Title
Philosophical Implications of Inflationary Cosmology

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Publication Date
2016

Peer reviewed|Thesis/dissertation

UNIVERSITY OF CALIFORNIA, SAN DIEGO

Philosophical Implications of Inflationary Cosmology

A Dissertation submitted in partial satisfaction of the
requirements for the degree
Doctor of Philosophy

in

Philosophy

by

Casey David McCoy

Committee in charge:

Professor Craig Callender, Chair
Professor Nancy Cartwright
Professor Brian Keating
Professor Kerry McKenzie
Professor David Meyer

2016

The Dissertation of Casey David McCoy is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California, San Diego

2016

DEDICATION

I dedicate this dissertation to the inquiring minds of St. John's College, who revealed to me a beautiful life of thought.

EPIGRAPH

The edge of evening...the long curve of people all wishing on the first star.... Always remember those men and women along the thousands of miles of land and sea. The true moment of shadow is the moment in which you see the point of light in the sky. The single point, and the Shadow that has just gathered you in its sweep...

—T. Pynchon, *Gravity's Rainbow*

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ACKNOWLEDGEMENTS

I acknowledge and express my gratitude to the many people who contributed to the completion of this dissertation. There are a few people that deserve a special thanks:

My greatest debt and gratitude go to my supervisor, Craig Callender, who encouraged me to take up inflation as a dissertation topic and provided invaluable guidance and suggestions throughout. His ability to master complex physics in his research while remaining resolutely focused on making important philosophical points *and* being engaging and accessible provides a model I have tried to emulate. I hope that this dissertation demonstrates his influence on my approach to philosophy.

I would also like to thank Nancy Cartwright and Kerry McKenzie for serving on my committee. Reading Nancy's influential work is what brought me to the philosophy of science, and I feel very fortunate to have had her advice, guidance, and assistance during my time at UCSD. Although Kerry only joined my committee quite recently, she has been wonderfully open to discussing my ideas and has been a great source of encouragement and inspiration. It is a great pleasure to have such philosophers whom I admire so much as a part of my dissertation committee.

Brian Keating and David Meyer deserve my hearty thanks for serving as committee members from outside the philosophy department. I am grateful to Brian in particular for the valuable role he played in the early stages of my dissertation, contributing significantly to my education in cosmology (I had a lot to learn about the subject!). I appreciated David's enthusiasm for my project and encouragement to share my results with his group in mathematics. If only I had been in San Diego over the last years, I would have been more easily able to take him up on his friendly offer!

Due to his departure from San Diego for the green pastures of Switzerland, Christian Wüthrich unfortunately had to leave my committee. However his contributions to my philosophical education and this project were many and great; I am deeply grateful for his dedication and willingness to reading and commenting on my earlier drafts. He also was always glad to dispense valuable professional advice, and served as a valuable model of a careful, thorough, and rigorous philosopher of physics.

I also owe a great deal to Richard Dawid, who has been a tremendous influence on this dissertation as well as my general philosophical outlook. Richard generously offered his time during my stints in Vienna for regular discussions of his work and my dissertation. I will never forget the many pleasant afternoons discussing philosophy at the Café Eiles in one of the great cities of the world. He has also been an unflagging supporter of my fledgling career, for which I cannot thank him enough. Besides Richard, I would also like to express my appreciation to others I met during my time in Vienna, especially Dejan Makovic and the rest of the Vienna Forum for Analytic Philosophy.

My subsequent time in Nijmegen also brought me into contact with wonderful colleagues. I especially thank Klaas Landsman and Fred Muller for their generous welcome and interest in my work. I feel incredibly fortunate to have gotten to know them while in the Netherlands. I also appreciate Klaas's enthusiasm for bringing a bunch of theoretical physicists together to talk philosophy, among whom I wish especially to thank Francesca Vidotto for being a great friend and a wonderful philosophical interlocutor.

The Department of Philosophy at UCSD has been a wonderful place to study philosophy. I thank the many members of the faculty that have contributed to my development—a special thanks to Eric Watkins for going well beyond the call of duty in helping me along. I have been quite lucky also to have been in close enough proximity to UC Irvine to study with the several excellent philosophers of physics there too. I have learned a lot about philosophy and more from David Malament, who welcomed me into one of his courses and made a strong impression on my approach to philosophy. I would like to take this opportunity to thank him in particular.

Finally I must acknowledge the many contributions and friendship of my fellow graduate students at UCSD, who have been a constant source of encouragement and support over the years. I recognize especially John Dougherty, with whom I have spoken more about philosophy than probably everyone else put together. My knowledge of physics, mathematics, philosophy, and far more would not be what it is if it were not for John. His patience in listening to my many (often random or confused) thoughts is just one way in which John has been a colleague without compare. Gil Hersch and Alex Marcellesi have been great friends and also willingly read a whole lot of my physics-heavy papers and forced me to make sense of it to them. Insofar as my work is accessible to a general audience, it is surely due to their helpful suggestions and discussion.

Chapter 2 includes material that is published in McCoy, Casey. “Does inflation solve the hot big bang model’s fine-tuning problems?” *Studies in History and Philosophy of Modern Physics* 51: (2015) 23-36. The dissertation author was the sole investigator and author of this paper.

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ABSTRACT OF THE DISSERTATION

Philosophical Implications of Inflationary Cosmology

by

Casey David McCoy

Doctor of Philosophy in Philosophy

University of California, San Diego, 2016

Professor Craig Callender, Chair

This dissertation is an investigation of the conceptual and physical foundations of cosmological inflation. Cosmologists have long claimed that inflation solves certain fine-tuning problems with the previous standard model of cosmology, the highly successful big bang model of the universe. The major contributions of the dissertation are an analysis and a critique of the arguments cosmologists could be construed as making to support this claim, arguments which have been taken as the standard rationale for the initial (and even continuing) widespread acceptance of inflation in cosmology. There are four chapters. In the first I establish the context of the

investigation by clarifying the notion of a cosmological model (the central component of which is a relativistic spacetime) and explaining important features of the cosmological models underlying the big bang model. In the second chapter I analyze and criticize the aforementioned standard motivation for the inclusion of inflation in modern cosmological models and address the question of whether inflation solves the big bang model's fine-tuning problems. The main conclusion of this chapter is that there is at present no good argument that inflation solves the big bang model's fine-tuning problems. In chapters three and four I delve deeper into the fine-tuning argument apparently favored by cosmologists, namely the one that depends on interpreting fine-tuning in terms of probabilities or likelihoods. In chapter three I show how probabilities are implemented, interpreted, and justified in classical statistical physics, introducing a novel interpretation of statistical mechanics along the way. In the final chapter I put the formal implementations, the interpretations, and the justification of probability from chapter three to work in the context of cosmology. I ultimately claim that the many challenges I raise in the chapter are decisive for current and even forseeable approaches to defining cosmological probabilities.

Introduction

This dissertation is an investigation of the conceptual and physical foundations of cosmological inflation, one of the central components of the modern standard theory of cosmology, the ΛCDM model. The ΛCDM model is essentially an elaboration of the previous standard model of cosmology, the well-known hot big bang (HBB) model. Whereas in the HBB model it was assumed that only matter and radiation of the kinds investigated by other branches of physics exist in the universe, the ΛCDM model also incorporates dark energy (which may be in the form of the cosmological constant Λ), cold dark matter (CDM), and a brief period of inflationary expansion (which may be caused by an unknown scalar field called the inflaton). This accelerated, exponential spatial expansion is assumed to occur in the very early universe.[1] (One may usefully and intuitively compare it to the inflation of a balloon, which decreases the curvature of the balloon's surface and smooths small irregularities.) The microphysical natures of these additions are under investigation, but remain unknown at the present; they at least do not appear to be related to anything in the standard model of particle physics. Observations however support the inclusion of the additional dark components, indicating that the present energetic constituents of the universe are dark energy (69% of the total energy density), dark matter (26%), and normal matter and radiation (5%).

Inflationary theory plays an interestingly different role in the ΛCDM model than dark energy and dark matter. Cosmologists have long maintained that inflation solves certain fine-tuning problems with the HBB model (Guth 1981; Albrecht and

[1] The idea was originally proposed by Guth in the early 1980s (Guth 1981), with the basic idea also found in work that appeared shortly after Guth's seminal paper (Albrecht and Steinhardt 1982; Linde 1982).

Steinhardt 1982; Linde 1982, 1984; Olive 1990; Dodelson 2003; Mukhanov 2005; Weinberg 2008). Observations over the past several decades have increasingly indicated that the spatial geometry of the universe is close to flat; they have also indicated that the constituents of the universe are remarkably uniform (on large, cosmological length scales). Cosmologists have argued (and continue to argue) that the HBB model requires "special" initial conditions in order to explain this observed flatness and uniformity of the universe. Due to (for example) their extreme precision or intuitive "unlikeliness," these initial conditions are thought to be unduly special, such that many cosmologists have felt that the initial conditions themselves are in need of explanation, and moreover present a significant conceptual problem for the HBB model. Cosmologists have offered inflationary expansion as an explanation for the presently observed conditions and as a solution to the problem, for it remains generally supposed that inflation gives rise to flatness and uniformity dynamically, and in a way that makes these conditions expected (Belinsky et al. 1985).

The major contributions of the dissertation are an analysis and a critique of this argument, which has been taken as the standard rationale for the initial (and even continuing) widespread acceptance of inflation in cosmology. There is, however, another important rationale for the adoption of inflation: Cosmologists generally believe that inflationary theory is empirically confirmed. When inflation is treated as a quantum theory, one obtains precise predictions of departures from the observed general uniformity of the universe, as was quickly discovered by cosmologists working on inflation and similar ideas (Mukhanov and Chibisov 1981; Mukhanov 1982; Guth and Pi 1982; Hawking 1982; Starobinsky 1982; Bardeen et al. 1983). These departures in particular appear in what is known as the cosmic microwave background (CMB) radiation, a remnant of the decoupling of photons from matter in the early universe, and have been spectacularly confirmed by satellite observations in the last couple of decades. Although assessing the empirical confirmation of inflationary theory is an interesting project worthy of careful investigation, it is beyond the scope of this dissertation.[2]

Although a clear account of the HBB model's fine-tuning problems and of how inflation is meant to solve them is of value in its own right, an analysis of this case is significant for its implications to broader issues in the philosophy of science as well. I note especially one particular motivating line of thought here. It begins with remarks by Earman and Mosterín (1999), who emphasize what I described above, namely that the HBB model's fine-tuning problems are not problems concerning the HBB model's consistency or empirical adequacy; rather the problems appear to raise concerns over the kind of explanation given by the model for certain physical features of the universe—features of the universe which are accessible to observation such as spatial uniformity and flatness. One might wonder though, "How can solving such mere explanatory problems represent progress towards an empirically successful theory?"

The context of inflationary cosmology provides a concrete case to investigate this question. At present the best argument for inflationary theory is not that it (allegedly) solves the HBB model's fine-tuning problems; instead it rests on the striking empirical confirmation of inflationary theory's predictions of a very precise spectrum of anisotropies of the CMB (White et al. 1994; Hu and Dodelson 2002).[3] If this

[2]See (Ijjas et al. 2013; Guth et al. 2014; Ijjas et al. 2014) for recent discussion based on observational results from the Planck satellite, and (Smeenk 2003, Ch. 7) for a philosophical assessment of the empirical confirmation of inflation which raises several important concerns.

[3]Potentially the CMB's polarization could play a confirmatory role as well (Kosowsky 1996). (Planck Collaboration 2015) has the latest observational results for both the anisotropy and polariza-

latter argument is successful—and it at least appears to be taken as such by most contemporary cosmologists—then inflationary theory may be reasonably considered an empirically successful theory whose predictive successes go beyond the HBB model. Therefore the inclusion of inflation in our cosmological models represents progress over the HBB model.

Yet it is important to note that these predictions were unforeseen at the time of inflation's proposal. Insofar as scientific progress may be gauged by solving scientific problems (Kuhn 1996; Laudan 1978), one nevertheless has an explanatory story linking inflationary theory's putative success at solving the HBB model's fine-tuning problems with the later confirmation of its observational predictions. Roughly speaking, one might say that by solving the HBB model's conceptual problems, inflationary theory proves itself to be a progressive research program suitable for further development and empirical test. Although even so there is no guarantee that its predictions will be borne out, one's confidence in the theory is justified by its past problem-solving success.

The viability of some such story depends however on whether inflation does in fact solve the HBB model's fine-tuning problems. If it does not, then the widespread adoption of inflationary theory well in advance of its striking empirical confirmation demands some other philosophical rationalization, else one is left to attribute this recent progress in cosmology on the irrational adoption of an ill-motivated idea—a collective decision which turned out to be implausibly lucky given the (ostensible) eventual empirical confirmation of the idea.

These considerations lead to a general argument that favors relaxing from

tion spectrums.

the strongly empiricist stance found in physics and in much philosophy of science (including that found in (Earman and Mosterín 1999)). I introduce this argument only to show how significant the case of inflation is for issues in the philosophy of science, and the ramifications of the work I undertake in this dissertation. First, to recap a few salient points from above, recall that in 1980 the old standard model of cosmology, the HBB model, was, within its ranges of applicability, empirically adequate and consistent. It could however only explain the observed flatness and uniformity of the universe by assuming extremely special initial conditions of the universe. One therefore would say that the Big Bang model is fine-tuned. Now, cosmologists claimed that this fine-tuning is problematic, and moreover that inflation solves these fine-tuning problems. On the basis of its solution to these fine-tuning problems, inflation is quickly and widely accepted within the discipline, although it appears initially to make the same empirical predictions as the Big Bang model.

At this point there is a general question to ask: Why is inflation accepted? Is it merely for sociological reasons? Or are there some methodological grounds? There were, at least, no apparent empirical reasons to do so. In familiar historical cases, like that of phlogiston theory, a prior theory tends to retain standing until there is an empirical reason to abandon it. Nevertheless, the attention paid to inflation quickly led to the discovery of novel predictions of a precise spectrum of tiny irregularities in the uniformity of the universe. These predictions were much later confirmed by satellite experiments built to go look for them. So, why should a theory that had no empirical motivation be so surprisingly or improbably successful? It seems that either cosmologists were extraordinarily lucky in adopting the theory (and their adoption of it was merely due to sociological reasons), or there is a methodological explanation for it (which in this case appears to be based on problem solving or explanation). Since it seems that we should generally favor methodological explanations over attributions of extraordinary luck, one naturally concludes that there is a truth-conducive (meta-empirical) method that explains the (empirical) success of inflation and its adoption by cosmologists.

The plan of the dissertation is as follows.

In chapter 1 I establish the context of the investigation. Cosmology as a science is principally concerned with modeling the large-scale structure of the universe through the use of the general theory of relativity (GTR), models of which are relativistic spacetimes. The crucial modeling assumptions underlying the HBB model are (roughly) that the universe can be decomposed into space and time, and what is known as the cosmological principle (CP), which holds that space and its material constituents are (nearly) uniform (in a precise sense). All the foregoing assumptions are standard in cosmology, and so are rarely explicitly justified. Although my general aim is not to investigate these basic assumptions and their justification, it is important to clarify them and their significance, for many of these details matter for my investigation.

Accordingly, I detail some salient features of cosmological models (§1.2) such as their ranges of applicability (§1.2.3), briefly explore the rationale for using relativistic spacetimes as such (§1.2.1), and indicate why I restrict attention to single universe cosmological models and do not consider multiverse models (§1.2.2). I clarify the symmetry assumptions in standard cosmology (the CP) (§1.3.1), present details of the Friedman-Robertson-Walker (FRW) spacetimes that follow from these assumptions (§1.3.2), introduce the initial value formulation of GTR (§1.3.3), and provide some important facts concerning the more realistic perturbed versions of FRW spacetimes

(§1.3.4). The existence of cosmological horizons in FRW spacetimes is important for understanding the HBB model's fine-tuning problems, so I summarize the arguments that reveal the presence of singularities of FRW spacetimes (§1.4.1) and therefore the presence of particle or event horizons therein (§1.4.2). Finally I relate in brief the observational basis for the HBB model (§1.5).

In the first half of chapter 2 I analyze and criticize the aforementioned standard motivation for the inclusion of inflation in modern cosmological models. First I rehearse the physical facts which lead physicists to intuit that the HBB model has fine-tuning problems (§2.2)—the horizon problem, the uniformity problem, and the flatness problem. The analysis and critique of their general argument scheme follows (§2.3). I consider several possibilities for interpreting the specialness of the required initial conditions and for judging them problematic, including that the initial conditions are too precise, that they are too symmetric, and that they are too unlikely. I argue that cosmologists' fine-tuning allegations are best interpreted as stating the latter. One would then say that a cosmological model is fine-tuned if it is improbable or unlikely according to some attribution of probabilities. In this chapter I merely assert that the attribution of probabilities to cosmological models is unjustified and fails to have a sensible interpretation; in the chapters 3 and 4 I support this claim fully by raising several conceptual and technical challenges which show the prospects of cosmological likelihoods to be quite dim at best, and empty at worst. Although some of the other possible, non-probabilistic ways to understand fine-tuning evade these considerations, I argue that there are strong prima facie challenges to the viability of these interpretations as well. I therefore conclude that there is at present no compelling justification for the adoption of inflation based on fine-tuning arguments.

In the second half of chapter 2 I address the question of whether inflation solves the HBB model's fine-tuning problems, supposing for sake of argument that these fine-tuning problems can be adequately justified. I supply some useful facts about inflation in the context of GTR (§2.4.1), as well as some details about the implementation of inflation as a scalar field (the inflaton) (§2.4.3). I discuss the usual stories of how inflation solves the fine-tuning problems, including the flatness problem and the uniformity problem (§2.4), and the horizon problem (§2.4.2). How inflation solves these problems ultimately depends on the interpretation of fine-tuning and what counts as a solution to a fine-tuning problem (§2.5.1). I therefore conclude the chapter by assessing whether inflation does in fact solve the fine-tuning problems according to the interpretations given in the first part of the chapter (§2.5.3), using a familiar debate over the need to explain the supposed low entropy state of the universe as a preliminary example (§2.5.2). In some cases it can be said that inflation undoes a fine-tuning, e.g. when fine-tuning is understood as a high degree of symmetry, however in these cases it is unclear why the fine-tuning is problematic. In other cases it cannot be said that inflation solves the fine-tuning problem, e.g. when fine-tuning is understood as a matter of likellihood.

In chapter 3 I begin to make good on my claim that the likelihood interpretation of fine-tuning fails. To understand how likelihoods (such as probabilities) can have physical significance, it is worth looking at how they are successfully implemented elsewhere in physics (§3.2). Indeed, cosmologists' intuitions about likelihoods appear to come from their implementation elsewhere in physics, especially in classical statistical mechanics. However many of these intutions, I suggest, are based on an inadequate appreciation of the philosophical and foundational issues involved in statistical mechanics.

Since the possible formal explications of likelihood are manifold, I mostly concentrate on the most familiar measure-theoretic applications of probability in physics; I do so also because the most prominent approach to cosmological probabilities (Gibbons et al. 1987) adapts the measure-theoretic implementation of probabilities in classical statistical mechanics to GTR.

I first demonstrate how the Liouville measure naturally arises from the symplectic geometry of classical mechanics in the hamiltonian formulation (§3.2.1). Since GTR is a classical field theory with an infinity of degrees of freedom and the symplectic manifolds of classical statistical mechanics are finite-dimensional, it is necessary to generalize the methods of the latter to classical field theories in order to attribute cosmological probabilities to cosmological models in all generality (§3.2.2). However there are clear technical problems facing this naive approach (for which reason, assumedly, cosmologists restrict attention to sets of cosmologies which can be described with a finite number of degrees of freedom). I then discuss the justification and interpretation of statistical mechanical probabilities (§3.3) to set up the following chapter, where I assess whether cosmological probabilities can be justified and interpreted similarly. Since the interpretation of probability carries a particular sense in philosophy, I first serve notice that I will be characterizing interpretation somewhat differently than usual (§3.3.1). My interest is not so much with general conceptual analyses of probability, but with identifying what aspect of a statistical mechanical system is stochastic. I argue that there are essentially three (observationally indistinguishable) things that can be considered random in statistical mechanics: the observable properties of a system, the microstate of the system, and the initial conditions of the system (§3.3.2). I then show how the so-called Gibbsian interpretation of statistical mechanics is best understood as a "stochastic observables" interpretation (§3.3.3), and the so-called Boltzmannian interpretation is best understood as a "stochastic initial conditions" interpretation (§3.3.4). I conclude the chapter by arguing that the justification of a particular choice of probability measure in statistical mechanics is not a priori, but must be a posteriori (§3.3.5).

In chapter 4 I put the formal implementations, the interpretations, and the justification of probability from chapter 3 to work in the context of cosmology. I first consider three philosophical problems with the application of probabilities to cosmological models (§4.2). I should emphasize that by cosmological probabilities I mean probabilities attributed to relativistic spacetimes. Probabilities find many justifiable applications in cosmology, but these are benign applications of statistical modeling within a spacetime. The first problem is the justification of a reference class (§4.2.1), i.e. of the appropriate space of possible cosmologies. The second is the interpretation of cosmological probabilities (§4.2.2). I argue that the only reasonable interpretation of cosmological probabilities is that they pertain to an initial random trial over the possible cosmologies. The third problem is the justification of cosmological likelihoods (§4.2.3). There I argue that since probabilities must be justified by correspondence with empirical frequencies, there can be no adequate justification in single-universe cosmology—the choice of measure is radically underdetermined and therefore without physical significance. In the next section I consider the implementation of likelihoods on the space of possible relativistic spacetimes (§4.3). I point out several technical challenges that face any such implementation—even if the foregoing conceptual problems can be overcome. Most of the discussion of cosmological likelihoods therefore sensibly concentrates on simpler finite-dimensional spaces of cosmologies, and especially on the measure proposed by Gibbons et al. (1987) (§4.4). I begin this section by giving a careful derivation of the measure by way of the initial value formulation of GTR (§4.4.1). Various cosmologists, including Hawking, Page, and Carroll, have argued that the flatness problem does not exist according to this measure. I show that this interpretation is seriously mistaken, for they overlook some important facts which I point out in my derivation or make arbitrary choices that undermine the justification of the measure (§4.4.2). I conclude by assessing arguments concerning the probability of inflation (§4.4.3) and the uniformity problem (§4.4.4) on the basis of this measure, although with these I mostly echo the judgments of (Schiffrin and Wald 2012) in their critique of this literature.

I ultimately claim that the challenges raised in this chapter are decisive for current approaches to defining cosmological probabilities. I believe they should also incline one to doubt that cosmological probabilities are justifiable or can be well-defined in general—at least insofar as one thinks of them as arising from classical considerations. Thus if inflation is to be adequately motivated as a component of our best cosmological theory, one must find grounds other than that it makes our universe more likely. As said, cosmologists generally agree that the best support for the theory is found in quantum mechanical considerations and their empirical consequences, especially the prediction of departures from uniformity of the cosmic microwave background (a point which deserves a careful philosophical assessment). Indeed, it seems that if cosmological probabilities are to have a firm, objective grounding, they must come from a fundamentally probabilistic theory with cosmological scope, i.e. some theory of quantum cosmology. Perhaps physics would therefore be best served by dropping talk of the horizon and flatness problems altogether (especially in texts and reviews), and concentrate on the issues of understanding the universe as a quantum system, and especially whether inflation can play an important role in this project. Although quantum cosmology goes beyond the scope of this dissertation, I believe that there is good reason to think that inflation can do so.

Nevertheless, the problem of understanding the widespread adoption of inflation remains. The putative empirical success of inflation suggests that cosmologists were right to adopt the theory, i.e. it represents real progress over the HBB model. Yet its adoption was based ostensibly on its success at solving conceptual problems with the HBB model. Although it is possible that these conceptual problems are of little moment and the choice was an irrational guess, it seems preferable to hypothesize that it was methodologically justified. I have argued however that if we interpret the HBB model's fine-tuning problems as depending on improbable initial conditions, then inflation is (probably) not a solution to them. Therefore either the likelihood interpretation of the fine-tuning problems is incorrect and there is another sensible interpretation of these problems, or these problems were not important drivers of scientific progress in cosmology. In the latter case there must be another rational explanation for the adoption and eventual empirical success of inflationary theory, else it seems that inflation was just a tremendously lucky guess. This latter view clashes strongly with the attitude that science progresses rationally. Only in the absence of an alternative should we be inclined toward it.

Although cosmologists appear to favor the improbability interpretation, I have suggested other plausible ways of interpreting the special initial conditions of the HBB model in a way that inflation does solve them. It may be, then, that the fine-tuning of the HBB model is real, and inflation does provide the solution to the fine-tuning problems. But it has not yet been made clear what is problematic about fine-tuning understood in these alternate ways; attempting to make it so seems to me to be a worthy project.

It is also possible that conceptual problems like these are not important or reliable drivers of scientific discovery, or at least not in this case. That is to say, it may be that thinking about such problems leads to the proposal of new theories, yet nothing in the problem statement actually tracks a real, objective problem of the previous theory. If that were the case, it is not so important to ask, "Does inflation solve the HBB's fine-tuning problems?" Rather one might be led to ask different questions altogether...for example: "Why is inflation a good theory?" or "Why was inflation a good theory when it was proposed?" Again, the best argument for inflation now is that it suggests new empirical predictions which can be and have been verified observationally, yet when inflation was proposed, none of these observational consequences was known. At that time there was nothing to suggest that inflationary models were more empirically adequate than the HBB model, but theorists enthusiastically adopted them. Were there reasons to think the inflationary approach was better than continuing with the basic theoretical framework of the HBB model and considering inflation only as a speculative theoretical alternative?

As I show in chapter 2, one of the most salient features of the inflationary mechanism is that it relaxes the horizon constraint while also reversing the dynamical instability of flatness in FRW models. Does this "unexpected explanatory coherence" have any methodological significance? This fact about violating the strong energy condition in FRW models was there all along, but was overlooked (perhaps because "matter" that failed to satisfy the strong energy condition was not thought to exist) until

Guth stumbled upon it (more or less accidentally). But without a prior investigation of the issue, the discovery that both problems are connected together might be seen as a strong suggestion to pursue the solution that solves both. Such a discovery mirrors in a way the unexpected empirical confirmation of novel predictions of a theory, the latter an often acknowledged virtue of a theory. Thus there is an intriguing suggestion that in this episode a kind of theory confirmation not based on direct observation has played an important role, and has moreover led to further scientific progress in cosmology.[4] These foregoing ramifications of this dissertation's arguments are ripe for further investigation.

[4]Amplifying this line of thought, Dawid (2013) argues that unexpected explanatory coherence can be an important facet of assessing a theory's viability and confirmation, particularly in the case of theories like string theory or in the case of cosmology where empirical confirmation often remains out of current, foreseeable, or even potential reach—"it gives the impression that physicists are on the right track" (Dawid 2013, 45). But an argument from such unexpected explanatory coherence can hardly be conclusive—there may be, for example, "so far insufficiently understood theoretical interconnections at a more fundamental level, of which the theory in question is just one exemplification among many others" (Dawid 2013, 46), or the correct theory may solve the problems in independent ways, such that the coherence previously found was ultimately irrelevant and misleading.

Chapter 1

Relativistic Cosmology

1.1 Introduction

Cosmology in the 20$^{\text{th}}$ and 21$^{\text{st}}$ centuries has been mostly based on the relativistic spacetime models of the general theory of relativity. In the latter half of the century attention focused on a particular standard model of cosmology, the hot big bang model, which is based on the Friedman-Robertson-Walker set of spacetimes.[1] The HBB model's successes are many. Particularly notable agreements between its theoretical predictions and observations are (1) expansion as exhibited by redshift-distance relations (e.g. the Hubble diagram), (2) light element abundances in accord with big bang nucleosynthesis (BBN), and (3) the observation of the cosmic microwave background, the relic blackbody radiation released when photons fell out of equilibrium with electrons.[2]

[1] Friedman's name is often written "Friedmann" in Roman script, but he was no German; he was a Russian with only one 'n' at the end of his name: See, e.g., (Friedman 1922)

[2] A classic review of the HBB model's successes is (Peebles et al. 1991). There are numerous physics sources on cosmology or general relativity. As general references I have especially drawn on (Dodelson 2003; Mukhanov 2005; Weinberg 2008; Baumann 2009) for cosmological material and (Hawking and Ellis 1973; Wald 1984; Malament 2012) for material on general relativity.

The HBB model has evolved in recent decades to what is usually called the ΛCDM model, the contemporary standard model of cosmology. It differs from the classic HBB model by introducing three features: (1) dark energy (Λ for the cosmological constant), (2) cold dark matter, and (3) inflation. This dissertation investigates the third feature—inflation. The initial motivation for inflationary theory was concern over certain "scientific problems" (§2.3) with the HBB model. Although the HBB model was highly successful and well-confirmed, there are certain features of it that some physicists have felt were puzzling. The most significant of these problems are the so-called horizon problem and flatness problem, although some others are sometimes mentioned (Linde 1990, 1984).

Chapter 2 is an exploration of the nature of these two problems and how inflationary theory is supposed to solve them. This chapter is intended in part to provide the basic cosmological background necessary for explaining these issues and to address some important preliminary issues. The plan of the chapter is as follows. First I discuss general issues concerning cosmological models (§1.2), such as their content, connection to theory and observation, scope, and applicability. Certain features of cosmology, e.g. the uniqueness of the object of study and the severe limitations on observation, are important themes in what follows. In §1.3.2 I summarize the most important aspects of FRW spacetimes and perturbed FRW spacetimes, the spacetimes which underlie the empirical successes of the HBB model. After that I discuss (§1.4) singularities in GTR and particle horizons in FRW spacetimes, both important topics for understanding the horizon problem. The final section summarizes the status of cosmological observations (§1.5), with particular attention on observations of the CMB.

1.2 Cosmological Models

The scientific practice of cosmology is centrally concerned with constructing models of the universe (or at least of parts of the universe), and testing their predictions against observations. It will be useful to introduce some terminology for discussing these models, which are sometimes referred to as *cosmologies*. A cosmology or cosmological model will be taken to include the following two elements:[3]

1. A *relativistic spacetime*, i.e. a model of GTR (Hawking and Ellis 1973; Wald 1984; Malament 2012). Such a model will sometimes be specified symbolically as $\mathcal{M} = (M, g)$ or $\mathcal{M} = (M, g, T, \Lambda)$, where M is the spacetime manifold, g the spacetime metric, T the stress energy tensor, and Λ the cosmological constant.[4] $\mathcal{M}$ satisfies the Einstein field equation (EFE)

$$R_{ab} - \frac{1}{2} g_{ab} \mathcal{R} - \Lambda g_{ab} = 8\pi T_{ab}, \tag{1.1}$$

   where R is the Ricci tensor and $\mathcal{R}$ is the Ricci scalar.

2. A *physical model* of various observational, cosmological phenomena—including phenomena that have a negligible backreaction on the spacetime geometry and those that are represented explicitly in the spacetime model via T (or Λ). These phenomena may be represented by particles (whose trajectories are represented by a curve in M) or fields (represented by tensor or spinor fields) of various kinds,

[3]An explicit definition of a cosmology is not typical in the cosmological literature, although something much like what I specify is what cosmologists have in mind. Ellis and his collaborators are a notable exception, providing fairly explicit definitions similar to the one given here. Cf. (Ellis and Stoeger 1987, 1698), (Ellis and van Elst 1999, 5), (Ellis 1999, A40), (Ellis et al. 2012, 20). A similar notion can also be found in (Cotsakis and Leach 2002).

[4]M is a Hausdorff, connected, paracompact four-dimensional manifold and g is a metric tensor field associated with M which is of Lorentz signature (1, 3).

e.g. perfect fluids (Misner et al. 1973, ch. 22), classical fields (Wald 1984, app. E), or quantum fields (Wald 1994); one also requires equations which describe their dynamical evolution (represented by the relevant equations of motion). More typically one specifies a statistical model, i.e. a distribution function of the particles in the model and an associated Boltzmann equation for the evolution of this distribution.

I also draw attention to the following two additional features of models for their bearing on the various issues I discuss later:

3. An identification of the empirically accessible properties of the model that provide a link to experiment or observation.[5] Roughly what one wants specified here is a set of predictions that are testable in principle (directly or indirectly), such as power spectra, galactic redshifts or counts, light element abundances, etc.

4. A range of applicability in terms of the physical quantities of the theory. Ranges of applicability for various models are limited in cosmology, for example, by

[5]There is a great deal of complexity in connecting cosmological models to observation(Ellis 1999, A45ff). As a representative example of the various challenges one faces when comparing theory with cosmological observations, Olive, Steigman, and Walker offer the following comments on comparing light element abundances to the predictions from big bang nucleosynthesis:

> To test the standard model it is necessary to confront the predictions of BBN with the primordial abundances of the light nuclides which are not 'observed', but are inferred from observations. The path from observational data to primordial abundances is long and twisted and often fraught with peril. In addition to the usual statistics and insidious systematic uncertainties, it is necessary to forge the connection from 'here and now' to 'there and then', i.e., to relate the derived abundances to their primordial values. It is fortunate that each of the key elements is observed in different astrophysical sites using very different astronomical techniques and that the corrections for chemical evolution differ and, even more important, can be minimized. (Olive et al. 2000, 394)

Although the various difficulties of comparing theoretical predictions to observational data are of considerable interest to investigate, they are beyond the scope of this investigation. The observations and their connection to the models that I discuss will avoid these complications and consider observations to be 'ideal' (Ellis et al. 1985).

choice of some averaging length scale, by energies where a theory is thought not to hold, or by times where assumptions about homogeneity or isotropy may not hold.

In the remainder of this section I comment on some issues attendant to these features of cosmological modeling, ones that are either of some interest or are recurring themes in what follows (the empirical significance of these models is discussed separately in §1.5).

1.2.1 Relativistic Spacetimes

It is plain that not all cosmologies which have ever been devised fit into the definition of cosmological model as a relativistic spacetime and physical model. Nevertheless, modern cosmology is principally concerned with the spacetime models of GTR. These spacetime models usually do not appear with a detailed physical model in practice, since the matter contents, as far as GTR is concerned, are completely represented by the stress energy T. In many applications the non-gravitational interactions of matter, as one would find in a physical model, are ignored and important general results achieved, for example gravitational lensing and the redshift-distance relation. Indeed, relativistic spacetime models, such as the standard FRW models, are frequently referred to as cosmological models themselves, without a full specification of the matter content of the model, because of gravitation's importance to cosmological dynamics. My rationale for decomposing cosmological models into a relativistic spacetime model and a physical model is (on the one hand) to highlight the importance of general relativistic dynamics on cosmological scales, and (on the other) to stress that the details of the matter contents of the model nevertheless depend on some underlying physics.

Why do gravitational phenomena play such an important role on cosmological scales? One occasionally finds attempts at an explanation by way of what one might call a "physicists' a priori" argument.[6] (That is to say, it is made prior to confirmation of a model, not prior to experience altogether.) It is somewhat of an aside to pursue the matter, but for a basic understanding of why cosmological models are essentially relativistic spacetimes it is worth a few words.

Physicists often remark that there are four fundamental "forces" which operate in our present universe. Three of these (the electromagnetic force, the weak nuclear force, and the strong nuclear force) are given a fundamental presentation through quantum field theory (QFT), namely in the standard model of relativistic particle physics (where "forces" arise from interactions via bosonic fields). The fourth, gravitation, is presently understood not as a force like the others (though particle physicists conjecture a particle, the graviton, that mediates gravitational interactions), but rather as spacetime curvature's influence on the motion of physical objects, which objects influence the curvature of spacetime in return. All known physical interactions therefore are presently understood to take place fundamentally in a relativistic spacetime of some kind: in elementary QFT this is Minkowski space; in GTR it is any relativistic spacetime permitted by the theory.[7]

The argument for why the gravitational force is the only relevant one for cosmological phenomena is by elimination. The nuclear forces are "short-range" forces,

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[6]Examples of the following argument can be found in (Ellis et al. 2012, 19) and (Ellis 2007, 1185).

[7]If all known physical forces were adequately unified under a single theoretical framework (as in string theory) or if there were just an adequate theory of quantum gravity, then a cosmological model would presumably be a model of this theory. Since no quantum theory of gravity has proven itself capable of producing physically applicable models as defined above, cosmological models are presently based on theories that may be viewed as incomplete.

so they cannot be directly responsible for the dynamics of large-scale structures in the universe. Both electromagnetism and gravitation by contrast are "long-range" forces which could be theoretically important to cosmological dynamics.[8] One then claims that electromagnetism is plausibly irrelevant (one would observe annihilation or repulsion on large-scales, and macroscopic bodies tend to be electrically neutral as well)—despite being enormously ($\sim 10^{25}$) stronger than gravity—whereas gravity is plausibly relevant (gravitational attraction is observed on large-scales and cannot be shielded). Thus one concludes that the basic ingredient of a cosmological model is gravity. Since our best theory of gravity is GTR, cosmology should be based upon general relativistic spacetimes.[9].

Although the argument appears straightforward and fairly plausible, there are nonetheless some significant assumptions being made. For example it assumes that the four fundamental forces are all the forces that exist in the universe. The evidential warrant, however, for these forces is derived from particular and proximate circumstances, viz. experiments on Earth or perhaps at best in our galactic neighborhood. By describing them as fundamental, one is clearly assuming the universality of these forces. Certainly this assumption is widely accepted. One need not, however, grant the universality of the theories that describe these forces.[10]

[8]Other long-range forces are not theoretically excluded (Dolgov 1999), and there have been some proposals of new forces to explain cosmological observations (Brisudova et al. 2002). Since the argument is by elimination of known forces and new forces have not been shown to exist, they are not relevant to this argument.

[9]Of course this is not to say that nuclear and electromagnetic effects are irrelevant—indeed they are, but they appear in the physical model of a given cosmological model. And just as one may use Newtonian gravitation theory as an approximation in appropriate circumstances, one may of course use classical electromagnetism, hydrodynamics, and other theories when they hold approximately.

[10]Cartwright, for example, claims that

> ...granting the validity of the appropriate inductions, we have reason to be realists about the laws in question. But that does not give us reason to be fundamentalists. To grant that a law is true—even a law of "basic" physics or a law about the so-called "fundamental particles"—is far from admitting that it is universal—that it holds

Of course it does make methodological sense to try to apply what one knows in novel circumstances. An extension of empirically-confirmed physics to the universe at large plausibly suggests that gravity dominates electromagnetism at large-scales, so it is sensible to use models of GTR when constructing cosmological models. It need not have been, however, even if we assume the universality of QFT and GTR. There is no reason to exclude theoretically, for example, a charge asymmetry in the universe which could potentially cause cosmological electromagnetic phenomena; indeed various attempts have been made to explore models of the universe incorporating such phenomena, from decades ago (Lyttleton and Bondi 1959) to more recently (Dolgov and Silk 1993).

It is no surprise that our empirically-tested physical theories are insufficient to constrain model choice in cosmology without significant input from cosmological observation. In a paper setting out such constraints on charge asymmetry, Caprini and Ferreira echo this remark, noting that

> ...even though electric charge conservation is well established on Earth, this may not imply directly the overall neutrality of the universe: to be able to draw this conclusion, one would have to assume in addition that charge has to be conserved on all scales and during the entire evolution of the universe. This may not be the case and the determination of a cosmological constraint on the charge asymmetry of the universe is of conceptual importance. (Caprini and Ferreira 2005, 10)

Yet there are (at least) two major obstacles that make this more than science as usual (Smeenk 2013). First, in cosmology the need for such observational constraints is in tension with significant observational limitations due to our severely constrained spatiotemporal extent (Ellis 2007, 1206). This is a theme of great importance in

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everywhere and governs in all domains. (Cartwright 1999, 24)

Harré (1962, 104), in an early philosophical investigation of modern cosmology, also makes a similar point: "...it is by no means clear that we are justified in accepting without question the peculiarly cosmological extension of the concepts of physical sciences."

cosmology, one which will arise in various places below, and will be discussed further in §1.5. Second, certain methodological and philosophical problems arise from the uniqueness of the universe (Ellis 2007, 1216), an issue to which I now turn because of its importance in delimiting the scope of this investigation.

1.2.2 Universe or Multiverse

The object of study in cosmology, the universe, is often stated to be the totality of everything that (physically) exists. Obviously some idealization is necessary because of the complexity of the universe on small scales. Thus the usual aspiration of cosmology as a science in practice is to produce models that capture the large-scale structure and evolution of the universe.

The assumption that the universe is everything that there is makes cosmology distinctive among physical sciences, since it is concerned with modeling an object in the complete absence of other similar objects to which it can be compared by empirical means (Ellis 2007, 1216). Various apparent problems, both physical and philosophical, arise from the supposed uniqueness of the universe (Munitz 1964; Goenner 2010). Other areas of research in physics concerned with modeling physical objects, such as condensed matter physics, high energy physics, etc., do not face such problems. In these areas the theories used to model the objects of interest are developed and assessed directly by empirical means—observation and experiment—through the variability and invariability of these objects.

If there are no other universes (or at least there are no other empirically accessible universes), then, on the face of it, it seems as if there can be no properly "cosmological laws of nature" and hence no theory of cosmology.[11] If cosmology is to be a science, then cosmologists are seemingly left to apply the physical theories that hold within the universe to the universe at large. In other words, the "laws of cosmology" must be adapted or borrowed from the known laws of physics, although, as noted earlier, there is perhaps some reasonable concern about the justifiability of this procedure. In any case, it does seem that "cosmology relies on extrapolating local physical laws to hold universally" (Smeenk 2013, 1).

One may address at least some of the symptoms of this methodological problem by attempting to extend the notion of a multiplicity of objects, characteristic of typical scientific investigations, to cosmology. By denying the uniqueness of the universe and accepting the possibility of a multiverse of universes, the degree of difference between cosmology and the rest of physics appears lessened. We again have a set of objects, whether conceived of as all physically existing or as mere possibilia, whose variability and invariability we hope to assess.[12]

There is little reason at the present, however, to think that the scientific

[11] The point has long been recognized by cosmologists and debated by philosophers (Munitz 1962) and physicists (McCrea 1970) in the context of the HBB cosmology. "For the fact that there is at least one but not more than one universe to be investigated makes the search for laws in cosmology inappropriate" (Munitz 1962, 37). In a classic early review of cosmology, McCrea writes that

> ...cosmology is usually described as the study of the universe as a whole or of the large-scale properties of the universe. We seem now to have discovered the reason why this description is suitable. It appears that at some stage in extending the scale of physics there comes a real difference from ordinary physics; at that stage we reach cosmology. (McCrea 1953, 324)

McCrea locates this difference in terms of coming to grip with what exists "actually" or "accidentally" versus with what exists "typically" or "generally". Although this terminology is not especially apt, it is meant to suggest the challenge to conventional scientific methodology raised by theorizing about the universe at large. Interestingly, contemporary cosmologists are very concerned to propose cosmological theories where our universe is indeed typical or general. The role of typicality or generality in inflationary theory, and cosmological theory more generally, is the central topic of chapter 4.

[12] (Carr 2007) is a recent collection of articles in which the status of the multiverse is debated by leading physicists.

method's epistemic successes will carry over to cosmology by adopting the multiverse view, especially owing to the exceedingly dim prospects of empirically assessing variability and invariability among universes (Aguirre and Tegmark 2005).[13] Indeed, in order to differentiate itself from the assumption of a unique universe, the multiverse assumption must hold that a single universe in the multiverse is a totality in some sense. For the terminology to make sense there ought to be in-principle-limits to empirical accessibility between universes, and so the crucial element of empirical assessment may even be completely lacking in an extreme multiverse scenario.[14]

If the restriction of scientific methodology to traditional experimental methods were an inappropriate limitation and there were other valid means of assessing variability and invariability in physics apart from empirical means, then perhaps introducing multiverse scenarios would be acceptable scientific practice. Frequently what motivates interest in multiverse scenarios is a desire to explain instances of "fine-tuning" and "special initial or boundary conditions," especially the much discussed fine-tuning of the universe for the existence of life. The usual scientific methods have few resources to explain the values of free parameters in theories and models and in principle no means to explain particular values of free parameters in fundamental theories. Thus recourse is often made to anthropic arguments (Ellis et al. 2012, 542ff.) or arguments based on the generality or probability of certain possibilities (Ellis et al. 2012, 546ff.)

[13] "Theorizing about the universe as a whole is a precarious enterprise, as Kant so forcefully argued. It requires a long stretch beyond the relatively safe routines of inductive science. Expectations as to what sort of order we should expect to find, or alternatively as to what should, at the cosmic level, count as order, are bound to dominate" (McMullin 1993, 360).

[14] There is by no means an agreed-upon definition of the multiverse. As an example of a different definition, McCabe (2010, 630) claims that "in modern mathematical physics and cosmology, a multiverse is defined to be a collection of possible physical universes." This however fails to clarify the concept of a multiverse without the additional specification of the relevant notions of possibility and of physical universe. As there is a range of such notions, one finds many kinds of multiverses. Tegmark (2004) offers a popular hierarchy of kinds.

that require a multiverse to make finely-tuned or special conditions expected.

Does everything in physics demand explanation, even when we do not have the relevant empirical constraints? To many cosmologists it is a matter for science to determine an answer to this question. Einstein once remarked, "What really interests me is whether God had any choice when he created the world." For cosmology to be a science, in the opinion of many of its practitioners, God must not have had much choice, "otherwise it would simply amount to 'cosmic archeology,' where 'cosmic history' is written on the basis of a limited number of hot big bang remnants" (Mukhanov 2005, 229). Yet some argue that explaining fine-tuning and special initial conditions by invoking multiverses and anthropic arguments is unjustifiable, in particular due to the lack of any means of empirically verifying such claims (Ellis et al. 2004). This claim however assumes that there are no other valid means of assessing variability and invariability in physics apart from empirical means. Is it justifiable to make this assumption?

The demand that something should be explained ought to depend on the existence of a truth-conducive method that can establish the grounds for that explanation. Empirical methods have for a long time now offered the means for reliably procuring the grounds of scientific explanations, and we think so when a theory's theoretical predictions are empirically confirmed. That these methods lack the means to explain everything cannot be taken as a fault in the method, until it is demonstrated convincingly that other such means exist and are truth-conducive. Since the matter is not settled, I choose to set aside multiverse-based and anthropic arguments in the following.[15]

[15]The epistemic situation in cosmology is similar in many respects to that in quantum gravity research, and notably string theory, which has long suffered criticism for a lack of any means of empirical confirmation. Dawid (2013) argues that non-empirical theory confirmation plays a crucial

In any case, inflationary theory does not necessarily presuppose a multiverse, although cosmologists sometimes claim that there is a sense in which a multiverse scenario is a natural consequence of standard inflationary mechanisms. This aspect of inflationary theory is known as eternal inflation (Linde 1986; Guth 2007), and it is even claimed to have predictions which are potentially observable (Feeney et al. 2011). While eternal inflation is on the forefront of the minds of many theoretical cosmologists (and introduces a host of fascinating issues worthy of philosophical investigation), I choose to focus on the simpler case of one-universe versions of inflation, for the philosophical and foundational issues there are of sufficient interest without introducing the additional quantum mechanical complications of eternal inflation. Multiverses that "physically exist" will therefore be set aside henceforth, although the role of a space of possible universes—which can be described as a multiverse—will be central in the following chapters on the inflationary proposal's merits.

1.2.3 Ranges of Applicability

Any physical theory or practical model of a physical object possesses in-principle or practical ranges of applicability—the fourth feature of cosmological models mentioned above. For example a fundamental theory has unlimited validity (in principle) but fairly restrictive ranges of practical application. These ranges are generally expressed in terms of physical quantities such as length, mass, etc. Outside the range of validity inaccuracies in the model become too large to ignore—the model gives the wrong results. So, for example, Newtonian particle mechanics (with gravity)

role in the development and assessment of string theory. Ideas such as this are relevant to inflationary theory, itself a speculative proposal in the vein of string theory, but an investigation into this is beyond the scope of this dissertation.

has both an upper and lower limit of validity in terms of length, which is just to say that above and below certain length scales Newtonian particle mechanics makes empirically falsifiable predictions (in principle). Similarly, the simple harmonic oscillator model has a limited range of application to macroscopic springs or pendula, outside of which the model is no longer a good representation of the physical object.

Empirical investigation attempts to establish the ranges of validity and the ranges of applicability, but theoretical considerations may suggest them as well. For example, it is often remarked that GTR "breaks down" at small "Planck-scale" lengths (or high energies).[16] We obviously have no empirical evidence that GTR fails in its description at these scales, since these scales are experimentally and observationally inaccessible, but it is widely thought that quantum effects become relevant such that the classical description of relativistic spacetime becomes invalid. It usually follows from this line of reasoning that a quantum theory of gravity is needed in order to describe the early universe, and there is therefore a small-scale limit of applicability on cosmological models.

This particular restriction on cosmological models is relevant to an important aspect of proposals of likelihood measures in cosmology (ch. 4), so it is worth saying a bit more on how it arises. As remarked above, GTR and QFT describe different phenomena, but since all of these phenomena occur in the universe they must either be accounted for in a cosmological model or be negligible at the scale of applicability of the model. The spacetimes of GTR can, and frequently do, serve as backgrounds on which other physical theories can be modeled in a physical model, e.g. as is done

[16]There is also the alleged breakdown of GTR in singular spacetimes to consider, a topic discussed at length in (Earman 1995). FRW spacetimes generically possess spacetime singularities (§1.4.1), which fact has provoked no small amount of effort by cosmologists to contrive ways to avoid them. In fact, early work by Starobinsky (1980) on inflationary-like models had precisely this goal in mind.

in QFT on curved spacetime (Wald 1994). This approach is permissible so long as there is a negligible backreaction on the spacetime geometry, i.e. effects governed by the "foreground" theory are "small enough" that the assumptions grounding the use of the spacetime background remain satisfied. Satisfying this condition means we can neglect the inclusion of foreground fields or particles in the stress-energy tensor T. This is necessary since fields and particles affect the spacetime geometry dynamically in GTR. But in GTR there is no sense to be made of quantum fields affecting the spacetime geometry, except insofar as they can be represented as classical fields or particles. So if quantum effects become significant enough, it seems that backreaction on the spacetime geometry can no longer be ignored—the cosmological model is no longer applicable.

There are also various issues that arise when one considers large-scale applicability that are currently under a great deal of discussion in the cosmological literature (Clarkson et al. 2011). These will not play a significant role in my discussion of inflation, but they are of independent philosophical interest so I will say a few brief words. When cosmologists say that the universe is homogeneous and isotropic, they generally mean that it is so when "small-scale" inhomogeneities and anisotropies are averaged out. Cosmological models are implicitly thought to have an averaging length corresponding to the lower end of the scale of large-scale applicability. This averaging process is never made explicit, and in any case there is currently no generally applicable notion of averaging in GTR, a problem which has been raised byEllis (1984); Ellis and Stoeger (1987).[17]

Still, it is reasonably simple to understand the relevant notion of averaging,

[17]Ellis continues to point to this as an outstanding problem in cosmology (Ellis et al. 2012, 16ff.). The most noteworthy efforts to solve it have been by Zalaletdinov (1992, 2008)

at least in principle. Suppose we wish to model some system of molecules as a fluid. Supposing that at some averaging length the individual masses of the molecules are sufficiently well-approximated by a mass density, it may then be the case that a theoretical description of the system can be given by fluid dynamics. In the example the averaging process depends on comparing a more accurate, but more complicated model (the molecular model) to the simpler yet approximately correct description (in terms of a continuous fluid). Using an inter-model comparison of this sort can be useful for determining the averaged model's range of applicability theoretically.

In GTR giving a precise explication of averaging is quite complicated. Ellis (1984) in particular has identified a cluster of problems which come from considering the effects of averaging in the context of GTR (Ellis 2011; Clarkson et al. 2011). Although these remain unsolved, on their basis some have even claimed that the appearance of an effective cosmological constant in the concordance model (the best fit ΛCDM model) may be solely due to backreaction from coarse-graining (Kolb et al. 2010; Buchert and Räsänen 2012); others have demurred and argued that this kind of backreaction is cosmologically negligible (Ishibashi and Wald 2006). Since these foundational issues of averaging and ranges of applicability involve inter-theoretic (and intra-theoretic) relations, they should be of great interest to philosophers.

1.3 Geometry of the Standard Big Bang Model

Given the nature of cosmology, i.e. its reliance on extrapolating known physical theory to model spacetime and its contents on large-scales, it may seem like cosmology is simply an exercise in fitting a model to observational data. There are two basic approaches that one might take in selecting a particular cosmological model to do this fitting. A specific relativistic cosmology may be proposed and then justified through the derivation of its empirical consequences and confirmation via observation, using observational data to fix remaining free parameters. Alternatively one may attempt to determine the spacetime geometry of a relativistic cosmology directly through observation (Ellis et al. 1985).[18] Unfortunately this observational approach is not possible given fairly severe observational limitations, both practical and in principle (§1.5). Thus it seems one must propose cosmological models based on certain assumptions of what the universe is like, and test their empirical adequacy with the recognition that such models are seriously underdetermined by observations (Butterfield 2012, 2014).

Accordingly this section begins with a brief discussion of the fundamental symmetry assumptions made in cosmology to produce the standard HBB cosmology (§1.3.1). These are the Copernican principle (CopP) and the cosmological principle (CP). Following a discussion of these principles, I reproduce standard results that follow from application of these assumptions in the context of GTR, in particular about the features of the FRW spacetime models (§1.3.2) and perturbed versions thereof (§1.3.4), as these results are utilized frequently in the following chapters. I also introduce in §1.3.3 the initial value formulation of GTR, for it will play an important role in ch. 4.

1.3.1 Symmetry Assumptions in Cosmology

The Copernican principle states that we do not occupy a privileged (spatial) position in the universe. Beisbart and Jung (2006) use this formulation in their logical

[18] Ellis (1984) calls the first the "standard approach" and the second the "observational approach." The latter was advocated originally by Kristian and Sachs (1966).

and historical analysis of the CopP and CP, claiming the former historically carried the existential presupposition that there is a privileged position in the universe. Logically it need not. For the Copernican Principle to be true either there is a privileged position in the universe and we simply do not occupy it, or else there just is no privileged position in the universe at all. The CopP is motivated historically by the past failures of cosmologies that held that we do occupy a privileged position in the universe, whether the exact center, which clearly would be a privileged position, or otherwise. Of course the failure of a handful of cosmologies in no way guarantees the truth of the CopP.

The CopP however is not the principle that is usually assumed by cosmologists. Cosmologists typically assume the logically stronger CP. The cosmological principle states that the universe is spatially homogeneous, i.e. it rejects the existence of privileged spatial positions in the universe. If one holds the CopP and denies that there are privileged (spatial) positions in the universe, then it follows that the universe has no privileged position, i.e. it is spatially homogeneous. Spatial homogeneity is typically glossed as "everything looks the same in every place (at every time)." The CP is also sometimes taken to state that the universe is spatially isotropic. Spatial isotropy can be thought of as amounting to the claim that "everything looks the same (from some place) in every direction (at every time)." I will reserve the designation CP for the assumption that the universe is spatially homogeneous, and refer to the stronger assumption of spatial homogeneity and isotropy as CP+.[19] Finally, I note

[19]It has appealed to some to extend the Cosmological Principle beyond spatial homogeneity (and spatial isotropy in the case of CP+) to spatial and temporal homogeneity (and isotropy). This is sometimes called the Perfect Cosmological Principle (PCP). The PCP was assumed in the formerly popular Steady State (SS) model, but observations, such as the CMB, suggest that the PCP is false in our observational past. The PCP, in some form, is revived in some multiverse scenarios like eternal inflation, which explain the apparent violation of the PCP in our observational past as a feature only of our universe and not the multiverse at large.

that a spacetime which is spatially isotropic around every point is necessarily spatially homogeneous, for which reason it is to some extent redundant to say that a spacetime is spatially homogenous and isotropic (McCabe 2004).

If one merely adopts the CP rather than the CP+, i.e. assumes that the universe is only spatially homogeneous, then a larger class of spacetime models are in accord with the EFE. Among the spatially homogeneous models of GTR are the Bianchi and Kantowski-Sachs models (Ellis et al. 2012, 456ff.). They are of interest as potential cosmological models under the supposition that the apparent approximate isotropy of the universe from our cosmic locale belies an overall anisotropic universe. Naturally there are isotropic but inhomogeneous models as well, and classification schemes have been developed, based on isometries for example, such that one can exhibit classes of models with various degrees of homogeneity and isotropy, and exhibit models with these symmetries that are exact solutions of the EFE (Wainwright and Ellis 1997; MacCallum 1979). My attention will be primarily focused on the FRW models in the following, although occasional considerations from other exact solutions will be relevant.

Beisbart (2009) argues that as yet there is no convincing justification of the CP for the entire universe. If one restricts justification to empirical confirmation, then he is surely correct on this point, since we can have no observational evidence of the character of the universe beyond our particle horizon (in principle), and only indirect evidence of events beyond our observational horizon (§1.4.2). The virtue of assuming the CP is that it puts a strong constraint on the permissible models of GTR. The CP+ places an even stronger constraint. If we assume that the universe is spatially homogeneous and isotropic, i.e. the CP+, then spacetime has a maximum number of spatial symmetries: six—three spatial translations and three rotations. Although time translation and spacetime boosts are not symmetries by the CP+, due to the spatial symmetries spacetime boosts in different directions are equivalent. Thus the assumption of the CP+ greatly simplifies the EFE, yielding two coupled differential equations from the typical ten. These models (those that satisfy the EFE under assumption of the CP+) are the FRW models.[20]

1.3.2 Friedman-Robertson-Walker Spacetimes

As I will be making use of some facts about the FRW models in the following chapters, it is convenient to record them here.[21] An FRW model is a relativistic spacetime $\mathcal{M}$ that can be foliated by a one-parameter (t) family of homogeneous and isotropic spacelike hypersurfaces Σ_t orthogonal to ξ, a future-directed, twist-free, unit timelike field on spacetime manifold M. The spacelike hypersurfaces can be thought of as "space" and the parameter t as time.

Recalling the EFE (1.1),

$$R_{ab} - \frac{1}{2} g_{ab} \mathcal{R} = 8\pi T_{ab} + \Lambda g_{ab},$$

we first specify the form of the stress energy tensor T.[22] The most general form T that is compatible with the assumption that the universe is spatially homogeneous

[20] These models are sometimes also referred to as FLRW models, recognizing Georges Lemître's contributions to understanding expanding cosmologies.

[21] For a complete derivation of the results given here in similar notation see (Malament 2012). Numerous cosmology texts cover similar ground. Standard, recommended references include (Dodelson 2003; Mukhanov 2005; Weinberg 2008; Baumann 2009), but I do not follow their notation or coordinate-heavy approach. Other useful, recent texts include (Peter and Uzan 2009; Ellis et al. 2012). A mathematically-oriented survey of cosmology in the philosophical literature is (McCabe 2004).

[22] The cosmological constant Λ is conventionally absorbed into the matter contents of an FRW spacetime—it appears on the "right-hand side" of the EFE rather than the "left-hand side."

and isotropic is equivalent to the energy of a perfect isotropic fluid. Therefore T is given by

$$T_{ab} = (\rho + p)\, \xi_a \xi_b - p g_{ab}, \tag{1.2}$$

where ρ is the fluid's energy density field, p is the fluid's isotropic pressure field, and our choice of ξ defines the fluid's four-velocity field.[23]

Since the FRW model exhibits such a high degree of symmetry, the EFE reduce to two coupled non-linear ordinary differential equations with matter described by T called the Friedman equations. The standard forms of these two equations, the first of which is usually just called the *Friedman equation* and the second of which I will call the *Friedman acceleration equation*:[24]

$$\left(\frac{\dot{a}}{a}\right)^2 - \frac{8\pi}{3}\rho = -\frac{k}{a^2}; \tag{1.3}$$

$$\frac{\ddot{a}}{a} = -\frac{4\pi}{3}\,(\rho + 3p). \tag{1.4}$$

The scale factor a, the energy density ρ, and pressure p, are the only dynamical variables. If the scale factor, which can be considered a real-valued, non-negative scalar field, uniform on spatial hypersurfaces of M, strictly increases or decreases for some period of time, as it does in HBB models, it can be used as a time parameter. The parameter k represents the spatial (Gaussian) curvature and has been scaled (by absorbing a constant factor into a) to take one of three values: $+1$, 0, and -1. These correspond to constant positive curvature, zero (flat) curvature, and constant negative curvature.

[23] I use the metric signature (+ - - -), so $\xi^a \xi_a = 1$.

[24] Here I write overdots to indicate directional derivatives with respect to the four-velocity of the cosmic fluid, e.g. $\dot{a} = \xi^a \nabla_a a$.

The scale factor a represents the expansion (or contraction) of the cosmic fluid. One also often makes use of the Hubble parameter H, which gives the expansion rate:

$$H = \frac{\dot{a}}{a}. \tag{1.5}$$

The Friedman equations can be written in terms of the Hubble parameter as

$$H^2 - \frac{8\pi}{3}\rho = -\frac{k}{a^2}; \tag{1.6}$$

$$\dot{H} + H^2 = -\frac{4\pi}{3}(\rho + 3p). \tag{1.7}$$

The energy contents of the spacetime determine the spatial geometry (curvature) for all time, so if the spacelike hypersurfaces of a spacetime are flat at one time, they are flat at all times (even though the energy density and pressure are typically time-dependent). It is therefore often useful to define the *critical density* ρ_{cr}, the energy density at which the spatial curvature is flat:

$$\rho_{cr} = \frac{3H^2}{8\pi}. \tag{1.8}$$

The Hubble parameter can be interpreted as the instantaneous rate of volume increase, per unit volume, under the fluid flow associated with the fluid's four-velocity ξ. In a volume V the rate of change of volume $\dot{V} = \xi^a \nabla_a V$ is given by

$$\dot{V} = 3HV. \tag{1.9}$$

As an illustration of the interpretation of a and H, consider that a unit volume at scale factor $a_0 = 1$ will grow with a according to $V = a^3$. Then it follows that $\dot{V} = 3\dot{a}a^2$ and equation 1.9 is clearly satisfied:

$$\dot{V} = 3\dot{a}a^2 = 3\frac{\dot{a}}{a}a^3 = 3HV. \tag{1.10}$$

A natural choice of coordinates in the context of FRW spacetimes is what are known as co-moving coordinates. These are coordinates chosen so that the distance between fluid elements does not change with time. This is the sense of the term co-moving—the coordinates move with the fluid. In such coordinates multiplying by the scale factor gives the physical distance. This distance explicitly accounts for physical expansion or contraction (unlike co-moving coordinates). Let r represent the radial distance between two galaxies that are co-moving with the cosmic fluid (so r is a co-moving distance). If the universe is expanding then the physical distance d is r: $d = ar$, and $a = d/r$, i.e. the scale factor is explicitly the ratio of physical distance to co-moving distance.[25]

So far only the concept of expanding and contracting spacetimes with constant spatial curvature has been introduced. In order to determine the temporal evolution (expansion or contraction rates) and curvature, one must specify the energy contents of the FRW model by specifying ρ and p. This is not the same as specifying a physical

[25]Taking directional derivatives with respect to the fluid four-vector ξ gives

$$\dot{d} = \frac{\dot{a}}{a}d = Hd. \tag{1.11}$$

This famous equation is known as Hubble's Law. It says that the physical (recession) velocity of an object (like a galaxy) is equal to the Hubble parameter times the physical distance. If the Hubble parameter is approximately constant then one has a linear relation between distance and recession velocity that Hubble famously verified by surveying galactic redshifts. His momentous discovery was crucial to the widespread adoption of the expanding universe cosmology. In general (and in particular in our universe) the Hubble parameter is not a constant, although it was thought to be in the past, hence the oft-used but unfortunate name Hubble "constant." One may let the term "Hubble constant" refer instead to the present value of the Hubble parameter, usually denoted H_0.

model, although the specification of a physical model would determine ρ and p, since certain assumptions about the form of these two fields results in different evolutionary behavior.

The first case to consider is pressure-free matter ($p = 0$), or what cosmologists call *dust*. It is easy to show that the Friedman equations imply the following equation, the continuity equation for a perfect fluid ($\nabla_a T^{ab} = \mathbf{0}$):

$$\dot{\rho} + 3H(\rho + p) = 0. \tag{1.12}$$

For dust this equation implies that $\rho \propto a^{-3}$, i.e. as the universe expands, the energy density of dust decreases volumetrically. All kinds of "normal" matter is treated as dust in cosmology, including dark matter and "baryons."[26].

The next case is *radiation*. Radiation has energy density equal to three times the pressure ($\rho = 3p$). All manner of relativistic particles are treated as radiation in cosmology, in particular photons and neutrinos. For radiation the Friedman equations imply that $\rho \propto a^{-4}$. As the universe expands, the energy density of radiation decreases faster than volumetrically. Because of this fact, in a universe that begins radiation dominated (like ours), i.e. the energy density of radiation is much larger than the sum of other kinds of energy density, there will eventually come a time when the universe becomes matter-dominated. For our universe matter-radiation equality happened quite long ago (§2.4).

The final case to consider is the *cosmological constant*. A cosmological constant is equivalent to a perfect fluid whose energy density is additively inverse to its pressure

[26] "Baryons" is a catch-all term in cosmology for non-relativistic matter, not just baryons. Non-relativistic leptons have a negligible cosmological effect, hence the choice in terminology.

($\rho = -p$). The energy density of the cosmological constant is usually denoted Λ. The Friedman equation implies that $\Lambda \propto H^2$ in an otherwise empty universe, one that has only a cosmological constant (the *de Sitter cosmology*). Such a universe expands at an accelerated rate, as the continuity equation shows ($\ddot{a} \propto \Lambda$). Our universe not long ago entered into a cosmological constant-dominated phase, or something much like it. In §2.4 I show that an inflationary phase also behaves like a cosmological constant dominated universe, since it expands at an accelerated rate. This behavior is crucial to the inflationary solution to the fine-tuning problems of the HBB model discussed in chapter 2.

It is typical to make energy densities dimensionless by dividing by the critical density, e.g. by defining a parameter $\Omega = \rho/\rho_{cr}$, the ratio of the total energy density to the critical density. Similarly, one defines Ω_γ as the energy density of photons, Ω_{dm} as the energy density of dark matter, Ω_b as the energy density of baryons, Ω_Λ as the energy density of the cosmological constant, and so on. One can then rewrite the Friedman equation as

$$1 - \Omega = -\frac{k}{(aH)^2}. \tag{1.13}$$

This equation is useful for understanding the flatness problem (§2.2.2).

Energy conditions are often assumed to hold for T for various purposes (Curiel forthcoming). For example, in standard versions of the singularity theorems (§1.4.1) the strong energy condition (SEC),

$$\left(T_{ab} - \frac{1}{2}T g_{ab}\right)\xi^a \xi^b \geq 0, \tag{1.14}$$

is assumed to hold. For FRW spacetimes the SEC is satisfied iff

$$(\rho + p) \geq 0 \qquad \text{and} \qquad (\rho + 3p) \geq 0. \tag{1.15}$$

The weak energy condition (WEC) is

$$T_{ab}\xi^a\xi^b \geq 0, \tag{1.16}$$

For FRW spacetimes the weak energy condition is satisfied iff

$$\rho \geq 0 \qquad \text{and} \qquad p \geq -\rho. \tag{1.17}$$

The weak energy condition is satisfied for all the kinds of energy and matter considered in cosmology. Dust and radiation (and combinations thereof) satisfy the strong energy condition, but a cosmological constant dominated universe does not.

It is useful to define the equation of state $w = p/\rho$. Then the continuity equation is

$$\dot{\rho} + 3H\rho(1 + w) = 0. \tag{1.18}$$

It follows that $\rho \propto a^{-3(1+w)}$ when w is constant, from which one can verify the previous results about the evolutionary behavior of dust, radiation, and the cosmological constant. One can also use the equation of state and the Friedman equations to derive the "deceleration" equation:

$$\frac{\ddot{a}a}{\dot{a}^2} = -\frac{1}{2}(1 + 3w)\left(1 + \frac{k}{(aH)^2}\right). \tag{1.19}$$

The sign of the equation's right hand side is necessarily the same as the sign of $\ddot{a}$, so we can interpret this equation as indicating whether the scale factor is accelerating or decelerating. Define the *deceleration paramater* q as the additive inverse of the left hand side. Then the equation reads

$$q = \frac{1}{2}(1 + 3w)\left(1 + \frac{k}{(aH)^2}\right). \tag{1.20}$$

One can immediately infer that the expansion is accelerating ($q < 0$) if $k \neq -1$ and $w < -1/3$.[27] This requires $\rho + 3p < 0$, i.e. a violation of the strong energy condition in FRW spacetimes. In short, a violation of the strong energy condition implies accelerated expansion—the basic condition of an inflationary epoch (§2.4).

Finally, the spatial metric h is given by projecting the spacetime metric g onto a spatial hypersurface Σ:

$$h_{ab} = g_{ab} - \xi_a \xi_b. \tag{1.21}$$

One can compute from the spatial metric the spatial curvature (in FRW spacetimes the spatial curvature $\mathcal{K}$ is a constant). The extrinsic curvature will be denoted π_{ab} and is defined as

$$\pi_{ab} = h^m{}_a \, h^n{}_b \, \nabla_m \, \xi_n. \tag{1.22}$$

The spatial metric h of a spatial hypersurface Σ in FRW spacetimes is homogeneous and isotropic. In FRW spacetimes the extrinsic curvature is given by

$$\pi_{ab} = H \, h_{ab}. \tag{1.23}$$

[27] It turns out that the expansion is accelerating if $k = -1$ and $w < -1/3$, but it is less obvious.

The spatial metric and extrinsic curvature will be important in §1.4.1 for showing that FRW spacetimes, with certain additional conditions, are singular, and in chapter 2 when the uniformity and flatness problems of the HBB model are explained in the context of the initial value formulation of GTR.

1.3.3 Initial Value Formulation

It is well-known that GTR has an initial value formulation (Wald 1984, ch. 10), i.e. GTR can be usefully reformulated as a deterministic theory for spacetimes of special cosmological interest, such that given appropriate initial data, future and past data are determined with certainty. Maxwellian electrodynamics, Newtonian particle mechanics, and relativistic Klein-Gordon theory (among other physical theories) all can be formulated in such a manner. Giving GTR an initial-value formulation is complicated somewhat by the lack of a background spacetime; one must determine what appropriate initial data *are* in such a theory.[28] If one however elects to split relativistic spacetimes into spatial hypersurfaces Σ_t that evolve in accord with a global time function t, then the EFE can be decomposed into evolution and constraint equations (as in Maxwellian electrodynamics, for example).

The initial value formulation of GTR makes use of the spatial metric h and extrinsic curvature tensor π of a spatial hypersurface Σ as initial data. Recall that the spatial metric is given by projecting the spacetime metric g onto the spatial hypersurface Σ, given some unit timelike vector field ξ:

$$h_{ab} = g_{ab} - \xi_a \xi_b. \tag{1.24}$$

[28] One must also assume that the spacetime is globally hyperbolic, or, equivalently, possesses a Cauchy surface (Dieckmann 1988).

The spatial metric can be thought of as "position" initial data in the context of a dynamical spacetime theory like GTR. It describes how space, represented by Σ_t, is curved. In other typical initial value formulations one also has the derivative of position data as initial data. This is so for GTR as well. In GTR the derivative of the spatial metric is proportional to the extrinsic curvature π_{ab}.[29] The extrinsic curvature is given by

$$\pi_{ab} = h^m_{\ \ a} \, h^n_{\ \ b} \, \nabla_m \, \xi_n. \tag{1.25}$$

It can be easily demonstrated that $2\pi_{ab} = \pounds_\xi(h_{ab})$ (Malament 2012, 110), so it is appropriate to interpret the extrinsic curvature as "momentum" initial data. It shows how the position data h changes along the timelike field specified by ξ. Thus if Σ is a spacelike Cauchy surface in relativistic spacetime $\mathcal{M}$, then an appropriate data set for this spacetime is (Σ, h, π).

The EFE are recast into two sets of distinct equations in the initial value formulation, the evolution and constraint equations (Malament 2012, 182). The constraint equations can be written as

$$\mathcal{R} - (\pi_a^{\ a})^2 + \pi_{ab}\pi^{ab} = -16\pi T_{ab}\xi^a\xi^b \tag{1.26}$$

and

$$D_c\pi_a^{\ c} - D_a\pi_c^{\ c} = 8\pi T_{mr}h^m_{\ \ a}\xi^r, \tag{1.27}$$

where $\mathcal{R}$ is the intrinsic curvature of Σ, T is the stress energy tensor, and D is the derivative operator induced on Σ. The evolution equations can be written (for the sake

[29] One can compute from the spatial metric h the *intrinsic* spatial curvature in complete analogy with the computation of the Riemann curvature tensor from the spacetime metric g (in FRW spacetimes the spatial curvature is a constant $\mathcal{K}$).

of simplicity ignoring “shift” and “lapse” since it will not matter for FRW spacetimes)

$$\pounds_{\xi}(h_{ab}) = 2\pi_{ab} \tag{1.28}$$

and

$$\pounds_{\xi}(\pi_{ab}) = 2\pi_{a}{}^{c}\pi_{cb} - \pi^{c}{}_{c}\pi_{ab} + \mathcal{R}_{ab} - 8\pi h_{a}{}^{m}h^{n}{}_{b}(T_{mn} - \frac{1}{2}Th_{mn}). \tag{1.29}$$

Let us now specialize to the case of FRW spacetimes. Take (M, g_{ab}) to be such a spacetime, where $M = \Sigma_t \times t$ is a foliation of M into homogeneous and isotropic spacelike hypersurfaces Σ_t parameterized by t, a global time function.[30] The spatial metric h_t of a spatial hypersurface Σ_t must of course be homogeneous and isotropic. The intrinsic and extrinsic curvature of spatial hypersurfaces Σ_t varies in time in general.[31] In FRW spacetimes the extrinsic curvature of Σ_t is given by

$$\pi_{ab} = H(t)\, h_{ab}, \tag{1.30}$$

where recall that H, the Hubble parameter, is a scalar field that is constant on the isotropic and homogeneous spacelike hypersurfaces, and is related to the expansion of space. Therefore initial data of an FRW spacetime is given by (Σ, h, Hh). The first constraint equation (1.26) is simply the Friedman equation (1.3), and the second evolution equation (1.29) is the Friedman acceleration equation (1.4). So one correctly recovers the characteristic equations of FRW spacetimes in the initial value formulation.

[30] Note that global time functions of FRW spacetimes are equivalent up to a scaling factor so there is no loss in generality in considering some specific time function t.

[31] Although recall that one can scale the scale factor a so that spatial curvature is a constant $k = -1, 0$, or $+1$, as one does standardly.

1.3.4 Perturbed Friedman-Robertson-Walker Spacetimes

FRW spacetimes have underlain the success of the HBB model. However, while the universe may or may not satisfy CP+ at a large enough averaging scale, it plainly is not satisfied at smaller averaging scales, due to the existence of galaxies, stellar systems, planets, etc.—just as a fluid description breaks down when one approaches molecular scales. The FRW models therefore have a clear lower limit of applicability. Naturally, cosmologists want to explain the existence and evolution of large-scale cosmological structures such as galaxies, but this is simply not possible with the highly symmetric FRW models. There are evidently small departures from isotropy (and homogeneity) in a more accurate model of the universe than that given by FRW spacetimes.

An appealing way to address observed anisotropies and potential inhomogeneities is to modify the empirically successful FRW spacetimes by perturbing these models. It turns out that the predictions of linearly perturbed FRW models accord with high accuracy to cosmological observations.[32] As perturbed FRW spacetimes play a significant role in work on cosmological likelihood measures (ch. 3), I record some of the relevant details here.

The notion of a perturbation is complicated in GTR by the absence of a background on which to perturb–GTR is (in some sense) a background-free theory (Giulini 2007; Rickles 2008; Belot 2011). This difficulty can be remedied by stipulation, but it remains unclear precisely how to deal with (and understand) the gauge freedom

[32] Useful reviews on the large subject of cosmological perturbation theory include (Mukhanov et al. 1992), (Kodama and Sasaki 1984), and (Malik and Wands 2009). The contemporary approach to cosmological perturbation theory is based in large part on the seminal paper by Bardeen (1980). (Stewart 1990) is a nice clarificaiton of Bardeen's approach. Early work on the subject includes, among others, (Lifshitz and Khalatnikov 1963; Hawking 1966; Olson 1976).

of GTR.[33] Here is the approach that cosmologists have taken.

In cosmological perturbation theory we are interested in finding spacetimes that are "close" to some unperturbed (typically FRW) spacetime $\mathcal{M}_0$. We therefore consider a one-parameter family of spacetimes $\mathcal{M}_\epsilon$ parameterized by ϵ in a 5-dimensional manifold $\mathcal{N}$. How does one compare, say, $\mathcal{M}_0$ and $\mathcal{M}_1$, in particular the tensor fields, e.g. the metric tensor field, defined on these manifolds? It is only possible in general if one stipulates which points correspond to one another in each manifold, since the tangent spaces of these submanifolds are not intrinsically related in any way. Making such an identification, however, is equivalent to choosing a gauge for most quantities. Most perturbations are, in other words, gauge dependent.[34]

To make the issue somewhat more vivid, let us focus attention on the notion of a density perturbation. Consider the FRW spacetime $\mathcal{M}_0$ with stress energy given by a perfect fluid (1.2). The energy density field ρ is constant on spatial hypersurfaces orthogonal to the timelike four-velocity field ξ^a, but it is not constant over all of spacetime. Intuitively, a more realistic model of the universe than that given by an FRW spacetime would include small spatial variations in the density field to account for departures from spatial homogeneity, the kind of variations that give rise to structure in the universe. For small perturbations, i.e. for small ϵ, one expects that only the linear terms matter, so higher-order perturbations can be neglected. The base or unperturbed spacetime $\mathcal{M}_0$ differs from the linearly perturbed spacetime $\mathcal{M}$ when there exists a diffeomorphism $\varphi : \bar{M} \to M$ such that the metric perturbation is given

[33]More extensive discussions of the "gauge problem" can be found in (Stewart and Walker 1974), (Bardeen 1980), (Ellis and Bruni 1989), and (Malik and Wands 2009).

[34]As Stewart and Walker (1974) show, only constant scalar fields, fields that vanish, and fields made up of combinations of Kronecker deltas will be invariant under perturbations.

by

$$\delta g = g - \bar{g} = \epsilon \, (\pounds_{\varphi} g)_0. \tag{1.31}$$

Strictly speaking the expression after the first equals sign makes no sense; the intuitive idea of a difference between the two metrics is given by the final expression using the Lie derivative of the metric evaluated at the unperturbed spacetime. Analogously, the linear density perturbation is

$$\delta\rho = \epsilon \, (\pounds_{\varphi} \rho)_0. \tag{1.32}$$

But the problem is that φ is arbitrary, from which it follows (in general) that $\delta\rho$ is arbitrary. In short, density perturbations are generally gauge-dependent quantities.

The issue is typically explained in the literature using coordinates. The basic point is essentially the same, but with coordinates one sees that linear perturbations are indistinguishable from the particular choice of coordinates. One can make the FRW model "look" inhomogeneous by choosing coordinates in which it appears to be so.[35] Ellis and Bruni put the issue in this way:

> The resulting problem is that the quantity [$\delta\rho$] (the variation in density along a single world line) often calculated in perturbation calculations is completely dependent on the gauge chosen, and unless this gauge is fully specified the modes discovered for this quantity are spurious modes (due to residual gauge freedom); while if it is fully specified, its relation to what we really want to know (the spatial variation of density in the Universe) is convoluted and difficult to interpret. (Ellis and Bruni 1989, 1804)

Ellis, Bruni, and collaborators have advocated a covariant approach to cosmological perturbation theory that relies on gauge-invariant quantities, e.g. spatial gradients of density, pressure, and the expansion field (Ellis and Bruni 1989; Ellis et al. 1989; Bruni

[35] Of course a change in coordinates that make the FRW model look homogeneous will have effects in the coordinate expressions of other quantities, for example spatial variations in the proper times of fluid elements.

et al. 1992), with relatively clear physical interpretations. Alternatively, however, one can fix coordinates and either carefully keep track of the gauge freedom associated with φ, or take Bardeen's metric-based approach which expresses the metric as a combination of gauge-invariant quantities from which one can make convenient gauge choices. The last approach is probably the most common in the literature.

The gauge issue bears further investigation, but as it exceeds the scope of this dissertation, I will merely note a few important results concerning perturbed FRW spacetimes which will be relevant in subsequent chapters. The subject of cosmological perturbations quickly becomes technically complicated in any case, so in order to avoid unnecessarily introducing a great deal of formalism I avoid giving many formulas.

The first essential fact concerns the exterior curvature of spacelike hypersurfaces, given by

$$\pi_{ab} = Hh_{ab} + \sigma_{ab}, \tag{1.33}$$

where σ_{ab} is the (trace-free, antisymmetric) shear(Malik and Wands 2009). Clearly the trace of the exterior curvature of a perturbed FRW spacetime is the same as in the unperturbed case.

Density perturbations play a central role in cosmological perturbation theory, for they give rise to the perturbation spectrum that is observable in the CMB. They evolve according to the equation

$$a^{-4}h^a{}_c(a^4h^b{}_a\nabla_b\rho)' = 3(\rho + p)h^b{}_a\nabla_b H, \tag{1.34}$$

where $'$ denotes the directional derivative along the flow lines of the fluid. One immediately notices that in the cosmological constant dominated universe (where

$\rho = -p$) the derivative of the spatial density gradient is constant. It follows that $\rho \propto a^{-4}$—density perturbations die away quickly ("gravitational smoothing") in such a universe. This is an important result (sometimes referred to as a cosmological no hair result), since an inflationary phase behaves in just such a way.

Cosmologists typically write density perturbations as the dimensionless (gauge-dependent) quantity $\delta\rho/\rho$. One can define a gauge-independent quantity that has the intended interpretation by

$$\delta\rho = (P^a P_a)^{1/2}; \qquad P_a = \frac{a h^b{}_a \nabla_b \rho}{\rho}, \tag{1.35}$$

which is the magnitude of the vector defined to be the spatial density gradient divided by the density (Ellis and Bruni 1989). Then it can be shown that in dust universes the density perturbation evolves as

$$\ddot{\delta\rho} + 2H\dot{\delta\rho} - 4\pi\rho\delta\rho. \tag{1.36}$$

One can easily see that in a non-expanding universe (H constant) density perturbations would grow exponentially ("gravitational collapse" or "aggregation"). The expansion of the universe works to counteract gravitational collapse, as one would expect (Padmanabhan 1993).[36]

It is also important to remark that if one decomposes these (scalar) density perturbations into Fourier modes, the modes evolve independently in linear perturbation theory. One can also investigate vector and tensor perturbations (see (Stewart 1990) or any standard review for the meaning of this decomposition of perturbations);

[36] Using these gauge-invariant variables, the radiation and "mixed" cases are discussed in (Ellis et al. 1989).

scalar, vector, and tensor perturbations evolve independently of one another as well. Vector perturbations decay in expanding spacetimes; tensor perturbations source gravitational waves (Dunsby et al. 1997).

1.4 Singularities and Horizons

As the name suggests, the horizon problem depends on the existence of horizons in FRW cosmologies. Horizons in cosmology are a certain kind of causal structure of spacetime. Since the uniformity and flatness problems depend on there being a horizon problem (ch. 2), the HBB model's fine-tuning problems which I discuss in this dissertation all depend on the existence of a particular causal structure in our universe.

The particular kind of horizon that is relevant to the horizon problem is the particle horizon. Particle horizons are defined relative to "fundamental observers," who are observers at rest with respect to the universe's expansion (one says that they move with the "Hubble flow"). The particle horizon at a time is the spatial distance that light could have traveled since the beginning of the universe from the initial point of the given fundamental observer's worldline. Thus the particle horizon at a time separates objects into two sets: roughly speaking, those that could have been influenced by the fundamental observer and those that could not. Universes with a finite age will intuitively have particle horizons, since light could only travel so far in that time. Cosmologies based on singular spacetimes where the singular behavior occurs toward the past will have a finite age, and therefore intuitively have particle horizons. It turns out that the HBB models are singular in this way, and do in fact have particle horizons.

This section is devoted to a brief discussion of spacetime singularities and cosmological horizons, both in general and in the specific spacetime models of interest in cosmology. The discussion of spacetime singularities in GTR is a familiar topic in the philosophy of physics literature (particularly due to (Earman 1995)) and long discussed in the physics literature; an appreciation of the nature of singular spacetimes is important for understanding modern cosmological models, so some of this material is reviewed here. Cosmological horizons are frequently discussed in modern cosmological texts and articles, but frequently with some degree of confusion. To avoid these confusions, I explain the nature of horizons in cosmology, especially to avoid misconceptions in the presentation of the horizon problem in the next chapter.[37]

1.4.1 Singularities in Cosmology

It is now widely held that singularities are a pervasive feature of GTR, although a precise and comprehensive definition of a spacetime singularity remains elusive.[38] Certain spacetimes demonstrably exhibit singular behavior, most familiarly the Schwarzschild spacetime and some FRW spacetimes. Moreover these spacetimes in particular find important application in astrophysics and cosmology by modeling black holes and the expanding universe, respectively. Whether singularities "truly exist in nature" (whatever that means) is debatable, with many believing that the appearance of singularities in GTR is a sign that there is something wrong with the theory, while the empirical success of the HBB model suggests we should take the existence of singularities in FRW spacetimes seriously.

[37](Ellis and Rothman 1993; Davis and Lineweaver 2004) highlight and correct common misrepresentations that appear in the physics literature.

[38]Some, e.g. (Earman 1995), argue that such a precise definition is necessary; others, e.g. (Curiel 1998) demur.

Various singularity theorems have been proved in the past several decades whose conditions are indeed met by some FRW models, such that these cosmologies can be said to have "initial singularities." Although this language is somewhat misleading, since singularities cannot be localized to a particular region of spacetime (Curiel and Bokulich 2012), the term "initial singularity" is meant to indicate that timelike geodesics are past-incomplete in the spacetime. Because of the non-locality of spacetime singularities, some prefer to talk of "singular spacetimes" as opposed to "singularities in spacetime." So long as one understands this, there is no real harm in using the term "singularity."

Confusion over the nature of singularities is however rampant in the history of investigations into singular spacetimes. Early confusions about apparent singularities in the Schwarzschild metric can be blamed on the coordinate-reliant approach then used (Earman 1995). That some quantity "blows up" along a curve in some set of coordinates is no sure indication that there is singular behavior in that spacetime. It may simply be that the coordinate description breaks down there; such a spacetime possesses merely a "coordinate singularity" which can be avoided by a different choice of coordinates. To identify "true singularities" one should identify them using the geometrical structure of spacetime. Since singularities are, again, not localized one must rely on global features of the spacetime to prove their existence.

Proving the existence of singularities in a spacetime requires sophisticated arguments, but these arguments tend to have a similar form. The main elements of typical singularity theorems (Wald 1984; Hawking and Ellis 1973) are the assumption of some energy condition, a condition on the global structure of spacetime, and the requirement that gravity is strong enough to trap some surface (Hawking and Penrose

2010, 15). Since these requirements are relatively weak, one expects them to be satisfied in a variety of spacetimes. Thus a historical consequence of the singularity theorems proved in the late 60s was the acceptance that singularities are pervasive in GTR. Before then it was thought that singular spacetimes were oddities in an otherwise well-behaved theory. It was an often-raised complaint against the HBB model in particular that it began from an initial singularity. The pervasiveness of singularities in the theory makes the complaint lose some of its force.[39]

FRW spacetimes can satisfy the conditions of the general Hawking-Penrose singularity theorem (Hawking and Penrose 1970). Not all FRW spacetimes do, because among the spacetimes that have a metric of the FRW form are all spacetimes that have the same symmetries. Among these is Minkowski spacetime, which, if any spacetime is free of singularities, is surely singularity free. I will skip over the details of the various singularity theorems since these are adequately covered in many places, but for an example of one, consider this one from (Wald 1984, 237)):

Theorem 1. *Let (M, g) be a globally hyperbolic spacetime with $R_{ab}\xi^a\xi^b \geq 0$ for all timelike ξ, which will be the case if Einstein's equation is satisfied with the strong energy condition holding for matter. Suppose there exists a spacelike Cauchy surface Σ for which the trace of the extrinsic curvature (for the past directed normal geodesic congruence) satisfies ${\pi^a}_a \leq C < 0$ everywhere, where C is a constant. Then no past directed timelike curve from Σ can have length greater than $3/|C|$. In particular, all past directed timelike geodesies are incomplete.*

Given the statement of this singularity theorem, it is easy to see that FRW

[39] At least it does against the HBB—the complaint can also be magnified to a general complaint against GTR. Although singularities were not at all well understood in the early history of expanding spacetime models, it was clear enough that something odd was happening in the models as one "approached the initial singularity."

models (which are globally hyperbolic) with the following conditions satisfy the conditions of the theorem: (satisfies SEC (1.14)) $(\rho + p) \geq 0$ and $(\rho + 3p) \geq 0$; (expanding) $H > 0$. It then follows that the trace of the (past-directed) extrinsic curvature (1.23) of spacelike Cauchy surfaces is $-3H < 0$, and no past directed timelike curve from this surface can have length greater than $|H|^{-1}$. It is also easy to see the perturbed FRW models satisfy the conditions of the theorem when they satisfy the strong energy condition and are expanding. The trace of the extrinsic curvature is identical to unperturbed FRW models (1.33), so perturbed models have the same "age" as unperturbed models. As mentioned previously, dust, radiation, and forms of matter in between these extremes satisfy the strong energy condition. Also, there is good evidence that the universe is expanding, which evidence comes from the variety of evidence in support of the HBB model (galaxy redshifts for example). Thus the FRW spacetime or perturbed FRW spacetime which models our HBB universe is certainly singular (depending on its range of applicability).

Modern extensions of the HBB model do not undermine this conclusion. While the inclusion of dark matter and dark energy in the concordance model represents a significant departure from the HBB model in many ways, the fact that the FRW spacetime is singular towards the past remains. Dark matter is apparently "normal" matter, in the sense that it obeys the energy conditions (not in the sense of its interactions with other forms of energy however). Dark energy however does not satisfy the strong energy condition. It is posited to account for the accelerated expansion of the universe, and, as I mentioned previously, accelerated expansion requires a violation of the strong energy condition. Therefore it might seem that a universe with dark energy could avoid the singularity theorem. For simplicity, suppose that dark energy is represented by a cosmological constant in the EFE. Recall that what makes the cosmological constant a "constant" is that its energy density remains constant over time. Although we have presently entered into a dark energy dominated phase of the universe, the universe was previously matter dominated and even earlier radiation dominated. The cosmological constant contributes little to the energy density of the universe at early times since the energy density of matter and radiation scale at a^4 and a^3 respectively. Dark energy only tells us that the universe, insofar as the FRW model is accurate to it, will not end in a singularity (a "big crunch"). Thus the best-fit ΛCDM model, the so-called concordance model, also satisfies the conditions of the singularity theorems, and therefore has an initial "big bang" singularity.

1.4.2 Particle and Event Horizons

There are many things that have been referred to as horizons in cosmology, but there are two kinds of horizons that cosmologists discuss most often as a way of conveying the causal structure of a cosmological model: *particle horizons* and *event horizons*.[40] The intent of identifying horizons is to identify limits on which particles can have "causally affected" (particle horizon) or will be able to "causally affect" (event horizon) other particles. From a relativistic point of view, it would seem that the appropriate way to express these ideas is by the past and future light cone, however this is not the usual approach in cosmology.

In the first place, the use of the term "particle" is somewhat misleading. The

[40] Some of the material on horizons is covered in (Earman 1995). (Davis and Lineweaver 2004) addresses these issues with remarkable clarity. (Ellis and Rothman 1993) covers similar ground, but is targeted to an audience less familiar with relativity theory. The first significant discussion of horizons was (Rindler 1956), and the topic was soon after taken up in the philosophical literature in (Swinburne 1966).

existence of horizons has to do with the causal structure of spacetime, not necessarily the existence of any particular particles. In particular, it is important to recognize that the particles relevant to the specification of a horizon must be co-moving, else the definitions used in cosmology make no sense. In any case, since cosmology is concerned with the universe on large scales, the "particles" are typically whole galaxies (which on the relevant scales are approximately co-moving). If our galaxy, for example, were to have moved significantly in space (was not approximately co-moving) since its creation, the notion of a particle horizon as defined would be unhelpful at best, as we shall now see.

In FRW spacetimes there is a privileged foliation of spacetime into spacelike hypersurfaces, namely those spatial hypersurfaces that have constant spatial curvature. The particle horizon at a given time is usually defined as the spatial distance that light could have reached by that time from a given spatial location (spatial location of a given fundamental observer). Since FRW spacetimes are singular toward the past, there is a "beginning to time" such that there are limits to how far light could have traveled since this time. For this reason FRW spacetimes have particle horizons.[41]

Consider the worldline of some fundamental observer (our galaxy, for example, that is approximately co-moving with the Hubble flow). Say we wish to know the particle horizon at the present time, $t = t_0$. In co-moving coordinates the distance that light emitted from our co-moving location at $t = t_i$ could have traveled is given by

$$\chi_p(t_0) = \lim_{t \to t_i} \int_t^{t_0} \frac{\mathrm{d}t'}{a(t')}, \tag{1.37}$$

[41]By contrast, consider the Einstein static universe, which is an FRW cosmology that is defined in such a way that it neither expands nor contracts. The Einstein static universe has no particle horizons because light could have reached any given point at any given time. Unlike the expanding FRW spacetimes, the Einstein static universe does not have a "beginning to time."

where we are integrating along the path of a light ray emitted from the given location at $t = t_i$. If t_i is finite, then the model has a particle horizon; if t is past infinite, then whether the spacetime has a particle horizon depends on whether the integral converges. An alternate way to write the expression for the particle horizon is in terms of the scale factor a:

$$\chi_p = \lim_{a \to 0} \int_a^1 \frac{\mathrm{d}a'}{a'^2 H}. \tag{1.38}$$

For a cosmological constant dominated universe the integral diverges, so there are no horizons in such a universe. The integral converges for matter and radiation dominated universes, and one can therefore compute the co-moving distance to the horizon. It is straightforward to see that matter and radiation dominated universes will have particle horizons. Assuming for simplicity that the cosmic fluid has an equation of state w and the spatial geometry is flat ($k = 0$), equation (1.18) indicates that

$$\rho \propto a^{-3(1+w)}. \tag{1.39}$$

Using this expression in the Friedman equation (1.6) gives H in terms of a:

$$H \propto a^{-\frac{3}{2}(1+w)}. \tag{1.40}$$

So a universe has particle horizons if and only if

$$\chi_p(t) \propto \lim_{a \to 0} \int_a^1 \frac{\mathrm{d}a'}{a'^{\frac{1}{2}(1-w)}} \tag{1.41}$$

converges. The integral converges so long as $w > -1/3$, i.e. so long as the expansion is not accelerating. Thus we see that the expanding FRW models with matter obeying the strong energy condition are not only singular (§1.4.1), but also have particle horizons.

The interpretation of particle horizons which cosmologists intend is roughly that all objects within the particle horizon have been or could have been observed by now, and all objects outside of the particle horizon could not have been observed by now. Clearly if the given particle had departed significantly from the co-moving trajectory, this interpretation would fail (maximally if the worldline in question was of one of the light rays). Of course we cannot see the objects referred to by this division as they are now, since at the present time they are spacelike separated from us. If we see them now, we see them as they were in the past. But insofar as the objects are co-moving, we can trace their trajectories back in time. Those objects that are within some observer's particle horizon at some time have trajectories that intersect the past light cone of the observer; those that are outside of the particle horizon do not.

Event horizons will not play a role in the issues I discuss, but I mention them because they are natural to introduce with particle horizons. An event horizon is essentially the time-reversed notion of a particle horizon. It is defined as the farthest spatial distance from which light could reach a given spatial location (location of a fundamental observer) before the "end of the universe." Objects outside of the event horizon will never be observed or causally affected by the fundamental observer in the future, whereas objects inside the event horizon will be observable or potentially affected by the fundamental observer in the future.

The event horizon is typically expressed in co-moving coordinates as the integral

$$\chi_e(t_0) = \lim_{t \to t_f} \int_{t_0}^{t} \frac{\mathrm{d}t'}{a(t')}, \tag{1.42}$$

where t_f is the time coordinate corresponding to the "end of the universe." A cosmological constant dominated universe has event horizons, since in such a universe objects are continually swept beyond the event horizon by the exponential expansion of space. These objects will never be seen again by the given observer; light emitted from them would have to move faster than the speed of light to reach the observer.

There are two additional horizon-like notions prevalent in cosmology, the *observable universe* and the *Hubble sphere*. Neither of these are true horizons, in the sense that they represent a boundary of causal connectability. They are also occasionally mistaken for the properly cosmological horizons just discussed, so it is worth explaining them briefly here.

As the name suggests, the observable universe is that part of the universe that is in principle observable. It might seem that in principle anything in our past lightcone is observable, but the sense of "observable" in cosmology is typically more restricted (Butterfield 2014, 58). It is not possible, according to the HBB model, to observe anything that happened before *recombination*, which is the time when the CMB photons were first allowed to free-stream through the universe (§2.2.1). Before that time radiation was in equilibrium with matter and had a short mean free path, so photons were frequently scattered by atoms. Not until the universe's temperature cooled sufficiently that electrons combined with nuclei to form neutral hydrogen could radiation travel without interacting through the universe. So when we see CMB photons today, we are seeing a snapshot of what the universe looked like billions of years ago, and that is the earliest possible snapshot of the universe we could have by direct observations.[42] Thus the extent of the observable universe depends on the

[42] Recombination does not represent the in principle limit of observability exactly. It may be possible to observe particles directly that decoupled from other particles before recombination. In particular it may be possible to observe primordial neutrinos. It may also be possible to detect

physical processes occurring in the universe.

The Hubble sphere is also often mistakenly identified as a horizon, and sometimes confused with the particle horizon. The Hubble sphere is defined as the surface beyond which the recession velocity of an event is greater than the speed of light. A Hubble sphere, like a horizon, is relative to an event along the worldline of a fundamental observer. It comes from taking the simple Hubble relation, $v = d \cdot H$, setting $v = c$, and using that to define a distance d_H, the Hubble distance. In a simple FRW model where one only has normal matter and radiation the Hubble sphere coincides with the particle horizon, but it need not in general. The existence of dark energy can change the expansion dynamics in such a way that events within the particle horizon are outside of the Hubble sphere.[43] This means that there are observable events that are moving away from us faster than the speed of light.[44] The Hubble sphere thus has nothing to do with causal connectability (although this is not to say it has no physical significance). The Hubble sphere merely sets a useful distance scale for gravitational interactions. Nonlinear gravitational collapse occurs on scales below the Hubble distance—within the Hubble distance we expect to see structure formation due to Jeans instability.

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primordial gravitational waves. There is also some hope that we may be able to detect gravitational waves indirectly by their effect on the CMB's polarization. Results pertinent to this from the Planck satellite should be forthcoming within the next year. This detection would not, however, change the extent of the observable universe because the gravitational waves are being detected indirectly in the CMB.

[43]This situation also points out how the Hubble Law can fail at large distances when the expansion of the universe is accelerating.

[44]This is, of course, no violation of relativity. (Davis and Lineweaver 2004) is dedicated in part to clearing up this confusion that one occasionally finds in the literature.

1.5 Cosmological Observations

The main observational facts about our universe that are important to describe here are those on which the HBB model's fine-tuning problems are based. These are that at a sufficiently early time observations suggest that the universe is extremely uniform (homogeneous), and that observations indicate that the spatial geometry of the universe is extremely close to flat, i.e. $k \approx 0$.

First, however, it is worth making a few remarks about observational limitations in cosmology. As I mentioned at the end of §1.2.1, one of the greatest challenges to modeling the universe is the stringent spatiotemporal localization of our observations (Ellis 2007; Smeenk 2013). We are confined to an exceedingly small spatial region of the observable universe, and the better part of the observational data collected by astronomers has been collected only over a time exceedingly small compared to the age of the universe. Although we can collect some data from (massive) particles reaching us from elsewhere in the universe and also infer something about the universe based on our solar system's own ("geologic") history (Ellis 1999), nearly all of our observations are of light, i.e. almost all of our cosmological observations are along the past light cone of our cosmological neighborhood (Kristian and Sachs 1966).

Such observational limitations significantly underdetermine a correct model of the universe; theoretical considerations make this underdetermination even worse than what seems evident due to the existence of observationally indistinguishable spacetimes that may differ in global properties from any given spacetime (Glymour 1977; Malament 1977; Manchak 2009, 2011; Butterfield 2012). In short, for all we know—for all we can know in fact—the unobservable universe could be significantly different than the observable universe. This could hold because our cosmological models are based on the wrong physics, e.g. GTR is simply incorrect in its cosmological import. However it also holds simply because what we can observe does not place significant enough restrictions on the unobservable regions of spacetime (Ellis et al. 1985). More specifically, observations along our past light cone alone cannot fully fix the spacetime metric or matter distribution so that one cannot fix a model of GTR. Thus the project of observational cosmology cannot be complete, in the sense of a *cosmography*—a "mapping of the universe" (Kristian and Sachs 1966; Dautcourt 1983a,b; Ellis et al. 1985).

> As a particular case of this observational indeterminacy, we find that we cannot prove on the basis of observations alone whether or not space-time is spherically symmetric about our position—even if all these observations are isotropic around us. Even if we go farther and make the important assumption that the universe has a Friedmann-Robertson-Walker geometry, we cannot determine observationally whether the spatial sections have positive, zero or negative curvature. Nor do isotropic observations, together with the assumption of spherical symmetry, prove FRW geometry. (Ellis et al. 1985, 319)

The observations that we can make of course do restrict the possible cosmological models that are candidates for modeling the universe. With additional assumptions (including, for example, the CP discussed in §1.3.1) these observations may fix a unique cosmological model up to parameter fitting by empirical observation. Fixing a model and the refinement of its parameters would amount to the successful attainment of the goal of physical modeling. So, although philosophically or physically motivated assumptions are a necessary component of cosmology (Ellis 1975), it is nevertheless possible to scientifically study the universe. The justification of the model, however, depends crucially on the strength of these assumptions. The CP+ is an especially strong assumption to make, as noted above, because it picks out a small and very special class of highly symmetric relativistic spacetimes. It is appealing to see how much one can weaken that assumption (Ellis et al. 1978; Ellis 1980) since the large-scale spatial homogeneity and isotropy of the universe (or at least near spatial homogeneity and isotropy) are two key conditions that give rise to the horizon and flatness problems.

There are two primary observational means we have presently available for evaluating cosmological assumptions (Ellis et al. 1985, 334): galactic surveys and maps of the CMB.[45] Both types of observations have become increasingly precise in recent years. Rather than survey these efforts in detail, I will merely indicate the extent to which they support the assumption of the CP+.

Major recent galactic surveys include the Sloan Digital Sky Survey (SDSS) and the Two Degree Field (2DF) survey. These projects have surveyed redshifts of hundreds of thousands of galaxies. The redshifts are then used to provide a spatial "map" of the universe. It should be emphasized that the redshift data cannot determine spatial homogeneity due to observational limitations.[46] At best one can detect local isotropy on a series of 2-spheres of constant redshift with these results. Even if all of the spheres of constant redshift were isotropic, the universe could be inhomogeneous (although admittedly one might find these circumstances rather suspicious). The data from redshift surveys can be used to determine the degree of statistical homogeneity of the galaxies, but this requires the assumption that the background cosmological model

[45] There are, however, many other possible cosmological observations, e.g. surveys of radio sources, cosmic neutrinos, gravitational waves, etc. I focus on these two because major efforts have been undertaken to obtain detailed data of these kinds, so the data are quite good.

[46] Because of observational limitations we have almost no means to determine whether the universe is spatially homogeneous at large scales. We certainly have no direct means to do so. Note that spatial homogeneity does not imply spatial isotropy, just as spatial isotropy (at a point) does not imply spatial homogeneity. A spherically symmetric spacetime, for example, is isotropic from the center, but it may not be homogeneous if the spacetime changes with radial distance like the Schwarzschild model does. Likewise, imagine a spacetime that is filled with a homogeneous fluid; the model could nonetheless be isotropic because of a uniform spatial flow in the fluid. Isotropy about every point, however, does guarantee homogeneity.

is already spatially homogeneous geometrically. Thus projects of this kind can at best provide a consistency check on the assumption of homogeneity. Observations of galaxy distributions can however imply isotropy, which they appear to do (Maartens 2011).[47]

The second means is observation of the CMB. A wealth of cosmological data has been obtained through detailed observation of the CMB in recent years. The CMB is strikingly isotropic, but eventually, as observations improved, small anisotropies were discovered (Smoot et al. 1977). Recent observations by the COBE satellite, the Wilkinson Microwave Anisotropy Probe (WMAP), and the Planck observer have mapped the CMB to a very high degree of precision, showing a precise spectrum of anisotropies. These anisotropies are a close match to the putative predictions of inflationary theory, which has been taken as strong confirmatory support of the theory.

Since the decoupling of radiation from matter occurred at a single time (a redshift of approximately 1100 according to the HBB model) we observe it on a 2-sphere of constant redshift. By itself isotropy of the CMB from our observations can enforce neither homogeneity nor isotropy of the spacetime geometry. Homogeneity and isotropy do indeed follow from the perfect isotropy of CMB observations with the additional assumption that all observers see perfect isotropy of the CMB. Since the CMB is not perfectly isotropic, one requires a stronger result. If all observers see the CMB to have a sufficiently small dipole, quadrupole, and octupole, then it follows that the spacetime geometry is approximately homogeneous and isotropic in that region (Clarkson and Maartens 2010; Maartens 2011).

Because of observational limitations we cannot possibly observe that the universe is everywhere spatially isotropic, so it is doubtful that we could ever empirically

[47] According to Maartens in the cited paper, it is not, however, clear whether almost isotropic data implies almost isotropy in the spacetime geometry, which would be the more realistic case.

justify the CP adequately (Mustapha et al. 1997). One may at best determine spatial isotropy relative to our particular worldline; the evidence for (almost) local isotropy from the CMB and galactic surveys is however quite strong.

On the assumption that the universe is homogeneous (CP), one may try to make a consistency check by evaluating the degree of statistical homogeneity. At around 100 Mpc it appears that statistical isotropy does in fact hold however (Wu et al. 1999).[48] Note again that this statistical isotropy at the cosmological present is typically inferred by assuming how the observed objects evolve from the time they emitted the light that we observe to the present. This requires a cosmological model, and naturally the model that is used is one that assumes spatial homogeneity and isotropy.

The basis for the claim that the spatial curvature is approximately zero rests on a couple different kinds of evidence. Recall that a spatial curvature of zero requires the density ρ to be near the critical density ρ_{cr}. One way to determine the density is to count up the contributions to the "energy budget" of the universe from the "cosmic inventory" of its constituents (matter, radiation, cosmological constant, etc.) (Dodelson 2003, 40ff.). There are numerous ways to go about this accounting exercise, for example by looking at the spectra of galaxies and quasars, the distributions of galaxies, etc. All such methods are in rough agreement, pointing to a present density very near the critical density.

The most stringent constraint, however, comes from analyzing the spectrum of anisotropies in the CMB data. It turns out that the first peak in the CMB power

[48]This claim is disputed by Pietronero and collaborators (Pietronero 1987; Coleman and Pietronero 1992; Pietronero and Sylos Labini 2000), who argue that the structure of the universe is fractal and continues beyond the scale of galactic clusters. See Dickau 2009 for a short introduction to the literature on fractal universes and (de Vaucoueurs 1970) for earlier arguments along the same lines.

spectrum is highly sensitive to the value of k (Dodelson 2003, 249). Combining the results from the CMB with those obtained through other means constrains the best fit model tightly (within a couple percent). This model, called the concordance model, gives a total energy density $\Omega \approx 1$, with the density of ordinary matter $\Omega_b \approx 0.05$, the density of dark matter $\Omega_{dm} \approx 0.23$, and the density of dark energy $\Omega_\Lambda \approx 0.73$.

Chapter 2

Fine-Tuning Problems in Cosmology

2.1 Introduction

Various fine-tuning problems are thought to beset the great triumph of 20th century cosmology, the hot big bang model.[1] Chief among them are the so-called horizon and flatness problems. These problems were essential to the motivation of the idea of inflation in its original proposal (Guth 1981), and their significance as fine-tuning problems was promoted immediately by subsequent proponents of inflation (Linde 1982; Albrecht and Steinhardt 1982), along with most of the discipline of cosmology soon thereafter.[2] These problems are still typically presented in modern

[1] Often the terms "fine-tuning" and "special initial conditions" are used in intuitive and roughly overlapping ways, but there is some conceptual space between them should one like to look for it. Nevertheless the distinctions are unimportant to my account and I will use them essentially interchangeably.

[2] The history of these problems is interesting and worthy of study, but I will not engage directly with it for the most part. The interested reader is directed to the following sources: (Longair 2006), a general history of astrophysics and cosmology in the 20th century; (Smeenk 2005), a short history of the development of inflation; (Guth 1997), a popular first-hand account by the father of inflation.

treatments of cosmology to motivate the introduction of cosmological inflation as a solution thereto (Dodelson 2003; Mukhanov 2005; Weinberg 2008; Baumann 2009).

Standard presentations in the cosmological literature of the fine-tuning problems and their putative solutions through inflationary theory briefly relate the relevant physics, and then conclude with some vague statement, the content of which is essentially that the HBB explanation of a certain cosmological feature depends on fine-tuning and is therefore inadequate; inflationary theory is then claimed to address this fine-tuning by introducing new physics which obviates the problem. Precisely how the HBB model is fine-tuned and what makes the alleged fine-tuning problematic is invariably left unclear in such treatments. Without sufficiently clear answers to these questions, however, no rationally compelling case can be made that inflation truly addresses the HBB model's fine-tuning. Although it is widely thought that inflation indeed does so, these claims are based too much on imprecise intuitions. One would prefer that these claims to be based on a sufficiently rigorous analysis of the fine-tuning problems, and on a clear demonstration that inflation solves these problems (so understood). Based on my own such analysis of the possible interpretations of the horizon and flatness problems, I ultimately argue that there is no unproblematic interpretation of either problem available for which it can be said that inflation solves the problem.

Philosophical analyses of the horizon and flatness problems with a similar motivation do exist (Earman 1995; Earman and Mosterín 1999; Smeenk 2003; Maudlin 2007a). Yet the available analyses are either very limited in scope or are generally motivated by peculiar philosophical concerns distant from the kinds of concerns relevant to the practice of cosmology.[3] The present chapter is an attempt to take a

[3]Earman (1995) makes a number of important points, but only considers that the problem might

more physically motivated point of view, and therefore takes the concerns voiced by cosmologists as a starting point.

I focus on giving a thorough and philosophically rigorous analysis of the nature of fine-tuning in the HBB model, and determining whether inflationary theory addresses it in a satisfactory manner. It is however not my brief to argue for or against inflationary theory; nor do I mean to argue at all that cosmologists are praise- or blameworthy for adopting the idea. My aim is only to investigate whether at the present time inflationary theory is justifiably motivated by its putative solutions to the HBB model's fine-tuning problems. I argue that it is not. There is no good reason, I claim, to think that inflationary theory in fact solves these problems. But I emphasize that (some of) the interpretive problems which I point out plausibly can be solved; indeed I believe there are some philosophically interesting possibilities to investigate further—particularly in connection to the idea of dynamical stability and high degrees of symmetry.

The aim of the first half of this chapter (§§2.2-2.3) is to formulate the HBB model's fine-tuning problems as clearly as possible in order to clarify their nature as scientific problems. Intuitions about the significance of the fine-tuning problems do vary somewhat among cosmologists (and the few philosophical commentators), so I will endeavor to "cast the net" somewhat widely by incorporating the comments of numerous expositors of the horizon and flatness problems. I find that cosmologists are best understood as holding that the initial physical conditions that give rise to the

be a lack of a common cause, a failure of Machian intuitions, or that horizons make uniformity unlikely, none of which appear to be behind physicists' concerns. Smeenk (2003) also concentrates on common causes and probability concerns. Earman and Mosterín (1999) eschew any analysis of the nature of the problem, resting their further argumentation on empirical claims that are now known to be false. Maudlin (2007a) argues that the lack of a dynamical explanation is what gives rise to the problem, but there is no qualitative difference in the kind of explanations given by inflationary theory and the hot big bang model, so his diagnosis cannot be correct.

horizon and flatness problems are in some sense "unlikely" and that this improbability is problematic because the explanation of present conditions given by these unverifiable initial conditions is not robust—for all we know they could have been otherwise, in which case the HBB explanation would fail. The essential point made in these sections, however, is that none of the intuitive interpretations of the fine-tuning problems I survey are free of serious technical or conceptual difficulties.

The second half of the chapter (§§2.4-2.5) then turns to the putative solution of the fine-tuning problems by inflation, supposing that the issues raised previously are soluble. Since inflation introduces fine-tuning problems of its own, it is important to understand what a solution to a fine-tuning problem actually accomplishes. I therefore argue for some reasonable success conditions on solving such fine-tuning problems. Based on these I evaluate the inflationary program's success at solving the fine-tuning problems understood according to the various interpretations investigated in the first half of the chapter. Under some interpretations inflation would indeed solve them, but since no interpretation is problem free, I suggest that there remains some important philosophical work in understanding the empirical successes of inflationary theory. The most salient point I wish to urge however is that either some interpretation must be justified which shows how inflation solves the HBB model's fine-tuning problems, or else an alternate rationalization of inflationary theory's widespread adoption in advance of its later empirical successes, one not relying on problem solving, is needed.

2.2 Problems in Big Bang Cosmology

There are perhaps any number of aspects of big bang cosmology that one could find puzzling (Dicke and Peebles 1979), but the two that (Guth 1981) emphasizes as major motivations for introducing inflation are the high degree of uniformity of the CMB and the near spatial flatness of the universe (§1.5), these being the features that lead respectively to the horizon problem and the flatness problem.[4] Besides being an important motivation for inflation, these problems remain the means of introduction to inflation in modern texts, lecture notes, and popular books on cosmology.[5]

In this section I give my own brief presentation of the two problems in order to introduce the relevant physics and highlight a few points that are regularly muddled or glossed over in standard presentations. It is worthwhile to go into the details in order to understand the conceptual significance of the problems, but the argument they are meant to support is simple. We assume the HBB model is correct and observe certain cosmological conditions (flatness and isotropy) consistent with the model. It is either the case that the HBB model explains these observed conditions by fixing certain initial conditions or that it is necessary to introduce some novel dynamical mechanism to the HBB model in order to explain the observed conditions. But the HBB model has particle horizons that preclude introducing some physically realistic dynamical mechanism which could explain the observed conditions. Therefore the HBB model can only explain the observed conditions by fixing initial conditions. These initial

[4]Guth also emphasizes the "monopole problem" as a motivation for inflation, yet I agree with Penrose (1989a) that problems like these are external problems (intuitively, and also in keeping with Laudan's sense of the term) to cosmology. The monopole problem arises because certain grand unified theories (GUTs) of particle physics predict the creation of magnetic monopoles in the early universe in such a quantity that they should have been observed by now. No monopoles have been observed. Either one takes this fact as suggesting a problem with cosmology or a problem with the GUT. GUTs remain speculative, so it is hard to see how a prediction of such a theory should constitute a significant problem for a highly confirmed model like the HBB model. (Linde 1990) and (Linde 1984) outline several other potential problems with HBB cosmology, many of which are external problems like the magnetic monopole problem or are equivalent to the horizon or flatness problems.

[5]In almost any text, set of lecture notes, etc. that treats inflation one will find a presentation of the horizon problem and the flatness problem. I recommend in particular the detailed lecture notes by Lesgourges (2006) for their treatment of these problems. Standard modern texts, to which the reader is invited to refer, include Liddle and Lyth (2000); Dodelson (2003); Mukhanov (2005); Weinberg (2008); Peter and Uzan (2009); Ellis et al. (2012).

conditions are thought to be problematic, for which reason the HBB explanation is rejected.

This section explains the cosmological scenario that gives rise to the horizon problem (§2.2.1) and the flatness problem (§2.2.2). The next section (§2.3) then investigates what about this scenario could be seen as problematic.

2.2.1 What Is The Horizon Problem?

The basic empirical fact that suggests the horizon problem is the existence of background radiation with a high degree of isotropy (uniformity in all directions): the CMB.[6] In every direction we observe the CMB to have the spectrum of a thermal blackbody with a temperature T_0 of 2.725 Kelvin (2×10^{-4}eV), and departing from perfect isotropy only to one part in 100,000.

A fundamental assumption of the HBB model is the CP+ (§1.3.1): the universe as a whole is (approximately) spatially homogeneous and isotropic. Assuming the cosmological principle, the high degree of isotropy in the CMB is not just a fact about our particular observational situation; the CMB is isotropic for every fundamental observer in the universe (§1.4);[7] in other words the present temperature of the CMB is inferred to be everywhere 2.725 Kelvin.The explanation for the observed background radiation according to the HBB model is that the observable universe was highly "uniform" at and before the time of recombination. Assuming the CP+, the high degree of isotropy in the CMB is not just a fact about our particular observational

[6]The CMB is called "background radiation" because it originates from the cosmos and not from discrete sources (such as stars, quasars, etc.).

[7]Recall that fundamental observers are those (not necessarily animate) cosmological objects which are at rest with respect to the universe's expansion, i.e. they follow (timelike) spacetime geodesics. For any geodesic there could have been an observer that followed it, so one permits oneself the use of the "fundamental observer" terminology for any such case.

situation; the CMB is isotropic for every fundamental observer in the universe. So there is a pervasive, uniform background radiation in the universe with the current low temperature of $T_0 \sim 2 \times 10^{-4}$eV.

The HBB model in fact predicts the existence of this radiation, since it is released as a consequence of the universe's expansion and simultaneous cooling past the temperature where neutral hydrogen can form (an event known as recombination), which prompts radiation (photons) to decouple from matter and "free stream" throughout the universe. This radiation then cools with the expansion down to the presently observed temperature T_0. Thus, according to the empirically well-confirmed aspects of the HBB model, what essentially gives rise to this uniform background radiation is that the observable universe was highly uniform as a whole in its matter distribution at (and presumably well before) the time of recombination, and it then remained so afterwards.

That is the basic HBB story, but some further details are needed to understand the horizon problem. Since observations indicate that the universe is expanding, one can parameterize this expansion by the scale factor a. The energy density of the CMB photons decreases with time (§1.3.2): $\rho_\gamma \propto a^{-4}$. It follows, with some simplifying assumptions (Dodelson 2003, 40-41), that

$$a(t) = \frac{T_0}{T}, \tag{2.1}$$

where $T_0 = 2.725K$ is the present temperature of the radiation background. Thus one sees that not only are early times characterized by higher energy densities, but high temperatures too.

Higher densities give rise to high reaction rates for the various particular constituents of the universe, and through these constant interactions the universe usually finds itself in a state of equilibrium.[8] When the expansion rate exceeds the reaction rate of some interaction, however, the particles participating in that reaction temporarily fall out of equilibrium. The reaction subsequently becomes "frozen out", i.e. essentially stops occurring, as temperature further decreases and a new equilibrium state is established.

Recombination is one such out-of-equilibrium event. It occurs when the temperature drops low enough ($T_* \sim .25$eV or at $a_* \sim 9 \times 10^{-4}$) such that neutral hydrogen can form from protons and electrons. The drop in free electrons during recombination leads to the rate of photon-electron (Compton) scattering to drop below the expansion rate, so that the photons decouple from matter. As the universe continues to expand, the rate of photon scattering only lessens. Thus the CMB photons have been traveling throughout the universe since decoupling essentially without interactions, i.e. they have been "free-streaming." Since the photons have been free-streaming since decoupling, the CMB provides a snapshot of "what the universe looked like" at the time of recombination—of an extremely uniform universe.

How did this uniform state of the universe at recombination come about? Assuming the HBB model is correct, either one extrapolates the uniform state of the universe back in time to some initial state of uniformity—back to the big bang itself (arbitrarily close to "$a = 0$") or as far back as one is willing to assume that the model remains accurate; else one supposes that some novel dynamical mechanism brings the universe to a state of spatial uniformity some time before recombination. In the first case, a uniform initial state plus the dynamical laws of GTR explain the uniform

[8]Expansion is important to maintain states of equilibrium in cosmology, since insufficient expansion would lead to gravitational collapse of the constituents of the universe (Carroll forthcoming).

state of the universe during recombination, which explains the observed isotropy of the CMB. In the second case, the initial state of the universe is supposed to be other than uniform, yet some dynamical mechanism drove the universe to a uniform state before recombination, which state can then explain the observed isotropy of the CMB.

The second explanation, however, is precluded in the HBB cosmology (at least insofar as dynamical mechanisms are thought to operate "locally" or "causally"), because the HBB model has particle horizons (§1.4.2). Recall that a particular fundamental observer's particle horizon represents limits (given in distance at a time) on physically possible causal interactions that could have occurred between the observer and other co-moving objects in the universe. A co-moving object beyond the observer's particle horizon (at a given time) could never have been influenced causally by the observer, just as the observer could not have been causally influenced by that object. When one investigates the horizon structure of our universe (assuming it is modeled by the HBB cosmology), one sees that no such local and causal dynamical mechanism could be responsible for the observed isotropy of the CMB, since the horizon of any given fundamental observer at the time of recombination is smaller than the distance over which the universe is assumed to be homogeneous (I explain this further shortly). These horizons are for this reason the namesake of the "horizon" in "horizon problem", since their presence represents one essential challenge to explaining the particular, uniform state of the CMB dynamically.

To illustrate the claim that dynamical explanations of the uniformity of the CMB are precluded by particle horizons, consider the following typical example from the literature. Suppose an observer points his telescope to the celestial north pole and detects a sample N of photons reaching the Earth, and another observer points her telescope to the celestial south pole and detects a sample S of photons reaching the Earth. If we trace N's and S's worldlines back to some fundamental observers n and s that emitted them, respectively, at recombination, then one would find that n's particle horizon at the time of recombination does not overlap at all with s's particle horizon. In short, n and s are causally disconnected. They could never have interacted—yet they have the identical blackbody spectrum at the identical temperature.

Indeed, the situation is considerably more "conspiratorial" than this. The CMB can be decomposed into an astronomical number of causally disconnected patches due to the existence of horizons in the HBB model.[9] Here is an explicit calculation that illustrates the magnitude of the causal disconnectedness of the HBB universe with presently observed conditions. The particle horizon of the observable universe at the present time (physical distance light could have traveled since the big bang) $\chi_{obs,0}$ is approximately 10^{28}cm (10^{10} light years). Since the universe has been expanding, the size of this observed homogeneous, isotropic domain at early times was smaller. At recombination the size $\chi_{obs,*}$ of the homogeneous, isotropic region that grew into the present one is equal to the size of the present horizon $\chi_{obs,0}$ times the ratio of scale factors:

$$\chi_{obs,*} = \frac{a_*}{a_0}\chi_{obs,0} \sim 10^{-3} \times 10^{28}\text{cm} = 10^{25}\text{cm} \tag{2.2}$$

Now we compare this to the size of the particle horizon at the time of recombination, $\chi_{cmb,*} \sim 10^{23}$cm. The ratio $\chi_{obs,*}/\chi_{cmb,*}$ is

$$\frac{\chi_{obs,*}}{\chi_{cmb,*}} \sim \frac{10^{25}\text{cm}}{10^{23}\text{cm}} \sim 10^2 \tag{2.3}$$

[9] Decomposing the CMB into causal patches involves partitioning the observable last scattering surface (space at the time of decoupling) into regions of space, each of which has all the other patches falling outside of its particle horizon at that time.

Thus the observed CMB can be divided into around 10^5 circular patches that were causally disconnected at the time of recombination. Since then a tiny number of the photons in adjacent patches have had time to interact with one another or with other matter, but for the most part they have not due to the low reaction rate at later times. It should be clear from the calculation that as one pushes the assumption of homogeneity farther back in time (to times much before recombination), the number of causally disconnected patches only increases.[10]

The local, causal dynamical explanation of a case of observed homogeneity and isotropy that one usually envisions in physics is a process of thermal equilibration. The CMB radiation has the spectrum of a near perfect black body (the spectrum one observes from a system in thermal equilibrium), so a seemingly natural explanation of this spectrum would be that the universe came to equilibrium at some early time. Statistical mechanical arguments are usually taken to show that interacting systems (like the universe) are expected to be found in a thermodynamic equilibrium given enough time. If the universe did not have particle horizons, then an explanation like this might well be expected to hold (since the universe is quite old, $\sim$ 14 Gyr). The presence of particle horizons makes it impossible for the observable universe as a whole to have equilibrated, since there has not even been enough time for the causally disconnected regions in the universe to interact at all by the time of recombination, much less come to an equilibrium.[11]

The upshot is that no realistic dynamical process could have coordinated the uniformity of the entire observable HBB universe, since it is usually assumed that any such realistic dynamical mechanism must act causally (the thought being that

[10] See, for example, Mukhanov (2005, 227) for similar calculations.

[11] Characterizing the horizon problem in terms of the failure of an equilibrium argument is somewhat misleading in any case—see (Carroll forthcoming, §§3.2-3).

influences and signaling occur "locally" in GTR and quantum field theory—in an appropriate sense of "locally" (Maudlin 2011)).[12]. Any given photon, and anything else for that matter, could only have interacted with a tiny fraction of the contents of the observable universe by the time of recombination, so the extent to which a dynamical process could drive the universe to uniformity is extremely limited. Thus it seems that this uniformity must depend on a "conspiracy of initial conditions;" in other words, each of the large number of causally disconnected patches had to have begun with similar initial conditions for the universe to be as uniform as it was by recombination. The only viable explanation of uniformity in the CMB in the HBB model, then, appears to be that the initial conditions of the universe, at some sufficiently early time, had to be quite nearly homogeneous and isotropic.

This discussion should make clear that the horizon problem (in this context) is really more of a "uniformity problem" (Earman and Mosterín 1999, 18), since it is the empirical fact of CMB isotropy, and by extension homogeneity via the cosmological principle, that is "puzzling" or felt to be in need of explanation. If one considers the horizon problem as the problem that horizons simply *exist* in the universe, then likely the worry is not over horizons but the existence of a singularity in the past.[13] While singularity avoidance remains a motivation in present theoretical research, it is not necessarily a motivation for inflation (since inflation is certainly possible in singular spacetimes).[14]

The existence of particle horizons does play an important role in the generation

[12]But see (Wald 1992) for a contrary view which discusses the possibility that quantum correlations beyond the horizon may play an important role in the early universe.

[13]My discussion of singularities and horizons in the previous chapter (§1.4) shows how these are connected in the HBB model. The past singularity of the HBB model is responsible for the existence of horizons in the model.

[14]Soviet precursors working on similar ideas to Guth's were in fact motivated by avoiding an initial singularity (Smeenk 2005). See, e.g. (Starobinsky 1980).

of the problem discussed in this section, namely as a constraint on possible explanations of uniformity, so the terminology "horizon problem" is apt. In the following I will prefer referring to the general problem of explaining the uniformity of the universe as the uniformity problem (for the sake of conceptual clarity), but I will nevertheless occasionally use "horizon problem" ambiguously to refer to the horizon constraint and the uniformity problem (in keeping with general usage); I expect context makes clear which usage is operative.

2.2.2 What is the Flatness Problem?

The basic fact inferred from observations which suggests the flatness problem is that the universe has a flat spatial geometry. The CP+ selects a set of highly symmetric spacetimes from the models of GTR, the Friedman-Robertson-Walker models (§1.3.2). These models have uniform spatial curvature k of three different kinds: positive like a sphere ($k = 1$), negative like a hyperboloid ($k = -1$), or flat like a plane ($k = 0$). Since there is a sense in general relativity in which "matter causes spacetime to curve," one can equivalently place a condition on the matter content of FRW models which determines the model's spatial geometry. If the energy density ρ is equal to the critical density ρ_{crit}, then the universe's spatial geometry is flat; if it is less than the critical density, then the spatial geometry is negatively-curved; if it is greater than the critical density, then the spatial geometry is positively-curved. Although we cannot directly observe the flatness of space, there is a variety of evidence, when interpreted in the context of the HBB model, that supports this conclusion (§1.5). In particular, the spectrum of small anisotropies in the CMB strongly constrain the density parameter $\Omega = \rho/\rho_{crit}$ to very close to one: $\Omega = 1.000 \pm 0.005$ (Planck

Collaboration 2015, 38).

Cosmologists often demonstrate the flatness problem by showing how flatness is an unstable condition in FRW dynamics. The Einstein field equations reduce to two equations in the highly symmetric FRW universes: the Friedman equations are two typical expressions of these two equations (§1.3.2). The Friedman equation (1.3) can be written in terms of Ω ((1.13)):

$$1 - \Omega(a) = \frac{-k}{(aH)^2}, \tag{2.4}$$

where $H = \dot{a}/a$ is called the Hubble parameter. Since we are interested in departures from the critical density when considering instability, let us ignore the $k = 0$ case and whether the departures are positive or negative. Then one can rewrite the previous equation as

$$|1 - \Omega(a)| = \left(\frac{1}{aH}\right)^2. \tag{2.5}$$

From this equation one can infer that in the HBB universe the right hand side is always increasing (normal matter decelerates expansion), and therefore the energy density of the universe had to have been even closer to the critical density in the past—the earlier the time, the closer to the critical density. One may do various calculations to show that, given the accuracy to which the density is known today, the critical density at early times had to be constrained to an extraordinarily accurate value; some calculations indicate, for example, fine-tuning to one part in 10^{55} at the GUT scale (Baumann 2009, 25).

For further illustration, let us assume that matter obeys a simple equation of state during the various epochs of the universe, namely the pressure $p = w\rho$ for some number w. Differentiating the Friedman equation with which we started and using the Friedman acceleration equation, one can derive that

$$\frac{\mathrm{d}\Omega}{\mathrm{d}\ln a} = \Omega(\Omega - 1)(1 + 3w). \tag{2.6}$$

We wish to see how Ω behaves under slight perturbations from the critical density, so assume that $\Omega = 1 \pm \epsilon$ at the present time, with ϵ small. At other times we assume $\Omega = 1 \pm \delta(a)$. We can integrate the previous equation easily, yielding

$$\delta(a) = \epsilon a^{(1+3w)}. \tag{2.7}$$

Thus flatness is unstable under small perturbations so long as $(1 + 3w)$ is positive, i.e. the strong energy condition is satisfied. Since the strong energy condition is indeed assumed to hold in the HBB model at early times, we conclude that flatness is dynamically unstable. This suggests that the initial conditions of the universe had to be very special—only a narrow range of initial densities of matter could have resulted in the universe we observe.[15] If the initial density had been much different, the universe would have collapsed by now (for $\Omega > 1$) or would have already cooled rapidly (for $\Omega < 1$). The current density parameter is bounded between $\Omega_+ = 1.005$ and $\Omega_- = 0.995$. So, according to the previous equation, at recombination ($w = 0$ and $a_* = 9 \times 10^{-4}$) the density parameter Ω must have been $\sim 1 \pm 1 \times 10^{-6}$; at the GUT scale the flatness was even more extreme: $\Omega = 1 \pm 1 \times 10^{-40}$.

Why is the universe so close to the critical density? I can afford to be brief, as the argument is parallel to the explanation of uniformity above. One explanation is

[15] Because of these facts, the flatness problem is occasionally called the "age problem." The question behind the problem is, "How did the universe get to be as old as it is?"

just that the universe is in fact that flat and indeed was even flatter at early times. One might even assume that k is exactly zero, which is certainly attractive due to its simplicity (Dicke and Peebles 1979, 507). It is also conceivable that some dynamical mechanism drove the universe to a flat geometry at some early time. However, just as in the case of the uniformity problem, horizons represent an obstacle to any such dynamical explanation, so one must conclude that very particular initial conditions (extremely close to flatness) are necessary in order for the HBB model to explain the presently observed flatness of the universe. Thus (although it is not usually pointed out or recognized) the horizon problem represents a constraint on both the uniformity problem and the flatness problem.

2.3 Scientific Problems

The previous section exhibited two cases where an explanation is sought for observed cosmological conditions. In the first case the explanandum is the remarkable uniformity of the CMB; in the second it is the remarkable flatness of the universe's spatial curvature. The existence of horizons in the HBB model precludes the possibility of some dynamical mechanism bringing these conditions about. Instead one is forced (in the context of the HBB model) to assume particular initial conditions which give rise to the presently observed conditions.

Thus, although one sometimes encounters comments to the contrary, the HBB model certainly has the resources to provide explanations of these features. That is, it is not at all the case that the HBB model is somehow empirically (or descriptively) inadequate (Earman and Mosterín 1999, 19). The model simply requires that the universe has always been remarkably uniform and flat (up to the limits of its range of applicability). This is completely in accord with familiar theories of explanation. According to Hempel's deductive-nomological theory of explanation, for example, uniform and flat initial conditions plus the dynamical laws of the general theory of relativity provide a sufficient explanans to account for the observed uniformity and flatness (Earman 1995, 139). More sophisticated theories naturally acknowledge such explanations (initial conditions plus dynamical laws) as well, since they are paradigmatic of most familiar and accepted physical explanations.

Cosmologists find the HBB model's explanation of uniformity and flatness unsatisfying. This dissatisfaction is exemplified through the identification of the uniformity problem and the flatness problem as problems, and is primarily directed toward the initial conditions that must be assumed. But what makes the HBB explanation problematic, such that an alternate explanation is desirable or even demanded? That is the question to be addressed in this section.

To try to get a handle on what the nature of the problems is, let us first see what cosmologists explicitly say about the initial conditions that figure into the uniformity and flatness problems. Usually one finds a presentation, similar to the one I have given in the previous section, in cosmological texts, but, as I mentioned already, little discussion of what makes the stated facts precisely problematic. One only finds appended to the statement of the relevant facts, e.g. that the density parameter Ω must have been $\sim 1 \pm 10^{-6}$ at recombination, a comment suggesting that such facts are "puzzling" and yield "impressive numbers" (Guth 1981), are "fantastic" and yield "large numbers" (Linde 1990), are "profound" and "disturbing" (Dodelson 2003), and even are "contradictory" (Weinberg 2008); the HBB explanations by way of initial conditions are said to be "unpalatable" (Rees 1972) "unnatural" (Wald 1984; Olive

1990; Goldwirth and Piran 1992; Mukhanov 2005), "unlikely" (Liddle and Lyth 2000), "special" (Riotto 2002), or improbable (Linde 1990). Although examples could be easily multiplied further, these remarks essentially cover what one finds in the literature.[16]

Few authors note that the HBB model is empirically adequate with respect to the observed uniformity, but some do. Among the remarks of those who do, one can find some suggestions of what problem might be behind the horizon problem. For example Baumann (2009) remarks that the HBB model has "shortcomings in predictive power" and Misner (1969) states that such models "give no insight" into the uniformity of the CMB.[17]

The trend in these comments suggests that the initial conditions of the HBB model are thought to be special in some respect, from which one infers that this specialness is somehow the cause of concern. Certainly philosophers have made claims that fit this general pattern (Munitz 1986; Earman 1995; Earman and Mosterín 1999; Maudlin 2007a).[18] But in most cases these available analyses misdiagnose

[16]Also one finds a complaint about the initial condition's lack of explanation itself. As Hawking points out in the introduction to the proceedings of the first workshop primarily focused on inflationary theory, "the [HBB] model does not explain why the universe was as it was at one second. It is simply assumed as an initial condition" (Gibbons et al. 1983, 2). Guth also remarks that "in the standard model this incredibly precise initial relationship [to insure flatness] must be assumed without explanation" (Guth 1981, 347). Since the HBB explanations appear to be of the sort found in classical, relativistic, and quantum mechanics, where there is usually no further need to explain these initial conditions in practice, the observation that the initial conditions are not explained by the model does not seem to be especially significant. Is cosmology somehow different? Indeed, there are some distinct explanatory considerations in cosmology, which will are discussed at the end of this section. Some cosmologists also have expect initial conditions to be eventually eliminated from physics.

[17]Misner is often acknowledged to have been instrumental in promoting the horizon problem (although not under that name) as a problem for cosmology. He adopted an uncharacteristic attitude (for the time) towards cosmology: "Rather than taking the unique problem of relativistic cosmology to be the collection and correlation of observational data sufficient to distinguish among a small number of simple cosmological solutions of Einstein's equations, I suggest that some theoretical effort be devoted to calculations which try to 'predict' the presently observable universe" (Misner 1968, 432). See (Smeenk 2003) for historical comments on Misner's approach and its influence.

[18]For example, Smeenk identifies cosmologists' complaint as being that "[the HBB model] is explanatorily deficient, because it requires an 'improbable' initial state" (Smeenk 2013, 632). My analysis is in agreement with Smeenk's statement, but fills in the details for why explanatory

cosmologists' concerns, replacing them with the kinds of concerns that would occur only to philosophers, e.g. a failure of the principle of the common cause or of sufficient reason. Little attention is paid to what cosmologists actually say about these problems (to be fair, they do say little!) or what physical or methodological grounds there might be to cause them concern. The goal of my subsequent analysis, then, is to be somewhat more systematic in surveying various ways that these initial conditions are physically special, and why special initial conditions of these kinds are problematic, based on the complaints that cosmologists actually make.

The arguments I presented in the previous sections, which underlie the uniformity and flatness problems, are as follows:

1. The present universe is observed to be spatially flat and uniform.
2. Either the HBB model explains these conditions by fixing an initial condition or by a novel dynamical mechanism that brings them about.
3. The HBB model's particle horizons preclude such a dynamical mechanism.
4. Therefore spatially flat and uniform initial conditions are required to explain the presently observed flatness and uniformity.

Cosmologists reject the conclusion of this argument as an adequate explanation roughly according to the following argument sketch:

1. Uniformity and flatness are special initial conditions.
2. Special initial conditions are problematic.

deficiencies are problematic in the context of cosmology, details missing in his analysis.

3. Therefore, uniformity and flatness are problematic initial conditions (give inadequate explanations).

For the sketch to be a full and rationally compelling argument, one requires grounds for the two premisses, i.e. one requires answers to the following questions: “Why are uniformity and flatness special?” and “Why are these special initial conditions problematic?”

Both Guth and Linde draw attention (in the remarks quoted above) to the size and accuracy of the numbers that fall out of the calculations involved in the horizon and flatness problems. There is certainly nothing special, mathematically-speaking, about large numbers in themselves; such numbers in fact appear ubiquitously in physics. Nor is there anything particularly problematic about them on the face of it. They are, after all, just numbers. Incredible accuracies also do not seem intrinsically suspicious. When explicitly represented by numbers, initial conditions and parameters have to take some such (more or less precise) value presumably. Why not one with a large exponent? It is thus hard to take seriously the idea that such calculated numbers are *by themselves* indicative of a problem.

If the concern of cosmologists is simply the numbers involved in their calculations, it would therefore appear to be easy to adopt a skeptical position and reject that the horizon and flatness problems are truly problems. This appears, in any case, to be the attitude adopted in (Earman 1995; Earman and Mosterín 1999). Regarding such large numbers, for example, Earman and Mosterín draw attention to a quotation by Guth in (Lightman and Brawer 1990, 475):

> In an interview Guth said that initially he was less impressed by the horizon problem than by the flatness problem because the latter but not the former involves a ‘colossal number’ that must be explained. (This fascination

> with colossal numbers is something that seems to infect many inflationary theorists.) Is there really a substantive difference here? (Earman and Mosterín 1999, 23, fn. 17)

Clearly, the tone of this footnote leaves little doubt that the authors believe there is no such substantive difference. If one takes Guth's comments merely at face value, then it is indeed hard to take seriously that there is any substantive difference. But a thorough analysis should obviously go beyond comments taken at face value. Indeed there are plainly at least some substantial differences between the two problems. For example, uniformity may be dynamically unstable in (some) expanding FRW models (when we consider small perturbations), but flatness is demonstrably unstable within the context of FRW models that satisfy the strong energy condition (§2.2.2). So it is at least plausible that the numbers to which Guth refers do arguably have a significance beyond their mere size or accuracy when suitably interpreted. This difference may not simply be in terms of "colossal" numbers, but in real features of the cosmology in question.

Thus whether cosmologists have a "fascination" or any other psychological reaction to such numbers is simply beside the point, insofar as one is after a philosophical analysis of fine-tuning. Although cosmologists certainly do use subjective psychological language to describe their reactions to the problems—puzzling, impressive, fantastic, profound, disturbing, unpalatable, etc.—what makes a problem a scientific problem is not these reactions alone. Science does indeed aim to explain things that are puzzling and profound, but it plainly does not aim to explain all such things.[19] As Nickles observes, "scientists know that some problems are more interesting than

[19] Cf. Callender's discussion of the flatness problem and the example of a surprising hand of cards in a card game (Callender 2004a, 202). "Surprisingness is of course a psychological notion, and we do not ordinarily demand that science explain away surprising events."

others" (Nickles 1981, 87)—certain problems attract attention, others do not. The interesting question is why the former do and the latter do not.

That a large or accurate number is suggestive of a problem therefore reasonably depends on more than just the number itself—but on what? The underlying mathematics of a theory cares little for particular numbers (apart from identities, etc.). For a number to be suggestive of a problem, it must be substantially linked to some theoretical expectation, otherwise it at best amounts to an uninteresting "oddity."[20,21] Theoretical expectations of a future theory are no sure guide to a correct future theory, but they do often derive from "positive heuristics" (Lakatos 1970) rather than being mere guesses. To understand how large numbers may be suggestive of a scientific problem, one should therefore understand the operative heuristics.[22]

One of course does not have to look far to find such heuristics guiding problem

[20]Before the advent of inflation many prominent cosmologists felt that the horizon and flatness problems were puzzling, but did not represent real problems (see the interviews by Lightman and Brawer (1990) and (Brawer 1995)). Once Guth clearly articulated the problems and a potential solution (which depended on relaxing the horizon constraint on dynamical solution), the community took them much more seriously. Laudan (1978) in particular comments on this frequent phenomenon, namely that problems often appear only after their solution. This is in keeping with the view of scientific problems defended in, e.g. (Nickles 1981), that "a problem consists of all the conditions or constraints on the solution plus the demand that the solution (an object satisfying the constraints) be found." In a slogan: "Knowing what counts as an answer is equivalent to knowing the question" (Hamblin 1958).

[21]Fine-tuning problems are often described by physicists as "aesthetic" problems, by which they mean that these problems are problematic only because of certain theoretical expectations that may not be satisfied: "There does not have to be a resolution to the aesthetic questions if there is no dynamical solution to the fine-tuning of the electroweak scale, it would puzzle us, but would not upset anything within the fundamental theory. We would just have to live with the existence of fine-tuning" (Donoghue 2007, 232).

[22]Laudan remarks that "anything about the natural world which strikes us as odd, or otherwise in need of explanation, constitutes an empirical problem" (Laudan 1978, 15). A statement like this may make it seem, at least for Laudan, that our proclivities to find something odd or in need of explanation are indeed grounds for raising scientific problems of the kind explored in this paper. Yet these proclivities are based on something more objective—as Nickles proclaims, "problems are entities which have 'objective' existence" (Nickles 1981, 111)—than mere subjective prejudice. Laudan agrees: "Our theoretical presuppositions about the natural order tell us what to expect and what seems peculiar, problematic or questionable" (Laudan 1978, 15). The operative heuristics or theoretical presuppositions thus are precisely the appropriate objects of philosophical investigation—at least from this point of view in the philosophy of science.

statements and solutions in theoretical physics. Behind expectations in fine-tuning cases in particle physics, for example, is the concept of naturalness. The notion of naturalness applied in particle physics has a precise sense (which I will not be discussing), but one might be inclined to think of it as a certain simplicity in parameters and initial conditions.[23] However, uniformity and flatness are clearly simple conditions (spatial homogeneity and isotropy, and $\Omega = 1$, respectively) so a lack of simplicity, at least, does not at least appear to be at work in problematizing the HBB model.

Perhaps instead it is precisely the simplicity of the initial conditions that is cause for concern. Spatial uniformity is certainly a special condition on models in GTR in a precise sense—indeed, spatial uniformity is the greatest amount of spatial symmetry a spacetime can have. Flatness is special as well insofar as it is precisely a point of dynamical metastability in FRW models. In ways that can be made precise, then, almost all other models of GTR do not have so many symmetries as the spatially uniform FRW models, and there are no other metastable FRW models besides the one with flat geometry.

It is therefore possible to identify the HBB model's initial conditions as special in a clear sense, i.e. to ground the first premiss in the argument sketch. This in part supports a heuristic such that a maximal degree of symmetry is deemed unphysical, or one which holds that cosmological models should not exhibit dynamical instabilities. But one should be able to explain why high degrees of symmetry and dynamical instability are problematic as well (at least in cosmology), i.e. to ground the second premiss. There are, on the face of it, some reasons to doubt that this can be done.

In the first place, symmetry considerations play a central role in contemporary

[23]See (Williams 2015) for this precise sense, and a clear, philosophical analysis of its physical significance.

theoretical research, so it cannot be that there is something problematic about a physical model simply possessing symmetries. There are certainly questions one can raise over their ontological and epistemological significance (Brading and Castellani 2003), but it does not seem to be the case that models with high degrees of symmetry necessarily lack, e.g., predictive or explanatory power. Is there something problematic specifically about cosmologies with high degrees of symmetry? There is some evidence (Isenberg and Marsden 1982) that symmetric spacetimes are exceedingly rare in the space of solutions of GTR. To turn this into argument that disfavors the possibility of symmetric spacetimes, however, requires additional considerations beyond the observation that symmetric spacetimes are special (in this sense). These considerations are usually advanced in the guise of probability theory (although measures and topologies can be used to this purpose as well)—so in this case it is improbability (or unlikelihood) that is essentially problematic, not symmetries in and of themselves.

There is a suggestion of another potential explanation of why symmetries are problematic in cosmology when one recognizes that cosmological models are highly idealized, coarse-grained representations of the actual universe. The actual universe is clearly not perfectly spatially homogeneous and isotropic, nor is it even a linearly perturbed version of such FRW models (gravitational collapse is much evident in the universe and is a non-linear process). Such cosmologies may be good “high-level”, coarse-grained approximations of the actual universe, but if the dynamics of the “low-level”, finer-grained model does not keep it in the neighborhood of the idealized, symmetric, high-level model, then one has the start of an argument to disfavor it as a reliable model.[24] This line of thought, however, again leads us away from the idea

[24]See (Wainwright and Ellis 1997) for an introduction, relevant to this line of thought, to the application of dynamical systems theory to cosmology.

that symmetries are problematic to other considerations, in this case to considerations of dynamical instability.

With respect to dynamical instability, one might first observe that the very notion, standardly used in investigating dynamical stability, of a perturbation is absurdly unphysical in the context of cosmology, as there is no external perturbing agent to be found "outside of the universe." Since perturbations evidently require some perturbing agent, it may seem that dynamical instability is not problematic in cosmology in the same way that it is in physical systems which are in environments where small perturbing agents are ubiquitous, in which case it is to be expected that one never finds a dynamically unstable system in a metastable state.

The famous case of Einstein's static universe being unstable under various perturbations (Earman 2001, 195), as shown by Eddington, is however an example where instability was and is widely thought to be problematic.[25] Eddington claimed that "the initial disturbance can happen without supernatural interference" (Eddington 1930, 670), i.e. the perturbation could come from within the universe rather than without. Although he gave suggestions, he did not give an explicit account. Subsequent work (Harrison 1967; Gibbons 1987; Barrow et al. 2003) has clarified under what conditions the Einstein universe is stable (and similarly for other cosmologies as well). But the necessity or even preference for stability in cosmology should not be assumed without argument. This is especially so since there are arguments from dynamical systems theory that might be taken to suggest that cosmological dynamics should in fact be expected to exhibit instabilities (Tavakol and Ellis 1988; Tavakol 1991; Coley and Tavakol 1992).

[25] The argument convinced Einstein himself that his model was unphysical (Nussbaumer 2014).

Although these cursory comments above cannot be said to decide the issue definitively, I believe the considerations raised should be enough to cast doubt on there being a strong and obvious case to be made for simplicity being problematic in cosmology. In particular the case has not been explicitly made by inflationary cosmologists when explicating the horizon and flatness problems. Without such an explication, it simply cannot then be said that simplicity is at the heart of the HBB model's fine-tuning problems.[26]

In any case, the kinds of arguments centered on simplicity appeared to lead directly to likelihood considerations, and indeed cosmologists do say frequently that uniformity and flatness are "unlikely" or "improbable."[27] Furthermore, the other complaints mentioned previously (simplicity, large numbers and accuracies) can be made to fit under the broad umbrella of "probabilistic" reasoning. For example, the high degree of symmetry exhibited by uniformity can be intuitively described as improbable (since there are so many ways that it could have lacked those symmetries), and that our universe is close to a metastable spacetime can be said to be unlikely (since it could have been curved in so many ways). It is therefore tempting to interpret the uniformity and flatness problems as problems based on the improbability of the observed conditions in the context of HBB theory. It captures most of cosmologists' complaints in a common framework. Perhaps for these reasons one finds philosophers generally concluding that special initial conditions are special because improbable (Earman 1995; Earman and Mosterín 1999; Price 2002, 1996; Smeenk 2003, 2013).

[26]Nevertheless I do think the issue is of some importance and interest, and deserves an independent philosophical investigation that goes beyond my treatment here. There is a rich physics literature on stability and symmetries in GTR and cosmology, but as yet very little philosophical attention has been paid to it.

[27]"The [FRW] models are clearly very special within the class of all cosmological models, and so a priori are highly unlikely" (Wainwright and Ellis 1997, 3).

There are, however, significant challenges to adopting improbability as the explication of specialness in this context which have not yet been adequately addressed. These challenges are the focus of chapter 4. Whether such descriptions can be substantiated objectively remains an open question, despite many earnest attempts to do so (Gibbons et al. 1987; Coule 1995; Gibbons and Turok 2008; Carroll and Tam 2010). The technical problems with defining a natural probability measure on the space of possible cosmologies are well-detailed in Schiffrin and Wald (2012); Curiel (2015). Curiel relates the difficulties of constructing suitable likelihood measures on the infinite-dimensional space of relativistic spacetimes; even restricting attention to minisuperspace, the finite-dimensional space of FRW models with a scalar field to drive inflation, Schiffrin and Wald show that a probability measure cannot be defined without some arbitrary choice of regularization of divergent integrals, since the natural Lebesgue measure gives the total space infinite measure. Although arguments for particular regularizations have been made (Carroll and Tam 2010; Gibbons and Turok 2008), Schiffrin and Wald (2012) and Curiel (2015) give compelling reasons to doubt whether such choices have any physical significance.

One may also raise various conceptual problems with making sense of probability in cosmology that cast doubt on interpreting specialness as improbability in the context of these problems (Ellis 2007; Smeenk 2013). Briefly, here are two simply stated issues, which will be discussed further in chapter 4. First, it is unclear what the appropriate reference class for cosmological probabilities is. How can we know what the space of possible cosmologies is when there is just one universe? Is it all relativistic spacetimes? Just FRW models? Second, it is not apparent what the empirical significance of such probabilities is, since we observe a single universe evolving deterministically. The probabilities of statistical physics, for example, can be empirically confirmed since one generally has a multiplicity of identically-prepared systems for which one can obtain statistics and verify the theory's probabilistic assumptions (ch. 3). The usual interpretation of cosmological probabilities then is that they are just probabilities of initial conditions. But if they are, then how can such probabilities be justified? Is one to imagine some creator picking one of the possible universes as Penrose (1979) vividly illustrates? It is important to recognize that the success of problematizing uniformity and flatness in terms of likelihood depends crucially on successfully meeting these difficult conceptual challenges. Although I give a fuller exposition of the challenges facing cosmological probabilities and other measures of likelihood in chapter 4 to drive the point in more definitively, an honest assessment of the prospects of meeting these challenges, even just on the basis of the papers mentioned here, appears to be rather dim.

Nevertheless, if these problems are surmountable such that one can say that uniformity and flatness are indeed improbable in a sufficiently rigorous sense, then it is, I claim, at least possible to explain why they are problematic—intuitively it is because improbable initial conditions lack explanatory power (and predictive power). Improbable initial conditions might be seen as problematic because the probabilities tell us that those conditions probably do not obtain. But confidence in our observations and models transfers to the initial conditions, i.e. our present observations are relevant to our credences. Nevertheless, in the cosmological case there is little hope of verifying the conditions of the universe before recombination since radiation cannot travel freely to our telescopes from those times. The real worry then is that the initial conditions of the universe might easily have been otherwise (for all we can ascertain from observational evidence) than what the HBB model tells us; if they were indeed different, even slightly, then our HBB explanation of the present conditions fails (catastrophically—the universe collapses before we're around or stars never form). The problem is not that the HBB model cannot explain and predict phenomena, it is that the explanations and predictions it provides are not robust. A theory which provides a robust explanation of some phenomenon is better—has a stronger explanation I would say—than one which depends on improbable initial conditions to explain the same phenomenon.[28]

Some, in agreement with Earman (1995, 146) and Smeenk (2003, 239), may not be convinced that lacking explanatory power is all that problematic for a theory (exhibiting as evidence various examples from elsewhere in physics). Surely, at least when all other things are equal, a theory is preferable to another when the former explains more or better or more robustly than the latter. But when they are not, it is not so clear that the more powerful explanatory theory is always the better. For example, a cheap way to increase the explanatory power of a theory is to limit the space of possible models of the theory, say by assuming an additional constraint (Maudlin 2007a, 44). If one assumes the strong energy condition, then the only permissible expanding FRW models are decelerating. Hypothetical observations that suggest that the universe's expansion is decelerating would in this case be explained by the nature of matter in such a model, namely by the strong energy condition holding.[29] But adding additional constraints on a theory limits the descriptive possibilities of that

[28]There are surely many dimensions of explanatory power; I only claim that robustness of explanation is one of them.

[29]It may be objected that assuming a constraint does not make a theory more explanatory. Why, after all, is the strong energy condition true? Indeed there is always room for more explanation. But here one can make the connection to another area of physics, say a microphysical theory of matter, to ground the assumption.

theory, so increasing explanatory power in this way is usually not desirable. When those descriptive possibilities are not thought to obtain, however, there appears to be no loss by excluding them. Still, even in this case, it should be recognized that there are general costs to excluding descriptive possibilities (less unification of phenomena, lack of simplicity, etc.) which presumably must be balanced them against the benefit of increasing the theory's explanatory power.

In cosmology the uniqueness of the universe changes the calculus of balancing explanatory and descriptive power. There are no other observable universes, and therefore no empirical motivations to preserve descriptive power in cosmological models. It thus appears always favorable to pursue cosmologies with greater explanatory power.[30] Indeed one occasionally hears expressed the idea that the perfect cosmology would include no free parameters, would leave no cosmological feature accidental, etc.[31] A cosmological theory that severely lacks explanatory power by depending on special initial conditions for crucial explanations, especially one that is thought to be correct only at certain averaging scales and in certain energy regimes, can therefore legitimately be seen as problematic, in the sense that it is plausible to think that more powerful theories exist which maintain the empirical adequacy of the prior theory.

To conclude this section, it may worth recapping the foregoing argument in

[30]There are certainly theoretical motivations to preserve some degree of descriptive power in cosmology. Insofar as one thinks that relativistic spacetimes are the appropriate models of the universe (because, for example, one holds that gravity is the relevant "force" on cosmological scales), there is a strong presumption that GTR tells us precisely what the permissible cosmologies are. Yet insofar as one believes that GTR has limited ranges of applicability or unphysical models, intuitions on which cosmologies are realistic diverge from this presumption.

[31]"My guess is that there really is only one consistent theory of nature that has no free parameters at all" (Guth 1987). Einstein also famously conjectured as much: "I would like to state a theorem which at present can not be based upon anything more than upon a faith in the simplicity, i.e. intelligibility, of nature: there are no arbitrary constants...that is to say, nature is so constituted that it is possible logically to lay down such strongly determined laws that within these laws only rationally completely determined constants occur (not constants, therefore, whose numerical value could be changed without destroying the theory)" (Einstein 1959, 63).

brief. I claimed that the most intuitive analysis of the uniformity and flatness problems (as understood by cosmologists who have commented on them) depends on the initial conditions being special because they are improbable or unlikely; I argued furthermore that improbable and unlikely initial conditions are problematic because models with such conditions lack explanatory power. While this may not seem much about which to make ado—it could be said of nearly any theory that requires initial conditions—I have argued that there is a strong inclination in cosmology towards theories with greater explanatory power, more so than elsewhere in physics. Empirical considerations do not pull so strongly against explanatory power and towards the preservation of descriptive power in cosmological theory due to the uniqueness of the universe.

Now, the fact that these improbable initial conditions are also observationally unverifiable represents a significant theoretical risk to the HBB model, making the aforementioned inclination considerably stronger. As a matter of risk reduction in theory construction, theorists would much prefer to hedge their bets on a theory with greater explanatory resources (by introducing, say, some dynamical mechanism that drives the universe towards the observed conditions) and to reject the HBB explanation of uniformity and flatness. Yet, as shown in §2.2, particle horizons represent an obstacle to devising a cosmology with greater explanatory power, since the HBB's particle horizons preclude the dynamical explanations that would come with it. Thus one has the horizon problem, and its two manifestations: the uniformity problem and the flatness problem.

This particular argument hinges on the success of substantiating the attributions of probability in cosmology, a task that faces many seemingly insuperable challenges as noted in this section. Other explications of specialness in cosmological fine-tuning problems, such as a high degree of symmetry or dynamical instability, could perhaps be substituted, but they do not clearly problematize the HBB model's explanations of these problems alone. Thus I conclude that there is at present no problem free interpretation of the HBB model's fine-tuning.

2.4 Inflation

Inflationary theory, in its basic version which I explain briefly in §2.4.1, suggests that the universe underwent a phase of accelerated expansion in its very early in its history. The crucial realization, made originally by Guth, is that this one simple assumption, a phase of accelerating expansion where the strong-energy condition is violated, relaxes the horizon constraint, reverses the instability of flatness, and gives rise to the possibility (at least) of a dynamical explanation of uniformity. In this section I show how inflation is generally understood to solve the uniformity and flatness problems, i.e. by explicitly relaxing the horizon constraint of the HBB model (§2.4.2). I first give a preliminary explanation here before turning to details.

The essence of the horizon problem, as commonly understood, is that the existence of horizons precludes any local dynamical explanation of uniformity (and flatness). If the horizon constraint were relaxed, then one might expect that a dynamical explanation of some kind would become possible, especially if the entire observable universe were within a single horizon volume at a sufficiently early time (at least by recombination). So long as the strong energy condition is maintained, this is not possible—there is a past singularity in the HBB model limiting the age of the universe and therefore the distance radiation could travel before recombination. The inflationary approach is to propose a phase of the universe where the strong energy condition is violated which is smoothly "spliced into" the big bang story. A period of sufficient inflationary expansion (and, crucially, finding a way for it to end) then leads to a universe where the entire observable universe shares a common past.[32]

It is often remarked in expositions of inflation that a sufficient amount of inflation puts the constituents of the observable universe in causal contact such that a thermal equilibration process can lead to uniformity. But this cannot be how the CMB photons came to have the same temperature—the huge amount of expansion during inflation thins all particles out such that the post-inflation universe is essentially empty.[33] The inflationary solution to the uniformity problem actually depends crucially on a post-inflation phase of the universe known as reheating, where the supposed decay of the scalar field responsible for inflation repopulates the universe uniformly with particles (except for a spectrum of inhomogeneities due to "quantum fluctuations"). Reheating depends on quantum field theoretic considerations beyond the scope of this paper, but for present concerns it is worth remarking that by no means is there a completely adequate model of reheating at this time (Amin et al. 2015). Such a model is obviously crucial for the empirical adequacy of the inflationary proposal, even apart from it being required to solve the uniformity problem.

Inflation addresses the flatness problem, understood as a stability problem, more directly. Recall that one can rewrite the Friedman equation as

$$1 - \Omega(a) = \frac{-k}{(aH)^2}. \tag{2.8}$$

[32]Cosmologists often quote amounts of expansion in terms of "e-foldings"; an e-folding is a measure of expansion in base e given by the natural log of the ratio of scale factors, a later scale factor to an earlier one.

[33]This is, however, how inflationary theory solves the monopole problem. If magnetic monopoles are produced through a GUT phase transition, a sufficient amount of inflation will sweep them far enough away from one another that it becomes astronomically unlikely that one could have been observed.

With $1/(aH)^{-1}$ being driven towards zero by inflation (during inflation a increases greatly while H remains approximately constant), $\Omega(a)$ is driven to one, i.e. the critical density. For this reason it is often said that spatial flatness is an attractor solution in inflationary universes or that $\Omega \approx 1$ is a generic prediction of inflation (Mukhanov 2005, 233). The dynamical instability of flatness in the standard big bang universe is in any case reversed (flatness becomes a point of stability instead of metastability), for which reason it is claimed that inflation makes it "more likely" that the present universe should appear flat.

2.4.1 Classical Dynamics of Inflation

The standard way to define inflation is as a stage in the early universe where the Hubble radius is decreasing:[34]

$$\frac{\mathrm{d}}{\mathrm{d}t}\left(\frac{1}{aH}\right) < 0. \tag{2.9}$$

It is often described as a stage of accelerated expansion, and as a stage where "gravity acts repulsively" (Mukhanov 2005, 230).[35] Both of these follow directly from the above condition. It may be illuminating to demonstrate these facts. First, take the temporal derivative of the Hubble radius:

$$\frac{\mathrm{d}}{\mathrm{d}t}\left(\frac{1}{aH}\right) = -\frac{1}{(aH)^2}\ddot{a} < 0. \tag{2.10}$$

[34]The *Hubble radius* defines the Hubble sphere discussed in §1.4.2. There it was defined as the (physical) distance at which space is receding at the speed of light: $d_H = H^{-1}$. In co-moving coordinates this distance is $\chi_H = (aH)^{-1}$.

[35]It is somewhat misleading to describe accelerated expansion as gravity acting repulsively, insofar as normal matter still gravitates attractively. The sense in which gravity acts repulsively is how accelerated expansion affects the structure of spacetime.

It follows that $\ddot{a} > 0$. Thus the given condition, decreasing Hubble radius, implies accelerated expansion.

Next consider the Friedman acceleration equation (1.4) and impose the condition $\ddot{a} > 0$:

$$\frac{\ddot{a}}{a} = -\frac{4}{3}\pi(\rho + 3p) > 0. \tag{2.11}$$

It follows immediately from this equation that

$$p < -\frac{1}{3}\rho, \tag{2.12}$$

and, assuming the weak energy condition holds, that the stress-energy responsible for inflation has negative pressure. The negative pressure during inflation represents a violation of the strong energy condition, $p > -\frac{1}{3}\rho$. The strong energy condition captures, in a sense, that gravity is attractive in GTR (Malament 2012, 166). Normal matter obeys the condition and gravitates (attractively). So during an inflationary stage gravity acts repulsively.

Finally, it is sometimes remarked that inflation is a stage of exponential expansion. What is usually meant by exponential expansion is the amount of expansion, not necessarily how the expansion occurs. The standard measure of expansion is given in terms of *e-foldings*, which is the amount of time for the scale factor to grow by a factor of e. Depending on when inflation takes place different numbers of e-foldings are required to solve the horizon problem. If H is exactly constant, i.e. has the form of a cosmological constant, then the scale factor does indeed increase exponentially with time.

Inflation is generally supposed to occur in the very early universe, but the details depend on the particular inflationary model. The only constraint is that sometime after inflation ends the standard hot big bang picture, the part that relies on well-confirmed theory and observations, must begin.

2.4.2 Inflation as a Solution to the Horizon Problem

It may be of interest to see slightly more detail on how inflation specifically solves the horizon problem through accelerated expansion of the universe.[36] First, let us see how the horizon changes during different phases of the HBB universe (radiation domination and matter domination) in order to make a comparison to the inflationary universe. Matter-radiation equality occurs when the energy density of matter Ω_m equals the energy density of radiation Ω_r. This occurs at a scale factor of

$$a_{eq} = \frac{\Omega_r}{\Omega_m} \approx 3 \times 10^{-4}, \tag{2.13}$$

which is somewhat before recombination ($a_* \approx 9 \times 10^{-4}$).[37]

The increase in (co-moving) size of the particle horizon radius over a change in scale factor $\delta a = a_2 - a_1$ is given by the following expression

$$\chi_{\delta a} = \int_{a_1}^{a_2} \frac{\mathrm{d}a}{a^2 H}. \tag{2.14}$$

During matter domination the Hubble parameter H is inversely proportional to $a^{3/2}$.

[36](Earman 1995, 150-2) gives a similar calculation to the one I give below with which some readers may be familiar, but as far as I can tell there appear to be some significant mistakes. Although correct such calculations can be easily found in the cosmological literature, e.g. (Lesgourges 2006), for the reader's convenience I provide my own version here.

[37]Matter-dark energy equality occurred much more recently, at a scale factor of about $a_{eq2} \approx 0.6$. Therefore the universe has only expanded slightly since then (only about half an e-folding), so dark energy domination will be neglected in these calculations.

Thus we can set $H(a) = H_0 a^{-3/2}$ for any a during matter domination, where H_0 is the present Hubble parameter. Let us evaluate the contribution to the present particle horizon since recombination, by integrating from $a_1 = a_*$, the scale factor at recombination, to the present, where by definition the scale factor is $a_2 = 1$:

$$\chi_{(*,1)} = \frac{1}{H_0}\int_{a_*}^{1} \frac{\mathrm{d}a}{a^{1/2}} = \frac{2}{H_0}(1 - a_*^{1/2}). \tag{2.15}$$

The universe was matter-dominated before recombination, and radiation-dominated before matter-radiation equality. Data indicate that the redshift of recombination is approximately 1090, and the redshift of matter-radiation equality is 3250. These correspond to scale factors of 0.0009 and 0.0003 respectively. Thus only a small amount of expansion occurred during matter domination before recombination (the universe expanded three-fold), so it is a reasonable approximation to take matter-radiation equality to occur at recombination. Setting $H = a_*^{1/2} H_0/a^2$, we evaluate the integral from some initial time where the HBB model is assumed valid, $a_1 = a_i$, to recombination, $a_2 = a_*$:

$$\chi_{(0,*)} = \frac{1}{a_*^{1/2} H_0}\int_{a_i}^{a_*} \mathrm{d}a = \frac{1}{H_0}\frac{a_* - a_i}{a_*^{1/2}}. \tag{2.16}$$

Therefore the present particle horizon is given by $\chi_{(0,1)} = (2 - a_*^{1/2})/H_0$. The ratio of the particle horizon at recombination to the present particle horizon is

$$\frac{\chi_{(0,*)}}{\chi_{(0,1)}} = \frac{a_*^{1/2}}{2 - a_*^{1/2}} \approx 0.015, \tag{2.17}$$

where the term proportional to a_i has been dropped because it is negligible. In the

HBB universe almost all of the distance light could have traveled has been since recombination (not surprisingly, given what has been said about the horizon problem).

Let us formulate a condition for "causal contact." Imagine that "at the big bang" two massless, non-interacting particles with the same temperature are released in opposite directions. When recombination occurs they will each have traveled a co-moving distance of $\chi_{(0,*)}$. Imagine now that they are reflected so that they travel back towards one another. If they meet exactly at the present time or any time after, then they each must have traveled a distance at least equal to the present co-moving horizon radius, $\chi_{(*,1)}$. So the minimum condition for causal contact is

$$\chi_{(0,*)} \geq \chi_{(*,1)}. \tag{2.18}$$

In an FRW universe that undergoes a radiation- to matter-dominated transition the condition is equivalent to the condition that $a_* \geq 4/9$. The scale factor at recombination (and matter-radiation equality) is clearly much, much smaller than this value ($a_* = 9 \times 10^{-4}$). So we see that it is not possible that CMB photons all have a shared causal past; thus we have another presentation of the horizon problem.

Now we let us see how an inflationary stage can solve the problem. An inflationary stage's dynamics depend on the details of the inflationary model, but the Hubble parameter in simple models remains approximately constant (we choose an equation of state $w = -1$, the equation of state for a cosmological constant, for simplicity). The particle horizon grows as before, but radiation domination only begins after the end of inflation, at a_f:

$$\chi_{(*,1)} = \frac{2}{a_*^{1/2} H_0}(a_*^{1/2} - a_*) \qquad \chi_{(f,*)} = \frac{1}{a_*^{1/2} H_0}(a_* - a_f). \tag{2.19}$$

The particle horizon grows during inflation according to

$$\chi_{(i,f)} = \frac{1}{H_f} \int_{a_i}^{a_f} \frac{\mathrm{d}a}{a^2} = \frac{1}{H_f} \frac{a_f - a_i}{a_i a_f}, \tag{2.20}$$

where $H_f = a_*^{1/2} H_0 / a_f^2$. So

$$\chi_{(i,f)} = \frac{1}{a_*^{1/2} H_0} \frac{a_f}{a_i}(a_f - a_i). \tag{2.21}$$

The factor a_f/a_i gives the expansion during inflation, and, since it appears in the expression for the growth of the particle horizon, is ultimately responsible for solving the horizon problem. Combining the particle horizon during inflation and radiation domination gives

$$\chi_{(i,*)} = \frac{1}{a_*^{1/2} H_0} \left[a_f \left(\frac{a_f}{a_i} - 1 \right) + a_* \right], \tag{2.22}$$

where the term proportional to a_i has been dropped because it is negligible. Applying the causal contact condition defined above, the following inequality is easily derived (neglecting additional small terms):

$$\frac{a_f}{a_i} \geq 2 \frac{a_*^{1/2}}{a_f}. \tag{2.23}$$

This inequality demonstrates that the amount of inflationary expansion required to solve the horizon problem depends on the energy scale where inflation takes place.

Taking a_f as a_{gut}, one finds that around 63 e-foldings of inflation are required. If a_f is instead taken to be approximately 1 TeV, only around 33 e-foldings are required (since less expansion occurs during the shorter radiation-dominated stage).

2.4.3 Inflation Implemented as a Scalar Field

Difficulties with connecting inflationary theory with particle physics (Hawking et al. 1982; Guth and Weinberg 1983) led cosmologists to generalize inflation's implementation to an unknown scalar field, generically called the *inflaton* (Linde 1983).[38] Although an early attraction of inflationary theory was a supposed connection with the standard model of particle physics or extensions thereof, it was quickly determined that no known field could serve the role of the scalar field driving inflation and solve the horizon problem. Nevertheless, many cosmologists thought that inflation was too good to be wrong, so they took to positing the existence of the inflaton and left its firmer connection to more fundamental physics to the future.[39] I introduce the implementation of inflation as a scalar field to clarify the model building associated with inflationary theory, and also because some of the features will arise in ch. 4.

Consider first the action of a homogeneous scalar field ϕ minimally coupled to gravity and the Einstein-Hilbert action for gravity:

$$S = \int \mathrm{d}^4x\sqrt{-g}\left[\frac{1}{16\pi}R + \frac{1}{2}\dot{\phi} - V(\phi)\right]. \tag{2.24}$$

[38]There is, however, a proliferation of models of inflation, including inflationary models with multiple fields. See, for example, (Bassett et al. 2006).

[39]These connections are explored, for example, through various extensions to the standard model of particle physics (Lyth and Riotto 1999), quantum gravity (Tsujikawa et al. 2004), and string theory (Linde 2005).

From this action one derives the energy-momentum tensor T for ϕ:

$$T_{ab} = \dot{\phi}^2 \xi_a \xi_b - \left(\frac{1}{2}\dot{\phi}^2 + V(\phi)\right) g_{ab}. \tag{2.25}$$

Recall the energy-momentum tensor of the FRW model (1.2):

$$T_{ab} = (\rho + p)\xi_a \xi_b - p g_{ab}. \tag{2.26}$$

One can easily verify, by equating these two expressions for the energy-momentum tensors, that a homogeneous scalar field can be represented as a perfect fluid with

$$\rho = \frac{1}{2}\dot{\phi}^2 + V(\phi), \tag{2.27}$$

and

$$p = \frac{1}{2}\dot{\phi}^2 - V(\phi) \tag{2.28}$$

Recalling (2.12), we see that inflation occurs when the scalar field is in the following state:

$$\dot{\phi}^2 < V(\phi). \tag{2.29}$$

On the original version of inflation ("old inflation") proposed by Guth, one supposed that this condition was satisfied because the potential had a "false vacuum" where the field was trapped. Thus $V(\phi)$ was positive and $\dot{\phi} = 0$. To escape from the false vacuum the field had to quantum mechanically tunnel through the potential barrier that trapped it to a new vacuum (the "true vacuum") where the big bang story could take over. It was quickly shown however that this scenario could not work because for realistic fields the tunneling events would never cause the entire universe to coalesce into one "bubble"—the space between bubbles would continue to inflate rapidly.

The solution ("new inflation") hit upon by Linde (and also proposed by Albrecht and Steinhardt) was to let the field "slow roll" down an appropriate potential, such that the inflation condition was satisfied during the slow rolling phase. This allowed for a "graceful exit" to inflation, since there was no abrupt transition between the inflationary phase and the big bang phase. The inflaton rolls down the potential gradient causing the universe to inflate until it reaches the true vacuum at the bottom of the potential hill.

Single-field inflation models are determined by the shape of the scalar field's potential. Two parameters are typically used to describe the dynamical behavior of the universe in such models. These will be important in the following section, for they give rise to new fine-tuning problems in inflationary models. From the Friedman equations, (1.3) and (1.4), we can derive (assuming for simplicity that $k = 0$)

$$\frac{\ddot{a}}{a} = -\frac{4\pi}{3}(\rho + 3p) = H^2\left(1 - \frac{3\dot{\phi}^2}{2\rho}\right). \tag{2.30}$$

Define a new parameter, ϵ, as

$$\epsilon = \frac{3\dot{\phi}^2}{2\rho}. \tag{2.31}$$

It follows that

$$\epsilon = \frac{4\pi\dot{\phi}^2}{H^2} = -\frac{\dot{H}}{H^2}. \tag{2.32}$$

Recall that the condition $p < \rho/3$ discussed so far in this section is equivalent to the condition $\ddot{a}/a > 0$; it is also equivalent to $\epsilon < 1$ by these definitions and calculations.

One also requires inflation to last long enough to solve the horizon problem, so there is a second parameter, η, that quantifies how slowly the field is rolling:

$$\eta = -\frac{\ddot{\phi}}{H\dot{\phi}}. \tag{2.33}$$

One can recast these parameters as conditions on the potential directly (Liddle and Lyth 2000, 42). Define

$$\epsilon_\nu(\phi) = \frac{1}{6 \cdot 8\pi}\left(\frac{V'}{V}\right)^2, \tag{2.34}$$

and

$$\eta_\nu(\phi) = \frac{V''}{V}. \tag{2.35}$$

The conditions on the "potential" slow roll parameters is $\epsilon_\nu < 1$ and $|\eta_\nu| < 1$. These conditions are necessary conditions, but not sufficient conditions for inflation, since $\dot{\phi}$ could be such to prevent inflation (according to the previous slow roll conditions). To see how the potential slow roll parameters are necessary, we need the equations of motion of the inflaton. One equation of motion for a homogeneous scalar field in an expanding universe can be derived from the conservation equation $\nabla_a T^{ab} = 0$, giving

$$\ddot{\phi} + 3H\dot{\phi} + V' = 0, \tag{2.36}$$

where $V' = \nabla^a\phi\nabla_a V$. The Friedman equation ($k = 0$) becomes

$$H^2 = \frac{8\pi}{3}\left(\frac{1}{2}\dot{\phi}^2 + V\right). \tag{2.37}$$

In the slow roll approximation, the first term on the right hand side is negligible, so

$$H^2 \approx \frac{8\pi}{3} V. \tag{2.38}$$

In the slow roll approximation, the acceleration of the scalar field is negligible so we also have

$$3H\dot{\phi} \approx V'. \tag{2.39}$$

Subsituting these expressions into the expression for ϵ_ν, we see that

$$\epsilon_\nu = \frac{1}{6\pi}\left(\frac{8\pi \cdot 3H\dot{\phi}}{3H^2}\right)^2 = \frac{4\pi}{3}\frac{\dot{\phi}^2}{H^2} < 1. \tag{2.40}$$

Similarly

$$|\eta_\nu| = \left|\frac{V''}{V}\right| = \left|\frac{8\pi \dot{H}}{H^2}\right| = 8\pi|\epsilon| < 1. \tag{2.41}$$

2.5 Fine-Tuning: From the Hot Big Bang to Inflation

Having presented how inflation allegedly solves the HBB model's fine-tuning problems, I conclude this chapter by evaluating that claim. To review, the fine-tuning problems, e.g. the uniformity problem and the flatness problem, of the HBB model arise because it requires special initial conditions in order to explain present observations. So it is an explanatory problem. These special initial conditions suggest to physicists that a deeper explanation of features of the HBB model may be possible. In particular, the special initial conditions themselves may be explained, whereas in the HBB model they must simply be assumed. Standing in the way of some such further explanation is the horizon problem, which precludes a local dynamical explanation of uniformity and flatness.

Inflationary theory attempts to solve the fine-tuning problems by modifying the HBB model so as to avoid the horizon problem. By positing a period of accelerated expansion preceding the hot big bang universe, inflationary theory increases the size of the present particle horizon so that all of the physics of the HBB model, in particular recombination, occurs within a single causal patch. Interestingly, addressing the horizon problem in this way solves the flatness problem, insofar as accelerated expansion makes spatial flatness a dynamical attractor instead of dynamically unstable. Inflation also gives rise to the possibility of a causal explanation of uniformity through post-inflation reheating.

Has inflationary theory solved the HBB model's fine-tuning problems? The answer of course depends on what counts as a solution to a scientific problem, and a solution to fine-tuning problems in particular. In this section I survey different conditions one might place on a proposed solution to a fine-tuning problem such that it may be considered solved. Inflationary theory satisfies some of these but not all. A particular intuition that many seem to share is that a proposed solution to a fine-tuning problem fails when it suffers from fine-tuning problems of an equal magnitude to and worse than the original one. It is indeed widely recognized that inflationary theory suffers from fine-tuning problems of its own (Penrose 1989a; Brandenberger 2007). I argue that this is an unreasonable condition to place on a proposed solution, as is simply requiring that a solution address the fine-tuning problem directly, i.e. in an ad hoc way. A proposed solution need not solve all fine-tuning problems, but I maintain that it must accomplish something beyond merely solving the problem. For example, a reasonable expectation is that it suggests new empirical predictions which can be verified observationally; a more modest expectation is that the theory should merely "pull its own weight," by, say, offering a more fruitful framework for investigations.

2.5.1 Problems and Solutions

Although it can be made reasonably clear what the uniformity and flatness problems are, such that a proposal like inflation is taken as a solution to them, the difficulty in answering the philosophical question about what precisely is problematic about the conditions at the root of the problems suggests that it is difficult to say what would count as a solution to those problems. Without knowing well enough what the problem is, one cannot know begin to know what the solution to that problem is.

What exactly is a scientific problem? According to Nickles, "a problem consists of all the conditions or constraints on the solution plus the demand that the solution (an object satisfying the constraints) be found" (Nickles 1981, 109). The constraints on the solution, however, "characterize— in a sense 'describe' —the sought-for solution"(Nickles 1981, 109). This might seem to "put the cart before the horse," since to know the problem one must (partly) know the solution. Yet something like this must be correct if one is to solve the "learner's paradox," famously posed in Plato's *Meno*. So I will accept Nickles' intuitive definition of scientific problems at face value, since it does at least satisfy quite a number of desiderata one might have for such an account (Nickles 1981).

With this viewpoint on scientific problems, it is premature to think of inflation as a solution to the HBB model's fine-tuning problems until we understand just what the problems are, i.e. we understand *how* it solves them. This amounts to knowing what the relevant constraints on a solution are for the uniformity and flatness problems. I have identified various constraints, including perhaps the most important one: the horizon problem.[40]

The typical understanding of the uniformity and flatness problems as fine-tuning problems suggests further constraints on their solutions. The initial conditions that give rise to uniformity and flatness are in some sense special; intuitively, a solution to such a fine-tuning problem would make such conditions no longer special. If the problem is one of "large numbers," then the solution would be to make those numbers small (or "natural"). If the problem is that such conditions are improbable or unlikely, then the solution ought to make such conditions probable or likely. If the problem is one of idealization, then the solution should avoid those idealities as appropriate, e.g. making unstable dynamical behavior stable.

Problems are not identical with their solutions, by which I mean only that the constraints on a problem do not select a unique solution.[41] This is, of course, as it should be. There may be multiple solutions to a single problem. Inflation, for example is not the only possible solution to the HBB model's fine-tuning problems (if it is indeed a solution); few other solutions, if any, have gathered much attention so far however.

[40]On Nickles' view of scientific problems, the horizon problem is not a problem per se, but a constraint on a problem solution. As discussed previously, the existence of horizons may be considered a problem, but then the problem really amounts to the existence of singularities being problematic. See (Earman 1995) for a nice discussion of the "singularity problem."

[41]Whether a problem is identical to its solution *set* is another matter. On the "erotetic" approach to explanation (van Fraassen 1980; Bromberger 1992), the approach where explanations are understood as answers to "why" questions, a particular request for explanation—a why-question—is defined by its set of admissible answers (Hamblin 1958), for example. While it may also be argued that defining a problem by its constraints is in effect defining it by its solutions, since solutions must satisfy the constraints, it is not particularly important since in practice one never knows the complete space of possible solutions.

2.5.2 The Special Low Entropy State of the Universe

To clarify the problem situation further, it will be helpful to compare to the alleged fine-tuning problem mentioned previously, since it is more familiar to philosophers: the low entropy state of the universe seemingly "required" by the statistical mechanical explanation of thermodynamic behavior. Whether it is sensible to connect this foundational issue in thermodynamics to cosmology is an issue that requires some consideration (Earman 2006; Wallace forthcoming). At the very least, one must make sense of the role of gravity in calculating entropy (Wald 2006; Wallace 2010; Callender 2010, 2011a).

In any case, Price argues that there is a call to explain the low entropy "past state" in order to ground the Boltzmannian explanation of thermodynamic phenomena:

> I am not claiming that boundary conditions always call for explanation, but only that at boundaries, as elsewhere, our explanatory appetites are properly guided by the principles we already accept. On this basis, boundary conditions which appear abnormal in the light of what we take to be the laws do call for further explanationand this principle should be applied in a temporally unbiased way. Initial smoothness calls for explanation becausejust as much as would final smoothnessit conflicts so radically with our ordinary expectations as to how gravitating matter should behave. (Price 2002, 115)

Callender (2004b) argues on the contrary that "when one sees what an explanation of this state involves, it is not at all clear that it can or should be explained." There are several points worth commenting on in these excerpts for their relevance to the situation in cosmology. First of all, one can be led astray by characterizing the debate as over whether something *should* be explained. We do not categorically know in advance what can be humanly explained and what cannot. Popper famously claimed that "if there is such a thing as growing human knowledge, then we cannot anticipate today what we shall know only tomorrow" (Popper 2013). Popper's claim is too strong, since we can and do have *some* idea in advance what we shall know tomorrow—indeed by the learner's paradox we *must* have some idea. Yet a *demand* for explanation amounts to a claim that one categorically knows in advance what can or cannot be explained.

To be sure, Price does not characterize his view as a demand, only as a "call" for explanation.[42] So a fair interpretation of his claim is that we have a reasonable theoretical expectation of explaining the past state. Physicists do however occasionally speak in terms of explanatory demands (pertaining to some problem or another);[43] typically these calls for explanations derive from their expectations on future theory too or are in the interest of promoting a particular theory. Such demands are rhetorical, or even polemic in nature, and are part of the complicated, thoroughly social, process of scientific discovery. Price appears to take his view as contributing to this process of scientific discovery—"I think that most physicists intuitions, like mine, will be that there is an important explanatory project here, that there is likely to be something interesting to find" (Price 2004)—which may be somewhat immodest given the influence of philosophical problems on contemporary physics (and a quick glance at citations of his papers on the subject).

What one can extract from Price's view is some intuitions that drive problematizing the past state. From the excerpt above, they are that the initial conditions are "abnormal" and that such conditions conflict with expectations about how gravitating matter "should" behave. The first intuition is not unlike those expressed by cosmolo-

[42] "As to whether we're right, of course, only time itself will tellbut only if we try!" (Price 2004)

[43] As a random, recent example: "One of the things such a theory [a dual formulation of QFT] should clarify is an a priori understanding of what the analytic structure of scattering amplitudes should be" (Arkani-Hamed et al. 2010, 71).

gists about the unlikelihood of flat, uniform initial conditions.[44] If unlikelihood were enough to make initial conditions special, then the past state would appear to be just as much a target for explanation as the special initial conditions at the heart of the flatness and uniformity problems.

There is a significant mistake in thinking that unlikelihood of the past state is emblematic of a new problem in the foundations of thermodynamics however (Callender 2004a, 210). If a constraint on the solution is that the past state should be probable, or at least more probable, then any solution that satisfies this constraint will contradict the statistical mechanical explanation for which it is meant to provide a foundation. The Boltzmannian measure assigns low entropy states like the past state a small probability *in order* to explain the time-directed approach to large measure equilibrium states. If low entropy states are accorded a high probability, "then we are not using the Boltzmann explanation anymore" (Callender 2004a, 210). If the original problem is explaining the time-directedness of thermodynamics phenomena, then accepting a low entropy initial condition solves this problem, and that is all there is to say about that from the perspective of statistical mechanics.

Viewed as an explanatory problem for cosmology, on the other hand, the

[44]The second, however, is rather suspect. If one's intuitions are only shaped by everyday experience with gravity and by Newtonian gravitation theory, then indeed one would find the past state quite abnormal (if not "contradictory"—cf. (Norton 1992; Malament 1995)). There has, however, been quite a lot of physics developed beyond Newtonian gravitation theory: observations and theory (GTR) suggest, for example, the existence of a cosmological constant that drives spacetime to expand at an accelerated rate (quite contrary to our mundane intuitions about gravity). With intuitions tutored by GTR, the past state is plausibly abnormal as well, much in the way that the initial state's flatness and uniformity seem abnormal. But most theoretical physicists are wary about how far one can trust GTR: "one might think that we already have an approximately local description of gravitational physics the long-distance effective theory of GR coupled to matter! However we know that this description can be drastically misleading" (Arkani-Hamed et al. 2010, 71). If there is an "important explanatory project" here, then it is already being pursued by many physicists investigating theories of quantum gravity and the relevant intuitions ought to be tutored through exposure to GTR, QFT, string theory, etc. Cf. (Callender 2004b, 246).

improbability of the low entropy past state may yet be an appropriate target for explanation. Some physicists indeed take this problem seriously (Penrose 1979; Carroll 2010). To make good on the claim that the low entropy state is improbable, however, one ought to be able to make sense of entropy and probability in this context well enough to formulate some criteria for success, i.e. constraints on possible solutions. Callender (2004a) offers surveys four possible approaches to solving "special initial conditions" problems: (a) providing a dynamical explanation, (b) providing a non-dynamical explanation, (c) eliminating the initial probability distribution, and (d) providing some manner of anthropic explanation. By (a), (b), and (d) special initial conditions would be "explained," while by (c) they would be "explained away."

Physicists generally seek dynamical explanations, and especially so in cosmology. What one means by "dynamical explanation" probably demands some elaboration, but some deterministic law-based explanation as in the Hempel model suffices for my purposes. Non-dynamical explanations are considerably less familiar from a physics point of view, yet have attracted some attention in cosmology, particularly in the context of quantum cosmology. The idea is to provide some physically motivated constraint on initial conditions that makes what seemed to be a special initial condition more natural. Sometimes such a theory goes under the name "theory of initial conditions" (TIC). Examples include Penrose's Weyl curvature hypothesis (Penrose 1979) and the Hawking-Hartle "no boundary condition" (Hawking and Hartle 1983).[45]

The idea behind "eliminating the initial probability distribution" is really a special case of (a) and only applies when fine-tuning problems are considered to be "improbability" or "likelihood" problems. I have argued already that this is not

[45]Callender describes his view, taking the past state as a law of nature, as of this kind, although he notes that "this move does not so much explain the past hypothesis as state that it does not need explanation because it is nomic" (Callender 2004a, 211).

necessarily the best way to understand fine-tuning problems, and make good on this claim more fully in the ch. 4. The notion of eliminating dependence on initial conditions has however been a strong motivation for many physicists. For example, Sciama urged that we must “find some way of eliminating the need for an initial condition to be specified. Only then will the universe be subject to the rule of theory...This provides us with a criterion so compelling that the theory of the universe which best conforms to it is almost certain to be right” (Sciama 2009, 2). Sciama was making this claim in the context of the major debate in cosmology of last century, that between the steady state model and the HBB model (Kragh 1996), but it is a powerful motivation to many proponents of inflation. In garden-variety theories of inflation there are still initial conditions and moreover new fine-tuning problems, as discussed below.[46]

Finally there is the idea that anthropic arguments could explain fine-tuning. There is a large and ever growing literature on anthropic reasoning in both physics and philosophy, with reactions among commentators ranging from disgust to unbridled enthusiasm. A modest view is Earman’s: “On one hand, anthropic reasoning does not deliver on the promise of a new methodology of scientific explanation; but neither, on the other hand, is the AP an unscientific idea which has no place in physics or cosmology” (Earman 1987). Explaining fine-tuning by an anthropic argument is acknowledged to require a multiverse; since I confine myself to discussing a single universe, I will not discuss anthropics further.

[46]Eternal inflation might have been thought to signal a return to the steady state universe-like idea of an absence of initial conditions, but the singularity theorem of (Borde et al. 2003) suggests otherwise. Nevertheless theorists have advocated eternal inflation for presenting the attractive possibility of providing simple, natural initial conditions (Aguirre and Gratton 2002, 2003; Carroll and Chen 2004, 2005).

2.5.3 Fine-Tuning Problems in Inflationary Cosmology

I now turn to evaluating inflation as a solution to the HBB model's fine-tuning problems, in the narrow sense of evaluating whether inflation *solves* these problems. As I have been emphasizing, how one understands the fine-tuning problems (in terms of constraints) will determine the relevant success criteria of proposed solutions to the problems. Since problems in science are never fully and explicitly characterized, one should expect that the evaluation of solutions to vague problems is subject to some variability. Nevertheless, I claim that the best interpretation of the HBB fine-tuning problems suggest that classical inflation does indeed solve the flatness problem, but only provides the possibility of solving the uniformity problem.[47]

The horizon problem is not so much a problem in and of itself, but a constraint on solutions to the uniformity and flatness problems. Conceiving inflation as merely a stage of accelerated expansion in the early universe, as is common, one sees that the inflationary approach attacks the fine-tuning problems by directly addressing a common constraint. Accelerated expansion in the early universe (pre-HBB universe) increases the size of the particle horizon by the ratio of scale factors post-inflation to pre-inflation (2.21)-(2.23).

Suppose, for the moment, that the horizon problem was not merely a constraint on solving the HBB model's fine-tuning problems, but was a problem in its own right. Does inflation solve the horizon problem? Obviously it does, in essentially an ad hoc way, which may strike one as somehow unsatisfying. Lakatos (1970) famously argues

[47]By classical inflation I mean its representation in GTR as defined above: an stage of accelerating expansion in the universe (pre-Big Bang). Quantum mechanical considerations must be invoked to understand the transition from inflation to the hot big bang via reheating. The HBB universe must be the exit state of inflation in order for inflation to solve the uniformity problem. Simply diluting the contents of the universe through exponential expansion is obviously not a solution to the uniformity problem, although the universe would indeed be uniform afterwards.

against ad hoc solutions to scientific problems, arguing that ad hoc maneuvers are a sign of a degenerating research program. Laudan claims, however, that "an ad hoc theory is preferable to its non-ad hoc predecessor" (Laudan 1978, 116), by way of solving more problems than its predecessor. In the case at hand, if the addition of an inflationary stage pre-big bang in order to solve the horizon problem only were to achieve the relaxation of the horizon constraint sufficiently and affected nothing else, it would be quite difficult to see how the maneuver accomplished anything of scientific value. But we need not consider such a possibility, since the sort of theory modifications made in science usually have effects that range far beyond their intended consequences. This is clearly the case with inflation, since the inflationary epoch not only increases the particle horizon an exponential amount but, for example, by doing so greatly dilutes the density of energy in space (the cosmological no hair result mentioned in §1.3.4)—an ad hoc maneuver leads to non-ad hoc theoretical consequences.

Consider now the uniformity problem, recognizing that the horizon problem is a constraint on the former problem's solution. Uniformity in the CMB's temperature suggests some sort of a equilibrium explanation, but the existence of particle horizons in the early universe precludes its possibility. By the inflationary maneuver the constraint of horizons is avoided, but at the cost of this possibility of an equilibrium explanation. Inflation essentially *empties* the universe of particles—which sounds a lot like a failed solution to the uniformity problem. But one cannot overlook the key contribution of inflation, which is that it showed a way to make the origins of the big bang an object of scientific study.[48] Although there may be perhaps a theoretical

[48] "That's what is so impressive—when you can actually push back your ignorance to a point where you can address a question that you didn't think was in the bounds of science at all." (Lightman and Brawer 1990, 352).

expectation that uniformity is achieved through a process of equilibration of the universe, there is no need to think of such a process as a constraint on the solution of the uniformity problem. So, classical inflation indeed does not solve the uniformity problem alone, but it is clearly a step in that direction since it affords the possibility of giving a dynamical explanation of the universe's uniformity.

Let us turn to the flatness problem. If the flatness problem is assumed to concern the dynamical instability of flatness in FRW spacetimes (by assuming a stability constraint on cosmological models), then inflation solves the problem by reversing the instability of flatness. It is rather surprising that classical inflation addresses, merely by positing a stage of accelerated expansion, the horizon constraint and the "stability constraint" simultaneously. While the horizon constraint is addressed in an essentially ad hoc way, the stability constraint appears to be solved by serendipity. There is surely something of a "common cause" here: Horizons exist in part because matter in the HBB universe obeys the strong energy condition, but matter obeying the strong condition also causes flatness to be dynamically unstable in FRW spacetimes. Defined as a stage of accelerating expansion, inflation implies that the strong energy condition is violated, from which it follows that an inflationary stage increases the particle horizon, and also that flatness is a point of dynamical stability during inflation.

Does this unexpected explanatory coherence have any philosophical significance in theory development? This fact about GTR and FRW models was there all along, but was overlooked (perhaps because nothing that failed to satisfy the strong energy condition was supposed to exist) until Guth stumbled upon it. Without having investigated the issue in advance, the discovery that both problems are connected together is a strong suggestion to pursue the solution that solves both. Such a discovery mirrors in a way the unexpected empirical confirmation of novel predictions of a theory, the latter an often acknowledged virtue of a theory. Dawid (2013) has recently argued that unexpected explanatory coherence can be an important facet of assessing a theory's viability and confirmation, particularly in the case of theories like string theory or in the case of cosmology where empirical confirmation often remains out of current, foreseeable, or even potential reach—"it gives the impression that physicists are on the right track" (Dawid 2013, 45). But an argument from unexpected explanatory coherence alone can hardly be conclusive—there may be, for example, "so far insufficiently understood theoretical interconnections at a more fundamental level, of which the theory in question is just one exemplification among many others" (Dawid 2013, 46), or the correct theory may solve the problems in independent ways, such that the coherence previously found was ultimately irrelevant and misleading.

Differently interpreted, however, the uniformity and flatness problems are not so clearly solved by inflation (even assuming that reheating is successful). In particular, when one views fine-tuning problems as "likelihood" problems, there is no convincing proof that inflation has solved them at all. Not only is it doubtful whether any precise sense of probability can be applied to the space of cosmologies (Schiffrin and Wald 2012; Curiel 2015), it is also not clear that inflation would itself be in any sense "likely" on such a measure.[49] For anyone interested in the justification of inflation as part of the standard model of cosmology, these probability issues are among the most pressing—at least if one understands HBB fine-tuning as a probability problem and

[49](Ijjas et al. 2013, §3), for example, use the sort of naive "indifference measure" found in many inflation papers to argue that the Planck satellite's results disfavor inflation. Such claims should of course be taken with a grain of salt, given that such "measures" are ill-defined and poorly justified. Also, I have confined my discussion to single universe cosmology, but it is worth noting that measure problems continue to trouble inflation in multiverse scenarios as well. See (Smeenk 2014) for a detailed analysis of measure problems in the multiverse.

inflation a solution thereof.

Moreover Penrose criticizes inflation (Penrose 1989a) (in part) for failing to solve the problem discussed above: the special initial condition embodied by the low entropy initial state. "I think it's all coupled to the second law of thermodynamics problem. The horizon problem is a minor part of that problem. It's not the big problem and, in a sense, that's what I always thought" (Lightman and Brawer 1990, 427). He argues that if the second law of thermodynamics is true, then the states of the universe making up inflation all must have lower entropy than the initial HBB state one is trying to explain, and therefore must be less likely by statistical mechanical arguments. Addressing Penrose's argument deserves more attention than I can give here, but for the purposes of this section there are two points to make. The first is that requiring inflation to solve the low entropy problem is an unfair demand. Inflation can certainly be a successful without solving all the problems that an envisaged theory of quantum gravity should solve. Secondly, if Penrose's argument is sound, it is a strong challenge to the prospects of inflation solving the HBB model's fine-tuning problems understood as likelihood problems.[50]

In the task of evaluating a theory's success at solving a problem, one should specify reasonable success conditions. So let us suppose for the sake of argument that the project of addressing the probability problems of inflationary cosmology remains tenable despite the serious challenges it faces, and consider briefly what it would take to solve the fine-tuning problems interpreted as likelihood problems. There are various ways one might approach the likelihood problem, and therefore what counts

[50] Another criticism along these lines is the failure of inflation to explain the cosmological constant. According to this argument, the true vacuum in inflationary models must be fine-tuned to account for the observed value of the cosmological constant, due to the many orders of magnitude larger vacuum energy obtained through quantum corrections.

as a solution to the problem. One possibility is that special initial conditions must be "explained away," in the sense that fine-tuning must be completely eliminated. Another is that fine-tuning is not a bump to push under the rug: to explain fine-tuning one must remove or lessen the fine-tuning in question without introducing further fine-tuning of an equal or greater magnitude. Finally, one might hold that solving fine-tuning only requires making the fine-tuned values more likely, without consideration of other possible fine-tunings. Although inflation has been criticized for not meeting any of these conditions, I claim that we should only demand that the last condition by met by any putative solution to a fine-tuning problem understood as a likelihood problem.

Evidently many physicists view the elimination of initial conditions as something of an ultimate goal of physics. Be that as it may, inflation certainly does not succeed at realizing this implausible goal. In whichever guise inflation takes, initial conditions of some kind or another are required (Vachaspati and Trodden 1999; Brandenberger 2007; Carroll forthcoming).As I discussed in §2.4.3, single-field inflation models possess potentials that can be characterized by two numbers: ϵ_ν, which for inflation to occur, must satisfy the condition $\epsilon_\nu < 1$ (the potential must have a region which causes accelerated expansion of the universe); η_ν, which for inflation to last long enough to solve the horizon problem must satisfy a further condition $|\eta_\nu| < 1$ (Liddle and Lyth 2000). In a rough way one can think of ϵ being a condition on "how high" the field starts on the potential and η being a condition on "how far away" from the minimum of the potential (the "true vacuum") the field starts on the potential. Only potentials of certain shapes satisfy these conditions; even with a valid potential, however, the field must have special initial conditions in order for inflation to occur, and occur sufficiently long to solve the fine-tuning problems (Turok 2002, 3457). Although other inflationary models differ from the single-field case, they too invariably have initial conditions that require varying amounts of fine-tuning.

If a solution to the fine-tuning problems must not introduce further fine-tuning of the order of the original fine-tuning, then inflation appears to fail here as well, since, as just pointed out, inflationary models have significant fine-tunings of their own. Yet this standard is an unreasonable demand to place on a solution. One should expect that solving one problem can introduce others. If solving one problem does introduce new ones, this does not mean that the original problem was not solved. It just means that the solution comes at a price. In any case, it is not necessarily a mark against a solution for introducing new fine-tuning problems, since these other problems may be more tractable and have natural solutions of their own.[51] More importantly, solving a fine-tuning problem, even if it introduces new fine-tuned parameters of its own, may contribute to a progressive research program by offering new predictions for empirical test, or by exhibiting unexpected explanatory connections and other non-empirical signs of progress.

I claim that the only reasonable standard of success is the weakest one mentioned: that solutions must make the fine-tuned conditions (sufficiently) more likely. In a transparent sense this is just what it means to solve a fine-tuning problem, understood as a likelihood problem. If the problem is that something is unlikely, the solution to that problem makes that thing more likely. It must be stressed, however, that showing that inflation does indeed make uniformity and flatness more likely faces the many challenges of incorporating probability into cosmology as discussed in §3.

[51]For example, Smeenk observes that " inflation exchanges the degrees of freedom associated with the spacetime geometry of the initial state for the properties of a field (or fields) driving an inflationary stage. This exchange has obvious advantages if physics can place tighter constraints on the relevant fields than on the initial state of the universe" (Smeenk 2013, 634).

The difficulties are both philosophical and technical, and of such a degree that, even with a weak condition on success, it seems that cosmologists can only have slim hopes of successfully solving the fine-tuning problems so understood. Thus, insofar as the HBB model's fine-tuning problems are probability problems, the likely verdict, for not only now but the foreseeable future, is that inflationary theory fails to solve them, even on the weakest reasonable standard of success.

To sum up this section, we have seen that inflation solves the horizon problem directly by relaxing the horizon constraint, but essentially in an ad hoc way. Whether this counts in the favor of inflationary theory depends on how one views ad hoc solutions to conceptual problems. The flatness problem is indeed solved by inflation when considered as a dynamical instability problem as well. Although it is often said that inflation solves the uniformity problem too, it plainly does not without some way to transition from the post-inflation vacuum to the empirically verified epochs of the hot big bang universe. Since the details of reheating remain unclear, it can hardly be said that inflation has definitively solved the uniformity problem, although it can certainly be claimed that inflation has afforded cosmologists with the possibility of providing a physical solution. If one interprets fine-tuning problems as probability problems, then at least one requirement, for which I have argued, is that inflation should decrease the fine-tuning of the HBB model. But as yet there is not even a justifiable way to assign cosmological probabilities, much less show that inflation is probable and increases the probability of uniformity and flatness.Evidently many physicists view the elimination of initial conditions as something of an ultimate goal of physics. Be that as it may, inflation certainly does not succeed at realizing this goal. In whichever guise it takes initial conditions of some kind or another are required.

Recall the discussion of slow-roll parameters in the section. Single-field inflation models possess potentials that can be characterized by two numbers: ϵ_ν, which for inflation to occur must satisfy the condition $\epsilon_\nu < 1$, i.e. the potential must have a region which causes accelerated expansion of the universe; $\eta_n u$, which for inflation to last long enough to solve the horizon problem must satisfy a further condition $|\eta_\nu| < 1$. In a rough way one can think of ϵ being a condition on "how high" the field starts on the potential and η being a condition on how far away from the minimum of the potential (the "true vacuum") the field starts on the potential. Only potentials of certain shapes satisfy these conditions; even with a valid potential, however, the field must have special initial conditions in order for inflation to occur, and occur sufficiently long to solve the fine-tuning problems (Turok 2002, 3457). Although other inflationary models differ from the single-field case, they too have initial conditions that require varying amounts of fine-tuning.

Chapter 2 includes material that is published in McCoy, Casey. "Does inflation solve the hot big bang model's fine-tuning problems?" *Studies in History and Philosophy of Modern Physics* 51: (2015) 23-36. The dissertation author was the sole investigator and author of this paper.

Chapter 3

Likelihood in Physics

3.1 Introduction

In chapter 2 I argued that the best characterization of cosmologists' complaints against the HBB model's special initial conditions is that these conditions are in some sense unlikely, and, since unlikely conditions plausibly lack explanatory power, are consequently objectionable. Insofar as one tries to understand the uniformity and flatness problems as a matter of likelihoods, there are certain specific claims in the corresponding fine-tuning arguments that demand support (Ellis 1988; Coule 1995). First, it must be shown that the uniform and flat FRW spacetimes underlying the HBB model are unlikely in some physically significant sense. Second, it must be shown that inflating cosmologies are generically uniform and flat. Third, it must be shown that inflating cosmologies themselves are generic, or at least are more likely than the special HBB spacetimes. (I argued (2.5.3) that the latter condition is the most reasonable standard to demand of inflationary theory—although it is the weakest among those surveyed there—as a solution to fine-tuning problems.)

All of these tasks presuppose that there is a justified way of assessing the likelihoods of cosmological models (Gibbons et al. 1987; Hawking and Page 1988). The present chapter therefore begins an investigation into whether some notion of likelihood, in particular probability, can be made precise and justified in the context of cosmology, such that it could be used to support the fine-tuning arguments discussed in the previous chapter.

There are, on the face of it, various approaches one could take for supplying the required support. Cosmologists have generally favored those that are similar to the putatively successful application of likelihoods in statistical mechanics. 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.

In chapter 4 I will argue that this presupposition is incorrect. My central claim there is that the justification and interpretation of cosmological probabilities 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 probabilistic arguments used to argue for an inflationary solution to the fine-tuning problems of the HBB model (a claim I repeatedly made in the previous chapter). My argument depends on the correct elucidation of the implementation, interpretation, and justification of likelihoods in statistical mechanics; this chapter serves to supply this foundation. Although the concept of likelihood may be formally implemented in various ways, I will concentrate on likelihoods implemented in measure theoretic terms, especially as probabilities, since most work on cosmological probabilities does so.

The plan of this chapter is as follows. The next section (§3.2) attends to how probability is implemented in classical physics by reviewing the application of probability in statistical mechanics in my favored formal framework. I also introduce classical field theory briefly in order to show how the statistical mechanical approach to probabilities becomes complicated for systems with an infinity of degrees of freedom (such as the spacetimes of GTR). The final section (§3.3) then covers the interpretation and justification of probability measures in the two main approaches to classical statistical mechanics in the foundations literature, the Gibbsian version and the Boltzmannian version. In particular I propose and defend a particular way of characterizing the interpretation of statistical mechanics that clearly separates these two versions based on how one understands randomness in the theory.

3.2 The Implementation of Probability in Physics

As I noted in chapter 1, observational limitations and the uniqueness of the universe represent significant obstacles to obtaining scientific knowledge of our universe at large. The analysis of systems possessing a large number of degrees of freedom face similarly significant epistemic challenges. Despite such challenges, the use of measure theory and associated statistical methods has met with striking success in describing and explaining the physical phenomena associated with such systems. These statistical methods play a central role in cosmology as well, for example in big bang nucleosynthesis and CMB anisotropy calculations, although such cases merely represent a typical application to subsystems of the universe of non-equilibrium statistical mechanics.

Many cosmologists would like to extend the use of such statistical techniques more broadly to the space of possible cosmologies:[1]

> 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 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 may appear on the face of it somewhat loose. It is clear anyway that one cannot count on any serious empirical confirmation of probability assignments to cosmologies due to the 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)

[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).

[2] Even in the case of the multiverse, such a measure would not be directly accessible by empirical means. Thus its choice and justification must come on other, theoretical grounds. The choice of an appropriate measure is rather more important in a multiverse theory like eternal inflation. The lack of a well-motivated measure for eternal inflation is widely known as *the* "measure problem". Although I will not be considering eternal inflation since it depends on quantum mechanical considerations beyond the scope of this thesis, the topics treated in this chapter are for the most part fully relevant to eternal inflation's measure problem. There has been only limited philosophical work on the topic of eternal inflation. I note (Smeenk 2014), which addresses the measure problem in eternal inflation directly, focusing primarily on the problematic use of anthropic reasoning to yield predictions in the theory.

Naturally enough, 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.

Indeed, the classic way to introduce probability in physics is to make use of the canonical Liouville measure on the hamiltonian phase space representation of classical dynamical systems.[3] This section of the chapter reviews the construction of measure structures in classical statistical mechanics.[4] The "naturalness" of the various mathematical structures defined on phase space will be important for the subsequent discussion of justifying likelihood measures, so in reviewing this material I direct particular attention to how these structures are "picked out" by the geometry of phase space.

Before diving into the mathematical details, it is worth making a few preliminary and general remarks on the topic of classical systems, both of particles and of fields. One specifies a classical physical theory or model of a physical system, minimally, by its space of (dynamically) possible "histories" or "motions."[5] One usually thinks of a classical physical system as having instantaneous states, where each state is individuated by its (idealized) observable consequences, and of a continuous temporal sequence of such states as constituting a history. In many cases of physical interest

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[3]Probability also enters physics in the analysis of measurement errors and also in a distinct way in quantum mechanics (Galavotti 2001). All of these ways of introducing probability find their way into cosmology, but the kinds of measures relevant here are introduced in analogy to the introduction of probability measures in classical statistical mechanics.

[4]The following presentation of hamiltonian dynamics makes abundant use of the formalism of symplectic geometry (Abraham and Marsden 1978; Arnold 1989; Souriau 1997). A nice presentation of classical dynamics in the symplectic approach can be found in the philosophical literature in (Butterfield 2007). A less technical, but clear and accurate, discussion of the main elements can be found in (Belot 2003).

[5]See also (Curiel 2014) for a discussion of such "abstract classical systems" in further detail.

the possible states are independent of time and can be associated with the possible histories such that there is a bijection between the space of states and the space of histories. I will call the space of histories the "covariant phase space". The "phase space" familiar from hamiltonian particle mechanics is actually the space of possible states, not the covariant phase space/space of histories. Nevertheless, in hamiltonian mechanics one has precisely such a one-to-one map onto the set of possible histories, so one may treat the phase space/space of states as the full covariant phase space/space of histories.

To introduce probability into a classical physical theory one supplies the (covariant) phase space with the structure of a probability space by specifying the measurable sets (which must form a σ-algebra) and defining a probability measure on them. One would of course like to have some justifiable way to do this. Classical theories are often represented mathematically using differentiable manifolds; such theories inherit measurable sets from the coordinate charts used to define their manifolds.[6] Classical theories can be usefully formulated on fiber bundles (which are, of course, differentiable manifolds), the geometry of which can be used to determine a natural measure in certain cases (for example when the phase space of the system is a cotangent bundle). In some cases such a measure is naturally a probability measure. Thus, in the best case the geometry of the classical theory furnishes one with all the tools one needs to construct a probability space—without making any additional choices.

There are however many steps where this natural construction of a probability measure can break down. My approach in the remainder of this section is to exhibit the

[6] To wit, for an n-dimensional manifold, by pulling back the Lebesgue-measurable sets on $\mathbf{R}^n$ to the n-dimensional manifold via the coordinate map of the charts.

basic prototype for the construction of a natural probability measure, viz. the measures of classical statistical mechanics, and then to show how this can be generalized to classical field theories like GTR. As the relativistic spacetimes of GTR do not have a finite number of degrees of freedom, their configuration spaces are therefore not finite-dimensional manifolds. The appropriate analog to the cotangent bundle for classical field theories is a different geometrical object, the dual jet bundle. I provide some cursory details on the analogous "multisymplectic" approach to classical field theories (which relies on dual jet bundles rather than cotangent bundles) to give a fuller perspective on the appropriate notion of phase space in GTR. However cosmologists generally focus attention on the simpler FRW spacetimes, which have a finite number of degrees of freedom, in order to avoid the greater complications introduced by systems with an infinity of degrees of freedom. Nevertheless it is important to mention the geometry of field theories since the limitations of such approaches become clearer in this more general theoretical setting (a setting which would be necessary to model the full space of cosmologies permitted by GTR).

3.2.1 Measure in Classical Particle and Statistical Mechanics

I consider first the implementation of classical (deterministic) particle mechanics in the hamiltonian framework. For a system (of particles) with n degrees of freedom (n finite), let the configuration space Q of the system of particles be a connected n-dimensional differentiable manifold, each point of which specifies the spatial location (in Euclidean 3-space) of the collection of particles.[7] In hamiltonian mechanics the

[7] An n-dimensional differentiable manifold M (with $n \geq 1$) is a non-empty set M, and a maximal set $\mathcal{C}$ of compatible n-charts which cover M and possess the Hausdorff property (Malament 2012, 2). I take having a topologically connected configuration space to be part of what it means to be a physical system.

$2n$-dimensional cotangent bundle T^*Q of Q is called the phase space—it is, again, the space of states.[8] The hamiltonian phase space is a classical system's state space for a time-independent hamiltonian function. I will call points (covectors) in this phase space "microstates" of the system (in contrast to "macrostates" which will be introduced below).

The cotangent bundle T^*Q, being a fiber bundle, has a (surjective) "canonical" projection map $\pi : T^*Q \to Q$. This map is defined such that, for any covector α_q in T^*_qQ, α_q maps to q, where T^*_qQ is the cotangent space at point q in Q—in other words π projects any covector α_q in a point q's cotangent space down to that point. One also says that T^*_qQ is the fiber over point q in base manifold Q, since $T^*_qQ = \pi^{-1}(q)$, where $\pi^{-1}(q)$ is the inverse image set of q.[9]

The map π gives rise to a well-defined pushforward map $\pi_* : T(T^*Q) \to TQ$ (sometimes called the differential $\mathrm{d}\pi$) onto the tangent bundle of Q.[10] This map also picks out a special one-form θ on phase space sometimes called the "tautological" one-form. It is constructed as follows. Consider the maps $\theta_{\alpha_q} : T_{\alpha_q}(T^*Q) \to \mathbf{R}$ defined by

$$\theta_{\alpha_q} = \alpha_q \circ \pi_*|_{T_{\alpha_q}(T^*Q)}. \tag{3.2}$$

Clearly these are linear maps on the tangent spaces $T_{\alpha_q}(T^*Q)$ (i.e. are covectors in $T^*_{\alpha_q}(T^*Q)$), since any element α_q of the cotangent bundle T^*Q is a covector, i.e. a

[8] For example, a classical system of N particles moving without external constraints in 3-dimensional Euclidean space would thus have $3N$ degrees of freedom and a $6N$-dimensional phase space.

[9] The fibers are isomorphic to one another in a fiber bundle. Thus one usually sees a "typical fiber" specified in the definition of a fiber bundle, to which all the fibers of points in the base manifold are isomorphic.

[10] The push-forward of a tangent vector X in $T(T^*Q)$, i.e. (π_*X), is defined to be

$$\pi_*(X)(f) = X(\pi^*(f)) = X(f \circ \pi), \tag{3.1}$$

where f is a real-valued function on Q.

linear map $\alpha_q : T_qQ \to \mathbf{R}$. Thus the maps θ_{α_q} are elements of the cotangent bundle of phase space $T^*(T^*Q)$. The tautological one-form θ is then defined as a section of $T^*(T^*Q)$ over T^*Q, namely the "canonical" map $\theta : T^*Q \to T^*(T^*Q)$ defined by $\theta(\alpha_q) = \theta_{\alpha_q}$.

The cotangent bundle T^*Q possesses a natural symplectic structure ω, i.e. a closed, non-degenerate two-form, obtained from the tautological one-form θ, namely by taking the exterior derivative of the tautological one-form on T^*Q; in symbols $\omega = -\mathrm{d}\theta$. So merely by specifying the configuration space Q of a classical system one straightforwardly obtains a natural symplectic manifold (T^*Q, ω). The physical significance of this symplectic manifold depends on the specification of an appropriate function—the hamiltonian—on that manifold, which justifies considering the cotangent bundle of configuration space as the system's phase space.[11]

The constructions just presented are admittedly rather abstract, so it may please the reader to see these objects represented in more familiar coordinate expressions. Any covector α_q in the cotangent space T^*_qQ of point q in Q (and therefore in T^*Q) may be labeled by the "canonically conjugate" local coordinates (q^i, p_i), $i = 1 \ldots n$, where the q^i are the configuration space (base) coordinates of point q and the p_i are the fiber coordinates determined by expressing α_q in the covector basis defined by

[11] The terminology used to track the "naturalness" of various structures defined along the way to the symplectic manifold (T^*Q, ω) is unfortunately multifarious and is used in inconsistent ways in practice. There are reasons for the various conventional choices among "tautological", "canonical", and "natural", but the fact that the meaning of these terms varies by context makes it difficult to evaluate whether some mathematical structure's use in a physical application is justified on the grounds of it being "picked out" as special without explicitly showing that this is the case. The intent of presenting the construction explicitly is to demonstrate that each structure is indeed "picked out" in a similar way and uniquely once a configuration space is chosen.

exterior derivatives of the base coordinates:[12]

$$\alpha_q = \sum_{i=1}^{n} p_i \mathrm{d}q^i. \tag{3.3}$$

In these local coordinates, the tautological one-form θ at α_q is also expressed as

$$\theta_{(q,p)} = \sum_{i=1}^{n} p_i \mathrm{d}q^i. \tag{3.4}$$

The symplectic structure ω is given in the canonical coordinates by taking the exterior derivative of the tautological one-form:

$$\omega_{(q,p)} = \sum_{i=1}^{n} \mathrm{d}q^i \wedge \mathrm{d}p_i. \tag{3.5}$$

With ω in hand, the sense in which coordinates q^i and p_i are canonically conjugate may be made clear. Namely, canonical coordinates are those coordinates that satisfy

$$\omega(\partial_{q^i}, \partial_{p_j}) = \delta_i^j; \qquad \omega(\partial_{q^i}, \partial_{q^j}) = 0; \qquad \omega(\partial_{p_i}, \partial_{p_j}) = 0. \tag{3.6}$$

These relations may be expressed in terms of the usual Poisson bracket algebra of functions on phase space. Given two functions f, g on the cotangent bundle, their

[12] This is guaranteed by Darboux's theorem. In case the abuse of notation is confusing, the following hopefully should clarify matters. The ith coordinate map on open subset U of Q for coordinate chart (φ, U) is denoted $q^i : U \to \mathbf{R}, i = 1 \ldots n$. This map is obtained by composing the coordinate projection $q^i : \mathbf{R}^n \to \mathbf{R}$, given by $q^i(q_1, \ldots q_i, \ldots q_n) = q_i$, on the coordinate chart $\varphi : U \to \mathbf{R}^n$, i.e. $q^i(q) = (q^i \circ \varphi)(q)$, for q in U. The coordinate q_i, a real number, is equal to $q^i(q)$. The exterior derivatives of these maps, written $\mathrm{d}q^i$, provide a basis for the cotangent spaces T_q^*Q of all points q in U. Any one-form α_q in T_q^*Q can be expressed as a linear combination in this basis, with coefficients p_i. Thus, each element α of T^*Q is uniquely specified by the coordinates p_i and q_i, where we define another map $q_i : T^*Q \to \mathbf{R}$ by $q_i = q_i(\alpha_q) = (q_i \circ \pi)(\alpha_q)$.

Poisson bracket $\{f, g\}$ is

$$\{f, g\} = \omega(X_f, X_g), \tag{3.7}$$

where X_f (and similarly X_g) is the unique vector fields determined by

$$\omega(X_f, \cdot) = \mathrm{d}f. \tag{3.8}$$

The familiar "canonical commutation relations" for the canonical coordinates can then be written using Poisson brackets:

$$\{q^i, p_j\} = \delta_i^j; \qquad \{q^i, q^j\} = 0; \qquad \{p_i, p_j\} = 0. \tag{3.9}$$

Having detailed the naturally symplectic geometry of the cotangent bundle, I now turn to the derivation of the natural measure on phase space, the Liouville measure. (I henceforth refer to the cotangent bundle T^*Q as the phase space Γ for brevity and in anticipation of the action of the hamiltonian dynamics justifying this terminology.) One obtains the canonical (Liouville) volume form $\mathrm{d}\Omega$ associated with Γ by taking the top exterior product of ω (normalized so that the second equality holds for canonical coordinates (q^i, p_i)):

$$\mathrm{d}\Omega = \frac{(-1)^{n(n-1)/2}}{n!} \wedge^n \omega = \mathrm{d}^n q \, \mathrm{d}^n p. \tag{3.10}$$

This volume form induces a measure μ, the Liouville measure, on measurable subsets

U (in the Lebesgue sense) of Γ:[13]

$$U \mapsto \int_U \mathrm{d}\Omega. \tag{3.11}$$

One thereby obtains the natural measure space $(\Gamma, \mathcal{L}, \mu)$, where $\mathcal{L}$ is the set of (Lebesgue) measurable subsets of Γ, given only a choice of the configuration space Q.

Since the Liouville measure is σ-finite, a probability measure may be defined that is "equivalent" to it, where the sense of equivalence is absolute continuity: the two measures assign measure zero to the same sets of $\mathcal{L}$. Let $\rho : \Gamma \to \mathbf{R}^+$ be a non-vanishing map such that $\int_\Gamma \rho \, \mathrm{d}\Omega = 1$. Define probability measure μ_ρ such that for all U in $\mathcal{L}$

$$U \mapsto \int_U \rho \, \mathrm{d}\Omega. \tag{3.12}$$

Then one has the probability space $(\Gamma, \mathcal{L}, \mu_\rho)$, where all one has done is "weighted" the Liouville measure in a way that makes the total measure one, viz. by the probability distribution ρ. If the Liouville measure μ on Γ is finite, then there is a canonical ρ, namely a constant function that normalizes the total measure of Γ to one, i.e. $\rho = 1/\int_\Gamma \mathrm{d}\Omega$. If the measure μ is not finite however, then clearly there is no canonical map ρ that makes μ_ρ into a probability measure. One has to make some further choice of a specific ρ.

Probability measures find one of their most important physical applications in classical statistical mechanics. In the usual Gibbsian approach to statistical mechanics one says that physical states ("statistical states") of the system are probability

[13] One might also call this a "Lebesgue measure" on Γ. The measurable subsets U are "Lebesgue measurable sets" in the following sense: for all charts $\varphi : O \to \mathbf{R}^{2n}$ on 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 only in this sense that a measure on a manifold is a Lebesgue measure. Note that when the phase space is $\mathbf{R}^{2n}$, the Liouville measure just is the Lebesgue measure.

measures associated with the phase space Γ, rather than the points of phase space (as in classical particle mechanics).[14] (Standard classical mechanics can even be recovered in this framework by generalizing it slightly, namely by allowing "Dirac measures" as statistical states, i.e. Dirac delta functions centered on a classical microphysical state.) Similarly, in the Boltzmannian picture of statistical mechanics one in effect uses probability measures to represent macroscopic states of the system, which states characterize the system in addition to the system's possessed microstate. In either case we can make a distinction between microstates (phase space points) and macrostates (probability measures associated with phase space).

For both particle mechanics and statistical mechanics we need a means of describing the time evolution of states, so the next topic is the hamiltonian dynamics of classical systems. Classical dynamics on phase space is implemented by the hamiltonian, a function on phase space $H : \Gamma \to \mathbf{R}$. Since ω gives an isomorphism between the tangent space $T_{\alpha_q}\Gamma$ and cotangent space $T^*_{\alpha_q}\Gamma$ at every point α_q in phase space (Butterfield 2007, 11), there is a correspondence between one-forms and vector fields on Γ. Thus there is a correspondence between the differential form $\mathrm{d}H$ and a unique vector field X_H, which is called the time evolution or hamiltonian vector field:[15]

$$\mathrm{d}H = \omega(\cdot, X_H). \tag{3.13}$$

The hamiltonian vector field X_H generates a local phase flow along the integral curves of X_H. This hamiltonian flow is a one-parameter group of symplectomorphisms

[14]This more geometrical point of view on statistical mechanics is presented lucidly in (Souriau 1997, ch. 4) and (Agricola and Friedrich 2002, ch. 8), among other places.

[15]This is the fundamental equation of motion ("Hamilton's equation") for a classical dynamical system in geometrical terms. The non-degeneracy of ω insures that there is a unique vector field X_H associated with the hamiltonian H. The anti-symmetry of ω insures the "conservation of energy," since $\mathrm{d}H = \omega(X_H, X_H) = 0$. Since ω is closed, it too is preserved under time evolution.

(symplectic structure-preserving maps) $\{\phi_H^t : U \to \phi_H^t[U]\}_{t\in I}$, where U is an open subset of Γ, I is an open interval of $\mathbf{R}$ containing 0, and $\phi_H^t(\alpha_q) = \phi_H(t, \alpha_q) = \gamma(t)$ for integral curve $\gamma : I \to \Gamma$ of X_H with initial value α_q. A choice of H on a phase space Γ thereby defines a (local) dynamical system (I, U, ϕ_H).[16] That the hamiltonian flow is a one-parameter group of symplectomorphisms and not just a one-parameter group of diffeomorphisms of Γ may be seen by taking the Lie derivative along the hamiltonian vector field generating the phase flow, written $\pounds_{X_H}$. The Lie derivative of the symplectic form ω is given by Cartan's formula. Using the fact that ω and $\mathrm{d}H$ are closed, we have

$$\pounds_{X_H}\omega = \mathrm{d}\omega(X_H, \cdot) + \mathrm{d}(\omega(X_H, \cdot)) = 0. \tag{3.14}$$

Thus the symplectic form is preserved under local phase flows.[17] It follows immediately that the Liouville measure is invariant under local phase flows as well, a fact which is known as Liouville's theorem.[18]

In canonical coordinates the hamiltonian vector field is

$$X_H = \sum_{i=1}^{n} \left(\frac{\partial H}{\partial p_i} \frac{\partial}{\partial q^i} - \frac{\partial H}{\partial q^i} \frac{\partial}{\partial p_i} \right). \tag{3.16}$$

Using this equation applied to the coordinate maps q^i, p_i along X_H's integral curves

[16] When the hamiltonian vector field is complete, i.e. the domain of t is the complete real line, then it generates a one-parameter group of symplectomorphisms of the entire phase space Γ.

[17] Note also that the hamiltonian itself is preserved under local phase flows:

$$\pounds_{X_H} H = \mathrm{d}\omega(X_H, X_H) = 0. \tag{3.15}$$

[18] Poincaré's Recurrence Theorem is easily proved thence, insofar as Liouville's theorem holds for the entire phase space.

γ, with γ parameterized by time t, yields

$$\dot{q}^i = \frac{\mathrm{d}}{\mathrm{d}t}(q^i \circ \gamma)(t) = X_H(q^i) = \sum_{i=1}^{n} \left(\frac{\partial H}{\partial p_j} \frac{\partial q^i}{\partial q^j} - \frac{\partial H}{\partial q^j} \frac{\partial q^i}{\partial p_j} \right) = \frac{\partial H}{\partial p_i}, \tag{3.17}$$

and

$$\dot{p}_i = \frac{\mathrm{d}}{\mathrm{d}t}(p_i \circ \gamma)(t) = X_H(p_i) = \sum_{i=1}^{n} \left(\frac{\partial H}{\partial p_j} \frac{\partial p_i}{\partial q^j} - \frac{\partial H}{\partial q^j} \frac{\partial p_i}{\partial p_j} \right) = -\frac{\partial H}{\partial q^i}, \tag{3.18}$$

which are of course the familiar form of Hamilton's equations:

$$\dot{q}^i = \frac{\partial H}{\partial p_i} \qquad \dot{p}^i = -\frac{\partial H}{\partial q^i} \tag{3.19}$$

In classical mechanics one is interested in the evolution of points in phase space, i.e. microstates. The hamiltonian formalism above implements this evolution via the hamiltonian function H's phase flow as just described. In statistical mechanics one is correspondingly interested in the evolution of probability distributions in phase space, i.e. macrostates. The hamiltonian formalism can be applied straightforwardly to this case as well, using the group action of the hamiltonian flow on the statistical state. The statistical state is a probability measure μ_ρ which is defined, again, on the measurable sets U as follows:

$$U \mapsto \int_U \rho \, \mathrm{d}\Omega. \tag{3.20}$$

This probability distribution is not necessarily invariant under the hamiltonian flow ϕ_H^t, since the probability distribution ρ is not necessarily so invariant. The time dependence of ρ is obtained by taking the Poisson bracket of ρ with H:

$$\{\rho, H\} = X_H \, \mathrm{d}\rho = X_H(\rho) = \frac{\mathrm{d}}{\mathrm{d}t}(\rho \circ \gamma)(t), \tag{3.21}$$

where γ is the integral curve of X_H, parameterized by t, that contains the point at which the Poisson bracket is being evaluated. Writing the right-hand side of (21) as $\dot{\rho}$, we have that the time dependence of the probability distribution is given by Liouville's equation:

$$\dot{\rho} = \{\rho, H\}. \tag{3.22}$$

Clearly, if the Poisson bracket of H with ρ is zero, then the measure μ_ρ is invariant, i.e. $\rho \circ \phi_H^{-t} = \rho$; in this case μ_ρ is said to be a state of statistical equilibrium.

3.2.2 Covariant and Canonical Classical Field Theory

The foregoing demonstrates how probability and measure can be introduced in a fully natural way using the geometry of classical physical systems (when represented as hamiltonian systems). This approach requires that the system have a finite number of degrees of freedom, since the manifolds used have finite dimension. Many physical systems are however described by an infinity of degrees of freedom, in particular those described by classical field theories, among which GTR is one especially notable example for my purposes.[19] The geometry of classical field theories requires a somewhat different and more complicated approach, which I survey briefly here for its relevance to assessing the application of probability to cosmology.

A field theory describes physical fields and their evolution on the background

[19]The existence of constraints on phase space, as one finds in GTR, complicates matters further. I defer discussion of GTR as a constrained hamiltonian system (as a gauge theory) until 4.4.

of spacetime (Giachetta et al. 2009). The notion of a configuration space for a field theory is naturally formulated in terms of fiber bundles, which in the special case of a field theory are called field bundles. A field bundle is a map $F \to E \to B$, where F is the typical fiber (the space of field values), E is the field bundle proper (a bundle of fields over spacetime), and B is the base manifold (spacetime). The second map is a projection π from the field bundle to the base manifold. What makes the bundle $F \to E \to B$ a fiber bundle is the existence of a local trivialization Z: a bundle with projection map π is said to possess a local trivialization if, for each point p in B, there exists a neighborhood U of p such that there exists a map $\zeta : \pi^{-1}[U] \to U \times F$, where $\pi^{-1}[U]$ is the set of elements of E which project to points in U and, for all p in U, $\zeta(\pi^{-1}(p)) = (p, f)$. The collection of maps ζ is called a local trivialization (Isham 1999). It is in this sense that a fiber bundle is said to have the structure of a trivial bundle, viz. $F \to B \times F \to B$, locally, and that π projects from a field value at a spacetime point to the spacetime point where that field value is realized.

A field configuration is a section $B \to E$ of the bundle, and therefore the configuration space, which will be denoted $\mathscr{E}$, is naturally construed to be the space of sections of the field bundle. This space, i.e. $\mathscr{E}$, is clearly infinite-dimensional. Taking this space as the system's configuration space and implementing the field dynamics on it is the standard approach to field theories, called the "canonical" approach. Yet for relativistic spacetimes covariance must be broken in this approach in order to describe the field dynamics.[20] An alternative approach, the "covariant" approach, takes E as the system's covariant configuration space, which in cases of physical interest will be finite-dimensional.[21]

[20]The standard developments of the lagrangian and hamiltonian formulations of GTR follow this canonical approach. See, for example, (Wald 1984, Appendix E).

[21]"There are two major approaches to the symplectic description of classical fields: the covariant

Some simple cases may help clarify the idea of formulating field theories as fiber bundles. Take, for example, a (real) scalar field theory on Minkowski spacetime, i.e. the four-dimensional manifold $\mathbf{R}^4$ equipped with the Minkowski metric η. Since the field takes a value at each spacetime point, and the value of the field is a (real) number, the field bundle is the trivial fiber bundle $\mathbf{R} \to \mathbf{R}^4 \times \mathbf{R} \to \mathbf{R}^4$, i.e. the trivial bundle with projection map $\pi(x, \phi(x)) = x$ for x in $\mathbf{R}^4$ and $\phi(x)$ a real number. Sections ϕ of this bundle are real-valued functions on $\mathbf{R}^4$, and $C^\infty(\mathbf{R}^4 \times \mathbf{R})$ is the (infinite-dimensional) configuration space. Similarly, the field bundle of a vector field theory on spacetime $\mathbf{R}^4$ is the tangent bundle $T\mathbf{R}^4$, sections of which are vector fields on $\mathbf{R}^4$.

I previously described the configuration space of a system of classical particles with n degrees of freedom, n finite, somewhat differently, namely as an n-dimensional manifold Q. Consider the special case where the system has no external constraints. The degrees of freedom of each particle are then just spatial degrees of freedom—each particle is located somewhere in Euclidean space. For a system of N particles, the configuration space Q is simply the manifold $\mathbf{R}^{3N}$. It turns out that such a system of classical particles can be represented as a field theory too. The base manifold is not spacetime, as in the previous examples, but just $\mathbf{R}$ (which represents time).[22] The "field" bundle for this "field" theory is the trivial bundle $\mathbf{R} \times Q \to \mathbf{R}$ (with

(Hamilton-Poincaré-Cartan) formalism and the instantaneous (or 3+1) framework. In the latter approach dynamics is described in terms of the *infinite*-dimensional space of fields at a given instant of *time*, whereas in the former dynamics is phrased in the context of the *finite*-dimensional space of fields at a given event in *spacetime*. Each formalism has its own advantages and goes hand-in-hand with the other, but the covariant approach...is generally regarded as the more fundamental" (Gotay 1991, 205). (Gotay et al. 2004) is a good source on the covariant formalism and for further references.

[22] If spacetime were the base manifold, the "fiber" would essentially be the set $\{0, 1\}$, i.e. represent whether the particle is there or not. But this set obviously cannot be made into a differentiable manifold. Apparently Newton himself conceived of representing particle theory as something like a field theory—see, for example, (Redhead 1982, 60) and (Stein 1970).

projection onto the first term). The instantaneous "field" configurations are particle configurations in Q, and sections of the field bundle are system histories (sets of particle trajectories), yielding a field configuration space $C^\infty(\mathbf{R} \times Q)$.

With the notion of a field bundle on the table, we can now ask if it is possible to derive a natural measure given some field bundle E. One might immediately think to take the cotangent bundle T^*E of E, which will, as before, have a natural symplectic structure ω. Indeed, in the case of classical particle mechanics (reformulated as a field theory as in the previous paragraph), this is perfectly sensible—$T^*E = T^*\mathbf{R} \times T^*Q$ for $E = \mathbf{R} \times Q$—since the dynamical equations of particle mechanics are ordinary differential equations.[23]

The dynamical equations of general field theories are partial differential equations, however, and so the "natural symplectic structure" ω has no manifest physical significance. One may however generalize the symplectic approach of classical particle mechanics into a "multisymplectic" approach built on the technical apparatus of jet bundles (Gotay et al. 2004; Giachetta et al. 2009) in order to gain geometric insight into the physics of fields. In this approach the field theory analog of the tangent bundle TE, on which one formulates lagrangian particle mechanics, is the (first) jet bundle J^1E; the field theory analog of the cotangent bundle T^*E is the (first) dual jet bundle J^1E^*; the field theory analog of the symplectic 2-form ω is the canonical $(n+2)$-form, where $n+1$ is the dimension of B for field bundle $E \to B$.

Since the formalism of jet bundles and multisymplectic manifolds (which is the pair composed of the dual jet bundle and the canonical $(n+2)$ form) is somewhat technically intricate and unfamiliar in the philosophical literature, I will not discuss

[23]In this case dynamics is implemented on the "extended phase space" formulation of classical mechanics (Arnold 1989, ch. 9), which may be reduced to the treatment given above for time-independent hamiltonians.

its details further; in any case, doing so would not add anything essential to the present discussion. This formalism has in any case emerged in recent decades as the appropriate mathematical grounding for lagrangian field theory and covariant field theory in the hamiltonian formulation, with much of the interest stimulated by interest in GTR. As it turns out, though, GTR is not the easiest classical theory to formulate in multisymplectic terms. Many theories of interest can be so formulated, especially when one only requires the first jet bundle.

A few important conclusions can now be drawn from the foregoing discussion. The most salient is that the procedure for defining a natural measure from the previous section does not work in general with field theories formulated as fiber bundles over spacetime. If one follows the canonical approach, i.e. takes the configuration space to be $\mathscr{E}$, then the phase space $T^*\mathscr{E}$ is infinite-dimensional and it is not so clear what one could mean by the infinitary "top exterior product" of the symplectic structure ω.[24] Relatedly, and more significantly, it is a well-known theorem that there is no analog to the Lebesgue measure on an infinite dimensional (Banach) space.[25] This alone scuppers any hope of using the Liouville measure approach for finite-dimensional systems in the infinite-dimensional case.

If one instead follows the covariant approach, i.e. takes the configuration space to be E, then the phase space $J^k E^*$ at least is finite-dimensional and comes equipped with a "natural" multi-symplectic form ω.[26] But one cannot in general use the

[24] "In the finite-dimensional case, one can get the conserved canonical (Liouville) measure by taking the top exterior product $\wedge^n \omega$ of the conserved symplectic form ω. But in the infinite-dimensional case this does not make sense" (Schiffrin and Wald 2012, 1).

[25] Thus, for example, the path measures written for Feynman path integrals are not Lebesgue measures nor rigorously defined; rather they are short-hand for rigorously defined finite-dimensional approximations (Cartier and DeWitt-Morette 2006).

[26] Authors take various approaches to defining multisymplectic spaces. See, for example, (Gotay et al. 2004; Giachetta et al. 2009) and references therein. The differences do not matter for the basic point being made here.

multi-symplectic form to define a volume form simply by taking exterior powers, since the dimension of the phase space is not in general divisible by the degree of the multisymplectic form.[27] This is certainly not to say that some notion of volume cannot be devised, but one requires additional structure beyond what one gets for free by formulating the theory symplectically. In short, the upshot of this subsection is that the well-worn strategy presented in the previous subsection is not so obviously available for general field theories. Serious mathematical work is needed to extend it to the infinite-dimensional case, if it is even possible.

3.3 The Justification and Interpretation of Statistical Mechanical Probabilities

With the formal groundwork laid, I now discuss the justification and interpretation of statistical mechanical probabilities. I will be using the term "interpretation" in a way that differs somewhat from the usual notion of interpretation discussed by philosophers of probability. I do so because a restricted sense of interpretation will be sufficient for assessing the application of probability in cosmology. I distinguish this sense of interpretation from the usual one hence by referring to the latter (non-prejudicially) as "so-called interpretation." Since my appropriation of the term "interpretation" will be somewhat unfamiliar to philosophers, I will explain and justify it immediately below. After that, I use this notion of interpretation as a way to cleanly distinguish three versions of statistical mechanics that differ with respect to

[27] Let $E \to B$ be a fiber bundle, with the dimension of B being $n+1$. Then the bundle's canonical multisymplectic form ω is an $(n+2)$-form. The dimension of E, and hence the dimension of J^kE^*, depends on the kind of field. Only when the dimension of J^kE^* is divisible by $(n+2)$ would one be able to take exterior products of the form to derive a volume form.

the interpretation (in my sense) of probabilities in the theory, two of which I suggest are related to the Gibbsian and Boltzmannian approaches to statistical mechanics found in the philosophical literature. Finally I briefly discuss the justification of probability measures in statistical mechanics and the notion of typicality.

3.3.1 Kinds of Interpretation

The so-called interpretation of statistical mechanical probabilities has received considerable attention by philosophers, as evidenced by a number of recent lengthy book treatments and featuring prominently in recent reviews (von Plato 1994; Guttmann 1999; Uffink 2007; Frigg 2008b; Myrvold 2016). Much of the recent work has involved taking some so-called interpretation of probability "off the shelf," so to speak, and seeing how it fits with some approach to statistical mechanical (Clark 2001; Lavis 2001; van Lith 2001; Emch 2005; Winsberg 2008).[28] On the whole, the conclusions of these various studies have not been encouraging. Certainly some accounts of probability receive more attention than others, yet the debate continues and there remains a distinct lack of real consensus on the physical, metaphysical, and conceptual significance of statistical mechanical probabilities.

One might therefore wonder whether probability in statistical mechanics needs some such "interpretation" at all.[29] Probability is of course a common target of interpretive rumination, as is evident from the quantity of work on so-called interpretations of probability. Indeed there seems to be a widespread concern that probability *in*

[28]These well-known off-the-shelf interpretations, e.g. the logical interpretation, the propensity interpretation, the frequency interpretation, etc., are cataloged and described in numerous books, e.g. (Gillies 2000; Galavotti 2005; Mellor 2005).

[29]I take the cue from Curiel (2009), who questions whether GTR needs an interpretation, as alleged in, e.g. (Belot 1996).

particular is a philosophically problematic concept, and a "philosophical interpretation" is necessary to solve its problems. For example, Guttmann, in his book about interpreting probability in statistical mechanics, seems to appropriate this general concern about probability and carry it over directly to statistical mechanics:

> As long as we do not have a good interpretation of our probabilistic concepts or at least a justification of the probabilistic arguments that appear in statistical mechanics, we have no way of understanding why certain types of events are considered improbable.(Guttmann 1999, 3)

So, clearly, Guttman is at least of the opinion that probability in statistical mechanics does need an interpretation, and that the needed interpretation would be crucial for understanding how probability figures in the theory. This is probably the consensus position in the philosophy of statistical mechanics.

Yet it seems to me that such concerns do not wear their object so clearly on their sleeves, especially when couched in terms of the rather subjective concept of "understanding." What understanding do we lack? What ill would a so-called interpretation cure? And just what distinguishes probability from other theoretical concepts in this regard anyway? Do Guttman's worries apply to any other theoretical concept? Do particles, for example, need an interpretation in classical particle mechanics? It seems so, since (pace Guttman)

> as long as we do not have a good interpretation of our particulate concepts or at least a justification of the particulate arguments that appear in particle mechanics, we have no way of understanding why certain types of events are considered particle-like;

or (perhaps better)

> as long as we do not have a good interpretation of our symplectic concepts or at least a justification of the symplectic arguments that appear in analytic mechanics, we have no way of understanding why certain types of processes are considered symplectomorphisms.

No doubt a million philosophical projects may be launched by anxieties such as these!

Granting, however, that these worries do suggest explanatory projects of some kind, one might wonder what the nature of these projects is meant to be. The term "explanation" is surely multifarious in its meanings, so there are likely many plausible ways of characterizing the nature of these projects. Curiel (2009) suggests a handful: One way, he suggests, is establishing a new, more fundamental theoretical basis that explains the appearance of the relevant concept in the "higher level" theory—what Curiel calls a "metalinguistic" interpretation. Another, similar way, is to exhibit a more fundamental metaphysical or conceptual basis for the employment of the concept (which would also fall under Curiel's metalinguistic category of interpretations). This latter approach is, I think, probably the one Guttman and others who voice such concerns have in mind, since they understand the project as one of giving an "interpretation of probability," where these interpretations are understood to be the products of conceptual analysis.[30]

If interpreting probability in statistical mechanics (taken in the sense of conceptual analysis or otherwise) is truly a worthy project, then it should at least be clear enough what an interpretation entails. Von Plato suggests some reasonable desiderata:

> An interpretation of probability should give meaning to [probabilities]. It should tell us what they can be used for. It should tell us what the things, events or whatever, are, to which probability numbers are attached. Most importantly, an interpretation should tell us how we arrive at these probability numbers. What is the particular way of determining probabilities in a given kind of application? (von Plato 1989, 427)

[30] As Hájek explains in the introduction to his encyclopedia article on the topic,

> "Interpreting probability" is a commonly used but misleading characterization of a worthy enterprise. The so-called 'interpretations of probability" would be better called "analyses of various concepts of probability," and "interpreting probability" is the task of providing such analyses." (Hájek 2012)

These desiderata (save the first one) are sufficiently accounted for, I claim, when one does not attempt to characterize probability as such and in general, but from the point of view of the theory where that concept is employed, in which case probability is at some level just a "theoretical concept" (Sklar 1979; Sober 2010). This kind of interpretation therefore does play an important role in physical theory by providing the "concrete interpretation" of a theory (Curiel 2009, 46), i.e. the specification of the conditions of use of theoretical concepts.[31,32]

As a simple example of giving a concrete interpretation, consider particle mechanics, described in some detail above in §3.2. The description of a classical system, when represented on phase space Γ, is specified completely by a trajectory in phase space, $\gamma : I \subset \mathbf{R} \to \Gamma$. We assume all of the physical degrees of freedom are included in the phase space description, so that any observable $\mathcal{O}$ is a real-valued function on phase space: $\mathcal{O} : \Gamma \to \mathbf{R}$. The trajectory γ is determined by the hamiltonian H and an initial condition $\gamma_0 = \gamma(0)$ in Γ, i.e. a complete description of a classical system is (Γ, H, γ_0) (including structures implicit in the given structures, such as the symplectic form ω and observables on Γ).[33] Given the collection of objects (Γ, H, γ_0) one obtains a complete description of the classical system—a curve in phase

[31] As such, interpretive principles are of a piece with such things as coordinative definitions, correspondence rules, bridge laws, etc. Curiel (2009, 46) explains that one can essentially learn the details of such an interpretation from good textbooks on the subject. As a good example of making such principles explicit, see the interpretive principles of general relativity given in (Malament 2012, 120-1).

[32] Curiel maintains that there are also other senses of interpretation which lend themselves to potentially fruitful interpretive projects as well (Curiel 2009, 46). For example, one might wish to extend the semantics of a theory to incorporate a concept which one suspects of having physical significance in the theory (determinism, locality, etc.). One may also find that the essential use of a concept, one whose interpretation is not sufficiently fixed in the interpretive principles of the theory, to derive the empirical content of some theory suggests that the theory is incomplete in a way that requires further interpretive work.'

[33] Cf. the definition of a relativistic spacetime as (M, g) in (Malament 2012, 119), where the connection ∇ is implicit in the definition, since there is a unique connection compatible with a given spacetime metric g on the manifold M.

space, from which all relevant physical observables can be determined. There is nothing in the collection that is not doing some "physical work" in producing curves in phase space. Also, since all of the physics one wishes to describe as classical particles is describable in terms of such curves in phase space, it follows that all of the physics is describable in terms of the mathematics given by (Γ, H, γ_0).[34]

Any interpretive principles for classical particle mechanics must therefore include statements associating physical degrees of freedom with dimensions of phase space (e.g. spatial positions and momenta), persisting physical objects (e.g. particles) with curves in phase space, and measurable kinetic and potential energies with a hamiltonian function. The justification of each of these interpretive principles can be said to derive from the empirical success of assuming them as part of the hamiltonian description of classical systems. But the foundation of this description is based on assumptions such as that space and time are Euclidean, that physical objects are approximately point-like particles which possess intrinsic masses, that due to the symmetries of space and time these systems have certain conserved quantities, etc. (Arnold 1989, Ch. 1). Note, however, that the justification of a particular system's possession of a certain hamiltonian and its initial conditions, however, is not something that these assumptions can provide; instead one must rely on heuristics and empirical investigation to determine them with any definiteness.

Are any of these principles or justifications inadequate, in a way that is "problematic" for classical particle mechanics? Perhaps they are, in the sense of "scientific problem" discussed in chapter 2, viz. "explanatory opportunities", but then

[34]Thus this framework is an exemplar of interpretive practice in physics according to Geroch: "One takes care at the beginning to be sure that 'the mathematics is appropriate for the physics,' that is, that everything in the mathematics has physical meaning and that all of the physics one wishes to talk about is describable in terms of the mathematics. In particular, one tries to avoid structural features...which have no physical significance" (Geroch 1985, 86).

they do not represent problems *for* classical particle mechanics. They rather represent problems in relating classical particle mechanics to other theories, whether current or future, or, in analogy to the so-called interpretations of probability, with finding a way of establishing accord with use of broader linguistic usage. In the case of classical mechanics the association of its theoretical concepts to familiar concepts is perhaps more straightforward than in theories which describe phenomena more distant from everyday experience, but even there one may find distinctly "philosophical" problems to raise.[35]

I set aside such philosophical problems in the following, especially those concerning the provision of a metaphysical or conceptual basis for some theoretical concept, for my concern is focused more narrowly. By an interpretation of statistical mechanics I mean a concrete interpretation of the formal framework of statistical mechanics. This section has merely served notice of my diversion from standard usage of the term "interpretation" in the philosophical literature, and provided some reasons why it is not necessary to furnish a so-called interpretation in order to engage with important interpretational issues in statistical mechanics and cosmology.

3.3.2 Interpretation of Statistical Mechanics

Although measures play a central role in statistical mechanics (3.2), the details of their intended interpretation vary between various versions of statistical mechanics. In the standard, "Gibbsian," approach probability distributions ρ on phase space are usually said to represent (in some sense) the statistical state of an ensemble of systems.

[35] As just one example, the notion that space is a continuum leads one to model it as a differential manifold, charts of which map into the real line, which itself is a remarkably complicated mathematical construction, whether constructed axiomatically, e.g. by Dedekind cuts, by Cauchy sequences, or by what have you; clearly little of this is inherent in a folk concept of space.

In the alternative "Boltzmannian" approaches probability distributions are intended to represent (in some sense) the dynamical evolution of single, concrete systems. There is however considerable room for interpretive maneuvering within each approach, so this distinction is hardly definitional. What is common to the two approaches is that one selects a system's probability measure by choosing some probability distribution to weight the Liouville measure; one of the most salient concrete differences between the two approaches centers on how this is done, and what is considered stochastic about the theory.

I make use of the discussion of the theory's formal framework from above (3.2), i.e. a statistical system is a dynamical probability space represented by its phase space Γ, a hamiltonian function H on Γ, and a probability distribution ρ defined on Γ. The associated probability space of the observables $\mathcal{O}_\lambda$ (indexed by λ in some indexing set Λ) is generated by inheriting measurable sets and probabilities from the phase space Γ, Lebesgue measurable sets $\mathcal{L}$ of Γ, and probability distribution ρ. Rather than define a probability space for each observable, it is simpler (although equivalent) to treat the observables directly as random variables on phase space (considered as a probability space). So, for example, the Gibbs entropy $\mathcal{O}_S \equiv S$ of the system is the random variable defined by $x \mapsto -\ln \rho(x)$ for $x \in \Gamma$; its expectation value is then computed as follows:[36]

$$\langle S \rangle_\rho = -\int_\Gamma \rho \ln \rho \, \mathrm{d}\Omega; \tag{3.23}$$

similarly, the energy of the system is given by treating the hamiltonian H as a random

[36]Typical statistical mechanical observables are used to compute expectation values of thermodynamic properties like temperature and pressure, and variances which give details of fluctuation phenomena (Wallace 2015)

variable on phase space, the expectation value of which is given by

$$\langle H \rangle_\rho = \int_\Gamma \rho \, H \, \mathrm{d}\Omega. \tag{3.24}$$

A natural interpretation of these expectation values is that statistical mechanical observables are themselves stochastic. The state of the system, ρ, evolves deterministically, but the properties of the system (entropy, energy, etc.) are not fully determined by the state of the system. The state only determines the probabilities of certain observable outcomes. I call this the "stochastic observables" interpretation.

Since phase space possesses canonical coordinate charts $x = (q^i, p_i)$, one might think that these can be treated as random variables on phase space as well, in effect treating the microstate x as a random variable:

$$\langle x \rangle_\rho = \int_\Gamma \rho \, x \, \mathrm{d}\Omega. \tag{3.25}$$

Clearly, if one assumes that the hamiltonian state evolution is deterministic (as I have been doing), then one lands in a contradiction by thinking of the microstate as a random variable—even if one accepts that the microstate of the system is not empirically accessible. True, the statistics of observables are "as if" the microstate is a random variable, so one may reject the underlying determinism of the microstates. This "stochastic microstates" or "stochastic dynamics" interpretation is a well-known approach to non-equilbrium statistical mechanics (Uffink 2007, 141ff.). If one were to give up a fixed, for-all-time deterministic dynamics, then the microstate alone may be treated stochastically, in which case the observables would be appropriately treated as in classical mechanics, viz. as functions on phase space, rather than as random variables. Alternatively, one could capture the idea by defining a set of phase flows and a probability measure over these, in which case one would describe this more literally as a stochastic dynamics. Interestingly, the two viewpoints, i.e. stochastic observables with deterministic dynamics and deterministic observables with stochastic dynamics, are, it seems, complementary and empirically equivalent (this is easiest to see in the equilibrium case), and in some ways analogous to the Schrödinger and Heisenberg pictures in quantum mechanics.

There is a third view possible—really a variation on the last one. In the Boltzmannian approach, discussed further below, one does accept deterministic evolution of microstates, but avoids the aforementioned contradiction by treating the initial microstate as the outcome of a *single* probabilistic trial, which evolves subsequently according to the hamiltonian dynamics (Loewer 2001). I call this the "stochastic initial conditions" interpretation.

Thus I claim that there are three ways of interpreting probability which come from locating the stochasticity of statistical mechanics in one of the elements of the formal framework of statistical mechanics: the observables, the initial state, or the dynamics (cf. (Maudlin 2007b)). In the next chapter I assess the prospects of applying each of these interpretations in cosmology.

3.3.3 Gibbsian Statistical Mechanics

In the Gibbsian approach to statistical mechanics a state in statistical equilibrium is a probability distribution ρ whose Poisson bracket with the hamiltonian is zero. Call such a ρ an equilibrium ensemble.[37] According to the popular Gibbsian story,

[37] Among the standard equilibrium ensembles used in statistical mechanics are the microcanonical ensemble, the canonical ensemble, and the grand canonical ensemble which were originally defined by

one computes statistical data concerning the thermodynamic properties of an "infinite ensemble of systems" using an appropriate equilibrium ensemble which evolves along the hamiltonian flow on phase space. In other words, the probability distribution ρ's evolution along the flow is the dynamical evolution of an infinite ensemble of identically prepared systems. This is, on the face of it, an odd story, one that is strictly speaking false since statistical mechanics is not applied to such an infinite ensemble of systems, but to single systems—or so goes a well known objection to this "interpretation" of Gibbsian statistical mechanics (Sklar 1993, 159).

The historically motivated association of a microstate to each statistical mechanical system is, however, completely dispensable in a concrete interpretation of statistical mechanics that locates the stochasticity of the theory in the observables (McCoy 2016).[38] The main reason why is that microstates play no indispensable role in generating the empirical content of statistical mechanics, as is evident from the formulas for expectation values, etc. In fact, it is sufficient in general to consider a system to have a *single* state for all practical and theoretical purposes—in statistical mechanics this state is a probability measure (macrostate); in classical mechanics is a phase space point (microstate). The state of some system as described by one theory may of course be relatable to the state of the same system as described in another theory, but this is an inter-theoretical relationship, not an intra-theoretical relation.

Thus, insofar as the Gibbsian approach to statistical mechanics depends on identifying the probability distribution ρ as the state of a statistical mechanical system,

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Gibbs (1902). There is perhaps a question why these particular ensembles should be used (Davey 2009), but statistical mechanics alone does not furnish the resources to answer this question.

[38]This is a point with which Gibbs himself might have agreed. As Uffink (2007, 992) notes, "Gibbs avoids specific hypotheses about the microscopic constitution of [general mechanical systems]." Fine-grained details about the microphysics of any particular system require a physical theory that describes these details.

I claim the appropriate interpretation of the Gibbsian approach is the stochastic observables one. The notion that a probability measure is the state of a statistical mechanical system is in fact frequently stated, e.g. (Souriau 1997, 271). What the interpretational significance of such statements is taken to be, however, varies widely among those who state them, or else is often simply not considered. As physical states (in the context of a particular theory) are complete representations of the physical content of a system at a time, it is not a large conceptual leap to assume that statistical mechanical systems are represented by a probability measure of some kind—the empirical, macroscopic content of the theory is, after all, statistical in nature.[39] To demand that physical states be completely determined amounts to a purely metaphysical demand that the unobservable features of statistical mechanical systems be deterministic.

Surely there are further interpretational questions one can ask given the different possible senses of interpretation; nevertheless, identifying the stochasticity of Gibbsian statistical mechanics in this way does answer some important interpretational questions we may have about the theory. For example, recalling von Plato's desiderata, we can easily answer the question as to what statistical mechanical probabilities are *probabilities of* in this interpretation—they are probabilities of observable outcomes (or at least represent relative frequencies of observable outcomes). The state provides a complete accounting of all of these probabilities. As described in the formalism above the probabilities of statistical mechanical observables "attach" to subsets of

[39] I note in passing that, since physical systems are usually represented by a state and a set of observables, there are two kinds of "indeterminism" which can be manifested in physics: (1) dynamical indeterminism, where the state evolution is not unique, and (2) observable indeterminateness, where the values of observables are not uniquely determined by the state. Classical mechanics, normally conceived, does not exhibit (1) or (2) (although if one conceives of classical mechanics broadly enough, then there are examples of (1) (Earman 1986; Norton 2008a). Gibbsian statistical mechanics, in my view, exhibits indeterminism of type (2), as does quantum mechanics.

phase space, but there is an alternative characterization, already mentioned, which may be more desirable: for each observable $\mathcal{O} : \Gamma \to \mathbf{R}$ there is a probability space with $\mathbf{R}$ as the space of outcomes, $\mathcal{L}_{\mathcal{O}} = \mathcal{O}[\mathcal{L}]$ are the Lebesgue measurable sets, and $\mu_{\mathcal{O}} = \mu_\rho \circ \mathcal{O}(\mathcal{L})^{-1}$ is the probability measure. As I previously noted however, rather than go about defining a probability space for each observable, it is simpler to treat Γ as a probability space and the observables as random variables on this space. However one describes it, though, the interpretation is available in principle.[40]

To sum up, I have argued that the concrete interpretation of Gibbsian statistical mechanics, as presented in this section and based on the framework of §3.2, holds that a classical statistical system is specified (completely) by a set of statistical mechanical observables (random variables on Γ) and the evolution history of a statistical state, $\rho : I \subset \mathbf{R} \to \mathcal{P}$ for some time interval I, where $\mathcal{P}$ is the set of probability measures on Γ. In the case of equilibrium statistical mechanics this evolution history is determined completely by the action of the hamiltonian H on $\mathcal{P}$ and an initial probability distribution $\rho_0 = \rho(0)$ in P.[41] Thus probability measures play a direct role in the description of the physical system, namely by specifying the state of the system (explicitly), which encodes all of the possible observational content of the system; like the classical case, the specification of the system depends on the specification of an

[40]When interpreted this way statistical mechanical probabilities are appealingly similar to quantum mechanical probabilities as well, i.e. they represent some inherent randomness in the empirical behavior of systems that can be described as statistical mechanical systems. What is the origin of this randomness? It seems that that is a question which statistical mechanics cannot answer on its own, since it assumes it in the theoretical description of systems that fall under its scope. The Boltzmannian presumes an answer by interpreting a (classical) microphysics into the theory and requiring special initial conditions. I am inclined to argue that the fuller answer is that the origin of the randomness is quantum mechanical in nature. The issue is not so important in the present context, so I do not discuss it further.

[41]As said, in equilibrium statistical mechanics it is assumed that ρ is an equilibrium ensemble; in non-equilibrium statistical mechanics ρ is time-dependent. Since in the latter case one is interested in a system's approach to equilibrium, hamiltonian dynamics is inapplicable (Liouville's theorem forbids it). This is sometimes thought to be an objection to the Gibbsian view (Sklar 1993, 55).

initial condition, which in the statistical case is a statistical state. Everything in the chosen framework has clear physical meaning (in terms of the empirical content of the theory), and the kinds of physical phenomena one expects to describe with statistical mechanics are describable in this framework. In other words, we have an adequate concrete interpretation of (Gibbsian) statistical mechanics.

3.3.4 Boltzmannian Statistical Mechanics

Many philosophers (and some physicists) criticize the Gibbsian approach to statistical mechanics for being conceptually misguided, lacking explanatory power, and not being an adequate reduction base for thermodynamics, among many other complaints.[42] These complaints do not necessarily concern the interpretation of the theory in the sense of interpretation I outlined above, as nearly everyone concedes that the Gibbsian approach is effective in practice and free of confusion in application.[43] Boltzmannians instead apparently suggest the possibility of further fruitful work developing the statistical mechanical theoretical framework in ways that improve (conceptually or metaphysically anyway) upon the Gibbsian approach. My aim here is to outline the justification and the concrete interpretation of Boltzmannian statistical mechanics, which differs in some respects from Gibbsian statistical mechanics.

In the alternative approaches to statistical mechanics, those which fall under the Boltzmannian banner, probability is ostensibly implemented in distinct ways, with various apparent "interpretations" associated to these implementations. The stated

[42] "In fact, the Gibbs entropy is not even an entity of the right sort: It is a function of a probability distribution, i.e., of an ensemble of systems, and not a function on phase space, a function of the actual state of an individual system, the behavior of which the Second Law—and macrophysics in general—is supposed to describe" (Goldstein 2001, 47). See also (Sklar 1993; Callender 1999, 2001; Albert 2000; Frigg 2008b).

[43] Boltzmannians included: "Let me happily concede that for the practice of science, Gibbsian SM is usually to be preferred" (Callender 1999).

goal of many of the proponents of these approaches is to give a microphysical basis for thermodynamics, including the second law of thermodynamics.[44] Thermodynamics is understood to deliver the dynamics of macroscopic thermodynamic states M_i (i in some index set I of the macrostates), where these macrostates are elements of the macroscopic thermodynamic state space—each macrostate has a unique set of values for thermodynamic observables, such as temperature, pressure, entropy, etc., these observables understood as maps of the form $\mathcal{O} : \{M_i\} \to \mathbf{R}$.

The Boltzmannian version of statistical mechanics assumes that systems not only possess thermodynamic macrostates, but that they also possess classical mechanical microstates. In order to forge a connection between micro- and macrostates, one "super-imposes" the set of macro-states on the relevant statistical mechanical phase space, since it is assumed that microstates in phase space are in a many-one relation to macrostates. More precisely, there exists a surjective map $M : \Gamma \to \{M_i\}$) which partitions phase space into macroregions (i.e. macrostates in the terminology of §3.2) corresponding to the thermodynamic macrostates,[45] i.e. $\cup_i M_i = \Gamma$, and $M_i \cap M_j = \emptyset$ for all i, j. Thus, metaphysically speaking, statistical mechanical systems possess a macrostate in virtue of having a microstate.

It is acknowledged that statistical mechanics does not make determinate predictions for single systems, so it is generally unjustified to assume that a system exists in a particular known microstate.[46] Treating statistical mechanical systems

[44] "*The* problem for *foundations* of statistical mechanics arises from the fact that not only do we have a thermodynamic description of our (say) sample of gas and its behaviour, but we also have a mechanical theory describing the behaviour of the entities that constitute the gas" (Callender 2001, 540).

[45] I denote macroregions and macrostates by the same symbol, since there exists an obvious bijection between them.

[46] Although some textbooks do not explicitly mention the term "microstate", those that do often point out this fundamental epistemic assumption: "...even though the energy is fixed, the system could exist in any one of a number of different microscopic states consistent with that energy. If we

as if they possess an unknown microstate is however adequately justified in normal applications of the theory, despite the microstate being empirically inaccessible, because the statistical predictions of identically prepared systems, i.e. systems prepared in identical macrostates, are in accord with the assumed random distribution of initial microstates. In short, the Boltzmannian assumption of microstates is empirically consistent—as of course it had better be.

It is seldom acknowledged by Boltzmannians that, since microstates evolve deterministically and have determinate observable outcomes, the only possible ontic interpretation of probability—an interpretation in which probabilities are taken "to be part of the furniture of the world" (Frigg 2008b, 115)—is in determining the initial microstate as the outcome of a single random trial (in other words as a random variable on phase space) which subsequent to this trial evolves deterministically according to the hamiltonian dynamics.[47] Of course one cannot ever know in which microstate the system actually is (since it is empirically inaccessible), so one must treat the system's observables as random variables on phase space with probabilities in accord with what is empirically accessible, namely the macrostate of the system. Thus in the Boltzmannian view statistical mechanical probabilities represent ontic randomness only in the sense that for each system there is a random trial which determines a system's particular initial microstate; hence the statistics of all (macroscopic) observables must be understood epistemically, viz. as concerning uncertainty over the system's actual

only know the total energy, we have no way of distinguishing one microscopic state from another" (Reichl 1998, 341); "A macroscopic physical object contains so many molecules that no one can hope to find its dynamical state by observation" (Penrose 1970, 2); "The definition requires one to know the initial positions and velocities of all n particles and to follow these motions for all time. Since n is typically of the order of 10^{24}, this is of course impossible" (Ellis 2006, 65).

[47]Loewer is clear about the point however: "While there are chances different from 0 and 1 for possible initial conditions the chances of any event A after the initial time will either be 1 or 0 since A's occurrence or non-occurrence will be entailed by the initial state and the deterministic laws" (Loewer 2001, 618). See also (Volchan 2007).

microstate as it evolves deterministically in phase space. These latter, epistemic probabilities are derivatively objective, of course, because they are inherited from the objective probability of the system possessing a particular microstate.[48]

To discuss probability and its interpretation in Boltzmannian statistical mechanics further, it is necessary to make a detour through its connection to thermodynamics via the second law of thermodynamics (Callender 2011b), the central concept of which is of course entropy. The Boltzmann entropy S_B of a macrostate is a map $S_B : \{M_i\} \to \mathbf{R}$ given by $M_i \mapsto \log[\mu(M_i)]$, where μ is the Liouville measure on phase space. Since the Boltzmann entropy is defined for macrostates, one may also naturally define the entropy of a microstate by using the map M, viz. by defining the map $S_B : \Gamma \to \mathbf{R}$ such that $x \mapsto S_B \circ M(x)$. The time-dependent entropy of a system with initial microstate x in Γ is then determined by the map $(x, t) \mapsto S_B \circ M \circ \phi^t_H(x)$: evolve the phase point x along the hamiltonian flow by time t, project the image point into the space of macrostates, and calculate the entropy. Since we have defined all three of these pieces of the time-dependent entropy, we can now examine Boltzmannian versions of the second law which attempt to specify the behavior of these maps.

Boltzmann recognized that classical particle dynamics alone could not give rise to the second law, and therefore demoted the law from a universal generalization (as it is in thermodynamics) to a generalization that holds with high probability. One might therefore intuit the second law in statistical mechanical form as Earman does:

> Suppose that at $t = 0$ the Boltzmann entropy $S_B(0)$ of the system is low; then for some appropriate $t > 0$, it is highly probable that $S_B(t) > S_B(0)$. (Earman 2006, 403)

[48] Frigg and Hoefer claim that "there is nothing chancy about a system's initial condition" (Frigg and Hoefer 2013). If they really believe this as Boltzmannians, then statistical mechanical probabilities can only be epistemic without qualification, since the only way for probabilities to represent physical randomness in the Boltzmannian view is as chancy initial conditions. Cf. also (Frigg 2008a).

This statement of Boltzmann's law is intolerably vague (and possibly without much content), but qualitatively, at least, it represents the aim of a statistical mechanical version of the second law. Still there is a significant difficulty in interpreting "highly probable" in this statement, since what is probable obviously depends on the particular probability distribution chosen for the system and there are various ways that have been proposed for doing this. Frigg (2010), in particular, describes two main implementations of probability in Boltzmannian statistical mechanics, what he calls the "proportionality postulate" and the "statistical postulate."

The *proportionality postulate*, according to Frigg, states that the probability of a macrostate M_i is proportional to the measure of that macrostate: $M_i \mapsto \int_{M_i} \mathrm{d}\Omega$; clearly the postulate is equivalent to the specification of a probability distribution μ_ρ on phase space where ρ is the uniform probability distribution on Γ_i. Obviously one must assume that the system's phase space is such that a uniform probability distribution exists and that the Liouville measure is finite, otherwise the proportionality postulate makes no sense. It is also evident that the proportionality postulate privileges the macrostates. It is hoped that the physics of statistical mechanics is relatively insensitive to these precise choices, since the choice of macrostates is not given in any natural way. Following Frigg, call these *macroprobabilities*. One is meant to interpret these probabilities as probabilities of the system having a particular macrostate, i.e. a particular set of macroproperties.

The *statistical postulate* takes what appears to be a slightly different approach. It stipulates that the probability of a measurable subset A_j of M_i is equal to the ratio of the measures of A_j to M_i:

$$\mathrm{pr}(A_j) = \frac{\mu(A_j)}{\mu(M_i)}. \tag{3.26}$$

Following Frigg, call these probabilities *microprobabilities*. They are meant to be understood as probabilities that the system's microstate, given a macrostate, is within the subset A_j; in other words, they are just conditional probabilities.[49] The statistical postulate is equivalent to specifying a set of probability measures μ_{M_i} on phase space, where measurable subsets U are mapped to $\int_U \rho \, \mathrm{d}\Omega$, with $\rho = 1$ if $x \in M_i$ and $\rho = 0$ otherwise. In other words, these probability measures are uniform probability distributions on the various macrostates—in other words these are just certain choices of statistical states.

Although these are two ways to introduce probability onto phase space which are in some ways different from the Gibbsian approach, they do not, as stated, yet have the resources to recover the second law, for they both are mere probabilities of states whereas the second law requires transitions between states to be highly probable—the second law is a dynamical condition, and these are non-dynamical probabilities.

For the proportionality postulate to ground the second law it must be the case that the macroprobabilities defined by the postulate represent the dynamical behavior of systems (in some sense). If particles randomly popped around phase space at discrete time intervals, then it would be correct to say that the equilibrium state, i.e. the macrostate M_{eq} for which the Boltzmann entropy is largest and therefore the macrostate whose corresponding phase space volume is largest, is the most probable state, but it would not be correct to say that the second law of *thermodynamics* held (although it would be possible to satisfy Earman's statement of Boltzmann's law in this

[49] These conditional probabilities are well-defined when μ is finite, but not in general. That they are conditional probabilities is evident in this case that μ is finite, since

$$\mathrm{pr}(A_j|M_i) = \mathrm{pr}(A_j \cap M_i)/\mathrm{pr}(M_i) = \mathrm{pr}(A_j)/\mathrm{pr}(M_i). \tag{3.27}$$

way). Since hamiltonian dynamics gives rise to continuous trajectories, there must be a way to interpret the behavior of particles moving on these trajectories as being related to phase space volume. A popular suggestion is to interpret the macroprobabilities as time-averages of particle trajectories; if the dynamics governing the system is such that particles spend an amount of time in macroregions proportional to the measure of those macroregions, then the system is called ergodic and the interpretation is secured. But in this case the macroprobabilities are serving merely as proxies for dynamical facts. Statistical systems are however under no obligation to be ergodic (Sklar 1973; Earman and Rédei 1996; van Lith 2001), so there could not be such an interpretive principle linking phase space volumes and macroprobabilities for statistical mechanics in general.

Consider next the subsets A_i of macrostates M_i which include just those microstates that satisfy the second law (in some presumably more specific version). For the statistical postulate to ground the second law it must be the case that the A_is have high conditional probabilities $\mathrm{pr}(A_i|M_i)$, i.e. most microstates in macrostate M_i satisfy the second law ("are thermodynamic"). That is, let A_i (at time $t = 0$) be the measurable subset of M_i that evolves into a higher entropy macrostate after some "appropriate" time t: $A_i = \{x \in M_i | S_B(x,t) \geq S_B(x,0)\}$. Then the second law holds in the statistical version if $\mathrm{pr}(A_j)$ is sufficiently high (for Earman's "appropriate" times t).

As is well known, the reversibility objection shows that this probability cannot be sufficiently high to secure the second law, so the popular move, following Boltzmann's suggestion, is to conditionalize this probability on the supposed (necessary?) fact that the system started (at time t_p) in a low entropy initial state

M_p, i.e. the relevant probability for assessing the satisfaction of the second law is $\text{pr}(A_i|M_i \cap \phi_H^{t_p}[M_p])$.[50] This is the microprobability of the system to be in a microstate which will evolve into a state of equal or higher entropy in time t, but which began at time 0 in a state of low entropy. It is then claimed that the probability $\text{pr}(A_i|M_i \cap \phi_H^{t_p}[M_p]) >> \text{pr}(A_i^c|M_i \cap \phi_H^{t_p}[M_p])$, where A_i^c is the complement of A_i. In short, the second law is claimed to be recovered from this proposal.

This approach to justifying the second law unfortunately suffers from a great deal of vagueness, in particular in the specification of the appropriate times t and the subsets A_i, this vagueness in turn infecting the final conclusion. It is lessened somewhat by asserting that the claim is insensitive to choice of t, but to substantiate the claim that the set A_i (conditionalized on the past state) is much larger than its complement (conditionalized on the past state) one must show that the hamiltonian dynamics makes it so. Thus to assert that the second law holds *in general* requires showing that the hamiltonians of diverse physical systems that exhibit thermodynamic behavior, perhaps even including the observable universe itself, insure the probable increase of entropy.[51]

This detour through attempts to justify Boltzmann's law has introduced the low entropy initial condition assumption that is thought to ground it—it plays an important role in chapter 4's discussion of cosmological probabilities. It has also

[50]There is some pressure to assume that the universe as a whole started in such a low entropy state, frequently called nowadays the "past state" (Albert 2000). The proposal that the universe began in such a low entropy state is called the "past hypothesis." In this scenario the statistical mechanical probabilities *are* cosmological probabilities. I object to the universalizing of statistical mechanics in this way in chapter 4. The past hypothesis is much discussed in the literature, including the following critiques: (Leeds 2003; Winsberg 2004, 2008; Parker 2005; Earman 2006; Wallace forthcoming; Weslake 2014).

[51]Werndl and Frigg (2015) usefully re-conceive this approach, and provide necessary and sufficient conditions for when a *specific* statistical mechanical system (in the Boltzmann approach), as described in the next paragraph, satisfies the second law.

usefully introduced the elements of a Boltzmannian statistical mechanical system, i.e. such a system is represented by its phase space Γ, a partition of phase space $Z = \{M_i\}$, a hamiltonian H, and an initial macrostate $M_p \in Z$. Macroscopic observables are functions of the macrostates, although these can be represented as functions on phase space via the map $M : \Gamma \to Z$ from phase space to the partition of phase space, i.e. macroscopic observables are also functions on phase space like microscopic observables (the latter of which are generally empirically inaccessible). Roughly speaking, the observables can be computed by representing the initial macrostate as a uniform probability distribution on that macrostate, evolving that distribution in time, and computing the statistics of observables by taking averages of the observables, determining variances, etc. as in the Gibbsian approach.[52]

The probability space formed by the phase space Γ, the Lebesgue measurable sets $\mathcal{L}$, and the uniform probability measure μ_M on the macroregion M_p is in part justified and interpreted in the same way as in the Gibbsian approach. The salient differences are that in the Boltzmannian case one has a seemingly principled way of choosing probability measures by way of the choice of macrostates. Also, I believe it is of some value to point out that in the Boltzmannian case probability pertains to microstates directly (as a single random trial determining the initial microstate) and to observables derivatively (epistemically), whereas in the Gibbsian case probability pertains to observables directly (observable values of a system are given by random trials) and to microstates derivatively (epistemically, if at all).

[52]Representing a statistical mechanical system in this way appears to avoid any direct reference to the statistical postulate, but this is because it is incorporated in the assumption of a uniform probability distribution on the initial macrostate.

3.3.5 Justification of Measure

The most used probability distributions in the theory are uniform with respect to the Liouville measure μ on some subset of phase space (e.g. a macrostate in the Boltzmannian approach). Whether and why it is justified to apply a uniform probability distribution on the Liouville measure of phase space in statistical mechanics is an increasingly debated topic in the foundations of the theory. Some take the justification of these measures to derive from a prioristic principles like the principle of indifference or maximum entropy (Jaynes 1957), or the proportionality postulate or statistical postulate (Albert 2000); others Davey (2008); North (2010); Hemmo and Shenker (2012, 2015) criticize any a prioristic grounding of specific choices of probability measure.

It appears to be an empirical fact, at least, that statistical mechanical systems in equilibrium are well-described by uniform probability distributions, and so there is at least an empirical justification to use such measures.[53] To be sure, other probability distributions that are similar to the uniform distribution will give approximately identical predictions, since from empirical frequencies one cannot infer a probability distribution. Indeed, an equilibrium state in the Gibbsian approach and a uniform probability distribution on the equilibrium macrostate in the Boltzmannian approach will give approximately identical predictions, even though they are slightly different probability distributions (Callender 1999).

Insofar as one considers statistical mechanical states and macrostates as probability measures, these uniform probability distributions are quite special. Almost all probability distributions on phase space are not uniform on some subset of phase

[53] "In our view the fit with the observed relative frequencies is the only compelling argument for the choice of measure in statistical mechanics" (Hemmo and Shenker 2015, 580).

space, and certainly there are systems that are appropriately described by these other probability distributions, e.g. classical mechanical systems have probability distributions that are "sharply peaked" around a microstate. The existence of such systems so described is at least one serious argument for why empirical considerations are necessary to determine which probability measure should be used to represent the system's state. Just as there is no a prioristic justification for the choice of a particular hamiltonian or initial state in classical mechanics, it is likewise dubious that there would be an a prioristic justification for the choice of a probability distribution in statistical mechanics.

In any case, there is clearly some empirical element involved in constraining the possible statistical mechanical probability measures. In the case of Gibbsian statistical mechanics, this empirical input comes from the choice of ensemble. In the case of Boltzmannian statistical mechanics, this empirical input (in part) comes in the choice of Z, the set of macrostates. The a prioristic principles are then meant to license the specific choice of the uniform probability measure with respect to the empirical constraints, e.g. the macrostates (in the version of Boltzmannian statistical mechanic based on a statistical postulate). However the assignment of probabilities depends importantly on the mechanism or method of randomness (Norton 2010), and it is not necessarily the case that some given sample space represents the effects of this mechanism or method uniformly (as Bertrand's paradox is meant to show for example).[54] Some do and some do not; statistical mechanics itself is not going to tell

[54] "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).

us which.[55]

While it may seem appealing to base a principle on the objective mathematical naturalness of the uniform probability measure (when it exists), that move alone is insufficient, since mathematical naturalness does not necessary guarantee physical significance—not only because of the reason just given, but also because there are any number of mathematical objects which are natural given some mathematical structure and yet have no physical significance at all.[56] Note that heuristic and pragmatic considerations may favor choosing the uniform probability distribution rather than a predictively similar non-uniform one, since in typical cases the observable statistics of a statistical mechanical system are relatively insensitive to the exact choice of probability distribution. This gives some freedom of choice, but this choice is motivated by essentially subjective criteria (Albert 2000, 67) and limited to cases where there is sufficient empirical justification to use the uniform distribution.

An alternative is to eschew the use of probabilities in statistical mechanics and rely on something slightly more general to establish claims like the second law: a typicality measure (Lebowitz 1993b,a; Dürr 2001; Goldstein 2001, 2012; Volchan 2007; Werndl 2013; Schech 2013; Lazarovici and Reichert 2015). 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 quantative successes of statistical mechanics (Wallace 2015), which depend on probability distributions

[55]Indeed, it seems quite likely that the probability measures of statistical mechanics could be given a deeper explanation by representing, e.g. quantum states (Albert 1994, 2000; Hemmo and Shenker 2001; Wallace 2012, 2015; McCoy 2016).

[56]Further, more detailed arguments along these lines are given by Wilson (2000); Davey (2003).

to derive the empirical content of the theory (for example to predict fluctuation phenomena). Thus, "the explanation though 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).[57]

Nevertheless, "typicality" arguments will be of especial interest here because cosmologists use similar such arguments as a way of establishing what does and does not demand explanation in cosmology (recall the quotations at the beginning of §3.2). The basic schema of a typicality argument (in statistical mechanics) is as follows: if one can show, using the natural measure on phase space, the Liouville measure, that some behavior or property is highly likely and its contrary is highly unlikely, then 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). We will examine several such arguments in the following chapter.

[57]For further criticisms of the typicality account in statistical mechanics, see (Frigg 2009, 2011; Frigg and Werndl 2012).

Chapter 4

The Generic Universe

4.1 Introduction

Last chapter's discussion of the implementation, interpretation, and justification of likelihoods in statistical mechanics paves the way for the present chapter's discussion of the same in the context of cosmology. My main strategy here is to compare the prospects for implementing, interpreting, and justifying cosmological likelihoods with those in statistical mechanics. In general I find that the prospects are exceedingly limited, supporting the claims I made in the conclusion of chapter 2 concerning the interpretation of the HBB model's fine-tuning problems as likelihood problems.

Before turning to this task, I think it is worth remarking on an issue that was left to the side in the previous chapter. That issue is whether the likelihoods allegedly invoked in cosmological fine-tuning arguments are subjective or objective.

On the one hand, one might expect that an objective way to assess cosmological likelihoods is required, i.e. a physically grounded way of defining cosmological likelihoods. The objective approach appears to be the favored approach of most physicists (although they tend not to make a clear distinction between the two kinds of likelihoods, complicating an assessment of their claims). The most well-known proposal in this vein is the already-mentioned canonical measure of (Gibbons et al. 1987) (hence the GHS measure).[1] One could alternatively use topological methods to give an objective measure of likelihood as well.[2] The basic strategy of these approaches centers on attributing likelihoods to sets of cosmologies in some relevant space of possible cosmologies. Then, for example, if spatially flat FRW spacetimes represent a negligible set of cosmologies and inflating FRW spacetimes are generic cosmologies (according to some justified measure of cosmological likelihoods), then inflationary cosmologists would seem to have a 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 fine-tuning based on some subjective measure of likelihood, i.e. one based on an attribution of rational degrees of belief. Subjective notions of likelihood are perhaps behind many cosmologists' intuitions about this case; there are, alas, few places in the literature where more precise formal methods are used to substantiate these intuitions.[3] A more thorough review of approaches to defining cosmological likelihoods would engage with these subjective approaches, but I will restrict myself to addressing the more prominent objective approaches (apart from some comments on the principle of indifference (4.2.2)). I do so mainly because these subjective measures of likelihood depend,

[1] The notable papers discussing this 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).

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

[3] 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.

plausibly, on an underlying objective measure of likelihood in order to be relevant to physical problems.[4]

For reasons of simplicity, familiarity, and relevance to the discussions in the literature, I will concentrate mostly on probabilistic measures of likelihood.[5] Recall that an application of probability theory standardly 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 $\mathcal{F}$. The probability spaces which are of interest are those whose sets of possible outcomes are sets of possible cosmologies. I take as conditions for a cosmological probability space to have physical significance that the choice of X and P must be justifiable and interpretable ($\mathcal{F}$ can be chosen on pragmatic grounds). They cannot be arbitrarily chosen, and they must have some demonstrable empirical significance. One would accomplish little, for example, by stipulating that X is the one element set consisting of our universe, and P the map which assigns our universe probability one.

The chapter is organized as follows. In §4.2 I address the interpretation and justification of probability in cosmology, especially by reference to the justification and interpretation of statistical mechanical probabilities. The main conclusion of this section is that implementing cosmological probabilities by analogy to the implementation of statistical mechanical probabilities, as is done in the most prominent attempts, is unjustifiable and can only be understood as assigning probabilities of initial conditions.

[4](Uffink 2011) is a recent review and defense of the relevance of subjective approaches to probability in statistical mechanics. See (Albert 2000; Meacham 2010) for some typical criticisms.

[5]At times I will generalize my arguments to other objective approaches (especially topological ones), but to maintain a reasonable scope my approach is not 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.

Although the space of possible cosmologies is by no means simply given to us, it is reasonable to suppose that the models of GTR give us *a* reasonable space of physically possible cosmologies. In §4.3 I investigate the potential for defining a measure for this space. There is, however, a significant technical obstacle to providing any such measure, since on any such infinite-dimensional space there is no known well-behaved probability measure (Curiel 2015). Cosmologists have circumvented this issue by truncating the degrees of freedom so that the probability space is finite-dimensional. This is the approach taken to define the GHS measure, which is discussed in the final section (§4.4) of the chapter. Although the GHS measure is taken to be physically objective and natural by several authors, there remain several technical problems with its application to cosmological questions, especially how to correctly account for inhomogeneous degrees of freedom and the divergence of the measure (Schiffrin and Wald 2012). I argue in particular that this divergence has been repeatedly misinterpreted by commentators, and that it does not show that there is no flatness problem as some authors claim.

4.2 General Problems of Objective Probability Measures

In the previous chapter I discussed (classical) particle, statistical, and field theories from a geometrical point of view, in particular showing how measure and probability measures may be canonically implemented, interpreted, and justified in these theories. This presentation was far from comprehensive, leaving out, for example, any discussion of statistical field theory. Nevertheless, the ground has been sufficiently laid to discuss general issues in establishing a probability distribution 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 this sample space. This section investigates these issues with reference to the analogous issues in statistical mechanics.

The basic point to make is obvious: the uniqueness of the universe makes the application of probability theory to cosmology—insofar as probabilities are assigned to entire cosmological histories—highly problematic. The uniqueness of the universe (1.2.2) has long been recognized as a problem for cosmology as a science, however its significance has often been overstated.[6] Thus it is necessary to draw the argument out in some detail in order to avoid an overly hasty conclusion. In any event, I do conclude that the problem is indeed real: there is simply no reliable physical content to be found in the addition of cosmological probabilities to classical, single-universe cosmology.

4.2.1 The Reference Class Problem

The first issue to face in defining an objective probability (or likelihood) space in cosmology is deciding on the appropriate reference class. A probability space is, again, standardly specified by a set X of outcomes, a σ-algebra $\mathcal{F}$ of these possible outcomes, and a probability measure P.[7] The problem of deciding the appropriate

[6]There is a proliferation of papers discussing the topic of cosmology as a science written in the middle of the 20$^{\text{th}}$ century: (Dingle 1955; Munitz 1962; Harré 1962; Davidson 1962). Many of thie views expressed have been 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).

[7]A topological space is specified by a set X 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.

reference class is determining of which objects X consists.[8] Plainly the appropriate reference class in this case is the set of (physically) possible cosmologies. But what are the possible cosmologies?

In the §1.2 I motivated the idea that a cosmology is a relativistic spacetime and a physical model of cosmological phenomena in that spacetime. The reason for that choice was that at large scales gravity appears to be the most important physical force, and general relativity is the best, most highly-confirmed theory of gravity that we have. Trust in our theories is usually thought to underwrite the belief that the models of that theory are physically possible. For example, taking X as the collection of models of GTR is justified 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)[9]) what the physically possible models are. Thus, by this argument, one is led to the view that a cosmological model is a model of general relativity.[10]

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 seemingly undesirable or pathological spacetimes are possible in general

In this way topology can be used to define a rough notion of likelihood: "almost always" and "almost never." Clearly, in this case too one must justify an appropriate reference class X.

[8]In the end it perhaps does not matter too much precisely what one chooses for X—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" spacetimes 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 0 probability is assigned.

[9]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.

[10]At least on a non-fundamental view of cosmology. In quantum cosmology and quantum gravity one may consider other spaces of possible cosmologies, but these theories remain empirically unconfirmed so they are relevant only in those theoretical contexts. General relativity, on the other hand, does appear to apply to our world.

relativity. Many authors are for this reason inclined to disallow models from physical consideration, for example models with closed timelike curves (CTCs).[11] 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 aim to take a stand on these questions here. The only salient point I wish to make is that in practice cosmologists certainly do exclude certain models from consideration, thereby presuming some alternative physical modality to the "straightforward" nomological one.[12] Without a doubt, the analytical nature of this 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 physical principles or observational grounds (Earman 1987; Manchak 2011).

I note especially, among the assumptions used to exclude certain models from consideration, the assumption that spatial sections of spacetime are topologically compact (as found in, for example, (Fischer and Marsden 1979; Schiffrin and Wald 2012).).

[11] 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" must be made. Whereas some find CTCs objectionable—"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. Manchak (2011), for example, demonstrates that the existence of observationally indistinguishable spacetimes that may exhibit many features which one may regard as undesirable. 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. (I set aside any discussion of a distinction between "physically reasonable" and physically possible." See (Earman 1995, 80) for some commentary.)

[12] That is, unless one is assuming that the laws are other than the ones we currently have available, and that such laws preclude the pathological feature excluded in one's favored physical modality.

Although compact topologies are mathematically convenient (so that integration on manifolds converges), there seems to be no obvious argument for the claim that they alone recommend themselves for physical application. Moreover, proceeding as if they did is hardly an innocent assumption. It therefore seems that any result that relies on such a strong assumption must be accepted with considerable caution, especially if some suggestion that the result appropriately generalizes to other spatial topologies is absent.[13]

Nevertheless, some weight of consideration should be reasonably accorded to practice, such that the possibility of justifying the exclusion of pathological spacetimes should not be quickly dismissed merely because adequate justifications have not been given. In short, the reference class problem should not be regarded as solved because one can easily identify nomological and physical possibility by fiat (or by philosophical artifice). Thus I am fully in agreement with (Earman et al. 2009, 93), who suggest that "the status of such judgments is a more interesting issue than philosophers have generally realized."

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 (or reasonable, etc.). 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

[13] It is of course not the case that compact spatial topologies are necessarily observationally indistinguishable from non-compact spatial topologies, but for "large" cosmologies distinguishing the spatial topology of our universe is practically impossible due to observational limitations (Geroch and Horowitz 1979).

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 §4.3 for details).

Partly as a consequence of these problems attention has so far been mostly directed at simple models which have a finite-dimensional state space, e.g. the GHS measure. This maneuver raises another problem. By what right shall we consider some probability measure contrived on such a reduced collection to have any physical significance? Would this measure tell us anything about the likelihoods of the members of that space? It would seem not. On the one hand, if the collection S of simple models is taken to be the correct 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, for how could we know that the correct possibility space is not larger? This is especially so when one considers the pressure to take seriously the physical import of nomological possibility and the apparent fact that our universe in fact has a seeming infinity of spatial degrees of freedom.

If, on the other hand, the collection S (equipped with probability measure μ_S) is just some subset of a larger possibility space X with nonzero measure (according to the larger space's 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.[14] The probabilities of the large space X thus put a constraint on the probabilities of the

[14] 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 $\mu_S(S \cap Z) = \mu_X(Z|S)$.

smaller space S, a constraint that a general probability measure associated with the smaller space is not guaranteed to meet. Formally, the probability assigned to a collection of models $Z \subset S$ must be, on pain of inconsistency,

$$\mu_S(Z) = \mu_X(Z|S) = \frac{\mu_X(Z \cap S)}{\mu_X(S)} = \frac{\mu_X(Z)}{\mu_X(S)}. \tag{4.1}$$

As a specific and relevant example, consider the subset of models of GTR that satisfy the strict cosmological principle (CP+). The CP+ constrains the set of models from general relativity to those that are spatially homogeneous and isotropic, i.e. the FRW models. First of all, is this assumption admissible as a way of narrowing the scope of X? Although the FRW models certainly 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 and isotropic.[15] Thus the probabilities that we obtain by making this assumption are plausibly not unconditional probabilities—the space of physically possible cosmologies is surely larger. How large? If the space of physically possible cosmologies is the space of nomically possible spacetimes (according to GTR), then the set of FRW models is almost certainly negligible.[16] It is then hard to see the point of trying to figure out what the restricted measure on the set of FRW spacetimes is, at least if the motivation is to derive an objective probability measure.[17]

[15]The strict CP (strict homogeneity and isotropy) is obviously false, since our universe is not homogeneous and isotropic—there are galaxies, planets, people, etc.

[16]Isenberg and Marsden (1982) show that models with symmetry (like FRW models) are negligible in the topological sense in the space of vacuum solutions of GTR. It is generally taken that this result shows that non-symmetric models are generic in GTR. Note that solutions to the Borel-Kolmogorov paradox show that conditional probabilities are sensible even in the case that the conditional probability space itself is a negligible set in the larger space.

[17]The measure μ_S could however be epistemically useful. Suppose we know our universe is FRW

4.2.2 Interpretation of Cosmological Probabilities

The discussion of statistical mechanics in §3.3 introduced a few possible ways to interpret the stochasticity of that theory. I review them briefly first before assessing their applicability to cosmological probabilities.

Under the first (essentially Gibbsian) interpretation of statistical mechanics one understands probability measures as system states, i.e. as complete physical descriptions of the system. These states evolve according to some appropriate dynamics and determine the statistics of observables, the latter of which are treated as random variables on the state space. If the dynamics is hamiltonian, as it is for equilibrium systems, then the state evolution is deterministic, but the observables are stochastically indeterminate. A second (essentially Boltzmannian) possible interpretation of statistical mechanics holds that probability measures represent macrostates, but these macrostates encode the objective chances of the system beginning in a certain microstate, which state evolves deterministically (for all systems, equilibrium or non-equilibrium) according to the hamiltonian dynamics.[18] The observables, as in classical mechanics, are completely determined by the system's microstate. However it is not possible to know typical system's microstates—the system's macrostate represents an objective limit on our ability to know in which microstate a statistical mechanical system is. So the statistical outcomes of observables in this Boltzmannian picture arise essentially from uncertainty over the system's initial and actual microstate,

(or more plausibly almost-FRW); in advance of the relevant empirical data, we may want to know how likely it is that our universe is spatially flat. The appropriate measure to use in this case would be the restricted measure $\mu_X|_S$. Of course, since we do not know the full measure μ_X on the space of physically possible cosmologies (§5), we would have to choose the measure μ_S on the subspace carefully, as it is not guaranteed to be consistent with μ_X (whatever it is).

[18] "One way to visualise this proposal is to think of the dynamics of the theory as containing one single stochastic event happening at the initial time, which sets the initial conditions" (Wallace 2014, 204).

and hence its observable properties. The third possibility in interpreting statistical mechanics is to understand probability measures as encoding a stochastic dynamics of microstates (which dynamics may be implemented in various ways) that possess determinate observable properties. So, in short, one may locate the stochasticity of statistical mechanics in one of three places: the observables (Gibbsian), the initial state (Boltzmannian), or the dynamics (stochastic dynamics).[19]

How do these interpretations apply to cosmological probabilities? Empirical considerations strongly militate against the first and third versions. Cosmological observables, e.g. spatial curvature, are apparently not stochastic, for which reason they are treated as determinate in cosmology. The stochastic observables interpretation only makes sense if one assumes that the appropriate probability measure is essentially the trivial one: the probability of our universe is essentially one.[20] It also does not seem sensible to understand the probabilities as concerning stochasticity in the dynamics, since the dynamics of our universe are quite well-described by the deterministic dynamics of GTR. There is, in other words, very little reason to think that the universe is "fluctuating" around the space of possible cosmologies dynamically, and very little reason to think that its observable properties are either.

So, by elimination, we are left only with the possibility of treating the initial condition of the universe as stochastic, i.e. as the outcome of a random trial. Probability measures represent the objective chances of our universe to begin in a particular (macro-)state. Indeed this is roughly the viewpoint that has suggested itself to many cosmologists, who compare the situation (sometimes pejoratively) to a blind-folded

[19]Cf. (Maudlin 2007b; Wallace 2014).

[20]Since our cosmological data is statistical in nature, it is also possible to treat the probability measure as something less trivial, but the support for this measure must be quite constrained given that classical GTR gives highly accurate predictions of observations.

creator selecting a universe by throwing a dart at the dartboard of possible universes.[21]

Should one adopt such a view in cosmology? It is on the face of it a coherent possibility. Although it is arguably tenable in statistical mechanics (where it is (tacitly) employed in typical Boltzmannian approaches), it is unclear why one would ever favor this interpretation in cosmology however (Hemmo and Shenker 2012). 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). Obviously, thinking of a single-sample probabilistic scenario in cosmology is observationally indistinguishable from a deterministic scenario that involves no probability at all, only an initial state.[22] There is at present no theoretical or evidential reason to suspect that there was a random trial of possible cosmologies at all. If we were to possess a theory that did suggest such a random start to the universe (perhaps a theory of quantum gravity would, for example), then we might indeed have sufficient reason to introduce a probability measure and interpret it in this way.

Therefore, at the present, it seems none of these accounts recommend themselves as ways to interpret probability measures in cosmology. Indeed, this problem of interpretation infects the treatment of the universe itself as a statistical mechanical system similarly. The only possibility is that the initial conditions of the universe are random, something we have relatively little reason to believe (except through the artifice of inductively generalizing from familiar statistical mechanical systems to the universe at large). Moreover, insofar as probability assignments are determined by the proportionality postulate, i.e. probabilities are assigned to macrostates (macroprobabilities) and not just to microstates conditional on a given initial macrostate (as in the

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

[22] Werndl (2009, 2011, 2012, forthcoming) discusses the general observational equivalence of deterministic and indeterministic descriptions of a system in detail.

past hypothesis), the idea of stochastic initial conditions is even less compelling, since it is even less clear that macrostates, like the past "low entropy" state of the universe, are improbable—in other words it is even less plausible that there is a random element involved in selecting the initial *macrostate* of the universe. Demands to explain this improbability of an initial macrostate, as in (Penrose 1989a; Price 2002, 2004), are based on a highly dubious application and interpretation of probability to cosmology.[23]

4.2.3 Justification of Cosmological Likelihoods

Finally, let us consider the justification of cosmological likelihoods, and specifically a probability distribution on some given sample space of cosmologies, assuming that initial conditions are subject to randomness (since this seems to be the only possible interpretation). To be sure there are significant technical problems with supplying such a structure to these possible cosmologies, as I will discuss in §4.3 and §4.4. Even if these technical problems could be overcome, however, I argue that it is not possible to justify any particular cosmological probability measure without some other theory (of quantum gravity or of initial conditions) providing it.[24] At best one has an a prioristic justification of a likelihood measure (such as the Liouville measure) based on its naturalness.

The first (and admittedly obvious) point is that cosmological likelihoods cannot

[23]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 (2.5.2). It seems to me that one ought to simply reject the presupposition as ill-motivated and inadequately justified.

[24]I am therefore 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. Whether that postulation is justified—they describe it as an "unsupported hypothesis"—is what I examine in this section.

be empirically justified insofar as cosmology concerns a single, unique 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. Cosmological probability measures are therefore vastly underdetermined (at least in the absence of some trustworthy theory providing one) with no scientifically respectable way to break the underdetermination. For example, 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. If a choice is to be made, it seems that this is actually the most favorable measure by parsimony, but then one may as well do away with the fiction of there even being a probability measure at all.

Thus the only possibility of justifying a likelihood measure in cosmology is non-empirical. Unsurprisingly, then, several prominent cosmologists have reasoned in accord with epistemic, 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. Indeed, in practice many authors help themselves to the principle of indifference without justifying its applicability, both in statistical mechanics and in the context of cosmological probabilities (3.3.5).

A similar principle is invoked in assigning a uniform probability distribution with respect to the Liouville measure. For example, 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 do assume that the probabilities should be uniform with respect to the Liouville measure (which is not in general a probability measure). Similarly, Carroll and Tam (2010) explicitly note that the Liouville measure does not necessarily imply any particular probability measure: "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.

In both cases which I mention there is an assumption that the assignment of a uniform probability distribution is justified in statistical mechanics, and an inference from the justification of a uniform probability measure in statistical mechanics to the justification of a uniform probability distribution in cosmology. I suggested in §3.3.5 that a prioristic principles like the PI are not generally applicable, 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 (here again I am in agreement with (Shackel 2007; Norton 2008b; North 2010)). If this is correct, then the presupposition of the inference fails. There is also no independent, compelling support for the PI or its cousins in the specific case of cosmology (here I am in agreement with (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, again 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.

These considerations alone cast considerable doubt on the veridicality of probability measures. Furthermore, 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 (3.2.1), but as I argued in §4.2.1 one then faces a dilemma: either this space delimits the full space of possible cosmologies (which would be quite difficult to justify) 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 (4.3), cannot be then justified by the PI, naturalness, etc.). Therefore even if a justification of uniformity, by way of mathematical naturalness, the PI, etc., were possible in statistical mechanics, it would not carry over to the case of cosmological models. Thus the claim that the uniform probability distribution in cosmology is justified because of its success in justifying the uniform probability distribution in statistical mechanics is highly doubtful, however one thinks it is justified in statistical mechanics.

Even if there is no unique probability distribution that can be justified, it may nevertheless be the case that there is a natural measure on the space of possible cosmologies. This is the case with the GHS measure. As I pointed out in §3.3.5, however, mathematical naturalness does not guarantee physical significance. It is a distinct step to interpret this natural measure as a likelihood measure, one which requires some justification. After all, measures play various roles in a physical theory, especially as a standard for integration along trajectories. There is no reason to assume that a measure must play the role of a likelihood measure in any theory that comes equipped with one.

One may try to infer from the putative success of typicality arguments in statistical mechanics to their applicability in cosmology. Yet, if typicality arguments are successful in statistical mechanics, then they are because they depend importantly on the precise structure of phase space. One generally has empirical evidence that suggests the correct phase space for a system in statistical mechanics. One does not have this in the case of cosmology (because of 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.

4.3 Likelihood in the Solution Space of General Relativity

In the remaining two sections of this chapter I set aside the conceptual problems which I have raised in the previous section, and consider the prospects of simply rigorously defining some notion of likelihood on the space of cosmologies, without worrying about whether it makes much sense to do so. First I deal with the case where the space of possible universes is taken to be the solution space of GTR; second 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.

Recall that a relativistic spacetime $\mathcal{M}$ is a 4-manifold M that is Hausdorff, connected, and paracompact, and a Lorentzian metric g, is a rank 2 covariant metric tensor field associated with M which has Lorentz signature (1, 3).[25] I begin my discussion by pointing out a series of obvious facts that are however seldom if ever mentioned explicitly in discussions of the space of relativistic spacetimes. First, this space of possible cosmologies ranges over the set of topological 4-manifolds; moreover, for each topological 4-manifold, there is also a range of smoothness structures on these 4-manifolds; finally, for each smooth manifold, there is a ("kinematic") range of Lorentzian metrics, which range is restricted by the EFE to yield the possible ("dynamical") set of cosmologies.

There is presently relatively little that we can say about the structure of this large and complicated set. With specific modeling assumptions, this set 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

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

3-manifolds and 1-manifolds. This is the topic of §4.4.

If one were to want a complete likelihood measure on the full space of possible cosmologies (relativistic spacetimes), one would require a means of classifying 4-manifolds. The classification of 4-manifolds is a notoriously difficult problem (and distinctly so in comparison to other dimensions, where classification is established by geometrization or surgery) (Freedman and Quinn 1990; Donaldson and Kronheimer 1997). It is therefore 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, one that is typically overlooked in cosmological work, where the Euclidean smoothness structure is automatically presumed on $\mathbf{R}^4$.[26]

Is this underdetermination of spacetime topology or smoothness structure an epistemic threat? Manchak (2009, 2011) argues that we do face a substantial epistemic predicament in cosmology because of this kind of underdetermination, in particular because 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;

[26] "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).

Norton 2011; Butterfield 2012, 2014).

However in some cases we may have good reason to reject this topological underdetermination (Magnus 2005; McCoy forthcoming). We may, for example, have good reason 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.[27] 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. 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 could also have astrophysical effects (Sładkowski 2009). In these cases we may have good physical reasons to favor a particular choice of cosmological model.

I do not wish to take a particular stance here on whether underdetermination of differential and topological manifold structures is a serious epistemological problem. Too little is known. My point is merely that one must either justify a choice to break the underdetermination, or else one must incorporate it in some way when defining cosmological likelihoods. All accounts of cosmological likelihoods presently ignore the issue, 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

[27] To be sure, some assumptions have been made already to limit the topological 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).

discussed in the philosophical or physical literature.[28] Nevertheless, a complete account of likelihood for the space of possible cosmologies ought to say something about these issues. Unfortunately, owing to the state of the art, little can be said definitively without considerable restriction of the general case. Thus this lacuna casts considerable doubt on an the viability of any general claim asserting the existence of 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 certain field theory on M using the framework of covariant classical field theory from §3.2.2 (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 topologizing it, for example as 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 (forthcoming) observes that some physicists have advocated a particular topology as appropriate for discussing similarity relations in general relativity. For

[28]Debates on conventionalism, the unity of space, and to some extent underdetermination do concern these specific kinds of underdetermination (Reichenbach 1958; Glymour 1972; **?**; Nerlich 1994; Torretti 1996; Callender and Weingard 2000; McCabe 2004; Magnus 2005).

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 forthcoming; 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 to the type of question at hand, ones that relevantly similar space-times should share" (Fletcher forthcoming, 15).[29] 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 obviously represent the space of solutions to the EFE anyhow, since one may interpret this space as redundantly representing physically distinct spacetimes.[30] The EFE are diffeomorphism covariant. Thus, 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 EFE, it follows that the pullback of g to M, i.e. $\varphi^* g$, is a solution to the EFE as well. Many interpret such

[29] Hawking (1971, 396) advocates a similar pluralism: "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."

[30] In this vein, there has been much discussion of whether it is appropriate to regard GTR as a "gauge theory" (Weinstein 1999; Teh forthcoming), particularly due to dissimilarities with Yang-Mills Theory (Earman 2003; Redhead 2003; Weatherall forthcoming a). 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 significance of gauge is (Healey 2007; Guay 2008; Rovelli 2014).

diffeomorphic spacetimes as "equivalent" in the specific sense that they represent the *same* physical spacetime, since they have the same observable consequences, etc.[31] 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.[32] 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.[33]

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.[34] 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

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

[32] 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) 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 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 not a problem.

[33] As discussed in §4.2.1, 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 EFE is taken to be "pure gravity" spacetimes, i.e. spacetimes that satisfy the vacuum EFE. 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.

[34] "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).

neighborhood of symmetric spacetimes (Fischer et al. 1980; Arms et al. 1982).[35] 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 sufficiently well-motivated for specific applications, as Fletcher (forthcoming) 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 forthcoming).

Also, such solution spaces will generally be infinite-dimensional, essentially because spacetimes have an infinity of degrees of freedom (Isenberg and Marsden 1982, 181).[36] This presents a problem for a measure-theoretic approaches, a problem to which Curiel (2015) draws attention. As he says, "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:

[35] 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.

[36] "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).

> 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.

In conclusion, the remarks in this section serve to indicate 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. Finally, as there remains much that is unknown about these components, some caution is clearly warranted in making claims that concern the entirety of this possibility space, even setting aside the serious interpretational and justificatory challenges discussed above.

4.4 Likelihood in FRW Spacetimes

As a way of avoiding the difficulties surveyed in the previous section, one may wish to limit one's attention to the simpler finite-dimensional cases, and hope that the results are consistent with more general cosmological possibility spaces. 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 (1.3), and therefore is a set of cosmologies that may represent our own universe; a scalar field is chosen as the matter content to represent the inflaton (2.4) because their aim is to investigate fine-tuning questions related to inflation.

4.4.1 The Gibbons-Hawking-Stewart Measure

Following the procedure of §3.2.1, we must first identify the appropriate phase space Γ for FRW spacetimes. To do this we need to look to the hamiltonian formulation of GTR, which is based on the initial value formulation of GTR which was briefly surveyed in §1.3.3. The Einstein equation simplifies considerably in the initial value formulation, reducing to two constraint equations and two evolution equations. It is, accordingly, a constrained hamiltonian system.

In the initial value formulation of GTR the "position" initial data are represented by the spatial metric h_{ab} and the "momentum" initial data by the extrinsic curvature π_{ab} (since $\pounds_{\nabla t}(h_{ab}) = 2\pi_{ab}$ (1.3.3)). FRW spacetimes are spacetimes with homogeneous and isotropic spacelike hypersurfaces, so we can foliate the spacetimes by a one-parameter family of these spacelike hypersurfaces Σ_t that are orthogonal to $\xi^a = \nabla^a t$, which is a future-directed, twist-free, unit timelike field on M (1.3.2).[37] The extrinsic curvature of an initial data surface Σ_t is Hh_{ab} (1.3.3), where H is the Hubble parameter; thus the initial data for an FRW spacetime are represented by both the spatial metric h_{ab} and the Hubble parameter H associated with a spatial hypersurface Σ.

The space of initial data is the product of the set of homogeneous and isotropic Riemannian 3-manifolds Σ and the set of Hubble parameters H. Homogeneous and isotropic Riemannian manifolds have constant curvature κ; complete, connected Riemannian manifolds of constant sectional curvature are called space forms. It is a theorem that every simply-connected three-dimensional space form is isometric to the sphere $S^3(\sqrt{(}1/\kappa))$ if $\kappa > 0$, $\mathbf{R}^3$ if $\kappa = 0$, or the hyperbolic space $H^3(\sqrt{(}1/\kappa))$ if $\kappa < 0$. The standard metrics on each of these manifolds is understood to be the metric induced on them by embedding them in $\mathbf{R}^4$. Every Σ is therefore isometric to one of these three classes of space forms. Spaceforms of each of the three kinds are moreover homothetic, i.e. they are isometric up to a scale factor a^2. Accordingly we may represent curvature κ as a function of the scale factor; namely, for any Σ, $a^2\kappa$ is a constant k. Then we can set any spatial metric $h_{ab} = a^2\gamma_{ab}$, where γ_{ab} is the standard metric on the appropriate space form, and all time dependence is located in the scale factor.

As said, the Einstein equation reduces to two constraint equations and two

[37]The scalar field t representing time may of course be re-scaled by a number N on every Σ_t, known as the "lapse" field. In developing the initial value and hamiltonian formulations of GTR one makes use of N as well as the "shift" vector field N^a.

evolution equations in the initial value formulation ((1.26)-(1.29)):

$$\mathcal{R} - (\pi_a{}^a)^2 + \pi_{ab}\pi^{ab} = -16\pi T_{ab}\xi^a\xi^b \tag{4.2}$$

$$D_c\pi_a{}^c - D_a\pi_c{}^c = 8\pi T_{mr}h^m{}_a\xi^r \tag{4.3}$$

$$\pounds_\xi(\pi_{ab}) = 2\pi_a{}^c\pi_{cb} - \pi^c{}_c\pi_{ab} + \mathcal{R}_{ab} - 8\pi h_a{}^m h^n{}_b(T_{mn} - \frac{1}{2}Th_{mn}) \tag{4.4}$$

$$\pounds_\xi(h_{ab}) = 2\pi_{ab}, \tag{4.5}$$

where $\mathcal{R}$ is the Ricci scalar of Σ, $\mathcal{R}_{ab}$ is the Ricci tensor of Σ, and D_a is the derivative operator on Σ. For FRW spacetimes, these equations reduce to the following three (the second equation is trivial since π_{ab} does not vary across Σ:

$$\mathcal{R} - 6H^2 = -16\pi\rho \tag{4.6}$$

$$\dot{H}h_{ab} = \Big(- H^2 - \frac{4\pi}{3}(\rho + 3p)\Big)h_{ab} \tag{4.7}$$

$$\dot{h}_{ab} = 2Hh_{ab}, \tag{4.8}$$

where ρ is the energy density and p the pressure of the matter. The first equation is the Friedman equation (1.3) and the second is the Friedman acceleration equation (1.4). Since $h_{ab} = a^2\gamma_{ab}$, $\dot{h}_{ab} = 2a\dot{a}\gamma_{ab}$, and $2Hh_{ab} = 2Ha^2\gamma_{ab}$, it follows from the third equation above that

$$H = \frac{\dot{a}}{a}, \tag{4.9}$$

as the usual definition of H has it.

Taking the matter contents of spacetime to be the inflaton and representing it as a scalar field ϕ in a potential V that evolves according to the coupled Einstein-Klein Gordon equation, we have the following equations of motion (Hawking and Page 1988, 790):

$$\mathcal{R} - 6H^2 = -16\pi\left(\frac{1}{2}\dot{\phi}^2 + V(\phi)\right); \tag{4.10}$$

$$\dot{H} = -4\pi\dot{\phi}^2 - \mathcal{R}/6; \tag{4.11}$$

$$\ddot{\phi} + 3H\dot{\phi} + V'(\phi) = 0, \tag{4.12}$$

where V' is the derivative of the potential with respect to ϕ.[38] The third equation can be derived from the previous two, however, so it is redundant.

For FRW spacetimes the spatial Ricci scalar is $\mathcal{R} = -6\kappa$. As noted before, we can cast κ in terms of the scale factor and a constant k: $\kappa = k/a^2$. By introducing a as a degree of freedom to replace κ as a degree of freedom, we have introduced an arbitrary constant k which has no physical significance beyond identifying whether the space form is flat, positively-curved, or negatively-curved. We therefore take equivalence classes in these three cases and choose $k = +1$, 0, and -1 as representatives, i.e. so that $k \in \{+1, 0, -1\}$ as is standard. Thus we finally write $\mathcal{R} = -6k/a^2$, and we have Friedman's equations in their usual form for a scalar field in a potential:

$$\left(\frac{\dot{a}}{a}\right)^2 = \frac{8\pi}{3}\left(\frac{1}{2}\dot{\phi}^2 + V(\phi)\right) - \frac{k}{a^2}; \tag{4.13}$$

$$\dot{H} = -4\pi\dot{\phi}^2 + \frac{k}{a^2}. \tag{4.14}$$

The foregoing development shows that our initial data h_{ab} and π_{ab} are therefore equivalently representable in the space $\{a, \dot{a}, \phi, \dot{\phi}, k\}$. This space is not the space of initial data, however, since the Friedman equation (the first one) is a constraint that

[38] If our interest were solely in assessing the HBB model's fine-tuning, we could do the following analysis for perfect fluid matter contents. The results would be qualitatively similar however, as shown by Carroll and Tam (2010, §4.2).

must be satisfied by initial data. One must also keep in mind that k is an index for three separate copies of the space $\{a, \dot{a}, \phi, \dot{\phi}\}$. There is no continuous path between the three spaces.

Now that we have identified the relevant spaces for representing FRW space forms, we put the theory into a hamiltonian formulation (Wald 1984, Appendix E). We begin with the lagrangian for our theory of FRW spacetimes with a scalar field as the matter contents, where we have re-introduced the lapse function N which serves as a Lagrange multiplier:

$$\mathcal{L} = \sqrt{-g}\left(\frac{R}{16\pi} + \frac{1}{2N^2}\dot{\phi}^2 - V(\phi)\right). \tag{4.15}$$

In terms of the variables we have chosen, this is

$$\mathcal{L} = -\frac{1}{8\pi}(\frac{3}{N}a\dot{a}^2 - 3Na^3\frac{k}{a^2}) + \frac{1}{2N}a^3\dot{\phi}^2 - Na^3V(\phi), \tag{4.16}$$

in agreement with (Hawking and Page 1988; Gibbons and Turok 2008; Carroll and Tam 2010). The momenta of a and ϕ are

$$p_a \equiv \frac{\partial\mathcal{L}}{\partial\dot{a}} = \frac{-3a\dot{a}}{4\pi N}; \qquad p_\phi \equiv \frac{\partial\mathcal{L}}{\partial\dot{\phi}} = \frac{a^3\dot{\phi}}{N}. \tag{4.17}$$

The hamiltonian on this phase space is

$$\mathcal{H} = p_a\dot{a} + p_\phi\dot{\phi} - \mathcal{L} = N\left(-\frac{2\pi p_a^2}{3a} + \frac{p_\phi^2}{2a^3} + a^3V(\phi) - a^3\frac{3}{8\pi}\frac{k}{a^2}\right), \tag{4.18}$$

from which we recover (after setting $N = 1$) our constraint (the Friedman equation) as the hamiltonian constraint $\mathcal{C}$:

$$\mathcal{C} \equiv -\frac{2\pi p_a^2}{3a} + \frac{p_\phi^2}{2a^3} + a^3 V(\phi) - a^3 \frac{3}{8\pi}\frac{k}{a^2} = 0. \tag{4.19}$$

The phase space γ of our system is thus the four-dimensional space $\{a, p_a, \phi, p_\phi\}$ equipped with the canonical symplectic form

$$\omega_{p_a,a,p_\phi,\phi} = \mathrm{d}p_a \wedge \mathrm{d}a + \mathrm{d}p_\phi \wedge \mathrm{d}\phi, \tag{4.20}$$

The dynamically accessible phase space points however are constrained to be on the three-dimensional hypersurface $\mathcal{C}$. Thus it would be inappropriate to use ω for constructing a canonical volume measure on phase space. We can pull the symplectic form onto the constraint surface by first solving the constraint for p_ϕ:[39]

$$p_\phi = a^3\left(\frac{4\pi}{3}\frac{p_a^2}{a^4} + \frac{3}{4\pi}\frac{k}{a^2} - 2V(\phi)\right)^{1/2}. \tag{4.21}$$

Following Carroll and Tam, we also switch coordinates from p_a to H, so that

$$p_\phi = a^3\left(\frac{3}{4\pi}(H^2 + k/a^2) - 2V(\phi)\right)^{1/2}, \tag{4.22}$$

and

$$\mathrm{d}p_a = -\frac{3}{4\pi}\left(2aH\mathrm{d}a + a^2\mathrm{d}H\right). \tag{4.23}$$

[39]The scalar field can have positive or negative momentum, so strictly speaking there should be a $\pm$ in the following equation. The reader is welcome to annotate the equations that follow.

The differential of p_ϕ is then

$$\mathrm{d}p_\phi = \frac{(3/4\pi)a^3H\mathrm{d}H - a^3V'\mathrm{d}\phi + 6a^2((3H^2+2k/a^2)/8\pi - V)\mathrm{d}a}{((3/4\pi)(H^2+k/a^2)-2V)^{1/2}}. \tag{4.24}$$

Substituting these into ω then gives the pullback of the symplectic form onto $\mathcal{C}$. The result is the following (pre-symplectic) differential form:

$$\omega_{a,H,\phi} = \Theta_{Ha}(\mathrm{d}H\wedge\mathrm{d}a) + \Theta_{H\phi}(\mathrm{d}H\wedge\mathrm{d}\phi) + \Theta_{a\phi}(\mathrm{d}a\wedge\mathrm{d}\phi), \tag{4.25}$$

where

$$\Theta_{Ha} = -\frac{3}{4\pi}a^2; \tag{4.26}$$

$$\Theta_{H\phi} = \frac{(3/4\pi)a^3H}{((3/4\pi)(H^2+k/a^2)-2V)^{1/2}}; \tag{4.27}$$

$$\Theta_{a\phi} = \frac{6a^2((3H^2+2k/a^2)/8\pi - V)}{((3/4\pi)(H^2+k/a^2)-2V)^{1/2}}. \tag{4.28}$$

This form is not symplectic (it is degenerate), so we cannot construct a natural volume measure on $\mathcal{C}$. Ideally, the "real" phase space of our system would be given by "solving the dynamics," and then taking equivalence classes of phase points that are part of the same trajectory. In this way we would obtain the space of motions and hence could pull the degenerate form back onto the space of motions to obtain a new symplectic form (of degree two less than ω), from which we could construct a canonical measure on this new phase space, again, the space of motions. Without specifying the potential V, this is not possible. The approach GHS take instead is to set H to some arbitrary value H_* in the differential form and define their measure accordingly, i.e. set

$$\mathrm{d}\Omega = \omega_{a,H,\phi}|_{H=H_*} = \Theta_{a\phi}|_{H=H_*}\mathrm{d}a\mathrm{d}\phi. \tag{4.29}$$

Finally, the GHS measure μ_{GHS} is defined on Lebesgue measurable sets U by

$$U \mapsto \int_U \mathrm{d}\Omega = -6\int_U a^2 \frac{(3H_*^2 + 2k/a^2)/8\pi - V}{((3/4\pi)(H_*^2 + k/a^2) - 2V)^{1/2}}\mathrm{d}a\mathrm{d}\phi. \tag{4.30}$$

This expression of the GHS measure is equivalent to those derived in (Carroll and Tam 2010; Schiffrin and Wald 2012).[40]

4.4.2 The Flatness Problem

The GHS measure (4.30) diverges for large scale factors (its integrand has a factor of a^2). This point was originally recognized by Gibbons et al. (1987, 745); it also converges to 0 for small scale factors. Due to the divergence, one might say that for any choice of Hubble parameter H_* almost all spacetimes will have a "large" scale factor. More precisely, pick any scale factor a_*; the set of spacetimes with $a < a_*$ is a negligible set (because the total measure of this set is finite). Thus it seems as if one can make typicality claims on the basis of the GHS measure.

What is the significance of this fact about the GHS measure, specifically for the flatness problem? The flatness problem, recall, arises because flatness appears to be a special spatial geometry among the set of FRW spacetimes. Space can be curved to any degree, so zero curvature seems unlikely on the face of it. Hawking and Page

[40] There are some complications with the $k = 1$ case. See (Schiffrin and Wald 2012, 8) for the details. Note that I have also chosen not to set $8\pi G = 1$, but rather maintained consistency with the rest of this dissertation's use of "geometrical units" by only setting $G = 1$. Also, Gibbons et al. (1987) use a simplifying, but less transparent coordinate choice. They choose to investigate only the special case where $V = m^2\phi^2/2$ too. Nevertheless it can be shown with some work that their expression is equivalent to this one as well (with their potential substituted for V).

argue that, on the contrary, the GHS measure shows that flatness is in fact highly likely among FRW spacetimes:

> Thus for arbitrarily large expansions (and long times), and for arbitrarily low values of the energy density, the canonical measure implies that almost all solutions of the Friedmann-Robertson-Walker scalar equations have negligible spatial curvature and hence behave as $k = 0$ models. In this way a uniform probability distribution in the canonical measure would explain the flatness problem of cosmology... (Hawking and Page 1988, 803-4)

By "arbitrarily large expansions" (and "arbitrarily low values of energy density") they mean what was just written before. Pick any arbitrary a_* (and any arbitrary ϕ_*). Then, according to the GHS measure, almost all spacetimes have $a > a_*$ (and $\phi > \phi_*$), or, equivalently, the spacetimes with $a < a_*$ (and $\phi < \phi_*$) compose a negligible set.[41] Furthermore, since this holds for any choice of a_*, one may infer that almost all spacetimes are arbitrarily close to having $\kappa = 0$. It is somewhat misleading to say that curved FRW spacetimes with large scale factors "behave as $k = 0$ models;" it is however outrightly false to say that a "uniform probability distribution" with resepct to the GHS measure would explain the flatness problem of cosmology. There is in fact no such uniform probability distribution, since the GHS measure is not finite. Moroever, as noted in §3.2.1, there is also no canonical probability distribution ρ at all which would make $U \mapsto \int_U \rho d\Omega_{GHS}$ into a probability measure. As noted then, one has to make a choice in order to obtain a probability measure in such a circumstance.

Carroll and Tam (2010, 14) invite us to consider the question in more "physically transparent" terms by recasting the measure in terms of the curvature κ (which we previously exchanged in favor of the scale factor a when deriving the GHS measure). One can replace the scale factor a by the curvature κ using the relation from before,

[41] Gibbons and Turok (2008, 6) point out that ϕ is always bounded given H_*, so it is not necessary to pick an arbitrary ϕ_*.

namely $\kappa = k/a^2$. (Note that this change of coordinates maps the entire set of scale factors for the $k = 0$ case to the single point κ.) One then defines the GHS measure for curved FRW spacetimes by the map

$$U \mapsto \int_U \mathrm{d}\Omega = -6 \int_U \frac{1}{|\kappa|^{5/2}} \frac{(3H_*^2 + 2\kappa)/8\pi - V}{((3/4\pi)(H_*^2 + \kappa) - 2V)^{1/2}} \mathrm{d}\kappa \mathrm{d}\phi. \tag{4.31}$$

It is clear that the measure diverges for small values of curvature, i.e. curvatures close to flat, due to the curvature term in the denominator. This is pointed out by Carroll and Tam. They suggest the following interpretation of this fact:

> Considering first the measure on purely Robertson-Walker cosmologies (without perturbations) as a function of spatial curvature, there is a divergence at zero curvature. In other words, curved [FRW] cosmologies are a set of measure zero—the flatness problem, as conventionally understood, does not exist. (Carroll and Tam 2010, 15)

Both of these claims (that of Hawking and Page, and that of Carroll and Tam) are as stated quite dubious. In the first place, the flatness problem in some sense does exist when understood as a stability problem (§2.2.2), so their claims only hold under the specific interpretation of the problem as a likelihood problem. Understanding the flatness problem as a stability problem is in fact relatively common (Baumann 2009), so it is unclear whether the likelihood interpretation is the truly "conventional understanding" of the flatness problem. Nevertheless, I did argue in chapter 2 that the flatness problem appears to be best characterized as a likelihood problem, since it can subsume other interpretations and the bulk of commentary on the problem suggests it strongly. In any case, Carroll and Tam assume that the flatness problem is conventionally understood in this way. They then claim that the problem is in fact illusory since the canonical GHS measure—according to them—shows that FRW spacetimes are generically spatially flat.[42]

Secondly, Carroll and Tam assert that all values of their curvature coordinate Ω_k (essentially equivalent to κ) can be integrated over. Depending on how one treats the $k = 0$ case, this claim may be true. Regardless, portraying the phase space in terms of curvature as they do is misleading. For curved FRW spacetimes it is true that the measure diverges for small values of curvature κ, as I noted above and as Hawking and Page suggest in the passage from their paper quoted above. But the recast measure is infinite *at* zero curvature merely because we have mapped the entire set of $k = 0$ scale factors to $\kappa = 0$. It is infinite at zero curvature however only because the GHS measure diverges for large scale factors, just as it does for curved FRW spacetime. For this reason it is misleading to describe a "divergence at zero curvature;" there is nothing special going on in flat FRW spacetimes (at least in this respect).[43]

Finally, and relatedly, curved FRW spacetimes are *not* a set of measure zero as Carroll and Tam claim—at least according to the GHS measure.[44] Recall that the initial data of FRW spacetimes is representable in the k-indexed phase space $\{a, \dot{a}, \phi, \dot{\phi}\}$. The curvature constant k serves as an index for *three different phase spaces*, each of which has an infinite total measure—even after taking into account constraints and choosing a hypersurface in the constraint surface according to GHS's procedure. The unboundedness of the total phase space measure for each kind of FRW spacetime is due, again, to the unbounded range of the scale factor Schiffrin and Wald

[42]Hawking and Page probably meant this as well, as would be plain if they had just written that "the canonical measure would explain the flatness problem of cosmology" and left off mention of a probability measure. The same basic claim is made as well by Coule (1995, 468).

[43]Carroll and Tam appear to equivocate several times between there being a divergence *at* $\kappa = 0$ and the measure diverging *as* $\kappa \to 0$: "The integral diverges near $[\kappa = 0]$, which is certainly a physically allowed region of parameter space" (Carroll and Tam 2010, 17); "The measure diverges on flat universes" (Carroll and Tam 2010, 28). These are obviously not equivalent statements

[44]For sure they are in some measure appropriate to a larger set of spacetimes—recall the discussion of reference class in §4.2.1.

(2012, 11).[45] This point is quite plain when one expresses the GHS measure in terms of the scale factor. Transforming to the curvature coordinate κ should not change the fact that the total measure of each of these phase spaces is infinite. So, while it is true that the GHS measure attributes infinite measure to flat FRW spacetimes (as Carroll and Tam appear to recognize), it also does so both to positively curved FRW spacetimes and to negatively curved spacetimes. Therefore it is absolutely not the case that the curved FRW cosmologies are a set of measure zero (or even a negligible set) according to the GHS measure. Hence one cannot conclude that the flatness problem does not exist.

One might try to rescue Carroll and Tam's claim about the flatness problem by choosing some set of "nearly flat" curved spacetimes, e.g. the set of spacetimes with curvature less than some κ_* (at some time corresponding to Hubble parameter H_*).[46] Almost all spacetimes will have a "small" curvature κ in comparison to this curvature κ_*. In other words, the set of spacetimes with $\kappa > \kappa_*$ is a negligible set. Since our universe's spatial curvature is "nearly flat," i.e. intuitively it should be less than κ_* (whatever it is), it then follows that our universe is actually typical, contra what is assumed in the flatness problem. In this case it would be correct to conclude that the flatness problem does not exist. Unfortunately this argument does not follow from the GHS measure alone, since we have had to make an independent choice in choosing κ_*, a choice that is not natural in any obvious sense. Moreover, it is doubtful that there is

[45] Besides in (Schiffrin and Wald 2012), this fact is correctly pointed out in (Gibbons et al. 1987; Hawking and Page 1988). While Carroll and Tam (2010, 20-1) observe that "this divergence was noted in the original GHS paper, where it was attributed to 'universes with very large scale factors' due to a different choice of variables," they object to this as an interpretation: "This is not the most physically transparent characterization, as any open universe will eventually have a large scale factor." For this reason they exchange the scale factor for curvature; it is not clear, however, how this characterization is more physically transparent since it amounts to the same thing.

[46] Indeed, Gibbons and Turok (2008) propose to introduce just such a "cutoff," albeit for somewhat different purposes. I discuss their approach below.

any reasonable argument to justify a particular choice of κ_*—an explication of "close to flat" in the language of GTR and in the context of FRW models; it appears to be a completely arbitrary choice in this context...and certainly not all choices of κ_* would count our universe as flat.

Let us try a slightly different tack into the same headwind then. Suppose κ_* is the (non-zero) spatial curvature of our universe at the present time. The GHS measure can be used to infer that almost all spacetimes with the same Hubble parameter will have flatter spatial curvatures. In such circumstances, one might be inclined to wonder "Why is my universe's spatial curvature so large? It seems like it ought to be much smaller if my universe is typical!" In other words, instead of a flatness problem, it seems like we actually have a curvature problem! Of course the same problem arises here, since one would say this for any κ_* whatever, regardless of its magnitude. Thus it is not clear how one would ever be in the position to be satisified with one's curvature in an FRW universe—at least insofar as one expects things in our universe to be typical (in accord with Copernican principle-style reasoning).

No matter. The measure suggests a question. What is the answer? Well, the only answer the theory gives is that the curvature of our universe depends on its actual dynamical history. It has no other explanation within the theory. Appearances to the contrary are deceiving, since there is evidently no such thing as a typical FRW spacetime. To think that the Liouville can explain typicality in the theory depends on an adequate justification that it can be treated as a typicality measure (3.3.5), particularly if one thinks that the GHS measure can be used as a probability measure, as Carroll and Tam evidently do:

> When we consider questions of fine-tuning, however, we are comparing the real world to what we think a randomly-chosen history of the universe

would be like. (Carroll and Tam 2010, 11)

Some popular conceptions (in physics and beyond) of statistical mechanics encourage this line of thought, specious though they be. What we think a random choice looks like only has epistemic value when what we think a random choice is accords well with apparent randomness in the world. Moreover, putatively successful typicality arguments in statistical mechanics (Goldstein 2012) depend not only on having a phase space measure, but also on both the dynamics of the system and especially on a specification of macroproperties or macrostates (defined as regions of phase space) (Frigg 2009; Frigg and Werndl 2012). Thus any claim of fine-tuning in FRW spacetimes on the basis of the GHS measure (which does at least incorporate the FRW dynamics) is bound to be undermined without additional justifiable assumptions about what is in fact typical in cosmology—yet this is precisely something which we have no hope of knowing in cosmology due to the uniqueness of the universe.[47]

Gibbons and Turok (2008, 6) correctly observe that universes with large scale factors are universes with small spatial curvatures. They then claim that the scale factor is neither "geometrically meaningful" nor "physically observable," and therefore propose to identify all the "indistinguishable" nearly flat spacetimes on the surface identified by H_*.[48] They do so by effectively choosing a "cutoff" curvature κ_* and

[47]Gibbons et al. (1987, 736) relate that "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." Consider that any individual universe is "not generic" according to the GHS measure, since any phase space point is measure zero. Does every "initial condition" therefore require explanation? The GHS measure also cannot tell us whether $k = 1, 0$, or -1 is generic, since each corresponding set has infinite measure. Should k be explained or not?

[48]It is not clear what they mean by "geometrically meaningful." The scale factor is clearly geometric in the relevant sense, since it relates spaceforms of the same kind by scalings. It is moreover physically meaningful because space is expanding (or contracting) in FRW spacetimes. The precise value of a does not matter, as it can be re-scaled, but that does not undermine its meaningfulness. It is also

throwing out all the spacetimes with curvatures smaller than it. The advantage to doing this is that the total measure of FRW spacetimes with curvatures larger than κ_* is finite.

The disadvantage is that this makes no sense. Carroll and Tam (2010, 20) comment, "to us, this seems to be throwing away almost all the solutions, and keeping a set of measure zero. It is true that universes with almost identical values of the curvature parameter will be physically indistinguishable, but that doesn't affect the fact that almost all universes have this property." Indeed, doing what Gibbons and Turok do is throwing away almost all the solutions (although contra Carroll and Tam the remaining set has finite measure, not measure zero). They are also right to point out that if nearly flat universes are physically indistinguishable, so are "nearly-κ" universes for almost any κ. Gibbons and Turok do not throw out these universes however (else they would not have been left with any universes at all).

Ironically Carroll and Tam effectively make the same error as Gibbons and Turok, essentially by identifying the flat and nearly flat spacetimes. Instead of throwing out all the flat and nearly flat spacetimes like the latter pair, however, the former pair throws out the complement of the flat and nearly flat spacetimes. They then conclude that nearly all spacetimes are essentially flat. In particular, Carroll and Tam propose to tame the divergence in the GHS measure by regularizing the integral, i.e. making the measure finite. Since the GHS measure is not finite, regularizing it makes it no longer the GHS measure, in which case any justification the measure had (by its naturalness) is lost.[49] One may as well have assumed the probability distribution

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unclear how the fact that a is physically unobservable should matter, since most features of spacetime are not observable, e.g. the metric g, the spatial curvature κ, etc. The physically relevant content of these, including the scale factor, can be inferred from observations and appropriate theoretical assumptions.

[49]Carroll more recently has conceded the artificiality of regularizing: "Earlier attempts to regularize

they end up with right from the beginning.[50] In any case, their justification for this move is pragmatic: "This non-normalizability is problematic if we would like to interpret the measure as determining the relative fraction of universes with different physical properties" (Carroll and Tam 2010, 17). However this pragmatic motivation is obviously an inadequate justification for radically modifying the GHS measure yet claiming their measure retains its naturalness.

To sum up the considerations raised in this section, there is a dilemma facing accounts that wish to use the GHS measure to explain away the flatness problem. If they wish to claim that there is no flatness problem, either there is no flatness problem according to the GHS measure alone or there is no flatness problem because some additional assumptions must be made. As tempting as it may be to say that "almost all FRW spacetimes are flat or nearly flat" according to the GHS measure, there is no way to state "nearly flat" in a natural way and it is false that almost all FRW spacetimes are flat alone. Thus the GHS measure alone cannot explain away the flatness problem. If additional assumptions need be made, then those additional assumptions require justification, for they would not be natural in the way that the GHS measure is (else they would be incorporated into the phase space from the beginning and therefore be correctly incorporated into the measure). No reasonable

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the measure, for example by considering an ϵ-neighborhood around the zero-curvature hamiltonian constraint surface (Carroll and Tam 2010) or by identifying universes with similar curvatures (Gibbons and Turok 2008) have not proven satisfactory" (Remmen and Carroll 2013, 7). He however remains convinced that almost all FRW spacetimes are "nearly flat:" "we should throw all of the others away and deal with flat universes," (Carroll forthcoming, 19); "the divergence for flat universes is an indication that, in the canonical measure, almost all cosmological spacetimes are flat" (Remmen and Carroll 2014, 1).

[50] Interestingly, there is a strong parallel with "natural" justifications of statistical mechanical measures uniform with respect to the Lebesgue or Liouville measure over some macrostate (Maudlin 2007b). These are not equivalent to or derived from the Lebesgue or Liouville measure in any sense; thus they do not benefit from the naturalness of this measure in their justification (as far as that goes). They are distinct probability measures associated with phase space which merely use the Lebesgue measure for convenience of definition.

justification has been offered so far for such additions, nor does it seem that there is one available, even setting aside the basic concern that mathematical naturalness does not guarantee physical significance. I therefore conclude that claims which state that there is no flatness problem according to the canonical measure are completely unsupported, and moreover it appears unlikely that any such claim can be sustained.

It does not follow, however, that there is a flatness problem understood as a likelihood problem. That would require the demonstration of an appropriate measure of likelihood and a justification that it is a likelihood measure. Insofar as that justification must be empirical, then there is no appropriate measure of likelihood in a one-universe cosmology. Insofar as that justification may be other than empirical, then perhaps there is an appropriate measure. These justifications, however, do not themselves admit of justification, if the situation in statistical mechanics is any indication (§3.3.5). Theoretical support which indicates that flat spacetimes are unlikely is required to explicate the flatness problem as a likelihood problem 3.1. At least in the case of FRW spacetimes, this support is apparently not to be expected.

4.4.3 The Likelihood of Inflation in FRW spacetimes

In the previous section I noted that there is no natural condition that picks out "nearly flat" FRW spacetimes, and therefore the GHS measure cannot be used to assess likelihoods in a way that would either support or dissolve the flatness problem. There is at least a precise condition, however, for when inflation occurs: $\ddot{a} > 0$, or $\dot{\phi}^2 < V(\phi)$ (§2.4), and so one can (potentially) pose precise questions about inflating spacetimes. In terms of our phase space variables, inflation occurs when

$$\frac{1}{4\pi}\left(H^2 + \frac{k}{a^2}\right) < V(\phi). \tag{4.32}$$

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. We also require 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.

To assess the flatness problem I considered two cases: either the flatness problem concerns strict flatness, i.e. $k = 0$, or near flatness, i.e. κ small. The only precise explication of "flat" is $k = 0$. Since the set of flat FRW universes has infinite measure and the set of curved FRW universes has infinite measure, no conclusion about likelihoods of flatness can be drawn on the basis of the GHS measure. Any explication of "nearly flat" requires an arbitrary choice of curvature κ_*, a choice on which any assessment of fine-tuning depends strongly. To answer questions concerning the likelihood of inflation, the same cases must be considered.

There are two questions (§3.1): Is flatness a generic outcome of inflation? Are inflating spacetimes generic?

Consider the first question (Ellis 1988): "Is flatness a generic outcome of inflation?" 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 curvature fine-tuning.

This leads to the second question: "Do FRW spacetimes generically undergo inflation?" To answer this question we should strictly speaking include the other matter constituents of the universe. In any case, what one seemingly wants to know by asking this question is not how likely it is that an FRW universe undergoes inflation, but how likely it is that an FRW universe undergoes *sufficient inflation to solve the flatness problem*.[51] 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.

In any case, 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 discussed in the previous chapter (§2.4). In this case the GHS volume element simplifies to

$$\mathrm{d}\Omega \propto a^2\sqrt{\frac{3}{4\pi}H_*^2 - m^2\phi^2}\,\mathrm{d}a\mathrm{d}\phi. \tag{4.33}$$

Schiffrin and Wald (2012, 11) 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

[51] 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 I explain in the previous chapter, the horizon problem is only a problem because it seems to be a constraint on solving the flatness problem and the uniformity problem.

the ones for which $|\phi| \gtrsim 2\sqrt{N}$. Thus the GHS-measure of this set is proportional to

$$\int a^2 \,\mathrm{d}a \int_{2\sqrt{N}}^{\sqrt{3/4\pi}H_*/m} \sqrt{\frac{3}{4\pi}H_*^2 - m^2\phi^2}\,\mathrm{d}\phi. \tag{4.34}$$

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 have infinite GHS-measure. Therefore the likelihood of inflation is indeterminate, as expected.

Both Carroll and Tam (2010) and Gibbons and Turok (2008) attempt to overcome this problem with the GHS measure by modifying it (as I discussed previously). They also choose to evaluate the likelihood of inflation at different values of H_* and come up with different answers. It is not surprising that they do, since dealing with the divergence in the GHS measure requires making some choice to make the measure finite. Once again, there is no canonical choice. Different choices will lead to different results (Schiffrin and Wald (2012) thoroughly discuss this issue in their paper).

To illustrate the point, consider the following approach. 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

$$\rho = \frac{1}{\int \mathrm{d}\Omega}. \tag{4.35}$$

Then μ_ρ is simply

$$\frac{\int a^2 \,\mathrm{d}a \int_{2\sqrt{N}}^{\sqrt{3/4\pi}H_*/m} \sqrt{\frac{3}{4\pi}H_*^2 - m^2\phi^2}\,\mathrm{d}\phi}{\int \mathrm{d}\Omega} = \frac{\int_{2\sqrt{N}}^{\sqrt{3/4\pi}H_*/m} \sqrt{\frac{3}{4\pi}H_*^2 - m^2\phi^2}\,\mathrm{d}\phi}{\int \sqrt{\frac{3}{4\pi}H_*^2 - m^2\phi^2}\,\mathrm{d}\phi.}, \tag{4.36}$$

since the integral over the scale factor drops out. This measure is not invariant under time evolution however because part of the measure has been effectively thrown away. Depending on how one chooses H_* one will compute wildly different probabilities of inflation (Schiffrin and Wald 2012).[52]

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)

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 are not justified by the physics of FRW spacetimes alone. Thus we should not believe that inflating spacetimes are generically flat based on likelihood arguments. Rather we should believe that inflating spacetimes flatten spatial curvature because of "no-hair"-like theorems (§1.3.4).

4.4.4 The Uniformity Problem

For various reasons one may not take the results concerning likelihoods in FRW spacetimes too seriously. Carroll and Tam (2010, 21) suggest that "examining a single scalar field in minisuperspace is an extremely unrealistic scenario;" more significantly,

[52] 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).

"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" (Schiffrin and Wald 2012, 12-3). I discussed this latter problem as a manifestation of the "reference class problem" above (§4.2.1). The approach taken in both of the just-cited papers is to examine the analog of the GHS measure on perturbed FRW spacetimes (§1.3.4). Obviously this does not solve the 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. Presumably, however, what they aim 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.

In any case, 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, so, as I did in §1.3.4, I will only mention the relevant results and crucial assumptions.[53] The canonical volume element Ω on "almost" FRW

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

models (according to Schiffrin and Wald (2012)) is

$$\Omega_{GHS} \wedge \left(\frac{a(H_*^2 + ka^{-2})}{H_*(3H_*^2 - V + 3ka^{-2}} \right)^{\mathcal{N}_1} \times \prod_{n=1}^{\mathcal{N}_1} (k_n^2 - 3k) \mathrm{d}\Phi^{(n)} \wedge \mathrm{d}\delta^{(n)} \wedge \left(\frac{1}{4} a^3 \right)^{\mathcal{N}^2} \prod_{n=0}^{\mathcal{N}_2} \mathrm{d}\dot{h}^{(n)} \wedge \mathrm{d}h^{(n)}. \quad (4.37)$$

Here there are additional terms involving inhomogeneous scalar perturbations (Φ and δ) and tensor perturbations ($\dot{h}$ and h). The $\mathcal{N}_1$ 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 (§4.2.1). 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).

There is no need to belabor the points made already in the previous chapters: the main problems remain the same. In particular, the total measure of phase space is infinite, so probabilistic arguments cannot be made on the basis of the canonical measure. 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 (2010) are not to be trusted because an arbitrary choice has to be made to derive them. One can, again, get any probability one wants by a suitable choice of H.

Can one however 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." Once again, this conclusion depends on the physical significance of the Liouville measure associated with the set of perturbed FRW spacetimes, something for which we lack a compelling justification in statistical mechanics, much less cosmology (§3.3.5).

References

Abraham, Ralph, and Jerrold Marsden. *Foundations of Mechanics*. Redwood City, CA: Addison Wesley, 1978, 2nd edition.

Agricola, Ilka, and Thomas Friedrich. *Global Analysis*. Providence, RI: American Mathematical Society, 2002.

Aguirre, Anthony, and Steven Gratton. "Steady-state eternal inflation." *Physical Review D* 65: (2002) 1–6.

———. "Inflation without a beginning: A null boundary proposal." *Physical Review D* 67: (2003) 1–16.

Aguirre, Anthony, and Max Tegmark. "Multiple universes, cosmic coincidences, and other dark matters." *Journal of Cosmology and Astroparticle Physics* 1: (2005) 2–16.

Albert, David. "The Foundations of Quantum Mechanics and the Approach to Thermodynamic Equilibrium." *British Journal for the Philosophy of Science* 45: (1994) 669–677.

———. *Time and Chance*. Cambridge, MA: Harvard University Press, 2000.

Albrecht, Andreas, and Paul Steinhardt. "Cosmology for Grand Unified Theories with Radiatively Induced Symmetry Breaking." *Physical Review Letters* 48: (1982) 1220–1223.

Amin, Mustafa, Mark Hertzberg, David Kaiser, and Johanna Karouby. "Nonperturbative dynamics of reheating after inflation: A review." *International Journal of Modern Physics D* 24: (2015) 1–46.

Arkani-Hamed, Nima, Freddy Cachazo, and Jared Kaplan. "What is the Simplest Quantum Field Theory?" *Journal of High Energy Physics* 2010: (2010) 1–92.

Arms, Judith, Jerrold Marsden, and Vincent Moncrief. "The Structure of the Space of Solutions of Einstein's Equations II: Several Killing Fields and the Einstein-Yang-Mills Equations." *Annals of Physics* 144: (1982) 81–106.

Arnold, Vladmir. *Mathematical Methods of Classical Mechanics*. New York: Springer-Verlag, 1989, 2nd edition.

Asselmeyer-Maluga, Torsten, and Carl Brans. *Exotic Smoothness and Physics*. Singapore: World Scientific, 2007.

Bardeen, James. “Gauge-invariant cosmological perturbations.” *Physical Review D* 22: (1980) 1882–1905.

Bardeen, James, Paul Steinhardt, and Michael Turner. “Spontaneous creation of almost scale-free density perturbations in an inflationary universe.” *Physical Review D* 28: (1983) 679–693.

Barrow, John, George Ellis, Roy Maartens, and Christos Tsagas. “On the stability of the Einstein static universe.” *Classical and Quantum Gravity* 20: (2003) L155–164.

Bassett, Bruce, Shinji Tsujikawa, and David Wands. “Inflation dynamics and reheating.” *Reviews of Modern Physics* 78: (2006) 537–589.

Baumann, Daniel. “TASI Lectures on Inflation.” ArXiv Eprint, 2009. http://arxiv.org/abs/0907.5424.

Beem, John, Paul Ehrlich, and Kevin Easley. *Global Lorentzian Geometry*. New York: Marcel Dekker, 1996.

Beisbart, Claus. “Can We Justifiably Assume the Cosmological Principle in Order to Break Model Underdetermination in Cosmology?” *Journal for General Philosophy of Science* 40: (2009) 175–205.

Beisbart, Claus, and Tobias Jung. “Privileged, Typical, or Not Even That?—Our Place in the World According to the Copernican and the Cosmological Principles.” *Journal for General Philosophy of Science* 37: (2006) 225–256.

Belinsky, Vladimir, Leonid Grishchuk, Isaak Khalatnikov, and Yakov Zeldovich. “Inflationary Stages in Cosmological Models with a Scalar Field.” *Physics Letters B* 155: (1985) 232–236.

Belot, Gordon. “Why General Relativity *Does* Need an Interpretation.” *Philosophy of Science* 63: (1996) S80–S88.

———. “Symmetry and gauge freedom.” *Studies in History and Philosophy of Modern Physics* 34: (2003) 189–225.

———. “Background-independence.” *General Relativity and Gravitation* 43: (2011) 2865–2884.

Bombelli, Luca, and Johan Noldus. “The moduli space of isometry classes of globally hyperbolic spacetimes.” *Classical and Quantum Gravity* 21: (2004) 4429–4453.

Borde, Arvind, Alan Guth, and Alexander Vilenkin. “Inflationary Spacetimes Are Incomplete in Past Directions.” *Physical Review Letters* 90: (2003) 4.

Brading, Katherine, and Elena Castellani. *Symmetries in Physics*. Cambridge: Cambridge University Press, 2003.

Brandenberger, Robert. “Conceptual Problems of Inflationary Cosmology and a New Approach to Cosmological Structure Formation.” In *Inflationary Cosmology*, edited by Martin Lemoine, Jérôme Martin, and Patrick Peter, Berlin: Springer-Verlag Berlin Heidelberg, 2007, volume 738 of *Lecture Notes in Physics*, chapter 11, 393–424.

Brawer, Roberta. *Inflationary cosmology and horizon and flatness problems: the mutual constitution of explanation and questions*. Ph.D. thesis, Massachusetts Institute of Technology, Cambridge, MA, 1995.

Brighouse, Carolyn. “Spacetime and Holes.” In *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*. Philosophy of Science Association, 1994, volume 1994, 117–125.

Brisudova, Martina, William Kinney, and Richard Woodard. “Cosmology with a long range repulsive force.” *Physical Review D* 65: (2002) 1–10.

Bromberger, Sylvain. *On What We Don’t Know: Explanation, Theory, Linguistics, and How Questions Shape Them*. Chicago: University of Chicago Press, 1992.

Bruni, Marco, Peter Dunsby, and George F. R. Ellis. “Cosmological Perturbations and the Physical Meaning of Gauge-invariant Variables.” *The Astrophysical Journal* 395: (1992) 34–53.

Buchert, Thomas, and Syksy Räsänen. “Backreaction in Late-Time Cosmology.” *Annual Review of Nuclear and Particle Science* 62: (2012) 57–79.

Butterfield, Jeremy. “The Hole Truth.” *British Journal for the Philosophy of Science* 40: (1989) 1–28.

———. “On Symplectic Reduction in Classical Mechanics.” In *Philosophy of Physics*, edited by John Earman, and Jeremy Buttefield, Amsterdam: Elsevier, 2007, volume A of *Handbook of the Philosophy of Science*, 1–131.

———. “Underdetermination in Cosmology: An Invitiation.” In *Proceedings of the Aristotelian Society Supplementary Volume*. The Aristotelian Society, 2012, volume 86.

———. “On Under-determination in Cosmology.” *Studies in History and Philosophy of Modern Physics* 46: (2014) 57–69.

Callender, Craig. “Reducing Thermodynamics to Statistical Mechanics: The Case of Entropy.” *The Journal of Philosophy* 96: (1999) 348–373.

———. “Taking Thermodynamics Too Seriously.” *Studies in History and Philosophy of Modern Physics* 32, 4: (2001) 539–553.

———. “Measures, Explanations and the Past: Should ’Special’ Initial Conditions be Explained?” *British Journal for the Philosophy of Science* 55: (2004a) 195–217.

———. “There is no Puzzle about the Low-Entropy Past.” In *Contemporary Debates in the Philosophy of Science*, edited by Christopher Hitchcock, Hoboken, NJ: Wiley-Blackwell, 2004b, chapter 12.

———. “The Past Hypothesis Meets Gravity.” In *Time, Chance, and Reduction: Philosphical Aspects of Statistical Mechanics*, edited by Gerhard Ernst, and Andreas Hütteman, Cambridge: Cambridge University Press, 2010, 34–58.

———. “Hot and Heavy Matters in the Foundations of Statistical Mechanics.” *Foundations of Physics* 41: (2011a) 960–981.

———. “The Past Histories of Molecules.” In *Probabilities in Physics*, edited by Claus Beisbart, and Stephan Hartmann, Oxford: Oxford University Press, 2011b, 83–113.

Callender, Craig, and Robert Weingard. “Topology Change and the Unity of Space.” *Studies in History and Philosophy of Modern Physics* 31: (2000) 227–246.

Caprini, Chiara, and Pedro Ferreira. “Constraints on the electrical charge asymmetry of the universe.” *Journal of Cosmology and Astroparticle Physics* 2: (2005) 1–10.

Carr, Bernard, editor. *Universe or Multiverse?* Cambridge: Cambridge University Press, 2007.

Carroll, Sean. *From Eternity to Here*. New York: Plume, 2010.

———. “In What Sense Is the Early Universe Fine-Tuned?” In *Time’s Arrows and the Probability Structure of the World*, edited by Barry Loewer, Brad Weslake, and Eric Winsberg, Cambridge, MA: Harvard University Press, forthcoming.

Carroll, Sean, and Jennifer Chen. “Spontaneous Inflation and the Origin of the Arrow of Time.” ArXiv Eprint, 2004. http://arxiv.org/abs/hep-th/0410270.

———. “Does Inflation Provide Natural Conditions for the Universe?” *International Journal of Modern Physics D* 14: (2005) 2335–2339.

Carroll, Sean, and Heywood Tam. “Unitary Evolution and Cosmological Fine-Tuning.” ArXiv Eprint, 2010. http://arxiv.org/abs/1007.1417.

Cartier, Pierre, and Cécile DeWitt-Morette. *Functional integration: action and symmetries*. Cambridge: Cambridge University Press, 2006.

Cartwright, Nancy. *The Dappled World*. Cambridge: Cambridge University Press, 1999.

Clark, Peter. "Statistical Mechanics and the Propensity Interpretation of Probability." In *Chance in Physics*, edited by Jean Bricmont, Detleff Dürr, Maria Galavotti, Giancarlo Ghirardi, Francesco Petruccione, and Nino Zanghi, Berlin: Springer Verlag, 2001, 271–281.

Clarkson, Chris, George Ellis, Julien Larena, and Obinna Umeh. "Does the growth of structure affect our dynamical models of the universe? The averaging, backreaction, and fitting problems in cosmology." *Reports on Progress in Phyics* 74: (2011) 1–17.

Clarkson, Chris, and Roy Maartens. "Inhomogeneity and the foundations of the concordance model." *Classical and Quantum Gravity* 27: (2010) 1–23.

Coleman, Paul, and Luciano Pietronero. "The Fractal Structure of the Universe." *Physics Reports* 213: (1992) 311–389.

Coley, Alan, and Reza Tavakol. "Fragility in Cosmology." *General Relativity and Gravitation* 24: (1992) 835–855.

Cotsakis, Spiros, and Peter Leach. "Is Nature Generic?" In *Cosmological Crossroads*, edited by Spiros Cotsakis, and Eleftherios Papantonopoulos, Berlin: Springer Verlag, 2002, 3–15.

Coule, David. "Canonical measure and the flatness of a FRW universe." *Classical and Quantum Gravity* 12: (1995) 455–469.

———. "Comparison of the minimal and canonical measures." *Classical and Quantum Gravity* 13: (1996) 2029–2032.

Curiel, Erik. "The Analysis of Singular Spacetimes." *Philosophy of Science* 66: (1998) S119–S145.

———. "General Relativity Needs No Interpretation." *Philosophy of Science* 76: (2009) 44–72.

———. "Classical Mechanics Is Lagrangian; It Is Not Hamiltonian." *British Journal for the Philosophy of Science* 65: (2014) 269–321.

———. "Measure, Topology and Probabilistic Reasoning in Cosmology." PhilSci Archive Eprint, 2015. http://philsci-archive.pitt.edu/11677/.

———. "A Primer on Energy Conditions." In *Towards a Theory of Spacetime Theories*, edited by Dennis Lehmkuhl, Gregor Schiemann, and Erhard Schulz, Boston: Birkhäuser, forthcoming.

Curiel, Erik, and Peter Bokulich. “Singularities and Black Holes.” Stanford Encyclopedia of Philosophy, 2012. http://plato.stanford.edu/archives/fall2012/entries/spacetime-singularities. Edward N. Zalta (ed.).

Dautcourt, Georg. “The cosmological problem as initial value problem on the observer’s past light cone: geometry.” *Journal of Physics A: Mathematical and Theoretical* 16: (1983a) 3507–3528.

———. “The Cosmological Problem as Initial Value Problem on the Observer’s Past Light Cone: Observations.” *Astronomische Nachrichten* 304: (1983b) 153–161.

Davey, Kevin. “Is mathematical rigor necessary in physics?” *British Journal for the Philosophy of Science* 54: (2003) 439–463.

———. “The Justification of Probability Measures in Statistical Mechanics.” *Philosophy of Science* 75: (2008) 28–44.

———. “What Is Gibbs’s Canonical Distribution?” *Philosophy of Science* 76: (2009) 970–983.

Davidson, William. “Philosophical Aspects of Cosmology.” *British Journal for the Philosophy of Science* 13: (1962) 120–129.

Davis, Tamara, and Charles Lineweaver. “Expounding Confusion: Common Misconceptions of Cosmological Horizons and the Superluminal Expansion of the Universe.” *Publications of the Astronomical Society of Australia* 21: (2004) 97–109.

Dawid, Richard. *String Theory and the Scientific Method*. Cambridge: Cambridge University Press, 2013.

de Vaucoueurs, Gérard. “The Case for a Hierarchical Cosmology.” *Science* 167: (1970) 1203–1213.

Dickau, Jonathan. “Fractal Cosmology.” *Chaos, Solitons and Fractals* 41: (2009) 2103–2105.

Dicke, Robert, and Jim Peebles. “The Big Bang Cosmology—Enigmas and Nostrums.” In *General Relativity: An Einstein Centenary Survey*, edited by Stephen Hawking, and Werner Israel, Cambridge: Cambridge University Press, 1979, chapter 9, 504–517.

Dieckmann, Jan. “Cauchy surfaces in a globally hyperbolic space-time.” *Journal of Mathematical Physics* 29: (1988) 578–579.

Dingle, Herbert. “Philosophical Aspects of Cosmology.” *Vistas in Astronomy* 1: (1955) 162–166.

Dodelson, Scott. *Modern Cosmology*. San Diego, CA: Academic Press, 2003.

Dolgov, Alexandre. "Long-range forces in the universe." *Physics Reports* 320: (1999) 1–15.

Dolgov, Alexandre, and Joseph Silk. "Electric charge asymmetry of the Universe and magnetic field generation." *Physical Review D* 47: (1993) 3144–3150.

Donaldson, Simon, and Peter Kronheimer. *The Geometry of Four-Manifolds*. Oxford: Oxford University Press, 1997.

Donoghue, John. "The fine-tuning problems of particle physics and anthropic mechanisms." In *Universe or Multiverse?*, edited by Bernard Carr, Cambridge: Cambridge University Press, 2007, chapter 15, 231–246.

Dunsby, Peter, Bruce Bassett, and George Ellis. "Covariant analysis of gravitational waves in a cosmological context." *Classical and Quantum Gravity* 14: (1997) 1215–1222.

Dürr, Detlef. "Bohmian Mechanics." In *Chance in Physics*, edited by Jean Bricmont, Detlef Dürr, Maria Galavotti, Giancarlo Ghirardi, Francesco Petruccione, and Nino Zanghì, Berlin: Springer Verlag, 2001, Lecture Notes in Physics, 115–131.

Earman, John. *A Primer on Determinism*. Dordrecht, Netherlands: D. Reidel Publishing Company, 1986.

———. "The Sap Also Rises: A Critical Examination of the Anthropic Principle." *American Philosophical Quarterly* 24: (1987) 307–317.

———. *Bangs, Crunches, Whimpers, and Shrieks*. Oxford: Oxford University Press, 1995.

———. "Lambda: The Constant That Refuses to Die." *Archive for the History of the Exact Sciences* 55: (2001) 189–220.

———. "Tracking down gauge: an ode to the constrained Hamiltonian formalism." In *Symmetries in Physics*, edited by Katherine Brading, and Elena Castellani, Cambridge: Cambridge University Press, 2003, chapter 8, 140–162.

———. "The "Past Hypothesis": Not even false." *Studies in History and Philosophy of Modern Physics* 37: (2006) 399–430.

Earman, John, and Jesus Mosterín. "A Critical Look at Inflationary Cosmology." *Philosophy of Science* 66: (1999) 1–49.

Earman, John, and John Norton. "What price spacetime substantivalism? The hole story." *British Journal for the Philosophy of Science* 38: (1987) 515–525.

Earman, John, and Miklós Rédei. "Why Ergodic Theory Does Not Explain the Success of Equilibrium Statistical Mechanics." *British Journal for the Philosophy of Science* 47: (1996) 63–78.

Earman, John, Chris Smeenk, and Christian Wüthrich. "Do the laws of physics forbid the operation of time machines?" *Synthese* 169: (2009) 91–124.

Eddington, Arthur. "On the Instability of Einstein's Spherical World." *Monthly Notices of the Royal Astronomical Society* 90: (1930) 668–678.

Einstein, Albert. *Albert Einstein: Philosopher-Scientist.* New York: Harper Torchbooks, 1959.

Ellis, George. "Topology and Cosmology." *General Relativity and Gravitation* 2: (1971) 7–21.

———. "Cosmology and Verifiability." *Quarterly Journal of the Royal Astronomical Society* 16: (1975) 245–264.

———. "Limits to Verification in Cosmology." *Annals of the New York Academy of Science* 336: (1980) 130–160.

———. "Relativistic Cosmology: Its Nature, Aims and Problems." In *General Relativity and Gravitation (Invited Papers and Discussion Reports of the 10th Int. Conf.)*, edited by B. Bertotti, F. de Felice, and A. Pascolini. Dordrecht, Netherlands: D. Reidel Publishing Company, 1984, 215–288.

———. "Does inflation necessarily imply $\Omega = 1$?" *Classical and Quantum Gravity* 5: (1988) 891–901.

———. "83 years of general relativity and cosmology: progress and problems." *Classical and Quantum Gravity* 16: (1999) A37–A75.

———. "Issues in the Philosophy of Cosmology." In *Philosophy of Physics*, edited by John Earman, and Jeremy Buttefield, Amsterdam: Elsevier, 2007, volume B of *Handbook of the Philosophy of Science*, 1183–1285.

———. "Inhomogeneity effects in cosmology." *Classical and Quantum Gravity* 28: (2011) 1–21.

Ellis, George, and Marco Bruni. "Covariant and gauge-invariant approach to cosmological density fluctuations." *Physical Review D* 40: (1989) 1804–1818.

Ellis, George, and Henk van Elst. "Cosmological Models." In *NATO Advanced Study Institute on Theoretical and Observational Cosmology*, edited by Marc Lachièze-Rey. 1999, volume 541, 1–116.

Ellis, George, Jai-Chan Hwang, and Marco Bruni. "Covariant and gauge-independent perfect-fluid Robertson-Walker perturbations." *Physical Review D* 40: (1989) 1819–1826.

Ellis, George, Ulrich Kirchner, and William Stoeger. "Multiverses and Physical Cosmology." *Monthly Notices of the Royal Astronomical Society* 347: (2004) 921–936.

Ellis, George, Roy Maartens, and Malcolm MacCallum. *Relativistic Cosmology*. Cambridge: Cambridge University Press, 2012.

Ellis, George, Roy Maartens, and Stanley Nel. "The expansion of the Universe." *Monthly Notices of the Royal Astronomical Society* 184: (1978) 439–465.

Ellis, George, Stanley Nel, Roy Maartens, William Stoeger, and Andrew Whitman. "Ideal Observational Cosmology." *Physics Reports* 124: (1985) 315–417.

Ellis, George, and Tony Rothman. "Lost horizons." *American Journal of Physics* 61: (1993) 883–893.

Ellis, George, and G. Schreiber. "Observational and Dynamical Properties of Small Universes." *Physics Letters A* 115: (1986) 97–107.

Ellis, George, and William Stoeger. "The 'fitting problem' in cosmology." *Classical and Quantum Gravity* 4: (1987) 1697–1729.

Ellis, Richard. *Entropy, Large Deviations, and Statistical Mechanics*. Berlin: Springer-Verlag, 2006.

Emch, Gérard. "Probabilistic issues in statistical mechanics." *Studies in History and Philosophy of Modern Physics* 36: (2005) 303–322.

Evrard, Guillaume. "Objective Prior for Cosmological Parameters." In *Maximum Entropy and Bayesian Methods*, edited by Kenneth Hanson, and Richard Silver, Alphen aan den Rijn, Netherlands: Kluwer Academic Publishers, 1996, 381–386.

Evrard, Guillaume, and Peter Coles. "Getting the measure of the flatness problem." *Classical and Quantum Gravity* 12: (1995) L93–L97.

Feeney, Stephen, Matthew Johnson, Daniel Mortlock, and Hiranya Peiris. "First Observational Tests of Eternal Inflation." *Physical Review Letters* 107: (2011) 1–5.

Fischer, Arthur, and Jerrold Marsden. "The initial value problem and the dynamical formulation of general relativity." In *General Relativity: An Einstein Centenary Survey*, edited by Stephen Hawking, and Werner Israel, Cambridge: Cambridge University Press, 1979, chapter 4, 138–211.

Fischer, Arthur, Jerrold Marsden, and Vincent Moncrief. "The structure of the space of solutions of Einstein's equations. I. One Killing field." *Annales de l'Institue Henri Poincaré* 33: (1980) 147–194.

Fletcher, Samuel. "Similarity, Topology, and Physical Significance in Relativity Theory." *British Journal for the Philosophy of Science* 0: (forthcoming) 1–25.

Freedman, Michael, and Frank Quinn. *Topology of 4-Manifolds*. Princeton: Princeton University Press, 1990.

Friedman, Aleksandr. "Über die Krümmung des Raumes." *Zeitschrift für Physik* 10: (1922) 377–386.

Frigg, Roman. "Chance in Boltzmannian Statistical Mechanics." *Philosophy of Science* 75: (2008a) 670–681.

———. "A Field Guide to Recent Work on the Foundations of Statistical Mechanics." In *The Ashgate Companion to Contemporary Philosophy of Physics*, edited by Dean Rickles, London: Ashgate, 2008b, 99–196.

———. "Typicality and the Approach to Equilibrium in Boltzmannian Statistical Mechanics." *Philosophy of Science* 76: (2009) 997–1008.

———. "Probability in Boltzmannian Statistical Mechanics." In *Time, Chance and Reduction*, edited by Gerhard Ernst, and Andreas Hüttemann, Cambridge: Cambridge University Press, 2010.

———. "Why Typicality Does Not Explain the Approach to Equilibrium." In *Probabilities, Causes and Propensities in Physics*, edited by Mauricio Suárez, Berlin: Springer Verlag, 2011, volume 347 of *Synthese Library*, 77–93.

Frigg, Roman, and Carl Hoefer. "The Best Humean System for Statistical Mechanics." *Erkenntnis* 80: (2013) 551–574.

Frigg, Roman, and Charlotte Werndl. "Demystifying Typicality." *Philosophy of Science* 79: (2012) 917–929.

Galavotti, Maria Carla. "What Interpretation for Probability in Physics?" In *Chance in Physics*, edited by Jean Bricmont, Detlef Dürr, Maria Galavotti, Giancarlo Ghirardi, Francesco Petruccione, and Nino Zanghi, Berlin: Springer Verlag, 2001.

———. *Philosophical Introduction to Probability*. Stanford: CSLI Publications, 2005.

Geroch, Robert. "General Relativity in the Large." *General Relativity and Gravitation* 2: (1971) 61–74.

———. *Mathematical Physics*. Chicago: University of Chicago Press, 1985.

Geroch, Robert, and Gary Horowitz. "Global structure of spacetimes." In *General Relativity: An Einstein Centenary Survey*, edited by Stephen Hawking, and Werner Israel, Cambridge: Cambridge University Press, 1979, chapter 5, 212–293.

Giachetta, Giovanni, Luigi Mangiarotti, and Gennadi Sardanashvily. *Advanced Classical Field Theory*. Singapore: World Scientific, 2009.

Gibbons, Gary. "The Entopy and Stability of the Universe." *Nuclear Physics B* 292: (1987) 784–792.

Gibbons, Gary, Stephen Hawking, and Stephen Siklos, editors. *The Very Early Universe*. Cambridge: Cambridge University Press, 1983.

Gibbons, Gary, Stephen Hawking, and John Stewart. "A natural measure on the Set of all Universes." *Nuclear Physics B* 281: (1987) 736–751.

Gibbons, Gary, and Neil Turok. "Measure problem in cosmology." *Physical Review D* 77: (2008) 1–12.

Gibbs, J. Willard. *Elementary Principles in Statistical Mechanics*. New York: Scribner's Sons, 1902.

Gillies, Donald. *Philosophical Theories of Probability*. London: Routledge, 2000.

Giulini, Domenico. "Remarks on the Notions of General Covariance and Background Independence." In *Approaches to Fundamental Physics*, edited by Ion-Olimpiu Stamatescu, and Erhard Seiler, Berlin: Springer Verlag, 2007, volume 721 of *Lecture Notes in Physics*, 105–120.

Glymour, Clark. "Topology, Cosmology, and Convention." *Synthese* 24: (1972) 195–218.

———. "Indistinguishable Space-Times and the Fundamental Group." In *Foundations of Space-Time Theories*, Minneapolis, MN: University of Minnesota Press, 1977, volume 8 of *Minnesota Studies in the Philosophy of Science*, 50–60.

Goenner, Hubert. "What kind of science is cosmology?" *Annals of Physics* 522: (2010) 389–418.

Goldstein, Sheldon. "Boltzmann's Approach to Statistical Mechanics." In *Chance in Physics*, edited by Jean Bricmont, Detlef Dürr, Maria Galavotti, Giancarlo Ghirardi, Francesco Petruccione, and Nino Zanghì, Berlin: Springer Verlag, 2001, 39–54.

———. "Typicality and Notions of Probability in Physics." In *Probability in Physics*, edited by Yemima Ben-Menahem, and Meir Hemmo, Berlin: Springer Verlag, 2012, chapter 4, 59–71.

Goldwirth, Dalia, and Tsvi Piran. "Initial Conditions for Inflation." *Physics Reports* 214: (1992) 223–292.

Gotay, Mark. "A multisymplectic framework for classical field theory and the calculus of variations I. Covariant Hamiltonian formalism." In *Mechanics, Analysis, and Geometry: 200 Years After Lagrange*, edited by Mauro Francaviglia, Amsterdam: Elsevier, 1991.

Gotay, Mark, James Isenberg, Jerrold Marsden, and Richard Montgomery. "Momentum Maps and Classical Fields." ArXiv Eprint, 2004. http://arxiv.org/abs/physics/9801019.

Guay, Alexandre. "A partial elucidation of the gauge principle." *Studies in History and Philosophy of Modern Physics* 39: (2008) 346–363.

Guth, Alan. "Inflationary universe: A possible solution to the horizon and flatness problems." *Physical Review D* 23, 2: (1981) 347–356.

———. "Interview of Alan Guth by Alan Lightman on September 21, 1987." Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, 1987. http://www.aip.org/history/ohilist/34306_1.html.

———. *The Inflationary Universe*. Cambridge, MA: Perseus Books, 1997.

———. "Eternal Inflation and its Implications." *Journal of Physics A: Mathematical and Theoretical* 40: (2007) 6811–6826.

Guth, Alan, David Kaiser, and Yasunori Nomura. "Inflationary paradigm after Planck 2013." *Physics Letters B* 733: (2014) 112–119.

Guth, Alan, and So-Young Pi. "Fluctuations in the New Inflationary Universe." *Physical Review Letters* 49: (1982) 1110–1113.

Guth, Alan, and Erick Weinberg. "Could the Universe Have Recovered from a Slow First-Order Phase Transition?" *Nuclear Physics B* 212: (1983) 321–364.

Guttmann, Yair. *The Concept of Probability in Statistical Physics*. Cambridge: Cambridge University Press, 1999.

Hájek, Alan. "Interpretations of Probability." The Stanford Encyclopedia of Philosophy, 2012. http://plato.stanford.edu/archives/win2012/entries/probability-interpret. Edward Zalta (ed.).

Hamblin, Charles. "Questions." *Australasian Journal of Philosophy* 36: (1958) 159–168.

Harré, Rom. "Philosophical Aspects of Cosmology." *British Journal for the Philosophy of Science* 50: (1962) 104–119.

Harrison, Edward. "Normal Modes of Vibrations of the Universe." *Reviews of Modern Physics* 39: (1967) 862–882.

Hawking, Stephen. “Perturbations of an Expanding Universe.” *The Astrophysical Journal* 145: (1966) 544–554.

———. “Stable and Generic Properties in General Relativity.” *General Relativity and Gravitation* 1: (1971) 393–400.

———. “The development of irregularities in a single bubble inflationary universe.” *Physics Letters B* 115: (1982) 295–297.

Hawking, Stephen, and George Ellis. *The large scale structure of space-time*. Cambridge: Cambridge University Press, 1973.

Hawking, Stephen, and James Hartle. “Wave function of the universe.” *Physical Review D* 28: (1983) 2960–2975.

Hawking, Stephen, Ian Moss, and John Stewart. “Bubble collisions in the very early universe.” *Physical Review D* 26: (1982) 2681–2693.

Hawking, Stephen, and Don Page. “How Probable is Inflation?” *Nuclear Physics B* 298: (1988) 789–809.

Hawking, Stephen, and Roger Penrose. “The Singularities of Gravitational Collapse and Cosmology.” *Proceedings of the Royal Society A: Mathematical, Physical and Engineering Science* 314: (1970) 529–548.

———. *The Nature of Space and Time*. Princeton, NJ: Princeton University Press, 2010.

Healey, Richard. *Gauging What’s Real*. Oxford: Oxford University Press, 2007.

Hemmo, Meir, and Orly Shenker. “Can We Explain Thermodynamics By Quantum Decoherence?” *Studies in History and Philosophy of Modern Physics* 32: (2001) 555–568.

———. “Measures over Initial Conditions.” In *Probability in Physics*, edited by Yemima Ben-Menahem, and Meir Hemmo, Berlin: Springer Verlag, 2012, The Frontiers Collection, chapter 6, 87–98.

———. “Probability and Typicality in Deterministic Physics.” *Erkenntnis* 80: (2015) 575–586.

Henneuax, Marc. “The Gibbs Entropy Production in General Relativity.” *Lettere al Nuovo Cimento* 38: (1983) 609–614.

Hollands, Stefan, and Robert Wald. “An Alternative to Inflation.” *General Relativity and Gravitation* 34: (2002a) 2043–2055.

———. “Comment on Inflation and Alternative Cosmology.” ArXiv Eprint, 2002b. http://arxiv.org/abs/hep-th/0210001.

Hu, Wayne, and Scott Dodelson. "Cosmic Microwave Background Anisotropies." *Anuual Review of Astronomy and Astrophysics* 40: (2002) 171–216.

Ijjas, Anna, Robert Brandenberger, and Abraham Loeb. "Inflationary schism." *Physics Letters B* 736: (2014) 142–146.

Ijjas, Anna, Paul Steinhardt, and Abraham Loeb. "Inflationary paradigm in trouble after Planck2013." *Physics Letters B* 723: (2013) 261–266.

Isenberg, James, and Jerrold Marsden. "A slice theorem for the space of solutions of Einstein's equations." *Physics Reports* 89: (1982) 179–222.

Isham, Chris. *Modern Differential Geometry for Physicists.* Singapore: World Scientific, 1999.

Ishibashi, Akihiro, and Robert Wald. "Can the acceleration of our universe be explained by the effects of inhomogeneities?" *Classical and Quantum Gravity* 23: (2006) 235–250.

Jaynes, Edwin. "Information Theory and Statistical Mechanics." *The Physical Review* 106: (1957) 620–630.

Kanitscheider, Bernulf. "Does Physical Cosmology Transcend the Limits of Naturalistic Reasoning?" In *Studies on Mario Bunge's Treatise*, edited by Paul Weingartner, and Georg Dorn, Amsterdam: Radopi Bv Editions, 1990, volume 18 of *Poznan Studies in the Philosophy of the Sciences and Humanities*, 337–350.

Kirchner, Ulrich, and George Ellis. "A probability measure for FLRW models." *Classical and Quantum Gravity* 20: (2003) 1199–1213.

Kodama, Hideo, and Misao Sasaki. "Cosmological Perturbation Theory." *Progress of Theoretical Physics Supplement* 74: (1984) 1–166.

Kofman, Lev, Andrei Linde, and Viatcheslav Mukhanov. "Inflationary Theory and Alternative Cosmology." *Journal of High Energy Physics* 2002: (2002) 1–14.

Kolb, Edward, Valerio Marra, and Sabino Matarrese. "Cosmological background solutions and cosmological backreactions." *General Relativity and Gravitation* 42: (2010) 1399–1412.

Kosowsky, Arthur. "Cosmic Microwave Background Polarization." *Annals of Physics* 246: (1996) 49–85.

Kragh, Helge. *Cosmology and Controversy.* Princeton: Princeton University Press, 1996.

Kristian, Jerome, and Rainer Sachs. "Observations in Cosmology." *Astrophysical Journal* 143: (1966) 379–399.

Kuhn, Thomas. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press, 1996, 3rd edition.

Lachièze-Rey, Marc, and Jean-Pierre Luminet. "Cosmic Topology." *Physics Reports* 254: (1995) 135–214.

Lakatos, Imre. "Falsification and the methodology of scientific research programmes." In *Criticism and the Growth of Knowledge*, edited by Imre Lakatos, and Alan Musgrave, Cambridge: Cambridge University Press, 1970.

Laudan, Larry. *Progress and Its Problems: Towards a Theory of Scientific Growth*. Berkeley: University of California Press, 1978.

Lavis, David. "The Concept of Probability in Statistical Mechanics." In *Frontiers of Fundamental Physics 4*, edited by Burra Sidharth, and Mikhail Altaisky, Berlin: Springer Verlag, 2001, 293–308.

Lazarovici, Dustin, and Paula Reichert. "Typicality, Irreversibility and the Status of Macroscopic Laws." *Erkenntnis* 80: (2015) 689–716.

Lebowitz, Joel. "Boltzmann's Entropy and Time's Arrow." *Physics Today* September: (1993a) 32–38.

———. "Macroscopic Laws, Microscopic Dynamics, Time's Arrow and Boltzmann's Entropy." *Physica A* 194: (1993b) 1–27.

Leeds, Stephen. "Foundations of Statistical Mechanics—Two Approaches." *Philosophy of Science* 70: (2003) 126–144.

Lerner, David. "The Space of Lorentz Metrics." *Communications in Mathematical Physics* 32: (1973) 19–38.

Lesgourges, Julien. "Inflationary Cosmology." Eprint, 2006. lesgourg.web.cern.ch/lesgourg/Inflation_EPFL.pdf.

Liddle, Andrew, and David Lyth. *Cosmological Inflation and Large-Scale Structure*. Cambridge: Cambridge University Press, 2000.

Lifshitz, Evgeny, and Isaak Khalatnikov. "Investigations in relativistic cosmology." *Advances in Physics* 46: (1963) 185–249.

Lightman, Alan, and Roberta Brawer. *Origins: The Lives and Worlds of Modern Cosmologists*. Cambridge, MA: Harvard University Press, 1990.

Linde, Andrei. "A New Inflationary Universe Scenario: A Possible Solution of the Horizon, Flatness, Homogeneity, Isotropy, and Primordial Monopole Problems." *Physics Letters B* 108: (1982) 389–393.

———. "Chaotic Inflation." *Physics Letters B* 129: (1983) 177–181.

———. "The inflationary universe." *Reports on Progress in Phyics* 47: (1984) 925–986.

———. "Eternally Existing Self-Reproducing Chaotic Inflationary Universes." *Physics Letters B* 175: (1986) 395–400.

———. *Particle Physics and Inflationary Cosmology*, volume 5 of *Contemporary Concepts in Physics*. Boca Raton, FL: CRC Press, 1990.

———. "Inflation and String Cosmology." ArXiv Eprint, 2005. http://arxiv.org/abs/hep-th/0503195v1.

———. "Inflationary Cosmology." In *Inflationary Cosmology*, edited by Martin Lemoine, Jerome Martin, and Patrick Peter, Berlin: Springer Verlag, 2007, volume 738 of *Lecture Notes in Physics*, 1–54.

Loewer, Barry. "Determinism and Chance." *Studies in History and Philosophy of Modern Physics* 32: (2001) 609–620.

Longair, Malcolm. *The Cosmic Century*. Cambridge: Cambridge University Press, 2006.

Luminet, Jean-Pierre, Jeffrey Weeks, Alain Rlazuelo, Roland Lehoucq, and Jean-Philippe Uzan. "Dodecahedral space topology as an explanation for weak wide-angle temperature correlations in the cosmic microwave background." *Nature* 425: (2003) 593–595.

Lyth, David, and Antonio Riotto. "Particle Physics Models of Inflation and the Cosmological Density Perturbation." *Physics Reports* 314: (1999) 1–146.

Lyttleton, Raymond, and Hermann Bondi. "On the physical consequences of a general excess of charge." *Proceedings of the Royal Society A: Mathematical, Physical and Engineering Science* 252: (1959) 313–333.

Maartens, Roy. "Is the Universe homogeneous?" *Philosophical Transactions of the Royal Society A* 369: (2011) 5115–5137.

MacCallum, Malcolm. "Anisotropic and inhomogeneous relativistic cosmologies." In *General Relativity: an Einstein Centenary Survey*, Cambridge: Cambridge University Press, 1979.

Magnus, P. D. "Reckoning the Shape of Everything: Underdetermination and Cosmotopology." *British Journal for the Philosophy of Science* 56: (2005) 541–557.

Malament, David. "Observationally Indistinguishable Space-Times." In *Foundations of Space-Time Theories*, edited by John Earman, Clark Glymour, and John Stachel, Minneapolis, MN: University of Minnesotra Press, 1977, volume 8 of *Minnesota Studies in the Philosophy of Science*, 61–80.

———. “Is Newtonian cosmology really inconsistent?” *Philosophy of Science* 62: (1995) 489–510.

———. *Topics in the Foundations of General Relativity and Newtonian Gravity Theory*. Chicago: University of Chicago Press, 2012.

Malik, Karim, and David Wands. “Cosmological Perturbations.” *Physics Reports* 475: (2009) 1–51.

Manchak, John. “Can we know the global structure of spacetime?” *Studies in History and Philosophy of Modern Physics* 40: (2009) 53–56.

———. “What Is a Physically Reasonable Space-Time?” *Philosophy of Science* 78: (2011) 410–420.

Maudlin, Tim. *The Metaphysics Within Physics*. Oxford: Oxford University Press, 2007a.

———. “What could be objective about probabilities?” *Studies in History and Philosophy of Modern Physics* 38: (2007b) 275–291.

———. *Quantum Non-Locality and Relativity*. Malden, MA: Wiley-Blackwell, 2011, 3rd edition.

———. *Philosophy of Physics: Space and Time*. Princeton, NJ: Princeton University Press, 2012.

McCabe, Gordon. “The structure and interpretation of cosmology: Part I—general relativistic cosmology.” *Studies in History and Philosophy of Modern Physics* 35: (2004) 549–595.

———. “The Non-unique Universe.” *Foundations of Physics* 40: (2010) 629–637.

McCoy, Casey. “The Stochastic Observables Interpretation of Statistical Mechanics.” Phil-Sci Archive Eprint, 2016. http://philsci-archive.pitt.edu/id/eprint/11658.

———. “Prediction in General Relativity.” *Synthese* 0: (forthcoming) 1–19.

McCrea, William. “Cosmology.” *Reports on Progress in Phyics* 16: (1953) 321–363.

———. “A Philosophy for Big-Bang Cosmology.” *Nature* 228: (1970) 21–24.

McMullin, Ernan. “Indifference Principle and Anthropic Principle in Cosmology.” *Studies in History and Philosophy of Science* 24: (1993) 359–389.

Meacham, Christopher. “Contemporary Approaches to Statistical Mechanical Probabilities: A Critical Commentary—Part I: The Indifference Approach.” *Philosophy Compass* 5, 12: (2010) 1116–1126.

Mellor, David. *Probability: A Philosophical Introduction.* London: Routledge, 2005.

Misner, Charles. “The Isotropy of the Universe.” *The Astrophysical Journal* 151: (1968) 431–457.

———. “The Mix-Master Universe.” *Physical Review Letters* 22: (1969) 1071–2074.

Misner, Charles, Kip Thorne, and John Wheeler. *Gravitation.* San Francisco: W. H. Freeman, 1973.

Mukhanov, Viatcheslav. “The vacuum energy and large scale structure of the universe.” *Soviet JETP* 56: (1982) 258–265.

———. *Physical Foundations of Cosmology.* Cambridge: Cambridge University Press, 2005.

Mukhanov, Viatcheslav, and Gennady Chibisov. “Quantum fluctuations and a nonsingular universe.” *JETP Letters* 33: (1981) 532–535.

Mukhanov, Viatcheslav, Hume Feldman, and Robert Brandenberger. “Theory of Cosmological Perturbations.” *Physics Reports* 215: (1992) 203–333.

Munitz, Milton. “The Logic of Cosmology.” *British Journal for the Philosophy of Science* 13: (1962) 34–50.

———. “On the Use of ‘Universe’ in Cosmology.” *The Monist* 48: (1964) 185–194.

———. *Cosmic Understanding.* Princeton, NJ: Princeton University Press, 1986.

Mustapha, Nazeem, Charles Hellaby, and George Ellis. “Large-scale inhomogeneity versus source evolution: can we distinguish them observationally?” *Monthly Notices of the Royal Astronomical Society* 292: (1997) 817–830.

Myrvold, Wayne. “Probabilities in Statistical Mechanics.” In *Oxford Handbook of Probability and Philosophy*, edited by Christopher Hitchcock, and Alan Háyek, Oxford: Oxford University Press, 2016, chapter 27.

Nerlich, Graham. *The Shape of Space.* Cambridge: Cambridge University Press, 1994.

Nickles, Thomas. “What Is a Problem That We May Solve It?” *Synthese* 47: (1981) 85–118.

North, Jill. “An empirical approach to symmetry and probability.” *Studies in History and Philosophy of Modern Physics* 41: (2010) 27–40.

Norton, John. “A paradox in Newtonian gravitation theory.” In *PSA 1992: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, edited by M. Forbes, D. Hull, and K. Okruhlik. Philosophy of Science Association, 1992, volume 2.

———. "The Dome: An Unexpectedly Simple Failure of Determinism." *Philosophy of Science* 75: (2008a) 786–798.

———. "Ignorance and Indifference." *Philosophy of Science* 75: (2008b) 45–68.

———. "Cosmic Confusions: Not Supporting Versus Supporting Not." *Philosophy of Science* 77: (2010) 501–523.

———. "Observationally Indistinguishable Spacetimes: A Challenge for Any Inductivist." In *Philosophy of Science Matters: The Philosophy of Peter Achinstein*, edited by Gregory Morton, Oxford: Oxford University Press, 2011, chapter 13, 164–176.

Nussbaumer, Harry. "Einstein's conversion from his static to an expanding universe." *European Physical Journal H* 39: (2014) 37–62.

Olive, Keith. "Inflation." *Physics Reports* 190: (1990) 307–403.

Olive, Keith, Gary Steigman, and Terry Walker. "Primordial nucleosynthesis: theory and observations." *Physics Reports* 333-334: (2000) 389–407.

Olson, Donald. "Density pertubations in cosmological models." *Physical Review D* 14: (1976) 327–331.

Padmanabhan, Thinakkal. *Structure Formation in the Universe*. Cambridge: Cambridge University Press, 1993.

Parker, Daniel. "Thermodynamic Irreversibility: Does the Big Bang Explain What It Purports to Explain?" *Philosophy of Science* 72: (2005) 751–763.

Peebles, P. J. E., David Schramm, E. T. Turner, and R. G. Kron. "The case for the relativistic hot Big Bang cosmology." *Nature* 352: (1991) 769–776.

Penrose, Oliver. *Foundations of Statistical Mechanics*. Oxford: Pergamon Press, 1970.

Penrose, Roger. "Singularities and time-asymmetry." In *General Relativity: An Einstein Centenary Survey*, edited by Stephen Hawking, and Werner Israel, Cambridge: Cambridge University Press, 1979, chapter 12, 581–638.

———. "Difficulties with Inflationary Cosmology." *Annals of the New York Academy of Science* 571: (1989a) 249–264.

———. *The Emperor's New Mind*. Oxford: Oxford University Press, 1989b.

Peter, Patrick, and Jean-Philippe Uzan. *Primordial Cosmology*. Oxford: Oxford University Press, 2009.

Pietronero, Luciano. "The Fractal Structure of the Universe: Correlations of Galaxies and Clusters and the Average Mass Density." *Physica A* 144: (1987) 257–284.

Pietronero, Luciano, and Francesco Sylos Labini. "Fractal Universe." *Physica A* 280: (2000) 125–130.

Pitowsky, Itamar. "Typicality and the Role of the Lebesgue Measure in Statistical Mechanics." In *Probability in Physics*, edited by Yemima Ben-Menahem, and Meir Hemmo, Berlin: Springer Verlag, 2012, The Frontiers Collection, chapter 3, 41–58.

Planck Collaboration. "Planck 2015 results. I. Overview of products and scientific results." ArXiv Eprint, 2015. http://arxiv.org/abs/1502.01582.

Popper, Karl. *The Poverty of Historicism.* London and New York: Routledge, 2013.

Price, Huw. *Time's Arrow and Archimedes' Point.* Oxford: Oxford University Press, 1996.

———. "Boltzmann's Time Bomb." *British Journal for the Philosophy of Science* 53: (2002) 83–119.

———. "On the Origins of the Arrow of Time: Why There is Still a Puzzle about the Low-Entropy Past." In *Contemporary Debates in the Philosophy of Science*, edited by Christopher Hitchcock, Hoboken, NJ: Wiley-Blackwell, 2004, 219–239.

Redhead, Michael. "Quantum Field Theory for Philosophers." In *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*. 1982, volume 1982, 57–99.

———. "The interpretation of gauge symmetry." In *Symmetries in Physics*, edited by Katherine Brading, and Elena Castellani, Cambridge: Cambridge University Press, 2003, chapter 7, 124–139.

Rees, Martin. "Origin of the Cosmic Microwave Background Radiation in a Chaotic Universe." *Physical Review Letters* 28: (1972) 1669–1671.

Reichenbach, Hans. *The Philosophy of Space and Time*. New York: Dover, 1958.

Reichl, Linda. *A Modern Course in Statistical Physics*. New York: John Wiley and Sons, 1998.

Remmen, Grant, and Sean Carroll. "Attractor solutions in scalar-field cosmology." *Physical Review D* 88: (2013) 1–14.

———. "How many e-folds should we expect from high-scale inflation?" *Physical Review D* 90: (2014) 1–14.

Rickles, Dean. "Who's Afraid of Background Independence?" In *The Ontology of Spacetime II*, edited by Dennis Dieks, Amsterdam: Elsevier, 2008, 133–152.

Rindler, Wolfgang. "Visual Horizons in World-Models." *Monthly Notices of the Royal Astronomical Society* 116: (1956) 662–677.

Riotto, Antonio. “Inflation and the Theory of Cosmological Perturbations.” ArXiv Eprint, 2002. http://arxiv.org/abs/hep-ph/0210162.

Rovelli, Carlo. “Why Gauge?” *Foundations of Physics* 44: (2014) 91–104.

Schech, Elay. “On Gases in Boxes: A Reply to Davey on the Justification of the Probability Measure in Boltzmannian Statistical Mechanics.” *Philosophy of Science* 80: (2013) 593–605.

Schiffrin, Joshua, and Robert Wald. “Measure and probability in cosmology.” *Physical Review D* 86: (2012) 1–20.

Sciama, Dennis. *The Unity of the Universe*. Mineola, NY: Dover Publications, 2009.

Shackel, Nicholas. “Bertrand’s paradox and the principle of indifference.” *Philosophy of Science* 74: (2007) 150–175.

Sklar, Lawrence. “Statistical Explanation and Ergodic Theory.” *Philosophy of Science* 40: (1973) 194–212.

———. “Probability as a Theoretical Concept.” *Synthese* 40: (1979) 409–414.

———. *Physics and Chance*. Cambridge: Cambridge University Press, 1993.

Sładkowski, Jan. “Exotic Smoothness and Astrophysics.” *Acta Physica Polonica B* 40: (2009) 3157–3163.

Smeenk, Chris. *Approaching the Absolute Zero of Time: Theory Development in Early Universe Cosmology*. Ph.D. thesis, Unversity of Pittsburgh, Pittsburgh, PA, 2003.

———. “False Vacuum: Early Universe Cosmology and the Development of Inflation.” In *The Universe of General Relativity*, edited by A. J. Kox, and Jean Eisenstaedt, Boston: Birkhäuser, 2005, chapter 13, 223–257.

———. “The Logic of Cosmology Revisited.”, 2008. Unpublished Essay.

———. “Philosophy of Cosmology.” In *The Oxford Handbook of Philosophy of Physics*, edited by Robert Batterman, Oxford: Oxford University Press, 2013, chapter 17, 607–652.

———. “Predictability crisis in early universe cosmology.” *Studies in History and Philosophy of Modern Physics* 46: (2014) 122–133.

Smoot, George, Marc Gorenstein, and Richard Muller. “Detection of Anisotropy in the Cosmic Blackbody Radiation.” *Physical Review Letters* 39: (1977) 898–901.

Sober, Elliott. “Evolutionary Theory and the Reality of Macro-Probabilities.” In *The Place of Probability in Science*, edited by Ellery Eells, and James Fetzer, Berlin: Springer Verlag, 2010, volume 284 of *Boston Studies in the Philosophy of Science*, 133–161.

Souriau, Jean-Marie. *Structure of Dynamical Systems*. New York: Springer, 1997.

Starobinsky, Alexei. "A New Type Of Isotropic Cosmological Models Without Singularity." *Physics Letters B* 91: (1980) 99.

———. "Dynamics of phase transition in the new inflationary universe scenario and generation of perturbations." *Physics Letters B* 117: (1982) 175–178.

Stein, Howard. "On the Notion of Field in Newton, Maxwell and Beyond." In *Historical and Philosophical Perspectives of Science*, edited by Roger Stuewer, Minneapolis, MN: University of Minnesota Press, 1970, volume 6 of *Minnesota Studies in the Philosophy of Science*, 264–287.

Stewart, John. "Perturbations of Friedmann-Robertson-Walker cosmological models." *Classical and Quantum Gravity* 7: (1990) 1169–1180.

Stewart, John, and Martin Walker. "Perturbations of Space-Times in General Relativity." *Proceedings of the Royal Society A: Mathematical, Physical and Engineering Science* 341: (1974) 49–74.

Swinburne, Richard. "Cosmological Horizons." *Philosophy of Science* 33: (1966) 210–214.

Tavakol, Reza. "Fragility and Deterministic Modelling in the Exact Sciences." *British Journal for the Philosophy of Science* 42: (1991) 147–156.

Tavakol, Reza, and George Ellis. "On the Question of Cosmological Modeling." *Physics Letters A* 130: (1988) 217–224.

Tegmark, Max. "Parallel Universes." In *Science and Ultimate Reality: Quantum Theory, Cosmology, and Complexity*, edited by John Barrow, Paul Davies, and Charles Harper, Cambridge: Cambridge University Press, 2004, 459–491.

Teh, Nicholas. "Gravity and Gauge." *British Journal for the Philosophy of Science* 0: (forthcoming) 1–34.

Torretti, Roberto. *Relativity and Geometry*. New York: Dover, 1996.

Tsujikawa, Shinji, Parampreet Singh, and Roy Maartens. "Loop quantum gravity effects on inflation and the CMB." *Classical and Quantum Gravity* 21: (2004) 5767–5775.

Turok, Neil. "A critical review of inflation." *Classical and Quantum Gravity* 19: (2002) 3449–3467.

Uffink, Jos. "Compendium of the Foundations of Classical Statistical Physics." In *Philosophy of Physics*, edited by Jeremy Buttefield, and John Earman, Amsterdam: Elsevier, 2007, volume B of *Handbook of the Philosophy of Science*, 923–1074.

———. “Subjective Probability and Statistical Physics.” In *Probabilities in Physics*, edited by Claus Beisbart, and Stephan Hartmann, Oxford: Oxford University Press, 2011, chapter 2, 25–49.

Vachaspati, Tanmay, and Mark Trodden. “Causality and cosmic inflation.” *Physical Review D* 61: (1999) 1–4.

van Fraassen, Bas. *The Scientific Image.* Oxford: Oxford University Press, 1980.

van Lith, Janneke. “Ergodic Theory, Interpretations of Probability and the Foundations of Statistical Mechanics.” *Studies in History and Philosophy of Modern Physics* 32: (2001) 581–594.

Volchan, Sérgio. “Probability as typicality.” *Studies in History and Philosophy of Modern Physics* 38: (2007) 801–814.

von Plato, Jan. “Probability in Dynamical Systems.” In *Logic, Methodology and Philosophy of Science VIII*, Amsterdam: Elsevier, 1989, 427–443.

———. *Creating Modern Probability.* Cambridge: Cambridge University Press, 1994.

Wainwright, John, and George Ellis, editors. *Dynamical Systems in Cosmology.* Cambridge: Cambridge University Press, 1997.

Wald, Robert. *General Relativity.* Chicago: University of Chicago Press, 1984.

———. “Correlations Beyond the Horizon.” *General Relativity and Gravitation* 24: (1992) 1111–1116.

———. *Quantum Field Theory in Curved Spacetime and Black Hole Thermodynamics.* Chicago: University of Chicago Press, 1994.

———. “The arrow of time and the initial conditions of the universe.” *Studies in History and Philosophy of Modern Physics* 37: (2006) 394–398.

Wallace, David. “Time-dependent symmetries: the link between gauge symmetries and indeterminism.” In *Symmetries in Physics*, edited by Katherine Brading, and Elena Castellani, Cambridge: Cambridge University Press, 2003, chapter 9, 163–173.

———. “Gravity, Entropy, and Cosmology: in Search of Clarity.” *British Journal for the Philosophy of Science* 61: (2010) 513–540.

———. *The Emergent Multiverse.* Oxford: Oxford University Press, 2012.

———. “Probability in Physics: Stochastic, Statistical, Quantum.” In *Chance and Temporal Asymmetry*, edited by Alastair Wilson, Oxford: Oxford University Press, 2014, chapter 10, 194–220.

———. "The quantitative content of statistical mechanics." *Studies in History and Philosophy of Modern Physics* 52: (2015) 285–293.

———. "The Logic of the Past Hypothesis." In *Time's Arrows and the Probability Structure of the World*, edited by Barry Loewer, Brad Weslake, and Eric Winsberg, Cambridge, MA: Harvard University Press, forthcoming.

Weatherall, James. "Fiber bundles, Yang-Mills theory, and general relativity." *Synthese* 0: (forthcoming a) 1–37.

———. "Regarding the "Hole Argument"." *British Journal for the Philosophy of Science* 0: (forthcoming b) 1–26.

Weinberg, Steven. *Cosmology*. Oxford: Oxford University Press, 2008.

Weinstein, Steven. "Gravity and Gauge Theory." *Philosophy of Science* 66: (1999) S146–S155.

Werndl, Charlotte. "Are deterministic descriptions and indeterministic descriptions observationally equivalent?" *Studies in History and Philosophy of Modern Physics* 40: (2009) 232–242.

———. "On the observational equivalence of continuous-time deterministic and indeterministic descriptions." *European Journal for Philosophy of Science* 1: (2011) 193–225.

———. "Evidence for the Deterministic or the Indeterministic Description? A Critique of the Literature About Classical Dynamical Systems." *Journal for General Philosophy of Science* 43: (2012) 295–312.

———. "Justifying typicality measures of Boltzmannian statistical mechanics and dynamical systems." *Studies in History and Philosophy of Modern Physics* 44: (2013) 470–479.

———. "Determinism and Indeterminism." In *The Oxford Handbook of Philosophy of Science*, edited by Paul Humphreys, Oxford: Oxford University Press, forthcoming.

Werndl, Charlotte, and Roman Frigg. "Reconceptualising equilibrium in Boltzmannian statistical mechanics and characterising its existence." *Studies in History and Philosophy of Modern Physics* 49: (2015) 19–31.

Weslake, Brad. "Statistical Mechanical Imperialism." In *Chance and Temporal Asymmetry*, edited by Alastair Wilson, Oxford: Oxford University Press, 2014, chapter 12, 241–257.

White, Martin, Douglas Scott, and Joseph Silk. "Anisotropies in the Cosmic Microwave Background." *Annual Review of Astronomy and Astrophysics* 32: (1994) 319–370.

Williams, Porter. “Naturalness, the autonomy of scales, and the 125GeV Higgs.” *Studies in History and Philosophy of Modern Physics* 51: (2015) 82–96.

Wilson, Mark. “On the Mathematics of Spilt Milk.” In *The Growth of Mathematical Knowledge*, edited by Emily Grosholz, and Herbert Breger, Berlin: Springer Verlag, 2000, volume 289 of *Synthese Library*, 143–152.

Winsberg, Eric. “Can Conditioning on the “Past Hypothesis” Militate Against the Reversibility Objections?” *Philosophy of Science* 71: (2004) 489–504.

———. “Laws and chances in statistical mechanics.” *Studies in History and Philosophy of Modern Physics* 39: (2008) 872–888.

Wolf, Joseph. *Spaces of Constant Curvature*. Providence, RI: AMS Chelsea Publishing, 2010, 6th edition.

Wu, Kelvin, Ofer Lahav, and Martin Rees. “The Large-Scale Smoothness of the Universe.” *Nature* 397: (1999) 225–230.

Zalaletdinov, Roustam. “Averaging Out the Einstein Equations.” *General Relativity and Gravitation* 24: (1992) 1015–1031.

———. “The Averaging Problem in Cosmology and Macroscopic Gravity.” *International Journal of Modern Physics A* 23: (2008) 1173–1181.