Under-determination in Cosmology: an Invitation

Jeremy Butterfield
Trinity College, Cambridge CB2 1TQ: jb56@cam.ac.uk

The Presidential Address to the Mind Association: Stirling, Scotland, July 2012

Mon 9 January 2012

Abstract

I discuss how modern cosmology illustrates under-determination of theoretical hypotheses by data, in ways that are different from most philosophical discussions.

I confine the discussion to the history of the observable universe from about one second after the Big Bang, as described by the mainstream cosmological model: in effect, what cosmologists in the early 1970s dubbed the 'standard model', as elaborated since then. Or rather, the discussion is confined to a (very!) few aspects of that history.

I emphasise that despite the under-determination, a scientific realist can, and should, endorse this description.

Contents

1 Introduction

I propose to advertise the philosophy of cosmology (Section 2); and by way of example, to sketch how modern cosmology illustrates kinds of under-determination of theoretical hypotheses by data that are different from those treated in most philosophical discussions (Section 3).

I can summarize the differences as follows. The usual philosophical discussion of under-determination idealizes by assuming that all possible observations are 'given', though of course they are spread through spacetime, i.e. never collected by a single scientific community. Then the discussion addresses whether the theory of tiny unobservable objects like electrons is under-determined; and relatedly, how good is our warrant for our accepted physical theories. But cosmology takes due cognizance of the difficulties of observing the distant universe and of collecting together observations; though of course it does not address the general philosophical debate about our warrant for our accepted theories, in particular our theories of tiny unobservable objects. This outlook is entrenched in cosmology's jargon—which I adopt. Thus cosmologists use *observable universe* to mean (roughly speaking) 'the past light-cone of Earth-now, and all physical events within it, even microscopic ones'; as against, say, 'all macroscopic or humanly observable events, anywhere in spacetime'.[1] Thus in cosmology, the issue of under-determination is not about our warrant for the general theories we accept, such as general relativity and quantum theory: but about whether the data about the observable universe determine a single model of these theories as being correct.

I confine the discussion to the history of the observable universe from about one second after the Big Bang, as described by the mainstream cosmological model: what cosmologists in the early 1970s dubbed the 'standard model', as elaborated since then. Or rather, the discussion is confined to a (very!) few aspects of that history.

I will also claim that under-determination in cosmology is entirely compatible with scientific realism. This is worth emphasizing since much discussion, in philosophy as well as in popular science, focuses on times earlier than about a second, and thereby on energies so high that the physics, let alone the philosophy, becomes controversial. The result is loose talk—and an unfairly bad press for scientific realism.

2 Cosmology as a spur to philosophy

As a branch of philosophy of physics, the philosophy of cosmology is less developed than the philosophy of quantum physics, or relativity or thermal physics. But it is an alluring field, for several reasons.

First, philosophy is about big questions: and our topic could hardly be larger! When

[1] Agreed, cosmologists also use 'observable universe' more restrictedly, reflecting their concern with what can be ascertained by observation. Thus it often means instead: 'the events in the past light-cone of Earth-now, and to the future of the decoupling time'. For the meaning of this last phrase, cf. the end of Section 2.2.

we are told that we now know that a second after the Big Bang, the observable universe had a temperature of about 10^{10} K and a density of about 2000 kilograms per cubic centimetre, surely every philosopher feels a school-child's thrill—quickly followed by worrying how we could ever know such a proposition? I take this time, temperature and density as my example because, as will become clearer below, we now know a lot about the observable universe's history from that epoch onwards.

Secondly, there have been countless connections between philosophy and cosmology: not just over the centuries, with figures like Aristotle and Kant, but also since the rise of scientific cosmology in the mid-twentieth century. That rise undoubtedly represents the third golden age of cosmology; (the first two being the third and second centuries BCE, with Epicurus, Aristarchus and Hipparchus, and the scientific revolution from Copernicus to Newton).[2]

For philosophy of science, that rise is a spur to reflection, along many avenues. Here are three well-explored ones. First: general relativity admits cosmological models in which time has a beginning and-or space is finite: which obviously bears on old metaphysical debates, dating from Kant's antinomies and earlier, about whether such scenarios are really possible. Second: general relativity admits closed timelike curves, i.e. curves which are future-pointing at every spacetime point in the curve but which form a loop: which suggests circles of causation. Both these cases concern the global as against local structure of spacetime. But third: general relativity's abandoning euclidean geometry as space's local structure had an even greater impact on philosophy, including general epistemology, through such figures as Reichenbach, Kuhn, Grunbaum, Putnam and Earman.[3]

So much by way of describing the general landscape. I turn to introducing my chosen topic, the under-determination of theoretical hypotheses by data.

2.1 Under-determination in cosmology

Under-determination in cosmology exhibits two main differences from the usual philosophical discussion:

(i): a different construal of the idea of 'all possible observations'; and

(ii): in cosmology, it is for the most part *not* theories, but rather their models, that are under-determined.

The modality in (i) has of course various construals: we will see in Section 3 that cosmology introduces some distinctive ones.

[2]Ellis (2007) is a recent masterly survey of the philosophical issues raised by modern cosmology; for this paper's topic, cf. his Sections 4, 5 pp. 1220-1234. Other fine surveys include several of his earlier papers (e.g. 1975, 1991, 1999, 1999a) and Smeenk (2012). Longair (2006) is a recent masterly history of twentieth-century cosmology; Kragh (1996) is a masterly, less technical history up to about 1970. Another reason for advertising the philosophy of cosmology, given our paymasters' now requiring 'outreach' and 'impact' from us, is that cosmology is one of the few sciences that engages the passionate interest of the wider public.

[3]Masterly surveys of these three avenues include: Earman (1989, 1995), Torretti (1983), and Ryckman (2005). Of course, there are several other such avenues: such as the changing nature of observation as instruments develop, or the role of cosmology in explaining the direction of time.

As to (ii), a theory usually comprises many possible solutions or models: i.e. histories—temporal sequences of states—for the type of system it is concerned with. So even granted a theory, there can be under-determination of which model or history is correct. This is bound to threaten any historical science, such as cosmology or palaeontology, which aims to describe a very complex, albeit single, history. And this threat arises even though the science addresses only certain features of the history, such as the production and preservation of fossils. Thus we will be concerned, not with choosing between rival general cosmological theories, nor with the obvious fact that most of the detailed history of the universe (as of the Earth) is bound to remain forever unknown; but with whether the main features of that history addressed by cosmology—the primeval fireball, its expansion and cooling, the formation of stars and galaxies, the synthesis of elements—are under-determined. As I mentioned, I will maintain, in the spirit of scientific realism, that in fact we now know a lot about these features.

Mention of historical sciences raises an old chestnut: whether there can be a science of cosmology given that there is only one universe. This preoccupied philosophers of cosmology in the mid-twentieth century, in connection with (i) speculations that cosmology would involve distinctive laws that go beyond local physics, and (ii) debate about principles that distinguished the Big Bang and steady-state cosmologies (cf. e.g. Balashov (1994, pp. 935-944), Kragh (1996, pp. 220-233, 240-246)).

I think it is now widely agreed that there is no problem here. But for clarity, it is worth spelling out why not. Even if laws need many instances, as Humeans like me say, laws of cosmology do not need many universes. Rather, the laws of cosmology—in so far as we know them—are the established laws of relativistic and quantum physics applied in regimes relevant to understanding the universe as a whole. And this tends to mean: (a) relativistic physics being applied on very large scales, at late times like now; and (b) relativistic and quantum physics applied together on much smaller scales, at early times. Here (b) means: quantum field theory on a curved spacetime background. And my 'in so far as we know them' is a deferential nod in the direction of the *terra incognita* of a quantum theory of gravity.

Of course (a) and (b) are linked: indeed, linked by one of the great over-arching narratives of cosmology's progress in recent decades. Namely: high-energy events in the early universe provide us, through their present-day relics observable on cosmological scales, with the 'ultimate laboratory' for testing quantum field theories at high energy (Longair 2006, p. 335, 407, 444). One main aspect of this is structure formation: the present-day large-scale structures of the universe—especially, galaxies—are nowadays believed to have originated from tiny early-universe inhomogeneities which are quantum in nature.[4]

Here we must beware of confusingly different jargons. Cosmologists often use 'theory' only for a very general theory such as general relativity or quantum theory, and use

[4]Agreed, the origin of *these* is unknown. Suffice it to say here that most popular models of the very early universe (i.e. for times well before one second) postulate a quantum field, the inflaton, the quantum fluctuations of which during an exponential expansion of spacetime at *very* early times (about 10^{-35} seconds to 10^{-33} seconds!) are later amplified and 'frozen out', so as to produce the observed large-scale structures.

'model' for its application to a specific system, even a 'total' system such as the universe. Thus what philosophers would call 'the Big Bang cosmological theory' was called by cosmologists 'the standard model', already in the early 1970s (Weinberg (1972, p. 469), Misner et al. (1973, p. 763)): the honorific name reflecting its recent confirmation by the discovery of the cosmic background radiation. The model has of course been much elaborated since then, so as to incorporate such major new ideas as cold dark matter and dark energy.

But even as thus elaborated, this model represents, not a single possible history of (the main features of) the universe, but a family of possible histories, differing in various ways: in particular in the values of several parameters, called 'cosmological parameters'—which it is the business of observational cosmology to try and determine. Thus cosmologists tend to speak of observations deciding between versions of this model, which is taken as being established; whereas philosophers would tend to speak of observations deciding between models of a cosmological theory.

These differing jargons are confusing, since in a philosopher's ear, the cosmologists' jargon can suggest (contrary to one of my claims) that cosmology faces no threat of under-determination of models. Thus they might say, 'the standard model is established'. But I have adopted the philosophers' jargon, so that 'model' represents a single history (more precisely: the main features of a single history). And in that sense, all hands must agree there is a threat that models are under-determined, even by all possible observations.[5]

Finally, here is a general motivation for choosing under-determination as a topic: it can be addressed using well-known philosophical notions, and well-established physics. As regards philosophy: we will see in Section 3 that cosmology gives vivid cases of under-determination of theoretical hypotheses by data. And the usual sorts of grounds, such as simplicity and explanatory power, can be and have been adduced as breaking the under-determination.

As regards physics: the *pros* and *cons* of the alternatives can be discussed in terms of various versions of, and some respectable rivals to, the standard model. Of course, a brief philosophical paper such as this can, and needs must, exclude all the details of physics. My point is just that by not considering times much before one second, in particular not considering inflation, we can stick to well-established physics.

This may make my topic seem tame—business as usual—both philosophically and physically. But I think it is no less interesting on that account: (there is a surely too much 'gee-whiz' in popular cosmology; so it is good to see that philosophy of cosmology need not follow suit). In any case, this topic will lead on to other questions, which are not only alluring, but as wild as you could wish for, both philosophically and physically. The

[5] A qualification, reflecting the fact that the theory-model contrast is of course vague. (Thanks to C. Beisbart, W. Stoeger and H. Zinkernagel for emphasizing this to me.) I do not mean to suggest that the standard model is established beyond doubt. There is respectable dissent: for example, there are rival inhomogeneous models without dark energy (cf. e.g. Ellis (2007, Section 4.2.2, pp. 1223-1227; 2011, Section 4, pp. 11-15), Nadathur et al (2011)). So a philosopher might well speak of a dispute over, or under-determination of, theories; though the dispute may well be resolved by observations over the next decade or two.

philosophical questions concern the explanation of initial conditions, and-or the values of physical constants: especially in relation to the ideas of selection effects (the anthropic principle), and a multiverse. The physical questions concern the universe at times much earlier than one second after the Big Bang: i.e. much earlier in logarithmic terms, such as 10^{-35} seconds, when inflation is supposed to have started. Proposals for the physics at those times are very speculative, since the energies, densities, pressures etc. far outstrip what we have evidence for—and may forever do so.

2.2 Defending scientific realism

The prospect of under-determination means that as a scientific realist, I owe you a statement of my position. Indeed: since I started with a description of the observable universe's temperature and density one second after the Big Bang, you are bound to ask whether I really believe it, and similar such propositions about the universe from then on, which cosmologists nowadays tell us in countless papers and books.

Credo: I answer, as a scientific realist: Yes I do, and claim that every scientific realist should. Despite subsequent Sections' focus on under-determination, a scientific realist can, and should, believe the main claims of today's standard model's description of what happened in the observable universe from about one second after the Big Bang onwards. The under-determination to be discussed in Section 3 does *not* prevent those claims being correct. And I believe that those claims are now, and will forever remain, as well established as countless other scientific facts, e.g. that the sun's surface temperature is about 5000 K, plants photosynthesize, and insulin has fifty-two amino acids.

Three clarifications of this *credo* are needed. All are straightforward. The first, in brief, is that scientific realism implies belief only when the evidence is sufficiently plentiful and varied. Of course, there can be no general statement of what would be 'sufficient'. And even for a specific topic which the scientific community regards as well understood, such as synthesis of atomic nuclei starting at about one second after the Big Bang, there are, often or always, details that remain recalcitrant (for this topic, the abundance of lithium is an example); and so there is room for rational dissent. But if the evidence is thin, we should of course all be more cautious.

So here I should admit two broad ways in which for cosmology, the evidence is indeed thin—and so clarify my weasel words 'main claims' and 'today's standard model'.

(1): As I said in Section 2.1, most of the detailed history of the universe will of course remain forever unknown. Besides, as I hinted there, major causal and structural factors are not understood, such as the nature of dark matter and dark energy, and the process of galaxy formation. Thus there are many versions of 'today's standard model'—and besides, various respectable rivals. In assessing these versions and rivals, we still have a lot to learn. But a great deal *is* now established. One main example is the overall thermal history of the observable universe. We know that about 13 billion years ago, there was an extremely hot and dense 'fireball' in which light nuclei (like hydrogen and helium) were synthesized, according to well-understood nuclear physics; it cooled and expanded, with the details being accurately described by quantum theory, thermodynamics and general relativity;

later, gravitational clustering led to galaxies and stars, in which still more elements were synthesized. All this is today as certain as the fact that insulin has fifty-two amino acids.[6]

(2): The evidence is also thin for times that are, in terms of physics, much before one second. Here one needs to think logarithmically: times like 10^{-10} or even 10^{-3} seconds count as much earlier than a second, since the temperatures, densities, energies etc. rise in a similar exponential way. Nowadays, it is common to take the boundary between known and speculative physics to be at about 10^{-11} seconds, before which the energies are too high for us to be confident that the (namesake) standard model of particle physics applies. But there is much we still do not understand about the energy regimes that *are* described by that namesake standard model, compared with the energies at which nuclei are synthesized, at about 1 second. Hence my cautiously taking the threshold of trust to be about 1 second.

I turn to my second clarification. Not only should we *now* be agnostic about regimes for which data are lacking and theory is speculative. It is also reasonable to expect that, however fortunate we may later be in developing theories and making observations, we shall never know all, or even much, about arbitrarily early times. And *a fortiori*, the same goes for the Big Bang itself—whatever exactly it was.

Two more specific points, touched on above, support this scepticism that we will ever understand cosmogony. (1): We might live in one part of a multiverse, with evidence from other regions that would be crucial to such understanding remaining forever unavailable to us. (2): Understanding a general theory does not secure understanding of one of its solutions, or of a specific object described by the theory. So even if we had a quantum theory of gravity (a 'theory of everything'), still quantum cosmology would be a historical science; and we might never know or even be able to formulate or understand the specific details of the singular origin. This limitation could hold even if there is a rationalist transparent intelligibility about the laws of the theory of everything, of the kind Einstein hoped for.[7]

To sum up these two clarifications: such agnosticism is no problem for scientific realism. We realists believe that nowadays our best theories are good enough that they are mostly true. That is: they are supported by enough detailed and mutually independent lines of evidence, that we believe them. But this realism does not imply that we can, now or ever, know or have warranted belief, or even gather any evidence, about all parts, or even every aspect of any single part, of the universe.

[6]Thus authoritative textbook descriptions of this thermal history, written over the last forty years, largely agree with each other. Cf. for example: Sciama (1971: Chapters 8, 12-14), Weinberg (1972, Chapter 15.6, pp. 528-545), Wald (1984, pp. 107-117), Barrow and Tipler (1988, pp. 367-408, Sections 6.1-6.7), Lawrie (1990, pp. 315-326), Longair (2006, 394-399), Weinberg (2008, pp. 101-113, 149-173; Sections 2.1, 2.2, 3.1, 3.2). For fine popular accounts, cf. Silk (1989, Chapters 6 to 8), Rowan-Robinson (1999, Chapter 5), Silk (2006: pp. 112-128). Besides, the thermal history of the universe is by no means the only 'main claim'—or rather: 'grand narrative'!—that is now firmly established. The theory of stellar structure and evolution is another example, for which one could similarly cite authoritative descriptions over several decades largely agreeing with each other.

[7]For philosophical discussion of the search for quantum gravity, cf. e.g. Ellis (1999, pp. 708-718; 1999a, pp. 62-65), Rovelli (1999, 2007), Butterfield and Isham (2001). For the Big Bang in relation to the cosmological argument for theism, cf. Pitts (2008).

The third clarification is that my *credo* is not intended to deny conceptual change or meaning variance: neither during the past process of establishing the present consensus about the universe's history after one second, nor during future research about that history. As to the past process, the point is agreed by all. Section 2.1 mentioned some examples that have long since become standard in philosophy: general relativity's allowance that time had a beginning, or that space is finite, or of closed timelike curves, or of non-euclidean geometry. As to the future, it is of course impossible to cite examples. No one can say whether today's problem of primordial lithium abundance will turn out to be an all-too-recalcitrant anomaly, harbouring some radical change, like the perihelion of Mercury did a century ago. Nor can we be confident that any such anomaly will concern only such amazingly early epochs as one second after the Big Bang or earlier. It might concern a much later epoch: for example, 400,000 years later, when the cosmic microwave background radiation decoupled from matter. The physics of that decoupling is intricate (Weinberg 2008, pp. 116-125, Section 2.3): and crucial for cosmology, since nowadays that radiation (predicted in 1950 and discovered in 1965) is our main 'direct' evidence for the Big Bang. Besides, we cannot now directly observe (by any electromagnetic means) events that occurred earlier than that epoch, since until that time the universe was opaque to radiation. And in general, it is bound to be controversial, even among scientific realists, and for a given science or theory, how much, and how radical, future conceptual change to envisage; (cf. e.g. Sklar (2010)). So even for the standard cosmological model, I will not venture a general statement about this.

So much by way of clarifications. Let me summarize by emphasizing that to trust today's main claims for times later than one second after the Big Bang, is not reckless or maverick: witness Rees' remark that he is 99% confident of this account (2003, pp. 24, 31; 1997, p. 65, 174).

3 Under-determination 'in practice', not 'in principle'

Section 3.1 briefly discusses how cosmology prompts some special considerations about under-determination. Then Section 3.2 gives some details by reporting recent results of Manchak (2009, 2011).

3.1 Four contrasts

In Section 2.1, I announced that cosmology would involve two main contrasts from the usual philosophical debate about whether all possible observations might fail to decide which of some set of alternative theories is correct. I shall first re-state these two contrasts; and then sketch two other mistier contrasts.

The tenor of all four contrasts (and of Section 3.2) will be that cosmology provides examples of under-determination 'in practice', rather than examples 'in principle' of the type philosophers usually focus on. But it will be obvious that the relevant senses of 'in practice' are far removed from the genuine practicalities of observation, or more generally, of assessing evidence. Thus I submit that the examples are of philosophical interest: they are not 'merely' (as someone snooty about science might say) a scientific challenge as to how to get more data to decide between alternatives.

(1): The first contrast is that in philosophical discussions of cosmology, 'all possible observations' is often construed as the observations that could be made by a single observer who lives forever dutifully collecting observations of all events in their past light-cone. This construal is made precise using the idea of a spacetime model (i.e. a possible course of history throughout all space and time), and allied ideas like the past light-cone: details in Section 3.2. (Of course, the analysis remains the same if instead of an immortal single observer, one considers, a bit more realistically, an eternal dynasty of observers with each generation bequeathing its observations to the next.) On the other hand, much philosophical debate (for example, over van Fraassen's advocacy of constructive empiricism, or Putnam's rebuttal of metaphysical realism) has construed 'all possible observations' with no regard to 'bringing it all together': i.e. as the set of all observations made by all possible observers located throughout spacetime—a set which, in most spacetime models, could never be collected together and collated.

(2): The second contrast is that in cosmology, we are faced with deciding between different models: under-determination is a matter of all possible observations, in the above 'eternal-observer' sense, not deciding between many different histories allowed by our theory. On the other hand, philosophers have usually construed under-determination as about *theories*: empirically equivalent, but theoretically radically different, theories. (As I mentioned in footnote 5: I agree that this contrast is not hard and fast, since the theory-model contrast is vague, and there is respectable dissent from what I dubbed 'our theory', i.e. the standard model.)

These two contrasts clarify Section 2.2's *credo* avowing that scientific realism is in no way undermined by our perhaps being forever unable to choose between rival theories of the earliest moments of the universe. In short: the boot is now on the other foot. That is: precisely because those earliest moments are so infernally hard to get evidence about, the usual philosophical construals of under-determination may apply. We may be forever unable to choose between different theories (not 'merely' models) of those moments: even when we envisage having access to 'all possible observations', even on some very liberal construal that disregards the 'mere practicalities' of someone collecting and collating the evidence; and even when we also envisage endorsing some powerful methodological principles of theory-selection.

There are also two other, mistier, contrasts; which I mention but will not pursue in detail.

(3): One arises from this last remark about Section 2.2's *credo*. It is the contrast between the predicaments of:

(a) being unable to choose between alternative theories we have formulated, and

(b) being unable to formulate a theory of the topic in question, and not because of lack of evidence or ingenuity or funding or ... but because we have good reason to doubt *any* of our concepts can apply to the topic.

For the topic of the very early universe or the Big Bang, one obvious suggestion for how we might face the second predicament, (b), is the Kantian thought that all our concepts and theories presuppose in some way claims about space and time that these topics violate. A second suggestion is the operationalist thought that scientific concepts must be potentially measurable; and that since at temperatures like 10^{10} K, we—or any being—would be hard pressed to measure anything, e.g. time, distance or temperature itself, our concepts break down. A third suggestion is the relationist thought that, irrespective of measurement, the concept of time and-or space needs to be appropriately anchored in matter: Rugh and Zinkernagel (2009) develop this suggestion in detail; (cf. also Zinkernagel (2002, pp. 510-514), Rugh and Zinkernagel (2011, pp. 419-422).

(4): This leads to the fourth and last contrast. For some recent philosophical discussion of under-determination of theory by data focusses on alternative theories that are *not* (but could be) formulated at the time in question; and it explicitly sets aside under-determination of models of a given theory of the sort which will concern me.

Thus Stanford, in the opening pages of his recent monograph (2006, p. 14), sets aside the sort of under-determination we will see in Section 3.2. His idea is that since our cosmological theory itself describes how we cannot have evidence to decide between the alternatives in question, this sort of under-determination is of little philosophical interest. The scientific realist could and should reply that they are realists only about theoretical claims that are 'amenable to empirical investigation'.[8] His book goes on to argue against realism that in the past, well-confirmed scientific theories often have had theoretically radically different alternatives, which were *not* conceived at the time, but which would have been at least as well confirmed by the available evidence (2006, p. 19).

I will not try to assess Stanford's claims and the ensuing debate: except to commend realists such as Chakravartty (2008, Sections 3, 4) who reply that Stanford's challenge: (i) is closer to the traditional 'pessimistic meta-induction' than he allows, and (ii) is answered by some of our theories giving us detailed causal knowledge of objects and properties. (I also commend Lipton's lucid and pre-cognitive (1993) reply to Stanford.)

But I submit that, however this debate is resolved, the conclusions are likely to be distinctly philosophical. In particular, being convinced by Stanford that one's current theory has an equally well-confirmed but *un*conceived alternative cannot much influence one's research. (Similarly, being convinced by Kant that one could not have a theory of a non-spatiotemporal topic could influence one's research on such a topic only by prompting one to give it up altogether.) On the other hand, in the more 'local' setting of deciding between models of a given theory, rather than entire theories—in our setting: of deciding between general relativistic cosmological models—there is better hope for one's philosophy being in contact with the scientific practice; as well as better scientific prospects for escaping the under-determination.

[8]Stanford also suggests the situation here is like the standard example of alternatives in Newtonian gravitation theory (say, for point-particles) differing about the velocity of the universe's material contents with respect to absolute rest (2006, p.13); or even like the example of alternatives obtained by conjoining arbitrary propositions to any given theory (2006, p.15). I think the three cases are importantly different, but I will not pursue the point.

3.2 Observationally indistinguishable spacetimes

I turn to illustrating under-determination of cosmological models in general relativity, by reporting the theorems of Manchak (2009, 2011); (which build on ideas and results of Glymour (1977) and Malament (1977)). Roughly, the theorems say that in almost every spacetime obeying general relativity, no observer, however long they live, could accumulate enough observations to exclude their being in another very different spacetime. The theorems also give a glimpse of the kind of result nowadays obtained in the philosophy of general relativity.

Some notation and jargon.

(1): In general relativity, a model is given by a spacetime (M, g) consisting of a four-dimensional manifold M of spacetime points, equipped with a metric tensor g, which generalizes special relativity's metric including light-cone structure.

(2) A region of one model, $U \subset M$, is *isometric* to another region of another model, $U' \subset M'$, if there is a suitable function $d : U \to U'$ that carries M's metric g as restricted to U into M''s metric g' as restricted to U'. This means full knowledge of all the metrical relations between points in U could not exclude one's being instead in U'.

(3): Of course, one usually knows much less than everything about metrical relations, and more than nothing about spacetime's matter and radiation content: which so far, seems culpably unrepresented in our notion, (M, g), of a cosmological model. But the theorems and discussion can probably be adapted to include facts about matter and radiation; (Malament 1977, pp. 74-76).

(4): I turn to representing the past light-cone of (an observer at a) spacetime point $p \in M$. This is the region from which signals travelling at most as fast as light can reach p. So it is the largest region that an idealized observer at p could observe (setting aside pre-cognition!). But it turns out, for technical reasons, to be easier to work with the interior of a point p's past light-cone, i.e. the points connectible to p by signals travelling slower than light. This is dubbed the *chronological past* and written $I^-(p)$.

Summing up this notation and jargon: Manchak's theorems show that an ideal observer at $p \in M$ who knows the full metric structure of $I^-(p)$ cannot know much about the global structure of her spacetime, since many different spacetimes, with widely varying global properties, have a region isometric to $I^-(p)$. More precisely: let us say that a spacetime (M, g) is *observationally indistinguishable* from (M', g') iff for all points $p \in M$, there is a point $p' \in M'$ such that $I^-(p)$ and $I^-(p')$ are isometric. (The fact that this notion is asymmetric will not matter.) Then the gist of the theorems is that almost every spacetime is observationally indistinguishable from another, i.e. a non-isometric spacetime.

The theorems also veto an observer's ascertaining some global properties of her spacetime. Manchak lists four such properties. Three are 'good causal behaviour' properties; e.g. one is that the spacetime be globally hyperbolic, a notion which allows a strong form of determinism. The fourth is spatial isotropy, i.e. there being, at every spacetime point, no preferred spatial direction: an assumption which is made by the standard cosmological model, and more generally, is central to modern cosmology (under the name 'the cosmological principle'). Thus the theorems veto an observer's ascertaining these properties:

given a spacetime (M, g) with any or all of these properties, there is an observationally indistinguishable spacetime with none of them.

One might worry about the scientific significance of the theorems. For the proofs build the non-isometric spacetime (M', g') using a mathematical 'cut-and-paste' construction, which might therefore look 'unphysical'. But that vague word covers several possible misgivings. One is that the theorems entirely concern *classical* general relativity, which we have every reason to believe fails for the extreme conditions in the very early universe and black holes. But this points beyond standard cosmology—and so beyond this paper.

Within classical general relativity, the theorems are not so easily dismissed. That we define a spacetime model by a cut-and-paste construction is no evidence at all that the features it exhibits are not generic among general relativity's models. And as to the specific four global properties that Manchak shows to be impossible to ascertain observationally: he reviews various authors' rationales for requiring one or more of these properties, concluding sceptically that none of these rationales are convincing (2011, Sections 5 and 6).

To conclude: I concur with Manchak's sceptical conclusion. Besides, it meshes with two other recent assessments, which I also commend. Norton (2011, especially Sections 5 and 6) connects Manchak's theorems and others like them with his own advocacy of a material theory of induction; and he concludes that we are at a loss to justify the inductive inferences, which favour 'reasonable' spacetimes over apparently 'gerry-mandered' observationally indistinguishable alternatives, that we intuitively endorse. And Beisbart has focussed on the fourth property in Manchak's list: spatial isotropy which, as I mentioned, lies at the centre of modern cosmology, especially its cosmological principle. Beisbart (2009; Beisbart and Jung 2006) reviews the various justifications which, over the decades, have been offered for the cosmological principle, especially as a principle for breaking under-determination of models. He also is sceptical: the justifications are not compelling. Can we do better? That is an invitation ... to work for another day.

Acknowledgments: This paper is dedicated to the memory of Ernan McMullin, whose contributions to the philosophy of science were so wise and scholarly; and whose energetic and generous spirit enhanced so many lives. For correspondence and comments on previous versions, I am very grateful to various audiences, and especially to: Nazim Bouatta, Claus Beisbart, Fraser Macbride, John Manchak, John Norton, Brian Pitts, Chris Smeenk, Kyle Stanford and Henrik Zinkernagel. I also thank John Barrow and George Ellis for encouragement, and for the inspiration of their writings. A longer version of the paper, with additional sections on the cosmological principle, and on inflation, is on the Pittsburgh e-archive for philosophy of science.

4 References

Balashov, Y. (1994), 'Uniformitarianism in cosmology: background and philosophical implications of the steady-state theory', *Studies in History and Philosophy of Science* **25**, number 6, pp. 933-958.

Barrow, J. and Tipler, F. (1988), *The Anthropic Cosmological Principle*, Oxford University Press.

Beisbart, C. (2009), 'Can we justifiably assume the Cosmological Principle in order to break model under-determination in cosmology?', *Journal of General Philosophy of Science* **40**, pp. 175-205.

Beisbart, C. and Jung, T. (2006), 'Privileged, typical or not even that? Our place in the world according to the Copernican and cosmological principles', *Journal of General Philosophy of Science* **37**, pp. 225-256.

Butterfield, J. and Isham, C. (2001), 'Spacetime and the Philosophical Challenge of Quantum Gravity', in C. Callender and N. Huggett (ed.s), *Physics meets Philosophy at the Planck Scale*, Cambridge University Press, pp. 33-89; gr-qc/9903072; http://philsci-archive.pitt.edu/archive/00001915/

Chakravartty, A. (2008), 'What you don't know can't hurt you: realism and the unconceived', *Philosophical Studies* **137**, pp. 149-158.

Earman, J. (1989), *World Enough and Spacetime*, MIT Press.

Earman, J. (1995), *Bangs, Crunches, Whimpers and Shrieks*, Oxford University Press.

Ellis, G. (1975), 'Cosmology and verifiability', *Quarterly Journal of the Royal Astronomical Society*, **16**, pp. 245-264.

Ellis, G. (1991), 'Major themes in the relation between philosophy and cosmology', *Memorie della Societa Astronomica Italiana* **62**, pp. 553-605.

Ellis, G. (1999), 'Before the beginning: emerging questions and uncertainties', *Astrophysics and Space Science* **269-270**, pp. 693-720.

Ellis, G. (1999a), '83 years in general relativity and cosmology: progress and problems', *Classical and Quantum Gravity* **16**, pp. A37-A75.

Ellis (2007), 'Issues in Philosophy of Cosmology', in part B of J. Butterfield and J. Earman eds, *Philosophy of Physics*, Elsevier, volume 2 of the North Holland series, *The Handbook of Philosophy of Science*, pp. 1183-1286; astro-ph/0602280.

Ellis, G. (2011), 'Inhomogeneity effects in cosmology', *Classical and Quantum Gravity* **28**, 164001.

Glymour, D. (1977), 'Indistinguishable spacetimes and the fundamental group', in J. Earman, C. Glymour and J. Stachel (ed.s), *Foundations of Spacetime Theories*, Minnesota Studies in Philosophy of Science volume 8, University of Minnesota Press, pp. 50-60.

Kragh, H. (1996), *Cosmology and Controversy*, Princeton University Press.

Lawrie, I. (1990), *A Unified Grand Tour of Theoretical Physics*, Institute of Physics

Publishing, Adam Hilger.

Lipton, P. (1993), 'Is the best good enough?', *Proceedings of the Aristotelian Society 93* pp. 89-104; reprinted in D. Papineau (ed.) (1996), *The Philosophy of Science*, Oxford University Press Readings in Philosophy, pp. 93-106.

Longair, M. (2006), *Cosmic Century*, Cambridge University Press.

Malament, D. (1977), 'Observationally indistinguishable spacetimes', in J. Earman, C. Glymour and J. Stachel (ed.s), *Foundations of Spacetime Theories*, Minnesota Studies in Philosophy of Science volume 8, University of Minnesota Press, pp. 61-80.

Manchak, J. (2009), 'Can we know the global structure of spacetime?', *Studies in History and Philosophy of Modern Physics* **40**, pp. 53-56.

Manchak, J. (2011), 'What is a physically reasonable spacetime?', *Philosophy of Science* **78**, pp. 410-420.

Misner, C., Thorne K. and Wheeler, J (1973), *Gravitation*, W.H. Freeman.

Nadathur, S., Hotchkiss, S. and Sarkar, S. (2011), 'The integrated Sachs-Wolfe imprints of cosmic superstructures: a problem for ΛCDM', http://arxiv.org/abs/1109.4126v1.

Norton, J. (2011), 'Observationally indistinguishable spacetimes: a challenge for any inductivist', In G. Morgan, ed., *Philosophy of Science Matters: the Philosophy of Peter Achinstein* Oxford University Press, 2011, pp. 164-176. Available at:
http://www.pitt.edu/ jdnorton/papers/Obs_Equiv_final.pdf

Pitts, J. B. (2008), 'Why the Big Bang Singularity Does Not Help the Kalam Cosmological Argument for Theism', *British Journal for the Philosophy of Science* **59**, pp. 675-708.

Rees, M. (1997), *Before the Beginning*, Simon and Schuster.

Rees, M. (2003),'Our complex cosmos and its future', in G. Gibbons, E. Shellard and S. Rankin (ed.s), *The Future of Theoretical Physics and Cosmology*, Cambridge University Press, pp. 17-37.

Rovelli, C. (1999), 'Halfway through the woods', in J. Earman and J. Norton (eds.) *The Cosmos of Science*, University of Pittsburgh Press.

Rovelli, C. (2007), 'Quantum gravity', in J. Butterfield and J. Earman eds, *Philosophy of Physics*, Elsevier (volume 2 of the North Holland series, *The Handbook of Philosophy of Science*, part B, pp. 1287-1330.

Rowan-Robinson, M. (1999), *The Nine Numbers of the Cosmos*, Oxford University Press.

Rugh, S. and Zinkernagel, H. (2009), 'On the physical basis of cosmic time', *Studies in History and Philosophy of Modern Physics* **40**, 1-19.

Rugh, S. and Zinkernagel, H. (2011), 'Weyl's principle, cosmic time and quantum fundamentalism', in D. Dieks et al. (eds.), *Explanation, Prediction and Confirmation: the philosophy of science in a European perspective*, Springer, pp. 411-424.

Ryckman, T. (2005), *The Reign of Relativity*, Oxford University Press.

Sciama, D. (1971), *Modern Cosmology*, Cambridge University Press.

Silk, J. (1989), *The Big Bang*, W.H. Freeman; revised and updated edition.

Silk, J. (2006), *The Infinite Cosmos*, Oxford University Press.

Sklar, L. (2010), 'I'd love to be a naturalist—if only I knew what naturalism was', *Philosophy of Science* **77**, pp. 1121-1137.

Smeenk, C. (2012), 'Philosophy of cosmology', forthcoming in ?? .

Stanford, K. (2006), *Exceeding our Grasp*, Oxford University Press.

Torretti, R. (1983), *Relativity and Geometry*, Pergamon Press: reprinted by Dover.

Wald, R. (1984), *General Relativity*, University of Chicago Press.

Weinberg, S. (1972), *Gravitation and Cosmology*, New York: John Wiley.

Weinberg, S. (2008), *Cosmology*, Oxford University Press.

Zinkernagel, H. (2002), 'Cosmology, particles and the unity of science', *Studies in History and Philosophy of Modern Physics* **33B**, pp. 493-516.