Weintraub's Response to Williamson's Coin Flip Argument

Matthew W. Parker

Rotman Institute of Philosophy

Western University

matthew.parker@uwo.ca

ORCID 0000-0002-7436-2149

Declarations

Funding - partly supported by the John Templeton Foundation under grant #61048.

Conflicts of interest/Competing interests – none.

Availability of data and material – not applicable.

Code availability – not applicable.

Authors' contributions – only one author.

Ethical approval – not applicable.

Informed consent – not applicable.

Abstract

A probability distribution is *regular* if it does not assign probability zero to any possible event. Williamson (2007) argued that we should not require probabilities to be regular, for if we do, certain "isomorphic" physical events (infinite sequences of coin flip outcomes) must have different probabilities, which is implausible. His remarks suggest an assumption that chances are determined by intrinsic, qualitative circumstances. Weintraub (2008) responds that Williamson's coin flip events differ in their inclusion relations to each other, or the inclusion relations between their times, and this can account for their differences in probability. Haverkamp and Schulz (2011) rebut Weintraub, but their rebuttal fails because the events in their example are even less symmetric than Williamson's. However, Weintraub's argument also fails, for it ignores the distinction between intrinsic, qualitative differences and relations of time and bare identity. Weintraub could rescue her argument by claiming that the events differ in duration, under a non-standard and problematic conception of duration. However, we can modify Williamson's example with Special Relativity so that there is no absolute inclusion relation between the times, and neither event has longer duration except relative to certain reference frames. Hence, Weintraub's responses do not apply unless chance is observer-relative, which is also problematic. Finally, another symmetry argument defeats even the appeal to frame-dependent durations, for there the events have the same finite duration and are entirely disjoint, as are their respective times and places.

This paper is dedicated to the memory of Colin Howson, a brilliant and cheerful scholar.

1. Introduction

A probability distribution is *regular* if it does not assign probability zero to any possible event. If, for example, we flip a fair coin infinitely many times, what is the chance it will come up heads every time? Normally we would express this as the infinite product ½ × ½ × ½ × … = 0. But if such an event is possible, a regular probability distribution must assign it a strictly positive probability (or no value at all).[1]

There have been many arguments that chances or credences should be regular (e.g., Carnap 1950, 1963; Kemeny 1955, 1963; Lewis 1980; Skyrms 1980; Wenmackers and Horsten 2013; Hofweber 2014; Benci et al. 2013; 2018). However, there are well known cases where constructing a regular distribution is problematic. For our coin flips, it requires a modification to the axioms of probability, for under the standard Kolmogorov axioms, if the tosses are fair and independent, the probability of an infinite sequence of heads must be smaller than any positive real number.[2] Hence, under the usual assumption that probabilities are real numbers, the probability of an infinite sequence of heads must be zero, and regularity fails.

---

[1] In general, a probability function assigns probabilities to all sets in some algebra of subsets of a sample space, not *all* subsets of the sample space. Hájek (unpublished) takes regularity to imply that *all* sets of possible outcomes are assigned non-zero probability, rather than non-zero or none at all. We could call Hájek's regularity *strong regularity*, and the weaker condition that every non-empty set of outcomes is assigned either non-zero probability or none at all, *weak regularity*. Hájek argues against strong regularity, but here we are mainly concerned with weak regularity, for, as we will see, the latter is already problematic, provided that a few very simple events do have probabilities.

[2] Proof: Suppose $0 < \varepsilon \in \mathbf{R}$. Choose $n > \log_{1/2} \varepsilon$. Write $H(m, m + 1,\ldots, n)$ for the event that flips $m, m + 1,\ldots, n$ all come up heads. Since the flips are fair and independent, $\mathrm{Prob}(H(1, 2, 3,\ldots, n)) = (½)^n < \varepsilon$. By independence, $\mathrm{Prob}(H(1, 2, 3,\ldots)) = \mathrm{Prob}(H(1, 2, 3,\ldots, n)) \times \mathrm{Prob}(H(n + 1, n + 2, n + 3,\ldots))$. By the normality axiom, $\mathrm{Prob}(H(n + 1, n + 2, n + 3, \ldots)) \leq 1$. Hence $\mathrm{Prob}(H(1, 2, 3, \ldots)) < \varepsilon \times 1 = \varepsilon$.

A common proposal (see above references) is to introduce infinitesimal and hyperreal probabilities. In that case, countable additivity must be abandoned, for sequences of hyperreal numbers do not generally have well defined limits. (Hence, when we speak of probabilities here, this is not meant to imply countable additivity.) But using infinitesimal values, we can keep finite additivity and still assign a positive probability to an infinite sequence of heads, just one that is smaller than any positive *real* number. Regularity is saved, if at a cost.

However, Williamson (2007) argues that regularity introduces an untenable asymmetry that is not corrected by introducing hyperreals. Regularity, he argues, implies that the probability that every coin toss in a given infinite sequence yields heads is smaller than the probability that every toss *after* the first yields heads. But these two events may be "isomorphic", in which case, he says, they must have the *same* probability. Introducing infinitesimals does not resolve this conflict.

Howson (2017) and Benci et. al (2018) have recently criticized Williamson's argument and related symmetry arguments against regularity, and their criticisms are answered in Parker 2019.[3] However, the first published criticism of Williamson's argument, due to Weintraub (2008), awaits a compelling response. Weintraub argues that Williamson's events are not physically identical, for the sequence of tosses after the first toss is a proper subsequence of the original, and this relation is not symmetric. In another case that Williamson discusses, where a

[3] Howson continued his critique of Williamson in 2019a and 2019b. His 2019a does not address the arguments of Parker 2019. Howson 2019b acknowledges a related argument, based on the claim that spacetime invariance is "a fundamental feature of nature", but Howson then downplays the infinitesimal asymmetries implied by regularity as lying within an empirically "more-than-acceptable margin of error". We will not pursue this dispute at length here, but a brief remark is warranted: If we are willing to accept an infinitesimal margin of error, then there is no need to introduce infinitesimals. Regularists could instead just accept the classical real-valued theory of probability as empirically close enough. On the contrary, it seems that what regularists about chances want is a better or more accurate theory of chance, at a level of detail that exceeds the empirical discernibility of individual chance values. For that purpose, we must decide whether the principle of spacetime invariance and other symmetry considerations outweigh the arguments for regularity, which even Howson (*ibid.*) regarded as weak.

third sequence is not contained in either of the others, Weintraub points out that the *times* at which the tosses in the third sequence occur are asymmetrically contained in the times for the first sequence. These asymmetries, she argues, can account for the differences in probabilities required by regularity.

Weintraub's argument has been criticized by Haverkamp and Schulz (2011). We will see that their response is inadequate, but also that Weintraub's argument is itself insufficient to vindicate regularity. On a plausible view, which Williamson seems to take for granted, the chances and credences of events should be determined by their *intrinsic, qualitative* properties and circumstances, not extrinsic or haecceitistic features. The differences Weintraub finds in Williamson's events appear to be extrinsic and haecceitistic, and hence to have no force against Williamson, given that view. However, Weintraub has suggested (in personal communication) that Williamson's events can be taken to differ in duration, which plausibly is an intrinsic, qualitative property. This purported difference in duration requires a radically nonstandard and problematic conception of duration, since all the sequences of coin flips are infinitely long, but that is not a conclusive refutation. Hence, the success of Weintraub's rebuttal to Williamson hinges on the proposition that (a) probabilities need not be determined by intrinsic qualitative properties, or (b) the sequences do differ in duration and that is an intrinsic, qualitative property.

However, Williamson's argument can be modified to circumvent these issues. In a relativistic version, there is no asymmetry to account for a difference in probability, not even an extrinsic or haecceitistic one. There Weintraub's solution would require making the probabilities relative to reference frames, and I will briefly argue that this is implausible. But even if one bites that bullet, we can imagine yet other experiments, each consisting of a single trial bounded in space and time, where regularity again implies that two outcomes must have different probabilities, with no asymmetry of inclusion, duration, or size, and no recourse to frame-relative probabilities.

Hence, one of the costs of regularity is that perfectly symmetric events under perfectly symmetric circumstances must be assigned different chances and credences. This is not merely counterintuitive but entails arbitrariness and other theoretical vices that make it unattractive. That does not conclusively refute Weintraub or regularity; one can always choose to bite one bullet or another. Our purpose here is to illustrate what pain the bullet biter must endure, and thus, hopefully, advance the debate. Our arguments will mainly focus on the unattractiveness of regular chances, with regular credences arguably coming in tow, if perhaps less clearly.

2. Williamson's argument

Williamson (2007) considers a countably infinite sequence of independent coin flips at one second intervals, with a fair coin. We write 'H(1)' for the event that the first toss comes up heads, 'H(1…)' for the event that all of the tosses come up heads, and 'H(2…)' for the event that the second and subsequent flips all come up heads. Independence then implies that

$$\text{Prob}(H(1\ldots)) = \text{Prob}(H(1)) \times \text{Prob}(H(2\ldots)) = \tfrac{1}{2} \times \text{Prob}(H(2\ldots)). \qquad (1)$$

For a regularist, Prob(H(1…)), Prob(H(2…)) > 0, since both events are possible. It follows from this and (1) that Prob(H(1…)) < Prob(H(2…)).

Williamson argues that the regularist is mistaken, for…

> H(1...) and H(2...) are isomorphic events. More precisely, we can map the constituent single-toss events of H(1...) one-one onto the constituent single-toss events of H(2...) in a natural way that preserves the physical structure of the set-up just by mapping each toss to its successor. H(1...)

> and H(2...) are events of exactly the same qualitative type; they differ only in the inconsequential respect that H(2...) starts one second after H(1...). Thus H(1...) and H(2...) should have the same probability.

Hence, if "isomorphic" events have the same probability, regularity must fail in this case. Williamson shows directly that if Prob(H(1…)) = Prob(H(2…)) then both are zero, despite the events being possible. These arguments use only first-order properties that apply equally to real and hyperreal numbers, so introducing hyperreals does not help the regularist here.

Williamson also considers a third sequence "to make the point vivid", but we will see that it has more significance than that:

> [S]uppose that another fair coin, qualitatively identical with the first, will also be tossed infinitely many times at one second intervals, starting at the same time as the second toss of the first coin, all tosses being independent. Let H*(1...) be the event that every toss of the second coin comes up heads […]. Then H(1...) and H*(1...) should be equiprobable, because the probability that a coin comes up heads on every toss does not depend on when one starts tossing, and there is no qualitative difference between the coins. But for the same reason H*(1...) and H(2...) should also be equiprobable. These two infinite sequences of tosses proceed in parallel, synchronically, and there is no qualitative difference between the coins… (175)

In other words, Prob(H(1…)) = Prob(H*(1…)), because the probability of an event does not depend on *when* it happens, provided the circumstances are otherwise fixed,[4] nor to which particular coin it happens, if the coins are exactly alike, and Prob(H*(1…)) = Prob(H(2…)),

[4] Of course, the probability of one being blinded by sunlight is lower at night than during the day, as one referee pointed out, but that is due to a difference in circumstances, not the mere times of the proposed events.

because the probability of an event does not depend on *where* it happens either.[5] By transitivity, we again get Prob(H(1…)) = Prob(H(2…)), and these must be exactly zero by the previous argument.

But the introduction of H*(1…) does more than make the point vivid. It circumvents the asymmetry between H(1…) and H(2…) consisting in the fact that the sequence (2…) is a subsequence of (1…) and not vice-versa. The events H(1…) and H*(1…) are entirely separate and qualitatively identical; they differ only in time (or, relativistically, in their spatiotemporal relations to other events). Hence, on Williamson's view, they should have the same probability. Likewise, H*(1…) and H(2…) are separate and qualitatively identical, differing only in place (or, again, spatiotemporal relations). So, one cannot in either case argue that the events are not symmetric simply because the coin tosses that make up one are included in those that make up the other and not vice versa. H(1…) and H*(1…) consist of entirely separate coin tosses, as do H*(1…) and H(2…).

Williamson claims that his arguments apply to both chance and rational credence. Whatever sort of probability we are concerned with, he thinks it is unreasonable to assign different probabilities to perfectly similar events:

---

[5] I am speaking loosely here of "the same event" occurring at different times or places or involving different coins. We can speak this way if we regard an event as a class of possible configurations in space and time (including any motions) of matter, energy, fields, or whatever physical entities exist, identified by the intrinsic structure of the configuration rather than the absolute place or time at which it hypothetically occurs or the bare identities of the entities involved. Alternatively we might understand an event as a class of spacetime configurations in a particular place or time, with specific samples of matter or what have you, so that it makes no sense to speak of the same event occurring in a different place or time or with a different coin. But in that case we can instead speak of qualitatively similar events at different times and places. The present point is just that, on Williamson's view, perfectly similar events in different times and places or involving numerically distinct but perfectly similar coins should have the same chance. For further clarification of Williamson's notion of an event, see Parker 2019.

> H(1...) has exactly the same chance as H*(1...) and H*(1...) has exactly the same chance as H(2...). It would be unreasonable to give H(1...) more or less credence than H*(1...) or H*(1...) more or less credence than H(2...). Likewise for other kinds of probability. (2007)

He says nothing about why such an assignment of credences would be unreasonable.

3. Isomorphism, Chance, and Credence

Williamson's arguments are clearly based on a tacit premise articulated in Parker 2019:

> ***Isomorphism Principle (IP).*** If two events are isomorphic (in the relevant sense), they should have the same probability,

where probability is not assumed to satisfy all of the Kolmogorov axioms,[6] but represents chance or rational credence. Williamson does not state this principle, but his inference from what he calls isomorphism to equiprobability makes it clear. What he means by 'isomorphic events' is not the existence of a structure-preserving bijection between two subsets of a sample space, as Howson (2017) takes it, but rather, as Williamson explains in the first passage quoted above, the existence of a map between the constituent *conjuncts* of two conjunctive events, which preserves the *physical* structure of the events (cf. Parker 2019).

More plainly, Williamson's isomorphism just consists in the qualitative physical similarity of events. Hence, Parker 2019 formulates a version of IP for chances as follows:

[6] In particular, this does not assume countable additivity. Regularists often introduce infinitesimal probabilities in order to obtain regular, uniform distributions over infinite sample spaces, and these infinitesimal probabilities are not countably additive. Since regularists are already willing to sacrifice countable additivity, Parker 2019 does not take it for granted, and nor will we here.

> ***IP′.*** Two events that differ at most in where and when they hypothetically occur (and perhaps in matters of bare identity but not in *qualitative* features) have the same chance.

This does not follow from or imply IP but is a variation of IP amenable to a simple argument involving the presumed spacetime invariance of natural laws. Here we want to defend the plausibility and attractiveness of IP more generally. For that purpose, we will reformulate it in chance and credence versions as follows:

> ***IPCh.*** Two events that differ at most in where and when they hypothetically occur (and not in intrinsic, qualitative features or circumstances) have the same chance.[7]
>
> ***IPCr.*** Lacking "inadmissible" information about actual outcomes (Lewis 1980), it would be irrational to assign different credences to two events that differ at most in where and when they occur (and not in intrinsic, qualitative features or circumstances).

We will now try to clarify these two principles and briefly consider some possible objections. However, we will not attempt to refute all such objections, since Weintraub's response does not rely on them. Our focus here is on Weintraub's responses, to which we will soon come.

An *intrinsic* property is roughly one that does not involve relations to other things. It is notoriously difficult to make this precise (see, e.g., Humberstone 1996; Francescotti 1999), but some properties are clearly not intrinsic. For example, the property of a sequence of coin flips being preceded by an additional flip, or equivalently, forming a proper part of another sequence,

[7] This is a slight elaboration of IP′, making explicit the restriction to intrinsic, qualitative properties. It is also similar to Schaffer's Stable Trial Principle (2003) and his Intriniscness Requirement (2007).

is clearly extrinsic to the properly included sequence of flips. Hence, IPCh implies that the chance of a sequence of coin flips having a certain outcome is not affected by the mere fact of being part of a larger sequence. This does not rule out the possibility that an earlier coin flip could in principle have *effects* that influence future flips by modifying the background conditions of those flips. For the purpose of Williamson's examples, though, we stipulate that there are no such effects.

Qualitative properties are often described as those that can be specified without reference to any particular individual, place, or time (e.g., Hempel and Oppenheim 1948; Francescotti 1999), though Hempel points out that it is not clear which properties can be so specified in a given natural language. (See Cowling 2015 and Hoffmann-Kolss 2018 for further efforts to define 'qualitative'.) Nonetheless, some properties are clearly qualitative, such as the property of a coin that its mass is unevenly distributed, and it is clear how some such properties might influence chances in a coin flip. Other properties are clearly not qualitative but "haecceitistic" (Kaplan 1975) or "impure" (Loux 1978). Suppose for example we name one sequence of coin flips Ximena and another Yasmin. Then Ximena has the property of being Ximena and Yasmin does not. Or, perhaps Ximena has the property of occurring in London, while Yasmin occurs in Tacoma. Matters such as where or when an event takes place are explicitly classified as haecceitistic (Hempel and Oppenheim), and this is defensible because points in space and time are individuals that lack any differentiating qualitative properties.

On the other hand, relationists might deny the very existence of individual points in space and time. They do not regard spatiotemporal relations as held in virtue of specific positions and times, but rather, positions and times as obtuse expressions of relations. But in that case, place and time are clearly extrinsic, concerning not things or events in themselves but their relations to other things or events. Thus, according to IPCh and IPCr, neither place, time, nor relative places or times can matter to probabilities.

Again, Williamson does not state IP or any of our variants. However, his argument appears to presuppose IPCh and IPCr. The events in his examples clearly differ in extrinsic and haecceitistic respects, being distinct events occurring at different times and places, with different inclusion or parthood relations. The intuition driving the argument seems to be that the events and their relevant conditions are *intrinsically*, *qualitatively* identical, and such events should be assigned the same probabilities. Similar principles are widely favoured and argued for elsewhere (Arntzenius and Hall 2003; Schaffer 2003; 2007). Furthermore, IPCh and IPCr have pragmatic significance. A system of chances or credences that depend on extrinsic or haecceitistic factors would be difficult to apply because the discernible, local properties of a system would not be sufficient to determine them. If the value of probabilities is to help us manage uncertainty, they must be somewhat accessible to us, and our variants of IP help to ensure that they are.

A possible objection to IPCh comes from Humean best-systems theories of chance. Lewis (e.g., 1994) thought that chances were just the stochastic values determined by those laws that make up the best theory of the world, where being best consists in some balance of simplicity, strength, and fit with the whole tapestry of "perfectly natural" qualities. One might be tempted to think that, on this kind of account, chances are not determined by intrinsic qualitative properties but by facts quite extrinsic to them, since it is the entire tapestry of natural properties that determines the best system and the chances. The problem of undermining, which threatened and perhaps still threatens Lewis's theory, consists in the observation that even the best theory of the world, if it is at all chancy, will allow for possible events that would make that theory no longer the best (see Ismael 1996 and Hájek 1997, 217 for enlightening discussions).

The important point for us is that, on Lewis's theory, chances do depend counterfactually on extrinsic facts.

However, that kind of dependence on extrinsic facts does not conflict with IPCh nor undermine Williamson's argument. All IPCh requires is that any two qualitatively identical events have the same chance in our *actual* world, not in every possible world. Undermining means that, if, counterfactually, some future runs of coin flips or appropriately related events were to come out differently than they will in the actual world, then the chances of Williamson's runs coming out all heads might be different than they are. But, arguably, those chances would still be the same for all three runs and therefore violate regularity. On Lewis's account, the whole tapestry of qualities determines the *laws of nature*, and the *laws of nature* in turn determine chances. And, historically, the laws of nature determine chances of events based on the intrinsic qualitative properties of those events, and background conditions, in a spacetime-invariant way.[8] It is hard to see how a system of laws that made chances depend on extrinsic and haecceitistic features of events could be a best system, as it would be complicated and hard to apply. Thus, in the relevant sense, Lewis's theory tends to support IPCh.[9] It is not important for

[8] This is essentially the argument for IP′ in Parker 2019. Of course, it is conceivable that the laws of nature might depend on haecceitistic properties or lack the relevant spacetime invariance, but it is at least plausible that they need not. To accept such capricious "laws" as fundamental would amount to saying that the way things behave, including chances, varies for no underlying reason, and it would be hard to accept such a system as the best possible. In any case, we have had considerable scientific success with spacetime invariant, qualitative laws so far and can plausibly continue to do so.

Note also that Arntzenius and Hall (2003) argue, contra Lewis, that uniform dependence on qualitative properties is a compelling requirement of chance and shows that the Principal Principle is not all we know about it. According to Schaffer (2007, footnote 17), Lewis himself considered accepting this argument.

[9] The fact that Lewis himself favoured regular, hyperreal probabilities is no counter-argument, since Lewis was not aware of Williamson's later argument and, to the best of my knowledge, never commented on any symmetry arguments against regularity (but see the preceding note regarding his response to Arntzenius and Hall 2003). Lewis apparently thought that the best system would involve regular, hyperreal chances, but that is a point that Williamson's argument calls into question.

Williamson's argument whether the *laws* are determined by distant extrinsic facts as long as they attach the same probabilities to his three infinite coin flip events.

This is less obvious for other theories of chance that are also subject to undermining. Hoefer (2007), for example, argues that objective chances are not necessarily derived from universal, invariant laws but directly from the mosaic itself,[10] and he even faintly suggests that they may vary over time (570). But it is still at least plausible that any varying chances in the best system could be attributed to changing circumstances. The counterfactual dependence of chances on extrinsic facts is irrelevant; the question is whether the best system will attribute different chances to qualitatively identical events in qualitatively identical local circumstances, and that in itself is already a demerit for such any such system.

Note also that Williamson's events are rather special. A coin flip is one of the paradigmatic examples of a special chance setup (a Stochastic Nomological Machine) that, for Hoefer, guarantees a stable distribution of outcomes. However, Williamson's experiments are *infinite sequences* of coin flips, where the chances of the individual outcomes are identical by hypothesis. What does Hoefer's theory say about such experiments? What it and Lewis's theory both imply, I suggest, is that the applicability of IPCh to such sequences is ultimately a question of theoretical virtue. In the actual world there will probably never be an infinite sequence of coin flips or similar independent, identically distributed trials, so the mosaic provides no direct information about their outcomes. Hence, the choice between a system in which an infinite sequence of heads has chance zero and one in which it has infinitesimal chance is more question of simplicity, strength, and other theoretical virtues than fit, and the latter sort of system has clear theoretical vices: The additional complexity of hyperreal numbers and of dependence on

[10] Thanks to an anonymous referee for raising this point.

extrinsic, haeccetitistic features, and, arguably, arbitrary and potentially misleading excess structure (Parker 2013; 2019; Pruss 2013; 2021a).[11]

Even on a bare frequentist theory, chances are relative to a reference class (the class of all setups to which the chance applies and whose outcomes count towards the relative frequencies that constitute or determine that chance). The chance of an event may depend on actual frequencies across spacetime, but outcomes of setups in the same reference class get the same chance, and a reference class that is delimited by extrinsic or haecceitistic features is of little use. So, again, in this world, IPCh plausibly holds, and Williamsons' argument plausibly succeeds.

IPCh, then, remains a live option, even in the face of undermining. It is attractive in that it makes chances simple and useful: They are straightforwardly determined by intrinsic, qualitative properties. To determine the chance of an outcome, we need not know everything about the larger context, nor the inscrutable identities of the objects, times, and places involved in the experiment.

Now, what about IPCr? Williamson says very little about credence in his 2007; he seems to think it obvious that isomorphic physical events should be given the same credence. One might argue that one simply has no reason to assign different credences to qualitatively identical events, but it is not obvious that extrinsic and haecceitistic facts, such as timing or set inclusion, cannot rationally affect credence. However, we can argue for IPCr from IPCh and the Principal Principle (PP), which essentially says that, other things being equal, if we know the objective chance of an event E on condition C, our credence in E on C should equal the chance. Hence, if chances are determined by intrinsic, qualitative properties, then so are rational credences, unless

[11] Bottazzi and Katz (2020; 2021) have argued against such arbitrariness claims in the context of Robinson- or Nelson-style non-standard analysis. They do not appear to have resolved all arbitrariness worries, and some of their arguments seem to be countered already in Barrett 2010, but we cannot take that up here.

we know something more than the chances. Lewis argues that this principle characterizes the role that a measure on events must play if it is to count as chance (1980; 1994). Hoefer (2007) goes further and attempts to deduce PP from his theory of objective chance, while Pettigrew (2020) offers a kind of Dutch book argument and a pragmatic utility argument for PP.

On the other hand, there is a well known argument that credences should be regular, which might override PP. If we assign an event a strictly zero probability, then no amount of Bayesian updating on evidence can change our minds, which seems unreasonably stubborn (Lewis 1980). But there are also well known replies to that argument (e.g., Easwaran 2014, 8–10). Indeed, there are many arguments *for* regularity that we have not refuted. Our focus here is on Weintraub's specific response to Williamson. All we have tried to establish in this section is that IPCh and IPCr have some appeal and plausibly underly Williamson's argument. Insofar as IPCr is appealing and defensible, the arguments below apply to credence as well as chance, though IPCr's standing may indeed be weaker than that of IPCh.

4. Weintraub's response

Weintraub (2008) points out a difference between H(1…) and H(2…):

> But in fact, Williamson's example shows that isomorphism doesn't preserve *all* basic physical properties. [… A]lthough all the physical properties of the *constituent events* are preserved by the mapping, as are the temporal intervals between adjacent tosses, there is a *global* property (of the complex event) which is *not* preserved. The second sequence is a *proper subset* of the first. So they are not physically identical in a way which would allow us to invoke supervenience and infer that they are equiprobable. (249, Weintraub's emphasis)

Thus Weintraub supposes that the inclusion or parthood relation between H(1…) and H(2…) is a physical property sufficient to account for an infinitesimal difference in probability.

This is why Williamson's second example is significant. Regularity implies that Prob(H*(1…)) must differ from Prob(H(1…)) or Prob(H(2…)), but there is no inclusion relation between H(1…) and H*(1…), nor between H*(1…) and H(2…), so Weintraub's response to the first example does not apply to the second. But Weintraub has an answer to the second as well:

> The *set of temporal points occupied by one sequence* [H*(1…)] is a proper subset of those occupied by the second sequence [H(1…)]. So the two sequences do not share *all* their physical properties. (249)

So, while H(1…) and H*(1…) do not differ in their mutual inclusion or parthood relations, they differ in their *times*, which *do* have an asymmetric inclusion relation.

For Weintraub, this difference is not merely set-theoretic. In personal communication she expands:

> Williamson's two systems are *not* physically identical. One occupies a longer temporal stretch. And surely this kind of temporal difference (as opposed to mere temporal delay) *may* be physically significant. If, for instance, one object is heated for a longer time than is another, we may well expect other differences.[12]

So the difference in times brings with it a very physical difference in duration. Of course, by any standard measure, H(1…), H*(1…), and H(2…) all have the *same* duration, namely infinite. But supporters of regularity sometimes ally with new sorts of measures and theories of number in

[12] Weintraub made this remark several years ago in an informal context. She should not be held accountable for it, but the suggestion it makes is important to consider here.

which a set is always larger than any of its proper subsets (Benci and Di Nasso 2003; Benci, Horsten, and Wenmackers 2013; 2018). It is a small step from this to an alternative measure of duration such that H(1…) is longer than H(2…) or H*(1…).

5. Haverkamp and Schulz on Weintraub

Haverkamp and Schulz (2011) argue that, if Weintraub is right that Prob(H(1…)) < Prob(H*(1…)), then by the same reasoning, "*two runnings of the same chance device* are not governed by the same probabilities" (398, their emphasis).[13]

To show this, Haverkamp and Schulz consider two coins. In order to make their point, they need each coin to undergo two separate infinite sequences of flips, so they consider supertasks in which a coin flip is executed at shorter and shorter intervals, yielding infinitely many results in finite time. We begin flipping Coin 1 first, and then commence flipping Coin 2 simultaneously with Coin 1's second flip. In order for Weintraub's argument to apply, the flips must stay synchronised as they accelerate. Thus the flips of Coin 2 again occur at times that are a proper subset of the times when Coin 1 is flipped, and indeed, in this case, the sequence of flips of Coin 1 will be uncontroversially longer in duration than that of Coin 2. In another experiment, after the first two sequences of flips have ended, Coin 2 starts flipping first, Coin 1 joins in when Coin 2 produces its second outcome, and again they remain synchronized and finish simultaneously. Now suppose that the chance of Coin 1 coming up all heads is the same for both sequences of flips with that coin. By Weintraub's reasoning, Haverkamp and Shulz say,

[13] Actually, we could say this about Williamson's original story as well. H(2…) is after all an outcome of a second run of qualitatively the same experiment with the very same coin as that in H(1…). It just happens also that the experiment of H(1…) properly includes that of H(2…), which gives Weintraub her foothold to argue that the events are physically different. Nonetheless, it is still a repeat of (qualitatively) the same experiment with (numerically) the same device.

the probability that Coin 2 comes up all heads on its first run is slightly greater than the probability that Coin 1 does. But by the same reasoning, the probability that Coin 2 comes up all heads on its second run is slightly *less* than the probability that Coin 1 does. Hence, if the probability of an all-heads outcome is the same for both runs with Coin 1, it is not the same for both runs with Coin 2.

But why is this a problem for the regularist? Why should the chance of producing all heads *not* vary over these experiments? While Weintraub argues that Williamson's experiments differ, Haverkamp and Schulz's experiments differ even more. The first run of flips of Coin 1, in their story, is uncontroversially shorter in duration than, and not entirely simultaneous with, that of Coin 2, while the second run of flips of Coin 2 is shorter than and not entirely simultaneous with that of Coin 1. Hence, for at least one of these coins, the first and second run must have different finite durations, and on Weintraub's view, it seems, a difference in duration is sufficient for a difference in chance. In any case, the runs here are not metrically isomorphic, so we no longer have as much reason to expect the probabilities to be the same. In Williamson's example, as Weintraub observes, "the physical properties of the constituent events are preserved by the mapping, *as are the temporal intervals between adjacent tosses*" (my emphasis). In Haverkamp and Schulz's example, this is not the case. Turning Williamson's infinite sequences into finite-time supertasks disrupts the symmetry between the events and thus undermines the argument for equiprobability.

Hence, Haverkamp and Schulz's critique of Weintraub is inconclusive, and not effective against one such as Weintraub who regards nearly any physical difference between events as sufficient for a difference in chance.

6. Differences that make a difference

The real problem with Weintraub's response is that it overlooks the implicit reasoning behind Williamson's argument. Williamson's events H(1…), H(2…), and H*(1…) are designed to share the same intrinsic, qualitative circumstances. He imagines three infinitely long experiments, each conducted in qualitatively the same way under the same intrinsic, qualitative conditions. The asymmetric differences of inclusion, parthood, and timing between these experiments are clear, and it is equally clear that Williamson does not regard such differences as relevant. He expresses this sort of view when he writes, "That H(2...) is preceded by another toss is irrelevant, given the independence of the tosses", and, "[T]he probability that a coin comes up heads on every toss does not depend on when one starts tossing," and again, "[T]hat the first coin will be tossed once before the H(2...) sequence begins is irrelevant" (175). Weintraub describes the differences between the events as physical, but even if granted, that is not sufficient. Williamson does not suppose that there are *no* physical differences between his events, but that there are none that make a difference.

The differences that do matter on this view are intrinsic and qualitative. Relations to other events, such as other coin tosses, are stipulated not to have a causal influence on the experiments in Williamson's examples and therefore to be irrelevant to the chances. Likewise, matters of bare identity are supposed irrelevant. The coins in H(1…) and H*(1…), for example, are not the very same coin, but if they are alike in every way then they should be associated with the same probabilities.

Weintraub's differences, as stated in 2008, concern relations of set inclusion, either between the coin tosses themselves or between the times they occupy. Her argument turns, not on the claim that H(1…) has some unary property that H*(1…) lacks or vice versa, but on the claim that there are asymmetric relations between them. Hence, the differences are extrinsic from the outset. But they are also haecceitistic rather than qualitative. Say that a relation (rather than a property) is purely qualitative if it holds between relata in virtue of the relata's purely qualitative properties alone, and otherwise it is haecceitistic. Set inclusion, I claim, is a haecceitistic one. A set A is a subset of B if and only if B contains the very same individuals as A (and possibly more). Whether this is so does not depend at all on the qualitative properties of the sets or their elements, but only on which particular individuals are in A and B. Hence, set inclusion, on its own, is not the sort of thing that can make a difference in chance or credence, on the reasonable view suggested by Williamson's argument.

The differences between Williamson's events can also be expressed in terms of mereology rather than set theory. Event H(2…) arguably forms a part of H(1…) and not vice versa, while the sequence or mereological sum of times occupied by H*(1…) is part of that occupied by H(1…). But parthood too seems to be a haecceitistic relation rather than a qualitative property. The individual coin flip outcomes that constitute H(2…) do so in virtue of *which* individual events they are, not *how* they are. The first heads outcome of H(1…), after all, is qualitatively identical to the rest, yet is not part of H(2…).

The relative positions and times of events are also haecceitistic relations. They hold in virtue of where or when the events occur, and, as argued above, such properties are haecceitistic because points in space or time are qualitatively undifferentiated individuals. On a relationist view of space and time, there are fundamentally no such points; positions and times are then relational and hence extrinsic.

So Weintraub is right that there are differences between H(1…), H(2…), and H*(1…), and we may even call them physical differences, but on the reasonable view underlying

Williamson's argument, they are not the kinds of differences that make any difference to the chances of those events, and if we know those chances, they should not make any difference to credences either, by PP. The differences in inclusion, parthood, and timing between H(1…), H(2…), and H*(1…) are all extrinsic as well as haecceitistic. Our IPCh and IPCr assert that relevant differences must be both intrinsic and qualitative, but either restriction on its own would suffice to disqualify these differences.

On the other hand, Weintraub's remarks about duration, quoted above, suggest that we *can* see the difference between H(1…) and H(2…) or H*(1…) as an intrinsic, qualitative difference, just as heating a rod for ten minutes is intrinsically, qualitatively different from heating it for nine. However, this view depends on a very non-standard conception of duration, under which two countably infinite sequences, or two one-way infinite continuous intervals, which are not only equal in cardinality but also *isomorphic* under the standard metric, can nonetheless differ in length. I have noted in Section 3 that such a notion of duration is not far from some new conceptions of number and measure. These are so-called Euclidean measures, under which proper inclusion implies a difference in size. But such conceptions are controversial and quite non-standard, and they suffer from difficulties similar to those affecting regular probabilities, namely that they assign different sizes to qualitatively identical sets. This makes them partly arbitrary and inhibits their usefulness (Parker 2013). If we wish to adopt a similarly Euclidean notion of duration, we are faced with this problem: Exactly which durations will we assign to H(1…) and H*(1…)? Suppose we assign them infinite hyperreal durations x and $x - 1$ seconds, respectively. We might just has well have assigned $x - 1$ to H(1…) and $x - 2$ to H*(1…). There is nothing to determine which particular hyperreal we should assign to which stretch of time. The most we could say non-arbitrarily is that H(1…) lasts one second longer than H*(1…). But if that *relation* implies that H(1…) has smaller probability than H*(1…), then again the probabilities are partly determined by extrinsic considerations, contrary to IPCh and IPCr.

This criticism of Weintraub's appeal to duration is not conclusive. Perhaps we could overcome the arbitrariness issues, or live with them, and adopt a non-relational notion of duration under which some eternities are longer than others. But the move is dubious, and we can form further, stronger rebuttals to Weintraub by further varying the examples.

7. Relativity

Even if we are willing to accept a notion of duration under which some eternities are longer than others, with all that entails, it is not always clear which eternity is longer. Williamson and Weintraub both seem to assume a non-relativistic framework in which times and simultaneity are absolute. However, in a more realistic relativistic setting, the inclusion relation between the times of H(1…) and H*(1…) only holds from one inertial reference frame. The corresponding flips in H(2…) and H*(1…) are space-like separated, so that for one inertial observer, H(2…) and H*(1…) are simultaneous, but for another they are not. For the second observer, the flip times of H*(1…) are *not* included in the flip times of H(1…), and hence Weintraub's argument does not apply.

Let us specify our experiments more precisely. Suppose that for an observer in an inertial reference frame F_1 in Minkowski spacetime, an eternal sequence A of coin flips A_1, A_2, A_3, … occur in the same place at, say, ten second intervals, while a sequence B of flips B_1, B_2, B_3, … occur at the same times in frame F_1, but so far from A that flips A_1 and A_2 are both spacelike separated from B_2. The flips are all qualitatively identical with respect to properties relevant to the probabilities, though still chancy and independent with probability ½ of heads, as in Williamson's examples. Relative to frame F_1 and to the intrinsic structure of Minkowski spacetime, the experiments A and B are perfectly symmetric. Let HA(n…) denote the event that each flip A_n, A_{n+1}, A_{n+2}, … – i.e., each flip after A_{n-1} – comes up heads, and similarly for HB(n…).

Following Williamson, we can now argue as follows: Events HA(1…) and HB(2…) are quantitatively identical and have no asymmetric inclusion relation; they are disjoint. Therefore, they should have the same probability. Likewise, HB(2…) and HA(2…) are qualitatively identical and disjoint, so they should have the same probability. By transitivity, Prob(HA(2…)) = Prob(HA(1…)) = ½ × Prob(HA(2…)), so Prob(HA(2…)) = 0 and regularity fails.

Following Weintraub, one can object that HA(1…) and HB(2…) are not alike in every way, since the times at which flips B_2, B_3, B_4, … occur are properly included in the times of A_1, A_2, A_3, …, and not vice versa. Thus, on a nonstandard "Euclidean" notion of duration, HA(1…) is longer in duration than HB(2…) and thus intrinsically and qualitatively different from HB(2…). But from another reference frame, these events are simultaneous and qualitatively alike; each is just a spatial translation of the other, with exactly the same duration, even on a Euclidean notion of duration. Weintraub's response might work for observers in frame F_1, if we allow her conception of duration, but not in F_2.

One option now open to the regularist is to suppose that durations and chances themselves are frame-relative. In that case, Weintraub's argument can still be made for some observers, namely those at rest in F_1, while for other observers, one can give a different version of her argument. For example, if HB(2…) is simultaneous not with HA(2…) but with HA(1…) for a given observer, Weintraub could argue that, for that observer, HB(2…) is less likely than

HA(2…), because the times of the former properly include those of the latter, and hence the argument by transitivity that Prob(HA(1…)) = Prob(HA(2…)) again fails. Intermediate cases are trickier; if HB(2…) is not simultaneous with any event HA(*n*…) for a given observer, then Weintraub cannot justify a difference in probability in terms of an inclusion relation, but she might still claim that the event that starts later for a given observer has shorter duration and therefore greater probability for that observer.

But to suppose that objective chances are frame-relative is quite awkward. It makes them much less objective in the sense that they depend on the observer's velocity. This is hard to make sense of. When one event has greater chance than another, it is supposed to be more likely to occur. But the actual occurrence of HA(1…) and the like is *not* frame-relative. HA(1…) occurs for one observer in our spacetime if and only if it occurs for all. In what sense then can it be objectively more likely for one observer than another? To hold that it is would be rather like holding that a random number is more likely to be even than to be divisible by two. These events are equally likely regardless of the observer's perspective, because they are in fact the same event.

Let us amplify this insight with a more elaborate example. Suppose that continuum-many coin flip sequences like our sequence A actually occur, all simultaneously in reference frame F_1. The sequences are infinitely long and qualitatively uniform as in Williamson's examples, and, as it happens, seven of these sequences come out all heads. An equally large continuum of infinite flip sequences like our sequence B also occur, with the second flip of each sequence in this second continuum spacelike separated from the first two flips of the sequences in the first continuum. Again, let's say seven sequences in the second continuum come out all heads.

In frame F_1, the sequences of coin flips are simultaneous, while in frame F_2, they are not; the sequences in the second continuum start later for observers in F_2. Now suppose two observers assign regular probabilities to all of the outcomes of infinite coin flips, including events in which a sequence comes out all heads. Observer 1, in frame F_1, assigns the same probability to 'all heads' for the sequences in both continua. Observer 2, in F_2, assigns higher probability to 'all heads' for sequences in the second continuum than those in the first – twice as high – due to the latter sequences starting earlier and having longer duration in some Euclidean sense. Then Observer 1's probabilities better fit the actual outcomes than Observer 2's, and we can contrive betting situations (involving goods with hyperreal utilities) where Observer 1 would consequently fare better. If there had been 14 outcomes of all heads in the second continuum, then Observe 2's probability would be a better fit, but in either case, one is a better fit than the other.

Strictly speaking, the facts in this example are compatible with both of the observers' probability distributions and many others. Probabilities need not match the frequencies exactly, so neither Observer 1 nor Observer 2 is necessarily wrong, but it is hard to see how they can both be right if they assign different probabilities when confronted with the same outcomes and frequencies. If such experiments are multiplied many times across the Humean mosaic with similar outcomes, then a Humean best system, or for that matter, any good theory of chance, ought to assign the same chances to those outcomes for all observers in all reference frames, since the actual outcomes are the same for all observers in all reference frames.

Of course, it is always possible to bite the bullet and accept frame-relative chances. But the pain that a bullet biter must suffer here is this: Different observers must assign different probabilities to the same events in light of the same actual outcomes. If regularity is to be defended, the debate should address the acceptability of such consequences. However, in the next section, we will see a final example where even an appeal to duration offers the regularist no escape.

8. Bounded, disjoint events

Even if we are prepared to accept such relativity of chance, and that mere differences in inclusion or infinite duration make a difference to probabilities, we can give another symmetry argument against which those defences are ineffective. There are examples of qualitatively identical events which must differ in chance under regularity but which do not even have the sorts of differences that Weintraub points to.

We will construct an example of an event such that, if regularity holds, a mere translation of this event in space or time must have a different probability. What is more, the event and its translation take place in a finite, bounded region of spacetime, and neither includes or even overlaps the other, nor do their times and places. This is similar to examples discussed elsewhere (Bernstein and Wattenberg 1969; Barrett 2010; Parker 2013; 2019; Pruss 2013; 2021b), but in those versions, regularity implies failures of invariance under rotations and what we might call *modular* translations, where the events are still related by inclusion. Here the events are related by an ordinary translation and entirely disjoint.[14]

For exposition, however, we begin with modular translations. Let us take as our sample space the half open interval $[0, 1)$.[15] Say a *translation mod 1* is a transformation T on $[0, 1)$

[14] Pruss (2021b) gives a similar mathematical example (p. 9) and an enlightening general theorem (p. 8), but does not take up questions of inclusion, intersection, or physical examples.

[15] We can think of this interval either as an abstract sample space, or as a physical interval in space or time. In the applications mentioned below, we can use the abstract mathematical interval $[0, 1)$ to represent the physical one, or the physical interval itself can serve as the relevant sample space, provided space and time are continuous.

equivalent to first performing a translation and then taking the fractional part of the result. That is, for some $c \in [0, 1)$,

$$Tx = x + c \ (\text{mod } 1) = \begin{cases} x + c, & \text{if } x + c < 1, \\ x + c - 1, & \text{otherwise.} \end{cases}$$

So a translation mod 1 is, so to speak, a "piecewise translation" made up of two translations: T_1, mapping $[0, 1 - c)$ rightwards to $[c, 1)$, and T_2, mapping $[1 - c, 1)$ leftwards to $[0, c)$.

In general, translations mod 1 fail to preserve regular probabilities. Suppose c is irrational. Then the points $T^n 0$ never coincide for different whole numbers n. Now let $X = \{T^n 0: n \in N\}$. Then $TX = \{T^n 0: n \geq 1\}$ is a proper subset of $X = \{0\} \cup TX$. If Prob is a regular finitely additive probability function over sets including $\{0\}$, X, and TX, then

$$\text{Prob}(X) = \text{Prob}(\{0\}) + \text{Prob}(TX) > \text{Prob}(TX).$$

Thus, translations mod 1 defined on such sets do not preserve regular probabilities.

Translations mod 1 are not translations per se, but the fact that translations mod 1 do not preserve regular probabilities implies that true translations do not either. Assume Prob is regular and assigns values to the sets X, TX, and $[0, 1 - c)$, as well as intersections and complements of these sets. Let $X_1 = X \cap [0, 1 - c)$ and $X_2 = X \cap [1 - c, 1)$. Then T_1X_1 and T_2X_2 are disjoint. Since $\text{Prob}(X) \neq \text{Prob}(TX)$, it follows by additivity that either $\text{Prob}(X_1) \neq \text{Prob}(T_1X_1)$ or $\text{Prob}(X_2) \neq \text{Prob}(T_2X_2)$. So at least one of the translations T_1 or T_2 fails to preserve Prob. Thus translations do not preserve regular probabilities that are defined on simple sets like X_1 and X_2.

Notice that T_1X_1 is not a subset of X_1 and T_2X_2 is not a subset of X_2. In fact, if Prob is defined on sufficiently small intervals in $[0, 1)$ as well as the set X, then we can split X up into finitely many smaller sets X_i such that each TX_i is *disjoint* from X_i. By finite additivity, there is at least one such set X_i for which $\text{Prob}(X_i) \neq \text{Prob}(TX_i)$.[16] So if a regular probability measure is defined over sufficiently small intervals (as probabilities on continuous spaces normally are), then for any translation T there are disjoint sets A and TA that differ in probability. This inequity cannot be made more palatable by pointing out that TA is a proper subset of A, because it is not.

Hence, on the regularist view, it is impossible to choose a random number in the interval so that no set is privileged over any of its disjoint translations. We cannot throw a dart at a rectangular dartboard in such a way that it is as likely to hit a point with x-coordinate in a set A as in a disjoint translation TA. Likewise, if quantum fluctuations occur in some otherwise vacuous region, then for the regularist there will be bounded sets A of points such that a fluctuation is slightly more likely to occur at a point in A than in certain disjoint translations of A, and similarly there will be bounded sets B of *times* such that a fluctuation is more likely to occur at a time t in B than in certain disjoint translations of B.

These asymmetries are implausible. The events in question are qualitatively identical. They differ only in where or when they occur. Thus, on the reasonable view that chances supervene on intrinsic, qualitative circumstances, an event E and a mere translation TE must have the same chance, and regularity must fail. Here, Weintraub cannot claim that there is a significant difference between the events consisting in their inclusion or parthood relations, or in such relations between their times or places, because neither the two events in question, nor their times or places, are so related. Nor can she claim that the events have different durations or lengths, or any other intrinsic, qualitative difference. Each event consists in a single occurrence within a specified set of points in space or time, and these point sets are mere translations of each

[16] More formally, let T be a translation $Tx = x + c$. Suppose $A, TA \subseteq [0, 1)$ and $\text{Prob}(A) \neq \text{Prob}(TA)$. Choose $n \in \mathbf{N}$ so that $1/n \leq c$. For each whole number $i < n$, let $A_i = A \cap [i/n, (i + 1)/n)$. Then A_i and TA_i are disjoint, and by finite additivity, $\sum_{i \in \{0, 1, \ldots, n-1\}}\text{Prob}(A_i) = \text{Prob}(A) \neq \text{Prob}(TA) = \sum_{i \in \{0, 1, \ldots, n-1\}}\text{Prob}(TA_i)$. So for at least one i, $\text{Prob}(A_i) \neq \text{Prob}(TA_i)$. Hence there is a set A_i and a disjoint translation of A_i that differ in probability.

other, with the same structure, extension, and finite radius. They do have *physical* differences, namely differences in time or place, but those are precisely the kinds of differences that should not matter to chances, on the view implicit in Williamson's argument.

9. Conclusion

Williamson's coin flip argument and the variations discussed here show that, if regularity holds, then qualitatively identical events must have different probabilities. But if probabilities are determined by intrinsic, qualitative properties, this cannot be so. Thus, the regularist must deny the latter, which is a heavy price to pay for the debatable virtue of regularity.

Weintraub's published response seems to overlook the plausible background assumptions of Williamson's argument. She points out that Williamson's events have physical differences but ignores the fact that they are not intrinsic, qualitative differences. They are differences of time, place, and bare identity only—precisely the kinds of differences that, on the view that Williamson's argument suggests, should not make any difference to probabilities. To this Weintraub could reply that the events in question differ in duration and durations is an intrinsic, qualitative property. The fact that such a position entails a radically nonstandard and problematic notion of duration is not enough to conclusively refute it.

However, in a relativistic setting, the asymmetric relations of inclusion between the times of Williamson's events H(1…) and H*(1…) and the ostensible differences in duration are frame-relative. If such differences are sufficient for a difference in chance, then "objective" chances are decidedly relative, depending on the velocity of an observer with respect to the events. This is difficult to make sense of, given that the events in question either occur for all observers or for none. To maintain Weintraub's position, different observers must assign different probabilities to the same events despite the same actual outcomes.

But worst of all for the regularist, there are other examples where Weintraub's response simply does not fit. We saw a construction of events – involving random numbers, dart throws, or quantum vacuum fluctuations – that are qualitatively identical, entirely disjoint in time and space, and of equal duration and extension, but which must differ in probability if regularity holds. Thus our claim is vindicated: If chances are regular, then they are not determined by intrinsic, qualitative circumstances, and if rational credences are regular, they do not track such supervenient chances. This does not spell the end for regularists but illustrates the costs of their commitments. Further debate over regularity will have to assess such costs against the arguments in favour.

Acknowledgements

I would like to thank Dimitrios Athanasiou, Mark Baker, Kenny Easwaran, Craig Fox, Marie Gueguen, Niels Linnemann, Yichen Luo, Chris Smeenk, the audience at PSA2018, and three anonymous referees for helpful comments and discussion. This work was partly supported by the John Templeton Foundation under grant #61048.

References

Arntzenius, Frank and Ned Hall. 2003. "On What We Know About Chance." *British Journal for the Philosophy of Science* ***54***: 171–9.

Barrett, Martin. 2010. "The Possibility of Infinitesimal Chances." In *The Place of Probability in Science*, eds. Ellery Eells and J.H. Fetzer, 65–79. Dordrecht: Springer.

Benci, Vieri, Leon Horsten, and Sylvia Wenmackers. 2013. "Non-Archimedean Probability." *Milan Journal of Mathematics **81***: 121–151.

----. 2018. "Infinitesimal Probabilities." *British Journal for the Philosophy of Science **69***: 509–552.

Benci, Vieri, and Mauro Di Nasso. 2003. "Numerosities of Labeled Sets: A New Way of Counting." *Advances in Mathematics **173***: 50–67.

Bernstein, Allen R., and Frank Wattenberg. 1969. "Non-standard Measure Theory." In *Applications of Model Theory to Algebra, Analysis, and Probability*, ed. Wilhelmus A. J. Luxemburg, 171–85. New York: Holt, Rinehard and Winston.

Bottazzi, Emanuele, and Mikhail G. Katz. 2020. "Internality, Transfer, and Infinitesimal Modeling of Infinite Processes." arXiv:2008.11513v1

----. 2021. "Infinite Lotteries, Spinners, Applicability of Hyperreals." *Philosophia Mathematica* ***29***: 88–109.

Carnap, Rudolf. 1950. *Logical Foundations of Probability*. Chicago: University of Chicago Press.

----. 1963. "Replies and Systematic Expositions." In Schilpp (1963), 859–1013.

Cowling, Sam. 2015. "Non-qualitative Properites." *Erkenntnis* ***80***, 275–301.

Francescotti, Robert. 1999. “How to Define Intrinsic Properties.” *Noûs* ***33***: 590–609.

Hájek, Alan. 1997. ‘“Mises Redux” - Redux: Fifteen Arguments Against Finite Frequentism.’ *Errkenntnis* ***45***: 209–227.

----. Unpublished. “Staying Regular?” http://hplms.berkeley.edu/HajekStayingRegular.pdf

Haverkamp, Nick, and Schulz, Moritz. 2012. “A Note on Comparative Probability.” *Erkenntnis* ***76***: 395–402.

Hempel, Carl G., and Oppenheim, Paul. 1948. “Studies in the Logic of Explanation.” *Philosophy of Science* ***15***: 135–175.

Hoefer, Carl. 2007. “The Third Way on Objective Probability: A Sceptic's Guide to Objective Chance.” *Mind* ***116***: 549–596.

Hoffman-Kolss, Vera. 2019. “Defining Qualitative Properties.” *Erkenntnis* ***84***: 995–1010.

Hofweber, Thomas. 2014. “Infinitesimal Chances.” *Philosopher's Imprint* ***14***: 1–34.

Howson, Colin. 2017. “Regularity and Infinitely Tossed Coins.” *European Journal for Philosophy of Science* ***7***: 97–102.

----. 2019a. “A Better Way of Framing Williamson's Coin-Tossing Argument, but It Still Does Not Work.” *Philosophy of Science* ***86***: 366–374

----. 2019b. “Timothy Williamson's Coin-Flipping Argument: Refuted Prior to Publication?” *Erkenntnis* online: https://doi.org/10.1007/s10670-019-00130-5

Humberstone, I.L. 1996. “Intrinsic/Extrinsic.” *Synthese* ***108***: 205–267.

Isamel, Jennan. 1996. "What Chances Could Not Be." *British Journal for the Philosophy of Science* ***47***: 79–91.

Kaplan, David. 1975. "How to Russell a Frege–Church." *Journal of Philosophy* ***72***: 716–729.

Kemeny, John G. 1955. "Fair Bets and Inductive Probabilities." *Journal of Symbolic Logic* ***20***: 263–73.

----. 1963. "Carnap's Theory of Probability and Induction." In Schilpp (1963), 711–38.

Lewis, David K. 1980. "A Subjectivist's Guide to Objective Chance." In *Studies in Inductive Logic and Probability, v. II*, ed. R.C. Jeffrey, 263-93. Berkeley and Los Angeles: University of California Press.

----. 1994. "Humean Supervenience Debugged." *Mind* ***103***: 473–490.

Loux, Michael J. 1978. "Bare Substrata." In: *Substance and Attribute*. Philosophical Studies Series in Philosophy Vol. 14. Dordrecht: Springer.

Parker, Matthew W. 2013. "Set Size and the Part–Whole Principle." *Review of Symbolic Logic* ***6***: 589–612.

----. 2019. "Symmetry Arguments Against Regular Probability: A Reply to Recent Objections." *European Journal for Philosophy of Science* ***9***: 8. https://doi.org/10.1007/s13194-018-0229-1

Pettigrew, Richard. 2020. *Dutch Book Arguments*. Cambridge: Cambridge University Press.

Pruss, Alexander. 2013. "Null Probability, Dominance and Rotation." *Analysis* ***73***: 682–85.

----. 2021a. “Underdetermination of Regular Probabilities.” *Synthese* ***198***: 777–799.

----. 2021b. “Non-classical Probabilities Invariant Under Symmetries.” arXiv:2010.07366v4, last updated 29 January, 2021.

Schaffer, Jonathan. 2003. “Principled Chances.” *British Journal for the Philosophy of Science* ***54***: 27–41.

-----. 2007. “Deterministic Chance?” *British Journal for the Philosophy of Science* ***58***: 113–40.

Schilpp, Paul Arthur, ed. 1963. *The Philosophy of Rudolf Carnap*, The Library of Living Philosophers Vol. XI. Chicago: Open Court.

Skyrms, Brian. 1980. *Causal Necessity: A Pragmatic Investigation of the Necessity of Laws*. New Haven and London: Yale University Press.

Weintraub, R. 2008. “How Probable is an Infinite Sequence of Heads? A Reply to Williamson.” *Analysis* ***68***: 247–50.

Wenmackers, Sylvia, and Leon Horsten. 2013. “Fair Infinite Lotteries.” *Synthese* ***190***: 37–61.

Williamson, Timothy. 2007. “How Probable is an Infinite Sequence of Heads?” *Analysis* ***67***: 173–80.