the wall appearing red with no trick lighting, it turned out you were hallucinating. If the wall wasn't red, something out of the ordinary must have happened. But suppose your evidence was a bit different—the wall still appears red, but you are at an alternate color perception exhibition in a modern art gallery. Perceptual conditions then would be abnormal—it would not be out of the ordinary if the wall appeared red but wasn't.

[74] was the first to propose the normic support theory of justification, and [76] is a book-length treatment. [43] and [3] raise some objections to accounts of normic support. Some recent problems in virtue epistemology can be solved, claim [8], if we incorporate a normality condition in the modal dimension of skillful action.

Normality theories are attractive to those who would like to defend KK from Williamson's margin for error arguments. As [30] argues, the normality modality iterates freely, and hence according to normal conditions theories of knowledge, knowing that one knows that one knows is the same thing as knowing that one knows. [28] take a similar approach to puzzles with knowledge-chance principles and KK. Similarly, [27] employs a normality theory to deal with problems of improbable knowing. [17] extend this approach to improbable knowledge in preface cases.

Normality theories have also been important in debates about legal evidence, specifically the relevance of statistical evidence. [77, 78, 79, 80] argues against the relevance of statistical evidence on the basis of Normic Support, while [68] defends its relevance. The debate is more complicated than this, however—see [3] for an argument that epistemological theories are not particularly relevant in evaluating legal evidence. See also [12], [9], and [25].

With the exception of [74, 76], authors above utilize the normality theory to solve a problem they are interested in and devote little to no time to defending the normality theory. In this paper I will take a few steps towards a defense by getting clearer on the structure of the theory and its motivations. In the first part I will discuss three different motivations for bringing the concept of normality into epistemology. Two of these can be found in various forms within the papers cited above, while one is original. I will then, in the second part, assess an objection posed in the literature. This paper also lays the groundwork for the next chapter of this dissertation, where I investigate the logic of normality in more detail.

3.3 Three Ways of Motivating Appeals to Normality

3.3.1 Lotteries and Buses

According to an intuitive picture of justification, a belief in p is justified only if it is sufficiently likely on one's evidence. Following [76], call this the *risk minimization theory of justification*. According to the risk minimization theory, the amount of justification one has for believing p is directly proportional to how likely p is given one's evidence, i.e. the evidential probability of p. The idea behind the risk minimization theory is that we want to believe true things, and we fulfill this duty by believing those things that have the highest chance of being true.

A widely discussed example brings this kind of picture to the fore. Suppose you buy a ticket to a large and fair lottery of one million tickets. You should be extremely confident that your ticket will not win—after all there is a one and a million chance. Do you *know* that your ticket will not win? Most, if not all, epistemologists would say no. You have no way of distinguishing your ticket from the winning ticket, and so, for all you know, you have the winning ticket.[2] But, according to the risk minimization picture, you have *extremely strong* justification that your ticket will not win. Moreover, this is some of the strongest evidence you could acquire.

Normality theories can be seen as alternatives to risk minimization theories. Here are two cases that motivate this. The first is a well-known case discussed by [1], [23], and [74, 76]

Blue Bus: A bus in Twin Peaks has hit a pedestrian. Consider two possible

[2][31] for more in-depth discussions of the lottery example. Knowledge in these lottery cases are usually ruled out by a safety condition. Your belief that the ticket will not win is not safe, since there is a 'near by' possibility where you hold the winning ticket. I will talk more about safety conditions in section 1.2.

trials. In one trial two independent eyewitnesses come forward and provides testimony that the bus was from Blue Bus Company. On this basis the company was fined. In the other trial, a prosecutor notes that 95 out of 100 buses in town are from the Blue Bus Company, and so we ought to be very confident that a blue bus was at fault. On this basis, the judge throws out the case. The Twin Peaks bus accident remains an unsolved mystery.

There is something puzzling about this scenario. Eyewitness testimony is known to be somewhat unreliable.[3] In the first trial we have two independent eyewitnesses, and although it's tough to estimate the evidential probability that these testimonies provide, it seems reasonable to assume that it is less than 95%. The evidential probability afforded by the evidence in the second trial, then, seems to be *stronger*. Given that any bus in Twin Peaks could have been involved in the accident, and we know nothing about which buses were in service that day, etc, it seems reasonable to assume each bus has an equal chance of being involved.[4] It then follows that we ought to think it 95% likely that a Blue Bus was at fault. But if all we care about is believing those things that have the highest chance of being true, then it seems like we have more reason to believe the Blue Bus Company is at fault in the second trial. However, according to the vignette, after the second trial the accident remains an unsolved mystery. Moreover, it appears that we can amend the case so that 950 out of 1000 buses in Twin Peaks are Blue Buses and nothing changes. There seems to be not enough reason to convict in the second case, regardless of how strong the statistical evidence is.

Of course, this is far from a knock-down argument against risk minimization theories. The legal issues surrounding the scenario muddy the intuitions,[5] and there are surely ways for such a theorist to object to the way I have portrayed the cases (e.g. that I relied on a suspicious use of the principal principle). Nevertheless, I think the force of the example still stands. While testimony is more fallible than mere statistical evidence, this case suggests

[3]See [90].

[4]This kind of reasoning relies, I think, on the principle of indifference. In chapter 1 of this dissertation I discuss indifference principles and give some reason to find them suspect.

[5]For an articulation of this, see [3].

that testimony possesses some kind of extra force which cannot be reduced to evidential probability.

Here is a second case, adapted from [74].

> **Neon Screen**: The R&R diner in Twin Peaks has a daily promotion. Every morning when the diner opens, the neon sign outside will either be red, like normal, or flicker green for a minute. If the sign flickers green, then all pies will be 25% off for the day. You know—because you installed it—that there is a computer chip attached to the neon light, and 0.01% of the time it causes the light to flicker green when turned on.

Suppose you happen to drive by the diner when it opens. Your friend, call them David, observes the sign turn on, while you were not paying attention and did not see it. While driving you consider whether or not you should stop by the diner and get a pie.

Consider the evidence you both have that bears on the proposition that the R&R diner's pies will be 25% off. Because you know the objective chances of the sign flickering, your evidential probability of the sale is 0.9999. Do you know that there is no sale? It would be difficult to say that you do—rather, your belief that there is no sale is more accurately described as a presumption. David, on the other hand, knows that there is a sale, because he saw the sign flicker. If David says to you 'I didn't see the sign flickering—there's no sale today', you could not respond by saying 'Oh yeah, I know. Those sales are extremely rare.' It would be more natural for you to say something like 'Oh yeah, I assumed there would be no sale—those are extremely rare.'

David, then knows there is no sale, while you do not. From this it is natural to infer that David has more justification as well. His reason to believe that there is no sale is stronger. Perception is fallible, however, and David could have been hallucinating or been tricked by lighting or blinked at just the right time, etc. Again, it is difficult to quantify this fallibility, but presumably his belief forming method is less than 99.99% accurate. (If one wants to quibble with this number then the case can always be adjusted.) Your belief, on the other hand, *is* 99.99% accurate. So high evidential probability is not sufficient for knowledge or justification.

More importantly, we can generate a Neon Screen-like scenario for any knowledge generating method. They will all have this sort of structure: David comes to know P on the basis of some knowledge generating method m. Since m is a fallible method, there is some chance c that beliefs based on m are false.[6] You, on the other hand, come to believe P only because you know the objective chance of P, which is $c*$. Your epistemic position is not strong enough to come to know P, while your friend's epistemic position is. But we can engineer the case so as to guarantee $c > 1 - c*$. So, the evidential probability is not what distinguishes the epistemic states of you and David.

What else is it then that distinguishes your epistemic state from David's? This shows that we cannot appeal to facts about *specific* belief forming methods—e.g. we cannot say that perception is, when exercised in appropriate conditions, knowledge generating, and this is why David's epistemic position is stronger than mere statistical evidence. Rather, we need an explanation, for any justification/knowledge belief forming method, why high evidential probability is not enough.

The normality theory can be seen as a way of answering this question. Here's the normic support condition again:

> **Normic Support**: A body of evidence E provides normic support for p iff p is true in the most normal worlds where E is true.

Crucially, we need some understanding of 'most normal worlds'. Here is one way to develop an account, outlined in [75]. If something abnormal occurs, then something out of the ordinary must have happened. If something out of the ordinary has happened, then there's an intelligible answer to the question "What went wrong?". The answer will be an explanation for why things turned out to be abnormal. So, p is true among the most normal worlds where E is true when, if p is false, the world has turned out to be abnormal with respect to p, and this deviation from normality can be explained. Normality rankings, on this view, measure

[6] I do think the motivation for normality theories (at least for knowledge) relies on fallibilism. However, normic theories of knowledge do not generally presuppose fallibilism. See [22] for a discussion of an infallibilist version of a normic theory of knowledge.

deviation from normal conditions, and such deviations can be understood qualitatively as needs for explanations.

To make this account completely clear, we will need some formal machinery. This task is fully discharged in the next chapter. But briefly, a set of worlds W is equipped with a normality ranking '$\leq$' which is a well-founded partial order.[7] We can think of the normal conditions as the worlds in W which are minimal according to $\leq$. (I.e. the set of $w \in W$ such that $\forall w' \in W \neg(w' \leq w)$) According to Normic Support, E supports p iff all the minimal worlds in E under $\leq$ are worlds where p is true. There are normal conditions consistent with your evidence, and E supports p just in case p is true in those conditions.

To illustrate, consider the Blue Bus case where two independent eyewitnesses say that a Blue Bus hit the pedestrian. Suppose that in fact a Red Bus hit the pedestrian. In such a scenario, some kind of explanation would be in order. Although it is consistent with our evidence that the two eyewitnesses provided false testimony, it would be abnormal for it to be misleading. The witnesses must have unknowingly hallucinated, misremembered, or been bribed, etc. These are the kinds of circumstances that could not have reasonably been foreseen—in terms of the semantic definition given above, such circumstances are worlds in E that are not minimal with respect to $\leq$. In fact, they will be quite abnormal.[8] Contrast this with the scenario where all the evidence you have is that 95 out of 100 buses are from the Blue Bus Company. If a Red Bus in fact hit the pedestrian, no explanation would be required and no kind of unforeseen circumstance must have occurred. There is no special reason why it should have been a Blue Bus rather than a Red Bus. This idea is perhaps more easily appreciated in the lottery. Intuitively, your ticket is just like any other. If your ticket happens to win against all odds, nothing special or out of the ordinary occurred—after all, it could have been any ticket.

This brings out an important feature of normality theories. It is highly likely that, for

[7] The kind of logic of justification that arises from these definitions depend on the features of the ordering. I will argue in the next chapter that a well-founded partial order is the most appropriate, and I will highlight one consequence of not taking the ordering to be a total order in section 2.2. In the next chapter we will also define a relation '$\ll$' which means 'much more normal than'.

[8] One might think here that an eyewitness misremembering is not very abnormal—this is precisely what makes eyewitness testimony weak evidence in court. But this is entirely in line with the normality theorist's account. If in fact it is normal for people to misremember things, especially in high stress situations, then such testimony is weak evidence.

each ticket t_i, t_i will not win. So for each ticket t_i, the evidential probability that t_i will not win is high. However, consider the conjunction of all the propositions 't_i will not win' for each i. Because you know that some ticket will win, this proposition has zero evidential probability. This shows that the set of propositions enjoying high evidential probability are not closed under deduction. The set of propositions enjoying normic support, on the other hand, is closed under deduction. You do not have normic support for the proposition 't_i will not win' for the same reason that you do not have normic support for the claim that the Blue Bus is at fault in the second Blue Bus trial. Generally, if we understand abnormal circumstances as those that require explanation, rules like conjunction introduction will be valid—an explanation of the falsity of $(A \wedge B)$ will also be an explanation for the falsity of either A or B, and vice versa.[9] Normic support is closed under deduction.[10]

3.3.2 Safety Conditions

The idea that there is some kind of necessary modal condition on knowledge is a popular one.[11] In particular, many endorse a safety condition, whether as an explicit condition on knowledge or as a consequence:

Safety: In all nearby worlds where S believes p, p is true.

My aim here is not to defend or criticize safety theories. Rather, I just want to suggest that we can understand safety conditions as normality conditions. One of the problems with safety and other modal conditions is that they rely on a fairly unspecified notion of 'nearby world'. If the notion is a kind of resemblance between worlds, which features should we take as salient? Our intuitions here usually derive from specific cases. Consider again the lottery. The world in which I win the lottery is nearby the world in which I did not. The

[9] For a more detailed discussion see [76]

[10] What about the preface paradox? The natural line for the normality theorist is that the proposition "some claim in the book is false" is based on a statistical generalization—because each claim has a chance to be false, it is extremely likely that at least one proposition in the book is false. But statistical generalizations are not normically supported. It would not be abnormal if all the claims in the book were true. It would just be unlikely.

[11] See [59] and [11]

only difference between the two worlds is that a random number was drawn differently—it could have very easily been the case that my number was drawn. Alternative situations that could have very easily been the case do not impart considerable distance between worlds.

Instead of relying on phrases like ‘could have very easily been false’, we can instead classify those worlds nearby some world w as the set of worlds that are at least as normal as w:

> **Normic Safety**: In all worlds at least as normal where S believes p, p is true.

In the lottery case, there is nothing abnormal about my number being drawn at random compared to any other number. So according to **Normic Safety** lottery beliefs are not safe. Safety plays a substantial role in anti-luck epistemology. If a belief is only luckily true, then there is some nearby world where one does not enjoy the luck and the belief is false. But lucky worlds are just as normal as unlucky worlds—if not there would need to be some substantial explanation for why the lucky circumstance obtained but not the unlucky circumstance. But then it’s not just merely a matter of luck. Matters of luck do not have substantial explanations—there is no substantial explanation for why the die landed on an even number rather than an odd number. So it seems that Normic Safety can also play the same role in anti-luck epistemology.

A defender of safety must deal with potential counterexamples. Let us see if our normic formulation helps with this. Here are two that I think are most illustrative:

> **Temp**: Temp forms beliefs about the temperature in her house on the basis of a broken thermometer. However, every time Temp glances at the thermometer, a guardian angel intervenes and ensures the thermometer reports the correct temperature. Only a lucky few have guardian angels looking out for them.[12]

> **Halloween Party**: Susan is hosting a halloween party. She decides she will invite everyone at the department’s reception this Friday. She really does not want

[12] Adapted from [60].

> Michael to come to the party, though, and Michael may or may not show up at the reception. So, she plans on giving everyone at the reception directions to her house for the party, but if Michael shows up to the reception, she will move the party to her friend Jacky's house but only afterwards tell this to everyone except Michael, who will mistakenly think it will be held at Susan's. Suppose Michael never shows up to the reception. Joe, however, does not, but very nearly does, decide to trick everyone at the reception by wearing a very realistic Michael costume. Jan attends the reception and receives the invite to Susan's party which will take place at her house.[13]

In **Temp**, Temp's belief about the temperature does not constitute knowledge—it is only a matter of luck that she has accurate beliefs. After all, had she not been one of the few to possess a guardian angel, her beliefs would have been false. But, assuming that the guardian angel ensures that the thermometer reports the correct temperature in all of the nearby worlds, Temp's belief will be safe. In **Halloween Party**, let us suppose that Jan forms her belief right after leaving the reception but before Susan would have told her about the location switch had Susan thought that Michael showed up. Jan's belief that Susan's party will take place at her house does constitute knowledge—after all Susan came to the reception intending to tell everyone about the party, and she indeed truthfully tells Jan where the party is going to take place. But, the counterexample goes, Jan's belief is not safe. It could have very easily been the case that Joe wore a Michael costume, making Susan switch the location of the party. In this situation, Jan's belief that Susan's party is at her house is false.

The response to **Temp** is fairly straightforward—Temp's belief is not normically safe. Temp's situation is quite abnormal. Guardian angels do not only normally not exist, but in the worlds where they do exist, surely it is not normal for them to spend their concerted efforts ensuring the veracity of our beliefs about the temperature. So situations where Temp forms the belief without a guardian angel will be at least as normal as those worlds where Temp has a guardian angel. But in those worlds where Temp does not have a guardian

[13] Adapted from [47]. This one is particurlarly troublesome—see [13].

angel, Temp will have false beliefs.[14]

As we can see, Normic Safety expands the situations that count as nearby to those worlds that are at least as normal. The effect is that when circumstances are abnormal, beliefs must, as it were, reach further into modal space in order to be safe. This corresponds to intuition that you know less when conditions are abnormal.[15] Any situation where a broken thermometer somehow actually tracks the temperature will be abnormal—a special explanation will be needed in order to explain why a broken thermometer is nevertheless tracking the truth.

In **Halloween Party**, on the other hand, Jan's belief is normically safe. The situation where Michael never shows up to the reception but Joe arrives with a very realistic Michael costume is abnormal. Such a situation would require some kind of explanation—why would Joe come to the reception with a realistic Michael costume? Because this situation is abnormal, it does not serve as a counterexample to normic safety—the situation is not at least as normal as the situation where Jan correctly believes the party will be at Susans and Joe does not come with the costume.

These analyses do not come without their questions. The defender of normic safety must claim that any sort of 'near miss' in cases structurally similar to **Halloween Party** must be abnormal. For example, suppose that Joe in fact normally dresses up as his fellow graduate students at department receptions. If someone asks 'Why is Joe dressed up as Michael?', you might respond 'Ohh, that's just something that Joe normally does. It's not creepy at all.'

I think there are a few responses one can give. First, you could just deny that this behavior is normal, even though Joe might do it every week. Moreover, in order for the case to make sense, Susan needs to be convinced of Joe's disguise. If Joe is in the habit of dressing up like others then Susan might be more wary—the situation where Susan is fooled even when she is looking out for fake Michaels is abnormal. Second, suppose we accept that it is possible that Joe normally shows up in disguise. I'm then tempted to say that it would be something like a barn façade case. In barn façade county, the conditions for seeing barns

[14] See [8] for more discussion of the case of Temp. There they focus on the modal dimension of skill, but what they say there also holds for belief.

[15] This is modeled more formally in the next chapter.

are not normal. Likewise, the conditions for spotting Michael at the party are not normal, in the sense that there are possible Michael façades. So if one is comfortable in denying knowledge in the barn façade cases one would be comfortable in acquiescing that, in this case, Jan does not know that party is at Susans.

There is a large and evolving literature on modal conditions in epistemology. Clearly this is only a beginning of a defense of Normic Support. Minimally, my claim in this section is that those who think safety conditions play a role in epistemology ought to consider the normality modality.

3.3.3 The Problem of the Prior

One natural way to fill in the risk minimization view is to have a broadly Bayesian picture of epistemic rationality. There are of course many flavors of Bayesianism—subjective, objective, modest, etc. But I will just focus on objective bayesianism for now. In this section I will outline a certain set of assumptions of the bayesian picture, and show that the normality picture can be seen as a way of motivating the rejection of these assumptions. Even if one does not find the flavor of objective bayesianism under discussion here plausible, I think it is still illustrative of a set of motivations for the normality theory.

On the Bayesian picture, an agent's confidence can be measured by a probability function C.[16] Upon getting some evidence E, the following rule relates the agent's prior confidence to her posterior confidence:

> **Conditionalization**: Let C_E be the agent's confidence after receiving evidence E. Then, for any proposition p, $C_E(p) = C(p|E)$, where
> $C(p|E) = C(p \wedge E)/C(E)$.

[16]That is, C is a function that obeys that axioms:

(1) $C(p) \geq 0$
(2) $C(T) = 1$, where T is a tautology
(3) If p and q are disjoint, then $C(p \vee q) = C(p) + C(q)$

Conditionalization is the corner stone of bayesian epistemology and where the majority of its explanatory power derives. Another principle implicit in discussions of bayesian epistemology and learning is the following:

> **No Learning**: For any body of evidence E and proposition p, if $C_E(p) < C(p)$, then one cannot learn or come to obtain justification in believing p on the basis of E.

Here is a specific instance of No Learning:

> (1) $C_E(E \supset p) < C(E \supset p)$.

From a little bit of calculation it is easy to see that this follows from the probability axioms and the definition of C_E.[17] It follows from (1) and the fact that $C_E(p) \leq C_E(E \supset p)$, which again is easy to check, that

> (2) $C_E(p) < C(E \supset p)$.

What's the epistemological significance of (1) and (2)? From (1) and No Learning it follows that you cannot come to have justification in believing $E \supset p$ on the basis of E. If one has justification to believe $E \supset p$, then one will have reason to believe p on the basis of E. Though the notion of confirmation in the bayesian framework is vexed, but we can think of one's estimation, at least a kind of tentative estimation, of the strength of E's bearing on p as one's confidence in $E \supset p$.[18] At least, it seems fairly implausible that, upon learning something that lower's one's confidence in $E \supset p$, one will have more reason to believe p on the basis of E. So, given (1) and **No Learning**, your estimation of E's bearing on p is set *before* you even come to learn E.

[17] Assuming here that E is contingent, i.e. that $C(E) \neq 1$ or 0, and that E does not entail p.

[18] For example, one might take $C_E(p) - C(p|E)$ as a measure of confirmation and hence the strength of E's bearing on p.

Further, suppose the bayesian endorses the following view about the connection between confidence and justification:

> **Lockean Thesis (LT)**: There is some threshold r such that, for any p, the agent is justified in believing p iff $C(p) \geq r$.

There's a lot of discussion in the literature about the Lockean Thesis, and there are many ways to weaken, contextualize, or amend it.[19] Something like it, though, is usually presumed when bayesians discuss justification, if they do at all. Taking it for granted for the moment, LT and (2) entail the following:

> **Prior Justification (PJ)**: One has justification to believe p on the basis of E only if one has prior justification to believe $E \supset p$.

Combining PJ with the observation made about (1), the following kind of picture emerges from bayesian rationality. Before you acquire any bit of contingent evidence, e.g. that someone testifies that they saw a Blue Bus hit a pedestrian, your estimation of the strength of that evidence is already been fixed—you were already justified in believing that if someone testifies against the Blue Bus, then they are likely guilty. On the face of it, this doesn't seem too implausible. Before you enter the courtroom, you know that plausible testimony will probably sway your vote, and once you hear it, you suspect that the Blue Bus Company is guilty. But consider the view at the peripheral of the picture. From the beginning of inquiry, a rational agent goes around collecting evidence and applying principles that resemble modus ponens. She anticipates everything she ought to believe conditional on how the world turns out. As it were, the whole evidential landscape is laid out before her at the start of inquiry.

I think there are reasons to think that this picture is implausible. It suggests that we have all the information we need to determine the evidential relationships in any world

[19] See [24] for the original discussion and [26] for a recent overview of Lockean approaches. Note that the Lockean Thesis entails Lower Learning, but you can accept Lower Learning without being committed to the Lockean Thesis.

whatsoever, no matter how different it is from ours. Why we would have access to that kind of information at the start of inquiry? I am not the only one to question this. During the course of defending dogmatism, [89] arrives at a similar sentiment:

> "So consider, for a minute, a soul in a world with no spatial dimensions and three temporal dimensions, where the primary source of evidence for souls is empathic connection with other souls from which they get a (fallible) guide to those souls' mental states. When such a soul conditionalises on the evidence "A soul seems to love me" (that's the kind of evidence they get) what should their posterior probability be that there is indeed a soul that loves them? What if the souls have a very alien mental life, so they instantiate mental concepts very unlike our own, and souls get fallible evidence of these alien concepts being instantiated through empathy? . . . those souls are presumably just as ignorant about the epistemologically appropriate reaction to the kinds of evidence we get, like seeing a cow or hearing a doorbell, as we are about their evidence." (10)

If the bayesian picture of rationality is correct, then a rational agent ought to know, before any inquiry or empirical information, how to interpret the evidential relevance of empirical propositions in any possible world, including those worlds where one gets, e.g. evidence that an angel seems to love them. A fully rational agent, according to the picture, is 'evidentially omniscient'. Weatherson and I are not merely pointing to the fact that bayesian rationality is excessively idealized. Models may make many idealizations and still be explanatory. Clearly real human agents do not have evidentially loaded priors—we probably do not even have very informative ones. But it is one thing to think that the bayesian picture does not describe actual human agents, and another to think that the picture does not do justice to the concept of epistemic rationality. If this picture is correct, then rational agents must know the conditionals, e.g. 'If a soul seems to love me, then there's a good chance. . . ' But why must rationality require knowledge of these kind of conditionals?

Evidential relations can be expressed as a proposition of the form 'X is good evidence for Y'. Having beliefs about evidential relations is just to have beliefs about propositions of this form. The debate here boils down to whether or not you think evidential relations are a priori or a posteriori. If they are a posteriori, then, at the beginning of inquiry one cannot know the evidential relations. If they are a priori, then one might take epistemic rationality to require beliefs about evidential relations in the same sense in which epistemic rationality requires one to believe 1+1=2.

In chapter 2 of this dissertation, I outline a general approach to induction which I call the no-rules approach. I frame things in terms of induction there because I would like to interface with debates in the the philosophy of science and inductive skepticism. But I think that, if you are drawn towards no-rules theories of induction, then you should be drawn to the idea that evidential relations are a posteriori. In the no-rules theory of induction, the empirical background information is what serves as a license for inductive inferences. It follows from this that without any background, no inductive inferences are licensed, and in order to learn what follows from what, one must first learn relevant background information. One cannot learn empirical background information a priori. As I see it, the normality theory can be seen as the epistemological underpinnings of the no-rules theory of induction.

I think it is natural to understand claims of normality as empirical—you need to know something about the world in order to know that, normally, graduate students do not attend receptions cleverly disguised as other graduate students. But then it follows that, if evidential relations are relations of normic support, evidential relations are empirical. Thus normic support allows us to develop a theory of evidential relations in contrast to views where evidential relations are encoded by evidential priors (in the objective bayesian case), or in contrast to any kind of view that takes evidential relations to be knowable a priori.

3.4 Moorean Absurdities

I now want to turn to an objection against Normic Support. Recall the lottery scenario discussed earlier. You possess a ticket to a large fair lottery. Consider the following proposition:

(1) This ticket lost, but I do not know that it did.

This proposition is highly likely to be true. Moreover, you should be extremely confident that it is true. It is overwhelmingly likely that your ticket lost. You also know, if you happen to do a bit of introspection, that you do not know that the ticket lost, and so you should be near certain, if not certain, that you do not know that it will lose. However, (1) is a Moorean absurdity. You are never in a position to know a Moorean absurdity—assuming knowledge distributes over a conjunction, if you know (1) then you know that you do not know that your ticket is lost. But since knowledge is factive, you do not know that your ticket lost. But if you know (1) then you also know that your ticket has lost, which is a contradiction. There's also reason to believe you cannot have justification to believe (1) either. Again, assuming that justification distributes over a conjunction, in order to be justified in believing (1) you must be justified in believing you do not know that your ticket has won. The contradiction then follows by the structural principle $J\neg Kp \supset \neg Jp$.[20]

The risk minimization picture fails to explain why we cannot be justified in believing Moorean absurdities like (1). Normality theories, on the other hand, can. Your evidence does not normically support the proposition that your ticket has lost—no explanation is needed if your ticket does win—and so your evidence does not support (1). [43] claim, however, that while normality theories can explain one's lack of justification in (1), there are other Moorean absurdities that the normality theory predicts you will have justification for.

Here is their argument. Let '$p \rightharpoonup q$' mean that p normically supports q. There is some body of evidence E and proposition p such that the following are jointly true:

$$E \rightharpoonup p$$

$$\neg(E \rightharpoonup Kp)$$

They have in mind cases like [94]'s unmarked clock. You glance at a clock which has no numbers. Let p be the strongest proposition you know about the position of the hand of the clock. You are justified in believing p because you know it. However, your evidence does

[20]See [81] and [66] for a defense of this principle.

not support that you know p. Williamson shows that on your evidence it is very improbable that you know p. Moreover, any world in which you know p does not seem to be any more normal than any world where you know something slightly different. There may be ways of resisting this claim,[21] but let us just grant this point for now—there is some E and p such that E supports p but E does not support that you know that p.

Smith's ([76], pg 89, 141; [77]) semantics for normic support validates rational monotonicity:[22]

$$E \rightharpoonup p$$
$$\neg(E \rightharpoonup \neg q)$$
$$\text{Therefore, } (E \wedge q) \rightharpoonup p.$$

As the name suggests, rational monotonicity is among the general class of monotonicity schemas. Full blown monotonicity is the following:

$$E \rightharpoonup p$$
$$\text{Therefore, } (E \wedge q) \rightharpoonup p.$$

Clearly, if our intended meaning for this conditional is evidential, then it should not validate full blown monotonicity. Full blown monotonicity is inconsistent with the existence of defeaters—if q is a defeater for E's evidence for p, then when one supplements E with q, p is no longer justified. However, there are ways of weakening monotonicity. Rational monotonicity is one such weakening. The idea is that, if you have justification for believing p, and q is a defeater for your justification for believing p, then you must have some reason for ruling out the defeater. Otherwise, the thought goes, the potentiality of the defeater's defeat of p is actualized.

[21] See [76] ch 1 for an argument for what he calls the normative coincidence thesis, which would rule out Williamson's case. However, I'm not convinced by Smith here—he uses cases like the lottery to motivate the thesis, but the phenomenon at issue in Williamson's case is quite different.

[22] I will discuss monotonicity schemas in the logic of justification in more detail in the next chapter.

In a case where $E \rightharpoonup p$ and $\neg(E \rightharpoonup \neg\neg Kp)$, it then follows by rational monotonicity that:

(1) $(E \wedge \neg Kp) \rightharpoonup p$.

Like the material conditional, if $(p \wedge q) \rightharpoonup r$ then $(p \wedge q) \rightharpoonup (q \wedge r)$ and so from (1) it follows that:

(2) $(E \wedge \neg Kp) \rightharpoonup (\neg Kp \wedge p)$.

But (2) means that you are justified in a Moorean absurdity. But it is impossible to be justified in a Moorean absurdity. Therefore, the argument concludes, normic support fairs no better than the risk minimization picture when it comes to Moorean absurdities.

This is an interesting little argument. But we should take it as a reductio of the normality framework, as Littlejohn and Dutant suggest, only if rational monotonicity plays an integral or ineliminable role in the framework. Otherwise, the normality theorist may treat the argument as a reductio of rational monotonicity. So, is there room to give up rational monotonicity?

I think there is. Consider a classic example of defeat. You walk into a normal looking factory and see a conveyer belt of what looks like blue widgets. You have good reason to believe that the factory makes blue widgets. Now suppose you are told by the foreman that the widgets are actually illuminated by focused blue lights, which allows the machinery to detect cracks and defects in the widgets. This defeats your justification that the factory makes blue widgets. The crucial question is what kind of epistemic status the defeater 'widgets are illuminated by focused blue lights' (call it d) has *before* you are told it by the foreman. Clearly you cannot have justification in d. If d is a defeater for E's justification for p, and you have a reason to believe d, then p is defeated. This is encapsulated by the weaker monotonicity principle (sometimes called *cautious monotonicity*):

$E \rightharpoonup p$

$E \rightharpoonup q$

Therefore, $(E \wedge q) \rightharpoonup p$.

Do you have justification to believe $\neg d$? It's not obviously clear that you must. Suppose the foreman asks, wanting to see if I noticed the focused blue lights, if there was any subtle lighting tricks I could detect. I could reasonably answer 'I don't know—there is a lot of machinery in here and I don't really know what any of it does.' I could reasonably suspend judgment on whether or not there is trick lightning in the factory, and still have reason to believe that widgets are blue. One can even imagine a case where I am told that there may or may not be trick lighting in the factory before I enter, and I thereby suspend judgment on the matter. If I seem to see blue widgets, I will still have reason to believe the widgets are blue, as long as I cannot decipher whether there is trick lighting.[23]

Denying rational monotonicity is not open to Smith's normality theory, as it is built into his sphere semantics. In the next chapter, however, I will propose an alternative semantics where rational monotonicity fails. The objection by Littlejohn and Dutant highlights the importance of a careful study of the logic of justification—we need a general framework in order to assess these kind of objections. I show that an evidence conditional is rationally monotonic only if the normality relations are totally ordered, i.e. for any two distinct worlds w and w', either $w < w'$ or $w' < w$. There are some independent reasons to not require that normality relations be totally ordered. One is that comparisons of normality rely on relevance. If w and w' are not relevantly similar, then there may be no fact of the matter whether one is more normal than another. Another is that, even if the set of worlds is finite, there need not be some uniquely normal world: for $w, w' \in N$, neither $w < w'$ nor $w' < w$. That is, if a description of the most normal conditions is N, it could be the case there are a number of worlds where N is true. In other words, the normal conditions need not be fine grained enough to distinguish between all worlds.

[23] One might worry that rational monotonicity is a fast lane towards skepticism: that I am an envatted brain is a defeater for my belief that this widget is blue. (If I'm envatted, there are no widgets in front of me) So, if rational monotonicity is valid, I must have justification to believe I am not envatted. Such justification is impossible. Therefore, I do not have justification for believing the widget is blue (even when it is a normal factory producing blue widgets). This is analogous to traditional forms of closure-based skepticism.

4.0 The Logic of Normality

4.1 Introduction

In this chapter I will propose and explore an evidence logic based on normality relations. I characterize the evidence conditional and show that it validates a number of desirable inferences. I also formalize knowledge and belief in the logic, and show that in this formalization KK holds. Normality theories are attractive to those who would like to vindicate KK in light of various arguments and puzzles. I show, however, that KK holds only under the assumption that normality relations do not depend on the world of evaluation. I argue that there are good philosophical and logical reasons why we should take normality relations to depend on the world. The logical investigations of this chapter reveal that a burden is placed on those who would like to defend KK by appeal to normality—the burden of justifying the assumption that normality relations are uniform.

In the previous chapter of this dissertation I outlined some of the motivations for normality theories in epistemology. By a normality theory, I just mean any theory of justification, knowledge or evidential support where the concept of normal worlds plays some important role. In the last chapter I focused on **Normic Support:**

> **Normic Support:** A body of evidence E supports p iff p is true in all the most normal worlds where E is true.

In this chapter, I will focus on formalizing this notion of evidential support, as well as notions of knowledge and justification based on it. I have three primary motivations for this. First, as I highlighted in the last chapter, [43] argue that Normic Support leads to undesirable Moorean absurdities. I argued that such absurdities only follow if normic support is rationally monotonic. How do we determine if a relation of evidential support ought to be rationally monotonic? One way through is via intuitions. But another way is through a logical characterization of the support relation. In particular, logical principles are valid in virtue of the structure of the normality relation. For example, the support relation is rationally monotonic only if the normality rankings among worlds is totally ordered. This illustrates a need for a complete characterization of the logic of normality and the examination of normality rankings. My second motivation is that normality theories have been used in the literature to vindicate the KK principle against various objections.[1] I think this deserves a bit of formal scrutiny for reasons I will discuss, and I will propose a logic of normal knowledge and investigate under what conditions KK holds. Lastly, my third motivation is that there is recent interest from the epistemic logic literature in investigating logics of justification.[2] As I will discuss later, there are certain limiting constraints on the logics proposed in the literature which undermine their ability to be applicable to epistemology. The normality logic I propose here will lift those constraints.

Here is the plan. In section 2 of this paper I will discuss the normality theory's relationship to KK. In section 3 I will discuss the preliminaries for the logic of normal justification and knowledge I propose in section 4.

4.2 Normality and KK

4.2.1 Arguments for KK

While **Normic Support** only speaks of evidential support, some have also proposed normality accounts of knowledge as well. In the last chapter I discussed the major component of those theories of knowledge, normic safety. The exact details are not too important here, but the main idea is that, when one knows p, p is true in all worlds at least as normal as the actual world. This means that if conditions are normal, then one should expect to know that conditions are normal.[3] After all, if w is a normal world, then all the worlds at least as normal as w will be normal as well. Moreover, if conditions are not normal, then one should

[1]See [28] and [30] for illustrations.

[2]E.g. [85, 86, 6, 5, 10].

[3]I am being deliberately sketchy here, as I want to speak generally about the flavor of normality theories. Most naturally, **Normic Support** will be a necessary condition on knowledge. Accounts may differ on what sufficient conditions there are.

expect to know less.

Normality accounts of knowledge are interesting because they provide a natural model where the KK principle holds. Here is a motivating example. Suppose you are considering when the next bus will come. You know that buses normally arrive every 15 minutes—the schedule says so, and this is confirmed by experience. In addition, it would be quite abnormal if the bus was over an hour late. If there is a bus within the next 15 minutes and conditions are normal, then you know that the bus will not be an hour late. Now, consider what it would take for you to know that you know that the bus will not be an hour late. You must know in all the those situations at least as normal as the one where the bus is coming within 15 minutes that the bus will not be an hour late. But those are all situations that are normal, and so by the same reasoning, you will know in those situations that the bus will not be an hour late. Knowing that one knows takes no more epistemic effort than knowing.

Here is a more precise argument, adapted from [30]. Consider the following necessary condition on knowledge, defended by Greco:

> **Normal Conditions:** S knows that p only if (1) S has some evidence E such that, whenever conditions are normal, S has E only if p and (2) conditions are in fact normal.

So then S knows that S knows that p only if

> (1') S has some evidence E' such that, whenever conditions are normal, S has E' only if S knows that p and (2') conditions are in fact normal.

Suppose S knows that p. It's clear that (2') is satisfied. So to show that when S knows p, S knows that she knows p, we must show that (1') follows from (1). Take $E' = E$. Then we need to establish

> (3) S has some evidence E such that, whenever conditions are normal, S has E

only if, whenever conditions are normal, S has E only if p.

Since S knows that p, S has evidence E such that whenever conditions are normal, S has E only if p. But since S knows p only whenever conditions are normal, (3) is true. Therefore, **Normal Conditions** entails the KK principle.

There is a sense in which the theory here just sketched is a kind of *reliabilist* theory of justification and knowledge. According the traditional flavor of reliabilism, a belief is justified just in case it is based on a reliable process, where a reliable process is one which would yield a high proportion of true beliefs. This idea of a high proportion is often understood statistically. But on the normality theory, a reliable process is understood as one which would *normally* yield true beliefs—if you justifiably believe p, then if the situation is normal, p is true. We can understand normality theories as externalist for the same reason reliabilist theories are externalist. Whether or not a belief is justified depends on whether it was reliably formed, and whether or not something is reliably formed is not something that one can determine by introspection and reflection alone (or insert here your preferred condition for internalism). The same goes for the normal conditions theories. Whether conditions are normal cannot be determined by introsepction and reflection alone. And this is why normal conditions theories of knowledge are interesting—normal conditions theories are externalist theories that motivate **KK**, a principle that is, as usually understood, internalist. On an internalist picture of knowledge, knowing that one knows is a matter of knowing that one's belief is warranted. And, on that picture, one is always in a position to know whether or not their belief is warranted, and so one is always in a position to know that one knows.This mix of internalism (the KK principle) and externalism (reliabilism) is what makes normality theories attractive.

4.2.2 Flipping Coins

I now want to briefly examine a puzzle about knowledge and chance discussed by [28], and consider a response to it. This will serve to illustrate another way normality theories are used in the literature, as well as give us a concrete scenario to test our formal definitions in section 4. Consider the following scenario:

> **Flipping Coins:** There are 1,000 coins laid out one after another: C_1, C_2, ..., C_{1000}. A coin flipper will flip the coins in sequence until either a coin lands heads or they have all been flipped. In either case the experiment will end.

Surprisingly, as [28] show, the following are an inconsistent triad:

> **No Skepticism:** You know that the coin C_{1000} will not be flipped.

> **Weak Fair Coins:** If a coin is fair and will be flipped, then for all you know it will land tails.

> **KK**: If you know p, then you're in a position to know that you know p.

Here is their reasoning. Suppose you set the coin flipper into motion, but have not yet observed what has happened, and suppose that in fact the ith coin landed heads (though this is not important for the argument). By **No Skepticism** you know that C_{1000} will not be flipped. Then, the reasoning goes, there is some first coin C_{n+1} such that you know C_{n+1} will not be flipped. Hence for all you know C_n will be flipped. If **Weak Fair Coins** is true, then you can at least come to know this principle after some amount of reflection, and so if you know that you know that a coin will not land tails, then you know that it is false that the coin is fair and will be flipped. But you know that C_n will not land tails, because if it did then C_{n+1} would be flipped, and you know that it won't. So by **KK** you know that you know that C_n will not land tails, and hence you know that it is false that the coin is fair and will be flipped. By the set up of the case, you know that it is fair, and so you know that C_n will not be flipped. But this is a contradiction–for all you know C_n will be flipped.

The idea underlying **No Skepticism** is that if you deny that you can know that the last coin will not be flipped, then you have capitulated to extreme skepticism, the reasoning

$$\begin{array}{ccccccccc}
T & T & & H & & & & & \\
f & f & & f & & \neg f & \neg f & & \\
Kf & \neg Kf & & \neg Kf & & \neg Kf & K\neg f & & \\
C_1 & C_2 & \ldots & C_i & \ldots & C_n & C_{n+1} & \ldots &
\end{array}$$

Figure 1: An example scenario from **Flipping Coins**. f is the proposition that the coin is flipped. C_{n+1} is the first coin you know will not be flipped.

being that such skepticism could not be confined to these kinds of artificial cases.[4] Take a natural, chancy process like leaves falling from trees in the colder months. Each day there is a small chance that the leaf falls, but it would be incredibly unlikely if the leaf were to survive the winter. If there is no i such that you know that coin C_i will not be flipped then it is not clear why, by parity of reasoning, you can know of some tree in particular that it will lose most of its leaves, as we clearly can.[5] The choice between **Weak Fair Coins** and **KK** remains. We can take this as a reason to reject KK–there are already a number of arguments against KK in the literature, and **Weak Fair Coins** is intuitively plausible, and so the simplest explanation for the inconsistent triad is that KK is false.

But what will it take to defend KK in the light of **Flipping Coins**? An adequate response to this argument must, one, explain why, despite being false, **Weak Fair Coins** is

[4]See [20] for a defense of this claim.

[5]Consider the following variant on the **Flipping Coins** puzzle. You buy a number in a large and fair lottery. Instead of choosing a winning number at random, a series of drawings take place. First, they chose half of the numbers N_1 at random and pay out \$1 to those number holders. They then randomly select half of N_1, call those numbers N_2, and pay out \$2. This processs is repeated until there is only one number left, N_n and this number wins the grand prize. The results are broadcast on TV, and you eagerly follow the numbers drawn at each stage. However, if your number is not randomly selected at some stage, you turn the TV off. Now, this kind of scenario mirrors **Flipping Coins** in the sense that the relevant chances for each C_i can be made the same for N_i at each point. In particular, before the numbers begin to be drawn, the chance that your ticket is the final N_n is just the chance that C_n lands tails, and this is precisely the chance that you will win the grand prize. But **No Skepticism** then says that you can know that you will turn the TV off before the final draw is made, i.e. you know that you will not win the lottery. But this runs counter to a widely shared intuition about the lottery paradox—one can perhaps justifiably believe that one's ticket will not win, but one cannot know it. If one shares this intuition in the lottery paradox one might find **No Skepticism** suspect. I'm sympathetic with this, but for the purposes of this paper I put this line of objection aside in order to focus on **KK**.

intuitively appealing, two, give independent reasons for why **KK** is true, and three, provide plausible principles governing knowledge and chance to replace **Weak Fair Coins**. [28] sketch a way a defender of **KK** might meet the first condition by showing that **Weak Fair Coins** is motivated by the following two principles:

> **Extremely Weak Fair Coins:** If a coin is fair and you know that it is about to be flipped, then for all you know it will land tails.

> **More is Better:** If I have observed, or been told, everything about a coin that you've observed or been told about it, then I can know everything about how it will land that you can know.

Consider two agents: S_1, who is in the other room and does not know the outcomes of the flips, and S_2 who is in the room watching the coins flip. Now, suppose that, for S_1, **Weak Fair Coins** is false. This means that there is some C_i such that C_i will happen to be flipped but S_1 knows that it will not land tails. Since S_2 is there observing the flips, it follows by **Extremely Weak Fair Coins** that right before C_i will be flipped, for all S_2 knows C_i will land tails. (For S_2, it's just a 50/50 chance.) But S_2 knows everything about the coin and the set up that S_1 knows (and more), and so by **More is Better**, S_2 must know that C_i will not land tails, a contradiction. A more general principle underlying **More is Better** is:

> **No Defeat:** If you know p at the beginning of the experiment, then you know p at the end of the experiment.

If **No Defeat** is true then no amount of extra information S_1 might receive from S_2 will defeat S_1's knowledge. The idea behind **No Defeat** is that if you really know p at the beginning of the experiment, then p is true, and hence there is no non-misleading defeater you could learn that could defeat your knowledge. For example, suppose you are watching a hand of poker and you have good reason to think that they are playing with a regular 52 card deck. Before the hand is played you know that no more than two people will end up with a pair of aces. You also know this after a hand is played. Now, suppose that three people get dealt pairs of aces. You then learn that the deck is in fact not a regular deck. This, however, is not a counterexample to **No Defeat**, because if the deck was irregular, then you did not know at the beginning of the hand that no more than two people will end up with a pair of aces. In other words, **No Defeat** is perfectly consistent with learning that you did not know, despite original appearances to the contrary. Rather, what **No Defeat** rules out are defeaters consistent with your original knowledge.

No Defeat is seemingly plausible, but there are reasons to doubt it on a normality theory. One has a default entitlement to believe that the world is normal. So at the beginning of **Flipping Coins** one has entitlement to think that conditions are normal, and so S_1 enjoys this entitlement. However, S_2, who has observed the outcomes of the flips, does not enjoy this entitlement, because S_2 has learned that conditions are not normal. Goodman and Sallow then argue that **More is Better** must be false–S_2 cannot know all the things S_1 knows because S_2 knows that conditions are not normal. Both **More is Better** and **Extremely Weak Fair Coins** are what explain the intuitive appeal to **Weak Fair Coins**. But on normal conditions theories, not only is **More is Better** false, normal conditions validate KK. And so, the argument goes, KK is saved by normal conditions theories in the light of **Flipping Coins**.[6]

I take somewhat of a detour in discussing **Flipping Coins**, but I think it is an interesting case for normality theories, and it is also influential in the literature.[7] The logic I propose in section 4 of this paper will be able to model counterexamples to **Weak Fair Coins** and **No Defeat**.

[6] I think much more can be said here about the **Flipping Coins** case. For example, one might grant that at the beginning of the experiment one knows that *current* conditions are normal, but if conditions turn out to be abnormal, i.e. many coins land heads, wouldn't one not have know conditions that *would* be normal? And isn't it that one's knowledge that conditions *will* be normal that has been defeated? Moreover, for a normality theorist like Smith, and on the flavor of the normality theory I sketched in the previous chapter, situations where 1 coin lands heads will be no more normal than situations where 100 coins land heads, which will even be no more normal than situations where 1000 coins land heads. This then motivates a rejection of **No Skepticism** along the lines I sketched in footnote 5. I want to put these worries aside because the question of *what* makes one world more normal than another, while important, is not answered by the logic of normality I discuss here.

[7] See in particular [16, 67, 77]

4.3 Evidence Logics

Let us now turn to developing our logic. Standard relational semantics for knowledge and belief encode a notion of evidence *implicitly*, as the range of worlds the agent considers possible. An agent considers a world possible just in case it is compatible with her current evidence, and a proposition is known just in case it is true in every such world. This is fine as far as it goes, but if we want to represent evidence *explicitly*, we must turn to richer structures. In particular, there are two epistemologically interesting things we cannot model in relational structures. (a) We cannot represent the basis on which an agent believes or knows something, and hence many of the traditional questions of what it takes for an agent to possess knowledge are put to the side. And (b), in relational structures we cannot explicitly talk about the finer structure of evidence modification, its dynamics, and how it affects knowledge and belief. Dynamic logics such as Public Anouncement Logic (PAL), for instance, can express how one's epistemic state changes when some proposition becomes common knowledge, but the mechanism used, i.e. world deletion, only implicitly represents the change of one's evidence. Therefore, if we want to explicitly model evidence and support, as we need for **Normic Support**, we must move to richer structures.

[86] (and further in [85]) develop such a richer set of tools using neighborhood models, where an agent's evidence is represented as a family $\mathcal{F}$ of subsets of worlds, where $\varphi \in \mathcal{F}$ when the agent has φ as evidence. [5] further develop evidence models using topological semantics. These approaches address (a) by defining belief and knowledge in terms of the agent's evidence—belief in Benthem and Pacuit as what is true no matter how one combines one's evidence, and knowledge in Baltag et al as possession of a factive justification. Benthem and Pacuit explore various dynamic evidence updates such as evidence addition, subtraction and modification, addressing (b).

These frameworks however make two substantial idealizations about evidence:

(1) Evidence need not be true and one's total evidence need not be consistent.

(2) φ is evidence for ψ only if every φ-world is a ψ-world

In Bentham and Pacuit (1) is borne out by the assumptions that the intersection $\bigcap \mathcal{F}$ may be non-empty, and that members of $\mathcal{F}$ need not contain the actual world. (2) is built into the semantics for the evidence and belief modalities. I take it that most, if not all epistemologists would find these assumptions objectionable.

Consider paradigmatic cases of evidence. For example—*that the bloody knife found in Professor Plum's vehicle*, or *thatBrown's fingerprints found on the doorknob*. These are the kind of things that can be used in court to establish the accused's guilt. We cannot say that the bloody knife is evidence of Brown's guilt if in fact there is no bloody knife. It's also not clear what to make of a situation where the total evidence brought to bear on some case is inconsistent. Two pieces of conflicting testimony need not be consistent, since Jones' testifying that Brown was at the scene of the crime is consistent with Smith's testifying that Brown was not at the scene of the crime. But if one's evidence in a case did consistent of inconsistent claims, like that a bloody knife was found in Brown's vehicle and a bloody knife was not found in Brown's vehicle, one could not use either as admissible evidence, and perhaps what's required is that one reevaluate and clear the inconsistency. In addition, Jones' testifying is evidence that Brown committed the crime, but there are possible situations where Jones is, for instance, trying to frame Brown, and so Brown is not guilty.

It seems clear that if we want to be able to represent ordinary epistemic scenarios our theoretical notion of evidence should also possess these features.It is natural to understand one's evidence as a part of one's reasons, so that one's evidence for p are one's reasons to believe p. But false propositions cannot play the kind of normative role distinctively played by one's reasons.[8] The falsity of (1) also follows from the widely defended view that E = K and that knowledge is factive.[9] I don't take any stances here on what conditions there are on evidence possession, I only claim that it is relatively uncontroversial that evidence is true, and also hence consistent. It is also the case that the evidence-for relation is (2) in all but the most uninteresting cases. While it's true that if one's evidence is that one *sees that* p, then one has factive evidence for p, it's not clear how much of our actual evidence is

[8] For a defense of this, see [42].

[9] See [93] and again [42].

this strong. And even if such evidence is required for knowledge, it is certainly not required for rational belief and other attitudes.[10] And therefore, models such as [6] where factive evidence is required for knowledge are idealizations.

These idealizations are far from harmless and restrict the kinds of epistemic scenarios our logic will be capable of modeling. The logic I will propose in the next section will rectify the situation. In particular, we will relax (2) by basing the evidential support relation on **Normic Support**: φ is evidence for ψ just in case *the most normal* φ worlds are ψ worlds. We can do this with a relation $\leq$ over worlds, where $w \leq w'$ means that w is at least as normal as w'. For example, the world where tweety is a bird and tweety flies is at least as normal as the world where tweety is a bird but does not fly. But not all normality comparisons are epistemically on par. For example, clearly the world where tweety is a bird and can prove Goldbach's Conjecture is much more abnormal than the world where tweety is a bird and cannot prove Goldbach's Conjecture. This corresponds with the fact that if all I know about tweety is that she is a bird, I am quite justified in believing she cannot prove Goldbach's Conjecture. So ranking worlds by their normality is not enough—we need a notion of being *much more* abnormal. We can denote this by the relation $\ll$.[11] **Normic Support** is then the following: φ is evidence for ψ just in case minimal φ worlds under the relation $\ll$ are ψ worlds, and w is a minimal φ world under $\ll$ iff there is no other φ world w' such that $w' \ll w$.

We will also construct a logic for knowledge and justification operators. Here is the idea. Agents have a body of evidence, E, which we will represent as a single proposition, i.e. as the set of worlds compatible with one's evidence. According to the normality theory, agents have a default justification for believing that the world is not abnormal. The proposition that the world is not abnormal is just the set $\{w \in W \mid \forall w' \in W, \;\; \neg(w' \ll w)\}$. And so the worlds $J_E(w)$ compatible with what the agent is justified in believing with evidence

[10] [61] and [48], for instance, defend the thesis that one's knowledge granting evidence must be entailing. But it's consistent with these views that *seeing that* p, while factive evidence for p, is non-factive evidence for some other proposition.

[11] We may also think of the relations $\leq$ and $\ll$ as generated by an underlying normality measure. Suppose $N(w)$ measures the degree to which w is normal, such that N is a probability function. Then $w \leq w'$ iff $N(w) \leq N(w')$ and $w \ll w'$ iff $N(w) \leq k \times N(w')$, where k is some threshold. This also suggests a view on the relation between beliefs and credences, where $N(w)$ is instead taken to be the agent's degree of belief in w. A similar view has been proposed by [41].

E at w is such that

$$J_E(w) \subseteq \{w' \in E \mid \forall w'' \in W, \quad \neg(w'' \ll w')\}.$$

While it is open to the normality theorist to claim that there are minimal worlds ruled out by other conditions on justification, I will take equality for granted so that we can focus on the contributing role of normal worlds. Notice that in these models justification is modally invariant in the sense that, holding E fixed, what you are in a position to justifiably believe does not depend on w.

As for knowledge, we want to capture the idea that all things considered, one knows less in an abnormal world than in a normal world. To know φ at w it must be that φ is true in all worlds compatible with your evidence that are at least as normal as w. Hence, the worlds compatible with what you know are

$$K_E(w) = \{v \in W \mid v \in J_E(w) \text{ or } v \leq w\}.$$

This embodies a kind of safety condition, where the neighborhood of nearby worlds around w depends on how normal w is—if w is abnormal than its neighborhood will be large.

4.4 Normal Evidence Logic

We are now in a position to develop the logic. The language for normal evidence logic, $\mathcal{NEL}$, is generated by the grammar

$$p \mid \neg\varphi \mid \varphi \wedge \psi \mid \varphi \rightharpoonup \psi \mid J\varphi \mid K\varphi$$

where $p \in \text{PROP}$ and $\varphi \in \mathcal{NEL}$. The interpretation of $\varphi \rightharpoonup \psi$ is "φ is evidential support for

ψ".[12] $K\,\varphi$ is interpreted as usual as knowledge. The intendended interpretation of $J\,\varphi$ will be 'one is justified in' such that if one were to believe φ on the basis of one's total evidence then one would justifiedly believe φ.

Definition 4.4.1. Let $M = \langle W, \leq, \ll, E, V\rangle$ be a normic evidence model, where W is a set of worlds, E is a function from W to $\mathcal{P}(W)$, associating with each $w \in W$ a proposition of W. I use $E(w)$ to refer to the agent's set of evidence in w. In what follows, however, I will restrict our attention to *uniform evidence models*, where $\forall w, w' : E(w) = E(w')$. This encodes the assumption that agents are not uncertain about their evidence. I leave the relaxation of this to future work. Lastly, V is a valuation function defined as usual.

I will take the relation $\leq$ to be a partial order (reflexive, transitive and anti-symmetric) over W. The reason why it is not a *total* order will be explained in the next section, but the idea is is that possibilities may be abnormal in various ways, and there might not be one standard by which all possibilities may be compared. The relation $\ll$ can be thought as saying "w_1 is *much more* normal than w_2". Thus interpreted there are two natural interaction conditions between $\ll$ and $\leq$:

(1) If $w_1 \ll w_2$, then $w_1 \leq w_2$.

(2) If $w_1 \leq w_2$, $w_2 \ll w_3$ and $w_3 \leq w_4$, then $w_1 \ll w_4$.

We also require that $\ll$ is well-founded, i.e. any subset of W has a $\ll$-least element. Note that normality rankings do not vary with the worlds. Later I will discuss what happens when this assumption is relaxed. In addition, we require that evidence never be contradictory, i.e. $E \neq \emptyset$. Otherwise both φ and $\neg\,\varphi$ would be justified.

We next need a definition that will make our statement of the semantics a bit easier:

Definition 4.4.2. *Truth sets.* Let $[\![\varphi]\!]_M = \{w \mid M, w \models \varphi\}$. We suppress the model where

[12] Note that strictly speaking this means 'if φ constitutes the total evidence, then ψ is justified'.

convenient. We also define

$$[\![\varphi]\!]_M^{\ll} = \{w \in [\![\varphi]\!] \mid \forall w' \in [\![\varphi]\!], \quad \neg(w' \ll w)\}$$

and, more generally, where $X \subseteq W$,

$$X_M^{\ll} = \{w \in X \mid \forall w' \in X, \quad \neg(w' \ll w)\}.$$

We can think of the set $X_M^{\ll}$ as the set of *least abnormal* worlds in X, or the $\ll$-minimal X worlds.

Definition 4.4.3. *Semantics.* Let $M = \langle W, E, \leq, \ll, V \rangle$ be a normal evidence model, and φ be a formula in $\mathcal{NEL}$. Truth for primitive propositions and connectives $\neg, \wedge, \vee, \supset$ are defined as usual.

- $M, w \models \varphi \rightharpoonup \psi$ iff $[\![\varphi]\!]_M^{\ll} \subseteq [\![\psi]\!]_M$
- $M, w \models J\varphi$ iff $E_M^{\ll} \subseteq [\![\varphi]\!]_M$
- $M, w \models K\varphi$ iff $\forall v \in E$ such that $v \in E_M^{\ll}$ or $v \leq w$, $M, v \models \varphi$

The evidence conditional is as expected.[13] It is easy to see that the semantics for J and K match those discussed in the last section. Note that if $E = [\![\varphi]\!]$, then $J\psi$ iff $\varphi \rightharpoonup \psi$, and so, in terms of the semantic definitions discussed in the last section, $\varphi \rightharpoonup \psi$ iff

[13]Another way of representing the normic conditional could be the following: define $[\![\varphi]\!] \ll [\![\psi]\!]$ iff every φ world is more much more normal than any ψ world. Then we could define $\varphi \rightharpoonup \psi$ iff $[\![\varphi \wedge \psi]\!] \ll [\![\varphi \wedge \neg\psi]\!]$, as, for instance, explored in [19] and elsewhere. This, however, would not allow us to model very natural evidential situations. For instance, given that the zoo is not notorious for short-changing visitors by cleverly disguising mules to be zebras, seeing what looks like a zebra at the zoo and it being a zebra is much more normal than seeing what looks like a zebra and it not being one. But much more *abnormal* than both situations is where the the zoo is secretly disguising their mules to be zebras, but I happen to see a zebra in the enclosure only because a zebra from a local exotic animal enthusiast escaped and somehow found its way into the enclosure. Representing this scenario would require that a world where I seem to see a zebra and it being a mule is less normal than a scenario where I seem to see a zebra and it is a real zebra, even though seeming to see a zebra is evidence that it is a zebra.

$J_{[\![\varphi]\!]}(w) \subseteq [\![\psi]\!]$. This shows that the evidence conditional is already implicit in our definitions. Let $\mathcal{F} = \langle W, \leq, \ll \rangle$ be an evidence frame. A model M is based on a frame $\mathcal{F}$ if $M = \langle \mathcal{F}, E, V \rangle$. A formula φ is *E-valid* if for every frame $\mathcal{F}$, φ is true in every E-world in every model M based on $\mathcal{F}$. We are principally interested in those formulas that are E-valid. This is simply a convenient way of requiring that one's evidence be true. Insofar as evidence must be true, it is unclear what $J\,\varphi$ would mean at a world where $w \notin E$. What purpose, then, does the wider set of worlds W play if we are only concerned with E-validity? First, it allows us to talk about the evidential relations between propositions that are not a part of the agent's evidence. And second, it allows us to model certain kinds of evidence updates. For example, define:

$$M^{+\varphi} = \langle W, \leq, \ll, E \cup [\![\varphi]\!]_M, V \rangle.$$

We can introduce a dynamic modality $[+\,\varphi]$ as follows:

- $M, w \models [+\,\varphi]\psi$ iff for some $u \in W$, $M, u \models \varphi$ implies $M^{+\varphi}, w \models \psi$.

This in effect this says that ψ is true after a weakening of one's evidence. I leave the exploration of these kinds of updates and their reduction axioms to future work, but without having a wider set of worlds in our models these updates are not possible.[14] Classical public announcements are also easily expressed. Define:

[14]We have defeasified the evidence-for relation. This will have interesting effects, for instance, on the dynamics studied in [86], which I will explore in future work. For example, classic public announcements are monotonic: $[!\,\varphi]\psi$ entails $[!\,\varphi \wedge \chi]\psi$. But this will not be the case if we have defeasible justification: there are models where $[!\,\varphi]J\psi$ and $\neg[!\,\varphi \wedge \chi]J\psi$. This suggests that the standard strategy of axiomatizing announcements by reduction axioms will not work, or will at least need to be supplemented in some manner. The same observation goes for the other dynamic evidence modalities, such as addition, subtraction, and upgrade. The most interesting application of defeasifying evidence logic will be that new evidence modalities yet to be studied will arise. Moreover, in addition to evidence updates, we can also study the dynamics of normality change. For example, let $[N\,\varphi]\psi$ mean that after learning that φ is normal, ψ is true. This work will also hook up more generally with the logical dynamics program championed by van Benthem. For a manifesto, see [84].

$$M^{!\varphi} = \langle W, \leq, \ll, E \cap [\![\varphi]\!]_M, V \rangle.$$

Accordingly we have:

- $M, w \models [!\,\varphi]\psi$ iff $M, w \models \varphi$ implies $M^{!\varphi}, w \models \psi$.

The evidential interpretation of $[!\,\varphi]$ here is that after the update φ is the strongest proposition compatible with your evidence. Therefore, $[!\,\varphi]\psi$ means that, after learning φ, ψ is true. Accordingly, I will call this evidence update.

Example. Consider the following model. Let $W = \{w_1, w_2, w_3\}$, $\leq$ be the relation $\{\langle w_1, w_2\rangle, \langle w_2, w_3\rangle\}$,[15] $\ll$ be the relation $\{\langle w_1, w_3\rangle\}$, and $[\![\varphi]\!] = \{w_1, w_2\}$. Suppose I have no evidence, so that $E = W$. No matter what the actual world is, $[\![J\,\varphi]\!] = W$, as $W^{\ll} = \{w_1, w_2\}$. If the actual world is among the most normal, then the agent will also know φ. But if the actual world is w_3, then the agent fails to know φ, because $w_3 \in E$ and $w_3 \leq w_3$, but $M, w_3 \not\models \varphi$. This is a simple model of the idea that if things are normal, then what you know is that they aren't extraordinary; if things aren't normal, you know less.

Example. We are able to state a simple model of **Flipping Coins**. Let $W = \{F_1, F_2, ..., F_{10000}\}$, where F_i is the world where the coins are flipped i times. Because of the setup of the case, F_i iff the ith coin landed heads. Let $\leq\, = \{\langle F_i, F_j\rangle \mid i < j\}$. Suppose m is the largest number such that 0.5^m is still a substantive chance (i.e. m is the measure of chance such that you will not be justified in believing that coin C_{m+1} will be flipped), and define $\ll\, = \{\langle F_i, F_j\rangle \mid j - i \geq m\}$. Suppose the agent has no evidence about the way the coins landed, and in fact the first coin landed heads. It follows that if $m = x$, then the most normal worlds compatible with your evidence, i.e. $[\![W]\!]^{\ll}$, are F_1, ..., F_x.

We can model loss of knowledge: at F_1 we have (abusing notation) $K\neg F_{10000}$, and

[15] Throughout this paper I omit reflexive pairs and transitive closures.

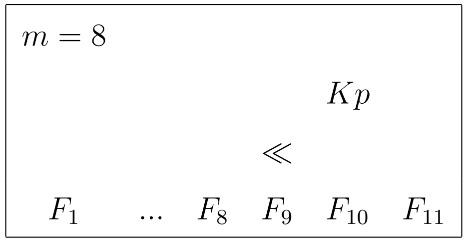

Figure 2: The counterexample to **Weak Fair Coins**. Here the proposition p is the set $\{F_1, ..., F_{10}\}$.

in fact $\neg K \neg F_i$ for each $i < m$. However, upon learning $\neg F_1$, ..., $\neg F_{10000-m}$, it follows that $\neg K \neg F_{10000}$. In terms of our evidence update operator: $[!(\neg F_1 \wedge ... \wedge \neg F_{10000-m})]\neg K \neg F_{10000}$. As one can see, our evidence update operator allows us to model any kind of evidence acquisition.

We can also model counterexamples to **Weak Fair Coins**. Suppose $m = 8$, and you have no evidence about the way the coins have landed. At F_{10}, i.e. when the 10th coin has landed heads, you know the proposition that the 11th coin will not be flipped. Why? Because this proposition is true at F_1 through F_{10}, as the 10th coin has landed heads, and these are all the worlds at least as normal as F_{10}. (See Fig 2.) But if you know that the 11th coin will not be flipped then you know that it will not land tails, and therefore this is a counterexample to **Weak Fair Coins**. In contrast, suppose $m = 12$ instead. Now one does *not* know the proposition that the 11th coin will not be flipped. Why? Because this proposition is false at F_{11}. This world, F_{11}, now matters because in order to know p one must know p in all the most normal worlds compatible with your evidence. Here, because your evidence is empty, this amounts to the claim that p must be true in all the worlds F_1 to F_{12}. (See Fig 3.)

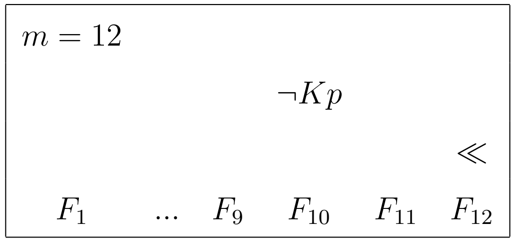

Figure 3: The case when $m = 12$. Here the proposition p is the set $\{F_1, ..., F_{10}\}$.

4.5 The Evidence Conditional

[28] do not explicitly formulate the evidence conditional when diagnosing **Flipping Coins**, but given what they say about their notion of justification, the evidence conditional is implicit. But by explicitly bringing such a conditional into our language we are able to show what properties and inferences it validates. For example, consider transitivity:[16]

$$\begin{array}{l} \varphi \rightharpoonup \psi \\ \psi \rightharpoonup \chi \\ \hline \varphi \rightharpoonup \chi \end{array}$$

It is clear that this is not valid on an ordinary notion of evidence. Suppose you have a device for measuring the color of walls, perhaps for the purposes of finding matching paint. That the wall is illuminated by strong overhead blue lighting is evidence that the device will claim that the wall is blue. Of course whether it does depends on how resistant the device is to trick lighting, but nevertheless you have good reason to think that the device will read blue. It is also the case that the device claiming that the wall is blue is evidence that the wall is blue—the most normal circumstances where the device claims the wall is blue the wall will be blue. But it does not follow that the wall being illuminated by strong overhead blue

[16] Understand these as valid formulas separated by material conditionals. I format them this way because it is easier to see their form.

lighting is evidence that the wall is blue. So the evidence-for relation cannot be in general transitive.

Fact 1. $(\varphi \rightharpoonup \psi) \supset ((\psi \rightharpoonup \chi) \supset (\varphi \rightharpoonup \chi))$ *is not valid on normic evidence models.*

Consider a variation on transitivity, sometimes called cumulative transitivity:

$$\varphi \rightharpoonup \psi$$
$$(\varphi \wedge \psi) \rightharpoonup \chi$$
$$\overline{\qquad}$$
$$\varphi \rightharpoonup \chi$$

The same situation is no longer a counterexample. We do not have evidence that the wall is blue if we know that despite the device reading blue the wall is illuminated by trick lighting.

Fact 2. $(\varphi \rightharpoonup \psi) \supset (((\varphi \wedge \psi) \rightharpoonup \chi) \supset (\varphi \rightharpoonup \chi))$ *is valid on normic evidence models.*

Next, consider monotonicity:

$$\varphi \rightharpoonup \psi$$
$$\overline{\qquad}$$
$$(\varphi \wedge \chi) \rightharpoonup \psi$$

It is easy to see that our conditional is non-monotonic, since the most normal φ-worlds need not be the most normal $\phi \wedge \chi$-worlds. Moreover we should expect that monotonicity fails, since evidence can sometimes be defeated: Jones' testimony that Brown was at the scene of the crime can be defeated if I come to learn that Jones has a desire to frame Brown. For similar reasons, we should expect that the following closure principle is not valid:

$J\,\varphi$

$\varphi \rightharpoonup \psi$

$J\psi$

The the jury knows and hence justifiably believes that Jones testified against Brown, knows that this is evidence for Brown's guilt, but also knows that Jones has a motive to frame Brown, and so does not believe that Brown is guilty. It is important remember here that $\varphi \rightharpoonup \psi$ means that if φ is one's *total evidence*, then φ supports ψ. In essence, the closure principle is not valid because the evidence that witnesses $J\,\varphi$ may be one which also includes a defeater for ψ.

It is well known that there are various weakenings of monotonicity, and one in particular, sometimes called *rational monotonicity*, has an interesting evidential interpretation:

$\varphi \rightharpoonup \psi$

$\neg((\varphi \wedge \chi) \rightharpoonup \psi)$

$\varphi \rightharpoonup \neg\chi$

Note that in our language the formula $\neg((\varphi \wedge \chi) \rightharpoonup \psi)$ says that χ is a defeater for one's φ-evidence for ψ. So rational monotonicity says that if χ is a defeater for one's φ-evidence for ψ, then one has φ-evidence against χ. In other words, if one has evidence for ψ then one has evidence against any possible defeaters for ψ. In the example just mentioned this means that if we have evidence that Brown was at the scene of the crime then we have evidence that Jones' testimony is not misleading. If we didn't have some (perhaps default) reason to think that Jones' testimony is not misleading, how could we end up justifiably believing that Brown was at the scene of the crime? The schema does have an air of plausibility. One might worry, however, that it leads to skepticism.[17] I have justification to believe that the earth is round, and it follows from rational monotonicity that I also have justification to dismiss any defeater for this belief. In particular I have justification to dismiss the proposition that I am

[17]Rational monotonicity is also discussed in chapter 3 of this dissertation.

dreaming, or I am a recently envattted brain, or that I am in a 3-d computer simulation, etc. Though none of these scenarios entail that the earth is not flat, if I were to learn any one of them my belief that the earth is round (among many others) would be defeated. Some will want to deny that we have justification to dismiss skeptical hypotheses, and hence think that rational monotonicity should be rejected as well.[18]

However, I don't think we should hold rational monotonicty hostage to our views on closure based skepticism. There are non-skeptical counterexamples. Imagine you're at the Mattress Factory (a modern art museum in Pittsburgh). You walk into a room with what looks like a red wall. You know that you are at Mattress Factory but have no reason to think anything tricky is going on with the lighting. Let φ be 'wall looks red', ψ be 'wall is red', and χ be 'There is some person somewhere, call him Bob, who would have told me that there's probably trick lighting in this exhibit had he walked into the room with me.' Lets assume Bob is Mattress Factory savvy—he's pretty reliable at detecting fake lighting art installations, though not perfect. Now, it seems right that φ justifies ψ, and also that χ is a defeater for φ. After all, if all the evidence you have is that the wall looks red, and someone walks in and tells you this is a trick lighting exhibit, you would think that you are being deceived. What, then, is the difference between Bob actually being there and knowing that there is someone like Bob who, had he walked in the exhibit with you, would have told you about the trick lighting? In both cases, it seems like your belief that the wall is red is undermined. Now, can φ justify $\neg\chi$? It's not obvious that it can. On basis of the wall looking red, how could it be that I'm justified in believing that there is no such museum patron? Perhaps on the basis of the wall looking red I am justified in dismissing possibilities such as that I am being deceived by trick lighting—under normal conditions, if a wall looks red then it is red, and I have no reason to suspect that conditions are not normal. But my evidence is silent on whether there's someone like Bob in, e.g. Austrialia, who, had he been in the room with me, would have told me that there's probably trick lighting. In other words, even if I am justified in dismissing the possibility that there is trick lighting, it does not follow that I am justified in dismissing the possibility that Bob could have walked into

[18]In particular I'm thinking of those, like [21] who think the correct response to closure based skepticism is to deny closure.

the room with me. Bob is not a perfect trick lighting detector, and so there are possibilities where there is no trick lighting but Bob does walk into the room with me.

Rational monotonicity is not valid:

Fact 3. $(\varphi \rightharpoonup \psi) \supset (\neg(\varphi \wedge \chi \rightharpoonup \psi) \supset (\varphi \rightharpoonup \neg\chi))$ *is not valid on normic evidence models.*

The counterexample is illuminating:

Proof. Let $W = \{w_1, w_2, w_3\}$, $\ll$ be the relation $\{\langle w_1, w_2\rangle\}$, $[\![\varphi]\!] = \{w_1, w_2, w_3\}$, $[\![\psi]\!] = \{w_1, w_3\}$, and $[\![\chi]\!] = \{w_2, w_3\}$. Notice that $[\![\varphi]\!]^{\ll} = [\![\psi]\!]$ and $[\![\varphi \wedge \chi]\!]^{\ll} \cap [\![\neg\psi]\!] = \{w_2\}$, which shows that the antecedent conditions are true. However, it is not the case that $[\![\varphi]\!]^{\ll} \subseteq [\![\neg\chi]\!]$, since $[\![\varphi]\!]^{\ll} \cap [\![\chi]\!] = \{w_3\}$. □

We've been assuming so far that $\ll$ is not a total order (otherwise $\leq$ would be total), and hence there may be some worlds w_1 and w_2 such that neither $w_1 \ll w_2$ nor $w_2 \ll w_1$. This is required for Fact 3.[19] One way of explaining why seeming to see a red wall is not evidence that Bob is not around the corner is that there is no normal connection between seeing the color of a wall and there being people like Bob. Granted, if I learn that someone like Bob is there with me, then my evidence is defeated, but his possible existence is not normically related to the evidence I have. We can formalize this idea in the following way:

> χ is a *total defeater* for φ's justification for ψ iff χ is a defeater and $[\![\varphi \wedge \psi \wedge \chi]\!] \cap [\![\varphi]\!]^{\ll} = \emptyset$.
>
> χ is a *weak defeater* for φ's justification for ψ iff χ is a defeater but not total.

[19] To be more precise, the fact that $\ll$ is a non-total order is not sufficient for the counterexample, though it is necessary. What is sufficient is that some set is what I call minimal-unconnected. A set X is minimal-unconnected if there is some $w \in X^{\ll}$ such that there exists a distinct $w' \in X^{\ll}$ such that for all w'' where $w' \ll w''$, it is not the case that $w \ll w''$. This says that the minimal worlds in X are minimal for, as it were, different reasons. Some subset of $X^{\ll}$ is minimal because they are the most normal compared to one group of worlds, while some other subset of $X^{\ll}$ is minimal because they are most normal compared to another, distinct group of worlds. In the Fact 3, w_3 witnesses the minimal-unconnectedness of $[\![\varphi]\!]$, as $w_1 \ll w_2$ but $w_3 \not\ll w_2$

If χ is a total defeater, then $(\varphi \rightharpoonup \psi) \supset (\neg(\varphi \wedge \chi \rightharpoonup \psi) \supset (\varphi \rightharpoonup \neg\chi))$ is valid, but if χ is a weak defeater it is not valid.[20] Intuitively, skeptical hypotheses are total defeaters—any world where I am a recently envatted brain but nevertheless have hands is not among the most normal worlds where I seem to see that I have hands—and so, as I have been presenting things, skeptical defeaters are not reasons for rejecting rational monotonicity. How does this distinction between total and weak defeaters fit with the well known taxonomy of rebutting defeaters, undermining defeaters, collective defeat, etc.? Rebutting defeaters are not in general total. For example, conflicting testimony is a weak defeater just in case it's not the case that in the most normal worlds compatible with Jones' testimony that Smith is the murder there is no other conflicting testimony. Whether or not this is true seems to me to depend on the particular case. If Jones is a highly reliable, expert FBI agent (or someone who murders anyone else who might provide conflicting testimony) then perhaps a conflicting report would be total. But I see no reason to think this would be true in general. Likewise, whether an undermining defeater is total depends on the relevant normality relations.[21]

[76] accepts a rational monotonicity principle when developing his own evidence conditional. This shows that our semantics is weaker, and allows us to model various kinds of defeat.[22] Another variant on monotonicity is cautious monotonicity:

---

[20]Notice that $[\![\varphi \wedge \psi \wedge \chi]\!] \cap [\![\varphi]\!]^{\ll} = \{w_3\}$ in our counterexample.

[21]This suggests that in addition to the well known distinctions we can also characterize defeaters by their *logical strength*. There are intermediate principles between rational monotonicity and full monotonicity, and we can characterize the strength of the defeat in terms of the logical strength of the variant principles they validate. I hope to explore this in future work.

[22]Smith works with Lewis style sphere models. Given some sphere system $\mathcal{S}$, we can define the relation $w_1 \ll w_2$ iff $w_1 \in S_1$ where $S_1 \subset S_2$ and $w_2 \in S_2$. In other words, for each sphere there is an 'outer layer', where a world is in the outer layer just in case it is not contained in any proper sub-sphere. Worlds in each outer layer are $\ll$-incomparable, but are less normal than all the same worlds. In general $\ll$ will satisfy: if $w_1 \not\ll w_2$ and $w_2 \not\ll w_1$, then $\forall w$, $w \ll w_1$ iff $w \ll w_2$. This rules out the minimal-unconnected sets I mentioned in footnote 19. Notice that this fails in the countermodel for rational monotonicity. w_3 is incomparable to w_2 but $w_1 \not\ll w_3$. In fact, rational monotonicity is built into sphere models, and so it is an advantage to our framework that the principle is a choice, and we can show under what conditions rational monotonicity is applicable.

$\varphi \rightharpoonup \psi$

$\varphi \rightharpoonup \chi$

——

$(\varphi \wedge \chi) \rightharpoonup \psi$

Our counterexample to rational monotonicity is not a counterexample to cautious monotonicity, since φ was not evidence for χ.

Fact 4. $(\varphi \rightharpoonup \psi) \supset ((\varphi \rightharpoonup \chi) \supset (\varphi \wedge \chi) \rightharpoonup \chi)$ *is valid on normic evidence models.*

Lastly, consider the following, which I will call *or-weakening*:

$\varphi \rightharpoonup \chi$

$\psi \rightharpoonup \chi$

——

$(\varphi \vee \psi) \rightharpoonup \chi$

On the face of it this principle appears valid. Suppose your total evidence is that the murder was committed with either a knife or a lead pipe. You know that if the murder was committed with a knife, then there is good reason to think that Jones is the murder, and likewise, if the murder was committed with a lead pipe, then there is also good reason to think that Jones is the murder (say, he's famously known to kill with both). Hence, it seems clear, either way you have good reason to think Jones is the murder if you know that it was committed either with the knife or lead pipe. If or-weakening is not valid, then it's unclear why this kind of reasoning would appear unproblematic.

However, this principle raises an issue unique to the normal conditions framework. Suppose φ is an K-abnormal proposition, where φ is K-abnormal just in case $K\varphi \vee K\neg\varphi$ is abnormal. Clearly the proposition that Jones is the murder is not K-abnormal in the usual circumstances. But consider something like the proposition that you are not recently envatted. It would certainly be abnormal for you to know that you are recently envatted. It's also not incoherent to think that knowing that you are *not* envatted is also abnormal.

Those who deny that we can know the denial of skeptical hypotheses would be attracted to this kind of view.[23]

Assuming that agents know their evidence, if φ is K-abnormal, then the worlds where the agent has evidence φ and the worlds where the agent has evidence $\neg\varphi$ will be much less normal than those worlds where the agent has evidence $\varphi \vee \neg\varphi$. i.e. no evidence. And so both φ and $\neg\varphi$ will support the proposition that the situation is abnormal (in whatever way that may be), while $\varphi \vee \neg\varphi$ will not. And so, this looks like a counterexample to or-weakening. However, or-weakening is valid on normic evidence models. The reason is the following. Take some abnormal proposition α such that $\neg(\varphi \vee \neg\varphi) \rightharpoonup \alpha$.[24] If $\varphi \rightharpoonup \alpha$ then φ must be abnormal because α, an abnormal proposition, must be true in all the most normal φ worlds. But then $\neg\varphi$ must contain some normal worlds, in which case it cannot be the case that $\varphi \rightharpoonup \alpha$, because in those normal worlds α is false.

The problem is that our models cannot represent K-abnormal propositions, i.e. propositions where obtaining φ as evidence is abnormal and obtaining ψ as evidence is abnormal. There is, however, a way of generalizing our models so that this is possible. We've restricted ourselves to *uniform* normic evidence models, where the normality relations do not depend on the world. However, if normality relations did depend on the world, then we could model situations where situations where one obtains φ as evidence are abnormal and situations where one obtains ψ as evidence are abnormal, as these would be different worlds, and at these different worlds the normality rankings would be different.

In the next section, I will discuss which logical properties this uniformity assumption encodes and what we would lose when we lift the restriction.

4.5.1 Iterating the Evidence Conditional

Is the uniformity constraint we have imposed epistemically warranted? I think the best way to approach this question is to understand the effects the constraint has on the logic.

[23] If you do not like the envatted brain case, you can substitute any unknowable or improbably known proposition you like.

[24] By abnormal proposition I just mean one where for every $w \in \alpha$ there is some w' such that $w' \ll w$.

First, it introduces the idealization that agents base their beliefs perfectly on the evidence:

$$\frac{\varphi \rightharpoonup \psi}{K(\varphi \rightharpoonup \psi)} \qquad \frac{K(\varphi \rightharpoonup \psi)}{K\varphi \rightharpoonup K\psi}$$

If $\varphi \rightharpoonup \psi$ is true then it is true everywhere, and hence the agent knows it. It is also clear that knowledge distributes over $\rightharpoonup$. If $K(\varphi \rightharpoonup \psi)$ then $\varphi \rightharpoonup \psi$ is true, and since the most normal worlds where $K\varphi$ are the most normal φ worlds, it follows that $K\varphi \rightharpoonup K\psi$. Note, however, that the following is *not* valid:

$$\frac{K(\varphi \rightharpoonup \psi)}{K\varphi \supset K\psi}$$

The agent can know φ but also know a defeater for ψ, in which case their total evidence is not φ. This is valid, however, in models where $E = [\![\varphi]\!]$.

We should expect that the uniformity constraint has the most interesting consequences for iterated evidence conditionals. Let the phrase w-normal mean normal according to the normality rankings provided by w. Iterated conditionals like $\alpha \rightharpoonup (\beta \rightharpoonup \gamma)$ will be true at w iff $\beta \rightharpoonup \gamma$ is true in all the most w-normal α worlds. But once we lift the uniformity constraint, the most normal β worlds might differ in their normality rankings compared to the most normal α worlds. So, we should expect a loss of validities for iterated conditionals once we lift the uniformity constraint.

Let us thus examine validities for iterated evidence conditionals in uniform models. Interestingly, the principle of *defeasible* modus ponens is valid:

$$(\varphi \wedge (\varphi \rightharpoonup \psi)) \rightharpoonup \psi$$

This is because $\varphi \rightharpoonup \psi$ is either true everywhere or nowhere—if it is true then the most normal worlds where $(\varphi \wedge (\varphi \rightharpoonup \psi))$ is true is just the most normal worlds where φ is true, and by assumption they are ψ worlds. If $\varphi \rightharpoonup \psi$ is false then the antecedent is empty and so $(\varphi \wedge (\varphi \rightharpoonup \psi)) \rightharpoonup \psi$ is trivially true. This principle strikes me as correct: that the thermometer is reliable and it reports that it is below freezing is evidence that it is below freezing. Just like nested conditionals in natural language, it hard to parse nested evidence conditionals. But if any such iterations are true, defeasible modus ponens is at least among them. We then at least get defeasible modus ponens to come out valid in uniform models.

However, there are iterated conditionals that are not as desirable. Unsurprisingly, our uniformity constraint imbues the evidence conditional with the following properties:

$$\text{(I)}\quad \frac{\varphi \rightharpoonup \psi}{\delta \rightharpoonup (\varphi \rightharpoonup \psi)} \qquad\qquad \text{(II)}\quad \frac{\neg(\varphi \rightharpoonup \psi)}{\delta \rightharpoonup \neg(\varphi \rightharpoonup \psi)}$$

If $\varphi \rightharpoonup \psi$ is true (false) at some point in a model then it is true at every (no) point in the model. That Jones testifies that Smith is the murder is evidence as such, but that Jones is a liar is not evidence that Jones' testimony is evidence that Smith is the murder. So one might think that in general these are not valid.

A normality theorist might defend their validity in uniform models, and hence claim that they are ultimately not committed to them, by appealing to the idea that they are an idealization on par with others already made. For instance, we made the assumption that our models are evidentially uniform. This is an idealization in the sense that real agents are sometimes uncertain about their evidence. But at the level of rationality we are modeling this idealization is fruitful. The same may be said for logical omniscience. Real agents do not know all propositional tautologies, but insofar as we are modeling a kind of ideal rationality logical omniscience is to be expected. Perhaps the uniform normality assumption can be justified in the same way. With the uniform normality assumption we are modeling a kind of first-order evidential rationality, where we set aside the way agents may reason about evidential relations.

Define a first-degree formula to be a formula that is a truth-functional combination of purely propositional formulas and/or formulas of the form $\varphi \rightharpoonup \psi$ for propositional φ and ψ. n-degree formulas are defined as usual by induction. The following lemma shows that uniform models really do model only first-order evidential rationality, in the sense that every n-degree formula for $n > 1$ is equivalent to a first-degree formula.

Lemma 1. *Every n-degree formula of $\mathcal{NEL}$ is reducible to a first-degree formula modulo equivalence over uniform normal evidence models.*

Moreover, the following axiom system **EL** is a sound and complete axiomatization of $\mathcal{NEL}$, the proof of which can be found in the appendix.

A1	All propositional tautologies
A2	$\vdash \varphi \rightharpoonup \varphi$
A3	$\vdash ((\varphi \rightharpoonup \psi) \wedge ((\varphi \wedge \psi) \rightharpoonup \chi)) \supset (\varphi \rightharpoonup \chi)$
A4	$\vdash (\varphi \rightharpoonup (\psi \wedge \chi)) \supset (\varphi \rightharpoonup \psi)$
A5	$\vdash (\varphi \rightharpoonup \psi) \wedge (\varphi \rightharpoonup \chi) \supset ((\varphi \wedge \psi) \rightharpoonup \chi)$
A6	$\vdash (\varphi \rightharpoonup \chi) \wedge (\psi \rightharpoonup \chi) \supset ((\varphi \vee \psi) \rightharpoonup \chi)$
A7	If φ is propositionally consistent, then $\vdash \neg(\varphi \rightharpoonup \neg \varphi)$
A8	$\vdash \varphi_1 \equiv \varphi_n$, where φ_1 is a first-degree translation of some n-degree formula φ_n
R1	From $\vdash \varphi$ and $\vdash \varphi \supset \psi$, infer $\vdash \psi$
R2	If $\vdash \varphi \equiv \psi$, then from $\vdash \alpha$ infer $\vdash \alpha[\varphi \,\vert\, \psi]$, where $\alpha[\varphi \,\vert\, \psi]$ is the formula where any number of subformulas φ are replaced by ψ.

Theorem 1. ***EL** is a sound and complete axiomatization with respect to uniform normal evidence models for formulas of the language $\mathcal{NEL}$.*

Theorem 1 fully characterizes the evidence conditional over uniform models. But it is also clear that it characterizes first-order validities in non-uniform models. Let $M_i = \langle W, \leq_w, \ll_w, E, V\rangle$, where $\leq_w$ is an indexed set of $\leq$ relations, and adjust the semantics accordingly. Further, suppose no other constraints are imposed on $\leq_w$, e.g. that w is minimal under $\leq_w$, etc.

Theorem 2. *If φ is a first-degree formula, then φ is E-valid on M iff φ is E-valid on M_i.*

Let us take stock. In discussing or-weakening in the last section, we saw that our uniformity constraint restricted our ability to model K-abnormal propositions.[25] In this section we have seen that uniform models characterize the first-degree evidence conditionals. This may lead us to think that uniform models are fine if all we care about is first-degree reasoning. However, in the next section I will argue that, one, we ought to care about higher-degree reasoning, and two, the **KK** principle is only valid in virtue of uniformity. If, as I have suggested in the first section of this paper, normality theories are to vindicate **KK**, then we must justify the uniformity constraint. I will argue that there are good reasons to reject it, and hence I argue that, despite what has been claimed in the literature, the normality theory does not vindicate **KK**.

4.6 Normality and KK

Let us now return to the **KK** principle. As discussed in section 2 of this paper, normality accounts are interesting because they provide a natural model where the **KK** principle holds. As advertised, $K\varphi \supset KK\varphi$ is E-valid in uniform normal evidence models. Suppose $M, w \models K\varphi$. There are two cases: first, suppose v is such that $v \in E^{\ll}$. Since $K\varphi$ entails $J\varphi$, it follows that every $E^{\ll} \subseteq [\![\varphi]\!]$, and since for any $v' \in E$ if $v' \leq v$ then $v' \in E^{\ll}$, it follows that $M, v \models K\varphi$. Second, suppose $v \in E$ and $v \leq w$, and suppose $v' \leq v$. Since $M, w \models K\varphi$, and $\leq$ is transitive, $M, v' \models \varphi$, and hence $M, v \models K\varphi$. This establishes that $M, w \models KK\varphi$.[26] The normal knowledge model just sketched is then an attempted solution to the **Flipping Coins** puzzle—the model provides a counterexample to **Weak Fair Coins**

[25]However, indexing the normality relations to worlds would not invalidate or-weakening, as Theorem 2 shows. To model K-abnormal propositions we can index normality rankings to propositions such that $[\![\varphi \vee \psi]\!]^{\ll} \neq [\![\varphi]\!]^{\ll} \cup [\![\psi]\!]^{\ll}$

[26]In fact K has the logic of **S4** and J has the logic of **KD45**, and in the combined logic J is definable from K by the formula $J\varphi \leftrightarrow \neg K \neg K\varphi$, resulting in the logic of **S4.2**. Proofs of this are beyond the scope of the current paper, but in other work I show that there are natural ways of blocking the reduction by providing an alternative semantics which makes $J\varphi \supset JK\varphi$ false.

and explains how knowledge may be defeated, and also predicts **KK**.

However, if we impose no further constraints on the normality relations, **KK** is not valid over non-uniform models. What's normal according to the most normal worlds compatible with your evidence in w might vary wildly, and so while φ might be normally true according to w, it might not be normally true according to worlds at least as normal as w.[27]

So what reason is there to impose the constraint that normality relations are uniform? **KK** depends on this, and so normality theorists who would like a model which vindicates **KK** need to have some reason to impose it. Here is perhaps one motivation for the constraint. One might think that some properties of hypotheses are epistemically privileged independently of what the world is like. For example, one has more justification to believe simpler hypotheses, or hypotheses that offer the best explanation, and so forth. The normality theorist could say that the normality relations encode these epistemic virtues; perception, for example, is normally reliable because a world where one perceives that p and p is true is simpler than one where one perceives that p but one is deceived.

This would, however, be an unhelpful way to for the normality theorist to defend the uniformity constraint. If all they mean by a situation being normal is that simpler hypotheses are true at the situation, etc., then it's unclear what the normality theory has to offer over and above theories of justification already defended in the literature. It would be unclear why we have to talk about normality at all. According to an admittedly simplistic picture of bayesian epistemology, what's propositionally justified by some evidence E is just those propositions p such that $P(p \mid E) > P(p)$, where P is a rational prior. The prior is taken to be a description of the intrinsic plausibility of hypotheses prior to investigation, according to simplicity, explanatory power, and so on.[28] A normality theorist, like [76], will want to distance themselves from this kind of bayesian account, but if normality just encodes the intrinsic plausibility, then it's unclear what grounds they have for rejection.

Moreover, there is at least some intuitive pull to the idea that what's normal depends on the circumstances. Consider the following scenario: you're at the zoo, and your friend points to an animal inside an enclosure and asks what kind of animal it is. Suppose your friend has

[27]Of course, there might be other constraints on non-uniform models that secure **KK**. I look into this at the end of the paper.

[28]For exmaple, see [93, 212].

never seen any exotic animals. You look at it, see the characteristic stripes, note the sign that reports that it is a zebra exhibit, and respond that they're looking at a zebra. Clearly, you know it's a zebra. The normality theory provides a natural explanation for this kind of case: the most normal worlds compatible with you seeing the stripes and the sign, and having no other reason to expect funny business, are worlds where the animal you're looking at is in fact a zebra. But! Breaking news! The *New York Times* has uncovered a massive zoo conspiracy. It turns out that most zoos in the US get their zebras from Zebra Corp, and Zebra Corp has been been committing fraud by supplying zoos with cleverly disguised mules that are, from the outside, indistinguishable from zebras, even by experts. As it turns out, this is much cheaper than sourcing from the wild or animal refuges. In a situation like this, animals in zebra enclosures are *not normally* zebras, though, presumably, in the actual world they normally are zebras. If your friend brought up the *New York Times* article, you would no longer be in a position to justifiably believe that the animal in front of you is a zebra.

How should the normality theorist think about this sort of example? In particular, how would they understand the evidential defeat of the *New York Times* article? If they say that, in the no Zebra Corp scenario, you know there is a zebra in the enclosure because the most normal worlds where you see the stripes, etc., are worlds where there is a zebra, then it looks quite natural to say that the article undermines your justification for believing there is a zebra because you now have justification to believe that things aren't normally the way you thought they were. But this only makes sense if what's normal depends on the world. In the world where the animals in the enclosure are normally zebras, you are normically justified in believing that the animal before you is a zebra. But in the case in which they are not normally zebras, you aren't. Undermining evidence like the *New York Times* article gives you evidence that you are in the second scenario, but this is only intelligible if in the two situations what's normal is different.

So on the face of things variance in what's normal is quite congenial to the normality theorist. If the normality framework explored here provides a model of **KK**, then it does so on the back of the uniform normality assumption. I've argued in this section that this assumption is not epistemically inert. And so, without an explanation for and defense of the uniformity assumption, the normality theory does not provide an independently motivated model of **KK**, and hence cannot serve as a solution to the **Flipping Coins** puzzle. And on the face of it, the situation is a bit puzzling. In the two cases just sketched I have not even mentioned **KK**, and it is not even clear how they relate to the principle. Nevertheless, these cases and **KK** are connected in the theory, via the uniform normality assumption. The worry is that **KK** is valid in normality theories only because of a simplifying constraint. Once this constraint is lifted, as I have argued here it should be, then **KK** is no longer valid. However, this may be too quick. In the next section I will explore ways that **KK** may be recovered once we lift the constraint.

4.6.1 Can We Recover KK?

Suppose we do not impose the uniform normality constraint, and instead have an indexed family of relations $\leq_w$ and $\ll_w$ for each world. As I pointed out in the last section, without any other constraints, **KK** is lost. I will now very briefly consider constraints on $\leq_w$ and $\ll_w$ that validate **KK**. I will not make any definitive claims about these constraints—more work needs to be done to understand them better. The discussion here will show, however, that the burden is on the normality theorist to defend a constraint that saves **KK**.

Consider the following:

> **Uniform Evidential Minimality (UEM):** For all $w, u \in W$, if $u \in E^{\ll_w}$ then $E^{\ll_u} \subseteq E^{\ll_w}$.

UEM is a necessary and sufficient condition for the validity of **JJ** over indexed normality models.[29] Moreover, **JJ** is a necessary condition for **KK**, and so any constraints on $\leq_w$ and $\ll_w$ that entail **KK** must also entail **UEM**.

But **UEM** gives up a component of the normality theory that some might want to keep—externalism. According to the normality theory, your belief in p is justified just in case your belief is based on a process that normally yields a true belief in p. Whether or not

[29] If **UEM** is true then if φ is true throughout $E^{\ll_w}$ then φ will be true throughout any $E^{\ll_u}$ where $u \in E^{\ll_w}$. Conversely, if there is some $v \in E^{\ll_u}$ such that $v \notin E^{\ll_w}$ for $u \in E^{\ll_w}$, then $JJ\,\varphi$ will be false at w in a model where φ is false at v.

this process is normally reliable depends on the world. In our semantics, this is reflected in the normality orderings. You're justified in believing p based on evidence E just in case in all the most normal worlds compatible with E, p is true, i.e. given E, p is normally true. But it follows from **UEM** that normality relations are a function of the agent's evidence, and so they can no longer be interpreted *ontically*, as relations in the world. Suppose an agent has a body of evidence E, and the normality relations N are such that they satisfy **UEM**. If the agent learns p then, assuming consistency with E, her new evidence is $E \wedge p$. But N need not satisfy **UEM** with respect to $E \wedge p$. So either the normality relations in the model update when an agent gets new evidence, in which case we give up externalism, *or* there is an implicit quantifier over any possible evidence an agent might have, in which case **UEM** becomes:

> **Quantified Uniform Minimality (QUM):** For all $w, u \in W$, and for all $S \subseteq W$, if $u \in S^{\ll_w}$ then $S^{\ll_u} \subseteq S^{\ll_w}$.

While this does not entail full uniformity, it is quite strong. It is unclear what kind of argument can be given by normality theorists for the principle if the normality relations are interpreted ontically. Moreover, even if normal evidence models satisfy **QUM**, you will still run into issues representing the examples in the last section—formulas like (1) cannot be true in such models. I do not purport to provide here a knock-down argument to the effect that **QUM** is false, and perhaps there is some way of thinking about normality rankings that makes **QUM** plausible. But normality theories on offer do not obviously provide motivation for it, and I do not see any such motivation as forthcoming.

So, the remaining option is that the normality relations are understood *epistemically*, as what the normality relations appear to be, based on your evidence. If the agent has introspective access to their evidence, then this potentially internalizes the normality relations. But even if the agent does not, what's normal must depend on the agent. Suppose we are modeling multi-agent scenarios, where different agents have different bodies of evidence. It follows from **UEM** that we will generally require not just one family of relations $\leq_w$ and $\ll_w$, but a family for each agent, since not all agent's bodies of evidence will satisfy **UEM** according to a single family. This all runs counter, however, to our ordinary understanding of normality—fixing the context, if tweety normally flies for me, he normally flies for you.[30]

Moreover, how the normality relations depend on one's total evidence would have to be explained, and it looks like any explanation must to appeal to something besides normality. In this paper I have not said what it means for one situation to be more normal than another, and its clear that an answer to this must be forthcoming from the normality theorist. But a normality theorist who also accepts **UEM** must in addition explain the relationship between one's evidence and normality.

Moreover, even if **UEM** is true, it is not enough to fully yield **KK**, as it says nothing about $\leq$. One natural way to do so is with the following:

Downward Preservation (DP): For all $w, u \in W$, if $u \leq_w w$ then $u \leq_u w$

This is quite a strong requirement, ruling out many possible orderings. The more abnormal a world is according to its own ranking, the more restrictive the condition becomes. If $\leq$ is interpreted ontically, then it is unclear why **DP** would be true in general. As for the epistemic interpretation of $\leq$, the same remarks about **UEM** will apply.

So, if one rejects the uniformity constraint, it seems like one must either lose **KK** or give up on externalism, one of the theory's most distinctive features. There would no longer be the promise of an externalist account of **KK**, and I suspect the theory might lose some of its appeal as a result. The burden is on the normality theorist to show how **KK** can be validated in normal evidence models.

[30] It is not inconsistent with the view sketched here that normality has a contextual aspect. Perhaps each model could represent a different context. In an everyday conversational context birds normally fly. When you mention that a bird was outside your window I'm going to assume it was not flightless. But for a room full of ornithologists perhaps it is not true that birds normally fly. This is all fine, but it would be implausible for contextualism to be a the grounds of a defense of **KK**.

4.7 Conclusion

In this paper I have proposed an evidence based logic of normality and discussed its features. This logic differs from other evidence based logics in the literature, e.g. [2, 6, 4, 5, 86, 85, 10], in that the evidence-for relation is monotonic. The use of normality orderings here may be incorporated into the these evidence logics. In particular, normality orderings can easily be built into the neighborhood models put forth by [86, 85]. This will have interesting implications for the dynamic modalities they discuss, which I will explore in future work.

I also showed that the uniformity constraint, which is currently assumed in the literature, validates **KK**, but there are reasons why a normality theorist will want to give up the constraint, especially if they want to model higher-order defeat. I think my discussion has shown that normality theorists should pay more attention to the logic of normality—not only are interesting epistemological questions raised concerning the structure and status of normality relations, but the defense of principles like **KK** may be more complicated than first appears.

4.8 Proofs

4.8.1 Axiomitization of the Evidence Conditional

Fact 1. *The evidence conditional is not transitive:* $(\varphi \rightharpoonup \psi) \supset ((\psi \rightharpoonup \chi) \supset (\varphi \rightharpoonup \chi))$ *is not valid on normic evidence models.*

Proof. Let φ = 'The wall is illuminated by trick blue lighting', ψ = 'The device reads 'blue'', and χ = 'The wall is blue'. Let $W = \{w_1, w_2\}$, $\ll$ be the relation $\{\langle w_1, w_2\rangle\}$, $[\![\varphi]\!] = \{w_2\}$, $[\![\psi]\!] = \{w_1, w_2\}$, and $[\![\chi]\!] = \{w_1\}$. Notice that $[\![\varphi]\!]^{\ll}_M \subseteq [\![\psi]\!]$ and $[\![\psi]\!]^{\ll}_M \subseteq [\![\chi]\!]$ but $[\![\varphi]\!]^{\ll}_M \not\subseteq [\![\chi]\!]$. $\square$

Fact 2. *The evidence conditional is cumulatively transitive:* $(\varphi \rightharpoonup \psi) \supset (((\varphi \wedge \psi) \rightharpoonup \chi) \supset$

$(\varphi \rightharpoonup \chi))$ *is valid on normic evidence models.*

Proof. Suppose $[\![\varphi]\!]^{\ll} \subseteq [\![\psi]\!]$ and $[\![\varphi \wedge \psi]\!]^{\ll} \subseteq [\![\chi]\!]$. Let $w \in [\![\varphi]\!]^{\ll}$. We then know $w \in [\![\varphi \wedge \psi]\!]$. Suppose for contradiction that $w \notin [\![\varphi \wedge \psi]\!]^{\ll}$, i.e. $\exists w'$ such that $w' \ll w$ and $w' \in [\![\varphi \wedge \psi]\!]$. But then $w' \in [\![\varphi]\!]$ which contradicts our assumption that $w \in [\![\varphi]\!]^{\ll}$. □

Fact 3. *The evidence conditional is not rationally monotonic:* $(\varphi \rightharpoonup \psi) \supset (\neg(\varphi \wedge \chi \rightharpoonup \psi) \supset (\varphi \rightharpoonup \neg\chi))$ *is not valid on normic evidence models.*

Proof. Let $W = \{w_1, w_2, w_3\}$, $\ll$ be the relation $\{\langle w_1, w_2\rangle\}$, $[\![\varphi]\!] = \{w_1, w_2, w_3\}$, $[\![\psi]\!] = \{w_1, w_3\}$, and $[\![\chi]\!] = \{w_2, w_3\}$. Notice that $[\![\varphi]\!]^{\ll}_M \subseteq [\![\psi]\!]$ and $[\![\varphi \wedge \chi]\!]^{\ll} \cap [\![\neg\psi]\!] = \{w_2\}$, which shows that the antecedent conditions are true. However, it is not the case that $[\![\varphi]\!]^{\ll} \subseteq [\![\neg\chi]\!]$, since $[\![\varphi]\!]^{\ll} \cap [\![\chi]\!] = \{w_3\}$. □

Fact 4. *The evidence conditional is cautiously monotonic:* $(\varphi \rightharpoonup \psi) \supset ((\varphi \rightharpoonup \chi) \supset (\varphi \wedge \chi) \rightharpoonup \psi)$ *is valid on normic evidence models.*

Proof. Assume that $[\![\varphi]\!]^{\ll} \subseteq [\![\psi]\!]$ and $[\![\varphi]\!]^{\ll} \subseteq [\![\chi]\!]$. Suppose that $x \in [\![\varphi \wedge \chi]\!]^{\ll}$. Suppose for contradiction that $x \notin [\![\varphi]\!]^{\ll}$. Then there must be some $w \in [\![\varphi]\!]$ such that $w \ll x$. Since $\ll$ is well-founded and transitive, there is some least such $w^* \ll x$. So $w^* \in [\![\varphi]\!]^{\ll}$, and so $w^* \in [\![\chi]\!]$. Moreover, $w^* \in [\![\varphi \wedge \chi]\!]^{\ll}$ since if not w^* would not be the least φ-world. However, this contradicts our assumption that $x \in [\![\varphi \wedge \chi]\!]^{\ll}$. Therefore, $x \in [\![\varphi]\!]^{\ll}$ and so by our assumption that $[\![\varphi]\!]^{\ll} \subseteq [\![\psi]\!]$ it follows that $x \in [\![\psi]\!]$, which is what we wanted to prove. □

4.8.1.1 Completeness Proof

Here we prove completeness for $\mathcal{NEL}$ over uniform normal evidence models. We proceed by first proving a triviality lemma which tells us that we only need to provide an axiom system that proves first-degree formulas of $\mathcal{NEL}$.

Lemma 1. *Every n-degree formula of $\mathcal{NEL}$ is reducible to a first-degree formula modulo equivalence over uniform normal evidence models.*

Proof. Here we provide a recursive procedure that translates a n-degree formula into an equivalent formula of degree less than n. If φ is a formula of degree n, then applying the procedure recursively to every subformula of φ that contains $\rightharpoonup$ as a main connective, we will find an equivalent first-degree formula.
First, recall that any propositional formula δ may be written in disjunctive normal form (DNF),

$$\delta = (P_0 \wedge ... \wedge P_j) \vee ... \vee (P_i \wedge ... \wedge P_n)$$

where P_i is either a literal (i.e. primitive proposition) or a negation of a literal. Let us extend the definition of DNF for the language of $\mathcal{NEL}$ by saying that a literal is either a primitive proposition, a formula whose main connective is $\rightharpoonup$, or a negation of one of these. It's clear that any DNF translation of $\mathcal{NEL}$ formulas preserve equivalence over uniform normal evidence models, since the translation is truth-functionally equivalent.

We now proceed in two steps. Let $\alpha \rightharpoonup \beta$ be a formula of degree n. First, translate β into its DNF equivalent:

$$(1) \quad \alpha \rightharpoonup ((P_0 \wedge ... \wedge P_i) \vee ... \vee (P_j \wedge ... \wedge P_n)).$$

Let Π be the DNF formula on the right, and let $\mathcal{C}_i$ be the i-th conjunction, e.g. $\mathcal{C}_0 = (P_0 \wedge ... \wedge P_j)$. Suppose P_k is a literal with $\rightharpoonup$ as a main connective. The following is equivalent to (1)

$$(2) \quad P_k \supset (\alpha \rightharpoonup \Pi[\mathcal{C}_i \mid (\mathcal{C}_i - P_k)]) \wedge \neg P_k \supset (\alpha \rightharpoonup (\Pi - P_k)),$$

where $\mathcal{C}_i - P_k$ is the conjunction $\mathcal{C}_i$ without P_k (and $\perp$ if the result is empty), and $\Pi - P_k$ is the DNF formula where any conjunction $\mathcal{C}_j$ is deleted (and $\perp$ if the result is empty) if P_k occurs in $\mathcal{C}_j$. The formula $\Pi[\mathcal{C}_i \mid (\mathcal{C}_i - P_k)]$ is then the DNF formula where the conjunction $\mathcal{C}_i$ is replaced with $\mathcal{C}_i - P_k$, for any $\mathcal{C}_i$ that contains P_k.

Suppose (1) is true at some world w. Since P_k is an evidence conditional, if P_k is true at w then it is true everywhere, i.e. $[\![P_k]\!] = W$, and so $P_k \wedge \gamma$ is equivalent to γ since $W \cap [\![\gamma]\!] = [\![\gamma]\!]$. It follows that $[\![\Pi]\!] = [\![\Pi[\mathcal{C}_i \mid (\mathcal{C}_i - P_k)])]\!]$. Note that when $\Pi[\mathcal{C}_i \mid (\mathcal{C}_i - P_k)])$ is empty $\Pi = P_k$, in which case, since P_k is true, $[\![\Pi]\!] = W$. By (1) we know that $[\![\alpha]\!]^{\ll} \subseteq [\![\Pi]\!]$, and so it follows that $[\![\alpha]\!]^{\ll} \subseteq [\![\Pi[\mathcal{C}_i \mid (\mathcal{C}_i - P_k)])]\!]$.

Similarly, if P_k is false at w then it is false everywhere, i.e. $[\![P_k]\!] = \emptyset$, and so $[\![P_k \wedge \gamma]\!] = \emptyset$ and $[\![(P_k \wedge \gamma) \vee \delta]\!] = [\![\delta]\!]$. It follows that $[\![\Pi]\!] = [\![\Pi - P_k]\!]$. By (1) we know that $[\![\alpha]\!]^{\ll} \subseteq [\![\Pi]\!]$ and so it follows that $[\![\alpha]\!]^{\ll} \subseteq [\![\Pi - P_k]\!]$.

For the other direction, suppose (2) is true at w. If P_k is true at w, then by similar reasoning $[\![\Pi]\!] = [\![\Pi[\mathcal{C}_i \mid (\mathcal{C}_i - P_k)])]\!]$, and hence $[\![\alpha]\!]^{\ll} \subseteq [\![\Pi]\!]$. If P_k is false at w then, again by similar reasoning, $[\![\Pi]\!] = [\![\Pi - \mathcal{P}_k]\!]$ and so $[\![\alpha]\!]^{\ll} \subseteq [\![\Pi]\!]$.

This shows that any evidence conditionals imbedded in the right side of $\alpha \rightharpoonup \beta$ can be pulled out. Notice that if (1) is of degree n, and P_k is the highest degree subformula occuring in α or β, then (2) will be a formula of degree less than n. But the highest degree subformula may occur in α.

For the second step, we need to apply a similar reduction to α. The reasoning is basically the same:

$$(1) \quad (P_0 \wedge ... \wedge P_j) \vee ... \vee (P_i \wedge ... \wedge P_n) \rightharpoonup \beta$$

is equivalent to

$$(2) \quad P_k \supset (\Pi[\mathcal{C}_i \mid (\mathcal{C}_i - P_k)] \rightharpoonup \beta) \wedge \neg P_k \supset ((\Pi - P_k) \rightharpoonup \beta)$$

by a similar argument. And so, if we apply the reduction to both α and β in $\alpha \rightharpoonup \beta$, we will eventually obtain a formula of degree 1. $\square$

We now turn to soundness and completeness. Let the axiom system **EL** consist of the following axioms and inference rules:

A1	All propositional tautologies
A2	$\vdash \varphi \rightharpoonup \varphi$
A3	$\vdash ((\varphi \rightharpoonup \psi) \wedge ((\varphi \wedge \psi) \rightharpoonup \chi)) \supset (\varphi \rightharpoonup \chi)$
A4	$\vdash (\varphi \rightharpoonup (\psi \wedge \chi)) \supset (\varphi \rightharpoonup \psi)$
A5	$\vdash (\varphi \rightharpoonup \psi) \wedge (\varphi \rightharpoonup \chi) \supset ((\varphi \wedge \psi) \rightharpoonup \chi)$
A6	$\vdash (\varphi \rightharpoonup \chi) \wedge (\psi \rightharpoonup \chi) \supset ((\varphi \vee \psi) \rightharpoonup \chi)$
A7	If φ is propositionally consistent, then $\vdash \neg(\varphi \rightharpoonup \neg\varphi)$
A8	$\vdash \varphi_1 \equiv \varphi_n$, where φ_1 is a first-degree translation of some n-degree formula φ_n
R1	From $\vdash \varphi$ and $\vdash \varphi \supset \psi$, infer $\vdash \psi$
R2	If $\vdash \varphi \equiv \psi$, then from $\vdash \alpha$ infer $\vdash \alpha[\varphi \,|\psi]$, where $\alpha[\varphi \,|\psi]$ is the formula where any number of subformulas φ are replaced by ψ.

Soundness of some key axioms has already been shown (Fact 1-4), and crucially, A8 is sound by Lemma 1. The rest are easy to check. Completeness, on the other hand, is a bit more complicated. Here is a sketch before I present the details. As usual, any **EL**-consistent formula α of $\mathcal{NEL}$ may be extended to a maximally **EL**-consistent set of formulas, Δ_α. We then construct, *for each* Δ_α, a uniform evidence model M_α such that $\varphi \rightharpoonup \psi \in \Delta_\alpha$ iff $M_\alpha, w \models \varphi \rightharpoonup \psi$ for some w.[31] Completeness will then shortly follow.[32] Because of Lemma 1 and A8, I will assume throughout that when a proposition of the form $\varphi \rightharpoonup \psi$ occurs, both

[31]Contrast this with the usual canonical model construction in modal logic, where each state is identified with a maximally consistent set of formulas where any consistent formula is satisfiable. Such an approach will not work here, since $\varphi \rightharpoonup \psi$ and $\varphi \rightharpoonup \neg\psi$ are individually **EL**-consistent, but there is no model that satisfies them both.

[32]The argument here is a synthesis of techniques in the non-monotonic logic literature, especially [15] and [39]. [15] has a similar proof theory but richer semantics, featuring a ternary relation instead of our binary relation. [39] formalize a related consequence relation, and their state-description models are slightly more general than the models here. (It is not obvious—at least to me—whether their results can be directly ported to our framework.)

φ and ψ are fully propositional. There is no loss of generality here, because if $\varphi \rightharpoonup \psi$ is of any higher degree, we can simplify the formula on the semantic side with Lemma 1 and on the syntactic side with A8. Now, let us first note some derived rules.

D1 From $\vdash (\varphi \supset \psi)$ infer $\vdash (\chi \rightharpoonup \varphi) \supset (\chi \rightharpoonup \psi)$

D2 $\vdash (\varphi \rightharpoonup \psi) \wedge (\chi \rightharpoonup \gamma) \supset (\varphi \vee \chi) \rightharpoonup (\psi \vee \gamma)$

D3 $\vdash (\varphi \rightharpoonup \psi \wedge \psi \rightharpoonup \varphi) \supset ((\varphi \rightharpoonup \gamma) \supset (\psi \rightharpoonup \gamma))$

D4 $\vdash (\varphi \rightharpoonup \psi \wedge \varphi \rightharpoonup \chi) \supset (\varphi \rightharpoonup (\psi \wedge \chi))$

For D1, suppose $\vdash (\varphi \supset \psi)$ and the antecedent condition. Note that $\varphi \supset \psi$ is truth-functionally equivalent to $\varphi \equiv (\varphi \wedge \psi)$. From R2 it follows that that $(\chi \rightharpoonup \varphi) \wedge \psi$, and hence the consequent follows from A4. A consequence of D1 that we will use a number of times is that from $\vdash \varphi \supset \psi$ we can infer $\vdash \varphi \rightharpoonup \psi$. To see this, replace χ with φ. It then follows by R1 and A1. For D2, suppose the antecedent conditions. Two applications of D1 allow us to infer $(\varphi \rightharpoonup (\psi \vee \gamma)$ and $\chi \rightharpoonup (\psi \vee \gamma)$, and the consequent follows from an application of A6. For D3, from the antecedent conditions we can derive $(\varphi \wedge \psi) \rightharpoonup \gamma$ from A5 and then $\psi \rightharpoonup \gamma$ from A3. Finally, D4: from the antecedent conditions and A5 it follows that $(\varphi \wedge \psi) \rightharpoonup \chi$. We also know that $(\varphi \wedge \psi \wedge \chi) \rightharpoonup (\psi \wedge \chi)$, and so by an application of A3 we have $(\varphi \wedge \psi) \rightharpoonup (\psi \wedge \chi)$, and since $\varphi \rightharpoonup \psi$, another use of A3 finishes the job.[33]

The goal is to construct a canonical model for each maximally consistent set. Important here will be the definition of a suitable $\ll$ relation. While syntatically we cannot express relative normality between worlds, we can, in a way, express relative normality between formulas. Here is the crucial definition:

Definition 4.8.1. *Let $\varphi \trianglelefteq \psi$ mean that φ is at least as normal as ψ, and define $\varphi \trianglelefteq \psi$ iff $\vdash (\varphi \vee \psi) \rightharpoonup \varphi$.*

The idea here is that if $\vdash (\varphi \vee \psi) \rightharpoonup \varphi$ then the most normal worlds among the φ or ψ worlds

[33]This kind of reasoning requires a deduction theorem, and it is easy to see that the standard induction on lengths of proofs works here.

are φ worlds, and hence if w is a minimal φ world, it must be at least as normal as any ψ world. The following two lemmas show that $\trianglelefteq$ possesses natural properties of a normality relation.

Lemma 1. *The relation $\trianglelefteq$ is reflexive and transitive.*

Proof. For reflexivity, substitute $\varphi \vee \varphi$, via R2, for the first occurrence of φ in A2. For transitivity, suppose $(\varphi \vee \psi) \rightharpoonup \varphi$ and $(\psi \vee \chi) \rightharpoonup \psi$. From both and D2 it follows that $(\varphi \vee \psi \vee \chi) \rightharpoonup (\varphi \vee \psi)$. Recall that if $\vdash \alpha \supset \beta$ then $\vdash \alpha \rightharpoonup \beta$ follows from D1, and so we have $(\varphi \vee \psi) \rightharpoonup (\varphi \vee \psi \vee \chi)$. The antecedents of an instance of D3 are now satisfied, so we may conclude from $(\varphi \vee \psi) \rightharpoonup \varphi$ that (1) $(\varphi \vee \psi \vee \chi) \rightharpoonup \varphi$. Now, from D1 it follows that (2) $(\varphi \vee \psi \vee \chi) \rightharpoonup (\varphi \vee \chi)$. Applying A5 to (1) and (2) and noting that $(\varphi \vee \psi \vee \chi) \wedge (\varphi \vee \chi)$ is equivalent to $\varphi \vee \chi$, we arrive at our result. $\square$

Lemma 2. *If $\varphi \trianglelefteq \psi$ and $\psi \trianglelefteq \chi$, then $\vdash \varphi \rightharpoonup (\chi \supset \psi)$.*

Proof. Suppose $\varphi \trianglelefteq \psi$ and $\psi \trianglelefteq \chi$. From D2 it follows that (1) $((\varphi \vee \psi \vee \chi)) \rightharpoonup (\varphi \vee \psi)$. From $\psi \trianglelefteq \chi$ it follows that $((\psi \vee \chi) \wedge (\varphi \vee \psi \vee \chi)) \rightharpoonup \psi$ by R2. By D1 we have $(\psi \vee \chi) \wedge (\varphi \vee \psi \vee \chi) \rightharpoonup ((\psi \vee \chi) \supset \psi)$. We also have $(\neg(\psi \vee \chi) \wedge (\varphi \vee \psi \vee \chi)) \rightharpoonup ((\psi \vee \chi) \supset \psi)$, since $\neg(\psi \vee \chi) \wedge (\varphi \vee \psi \vee \chi)$ entails $((\psi \vee \chi) \supset \psi)$. So then by A6 and an application of R2, we get $(\varphi \vee \psi \vee \chi) \rightharpoonup ((\psi \vee \chi) \supset \psi)$. By D1 we simplify the right side, (2) $(\varphi \vee \psi \vee \chi) \rightharpoonup (\chi \supset \psi)$. Now, we can apply A5 to (1) and (2), and arrive at $(\varphi \vee \psi) \rightharpoonup (\chi \supset \psi)$. From $\varphi \trianglelefteq \psi$ it follows that $(\varphi \vee \psi) \rightharpoonup \varphi$. And so by another application of A5 we arrive at $\varphi \rightharpoonup (\chi \supset \psi)$ through R2. $\square$

The idea behind lemma 2 is that, if φ is at least as normal as ψ, and ψ is at least as normal as χ, then, among the most normal φ worlds, if χ is true, then, since ψ worlds are at least as normal as χ worlds, ψ must be true. We need one more lemma in order to construct our canonical models.

Lemma 3. *Let Δ be a maximally **EL**-consistent set of formulas. Let $\Psi = \{\psi_0, \psi_1, ...\}$ be a set of formulas such that $\varphi \rightharpoonup \psi_i \in \Delta$. If φ is consistent, the set $\Psi \cup \{\varphi\}$ is satisfiable.*

Proof. Suppose $\Psi \cup \{\varphi\}$ is unsatisfiable. By propositional reasoning and A1 it follows that $\vdash \varphi \supset \neg(\psi_i \wedge ... \wedge \psi_j)$ for some finite subset of Ψ. Because Δ is maximally consistent, from D1 it follows that $\varphi \rightharpoonup \neg(\psi_i \wedge ... \wedge \psi_j) \in \Delta$. But $\varphi \rightharpoonup \psi_i \in \Delta$ for each $\psi_i \in \Psi$, and since Δ contains every instance of D4, it follows that $\varphi \rightharpoonup (\psi_i \wedge ... \wedge \psi_j) \in \Delta$. By another instance of D4, $\varphi \rightharpoonup ((\psi_i \wedge ... \wedge \psi_j) \wedge \neg(\psi_i \wedge ... \wedge \psi_j)) \in \Delta$, and by D1 it follows that $\varphi \rightharpoonup \neg\varphi \in \Delta$. (The contradictory conjunction propositionally entails $\neg\varphi$.) But φ is consistent, and so Δ contains $\neg(\varphi \rightharpoonup \neg\varphi)$ by A7, and thus contradicts the fact that Δ is consistent. $\square$

Let α be a formula of $\mathcal{NEL}$. We note without proof that α may be extended to a maximal **EL**-consistent set of formulas, Δ_α, in the usual way.

Definition 4.8.2. *Let i be a valuation function defined on propositional formulas in the usual way, and let φ be a propositional formula. We say that i is a* normal *interpretation for φ just in case for every ψ_j such that $\varphi \rightharpoonup \psi_j \in \Delta_\alpha$, $i(\psi_j) = \top$.*

Let us now prove an important fact about normal interpretations.

Lemma 4. *Let φ and ψ be propositional formulas. If every normal interpretation of φ satisfies ψ, then $\vdash \varphi \rightharpoonup \psi$.*

Proof. Let us prove the contrapositive. Suppose $\nvdash \varphi \rightharpoonup \psi$. We need to show that there is a normal interpretation for φ that does not satisfy ψ. It is enough to show that $\{\gamma \mid \varphi \rightharpoonup \gamma\} \cup \{\neg\psi\}$ is satisfiable. Suppose not. It follows by propositional reasoning that $(\gamma_1 \wedge ... \wedge \gamma_n) \supset \psi$ is a propsitional validity, and hence $\varphi \supset ((\gamma_1 \wedge ... \wedge \gamma_n) \supset \psi)$ is as well. By A1 and D1 it follows that $\vdash \varphi \rightharpoonup ((\gamma_1 \wedge ... \wedge \gamma_n) \supset \psi)$. For each γ we have $\vdash \varphi \rightharpoonup \gamma$, and so by successive use of D4 we have $\vdash \varphi \rightharpoonup (\gamma_1 \wedge \ldots \wedge \gamma_n)$. By D1 ($\chi$ there is φ) and R1, we then have that $\vdash \varphi \rightharpoonup \psi$, contradicting our assumption. $\square$

Definition 4.8.3. *Let Π_α be the set of consistent purely propositional formulas built out of the propositional letters occuring in α.*

It follows from Lemma 3 that for any consistent propositional φ there is an normal interpretation for φ. We use these normal interpretations to build the canonical models.

Let $M_\alpha = \langle W_\alpha, \ll_\alpha, V_\alpha \rangle$ be defined as follows:[34]

- $W_\alpha = \{(i, \varphi) \mid \text{where } i \text{ is a normal interpretation of } \varphi \text{ for } \varphi \in \Pi_\alpha\}$
- $(i, \varphi) \ll (i', \psi)$ iff $\varphi \trianglelefteq \psi$ and $i(\psi) = \bot$
- $V_\alpha(p) = \{(i, \varphi) \mid i(p) = \top\}$

The basic idea is that to construct our canonical model we must build in the normality relations as they are 'expressed' in Δ_α. We do this by populating our model with points identified as formulas (plus valuations), because normality of formulas are what Δ_α can distinguish. Intuitively, φ is much more normal than ψ just in case φ is at least as normal than ψ and a normal interpretation of φ fails to make ψ true. Note that there might be many normal interpretations of φ, and hence the models will be quite large. However, without loss of generality they will be finite. It is easy to see that Π_α is finite modulo logical equivalence, and so we may restrict W_α accordingly without harm. Now, we need to show that M_α is indeed an evidence model, i.e. that $\ll$ is irreflexive, transitive and well-founded. But first, we prove a lemma which characterizes the the set $[\![\varphi]\!]^{\ll}_{M_\alpha}$.

Lemma 5. *For all propositional $\varphi \in \Delta_\alpha$ the following equivalence holds:*

[34] Note that E and $\leq$ do not appear in the semantic clause for $\rightharpoonup$, and so we can ignore them when axiomatizing the restricted language.

$$[\![\varphi]\!]^{\ll}_{M_\alpha} = \{(i,\psi) \mid i(\varphi) = \top \text{ and } \psi \trianglelefteq \varphi\}$$

Proof. Suppose $(i,\gamma) \in [\![\varphi]\!]^{\ll}_{M_\alpha}$. Since φ is propositional, it follows that $i(\varphi) = \top$. Suppose for a contradiction that there is a $(i', \varphi \vee \gamma) \in W_\alpha$ such that $i'(\gamma) = \bot$. But, (by definition of $\trianglelefteq$) we know $\varphi \vee \gamma \trianglelefteq \gamma$, and hence $(i', \varphi \vee \gamma) \ll (i,\gamma)$. But since by R2 it must be the case that $i'(\varphi \vee \gamma) = \top$, and so $i'(\varphi) = \top$ and so $(i', \varphi \vee \gamma) \in [\![\varphi]\!]$. But this contradicts our assumption that (i,γ) is minimal. This shows that there is no normal interpretation for $\varphi \vee \gamma$ that makes does not satisfy γ, i.e. every normal interpretation for $\varphi \vee \gamma$ satisfies γ. But then by Lemma 4, it follows that $\vdash (\varphi \vee \gamma) \rightharpoonup \gamma$, i.e. $\psi \trianglelefteq \varphi$.

For the other direction, suppose $(i,\gamma) \in W_\alpha$ such that $i(\varphi) = \top$ and $\gamma \trianglelefteq \varphi$. Suppose that $(i', \beta) \ll (i,\gamma)$ such that $i'(\varphi) = \top$. Then, by the definition of $\ll$, $\beta \trianglelefteq \gamma$. By Lemma 2 we have that $\beta \rightharpoonup (\varphi \supset \gamma) \in \Delta_\alpha$. But so $i'(\varphi \supset \gamma) = \top$, since i' is a normal interpretation of β. It then follows that $i'(\gamma) = \top$, which contradicts our assumption that $(i',\beta) \ll (i,\gamma)$. $\square$

Lemma 6. *The relation $\ll$ is irreflexive, transitive, and well-founded.*

Proof. For irreflexivity, note that by A2, $\varphi \rightharpoonup \varphi \in \Delta_\alpha$ and so if i is a normal interpretation of φ then $i(\varphi) \neq \bot$. For transitivity, suppose $(i,\varphi) \ll (i',\psi)$ and $(i',\psi) \ll (i'',\chi)$. From $\varphi \trianglelefteq \psi$ and $\psi \trianglelefteq \chi$ it follows by Lemma 1 that $\varphi \trianglelefteq \chi$. We then just need to show that $i(\chi) = \bot$. By Lemma 2, it follows that $\varphi \rightharpoonup (\chi \supset \psi) \in \Delta_\alpha$, and since i is a normal interpretation for φ, it follows that $i(\chi \supset \psi) = \top$. If $i(\chi) = \top$, then $i(\psi) = \top$, which contradicts our assumption that $(i,\varphi) \ll (i',\psi)$. As for well-foundedness, note that M_α is finite, and every relation over a finite set is well-founded. $\square$

Lemma 7. *For any φ, ψ and α:*

$$\varphi \rightharpoonup \psi \in \Delta_\alpha \text{ iff } M_\alpha \models \varphi \rightharpoonup \psi$$

Proof. For the left to right, suppose $\varphi \rightharpoonup \psi \in \Delta_\alpha$. We need to show that $[\![\varphi]\!]^{\ll}_{M_\alpha} \subseteq [\![\psi]\!]_{M_\alpha}$. Suppose $(i, \gamma) \in [\![\varphi]\!]^{\ll}_{M_\alpha}$. It will suffice to show that $i(\psi) = \top$. From $\varphi \rightharpoonup \psi$ we have $(\varphi \wedge (\varphi \vee \gamma)) \rightharpoonup \psi$ from R2, and hence $(\varphi \wedge (\varphi \vee \gamma)) \rightharpoonup (\varphi \supset \psi)$ by D1 (ψ propositionally entails $\varphi \supset \psi$). We also have $(\neg \varphi \wedge (\varphi \vee \gamma)) \rightharpoonup (\varphi \supset \psi)$. By D2 and R2 we then have (1) $(\varphi \vee \gamma) \rightharpoonup (\varphi \supset \psi)$. Since $\gamma \trianglelefteq \varphi$ (via Lemma 5) it follows that (2) $(\gamma \vee \varphi) \rightharpoonup \gamma$. We then conclude $\gamma \rightharpoonup (\varphi \supset \psi)$ from (1) and (2) by A5 and R2. But i is a normal interpretation for γ, so $i(\varphi \supset \psi) = \top$ and so $i(\psi) = \top$ as desired.

For the right to left direction, suppose $\varphi \rightharpoonup \psi \notin \Delta_\alpha$. We need to show that there is some normal interpretation for φ where $i(\psi) = \bot$. Suppose there is not. Then by Lemma 4 it follows that $\vdash \varphi \rightharpoonup \psi$. But since Δ_α is maximal, $\varphi \rightharpoonup \psi \in \Delta_\alpha$, which contradicts our assumption. $\square$

We are now in a position to state our main theorem.

Theorem 1. ***EL*** *is a sound and complete axiomatization with respect to uniform normal evidence models for formulas of the language* $\mathcal{NEL}$.

Proof. Facts 2 and 4 illustrate the soundness of A3 and A5. The others are straightfoward, with the exception of A8, which is sound by Lemma 1. It is also clear that R1 and R2 preserve truth.

For completeness, we show that every **EL**-consistent formula is satisfiable in a uniform normal evidence model. Suppose α is a **EL**-consistent formula. Note that because of Lemma 1, α is logically equivalent to a DNF formula of the form

$$(P_0 \wedge ... \wedge P_j) \vee ... \vee (P_i \wedge ... \wedge P_n)$$

where each P_k is either a (negated) atomic proposition or a formula of the form $(\neg)\, \varphi \rightharpoonup \psi$.

Because α is **EL**-consistent, some such disjunct will be **EL**-consistent. Let α^* be such a disjunct. We then extend α^* to a maximal consistent set of formulas Δ_{α^*}, and build a uniform normal evidence model M_{α^*} as described above. Let $\bigwedge P_k$ be the conjunction of atomic propositions occurring as a conjunct of α^*. Clearly $\bigwedge P_k \in \Pi_\alpha$, and so there is some state $(i, \bigwedge P_k)$ in M_{α^*}, and hence $M_{\alpha^*}, (i, \bigwedge P_k) \models \bigwedge P_k$. The rest of the conjuncts of α^* are formulas of the form $(\neg)\, \varphi \rightharpoonup \psi$. By Lemma 7 it follows that $M_{\alpha^*} \models (\neg)\, \varphi \rightharpoonup \psi$, and a fortiori, $M_{\alpha^*}, (i, \bigwedge P_k) \models (\neg)\, \varphi \rightharpoonup \psi$. It follows that $M_{\alpha^*}, (i, \bigwedge P_k) \models \alpha^*$ and hence α is satisfiable in M_{α^*}.

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