The Scientific Status of Cosmology

by

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A Thesis
Submitted to the Faculty of Graduate Studies
in Partial Fulfillment of the Requirements
for the Degree of

MASTER OF ARTS

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Winnipeg, Manitoba

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Acknowledgments

This work has been much improved by the oversight of Dr. M. Gerwin and Dr. C. Matheson, as well as that of Dr. M. Clutton-Brock. Their wisdom is greatly appreciated. I also thank the Philosophy Department of the University of Manitoba for the excellence of their instruction, the Philosophy Department of Sir Wilfred Grenfell College (M.U.N.) for their patronage, and Bettina and George Nyman for their support and generosity.

Abstract

This project is concerned with protecting modern physical cosmology from arguments which wrongly conclude that it is unscientific. It is shown that these arguments rely upon demarcation criteria which are misapplied, and that such criteria are, in general, incapable of demonstrating that the theory, discipline, or practice in question is epistemically flawed. This follows in part from an analysis of arguments made by Mario Bunge and Ian Hacking, to the effect that cosmology is unscientific. Bunge argues that this is so with regard to the Steady State Theory because of demarcation criteria comprising the Popperian demand that scientific theories be testable, and his own requirement that scientific theories posess particular ontological postulates; Hacking argues that this is so with regard to astrophysical theories generally because they cannot be given the realist interpretation he requires of scientific theory. I demonstrate that each criterion is wrong, and examine the empirical status of modern cosmology to rebut criticisms that it is unscientific because its theories are overly speculative. Particular attention is paid to theories about the origins and early history of the universe, and it is shown that scientists must employ a Uniformitarian thesis if they are to remain empirically credible about this subject. Finally, an argument put forward by Larry Laudan is refurbished to show that scientific status of theories is irrelevant to any genuine criticism of them. From these considerations it follows that arguments to the effect that cosmology is unscientific are mistaken in their criteria and overstated in their significance.

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Introduction

Critics who argue that cosmology is unscientific are skeptical about the knowledge scientists may have of the universe. By their argument, the categories 'scientific' and 'unscientific' segregate that which can be known from that which cannot, and because cosmology falls in the latter category it is epistemically flawed. In reply, I propose that we concern ourselves instead with more relevant standards of assessment. Not only are critics wrong in their judgement that cosmology is unscientific because their demarcation criteria are poor or misapplied; they are wrong to think that such a judgement is relevantly important because measures of scientific status do not effectively assess a theory's epistemic qualifications.[1]

The problem facing cosmology, and the reason that it has been challenged by philosophers and followers of developments in science, is that its theories appear to ignore the empirical standards that are expected of good scientific practice. This perception has been encouraged by extensive and critical surveys of the discipline which employ certain preconceptions about science, and claim that cosmological theories are unscientific because they are not testable. By this it is understood that these cosmological theories cannot presume to belong in our body of knowledge. Notable in the popular literature

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[1] The term 'demarcation criteria' refers to those criteria which differentiate scientific theories, methods, and disciplines from unscientific theories, methods, and disciplines. The term 'cosmology' here refers to the *modern physical cosmology* of physicists and astronomers who, since the advent of General Relativity and Quantum theories, have addressed the history and large scale properties of the universe from the perspective of the Steady State and Big Bang theoretical traditions.

about the subject are authors John Horgan and John Boslough, who argue that the theoretical physics of the late twentieth century experienced a crisis in which scientists' theories became detached from empirical evidence.[2] As a result, cosmologists no longer practiced 'real' science. According to Horgan, cosmology became an 'ironic science' whose speculative conclusions are unsupported by evidence. "At its best, ironic cosmology can keep us awestruck. But it is not science."(Horgan [1996], p.113) Boslough writes, "[T]he answers many physicists were giving to fundamental questions - Where did the universe come from? Why does it contain matter? - were being labeled science. In fact, these answers could be considered at best only informed speculation."([1992], p.209) In his opinion, the Big Bang theory is speculative to such a degree that it is only a "compelling although simplistic pseudo-scientific creation myth."(Boslough [1992], p.223).

These are strong words, and they use the weight of scientific authority to exclude cosmology from a position of epistemic privilege. Their effectiveness relies largely upon

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[2] Addressing the popular literature in this way is, perhaps, seen to be of questionable academic merit. This is not my own opinion. The pervasiveness of these sorts of arguments in the public forum is indicative that the academic literature, while going far beyond the well-touted exploits of Popper and Kuhn, is simply not being read by those who write about science. Responses to them by philosophers is thus particularly important, as it not only serves to correct prevalent misconceptions about science but also more clearly details what philosophers think of science. More specifically, the philosophic literature regarding cosmology is strangely rare, and tends to be outdated. As the bibliography shows, most philosophy about modern cosmology was undertaken in the era of the Steady State theory and the nascent Big Bang theory (circa 1960), and followed the debates over the creation of matter. After this period, there appears to have been a lapse of interest, and only after the advent of Inflationary theory (circa 1980) and the consolidation of Big Bang theory did philosophers once again begin to look at cosmology, with particular interest being given to the points of popular contention, e.g., the anthropic principle.

our being willing to accept that cosmological theory is incapable of finding evidence to support its grand conclusions, and that 'being scientific' is an important measure of a theory's belief-worthiness. In the chapters to follow I will respond to both of these theses, and demonstrate that each is wrong.

I begin by assessing the arguments of Mario Bunge and Ian Hacking, two philosophers whose position regarding the scientific status of cosmology shall provide a comprehensive introduction to the problem of demarcating science from non-science. These two chapters shall also demonstrate the dangers inherent in criticisms which rely upon notions of what is 'unscientific' to castigate theories. In the third chapter it will be shown how cosmological theory can and does have evidence with which to support its conclusions, and that it need not be overly speculative in the sense that its theories are not testable. Following this description of the evidence used to substantiate modern cosmological theory, the fourth chapter will show the extent to which cosmologists may legitimately arrive at well-founded, 'scientific', conclusions about cosmic history. Of particular importance will be an analysis of the approaches which scientists may use to explain the origins of the universe, as it is in this troublesome area of investigation where critics most readily declare the subject to be philosophical metaphysics, and not scientific physics.

As a result of this study, the scientific status of cosmology will be retrieved, only to be turned away in my final analysis of the value to be given 'scientific' status. This is by no means a rejection of the standards of evidence and analysis which we are usually given to understand characterizes scientific investigation. Rather, it is an acknowledgment that our ideas about what makes a theory scientific are not really pertinent to the determination of theories' belief-worthiness. In the concluding chapter I will thus reintroduce the matter of the demarcation problem, and examine Larry Laudan's argument that there can be no satisfactory demarcation of scientific theory from unscientific theory. As it happens, Laudan's treatment of the demarcation problem is found to be insufficient but germane. Recast, it provides the seed for an even more sweeping declaration about the inadequacy of demarcation criteria to declare anything significant about the epistemic capacities of a theory. In light of this conclusion, my defense of cosmology's scientific status may seem to belie my criticism of the value to be given such status. Not so. The argument that cosmology is scientific is an important demonstration of the failure of critics' demarcation strategies, and of how cosmological theories can be credible claims to knowledge.

Bunge's Critique of Steady State Cosmology

The Steady State theory, according to which the universe is (globally) changeless and eternal, has long since been dismissed as false by most cosmologists, because it is contradicted by evidence which instead supports a Big Bang theory of the universe's history. Nevertheless, Bunge's criticisms of the Steady State theory remain relevant because they have also been directed against theories of the Big Bang, and bear upon the theoretical practice of cosmology in general. Two demarcation criteria provide the basis for his argument:

> The difference between the scientific and the semi-scientific cosmologist does not lie in the amount of imagination spent in building the hypothetical model of the cosmos, but in the material used to construct the model, and in its test: the model may or may not be based on accepted physical laws, and it may or may not be testable and satisfactorily tested for the time being. (Bunge, p.117)

Bunge's Popperian argument against the Steady State Theory thus appeals to the empirical features of science:

> (A1) The Creation Hypothesis of the Steady State Theory is ad hoc and is not falsifiable, and so evades empirical test.
> (A2) Theories which evade empirical testing (i.e. dogmatically profess ad hoc or unfalsifiable hypotheses) are not scientific.[3]
> (A3) By these premises, the Steady State theory is unscientific.

His other argument invokes a very different demarcation criterion, in the form of a physical law that is inherent to science:

> (B1) The Creation Hypothesis of the Steady State theory contradicts the principle of the conservation of matter (the Genetic Principle).

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[3] See Popper, [1963].

(B2) The Genetic Principle is basic to all scientific theory.
(B3) By these premises, the Creation Hypothesis and the Steady State Theory are unscientific.

Similar arguments can also be applied to Big Bang cosmology and particular of its hypotheses.[4] A common criticism of present theories of the Big Bang is that they are not falsifiable, and Rhook and Zangari [1994] have argued that its Inflationary Hypothesis, first proposed by Guth and since developed into a cornerstone of modern cosmological theory, is dangerously *ad hoc*. They have used similar arguments to criticize theories about 'dark matter', and their role in cosmology. As well, if theories of the Big Bang are correctly interpreted to include a Creation Hypothesis (which would concern the creation of the universe in the distant past, not the creation of particles over time), they too would be unscientific by Bunge's criteria.[5] Bunge's arguments, should they prove successful, thus represent a real threat to cosmology.

The continuous creation of Popperian criteria

Even were we to accept its demarcation criteria, the Popperian argument collapses because of the failure of its first premise, (A1), by which the Steady State theory is not empirically testable, and is thus not falsifiable, because the theory makes no definite predictions. (Bunge, p.130) This premise is false because the Steady State theory does indeed make definite predictions. According to its Creation Hypothesis scientists should

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[4] See, for example, Boslough (pp.208-10).

[5] Bunge notes this as well (p.126). For discussions about the place of the concept of creation in physicists' theories of the Big Bang, see Jaki [1995], who argues that creation cannot be empirically studied, and Grunbaum [1991], who argues that the concept of creation does not belong in theories of the Big Bang. See also Narlikar [1992].

observe the 'spontaneous' appearance of particles, and according to its Perfect Cosmological Principle astronomers should observe a static universe where dynamical processes only occur on a local scale. Given this, the theory is scientific by Popper's criteria because the main hypotheses of the Steady State Theory are neither untestable nor unfalsifiable. In fact, the theory is believed by most cosmologists to be falsified: the creation of matter is not apparent, and there is evidence (such as the cosmic microwave background radiation) which suggests that previous cosmic eras were very different from our own. This shows that Bunge has wrongly applied Popper's falsification criterion, but his argument faces a much graver problem: it is inconsistent. He may claim either that the Steady State theory is not empirically falsifiable, or that it is empirically known to be false, but he may not claim both. Nevertheless, he does just this, and appeals to empirical tests in his own attempt to falsify the Steady State theory, apparently forgetting his Popperian argument against the theory's scientific status.[6] To make matters worse, Bunge argues that *all* theories of modern cosmology are in a state of crisis because they have been empirically refuted:

> Each cosmological theory fits some observational data, and it is even conceivable that some models may correctly hit on several future observations; but none of this is even fairly consistent with the totality of the evidence. In other words, all cosmologies have so far been refuted by observation; and the steady state theory has, in addition, been refuted by theory. (Bunge, p.137)

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[6] This is not his only inconsistency. In his argument against ad hoc hypotheses, Bunge complains that they are not falsifiable because of the way they are corrected. By this view, theories are most falsifiable when they are incapable of self-correction and growth. But he also claims that, "the incapacity for self-correction and growth takes the theory dangerously close to dogma." (Bunge, p.127) If he is to avoid contradiction, he must not oppose theory-modification on the one hand while arguing for it on the other.

But this just accords with Nancy Cartwright's [1984] observation that any theory will be observed to have anomalous falsifying instances because of the empirical limits of scientific procedure. If anything, this should lead us to suspect that Popper's criterion must face severe problems of application because *any* scientific theory is not only falsifiable but is actually falsified - simply because the world is too complex to fit neatly into our theoretical and experimental schemes.[7] Given that scientists may legitimately declare that some instances of falsifying evidence are anomalous and not problematic, and that modern cosmological models are empirically plausible to such a degree that contrary evidence can be 'explained away', Bunge's contention that all cosmological models have been empirically refuted not only confounds his own contention that the Steady State theory is unscientific, but seems very premature.[8]

A more sweeping criticism may yet be directed against the Popperian argument, which is not limited to its questionable application in the case of the Steady State theory. Its second premise, (A2), expresses a demarcation criterion according to which scientific theories are falsifiable. This criterion suffers from a multitude of problems, not the least of which is the fact that there are putatively unscientific but falsifiable theories, and its demise is widely recognized in the philosophic literature.[9] This does not necessarily force us to conclude, however, that Bunge's second argument is entirely without merit. Perhaps the

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[7] See also (Kuhn [1962], pp.146-7).

[8] In the coming discussion I will have more to say about the weight given to evidence in scientific theory assessment.

[9] See Laudan [1988], Feleppa [1980], Morris [1987].

force of his criticism can be recovered by abandoning the concept of falsification as it applies to theories, and turning instead to an analysis of the scientific method and its approach to theory. By such a view, postulating ad hoc hypotheses and maintaining a dogmatic attitude towards particular theories may thus be unscientific, not because it renders theories unfalsifiable, but because the attempt to explain away evidence smacks of a grave contempt for the value which scientists should give to that evidence. Resorting to this approach does not usually address features of particular theories, and instead concerns scientists' methods of evaluating those theories, so Bunge cannot by this alone fault the Steady State theory. However, the argument preliminary to the conclusion that the theorists are overly protective will necessarily include a criticism of the theory, so Bunge may yet have won a victory, if not in the way he intended:

> (C1)The Steady State theory is in a state of crisis because it conflicts with the evidence.
> (C2) Steady State theorists refuse to allow the evidence to contradict their theory, and present additional (ad hoc) hypotheses which dissolve the contradiction.[10]
> (C3) Steady State theorists do not respond properly to the evidence, because the evidence shows the theory to be false.
> (C4) Failing to respond properly to evidence is unscientific.
> (C5) Steady State theorists' response to the evidence is unscientific.

Bunge reasons that the Steady State theory is dogma because its practitioners are not willing to accept the possibility of its being false, and to some extent this is true. Steady

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[10] In response to the discovery that the universe is expanding Steady State theorists hypothesized the creation of matter as space expands. This Creation Hypothesis was intended to preserve the Perfect Cosmological Principle, according to which the mean density of the universe must remain uniform over space and time at large scales, and it is thus, "the *ad hoc* unsupported support of a controvertible conjecture (the 'perfect cosmological principle')." (Bunge, p.139)

State theory follows from the Perfect Cosmological Principle, according to which the universe's structure is uniform across space and time at large scales, and this is an extension of the Cosmological Principle, by which the universe's structure is only uniform across space. The theory's most influential advocates, Bondi and Gold, were mindful of Dirac's Conjecture that natural laws might themselves change over time, and observe that the scientific inspection of the cosmos is strictly limited to the study of a region of space-time where the nomological structure is the same as that found terrestrially. It follows from this that a scientific cosmology will only succeed if the entire universe really does have an invariable nomological structure; i.e. scientific theories in cosmology will be true only if the Perfect Cosmological Principle is true.[11] Bondi and Gold also argue that scientific cosmologists *must* affirm the Perfect Cosmological Principle, even though it may be false, because the alternative is to arbitrarily decide that physical laws are not affected by the universe's large scale material structure. By their view, it is unscientific to arbitrarily assume that the material structure of the universe does not affect the universe's nomological structure, because this decision cannot be logically justified.[12] Since science

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[11] "The fate of cosmology *as a science* hinges on the truth of the assumption that the universe possesses a temporal invariability of structure" (cited in Grunbaum [1951] p.495).

[12] Bunge's response to the theory thus misses the point. He argues (correctly) that the material distribution of the cosmos need not be the same at all times and at all places for there to be universal laws of physics. The principle according to which "the universe presents the same aspect from every point and at every instant, except for local irregularities," (Bunge, p.120) can be false even if a Weak Cosmological Principle, according to which physical laws exist and do not change over time and space, is true. "[T]he constancy of laws only involves the constancy of relations among properties and events; it does not involve the immutability of the relata... unchanging laws may describe changing states - and even irreversible changes of state." (Bunge, pp.132-3) Therefore,

does not accept arbitrary or unfounded hypotheses, the only scientific cosmology is Steady State cosmology. Given the strength of the Steady State theorists' avowed devotion to their theoretical programme, premise (C2) seems to be *prima facie* true.

More contentious are the premises which concern the propriety of the theorists' response to the evidence, (C3), and the demarcation according to which science properly responds to the evidence, (C4).[13] In response to these, two problems become apparent. Firstly, is it necessarily a bad strategy to protect theories which are confronted by contrary evidence? Secondly, how should scientists respond to evidence? (Insofar as we are concerned with a normative demarcation an answer to this defines the 'scientific' response to evidence.) Taken together, these questions ask, 'is stubborn science bad science, or is it not really science at all?'

The normative problem here is whether or not being dogmatic (i.e. appealing to ad hoc hypotheses to save a theory) is a sound strategy to achieve epistemic goals.

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the universe can be described by laws even if it is not globally static. But while this may be true, it just ignores the Steady State theorists' argument that the universe's laws *might* be related to its material structure, and that it is unscientific to assume otherwise. Bunge does not realize that Steady State theorists do not argue that the universe's nomological and material structures *must* interact (although they do have reasons to think that this interaction does exist); they instead argue that the *possibility* of such an interaction has important consequences. In particular, theories about globally dynamic universes must assume that such interactions do not exist if they are to proceed, whereas theories about globally static universes need no such assumption. Bondi and Gold then claim that since the assumption in question cannot be substantiated, scientific cosmologists must choose a Steady State theory. See (Balashov [1994], pp. 941, 943-4).

[13] There is a sense in which premise (C4) is trivially true, because according to normative conceptions of science the scientific method is simply the proper method, but this just begs the question 'what precisely is the proper method?', and it is here that the premise becomes contentious.

According to Bunge, invoking ad hoc hypotheses weaken a theory's testability because they allow it to evade the evidence. (p.134) This is not strictly true, however, because the resulting theory and its hypotheses may together (and individually) be tested; such was scientists' response to the Steady State theory. A greater problem is that the sustained attempts at theory-protection tends to preserve a theory long after it should be abandoned, often at the expense of alternative and better theories, and may promote methods which forgo an adequate critical analysis of the theories.

Despite this *prima facie* concern, the sustained protection of a theory does have its advantages. Not only does it more fully explicate a theory, it prevents the premature abandonment of an otherwise viable research programme. This may be the case in a theory's formative stages, where anomalies, questions, and doubts may be more prominent than in more mature research programmes. More commonly, however, dogmatic methods may be found in established theoretical programmes. The practice of normal science and dogma is advantageous in such cases because scientists have only limited resources, and they must adopt some perspective if they are to be productive. "One great virtue of commitment to paradigms is that it frees scientists to engage themselves with tiny puzzles." (Kuhn [1963], p.262) This commitment is an asset because it provides scientists with 'the rules of the game', without which they would not be practicing scientists, and it also aids scientists' detection of anomalies which might threaten a paradigm. (p.254) According to Kuhn [1962], science does not evolve towards true theories, but away from empirical problems, and during periods of normal science a theoretical programme (or paradigm) is perpetuated until it can no longer accommodate the accumulation of puzzling anomalies. Successive generations of scientific practitioners thus adhere to a theoretical vision for as long as possible. By this view, attempts to explain evidence in the context of a theory while normal science remains viable is just sensible problem solving, but when science enters a state of crisis and there is an accumulation of puzzles which do not appear to be solvable within a paradigm, scientists should pursue theoretical alternatives or find themselves scientists no longer. (Kuhn [1962], p.159) At some point, theories in a state of crisis should not be sustained and the problem for philosophers of science is to determine at what point a theory should be abandoned and more viable alternatives pursued.[14] This shows that worries about dogmatic attitudes towards theories are not entirely unfounded, because there are appropriate and inappropriate instances of perpetuating a theoretical programme.

The tension between the virtues and vices of dogmatic science has also been recognized in Lakatos [1970]. He argues that research programmes are composed of a 'hard core' of unquestioned propositions which are protected by a 'belt' of auxiliary hypotheses. The conservative character of the protective belt's 'positive heuristic' is advantageous because it gives continuity to science, and "saves the scientist from becoming confused by an ocean of anomalies."(p.193) Unfortunately, it also has the problem of "making us unable to get out of our self-imposed prisons, once the first period of trial and error is over and the great decision taken."(p.194) At some point, beyond an

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[14] Kuhn [1962] notes that even the existence of a crisis does not necessitate the perception of anomalies as falsifying instances. "Even the existence of a puzzle does not by itself transform a puzzle into a counterinstance. There is no such sharp dividing line." (p.80)

as-yet nebulous threshold, theory-protection really does act against the best interests of scientists and science, and perhaps this can be construed as unscientific.

Any demarcation distinguishing between scientific and unscientific sorts of 'dogmatism' must clarify the degree to which evidence necessitates our decisions about theories. There is considerable disagreement amongst philosophers of science about the relationship between evidence and scientific decision-making, however, so the promise of a demarcation based upon notions of empirical support and empirical denial seems bleak.

Firstly, there is the question of how a theory is to accrue empirical support. As the anti-realists since Duhem have argued, the evidence will always 'underdetermine' a theory under review, and will not necessarily demonstrate a preference for any given theory. Even the empirical efficacy of theories remains contentious for those who follow Hume's path. Nelson Goodman revealed this when he posed his 'new riddle of induction', and demonstrated that evidence statements may support contradictory hypotheses. Bayesian confirmation theory, which assigns probabilities to hypotheses, has been proposed to solve the matter, but it is not clear that Bayesian calculus can account for the complexities of theory assessment as it is found in normal scientific activity. Clark Glymour has argued that the Bayesian theory of confirmation cannot account for the persuasive force which may be given to old evidence, and R. W. Miller has argued that it cannot account for the *ad hoc* modification of theories which takes place upon the assessment of new evidence. In the absence of a better account which would tell us what it is for a theory to be confirmed by the evidence, confirmation would appear to be a concept in distress.

Secondly, if the evidence does appear to contradict a theory, scientists may elect to discount the evidence or to adjust the theory: falsification does not entail refutation, and with enough ingenuity theories can become immune to refutation. Duhem, and later Quine, showed that crucial experiments which might decide the proper fate of theories do not exist, because the failure to observe predicted phenomena falsifies only the conjunction of hypotheses which lead to the prediction, and cannot point to the hypothesis which is in fact false.[15] Nancy Cartwright applied the thesis to actual scientific practice and argued that theories portray idealizations that cannot entirely account for a complex real world. She demonstrated that in the normal practice of science, data which falsify theories are regularly discounted on the grounds that they are not representative of the complicated and 'impure' physical system which is being investigated. Generalizing from these insights, it appears that scientists always have opportunity to 'explain away' the evidence they collect.

Even were these arguments not convincing, there exists a more general thesis which damages the decision-making influence of empirical evidence. This thesis, developed by Quine and elaborated upon by Feyerabend, dissolves the distinction between theory statements and observation statements, and its reasoning applies to both confirming and falsifying instances. According to their arguments, observation statements can only occur in the context of a body of theory, because observation statements are themselves 'theory-laden', and rely upon attendant theoretical claims to interpret observational information. Insofar as this is correct, and observation reports are dramatically

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[15] This line of argument has not gone uncontested. Yoshida [1975] has argued persuasively that in a well-designed experiment, evidence can indeed show a theory to be false.

predisposed by the sorts of theory which guide empirical procedures, there appears to be little reason to think that observation reports of any sort should preferentially influence the selection of theories. Instances of falsification may always be ignored (either by suitable modifications of the theory or by 'explaining away' the bad or irrelevant data), and instances of confirmation do little more than reaffirm the notion that scientists will observe what the theory demands is to be observed.

In light of these arguments, what may be concluded about the role observation is to play in providing us with knowledge of the world? They show that comparing a theory's description of the world with nature itself need not direct the scientist's determination of the theory's truth or falsity, or even its empirical adequacy. According to this view, which might not surprise post-Kantian philosophers although it may surely alarm scientists, empirical data do not offer conclusive answers because they are always subject to theoretical interpretation. Given these conclusions, the force of evidence upon scientific decision-making seems remarkably weak, and normative questions about when scientists should take evidence to conclusively exclude a theory or promote its modification seem to rest on shaky ground.

All of the foregoing discussion about the epistemic and normative problems of evidential relevance to theory selection recognizes that science does, as a matter of fact, save theory from contrary evidence. Here the history of science provides the test, and reveals that adjusting and refining theories is a normal part of the problem-solving which takes place in science. From this it is clear that attempts to recover theories from contradicting evidence are not necessarily unscientific, and a normative demarcation criterion should recognize both scientific (appropriate) and unscientific (inappropriate) sorts of theory-protection if it is to say anything at all about the scientific status of 'dogma'. Until there is some good explanation of what is and is not appropriate theory protection, such a demarcation is beyond us.

Ontological Postulates: Magical or Scientific

While Bunge's first argument concerns the empirical features of the Steady State theory, his second condemns the sort of world it describes. According to his second thesis, scientists must respect well-corroborated theory and its ontological postulates, but Steady State theorists ignore this directive and place cosmological considerations above those of the physicist. (Bunge, p.139-40) In particular, Bunge believes that the law of conservation of matter put forward in Lucretius' Genetic Principle is indispensable for any legitimate and scientific physics. He argues that theories without it are better suited to magic than physics, because such are indeterministic and so fail to provide a physical account of the mechanism by which the creation of matter would occur. Consequently, "In assuming that the emergence of matter, though lawful, is determined by nothing (indeterminate), the steady-state theory endorses radical indeterminism - or, to put it bluntly, it endorses magic." (Bunge, p.131) Bunge thus equates the rejection of the Genetic Principle with the rejection of scientific physics when he writes, "The [creation] hypothesis conflicts with the whole 'spirit' of modern science, which abhors creation *ex nihilo*, and accepts, on the other hand, Lucretius' genetic principle, according to which nothing comes out of nothing or goes into nothing." (Bunge, p.130; see also p.118)

Is the rejection of the Genetic Principle really a rejection of physics, even if science has hitherto used such a principle? There are reasons to think otherwise. The 'entrenched ontological postulate' that Bunge considers unique and integral to physics is less entrenched than he might suppose. Quantum physicists seem quite willing to abandon the principle when they study the creation and annihilation of matter and energy: they have used quantum uncertainty to place limits on the Genetic Principle, and postulate the existence of short-lived pairs of 'virtual particles' which appear and disappear before the universe can 'discover' that a Law of Conservation has been broken. Such quantum events are not entirely 'virtual': the unique conditions around gravitational singularities (i.e. black holes) separate the paired particles such that one falls into the event-horizon, while the other is emitted - and detected - as Hawking radiation. Indeed, there is good reason to conclude that *any* ontological postulate, entrenched or not, has only a provisional status in the body of scientific knowledge. Consider, for example, that physicists working in the field of quantum theory have designed experiments which test the long-standing 'scientific' postulate that causality operates locally. Those experiments show that particles interact in ways which forbid a local cause-effect relationship, forcing scientists to rethink their theories and question the locality postulate. Other ontological postulates may share a similar fate, or may be (temporarily) validated by experiment. Identifying those physical principles which are central to scientific activity is a more troublesome affair than Bunge might think, because the science of today would perhaps be scarcely recognized as such by the scientists of yesterday; modern science has overturned ontological principles of past scientific renown, and even if there are ontological principles which lie at the core of science, they are not the ones Bunge uses.

More importantly, however, Bunge does not explain why we should preferentially adhere to those ontological postulates which are entrenched in past scientific practice. The fact of their being orthodox surely does not suggest that they are true, and the change which takes place within science is evidence that the best theories need not remain within past and authoritative paradigms. It is also not clear why some ontological postulates are scientific and others are unscientific. A solution to this problem awaits a meta-criterion which would allow us to chose amongst the myriad of possible ontological postulates, and partition those postulates which are scientific from those which are not.

Part of Bunge's argument rests on his contention that cosmological ideas should not supercede respected physical theories because "[n]othing can justify the rejection of physics in the name of cosmological considerations." (Bunge, p.139) This is a response to Steady State theorists who claim that cosmology provides insights which are crucial to the formulation of a scientific physics, and a cosmological physics need not be a mere extension of traditional physics. (Bondi, cited in Bunge, p.139n.) According to Bunge, however, a cosmology is scientific only to the extent it is 'mega-physics' - or terrestrial physics writ large - and cosmology cannot provide any good insights independently of that physical basis; such considerations are mere fantasy (Bunge p.139; 116). However, it is not clear why this must be so. The demand that theories are scientific only if they accord with orthodox science forgets that scientists may legitimately treat their theories as if they were only contingent hypotheses that may require revision or abandonment. Some of the most dramatic developments in science have occurred when new ideas have overturned the foundations of the old, and it is not clear why the Genetic Principle is any less provisional: while the Genetic Principle may belong to the 'hard core' of many past and present research programmes, Bunge does not sufficiently explain why it *must* belong to *all* scientific research programmes. He does make the case that indeterministic accounts of nature (such as Bondi's Law of Continual Creation) are unscientific because they fail to explain the physical mechanisms by which creation occurs. (p.131) But indeterministic theories are not inherently unscientific, because there do exist laws of physics which are accepted parts of science even though they do not suggest mechanisms which explain the phenomena they describe. Quantum theories, for example, do not have deterministic interpretations of how phenomena come to be manifest. Given that physicists are willing to question their 'hard core' of ontological postulates when the 'protective belt' of auxiliary hypotheses becomes too unwieldy, why should they not make use of cosmology to question the principles of orthodox physical theory? This is especially so if the 'scientifically irrelevant' cosmological considerations have empirical support, and deserve the attention of physicists.

As the discussion so far has shown, demarcation criteria which appeal to falsifibility or visions of scientific orthodoxy (especially as they regard ontological postulates) fail to capture the sense in which theories are either scientific or unscientific. As a result, Bunge's arguments fail to show that the Steady State theory - or any other theory, for that matter - is unscientific. His arguments do show, however, that the theory faces severe philosophical and empirical objections, and this should serve as a reminder that one does not need to demonstrate that a theory is unscientific to show that it is not a good theory. My examination of Hacking will emphasize this further, and further prepare for the conclusion that demarcation criteria are flawed because they fail to catagorize theories according to their belief-worthiness.

Hacking's Critique of Astrophysics

While Bunge has found science in empirically scrutable theory, Hacking has closely identified science with the acquisition of truth. So, when Hacking argues we cannot be scientific realists about astrophysics, Shapere reasons that he is also arguing that astronomical studies are unscientific.[16] Shapere's analysis of Hacking credits him with establishing a demarcation between scientific and unscientific investigations, such that "legitimate science is restricted to 'manipulating and interfering with the world in order to understand it.'"(Shapere [1993], p.134) This is a misstatement of Hacking's position, however, because it is taken out of its proper context. According to Hacking,

> [van Frassen] holds that all science aims only at saving the phenomena, at empirical adequacy, at ability to derive observable phenomena [from theory]. I don't imagine that science 'aims' at anything at all, but if we do use the metaphor, then it seems to me that *most natural science aims more at manipulating and interfering with the world in order to understand it.* Hence saving the phenomena seems an entirely subsidiary aspect of scientific activity. There is one, and perhaps only one, *branch of science* where the tag 'to save the phenomena' has a central place: astronomy and astrophysics. (Hacking [1989], p.577, italics added)

In this passage Hacking distinguished between two sorts of scientific activity, such that

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[16] In the popular literature the central aspect of scientific realism (that science has a truth-discovering status) has commonly been confused with providing a demarcation criterion which would distinguish science from non-science. Science is equated with truth-finding, and thus the doubts as to the truth of an activity and the doubts about how certain we may be of accounts of the world become mistakenly translated into discussions about the scientific character of an activity or idea. This reflects the degree to which we consider science to be an arbiter of certainty and truth. It is less usual to discover this tendency in the philosophic literature as the realism and demarcation issues undergoing debate are sufficiently disparate as to promote their own and independently undertaken specializations.

theoretical knowledge through manipulation is more important in science than the empirical adequacy of the theory. He also observes that astronomy and astrophysics are sciences which are unable to manipulate their subject. "Astrophysics," he writes, "is almost the only human domain where we have profound, intricate knowledge, and in which we can be no more than what van Frassen calls constructive empiricists." (Hacking [1989], p.578) It is thus clear that Hacking is not here claiming that 'constructive empiricism' is unscientific *per se*, or that realism is a necessary aspect of genuine science.

But there is a tension, if not an outright contradiction, in Hacking's work which confuses the issues of scientific realism with the demarcation of science from the unscientific. In the following statement it is revealed that Hacking does consider manipulative experiments to be crucial aspects of genuine science:

> The technology of astronomy and astrophysics has changed radically since ancient times, but *its method* [of saving the phenomena] *remains exactly the same*. Observe the heavenly bodies. Construct models of the (macro)cosmos. Try to bring observations and models into line. In contrast: the methods of the natural sciences have undergone a profound transformation, chiefly in the seventeenth century. Or one might say, *the natural sciences came into being then and thereafter, while astronomy is not a natural science at all*... the transition to the natural sciences was precisely the transition to the experimental method, to interfere with nature, to the creation of new phenomena... Natural (experimental) science is a matter not of saving phenomena but of creating phenomena... But in astrophysics we cannot create phenomena, we can only save them." (Hacking [1989], pp.577-8; Italics added.)

In this passage it is clear that Hacking has in mind some distinction between natural science and the scientifically inferior practice of 'saving the phenomena'[17]. This

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[17] 'Saving the phenomena' is identified by Shapere to mean "the doctrine that the aim of science, or at least certain areas of it, is not to learn about the structure of the universe, but at best only to correlate observables."(Shapere, p.134) According to

interpretation becomes all the more credible when Hacking reiterates his [1983] claim that theoretical entities which fail to be manipulable are 'occult'.[18] (Hacking [1989], p.561) Given all of this, Hacking does appear to have in mind a demarcation criterion according to which astrophysics is not genuine science, even though he seems to be willing to admit that it is 'scientific' in a very inferior sort of way. It is clear that a less ambiguous declaration about the unscientific status of 'saving the phenomena' is needed, but in the absence of such a clarification I will assess the argument as Shapere has interpreted it.

Hacking's Modest Astrophysical Anti-Realism

Hacking has two anti-realist arguments against celestial sciences; one is directed towards astrophysical entities, the other towards (astrophysical) models, and by each he concludes that we should be anti-realists about astrophysics in general. In his first argument:

> (D1) We can only be scientific realists about entities which can be experimentally manipulated.
> (D2) Astrophysical and astronomical theories make use of entities which cannot be manipulated.[19]

Hacking, saving the phenomena is "to reconcile observed phenomena and a theory they contradict," or "to construct an empirically adequate theory." (Hacking [1989], p.576-7) Whatever the particulars, each formulation signifies a disregard for the truth of the matter and a general satisfaction with being able to accurately portray and predict phenomena.

[18] By describing a theoretical entity as 'occult', Hacking might mean merely that the entity is hidden, unexplained, or inscrutable. The term does, however, have associations with the 'supernatural' and so seems to allude to an unscientific subject. This is yet another ambiguity which Hacking would do well to clarify.

[19] Hacking is particulary concerned with the astrophysical theory of gravitational lensing, according to which massive objects (including galaxies and 'black holes') may cause rays of light to diverge from a large luminous object of origin (e.g. a more distant galaxy) in such a way that they come to manifest multiple images in astronomers'

(D3) It follows from D1 and D2 that we cannot be scientific realists about the entities used by astrophysical and astronomical theories.

In his second argument:

(E1) We should be anti-realists about theoretical models, because different and often inconsistent models are often used to solve identical classes of problems.
(E2) Astrophysical theories are only theoretical models.
(E3) It follows from E1 and E2 that we should be anti-realists about astrophysical theories.[20]

Shapere thinks that both of these arguments are supplemented by the following:

(F4) A practice is scientific only if we can be scientific realists about it and its entities.
(F5) Therefore, by the previous arguments' conclusions and F4, astrophysical and astronomical theories are not scientific.

In response to the first argument, Shapere argues that it is false to suppose that our only reasons to believe that a theoretical entity exists are to be found in our ability to experimentally manipulate it. The main problem with the first premise (D1) is that Hacking has not demonstrated how interference and manipulation in natural affairs is relevantly different from more passive forms of observation.(Shapere [1993], pp.147-8) Unfortunately, dissolving the distinction does not itself settle the matter of whether or not we can be realists about the theoretical entities scientists think they observe, and some further argument is needed.

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telescopes.

[20] "In astrophysics we have only models, and all too many of them, at every possible layer of investigation. There are no propositions of detail, at the level of science which I have been describing, which are more than models. These models are not literally true. Nor are they 'converging on the truth', for all we will ever have are more models. That is why I am an anti-realist about astrophysics." (Hacking [1989], p.577)

Responding to the second argument, Shapere points out that Hacking is promoting a false distinction amongst astrophysical and other theories because many terrestrial sciences make use of models to great effect. This success bolsters his realist defense of theoretical models, according to which we can be scientific realists about astrophysical models (and models generally) because the fact that theoretical models succeed and improve is evidence that they describe real entities.[21]

> It is possible in principle to make those models increasingly realistic, and astronomers have done so in fact... The models used in astrophysics do not affect sought-for results in a way that cannot be improved or taken into account; they can be, and are often justifiably, accepted as realistic (or at least as more realistic than others) and used to further investigation. (Shapere [1993], p.145)

Shapere thus agrees with Hacking that scientists are realists who aim to "get more adequate accounts of what is really happening in the phenomena." (Shapere [1993], p.144) The cognitive aims of scientists do not justify a realist view, however, so some other argument is necessary if scientists are to legitimately model theoretical entities with some certainty of their existence.[22] In particular, some response must be given to the anti-realists' thesis that we cannot know when a theory is true because empirically adequate

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[21] This is a version of the No-Miracles Argument, also articulated by Putnam [1978] and Boyd [1980], according to which a theory's history of empirical success indicates its truth.

[22] The cognitive aims of scientists are largely irrelevant to philosophical debates about realism. There are both realist and anti-realist scientists, but both cannot be correct. Even were we to permit there to be a shared consensus, (to forestall questions regarding prevailing attitudes in the scientific community, and worries about self-deception amongst anti-realist scientists) it is not clear why scientists' opinions should matter. The mere fact of their belief that they seek the truth in no way establishes that there is a truth to be sought, or that it is achievable.

theoretical alternatives are always possible. According to Shapere, this thesis only shows we ought to be agnostic about which theories are true, and that it does not show that we do not have reason to believe that particular theoretical entities exist; "We would still have reasons to believe the objects exist, even though certain facts preclude our knowing at least some (not necessarily all) things about those objects."(Shapere [1993], p.144)

In response to the third and supplementary argument, Shapere argues that the scientific method does make legitimate use of theoretical entities even if it does not interfere with them.

> The fact is that scientists do build upon what they have learned to make inferences even in cases where they cannot lay hands on the entities about which the inferences are made: they use what they have already found out - whether by interfering actively or passively observing - to probe further. Surely this is a most important aspect of the scientific enterprise. (Shapere [1993], p.148)

By this view, science not only arrives at conclusions by manipulating nature, but also learns from simple and non-interfering observation. This point is well made, as it is clear that inference from observation is indeed an epistemically successful scientific activity, even in the absence of the scientist coaxing phenomena from nature. We do indeed know of real things without our having to experimentally interfere with them; in biology, for example, evolutionary processes may be scientifically studied without affecting the subject, and environmental conditions are successfully correlated to modes of adaptation without the scientist manipulating evolutionary patterns. Since 'saving the phenomena' is clearly a legitimate part of science, the demarcation of premise (F4) is false, and it cannot be argued on such grounds that astrophysics and astronomy are not genuine sciences.

While neither of the arguments in defense of realism may prove convincing, at least it has been shown that scientific realism is only an interpretation of scientific theory's epistemic capacity, and in this capacity it does not identify what is and is not genuine science. This is just as well. Were it taken to extremes, Hacking's conception of science would have us deny all scientific knowledge of the past because the subject in question is beyond manipulative scrutiny. Theories which 'save the phenomena' can explain artifacts of the historic record when they model the processes which lead up to presently observed conditions, but by Hacking's reasoning this does not constitute scientific knowledge of the past. Similar arguments can also show that scientific knowledge of spatially distant conditions, and of the future, are not possible if Hacking's thesis is taken to its logical conclusion. With all of these objections arrayed against it, Hacking's thesis that science must do more than 'save the phenomena' is in grave difficulty, and does not present a real threat to either the possibility of a scientific astrophysics or the possibility of a scientifically undertaken cosmology.

At this point we would do well to observe that Hacking may be overstating his case, especially as it concerns our inability to use scientific manipulation to further cosmological ends. His premise (D2), according to which astrophysical entities cannot be manipulated, forgets that the theories which are used to model astrophysical entities are themselves extrapolated from conclusions reached by physicists in terrestrial laboratories who have had the opportunity to manipulate their subject.[23] Terrestrial experiments can

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[23] This important fault of Hacking's position was raised by Dr. M. Gerwin upon review of this thesis.

inform astrophysical theory, in such a way that astronomers' passive observations are interpreted in the context of a theory brought about by the active manipulations of laboratory physicists. Hacking is also wrong to assume that astrophysical entities can never be experimented upon, because it may be possible for astronomers to find opportunities to study rare astrophysical events in an experimental setting. While astronomers may not directly manipulate the entities they observe, carefully selecting subjects of study which are naturally exposed to conditions of interest would do the work Hacking expects of experimental manipulation. For such cases, premise (D2) is, in effect, false. Cosmology, it would seem, is safe from those who would argue that science cannot know the universe because it cannot manipulate it. The task of the two chapters to follow is to show that cosmology is also safe from those who would argue that it is too speculative, and is on these grounds unscientific.

The Empirical Face of Cosmology

In this era of robust science, where technological inventions, astronomical discoveries, and theoretical insights seem to be the norm, perhaps cosmologists can be forgiven their optimism.

> We are on the threshold of confirming whether our present ideas about the evolution of the universe and the origin of large-scale structure have any validity. This is a golden age in cosmology, which is rapidly become as respectable a science as, say, archeology, and the field promises to provide an equally reliable probe of a far more remote past. (Silk [1994], p.234)

The empirical features of cosmological knowledge and the way scientists can use methods of experiment and observation to select their theories are held to justify this attitude, as well as substantiate the cosmological theories themselves. In the limited sense that scientific knowledge is just knowledge which is informed by observation, experimental testing, and empirically founded analysis, the following discussion will show that cosmology can indeed be scientific. This should dispel arguments against cosmology which complain that it must be unscientific because it cannot be empirical enough to satisfy our epistemic standards.

Evidence for the expansion of the universe

Hubble's analysis of galactic redshifts provided the first evidence for the expansion of the universe. In 1929, Hubble showed that galactic redshifts, which are a measure of galaxies' radial velocities, are linearly proportional to their distances from Earth: the further the galaxy, the faster its motion away from Earth. The galaxies populating the universe are all moving away from our own location, but rather than supposing that the universe preferentially selects our spatial region to be abhorrent to these galaxies in some way, it is suggested instead that such results would be obtained by any observer regardless of the galaxy in which the observation is made. This is consistent with the principle that the terrestrial environment, or indeed the environment of any observer, does not occupy a preferred location. On this basis, each galaxy is taken to recede from all other galaxies.

Given this restriction, Hubble's observation of galactic redshifts has two admissible interpretations: either galaxies are dispersing within a static volume of space (Hubble's preferred alternative), or space is itself expanding; this latter, the Universal Expansion hypothesis, became the basis of both the Steady State and Big Bang theoretical traditions.

The question was largely decided by the earlier discoveries by both Alexander Friedmann (in 1922) and Georges Lemaître (in 1927) that the equations of General Relativity yield models of dynamic global space-times which expand or contract. The theoretical motivation to prefer the Universal Expansion hypothesis is itself empirically supported, because Einstein's theory of General Relativity is generally accepted as having been experimentally confirmed by scientists: common and dramatic examples of the successful tests to which general relativity has been put include its predictions about the perihelion of Mercury, the distortion of the paths of light rays from stars passing through the sun's gravitational field, and the observed times of decay for particles traveling at relativistic velocities.

The Universal Expansion Hypothesis also finds support because it accounts for much older evidence: the existence of a dark night sky. In an expanding universe, light from a source becomes attenuated as it passes through space, because the energy of

Doppler-shifted photons is decreased. [24] This explains why the sky need not be perpetually bright in an infinite universe filled with luminous objects (i.e. stars and galaxies), and thus resolves Olber's 1823 'paradox'.

The Steady State account of the universal expansion interprets it to be an eternal process, and postulates the spontaneous creation of matter to explain how the mean density of the universe would remain constant over time. The reasons for requiring a constant density are explicitly philosophical, and in additional to theoretical arguments against this hypothesis, the evidence suggests that the universe is in fact much more dynamic than the Steady State theory would allow. The hypothesis that the universe was fundamentally different in earlier cosmic eras is substantiated in part by astronomers' observations of the cosmic background radiation, as well as their determination that radio galaxies are more prevalent at great distances from us, and were therefore more common in earlier cosmic eras.

According to the 'Big Bang' account of the universal expansion, we may infer from the universal expansion a time at which the entire universe was collected at a point of origin. In the very distant cosmic past the universe was a very small, extremely dense, and highly energetic collection of matter and energy - a 'primordial atom' - from which all later structure was formed. While the Big Bang theory is thought to provide the

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[24] Several explanations of the dark night sky are possible: If the universe is spatially finite, the sky is dark because the number of luminous objects is too few; if the universe is spatially infinite, it must either be very young, so that light from the most distant sources has not yet crossed large cosmic distances to brighten our sky, or it must be expanding (or both). Advocates of the Big Bang Theory adopt the latter alternative, with evidence suggesting that the universe is both relatively young and expanding.

authoritative account of the universe's expansion because of its empirical and theoretical successes, cosmologists have differing opinions about how the past limit of expansion is to be interpreted. For most it is boundary marking a state of transition from previous and presently unknown conditions: perhaps the universe originated as a quantum event taking place in a previously existing vacuum, or perhaps a space-time singularity isolated the universe from previous conditions. Hawking has suggested that finite and expanding space-time had no boundary (and no singularity) which would mark the beginning of time. (Hawking [1984], pp.135-47)

While there is little consensus regarding existential questions about the universe's origin, and theorists' descriptions of the universe before and during the first moments of the universe's expansion remain contentious, other specifics pertaining to the history of the universe and the processes which led to presently observed structure are better understood. For example, the nucleosynthesis calculations of Big Bang theory predict the relative abundance of the light elements - deuterium, lithium, and helium - as well as existence of only three families of neutrinos, and observations concur with these predictions.

In large part, the sophisticated treatments of the universe's past, present and future are possible because Einstein's Theory of General Relativity permits of global solutions that model the space-time geometry of the entire universe, and have physical consequences which provide tests of the models which cosmologists use to describe the universe.

Measuring Cosmic Parameters

The equations of general relativity incorporate several parameters which are used by cosmologists to model the expansion of the universe. While the various theoretical models offer predictions which may be used to test the model as a coherent whole, there are also empirically direct measures of the values for each of the parameters.

The Hubble Constant, H_0, is the parameter that refers to the current rate of the universe's expansion. Current values for H_0 vary between 45 km/s per Megaparsec and 90 km/s per Megaparsec.[25] The value for H_0 may be measured by several techniques, each involving a calculation that relates objects' spectral redshifts (i.e. their recessional velocities) with extra-galactic spatial distances, the two being related by the Hubble Law.[26] Redshifts can be determined by inspecting spectral features of an astronomical object's light, and astronomical distances can be measured using several methods. 'Standard candles', such as supernovae, Cepheid variables, and planetary nebulae, are objects of known intrinsic luminosity, and observations of their apparent luminosity permit us to calculate the distance to the object.[27] Measuring the time delay between signals arriving from the multiple images of a gravitational lens also allows astronomers to calculate extra-galactic distances independently of standard candles. The Sunyaev-Zel'dovich method determines spatial volumes by comparing the luminosity of x-ray emissions from hot gas

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[25] 1 Megaparsec = about 300 million light years

[26] The Hubble Law is expressed as: $v = H_0 R$, where v refers to the object's radial velocity, H_0 is the Hubble constant, and R refers to the radial distance to the object.

[27] Barring external influences on the path of light rays (such as gravitational lensing), the apparent luminosity of an object is dependent upon its intrinsic luminosity and its distance from the observer.

clouds in galaxy clusters with the cloud's scattering of the cosmic background radiation. Scientists then relate the volume of the gaseous region with its redshift to establish a value for the Hubble constant.

Experimentally determining accurate values for the Hubble Constant is all the more important because it has several cosmological applications. In addition to being used to calculate the distances to objects of known redshifts, accurate values for the Hubble Constant would also determine the adequacy of our theories about cosmic structure formation. The size of the largest possible physical structures is proportional to H_0^{-2}, because the size of causally defined material structures is bounded by the distance traveled by light before the decoupling era when matter became dissociated from the background radiation. Our theories must therefore reconcile size measurements of large-scale structures with values for H_0.[28]

Measurements of the Hubble Constant also establish estimates of the approximate age of the universe, or more precisely, the length of time the universe has been expanding, which is inversely proportional to the Hubble Constant, H_0. Knowledge of Ω, a parameter which denotes the global curvature of space-time, is also important for this calculation, and this quantity is presently thought to closely approximate unity. For a 'flat' inflationary universe, $\Omega = 1$, and so the age of the universe = 2/3 H_0^{-1}. If $\Omega > 1$, then universe is younger than 2/3 H_0^{-1}; if $\Omega < 1$, then universe is older than 2/3 H_0^{-1}. Using current values for H_0, the age of the universe is between 7 and 14 billion years, if the cosmological constant = 0 and the critical density = 1.

[28] National Research Council, [1995].

Independent methods have placed boundaries upon possible values for the age of the universe, and thus restrict the range of permissible cosmological models and parameters. For example, the radioactive decay of chemical elements permits one to determine the age of a material sample, and using this method scientists are able to calculate the age of earth (3.8 billion years), the solar system (4.56 billion years), and provide estimates for the age of universe. In the latter case, the calculated value is dependent upon theoretical models of isotope creation in early cosmic eras. The ages of globular clusters and the Milky Way also determine a lower limit to the age of the universe. In each case, the age can be inferred from the age of the population of oldest stars, white dwarfs. The luminosities of faint white dwarfs in the disk of the galaxy allow scientists to calculate the length of time they have been cooling. This, in turn, permits them to estimate the age of the stars and the galactic disk. Similar strategies are used to determine the age of old stars in globular clusters. There had been worries that data about the ages of globular clusters contradict standard $\Omega = 1$ cosmological models - early calculations of the age of clusters have been greater than that of the universe according to the Big Bang theory. These worries appear to have been allayed by Cepheid parallax measurements that show the clusters' stars are more luminous and younger than previously thought, and this data reconciles cluster ages with values for H_0.

The rate at which the expansion is slowing because of gravitational force is denoted by the parameter q_0, which also refers to the curvature of space, and measurements quantifying this value can reveal how long the universe will continue to expand. Using measurements of the apparent luminosities of bright light sources having calculable luminosity (e.g. standard candles such as supernova explosions), and relating these to their distance from us using measured redshifts, scientists may be able to determine if the geometry of the universe is open or closed: distant objects that are abnormally bright indicate a closed universe, while their being abnormally dim indicates a open universe, because curvature acts as a lens to curve light paths.

This curvature parameter is closely associated with the aforementioned parameter, Ω,. which refers to the ratio of the mass-energy density to the critical density (that required to close the universe). This parameter may be independently calculated, by measuring the kinetic energy of galaxies for example, but because the curvature of the universe corresponds to different mass-energy densities, i.e. different values of Ω, scientists may use this parameter to investigate curvature. For values of Ω greater than unity (1) the universe is closed, for Ω equal to unity the universe is flat, and for values of Ω less than unity it is open. Several experimental methods have been developed by astronomers to investigate the universe's curvature and density.

Firstly, theory predicts that the distribution of galaxy populations is an indicator of Ω. Volumes of space are charted and their populations of galaxies having particular red-shfts are counted. If volumes of space nearer to us contain more galaxies of the chosen redshift than more distant volumes of space, then the universe is closed. If the reverse is true, and the numbers of galaxies having that redhift are greater in more distant volumes, then the universe is open. Correlating galaxies' magnitudes with their redshifts thus offers an opportunity to determine values for Ω.[29] Secondly, the angular size of objects (or

[29] See (Silk, p.111), (Wright, pp.94-7).

separation of objects) decreases as their distance from us increases, but the effects of curvature upon light rays increase the observed angular size of distant objects. Universes of greater curvature, and thus greater density, would have greater effects upon the observed angular sizes of objects. Comparisons of observed and intrinsic angular sizes would thus permit calculations of spatial curvature. Thirdly, cosmological models use values for q_0 to make predictions about the size of volumes which are occupied by populations of galaxies having particular redshifts. By a fourth and related technique, astronomers hope to directly measure the expansion of space, and thus determine its curvature, by measuring those changes in extremely distant volumes which are due to the expansion of space. More generally, 'object counts' provide a good technique by which to assess cosmological models, because these theories make predictions about the population of objects which may occupy a volume. Observations that gravitational lenses are rare, for example, would forbid cosmological theories which predict greater numbers of gravitational lenses.[30]

The remaining parameter, the Cosmological Constant, refers to the universe's vacuum energy density, which counters the force of gravity and encourages expansion. If the value of the Cosmological Constant is non-zero, then energy - and therefore gravity - is present in a vacuum. Small values for the cosmological constant might account for galactic motions attributed to dark matter, and would have important consequences for the universe as a whole by accelerating the universe's expansion even after the initial

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[30] For examinations of the use of gravitational lenses in testing cosmological theories, see Wambsganss [1995], pp.274-7, and Kochanek [1996], p.379.

period of rapid inflation. Laboratory experiments to investigate the energy density of the vacuum suggest that quantum effects exert a pressure upon pairs of plates in close proximity, and that this may imply the existence of a small cosmological constant.

Testing the Cosmological Principle

Even the most basic of assumptions which are used by cosmologists to construct their theories may come to be examined by the scientifically-minded experimenter. The cosmological principle, for example, may be tested by experiment once it is reformulated in terms of isotropy and homogeneity. Universal isotropy refers to measurable quantities being the same in all directions, whereas universal homogeneity, or uniformity, refers to measurable quantities being the same everywhere. Isotropy entails homogeneity only if several observers at a distance to one another each find the universe to be isotropic. Scientists who are unable to test the isotropy of the universe from the perspective of distant cosmic locales must determine if the universe is homogeneous by some other means.

Hubble's Law offers one opportunity by which the isotropy of the nearby universe may be tested. In addition to being mathematically linear at close distances from observers, the Hubble Law is also isotropic in the sense that all galaxies, irrespective of their direction, are observed to conform to the law. If the Hubble Law were nonlinear, however, scientists' observations that it was isotropic would indicate we reside at the centre of the universe. When this anti-Copernican conclusion is denied to us as a matter of principle, the linearity of the Hubble Law becomes evidence that thelocal region of the universe is isotropic. More generally, this has been taken to be evidence for the cosmological principle: "Hubble's result is precisely what one would expect of an expanding universe governed by the cosmological principle. Any law of expansion but the linear one results in some direction being preferred over another. Hence the cosmological principle would be violated." (Silk [1994], p.34.)[31]

More direct evidence encompassing is also available. The charting of large-scale galactic distributions reveals that at scales of 10 Mpc, collections of galaxies form large, complex structures ('walls' and 'voids'). These anisotropies are thought to represent local density variations from global homogeneity, and greater scales (above 100 Mpc) do express more uniformity. In a homogeneous universe the numbers of galaxies increases as the observed volume is increased, and the number of galaxies is proportional to their distance from us. Astronomers have observed this proportional relationship up to 1000 Mpc, indicating that at this scale the universe is of uniform density. At greater distances and large redshifts, astronomers observe earlier cosmic eras which are expected to be of greater density, and this introduces deviations from the linear relationship. Strong indications of isotropy are also provided by radio galaxy counts, which demonstrate that at large scales galaxies do not preferentially populate different regions of the sky. The isotropy and large-scale homogeneity of galaxy counts accords with the CMBR data. The isotropic blackbody spectrum of this radiation indicates that the universe was, in early cosmic eras, thermally uniform, with matter-energy densities being both isotropic and

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[31] Silk's claim that the Hubble Law is linear is in fact false, though the law closely approximates a linear equation at (relatively) small cosmic distances. As such, it cannot be considered a test of the global isotropy of the universe. Silk's error was explained upon review of this thesis by Dr. Clutton-Brock.

homogeneous. The observations in each case act to independently confirm the cosmological principle.

The Modern Rejuvenation of Empirical Cosmology

Modern physical cosmology has for a long time been informed by the efforts of astronomers, but for a brief period during the twentieth century there was a trend towards the development of new theories about the origin of the universe which appeared to have very little to do with astronomical data at all. Exploring the realm of sub-atomic particles and forces, physicists began the task of formulating a theory which could then be applied to the first moments of the Big Bang. The physics of the very small came to be very relevant to the physics of what was now very large, and the field of Quantum Cosmology was born. This did not take place in isolation, entirely apart from laboratories and the observations of astronomers, but there was nevertheless a new emphasis given to the development of highly mathematical theoretical systems. Very briefly, the modern history of physical cosmology could be crudely characterized in the following way: it began as deep-space astronomy, became the construction of space-time models in light of General Relativity, then, after the synthesis of primordial matter began to be described by high energy nuclear physics, it was established as a discipline in which a Unified Theory would bring together the various forces of interaction among sub-atomic particles.[32]

As this happened, there was a temporary departure of modern physical cosmology from its astronomical perspective. In general, while astronomy has refined the details of

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[32] For greater depth of analysis regarding the history of modern cosmology, see North [1965], Merleau-Ponty [1976], and Bartuski [1993].

Big Bang Theory in the manner of Kuhn's 'normal science' - by, for example, quantifying the cosmological parameters, establishing better distance measures, and influencing estimates of the universe's age - the emphasis upon cosmo*genetic* theories provided remarkable advances with only occasional astronomical contributions. The observation of the heavens has thus only intermittently influenced modern cosmological theory, and while those influences have been revolutionary - as with the detection of galaxies' recessional motions, the discovery of a cosmic background radiation, and the charting of large-scale spatial distribution of galaxies - astronomy was largely unable to provide the court in which cosmogenetic theories would be tested.

This happened despite the fact that as high energy physics reached experimental boundaries which threatened to *under*-determine particle and field theories, the first moments of the Big Bang were thought to provide a laboratory in which they might be tested:

> Experimentally, because the energies needed to probe the GUT and the superstring theories are so fabulously large, the ultimate verification may come from the field of cosmology (the study of the origin of the universe). In fact, the energy scale in which this unification takes place can only be found at the beginning of time. In this sense, solving the puzzle of the unified field theory may very well solve the riddle of the origin of the universe. (Kaku and Trainer [1987], p.18)[33]

Cosmology has not restricted itself to astronomy in the past, and I see no reason

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[33] This attitude, that the early universe can act as a laboratory *in absentia*, is common in the Big Bang literature (See Silk [1994], p.72,), as is the attitude that cosmology is first and foremost concerned with *origins*. Kaku and Trainer's [1987] text also illustrates the historical divergence that occurred when physical cosmology was removed from its astronomical cradle by particle and field theorists: the cosmological theories described receive only perfunctory and fleeting evidence from astronomy.

why it should do so, especially in light of the fact that there are limits to astronomers' capacities, but this observational deficit has troubling overtones. Astronomy and the related field of astrophysics are, after all, involved in the direct observation of the cosmos, while other branches of physics investigate terrestrial conditions and apply their conclusions to distant space-time locales. The difference between the terrestrial and astronomical perspectives is akin to that of scientists empirically testing theories within a room so as to arrive at conclusions about what goes on outside the room, and scientists looking through a window to see what may be observed of the larger world. Ideally, both strategies are used by scientists who work together in their explorations of the world outside the room, and so it should be with cosmology. There is, therefore, good reason to have cosmologically relevant astronomical data, if only to test the accuracy of terrestrially formulated physics writ large.

In the last decade of the twentieth century, however, it came to be that astronomers' observations of the cosmic background radiation, which had decades earlier authoritatively established the significance of Big Bang theory and were touted as having scuttled all alternatives, became detailed enough to inform scientists about details of the universe's early structure and history. This evidence, taken to be 'fossils' which are indicative of the universe's initial structure, has been an invaluable test of physicists' models of the Big Bang and structure formation. "In principle," writes Silk, "one could hope to distinguish cold dark matter from hot dark matter, primordial adiabatic from primordial entropy fluctuations, and even an open universe from one at a critical density. All of these cosmological alternatives leave an ineradicable imprint on the sky." ([1994], p.234)

The Cosmic Background Radiation

Following the 1948 prediction by Alpher and Herman that the universe would have a measurable temperature, in 1965 the microwave background radiation was discovered.[34] While the observed values differed from early theoretical predictions, the initial measurements indicated that the universe was bathed in a uniform and isotopic microwave radiation which survived as a relic of conditions present soon after the Big Bang. As a result of these astronomical data cosmologists were able to conclude that the Cosmic Microwave Background Radiation (CMBR) is a record of conditions prevailing during the early universe, and that the formation of galactic systems and large-scale structure did not affect it. Later, beginning in 1989, more precise data from the Cosmic Background Explorer satellite (COBE) showed that the CMBR has a blackbody temperature of 2.728 K, and while it is isotropic at large scales, it exhibits anisotropies at greater resolutions.

The anisotropy is of two sorts: The *dipole* anisotropy, referring to variations in intensity corresponding to opposite regions of the sky, is a result of our motion relative to the 'last scattering surface', or the horizon corresponding to the point in time at which the universe cooled and the CMBR last interacted with matter (the decoupling era). Observations of dipole anisotropy allow scientists to calculate the velocity of the solar system with respect to the rest of the observable universe to be 370 km/s, and reveal that the CMBR is of cosmological origin - i.e., it does not have a local source, and does not

[34] This is attributed to Penzias and Wilson, who first reported the phenomena as radio noise, although Dicke and Peebles were to explain the accidental discovery as cosmic background radiation.

exist as a cloud of radiation that only surrounds our solar system, galaxy, or group of galaxies. The *intrinsic* anisotropy of the CMBR, however, refers to minute variations in its intensity which represent the physical conditions during the decoupling era, and the conclusions which may be inferred from observations of this anisotropy are profound.[35]

The fact that measurements of the CMBR spectrum are that to be expected of a blackbody object suggests that the universe was, at early cosmic epochs, of uniform temperature. This supports the Big Bang theory, and data about the CMBR spectrum (i.e. its thermal profile) thus restrict scientists' hypotheses about physical processes taking place in early cosmic eras. For example, the measured characteristics of the thermal spectrum are contrary to those expected of a universe that experienced energy-releasing (i.e. re-warming) processes which might have ended the inflationary phase of expansion. As well, the Big Bang theory predicts that the temperature of the universe decreases over time, (i.e., the CMBR cools) as the universe expands. CMBR temperatures would thus be associated with cosmic eras and their redshifts, and observations agree with this account. In one case the temperature of carbon atoms in an intergalactic cloud of known redshift was observed to be 7.6 K. This effectively measures the temperature of the CMBR, at that redshift, because the temperature of these atoms is assumed to be in a state of equilibrium with the CMBR. This result is consistent with the temperature theorized to

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[35] COBE measurements were at angular scales over 10 degrees, and scales of 1 degree here correspond to regions of space about 300 million light years in extent. These scales are greater than those involving the formation of galactic structures, and pertain to large-scale processes. The decoupling era, when matter last interacted with the CMBR and atoms were formed, involves medium-scale processes which would be observed in anisotropy measurements having accuracies of 1.5 degrees to 10 degrees.

obtain at that redshift, and as the present temperature of the CMBR has been measured to be 2.7 K, it is clear the universe has cooled in the intervening time.[36]

This is further evidence that early cosmic epochs were very different from our own. Devoid of galaxies, stars, and planets, the early universe was filled instead with ionized gas and radiation at extraordinarily high energies and remarkably uniform temperatures. This uniformity poses two problems. Firstly, how is it that similar temperatures existed in causally disconnected regions which would not have been able to achieve a state of equilibrium? Secondly, how did a thermally uniform universe form the material structures which are observed now?

Theorists answered the first question by postulating an inflationary process which rapidly expanded the universe from an early epoch in which there was a causal connection. This response proved to be extremely helpful because it also explained the existence of other troubling observations. For example, it is known that the present curvature of the universe is approximately 'flat', and that the density of the universe must therefore very closely approximate the critical density. This has important theoretical consequences for a description of an expanding universe, because it demands that the kinetic energy of matter is very nearly equal to its gravitational potential energy. This near equivalence has troubled cosmologists, because it appears to require that we ascribe precise initial conditions to the early universe. The reason for this is that in late cosmic eras such as our own, and for an expanding universe, small variations could only exist were there to have been even smaller variations in earlier cosmic eras. The existence of such precise initial

[36] National Research Council, [1995].

conditions troubles the sensibilities of theorists, who would much rather that physical processes cause conditions observed in the present to follow from arbitrary initial conditions.[37]

In part, this preference for a theory of origins in which the initial conditions are arbitrary arises because the existence of particular initial conditions cannot be explained. Other initial conditions are always possible alternatives, and one wonders why one initial condition came to exist instead of some other possibility. Modern formulations of the Anthropic Principle are one response to this problem, but cosmologists have found a solution to the 'fine tuning problem' which obviates the entire question of initial conditions. One of the advantages of Inflationary Big Bang theory is that it explains why the observed equivalence - a very fine balance found to have a precision of 1 part in 10^{60} at the Planck time - arises from arbitrary initial values for potential and gravitational energies.[38] Were there to be an imbalance between the kinetic energy and the gravitational potential energy, the excess would be manifested as a spatial curvature. In an inflationary universe, however, curvature decreases, or 'flattens', as time passes, and the kinetic energy due to expansion comes to balance the gravitational potential energy in later cosmic eras.

The second question about galaxy formation in a homogeneous universe was

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[37] This has been a prominent view in the history of cosmology, and has its early genesis in the cosmologies of the philosophers of classical Greece. See my discussion of the anthropic principle to follow, in *Theoretical Over-Extension and the Uniformitarian Thesis*.

[38] See Silk, [1994], p.108.

answered by suggesting that relatively small but statistically and *locally* significant variations from uniformity would supply regions which were more dense then others, and that the force of gravitation acted to collect matter in these regions, and form both galactic and stellar structures. According to this 'gravitational instability hypothesis', the regions of relatively high densities must have existed at the time the CMBR last interacted with matter (about 150,000 years after the Big Bang), because in later eras the expansion of the universe would have inhibited the formation of the structures we observe. More generally, the structure which is now observed must have had its origins in the very early universe, and that structure's early beginnings can be observed in the variations in CMBR intensity because anisotropies are indicative of density variations. This permits cosmologists a test of their theories of structure formation, because particular sizes of these anisotropies are required for gravitational instability to successfully form galaxies within the time that has elapsed since the decoupling era.

Other tests are also available. Mapping 'cosmic flows', or motions of galaxies which occur independently of the Hubble expansion, reveals the distributions of mass density which gravitationally caused galaxies' motions without relying solely upon the observation of luminous matter.[39] Using this technique, maps of groups of galaxies have been shown to also map the distribution of matter (both luminescent and dark) at large

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[39] Measuring cosmic flows requires that the distance to galaxies be measured independently of Hubble's redshift-distance relation. This involves, for example, the determination of a galaxy's apparent brightness and rate of rotation, and the application of the Tully-Fisher relation (which relates galaxies' rotational velocities with their luminosity), so that the galaxy's true brightness may be calculated. The distance to the galaxy is then calculated using these apparent and intrinsic luminosities.

scales with reasonable approximation, and knowledge of the amount of matter associated with each galaxy also permits statistical estimates to be made of the mass density of the universe, which is used by theorists to further investigate the critical density parameter. Measuring the sizes of the high-density regions and correlating these to their CMBR intensities thus informs us of important cosmological parameters. In addition, the data about mass densities in relatively late cosmic eras can be compared to the CMBR data about mass densities of early cosmic eras, a technique which permits tests of competing theories about the formation of galactic structure. In particular, the correlation of fields of cosmic flow with CMBR anisotropies is evidence that anisotropic matter distribution seeded the gravitational formation of galaxies; this is in agreement with the gravitational instability hypothesis, which predicts the motion of galaxies to deviate from the uniform rate of Hubble expansion as galaxies are formed and cluster together under the influence of gravity.

Observations of the CMBR also promise to reveal further information about the processes which formed later structures, and in so doing test theories of much earlier conditions. Theory suggests that 'topological defects' appeared when the separation of the electromagnetic and nuclear forces ended the 'grand unified era'. The separation of forces as the universe's density and temperature decrease is interpreted to mark a phase transition from a state of high energy to a state of relatively low energy. The topological defects, which remain despite the 'smoothing' influence of inflation because of their relatively late appearance, have the extremely high energy densities characteristic of the universe's earlier conditions, and consist of point 'monopoles', one-dimensional 'cosmic strings', two-dimensional 'sheets' or 'walls', and three-dimensional 'textures' or 'knots'. The gravitational effects of these defects would influence the formation of galaxies, and would also produce detectable temperature variations in the CMBR through lensing and spectrum-shifting processes. These CMBR variations would have a distribution that would contrast with predicted distributions of standard theories of structure formation, and measurements of the CMBR thus provide a good test of the topological defect hypothesis.

Conclusions and Qualifying Remarks

According to skeptics such as Boslough, we cannot have cosmological knowledge, but the astronomical techniques described above show that this is not true, and that we may have empirically grounded knowledge of a host of cosmologically important entities, structures, processes, and properties. There are, for example, good grounds to believe that the universe is expanding, and that the universe is both isotropic and homogeneous. Each of the cosmological parameters is empirically scrutable, and experimentally informed estimates of the age of the universe are possible. Scientists' theories of primordial nucleosynthesis and galaxy formation are testable, as are their descriptions of the history of the universe's expansion.

Given this wealth of information, we appear to know a great deal about the universe.[40] This is so despite the fact that some astronomical observations are subject to large margins of error, and may come to be revised upon further testing. Measurements of

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[40] An optimistic review of the state of late twentieth century cosmology is provided by the United States National Research Council, [1995].

the Hubble Constant, for example, are contingent upon distance measures which are prone to margins of error, and different techniques often provide different measures of this parameter. As a result, calculations of the age of the universe become estimates which are constrained within a range determined by previously obtained values, and these change as new data are acquired. Should this be considered a real problem for cosmology? I do not believe so. The uncertainties moderating the state of cosmological knowledge are no different from those found in the rest of scientific knowledge, which itself vacillates from theory to theory as new evidence is acquired, and must also provide for margins of error. If the possibility of error is an epistemic problem for cosmology then it is also an epistemic problem for science, and if we do not worry about the latter then there seems no reason to worry about the former.

If we understand theoretical knowledge to be fallible in this way, and permit uncertain but empirically defensible conclusions to be genuinely scientific results, then cosmology is indeed a provider of scientific knowledge. This view relies upon the fallibilist redefinition of the concept of theoretical knowledge, although it need not succumb to rampant skepticism. "Our fallibility is an insufficient basis for skeptical victory. We may accept the premise of the skeptic concerning conceptual change and the universal chance of error implicit therein without accepting the deep skeptical conclusion of universal ignorance." (Lehrer [1990], p.178) The skepticism Lehrer is addressing here is the profound philosophical skepticism which denies the possibility of knowledge, and it concerns the epistemic status of individuals rather than that of a scientific community, but his point is nevertheless a good one in this context as well. Claims to theoretical knowledge are not defeated by the demonstration that they are merely fallible, precisely because the justification which we do take to be the demonstration of theoretical knowledge does not guarantee truth. Our epistemic standards are simply not rigorous enough to promote skepticism, and the possibility of ignorance does not forbid the justified belief that theoretical knowledge is had. Empirical justification in the sciences, be it in physics, biology, archeology, or cosmology, is just this sort of demonstration of theoretical knowledge, and while we may lament limits imposed either by measurement error or by features inherent to the natural world itself (as with Heisenberg's Uncertainty Principle), they need not diminish the achievements made in the attempt to know the world: for example, we infer from the data that the universe is between 7 and 14 billion years old, and this is a significant contribution to our cosmology despite the margin for error.

My demonstration that cosmology can proceed empirically does not show that it does so in all cases. Critics such as Horgan do agree that we may have cosmological knowledge, and that it can be scientific, but they are quick to point out that there are *parts* of cosmological theory and practice which are unscientific, and cannot yield knowledge. In the next chapter I will examine the portion of cosmology deemed most contentious in this regard: the study of the universe's early history and origins.

Theoretical Over-Extension and the Uniformitarian Thesis

At the heart of the problem of learning about the origins of the universe is the problem of theoretical over-extension. Stated simply, it is the problem faced by theories which are extrapolated beyond the available evidence. In cosmology this problem is expressed in two ways, so that it concerns both the evidential warrant of hypotheses about the early history of the universe, and also the evidential warrant of hypotheses about high-energy interactions. Both problems, whether they concern the extrapolation of theories over time and space or the extrapolation of theories over different physical conditions, are answered by an argument about inferential generalization which resides at the foundations of scientific practice. This is the Uniformitarian thesis, and according to it theoretical generalizations are possible because fundamental natural regularities exist which transcend apparent physical differences.

Extrapolating Unified Theories

The high-energy conditions present during the very early universe cannot be described by present theories, and cannot be duplicated in the laboratory, so theoretical boundaries are thus compounded by a lack of evidence which might otherwise help scientists sort amongst their speculations. The first difficulty may have a remedy, and it may yet come to pass that a theory will satisfactorily account for events taking place during the Planck era (i.e. before $t=10^{-43}$ s). Physicists believe that such a theory would unite all of the physical forces, and some progress has been made in this regard, but while the electromagnetic, strong, and weak forces have been successfully described as different manifestations of a single force, gravity remains a puzzle: quantum theory and general relativity have yet to be reconciled, and details about the universe during the epoch of quantum-gravity are not known.

Testing a Unified theory is difficult because the energies required for a direct examination of conditions during the Planck Era (10^{19} billion electron volts) are unattainable. The theory may tell us what was supposed to have happened at those high energies, before symmetry breaking began, and it may successfully account for physicists' observations at relatively low energies - even to the point of confirming novel predictions[41] - but physicists will never be able to examine particle interactions under those conditions. Instead, evidence of a different sort is required, and theorists ask: 'what would be observed if the mechanisms and entities described by the Unified Theory had effects upon the Big Bang?'

By using the Big Bang as a 'laboratory at remove', cosmologists interested in fundamental forces and particle interactions are really using a well-corroborated theory of what might have been to test another theory of what might be. The theorists are fortunate, because in modern cosmology the models which find evidential support agree about important details of the universe's history: Modern physical cosmology is now Big

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[41] As yet, the problem with String theory is that it might not predict anything new which can be tested, while accounting for everything old. Its description of gravity, for example, is a startling 'post-diction' which surely counts in its favour. Nevertheless, String theorists hope that their conception of a Unified theory will indeed have novel empirical consequences at relatively low energies. See (Kaku and Trainer [1987], pp.138-9). The less ambitious Super-symmetry (SUSY) theory may be tested astronomically through a search for 'photino' particles, and their detection would further corroborate laboratory experiments which indicate a convergence towards unification of the weak and strong nuclear forces. See (Silk [1994], p.131).

Bang cosmology (which is one reason, it may be supposed, that cosmology is wrongly identified as being merely the study of the origin of the universe), and as a result it provides a context in which physicists develop new theories about how the universe changed over time. The 'paradigm' set out by the Big Bang tradition thus circumscribes the holes into which all theoretical plugs must fit, and it will continue to do so until evidence demonstrates that some other theoretical tradition is necessary. Given that observations may be interpreted within the context of Big Bang theory, tests of a Unified theory may be possible. For example, the interactions described by a Unified theory may have consequences for structure formation in the CMBR. It is thus at least *possible* to have empirically justified theories about the high-energy interactions which took place before the symmetry which 'bound' the forces together was broken as the universe cooled and expanded.[42] Whether or not *actual* Unified theories will yield (a) novel predictions for low-energy interactions, and (b) hypotheses about astronomical 'relics' of high-energy interactions taking place in the early universe, is another question entirely, but at least early-universe cosmology and Unified theory strategies are not 'unscientific' in principle.

Even if hypotheses about high-energy interactions cannot be empirically tested using the astronomical record, we should not immediately dismiss them. Indeed, if normal scientific practice is to provide the standard, then such hypotheses can even be authoritative. When each claim made by a Unified theory about low-energy interactions is

[42] This reveals an interesting point: the universe appears to be extremely well suited to our exploration of it. If the universe were really of the sort modeled by Steady State theory, for example, then physicists would have no opportunity to test high-energy interactions by observing the astrophysical record supplied by the Big Bang.

found to be empirically successful, then the entire theory receives support, and this includes its hypotheses about high-energy interactions. Once a generalization is affirmed, the evidential support for a theory is carried over to its hypotheses about unobserved events.[43] This strategy is scientifically legitimate for those cases where the theory uses unchanging fundamental structures to arrive at hypotheses, so the crucial task is to show precisely what similarities exist. For Unified theories, the mechanisms governing low-energy interactions are the same as those governing high-energy interactions, and so extrapolations to high-energy conditions are possible even when direct evidence about those conditions is unobtainable.

Before I address the Uniformitarian position in greater detail, and show how scientists use the same strategy when they reconstruct the history of a physical system - a crucial aspect of cosmological inquiry - it may help if I at this point examine an idealized example in which theories go beyond a limited range of evidence to provide hypotheses about observationally inaccessible events. The following analogy should thus help to illustrate the way an appeal to an underlying uniformity can be used to extend theories beyond the range of the available evidence.

The general problem of theoretical over-extension: the analogy of the graph

In this idealization, the totality of evidence is represented by a set of data-points on a graph (see below), and these are arrayed in a distribution that is seen to closely

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[43] See the introduction to Peebles [1993], for his comments upon the use of the uniformitarian thesis. "[W]e are going to extrapolate the physics that is known to be successful until it is shown to fail." (pp.7-9) He also notes that there is empirical support for this strategy in the astronomical evidence.

approximate a straight line.[44] The scientists using this graph generalize from the individual data-points, and infer that the evidence exhibits a natural regularity (or law) that has a linear mathematical description. From this they are able to associate the straight line with a theory, T(s).[45]

My analogy here has an historical cousin in the field of astronomy. Hubble's linear correlation between galaxies' redshifts and their distances from us was established with just such a method: the data were plotted, and a line of best fit was drawn through the data points. This linear relationship between galaxy motion and their position with respect to us came to be strong evidence that the universe is expanding. Initially, Hubble's Law was afflicted by problems of evidence: the data sample was relatively small and did not include the most distant galaxies. As a result, the sample provided an irregular distribution over a limited range, and there were genuine worries that the data might not represent the entire population of galaxies, and that the data might not rigidly determine a value for the slope of the line, i.e. Hubble's Constant. These problems have since been moderated by the gathering of more representative data, and in my own analogy I will assume that the data set is ideal in this way.

Philosophers of science have noted that for any set of data-points on a graph, an infinite series of lines may be drawn which connect the points, and from this it has been

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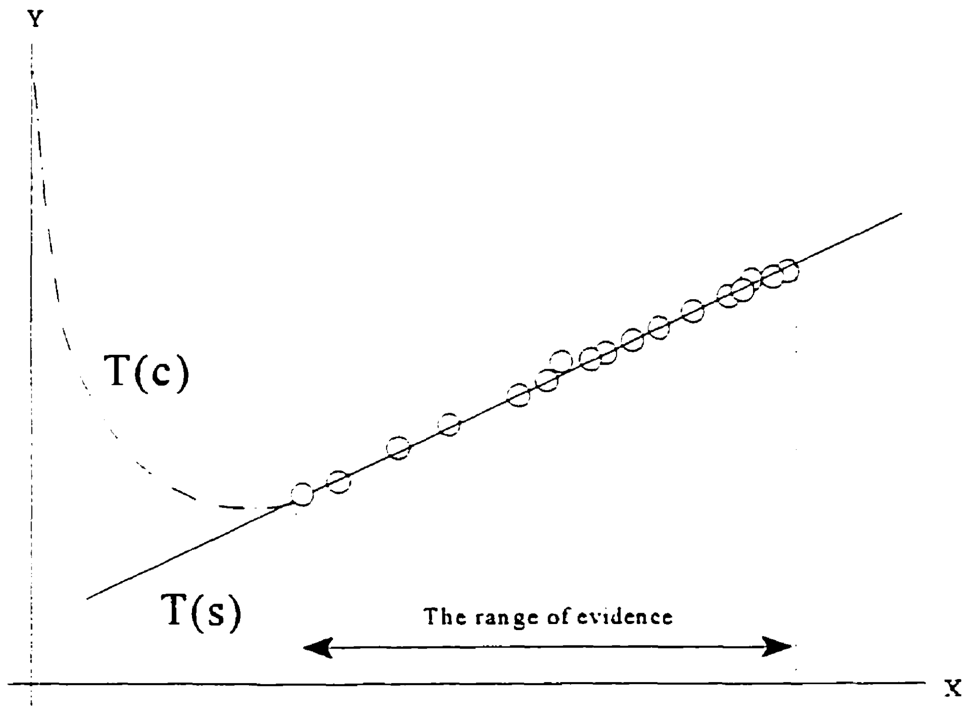
[44] Science is rarely so tidy, and scientists often have to spot regularities in data-distributions by using sophisticated statistical analyses, as well as by deciding which data-points are anomalous and which are not.

[45] For a brief discussion of this strategy and its problems, see Wright [1989], who includes a graph plotting "the sum total of our knowledge about cosmological models with which we have some confidence.' (pp.99-100.)

concluded that the law-like generalizations are either dangerously arbitrary or extremely convenient. For the moment, however, let us assume - as scientists often do - that this sort of 'underdetermination' of theory by evidence need not be problematic when we operate within the range occupied by the set of data points. Instead, let us investigate what happens when we try to extend the lines of the graph far beyond that range.

Once it has been decided that the data can be plotted linearly, it seems natural enough to extend the straight line indefinitely - or at least as far as the theory permits, since theories may specify the range over which they can be applied. Scientists have no reason to suspect that things will be significantly different in other experiments, and may legitimately expect that new data, could they be plotted, would conform to the straight line. But what should be done if ingenious theorists develop a theory which demands that the evidence be viewed differently? Such a theory may claim that the natural regularity is not really linear; instead, the data conform to what appears to be a straight line over a limited range, but beyond that range the new theory, T(c), predicts the line to curve.

Figure 1: The analogy of the graph



How are we to decide amongst these alternatives? For each line, individual points (x,y) correspond to statements about the world: 'when X exists, Y exists'. So, as values for (x) approach the axis, T(s) and T(c) make very different (y) claims about the world, and these form two sets of individual hypotheses. Ideally, scientists would be able to avail themselves of more data-points which would lie on either T(s) or T(c), but not both, and so lead us to favour one theory over the other. For the purposes of this analogy, however, I will stipulate that there is no way to collect data within the range where the line might become curved. In such a case, where the individual hypotheses cannot be tested, can there be good reasons to prefer either theory, and can those reasons be scientific?

There may indeed be reasons to think that one theory is superior to the other, but these would necessarily refer to various theoretical virtues. Such non-evidential reasons might even be scientific, in the sense that science tends to use such reasons when it judges theories. But we shall need very good, very persuasive reasons if we wish to say 'we know that where there is X there is also Y, because T(s) must be true or is most likely to be true.' Justifying the necessity or likelihood of a theory's being true without decisive empirical evidence requires an epistemology which depends (at least in part) upon Rationalist premises, and the worry, of course, is that there may be reasons to doubt the necessity or likelihood of T being true. In light of these concerns, perhaps the best thing to do is to admit that we just do not know which Y corresponds to which X near the axis, even if we do have reasons to advocate some T.

Before we conclude that the possibility of such knowledge should be dismissed out of hand, it should be recognized that scientists have promoted what would become empirically successful theories by appealing only to extremely compelling theoretical reasons. Einstein provides a convenient example: independently of evidence in its support, he was convinced by the logic of his General Relativity theory that it was a correct description of the world. By this view of scientific discovery, which is common to Unified theorists in general, the mathematical description of nature corresponds with real relations and structures in the world, and from truths about the former theorists may deduce truths about the latter. In addition to being scientific in the sense that scientists apply this method to problems, a theory-led strategy may also be empirically successful. The success of this strategy in the case of relativity theory would appear to be evidence that scientists can successfully place theoretical demands upon the world, and reasonably expect them to be correct, even though convincing but false theories must remind us of scientists' fallibility in this regard. The reliability of this approach in cosmology, however, remains to be seen. Inferring from the history of science that theory-led strategies are successful relies upon empirical measures of success, so it is clear that standards of empirical evidence provide the final and most important arbiter in scientific decision-making.

The decision-making involved in an analysis of the graph is comparable to that taking place in modern cosmology, where there is the worry that theories may become over-extended and underdetermined. The analogy can more closely approximate the case of modern cosmology if we stipulate that T(s), which here represents a standard model of the Big Bang, does not extend far beyond the range for which there are representative data-points, whereas T(c), which represents a Unified Theory, is used to extend the line even further towards the axis. Adherence to a principle of uniformity is the reason we might intuit that T(s) should be extended; the straightness of the line within the range defined by the data-set is taken to be an indication that the same linear relationship between X and Y will hold regardless of the values for X and Y. If we have good reasons to abandon the uniformity principle, however, then the data only represent the behaviour of phenomena within a limited range of conditions; this is precisely the problem for present models of the Big Bang, and theorists cannot describe early cosmic times because Big Bang theory predicts changes and differences which forbid the unrestricted use of a uniformity principle. Before cosmologists can make claims about the early universe, their theories must reinstate a uniformity principle by identifying natural regularities which govern interactions during early cosmic times, and also manifest themselves in later cosmic eras as more recognizable forces. Recovering the uniformity principle in this way would, in the analogy, extend T(c) rather than T(s), even though the uniformity expressed by the curve is not fully represented by the data-points.

If theoretical knowledge of the past is contingent upon a uniformity principle, then knowledge of the universe during the Planck and Unification Eras is not possible without knowledge of the 'hidden' uniformity underlying all natural phenomena. Is such knowledge possible? The answer, for now, is an inconclusive, 'perhaps.' It is possible that a Unified Theory may make novel predictions which are testable within the range of evidence available to scientists. This would be akin to discovering new data-points within the range of evidence which do not lie on either T(s) or T(c), but do agree with some other theory, T(u). On the graph, the relationship would no longer be linear within the range of evidence, but would 'spike' in the regions of new data, and the new theory would clearly be preferable. The only alternative is to somehow extend the range of evidence into the region where the T(c) begins to curve. Either strategy, were it to be successful, would go some way toward solving the problem.

Unfortunately, this does not really dissolve the problem of theoretical over-extension, because it does not *fully* extend the range of evidence, and it does not solve the problem of underdetermination, because alternative theories may be proposed which are empirically equivalent to either of the theories.[46] But these are not problems which Big

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[46] In the present context, the underdetermination problem as it is usually presented seems unlikely in practice, given that there is no proliferation of good Unified Theories. Theorists have yet to formulate even a single adequate Unified Theory. Obviating the

Bang cosmology faces alone, and other scientific theories are similarly vulnerable because they too go beyond the available evidence. The worry, however, is that cosmology may go beyond the evidence in ways which are not permitted of legitimate science, because the important similarities demanded by the Uniformity assumption do not exist globally. To examine this further, we need to understand something of how principles of uniformity are used to justify scientists' hypotheses about the past, and if these can be legitimately used in cosmology.

Uniformity and Cosmological Principles in Cosmology

Hypotheses about the remote past are common to all palaetiological sciences, and these rely upon uniformitarian principles to restrict speculation about unobservable antecedent conditions which are the cause of presently observed phenomena.[47] According to the Uniformitarian school of thought, "unless the past of the system under study is in some important way similar to its present, the freedom involved in hypothesizing about the [remote past] is so great that no genuine science of the system at hand is possible" (Balashov [1994], p.935). The argument which leads to this conclusion claims:

> (G1) We can only base our knowledge of the world upon that which it is observed to be now.
> (G2) If the past is like the present, we can use our knowledge of the present to know the past; otherwise our knowledge of the present tells us nothing about the past.
> (G3) It follows that if we cannot apply a Uniformity assumption to the effect that the past is like the present, knowledge of the past is impossible.

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problem of underdetermination in this way has been remarked upon by (Kitcher [1997], pp.247-55.)

[47] See Balashov [1994], especially (pp. 935-6, 942), and Schild [1962],

Scientific knowledge of physical systems thus relies upon there being a connection - an 'epistemic bridge' - which extends from the present to the past, and this connection is parasitic upon those features of the system which do not change over time. In an otherwise changing universe, one similarity across time which is thought to support our knowledge of its past is to be found in the unchanging natural laws which govern dynamic systems. This reasoning resides at the core of most science, and the Uniformitarian thesis is in this regard so important that "the constancy of laws is an indispensable assumption of scientific method in general, since no generalization from experience is possible without it." (Balashov [1994], p.935)

Modern cosmology forces us to ask the question, 'while the laws of nature appear to be locally invariant, do they change globally?' Steady State theorists were especially worried about this question, because they thought that the changing material structure of the universe could act to change natural laws. This was one of the reasons that they were so adamant about denying the possibility of a globally dynamic universe.[48] Even the more successful Big Bang cosmology posits just this sort of relationship between physical laws and the universe's global structure: "In this currently accepted picture, the laws of nature (though not the most fundamental ones) turn out to be dependent upon the particular state the universe happens to be in as it undergoes its evolutionary development." (Balashov [1994], p.956) This is not, however, reason enough to object to the way cosmology uses the Uniformitarian principle to address the remote past. The Uniformitarian principle is reinstated at a more fundamental level, because the laws of nature which do change as the

[48] See my earlier discussion of Bondi and Gold's arguments.

universe changes are themselves governed by natural regularities which would be described by a Unified theory. The Uniformitarian thesis rebuffs arguments that cosmological hypotheses about the early universe are too speculative, and shows that they are no different from any other hypotheses about the past. The Uniformity principle applies regardless of events' temporal proximity to our present observations, and cosmology cannot be singled out as a special case.

Uniformity and Principles of Indifference in Cosmology

The Uniformity principles can, in various guises, be used to recommend the sort of cosmological theory we should prefer. As cosmological principles, for example, arguments from uniformity have been used to promote Steady State theories and to affirm that we can have knowledge of the early universe.[49] They have also acted in debates about humanity's place in the universe, suggesting that in a uniform universe of indistinguishable parts we cannot argue sensibly that we occupy a privileged position. As this argument is used to deflate anthropic (or more properly, 'anthropocentric') recommendations that scientists explain the universe's initial conditions with some appeal to humanity's existence, the Uniformitarian argument has ramifications beyond its statement that the cosmological principle must be true if we are to know the universe's past. Given that the anthropic principle has often been construed as unscientific by the critics who appeal instead to a principle of indifference, this has particular relevance to the present argument

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[49] See my earlier discussion of the Perfect Cosmological Principle.

that cosmology can be scientifically undertaken if it uses a Uniformitarian strategy.[50]

The indifference principles noted by McMullin ([1993], pp.364-6) have two aspects, and these articulate different instances in which the cosmos ignores humanity. According to the 'locality thesis', the universe is indifferent to our existence in the sense that we do not have a privileged or necessary position in it. The Copernican indifference principle (familiar to us from the Uniformitarian argument) is just such a statement, and is contingent upon the cosmological principle, because it claims that our location in the cosmos is not significantly different from other possible locations.[51] According to the 'emergence thesis', the universe is indifferent to our existence in the sense that its initial conditions are not necessarily arranged to cause our existence. The atomist philosophy of classical Greece promoted this thesis when it claimed that the universe did not have a specified or ordained initial state, and that complex order manifests itself according to natural physical mechanisms without the intervention of an organizing agency or mind.

Recent work in cosmology has encouraged an emergence thesis while showing that the locality thesis is partly false, and this leads us to question the Uniformity assumption. Regarding the latter, the dynamic universe described by Big Bang theory has cosmic eras when humanity cannot exist, so there is thus a sense in which we do occupy a preferred 'location' in the universe. A robust Copernican principle is thus clearly false, because our

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[50] For an excellent scholarly examination of the anthropic principle, see Barrow and Tippler [1986].

[51] McMullin [1993], p.737n.

spatio-temporal location in the cosmos *is* different from others.[52] The locality thesis can accommodate this point to some extent, with an appeal to a weaker cosmological principle which need only show that other parts of the universe are similar to our own. By this formulation, our cosmic location is not unique or special because it is just one of a great many similar regions which could also have produced human life. Regarding the emergence thesis, the Inflationary Hypothesis has shown how currently observed cosmological structures and conditions could be the product of many different initial conditions. Many features of the original system, such as mass-energy density heterogeneities and curved geometries, are lost as the universe rapidly expands, and present cosmological structures become all the more likely as a result of the inflationary process. The inflationary hypothesis thus explains why the present universe exists as it does without explaining why its particular initial conditions came to exist - indeed, its strategy is to explain the present universe by showing how an explanation of the initial conditions is not really necessary in this context.

Despite the very real achievements of the standard inflationary hypothesis, it does remarkably little to settle the anthropic furore about initial conditions: Firstly, the 'coming into being' of particular initial conditions still seems to demand an explanation which the inflation hypothesis cannot provide. Secondly, it does not settle the matter of how we should understand humanity's relationship with the cosmos, and as with any effort to show that the structure of the present universe is a likely outcome, the inflationary hypothesis is

[52] See Katz [1988], pp.112-3, and McMullin [1993], pp.373-4.

vulnerable to anthropic interpretations.[53] Inflation is itself a product of initial conditions which are acted upon by inflating mechanisms, and since this contingency is advantageous to human existence, its explanation can itself be anthropic. Thirdly, the inflationary hypothesis only shows how "the present state of the universe could have arisen from quite a large number of initial conditions" (Hawking [1988], p.132). It does not - and cannot - show that the present state of the universe could be the result of *any* and all possible initial conditions. This means that the inflationary hypothesis does not really advance our understanding of that which it is often touted to explain.

Why this is so deserves an explanation of its own, and the following analogy may prove helpful. A bottle is observed to be full, and the only (known) cause is a splash of water. It is very unlikely that the narrow opening of the bottle would have caught the splash of water, and this improbability seems to be a puzzle; it is analogous to the 'fine tuning' of the universe's initial conditions. The splash of water is more likely to enter the bottle if a wide funnel is used, because the funnel increases the probability that droplets of water which would not otherwise enter the bottle may now do so - just as inflation increases the probability that a young universe will come to exhibit the order which is now observed. But explanatory appeals to the action of the funnel do not explain the splash itself, nor do they necessarily make the likelihood that the splash will land in the funnel any greater than the probability that the splash might miss the funnel altogether. All they explain is how the action of the funnel fills the bottle once the water is splashed into it.

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[53] This illustrates a curious feature of the anthropic principle. It is used to explain improbable occurrences, but works to best effect when other occurrences which depend upon them are shown to be necessary.

Since the relative probability that the 'inflationary' action of the funnel will have a chance to fill the bottle still remains very low, the original puzzle still remains: 'why is the bottle now filled, given that it is an unlikely occurrence?' Similarly, the mechanisms governing inflation do not explain why the universe exists as it does given that it had unlikely initial conditions.[54]

The fact that we have origins in favourable initial conditions, despite their being extremely unlikely, still seems to mark a puzzle deserving of explanation. One response, which is at most weakly anthropic, is that the instantiation of the initial conditions is not really perplexing after the fact because we could hardly observe the universe to be otherwise.[55] The problem with this approach is that it does not tell us *why* the universe had particular initial conditions while other and equally likely initial conditions remained unrealized. It only tells us why our observations are unsurprising. Ideally we would be able to glean a *cause* for our universe's particular initial conditions, and this would

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[54] It may be that inflation is a process which can occur through many possible 'inflating' mechanisms, making it a probable result of arbitrary (and thus relatively unlikely) initial conditions. In my bottle analogy, many funnels could catch the splash. If this is so, as with Linde's 'Chaotic' Inflation Theory, inflation can indeed explain why the universe exists as it does.

[55] According to what has been called the 'weak anthropic principle', our existence has consequences for our observation of the universe, but our existence is not a necessary outcome of the universe's history. The fact that we require certain conditions to exist as we do implies that we should expect to observe those conditions which permit our existence. In an important sense this principle is not genuinely anthropic because it is does not propose that the structure of the universe is a caused consequence of our necessary existence.

dissolve the puzzle by removing the troublesome improbability.[56]

Making the 'coming to being' of initial conditions less improbable is also the purpose of a robustly anthropic explanation, according to which the universe is necessarily structured to bring humanity into being *because humanity's existence is important to the cosmos*. This is the crux of the anthropic thesis and means that the universe exists so that we may exist. Understanding the way in which humanity is 'important' in this way has been the problem facing formulations of the principle. Historically, anthropic principles have been interpreted theistically, that the universe was created by design so that humanity was the eventual and necessary outcome. But not all anthropic principles have agreed about the way in which this divine plan may be carried out: physical change may or may not be directed by teleological natural laws, and may or may not require perpetual divine supervision.[57] The more recent 'many-universes' hypothesis states that we necessarily exist because all possible universes exist.[58] But while its claim that humanity necessarily exists seems to agree with the anthropic view, the peculiarity of that necessity means that it is really only an ontologically replete indifference principle: it is not genuinely anthropic

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[56] For an examination of the explanatory power of the anthropic principle, see Wilson, [1991].

[57] McMullin ([1993], p.373) notes that according to the *Cartesian* 'indifference' principle, the universe is designed so that natural order is a product of physical mechanisms governing the change from initial conditions. It is strongly anthropic if it is interpreted to claim that the universe was designed so we would be a necessary consequence of the changing natural order.

[58] See also McMullin [1993], Katz [1988], Kanitscheider [1985]

because it avoids assigning cosmic importance to humanity's existence.[59]

The perceived problem with anthropic principles is that they do not recognize the universe could well exist as it does without us (or observers generally)[60], and it is the claim that our existence is a contingent aspect of natural order which distinguishes indifference principles from anthropic principles.[61] The Uniformity thesis makes the indifference position all the more clear by showing how humanity's existence is contingent in the spatio-temporal context.

Whatever the problems with anthropic principles, the problem with anthropic modes of *explanation* is that they spot a problem where there is none: the fact that the universe's initial conditions were unlikely does not mean that they need explaining when they happen. Similarly, the fact that a particular result of a roll of the dice is unlikely does not mean that it needs an explanation when it happens.[62] Katz observes that "the occurrence of improbable events, *by themselves*, engender no need of explanation,"

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[59] McMullin ([1993], p.380-2; p.387) argues that the strong anthropic principles cannot be scientific because science promotes a principle of indifference which strong anthropic principles deny. Indifference principles lend themselves to scientific application because they minimize ontological commitments and do not require us to acknowledge things for which there is no evidence. For example, the existence of a deity, the preferred existence of humanity, and the existence of other possible universes are not ontologically respectable in modern science, and are thus *de facto* unscientific, because there is no evidence that such things really do exist. This relies upon a very limited and relativistic interpretation of the term 'scientific', since evidence, and thus 'scientific ontologies', may change.

[60] See Leslie [1992] regarding the concept of observership.

[61] See (Katz [1988], p.144).

[62] See also Leslie [1988] and McGrath [1988].

because such events only need explanation if they are more frequent than they should be. ([1988], p.119; see also p.116) Therefore, the appearance of humanity from improbable (but possible) events is only puzzling if it happened more often than it would have done had things proceeded arbitrarily and without regard for human existence. The anthropic principle must thus show that the fact that it happened at all is somehow exceptional, but it cannot do so with an appeal to our unlikely origins because it cannot demonstrate that those origins are any different from what would be the 'normal' instantiation of initial conditions. The improbability of initial conditions simply does not count as evidence that the universe is anthropically directed, and as a result, anthropic explanations fail because they cannot demonstrate that our universe is even the most probable one, let alone a necessary one.

Another consequence of the above reasoning is that if we are puzzled by unlikely events generally, and all possible universes are equally improbable, then the instantiation of *any* set of initial conditions is a puzzling occurrence without some further explanation of why those particular conditions came to be. Anthropically directed universes are not special in this regard. If, then, the universe's initial conditions really do constitute an improbable 'event' which needs explanation, what sort of explanation must that be? Railton [1978] concludes that claims of the sort, 'improbable event E occurred because there was a small likelihood of E happening,' are the *only* explanatory account available for a single event which cannot be explained in terms of necessary or probable conditions. It follows that arbitrary initial conditions can only be explained by the claim, 'they had some likelihood of existing as they did'. This is by no means the *final* explanation, however, because cosmologists must examine the sense in which the universe had a (remote but non-zero) probability of existing as it does. If this is not possible, the universe's existence is a 'brute fact' which can have no further explanation.

Attempts to avoid this conclusion, when they show how the universe had some likelihood of existing at all, seem to presume the prior existence of 'tuning' mechanisms which initiate particular initial conditions. This strategy is not without its own problems. The existential questions about our universe's reason for coming into being can all too easily be asked of the mechanisms' existence as well.[63] Also, worries that the laws governing events within our universe do not apply to these mechanisms will immediately lead to the objection that the Uniformity principle ceases to apply under such circumstances. It is at this point, where the Uniformitarian thesis can go no further, that it seems theorists must circumscribe the boundaries of scientific cosmology.

Limits to the cosmological principle and cosmological science

Legitimate applications of a uniformity assumption can protect theories about the distant past from being unscientific. Not only is the assumption a recognized element of scientific generalization and extrapolation, it is a necessary aspect of any attempt to know the world. Moreover, the assumption allows scientists to bring their empirical skills to bear upon theories which might otherwise appear to be detached from the available

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[63] It might be optimistically suggested that some future physics could answer the question of how the universe came to exist. Even so, any new species of physical theory would necessarily apply the Uniformitarian thesis to that question, and this certainly does not avoid the problem of 'existential explanatory regress'. If we explain the universe's existence by appealing to that which causes it to exist as it does, then what is to explain the existence of that cause?

evidence. In light of this, theorists can proceed scientifically and with careful regard for the evidence when they posit hypotheses about the early universe, because theories' empirical support is sustained by the Uniformitarian thesis. This effectively dissolves the criticism that there is a speculative freedom in cosmology which prevents its examination of the universe's early history from being scientific.

While the uniformity assumption allows cosmologists to make use of a sizable body of evidence so as to arrive at conclusions about the past, the same cannot yet be said of current hypotheses about the causes of the Big Bang's initial conditions, although this may change as new and better theories are devised. However, at some point the uniformity assumption may no longer apply, and this eventuality imposes limits upon scientists' investigations. If there are conditions under which the Uniformity thesis is violated, should fundamental physical laws either change in unknown ways or cease to exist at all, then those laws tell us nothing which might inform us of those conditions. If, for example, the concept of time cannot be used in accounts of conditions that existed before the Big Bang because it has no possible referent, then physical laws for which time is an independent variable are simply made irrelevant in any description of those conditions. At the boundary beyond which the physical laws of later epochs do not apply, the most that can be said of an earlier physics is that it ended by manifesting the first conditions which are recognizable to our theoretical extrapolations. It is thus clear that where there are limits to the application of the Uniformitarian thesis, there shall also be limits to scientific - i.e., empirically founded - knowledge. Where there are fundamental differences which cannot be bridged by some recognizable natural order, the investigation and extrapolation of that natural order must come to a halt and nothing more may be learned. As the next and final chapter will show, this epistemic matter is entirely unrelated to the question of whether or not cosmological theories lose their scientific status once they become too speculative.

Laudan and the Epistemic Failure of Demarcation Criteria

At the core of any attempt to cast doubt upon the scientific status of a theory is a demarcation criterion, or perhaps a set of demarcation criteria. It has been usual in the philosophy of science for the demarcation problem to be formulated in terms of science and 'pseudoscience', but here I examine the more general divide between scientific and unscientific theories. In either case, however, the demarcation criteria are normative, and so declare that science should accord with certain standards which are designed to further the pursuit of knowledge. Were this true, it would follow that theories which fail to be scientific also fail to be knowledge, because they do not satisfy the normative demands of the specified demarcation criterion.

Laudan's analysis of demarcation

According to Laudan ([1988] pp.343-5), normative demarcation criteria must themselves satisfy several requirements. They must accord with our intuitions regarding unequivocal cases of science and non-science, and so resist counter-examples; they must demonstrate that science has epistemically important features which non-science lacks, and so establish the superiority of scientific knowledge; they must also permit us to unambiguously sort science from non-science. In addition to these essentials, demarcation criteria must present the necessary and sufficient conditions by which a proposition, belief, or practice is known to be either scientific or unscientific. Conditions which are merely sufficient, or simply necessary, will not suffice: "Without conditions which are both necessary and sufficient, we are never in a position to say '*this* is scientific: but *that* is unscientific.'" (Laudan [1988], p.344) These requirements provide valuable meta-criteria by which attempts at demarcating science from non-science may be assessed.

Laudan has used these meta-criteria to good effect in his analysis of the history of the demarcation problem, and has shown that, while we are urged to have scientifically sanctioned beliefs (i.e. theories) and to denounce unscientific beliefs, good demarcation criteria have not been formulated which would identify those beliefs. In particular, there is an unavoidable tension between the classical, idealized view of science and modern, fallibilist epistemology, and demarcation criteria cannot accommodate both conceptions of scientific knowledge. According to Aristotle, scientific knowledge is the comprehension of causal first principles, and the certainty which distinguishes it from opinion is a consequence of its infallible foundations. 17th century theorists (e.g. Galileo, Newton) and philosophers (e.g. Bacon, Descartes, Kant) redefined science so as to forgo the classical reasons for upholding its certainty, but maintained nevertheless its epistemic superiority; "the infallibility of results, rather than their derivability from first-causes, comes to be the single touchstone of scientific status." (Laudan [1988], p.340) The fallibilist epistemology emerging in the 19th century rejected the view that science offered certain conclusions, and the epistemic fecundity of the scientific method was invoked to preserve a distinction which would render scientific attempts at knowledge superior to unscientific attempts at knowledge. Despite many efforts to articulate the epistemic merits of the scientific method, and to demonstrate that the distinctively scientific character of its features contributes to that merit, demarcation criteria have been largely unsuccessful; the lack of consensus among philosophers, ambiguous points of demarcation, as well as the failure to account for the actual methods of scientists, inhibited a good and explicit definition of the scientific method. (Laudan [1988], p.341-2)

Early 20th century philosophy of science offered new alternatives for demarcation. The Vienna Circle, following Wittgenstein, attempted to formulate a theory of meaning by which all meaningful statements about the world (and thus scientific theories) genuinely refer. The positivists' criterion of 'verifiability' was unable to identify science and condemn metaphysics, however, and this was so for many of the same reasons that Popper's 'falsifiability' criterion would later be observed to fail: counter-examples prove both criteria to be wrong, as unscientific theories may be both verifiable and falsifiable, and scientific theories may be both unverifiable and unfalsifiable. More generally, Laudan concludes that demarcating science from non-science using some concept of testability does not suffice to establish that scientific theories warrant our belief, whereas unscientific theories do not, because "testability is a semantic rather than an epistemic notion, which entails nothing about belief-worthiness." (Laudan [1983], p.346)[64] He is here using the term 'semantic' to emphasize the hypothetical and contingent character of theoretical claims. These hypotheses can be compared with the world, but as they are independent of empirical evidence, they entail nothing at all about whether or not the theory really does describe the world. They are 'merely words', and it is perhaps in this sense that Laudan

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[64] Testable theories entail predictive observational claims of the form 'Given a physical system P having conditions X_1, X_2, X_3... X_n, if theory T(P) is true, the physical system will also be observed to have conditions Y_1,Y_2,Y_3 ...Y_n.' This is a minimal requirement for a testable theory. For a theory to be verifiable, there is the additional condition that there be some Y which, if observed, would verify T(P). For a theory to be falsifiable, there is instead the additional condition that there by some Y which, if observed, would demonstrate that T is false.

uses the term 'semantic' or 'syntactical' to emphasize the difference between hypotheses which exist in an evidential vacuum, and theories for which there is decisive evidence (be it confirming or disconfirming). His point is that testability is a concept which tells us that a theory can be related to the world, but in no way tells us if that relation will affect the theory's belief-worthiness.

While there is much to commend it, Laudan's analysis is overly simplistic for two reasons. Firstly, if they had been successful, demarcation criteria of the early 20th century could have been used to determine standards of belief-worthiness. A good verifiability criterion would have shown that metaphysical nonsense was not suited to our beliefs, and our justified beliefs would necessarily be scientifically meaningful. As it happens, this criterion simply does not work as a theory of meaning, and neither does it work as a demarcation criterion. Popper's criterion came closer to the mark, though not as a theory of meaning. By identifying both the scientific method and what it is for a theory to be scientific, the criterion was supposed to be a comprehensive statement of how the method could - and should - approach theories to determine their belief-worthiness. The criterion was thus a statement of how physical knowledge could not be possible without the application of the scientific method, and that the application of the method is itself contingent upon the theory being 'semantically' scientific. Testability in its early incarnations (verifiability and falsifiability) was thus both a methodological and 'syntactical' criterion which could legitimate the formation of scientific beliefs, and show why unscientific beliefs could not be empirically justified. Laudan's analysis misses this complexity, and he incorrectly concludes that "scientific status, *on their analysis*, is not a matter of evidential support and belief-worthiness." (Laudan [1988], p.346, my italics) Laudan has thus confused the reason for the criterion's being untenable (i.e. its being merely 'semantic') with its *intended* scope of application, and so misrepresents the history of early twentieth century philosophy of science.

Secondly, it is clear that the 'syntactical' and 'semantical' attributes of theories do indeed have some bearing upon their belief-worthiness. While Laudan is correct to say that the semantic attribute of testability does not determine a theory's belief-worthiness (because a theory can be testable and not be worthy of our belief), he forgets that the semantic attribute of *un*testability does indeed determine a theory's belief-worthiness: a theory which cannot be tested is denied an avenue by which we would judge it belief-worthy.[65] This asymmetry has important consequences, which will be examined in the next section.

More on untestability

Testability may remain a necessary, but not sufficient, condition for belief-worthiness, but it is not a sufficient condition for something to be science because testability is not unique to scientific theories. As such, and by Laudan's meta-criteria, it cannot be a demarcation criterion. But is it also true that testability is a necessary condition for science, and is an untestable hypothesis always unscientific? There are

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[65] In fact, some falsified theories which have some supporting evidence are more worthy of our belief than untestable theories. Newtonian gravitation, for example, has been falsified and replaced by Einsteinian gravitation, but it remains that Newtonian gravitation is more worthy of our belief than a gravitational theory for which there can be no evidence. Newtonian theory, while falsified, nevertheless has evidence in its support which an untestable theory can never have.

reasons, in the form of counter-examples, to conclude otherwise. Within the body of scientific theory there are claims for which there is no conceivable experimental or observational test. These claims comprise what are, in effect, assumptions which are tacitly accepted as being true. For example, each of the following claims is extremely basic from the scientific point of view, and their truth is only questioned by the philosopher and metaphysician: 'our observations of the world do not systematically deceive us, the laws of physics apply irrespective of time or place, and for every event there is a cause.' These are 'scientific' claims about the world, in the sense that they are included within the body of scientific theory and are presupposed by scientific practice, but they are also untestable. This also extends to epistemologically important principles which guide scientists' methods of inspection and analysis: the claim 'simple theories are more likely to be true', for example, is not empirically testable, although it is often used in scientific practice as part of its governing theory of knowledge. The truths of mathematics, as well as the logical rules of deduction and inference, are also non-empirical parts of science.

But should we conclude that untestable theories can be scientific because there are untestable portions of science? Not necessarily, if we adopt a model of scientific theories which establishes a hierarchy of scientific claims that are made up of two classes of scientific propositions: propositions in the 'periphery' must be empirically tested whereas those in the 'core' are (provisionally) exempt from such testing during the life of the research programme.[66] The core contains the governing epistemology, the normative principles of scientific practice, the systems of logical reasoning, and a set of 'metaphysical' presuppositions, which together provide a starting point for an inquiry which is directed towards learning about the natural world. In the periphery, contingent statements about the natural world provide hypotheses which demand an empirical decision. According to this view, if a theoretical claim is not testable, and does not belong in the core, then it is not scientific.

I am not entirely satisfied with this position because it generates new quagmires of demarcation. In the first instance, how are we to distinguish between testable and untestable theories, and in the second, how are we to know if a theoretical claim should (or should not) be testable? [67] If the former is not a problem, surely the latter must be. Demarcating amongst claims must allow some movement within the hierarchy, because there are *prima facie* instances of metaphysics becoming science.[68] But if we allow theoretical claims to move within the hierarchy, when should claims in the core move to

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[66] This model of scientific theory mirrors that proposed by Lakatos [1970], but emphasizes the distinction between those theoretical claims which are subject to testing, and those which are not.

[67] Rothbart [1982] approaches this last question when he notes that a meta-criterion for any demarcation of science from pseudoscience must indicate when a theory should be tested.

[68] The historical transition from scientists assuming that the shape of space is Euclidean to their investigation of non-Euclidean spatial geometry could be interpreted in this way. It is also important to note that movement within the hierarchy might not be limited to supposedly 'metaphysical' claims abut the world. It may even be legitimate to understand the methodological and mathematical tools as being subject to empirical review: some methods and sorts of logical reasoning may be observed to be more successful than others.

the periphery (i.e. be actively investigated), and when should theories in the periphery become part of the core (i.e. become 'received wisdom' or 'dogma')?

Even if these questions can be properly answered, and we conclude that untestable theory claims are unscientific with only a few exceptions, a demarcation criterion would have to accommodate testable but unscientific theories. One attempt at solving this problem involves *degrees* of testability, such that theories which are more testable are more scientific. Unfortunately, it is difficult to associate high degrees of testability with high degrees of belief-worthiness, and low degrees of testability with low degrees of belief-worthiness, so such a criterion would not be epistemically significant. Apart from problems with comparing different theories' degrees of testability (Laudan [1988], p.346), there are also unscientific theories which are, *prima facie*, highly testable. In fact, most unscientific theories (e.g. the claims of astrology and creation 'science') are thought by scientists to be tested to such a degree that they are falsified - which surely constitutes a set of theories which is highly testable. This is yet another instance of unscientific theories becoming 'scientific' by being demonstrably false, so that "*by virtue of failing the epistemic tests to which they are subjected, these* [unscientific] *views guarantee that they satisfy the relevant semantic criteria for scientific status!*" (Laudan [1988], p.347, his italics) But this is hardly the way to show the epistemic superiority of scientific theory.

The myth of the scientific promise

Laudan has cut to the heart of the demarcation problem with his claim that semantic demarcation criteria do not have epistemic consequences, but he has not gone far enough. I would add to Laudan's analysis the following: If a demarcation criterion is to show scientific theories to be epistemically superior to unscientific theories, it must refer to something other than the mere *possibility* of a theory having confirming or disconfirming evidence (i.e. its testability). Prospective criteria must show that scientific theories are, as a matter of fact, more worthy of our belief than unscientific alternatives, and this can only be so if they are more able to be confirmed than unscientific theories - the assumption here being that a theory's belief-worthiness accrues through its confirmation.

What sort of features allow a theory to be *disposed* to be confirmed, and so become scientific? Minimally, the theory must be testable in some way, so that there is an opportunity for some evidence to convince us that the theory is (approximately) true. However, for a theory to actually be disposed to be confirmed, and be given to empirical success (as this is how confirmation comes about), there must be the further demand that when observations are made of the world, they are *likely* to be found to confirm the theory to some degree. This is the 'promise of confirmation' criterion, and *any* attempt at normative demarcation must satisfy this condition if it is to establish the epistemic superiority of scientific theories, and so affirm the privileged status we give to science. Indeed, according to this criterion, 'theory T(P) is disposed to empirical success and confirmation' just means, 'the physical system P will likely be observed to exist as theory T(P) describes.' But this is a statement about the world, for which there can be no justification without some appeal to the evidence. According to the 'promise of confirmation criterion,' the scientific status of a theory is contingent upon its observational claims being approximately true (i.e. the theory being empirically adequate).

Such a criterion is wrong on several counts, however. Firstly, the 'promise of confirmation' criterion just amounts to a mode of demarcation which holds that scientific theories are empirically successful, but the fallibility of science shows that this is an impossible point of demarcation: not all scientific theories are empirically successful. Secondly, there is no sense in which theories are genuinely disposed to have systematically true observation statements just because they express scientific features. If theories are (approximately) true, they are so because of their relationship with the world, and not because of their 'semantic' constitution. But demarcation criteria must assess theories independently of the evidence for or against them - such is their *raison d'etre*, to provide grounds for the quick dismissal of a theory on the basis of its uniquely 'scientific' features, without having to assess its evidential merit. In general, the related concepts of epistemic merit and belief-worthiness necessarily concern a theory's justificatory evidence, but theories themselves do not bring such evidence with them independently of some investigation: there is nothing about a given theory, independently of its relation to the world and the body of scientific knowledge, which demands that there be evidence either for or against it. Therefore, demarcation criteria which only concern the non-evidential attributes of theories, such as the 'semantic' features noted by Laudan, cannot *in principle* successfully associate belief-worthiness with scientific theories. This observation offers a compelling reason (perhaps the most compelling reason) to abandon efforts at normative demarcation, but two others remain.

Laudan on scientific diversity and privilege

Laudan has used a very different strategy to profess the death of the demarcation problem. Using the history of science, he shows that an authoritative demarcation is likely to be impossible, and science does not really merit the privilege that demarcation advocates wish to give it. The first conclusion is substantiated by the occurrence of important epistemic differences among scientific theories, to such an extent that there is no non-trivial epistemic characteristic which sets scientific theories apart from, and above, unscientific theories.

> Some scientific theories are well tested; others are not. Some branches of science are showing high rates of growth; others are not. Some scientific theories have made a host of successful predictions of surprising phenomena; some have made few if any such predictions. Some scientific hypotheses are *ad hoc*; others are not. Some have achieved a 'consilience of inductions'; others have not. (Laudan [1988], p.348]

Within the class of theories which are scientific, it appears to be the case that there is no epistemic condition which is common to all the member theories. This suggests that identifying uniquely scientific and epistemically superior features of theories is a lost cause, and Laudan concludes it is likely that there are no necessary and sufficient conditions by which a theory is identifiably scientific or unscientific.

It might be suggested in response that the identification of necessary and sufficient conditions for science and non-science is not strictly necessary for such arguments, because properly identified sufficient conditions may alone show examples of non-science to be incapable of yielding knowledge. We may thus have reason to argue that unscientific propositions and beliefs are never knowledge if, but only if, non-science does indeed lack those epistemically important features which uniquely characterize science, and so give it privileged status.

Laudan's second argument demonstrates why science is not privileged in this way. While I have argued (in *The scientific promise*) that there are good philosophic reasons to conclude that this privilege is not really warranted, Laudan has used his historical analysis to do the same: "There seems good reason, given from the historical record, to suppose that most scientific theories are false: under the circumstances, how plausible can be the claim that science is the repository of all and only reliable or well-confirmed theories?" (Laudan [1988], p.348) The fact that science is composed of false theories is held to demonstrate that reliability and high degrees of confirmation are not necessary and sufficient conditions by which a theory is scientific. Features that would otherwise confer privilege to theories are thus unrelated to scientific status.

In reply to Laudan, however, it must be said that while many, possibly all, scientific theories are probably false, this does not show that they are also unreliable and unconfirmed. Indeed, we have reasons to think otherwise, because there are reliable and well-confirmed theories which are at least not egregiously false. At most, Laudan's argument shows that the privilege given to scientific theories cannot be justified by an appeal to their truth. Any privilege which remains for false but well-confirmed and reliable theories accrues from their being either *approximately* true, or because they are just the most reliable and best confirmed theories available.

Fortunately for my own purposes, Laudan's argument can proceed without relying upon the ubiquitousness of false scientific theory, because an inspection of the history of science will show that not all scientific theories have been (or are) either reliable or well-confirmed. This is especially so when a theory has been initially proposed, and in its state of infancy has not acquired the degree of evidential support which is common to more mature theory. It follows that reliability and high degrees of confirmation cannot constitute a normative demarcation criterion, and scientific status is thus effectively disassociated from those features which might give it epistemic privilege.

Conclusions

By showing how critical modes of analysis which rely upon demarcation criterion are usually wrong, and always irrelevant, I hope to draw attention instead to the genuinely important points of theory analysis. It has been noted by other philosophers that we should assess the state of our knowledge about the universe by examining evidence rather than by concerning ourselves with identifying what theories are or are not scientific. According to Laudan, for example,

> Insofar as our concern is to protect ourselves and our fellows from the cardinal sin of believing what we wish were so rather than what there is substantial evidence for (and surely this is what most forms of 'quackery' come down to), then our focus should be square on the empirical and conceptual credentials for claims about the world. (Laudan [1988], p.349)

This view has also been put forward by Quinn ([1984], p.49) in the context of assessing 'pseudosciences' such as Creationism. Unfortunately, this has not been the prevailing attitude, and demarcation criteria continue to be used long past their time of philosophical credence to discredit theories. This is no doubt largely due to the tremendous authority which is given to science, and the notion that the certainty we tend to give to scientific knowledge is due to its theories having been empirically tested. The suspicion that a theory cannot be tested is thus enough to render it both unscientific and epistemically suspect, but rarely is it noticed that these two features are not necessarily conjoined.

Bunge and Hacking make this error, as do Boslough and Horgan. The foregoing examination of the way demarcation criteria have been applied to cosmology has been an attempt to rectify this philosophical muddle while also demonstrating that cosmology is not as speculative as might be thought. Not only are there empirical methods with which to test cosmological models, but the application of the Uniformitarian thesis offers scientists an opportunity to extrapolate their theories and form empirically credible conclusions about the early history of the universe. If there are to be limits to this knowledge, then it is not because the discipline suddenly fails to be scientific, but because some of its hypotheses cannot be properly substantiated.

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