The implementation, interpretation, and justification of likelihoods in cosmology

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Abstract

I discuss the formal implementation, interpretation, and justification of likelihood attributions in cosmology. I show that likelihood arguments in cosmology suffer from significant conceptual and formal problems that undermine their epistemic credentials.

1 Introduction

In the past several decades a variety of probabilistic arguments (or, more generally, arguments based on "likelihoods") have increasingly been made in cosmology—one the face of it, a discipline that appears to be an unlikely home for such reasoning. There is, after all, only one universe (so far as we know), so it would seem to be an almost pointless exercise to attribute probabilities to the universe, its creation, or its particular history. Nevertheless, perhaps owing to the significant observational limitations that exist in cosmology—we are, in short, confined to only a very small corner of the cosmos—cosmologists have based important and influential arguments on the essential use of probabilistic assumptions, assessments of likelihood, notions of typicality, and so forth.

The widespread acceptability of such arguments and their basic presuppositions are taken for granted in cosmology:

> Cosmologists often want to make such statements as "almost all cosmological models of a certain type have sufficient inflation," or "amongst all models with sufficient baryon excess only a small proportion have sufficient fluctuations to make galaxies." Indeed one popular way of explaining cosmological observations is to exhibit a wide class of models in which that sort of observation is "generic." Conversely, observations which are not generic are felt to require some special explanation, a reason why the required initial conditions were favoured over some other set of initial conditions." (Gibbons et al., 1987, 736)

It is not hard to find other cosmologists expressing similar ideas.[1]

One of the more prominent applications of such likelihood-based reasoning in contemporary physics is in the context of so-called "fine-tuning" problems. In cosmology fine-tuning problems with the old standard model of cosmology, the hot big bang (HBB) model, motivated the introduction of the theory

[1] "The problem of constructing sensible measures on the space of solutions is of undeniable importance to the evaluation of various cosmological scenarios" (Gibbons and Turok, 2008); "...the measure could play an important role in deciding what are the real cosmological problems which can then be concentrated on. In other words, we assume that our Universe is typical, and only if this was contradicted by the experimental data would we look for further explanations" (Coule, 1995, 455-6); "Some of the most fundamental issues in cosmology concern the state of the universe at its earliest moments, for which we have very little direct observational evidence. In this situation, it is natural to attempt to make probabilistic arguments to assess the plausibility of various possible scenarios (Schiffrin and Wald, 2012, 1).

of cosmological inflation to solve them (Guth, 1981). In high energy physics the failure of naturalness in the standard model of particle physics, known as the hierarchy problem, is often described as a fine-tuning problem as well (Williams, 2015). Although these problems are conceptually distinct, one finds them regularly interpreted in probabilistic language; in some sense (usually unspecified) the fine-tuning is thought to be unlikely, improbable, etc.

In cosmology the two most salient fine-tuning problems in recent history are the flatness problem and the horizon problem. The problems begin with observations which suggest that the universe is remarkably uniform at large scales and has a spatial geometry very close to flat. In the context of the old standard model of cosmology, the hot big bang (HBB) model, these conditions require very special initial conditions: an extraordinary degree of uniformity and flatness. If the conditions at the beginning of the universe were ever so slightly different than these initial conditions, the universe would be nothing at all like what it is now. The fine-tuning of the HBB model, then, is taken to be precisely this specialness of initial conditions.

How should one interpret the flatness and horizon problems, and does inflation solve them? Indeed the most intuitive characterization of fine tuning complaints is the one already mentioned, i.e. this fine-tuning is in some sense unlikely. What makes fine-tuning problematic, given this rendering, is never made especially clear, but one might think that unlikely circumstances are either simply unlikely to be true or perhaps that such circumstances lack explanatory power (making such fine-tuning problems explanatory problems). Physicists are often easily read as holding this interpretation, and some philosophers have considered interpretations along these lines as well (Earman, 1995; Earman and Mosterín, 1999; Smeenk, 2013; McCoy, 2015). The philosophical analyses made so far have not focused on assessing the case for the "likelihood" interpretation and are accordingly somewhat less than thorough. My aim in this paper is to provide a rather more detailed evaluation of the prospects of providing some such interpretation of fine-tuning.

For the fine-tuning arguments which support inflationary theory to be successful, there are certain specific claims which require support (Ellis, 1988; Coule, 1995). First, it must be shown that the uniform and flat spacetimes underlying the HBB model are indeed unlikely, i.e. that they are in fact unlikely. Second, it must be shown that inflating cosmologies generically lead to uniformity and flatness. Third, it must be shown that inflating cosmologies themselves are not unlikely, or that they are at least more likely than the special HBB spacetimes. The success of all of these tasks presupposes that there is a justified way of assessing the likelihoods of cosmological models (Gibbons et al., 1987; Hawking and Page, 1988) and some reasonable way of interpreting these likelihoods.

On the face of it, there exist various formal implementations of likelihood notions which one could use to supply the required support, e.g. via topology, measure theory, probability theory, etc. Cosmologists have generally favored those that are similar to the application of likelihoods in statistical mechanics, a context where they are generally acknowledged as successful. Simply inferring from the success of those arguments in statistical mechanics to similar ones in cosmology presupposes, however, that the justification and interpretation of likelihoods in statistical mechanics appropriately carries over to the cosmological context. I will argue that this presupposition is incorrect. Indeed, a central claim defended in this paper is that the justification and interpretation of cosmological likelihoods cannot be secured by similar strategies used to justify and interpret the use of likelihoods in statistical mechanics. Since the attempts to secure them ostensibly rely on precisely these strategies, they ultimately fail to ground the arguments for an inflationary solution to the fine-tuning problems of the HBB model.

Although investigating a variety of formal implementations of likelihood notions would be of interest, for reasons of simplicity, familiarity, and relevance to the discussions in the literature, I will concentrate mostly on probabilistic measures of likelihood.[2] Recall that an application of probability theory stan-

[2]At times I will generalize my arguments to other approaches (especially topological ones), but to maintain a reasonable scope my approach is not entirely systematic. I only remark on other approaches when the considerations raised for probability measures transparently apply to them as well or when they are importantly relevant to some argument I make.

dardly requires three things: a set X of possible outcomes (the "sample space"), a σ-algebra $\mathcal{F}$ of these possible outcomes (a collection of subsets that is closed under countable set-theoretic operations), and a probability measure P that assigns probabilities to elements of $\mathcal{F}$.[3] The probability spaces which are of interest are those whose sets of possible outcomes are sets of possible cosmologies. Since the success of the cosmological fine-tuning arguments depends on an adequate justification of the relevant probability space and an adequate interpretation of probability in this context, I take as conditions for a cosmological probability space to have physical significance that the choice of X and P must be justifiable and physically interpretable. (I take it that $\mathcal{F}$ can be chosen on essentially pragmatic grounds.)

The plan of the paper is as follows. In §2 I consider general conceptual issues of probability measures in cosmology, including the specification of the appropriate reference class X, and the interpretation and the justification of the probability measure P. The main conclusions of this section are that implementing cosmological probabilities by analogy to the implementation of statistical mechanical probabilities, as is done in the most prominent attempts, can only be understood as an assignment of probabilities to initial conditions of the universe and, more crucially, that there is no acceptable justification for any particular probability measure in the context of (single universe) cosmology. I then investigate the potential for formally implementing a measure associated with the space of possible cosmologies permitted by the general theory of relativity in §3. I point out a variety of significant technical obstacles to providing any such measure. One can avoid (or at least ignore) most of these general issues by truncating the spacetime degrees of freedom so that the relevant probability space is finite-dimensional. This is the approach taken to define the most discussed measure, the Gibbons-Hawking-Stewart (GHS) measure (Gibbons et al., 1987). In §4 I argue that even setting aside the problems raised in §§2-3 there are serious interpretive and technical problems with taking this narrower approach, in particular for supporting the fine-tuning arguments in the three ways mentioned above. I offer concluding remarks in §5.

2 General Conceptual Problems

In this section I discuss a few significant conceptual issues that stand in the way of establishing a likelihood measure on the space of possible cosmologies. These general issues concern the choice of an appropriate reference class of cosmologies to serve as the sample space, and the justification and interpretation of a specific measure associated with the sample space.

The basic issue which makes applying probability theory to cosmology difficult is an obvious one, but one which should be explicitly noted at the outset: there is, so far as we know, only one universe. The uniqueness of the universe has long been recognized as a problem for cosmology as a science, however its significance has often been overstated.[4] Thus it is necessary to draw the argument out in some detail in order to avoid an overly hasty conclusion. In the end, though, I do conclude that the point is decisive. There is simply no physical content to be found in the addition of cosmological probabilities to classical, single-universe cosmology.[5]

[3]Similarly, a topological space is specified by a set X (of possible spacetimes in this context) and a topology on X, i.e. a collection of subsets of X (the "open" sets). With a topology on X one can define a suitable notion of "negligible set" in the topology on X, for example a set whose closure has empty interior. The complements of negligible sets, "generic sets," are then sets with properties that are "almost always" possessed by the set X. In this way topology can be used to define a rough notion of likelihood: "almost always" and "almost never."

[4]There is a proliferation of papers discussing the topic of cosmology as a science written in the middle of the 20th century: (Dingle, 1955; Munitz, 1962; Harré, 1962; Davidson, 1962). Many of the views expressed, however, have been justly criticized more recently for being overly skeptical towards addressing the scientific problems arising from the uniqueness of the universe (Kanitscheider, 1990; Ellis, 2007; Smeenk, 2008, 2013).

[5]Quantum cosmology and multiverse cosmology could make for a different conclusion. Although I focus on classical, single-universe cosmology here, I do believe further investigation into these larger contexts is warranted. Although some of my conclusions would carry over, there are some novelties which may make the case for cosmological probabilities better there. The interested reader should see the critiques in (Smeenk, 2014) and (Ellis et al., 2004) as a starting point.

2.1 The Reference Class Problem

The first issue to face in defining a probability space in cosmology is deciding on the appropriate reference class (sample space). Again, a probability space is standardly specified by a set X of possible outcomes, a σ-algebra $\mathcal{F}$ of these possible outcomes, and a probability measure P that assigns probabilities to elements of $\mathcal{F}$. The problem of deciding the appropriate reference class is the problem of determining precisely what X should include.[6]

In cosmology the appropriate reference class X will be the set of (physically) possible cosmologies. A cosmology here will be taken, as is standard, to be a relativistic spacetime, i.e. a model of the general theory of relativity (GTR).[7] At large scales gravity appears to be the most important physical force in the universe, and general relativity is the best, most highly-confirmed theory of gravity that we have. Thus cosmological models are appropriately taken to be relativistic spacetimes.

But what are the possible cosmologies, i.e. which models of GTR are possible models of the universe? Trust in our theories is usually thought to underwrite the belief that the models of that theory are physically possible. For example, taking X to be the collection of models of GTR is justified from this point of view by our justified belief in the laws and modal structure of GTR, these together telling us what the nomologically possible models are, and therefore (on "the most straightforward reading of physical possibility" (Earman, 1995, 163)[8]) what the physically possible models are. Thus, by this argument, one is led to the view that X should be the set of spacetimes of general relativity.

The practice of cosmologists (and relativists), however, does not appear to accord with this line of thinking. By any measure general relativity is a permissive theory; any number of undesirable or pathological spacetimes is possible according to it.[9] Many authors are for this reason inclined to exclude certain models from physical consideration, such as models with closed timelike curves (CTCs).[10] Should one include these spacetimes in X? Should one exclude pathological examples because they do not strike one as "physically possible?"

It is not my intention to take a stand on these questions here. The only point I wish to note is that in practice cosmologists do exclude certain models from consideration, thereby presuming some alternative physical modality to the "straightforward" nomological one. Without a doubt, the nature of this alternative modality is unclear, despite its seeming "intuitiveness." Indeed, models are often excluded merely because they are thought to be physically impossible or physically unreasonable. The available justifications for these exclusions tend to be rather dubious (if given at all), as they do not rely on well-motivated

[6]In a way it does not matter too much precisely what X is so long as it is large enough, since one can always use the probability measure to assign zero probability density to any subset of spacetimes, in effect counting them as impossible. It may be mathematically convenient to include some "extra" objects in X for mathematical convenience, simplicity, etc. Nevertheless, the reference problem will remain, whether in the guise of choosing X or choosing elements of X to which probability zero is assigned.

[7]In full detail it should also include a physical model of relevant cosmological phenomena in that spacetime (Ellis and van Elst, 1999; Cotsakis and Leach, 2002; Ellis et al., 2012), but for my purposes it is sufficient to consider just the spacetime component of a cosmology.

[8]This straightforward reading of physical possibility is actually spelled out in two ways by Earman and his collaborators (Earman et al., 2009, 95); for my purposes this reading is simply the identification of nomological and physical possibility.

[9]Among the more exotic models are the "causally bizarre" Gödel spacetime and Taub-NUT spacetime which have CTCs. It must be acknowledged, however, that even the most familiar examples of spacetimes permitted by GTR have fairly unintuitive features: expanding space (Friedman-Robertson-Walker, de Sitter), spacetime singularities (Schwarzschild, Friedman-Robertson-Walker), etc. Therefore some distinction between the "undesirable" and the merely "unintuitive" need be made.

[10]The inclination to disbar spacetimes with CTCs is sometimes grandiosely characterized as the "cosmic censorship conjecture." Wald (1984, 304) states it simply (albeit imprecisely) as "all physically reasonable spacetimes are globally hyperbolic." Since globally hyperbolic spacetimes do not have CTCs, it follows that all physically reasonable spacetimes do not have them either, at least if this version of the cosmic censorship conjecture is true. But then one wonders what it takes to be a physically reasonable spacetime. Whereas some find CTCs objectionable on philosophical grounds—for example, "those who think that time essentially involves an asymmetric ordering of events...are free to reject the physical possibility of a spacetime with CTCs" (Maudlin, 2012, 161)—others encourage a certain degree of epistemic modesty with respect to physical reasonableness. Indeed, Manchak (2011) demonstrates the existence of observationally indistinguishable spacetimes, some of which exhibit the features which one may regard as undesirable or pathological.

physical principles or observational grounds (as has been noted by some commentators (Earman, 1987; Manchak, 2011)).

Nevertheless, some weight of consideration should be reasonably accorded to practice, in which case the possibility of justifying the exclusion of pathological spacetimes should not be quickly dismissed merely because adequate justifications have not been given. If that is accepted, then the reference class problem cannot be regarded as solved simply because one can identify nomological and physical possibility by fiat (or by philosophical artifice).[11]

Still, even permitting the kinds of assumptions that exclude pathological spacetimes (such as global hyperbolicity which rules out CTCs) or mathematically inconvenient spacetimes (such as those that lack compact spatial sections), one is still left with a vast collection of cosmological models which will then be considered physically possible. If one furthermore arbitrarily restricts attention to spacetimes with some specific manifold M, as is common in the cosmology and relativity literature, i.e. to the subcollection of physically possible cosmologies with underlying manifold M and a physically possible metric g on this manifold (Lerner, 1973), one generically has an infinite-dimensional space (GTR is a field theory, after all). Although such a collection will (perhaps) possess some mathematical structure, the problems with defining a probability measure (in particular) on such a space are manifold (see §3 below).

Whether because of these problems or in ignorance of them, physicists' attention has so far been mostly directed at simple models which have a finite-dimensional state space, e.g. the state spaces on which one can define the GHS measure (the main topic of §4). While this brings the cosmological framework closer to the statistical mechanical one, where state spaces are generally taken to be finite-dimensional, this maneuver raises a problem not found in the latter context. How can a measure contrived on a special (sub)set of physically possible spacetimes represent cosmological probabilities correctly?

On the one hand, if the collection S of simple models, e.g. spatially homogeneous and isotropic spacetimes, is taken to be the full set of physically possible models, then it is difficult to see how this can be justified on any well-motivated physical principle or on observational grounds. Surely, in other words, GTR (or even purely physical intuition) suggests that there are spacetimes which may be physically possible cosmologies besides the particular simple collection of spacetimes S.

If, on the other hand, the collection S (equipped, say, with probability measure μ_S) is a subset of a larger possibility space X (equipped with probability measure μ_X), then the likelihood of a set of models $Z \subset S$ in the subspace must be a conditional likelihood $\mu_S(Z) = \mu_X(Z|S)$. In other words, the probability space S must be a conditional probability space of X.[12] In this case the probabilities of the large space X put a constraint on the probabilities of the smaller space S, a constraint that probability measures that can be associated with the smaller space are not guaranteed to meet. Formally, the probability assigned to a collection of models $Z \subset S$ must be

$$\mu_S(Z) = \mu_X(Z|S) = \frac{\mu_X(Z \cap S)}{\mu_X(S)} = \frac{\mu_X(Z)}{\mu_X(S)}. \tag{1}$$

To illustrate these considerations in the most important case in cosmology, consider the subset of models of GTR that satisfy the cosmological principle (CP). The CP constrains the set of models from general relativity to those that are spatially homogeneous and isotropic. These models are known as the Friedman-Robertson-Walker (FRW) spacetimes. First, is this assumption admissible as a way of specifying X? Although the FRW models have been observationally successful, there is certainly no good argument that justifies the CP as definitively delimiting the space of physically possible spacetimes (Beisbart and Jung, 2006; Beisbart, 2009) (especially since the universe is not strictly speaking homogeneous

[11] In stating this I echo (Earman et al., 2009, 93), who suggest that "the status of such judgments is a more interesting issue than philosophers have generally realized."

[12] Let X be the larger sample space with σ-algebra $\mathcal{F}$ and probability measure μ_X. Suppose S is a measurable subset of X with non-zero measure according to μ_X. Then the conditional probability space S has σ-algebra $\{S \cap Z | Z \in \mathcal{F}\}$ and probability measure $\mu_S(S \cap Z) = \mu_X(Z|S)$.

and isotropic!). More plausibly, then, the probabilities that we would obtain by making this assumption and building a probability measure on the set of FRW spacetimes are not unconditional probabilities—the space of physically possible cosmologies is surely larger. How large? If the space X of physically possible cosmologies is the space of nomically possible spacetimes (according to GTR), then the set S of FRW models is almost certainly negligible, a problematic result given the uses to which cosmologists want to put likelihood measures in cosmology. Even if S were not negligible, it is hard to see what the point of constructing measures associated with S, the set of FRW spacetimes, is without knowing what the measure is associated with the full possibility space X (at least if the motivation is to derive an objective probability measure). One simply needs to know the full measure (well enough, anyway) in order to know the correct conditional measure.

To conclude this section, I should emphasize that I do not discuss the reference problem as an argument against the applicability of probabilities to cosmology. It is, after all, a challenge for any theory that makes use of likelihoods. Nevertheless, considerations pertaining to the choice of reference class do matter quite generally and, more particular to the case at hand, will matter for the more decisive challenges that follow.

2.2 Interpretation of Cosmological Likelihoods

Concerning the interpretation of cosmological likelihoods, a preliminary issue that must be broached is whether the likelihoods invoked in cosmology should be understood as "subjective" or "objective". Although the distinction is somewhat vague, I will characterize it roughly as that between whether probabilities are substantively determined by the physical situation or else substantively determined by the agent.

On the one hand, one might expect that an objective way to assess cosmological likelihoods is required for them to have physical significance. An objective approach appears to be the favored approach of most cosmologists who have written on the topic. The most well-known proposal in this vein is the already-mentioned canonical measure of (Gibbons et al., 1987), the GHS measure.[13] Topological methods may be used to give an objective measure of likelihood as well.[14] The basic strategy of these approaches is to begin with some physically motivated attribution of likelihoods to sets of cosmologies in some relevant space of possible cosmologies. The motivations may come, for example, from the structure of the space of models of GTR or from intuitions on how models in such spaces are physically related. Then, for example, with objective likelihoods in hand, if one finds that spatially flat FRW spacetimes represent a negligible set of cosmologies and inflating FRW spacetimes are generic cosmologies, arguably one has a warranted basis for making an explanatory argument (a typicality argument) in favor of inflation.

On the other hand, one might try to give an analysis of cosmological likelihoods based on a subjective measure of likelihood. Subjective notions of likelihood are perhaps behind many cosmologists' intuitions about fine-tuning cases. There are, alas, few places in the literature where more precise formal methods are used to substantiate these intuitions. Accordingly, it is difficult to analyze and assess the merits of subjective measures of likelihood in cosmology in general.[15]

Although a more thorough review of approaches to defining cosmological likelihoods would engage with subjective approaches, I will restrict myself to addressing the more prominent objective approaches (apart from some comments on the principle of indifference below). This is due to the greater importance of the latter approaches in the physics literature and to maintain a reasonable scope in this paper.

[13] The notable papers discussing their approach include (Henneuax, 1983; Gibbons et al., 1987; Hawking and Page, 1988; Coule, 1995; Gibbons and Turok, 2008; Carroll and Tam, 2010; Schiffrin and Wald, 2012).

[14] Hawking (1971), for example, proposes the application of such methods in cosmology. (Isenberg and Marsden, 1982) is another well-known example.

[15] Examples do however exist, such as (Evrard and Coles, 1995) and (Kirchner and Ellis, 2003). Evrard (1996) and Evrard and Coles (1995) argue for a dissolution of the flatness problem using a subjective approach to cosmological parameters. Their approach is criticized by Coule (1996), who favors the canonical measure.

I will also be more restrictive than is usual in the philosophical literature in how I employ the term "interpretation" in what follows. In philosophy an "interpretation of probability" is usually understood to refer to an account of how the concept of probability should be analyzed (Hájek, 2012). For the purposes of my argument it is not necessary to make use of the standard accounts, e.g. the logical interpretation, the frequentist, the propensity, etc. By "how probability is interpreted" in cosmology I intend "how randomness is understood". As randomness is the central concept in probability theory (like truth in logic), it is essential to identify the source of randomness in an application of probability theory in order for that application to be justified. As Hollands and Wald say in their discussion of applications of probability in cosmology,

> probabilistic arguments can be used reliably when one completely understands both the nature of the underlying dynamics of the system and the source of its 'randomness'. Thus, for example, probabilistic arguments are very successful in predicting the (likely) outcomes of a series of coin tosses. Conversely, probabilistic arguments are notoriously unreliable when one does not understand the underlying nature of the system and/or the source of its randomness. (Hollands and Wald, 2002b, 5)

Identifying the source of randomness is an important way to distinguish accounts of probability. For a subjective theory this randomness pertains to the agent, whether in terms of her independent choice or of her knowledge of a physical situation. For an objective theory this randomness pertains to the physical situation in some respect. This randomness need not be taken as a full-fledged feature of nature ("ontic randomness"). For example, in Humean accounts of chance all that is understood to exist is a so-called "Humean mosaic" of events; laws and probability are understood as systematizations of this mosaic. Thus the Humean account is objective (Loewer, 2001, 2004), in that it is determined substantially by the physical situation, but not ontic.[16]

In physics the structure of theories can helpfully suggest where objective randomness may be "realized". Conventional physical theories are described in terms of a set of physically possible states, a dynamics that determines the evolution of a system from physical state to physical state, and a set of functions that determine observable quantities defined on the set of states. Therefore there are naturally three ways randomness can enter into a physical description: the initial state of the system, the dynamical evolution of the system, and the observable properties of the system.[17] From a metaphysical point of view one might question the association of randomness with these formal, descriptive features of theories as ontologically significant. Nevertheless, for my purposes it is quite sufficient to address the interpretation of probability at this theoretical level of description.

So, where can one attribute randomness in the case of cosmological theories? As a cosmological model is essentially a relativistic spacetime, we can take the space of states to be the set of possible spacetimes (or perhaps initial data on a spacelike hypersurface) permitted by GTR (or some subset thereof, as discussed in the previous section, depending on how one solves the reference class problem). The dynamics is given by the Einstein equation and the observables are going to be certain geometric properties of spacetime (which must in practice be supplemented by other physical models to derive proper observables like galaxy counts, galactic redshifts, light element abundances, etc.). Since general relativistic dynamics is essentially deterministic (setting to the side the issue of gauge and pathological spacetimes with, for example, closed time-like curves, as these latter do not introduce randomness into the dynamics), one cannot locate the randomness there except by making the randomness so insignificant as to give rise to an essentially deterministic dynamics. Empirical considerations strongly militate against the idea that cosmological observables are substantially stochastic as well. In short, there is very little reason to think that the universe is "fluctuating" around the space of possible cosmologies dynamically and very little

[16]For more on the distinctions between ontic and epistemic, and objective and subjective, see (Frigg, 2008).

[17]Cf. (Maudlin, 2007; McCoy, 2016).

reason to think that its observable properties are either. That leaves the initial state, the initial "choice" of spacetime, as the only way randomness can enter into cosmology objectively.[18]

On this interpretation the (initial condition of the) universe is the outcome of a random trial. A cosmological probability measure, in other words, can only represent the objective chance of our universe being in a particular state (initially). Indeed, the possibility of this viewpoint has suggested itself to some cosmologists, who compare the situation (usually pejoratively) to a blind-folded creator selecting a universe by throwing a dart at the dartboard of possible universes.[19]

Should one adopt such a point of view in cosmology? It is on the face of it a coherent possibility at least. It is arguably tenable in statistical mechanics (where it is (tacitly) employed in typical Boltzmannian approaches) as one can at least verify the consistency of frequencies of microstates with empirical frequencies of observables (Hemmo and Shenker, 2012). In the absence of an analogous micro-theory, however, it is unclear why one would want to accept this interpretation in cosmology. As Loewer observes, "one problem is that it does not make sense to talk of the actual frequency with which various initial conditions of the universe are realised" (Loewer, 2001, 615). A single-sample probabilistic scenario in cosmology is obviously observationally indistinguishable from a deterministic scenario that involves no probability at all, only an initial state.[20] Moreover, there is relatively little theoretical reason to suppose that there was a random trial selecting among the space of relativistic spacetimes. Without any input from physics about the source and nature of this randomness of initial conditions (recalling the Hollands and Wald quotation above) and no way to verify it empirically, we should find the "dart throwing" interpretation highly unsatisfying. If we were, however, to possess a trustworthy theory that did suggest such a random start to the universe (a multiverse theory or a theory of quantum gravity could do so, if sufficiently warranted), then we might have sufficient reason to introduce a probability measure and interpret it in this way. It would likely not be, however, associated with the space of classical relativistic spacetimes.

Indeed, this problem of interpretation also infects foundational discussions of statistical mechanics when they move in response to the pressure to "globalize" the theory (Callender, 2011b), i.e. to treat the universe as a whole as a statistical mechanical system. As said, in the Boltzmannian approach the only possibility for interpreting statistical mechanical randomness is in understanding the the initial conditions of the system as random (either in actuality or as the best systematization of the "Humean mosaic"). As before, we have relatively little reason to believe that the *universe* began as the outcome of a random trial in this scenario. Only the artifice of "inductively" generalizing from familiar statistical mechanical systems to the universe at large lends the idea some plausibility. This simple-minded induction is, however, disturbingly close to a composition fallacy.

Therefore, in both theoretical contexts mentioned, general relativity and statistical mechanics, I conclude that the only admissible interpretation of cosmological probabilities locates the associated randomness with initial conditions. Although the interpretation is coherent, in both cases this interpretation has very little evidence in its favor. Of course this latter claim is not so much a matter of interpretation as one of justification.

2.3 Justification of Cosmological Likelihoods

So, let us consider the justification of cosmological likelihoods, specifically of probability distributions on some given sample space of cosmologies, assuming that initial conditions are subject to (real or imagined) randomness (since this seems to be the only available interpretation). To be sure there are significant technical problems with supplying such a structure to these possible cosmologies, as §3 will demonstrate.

[18] At least this is so at this level of description. The context of quantum cosmology would open up alternative possibilities. However the present discussion is, again, focused only on classical cosmology, in keeping with literature discussed.

[19] Cf. (Penrose, 1989b, 444) and (Hollands and Wald, 2002a, 2044).

[20] See (Werndl, 2016) for discussion of the general observational equivalence of deterministic and indeterministic descriptions of a system.

Even if these technical problems could be overcome, however, a more immediate concern is that it is not possible, so I claim, to adequately justify any particular cosmological probability measure (again, at least without some other theory of quantum gravity or of initial conditions grounding it).[21]

The first (admittedly obvious) point to make is that cosmological likelihoods cannot be empirically justified, at least insofar as cosmology concerns a single universe. If we suppose that probability theory applies to cosmology, then it must be the case that our universe is the outcome of a single random trial over possible initial conditions, as argued above. Cosmological probability measures are therefore vastly underdetermined (at least in the absence of some trustworthy theory providing a particular one), with no apparent empirical grounds to break the underdetermination. Thus, if one tries to make GTR into a probabilistic theory (Gibbons et al., 1987) by defining a probability distribution over possible cosmologies, it is clearly the case that *any* choice of probability measure that assigns some probability to the cosmological model best representing our universe is empirically adequate. Note that the adequate probability measures include the probability distribution that makes our universe "quasi-determined"—assigns the cosmology representing our universe probability one.

The only hope for a justification of cosmological likelihood measures is thus an a prioristic justification, e.g. one based on its "naturalness". Given the obvious limitations of an empirical approach, several prominent cosmologists have unsurprisingly reasoned in accord with epistemic-based, a priori principles, like the principle of indifference (PI), to uniform probability distributions (Kofman et al., 2002; Linde, 2007). The PI holds that if there is no salient reason to prefer any other probability distribution, given some sample space, one should assign a uniform probability density to that space. A similar principle is invoked in assigning a uniform probability distribution with respect to the Liouville measure. Although Gibbons, Hawking, and Stewart note the problematic nature of the PI—"indeed...it is not at all clear that every model should be given equal weight if one wishes the measure to provide an inductive probability" (Gibbons et al., 1987)—they assume that the probabilities should be uniform with respect to the Liouville measure. Carroll and Tam (2010) recognize that the Liouville measure does not necessarily imply any particular probability measure in general: "Since the Liouville measure is the only naturally-defined measure on phase space, we often assume that it is proportional to the probability in the absence of further information; this is essentially Laplace's 'Principle of Indifference'." They too go for the uniform mapping, and provide as precedent the practice of assuming a uniform probability distribution on the Liouville measure of phase space from statistical mechanics.[22]

Unfortunately for such principles, in a reference class of cosmologies with infinite total measure there is no mathematically natural choice of probability measure and no uniform probability distribution. In special cases (for example when the total measure of the space is finite) there may be a canonical choice of probability measure that is uniform, but one then faces a dilemma raised earlier: either this space delimits the full space of possible cosmologies (which is highly implausible, if not clearly false) or its probabilities must be conditional probabilities in a larger space of possible cosmologies (which, insofar as this larger space has infinite total measure and therefore no uniform probability measure, cannot then be justified by the PI, naturalness, etc.). Therefore even if a justification of uniformity, by way of mathematical

[21] I am therefore essentially in agreement with Schiffrin and Wald (2012, 9), who claim that "the only way to justify the use of the Liouville measure in cosmology would be to postulate that the initial conditions of the Universe were chosen at random from a probability distribution given by the Liouville measure." Now, what they seem to mean is that the only possible interpretation of the posit is the stochastic initial conditions interpretation, and that the only "justification" of doing so is as a theoretical posit (which they assume must include positing the Liouville measure). Whether that postulation is justified—they describe it (charitably) as an "unsupported hypothesis"—is what I examine in this section.

[22] One sometimes sees the Liouville measure associated with a mechanical phase space called the "Lebesgue measure". The Lebesgue measure standardly refers to the natural measure associated with $\mathbf{R}^n$. The measurable subsets U of phase space can be called "Lebesgue measurable sets" in the following sense: for all charts $\varphi : O \to \mathbf{R}^n$ on n-dimensional phase space Γ, $\varphi[U \cap O]$ is a Lebesgue measurable set in the usual sense. Since in general there is no canonical pull-back of the Lebesgue measure to Γ, however, it is somewhat misleading to call the Liouville measure the Lebesgue measure. Of course when the phase space *is* $\mathbf{R}^n$, the Liouville measure just is the usual Lebesgue measure and the terminology is justifiable, although consistency suggests that the former term is preferable.

naturalness, the PI, etc., were possible in statistical mechanics, it would not easily carry over to the case of cosmological models.

However, in my view it has been made sufficiently clear in the literature that a prioristic principles like the PI are also not generally applicable in statistical mechanics, mainly because empirical frequencies depend importantly on the nature of a physical system's randomness and there is no reason to expect that the source of randomness acts uniformly on some space of possibilities (Shackel, 2007; Norton, 2008; North, 2010). If this is correct, then the presupposition of the inference from statistical mechanics to cosmology fails. There is also no independent, compelling support for the PI or its cousins in the specific case of cosmology (McMullin, 1993; Ellis, 1999; Earman, 2006; Norton, 2010; Callender, 2010). In cosmology very little at all is known about the mechanism that brings about the initial conditions of our models of the universe, and so assigning equal weights to distinct cosmological possibilities (especially if based merely on a lack of knowledge) is highly dubious (at least on the face of it) since it may well be the case that certain initial conditions are in fact more likely according to the true (presumably quantum) mechanism responsible for them.[23]

I have so far argued that there is no direct empirical or a priori justification of cosmological probabilities. Yet another alternative is that the empirical justification of a uniform probability distribution in statistical mechanics indirectly justifies a uniform probability distribution in cosmology. Similarly, in foundational discussions of statistical mechanics (especially in the context of the "past hypothesis" scenario described in (Albert, 2000), philosophers often suppose that the justification of a uniform probability distribution for usual statistical mechanical systems justifies a uniform probability distribution for the universe as a statistical mechanical system. This supposed transfer of empirical justification plausibly only holds for target systems that are sufficiently similar to the antecedently justified systems. Our best understanding of the universe, however, suggests that it is not very much like statistical mechanical systems. Among the most salient differences is the importance of gravitation, as has been pointed out and discussed in (Earman, 2006; Wallace, 2010; Callender, 2010, 2011a). Even apart from this significant issue, it is crucial to recognize that cosmological probabilities are (in statistical mechanical jargon) macro-probabilities, not micro-probabilities (there is no micro-theory of cosmology). As statistical mechanical probabilities are micro-probabilities and not macro-probabilities—the empirical content of statistical mechanics pertains to microscopic frequencies and macroscopic frequencies entirely supervene on these—the transfer of justification is clearly inadmissible.

The failure to locate the empirical content of statistical mechanics in micro-probabilities and to recognize the lack of an a prioristic justification of statistical mechanical probabilities has led to some dubious arguments. When probability assignments are determined by the so-called proportionality postulate, i.e. assigning probabilities to macrostates (macroprobabilities) and not just to microstates conditional on a *given* initial macrostate (as in the past hypothesis), one is quickly led to the idea that macrostates, like the past "low entropy" state of the universe, are improbable. Demands to explain this improbability of an initial macrostate, as in (Penrose, 1989a; Price, 2002, 2004), are therefore based on an ill-motivated application and interpretation of probability to cosmology (and statistical mechanics as well).[24]

Finally, I should note that even if no unique probability distribution can be justified, it may nevertheless be the case that there is a natural non-probabilistic measure on the space of possible cosmologies. This is the case with the GHS measure (see §4). However it is important to recognize that mathematical naturalness is no guarantee of physical significance. It is, in other words, a distinct step requiring its own justification to interpret a natural measure as a likelihood measure. After all, measures can play a variety of roles in a physical theory (especially as a standard for integration along trajectories). There is

[23] The only viable response I can see to these points is to modify the space of possibilities to make the PI hold, but then it is clear that the correct space of possibilities is not in fact justifiable a priori.

[24] Responses to these demands have accepted their presupposition—that the initial macrostate of the universe is improbable—and weighed whether and how some explanation could be provided (Callender, 2004). One ought to simply reject the presupposition as ill-motivated and inadequately justified.

no reason to assume that a mathematical measure must play the role of a likelihood measure in any theory that comes equipped with one.

Nevertheless, in statistical mechanics it has been argued that the Liouville measure can be used in such a way, i.e. as a typicality measure (Lebowitz, 1993; **?**; Dürr, 2001; Goldstein, 2001, 2012; Volchan, 2007; Werndl, 2013; Schech, 2013; Lazarovici and Reichert, 2015). In this approach to interpreting statistical mechanics one makes do with the Liouville measure alone, and uses it as a standard of typical and atypical behavior. The basic schema of typicality arguments (in statistical mechanics) is to show that some behavior or property is highly likely and its contrary is highly unlikely, in which case one can infer that that behavior or property holds; "In other words, typical phase space points yield the behavior that it was our (or Boltzmann's) purpose to explain. Thus we should expect such behavior to be prevail in our universe" (Goldstein, 2001, 58). Although some enthusiastic disciples of Boltzmann claim that typicality is the heart of all foundational matters in statistical mechanics—as Dürr (2001, 122) remarks, "we have the impression that we could get rid of randomness altogether if we wished to do so"—full reliance on typicality arguments clearly represents a significant retreat from the quantitative successes of statistical mechanics (Wallace, 2015), which depend on probability distributions to derive the empirical content of the theory (for example to predict fluctuation phenomena).[25]

One of course may try to infer from the putative success of typicality arguments in statistical mechanics to their applicability in cosmology. Yet, if typicality arguments are indeed successful in statistical mechanics, then they are because they depend importantly on the full complement of structures in statistical mechanics, e.g. the space of possibilities, the collection of macro-states, etc. One generally has empirical evidence that suggests the correct structures for a system in statistical mechanics. One does not have this in the case of cosmology, in particular because of the issues mentioned above in connection to the reference class problem. Thus what is typical in cosmology depends very much on what set of cosmologies one is considering—and there is no guarantee that what is typical in one context is typical in another.

Thus I conclude that the most serious conceptual problem for attributing likelihoods to cosmologies is justifying a choice of measure. The justification of measures in statistical mechanics does not carry over straightforwardly to cosmology, and there is no independent justification for cosmological likelihood measures. Justificatory problems may not impress physicists much—they are merely philosophical problems after all—but these are not the only problems facing attributions of likelihood in cosmology.

3 Likelihood in the Solution Space of General Relativity

In the remaining two sections of this paper I set aside the conceptual problems which I have raised in the previous section and consider the prospects of rigorously defining some notion of likelihood on the space of cosmologies, i.e. without worrying about whether it makes much sense to do so. In this section I deal with the case where the space of possible universes is taken to be the solution space of GTR; in §4 I deal with the case where the solution space is restricted to be finite dimensional by specific modeling assumptions, focusing on the GHS measure on minisuperspace.

I begin my discussion by pointing out a series of basic facts that are, however, seldom (if ever) mentioned explicitly in discussions of the space of relativistic spacetimes. As is well-known, a relativistic spacetime $\mathcal{M}$ is standardly defined as a differentiable four-dimensional manifold M that is Hausdorff, connected, and paracompact equipped with a Lorentzian metric g, a rank two covariant metric tensor field associated with M which has Lorentz signature.[26] Given this definition of a relativistic spacetime (cos-

[25] Thus "the explanation...is a weak one, and in itself allows for no specific predictions about the behavior of a system within a reasonably bounded time interval" (Pitowsky, 2012, 41). For further criticisms of the typicality account in statistical mechanics, see (Frigg, 2009, 2011; Frigg and Werndl, 2012).

[26] Left tacit in defining a spacetime this way is the Levi-Civita derivative operator ∇, the unique covariant derivative operator compatible with the metric g.

mology), note that the space of possible cosmologies ranges over the set of four-dimensional *topological* manifolds; moreover, for each four-dimensional topological manifold, there is also a range of *smoothness structures* on these manifolds that make them into differentiable manifolds; finally, for each smooth, four-dimensional differentiable manifold, there is a ("kinematic") range of Lorentzian metrics, which range is restricted by the Einstein equation to yield the possible ("dynamical") set of cosmologies. We must not neglect the full range of possibilities without arguments suggesting that some of these should be excluded as irrelevant.

There is presently relatively little that we can say, however, about the structure of this large and complicated set. With specific modeling assumptions it can be reduced to something much more tractable. For example, in the case of FRW spacetimes one chooses a particular solution to the EFE which restricts the possible 4-manifolds to those that can be expressed as twisted products $I \times_a \Sigma$ (a is the scale factor), where I is an open timelike interval in the Lorentzian manifold $\mathbf{R}^{1,1}$ and Σ is a homogeneous and isotropic three-dimensional Riemannian manifold (McCabe, 2004, 530). Since one can fully classify these 3-manifolds (Wolf, 2010), one can enumerate the different possible FRW spacetime manifolds (McCabe, 2004, 561). With such an enumeration one could (at least conceivably) specify the likelihoods of each kind of product of 3-manifolds and 1-manifolds.

If one were to want a complete likelihood measure on the full space of possible cosmologies—and according to arguments in §2 one would—it would require a means of classifying four-dimensional manifolds. The classification of such manifolds is a notoriously difficult mathematical problem (and distinctly so in comparison to other dimensions, where classification has been established by geometrization or surgery techniques) (Freedman and Quinn, 1990; Donaldson and Kronheimer, 1997). It is therefore quite unclear what structure can be put on this set of manifolds which would make it amenable to attributions of likelihood. The discovery of so-called exotic smoothness structures on the topological manifold $\mathbf{R}^4$, i.e. smoothness structures that are homeomorphic to $\mathbf{R}^4$ but not diffeomorphic to the standard Euclidean smoothness structure on $\mathbf{R}^4$, are a particularly notable and surprising underdetermination of spacetime models as well, one that is almost entirely overlooked in cosmological work, where the Euclidean smoothness structure is automatically presumed on the topological manifold $\mathbf{R}^4$.

Is this underdetermination of spacetime topology or smoothness structure an epistemic threat in cosmology? It might seem so, since exotic smoothness structures could have physical consequences:

> The discovery of exotic smoothness structures shows that there are many, often an infinity, of nondiffeomorphic and thus physically inequivalent smoothness structures on many topological spaces of interest to physics. Because of these discoveries, we must face the fact that there is no a priori basis for preferring one such structure to another, or to the 'standard' one just as we have no a priori reason to prefer flat to curved spacetime models. (Asselmeyer-Maluga and Brans, 2007, 13)

In the philosophical literature, Manchak (2009, 2011) has argued that we do face a substantial epistemic predicament in cosmology because of similar kinds of underdetermination. In his cases global properties of spacetime, such as inextendibility and hole-freeness, are underdetermined by the theoretical possibility of observationally indistinguishable spacetimes which do or do not possess these properties (Glymour, 1977; Malament, 1977)—even assuming robust inductive principles for local conditions on spacetime. His arguments have influenced several commentators to claim that knowledge of any global property of spacetime is indeed beyond our epistemic horizon (Beisbart, 2009; Norton, 2011; Butterfield, 2012, 2014).

Although cosmological underdetermination is a topic worthy of further investigation, the epistemological consequences for cosmology may not be as severe as some have thought, essentially for the kinds of reasons Laudan and Leplin (1991) give in criticizing over-inflated claims of underdetermination.[27] We

[27] See, e.g. arguments against certain cases of underdetermination in (Magnus, 2005; McCoy, 2017).

may, for example, have grounds to favor a specific choice that breaks the underdetermination, or it may be the case that the underdetermination in question is of a superfluous feature that only arises because of our choice of theoretical framework.[28] Indeed, in some cases we may be able to infer that our spacetime has a certain topology if it best explains observational phenomena. For example, if we inhabited a "small universe" (Ellis and Schreiber, 1986) in which light has had time to travel around the universe multiple times, we might be able to observe multiple images of galaxies, etc. which would suggest a compact spatial topology. If the topology of our spacetime were multiply-connected, the same would perhaps be possible (Lachièze-Rey and Luminet, 1995; Luminet et al., 2003). Exotic smoothness structures may also have detectable astrophysical effects (Sładkowski, 2009).

I do not intend to take a particular stance here on whether underdetermination of differential and topological manifold structures is a serious epistemological problem. Too little is known at present. The point is rather that one must either justify a choice to break the underdetermination, or else one must acknowledge it by including *all* the physically relevant models when defining cosmological likelihoods. All accounts of cosmological likelihoods presently ignore the issue completely, making specific choices of topology and smoothness structures without justification. This is not at all surprising, since the potential threat of underdetermination from non-standard topologies and smoothness structures is little discussed in the philosophical or physical literature thus far.[29] Nevertheless, an account of likelihood for the space of possible cosmologies ought to say something about these issues if the account is to be used to make inferences . Unfortunately, owing to the state of the art, one must admit that little can be said definitively about likelihoods without considerable restriction of the general case. Still, at the very least this lacuna casts considerable doubt on the trustworthiness of any general claim which relies essentially on cosmological probabilities.

The restriction generally taken by workers investigating the solution space of general relativity, and derivatively the space of cosmologies, is to assume a fixed spacetime manifold M (Isenberg and Marsden, 1982, 188). Then one can understand general relativity as a particular field theory on M using the framework of covariant classical field theory (Fischer and Marsden, 1979). This field bundle is a map $\pi : L(M) \to M$ with typical fiber L, where L is the vector space of Lorentzian metric tensors, e.g. for p in M, $L_p = \{g_p \,|\, T_pM \times T_pM \to \mathbf{R}\}$ with g_p normally non-degenerate, symmetric, and possessing a Lorentzian signature. A configuration of the field is represented by a section of this bundle, viz. a tensor field g on M. The canonical configuration space of the theory is then the space of sections, which I denote hence as $\mathscr{L}$.

This canonical configuration space can be given some structure by, for example, topologizing it, as a way of introducing likelihoods topologically. Unfortunately, since there is an infinity of sections of the field bundle, there is an infinity of topologies which one can define on the set. How can one decide which topology is appropriate? Fletcher (2016) observes that some physicists have advocated a particular topology as appropriate for discussing similarity relations in general relativity. For example, Lerner (1973) favors the Whitney fine topology, a topology that is widely used to prove stability results in GTR (Beem et al., 1996). Geroch has furnished some examples, however, which suggest that this topology has "too many open sets," i.e. the topology is intuitively too fine (Geroch, 1971)—at least for some purposes. Other topologies have unintuitive results as well. The compact-open topology for example, renders the verdict that chronology violating space-times are generic in $\mathscr{L}$ in any of the compact-open topologies (Fletcher, 2016; Curiel, 2015, 12). Such considerations, and some further results of his own, lead Fletcher to conclude that "it thus seems best to accept a kind of methodological contextualism, where the best choice of topology is the one that captures, as best as one can manage, at least the properties relevant

[28] One should also note that some assumptions have been made already to limit the theoretical possibilities from the beginning. For example, we only consider locally Euclidean Hausdorff manifolds that are connected, and paracompact. There are relatively straightforward arguments to favor these particular choices though (Ellis, 1971; Hawking and Ellis, 1973).

[29] Debates on conventionalism, the unity of space, and (to some extent) cosmological underdetermination have, however, taken some note of topological underdetermination (Reichenbach, 1958; Glymour, 1972; Weingard, 1976; Nerlich, 1994; Torretti, 1996; Callender and Weingard, 2000; McCabe, 2004; Magnus, 2005).

to the type of question at hand, ones that relevantly similar space-times should share" (Fletcher, 2016, 15).[30] Of course, whether any intuitions one has about which properties spacetimes should share can be adequately justified (in a particular context) is then an issue which must be addressed in each individual case.

That being said, the space $\mathscr{L}$ does not so obviously represent the space of solutions to the Einstein equation, since one may interpret this space as redundantly representing physically distinct spacetimes.[31] The reason that one might think so is as follows. First, the Einstein equation is diffeomorphism covariant: if M and N are spacetime manifolds and $\varphi : M \to N$ is a diffeomorphism, then if the Lorentzian metric g on manifold N is a solution to the Einstein equation, it follows that the pullback of g to M, i.e. $\varphi^* g$, is a solution to the Einstein equation as well. Many interpret such diffeomorphic spacetimes as "equivalent" in the specific sense that they represent the *same* physical spacetime, since they have the same observable consequences, etc.[32] The interpretive assumption that diffeomorphism-related spacetimes are physically identical is often recognized by calling the diffeomorphism group $\mathscr{D}$ the "gauge group" of Einstein gravity. The natural thing to do to disregard unphysical differences is to quotient $\mathscr{L}$ by the group of diffeomorphic automorphisms $\mathscr{D}$ (written $\mathscr{L}/\mathscr{D}$) to yield the space of physical possibilities.[33] Let us denote this space of solutions $\mathscr{L}/\mathscr{D}$ simply as $\mathscr{E}$, and take it as the space of "gauge-equivalent" physically possible cosmologies.[34]

What structure does $\mathscr{E}$ or $\mathscr{L}$ have naturally? Unfortunately this question has not been studied in nearly as much detail as the geometry of spacetime itself.[35] Some things are known. For example, it is desirable for many applications to treat (subsets of) $\mathscr{E}$ or $\mathscr{L}$ as a manifold, but in general it is not possible to treat the entirety of these spaces as a manifold, because of the existence of conical singularities in the neighborhood of symmetric spacetimes (Fischer et al., 1980; Arms et al., 1982).[36] What then are the topological and measure-theoretic possibilities for defining likelihoods in $\mathscr{E}$? Just as in the case of $\mathscr{L}$, there is an infinity of topologies on $\mathscr{E}$. In more restrictive contexts a specific choice may perhaps be

[30] Hawking (1971, 396) advocates a similar contextualism: "A given property may be stable or generic in some topologies and not in others. Which of these topologies is of physical interest will depend on the nature of the property under consideration."

[31] In this vein, there has been much discussion of whether it is appropriate to regard GTR as a "gauge theory" (Weinstein, 1999; Teh, 2016), despite dissimilarities with Yang-Mills Theory (Earman, 2003; Redhead, 2003; Weatherall, 2016). To some extent debating the semantics of the term "gauge theory" is "essentially sterile" (Wallace, 2003, 164). Yet there still remains the important question as to what the physical significance of gauge is (Healey, 2007; Guay, 2008; Rovelli, 2014). This question has not been definitively answered yet.

[32] This point of view is disputed by proponents of the so-called "hole argument" (Earman and Norton, 1987). (Weatherall, forthcoming) is the strongest rebuttal of the argument, although similar conclusions to Weatherall's were reached in more metaphysical language earlier (Butterfield, 1989; Brighouse, 1994).

[33] Since hamiltonian time evolution in canonical approaches to GTR is implemented by diffeomorphisms of 3-geometries, quotienting by diffeomorphisms does away with time evolution. This is the well-known "problem of time." Since time evolution is not relevant to the (macro-)probabilities of universes, however, there is no issue with leaving time out of the picture. Schiffrin and Wald (2012, 2) nevertheless complain that "in the absence of nontrivial time evolution, one cannot make any arguments concerning dynamical behavior, such as equilibration, the second law of thermodynamics, etc." That much may be true, but if one is not making equilibrium arguments based on statistical mechanical probabilities, i.e. if one is making a typicality argument, then timelessness is clearly not a problem.

[34] One might have plausible physical motivations to restrict this set further (or perhaps even enlargen it), but taking $\mathscr{E}$ as the correct space of possible of cosmologies is at least supported by the straightforward reading of physical possibility, i.e. as nomological possibility (Earman, 1995; Earman et al., 2009). Note that often the space of cosmological solutions to the Einstein equation is taken to be "pure gravity" spacetimes, i.e. spacetimes that satisfy the vacuum Einstein equation. This is done, in part, in the interest of finding a theory of quantum gravity by canonical quantization. My interest is in classical models of the universe including their matter content, so I do not make such a restriction.

[35] "What is not nearly as well developed is the study of the space of Lorentzian geometries, which from the mathematical point of view includes questions about its topology, metric structure, and the possibility of defining a measure on it, and from the physics point of view is crucial for addressing questions such as when a sequence of spacetimes converges to another spacetime, when two geometries are close, or how to calculate an integral over all geometries" (Bombelli and Noldus, 2004).

[36] That said, for vacuum spacetimes Isenberg and Marsden (1982) are able to show that near generic points the space of solutions is a symplectic manifold and as a whole is a stratified symplectic manifold, at least with their choice of topology, and restricting to globally hyperbolic spacetimes and spatially compact manifolds.

sufficiently well-motivated for specific applications, as Fletcher (2016) urges, but a choice for the entire space can be challenged by any number of counterexamples for which the measure seems to give the wrong intuitive result (Geroch, 1971; Fletcher, 2016).

Also, such solution spaces will generally be infinite-dimensional, essentially because spacetimes have an infinity of degrees of freedom (Isenberg and Marsden, 1982, 181).[37] This presents a problem for a measure-theoretic approaches, a problem to which Curiel (2015) draws attention. As he notes, "it is a theorem...that infinite-dimensional spaces of that kind do not admit non-trivial measures that harmonize in the right way with any underlying topology" (Curiel, 2015, 4). He concludes on this basis alone that it is not possible in such spaces to indulge in the sort of "pseudo-probabilistic" reasoning that cosmologists would like to employ (as a way of reducing the underdetermination of possible cosmological models). Curiel's main point is that the kind of infinite-dimensional spaces one might expect to use do not allow for the measure and topology to harmonize in the way to make claims such as "most spacetimes (of some kind) are similar with respect to property *X*"—"most" is a measure-theoretic notion and "similar with respect to" is a topological notion (Curiel, 2015, 4). He explains the needed connection between measure and topology as follows:

> Say we are interested in the likelihood of the appearance of a particular feature (having a singularity, e.g.) in a given family of spacetimes satisfying some fixed condition (say, being spatially open). If one can convincingly argue that spacetimes with that feature form a "large" open set in some appropriate, physically motivated topology on the family, then one concludes that such spacetimes are generic in the family, and so have high prior probability of occurring. If one can similarly show that such spacetimes form a meagre or nowhere-dense set in the family, one concludes they have essentially zero probability. The intuition underlying the conclusions always seems to be that, though we may not be able to define it in the current state of knowledge, there should be a physically significant measure consonant with the topology in the sense that it will assign large measure to "large" open sets and essentially zero measure to meagre or nowhere-dense sets. (Curiel, 2015, 3)

In finite-dimensional spaces it is possible to harmonize these notions in a way to make such claims have content. Although he points out that the natural infinite-dimensional extension of finite-dimensional manifolds depends on the differentiability class of the manifold one with which one starts, Fréchet manifolds cover the two relevant cases, and it is a theorem then that "the only locally finite, translation-invariant Borel measure on an infinite-dimensional, separable Fréchet space is the trivial measure (viz. the one that assigns measure zero to every measurable set)" (Curiel, 2015, 13). It follows that there is no sensible application of measure theory for the kinds of topological manifolds one would expect to use for rigorously discussing cosmological likelihoods.

A variety of issues were raised in this section, but the main point to which I wish to draw attention is just how complicated the space of possible cosmologies is and how little its details are considered in claims concerning the probabilities of its elements. One must consider the full range of possible topological manifolds, the range of smoothness structures on them, and the range of Lorentzian metrics that can be defined upon them. Restrictions from this set require justification if one is going to make reliable likelihood arguments on their basis. As there remains much that is unknown about all of these components, some caution is clearly warranted in accepting claims that concern the entirety of this possibility space, even setting aside the serious interpretational and justificatory challenges discussed above.

[37] "In cosmology, however, the systems one most often focuses on are entire spacetimes, and families of spacetimes usually form infinite-dimensional spaces of a particular kind" (Curiel, 2015, 4).

4 Likelihood in FRW Spacetimes

As a way of avoiding the technical difficulties surveyed in the previous section, one may limit one's attention to simpler finite-dimensional cases and hope that the results are consistent with containing cosmological possibility spaces (§2.1). In the most well known account of cosmological probabilities, Gibbons et al. (1987) adopt this general approach and show how to derive a natural measure (the GHS measure) on the set of FRW spacetimes with a scalar field as the matter component. GHS choose the set of FRW spacetimes because it is the set of models on which the HBB model is based and therefore is a set of cosmologies that may be used for representing our own universe. A scalar field is chosen as the matter content in order to represent the field driving inflation, because their primary aim is to investigate fine-tuning questions related to inflation.

It is, of course, natural to attempt to extend successful methods into new contexts, even if the physical analogy between the state spaces of systems typically considered in classical statistical mechanics and the space of possible cosmologies is loose. It is clear too that one cannot count on any serious empirical confirmation of probability assignments to cosmologies due to the assumed uniqueness of the universe (§2). The exercise might therefore appear futile:

> The question of an appropriate measure, especially in cosmology, might seem to be more philosophical or theological rather than mathematical or physical, but one can ask whether there exists a 'natural' or privileged measure on the set of solutions of the field equations. (Gibbons et al., 1987, 736)

This is exactly what Gibbons, Hawking, and Stewart (GHS hence) do. They argue that by adapting the canonical Liouville measure familiar from statistical mechanics to the case of general relativity one does find precisely such a natural likelihood measure. I criticized some aspects of this approach in §2.3, but I dedicate this section to a more in depth analysis of it and its applications.

4.1 The Gibbons-Hawking-Stewart Measure

To assess these applications, it will be worth detailing some of the principal features of the GHS measure. I follow in outline the detailed derivation in (McCoy, forthcoming), since the derivation in (Gibbons et al., 1987) and other papers is misleading on some important points which are corrected in McCoy's paper.

Since the GHS measure is intended to be the Liouville measure associated with the phase space of FRW spacetimes, one must first identify the appropriate phase space Γ for these spacetimes (with a scalar field as the matter source). This requires making use of the initial value formulation of GTR, where one takes a physical state to be represented by a spatial metric h and an extrinsic curvature π. Since FRW spacetimes are spatially homogeneous and isotropic, the spatial metric h is homogeneous and isotropic and the extrinsic curvature can be shown to be Hh, where H is the expansion coefficient known as the Hubble parameter. Thus the initial data for an FRW spacetime are adequately represented by h and H associated with some initial spatial hypersurface Σ.

The Einstein equation for FRW spacetimes can be expressed in two equations, usually called collectively the Friedman equations:

$$\left(\frac{\dot{a}}{a}\right)^2 = \frac{8\pi}{3}\left(\frac{1}{2}\dot{\phi}^2 + V(\phi)\right) - \frac{k}{a^2}; \tag{2}$$

$$\dot{H} = -4\pi\dot{\phi}^2 + \frac{k}{a^2}, \tag{3}$$

where the scale factor a is related to the Hubble parameter H according to the equation $H = \dot{a}/a$ (dots represent differentiation with respect to cosmological time t), $k \in \{-1, 0, 1\}$ represents whether space is negatively curved, flat, or positively curved, ϕ is the field value of the scalar field sourcing the Einstein equation, and V is the scalar field's potential. One can show that the initial data h and H can be re-expressed in the four-dimensional space $\{a, p_a, \phi, p_\phi\}$, where p_a (the conjugate momentum of a) is $-3a\dot{a}/4$ and p_ϕ (the conjugate momentum of ϕ is $a^3 \dot{phi}$. This space comes equipped with the canonical symplectic form

$$\omega_{p_a,a,p_\phi,\phi} = \mathrm{d}p_a \wedge \mathrm{d}a + \mathrm{d}p_\phi \wedge \mathrm{d}\phi, \tag{4}$$

This space is not yet the correct space of initial data and this form cannot be used to construct a natural measure, since the first of the two Friedman equations above is a constraint on this space that must be satisfied by initial data. One can pull the symplectic form onto the surface in phase space that satisfies the constraint equation, but the result is only a (pre-symplectic) differential form since it is degenerate. There are, in other words, redundancies among the states in the three-dimensional constraint surface, so one cannot construct a natural volume measure on the constraint surface as in the usual approach from statistical mechanics. These redundancies are due to dynamically-related states, so the natural next step would be to "solve the dynamics" so that one can take equivalence classes of phase points that are part of the same trajectory. There are difficulties with implementing this strategy in this context, so the simplest thing to do is take a two-dimensional surface that intersects all the histories by setting a (or by substitute H) to a particular value. This finally yields the GHS measure μ_{GHS} by defining a map from Lebesgue measurable sets U according to the following map:[38]

$$U \mapsto -6 \int_U a^2 \frac{(3H_*^2 + 2k/a^2)/8\pi - V}{((3/4\pi)(H_*^2 + k/a^2) - 2V)^{1/2}} \mathrm{d}a\, \mathrm{d}\phi, \tag{5}$$

where $H = H_*$ is used here to pick out a specific two-dimensional surface. Although the measure is evidently fairly complex in detail, what matters most for its application is the leading quadratic term a^2.

4.2 The Flatness Problem

Let us consider first its application to the HBB model's flatness problem in order to see how the likelihood reasoning in cosmology has gone awry. This particular case has recently been analyzed in detail in (McCoy, forthcoming), but since the points made there are relevant to other applications of the measure, I will summarize his results briefly here.

The leading a^2 factor just mentioned obviously causes the integral to diverge for large scale factors (as noted in (Gibbons et al., 1987, 745)) and (less obviously) to converge to 0 for small scale factors. This feature of the measure allows one to claim that almost all spacetimes have a large scale factor, since given any choice of scale factor a_* the measure of spacetimes with larger scale factor is infinite and the measure of spacetimes with smaller scale factor is finite. Hawking and Page claim that this fact should be taken to imply that almost all spacetimes are spatially flat, since the FRW dynamics insures that curved spacetimes become flat as the scale factor increases (the curvature $\kappa = k/a^2$). Carroll and Tam (2010, 15) take the measure to show that there is a divergence at zero curvature (large scale factors), from which they conclude that curved spacetimes have negligible measure and that flat spacetimes have infinite measure. If these claims are correct, then flat spacetimes are typical and there is in fact no flatness problem as cosmologists have thought.

These claims are, however, quite dubious, as pointed out by McCoy. The main reason is that curved spacetimes simply do not have negligible measure. Indeed, each of the sets of negatively curved, flat, and positively curved spacetimes has infinite measure. It makes no difference whether one puts $k = -1$, 0, or $+1$; the total measure of each of these three phase spaces is infinite. For this reason no conclusion can be drawn about the typicality of spacetime curvature on the basis of the GHS measure alone.

[38]Compare the derivations in (Carroll and Tam, 2010; Schiffrin and Wald, 2012; Gibbons et al., 1987).

One way to make their claims coherent, however, is to suppose that they are tacitly introducing a "curvature cutoff" by including "nearly flat" spacetimes with the flat spacetimes. Then one could state that nearly flat spacetimes are typical. But what standard of "nearly flat" should one use? Gibbons and Turok (2008) introduce just such a curvature cutoff, arguing that nearly flat spacetimes are empirically indistinguishable from flat spacetimes, and identify all the spacetimes flatter than the cutoff. They are then able to argue that the flatness problem does exist. As Carroll and Tam point out, they are able to conclude this only because they threw away almost all the solutions! Ironically, though, Carroll and Tam appear to commit the same mistake as Gibbons and Turok. Instead of throwing out all the flat and nearly flat spacetime, however, they essentially throw out the complement of the flat and nearly flat spacetimes by assigning them zero measure. This is what allows them to conclude that the flatness problem does not exist. Both of these moves are unjustifiable.

The correct conclusion in McCoy's analysis is that the GHS measure by itself does not tell us anything at all about the flatness problem in FRW spacetimes. If supplemented in some way, either by a choice of probability measure or some choice of "macrostates" by introducing a curvature cutoff, then the GHS measure may be relevant, but these additions must be physically justified. So far the justifications employed for any such choice have been absent or highly dubious.

4.3 The Likelihood of Inflation in FRW spacetimes

Although there is no obvious choice for a curvature cutoff in FRW spacetimes, there is at least a precise condition for when inflation occurs: $\ddot{a} > 0$, or $\dot{\phi}^2 < V(\phi)$, and so one can (potentially) pose precise questions about inflating spacetimes, unlike what appears to be the case for the flatness problem. In terms of our recently-introduced phase space variables, inflation occurs when

$$\frac{1}{4\pi}\left(H^2 + \frac{k}{a^2}\right) < V(\phi). \tag{6}$$

Since the GHS measure is evaluated at a particular Hubble parameter H_* and whether inflation is occurring depends on H, the GHS measure cannot give a definitive assessment of the likelihood of inflation without considering full histories. One also requires a specific model of inflation (a specific choice of $V(\phi)$) in order to make the assessment, since the condition depends on the precise shape of the potential.

That said, there are two natural questions one can pose concerning the likelihood of inflation: Is flatness a generic outcome of inflation (Ellis, 1988), and are inflating spacetimes generic?

Consider the first question. Obviously for the $k = 0$ case the question is moot since space is flat whether there is or is not inflation. For the $k \neq 0$ case we require some specification of "nearly flat," but, again, because the GHS measure diverges as $\kappa \to 0$ (due to arbitrarily large scale factors in phase space), any choice is arbitrary—almost all spacetimes are "nearly flat" because of this divergence, regardless of whether inflation occurs or does not. Therefore only if all spacetimes undergo inflation can one claim that flatness is a generic outcome of inflation in FRW spacetimes. Otherwise one is left with an indeterminate result as before in the case of the flatness problem.

This then leads to the second question. What one wants to know by asking this question is not how likely it is that an FRW universe undergoes inflation, but rather how likely it is that an FRW universe undergoes *sufficient inflation to solve the flatness problem*.[39] If the universe is spatially flat, then obviously there can be no flatness problem for inflation to solve, in which case there is no reason to ask the question. If the universe is curved, then it is clear that "solving the flatness problem" depends on an assessment of the problem in terms of "near flatness," which, once again, is not possible on the basis of the GHS measure alone.

[39] One can also ask how likely it is that an FRW universe undergoes sufficient inflation to solve the horizon problem (Remmen and Carroll, 2014), but, as explained in (McCoy, 2015), the horizon problem is only a problem because it is a constraint on solving the flatness problem and the uniformity problem.

If one simply wants an answer to the question, regardless of its relevance to the problem inflation is meant to solve, then one can attempt some calculations. Schiffrin and Wald (2012, §IV) elect to treat the $k = 0$ case for a scalar field in a simple self-interaction potential $V = m^2\phi^2/2$, i.e. the slow roll inflation scenario. In this case the GHS volume element simplifies to

$$d\Omega \propto a^2 \sqrt{\frac{3}{4\pi}H_*^2 - m^2\phi^2}\, da d\phi. \tag{7}$$

They consider the histories of inflating spacetimes in the slow roll regime and show that spacetimes which undergo at least N e-folds of inflation are the ones for which $|\phi| \gtrsim 2\sqrt{N}$. Thus the GHS-measure of this set is proportional to

$$\int a^2\, da \int_{2\sqrt{N}}^{\sqrt{3/4\pi}H_*/m} \sqrt{\frac{3}{4\pi}H_*^2 - m^2\phi^2}\, d\phi. \tag{8}$$

The ϕ integral is finite; the a integral obviously is not. Moreover, it is clear that the set of spacetimes which do not undergo at least N e-folds of inflation also has infinite GHS-measure. Therefore the likelihood of inflation is (by now unsurprisingly) indeterminate.

Both Carroll and Tam (2010) and Gibbons and Turok (2008) attempt to overcome this problem with the GHS measure by regularizing the integrals to make them finite. They also choose to evaluate the likelihood of inflation at different values of H_*. Naturally they come up with different answers, since dealing with the divergence in the GHS measure requires making some (potentially consequential) choice to make the measure finite. There is no canonical choice: different choices will lead to different results.[40]

To illustrate the point in a simple way, consider the following. Since the GHS measure factorizes into two integrals, one over a and one over ϕ, one might think to define a probability measure μ_ρ with probability distribution ρ defined by

$$\rho = \frac{1}{\int d\Omega}. \tag{9}$$

Then μ_ρ is simply

$$\frac{\int a^2\, da \int_{2\sqrt{N}}^{\sqrt{3/4\pi}H_*/m} \sqrt{\frac{3}{4\pi}H_*^2 - m^2\phi^2}\, d\phi}{\int d\Omega} = \frac{\int_{2\sqrt{N}}^{\sqrt{3/4\pi}H_*/m} \sqrt{\frac{3}{4\pi}H_*^2 - m^2\phi^2}\, d\phi}{\int \sqrt{\frac{3}{4\pi}H_*^2 - m^2\phi^2}\, d\phi.}, \tag{10}$$

since the integral over the scale factor drops out. This measure is not invariant under time evolution however because part of the time-invariant measure has effectively been thrown away. Depending on how one chooses H_* one will therefore compute (potentially wildly) different probabilities of inflation (Schiffrin and Wald, 2012).[41]

Schiffrin and Wald conclude their analysis with the following thoughts:

> Should one impose a cutoff in a at, say, the Planck time and conclude that inflation is highly probable? Or, should one impose a cutoff in a at a late time and conclude that inflation is highly improbable? Or, should one impose an entirely different regularization scheme and perhaps draw an entirely different conclusion? Our purpose here is not to answer these questions but to emphasize that, even in this simple minisuperspace model, one needs more information than the GHS measure to obtain the probability of inflation. (Schiffrin and Wald, 2012, 12)

[40] This issue is thoroughly discussed in (Schiffrin and Wald, 2012).

[41] Carroll has since acknowledged Schiffrin and Wald's criticism: "The procedure [Tam and I] advocated in (Carroll and Tam, 2010) for obtaining such a measure was faulty, as our suggested regularization gave a result that was not invariant under a choice of surface on which to evaluate the measure" (Carroll, forthcoming , 19).

I am in agreement with their final point (that one requires more information than the GHS measure), but it seems to me that the situation is more intractable than they let on: there is in fact no adequate choice of cutoff in a. In short, the "regularization schemes" are employed for the instrumental purpose of deriving a finite measure—they cannot be justified by the physics of FRW spacetimes alone.

4.4 The Uniformity Problem

For many reasons one should not take the results concerning likelihoods in FRW spacetimes too seriously however. For example, Carroll and Tam (2010, 21) note that "examining a single scalar field in minisuperspace is an extremely unrealistic scenario;" Schiffrin and Wald (2012, 12-3) also observe that "minisuperspace is a set of measure zero in the full phase space. Even if we are only interested in nearly [FRW] solutions, it is far from clear that the GHS measure will give a valid estimate of the phase space measure of the spacetimes that are 'close' to a given [FRW] solution." The approach taken in both of the just-cited papers is to examine the analog of the GHS measure on perturbed FRW spacetimes. Obviously this does not solve the "measure zero" problem, since one can run the same argument on perturbed FRW spacetimes as one did with FRW spacetimes—likelihoods assigned to the perturbed FRW spacetimes are only significant if they are consistent with likelihoods assigned to the full space of possible cosmologies.[42] Presumably, however, what one aims for is some "inductive" support for conclusions which are consistent in both the containing and contained reference classes, since it is not entirely clear what the set of possible cosmologies is.

Nevertheless, setting the reasonableness of proceeding to the side, for the uniformity problem to be a problem of likelihoods one must obviously consider a larger set of spacetimes than the FRW spacetimes, since these are by definition spatially uniform. The technical details involved in constructing a Liouville measure on perturbed FRW spacetimes are somewhat more complex than the technicalities so far discussed and not particularly illuminating, so I will only mention the relevant results and crucial assumptions.[43] The canonical volume element Ω on "almost" FRW models (according to Schiffrin and Wald (2012)) is

$$\Omega_{GHS} \wedge \left(\frac{a(H_*^2 + ka^{-2})}{H_*(3H_*^2 - V + 3ka^{-2})}\right)^{\mathcal{N}_1} \times \prod_{n=1}^{\mathcal{N}_1}(k_n^2 - 3k)\mathrm{d}\Phi^{(n)} \wedge \mathrm{d}\delta^{(n)} \wedge \left(\frac{1}{4}a^3\right)^{\mathcal{N}^2} \prod_{n=0}^{\mathcal{N}_2} \mathrm{d}\dot{h}^{(n)} \wedge \mathrm{d}h^{(n)}. \tag{11}$$

Here there are additional terms (beyond those in the FRW volume element Ω_{GHS}) involving inhomogeneous scalar perturbations (Φ and δ) and tensor perturbations ($\dot{h}$ and h).[44] $\mathcal{N}_1$ and $\mathcal{N}_2$ correspond to short-wavelength cutoffs for the scalar and tensor modes, respectively. These are necessary to make the phase space finite. One must also impose a long-wavelength cutoff, which Schiffrin and Wald implement by restricting attention only to spatially compact spacetimes. Finally, some explication of "almost" FRW must be made: Schiffrin and Wald take it to mean that the magnitude of the metric perturbation Φ and the magnitude of the density perturbation δ are small in comparison to the background FRW metric, as do Carroll and Tam (2010).

Although some of the assumptions made to obtain this volume element may be challenged, the main problems revealed by adapting the GHS measure to perturbed FRW spacetimes are, perhaps unsurprisingly, the same as before. It will only belabor the points made already in the previous sections to go into detail, so I will only mention the essential point. As before, the total measure of phase space is infinite, so probabilistic arguments cannot be made on the basis of the canonical measure alone. Indeed, Schiffrin and Wald note that "including more perturbation modes makes the large-a divergence more severe" (Schiffrin and Wald, 2012, 17). It follows that the results on the probability of inflation given by Carroll and Tam

[42] Schiffrin and Wald are clearly aware of this point, but still forge ahead with the calculations.

[43] The interested reader is directed to (Schiffrin and Wald, 2012, §V) and to (Carroll and Tam, 2010, §5) for further details.

[44] Note that h here is not the spatial metric as it was previously.

(2010) cannot to be trusted because an arbitrary choice has to be made to derive them. Once again, one can get any probability one wants by a particular choice of H_* (the value of the Hubble parameter where the measure of sets of spacetimes is evaluated).

Can one nevertheless make a likelihood argument with respect to uniformity? Is there a uniformity problem according to the canonical measure? Carroll and Tam (2010, 25) claim that there is: “There is nothing in the measure that would explain the small observed values of perturbations at early times. Hence, the observed homogeneity of our universe does imply considerable fine-tuning; unlike the flatness problem, the horizon problem is real.” In some sense this conclusion is (intuitively) correct, since one expects that (nearly) uniform spacetimes are highly unlikely given all the spacetimes that seem physically possible. This goes for all spacetimes with symmetry, insofar as one takes GTR to delimit the space of possible cosmologies. Of course if one makes this claim, then one should say the same thing about spatial flatness (which is plausibly even less likely in the space of possible cosmologies). But these conclusions have nothing to do with the GHS measure; rather they are judgments based on expectations related to the space of possible cosmologies.

5 Conclusion

I have discussed the formal implementation, interpretation, and justification of likelihood attributions in cosmology. A variety of arguments and issues were raised which, taken together, strongly suggest that the use of probabilistic and similar reasoning is misplaced in the context of single-universe cosmology.[45] Some of these concerned conceptual problems and some concerned technical problems. Some of these concerned independent considerations in cosmology and some concerned the application of considerations from statistical mechanics in cosmology. In all cases the verdict is the same: likelihood reasoning is most likely highly unreliable and unjustified in cosmology.

Since the discussion was widely ranging, a brief summary of the main points is in order.

The first issue I raised was the well-known reference class problem from probability theory (§2.1): What is the appropriate reference class of cosmologies for attributing cosmological likelihoods? The problem is particularly important in the context of likelihood arguments, since such arguments depend sensitively on the choice of possibility space. Although the space of models of GTR is a natural choice for this space, it is not necessarily the correct one. Nevertheless, it seems plausible to suppose that the space of models is “large” like the space of relativistic spacetimes. In this case one requires a way to attribute likelihoods on the full space of possibilities, since the appropriate likelihood attributions to subsets of this space may depend on the likelihoods on the full space. The formal challenges of implementing some likelihood measure on the full space was related in §3. Although there is potential for much work here, it is rather doubtful whether these challenges can be fully met.

However, even if they can be met, the attribution of likelihoods in cosmology faces significant conceptual difficulties. First is the difficulty in interpreting these likelihoods (§2.2). The only available interpretation of cosmological probabilities (in the particular sense of locating the origin of the probabilistic “randomness”) is that they pertain to an initial random trial to select an otherwise deterministically-evolving universe—the “god throwing darts” interpretation. While this strikes me as a reductio ad absurdum of the project of attributing probabilities to entire universes, it is at the very least coherent. If this is the only interpretation, though, then it becomes quite hopeless to justify any particular choice of probability measure (§2.3): any choice of measure which attributes a non-zero probability to the model(s) describing our universe is admissible, in the sense of being empirically adequate.

One might avoid this latter problem by proposing that the choice of measure is a priori. This, however, is a counsel of desperation. It has been claimed that probability and typicality measures are “natural” in

[45] Some of these considerations carry over to multiverse cosmology as well, but this topic is, again, outside of the scope of the present paper.

statistical mechanics. There is indeed a precise sense in which some mathematical objects "come for free" with some mathematical structure, but that does not imply that their physical interpretation comes for free. In any case, for the spaces of possibilities considered by cosmologists there is no natural probability measure, since the total measures of these spaces are infinite. A choice has to be made, and none of the choices made by cosmologists is well-motivated, let alone well-justified.

Finally I considered a specific case considered in the physics literature. For certain spaces of possibilities, e.g. minisuperspace, there is a natural measure, namely the Liouville measure associated with the phase space of minisuperspace, called the GHS measure (§4). I showed (relying partly on arguments made in (McCoy, forthcoming) and (Schiffrin and Wald, 2012)) that the GHS measure cannot be used for the purposes to which it has been put by cosmologists: it cannot be applied to the flatness problem (§4.2); it cannot be used to calculate the likelihood of inflation in FRW spacetimes; it cannot be applied to the uniformity problem (§4.4). In each case the essential issue is that there is no typical spacetime in the spaces of possibilities considered. Thus one must introduce a choice (of cutoff, of probability measure, etc.), none of which is well-motivated or justified. This discussion, however, is entirely academic, since the correct likelihood measure on these spaces depends, as said, on the correct measure of the full space of possibilities. What is natural on the latter space may not be natural on the former spaces.

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