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INTELLIGENT INFERENCE AND THE WEB OF BELIEF: IN DEFENSE OF A
POST-FOUNDATIONALIST EPISTEMOLOGY

A DISSERTATION SUBMITTED TO THE GRADUATE DIVISION OF THE
UNIVERSITY OF HAWAI'I IN PARTIAL FULFILLMENT OF THE
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By

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Abstract

This thesis aims at casting the Copernican Revolution in a new light. By examining and articulating in much more detail the role of auxiliary hypotheses in debates related to the Duhem-Quine thesis, and by displaying the underlying rationality of the favorable appraisal scientists often give theories that rigorously determine parameters, this thesis attempts to walk the difficult path between what Popper called the myth of the framework (relativism and holism) and what Kitcher calls the myth of Legend (logical positivism, logical empiricism, and foundationalism). Unlike Legend, this thesis does not rationally reconstruct away the importance of the messy cultural influences that externally affected the normative decisions of methodology and evidence. It embraces them as helpful in the long run in promoting science, and denies that any transcultural, transtemporal methodological features exist that select out the Copernican system as clearly superior to the Ptolemaic early in the debate. Yet, unlike relativism this thesis portrays the Copernican episode as a highly rational affair for the most part, where the major players entertained a robust debate over numerous issues, but gradually became more aware that planetary linkages with the sun were much more important than previously thought. Although not agreeing on what path this might take, they became convinced that these linkages provided the fruitful key to an eventual correct understanding of planetary motion. Supporters of heliostasis drew attention to an impressive harmony and fixity of parameters in the Copernican system regarding the saving of the core observational problem, a harmony and fixity lacking in the Ptolemaic system. Nonsupporters agreed, but either disagreed that these systemic features were decisive in selecting out the Copernican system and/or that these systemic feature could not be incorporated into a modified geostatic system. So there was a major move in developing Tycho-like geoheliocentric systems. Although successful in the short term, there was long term degeneration of this major auxiliary patch to geostasis when it was recognized that its development conflicted with one of the main motivating factors for its advancement in the first place, i.e., allegiance to Aristotelian dynamics.
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Preface

First of all, I would like to thank the philosophy department and the members of my committee for giving me the opportunity to address some issues that have preoccupied me for over 20 years. It has been a wonderful opportunity to face these issues systematically and within the context of being given such stimulating feedback from the esteemed members of my committee.

No matter how many times it has been studied, no matter how many papers have been written, it seems to me that anyone interested in scientific methodology, change, and evidence returns to the Copernican episode again and again for guidance and enlightenment. In my case attempting to understand it, and overcoming mistakes in understanding it, have played a large part in my intellectual development.

Speaking of enlightenment, we could probably all provide an interesting list of illuminating events in our intellectual development. Here is my list:

1. The discovery in my first college history class that Columbus was not the nice guy my grammar school teacher said he was.

2. The discovery in an excellent physics course that ideas could exist and have substantial evidence for them, even though those ideas conflict radically with my Kantian categories of common sense and what I thought to be possible. (I am speaking here, of course, of Einstein’s theory of relativity and quantum mechanics.) Related to this would be the discovery in an astronomy course that the Earth is but a small blue dot in an immense space, and that the universe did not wait until
we could take center stage -- that it had billions of things to do, over billions of years, before we showed up to declare how important we are.

3. The humbling discovery, in reading on my own Darwin's *Origin of Species*, in conjunction with a superb Zoology course, of the full ramifications of historical contingency -- that natural selection does not imply inevitable progress towards human beings, that we are just another (perhaps temporary) species, that we have been very lucky to be on one of the little tips of Darwin's evolving bush, and that we should take great care how we act on this planet.

4. Last but not least, and most relevant to this thesis, was the shocking discovery, for me at least, that Copernicus and Galileo were both Catholics, that the Catholic Church supported scientific development, that the Church did very little to prohibit the study of Copernicus's *De revolutionibus* for over 50 years after its publication, that Galileo's controversial book was actually published with the approval of the Catholic Church and his friend the Pope, and that the Copernican system was entertained and studied by many supporters of geostasis.

Somewhere I had picked up the impression that Copernicus and Galileo were detached logical positivists totally unconcerned with religion, probably either agnostics or atheists, and their writings underground manifestos for a new counter-culture world order, in search of which gestapo-like soldiers for the Inquisition broke into homes.

I should quickly add that this list is not finished. Just a few short years ago I published a book that contained within it one of the major myths of the Copernican episode, i.e., that the Copernican system uses fewer circles than Ptolemy. Although I thought I had made it clear that the full Copernican system was just as complicated as Ptolemy's, and that there was a major epistemological question whether
the feature of simplicity should be considered evidential, my students would invariably pounce on the numerical reference in their papers as to why the Copernican system was better than Ptolemy’s. It did not help mitigate my horror that I had graduated from a receiver to a promulgator of myths.

In many ways this thesis is about myths. It is an attempt to see the Copernican episode anew, to walk a difficult path between what Popper called the myth of the framework (relativism and holism) and what Kitcher calls the myth of Legend (logical positivism, logical empiricism, and foundationalism). Unlike Legend, this thesis does not rationally reconstruct away the importance of the messy cultural influences that externally affected the normative decisions of methodology and evidence. In fact, it embraces them as helpful in the long run in promoting science. Furthermore, it denies that any transcultural, transtemporal methodological features exist that select out the Copernican system as clearly superior to the Ptolemaic early in the debate. On the other hand, unlike relativism this thesis portrays the Copernican episode as a highly rational affair for the most part, where the major players entertained a robust debate over numerous issues, but gradually became more aware that planetary linkages with the sun were much more important than previously thought. The thesis claims that although they did not agree on what path this might
take, the major players became convinced that these linkages provided the fruitful key to an eventual correct understanding of their deity's construction of the universe.

This thesis also finds no fault with the relativist in part, that supporters of a theory often act very conservatively by not immediately bending to recalcitrant empirical problems or perplexing conceptual inconsistencies. Instead, we often, and rationally so, tinker with our theories, especially when they have been successful for a long time, and patch away to our heart's content. But the thesis takes issue with the claim that just any patch will work, that the process of patching appraisal is circular and framework dependent. Rather, it shows that one might win a short term battle with a patch, but lose the war in the long run of full scientific appraisal when one's patches are developed and scrutinized carefully.

In a nutshell, here is the story I tell about one important strand in the Copernican episode. Supporters of heliostasis drew attention to the fact that there was an impressive harmony and fixity of parameters in the Copernican system regarding the saving of the core observational problem, a harmony and fixity lacking in the Ptolemaic system. Nonsupporters agreed, but either disagreed that these systemic features were decisive in selecting out the Copernican system and/or that these systemic feature could not be incorporated into a modified
geostatic system. So there was a major move in developing geostatic transforms -- Tycho-like geoheliocentric systems. Although successful in the short term, there was long term degeneration of this major patch to geostasis when it was recognized that its development conflicted with one of the main motivating factors for its advancement in the first place, i.e., allegiance to Aristotelian dynamics.

Being committed to this story and the rationality of most of the moves of the major players, I must also tell a normative story regarding harmony and fixity of parameters. In the literature one finds three different responses, all of which I challenge.

1. The systematic, locked-in nature of Copernican parameters allows us to double-count the evidence in favor of Copernicus. The empirical evidence might be equal on a first-order perspective of planetary positioning, but bears differentially in a second-order perspective in favor of Copernicus. Conclusion: The most rational players supported the Copernican system early. (Hanson, Glymour, and Lakatos)

2. The perception of harmony and fixity of parameters as important was culturally dependent, subjective, and irrational. It originated from, and hence was forever tainted by, mystical neoplatonism and pythagoreanism. It was "nothing" (Kuhn), a mere expression of a "metaphysical urge" (Feyerabend), an expression of a primitive theism, a desire for security and convergence of belief (Rorty). Conclusion: The major players were not detached objective scientists in the pursuit of truth; they were sleepwalkers and pons in a major social power shift.

3. Although one can grant perhaps a weak pragmatic status to harmony and fixity of parameters, it ought not to have been very important. Not only is the whole notion of systematicity transtemporally problematic (for its time Ptolemy obviously had an elegant system), but fixed parameters only grant at best an ease of
prospective testability. As empiricists we should never be allowed to retrospectively double-count any observations. Hence no salient epistemic status can be given to parameter determination. Conclusion: One is under no obligation to tell a story of early rational commitment to the Copernican system, other than that it was new and did a good job accounting for the core observational problem. Relativists can sociologize away to their hearts content regarding the irrationality of early commitment to heliostasis, and this will not phase the perception that what was really important epistemically was the gradual empirical and conceptual problem solving ability of heliostasis. (Comets, novae, and telescopic observations for the former, and the destruction of crystalline spheres, Galileo's dynamics, Kepler's ellipses, and Newton's physics for the latter) (Laudan)

I argue that (1) is not only wrong epistemically, but it forces us to conclude that all supporters of geostasis were bad scientists and irrational. On the other hand, (2) is not only wrong epistemically, but confuses the message with its origin. A belief can possess epistemic merits regardless of its cultural origin. Although my thesis possesses deep sympathies with (3), I argue that it paints a picture of too much false consciousness and capitulates too much to the relativists, handing over as it does a large chunk of what key players in the Copernican episode discussed and deemed important. Even prior to the 1543 publication of *De revolutionibus*, discussion and a "lively expectation" regarding Copernican features existed. After publication, the now crystal clear planetary linkages with the sun received intense discussion, reaching a crescendo in the late 16th and early 17th centuries with the advancement
of geoheliocentrism and the Church's realization that it had a formidable cosmological alternative on its hands that could no longer be written off as a mere calculation tool. I argue that it is important to see a normative argument for this major chunk of history to counter the bad stories that the relativists tell about this time, to undermine a key historical premise that they use repeatedly for their general disconsolate conclusions in matters of methodology.

It seems to me that relativists argue ala Zeno thusly: If the initial period of theory creation and pursuit is irrational, then all later periods of acceptance must be likewise. We cannot get to point B (rationality) from point A (irrationality) without going half way, and we cannot get to a half way point without also going half that distance, and so on. So, we never "move"; we are always at point A. Both (3) and my thesis believe this is wrong, but I argue that more piecemeal change is needed, that (3) gives the impression that the change from A to B was too abrupt, and that the very problem solving ability that (3) appeals to must be seen in a wider context. In other words, problems cannot be seen to be problems, let alone solved, unless shifts have taken place in a wider horizon of perception. Furthermore, the move to develop geostatic transforms occurred before the empirical and conceptual transitional developments that (3) cites.
Why do a few scientists boldly pursue a new theory while a majority either passively entertain or actively reject the new theory? The old view of Legend paints a picture of simple heroism, of a few reasonable scientists attempting to pull the rest out of Plato's cave of ignorance and dogmatism. Relativism easily demolishes this myth by using Legend's own methodological prescriptions to show that there was no decisive evidence in favor of the new theory, and then concludes that early allegiance to a new theory was due simply to idiosyncratic personality traits and/or circumstances. My response is to say, personality and circumstance involved? -- sure, but there must be something promising in the new theory to activate the circumstantial differences. I claim that in the Copernican episode case this is easy to explain. The feature that showed promise was commensurably acknowledged by both supporters and non-supporters of heliostasis. Tycho's personality and political situation may have been a significant cause for his advancement of geoheliocentrism, but it did not keep him from recognizing the same positive feature of heliostasis recognized by Galileo and Kepler, i.e., the fixed planetary linkages with the sun.

Crucial then to my thesis is the role I attribute to parameter determination and the distinction between the theory relationship modalities of pursuit and acceptance. I argue that a major message learned from successful
scientific practice is that coincidences often occur, but they are not our preferred conceptual way of interfacing with the world. In general, we prefer theories that tell stories about fundamental processes by linking postulates and parameters, so that we are not only granted answers to important questions, such that we are inclined to believe that when they get observations right they are not doing so by coincidence, but also are given leads to many other fields and answers to questions we did not even know we had.

For a sufficiently accurate analogy to make my point (it is somewhat of a caricature), consider the following. Suppose it were true that the standard quantum model and the new string theory both saved the core observational problem of what kind of universe we have. But string theory not only determined the 18 undetermined parameters in the standard model, but provided a major hint of how to unify quantum theory with relativity theory. Would there be any doubt as to what to advise a young physicist regarding a career move? We would be in no position to declare that string theory was true, or even that the evidence for it was overwhelming, because the distinction of what is coincidence and what is fundamental process is most often what needs to be determined by future testing. However, there would be little doubt in the distinct fruitfulness of string theory and the wisdom of pursuing its development. Furthermore, as to why this is so would not be because of just personality
and circumstance, of a need for unity in our lives, belief in a benevolent deity, or a political and economic power shift, but simply because we have a good ampliative reason to believe that the world works in some sort of unified fashion, responding to us along many fronts, with unconnected differences in those responses only apparent.

Scientists know, regardless of their historical situation, that if they want to be successful theologically, economically, and/or politically, they had better back the most promising or reliable scientific theory. Legend says that good scientists are always looking straight ahead at the objective evidence. Relativists say that they are always looking over their shoulders, worrying about the social and political environment, and sleepwalking forward. My way of putting this is that scientists are indeed messy human beings, as are we all, and it is precisely because they have a lot to worry about over their shoulders that they pay acute attention to the epistemic situation in front of them.

I am not arguing that the Copernican episode was as clear as the string theory example. In fact, we need to see that the situation was messy to understand why every competent astronomer did not immediately back heliostasis. However, I am arguing that the situation was sufficiently close to understand why both supporters and nonsupporters of heliostasis abandoned strict Ptolemaic geostasis when its
key parameters were perceived to be undetermined, offering a
disturbingly flexible response to the core observational
problem. They believed that their deity had created one
world in such a way that everything would fit together.
There may be many things that divide us from their time and
our time, but today whether we are Duhemian instrumentalists
or Popperian realists we can argue without any embarrassment
whatsoever that a belief in one independent world that stays
the same as our beliefs change and grow with time is better
supported ampliatively than the relativist's story that we
sculpt alternate realities with time and live in different
worlds.

For what it is worth, I am very happy with the thesis.
This satisfaction, of course, does not entail that I should
pass or that its argument is flawless. But the thesis does
go along way toward saving for me the core historical
problem which I have been uncomfortable with for so long.
Ever since I read the likes of Koestler, Koyré, Kuhn, and
especially Feyerabend, while simultaneously experiencing the
power and beauty of scientific methodology, I have struggled
to reconcile human folly with the marvelous vision we have
obtained of the universe. I believe I have made a signifi-
cant contribution to getting it right, of explaining the
meandering parade of debate and adjustment, of jockeying for
position, and the gradual strengthening of some positions

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and eventual crumbling of others that was the Copernican Revolution.

Once again, much gratitude is owed to my committee members. I think it is safe to say that just about everyone practicing philosophy today has been a student in one way or another or Irv Copi. I can think of no other mentor in my life for which Henry Adam’s aphorism regarding a teacher affecting eternity applies better. Ron Bontekoe’s own Ph.D. thesis and our e-mail exchanges helped immensely in jump starting my own thesis. Rachel Laudan helped keep my historical analyses balanced, and forced me to make the concept of a hypertextual adjudicatory trail clearer. Ron Amundson’s deep understanding of Ptolemy and challenging e-mails were not only incredibly entertaining, they made chapter 5 far superior to my initial draft. Larry Laudan was my most relentless critic. As Larry explains it, this criticism is due to the fact that we agree on so much! It was a great honor and opportunity to have him as my chair.

Finally, I would like to thank John Winnie who years ago as a young assistant professor first taught me that philosophy of science is philosophy enough.
All science is intelligent inference: excessive literalism (in acceptance of data) is a delusion, not a humble bowing to evidence.

Stephen J. Gould
Introduction

In many postmodern attempts to understand scientific practice socio-cognitive relativism is no longer an issue for debate; it has become a foundational premise for an allegedly correct approach to understanding method and history. Contrasted with the traditional belief in the existence of a single, transferable, inevitably progressive scientific method, and the traditional epistemological project of a reflective distillation of rational rules from successful practice -- rules which will serve as normative prescriptions for future success -- we are asked to see science as an "empty label" for a diverse collection of methodologies, as a contingent "knowledge-making game," as a set of "discourses" engendered by prior ideological commitments and social interests, and a collection of narratives put into service primarily as post hoc rationalizations of these commitments and interests. Scientific methodologies, we are told, are best seen as rhetorical and political resources, as argumentative resources in negotiation and conflict resolution, not as a foundational objective corpus of algorithms for the genesis or evaluation of scientific knowledge. Thus, knowledge-making is said to depend in an essential way upon the generation of a perspective, and perspective depends in an essential way upon a wide spectrum of social interests, from professional vested interests and
technological competencies to wider cultural allegiances: religious views, ontological commitments, views on the nature of humankind, and so on.\textsuperscript{2}

Even reality, we are told, must be relativized; it has a flexible, yielding, ambiguous texture, allowing for alternative, often incommensurable, definitions and articulations based upon equally useful perspectives and traditions. What constitutes knowledge at any given time is but an idiosyncratic pie slice or participatory, inevitably incomplete mapping of an inarticulate and in principle fuzzy, implicate whole. There are no preferred cognitive frames of reference and many equally valid ways of "getting along" in the universe. We are said to be "sculptors of reality," not detached Newtonian observers robotically compelled by a brute definable reality of separate, independently locatable things. "Good physics" in the 20th century has taught us that this locality assumption is but an anthropocentric projection and a species of naive realism. From modern physics we have learned that we are always engaged from and in a perspective.\textsuperscript{3} Thus, for the postmodern philosopher, progress in our interface with nature, if progress can be said to exist at all, is Darwinian, not Lamarckian, where diversity and proliferation of perspectives and hence realities rule, not accumulative expansion toward some ideal perspective.\textsuperscript{4}
This new postmodern mainstream asks us to see a seamless network of self-discovery in 20th century philosophy, from Wittgenstein's language games, Koyré's neo-Kantian historical analyses, Kuhn's gestalt shifts and paradigms, Duhem's and Quine's holism, Feyerabend's anything-goes, non-hegemonic pluralism, Bohr's complementarity, to Rorty's version of pragmatism and prescriptions for edifying conversation, whereby the logical underdetermination of scientific accounts of reality and the theory-laden nature of fact-production are taken as canonical deconstructions of previous foundationalist pretensions. Whether our perspectives are called paradigms, webs of belief, different worlds, traditions, research programs, supertheories, or situational horizons, they are seen as potentially infinitely flexible cognitive nets, adjustable to any experience, because experience is incapable of compelling any refutation or change in perspective. At best, experience can stimulate "recontextualizations" (Rorty's term) of existing perspectives or arational gestalt shifts to new ones. Hence, the epistemological project is said to be over, philosophers of science to be no more than a parasitic "froth" on the real work of engaged scientists, and philosophy of science dissolves into the more modest work of various descriptive and interpretive sociological, historical, and literary studies of science.
Philosophers will no longer be allowed to posture as androcentric epistemological police dictating how we should think. Instead 21st-century philosophers will entertain us by helping us learn how to play in, or at least appreciate, different worlds by provoking, challenging, needling, and disturbing established ways of thinking. We will combine the cheerful anti-authoritarianism of Socrates and the pluralistic liberalism of Mill with the honesty of Camus' Sisyphus -- we will investigate different cognitive paths with grace, charm and humor without interference or burden from the concern whether our paths are the right paths, that our paths may not have a Platonic Good at the end. We will no longer seek convergence of belief. We will no longer debate perspectives with the goal of finding a more inclusive, right, or objective perspective. We will immerse ourselves in different perspectives for the hell of it, or at best, to keep ourselves intellectually honest, humble, and morally tolerant. According to Rorty, we can learn to be "anti-anti-ethnocentrists."

At its most extreme, this postmodern story tells us that scientific method only works to the extent that it has a mythical prestige as a rationalizing corpus of rules used to persuade in debate, as a source of successful global public relations. Furthermore, we have learned that not only does science have no epistemic or cultural primacy, but that it does not need any rational reconstructing as a
public relations interface, that it can be trusted to stand on its own "locally" with no need for a global, essentialist narrative. And we are warned and lectured that the traditional dogmatic allegiance to this Church of Reason and the acceptance of this limited aspect of a Eurocentric tradition are creating a species of social adaptation that privileges a specialized development of certain human abilities over others. That from a Darwinian perspective this cultural imperialism is producing a dangerous uniformity that will inevitably block the diversity needed for growth and a satisfying human existence. 

In its more positive moments we are told that the goal of this postmodern understanding is not to promote a neoromantic irrationalism, but to provide an expanded notion of rational understanding and promote human responsibility. In turning around the charge of promoting relativism and irrationalism, the traditional foundational approach is said to portray human beings, and scientists in particular, as non-human "judgmental dopes" where rules and objective data compel decisions without any human judgment or interpretation required, not unlike the use of the gigantic translation manual robotically employed by the man in Searle's Chinese room thought experiment. Rather than portraying historical actors sensitively immersed in a broad network of social concerns, of having a mind with all sorts of things on their minds when they are making decisions, judgments,
and commitments, rather than portraying human decision making as a messy process that is more like a judge weighing evidence in a difficult court case, foundationalist driven historians issue forth what Koyré called "Whiggish hagio­ graphies" that characterize decisions as a process in which any manual could have been used, because no judgement or interpretation was needed by the participants. Scientists are falsely portrayed as having been coerced and "mugged" by methodology because no truly human decisions are required.  

There is much in the above description of postmodern themes that this thesis will be in agreement with: The demise of foundationalism and the recognition (although trivial) of logical underdetermination; the rejection of simplistic (entailment) confirmation theory, naive correspondence theory and naive realism; the recognition and endorsement of fallibilism and defeasible judgmental processes whereby epistemic constraints are not employed algorithmically; the theory-influenced nature of observation and interpretation of experimental results; and the promotion of pluralism and the value of cultural diversity. However, the main intent of this thesis is to defend within this broad context the view that the epistemological project in the philosophy of science is alive and well, that a humbler philosophy of science is possible that avoids the polar evils of scientism and technological overconfidence on the one hand, and epistemological melancholy, indolence, and
anarchism on the other. In other words, my intention is to defend the position that there are epistemic constraints on our webs of belief, and that the use of these constraints is best seen as what I will be calling acts of intelligent contextual deliberation and inference given alternative hypertextual justificatory trails.

This thesis will be supported by:

(1) Contrasting the traditional foundationalist approach to epistemology with what I believe an epistemology must be that takes fallibilism seriously; one that recognizes the epistemological project as inherently comparative -- that our goal is not to separate the reasonable from the conceivable, but rather to separate the reasonable from extant alternatives; that further recognizes that constraints exist because we are not so cognitively powerful as to be able to create a monolithic, conceptual structure that will be forever impervious to the world's surprises.

(2) Supporting a post-foundationalist epistemology that reveals scientific methodology as flexible, fallible, historically piecemeal in its development, and itself experientially cumulative.

(3) Showing that notable versions of relativism (Quine, Rorty, Feyerabend), while convincingly attacking foundationalism by showing that it is impossible to definitely separate a true system of belief from all conceivable belief systems, by showing that propositional justificatory regress is always potentially infinite and cannot be stopped by the application of indubitable, trans-temporal rules, a priori methodology, or "brute" empirical givens that force or compel satisfaction and closure -- that while undermining foundationalist pretensions once and for all, these otherwise excellent critiques nevertheless leave contemporary philosophy with a false dilemma, i.e., foundationalism or an end to the epistemological project, foundationalism or "anything goes," a fixed methodology or nothing at all, a fixed, atemporal theory of rationality or just change based on interests, social forces, propaganda and rhetorical persuasion.
(4) Demonstrating that my thesis can pass an important test by examining what I will be calling the Copernican episode (1543-1616).

In developing my critique of the postmodern use of the philosophies of Quine, Feyerabend, and Rorty, I will be claiming that

a. Modern relativism is itself based upon a closet or implicit foundationalism, and that ironically modern advocates of relativism have not taken fallibilism seriously. They argue to this effect, "Since there is never any compulsion to stop (a foundationalist criterion for rationality) a justificatory trail of propositions—brought-forward-in-defense-of-other-propositions, that trail can go on forever and hence anything goes."¹²

b. This false dilemma can be exposed by "therapeutically" exorcising the need for complete justificatory closure by making a case for a third alternative: an epistemological project that seeks not indubitable rules that compel us to stop a justificatory regress, but flexible, ampliative, bootstrapped rules, themselves historically and experientially based and fallible, that guide, focus, and constrain scientists as strategic rules of thumb in making intelligent inferences given the alternatives they face at any given time.

c. That previous philosophies of science like that of Duhem and Kuhn also sought to break this false dilemma, but that decisions based on an unarticulated "good sense" (Duhem), or trans-temporal rules of appraisal that are empty of content because they are interpreted differently by each scientist bound by a paradigmatic tradition (Kuhn)

¹² I will use the closing date of 1616 because of the Church's action during that year, and because the debate between world systems, in terms of my notion of hypertextual adjudicatory trials, was well-articulated by that time. For instance, although Galileo's tower argument occurs in his 1632 Dialogue Concerning the Two Chief World Systems, the arguments he uses were available at the time of the Church's action in 1616.
-- that such notions, although well-intentioned in their exposition of the myth of a scientist as a detached observer cheerfully applying algorithms to an unproblematic brute experience of an external reality, nevertheless are unnecessarily vague and fail to break out of the false dilemma and ultimately leave us with relativism and an existential philosophy of science, i.e., we choose first and rationalize later. In short, I will be showing that in the case of the Copernican episode the details of Duhem's good sense can be specified and unpacked as flexible ampliative guides. Furthermore, I will claim that properly viewed from a postfoundational perspective that exorcises once and for all vestiges of the need for certainty, a case for rationality without ultimate satisfaction, comfort, or compulsion can be made.

Although I will not be making any exhaustive attempt to catalog all the ampliative techniques used by scientists, I will show that a key feature of the intelligent-inference-given-the-alternatives ampliative scaffolding that I will be defending involves decisions scientists make about auxiliaries, whether to pursue or accept them, whether to patch a web of belief or reject it, whether to pursue a justificatory trail to a certain level of depth and agree that the level is sufficient given the alternatives, or that the depth is insufficient and requires exploring alternative justificatory trails.

Accordingly, I want to make a distinction between (1) epistemic criteria used by scientists to constrain choices and (2) the rational scaffolding (acts of intelligent ampliative inference given alternative justificatory trails) used in applying such criteria. I contend that the process
of identification and analysis of (1) is viable and on-going. What criteria do scientists actually use? What does history reveal? What contexts best fit which criteria? Which criteria can be recommended for future reliability? And so on. However, my focus is mostly on (2), that insufficient attention has been given to this relatively simple methodological scaffolding, and that a great deal can be understood about the rationality of scientific debate, change, pursuit, and acceptance by focusing on scientific reasoning as a series of acts of intelligent contextual ampliative inference given alternative adjudicatory trails of reasoning.

To explicate what I have in mind by acts of intelligent contextual ampliative inference given alternative hypertextual justificatory trails, an expanded analysis of the Quine-Duhem thesis can be given. According to the original thesis, decisive refutations are always in principle impossible because given any hypothesis $H$ and an auxiliary $A$ required for the derivation an observation $O$, $[(H \cdot A) \supset O]$, from an observation $\neg O$ all that can be concluded is $\neg H \vee \neg A$. Moreover, because it is impossible to prove either that $A$ is true or that an $A^*$ does not exist such that $H$ is saved, $[(H \cdot A^*) \supset \neg O]$, decisive refutations are logically impossible. According to Duhem, "Pure logic

* Simple and less dramatic compared to such grandiose schemes as the methodology of research programmes (Lakatos), paradigm shifts (Kuhn), and research traditions (Laudan).
is not the only rule for our judgments; certain opinions which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable,"¹³ and according to Quine, "Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system (of belief)."¹⁴

In actual practice the situation is much more complicated than this. Any scientific theory $T$ consists of a set $H$ of hypotheses, $\{H_1\ldots H_n\}$, used with a set of background assumptions and auxiliaries $A$, $\{A_1\ldots A_n\}$. The conjunction of these two sets is in turn linked with a set of evidence $E$, $\{E_1\ldots E_n\}$. Leaving aside whether this evidence set can consist of more than just observation statements entailed by the sets $H$ and $A$, the original set $T$ is hypertextual, to use a metaphor of our time, in the sense that the relationship between $H$, $A$, and $E$ is more than just the linear deduction, $\{H_1\ldots H_n\} \land \{A_1\ldots A_n\} \subseteq \{E_1\ldots E_n\}$. Each particular $H_n$ in the set $H$, and each particular $A_n$ in $A$, and even each particular $E_n$ in $E$ can be seen as a node on another adjudicatory trail with a partially independent branch to its own sets of auxiliaries and sets of evidence. For instance, any particular auxiliary within $\{A_1\ldots A_n\}$, say $A_j$, may be acceptable to scientists because of its connection with an independent set of $H$'s, $A$'s, or $E$'s. Any weakening along the nodes of this independent set could in turn weaken $A_j$, or a potential $A'_j$ could be found that would restore the confidence in $\{A_1\ldots A_n\}$,
and hence in the original adjudicatory relationship, 
\{H_1\ldots H_n\} \cdot \{A_1\ldots A_n\} \leftrightarrow \{E_1\ldots E_n\}.

An identical situation exists for the set E. Given any E_n within this set, a complex trail of justification can be analyzed -- from assumptions concerning laboratory procedures and reliability of instruments, to concerns about the expertise of experimenters. Further compounding the complexity of these hypertextual, adjudicatory linkages is that judgments concerning the validity of the linkages themselves will involve a set of methodologies M, \{M_1\ldots M_n\}. See Figure 1.

Thus, the vagueness of Quine's "web of belief" and Kuhn's "paradigm," and the traditional notions of background information and auxiliary support are articulated and represented as a hypertextual network. This network model is analogous to current neural network models of human brain functioning and artificial intelligence computer modeling. In neural network theory learning is modelled as a gradual process of piecemeal and habitual strengthening of the nodes and connections amongst the nodes in a network. Similarly, we will see that the rationality of scientific pursuit and acceptance, as well as its piecemeal and gradual nature, can best be understood as a comparative gradual clarification, strengthening and concomitant weakening of different

* The symbol $\leftrightarrow$ is used here to indicate that the relationship between the evidence set E and the union of H and A is not necessarily one of entailment.

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hypertextual adjudicatory reasoning trails. In this way, the insights of holism can be preserved without the exaggerations that lead to relativism.15

Glossed this way, the essential point of the Quine-Duhem thesis is, of course, that any particular H, A, or E, in any particular hypertextual direction, can be questioned or saved given suitable adjustments in the justificatory trail. To use Quinian terminology, each "web of belief," \{H_1..H_n\} \cdot \{A_1..A_n\} \leftrightarrow \{E_1..E_n\}, consists of an infinite texture of affiliated webs, of nodes that are in turn constituted by their own webs of belief. To use Rorty’s terminology, any node H_n, A_n, or E_n is constituted by its own justificatory trail of propositions-brought-forward-in-defense-of-other-propositions. Since these trails "can go on forever," since justificatory regress is inevitable because there is no compulsion to stop at any node and say "this is it," the commonly accepted, negative epistemic message of the Quine-Duhem thesis is that there is never any epistemic warrant for knowing, when confronted by apparent recalcitrant evidence, whether to put our efforts into recontextualizing our webs to save the recalcitrant evidence or gestalt switch to another web.

My essential theme is that the negative message of the Quine-Duhem thesis only follows when viewed through foundationalist and deductive spectacles. That Quine, Feyerabend, and Rorty search for complete justificatory
closure via traditional foundationalist lines -- for either some indubitable, trans-temporal, a priori set $M$, and/or brute empirical givens that would compel acceptance of individual $H$'s, $A$'s, and $E$'s -- find none, and then conclude that any attempt at preserving some version of the traditional epistemological project is misguided. My essential claim is that much of the history of science and current scientific practice can be better understood by seeing that scientists make intelligent ampliative inferences concerning the reliability of these hypertextual trails, that they make comparative assessments regarding particular nodes, whether to accept, reject, or further pursue justification for them. That at any particular time, they may have very good ampliative reasons for accepting a particular $M_n$ that is used to link a particular $A_n$ with a set $E$, which in turn supports a set $T$, {$H_1..H_n$} • {$A_1..A_n$} <-> {$E_1..E_n$}. Or, that at any particular time they may have good reasons to doubt the reliability of a particular $A_n$, causing scientists to reinterpret evidence in the light of pursuing different $H_n$'s and $A_n$'s.

For an example of what I have in mind, consider a few durable propositions from the corpus of scientific belief.

That the sun is at a greater distance from the Earth than the moon. ($H_1$)

That the planets are at a greater distance from the Earth than the moon. ($H_2$)
That the sun is larger than the moon. \( (H_3) \)

That the planets are not always the same distance from the Earth. \( (H_4) \)

These propositions have been accepted since Greek astronomy.\(^{16}\) Why have they been accepted? Let's call the set of these beliefs a theory \( T \) about the relative distances from Earth of the major astronomical bodies in our planetary system. The moon is observed to occlude the sun and the planets. \( (E_1 \text{ and } E_2) \) In our everyday experience when one object passes in front of another object and blocks it from view we have learned that the former is in front of the latter. \( (\text{An independent set } E' \text{ supporting an } A_1) \) So, if one celestial object passes in front of another celestial object, and never vice versa, we have a good reason to believe that the occluded object is farther away from us.\(^{17}\) \( (A_2 \text{ supported by } A_1, \text{ with } E_1 \text{ and } E_2 \text{ to support } H_1 \text{ and } H_2) \) During a total eclipse of the sun by the moon, the moon is observed to fit perfectly over the sun. \( (E_1 \text{ and } E_4) \) If the sun is farther away than the moon, then basic geometric perspective implies that the sun must be larger than the object that occludes it. \( (A_3 \text{ supported by another evidence set } E'', \text{ all supporting } H_3) \) Similarly, the planets are observed to vary in brightness and apparent diameter. \( (E_5) \) Our everyday experience indicates that in many cases when an object varies in brightness and apparent size, that object
also varies in its distance from us. \((A_4, \text{ supported by } E^{***}, \text{ in conjunction with } E_5 \text{ supporting } H_4)\)

There are many points along the trail of these propositions—brought-forward-in-defense-of-other-propositions in which one can imagine alternatives, that if plausible, would destroy the ampliative comfort we have with the chain of inferences. Perhaps the analogical reasoning that infers the reasonableness of generalizing our terrestrial experience to celestial matters is in error. \((-A_2\) Or, perhaps the sun and the planets are made of a special transparent material (a new theory \(T_3\)), such that when they pass in front of the moon the illusion of occlusion \((-E_3\) is produced. \((\text{Thus, } H_1, H_2, \text{ and } H_3\) Or, perhaps as planets revolve on what are actually homocentric circles, some internal celestial mechanism within each planet causes an alternating expansion and contraction of the planet that just happens to coincide with the observed periodic variation of brightness and diameter. \((T_3, \text{ which implies the nonapplicability of } A_4, \text{ implying } H_4)\) Perhaps naked eye observations of the moon occluding planets are in error due to the relative brightness of the moon and the difficult nature of inferring what one is seeing under such circumstances. \((-E_1. \text{ After all, observations are inferences also. There are no brute facts.})\) Perhaps, the variation of brightness and diameter of planets is due to perfectly synchronized periodic atmospheric fluctuations. \(16\)
(E₃ and again the nonapplicability of A₄, implying -H₄) And, so on.

Given sufficient imagination, there are always conceivable alternatives. At any point along an adjudicatory trail one can imagine an alternative. But in acts of intelligent ampliative inference, the issue is always whether there are reasonable alternatives given the evidence. All of the above alternative possibilities were discussed by ancient astronomers and eventually rejected as unlikely. All of the above alternative possibilities could be defended today given sufficient imaginative effort. For instance, if one argued that modern parallax detection methodology corroborates the relative distances of the moon and planets, an alternative explanation for differential parallax could be conjured up. A modern scientist, who has made the mistake of accepting Feyerabend's conclusions, could conceivably spend a lifetime pursuing the necessary auxiliary patches and alternative justificatory trails for the thesis that the planets are not at a greater distance than the moon.

It is my contention that the epistemological project is not in any jeopardy by the mere logical conceivable alternatives, provided that a sufficiently fallibilistic perspective is constantly kept in focus. Scientists are capable of making rational decisions concerning alternative justificatory trails without complete justificatory closure, whether to pursue or accept auxiliaries, whether to patch a
web of belief or reject it, whether to pursue a justificatory trail to a certain level of depth and agree that the level is sufficient or insufficient.

**Feyerabend**

I begin with a chapter on Feyerabend for two reasons: (1) I wish to concentrate on what I will be calling the Copernican episode, the historical analysis of which is the source of much postmodern relativism, and according to Feyerabend in *Against Method* it is "a perfect example," because during this time there was "not a single objective reason" to be a Copernican; (2) Feyerabend is a perfect example himself of the polemical tactics of the relativist — conjure up a conceivable justificatory trail, claim that its competitor was thus "not decisive," then conclude that there was no rational reason to pursue or accept one trail or the other.

Thus, in this chapter I analyze Feyerabend's critique of Galileo's tower argument, his criticism of the epistemic weight given to telescopic observations, and his historical sequencing of important events during the Copernican episode. I will claim that all Feyerabend has demonstrated

* It is my contention that there is no difference, in terms of the following, between such singular propositions as above and research programmes or paradigms: All beliefs regardless of generality involve an infinite texture of propositions—brought-forward-in-defense-of-other-propositions, a web of propositional relations.
is that justificatory regress concerning these matters could have continued, not that some paths did not have more plausibility than others. I will claim that Galileo's tower argument was not a "counterinduction," as claimed by Feyerabend; that Galileo's argument is best seen as the following of a different justificatory trail and that the key issue is whether, given the scientific context, Galileo had good reasons to believe in the fruitfulness of this trail. My argument is that by the date of the tower argument he had ample reason.

I will also show that a careful analysis of Feyerabend's history involves misleading conclusions due to his "jumping around" from the mid 16th-century to the early 17th-century and ignoring significant astronomical developments that took place in the late 16th and early 17th centuries, i.e., comets, novae, and Tycho's observations.

I will conclude this chapter by claiming that there are significant inconsistencies in Feyerabend's "anything goes" relativism, indeed that Feyerabend is not a relativist but rather is advocating what I will call normative relativism, a plea for methodological flexibility and ideological tolerance in the tradition of John Stuart Mill whom Feyerabend cites often. For instance, in spite of his claims to the contrary, Feyerabend's persistent citation of "progress" conditions and his endorsement of "an elastic and historically informed methodology" constitute
methodological advice. In other words, in spite of his rhetoric, much of Feyerabend's work is consistent with a contemporary epistemological perspective that sees scientific methodology as flexible, fallible, and historically piecemeal in its development, rather than as a set of trans-temporal, a priori rules whose application is algorithmic regardless of context.

The Copernican Episode and Influential Interpretations

Chapter 2 will be an exegetical setting of the stage for the claims I will be making in the ensuing chapters. I will describe the events, attitudes, environments, and labyrinth of philosophies and scientific positions of the period of time I will be calling the Copernican episode (1543-1616), revealing the tension of massive empirical and conceptual problems underlying the choice between helio-static and geostatic pursuit. Next, I will examine the philosophies of science of Duhem, Kuhn, and Lakatos. For Duhem and Kuhn (chapter 3), I show that as "unintentional" relativists they misread and epistemologically underrated the parameter fixating and systemic virtues of heliostasis. For Lakatos (chapter 4), I will show that his amended version of novel facts and its use in the methodology of scientific research programmes led him to overrate these same virtues in claiming that heliostasis superseded
geostasis because the former had immediate and decisive support.*

This chapter concludes with an examination of the distinction between the pursuit and acceptance stages of scientific theories. In particular, supported is the claim that scientists often begin to rationally explore the ramifications of a new theory -- its relationship to other sciences, its potential problem solving ability, its potential replacement ability over an older rival, and its potential completion with new auxiliary hypotheses -- long before its success is clear to the scientific community at large. This notion of pursuit will be expanded by examining it in the light of intelligent ampliative inference and the relative fruitfulness of alternatives.

The Copernican Episode -- Auxiliary Hypotheses and Rational Pursuit

A key historical source of modern relativism is the Copernican episode. Even many defenders of the post-foundational epistemological project often take a more or

* Throughout this thesis I will be using "geostasis" or "heliostasis" when referring to an earth-stationary or sun-stationary model or world system. Neither the Ptolemaic or Copernican systems are truly geocentric or heliocentric. In Ptolemy's universe the earth is eccentric to the true center of the universe. In Copernicus's universe, the center of the universe is the center of the earth's orbit, but the sun is not at that position. Only Tychonic and semi-Tychonic systems have the earth as the exact center of the universe. Hence, they will be referred to as "geoheliocentric."
less "hands-off" approach to this period in response to relativist's critiques of traditional universal methodologies -- claiming that the situation was so unclear and epistemologically messy that followers of almost any position were justified in holding the positions they did -- and reserve their epistemological claims for a later period of time (1630-1700). Thus, in chapter 5 my thesis will be submitted to a strong test by examining the acceptance, rejection, and pursuit of auxiliaries during this time. In short, I argue that modern epistemologists ought not to hand over this episode to the relativist.

Although I reject Whiggish positivistic historical interpretations of this period and decisive superseding positions like that of Lakatos, I argue that Copernicans had good reasons to pursue the needed auxiliaries for heliostasis and that this can be revealed from a perspective of decision-making guided by emerging constraints that were the result of judging justificatory trails given the alternatives. In other words, we can do better than, "Well, an historical analysis of the time shows that everything was such a mess that it was rational for anyone to do just about anything." I believe it is possible and necessary to replace this hands-off stance with the above approach, because relativists respond to the mess-analysis by saying, "Well, if the situation was so unclear, then commitment to heliostasis must have been based solely on non-rational
external factors -- and this is historical evidence that scientists choose their theories first and then create later, through propaganda, rhetorical persuasion, and deconstructive reinterpretation, the reasons and experiences to support their original irrational choices."

I am, of course, not claiming that a theory of scientific methodology is under an obligation to explain every early episode of theory choice as rational. However, on the other hand, it is a mistake, as Feyerabend claims, that every example of new theory pursuit is the irrational introduction of a new perspective that can only be accepted initially by blind faith. My point is that to concede to the relativist the initial irrationality of all early theory commitment is to already become unnecessarily entangled in the relativist game. What Feyerabend and others try to do is similar to Zeno’s paradox -- we can’t move from point A (irrationality) to point B (rationality) because to move to B we must first go half way, and before that half way, and so on. So we really never leave point A (irrationality). My claim is that we need not accept that the initial point A is completely irrational (even though there are typically some "external," non-methodological influences), or that at some early point along the way there are not some very good reasons to believe in the fruitfulness of moving to B. My position is that non-epistemic influences may cause scientists to look in certain directions, to see certain
things not seen before, but that it is a mistake to then conclude that all further actions -- commitment, acceptance, or serious pursuit -- have as their principal cause the non-epistemic influences. When scientists make decisions, they want to be right and they want support for their biases. So they must find a network of support for their biases. Often that support cannot be found, or as we will see, the very pursuit of that support exposes serious weaknesses in the network and their positions crumble.

Thus, chapter 5 constitutes the core of my thesis. I support the position that early personal commitment by major Renaissance figures to the pursuit of making heliostasis superior to geostasis was not irrational, rejecting the view that it was "nothing but blind faith until ... the auxiliary sciences, the facts, the arguments ... (were found) that turn(ed) the faith into sound knowledge." I argue that the major players had very good reasons to pursue needed auxiliary patches to heliostasis, such as an alternative dynamics to Aristotelianism and better model orbital fit with planetary position data.

I support this claim by first showing that the relativist's charge -- that allegiance to a geostatic or heliostatic world view during this time can be explained only by an understanding of the external sociological forces, personal interests, and propaganda -- is based on an accurate critique of traditional, fixed methodological
proposals (inductivism or naive confirmationism, falsificationism, simplicism). However, I then argue that this critique is erroneously generalized to conclude that there was no other rational basis for heliostatic pursuit.

Concerning the relativist critique of traditional, fixed methodological proposals, we can concede the following.

**Against Inductivism:** The Copernican system was not a simple inductive generalization from either new or old observations. Both the Copernican and Ptolemaic systems were factually inadequate quantitatively in terms of predicting planetary positions. Hence, from an inductivist standpoint the Copernican and Ptolemaic systems were locked in a normative standoff and one is left with only nonnormative factors to understand why scientists accepted or pursued one or the other.

**Against Falsificationism:** One cannot make a case for falsificationism -- that Ptolemy's system was irrefutable, and hence not scientific, and Copernicus's refutable, and hence scientific -- nor, if one grants that the Ptolemaic system was refutable, that a crucial deciding observation emerged that falsified this system at an early stage of appraisal of the two systems. The charge of irrefutability is based on the "adding epicycles myth." Astronomers of the time were not adding epicycles to epicycles to the Ptolemaic system to keep it from being falsified by the observational data. Contemporary computations show that the Alfonsine Tables were based upon the strict Ptolemaic system of single epicycles. Furthermore, followers of heliostasis added epicycles to epicycles to adjust the Copernican system to known observational problems. To counter the charge that the Ptolemaic system was falsified, neither the Prutenic Tables, the observations of the phases of Venus, nor stellar parallax will help a falsificationist. The Alfonsine Tables were equal to or more accurate than the Prutenic Tables often enough to prohibit establishing a clear case of superiority of the latter, an appeal to the phases of Venus ignores Tycho's geoheliocentric solution, and the observation of stellar parallax occurred much too late to provide a normative reason for being a Copernican. Hence, from a falsificationist perspective the Copernican and Ptolemaic systems were locked in an normative standoff and one is left with only nonnormative factors to understand why scientists accepted or pursued one or the other.
Against Simplicism: Finally, the revelation of the "80/34 myth" discredits simplicism. To pay for removing the equant, Copernicus must use numerous epicyclets, and by the time of the Copernican system's evolution from the Commentariolus to the De revolutionibus, it is difficult to tell exactly how many circles the Copernican system had. Hence, from the standpoint of simplicism, nonnormative factors must be appealed to solely to account for why key scientists committed to the Copernican system.

However, I will then show that the relativists use the results of these analyses to paint a purely "anything goes" perspective of the choices facing Renaissance scientists, that any proposition could have been brought forward in defense of any other proposition given the requisite commitment and creative effort, and that this conclusion ignores the overall effect of the combination of many scientific developments (observational and collateral theoretical), particularly between 1570 and 1610, and the parameter fixating features of heliostasis and how this was viewed given the alternatives. In short, I will claim that in spite of, indeed because of, the massive empirical and conceptual problems for both world systems during the Copernican episode, certain systemic unifying features of heliostasis, in conjunction with the core problem situation, stood out as a constraint on decisions of pursuit and fruitfulness. I intend to show that these features were recognized and discussed by both supporters and nonsupporters of heliostasis, that this recognition and discussion constituted an emerging appreciation of the potential normative importance of parameter determination, and that
during the Copernican episode this recognition served at least as a rational basis for judging the fruitfulness of auxiliary pursuit.

In using the notion of parameter determination, I will be referring to the explanatory fixing of a numerical or mathematical value. For instance, today we find physicists bothered by the fact that they do not yet have a coherent theory of everything that explains exactly why a proton has the mass that it does in relation to the mass of an electron. Similarly, as we will see, both supporters and nonsupporters of geostasis were eventually bothered by the fact that this system does not fix and explain key parameters of planetary motion, such as the number of retrogressions observed for each planet, epicycle radius sizes, durations, and periodic changes in luminosity, and the obvious linkage of these parameters with the sun. When the parameters of one part of a theory are intricately linked with the parameters in another part of a theory, such that one set of parameters cannot be adjusted without affecting all the others, we speak of that theory having theoretical unity -- although theoretical unity can also refer to overall coherence with auxiliary hypotheses and background knowledge. (We will see that initially heliostasis had a high degree of the former, but a low degree of the latter.) Hence, we see a close relationship between parameter determination, theoretical unity, and explanatory power. In
this regard, following Shapere, we can be

... aware that no precise analysis is available of the notion of 'synthetic unity,' or even of 'relative degree of synthetic unity.' However, philosophers err in the most fundamental way in supposing that such judgments as ... made ... about the degree of synthetic unity achieved by Copernicus cannot be made without such prior analysis. On the contrary, any such precise analysis must be made on the basis of close study of paradigm cases of what are naturally called synthetic unifications. The general notion of 'synthetic unity' may not have been given a precise analysis; but the respects in which the present case achieves such unification, relative to its predecessors, are clear, and the adequacy of any analysis of the general notion must be judged in the light of this and other such case studies. 23

Accordingly, in examining the arguments and work of major players such as Kepler and Galileo, we will see contrary to the current popular expositions of this period, these scientists were reflective enough to be aware of many of the sociological forces that surrounded them, of potential biases in their thinking, and were capable of separating these in crucial decisions where they wanted to be right." That scientists concerned with career, patronage, and influence do not commit to theories for the "hell of it," and they make every effort to look for features

For instance, Kepler was fully aware that much more than Pythagorean elegance was required of an astronomical theory, Tycho was fully aware that much more than scriptural authority was needed to commit to geoheliocentrism, and Galileo based much of his career on making a better case for Copernicanism (involving both methodology and collateral theoretical support) precisely because he recognized certain external appeals were insufficient.

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that will guide them in making intelligent inferences and choosing plausible and fruitful reasoning trails. And that the relativist analysis of this period is about as helpful in guiding our understanding of the choices scientists made during this period as Yogi Bera’s moronic advice, "When you come to the fork in the road, take it!"

Finally, this analysis will be combined with my general methodological stance -- that scientists engage in a rational process of making intelligent ampliative inferences given alternatives, that constraints are used in focusing attention when faced with confusing situations full of apparent disconnected details and disciplining decision-making given a plethora of alternatives, and that this justificatory process cannot, but need not, have indubitable closure.
Notes for Introduction:

1. This traditional project was described clearly by Whewell. An historical survey of successful practice, "to review the journey. . .

may not only remind us of what we have, but may teach us how to improve and increase our store . . . (and) afford us some indication of the most promising mode of directing our future efforts to add to its extent and completeness." (Whewell, 1857, vol. 1, p. 4)


Although Galison would not associate himself with all the features of this postmodern story, particularly its central emphasis on social determinants of knowledge at the expense of nonsocial constraints, his experimental or technological constructivism, and emphasis on data production as a result of complex technological choices constituting variable interfaces with phenomena, is used by social constructivists to critique that part of the traditional project that saw empirical evidence as unproblematic and logically related to theories.

3. For the relativization of reality, the flexibility of cognitive frames, and "getting along in the universe," see Munevar (1981, pp. 116-17). The phrase "sculptors of reality" is Feyerabend's (1989, p. 404). The notion of an "implicate" reality is that of David Bohm (1980).

According to Shapin (1982, p. 194) an analysis of numerous observational disputes in the history of science show that

". . . natural reality did not possess the coercive force with which actor's discourse often imbued it. Reality seems capable of sustaining more than one account given of it, depending upon the goals of those who engage with it. . ."

According to Feyerabend, "good physics" supports the view that

"People have acted upon the world in many different ways, partly physical, by actually interfering with it, partly conceptually, by devising languages and making inferences in them. Some of the actions found a response, others never got
off the ground. (And) . . . this suggests that there is a reality and that it is more yielding than is assumed by most objectivists. Different forms of life and knowledge are possible because reality permits and even encourages them. . . ." (1991, p. 516, emphasis added)

Here are further samples of Feyerabend's use of modern physics and what I have elsewhere (Pine, 1989, Chapter 8: Quantum Physics and Reality) called a "participatory ontology":

"It is true that the validity of Maxwell's equations is independent of what people think about electrification. But it is not independent of the culture that contains them. It needed a very special mental attitude inserted into a very special social structure combined with sometimes quite idiosyncratic historical sequences to divine, formulate, check, and establish the laws scientists are using today. (Feyerabend, 1987, p. 125)

". . . as the most fundamental and most highly confirmed theory of present day physics, the quantum theory rejects unconditional projections and makes existence depend on special historically determined circumstances. Molecules, for example, the basic entities of chemistry and molecular biology, do not simply exist -- period -- they appear only under well-defined and rather complex conditions." (Feyerabend, 1989, p. 402)

"Scientists, being equipped with a complex organism and embedded in constantly changing physical and social surroundings, used ideas and actions (and much later, equipment up to and including industrial complexes such as CERN) to manufacture, first, metaphysical atoms, then, crude physical atoms, and, finally, complex systems of elementary particles out of a material that did not contain these elements but could be shaped into them. . . . The material humans (and, for that matter, also dogs and monkeys) face must be approached in the right way. . . . this material is more pliable than is commonly assumed." (Ibid., pp. 404-405)

Also, according to Feyerabend, such thinking is "firmly based on Bohr's ideas. . . and almost identical with. . . Kuhn's . . . later philosophy." (Ibid., p. 405, note 26)

According to Pickering, "New physics phenomena were . . . brought into existence by appropriate 'tuning' of interpretative practices," (1984, pp. 409-410) and idiosyncratic un-obligated choices "produced the world of the new physics, its phenomena and its theoretical entities." (Ibid., p. 404)

Hence, knowledge construction is best seen as primarily social.
"In principle, the decisions which produce the world are free and unconstrained. They could be made at random, each scientist choosing by the toss of a coin at each decision point what stance to adopt. Instead... we have seen that... scientific judgments... (were) socially produced." (Ibid., pp. 405-6)

Finally, according to Feyerabend, Pickering's work shows that there are "many points of contact between the establishment of a scientific result and the conclusion of a complicated political treaty." (1988, p. 394).


As far as I can tell, Feyerabend defines most clearly this new notion of progressive knowledge as follows:

"Knowledge so conceived is not a series of self-consistent theories that converges towards an ideal view; it is not a gradual approach to the truth. It is rather an ever increasing ocean of mutually incompatible (and perhaps even incommensurable) alternatives, each single theory, each fairy-tale, each myth that is part of the collection forcing the others into greater articulation and all of them contributing, via this process of competition, to the development of our consciousness." (Feyerabend, 1988, p. 21.)


7. Feyerabend in his 1978, specifically Part II, chapters 2-5, 10, and his 1987, particularly chapter 11.

According to Feyerabend, "proliferation" of incommensurable views is important because along with the tenacious living within a particular tradition this interplay

"amounts to the continuation, on a new level, of biological development of the species and it may even increase the tendency for useful biological mutations. It may be the only possible means of preventing our species from stagnation. (Feyerabend, 1985, vol. 2, p. 144)
According to Nordmann, philosophers should take Feyerabend's major message more seriously, getting beyond "well-worn strategies of denial," and realize that his challenge amounts to

"... a simple, but deeply troubling question: do the success and the benefits of science and technology warrant or require a social arrangement which privileges the highly specialized development of certain human faculties at the expense of others." (Nordmann, 1991, p. 325.)

As I understand Fine's (1986a, 1986b) "natural ontological attitude" and its trusting attitude towards the "local" validity of scientific practice, it is intended to not only undercut the image of the philosopher of science as an expert giving advice, but also the Lakatosian program of rational reconstruction as a valuable public relations interface with politicians and the general public that supports science against mysticism and quackery.

8. Thus it appears that postmodern philosophers would reject as a caricature of their position Laudan's claim (1977, pp. 201-210) that they advocate a premature sociological analysis of scientific decisions because they are prone to accept a simple-minded theory of rationality.

9. This phrase is used by Shapin (1982, p. 198) but borrowed from Harold Garfinkel, Studies in Ethnomethodology (Englewood Cliffs, N.J., 1967). In discussing Kuhn, Gutting claims that both critics and supporters of Kuhn are "mistaking a new approach to scientific rationality for an attack on it," (Gutting, 1980, p. 10) and Kuhn himself claims in The Essential Tension that "... my work has been deeply sociological, but not in a way that permits that subject to be separated from epistemology." (1977, p. xx).

10. In his classic paper "Minds, Brains, and Programs" (1980), John Searle argued against what he termed the "strong AI thesis" by describing a man in a room using a gigantic manual that gave instructions in English for answering questions in Chinese. Both the answers and the questions were nothing but meaningless marks on paper for the translator. Searle's point, of course, was that no thinking, judgment, or understanding would be involved is using the manual.

11. Shapin, 1982, pp. 197; Rorty's 1979, and of course Feyerabend's claim that "anything goes" is not his principle, but an unintentional implication of dogmatic rationalism (1988, p. vii). Kuhn also makes a similar claim in the Postscript to the second edition of his Structure of Scientific Revolutions (1970), and in his "Logic of Discovery or Psychology of Research," and "Reflections on my Critics" in Lakatos and

According to Kuhn, in commenting on areas of alleged agreement with Feyerabend,

"... to describe the argument as a defence of irrationality in science seems to me not only absurd but vaguely obscene. I would describe it ... as an attempt to show that existing theories of rationality are not quite right and that we must readjust or change them to explain why science works as it does. ... One scientific theory is not as good as another for doing what scientists normally do. In that sense I am not a relativist. (1970, p. 264)

According to Gutting,

"Philosophers need to get beyond their caricatures of Kuhn as a proponent of science's irrationality and instead develop and evaluate the very promising positive theory of rationality sketched in his work. ... (that) his frequent insistence that he is merely sketching in a tentative and often halting way a new picture of science should have suggested the need for sympathetic attention to context and qualifications. (Gutting, 1980, pp. 18, 20, n. 8)

12. According to Rorty in his *Philosophy and the Mirror of Nature* (1979, p. 159), "... we may think of both knowledge and justification as privileged relations to the objects those propositions are about. If we think ... (this way), we will see no need to end the potential infinite regress of propositions-brought-forward-in-defense-of-other-propositions."

13. Duhem, 1954, p. 217. This statement from Duhem is often cited out of context. It occurs in the last section of Duhem's famous Chapter VI in his *Aim and Structure of Physical Theory* where the notion of "good sense" is introduced. Prior to this section, Duhem has made the point that a recalcitrant experiment "does not tell us" what to do; "it leaves to our sagacity the burden of guessing. ... (so) what impels the physicist to act ... is not logical necessity." (p. 211)

At the beginning of his "Good Sense" section, Duhem summarizes,

"When certain consequences of a theory are struck by experimental contradiction, we learn that this theory should be modified but we are not told by experiment what must be changed. It leaves to the physicist the task of finding out the weak spot that impairs the whole system. No absolute principle directs this inquiry, which different physicists may conduct in very different ways without having the right to accuse one another of illogicality." (p. 216)
Duhem then talks about the different actions and attitudes of two physicists: One who timidly attempts to patch an apparently refuted theory by "complicating the schematism" or "invoking various causes of error," and one who boldly jettisons a fundamental theoretical postulate. According to Duhem, neither physicist "has the right" to condemn the other. He then makes his claim about the impotence of pure logic and the necessity of good sense to decide such matters. But one searches in vain in Duhem for an ampliative articulation or story of this good sense.


Also, according to Quine,

"Any one of the statements (in a theory) can be adhered to in the face of adverse observations, by revising others (in the theory). . . . If in the face of adverse observations we are free always to choose among various adequate modifications of our theory, then presumably all possible observations are insufficient to determine theory uniquely." (1975, p. 313)

Although Quine himself has since backtracked on his adherence to the Quine-Duhem thesis -- he agrees with Grunbaum that it "is untenable if taken nontrivially" -- and has since claimed that its force is more applicable to a holistic theory of meaning (See his 1962 letter to Grunbaum, reprinted in Harding, 1976, p. 132.), his statements are still cited by Rorty and other postmodernists as canonical.

15. It is my contention that the vagueness of such holistic notions as web of belief and paradigm is partially the source of the appeal of relativism. Larry Laudan's *Science and Values*, 1983, also models the process of scientific change as a network of relationships of goals, theories, methodologies, and empirical results. The piecemeal and rational nature of scientific change is seen in that these nodes need not, and often do not, change all at the same time. In addition to neural network theory, my thesis is heavily indebted to Laudan's work.

16. Commenting on the varying distances of each planet from the Earth, Simplicius says, "This is indeed obvious for the star called after Aphrodite [Venus] and also the star Ares [Mars] seem, in the middle of their retrogressions, to be many times as large, so that the star Aphrodite actually makes bodies cast shadows on moonless nights." Heath, 1913, p. 222.

17. Further away at that moment and at all moments, assuming in the latter case that one has good reason to also believe that the eccentricity of the orbit of the occluded object is not
extreme as is the case with the planet Pluto.

18. According to Feyerabend, one can still more than adequately defend an Aristotelian framework such that it would encourage research and lead to results -- it is simply a matter of taking "money away from intellectuals." (1978a, pp. 143-180; and 1978b, pp. 53-65)

19. 1988, pp. 17, 118. According to Feyerabend, the tactics used to defend a new idea such as Copernicanism can only be "... verbal gesture(s), a gentle invitation to participate in the development of the new philosophy."

According to Feyerabend, it is clear that the Copernican episode shows us that,

"... allegiance to the new ideas will have to be brought about by means other than arguments. It will have to be brought about by irrational means such as propaganda, emotion, ad hoc hypotheses, and appeal to prejudices of all kinds. We need these "irrational means" in order to uphold what is nothing but blind faith until we have found the auxiliary sciences, the facts, the arguments that turn faith into sound 'knowledge'. (Ibid., p. 119.)


22. Ibid., p. 119.

Chapter 1: Relativism, a Case Study -- The Philosophy of Paul Feyerabend

Crombie once remarked that philosophers "looking for historical precedent for some interpretation or reform of science which they themselves are advocating, have all, however much they have differed from each other, been able to find in Galileo their heart's desire."\(^1\) Although other critics have claimed that Feyerabend turns Galileo into an "unrepentant" supporter of Feyerabend's claims on methodology through a biased selection of Galileo's statements and historical aspects of the Copernican episode,\(^2\) my intention in this chapter is to go beyond this and take Feyerabend's overstatement of Galileo's role in the Copernican episode and show how Feyerabend's version of relativism is a result of a foundationalist reading of the rational scaffolding of the hypertextual networking of evidence, hypotheses, auxiliaries, and methodologies as outlined in the introduction. Furthermore, this new look at Feyerabend will be placed within the context of two essential claims of his philosophy: (1) That "anything goes" is allegedly not a "deep conviction" of Feyerabend's, "but the terrified exclamation of a rationalist who takes a closer look at history"\(^3\); (2) that knowledge and progress are not to be defined in convergent, cumulative, or directional terms, but rather must be seen as non-directional -- as
proliferation and diversity of locally articulated perspectives, which are the natural result of freedom and self-actualization.

Concerning (1), it is my contention that while providing an excellent critique of foundationalism, Feyerabend misses the mark badly in terms of failing to see the possibility of a post-foundationalist epistemology and this is due in large part to being a frustrated foundationalist himself. Concerning (1) and (2), it is my contention that much of Feyerabend's rhetorical and philosophical bravado should be seen as "shock" terminology related to his foundationalist and/or universal methodology target audience, an audience that he variously calls the "rodents of neopositivism and . . . critical rationalism," and "propagandists of naive scientism." Furthermore, I argue that placed in the proper post-foundationalist perspective we can see Feyerabend as a "regular guy, methodologywise," who advocates a normative position that values self-actualization and freedom, and pluralism as means to these goals.

Feyerabendian exegesis is precarious. For like a Dadaist he refuses to be identified with a "programme," claims that he is only a "journalist who is interested in strange and bizarre events" who intends to be "crude and superficial" as an antidote to "empty sophistication," and in his own words, "When writing a paper I have usually
forgotten what I wrote before and application of earlier arguments is done at the applier's own risk." No

evertheless, note how mild by the standards of many post-positivist philosophers of science, some of Feyerabend's (past?) essential claims are:

(1) That progress in science requires theoretical pluralism and the proliferation of competing ideas.

(2) That contrary to the traditional foundationalist epistemological project there is an inherent but wise historicity to methodology; that methods change and old methods are violated as new views are tried and accepted. That methodologies like ethical prescriptions "may be useful rules of thumb but . . . are deadly when followed to the letter."10

(3) That factual interpretation is not theoretically neutral; that a clash between theory and fact is not a clash between a theory and a neutral brute facticity which always compels either simple confirmation or falsification, but is rather a clash between theory and theory -- thus opening the possibility for theory to overturn so-called facts.

(4) That because of 1-3, naive critical rationalism (naive empiricism or falsificationism) shipwrecks into relativism; that scientists neither use nor ought to use these a priori straightjackets; that an analysis of various versions of critical rationalism shows that by their own standards "anything goes" because no locus of rationality can be found for action.

(5) That acceptance of Copernicanism was a messy process; that the real historical situation refutes (by its own standard) naive critical rationalism.

As acceptable or mild as these claims may be to many contemporary philosophers of science, what is clearly not accepted by these same philosophers is Feyerabend's generalization from the failure of previous analyses of methodology "that the path to relativism has not yet been
closed by reason," that scientists make unconstrained decisions and leaps of faith concerning new theories first and then rationalize new perspectives into existence via propaganda, brainwashing, and reinterpretation of experience;" that during the Copernican episode, "The Church was on the right track. . . . that its indictment of Galileo was rational. . . ;"12 that the establishment of a scientific result is not unlike a political process of persuasion and negotiation;13 and that the "more Lysenko affairs the better (and) cheers to the fundamentalists in California."14 According to Margherita Von Brentano, this clash between insights and inferences ought to make us "eager to discover why . . . (in Feyerabend's) way of reasoning, from highly reasonable and pragmatic propositions occasionally rather absurd conclusions emerge."15 Eager, indeed, and in this chapter I will show how a wedge can be driven between Feyerabend's methodological insights and his anarchist conclusions once his closet foundationalism is exposed.

First, however, the Galileo affair. According to Feyerabend, the Copernican episode, and Galileo's role in particular, is a "perfect example" of what scientists really do. Scientists do not follow rules to achieve progress in either contexts of discovery or justification. Rather, they break all existing rules and conceptual boundaries by not only introducing radically new hypotheses that are
immediately seen to be inconsistent with accepted facts, but then rationalize these hypotheses in the face of the recalcitrant accepted facts with other ad hoc and/or refuted auxiliaries. In other words, typically scientists build up systems of mutually refuted hypotheses (heliostasis, a new dynamics, the reliability of telescopic observations) with the goal of turning apparent refuting instances (the tower experiment, apparent size and magnitudes of Venus and Mars) into support by "inventing" new experiences, what Feyerabend calls "natural interpretations." In short, revolutionary scientists like Galileo proceed "counter-inductively."

Furthermore, according to Feyerabend, "a more critical attitude," so beloved of methodologists both of the confirmationist and falsificationist variety, on the part of Galileo and other supporters of Copernicanism "far from accelerating . . . development, would have brought it to a standstill." In particular, Galileo's reinterpretation of the tower experiment is an example of the creative "trickery" that is necessary to buy time for a new view by confusing and thus defusing the "well-entrenched reactions" of an older view; and, his use of the telescope and the acceptance of its results as reliable was a clear case of "gross negligence" and a "fruitful disorderliness" where "ignorance, or sloppiness, . . . or superficiality, or muddleheadedness turned out to be bliss." Thus, an analysis of the Copernican episode shows us that,
It is clear that allegiance to ... new ideas will have to be brought about by means other than arguments. It will have to be brought about by irrational means such as propaganda, emotion, ad hoc hypotheses, and appeal to prejudices of all kinds. We need these 'irrational means' in order to uphold what is nothing but a blind faith until we have found the auxiliary sciences, the facts, the arguments that turn the faith into sound 'knowledge'.

To understand the full force of Feyerabend's argument, several elements of his philosophy must be kept sharply in focus.

(1) Like an artist, a revolutionary scientist such as Galileo is creating a perspective, not uncovering an independent reality of separate, objective things. He is "inventing" new experiences.

A scientist, an artist, a citizen is not like a child who needs papa methodology and mama rationality to give him security and direction, he can take care of himself, for he is the inventor not only of laws, theories, pictures, plays, forms of music, ways of dealing with his fellow man, institutions but also of entire world views, he is the inventor of entire forms of life.

Speaking paradoxically, but not incorrectly, one may say that Galileo invents an experience that has metaphysical ingredients.

(2) Progress for Feyerabend does not involve creating more inclusive perspectives or convergence, but rather proliferation of local successes.
Knowledge so conceived is not a series of self-consistent theories that converges towards an ideal view; it is not a gradual approach to the truth. It is rather an ever increasing ocean of mutually incompatible (and perhaps even incommensurable) alternatives, each single theory, each fairy-tale, each myth that is part of the collection forcing the others into greater articulation and all of them contributing, via this process of competition, to the development of our consciousness.24

(3) (1) and (2) are possible and desirable because they are reconcilable with our humanitarian goals,25 and because reality . . . is more yielding than is assumed by most objectivists. Different forms of life and knowledge are possible because reality permits and even encourages them.26

Hence, what Galileo did was good, it was "an entirely legitimate move;"27 what is bad from the present standpoint of rationalist cultural imperialism and hegemony is that it was too successful! Galileo created a new "local" success story, but not a more inclusive story that contained the previous local success story. Galileo created (or substantially helped create) a new perspective, and he proceeded in the only way possible in such matters: He broke out of the old perspective by violating its rules and standards, and reinterpreting experience by gradually putting together the pieces of a new perspective; and like a portrait artist who wishes to postpone criticism until the entire painting is complete, Galileo needed time to develop the auxiliary moves to support a theory that was clearly
absurd given the old perspective alone. To create new natural interpretations via the creation of a new perspective, Galileo had to break through the hypertextual grip of Aristotelian common sense -- the lock of thought (concepts), description, and experience. Because experience is "fluid" and never presents a brute neutrality, Galileo was able to introduce a "new observation language."

According to Feyerabend,

In order to progress, we must step back from the evidence, reduce the degree of empirical adequacy (the empirical content) of our theories, abandon what we have already achieved, and start afresh.

Although this is all quite dramatic, it is my contention that there is a much more modest and accurate story to be told for what Galileo was doing. Put simply, by the time of Galileo's famous arguments Copernicanism was clearly a robust alternative to Ptolemaic geostasis, having achieved a widely recognized amount of conceptual progress in terms of offering an impressive counter-solution to the core astronomical problem situation of the time, and at least an impressive "match" of the empirical successes of Ptolemaic geostasis. Furthermore, given that criticism and the offering of alternative theories of dynamics for the traditional Aristotelian theory of motion were hardly new, given that particular aspects of Aristotelianism, such as the celestial-terrestrial distinction, were being questioned by supporters and nonsupporters alike, and given that even
nonsupporters of heliostasis were concerned about planetary linkages with the sun, Galileo had a sound rational basis to pursue needed auxiliary patches to Copernicanism and to reinterpret apparent falsifications in the light of finding support for these auxiliary patches.

In other words, in the language of this thesis, all Feyerabend has shown is that by the time of Galileo's arguments there was a robust debate between conflicting hypertextual adjudicatory trails, and that when one analyzes the newly offered auxiliary nodes of the Copernican system we see that given the historical context they "simply were not regarded as decisive, nor were they that decisive. . .".

It is my contention that Feyerabend represents the typical relativist ploy writ large: Show that a theoretical-experiential structure can be analyzed into a hypertextual adjudicatory trail; then show that given any node on that trail further regress is possible and complete closure is impossible. This then is followed by a claim of enlightenment and a great display of post-modernist crowd pleasing rhetoric describing how any idea can be defended no matter how apparently absurd and how experience is simply a matter of power and effort, how successful beliefs are simply a matter of aspiration and ingenuity. As Popper once remarked, relativism "simply exaggerates a difficulty into an impossibility." And we should add, it simply
exaggerates an insight (fallibilism) via a frustrated foundationalism into a completely unconstrained mobocratic ontology and epistemology by muddling "the difference between the rational and the possible."\(^{35}\)

There are many ways this case can be presented, but the simplest in my opinion is to examine the handwaving tactics employed by Feyerabend in replying to critics. For instance, in 1973 Machamer published a detailed rejoinder to Feyerabend's early presentation of his interpretation of the Galileo episode.\(^{36}\) In an Appendix to the first edition of *Against Method* Feyerabend responded,\(^{37}\) but in the second edition he was apparently so confident of the decisiveness of this response that the Appendix is omitted as "material no longer of interest."\(^{38}\) This in spite of the fact that many other philosophers of science cite Machamer's article as a probative challenge to aspects of Feyerabend's arguments.\(^{39}\) It is my contention that this Appendix is a smoking gun for the case against Feyerabend's relativism and how it emerges from frustrated foundationalism.

First Machamer's argument. In general, although Feyerabend is to be commended for his insights into the role of auxiliaries and the historical description of their development in scientific pursuit, his conclusions are "extravagant." His slanted use of quotations from Galileo's *Dialogue* ignores Galileo's discussion of the phases of Venus, sunspots, and the theory of tides -- the latter,
according to Machamer, being Galileo's most important "independent" argument for heliostasis. According to Machamer, a more inclusive historical account shows "that Galileo had all kinds of reasons for supporting the Copernican theory and for rejecting the Ptolemaic theory," that there was independent support for the auxiliaries either used or pursued by Galileo, and that Galileo was not ignoring refutations and arguing circularly by patching together a consistent system of refuted hypotheses.

Specifically, Feyerabend is wrong concerning the relevance of and time-indexed support for the celestial-terrestrial distinction as it applied to the reasonable acceptance of the reliability of telescopic observations. According to Feyerabend, it could not be assumed that the reliability of terrestrial telescopic observations was transferrable to the celestial realm. There were several glaring problematic inconsistencies, noted by Galileo himself, of telescopic celestial observations -- the telescope magnified the moon and planets, but made stars appear smaller, and although it removed considerable irradiation from the planets it did so to a much lesser extent for the stars. But most important, because Galileo had no adequate independent theory of the telescope to appeal to, Galileo's use of the telescope was ad hoc and an example of exploiting the harmony between two questionable theories.
However, according to Machamer, optical considerations (hypotheses, assumptions) had "been applied to both realms since antiquity," and that although Galileo may have been unaware of Kepler's optics of 1604, there is substantial circumstantial evidence that Galileo was aware of enough optical theory to believe in the reliability of the telescope for celestial observations. In other words, even if the celestial-terrestrial distinction still received strong support (it did not), it was irrelevant as shown by the long tradition of making judgments on apparent sizes and magnitudes, transits and observed positions of astronomical bodies, as well as judgments concerning which bodies were self-luminating. That optical considerations played a major role as auxiliaries in these judgments there is no doubt, but according to Machamer,

Since such laws were applied independently of realm considerations, it followed that students of optics could raise no general optical problems based upon the celestial-terrestrial distinction to impugn the reliability of the telescope."

Accordingly, the "intelligent position to hold" was to raise no "theoretical scruple" for application of the telescope to the celestial realm. For those who did raise theoretical objections, the auxiliaries appealed to were either less well supported -- such as that of Horky who invents an optical theory to argue for the illusory nature of the observations of the moons of Jupiter -- or, counted
equally against all astronomical theories and procedures -- such as Sizzi's questioning the reliability of the sense of sight. According to Machamer,

... any argument to show, as Feyerabend ultimately wants to, that the telescope was a cause of illusion would have to apply equally to both celestial and terrestrial realms (sic). Any argument which claimed to show that the telescope by virtue of its reflections and refractions gave rise to illusions would be independent of realm considerations -- a fact which is all the more obvious once one realizes that the reflections and refractions of the telescope will always occur in the terrestrial realm (at the hands of the observer, one might say.)

Thus, given that it was well established that the telescope succeeded in magnifying terrestrial objects, making observed objects appear closer, that a contemporary optical explanation for this existed and was no doubt known by Galileo, and that any epistemological problems with naked eye perception were seen to be optically solvable independently of the celestial-terrestrial distinction -- that there was widespread agreement on the reliability of triangulations, transits, eclipses, etc. in spite of the fallibility of perception -- so it was reasonable to believe that any epistemological problems with the telescope were optically solvable independent of the celestial-terrestrial distinction. In short, the relative reliability of the telescope was discussed and established independently of one's cognitive stance toward Copernicanism. All Feyerabend has done is show that the telescopic observations of the
apparent diameters and magnitudes of Mars and Venus, the phases of Venus, the moons of Jupiter, and mountains on the moon offered significant evidence for Copernicanism only if one accepted a fallible, but reasonable auxiliary.

But what of the celestial-terrestrial distinction itself? Here Machamer shows how Feyerabend "jumps around" historically, ignoring the proper time-indexed placing of the discussion of auxiliaries. Feyerabend invokes for the defenders of the status quo in the early 17th-century a widely accepted hypothesis of the 16th-century and ignores how new observations and developments in the late 16th-century and early 17th-century had undermined this hypothesis. The celestial-terrestrial distinction was clearly in trouble by the time of Galileo’s observations of 1610. The novae of 1572 and 1604, and the comet of 1577, and the authoritative placement (by Tycho) through parallax measurements of these phenomena beyond the sublunar sphere had led even supporters of Aristotle and Ptolemaic geostasis (Tycho and Lydiat) to acknowledge the corruptibility of the heavens and the existence of terrestrial qualities in the sky. Thus, according to Machamer, Galileo was,

... entitled by the arguments from Tycho Brahe et al., to disregard the old and then defunct terrestrial/celestial distinction. This disregard, when added to Galileo’s knowledge of optical theory, is certainly sufficient to establish the falsity and inadequacy of Feyerabend’s claim that there was no possible justification of the reliability of the telescope because of the
terrestrial/celestial distinction, and because Galileo knew no optics.\textsuperscript{47}

Here Machamer rightly turns the importance of appealing to a temporal element used by Feyerabend -- Galileo had no objective reason to be a Copernican because Keplerian optics was not yet known, accepted, or supported -- against Feyerabend's own argument. According to Machamer,

\begin{quote}
It is, in large part, this temporal factor which makes Feyerabend's analysis of Galileo misleading, if not downright false. Feyerabend would have had a better case had he chosen Copernicus' working out of the heliostatic theory; for instance, the reasons for which Copernicus developed his theory in \textit{De Revolutionibus} after its nuclear presentation in the \textit{Commentariolus}.\textsuperscript{48}
\end{quote}

How does Feyerabend respond to this criticism in the first edition of \textit{Against Method}? According to Feyerabend,

\begin{quote}
... when saying that Galileo did not know optics I did not mean to imply that he did not know baby-optics. What I meant was that he was ignorant of those parts of optics which \textit{at the time in question} were \textit{necessary for building the telescope}, assuming the telescope was built as a result of an insight into the basic principles of optics.\textsuperscript{49}
\end{quote}

Thus, Machamer should

\begin{quote}
... realize that the 'laws of refraction and the nature of light' \textit{do not suffice} (my emphasis), that one has to consider the reactions of the eye and of the brain, and these reactions are unknown in the case of refracting media.\textsuperscript{50}
\end{quote}

51
And, if no one but Kepler was raising quality issues related to the paradoxical aspects of telescopic observations,

... this just shows that people didn’t observe very carefully and were therefore ready to accept the new astronomical miracles of Galileo. ... ignorance, or sloppiness, was bliss. 51

In other words, even if more forceful objections were not leveled against the reliability of the telescope, they ought to have been! All Feyerabend is saying is that the historical actors, in the language of this thesis, could have been more critical of this auxiliary node in the adjudicatory trail they were using; they could have explored it more, doubted it more, etc. But this is the typical relativist’s ploy -- show that there is always a potential infinite justificatory regress. Could the telescope have been better understood? Yes. Could the nature of light have been better understood? Yes. However, it does not follow that those who did doubt the reliability of the telescope were more critical, that their objections were based on more plausible and better supported auxiliaries. Could the nature of vision have been better understood? Yes. But this did not stop supporters of both geostasis and heliostasis from reaching agreement on triangulations and planetary positions. According to Feyerabend,

... it is quite true that opticians ignored .. . (difficulties) and boldly triangulated into space. In doing so they showed either gross negligence, or ignorance, or a complete disregard for the demands of consistency. ... Yet they
were successful. Once more ignorance, or superficiality, or muddleheadedness turned out to be bliss.2

For many scientists and philosophers today, quantum discontinuities and the wave-particle nature of electromagnetic phenomena still engender conceptual perplexity upon attention, but this does not stop us from building reliable devices and instruments, such as radio and optical telescopes, and to achieve theoretically independent calibration and consequent intersubjective agreement over the data gathered by the latter with attached devices (cameras, spectroscopes, CCDs). We do not think of modern astronomers as ignorant, superficial, or muddleheaded because they accept particular observations of electromagnetic spectral wavelengths as indicative of the nature and composition of astronomical objects. When the light from a container of hydrogen gas on Earth shows distinctive emission and absorption lines (Balmer, Lyman series) in a spectrograph, and the light from a distant star reveals these same lines, we do not charge astrophysicists with inferential muddleheadedness or theoretical superficiality when they infer that the star contains hydrogen, even though the discontinuous electron transitions within atoms of hydrogen which produce the photon emissions and absorptions are perplexing and still the subject of much philosophical discussion. There is a theoretically independent reason, although ampliative and fallible, for accepting the
inference that stars contain hydrogen. Nor do we charge astrophysicists with blissful ignorance when they use the width banding, splitting, and shifting of spectral lines, massive X-ray bursts, and pulsating radio waves to independently detect and distinguish white dwarfs, normal neutron stars with massive magnetic fields, millisecond pulsars, and black holes, although the auxiliary linkages are more fallible for these inferences than for simple hydrogen detection. It does not follow from the mere existence of foundational questions, and the mere possibility that a particular future resolution of foundational problems in quantum mechanics may undermine a plethora of presently accepted observational technology, that the present use of this technology is irrational or of questionable reliability.

Feyerabend’s best argument (by implication) is that Galileo’s use of the telescope is not like that of hydrogen detection, but is more like neutron star and black hole detection. The telescope was a new instrument, and as with all new instruments there was not yet reliable calibration. Which phenomena were real? Which were simply instrumental artifacts? As with the case of hydrogen detection, a terrestrial telescopic observation could be independently tested by first observing a distant object, recording the details seen, and then observing that same object close up, thereby easily separating the real observational data from
instrumental artifact, such as chromatic aberration in the case of refracting telescopes. On the other hand, as with the cases of black holes and neutron stars -- we cannot create mini versions of these objects on Earth complete with accretion disks and X-ray emissions -- Galileo could not corroborate celestial observations up close and separate with certainty reliable phenomena from instrumental artifact. Hence, according to Feyerabend, one's assumptions concerning the nature of the intervening medium and the behavior of light in that medium must play a major role.

But this only returns us to the status of the celestial-terrestrial distinction, and whether or not supporters of telescopic observations had a reasonable basis for transferring what was learned with terrestrial observations as to the separation of real phenomena from instrumental artifact to the celestial realm. Whether or not the transfer, comparatively, is more reasonable, as reasonable, or less reasonable than the alternative of questioning the validity of the transfer. Feyerabend has only made the case that there was an important issue, that those who did cite the celestial-terrestrial distinction, or who attributed celestial observed phenomena to the possibility of instrumental artifact, were not being stupid or merely dogmatic. He has not made the case that anyone who accepted the transferability was being irrational, acting only on blind faith and accepting a refuted
auxiliary. All Feyerabend has shown (in his own words) is that there was a "fruitful disorderliness," that

. . . 'the Aristotelian distinction' between a celestial realm and a terrestrial realm cannot have 'collapsed completely' by 1577 as Machamer insinuates (p. 21). It collapsed with some, it did not collapse with others, nor did it collapse without a trace.\textsuperscript{54}

It is my contention that "fruitful disorderliness" should be read as "robust debate" between different adjudicatory trials, and that recognizing that a new auxiliary is fallible implies neither that the core hypothesis (helio-stasis) has no original independent support nor that the supporting auxiliary has no independent support. It is significant in this regard that with the exception of citing Kepler's theoretical misgivings about the wisdom of constructing a telescope, Feyerabend does not reply to Machamer by describing in his Appendix what the historical players actually said against Galileo, but rather what they ought to have said. According to Feyerabend, Galileo's "contemporaries with very few exceptions overlooked fundamental difficulties that existed at the time," that it was the "thoughtlessness of his contemporaries which enabled Galileo to get ahead as well as he did."\textsuperscript{55}

And what of Kepler? Standard medieval cosmology hypothesized a series of substances from air to fire to aether as one moved "up" from Earth, through the sublunar
realm and into the celestial realm. According to Feyerabend, "nobody paid attention" to this, they "forgot it," and "nobody seemed to raise the problem of the refractions arising therefrom." Within this context, Feyerabend then cites one of Kepler's theoretical reasons for not constructing a telescope, i.e., that not only is the air dense and blue in color and gets bluer as it gets thicker "as it extends between a visible object and the eye," the celestial essence itself must "have its own proportion of density," gradually becoming more and more tenuous as one moved into and through the celestial sphere. Hence, thought Kepler, any magnification would only enhance the obscuring and distorting effects of these dense mediums. Feyerabend then cites Kepler's praise of Galileo for boldly ignoring these problems and experimenting with the telescope anyway.

This reference to Kepler's comments on Galileo's use of the telescope is sufficiently important to my points against Feyerabend that it warrants quoting the relevant passage from Kepler's *Conversation with Galileo's Sidereal Messenger* in full. According to Kepler,

\[\ldots\ I \text{ believed that the air is dense and blue in color, so that the minute parts of visible things at a distance are obscured and distorted. Since this proposition is intrinsically certain, it was vain, I understood, to hope that a lens would remove this substance of the intervening air from visible things. Also with regard to the celestial essence, I surmised some such property as could prevent us, supposing that we enormously} \]
magnified the body of the moon to immense proportions, from being able to differentiate its tiny particles in their purity from the lowest celestial matter.

For these reasons, reinforced by other obstacles besides, I refrained from attempting to construct the device.

But, now, most accomplished Galileo, you deserve my praise for your tireless energy. Putting aside all misgivings, you turned directly to visual experimentation. And indeed by your discoveries you caused the sun of truth to rise, you routed all the ghosts of perplexity together with their mother, the night, and by your achievement you showed what could be done.

Under your guidance I recognize that the celestial substance is incredibly tenuous. . . . If the relative densities of air and water are compared with the relative densities of the aether and air, the latter ratio undoubtedly shows a much greater disparity. As a result, not even the tiniest particle of the sphere of the stars (still less of the body of the moon, which is the lowest of the heavenly bodies) escapes our eyes, when they are aided by your instrument. A single fragment of the lens interposes much more matter (or opacity) between the eye and the object viewed than does the entire vast region of the aether. For a slight indistinctness arises from the lens, but from the aether none at all. Hence we must virtually concede, it seems, that the whole immense space is a vacuum (emphasis added).59

There are two points that should be obvious from this passage:

(1) Kepler has changed his mind! He now realizes that the "celestial substance is incredibly tenuous," and that this would have to be so or we would not be able to see anything with the telescope.

Later, Kepler was to add that his previous misgivings about densities must be wrong or we would not be capable of
even naked-eye observations of the stars. According to Kepler,

The distance between us and the fixed stars cannot be estimated. Yet the intervening aethereal substance, which is so extensive, transmits right down to us the light of the tiniest stars undiminished and with a differentiation of colors. This could not happen if the aether had a minimum either of density or of color. . . . Therefore if physics permitted, an astronomer could assume that the entire space of the aether is an absolute vacuum.⁶⁰

(2) Kepler's problem stems in part from his previous and still prevalent confusion of cosmologies! A confusion that Feyerabend repeats.

Notice that Kepler's references to the moon and "the lowest celestial matter" and "the lowest of the heavenly bodies" are equivocal. They can mean either a reference to geocentric observations, i.e., we make all our astronomical observations from Earth, so the moon is the lowest (closest) body, or a reference to medieval cosmology which assumed a transmigration through different substances in the direction of "up." The context of Kepler's use of the phrase "the lowest celestial matter" clearly indicates that he has inadvertently assumed that the proportion of celestial matter near the moon is denser because of its location as "the lowest of the heavenly bodies." In short, Kepler's previous theoretical misgivings stem in part from his unconsciously lapsing back into an Aristotelian cosmology -- the very cosmology in trouble and one that ought not to be
assumed any longer, especially by an early 17th-century follower of Copernicanism. 61

In other words, two can play Feyerabend's game. "Nobody" paid (read "many did not pay" contra Feyerabend's usual exaggeration) attention to the potential problem of light passing from a celestial realm to a sublunar realm to an earthly realm, because the foundational cosmology for this "problem" was in serious trouble. According to Feyerabend, one ought to have paid attention to it, and he praises and criticizes Kepler for being rational and having "clear" theoretical reasons for not constructing the telescope. Contra Feyerabend, a consistent Copernican in the process of developing a new dynamics, comparing alternative auxiliaries and questioning the noncorruptibility of celestial objects given new phenomena (novae, comets, sunspots) "ought not" to have paid attention to it, or at least not worried about it to the point of allowing it to be an inhibiting factor in experimenting with the telescope. Why should a Copernican be obligated to accept an Aristotelian auxiliary when that auxiliary is losing support?

Feyerabend is correct to argue that the transitional nature of the recognition of inferential relationships between core hypotheses, auxiliaries, and experience is a

* For Feyerabend any praise for being rational is simultaneous criticism for not being boldly irrational.
messy process, that new adjudicatory trails are not born all at once and that auxiliaries are often recognized and developed "out of phase" with the core hypothesis. However, unlike Kepler in this instance, a case can be made that Galileo was most aware of all the inferential relationships involved in supporting Copernicanism. His life-long dogged pursuit of a new dynamics and consequent ignoring of potential Aristotelian problems with the telescope are testimony to this. As Galileo himself put it in his Dialogue, given the "many and grave difficulties" in Aristotelian foundations, "it is reasonable to doubt everything else that is built upon them . . . . (and) it would not be amiss to see whether (as I believe) we may, by taking another path, discover a more direct and certain road. . . ." Notice that by the time of the Epitome Kepler has become more consistent. In the quote above, we now see him appealing to a Copernican auxiliary -- the vast distance to the stars -- in helping understand and solve what was previously a theoretical problem for celestial telescopic observation.

What now of Feyerabend's use of the tower experiment? Let's make short work of this, because I believe the same point can easily be made, using the terminology of this thesis, that Galileo is not ignoring refutations but comparing and pursuing alternative auxiliaries and adjudicatory trails.
If the Earth rotates eastward at an appreciable rate of velocity and Aristotelian dynamics is assumed, then a stone dropped from a tower should not hit the ground at the base of the tower, but a measurable distance to the west of the base of the tower. Thus, according to Feyerabend, to save Copernicanism from falsification Galileo introduces the *ad hoc* hypothesis of circular inertia. However, much of Galileo's *Dialogue* involves showing that certain experiences are refutations of heliostasis only if particular auxiliaries are accepted as non-problematic. The tower experiment refutes Copernicanism only if one accepts the adjudicatory trail of heliostasis (H) + Aristotelian dynamics (A₁) + tower experiment (E₁), and provided that A₁ is non-problematic. But by the time of the *Dialogue* there were well-worked out alternatives to A₁,⁶⁴ and Galileo had been working on an alternative for decades.

The implication of Feyerabend's charge of Galilean *ad hocness* is not only that Galileo's pursuit of a new dynamics is for "this specific purpose," but also that it is conjured up without any independent support or reason for its introduction. Although from a purely formal perspective pursuit of a new dynamics can be seen as an auxiliary save for heliostasis, an examination of the historical context shows that (1) a new dynamics was not just being conjured up on the spot, so to speak, (2) the Copernican core hypothesis had achieved a sufficient amount of support to warrant
pursuit of such an auxiliary save, and (3) in Galileo’s particular situation, a persuasive case can be made that dynamical concerns caused him to be a Copernican, rather than blind acceptance of Copernicanism first, then recognition of falsifications, then frantic appeal to after-the-fact auxiliary saves that had no independent support or scientific rational for their introduction.

Concerning this last point, Drake has argued that a more accurate arranging and dating of Galileo’s pre-Paduan manuscripts will help show "as new that Galileo moved step by step to full Copernicanism, rather than some single insight and a search for proofs, or because of some metaphysical conviction that entailed motions of the Earth in a preconceived scheme of the universe." According to Drake, between the years 1584-1590 Galileo accepted the Copernican arrangement as a purely mathematical device for saving the phenomena, a typical cognitive stance for many astronomers during the latter decades of the 16th-century. Physical considerations -- the same traditional physical arguments used to argue against the Earth’s movement -- kept Galileo from going any further at this time. Then by 1591 Galileo’s concern with the possibility of a perpetual spherical motion that was neither natural nor forced in the Aristotelian sense, plus the general understanding of the purely mathematical benefits of the Copernican system, led Galileo to become a semi-Copernican. In other words, although
Galileo adhered at that time to the conventional instrumentalist notion that to avoid conflict with physics various astronomical devices used to save astronomical phenomena should be treated as mathematical fictions, and hence did not accept the revolution of the Earth as real, Galileo was led to accept the daily rotation of the Earth as real based on his proposal of the inevitable rotation of a heavy sphere centered at the center of the universe. This rotation, Galileo proposed, was a perpetual motion that was neither natural nor forced in an Aristotelian sense. Thus, given that the most notable development in astronomy around 1590 was to explore geostatic transforms of Copernican heliocentric planetary motions, given that the mathematical advantages of centering planetary motion on the sun were widely discussed, Galileo "became a semi-Copernican as followers of the Ursine system came to be called."\(^6\) In short, Galileo became a supporter of geoheliocentrism. Finally, Galileo became a full Copernican after experiencing impressive Venician tides in 1594, believing by 1595 that an epicyclic motion (requiring the Earth's revolution as well as rotation) of sea-basins best explained tidal phenomena. This was followed by Galileo's famous letter of 1597 to Kepler announcing publicly for the first time that he was a follower of Copernicus.

Whether or not Drake's speculations are accurate, he is surely correct about the chronological focus of Galileo's
thought in terms of his preoccupation and development of physical ideas. Galileo began his professional career writing folios on natural philosophy and local motion, and he ended his career dictating (because of blindness) additions to his Two New Sciences, a systematic presentation of his physics. From this perspective, it is understandable also why Galileo begins his famous Dialogue with a discussion of dynamics and concludes it with a climatic theory of tides. Given this history, Galileo’s work in dynamics was not a panicked patch of Copernicanism, but rather the rational pursuit of what would become a crucial auxiliary for Copernicanism.

According to Feyerabend, in characteristic overstatement,

Wherever we look, whatever examples we consider, we see that the principles of critical rationalism (take falsifications seriously; . . . avoid ad hoc hypotheses. . . .), . . . give an inadequate account of the past development of science and are liable to hinder science in the future . . . These "deviations"; these "errors" are preconditions of progress. They permit us to remain free and happy agents.67

It is clear from Feyerabend’s analysis of the Galileo episode that ad hoc for him means any auxiliary save of a core hypothesis that is "out of phase" (developed at a different time) with the core hypothesis. And in typical relativist’s fashion we are asked to see that "anything goes" once we recognize that any hypothesis can be patched
given sufficient boldness, creativity, and financial backing. According to Feyerabend, progress results from the anti-methodological realization that reality will "yield" to different adjudicatory trails, that we can create different perspectives by, to use Rortian language, "recontextualizing" our adjudicatory trails to our hearts' content, or to use Quinean language, by creating different adjudicatory webs "come what may." Rather than avoid out of phase auxiliary saves, they should be encouraged.

The purpose of this chapter has been to show how relativists like Feyerabend take a difficulty and exaggerate it (recall Popper's comment above), to show how the insight that core hypotheses are only part of a complex hypertextual adjudicatory trail is epistemologically abused. It should be clear that Galileo's work did not amount to an unconstrained, leap-of-faith commitment to a core hypothesis and then a life-long creative patching of that initial commitment. By contrast with the historical situation, I suggest that the negative normative message of Galileo's work is:

Avoid auxiliary saves that have no independent support, provided that the core hypothesis also does not have independent support and is not at least a robust alternative to another core hypothesis. 68

And consistent with the historical situation, the positive normative message of Galileo's work is:

Pursue evidence for an auxiliary save (a new dynamics) if there is weakening support for its alternative.
(Aristotelian dynamics) and your core hypothesis (heliostasis) has a high rate of progress and is at least a robust alternative to another core hypothesis (geostasis).

Feyerabend is correct in bringing to our attention that a great deal of scientific activity involves filling in holes, scratching for answers, and solving problems created by the clash of recalcitrant experience with cherished hypotheses. But unless one accepts the most naive version of falsificationism, he is wrong that anyone should see this activity as always irrational.

In this vein, Putnam has commented, "Philosophers of science frequently write as if it is clear, given a set of statements, just what consequences those statements do and do not have." In other words, an over reliance on purely formal considerations misleads us into believing that much of science involves the schema: hypothesis + (ready made) auxiliaries + (clear) prediction; whereas much more widespread in actual science is the schema: hypothesis + ???? + phenomena to be explained. According to Putnam,

Failures do not falsify a theory, because the failure is not a false prediction from a theory together with known and trusted facts, but a failure to find something . . . a failure to find an AS (auxiliary)."

Feyerabend attempts to make his case against methodology by assuming that the necessary auxiliaries were, in Putnam's language, "known and trusted." Clearly this was
not the case by the early 17th century. Feyerabend is correct to draw our attention to the comparative nature of the auxiliary debate, but does not properly time-index the adjudicatory trails. Accordingly, he misses the inferential sagacity, consistency (compare with Kepler noted above), and rationality of Galileo’s pursuit of a new dynamics and bold experimentation with the telescope.

Feyerabend would have had a much better case, even though this would also fail, if he had concentrated on the move early supporters of Copernicanism (mid-16th century) had to make in terms of what heliostasis implied concerning the distance to the stars. Given the lack of a detectible parallax for the stars, supporters of heliostasis had to support the apparently preposterous patch that the stars were much further away than anyone previously believed and that the solar system was but a mere central point surrounded by a vast celestial realm. There was little independent reason at this time for believing that the noncorruptible celestial essence would extend so far beyond the cozy domain of the Earth, sun, and planets. Support for this move was decidedly weak at this time. The considerations that kept it from being totally absurd were the relative uncertainty of the accuracy of parallax measurements in general (there were parallax problems related to the planets for both geostasis and heliostasis) and, as will be argued in chapter 5 below, the success of the core
problem solving ability of heliostasis (no equant point, elegant, parameter fixating solution to retrograde motions and their frequencies, etc.). No one would have or should have taken this move seriously, if Tycho's authoritative work on parallax had occurred in the mid-16th century, if geostasis had fewer problems than it did, and heliostasis was not at least a robust alternative to geostasis regarding the core problem situation.

By way of contrast, consider the current mega-problem of the mystery of missing neutrinos for the standard model of the sun's operation and internal structure. We are now approaching 30 years since Raymond Davis designed and had constructed a large neutrino detector using 100,000 gallons of perchloroethylene cleaning fluid placed in a deep mine in South Dakota. Other detectors have also been built (Kamioikande, Japan; Irvine-Michigan-Brookhaven, Lake Erie), varying the detection substance and methodology (large tanks of water, Cerenkov radiation, and photomultipliers), and the results are still basically the same -- only one-third of the neutrinos predicted by the standard model are detected. Few physicists now believe that the detection methodologies are at fault. Given that the standard model has enormous independent support, not the least of which involves most of our ideas on nuclear fusion, and given that there is no robust alternative to the standard model, the scientific focus has been on exotic (some would say outlandish)
auxiliary saves, i.e., neutrinos with mass, different types of neutrinos, and even neutrino oscillation. Surely, given a robust alternative to the standard model, massive skepticism would meet the auxiliary patch that the right amount of neutrinos are produced by the sun's interior, but a portion somehow changes (oscillates) to a nondetectible variety just before they get to our detectors. But given the lack of a robust alternative it is not unreasonable for physicists to pursue the finding of support for this patch.

It is worth noting in passing here that although I have argued that the neutrino patch is disanalogous to the Galileo episode due to the existence of a robust alternative for Galileo, there is the similarity of having theoretically independent means of calibrating the reliability of the instrumentation. In the neutrino case most scientists have decided after achieving independent results that the neutrino detection equipment is not at fault. In the case of the telescope, many could conclude that the telescope was reliable because it gave results that supported both helio-stasis and geostasis, i.e., it gave results that solved problems for both world systems!

Chalmers has pointed out that prior to the telescope, naked eye observations of the apparent diameters of Mars and Venus were inconsistent with what is predicted by both the Ptolemaic and Copernican theories. The geometric arrangements in both of these systems predict that the apparent
diameter of these planets should vary by several detectable factors. There was widespread agreement that these variations were not observed, and Osiander in his famous preface to Copernicus's *De Revolutionibus* uses this clash between experience and both world systems as support for his claim that these systems should only be accepted as useful mathematical devices for saving the phenomena. However, when these planets are viewed through the telescope the difficulties are removed for both world systems -- the observed changes in size are consistent with that predicted by both systems.

According to Chalmers, this supplied an independent reason in favor of the reliability of telescopic data, and put one "in a stronger position to appeal to other telescopic evidence, such as observation of the phases of Venus as genuine support for Copernicus." Furthermore, even though the latter still does not distinguish the Copernican theory from that of Tycho, this independent support for the reliability of the telescope puts one in a stronger position to use the observations of mountains on the moon, sunspots, and the moons of Jupiter against any version of geostasis that maintained traditional Aristotelian auxiliaries.

To conclude this chapter, it is time to address my claim that if we separate Feyerabend's rhetorical
exaggerations and frustrated foundationalism from his insights, we will find a "regular (post-foundationalist) guy methodology-wise" and an advocate of what I call normative relativism. I define normative relativism as the view held by Mill, a view that Feyerabend cites often approvingly:

Variety of opinion is necessary for objective knowledge. And a method that encourages variety is also the only method that is compatible with a humanitarian outlook (Feyerabend's emphasis).  

... the only way of arriving at a useful judgment of what is supposed to be the truth, or the correct procedure is to become acquainted with the widest possible range of alternatives. The reasons were explained by Mill in his immortal essay On Liberty. It is not possible to improve upon his arguments (my emphasis).  

With this in mind, one does not need to read much of Feyerabend to see that in terms of our ultimate values anything ought not to go. According to Margherita Von Brentano, Feyerabend's work

is a vehement plea on behalf of layman and mature, emancipated citizens, in the name of all those who are hindered in their attempts to determine what their own welfare is and settle their own affairs accordingly (emphasis added). It clearly opposes all oppressive forms of guardianship, expertocracy, the monopoly of bureaucrats in decision-making and their treating of suffering as an object of administration.  

According to Feyerabend,

It seems to me that happiness and the full development of an individual human being is now, as ever, the highest possible value. ... Adopting this basic value we want a methodology and a set of institutions which enable us to lose
as little as possible of what we are capable of doing and which force us as little as possible to deviate from our natural inclinations. . . .

(and) that a scientific education as described (by naive critical rationalism) . . . cannot be reconciled with a humanitarian attitude. 77

Despite his denials that he is advocating any particular position himself, despite his claim that he has no philosophy, is really only telling stories, 78 and only "manipulating" rationalists, 79 Feyerabend is clearly committed to good old fashioned liberal self-actualization theory. We should have faith in people in the long run for knowing what to do for themselves; we should encourage freedom, not only because it encourages responsibility, but because it forces the actualization of individual potential, which in turn encourages the development of "maturity" and "a higher stage of consciousness," and benefits the totality of human existence. 80

Nowhere does Feyerabend mention or deal with the problem that Mill and Bohr (who he also cites often) were not relativists. Indirectly he deals with this tension by claiming that he only cites the heroes of rationalism to show how wide of the mark are the methodologies of critical rationalism in terms of matching actual practice. He is not endorsing any particular practice, as part of a particular tradition, as better than any other. He is not endorsing "democratic relativism" (read my normative relativism) as
better than totalitarianism. 81 Like Von Brentano's anti-liberal liberalism 82 and Rorty's anti-anti-ethnocentrism, 83 Feyerabend undoubtedly thinks that this is just being macho consistent or, shall we say, politically correct, anti-androcentric sensitive. "Well yes, I hold certain values as ultimate, but I won't pretend that they are the best and attempt to impose them on you; they are just my values, part of my tradition."

There are also hints in Feyerabend's writings that there is a typically postmodern reason for his philosophical sidestepping of any position that smacks of a traditional commitment. As noted above, in Feyerabend's writings can be found references to "a higher stage of knowledge and consciousness" and "maturity," and these are contrasted with local success stories and "special forms of knowledge." 84 Vine Deloria in commenting on this speculates that Feyerabend "shows every indication that he (was) moving toward a major breakthrough in his thinking." 85 The fully self-actualized individual, as well as the enlightened culture, moves from information, to knowledge, to wisdom. When information reaches a critical mass we develop codification and organization schemes of special knowledge. In the West we have called this science. Eventually we learn that we are not objective observers, but participant-observers, and we learn not to take our schemes of organization too seriously, that they are only phenomenal
screens created by our own concepts. The role of a variety of workable conceptual schemes is crucial here for forcing upon us the realization that no conceptual scheme "re-presents" the structure of reality. According to Deloria, the illusion of Western science is that it "prematurely derives its scientific 'laws' and assumes that the products of its own mind are inherent in the structure of the universe." So the truly enlightened can not hold any real allegiances, not even to that of liberal democracy and the pursuit of better ideas through rational debate and exchange of well-thought out and articulated opinion, for this is beneath one who has obtained such a breakthrough.

I would like to suggest a far simpler explanation. What better way to display your frustrated foundationalism than to decry real allegiance to any position. If you are only telling a story and have no philosophy yourself, then there is nothing to doubt and no target to criticize. All criticism is easily deflected by claiming that you were misunderstood by idiots who failed to see that you were just manipulating the beliefs of others. But such a subterfuge will not do. Agassi is on the mark in recognizing that a large part of Feyerabend's motivation was that he was reacting cowardly to the realization that "the strongest (positions) cannot be defended well enough to bring conviction and satisfaction." In a post-foundationalist era we ought to be able to do better than to allow such
fear, trembling, and timidity result from the recognition that our core beliefs and their auxiliary support can never be regarded as decisive.

Upon further reflection it should be obvious that this epistemological timidity is a strange position to take for someone who supposedly is advocating responsibility and that we should have more faith in people, especially more faith in scientists to do good science without the philosopher butting in attempting to "regulate knowledge from afar." The lesson of the Galileo episode is not that anything goes, but that we can have faith in scientists to take seriously issues of change and transition, to make auxiliary moves for the most part for good reasons, that we can in turn reflect upon what those good reasons were and possibly learn from these moves some valuable guidelines for the future. In the chapter 4 I will argue that Feyerabend had less faith in people than Kuhn because he feared potential dogmatism more, that normal science is inevitably hegemonic in the conceptual prisons it imposes on helpless individuals. Whereas Kuhn has less faith in our ability to completely close out nature for long and more faith, not only in nature for "seeping in" and rejecting eventually everything we propose, but also in people for recognizing this.

There well may be some mystical divide between a realm of theories and concepts and their constituting objects on the one hand, and a pure experience uncontaminated by
phenomenal screens on the other. But the history of humanity and science in particular shows that it is on this side of the divide -- the side with conflicting opinions as to how best to organize experience and solve problems, the side where we must make critical decisions concerning species depletion, ozone destruction, and global warming -- that deservedly hold our attention. Let us hope Deloria was wrong about Feyerabend's possible breakthrough, that his next book would not have been a recording of conversations with Krishamurti or some other mystic, and that Feyerabend would have realized that he had never left the larger project of critical rationalism, that Mill's arguments regarding the free exchange of opinion to achieve better ideas indeed cannot be improved upon, that with experience we learn, with learning we reflect upon what works and generalize some reliable guides to the future. Let us hope that Feyerabend would have returned to the mundane side of the divide and realized that his goal had always been "not to eliminate methodologies, but merely (to) reform them," to recognize that although experience is always too rich to allow for overruling, algorithmic approaches to science and that our guides are best seen as fallible "rules of thumb," this in no way threatens a robust but humble, post-foundationalist epistemological project.

Feyerabend did not make the case that reality will yield to any conceptual ingenuity and methodological effort.
Starting with an insight on the theory-ladenness of observations and borrowing from Duhem the holistic nature of tests, Feyerabend tried to make much of what Popper called the "myth of the framework." Feyerabend attempted to retrospectively test the relativist’s ingenuity-and-effort thesis with an historical analysis of the Galileo episode. The failure of this test provides an ampliative basis for believing that a prospective test would fail as well.

This is no small matter and it dramatically underscores what is at stake in these debates. For it seems to be insufficiently recognized that the relativist’s thesis of the ontological efficacy of effort and ingenuity is prospectively testable. Consider a future society -- say in the beginning decades of the next century -- that has been deeply influenced by the present popularity of postmodern writings. Due to this influence they decide to "make" cold fusion work. They decide to invest billions of dollars -- tenure track positions at major universities, grants, corporate subsidies -- into forcing reality to yield to this concept.

Has history given us any ampliative basis for believing in the rationality of such pursuit? It is the argument of this thesis, continued in the remaining chapters, that a much more humble story is in order, that our epistemic fallibility insures against our being able to create monolithic conceptual packages that remain invulnerable to all
that nature throws at us; that we are not as powerful as the relativist would have us believe. The process of auxiliary adjustment, rejection and pursuit is messy. Human beings are capable of great feats but like our evolution there is always at least a touch of bizarre happenstance, meandering folly, and unplanned contingency that cannot be (and should not be) eliminated by any rational reconstruction. In other words, scientists are neither infinitely smart nor stupid. On the one hand, they cannot create, and continue indefinitely to patch, all-powerful frameworks that are impervious to nature's surprises. On the other hand, scientists are not as dogmatically dumb as the relativists would have us believe. They are capable of recognizing and modifying assumptions, or creating and pursuing new ones in the face of the relentless encroachment of experience at the peripheries of our frameworks.
Notes for Chapter 1:

4. Ibid., p. viii.
5. Ibid., p. 35, n 3.
6. Feyerabend, 1975, p. 112. This is Feyerabend's mocking description of Machamer's interpretation of Galileo. See discussion below.
9. Feyerabend, 1975, p. 114. In this chapter I will be citing both the 1975 and 1988 editions of Feyerabend's Against Method. As explained below, this passage is from an Appendix that Feyerabend eliminated in his 1988 edition.
11. Followers such as Vine Deloria (1991, p. 400) claim that Feyerabend has
   "... demonstrated that scientific discovery is a process of propaganda, faith, clever phrasing, and sleight of hand in order to get others to see from a new perspective. . . ." (emphasis added)
13. See notes 3 and 19, Introduction.
15. Von Brentano (Free University, Berlin), 1991, p. 199. Answering Brentano's question would go a long way towards explaining the diverse reactions one can find to Feyerabend's philosophy. Consider this sample.
   [Feyerabend is] "a philosopher who keeps coming back to haunt us with his challenge that science (and) methodology . . . are . . . bright meadows that will turn into swamps, . . . hopes that will become utter disappointments." Robert E. Butts and Joseph C. Pitt, 1978, p. x.
[Feyerabend offers us] "a sloppy pluralism" (whereby we are shown that) "If the strongest (positions) cannot be defended well enough to bring conviction and satisfaction, at least the weakest can be defended by some arguments that are not that bad, and with time we can learn to live with ever poorer arguments, provided we supplement them from time to time with really brilliant ideas to show that we say these silly things not out of stupidity." Joseph Agassi (University of Tel Aviv), 1991, pp. 380, 385.

[That Feyerabend] "is one of the most exciting philosophers of science of this century (is) an opinion that is becoming general around the world as . . . history begins to cast its appraising eye upon the intellectual harvest of our era." Gonzalo Munevar, 1991, p. ix.

[Feyerabend is a] "threat . . . because he asks penetrating and embarrassing questions in fields which most people feel have been laid to rest . . . . (and his work) will prove critical in opening enough breaches in the walls of Western intellectual chauvinism so that some exchange of ideas can occur. . . . What we are discussing when we look at Feyerabend’s philosophy is the re-emergence in western (sic) philosophy of a rare form of honesty." Vine Deloria (Center for Ethnicity and Race, University of Colorado), 1991, pp. 390, 392, 400.

17. Feyerabend, 1975, p. 112.
18. Ibid., pp. 78-81.
19. Ibid., pp. 117, 118.
23. 1975, p. 92.

"Feyerabend is one of the few voices which sees that the body of human knowledge is not merely an instance of adding insights on non-Western peoples to the already constructed edifice of Western knowledge but that the full content of human knowledge must be a discontinuous arrangement of smaller bodies of knowledge derived from the many human traditions"
represented in planetary history." (1991, pp. 390-391)

And in terms of the connection of proliferation and diversity with evolution:

"The interplay between proliferation and tenacity also amounts to the continuation, on a new level, of the biological development of the species and it may even increase the tendency for useful biological mutations. It may be the only possible means of preventing our species from stagnation." (Feyerabend, 1981, p. 144.)

25. 1988, p. 12. Also, according to Feyerabend,

"My main motive in writing the book was humanitarian, not intellectual." Ibid., p. 3.

"It seems to me that the happiness and the full development of an individual human being is now, as ever, the highest possible value." 1985, p. 143.


27. 1975, p. 79; 1988, p. 65. According to Feyerabend,

"Galileo's 'trickery' . . . is 'trickery' only for philosophies that set narrow conditions on conceptual change. . ." (1988, p. 79, n. 17.)

28. According to Feyerabend, in general

"Basic theories and auxiliary subjects are often 'out of phase'. As a result we obtain refuting instances which do not indicate that a new theory is doomed to failure, but only that it does not fit in at present with the rest of science. This being the case scientists must develop methods which permit them to retain their theories in the face of plain and unambiguous refuting facts, even if testable explanations for the clash are not immediately forthcoming." 1985, p. 138.

29. 1988, p. 75.

30. 1975, p. 79.


32. Which, of course, is the reason Galileo wrote his Dialogue.

33. 1978b, p. 45.

35. Laudan, 1990b, p. 83.


37. 1975, pp. 112-119.


40. According to Machamer, although incorrect,

"at the time of its inception and development . . . it pro-
vided the only known natural, mechanical attempt to account
for the tides and it did so in a mathematical way . . . .
Nowhere in his argument for the plausibility of the earth’s
motion did Galileo presuppose the Copernican thesis." (1973,
pp. 9-10.

41. Ibid., p. 35.
42. Ibid., p. 17.
43. Ibid., pp. 16-20.
44. Ibid., p. 18.
45. Ibid., p. 18, notes 59 and 60.
46. Ibid., p. 20.
47. Ibid., pp. 22-23.
48. Ibid., p. 34.
49. Feyerabend, 1975, p. 115.
50. Ibid., p. 116.
51. Ibid., p. 117.
52. Ibid., p. 118.
53. We can, however, conduct independent terrestrial tests that
link spectral line broadening and shifting with density and
motion, and connect X-ray emissions with temperature and radio
waves with spinning magnetic fields. Furthermore, if an ob-
ject such as a neutron star is part of a binary system, inde-
pendent means exist for its identification.
54. 1975, p. 118.
55. Ibid., pp. 112, 117.
56. Ibid., p. 118.
57. Ibid., pp. 18, 83, notes 131 & 132.
60. Kepler, 1618.
61. Kepler's lapse is also pointed out by Rosen in his commentary on *Kepler's Conversation*, 1965, pp. 84-85, notes 132 & 139.
62. An exception, of course, is his life-long adherence to circular motion, an *idée fixe* for Galileo. This will be discussed further in the next chapter.
63. Galileo Galilei, 1632, pp. 18-19.
64. Andersson, 1991, p. 289-290; Kuhn, 1957, p. 120; Shapere, 1974, Chapters 3 & 4.
66. Ibid., p. 97. Geoheliocentrism will be discussed extensively in chapter 5. For now, note that according to Drake,

"How Galileo hit upon the idea of daily rotation of the earth is so evident from careful study of his chapter (in *De motu*) on rotations that I should say it was an inescapable conclusion for him, as was the logical superiority of the related world system (geoheliocentrism) over all rivals -- even the system of Copernicus, as Galileo saw things in mid-1591." (p. 98)

According to Drake, Galileo's promised commentaries on Ptolemy's *Almagest* would have involved a presentation of the superiority of geoheliocentrism. But learning belatedly and embarrassingly of Tycho's system and the Tycho-Ursus controversy prevented the commentaries from ever being published.
67. 1975, p. 179.
68. This rule would separate Galileo's move and the attempt to save the standard model of the sun discussed below from the following. Suppose some supporters of UFO visitation claim that they can obtain pictorial evidence of visitation. Upon obtaining these pictures intersubjective agreement exists that
these pictures are blurry, and skeptics are decidedly unimpressed. However, supporters of visitation argue that this is positive evidence for their theory, because UFOs always emit interfering radiation when someone attempts to take a picture of their space craft. In this case, the visitation hypothesis is not a robust alternative to our best current scientific theories, nor does the radiation hypothesis have any independent support.

70. Ibid.
71. Although this will be developed more thoroughly below, it is worth noting at this point that Galileo's appeal to the "ease and simplicity" of the Copernican system was not just an appeal to a metaphysical aesthetics, but concerned the "knotting" together of several apparent anomalies, i.e., not just retrogressions, but frequency of retrogressions for each planet tied to distances from the sun within the Copernican system. Dialogue, pp. 344-345.

As is the case with many other Copernican episode commentators emerging from the logical positivist tradition and its non-cognitive interpretation of values, Feyerabend misses the parameter fixating significance as one aspect of Copernican appeal. According to Feyerabend, the "motive for change . . . comes from the 'typically metaphysical urge' for unity of understanding and conceptual presentation. 1988, p. 74. This will be discussed further in chapter 5.

73. 1988, p. 32.
77. 1988, p. 12.
79. 1978, p. 143.

82. 1991, pp. 199-212.


86. Ibid., p. 399.

87. See note 15.


89. "It is difficult, and perhaps entirely impossible, to combat the effects of brainwashing by argument." 1981, p. 150.

90. I will also argue that the major problem with Kuhn is just this: He outlines only a faith in rational transitions and tells no detailed epistemological story for these transitions.

91. I am here comparing the direction Deloria thinks Feyerabend is taking with that of the physicist David Bohm, who after recognizing a superficial similarity between Eastern mysticism and the results of quantum physics published two books of conversations with Jiddu Krishamurti, Truth and Actuality, 1980, and The Ending of Time, 1985.


93. Ibid.
In the preceding analysis of Feyerabend’s claims and the application of the notion of intelligent inference along hypertextual adjudicatory trails of reasoning, there is no intention to endorse an historical chauvinism. Although the notion of a hypertextual adjudicatory trail is meant to preserve the logical-historical approach to epistemology against the psychological-sociological, no slide is intended that sees individual scientists as always algorithmically calculating the cost-benefit analysis of endorsing a particular node along an adjudicatory trail, such that the decisions they made appear inevitable given our present conception of successful theories. As was noted in the previous chapter, in the work of Galileo and Kepler inferential sagacity should not be overlooked as an important element in theory competition. It is seldom immediately clear which auxiliaries are consistent, and hence needed, with a core hypothesis. Although Kepler, unlike Galileo, became aware that allegiance to circular uniform motion must be given up to be a consistent Copernican, also unlike Galileo, he was initially inhibited from endorsing the validity of celestial telescopic observations because of a residual allegiance to aspects of Aristotelian cosmology.

As Stanley Jaki has noted, "History is the great equalizer. Sooner or later it cuts all things and all men down to
their true size." The notion endorsed in this thesis of hypertextual adjudicatory trails is not meant to deny that refined historical analyses of the Copernican episode have revealed a messy, vague, "circuitous, mazelike" road full of "blind alleys" and "wrong paths." As Koyré has noted, the history of scientific thought should not be treated as "a catalogue of errors or achievements, but as the entrancing, instructive history of the efforts of the human mind." The rational scaffolding I have proposed, however, is intended to avoid such extreme psychological conclusions as that of Koestler,

The history of cosmic theories. . . may without exaggeration be called a history of collective obsessions and controlled schizophrenias; and the manner in which some of the important individual discoveries were arrived at reminds one more of a sleepwalker's performance than an electronic brain's.

Koestler's stance has, of course, much in common with Feyerabend's anything-goes relativism. The initial review of Koestler's book by Santillana and Drake was equally extreme, prompting further shrill responses in defense of Koestler. According to Santillana and Drake, Koestler depicts the founding fathers of the Copernican revolution as "antisocial schemers, cowards, liars, hypocrites, irresponsible cranks or contemptuous snobs." In portraying the revolution this way, they claim Koestler has the "unique inability to understand what Galileo wrote" and "the
distinction of being the first writer to misunderstand (Galileo's sunspot argument) entirely," that Koestler's book is full of "insolent misrepresentations" and his "ulterior motive" is the "blackening of science as the destroyer of 'spiritual values'," and finally that his thesis is "repugnant to everything we have written, and in contradiction with all that we have learned in the course of years devoted to these studies." According to Santillana and Drake, one of the many things we have learned is that "Galileo seems to have been practically the only man of his age who was fully aware of what was happening and what would follow."7

For a response to this review, consider Mark Graubard's summation after noting that Santillana's and Drake's attack of Koestler was "hostile to a degree rarely encountered in academic literature":

"history shows reason and scientific rightness to have been with Bellarmine and not with Galileo, who truly had no evidence besides the phases of Venus which disproved Ptolemy, but not Brahe. There was indeed no real proof... The overall work of traditional historians such as Santillana, compares to the contribution of Koestler much as the work of rat-psychologists compares to the contribution of a Sophocles, Dostoyevsky, Tolstoy or Faulkner to our knowledge of the human psyche, even though here and there Santillana does point to a slight inaccuracy of no genuine relevance.8"

In this exchange we see the same questionable dilemma noted previously. Rationality is either to be the
algorithmic calculation of a computer combined with an input process analogous to the mundane and unproblematic observational tabulations of the rat psychologist, with the historical actors completely aware of all the needed auxiliary nodes in their webs of belief, or it is nothing at all.

My notion of a hypertextual adjudicatory trail is similar in concept to the insights gained recently in neural net theory regarding learning, the functioning of the human brain, and artificial intelligence. Learning and understanding do not involve merely making crisp deductive connections that occur all at once based on fixed rules, but the gradual strengthening and articulation, via the test of experience, of a very involved net of neural nodes with the rules emerging from the interaction of the network. Unlike current digital computers based on the assumptions inherent in propositional logic, the brain does not seem to be a discrete state system. The model of a symbol manipulating system that maps discrete elements of a data-structure to features of the world is replaced by a much more "holistic system." Similarly, the acceptance of a theoretical structure must be viewed as a gradual, comparative strengthening of a vast network of theories, auxiliaries, methodologies, and experimental results. Furthermore, our meta-methodological judgments regarding the strengthening or comparative weakening are hypertextual in the sense that they are not entirely transcendent of the networks in
question but are a non-linear node on potentially several networks.\textsuperscript{10} As we will see, we must replace such extreme notions of deductive fit of theory and experience on the one hand, and framework gestalt switches on the other hand, with concepts of comparative reliability, gradual reinforcement or erosion, and threshold illumination.\textsuperscript{11}

In this way one can endorse the overall rationality of science without endorsing the rationality of every move of individual scientists. One can endorse wholeheartedly the relevance of the sociological, psychological, and cultural contexts for understanding theoretical allegiance, arguing in fact that such messy, complex milieus are necessary conditions for the creation of ideas and the articulation of adjudicatory trails, and still capture a supervising reasonableness to scientific change. For instance, that individual scientists may have interests that affect their attempts to prove other scientists wrong contributes to the criticism and competition necessary for the creation and eventual thorough analysis of the nodes of a hypertextual trail. Similarly, changes in allegiance of theoretical structures may also be causally related to interests. An individual scientist may realize that it is not in his or her best interest in terms of career and institutional support to appear stupid and dogmatic in the light of overwhelming evidence that shows that one’s former position is crumbling.
One need not be put off by Tycho's obsession for patronage, credit, and reputation, Kepler's wacky mysticism, or Galileo's egotistical social climbing unless one thinks of science as the sole creation of an "electronic brain" in Searle's Chinese room, and not by fallible human beings wanting to be right and successful in a social context. We need not jettison rational assessment just because we discover that individual pursuits and allegiances were powered in part by motivations other than the pursuit of truth. It is an elementary principle of informal logic -- one still worth defending against those who would advocate the existence of "alternate rationalities" -- that the circumstances of belief can be separated from the assessment of the reasonableness of belief. As Stephen Jay Gould has remarked, "People may believe correct things for the damnedest and weirdest of wrong reasons." In short, we can accept with Feyerabend without controversy in this post-positivist age that

... the development of knowledge is not well planned and smoothly running process; it ... is wasteful and full of mistakes; it ... needs many ideas and procedures to keep it going.\(^12\)

but still celebrate along with Darwin that there is "grandeur" in a view of life that sees beautiful adaptation as the result of historical contingency and the vagary and happenstance of individual pursuit.\(^13\) Furthermore, we can
also identify, as did Darwin, a governing process that constrains and makes use of contingency.

This said, we need to distinguish this position from the so-called compromise model recently advocated by Philip Kitcher.\textsuperscript{14} According to Kitcher, we need a model of scientific change, debate, and assessment that "embodies some of the ideas of rationalism and some of antirationalism," that recognizes the "crucial step" and "general moral . . . that epistemology should be psychological."\textsuperscript{15} According to Kitcher, his model "may be viewed as making some rather obvious amendments to classical rationalism. . . . The central thrust of rationalism (being) that the power of the right kinds of inferences is sufficiently strong to overwhelm effects of interdependence of nonepistemic goals, or of background variations in practice and stimuli."\textsuperscript{16} According to Kitcher, against this rationalist thesis we must acknowledge the thrust of the cognitive sociologists that scientists are moved by nonepistemic as well as epistemic goals, that there are causal relationships between beliefs and nonepistemic matters, that community decision is related to the relative power of subgroups, that there is variation within a scientific community as to practice, and that at certain stages in scientific debate reason is not necessarily on the side of the ultimate victors. Kitcher's main point is that it is a mistake to think that reason overwhelms nonepistemic concerns. Rather nonepistemic
features often work with epistemic features, provided we promote the nonepistemic features that are conducive to cognitive progress.

Apparently, Kitcher thinks that the great insight behind his *compromise model* is that the work of Bloor and other sociologists poses no threat to rationality as long as we recognize that "there are important distinctions among the types of processes that generate and sustain beliefs, decisions, and actions." There are "processes that reliably generate true beliefs, while others . . . have a very small chance of yielding true beliefs." In short, we can distinguish nonepistemic processes that favor good cognitive design and those that do not. Rule-based logical approaches do not sufficiently acknowledge that scientists are part of a social situation, that they have, for instance, "conversations" with colleagues, and that rules and reasoning must be supplemented by an "appropriate educational regime."

There are many points of agreement between Kitcher and my thesis. According to Kitcher, "It is, I believe, equally wrong to insist on the presence of decisive reasons for being a Copernican in 1543 and to deny that there were decisive reasons after 1632," and "Legend (Kitcher’s categorization of logical positivism, but also sometimes any apsychologistic approach to epistemology) does not require burial but metamorphosis." But have the sociologists provided us with "deep and important insights"? His key
thesis is trivial (he admits that it can be viewed as "rather obvious amendments") once one recognizes a simple questionable dilemma that he sets up with the help of the sociologists. He claims that we must get psychology into the "meliorative epistemological project," that the biggest mistake positivists and even all post-positivists, who have emphasized ampliative reasoning and fallibilism, have been making is to see the epistemological enterprise as basically one of logical analysis. In other words, he sets up a false dilemma between the defenders of Legend and all uses of psychology and then shows how we need a compromise model: to wit, the sociologists are right, sociological and psychological causative factors are heavily implicated in theory choice and transition, but the sociologists are also wrong -- it is a mistake to not make a distinction between good sociological processes and bad ones.

What defender of Legend ever disagreed with this? What defender of Legend ever disagreed with the belief that we should strive for optimal sociological conditions for knowledge acquisition? That good science requires conversations with colleagues and an "appropriate educational regime"? Once one translates Kitcher's "preferred idiom," all he is saying is that we have learned that scientists should go to college, learn to read, speak more than one language, know how to communicate with colleagues via journal articles, and so on. Furthermore, that we have
learned that such conditions are better at activating the "right propensities" for theory choice and progress than "lexographically ordering" alternative beliefs and then always choosing the number of the day of the month!  

This is not a deep insight. Kitcher's model of cognitive progress adds very little to what Duhem noted at the turn of this century.

[If we wish] to increase the rapidity of scientific progress by trying consciously to make good sense within [scientists] more lucid and more vigilant... nothing contributes more to entangle good sense and to disturb its insight than passions and interests... it is not enough to be a good mathematician and skillful experimenter; one must also be an impartial and faithful judge.

The only difference between Duhem and Kitcher is that Duhem does not make a big deal out of this obvious goal.

Furthermore, it should be clear that to distinguish between good cognitive design and bad cognitive design (good and bad sociological and psychological support structures) one still needs to tell an epistemological story of reliable criteria that will enable one to separate the two. Without such a rational reconstructive approach, one is prone to take too seriously sociological analyses of the causally linking of belief with interests such as the following:

1. Why did Rheticus become a Copernican so early when all other Wittenberg astronomers did not? Answer: When Rheticus was a teenager his father was convicted of sorcery and beheaded. In Copernicus, Rheticus found a kind and strong father he had always wanted, but one who was also a little rebellious as his father had
been. Furthermore, in the Copernican system, Rheticus found the unity that his father lacked after his beheading! Rheticus was in "search for wholeness, strength, and harmony," and was "unconsciously (trying to) repair the damage earlier wrought on his father." Also, Copernicus "had a head and heart which were connected to the same body."25

2. Why did Tycho not accept the Copernican system when he appreciated the unity and parameter determination linkages as much as that of Kepler? Answer: Tycho's personality was very different from that of Kepler. Unlike Kepler's constant religious introspection, Tycho was a man of this world, heavily attached to la dolce vita. He drank heavily and loved to party. (He died when his bladder burst from the consumption of numerous libations during a long speech by a nobleman.) As such, in an era of heavy religiosity, and as a Lutheran, he carried around with him considerable guilt and concern about his fate in the next life. Thus, although he acknowledged the astronomical superiority of the Copernican system, he could not shake the fact that its physics conflicted with the Bible. So, not wanting to risk any conflict with the Bible, he developed his famous compromise.26

Human beings are indeed messy creatures. We cannot avoid the fact that such speculative causal factors may be involved in belief commitment. There are no doubt some interesting psychological reasons for why some people flock to the U.S Airforce's top secret Area 51 in New Mexico claiming to be aliens from another planet. They say they have come to be picked up to go home. However, not only would our time be better spent analyzing evidence for actual visitation by extraterrestrials, but it is doubtful that we need spend much time on analyzing whether the noncognitive source of this belief is good or bad in terms of promoting progress. Even if we did, our assessment would be based on
epistemic criteria. Similarly, our time will be better spent understanding the epistemic significance of Rheticus's important insight that if the Copernican system is true, Mars should show a greater parallax at opposition than the sun, or how Tycho's vacillation on and eventual acceptance of geoheliocentrism were related to the gradual weakening and strengthening of hypertextual adjudicatory trails.

For that matter, a better sociological explanation for Tychonic pursuit of geoheliocentrism would cite his concern for patronage. Because he needed to be very careful politically, he did not have the freedom of vision that Kepler did. He vacillated on many issues -- whether Mars showed a parallax greater than the sun at opposition, whether the crystalline spheres should be dropped -- because he had much to lose financially if he was wrong. In short, Tycho was absorbed by constant machinations to maintain the attention of financial backers and was easily distracted. By contrast, Kepler's relative poverty, eccentricity, and steadfast vision of being the first to find the mathematical clockwork with which God made the universe -- to literally read the mind of God -- allowed him to be less distracted by political winds.

In Kitcher's terminology, both Tycho and Kepler had propensities. Both wanted to be right, but for different reasons. Kepler desired a Platonic glory, Tycho a this-world glory and substantial financial commendations. But
our time will be better spent analyzing what kinds of adjudicatory trails activated these propensities. Ironically, considering how much reassurance Kuhn’s work has given the sociologists, we should follow his advice here: "To understand why science develops as it does, one need not unravel the details of biography and personality that lead each individual to a particular choice, though that topic has vast fascinations."³º

Contra Kitcher, we need neither compromise with the sociologists nor spend much time separating good from bad noncognitive processes. As Kitcher admits, even so-called bad noncognitive factors may aid progress. A modern Kepler who has a passionate religious interest in being the first to arrive at the God equation, a super unified field theory that can explain not only the origin of the big bang out of a quantum fuzz and the crystallization of the forces of nature immediately after the big bang, may have just as much chance at success as the most dispassionate secularist. What matters is for us to become more rational over time, to learn from the past, to see what normative factors did eventually overwhelm or activate "the damnedest and weirdest of wrong reasons." Perhaps, if there is any lesson to be derived from a sociological analysis of Tycho’s life, it is that the modern scientist should reflect on how much the incessant pursuit of our contemporary version of patronage, i.e. grants -- the forms, the networking, the establishing
of reputation -- may distract one from fruitful pursuits, seeing new approaches to problems, and/or the merit of maverick positions.\textsuperscript{31}

Thus, the "meliorative epistemological project" is still primarily logical, albeit ampliative and historical. This said, the concepts defended in this thesis do not deny that research into cognitive psychology and sociology may provide some empirical data relevant to epistemological issues, but the traditional epistemological project, stripped of foundationalism, is still primary. This thesis attempts to handle the complexity and messiness that one finds in the historical record without capitulating to the sociologist and by using the notion of complex hypertextual adjudicatory trails while simultaneously recognizing the contextual nature of scientific problems.\textsuperscript{32}

However, before proceeding any further, it is important to appreciate the refined historical analyses that have revealed the complexity of the Copernican episode in all its grand detail. First, by way of contrast, popular treatments in introductory science textbooks, substantially influenced by Legend, often portray Kepler, Galileo, and Tycho as heroic empiricists, not only vanquishing resistance to the truth of our noncentral location in a vast universe, but ushering in the very methodology of modern science, as if such a methodology was sitting in some dusty drawer of rationality waiting to be plucked by brilliant minds, thus
implying, of course, that all that had gone before was mysticism and ignorance. From these models of good scientific practice students are taught that we should treat with derision any attempt to prove a priori any feature of the world, such as the alleged medieval attempt to prove that the number of planets must be six. Kepler is then seen as honorably giving up his cherished five-perfect-solids hypothesis when he realized that he could not make it agree with observations. Galileo is seen as attempting to get resistant dogmatic supporters of the Church to look through the telescope, as if every school boy at the time should have known that this instrument was a reliable empirical tool. Moreover, Galileo’s Assayer is quoted over and over again, as a manifesto of a new science of observation and mathematical analysis in preference to philosophical speculation and dogmatic authority. And Tycho is seen as working collaboratively with Kepler, cheerfully supplying the astronomical data that will prove the Church wrong.

Yet, Kepler never gave up on his five-perfect-solids conviction and argued, like Rheticus, an early promoter of Copernicanism, that there could be only five planets besides the earth, even after the discovery of his three laws of motion. Furthermore, Kepler supported, as did Copernicus, heliostasis in part because as a neoplatonist he thought that the sun was the "material domicile" of God, and his tense relationship with Tycho, theoretical and personal, is
well known by any first-year historian of science. Kepler also opposed Bruno’s promotion of an infinite universe, later adopted by Newton, and thought that the stars were part of a packed, relatively narrow, celestial vault composed of ice. He was also fully convinced that God was on his side in the race to be the first to truly understand the architecture of the universe, for why else, he thought, would he arrive in Prague at the very time that Logomontanus, Tycho’s assistant, was working on Mars, the relatively greater eccentricity of which was the key to the discovery of elliptical orbits.

Speaking of Bruno, the revolutionary nature of his work is clearly attenuated by the fact that he does not appear to have read carefully or understood Copernicus’s *De revolutionibus*. His personal copy lacks any annotations. In his *La Crena* he pictures Mercury and Venus being on the same epicycle, which in turn revolves on the same deferent for the Earth and moon, the latter also revolving on a single epicycle, and in his *De immenso*, Bruno claims that the Earth, moon, and planets must be approximately the same size and have the same revolution around the sun, just like animals of the same species. According to Ernan McMullin,

It is hardly necessary to say that these constructions were at odds not only with the Copernican system but with the accumulated observational evidence on which mathematical astronomy had rested for more than two millennia. It was obviously not on observational evidence that Bruno was relying.
Then there is the complexity of Galileo. We have already acknowledged Feyerabend's attention to the convoluted nature of the acceptance of the telescope. (Actually Feyerabend is only "borrowing" this analysis from the work of Vasco Ronchi.\textsuperscript{38}) Galileo paid little if any attention to Kepler's work and supported the notion of uniform circular motion as dogmatically as any supporter of Ptolemy. He was convinced that his mistaken theory of tides proved heliostasis correct, and perhaps worst of all, balked at the thought of a vast universe to incorporate the Tychonic proposal of large elliptical orbits of comets, in the process changing his mind on the nature of comets in such a way as to seriously weaken arguments against Aristotelian cosmology.\textsuperscript{39} Also seldom mentioned by supporters of a simplistic Legend is that Galileo's great philosophy of science treatise, \textit{The Assayer}, is full of ad hominem polemic against taking as authoritative the Tychonic observational and mathematical analysis of comets ("Tycho's monkey-planets")!\textsuperscript{40} And speaking of Tycho, also rated X for historical virgins is any mention that the great observationalist Tycho could not bring himself to believe in heliostasis in part because he was convinced that God would not "waste" the vast amount of empty space implied between Saturn and the stars.\textsuperscript{41}

Aside from these historical embarrassments for the supporters of a simple scientism, consider the confusing
situation an impartial observer, say during the first decade of the 17th-century, would find attempting to apply a purely linear-logical fit between evidence and theory for deciding between geostasis and heliostasis. For a defender of Ptolemaic-geostasis there were the following problems.

1. There were many failures to save the most basic observational phenomena.

   Most of these were well documented by Regiomontanus as early as the second half of the fifteenth century.\textsuperscript{42} It was no secret that by the 16th century serious calendar reform was needed due to the growing embarrassment of when important religious dates fell. Some of the Ptolemaic predictive errors were very large. In 1504 a Ptolemaic prediction for a conjunction of planets was off by 10 days, and in 1563 another predicted conjunction was off by a month. Gingerich believes there is strong evidence that Copernicus observed the 1504 failure. For the 1563 conjunction, the Copernican prediction was off by only a day or two.\textsuperscript{43} There were also errors in predicting the time and duration of lunar eclipses, and there was a total failure to predict annular eclipses.

2. The appearances of the magnitudes and sizes of Venus and the moon were anomalous.

   Although Ptolemy's solar and lunar models, when combined, allow for the calculation of solar eclipses for
any particular geographical location for the first time in the history of astronomy, a major problem occurred when the model of the moon is combined with that for Venus. Based on Ptolemy's lunar model, the moon should appear twice as large at its closest approach to Earth. The model for Venus predicted that it should appear 7 times brighter on its closest approach to earth, and when linked with the lunar model it should appear at times 40% the size of the moon. Part of the problem is that Ptolemy was trying to account for what has come to be called evection, a periodic change in the range of the speeds of the moon. To account for what thus appears to be a regular variation in the eccentricity of the moon's orbit, Ptolemy used a device later used many Arab astronomers and in a substantial way by Copernicus. Sometimes known as an epicyclet, the center of the lunar deferent is placed on its own small internal circle. As this circle turns it cranks the deferent nearer and farther from the Earth. There were similar diameter and magnitude appearance problems for Mars.4

3. The prediction of a pattern of transits for Venus and Mercury of the sun were not observed.

4. A substantial parallax can be observed for the moon but not for Mercury.

Since Ptolemy has Mercury being the next celestial object, being positioned between the moon and Venus and with its orbit nested immediately on top of the moon’s, parallax
for this planet should be observed at perigee. But no such parallax was observed. According to Ptolemy in his *Planetary Hypotheses*, "If (planetary) distances are correctly given, Mercury, Venus, and Mars (should) display some parallax... The parallax of Mercury at perigee is equal to that of the Moon at apogee."\(^{45}\) Although previously, he says that "no phenomenon allows us to fix their (the planets) parallax with certainty."\(^{46}\) Presumably, and this is the way Goldstein interprets this remark, Ptolemy is referring to the inability of naked-eye observations to measure parallax for any of the planets. This is clearly true of Mercury, given its closeness to the sun and the relatively small amount of time it is visible after sunset.

Furthermore, as Ptolemy himself notes, his purely geometric model does not allow one to ascertain whether Venus or Mercury is closer to the Earth.\(^{47}\) Thus, opening up his astronomical model to the charge of ad hoc flexibility discussed next.

5. By the end of the 16th century Tycho's parallax measurements showed that comet orbits would have to cut through the orbits of Saturn, Jupiter, and Mars. This and other problems drew increasing attention to a long standing tension between Ptolemy's purely astronomical models and Aristotelian auxiliaries as a system.

Because Ptolemaic geostasis, as a complete system able to ascertain planetary distances, depends crucially upon referential commitment to solid celestial spheres, this was
a major defect judged against a criterion of systematicity - a criterion, if not new, at least emerging as more important due to the clamoring of the supporters of Copernicanism. Furthermore, the Aristotelian cosmological auxiliary support for Ptolemy was further weakened by the observation of several novae by 1604. Finally, as the criteria of systematicity and parameter determination became increasingly important, Ptolemaic planetary models became vulnerable to the charge that they were ad hoc and created to handle individual planetary problems and could not work well as part of a system.

We have already seen how Ptolemy can get an adequate fix for solar eclipses with his solar and lunar models, but that the lunar model will not only not match other observations well but conflict with the model for Venus. Consider another important example. Using the mechanisms of deferent, epicycle, eccentric, and equant point, Ptolemy is able to fashion an adequate model for its time for Saturn. He could account for the major anomalies of retrograde motion and frequency of such motions, and couple these fairly well for its time with observations of longitudinal positioning. Furthermore, he is able in his Planetary Hypotheses, to use the deferent and epicycle devices, coupled with the nesting sphere hypothesis, to fashion a system that predicts planetary distances. However, when the positioning of the equant point, created as an
individual fix to square with planetary positional concerns and the pythagorean maximum of uniform circular motion, is placed within the nested spheres system, it must fall within the nested sphere of Mars!\textsuperscript{49}

Ptolemy can be found arguing in his \textit{Planetary Hypotheses} for a cosmological justification for treating each planet's motion as a separate problem.

The heavenly bodies suffer no influence from without; \textit{they have no relation to each other}; the particular motions of each particular planet follow from the essence of that planet and are like the will and understanding in men.\textsuperscript{50}

This is often overlooked by Copernican apologists when arguing that it is obvious that heliostasis was the better system. However, Ptolemy and his supporters could give no explanation for how Saturn's equant point could function as part of a system of any kind, of how the celestial machinery expressing the essence of one planet could not interfere or would work with that of another planet.

6. In this light, a major conceptual problem that bothered just about everyone was the equant point used by Ptolemy to save uniform motion.

Because a planet in the Ptolemaic system will have uniform motion relative to the equant point, but not to the earth or the center of the planet's deferent, this device not only appeared to be inconsistent with pythagorean-aesthetic requirements, but would not work well with
referential commitment to solid celestial nesting spheres. Hence, the equant point was seen not only as a conceptual embarrassment but was open to the charge that it was another individual ad hoc patch that did not work well as part of a system. 51

7. Although much was made that the Copernican system was committed to the "absurd" physical notion that the heavy earth spun on its axis at a great speed and revolved around the sun at an even greater speed, in the Ptolemaic system the velocity of fixed stars would have to revolve around the earth in excess of 20 million miles per hour to account for their diurnal motion.

Although the plausibility of this belief was supported by Aristotelian cosmology and its commitment to the celestial nature of the stars, any weakening of this cosmology imposed by the observation of novae and comets made acceptance of this great speed problematic.

8. Finally, Ptolemy as well as the later Alfonsine Tables give an incorrect prediction for the precession of the equinoxes.

This was thought to be very serious by Regiomontanus. 52 Instead of 1 degree per 70 years, Ptolemy used 1 degree per century. As it became apparent that the stars were drifting more rapidly than Ptolemy's figure predicted, an auxiliary patch called trepidation was concocted by geostatic supporters. To be able to maintain that Ptolemy was not in error at his time, it was proposed around A. D. 1000 that precession was variable. In the centuries that followed
Ptolemy, precession was said to be gradually occurring at a faster rate. By the time of Regiomontanus, however, this patch was hard to accept.

As early as 1464, in noting many of these discrepancies between observation and prediction, Regiomontanus complains that most astronomers were like "credulous women" for accepting Ptolemaic predictions from tables without noting their inconsistency with observations. According to Regiomontanus, a major undertaking was necessary "to restore the heavens. . . . (and) remove the rust from the heavenly spheres."53

But did obvious need of reform require a radical new cosmology? Did the Copernican system fair any better? For a defender of Copernican-heliostasis there were the following problems.

1. There were also many failures to save the most basic observational phenomena. In fact, in spite of Copernicus's stated goal of achieving an accuracy of planetary positioning within 10' of arc, Copernicus was only trying to match most of Ptolemy's predictions.54

Although much is sometimes made of the superior predictive accuracy of the Copernican system,55 the actual situation was far from decisive. Here is Owen Gingerich's description of what Tycho experienced:

Tycho frequently compared his own observations to the predictions from the Alfonsine and Copernican tables,
usually to the advantage of Copernicus. A particular favorable comparison occurred at the time of the great conjunction of Jupiter and Saturn in 1583, although by August 20, 1584, Tycho’s comparison for Jupiter showed the two schemes equally in error, and by 21 December, 1586, the Alfonsine calculation was decidedly better, especially in latitude. Frequently, the Copernican latitudes proved inferior, even when the longitude excelled -- for example, for Saturn on January 24, 1595. Tycho compared lunar positions in December 1594, and toward the end of the month the Alfonsine-based Leovitius ephemeris was superior. The most conspicuous Copernican errors found by Tycho occurred during the August opposition of Mars in 1593, exceeding 5 (degrees).56

Tycho’s experience is important because it must be remembered that prior to his more accurate measurements, astronomers could not ascertain whether Copernican predictions were better than Ptolemy’s. Furthermore, "ease of calculation" has often been confused with "superior predictive ability." By the late 16th century more and more calculating almanacs were based on Copernican tables. Johannes Praetorius, a contributor to the Wittenberg tradition, summarized the situation well in 1592.

Now, just as everyone approves the calculations of Copernicus [Reinhold’s Prutenic Tables], so everyone clearly abhors his hypotheses on account of the multiple motions of the earth . . . we follow Ptolemy, in part, and Copernicus, in part. That is, if one retains the suppositions of Ptolemy, one achieves the same goal that Copernicus attained with his new constructions.57

According to Gingerich, the Ptolemaic Alfonsine Tables were very difficult to work with,58 and

After De revolutionibus was published, Erasmus Reinhold reworked the planetary tables into a far handler form.

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His *Prutenic Tables* superseded the *Alfonsine Tables* remarkably quickly. This is actually very curious because, in the absence of systematic observations, nobody really knew how good or bad any of the tables were. In fact, it was not until Tycho Brahe that a regular series of observations established the inadequacies of all the tables.\(^9\)

2. Similar to that of the Ptolemaic system there were significant inconsistencies between observation and prediction regarding the diameter and brightness of Mars and Venus. For Copernicus the apparent diameter of Mars and Venus should vary by factors of 8 and 6 respectively. But Mars appears to change size only by a factor of 2 and that of Venus is negligible.

Osiander in his famous nonsolicited preface to *De revolutionibus* cites these discrepancies as conclusive proof that both the Copernican and Ptolemaic systems were not true. Some supporters, however, would cite the Mars variation as a positive confirming instance of the Copernican system. But Galileo can be found arguing in the *Assayer* that until his telescopic observations, "the movements of Mars and Venus stood always in the way (of accepting either the Tychonic or Copernican systems.)"\(^60\)

3. Copernicus predicts a full range of phases for Mercury and Venus which could not be observed.

It is doubtful, however, whether or not the players in the Copernican episode were fully aware early in the debate that this was an apparent refutation of Copernicus, or even if phases were observed, as they eventually were by Galileo, that this would be a dramatic confirmation of heliostasis. It must be kept in mind that it was not a closed issue.
whether the planets reflect light from the sun or are self-generators of the light observed. Or, even if they do reflect light from the sun whether they do so directly or somewhat indirectly, being transparent, such that "the light of the sun becomes incorporated with these stars and gets soaked up in all their parts, which does not happen for the moon." (Albert of Saxony, 1360)

Thus, even if phases were not observed in planets, this was sometimes interpreted to mean that planets do not reflect light from the sun. If phases were observed, even a full phase for Venus, Aristotelian and Ptolemaic cosmology could be saved by the reemission theory. Even if the angle between the planet, Earth, and sun is such that only a portion of the planet is "soaking" in the light from the sun, as it should always be in the Ptolemaic system, the planet itself could reemit the light such that a full phase appears!

Again we see the importance of auxiliaries, and how often not all the necessary auxiliaries are in place for a crisp confirmation or falsification of a particular theory. We view the Venus episode from the modern standpoint of connected auxiliaries that produce a crisp falsification for Ptolemaic geostasis, and forget that astronomers in the 16th and early 17th centuries did not necessarily share the same auxiliaries. Required were a further weakening of Aristotelian cosmology, greater support for the belief that
the moon and the planets are physical places that reflect light, and an understanding of the negative ramifications of the reemission theory (If this theory is true, why didn’t Venus show a full phase at all times?), before the adjudicatory trail for Ptolemaic astronomy saving the Venus full phase observation by Galileo could be significantly weakened.

4. It was assumed that parallax for the sun could be established. In the Copernican system, Mars and Venus should show parallax, but there are no naked-eye observations of this. Ptolemy predicts a substantially smaller parallax for Mars.

In the Copernican system Venus is closer to the Earth than Mercury; whereas in Ptolemy’s system Mercury is closer. A clear potential observational difference in terms of parallax except for the fact that the observing time for Venus and Mercury is small and parallax measurements of these planets is not within naked-eye resolution. However, Tycho knew that since Ptolemy always has the orbit of Mars beyond the sun, whereas Copernicus has Mars approach the Earth at less than the sun’s distance at opposition, the extent of parallax for Mars was a pivotal potential observation that would separate a geostatic system from a heliostatic or geoheliocentric system. As a defender of the geostatic system, he was convinced that no parallax for Mars could be determined within the naked-eye resolution. Later as he became interested in preserving Copernican linkages by
promoting a geoheliocentric system, he convinced himself that he did observe parallax for Mars! It is not possible to observe parallax for Mars without a telescope.

Tycho also argued that the relative closeness of Mars is confirmed by the swift speed of its retrograde motion. But a Ptolemaic astronomer has no trouble saving and explaining (to some extent) this appearance. The speed of Mar's epicycle is adjusted a posteriori to match the relatively faster retrograde motion.

5. Careful observations by Tycho showed no stellar parallax. Hence, a Copernican must not only assume that the stars are very far away but also of humongous size (800 times size of sun). In the Copernican system, the entire solar system becomes virtually a mere point. Why should the sun be such a minute center of such a vast system?

In *De revolutionibus* Copernicus can cite no persuasive independent evidence for this startling claim. He thinks that the immense distance to the stars is "proved" by the fact that they do not show retrogressions like the planets. At the beginning of his book, after citing how well heliostasis explains the linkages of retrogressions and their frequencies for the planets, Copernicus says, "Yet none of these phenomena appears in the fixed stars. This proves their immense height, which makes the annual parallax vanish from before our eyes." Copernicus is guilty here of some rather obvious sleight-of-hand that fooled very few. Although Copernicus can cite this as a consistency condition
of his system, to claim much more is circular reasoning, because "no observed retrogressions" is just another way of saying that the stars don't show parallax. The closest Copernicus comes to citing an independent reason for believing in the immense distance between Saturn and the stars is that the stars "twinkle"!56

Related to the stellar distance problem is that Copernicus appeared to still endorse the Ptolemaic notion of solid spheres. If so, there are huge unexplained gaps between the spheres, that in the old system needed to be in contact to explain their coordinated motion, not to mention the problem of an even greater gap between Saturn and the fixed stars. According to Barker, Copernicus's objection to the equant is better understood in terms of referential commitment to uniformly rotating solid spheres.67 Recall that commitment to solid celestial spheres appears to be radically inconsistent with a Ptolemaic equant point for Saturn. But followers of Copernicus can not have it both ways. They can not object to the equant point because it is inconsistent with commitment to celestial spheres, and then ignore how a heliostatic system is also inconsistent with such commitment.

6. In the Copernican system the moon's motion is not around the center of the universe. Why the exception? Even in a heliostatic system, all the other astronomical bodies revolve around the center of the universe. Furthermore, although Copernicus's lunar theory is an improvement in terms of avoiding the dramatic variation in apparent size predicted by
Ptolemy, Copernicus's multiple epicyclic motion for the moon results in not having the same side of the moon always facing the earth.

7. A supporter of the Copernican system had to face a huge physical problem. Whatever kinematic benefits might accrue to a heliostatic system were surely offset by the dynamical problem of a triple motion for the Earth.

In *De revolutionibus* Copernicus is able to offer only a very weak auxiliary patch -- actually only an extension of the uniform circular motion dogma. According to Copernicus, large amounts of substance, whether terrestrial or celestial, tend to form perfectly round circular wholes and such objects naturally rotate. Furthermore, although problems with Aristotelian dynamics were highlighted by competition from impetus theory, it was not clear how the latter would help a Copernican and the former, to use Feyerabendian language, was far from a "dead dog."

8. In contrast to the sketch of a heliostatic system presented in the *Commentariolus*, the full heliostatic system presented in *De revolutionibus* is quite complex.

The myth that Copernicus used far fewer circles than Ptolemy originates with Copernicus's summation in his *Commentariolus* to the effect that a heliostatic system needed only approximately 30 circles whereas Ptolemy used about 80. It is known that Copernicus wrote his *Commentariolus* before 1514.\(^6\)\(^8\) Ironically, Copernicus did not get his hands on the full edition of Ptolemy's *Almagest* until 1515. According to Gingerich, "Through this work he must have
become more fully aware of the tremendous task facing any astronomer with the courage to construct a complete celestial mechanism."69 Thus, in order to be a serious contender -- to at least match Ptolemaic planetary positioning -- and to eliminate the equant point, Copernicus must use numerous epicyclets. To this day there is not agreement on just how many circles Copernicus used. According to Gingerich, "Even Copernicus would have had difficulty in establishing an unambiguous final count."70 See Figure 2.

9. Finally, we can add to this list the fact that many Tycho-like, geoheliocentric systems competed with the Copernican system by the first decade of the 17th century, and every astronomer was familiar with the merits of a third approach.71

Adding to the confusion and possible choices, some of these systems, such as Tycho's own, were fully geoheliocentric with all the planets other than the Earth revolving around the sun, and some, such as that of Andreas Cesalpinis in his *Peripatetic Questions*, were partially geoheliocentric with only Mercury and Venus revolving around the sun. (Tycho had also experimented with this arrangement as early as 1578.72) Some of these systems had Mars enclosing the orbit of the sun (Ursus), and some had the orbit of Mars intersecting the orbit of the sun (Tycho). Some retained solid celestial spheres and some did not. There were also different schemes in terms of the distances to the superior planets, and hence heaven. Some used elliptical orbits;
some eccentrics, and some were multiple epicyclic using the same interior epicyclet device used by Copernicus. Some had the Earth rotating and some did not. Finally, John Craig even suggested that a third point be added, closer to the Earth than the sun, for the true center of planetary motion.73

Just as Copernicus's work revived that of Aristarchus, these systems resurrected the approach of Heraclides Ponticus, Adrantus of Aphrodisias, and Theon of Smyrna.74 Some of these systems accommodated comets, preserved many of the parameter linkages that were appealing in a Copernican system, and made similar empirical predictions. But with a stationary and centralized Earth, they were consistent with lack of observed stellar parallax and did not require a new terrestrial dynamics.75

As we will see in more detail in chapter 5, Howard Margolis has argued that in fact by the first decade of the 17th century, support for a pure Ptolemaic system had almost completely faded from the scene, and that the comparativist situation was between heliostasis and geoheliocentrism.76 He argues that even in Galileo’s Dialogue, "Galileo’s identification of the Tychonic arrangement (not the Ptolemaic) as the surviving non-Copernican alternative is unambiguous and emphatic, but discrete..."77 Margolis builds an interesting case that Galileo had been ordered by the Pope not to attack the Tychonic system, which the Church
then supported, but Galileo nevertheless did so indirectly forcing the Pope to realize that he had been deceived by Galileo. As part of the evidence for this thesis, Margolis notes the number of pages devoted in the Dialogue to issues that would only concern a debate between supporters of heliostasis and geoheliocentrism (Galileo’s trick), and the fact that Inquisition reports never complain that Galileo fallaciously weakens the geostatic case by leaving the Tychonic system out of the debate. Given that Galileo has often been criticized by historians and philosophers of science for so blatantly creating a false dilemma in his Dialogue, but given that the historical actors did not so criticize him for this when they surely would have been aware of it, Margolis’s new interpretation does explain some long-standing puzzles about the Galileo affair.

According to Margolis, Galileo indirectly sets up an opposition between geoheliocentrism and heliostasis where geoheliocentrism is the weakest, but does not mention the geoheliocentric alternative when it would be the strongest competitor to heliostasis, such as the observation of a full set of phases for Venus. Instead, Galileo indirectly draws just enough attention to the geoheliocentric alternative to make the informed reader wonder how this cumbersome system would work, then drops it in such a way as to leave the reader realizing that no one could possibly develop a dynamics for it in contrast to an emerging dynamics for
Copernicanism. In effect, Galileo is saying to the Pope, "Ok, you won't let me mention Tycho's system, so I won't even in cases where you would want me to!"

In some ways the Copernican episode was more like a grand soap opera than the neat laboratory setting of a rat psychologist. There was a rich labyrinth of possibilities and a meandering parade of characters with diverse interests defending many positions. Indeed, no viable rational reconstruction can ignore the messiness and contingency that history reveals. But complexity does not automatically endorse "anything goes." Given this complex background, I would now like to develop my thesis further by contrasting it to three well-known interpretations of the Copernican episode. It is my contention that these three influential interpretations are either wrong on significant details or incomplete in such a way as to set the current stage for the epistemological malaise found in socio-cognitive relativism.
Notes for Chapter 2:

1. In the Introduction to Duhem, 1969, p. xxv. Jaki’s statement is worth quoting in full.

History is the great equalizer. Sooner or later it cuts all things and all men down to their true size. Science looms up as a savior only for those whose familiarity with it is restricted to what Duhem so aptly call 'the gossip of the moment.' Those who are brave enough to look past the popular but ephemeral truths of the day will find in history a most instructive teacher. The history of physical science can indeed forcefully show the student that myths are present in science no less than in other areas that owe so much to science for the reduction of their myths. Recognition of this may be a humbling experience in a scientific age such as ours; yet it is indispensable if science is to become man’s servant rather than his tyrant.

3. Ibid., p. 10.
7. Ibid., p. 255, n. 2. An extreme characterization when one considers Galileo’s adherence to circular motion and his flipflop on comets. See below.
10. Otherwise, according to Dreyfus and Dreyfus, the traditional approach of seeing appraisal, understanding, or learning as a neutral rule-based activity leads to "not only a regress of rules for applying rules but an exponential explosion of them. . ." Ibid., p. 80. As we will see (Chapter 5), parameter determination became a theoretical constraint for both geostasis and heliostasis by the late 16th century. Although it can be seen as emerging from heliostasis, it gradually became linked with the pursuit of patching geostasis.
11. As such my portrayal of scientific assessment will have much in common with Lakatos's notions of progressive and degenerating research programs. However, as we will see in chapter 4, because Lakatos does not completely extricate himself from infallibilism and deductive assessment, he becomes too sensitive to criticisms based on the alleged need for crisp cut-off points for acceptance and rejection of scientific theories. In answering critics, he finds something in the Copernican episode that he does not need.

12. Feyerabend, 1987, p. 188.

13. Darwin finishes his epic *On the Origin of Species* with a stunning rhetorical flourish:

"There is grandeur in this view of life, with its several powers, having been originally breathed into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being, evolved." (1900, pp. 669-700)


15. Ibid., pp. 184, 200, 201, n. 27.

16. Ibid., p. 201, n. 27, p. 197.

17. Ibid., p. 185.

18. Ibid., p. 186.

19. Ibid., p. 209.

20. Ibid., p. 391.

21. Ibid., p. 162.

22. Ibid., p. 196.

23. Ibid., p. 185.


26. As far as I know, no sociologist has advanced this thesis. But given time and encouragement, encouragement that Kitcher is unintentionally providing, we are likely to see such nonsense published eventually.
27. *Narratio Prima*, in Rosen, 1971, p. 137. Rheticus thinks that Mars can be observed to have a closer approach to the Earth than the sun. Tycho vacillates on this issue, eventually claiming that he has measured parallax for Mars. Both men have an interest in this particular result — it supports heliostasis and geoheliocentrism over geostasis. Relativist fodder? No. Kepler too has an interest in this particular result, but admits that parallax for Mars cannot be determined with naked-eye instrumentation. Gingerich and Westman, 1988, p. 70-71; Gingerich, 1993, p. 149.

28. Developed below, chapter 5.

29. Kepler, of course, was not totally uninterested in patronage and a "job." The claim here amounts to a matter of degree. Kepler wanted to survive; Tycho wanted castles.


31. And perhaps we should be wary of drinking too much during long speeches.

32. There are, however, other points of agreement with Kitcher's work and my thesis once terminological differences are made clear. His use of "admissible cost functions," "rival admissible cost functions," (pp. 250-51) and "escape trees," and "Duhemian predicaments" (p. 256) are very similar to my "hypertextual adjudicatory trails and auxiliary nodes," and related language. Kitcher also emphasizes the gradual nature of acceptance, "position crumbling," and crystallization of debate focus (pp. 204-205) which are similar to my strengthening of a hypertextual trail. These points of agreement will be explored in more detail in chapter 5.

33. Over the years I have sat in on three different Introductions to Astronomy, at the University of Washington, University of Hawaii, and Honolulu Community College. Almost on cue, usually during the second lecture, all three instructors, all excellent lecturers otherwise, would trot out this claim as an obvious example of poor methodology and ignorance, apparently totally unaware that it was Copernican supporters, Rheticus and Kepler, that often made this a priori argument for six planets. In the case of the University of Washington professor, Rheticus's *Narratio Prima* was quoted (Rheticus comparing the number of bodily orifices to the number of planets!), but students were left with the impression that this was theasinine thinking of a medieval cleric.

34. See for instance, Carl Sagan's treatment of Kepler in his *Cosmos*, chapter 3. Although Sagan does record the discordant personal relationship between Kepler and Tycho, Kepler is shown rejecting his five perfect solids hypothesis because the
facts did not support it. In introductory astronomy books one finds statements such as: "Their (Galileo, Kepler, and Tycho) lives culminated in conclusive proof that the Copernican system could work. . ." (Seeds, 1986, p. 71); "Tycho proved that the stars and planets were many times farther away than the moon. . . (Hartmann, 1985, p. 114); and "Kepler derived (his three laws) from Tycho's extensive observations, not from any fundamental assumptions or theory" (Seeds, p. 76-77). In Kaufmann (1993, p. 38) we still find,

"Copernicus's astronomical studies detailed the advantages of the straightforward Sun-centered cosmology over the cumbersome Earth-centered theory. This work instilled a revolutionary concept that pervades modern science. Simplicity is the hallmark of correctness. Thus, nowadays, when a theory becomes unduly elaborate and complicated, scientists begin to suspect that the theory is probably wrong."

In this same vein it is worth noting that as late as 1969 the Encyclopedia Britannica promoted the myth (p. 645) that the Ptolemaic system needed a monstrous amount of patching (40 epicycles per planet!) to save the phenomena. Hartmann (p. 112) repeats the Alfonso myth of tacked-on epicycles and then cites Occam's razor as the origin for the basic scientific principle that "the best is simplest." Hartmann also gives students the impression in discussing Galileo (p. 116) that "intelligent people could see the plain truth through the telescope."

35. Well almost. In his apology for Galileo's role in the controversy of the comets of 1618, Drake refers to Tycho as Kepler's "revered predecessor," by way of explaining in part how Kepler came to comment on the comet controversy. (Drake, 1960, p. xxii.) In this same work Drake claims that Galileo's advocacy of the modern "scientific method as a road to truth" was the novel element that was responsible for the revolution that took place in the 17th-century, and that "only in Galileo's day, and largely through his efforts, that a clear parting of physics and metaphysics was eventually reached." (pp. viii-ix.)


38. Ronchi, 1957. According to Ronchi, Galileo's advocacy of the telescope as a reliable instrument was initially the "new faith" of one man against all of "conventional science" that was "hostile and distrustful." According to Ronchi, "The entire academic world reacted violently (to Galileo's Sidereal Message), with one voice accusing Galileo of extolling as real discoveries figures seen only with the telescope, a notorious-
ly misleading and untrustworthy contraption." (p. 46.)

39. Koestler claims that Galileo's flipflop on comets from the Tychonic position he appeared to endorse in his Letters on Sunspots was due primarily to ego -- Grassi failed to mention Galileo's contributions in his Jesuit manifesto On the Three Comets of the Year 1618. (Koestler, pp. 466-471.) More consistent with my thesis is the explanation, also noted by Koestler to his credit (p. 467), of Galileo's entrenched resistance to elliptical orbits. For Galileo's resistance to the idea of a vast universe to incorporate such orbits, see his Discourse on the Comets, in Drake, 1960, p. 27. For Drake's apology for Galileo on resisting elliptical orbits, see note 7, p. 363. For Grassi's reply to Galileo on the possible nature of the orbits, see his The Astronomical and Philosophical Balance in Drake, 1960. Grassi's reply is particularly interesting because he admonishes Galileo for not realizing how such orbits could be seen as consistent with his own position. According to Grassi, "What if it be not even elliptical but entirely irregular -- since especially in Galileo's system it would be able to move freely without any hindrance?" (p. 75)

In Tycho's discussion of comets, De Mundi Aetherei Recentioribus Phaenomenis (On the Most Recent Phenomena of the Aetherial World), he refers to the possibility of the orbits "not being at all points exquisitely circular, but somewhat oblong, in the manner of the figure commonly called ovoid." Boas and Hall, 1956, p. 263.

40. According to Drake, the "difficulty of rationally accounting" for Galileo's outburst against the "reasonably sensible discourse" of Grassi on the celestial nature of comets can be solved by believing that Galileo was prohibited from directly advocating Copernicanism but not from the new methodology by which he had arrived at heliostasis. So, he was using the controversy of the comets of 1618 as a pretext to advocate his new method, which in turn would win converts to Copernicanism. Massively problematic for this apologia is that Galileo was attacking the anti-Aristotelian, empirically derived position of Tycho! (See Drake, 1960, xiii-xv) In this case, the psychological explanation of Koestler is preferable: Galileo's wrath is best seen as a pure outburst of ego kindled by frustration -- he was not able to get his theory of tides published and because of the ban on promoting a realistic heliostasis his work was in danger of being forgotten or co-opted by the Jesuits in defense of a geoheliocentric system. (Koestler, 1959, pp. 468-471)

Concerning the number of times The Assayer is quoted to illustrate the birth of modern scientific methodology and an antimetaphysical positivism, small wonder that quotations such as
the following are never to be found in introductory science texts. According to Galileo,

"You cannot help it . . . that it was granted to me alone to discover all the new phenomena in the sky and nothing to anybody else. This is the truth which neither malice nor envy can suppress." (Quoted from Koestler, p. 468)

Given Drake's apology for Galileo, small wonder that this passage does not occur in Drake's translation!

41. Given the traditional view that there were no voids between the celestial spheres, Tycho's reaction is, of course, understandable. My point, however, is that his "wasted space" argument is never mentioned in popular texts that emphasize Tycho's observational contributions. Typically, students are left with the impression that Tycho, Kepler, and Galileo all worked collaboratively and empirically to bring about the downfall of dogmatic geostasis.

42. Swerdlow, 1990.

43. Gingerich, 1973b, p. 90.

44. Note that for the relativist who advocates that we can always patch our networks when they fail to mesh with experience, one could argue that Venus, Mars, and the moon gradually shrink in size as they approach the Earth, thus accounting for the magnitude problem. However, no defender of geostasis suggested or advocated this auxiliary patch for obvious reasons: anything does not go.


46. Ibid., p. 6.

47. Ptolemy's reason for picking Mercury as closer to Earth than Venus, he says, has to do with Mercury's motion. Invoking Aristotelian cosmology, Ptolemy says in his *Planetary Hypotheses*, "The spheres nearest to the air move with many kinds of motions and resemble the nature of the element adjacent to them. The sphere nearest to universal motion is the sphere of the fixed stars which moves with a simple motion. . ." (Goldstein, 1967, p. 7) Hence, since Mercury's orbit is more erratic than that of Venus, it must be closer to the sublunar realm.

49. Gingerich, 1993, p. 27. For diagrams of the basic Ptolemaic system, see Figure 3.


54. Gingerich, 1975b, pp. 103-104. Gingerich demonstrates this by comparing computerized actual locations of planetary positions that would have easily been observable for Copernicus with what both Copernicus and Ptolemy predict. For instance, a prediction for Mars on February 22, 1523 is off by more than two degrees. According to Gingerich, it is obvious that Copernicus adjusted his initial parameters to match what were thought to be authoritative observations, "rather than to reform the accuracy of astronomical predictions."

According to Gingerich, "it is in fact shocking that Copernicus, with the accumulated experience of fourteen more centuries, did not come up with a substantial advance in predictive technique over the well-honed mechanisms of Ptolemy." Gingerich, 1975a, p. 90.

55. Erasmus Reinhold seems to be the original source of this claim. Although a Wittenberg astronomer and anti-realist in terms of referential commitment to astronomical devices used to save the phenomena, in Reinhold's introduction to his 1551 Prutenic Tables, he claims that the Copernican-based tables will show that "the science of celestial motions was almost in ruins" but that Copernicus "has restored it." (Quoted from Duhem, 1969, pp. 72-73)

56. Gingerich, 1975a, note D, pp. 92-93, emphasis added.

57. Quoted from Westman, 1975b, p. 293. Emphasis added.


59. Gingerich, 1993, pp. 171-172.


62. Ibid., pp. 85-86.
Tycho, of course, also recognized that a measurement of stellar parallax could separate the two systems empirically. For future reference (chapters 4 & 5), note that from one point of view the issue of empirical equivalence (EE) is decided right here -- the Copernican and Ptolemaic systems were not empirically equivalent. Kepler is emphatic about the role of Mars in this regard in his *Apologia Tychonis contra Ursum*, 1601 (Jardine, 1984, p. 141).

However, a defender of EE would argue that we should not be so hasty. At the time it was not possible to measure parallax for Mars, so a defender of Copernicus could argue for an auxiliary patch and that this discrepancy was not a falsification of heliostasis. On the other hand, when parallax for Mars at opposition was eventually observed with the invention of the telescope (a half a minute of arc, well below naked-eye resolution), a defender of geostasis could argue for some creative auxiliary patch. Perhaps, to take a deliberately bizarre example, the epicycle of Mars swells up only when the planet is being observed by the telescope! This shows that the real issue is not whether theories can be made empirically equivalent, but whether our attempts at auxiliary patching are rational, whether there is evidence for an auxiliary patch and whether the patch has positive or negative ramifications throughout one's adjudicatory trail, whether such patches are consistent with what else is well established or is intolerably inconsistent with it.

Relativists, using EE as a major premise for their positions, usually reply that my example in unfair. That the issue is whether it is always possible to find an adequate auxiliary patch, one that is consistent with what else is well established and saves the theory in question. With this response their position unravels. We cannot assume that it is always possible to find an adequate auxiliary patch, and with this response they have capitulated to my major thesis, that we can distinguish ampliatively between rational and irrational auxiliary patches. See the discussion of Doppelt below, chapter 4.

Also for future consideration (chapter 5), Tycho sees this as a corroboration of Mars's distance at opposition because by this time he is very impressed with Copernican linkages. The Ptolemaic system has its own planetary linkages. For instance, a Ptolemaic astronomer cannot adjust the rate of epicycle revolution independent of the sun's revolution about the earth. However, there is more freedom to adjust planetary parameters individually a posteriori, whereas frequency and size of retrograde motions are much more determined all at once by a heliostatic or geoheliocentric system. As is well
known, some philosophers of science have interpreted this all-at-once fixing of key parameters to be decisive evidence in favor of heliostasis. See chapter 4 on Lakatos. My argument will be that this is historical chauvinism. Although we find the key players in the Copernican episode gradually more and more impressed with heliostatic linkages, contextually we do not see consensus in the late 16th, and even early 17th, century that such linkages should constitute a decisive constraint on theory choice. Without clear evidence to the contrary, it is possible for one to argue that each planet should be treated individually to some extent. After all, even today we don't suppose that the planets must all contain the same elements, be the same density and size, etc. What is needed, and did eventually occur, is the weakening of auxiliary support for an approach that would allow one to continue to argue rationally that it is permissible to treat the planets separately in terms of adjusting parameters to match core observational problems. See point 5 above on Ptolemaic problems for Ptolemy's Aristotelian defense for treating planets individually.

68. At this time it was known to be part of the personal library of Matthew of Miechow, a Cracow University professor. Gingerich, 1993, p. 163.
69. Ibid., p. 164.
70. Gingerich, 1975a, p. 87; also see Koestler, 1959, pp. 572-3, and Palter, 1970.
72. Gingerich, 1993, p. 179.
75. This needs qualification, as we will see in detail later. A complete Tycho-like system was never fully articulated. However, being a geostatic transform of the Copernican system, it has customarily been assumed that astronomical tables based on this system would be close to the predictions of Copernicus. Furthermore, a geoheliocentric system will position Venus in such a way that it will show full phases like that predicted in the Copernican system. The latter is not controversial.
However, the following should be noted against the former. Given that a rough geometric model is not the same as a system that must be used to get down to the business of creating astronomical tables, and given that the rough Copernican system can be seen as a heliostatic transform of the Ptolemaic system (Price, 1959; Margolis, 1993), yet as fully articulated systems their predictions do not always agree, it could not be assumed that a fully articulated system would be empirically equivalent to the Copernican alternative.

Concerning the issue of terrestrial dynamics, there is no need for a non-Aristotelian one provided that the Earth does not rotate. In some versions of the geoheliocentric alternative, the Earth is made to rotate to cut down on the complexity of the system. For instance, William Gilbert advocated a Tychoic system with a rotating Earth, and Longomontanus, Tycho's disciple, developed such a system after Tycho's death. (Schofield, 1981, p. 180).


77. 1991, p. 266.
Chapter 3: Historical Consolation for the Relativist
Duhem and Kuhn

Duhem

An alternative framing of the relativist-anti-relativist debate often revolves around the notions of empirical equivalence and underdetermination. In its simplest gloss, according to the relativist, a rational decision regarding the competition between two theories is radically underdetermined because these theories are empirically equivalent or can be made so with enough effort and ingenuity in terms of auxiliary patching. Pierre Duhem's *To Save the Phenomena* and *The Aim and Structure of Physical Theory* have been massively influential in this regard. In *Aim and Structure* Duhem hammers home the possibility of auxiliary patching in blocking allegiance to a naive falsificationism. In *Save the Phenomena* he highlights the continuity, from Plato to the Renaissance, of the importance of empirical adequacy, destroying with massive historical detail the modernist myth of the "scientific night of the Middle Ages," that empirical inductive science began with Copernicus, Galileo, and Kepler. By showing the progressive continuity of the evolution of astronomy and physics from the ancient Greeks, through the Middle Ages, to the Renaissance, Duhem makes his famous plea that the aim of science should acknowledge the wisdom of Plato, Geminus, Ptolemy, Proclus, Posidonius, Simplicius, Maimonides, Aquinas, Bonaventura, John of
Jandun, Lefevre d’Etaples, Osiander, and of course
Bellarmine and Pope Urban VIII -- and all who "protested
without letup against the realism of thinkers like Adrastus
of Aphrodisias and Theon of Smyrna, the Arab physicists, the
Italian Averroists and Ptolemaists, Copernicus and Rheticus
themselves."1 According to Duhem, the former were "logical
and prudent" while the latter were guilty of an "impenitent
realism" and succumbed to "folly and delusions."

The realism-anti-realism debate is much too involved to
address in this thesis. Rather, my concern with Duhem will
be to show:

1. By oversimplifying the debate between the supposed
realists and anti-realists, Duhem misses the sustained
concern for consistency in astronomical theory by those
that he caricatures as naive realists. This point will
be important to the development of claims I will be
making in chapter 5 regarding the contextual force
during the Renaissance of the emerging constraints of
systematicity and parameter determination.

2. In stating the classic argument against realism, Duhem
set the stage for post-modern relativists to misuse
this argument by blanket application to all attempts to
support the traditional epistemological project.

3. Although Duhem does not himself attempt to undermine
the traditional epistemological project by applying the
argument against realism to all cases of rational
acceptance, he leaves us with only a vague and unarticu-
lated "good sense" as a method for accepting one
theory rather than another, and/or as a method for, in
the language of this thesis, continuing to pursue and
patch an adjudicatory trail or abandon it.

Although at the very end of Save the Phenomena, Duhem
claims that what Copernicus, Kepler, and Galileo were
actually doing was showing the importance of conceptual

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consistency and unity as pragmatic aims of scientific methodology, there is neither mention nor sympathetic attachment of these aims with the Arab thinkers when attacking their realism. Not until the very end of Duhem's historical story do we find, "... the truth which, little by little, they (the Renaissance realists) were introducing into science was that one form of dynamics, by means of a single set of mathematical formulae, must represent the movement of the stars, the oscillations of the sea, the fall of heavy bodies."2 As such, we are left with a very disjointed portrayal of history. As Duhem acknowledged, given the epistemological stance he wished to defend, it is ironic that it "received its greatest setback precisely at the time when astronomy and physics were making new and rapid progress."3 There is no explanation by Duhem as to why unity and consistency would suddenly become so important. Instead we find that from Plato to Kepler there were two competing views on the aim of science: the good guys who understood that empirical adequacy and "saving the appearances" should be the principal constraint on the "intellectual freedom of the astronomer," and the bad guys who, in advocating a natural philosophy, meddled with this freedom by forcing the astronomer to "reconcile ... hypotheses with the teachings of that philosophy."4 Thus, we are left with only irony and hand-waving false consciousness explanations like, well, the "greatest artists are not
necessarily best at philosophizing about their art," as to why realists like Copernicus, Galileo, and Kepler would pop onto the scene and be so successful.

As Lloyd, Gardner, and Ragep have shown, Duhem's bifurcated portrayal of history ignores the strong evidence that his anti-realist heroes made and were guided by many referential commitments. Lloyd, especially, shows in detail how Duhem's instrumentalists needed realist commitments to attack what Duhem considers naive realism. In other words, before one can decide what theoretical postulates are purely fictive calculation devices, one must have a realist theory as to what referential commitments are permissible given the nature of evidence.

For instance, in discussing Proclus, Duhem's star witness, Lloyd shows that Proclus' concern with withholding referential commitment to epicycles, eccentrics, and equants in the Ptolemaic system had to do with consistency with the realist commitment to the continuity of the spheres and the dynamical commitment to, in Proclus' words, the "common axiom of the physicists', namely that every simple movement is either round the centre of the universe or to or from that centre." (Recall the problem of the location of Saturn's equant point discussed in item 5 of Ptolemaic problems in the previous chapter.) Also according to Lloyd, unlike Duhem, Proclus was aware of problems with both the realist and the instrumentalist positions, that although
Proclus "argues against the realist way of taking these hypotheses on realist assumptions, he argues against the instrumentalist way of taking them also on realist assumptions." In other words, the instrumentalist must be wary of unwittingly being a naive realist! He must be aware that not only are referential postulates necessary, but also that one must have good reasons for the referential postulates used in separating out what are purely calculation devices.

On this point, consider Ptolemy’s acknowledged struggle in making his geocentric model consistent with the realist commitment that there should be no gaps between the spheres. In his Planetary Hypotheses, Ptolemy notes that after computing lunar and solar distances, and then nesting the epicycle radii of Mercury and Venus, such that the greatest distance of the moon will equal the least distance of Mercury and the greatest distance of Mercury will equal the least distance of Venus, "there is a discrepancy (gap) between (Venus and the sun) . . . which we cannot account for." According to Ptolemy, an adjustment must be made by increasing the distance to the moon slightly, such that "this distance to the Sun will be somewhat diminished and it will then correspond to the greatest distance of Venus." Thus, Ptolemy is forced to make the moon’s orbit more eccentric causing a significant clash with observation (see item 2 of Ptolemaic problems, previous chapter).
As Lloyd further shows, Duhem's instrumentalist reading of Ptolemy cannot be right given the "straightforward realist account offered in the second book of the Planetary Hypotheses." Nor does Duhem's instrumentalist reading of Geminus and Simplicius work either. All told we find numerous referential commitments made by these historical figures: that the earth is at rest in its natural place, that the movement of the heavenly bodies are circular and orderly, and the sphericity of the heavens as a whole, that there are no gaps between the celestial spheres, that the stars and spheres are made of a special kind of fire (Proclus), that the nature of the heavenly region is eternal, unchanging, homogenous, and transparent. Not to mention, of course, the obvious ampliatively derived realistic conclusions and agreement reached by most ancient astronomers on relative distances and sizes of celestial objects: that the sun is at a greater distance from the earth than the moon, that the planets are at a greater distance than the moon, that the sun is larger than the moon, and that the planets are not always the same distance from the earth.

The real historical continuity that Duhem misses, emphasized by Lloyd and Ragep, is a concern for and debate over consistency. According to Lloyd, in discussing Proclus for instance, the concern was for "a consistent physical account." And according to Ragep,
If Duhem had simply drawn the obvious conclusion from his own research that the major Greek philosophers and astronomers were committed in varying degrees to the proposition that the principles of mathematical astronomy must come from both mathematics and physics, he would have been more sensitive to the fact that the Arab thinkers ... were not so much naive realists as scientists interested in reconciling the inconsistencies in astronomical theory that they had inherited from the Greeks. ... (that) Arab astronomers reached a rather simple conclusion -- the mathematical models had to be consistent with the physical principles.13

Ragep is speaking primarily of the tension that exists in Duhem's Le Systeme du monde,14 where Duhem has become aware ("no doubt sadly," according to Ragep) of the authenticity of Ptolemy's Planetary Hypotheses. Even in To Save the Phenomena we can see Duhem struggling to sustain his purely instrumentalist interpretation of his heroes. Even here Duhem acknowledges an "apparent contradiction" within the positions of the holders of the instrumentalist aim, in that they were committed to numerous realistic-physical conceptions. In commenting on the apparent inconsistency in Peucer's geographical commitment to the earth as a globe and its real size compared to the tallest mountains, Duhem asks, "... how can he without risk of inconsistency, drop the requirement of truth when dealing with astronomical hypotheses?"15 Duhem's response to this is to invoke the then accepted celestial-sublunary distinction, and to cite, with apparent time-indexed approval of
the rationality of this move, the religious, metaphysical, and physical reasons for this distinction. It was permissible to be a realist about sublunary matters, but not celestial matters. But this appeal fails as well. Duhem’s instrumentalists also made referential commitments regarding some features of the celestial realm. Francesco Giuntini, a developer of astronomical tables from Ptolemy’s astronomy, argued that some combinations of eccentrics and epicycles must be considered more reasonable than homocentrics, because “the planets do not have constant apparent diameter, (so) . . . they are not always at the same distance from the earth. . . .(and) it must be admitted that some celestial revolutions do not have the earth as their center.” According to Giuntini, “this is not a mere assumption.”

This illustrates the same point cited previously: to be an instrumentalist one must first make some sort of realistic commitment, given what one counts as reasons and evidence, to separate what items in one’s theory will be counted as mere calculation devices.

Thus, the real issue, the continuity that Duhem missed, was conceptual consistency: How can Ptolemaic astronomy be understood as a complete system, a cosmology with a physical dynamics and a metaphysical view of the world. Viewed this way, we see that the marriage between Aristotelian physics and Ptolemaic astronomy was not an easy marriage, and that apparent instrumentalist stances were only a symptom of this
tension. Accordingly, where Duhem sees imprudent metaphysical constraints being invoked by the Arabs inhibiting progress in astronomy, we should see instead the constraint of conceptual consistency operating. Many of the Arabs may have been wrong by the standards of our modern physical knowledge as to what dynamics astronomical models should be consistent with, but only an historical chauvinism should keep us from understanding the validity of their concern.

Less noted but equally influential is Duhem's technical argument against realism that occurs in To Save the Phenomena. Throughout his exegesis of the history of astronomy, Duhem pummels the realist with the simple logical point that referential commitment to the concepts of a particular hypothesis is justified if and only if one is able to prove "that no other set of hypotheses could possibly be conjured up that would do as well at saving the phenomena." Such a demonstration can, of course, never be given. So realism is thus a quixotic quest. According to Duhem,

Never will our understanding lay hold of truth in so exact a manner that it may not grasp it still more exactly, and it will do so indefinitely. . . . Reason is a possibility ever susceptible of new development, . . . . no matter how numerous and exact the confirmations by experience, they can never transform a hypothesis into certain truth, for this would require, in addition, demonstration of the proposition that these same experiential facts would flagrantly contradict any other hypotheses that might be conceived.
And elsewhere, Duhem wrote,

To prove that an astronomical hypothesis conforms to the nature of things, it is necessary to prove not only that the hypothesis is sufficient to save the phenomena, but also that these same phenomena could not be saved if the hypothesis were abandoned or modified. ...(and) If the hypotheses of Copernicus succeed in saving all the known appearances, the conclusion will be that these hypotheses may be true, not that they are certainly true. To make the latter conclusion valid it would be necessary first to prove that no other combination of hypotheses could be devised which permitted the appearances to be saved equally well; and this demonstration has never been given.19

In other words, given any hypothesis, even if that hypothesis is far better supported by the evidence than an extant rival, there remain an infinite number of potential hypotheses that could save the evidential context equally well. It is not possible to separate the reasonable from the conceivable, if reasonable must mean establishing decisive conditions for truth. However, as Laudan has argued, truth and rational acceptance and/or pursuit between extant rivals need not covary, although historically many readers of Duhem have concluded that they must and haven fallen prey to relativism.20 Although even well-supported criteria of ampliative inference may be powerless in terms of supporting referential commitment, because given any rationally selected theory we must admit that it is only one member of a larger class of possibly true theories, given many evidential contexts such criteria can potentially allow
us to accept and separate the reasonable from extant alternatives. In other words, once one recognizes that traditional problems of confirmation and consequent extremist claims of empirical equivalence and underdetermination are parasitic upon the logical shortcomings of the traditional realist project, one can recognize that it is an unreasonable demand to place a burden on scientific methodology of separating the reasonable from the conceivable in terms of acceptance. According to Laudan, "The fact that a theory is deductively underdetermined (relative to certain evidence) does not warrant the claim that it is ampliatively underdetermined (relative to the same evidence)."21

Much skepticism and relativism are based upon failing to appreciate this point. A case can be made against blanket claims of empirical equivalence and strong underdetermination, and for rational acceptance, albeit not against potential empirical equivalence and in-principle underdetermination, and for truth. An epistemological wedge can be driven between transparently underdetermined and unwarranted truth claims founded upon bankrupt correspondence theories of truth and even, Laudan claims, nebulous notions of approximate truth,22 and relativistic stances motivated by the failure of such theories and notions.

Thus, however strong Duhem's technical argument against realism may be, it should not automatically indict the
traditional epistemological project of complete failure. Only if one views the matter through foundationalist spectacles need we despair over the purely logical truth that, in the language of this thesis, our hypertextual adjudicatory trails can be potentially patched "come what may," (Quine) or that we will never encounter a type of evidence that will allow us to be "decisive" (Feyerabend) or will leave us "speechless" (Rorty) at any node along an adjudicatory trail.

As noted in the previous chapter, Rheticus, Tycho, Kepler, and Galileo recognized that the Ptolemaic and Copernican systems were not empirically equivalent with respect to Mars. Even if they were de facto empirically equivalent with respect to naked-eye resolution, it does not follow that a future technological advance and/or change in strength and confidence in auxiliaries could not resolve the apparent empirical standoff. Relativists and the defenders of Legend that they criticize are often guilty of the same mistake, although they commit this mistake in an opposite fashion. Both fail to time-index the adjudicatory trails and the relative strength and weaknesses of particular auxiliary nodes at particular times. Defenders of Legend view history through the spectacles of well-supported modern auxiliaries and see the decisions of their historical heroes as virtually inevitable. Relativists note momentary standoffs between adjudicatory trails, the messy and unclear
nature of formative networks, and ignore that there is constant debate involving good reasons for individual as well as group pursuit, with eventual ampliative consensus reached concerning the rationality of the particular auxiliary nodes.

To maintain that the reaching of an eventual consensus is not rational, relativists are committed to the belief that any future advance could be matched by a good auxiliary patch of the adjudicatory trail now out of favor. But even relativists are willing to admit that there are "bad" auxiliary patches. If there are always potential good patches distinguishable from bad patches, this implicitly commits us to a process that would be used to make this distinction. Once parallax was measured for Mars, no one argued that it could be accounted for by a swelling of the epicycle at the exact moment a telescope was in use. Once Galileo claimed to have observed a full phase of Venus, as telescopes became better calibrated and confidence in this new instrument increased, while confidence increased that the moon and planets were physical places that reflected sunlight, and other aspects of Aristotelian cosmology became increasingly vulnerable, the reemission theory fell out of favor and it became very difficult to save Ptolemaic astronomy with respect to Venus. Hence, the relativist's position unravels because one needs a methodology to distinguish between good and bad patches.
Duhem, contrary to many whom he influenced, recognized this. According to Duhem, if one scientist decides to continue to patch a theory in the light of experimental contradiction, and another to boldly abandon it, neither scientist has the right to accuse the other of "illogicality." However, "Pure logic is not the only rule for our judgements." We do not require foundationalist logical certainty to make rational decisions. According to Duhem, in the light of experimental recalcitrance there are times that we can recognize the foolish "haste" of abandoning a "vast and harmoniously constructed theory" rather than simply making technical adjustments and searching for auxiliary patches. On the other hand, there are times when it is clearly "unreasonable" for a scientist to continue to seek auxiliary patches for a theoretical structure that resembles "the worm-eaten columns of a building tottering in every part."23

But how does one know when it is "proper" to patch or reject a theoretical structure? At this point Duhem opens the door to a robust relativism. Because "experimental contradiction always bears as a whole on the entire group constituting a theory without any possibility of designating (deductively) which proposition in this group should be rejected," it is left to "our sagacity the burden of guessing" what to do based upon "'reasons which reason does not know' and which speak to the ample 'mind of finesse'..
what is appropriately called good sense." Furthermore, it is often the nature of "genius" that allows the right move at the right time.\textsuperscript{24} Other than prohibiting self-contradiction, logic only demands a unity based on "an intuition we are powerless to justify, but which it is impossible for us to be blind to," for "only on this condition will theory tend towards its ideal form, namely, that of a natural classification."\textsuperscript{25}

Given what Duhem leaves us with, the indecision that an objective observer confronted in the first decade of the 17th-century regarding the choice between geostasis and heliostasis did not last forever, because "The day arrives when good sense comes out so clearly in favor of one of the two sides that the other gives up the struggle even though pure logic would not forbid its continuation." The most we can say in terms of articulating the conditions of a nonfoundationalist rationality is that the intuitive sagacity that allows us to reach such "fortunate reform" is enhanced by encouraging scientists to be "impartial and faithful judge(s)," for "nothing contributes more to entangle good sense and to disturb its insight than passions and interests."\textsuperscript{26} In short, Duhem astutely diagnosed a problem without offering a robust ampliative solution, leaving the impression that there was none.

Given Duhem's great influence, it is not surprising that we can find modern commentators concluding that
Copernicus, Galileo, and Kepler "sleepwalked" into a revolution (Koestler), or that it was their "leap-of-faith irrationality" (Feyerabend) that was the key to success, or that they made only a psychological "gestalt switch" to see how to "live in a different world" (Kuhn). For Duhem has left us only with a story about "great artists" who were not very good "at philosophizing about their art."

Thus, for a defense of the rationality of scientific change we seem to be left with three positions. First, although science may appear illogical, we know how to prepare the mind of scientists to enhance the likelihood that they will make the right decisions via intuitions (Duhem and to a large extent Kitcher). Second, we can see science as virtually an algorithmic process of simply comparing theories with experience applying fixed rules (Legend). Third, we can see science as a very complex process of the gradual ampliative strengthening and weakening of hypertextual adjudicatory trails of reasoning with the criteria used to judge strength and weakness themselves gradually emerging and learned via connection to the networks. The first two choices are easy targets of relativism. The third position is defended in this thesis. It is my contention that from a nonfoundationalist perspective it can be shown to be coherent -- it avoids circularity and infinite regress -- and historically well-supported.
According to Kuhn, an historically grounded post-positivist philosophy of science shows that scientific assessment and change are a "mixture of objective and subjective factors." Although it has become fashionable for both critics and supporters alike to emphasize the implications of the latter in Kuhn's work, let us begin by giving Kuhn his due by discussing what he at least sees as the objective and rational basis of the scientific enterprise. In this way we can not only better understand Kuhn's own "difficulties in understanding" both his critics and supporters and the "surprise and chagrin" over how his The Structure of Scientific Revolutions (hereafter SSR) has been read, but also properly understand the historical context of Kuhn's work and consequent reaction, as well as the shortcomings of Kuhn's analysis from the point of view of my thesis.

Even viewed sympathetically there is an obvious tension in SSR between what Kuhn has to say about "gestalts," "the switch of gestalt," "faith," "conversion experiences," and "viewing different worlds," on the one hand, and the role of "anomalies," the recognition of "crisis," "progress," and "problem solving" on the other hand. It is worth noting that the infamous reference to a "switch of gestalt" does not occur in SSR until midway through the book, and it is introduced at first with caution, as unoriginal, and the
insight of others in terms of a possible parallel with scientific change. Citing the work of Butterfield and Hanson, Kuhn says that although the "parallel (with paradigm shifts) can be misleading," it is nevertheless, "because it is today so familiar, . . . a useful elementary prototype." On the very next page Kuhn discusses the frequency of scientists facing "admittedly fundamental anomal(ies)," and on the pages that follow Kuhn talks of the "suggestive" nature of the analogy with switches in visual gestalt and current psychological research, but cautions that psychological experiments and concepts cannot be more than suggestive. According to Kuhn, in referring to the work of Bruner and Postman (the card experiment), and others, psychological experiments

. . . do display characteristics of perception that could be central to scientific development, but they do not demonstrate that the careful and controlled observation exercised by the research scientist at all partakes of those characteristics.30

For Kuhn, it is clear that there is an objective world that will inevitably penetrate our conceptual webs, given "prolonged exposure"31 to nature and the careful and controlled activity of scientists. In a nutshell, for Kuhn the locus of objectivity in the scientific enterprise is to be found in the following:
To experiment, to confront nature, to perceive anything meaningful at all, to overcome James's "bloomin' buzzin' confusion" we must have a framework, a "conceptual web," a "disciplinary matrix," a "paradigm." Once a paradigm is in place, nature will be articulated in earnest via the overlay of this rational grid. In the language of this thesis, hypertextual adjudicatory trails will be created, probed, and followed; puzzles will be confronted and patched with new auxiliaries; features, relationships, aspects, and pictures of nature will be articulated that were always there in one sense. Like potential music contained in the electromagnetic waves broadcast from a radio station, newly discovered relationships are objective, independent features of nature, but they require a receiver before they become manifest to human observers.

For Kuhn we should distinguish between "that something is" and "what something is." Successful paradigms are required to "pull out" connections and will do so given the backing of a scientific community. However, given enough time, because no paradigm is complete, because no paradigm will ever match reality in a one to one correspondence of concepts and details, it is inevitable that the world will overwhelm our rational grids. The very articulation of nature from a perspective will eventually reveal static and dissonance that cannot be assimilated. In Kuhn's language, "the very nature of normal research ensures that novelty
shall not be suppressed for very long. ... (for) normal science repeatedly goes astray," it is a "uniquely powerful technique for producing surprises," and it is a pursuit that although "not directed to novelties and tending at first to suppress them ... nevertheless (is) ... so effective in causing them to arise."34 Unlike disciplines that lack a careful and controlled interface with nature, because normal science will "explore nature to an esoteric depth and detail otherwise unimaginable. ... that exploration will ultimately isolate severe trouble spots."35

According to Kuhn, although it would be a mistake to consider that the features articulated by a paradigm represent truth in the traditional sense -- "there is ... no theory-independent way to reconstruct phrases like 'really there'"36 -- nevertheless "scientists (cannot) choose any theory they like so long as they agree in their choice and thereafter enforce it. ... (because) nature cannot be forced into an arbitrary set of conceptual boxes."37 An objective world resists our paradigm mapping, even though it is ambiguous to the point of allowing for different (but not arbitrary) successful maps and articulations at different times. This may be relativism, Kuhn says, but it is not "mere relativism" because it is "fundamentally evolutionary": scientific change is "unidirectional and irreversible. One scientific theory is not as good as another for doing what scientists normally do."38
In other words, Kuhn claims his philosophy of science is no more mere relativism than natural selection is mere randomness.

It is here that we will begin our criticism. Kuhn’s use of an analogy with natural selection theory to explain how he is an anti-realist but not a mere relativist is very revealing. To Kuhn’s credit he remains consistent on this feature of his philosophy of science in his major writings. However, it is in the defensive Postscript to the second edition of SSR that Kuhn’s position most clearly unravels in his attempt to defend an objectivist position with the connection between scientific change and evolution. According to Kuhn,

... scientific development is, like biological, a unidirectional and irreversible process. Later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied. That is not a relativist position, and it displays the sense in which I am a convinced believer in scientific progress. ... I do not doubt, for example, that Newton’s mechanics improves on Aristotle’s and that Einstein’s improves on Newton’s as instruments for puzzle-solving. But I can see in their succession no coherent direction of ontological development.

But what does Kuhn mean by "quite different environments"? If a point to point correspondence with natural selection theory is maintained, this would mean that the natural world literally changes under our feet so to speak and we must adapt with a new paradigm, just as life forms
must adapt to a changing local environment or become extinct. Read this way there is then "no coherent direction of ontological development," because there is not one world, one environment giving the same feedback. Instead, we must deal with "different worlds."

Although Kuhn tells us that an expanded theory of rationality must make some sense of this locution, Kuhn is obviously uncomfortable with interpreting the reference to different worlds ontologically. He tells us that he wishes to explore the "possibility of avoiding this strange locution," asserts that scientists are not literally geographically transplanted into different environments because the world stays the same during revolutions, and acknowledges that the more traditional view that only interpretations of fixed, objective observations change is, although "somehow askew . . . . neither all wrong nor a mere mistake." In these contexts Kuhn does not want to say that the natural world independent of human constructs changes as does the biological environment forcing species diversification, adaptation, and extinction. For this would be to make an ontological commitment about a theory-independent world -- a "really there" ontological pluralism. Kuhn does not want to say that at one time the objective world was really Aristotelian, but that the world changed such that Newtonianism solved more puzzles.
So what does Kuhn mean by different environments? In natural selection there is only local adaptation. Animals do not inevitably improve towards some ideal set of physical characteristics. Eyesight and flight in one environment are advantageous, in another disadvantageous, i.e., blind moles and fish that no longer need eyesight, and the ostrich and many other examples of birds that have evolved from ancestors that could fly. Even "complexity is only a broad trend, not a grand highway toward life's primary goal." Likewise, according to Kuhn, in science we must account for both science's existence and success by substituting "evolution-from-what-we-do-know for evolution-toward-what-we-wish-to-know." In natural selection theory it is appropriate to speak of local progress only; there is global progress only in the sense of more examples of local progress, more examples of adaptation and diversity. But no particular adaptation is better or optimal. Should we push the analogy with science this far? Evidently, the answer for Kuhn is yes.

Although Kuhn cautions us that the analogy can "be pushed too far," when it is combined with what Kuhn says about progress and problem solving we see that the analogy is "very nearly perfect." Since we must not think of problem solving success in terms of scope and cumulativity -- historical counter examples of problem solving loss make this untenable -- we can think only of inevitable problem
solving success in terms of "an increase in articulation and specialization." Although Kuhn will go kicking and screaming, we have fallen back into Feyerabend's notion of progress. Over time perspectives proliferate and competition between them may force greater articulation within each, but descriptively and normatively, diversity is all we see.  

For Kuhn, "a sort of progress . . . inevitably characterizes the scientific enterprise." Other than the total list of problems solved over time, we do not have better articulations and specializations in any cumulative sense. For "it is only during periods of normal science that progress seems both obvious and assured," and to some extent progress "lies in the eye of the beholder," because the victors in a scientific revolution tend to rewrite history and "are particularly blind to . . . losses as well as gains in scientific revolutions." Thus, it would seem that we can say only that a modern theory is better for a modern environment analogously to the trivial fact that driving a car and knowing how to tie a tie are better in a big city environment of today than would be knowing different types of snow and hunting tactics, skills more appropriate for early European homo sapiens. Apparently for Kuhn, other than an expansion of the sheer number of specializations and the depth of articulations there is no growth aspect to problem-solving; we just solve different
self-induced problems that may have a historical unidirectionality, but are also the result of massive historical contingency. Thus, for this reason the ability to solve problems cannot be "an unequivocal (argumentative) basis for paradigm choice."\textsuperscript{50}

We are, of course, left with this conclusion because for Kuhn the acceptance or pursuit of a new scientific theory is never simply a matter of applying objective, a-historical rules. The application of the normally agreed upon but inherently vague standards of greater problem solving ability, accuracy, fruitfulness, and simplicity cannot be decisive or absolutely compelling. It is here that we reach the heart of the matter and the diagnosis of false dilemma in need of a post-foundationalist therapy. For Kuhn, because rule-based decisions on evidence can never be absolutely decisive or compelling, the "conversion experience that I have likened to a gestalt switch remains, therefore, at the heart of the revolutionary process. Good reasons . . . (only) provide . . . a climate in which (conversion) is more likely to occur."\textsuperscript{51} In short, either we have an absolutely compelling process that "resembles logical or mathematical proof"\textsuperscript{52} or we must resort to the suggestive perspective of psychology.

Thus, according to Kuhn,

What is vague . . . about my position is the actual criteria . . . to be applied when deciding whether a particular failure in puzzle-solving is
or is not to be attributed to fundamental theory and thus become an occasion for deep concern. That decision is, however, identical in kind with the decision whether or not the result of a particular test actually falsifies a particular theory.33

There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision.44

What I am denying then is neither the existence of good reasons nor that these reasons are of the sort usually described. I am, however, insisting that such reasons constitute values to be used in making choices rather than rules of choice.55

Seen in its own historical context, after decades of an algorithmic progress myth of logical positivism and empiricism (Kitcher's Legend), Kuhn's analysis of scientific change in SSR as an historically contingent, messy process was liberating. He viewed his work as the preliminary stage of an expanded understanding of the rationality of science. From hindsight, we can even find Kuhn warning present day sociologists not to make too much of psychological and sociological variables:

To understand why science develops as it does, one need not unravel the details of biography and personality that lead each individual to a particular choice, though that topic has vast fascinations.56

However, for Kuhn acceptance or pursuit of a new perspective is not the result primarily of evidence but of "comfort," a sense of global satisfaction that connects and gives meaning to all the parts of a hypertextual
adjudicatory trail, to use the language of this thesis. Whether one talks of the decision to accept or pursue a new theory, to pursue an auxiliary patch for a theory in the light of anomalies, or that a theory has indeed been falsified by evidence, these decisions can never be "straightforward . . . or comfortable." Decades removed from the intimidation of logical positivism and the backlash it produced in Kuhn and others, we can now see the mistake in giving this much prominence to psychology: As fallibilists, scientists should never be completely comfortable with any ampliative justification -- all adjudicatory trails are nonterminating -- and this lack of complete comfort need not be made so much of as to engender a slide to socio-cognitive relativism. My argument is that even if we accept the problematic notion of a paradigm and its importance in defining a "horizon" of perception, paradigmatic hegemony does not entirely obscure the rational scaffolding of debate between rival paradigms. In fact, it is the very messiness that Kuhn wishes to draw our attention to that insures that holistic hegemony and incommensurability are never complete.

For Kuhn the influence of a paradigm is so overpowering that it redefines what science is in terms of not only substance but methods, problems, and standards. An individual scientist must grasp the whole before he or she can understand the parts. Any debate within the scientific
community over the parts is indecisive and a result of miscommunication until there is a switch of gestalt and an understanding of the whole. Scientists will "inevitably talk through each other," because "in learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mixture." For instance, in the case of the Copernican revolution Galileo and Kepler compiled "impressive evidence for the earth's status as a moving planet," but a switch to Copernicanism was incomplete until there were adjustments to the whole field of thought. In the case of the Copernican appeal to harmony, considering the entirety of the evidence drawn from it, it "is nothing if not impressive." But without adjustments to the whole field of thought such an appeal "may well be nothing" in the sense that it was merely an aesthetic, subjective consideration that appealed to an "irrational subgroup" of neoplatonic mathematicians.

My argument, brought to closure in chapter 5, is that even if the rules are different, scientists are capable of recognizing this difference and entering into debate over the merits of various constraints on choice. Furthermore, I will argue that what is typically referred to as harmony and aesthetics by commentators of the Copernican episode can be couched in terms of parameter determination and seen as an emerging, albeit debatable, constraint. Put simply and preliminarily, Copernicans were arguing that parameter
determination ought to be a constraint on theory choice and that it was an important node on an adjudicatory trail that supported heliostasis. Initially, non-Copernicans can be seen as either arguing that it was not important because of their non-realist goals for astronomy or that it was important but did not necessarily support heliostasis. Eventually, a substantial number of influential non-Copernicans were persuaded that parameter determination was an important constraint on theory choice, so important in fact that it drove the construction and pursuit of geostatic transforms (geoheliocentric systems) of Copernican planetary models. Kuhn, because of his holism and his write-off of parameter determination as subjective, sees early helio­static pursuit and commitment to its future promise as "only . . . made on faith" due to "only personal and inarticulate aesthetic considerations." My argument will be that although parameter determination as a constraint was not decisive, it was and ought to have been an independent feature relevant to theory choice, one capable of being judged itself by ampliative evidence.

Scientists during the Copernican episode were capable of understanding and debating the merits of a particular node on an adjudicatory trail independent of their theoretical allegiances. As we will see, the nature of this debate was not simply a matter of "talking through each
other," and it had a reasonable chance of ampliative closure. Finally, I will argue that too much has been made of the fact that parameter determination as a constraint emerged out of particular cultural biases. That parameter determination as a constraint emerged out of a cultural environment of mystical pythagoreanism and neoplatonism need not automatically indict this constraint as subjective and merely aesthetic. Percepts may be blind without some conceptual orientation, but they are quite capable of having evidential content even if the original conceptual orientation is later abandoned in favor of a new one. It is not unusual in the history of science to see that an important feature of modern methodology and/or theory had a suspect origin by modern standards. The concept of natural law evolved out of the ancient Greek notion of cosmos and the "fate" to which both humankind and the gods were subject. Linnaeus believed that he was establishing a system of organization that revealed God's created order. But by designating species as basic units, providing principles for their uniform definition and naming, and arranging them in a wider taxonomic system, he severed the natural order from interpretation in terms of human preference and convenience -- a crucial, preparatory conceptual shift for Darwin's development of natural selection theory.65

Similarly, Kepler was surely biased in his pursuit of heliostasis. Given all the confusing choices that lay
before him in the late 16th-century, it would have been impossible for him to objectively pursue each one, and his mystical neoplatonism (the sun as the "material domicile of God") was a major factor in his pursuit of heliostasis rather than a geostatic transform of Copernican models. However, he would not have pursued just any theory regardless of its evidential features. Heliostasis had to work. It also had to have been selected by Kepler because of certain constraints on choice that Kepler felt were important -- and later one did not have to be a neoplatonist to appreciate the accuracy of elliptical orbits and the Rudolphine Tables. As Kuhn even admits,

> Ultimately his version of Copernicus' proposal would almost certainly have converted all astronomers to Copernicanism, particularly after 1627 when Kepler issued the Rudolphine Tables, derived from his new theory and clearly superior to all the astronomical tables in use before.⁶⁶

The insight that relativists invariably miss is that being biased allows one to notice certain evidential features of the world, and our biases do not always overwhelm these features.
Notes for Chapter 3

3. Ibid., p. 61.
4. Ibid., p. 28.
5. Ibid., p. 61.
8. Ibid., p. 211.
10. Ibid.
12. Ibid., note 52, p. 211.
16. Ibid., p. 85, emphasis added.
17. Ibid., p. 110. This point allows Duhem as a Catholic to argue for the "wisdom of Bellarmine" and the excellent logic of the Pope against the "impenitent realism" of Galileo (pp. 107-113)
20. Laudan, 1984, 1990b, 1991, and forthcoming. Laudan's 1984 shows how a reticulation model (evidence, methodology, and cognitive values) of scientific rationality can be used to defend rational acceptance and simultaneously reveal the shortcomings of realism. His 1990b attacks Quine and is probably one of the best sources for showing that the mere
conceivability of auxiliary patches and future hypotheses need not impair decisions of acceptance between extant alternatives. His 1991 and forthcoming further articulate a comparativist methodology.

As near as I can ascertain, the historian Edward Rosen was the first (1939) to protest against the questionable dilemma invoked by Duhem. In his introduction to Copernicus's *Commentariolus*, he notes in passing that

. . . it should be indicated that Duhem's view is not without alternative. We are not limited to the choice offered by Duhem between realism and fictionalism: any proposition or hypothesis is either the Ultimate Truth or mere fiction. We may properly accept a hypothesis as the best statement at the moment and be ready to revise or to reject it when fresh empirical data require a modification of it, or a rival and superior hypothesis emerges to replace it. (Rosen, 1971, p. 33; 1st edition 1939.)

21. Laudan, 1990b, p. 291. Although there is considerable difference in Laudan's position and that of Kuhn's on the viability of the traditional epistemological project and our ability to delineate well-supported ampliative criteria that offer objective constraints on theory choice, it is worth noting that in the *Structure of Scientific Revolutions* Kuhn makes this same point concerning the problems produced by realism. According to Kuhn,

"If we can learn to substitute evolution-from-what-we-do-know for evolution-toward-what-we-wish-to-know, a number of vexing problems may vanish in the process. Somewhere in this maze, for example, must lie the problem of induction." (1962, p. 170)

22. According to Laudan,

Virtually all the proponents of epistemic realism take it as unproblematic that if a theory is approximately true, it deductively follows that the theory is a relatively successful predictor and explainer of observable phenomena. Unfortunately, few of the writers of whom I am aware have defined what it means for a statement or theory to be "approximately true". . . . This reservation is more than perfunctory. Indeed, on the best-known account of what it means for a theory to be approximately true, it does not follow that an approximately true theory will be explanatorily successful. (1984, p. 118.)


24. Ibid., pp. 211, 216-217.
25. Ibid., p. 220.

26. Ibid., p. 218.

27. Kuhn, 1977, p. 325. In this context, although Kuhn is referring to action on the part of "individual" scientists, it is clear that the philosophy of science he develops commits him to the subjective factors being present at the social level as well. "If subjective factors are required to account for the decisions that initially divide a profession, they may still be present later when the profession agrees." (p. 329) Kuhn's problem, as shown below, is that his philosophy of science leaves us (and Kuhn himself) in a quandary as to exactly how much power the subjective factors have. He does not deny that epistemic factors are present and move the scientific community to some extent. But he leaves the possibility open --- and his unintended socio-psychological relativists supporters clearly read him this way --- that the subjective factors always overwhelm the objective factors. My thesis does not deny that subjective factors are always present. However, it not only sees this as often a positive development in terms of supporting the discovery of objective factors --- subjective factors often help us focus on certain objective factors --- it denies also that subjective factors always overwhelm the objective factors. I believe Lakatos had the best handle on this; see the development below in chapter 4.


30. Ibid., pp. 110, 112. Kuhn's emphasis.

31. Ibid., p. 111.

32. Ibid., p. 112.

33. 1962, p. 55.

34. Ibid., pp. 5-6, 52, 64.


36. 1970, Postscript, p. 206. To complete the above analogy with electromagnetic waves and music: we will never be in touch directly with the electromagnetic waves, but only with the music. But the latter is to some extent at least a human artifact; it is not "really there" independent of the receivers we use to articulate its presence.


41. 1962, p. 120. According to Kuhn, "we must learn to make sense of statements that at least resemble . . . though the world does not change with a change in paradigm, the scientist afterward works in a different world."

42. 1962, pp. 117, 110, 120. According to Kuhn, "In the absence of a developed alternative, I find it impossible to relinquish entirely that (traditional) viewpoint." (p. 125) And, "Whatever he may then see, the scientist after a revolution is still looking at the same world." (p. 128)


44. 1962, p. 170.

45. Ibid., p. 171.

46. Ibid.

47. See Introduction, p. 2 and note 4; Chapter 1, p. 39.

48. 1962, p. 169, emphasis added.

49. Ibid., pp. 162, 166.

50. Ibid., p. 168.


52. Ibid., p. 199.


55. 1970, p. 262


57. In other words, one cannot even understand the rational arguments -- accept T because E, accept E because M, accept M because of another trail of T's and E's, etc. -- until one grasps something about the whole. See Kuhn's discussion of disagreeing scientists as members of different language communities, of the need for persuasion, translation, and
conversion, and how this process is never "straightforward, or comfortable." 1970, Postscript, pp. 201-203.

58. See Figure I-1 again, especially the last paragraph.

59. 1962, p. 102.

60. Ibid., p. 108.

61. 1957, p. 229.


63. Furthermore, it achieved sufficient independence to be used to criticize heliostasis! Or at least versions of heliostasis. One can see Kepler pursuing a better version of heliostasis, not only because of predictive inaccuracies generated by Copernicus's epicycle system, but also because of the complex number of circles used by Copernicus and the concomitant parameter flexibility this entailed. Thus, contrary to Kuhn's view on the hegemonic power of a paradigm, a constraint, rule, or methodology that originates within a paradigm, or has a certain attachment originally to a theory, may achieve a sufficient amount of independence to later be used to criticize that very theory.

64. 1962, p. 157.

65. Gould, 1993b. According to Gould,

   Linnaeus's definition fractured the conceit of a human-centered system with basic entities defined in terms of our needs and uses. Linnaeus proclaimed that species are the natural entities that God placed on earth at the Creation. They are his, not ours -- and they exist as they are, independent of our whims. . . . (Thereafter) species are real whether created by God or constructed by natural selection -- and Darwin's conceptual shift . . . required little revision in Linnaean methods. pp. 18-19, emphasis added.

66. 1957, p. 219. Note what a strange statement this is for Kuhn. Conversion based upon predictive accuracy alone? Without a complete conceptual shift to a new whole which incorporated physics, cosmology, and religion? Throughout his work he never quite resolves the question of the exact mix of subjective and objective features in scientific change and assessment.
Before developing my argument in greater detail regarding the role of parameter determination during the Copernican episode, we need to clearly distinguish it from positions that have made too much of parameter determination. Soon after Kuhn's *Structure of Scientific Revolutions* many philosophers of science labored with increased urgency to separate the rational-normative features of scientific change from the historically contingent, cultural, and socio-psychological features involved in any human endeavor. Implied in this task was a simultaneous acknowledgement of the important role historically contingent factors play in scientific change, and the importance of viewing such factors in a proper epistemological perspective to avoid the seductive trap of relativism.

Imre Lakatos summarized this challenge well in his paraphrase of Kant's famous remark, "Philosophy of science without history of science is empty; history of science without philosophy of science is blind." According to Lakatos, we ought to be able distinguish between the internal, rational-normative explanatory factors of scientific change and the external, socio-psychological supplementary factors necessary for a complete understanding.
of history. For Lakatos, this distinction serves two important functions. It provides the historian with a proper focus -- external history is just "confused rambling" until internal history is clarified\(^2\) -- and it provides a means for evaluating competing theories of scientific methodology, because a rational reconstruction of scientific change based upon a proposed theory of scientific methodology should reveal the greatest possible content in internal history and should not relegate important moments of scientific change solely to external history. In other words, assuming that science is ultimately a rational process, internal history is primary to external history. According to Lakatos, for all its insights regarding the importance of history, the problem with Kuhn's philosophy of science is that it relegates too much to external history; scientific change becomes a matter of "mob psychology."

In an analysis of the Copernican episode, for instance, it may have been necessary for Copernicus and Kepler to be pythagoreans and neoplatonists, but it surely was not sufficient. There must have been "good reasons" in some sense for allegiance to heliostasis before the external cultural factors could act on a scientist of this time. To use Kantian language, the external factors may have played a crucial role as the "occasioning cause" of the Copernican inversion, and help us understand why the change took place
when it did, but the internal factors were epistemologically prior in the sense of making any change at all possible.

In this chapter we will see that a critical ingredient of Lakatos's own theory of internal history -- the role of novel facts in the theory of scientific research programmes -- fails by his own method of appraisal; the rational reconstructive use of novel facts is degenerative by relegating too much of the Copernican episode to external history. We will also see that in spite of his explicit commitment to Popperian fallibilism, his philosophy of science involves a latent commitment to an implicit, unnecessary, vestigial infallibilism and this slip causes Lakatos to grant an extravagant epistemological role to parameter determination.

Using the distinction between internal and external history in two important papers written towards the end of his distinguished career, Lakatos was able to explicate and bring to completion the major themes of his philosophy of science. (1) Against his close friend, Feyerabend, and other relativists, Lakatos argued that ultimately science is a rational process; that there are normative-rational constraints involved in scientific change, and thus, anything does not go. (2) In addition to problems with Kuhn's work, a meta-methodological historiographical level of appraisal reveals that major theories of normative scientific methodology (strict inductivism, probabilistic
inductivism, falsificationism, and simplicism or conventionalism) are inferior to Lakatos's own theory of the methodology of scientific research programmes, in the sense that the former relegate too much to external history and shipwreck onto the shifting sands of relativism by their own standards.\(^7\)

To substantiate these claims, Lakatos argues that an historical test of theories of normative scientific methodology requires a meta-methodological standard of appraisal, and Lakatos is quick to argue that a simple meta-methodological falsificationism will not get us very far. Just as all scientific theories are born refuted, the rich and complex interplay between external and internal history always will result in refutations for any theory of scientific methodology.\(^8\) Lakatos thus introduces a purported new meta-methodological approach, one modelled on his own methodology of scientific research programmes. Accordingly, appraisal of competing normative scientific methodologies will be the result of whether or not the competing methodologies are progressive or degenerative, whether or not they reveal novel normative historical internal insights -- or simply collapse into relativism by relegating too much of scientific change to mere psycho-social, or cultural Weltanschauung shifts.

A year before Lakatos's untimely death, he presented a paper at the Quincentenary Symposium on Copernicus of the
British Society for the History of Science. This paper, which was the result of a joint effort with Elie Zahar, is doubly important because, in addition to providing a detailed historical test of Lakatos's methodology, it contains an "important amendment" (provided by Zahar) to the methodology of scientific research programmes, i.e., that empirical progress is shown by a research programme's ability to account for atemporal novel facts as well as temporal ones. Although Lakatos does not say so, this amendment can be viewed as a modification of the protective belt that surrounds the hard-core of Lakatos's meta-methodology, the hard-core postulate that ultimately science is a rational process. This amendment is necessary, because of the "unwelcome conclusion" that according to Lakatos's previous conception of "temporally novel facts," the Copernican programme cannot be seen as empirically progressive until 1616 (1610?), with the observation of the phases of Venus.

Prior to the Venus observations there were no spectacular confirmations for heliostasis. This is a major problem for Lakatos because although his theory does not demand falsifications -- all theories are born refuted, and supporters of a research programme are constantly adjusting the protective belt (auxiliaries) of a theory -- it does demand conceptual and empirical progress. Concerning the
latter, a progressive research tradition must not only make some novel predictions but some must also be observed.

Hence, without some amendment regarding empirical progress, commitments to the Copernican system prior to 1610 can be explained only by external factors. In other words, without some amendment there would be no clear, rational internal factors that key scientists prior to Galileo could recognize, no decisive foundation upon which external factors could act.¹²

Using the research of Gingerich, Kuhn, Neugebauer, Price, and Westman¹³, Lakatos is able to make an excellent case against inductivism, falsificationism, and simplicism as degenerative in the sense of relegating too much of the Copernican episode to external history.

The Copernican system was not a simple inductive generalization from either new or old observations, because both the Copernican and Ptolemaic systems were factually inadequate quantitatively in terms of predicting planetary positions.¹⁴ Hence, from an inductivist standpoint the Copernican and Ptolemaic systems were locked in an internal standoff and one is left with only external factors to understand why scientists accepted or pursued one or the other. Also, one cannot make a case for falsificationism -- that Ptolemy's system was irrefutable, and hence not scientific, and Copernicus's refutable, and hence scientific -- nor, if one grants that the Ptolemaic system was refut-
able, that a crucial deciding observation emerged that falsified this system at an early stage of appraisal of the two systems. The charge of irrefutability is based on the "adding epicycles myth." Astronomers of the time were not adding epicycles to epicycles to the Ptolemaic system to keep it from being falsified by the observational data. A recomputation by Gingerich shows that the Alfonsine Tables were based upon the strict Ptolemaic system of single epicycles. Furthermore, Copernicus added epicycllets to epicycles to adjust his system to known observational problems. To counter the charge that the Ptolemaic system was falsified, neither the Prutenic Tables, the observations of the phases of Venus, nor stellar parallax will help a falsificationist. The Alfonsine Tables were equal to or more accurate than the Prutenic Tables often enough to prohibit establishing a clear case of superiority of the latter. An appeal to the phases of Venus ignores Tycho's geoheliocentric solution, and the observation of stellar parallax occurred much too late to provide a normative reason for being a Copernican. Hence, from a falsificationist perspective the Copernican and Ptolemaic systems were locked in an internal standoff and one is left with only external factors to understand why scientists accepted or pursued one or the other. Finally, the exposure of the "80/34 myth" discredits an appeal to the much invoked simplicism. To pay for removing the equant, Copernicus must
use numerous epicyclets to bring the general heliostatic scheme in line with Ptolemaic predictions, and by the time of the Copernican system's evolution from the *Commentariolus* to the *De revolutionibus*, it is difficult to tell exactly how many circles the Copernican system had. Hence, Lakatos argues, simplicity becomes a matter of taste, and external historical factors must be appealed to solely to account for why key scientists committed to the Copernican system.

According to Lakatos, the methodology of scientific research programmes succumbs to the same degenerative fate unless his previous conceptions of novel facts and empirical progressiveness are amended. By this point in the development of his normative theory of scientific change, Lakatos had become aware of two problems. First, given the old conception of empirical progressiveness, we are at a loss to point to any decisive, "immediate," normative-internal reasons why any scientist in the middle or late 16th-century would consider committing to the Copernican theory. Second, in criticizing falsificationism, Lakatos had argued that crucial falsifying observational events are never temporally dramatic, but always the result of hindsight. So if temporal falsifications are a myth, why should temporal confirmations be any less so? Hence, Zahar's amendment.
Essentially Zahar's amendment allows for the consistent completion of the Lakatosian programme by showing that the Copernican system had "immediate support" if we recognize that 'novel' is not limited to 'new' phenomena; that previously known but problematic or unintelligible (apparently non-linked) facts of celestial motion gave dramatic support to the new theory, because they can be seen as distinctively intelligible by heliostasis in the sense of being directly explained and linked by that new theory, and they played no role in the construction of that theory. In other words, if there are well-known but apparently disjointed observations, and a new theory is able to link these observations with a single explanation, whereas a previous theory explains them separately, the new theory has immediate support.

Zahar and Lakatos then list previously known factors that they claim were not instrumental in the development of heliostasis, but nevertheless were handled by the new theory in a dramatic, straightforward manner. The most important of these relative to the historical context are: (1) retrograde motion, increased brightness of each planet during retrogression, and frequency of retrogressions for each planet, and (2) the fact that the calculated periods of the planets strictly increase with their calculated distances from the sun. According to Lakatos and Zahar both are distinctly intelligible under even the most rudimentary
version of heliostasis. Thus, even though there may have been a nondecisive mixture of subjective and objective motivating factors for Copernicus’s development of a heliostatic system, such as the important roles played by neoplatonism and pythagoreanism in Copernicus’s dissatisfaction with the use of the equant and the factual inadequacy of the Ptolemaic system, once developed there were immediate, rationally compelling reasons to accept Copernicanism. Furthermore, this cannot be revealed via inductivism, falsificationism, or simplicism, but only by the normative internal spectacles of Lakatosian philosophy of science -- heliostasis can be seen as progressive and geostasis as degenerating for a long time.18

In a strikingly revealing passage Lakatos gives an "historical thought-experiment" to corroborate this interpretation. Of paramount importance for my purpose, it is worth quoting in full.

Let us imagine that in 1520 -- or before -- all we knew about the heavens was that the Sun and the planets move periodically relative to the earth; but our records, because of, say the cloudy Polish skies, were so scanty that stations and retrogressions have never been experimentally ascertained. Because of his Sunworship and his belief in the Platonic heuristic, astronomer X proposes the basic Copernican model. Astronomer Y who adheres to the Platonic heuristic but also to Aristotelian dynamics puts forward the corresponding geocentric model: the Sun and the planets move uniformly on circles centered around the Earth. If so, then X’s theory would have been dramatically confirmed by observations carried out later on the coast of the Mediterranean. The same observations would have
refuted Y's hypothesis and compelled Y to resort to a series of *ad hoc* maneuvers (assuming that Y was not so disheartened as to abandon his programme instantly).

Zahar's account thus explains Copernicus's achievement as constituting genuine progress compared with Ptolemy. The Copernican Revolution became a great *scientific revolution not because it changed the European Weltanschauung*, not -- as Paul Feyerabend would have it -- *because* it became also a revolutionary change in man's vision of his place in the Universe, but simply because it was *scientifically* superior. It also shows that there were good objective reasons for Kepler and Galileo to adopt the heliostatic assumption, for already Copernicus's (and indeed, Aristarchus's) *rough model* had excess predictive power over its Ptolemaic rival.19

Lakatos would have done well to finish this rhetorical flourish with a quote from Kepler: "Copernicus alone gives an explanation to those things that provoke astonishment among other astronomers, thus destroying the source of astonishment, which lies in the ignorance of causes."20 In other words, key parameters of the Ptolemaic system must be "read off" from observation; there is a parameter flexibility to the system such that if certain features of celestial movement had not already been observed, they would not have been predicted. The flexibility allows *ad hoc* adjustments after the observations. By contrast, the Copernican system *locks us into the most important observations* of celestial motion; even if they had not already been observed, they would have been predicted. Thus, there is a rational sense of confidence that it is unlikely that the Copernican system

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would predict these phenomena by chance unless it was
going something fundamentally correct, whereas the adaptability of the Ptolemaic system engenders the suspicion that
its resuscitation is more the result of the ingenuity of its proponents in patching an inaccurate model.

At first glance this historical caricature may seem like Lakatos inadvertently slipped into his old conception of novel facts. Note that this example uses a temporally new observation. However, because Lakatos and Zahar are now using "novel, even though well known" to refer to facts or observations that could have been predicted prior to any observation, we will see that it is more fruitful to develop the train of thought in Lakatos's historical thought experiment, rather than engage in an acrimonious analysis of consistency and/or further definitional emendation. In this way we will see that the most telling aspect of this historical caricature is that if it is supplemented with details of the actual historical context, there were equally "good reasons" for early followers of Copernicus not to accept realistic heliostasis, that novel facts in the Lakatosian sense existed in the latter half of the 16th-century that would equally support acceptance of geostasis, or Tychonic geo-heliocentrism.

It is important to grant that Lakatos and Zahar have chosen items that were important in the commitment of some scientists of the time. The so-called "fixed symmetry" --
elegant determination of the aspects of retrograde motion (size and frequency) and the fixed ordering of planetary orbits that both were a direct consequence of heliostasis — had a profound effect on Copernicus, Rheticus, and Maestlin, and one can indeed follow a direct internal and external historical trail on this basis from Copernicus to Kepler.23

Problematic, however, in terms of what Lakatos and Zahar want to show, is that one can also follow a direct internal and external historical trail from Melanchthon, Reinhold, Peucer, Praetorius, Ursus, and Wittich to Tycho and the development of geoheliocentrism, complete with the ability to incorporate novel facts in the new and old Lakatosian senses. As the work of Westman and Gingerich has made clear, what is most striking about the reception of the orbital interconnectivity and "symmetria" of the Copernican system is the "silence" by which it was greeted initially by virtually a whole generation of astronomers concerned primarily with the pragmatic, methodological aspects of planetary motion, and the struggle of the best minds of the time to reconcile a host of discordant features which affected not only the acceptance of heliostasis but the continued pursuit of geostasis.24

Hence, one could be unimpressed for cosmological or methodological reasons, or both. From the standpoint of modern astrophysics, parameter determination is one of the most important constraints on theory evaluation. Witness
the dissatisfaction with the Standard Model of quantum field
theory and its application to the origin of the universe.
It can solve many problems, not the least of which is the
temporal sequencing of the origin of the laws of nature
microseconds after the big bang. However, 18 numerical
parameters are not fixed and must be taken from experiment.
Thus, we find many physicists pursuing the "God equation"
and esoteric theories such as string theory that will lock
in the parameters all at once while explaining all the
things the Standard Model does. According to Steven
Weinberg, a contributor to the unification of the electromagentic and weak nuclear forces, "The intellectual investment now being made in string theory without the slightest encouragement from experiment is unprecedented in the history of science." An analogous situation would have arisen if the majority of Ptolemaic astronomers had "jumped ship" and begun to work on heliostasis immediately after the publication of *De revolutionibus* without the slightest encouragement from any new observation supporting Copernicus.

Either the majority of the astronomers of the Copernican episode were irrational, or there was an obvious difference that Lakatos ignores in the weighing of this constraint, a difference in the constraint's efficacy between our time and this historical episode. Parameter
determination as a constraint on theory choice is not going
to be impressive unless it is backed by a clear cosmological commitment and is part of a gradual strengthening of a hypertextual adjudicatory trail of inference. As we have seen, a Ptolemaic astronomer had a cosmological basis for a certain amount of "individualization" of planetary motion within a geostatic system. 26

In short, parameter determination was hardly a fixed, universally agreed upon constraint for theory appraisal. Lakatos not only grants an extravagant epistemological status to parameter determination, but also modernizes it, presenting it as if it were a timeless, fixed constraint recognized and agreed upon by all generations of scientists.

To see that the Lakatosian interpretation of novel facts does not work, let us supplement his Gedanken drama by adding an historically plausible astronomical character. Suppose that in the middle of our story is a Tycho-like figure, who in spite of Lakatosian cloudy skies is making the most accurate observations of planetary positions since humankind began to survey the heavens systematically. He knows that his observations show that the rough models of geostasis and heliostasis are only qualitatively accurate; that precise measurements show that both systems are inaccurate quantitatively. 27 He knows that these inaccuracies are roughly equivalent over the course of a year, but that on any given date one system would come closer to predicting an actual planetary position than the other. He
also knows that geostasis is consistent with the best physics of the day (Aristotelian dynamics), but seems to violate a major accepted pythagorean mathematical postulate, i.e., the planets do not seem to move uniformly about their perfect circles in relation to the earth. He knows that the advocates of heliostasis are attempting to get much mileage from this apparent defect of geostasis and knows that this was a key motivating factor for the creation of heliostasis in the first place. Furthermore, he knows that heliostasis can account for the fact that planets appear brighter at various times during the course of a year, and that in doing so it makes a strange prediction -- that planets will show retrogressions, the number of which depend on the distance of each planet from the sun. On the other hand, he knows that geostasis predicts that a careful observation of the stars in a six month period will show no stellar parallax, and that heliostasis makes the strange prediction (given the then widely accepted estimate on the size of the universe) that there will be stellar parallax. Finally, he knows that because of their epistemological realism, the followers of heliostasis are very impressed with the orbital interconnective "symmetria" that follows from their theory, but that because of the accepted "modest ignorance" or instrumentalism in astronomical matters, many in the respected scientific community are not only initially unimpressed by this, but bothered more by the many inconsis-
tencies of heliostasis with the best physics of the day, not the least of which is not a single, but a triple motion of the "lazy, sluggish" earth.

Now, suppose our Tycho-like figure gets word of the dramatic confirmation from the coast of the Mediterranean of retrogression, but suppose at the same time during the last six months he has been carefully applying his observational skills looking for stellar parallax, and finds none. Suppose that during this time he has also observed a supernova and a comet, and that after some time is able to determine that the latter shows no retrogression, that neither can be sublunar phenomena because they show no parallax, and that the comet must somehow penetrate the solid celestial spheres that both the followers of heliostasis (Maestlin-like figures) and geostasis (Praetorius-like figures) accept. Suppose now that as all these observations are announced (more accurate measurements of planetary position, retrogression, but no stellar parallax, a comet with no retrogression, etc.), both camps begin to tinker with their systems. The followers of heliostasis add epicycles, eccentrics, epicyclets, and indulge in numerous parameter adjustments to bring their system more in line (it is still shockingly inaccurate, especially for Mars) with the new observations of our Tycho-like figure. They also explain that the apparent lack of stellar parallax can be understood by an appeal to a larger universe and they
win support for this (ad hoc, post hoc) move from radical (Bruno-like) theologians who are arguing for a decentralized, non-hierarchial cosmos and a greater conception of God's creative ability. On the other hand, the followers of geostasis also add epicycles, eccentrics, and equants bringing their system more in line with the new observations, and explaining (ad hoc, post hoc) planetary retrogression and variations of brightness.

Put this way, Lakatos cannot successfully argue that the heliostatic moves were a continuation of the positive heuristic of heliostasis due to the dramatic progressiveness of the retrograde motion prediction, and that the geostatic moves were ad hoc, post hoc examples of degeneration. From the perspective of the middle and late 16th-century the "could-have-been-predicted-prior-to-any-observation" game could have been played by both sides. The geostatic moves can also be seen as a continuation of a positive heuristic due to the dramatic confirmation of the no-stellar-parallax prediction.\textsuperscript{32} Lakatos cannot argue that "in the long run" we see that this was only a momentary success for geostasis (just as the momentary success of a planetary prediction), for then the claim of immediate success in the sense of decisive, internal, normative appraisal factors falls apart because excess predictive value in the middle and late 16th-century was a matter of context, interpretation, and contingency.\textsuperscript{33} But it gets worse.
Our Tycho-like figure is very impressed, as are other influential astronomers, with the lack of the equant in the heliostatic system. He knows, however, that others are impressed primarily due to their instrumentalism, that the equantless heliostatic system is intriguing as a "hypothesis," as constituting a new set of calculation devices used to save the phenomena, and that many of these astronomers are busy following the "reasonable" strategy of seeing to what extent these devices can be incorporated into geostasis. Also, because of his own realistic tendencies, our Tycho-like figure, (1) knows that his observation of the comet is problematic for both systems, and especially for any system that maintains celestial spheres, (2) knows that although the modified heliostatic system preserves the pythagorean requirement of uniform circular motion, it does so at the expense of proposing the realistic absurdity of objectless centers of motion, and (3) he is very disturbed by the "big universe" claim of the heliostatic apologists. The latter makes no rational sense; what would all this new "wasted space" be for? Finally, he knows better than anyone else that the factual accuracy situation has not changed: During any given month one system would be more accurate than the other with a slight, but nondecisive overall edge to heliostasis.

Perhaps our Tycho-like figure knows too much! Eventually he sees a good reason to develop a new system, a
geoheliocentric system\textsuperscript{37} that will not only be conceptually progressive in reconciling and maintaining the reasonable elements in both systems (the equantless elegance of heliostasis, and the well established, traditional Aristotelian physics), but empirically progressive as well, predicting the same novel facts as heliostasis -- retrogression,\textsuperscript{38} planetary brightness during retrogressions, systematic linkages in planetary distances, and a full phase for the planet Venus -- in addition to being consistent with the novel fact (Zahar's amended version) of no stellar parallax. It will also be the first system that will have a physical body, the earth, at the exact center of the celestial sphere.\textsuperscript{39} Thus, Tycho's new system can be seen as a continuation of the heuristic power of geostasis and Lakatosian apologists are wrong about geostasis having a weak heuristic at this time.\textsuperscript{40}

Based on the history of late 16th-century astronomy, it is clear that Lakatos and Zahar have conveniently ignored Tycho\textsuperscript{41} and certain aspects of the planetary motion debate that were of importance to the best astronomers of the time. According to Owen Gingerich,

Today we view Tycho's scheme as a giant step backward, but we are nonetheless disconcerted by the fact that it was proposed by the most innovative astronomical observer since antiquity. ... [and that] We can well imagine that Tycho believed he was taking a great step forward toward understanding the physical reality of the universe...\textsuperscript{42}
And, according to Westman, when seen in the full light of the empirical and conceptual problems facing the best astronomers of the time, "Here was no mere philosophical conjecture, but a world system with potentially serious predictive value."43 Clearly, Tycho made rationally constrained moves in attempting to reconcile conceptual and empirical difficulties.44 Like Kepler, and unlike the Wittenberg astronomers who were very influential in the latter half of the 16th century, he began to see the importance of parameter determination as a constraint in astronomy, and of moving beyond mere "hypothesis" and "moderate ignorance" to unification of physics and astronomy. With hindsight we see that he made the wrong choice in terms of which physics to preserve. Prompted by the realization that the comet of 1577 would have crashed through crystalline spheres, Tycho was faced with the apparent choice of preserving the harmonious orbital interconnectivity of heliostasis (its "more exquisite order") and the stability of the sluggish, heavy earth. As we have seen there was "progressive" support in the Lakatosian sense for Tycho's choice: Aristotelian dynamics as well as no stellar parallax. In Lakatosian language, were there decisive reasons for Kepler's neoplatonism to act and counter this progressive support for geoheliocentrism? Would not a comparative analysis of Tycho's 1588 De mundi aetherei recentiorbus phaenomenis reveal better or equally
good appeals to internal reasons as Kepler's 1596 *Mysterium cosmographicum*? No doubt yes, and hence such a comparative analysis would show more degeneration for the Lakatosian theory of scientific appraisal from a meta-methodological perspective.\(^\text{45}\)

Previously Lakatos would have argued that "immediate support" here does not mean immediately decisive or obvious to all concerned, but only that some good reasons were there to act on a scientist who was receptive to them. As he had argued previously, scientific change involves long protracted battles. There are no immediate quick kills, and it is difficult, if not impossible to give "advice" as to exactly when a degenerating research programme should be given up. But at this point in the development of his work, he does not seem to have his heart in this any longer after it has become apparent via several critical assaults on his philosophy of science\(^\text{46}\) that talk of progressive and degenerative research programmes is subject to the same historical contextualism and contingency, hindsight and timing arguments that Lakatos had levelled against Popperian falsificationism. If degeneration is such a slippery concept that it is impossible to pin down a time when there is decisive degeneration versus decisive progressiveness, then of what help is it in identifying the internal history which then avoids the necessity of appealing too much to external history? Something new is needed, a new
Archimedeans for the clear internal, decisive rationality for Copernican superiority.

So sensitive is Lakatos at this point to the charge of relativism that he is compelled to argue for immediate and decisive progressiveness of heliostatic allegiance based on the parameter determination features of heliostasis. His examples are of the middle and late 16th-century rather than the early and middle 17th, and he has appealed to a constraint that was not yet well accepted. Here, like the positivists and logical empiricists before him, he has succumbed to the trap of implicit infallibilism, that an adequate analysis of scientific methodology must reveal those universal, internal fixed principles that when applied properly give us rationally compelling and decisive reasons to follow one path rather than all others.\textsuperscript{47}

Lakatos is aware (of the "unwelcome conclusion") that all he has done previously is to show that there were good reasons to pursue heliostasis, not that there were not equally good reasons to continue to pursue geostasis, which means, he thinks, that one must appeal to external history as the decisive factor to account for why scientists would commit to one or the other. For Lakatos, commitments based on external reasons are not out of the question (we often in fact need these to supplement internal analyses), but a sound rational reconstruction should show the internal good reasons that allowed the external to be relevant, that were
rationally instrumental in activating the external reasons. But need the internal reasons be rationally decisive constraints? Now it appears that he thinks so; otherwise his position produces by his own demarcation criteria an unwanted relativism. If Lakatos is not arguing that the internal reasons that he and Zahar have identified were "decisive," that any rational scientist of the time "ought" to have switched to Copernicus, then what is all the talk about "superseding" based on internal reasons?\(^4\) If he is arguing this, then, as we have seen, he is wrong about there being rationally decisive internal reasons in favor of heliostasis in the late 16th-century. In either case his position is reduced to the position of the moderate relativist\(^5\): There were good reasons to develop heliostasis, but there were equally good reasons to continue to explore some version of geostasis, and that for external reasons the right men at the right time picked heliostasis and produced the refinements (elliptical orbits, a new theory of motion) that made heliostasis the preferred choice in the 17th century.

Typically, infallibilist responses to relativism involve two mistakes: (1) the assumed need of having rationally compelling and decisive commandments -- some clear "sign" -- for early theory choice in the sense that one is better to develop than all others; (2) which in turn is a symptom of a larger mistake, an "infallibilism
fallacy," the assumed need that some final resting point of certainty along an adjudicatory trail is needed to answer the relativists. Since Lakatos has made much of the fact that relativism emerges primarily when one's standards of appraisal are too utopian, my infallibilist charge should be a rather surprising claim. According to Lakatos,

...utopian scientific standards either create false, hypocritical accounts of scientific perfection, or add fuel to the view that scientific theories are no more than mere beliefs bolstered by some vested interests. This explains the 'revolutionary' aura which surrounds some of the absurd ideas of contemporary sociology of knowledge: some of its practitioners claim to have unmasked the bogus rationality of science, while, at best, they exploit the weakness of outdated theories of scientific rationality.50

However, as I have argued, Lakatos blinks when his fallibilism is challenged by relativism. Now a clear sign is needed or we are "relativists in disguise."

By the late 16th-century, parameter determination was emerging as an important consideration for theory evaluation, but it was neither universally accepted nor did it in isolation clearly select heliostasis over geostasis. Lakatos, in his search for a universal, fixed scientific methodology, has imposed a modern standard of theory appraisal on a previous time, ignoring the transitional, formative nature of that time and the as yet unclear role of parameter determination.
To prepare for a proper understanding of parameter determination in the Copernican episode, and its role as a gradually strengthening node along an adjudicatory trail, we can conclude this chapter by sketching a consistent fallibilist position and show in the process that Lakatos's implicit infallibilism slip is symptomatic of much of post-Kuhnian philosophy of science. As I have argued, if we are to take fallibilism seriously we must acknowledge that science is a somewhat messy, meandering, contingent process, but that in doing so, the fallibilism and open-endedness this implies is not an endorsement of irrationalism and/or relativism.

A simple way of providing such a sketch is to see how some post-Kuhnian philosophers of science attack avowed fallibilist positions. Larry Laudan has provided a fruitful model of fallibilistic scientific deliberation with his reticulational model of scientific change. Using this model we can view science as a process where scientists either do (during their best moments) or ought to follow adjudicatory trails among empirical results and theories, methodologies, and aims or cognitive values in resolving disagreements. Empirical disagreements (theories or facts) are resolved by applying agreed upon methodologies; methodological disagreements involve appeals to agreed upon aims and cognitive values; disagreements in cognitive values are resolved ultimately by appeals to factors that are also
empirical and methodological. All justificatory appeals are instrumental, relational, ultimately empirical (at either a primary or meta-level), and hence fallible. No node on the network has an intrinsic status. According to Laudan, as we probe nature for its secrets we presuppose and impose in the process at any given time a network of theoretical, methodological, and axiological stances. In the process we may not only find weaknesses in our theories and auxiliaries, but learn about our methodologies and aims (neither are trans-temporally fixed) as well; discoveries may be produced that expose weaknesses in the nodes of our networks, in either our methodologies or aims or both. In this way scientific change is piecemeal, comparative, and rational. All three levels need not be questioned at the same time. This process will allow for a relative best selection from a number of alternatives most of the time, but is of course still subject to "underdetermination" in terms of some ultimate best solution. It is imperfect and nonutopian epistemologically. However, as fallibilists we do not attempt to separate the reasonable from the conceivable -- a Platonic quixotic goal -- but only the most reasonable and thus most reliable given the alternatives.

Laudan's position has been attacked by alleging that his position cannot escape either a vicious circularity or infinite regress, or an implicit foundationalism. In short, either the adjudicatory trails scientists are alleged to
follow in Laudan's scheme are "aimless," and hence endless, or there must be some implicit, overriding meta-meta-criterion of "self-justificatory" judgment to determine the rationality of any given trail and/or the rational, indubitable terminus of any given trail.

Typically, only a formal argument is offered in these criticisms -- there is hardly ever any scientific flesh added, any actual analysis of real scientific cases. A good example is Doppelt. The only examples he uses are those of the people (Shapere and Laudan) he criticizes. In both cases his argument reduces to something like this: Both S and L have shown an actual historical case where scientists had some good reasons -- because of their acceptance of new theories T1, T2, T3 -- to change allegiance to methodologies from M1 to M2. But, according to Doppelt, logically this does not prove that there were not also some good reasons to maintain acceptance of M1. As a bare, formal, logical fact this is true. But Doppelt ignores, or is ignorant of, all the good reasons the scientists of the time had for accepting T1, T2, and T3, and the rational force that these reasons produced in favor of M2.

So, stripped of its constricted logic what does this criticism amount to? Given any particular item of disagreement, scientific discussions are potentially endless, but are as a matter of historical fact not endless because a scientific community will eventually reach agreement about a
particular node along an adjudicatory trail. Consensus is reached, but seen in the light of the historical context it is not "mere" consensus as relativists often make it out to be. Science is an open-ended process. This process is potentially endless, but clearly not aimless, because directed adjudicatory trails are followed; decisions are based on reasons that are in turn based on other reasons, that stopping points of consensus are reached along adjudicatory trails (facts, theories, methodologies, aims.) Key stopping points are fallible — we take a risk and might be wrong in thinking that any given stopping point is a good reason, a good place to stop, and some day, based on new developments, we might need to reevaluate this decision.

Doppelt, for instance, makes much of the fact that when a choice is made we may or do lose something. But first of all what we may lose may not be weighted very highly in the light of the scientific developments of the time. Tycho was disturbed at first by the prospect of giving up crystalline spheres, but as we have seen, the comet of 1577 in conjunction with other considerations changed his mind. Doppelt puts no independent scientific flesh on his a priori arguments, and hence is misled by the trivial logical fact that in every decision that involves great risk a different trail is followed than one that could have been followed. Second, how much infallibilism is implicitly assumed by this notion of loss, becomes clear when Doppelt tinkers briefly
with the notion of "epistemic possibility." After bringing up the issue, Doppelt admits that "Clearly the rationality of a choice to accept a new theory, standard, etc. is not thrown into question by other 'possible choices' of aims, theories, standards, etc. unknown at the time." This would be like saying that Copernicus and his initial followers were irrational because they did not consider the possibility of elliptical orbits. But this unrealistic requirement is precisely what is also behind Doppelt's allegedly more moderate requirement that the rationality of a choice is called into question because an "existing rival," may have possible epistemic advantages and may, if followed, lead to actual gains. This is similar to saying that the rationality of a decision is thrown into question, because although a person followed a valid trail of reasoning, there existed or may exist a different valid path with a different conclusion. Doppelt's a priori analysis reduces to the trivial fact that it is possible to have two valid chains of reasoning with contradictory conclusions. In following one rather than another we may be disappointed when we find that our conclusion does not work when tested against the world, but it does not follow either from our being wrong or the possibility of our being wrong that we acted irrationally.

What the critics of Laudan's position are clearly assuming is that unless we find some absolute, assured
resting point, some indubitable rational terminus, reason ends up chasing its own tail and the slide to relativism and irrationalism is inevitable. Western philosophy, both empiricist and rationalist traditions, has suffered much from this "Platonic fallacy." Whitehead may be right that all Western philosophy is but a footnote to Plato, but in this sense it is no longer something to brag about and it is time to begin writing some new chapters. What I believe is needed is a new metaphor for rationality: the open-ended, yet constrained following of hypertextual adjudicatory trails of reasoning with fallible, empirically-linked decisions made along the way, without sky-hook clear signs that we are on the right trail.9

The justification of science as a rational process is not much different from the answer one must often give to perceptive first year logic students. Why be logical they ask? If our premises are often in doubt, and hence valid deductive arguments can have false conclusions, and whether our premises are in doubt or not, invalid deductive arguments can have true conclusions, why prefer valid reasoning? The answer, of course, is that in the long run valid reasoning forces us to test our ideas against the world, whereas invalid reasoning offers an attempted safe haven from confrontation with the world by offering excuses for not testing our beliefs. The justification of the rational framework offered by the adjudicatory trails
followed by science is similar: It is a process that forces interaction with the world; a process that allows the world to "kick back" empirically and endlessly.60

In post-Kuhnian philosophy of science it is fashionable not to generalize too much from the Copernican episode. It involves only one kind of science and a rather special, "clean" mathematical one at that. However, much can be learned by observing the meandering trails followed by late 16th-century astronomers, and as we have seen, the tension in Lakatos's response to this episode, between his avowed fallibilism and the alleged immediate clear sign offered by novel facts and parameter determination.

As Laudan has also pointed out, almost all discussions of scientific appraisal assume a uni-contextual modality.61 They assume that for an individual scientist or a scientific community to be rational in using a particular theory there must not only be some clear transtemporal and trans-cultural criteria that are being applied, but the theory in question must be "done" so to speak; it must have already been completely successful in fulfilling those criteria. As we have seen, philosophers like Feyerabend can then use a little history and make short work of theories of scientific appraisal based on this assumption. In other words, it can easily be shown that scientists have often pursued theories that have not yet lived up to proposed criteria and ought not to be accepted yet based on those criteria.
However, by distinguishing a context of rational pursuit from that of acceptance one can show, as I intend to do in the next chapter, that although scientists had good reasons not to accept heliostasis during the Copernican episode, it would have been irrational not to pursue it. Heliostasis created an "immediate stir" in terms of the way it solved the core problem situation of astronomy, and contra Kuhn's leap of "faith" on the part of those converted to a new perspective, or Feyerabend's pursue-anything-because-the-situation-was-such-a-mess, we find astronomers of many different persuasions studying the heliostatic model and acknowledging its parameter determination features in terms of the core problem situation of astronomy. Whereas Kuhn finds only faith and aesthetics, Lakatos, by assuming that acceptance is the only valid cognitive modality and in response to the criticism of Feyerabend and others, is now looking for some clear sign for why Copernicus superseded Ptolemy.

Clearly, rational constraining trails were followed by the key players. But it was a time of transition and emerging constraints, and because new trails were not completely worked out, there was no clear choice. It was a time of new empirical developments (comets, novae, and more accurate observations), and an emerging appreciation of parameter determination and the possibility of unifying physics and astronomy. Not until several decades into the
17th century were the trails sufficiently worked out and key developments present (Kepler's development of elliptical paths for the planets and his Rudolphine Tables; Galileo's explication of a new dynamics and observations of the moons of Jupiter, mountains on the moon, and spots on the sun) that a rational threshold or crescendo was reached, and the stage set for heliostasis being accepted as a preferred choice from the alternatives.  

In other words, when seen in the light of the meandering and transitional developments of the late 16th-century, we can understand the powerful (psychological and rational) persuasive nature of the work of Kepler and Galileo in the first decades of the 17th century. Tycho, as we will see, was already impressed with the parameter determination features of heliostasis. If he were still alive, would he have continued to promote his version of geostasis, say in 1627, once he had used Kepler's astronomy and the Rudolphine Tables to predict a position for Mars, assimilated Galileo's dynamics and the implications for a moving Earth, or used a telescope himself to see the evidence that the moon and the sun were physical places and to make more accurate measurements of parallax? Would the man who was willing to give Copernicus a "fair hearing" not be powerfully affected by these developments and consequently troubled by the censoring of heliostasis by the Church? Or, would he, as the relativists maintain is always equally rational,
continue to seek auxiliary patches for geostasis, "maintain­ing obstinately at any cost" and bearing any "price of continual repairs and many tangled-up stays, the worm-eaten columns of a building tottering in every part. . ?" Would he have continued to seek alternate explanations or interpretations for Galileo’s observations of the moons of Jupiter, mountains on the moon, and spots on the sun? Would he have continued to support an underlying commitment to Aristotelian dynamics in the light of these observations, when he also realized that a full articulation of his own geoheliocentric system would need to have the Earth rotate to even begin to approach the simplicity of Kepler’s elliptical orbits and produce a competitor to the *Rudolphine Tables*? Or, would he not have recognized that there were now too many fingers in the geostatic dike?

We can close this chapter with a short test of the position I have outlined by comparing it with the responses of relativism on the one hand and Lakatos on the other to the Church’s action of 1616. In doing so, let’s translate the Church’s position and consequent action of censoring heliostasis into what would be its modern equivalent, i.e., the complete withdrawal of institutional support and research funding due to alleged poor scientific rationale. Relativism is clearly committed to the view that the Church’s position was as good as that of Galileo’s; because "anything goes," the reasons for the Church’s position were
just as good (or just as bad). And since truth is simply a question of power, we can not say that it was wrong in any sense for the Church to act on its position, to attempt to implement its position by molding reality according to its views. The amended Lakatos, in spite of his previous epistemological leniency in such matters, is now committed to saying that the Church's position was irrational because an application of timeless, universal principles of scientific appraisal show that the Ptolemaic-Aristotelian cosmology was degenerating for centuries, that it was no longer possible for rational people to deny its poor public record, and that decisive internal support had existed for Copernicanism for decades. Hence, for Lakatos research money should have been withdrawn from the Ptolemaic-Aristotelian program. 68

The position I have sketched is committed to saying the Church's position was irrational, not necessarily because by this time all the discordant developments of late 16th-century and early 17th-century were completely assimilated by all the key players, but because by this time a well-worked out adjudicatory discussion had been taking place for several decades, and at precisely the time the discussion was reaching an adjudicatory threshold, the Church decided to withdraw funding and institutional support for further development of what was clearly by this time a viable and robust scientific alternative. In short, the Church decided
to cut off discussion of precisely the type that would allow
the world to "kick back" further. Given the rate of
progress achieved by heliostasis, it was a mistake not to
continue to pursue it. Nowhere is this put better than in
Galileo's letter of 1615 to the Grand Duchess Christina.
According to Galileo,

... to ban Copernicus now that his doctrine is
daily reinforced by many new observations and by
the learned applying themselves to the reading of
his book, after this opinion has been allowed and
tolerated for those many years during which it was
less followed and less confirmed, would seem in my
judgment to be a contravention of truth, and an
attempt to hide and suppress her the more as she
revealed herself the more clearly and plainly. 69

Contrary to relativism, some decisions are just not
well supported. Contrary to Lakatos there is no universal
analysis that can be applied to the discussions of the
middle and late 16th-century that shows the inevitable
progressiveness of heliostasis. Tycho-like figures abound
in science. They make solid contributions. They give
competing ideas fair hearings. They follow adjudicatory
trails and impose axiological, methodological, and theoret-
cal stances in the process. And because of this, not in
spite of this, they pay close attention to what the world is
telling them. The world, however, does not reveal all its
secrets at once, and so scientists must make decisions and
take a risk on where to stop and place their bets. Many
times they are wrong, and they become a minor footnote in history.

Science is a process that attempts to walk a complex, difficult epistemological path between the extremes of a reassuring, but unrealistic, Platonic absolutism and a seductive, easy relativism. Both are reactions to the insecurity that we must live with to maintain a consistent fallibilism. But as Socrates admonished us centuries ago, we will be "better, braver, and less idle" when we take this epistemological path.
Notes for Chapter 4:

2. Ibid., p.121.
4. I will be resisting the temptation to indulge in "a prioristic" definition bashing of Lakatos's conception of novel facts. I will take Lakatos's definition (what has come to be called the heuristic conception following the work of Worrall and Zahar) at face value and show that it does not do what he thinks it will, i.e., show decisive internal-rational factors for early theory choice in the case of the Copernican episode. For a summary of various definitions and amendments, see Nancy Murphy, 1989. Murphy suggests that a novel fact for a new theory be one that "was first documented after that theory was proposed." But planetary retrogressions and increased brightness would be ruled out as novel for realistic heliostasis under this amendment. See discussion below. For other discussions of the definitional question see Gardner, 1982, Campbell and Vinci, 1983, and Nunan, 1984. For an excellent defense of the heuristic conception see Worrall, 1978.


6. Because external history must supplement internal history, normative appraisal alone will not explain or justify commitment, acceptance or rejection (Ibid., pp. 168-169, 190). Although Lakatos seems to vacillate on this point (..."we may in the end have to admit that Copernicus's and Kepler's and Galileo's adoption of the heliocentric theory...is not rationally explicable...", p.178, (but) "Copernicus recognized the heuristic degeneration of the Platonic programme at the hands of Ptolemy...", p.181, emphasis added), surely Lakatos wants to argue that being rational human beings, key scientists, as a matter of fact, will often choose for good reasons the most progressive research programme to work on. If objective appraisal features are involved in scientific change, they must be recognized. What good would they be, if no one ever

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recognized them? However, in deference to Feyerabend, Lakatos says this does not mean that the philosopher of science can give advice, that scientists "ought" to always choose what appears to be the most progressive research programme (p.117). This entanglement between psychological factors behind commitment and adoption on the one hand, and the recognition of rational factors on the other is no doubt one reason for his "agonizing reappraisal" of a key feature of his philosophy of science. See below and note 12.

7. Although the main focus of this chapter is a criticism of a key element in the machinery needed to establish 2, it is worth pausing to reflect on the indirect method Lakatos used to argue 1. In admitting that it is difficult to argue against relativism directly (Ibid., p. 178.), Lakatos says in effect that we must assume the ultimate rationality of science, which in turn then reveals the heuristic fruitfulness of this assumption. Although shockingly Augustinian in one sense -- we don't believe because we see, we see because we believe -- this approach is consistent with the "hard-core" feature of Lakatos's theory of scientific research programmes. In this case, the rationality of science is made a hard-core element of a meta-programme which in turn is not judged on the basis of simple confirmations or disconfirmations, but on whether an analysis of scientific change shows this programme to be progressive or degenerative. Although a complete analysis of this response to relativism is beyond the scope of this thesis, it is also worth noting Laudan's point in this context that many philosophers of science purporting to go beyond the closet or implicit infallibilism of logical empiricism, end up advocating either torturous theories of normative methodology or some version of relativism, because they unconsciously or unwittingly acquiesce to the logical empiricist's axiological axiom that in matters of cognitive value there is little or no possibility of rational disputation and adjudication. See Laudan, 1984, pp. 47-50. In agreement with Laudan, in the terminology of this thesis, cognitive values are nodes on a hypertextual adjudicatory trail with their own network of connections to methods, theories, and evidence.


9. See note 5.


11. Lakatos refers to the observation of a full set of phases of Venus taking place in 1616 and mentions Galileo at the same time. (Ibid., p. 184) He seems to be confused on dates here. Galileo's observations of Venus were conducted throughout 1610 and completed on New Year's day, 1611. The Church's first
action against realistic heliostasis took place in 1616. If Lakatos knows of, or is referring to, a special corroboration of Galileo’s observations, he does not say, and I am unaware of any special corroboration by another astronomer taking place in 1616.

12. According to Toulmin in his commentary on Lakatos’s paper (in Westman, 1975b, pp. 384-391) this amendment represents an "agonizing reappraisal."

13. The references Lakatos cites are: Gingerich,1975a; Kuhn, 1957; Neugebauer, 1968; Price, 1959; Westman, 1972.

14. Lakatos cites Gingerich (1975a). As we have already seen, what is less well known is that the so-called factual equivalence of the two systems was in actual practice an average, that at any given time during the course of a year one model might be making better predictions of planetary positions than the other.

15. Curiously, Lakatos says that although the observation of a full phase for Venus in 1616 (1610?) cannot help Popper because of its late occurrence and an adequate geoheliocentric solution (Lakatos, 1978, p. 171, N2), it can help the Lakatosian theory because its prediction was novel in the sense described below, and because "Copernicans predicted the phases of Venus, while Tychonians only explained them by post hoc adjustments." (Ibid., p.115, N1) However, Lakatos gives no evidence for the latter claim, it conflicts with authoritative astronomical conclusions on the predictive value of the Tycho­nic system (see note 41 below), and it is puzzling to say the least how the Tychonic solution to the full phase of Venus works against Popper because it is adequate, but works in favor of Lakatos because it is ad hoc.

As we will see in the next chapter, a better way of handling geoheliocentrism is to ask whether as an auxiliary patch to geostasis, as an adjusted hypertexual adjudicatory trail in the light of apparent falsifications, it showed promise or whether it exposed many weak nodes in the set of theories, auxiliaries, etc. that constituted the general geostatic defense. I will argue the latter, and that

1. Its production was driven by an admission that parameter determination was an important feature that astronomical models should have. One of the main arguments being made by supporters of heliostasis.

2. Many of the details for geoheliocentrism were not worked out. There were no predictive tables, for instance.

3. What little details were worked out forced supporters to
make the Earth rotate! This was a dangerous admission to the supporters of heliostasis and further weakened Aristotelian dynamics, one of the important nodes in the geostatic set.

16. Gingerich, 1975a, p. 87.


18. This long-term degeneration is, of course, the result of hindsight. The Ptolemaic system cannot be seen as degenerating unless there is another research programme whose progressive-ness reveals the degeneration of geostasis. Although Lakatos does not say so in this article, his charge that geostasis can be seen as degenerating by the middle of the 16th century, and that this degeneration begins with Eudoxian tinkering with homocentric spheres to account for retrogression, does not imply that pre-sixteenth century adherents to the Ptolemaic system where acting irrationally.

19. Ibid., pp. 187-188. Lakatosian apologists could argue that the first paragraph in this citation is just a slip of the pen, that Lakatos gets carried away here and that he does not need this thought experiment to make his point. My argument is that it is no accident that it occurs where it does. It consistently carries out features of Lakatos's argument and reveals a major defect in the his amendment that is more difficult to see otherwise.

20. Quoted from Gingerich, 1993, p. 327.


22. We will also resist the temptation to be picky here and assume that Lakatos knows full well that Galileo and Kepler were not intellectually active in the 1520s, and is here discussing the developments that led up to their commitment and consequent work in the latter half of the 16th and early 17th centuries.

23. Is this "fixed symmetry" an internal or external factor for Lakatos? In arguing for his own position it is a powerful internal factor; in arguing against simplicism and Kuhn, "geometric harmony" is rejected as an inadequate internal reason because perceiving it is a matter of taste. In the 20th century it might be possible to argue that the quantitatively fixed positions of planetary orbits and the aesthetic appreciation of this are distinguishable in terms of different "types" of reasons (the former, internal, and the latter, external), but they clearly were not distinguishable for 16th century heliostatic realists. Parameter determination, as I will argue in the next chapter, was still inextricably linked to pythagorean elegance and a picture of God as the great
mathematician who would do nothing superfluous.


26. See Chapter 2 and point 5 on Ptolemaic problems. For future reference, however, consider that one can push the cosmological treatment of each planet individually only so far and still maintain some sort of system. If the individual features of one planet clearly interfere with those of another, it is difficult to see how any cosmological fix would be forthcoming to explain this. Recall (also from Chapter 2, point 5) that in Ptolemy's system, Saturn's equant point falls within the nested sphere of Mars.

27. Remember that Lakatos uses only the "rough models" of both theories. Presumably if we are to be fair, this means not only nonepicyclic, noneccentric, and equantless geostasis, but also nonepicyclic, noneccentric, and nonepicyclet heliostasis. Hence, my story will also be inaccurate in terms of real historical sequencing -- it ignores the timing of the Appollonius' and Hipparchus' epicyclic-deferent modification of Eudoxus and its role in the full Ptolemaic system. Given Lakatos's caricature we are to see this as a clear example of long term degeneration, but Lakatos can't have it both ways; he can not use an inaccurate historical sequence that favors heliostatic dramatic facts and then claim that changes to geostasis to respond to these facts is ad hoc, post hoc. The same game can show heliostasis in need of ad hoc, post hoc moves. See the development below.

28. Here my historical caricature suffers from the same defect of that of Lakatos; the rough heliostatic model would also violate the uniform motion postulate. (It is intriguing to speculate that this may be one of the reasons that Aristarchus's system was rejected immediately.) This shows that if we stayed true to the rough model illustration that Lakatos wants to use, heliostasis would be shown to degenerate on the very same basis which supposedly motivated its creation in the first place and require an ad hoc fix of multiple epicycles and eccentrics just as geostasis would of epicycles, eccentrics, and equants.

29. This "ridiculous penetration of the orbs" will later help our Tycho-like figure reconcile his critical misgivings about his own system where the orbit of Mars is made to intersect that of the sun.
30. It is intriguing to speculate to what extent the mode of communication available played in the commitments of the time. Although Gingerich (1975c), and Eisenstein (1968, 1969) have written on the crucial role printing had on objective discussion, one can still hypothesize that a significant "delay factor" existed in acknowledging the full force of the evidence. Would the Church have made the decision to censor heliostasis if FAX and electronic mail were available? Today, Lakatos is wrong on another account. There are apparent quick kills. Electronic mail and FAX transmissions played a major role in the North American and Western European quick kill of cold fusion. For some of the involvement of electronic mail and FAX transmissions in the cold fusion debate see how Pon's and Fleishman used FAX (Cookson 1989, p. 20.), and Douglas R.O. Morrison's discussion of his cold fusion electronic mail newsletter (from CERN laboratory), and his analysis of a "regionalization" of findings -- North America and Western Europe reported predominantly negative results, but Eastern Europe, Italy, Asia, and most of South American reported positive results ("Cold Fusion Sessions at the American Physical Society Baltimore Meeting," PR NEWSWIRE, May 9, 1989.).

31. To see how Copernicus himself adjusted the parameters of his system to match "inaccurate" Mars data, see Owen Gingerich, 1975b, pp. 103-4. According to Gingerich, the comparison of recomputations and the planetary data used by Copernicus shows that Copernicus was not "testing" his theory with new observations that were becoming available, but rather using ad hoc parameter adjustments to make sure his theory matched accepted data. According to Gingerich, "...the entire exercise was carried out primarily to show that the heliocentric cosmology was compatible with reasonable planetary predictions rather than to reform the accuracy of astronomical predictions." To see how Tycho vacillates ("deluded himself") on another Mars problem (parallax), see Gingerich and Westman (1988), pp. 70-72.

32. The lack of stellar parallax follows naturally from geostasis, just as the features Lakatos and Zahar appeal to in supporting Copernicanism. For instance, the use of parallax for geostasis would be no different than using the bonded elongation of the inferior planets for Copernicanism. To see how this would be consistent with the heuristic account of novel facts, see Worrall's defense and elaboration of Zahar and Lakatos (Worrall, 1978, especially note 23). According to Worrall, the relevant question would not be whether any defender of geostasis was thinking about the lack of stellar parallax during the theory's formative phase, but whether this measurement was fixed by the theory or had to be first "read off" from observation. It is also important in this context to remember that there was renewed interest in the relevance of stellar parallax during this time, after geostasis had been
fully developed and challenged by heliostasis. And geostasis handles Tycho’s more accurate measurements in a straightforward dramatic way, whereas heliostasis has to bring in radical theologians and a new conception of God.

33. According to Gingerich and Westman (1988), we can also throw into this epistemological stew personal conflict and claims of priority. And when we do so, claims of "inevitable" scientific developments are suspect. Hence, we should be wary of historiographical analyses that show that scientific change is "historically and logically sequential."

34. According to Gingerich, 1973a, p. 520, "The matter is very nicely put around 1555 in a letter from Gemma Frisius that was published in several editions of Stadius’ *Ephemerides* . . . . Gemma allowed that the Copernican system gave a better understanding of planetary distances as well as certain features of retrograde motion. He added, however, that those who objected to the ephemerides because of the underlying hypothesis understood neither causes nor the use of hypotheses." (emphasis added) Gingerich then quotes sections of the letter that reveal the importance of the standard instrumentalist interpretation.

35. Geostasis, of course, also does this. But most of the advocates of this view are not arguing for a realistic interpretation of eccentrics and epicycles. Remember that one of the primary selling points of heliostasis is supposed to be its realism, particularly its ability to fix the spacing between the planets so that specification of the orbits are no longer arbitrary. This blatant conceptual inconsistency is one of the many reasons it was difficult to take seriously a realistic interpretation of Copernicus’s full version of heliostasis.

36. But according to Kuhn, 1957, "...the length of the year determined from the *Prutenic Tables* was actually slightly less accurate than that determined from the older tables." (Vintage Books ed., p.188.) Hence, one could weigh as important either daily predictions, overall monthly accuracy, or overall calendar construction and get a different result as to which system was better.

37. He is also pushed by the fact that issues of patronage and reputation are at stake, that pressures for cosmological reform are building, and others, if he doesn’t act fast, may get credit for introducing a new system. See Gingerich and Westman, 1988.

39. As noted in the introduction, neither Ptolemy nor Copernicus had the earth or sun respectively at the exact center of their systems. Hence, my repeated reference to geostasis and heliostasis, rather than geocentrism and heliocentrism.

40. See Worrall, 1978, p. 60.

41. As noted above (note 15), Lakatos claims that Popper conveniently ignores the importance of Tycho prior to ignoring him himself (see p. 171, N2). Kuhn does not; see 1957 (Vintage Books ed.), p. 224. Nunan (1984, p. 285) also does not, but by looking at the matter through Bayesian spectacles, he fails to see the significance of parallax as a novel fact for geostasis. Although beyond the scope of this thesis, I would argue that Nunan’s definition and use of novel facts are even more utopian (see development below) than that of Lakatos and Zahar.

Lakatos also claims that "Copernicus predicted the phases of Venus, while Tycho would only explain them by post hoc adjustments." p. 115, N1. But Tycho's geoheliocentrism was developed several decades before Galileo's observations (hence there is no "reading off" from observation for parameter adjustment) and Lakatos gives no evidence for his conclusion. There is no evidence that Tycho used the Copernican prediction to construct his theory, and it is not necessary in terms of parameter adjustment for the development of geoheliocentrism. It also runs counter to what other authoritative sources have claimed. According to Gingerich, "Until Galileo's telescopic observations of the phases of Venus in 1610, no observational evidence could be brought against the Ptolemaic system. Even Galileo's observations could not distinguish between the geoheliocentric system of Tycho Brahe...and the purely heliocentric system of Copernicus." (1986, p. 81). Also see Gingerich, 1982, p. 137.

42. Gingerich, 1973b, pp. 87, 101. Emphasis added. Westman (1975b, pp. 329, 332) also refers to Tycho's "modern, critical attitude" in reference both to his appreciation of the scientific merits of heliostasis and his own geoheliocentrism, in contrast to Maestlin's stance as a "slavish admirer" and "faithful disciple" of heliostatic particulars.


44. According to Westman, "Tycho offers a fair hearing (emphasis added) to the physical arguments of Copernicus. His annotations show him exploring and paraphrasing the inner logic of Copernicus' theses without formulating any rejoinders. There seems little question, however, that he was never quite persuaded by these arguments." (Ibid., p. 317.)
45. A complete analysis of Maestlin’s work and comparison with Tycho’s would also reveal Lakatosian degeneration. Although a pivotal figure in an assumed internal trail leading to Kepler, the former clearly ignored many of the internal considerations that Tycho took seriously. See Ibid., pp. 329-339.

46. See Feyerabend and Kuhn, 1970.

47. This in spite of Lakatos’s observation in reply to Feyerabend that "If it were irrational to work on a theory whose superiority was not yet established then almost all of the history of science would indeed be rationally inexplicable." Lakatos, 1978, p. 178.

48. Followers of Lakatos may argue that I have misconstrued Lakatos’s point; that all he is arguing is that a rational reconstruction shows a "fruitfulness" or progressiveness "edge" to heliostasis, that although the situation was not absolutely clear, one could not deny in the late 16th century the "poor public record" of geostasis (Ibid., p. 117). But we have seen that there was no such edge in the late 16th century in favor of heliostasis, and a distinction between "decisive fruitfulness" and "rationally compelling and decisive" good reasons is no distinction at all. Lakatos’s analysis degenerates into moderate relativism (see below). Further evidence of Lakatosian confusion is seen in that Lakatos wants a cake-and-eat-too situation: the Copernican proposal was immediately progressive on the basis of a rational reconstruction of the late 16th century, but it was also immediately degenerative, and this is why Kepler eventually produced elliptical orbits. (p. 188.)


52. According to Owen Gingerich (In Westman and Gingerich, 1975b, p. 244) in science we have "not merely an extension of man’s knowledge of the universe, but an evolution of his entire mode of assimilating his discoveries." (emphasis added)


54. Laudan does not deny some circularity, but calls it a "central, but nonvicious, circularity." (1984, p. 39 n13.)

56. Even if agreement is not sometimes reached, the debates will still have a rational framework. See Laudan, 1984, p. 86.


58. 1988, p.28.

59. In this regard, Laudan's own appeal to a "shared canon" to evaluate the aims of science is suspect. See Laudan, 1990a, and Leplin's criticism (1990) of Laudan's "strange conservatism."

60. A quick comment on the epistemological hubris implied by relativism is in order here, behind the notion that it is just a question of power, that any trail of ideas will work with the world as well as any other. Although it may be true, it is clearly egocentric to assume that human concepts can be so powerful as to veil nature completely. Metaphysically it may be true that the world is itself ambiguous or flexible enough to allow for any trail of ideas. Then the main issue is whether our meandering is constrained at all by nature, that nature is so flexible as to allow for any trail whatsoever, whether "anything goes." But is this not also then an empirical matter? If there are indeed "complementary" trails possible, it will be nature that tells us so.

It is at this point that a Doppeltian-like philosopher of science will step in and say, "Ah-ha! You have identified an absolute: Prefer theories that force interaction with the world." My response is that at this point we see how silly this "relativist challenge at a new level" game gets. Of course 'science' must have a definition. At issue is not whether an identifiable activity of systemically forcing intimacy with nature is an absolute; at issue is whether this activity is better than meditation, palm reading, astrology, and so on, in terms of accomplishing the goal of finding reliable beliefs with which to interface with the natural world. It may not be, but a combination of reason and experience will tell us so.


62. This phrase is Laudan's, Ibid., p. 113.

63. 1962, p. 157

64. As we have seen, Feyerabend also assumes an implicit infallibilism in arguing his infamous version of Copernican-revolution-relativism. In addition to assuming that scientific rationality is in trouble because the arguments of Copernicans "were not regarded" (his emphasis) as decisive (my emphasis). Nor were (his emphasis) they that decisive (my emphasis) as
shown in AM." (Feyerabend, 1978, p.45), typically he fails to
distinguish between the developments of the middle and late
16th century and the early to middle 17th century, and the
examples he uses jump around between these two epistemolog­
ically distinguishable periods of time. In short, like most
relativists he commits an epistemological slippery slope in
arguing that because early theory pursuit is not the result of
overwhelming rationally compelling reasons, so must later
acceptance be just a leap of faith.

65. Not of the stars, the primitive level of telescopic observa-
tion would have been no help in determining stellar parallax
yet. But more accurate measurements of parallax of closer
astronomical bodies, and the further weakening of Aristotelian
celestial theory (the moon, the sun, and planets as physical
places rather than etherial bodies) would surely have affected
a man who gave Copernicus a "fair hearing."

66. The language here is borrowed from Duhem, 1954, p. 217. Duhem
is not endorsing the rationality of this move, other than to
acknowledge that it is "good sense" rather than logic that
allows us to see that our buildings are tottering. According
to Duhem,

"The day arrives when good sense comes out so clearly in favor
of one of the two sides that the other side gives up the
struggle even though pure logic would not forbid its
continuation." Ibid., p. 218.

Duhem’s insights were clearly a contribution to fallibilistic
epistemology. The problem with Duhem, as discussed earlier,
is that this good sense remains unarticulated; we are given no
ampliative theory.

67. The claim implied in my rhetorical question is highly contro-
versial and at his point totally unsupported. In the next
chapter, I will show that geoheliocentrism was never fully
articulated in terms of producing tables and ephemerides.
This was not due only to Tycho’s death in 1601, for this model
had many supporters and versions (Gingerich and Westman, 1988;
Schofield, 1981). However, supporters of this model experi-
mented with rotating the Earth to lesson their system’s reli-
ance on a cumbersome dynamics — a dangerous admission to the
supporters of heliostasis and another severe blow to the Aris-
totelian auxiliary. Even Kuhn admits that,

"Ultimately (Kepler’s) version of Copernicus’ proposal would
almost certainly have converted all astronomers to Copernican-
ism, particularly after 1627 when Kepler issued the Rudolphine
Tables, derived from his new theory and clearly superior to
all astronomical tables in use before." Kuhn, 1957, p. 219.
The *Rudolphine Tables* generated an overall accuracy of planetary positioning 30 times better than any predecessor. It also was the basis for a prediction and Gassendi's successful observation of a transit of Mercury across the face of the Sun on November 7, 1631. According to Gingerich, referring to Kepler's "cleansing and mechanization" of the Copernican system, "For the professionals, this improvement was a forceful testimony to the efficacy of the Copernican system," and "the evidence for the new system was overwhelming." Gingerich, 1993, pp. 329, 342.

68. According to Lakatos, one "may rationally stick to a degenerating program," but "scientific journals should refuse to publish" the papers defending such a program and research foundations should refuse monetary support. (Lakatos, 1978, p. 117.)

Chapter 5: Parameter Determination
and the
Great Argument

What then are the conditions logically imposed on the choice of hypotheses to serve as the base of our physical theory? One condition is that physical theory is not to be resolved into a mass of disparate and incompatible models; it aims to preserve with jealous care a logical unity, for an intuition we are powerless to justify, but which it is impossible for us to be blind to, shows us that only on this condition will theory tend towards its ideal form, namely, that of a natural classification. Pierre Duhem, The Aim and Structure of Physical Theory

Coincidences do occur, but we should not seek their complexities as favored modes of explanation. Stephen Jay Gould

Introduction

A considerable number of promissory notes remain from the preceding chapters. The principal epistemological challenge is to explain how major players in the Copernican episode, such as Galileo and Kepler, could rationally become Copernicans at a time when not many other scientists were, how their pursuit of heliostasis could occur prior to their discoveries, and how their discoveries could then eventually convince virtually a whole generation of scientists to become Copernicans prior to Newton’s Principia. I have argued that the traditional polar opposites of Legend and psycho-social relativism are equally extreme and wrong; that an epistemological story can be told that answers the principal challenge of the Copernican episode by showing that what was occurring was a gradual clarification,
strengthening and concomitant weakening of different hypertextual adjudicatory trails, with a major role played by the emerging theory choice constraint of parameter determination. The relativists are right that scientists often do choose to patch a hypertextual adjudicatory trail in the light of anomalies or apparent falsifications, and that in principle with enough creative investment of time and resources such patches can be successful in a purely logical sense. But they are wrong that such patches will always be successful in an ampliative sense, that such patches are incapable of being appraised objectively by well-supported criteria, and that there is always an incommensurability of such appraisal dependent upon theoretical allegiance.

Accordingly, what must now be defended in depth are the following:

1. Although Ptolemaic geostasis and Copernican heliostasis were both systems, the interconnected way that Copernicus explains the core problem situation of observational astronomy impressed both supporters and nonsupporters of heliostasis. In other words, key parameters in the heliostatic system are fixed and linked in such a way that they must produce an elegant solution to the "principle challenge" of observational astronomy.

2. The impressive nature of heliostatic parameter determination became a principal motivating factor for nonsupporters of heliostasis to pursue a major auxiliary patch to geostasis, i.e., geoheliocentrism.

3. The pursuit of this auxiliary patch was successful, such that by the time the Church took a robust stand against realistic heliostasis, geoheliocentrism was the principal surviving
geostatic alternative to the heliostatic challenge. However, although defenders of geostasis won a major battle against the heliostatic challenge, the winning of this very battle caused them to lose the war.

4. Thus, from the standpoint of the strengthening and weakening of hypertextual adjudicatory trails, adoption of parameter determination as a constraint not only motivated geoheliocentrism, but this auxiliary patch in turn exposed many weak nodes in the general defense of geostasis. In fact, it is not an exaggeration to say that in the light of empirical developments (comets, novae, Galileo's telescopic results) and continuing successful conceptual development of heliostasis (Kepler's elliptical orbits and Galileo's work in dynamics), the very auxiliary patch that was supposed to save geostasis exposed geostasis as a "worm-eaten" system, as a hypertextual adjudicatory trail unraveling along many nodes.

In addition to defending these points, a crucial issue that must be discussed is the epistemic status of parameter determination itself. In other words, even if the above historical sequence is correct, that major players behaved this way, the question remains "ought" they to have behaved this way? Even if parameter determination ought to be a constraint on theory choice, what kind of constraint is it? Is the desirability of theories that display a greater comparative logical unity, an interlinking of postulates and parameters linked in turn with disparate but well-known observations, in the words of Duhem quoted above, only "an intuition we are powerless to justify"? Is its value purely pragmatic, heuristic, and prospective in that theories that fix key parameters are more fruitful in terms of ease of
future testability? Or can something substantially more be claimed for this constraint? Can its value also be perceived as retrospective in providing a kind of empirical evidence for a theory?

In answering these questions one must be careful not to impose a modern perspective upon the Copernican episode players. In scientific practice today, parameter determination and intertheoretical consistency play major roles in motivating and constraining theoretical pursuit. As noted previously, the Standard Model of quantum field theory solves many problems, but when applied to explaining the first microseconds of the universe, it leaves numerous initial conditions flexible. Hence, we find large scale pursuit by physicists of a better theory that will solve the same problems, but lock in all the parameters at once. There is a suspicion that because of flexible parameters, the Standard Model has not quite got it right. Similarly, the fact that quantum mechanics and the general theory of relativity remain inconsistent has motivated the pursuit of numerous alternatives to Einstein's conception of gravity. There is a similar suspicion that something is not right about the general theory, because as it is currently developed we do not possess a unity of treatment with quantum physics. However, assuming the meta-methodological standpoint that methodologies and theoretical constraints are themselves justified over time empirically, one must
remember that modern scientists and philosophers of science have had an additional 400 years plus of experience testing methodologies.

My contention will be the following: Although attributing an oversimplified epistemic status to parameter determination is incorrect, and although the heuristic value of parameter determination in terms of ease of future testability is no doubt correct -- every test is potentially a "deadly test" if predictions are fixed -- something more can be said for parameter determination when it is not seen in isolation, when it is combined with the concepts of pursuit and hypertextual adjudicatory trails.

The Golden Chain Argument

We must first establish then what the major players, supporters and nonsupporters alike, found impressive about Copernican heliostasis. Why did it create an immediate stir? Why was it pursued by some and at least studied carefully even by those who could not accept its cosmological implications? Prior to examining the technical details, it is important to let the major players speak for themselves.

Rheticus

With regard to the apparent motions of the Sun and Moon, it is perhaps possible to deny what is said about the motion of the Earth, although I do not see how the
explanation of precession is to be transferred to the sphere of the stars. But if anyone desires to look either to the order and harmony of the system of the spheres, or to ease and elegance and a complete explanation of the causes of the phenomena, by no other hypotheses will he demonstrate more neatly and correctly the apparent motions of the remaining planets. For all these phenomena appear to be linked most nobly together, as by a golden chain; and each of the planets, by its position and order and very inequality of its motion, bears witness that the Earth moves. . . .

I sincerely cherish Ptolemy and his followers equally with my teacher, since I have ever in mind and memory that sacred precept of Aristotle: "We must esteem both parties but follow the more accurate." And yet somehow I feel more inclined to the hypotheses of my teacher. This is so perhaps partly because I am persuaded that now at last I have a more accurate understanding of the delightful maxim which on account of its weightiness and truth is attributed to Plato: "God ever geometrizes"; but partly because in my teacher's revival of astronomy I see, as the saying is, with both eyes and as though a fog had lifted and the sky were now clear, the force of that wise statement of Socrates in the *Phaedrus*: "If I think any other man is able to see things that can naturally be collected into one and divided into many, him I follow after and 'walk in his footsteps as if he were a god.'" 1540, *Narratio Prima*

Maestlin

Certainly this is the great argument, viz. that all the phenomena as well as the order and magnitudes of the orbs are bound together in the motion of the Earth. (This is one of Maestlin's many annotations to his copy of Copernicus's *De revolutionibus*. Westman believes it was made between 1570 and 1580.)

Kepler

. . . the ancient hypotheses clearly fail to account for certain important matters. For example, they do not comprehend the causes of the numbers, extents and durations of the retrogradations and of their agreeing so well with the position and mean motion of the sun.

Copernicus alone gives an explanation to those things that provoke astonishment among other astronomers, thus destroying the source of astonishment, which lies in
the ignorance of the causes. 1596, *Mysterium Cosmographicum* 10

Galileo

Salviati

In the Ptolemaic hypotheses there are the diseases, and the Copernican their cure... With Ptolemy it is necessary to assign to the celestial bodies contrary movements, and make everything move from east to west and at the same time from west to east, whereas with Copernicus all celestial revolutions are in one direction, from west to east. And what are we to say of the apparent movement of a planet, so uneven that it not only goes fast at one time and slow at another, but sometimes stops entirely and even goes backward a long way after doing so? To save these appearances, Ptolemy introduces vast epicycles, adapting them one by one to each planet, with certain rules about incongruous motions -- all of which can be done away with by one very simple motion of the Earth.

Sagredo

I should like to arrive at a better understanding of how these stoppings, retrograde motions, and advances, which have always seemed to me highly improbable, come about in the Copernican system.

Salviati

Sagredo, you will see them come about in such a way that the theory of this alone ought to be enough to gain assent for the rest of the doctrine from anyone who is neither stubborn nor unteachable. I tell you, then, that no change occurs in the movement of Saturn in thirty years, in that of Jupiter in twelve, that of Mars in two, Venus in nine months, or in that of Mercury in about eighty days. The annual movement of the Earth alone, between Mars and Venus, causes all the apparent irregularities of the five stars named... (explains Jupiter’s motion, then follows with)

Now what is said here of Jupiter is to be understood of Saturn and Mars also. In Saturn these retrogressions are somewhat more frequent than in Jupiter, because its motion is slower than Jupiter’s, so that the Earth overtakes it in a shorter time. In Mars they are rarer, its motion being faster than that of Jupiter, so that the Earth spends more time in catching up with it.
Next, as to Venus and Mercury, whose circles are included within that of the Earth, stoppings and retrograde motions appear in them also, due not to any motion that really exists in them, but to the annual motion of the Earth. This is acutely demonstrated by Copernicus.

You see, gentlemen, with what ease and simplicity the annual motion -- if made by the Earth -- lends itself to supplying reasons for the apparent anomalies which are observed in the movements of the five planets.

It removes them all and reduces these movements to equable and regular motions; and it was Nicholas Copernicus who first clarified for us the reasons for this marvelous effect. 1632, Dialogue Concerning the Two Chief World Systems.

Gemma Frisius

While at first glance the Ptolemaic hypotheses may seem more plausible than Copernicus', nevertheless the former are based on not a few absurdities, not only because the stars are understood to be moved non-uniformly in their circles, but also because they do not have explanations for the phenomena as clear as those of Copernicus. For example, Ptolemy assumes that the three superior planets in opposition -- diametrically opposite the sun -- are always in the perigee of their epicycles, that is, a "fact-in-itself." In contrast, the Copernican hypotheses necessarily infer the same thing, but they demonstrate a "reasoned fact." 1560

Praetorius

Now . . . everyone approves the calculations of Copernicus . . . . (and) this symmetry of all the orbs appears to fit together with the greatest of consonance . . . . (so) we follow Ptolemy, in part, and Copernicus, in part. 1592

Tycho

[In examining the Ptolemaic hypotheses] . . . it gave me great concern that no necessary cause or natural combination explained why the superior planets are bound to the sun in such a way that at conjunction they always occupy the top of their epicycles, at opposition the lowest point of the same, and that the two planets that are called inferior always have the same mean position with the sun and are close to it at apogee and
perigee of their epicycles. 1588 Letter to Casper Peucer

The testimonies of the planets, in particular, agree precisely with the Earth's motion and thereupon the hypotheses assumed by Copernicus are strengthened. (Tycho's annotation of De revolutionibus -- approximately 1575 -- regarding the more "exquisite order" implied by the heliostatic arrangement.)

I considered that the old Ptolemaic arrangement of the celestial orbs was not elegant enough, and that the assumption of so many epicycles by which the appearances of the planets towards the Sun and the retrogradations and stations of the same, with some part of the apparent inequality, are accounted for, is superfluous. . . . At the same time I considered that newly introduced innovation of the great Copernicus . . . by which he very elegantly obviates those things which occur superfluous and incongruously in the Ptolemaic system, and does not at all offend against mathematical principles. (Upon introducing his geo-heliocentric alternative in his De Mundi Aetheri Recentioribus Phaenomenis (Uraniborg, 1588), explaining why, although he continued to believe "the Earth, large, sluggish and inapt for motion," he became convinced that "the simple motion of the Sun is necessarily involved in the motion of all five planets," that "the Sun regulates the whole Harmony of the Planetary Dance." )

And, of course,

Copernicus

We find, then, in this arrangement the marvelous symmetry of the universe, and a sure linking together in harmony of the motion and size of the spheres, such as could be perceived in no other way. For here one may understand, by attentive observation, why Jupiter appears to have a larger progression and retrogression than Saturn, and smaller than Mars, and again why Venus has larger ones than Mercury; why such a doubling back appears more frequently in Saturn than in Jupiter, and still more rarely in Mars and Venus than in Mercury; and furthermore why Saturn, Jupiter and Mars are nearer to the Earth when in opposition than in the region of their occultations by the Sun and re-appearance . . . . All these phenomena proceed from the same cause, which lies in the motion of the Earth.
(In contrast with the Ptolemaic models) . . . although they have extracted from them the apparent motions, with numerical agreement, nevertheless . . . . They are just like someone including in a picture hands, feet, head, and other limbs from different places, well painted indeed, but not modelled from the same body, and not in the least matching each other, so that a monster would be produced from them rather than a man. Thus in the process of their demonstrations, which they call their system, they are found either to have missed out something essential, or to have brought in something inappropriate and wholly irrelevant, which would not have happened to them if they had followed proper principles. For if the hypotheses which they assumed had not been fallacies, everything which follows from them could be independently verified. 1543

See Figures 3-6; especially 5 & 6.

It is crucial to note from these assessments that there is no incommensurability of appraisal regarding the comparative harmony, symmetry, elegance, and, what we have called, the parameter determination capability of the Copernican system. All of the above, whether supporters or non-supporters of heliostasis (Frisius, Praetorius, and of course Tycho, were not supporters) were impressed with the heliostatic ability, not only to produce the core problem observations as a necessary consequence of the model, but to link the observations and the parameters in such a way that there is relatively little flexibility in what they must be. For instance, the Copernican system not only elegantly produces retrograde motion, but fixes the number, size, and frequency of retrogressions for each planet. (Figure 6)

True, as Kuhn has noted, there was a different weight attached to this harmony. Copernicans tended to see it as
discriminating evidence for heliostasis. However, it is clear that, contra Kuhn, supporters and nonsupporters of heliostasis were not "talking through each other." As Tycho noted, the testimony of the core observational problem and the linking of the details strengthened heliostasis. This feature achieved independent recognition as a desirable constraint on theory choice. A gestalt switch was not needed for this recognition; one did not need to be a Copernican to recognize and appreciate the parameter determining features of heliostasis.

Praetorius, a member of the Wittenberg circle and a supporter of geostasis, was one of the first astronomers to begin experimenting with geostatic transforms of heliostatic parameters, because he recognized that it was necessary to follow Ptolemy in part (the Earth does not move) and Copernicus in part (the interlinking features of Copernicus's models are too appealing to be ignored). And by the time of Tycho, we see that he is convinced based on "the more exquisite order" of Copernican models that the Sun must regulate "the whole Harmony of the Planetary Dance."

In the light of major debates regarding scientific change and the notion of hypertextual adjudicatory trails discussed in this thesis, we see an example of piecemeal change: the appeal of parameter determination became a node on both the geostatic and heliostatic webs of belief. Although one can see a continuity of concern for a unitary
account from the time of the ancient Greeks, in Ptolemy's *Planetary Hypotheses*, through the medieval Arab astronomers, and up to the Renaissance, the story told here is that the appeal of a unified treatment in terms of linking parameters that fixed the observations of the core astronomical problem situation became more vivid upon the challenge of heliostasis. Just as defenders of heliostasis eventually recognized that they needed a much stronger dynamical story in response to the challenge of geostasis than the pitiful tale told by Copernicus in *De revolutionibus*, so defenders of geostasis recognized that they needed a much stronger account regarding the many linkages of planetary motion with the sun than the "fact-in-itself" coincidences of Ptolemy noted by Frisius above.

Although we will consider more of the technical details in depth shortly, it is important to note the point both Frisius and Tycho are making regarding one superior technical feature of linking planetary motion with the sun. When the superior planets are at opposition, they are not only in the middle of a retrogression, but they are noticeably brighter and thus presumably closer to the Earth as well. Hence, there is an apparent link between the positioning of the sun, the Earth, the superior planet, and the latter's brightness and direction of motion. The basic difference between the Ptolemaic and Copernican systems regarding these observations is that they are geometrically
required by the latter (Frisius's "reasoned fact"). In other words, although these observations are handled remarkably by Ptolemaic geometry ("well-painted," according to Copernicus), one realizes upon comparison with Copernican geometry that the linkages are a coincidence for Ptolemy in the sense that the linkages are separable from the geometry. In the basic geostatic deferent-epicycle system the opposition observation of a superior planet is saved by having the planet revolve on its epicycle at just the right rate such that it reaches its closest approach to Earth at the moment that the Earth is positioned between the superior planet and the sun. However, it was now recognized that there is nothing in the Ptolemaic geometry that necessitates this result; at the moment of opposition the position of the planet could be, a priori, at any position along its epicycle. (Figures 5 & 6)

This difference is perhaps best understood in terms of what each model would predict regarding the discovery of a new superior planet. Although there may have been a strong inductive reason to expect a continuation of a very notable pattern, there is no requirement in the Ptolemaic system that the new planet follow the same pattern of the previously known superior planets. Whether the planet is retrogressing and brighter at opposition or not, the Ptolemaic parameters can be adjusted to match the observation. Although in the Ptolemaic system the epicycle radius
vectors of the known superior planets are made parallel to the Earth-sun radius vector, and turn at the same rate of time as the sun's radius vector, such that the planets are positioned so that at opposition they are at the bottom of their radius vectors (see Figure 4), as Tycho notes above, there is no requirement ("no necessary cause") for this, i.e., there is neither an a priori requirement for the bottom positioning nor the parallelism. Hence, there is no clear implication for what a new superior must display.\textsuperscript{18} On the other hand, in the Copernican system, because retrogression is an illusion caused by the swifter moving Earth bypassing a slower moving superior planet, the superior planet must not only be closer to the Earth but be in a direct line with the sun, with the Earth between the planet and the sun (Figure 6). Accordingly, there is no flexibility for Copernicus in this regard: A new superior planet must follow the same pattern as the known superior planets.

Should this inflexibility have constituted a kind of decisive "second-order" empirical evidence for heliostasis? Can we double-count what we observe in favor of Copernicus? Lakatos, as we have seen, has argued affirmatively; being able to explain well-known observations in this special way was a kind of novel fact. I have argued in the previous chapter that this is to make too much of parameter determination. We can add to that analysis here by noting the
obvious point that prior to the observation of additional superior planets that fit the same pattern, this might only mean that heliostasis was "rigidly wrong"! Furthermore, that major players were impressed with certain features of the Copernican system neither provided an overwhelming evidential context that the whole system was correct nor eliminated the possibility that those same features could be incorporated into a modified geostatic model. However, it is clear that from the perspective of pursuit and fruitfulness, Praetorius, Frisius and Tycho believed that the impressive Copernican links with the sun were an indication that the basic geostatic position must be adjusted. The lack of necessitated links created the suspicion that the basic geostatic system just did not have it right. In the light of many other developments affecting various nodes along the Ptolemaic web of belief, it provided an evidential context such that one had a distinct suspicion that the basic Ptolemaic arrangement just would not do and something else was needed; that significant problem solving progress would be made by incorporating Copernican links with the sun into a geostatic system. In short, the significance of parameter determination should neither be seen as "mere aesthetics," nor as decisive empirical evidence for Copernicus at this time.

Another mistake related to making too much of parameter determination in isolation, is to couch the impressive
difference between geostasis and heliostasis in terms of a radical discontinuity in the latter's attempt to create a system. In other words, so this mistaken story goes, the defenders of geostasis were not concerned with a requirement of internal logical consistency or a unified broad-based explanatory theory, such that the problem context for each planet could be solved piecemeal without regard to needed adjustments in the models for other parts of the system. Hence, to cite one feature of Ptolemaic geostasis discussed earlier, little concern resulted from the fact that Saturn's equant point must be positioned within the nested sphere of Mars.

Indeed, one can find statements from Ptolemy himself apparently supporting this radical discontinuity.

The heavenly bodies suffer no influence from without; they have no relation to each other; the particular motions of each particular planet follow from the essence of that planet and are like the will and understanding in men. Ptolemy, Planetary Hypotheses

Massively problematic for this interpretation, however, are the following:

1. Ptolemy's Planetary Hypotheses is clearly an attempt to wed Aristotelianism and the evolved geostatic planetary models into a complete system. In this work, Ptolemy attempts to show how the entire geostatic entourage of deferents, epicycles, eccentrics, and equants can be embodied in an Aristotelian plenum universe of nested, incorruptible celestial spheres surrounding, of course, a corruptible Earth.
2. Throughout the middle ages, Arab natural philosophers commented and struggled with the unhappy systemic marriage between Ptolemaic models and Aristotelian cosmology.

3. Ptolemy's statement above is not an acknowledgment of disinterest in a complete cosmological system; rather it is a statement regarding the systemic relationship between Aristotelian philosophy and geostatic astronomy! In other words, it must be remembered that in Aristotelian celestial dynamics an individual planet moves the way it does, not because it is a physical place influenced by outside gravitational forces, but because like the will in an individual person, it is "striving" (as a celestial object, not a physical object) to achieve a goal, the goal of perfect circular motion. Galileo and Kepler proposed a different explanation, but they were clearly not the first to be interested in a broad-based explanatory linking of dynamics and kinematical models. To propose otherwise is to impose a modern cosmological perspective upon an ancient historical context.

4. In this light, the supporters of geostasis quoted above are best seen as being interested in "patching" the geostatic system, precisely because they believe that realistic heliostasis as argued for in De revolutionibus by Copernicus is a terribly weak system! In other words, Copernican dynamics was woefully inadequate, giving one little reason to believe, in the words of Tycho, that "the body of Earth, large, sluggish and inapt for motion . . . (could be) disturbed by movement, especially three movements..." According to Tycho, from the point of view of systemic considerations, "both these hypotheses (the Ptolemaic and Copernican) admitted no small absurdities." De Mundi Aetheri Recentioribus Phaenomenis

From the standpoint of this thesis, to claim that only Copernicans became interested in a logically unified system is tantamount to claiming that prior to De revolutionibus, pre-Copernicans were not interested in problematic relationships between the hypertextual adjudicatory linking
of theories, auxiliaries, methodologies, and experience. Although one can find such a claim being made by historians and philosophers of science, from the standpoint of this thesis we see much more continuity. Just as we do not see a radical gestalt incommensurability, we are not forced to see a radical discontinuity in terms of overall rational skeletal structure. Both pre-Copernicans and post-Copernicans were concerned with arguments and reasoning trails. Both were concerned, trans-culturally and trans-temporally, with adjudicatory relationships between core theories, auxiliaries, methodologies, and observations. The discontinuity that existed was piecemeal in the sense that there was a dissimilarity between some of the nodes of developing adjudicatory trails, between the clarifying and justifying moves made in supporting and developing different, but at times overlapping, webs of belief.

Before leaving this section, one more prefatory matter must be discharged. In the previous chapter, against Lakatos, the work of Westman was cited to show that if the parameter determining features of heliostasis, matched with the core problem situation, ought to have constituted immediate and decisive empirical evidence favoring this theory, then contra Lakatos’s own methodological claims the initial reception of Copernicanism must be seen as irrational and relegated to external explanatory sources. In other words, since the initial reaction to Copernican
linkages was met with "silence," too much scientific behavior must be relegated to external psycho-social causation. Yet, in this chapter I have claimed that the parameter-determining linkages highlighted by Copernicus created an immediate stir, impressing both supporters and nonsupporters alike. Is this a massive inconsistency?

No. Moreover, the apparent tension in these claims and its resolution reveal the central value of the distinctions made in this thesis. We must be able to drive a wedge between the extreme interpretations of

1. Psycho-social relativism and claims that the unique behavior of Rheticus can be explained by an "anything goes" scenario -- that the situation was so confusing that pursuit of anything could not be seen as irrational -- and that his particular response was due to his unique personal history, i.e., he had a disparate need for unity because of his father's beheading.

and

2. Legend and methodological scenarios that leave us puzzled why every rational, technically proficient scientist did not immediately jump ship to what proved to be from hindsight the more successful theory.

A reading of Westman might lead one to object that there must be something wrong with any methodological scenario that highlights Copernican linkages, because the initial phase of reception was not marked by "intense debates and polemics" in general, and the linkages themselves were met with distinct silence in particular. The only exception to the response of the majority of
technically proficient astronomers and natural philosophers was that of Rheticus. But, according to Westman, Rheticus was unbalanced; he had a traumatic childhood experience to deal with and was not seeing the evidence more clearly.

My response is two-fold. Rheticus was not acting that much different than the other Wittenberg astronomers, and a correct analysis of the attitudes of the Wittenberg astronomers must be seen not so much in what they said, but what they did. According to Westman, the reception of Copernicanism was marked by two phases:

(1) A first phase, 1543-1570, and the Wittenberg interpretation -- silence regarding the symmetrical linkages displayed in heliostatic models and a view that Copernicanism should be seen "merely as a useful set of auxiliary mathematical hypotheses and tables to be exploited by the practitioners of geostatic astronomy." 28

(2) A second phase, beginning in the 1570s, in which there is a "new appreciation" for the work of Copernicus in general, and the symmetrical linkages in particular. 29

My quibble with Westman's phases is that this interpretation leaves too much discontinuity between the phases. Why the all-of-a-sudden appreciation of Copernican linkages? According to Westman with time Copernicus's own realistic intentions were becoming clearer, and the occurrence of the 1572 nova and the 1577 comet rocked the Aristotelian boat sufficiently to force a greater appreciation of Copernicanism in general, such that the linkages
were then finally recognized as important by Tycho and others.

However, by Westman's own admission the Copernican system began to be studied immediately at the master's level at the University of Wittenberg, Rheticus is not reprimanded by any member of the Wittenberg circle, including the strict Lutheran leader Melanchthon, for his "golden chain" emphasis on Copernican models, and an "important plank of the Wittenberg program" was to translate Copernican devices into "a geostatic reference frame." How is the latter possible without an appreciation of the distinctive features of Copernican models?

A good example of the more gradual nature of parameter appreciation is the education and work of Johannes Praetorius (1537-1616), one of the first astronomers in the 16th century to tinker with geoheliocentrism. He appears to have started his affiliation with the University of Wittenberg in 1555, received a copy of De revolutionibus in 1560 when he began the master's level, met Rheticus in 1569, and no doubt contemplated how to fulfill the Wittenberg plank during this time. Another example, is that of Erasmus Reinhold. Even prior to the publication of De revolutionibus, we find him writing in 1542 that he knows (through Rheticus) of an "exceptionally skillful" astronomer who "has raised a lively expectancy in everybody." Later, via annotations of his personal copy of De revolutionibus, we
see Reinhold clearly appreciating Copernican models. This appreciation culminates, of course, with Reinhold's systematization and recalculation of planetary motions, published as the Prutenic Tables (1551), based upon Copernican models. According to Westman, Reinhold's work had the "full moral and financial support of Melanchthon. . ."\(^{35}\)

Westman claims that the Wittenberg interpretation was a "split interpretation," and his main concern is to educate Kuhnians to the fact that the initial period of heliostatic reception was not marked by revolutionary cosmological debates and incommensurable standoffs between two clearly demarcated paradigms. With this I readily concur. However, two important points must be added to this account to underscore the gradual and piecemeal nature of eventual full appreciation of the parameter determining features of heliostasis. First, recall that it was not conclusively clear that the Prutenic Tables were more accurate than the Alphonsine Tables, and that "ease of calculation" is often confused with "superior accuracy."\(^{36}\) What impressed Reinhold was the ease of calculation of planetary positions given the technical features of heliostasis. Second, one cannot appreciate the ease of calculation capability of heliostatic models without an early study and appreciation of the technical parameters of the models. We must now examine those technical features in depth.\(^{37}\)
Technical Treasures

To fully understand the appreciation of Copernican parameters experienced by supporters and nonsupporters of heliostasis, we must now flesh out with greater detail what I have called the core observational problem. First, one finds in the literature consistent references to anomalies or inequalities, specifically the first and second anomalies or inequalities. The first observational anomaly is that the rate of motion of each planet through the zodiac is irregular. The second observational anomaly is that each planet displays retrograde motion; that is, in addition to an overall eastward motion through the zodiac in the course of a year, planets also "wander" by changing directions and looping backwards in a westward direction. It is important to note that these anomalies are observationally connected in the sense that as a planet is in its retrograde phase, it appears to be moving faster than when not in this phase.

Articulating these anomalies in fine detail, ancient astronomers also recorded comparative regularities and differences in the size, shape, and frequency of planetary retrogressions. The eastward motions of the planets occur, of course, at different rates. Saturn as the slowest is thus presumed to be farthest from Earth. However, Saturn also displays 28 retrogressions in the course of a Saturn year, while Jupiter moves a little faster but only shows 11
retrogressions in a Jovian year. While Mercury moves much faster, it shows a retrogression only once every 116 Earth days. Mars, while displaying fewer retrogressions than Jupiter, has larger retrogressions than Jupiter, and Jupiter’s are larger than that of Saturn. Moreover, in the course of a planetary year the shape of the retrogressions of any particular planet will not be the same. Sometimes the loop will be thin and sometimes relatively fat.

Also observed were several important relationships with the sun. The inferior planets, Mercury and Venus, have a bounded elongation, whereas the superior planets do not. That is, the inferior planets always appear to move in relation to the sun within certain limits; whereas the superior planets can obtain any elongation from the sun. However, the superior planets do show another type of a distinct linking with the sun. As each superior planet retrogresses it appears brighter, achieving maximum brightness at the midpoint of its retrogression. At this midpoint the planet is said to be in opposition, because the sun will always be located such that the Earth is situated in a direct line between the sun and the planet. The inferior planets also show a relationship between retrogression and the sun. Although greater illumination is not observed, at the midpoint of retrogression the planet is said to be in conjunction because it will be situated on a direct line between the Earth and the sun.
In addition to these core phenomenal features, a successful geometric model had to give an account of other complex relative motions, and do so while predicting the location of all the astronomical bodies throughout the year. In addition to the planets, the sun has three motions: its daily motion, east to west; an eastward motion in relation to the stars; and a yearly north-south motion, noticeable as a change in seasons, where in northern latitudes the sun is higher (more northerly) in the summer sky and lower (more southerly) in the winter sky. Furthermore, any explanation had to account for the sun completing these motions and returning to any given position in just over 365 days. The moon not only has an east-west nightly motion, a monthly eastward motion, and an even larger north-south motion than the sun but phases as well. The appearance of the moon changes perceptively as it moves, such that successive full moons will not occur in the same place.

If all of this makes you dizzy, then you can well appreciate any geometric solution that accounts for all these motions qualitatively, quantitatively predicts locations relatively accurately, is able to link these predictions such that lunar and solar eclipses are also foretold, and can be used to estimate distances to all the major astronomical bodies. It is no small wonder that the Ptolemaic system was accepted for 1500 years. Combined with Aristotelian physics and a cosmology of nested spheres, and
matched by no serious alternative in terms of a different hypertextual adjudicatory trail, Ptolemaic geostasis was an "elegant cosmology." How does Ptolemy save all these phenomena?

The basic Ptolemaic geostatic arrangement involves deferent and epicycle circles with eccentric and equant devices. That is, a planet revolves on an epicycle circle around a point that lies on a deferent circle that in turn revolves around a centrally located Earth. The deferent circle marks the midpoint of a planetary epicycle that is nested and revolving within a sphere, such that the epicycle radius marks off the thickness of the sphere. The Earth (the center of observations), however, is not in the exact center of the deferent circle, but is displaced some distance from (eccentric to) the central point of deferent revolution. An equant point, oppositely displaced than the Earth from the central point of the deferent, is the geometric location of uniform motion for a planet. That is, the latter is not observed; it is geometric only in the sense that if an observer could be placed at the equant point, uniform motion would be observed. The equant point is not a mere a priori requirement, used by Ptolemy to haphazardly harmonize his system with the pythagorean requirement of uniform circular motion. It plays a crucial role in adjusting the degree of eccentricity of the deferent orbit from the point of astronomical observation (the
Earth), since the midpoint of the deferent orbit (called the eccentric point) will lie at the midpoint of a line connecting the Earth and the equant. See Figure 3.

Ptolemy solves the core problem situation in the following way. As a planet revolves on its deferent in relation to the background stars (also moving in Ptolemy’s system), the planet also revolves around its epicycle in such a way that at various times during the year it will be moving (looping) in the opposite direction of its overall eastward motion. By adjusting the radius ratios and periods of revolution of the epicycle-deferent arrangements for each planet, Ptolemy can match the frequency and extent of planetary retrogressions. Variable speeds are accounted for by combining the effects of the eccentric, equant, and direction of motion of the epicycle compared to the direction of motion of the deferent. For instance, Saturn’s epicycle radius will be smaller than that of Jupiter’s and adjusted to account for the planet’s 28 epicyclic loops in the course of its overall movement around the Earth. Jupiter’s epicycle radius will be larger, thus accounting for larger loops, and so on. Shape is handled by inclining the orbits of the planets to the sun’s orbit about the Earth. When these adjustments are combined with eccentric deferents and the displaced equant point, the core problem situation is solved by Ptolemy: irregular speed and retrogressions along with the latter’s extent and frequency.
Recall, however, that these solutions must also be linked with the sun. A geostatic Ptolemaic system saves these linkages by first fixing the epicycle centers for both Mercury and Venus on the same line from the Earth to the sun, and then keeping the epicyclic radius-vectors of the superior planets parallel to this line at all times. In this way, the motion about the sun of the inferior planets is limited (bounded elongation), and although the superior planets can attain any elongation, their motion on epicycles can be fixed with the motion of the sun such that the retrogression, opposition, brightness relationships will be saved. 41 This is accomplished by making sure that not only does the epicyclic radius vector remain parallel to the Earth-sun line for each superior planet, but that any given superior planet is at its lowest point at opposition (perigee, closest approach to the Earth). See Figure 4.

We are now in a position to summarize Ptolemaic parameters and to comment on their a priori flexibility.

1. The ratio of the epicycle and deferent radii.
2. The period of the revolution of the deferent.
3. The period of the revolution of the epicycle.
4. The directions of the revolutions of the deferent and epicycle.
5. The positions in the deferent and the epicycle for any given starting date.
6. The eccentricity of the orbit and equant. The extent of displacement from the exact center, of the Earth and equant point.
7. The direction in which the equant-eccentric line lies.
Of paramount importance for understanding the quotes above, particularly those of Frisius, Tycho, and Galileo, the Ptolemaic scheme offers a very flexible solution to a large part of the core problem situation. For instance, to return to the superior planet example discussed above, the geostatic arrangement does not absolutely require that a superior planet be in a perigee position in its epicyclic revolution when at opposition. Even if the epicycle-radius-vector-sun-Earth parallelism was a geometric requirement (it is not), there is nothing in the Ptolemaic system forbidding a newly discovered superior planet from being at apogee (farthest point from Earth) in its epicycle at opposition. In other words, other than the strong inductive expectation based on what had already been observed, and the dramatic success of Ptolemy's standard geometric arrangement to save it, there is nothing in the Ptolemaic overall machinery that forbids the discovery of a new superior planet that becomes noticeably dimmer at opposition and the mid-point of its retrogressions.

There is no necessity for the parallelism as well. Independent of observation, a superior planet could be at any position on its epicycle in relation to the sun's position, such that a superior planet could, a priori, show retrogression at opposition, conjunction, or in-between opposition and conjunction. Figure 5.
Consider next the inferior planet Venus. Ptolemy had measured the elongations from the sun to be 47° 20' and 44° 48'. From these observations he was able to set the eccentricity of the deferent circle and the radius of the epicycle by making them fit the appropriate angles of observation. Other than the relationship of Venus's epicycle to that of the sun -- recall that the epicycle centers for both Mercury and Venus are fixed on the same line from the Earth to the sun -- and distance relationships -- recall that once the length of the radius is fixed for an epicycle this determines the thickness of a sphere -- the results are "not linked to those of the other planets in any other way." In other words, the elongation observations dictate the parameters, and these parameters have no effect on the frequency, extent, and brightness relationships of the retrogressions of other planets.

Of the seven parameters listed above, five are independent. From this perspective, it is no small wonder that upon introduction of Copernican parameters we see even the Wittenberg astronomers jumping ship to Copernicus in terms of calculation (Reinhold and the Prutenic Tables). As Owen Gingerich has noted,

Given the parameters and the geometry of the [Ptolemaic] model, we could find the longitudinal direction of the planet for some specified time, but in the days before pocket calculators this would have been rather hard work. Even with a small calculator the procedure is tedious enough to ruin a morning."
As Kepler noted, but was not alone in so noting, there is much astonishment in the match of Ptolemaic parameters and observations. Why is the elongation of the inferior planets bounded? Ptolemaic silence. Why do the planets show particular retrograde sizes? Ptolemaic silence. Why do the superior planets show a steady progression in terms of retrogression size from Saturn to Mars? Ptolemaic silence. Why do they show different retrograde frequencies? Ptolemaic silence. Why does the mid-point of retrogression always occur at opposition? Ptolemaic silence. In fact, why are all these phenomena so intimately linked with the sun? Ptolemaic silence.

There are, of course, parameter matters the Copernican system is silent about as well. Why are all celestial motions in one direction and basically in the same plane? Why is the Earth-sun distance what it is? Before Galileo’s observations of the moons of Jupiter, why was the Earth’s moon apparently an exception in terms of motion about the center of the universe? But Copernicus is not silent regarding the features of the core problem situation, the problem situation of the greatest concern since antiquity. All of the above Ptolemaic perplexities receive a dramatic response in a basic heliostatic system. By placing a revolving Earth between Venus and Mars, all inferior planets must have bounded elongation, all superior planets must show retrogression only at opposition, and the observed frequency
and extent of retrogression are precisely linked with distances (Figure 6). Galileo may have been wrong that this argument alone -- the "great argument" (Maestlin), the "golden chain" argument (Rheticus) -- should have convinced all to accept Copernicanism, but it was appropriate that he gave it such prominent attention in his *Dialogue*. It impressed everyone, and convinced the majority of technically proficient astronomers, in the words of Tycho, that the Sun must regulate "the whole Harmony of the Planetary Dance."

**Geoheliocentrism**

So impressive were the Copernican linkages with the sun that Margolis claims that by the last decade of the 16th-century "astronomers began to openly abandon Ptolemy," such that by the opening decades of the 17th-century "the only coherent choice available to a competent astronomer became Copernicus versus Tycho." Furthermore, according to Margolis, sense can be made of an "otherwise bewildering situation" only if it is understood that the arrangement made between Galileo and the Pope, regarding the content of Galileo's *Dialogue*, was to leave the Tychonic system "undiscussed, and hence . . . unchallenged." Accordingly, the Pope's rage and consequent action are best understood by realizing that after thoroughly
discussing the content with Galileo and personally approving the project after a thorough year-long review by Church censors, thinking, as the title page suggests, that only the Ptolemaic and Copernican systems would be discussed, nevertheless "Galileo's identification of the Tychonic arrangement (in his Dialogue). . . as the surviving alternative is unambiguous and emphatic, but discrete. . ." 47 Worse, after publication and dissection by a wider and more technically proficient audience, the Church became aware of Galileo's grand trick, that Galileo not only had made clear to any knowledgeable reader that the alternative to heliostasis was geoheliocentrism, but alluded to the latter in just those contexts that would then make such readers puzzle over how it might actually work. 48 In other words, Galileo's silence is deafening on how complicated the Tychonic motions would need to be. He embarrassed the Tychonic system without mentioning it directly. According to Margolis, it is as if one were to criticize the physical layout of the city of New York with persistent reference to a large city at the mouth of the Hudson river, without ever using the words 'New York' in the criticism. 49 Finally, according to Margolis, the Pope and censors failed to foresee this potential embarrassment because no one had worked out the details of the Tychonic system. To cite the bare observational equivalence of the Copernican and Tychonic systems was one thing,
dealing with dynamical issues (raised constantly by Galileo in his Dialogue) was quite another.

What is the evidence for this startling claim?

First, there is the intellectual environment. As early as 1601, Kepler is able to say, "today there is practically no one who would doubt what is common to the Copernican and Tychonic hypotheses, namely that the sun is the center of motion of the five planets . . ." By the second decade of the 17th-century the Jesuit astronomers of the Collegio Romano had clearly abandoned Ptolemaic geostasis in favor of the Tychonic system, such that by the time of publication of the Dialogue, the Tychonic system had been taught for many years in a Jesuit introductory textbook on astronomy. This endorsement of geoheliocentrism was made quite clear by the Jesuit Grassi in his polemic against Galileo’s theory on comets. According to Grassi, "Tycho remains the only one."51

In response to Grassi, Galileo had written that he regarded the Tychonic system as an unfulfilled promise and a "null" system because of its lack of articulation and physical impossibility,52 and he had made his intention clear that he would demonstrate this in the Dialogue.53 Why would he change his mind? Would Galileo, who loved to argue, who was obviously very aware of the state of cosmological discussion in the astronomical community, acutely aware of the position of his arch enemies, the Jesuits, and
who had been stewing in frustrated contemplation for many years seeking a forum for his ideas, despondent that he would soon be totally forgotten, finally, when he got his big chance, spend 465 pages ignoring his most worthy opponent and leave himself vulnerable to such a simple charge of questionable dilemma sophistry or the appearance of an old man out of touch with modern positions? Margolis's insight is that Galileo would not open himself to such a perception and criticism unless he was prohibited from mentioning the Tychonic compromise, and unless he had brilliantly figured out a way to embarrass the compromise without directly mentioning it. Koestler may be right that we sometimes give our historical heros too much credit by ignoring their fallibility and foolishness, but to think that Galileo would either deliberately ignore Tycho or think that no one would notice if he did ignore him, is to give this historical figure much too little credit.

Next there is the political situation. Why allow Galileo to publish at all? How could the Pope think that publication of the Dialogue would serve Church purposes in any way? The Church was involved in a massive struggle for the hearts, minds, and pocketbooks of the European community. It was perceived to have taken a harsh stand in its 1616 Edict against the Copernican planetary linkages with the sun. (Actually, De revolutionibus was only edited in a minor way to eliminate any reference to heliostatic
referential commitment.) Those linkages had now received widespread acceptance and praise, and the Church needed to clear up misperceptions by sending a positive message of its forwardness and openness regarding this matter in particular and its relationship with science and astronomy in general. In other words, pressure had been building for almost two decades to reconcile the Edict of 1616 with the continual progress and consequent acceptance by most competent astronomers throughout Europe of linking planetary motion with the sun. What better way was there than to allow the major spokesperson involved in the 1616 Edict to come forward and straighten things out?\textsuperscript{54}

Then there is the textual evidence. Massively curious is the fact that a book personally approved by the Pope and scrutinized carefully by censors for a year would be allowed publication when the traditional Ptolemaic position is so promptly dismissed when the issue finally arises whether the planets revolve around the sun or the Earth. The issue is not even raised in a substantive way until well into Day 3. Significant is that the discussion of this day is introduced by a defense of Tycho's position, and other "assailers of the sky's inalterability,"\textsuperscript{55} on the celestial location of novas. Next there is an apparent puzzling discussion on the meaning of "center of the universe." Is the center (1) the place that stands still while everything else moves, or (2) the place around which every other celestial body moves in
circular motion? It is crucial to realize that in (1) that place need not be anywhere close to being the exact center of circular planetary motion. In (2) the place around which every other celestial body moves in circular motion would also be the place that stands still. This discussion makes no sense if the debate is only between Copernicus and Ptolemy, because although they have a different object in this center, both Copernicus and Ptolemy are advocating (2). According to Margolis, "It is only in the Tychonic system that there are two different candidates for the body (Sun or Earth) which is the center." Bottom line: Galileo is alerting his discerning readers that there is a third possibility, the Tychonic system. He is also embarrassing supporters of that system as we will see below.

Next Galileo has Salviati refer to the well known planetary linkages to the sun. If we define 'center' as that place around which the celestial bodies move in circular motion, then "it is certain that the sun not the Earth is at that center." Why would Galileo be allowed to make this statement? Because the consequent of this statement, if true, does not eliminate all geostatic systems. Furthermore, one need not define 'center' in this way. One can acknowledge that the planetary linkages with the sun are so overwhelming as to dictate that the planets revolve around the sun, but still have the sun revolve around a stationary Earth, i.e., the Tychonic system.
According to Margolis, what follows at this point in the discussion -- Simplicio's expected objection and then Salviati's having Simplicio himself draw a diagram -- is best understood as Simplicio not objecting necessarily to the notion of the planets revolving around the sun (it is a realization on his part as well as for the reader), because even if the planets revolve around the sun, the center of the universe could still be the stationary Earth around which the sun revolves.58

Galileo would not have been allowed to claim that it is "certain" (pp. 321, 455), "true" (p. 326), "indubitable" (p. 340), and that planetary revolution around the sun is "the true arrangement" (p. 455) in a book personally approved of by the Pope and in which he was clearly forbidden to argue for a realistic interpretation of heliostasis, unless these statements meant that it is certain that the planets revolve around the sun, and it was still left open what stationary object is in the center and at rest. It is very hard to accept the traditional interpretation -- that the Pope and the censors were buffoons -- when it would have been obvious to anyone who could read that these statements would be an obvious violation of the Church injunction for publication. These statements were not perceived to be a violation, because the intention (at least Galileo's intention for passing the scrutiny of the Pope and censors; he also had another intention) was to refer to the settled matter that
the planetary linkages with the sun are so overwhelming that it is no longer an issue whether the planets revolve around the sun. What remained was whether the Earth moves or the sun. Bottom line: When the Ptolemaic perspective of the planets revolving around the Earth is finally brought up, it is dismissed quickly; what follows from this point on (the discussions of sun spots and tides) address the main issue of whether the Earth moves.

Finally, a major corroboration of Margolis's controversial thesis is that the Inquisition's condemnation of Galileo never raises the obvious objection that he has blundered astronomically as well as theologically by failing to consider geoheliocentrism. It cannot make this complaint, because the members are no doubt aware that Galileo was prohibited from explicitly debating this alternative.

As noted above, with the Tychonic system so well known, it is very hard to believe that Galileo would think that he could get away with such questionable dilemma sophistry of framing the debate so simply, as only between Ptolemy and Copernicus, or that discerning readers would not jump all over him for such an obvious omission. By the time of the Inquisition proceedings it is clear that Galileo had alluded to geoheliocentrism repeatedly and in such a way as to embarrass holders of this position. But the Inquisition cannot refer to this matter, because it cannot refer to a
private agreement made between Galileo and the Pope, even though Galileo has violated that agreement.

The Pope thought he had an agreement that Galileo would not attack the Tychonic system. Perhaps he even thought that this would be a way of embarrassing Galileo for this omission, drawing attention to the Church's forward-looking acknowledgement of the sun-planet linkages, but at the same time discrediting heliostatic supporters for their poor logic. Knowing what the Pope's agenda was, knowing how a simple presentation of Ptolemy vs. Copernicus would be perceived, wanting to display the full support for heliostasis and discredit all versions of geostasis, Galileo not only eliminates Ptolemy (which the Church has no problem with at this time) but embarrasses geoheliocentrism as well without even mentioning it by name. The Pope thought he was using Galileo; he ended up realizing he was used by Galileo. When he is asked, a few days before handing the matter over to the Inquisition, what Galileo has done, the Pope responds furiously that he has been deceived, that it is not any particular passage that is offensive but the whole book, and that Galileo knows perfectly well what he has done, "since we [i.e., the Pope] have discussed . . . [these issues] with him . . ." Galileo was able to signal to discerning readers that he was fully aware of geoheliocentrism as an alternative, pass the scrutiny of the censors by never mentioning it, and yet embarrass it at the same time.
How does Galileo embarrass geoheliocentrism? By repeatedly linking astronomical matters with dynamics. Returning to the key exchange noted above between Salviati and Simplicio, Simplicio, the defender of geostasis, himself draws a diagram that has the planets revolving around the sun (for his "greater satisfaction and . . . astonishment"). He is portrayed by Galileo as being most impressed by this arrangement, but he is then appropriately silent about whether the sun revolves around a stationary Earth or the Earth revolves around a stationary sun. Salviati, of course, completes the diagram by having the Earth revolve around the sun. However, before he does so, he makes it clear that he has trapped Simplicio by referring to a remark Simplicio has made earlier.

Earlier, in what appeared to be an abstract, almost irrelevant discussion concerning the definition of 'center,' Simplicio admits that if our choices are that 'center' means either the stationary place around which key celestial objects move (but not necessarily in circular motion), or the central location around which key celestial objects move, then

If we could stop with this one assumption and were sure of not running into something else that would disturb us, I should think it would be much more reasonable to say that the container and the things it contained all moved around one common center rather than different ones.
But Galileo now makes it clear that there is much to disturb us about any system that has multiple centers. He has allowed Simplicio himself to acknowledge the planetary linkages with the sun, and then has Salviati say,

... it seems most reasonable for the state of rest to belong to the sun rather than to the Earth -- just as it does for the center of any movable sphere to remain fixed, rather than some other point of it remote from the center.63

Galileo's move here is quite subtle, but very clear once read from the standpoint of Margolis's thesis. Galileo has Simplicio the Aristotelian admit that from a dynamical perspective the concept of multiple centers does not seem reasonable. From a purely dynamical perspective, there is apparently no problem on this point for an Aristotelian. In fact, this was often cited as a reason against the Copernican system, because the moon revolves around the Earth rather than the center of the universe. Then Galileo has Simplicio acknowledge the planetary linkages with the sun while framing the discussion from a purely astronomical perspective. Now there is no problem for a defender of some altered version of geostasis. Then Galileo combines the two perspectives, but without great fanfare, demonstrating to the very careful reader that there are major problems when the two perspectives are combined.

Compare this exchange, which never mentions Tycho, with Kepler's direct criticism of Tycho many years earlier.
... many reasons render it likely that the Sun remains in one position at the center of the universe, most of all because in it is the source of motion for at least five of the planets. Whether you follow Copernicus or Brahe, the source of motion for the five planets is located in the Sun. ... However, it is more likely for the source of all motion to remain in one place than to move.

Galileo is repeating the same argument against Tycho made by Kepler! But unlike the freedom Kepler enjoyed, Galileo cannot attach this criticism openly to the Tychonic system. Thus, Galileo's entrapment must be very subtle. Salviati does not remind Simplicio that the unreasonableness of multiple centers is his position. This would be like saying, "But earlier my dear Simplicio you said X. Do you not now realize that this is a criticism of Tycho?"

Galileo, in a most subtle way, has drawn attention to the cumbersome arrangement of a geoheliocentric system and its multiple centers, and planted a major seed of doubt for any "discerning reader" who would begin to wonder how such a system would work.

At this point, Galileo is not allowed to develop this argument by raising more explicitly the issue of exactly how the Tychonic system would work. He has only drawn attention to this next stage of discussion. It is most significant that Kepler, after using the same argument, does proceed to draw out the complexities of the Tychonic arrangement.

According to Kepler,
the motions were vainly multiplied by Brahe as they were by Ptolemy before him. (and) the following schema for physics would have to be set forth: the Sun with all this great burden of the five eccentrics being moved by the Earth, or the source of the motion of the Sun and the five eccentrics attached to it residing in the Earth. 67

What is the "great burden" in the Tychonic system that Kepler is referring to? First, the sun moves in an eccentric orbit around a much smaller Earth, producing variation in speed and a faster motion as it is closer to the Earth. Second, each planet also has an eccentric orbit around the sun, producing variation in speed. 68 Third, combining these motions, the eccentric motion of each planet linked with the sun is in turn linked with the eccentric motion of the sun around the Earth. Fourth, to account for the core observational problem, like a ferris wheel, each planet must have a counter-revolution in the direction opposite that of the sun around the Earth, with the period of the counter-revolution being equal to the period of the sun's revolution around the Earth. Fifth, all other celestial objects in the universe besides the Earth must revolve around the Earth once a day, such that they "must turn in the opposite direction from the Sun's annual motion, 365 times as fast as that motion, along an axis tilted back from the axis of the annual motion." 69

According to Kepler, a heliostatic system is able to strip the five planets of these "coiled" courses and "extraneous" 70 motions by adding only a few motions to the
Earth and having a single motion for each planet (a single motion around the sun). Even Tycho seemed prepared to lessen this great burden by having the Earth rotate daily! Logomontanus, Tycho's principal disciple, continues the auxiliary patch by making the Earth's rotation explicit, and by the time of Kepler's *Astronomia Nova* this no longer seems to be an issue. But what has happened? The auxiliary patch has become more trouble than it is worth. The very patch introduced to save the Biblical, Aristotelian, and common sense position that the large and sluggish Earth could not move has ended up undermining that position. The very auxiliary patch that was supposed to save geostasis has exposed geostasis as a "worm-eaten" system, as a hypertextual adjudicatory trail unraveling along many nodes. The relativists are right in principle. We can patch away to our heart's content. They are even partially right in terms of historical fact -- scientists will often attempt to patch their theories in the light of problems. But they are conspicuously wrong that just any patch will be accepted in the long run, that all patches can be made to work given effort and resources, and that scientists do not abandon adjudicatory trails for very good reasons, being fully capable of seeing their positions crumbling and the proverbial writing on the wall.

The social-psychological relativist would assert that a scientist may change his or her mind at such a point,
because they do not want to be embarrassed and consequently socially excluded from a new emerging consensus group. From the standpoint of this thesis, a trivial point. What causes the recognition of potential embarrassment is recognition of the epistemic situation, and relativists have their own regress problem unless we postulate that at least some psychological states are caused by a reaction to epistemic explorations. Psychological states may predispose a scientist to view the world in a particular way, to defend or explore initially a hypertextual adjudicatory trail, but different psychological states may emerge after such exploration. A young Tycho apparently felt no embarrassment in defending Ptolemy, but the mature Tycho, after a lifetime of debate and detailed study, was experiencing many doubts and would have surely experienced embarrassment if he were still defending the basic Ptolemaic system.

Tycho hesitated for a long time in publishing his system (at least 10 years\textsuperscript{73}). When he finally did introduce the geoheliocentric system it was only a sketch as part of a brief three page digression in a treatise on comets and novae. Even this introduction seems to have been offered reluctantly due primarily to the fear that someone else (Ursus) would get the credit.\textsuperscript{74} In the light of the above analysis, his hesitation is most likely due not only to his acknowledged, transitional struggle with crashing celestial spheres, but that jettisoning these spheres is another slap
at Aristotelian cosmology and dynamics. Without the spheres there is now no obvious means of generating the motions of the planets, of transmitting the motion from the celestial sphere on down to the moon.

Relativists will often make much of the fact that just because Tycho and others gave up on celestial spheres does not mean that they ought to have done so. Aristotelian cosmology could still have been saved with enough effort. Comets and novae were observed before. Observations are just "stimulations"; they require interpretations before they acquire meaning. If our theories clash with observations, we can always reject the offending interpretation of the observations. But this purely logical argument, disconnected as it is from the actual scientific and historical context, ignores the ampliative basis for accepting the observations. It ignores the vast difference in evidential contexts. For instance, the comet of 1577 was very bright and located in a well known constellation (Cassiopeia). Tycho's observatory at Uraniborg was well established, and his instruments and instrumental techniques were far superior to any previously used. Furthermore, from the late 16th- to the early 17th-century a significant number of comet and nova sightings occurred (1577, 1580, 1582, 1585, 1590, 1602). Holes were literally being punched into Aristotle's celestial sphere. Aristotelian cosmology was under pressure along many fronts.
The very title of Tycho's book, *On the Most Recent Phenomena of the Aetherial World*, is a challenge to the orthodoxy of the Aristotelian world view. The very world view that Tycho set out to preserve. Tycho may have set out to rescue "the Copernican harmonies . . . from the Copernican heresies," but he ended up showing that orthodoxy had just as many problems if not more than the heresies.

**Parameter Determination**

According to Philip Kitcher in his recent book *The Advancement of Science*, "A central problem of scientific inference consists in showing how endeavors to find paths through escape trees yield various types of modifications of practice." Scientists routinely experience "Duhemian predicaments" and must address the "costs" of amending such situations. Put in terms of this thesis, much of intelligent scientific inference involves probing and pursuing, adjusting, testing, perceiving a strengthening or weakening, and ultimately accepting or rejecting hypertextual adjudicatory trails. These activities have constraints, but these constraints are themselves part of the developmental process. They are not a priori, transtemporal fixed principles, but emerge as hypertextual nodes that may be linked with conflicting adjudicatory trails. Furthermore,
they can be argued about and accepted or rejected independent of theoretical allegiance.

I have argued that one of the standard defenses of scientific rationality during the Copernican episode is an historical myth. It is not true that by the time of Copernicus's *De revolutionibus* the Ptolemaic system was so patched up and in crisis, that rational people clearly saw that the Copernican system was so simple that it must be right. It is not true that only the heliostatic system had a basis for rational pursuit in the 16th-century. I have also argued that a finer-textured analysis shows the traditional view to be partly right. The Copernican system displayed an order, unity, and parameter fixity that gradually impressed every competent player. The supporters of heliostasis repeatedly raised this issue. Should not our theories display an overall unity and fix as many parameters as possible? And like a red flag that could not be ignored, the defenders of geostasis responded, not by rejecting the importance of this general aim, but by accepting it and pursuing a geostatic patch that would fulfill it.

The key epistemological question that can now no longer be postponed is, "What normative basis is there for judging this to have been a rational move on the part of the geostatic and heliostatic participants?" There are usually three answers to this question, all of which I will reject.
First, relativists respond to this question by claiming, of course, that there was no normative basis. Helio-static and geostatic supporters agreed simply because of the cultural milieu in which they were embedded. It was "mere" agreement due to the neoplatonic and neopythagorean metaphysical ambience that permeated the renaissance. It was merely "aesthetics" backed by a world view. The participants had some silly anthropocentric views about the relationship between God and human intellects: God had created everything according to some elegant mathematical floor plan; the most elegant astronomical system therefore must be true, and participants saw their lives as a religious and scientific race to be the first to read the mind of God.

Second, defenders of Legend would argue that this was not mere agreement. That logical unity, order, and parameter determination are a priori, fixed principles that have always governed rational scientific practice. When they are not recognized as such, then those involved are acting irrationally. Hence, Copernicus, Rheticus, Galileo, and Kepler were acting rationally; all defenders of Ptolemaic geostasis were acting irrationally, even from day one of the introduction of his system -- they should have known better.

Third, we have learned over time that theories that display a high degree of logical unity, order, and parameter
determination are not only easier to test, but also generally have a long term reliability. A scientist today would not be very comfortable supporting or pursuing a theory analogous to that of Ptolemy, where a model explaining some individual feature of observation was not consistently linked with another model explaining another feature of observation.\textsuperscript{80}

My response to the first view has been similar to that of Lakatos. That an external, historically contingent feature helps participants to see a particular internal feature as important does not unequivocally indict the internal feature as not important. A methodology or a theory is perfectly capable of possessing long term reliability, and can display features indicative of long term reliability, even if the motivating factors for their origin are later abandoned. The reliability of the current species classification scheme used by evolutionary biologists is not called into question because Linneas believed he had discovered God's floor plan for life on Earth.

My response to the second view is that it makes too much of parameter determination. If parameter determination and associated concerns of theoretical unity are fixed, a priori principles, then large tracts of history must be seen as consisting of irrational participants. Ptolemy must be seen as a poor scientist who should have known better right away that his system could not be true. Yet Ptolemy was
obviously a great scientist, and we need a story of scientific change that captures this appraisal. Popper may have taught us that a good theory should prohibit as much as possible, but this is 20th-century advice. Such a constraint is something we have learned over a vast stretch of time by observing the success and failure of competing theories. We can hardly expect that such advice was fully formed at Ptolemy’s time, especially without a robust competing theory.

Of the three views, this thesis has the most sympathy with the third view. Constraints, I have argued, emerge as part of a learning process over time. However, what applies to Ptolemy still applies to a certain extent to the formative stages of the Copernican episode. Problematic for this third view is its hindsight perspective that does not help us very much in explaining the rational moves made in the late 16th- and early 17th-centuries. Even if there is agreement that parameter determination has been important in the 20th-century science, modern scientists have had the backing of almost 400 years of experience with theories succeeding and failing. So, why should parameter determination have been considered normatively important in say 1590?

As noted previously, there would have been no expectation in the Copernican episode for any new planets. However, theories make predictions, and whether one is a realist or an instrumentalist one is interested in backing a
theory that makes the most successful predictions. To make the most out of a theory’s predictive potential it must be linked with as many nodes on a hypertextual adjudicatory trail as possible. In this light consider the following cases.

**Case 1:** A theory T1 that models a feature F1 that predicts an observation O1, and a theory T2, with F2, that does not predict O1 or -O1, but is compatible with either O1 or -O1 occurring. If -O1 occurs, comparatively, this is discriminating evidence for T2. On the other hand, if O1 occurs, this is discriminating evidence for T1.

As we have seen, for Ptolemy a future observation of a new superior planet need not show increased brightness at opposition. In the late 16th-century if a new superior planet had been discovered and there was clear observational agreement that it did not show increased brightness during opposition, this would have been well-defined evidence for Ptolemy. I want to claim now that if a new superior planet was discovered in the late 16th-century and it did show increased brightness at opposition, defenders of heliostasis would have had an easier time claiming that this was evidence for their system, even though defenders of Ptolemy would have had little trouble in covering the observation once it was already made.

A defender of geostasis or a relativist might object to my last claim. They might respond that the new observation is perfectly compatible with the planetary linkages with the
sun already established by Ptolemy’s models, i.e., that superior planets are always at perigee in their epicycle orbits at opposition. It is simply a fact of the matter that the epicycle radii of superior planets have the orientation that they do in relation to the sun. It is simply a basic principle of epicycle theory that the motion of the radius vectors are linked with the sun such that they turn at the same uniform rate as the sun’s radius vector.

Accordingly, there are two problems with using Copernican parameter determination as discriminating evidence. First, the above scenario is purely prospective. No new observations were available to show definitively whether the Copernican system was surprisingly correct or rigidly wrong. The observed pattern could have been only a coincidence; it could have been discovered that superior planets need not continue to show the same pattern of retrogression frequencies, brightness, and orientation in terms of the Earth and sun. Second, an established pattern is relative to the eye of the beholder; that the Ptolemaic system is compatible with 01 or -01, and 01 occurs, can be seen as a continuation of a pattern nevertheless.

There is a robust response to the second objection. Ptolemy’s prediction is at best strongly inductive; whereas that of Copernicus is rigorously geometric. There ought to be an uneasiness over any theory that does not make a precise prediction relevant to a core observational problem.
All the superior planets had shown a particular pattern in terms of frequency, brightness, and relationship with the Earth and the sun. Does Ptolemy predict that this pattern will continue? If so, then in what sense? We ought to be uneasy about a theory that lets its supporters hedge on the answer to this important question, supporters who could say, "I'll let you know after we make the observation." Although their way of putting the matter was different than my new planet example, I contend that what was being realized by the major players, after a comparison was available, was that the basic principle of epicycle theory cited above had been invoked (albeit elegantly) to cover only the current observational situation. If Ptolemy is not making a precise prediction about future observations, an uneasiness is engendered about how the models match current observations. As we have seen, the "looseness" of Ptolemaic fit between models and observations was now apparent to supporters and nonsupporters.

Consider the planet Mercury in the Ptolemaic system. Ptolemy's model for the inferior planets is consistent with Mercury being either the closest planet to the sun or the second closest to the sun. Furthermore, if a new inferior planet were observed, the observation of its degree of elongation would leave us with the same indeterminacy. In fact, the indeterminacy would now be compounded. We would not be able to tell which interior planet -- Mercury, Venus,
or the new planet -- was closest to the sun. For Copernicus, elongation observations rigorously determine that Mercury must be the closest to the sun, and the placement of any future inferior planet would be rigorously determined by the observation of its degree of elongation compared to that of Venus and Mercury. So, suppose that during the peak of the Copernican episode debates Tycho had figured out some independent means for determining that indeed Mercury was the closest planet to the sun? A clear evidential plus for Copernicus, even though Ptolemaic supporters could easily adjust once the determination was made. On the other hand, if Tycho had been able to detect that Venus was the closer planet to the sun, the Copernican system would have been in serious trouble.

Although none of these observations or determinations could be made, the story told here is that the major players are starting to realize that the indeterminateness inherent in the Mercury-Venus ordering problem permeates much of the Ptolemaic system when it is compared to that of Copernicus. As noted previously (note 18), realizing that Ptolemy has a more flexible response to surprise changes in the core observational problem, helps us realize that the major players were beginning to see that Copernicus had a more fixed response to the known core observational problem.
Today we would be just as apprehensive about a theory that predicts the next car that enters my college's parking lot will be red or blue, black or white, or green or yellow. If our domain of inquiry was my college parking lot and the core observational problem was that an observational pattern has been established regarding the colors of cars, their frequency and timing of appearance -- red, black, and green cars at a certain times -- should one have a very high motivation for pursuing a theory that makes such a vague prediction, provided also that another theory existed that predicted only the red, black, and green pattern? The reason a Copernican would have had an easier time claiming predictive success, if the pattern regarding superior planets continued for a new superior planet, is that Ptolemaic supporters would have had to face the fact that the real prediction that the Ptolemaic system was making is that the pattern need not continue. Ptolemaic supporters cannot have it both ways. They cannot argue that a future $\sim 01$ is compatible with the basic geostatic model and simultaneously argue that 01 is a requirement.

As to the first objection consider the following case.

**Case 2:** T1, having features F1, F2, and F3, predicts 01, 02, and 03. Furthermore F1 is such that it could not exist unless F2 and F3 also exist, F2 could not exist unless F1 and F3 exist, and F3 could not exist unless F1 and F3 exist. Thus, T1 predicts 01, 02, and 03 as a unified sequence. The occurrence of $\sim 01$ would significantly and negatively affect the ability of T1 to take credit for saving 02 and 03, even if they occur. Comparatively, T2,
having features $F_1'$, $F_2'$, and $F_3'$, is compatible with $O_1$ or $-O_1$, $O_2$ or $-O_2$, and $O_3$ or $-O_3$. If $-O_1$ occurs, $T_2$ is not significantly impaired in terms of covering $O_2$ and $O_3$.

$T_1$ is of course the Copernican system. Although many other features could be selected, $O_1$, $O_2$, and $O_3$ can represent, respectively, superior planetary brightening at opposition, frequency of retrogressions, and degree of elongation of inferior planets from the sun, the latter in the Copernican system implying a definite commitment on planetary ordering of these planets.\(^2\) $T_2$ represents the Ptolemaic system. The Ptolemaic system is not committed a priori to the necessity of planetary brightening at opposition for the superior planets, nor a particular frequency of retrogressions. Furthermore, there is no simple relationship between observations of degrees of elongation and the ordering of Mercury and Venus. Ptolemy’s geometric models are compatible with either Mercury or Venus being the closest planet to the sun. Ptolemy was fully aware of this, but also knew that he needed to make a choice. (It is obviously one way or the other in reality.) He chose Mercury as being closer to the Earth than Venus by invoking an aspect of Aristotelian cosmology. Venus must be closer to the sun, because Mercury had the most erratic orbit of the two, so it must be closer to the corruptible, vaporous sublunary realm.

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Again the obvious objection to make against Lakatos and others who view this comparison as representing decisive empirical evidence for Copernicus is that the situation is still prospective. Parallax measurements of Mercury and Venus were not possible, nor were any new inferior planets discovered showing bounded elongation consistent with Copernicus's prediction. However, a case can still be made as to why the Copernican system impressed everyone, and continues to impress us to this day when one fully appreciates all the initially appearing, discordant observable details it ends up linking. But what is it? And why should we be rationally impressed? What is it, in Duhem's words, that it is impossible for us to be blind to?

Consider carefully the difference between Cases 1 and 2 above. In Case 1, the fact that T1 fixes one observation may be interesting, but not overly impressive. At this point in any investigation of a rivalry between two theories, it could simply be a coincidence that T1 fixes this observation, whereas T2, although compatible with 0₁ and -₀₁, does not. From the perspective of the 16th-century, and without any other information, the world could have indeed been such that although superior planets to date had increased brightness at opposition, future ones may not have.

However, the paramount difference between Cases 1 and 2 is that in Case 2 the fixation of observations is no longer
just individual fixations. They are now not only beginning to add up in a purely quantitative sense, but they are linked. What is operative here in terms of our "astonishment," to use Kepler's portrayal, is that when faced with such unity, we are less likely inclined to believe the features are a coincidence. Contra the philosophical positions of the likes of Lakatos and Glymour, I am not suggesting that such linkages can be counted as extra empirical evidence for the Copernican system, nor that prior to other matters being settled along a hypertextual adjudicatory trail can such linkages be decisive relative to acceptance. What I am suggesting is that we find such unity impressive because it supports the primary aim of intelligent ampliative inference -- our desire to obtain long-term reliable beliefs -- and as such, one can clearly see the very good reasons a 16th- or early 17th-century scientist would have for a robust pursuit of heliostasis.

We make use of ampliative inference knowing full well that we cannot achieve certainty for our conclusions. As such, we develop methodologies attempting to tease from nature her secrets, by testing those methodologies over time, hoping to find those that produce conclusions that are less likely to be wrong. Of paramount importance in this endeavor is to get nature to respond in such a way that the response is less likely to be a coincidence. We know, from painful past experience, that she is in the business of
fooling us with her responses, of displaying what we think are regularities initially only to find later that they were coincidences.

For instance, consider the use of the technique of a controlled study that has exposed the relationship between cigarette smoking and lung cancer. We do an initial study, setting up two groups of say 100,000 people to follow, controlling as many variables as possible, attempting to limit the major difference between the two groups to the fact that one group consists of cigarette smokers and the other does not. After ten years we find that in the group that smokes cigarettes almost 5,000 people have developed serious cases of lung cancer; whereas, in the non-smoking group only 34 cases of lung cancer exist. We are impressed, but the tobacco companies, to use Kitcher's language, have an escape tree. Some major variables have not been controlled. At this stage it is possible that nature's response is a coincidence, that the 5,000 people in the smoking group and the 34 in the non-smoking group have something in common other than prolonged exposure to cigarette smoke. Perhaps lung cancer is very common in their family histories. Perhaps they have inherited a tendency to develop lung cancer and it matters little whether they were in the smoking group or not. We could repeat the study with different people and might find the numbers reversed, and that the real common denominator was heredity. Or, perhaps all of the cancer
victims lived in homes with high concentrations of radon gas, and it was just a coincidence that our study placed so many people in such homes in the smoking group. So, the ampliatively intelligent thing to do is to repeat these studies, using different investigators, different people, and control for variables left out in the first study.

Suppose we conduct such studies literally thousands of times.83 If the results are virtually the same in every case -- a significantly (140 times) higher incidence of lung cancer cases in the smoking groups -- it is still possible that these results are a massive coincidence. But the ampliatively intelligent inference to make is that it is not a coincidence, and it is very unlikely that most of these people have something in common causing the cancer other than cigarette smoking. It is possible that the thousands of people with lung cancer did something on their third birthdays that eventually produced lung cancer. It is possible because this and many other lifestyle behaviors have never been controlled for. However, there are no rational grounds for pursing any of these escape trees. Nor, if we were to start worrying about classical confirmation problems at a purely logical level (Goodman's emerald paradox), is there any rational reason to believe that although smoking may be the cause of lung cancer now, this will change in the year 2010. We do not have any ampliative reason to believe that nature's causal laws work one way in
the 20th-century and then work a different way in the 21st-century.

Consider another example. If one was overly impressed with a strictly logical approach to confirmation theory, one could make a case that we should not be too impressed with how Darwin's theory of natural selection predicts and explains the massive amount of extinction in the fossil record. One could see it as no more than simple and highly suspect induction by enumeration. Each example of species extinction is seen as a positive case for Darwin's theory, but since such simple generalizations have been wrong so many times before, and because the set of possible species outcomes is infinite, the positive cases of extinction already observed confer zero probability on Darwin's theory. However, even without bringing to the table other substantial support for Darwin's theory and the fact that competing theories do not predict massive amounts of extinction (Lamarckism and Scientific Creationism), when one comes out of one's lofty logical tower and gets paleontologically dirty with the actual observational situation, seeing how difficult it is to explain anything, one does not see just individual positive cases for Darwin, but rather thousands of linkages of species extinction, mutation and variation, and environmental change, such that we are left with the ampliative feeling that it is very unlikely that the connec-
tion between natural selection theory and this evidence is a coincidence.

According to Kepler, if one looks at "only the numbers" -- the ability of the Copernican and Ptolemaic systems to predict planetary positions -- then we cannot provide any rational grounds to pursue or accept heliostasis. However, if we are also interested in how the world works and/or in possessing theories with long term reliability, we must be also interested in a unified theoretical approach. We must be interested in how our geometric theory works with other fields of study, not the least of which is physics. Furthermore, even within the purely geometric models we ought to be concerned with how the various aspects of the models link up with the observations. We ought to be concerned with such unity, because realists and instrumentalists alike concur that it is most likely that the world we are dealing with is one world, one world of universal, objective features and regularities that do not show one face on Mondays, Wednesdays, and Fridays, and a different face on Tuesdays and Thursdays. If we believe that there is an objective world, we also believe that its fundamental features are locked together in a functioning whole (Duhem's natural classification), that the different faces nature shows us are only apparent, that behind the scenes are natural mechanisms of some sort that stay the same. Thus, even if we are instrumentalists we must be concerned with
unity and linkages, because if our metaphysical hypothesis of the general nature of reality is true, then those theories that show such unity and linkages are more likely to have staying power in terms of long term reliability. Merely having this unity does not guarantee truth or reliability, but without it, it is very unlikely that our theories will have staying power; it is a goal we seek because of our general belief about the nature of reality.86

Very much related to this metaphysical justification is the pragmatic and heuristic value of finding theories that, if reliable, will have ramifications for future and/or collateral domains of experience. According to the astrophysicist Neil de Grasse Tyson, 

In scientific inquiry, the answer to one simple question often fortuitously explains the answers to many others; it may even answer questions that have yet to be conceived. Powerful ideas also unify concepts or phenomena that were previously thought to be unrelated.87

Once we think we have found a pattern shown by nature, we desire a theory that not only captures that pattern but provides some distinct leads about what we should find "down the road" so to speak. We are uneasy about any theory that captures a pattern already observed, and then predicts that next we might observe 01 or ~01, 02 or ~02, or 03 or ~03, and so on. We have learned from experience that such theories are not helpful and we believe that they are unlikely to be helpful, because we do not believe the world
works this way. We believe that the patterns we observe are linked with many other distinct features and patterns not yet observed.

Today physicists are unhappy simply knowing that the fundamental constants of matter have the values that they do. Why is a proton 2,000 times more massive than an electron? Why is the force of gravity so much weaker than the other forces of nature? They do not believe that such parameters are coincidences, but must be linked by some fundamental process. Thus, they pursue a logically consistent theory of everything in which all such constants would be derived, in which all such constants would be fixed as having these values and no others. They do so, not only because the fruitfulness of such a theory would be enormous -- from a seamless linking of every stage of the Big Bang, to why we have the kind of universe that we do rather than a very different one, to interactions of matter heretofore undreamed of -- but also because of our justifiable suspicion that reality works this way, i.e., we live in one world and the patterns that nature shows us are linked. Our metaphysical theory could be wrong, but ampliatively speaking, it is the best we have at present.

There is no puzzle then why rational human beings during the Copernican episode began to pursue either the Tychonic or Copernican systems. Once the Copernican supporters drew attention to the unity that could be
achieved by modifying the geometrical account of the linkages with the sun (and how this unity extended to the settings of other parameters), there was no going back. It would not have been rational to pursue a theory that had theoretical unity but could not deal successfully with the core observational problem, but it surely was rational to pursue a theory that could match another observationally and had greater overall unity. Thus, we see that Duhem was partly right -- it is impossible for us to ignore the need for unity; but he was also partly wrong -- we are not powerless to justify using it as a constraint. We are powerless to justify this goal of unity from some transcendental perspective -- our metaphysics, of course, could be wrong, and our ampliative methodologies may have been tricked all these years. We may not be part of one world and all our experience of repeatable patterns may not be indicative of a unity behind nature's complexity. We could simply be brains in a vat of preservative chemicals connected to a computer via electrodes simulating the reality of a unified external world, and our ampliative goal of using methodologies that pierce through the top layers of nature's tricks and coincidences thus totally quixotic. But we have little reason to believe such skeptical scenarios.  

A large part of the interpretation of scientific practice defended in this thesis appears clearly in Kepler's philosophy of science. First of all, according to Kepler,
the oft cited empirical equivalence of various hypotheses used by skeptics is merely the result of appraising such hypotheses in isolation. For instance, one historical source of empirical equivalence arguments is Hipparchus's demonstration that the motion of the sun can be saved by either an epicycle-deferent or eccentric device. But according to Kepler, such hypotheses are "small change" and are not "truly astronomical hypotheses." This demonstration was for one astronomical object only, and was neither linked with many of the other details of the core observational problem nor a complete set of auxiliaries.

In other words, as in Case #1 above, the interpretation of evidence for an individual hypothesis is misleading if we do not consider all sources of evidence; specifically, how each hypothesis is linked with other hypotheses in a full web of belief, both within a particular field, such as astronomy, and related sciences, such as physics, and the ramifications that result for the whole system in accepting the particular hypothesis. According to Kepler, it is a mistake to ignore "the diverse outcomes which weaken and destroy [the] vaunted equipollence when one takes account of related sciences." 

Secondly, "that which is false by nature betray itself as soon as it is considered in relation to other matters." In the language of this thesis, accepting a particular node along an adjudicatory trail has ramifications. Although
from a purely logical point of view with sufficient imagination and creativity, any particular node can be defended "come what may," in actual practice this can only be done by ignoring well supported hypotheses in related fields and accepting a possible but highly unlikely support structure. According to Kepler, this can only be accomplished if "you would willingly allow him who argues thus to adopt infinitely many other false propositions and never, as he goes backwards and forwards [in his arguments], to stand his ground."92

To return to an example from the Introduction, most astronomers of antiquity were convinced that the observed variation in planetary brightness was best explained by variation in distance from the Earth, and hence eccentric orbits of some kind. However, a defender of homocentric orbits could appeal to the auxiliary hypothesis that planets oscillate in size, and that during opposition planets swell up, becoming brighter, providing the illusion of approach to and variation in distance from the Earth. As a mere possibility, there is no absolute way to block this escape tree, but it is not without significant costs. If we ask the defender of homocentrics "to stand his ground," he must explain how this auxiliary is consistent with the then prevalent conception of planets. Even within an Aristotelian cosmology, such a conception of the nature of planets would have massive negative ramifications. It is possible
given sufficient creativity and imagination to tinker further with this save, to patch the patch, and possibly find a way of making the patch consistent with Aristotelian cosmology. However, the rational move taken by the vast majority of astronomers was to assess the situation ampliatively and conclude that such a trail was unworthy of pursuit due to its great likelihood of failure.

Similarly, although tobacco companies can continue to safely state on purely logical grounds that the link between cigarette smoking and lung cancer has not yet been proved, no scientist today would propose a grant to examine what all the lung cancer victims, in all the studies of the last 40 years, were doing on their third birthdays. Without an examination, it is possible that chewing a particular type of gum on a third birthday was the real common denominator of lung cancer victims in both groups. Such a variable, and many like it, have never been tested. But the scientific community knows full well that grant money will not be forthcoming for such studies. This response is not a mere conservative consensus. There is no ampliative basis for opening up this escape tree; we have no reason to believe that chewing a particular type of gum on one's third birthday is causally linked with eventual lung cancer.

Note that the situation was notably different in the late 16th and early 17th centuries for supporters of geostasis and heliostasis. For the defenders of geostasis,
the relative merits of its empirical strengths and weaknes-
ses were well known, and a major rival had been proposed
that was not only able to match the empirical success of
gestasis but propose, unlike the tobacco companies,
auxiliary nodes that had to be taken seriously. As we have
seen, there was a sufficient ampliative basis for defenders
to pursue geoheliocentrism. Furthermore, the very reasons
that supported pursuit of an escape tree for geostasis, also
supported heliocentric pursuit. The nodes on the various
hypertextual adjudicatory trails were not mere logical
possibilities, but serious contenders based on the total
empirical and conceptual developments of the time.

The misleading point that relativists consistently
flaunt is that from a purely logical point of view the
development of empirically equivalent hypertextual
adjudicatory trails is "an easy matter." In his attack on
Tycho, Ursus made use of this point to claim that priority
in the development of geoheliocentrism was not a big deal.94
In his Apologia pro Tychone contra Ursus, Kepler seizes upon
this point and responds that only "a thoughtless man who
pays attention only to the numbers will think that the same
result follows from different hypotheses and indeed that the
truth can follow from falsehoods."95 Scientists do not
propose solutions to problems while ignoring the linkages
that a particular hypothesis has with many other nodes along
an adjudicatory trail. Such a characterization
underrepresents what scientists do, and ignores "the long and tortuous course" of real science. According to Kepler,

To predict the motions of the planets Ptolemy did not have to consider the order of the planetary spheres, and yet he certainly did so diligently. To predict and expound a method of calculation for the heavenly motions Copernicus and Lord Tycho after him did not have to ask why it is that the planets at their evening risings become nearest to the Earth. For they could have produced the same results even by using the Ptolemaic form of the heavens with the dimensions corrected. But love of finding out about nature made astronomers take up the exploration of this part of physics on astronomical grounds. . . . What about Copernicus? He censored a certain non-uniformity of the motion of the epicycle in Ptolemy, not on the grounds that this motion conflicts with what is seen and with our experience or observations of the stars, but because it is in conflict with the nature of things; so from this, he declared, he derived his motive for parting company with Ptolemy.

For Kepler, even if the numbers in terms of celestial coordinates are the same, even if one can make neat mathematical models and elegantly demonstrate their equivalence in the cozy confines of pure geometry, systemic linkages with other considerations produces a whole host of ramifications in terms of present and future evidence. To cite, according to Kepler, only a few examples: In comparing the Ptolemaic and Copernican systems, there is a different treatment of the superior planets in terms of distance to the Earth during opposition providing the possibility of discrimination by a parallax effect. The Copernican system
predicts that Mars should show a greater parallax than the sun.\textsuperscript{98} In comparing the Ptolemaic and the Tychonic systems, on the one hand, with the Copernican, on the other hand, there is a significant difference in prediction of stellar parallax.\textsuperscript{99}

The fact that Tycho would initiate a systematic attempt to observe these projected parallax discriminations shows that the astronomers of the time were well aware of many of the ramifications of different hypertextual adjudicatory trails and were testing the nodes along those trails.

To sum up. The major virtue of emphasizing the role of parameter determination in the light of scientific practice as pursuit, adjustment, and acceptance of adjudicatory trails is that we are allowed to see the majority of players during the Copernican episode, both supporters and nonsupporters of heliostasis, as rational. Furthermore, although the major players had many nonepistemic influences, these influences not only helped articulate the competing adjudicatory trails, thus producing variety of practice and a robust debate, but also, in the end, the nonepistemic factors did not overwhelm epistemic considerations. If nonepistemic influences had been paramount, it is most likely that the Copernican revolution would have never taken place, that at least some version of geoheliocentrism would have been defended for far longer than it was. But as we
have seen the position of geostasis crumbled in the light of its own articulation and competition from heliostasis.
Notes for Chapter 5


3. Below I will deal with the issue of whether or not Ptolemaic astronomy was a complete theoretical system. My contention will be that it was not only intended as such by its author, but it is a misrepresentation of the debate to assert that Ptolemaic astronomy was not a system and that of Copernican astronomy was. By the end of the 16th-century, it was clear that both sets of astronomical models had an uneasy marriage with their respective supportive physics, and major questions were being raised concerning the implied cosmologies of both theories.

4. According to Gingerich and Lightman, "The principal challenge for the astronomers of antiquity and the Renaissance was to account for the seemingly irregular motions of the planets among the stars, especially the so-called retrograde motion. . . . In the sun-centered system of Copernicus, this phenomenon is easily explained." 1991, p. 691.

5. This claim, of course, appears massively inconsistent with the date and content of Galileo's Dialogue. However, I will argue below that Howard Margolis is correct: The long standing puzzles of the Galileo affair can best be explained by seeing that one of Galileo's main targets in his Dialogue is not Ptolemaic geostasis, but rather Tychonic geoheliocentrism.

6. Peterson, 1994. Recall from the Introduction that I am claiming that there is a very close relationship between parameter determination and at least one type of theoretical unity.

7. One response to this question is that it did not. In this regard I will comment on Westman's Wittenberg Interpretation below.

8. Rosen, 1971, pp. 164-165; 167-168. Emphasis added. These paragraphs show the connection perceived by Copernican supporters between parameter determination, explanation, and theoretical unity. The phenomena, according to Rheticus, of the core observational problem are not only saved but linked in a rigorous geometric way, and the details of each planet -- position, order, and details of inequalities (retrogressions) -- bound up "into one" theoretical unity. Note also that Rheticus "sincerely cherish(es)" what Ptolemy had accomplished, but is arguing that upon comparison of the two systems there is more unity in the Copernican treatment.
9. Westman, 1975c, p. 333. Emphasis added. Again, the "great argument" was viewed to be that the "order and magnitudes" of planetary parameters are fixed and "bound together" in a heliostatic model.

10. Jardin, 1979, p. 157; Gingerich, 1993, p. 327. Emphasis added. Here we see the connection between fixing key parameters and explanation, the latter being Kepler's main concern.

11. Galileo Galilei, 1632, Drake translation, 1953, pp. 341-342; 344-345. Emphasis added. Here Galileo lists the details the others are referring to -- the qualitative and quantitative details of the anomalies (inequalities) in the motions of all the planets -- and repeats the connection with explanation and theoretical unity.

12. Gingerich and Lightman, 1992, p. 692. Here Frisius refers to an example of parameter determination that figured prominently in the persuasiveness of heliostasis. All the planets show increased brightness at opposition and this is geometrically required by heliostasis. It is debatable whether this is required by Ptolemy (see below), but Frisius is drawing attention to the fact that none of the historical players perceived it as required ("a reasoned fact") in geostasis.

13. Gardner, 1983, pp. 239-240. Emphasis added. Praetorius's "symmetry of the orbs" is equivalent to Rheticus's "golden chain." Here Praetorius expresses the ambivalence of many nonsupporters of heliostasis: calculations were easier and heliostasis surely possessed a desirable theoretical feature, but the motion of the earth conflicted with accepted auxiliaries.

14. Blair, 1990, pp. 359-360. Here Tycho repeats the fixation of planetary oppositions and refers to another parameter that featured prominently in the minds of those who supported planetary linkages with the sun, i.e., bounded elongation of the inferior planets (Mercury and Venus).

15. Westman, 1975c, p. 317, 319. Read "exquisite order" as "golden chain" and "symmetry of the orbs."

16. Translation by Boas and Hall, 1956, pp. 258-259. Emphasis added. Here Tycho makes clear that the superior elegance of the Copernican linkages with the sun should not be couched simply in terms of the number of epicycles. Rather, it is the synchronized fixation of the details of planetary motion by Copernicus that is most impressive.

18. According to Gingerich, "(I)n an Earth-centered system, such a coincidence (parallelism and bottom positioning) is not required by the geometry." (1991, p. 691).

This needs an important qualification. Although Ptolemy does not explain the parallelism and bottom positioning, they are required in his system to save the known observations. What the major players are realizing ("no necessary cause or . . . explanation," according to Tycho; no "reasoned fact," according to Frisius) is that if the observations of linkages with the sun had been different, Ptolemy has the geometric machinery to adjust. No one, of course, at the time would have expected any other planets to exist, but my appeal to this thought experiment is a way of showing the dramatic difference between the two systems recognized by those who carefully studied the two models.

The flexibility inherent in the Ptolemaic system is often misinterpreted to mean that the geometry is full of ad hoc jiggery. This interpretation fails to recognize the elegant response Ptolemy provides to the core observational problem when viewed (as it was for over 1400 years!) with no competitor. However, realizing that Ptolemy has a more flexible response to surprise changes in the core observational problem, reveals that Copernicus has a more fixed response to the known core observational problem.

19. Both Laudan (forthcoming) and Margolis (1987, p. 237, n. 2) make this point against the strong epistemic interpretations of Lakatos and Glymour (1980, chapter 5). According to Glymour, although the evidence is relatively equal empirically,

"The question is whether the (same) evidence provides better grounds . . . for the Copernican theory than for the Ptolemaic. I believe it does. There are several respects in which the bearing of the evidence is different for the two theories. (p. 193, emphasis added)

My way of rendering (developed below) how I differ from Glymour is to admit that the new planet examples are counterfactual, and hence, cannot consist of crucial tests of the two theories and a basis for acceptance of one theory over the other. Furthermore, that one theory T1 determines key parameters while another theory T2 treats them as coincidences,
does not decisively tell us that T1 has got things right. The question of whether we are dealing with coincidences or fundamental processes is often the fundamental ampliative issue to be decided by further testing. However, we desire knowledge of fundamental processes because we have learned from experience that such theories have staying power and lead fruitfully to many other discoveries, providing connections to many other fields and answering questions that we did not even know we had. So, we pursue theories that tell us stories about fundamental processes.

20. Kuhn, 1957, p. 180. In his 1957, Kuhn can not seem to make up his mind on how much weight to give parameter determination. While acknowledging that overall the Copernican ability to fix key parameters related to the core observational problem is the "the single most striking difference" (p. 141) between Copernicus's system and that of Ptolemy, that heliostasis offers "a simpler and more natural account" (p. 171) of the phenomena, that it possesses "a naturalness and coherence" (p. 176) not possessed by geostasis, and that the "sum of the evidence drawn from harmony is nothing if not impressive" (p. 180), he nevertheless concludes with his famous claims that "it may be nothing" (p. 180), that the "apparent economy" was simply "a propaganda victory" and "largely an illusion" (p. 168), and that the appeal of aesthetics was only persuasive to a "perhaps irrational subgroup" of neoplatonic mathematicians (p. 180). This indecisiveness on Kuhn's part is due to a flawed holistic interpretation of scientific change, and the fact that Kuhn makes no attempt to unpack the notions of harmony and coherence, as I have attempted to do.

Shapere is certainly correct when he writes (1975, p. 102 n4), "Nothing is gained in illumination, and much is lost because of highly misleading associations, by referring to such considerations (unity and linkages in the Copernican system) as 'aesthetic'.'"


22. According to Goldstein (1988, p. 317),

... up to the time of Copernicus, and for a short time thereafter, modifications in the Ptolemaic system were made piecemeal. When one part was changed no modification of any other part was required precisely because the whole was not yet regarded as subject to the requirement of internal logical consistency. The new mode of criticism, where systems could be rejected because of defects in their parts, may be found in Kepler and Galileo.

24. Duhem's *To Save the Phenomena* (1959) is perhaps the best single source that depicts this struggle.


26. I would claim that both Drake and Duhem make this mistake. See notes 35 and 40, chapter 2, and chapter 3. Hanson also, 1973. Shapere comes close when he says (1975, pp. 102-103) that characteristic disunities in the Ptolemaic system "were not *problems*, facts requiring explanation, which were recognized as existing with regard to Ptolemaic theories; they were not, and certainly did not need to be, so recognized as long as astronomical theories were considered as mere collections of devices, to be applied to different cases in different ways according to need, for the sake of prediction." But Shapere qualifies this by saying that he only wants to emphasize that apparent Ptolemaic disunities "did not constitute a 'crisis' for Ptolemaic astronomy." (p. 103)


28. Ibid., p. 286.

29. Ibid., pp. 288-289.

30. We are not talking about e-mail here, but 'immediately' given the mid-16th century logistics of printing and transmission of important works.

31. Westman, 1975b, p. 395

32. In an annotation to a series of lectures, he drew a model of the inferior planets circling the sun while the sun revolved around a stationary Earth, but then crossed it out.

33. Quoted from Westman, 1975b, p. 402.

34. Ibid., p. 403.

35. Ibid., p. 404.

36. Here it is appropriate to make a major point regarding historiography. When is it appropriate to normatively appraise statements made by historical figures? Answer: when we see that they made mistakes by reliable standards, theirs and ours. Although statements can be found claiming a greater accuracy for the Copernican system, and this has been mistakenly picked up as authoritative by some modern commenta-
tors (see Kitcher, 1993, p. 206), Gingerich has shown that a recomputation of planetary positioning using Ptolemaic and Copernican models does not show superior accuracy for the latter. According to Gingerich, this is to be expected when one realizes that Copernicus was only trying to "match" Ptolemy's achievement observationally. (Gingerich, 1975a) Furthermore, using the best observational tools available for the time, Tycho also concluded that overall observational values given by Copernicus were not better than that of Ptolemy. (Schofield, 1981, pp. 36-37) Relativism has made us overly fearful of making any historical normative judgments "by our own lights."

That we do sometimes make mistakes imposing a modern perspective on ancient texts (eg. the misinterpretation of Ptolemy's statement on individuality and the planets above), does not mean that texts can mean anything we want them to mean.

37. There remains more to my quibble with Westman that we must pass over quickly. Westman claims that regarding the so-called Copernican linkages there was a remarkable silence during the first phase of reception. But it is clear from his papers that what he has in mind is a silence regarding the Copernican ability to fix planetary distances. But there is much more to the Copernican linkages than just an impressive ability to specify planetary distances. As I have argued, there is the Copernican ability to link parameters that necessitate a solution to the core problem situation, i.e., retrograde motion, frequency per planet and links with the sun. The latter, as shown below, is clearly linked with calculation and would be recognized by any competent astronomer immediately. Westman recognizes the Wittenberg astronomers appreciation of the Copernican elimination of the equant, but couches this in a purely philosophical way -- everyone objected to the equant a priori. He fails to see how the elimination of the equant is also linked with calculation based upon Copernican parameters.

Thus, the Copernican linkages created an immediate stir even if they were not fully appreciated in terms of linking the heliostatic retrogression connection with the sun and planetary distances. Viewed this way, a much more likely explanation than childhood trauma for Rheticus's awareness of this connection is that he was "a bright young man devoted to higher learning rather than money and profit." This characterization is due to Johannes Petreius, the printer of De revolutionibus. (Swerdlow, 1992, p. 270.) It is worth noting that Petreius, as part of the Nuremberg circle of "learned men," in writing to Rheticus as early as 1540, refers to the Copernican system as described by Rheticus in his Narratio prima (1540) as a "glorious treasure" even though "he does not follow the common system." (Ibid., p. 274.)

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38. In the above quotations, Galileo refers to "anomalies" and Tycho refers to "inequalities." Both were using language common in discussing astronomical observations that models were required to save. The canonical description of these observational problems is in Dijksterhuis, 1961, p. 57.


40. Ibid., pp. 96-97.

41. I say "can be" because a priori Ptolemy has the machinery to adjust if the well-known planetary linkages with the sun did not exist. I am not claiming that the linkages with the sun were an afterthought for Ptolemy. A posteriori, Ptolemaic geometry rigorously saves the qualitative features of the core observational problem.

42. According to Owen Gingerich, "in an Earth-centered system . . . a planet at the moment of opposition could, a priori, lie at any position on its epicycle. . . . a striking observational fact that would later have a completely natural explanation in the heliocentric system of Copernicus had to be accepted as a given, without explanation, in the geocentric system of Ptolemy." (1991, p. 691-692)

43. Van Helden, 1985, p. 43.

44. Gingerich, 1993, p. 123.

45. Margolis, 1987, p. 277. With this language Margolis does not mean to exclude Kepler's elliptical version of heliostasis or other versions of geoheliocentrism. It is clear in the original context of his argument that he means that the choice was between some version of heliostasis (Kepler introduced his elliptical orbits in 1610) and some variant of geoheliocentrism.

46. Ibid., p. 280.

47. Margolis, 1991, p. 266.

48. It would not be long before commentators would be referring to Galileo's book as discussing "the three systems of the world," and that the controversy was 'twixt Ticho Brahe and Copernicus." (Schofield, p. 250).

49. 1991, p. 266.


54. Problematic for this claim is that from our perspective today it would seem that the Pope would have simply forced Galileo to discuss directly all three systems, allow the Ptolemaic to be destroyed, and then conclude with the Tychonic and Copernican systems as surviving equals.

Margolis does not address this objection. Let’s help him a little. His response might be threefold. (1) This is indeed a puzzle, but there are a greater number of puzzles for the traditional view that Galileo was indulging in questionable dilemma sophistry. (2) This objection is the result of hindsight and we do not know what kind of detailed negotiations took place between Galileo and the Pope. It is possible that this requirement was suggested by the Pope, and Galileo, using his friendship with the Pope, was able to negotiate his way out of it. (3) It is also possible that Galileo was able to suggest to the Pope that this be postponed, because the details of the Tychonic system had not been worked out by anyone. Margolis does mention this lack of Tychonic articulation, but applies it to a different puzzle. Why did the Pope not foresee the embarrassment of the Tychonic system? See below. Concerning (3), however, the Pope and his close advisors were also probably unaware, and Galileo was unlikely to volunteer this information even if he was aware of it, of Kepler’s devastating reasons to prefer heliostasis over geoheliocentrism, published much earlier in his *Astronomia Nova* (Gingerich translation, p. 313). If Galileo’s plan was to indirectly embarrass the Tychonic system, he would not want to show his cards too soon. If the Pope was aware that the Tychonic system was in big trouble, Galileo would not be allowed to publish at all. Better would be to cut a deal that allowed Galileo to champion the planetary linkages with the sun, supporting Copernicus and Tycho over Ptolemy, but agree not to discuss the Tychonic system, leaving the impression that future articulation of this system could match Copernican successes.


58. Ibid., pp. 262-263.

60. Drake 1953 translation, p. 322.

61. To cite but one clear indication that Simplicio is not defending the Ptolemaic system, he says there is "no doubt" that Venus and Mercury circle the sun with the latter being closer to the sun (p. 324), a Copernican and Tycho's arrangement. In the Ptolemaic system, not only do these planets not circle the sun, of course, but Venus is closer to the sun than Mercury.


63. Ibid., p. 326. Emphasis added.

64. Gingerich translation (1983) of Kepler's Astronomia Nova, 1610b, p. 314, emphasis added.

65. I am not suggesting that Galileo has taken this argument directly from Kepler, although the pattern in both arguments is virtually identical. It is still unclear how much attention Galileo paid to Kepler. Galileo may well have arrived at this argument independently and/or it was a common argument used by Copernicans by this time against supporters of the Tycho's system.

66. Galileo's move here is not unlike writing an apparent positive letter of recommendation for a student while making clear to the discerning reader that your appraisal of this student's work is somewhat short of enthusiastic!


68. Rather than using a Ptolemaic eccentric, Tycho used Copernican epicycles to achieve the eccentricity.


75. Feyerabend, 1978a.


Let's assume here that there is some truth to this caricature of Ptolemy. Recall that Ptolemy's model for Saturn must place the important equant point within the sphere of Mars. In isolation, in terms of just observational features, Ptolemy's models for Saturn and Mars work. Placed within a system they don't appear to work well together.

This example needs a time-indexed qualification to avoid appearing a clear case of questionable analogy. My point is not that Ptolemy's models are obviously atemporally ad hoc. (In fact, it is an implication of the notion of hypertextual adjudicatory trails that there is no such thing as atemporal ad hocness. In its place we have adjustments to hypertextual adjudicatory trails with nodes that fail or succeed.) If we had no other theory for comparison, we might find that our theory of car color patterns is doing a marvelous job when it always gets it right by predicting red or blue, black or white, yellow or green. The flexibility of the Ptolemaic models were perceived only after a comparison with Copernican models. Thus, the analogy is valid to this extent. Recall that in Ptolemy's system it is theoretically possible for any new superior planet to be anywhere in its epicycle orbit when its epicycle center is in opposition. Copernicus predicts that all superior planets must be at a precise position P1. Ptolemy makes no such essential prediction; we must wait to see if it is at position P1, P2, P3, etc. Recall Figures 5 and 6. Furthermore, because Ptolemy makes no essential prediction regarding the frequency of retrogressions, a new superior planet could have any number relative to the known planetary values.

It would even be possible for the known planets in Ptolemy's system to very slowly revolve out of their current linkages with the sun, such that a million years from now all planets are then at apogee positions in their epicycles! Recall that a similar move was made by Ptolemaic apologists to explain Ptolemy's original value for precession. See chapter 2, point 8 of Ptolemaic problems. Comparing this possible situation with Goodman's paradox, suppose T1 predicts that future emeralds will be green and T2 predicts that they will be green or maybe blue sometime in the future. Why should we give T1 greater credibility given current observations? Why would it be more fruitful to work on T1. Below I will argue that we give greater credibility to T1 because we do not believe we have any evidence that the fundamental objective features of the world are time dependent. Particular processes may change over time (continents drift, weather patterns change), but overwhelming ampliative evidence suggests that there is one
world whose time-independent, fundamental objective features cause such changes.

82. We could also use the Copernican linking of the eccentricity of the Earth's orbit with the long-standing problem of calculation of precession. According to Gingerich (1993, p. 35), this "impressed everyone."

83. In October of 1994 the Honolulu Advertiser reported that over 60,000 studies have been done on the relationship between cigarette smoking and health hazards. Probably an exaggeration, but there have been a lot. Basically, these studies can be seen as follow-up studies to the seminal work of Hammond and Horn, 1958.

84. According to Stephen Jay Gould, "It's hard enough . . . to find one way of accommodating experience, let alone many. And these supposed ways of modifying the network of beliefs are changes that no reasonable -- sane? -- person would make. There may be a logical point here, but it has little to do with science." Paraphrased by Kitcher, 1993, p. 247.

85. Taking a few days of rest and showing only quantum fuzz on Saturdays and Sundays!

86. In this way, according to Duhem, we can still side with Osian­der and Bellarmine while appreciating the mathematical innovations of Kepler and Galileo. See Duhem's summation in his To Save the Phenomena, pp. 116-117.


88. Note that even this skeptical scenario uses laws of nature and repeatable patterns!


90. Ibid., p. 166.

91. Ibid., p. 157.

92. Ibid.

93. Even after an examination that produces a negative result, it would still be possible that the real common denominator is the use of a particular chewing gum. Our study could be flawed.


96. Ibid., p. 140.

97. Ibid., p. 145.

98. It is significant that Tycho, although supporting a different system than Kepler, also recognized the great importance of this non-equivalent feature of the two systems. Schofield, p. 56.

Rorty has an unsettling vision of philosophy, science, and culture, and it matters to what extent he is right. . . . and his challenge to the standing of what analytic philosophy calls clarity and rationality remains one to be taken seriously. Bernard Williams

Postmodernists would be decidedly unimpressed with my defense of parameter determination offered in the last chapter, relying as it does on the age-old, and much maligned, metaphysical hypothesis that our empirical relationship with the world is best explained by assuming that we are dealing with one objective world with interlinking parts. We return then to where we began. In the introduction it was noted that the postmodernists believe that we have ample historical evidence that this hypothesis has failed, and hence, that we interface with an ambiguous, yielding reality that can be sculpted by a multitude of different perspectives and webs of belief. Furthermore, they believe that much havoc and many ills have been fostered upon an innocent humanity by this objectivist ontology. In the name of expertise in objective truth has been much oppression, and currently a massive onslaught of cultural imperialism. In short, the myth of objectivity endorses a church of reason and a fraudulent power to enforce some interests over others. Remove the philosophical myth and we create truly free human beings.
Thus, postmodern heros parade their works as if they are the enlightened leaders of a new philosophical era, freeing us via hermeneutical deconstruction from a stifling ontology. In *Philosophy and the Mirror of Nature*, Richard Rorty tells a story of how the metaphor of the mind as mirror that accurately represents an objective reality emerged from the notion of a God who has an absolute point of view, who can apprehend the world's intrinsic nature. This notion is deconstructed as anthropocentric. Humans build houses with interlinking parts based on blueprints, so God must have built the big house (the universe) based upon the ultimate blueprint (mathematics). Without this deconstruction scientists are mistakenly seen as priests who have a special divine access to this independent realm, and philosophers (especially epistemologists) are mistakenly seen as bishops who guide the priests via rules along the correct path to this independent non-human reality. Rorty's conclusion is that by giving up ontology, we rid ourselves of the need for elitist epistemology.

It seems to me that this does not work. Like the existentialist who professes not to be an existentialist, and Feyerabend who professed not to have a philosophy but only to be making fun of positivists and post-positivists, postmodern relativists are clearly advocating an ontology themselves. No amount of handwaving, semantic sophistry, or protests that they are being misunderstood because analytic
philosophers do not understand hermeneutics can eliminate the fact that they are generalizing an ontology via an historical induction when they tell their stories of history, and then say that it shows that reality yields to different perspectives, that *its nature is such* that it is capable of being sculpted into any form by human effort, that *its substance is flexible and capable of alternate representations*, that there are alternate realities that go with alternate rationalities, and that it is possible to live in different worlds. My argument is that similar to Plato's response to Protagoras centuries ago that a claim that there is no truth is still a claim about truth, postmodernists do more than just draw our attention to the fact that our interface with a noumenal realm is a fallible human interface -- hardly a great new insight. They are also saying many things that imply something about the nature of that noumenal realm.

Have we then reached one of those divides in philosophy, an antinomy incapable of rational resolution, whereby one just stakes out his or her hard-core ontological postulate, defending it come what may with circular arguments, remaining at best consistent? Is it not undignified of modern philosophers and a freshman philosophical mistake to think that we can argue about metaphysics? My claim is that we can argue about metaphysics to the extent that we can at least compare contrasting arguments and eliminate those that
present the weakest case. Rorty is using the very process he criticizes: he is reasoning inductively (generalizing) to what the world is like via an analysis of history. The issue then is who has presented the best case. My argument is that with dramatic and poetic flair postmodern writers such as Rorty generate grandiose conclusions based on historical inductions, the latter of which my thesis has shown to border on being flippant and slapdash.

According to Rorty, an analysis of history shows that we cannot say that Bellarmine’s arguments against Galileo were "illogical or unscientific," and that our acceptance that Galileo was right and Bellarmine wrong is simply the result of Galileo’s successful rhetoric and our present loyalty to the Galilean tradition. But Rorty draws this conclusion without providing his own analysis of the Copernican episode. Instead he claims that the epistemological project of Western philosophy has been deconstructively exposed as an extinct enterprise by such thinkers as Wittgenstein, Heidegger, Dewey, Quine, Feyerabend, and Kuhn. However, it is doubtful that Wittgenstein and Dewey would endorse Rorty’s characterization of Galileo, Heidegger knew very little science -- he wrote a book on time without any knowledge of Einstein’s theory of relativity -- Quine has backtracked on his early "come-what-may" pronouncements, and we have seen that Feyerabend and Kuhn fail to give an accurate account of the Copernican episode. Rorty may be
telling us a good story about history, but it is fiction. The premises for his imposing conclusions are empty of empirical content.

Consider some more of the dazzling conclusions. According to Rorty, in this new era the best we should hope for is a "criterionless muddling through" that replaces the traditional notion of the desire to know the truth with a constant, ongoing recontextualizing and reweaving of beliefs in response to the incoherence among beliefs produced, not by empirical observations that "convey" knowledge, but by novel "stimuli" that constantly put pressure on our webs of belief. Hence, we should hope that "our culture should become one in which the demand for constraint and confrontation is no longer felt," and replace a search for "forced" interpretation based on a foundational common ground with unforced agreement ("or at least, exciting and fruitful disagreement"), an understanding of the hermeneutical circle and the need for edifying conversation in which one immerses oneself in the whole of a perspective to understand its parts. According to Rorty, we will no longer "argue" as to what are the best inferences based on foundational constraints, rather we will become informed dilettantes who are able to transcend our Whiggishness by getting inside new perspectives, trying them out, and getting a new angle on things.
From the standpoint of this thesis, for Rorty there is no need to worry about distinguishing between adjustments to an adjudicatory trail that fail and creative advances in belief through collateral theoretical analysis, pursuit and acceptance, no need to worry if the pursuit or acceptance of one reasoning trail -- one path of propositions-brought-forward-in-defense-of-other-propositions -- is better constrained than another, for the old notion of convergence of belief is not only not possible, it is not desirable. It is not possible, because the search for a mirror of nature is an illusory quest and there is no foundational or compulsory point to be found that will leave us "speechless" and incapable of keeping the discourse going. It is not desirable, because the search for a locus of forced interpretation is a symptom of what Sartre called "bad faith," our constant propensity to deny human freedom and responsibility. Agreeing with Feyerabend, instead we should seek and encourage an unconstrained proliferation of belief or "we shall never be free of the motives which once led us to posit gods."

Rorty's recommendations may be good travelogue advice -- when visiting another country immerse yourself in the culture and judge not by your own lights -- but not in a global context where so many important decisions must be made. My argument is that given the methodological scaffolding discussed throughout this thesis, and a proper
understanding of the fallibilism it implies, we see that the responsibility Rorty fears the loss of is left intact and in fact brought forward with focus and urgency: We have the responsibility of making intelligent inferences given many alternatives, knowing all the while that there can never be complete justificatory closure to any of our inferences, no ultimate epistemic security for any point of consensus.

If Rorty is correct, then we are left with a world-view of modern science that is just another story, another narrative; albeit one that is the result of a painstakingly recontextualized trail in which we have nothing to be ashamed of, that stems from the ancient Greeks, through Ptolemy, Copernicus, Galileo, Kepler, Newton, Einstein, and Bohr. My argument is that we can do better than this and that there is a lot at stake. Proliferation of purported equally valid beliefs produces not only toleration, but confusion and delay in allegiance to belief as well. As Philip Kitcher has remarked, "To represent as equal ideas of unequal merit is to mislead and confuse." With daily reports of ozone depletion, species extinction of over 100 per day, greenhouse warming, and the dire consequences narrated as objective facts within our tradition, delay in allegiance and acceptance of many well-supported beliefs is dangerous. It is important to decide on the fruitfulness and reliability of many beliefs and inferences. It is important to decide if meditating together during a harmonic
conversion of the planets, or salvation from a superior, benevolent extraterrestrial culture will save us and we need not worry about our problems. It is important to know if John Sununu was right that we have nothing to fear concerning greenhouse warming. It is important to know after 60,000 deaths by handguns in the United States in just two years whether we should accept the Maharishi's proposal to spend ten cents per person per city to send in TM meditators to each city to meditate and think good thoughts. It is also important to know for purposes of education and public policy whether scientific claims, such as "smoking cigarettes is the principal cause of lung cancer," or "sexual orientation is genetically based," have greater rational support than the alternatives, that the alternatives can not be equally well-supported in terms of the extant evidence. Similarly, are there any epistemic signs indicating that we should continue to invest in cold fusion research, investigating perhaps the construction of better neutron detection equipment and continuing to pursue reproducibility?

Rorty has taken a Kantian truism (of course our theories do not re-present a noumenal realm untouched by human filters) and blown it all out of proportion. Providing a good argument that we do not and cannot accurately represent the world as it is itself does not automatically entail, as Rorty assumes, that we do not interface with one world that responds evidentially to our webs of belief, such
that we learn that some belief networks are better than others. This poor inference is another species of the same general post-foundational mistake that we have seen in another guise. That we can never achieve either certainty or a secure resting point at any node within a web of belief does not mean that some webs do not hang together better than others.

According to Rorty, philosophers should realize that the reflective enterprise of standing back from a practice and distilling some sort of profound intrinsic message is no different than a sheltered priest interfering with the lives of real people. How ironic! Rorty should have immersed himself more in the real practice of the Copernican episode and the flesh and blood attempts to get different webs of belief to work.

A post-foundationalist normative epistemological project remains, I argue, for those who see the urgency of walking a difficult path between the false dilemma of a bankrupt foundationalism and an easy relativism. It is important to know if harmonic conversion, extraterrestrial salvation, and numerous other new wave competitors to the world view of modern science are equally valid. We have enough results of "stimulations" from the world; we need to know when the world is "speaking" to us, and this requires a theory of ampliative inference for rational pursuit and acceptance, and at the very least contextual and tentative
criteria that are themselves well-supported by historical evidence as to their reliability.

We need be neither foundationalists nor naive realists to accept the notion that it is most likely that we are dealing with one objective world that speaks to us in a consistent, unified fashion. Furthermore, we need not have some sort of ultimate, self-evident justification for believing in the long-term reliability of the general methodological strategy of confronting the world empirically, and constantly reflecting on which of competing webs of belief, or which adjustments to an existing web of belief best match that empirical confrontation. It is the best we can do, but as to why it is the best, it is a simple matter: we have learned that we will be more successful in unifying more experience and diverse fields of study if we proceed this way.

Recall Kuhn's struggle (chapter 3, pp. 139-140) with the notion that we are dealing with "one world." He tells us that in one sense he still believes in the traditional notion that the objective world does not literally change beneath our feet when paradigms change, that only our interpretations change. But he then turns around -- almost as if he envisions Feyerabend and other chic postmodernists laughing at him at a Berkeley coffee shop or bar for adopting such an old-fashioned metaphysics -- and tells us that the notion of "living in different worlds" must have some
substantial meaning. In a strikingly revealing passage, in one of the last responses Kuhn made to critics, I think such waffling leads Kuhn to say,

"... those who have followed me thus far will want to know how a value-based enterprise of the sort I have described can develop as a science does, repeatedly producing powerful new techniques for prediction and control. To that question, unfortunately, I have no answer at all, but that is only another way of saying that I make no claim to have solved the problem of induction."

No answer at all! And, according to Kuhn, "idiosyncracy must be invoked to explain why Kepler and Galileo were early converts to the Copernicus's system..."

To be fair to Kuhn here, the rest of this passage reads, "(but) the gaps filled by their efforts to perfect it were specified by shared values alone." In other words, as Feyerabend has claimed, initial commitment was irrational and nonepistemic and requires psycho-social explanation. For Kuhn, they switched gestalts (worlds) and saw within this new perspective things others did not see. They then filled in the picture to convince others. They saw unity and fruitfulness; others did not. So, these values, according to Kuhn, cannot function algorithmically in choice.

The burden of this thesis has been to show that such torturous responses are not necessary. Yes, we must acknowledge that neither scientific change, commitment, pursuit, nor acceptance is the result of an algorithmic
process and a tidy response to empirical data. Yes, we must acknowledge that history reveals a rich texture of flesh and blood idiosyncracies, soap-opera-like contingencies, and messy meandering trails. However, the burden of this thesis has been to show that these acknowledgments underscore all the more that we ought to invest in an enterprise that stands back from the fray and attempts to see what we do when we are at our best. That we do indeed confront the world with webs of belief, but these webs are best seen as networks that can be adjusted piecemeal and not as hegemonic paradigms or gestalts with all aspects of a point of view locked neatly in place. It is ironic that Kuhn and others, who have been taken in by what Popper called the myth of the framework, did not see that such holism is inconsistent with the very messiness of history they so often point out. History would be a much neater appearing story, if our webs of belief were such locked-in frameworks. The complexity of history is much better understood by seeing scientists as debating, adjusting, pursuing, and accepting complex hypertextual adjudicatory trails of reasoning; of making the best ampliative inferences that they can given the context.

On several occasions in this thesis I have pointed out the parallel between my view of epistemology and current efforts in artificial intelligence. Standard computers operating via the assumptions given to us by logical positivism and logical empiricism respond only to direct
manipulation and the directed control of an algorithm. They
do not test new trails of thought on their own, create their
own algorithms, and they surely do not learn. New
approaches involving neural networks and artificial life
function on the notion of on-going testing -- gradual
strengthening or weakening -- of reasoning networks. In
neural networks, a reasoning trail is not set in stone and
then made to respond to input. Instead a "soft" trail is
tried in response to input (experience), and initially
various nodes along this soft trail may be given very weak
values. In conjunction with fuzzy set theory these values
are not constructed to be crisply true or false, on or off,
but are given the potential to have any degree of truth, any
value within a full range of values between totally true and
totally false. Gradually, via repeated input and testing,
the relative strength of various nodes will change, being
given higher or lower values. Change in such values will
have ramifications throughout the network. In artificial
life, a diverse profusion of reasoning trails are tried, and
then some characteristics of some trails self-replicate
(survive) in response to their environments in a natural
selection process.

My contention is that there is a direct parallel
between the failure of the epistemological machinery of
logical positivism and empiricism to explain scientific
change, and the failure of artificial intelligence efforts
to fulfill the high-flown promises made in the 1960s. The fruitfulness of new approaches to artificial intelligence should alert us that the failure of one approach need not force us to conclude that there is no more thinking about thinking to do. Kuhn's perplexity is due to his being grounded in many of the assumptions of logical positivism and his discovery of many instances in history inconsistent with the implications of these assumptions. Although philosophers such as Feyerabend and Rorty have contributed poignantly to our understanding of the failure of one approach to epistemology, their grandiose negative conclusions regarding all versions of critical rationalism are best seen as a blip of despair in response to this failure of only one approach.

What postmodernists fear most is that unless the pretensions of scientism are undermined, a bounty of reliable beliefs in a multitude of successful cultures will be obliterated, like so many shining sea shells on a beach of diversity washed away by a tidal wave of the world view of modern science. They believe that we must protect diversity by eliminating all desire for convergence and unity. Wrong. Can we not believe in galaxies and the efficacy of herbal medicine at the same time? A much healthier possibility is that the full articulation of a humble philosophy of science that retains the faith in reason of the critical rationalist will establish a
fallible, but self-corrective path that promotes unity without destroying diversity. It is toward that end that hopefully this thesis has made some small contribution.
Notes for Conclusion:

1. 1990, pp. 27 & 35.

2. An exception is Feyerabend. Although I have shown that his analysis of the Copernican episode does not support his conclusions on epistemology or ontology (scientists as "sculptors of reality"; reality as "yielding"), his use of the results of quantum physics constitutes a more serious challenge. See note 11 below. Responding fully to this challenge, however, is beyond the scope of this thesis.


5. 1979, p. 315.

6. 1991, p. 27.

7. Kitcher, 1983, p. 173. Kitcher’s comment concerned the court battle between so-called Creationist science and the theory of natural selection. Although it is true that in a democracy we assume, because of our faith in people, that the unequal merit of ideas will be revealed if they are allowed to compete side by side, there is a difference between allowing ideas to be presented in a biology class as equal scientifically and allowing those same ideas to be debated in the public arena. Within the context of a scientific curriculum we have the responsibility to present the best ideas, those backed by the best evidence. Furthermore, with so much at stake, even in this age of multiculturalism scientists should not be so timid as to not make firm recommendations on reliable beliefs.

8. When Sununu was George Bush’s chief of staff, he drew attention to the fact that the standard model of greenhouse warming was in trouble, that it needed, in our terminology, some auxiliary patching. Thus, it is important to decide whether to pursue this patch or not. See Broeker, 1992, and Monastersky, 1992 for a discussion of this problem.

9. The Maharishi took out full page ads in major city newspapers in the Fall of 1992 claiming that this would substantially reduce the crime rate in these cities.

10. Rorty and his followers will no doubt point out that his acceptance and expansion of Kuhn’s distinction between normal and revolutionary science -- couched in Rorty’s philosophy as a distinction between normal and abnormal discourse -- allows Rorty to claim that all the above issues can be rationally
decided within a limited rule-based epistemology of normal science. I have argued that scientific change is much more piecemeal than that reflected in Kuhn's distinction. Furthermore, once it becomes meaningless, its collapse along with the other claims Rorty makes leaves us with an "anything goes" stance within so-called normal science as well.

11. Even in quantum physics there is a consistency (limits) to how the world speaks to us. The interfaces that we create with the microcosm reveal either particles or waves, but not elephants. Even Feyerabend admits that not all interfaces with reality will work. For instance,

I do not assert that any combined causal-semantic action will lead to a well-articulated and livable world. The material humans (and, for that matter also dogs and monkeys) face must be approached in the right way. It offers resistance; some constructions (some incipient cultures -- cargo cults, for example) find no point of attack in it and simply collapse. Feyerabend, 1989, p. 405.

Well then, it seems there is not much difference (other than rhetoric) between Feyerabend's position and the traditional epistemological project modified by fallibilism. If not all theories work, we want to know why. We want to be able to spot for future reference, the features of methodology that enabled us to rationally pursue and accept one theory rather than another. We want to know why it is not a good idea to invest a lot of resources in cargo cult beliefs, and/or perhaps cold fusion. As epistemologists this is what we do: we stand back from the fray an attempt to grasp how to approach a "resisting" reality "in the right way."

It is worth noting that Feyerabend follows the above passage with the claim that there is no difference between the interface offered by CERN and that which produces a world alive and full of gods!


13. Ibid.

14. According to Feyerabend, "we are stuck in a scientific environment." Our environment was "once full of gods; it then became a drab material world; and it can be changed again, if its inhabitants have the determination, the intelligence, and the heart to take the necessary steps." 1989, p. 406.

15. See Wu, 1995. This question is not meant to imply that the benefits of herbal medicine will be or must be reduced to current scientific theories or even practice. The results
produced by another culture could very well effect a rational change in current theories and methodology. For instance, Wu notes that Western medicine tends to see drug efficacy in terms of discrete magic bullets, and even U.S. regulations require that a drug be identified as a single chemical entity. Whereas in Chinese philosophy, a much more holistic approach is used based on the underlying belief that chemicals not only work in conjunction, but that the whole is not simply the sum of its parts. According the Wu, the influence of this approach is leading to changes in cancer treatment where combination therapies are beginning to be used.
The various holistic notions found in the literature, such as web of belief, paradigm, theory and background knowledge are unpacked in this thesis and modelled as hypertextual adjudicatory reasoning trails. A theory $T_1$ is not just seen as a hypothesis, some auxiliaries, and a body of evidence. Rather, any theory $T_1$ is seen as a set of hypotheses, a set of auxiliaries, and a set of evidence. Each item within a set is itself seen as a node, the acceptance of which is hypertextually linked with another reasoning trail of hypotheses, auxiliaries, and set of evidence. For instance, in the above diagram, the auxiliary $T_1-A_s$ is accepted by scientists at a particular time because they believe the trail (2) unproblematic. Similarly, they accept $T_1-E_s$ because they believe the reasoning trail of (3) unproblematic. In the latter case, $E_s$ might be an observation made with a particular instrument. The reliability of the instrument is based upon accepting (3). In my thesis three points are made repeatedly against the relativist.

1. As a matter of pure logic our justificatory trails can go on forever; all hypertextual links are nonterminating and there is no foundational resting point for any proposition. However, this does not mean that scientists at a particular time do not have good ampliative reasons for accepting a particular node and its hyperlinked justification. For instance, given that $H_1$ in (2) is crucial to (2) which in turn supports $A_s$ in (T1), and given that $H_1$ could be examined or questioned at any time, does not mean that there are good reasons for doing so at any particular time. In Chapter 1 we will see that Feyerabend claims that scientists could have begun to question, via an examination of optical theory, the foundation for centuries of accepted naked-eye triangulations necessary for fixing planetary positions. That
they did not do so, Feyerabend claims, shows that they were muddleheaded and thoughtless. My point is that they were amplitative "thoughtful," because there were no good reason for doubting the reliability of these observations. (Note also the independence from theoretical allegiance of the acceptance of these triangulations. See point 2 below.) Similarly, we have no good reason for doing another study on cigarette smoking and controlling for what people in a study did on their third birthdays, examining on that date what they ate, what environment they were in, and so on -- even though without studying such a variable, and innumerable others, there remains mere conceivability that we are wrong about the link between cigarette smoking and lung cancer.

2. The reasoning trails (2) and (3) can be and often are independent of T1. That is, their acceptance is often based on their own links and such acceptance may be involved in supporting a node on a rival to T1 as well. In other words, the myth of the framework has caused many to misplace the notion of and/or overemphasize the hegemonic force of theory ladeness. E5 in T1 may be theory laden in terms of relying on (3), but (3) may not only be independent of T1 but also be used by (hypertextually linked to) a rival to T1. The acceptance of a telescopic observation may be linked with a reasoning trail, but that observation could be part of an evidence set of a rival theory. See Chalmers point against Feyerabend, Chapter 1.

3. When a particular node and its hypertextual justification are questioned, say E5 and (3), an examination of the justification can be carried out independent of T1. For instance, Feyerabend argues correctly that during Galileo’s time a rival to (3) existed (the celestial-terrestrial distinction and related matters), such that if it were true, it would have destroyed the supporting structure of (3) for E5 (say, the observation of mountains on the moon). However, I argue in Chapter 1 that a resolution of this matter and an examination of the reliability of the celestial-terrestrial distinction could be cared out independent of whether one accepted T1 (Copernicanism) or not.

The captivating oversimplification of such notions as web of belief (Quine), paradigm (Kuhn), discourse recontextualizations (Rorty), are partly responsible for many postmodernists missing the many instances of piecemeal rational change in the history of science. I also argue that the messiness and contingency revealed by historical analyses are best captured
by seeing scientists not only immersed in idiosyncratic contexts, but also attempting to make intelligent ampliative linkages modelled above. In this way an overall rational scaffolding is preserved, but an inevitable residue of disquiet and insecurity remain (allowing for robust disagreements), because of the nonterminating nature of all the links and because it is seldom clear what all the links should be at any given time. At no time in the history of science do we find a whole scientific community experiencing the nonargumentative comfort implied by Kuhn’s notions of paradigm and normal science. Even during the monumentally successful Ptolemaic era we find the participants debating a whole host of issues -- the reality of epicycles, the appropriateness of the equant point, the unhappy marriage of Ptolemaic geometry and Aristotelian physics, the possibility of replacing Aristotelian physics with some sort of impetus theory, and so on.
Figure 2: Copernican Complexity. To account for the observation of the sun, the Earth revolves around a central point $C_E$, which in turn revolves around the point $CC_E$, which in turn revolves around the sun.
Figure 4: Ptolemaic System -- parallel positioning of the radius vectors of the superior planets with the sun's radius vector. Note that for the inferior planets (Mercury and Venus) their epicycle centers are fixed on the sun's radius vector. Also note that in this diagram Mars is at opposition and at perigee.
Figure 5: Ptolemaic a priori flexibility for a superior planet. Tycho's "great concern" and Frisius's "fact-in-itself" enigma is illustrated here in this adaptation from Owen Gingerich, 1991. In the Ptolemaic system, there is no a priori reason why a planet at opposition could not be positioned in locations other (A, B, C,) than that shown in D, the perigee position.
Figure 6: The "marvelous effect" of retrograde motion is, according to Galileo, clarified with remarkable simplicity in a heliostatic system. In this figure we see how the geometry must produce retrograde motion at opposition, increased brightening of a planet at its midpoint of retrogression, and a pattern of retrogressions as well. The extent of Jupiter's retrogression must be smaller than that of Mars'.
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**Supplementary References**

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