

**“We Shall Have to Make the Best of It”: The Conversion of Dennis
Sciama**

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ABSTRACT

The cosmologist Dennis W. Sciama (1926-1999) was a long-standing advocate of the steady state model of the universe. This theory, originally proposed in 1948 by Hermann Bondi, Thomas Gold, and Fred Hoyle, suggested that the universe was eternal, and unchanging on the largest scales. Contrary to the popular image of a scientist as a dispassionate, unbiased investigator of nature, Sciama fervently hoped the steady state model to be correct. In addition, and also *pace* the stereotypical image of a scientist, Sciama was motivated significantly by “extrascientific” or aesthetic factors in his adoption of the model. Finally, Sciama, in a stark contrast to the naïve falsificationism usually presented as a virtue of the “scientific method,” went through a several-year period of attempting to “save” the model from hostile data.

However, Sciama abandoned the model in 1966 due to increasingly reliable data relating to the distribution of quasars. Thus the Sciama case also stands as a counterexample to irrationalist criticisms of science, according to which scientists can and will always find ways to hold on to their “pet” theories until they die, regardless of contradictory data. Sciama’s conversion also sheds light on the iterative process that goes on as scientists localize and attempt to repair faults in their theories.

DEDICATION

To my parents—the finest people I have ever met

PREFACE AND ACKNOWLEDGEMENTS

I have loved science, and particularly astronomy, as long as I can remember. Amongst my earliest memories is watching the Apollo moon landings; I also recall vividly as a child making quarts out of notebook paper, and adorning them with drawings of the planets and text describing the then-current state of knowledge regarding each. This passion for science followed me to university; as my institution had no astronomy major I declared for physics instead, pursuing astronomy as one of my minors.

However, at university I also renewed my love affair with the humanities, which I had discovered in high school, taking as many courses in history and literature as time permitted. (I well recall the sour face my faculty advisor made when I informed him I'd be taking classical literature instead of technical writing.) So far as I knew, however, the Two Cultures, as C.P. Snow famously characterized them, were completely isolated from one another, things to be compartmentalized, things between which one had to "choose."

In 1987, I moved to the University of Maryland to pursue an advanced degree in physics. I learned much, but soon realized I was neither inclined, nor was I cut out to be, a research physicist, though while there I did have the good fortune to learn under teachers such as Joseph Weber and Carroll Alley. In a happy coincidence, however, in 1989 I discovered UM possessed a program in the history and philosophy of science—a field of which I had never heard, but was delighted to find existed. The interdisciplinary mix for which I'd longed as an undergraduate (without even knowing it) was there before me—and it was one to which I felt I could make a contribution.

While a student of UM's Committee on the History and Philosophy of Science (CHPS), I had the great privilege of studying with the likes of William Wallace, Jeffrey Bub, and my Master's advisor, Stephen Brush, to whom I will always be indebted for introducing me to the rich field of history and philosophy of science (HPS) and for holding me to high standards of scholarship. I entered CHPS with what I now realize was a naïve, if standard-for-physics-students, attitude toward science. I left the program with a much fuller appreciation for the historical and philosophical aspects of science as a human activity, of the complexities inherent in doing science, and of the embedded nature of science in the culture.

After taking three years off to run the Honors Program of the college at which I am employed, Prince George's Community College, I discovered in 1999 the existence of both Virginia Tech's science and technology studies (STS) program and its satellite campus in the DC area. I found in that program not only some excellent instruction, but also sensitivity to the issues facing working professionals returning to school (exemplified by things such as their scheduling of courses at night, etc.). Tech's STS program also filled in a piece of the puzzle regarding science that I had not fully appreciated while with CHPS—a greater understanding of the social issues that infuse science (but which far from invalidate it).

This paper is the culmination of my Virginia Tech experience. It is also the result of a great many kindnesses which I now happily acknowledge.

First and foremost, for their unfailing support through the years I thank my loving parents Jim and Pat Hunt (to whom this work is dedicated), who have my entire life

encouraged me to excel and to value learning. Thanks go to my sister Carrie for her support, as well as to my large extended family—aunts, uncles, cousins, grandparents—some of whom are no longer with us, but whose memory I will carry with me always.

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My cats Sacha, Alexei, and Tomppa have provided me with much love and companionship, but I thank them also for, *inter alia*, their vocal complaining at being ignored during critical moments of the writing, their walking across (and lying down

upon) the computer keyboard, their helpful rearranging of carefully-organized research materials, their waking me at inhuman hours, etc.

For their support during this long process I also thank my colleagues at Prince George's Community College. There have been so many who have been so encouraging through the years I hesitate to mention any names at all for fear of omitting some. However, I would be remiss if I did not mention Alicia Juarrero, Isa Engleberg, Janet McMillen, Mimi Bres, Scott Sinex, Cathy Sinex, Barbara Gage, Barbara Blum, Marie Robinson, Cindy Gossage, Wendy Perkins, Dave Dyer, Odeana Kramer, and Meg Ryan. I am grateful to Jack Bailey for the timely loan of a laptop, and to Majoree Graves for her assistance in translating an old paper to electronic form. *Merci beaucoup aussi* to Jacques Vieyra and Betty Charro for helping me (re)learn French. To all those whom I have omitted, please forgive me—you know who you are and I appreciate your support.

For their support as comrades-in-arms in the Tech STS program I thank Lisa King, Jean Suplizio, and Diana Hoyt. Major gratitude goes to Chris Cosans, who sought me out at a CHPS colloquium to recruit me to the Tech program personally; without his outreach I would not be writing this right now. I owe a great debt as well to the late Bert Moyer, my first-year advisor at Tech, for helping me to get off to a good start in the program.

For helpful comments on earlier versions of the chapters of this paper, I thank Sarah E. Newcomb, Mark Lesney, Eric Saidel, and Stephen Brush. Particularly, I thank Dr. Brush for his encouragement vis-à-vis the embryonic version of Chapter 2 of this paper, written many years ago during a graduate seminar at the University of Maryland,

which led to my first mention in the literature; it was a very minor thing, but nonetheless thrilling and flattering for a humble graduate student.¹ I also thank Dr. Brush for his consultation on aesthetic induction upon my embarkation on this project, when it seemed that topic would encompass more than merely one chapter of the work.

I thank Lindley Darden for a helpful discussion on anomalies over a memorable dinner at Lupo's in College Park. I very much also appreciated the encouragement and collegiality of the participants—particularly Robert Smith, Steven Dick, and Helge Kragh—in the Fifth Biennial History of Astronomy Workshop in 2001 at Notre Dame, at which I presented a short version of the research as it then stood. They may not even remember their comments to me then, but they meant a lot nonetheless as I was going forward with the project. Similarly, I thank Ann Laberge and other Virginia Tech folk for their kind words and suggestions after my presentation of the work in 2003 at Blacksburg.

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¹ (Brush 1993, p. 586)

It is hard to imagine having a better student advisory committee. I thank Richard Hirsh for his great humor, and also his willingness to step up to the plate, filling in when Bert Moyer died so tragically. I thank Steve Weiss for his excellent classes, his great advice, and the genuine camaraderie I've felt from him over the years. To Barbara Reeves, who never failed to find time to meet with me during her trips to DC, I owe many thanks, particularly for her encyclopedic knowledge of the literature. And I thank Dick Burian for his genuine enthusiasm, unfailing generosity, and razor-sharp insights.

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Joe has recently stopped taking on students, and I consider myself extraordinarily fortunate to have slipped in under the wire to be one of the last cohort. I am genuinely saddened no more advisees will benefit from his wisdom, determination, festivity, and personal care—but if anyone has earned respite from such duties it is The Mighty Joe Pitt. Enjoy it, Joe—you've earned it. I'll always remember what you've done for me.

Having said all of the above, of course, any and all errors that are present in this work are mine and mine alone.

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LIST OF ABBREVIATIONS

α - β - γ	“Alpha-Beta-Gamma” theory of nucleosynthesis
ADTR	anomaly-driven theory redesign
B ² FH	Burbidge, Burbidge, Fowler and Hoyle’s theory of nucleosynthesis
BKS	Bohr-Kramer-Slaters theory
CHPS	Committee on the History and Philosophy of Science
CMBR	cosmic microwave background radiation
D-Q	Duhem-Quine thesis
GR	general relativity
HA	heuristic appraisal
H-D	hypothetico-deductive method
HPS	history and philosophy of science
FLC	FitzGerald-Lorentz contraction
PCP	perfect cosmological principle
QED	quantum electrodynamics
QM	quantum mechanics
QSO	quasi-stellar object
QSS	quasi-stellar source
SDIC	sensitive dependence on initial conditions
STS	science and technology studies
TRE	Telecommunications Research Establishment
UM	University of Maryland

CHAPTER 1

INTRODUCTION

Those who cannot remember the past are condemned to repeat it.
—George Santayana, *The Life of Reason or The Phases of Human Progress: Reason in Common Sense*

Studying the Study of Science Scientifically

“History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation of the image of science by which we are now possessed” (Kuhn 1970 [1962], p. 1). Thus begins the first chapter of Thomas Kuhn’s *Structure of Scientific Revolutions*, perhaps the most influential book in the history of the philosophy of science. Rejecting the earlier logical positivist program of Moritz Schlick and Rudolf Carnap, which focused on the logical analysis of scientific knowledge, Kuhn instead encouraged scholars to use historical data to foster understanding of how science actually works.

However, forty years on, Kuhn’s influence has left Science and Technology Studies (STS) in a peculiar situation. The conclusions STS researchers have drawn from this “historical turn” are legion, ranging from the fairly traditional to the radical and postmodern. Among them are that scientists:

- Are members of a community that shares the same overarching worldview

- Blame their skills or equipment rather than their theories when the latter are faced with falsification
- Are always able to “save” a pet theory by via the use of ad hoc hypotheses
- Ignore troublesome anomalies altogether, or perhaps only until they are solved by a rival model
- Are simply practicing “politics by other means”
- Cannot communicate (at least not fully) with scientists in rival camps
- Abruptly switch models in a *gestalt* way
- Go to their graves without changing pet theories, or if they do change them, are more likely to do so when they are younger (“Planck’s Principle”)
- Produce science biased by their races, genders, and cultures
- Socially construct or negotiate models having no real relation to the world

Certainly, *all* these claims cannot be true, for the simple reason that many are logically inconsistent with each other. So which—if any—of these mechanisms accurately reflects what really happens in science? The question is an important one, given that many of the above claims flatly deny that science tells us anything objective about the world—i.e., that there is nothing special about scientific knowledge compared to other so-called “ways of knowing.” Thus the door is opened to radical relativism—and not just cultural relativism, but relativism vis-à-vis the *ontology of the physical world*. Considering the value that modern society places on science due to its perceived objectivity about the nature of the world, the project of confirming or refuting such claims can be seen as urgent.

Commenting on these contradictory and often provocative conclusions, the philosopher of biology David Hull and colleagues complain that while a result of the “historical turn,” much STS research has in fact been

carried on in the abstract. Anecdotes and casual impressions pass for evidence. In fact, the idea that such disputes [regarding the operation of science] might actually be settled by recourse to evidence hardly seems to have occurred to those engaged in them” (Hull, Tessner et al. 1978, p. 717).

Hull proposes what might be termed a “scientific approach” to evaluating these wildly disparate STS claims. Much as theories about *nature* are put to the test in science by comparison with experimental data, in this “meta-analysis,” theories about *science* should in Hull’s view be similarly evaluated based on data from an even closer look at the historical record.

Hull concedes up front the non-triviality of constructing fair and accurate tests of STS ideas, but urges the project forward nonetheless:¹

Testing the claims that scientists make is extremely difficult. Testing the claims that philosophers of science make is even more difficult, difficult but not impossible....Without attempting to test philosophical claims, it is difficult to know what they mean....I urge more students of science, especially philosophers of science, to test their claims about science as often and as rigorously as possible....Too often those of us who study science tend to employ methodological practices that we would condemn as inadequate in the work of those scientists whom we study (Hull 1998, p. 209-211).

Hull’s call has been taken up by a number of STS researchers. Stephen G. Brush, for instance, in several papers tested the familiar claim of Karl Popper and other philosophers that scientists prefer theories that make novel predictions over those that can only explain already-known phenomena,² finding scant evidence for the claim. The dramatic prediction of light-bending in Albert Einstein’s general relativity, for example, Popper himself held up as the quintessential kind of “novel” prediction theories should

¹ Some of the difficulties Hull points out in testing even a seemingly simple and straightforward claim as Planck’s Principle are: is it biological age that is the crucial factor, or *professional* age (i.e. how long ago one entered the field); is Planck talking about all scientists or just the scientific elite; must older scientists literally die in order for the field to advance, or is “professional death” (i.e. retirement or leaving the field) sufficient. Planck’s Principle is scrutinized more carefully in Chapter 3.

² “We require that the new theory should be independently testable. That is to say, from explaining all the explicanda which the new theory was designed to explain, it must have new and testable consequences (preferably consequences of a new kind); it must lead to the prediction of phenomena which have not so far been observed. This requirement seems to be indispensable,” said Popper (Popper 1965, p. 241).

make in order to be accepted.³ But Brush's historical analysis shows physicists did not, in fact, value this prediction more than the theory's explanation of the advance of perihelion of Mercury's orbit—even though light-bending was a risky, novel prediction and the explanation for Mercury's behavior a mere “retrodiction” (Brush 1989). Other scholars have, per the Hull program, tested STS theses regarding anomaly resolution, incommensurability, and so-called “crucial tests,” among many others; see (Donovan, Laudan et al. 1988) for a volume of such case studies.

This dissertation aims to add to this literature by providing an in-depth case study of the conversion of the cosmologist Dennis W. Sciama from the steady-state model of the universe to the big bang model. Such a study is particularly appealing for a number of reasons. First, Sciama did not merely accept the steady state model, but was rather an ardent and enthusiastic believer in it; as such, his changeover is of particular interest. Second, Sciama consistently described himself as having a “philosophical bent;” as such, an analysis of his conversion may give some perspective on at least some the philosophical theses about theory change mentioned above. Third, Sciama left an unusually large “paper trail” in the wake of his changeover; as such, this case presents a significant opportunity historiographically speaking. His unusually frank and explicit statements regarding his conversion further facilitate possible testing of some of the various STS claims outlined above. Fourth, as an episode in the history of 20th century cosmology, Sciama's scholarly journey falls into the realm of history of recent science, an understanding of which arguably is of particular import to us in the contemporary

³ See (Popper 1965), Chapter 1.

world. Finally, as Helge Kragh notes in his book *Cosmology and Controversy*, most studies of cosmology are focused on the pre-20th century era (Newton, Kepler, et al.), and what few studies are focused on the 20th century are of the prewar period. “This scholarly lapse is dismaying,” as I have written elsewhere, considering the 20th century witnessed cosmology’s emergence as a true science—a rare disciplinary transition that should be of keen interest to scholars. Further, postwar cosmology in particular offers scholars an unusual opportunity to study a scientific community’s grappling with “debate rife with metaphysical, even religious, presuppositions” (Hunt 2001, p. 249). A study of Sciama’s conversion would thus follow the work of Kragh, who sees his *Cosmology and Controversy* as a “beginning point” for further scholarship, and others in helping to redress a “sad state of affairs” in the historiography of modern cosmology.

Focusing on an individual scientist’s theory choice should not be mistaken as a return to so-called “Great Man” history. Rather, it should be seen as an acknowledgement that while science is a social process, as Ronald Giere puts it, in science studies “individuals are primary” (Giere 1989, p. 8). That is, often

Higher level structural theories fail to capture the basic causal interactions that drive science as a human activity. These interactions occur primarily among individual people. Crudely put, it is only individual people that possess the motive power to generate social activities....The causal locus remains with the individuals who make up...groups, not with groups as higher level entities (Giere 1989, p. 6).

And, as Thomas Nickles says, today

Few are comfortable with the Hegelian-sounding idea that the primary agents of human history are whole societies or cultures, much less abstract Reason....There are now many attempts to restore human agency, albeit agency located in rich socio-cultural fields of action. Even scientific biography has not lost its defenders. Several recent biographies are sophisticated productions that allow for agency without hagiography.... (Nickles 1996, p. 11).

The Dilemma of Case Studies

However, some commentators have raised concerns about whether case study analysis, *pace* Hull, is a useful or valid method for philosophical scholarship after all. Joseph C. Pitt, for instance, has recently objected to case study methodology by framing what he sees as a dilemma regarding it: on the one hand, if one starts by looking at a particular case study with an eye toward drawing conclusions or evaluating claims about science, one risks drawing unwarranted generalizations based on a paucity of data. That is, case studies are in some sense inevitably parochial. On the other hand, if one starts with generalized theses about science, and then attempts to use case studies to evaluate or demonstrate said theses, one risks being accused of cherry-picking data points (i.e., case studies) selectively to support the author's point of view on that which is being evaluated.

I will address both horns of Pitt's dilemma, beginning with the claim that case studies are inherently myopic. This is certainly true in one sense, since any given scholar must by necessity provide limits of some kind to her case study (otherwise the project's lifespan would likely exceed her own), she by definition is excluding material that could be relevant to a fuller understanding. The subsequent product is, it is argued, the academic equivalent of looking at a landscape through soda straws—some straws being larger than others, but straws nonetheless.

Pitt, for instance, cites approximately 25 different facets of Galileo's character and circumstances around which one could frame a case study: Galileo as Platonist,

Galileo as Instrument Maker, Galileo as Catholic, and so on. Taking the first of these as an example, a detailed exposition on the Platonic and Neo-Platonic influences on Galileo might miss, Pitt argues, the Aristotelian and Archimedean aspects of Galileo's science (also on Pitt's list), both of which were crucial to understanding the Pisan's work.

This might be termed the meta-Duhem-Quine (D-Q) problem. The D-Q thesis (as usually defined in contemporary STS) despairs of ever being able to disprove any single hypothesis *within* science due to the enormous interconnected web of background knowledge that could be altered in principle to "save" the hypothesis. In a similar vein, the meta-D-Q thesis despairs of being able to investigate any specific claim *about* science in isolation, since there are always other relevant contexts one could cite vis-à-vis the science in question that could impinge on the (claimed) new knowledge.⁴

The answer to this problem, of course, is to be mindful that the context one chooses to study is only one aspect of the topic in question—as Pitt himself later concedes. However, this limitation does not negate the possibility of uncovering new knowledge about the topic. To use the metaphor of a jewel, a detailed mapping of one facet might not provide a complete picture of the whole gem, but it does provide undeniably useful information about it.

Further, not all contexts are relevant. In both science and science studies, it is indeed possible to rule out legitimately some factors when trying to understand a given episode. Just as the falling of a methane snowflake on Titan will not significantly affect the operation of the Hubble Space Telescope, we can with equal validity neglect in

⁴ The D-Q Thesis will be discussed more fully in Chapter 5.

science studies some of the universe of factors in assessing a historical case study. The effect of that same snowflake on the scientist himself, for instance, may be justifiably neglected, as can the fact that the scientist in question happened to be a fan of the *Hollywood Squares* in his younger days. While there may be no algorithm for such a discernment process, it nonetheless happens, and does so based on reasonable standards of argument.

Pitt argues against the validity of extending any given lesson learned from an historical case study to present or future science: “It is unreasonable to generalize from one case or even two to three....The philosopher who looks to the past as revelatory of the present is doing bad history.... (Pitt 2001, p. 373, 379). That is, since past contexts are invariably different from present ones—every moment in history is unique, things are always changing—it does not follow immediately that past lessons hold any value whatsoever for us today.⁵ “As philosophers we seek universals, but the only universal regarding science is change,” he writes (Pitt 2001, p. 374). Pitt’s desire for a “Heraclitian” philosophy of science—so-called due to its resonance with that pre-Socratic philosopher’s Doctrine of Flux—has this at its centerpiece.⁶

⁵ This argument is, incidentally, similar to the physical one used to justify the steady state model of the universe. If the universe evolves over time (as the rival big bang model suggests), then the universe is “different” at every single moment of its existence, and there is therefore no guarantee that the laws of nature we perceive now will apply in the future or have applied in the past. As such a state of affairs is undesirable in the extreme, steady state proponents considered this a powerful argument in favor of their model; this, however, did not prevent the model’s abandonment in the face of mounting evidence against it. See Chapter 2 for details.

⁶ Richard Burian takes issue with Pitt’s desire for “Heraclitian” philosophy of science, saying his work with case studies has shown scientific changes “are not Heraclitian, but orderly and strongly based on evidence....Scientific investigation...does not take place in an epistemic vacuum, a Heraclitian morass.” He also takes pains to point out, as Pitt himself surely also would, that “Pervasive change does not imply

If this is the case, philosophers are limited in their use of case studies essentially to a kind of philosophical antiquarianism, any lessons learned rigidly delineated within the specific past contexts to which they are native. We might gain keen understanding of isolated episodes, but for Pitt this is insufficient. He complains that this work of “doing history in context limits the possible range of philosophical ideas and explanations....A serviceable universal account of scientific observation is [therefore] not possible” (Pitt 2001, p. 374). For these reasons, Pitt sees case studies as being useful as heuristic devices at best.

However, while one can certainly see the need for caution when trying to apply lessons gleaned from extremely dissimilar temporal situations, it simply does not follow that *all* lessons from the past can hold *nothing* for the present. Indeed, Pitt’s own definition of irrationality, namely not learning from previous experience, has embedded within it the notion of looking to the past to enlighten present understanding.⁷ It thus seems a stretch to disqualify attempts that use particular slices of the past (i.e., historical case studies) to learn aspects of how science works.⁸

radical relativism or loss of contact between science and an external world” (Burian 2001, p. 399). Further, according to some readings of Heraclitus, the Doctrine of Flux should not be taken to imply “*everything* is changing, but that the fact that *some* things change makes possible the continued existence of other things.” That is, “Heraclitus does not hold Universal Flux, but recognizes a lawlike flux of elements....” (Graham 2002).

⁷ See (Pitt 2000, p. 22) for a fuller discussion of this.

⁸ Further, if one takes this argument to its logical extreme, one finds another reason to object to the line Pitt wishes to draw between present understanding and past contexts. The “present” is by definition an infinitesimally small, fleeting moment in time—a fiction, really. *All* is past. No sooner is a thought had or does an action occur than does it belong to history. All human endeavors, from the most abstract to the most mundane, thus rely on “the past” in a very real way—including the search for philosophical universals. Thomas Nickles agrees, writing, “It is trivial but nonetheless important to note that all inquiry occurs in the present, the ‘now,’ of the investigators and aims at solving problems and satisfying present

Indeed, Richard Burian, in a rejoinder to Pitt, argues that case studies are not only useful but are uniquely so, as they, “properly deployed, illustrate styles of scientific work...that are not well handled by currently standard philosophical analyses.” As such, they “ought to play a greater role in philosophy of science than the mainly heuristic one to which [Pitt] relegates them” (Burian 2001, p. 384). For Burian, case study methodology is appropriate in that it is the means by which one can “work up from an appreciation of scientific work in its context.” This “bottom-up” approach is in contrast to the “top-down” philosophy of science of Carnap et al., a deriving of “norms for science or standards of scientific knowledge from strictly philosophical considerations”—an approach now considered discredited by most in the post-Kuhn era (Burian 2001, p. 386).⁹

Historical case studies are, of course, interesting in and of themselves and can illuminate our knowledge of an extended episode in science history, or a “problematic,” to use Pitt’s term. But further, it seems reasonable to believe that enough individual case studies can, *pace* Pitt, “build up” a reasonable picture of an episode of a community’s change of theory, or of the status of a philosophical claim about theory change. This is

interests of the agents...Given that most all of the cultural resources available...are bequeathed from the past, these resources are bound to be whiggishly redeployed...In this sense presentism plus historical change entails a multi-pass account of inquiry. Inquiry has a history” (Nickles 1997, p. 38). This is true of scientists as well, Nickles writes: “Whiggism is the necessary cost of transforming hindsight into knowledge useful at the research frontier....Working scientists, as such, cannot avoid being whigs, nor should they” (Nickles 1997, p. 36).

⁹ One of the many reasons for the abandonment of the logical-positivist program was the eventual realization that because it was “top-down” and derived from “strictly philosophical considerations” that it was not at all clear that the work and conclusions of Carnap et al. had anything to do with reality whatsoever. That is, the logical positivists’ conclusions were really normative of how science *should* operate, rather than *descriptive* of how it really worked.

not unlike the way in which individual scientific data points can build up a picture of a natural phenomenon and lead to more abstract theoretical discoveries. Just as in science, it is of course correct to urge caution when attempting to extrapolate conclusions from a small number of data points. But with enough data—and caveats—it seems reasonable to assume that at least some conclusions might be drawn. To put it another way, Pitt seems to conflate the possibility of misusing case studies to produce *bad* STS with the impossibility of using them to produce *good* STS. As Burian says, “case studies need not be philosophically innocent and need not proceed to grand conclusions by induction from absurdly small samples” (Burian 2001, p. 388).

Regarding the other horn of Pitt’s dilemma, however, Burian is careful to distinguish his carefully circumscribed “bottom-up” philosophy of science from the Hull program of using case studies “in hypothetico-deductive style as a test of (universal) philosophical theses” (Burian 2001, p. 385). This he rules out, along with using cases even to illustrate or support a “general philosophical or methodological claim about science,” because in doing so “our sampling procedure and interpretation of the case will be, indeed, must be, systematically biased” (Burian 2001, p. 385).

Further, since, in Burian’s view “there is no such a thing as...*the* scientific method, or *the* epistemology or metaphysics of science”—that is, since “science is not one thing”—testing of any abstract claims regarding the workings of “science” is pointless at best and misleading at worst (Burian 2001, p. 385, 387). Burian calls instead for case studies to be used to obtain a more “regional” understanding of science’s

operation rather than to attempt a universal one: “We must work in, and study, particular contexts and do our best to find valid, but limited, generalizations” (Burian 2001, p. 399).

Burian’s doubts about the viability of an eternal, unchanging “scientific method” or similar principles that apply to all science at all times are certainly legitimate; indeed they are not only shared by many (if not most) STS commentators but by many scientists as well. P. W. Bridgman, for instance, famously wrote in his *Reflections of a Physicist*:

No working scientist, when he plans an experiment in the laboratory, asks himself whether he is being properly scientific, nor is he interested in whatever method he may be using *as method*.... What appears to him as the essence of the situation is that he is not consciously following any prescribed course of action, but feels complete freedom to utilize any method or device whatever which in the particular situation before him seems likely to yield the correct answer. In his attack on his specific problem he suffers no inhibitions of precedent or authority, but is completely free to adopt any course that his ingenuity is capable of suggesting to him. No one standing on the outside can predict what the individual scientist will do or what method he will follow. In short, science is what scientists do, and there are as many scientific methods as there are individual scientists (Bridgman 1955, p. 12).

And Peter Medawar has similarly written:

Ask a scientist what he conceives the scientific method to be and he will adopt an expression that is at once solemn and shifty-eyed: solemn, because he feels he ought to declare an opinion; shifty-eyed, because he is wondering how to conceal the fact that he has no opinion to declare (Medawar 1984, p. 80).¹⁰

However, Burian’s stance seems to overlook the fact that, these convictions aside, many STS commentators—and scientists themselves—can and frequently do assert just such generalities about science. The Hull program simply suggests that such claims can be evaluated against the evidence, and case studies are the mechanism by which to do so. Burian himself states that “in favorable cases hard-won experimental findings can be used to adjudicate scientific disputes;” it seems not too great of a leap to suggest a similar

¹⁰ As another example, the cosmologist William Hunter McCrea once criticized Herbert Dingle for his “obsession over something called the ‘scientific method.’ And there’s no such thing” (McCrea 1990).

adjudication is possible in STS disputes as well (Burian 2001, p. 399). There is no reason that case studies should not be used *both* to glean Burian's carefully limited, contextualized understandings of scientific episodes, as well as to evaluate theses claimed about science by STS practitioners. In a largely negative review of (Donovan, Laudan et al. 1988), Colin Howson grudgingly admits this very thing. He says that the "great majority" of case studies in the volume

Yield reliable and informative accounts of the historical episodes of which they are concerned, and do in fact manage to muster convincing evidence against many of the theses produced at one time or another by members of the 'historical' school (Howson 1990, p. 177).

Pitt's view that no philosophical work is achieved from studying contextualized cases recalls the similar attitude of generations of Aristotelians,¹¹ who felt the study of so-called "particulars" in nature was invalid for drawing general conclusions about the world. Aristotelians relied almost exclusively on what they considered to be the more rigorous deductive method of the syllogism instead, a less-than-productive attitude that was overcome fully only with the Scientific Revolution.¹² On the other hand, Burian's admonition against any form of "top-down" philosophy of science harkens back, arguably, to certain aspects of science in 19th Century America, when Baconian empiricism dominated the scene. Scientists then embraced data collection but eschewed the postulation of abstract theories or generalizations regarding said data; to them, an

¹¹ As is often the case, things referred to eponymously (e.g. "Aristotelianism") as commonly understood are often different from what the originator actually intended. One must differentiate the attitude of later Aristotelians (particularly Scholastics in the Middle Ages) vis-à-vis inference from observation, with that of Aristotle himself, who famously broke with his mentor Plato and advocated observation of the world to lead one to conclusions about its workings.

¹² Hume's problem also comes to mind.

“adequate table was the essence of science” and botanical taxonomy was its epitome (Daniels 1968, p. 86, 89).¹³ This anti-theoretical attitude, however, in the end proved a drag on American scientific advancement, which was overcome (in America) fully only in the 20th century.

The historical lesson one might draw from the Aristotelians and then the Baconians’ overreaction to them is that *both* “top-down” and “bottom up” methods are needed for science to advance. That is, science operates with elements of both induction *and* deduction. Is it not unreasonable to posit that STS might operate similarly?

Let us suppose that we establish some Burianesque, regional understandings of an episode in the history of physical science, say, the development of radio astronomy. For instance, one of the conclusions that David Edge and Michael Mulkey arrive at in their book *Astronomy Transformed* is that the major innovations in that infant field came not from (optical) astronomers themselves, but from the “margins” of the field—“migrants” from areas like wartime radar research. Michel Morange in *A History of Molecular Biology* notes something similar about the birth of molecular biology, namely that while genetics and biochemistry were the “ancestors” of the field, an influx of physicists into the field proved decisive—as did the geographic migration of scientists from Europe to America. Martin Harwit in *Cosmic Discovery* also documents that frequently discoveries in astronomy come from people trained in other areas, “marginal workers” who become

¹³ Again, one must be careful with eponymy here, this time with so-called “Baconian empiricism.” As George H. Daniels puts it, “Where Bacon had intended classification to be the beginning of science, the naïve realism of Scottish philosophy could be used to make it appear that this was the whole of science” (Daniels 1968, p. 82).

central to the discipline. A good example of this might be the fact that very few of the premier cosmologists of the last century actually trained as astronomers (Edge and Mulkay 1976; Harwit 1984; Morange 2000).

Surely, it would be reckless to make on the basis of the above the universal claim: “It is impossible for a new field to develop without an influx of ‘outsiders.’” But it is the position of this paper that, *pace* Burian, it is not unreasonable to posit based on the above the more nuanced claim that “Outsiders often play a critical role in the development of emerging disciplines.” If this claim is indeed ill-conceived, fair enough—this will surely be uncovered by the investigation of further case studies on embryonic disciplines,¹⁴ and STS will be advanced by its refutation or revision.¹⁵ And from the new case studies spurred by the thesis, will we not have learned something positive about all the disciplines in question along the way?¹⁶

Drawing on the work of N.R. Hanson, Lindley Darden suggests that scientists have a critical use for the knowledge of the history of their disciplines. Scientists “find types of hypotheses proposed in the past, and analyze the nature of the puzzling

¹⁴ One such study is, in fact, (Gieryn and Hirsh 1983).

¹⁵ One is put in mind of Popper’s assertion that every falsified scientific theory should be celebrated as having advanced science.

¹⁶ I have to note that this lesson *vis-à-vis* science’s both involving elements of induction and deduction has been forgotten by scientists themselves on more than one occasion. For instance, in the first half of the twentieth century, a vitriolic debate erupted in the new science of cosmology over the kinematic relativity model of E.A. Milne. Milne had started from first principles and produced an entirely deductive model of the universe; many scientists such as Herbert Dingle saw this methodology as entirely illegitimate—a return to Aristotelianism and a betrayal of all the gains of the Scientific Revolution. See (Hunt 1996) for details. Kuhn suggests that such arguments over methodology and fundamentals are part-and-parcel of the immature phase of a science’s development; could Pitt and Burian’s debate on methodology signal something similar for STS?

phenomena to which the type applied,” she suggests. They then “use this ‘compiled hindsight’ in future instances of theory construction” (Darden 1987, p. 36). Darden continues:

Preserving such hindsight becomes particularly important when theories are counterintuitive, and scientists have produced insights that serve to correct naïve, commonsense views....In considering the hindsight provided by these historical cases, consideration of the incorrect theories, as well as the correct ones, has proved useful. Scientists often consider disproved scientific theories in a scientific graveyard, not worth a second glance by those researchers pushing ahead at the forefront. However, in these cases, incorrect but plausible, as well as confirmed, theories proved worth considering. They provide possible theory types, and also hindsight about “appropriate conditions” for instantiating.... (Darden 1987, p. 39-40).¹⁷

James McAllister’s model of scientific practice (the “aesthetic induction”) also relies upon scientists’ knowledge of past successes.

Such drawing on past cases in the practice of science has also been commented upon by Thomas Nickles. “It is hindsight that enables research at the frontier to proceed so rapidly and efficiently, in some cases, that the researchers appear to possess foresight,” he writes (Nickles 1997, p. 33). Nickles links the utility of such hindsight to what he calls a “multi-pass” inquiry:

Knowledge of cases enables experts to anticipate errors....It is precisely multi-pass inquiry that enables us to generate hindsight in such an efficient and useful manner....Thanks to multi-pass inquiry, we can have our cake and eat it too. We can return to previous problems and solutions, revisit previous applications, and provide new accounts of them.... (Nickles 1997, p. 35).

Nickles continues: “The prevailing view seems to be that historical cases either have a purely illustrative value or else they must be used, in a theory-centric manner, as

¹⁷ Darden goes on to suggest that scientists should “Study the history of science to find recurring problem types and theory types, devise computationally useful abstractions for them, and build AI systems to use such hindsight in new problem situations” (Darden 1987, p. 40).

evidence (empirical data) for or against some methodological rule.” Nickles rejects this, saying that

Research in psychology and artificial intelligence (AI) increasingly suggests that a good deal of intelligent behavior is not, or need not be, guided by rules...Particular judgments about new cases are made on the basis of past cases, not rules (Nickles 1996, p. 38,39).

That is, scientists as a matter of routine use knowledge of past cases as inspiration, or fodder, for new theorizing. As the historian Daniel Boorstin has said, “Trying to plan for the future without knowing the past is like trying to plant cut flowers” (Boorstin 2000).

Again, I suggest that just as knowledge of historical cases is a non-trivial part of the progress of science, similarly, knowledge of historical cases, properly situated, can similarly inform STS scholarship. My study will be far from an out-of-context “snapshot” (the issue that concerns Pitt), but will rather follow the development of Sciamia’s thought on the issues he faced over an extended period; thus it will qualify as a “problematic” in Pitt’s sense and will achieve not only historical but also philosophical results.

To wit, my analysis of the Sciamia case will, among other things, problematize the notion of scientific work as rigid and algorithmic, carried out in an almost robotic fashion by workers slavishly chained to a predetermined “method.” However, it will also equally problematize the radical relativism found at the other end of the STS spectrum, *viz.* that since scientific theories are “underdetermined,” that scientists can (and do) simply “make it up as they go along,” and that the product they produce is therefore nothing more than a “social construction” or “negotiation” having no (necessary) relation to the real world.

My audience is thus four-fold: the public, who generally speaking hold the former opinion in the above paragraph; science critics on the radical left, many of whom hold the latter view; the broad mainstream of STS practitioners, who tend to occupy a middle ground between the two; and finally scientists themselves, who most certainly do not hold the latter view, but who also would (I am certain) see the former view of their work as oversimplified. Thus they, like the STS mainstream, are somewhere in that middle ground. What I will demonstrate to this audience, humbly (I hope), is that there are at least some instances in which STS analyses of past scientific episodes can indeed inform positively the assumptions of and direction of ongoing research.

Outline of the Project

I will begin the main part of this project in Chapter 2, by laying out the background of the scientist in question, Dennis W. Sciama. The focus will be his allegiance to the steady state model of the universe, which flourished from 1948-1966. I will go into some detail as to the tenets of the model, its successes, and its failures. I will pay particular attention to Sciama's reactions to the increasingly hostile data that came in contradicting the model as the years went by.

Once Sciama's scientific journey is explicated, I will then look at it in the next few chapters through four different STS prisms. In Chapter 3, I will consider the role of aesthetic factors in theory choice, which were certainly significant in the Sciama case. I will pay particular attention to the work on aesthetics of James W. McAllister of the

University of Leiden. In Chapter 4, I will look at “Planck’s Principle,” noting that the Sciama case not only seems to refute this oft-quoted dictum, but also that the Sciama case closely tracks the conversion experience of James Jeans in his conversion from classical radiation theory to the quantum theory of light. In Chapter 5, I will consider two models of theory development: Lindley Darden’s “anomaly-driven theory redesign” and Thomas Nickles’ “heuristic appraisal”; the Sciama case provides useful insights on both. Finally, spinning out of Chapter 5, I will take in Chapter 6 a brief foray into well-trod philosophical territory to consider the notion of ad hoc hypotheses and how they relate to the Sciama case.

CHAPTER 2

SCIAMA AND THE STEADY STATE

Imagination will often carry us to worlds that never were. But without it we go nowhere.

—Carl Sagan, *Cosmos*

Sciama's Background

Dennis William Sciama was born November 18, 1926 in Manchester, England. Sciama's mother was an immigrant from Cairo, Egypt; his father, a clothing merchant, was a first-generation Briton whose father emigrated from Aleppo, Syria. The Sciama surname, in fact, was originally spelled "Shama," ("he who watches") but was "Europeanized" upon the family's immigration from Syria (Pagan 2000).

The Sciamas were non-practicing Jews; Sciama's father was an atheist, and Dennis himself was either a lifelong atheist or stopped believing in God at a very early age (Sciama 1978, p. 1, 4). Reacting once to a lecture by a Jesuit that followed his own talk at a conference, Sciama stated,

I'm afraid...the word 'God' is just a word. When this Jesuit spoke after me, he knew so much about God. It was amazing. God was a person, he said...not just that God was some force that made the world, it was a person. How can he possibly know such things? It's ridiculous. If you had a concept of something that made the world, and it was needed in order that the world be made, then who made that person or thing or whatever it was, and so on...It's true that people have, internally, a religious feeling, which they use the word God to express, but how can a feeling inside of you tell you that a thing made the whole universe? There is no relation between the two matters of

concern. Therefore, while I'm prepared for and I can't rule out that there is another order of structure than ordinary matter, I know nothing about that order. There could be many orders. The word God just doesn't denote any structure (Lightman and Brawer 1990, p. 153).

The Sciama household did not have a very academic atmosphere, but despite this young Dennis became a voracious reader, and by age twelve discovered he possessed an aptitude for mathematics. He was educated at a prep school in Manchester, and then went on to Malvern College, an English public school in Worcestershire.¹⁸ Sciama's mathematical prowess was fostered at Malvern, which boasted excellent instruction in mathematics and physics from Cambridge alumni, including one from Trinity College (Sciama 1978, p. 1-3).

Over the objections of his father, who had wanted Dennis to follow him into the family business, Sciama by the age of 15 or 16 decided to pursue a career in mathematics, finding topics such as projective geometry "beautiful," and having been inspired by the writings of Arthur Eddington, James Jeans, G.H. Hardy, and Bertrand Russell; Sciama found Hardy's *A Mathematician's Apology* particularly thrilling.¹⁹ With the help of his aforementioned Cambridge-educated mathematics teacher R.H. Cobb, Sciama was awarded a minor scholarship to Trinity College, Cambridge's top mathematics college, and started there in 1944. His choice of Trinity was at least in part due to the influence of his Trinity-educated physics teacher at Malvern, but also to his

¹⁸ I.e., Malvern was a private school in the American sense. Sciama rates it as "reasonably well known, but not one of the greats, not an Eton or Harrow or a Winchester, but very good at a lower level" (Sciama 1978, p. 1).

¹⁹ Hardy did not believe in God either. "With him, this was a black-and-white decision, as sharp and clear as all other concepts in his mind" (quoted in Snow 1967, p. 20). However, from Sciama's statements, it seems likely that Sciama had settled on his own atheism before learning of Hardy's.

admiration of such Trinity Fellows as Hardy and Russell (Sciama 1978, p. 2; Lightman and Brawer 1990, p. 137-9).²⁰

During this time war raged in Europe. Sciama had been deferred from military service with the understanding that he would study physics rather than mathematics, as training in the latter subject was considered by the British government to be “less effective” for practical purposes. Thus, after a year of studying mathematics, Sciama segued into physics, doing the Natural Sciences Tripos. Though he did not realize it at the time, in later years Sciama considered his diversion into physics to be a fortunate turn of events, considering his talents to be “more along the physics line than the mathematics line” (Sciama 1978, p. 1-2).

Sciama developed a real passion for “understanding fundamental physics and astronomy,” but did not consider this something he had learned at Malvern or Trinity, but rather was something “very deep in him,” an inherent interest that his education simply helped bring to the fore. The gradual awakening of this passion strengthened Sciama’s resolve to make a career in science and research, rather than returning to the family cotton trade (Sciama 1978, p. 3).

Sciama “had always had a mild interest in philosophy,” which flowered at Cambridge. He, for instance, attended an intimate course of lectures given by Ludwig Wittgenstein while in his first year as a student at Trinity, and considered the philosopher

²⁰ Interestingly, Hardy himself was also influenced by a childhood book, *A Fellow of Trinity*, to choose that same college. He writes, “From that time, until I obtained one, mathematics meant to me primarily a Fellowship of Trinity” (quoted in Snow 1967, p. 19).

to be a “very remarkable man.”²¹ Sciama also made Trinity’s football team, which he attributed to the college’s being smaller than normal due to the war (Sciama 1978, p. 4-5; Lightman and Brawer 1990, p. 138).

Sciama received his B.A. in 1947; however, his exam results (a lower second in finals and two thirds in earlier exams) were a “disgrace,” forcing him to leave Cambridge for a two-year stint in the army, as conscription was still in effect (Lightman and Brawer 1990, p. 140; Ellis 1993, p. 2). About a quarter of the way through his tour of duty, through the influence of Cambridge Professor Douglas Rayner Hartree, Sciama was transferred to a government laboratory, the Telecommunications Research Establishment (TRE), which was known for, among other things, research on radar. There, Sciama worked on solid state physics, particularly the “quantum mechanics and group theory” of photoconductive materials such as lead sulfide, “which were of interest for the purpose of detecting enemy airplanes” (Sciama 1978, p. 6).²²

It was on the basis of the internal reports on these subjects that Hartree accepted Sciama back at Cambridge as an unfunded research student in 1949, after the latter’s military tour had ended; Sciama relied largely upon his father for financial support during this time. Upon his return to Cambridge, Sciama was given a desk in an office “in the main old part of the Cavendish” belonging to the Mond Laboratory, and commenced

²¹ One of the first overseas conferences Sciama attended, for instance, was the first annual New York University Institute of Philosophy, February 9-10, 1957, for which Sciama gave a paper titled “Determinism and the Cosmos” (Sciama 1958).

²² According to G. Ellis, Sciama’s time at TRE was “during the war” (Ellis 1993, p. 2) but by Sciama’s own account (Sciama 1978, p. 6) this work does not start until 1947.

research. He worked initially on statistical mechanics under H.N.V. Temperley, having become interested in cooperative phenomena such as Ising models²³ while at TRE.

Sciama took his M.A. in that same year (Sciama 1978, p. 6-7; Lightman and Brawer 1990, p. 140; Ellis 1993, p. 2).

After a year to eighteen months of studying cooperative phenomena, Sciama lost interest in that topic. His interests turned instead to the likes of relativity and cosmology, due to the “extremes of possibility” therein; he felt that statistical mechanics could not compete with cosmology’s “connotations of understanding the origin of the universe” (Sciama 1978, p. 7; Sciama 1990). Sciama realized his

real passion was for understanding the fundamental nature of the universe....to understand the way the world is made, where it comes from, and what it means in the scientific sense....to work...on problems that in some way help to understand the great questions” (Lightman and Brawer 1990, p. 139).

It is easy to speculate that his interest in philosophy, cultivated at Cambridge, had something to do with this switch as well.

Though he had had no “special contact” with astronomy and had been essentially self-taught in relativity as an undergraduate, Sciama became interested in Mach’s Principle, which, loosely speaking, is the idea that the inertia of bodies is determined by the configuration of all the distant masses in the universe relative to them; Sciama had in fact initially been skeptical of the idea, writing an unpublished paper attacking it before he “came around.” Sciama went so far as to correspond with Einstein on Mach’s Principle, and even had a lengthy interview with him in 1955 on the topic the week

²³ First proposed in 1924 by Ernst Ising, these are models of systems in which each element tries to imitate the behavior of other nearby elements, e.g. ferromagnets.

before Einstein died.²⁴ Sciama's interest in Mach's Principle was in all likelihood picked up from Hermann Bondi, whose course on cosmology Sciama took at some point while a research student, and who encouraged this line of thought in him (Sciama 1978, p. 5-8; Lightman and Brawer 1990, p. 143; Sciama 1990). Another powerful influence on Sciama during this period vis-à-vis Mach's Principle was Thomas Gold, who was also "very interested" in the topic, and with whom Sciama built a strong friendship (Sciama 1978, p. 10).²⁵

Noticing Sciama's change of interests, the powers-that-be at Trinity changed his research supervisor from Temperley to P.A.M. Dirac, who was also "fascinated" with Mach's Principle (Ellis 2000, p. 722).²⁶ Sciama had taken a course from Dirac in his third year as an undergraduate and had been very impressed, but by this time Sciama had essentially already worked out all his ideas on Mach's Principle and as such Dirac's influence on Sciama's subsequent thesis on the topic was minimal (Sciama 1978, p. 5,7; Sciama 1990).

On the strength of his work on Mach's Principle, Sciama obtained a Junior Research Fellowship at Trinity in 1952; this was a "defining moment" in Sciama's career. It was only due to this fellowship that Sciama evaded being drawn back into the

²⁴ Einstein had coined the very term "Mach's Principle" in 1918, but had since soured on the idea. Sciama began his interview by announcing, "Professor Einstein, I've come to talk to you about Mach's Principle, and I've come to defend your former self against your latter self," to which Einstein laughed heartily, saying, "Ho, ho, ho, that is *gut, ja!*" (Sciama 1978, p. 30-1).

²⁵ Mach's Principle would play a significant influence on the development of Bondi and Gold's steady state model of the universe, as will be described presently.

²⁶ This made Sciama "almost the only research student supervised by Dirac," a fact of which Sciama was "proud" (Rees 2000, p. 3.37).

family business in Manchester; he had made a “business arrangement” with his father that this is what he would do were he not awarded a fellowship. Sciama ultimately took his Ph.D. in 1953 (Sciama 1978, p. 8,13; Lightman and Brawer 1990, p. 140; Rees 2000, p. 3.37).²⁷

Sciama took two years off from his four-year fellowship to spend 1954-55 at the Institute for Advanced Study at Princeton, where he met Einstein; in his later years Sciama would tell the story of his encounter with the great man with gusto, even imitating Einstein’s German accent. Sciama then spent 1955-6 as an Agassiz Fellow at Harvard, returning to Trinity for the final two years of his fellowship, 1956-1958. Sciama was then affiliated with King’s College, London, from 1958-1960, where he worked in a “lively” research group “spearheading advances in general relativity theory;” this group included Bondi and Felix Pirani (Pagan 2000; Rees 2000, p. 3.35).²⁸

During this period, in 1959, Sciama married Lidia Dina, a Venetian whom he had met at a party while visiting the Weizmann Institute in Jerusalem (Pagan 2000). Dina, who was also Jewish, had transferred from the University of Venice to Hebrew University to study English. Sciama was then invited by Thomas Gold to be Visiting Professor at Cornell University from 1960-1961, where Lidia did a Master’s degree in English. Sciama then returned to Cambridge as Lecturer in Applied Mathematics in 1961, serving also as a “non-teaching, non-stipendary” Fellow of Peterhouse from 1963-

²⁷ Lightman and Brawer date this as 1952, but Sciama himself dates this as 1953 (Sciama 1978, p. 8; Lightman and Brawer 1990, p. 136).

1970 (Sciama 1978, p. 22).²⁹ Martin Rees rates Sciama's group at Cambridge in the 1960's as "one of three in the world that transformed relativistic cosmology into one of the liveliest frontiers of science" (Rees 2000, p. 3.37).³⁰ During this time the Sciamas had two daughters, Susan (1962), now a painter; and Sonia (1964), now a psychologist (Pagan 2000). After their births Lidia then earned a doctorate in social anthropology from Cambridge.

Sciama then applied for a Senior Research Fellowship at All Souls College, Oxford, feeling his teaching and research supervisory load at Cambridge had become "very heavy," whereas at All Souls "there were no duties at all, you just do what you like." Though All Souls had no tradition in science, mathematics was one of the areas that qualified one for the fellowship, which Sciama won, moving to Oxford in 1970.³¹ There, he built up a research group in the Astrophysics Department of the kind he had had at Cambridge. During this time Lidia took a diploma from Oxford and then a B. Litt. (Sciama 1978, p. 21, 31; Ellis 1993, p. 2).

Sciama stayed at his All Souls position until 1985, except for 1977-78 when he was Luce Professor of Cosmology at Mt. Holyoke College in South Hadley,

²⁸ The specifics of research program Sciama undertook in the 1950's and 1960's will be discussed *in extenso*.

²⁹ Sciama also ca. 1965 briefly visited the University of Maryland, College Park, under a NASA grant (Haddock and Sciama 1965, p. 1007; Rees and Sciama 1965a, p. 740).

³⁰ The other two were Y.B. Zeldovich's in the USSR and John Archibald Wheeler's at Princeton (Rees 2000, p. 3.37).

³¹ Rees dates this as 1971 (Rees 2000, p. 3.37), but according to Sciama's own account, this occurred in 1970 (Sciama 1978, p. 31).

Massachusetts, where Lidia also taught for the year. Sciama was part-time Professor of Physics at the University of Texas at Austin from 1978-1982. He was first affiliated with the International School of Advanced Studies (*Scuola Internazionale Superiore di Studi Avanzati*, or SISSA) in Trieste in 1983, after being offered a professorship in physics and the leadership of a research group there. He left All Souls in 1985, remaining Director of the Astrophysics Sector at SISSA until 1999 (Ellis 1993, p. 2; Pagan 2000; Rees 2000, p. 3.37). During this period Sciama's research focused on dark matter; he formulated the so-called "Decaying Dark Matter" (DDM) hypothesis, which suggested that the universe's "missing matter"³² was composed of massive neutrinos which decayed after a long half-life, producing photons and causing other observable effects.

Sciama continued to work right up almost until his death in 1999, submitting two papers on dark matter and cosmic rays for publication in June and September of that year (Sciama 2000a; Sciama 2000b) and presenting them at a November 1999 meeting of the Royal Astronomical Society honoring Bondi (Rees 2001, p. 367). He had also recently completed a popular work in Italian (Sciama 1998). Sciama died in Oxford on December 18 of that year, of a tumor (Pagan 2000).

While his lifetime of cosmological and astrophysical research was significant—he is remembered as one of the "far-sighted physicists" involved in helping relativity and

³² Overwhelmingly most of the matter in the universe is so-called "dark matter," not emitting (much) light but evident from its gravitational effects; recent estimates from NASA's Wilkinson Microwave Anisotropy Probe put the percentage at about 85%. Pioneers in the study of this mysterious matter include Fritz Zwicky, who speculated in 1933 that such matter had to exist to hold the galaxy clusters together; J.P. Ostriker and J. Einasto, who suggested in 1973-4 a theoretical argument for dark matter within galaxies; and Vera Rubin, who in the 1970's and 1980's provided the first clear observational evidence for galactic dark matter. See (Trimble 1993) for a brief overview of the history of this topic.

cosmology make the transition from “only of philosophical interest” to “central strands in physics”—most of his colleagues consider his most lasting influence to be his “enormously influential school of students,” whom Sciama had attracted by his “charisma as a teacher” and “breadth of interests,” and to whom he was an inspiration and “unfailingly supportive” (Ellis 2000, p. 722; Rees 2001, p. 366).

Sciama dedicated himself to finding such “high-quality, dedicated students and helping them in their careers” (Ellis 2000, p. 722) and in them inculcated

the importance of physical and astrophysical understanding, the significance of rigorous mathematical analysis whenever this is possible, and the power of combining the two in a way leading to testable predictions. He encouraged them to be both adventurous and rigorous, and to make their thinking relevant to the physical problem at hand (Rees 2000, p. 3.37).

He “communicated to them a passion for physics and the understanding of the universe, and a feeling for what is important and what not: a vision of what can be done, based as far as possible on a solid mathematical foundation, to increase physical understanding of what is happening” (Ellis 1993, p. 4).

Sciama’s students included Brandon Carter (inventor of the Anthropic Principle), Stephen Hawking (pioneer of black hole thermodynamics), Martin Rees, George Ellis, John Barrow, James Binney, Philip Candelas, and David Deutsch (the inventor of quantum computing), among many others (Ellis 2000, p. 722; Rees 2000, p. 3.37). A “family tree” of well over 200 of Sciama’s students and “grand-students” was compiled for a meeting at SISSA in 1992 celebrating Sciama’s 65 birthday, the proceedings from which were collected into a *Festschrift* titled *The Renaissance of General Relativity and*

Cosmology: A Survey Meeting to Celebrate the 65th Birthday of Dennis Sciama (Ellis, Lanza et al. 1993).

Further, it was also Sciama's influence that caused Roger Penrose, originally an algebraic geometer, to take up relativity. Sciama, a friend of Penrose's brother, was so impressed with Penrose's questions regarding the steady state theory he took it upon himself to cultivate Penrose's interest in physics and tried to convey the excitement of doing that subject to him (Garcia-Prada 2000, p. 18). Relativity's "resurgence in the 1960s owed a great deal to the novel insights and techniques introduced" subsequently by Penrose, one of whose many accomplishments in the field was a 1965 theorem that proved collapsing stars of a certain mass or higher must necessarily end up as a singularity. Sciama called this result "a bombshell" (Lightman and Brawer 1990, p. 145). Hawking, in turn, for part of his thesis under Sciama adapted this result for cosmology, applying it to the case of the universe's singular beginning in the Big Bang (Sciama 1978, p. 28; Rees 2001, p. 366).

Through such colleagues and students, Sciama's influence "is widely pervasive in general relativity theory, astrophysics, and cosmology, and extends to quantum theory and string theory" (Ellis 1993, p. 4). Sciama, however, typically downplayed this profound effect he had on colleagues and students. "My role was to create the ambience at Cambridge that these problems were discussed," he said in a 1978 interview (Sciama 1978, p. 28) also saying on another occasion that:

I always feel I've been in a false position, particularly by being at Cambridge, and to some extent also at Oxford. We've had the best students in England. And so, if you have a very good student, you just sit back and let him go, and he does wonderful things. That's what's happened in quite a number of cases. My only role was enabling them to do relativity and cosmology. That required

a certain structure and someone who was willing to take them on, but then they did their own things....I'm the kind of person who suggests problems to people....I regard it as a matter of sheer luck that I've been associated with all these students (Lightman and Brawer 1990, p. 144-5).

Sciama was awarded many honors during his career, including (but not limited to) the Guthrie Prize of the British Institute of Physics, and being elected: Emeritus Fellow of All Souls, Oxford; Research Fellow of Churchill College, Cambridge; Fellow of the Royal Society; Fellow of the Royal Astronomical Society; Foreign Member of the American Philosophical Society; President of the International Society of General Relativity and Gravitation; and Foreign Member of the *Accademia dei Lincei* (Ellis 1993, p. 2-3).

Sciama is remembered for his “enthusiasm and personal warmth,” his “wide interests and engaging wit,” and for being “highly articulate” and a “lucid thinker with a synoptic vision, deep physical insight, and a ‘feel’ for what was important.” He was a “warm person with a love for the civilized things in life” such as “good company, music, and opera.” He is survived by his wife and daughters—as well as the above-mentioned 200-plus students and colleagues, to many of whom he was “like a father” (Lightman and Brawer 1990, p. 136; Ellis 2000, p. 3.37; Rees 2001, p. 367).

Steady State – An Overview

By the time Sciama returned from the army to university in 1949, the steady state model of the universe had burst onto the cosmological scene, the brainchild of three Cambridge professors—Bondi, Gold, and Fred Hoyle, who had met during World War II

while working on naval radar, and who discussed astronomy and other topics together “after hours” (Sciama 1978, p. 19; Bondi 1993, p. 476; Bondi 1998, p. 1). To understand the level of Sciama’s dedication to the steady state model, it is necessary to provide some background on this theory, as well as its rival, the “big bang” model.

Though the term “big bang” was not coined until 1949 by Hoyle,³³ the concept itself dates back to Georges Lemaître, who in 1931 suggested the physical concept that there was an early, dense state from which the universe is expanding.³⁴ By the end of the 1940’s, and largely due to the work of George Gamow, this notion had evolved into what became known as the “hot” big bang model—the idea being that the sudden, early expansion of this “primeval atom” was accompanied by very high temperatures.³⁵ One of the most significant milestones in this development came in 1948 when Gamow and his student Ralph Alpher published a paper in which the hot, dense conditions of the early universe were postulated to be responsible for the origin of the elements.³⁶

³³ Hoyle, being an opponent of the model, introduced the term derisively during one of a series of BBC radio programs on cosmology, which he compiled into a book called *The Nature of the Universe*, published the following year. However, A.G. Walker recalls that (Petr?) Kapitzka “says he was the one who first used” that term (Walker 1990).

³⁴ “If we go back in the course of time we must find fewer and fewer quanta, until we find the all the energy of the universe packed in a few or even a unique quantum” (Lemaître 1931). Alexandr Friedmann had earlier introduced a model of a universe expanding outward from a singularity in 1922, but he and his contemporaries considered it not a physical model but rather a “mathematical curiosity” (Kragh 1993a, p. 31). Lemaître himself had four years earlier proposed an expanding universe model, but “had not identified the start of the expansion with the beginning of the universe,” reversing himself publicly in his 1931 paper. Note that Lemaître does not seem to be referring to a true singularity here, but rather a “unique atom whose atomic weight was the total mass of the universe” (Smith 1993, p. 365).

³⁵ The specifics of this evolution are documented in (Kragh 1996), Chapter 3.

³⁶ Gamow as a joke listed Hans Bethe as an author on this paper as well—even though the latter was not involved with the writing in any way—simply due to the phonetic similarity of Bethe’s surname to the Greek letter beta. Thus along with Alpher (alpha) and Gamow (gamma), the paper and the model it

However, a puzzle regarding this model had emerged, if one considered the “explosion” of the primeval atom to mark the universe’s beginning. By measuring the velocities of the receding galaxy clusters (calculated from their observed redshifts, first measured by V.M. Slipher in 1912) and the distances to them (using the period-luminosity relationship of the Cepheid variables discovered by Henrietta Swan Leavitt, also in 1912), one could determine the so-called “Hubble constant,” the reciprocal of which enabled one to estimate the time since the universe’s expansion began, or its maximum age. However, the measured value of the Hubble constant in 1948 (about 530 *km/s/Mpc*) was such that it suggested an age of the universe (about 1.8×10^9 years) that was less than the age of the earth calculated from the decay of radioactive materials in the earth's crust (about 3×10^9 years)—an apparently nonsensical result. This “age of the universe” anomaly or “time-scale problem” had persisted for years, but by the late forties was becoming critical. William Hunter McCrea explained the community’s sudden concern over the topic this way:

Until recently, the view was that all the stages in the evolution of the Galaxy, before that when the Earth was formed, could have been rushed through in much less than 10^9 years in the highly congested state in which the universe was inferred to be at the epochs concerned. So it was not found strange that the age of the Earth and that of the whole universe should be the same to a first approximation. Indeed, this and certain somewhat similar results were taken as generally favourable to the inferred age of the universe (McCrea 1950, p. 5).

However McCrea then added that the above interpretation had changed due to recent ideas concerning stellar evolution” and that the community was

described became known simply as “ α - β - γ .” The details of this famous episode have been related many times; see, for instance (Kragh 1996, p. 113).

reverting...to the common-sense view that the Earth is considerably younger than a good many stars....So we have to conclude that the ordinary theory of the expanding universe fails to provide sufficient time for the evolution of its contents (McCrea 1950, p. 5).

It was into this atmosphere that the steady state model was introduced in 1948.

After the war, Bondi, Gold, and Hoyle “returned one by one to Cambridge University” and continued their “habit of discussing scientific questions morning to night,” including the age of the universe anomaly, which occupied them “a great deal.” It was Gold who proposed the germ of the steady state idea, with Bondi and Hoyle initially skeptical, but eventually warming to it (Bondi 1993, p. 476). With the encouragement of McCrea, then the Secretary of the Royal Astronomical Society and an early steady state sympathizer, Bondi discussed the new theory with “some of the world’s leading astronomers” at the Assembly of the International Astronomical Union in Zurich in August 1948. The full trio did the same at the Edinburgh meeting of the Royal Astronomical Society in November 1948.

Hoyle’s preferred approach to the model was a field-theoretic one, whereas the Bondi and Gold approach was of a philosophical or phenomenological nature. Hence the steady state model of the universe was presented in two separate papers were published late that year in the same volume of the *Monthly Notices of the Royal Astronomical Society* (Bondi and Gold 1948; Hoyle 1948). The following passages from the latter paper show that despite the role of the time-scale problem in the genesis of the steady state model, one of the main reasons for the theory’s appeal was epistemological, based essentially on an *a priori* axiom.

Their argument goes as follows: if the cosmos had been substantially different in the past (i.e., much denser, as contended by defenders of the big bang model), how would one know if the physical laws of such a universe were the same as those now? Not only was there no guarantee that this was true, there was no way even to figure out what the earlier laws were. That the laws of the universe of the present were “intrinsic” to our universe, and therefore were also the ones that were operative in the past and will be operative in the future, was at bottom, Bondi and Gold argued, simply an assumption.

Bondi and Gold considered this claim unwarranted, writing:

Such a philosophy may be intellectually very agreeable.... There are, however, grave difficulties with such a view.... If as is now widely agreed, we adopt Mach's principle, then we imply that the nature of any local dynamical experiment is fundamentally affected by distant matter....Any interdependence of physical laws and large-scale structure of the universe might lead to a fundamental difficulty in interpreting observations of light emitted by distant objects. For if the universe, as seen from those objects, presented a different appearance, then we should not be justified in assuming familiar processes to be responsible for the emission of the light which we analyse (Bondi and Gold 1948, p. 252).

“This difficulty is partly removed by the ‘cosmological principle,’” they continued, which says that the universe looks basically the same to all observers from all spatial vantage points in it.³⁷ “The observations of distant nebulae have contributed much evidence” in favor of the cosmological principle, Bondi and Gold said, but that

we might have looked to the cosmological principle for a justification of the assumption of the general validity of physical laws; but whilst the principle supplies the justification with respect to changes of place, it still leaves the possibility of a change of physical laws with universal time....Indeed, we are not even in a position to interpret observations of very distant objects without such an assumption for the light which we receive from them

³⁷ This principle was enunciated as such in 1933 by Milne; Milne however gave the credit for the modern concept to Einstein, as the assumption (of the universe’s large-scale isotropy and homogeneity) was necessary for the latter’s 1917 cosmological model. Neither man, however, is responsible for the specific term itself; Milne wrote in 1952, “My friend Erwin Finlay Freundlich coined the term ‘cosmological principle’” (Milne 1952, p. 68). Milne, in a spasm of precursoritis, also pointed out that Giordano Bruno espoused the cosmological principle as well, without calling it such (Milne 1952, p. 71-2).

was emitted at a different instant in this scale of universal time, and accordingly the processes responsible for its emission may be unfamiliar to us.

This situation, to Bondi and Gold, was untenable because

the unrestricted repeatability of all experiments is the fundamental axiom of physical science. This implies that the outcome of an experiment is not affected by the position and time at which it is carried out. A system of cosmology must be principally concerned with this fundamental assumption and, in turn, a suitable cosmology is required for its justification (Bondi and Gold 1948, p. 252).

This led Bondi and Gold to propose a temporal extension of the cosmological principle: the *perfect cosmological principle* (PCP), which said that the universe looked basically the same to all observers in time, as well as space. That is, though things may change and evolve on a local level, on the whole, the universe is unchanging in time, or “self-perpetuating”—that is, in a steady state. They continued:

We regard the reasons for pursuing this possibility as very compelling, for it is only in such a universe that there is any basis for the assumption that the laws of physics are constant; and without such an assumption, our knowledge, derived virtually at one instant of time, must be quite inadequate for an interpretation of the universe and the dependence of its laws on its structure, and hence inadequate for any extrapolation into the future or past (Bondi and Gold 1948, p. 254).

The perfect cosmological principle became the lynchpin of the steady state model; however, because of this, the observed recession of the galaxies presented a problem. According to the PCP, the universe is infinitely old. The density of a universe that had been expanding for an infinite amount of time would necessarily approach zero, contradicting the PCP—not to mention everyday observation. To escape this dilemma, Bondi, Gold, and Hoyle proposed that matter was being *continually created* uniformly throughout space, out of nothingness. Only in this way could the density of the universe always be kept constant (as required by the PCP), the amount of matter produced by the constant creation just balancing the thinning out of matter by the expansion. Thus,

Bondi, Gold, and Hoyle gave up the conservation of mass,³⁸ a concept at the cornerstone of physics.

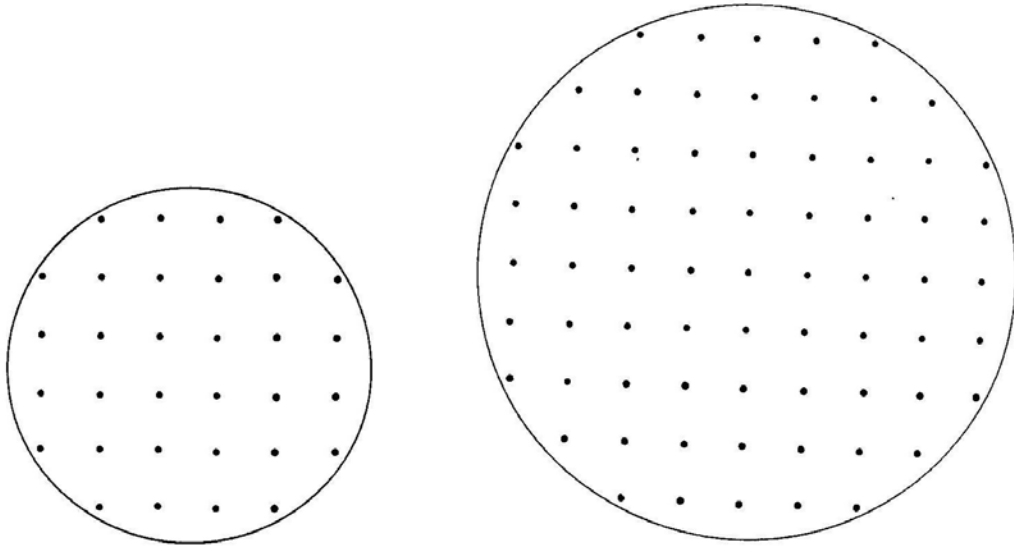


Figure 1: The Continual Creation of Matter. Hydrogen atoms created *ex nihilo* keep the universe's density constant as it expands, in accord with the perfect cosmological principle. From (Hoyle 1962).

The trio was quick to point out, however, that this notion, however dramatic, did *not* contradict experiment; the conservation of mass was, after all, an idealized principle extrapolated from experiments in which measurements of mass (or anything else, for that matter) were never exact. And furthermore, the amount of matter that would be required to be created continually to keep the universe at its present density would only be about

³⁸ More specifically, the principle being given up was the conservation of mass-energy, as energy and mass can be converted back and forth to each other, a process described by Einstein's familiar equation $E = mc^2$. Bondi, Gold, and Hoyle were not talking about *conversion* of matter from energy; they suggested *creation* of matter *ex nihilo*. In his field-theoretic formulation of the steady state model, Hoyle incorporated this mathematically by adding an extra term representing a scalar creation (of matter) field to the Einstein field equations, symbolized by the letter *C*, and called the *C*-field.

$7.5 \times 10^{-48} \text{ g/cm}^3/\text{s}$ —a quantity too small to be detected.³⁹ Postulation of such continual creation of matter thus, strictly speaking, did not violate observation.

The “continual creation of matter” hypothesis was often criticized as being an ad hoc hypothesis that had to be introduced in order for the steady state cosmology to work.⁴⁰ However, in response to this line of thinking, Bondi pointed out that this assumption was not an additional one; it simply *replaced* the idea of local conservation of matter:

The overriding principle [in science] must be that of the economy of hypotheses, but in comparing different theories according to this principle, one must take account of *all hypotheses* involved in them whether originally tacitly assumed or not. Replacing an old assumption with which many are acquainted by an new one (even if strikingly novel) does *not increase* the number of hypotheses required, and it is quite wrong to consider such a change a disadvantage (Bondi 1961).

Furthermore, as Hoyle later pointed out, at a certain point galaxies in the (infinitely large) steady state universe would be so distant that the light traveling to the observer from said galaxies would be redshifted completely. Thus an observer could never see the object, no matter how powerful a telescope he built. There would be a certain “horizon” for any observer around the edge of his universe; within the horizon, mass *would be* conserved—the continually created particles just balancing the ones going over the edge, and therefore out of the observable universe. Thus in this sense there was a kind of conservation of matter principle in the steady state model.

³⁹ This corresponds to “one hydrogen atom in a liter every trillion years” (Sciama 1959, p. 174).

⁴⁰ Ad hoc hypotheses will be considered further in Chapter 6.

While it may never have eclipsed the big bang model in popularity, the steady state model attracted a substantial following, especially in Britain.⁴¹ And despite the fact that the time-scale problem was solved in 1952 when more careful observations brought about a downward revision of the Hubble constant, implying an older universe (around 10 billion years),⁴² the steady state model remained.

Adopting the Steady State

Though Dirac had been Sciama's formal Ph.D. advisor, Sciama consistently stated the real influences on him at Cambridge were Bondi, Gold, Hoyle, and R.A. Lyttleton.⁴³ While Sciama was not a believer in the steady state model initially, under the influence of this "famous quartet" he at some point became "attracted" to the model while writing his thesis (Sciama 1990). There were several reasons for this.

⁴¹ Sciama suggested several reasons for this, with the first being it was due to that nation's traditional "interest in cosmology," dating back to Eddington. The second was the influence of Hoyle's popularization of the model during his series of lectures on BBC radio. Third was the greater general tolerance for "speculative" science in the UK, due to greater independence and decentralization in its funding system for young researchers, as compared to America and other countries (Sciama 1978, p. 19, 37). However, later Sciama criticized British cosmology as having "a tradition of being very speculative, in some cases, perhaps too speculative—not sufficiently controlled by the pragmatic element"(Sciama 1990). We will see this concern with "controls" from experimental data is a crucial theme in Sciama's science.

⁴² Walter Baade showed in 1952 that there exist two populations of Cepheid variables, with different period-luminosity relations. Not realizing this had caused astronomers to underestimate the luminosities of the Cepheids, and therefore the distances to them as well. This led to an overestimation of the Hubble constant, and therefore an underestimation of the universe's age.

⁴³ Sciama also had significant discussions on cosmology during this period with the likes of Pirani and McCrea, as well as during meetings of the Del Squared V and Kapitza clubs (Sciama 1978, p. 13-14, 18; Sciama 1990). As mentioned before, it was during this period Sciama "infected" Roger Penrose with his interests, prompting the latter to change research topics from algebraic geometry to relativity (Sciama 1978, p. 29).

First among these was Mach's Principle. Though his own work on this topic "didn't lead automatically" to the steady state model, Sciama says that "certain features" of his theory "were suggestive of [steady state]...the ideas have a certain congeniality, [though] they have no logical connection, just a natural similarity in texture...." (Sciama 1978, p. 10). It certainly also did not escape Sciama's notice that Mach's Principle was addressed in Bondi and Gold's 1948 paper as well as in Bondi's 1952 book.

So Sciama was "predisposed...to get sucked into the Steady State view,"—but, it seems, not just because of the reasons related to Mach's Principle cited by Bondi and Gold. "I found [steady state] attractive for a number of reasons unrelated to Mach," he wrote (Sciama 1978, p. 10). He was critical of those who abandoned the model after the age of the universe anomaly was corrected:

I am anxious to stress this approach to the steady state model because many people believe that the main reason it was introduced was the time-scale problem that existed in 1948....However...astronomers revised the extragalactic distance scale in the early fifties, and thereby...the time-scale difficulty disappeared....For many people, this development removed whatever attractions the steady state model may have had for them. I believe this was a profound mistake. While the relation of the steady state model to observation is of vital importance, the reason for introducing the model in the first place was not so much an empirical one but a logical one(Sciama 1973a, p.57).

Sciama here was referring to the other qualities of the steady state model that its adherents found appealing. For instance, Sciama considered the steady state model to be "a rather simple, appealing one" (Lightman and Brawer 1990, p. 141) that possessed a "general beauty" he found attractive (Sciama 1978, p. 23).

One example of this conceptual beauty, Sciama noted, was being able to avoid awkward questions like "Did time exist before the big bang, and that sort of thing"

(Sciama 1990). Also, the steady-state model removed the singularity that starts off any big bang model; many physicists had found such a singularity troublesome aesthetically or philosophically. The tradeoff for this was the problem of the creation of matter *ex nihilo* required by the steady state model. However, regarding this Sciama wrote:

The world is very weird....Therefore the fact that a proposal is weird or different from established traditions and so on...doesn't mean it's unthinkable or you shouldn't even take it seriously. And in order to determine which weird things are true and which weird things are false, you've got to play with the ideas and see what they look like....So I was attracted, in fact, by the idea of continual creation rather than being repelled by it. If you like, it even seemed less weird...than the big bang, where everything is created in one go. If creation is occurring all the time, it becomes a scientific process you can study because it's repetitive and you can start saying the creation rate is influenced by the environment....You can make a physical study of it. So it seemed to me attractive and the big bang was an awkward thing (Sciama 1990).

Sciama and others preferred the steady state over the big bang because in their opinion it made more *falsifiable predictions*. “The steady state theory did have predictive power, which was good,” Sciama reflected in 1990; this gave the model certain “methodological advantages” (Lightman and Brawer 1990, p. 141; Sciama 1990). Bondi et al. had repeatedly invoked the “profound” ideas of the philosopher of science Karl Popper, according to whose model science advanced not by theories’ being confirmed (which Popper considered a logical impossibility), but by theories’ making risky predictions and opening themselves up to be disconfirmed (see Bondi 1960, p. 18). We hear Popper’s influence in quotes such as, “Disagreement with observation would serve to disprove the steady state model, but the agreement of other models with observation is irrelevant” (Sciama 1973a, p.57).

For instance, the PCP demanded that the distribution of galaxies would look the same for all observers regardless of their positions in time. So, while galaxies certainly would age, as the universe expanded the older ones would get farther apart, and newer ones would form in between them. Thus, the steady state model predicted there should be no groupings of old galaxies together, or young ones together; the distribution should look the same no matter where or when one is. If this were found not to be so, in Bondi's own language, the steady state would be "stone dead." (Bondi 1961).

On the other hand, proponents of the big bang model, at least according to the model's detractors, could explain away any behavior whatsoever by chalking it up to "evolutionary effects" that could be tailored to fit the observations, i.e., by imposing arbitrary initial conditions in an *ad hoc* way. "What the cosmologist requires...is a theory which is able to account in detail for the contents of the universe. To do this completely it should imply that the universe contains *no* accidental features whatsoever," Sciama wrote. "We believe this criterion to be so compelling that the theory of the universe which best conforms to it is almost certain to be right" (Sciama 1959, p. 182).

But there were more intangible reasons for Sciama's preference for the steady state as well. "The more imaginative people seemed to like the theory," he wrote. (Sciama 1978, p. 16). He also explicitly stated that he was attracted to the "rumbustious, rebellious characters" behind the model, approvingly referring to them as "flamboyant, aggressive...young Turks" that challenged the establishment (Sciama 1978, p. 18; Sciama 1990). This attitude can plausibly be linked to Sciama's childhood adoration of G.H. Hardy, whom C.P. Snow described as "unorthodox, eccentric, radical, ready to talk about

anything” (Snow 1967, p. 9). For instance, Hardy once wrote, “It is never worth a first class man’s time to express a majority opinion. By definition, there are plenty of others to do that” (quoted in Snow 1967, p. 46). Similarly, Sciama later said, “I’m not so happy just trying to work out the consequences of existing theory. I’m always trying to push the boundaries of theories, seeing how far you can get with strange new ideas...without being cranky, but pushing a bit” (Sciama 1990).

Finally, the most important reason for Sciama’s preference for the steady state model is a surprising one, and one I find no record of in any of his published works.

In a 1978 interview with Spencer Weart, Sciama says of the steady state universe:

It’s the only model in which it seems evident that life will continue somewhere. On the [big bang] view, if the universe recollapses we’ll all get crushed, and if it expands forever, then everything dies out when all the fuel is spent. So life is a very transitory thing. Whereas on the Steady State theory, even if our galaxy ages and dies out, there will always be new, young galaxies where life will presumably develop. And therefore the torch keeps being carried forward. I think that was probably the most important item for me.... If you ask me for the one thing that dominated, it was probably that life would exist in some way (Sciama 1978, p. 22-23).

This surprising reasoning will be discussed further in Chapter 3.

Promoting the Steady State

Sciama began making original contributions to the steady state research program almost immediately after publishing his thesis work on Mach’s Principle (Sciama 1953). As we shall see, a theme that connects all three of these developments is the desire that the laws of physics should determine the contents of the universe naturally, without any arbitrary initial conditions or *ad hoc* assumptions applied.

Sciama's first major contribution came at some point in the early 1950's when it struck him that "it was difficult to make galaxies in a universe that didn't already have them...Whereas clearly, any single galaxy in the Steady State theory would form in a universe full of them, so why not use pre-existing ones...to do the job for you" (Sciama 1978, p. 17). Thus at a July 6-11, 1953 International Astronomical Union symposium held at Cambridge on the gas dynamics of cosmic clouds, Sciama debuted a model for galaxy formation within a steady state universe, which was published in the symposium proceedings in 1955 (Sciama 1955b). Sciama further developed the model into a paper submitted to the *Monthly Notices of the Royal Astronomical Society* in 1954, and published therein also in 1955 (Sciama 1955c).

The gist of the model, which Sciama said accounted "roughly" for numerous observed properties of the galaxies,⁴⁴ is that a given galaxy would perturb the intergalactic gas (constantly replenished from the continual creation) while moving through it in such a way as to make a new, "child" galaxy coalesce gravitationally in its wake. The child galaxy in the course of time would beget its own children and so forth; thus this amounted to a self-propagating scheme for galaxy formation.

⁴⁴ Among these were the average linear dimension of a galaxy, period of rotation, peculiar velocity, and distance between the galaxies (Sciama 1955c, p. 4).

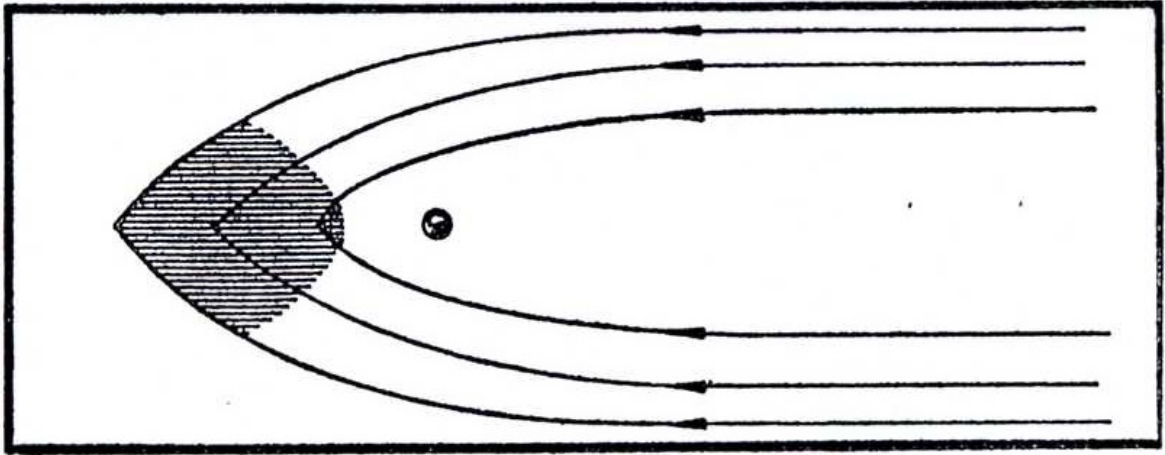


Figure 2. Sciama's Model of Galaxy Formation. Intergalactic gas is attracted by a galaxy's gravitational pull, becoming dense enough to collapse into a new galaxy (shaded area). From (Sciama 1959).

Further, according to the model, only about half of these child galaxies would escape from the original galaxy's gravitational influence, whereas the other half would remain gravitationally bound to the "parent;" in this way Sciama's model explained the clustering of galaxies as well. Since the steady state model suggested there have *always been* galaxies, explaining the formation of the "first" galaxy to get the ball rolling was a non-issue, something Sciama considered an advantage over the big bang cosmology. Sciama went on to further develop his model, later incorporating "thermal and magnetic forces" in addition to gravity (Sciama 1964c).

Sciama was "quite pleased" with this model because the steady state condition (that galaxies' properties must be independent of time) determined the galaxies' "properties uniquely, i.e. without any adjustable parameters" (Sciama 1955c, p. 3; Sciama 1978, p. 17). On the other hand, galaxy-formation models suggested by Gamow and others within the big bang scenario hinged on "arbitrary further assumptions"

(Sciama 1955b, p. 175). The gases had to be turbulent in certain specific ways and in certain specific areas for galaxies to form, the theory having

parameters...which are not determined by the basic laws of physics but can be adjusted so as to give agreement with observation. Thus the predictions of the theory are not very specific. Furthermore, this arbitrariness implies that many of the actual properties of the universe are accidental. It would seem preferable to have a theory in which the actual distribution of matter in the universe could be accounted for entirely in terms of the general laws and constants of nature (Sciama 1955c, p. 4).

Referring to this contrast, Sciama wrote

The steady state condition here replaces the initial conditions normally required, but now no arbitrary numerical quantities have to be specified. In the steady state model the properties of galaxies are as intrinsic to the universe as the laws of nature themselves. We regard this as a compelling argument in its favor (Sciama 1959, pp. 8, 194-5).

The galaxy formation model “certainly reinforced” Sciama’s belief in the steady state, he would later recall, “because it implied the problem could be solved. There was the means within the theory of solving it, which couldn’t be solved in the alternate theory. So that gave more power to the steady state theory” (Sciama 1978, p. 18).

Sciama’s first book, *The Unity of the Universe*, was published in 1959. In this popular book, Sciama’s Ph.D. work on Mach’s principle came to the fore. And, not surprisingly, the book (dedicated to Bondi, Gold, and Hoyle) served as a vehicle for the promotion of the steady state model. In it Sciama once more touted what he saw as the methodological superiority of the model⁴⁵ as well as other features he found positive, such as the potential of science one day to study the continual creation process.⁴⁶

⁴⁵ “If the universe does *not* evolve, we can account for many of its features which would otherwise be accidental” (Sciama 1959, p. 8).

⁴⁶Sciama says of physicists’ ironclad opposition to continual creation’s violation of the conservation of matter that “since the breakdown of reflection invariance [parity conservation] has been discovered, [physicists’] faith in the absolute validity of conservation laws has been noticeably weakened” (Sciama 1959, p. 174).

However, in a notable passage, Sciama was also quick to stress the steady state's "increase in range" was to be considered a "*provisional* advantage" only, since

one is influenced by it only when the available observations do not yet distinguish between the various possibilities. Such arguments have their place in science, because the time available for research is limited, and they suggest which theories are likely to be the most fruitful to work on. Nevertheless, the steady state model can be decided only by observation (Sciama 1959, p. 175).

Toward the end of the 1950's we see emerging another instance of Sciama's work within the steady state model, having to do with the irreversibility of radiation. As is well known, accelerating charges emit electromagnetic radiation; this process is described by so-called "retarded" solutions to Maxwell's equations. However, as Maxwell's equations are symmetrical in time, "advanced" solutions exist as well; these would "permit the radiation to exist before a charge is accelerated," i.e., these solutions suggest that accelerated charges should *absorb* energy (Sciama 1960a, p. 8; Sciama 1963b, p. 484). As this is in disagreement with observation, physicists' typical solution to this quandary is simply to ignore the advanced potentials as nonphysical or unrealistic.

Sciama found such a rejection a blatantly "*ad hoc* resolution" to the problem (Sciama 1963b, p. 484). He drew a parallel between the apparent existence only of retarded solutions and the resolution to Olbers's paradox,⁴⁷ namely that the redshift of radiation due to the expansion of the universe prevents equilibrium from occurring between the stars and their radiation. Sciama wrote:

⁴⁷ This is usually phrased as: given an infinite (or near-infinite) number of stars in the universe, "Why is the sky dark at night?" The paradox was first pointed out by Heinrich Wilhelm Olbers (1757-1840) who wrote about it in 1823. In fact versions of the paradox had been discussed by others earlier, including Johannes Kepler and Edmund Halley.

The lack of thermodynamical equilibrium between sources and radiation is thus a cosmological phenomenon. It is natural to suppose that the observed restriction to retarded solutions is a cosmological phenomenon also....We should be able to prove that sources radiate in the sense of time in which the universe expands.

In this, while Sciama stuck to working within “conventional Maxwell theory,” he was building on work done by J.E. Hogarth, a student of McCrea’s, who in his 1953 Ph.D. thesis applied a particular theory of action-at-a-distance electrodynamics to cosmology, a theory which had been originally developed by Richard Feynman and John Archibald Wheeler in the 1940’s. Sciama approvingly paraphrased Hogarth’s resolution of the time-symmetry paradox, saying that in the Wheeler-Feynman framework a given charge (“A”)

cannot be considered in isolation from the other charges in the universe. Indeed, the behaviour of these other charges becomes the deciding factor in determining whether A absorbs or emits energy....It turns out that each charge has two influences on A’s behaviour, one arising from a moment in the past, the other from a moment in the future.... If the influence from the future is stronger A emits radiation, and if the influence from the past is stronger A absorbs radiation (Sciama 1960a, p. 9).⁴⁸

If the big bang model⁴⁹ were correct, Hogarth had found that since in the future charges in the universe would be spread out more thinly due to the expansion, their influence would thus be diminished—leading to a stronger influence from past charges and the incorrect prediction that accelerated charges in the present should absorb energy.

Sciama’s further work, however, uncovered that there were in fact some distributions of charges that would lead to the required retarded potentials in this model—but that these solutions also produced an “arbitrary amount of source-free

⁴⁸ It seems natural to suggest that Sciama must have approved of the echoes of Mach’s Principle apparent in this theory.

⁴⁹ In particular, Hogarth applied Wheeler-Feynman theory to the Einstein-deSitter model of the expanding universe.

radiation,” which caused the Wheeler-Feynman theory to break down, and which was also in then-disagreement with observation (Sciama 1963b, p. 484, 493) On the other hand, since in the steady state model matter is constantly being created, the “influence from the future will be enhanced,” and thus the model predicted that accelerated charges in the present should indeed emit energy—and without paying the price of postulating the existence of unobserved source-free radiation.⁵⁰ Thus, in the “steady-state model Maxwell’s theory *implies* the Wheeler-Feynman theory, and leads to retarded potentials with no source-free radiation, in agreement with the (somewhat crude) observational data” (Sciama 1963b, p. 494, emphasis added). And perhaps most importantly, the steady state theory did this without any ad hoc assumptions.

Thus we have seen several examples of Sciama’s original contributions to the steady state model. I will now turn my attention to Sciama’s attempts to defend the model from hostile data.

Defending the Steady State

Stebbins-Whitford Effect

The steady-state model of the universe was under attack almost from the moment of its proposal. The first major salvo against the theory came in the year of its

⁵⁰ Hogarth phrased it even more strongly—that “the simple observation that accelerated charges radiate enables one to infer the existence of continual creation” (paraphrased in Sciama 1960a, p. 10). Sciama’s later recollection of his work on retarded potentials seems to differ somewhat from his published implications; in 1978 he recalled that his take on Hogarth’s work “didn’t particularly support the Steady State theory” because of the retarded solutions that he’d proved did indeed exist in the big bang model under the right conditions (Sciama 1978, p. 25).

conception, 1948, with the announcement by Joel Stebbins and Alfred Whitford of an apparent reddening in the spectra of distant galaxies, over and above what was expected due to the recession described in Hubble's Law. This implied that distant galaxies (seen at a much earlier stage of development due to the time it takes their light to travel to Earth) differed intrinsically from nearby galaxies—a clear violation of the perfect cosmological principle, which predicted that fundamental properties of galaxies should not change systematically with distance from Earth.

This “Stebbins-Whitford effect” thus became the “main argument” against the steady state model in the early 1950's. Sciamia and his collaborators considered it a “matter of serious concern.” But at the same time, Sciamia believed that Stebbins and Whitford's “hostile evidence was rather weak... [the steady state] seemed to me to be too beautiful and desirable a theory to be defeated by weak arguments. So I would get a bit intense and argue eagerly and so forth” (Sciamia 1978, p. 15-16).

Sciamia wrote a paper with Bondi and Gold for the *Astrophysical Journal* in 1954 in which they criticized Stebbins and Whitford's data, saying that their use of the galaxy M32 to calculate the correction for normal Hubble red-shifts in other galaxies called into question their results, since M32 might be an atypical galaxy (Bondi, Gold et al. 1954).⁵¹

The Stebbins-Whitford effect did indeed turn out to be spurious, just as Sciamia et al. had predicted; it was withdrawn in 1956. Sciamia, however, was “annoyed that it was withdrawn in a very obscure place—namely the progress report of [Whitford's]

⁵¹ Gérard de Vaucouleurs had suggested a similar thing in 1948, but for some reason this was “largely ignored” (Kragh 1996, p. 278).

observatory;⁵² “Whitford and Code withdrew it, tucked away there rather than blazing it forth—since it had been used so strongly as a weapon against the steady state” (Sciama 1978, p. 16).

Thus Sciama’s first defense of the model was an unqualified success.

B²FH vs. α - β - γ

As mentioned before, in 1948, Gamow and Alpher had proposed a model (α - β - γ), linking the origin of the elements to the hot, dense conditions of the big bang. Hoyle, along with fellow steady-state adherents William Fowler and Geoffrey and Margaret Burbidge, had on the other hand proposed an alternate model (known as B²FH—their initials) in which the heavy elements were explained as having been “cooked” in stars (since in the steady state model there was no big bang to serve this function).⁵³

The best defense being a good offense, in 1955 Sciama went on the attack, pointing to the growing evidence that α - β - γ was seriously flawed;⁵⁴ he considered B²FH to be superior, both empirically and for philosophical reasons as well, again invoking negatively the “arbitrary” conditions inherent in the big bang model and α - β - γ :

⁵² This was the Wisconsin Washburn Observatory. Whitford also mentioned the withdrawal in an address to the 96th Meeting of the American Astronomical Society in December 1956, which was subsequently abstracted in *Sky and Telescope* (Kragh 1996, p. 278).

⁵³ Stellar evolution was not considered contradictory to the steady state model. Evolution on a small, local scale was acceptable in the model; only on a large scale was it forbidden.

⁵⁴ One of the problems α - β - γ faced was its inability to produce elements of an atomic weight of five and higher during the first half-hour after the big bang, after which the universe would be too cool to synthesize such heavier elements (Sciama 1959, p. 211).

But to my mind there is a more important reason for preferring the steady-state theory. For in theories which start from an explosion the initial properties of the universe are entirely arbitrary. Thus it is possible to find an initial temperature which is favourable for making heavy elements, and then one simply has to assume that this was the initial temperature....This means that in this type of theory the laws of physics do not specify the contents of the universe....The steady-state theory opens up the exciting possibility that the laws of physics may indeed determine that contents of the universe through the requirement that all features of the universe be self-propagating (Sciama 1955a).

Sciama reaffirmed this later, writing that “in the α - β - γ cosmology source-free radiation plays a decisive role near $t = 0$This arbitrariness is one example of the difficulty that in evolving models of the universe some of the initial conditions are not determined” (Sciama 1960a, p. 492).

It soon became apparent to the community that B²FH was indeed the superior theory; thus Sciama’s attack on the big bang and defense of the steady state had scored another important victory. B²FH is still (basically) the accepted model today for nucleosynthesis.

Radio Source Counts

The next onslaught against the steady state model came from Cambridge radio astronomer Martin Ryle's “2C” and “3C” surveys of radio source counts; the infant science of radio astronomy was realized to have cosmological implications in the 1950’s when it was concluded that most of the radio sources in the sky were of an extragalactic nature. According to the steady state model, since the universe is unchanging on the large scale, such radio sources should be spread out *uniformly* through static, Euclidean space. This, in turn, entailed that the number of sources N seen per unit solid angle should vary with the $-3/2$ power of their power per unit area (i.e. flux density, or

intensity), S .⁵⁵ Thus, the steady state model was correct, if plotted on a log-log graph of N vs. S , the sources should produce a line with a slope of -1.5 —a very specific prediction.

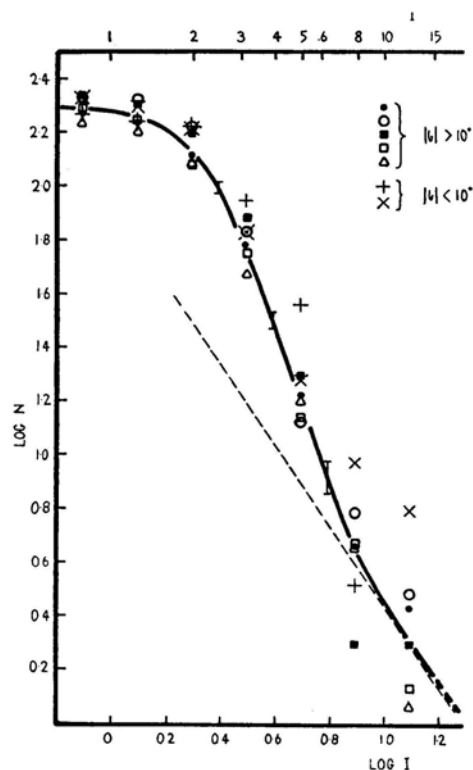


Figure 3. Ryle's Plot of the 2C Data of Radio Sources. The dotted line is the steady state prediction of -1.5 .

Ryle's data from the 2C survey (published in 1955 with Peter Scheuer) produced a significantly *steeper* slope than this, namely with a slope of -3 . Ryle explained this steeper slope as being due to an excess of distant radio sources, meaning that there were significantly more radio sources in the distant past as compared to now. This violated the perfect cosmological principle and therefore the steady state model of the universe.

⁵⁵ A derivation of the $-3/2$ power law is given in (Sciama 1971, p. 85).

Sciama was unconvinced by Ryle and Scheuer's results. He called into question the reliability of the 2C and preliminary 3C data, many of the radio sources of which were optically unidentified. He suggested in mid-1958 a number of possible systematic errors in Ryle's data collection. He further pointed out problems with Ryle and Scheuer's interpretation of the data, noting that it hinged on an unfounded assumption that the radio sources all had the same absolute intensity, and furthermore it was "not yet definitively established" that the radio source were indeed extragalactic in nature. Sciama also went on to point out new radio source count results from an Australian group headed by B.Y. Mills, which gave a significantly flatter slope of -1.8, with Mills saying that the difference between this and the predicted steady state value was "not significant, and is attributed to instrumental effects" (Sciama 1960b, p. 315, 316).

Thus as of 1958, Sciama could still write that "it is still not possible to decide whether the Universe is in a steady state or whether it has evolved from a much denser configuration" (Sciama 1960b, p. 311). However, he was careful to add that the various troubles he saw facing radio astronomy

will no doubt be removed by further investigations....If such results could be obtained, they might have cosmological value despite the complexities of the theoretical relations. For instance, certain theoretical possibilities might be ruled out if they could fit the observations only with unreasonable assumptions.... (Sciama 1960b, p. 317).

This can be read as a dig at the big bang model, or a 'marker' as to how far Sciama would go to save the steady state model.

Sciama was not alone in his suspicion of Ryle's data; according to Kragh, "By 1959 it had become evident to most astronomers that the results from Cambridge 2C

contained large systematic errors....” However, the improved results from the 3C survey were to be another matter. Announced in 1961, these data were significantly better in terms of both quantity and quality than the 2C survey, producing a slope of -1.8 on the $\log N$ - $\log S$ graph, again in contradiction to the steady state prediction of -1.5 . Further, owing to the greater reliability of the data, unlike the result of Mills in 1958, the 3C slope could not easily be “forced down” or argued away by appeals to instrumental effects, data analysis errors, or the like (Kragh 1996, p. 317, 323-4). Many saw this as a fatal blow to the steady state model.

Sciama concluded that the radio data had become “essentially correct,” if not “beyond all doubt;” the increased stability of the radio source data forced him to change tack in his defense of the steady state (Sciama 1965, p. 1). Again, the too-steep slope of the $\log N$ - $\log S$ graph had been proposed by Ryle to be due to an excess of weak (i.e., distant) radio sources, which he considered evidence of the universe’s evolution over time. But an alternate hypothesis was to propose the steep slope as being due to a deficit of intense (i.e. nearby) sources, rather than an excess of weak sources. That is, we are surrounded by some sort of “hole” in the extragalactic sources.

Robert Hanbury Brown proposed a model of just this sort, a variation of which was developed by Hoyle and J.V. Narlikar. However, to flatten the $\log N$ - $\log S$ slope sufficiently to save the steady state model, such a hole would have to be 300 million parsecs across—one-tenth of the radius of the universe. However, under the Hoyle-Narlikar model, the largest predicted irregularity would top out at only about 2 million parsecs (Sciama 1965, p. 10, 12). With admirable understatement, Sciama wrote it was

“somewhat premature to postulate the existence” of such a 300 million parsec hole, and likewise dismissed the 2 million parsec hole idea as well (Sciama 1963a, p. 195).

Sciama’s solution was to suggest a two-population model for the radio sources—that many radio sources were in fact not extragalactic, but *inside* the Milky Way—and that the “hole” we are in is simply a local one in the distribution of galactic sources. If this were the case, the hole would only have to be a mere 17 parsecs across, which he found acceptable (Sciama 1963a, p. 196). Thus, Sciama could write, “contrary to the claims of Ryle and his co-workers, their counts of radio sources can be interpreted in terms of the steady state model of the universe”(Sciama 1963a, p.195).

Sciama published a follow-up paper, in which he proposed an astrophysical mechanism to produce the hole called for in his previous work; this mechanism required the sources not only to be partially in the galaxy but also to have certain properties. Sciama termed the mechanism in question (which suggested that faint, low-mass stars went through a phase of instability in which they gravitationally collapsed in such a way as to produce a radio source) “not astrophysically unreasonable” or “untenable,” but “extremely tentative.”⁵⁶ Once more Sciama explicitly stated that his local hole hypothesis “was constructed in order to show that the steady state theory of the universe can be reconciled with the Cambridge counts of radio sources” (Sciama 1964a, p. 49, 59).

About a year later, Sciama published a paper in which he fine-tuned his model based on new data. He also considered the possibility of a variation of his model, namely the proposed second population of radio sources required could in fact be extragalactic in

⁵⁶ This paper was in written in part as a rejoinder to criticisms by P.F. Scott of his “local hole” model.

nature after all, but of significantly smaller luminosity than “normal” extragalactic sources. Sciama quickly concluded however that “the steady state model cannot be saved by postulating the existence of a second population of extragalactic sources. On the other hand, our original postulate of a population of galactic sources still remains a possibility” (Sciama 1964d, p. 262-263).

At this point, Sciama was undeterred, writing that while his model had “not found favor with radio astronomers...more work will have to be done before it can be ruled out...The steady state model remains in the field, bloody but unbowed” (Sciama 1965, p. 15).

Extragalactic Radio Noise

Sciama around this time also published a paper suggesting a mechanism for the absorption of extragalactic radio noise. An explanation for this had been proposed by which the source of the absorption is a more-or-less spherical HII region surrounding us. Sciama found this unappealing, because of the “unattractive feature that we are required to be near the center of this region...” (Sciama 1964b, p. 767). He instead proposed a cosmological explanation, suggesting that intergalactic gas might be responsible for the absorption, calculating that either Einstein-de Sitter or steady state cosmology could do the job in agreement with observation.

This might be seen as an attempt to go on the offensive amidst the troubles caused by the radio source counts, showing that the steady state model could explain a different, puzzling, cosmological phenomenon at least as well as the standard model would. That

is, there was life still in the steady state model. However, new data (noted by Sciama in the proofing of his article) appeared to support the local hypothesis for the phenomenon after all, if perhaps not conclusively, thus mooting the effort altogether (Sciama 1964b).

X-Ray and Infrared Lines

Working with R.J. Gould, Sciama around this time also suggested “it may be possible to obtain significant cosmological information from” x-ray and infrared observations. If certain x-ray spectral lines were superimposed on the diffuse x-radiation background first detected by Riccardo Giacconi, which might be possible if the radiation were due to the integrated x-radiation of all the galaxies, then the shape of this spectrum would depend “*critically on the large-scale structure of the universe*” (Gould and Sciama 1964, p. 1634, italics in original). Gould and Sciama made a similar argument for the infrared region as well. However, as Kragh says, “no characteristic lines were observed, and so the test remained ineffective” (Kragh 1996, p. 323). Yet it still showed Sciama viewed the steady state model as still having “legs.”

Quasars

The next major blow against the steady state model had to do with the discovery of the quasars. Allan Sandage in late 1960 had identified the 48th object in Ryle’s 3C catalogue (3C 48) with a star-like object (i.e., possessed of very small angular diameter), which had an extremely unusual spectrum that had baffled astronomers. The mystery was solved on the afternoon of February 5, 1963 by Maarten Schmidt, who realized that the similarly-puzzling lines in the spectrum of another star-like radio source (3C 273) were simply those of normal elements one would expect redshifted to a great degree.

These bizarre objects became known variously as quasi-stellar objects (“QSO’s”), quasi-stellar sources (“QSS’s”), or quasi-stellar radio objects (“quasars”).

Schmidt and his colleague Jesse Greenstein quickly concluded on theoretical grounds the redshifts of the quasars could not be due to the gravitational redshift predicted by general relativity; their spectra revealed the quasars’ gases were not dense enough to produce this effect (Schmidt 1990). This left two possibilities: that the quasars were extremely distant objects, receding due to the expansion of the universe (i.e., they were cosmological), or that they were nearby objects simply receding from us kinematically at a great rate.

If the high redshifts were indeed cosmological, and the quasars were thus all at great distances, the perfect cosmological principle would clearly be violated since no quasars were observed nearby. Ardent steady-state cosmologists (such as Hoyle, the Burbidges, and J. Terrell), not surprisingly, thus preferred the local hypothesis, suggesting that the quasars had, for instance, been ejected violently from the center of the Milky Way or a nearby galaxy such as Centaurus A (Sciama 1966a, p. 438-9). Indeed, Hoyle and others produced models of quasars purporting to show that they could not possibly be at cosmological distances, due to the energy losses that would occur from the inverse Compton effect.⁵⁷ However, the local model was complicated by the fact that no quasars were observed with blueshifts; in the case of a local explosion, for instance, one would expect just as many quasars to be moving towards us as away.

⁵⁷ Collisions between photons and an energetic (cosmic-ray) electrons, in which some of the energy of the electrons is transferred to the photons.

From the beginning, Sciama was unwilling to declare that *no* quasars could be cosmological. His earliest published work on the topic was a 1965 paper (coauthored with F.T. Haddock) on the variable quasar *CTA 102*,⁵⁸ which accepted the cosmological interpretation for this object (Haddock and Sciama 1965).⁵⁹ In subsequent papers in 1965 (coauthored with his student Martin Rees), Sciama suggested composite models for both *CTA 102* and *3C 273 B*;⁶⁰ in the former case cosmological distances were again assumed (Rees and Sciama 1965a), whereas the latter paper was more explicit, showing that the quasar's radio variations were "consistent with its red shift being cosmological," and saying that recent data on the object "considerably weakens the case for the local model" (Rees and Sciama 1965b, p. 371, 374).

Sciama reiterated this belief in the cosmological redshift for both of these objects when teaching his course as part of the 1965 Enrico Fermi summer school held in July at Varenna, Italy (Sciama 1966a, p. 439). During this course he took the opportunity to critique his and Rees' own model of *CTA 102*, saying that the model's requisite "special geometry for the smaller component, [and its assumption of] a special orientation with respect to the line of sight" made it undesirable. "The probability of the source having such a special orientation is very small. It seems advisable therefore to wait for the

⁵⁸ Evidence was later put forward, which Sciama duly noted, that *CTA 102* was not variable after all (Sciama 1966a, p. 439).

⁵⁹ This work suggested using the radio variable's properties to detect the ionized intergalactic gas Sciama had suggested in his 1964 paper, which in turn could produce an *experimentum crucis* for deciding between the Einstein-de Sitter and steady state models.

⁶⁰ It had been realized that *3C 273* had to be composed of two distinct components, *3C 273 A* (a jet) and *3C 273 B* (the actual QSO).

observational decision on whether this source does actually vary before pursuing this question further theoretically,” Sciama wrote. However, his and Rees’s models for 3C 273 he still considered “reasonable,” but once more stressed the critical import of further observations to help decide the validity of these models (Sciama 1966a, p. 441-2, 450). Sciama once more said that recent conclusions reached about the quasars “considerably weakens the case for the local model of quasi-stellar radio sources...” (Sciama 1966a, p. 450).

Further, Sciama and Rees published a 1966 paper in *Nature* in which they attacked Hoyle’s model that suggested that losses due to the inverse Compton effect in intense radio sources eliminate the possibility that quasars were at cosmological distances. Rebuking his former mentor, Sciama said, “there *are* reasonable cosmological models capable of accounting for the optical variability of quasars on the assumption that they are at cosmological distances,” and that “there are no compelling arguments which force us to abandon the cosmological hypothesis” (Rees and Sciama 1966, p. 607).

However, in his next paper (co-written with his student W.C. Saslaw), Sciama suggested that some—though not all—quasars might be within the Milky Way after all. He pointed out that of the roughly fifty QSO’s observed, only 23 had their redshifts measured. While these values in his opinion definitively showed those quasars (including 3C 273) were at cosmological distances,⁶¹ it was entirely possible that the remaining unmeasured ones could well be inside the galaxy (with “an essentially zero red

⁶¹ “It is...natural to adopt the cosmological hypothesis....The cosmological hypothesis seems by far the most likely to be correct” (Sciama and Saslaw 1966, p. 348).

shift”). Thus the quasars Sciama theorized to be comprised of two “entirely different and unrelated components.” He identified this hypothesized local population of quasars with the second, galactic population of radio sources earlier predicted by his 1963 revision of the work of Hanbury Brown, and Hoyle and Narlikar, saying around this same time that the new information was “gratifyingly consistent with the requirements of the [1963] model” (Sciama 1966a, p. 457). Sciama also explicitly stated the “reason for introducing this possibility has to do with the relation between counts of quasi-stellar radio sources and the steady-state model of the universe” (Sciama and Saslaw 1966, p. 348-9).

Sciama pointed out recent analyses due to P. Véron and M.S. Longair, which segregated the quasars from the other radio sources, permitting them to be plotted separately on the $\log N - \log S$ graph. When this was done, the non-QSO radio sources produced a slope of almost exactly -1.5 , just as predicted by the steady state model, whereas the quasars alone produced a steeper slope of about -2 . Sciama concluded “the quasi-stellar radio sources are entirely responsible” for the too-steep slope of the combined $\log N - \log S$ graph (still measured to be roughly about -1.8) (Sciama and Saslaw 1966, p. 349). Thus Sciama proposed that further observations should reveal the existence of nearby quasars just outside the local “hole,” in addition to the distant (cosmological) ones, in accord with the PCP. These could be distinguished from the distant ones by their large proper motions and roughly-zero redshifts. Sciama concluded his Enrico Fermi course with the following comment: “This prediction should be tested

soon, perhaps even before this lecture sees the light of day”⁶² and repeated his description of the steady state as remaining “in the field, bloody but unbowed” (Sciama 1966a, p. 460).

Cosmic Microwave Background Radiation

1966 also saw Sciama address in print for the first time the cosmic microwave background radiation (CMBR) dramatically (if accidentally) discovered by Arno Penzias and Robert W. Wilson in 1965. This space-filling radiation had been predicted as a necessary consequence of Gamow’s hot big bang model in 1948—though the prediction had been largely forgotten by 1965. Importantly, according to Gamow’s model, the radiation should be of a blackbody nature, a product of the universe’s being in a very dense state early on. This, in turn, meant the radiation would fit a precise blackbody curve when its intensity was plotted against its wavelength.

Sciama appreciated the import of this prediction immediately. As no big bang ever occurred in the steady state model, no such primordial blackbody radiation was therefore predicted by it. Further, in a very dilute universe (as the steady state predicts the universe is and always was) there was no mechanism for the radiation of, say, stars to come into thermal equilibrium with matter, either. The discovery of the CMBR—if not spurious—thus posed a significant threat to the model. “The demonstration that the microwave background radiation consists of black-body radiation would be of the utmost

⁶² Sciama was prescient; this had indeed occurred by the time he was proofing this lecture for publication. He added the following note in proof: “Recent red shift determinations show that the steady-state model can probably be ruled out, if the red shifts are cosmological in origin,” and referenced his 1966 paper in *Nature* with Rees on this topic, discussed below (Sciama 1966a, p. 460).

importance for cosmology. In the first place, it would almost certainly enable the steady-state model to be ruled out,” he wrote in the July 16 issue of *Nature*.

However, Sciama then proceed to propose an alternate model for the radiation’s formation, arguing “it is important to be sure that there is no alternative explanation of the observations. My aim in this article is to show that an alternative explanation does in fact exist” (Sciama 1966b, 277, 278). Sciama’s alternative model proposed that suggested the CMBR was only *apparently* blackbody in nature, the radiation really being due to the integrated effect of a population of radio sources, if certain specific conditions also applied. Sciama considered these proposed sources to have “a reasonable structure” and concluded his article by stating that if he was correct, “the microwave background would be consistent with the steady state model of the universe” (Sciama 1966b, 279).

Sciama Recants

About a month later Sciama encountered a turning point, brought about by increasingly abundant, reliable quasar data which showed that quasars were not uniformly spread through space, in violation of the PCP. In 1990 Sciama recounted these events:

The thing that actually converted me was the following, which involved my student Martin Rees. He was still working on his Ph.D. at the time, must have been late '65, early '66. By then quite a lot of quasars had been discovered.... The key quantities you would compare steady-state theory with observation was the number of sources of a given redshift if you have a certain class of sources which is supposed to be distributed uniformly in a steady-state universe. There is a simple formula of the number of sources at redshift z as a function of z . And by...early '66...there were enough quasar redshifts known... to start plotting this relation....

And so I did this...and I got a result that was nicely in agreement with the steady-state formula. So I rushed to Martin Rees...and I said, "Look, Martin," I said, "I've just plotted out the N vs. z relation for quasars and it fits steady-state beautifully, how about that.... But Martin was never particularly enamored of steady state and was also rather critical and careful. He said, "Well, I'll take it away and look at it...." He comes back a few weeks later and says, "Well, I've done it more carefully than you and it is clearly inconsistent with steady-state and shows...at large redshifts there are far more quasars per unit volume.... (Sciama 1990).

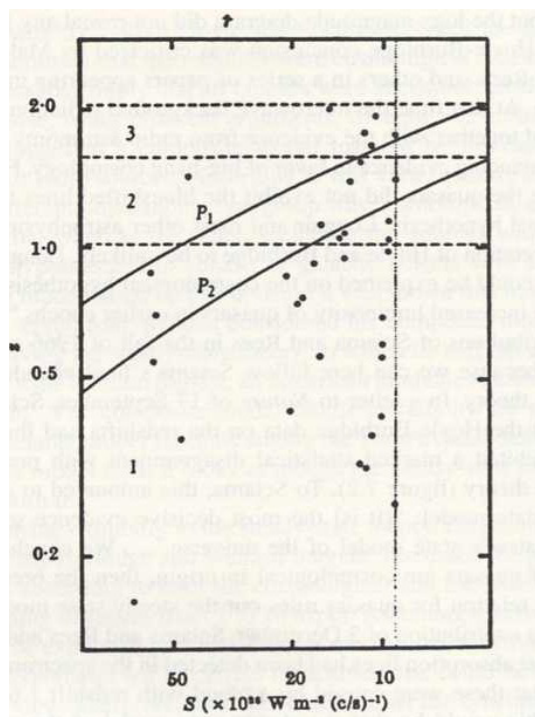


Figure 4. Sciama and Rees's Plot of Quasar Distribution. The distribution contradicts the steady state model's prediction. From (Sciama and Rees 1966a).

Sciama and Rees reported this finding in a letter to the editor of *Nature*, outlining that the quasars displayed an evolution with red shift of either intrinsic radio luminosity, or number of sources, or both—either of which would violate the PCP. Sciama and Rees called this “the most decisive evidence so far obtained against the steady state model of the universe” and concluded “that if the red-shifts of quasars are cosmological in origin, then the present red-shift-flux density relation for quasars rules out the steady state model of the universe” (Sciama and Rees 1966b, p. 1283). Sciama and Rees published another

paper subsequently in 1966 reaffirming this, saying that if current data were correct “the steady state model of the universe could almost certainly be ruled out” (Sciama and Rees 1966a, p. 1002).

Thus it was discovery that quasars showed an evolutionary effect, contrary to the PCP, that caused Sciama explicitly to reject his beloved steady state; Sciama confirmed this explicitly several times in later years.⁶³ Other cosmologists did not place as much import on the quasar data; as Helge Kragh puts it, “Sciama’s evaluation of the significance of the quasar redshifts was not shared by most astronomers, who rather saw them as just one more piece of evidence, should one be needed, for the correctness of an evolutionary universe” (Kragh 1996, pp. 337-8).⁶⁴

My take on this episode is slightly different from Kragh’s; while Sciama was certainly clear that the quasar data were what prompted his conversion, he was equally clear in other places that the result was not to be taken in isolation, but rather along with the radio source counts and blackbody background radiation, each of which was becoming more and more reliable, for instance: “If the red shifts of the quasars are cosmological in origin, and if the universe is filled with black body radiation, then the

⁶³ E.g., “Unfortunately, [Ryle’s radio source counts were] *not* the only evidence. Hard on the heels of the radio source counts came the discovery that amongst these radio sources were the now notorious quasars.... At first, the comparison [with the predictions of the steady state model] seemed to be working out favourably until my student, Martin Rees, pointed out to me that there were far too many quasars of large red-shift to be compatible with the steady state model. My own disillusionment with the model dates from that time....” (Sciama 1973a, p.59-60).

⁶⁴ As to why other astronomers made the conversion, opinions vary. Sciama, for instance indicates that the discovery of the cosmic microwave radiation was decisive: “most people were mostly converted by the three degree background” (Sciama 1990). (Brush 1993) agrees. Kragh, however, believes this claim is overstated.

chances of the steady-state theory surviving are very small indeed” (Sciama 1977 [1967], p. 31).

Sciama also later piled on the abundance of helium as another piece of evidence for the big bang:

Nearly all [estimates] agree on a helium abundance of 10 percent [that of hydrogen] and this is precisely what would be expected if the helium were formed in the hot big bang. The resulting relation between the 3 degree background, the helium abundance and the hot big bang represents the most important advance in cosmology since the discovery that the universe is expanding (Sciama 1973a, p.64).

Sciama’s conversion was complete and unequivocal, but unenthusiastic. Sciama would lament,

The steady state theory . . . of all the heretical theories . . . is the one that has irritated and excited and provoked the most people, has provoked the most good astrophysics. . . . I must add for me that the loss of the steady-state theory has been a cause of great sadness. The steady-state theory has a sweep and beauty that for some unaccountable reason the architect of the universe appears to have overlooked. The universe is in fact a botched job, but I suppose we shall have to make the best of it. . . . [The steady state] is. . . [a] magnificent conception we must now reluctantly abandon (Sciama 1977 [1967], p. 31).

W. T. Sullivan III recalls a speech by Sciama at the University of Maryland, College Park “in 1966-7 in which he stated that he was *recanting* from the steady-state cosmology. . . .” (Sullivan 1990, p. 344). Similarly, Bernard J. Carr recalls Sciama’s remark at a Cambridge University Astronomical Society lecture in 1968 that he was “wearing sackcloth and ashes’ as a result of his previous endorsement of the Steady State theory” (Carr 1993, p. 258).

Sciama, of course, continued to have a productive career after his conversion; as mentioned in Chapter 1, he was an active cosmologist until the end of his life. However, this ended Sciama’s journey as a steady state backer. He remained a steady-state enthusiast, however, in the sense that he continued to see the theory as a beautiful model,

if an incorrect one. The aesthetics of science, and of this theory in particular, will be considered in the next chapter.

CHAPTER 3

AESTHETICS AND SCIENCE

Beauty is truth, truth beauty,—that is all
Ye know on earth, and all ye need to know.

—Keats, “Ode on a Grecian Urn”

Overview

“Aesthetic factors play a distinctive part in shaping scientific practice,” writes philosopher of science James W. McAllister, who has written extensively on the subject (McAllister 1991a, p. 339). Astronomer Mario Livio agrees, saying that “a close examination of the history of physics and cosmology reveals...that physicists have in fact long adopted” aesthetic reasoning in their work (Livio 2000, p. 263). Steven Weinberg also concurs, writing that aesthetic factors have been “much help” in scientific practice (Weinberg 1992, p. 166).

Examples abound. Albert Einstein, for instance, famously valued the “naturalness,” “logical simplicity,” and “inner perfection” of theories, writing that such things have “played an important rôle in the selection and evaluation of theories since time immemorial,” that a “reciprocal weighing of incommensurable qualities” is always at play in theory choice (Einstein 1949a, p. 23). Aesthetic valuations of Einstein’s work are many: Lorentz felt general relativity (GR) had “the very highest degree of aesthetic

merit” and that “every lover of the beautiful must wish it to be true.” Similarly, on this basis, Rutherford said of GR that “quite apart from any question of its validity, cannot but be regarded as a magnificent work of art” (McAllister 1991a, p. 335). And Subramanyan Chandrasekhar termed GR “the most beautiful creation of human thought....‘Scarcely anyone who fully comprehends the theory can escape from its magic’” (quoted in Impey 2004, p. 35-6).

Nicolaus Copernicus opened Book One of his *On the Revolutions of the Heavenly Spheres* with a lengthy description of the “elegance” and “purity” of the heavens, and in Book Ten of that same work suggested a reason for preferring the heliocentric model was that in it “we find that the world has a wonderful commensurability and that there is a sure bond of harmony for the movement and magnitude of the orbital circles [of the planets] that cannot be found in any other way” (Copernicus 1995 [1543], p. 26). Owen Gingerich considers statements like these, along with computer analysis showing the errors made by the two systems of the heavens (Ptolemaic and Copernican) were of roughly the same magnitude, to be evidence that Copernicus’s *primary* motivation in constructing his model was aesthetic.⁶⁵ He writes:

What has struck Copernicus is a new cosmological vision, a grand aesthetic view....Copernicus’ radical cosmology came forth not from new observations but from

⁶⁵ However, Gingerich is quick to point out that, contrary to popular opinion, the increased aesthetic benefit of the Copernican model was not a dramatic decrease in number of epicycles required; the two models were roughly equivalent in such complexity. Instead, Gingerich links a phrase, the “fixed symmetry of its parts,” that Copernicus uses to describe his model to the fact that the sizes of the planetary orbits were now fixed and no longer scalable, as they had been in the Ptolemaic model. “This is certainly one of the most striking unifications brought about by the Copernican system—what I would call a profound simplification” (Gingerich 1975, p. 89-90). As to why this simplification occurred in the early 1500’s and not earlier, Gingerich states that “the flowering of new world views must be considered within the context of complex sociocultural structures,” and suggests that the invention of the printing press, as well as the flowering of the Renaissance, may have been catalysts (Gingerich 1975, p. 90-91).

insight. It was, like Einstein's revolution four centuries later, motivated by the passionate search for symmetries and an aesthetic structure of the universe (Gingerich 1975, pp. 86, 90).

In this chapter I will look at scientists' uses of aesthetics in scientific practice. I will consider a current philosophical model of aesthetics in theory choice, and will apply this analysis to the Sciama case. In the end it will be shown, while ubiquitous, aesthetic arguments in science are not reliable indicators of truth—despite many scientists' certainty to the contrary. Scientists seem to conflate their hope that the universe (or the laws governing it) be simple, with the ontological reality of same. While aesthetic arguments are useful (or at least often-deployed) tools in constructing or adjudicating between theories, particularly in the early days of a field, such arguments become more and more unreliable as solid data are accumulated.

Aesthetics in Theory Choice⁶⁶

While some scientists, such as Bohr, equated the beauty of a theory with its truthlikeness,⁶⁷ many other scientists differentiate between aesthetic features in science

⁶⁶ Note here I do not consider the aesthetics of experiment, in which scientists' aesthetic evaluations are often of a different character than those for theories. When writing of a beautiful experiment, for instance, scientists often speak of things like the degree to which an experimental finding jibes with a theoretical prediction (especially upon repeated trials), the ingenuity of an experimental apparatus, or a clever, economical arrangement of such apparatuses for the purpose—"how efficiently and dramatically the experiment made an important result stand out." Ernst Öpik's 1922 estimate (Oepik 1922) of the distance to the Andromeda Nebula (i.e., Galaxy)—based on rotation velocities and its mass-to-luminosity ratio—was termed by one commentator, for instance, as "beautifully simple, yet elegant" (quoted in Smith 1982, p. 103). For more on "beautiful" experiments, see (Crease 2003). Similarly, the aesthetics of technology is also not considered here.

and features McAllister calls “logico-empirical” criteria (McAllister 1991a, p. 332).

Logico-empirical criteria are the familiar ones most usually associated with scientific practice, including: novel prediction (i.e., fruitfulness), explanatory power (i.e., scope), empirical content (i.e., the avoidance of tautologies), and consistency (internally, with experimental data, and with “current well-corroborated theories”) (McAllister 1991a, p. 9-12).

However, frequently scientists espouse other desiderata for theories, unrelated to the above—aesthetic qualities. McAllister outlines five classes of such properties: form of simplicity, form of symmetry, invocation of a model, visualizability/abstractness, and metaphysical allegiance. All of these, he says, invoke a certain sense of “aptness” in scientists’ evaluation of theories that is quite separate from their empirical adequacy (McAllister 1996, p. 36-7, 40). Scientists also speak regularly of the importance of “elegance” in theories. However, my impression is that this is frequently used as a quasi-synonym for “beauty” itself, so I will consider it thus, rather than as a sub-category of what *makes* a theory beautiful like the five properties listed by McAllister.⁶⁸

But how can one be sure that the properties listed above are indeed aesthetic, and not logico-empirical in disguise? McAllister offers two pieces of data. First, if scientists themselves describe such features in blatantly aesthetic terms, they may

⁶⁷ “I cannot understand...what it means to call a theory beautiful if it is not true” (quoted in McAllister 1996, p. 200). Bohr also commented that a theory’s aesthetic value can only be “properly judged after the event” (quoted in Feyerabend 1970, p. 99).

⁶⁸ Livio disagrees, defining elegance as a kind of ingenuity, so that a very complicated theory may be elegant, but fail to be beautiful as it is not simple—simplicity being a criterion he, like most scientists, considers to be necessary for beauty (Livio 2000, p. 31).

arguably be safely considered aesthetic (McAllister 1996, p. 36-37). Further, scientists are often on record as advocating using such criteria as “tie-breakers” between models that are empirically equal. McAllister writes, “If aesthetic criteria are to act as tie-breakers when theory-choice is underdetermined by the community’s set of logico-empirical evaluative criteria...they cannot themselves be logico-evaluative criteria...” (McAllister 1991a, p. 334).

I will now turn my attention to McAllister’s five classes of aesthetic criteria, providing examples of each in scientific practice, showing that they do indeed seem to play a significant role in same.

Form of Simplicity

It is hard to imagine a dictum more quoted in science than Ockham’s razor, propounded by William of Ockham in the 14th century: *Pluralitas non est ponenda sine necessitate*, or “entities should not be multiplied unnecessarily.” This principle of parsimony is almost universally valued among scientists; Einstein, for instance said, “in nature is actualized the ideal of mathematical simplicity” (quoted in Pais 1982, p. 467). Hideki Yukawa’s opinion was also that, “In essence, nature is simple” (quoted in Hovis and Kragh 1993, p. 104). Here Einstein and Yukawa echo Newton, whose opinions that “Nature is pleased with simplicity, and affects not the pomp of superfluous causes” and that “Nature...is wont to be simple, and always consonant to itself” he made part of his rules of reasoning in philosophy (Newton 1966 [1729], p. 398-399). We can trace similar quotes going all the way back to Aristotle, and perhaps even before, arguably, to (what some argue is) the dawn of science in Asia Minor in the 6th century BCE, in the very

notion that observations seen in nature are, underneath it all, the product of a combination of simple, regular causes.

Einstein's student Ilse Rosenthal-Schneider describes how simplicity criteria guided his development of general relativity, in which

the physical world is represented as a four-dimensional continuum, a Riemannian metric is adopted, and, in looking for the 'simplest' laws which such a metric can satisfy, [Einstein] arrives at his relativistic theory of gravitation of empty space. Adopting in this space a vector field, or the antisymmetrical tensor field derived from it, and again looking for the 'simplest' laws which such a field can satisfy, he arrives at the Maxwell equations for free space....[Also,] the general theory includes the special theory for the special limiting case of $g_{\mu\nu} = \text{const}$ (Rosenthal-Schneider 1949).

Thus, while mathematically formidable, relativity's simplicity "is to be understood as including the reduction of the logically independent basic elements...The logically simpler is not always the mathematically simpler" (Rosenthal-Schneider 1949). Simplicity criteria thus led Einstein to great truths about the world—extricated by the principle of parsimony from the thicket of tensor calculus and counterintuitive notions such as curved space—truths that have been confirmed in numerous experiments over the past ninety years.

Form of Symmetry

Symmetry has always been considered not only by scientists to be of enormous import, but also "has occupied a very important position in the history of human civilization" (Wu 1986, p. 19). To call a thing symmetrical is to suggest that it looks the same from certain different points of view; among the most significant of symmetries is the bilateral, or left-right, symmetry. Examples of objects with bilateral symmetry include snowflakes, starfish, crystals, and even the human face, all of which convey a

pleasing sense of balance to the human mind. Symmetries of laws of nature are called *invariance principles* (Wu 1986, p. 26) and essentially say that “when we make certain changes in the point of view of which we observe natural phenomena, the laws of nature we discover do not change” (Weinberg 1992, p. 137).

Maxwell’s equations are often touted for their symmetries, as is Einstein’s work on general relativity. Symmetry arguments are often invoked in modern elementary particle physics. As Anthony Zee writes, “To read His mind, [physicists] search their own minds for that which constitutes symmetry and beauty. In the silence of the night, they listen for voices telling them about yet-undreamed-of symmetries” (Zee 1986, p. 99). The discovery of the omega minus particle at Brookhaven in 1964 Mario Livio considers “a remarkable achievement of the human mind” as it was “the direct consequence of being guided entirely by symmetry, by the intuitive belief in underlying beauty” (Livio 2000, p. 88).⁶⁹

P.A.M. Dirac’s dramatic prediction of the existence of antimatter (starting with the positron), as is oft-told, was based on a symmetry discovered in the solutions to the equations of quantum theory. “The complete symmetry further impelled [Dirac] to admit the antiproton to the realm of theoretical existence,” which was also later confirmed, in 1955 (Hovis and Kragh 1993, p. 107).

Regarding Einstein’s *annum mirabilis* in 1905, Gerald Holton points out that all three of the scientist’s great papers began with aesthetic arguments based on symmetry:

⁶⁹ “Yuval Ne’eman, one of the two physicists who predicted the existence of the omega minus,” told Livio half seriously that “so strong was his conviction that the omega minus had to be discovered that he considered returning to the army if the experimental search had failed” (Livio 2000, p. 88).

[Einstein's] desire to remove an unnecessary asymmetry was not frivolous or accidental, but deep and important. At stake is nothing less than finding the most economical, simple, formal principles, the barest bones of nature's frame, cleansed of everything that is ad hoc, redundant, unnecessary.... In fact, sensitivity to previously unperceived formal asymmetries or incongruities of a predominantly aesthetic nature (rather than, for example, a puzzle posed by unexplained experimental facts)—that is the way each of Einstein's three otherwise very different great papers of 1905 begin. In all these cases, the asymmetries are removed by showing them to be unnecessary, the result of too specialized a point of view. Complexities that do not appear to be inherent in the phenomenon should be cast out. Nature does not need them (Holton 1988, p. 384).

Invocation of a Model

McAllister points out several instances in the history of science where invocation of a model, or analogy, is considered to increase a theory's aesthetic appeal. Laplace considered his theory of heat, while a mathematical model, superior to others (also mathematical in nature) because it "offered a model" of the phenomenon, namely, that of "heat as a fluid." The "style of theorizing in nineteenth-century physics known as mechanicism" would qualify as "invocation of a model as well," writes McAllister.⁷⁰ (McAllister 1996, p. 45-6)

Another prism through which to view this category is in terms of Gerald Holton's notion of "themata"—recurring pairs of essentially aesthetic themes and antithemes that he claims alternate dominating scientific thought. The theme of "mechanicism," for instance, might carry the day in one scientific epoch, whereas the opposing antitheme, say, vitalism or energeticism, might do the same in another era. See, for instance, (Holton 1988).⁷¹

⁷⁰ D. Davies objects, saying, "Relation to a model is not an aesthetic reaction...." (Davies 1998, p. 28).

⁷¹ Holton's thought can also be applied to McAllister's category of "metaphysical allegiance," as discussed below.

Visualization/Abstractness.

Mental images, says McAllister, “typically drawn from everyday experience,” guide our understanding. McAllister suggests the familiar picture of “two-dimensional surface curved in the third dimension” as a picture of non-Euclidean space as an example of this. Similarly, the quantum-mechanical quantity of spin, even though it has no macroscopic counterpart, is often pictured as rotation on an axis. At the other end of the spectrum, some scientists, McAllister points out, value abstractness, letting the abstruse mathematical equations of the theory speak for themselves (McAllister 1996, p. 49). McAllister suggests the difference between visualization and “invocation of a model” is the difference between metaphor and analogy (McAllister 1996, p. 51).⁷²

Metaphysical Allegiance

Perhaps the most controversial of the five of McAllister’s categories is this one. Breaking with philosophic tradition, McAllister proposes “to regard the allegiances that scientific theories have to metaphysical world views as aesthetic properties of them,” whereas the hitherto-standard view has been that “scientists’ aesthetic tastes are shaped by their metaphysical outlook.” McAllister justifies his inversion by noting that “a beholder who perceives an accord between the claims of a given theory and his or her metaphysical commitments is likely to experience a sense of aptness,” in line with his earlier criterion for an aesthetic quality. “Conversely, a theory whose metaphysical

⁷² Metaphors use one thing to mean another, “to see something as something else” (McAllister 1996, p. 51). Analogies, on the other hand, show similarities in things that might be different. Caloric theory, for instance, presented a model of heat as a fluid (i.e., it made an analogy), whereas August Kekulé’s visualization of a benzene ring as a snake swallowing its tail is more akin to a metaphor (McAllister 1996, p. 45, 52).

allegiance conflicts with the convictions of the beholder will elicit distaste.” His second justification for this is that “the procedure by which scientific communities form and update the metaphysical criteria on which they judge theories is identical to the procedure by which they choose” the other four aesthetic criteria described above (McAllister 1996, p. 55). So just as Einstein’s equivalence principle asserted that if in a closed spaceship one can’t tell the difference between a gravitational field and an acceleration, then there isn’t a difference, McAllister says that if communities treat metaphysical concerns in the same way that they do aesthetic ones, these too should be considered one and the same.⁷³

An example of a scientist who exemplifies McAllister’s view that metaphysical allegiance should be considered an aesthetic criterion is found in Mario Livio. In addition to simplicity and symmetry concerns, Livio says he finds a theory beautiful only if it obeys what he calls “The Copernican Principle,” namely, that “*we do not occupy a privileged place in the universe*” (Livio 2000, p. 30). “Scientists absolutely detest theories that require special circumstances,” says Livio, linking this criterion to the desire for a sense of inevitability in theories (Livio 2000, p. 35). In overtly aesthetic language, he describes violations of this “a slap in the face of all encompassing inevitability, and is therefore ugly” (Livio 2000, p. 36).

Many are critical of this view of metaphysical concerns as aesthetic. David Davies says McAllister’s view “misrepresents the manner in which appeals to metaphysical criteria typically function in scientific reasoning.” He suggests that

⁷³ Or, more precisely, metaphysical allegiance should be considered a subset of aesthetics.

metaphysical allegiances “express a substantive claim about how the world is...[but] *it is not in virtue of these aspects* that such allegiances serve as a criterion of theory choice (nor...is it clear why such affective aspects should be deemed ‘aesthetic’)” (Davies 1998, p. 28). That is, metaphysical claims are about the fundamental nature of the world, and thus, in Davies’ view, must therefore underlie all aspects of a scientist’s approach, including his sense of aesthetics. Therefore metaphysical allegiances would not be invoked *explicitly* as criteria for theory choice—in an aesthetic sense or otherwise—as the metaphysical allegiances would precede the aesthetic criteria, and indeed all facets of the scientist’s theory development and choice. They are “one layer down,” if you will, from all other considerations.

So, do metaphysics precede aesthetics, or are metaphysical allegiances a manifestation of aesthetic criteria? It seems to me there is a chicken-and-egg problem here. There are a great number of scientists who hold as their most fundamental belief that “the universe is simple,” and this core belief is what underlies their work at its most basic level.⁷⁴ This would seem to qualify as a kind of metaphysical allegiance in Davies’ sense, something that “precedes” everything else. Now, I posit that most would agree that “simplicity” is indeed an aesthetic criterion. Therefore it seems to me that McAllister’s view that we treat metaphysical allegiances—while certainly the most controversial of his five criteria—is not unreasonable, especially considering that I believe, as McAllister does, that metaphysical criteria are “updated” in the same way as the other four aesthetic categories, such as form of symmetry. In other words, if it looks

⁷⁴ Presently we will see quotations to this effect from Einstein, Newton, Yukawa, and many others.

like a duck, and quacks like a duck—maybe it’s a duck. So for the purpose of this paper I will adopt McAllister’s view on this subject, though with the concession that it is the most problematic of his five aesthetic criteria.

The many aesthetic “success stories” based on all five of the above aesthetic criteria have led quite a few scientists of the first order to suggest that aesthetically pleasing theories are more likely to be true than “ugly” ones. In other words, in scientific theorizing, beauty is a symptom of truth.⁷⁵ Consider the following passage by James Watson, concerning Rosalind Franklin’s reaction to the proposed double helix structure of DNA:

Rosy’s instant acceptance of our model at first amazed me. I feared that her sharp, stubborn mind, caught in her self-made antihelical trap, might dig up irrelevant results that would foster uncertainty about the correctness of the double helix. Nonetheless, like almost everyone else, she saw the appeal of the base pairs and accepted the fact that the structure was too pretty not to be true (Watson 1968, p. 134).

Or, consider the following passage by Werner Heisenberg (from a conversation with Einstein):

I believe...that the simplicity of natural laws has an objective character, that it is not just the result of thought economy. If nature leads us to mathematical forms of great simplicity and beauty—by forms I am referring to coherent systems of hypotheses, axioms, etc.—to forms that no one has previously encountered, we cannot help thinking that they are ‘true,’ that they reveal a genuine feature of nature. It may be that these forms also cover our subjective relationship to nature, that they reflect elements of our own thought economy. But the mere fact that we could never have arrived at these forms by ourselves, that they were revealed to us by nature, suggests strongly that they must be part of reality itself, not just of our thoughts about reality.

⁷⁵ Brush points out that this common belief among scientists severely undercuts the social constructionist argument that social factors have to be invoked to explain scientists’ choices amongst allegedly-underdetermined theory alternatives. “Although some historians and sociologists seem to believe that empirical (or ‘positivist’) and social factors are the only alternatives for explaining how scientists choose theories, other students of (and participants in) the scientific enterprise have stressed the importance of a third factor: the belief that a theory must be correct because it is mathematically elegant, aesthetically pleasing, and expresses a necessary truth about nature” (Brush 1999, p. 191).

You may object that by speaking of simplicity and beauty I am introducing aesthetic criteria of truth, and I frankly admit that I am strongly attracted by the simplicity and beauty of the mathematical schemes which nature presents us. You must have felt this, too: the almost frightening simplicity and wholeness of the relationships which nature suddenly spreads out before us and for which none of us was in the least prepared. And this feeling is completely different from the joy we feel when we have done a set task particularly well (Heisenberg 1971, pp. 69-70).

In 1919, when his student Ilse Rosenthal-Schneider handed Einstein a telegram from H.A. Lorentz validating the results of Eddington's 1919 eclipse test of general relativity, Einstein reportedly put it aside, saying that he *already knew* the theory was correct. When she asked what he would have done if the telegram had given the other answer, he replied, "Then I would have been sorry for the dear Lord—the theory *is* correct." (quoted in Holton 1988, p. 255) The strong implication here, again, is that pleasing aesthetics are a reliable indicator of truth. Einstein does acknowledge the apparent slipperiness of aesthetic qualities ("an exact formulation...meets with great difficulties") but goes on to say, "It turns out among the 'augurs' there is usually an agreement in judging in the 'inner perfection'" (Einstein 1949a, p. 23-5).

Einstein reaffirmed this at a 1934 speech at the University of Oxford:

Nature is the realization of the simplest conceivable mathematical ideas. I am convinced we can discover by means of purely mathematical constructions the concepts and laws connecting them with each other, which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts, but they most certainly cannot be deduced from it. Experience remains, of course, the sole criterion of the physical utility of a mathematical construction. But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed (Einstein 1956, p. 274).

Perhaps the most outspoken scientist on the matter of aesthetics in theory choice was P.A.M. Dirac. At the University of Moscow, where visiting VIP physicists are asked

to leave an aphorism on a blackboard, Dirac chose “A physical law must possess mathematical beauty.” In 1963 Dirac wrote that

It is more important to have beauty in one’s equations than to have them fit experiment....It seems that if one is working from the point of view of getting beauty in one’s equations, and if one has really a sound insight, one is on a sure line of progress. If there is not complete agreement between the results of one’s work and experiment, one should not allow oneself to be too discouraged, because the discrepancy may be due to minor features that are not properly taken into account and that will get cleared up with further developments of the theory (Dirac 1963, p. 47).

In 1980, Dirac wrote of general relativity:

The Einstein theory of gravitation has a character of excellence of its own. Anyone who appreciates the fundamental harmony connecting the way Nature runs and general mathematical principles must feel that a theory with the beauty and elegance of Einstein’s theory *has* to be substantially correct. If a discrepancy should appear in some application of the theory, it must be caused by some secondary feature relating to this application which has not been adequately taken into account, and not by a failure of the general principles of the theory (Dirac 1980a, p. 44).

It is the essential beauty of the theory which I feel is the real reason for believing in it. This must dominate the whole future development of physics. It is something which cannot be destroyed.... (Dirac 1980b, p. 10).

Quotes like this from Dirac abound: “A theory with mathematical beauty is more likely to be correct than an ugly one that fits some experimental data” (quoted in Hovis and Kragh 1993, p. 104). “Schrödinger and I both had a very strong appreciation of mathematical beauty....It was a sort of faith with us that any equations which describe fundamental laws of Nature must have great mathematical beauty in them” (quoted in Hovis and Kragh 1993, p. 107).⁷⁶

Hermann Weyl once said, “My work always tried to unite the true with the beautiful; but when I had to choose one or the other, I usually chose the beautiful.” Of

⁷⁶ S. Weinberg comments: “I once heard Dirac say in a lecture, to an audience which largely consisted of students, that students of physics shouldn’t worry too much about what the equations of physics mean, but only about the beauty of the equations. The faculty members present groaned at the prospect of all our students setting out to imitate Dirac” (quoted in McAllister 1996, p. 90).

this, Mario Livio points out of this that there “exist at least two known examples, one in relation to gravity and one to the neutrino, in which Weyl’s aesthetic sensibility proved right, in spite of early apparent contradictions with the prevailing wisdom” (Livio 2000, p. 254).

Nor is this sentiment restricted to scientists of yesteryear. A recent book by Livio, the head of the Space Telescope Science Institute, argues “that the laws of physics *are* actually determined largely by aesthetic principles” and suggests “a new principle—the *cosmological aesthetic principle*...[which requires that] fundamental theories of the universe ... should be beautiful” (Livio 2000, p. 11, 263). The passion that many modern scientists vocalize for string theory is based entirely on elegant mathematics, without a whit of experimental proof. The list goes on.

Thus many prominent scientists have written not just on the desirability of “beautiful” theories, but rather on the fact that the development of a beautiful theory is a sign of progress toward discovering truth about the world. But is this faith justified? This is the question I will explore in the next section.

Pulchritudo Splendor Veritatis?

Is beauty the splendor of truth? In the previous section I covered several celebrated cases in the history of science in which purely aesthetic concerns certainly seemed to lead the way to accurate knowledge about the world. As noted, the apparent

success of such reasoning led many scientists, particularly Dirac, to conclude that positive aesthetic features *inevitably* yield truth, or at the very least should be considered superior to other forms of evaluation when considering a theory.

However, a look at the historical record yields ample evidence that this is in fact not inevitably so. In this section I point out a number of instances where aesthetic reasoning led scientists, so to speak, down the primrose path.

Geocentricity

Symmetry arguments have been made in support of scientific models, going all the way back to the dawn of science; these were often made to justify various geocentric/geotstatic models of the cosmos. Aristotle, for instance, cites Anaximander's argument for the centrality of the earth in the cosmos, based on symmetry:

There are some, Anaximander, for instance, among the ancients, who say that the earth keeps its place because of its indifference. Motion upward and downward and sideways were all, they thought, equally inappropriate to that which is set at the centre and indifferently related to every extreme point; and to move in contrary directions at the same time was impossible: so it must needs remain still (*On the Heavens*, Book II, 295v10ff. DK 12A26, in Barnes 1984).⁷⁷

As we know, the earth does not sit immobile at the center of the cosmos, but like all the planets, revolves around the sun in an elliptical orbit as first shown by Kepler.⁷⁸

Conservation of Parity

One of the most notable misfires in 20th Century physics was based on a symmetry argument. Originating with the development of quantum mechanics, *parity*—

⁷⁷ Though Aristotle himself was also a geocentrist, he concludes in the very next line, “This view is ingenious, but not true.” N.B. this portion of the Barnes volume (*On the Heavens*) was translated by J. L. Stocks.

like baryon number, spin, and strangeness—is a nonclassical concept introduced to help characterize the properties of subatomic entities. Specifically, it is used to describe wave functions as even (parity = 1) or odd (parity = -1), depending on their symmetries. In the 1920's, parity had been shown to be conserved in atomic transitions. In 1927, Eugene Wigner showed that this principle of parity invariance was a consequence of the mirror-image symmetry of the electromagnetic force. That is, “the conservation of parity is the direct consequence of the law of left-right symmetry” (Wu 1986, p. 27).

Thus the supremely intuitive notion that nature and its laws should be thus symmetrical was carried over into quantum mechanics; “for any atomic or nuclear system, no new physical law should result from the construction of a new system differing from the original by being a mirror image....there is no absolute distinction between a real object (or event) and its mirror image” (Wu 1986, p. 27).

However, a puzzling anomaly called the “Tau-Theta problem” in the 1950's (ultimately) led Tsung Dao Lee and Chen Ning Yang to propose the unthinkable—that parity was in fact not conserved in reactions that involve the weak nuclear force. It turned out, they found, that parity conservation for this kind of reaction *had simply been assumed* based on symmetry and had never been tested. Their radical idea was ultimately borne out in experiments, notably by Chien-Shiung Wu. Thus it was shown that nature possesses a fundamental asymmetry, or, “If a certain phenomenon can happen, its

⁷⁸ Or, I should say, as most of us know. There are still holdouts to this view, including within the STS community.

translated or rotated version can also happen, but not its mirror-reflected version, if the phenomenon involves weak interactions” (Shankar 1980, p. 311).

A number models and theories had been rejected prior to this discovery, based simply on the grounds that they entailed parity nonconservation.

“Island Universes”

For centuries, a debate raged among astronomers over whether so-called “nebulae” seen through telescopes were merely nearby clouds of gas, or were really entire other Milky Ways—enormous “island universes” seen at great distances. The answer to this question was of critical import; if the latter was the case it meant that the Milky Way occupied a small and, in fact, insignificant part of a much larger universe.

Of course, we now know that some of the “nebulae” are just that—clouds of gas in our Milky Way—whereas others are indeed distant galaxies. However, this seemingly straightforward solution to the (some are of this character, some of that) was considered unacceptable on aesthetic grounds. As Robert Smith writes, “Many astronomers, through a desire to adopt the simplest possible hypothesis, had taken up one of two extreme positions: *all* unresolved nebulae are systems of stars or *all* unresolved nebulae are masses of true nebulosity” (Smith 1982, p. 1).

So, for instance, once Lord Rosse (with the aid of his 72-inch telescope) established in the 1840’s that many nebulae were resolvable into individual stars, astronomers’ application of Ockham’s Razor to the problem meant that for the remainder of the century, the suggestion that there were “masses of true nebulosity” in our stellar system was considered disreputable. Other aesthetic factors were cited to back the

“island universe” model as well. Smith writes of H. Curtis’s “reverence for the ‘grandeur and majesty’ of the theory, and for him this may have been as important as the available observational evidence” (Smith 1982, p. 29). Later, when faced with an estimate of only 10,000 light-years for the average distance to the spiral nebulae (which would damn the model), Curtis came up with various justifications to impeach this measurement, rather than give up his elegant theory (Smith 1982, p. 30-1).

Only “in the last decades of the nineteenth century” did this interpretation start to lose traction, and “by 1910 astronomers had thrown off the shackles that had bound their nineteenth century predecessors to this restrictive belief” (Smith 1982, p. 15).

Later, Ockham’s Razor led astronomers astray again in this same controversy. Harlow Shapley had estimated a size for the Galaxy (i.e., our Milky Way), which was at odds with the estimates of the island universes. Thus

the belief that nature must be simple...was influencing the island universe debate since most advocates...had actually adopted a comparable-galaxy theory....This assumption, which ignored the diversity in size within other classes of astronomical body, from planets to stars to clusters of stars, now created difficulties for the island universe theory.... (Smith 1982, p. 66-7).

Thus the presupposition that “nature is simple” retarded for some time the advancement of knowledge regarding the nature and size of the universe.

The comma-free code

The discovery of DNA in 1953, while dramatic, was far from the last puzzle in molecular biology. Understanding the code contained in the double helix—i.e., how DNA expressed its genes—was a significant problem left to solve. Cracking it was a mathematical problem, involving ideas from combinatorics and information theory. In

response, in 1957 Francis Crick proposed the “comma free code,” a solution that “seemed at once so clever and so obvious that it just *had* to be right” (Hayes 1998, p. 11).

The problem was this: given a string of nucleotide bases in DNA or RNA, how does one discern where one codon (i.e., genetic code-word, or triplet of bases) begins and ends? For instance, the string GCAUUGACCCUU could be read: GCA, UUG, ACC, CUU—or it could be read GC, AUU, GAC, CCU, U—or any number of other ways, each with a completely different meaning. That is, errors in reading were possible due to being “out of phase” or “frame shifted” with the beginning of each three-letter code word.

The “pretty, almost elegant” comma-free code solved this problem with “pleasing ingenuity” (Darden 1998, p. 9). Though there are 64 (4^3) possible codons, Crick suggested that only a small subset of these were actually meaningful, with the rest being “nonsense codons.” He then “constructed a code in such a way that when any two meaningful codons are put next to each other, the frame-shifted overlap codons are always nonsense.” Crick and his coworkers worked out that of the 64 possible codons, a comma-free code would have only 20—precisely the number of amino acids (Hayes 1998, p. 11-12).

Though Crick was careful to point out that the comma-free code was not yet experimentally verified by any data, he did extol its “aesthetic properties” (Darden 1998, p. 9).⁷⁹ Brian Hayes writes that the “magic number” of 20 amino acids

⁷⁹ I thank Barbara Reeves for reminding me that Crick trained as a physicist—he held a B.Sc. in physics, and had started Ph.D. work in the field before being interrupted by World War II. After leaving the military in 1947, he switched to biology.

was enough to persuade both biologists and the wider public. [Microbiologist] Carl Woese later wrote, ‘The comma-free code received immediate and almost universal acceptance....simply because of...intellectual elegance....For a period of five years most of the thinking in the area either derived from the comma-free codes or was judged on the basis of compatibility with them.

The “intellectual elegance” of the code also “attracted the attention of coding-theory professionals...going on to explore more abstract and generalized ideas” (Hayes 1998, p. 12).

However, in 1961, Johann Matthaei and Marshall Nirenberg of NIH showed that the amino acid phenylalanine was decoded as being UUU—a codon which was a “nonsense” codon in the comma-free code (since placing two UUU strings together would produce ambiguity). The code was not comma-free. As Darden writes, “Nature’s code is less elegant than Crick’s because some of the twenty amino acids are coded for by more than one triplet, a messy result excluded by Cricks’ comma-free code with its ‘magic number’ of twenty” (Darden 1998, p. 9).

H.F. Judson called the comma-free code the “most elegant biological theory ever to be proposed and proved wrong” (quoted in Darden 1998, p. 2). Hayes writes, “It was hard not to feel a twinge of regret...Compared with the elegant inventions of the theorists, nature’s code seemed a bit of a kludge” (Hayes 1998, p. 8).

Einstein on Quantum Mechanics

A classic case of aesthetic concerns’ driving a scientist’s work is seen in Einstein’s reaction to quantum mechanics, *vis-à-vis* his own relativity theory. Since the two theories resisted all attempts (in Einstein’s opinion) at reconciliation, a choice, in a sense, had to be made between the two. In spite of the empirical superiority of quantum

mechanics over relativity, Einstein picked his own theory because *he saw it as more beautiful*.

Why was this? Though there are many quotes to the effect that Einstein saw relativity as simpler, more symmetrical, and more analogically interpretable than quantum mechanics, I believe the most important aesthetic criterion that Einstein applied was that quantum mechanics was inconsistent with certain of his metaphysical presuppositions. Einstein believed firmly in separability and realism. He simply could not fathom a universe without these properties, as the orthodox interpretation of quantum mechanics seemed to imply, and strove exhaustively to show that it indeed could not be that way.

Furthermore, his metaphysical presuppositions on what the role of a physicist was and what a physicist's role was (getting close to the secrets of the “old one”) were hinged on the fact that there had to be a separable, independent, objective reality that was fundamentally nonprobabilistic. These notions affected him to the core of his being, as evidenced in his belief in non-anthropomorphic God (not unlike that of Spinoza)—a “harmony” in the universe that one could and should strive to appreciate.

Thus Einstein lived out the remainder of his days fighting against the extraordinarily successful science he founded in 1905 with his paper on the light quantum.

Cosmological Constant

One of the most oft-cited episodes in the role of aesthetics in science comes from the early 20th century. In 1917 Einstein applied his field equations of general relativity to

the universe as a whole, (arguably) founding cosmology as a science. However, he found his equation predicted a dynamic universe (either expanding or contracting)—which was then in conflict with the observational data all of which indicated the universe was static.

To remedy this, Einstein added a term to his equation, Λ , the “cosmological constant.” This term preserved general covariance of the equation, but had the effect of canceling out the dynamism of the universe. In particular, since it was reasonable to assume that the combined mass of the universe should cause a gravitational collapse, the Λ term was interpreted as a repulsive force, which increased with distance between objects, to counteract this. Of this, Einstein wrote:

We admittedly had to introduce an extension of the field equations of gravitation which is not justified by our actual knowledge of gravitation....That [cosmological] term is necessary only for the purpose of making possible a quasi-static distribution of matter, as required by the fact of the small velocities of the stars (Einstein 1986 [1917], p. 26).

Yet, despite the Λ term’s practical success in meshing theory with observation, Einstein was never happy with the term in his field equations, seeing it as “contaminating the purity of their beauty” (Hoffman 1972, p. 212). As Rosenthal-Schneider writes, the cosmological constant “seemed to disturb the logical coherence and homogeneity of the system. That is why [Einstein] welcomed every suggestion which promised a way out of the dilemma....” (Rosenthal-Schneider 1949, p. 139). Smith points out that Einstein wrote as early as 1919 that he “hoped soon to expunge it from his field equations” due to its having “impaired the simplicity and elegance he believed all fundamental physical equations should possess” (Smith 1982, p. 170).

Also around 1917, Vesto M. Slipher established that the clusters of the so-called “spiral nebulae” were all redshifted—and therefore receding from us. By 1929 Edwin P.

Hubble had established that these “spiral nebulae” were nothing of the sort, but in fact entire other galaxies of stars like our own Milky Way, seen at great distances. The two discoveries, together, implied an expanding universe.

Once this result was well-established, Einstein’s distaste for the cosmical term grew; by 1931 he had decided it was “theoretically unsatisfying” (Berenstein and Feinberg 1986, p. 13). By 1932 he removed the cosmological constant from his field equations altogether. Of this, Einstein wrote of the constant that

The introduction of $[\Lambda]$ implies a considerable renunciation of the logical simplicity of theory, a renunciation which appeared unavoidable only so long as one had no reason to doubt the essentially static nature of space....The introduction of such a constant appears to me, from the theoretical standpoint at present unjustified.

And in 1950, he wrote:

If Hubble’s expansion had been discovered at the time of the creation of the general theory of relativity, the cosmologic member would never have been introduced (quoted in Berenstein and Feinberg 1986, p. 13).

Gamow recalled in his book *My World Line*:

Much later, when I was discussing cosmological problems with Einstein, he remarked that the introduction of the cosmological term was the biggest blunder he ever made in his life. But the “blunder,” rejected by Einstein, and the cosmological constant...rears its ugly head again and again (quoted in Clark 1971, p. 215).

For decades, the story of the cosmological constant was held up as a cautionary tale in favor of erring on the side of formal beauty in one’s mathematical work, and against so-called “fudge factors” in theories to bring them in line with experimental results. Henry Margenau, for instance, characterized Λ as an “unwelcome sacrifice” (Margenau 1949, p. 257). Leopold Infeld called it an ad hoc hypothesis, and called “valid” the objections to it (Infeld 1949, p. 480-1). Eddington wrote of its inclusion that it was “not very convincing, and for some years the cosmical term was

looked on as a fancy addition rather than an integrated part of the theory” (quoted in Lemaître 1949, p. 443).

Trust your aesthetic instincts, even in the face of conflicting data, was the lesson.

The fact that, in a moment of weakness, Einstein did not do so meant he missed out on what would have been perhaps the most dramatic prediction in the entire history of science—that of a dynamic universe. This lost opportunity was grieved by generations of physicists. “If only he hadn’t second-guessed himself, it would have been another feather in his cap....I don’t know how he could have missed it,” physicist Joseph Weber once remarked (Weber 1987).

On the other hand, some other scientists took a different view, pointing out that since the Λ term did not alter the covariance of the theory, that the theory is actually more general with the constant left in it. In other words, an arbitrary choice of *precisely zero* for the cosmological constant, out of an infinity of possibilities, is really the ad hoc hypothesis, rather than the ad hoc hypothesis’ being the inclusion of the constant to begin with. An early espouser of this view was Georges Lemaître, who was the first who, even after the discovery of the expansion, “made the straightforward generalization to include a pressure term” anyway (Berenstein and Feinberg 1986, p. 12). Lemaître later wrote,

The structure of [Einstein’s] equations quite naturally allows for the presence of a second constant besides the gravitational one. This raises a problem and opens possibilities which deserve careful consideration. The history of science provides many instances of discoveries which have been made for reasons which are no longer considered satisfactory. It may be that the discovery of the cosmological constant is such a case (Lemaître 1949, p. 443).

Lemaître then went on to make a case for the utility of the constant, in addressing problems of the day, including the time-scale problem mentioned in Chapter 2.⁸⁰ Thus many scientists saw the Λ term as valuable, practically and aesthetically; various inflationary theories⁸¹ of the early universe later made use of a non-zero cosmological constant, for instance. Yet many scientists still shared Einstein’s view. Mario Livio writes,

Most pre-1998 guesses as to the value that will eventually be found for the cosmological constant put it precisely at zero. This is actually an interesting psychological reaction. It is not that anybody *knew* the value has to be zero, but that simple estimates gave such unreasonably large numbers that most physicists felt that the only acceptable value would be zero. In a scorecard composed in 1998 by the Princeton cosmologist Jim Peebles, he noted that cosmological models involving a cosmological constant fail the test of aesthetics; and the University of Chicago cosmologist Rocky Kolb called such models “unspeakably ugly” (Livio 2000, p. 129).

However, where the Λ term really returned to the fore was with the 1998 discovery that, based on observations of distant supernovae, the universe’s expansion is accelerating—thus necessitating not only a cosmological constant in the field equations but a rather large one at that. In fact, the “dark energy” (i.e., vacuum energy density) causing this acceleration accounts for most of the stuff of the universe—over seventy percent, according to recent estimates. Yet this “apparent resurrection of the cosmological constant is not welcomed by many physicists,” writes Livio (Livio 2000, p. 237).

⁸⁰ Einstein said of this argument, “These arguments do not appear to me as sufficiently convincing in view of the present state of our knowledge” (Einstein 1949b, p. 684).

⁸¹ These models, first proposed by Alan Guth in 1981, suggest that well within a second after the big bang, the early universe underwent a vast and sudden (exponential) inflation. This was to explain why regions in the universe which ought to have been causally unconnected in the standard big-bang scenario are observed to have such similarities to each other—before the inflationary epoch they *were* causally connected.

Thus Einstein's "ugly" choice, so long lamented, and then a topic of such aesthetic hand-wringing in the scientific community, turns out to have been correct after all, and its confirmation in fact is one of the breakthrough experimental discoveries of the latter half of the 20th century. And yet it is lamented by many, still, on aesthetic grounds.

Omega

Another good contender of aesthetic reasoning in cosmology is in regard to Ω —the parameter that will determine the universe's fate. "What will the universe do in the future?" writes Livio. "Perhaps never in the history of physics have aesthetic arguments played a more dominant role than in the attempts to answer this question" (Livio 2000, p. 105).

Ω is the ratio of the density of the universe to the critical density needed to close the universe. If Ω is greater than 1, the universe is closed (and therefore one day headed to a "Big Crunch"), and has positive curvature. If Ω is less than one, the universe is open, will continue to expand forever, and has negative curvature. If Ω is exactly equal to one, the universe is "flat," meaning it has no overall curvature, and its expansion will eventually "coast" to a halt (albeit in an infinite amount of time).

Inflationary theory (or at least the simplest version of it) predicts that omega should be exactly equal to one; that is, the universe is flat. Livio writes of this,

Many (if not most) cosmologists maintained that the most likely cosmological model is the one in which the density of the universe is exactly equal to the critical density. This prejudice was not formed on the basis of compelling theoretical arguments, but rather largely on the basis of aesthetic arguments (Livio 2000, p. 12).

Various measurements of fluctuations in the cosmic microwave background radiation have confirmed that the universe is indeed flat, to within a two percent margin of error. That is, ω is indeed about one, just as the (at-least-partially) aesthetically based arguments for inflation suggested.

The problem is that the aesthetic sense of many cosmologists told them that ω should be almost entirely due to the density of the universe's *matter*. Instead, the majority of this value was found to be not due to the density of matter in the universe, but rather to the cosmological constant (i.e., due to the vacuum, as discussed previously). So, even though the observations jibe almost exactly with the theoretical (and aesthetically driven) prediction, then, Livio and others find this unacceptable, as the following quotes attest:

The possibility that the density of matter is smaller than critical and the potential existence of this cosmic repulsion also contradict notions of what a 'beautiful' theory of the universe should look like (Livio 2000, p. 13).

Most physicists are not thrilled...by the prospect of having to rely on the energy of empty space, or the cosmological constant, to achieve flatness. Most physicists, if asked for their biased preference, would much rather have the ω of matter be equal to one without a need for a poorly understood cosmological constant (Livio 2000, p. 159).

The results of the two supernova projects (if fully confirmed) appear to rule out at a very high confidence level the possibility that ω of the matter is equal to 1.0, the aesthetically preferred result (Livio 2000, p. 166).

Thus even the triumphant confirmation of one of the most abstruse, counter-intuitive, and often-derided theories in science is seen as a failure (at least by some) because of a certain aesthetic view. Livio's tearing of hair and rending of garments continues:

What does all of this mean? Is it possible that we have come all this way, where in every step along the path our belief in the beauty of the universe has only been strengthened, to see it all collapse at the very end? I remember the first time that this realization hit me, in

early 1998. I had a feeling in my stomach similar to the one I had in 1975, when I heard that somebody had carried a knife into the Rijksmuseum in Amsterdam and managed to gouge twelve deep slashes into Rembrandt's masterpiece *The Night Watch* (Livio 2000, p. 195).

Chaos

Pierre Simon Laplace famously observed in 1814 in his *Philosophical Essay on Probabilities* that if one could know the position and momentum of every particle in the universe, given Newton's Laws of Motion and Gravity, that one could then calculate with exact certainty the state of the universe at any future or past time. For such an observer, "nothing would be uncertain, and the future, as the past, would be present to its eyes" (quoted in Holton and Brush 2001, p. 323).

We now know, of course, that Heisenberg's Uncertainty Principle (HUP) rules out this scenario even as an idealization, as the exact position and momentum of a particle may not be simultaneously known. But even if HUP were not true, in the *real* world the completely precise measurements on which Laplace's scenario would rest would be impossible to obtain; in any real measurement there is always some margin of error, tiny though it might be. However, the traditional view of such miniscule errors is that they are trivial; tiny errors in the initial conditions of a system should cause only tiny deviations from its predicted final state. Thus even if Laplace's observer could obtain only the *approximate* positions and momenta of all the particles in the universe, he could still, in the traditional view, still reasonably calculate the *approximate* state of the universe for any future or past time.

However, it turns out that for many systems this commonsense notion simply does not apply. Such systems possess "sensitive dependence on initial conditions"

(SDIC); unlike most traditional systems, very small deviations cannot be ignored as they can “build up” and influence the final state of the system significantly.⁸² SDIC was anticipated as early as Poincaré, but after his death it was largely ignored (Poincaré 2001 [1914]).

One reason for this is that the equations that describe such systems often contain nonlinear terms, which are notoriously difficult with which to deal. Nonlinear equations in fact usually do not possess closed-form solutions at all; this inclined scientists to ignore such “pathological” systems in favor of linear ones that admitted exact solutions. For instance, for the scientist Joseph Keller, “it was the ‘completeness’ and ‘beauty’ of the solutions of linear equations that led to their ‘domination of the mathematical training of most scientists and engineers’” (Kellert 1993, p. 145). SDIC was seen as an “aberration” that “signaled to the researcher that the mathematical model [in question] was defective” (Kellert 1993, p. 44).

In 1961, the Massachusetts Institute of Technology meteorologist Edward Lorenz rediscovered the types of systems Poincaré had discussed. In performing simulations of the weather on an early electronic computer, he found that tiny differences in data input produced radically different outputs from the program, contrary to contemporary thinking. His 1963 article “Deterministic Nonperiodic Flow” on SDIC in fluid convection introduced to the world what is now called the Lorenz attractor, a graphical representation of the chaos inherent in such systems.

⁸² This phenomenon is sometimes referred to as the “Butterfly Effect,” so-called due to a famous paper (“Predictability: Does the Flap of a Butterfly’s Wings in Brazil Set Off a Tornado in Texas?”) given by Edward Lorenz at the 1979 AAAS meeting in Washington, DC.

However, even with the work of Lorenz and others such as Stephen Smale, nonlinear dynamics really didn't begin to flower until the 1970's; prior to this period, scientists who turned to study this field each "had a story to tell of open discouragement or hostility" from their colleagues (Gleick 1988, p. 37). The field became known as "chaos theory" thanks to a widely-read paper in *American Mathematical Monthly* entitled "Period Three Implies Chaos," written by the University of Maryland's James Yorke. By the 1980's, the term "chaos" had become a "shorthand for a fast-growing movement," "chaos conferences and chaos journals" abounded, and institutions dedicated to the study of chaos "appeared on university campuses across the country" (Gleick 1988, p. 4).

Yorke believes that scientists had learned "not to see" chaos in systems. But as James Gleick puts it, "Now that science is looking, chaos seems to be everywhere" (Gleick 1988, p. 5). Thus the perception of "ugliness" of the nonlinear equations of such systems delayed the appreciation and study of such systems for several decades.

Dirac on Quantum Mechanics and QED.

As mentioned in the previous section, one of the most commonly-cited illustrations of a theory's aesthetics leading the way to truth is Dirac's prediction of the existence of the positron, based on symmetry concerns. The story is often-told: in 1929 Dirac realized that his eponymous equation "pertained not only to familiar, positive-energy electrons, but also to electrons having *negative* energy." This led to a number of theoretical difficulties, which he rectified with his theory of "holes." This is described by Hovis and Kragh thus: "[Dirac] imagined the vacuum to consist of a uniform "sea" of

negative-energy states all *filled* by electrons. Since the Pauli Exclusion Principle prohibits two electrons from occupying the same quantum state, positive-energy electrons would be kept above the invisible sea, to form the ‘excited’ states seen in nature” (Hovis and Kragh 1993, p. 107).

However, then the question was: what particles physically corresponded to Dirac’s “holes?” Two choices presented themselves: either the proton, or an entirely new particle—the positive electron (later “positron”). However, based on aesthetic concerns, *Dirac initially chose the former*. He considered it the “simpler” model, since it did not require positing an entirely new particle (and thus increasing the subatomic bestiary by 50%, since at that time the only material particles known were the proton and the electron).⁸³ Indeed, since (if correct), this model implied protons were simply “negative energy states vacated by electrons,” this in fact reduced the number of types of subatomic particles in the universe to one—the electron. “Such a simplification would be the ‘dream of philosophers,’ Dirac declared.” But “the objections to his initial interpretation of holes soon became overpowering, and in May 1931 he settled, reluctantly, on the second candidate for the hole, the antielectron (i.e., positron)...” (Hovis and Kragh 1993, p. 107).⁸⁴

⁸³ Dirac’s interpretation of Ockham’s Razor here was not shared by all contemporaries. Rutherford once told an audience of his belief that “the nucleus must be simple in structure. ‘I’m always a believer in simplicity, being a simple person myself,’” he said. Yet he still insisted the nucleus must contain a hitherto undiscovered, neutral particle, the neutron, later discovered by his “right-hand man,” James Chadwick (quoted in Kevles 1995, p. 224).

⁸⁴ In yet a further aesthetic kerfuffle, once Carl D. Anderson experimentally verified the positron’s existence in 1932, he “ran into a wall of resistance....The positron seemed simply to complicate matters. It is said that Niels Bohr dismissed Anderson’s finding out of hand, and when in the fall of 1932 Millikan

Later, Dirac suggested in 1936 that certain difficulties the quantum mechanics of the day faced in dealing with beta decay and other issues indicated that

The present quantum mechanics, with its conservation of energy and momentum, forms a satisfactory theory only when applied non-relativistically, to problems involving small velocities, and loses most of its generality and beauty when one attempts to make it relativistic. In this way...we see the need for a profound alteration in the current theoretical ideas, involving a departure from the conservation laws, before we can get a satisfactory relativistic quantum mechanics (quoted in Leplin 1975, p. 341).

Thus aesthetic factors in this case led Dirac to propose abandoning strict conservation of momentum and energy—a move that proved to be unnecessary and unfounded.

Hovis and Kragh also note that Dirac never accepted quantum electrodynamics, calling the theory “illogical and ‘ugly.’” The renormalization procedure that Richard Feynman, Julian Schwinger, and Freeman Dyson invented to banish infinities from QED Dirac rejected, despite its success, calling it “complicated and ugly.” This attitude caused him to become “isolated in the physics community” at the end of his life as he struggled unsuccessfully to replace the QED he saw as aesthetically lacking (Hovis and Kragh 1993, p. 108).

Thus here we see three instances where *the primary proponent* of aesthetic reasoning in science was led astray by such reasoning.

The problem, of course, is that given any aesthetic criterion, there are multiple ways of interpreting it. It’s very easy to trot out Ockham’s Razor and proclaim that the “simpler” model is to be preferred when faced with two competing theories—scientists

discussed the positron in a lecture at the Cavendish, various members of the audience coldly suggested that Anderson had doubtless become tangled in some fundamental interpretive error” (Kevles 1995, p. 233).

certainly do this all the time. But how does one judge of two competing models which is “objectively” simpler? There are, after all, differing degrees and kinds of simplicity (McAllister 1991b).

Is Aristotelian chemistry simpler than modern chemistry because it has only four elements, or is modern chemistry simpler because it foregoes notions of innate qualities for matter? Are mathematical theories with fewer variables to be preferred, or ones with lower orders of polynomials? Is a theory that uses algebra simpler than one that uses the tensor calculus? Is Copernicus’s model simpler than Ptolemy’s because it has 34 epicycles instead of 80, or is Ptolemy’s simpler because it avoids the *ad hoc* hypothesis of Copernicus that the sphere of the fixed stars is much further away than the planets’ spheres?⁸⁵ Regarding this, Butterfield points out that

at least some of the economy of the Copernican system is rather an optical illusion of more recent centuries. We nowadays may say that it requires smaller effort to move the earth round upon its axis than to swing the whole universe in a twenty-four hour revolution around the earth; but in the Aristotelian physics it required something colossal to shift the heavy and sluggish earth, while all the skies were made of a subtle substance that was supposed to have no weight, and they were comparatively easy to turn, since turning was concordant with their nature. Above all, if you grant Copernicus a certain advantage in respect of geometrical simplicity, the sacrifice that had to be made for the sake of this was tremendous (Butterfield 1951, p. 23).

Given these complexities, how may we apply the study of aesthetics to understand how science operates? In the next section I consider McAllister’s ideas on this.

McAllister’s Model

⁸⁵ Otherwise, we could see stellar parallax with the naked eye.

James W. McAllister has proposed what is, to my knowledge, the first robust, fully-fleshed-out theory of how aesthetic concerns operate in science. In this section I lay out the theory, and then address its strengths and weaknesses.

Many scholars of aesthetics, such as Edward Bullough, have suggested that aesthetic evaluations involve a certain amount of “psychical distance” between observer and observed, leading to a kind of “detachment.” This, in turn, makes an aesthetic valuation in a sense “disinterested,” and thus divorced from pragmatic concerns (McAllister 1991a, p. 333). Applying such a view to science would suggest a “wall of separation” between scientists’ logico-empirical concerns and their aesthetic ones.

In McAllister’s view, while logico-empirical concerns are of a different type than aesthetic ones, these two types are neither completely separate from each other (as suggested by Bullough), nor are they identical to each other either (as some scientists, such as Bohr, contended). The truth, says McAllister, is somewhere in between.

McAllister begins by assuming that “modern scientific communities attribute to science the goal of formulating theories that possess the highest possible degree of empirical adequacy” (McAllister 1996, p. 76). That is, the goal of science is to describe the world. He then points out, however, that scientists’ aesthetic judgments are often in conflict with their empirical ones. As one example, McAllister cites Schrödinger on Lamarckism, the physicist saying of this theory that it is “beautiful, elating, encouraging, and invigorating....[But] unhappily, Lamarckism is untenable. The fundamental assumption on which it rests, namely, that acquired properties can be inherited, is wrong” (McAllister 1996, p. 69).

The aesthetic canon and aesthetic induction

Whether or not a community considers a given property of a theory beautiful, according to McAllister, is based on the community's "aesthetic canon." The way the community arrives at this canon is "by attaching to each property a weighting proportional to the degree of empirical adequacy then attributed to the set of current and recent theories that have exhibited that property...." McAllister calls this process "the aesthetic induction" (McAllister 1996, p. 78).⁸⁶ Note that since in McAllister's view an aesthetic canon is based ultimately on empirical successes, positive valuation of aesthetic features of theories is, in turn, arguably a kind of rational process.

According to McAllister, a necessary consequence, however, of aesthetic induction is that scientists "will...regard as beautiful a theory that shows little fitness to its purpose—i.e., empirical success—if this theory shares the aesthetic properties of theories that scored notable empirical success." Thus McAllister's view is anti-functional; this is what makes evaluation of scientific theories based on aesthetics seem entirely irrational or inappropriate, when in fact the aesthetic canon at the end of the day was formed on a rational basis (McAllister 1996, p. 80-1)⁸⁷ Further, an aesthetic canon will necessarily show "a damped response to changes in the empirical performance" of

⁸⁶ Mario Livio seems dimly aware that that this is a possibility. "Could our ideas of beauty itself change?" he asks, when faced with the specter of an ugly cosmos ("We are facing the frightening possibility of having possibly to question, or maybe even abandon, the idea of a beautiful theory of the universe.") However, he does not pursue the issue, seeming to hope somehow that his original ideas of beauty will somehow be rescued instead of changing (Livio 2000, p. 13).

⁸⁷ D. Davies disagrees, saying that since scientists' aesthetic induction is "unselfconscious," it is not necessarily rational (Davies 1998, p. 29).

theories....That is, there's a lag time" between the publication of new data and a canon's updating (McAllister 1996, p. 82).

Of this "lag time," D. Davies suggests that it

might be argued that individual scientists' 'irrational' reliance on inductively derived aesthetic criteria in theory choice acts as a force for conservatism in science, and that such conservatism serves the interests of science as a whole in that it allows a theory to be sufficiently developed before a decision is made by the scientific community as to whether further support of the theory is justified (Davies 1998, p. 30).⁸⁸

The theory of aesthetic induction seems to be supported by many examples in the history of science. McAllister cites the case of Newtonian gravity; under the predecessor theory to this, Cartesian corpuscularism, the proposal of "occult" (i.e., action-at-a-distance) forces was considered out-of-bounds in theorizing. Such matters were seen as a throwback to earlier modes of thought (e.g., astrology). Instead, all actions in Nature were to be explained strictly mechanistically. Newton's formulation of the inverse-square law of gravity, without any mechanism to explain how one object pulled on another at a distance ("*Hypotheses non fingo*"), was thus met with near-universal indignation on aesthetic grounds. (Recall that in McAllister's scheme metaphysical allegiances are considered aesthetic.) Even Newton himself admitted that it made no sense to propose a disembodied force was actually somehow communicating gravity over a distance, but the mathematical inverse-square law was so accurate, so useful, that the community began to use the equation routinely despite the aesthetic unacceptability of its underpinnings. In time, however, the inverse square law was *so* successful, that the community acquiesced to

⁸⁸ This theme will be explored further in the next chapter.

the notion of disembodied forces as a reality in the world, their aesthetic distaste for them faded, and indeed, scientists eventually began to attach positive aesthetic value to such things (McAllister 1996, p. 56-8; Brush 2003). As Feyerabend says, “Theories become clear only *after* incoherent parts of them have been used for a long time” (Feyerabend 1970, p. 25).

In the adoption of the heliocentric model of the cosmos, we find an example of what seems to be aesthetic induction in Galileo’s *The Starry Messenger*. The centrality and stability of the earth in the cosmos had been seen as a virtue of the geocentric model. However, Galileo turns this on its head: “We shall prove the earth to be a wandering body surpassing the moon in splendor, and not the sink of all dull refuse of the universe,” here referring to the earth’s location as the “natural place” toward which all things made mostly of the element earth naturally moved under Aristotelian physics (Galilei 1957 [1610], p. 45). Galileo’s explicitly aesthetic language makes plain his feelings concerning the beauty of the Copernican cosmos, and the rhetorical flourish in his words indicates his attempting to “sell” the aesthetics of heliocentrism to a community with a different aesthetic canon—i.e., his attempt to accelerate an aesthetic induction.

Another example of aesthetic induction touches on the comma-free code mentioned in a previous section. After the identification of UUU, which showed that multiple codons can code for the same amino acid, nature’s actual code was seen as inelegant by the community. However, now, the genetic code is seen as far from that. Consider the following statement by Hayes:

When I mentioned to a biologist friend that I find some of the hypothetical genetic codes of the 1950s more appealing than the real thing, she protested that the actual code is one

of the most elegant creations of biochemistry and she pointed out some of its subtle refinements...Its redundancies confer a kind of error tolerance in that many mutations convert between synonymous codons... [and she suggested that] comma-free codes are... brittle...since a mutated codon is likely to become nonsense and terminate translation.... (Hayes 1998, p.85-6).

Similarly, Lorenz's 1963 paper on chaos, overlooked for so long, is now seen as a "beautiful marvel of a paper" (Gleick 1988, p. 30). The nonconservation of parity, once so shocking to many, now seems to be viewed by physicists with a kind of glee and giddiness, epitomizing Nature's endless surprise and variety.

Revolution as aesthetic rupture

The lag time in an aesthetic canon's being updated is pivotal for the other major allegation of McAllister's model, pertaining to scientific revolutions. Suppose a "beautiful" model (considered so on the basis of the aesthetic canon of the day) starts facing empirical difficulties. Further suppose a model whose features do not fit the aesthetic canon (and is thus considered "ugly"), starts to solve these same problems. In McAllister's model, the community will resist the new theory *because of its ugliness*, but as the empirical successes of it start piling up, it may eventually may hold its collective nose and adopt it anyway, because of its empirical success.

In McAllister's view, this is what constitutes a scientific revolution (or "aesthetic rupture").⁸⁹ What is incommensurable in a revolution then is *aesthetics*—scientists indeed can and do evaluate the same empirical data across paradigms, *pace* the usual interpretations of Kuhn. In this, McAllister purports to preserve a rationalist view of

⁸⁹ McAllister seems here consciously to echo the terminology of Gaston Bachelard, one of the earliest thinkers on the concept of scientific revolutions, who in the 1930's "described science as undergoing *ruptures épistémologiques*...." (McAllister 1996, p. 126).

scientific revolutions in the face of the standard Kuhnian concerns. Eventually, as the new model continues solving problems, its features of it are seen increasingly as “beautiful,” and a new aesthetic canon is established. Henk W. de Regt crystallizes two points worth noting here regarding McAllister’s model:

First, McAllister holds that his model, in contrast to Kuhn’s, explains both the continuity and the radical changes exhibited by scientific development. Secondly, the role of aesthetic values is precisely opposite in the two models: in Kuhn’s view, they induce revolutions, whereas in McAllister’s view they inhibit revolutions (de Regt 1998, p. 157).

McAllister claims his model of revolutions can be tested against history, analyzing several cases in the history of science based on his model. I will consider two of these—his studies of the adoption of quantum mechanics and relativity.

Regarding quantum mechanics, McAllister concludes that since it was accepted on the basis of its empirical track record, despite the serious aesthetic misgivings of the scientific community, this jibes with his theory of revolutions. Relativity, however, he finds was accepted largely based on aesthetic grounds to begin with, in advance of the empirical evidence, and therefore should not be considered revolutionary. There are problems with both of these conclusions.

First, regarding quantum mechanics, de Regt takes issue with McAllister’s claims that “a theory is either aesthetically pleasing or not, and that appraisal of aesthetic merit is always conservative.” Theories’ features, according to de Regt, in fact can be “aesthetically innovative in one respect,” and conservative in others. Further, there were “alternative” aesthetics present in the quantum revolution. As an example, some physicists accepted the loss of visualizability in quantum mechanics

due to their having different aesthetic values, whereas others did indeed find it “ugly” despite their accepting the theory, thus being revolutionaries in McAllister’s sense. The reaction was not uniform, as McAllister seems to suggest (de Regt 1998, p. 162). For instance, Heisenberg said of Schrödinger’s formulation of QM that “The more I ponder the physical part of Schrödinger’s theory, the more disgusting it appears to me” (quoted in Holton 1988, p. 363). On the other hand, Schrödinger said of Heisenberg’s approach that “I was frightened away, if not repelled, by what appeared to me a rather difficult method....” (quoted in Holton 1988, p. 363).

Further, de Regt points out that, *pace* McAllister, positive aesthetic judgments did not always “lag” behind empirical concerns in the adoption of quantum mechanics. For instance, “In 1923 Louis de Broglie advanced the hypothesis of matter waves on purely aesthetic grounds; empirical evidence for it came only in 1927” (de Regt 1998, p. 161). And, regarding Wolfgang Pauli, de Regt says that

Pauli valued ‘legitimacy and consistency’ of a scientific theory above its empirical adequacy. Though legitimacy is not easily definable, it clearly is an aesthetic feature...ultimately a matter of ‘physical intuition.’... In addition, Pauli endorsed operationalism...[which] should therefore be classified as an aesthetic demand. Pauli’s aesthetic preferences were directly important for his discovery of the exclusion principle and influenced Werner Heisenberg during his discovery of matrix mechanics. The quantum-mechanical revolution was thus induced by more than empirical considerations alone (de Regt 1998, p. 161).

I know of no one who would deny that quantum mechanics was indeed revolutionary in character (indeed, perhaps the most revolutionary theory in the history of science). Thus McAllister’s model of revolution—with empirical concerns trumping ugly aesthetics, followed later by positive aesthetic judgments—fails accurately to describe the quantum revolution.

But McAllister's model runs into even more serious concerns in its treatment of relativity. Since Einstein's aesthetic motives were "derived...from an extant 'classical' aesthetic canon," McAllister claims that the aesthetics of both special and general relativity theory jibed with the community's aesthetic canon from the get-go, prior to the arising of any empirical concerns, and this paved the way for their acceptance. He therefore concludes that the relativity "revolutions" were nothing of the kind, since positive aesthetic valuation preceded empirical confirmation.

Now, there is some support for this view as relativity as non-revolutionary. Brush points out that

Holton sees Einstein's relativity break as a 'return to classical purity' rather than a discontinuous break....Lewis Pyenson argues that physicists and mathematicians in Germany were motivated to accept relativity because it satisfied their desire to believe in a 'preestablished harmony' between mathematics and physics (Brush 1999, p. 191).

And Carl Seelig quotes Einstein as saying "With respect to the theory of relativity it is not at all a question of a revolutionary act, but of a natural development of a line which can be pursued through centuries" (Holton 1988, p. 375). However, there are three problems with McAllister's view.

First, relativity was *not* easily accepted by all, on the basis of its aesthetic value. Einstein's 1921 Nobel Prize, for instance, very explicitly was not for his work on relativity, but rather his work on light quanta; indeed, in his presentation speech, the Chairman of the Nobel Committee for Physics, Svante Arrhenius, painted relativity as, while much-discussed, still tentative and controversial. And, as de Regt points out,

Lewis Pyenson has provided a detailed description of the reception of relativity in Germany which shows that the theory was not so easily accepted....Instead, the community was divided into advocates and opponents, engaging in heated debates, precisely as McAllister...predicts for revolutions (de Regt 1998, p. 160).

Second, though many scientists did back relativity initially due to aesthetic concerns,⁹⁰ the appeal of relativity was not clear to *all* physicists upon the theory's debut. Just as with quantum mechanics, the theory's aesthetic value was unclear. Jarrett Leplin notes that, regarding relativity and its chief rival, Lorentz's electron theory, that

The [aesthetic] advantages and disadvantages of the electron theory and special relativity ... [were] apparently offsetting. Relativity offered a simplicity and generality acknowledged even by its opponents to be an important advantage over electron theory. Lorentz himself, on a number of occasions, contrasted the complexity and restrictiveness of the assumptions he required to achieve Maxwell covariance with the fundamentality and generality of Einstein's postulates. But the preclusion of an ether was widely considered an important disadvantage of special relativity (Leplin 1975, p. 311).

Stanley Goldberg has pointed out that in Britain, in particular, relativity was resisted on the basis of an entrenched belief in an aether (Goldberg 1970). Believing in the necessity of an aether falls neatly into McAllister's own category of a "metaphysical presupposition," and thus qualifies as an aesthetic argument against relativity.⁹¹

Finally—and this is the most serious problem—in his analysis of the relativity case, McAllister conflates the normative and the descriptive modes of analysis. He starts out with the claim that his model of revolutions can be tested against the historical record. However, upon choosing the "revolutionary" episode of relativity for the test, and finding it does not match his model, he changes the rules in the

⁹⁰ "Thus, for many scientists, mathematical-aesthetic factors were more important than empirical factors in persuading them to accept relativity" (Brush 1999, p. 202).

⁹¹ Goldberg himself links this resistance to Holton's notion of "themata," mentioned previously.

middle of the game and uses his model to impeach the revolutionary status of relativity.

One can't have it both ways. If we take at face value the testimony of scientists themselves as to what is revolutionary in their fields, the consensus seems to be that relativity should indeed be considered revolutionary. Eddington, Ehrenfest, and many others are on record as thinking so (even if Einstein and some later analysts didn't). Further, as de Regt writes, "Around 1911, its adversaries called relativity theory a 'jest' and both parties testified to the revolutionary character of Einstein's theory"(de Regt 1998, p. 160).

And if this is the case, as de Regt puts it,

Since Einstein's revolutionary proposal was inspired not by empirical but by aesthetic considerations (indeed, from the empirical point of view, Lorentz's 1904 theory was equally acceptable), it is therefore a counter-example to McAllister's own model (de Regt 1998, p. 160).

Despite its flaws, McAllister's model, as H. de Regt puts it, "sheds new light on the aesthetics of science" and "differs from many earlier studies of this topic in offering a clearly stated theory instead of vague speculation" (de Regt 1998, p. 165). And even if his notion of revolution as "aesthetic rupture" is flawed, his innovative concept of "aesthetic induction," while a less dramatic claim, seems to have merit and is testable against the historical record.

Sciama and Aesthetics

It seems undeniable that Dennis Sciama was heavily influenced by aesthetic factors in his backing of the steady state model of the universe. His repeated use of words like “sweep and beauty,” “simple,” and “magnificent” (as described in Chapter 2) to describe that cosmology bears this out.

One might ask, whence came Sciama’s high valuation of aesthetics in science? It seems appropriate to note that aesthetics played a central role in the work of G.H. Hardy, Sciama’s idol. As noted in Chapter 2, Sciama has written of the profound influence this mathematician had on him, and in particular his work *A Mathematician’s Apology*.

C.P. Snow has written that the “deepest attractions” of mathematics for Hardy were “technique, tactics, [and] formal beauty” (Snow 1967, p. 45). Indeed, Snow also speculated that Hardy’s love of cricket was due to its being “a game of grace and order, which is why he found formal beauty in it. His mathematics, so I am told, had these same aesthetic qualities, right up to his last creative work” (Snow 1967, p. 45).

Hardy himself wrote that the best mathematics has “permanent aesthetic value” that causes “intense emotional satisfaction...” (Hardy 1967 [1940], p. 131). He also wrote that

The mathematician’s patterns, like the painter’s or the poet’s, must be *beautiful*; the ideas, like the colours or the words, must fit together in a harmonious way. Beauty is the first test: there is no permanent place in the world for ugly mathematics....It may be very hard to *define* mathematical beauty, but that is just as true of beauty of any kind.... (Hardy 1967 [1940], p. 85).

As for how to pin down the characteristics of mathematical beauty, Hardy suggested that the “best mathematics is serious as well as beautiful—‘important’ if you like, but the word is very ambiguous, and ‘serious’ expresses what I mean much better.”

He spoke of the “*significance* of mathematical ideas,” saying that the “beauty of a mathematical theorem *depends* a great deal on its seriousness” (Hardy 1967 [1940], p. 89, 90).

Hardy also writes of the best math having “two things at any rate which seem essential, a certain *generality* and a certain *depth*....” (Hardy 1967 [1940], p. 103). His description of the proofs of Pythagoras and Euclid illustrate this, speaking of their

high degree of unexpectedness, combined with inevitability and economy. The arguments take so odd and surprising a form; the weapons used seem so childishly simple when compared with far-fetching results; but there is no escape from the conclusions. There are no complications of detail....A mathematical proof should resemble a simple and clear-cut constellation.... (Hardy 1967 [1940], p. 113).

It seems likely to me that we can lay some of the reason for Sciama’s aesthetic mode of doing science at Hardy’s doorstep. As seen here, Hardy placed the highest value on “beautiful” theorems, and the attaining of these guided his work—as it did for Sciama. In Hardy’s emphasis on “generality,” we see a forerunner of Sciama’s demand that a suitable cosmology be free of arbitrary parameters and ad hoc assumptions. Simplicity (or “economy”) criteria are explicitly mentioned in both men’s work. And it is hard to imagine a subject with more “depth,” “seriousness,” and “significance” than cosmology.

One more motivation of Sciama is that he “liked the steady state theory because it's the only one in which life will always be possible somewhere.” Some may find this a shocking motivation for a reputable scientist to have in pursuing his work (see also Sciama 1978, p. 22-23; Overbye 1991, p.85-6). But perhaps it is not so much so if viewed as just another kind of an aesthetic evaluation, namely a *metaphysical*

presupposition, one of McAllister's five categories of aesthetic features scientists may value.⁹²

Given his emphasis on the significance of life in the cosmos, I cannot help but note that Brandon Carter, one of Sciama's students, was the inventor of the anthropic principle. There are numerous formulations of this controversial theory, first proposed by Carter in 1973 during the symposium "Confrontation of Cosmological Theories with Observational Data," held (ironically—or appropriately?—enough) in commemoration of Copernicus's 500th birthday. The two most common versions are the so-called Weak and Strong Anthropic Principles. The former suggests simply that the very existence of carbon-based life forms itself places limits on the universe's parameters (its age, the values of fundamental constants, and so forth). The latter formulation, much more speculative and teleological, asserts that the universe *must* have said properties in order that life *will* develop.

Sciama was uncomfortable even with the Weak Anthropic Principle. Though on the one hand, it can be argued that the Weak Anthropic Principle is simply a kind of tautology (the universe must have certain properties to permit the development of life; if it didn't we wouldn't be here to talk about it), the coincidence that the universe did in fact have *just these properties* was still a puzzle to Sciama. Here we see an echo of Livio's distaste for anti-Copernican thinking; this particular metaphysical presupposition

⁹² One cannot help but notice that of all the aesthetic motivations Sciama cited for supporting the steady state model, the "there-will-always-be-life" criterion is the only one, to my knowledge, that Sciama did not mention in print during the debate itself—he only mentioned it retrospectively in interviews and such.

would then seem to guide both men's work.⁹³ Another echo of this would be in his paper on the absorption of extragalactic radio noise, mentioned in Chapter 2, which was motivated by his seeing the alternative model as lacking because of its “unattractive feature that we are required to be near the center of this region....” (Sciama 1964b, p. 767).

His resolution of the anthropic problem was to argue that “all logically possible universes exist in an ensemble of disjoint universes. An intelligent observer would automatically find himself in a universe whose properties are compatible with his own development” (Sciama 1993, p. 107). Once again, Sciama makes an aesthetic argument to justify this, at the same time demonstrating the differing interpretability of aesthetic criteria of *simplicity*:

At first sight this proposal might seem to fall afoul of Ockham's razor. But I believe the opposite to be the case. On the conventional view of a unique universe we have to assume that it was *decided* that all but one of the logically possible universes should not exist.⁹⁴ This is a very strong assumption and it is completely obscure how this decision was taken. My own view is that we should invoke as few constraints on reality as it is compatible with observation, and that it is this view which is in harmony with Ockham's Razor. Thus I am advocating that everything which is not forbidden is compulsory (Sciama 1993, p. 108).

Note the echoes here to the argument about recounted earlier regarding the aesthetics of the cosmological constant; leaving Λ out of the field equations was seen by

⁹³ Livio considers the anthropic principle in Chapter 9 of (Livio 2000), where he lays out his dim view of that theory.

⁹⁴ Though it has been suggested that an aggressive atheism was a motivating factor for many steady-state proponents, particularly Hoyle, in advancing that theory, I find no evidence that Sciama was similarly motivated. Though Sciama, like his idol Hardy, was indeed an avowed atheist, I read this as Sciama's ruling out such a “decided” universe on simplicity or methodological grounds, rather than anti-theist grounds, as the next sentence seems to suggest. For more on the atheism issue, see (Kragh 1996, p. 253-255).

some scientists as being equivalent to making a decision to choose for it a value of zero for Λ out of all the “logically possible” choices, whereas others saw the equations without the term as simpler.

Related to this we find another case of aesthetic wrangling in Sciama’s science. His aesthetic argument that there should be an infinity of disjoint universes leads Sciama to a novel prediction regarding Penrose and Hawking’s calculation of certain conditions required for the early universe. Though Sciama calls these calculations “mathematically elegant and precise,” he still predicts they will be incorrect in order to jibe with his theory (Sciama 1993, p. 109). Thus his metaphysical allegiance to the Copernican Principle is ranked higher in his personal aesthetic canon than elegant mathematics.

At the end of the day, despite his strong aesthetic preference for the steady state model, Sciama abandoned it when the evidence against it became too much. “The steady state theory is very beautiful but is now in serious conflict with observation....It seems very unlikely that the steady state theory can be saved,” he wrote in 1973 (Sciama 1973b, p. 20). McAllister writes of this that Sciama’s “unfavorable empirical evaluation does not preclude [his] simultaneously feeling the theory has aesthetic merits, revealed to him through a judgment which abstracts from the utilitarian dimension of the theory” (McAllister 1991a, p. 337).

Further, instead of dropping out of cosmology altogether (which many of his steady state colleagues did), Sciama switched over to the big bang model of the universe, and worked within that paradigm for the rest of his life. He became a “strong supporter and vigorous defender of the big bang model. The evidence for this model is now quite

strong,” he wrote (Sciama 1994). He went on to publish numerous papers, and even a book, in the field (Sciama 1995 [1993]).

However, the question now arises as to whether Sciama himself went through any kind of personal change of view regarding the aesthetics of the big bang model. That is, does McAllister’s model of aesthetic induction apply to individual scientists or only to them in the aggregate?

One would think it would have to apply to the individuals, since they are what make up the community to begin with.⁹⁵ However, despite his adoption of the big bang model and recognition of its empirical superiority, I find little evidence in his writings Sciama ever came to see it as aesthetically appealing as the steady state model had been to him. Of the discovery of the cosmic microwave background radiation by Penzias and Wilson in 1965 (in many ways the final nail in the coffin of the steady state model), Sciama called it “perhaps...the most magical of all astronomical discoveries” (Sciama 1973a, p.60). Quotes such as these, however, refer to beauty of this specific experimental finding, rather than to the formal features of the big bang model itself. Sciama also referred negatively in a 1990 interview to conceptual issues that the big bang model (still) faced such as “Did time exist before the big bang and that sort of thing” (Sciama 1990). He described the primeval atom singularity that begins a big bang model as one of its “botches” (Sciama 1977 [1967], p. 31). And in fact, in 1978 he also told

⁹⁵ The only other alternative would be if something akin to Planck’s Principle were at play; this will be discussed in the next chapter.

Spencer Weart that if he could design a universe, he would make it a steady state one, again citing the propagation of life as the primary reason (Sciama 1978, p. 22).⁹⁶

The closest I have come to Sciama's showing a kind of preference based on non-empirical concerns for the big bang model over the steady state comes from his thinking on the anthropic principle:

In the steady state theory there would be no immediate explanation of the coincidence that the present expansion timetable of the universe is about the same as the lifetime of a typical main sequence star. By contrast in the big bang evolutionary theory of the universe there is a trivial explanation, indeed there are two....The weak explanation is based on the fact that the Galaxy still contains vast numbers of main sequence stars which have already formed but which have not burned out. Thus we must be observing at the appropriate epoch after the big bang—about ten billion years—when the expansion timescale would be expected to be also of this order. The strong explanation is based on the fact that **I** exist. This requires me to be near a main sequence star, so it is not surprising that in the present epoch of the universe such stars exist (Sciama 1993, p. 107).

However, it is not clear to me that this argument should be considered aesthetic; instead it might be termed a “methodological” argument.⁹⁷

Thus I conclude that Sciama's aesthetic preference for the steady state model was never supplanted, even by his working in the big bang paradigm for the last thirty-odd years of his life. To him, it the universe was still apparently “a botched job” (Sciama 1977 [1967], p. 31). In McAllister's terminology, Sciama's aesthetic and empirical judgments remained independent. Thus, so far as Sciama is concerned, the steady state affair would seem to epitomize T.H. Huxley's memorable quote regarding “the great

⁹⁶ He also says that this criterion “must have” played a role in the thinking of other steady state proponents, which he confirmed he discussed with them, and that this particular aesthetic requirement “dominated” for him personally (Sciama 1978, p. 23).

⁹⁷ This too will be touched upon more in Chapter 4.

tragedy of Science—the slaying of a beautiful hypothesis by an ugly fact” (quoted in McAllister 1996, p. 83).

Summary

That aesthetic factors play a significant role in theory construction and theory choice seems undeniable. Dennis Sciama’s aesthetic approach to science, then, far from marking him as an outsider, places him squarely in the mainstream of the scientific community in this regard. However, it is simply unwarranted for scientists, such as Dirac, to conclude that aesthetics lead the way to truth.⁹⁸ When scientists make this claim, they are either unaware of, have forgotten about, or are in denial about the copious counter-examples to this available in the historical record.

Now, there are some, including Mario Livio, who seek to impeach such examples of aesthetic reasoning gone wrong, arguing that such instances are “misapplications” of aesthetic principles:

One has to realize that the aesthetic principles are generally applied to the *fundamental* aspects of the theory and to its cornerstone *idea* and not to the more peripheral details.... The identification of what is fundamental is not always easy. The history of science is full of examples of concepts and entities once considered absolutely fundamental...but that have been knocked off their pedestal of fundamental status at later times....As a result of this ambiguity, it can definitely happen, and indeed has happened, that the aesthetic principles would be applied to the wrong entities all together or only to a subset of all the relevant entities (Livio 2000, p. 103).

⁹⁸ Feyerabend agrees, reasoning thus: if Bohr is correct in that aesthetic valuations of theories can only properly be made after the fact, since science “does not achieve final results,” there is not an “after the fact” and thus aesthetics “are never a *conditio sine qua non* of scientific knowledge” (Feyerabend 1970, p. 99).

However, Livio's view is Whiggish in the extreme—retrospectively *correct* aesthetic arguments he seems to see as vindications of his thesis, but *incorrect* ones are defined away as misapplications. If we disallow such Whiggish reasoning, and we should, it seems the record is clear that perceived positive aesthetics simply cannot be considered a reliable indicator of truth.

The issue with aesthetic criteria is two-fold. First, as McAllister has suggested, scientists' criteria of "what is beautiful" have demonstrably changed over time. As McAllister says, "Every property that has at some date been seen as aesthetically attractive has at other times been judged displeasing or aesthetically neutral" (McAllister 1996, p. 78). Furthermore, even in one given time, scientists may differ on what is beautiful and what is not; consider Murray Gell-Mann's referring to solid state physics as "squalid state" physics⁹⁹—an aesthetic judgment with which it is safe to say solid state physicists disagree.

Second, even if a certain aesthetic quality (such as "simplicity" or "symmetry"), were agreed-upon to be universally desirable, the near-infinite ways scientists might interpret or apply such a criterion in a given instance of theory choice makes it unreliable as an indicator of truth. Einstein's insistence that "I should never claim that I really understood what is meant by the simplicity of natural laws" is an admission of this (despite Einstein's own other quotes regarding the value of simplicity) (quoted in Heisenberg 1971, p. 69).

⁹⁹ Quoted in (Kevles 1995, p. xxv).

However, despite the above, it would be incorrect to suggest that Sciama, or any of the steady state proponents for that matter, was unscientific in his use of aesthetics as a guideline in his theorizing. Recall that Einstein's rejection of the luminiferous aether in 1905 was largely due to his insistence on the relativity principle, which is at the end of the day an "aesthetic requirement" (Leplin 1975, p. 328-9).¹⁰⁰ As Holton points out, indeed, *all three* of Einstein's great 1905 papers begin

with a statement of formal asymmetries or other incongruities of a predominantly aesthetic nature...[in response to which Einstein] *then proposes a principle—preferably one of ...generality...which removes the asymmetries as one of the deduced consequences, and at the end produces one or more experimentally verifiable predictions* (Holton 1988, p. 193, emphasis added).¹⁰¹

The similarities between Einstein's actions as described above, and the formulation of the perfect cosmological principle and steady state model are undeniable. The perfect cosmological principle was a general, aesthetically driven principle which certainly made verifiable, and novel, predictions. It is simply unfair retrospectively to laud Einstein's "breathtaking insights" of 1905 (as is often done), but at the same time criticize Bondi, Gold, Hoyle, and Sciama for essentially operating in the same way. The perfect cosmological principle turned out to be incorrect, but the methodology was at root the same as Einstein's and that of many other respected scientists as well.

Regarding McAllister's broader claims, it seems his view of scientific revolution is flawed, being unable to account adequately for the revolutions of relativity and

¹⁰⁰ His rejecting Newton's laws as the basis for all phenomena also played a significant role in this thinking.

¹⁰¹ See (Hunt 1996; Gale and Urani 1999) for more on this methodology as it relates to 20th century cosmology.

quantum mechanics. However, his more circumscribed notion of aesthetic induction seems to have some support based on the historical evidence. Other than this, however, we find ourselves still without a workable model or algorithm of how aesthetics in science may operate in theory change in the broadest sense.

But this should not be very surprising. If we believe there is no one, universal, eternal “scientific method” (as seems generally accepted today), it should not surprise us too much that one aspect of the scientific enterprise, namely, the aesthetics of science, resists being reduced to an algorithm as well. The use of aesthetics in science seems to be simply one more tool in the scientist’s varied toolbox. It is applied in different ways at different times by scientists, much as the other “tools” in their kit are.

The disrepute in which the aesthetics of science is sometimes held, and in fact in particular when discussing the steady state model of the cosmos, is thus misplaced. The steady state proponents proceeded in a way of theorizing common to scientists, especially when in the infancy of a field and data are scarce. In such a situation, scientists have to have *some* criteria on which to base things, on which to direct research; aesthetic principles are as good as any in such a situation. Indeed, considering how inaccurate early data often are, arguably it makes sense to let aesthetics guide one at least during this phase, rather than strict adherence to empirical concerns. “A thematic hypothesis,” Holton writes, in fact “becomes more persuasive the longer the period of unsuccessful

attempts to use other hypotheses, namely, those that *are* coupled to phenomena” (Holton 1988, p. 36).¹⁰²

If the formulation and pursuit of the steady state model was unexceptional in the way discussed, then, whence comes the whiff of disapproval often evinced when the model is being discussed? I assert this is probably due to the actions of a few, recalcitrant steady state theorists who clung to their aesthetic preference for far too long, long after the conclusive evidence to the contrary had surfaced. This band, led by Hoyle “tarred” the entire steady state programme, and those associated with it, I’m afraid forever, as being something not quite cricket—even though, as I have shown, it was an entirely reasonable programme to propose at the time.¹⁰³ As mentioned before, Dirac and Einstein also acted similarly (in regard to QED and QM respectively), and paid a high social price for their lifelong resistance as well. However, they (unlike Hoyle with the steady state) were not strongly associated with a specific, fully-formed, *unsuccessful* rival to the established paradigm they were resisting. Thus their lifelong resistance, while reflecting poorly on them personally in the minds of many, did not place the Mark of Cain on any one particular (failed) model, so to speak, as it did in the case of the steady state theory.

¹⁰² Assuming this is an accurate analysis—that aesthetic principles dominate at first, but then give way when the data becomes too overwhelming against them, a question arises: what are we to make of the situation in which the experimental data needed to confirm or disconfirm a theory is unlikely ever to be found? The energies required to test, for instance, string theory, are hard to imagine ever being created by humanity. But this would take us too far afield.

¹⁰³ The endless parade of variations on Hoyle’s model, made to accommodate new data, is chronicled in (Kragh 1996).

Thus we see another way in which science is indeed a social enterprise. Beauty may be in the eye of the beholder, but if an individual persists in being out-of-step with the community's view (i.e., recalcitrant, sticking to an older theory with poor empirical support) because of its aesthetic features, he will be ostracized.¹⁰⁴

Now, alternative interpretations of many of the “beauty = truth” quotes espoused by scientists do present themselves upon careful reading. For instance, Einstein, despite the beauty he saw in his own later field theories, as well as those of Eddington, considered them “dubious”—showing that seeing great beauty in something did not necessitate its truth after all (McAllister 1996, p. 69). Or, consider Livio's quote that “The notion of beautiful theories should not be abandoned, at least not without a fight” (Livio 2000, p. 254). One reading of the final phrase of this quote might be: it is okay to prefer a model on aesthetic grounds, but at the end of the day, the scientific data, *once reliable*, are of course the final arbiter of what is correct and what is not, despite the beauty of whatever theories are in question.

One can find similar qualifications and nuances, if one looks hard enough, in the words and actions of even the most “aesthetic diehards.” For instance, as we saw above, Dirac did eventually propose the existence of the positron, despite his initial aesthetic preference against it, when the criticisms and evidence for the alternative became too much to resist; thus his position on preferring beautiful theories could be taken to be that beauty should be a guide in the face of *tentative* data, or being too quick to abandon a beautiful theory without giving its due. Dirac also once wrote that

¹⁰⁴ The themes discussed in the preceding several paragraphs will be discussed further in the next chapter.

from a theoretical point of view one would think that [magnetic] monopoles should exist, because of the prettiness of the mathematics. Many attempts to find them have been made, but all have been unsuccessful. One should conclude that pretty mathematics by itself is not an adequate reason for nature to have made use of a theory (Dirac 1982, p. 604).

Anthony Zee writes of Georgi-Glashow theory (a Yang-Mills theory based on $SU(5)$) that “many physicists, myself included, are willing to believe in [it] on aesthetic grounds alone.” However, he does then add “But physics ultimately is to be grounded in empirical verification” (Zee 1986, p. 235). Newton himself, as part of his third rule of reasoning, and in the midst of his discussion of Nature’s simplicity, cautioned however that “we are certainly not to relinquish the evidence of experiments for the sake of dreams and vain fictions of our own devising” (Newton 1966 [1729], p. 398). And during the “island universes” controversy, A.C.D. Crommelin wrote:

Whether true or false, the hypothesis of external galaxies is certainly a sublime and magnificent one. Instead of a single star system, it presents us with thousands of them...Our conclusions in Science must be based on evidence, and not on sentiment. But we may express hope that this sublime conception may stand the test of further examination (quoted in Smith 1982, p. 16).

But it would be a misuse of this more nuanced reading on aesthetics to wallpaper over the copious cases of these same scientists’ aesthetic views’ leading them astray, as I enumerated before. Such retroactive “tidying up” of scientific history is precisely one of the things STS was founded to counter. Those two same scientists I mentioned two paragraphs ago, remember, did resist QED (in the case of Dirac) and QM (in the case of Einstein), despite copious data in support of those theories—and despite their occasionally seeming to hedge their views on aesthetic “reliability”—largely on aesthetic grounds, until their respective deaths. It seems a certain kind of professional “amnesia”

often occurs regarding aesthetics, causing scientists to forget not only when aesthetic arguments have not been successful in the past, but also when *they themselves* have given up the aesthetic ghost in the face of stubborn experimental results in other cases.

Sciama's behavior in the matter, on the other hand, was anything but unreasonable in this regard. He made reasonable accommodations to attempt to preserve his strong aesthetic preference for the steady state model, but when the evidence became too much, unlike Hoyle et al., he gave up the ghost. Indeed, even before the critical evidence came in, he was clear on this matter, writing in 1959 that the steady state had only "*provisional* advantage" over the big bang model, since

one is influenced by it only when the available observations do not yet distinguish between the various possibilities. Such arguments have their place in science, because the time available for research is limited, and they suggest which theories are likely to be the most fruitful to work on. Nevertheless, the steady state model can be decided only by observation (Sciama 1959, p. 175).

Similarly, in 1965 one of his last defenses of the model, he wrote,

Of course, although these [largely aesthetic] advantages make the steady state theory worth investigating, they do not make it true. Its validity must be decided by observation, or a combination of observation and theory (Sciama 1965, p. 3).

And in 1990 he said

The reason for supporting it was not...that it *had* to be right, but just that it was very attractive and the penalty of having creation of matter didn't seem to be such a terrific penalty....But, I never felt then, and I don't now feel so alarmed about outrageous proposals in physics, unless they're easily disposed of by experimental evidence (quoted in Lightman and Brawer 1990).

So we see that Sciama's aesthetic approach to science, while important, did not in a sense *define* him. He resisted changing models for a time, but in the end, the empirical evidence trumped his aesthetic sense. Thus in Sciama we see a scientist who does not, in the end, conflate his *hope* that the universe's properties have characteristics he sees as

“beautiful” with the ontological reality of same. Aesthetics were important to him, particularly during the youth of his field, but in the end he acknowledged they did not carry the day.

In the next chapter I will look at other aspects of resistance to theory change.

CHAPTER 4

PLANCK'S PRINCIPLE

I used to be 'with it.' But then they changed what 'it' was. Now what I'm with isn't 'it,' and what's 'it' seems scary and weird. *It'll happen to you!*

—Abe Simpson, “Homerpalooza”

In this chapter, I will address aspects of what has been described by some as “science’s collective psychology of conformism”(Gernand and Reedy 1986, p. 475). This is perhaps best exemplified by “Planck’s Principle,” a statement regarding the nature of scientific progress made by the physicist Max Planck, the winner of the Nobel Prize for his introduction of the quantum hypothesis in 1900.¹⁰⁵ As will be discussed, Planck called into question the very root of scientific rationality—scientists’ weighing the truth or falsity of a scientific claim based on experimental evidence. Thomas Kuhn's citation of Planck’s hypothesis in his landmark 1962 book *The Structure of Scientific Revolutions* firmly established this dictum in the annals of modern academia (Kuhn 1970 [1962], p. 151).¹⁰⁶ Because of this, Planck’s Principle has, as Geoffrey Gorham puts it, “become a favourite slogan of post-positivist history and philosophy of science.” It is in

¹⁰⁵ Planck apparently did not intend his 1900 hypothesis to be taken physically, but rather simply as a mathematical device. In his Nobel Lecture Planck explicitly assigns credit for this revolutionary idea to Einstein, who in 1905 had suggested light quanta as a way to explain the photoelectric effect. See (Kuhn 1987).

¹⁰⁶ According to Kuhn, Planck’s Principle was “too commonly known to need further emphasis....” (Kuhn 1970 [1962], p. 151).

many academic circles simply taken for granted as a self-evident truth, and as evidence of the irrationalism of science (Gorham 1991, p. 472).¹⁰⁷

However, despite such claims, I will show that the conversion of Dennis Sciama from the steady state model of the universe to the big bang model provides a clear counterexample to the claims of Planck. Further, I will show that the *way* in which Sciama converted from the one theory to the other is of special note, and in fact closely parallels a similar conversion made earlier in the 20th century—that of James Jeans from classical to quantum theory.

Background

In his 1932 Guthrie Lecture to the Physical Society of London entitled “*Ursprung und Auswirkung wissenschaftlicher Ideen*,”¹⁰⁸ Max Planck put forward a surprising view of how science progresses. Planck reflected on his struggles as a young physicist to get the theory that the conduction of heat is irreversible accepted by an older generation of physicists (such as Mach, Ostwald, and Helmholtz) who were invested in a rival theory (energetics) which denied this irreversibility. He stated:

The historical development [of the controversy] may well serve to exemplify a fact which at first sight may appear somewhat strange. An important scientific innovation rarely makes its way by gradually winning over and converting its opponents: it rarely happens

¹⁰⁷ This is despite the fact that on the page following Kuhn’s invocation of Planck’s Principle, he specifically admonishes that his model of paradigm change should not be construed “to say that no arguments are relevant or that scientists cannot be persuaded to change their minds...” (Kuhn 1970 [1962], p. 152). I will explore Kuhn’s take on Planck’s Principle more fully later in this chapter.

¹⁰⁸“Scientific Ideas: Their Origin and Effects”

that Saul becomes Paul. What does happen is that its opponents gradually die out and that the growing generation is familiarized with the idea from the beginning.... (Planck 1936, p. 90).

Did Planck really believe this radical notion, or was he espousing “a mere *obiter dictum*” which did not reflect his most fundamental beliefs (Blackmore 1978, p. 347). To underscore that Planck apparently believed that this is how science really progresses (as opposed to being a mere offhand comment), it should be noted that Planck reiterated this same proposal years later in his “Scientific Autobiography,” saying that his conflict with the energeticists gave him

an opportunity to learn a new fact—a remarkable one, in my opinion: A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because opponents eventually die, and a new generation grows up that is familiar with it (Planck 1949, pp. 33-34).

Planck was neither the first nor last to espouse such opinions; numerous other thinkers of the first order have also done so. Auguste Comte wrote, “There is no denying that our social progression rests upon death” and of the “perpetual conflict which goes on between the conservative instinct that belongs to age and the innovating instinct which distinguishes youth” (quoted in Hagstrom 1965, p. 282). Joseph Henry remarked that a paper he read to the American Physical Society in 1844 (on new ideas in mechanics) “found favor with the younger men of science to whom I have communicated it, but met with considerable opposition from some of the older members of the society” (quoted in Moyer 1997, p. 183). In 1852, T.H. Huxley (then in his twenties) made the following statement regarding the reluctance of older scientists to consider new ideas:

I know that the paper I have just sent in is very original and of some importance, and I am equally sure that if it is referred to the judgment of my ‘particular’ friend that it will not be published. He won’t be able to say a word against it, but he will pooh-pooh it to a dead certainty. You will ask with wonderment, Why? Because for the last twenty years

[...] has been regarded as the great authority in these matters, and has had no one tread on his heels, until, at last, I think, he has come to look upon the Natural World as his special preserve, and 'no poachers allowed' (quoted in Barber 1961, p. 600).

One can find similar quotes by Lavoisier, Darwin, and many others.¹⁰⁹ The fact that such luminaries believed something akin to Planck's Principle was operating within science means that it cannot be summarily dismissed.

Planck's Principle – Weak Version

A close reading of Planck's Principle reveals that Planck is actually making two separate claims about science. The weaker of the two claims is that *younger* scientists are more likely than *older* ones to adopt new models. Regarding this weaker version, while there is little evidence that purely biological or physiological aspects of senescence could be responsible for such a thing (Zuckerman and Merton 1972), a number of possible reasons have been suggested for such a tendency.

Younger scientists, being not long removed from their graduate studies, are arguably more "apt than their expert teachers to be abreast of the range of knowledge in their field" (Zuckerman and Merton 1972, p. 510). Or, to quote Peter Messeri:

Young scientists may adopt a new theory before their elders in part because they are better informed about current research in a broader range of fields...[they are] closer in time to their formal training.... Defects in existing knowledge may be more apparent to young scientists burdened with fewer preconceptions.... (Messeri 1988, p. 94).

¹⁰⁹ For an extensive list of references to Planck's Principle or something akin to it by scientists and STS commentators, see (Hull 1998, p. 213).

“Expert teachers,” on the other hand, in order to make productive contributions to science must necessarily be narrowly specialized rather than broadly based. Young scientists are “more likely to embrace a new field without the hesitation that comes from being too cognizant of prevailing opinions about what is or is not a legitimate area of research. Such naivete may work in favour of young scientists....” Thus, the “very skills [an older] scientist has accumulated” which make him successful and productive, “paradoxically constrain his ability to innovate”(Rappa and Debackere 1993, p. 16).¹¹⁰ Stephen Hawking seems to be suggesting something like this when saying of the early days of scientific cosmology,

It was an exciting time to be a student in the field. Startling discoveries were being made in both the theory and the observations. Everything was new, so a research student could see possibilities that more established workers didn't have the mental agility to adjust to. (Hawking 1997)

As “advanced research in science demands concentration on a narrow range of problems at hand,” older experts must necessarily be invested in a certain set of subdisciplinary practices and terminology (Zuckerman and Merton 1972, p. 510). Working within a research program (i.e., with others who share said practices and terminology) may bring about a kind of “socially induced commitment” to the prevailing view as a result of the constraints of these practices and even the very language one speaks (Messerli 1988, p. 94). Younger scientists, on the other hand, are “not necessarily bound into a network of interpersonal relations that strengthens support of existing approaches” (Hagstrom 1965, p. 284). That is, adoption of new theories may therefore

¹¹⁰ Rappa and Debackere go on to suggest that graduate students should therefore be freer in their choice of dissertation topics. Relying on an older advisor to guide one to a topic that is both “interesting” and “doable” may be limiting one’s potential for youthful innovation.

come “particularly easy to [scientists] just entering the profession, for they have not yet acquired...special vocabularies and commitments” (Kuhn 1970 [1962], p. 203). Rappa and Debackere suggest along these lines that

It may very well be the nature of scientific ideas that make generation an element of change in science. Fundamentally new ideas in science are a magnet for controversy because they are so easily misunderstood or misconstrued....The confusion may be in part because of the formative state of such ideas when they first appear....A new theory may come with its own vocabulary....New theories are not readily digested....Embracing a new theory requires substantial effort in deciphering its meaning (Rappa and Debackere 1993, p. 6).

With new approaches, the tradition to be learned is minimal as compared to older, more fleshed-out avenues; younger scientists might find this an attractive feature, making career advancement easier. Younger scientists “feel more strongly the necessity of quickly producing important results....They are in a position that makes it convenient or necessary to select a new special field”(Hagstrom 1965, p. 284). H. Gilman McCann explains:

Fundamental discoveries are made more easily and result in a shorter wait for significant recognition. In addition to these practical advantages, there is likely to be the pure excitement value of a field undergoing transition.... Since basic assumptions have been called into question, the time is ripe for new ideas.... Younger scientists may think that such a field offers them greater opportunity, since the authorities of the past...would have no technical advantage over them.... younger ... scientists... would have less commitment to the old paradigm and more to gain from adopting the new one (McCann 1978, p. 18-19).

Pride in one’s past successes might render an older scientist’s judgment nonobjective, as might the fact that scientists’ standing and prestige are based on past accomplishments. Sharon Levin et al. write, “self-interest life-cycle models in economics and common sense all suggest that older scientists will have incentives to oppose new discoveries, especially those that may overturn or render irrelevant some part

of their own research record” (Levin, Stephan et al. 1995, p. 275). Arthur Diamond cites Kuhn’s view that Planck’s Principle is explained by “the costs of intellectual retooling when a new theory is adopted. Those who are older will...have more human capital invested in the old theories, and hence, will have more to lose,” and that “with the accumulation of human capital in the current theory, a scientist develops a comparative advantage in working to improve that theory. Thus older scientists would [continue to do so, whereas]...we would expect those who spend more time devising new theories (namely the young) to accept the new theories sooner”(Diamond 1980, p. 839; Diamond 1988, p. 193). Or, as Peter Messeri puts it:

Older scientists have a greater social and cognitive investment in...perpetuation, and have less to gain from adopting new ideas. This may be particularly important when adoption of a new theory requires substantial effort to master new research skills and concepts (Messeri 1988, p. 95).

Finally, older scientists’ alleged reluctance to embrace new ideas might also be due to the straightforward reason that they believe they are wiser than their youthful counterparts and less likely to fall for a scientific flash in the pan. David Hull states, “Older scientists may simply know much more than their younger colleagues and see more of the ramifications of a new idea...” (Hull, Tessner et al. 1978, p. 717). Or, as Messeri says, “Older scientists may take longer [in adopting a new model]...because of greater familiarity with past successes of established theory in overcoming previous theoretical or empirical challenges” (Messeri 1988, p. 94). Michael Polanyi addresses something like this when he says, “If every anomaly observed in my laboratory were taken at its face value, research would instantly degenerate into a wild-goose chase after imaginary fundamental novelties....” (quoted in Hon 1989, p. 473).

In recent years a number of authors have used case study methodology to test the “weak” version of Planck’s principle, and have found the evidence to be mixed; a partial list of these references may be found in (Hull 1998).¹¹¹

Planck’s Principle – Strong Version

Planck’s second, stronger claim is “It rarely happens that Saul becomes Paul”—that *generally speaking* scientists carry their fundamental beliefs to their graves rather than change them. As Geoffrey Gorham has pointed out, on this view, science advances by a Darwinian mechanism rather than a Lamarckian one, with scientists dying out like ill-adapted species, and their pet theories dying out with them the way species’ genetic

¹¹¹ One of these authors, Peter Messeri, has suggested that Planck’s Principle might be turned on its head. Perhaps it is older scientists that should be *more* likely—and younger scientists *less* likely—to adopt risky new theories, due to the social structure of science. “Resources which scientists accrue during their careers may well buffer the increased intellectual risk taken in advocating speculative theories. Older scientists may therefore be better positioned than their younger colleagues to speak out earlier in support of new but controversial theories” (Messeri 1988, p. 91). For instance, “Access to various modes of informal channels of scientific communications increases with age” and therefore older scientists should be more “plugged in” to what’s at the cutting edges of their fields (Messeri 1988, p. 96). He also writes:

Theory choice has social consequences for professional advancement. Anticipation of long-term professional gain [in the traditional Planckian view] is thought to motivate speedier adoption of a major new innovation by hungry young scientists. There are also short-run professional costs, however, in the early adoption of a yet untested theory. These can range from professional censure, to difficulties in publishing articles espousing controversial ideas, to the increased risk of expending scarce time and resources on an unproductive line of research. These social pressures may act as deterrent to the translation of private preferences into publicly expressed support for a new theory (Messeri 1988, p. 95).

That is, young scientists might not be willing to risk their fledgling careers on new, radical ideas. On the other hand, since social standing in science is through cumulative recognition, older scientists have less to lose (as they have already established their “track records”), and should be more likely to take a risk on a new theory. Also, since older, more established scientists should have more resources to deploy in research, “The tendency of scientists to enlarge the size of their problem set is a second structural resource which permits older scientists leeway....” That is, they can hedge their bets by pursuing several safer, more conservative theories in other areas to offset their risky one (Messeri 1988, p. 96).

information (DNA) vanishes with their extinction (Gorham 1991, p. 471).¹¹² If true, this claim is a much more serious challenge to the notion that science reveals something real about the world than the mere notion that older scientists are resistant to change.

But are scientists, in fact, resistant to theory change, generally speaking? In David Hull's view, the answer depends on what point in the theory-vetting process is in question. In Hull's view, scientists are fairly open-minded about choosing a theory—individually and inside their own research groups. However, once they announce their allegiance to larger circles (within the larger “demes” of research groups and the so-called “invisible college”), then scientists “go to great lengths to salvage the views to which they are publicly committed”(Hull 1988, p. 13). Such behavior is very different from the stereotypical view of the dispassionate scientist, willing and eager to discard a favorite theory in the face of even one contradictory data point—a view still widely held in the public's imagination.

But according to Hull, this obstinate behavior is not a bad thing, but in fact ensures that “scientific hypotheses get a run for their money”(Hull 1988, p. 13).

Zuckerman and Merton agree that resistance plays a positive role in science, preventing fads from sweeping science (Zuckerman and Merton 1972). Kuhn writes, “Individual variability in the application of shared values may serve functions essential to science....

If all members of a community responded to an anomaly as a source of crisis or embraced

¹¹² Hull and others have proposed evolutionary mechanisms for science's advancement based on natural selection (Darwinism), but the units of selection in these models largely are *scientific theories* rather than *scientists themselves*, who, *pace* Planck, are presumed to be able to alter their views.

each new theory advanced by a colleague, science would cease....” (Kuhn 1970 [1962], p. 186). As I. Bernard Cohen says, “If every revolutionary new idea were welcomed with open arms, utter chaos would be the result” (Cohen 1985, p. 35).

Indeed it has also been suggested that “it may be good for the community for some diehards to remain, since they may produce problems for the revolutionaries to solve” (Worrall 1990, p. 348). Examples of scientists providing such a “gadfly effect,” as Stephen Brush calls it, might be the Dutch meteorologist C.H.D. Buys-Ballot, and the English scientist Franciscus Linus. The former’s criticism of the kinetic theory’s inability to explain why such extraordinarily fast-moving molecules (as the theory entailed) took so long to traverse a room spurred Rudolf Clausius to think more deeply about molecular collisions, and to introduce the concept of the mean-free-path. The latter’s criticism of Robert Boyle’s theory of air pressure ultimately resulted in the formulation by Boyle of his eponymous Law (Holton and Brush 2001, p. 319).

For scientists who have worked within a successful research tradition for many years, it is switching to a new one too quickly that would seem illogical and unfounded, withholding commitment to the new model until the evidence for it accumulates further (Rappa and Debackere 1993, p. 6). “Given a crisis or an innovation,” Hagstrom writes, “the older generation may firmly believe in the possibility of reconciling it with established theory” (Hagstrom 1965, p. 283-284). “The individual scientist is wisely predisposed to favor the type of advance which he knows and believes in from personal experience,” Holton writes (Holton 1988).

Imre Lakatos writes, “Criticism of a programme is a long and often frustrating process and one must treat budding programmes leniently.” Discovery of an inconsistency need not “*immediately* stop the development of a programme: it may be rational to put the inconsistency into some temporary, *ad hoc* quarantine, and carry on....” (Lakatos 1970, p. 179, 143). Lakatos goes on to cite the case of Niels Bohr’s theory of the atom, which at first could not explain the doublets of the hydrogen spectrum, but “Bohr was not upset: he was convinced...his research programme would, in due course, explain and even correct” the observations of the doublets, which it did (Lakatos 1970, p. 150).

And what of the extreme emotion that accompanies resistance to new theories, particularly from rival groups? Is this “unscientific?” Far from it. Hermann Bondi writes of his dislike “for the widely purveyed picture of scientists as objective and cool, working and thinking in a thoroughly impersonal manner. This absurd view is unhappily held by many who could enjoy science and contribute to it” (Bondi 1998, p. 2)¹¹³ “Fervent devotion to the cause advances science,” David Hull adds, seeing such competitive impulses as hallmarks of science, as much as cooperation is (Hull 1988, p. 370). Rivalries between scientists indeed do often become intense and exchanges vitriolic. But this is the norm in science, in Hull’s opinion, not an aberration. “Scientists acknowledge that among their motivations are natural curiosity, the love of truth, and the

¹¹³ Bondi goes on to add that this myth can only be dispelled “by seeing [science] in the making. Preferably such a picture should be conveyed by its active creators, presenting their corners of the subject in a thoroughly personal manner and in a widely intelligible form....” (Bondi 1998, p. 2).

desire to help humanity, but other inducements exist as well, and one of them is to ‘get that son of a bitch’”(Hull 1988, p. 160).

Hull thus to some degree rejects Merton’s norm of “universalism” in science—whose theory it is *does*, or at least can, matter (Merton 1973). But, again, in Hull’s view such “baser” motivations are useful, and are actually one of the engines of scientific development. Competition between research groups influences not only forces the rival groups to reanalyze and defend their results, it also reinforces the social cohesiveness of both groups.¹¹⁴

An example of the above might be seen in Ian Mitroff’s important work on the Apollo moon scientists. In this study, Mitroff found the scientists he interviewed, in sharp contrast to the stereotype of the disinterested scientist, “were affectively involved with their ideas, were reluctant to part with them, and did everything in their power to confirm them” (Mitroff 1974, p. 586). Citing copious examples of extraordinary emotionality on the scientists’ parts, Mitroff found that “Every one of the scientists interviewed...indicated that they thought the notion of the objective, emotionally disinterested scientist naïve,” and further, that “strong reasons were evinced why a good scientist ought to be highly committed to a point of view” (Mitroff 1974, p. 587-8).

¹¹⁴ In Hull’s view, science is also by nature elitist—not just anyone has the skill set necessary to do science. In defending this elitism over a more democratic ethos, Hull writes, “Why should science mirror what we value in society” (Hull 1988, p. 158). He similarly wonders why we should expect scientists to be particularly humble, and shirking of praise (e.g. awards and accolades), as is the stereotypical image. “The desire for recognition by one’s fellows is very close to a cultural universal among human beings....” (Hull 1988, p. 283). In Hull’s view, the desire for recognition—to be in the spotlight—is another social mechanism that advances science, driving scientists to produce results.

Scientists “could not react to a theory without reacting simultaneously to its proponents” (Mitroff 1974, p. 585).

Even those scientists in the study critical of such behavior in their colleagues, paradoxically, agreed the colleagues who were the most dogmatic, vindictive, and arrogant were “among *the* most outstanding scientists in the program” (Mitroff 1974, p. 586). Mitroff, citing Barber and Merton, suggests that such behavior best explained as the result of a dynamic tension between two conflicting sets of norms (faith in the moral virtue of rationality, emotional neutrality, universalism, communism, disinterestedness, and organized skepticism) and counter-norms (faith in the moral virtue of rationality and nonrationality, emotional commitment, particularism, solitariness, interestedness, and organized dogmatism) in science.

So it is acceptable, and even *expected* for scientists to “wriggle” and “fudge” to attempt to preserve the theories to which they are committed. Kuhn views such stubbornness as “inevitable and legitimate,” and part of “what makes normal science...possible.” As such it is of critical import, since “it is only through normal science that the professional community of scientists succeeds....” (Kuhn 1970 [1962], p. 152).¹¹⁵ Kuhn concurs with Hull that some acceptance of individuals’ resistance to change may be a social structure built into science to ensure its progress at the most macro level:

¹¹⁵ For many analysts, these kinds of commitments make Kuhn’s *Structure* essentially a very conservative (reactionary?) work, rather than the radical piece as which it is often portrayed.

In matters like these the resort to shared *values* rather than to shared *rules* governing individual choice may be the community's way of distributing risk and assuring the long-term success of its enterprise (Kuhn 1970 [1962], p. 186, emphasis added).

That is, the notion of an algorithm or ironclad set of rules prescribing a clear-cut point at which theory change is mandated in a given situation is not even a desirable thing. Resistance to change serves a positive function in science. The scientists who soldier on in the old model not only serve as “gadflies,” but also provide a “fallback” position to which the community can retreat. That is, in the (perhaps increasingly unlikely) event the new paradigm to which the community is converting ends up being a *cul-de-sac*, research in the old one does not have to resume at “ground zero.” Thomas Nickles writes, “It would be foolish, given the uncertainty and risk of the judgments, for the scientific community to risk everything on a single research program—or a single, tight, scientific method” (Nickles 1996, p. 29).

“However, at some point it is clear that if [scientific wriggling] becomes too pervasive, the scientist ceases to be a ‘scientist,’” writes Hull (Hull 1988, p. 280). In other words, “Scientists need not abandon their most fundamental views in the face of a single apparent counterinstance, but they cannot totally ignore data either” (Hull 1988, p. 281). Kuhn writes something similar in the following passage:

Though the historian can always find men—Priestley, for instance—who were unreasonable to resist for as long as they did, he will not find a point at which resistance becomes illogical or unscientific. At most he may wish to say that the man who continues to resist after his whole profession has been converted has *ipso facto* ceased to be a scientist (Kuhn 1970 [1962], p. 159).

In Kuhn's view, Joseph Priestley's lifelong resistance, in the face of mounting evidence, to the idea he had himself discovered oxygen (as opposed to “dephlogisticated air”) was *unreasonable* but not *unscientific*. What Kuhn seems to mean here is that if we

accept the premise that resistance to change is a fact in science, and indeed a positive aspect of it, it follows immediately that even protracted resistance is allowed for in science writ large, and thus by definition cannot be “unscientific.” Thus though Priestley’s *reasoning*, upon which he based his resistance, may be called into question (i.e., he was *unreasonable*), his resistance itself was not *per se unscientific* in Kuhn’s view.

In the final sentence of the quote above, Kuhn may seem paradoxical: how can someone behaving scientifically (i.e., resisting theory change) cease being a scientist by virtue of doing so? The apparent contradiction is resolved by the crucial phrase “*ipso facto*.” Specifically, though the holdout scientist has behaved consistently (and, again, all the while “scientifically,” according to Kuhn), the community’s mass conversion to a new paradigm without him has left him now socially and professionally isolated—not a member of the (new) scientific community and thus not a scientist in that sense. (Recall that for Kuhn, part of the definition of who a scientist is involves being a member of a specific community of individuals who identify their members as scientists.)¹¹⁶

Hull concurs, but goes a bit further, seeing the isolated scientist as not just *ipso facto* no longer a scientist, but as having crossed over into the realm of “non-science.” Hull, further, sees such isolation as a “safety valve” of sorts to prevent recalcitrance in the community from getting out of hand. The larger social structures of science thus limit this behavior from spreading, in turn preserving the integrity of the field as a whole.

¹¹⁶ I thank Joe Pitt for suggesting the addition of this phrasing. Richard Burian’s reading of Kuhn suggests that the only thing that would make a scientist “unscientific” in Kuhn’s view would be switching out of one paradigm without going to another (Burian 2005).

Scientists who cling too long to a theory viewed as outmoded will be ostracized, considered fringe figures in the fields they formerly led—a state their colleagues would (presumably) not want to emulate.

Even the most elect scientists are not immune to such intellectual banishment. Though held in the highest esteem for his lifetime body of work, Einstein's (in)famous reluctance to embrace quantum mechanics provoked a reaction summed up by Max Born, who regarded Einstein's "aloof and skeptical" attitude as "a tragedy—for him, as he gropes his way in loneliness, and for us who miss our leader and standard-bearer" (Born 1949, p. 161-2). More dramatic examples of this include Fred Hoyle's lifelong dedication to the steady state model of the universe and Linus Pauling's contention that megadoses of vitamin C cure various illnesses. Hoyle and Pauling clung to their respective theories until their dying days, long after the scientific community had issued a negative verdict on each. It is fair to say such willful ignoring of the copious evidence contradicting these scientists' pet theories damaged Hoyle's and Pauling's reputations seriously. Thus the social structures of science permit instances of "Planck's Principle"—encourage them, even—but simultaneously constrain them from getting out of hand.

This quote may help to shed some light on one of the more obscure passages in Kuhn's *Structure*:

Lifelong resistance, particularly from those whose productive careers have committed them to an older tradition of normal science, is not a violation of scientific standards, but *an index to the nature of scientific research itself* (Kuhn 1970 [1962], p. 151, emphasis added).

This sentence is one of the many in *Structure* that frankly acknowledges the existence of recalcitrance in the scientific community in the face of contradictory evidence. It is often used by critics to bolster the claim that Kuhn's view of science is, in its most basic essence irrational; i.e., the "nature of scientific research itself" is based upon the blatant disregard of experimental evidence—precisely the opposite of the traditional view.

However, given Kuhn's strong tendency to disassociate himself from the relativism often ascribed to him, an alternative reading of "an index to the nature of scientific research" suggests itself. Since resistance to change in science is "legitimate and inevitable" in Kuhn's view, it only makes sense that there will be "outliers" in the spectrum of resistance, at one end of the Gaussian distribution, just like in any other population. They have taken resistance to its logical extreme, just as there will be some scientists at the other end of the bell-shaped curve who are all-too-eager to adopt a new model. The lifelong resisters, indeed, may have a positive role, as described above, in terms of being gadflies or "distributing risk," just in case the new theory ends up being a blind alley.

It seems strange that a scientist of Planck's erudition could not see the positive value in scientists' resistance to new ideas. As Bernard Barber puts it, Planck's

bitterness is not tempered by objective understanding of resistance as a constant phenomenon in science, a pattern in which all scientists may sometimes... participate.... Instead such bitterness takes the moralistic view that resistance is due to 'human vanities,' to 'little minds and ignoble minds' (Barber 1961, p. 597).

But does this resistance, often evinced by scientists (as described above), translate into, as Planck suggested, their carrying their pet theories with them to their graves?

Despite post-positivist philosophy of science claims to the contrary, it is, in fact, very easy to find examples of scientists' having significant changes-of-heart regarding theories—and Kuhn himself acknowledges this. “Scientific communities have again and again been converted to new paradigms,” he writes. “Though some scientists, particularly the older and more experienced ones, may resist indefinitely, most of them can be reached in one way or another” (Kuhn 1970 [1962], p. 152).

This explicit qualifier of Kuhn's is often overlooked, by both post-positivist critics of science, as well as *their* critics. For instance, John T. Blackmore writes that “Thomas Kuhn and many of his followers have been misled by Planck's much-quoted observation and have treated it as more reliable and important than it is.” Blackmore then goes on to point out that *Planck himself* is a counterexample to his own principle, having adopted Ludwig Boltzmann's statistical interpretation of the second law of thermodynamics¹¹⁷ after having opposed it for over twenty years. Blackmore then points out Hermann Helmholtz was an ardent opponent of Young's three-color theory of perception¹¹⁸ in 1852, but by 1858

had altered his opinion on scientific grounds until he became its chief advocate such that it is now called 'The Young-Helmholtz Theory'....Ostwald, the leading opponent of atomic theory...since 1892...was influenced to change his mind in 1908 and accept the indispensability of the 'atomic hypothesis'.... (Blackmore 1978, p. 347-8).

¹¹⁷ This is the notion that microscopic entropy may spontaneously decrease, i.e., there is a finite probability that spontaneous ordering will occur in microscopic systems in which the particles are moving essentially randomly. However, this probability is vanishingly small.

¹¹⁸ This model proposed that retinas contain three kinds of receptors, for red, blue, and green light. These three kinds then would interact to register all the other colors.

Like Planck, Helmholtz, Ostwald, and many others one might mention, Sciama seems to be a clear counterexample to Planck's Principle; in this case, Saul did indeed become Paul. Helge Kragh agrees with this assessment, writing that Sciama's "metamorphosis is interesting also from the point of view of the philosophy of science, contradicting...the Planck-Kuhn thesis that supporters of an old paradigm do not convert to a new one, but carry their paradigmatic beliefs to their graves" (Kragh 1996, p. 337).

The case of Sciama's conversion is of particular interest, however, because of the way in which he converted. In particular, his conversion from the steady state to the big bang seems closely to parallel that of James Jeans' conversion in 1912 from being one of the most ardent opponents of the quantum theory early in the twentieth century to being one of "its chief early missionaries." In the following section I draw heavily on the work of Geoffrey Gorham, who has studied Jeans's change-of-heart, in order for me to compare the stages of conversion that Jeans went through to those of Sciama.

Jeans and the Quantum Theory

Between 1900 and 1905 James Jeans and Lord Rayleigh engaged in a debate (often played out in public forums such as the pages of *Nature*) over the correct (classical) formula for radiation distribution. This expression, arrived at in 1905, and known as the Rayleigh-Jeans Law, has the form:

$$E_R(\lambda T) = 8\pi RT\lambda^{-4} dT$$

With this accomplished, Jeans turned his attention to attacking Planck's quantum version of the law, which contained a new constant of nature h (later called Planck's constant):

$$U_{\nu} = \frac{8\pi\nu^2}{c^3} \left(\frac{h\nu}{\exp\left(\frac{h\nu}{kT}\right) - 1} \right)$$

Thus after 1905, for Jeans the debate was “no longer confined *within* a theoretical research tradition, but was now between the old and a radically new paradigm” (Gorham 1991, p. 478). A significant problem for Jeans, however, was that Planck's formula was in better agreement with the experimental data, whereas the classical version only comported with the data for low frequencies, and in fact diverged for high frequencies.¹¹⁹

Jeans criticized Planck's formula for the entropy S of a system of oscillators, $S = k \log W$. His objection to this formula was based on its use of the probability function W , which was a measure of the number of ways the oscillators can be distributed—a combinatorial description of molecular disorder. Jeans charged that such a function had no determinate meaning, and likened it to asking “What is the probability that the temperature of a gas shall be T , or that the gas shall be hydrogen.” This objection, however, only made sense if viewed from a classical viewpoint. Planck saw W as a “*definition* of the probability of a given complexion of oscillators;” he used this definition as a mathematical device to help count the possible number of states in the system, a crucial development that helped lead to the quantum radiation law. Thus Jeans

¹¹⁹ This is the so-called “ultraviolet catastrophe.” There is no evidence that Planck's motivation in developing the quantum theory had anything to do with this solving this problem, despite frequent assertions to the contrary (see Kuhn 1987).

is charging Planck with a *methodological* impropriety in his derivations, namely, that *W* had no “definite and independent meaning” (Gorham 1991, p. 479-80).

Jeans then went on to criticize Planck’s relation for quantized energy:

$$\varepsilon = h\nu$$

This equation was another crucial link, required by the Wein displacement law, in the mathematical chain that led Planck to his radiation law. However, if one takes the limit of $\varepsilon \rightarrow 0$ (i.e., if h is taken as 0), Planck’s radiation law reduces to the Rayleigh-Jeans formula. Jeans thus argued that (classical) statistical mechanics required Planck to do just this, completely missing the point of Planck’s new approach. Further, Jeans urged this step, setting $h = 0$, be taken despite the fact that it would *worsen* the formula’s agreement with the data. Jeans wrote:

Of course, I am aware that Planck’s law is in good agreement with experiment...while my own law, by putting $h = 0$, cannot possibly agree with experiment. This does not alter my belief that the value $h = 0$ is the only value which is possible to take....
(Gorham 1991, p. 481).

Thus Jeans is expressing an *aesthetic preference* for the empirically inferior model, based on his metaphysical allegiance to the notion of continuity, as opposed to discontinuity. (Recall that for McAllister, metaphysical allegiance is a form of aesthetic preference.) As for why the classical approach failed empirically at high frequencies, Jeans’s explanation was that in experimental conditions true equilibrium between the energy of the ether and matter is never reached for such frequencies—a condition on which the classical formula is predicated. Thus he *presumes* the Rayleigh-Jeans Law to be correct even for high frequencies, even though experiment could never demonstrate

this fact.¹²⁰ Gorham points out that such a claim vis-à-vis equilibrium “had never received any experimental support.” It can be argued this *ad hoc* maneuver only called into question the (classical) fundamental philosophical premises that drove Jeans to make the suggestion to begin with (Gorham 1991, p. 482).

Between 1905 and 1910 (and particularly between 1909-10) Jeans continued to attempt to reconcile classical mechanics with the experimental data. He derived revised formulae for the radiation of energy emitted by both imperfect (i.e., real-world) as well as perfect blackbodies; the latter had to include a special exponential factor for short wavelengths to bring it into agreement with data. He continued to suggest that there is a difference between true equilibrium and a kind of faux equilibrium (which he called a “steady state”), which to all appearances was like normal equilibrium, but was really not so, depending on “features peculiar to the material system in question.” Again, this had no empirical basis (Gorham 1991, p. 483).

Jeans’s attempts to save the older model, Gorham argues, ironically, only served to underscore the dire straits that classical theory was in, and as such, his “work of 1909 and 1910...actually served, quite contrary to [his] intentions, to hasten the rejection of the classical theory.” In any case, Jeans had soon admitted that his “method of saving the phenomena within the classical theory came at a considerable cost to otherwise strongly

¹²⁰ Again, one is reminded of the Aristarchan/Copernican *ad hoc* hypothesis that the reason one can’t see naked-eye stellar parallax is due to the stars’ being much further away than previously thought—a hypothesis that was also not testable with the (pre-telescope) technology of the day. This hypothesis, however, happens to be correct.

held physical assumptions.”¹²¹ Jeans, it seems, was starting to see the handwriting on the wall (Gorham 1991, p. 484-5).

At the Solvay conference of 1911, Jeans and his colleague Henri Poincaré were the only two participants with a “clearly negative position with respect to the quantum theory.” At the conference, Jeans presented his theories on the difference between “steady states” and “true equilibrium” in systems, and insisted that the abandonment of continuity was premature—even if he did not offer an “explicit defense” of classical theory beyond this (Gorham 1991, p. 486-7). A turning point was reached at this meeting as Poincaré was won over to the quantum theory. His conversion Gorham cites as being “almost certainly” the final factor that convinced Jeans to make the switch as well in 1912.¹²² Jeans thought highly of Poincaré, with whom he shared many qualities in common, including an interest in the philosophical implications of physics.

Jeans then chose a very public forum—the 1913 meeting of the British Association for the Advancement of Science—to announce not only his switch, but that his desperate attempts to save classical theory were a “dead end” (Gorham 1991, p. 487). Of his intellectual struggle, Jeans wrote (quoting Poincaré) of the quantum hypothesis being

¹²¹ Namely it implied a “very unlikely law of force between the atom and the electron,” as well as an *exactly* equal (and miniscule) time for *all* collisions occurring in the system—both very dubious propositions.

¹²² Specifically, Jeans’s reading of Poincaré’s paper “*L’hypothèse des Quanta*,” according to Gorham, was the crucial moment. Gorham also cites as a significant but less-important factor in Jeans’s conversion the increasing adoption of the quantum theory to solve classical problems in Jeans’s native Britain—particularly by Jeans’s Trinity colleague William Nicholson (Gorham 1991, p. 487).

so strange a hypothesis that every possible means was sought for escaping it. The search has revealed no escape so far, although the new theory bristles with difficulties, many of which are real and not simple illusions caused by the inertia of our minds, which resent change (Gorham 1991, p. 488).

Jeans spoke of Poincaré's work as showing "there is no middle way" and therefore that he was "logically compelled to accept the quantum hypothesis in its entirety." Gorham writes, "Poincaré's gentle declaration that all hope for escape was now exhausted was what was needed to finally turn Jeans over to a defender of the new theory" (Gorham 1991, p. 489).

Jeans's conversion, when it finally came, was complete and unequivocal. This is perhaps best illustrated by the "missionary role" he adopted in promoting the new model, in particular with his *1914 Report on Radiation and the Quantum Theory*.

Sciama and the Big Bang

I find that Sciama went through an almost identical sequence of stages in his conversion to the big bang model of the universe. Recall that Jeans's initial attack on the quantum theory was a *methodological* indictment; Sciama, in his criticisms of the big bang model of the universe seemed to take a similar tack. Recall that Sciama repeatedly argued that the steady state model of the universe was to be preferred over the big bang one due to its lack of "arbitrary" initial conditions required to match observation. For instance, in 1959 Sciama wrote concerning the big bang model's theory of galaxy formation, which is predicated on turbulent gas coalescing in a specific way:

[N]o reason is given why the turbulence had just these characteristics, rather than quite different ones. Its actual characteristics are thus an accidental initial condition devoid of theoretical significance. As a result the sizes of galaxies are purely accidental....This state of affairs, while logically possible, is very unsatisfying. We should surely keep to a minimum those elements of our experience which we are forced to regard as arbitrary (Sciama 1959, p. 187).

Then, Jeans's arguments against the quantum theory took on an aesthetic character. Likewise, Sciama throughout the late 1950's and early 1960's repeatedly offered his view that the steady state model of the universe was superior aesthetically to the big bang model, as we saw in Chapter 2, and was thus worth defending even in the face of increasingly hostile data. Was such ignoring, or at least holding in abeyance, of data, justified?

Early experimental data do indeed often turn out to be unreliable. Take, for instance, Walter Kaufman's apparent disconfirmation of special relativity in 1907 based on electron mass measurements. Einstein and others went about their business despite these troublesome data, and were ultimately vindicated for doing so. Einstein, wrote, "Whether or not there is an unsuspected systematic error or whether the foundations of relativity theory do not correspond with the facts one will be able to decided with certainty only if a great variety of observational material is at hand" (quoted in Holton 1988, p. 235).

James Clerk Maxwell continued developing kinetic theory, despite its being contradicted by experimental results on both the viscosity and the ratio of specific heats of gases. The theory ultimately was vindicated despite these apparently contradictory data. "In this case," writes Brush, "the theory refuted the experiment... [Maxwell] would not abandon an otherwise plausible and successful theory simply because it had failed to

account for *all* the experimental facts” (Brush 1974, p. 1169). There are any number of quotes by Eddington, Dirac, and other highly-regarded scientists supporting this view. The astrophysicist and cosmologist E.A. Milne wrote, for instance, “The theoretical scientist must have the courage to stick to his theories...in spite of attacks from the laboratory worker... observations are by no means like the law of the Medes and Persians, which altereth not...” (Milne 1952, p. 8-9).

Just as Kaufmann’s data, and the data that vexed Maxwell, were indeed unreliable, the Stebbins-Whitford data and much of Ryle’s Cambridge 2C data were as well, not to mention the failed α - β - γ model. Sciama’s viewing them all with a skeptical eye was vindicated. From his point of view, then, arguably it would be reasonable to attempt a similar strategy with the 3C data, the quasars, and the cosmic microwave background radiation as well.

When the hostile data could not be impeached, we saw that Jeans’s next stage of defense was to proposing various auxiliary hypotheses to “save” classical radiation theory. Sciama’s experience again matches this pattern. When the data from radio source counts, quasars, and the CMBR became more and more reliable, Sciama switched tactics to proposing in his own words “slightly artificial” models to explain away the new phenomena and save the steady state. These included the hypothesis that there were two distinct classes of radio sources, that we were in a “local hole” in the distribution of one of them, the identification of the quasars with this population, and the new model for the CMBR, as discussed in Chapter 2.

When Jeans's conversion came, it was due to the catalytic effect of a trusted colleague, Henri Poincaré. Once again, we see similarities with Sciama. By the early 1960's Sciama had become aware of significant difficulties in the steady state model of the universe, and had been working to explain them away with various *ad hoc* assumptions. The turning point for Sciama came when his student Martin Rees checked the quasar distribution plot that Sciama had done, which had apparently confirmed the steady state model. Reminiscing in 1989, Sciama recalled,

The idea was to defend the steady state...so I plotted out the number-redshift relation...it was sloppy.... [Martin Rees] came back and said, "I've done it properly, and it's very bad for the steady state...." I looked at what he'd done, and I agreed that he'd done it properly. That was the thing that for me made me give up steady state. There was a conceivable let out from people like Hoyle and Geoffrey Burbidge, who were then saying that quasars are local, but I didn't like that.... It really wasn't reasonable. I said, "Okay, quasars are cosmological, and therefore this decides it." So, for me at least—though not for most people—it was this study that was decisive, and I had a bad month giving up steady state (Lightman and Brawer 1990, p. 143-144).

This was difficult for Sciama, who later recalled his dismay at the universe's not being in a steady state:

feeling very upset—not I think because I had been shown to be wrong, because I never said I thought the theory was right, there was [sic] no grounds for thinking it was right, as it were, if you see what I mean; it was rather that one would like to be right for these other reasons. I was upset that I wasn't right (Sciama 1978, p. 26).

The roles of Poincaré and Rees in these conversions underscore once more how science is a social enterprise. Reflecting in 1978 on his initial, too-optimistic plotting of the quasar data, Sciama said, "That's no doubt an example of the mind wanting a certain result." However, his student Rees kept Sciama honest; Rees "had no particularly great investment of emotion" on the matter (Sciama 1978, p. 26). In another interview, Sciama drew a parallel with another scientist of the twentieth century, E.A. Milne, who had also

proposed a heretical cosmology (“kinematic relativity”) which was considered by many to be highly speculative. However, Milne did not have a similar group of advisees to serve in a similar role as Rees and others had to Sciama:

Milne would never have gotten away with the nonsense part of his work...if he'd had some good critical students. My students like Hawking and Rees and so on wouldn't allow nonsense, would've roasted me if I'd proposed a thing like [kinematic relativity]...[Milne] was never criticized right at home, and he could always object to external criticism in one way or another (Sciama 1990).¹²³

Thus, *pace* the claims of some postmodern critics of science, not just any old reading of the raw data will do. All is not interpretation. In science, the real world intrudes into the proceedings, as vividly demonstrated here—but with the able assistance of the scientist's network of students and trusted colleagues.

And finally, like Jeans, when Sciama did finally make the switch of models, he was far from quiet about it, “blazing forth” (to use his own term) the news of his conversion, to any and all audiences that would listen. Sciama's repeated statements regarding the death of the steady state theory are mirrored in Jeans's attempts “to cut off further efforts to reconcile the classical and quantum theory,” explicitly “recanting on all his former efforts to save the classical interpretation” and stating that “it seems useless to attempt to explain away the conflict between the radiation-laws and the classical mechanics by ingeniously devised special models....” (Gorham 1991, p. 491-2).

¹²³ Note also that Milne's case seems to jibe with Hull's notion earlier that scientists may be more receptive to criticisms within their research groups, but dig in their heels once committed publicly.

So Sciama did as well. Far from being silent on the matter (or leaving cosmology altogether as many others disillusioned with the steady state did¹²⁴) Sciama used every opportunity he could vocally to underscore how the model was no longer supported, whereas the big bang model was supported, by increasingly reliable data. Sciama became an ardent supporter of and researcher in the very model he had resisted for so long, in the words of Bernard Jones, embracing “the new Hot Big Bang Theory and...working on the cosmic singularity, radio source evaluation, galaxy formation, and other relevant topics” (Jones 1993, p. 159). Again, this is similar to Jeans’s “missionary” work for the quantum theory.

Summary

The parallel between the conversions of James Jeans and Dennis Sciama is not perfect; Jeans was willing to go further during his ad hocery period than Sciama, who seems to have had his limits, as described in Chapter 2. Recall Sciama never signed onto the Hoyle-Burbidge notion that *no* quasars could be local in order to save the steady state model.¹²⁵ Jeans, on the other hand, was at least temporarily willing to suggest a completely unheard-of form of non-equilibrium equilibrium to explain away the classical

¹²⁴ Brush has compiled a list of a great many steady state cosmologists who stopped publishing in the field after the theory’s demise. See (Brush 1993).

¹²⁵ Sciama later wrote of this, “Do the red-shifts of the quasars demand a new law of physics,” as many steady state proponents were suggesting? “My own view is that in discussing these localised phenomena, one should work extremely hard to fit them into the *accepted* laws of physics. Only after persistent failure should one introduce new laws; otherwise science loses one of its most important characteristics—its internal discipline” (Sciama 1973a, p. 56).

radiation formula's lack of agreement with the data.¹²⁶ Further, it is not clear that Sciama's attempts to save the steady state model had the unintended effect of hastening the demise of his pet model, as Jeans's seem to have.

Still, the many parallels that do exist between the two situations are striking. Both men were at the center of research in their old models, and were wedded to said models due in large part to aesthetic and philosophical considerations. Both men went through a prolonged stage of trying to "save" their models; the strategies of this stage included both attacking and ignoring experimental data as well as methodological indictments against their opponents. Both men's eventual conversions necessitated a change in worldview—though it is unclear if Sciama's met the criteria for a full-blown Kuhnian paradigm switch.¹²⁷ Both converted only after the catalytic action of a respected colleague's also converting--in the case of Sciama, his student Martin Rees; in the case of Jeans, Henri Poincaré. Once converted, both then vocally denounced their original models, and became active researchers in and promoters of the new model.

¹²⁶ Another difference between the two men is that in 1928 "Jeans ceased original research all together and devoted himself to the popularization of science" (Gorham 1991, p. 492). Sciama, on the other hand, while considering writing popular articles "very important" (Sciama 1978, p. 32), continued doing original research right up until his death.

¹²⁷ It is unclear whether cosmology circa this era should be considered a "mature" science. In many ways the situation in cosmology at this time resembles Kuhn's description of the "pre-paradigm" stage of a science's development, in which competing schools debate fundamentals, etc. In *Structure*, Kuhn suggested the transition to the 'mature' phase of the science is marked by the establishment of a paradigm (Kuhn 1970 [1962]). However in 1972, Kuhn repudiated this view, saying that even in the 'immature phases' of the science's development there are paradigms (Kuhn 1977). Some commentators mark cosmology's transition to a mature science with the 1965 discovery of the cosmic microwave background radiation, which can be argued established a definitive, empirically-backed paradigm for the science for the first time (Brush); others place cosmology's transition to a science much earlier (e.g., Gale, McCrea, Kragh). For a fuller discussion, see (Hunt 1996).

Study of scientific conversions such as those of Jeans and Sciama are important for two reasons. First, as Gorham points out in his paper, radical, dramatic conversions such as those of Jeans and Sciama demonstrate an “especially notable” exception to “Planck’s Principle,” and thus to the post-positivist notion that “scientific progress [is] essentially independent of proof.” That is, at the end of the day, both men’s conversions were based on argument, not an irrational gestalt “moment” (Gorham 1991, p. 493).

However, the other important lesson to be gleaned from the two men’s conversions is that scientific progress is far from the cut-and-dried “scientific method” presented in textbooks. As Thomas Nickles puts it,

Once thought necessary to (explain) scientific progress, a rigid method of science is now widely considered impossible....Such a method would make progress impossible....It is not so much science studies experts so much as laypersons, including college administrators, who believe in a single, definite scientific method—as something that every student should be taught....Having learned the right method, any fool could do science just about as well as any other—and in exactly the same way. There would be minimal scope for individual initiative or skill under such a regime (Nickles 1996, p. 9, 12).

Instead, the evaluation of scientific theories often requires a weighing of intangible qualities, a willingness to hold in abeyance troublesome data (if not ignoring it altogether), and a social process of give-and-take between trusted colleagues. The process lays bare aesthetic and metaphysical concerns frequently glossed over in the standard accounts of science presented in textbooks. Resistance to scientific change not only exists, but it is logical—it is to be expected that scientists become invested in their theories, both professionally and emotionally. Further, it can even be argued that resistance serves a useful function in the scientific community.

In the next chapter I will go into more detail on some of the implications of the period that many scientists, such as Sciama and Jeans, go through in attempting to retool a scientific theory to incorporate hostile data.

CHAPTER 5
ITERATIVE THEORY CHANGE

Oh, thou hast a damnable iteration, and art indeed able to corrupt a saint.

—Falstaff, *Henry IV*

The Duhem-Quine Thesis

Sciama's behavior in the period leading up to his conversion in 1966 is not unique; it is easy to find in the literature examples of other scientists' going through periods of attempting to preserve or defend their "pet" models against hostile data, often by means of hypotheses considered ad hoc or artificial, even by the standards of those scientists themselves. As discussed in the previous chapter, even though it could be argued this resistance to change serves a positive function, commentary on these periods nevertheless is often harsh, with such periods seen as unfortunate wastes of time. The inference behind such quotes is that scientists should, well, *know better*, in some sense, than to behave so—that such reactions to data are a bad thing.

Indeed, that scientists can always find a way to "save" a theory without altering fundamental assumptions leads, at its extreme, to one of the most discussed ideas in STS:

the so-called Underdetermination or “Duhem-Quine” thesis (D-Q).¹²⁸ According D-Q, there is no clear-cut distinction between theory and observation; all observations are always interpreted in terms of a vast, interconnected matrix of background theories and assumptions; Quine in particular discussed the scientific endeavor in terms of the metaphor of a “man-made fabric which impinges on experience only along the edges.” “Crucial experiments” are on this view *prima facie* impossible, as it will always be possible instead to alter a background assumption to account for the data in question, rather than actually giving up the main theory allegedly under test (Quine 1953 [1951], p. 42).

But since hostile data can be accounted for by adjusting said fabric’s strands in any number of ways (an infinite number, really),

the total field is so underdetermined... that there is much latitude of choice as to what statements to reevaluate in the light of any single contrary experience....Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system (Quine 1953 [1951], p. 43).

Because of this, philosophers such as Larry Laudan suggested “spreading the blame for an anomaly evenly among the parts of the theory” (Darden 1992, p. 254). But Quine went even further, concluding that rejecting a theory wholesale was not sufficient to address underdetermination, and that instead, “The unit of empirical significance is the whole of science” (Quine 1953 [1951], p. 43). That is, only *the entirety of science itself* can be evaluated by any given experiment (as opposed to one specific theory).

¹²⁸ It has been pointed out by Roger Ariew and others that that the “Duhem-Quine” thesis as it is usually stated bears little resemblance to Pierre Duhem’s actual feelings on the matter; the same argument can even be made to a lesser degree vis-à-vis Willard von Orman Quine as well. See (Ariew 1984).

In modern day STS, the Duhem-Quine thesis is often slapped down on the philosophical table like a trump card to put the kibosh on any talk of rationality in theory choice. The argument goes like this: since, in principle, a scientist cannot know for sure that the fault she's identified in a theory is really what is to blame for the discrepancy with data (it could be due to any number of the other [infinite] assumptions in the Quinean web), an unavoidable consequence of D-Q is that it is *impossible* to localize and fix problems within any particular theory. Thus science cannot be a progressive enterprise.

The only problem with the Duhem-Quine thesis is that it is patently false, at least insofar as it purports to describe science as it actually happens. Every day in labs around the world scientists routinely isolate problems in theories and fix them, theoretical protests as to the impossibility of same notwithstanding. STS commentators' blithely ignoring this obvious fact is reminiscent of Parmenides' argument that change in the world is impossible in principle;¹²⁹ when faced with the thousands and thousands of kinds of change and motion apparent in the world, in unkind defiance of their supremely logical conclusions, the Parmenideans' response was, essentially, "This can't be happening." When faced with obvious progress in our knowledge about the world, science critics often respond similarly, with a straight face, insisting that, really, no honestly, we don't know any more about the world than we did in Aristotle's day—well, because Quine proved we cannot in principle.

¹²⁹ The stadium and Achilles-and-the-tortoise paradoxes of his student Zeno come to mind as well.

Just as Diogenes of Sinope refuted Parmenidean arguments against motion by getting up and walking around, in recent years, in fact, a small but growing number of academics have begun pointing out the nakedness of the Duhem-Quine Emperor.

William Wimsatt writes,

There is a mythology among philosophers of science...that a theory or model meets its experimental tests wholesale, and must be taken or rejected as a whole. Not only science, but also technology and evolution, would be impossible if this were true in this and in logically similar cases. That this thesis is false is demonstrated daily by scientists in their labs and studies.... (Wimsatt 1987, p. 30).

And Douglas Allchin writes, “Investigators often rule out error by dissecting or teasing apart...observational variables experimentally. That is, they *resolve*....” (Allchin 2001, p. 46, italics added).

Regarding Quine’s claim that a theory can always be saved from hostile data, Ian Hacking calls this “ill-argued,” and says it “illustrates another kind of sloppiness. From the historical fact that hypotheses have sometimes been saved it is inferred that hypotheses can always be saved” (Hacking 1983, p. 251). Jarrett Leplin writes,

It might be suspected that theories do not have essential propositions, since a theory can compensate for rejection of any individual proposition through modifications elsewhere. This shows at most that theories need not have essential propositions. In fact, they do have them.

Leplin goes on to point out that the adjustments needed in such a case may be quite “peripheral to the theory, perhaps not even formulated explicitly” (Leplin 1975, p. 327).

Wimsatt and others take this criticism of D-Q a step farther, seeing episodes of error or anomaly recognition and subsequent theory adjustment not embarrassing lapses in scientific behavior, but rather as the very key to understanding the scientific endeavor

itself. In this chapter, I will discuss several scholars' approaches to this anomaly and error, then I will attempt to apply their thought to the Sciama case.

Anomaly-Driven Theory Redesign

While much philosophical attention was given to anomalies in 20th century philosophy of science, Lindley Darden finds that

somewhat surprisingly, methods for anomaly *resolution* have received comparatively little attention. Popper, for example, concentrated on falsifying instances as indicators of the inadequacy of a theory, but gave no hints as to how to use the anomaly to localize and correct the problem to produce an improved version of the theory (Darden 1992, p. 254, italics added).

Darden sets out to remedy this situation with a model for addressing faults in a scientific theory she terms “anomaly-driven theory redesign” (ADTR).

In ADTR, first an anomaly is *confirmed*—a process that itself can be nontrivial. For as Susan Leigh Star and Elihu M. Gerson point out, “The very definition of an event as anomalous is negotiated and context-dependent” (Star and Gerson 1986, p. 149). Initially, that is, some may see a phenomenon as a genuine discovery and others may not. “Anomalous data might turn out to be the artifact of particular experimental procedures, or they might be the result of misguided interpretations of low-level experimental results,” Kevin Elliott writes. “Therefore, researchers who encounter an anomaly¹³⁰ may not be sure precisely what error is responsible for the anomaly; it could be a fluke result,

¹³⁰ Note the sloppiness of language here; some analysts define an anomaly as any kind of unexpected datum; thus a simple experimental error would qualify as such. Others consider only true discoveries, not fluke results, to be anomalies definitionally. Thus they distinguish between “anomalies” and “true” or “genuine anomalies.”

an experimental error....” (Elliott 2003, p.7). Obvious ways to confirm whether or not a genuine anomaly has been found at this state include repeating the experiment, checking the calibration of instruments, reviewing the statistical analysis of the data, and so forth. Presumably, overwhelmingly most unexpected results are at this stage found to be due to such errors, and are thus not true anomalies. As such, they do not make it into the published literature.

That minority of unexpected results that cannot readily be chalked up to error and the like go to the next stage of ADTR, as confirmed anomalies. “Once people recognize an anomaly...it begins to create more interruptions,” Star and Gerson write.¹³¹ As such, anomalies are not always welcome, despite their potential import. “The appearance of a discovery actually can be an unwanted interruption to routine research,” they say (Star and Gerson 1986, p. 149, 152).¹³²

In Darden’s scheme, anomaly *classification* comes next. According to Darden there are two basic types of anomalies: monster anomalies and model anomalies. Monster anomalies are those that are the result of a unique case or a peculiar malfunction of the theory in question, and as such do not require adjustment.¹³³ “Monsters can be localized in such a way that they do not pose a problem for any component of a particular theory, either because the anomaly is outside the scope of the theory’s domain or because

¹³¹ Star and Gerson go so far as to *define* anomalies as interruptions to scientists’ work flow.

¹³² A classic example of this is probably William Herschel’s accidental discovery of Uranus on March 13, 1781 during a routine sky survey. Thinking it a mere comet, he at first he expressed some irritation that it was distracting him from his survey.

¹³³ This term derives from (Lakatos 1976).

it is rare or atypical,” Darden writes (Darden 1991, p. 258). That is, a monster “is an anomaly that does not present a challenge to the general theory; it is a phenomenon that is abnormal” (Darden 1995, p. 142).

Model anomalies, on the other hand, require some kind of theory change.

They

show the need for a new exemplar; they turn out to be examples of a typical, normal pattern than had not been included in the previous stage of theory development.... After the theory change, the [model] anomaly turns out not to have been an anomaly at all (Darden 1995, p. 137, 143).

In other words, monsters fail in a particular case, models in numerous ones (Darden 1990, p. 341).

A good illustration of the difference between these two types of anomaly may be found in Lucien Cuénot’s 1905 discovery of 2:1 ratios in mice, rather than the 3:1 ratios expected from Mendelian genetics. Once it was confirmed as genuine, Thomas Hunt Morgan and Cuénot separately proposed explanations for this anomaly. Morgan impeached the purity of Cuénot’s breeding dominants (and by extension the purity of gametes in general), and thus called into question Mendel’s essential discovery of segregation. Cuénot proposed simply that some germ cells fertilized selectively, rather than randomly. Both hypotheses could explain the 2:1 ratios, but at the cost of altering Mendelian genetics significantly. Thus Morgan and Cuénot considered the 2:1 ratios a *model* anomaly. W.E. Castle and C.C. Little instead proposed that certain gene combinations were lethal; embryos resulting from these pairings died off, thus giving only the appearance that 3:1 ratios did not obtain, when in actuality they had. Castle and

Little thus barred this anomaly as a *monster*, requiring no alteration of Mendelism (see Darden 1991, Chapter 8).

In addition to the two basic types of anomalies (model and monster), Darden also suggests a third type: the “special case” anomaly. “A special case anomaly shows what is *normal in a few, special cases*. It is not a monster in the sense of being a *malfunction of the normal*. Nor is it a model anomaly in the sense of being a *general model representing most cases...*” (Darden 1995, p. 153, italics in original). Special case anomalies indicate what is normal in a small class of instances, but are different from the norm. That is, special case anomalies require only “minor theory change” (Burian 1996, p. 326).

An example of a special case anomaly concerns the so-called “central dogma” of molecular biology, put forth in 1958 by Francis Crick, which has to do with information flow between DNA, RNA, and protein polymers. It might be symbolized thus:

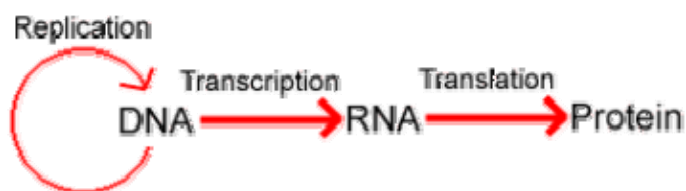


Figure 5. The Central Dogma. From <http://library.thinkquest.org/18258/retrovirus.htm>.

However, Howard Temin in the 1960’s made the bold “provirus hypothesis,” suggesting that sometimes there was an inversion of this information flow, from RNA back to DNA, with a hitherto unknown enzyme carrying out the reverse transfer:



Figure 6. The Provirus Hypothesis. From <http://library.thinkquest.org/18258/retrovirus.htm>.

Subsequently, he and David Baltimore in 1970 independently isolated the predicted enzyme (“reverse transcriptase”) that facilitated this reaction; for this they later shared the Nobel Prize. Many scientists, upon learning of this discovery, attempted to use it to revise radically, or even overthrow, the central dogma altogether; that is, they considered it a *model* anomaly. However, Crick instead maintained it should be considered merely a special case anomaly instead, that it did not require “a new model to show the usual flow of genetic information in most organisms”(Darden 1995, p. 143, 153).

This process of anomaly identification and classification can be a lengthy one, as Richard Burian points out:

A long and difficult process must intervene between finding a potential anomaly and determining that it cannot be accommodated by existing theory—i.e., that the anomaly is an appropriate vehicle for producing at least a certain amount of change in the available theory and that it is useful tool for guiding theory redesign.

He also writes, “It should be clear there can be no sharp boundary between the various classes of anomalies, especially when they are first explored, for their import cannot be known in advance.” Burian also points out that since scientists with different backgrounds will necessarily approach each anomaly differently, “they will generate a variety of approaches....[and] often vary greatly in the degree of ‘depth’ they assign to a potential anomaly,” leading to quite different solutions (Burian 1996, p. 329).

Next in Darden's view comes the step that the Duhem-Quine model explicitly denies as being possible: *localization*. Darden describes anomaly localization as the process of either shifting an anomaly outside a primary theory's domain or identifying a particular theory component as problematic. That is, localization "identifies particular theory components as candidates for alteration" (Elliott 2003, p.8, 20). Darden points out that similar localization of faults is common in fields such as artificial intelligence (AI),¹³⁴ so it is not at all outrageous to suppose, *pace* D-Q, scientists can successfully accomplish this as well. This stage would correlate to what Deborah Mayo calls "error probing," which includes "the design of separate severe tests for the multiple auxiliary hypotheses" of a theory "in order to isolate the specific locus of difficulty" in a theory; in Mayo's picture, use of error statistics buttresses this approach (Elliott 2003, p.7).

Allchin suggests developing a typology of errors to aid in this process.

No method is yet available for pinpointing a theoretical error immediately and unambiguously. But a catalog of possibilities can guide or enhance search...such a list can aid a systematic scan of all possible errors....Strategies for anomaly resolution can prompt consideration of numerous possibilities or alert investigators to options otherwise overlooked. One major role for a typology of error, then, is guiding analysis of anomalies or discordant results that signal the likely presence of error (Allchin 2001, p. 50-51).

Once an anomaly is localized, scientists can then tinker with the appropriate components of the theory in an attempt to incorporate the anomaly; much of Darden's work is focused on this redesign process. "Providing a new hypothesis to resolve the

¹³⁴ In fact, Darden suggests a fruitful approach to localization and the theory redesign that follows might be the construction of flow charts, along the lines of those used in AI, to suggest "modular" steps in the theory development process. "Such a stepwise representation can be a useful guide in hypothesis generation," she writes (Darden 1990, p. 334).

anomaly is like fixing a faulty component in a device or providing treatment for a disease,” she writes (Darden 1990, p. 319). Darden continues:

Reasoning in design involves designing something new to fulfill a certain function, in the light of certain constraints. Redesigning theoretical components involves constructing a component that will account for the anomaly, with the constraints of preserving the unproblematic components of the theory and producing a theory that satisfies criteria of theory assessment (Darden 1992, p. 255).

To address anomalies, scientists may either alter a current component of the theory, or add a new component. Strategies for altering current components of the theory include: deleting, generalizing, specializing, complicating, or “tweaking” various components of the theory (Darden 1991, p. 269).¹³⁵

One take on this hypothesis-generation process is due to N.R. Hanson, who, “drawing on the work of Charles Peirce, argued for an alternative to the hypothetico-deductive method called ‘retroduction’ or ‘abduction’” (Darden 1987, p. 36). Abduction is often referred to disparagingly as “affirming the consequent,” i.e., suggesting that:

$$P \rightarrow Q$$

Q, therefore P

While strictly speaking a logical fallacy, and thus offensive to earnest college students taking sophomore logic, science nonetheless often proceeds just by this method—albeit in not so simplistic a form.¹³⁶ William Wallace refers to this process as

¹³⁵ Darden is quick to point out that scientists do not consciously or explicitly address anomalies in the terms she uses. Her “claim is not a historical one, that any particular person consciously employed a specific strategy. Instead, the claim is that a change between a component of the theory at one time and that component at a later time ‘exemplifies’ a strategy” (Darden 1991, p. 4).

¹³⁶ For Peirce, abduction was not just an inference to a possible explanation, but the inference to the *best* explanation, and therefore the more *probable* explanation.

the “demonstrative regress,” and points out that the key legitimizing step lies between the two steps above, when the scientist does the “work of the intellect” in ruling out other potential causes, besides P, that might have made Q obtain. Wallace points out that Aristotle himself explicitly authorized such effect-back-to-cause reasoning in the *Posterior Analytics*. Wallace identifies numerous examples of the demonstrative regress in Galileo’s work (Wallace 1993).

“After hypothesis generation, the next task is to assess the adequacy of the various alternatives”; that is, the revised model is then evaluated (Darden 1990, p. 342). Of course, if the ADTR process has addressed the anomaly adequately in the community’s eyes, the revised theory is then accepted and the community moves on. However, if the ADTR process was not judged as successful three possibilities then present themselves for dealing with the situation.

The first possibility is that various scientists could simply “have another go” at revising the model, generating more or different hypotheses to address the problem at hand. Here, again, is illustrated what Nickles calls a “multipass system...involving feedback” (Nickles 1997, p. 18). And it is here that Darden’s analysis reveals the true strength of the scientific process. As Burian points out,

the process is an *iterative* one, with feedback loops from each step to the preceding ones; it can heighten anomalies, or reduce them to artifacts or ‘clouds on the horizon.’ Continued traffic among these various phases of theory redesign is a central feature of the activity of a scientific community successfully engaged in the attempt to fathom the unknown (Burian 1996, p. 330).

If, on the other hand, the community judges the anomaly to be insurmountable, only two options remain. Either the community chucks the theory altogether in favor of a

new model—in which case the anomaly might be termed “Kuhnian”(Burian 1996, p. 326-7)¹³⁷—or the anomaly is “set aside.” In the latter case, the community continues to use the overarching theory, either simply accepting that it is flawed, or in the hopes that some future scientist might find a way to rectify things. This resonates with the discussion in Chapter 4 on scientific resistance to switching models, and, as Star and Gerson write, “underscores the importance of work organization and commitments over pure logic in determining scientific results” (Star and Gerson 1986, p. 153).

Examples of such “set-asides” are numerous. The “age of the universe” anomaly in Big Bang cosmology, mentioned in Chapter 2, comes to mind, as does the advance of perihelion of Mercury’s orbit. This anomaly in Newtonian gravity theory, discovered by U.J.J. LeVerrier in 1859, was tolerated for nearly sixty years before Einstein’s general relativity dealt with it. And Star and Gerson cite the adoption of Gould and Ethridge’s punctuated equilibrium model, which explained away what was otherwise chalked up to be anomalous or incomplete data for traditional Darwinism that had been tolerated for some time.

Though these anomalies all falsified their respective theories in the strictest sense, scientists put up with them nonetheless. Allchin writes, “Error is common in scientific practice. But pervasive error threatens neither the search for trustworthy knowledge nor the epistemic foundations of science” (Allchin 2001, p. 38). Gloria Hon writes, “Falsity

¹³⁷ Burian is clear that he doubts the existence of Kuhnian anomalies (“at least in biology”), but lists them as part of his discussion for completeness/argument’s sake (Burian 1996, p. 327).

need not undermine the theory itself if it is ‘isolated’” (Hon 1989, p. 482). Or, as Wimsatt puts it,

The total collapse [upon encountering a contradiction] suggested by first-order logic...seems not to be a characteristic of scientific theories. The thing that is remarkable about scientific theories is that the inconsistencies are walled off and do not appear to affect the theory other than very locally....When an inconsistency occurs, results which depend on one or more of the contradictory assumptions are infirmed. This infection is transitive; it passes to things that depend on these results...like a string of dominoes—until we reach something that has independent support...The collapse propagates no further (Wimsatt 1981, p. 134).¹³⁸

Richard Burian writes that the piecemeal change suggested by ADTR is much more likely a reaction to anomalies, generally speaking, than is immediate, wholesale Kuhnian overthrow. He also points out scientists such as M.J. West-Eberhard have suggested such a process independently of Darden:

The apparatus of “anomaly-driven theory redesign”...goes some distance toward the recognition of what [West-Eberhard] has in mind....The key innovation is the use of a set of iterated steps, with corrective feedback....Model testing in accordance with anomaly driven theory redesign is comparative.... (Burian 1996, p. 334-5).

Heuristic Appraisal

Another lens through which to view scientists’ struggles to save theories from contradictory data involves what Thomas Nickles calls “heuristic appraisal.” Nickles sees the stereotypical “scientific method” as a mere “sausage grinder” that “captures very little of scientific practice” (Nickles 1996, p. 12, 16). He argues the standard

¹³⁸ Wimsatt links this ability to isolate and tolerate anomalies to a theory’s “robustness,” which he defines as its being able to be evaluated by different types of evidence or experiments; that is, it is a kind of overdetermination. “*Only robust hypotheses are testable*,” he writes. “A theory in which most components are multiply connected is a theory whose faults are relatively precisely localizable. Not only do errors not propagate far, but we can find their source quickly and evaluate the damage and what is required for an adequate replacement” (Wimsatt 1981, p. 136, italics in original).

hypothetico-deductive method (H-D), upon which the “scientific method” is predicated, has been carried too far.

Initially, H-D’s development was a positive thing, “an early 19th century Romantic reaction against Baconian induction and Enlightenment methodology.” That is, in sharp contrast to strict, bottom-up, data-driven methods, H-D, he argues, “permits deep theories, thrives in data-sparse environments, permits a division of labor between theory and experiment, and lets scientists use logical reasoning as if premises were established.” It “encourages risky entrepreneurship, and hence individuality,” as well as “group initiative and competition.... Scientists are liberated from strict logic and are free to use rhetorical devices such as analogy and metaphor and, indeed, the resources of the free imagination....” (Nickles 1996, p. 14-15).

But the “H-D model is too simple and linear, too single-pass,” in Nickles’ view, to capture what really happens in science. He writes:

Important scientific results rarely persist in their original form. Rather the techniques and/or derivations are streamlined, some features are identified as ‘noise’ and eliminated, other items are classified as errors or failures and perhaps explained, and relations to other work are clarified....The world rarely presents itself in an ideal, order-of-knowing sequence....Accordingly, it takes multiple passes for us to sort things out (Nickles 1997, p. 19).

We develop techniques, make trial runs, and propose hypotheses, and then we correct and streamline them as more information comes in....cognitive economy requires that we start simply and add refinements sequentially, that we simplify, lump, generalize, universalize, and methodize or routinize....As we learn more, the new knowledge forces us to return to reappraise our previous results (Nickles 1997, p. 20).

In real science a successful claim or technique itself becomes the focus of intense interest, rather like a novel phenomenon....A search space is constructed around it. Scientists “wiggle” (vary) it and tinker with it in various ways until they attain a better understanding of the structure of that space. This enables them to streamline, generalize, and even to methodize the result by incorporating it into a new or revised search

procedure. This process sometimes goes through several stages of refinement, in which the noise, blind alleys, unhelpful variants, logical gaps, and other deficiencies are eliminated (Nickles 1996, p. 21).

In light of this, Nickles writes, scholars have since the 1970's proposed a three-stage model for the scientific process, inserting between the familiar stages of discovery and justification an intermediate stage called "heuristic appraisal" (HA); others, including Larry Laudan, call this intervening phase (or something like it) the "context of pursuit" (see Laudan 1977). It is this intermediary "consideration phase" of HA that is the focus of much STS scholarship.

In HA, the standards and procedures scientists maintain are different from those of the stereotypical, cut-and-dried logic purported to obtain in the context of justification.

In HA, Nickles says, scientists utilize

another type of accounting system (actually a whole family of them), for the different purpose of assessing opportunities and opportunity costs....HA is more concerned with whether something is do-able, whether it is possible, whether something is a genuine opportunity, or a more inviting opportunity than something else (Nickles 1996, p. 30).

It is in HA that so-called "external factors" impact the development of the science significantly, and in which the "motivational side of inquiry," often ignored in traditional accounts, may be seen to be at work (Nickles 1996, p. 33).

HA, according to Nickles, contains

all manner of assessments of the comparative prospects, the promise, the likely fertility, the opportunity profile, of just about anything in science....It is the collective HA of the relevant scientific communities that defines the frontier of research and thus determines the overall direction of research.... (Nickles 1996, p. 28).

Thus, far from being a kind of purgatory between the creative rush of discovery and the hard-nosed justification phase, HA has a critical role in shaping the future of the science in question. As the stakes are so high, scientists often engage in rhetorical flourishes to

bolster the fortunes of their models—a fact often ignored in “traditional confirmation theory,” according to Nickles, who says that rhetoric is in fact

surely a better indicator of the conceptual and experimental growth points of science—the frontier—than is logic, which presupposes an already formed, stable, clear terminology....HA must persuade...HA must instill optimism (or pessimism).... (Nickles 1996, p. 32)¹³⁹

As discussed in Chapter 3, the fact that less-than-strictly-logical methods are found to be operative in this phase of science is far from a cause for alarm. “Society is unlikely to invite future philosopher kings to redesign science along more ‘rational’ lines, since such an attempt would be expected to straitjacket science,” Nickles writes. That is, it is from the crucible of HA, messy as it is, that contenders for legitimized and ultimately reliable knowledge ultimately emerge.

“So, must we conclude that science is an irrational, chaotic jumble?” Nickles asks?

No....A global methodology is precisely what we do not want....[While] there is no overarching Rationality of Science anymore than there is a General Method of Science... Nonetheless there is a lot of rationality in a quite ordinary sense in the more local judgments and decisions of HA. In many cases HA involves calculating a kind of return on investment (Nickles 1996, p. 43, 37).

Nickles’ stance is reminiscent of Kuhn’s opinion that scientists rely on an intuitive, perception-like expertise, not on rules. William Wimsatt agrees, writing positively of the

replacement of the vision of an ideal scientist as a computationally omnipotent algorithmizer with one in which the scientist as decision maker...[who] must consider the size of computation and the cost of data collection, and in other very general ways must be subject to considerations of efficiency, practical efficacy, and cost-benefit constraints. This picture has been elaborated over the last twenty-five years by Herbert Simon and his co-workers, and their ideal is ‘satisficing man,’ whose rationality is bounded....A key feature of this picture of man as a boundedly rational decision maker is the use of heuristic principles where no algorithms exist or where the algorithms that do

¹³⁹ The rhetoric of science has been addressed in (Moss 1993) and (Gross 1996), among many others.

exist require an excessive amount of information, computational power, or time¹⁴⁰ (Wimsatt 1981, p. 153).

The “evaluation space” that HA provides encourages the existence of a proliferation of models, or at the very least prolongs the lifespans of such models as they are considered by the community. Again dovetailing with the discussion in Chapter 4, Nickles sees this as central for the scientific endeavor, writing that “it is not rational to put all our eggs in one basket. Group rationality demands that there be variation.... Science should have a broad investment portfolio” (Nickles 1996, p. 37). “Opportunistic competition within a pragmatic framework” is key to science’s advancement, he writes, with blind variation and selective retention replacing “method or logic of discovery. Variation is crucial. Too much consensus...at this stage would soon produce sterility” (Nickles 1996, p. 43; Nickles 1997, p. 30).

To bolster his case, Nickles also cites Paul Feyerabend, who invoked John Stuart Mill in stressing the importance of free speech in the scientific process; HA’s tolerance of multiple lines of inquiry would certainly seem to fit with this. Also, in addition to adding to the number of ideas in the scientific marketplace to be considered by the community, variation provides another benefit. Nickles writes, “a position can be adequately developed and clearly understood only in response to critical opposition.” He calls this

¹⁴⁰ Wimsatt defines a heuristic principle as having three “important properties:” a solution to the problem at hand is not guaranteed; it takes less time and effort than an algorithm, and the failures and errors produced when a heuristic is used are not random but systemic (Wimsatt 1981, p. 153).

back-and-forth between opponents “a major source of multi-pass conceptions of inquiry” (Nickles 1997, p. 25).¹⁴¹

In Chapter 1, I discussed the important role of case studies in STS. Nickles makes a powerful argument that the same import can to be assigned to them in science proper as well:

HA...attempts to convert hindsight...into (a fallible, limited) foresight, or at least to convert past successes into heuristics for future research. For this reason, we would not expect there to exist a precise, uniform method of HA. Rather, HA is likely to issue in particular judgments (usually practical decisions or simply practical responses) informed by training and experience, sometimes ‘case-based’ judgments backed by citation of relevantly similar cases. Knowledge of past cases can at least inform us of what might or might not happen, in a more realistic way than logic can.... (Nickles 1996, p. 29).

HA enables us to make more sense of appeals to concrete historical precedents.... This is important because HA is a form of reasoning based on casuistry, on consideration of precedents, rather than rules.... And at the opposite end of the spectrum from rules, skilled practice seems to be shaped by collective experience in an even less explicit manner. Here one cannot usually recall specific cases; one ‘just knows’ what to do by a kind of experienced intuition (Nickles 1996, p. 39).

The work of Nickles and Darden cast a new light on Sciama’s actions leading up to 1966. In the next section, I will explore this.

Sciama’s Actions Reconsidered

Anomaly-Driven Theory Redesign

¹⁴¹ Nickles goes on to expand his notion of multipass inquiry in science into a kind of “reader response theory” of sorts, in which the very meaning of scientific text is continually redefined based on subsequent discoveries, understanding, and interests. He goes on to propose such an idea this not only to scientific inquiry, but all human inquiry in general. “Each historical ‘moment’ involves a refashioning of the epistemic situation. That is what I mean by saying that all human inquiry is multi-pass in this very basic sense. Previous knowledge claims are constantly, yet almost imperceptibly, fed back through gradually changing interests and goals and capabilities,” he writes (Nickles 1997, p. 21). These ideas, while provocative, are beyond the scope of this paper.

Many, including Sciama himself, have expressed some regret or embarrassment at his having tried so hard to “save” the steady state model in the face of increasingly hostile evidence. However, the work of Darden puts Sciama’s actions in quite a different light. Now, Darden’s work, while groundbreaking, has heretofore focused on instances of anomaly-driven theory redesign that were ultimately successful. However, I find that Sciama in his attempts to save the steady state model, while in the end unsuccessful, went through the sequence of steps exactly as Darden outlined.

As discussed in Chapter 2, the first significant challenge to the steady state model was the Stebbins-Whitford Effect in 1948. Sciama thought this “hostile evidence was rather weak” and that the steady state was “too beautiful and desirable a theory to be defeated by weak arguments” (Sciama 1978, p. 15-16). Here we see Sciama participating in Darden’s first stage of ADTR—namely, assessing whether an anomaly presented is indeed genuine, and not instead explainable by some error. Sciama, along with Bondi and Gold, charged that this in fact was not the case since the data used by Stebbins and Whitford in their paper, based on the galaxy M32, were atypical. This nicely illustrates Star and Gerson’s depiction of a “complex trade-off between defining anomalies as mistakes or artifacts on the one hand and as discoveries on the other....” (Star and Gerson 1986, p. 152). In the end, Sciama’s criticism was upheld as valid, and this was formally recognized in the literature in the mid-fifties.

Sciama took a similar tack in addressing the next hostile data, namely Martin Ryle’s 1955 2C survey of radio sources. Though the analysis of this data indeed did seem to indicate a violation of the perfect cosmological principle, and thus threatened the

steady state model, Sciama, fresh from his success with the Stebbins-Whitford Effect, took an understandably similar tack, arguing that the data were unreliable. Again, in addition to the Stebbins-Whitford effect, this tactic has copious precedent in the scientific community. Allchin writes of scientists' viewing errors "as fixable problems rather than inherent flaws" (Allchin 2001, p. 40). Also, Burian has written that

M.J. West-Eberhard...has emphasized the importance of giving pre-existing theory its due...it is all too common (and in the investigator's narrow self-interest) to portray interesting findings as more powerful or important than they are by making it appear that they contradict or cannot be fit with existing theory, when, in fact, the resources of that theory have been misconstrued or have not been properly exploited. In Darden's terms, this speaks to the difficulty of establishing that a puzzling finding ought genuinely to count as an anomaly (Burian 1996, p. 337).

However, as the 3C data came in and became more reliable, Sciama had to admit that this did indeed pose a genuine anomaly. As such, the next step was to localize the problem that the steady state now faced. Sciama found this in the usual interpretation of the perfect cosmological principle; what threatened the steady state model in this case was the fact that data had apparently been uncovered that showed the universe was not uniform in both time and space as the PCP maintained. Thus it would be the usual interpretation of the PCP that would have to be altered.

Next came anomaly classification. To use Darden's terminology, rather than barring this anomaly as a monster, which would have required no theory change whatsoever, Sciama acknowledged some theory change was required to address the matter. Thus he saw this as a model anomaly. However, as we shall see, Sciama's goal was to change the PCP, and therefore the steady state model, as little as possible.

The hypothesis-generation phase came next. Sciama's strategy was as follows: Of course the PCP had never required complete uniformity in the universe's mass

throughout time and space—if that were the case there would be no agglomerations of mass whatsoever, and we would not be here to discuss the matter. It is self-evident that the PCP permitted “clumping” of matter such as stars and even galaxies—but if one viewed, say, the galaxies on a large enough scale, the distribution of them would be uniform. Could the same argument not also apply to the radio sources? Sciama pointed out that if the PCP were understood to mean that holes in the distribution of radio sources as large as 17 parsecs in diameter were acceptable (meaning that such things, if viewed from the larger scales, would not be seen as irregularities any more than the holes in a colander would be), there would be no contradiction, and the model would remain consistent.

In Darden’s terms, Sciama complicated an oversimplification; he refined, or clarified, what was meant by the PCP itself.¹⁴² Darden points out that in one sense the prediction and subsequent discovery of reverse transcriptase, might also be seen as complicating an oversimplification, quoting biologists of the time as saying: “The central dogma, enunciated by Crick in 1958 and the keystone of molecular biology ever since, is likely to prove a considerable over-simplification” (Darden 1995, p. 150). In Deborah Mayo’s terms, Sciama’s actions might be viewed as “parameter adjustment,” in which “experimental results are ...accommodated by suitably fixing the values of...

¹⁴² “If bold, general simplifying assumptions marked the beginning stages of theory construction, then specialization and complication will be likely strategies to use as anomalies arise” (Darden 1992, p. 260). Leplin concurs with Darden here, writing, “Although useful in the early stages of the development of a theory, simplifications must be replaced by more accurate or ‘realistic’ descriptions in order to make the theory fundamental. The term ‘fundamentality’ is suggested by the problem of oversimplification” (Leplin 1975, p. 325). Leplin then goes on to cite the example of Bohr’s work on the atom as exemplifying such a “complicating” strategy.

parameters,” the parameters in this case being the scale or level at which “irregularities” in the universe’s mass distribution were not violative of the PCP (Mayo 1991, p. 527).¹⁴³ Another take on situations like these is due to Kevin Elliott, who suggests a number of “strategies that are useful for probing error” in situations like these, including “looking for new variables” and “suggesting plausible mechanisms that might produce the anomalous result,” both of which would seem to describe pretty closely what Sciama was doing (Elliott 2003, p.17).

However, a diameter of 17 parsecs (about 55.4 light-years) in the radio sources put the hole squarely within the Milky Way, so Sciama’s first theory adjustment required another in turn. First, he had to suggest that a significant number of the radio sources were really within our galaxy, and not outside it; this, too, might be seen as complicating a previously-held oversimplification. His further proposal of an entirely unheard-of mechanism to explain how low-mass stars in our Milky Way might become converted to such radio galactic sources might be considered what Darden calls “adding a new component.”

The discovery of the quasars posed the next problem for the steady state; again, the issue was that these objects were only seen at great distances, whereas the PCP required that they should be seen everywhere. In this case, Sciama again suggested that galactic quasars would soon be discovered, thus rendering the quasar anomaly

¹⁴³ Another lens through which to alter this change is as a change of scope, i.e., that the PCP was applied at too small a level. Allchin discusses errors of scope: “Scientists sometimes err by assuming or promoting too broad a range of application (domain or territory). In such cases, the critical issue is not whether some rule, law, claim, or model is true or false (even probably so), but under which precise circumstances it holds and does not hold” (Allchin 2001, p. 47).

nonexistent. Sciama's building on the work of Véron and Longair, suggesting that the too-steep slope of the Log N-Log S graph was due entirely to the quasars and not the non-QSO sources, might be an example of what Darden calls "delineate one component into two and change one but not the other" (Darden 1991, p. 272). Sciama thus honed in or resolved the scope of the anomalous data, showing the scope of the problem was smaller than the community might think—not all radio sources posed a problem for the model after all, he suggested, only the QSO's. If, as he predicted, galactic quasars were discovered, the anomaly would be rendered moot altogether. Thus Sciama suggested that the PCP/steady state—as it had already been modified/reinterpreted by him to account for the 2C/3C data—needed no further modification by him at this time. Thus the second iteration of the ADTR process, prompted by the discovery of the quasars, was held in abeyance at the very first stage, with Sciama not ready to concede that the quasar "anomaly" was not simply an apparent problem only due to incomplete data.

Finally, Sciama's reaction to the cosmic microwave background radiation was initially similar to his approach to the 2C data, in that he initially proposed that the radiation might not be of a blackbody nature after all. He then, however, suggested that the blackbody nature of the CMBR might be apparent and not genuine. He proposed a mechanism under which an integrated population of radio sources might produce radiation of the same apparent character, without being truly blackbody in nature.¹⁴⁴ If

¹⁴⁴ Note Sciama suggested now two concrete causal *mechanisms* (one for galactic radio sources, one for the CMBR). Mechanisms are also a source of study for Darden, who writes, "Wimsatt suggested how mechanical and causal models might aid in forming hypothesis for resolving anomalies that arise for them; his analysis extends that of Hesse" (Darden 1992, p. 255).

true, alterations in no significant components in the steady state model were required. In Darden's terms, Sciama proposed barring this as a monster, just as Castle and Little had explained the 2:1 ratios in mice without changing any of the main planks of Mendelian genetics.

Thus we see Sciama engaging in ADTR and using several of Darden's suggested hypothesis-generation strategies.

Heuristic Appraisal

Repeatedly, Sciama made clear that he thought the steady state model was a theory worth fighting for, due to what he saw as its positive aesthetic features (including its being able to make life possible at all times) as well as its superiority methodologically to the big bang model. Sciama saw the steady state model as being potentially more fruitful than the big bang model, in that more things (such as the values of the universe's parameters) might be explained by the laws of physics, rather than being merely coincidental. He also relished in provoking the establishment, being one of the "young Turks." Necessarily these evaluations were made in the context of some sort of comparison with the big bang model.

The prism of Heuristic Appraisal also goes a long way in helping us understand Sciama's actions in this period. In the "context of justification," scientists are not "supposed" to use in their arsenal of arguments things such as were mentioned in the previous paragraph. However, viewing this period as part of the looser, more tentative

“context of pursuit,” in which the rules of the game are different, puts things in a better perspective.

On more than one occasion Sciama mentioned that while he fervently “hoped” the steady state were true, he did not know for sure. Consider:

Once I decided I liked the steady state theory, even though we didn't know it was true... [O]ne could find these slightly artificial models of the discrete population of radio sources. Although slightly artificial, I felt the price wasn't too high for the virtues of the steady state theory, as judged at that time (Sciama 1978, p. 16, 26).

This seems to indicate a scientist, while partisan, who is still not 100% convinced that his model is the correct one. However, the potential benefits he sees from it made it certainly worth continuing to work on, fervently, even as the hostile evidence grew stronger. HA “is the means by which scientists make decisions about which problems to work on,” Nickles writes. In HA,

assessment implies the use of some sort of accounting system (though it does not imply use of a rule-based procedure of making individual judgments; the accounting system may be loose and informal).... The HA question is not... ‘Is it true,’ but the pragmatic, process question, ‘Will it work here?’ (Nickles 1996, p. 28, 29, 31).

Sciama’s emotional defense of the steady state model, his passionate arguments for its superiority over the big bang model were on more than one occasion quite rhetorical, even in the face of negative data; “I would get a bit intense and argue eagerly and so forth,” he wrote (Sciama 1978, p. 15-16). However, again, though, it is in the HA stage that such strong rhetoric such as Sciama’s, as discussed earlier, is expected to be the norm and not the exception: HA is the stage in the scientific process in that advocates for theories “carve out” a niche in the scientific marketplace, in which they get on the “radar screen” of the community; in a competitive environment, in which intellectual and material resources are scarce: it makes perfect sense that scientists use

every rhetorical tool at their disposal in the service of their models. “HA must persuade...HA must instill optimism (or pessimism),” Nickles writes (Nickles 1996, p. 32).

Earlier thought might have dismissed such passion as not only untoward but also inappropriate. Under Popper’s regime of theory falsification, a ruled-out theory is something to be celebrated, not tenaciously defended; this winnowing-down process is the only way for science to progress in his view, since theories are logically impossible to confirm conclusively. “There is no such thing as bad information,” Michael Ruse writes. “Famously, for Popper the name of the game is falsifiability—the aim of the scientist must be to show false the most cherished of hypotheses—and a negative finding is the best possible grist for the mill” (Ruse 1999, p. 302). But, as Allchin writes,

Popper was no research scientist. His claims betrayed an idealization of science as governed by relatively simple formal logic and expressing all its conclusions in the form of universal laws...In practice, the art of falsification is more subtle. Researchers must consider methodological assumptions, statistical analyses, details of experimental design and test conditions that Popper never fully addressed (Allchin 1999, p. 303).

Another interesting feature of HA is the sense of playfulness and creativity that scientists feel when evaluating potential theories to pursue. For instance, the Princeton mathematician Manjul Bhargava has spoken of his approach to theorizing that “I still feel like I play around. Yeah, mathematician [*sic*] life is still very much like a playful existence” (Bhargava 2004).¹⁴⁵ Dirac once wrote of his “playing around with” certain

¹⁴⁵ Bhargava goes on to discuss the beauty he sees in mathematics, linking this to a feeling of “creative release,” a “sense of enlightenment” in which “something that was sort of unclear suddenly falls into place.” He also links this beauty to a sense of “surprise;” admirers of Bhargava’s work also mention this, his mathematics weaving “together seemingly unconnected ideas to reach a surprising and elegant conclusion” (Bhargava 2004).

matrices in 1927, which ultimately led him to an accurate wave equation for the electron, work which “all followed from a study of pretty mathematics, without any thought being given to...physical properties of the electron” (Dirac 1982, p. 604). Similarly, Sciama has written of this in describing his attempt to save the model against the background radiation:

When that was discovered, I was still supporting steady state, because in the very early days of that, one could again start making models [that save the theory].... I made a model...of a new kind of radio source. It was a perfectly reasonable object, whose integrated emission would simulate what had then been observed of the microwave background. *It was a challenge; it was rather fun....* (Sciama 1990, emphasis added).

The world is very weird.... Therefore the fact that a proposal is weird or different from established traditions and so on...doesn't mean it's unthinkable or you shouldn't even take it seriously. And in order to determine which weird things are true and which weird things are false, *you've got to play with the ideas* and see what they look like.... (Sciama 1990, emphasis added).

In this sense of play, Sciama's again mirrors his idol G.H. Hardy. C.P. Snow writes of Hardy's being “clearly superior to Einstein or Rutherford or any other great genius” in his “turning any work of the intellect, major or minor, *into sheer play*, into a work of art” (Snow 1967, p. 13, emphasis added). And Hardy's value of this creative play in theorizing is evident in this passage, in which Hardy writes disparagingly of the plight of the “mere” applied mathematician:

If he wants to be useful, he must work in a humdrum way, and he cannot give full play to his fancy when he wishes to raise such heights. ‘Imaginary’ universes are so much more beautiful than this stupidly constructed ‘real’ one; and most of the finest products of an applied mathematician's fancy must be rejected, as soon as they have been created, for the brutal but sufficient reason that they do not fit the facts (Hardy 1967 [1940], p. 135).

At such talk of the joys of creating “imaginary universes” from Hardy, one cannot help but think of Sciama and the steady state.

The competitive, free-wheeling environment of HA encourages such creativity and fun in a way in which a more rigid, rule-based scheme might not. Such zest, even élan, for concocting alternative hypotheses is something that has not been studied extensively. “A clever theorist can build an alternative hypothesis and hope to gain a ‘market share’ from the scientific community,” Nickles writes (Nickles 1996, p. 15).

This proliferation of theories—often of wildly different types—in turn drives the engine that makes science progress through the mechanism of “BV + SR” (blind variation plus selective retention). “Science must tolerate a certain amount of difference, even dissent,” Nickles writes. “Ironically, the abundant proliferation...of hypotheses... turns out to be economical”(Nickles 1996, p. 15-6). Lindley Darden quotes Linus Pauling: “You have a lot of ideas and you throw away the bad ones...this is how to get good ideas” (Darden 1998, p. 2). Francis Crick described in *The Double Helix* of the many failed models produced on the road to ascertaining the structure of DNA. He later wrote:

Theorists in biology should realize that it is...unlikely that they will produce a good theory at their first attempt. It is amateurs who have one big bright beautiful idea that they can never abandon. Professionals know that they have to produce theory after theory before they are likely to hit the jackpot. The very process of abandoning one theory for another gives them a degree of critical detachment that is almost essential if they are to succeed (quoted in Darden 1998, p.10).

And Joshua Lederberg has written that it is a “necessary part of scientific inquiry that alternative, plausible hypotheses be considered” (quoted in Darden 1998, p. 10).

Sciama’s recalcitrance can also be seen to serve yet another function. In helping keep the steady state model alive, and thus maintaining a viable alternative to the big

bang model in the public discussion, Sciama arguably helped prevent the sort of “echo chamber” effect one might get if only one model were in the arena. Allchin writes,

Critical exchange between advocates of discordant theories (at the level of discourse) can help expose the role of contrasting theoretical commitments and lead to resolving evidence more finely to test their different implications.... For example, sometimes mutual criticism among... scientists whose perspectives differ functions as an epistemic system of checks and balances (Allchin 2001, p. 48).

Sciama’s several-years-long period of not giving up the ghost on this model parallels the journey of Harold Jeffreys on the road to accepting general relativity (GR); initially Jeffreys, like Sciama, cast about for alternative explanations to explain away GR’s successes: “Before the numerical agreements found are accepted as confirmations of the theory, it is necessary to consider whether there are any other causes that could produce effects of the same character and greater in magnitude than the admissible error,” he wrote (quoted in Mayo 1991, p. 543). Again, the only difference here is that general relativity turned out to be correct in the community’s eyes, whereas the steady state model did not.

Sciama’s actions to explain away the CMBR or the 3C radio source distribution may be viewed as instances of “exception barring” or “exception incorporation,” which Mayo describes as occurring “when confronted with an apparent piece of counterevidence, one constructs a new hypothesis to account for the exception while still saving the threatened hypothesis....” (Mayo 1991, p. 541). Examples of exception barring can be found to be common in the history of science, and STS critiques of these are often harsh. Millikan (“error high – will not use”) comes to mind. Scholars such as

J. Earman and C. Glymour indict Eddington for exception barring in 1919 with the Sorbal eclipse data used to test GR. But Mayo points out that

as the journals of the time make plain, the numerous staunch Newtonian defenders [i.e., GR opponents] would hardly have overlooked the discounting of an apparently pro-Newtonian result if they could have mustered any grounds for deeming it biased. And the reason they could not fault Eddington's 'exception incorporation'... is that it involved well-understood methods for constructing such a hypothesis (Mayo 1991, p. 542).

In other words, "exception barring" is sometimes methodologically sound practice; the fact that Sciama employed it in an *ultimately unsuccessful* attempt to bolster a theory does not automatically make his attempts to do so invalid.

But when should this practice be entered into, and when should it not? There is no algorithm. Michael Ruse writes:

Doing science is like doing auto mechanics—it is a skill as much book learning. A first-rate mechanic just knows when something strange is up, even if he cannot articulate his feelings. The sound is just not right. Similarly, a first-rate experimentalist just knows when an experiment's failure is interesting. He knows how reliable his test organisms or his equipment or whatever are. He knows when negativity might be more than that (Ruse 1999, p. 303).

Though Ruse is writing specifically about experimentalists, a similar argument can be made for theorists evaluating theories, both in terms of their theoretical components and as well as against experimental data. The intuition that scientists, like auto mechanics, have is not innate—it is developed. "A less rule-based view holds that method is the interiorized, collective wisdom passed down from masters to apprentices," Nickles writes (Nickles 1996, p. 12).

Sciama's attempts to save the steady state model nicely showcase the "multipass" nature of HA. Put another way, such scrutiny and reanalysis of (apparent) disconfirmations can be argued to be a return to a truer understanding of Descartes's

fourth rule of method, “which requires that we review the work thoroughly to make sure nothing has been overlooked;” in the standard accounts of science, according to Nickles, this step was “was largely ignored” and made “feedforward” or “unipass” (Nickles 1997, p. 22). Sciama’s insisting the Stebbins-Whitford and 2C data were suspect come to mind here, as do his hypotheses for the galactic radio sources and CMBR. His identifying the quasars with his “second population” of galactic sources called for in his reaction to the 3C data especially illustrates the iterative nature of scientific inquiry, as he altered *his own revised model* (described in the previous paragraphs) to reflect this new information.

Proposing alternate solutions also ameliorates, as Sunderland emphasizes,

the pervasive cognitive tendency to prefer first solutions and to lower awareness or appreciation of exceptions and alternatives (also noted by Kuhn). A countervailing strategy, then, is systematic review. Because error can masquerade as fact, neither agreement between observation and theory nor concordance of results can, by themselves, guarantee reliability. Deeper reliability depends on demonstrating that the conclusions are also free from error. This gap between ostensible verification and ultimate reliability is a basic principle of error analytics. “Nothing’s concluded until error’s excluded,” so the maxim goes (Allchin 2001, p. 52).¹⁴⁶

Should the fact that Sciama proposed *hitherto-unknown* mechanisms for the production of galactic radio sources and the CMBR be held against him? Machamer et al. write of scientists’ often being “compelled to add new entities and new forms of activity in order to explain better how the world works. To do this they would postulate

¹⁴⁶ Allchin also warns about the dangers of misunderstanding the nature of error in scientific inquiry, and points out that the multipass, iterative nature of HA helps to clarify the nature and role of error and anomaly in science. Sociologists of knowledge, such as Collins and Pinch, he says “succumb to an impotent epistemological nihilism” due to their legitimizing all errors. Nickles agrees, saying such critics look at the initial (more error-prone) efforts of scientists and assume this “stamps the character” of the science forever (Nickles 1996, p. 23). At the other end of the spectrum, others of a more rationalist bent are quick to define all error as “pathological science.” Allchin calls this practice “semantic gerrymandering” (Allchin 2001, p. 40). Instead, Allchin writes, “One can acknowledge error in science without abandoning the goal of reliability” (Allchin 2001, p. 54). The multipass, iterative nature of ATDR

an entity or activity, present criteria for its identification and recognition, and display the patterns by which these formed a unity that constituted a mechanism” (Machamer, Darden et al. 2000, p. 15). Was Howard Temin’s provirus hypothesis not contingent on the existence of a “hitherto unknown enzyme” (Darden 1995, p. 148), reverse transcriptase, which was in fact later found, to copy RNA into DNA?

Is being overtly “for” a particular theory a black mark for a scientist? Does such partisanship render scientists’ work automatically suspect? Sciama certainly was explicit about his “bias in favour of the steady state theory,” writing once, for instance, that “I should warn the reader that I want to see the steady state model survive....” (Sciama 1965, p. 1, 2). “Cognitive limits and biases are...inescapable,” Allchin writes. “Theory-laden perception is normal. Still...one may search for and counteract it...Cognitive bias need not threaten credibility in science. One merely needs to be aware of it and apply a system of checks and balances” (Allchin 2001, p. 47). Sciama certainly seems to be doing the latter, being careful not to give the steady state a “blank check,” but rather walking a line between being an advocate for the model, while fairly constantly stating in a very public way that certain observations would rule it out.

As Elliott wrote earlier, it is acceptable for “plausible” hypotheses to be proposed to attempt to save a model. Here we find the nature of what Sciama was doing. He rejected notions that other steady state defenders (notably Fred Hoyle) had proposed in their attempts to defend the model—such as claiming *no* quasars could be at

and HA help to clarify this. For more on “pathological science,” see (Langmuir 1989 [1953]; Rousseau 1992).

cosmological distances, and that “holes” in the radio sources considerably larger than 17 parsecs would not threaten the PCP. Sciama has explicitly spoken of having had “a real desire to save the [steady state] theory without cheating, against the hostile evidence....” (Sciama 1990). No formal algorithm ruled out Hoyle’s arguments for him, but rather his intuition told him that such arguments as the others were making simply implausible. They went too far. Here we see Sciama exercising what Duhem himself referred to as “sagacity” in his attempts at theory redesign, employing “an intuition we are powerless to justify, but which it is impossible for us to be blind to.” (Duhem 1954 [1906], p. 211, 220). Duhem, quoting Pascal, also writes of this:

These motives which do not proceed from logic and yet direct our choices, these “reasons which reason does not know” and which speak to the ample “mind of finesse” but not to the “geometric mind”, constitute what is appropriately called ‘good sense’ (Duhem 1954 [1906], p. 217).¹⁴⁷

Resistance to growing data, if taken too far, could lead to ostracism, as discussed in Chapter 4. However, up until the moment Martin Rees correctly plotted the quasar data, as described in Chapter 2, Sciama felt the risk was worth it, at least to a degree; the proposed saving hypotheses could be “slightly artificial,” so long as they were not implausible. “As Lakatos pointed out, it is not irrational to play a risky game as long as one is aware of the risks,” Nickles writes {Nickles, 1996 #205, p. 29. But Rees’s correct plotting of 1966 quasar data for Sciama

¹⁴⁷ I note here one similarity between this sort of “situational” theory modification and decision-making and Aristotle’s virtue ethics, one goal of which is to achieve *eudaimonia*, or well-being. This, in turn, requires (among other things) *phronesis*, or practical wisdom. This knowing when to “do the right thing” in a given situation is not rule-based or virtue based—any given virtue (e.g., honesty) or rule (“Do unto others....”) can be inappropriate given certain circumstances.

was the turning point. I could've said, following various people like Hoyle and Burbidge... that maybe the quasar redshifts are not cosmological. And if quasars are local then the n-z relation has nothing to do with cosmology. But I felt that was piling things on too much. My instincts were no, that's unreasonable.... {Sciama, 1990 #42}.

And, as the data on the cosmic background radiation's blackbody nature became more reliable, Sciama writes that the resulting “spectrum was so thermal over such a wavelength range that my little models could no longer cope” and he abandoned further attempts to explain the data within the steady state theory (Sciama 1990).

The notion of a track record loomed large in Sciama's mind as well; while the discovery that quasars evolve with cosmic epoch was the turning point for him in abandoning the steady state model, he had on the other hand made statements like the following, indicating he saw the steady state programme as starting to degenerate:

I must say that if [the quasars] were the only evidence, I don't think I would have abandoned the steady state model. But, taken together with the radio source counts, the evidence is clearly beginning to mount up.... (Sciama 1973a, p.59-60). Taken together with the evidence from the radio source counts and the quasar red-shifts, the excess background of radiation creates very grave difficulties for the steady state theory (Sciama 1973a, p.62-3).¹⁴⁸

Sciama seems to have engaged in a weighing of intangibles—slightly artificial and unheard-of processes on the one hand, the beauty and sweep of the steady state on the other—in considering the potential risks and advantages of continuing the programme. The less-rule-based nature of the heuristic appraisal phase gave him this freedom. But eventually his attempts at redesign, the various strategies he used to

¹⁴⁸ Similar quotations may be found from 1971: “The steady state model is attractive in many ways, although the physical origin of the tension [that causes the expansion] has never been satisfactorily explained. However, the recent evidence from the radio source counts...the red shifts of the...and the cosmic microwave radiation all tell heavily against it, and we shall consider it no further” (Sciama 1971, p. 117). Another from 1973 is Sciama's statement that, “[T]he debates on this question [the steady state] have been overtaken by recent developments which strongly suggest that the universe is *not* in a steady state.” Note the plural, “developments” (Sciama 1973a, p.55).

address the several anomalies within ADTR, came to an end. Sciama was very clear on this point. Regarding the contradictory evidence from the first Cambridge radio source counts, he said:

Obviously one of the first defences was “It’s not reliable yet.” Even as it became more reliable...you could...build up models which would save the steady-state theory which were not too far-fetched at that time. And given the importance of the issue, it was worth trying. And then as further evidence developed...it then became not reasonable any more to resist (Sciama 1990).

Thus Sciama epitomizes the passionate scientist, excited about understanding the “big questions” of the universe, and yes, having a preference for how he would like it to be. And yes, he did adjust several times threads in the “Quinean fabric” to attempt to save the steady state. But as the last sentence in the previous paragraph implies, he eventually came to a point when—even though he could have continued attempting to save the model—his intuition told him it was time to give up the ghost. Just because it is *possible* to continue resisting indefinitely—something cheerfully admitted by many scientists¹⁴⁹—does not mean that scientists will in fact do so. This intuition, based on not just his innate character but his training and social commitments within the scientific community, is what proponents of the Duhem-Quine model of scientific irrationality miss.¹⁵⁰

¹⁴⁹ “It is often, perhaps even always, possible to adhere to a general theoretical foundation by securing the adaptation of the theory to the facts by means of artificial additional assumptions” (Einstein 1949a, p. 21).

¹⁵⁰ I thank Dick Burian for pointing out this approach is to some degree in consonance with Feyerabend’s being “Against Method,” in the sense that *any* rigid, algorithmic method carried too far is undesirable. However, in my view Sciama’s approach would not support Feyerabend’s claim that “anything goes” in science (Feyerabend 1970, p. 26). As we saw, there were boundaries Sciama would not cross. Not everything “went.”

Summary

Thankfully, STS has in recent years begun to pay more attention to the development and reception theories ultimately judged to have “failed,” rather than maintaining an exclusive focus on those theories that prevailed in the end.¹⁵¹ This is a good thing, and not simply because, in the words of C. Bernard, “Even mistaken hypotheses and theories are of use in leading to discoveries...It seems, indeed, a necessary weakness of our mind to be able to reach truth only across a multitude of errors and obstacles” (quoted in Hon 1989, p. 482). For in addition to getting STS analysis away from triumphal Whiggish and “Great Man” history, the study of rejected models is a welcome development for another reason: after all, overwhelmingly, most scientific theories are incorrect. Thus, attention to the development of such failed models should necessarily provide a keener insight into the scientific process writ large. It follows that serious attention to the iterative processes that may have been employed in the attempt to save a model now known to be incorrect, rather than being viewed with ridicule or regret, is simply an extension of this positive trend in STS.¹⁵²

¹⁵¹ The genesis of this trend could be argued to be the putting forth of the “principle of symmetry” of the so-called “strong programme” in sociology of science, first enunciated in the 1970’s. This principle advocates looking at how social factors influenced not only the development of theories found to be incorrect, but also those that ended up being considered correct as well (see, for instance, Bloor 1976). However, I would certainly hasten to distance myself from the claims of some within the that camp that social factors *entirely* explain scientists’ theory choices—that all science is a mere “social construction” with no relation to the world. See Chapter 7 for a fuller discussion of this.

¹⁵² Feyerabend suggests another reason for paying attention to failed models—that “hidden virtues” of such models might yet be demonstrated by “sympathetic and intelligent” analysts (Feyerabend 1970, p. 117).

Dennis Sciama went through precisely the same processes in attempting to save the steady state model that many other prominent scientists did with their pet models; the fact that he was not successful, whereas some of his colleagues were successful, matters not a bit. In Sciama we also saw the very picture of a “boundedly rational” decision-maker, engaging in “heuristic appraisal,” who on the one hand was engaged in a systematic, logical attempt to find the flaws in his steady state model and address them (through the mechanism of ADTR), but who on the other hand tenaciously backed his model for largely “extrascientific” reasons and promoted it with rhetorical techniques. As he was assessing the possible ways to correct his model, he weighed intangibles, not following concrete rules on what would be “going too far” in trying to save the model, but instead relying on a kind of intuition that necessarily must have been picked up from his training, interaction with colleagues, and knowledge of previous cases.

Heuristic appraisal and anomaly driven theory redesign thus shed considerable light on Sciama’s actions. Yet one issue remains, insofar as ADTR is concerned. When describing acceptable forms of hypotheses in the theory adjustment phase, Darden writes: “Especially important criteria [in ADTR]...are systematicity and lack of ad hocness. It is important that the new theoretical component be systematically connected with the other theoretical components and not be merely an ad hoc addition that serves to account for the anomaly” (Darden 1992, p. 255). Here, the Sciama case seems to stray from Darden’s account, as for him, proposing a “slightly artificial” or ad hoc hypothesis was completely acceptable as part of his ADTR process.

Yet, what is really meant by this term, ad hoc, so often bandied about? I will explore this topic in the next chapter.

CHAPTER 6
ON AD HOC HYPOTHESES

If you want things to stay as they are, things will have to change.

—Giuseppe di Lamedusa, *The Leopard*

The FitzGerald-Lorentz Contraction

In 1887 A. A. Michelson and E. W. Morley performed an ingenious experiment to detect the motion of the earth relative to the luminiferous ether, which was thought to pervade all space and which provided the medium through which light waves were thought to propagate.¹⁵³ The frame of this ether comprised an absolute (i.e., a preferred) frame by which one could define concepts such as absolute space and absolute rest.

The experiment was as follows: a beam of light was directed at an angle of 45 degrees at a half-silvered mirror, so that half of the beam was reflected, and half was transmitted through the glass. (See below.)

¹⁵³ Many physicists consider this experiment to be one of the most beautiful of all time. Though it did not make his top ten most beautiful experiments, it was one of Creases's runners-up in (Crease 2003).

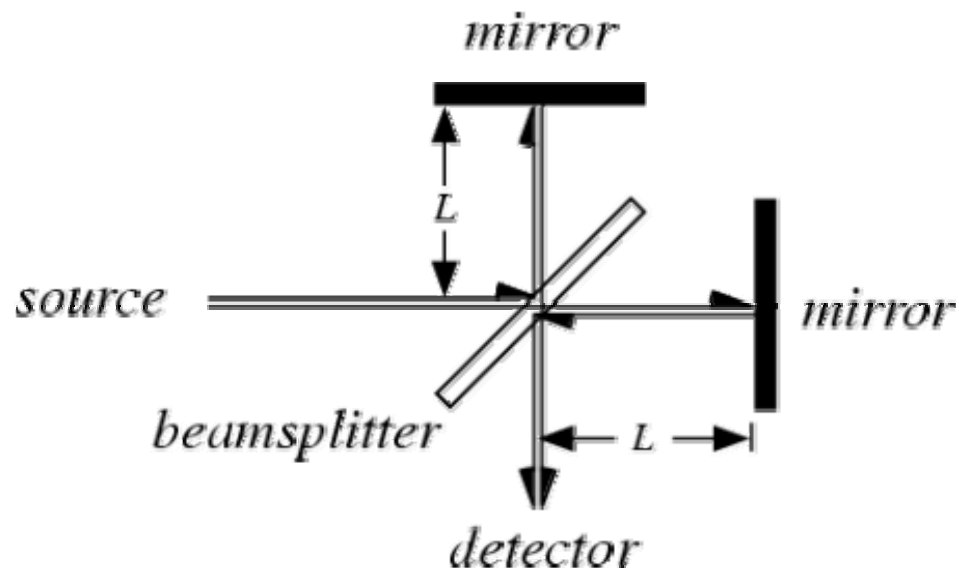


Figure 7. Michelson-Morley Experiment. From Weisstein, Eric W., “*Eric Weisstein's World of Physics.*” <http://scienceworld.wolfram.com/physics/Michelson-MorleyExperiment.html>.

The pulses then traveled to equidistant mirrors which reflected them back to the half-silvered mirror, where they were again half-transmitted and half-reflected. A telescope was placed behind the half-silvered mirror to receive the returning pulses. If there were an “ether wind” due to the earth’s motion through space, someone looking through the telescope should see the halves of the two half-pulses arrive at slightly different times, due to one pulse’s having traveled perpendicular to the direction of the “ether wind” and back, and the other’s having traveled parallel and back. This would manifest itself in an interference pattern in the light.

However, the velocity Michelson and Morley measured for the “ether wind” was *zero*. This, the famous “null result” of the Michelson-Morley experiment, was vexing to many, and was ultimately only explained in an acceptable fashion with the advent of special relativity, introduced by Einstein in 1905. There was no velocity measured relative to the ether because, according to special relativity, there is no ether; with

breathhtaking audacity Einstein banished the very concept: “The introduction of a ‘light ether’ will prove to be superfluous....” (Einstein 1998 [1905], p. 124). However, in the interim, G.F. FitzGerald (in 1889) and H. A. Lorentz (in 1895) independently suggested another explanation as to why the velocity of the earth relative to the ether was not detected: that the length of the measuring rods in the experiment shrank, due to an electrical effect caused by the ether, by exactly the right amount (a factor of $(1 - v^2/c^2)^{1/2}$) to produce a null result.

The suggestion of the FitzGerald-Lorentz contraction (hereafter “FLC”), independent of and prior to the framework of special relativity, has been held up as the very essence of an ad hoc hypothesis in science. For instance, Karl R. Popper wrote, “An example of an unsatisfactory auxiliary hypothesis would be the contraction hypothesis of FitzGerald and Lorentz which had not falsifiable consequences but merely served to restore agreement between theory and experiment....” (Popper 1959, p. 83). Indeed, Lorentz commented similarly on his own theory:

Surely this course of inventing special hypotheses for each new experimental result is somewhat artificial. It would be more satisfactory if it were possible to show by means of certain fundamental assumptions and without neglecting terms of one order of magnitude or another, that many electromagnetic actions are entirely independent of the motion of the system (quoted in Leplin 1975, p. 313).

Few things are spoken of more derisively in science or philosophy of science than ad hoc hypotheses. Gorham disparaged Jeans’s work on classical radiation theory between, described in Chapter 4, as being a series “ingenious but clearly *ad hoc* adjustments,” for instance (Gorham 1991, p. 474). Einstein said, “This manner of

theoretically trying to do justice to experiments with negative result through *ad hoc* contrived hypotheses is highly unsatisfactory” (quoted in Leplin 1975, p. 314).

Yet, what is really meant by *ad hoc*? The term means “to this [specific purpose],” and at first blush, this seems to describe very well something like FLC, something “cooked up” to explain the null result of the Michelson-Morley experiment. But surely *all* scientific hypotheses are generated for *some* purpose, and indeed usually to explain something or another about nature.

Restoring agreement between experiment and theory is done all the time in science. Feyerabend argued for the “progressive” role of *ad hoc* hypotheses (Feyerabend 1970, p. 63 *et seq.*) And Hilary Putnam writes, “It is possible to make *ad hoc* alterations in one’s beliefs without being unreasonable.” He continues:

In the example of dark companions to stars...the assumption that stars have dark companions is *ad hoc* in the literal sense...the assumption being made for the purpose of accounting for the fact that no companion is visible. The assumption is also highly reasonable.... (Putnam 1977, p. 432-3).

Another example of successful theory modification was the postulation of the staggering distances to the stars (by Aristarchus of Samos, and almost 2000 years later Copernicus) to explain the absence of observed stellar parallax predicted by their heliocentric models of the cosmos.

Certainly not all hypotheses should be considered *ad hoc*; to do so would render the term meaningless. So exactly what is meant by an *ad hoc* hypothesis? Why are they to be avoided, and how? Do scientists really eschew them, or are they only “supposed” to? These are the issues I will explore in this section.

What Is Ad Hoc?

Independent Testability

Popper claimed that in order to avoid *ad hocery*, a “new theory should be *independently testable*,” that “it must lead to the prediction of phenomena which have not so far been observed” (Popper 1965, p. 241).¹⁵⁴ That is, auxiliary hypotheses are only acceptable if “the degree of falsifiability or testability of the system...increases;” Popper cites the Pauli Exclusion Principle¹⁵⁵ as an example of “an auxiliary hypothesis which is eminently acceptable in this sense” because it led to new predictions (Popper 1959, p. 83).¹⁵⁶ C. G. Hempel agreed with this view, writing that while there is “no precise criterion for ad hoc hypotheses,” one should ask the following questions to determine if an hypothesis is *ad hoc*: “is the hypothesis proposed just for the purpose of saving some current conception against adverse evidence, or does it also account for other phenomena, does it yield further significant test implications” (Hempel 1966, p. 30)?

¹⁵⁴ Furthermore, the bolder the novel prediction is the better, as it is very unlikely that an *ad hoc* hypothesis would also make a very unexpected, risky prediction that also turns out to be true. Note that Sciamma’s hypotheses on radio source counts and quasars, made to save the steady state model of the universe, discussed in Chapter 5, did in fact make novel predictions. Deborah Mayo suggests that even if a hypothesis retrodicts an already-known phenomenon, if said phenomenon was not specifically used in constructing the hypothesis, its “prediction” may still be considered novel vis-à-vis the theory. She calls this characteristic “*use-novelty*” (Mayo 1996, p. 258).

¹⁵⁵ Proposed by Wolfgang Pauli in 1925, this suggested that no two electrons (later fermions) can have identical quantum numbers.

¹⁵⁶ Popper points out however that “one can show that the probability theories of induction imply...the unacceptable rule: always use the theory that is the most *ad hoc*, i.e. which transcends the available evidence as little as possible” (Popper 1965, P. 61).

On the other hand, if a hypothesis is postulated that only “saves the phenomena,” and does not lead to new predictions, Popper called it a *conventionalist stratagem*.

Hempel said:

Some genuinely testable theories, when found to be false, are still upheld by their admirers—for example by introducing *ad hoc* some auxiliary assumption, or by re-interpreting the theory *ad hoc* in such a way that it escapes refutation. Such a procedure is always possible, but it rescues the theory from refutation only at the price of destroying, or at least lowering, its scientific status (Hempel 1966, p. 37).

In the case of FLC, Mary Hesse wrote that the hypothesis was *ad hoc* not only because it was not independently testable, but indeed “because it entailed that motion in the aether is in principle unobservable.” This is because if the moving measuring rods shrank, any measuring rods used to measure the measuring rods would necessarily also shrink by precisely the same amount.¹⁵⁷

Popper went on to say, however, that theories should not only make new predictions, they should pass “new and severe tests,” i.e., the novel predictions they make should be borne out. Otherwise “it is always possible, by a trivial stratagem, to make an *ad hoc* hypothesis independently testable, *if we do not also require that it should pass the independent tests in question*: we merely have to connect it (conjunctively) in some way or other with any testable but not yet tested fantastic *ad hoc* prediction which may occur to us (or to some science fiction writer)” (Popper 1965, p. 244).

¹⁵⁷ Hesse went on to say that “the impossibility of measuring a quantity postulated ... does not always mean that the quantity is meaningless...for the unobservable quantity may be an essential ingredient in a theory which is supported by experiment in other ways, but the absence of a means of measurement does suggest that the quantity may be fulfilling no function in the theory, and that it therefore ought to be erased” (Hesse 1961, p. 228).

But do scientists behave in the way Popper suggests they should vis-à-vis novel predictions? *Pace* Popper, Stephen G. Brush in a series of case studies has found scant evidence that scientists actually give greater weight to novel predictions than they do to “retrodictions” (Brush 1995). Indeed, this was true even in the case of the quintessential example Popper cites as the way science “should” work—the dramatic novel prediction of light-bending in general relativity theory (Popper 1965, p. 34). This bold prediction may have influenced the *public*, Brush found, but in evaluating general relativity scientists by and large actually gave equal or even more weight to its success in explaining the behavior of Mercury's orbit—a retrodiction. As Brush puts it,

Because the Mercury discrepancy had been known for several decades, theorists had already had ample opportunity to explain it from Newtonian celestial mechanics and had failed to do so except by making implausible *ad hoc* assumptions. Einstein's success was therefore immediately impressive; it seemed unlikely that another theory would subsequently produce a better alternative explanation. It was a few years before Einstein's supporters could plausibly assert that no other theory could account for light bending, and this phenomenon therefore counted as evidence in favor of Einstein's theory over the others.... (Brush 1995, p. 138).

Mayo couches this in terms of the “severity” of tests:

The known fact about Mercury—being an anomaly for Newton—was sufficiently important to have led many to propose and test Newtonian explanations. These proposed hypotheses, however, failed to pass reliable tests. If it as if before this novel effect could count as an impressive success for Einstein's theory, scientists had to render it old and unsatisfactorily explained by alternative accounts.... (Mayo 1996, p. 288).

Those who did look favorably on the success of the light bending prediction did so without reference to the fact that the prediction was novel, *i.e.*, it was made prior to the observation of the phenomenon (see Brush 1989; Brush 1999). Furthermore, in the 1987 Virginia Tech conference on scientific theory change (which produced the book *Scrutinizing Science*), none of the case studies investigated provided evidence that novel predictions count more than retrodictions in theory evaluation. This claim was

specifically looked at in *Scrutinizing Science* by Finocchiaro and Hofmann, studying Galileo and Ampère respectively. However, the editors caution, “As telling as these cases are, they could partially be neutralized by a defender of the novelty requirement who could claim that the sciences in which they were working were still immature”(Laudan, Laudan et al. 1988, p. 20). That is, “budding” programmes tolerate more *ad hocery*. Deborah Mayo has also given considerable thought to the eclipse test, as well as to the concept of novel tests in general, and comes to much the same conclusion, saying that “What lies behind the intuition that novelty matters is the deeper intuition that *severe* tests matter” (Mayo 1991, p. 523). See also her (Mayo 1996).

Different senses of *ad hoc*

However, it turns out that the FLC hypothesis *did in fact* have independently testable consequences—it could be confirmed in an experiment different from the Michelson-Morley type. As Jarrett Leplin writes:

Part of Lorentz’s motivation for undertaking the generalization of the theorem of corresponding states in 1904 was the failure of Rayleigh and Brace to detect a double refraction in water or glass expected to result from contraction.¹⁵⁸ And the null result of the Trouton-Noble experiment¹⁵⁹ ... while not directly a test of contraction, showed at least that the electron theory was still subject to second-order difficulties. In addition, it has been argued that the null result of the Kennedy-Thorndike experiment of 1931 constitutes a refutation of the contraction hypothesis (Leplin 1975, p. 315).¹⁶⁰

¹⁵⁸ Lord Rayleigh first performed this experiment in 1902, and DeWitt Bristol Brace repeated it in 1904 with much greater precision. The idea was that light polarized parallel to the direction of the Earth’s motion in the ether would have a different velocity in a medium than one polarized perpendicular to that direction, thus causing double refraction.

¹⁵⁹ In this experiment, suggested by FitzGerald, and performed in 1901-3, a charged parallel-plate capacitor was suspended by a wire. If the ether is real, the change in Maxwell’s equations due to the Earth’s going through it would cause torque on the system, which in turn would cause the plates to orient themselves perpendicular to the motion. If Einstein were correct, and Maxwell’s equations are invariant for all frames of reference, a null result would obtain.

¹⁶⁰ This was a modification of the Michelson-Morley experiment, to address the possibility of ether drag. If the earth pulled along a certain amount of ether with it as it moved through space, a null result would be

Thus, *pace* Hesse and Popper, FLC would not be *ad hoc* by the standard definition.

But what if the hypothesis *does* make a novel prediction and the progenitor of the idea isn't aware that it does so when he proposes it? Should it still be considered *ad hoc*?

Grünbaum said no:

If Lorentz and Fitzgerald were in fact unaware of and disbelieved in the latter independent testability of their auxiliary hypothesis, their unawareness and disbelief cannot possibly render that hypothesis systemically *ad hoc*. If these theoreticians did espouse their contraction hypothesis while mistakenly believing it to be systemically *ad hoc*, this espousal would merely establish their own methodological culpability in this respect. In that case their espousal of the contraction hypothesis can be said to have been psychologically *ad hoc* (Grünbaum 1964, p. 1409).

Thus Grünbaum proposed the idea that things can be *ad hoc* in some senses and not others. However, Leplin and others found the notion of “psychological” *ad hocery* untenable, writing that “Lorentz’s familiarity with experiments that can falsify FLC does not mitigate the methodological illegitimacy he sees in the hypothesis” (Leplin 1975, p. 314). And indeed Grünbaum apparently withdrew these proposed categories following criticism from Hempel (Holton 1988, p. 329).

“Interestingly different” predictions.

Hempel, however, also pointed out a problem with the standard, Popperian definition of *ad hoc*, leading him to conclude that “no auxiliary [hypothesis] which is offered to save a theory...is independently testable by itself or is ever *ad hoc* by virtue of failure to be independently testable in isolation...” Suppose there exists a theory T

expected from the Michelson-Morley experiment after all. Kennedy and Thorndike made one arm of their interferometer longer than the other; thus their experiment predicted that differences in rotational speed relative to Earth between the two ends would cause detectable interference. Also, since the two ends of the experiment had different rotational speeds, different FLCs were predicted, thus an interference effect should be observed despite any FLC effect.

which is “saved” from an observation F by tacking onto it an hypothesis H to make, essentially, a new theory TH. If the *only difference* between T and TH is F, according to Popper H should be considered *ad hoc*. So, in this scheme of things, “all observational consequences of T other than F must be identical with those of TH.” However, Hempel pointed out that F pertains to one particular outcome of one particular experiment. *Thus in every other possible experiment T and TH should predict exactly the same result, according to Popper.*

However, Hempel suggested imagining “variants” of the experiment that produced F, experiments that differ only minimally from the original. Being only minimally different, any variant would *also* give different (albeit minimally so) predictions for T and TH. Thus T and TH do *not* produce exactly the same result in every other possible experiment, and thus H is non-*ad hoc* under Popper’s definition. Thus according to Hempel, no hypothesis

can ever qualify as ad hoc on the strength of the feasibility of a specification on purely logical rather than ‘denotative’ terms of what constitutes (i) one particular or single kind of experiment, and (ii) one single observational consequence.... (Holton 1988, p. 1410).

Hempel then suggested a modification of the definition of ad hoc:

An auxiliary [hypothesis] which enables a theory...to explain an [embarrassing] result in conjunction with [the hypothesis] is *ad hoc* if it does not have any observational consequences that are significantly or interestingly different from the [embarrassing] result (Holton 1988, p. 1410).

Thus Hempel’s definition of *ad hocery* depended on a subjective judgment on what the definition of “significantly or interestingly different” might be.

Ad Hocery by Comparison

Grünbaum's criticisms persuaded Popper to retract his charge of nonfalsifiability against FLC. However, Popper still maintained that since FLC was "less testable" than special relativity, and that this illustrated "*degrees of ad hocness*" (Popper 1959, p. 83). This leads us to consider that ad hocery might be not an intrinsic property of an hypothesis, but rather a characteristic that can only be defined in terms of another theory, i.e., by comparison. Again, according to Grünbaum, Einstein himself considered FLC's *ad hocery* to be on some sort of comparative basis, because

though independently testable ... [FLC] would fail to secure subsequent independent experimental confirmation *as against the claims of a new rival theory*¹⁶¹ The justification for rejecting the Lorentz-Fitzgerald hypothesis...depends on having philosophical reasons for accepting Einstein's rival theory.... (Grünbaum 1964, p. 1409, 1411).

Hempel also seemed to have some sort of comparative basis for ad hocery in mind when he wrote,

if more and more qualifying hypotheses have to be introduced to reconcile a certain basic conception with new evidence that becomes available, the resulting total system will eventually become so complex that it has to give way *when a simple alternative conception is proposed* (Hempel 1966, p. 30, italics added).

Let us suppose that all of Popper's, and indeed Hempel's, above definitions of good theorizing from the previous section were met. A theory still might be considered *ad hoc*, according to Imre Lakatos. A theory "may predict novel facts some of which may even be corroborated," he wrote. "Yet one may achieve such 'progress' with a patched up, arbitrary series of disconnected theories. Good scientists will not find such makeshift progress satisfactory; they may even reject it as not genuinely scientific...."

¹⁶¹ And the rival, of course, turned out to be Einstein's own special relativity in 1905. Gerald Holton agrees with Grünbaum's take on Einstein at least in this regard, writing of the theoretically-possible independent tests of FLC, "even if such tests had been carried out successfully, it is unlikely that they would have increased the appeal of the hypothesis to Einstein" (Holton 1988, p. 326).

(Lakatos 1970, p. 175).¹⁶² Lakatos called such a state of affairs a “degenerating research programme.”¹⁶³ Thus good theorizing, as opposed to *ad hocery*, requires building up some kind of coherent track record; a *sequence* of theories is required to make the determination. As Lakatos put it, “any scientific theory has to be appraised together with its auxiliary hypotheses, initial conditions, etc., and, especially, with its predecessors....What we appraise is a *series of theories*, rather than isolated *theories*” (Lakatos 1970, p. 117-8). This assessment, again, involves a kind of comparison on the scientist’s part.

William Whewell seemed to be describing this phenomenon when he wrote:

When a prevalent theory is found to be untenable, and consequently, is succeeded by a different, or even by an opposite one, the change...is effected by a transformation, or series of the earlier transformations of the earlier hypothesis....The original hypothesis...breaks down under the weight of the auxiliary hypotheses thus fastened upon it, in order to make it consistent with the facts (Whewell 1968 [1851], p. 252).

However, one flaw in Lakatos’s idea would seem to be that one cannot tell whether the “track” one is on is a degenerating one until *after the fact*. How many adjustments are too many? Events may be seen, he writes, “*with hindsight*, to have been ‘crucial’” (Lakatos 1970, p. 158). Ian Hacking doesn’t see this as problematic, writing, “Is it a defect in Lakatos’s methodology that it is only retroactive? I think not.

¹⁶² Lakatos calls such science “immature”—“consisting of a mere patched up pattern of trial and error.” I infer Lakatos is referring disparagingly to the technique of such science by calling it “immature,” rather than to chronological age. After all, the aether theory which FLC attempted to “patch up” was quite old.

¹⁶³ According to Ian Hacking, a program is “theoretically degenerating” when each modification in the theory comes only after some novel observations, and is “empirically degenerating” when predicted observations are not borne out (Hacking 1983, p. 118).

There are no significant general laws about what, in a current bit of research, bodes well for the future. There are only truisms” (Hacking 1983, p. 121).

However, making judgments about ad hocery only in retrospect could seem to lend itself to the worst kind of Whiggishness. As Hempel warned, “We should remember, however, that with the benefit of hindsight, it seems easy to dismiss certain scientific suggestions of the past as ad hoc hypotheses, whereas it might be quite difficult to pass judgment on a hypothesis proposed in a contemporary context” (Hempel 1966, p. 30). “When the theory is first proposed,” Ernan McMullin writes, “it is often difficult to tell whether or not it is ad hoc on the basis of other criteria of theory appraisal” (McMullin 1984).¹⁶⁴

For instance, take the example of U.J.J. Leverrier. In 1845, Newtonian gravity theory predicted an orbit for the planet Uranus that disagreed with observation. Leverrier’s solution was to hypothesize that an eighth planet, further out, was perturbing Uranus’s orbit gravitationally. His analysis of the perturbed orbit and prediction of Neptune’s location was hailed as a triumph. Yet in 1859, Leverrier applied the very same stratagem to the planet Mercury, hypothesizing that a planet closer in to the sun (“Vulcan”) was responsible for deviations in that Mercury’s orbit. There was and is no such planet; the deviations in Mercury’s orbit are due to gravitational effects of the sun explained by Einstein’s general theory of relativity. Leverrier’s Vulcan gambit is now

¹⁶⁴ McMullin also links *ad hoc* hypotheses to the realism/antirealism debate: “The realist takes an ad hoc hypothesis not to be a genuine theory, that is, not to give any insight into real structure and therefore to have no ground for further extension. The fact that it accounts for the original data is accidental and testifies to the ingenuity of the inventor rather than to any deeper fit.... Is an ad hoc hypothesis one that

seen as a mere *ad hoc* hypothesis, whereas his prediction of Neptune—using exactly the same methodology in roughly the same time period—is viewed as a triumph of “good” science. It seems Whiggish in the extreme to call the “unsuccessful” hypothesis *ad hoc* simply because it was unsuccessful.

Darden writes, “What counts as a legitimate addition to the theory and what is an illegitimate *ad hoc* change may be a matter of debate, especially when a new component is first proposed to resolve an anomaly;” she goes on to give examples of this phenomenon in the development of the gene concept (Darden 1992, p. 262). She also points out similar charges were made against the provirus hypothesis: “For the next 6 years, this provirus hypothesis was, as Temin put it, ‘essentially ignored.’ Robert Gallo...went further: ‘This notion...was met with almost uniform incredulity....Some critics went further and ridiculed the experiments and the idea....’” (Darden 1995, p. 147).

In the FLC case, too, different scientists reacted differently, with some having more of a stomach for the hypothesis’ *ad hoc* nature than others. Einstein, for instance, in a 1907 review article stated he found the hypothesis “more objectionable” than most, saying “This assumption, introduced *ad hoc*, appeared however to be an artificial means to rescue the theory” (quoted in Holton 1988, p. 352, Holton’s translation). Poincaré and Lorentz, on the other hand, while recognizing the somewhat artificial nature of the idea, took the hypothesis more “seriously, considering its ‘*ad hocness*’ compensated by other advantages, while Einstein discounted it entirely” (Leplin 1975, p. 332). Thus we see a

just happens not to be further generalizable, or is it one that does not give sufficient insight into real structure to permit any further extension” (McMullin 1984, p. 30)?

similarity with the Sciama case in that, like Lorentz and Poincaré, “slightly artificial” models were acceptable to him, given what was at stake—but yet gained little traction in the scientific community (as evidenced by the lack of citations for them in the *Science Citation Index*). Sciama, in turn, saw the even more radical suggestions to save the steady state proposed by Hoyle, Burbidge, et al. in the same light as the community did—as being too contrived to be considered seriously.

Thus far, then, we have yet to come to a tenable logical definition of *ad hocery*. Let us now look at a different approach to the subject—that of philosopher Jarrett Leplin.

Leplin’s Approach

Saying there was a “clear conviction” on the part of the scientific community “that the contraction hypothesis was ad hoc and that the Lorentz theory, largely on this basis, was methodologically inferior to Einstein’s,” Leplin simply takes the FLC hypothesis’ being ad hoc as a given (at least as far as the scientific community was concerned). That is, despite the verdict of contemporary philosophers that FLC was not, in fact, ad hoc on the grounds discussed above, this case remains in the minds of the scientists, both then and now, the quintessential case of ad hocery in action.¹⁶⁵ Thus, if our goal is to understand how scientists think and work, Leplin argues, one should use the data from this historical case to develop criteria for scientific ad hocery, rather than

¹⁶⁵ Wryly, Leplin notes philosophers’ back-and-forth on the logical status of FLC itself resembles a degenerating research programme, “many of whose entries are patently ad hoc” themselves (Leplin 1975, p. 314).

defining the term *ad hoc* in advance on the basis of more abstract, philosophical characteristics and then seeing if the FLC affair fits that definition. That is, Leplin's approach is inductive, rather than deductive.

Leplin's explicit goal is to dispute the claim on the part of many STS scholars that "no adequate rationale existed" for choosing between relativity and the Lorentz theory—a claim based, again, on normative definitions of *ad hocery* applied retroactively to the case, and one that Leplin says is "prima facie impossible." He writes, "A good principle of philosophical methodology in such a case is to presume that the rationale has yet to be uncovered, and to press the search as far as possible" (Leplin 1975, p. 316).

Leplin concedes that between the two models there was no *experimentum crucis*, and that the two were mathematically equivalent. "Given such experimental parity, we might look to aesthetic and pragmatic considerations to decide between competing theories," he writes.¹⁶⁶

But the advantages and disadvantages of the electron theory and special relativity in this regard are apparently offsetting. Relativity offered a simplicity and generality acknowledged by even its opponents to be an important advantage....But the preclusion of an ether was widely considered to be an important disadvantage of special relativity.

Relativity was also by many considered "a return to outmoded mechanistic conceptions. Relativity offered no contribution to the investigation of the structure of matter and electricity, which appeared to be progressing satisfactorily on an ether-theoretic basis" (Leplin 1975, p. 311-12).

¹⁶⁶ One finds similar language in Quine: "It turns upon our vaguely pragmatic inclination to adjust one strand of the fabric of science rather than another in accommodating some particular recalcitrant experience. Conservatism figures in such choices, and so does the quest for simplicity" (Quine 1953 [1951], p. 46).

Based on the FLC case, Leplin suggests five criteria that must be met for a hypothesis to be ad hoc in scientists' eyes: "An hypothesis H introduced into a theory T in response to an experimental result E is ad hoc if and only if:

- 1) E is anomalous for T but not for T as supplemented by H
- 2) E is evidence for H but
 - a. no available experimental results other than E support H
 - b. H has no application to the domain of T apart from E
 - c. H has no independent theoretical support
- 3) There are sufficient grounds neither for holding that H is true nor for holding that H is false.
- 4) H is consistent with accepted theory and with the essential propositions of T.
- 5) There are problems other than E confronting T which there is good reason to hold are connected with E in the following respects:
 - a. these problems together with E indicate that T is non-fundamental,
 - b. none of these problems including E can be satisfactorily solved unless this non-fundamentality is removed,
 - c. a satisfactory solution to any of these problems including E must contribute to the solution of the others.

Note that none of the five points above include any of the familiar concepts of independent testability or novel predictions that are usually referenced vis-à-vis ad hocery. The main thrust of Leplin's argument (point five in the above list) is that charges of ad hocery are really linked to trouble with the underlying theory, rather than to inherent qualities of the hypothesis itself. That is, scientists' criticisms are "directed primarily at the theory, and only indirectly at the particular hypotheses proposed as supplementation" (Leplin 1975, p. 320). In other words, a charge of ad hocery really amounts to a charge about fundamentality, rather than about completeness.

Leplin's fifth criterion also relies then on a kind of track-record; auxiliary hypotheses are not viewed in isolation, but rather in the context of these "other problems"

facing the theory that have amassed.¹⁶⁷ It also seems to rely on scientists' intuitions regarding what is a "satisfactory" solution to a problem and when there is "good reason" to believe a proposition. Thus "ad hocness will be a subject of uncertainty and dispute" between scientists, necessarily, since there is no precise, universal way to parse Leplin's fifth criterion.

Another aspect which makes Leplin's reformulation interesting is found in the third criterion above. Namely, an hypothesis is considered ad hoc if there are no grounds to believe either that it is true or that it is false. Thus if further evidence is uncovered later on that supports the hypothesis, it may *retroactively* be declared non-ad hoc after all by the scientific community—regardless of the circumstances and status that existed when the hypothesis itself was proposed.¹⁶⁸ Put another way, if the qualms about the overall theory's nonfundamentality are ultimately assuaged, the ad hoc character of the proposed theory changes are (again, retroactively) considered to have been non-ad hoc.

Leplin then applies his definition of ad hocery to another celebrated case in the history of science: the discovery of the neutrino. The situation was as follows:

In 1911 Lise Meitner and Otto Hahn showed there were difficulties in physicists' theories of beta-decay, a process by which an unstable parent radioactive nucleus decays into a nuclide (or "daughter" nucleus) and some beta rays (electrons). The continuous

¹⁶⁷ Thus in Leplin's view, it would seem to follow that if an anomaly were to arise for an otherwise fully-successful theory, any hypothesis proposed to address this anomaly would definitionally be non-ad hoc--since there would be at that time no "other problems" confronting the theory.

¹⁶⁸ Gerald Holton points out a seeming instance of this, in that even though when proposed, FLC "was clearly and quite blatantly ad hoc," considered so even by its creators, FitzGerald considered FLC to be more and more legitimate as time went on (Holton 1988, p. 328).

spectrum of the electrons observed in beta decay seemed to indicate a violation of the conservation of energy. Wolfgang Pauli in 1930 proposed that this missing energy might be accounted for if a hitherto-undiscovered particle existed and was emitted along with the electron in the decay reaction. This particle he called the neutron, which was later renamed “neutrino” by Enrico Fermi after the (true) neutron was later discovered by James Chadwick in 1932.

The reaction to the neutrino hypothesis was very negative; scientists described it as “‘unlikely,’ ‘unsettling,’ ‘strange,’ ‘artificial,’ ‘outlandish,’... ‘desperate’ and ‘unlikely’” (Leplin 1975, p. 338). Leplin points out that at this stage of the history of particle physics, the only subatomic particles that were known to exist were protons, electrons, and photons. Discounting the photon, which was at the time still of dubious ontological status, this proposal then increased the number of particles in the subatomic bestiary by fifty percent, simply in reaction to a troubling experimental result. This seemed a radical and even precipitous idea at the time.

However, Leplin argues that the reason, at least in part, for such negative reactions was due to not to the neutrino hypothesis itself, but rather to larger theoretical issues of the day. Various difficulties facing quantum mechanics had called into question the very concept of the conservation of energy itself, with scientists suggesting that energy conservation was not strictly observed but rather only applied in the statistical aggregate. The Bohr-Kramer-Slaters theory of 1924 (BKS) is perhaps the best example of this thinking. Though the fortunes of the theory of energy nonconservation waned a

bit, in 1936 the theory made a comeback in a big way when experiments performed by Robert S. Shankland seemed to support this notion.

Thus “theoretical considerations... discredited Pauli’s hypothesis,” Leplin writes. Some of the finest minds of the day saw the neutrino hypothesis in exactly the same way as the FLC was seen—as indicating serious underlying problems with the larger theory, a mere “band-aid” to cope with an embarrassing datum rather something that cut to the root of the problem. This number included Dirac, who saw the beta-decay problem as symptomatic of “the problem of constructing a satisfactory relativistic quantum mechanics” (Leplin 1975, p. 341). Thus, Leplin writes,

to scientists prepared to reinterpret the beta-decay spectrum as a refutation of energy conservation and already engaged in the theoretical innovations this step required [such as BKS], Pauli’s new particle was unnecessary and undesirable as well as intrinsically problematic.

Of course, neutrinos are now known to exist and have been studied extensively by scientists. Thus the verdict of the community—that the neutrino hypothesis was simply ad hocery to save an outmoded theory—was clearly reversed at some subsequent point.

As Leplin points out,

the discovery of important theoretical uses for the neutrino other than in the problem of beta-decay, and the failure of the view that rigorous conservation laws prevented the relativization of quantum mechanics, were primarily responsible for the neutrino’s eventual acceptance.

As for when this reconsideration took place, Leplin says that it was “a gradual process, and it is difficult to fix a point at which the neutrino’s theoretical utility became sufficient in strength and diversity to overcome its initial ad hoc character.”

Is it a drawback of Leplin’s scheme that ad hocery hinges on scientists’ intuitions of concepts like “satisfactory” and “good reason?” If this is the case, is it not arguable

that ad hocery is not unlike Justice Potter Stewart's 1964 definition of obscenity—one knows it when one sees it, but it is impossible precisely to define? And if so, is this necessarily a bad thing? I argue it is not. After all, if, as is almost universally now agreed, that a carved-in-stone scientific method does not exist (and moreover is in fact undesirable), is it not so outrageous to posit that a specific concept within science, namely ad hoc hypothesizing, is similarly hard to reduce to an algorithm?

It is impossible to ignore the colorful language directed at both the FLC and neutrino hypotheses. This was not mere hyperbole, but rather reflective of a much deeper truth, in my opinion. Just as aesthetic factors, for good or ill, clearly play a significant role in science writ large, as discussed in Chapter 3, surely they play a role—perhaps even a decisive role—in determining whether or not an hypothesis is ad hoc. Gerald Holton seems to agree, suggesting that “an ad hoc hypothesis, particularly a poor one, leaves the feeling that the operations of nature are constricted or restricted by arbitrary human intervention. On the other hand a large-scale generalization leaves the feeling that it expands the realm of application....” (Holton 1988, p. 365). His definition, as before, depends on a “feeling.” Holton admits this freely, and indeed goes on to suggest that this “feeling” (rather than some logical property) is central to the very notion of what is *ad hoc* and what isn't. He writes,

How is one to decide whether an hypothesis is ad hoc or not? And, moreover, whether it is repulsively ad hoc or acceptably so? It is here that we connect with Einstein's criterion of the 'inner perfection' of the theory. The criterion is the feeling for the 'naturalness' of 'logical simplicity' of the premises....What is important [is] the scientist's feeling of ad hocness about an hypothesis whether his own or not....To understand what almost any working scientists feels when he has to evaluate an hypothesis seems to be difficult for those who are not actually engaged in creative scientific work....This criterion of choice

is familiar to every working scientist (Holton 1988, p. 321,326).¹⁶⁹

Holton goes on:

The scientist who adopts somebody's hypothesis or creates his own for a specific purpose, 'in order to account' for a bothersome result or feature of the theory, regards it as *ad hoc*—not necessarily in a derogatory sense—regardless of its 'logical' status. This helps to explain the significance of the passionate and personal 'unscientific' language generally used to describe such hypotheses....

Acceptable *ad hoc* hypotheses are indeed often described as “plausible,” “likely,” “bold,” or even “elegant;” whereas unacceptable ones are described as “artificial,” “arbitrary,” “contrived,” “strange,” or “ugly” (Leplin 1975, p. 314; Holton 1988, p. 327).¹⁷⁰

The Sciama case illustrates this phenomenon to some degree. Sciama explicitly was on record as disliking the big bang model for what he considered its *ad hoc* assumptions, yet he did not see the continual creation of matter hypothesis (required for the steady state model to work) as being *ad hoc*—even though it was unobservable in practice (if not in principle), occurring at a rate of only one hydrogen atom in a liter in a

¹⁶⁹ Holton, however, also attempts to draw a distinction between “the scientist's use of *ad hoc* and the logician's” in the following way: “the former regards it as largely a matter of private science, or science-in-the-making...whereas the latter regards it as a matter of public science.... For a scientist engaged in original activity, his designation *ad hoc* (or its equivalent term) is an essentially aesthetic judgment which he makes ... while he imagines, considers, introduces, or rejects an hypothesis. [This] differs fundamentally...from *ad hoc* ... in the sense of a public statement with permanent, more or less clear epistemological properties, one that has been published and has become part of science-as-an-institution.” I infer from this that in Holton's private-versus-public science scheme, he considers only such things as textbooks and popular expositions to fall in the “public science” category, whereas things like journal articles and conference proceedings do not. Holton admits there is no sharp line of demarcation between these two senses of science; however, to me it seems inaccurate to suggest that conference proceedings and journal articles are in some sense not public. Perhaps a more useful distinction would be “science in the making” versus “settled science,” an example of the latter being that which has begun filtering into the textbooks (Holton 1988, p. 326, 327).

¹⁷⁰ The rhetorical impact of such valuations is significant. As discussed in Chapter 4, during the stage of “heuristic appraisal” or “the context of pursuit,” such terms may be used tactically by scientists in an attempt to convince their fellows of the superiority of one hypothesis over another.

trillion years.¹⁷¹ Similarly, recall the inclusion of the cosmological constant in Einstein's field equations—was that the *ad hoc* move, or was setting it at precisely zero the *ad hoc* choice?

The fact that one can't describe this aesthetic evaluation process of *ad hoc* hypotheses in strict, logical terms does not bother Holton. Indeed, in the following passage, Holton chastises those who might myopically pooh-pooh such aesthetic evaluation:

So-called philosophical mastery must be supplemented by an understanding of matters of scientific taste and feeling. Otherwise...it can lead ...a person to scold an Einstein for not having behaved like an obedient student in the classroom of a logician...for not having shouldered an 'obligation' to his philosophical masters (Holton 1988, p. 332).

This makes it all the more important, in the words of Holton, “to develop a field that can fairly be called the aesthetics of science....” (Holton 1988, p. 326).

As to how scientists arrive at these aesthetic judgments, this is certainly in part due to their formal training, as well as due to their evaluation (as part of a community) of the track record of a theory's success. McAllister's concept of aesthetic induction, as discussed in Chapter 3, seems very useful here. Recall that in McAllister's view, theories are seen as “beautiful” or “ugly” based on an aesthetic canon held at a given time by a community. But the criteria in this canon change over time based on the empirical track record of the theory. The characteristics that make a theory ugly are soon seen as not-so-ugly if it starts to amass a successful experimental track record. Is this not akin to what

¹⁷¹ Note in 1960 Sciama wrote the steady state and its associated theories provided a way in which the “*actual* behaviour of the universe can be accounted for without *ad hoc* assumptions” (Sciama 1960a, p. 10). But in an interview with Spencer Weart, Sciama acknowledged that later it was the steady state model which he recognized was getting perhaps more *ad hoc* as the hostile data came in, and that he was “preparing [his] mind for the trauma that came along” (Sciama 1978, p. 26).

Leplin suggests? The neutrino hypothesis was eventually cleansed of its original sin of ad hocery based on changing standards of what was acceptable in scientific theorizing (an abundance of subatomic particle types rather than a few, the preservation of strict energy conservation, and so forth), which in turn was based on a track record of empirical usefulness and success.

Summary

There is no agreed-upon, acceptable definition of an ad hoc hypothesis. Every definition yet put forward by philosophers has either been picked apart by other philosophers on logical grounds or when evaluated against the historical data. The normative, “top-down” approach to this topic seems to be exhausted. Thus Jarrett Leplin’s research on ad hocery in theorizing strikes me as a step in the right direction, taking a descriptive approach to scientists’ efforts, rather than a normative one. However, two features of his five-point definition of ad hocery raise significant questions.

First, Leplin suggests that a community’s scientific judgment about what is ad hoc and what is not can change retroactively, as with the case of the neutrino. Putting aside the Orwellian overtones of this (“Oceania is at war with Eurasia. It has always been at war with Eurasia”), if this is the case, then it is not clear to me what value there is in scientists’ ever worrying about whether an hypothesis is “ad hoc” or not. How is this

retroactive judgment on ad hocery any different from the distinction between an hypothesis' being validated by the community (i.e., turning out to be "right") and one that has been ruled out as "wrong?" If there is no distinction between right/wrong and ad hoc/non-ad hoc, then is there any use for the term ad hoc to begin with? Are we simply talking about the clarity of hindsight here?

Second, Leplin's definition brought him to a place visited by numerous thinkers before him, including Einstein, Holton, Hempel, and others: there is an irreducible element of aesthetic valuation that goes into the declaration of something as being ad hoc or non-ad hoc. Given the extensive discussion in Chapter 3 of the role of aesthetics in science writ large, this should come as no surprise. Science is not reducible to a cold algorithm; it is a human enterprise, and aesthetics (for good or ill) play a significant role in it. It thus makes sense for aesthetics to play a role in the evaluation of something as ad hoc or not as well.

It seems that hand-wringing by both philosophers and scientists over whether or not an hypothesis is ad hoc, and therefore was "illegitimately proposed," is simply energy wasted. At the end of the day there seem to be only hypotheses--not ad hoc ones and non-ad hoc ones.

This is not to say all hypotheses are created equal. A more fruitful concept for evaluating hypotheses might be to use notions of being "warranted" or "unwarranted," or "informed" or "uninformed." The study of historical case studies, as discussed in Chapter 1, provides a useful mechanism for evaluating what has been successful in the past and what has not; this could provide one avenue for determining whether a

hypothesis seems warranted. The Sciama case provides examples of this evaluation, as does FLC, legitimately made at the time—though history ultimately found the hypotheses in question incorrect. Such evaluations would also necessarily be based in part on the track record and background of the person proposing such an hypothesis, and not simply on the logical features of the hypothesis. For instance, a layperson who proposes that X causes Y in the world, may indeed produce an independently testable hypothesis, meeting the criterion for the classical definition of non-ad hoc theorizing. However, if the person has not been formally trained in the field in question, or if the suggestion falls well outside the standard domain of theorizing within the field, or if the historical track record exhibits very few examples of similarly successful theories, it is legitimate to consider such a suggestion unwarranted (and uninformed), and thus not fruitful to pursue—regardless of the purely logical status of the hypothesis (falsifiability, independent testability) itself. As Joseph Henry put it, in science, “opinions are weighed, not counted.” And, as David Hull has pointed out, there is no reason we should value in science what we value in society (i.e., democracy). Michael Ruse points out, “Obviously track record is important—someone who has found interesting things in the past is worth listening to in the present. And combined with this is experience” (Ruse 1999, p. 303).

CHAPTER 7

CONCLUSIONS

I shall try to correct errors where shown to be errors, and I shall adopt new views as fast as they shall appear to be true views.
—Abraham Lincoln, Letter to Horace Greeley, August 22, 1862

This study of Dennis W. Sciama's abandonment of the steady state model in favor of the big bang model suggests several interesting results.

The steady state model of the universe is often viewed with a certain sense of ridicule. Some see it as an embarrassing cul-de-sac, ill-founded, by scientists who, well, should have *known better*, and who universally clung to the model despite all evidence to the contrary. I have shown that though it turned out to be incorrect, the steady state model was in fact, reasonably proposed, and further, is worthy of remembrance and study as part of the history and development of the discipline.¹⁷² Yes, it was based on a

¹⁷² I am grateful to Steven C. Weiss for posing the question to me that if such falsified *scientific* theories are worthy of the attention of STS scholars, as I am claiming, might not the same be said of falsified *STS* models and theories—of which I consider there to be many. My answer is, certainly—if they are viewed in the same context as falsified scientific theories. That is, they can be illuminating for the understanding of a discipline's history, seen as artifacts of the time they were proposed. They are thus useful to scholars of the history of STS, and also useful pedagogically in helping novice STS students understand the development of their own field. The partisans for these models could certainly have been proceeding legitimately at the time, as Sciama did, and thus could even now exemplify a “significant moral force,” to use Weiss's term, even though their approaches turned out to be invalid in the end. So far, so good, I say. However, all too often, in my experience many thoroughly debunked STS approaches and theories are still taught uncritically to students, the implication to students being they are still robust research programmes, and the copious contradictory evidence against them since their proposals are simply overlooked. (The fact that very few of these critics/analysts of science have recanted their views in the face of mounting contradictory evidence, unlike Sciama, aides and abets this.) Thus shoddy work is presented as still being

sweeping principle of generality that was largely aesthetic in nature (the perfect cosmological principle). But this was certainly true of many other groundbreaking (and in retrospect, correct) scientific theories, such as relativity. The steady state model also made novel and falsifiable predictions, as many suggest a “good” scientific theory should.

My study of Sciama paints a picture of a scientist whose methodology contradicts the popular notions both of a rigid “scientific method” algorithm, as well as that of simplistic falsificationism. Sciama’s scientific process was far more complex than that. He adopted the model largely due to aesthetics (finding its simplicity “seductive”) and was very passionate (i.e., far from unbiased) about it (Sciama 1959, p. 174). At the first sign of contradictory evidence against the steady state model, Sciama held his ground, impeaching the validity of the troubling data against him (the Stebbins-Whitford effect and the 2C radio source counts). He was vindicated for doing so. As he stated, the steady state model to him was “such an attractive picture of the universe that [he] started helping to defend the theory when hostile evidence came out” (Sciama 1990).

Sometimes theory does trump data.

As the newer data resisted impeachment, however, Sciama then had to alter his strategy, and proposed models that could account for the data without giving up the steady state model. I have shown his behavior provides evidence for the models of Lindley Darden on anomaly-driven theory redesign (ADTR), as well as that of Thomas

a valid mode of analysis, perpetuated long past its natural expiration date. On the other hand, it is hard to imagine, say, the steady state model or Cartesian vortices still being taught as valid physics (as opposed to a vital and interesting episodes in the *history* of physics).

Nickles on heuristic appraisal and multi-pass inquiry. These processes, which occur between a theory's discovery and its establishment in textbooks as "settled" science, are not widely appreciated by the public, by many within STS, and perhaps not even (as such) by scientists. The Sciama case provides a concrete example of these things in action, and adds an appreciation for the complexity of the scientific process. Further, I have shown an ADTR process that, unlike the examples cited by Darden and others, ultimately was *unsuccessful*. Yet the methodology, or strategy, was the same as that utilized in successful cases. These all problematize the Duhem-Quine thesis in the sense that scientists were able to localize the faults in their theories and propose solutions, and in Sciama's specific case, in that he eventually did give up the cause.

Sciama's attempts to "save" the model were also motivated in part also by a sense of fun and "play" in theorizing, something not widely appreciated either.

There was probably a slight element of rebelliousness in it, the fact that one was contradicting traditional notions made it rather fun...scientifically useful and fun. So there must have been a psychological element of that as well. And therefore it became, if you like, not just a game, but a real desire to save the theory without cheating, against the hostile evidence (Sciama 1990).

However, Sciama did ultimately abandon the steady state model in 1966. He did so not at a point dictated by some pre-determined algorithm, but when his intuition told him the time was right, and further resistance would be "cheating" and "not reasonable." If Holton is correct, this decision too, like much of science, would seem to involve some kind of an aesthetic judgment, one which is perhaps induced after a certain track record is built up for a given theory. As he says, speaking of a similar case, "Here again we face

the role of what can only be called scientific taste in deciding which theory or hypothesis to accept and which to reject” (Holton 1988, p. 314).

The Sciamia case illustrates that in addition to aesthetic factors, there were also social factors at play as well. The critical role of Martin Rees in convincing Sciamia to abandon the steady state model is not to be overlooked. Consider the following quote of Sciamia’s concerning E.A. Milne’s Kinematic Relativity:

Milne never would have gotten away with the nonsense part of his work...if he’d had some good, critical students....My students like Hawking and Rees and so on wouldn’t allow nonsense, would’ve roasted me if I’d proposed a thing like that....He was never criticized right at home, and he could always object to external criticism in one way or another.... (Sciamia 1990).

Sciamia also said, “While I hope I’m not a crank, I’m not scared of outrageous, unconventional, or even bizarre proposals, unless...the one rule about that is that they mustn’t violate established experiments” (Sciamia 1990).¹⁷³ His remark about not wanting to be seen as a “crank” implicitly acknowledges the judgment his scientific community might make if he were seen to be holding out against hostile evidence against the steady state for too long. Any number of times Sciamia says or implies he did not want to go down the primrose path of perpetual denial that Hoyle and others did. For instance:

I wished [the steady state] were true, and I would fight to make it true as long as I did not distort the evidence.... I never wanted to say that if a quasar had a large redshift it was still local.... Hoyle and Burbidge tried that for many years, but to me that was pushing too hard to keep the steady state theory. So I never took that view (Sciamia 1978, p. 27).

¹⁷³ I do not see this statement as contradicting Sciamia’s earlier behavior in disbelieving the Stebbins-Whitford and 2C data. The critical word, “established,” I take to imply well-corroborated, and not tentative, data.

Holdouts who persist too long in defending a faltering theory tend to be ostracized and their attitudes are no longer considered scientific (at least on the issue in question)—i.e., they are considered “cranks.” This points up one of the ways in which science is a *social* enterprise—it is the scientific *community* that makes this evaluation.¹⁷⁴

Sciama’s abandonment of the steady state, despite his vested interest in it, thus problematizes certain irrationalist models of theory choice, such as Planck’s Principle, and so called “interest” models—it is hard to imagine someone who had invested more in a theory than Sciama had in the steady state model.¹⁷⁵ His ability to be in dialogue between the proponents of both the big bang and steady state models also casts doubt on the incommensurability thesis as well.

Sciama’s original belief in, and attraction to, the model was largely aesthetic in nature, but this too I have shown is a typical form of reasoning amongst scientists, and as such, should not be held against him any more than it should against any other scientist whose aesthetic sense happened to lead him to a theory that proved to be correct. As we saw, aesthetic arguments are often made by scientists, particularly in younger fields in which there is a paucity of data with which to work (though I would hesitate to say such

¹⁷⁴ Sciama makes another perceptive observation regarding social factors in young sciences. Since a young science necessarily will have a smaller population of workers in it, such fields are often dominated by a few, idiosyncratic personalities, which can dramatically affect the direction the science takes. Referring to the early days of cosmology, Sciama said, “The subject developed in a very chancy way....Whereas if you take more normal things in physics where these days there are hundreds of people in the field, there’s a kind of averaging process and there’ll be some eccentric individuals near the fringes, but there’ll be a great body of workers in the middle who’ll pour out the stuff....” (Sciama 1990).

¹⁷⁵ Jeans’ conversion to the quantum theory stands as a counterexample to interest models here too, as Gorham points out: “Jeans was himself at the centre of late developments in the classical interpretation of radiation phenomena. He therefore has as much at stake as anybody in the classical theory” (Gorham 1991, p. 473).

arguments are *limited* to such situations.) Simply put, aesthetic arguments are a fact of life in the sciences, so we'd better just get used to it—particularly in emerging sciences.

Yet, as we saw, as an indicator of truth, or being “on the right track,” aesthetic arguments are unreliable. As an *a priori* philosophical argument this seems obvious—why in the world should the universe be “simple?”—but a great many scientists nonetheless seem to hold this as a given. The copious examples I provided establish definitively that sometimes aesthetic arguments end up being right, and sometimes they end up being wrong.

Threads in the Gordian knot of the aesthetics of science, as we saw, include that aesthetic criteria not only change over time, but at any given time, an aesthetic criterion agreed-upon as being desirable (such as “simplicity”) is not easily parsed in its details. Ockham, it seems, has several different kinds of razors. Sometimes tradeoffs between aesthetic criteria (the imposition of one creates problems with another) muddle the picture further. Individuals' aesthetic senses can also conflict, not only with those of other individuals, but also with those of the scientific community as a whole.

And, as we saw with Sciama, his aesthetic sense served him well in not abandoning the theory in the early days when the data were sketchier, but began to do him disservice as the stronger data came in. One thing we might conclude could be that the reliability or usefulness of purely aesthetic arguments is at best in inverse proportion to the amount of reliable data available on a given subject. If we adopt McAllister's view of switching to a theory seen as less aesthetically pleasing as being a revolutionary act,

Sciama's abandonment of the steady state was indeed revolutionary—though it is unclear if we can apply that definition to an individual, as opposed to a community.

Gerald Holton has urged the development of “a field that can fairly be called the aesthetics of science”; I hope this essay serves to draw attention to the significant role of aesthetics in science, and so I suggest the same (Holton 1988, p. 326). However, if, as most now agree, there is no “one” universal scientific method, it seems unlikely that “one” aesthetics of science will be found or agreed upon. Further compounding the issue is that if (as is commonly agreed) there is not one thing called “Science,” but rather numerous smaller “sciences,” that the aesthetics of each may be (and almost certainly are) different. Still, given the attention that scientists give to the subject, its study (including the theories of McAllister) is worth pursuing, just as any other aspect of science is.

My look at science aesthetics in science has shown me another thing. Many have heard the remark often attributed to Richard Feynman that “philosophy of science is about as useful to scientists as ornithology is to birds” (quoted in Kitcher 1998, p. 32). Feynman and other scientists such as Steven Weinberg (Weinberg 1992) point out that science has made its remarkable progress with only occasional attention to the “normative” admonitions of philosophers outside the field.¹⁷⁶

Philosophers of science, such as Joseph Pitt, on the other hand suggest that STS

¹⁷⁶ One exception of a few that come to mind is that of the influence of Karl Popper on the steady state school of cosmology, as described in Chapter 2.

commentators on science do have a “normative, not merely a descriptive role to play” (Pitt 2001, p. 375).

I confess to having shared something of Feynman and Weinberg’s sentiment myself, even after all these years of practicing STS. For me, the function of STS was strictly to try to understand *how* science advances, in all its glorious messiness, and that was sufficient. The failure of the logical positivist program seemed to bear this approach out in my view, and the quote of Holton’s pointing out the inadvisability of scolding “an Einstein for not having behaved like an obedient student in the classroom of a logician...for not having shouldered an ‘obligation’ to his philosophical masters” certainly rang true to me (Holton 1988, p. 332). And to be honest, it still does.

However, my research on scientists’ aesthetic arguments changed my mind about the normative role of STS. Again and again, as we saw, scientists’ aesthetic arguments led them astray. And many scientists, such as Dirac, Livio, Impey, and others seem to believe that (somehow) by following an inherent aesthetic sense, they are led reliably (inevitably?) to truthlike results in research. As I showed in Chapter 3, this is just not the case. As Michael Shermer warns, “Beauty by itself does not make a theory right or wrong, but when a theory fulfills our deepest wishes we should be especially cautious about rushing to embrace it” (Shermer 1997, p. 268).¹⁷⁷ If scientists cannot maintain clarity on this particular issue, it falls to those who study

¹⁷⁷ Shermer goes even further: “When a theory seems to match our eternal hopes, chances are that it is wrong” (Shermer 1997, p. 268). However, I cannot agree to this, given that, as we saw, many times scientists’ aesthetic senses ended up leading to reliable results.

science to do so, to issue reminders when necessary, of this and other confluences, and the concrete troubles such things have caused in the past.

Thus I conclude STS does have a normative role after all—however, this role should be exercised only through a careful, rigorous, well-documented examination of the historical record (i.e., by looking at past cases), and sound argument. Instead, much of the normative tut-tutting we see going on in STS these days involves not constructive reminders to scientists who have developed collective amnesia or a “blind spot” about a given subject (e.g., the unreliability of aesthetic arguments, or the methodological legitimacy of a given mode of argument), but shrill attacks on science itself based on the philosophical, cultural, or other biases of science critics.

Many (myself included) see this situation as a crisis in STS, caused by the sloppy and often preposterous claims of said critics (largely on the far left).¹⁷⁸ These folks are simply not being responsible in their normative role. Armed with postmodern pseudo-profundity, these individuals engage in what Larry Laudan called “repeated acts of wish fulfillment” (Laudan 1990, p. x) in their outrageous charge that science does not really tell us anything about the world—all the while typing their screeds on word processors made possible by science, taking flights to their self-congratulatory conferences in jets made possible by science, and so on.

¹⁷⁸ Philip Kitcher, for instance, says “Something has gone badly wrong in contemporary science studies” (Kitcher 1998, p. 32).

This silliness has to stop.¹⁷⁹ Critics of science cannot wish away the reality of its successes any more than scientists can by force of will make the universe or its laws “simple.” STS, if it is to be taken seriously, must stop pretending that just because the Scientific Revolution was conducted largely by European, white men that it follows somehow that *science itself* is illegitimate. Sandra Harding’s calls for “stronger objectivity” in science (which, in Orwellian fashion, means non-objectivity), for instance, or the replacement of science by other subjective so-called “ways of knowing” about the physical world, and so on simply bring disrepute to our field.

Is the dialogue really advanced by pretending we don’t know that the world is round, and orbits the sun in an ellipse, just like the other planets? Is our conversation really deepened by pretending that the Voyager II probe’s arriving at Neptune, 30 AU from the sun, twelve years after launch, right on schedule, was just a coincidence? Do we really want to pretend that invocation of the goddess of smallpox in India is just as efficacious as Western medicine? Must we really hold that Native American creation myths are just as valid as the big bang model (and further that they, and all other cosmogonies, are equally valid as descriptors of the real world)?¹⁸⁰ Our

¹⁷⁹ I may be the only person in the history of our program who has suggested in his Ph.D. exams exiling Sandra Harding to a remote island so as to facilitate her development of the superior, less oppressive science she has long advocated, the accuracy of which could then be compared, side-by-side, with the overly “masculine” and “unobjective” science with which we are now burdened.

¹⁸⁰ Of course, such creation myths have enormous value historically and culturally, and my respect for them is deep. My point is that they must be evaluated differently so far as they pertain to the physical universe, i.e., insofar as they are purported to be *scientific* models. Otherwise we are left with the conclusion that all such cosmogonies (again, of the actual world) are somehow all *simultaneously true*.

tolerating such claptrap¹⁸¹ has the effect of “swamping” the field, and obscuring—both to us and to those outside our field—those legitimate STS analyses and critiques of science (which certainly do exist), diminishing their import and potential impact.

¹⁸¹ Just to illustrate I am not exaggerating, consider the following quotes from highly-regarded scholars:

- Roger Anyon states that “Science is just one of many ways of knowing about the world” and that the Zuni tribe of New Mexico’s creation myths, though contradicted by archeological and other scientific findings, are “just as valid” (quoted in Boghossian 1998).
- On the theory that the pharaoh Ramses II died of tuberculosis, Bruno Latour states that before its discovery by Robert Koch in 1882, the tuberculosis bacillus had “no real existence,” and thus this was impossible (quoted in Sokal and Bricmont 1998, p. 97). Latour and Steve Woolgar assert that “reality is the consequence rather than the cause” of scientific inquiry (Latour and Woolgar 1979, p. 237). Also, for Latour, despite the near-century of data supporting it, relativity is a mere exercise in the “sociology of delegation” (Latour 1988).
- Disparaging what he sees as disrespect of New Age theories, Andrew Ross asks of how such things “taken seriously by millions be ignored or excluded by a small group of powerful people called ‘scientists’” (quoted in Gross and Levitt 1994, p. 91). Ross also dedicated his book critiquing science, *Strange Weather*, “To all the science teachers I never had. It could only have been written without them.”
- For Harry Collins, “The natural world has a small or non-existent role in construction of scientific knowledge” (Collins 1981, p. 3).
- Sandra Harding in numerous places asserts rape metaphors infuse science and thus taint its content and conclusions. She says that “If [scientists] were to excite people’s imaginations in the way that rape, torture, and other misogynistic metaphors that have apparently energized generations of male science enthusiasts, there is no doubt that thought would move in new and fruitful directions.” That is, as it stands, science is distorted, and a female-centered science would be a more accurate reflection of the world (Harding 1991, p. 267).
- Noretta Koertge points out Mary Daly’s description “as necrophilia the essential message of science under patriarchy and states that ‘phallotechnology’ has ‘rapism as its hidden agenda and the destruction of life as its final goal’Jane Caputi...claims to have shown a ‘compelling connection between incest and nuclearism’” Koertge also points out Katharine Hayles’s promotion of Luce Irigaray’s linkage of fluid mechanics with an overly masculine character inherent in modern science. And the biggest howler of all, perhaps, is the suggestion that due to Navajo sensibilities, more ‘holistic’ than those of the West, that math classes for such persons should begin with non-Euclidean geometries and calculus, rather than decimals and fractions (Koertge 1998, p. 259-261). She also notes one episode in which women’s studies students insisted that the pain of childbirth—lessons of biology, anatomy, and physiology aside—was a “‘construction’ of patriarchal society” (Koertge 1996, p. 267).
- Martin W. Lewis points out radical ecophilosophers’ patently false, but repeated, claims that prior to the scientific revolution, people, especially non-Western people, but also including Europeans of the Middle Ages, “inhabited an ‘organic female universe’ replete with a ‘sense of oneness, continuity, and organic justice.’” He also notes Margaret Conkey and Ruth Tringham’s urging that “the historical data on goddesses should be ignored if they do not present an image that is healthful for modern feminists....” (Lewis 1996, p. 212, 219).
- Mario Bunge notes that Michael Mulkey has “waxed indignant” over the scientific community’s “abusive and uncritical rejection” of Velikovskyism, as well as Collins and Trevor Pinch’s

My study of Sciama shows an example of a well-respected, reputable scientist who neither practiced the “sausage grinder” scientific method proposed by traditionalists, but nor was he completely irrational—inventing whatever results he pleased—as the critics of science often contend. I hope this study helps further to build a solid, “middle ground” in STS, rejecting the oversimplified (and all-too-easy-to-charge) caricatures of science at both ends of the spectrum—the science haters on the left, and the hagiographers on the right.¹⁸²

As Carl Sagan once wrote regarding scientific process,

Too much openness and you accept every notion, idea, and hypothesis - which is tantamount to knowing nothing. Too much skepticism - especially rejection of new ideas before they are adequately tested - and you're not only unpleasantly grumpy, but also closed to the advance of science. A judicious mix is what we need (Sagan 1995, p. 30).

“spirited defenses of astrology and parapsychology” as just as legitimate as traditional science (Bunge 1996, p. 106).

- In a very important essay, Meera Nanda points out that, among other absurdities aided and abetted by postmodern attitudes in the academy, Frederique Apfell Marglin “recently declared that the eradication of smallpox from India using the modern cowpox-based vaccine was an affront to the local custom of variolation, which included inoculation with human smallpox matter accompanied by prayers to the goddess of smallpox, Sitala Devi. Despite her own admission that the traditional variolation is at least 10 times more likely actually to cause the disease as compared to the modern vaccine, Marglin persists in deriding the introduction of modern vaccine in India...as an imposition of ‘Western logocentric mode of thought....’” She also draws attention to Harding’s suggestion that “if allowed to break from the West, different cultures will discover many more alternative universal laws of nature....” (Nanda 1998, p. 291, 306).

¹⁸² I am grateful to Barbara Reeves for pointing out this dissertation could be seen as contribution to just such a recently-initiated discussion between scientists and STS. This conversation, begun in the wake of the Sokal Hoax and the “Science Wars,” attempts to find that civil, middle ground of analysis I alluded to above, exploring the rich texture of science as it is really practiced, without necessarily impeaching its credibility—a state of affairs for which I have long hoped. See (Labinger and Collins 2001) for one volume of such conversations. I am also grateful to Dr. Reeves for pointing out that just as there are numerous scientific communities (as, again, is commonly agreed), neither is STS monolithic. Further, she points out that some of these communities within STS do not consider the harsher critics of science (e.g., Andrew Ross), who are often portrayed as typical of STS by outsiders, to be within STS *qua* STS at all. That is, STS is not only made up of different communities with different norms, but that it also (at least to some degree) policies its boundaries just as science does. To go further into these provocative ideas would take this project too far afield, however.

Trying to understand Sciama's complex, but far from atypical, approach to science in some ways epitomizes the utility of STS—and of the case study approach, as discussed in Chapter 1. The Sciama case can be seen as helping illuminate a Burianesque “regional” understanding of the development of the cosmology of the mid-20th century, but also as a datum against certain hypotheses about science's operation (e.g., Planck's Principle), as well as a Nicklesian example of modes of analysis that “worked in the past” and what therefore might be worth trying in the future—or of what *didn't* work in the past. Further, I argue that my study of the Sciama case, being an *extended* one, which highlighted not only whence the various issues facing Sciama came, but also on how the proposed solutions evolved over time, would qualify as a “problematic” in Joseph Pitt's sense. One of Pitt's concerns with case studies (as discussed in Chapter 1) is that all too often they are mere “snapshots,” rather than extended analyses of a problem over time. I believe this study has provided more than a snapshot, and thus does not only historical but some philosophical work as well.

Thus I hope there has been something useful for all four of my audiences—leftist critics of science, mainstream STS, the public, and mainstream scientists—in this work.

Possibilities for Future Research

The Sciama case raises a number of possibilities for future research. As Kragh has pointed out, the history of 20th century cosmology, particularly post-war cosmology, is rife for exploration. For instance, the history of late twentieth-century cosmological models, such as inflationary theory, is virtually untouched. There are any number of extended case studies (or “problematics”) that might be undertaken within this field.

As a new science, cosmology during this period is of particular interest. It might be interesting to compare cosmology’s development to that of other young sciences, such as modern planetary astronomy, radio astronomy, molecular biology, and so forth. Did the lack of data lead similarly to aesthetic reasoning or bold conjectures? Were there similar squabbles over fundamentals, over methodology? Did a few, strong personalities dominate the field? What could one say about the “actors” and “networks” in such young disciplines?

In the career of Sciama himself, his work on the decaying dark matter (DDM) hypothesis would be another interesting study. As mentioned in Chapters 1-2, Sciama after abandoning the steady state model of the universe began working within the big bang paradigm. Specifically, he proposed a model to explain the universe’s dark matter, which involved the decay of massive (tau) neutrinos. According to the model, this decay in turn would produce photons which would ionize hydrogen both in the Milky Way and in intergalactic space as well. A comparison of this with the steady state model might be of interest since the DDM hypothesis also made very specific, falsifiable predictions; it addressed a well-known problem by proposing a hitherto-unobserved mechanism; and within Sciama’s lifetime the technology became such that it was tested, with a negative

result. It would be instructive to see if Sciama's method of theorizing and doing science in the DDM case paralleled his behavior in the steady state case. Did he begin the process of anomaly-driven theory redesign, only to be cut short by his premature death? Was he similarly frank about how far he'd go to save the model? Did he find it to have "sweep" and "beauty," and if so, was his defense of it motivated by these things?

Tests of Planck's Principle also provide an intriguing area for research. One might test the weak version of the principle—that younger scientists are more likely to accept new models than older ones—in cosmology, for instance, by doing an age analysis on when various scientists adopted new cosmological models. Examining in detail more "conversions" (such as that of Jeans and Sciama) might also be enlightening. Given the very similar stages in those two cases, do other scientists go through similar phases? If so, is it due to the type of field in question, its age, its "revolutionary" status? The emerging field of the psychology of science might have something to say on this as well.

Anomaly-driven theory redesign and heuristic appraisal lead to yet more possibilities for research. Are a theory's anomalies addressed only after another model has explained them, as is sometimes claimed (Lightman and Gingerich 1992)? Which strategies of addressing anomalies are most common? Which are most successful? One avenue to explore might be to see if there is a resonance in theory redesign with Joseph Pitt's model of technology (which he defines as "humanity at work") as an "input/output transformation process," which involves "assessment feedback" (Pitt 2000). One might also find similarities regarding Sciama's decision on when to "give up the ghost" vis-à-

vis the steady state model to Peter Galison's study on *How Experiments End* (Galison 1987).

Looking at the role of error in the scientific processes might yield similar results, an area that Hon, Allchin, and others are pioneering. Stephen Jay Gould considered the issue of negative results to be one of the most serious but least discussed problems in science (Gould 1993). It has recently been proposed, for instance, that pharmaceutical companies be required to disclose to the public the results of negative trials for drugs, in addition to the positive ones.

McAllister's model of aesthetic induction is a very promising area, and could yield important cautionary tales for scientists who are unaware of or forget the fungibility of aesthetic judgments. In addition to the cases already mentioned in Chapter 3, other possible aesthetic inductions might include the following:

Has quantum mechanics, as McAllister suggested, really come to be viewed in positive aesthetic terms? That is, has QM's tremendous empirical track record caused physicists see the model as "beautiful" yet, as aesthetic induction would suggest? There is some anecdotal evidence that says perhaps, but the philosopher George Gale and others are dubious. My suspicion is that it is possible that one community (physicists) may have undergone this transformation, where as another community (philosophers) has not. This, of course, raises another set of issues regarding "cultural" differences between communities of scholars.

Another possible aesthetic induction to consider might be the case of particle physics. As seen in this dissertation, the neutrino and the positron were both resisted on

aesthetic grounds after their proposals. I.I. Rabi famously responded to the discovery of the muon with the comment, “Who ordered that?” As the subatomic bestiary continued to explode in numbers, many scientists and philosophers continued to express their aesthetic displeasure at having to deal with such a plethora of particles, rather than a “simple” few; Lakatos, in fact, at one point proclaimed particle physics a degenerating research programme. However, as Murray Gell-Mann and others imposed a kind of order on the bestiary using various symmetry arguments, the aesthetic tide seemed to turn. That is, simplicity criteria gave way to symmetry criteria, and as the latter started to yield accurate predictions, an aesthetic induction may have taken place—or is still taking place.

In Sciama’s case, of course, we saw that he abandoned his cherished steady state model despite (as near as I can gather) his continuing to see it as aesthetically superior to the correct model (i.e., the big bang). If scientific communities do go through aesthetic inductions, how common are the “Sciamas” of the field (who adopt the new model, but never do see it as “pretty”), compared to those who eventually do make such an induction, as well as compared to the “Hoyles” of the field who simply refuse to change over at all? Or another research project on aesthetics might be to compare how aesthetic canons, inductions, and ruptures vary by scientific discipline—or even within disparate communities within a discipline.

Whatever the next phase of the research, my intention (once again) ideally would be to illuminate that fertile middle ground of science as a legitimate, but human and

thoroughly social,¹⁸³ activity, providing counterexamples both to the oversimplified “scientific method” model, but also the radical relativism that dominated much of the critiques of science from the 1970s to the 1990s. Both of these views are caricatures, and the sooner more studies that can showcase science as it is actually done—as a social enterprise, yes, but far from an irrational one that tells us nothing about nature—the sooner these caricatures can be dispensed with and we can proceed with the more conversation as alluded to earlier. To quote Dick Burian, let us “Exorcize the silliness and exhort the responsibility” of our field (Burian 2005).

¹⁸³ Of all the aspects of the operation of science, this may be the most underappreciated—at least outside of STS circles. I studied physics for seven years, three of which were at the graduate level. I was surrounded *every day* by practicing scientists, none of whom worked alone, all of whom belonged to research groups within the department, with their own staffs, graduate students, and seminar series; who had their own “Invisible Colleges” of colleagues outside our University; who attended their own specialized conferences and had their own journals, and so forth—and *still* the tremendously social processes of knowledge validation that go on in science were not apparent to me, or I’d hazard, to most of my fellows. It was only upon my studying STS that this became clear to me. Further, these social mechanisms within science are what legitimate scientific knowledge, in my opinion (rather than some mythical, unchanging, Platonic “scientific method” ideal type); therefore it seems to me these social facets are what are most important for STS scholars to communicate to the wider public.

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P R O F E S S I O N A L E X P E R I E N C E

June 2001-present **Professor, Department of Physical Science and Engineering**
Prince George's Community College, Largo MD

Taught all versions of Introductory Astronomy, including day, evening, distance learning, weekend, honors, extension center. Also taught Introductory Astronomy Lab, Introductory Physics I and II. Developed and taught Interdisciplinary Honors Colloquium in History of Science. Involved in curriculum development project to introduce guided inquiry-based learning to Astronomy Lab. Developed and taught interdisciplinary honors colloquium in history and philosophy of science. Served on various departmental, divisional, and college-wide committees. Involved in numerous Science and Technology Resource Center projects, including Science Trek, Weekend Astronomy Workshop for Elementary Teachers, Lunar Observing Workshop for Elementary Teachers. Held occasional viewing sessions with PGCC telescope. Created web pages/Blackboard sites for courses. Received consistently high student and overall evaluations.

August 2003-present **Adjunct Associate Professor, Instructional Services**
University of Maryland University College, Adelphi MD

Taught voice-mail and web-based versions of introductory astronomy course. Received consistently high student evaluations.

Summers 1996-present **Lecturer, Department of Astronomy**
University of Maryland, College Park MD

Taught Introductory/General Astronomy. Oversaw teaching assistants. Received consistently high student evaluations.

August 1999-August 2003 **Adjunct Assistant Professor, Instructional Services**
University of Maryland University College, Adelphi MD

June 1990-June 2001 **Instructor/Assistant Professor/Associate Professor, Department of Physical Science**
Prince George's Community College, Largo MD

August 1987-July 1990 **Graduate Teaching/Research Assistant, Departments of Physics and Chemistry**
University of Maryland, College Park MD

Taught physics and chemistry labs and recitations. Participated in cross-Washington optical experiment in Quantum Electronics Group.

Summer 1987**Lecturer, Department of Natural Sciences**
Madisonville Community College, Madisonville KY

Taught Conceptual Physics.

August 1984-May 1987**Undergraduate Teaching Assistant, Department of Physics and Astronomy**
Murray State University, Murray KY

Taught one-credit astronomy lab. Operated MSU telescope and observatory. Held regular night observing sessions.

E D U C A T I O N

2005	Ph.D.	Science and Technology Studies	Virginia Polytechnic Institute and State University <i>GPA = 3.9; Advisor – Joseph C. Pitt</i>
1996	M.A.	History and Philosophy of Science	University of Maryland <i>(GPA = 4.0; Advisor – Stephen G. Brush)</i>
1990	M.S.	Physics	University of Maryland
1987	B.S.	Physics (<i>cum laude</i>)	Murray State University Minors: Mathematics, Astronomy

P R O F E S S I O N A L O R G A N I Z A T I O N S

History of Science Society
 Astronomical Society of the Pacific
 National Center for Science Education
 Committee for the Scientific Investigation of Claims of the Paranormal (associate)
 Planetary Society (1990-1996)

P R O F E S S I O N A L A C T I V I T I E S

Spring 2001-Fall 2003**Co-Director, Aristotle and a World of Wonder**
Prince George's Community College, Largo MD

Co-wrote and co-administered \$25,000 NEH grant bringing scholars of Aristotelian thought to the PGCC campus. Developed web site.

Fall 2001-Summer 2003**Coordinator, Scholarship Across the Curriculum**
Prince George's Community College, Largo MD

Founded and administered interdisciplinary program to promote and encourage original scholarship and research by PGCC faculty. Responsible for organizing scholarly talks and events, including the annual Lyle Linville Lecture. Ran the College's internal grant program. Developed web site. Developed criteria for new College scholarship prize.

Fall 1999-Spring 2000**Peer Reviewer**
University of Maryland University College, Adelphi MD

Served as peer reviewer of development team for web based introductory astronomy course.

June 1997-July 1999**Honors Program Coordinator***Prince George's Community College, Largo MD*

Personally responsible for coordinating Honors course offerings across various departments and divisions. Advisor to Honors Society, Phi Theta Kappa, and College Bowl team. Articulated with administration and faculty on various Honors topics. Organized Honors events, including annual college-wide convocation. Oversaw publication and distribution of various Honors publications. Determined eligibility of students.

August 1996-June 1997**Honors Program Assistant Coordinator***Prince George's Community College, Largo MD*

O T H E R A C T I V I T I E S

July 2001-December 2004**Board of Trustees, All Souls Church, Unitarian***Washington, DC*

Charged with ensuring fulfillment of broad policy goals and mission of the church. Integral part of ongoing effort to restructure church governance. Board President, 2003-2004.

July 1994 – July 1996**Extension Chair, Delta Lambda Phi National Social Fraternity**

Board of Directors member responsible for helping found new chapters across the country.

December 1991-December 1993**Chapter President, Delta Lambda Phi National Social Fraternity***Washington DC (Alpha) Chapter*

Personally responsible for leading chapter through semi-annual rush and pledge periods, including community service projects. Quintupled membership in two years. Featured in several national publications. Served also as National Ritual Chair during this time, helping write new rituals still in use by the national organization.

H O N O R S

Who's Who among America's Teachers (1996, 2002)

Selected for PGCC Master Teachers Retreat (2001)

Awarded Tenure (1999)

Beamon-Raymer Outstanding Alumni Award, Delta Lambda Phi Alpha Chapter (1996)

Outstanding Chapter President, Delta Lambda Phi (1993)

Founder's Award, Delta Lambda Phi Alpha Chapter (1992 – highest chapter award)

Sigma Pi Sigma (National Physics Honors Society – 1987)

Murray State University Outstanding Physics Senior (1987)

Member, Mensa (1987-1988)

P R E S E N T A T I O N S

1991-1995

Weekend Astronomy Workshop for Elementary Teachers

1992-1994

Lunar Observing Workshop for Elementary Teachers

1992-1993

Twinkle, Twinkle Little Star (for Science Trek, an annual PGCC event)

January 14, 1994

The Brat Pack: Younger Faculty Respond to the Contemporary Community College (AFACCT conference presentation)

February 11, 1997	<i>A Brief Introduction to Science and Technology Studies</i> (invited presentation, PGCC Reasoning Across the Curriculum program)
May 29, 1998	<i>The Age and Scale of the Universe</i> (invited presentation for the International School of Helsinki, Finland)
May 1999	Astronomy Lecture and Night Sky Observing (invited presentation – All Souls Church, Unitarian)
May 2000	Annual Astronomy Lecture and Night Sky Observing (invited presentations – All Souls Church, Unitarian)
May 2001	Night Sky Viewing at the Master Teachers' Retreat
July 5, 2001	“Dennis W. Sciama and the Steady State Cosmology,” at the Fifth Annual History of Astronomy Workshop, University of Notre Dame
November 15, 2001	“Aristotle and a World of Wonder,” invited presentation for the NEH Committee on Education Programs (with Alicia Juarrero).
January 2002	“The One Culture,” part of a panel on interdisciplinarity for PGCC's professional development day
January 2002	Night Sky Viewing at the All Souls, Unitarian Board of Trustees Retreat
January 15, 2002	“Wonder and the Order of Science,” at the opening session of Aristotle and a World of Wonder
April 18, 2002	“A Cosmologist Recants,” for PGCC's Scholarship Across the Curriculum program
December 10, 2003	“ <i>We Shall Have to Make the Best of It: The Conversion of Dennis Sciama</i> ,” for the Virginia Polytechnic Institute and State University Science and Technology Studies Seminar Series

P U B L I C A T I O N S

- “Differential Comparison of the One-Way Speed of Light in the East-West and West-East Directions of the Rotating Earth,” *Proc. 20th Annual PTTI Applications and Planning Meeting*, November 1988. (coauthor)
- *Exploring the Universe: Laboratory Activities for PSC 102* (1999, PGCC Press). (coauthor)
- Review of Helge Kragh's *Cosmology and Controversy*, *Physics in Perspective* **3** (2001), 249-250.

G R A N T S

- Fall 1997 - PGCC Pathfinder Grant (\$500) for attendance at History of Science Society Annual Meeting
- Summer 2001 - PGCC Pathfinder Grant (\$500) for presentation at Fifth Annual History of Astronomy Workshop, University of Notre Dame and Archival work at the University of Illinois, Urbana-Champaign
- Summer 2001 - NEH Focus grant (\$25,000) for Aristotle and a World of Wonder project (co-awardee)
- Fall 2001 - PGCC Trailblazer grant (\$2,500) for Aristotle and a World of Wonder project (co-awardee)
- Fall 2003 - Pathfinder grant (\$500) to attend the History of Science Society Annual Meeting