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Simplicity in science

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SIMPLICITY IN SCIENCE

by
Daniel Benjamin Schulz

An Abstract

Of a thesis submitted in partial fulfillment of the
requirements for the Doctor of Philosophy
degree in Philosophy
in the Graduate College of
The University of Iowa

May 2012

Thesis Supervisor: Associate Professor Evan Fales

ABSTRACT

This dissertation investigates the possibility of justifying simplicity principles in science. The labor of these projects is organized into three chapters.

The first chapter introduces some of the key authors and issues in the history of simplicity in science. This chapter also gives a detailed discussion of the work of the 19th century physicists Le Verrier and Newcomb who played a crucial role in setting the stage for Einstein's theory of relativity. These examples are used to illustrate points in the following chapters. However, they play a specific role in the first chapter to show serious problems with a view defended by an important contemporary author, Richard Swinburne, that one version of the principle of parsimony contributes to the probability that scientific theories will be true.

The second chapter elucidates the problems involved in specifying and measuring the simplicity of scientific hypotheses and theories. Several twentieth century authors like Harold Jeffreys, Nelson Goodman, and Mario Bunge made significant contributions to the clarification and organization of various kinds of simplicity criteria. When simplicity criteria are employed in a scientific methodology, we find that simplicity judgments of one kind are always traded-off with simplicity judgments of another kind. We also find that the scientific project involves a delicate balancing of several aims. This analysis renders a valuable result: that some dogmas, in particular, the dogma that principles of parsimony are the final court of appeal in scientific theory selection must be jettisoned. I also find that it is misguided to ask the question of whether or not simplicity of some clearly specified

kind is related to the truth. In point of fact, the legitimate questions about the justification of specific simplicity judgments in science are much more complex and nuanced than this. This becomes clear when it is seen exactly how different simplicity criteria are related to one another and to the various desiderata of science.

The third chapter investigates which *argument forms* may be available to justify simplicity principles in science. The results from the second chapter are employed in two ways. First, methodological principles stand in a tight-knit set of interrelations, so our analysis of justificatory argument forms must incorporate the complexity of these relations. Second, simplicity is extremely heterogeneous and since no conceptual reduction of all of the various simplicity criteria is possible, justificatory arguments must deal with clusters of interrelated principles. This result may have certain advantages and other disadvantages for inductive, transcendental, or inference to the best explanation approaches to the justification of simplicity. My analysis shows what will and what will not work for these possible approaches to the question of justification and shows what some of the systematic and metaphilosophical commitments would have to be were philosophers to pursue this project.

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Thesis Supervisor

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Date

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A thesis submitted in partial fulfillment of the
requirements for the Doctor of Philosophy
degree in Philosophy
in the Graduate College of
The University of Iowa

May 2012

Thesis Supervisor: Associate Professor Evan Fales

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CERTIFICATE OF APPROVAL

PH.D. THESIS

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Never tell me the odds.
— Han Solo, *The Empire Strikes Back*

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CHAPTER 1 INTRODUCTION

Nearly every insightful author who has written on simplicity has made note of the little irony that simplicity is notoriously complex. While there is widespread agreement that there are things to be said about the methodological roles played by simplicity judgments in science, there is very little agreement on what is to be said, and very little agreement about how we ought to approach the relevant issues. In the cycles of human intellectual history, simplicity has made more than one appearance. As the planetary gears of epistemology go round, the philosophy of science takes paths that sometimes track the motions of classical epistemology and, sometimes wandering paths. Questions about simplicity are approaching perigee in both fields once again, and I believe that this has something to do with the popularity of investigations into computational modeling, database warehousing, and curve-fitting. Simplicity's number has come up and I turn the spears of analysis upon it to see what clarity might contribute to this tremendous and highly technical discourse.

Despite one hundred years' trees sacrificed to contrary ink, I show that clarity about simplicity is possible and that philosophical inquiries into the justification of simplicity judgments are both motivated and far from being resolved. I offer a critique of the simplicity in science discourse which I hope will provide a useful framework for future inquiries into simplicity judgments generally. I conclude that the traditional study of human knowledge is discontinuous with the study of scientific theory construction and appraisal in important ways. If I am right about this, then some issues in the philosophy of science may not need to wait for the skeptic to be given a final answer. However, the justification of

simplicity in science may not be one of those issues. My results show that questions about the justification of simplicity judgments in science are at least not crippled by formal problems and my analysis lays the groundwork for new research programs. I wish to convince scientists and philosophers to stop using the phrase, “Occam’s Razor” because it is both vague and subject to misguided historical interpretations. More fundamentally, I will argue that, although it is possible (and often tempting) to formulate principles of parsimony with *ceteris paribus* clauses, they are impossible to apply in theory construction or evaluation. My critique aims to convince philosophers to stop using these these mistaken terms and to lay the groundwork for future investigations into the actual roles played by simplicity in science.

Some have hoped that specific judgments of simplicity might guide us in ontology. The notion of treating simplicity as an attribute or property has been associated with beauty, perfection, and truth. Where simplicity is instantiated so are these others expected to be instantiated as well. But I wonder why we should not expect beauty and perfection to be found in that which hangs on the very edge of chaotic complexity. It does seem as if a great artist, and perhaps a great engineer, would achieve the finest of works by balancing order at the very threshold of chaos - unless, of course, the engineer could explain, in a few simple words, why things worked out that way. The abstract impressionist or the jazz musician seeks to get a *feel* for the patterns of sensation and then to make just one contribution to the darn-near-chaotic ripples of the media - not too little not to be noticed - not too much to destroy.

I am stuck. Is it simplicity or complexity that is a guide to knowledge about how

to make the best contribution to art or to science? I need only to think about one (either simplicity or chaos) to notice that the one that I am thinking of seems to be governed by the other. On the one hand, it seems that the world is complex, and my contribution (as an artist) to the world is simple if it is well placed. Is it simple because it is one contribution or because it is *well placed*? On the other hand, I wonder if a complex series of sensations is simple in some respect - in that they all occur within one medium or in that they are governed by few laws. In science, as in art, we seek to balance the complex with the simple. In art this involves a science and in science I suspect an art. Simplicity judgments are a mixed bag. We must distinguish them so that we can tell which are relevant to ontology and which are not. Future projects can show which of the simplicity judgments that are relevant to ontology are actually justified, if any are.

Sometimes “simplicity” is a word used to indicate *easiness* with respect to certain physical or mental efforts. Perhaps the association of simplicity with low mental effort explains why the word “simple” has been used to connote stupidity or ignorance. As it turns out, the “simple” which indicates ignorance is totally incompatible with another association often made with the word: that simplicity is a divine attribute, and therefore naturally associated with omnipotence, omniscience and perfection. It asserts no mere tautology to say that simplicity either is or is not a divine attribute. We must first disambiguate senses of the word “simplicity”. Perhaps the “or” in this sentence is inclusive. Even if we restricted our discussion of simplicity to the mind, we may still run the risk of equivocation. A thought may be simple in some ways but complex in others.

Many have thought that our non-deductive inferences depend upon judgments of

simplicity. Many have also thought that if there is a solution to the problem of induction, then that solution depends upon the success or failure of the project to justify some criterion of simplicity. Naturally, a subset of these folks consists of those suspicious that no such justification is available and that the problem of induction has no solution. This discussion is focused on the roles of simplicity judgments in the construction and evaluation of scientific theories. Some might meet this with despair - that the philosophy of simplicity in science is, at best only a partial guide to the problems of simplicity in traditional epistemology and metaphysics, and at worst it is no guide. I meet this conclusion with optimism - progress on some issues in the philosophy of science would not wait for the skeptic to be given a final answer. However, in the final analysis, it is possible that the questions of traditional epistemology will be revealed to be more fundamental than questions about the justification of simplicity in science. If that is the case, the so be it. We would have learned something about the order of priority of our intellectual divisions of labor. Alternatively, the philosophy of science and traditional epistemology may inform one another.

This project takes the approach that this may be the case, by investigating some of the history of science and some of the arguments from the philosophy of science to see if we can learn anything about the structure of the simplicity dialectic generally. I leave open the question of whether or not we could have discovered any of the things that my investigation reveals by searching the depths of our imaginations alone and say only that *this* philosopher lacks the imagination to have generated these results without looking into both the philosophy of science and the history of science.

The result that we can settle some issues in the philosophy of science without an-

swering the skeptic is a game-changer for some controversies in the philosophy of science. The realism/antirealism controversy in the philosophy of science may be characterized as an epistemological debate about whether or not *the truth* is a desideratum of science. Philosophers like Richard Swinburne attempt to argue that simplicity is related to the truth and if simplicity is not a desideratum of science, then science is non-rational. Swinburne would agree with Paul Churchland on an additional point; that the scientific antirealist view which does not include non-empirical criteria of theory choice (like simplicity) collapses into skepticism. I do not engage the scientific realism versus antirealism controversy directly. However, some of these arguments are useful to present and evaluate because they involve the claim that simplicity judgments are essential for the construction and evaluation of our successful scientific theories. Swinburne and Churchland could be right that simplicity is essential for science. However, Swinburne has not yet successfully argued that simplicity is related to the truth and neither Churchland nor Swinburne are able to show that the mind has simplicity concepts. By analyzing the forms of their arguments, I show that the final justification for simplicity judgments (if one is available at all) does *depend* upon arguments in traditional epistemology, but that the scientific realism/anti-realism controversy does not collapse into the problem of skepticism. We need only to give a careful analysis of scientific methodologies involving simplicity judgments to reach the conclusion that an elucidation of science that takes empirical adequacy to be the sole criterion of theory acceptance (the scientific anti-realist view) is very implausible. This conclusion shows that we need not level the playing field - so that everyone must answer the skeptic - in order to decide some crucial things about how to elucidate science. I show how an analysis

of simplicity judgments and their interrelations to the other desiderata of scientific theory construction and evaluation contribute to these conclusions.

My analysis contributes to projects that began in the early and middle parts of the twentieth century to elucidate simplicity judgments. These efforts were spearheaded by notable philosophers, scientists and mathematicians like Harold Jeffreys, Nelson Goodman and Mario Bunge. In the contemporary literature, Jeffreys is cited far more frequently than Goodman or Bunge, which I think is a serious oversight. Additionally, attempts to elucidate simplicity seem to have been generally neglected after about 1970. One of the last attempts at this was given by Elliott Sober in 1975. I believe that it is a shame that Goodman and Bunge are not cited more often because their arguments might temper the speed at which contemporary papers on curve-fitting fly into publication.

The fact that very little has been done to bring analytic clarity to the roles of simplicity in science is at once tantalizing and vexing. Why would simplicity judgments that have long been recognized to play crucial roles in scientific theory construction and evaluation generally escape the efforts of analytic philosophers? I have no answer for this. Scientists are certainly aware that simplicity judgments play roles in theory construction and evaluation. Newton himself put forth four principles of simplicity. The astronomer Simon Newcomb, who made important contributions to the scientific dialog that set the stage for Einstein's papers on Special and General Relativity, says of his adjustments to the Hall hypothesis (1895) that,

I can only remark that its simplicity and its general accord with all optical phenomena are such that it seems to me that it should be accepted, in the absence of evidence against it ([50] p.63).

Newcomb also makes a general claim regarding simplicity in scientific methodology.

Should we aim simply at getting the best agreement with observations by corrections more or less empirical to the theory? It seems to me very clear that this question should be answered in the negative. No conclusions could be drawn from future comparisons of such tables with observations, except after reducing the tabular results to some consistent theory,... Our tables must be founded on some perfectly consistent theory, as simple as possible, the elements of which shall be so chosen as best to represent the observations ([50] p.64).

We should expect that the explicit invocation of principles of simplicity by scientists would draw philosophers with analytic ambitions to simplicity. We should expect that the fundamental issues of simplicity in epistemology would draw philosophers. Draw them simplicity does, but like vultures to something believed to be dead. The sheer volume of articles published in the last one hundred years on simplicity in science is intimidating to say the least. However, little of this literature aims to elucidate simplicity in science rather than either to avoid questions about its justification or to point out that it has already been settled that simplicity is indispensable for science. What is worse is that many contemporary authors believe that the problem of the role of simplicity in theory choice has been successfully boiled down to the question about curve-fitting algorithms. This literature is extremely technical and much of it is mathematical in nature. This perhaps scares some people away while others it sucks in, tantalized by its mystifying rigor. It is not true that the philosophical problems of simplicity have been dispatched to aesthetics or that they have been analyzed in purely pragmatic terms. It is not true that the role of simplicity in theory selection can be wholly analyzed using curve-fitting algorithms. It is frustrating that contemporary journal articles are littered with vague and confused appeals to simplicity,

but we need not scuttle the literature for errors or oversights to find puzzling claims involving simplicity. Some of the most influential and careful thinkers have said things that deserve some clarification.

Lavoisier said,

I have deduced all the explanations from a simple principle, that pure or vital air is composed of a principle particular to it, which forms its base, and which I have named the oxygen principle, combined with the matter of fire and heat. Once this principle was admitted, the main difficulties of chemistry appeared to dissipate and vanish, and all the phenomena were explained with an astonishing simplicity. ([60])

It is a weird kind of thing to say. Lavoisier started with a simple principle and then was *astonished* when simplicity was exemplified by his resulting theory. I do not know why a scientist who has just struck upon the *oxygen in - oxygen out* theory of chemistry would be *astonished* when a simple principle generates a theory that exemplifies simplicity - *simplicity in - simplicity out*. Perhaps Lavoisier noticed that a principle of one kind of simplicity could be used to construct a theory that exemplified another kind of simplicity. That would be astonishing. This is precisely the sort of thing that requires clarification. I aim to provide a framework of analysis that will aid in clarifying claims of this sort.

Another highly regarded author, J.J.C. Smart, makes use of the a principle involving some form of simplicity in *Sensations and Brain Processes* (1959). Smart says,

The suggestion that I wish if possible to avoid is a different one [different from the suggestion that saying that one is in pain replaces another behavior like crying], namely that “I am in pain” is a genuine report, and what it reports is an irreducibly psychical something. And similarly the suggestion I wish to resist is also that to say “I have a yellowish orange after-image” is to report something irreducibly psychical.

Why do I wish to resist this suggestion? Mainly because of Occam’s Razor. ([53] pg.141)

The term “Occam’s Razor” has a long history in science and it involves some appeal to simplicity. It is not always clear what authors have in mind when they use the term. Smart, however, perhaps can be clearly interpreted. Smart wishes to resist explanations about the mind that float free of any causal laws. Smart may have in mind a principle of simplicity about ad hoc hypotheses. However, when it comes to science, I am not sure how many hypotheses that float free from causal laws would be permitted. My intuition is that *strict prohibition* rather than *simplicity* governs this kind of reasoning in science.

Smart goes on to say that the hypothesis of substance dualism, “offends against the principles of parsimony and simplicity.” ([53], pg.155) I wish to know what kind of offense this is. If it is merely a misdemeanor, then I might just go in for it. After all, revolutions arise when individuals risk breaking a few norms. However, if I could be shown that violating the principle of simplicity meant that I was not doing science, then I would not violate the principle while doing science.

For reasons like these, I believe that both philosophy and science will benefit from a bit of conceptual clarification about simplicity and the roles it plays in scientific theory construction and evaluation. I divide the labor of the analysis of simplicity in science between three chapters.

Chapter one introduces the motivations for the project of justifying simplicity in science and investigates several historically important arguments from both science and philosophy to help distinguish key terms.

Chapter two applies the terms and distinctions from the first chapter to analyze the roles played by simplicity in science. The first part of the chapter categorizes simplicity

judgments and introduces several of the problems involved in specifying and justifying simplicity criteria. The second part of the chapter shows how these criteria, when they can be clearly defined, are related to one another and to the other desiderata of science.

Chapter three investigates the justification of simplicity in science. The first part of this chapter motivates the problem of justification by showing that it is very difficult to avoid, and that the assertions made by many authors fail to demotivate an investigation into the justification of simplicity in science. The remaining sections analyze argument forms which might be employed to justify simplicity. My analysis shows what are the problems with these argument forms and helps to reveal why, what intuition might first suggest are problems, are not obviously problems. I conclude that inductive arguments, arguments involving an inference to the best explanation, and arguments involving an indispensability principle are not obviously out of contention. However, I will only analyze the forms of these arguments as they apply to the problem of simplicity in science. I will not actually give the justificatory arguments, nor will I settle other issues having to do with the systematic reasons that philosophers might have for favoring or rejecting any of these argument forms. As it turns out, these three argument forms are in a sort-of three way stand off. Although I argue that none of them suffer from any obvious formal problems if applied to justify simplicity in science, it is not clear which is more epistemologically fundamental, or whether any depend upon some answer to the problem of skepticism.

CHAPTER 2 SIMPLICITY ON THE ROAD TO PERDITION

The philosophy of science literature on simplicity has grown into a hydra-headed monster. The only way to deal with it is to hook any neck which presents itself most naturally and, from there, slash our way into the breast of the beast. We must start somewhere, and the ride will never be less violent than it is in the opening. I suggest that the best way to begin is to consider some very basic examples of the ways that simplicity is involved in making inferences and then to consider a few more detailed historical case studies.

This chapter is divided into four sections. The first section introduces some of the roles that simplicity judgments might play in non-deductive scientific reasoning. Sir Arthur Conan Doyle's *Sherlock Holmes* novels are often referred to on this topic. I discuss Doyle's famous *Sign of the Four* to help illustrate some of the ways in which simplicity judgments are involved in non-deductive inferences. The second section introduces the fundamental motivations, terms and distinctions for a philosophy of simplicity. The philosophical problems of the principle of parsimony are introduced in this section. I also show why it is difficult to bracket questions of metaphysics in analyzing or applying principles of parsimony. The third section makes several suggestions about the roles that simplicity judgments might play in theory construction and evaluation. These roles are not necessarily involved with principles of parsimony. In the fourth section I show how deeply embedded concepts of simplicity are in science. Many philosophers who work on issues involving simplicity and parsimony have pointed to the Ancient Greeks when they think about the archetypes for the philosophical dialectic involving these issues today. I find it useful to

organize the simplicity dialectic by doing just a bit of the genealogy of simplicity.

There is a secondary objective for this chapter. The chapter is dialectical in the sense that it aims for conceptual clarification. In addition to introducing examples, terms and motivations, I also launch a few arguments. I am critical of some of the popular approaches to a philosophy of simplicity. Many philosophers of science who investigate issues involving simplicity appear to have come around to the view that progress in the dialectic on simplicity in science has ground to a halt. I have wondered if this might be due, in part, to how easily philosophical inquiry into simplicity is motivated. It is very easy to see how judgments of simplicity could solve some of the biggest problems in philosophy like: the problem of universals, the problem of induction, and questions about the nature of the divine. Yet to solve these problems simplicity criteria would have to be clearly defined and the judgments of simplicity would have to be justified. Many philosophers are overtaken by frustration on the issue of clarity and overtaken by skepticism on the issue of justification. However the obsession with solving the big problems in philosophy might just involve letting the smaller game slip away.

It is easy to gesture to a vast philosophical literature where opposing camps, bristling with Ockham's bayonettes, charge one another with having committed the sins of complexity. Immaterialists and materialists might end up doing this on the issue of what an ordinary sensible thing is. Berkeley, for example, appears to hold that it would be an unnecessary complexity in God's universe if material substance caused sensations when God can cause sensations straight away. A materialist like the Bertrand Russell of *The Problems of Philosophy* might accuse the Idealist of introducing so many *ad hoc* hypotheses to systematize an

unwieldy metaphysics that it would be simpler to admit material substances to ontology.¹

Trope theorists and universal theorists may stagnate in this bog as well. The trope theorist might wish to keep an ontology free of extravagances like propositions or universals by positing only instances of properties. But the universal theorist charges that the trope theorist is left either with the mystery of explaining what binds individual properties together in a *thing* or with some universal binding relation. Perhaps the intuition is that *universals alone* is a simpler view than *tropes + binding relations* or *tropes + universals*.

In the sciences this issue may be resurrected each time some party disputes the foundations of an evolutionary theory committed to excluding final cause explanations, and evolutionary theorists univocally snort, “we need no additional causes.” A similar issue may have served as the basis of the dispute between Einstein and Heisenberg about whether or not *randomness* needs to be introduced to the ontology of causation. Perhaps these disputes are about whether or not we ought to prefer explanations that are simpler in that they posit fewer kinds of causal mechanisms.

These are some of the heavyweight issues in metaphysics, epistemology, science and theology no doubt! If arguments are not available to reduce disputes about simplicity to disputes about other things then the philosophy of simplicity is *well* motivated. Yet these exciting lines of inquiry may end up being a sort of distraction. It is easy to get swept

¹Some of Russell’s claims appear to be obvious misinterpretations of Berkeley. So it is not *easy* to see what Russell means when he says that, “the way in which simplicity comes in from supposing that there really are physical objects is easily seen.” Russell goes on to say that “every principle of simplicity urges us to adopt the natural view, that there really are objects other than ourselves and our sense-data which have an existence not dependent upon our perceiving them.” I base my interpretation of Russell on this passage and the fact that it is located in the Second Chapter of *Problems* which argues against the idealism and immaterialism.

away by the desire to answer the skeptic or to solve the problem of universals and it is especially easy to slip into these projects as we cavort in the neighboring dialectical space. But the focus of this project must be to take a bite out of the philosophical problems of simplicity; the problems involving the roles played by simplicity judgments in scientific methodology. I have heard it said that *when you are young it is fun to think that you'll generate the scorched earth argument that will leave the army of the skeptic decimated on the battlefield. As you grow older, you settle for the skirmishes.*²

2.1 Non-Deductive Inference

“Elementary my dear Watson.” Even people who have never read Sir Arthur Conan Doyle’s novels know of the famous tag line uttered by the cocaine, heroine, nicotine, and alcohol addicted genius detective who regularly shocked and amazed his side-kick Watson with his forensic inferences. The line has always seemed to me like the sort of thing that a bully would say. Doyle may have been employing a literary device to build the extreme brilliance of the Holmes character by having him *talk down* to Watson, a well educated physician. But for a non-fictional character, it would be rude to talk to one’s friend in this way. It has always struck me as if Holmes was happy to celebrate the fact that he had some perhaps God-given access to a principle of inference which poor Watson (perhaps due to his less fortunate breeding) lacked. “There’s no point in explaining it. It’s easy for me but not for you”, I understand Holmes as meaning. Were it Doyle’s plan to build such a character, it would not be surprising given Holmes’ other traits since heroine and cocaine

²Professor Fales said something very much like this in my first graduate seminar in Epistemology. I cannot resist referring to it because I cannot think of a better way to put the point.

addicts are not famous for their politeness or humility *e.g.* Sid Vicious, Miles Davis, and G.G. Allin.

Doyle often describes Holmes as performing one of his famous *deductions*. Given his fabled intellectual brilliance, Holmes may well have known what Doyle did not: that his arguments were not deductively valid. To illustrate this, consider an example from the first chapter of *The Sign of the Four* (the entire selection is included in the appendix).

After briefly examining a watch Holmes makes the following statements which are confirmed by Watson who happens to know the history of the watch. The watch has recently been cleaned and the watch had recently been given to Watson by his brother. Big Brother Watson had inherited the watch from their father many years ago.

Holmes says that he made these judgments for the following reasons: the “H.W.” on the back suggests of the Watson family name, the initials are as old as the manufacture date of the watch, it is customary for jewelry to be passed from father to oldest son, and as Holmes recalls, Watson’s father had been dead for many years.

Holmes then amazes Watson by saying of his partner’s late brother that,

He was a man of untidy habits – very untidy and careless. He was left with good prospects, but he threw away his chances, lived for some time in poverty with occasional short intervals of prosperity, and finally, taking to drink, he died. That is all I can gather.

In response to Watson’s declaration that Holmes was correct in every particular, Holmes says,

Ah, that is good luck. I could only say what was the balance of probability. I did not at all expect to be so accurate.

Nevertheless, Holmes insists that he did not guess in drawing his conclusions. He explains in a bit more detail the bases upon which his judgments were made. Watson's brother was judged to be careless on the basis that the scratches and dings indicate to Holmes that the expensive watch was kept in the pocket with keys and coins. Holmes adds that Watson's brother must have then been careless in general. Holmes noticed several of the sort of scratches which pawnbrokers customarily put on items to match with tickets and inferred that Watson's brother was in financial trouble when he pawned the watch and in better standing on those occasions when he redeemed it. Lastly, Holmes cites the many scratches around the key hole as evidence of the unsteady hand of a drunkard who wound the watch each night.

Before attempting to formalize Holmes' inferences it is worth while to draw attention to what Holmes *does not say*. Many other details which could be relevant to the inferences which Holmes draws are not specified. Holmes does not say anything about the specific character of the watch scratches which are supposed to have been caused by keys and coins. Keys and coins are of different shapes and are made of different materials. A piece of iron can be cleaned with a brass brush, for example, and the soft brass will lightly scratch the iron leaving a golden residue on the iron which looks different than a piece of iron which has been brushed with aluminum. It is possible that some scratches might be classified more precisely by saying something about the specific gold alloy that the watch is made of and how various metals transfer material to that alloy. Coins of different sizes might be expected to leave different sized impressions on the gold watch. An expert might be able to distinguish between the marks left by a six pence piece and a shilling. Another

bit of specificity which Holmes does not give involves the possible idiosyncrasies of the local pawn-brokers. Some, perhaps, are known to use more digits to match the tickets than others do. In a way, the fact that Holmes omits these details contributes to the probability that his inferences will be true. If he had been so bold as to hazard assertions about the kind of money Big Brother Holmes spent or the specific pawn-brokers who handled the watch, Holmes would have opened his hypotheses to possibility that some of his assertions might be falsified. As it is, Holmes claims that he did not expect his assertions to be so *accurate*. This could be due to the fact that Holmes' could have been right about many of the general facts about Watson's brother but wrong about some of the specifics.

Holmes has asserted three things about Watson's brother: that he was, in general, careless, that he had experienced extreme ups and downs in financial security, and that he was a drunk.³ Holmes also explained what was the evidence for each of these assertions: initials, scratches of various sorts, and customs common to his culture.

Deductively valid arguments are those arguments where the truth of the premises guarantee the truth of the conclusion. Two classical rules of inference are *modus ponens* and *modus tollens*:

1. If P then Q
 2. P
-

³Doyle has Holmes offer a list of three inferences whose conclusions are: 1. Bad financial times for BBW. 2. Good financial times for BBW. 3. BBW was a drunk. I find this list a bit odd because financial ups and downs are a cycle which, if analyzed, amount to either ups or downs. Drunkenness and gambling are the normal causes of this kind of financial history. Finally, though not all alcoholics are careless, carelessness could be the cause of all of this. I have tried to carve up the inferences as clearly as possible. At a certain level of analysis, it must be admitted that Doyle probably does not provide the best case studies.

Therefore: Q

and

1. If P then Q
2. not- Q

Therefore: not- P .

But were Holmes to be giving arguments of this sort, his arguments would be, at best, unsound because they would contain implausible or false premises. In order to get his first two assertions by deductive inferences Holmes would need principles of the following sort:

- If a watch is scratched then it has been handled carelessly by the owner.
- If a person is careless about one of his or her possessions then he or she is careless with all possessions.

Principles such as these *could* be employed in a sequence of deductively valid arguments like the following:

1. If a watch is scratched then it has been handled carelessly by the owner.
2. The watch is scratched.

Therefore: The watch has been carelessly handled by the owner.

3. If a person is careless about one of his or her possessions then he or she is careless with all possessions.
4. The pocket watch was one of the possessions belonging to Watson's brother.
5. Watson's brother was careless with his watch.

Therefore: Watson's brother was careless with all of his possessions.

The problems with this chain of reasoning are obvious. Firstly, these arguments are unsound. Premises 1 and 3 are at best implausible and at worst false. A scratched watch may not have been handled carelessly *by the owner*. After all, if Holmes is right that the watch has a history of passing through the hands of pawnbrokers, then it might be the case that the pawnbrokers handled the watch carelessly while it was in their possession. If Holmes is right that drunkards mishandle the winding of watches in ways distinct from the ways that cocaine and heroine addicts mishandle things, then there is still the possibility that the live-in drunkard prostitute girl friend of Watson's brother was responsible both for the key scratches and the frequent visits to the pawnbroker. Big Brother Watson may have thought that the watch was in good hands and thus never got wise to its cycles. Doyle's readers might speculate about many other hypotheses which Holmes does not discuss in connection with watch-scratches and wonder how it was that Holmes ruled out all but the one he gave. Holmes never does consider the possibility that Big Brother Watson may have been an epileptic and he never does consider that people other than Big Brother Watson may have caused the scratches. Explanations for watch-scratches such as this might undermine assertions about Watson's brother, his carelessness and his drunkenness.

Perhaps my initial versions of some of the premises are not the correct way to interpret Doyle. This could well lead to a worse situation for Doyle's genius character, where the arguments he offers are invalid by deductive standards. This would be the case if Holmes was reasoning from effects to causes. I do not believe that the above principle stated in premise 1 is really the correct interpretation of what Holmes asserted. I believe that Doyle

was attempting to model the inferences of Sherlock Holmes on the inferences characteristic of scientific reasoning. One important component of scientific reasoning involves collecting data and then inferring what are the likely causes of the data. As a forensic investigator Holmes does exactly this sort of thing. The phrase “has been” appearing in some of these premises inverts what I believe to be an implicit causal relation which Holmes’ reasoning is supposed to track. However, if these arguments are rewritten as expressing some principle of causation and an inference from effect to cause then they are no longer merely unsound, but they would be invalid by deductive standards of argument evaluation. If the correct analysis of premise 1 is that it asserts a causal relation then it should read instead:

- The careless handling of watch W causes watch W to be scratched.

But, if Holmes observes that the watch is scratched and infers that mishandling is the cause, then he would have committed the fallacy of affirming the consequent. As stated above, this is due to the fact that watch scratches *can* come about in many ways.

For these reasons, many readers of Sherlock Holmes novels have suggested that Holmes was performing non-deductive inferences. The suggestions are that Holmes employed either the *inductive inference* or the *inference to the best explanation*.

2.2 Induction

The Eighteenth Century Scottish philosopher David Hume famously analyzed and criticized inductive reasoning. Hume pointed out that many inferences essential to the scientific process do appear to take the form taken by the argument:

1. If a watch is scratched then it has been handled carelessly by the owner.

2. The watch is observed to be scratched.

Therefore: The watch has been carelessly handled by the owner.

However, this argument should be analyzed differently so that it does not look like an invalid deductive argument. Perhaps a better way to write this argument would be:

1. Past Observation Set 1: this watch has been carelessly handled by its owner *and* it ended up scratched.
2. Past Observation Set 2: A different owner carelessly handled a watch *and* it ended up scratched.
3. Past Observation Set 3: Yet another owner carelessly handled a watch *and* it ended up scratched.
4. On the basis of uniform past experiences, I have come to expect that when a watch is scratched, it has been caused by the careless handling of the owner.
5. I observe that the current watch before me is scratched.

Therefore: It is probable that the watch has been carelessly handled by the owner (just like in every other case that I have observed)

This argument form would appear to fit Holmes' reasoning. It may also be indicated by Holmes' references to what is *customary* and to what is *on the balance of probability* that he is thinking of some principles which he has derived from more-or-less uniform cases of past experience.

There are a couple of different ways to think about what was illuminated by Hume's analysis of the inductive argument form. One of Hume's general projects may well have

been to partake in an Enlightenment Period movement to reign in an inflated view of human cognitive powers which Europeans inherited from ancient thinkers.⁴ Some people, straddling ancient values and an optimism imparted by the successes of the scientific revolution may have gotten to thinking that it was only a matter of time before the methodological principles that have been so successful in science, would also solve the ancient puzzles of metaphysics and perhaps even the ‘Big One’ (the question of God’s existence) would be settled as well. One unifying thread in Hume’s work may be his arguments that scientific principles cannot offer answers of this sort. The theme of Hume’s *Dialogues On Natural Religion* is to reveal the impieties of arguing for God’s existence on scientific principles and this may be evidence that Hume’s arguments had specific Enlightenment characters as their targets.

Hume suggests that we should turn the principles of science upon human thought. But when Hume does this, he finds that the traditional problems of metaphysics are intractable. Hume argues that nothing in the content of sense observations themselves will bestow upon us knowledge of the necessary connection between cause and effect. Nor do sensations themselves reveal to us what *kinds* of things are causes. A pattern of repeated observations will not, at some point, cough-up for the careful scientist the theory which explains them. Hume can see no other explanation for how it is that humans carry out inductive inferences except that our volatile psychologies or habitual conditioning somehow dispose us to form the expectation that the future will resemble the past in certain respects.

⁴This reading of Hume is suggested by Tom Beauchamp in his introduction to an edition of *An Enquiry Concerning Human Understanding*.

It can be seen from the example above, that although watch scratches may have been caused by the careless handling of watch owners in the past, there is no reason not to expect watch scratches to arise due to the careless handling of watches by non-owners, or by seizures or by micro-meteorites or.... This is the pattern of introducing competing hypotheses loved by all skeptics. The pattern involves introducing alternative hypotheses about the causes of observed effects, because induction cannot give us an argument that any particular causes are any more likely to have preceded some observed effects than any others. This is because the regularities of past experience alone give us no reason to expect that things will not be different in the future. After all, past experience also informs us that things change.

Hume's result is discussed in the philosophy of science literature as the problem of the *underdetermination of theory by data*. No accumulation of *data* (or *cases* or *observations*), would be sufficient to determine the theory that explains them. It is easy to think about this situation with a geometric example. However, the phrase "underdetermination of theory by data" can be a little misleading for the graphic example since it would be unusual for a *theory* to consist of a single equation. For this example, it will be sufficient to discuss the problem of the *underdetermination concerning the functional relation between physical variables by data*. Since many theories contain such hypotheses it should be sufficient to show how the problem arises for an essential component of theories. Consider a set of data points represented on the Cartesian coordinate plane in figure 2.1.

Different functions describing the plot in figure 2.1 could be *fit* to the points. Consider the different graphs in figure 2.2, all of which are fit to the data points from 2.1.



Figure 2.1: Data Points

All of these graphs would be generated by different functions. Some of the functions appear to *fit* the data points more precisely (or we might say ‘better’) than others do. Hypothesis h_3 fits the data very well and it also has a few bumps and dips which do not correspond to any of the points of data presented. This helps to illustrate the fact that we could fit infinitely many functions to a finite set of data points just by making the functions *bumpy* in between points. Hypothesis h_4 is a discontinuous or piecewise function. Infinitely many functions of this sort could be generated as well to fit equally well any finite set of data points.

Infinitely many hypotheses could be offered to *fit* the data. It is worth noting, however, that it would not be sufficient to say that a hypothesis is a *scientific* one just because it fits some data. Scientific hypotheses involve several components. A scientific hypothesis is never merely a mathematical equation. For one thing, the signs in the functional expression must be given a physical interpretation. Often the physical interpretation involves positing underlying mechanisms which would, if the hypothesis is true, generate the data. A functional hypotheses which fits some data may only show that the data is correlated. But, correlation is not causation. Additionally, a *scientific* hypothesis will have to do much more than merely fit and the data and offer a putative explanation of it. Many of the interre-

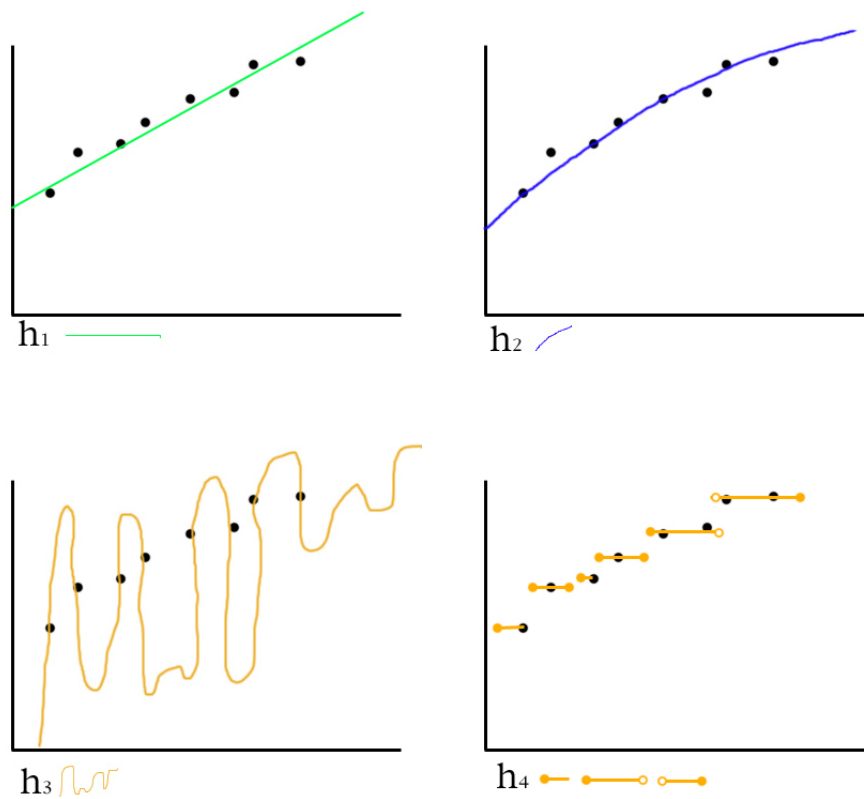


Figure 2.2: Curve Fitting

lated criteria that scientific hypotheses satisfy will be discussed in the next chapter. Here, I wish to draw attention to the relation between the problem of induction and the fact that we need more than just the ability to generate hypotheses that fit the data to justify the methods employed to generate hypotheses.

Suppose that a hypothesis involving a mathematical description is given such that data of this sort are caused in the way described by hypothesis h_j . This hypothesis would be competing in conceptual space with infinitely many other hypotheses which would cause the data and also, with infinitely many hypotheses which would, if true, explain the disunity of the data. Perhaps each datum is caused in a unique way and deserves a unique causal explanation. Although it is not entirely clear to me exactly what Aristotle had in mind in his famous proof of the unmoved mover in Book VIII of the *Physics*[4], he seems to suggest that there may be infinitely many *movers* (which I take to mean causes) for states of affairs which are realized in an infinite time interval. However, a crucial part of Aristotle's argument is that some additional causal explanation must be given for what would unify these unique movers in certain respects. My guess is that all movers would agree on basic things like that effects always follow causes. Aristotle's appeal to some additional principle about unified causal explanations goes beyond merely explaining the sequences of events. I do not know precisely what Aristotle's additional principle was (or principles were). Perhaps he had general methodological principles that endorsed unified explanations, or perhaps he had reasons to posit just one mover because this best reconciled with his general systematic thought - perhaps both. Whatever was the case, Aristotle had to reach beyond induction alone to give a causal hypothesis.

The problem of the underdetermination of a hypothesis by data is twofold. 1) It is difficult to see how one hypothesis can be selected when infinitely many curves will fit the data; infinitely many smooth curves will fit the data and infinitely many discontinuous curves will fit the data. 2) Even if we settle upon a mathematical or logical method by which a single curve which fits the data may be selected over its competitors, this is still not sufficient to justify the judgment that the data was all caused in the same way – that there is a unified causal explanation.

This is one way of illustrating the problem of induction in science. The problem is one of determining which of infinitely many hypotheses to select for a given data set. In addition to *fit*, what criteria must our scientific explanations satisfy? Aristotle appears to argue in the *Physics*[4] that no hypotheses involving random causes will be physical explanations, since he has argued that there *must* be some unifying causal explanation for the laws of nature, and that chance (randomness) is not a cause.

There are two points which may serve to launch inquiry on the basis of these illustrations. It is a well entrenched feature of the scientific process that scientists collect data and then fit curves to the collection. Firstly, it is often the assumption that curve-fitting gives a function which does not merely describe correlations, but rather that the function which is selected to fit that data will contribute to the formation of a causal hypothesis. Secondly, scientists often fit curves to data imprecisely to some degree - this represented better by h_1 and h_2 than by h_3 or h_4 in the examples above. But some scientific principles must constrain the sloppiness of curve fitting. Hypotheses can get so vague that they are never false. Fortune tellers have mastered the art of sloppy curve fitting. If Doyle success-

fully modeled the inferences of his character on the usual methods of science then he may have left some clues for the reader that he did so. Holmes' career as a forensic investigator would demand of him that he resist the siren's song of the skeptic, and would force him to reason from effects to causes and to choose just one explanatory hypothesis from infinitely many possible hypotheses equally compatible with the data.

On this view of Holmes as an exemplary scientist, the questions we ought to ask of Holmes are the questions that we ought to ask of all scientists. 1. What, if anything, would justify hypotheses which track causation if induction cannot give to us an idea of the necessary connection between causes and effects, but only an idea of their being correlated in the past? 2. What, if anything, would justify selecting just one hypothesis from among infinitely many that would also account for the data? 3. What, if anything, would justify a certain degree of inaccuracy in hypothesis formation and what would constrain hypotheses from becoming incorrigible due to vagueness?

The first two questions are the questions raised by Hume. The third question has taken center stage in contemporary debates regarding curve-fitting algorithms and it is likely that the popularity of computational modeling in science has contributed to the recent spotlighting of this topic. Judgments of simplicity may play roles in answering each of these questions. However, simplicity has not yet been elucidated sufficiently to discuss the relations between various types of simplicity judgments and their roles in constructing and selecting hypotheses. Elucidating the various species of simplicity judgments and their relations to the desiderata of science is the goal of the next chapter. The first two questions have been taken by many to launch inquiry in classical epistemology.

Consider the two components of Hume's criticism of induction. The first component involves pointing out the problems of the underdetermination of theory by data. The second component involves pointing out that justifying induction on empirical principles involves a circularity. Whether we reason from causes to effects or from effects to causes, the problem of underdetermination by data presents a problem for non-deductive reasoning.

There is a version of inductive skepticism with some sobering consequences: the problem of skepticism with respect to the external world. Just change the above examples around a bit in order to generate this problem. Say that instead of data points we have a collection of experiences and say that instead of algebraic hypotheses we have hypotheses about the causes of our sense experiences. Now the trick is giving an argument that will justify inferring that material objects cause our sense experiences instead of evil spirits, or *Matrix*-style insect-shaped robots with sophisticated hardware and software systems⁵, or the mind itself in a dream state. All of these are hypotheses which would (at least it seems to some philosophers) explain the sequence of our perceptions and this is the problem of the underdetermination of theory by data. Additionally, it will do no good to say that in every case in the past our sensations have been caused by material objects external to our minds. We cannot get outside of experience, as it were, to check what are the usual causes of experience. It seems that we have only past experience to go on to justify the method of inference. This is the problem of circularity.

Now *if* it turned out that we had some idea of the necessary connection between one cause-effect pair, we could reason from cause to effect in that kind of case. However, we

⁵The reference is to the 1999 film *The Matrix*

still would be unable to reason from effect to cause. This shows that both the problems of underdetermination by data and the problem of circularity apply to reasoning from cause to effect. But reasoning from effect to cause is crippled by the underdetermination of theory by data alone.

The strategy may not be acceptable to all philosophers, but it may be possible to keep some dialectical separation between the questions for the philosophy of science and the general problem of skepticism. Some people may just give up on knowing anything about what causes of our sensations (the *dinge an sich*) and content themselves to discover whatever science might tell us about the general structure of the mind or the matrix or the *whatever it is* that causes sensations. This acquiescence to the Cartesian skeptic will not side-step all of the problems of induction. The questions about what justifies the selection of one hypothesis over its competitors remain for science. Perhaps this turns into the question of which concepts or cognitive structures are in the mind. Some will also argue that the Cartesian skeptic lurks at the terminus of even this dialectical path. That is, the question of what justifies the methods or the principles by which we proceed to reveal what are the fundamental cognitive structures which are somehow filled out by experience poses itself even to thinkers like Berkeley or Kant.

Of course if there were some principled way to judge which, among competing hypotheses, is most likely to be true, then some (if not all) of the questions about what justifies inductive inferences might be answered. The notion that simpler hypotheses are somehow more likely to be true than more complex competitors has suggested itself to many thinkers. Some people find it intuitively plausible that the so-called *Real World Hypothesis* is simpler

than the evil demon hypothesis. This might be correct for all I know, but I will not weigh in on this field here. I can imagine how arguments could be given to show that one scientific hypothesis is simpler than another. Formal apparatus may be available to gauge the relative complexity of logical or mathematical hypotheses. The problem for the philosophy of science would be to show that the simplicity of a scientific hypothesis is somehow related to the truth in such a way that it is made more likely than its competitors. This problem is the same as the general problem of external world skepticism in that we cannot argue on inductive principles that the truth is usually tracked by hypotheses which are simple in this or that way, because such an argument would involve checking what the relationship between simplicity and the truth is, but this is exactly what demands an argument.

Perhaps the fun of the Sherlock Holmes's novels has to do with the fact that Holmes is supposed to have a special divine gift, and like a modern Job or Moses, he wrestles with his human imperfections. Perhaps Sherlock Holmes has immediate intuitive access to the degree of simplicity of hypotheses. Perhaps he has an even more grand gift and has knowledge of the relation between simplicity and the truth. Instead of telling us what this relation between simplicity and the truth is, Holmes just laments the industrial-age version of the problem of evil: the problem of boredom. Perhaps it is at least clear that there are two lines of inquiry which might contribute to solving the problems of induction: 1. the question of how to classify and gauge the simplicity of hypotheses, and 2. the question of what the relationship between simplicity and the truth might be.

2.3 Inference to the Best Explanation

A different interpretation of Doyle is that Sherlock Holmes' inferences are modeled on the the style of argument which takes the form of an inference to the best explanation.

Very basically the inference to the best explanation might take the form:

1. Observation set O .
2. If hypothesis h were true, h would best explain O

Therefore: probably h .

This formulation is a bit too basic though, because this characterization may not clearly distinguish this form of argument from induction. The second premise suggests that h is compared to competitors so it should read "If hypothesis h were true, then it would be better than any of its competitors at explaining O ." If the observation set turns out to be just a bunch of uniform cases, then this argument may be indistinguishable from induction. So, to make this argument form unique it should be open to contain observation sets which are not necessarily uniform.

1. Observation set O .
2. Observation set O' .
3. Observation set O'' .
4. If h were true then it would be better than all of the (infinitely many) hypotheses which give a unified explanation for the bunch of observations which would otherwise be a totally disconnected set of coincidences. Therefore: h is probably true.

Considered this way the inference to the best explanation certainly appears to be unique. If we come home and open the front door to see that the sliding glass door is broken, there are muddy shoe prints going both directions between the broken glass and the entertainment center and the high fidelity stereo is missing, then most of us make an immediate inference as to what must be the explanation for all of this – that the stereo has been stolen.⁶ Most people immediately discount the neighbor with the tin foil hat who interrupts his ravings about the Mayan calendar only to say that he has no idea what caused the broken glass or the stereo to go missing, but the footprints are evidence in favor of his alien invasion hypothesis. After calling the police to report the stolen electronics it may be tempting to explain to the neighbor that his offering of the disunified hypothesis is, once again, evidence that he is not rational. But this would be a waste of time. There is nothing crazier than reasoning with the insane.

The example of a probable stereo theft is an example of a forensic inference. This example can easily be extended to the type of inferences made by Sherlock Holmes. Holmes may have a set of uniform experiences (say, observation set O) about how people customarily pass watches on to eldest male offspring. He may have another uniform set of experiences (observation set O') involving the behavior of drunks and another (observations set O'') involving the behavior of pawnbrokers and so on. Holmes may infer that the best explanation for this, otherwise heterogeneous, set of data is that Watson had a reckless, drunken older brother.

⁶This basic example has been presented to students in Richard Fumerton's classes. I picked it up as a teaching assistant.

Of course it does no good to let the word “best” bear the weight of all the really nasty philosophical problems of non-deductive inference. Of course we might object that the word “best” is philosophically loaded in this deployment. The introduction to philosophy student can easily challenge the view, “Does ‘best *h*’ mean that I like it?” “Does ‘best *h*’ mean that it gets me access to fancier parties?” “Does ‘best *h*’ mean that *h* is more likely to be true?”

Despite the fact that philosophers may not yet have generated replies which will satisfy even the most rudimentary objections to the argument form involving the inference to the best explanation, the form may well capture what scientists actually do. For that matter, the argument form may well capture what most of us (non-scientists) do on a daily basis. Again, simplicity may play a key role in the analysis of the inference to the best explanation. If, for some reason, simpler hypotheses are more likely to explain what unifies a set of data, as opposed to leaving that data as a wild set of coincidences, then perhaps we can justify judgments about what the ‘best’ explanation for a given data set would be. The philosophical questions regarding this type of inference are launched in the same way as are the questions regarding induction: 1. How do we gauge simplicity? 2. What would be the relation between simplicity and the truth? 3. Why would it be a mark of rationality or of our physical explanations that they give unity to experience?

2.4 Motivations, Terminology and Distinctions

When I first tell people that do I research on the role of simplicity in science, two reactions are most common. One reaction involves something like a shoulder-shrug, “I guess

I never thought about *that* being important to science.” The other reaction is disgust, “I think there really is no philosophical problem on this matter.” Both the shoulder-shrug and the retreat to naïve pragmatism indicate suspicions about the motivations for a philosophy of simplicity. It will not satisfy people harboring such reservations to point to a long scientific tradition where in scientists have both employed and endorsed principles of parsimony. Scientists may have been mistaken, unclear, talking past one another or philosophers may have misinterpreted them thinking that simplicity played a role which no scientist took it to play.

Judgments of simplicity might solve the problems of non-deductive inference. In this way a philosophical investigation of simplicity is motivated at least minimally. But three typical types of assertions are nevertheless given in resisting the philosophy of simplicity: 1) that simplicity is a hopelessly vague concept, 2) that simplicity reflects nothing more than pragmatic or aesthetic values, and 3) that judgments of simplicity are unlikely to be found playing any important role in the scientific method. But such claims demand arguments! For my money, some of these claims might just be true. It would be nice to know if arguments might be marshalled for or against these views.

Philosophers recognize three distinct projects for a philosophy of simplicity: 1) clarification 2) justification 3) application ⁷. There are philosophical reasons for considering what role, if any, some criterion of simplicity might play in governing non-deductive inferential judgments. There are also reasons for considering the roles played by judgments of simplicity in hypothesis construction and testing. Simplicity is subject to vagueness and

⁷Alan Baker parses the discourse this way in the Stanford Encyclopedia article on *Simplicity*[5]

therefore might stand to benefit from a bit of analytic clarity. So, the project of organizing, categorizing and clarifying species of simplicity well motivated. I intend to show that it is very difficult either to ignore or to analyze away the roles of simplicity in scientific methodology and this leaves the epistemological questions as the really interesting ones. These issues will be discussed in the fourth chapter.

If simplicity can be clarified and categorized, then perhaps an epistemological project is possible. The epistemological project is to investigate which species of simplicity, if any, might be justified and if so, in which ways. Pragmatic justification for simplicity judgments in specific roles may be easy to come by. The real problem would be to show that there is some relation between a particular species of simplicity and the truth.

2.4.1 The Venerated Tradition of Simplicity

I can at least guess why Christian Europeans cared about simplicity. Thinking about their preference for simplicity reveals something about why so many Early Moderns were systematic thinkers. The rough idea was that simplicity was a divine attribute. God made the world resembling God's nature and God made human minds also resembling God's nature such that humans could appreciate creation. I imagine that if simplicity is a feature of human cognition and if it is also a feature of the world then there might be a causal relation between features of the mind and features of the world. Such a relation would be exactly what one would expect if simplicity judgments of some sort were justified.⁸

There may be a great deal to say just about the role of simplicity in the thought of

⁸A proper philosophical investigation into the role of simplicity in the methodologies of Early Modern thinkers is a project that I intend for the future.

some philosophers, but also a great deal might be said about the ways some philosophers inherited methods involving simplicity and then changed them somehow. In other words, there may be an illuminating story to be told about the morphing principles of parsimony in the period of the Copernican revolution. Here, I will point out just a few examples to show that major figures in this period probably saw the relationship between the human mind, the world, and God's simplicity as some fundamental commitment of theory construction.

Descartes thought that simplicity was a divine attribute and that the only way this idea is caused to be in his mind is by God. In the *Third Meditation* Descartes considers the possibility that his idea of God was brought about by partial causes. "The supposition here being," Descartes says "that all the perfections are to be found somewhere in the universe but not joined together in a single being, God." But Descartes abandons this hypothesis because the divine attributes are a package deal so-to-speak. "On the contrary," says Descartes "the unity, the simplicity, or the inseparability of all the attributes of God is one of the most important perfections which I understand him to have. ([20]pg. 34)

Leibniz opens the *Discourse on Metaphysics* saying that God has all of the perfections and nature has some of them. He says this in a way which suggests that nature resembles God.

The most widely accepted and meaningful notion we have of God is expressed well enough in these words, that God is an absolutely perfect being; yet the consequences of these words are not sufficiently considered. And, to penetrate more deeply into this matter, it is appropriate to remark that there are several entirely different perfections in nature, that God possesses all of them together, and that each of them belongs to him in the highest degree. ([44] I.)

Leibniz also appears to hold that the perfections of nature and of God can be known by humans. Lloyd Strickland places Leibniz's views about simplicity in a larger historical

context also.

Now we will recall that richness of phenomena was only one of the elements in Leibniz's characterization of the most perfect world, the other being simplicity, or simplicity of hypotheses. By identifying simplicity as a criterion of worldly perfection Leibniz appropriated another idea already in circulation, this time incurring a debt to his contemporary Nicolas Malebranche; for the doctrine is present in many of Malebranche's works, from the sprawling *Search after Truth* (1674) onwards, though it was in that work that Leibniz apparently first came upon it.

Interestingly, Malebranche touted the idea of simplicity for precisely the same reason that some earlier thinkers had promoted the idea of plenitude: it was the best way for a completely perfect being to express itself. In Malebranche's view, God 'acts only for His glory', and is therefore 'determined to will that work which could be produced and conserved in those ways which, combined with that work, would honor Him more than any other work produced in any other way'. Therefore, 'He formed the plan which would better convey the character of His attributes, which would express more exactly the qualities He possesses and glories in possessing.' ([58] pg.67)

Bishop George Berkeley motivates his empiricism on the basis that God would not have created a universe and yet leave humans ill-equipped to understand it. One of Berkeley's most famous passages is the third entry from the introduction to *A Treatise Concerning the Principles of Human Knowledge*.

But, perhaps, we may be too partial to ourselves in placing the fault originally in our faculties, and not rather in the wrong use we make of them. It is a hard thing to suppose that right deductions from true principles should ever end in consequences which cannot be maintained or made consistent. We should believe that God has dealt more bountifully with the sons of men than to give them a strong desire for that knowledge which he had placed quite out of their reach. This were not agreeable to the wonted indulgent methods of Providence, which, whatever appetites it may have implanted in the creatures, doth usually furnish them with such means as, if rightly made use of, will not fail to satisfy them. Upon the whole, I am inclined to think that the far greater part, if not all, of those difficulties which have hitherto amused philosophers, and blocked up the way to knowledge, are entirely owing to ourselves—that we have first raised a dust and then complain we cannot see.([6] pg. 6)

Additionally, Berkeley argues in various places that material substance would be a

needless extravagance of God's creation since an all powerful being could certainly cause sensations in us directly.

If therefore it were possible for bodies to exist without the mind, yet to hold they do so, must needs be a very precarious opinion; since it is to suppose, without any reason at all, that God has created innumerable beings that are entirely useless, and serve to no manner of purpose. ([6] pg. 40)

Many of the major contributors to Western thought in the period of the scientific revolution appear to have shared a certain family of views which involved the resemblance of the simplicity of the universe to God's simplicity *and* that God's universe is knowable for humans. If such a view is correct, then judgments of simplicity may at least be a guide to true judgments of ontology. This view *is* the situation that we ought to question⁹. Firstly, God's simplicity is a matter for serious theological debate. God might just have a view of beauty more in common with Jackson Pollock or Charlie Parker. God may have slung the world together in total defiance of structure so that the loveliness of creation is that it hangs together on the very edge of chaos. Certain naturalistic arguments for the existence of God would do better on this theology anyway. This is the sort of thing which Hume points out in *The Dialogues on Natural Religion*. What some people see as an impious result of the Design argument is that it makes God similar to humans. God may have splattered the primordial stuff onto the canvas of very minimal laws just to impress deities in other more simple universes. Some philosophers and theologians appear to embrace this notion. William Paley appears to be committed to accepting that God is similar to

⁹Elliot Sober makes similar points about the history of the role of parsimony in science in *Reconstructing the Past; Parsimony, Evolution, and Inference*. Sober also says that in this century the idea that parsimony must involve ontological commitments has fallen into disrepute, and that parsimony can be treated as a purely methodological principle. He argues against this modern view. See Sober pg.37

humans in some ways. He defends the Design Argument by arguing that the universe is a teleological system just like a watch is. However, by the rules of analogical reasoning, the inference would only be a strong one if God resembles human designers also ¹⁰.

Pending the results from theologians about the best way to know what are the divine attributes, it is not yet clear to me that positing a chaotic universe is an impiety. Even if God is simple and so is God's creation, then we still face philosophical puzzles. Unless Spinoza is right and the universe and God are to be *identified* then the universe would at best *resemble* God's simplicity in some respects and not in others. The philosophical project remains to establish which, if any, judgments of simplicity correspond to God's plan. Even on the chaotic universe hypothesis it is difficult to avoid considering simplicity. Saying that the constraints for God's post-modern styled universe were *very minimal* suggests a kind of simplicity too, and we might even wonder if darn-near-to-chaos is in some way simpler than a number of highly structured moral and physical laws.

There is also a naturalistic hypothesis which would be compatible with some simplicity judgments being justified. It has been said that the natural history of human kind is red in tooth and claw but it is also spattered with clandestine affairs. Humans have always been busy trying to outwit one another to secure power, wealth and preferable breeding rights. Succeeding in the hominid breeding pool takes some rather clever maneuvering. On this matter, I could not disagree more with Patricia Churchland: mere chance just won't cut-it at my usual watering holes.¹¹ Humans have become very successful at reading subtle

¹⁰See William L. Rowe's *Philosophy of Religion: An Introduction* for a discussion of the various versions of the Design Argument which have been defended.

¹¹Patricia Churchland says that, "from an evolutionary point of view, the principal function of

behavioral cues, anticipating responses and moving to head off competition for breeding. If gripped for even a moment by the temptation to say that hominid courtship is not this ferocious, just stand on a street corner on any Friday night in a college town. The roving packs of males are in the market for romance but their behavior is indistinguishable from what it would be if they were going to a fight! Since clever maneuvering may play a key role in the long run for who gets the breeding rights and who does not, there is reason to think of the cognitive capacity for reasoning *causally* in the social arena as playing an important evolutionary role. On this hypothesis, next to Cleopatra, Cerano was a sissy. Perhaps, humans might never be expected to have true beliefs about quantum physics, gravity, or even about the shape of the Earth, but they might be expected to get very good at judging cause and effect relations of some sort. Patricia Churchland appears to assume that evolution exhibits alethic blindness, but this assumption is not justified in some obvious way.

The question of which simplicity judgments might be justified remains whether we think that God made the world in a certain way or that humans evolved to navigate it in the often horrendous ways that we do. On the eighth day could God have said, "Let ye who is the meanest proliferate"? So, a naturalistic hypothesis may give reason to think that human minds came to have features which correspond to the features of reality in such a

nervous systems is to enable the organism to move appropriately. Boiled down to essentials, a nervous system enables the organism to succeed in the four F's: feeding, fleeing, fighting, and reproducing. The principal chore of nervous systems is to get the body parts where they should be in order that the organism may survive. Insofar as representations serve that function, representation are a good thing."(pg. 548-549) Churchland's account seems to leave out something very important. It is reasonable to think that social interactions provided the radical evolutionary pressures necessary for the human brain to get as folded as it did in what anthropologists tell us was a stunningly short period of evolutionary time. Churchland argues in favor of a connectionist model of the mind in this article, and this may still be correct.

way that some judgments are true judgments. A theistic hypothesis might do the trick also. But, in either case, questions of the justification of simplicity judgments demand first the clarification of simplicity.

2.4.2 Ockham's Razor

The first issue deserving clarification is the famous methodological principle known as *Ockham's Razor*. For many years it has been rumored that *Ockham's Razor* (or *Occam's Razor*) is a fundamental principle of science. Roughly, the idea gestured at by the moniker "Ockham's Razor" is that other things being equal, simpler hypotheses are more likely to be true than are their competitors. Ockham's Razor is usually associated with the slogan *Entia non sunt multiplicanda praeter necessitatem* (entities must not be multiplied beyond necessity). J.J.C. Smart probably had in mind either some scientific principle, or at least a heuristic principle, when he said that he wished to resist saying that statements about subjective mental states are reports about irreducibly non-physical things "mainly due to Occams razor." [53]

I prefer to avoid using the term "Ockham's Razor". It contributes confusion to an already convoluted discourse. First of all, the principle is named after the Thirteenth Century Franciscan friar William of Ockham. William of Ockham never said "*Entia non sunt multiplicanda praeter necessitatem*". Not only is the slogan not to be found in Ockham's work, but Roger Ariew has argued that this principle is incompatible with Ockham's theology [3]. On Ockham's theology, God's omnipotence includes God's ability to make all the entities God wants to make.

“Ockham’s Razor” is also subject to the vagueness that any principle of parsimony is subject to. The principle involves an appeal to simplicity but the word “simpler” can be interpreted in many different ways. The slogan often associated with *Ockham’s Razor* suggests a constraint of some sort placed on inferential judgments of ontology. It suggests that we count the number of entities posited by a theory. The notion is that the principle governs our judgment that the hypothesis which posits the smaller number of entities is the hypothesis which is more likely to be true. It would be unusual, however, for a philosopher to invoke *Ockham’s Razor* intending that it be understood this way. Most of the time, philosophers do not mean to count the number entities posited by a hypothesis, but the number of *kinds of entities* posited by a hypothesis. Perhaps J.J.C. Smart intended this, or it was a consequence of a more general principle. The universe may contain infinitely many entities, but perhaps there are a finite number of *kinds* of entities. Similarly, there may be infinitely many properties or relations instantiated in the universe, but only a finite number of *kinds* of properties or relations. For these reasons I prefer to avoid using the term “Ockham’s Razor” altogether. Some careful philosophers (like Smart) probably do have something precise in mind when they used the term, but I fear to say that some people have fallen into careless habits. We might try to avoid propagating unnecessary confusion by stating these principles as clearly as possible whenever they are discussed or invoked.

Ockham’s Razor would be a version of what is more generally a principle of parsimony. I suggest that we analyze the more general concept.

2.4.3 The Anatomy of Parsimony

Suppose that we have a methodological principle like the following:

Principle: 2.4.1. Other things being equal, if hypothesis h_1 is simpler than competing hypotheses h_2, h_3, \dots, h_n , then h_1 is more likely to be true than its competitors.

Principle 1.2.1 is an example of what would be called a principle of parsimony. Consider the following problems with this principle. Such a principle cannot be put into practice. Even this formally presented principle is not precise enough to be employed. The word “simpler” is subject to ambiguity. Also, it is not clear what would justify such a principle – that is, it is not clear why we ought to expect there to be any relationship between simplicity and the truth. There is further a practical problem for the application of this principle in science, since the *ceteris paribus* (other things being equal) clause is never satisfied in actual scientific cases.

Sometimes the words “parsimony” and “simplicity” are treated as if they are interchangeable¹². I suggest that this should be avoided. See the Appendix for the detailed defense of the view that these terms should be kept distinct. Roughly, the reason is that *parsimony* is a methodological principle and *simplicity* is a state of affairs, or property. The principle of parsimony *involves* a criterion of simplicity¹³. But what does “involves” mean?

To make a particular principle of parsimony sufficiently clear to distinguish it from other principles of parsimony and to make it possible to apply it to hypothesis selection,

¹²Alan Baker mentions this in the *Stanford Encyclopedia* article [5]

¹³This discussion follows closely Sober’s second chapter in *Reconstructing the Past; Parsimony, Evolution, and Inference* 2002[54]

simplicity criteria must be *defined for a principle*. So a few notes on criteria are essential for this discussion. It is rather difficult to find philosophical discussion devoted to an analysis of criteria, so I follow the lead of Stanley Cavell from the first chapter of *The Claim of Reason*[15].

Firstly, criteria sometimes govern judgments, and sometimes judgments govern criteria. Secondly, criteria are object-specific. What makes a bridge unstable is not the same as what makes a person the president of a club, or what makes one recognize a chess piece as being a queen¹⁴. Thirdly, Cavell says that criteria may be epistemic (they tell us what counts as knowledge or as evidence) or they may be ontological (they tell us what a thing is). I might add that criteria may also be pragmatic (telling us what is useful for some purpose or what requires the least amount of effort under certain circumstances). Perhaps also there are aesthetic criteria. My guess is that utilitarians must at some point identify what counts as suffering, so there may be phenomenological criteria as well.

Consider the way that criteria would govern judgments in our present case. The principle of parsimony would govern non-deductive inferential judgments. Defining simplicity criteria for a principle of parsimony would tell us which features of our candidate hypotheses are to be compared. There are two ways that judgments may govern criteria. We may judge to what degree criteria are satisfied and we may also make judgments about which criteria to define for a particular principle of parsimony.

A helpful way to start thinking about the clarification project in the philosophy of simplicity is in terms of the clarification of simplicity criteria. Simplicity criteria define the

¹⁴[15] (pg.15)

bases upon which simplicity judgments are made. Our judgments of simplicity are comparative judgments. In comparing hypotheses, we have some simplicity criteria which specify the objects of comparison. In order to apply a principle of parsimony at least one criterion of simplicity must be defined. We compare the relevant features of the hypotheses in question and judge to what degree the criterion is satisfied in each case. Once the hypothesis that is simpler than its competitors is identified the principle of parsimony would govern the inferential judgment about which hypothesis is the most likely to be true.

Now the problem is that “simpler than” is subject to ambiguity. The reason is that there are many different simplicity criteria which may be defined. One natural way to think of the word “simpler” is that it is related to the word “fewer”. This suggest that simplicity criteria could be defined on the basis of some counting project. But there are many ways to count objects in the world and many ways to count the features of hypotheses. Consider this short list of possible simplicity criteria:

- fewer numbers of entities
- fewer *kinds* of entities
- fewer numbers of relations
- fewer *kinds* of relations
- fewer numbers of terms
- fewer *kinds* of terms
- fewer ad hoc posits
- fewer axioms

- fewer sentences
- fewer logical connectives in sentences
- fewer *kinds* of logical operators in a sentence (“and”, “or”, “not”)
- fewer modal operators
- fewer *kinds* of modal operators
- fewer nested modal operators

This list is far from exhaustive. But in this way we can begin to see the massive ambiguity which principles of parsimony might be subject to. There are many ways to define simplicity criteria. This does not show that there is a deep philosophical problem plaguing the analysis of principles of parsimony. We can just be clear about what the simplicity criteria are. Nor are there any obvious formal problems involved in applying principles of parsimony to theory construction or evaluation. It is not a problem to apply a principle of parsimony in science provided that the simplicity criteria are clearly defined.

Isaac Newton, gave four rules of reasoning which he offered in Book III of the *Principia Mathematica*.¹⁵

1. We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances. To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity and affects not the pomp of superfluous causes.
2. Therefore to the same natural effects we must, as far as possible, assign the same

¹⁵Reprinted from Sober 1998 pg.52. Originally from *Newton* 1953, pg.3-5

causes. As to respiration in a man and in a beast, the descent of stones in Europe and in America, the light of our culinary fire and of the sun, the reflection of light in the earth and in the planets.

3. The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever. For since the qualities of bodies are only known to us by experiments, we are to hold for universal all such as universally agree with experiments, and such as are not liable to diminution can never be quite taken away. We are certainly not to relinquish evidence of experiments for the sake of dreams and vain fictions of our own devising; nor are we to recede from the analogy of Nature, which is wont to be simple and always consonant to itself.
4. In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypothesis that may be imagined, till such time as other phenomena occur by which they may either be made more accurate or liable to exceptions. This rule we must follow, that the argument of induction may not be evaded by hypotheses.

I will leave it to the Newton scholars to tell us exactly what Newton meant by each of these assertions. Context does suggest some ways in which Newton's principles might involve specific simplicity criteria. Perhaps (1) and (2) reveal a preference for theories involving fewer *kinds* of causes. Principle (4) may suggest a principle of parsimony which selects against theories with the larger number of ad hoc hypotheses. The third principle

is left vague by this limited context alone. Principle (3) might suggest of some sort of property nominalism, or perhaps simplicity criteria which select epistemic objects. Perhaps this principle encourages the scientist to posit few qualities of bodies that go beyond what we access by way of sense experience. Consider again what has been said about the general package-deal God might give to the world in creation. It would be expected that the Early Modern philosophers mentioned earlier would think that certain criteria defining what counts as knowledge or understanding would naturally be related to ontological criteria due to their theological commitments. It appears likely that Newton held views of this sort as well.¹⁶

There is something else which is important to notice about Newton and parsimony. This is that Newton is simultaneously committed to a universe which is simple in some respects and complex in others. Newton gave just *one* law governing the motion of bodies by gravitation. In taking there to be only one type of law governing gravitational motion and in taking there to be many particles in the universe it is a logical consequence of Newton's view that he was committed to plenitude with respect to instances of this relation. Every particle stands in the $F = -G \frac{Mm}{r^2}$ relation to every other particle.

2.4.4 Ockham Revisited

It is difficult to interpret Ockham as holding a razor that urged us to posit few *numbers of entities*. Newton did not hold such a principle either. How, then, did confusion about Ockham's Razor arise? This is one of the subjects of Roger Ariew's 1976 disserta-

¹⁶Elliott Sober discusses Newton's theological justification in *Reconstructing the Past; Parsimony, Evolution, and Inference*. (pg.53-54)

tion. Some people have identified Ockham's principle of absolute divine omnipotence with Ockham's Razor. But this is a principle of possible plenitude. Ariew says that this principle says that it was Ockham's view that, "all things are possible for God save such as involve a contradiction."([3] pg.6)

Most of Ockham's arguments are directed against John Duns Scotus. They are theological arguments (and this point, for various reasons, has been muddled by historians). However, Ockham did hold principles of parsimony. Ockham held the principle "*frustra fit per plura quod potest fieri per pauciora.*" This means *in vain we do by many that which can be done by means of fewer.* He also held the principle "*non est ponenda pluralitas sine necessitate*" which means *pluralities ought not be supposed without necessity.* On the surface these do seem like *Entia non sunt multiplicanda praeter necessitatem* but Ariew argues that they are different.

Entia non sunt... appears to be a rule about entities (or real things) whereas the others do not. At the very least, the other two formulations can lend themselves to another interpretation. They are not bound to "entities." *Frustra fit...* seems to be a rule about our explanations and *pluralitas non est ponenda...* seems to be a rule about statements or concepts.([3] pg.17)

2.4.5 Hume Revisited

In thinking about how the general problem posed by the inductive skeptic, it is clear how a principle of parsimony might be applied. Hume suggested variously that people form some habit which explains how they make inferences, and that there may be some unity of nature principle which governs inductive judgments. There is an apparent duality in Hume's account. On the one hand our inferences appear to be explained by habit and on the other by a principle that experience, it would seem, cannot justify. Sober reconciles

the apparent duality of Hume's account of induction saying that Hume does not say that the unity of nature principle *never* plays a role in inductive reasoning. Perhaps people are creatures of habit most of the time, but sometimes a principle of unity governs inductive inferences [54](pg.41). Unity is the limiting case (or lower bound) for simplicity judgments. Unity is perfect simplicity. So, we can see how a principle of simplicity may be involved in inductive reasoning.

I will mention briefly here the problem of justification for principles of parsimony. In every case there is a very serious philosophical puzzle about what would justify a specific principle of parsimony. Hume's creatures of habit are irrational, and inductive arguments to justify the unity of nature principle are viciously circular. I give a detailed analysis of the types of arguments which might be given to justify principles of parsimony or the selection of simplicity criteria in the fourth chapter. What is interesting to notice is that the inductive skeptic, on this particular issue, does not force the epistemological project to the highest philosophical priority but rather, pushes metaphysical issues into the foreground. This is Sober's main point in Chapter 2 of *Reconstructing the Past; Parsimony, Evolution, and Inference*. Principles of parsimony often make substantive claims about the way the world is by involving ontological simplicity criteria. Even Hume's unity of nature principle does this and Hume's view that people naturally form irrational habits suggests that there are *mechanisms* of the mind which cause this irrational behavior. In either case the metaphysical issues are forced to the foreground. So, the project which philosophers are faced with is one of coming up with a way to categorize and analyze simplicity criteria. Mario Bunge has given us the best way to do this. How simplicity criteria are categorized and analyzed

is the subject of Chapter Three.

Another problem about principles of parsimony in science arises because it is very difficult to give clear examples of the principle at work in science. It is notoriously difficult to find a case where the *ceteris paribus* clause is satisfied. There is a problem getting a handle on the notion of what it would be for hypotheses to be competitors in actual cases in science. Often we expect that scientific hypotheses do not merely describe a data set, but that they explain it. So, we can imagine that the phrase “other things being equal” suggests that two competing hypotheses would both equally well explain the same data and there would be no way of deciding which hypothesis to select other than by some appeal to simplicity. Although this is conceivable, it is difficult to find unproblematic examples of such a principle at work in the history of science.

This could mean that we should think about the work that scientists do in a different way. Perhaps instead of *selecting* hypotheses over their competitors, scientists give arguments for why we ought to *reject* certain hypotheses. Scientists would not, for instance, say that one hypothesis is acceptable because it explains the data and is simpler than its competitors, despite the fact that it also entails absurdities.¹⁷ Also, scientists would not say that although one hypothesis explains the data (in some sense of the word “explains”), the explanation is scientific, although the hypothesis depends on the actions of gnomes or other types of causes which are far removed from the accepted scientific ontology.

It is very difficult to find an example of a case where scientists had before them

¹⁷Although, to be fair to philosophers, scientists do not seem to think of things like *singularities* or mysterious *observer effects* as absurdities.

a pair of different hypotheses which are perfectly acceptable scientific explanations for exactly the same data and neither of which failed to reconcile with the body of science in general, so that we are left only with an appeal to simplicity (in some respect or another) to decide between them. It might turn out that no cases actually arise where judgments of simplicity are the only court of appeal in selecting which of a set of genuinely competing hypotheses science is to embrace.

Scientists, in a venerated tradition, have invoked and defended principles of parsimony. It is at least clear that there are many roles for principles of parsimony to play in scientific methodology, although they would need to be precisely stated and justified. So suspicions about the practical applicability of a principle of parsimony do not head off the motivation for the analysis of principles of parsimony. This kind of thing occurred to Kant when he read Hume. In just clarifying the principles that would govern rational inductive inferences we end up either doing traditional metaphysics or doing metaphysics of mind. Kant's Copernican Revolution is to recast the traditional study of metaphysics as the science of mind.

2.5 More Than Just Ockham's Razor

It is easy to fantasize that some clearly defined and justified principle of parsimony would solve the problems of non-deductive inference. Since non-deductive inferences play prominent roles in scientific methodology it is tempting to think that some principle of parsimony is at work in science. It is tempting to imagine that scientists are occasionally confronted with hypotheses which, if true, would explain some data and it is tempting to

think that scientists occasionally select one hypothesis over a competitor for testing because it is simpler, in some sense, than its competitors. Nevertheless it is notoriously difficult to find unproblematic examples of the principle of parsimony in science. Two general problems present themselves in finding examples of the principle of parsimony at work in science. One problem is that the *ceteris paribus* (other things being equal) clause is rarely satisfied. The other problem is that arguments which are meant to justify the positive selection of a hypothesis over its competitors are rarely given by scientists. Rather, scientific arguments often appear to take forms which would not fit the principle of parsimony at all. The *reductio ad absurdum* is doing a great deal of work for scientists and it would be an error to assume that judgments of simplicity are playing a role in scientific method if, in fact, scientists are employing *reductios* instead. Many of the arguments which are paradigmatic of good scientific practices either rule out certain hypotheses due to their failure to unify with the body of accepted theories and data or they show that current theory and ontology leave some phenomena unexplained, thus opening the door for scientific revolutions. Does this mean that it is difficult to find *judgments of simplicity* playing roles in scientific methodology? It does not. Judgments of simplicity may play a variety of important roles in scientific methodology. It is just that the role for simplicity is, in many cases, not one which involves the form of the principle of parsimony sketched above. Philosophers could speculate about the scientific process and imagine cases where the principle of parsimony plays a role in hypothesis selection *a priori*. But the transworld travel agent may sell only one-way tickets. It would be nice to have a few actual cases.

An example which tends to crop up in casual conversations is that the postulation of

the planet Neptune was simpler than other hypotheses which might explain Uranus' orbital data. Neptune was posited theoretically and then, a very short while later, its existence was confirmed empirically. This example is assumed by some to illustrate the success of scientific methodology. Furthermore, some people assume that the principle of parsimony is the method which played the crucial role in this story. Fortunately, Richard Swinburne has put this example into writing. So, in the spirit of engaging the polemic, I can engage Swinburne on the use of this historical case.

Swinburne has managed to give a very lucid defense of a systematic view of simplicity in his very short book *Simplicity as Evidence of Truth*. Swinburne defends a principle of parsimony but there are other significant features of his view as well. Another part of Swinburne's view involves a multi-faceted simplicity concept which can be filled out in a variety of ways. In looking closely at the history of the discovery of Neptune, I cannot figure out how Swinburne's principle of parsimony plays a role. However, simplicity may play a multitude of roles in the construction and testing of scientific hypotheses. My guess is that Swinburne would not be disheartened by these results. Swinburne might just say that my research illustrates features of his view involving the multiple facets of simplicity. However, he should choose a different example. I investigate this important bit of eighteenth century astronomy to criticise Swinburne's principle of parsimony and also to supply the second and third chapters with rich examples.

2.6 Neptune and Vulcan

In 1845 it was known to scientists that Uranus did not follow a Keplerian orbital path. Working independently, the French astronomer Urbain LeVerrier and the English astronomer John Couch Adams each posited the existence of a planet with a specific mass and with a particular orbital path which would interact gravitationally with Uranus in just such a way so as to explain the data. LeVerrier's calculations were sent to the Berlin Observatory where, in 1846, Johann Gottfried Galle and Heinrich d'Arrest were able to observe the planet now known as Neptune.

Swinburne argues in defense of a principle of parsimony: that other things being equal, the simplest of competing theories is most probably true. Swinburne refers to the story of LeVerrier's famous successful hypothesis to help illustrate his view:

Assuming Newton's theory to be true, LeVerrier wondered why Uranus moved along a path which was irregular in comparison with the path which Newtonian theory seemed to predict for it. On the supposition that the only other planets were the five known to the ancients together with Uranus, the irregularities were not to be expected. So LeVerrier postulated a seventh planet, which pulled Uranus out of its regular orbit when close to it. He could have postulated seven hundred heavenly bodies with a common centre of gravity at the position at which he postulated Neptune, but the former supposition was simpler (in virtue of the first facet which I described)¹⁸. Or he could have put forward a yet more complicated proposal, amending Newton's theory and postulating two further heavenly bodies at the same time. But of course he would not have supposed that the evidence supported that complicated hypothesis.¹⁹([59] pg. 57)

At first blush some things seem to be intuitively clear about the example. Unless

¹⁸The word "facet" is Swinburne's term. It indicates that there are different ways of conceiving of simplicity.

¹⁹I do not know what Swinburne had in mind. I do not see that LeVerrier was concerned with 5 planets. LeVerrier was already concerned with seven planets, one asteroid belt, and one sun in his studies of the perihelion advance of Mercury when he posited Neptune.

we wish to rework our successful scientific theories then it seems to be simpler to posit one thing rather than seven hundred to explain specific data and to explain away anomalies. Some people find *one* to be intuitively simpler than *seven hundred* because *one* is less than *seven hundred*. Some people also find *not reworking our successful scientific theories* to be simpler than reworking them in the sense that it is *easier* not to rework entire theories.

Most people likely to read the works of philosophers like Swinburne have been taught that there is a planet called “Neptune”. I learned about it from an arrogant elementary school science teacher responsible for a mixed bag of facts, poisonous errors and infelicities. Since my initial introduction to the word “Neptune” art, popular culture, and media sources have used the word often together with the words “science” and “scientists” in ways which have reinforced the things that I have come to believe about Neptune. My guess is that most readers of Swinburne have a similar history associated with their use of the word “Neptune” and with the notion of its participation in the history of science. And although very few of us have actually worked out LeVerrier’s calculations and although few of us have actually viewed Neptune through a telescope, most people are fairly confident that the story of the discovery of Neptune is a story of scientific success. I suppose that the rough idea about why this would be a story of scientific success is that the existence of Neptune would have been predicted by mathematical theory before it was observed.

Despite these intuitive footholds, I am not convinced that this case has been described clearly enough to serve in any regard relating either to *simplicity* or to *science*. Attempting to employ this example in the service of simplicity in science may be a joust with a pivoting target on the short end of a trebuchet. The example must be spelled out in a

bit more detail if there is to be any hope that it will not immediately supply its own counter examples. There are three basic reasons to reconsider how this example might work: two are historical and one is philosophical.

All planets wander from their Keplerian paths (depending on how accurately we demand that the data fit the mathematical predictions). Some planets diverge from their Keplerian orbital shapes more than others. LeVerrier famously studied *two* of the more poorly behaved planets: Mercury and Uranus. LeVerrier was actually engaged in a study of Mercury prior to his study of Uranus. The successful confirmation of the existence of Neptune was thought by some also to be confirmation of the *methods* LeVerrier employed in his studies of Mercury. Mercury, which was notoriously difficult to observe due to its small size and nearness to the sun accumulated non-Keplerian data as telescope construction methods evolved and as generations of astronomers went to painstaking lengths to take and record precise observations. LeVerrier was the scientist who gave the argument that there was something unique about the advance in Mercury's perihelion; something which could not be explained only by Mercury's gravitational interactions with the other known planets in the solar system. LeVerrier posited additional planetary matter to explain this just as he did to explain the perturbation in Uranus' orbit. Eventually LeVerrier endorsed the view that the planet Vulcan which some people (not all of whom ought to be called *scientists*) already thought to be located between the Sun and Mercury was part of the inter-planetary matter that he had posited.

On the basis of the same sources of authority which lead most people today to believe that Neptune exists, it is also believed that Vulcan does not. The reason that we

might be suspicious about the strength of this example is that it is possible that LeVerrier came to posit a roughly correct hypothesis about the orbital shape and mass of Neptune, but stumbled upon it by methods which we might think of today as non-scientific. Even during LeVerrier's lifetime there were scientists critical of his methods. Emmanuel Liais was very suspicious of the inter-Mercurial matter hypothesis and critical of LeVerrier's methods, saying of the apparent success of the Neptune hypothesis, "To Galle therefore, and not to LeVerrier, the honour of the discovery, as to Newton and not to the apple, that of universal gravitation." [?] p.26

If this sort of criticism turns out to be on point then we should not accept the story of the discovery of Neptune as a *scientific success story*, but rather as a fable about the treasures that can occasionally be acquired by blindly groping about. Naturally a tale of non-scientific groping is not the sort of tale appropriate for the job which philosophers like Swinburne intend for it.

Another problem with the story is that hindsight indicates that one of LeVerrier's assumptions may be false. The Newtonian inverse square law of gravity has been replaced. The accepted theory is now General Relativity which describes the shape of space-time by a set of equations: Einstein's field equations. Today gravitational motion is explained by General Relativity rather than by Newton's inverse square law. It is now accepted that the additional (roughly) 40'' per century ²⁰ of advance in Mercury's perihelion which is not explained by interactions with other planets is explained by General Relativity. From

²⁰LeVerrier calculated 39'' of perihelion advance not explained by gravitational interactions with other planetary matter, but Simon Newcomb later calculated the value to be 43'' per century.

a contemporary perspective, the advance in Mercury's perihelion might appear to be a symptom of the fact that the inverse square law of gravitation was not exactly correct.²¹ I would not wish to defend the principle that *other things being equal the conjunction of simplicity and at least one false assumption generates theories which are most probably true*. So, a reason to proceed carefully is so that the example does not do better to illustrate this principle rather than the sort of principle defended by Swinburne. Also, the fact that science has embraced a more complicated mathematical description of gravity might do better for the view that *complexity* (rather than *simplicity*) is a guide to truth.²²

I am not quite sure what to make of Swinburne's suggestion that the simplicity exemplified by $1 < 700$ is a guide to the truth of the theory committed only to 1 thing rather than 700 things. If 1 and 700 are both numbers, then it is not at all obvious why they are not equally simple. It is the tradition to offer scientific theories in general form and so we might not be surprised to see a theory where x and y are the variables which contingently take the values of 1 and 700 on some interpretations. The variables x and y might be said to be equally simple.

Also, as a historical matter, it was not the case that LeVerrier posited his "there is one Neptune" hypothesis and suggested tests for it in competition with some "there are 700 things" hypothesis. Even though I realize that philosophers are supposed to be able to imagine empirically equivalent hypotheses, I am never quite sure how far I might fly on

²¹Some scientists have even given arguments that the shape of space-time can be derived from the advance in Mercury's perihelion and the Lagrangian [50] p.166

²²Of course Swinburne knows that GR is more complicated in some sense than Newtonian mechanics. He discusses this in his book. I discuss his view in more detail in Chapter 3

the wings of imagination before I express only ignorance about laws or initial conditions relevant to the scientific discourse. If I assume the Newtonian law of gravitation to be true and then imagine seven hundred objects with a center of mass located on an orbit the same as the one occupied by Neptune, then I am just imagining a big, less-dense-Neptune. Now only the density of Neptune is disputed by the rival theories of which I have conceived. The densities of Neptune₁ in hypothesis₁ and Neptune₂ in hypothesis₂ are not relevant to Uranus' goofy orbit since Newtonian mechanics models masses as point masses. The example could be tweaked so as to remove this triviality. If it were also suggested that the odd flip-flopping about of irregular objects with a common orbital center of mass helped to explain additional features of Uranus' behavior (perhaps some nutation in its precession²³) or some other dynamic in the solar system (perhaps an advance or retrograde motion in the perihelion of yet another planet) then the "one thing" hypothesis would not exactly be a competitor with the "seven hundred thing" hypothesis because they would predict different data. In this case the *ceteris paribus* clause of the principle of parsimony is not satisfied. If perhaps the view that the laws of motion are invariant under Galilean transformations were changed or if we conjure theories with different space-time shapes or different numbers of dimensions of space-time then we might imagine a "seven hundred thing" hypothesis which is a genuine competitor with the "one object" hypothesis, but that would not be holding *other things equal* either, and thus would be a poor way to advance Swinburne's project.

A closer look at the historical details of LeVerrier's work may help to clarify this

²³A nutation is a periodic oscillation in the precession of the axis of a planet.

story enough to permit us to reconsider the relevance (or irrelevance) of certain features of the example to the role of simplicity judgments in scientific methodology. N.T. Roseveare authored the book, *Mercury's Perihelion: from LeVerrier to Einstein*, the purpose of which was to counter the murky things that have been said about the efforts from the 1840's to the early 1900's to account for the non-Keplerian orbital data recorded for certain planets. Roseveare holds that what had been said was lacking in crucial details [50] and that only by hindsight does it seem as if the issues which led to the overturning of the inverse square law of gravity are clear. The problems faced by astronomers and physicists during this roughly one hundred year period were complex and difficult, and Roseveare holds that some accounts have missed crucial details. Roseveare's historical investigation of this period helps to shed some light on the scientific process and some of the roles for simplicity judgments in it.

Questions ought to be raised about which of LeVerrier's methods are thought to be exemplary scientific methods. In this regard, LeVerrier's studies of both Mercury and Uranus are relevant. Questions also ought to be raised about what LeVerrier's contributions to science might be. In this regard both his methods and his arguments are relevant. On this issue, I think that LeVerrier's arguments involving the orbital data on Mercury supply especially rich material for examples. The reason is that LeVerrier's arguments involving the perihelion advance of Mercury fueled the next one hundred years of scientific debate about general theories of gravitation and eventually took center stage in discussions about General Relativity.

LeVerrier and John Couch Adams both made their initial predictions about the or-

bital position of the planet presumed to be perturbing Uranus in accordance with Bode's Law. Bode's Law states that each solar system body will occupy an orbit with a semi-major axis located at a distance from the sun twice the distance of the semi-major axis of the orbital body just inside of it. Bode's Law (or the Titus-Bode Law) is based on the series: 0,3,6,12,24,48... . Adding 4 to each number and then dividing each by 10 gives Astronomical Units for the distances of the semi-major axes of the planets from the sun (where Earth is 1.00au from the sun). Planets seemed to fit *roughly* Bode's Law until the discovery of Neptune. As it turned out, the telescopic data which confirmed the existence of Neptune contributed to the dislodging of Bode's Law from the corpus of accepted scientific principles. Apparently, for many scientists, the semi-major axis of Neptune's orbit a bit *too* roughly fit Bode's Law predictions of its distance from the Sun. As an extra twist to this story, many nineteenth century scientists argued against hypotheses which posited matter between Mercury and the Sun on the basis that such hypotheses would violate Bode's Law!

Multi-bodied gravitational problems are very difficult to solve mathematically. There is no general solution to the three body problem in Newtonian mechanics. The positions and velocities for each body at some time step all depend upon one another. There is no way to 'peek' at Uranus' future position without knowing what Neptune's future position at that same time is. But this depends upon Uranus' position at that time. Still mathematicians have worked out methods to *model* multi-bodied systems and these calculations are quite laborious to do by hand.

Consider the sort of skill that LeVerrier had mastered. Newton's equation for the gravitational force between two masses M and m_1 separated by a radius r_1 is,

$$F = -G \frac{Mm_1}{r_1^2} \hat{r}. \quad (2.1)$$

Where \hat{r} is a unit vector in the direction of the radius between from the first mass to the second. This shows that the force of gravity acts in the direction opposite the unit vector.

If we wish to know the perturbing force on a planet m_1 we need another equation of the form,

$$F = S + P, \quad (2.2)$$

where S is the force between the sun and the planet and P is the perturbing force.

If the perturbation is caused by a third mass m_2 at distance d from m_1 then these equations combine to give,

$$F = -G \frac{Mm_1}{r_1^2} \hat{r} + G \frac{m_1 m_2}{d^2} \hat{d}. \quad (2.3)$$

If m_2 is not a moon of m_1 but rather another planet orbiting M then another equation is needed for the total force on m_2 ,

$$F' = -G \frac{Mm_2}{r_2^2} \hat{r} + G \frac{m_1 m_2}{d^2} \hat{d}. \quad (2.4)$$

Substituting Newton's Second Law $F = m\ddot{a}$ for F and F' in equations 2.3 and 2.4 canceling terms and factoring we have,

$$\ddot{a}_1 = -G \left(\frac{M}{r_1^2} \hat{r} + \frac{m_2}{d^2} \hat{d} \right) \quad (2.5)$$

$$\ddot{a}_2 = -G \left(\frac{M}{r_2^2} \hat{r} + \frac{m_1}{d^2} \hat{d} \right). \quad (2.6)$$

Acceleration is the first derivative of velocity with respect to time and the second derivative of position with respect to time. So equations 2.5 and 2.6 can be used to describe the velocity and position of m_1 and m_2 . However, at this point a few decisions must be made about how to proceed. We must decide whether spherical or Cartesian coordinates will be used. Assume Cartesian coordinates. Then it is convenient to place M (the sun) at the origin and generate the following equations which can be solved for velocity,

$$\frac{\partial \sqrt{\hat{v}x_1^2 + \hat{v}y_1^2 + \hat{v}z_1^2}}{\partial t} = -G \left(\frac{M}{x_1^2 + y_1^2 + z_1^2} + \frac{m_2}{\sqrt{x_1^2 + y_1^2 + z_1^2} + \sqrt{x_2^2 + y_2^2 + z_2^2}} \right) \quad (2.7)$$

$$\frac{\partial \sqrt{\hat{v}x_2^2 + \hat{v}y_2^2 + \hat{v}z_2^2}}{\partial t} = -G \left(\frac{M}{x_2^2 + y_2^2 + z_2^2} + \frac{m_1}{\sqrt{x_1^2 + y_1^2 + z_1^2} + \sqrt{x_2^2 + y_2^2 + z_2^2}} \right) \quad (2.8)$$

and for position,

$$\frac{\partial^2 \sqrt{x_1^2 + y_1^2 + z_1^2}}{\partial t^2} = -G \left(\frac{M}{x_1^2 + y_1^2 + z_1^2} + \frac{m_2}{\sqrt{x_1^2 + y_1^2 + z_1^2} + \sqrt{x_2^2 + y_2^2 + z_2^2}} \right) \quad (2.9)$$

$$\frac{\partial^2 \sqrt{x_2^2 + y_2^2 + z_2^2}}{\partial t^2} = -G \left(\frac{M}{x_2^2 + y_2^2 + z_2^2} + \frac{m_1}{\sqrt{x_1^2 + y_1^2 + z_1^2} + \sqrt{x_2^2 + y_2^2 + z_2^2}} \right). \quad (2.10)$$

In order to integrate 2.9 and 2.10 we need some values for the position. However, in order to get the relevant $\langle x, y, z \rangle$ values for some time step we need to know the velocity. But the velocities depend upon the positions, and the velocities *and* the positions of

each planet depend upon the position of the other. This illustrates why there is no general solution for this sort of problem. Nevertheless, mathematicians have worked out ways to approximate solutions to these differential equations. These sorts of methods involve getting solutions to much simpler equations, then using those values to feed back into the more complex equations. Such methods have been called *the method of variation of elements* or *the finite element method*. We might, for example, treat each planet as if it is only in a two bodied system, solve for the position at some δt and then use those results to go back and start the first round of rough calculations, knowing that the initial values could not be exactly correct.²⁴

Now it might be easy to appreciate what LeVerrier accomplished. He worked out the solutions not for a three bodied problem (like the example given) but for a 8 bodied problem (because he was concerned with 7 planets and the sun)! LeVerrier demonstrated his great skill as a mathematician by working out these complicated partial differential equations using the finite element method. LeVerrier's results on Mercury's orbit were included in nautical almanacs and astronomical ephemerides internationally for over forty years.

LeVerrier employed Newtonian mechanics in an argument which contributed to refuting Newtonian mechanics. This is the sort of method which, I think, deserves more focus from philosophers of science. It is a method which generates an argument that current

²⁴LeVerrier probably used spherical coordinates to work out his solutions. This is what one would expect given that LeVerrier was interested in Mercury's perihelion advance. That is, he would want to know what the change in the changing angle swept out by Mercury in its orbit was over long periods of time. But the choice of coordinate system does not alter the point about the mathematics - that there is no solution to the three bodied problem. I find it a bit less difficult to illustrate how the interdependent variables arise in the three bodied problem using Cartesian coordinates.

theory and ontology leave some phenomena unexplained. LeVerrier gave the argument that the advance in Mercury's perihelion could not be accounted for by gravitational interactions with the known planets alone. Later the ability to explain Mercury's advance was thought to be one of the explanatory virtues of General Relativity. Just like all astronomers at the time, LeVerrier was aware of the two general strategies for pursuing a scientific explanation of Mercury's behavior: 1) posit changes in the amount or configuration of planetary matter in the solar system, 2) reject the inverse square law of gravity. Scientists could see that the equation for the perturbing force,

$$F = S + P,$$

might be expressed by a law involving only two bodies like,

$$F = -G \frac{M_1 m_2}{r_1^2} \hat{r} + G \frac{M_1 m_1}{r_2^3} \hat{r}.$$

Rosveare says,

that laws differing from the inverse square law gave precessing orbits was known long before Mercury's anomalous advance was discovered. Such results may be found in Newton's *Principia*, the principle work in which Newton put forward his gravitational theory. ([50] pg. 12)

In 1745 Alexis Clairaut suggested laws differing from the inverse square law in this way. Yet LeVerrier resisted challenging the inverse square law, saying that,

If the tables [of Mercury's positions] do not strictly agree with the group of observations, we will certainly not be tempted into charging the law of universal gravitation with inadequacy. These days, this principle has acquired such a degree of certainty that we would not allow it to be altered; if it meets an event which cannot be explained completely, it is not the principle itself which takes the blame but rather some inaccuracy in the working or some material cause

whose existence has escaped us. Unfortunately, the consequences of the principle of gravitation have not been deduced in many particulars with a sufficient rigour: we will not be able to decide, when faced with a disagreement between observation and theory, whether this results completely from analytical errors or whether it is due in part to the imperfection of our knowledge of celestial physics. ([50] pg. 21)

Roseveare suggests that social forces may give another reason for scientists to be reluctant to challenge the inverse square law of gravitation. He says that,

Only a highly reputable mathematician would have had sufficient stature for his results to stand up against the authority of Newton. ([50] pg 38)

So a principle common in LeVerrier's study of Mercury and his study of Uranus was the principle that it is better to *posit more planetary matter* than to *muck with Newton's laws* when explanations are constructed to explain the orbital data of these planets. To explain the behavior of Uranus, LeVerrier favored a hypothesis positing only one other planet. However to explain Mercury's behavior LeVerrier favored a multiple bodies (or an asteroid belt) hypothesis. Even when LeVerrier endorsed certain claims which would have suggested confirmation for his hypothesis (that the transit of an inter-Mercurial object had been observed) he still held that there would have to be more than one such object. It is interesting to note that it appears to be consistent with LeVerrier's methodology to posit one thing or hundreds of things to explain certain data, since the number of things was not a basis upon which LeVerrier expected the truth of his hypotheses to depend. Rather, LeVerrier was committed to explaining data with a very high degree of precision without changing general laws of motion. Perhaps a more subtle commitment is that LeVerrier posited more of the same *kind* of stuff with which scientists were already familiar. He posited matter either balled up in a planet or distributed in rings to explain specific data and

yet never forwarded hypotheses involving gnomes, spirits, ethers, or any other sort of stuff different from that which planets are thought to be made of.

Although scientists were aware of the logically possible strategies for constructing hypotheses to explain Mercury's perihelion advance, it took several years for an array of competitors to be published. LeVerrier himself suggested several hypotheses involving inter-Mercurial matter, but other approaches were reflected in the hypotheses given over the next thirty to forty years. In the late nineteenth century a brilliant astronomer, Simon Newcomb, collected these hypotheses and constructed arguments regarding the plausibility of each. The following is a list of the hypotheses with condensed versions of the reasons given by scientists that each hypothesis was unlikely to be true.

1. Inter-Mercurial planet (Le Verrier): Le Verrier thought that this hypothesis was implausible because a planet of sufficient mass to account for the advance would have been easily observable in transit and no observations of a planet this large had been made.
2. Inter-Mercurial astroid ring (Le Verrier): Le Verrier also thought that the transit of astroids would be observable. He ended up endorsing contentious claims made by some astronomers to have observed objects of this sort. Even so, Le Verrier knew that astronomers had not recorded the sort of data that one would expect on the astroid-ring hypothesis because the observations (mistakenly made of sun spots not planetoids) were of objects too small to account for Mercury's advance. He may have endorsed this view in hopes that more planetoids would be observed.
3. Inter-Mercurial rings (Le Verrier): It was known that on certain intervals of Saturn's

orbit the rings are at such an inclination with respect to Earth that they were not visible to Earth-bound observers. Inter-Mercurial rings might not be visible from Earth. Le Verrier argued against this hypothesis concluding that in order to be at the correct inclination to cause the advance in Mercury's perihelion the rings *would* be observable from Earth.

4. Zodiacal Light Matter: There is a pancake shaped haze of dust around the sun which diminishes as the distance from the sun increases. The evidence of this dust can be viewed at certain times of the year on very dark nights as a glow in the night sky. It was suggested that this haze of matter might interact gravitationally with Mercury so as to account for the advance in Mercury's perihelion, but Simon Newcomb argued that the zodiacal cloud would cause a braking action on the orbits of Venus and Earth causing retrograde motion in their perihelions. This was not consistent with the orbital data on Earth and Venus. ([50] p. 48) Later scientists revised the zodiacal cloud hypothesis to account for Mercury's perihelion advance and obtained very good results. This later hypothesis was accepted as the correct explanation until after General Relativity when in 1918 Harold Jeffreys argued that the particles in the zodiacal cloud could not be large enough to produce the effect due to the way the way that the particles reflect light and given the sizes they would have to be in order not to get blown out of the solar system by solar winds.
5. Oblate Sun: It is natural to think that if there are not inter-Mercurial rings, then perhaps the sun is oblate. However, arguments had been given by the end of the nineteenth century that the sun was actually slightly prolate. ([50] p. 46-47)

6. Non-inverse square law (Newton): As Newton realized, such laws might well account for Mercury's orbit, but they would not describe terrestrial gravitational motion (objects falling near Earth) ([50]p.51).
7. Dynamic non-inverse square laws (Zöllner and Tisserand, 1872): The suggestion was made that gravitational laws may be like Weber's velocity-dependent laws of electrodynamics: $F = \frac{GMm}{r^2} \left\{ 1 - \frac{1}{h^2} \left(\frac{dr}{dt} \right)^2 + \frac{2r^2}{h^2} \frac{d^2r}{dt^2} \right\}$; where h was a constant *similar* to a constant used in electro-dynamics. Scientists raised objections to Weber's electro-dynamics at the time, so basing a theory of gravity on a hotly contested theory of electro-dynamics raised immediate objections. Newcomb noted that the choice of h to get a theory which accounted for orbital data would be a value of h different from the one accepted in electro-dynamics. Newcomb neither accepted nor rejected this. The choice of h which accounted for Mercury's perihelion advance was $\sqrt{2}c$ ([50]p.44). Another type of objection involved the suggestion that if the space were infinite then infinite forces might be possible ([50] p. 130). This was viewed as an absurdity since matter was not observed to be moving at velocities approaching infinite velocities²⁵.
8. Non-inverse square law (Asaph Hall, 1894): Newton had shown that the angle between successive perihelia was $\theta = 2\pi \left(\frac{b-c}{mb-nc} \right)$ where b, c, n, m are constants which might be chosen for a force law of the form: $F = \frac{br^m - cr^n}{r^3}$. Hall chose $n = 2.0000001574$ so that it accounted for Mercury's advance. The law seemed to work

²⁵I do not know why scientists thought that this was absurd or why they expected to observe things moving with near infinite velocities.

well to describe the orbits of other planets as well. Newcomb even appears to entertain the simplicity of Hall's hypothesis relative to other non-inverse square laws as a possible virtue saying that,

This hypothesis seems to me much more simple an unobjectionable than those which suppose the force to be a more or less complicated function of the relative velocity of the bodies. ([50] pg.51)

However, Newcomb went on to argue that if this law were applied to the data on the motion of the moon, then the moon would have a center of gravity offset from its geometrical center. Many scientists saw this result as absurd since it would change what they thought about how planets travel orbital paths, and would lead to apparently absurd conclusions like that the moon may well have an atmosphere and life collected on the side away from the Earth. ([50]p.51-55)

One interesting feature of the methodologies of LeVerrier and Newcomb is that they rule-out, by way of argument, far more hypotheses than they defend. Newcomb was a well respected scientist and his arguments were taken quite seriously by the scientific community. One way to see the result from the contemporary perspective might be to say that it is not surprising that all eight hypotheses are false because none of them is General Relativity. However, another way to see this situation is to notice that Newcomb gave arguments showing the explanatory failure of each of these hypotheses, and thereby motivating an investigation into what would be an acceptable explanation for the anomalous advance in Mercury's perihelion, thus helping to set the stage for General Relativity in 1917. The real argument might have been that *nothing* in physical theory at the time could explain Mercury's perihelion advance.

Another thing to notice is that some of these arguments would not be very good arguments by philosophical standards. Why would anyone think that a velocity dependent force law for gravitation would have the same constants as a velocity dependent force law for electro-dynamics? Why would anyone think it obviously absurd if the moon's gravitational center were not at its geometrical center and that there may be an atmosphere on its dark side as a result? Nearly every hypothesis advanced by brilliant scientists like LeVerrier and Newcomb strike me as being as surprising and counterintuitive as the hypotheses they refute. Nevertheless, I believe that the reasons given *are* good reasons for scientists to resist embracing certain hypotheses for further study or testing. The reasons scientists did not embrace wild theories like these is that they would disrupt widely accepted value for unity in science. The value for some unity of science principle is reflected in several of these arguments, but especially in the argument that the constant h in a velocity-dependent gravitational law would be the same as the constant h in a velocity-dependent electro-dynamic law.

Scientists expect that hypotheses should account for the target data. The first three hypotheses do not account for the data in the sense that LeVerrier expected; so that if these hypotheses were true then either they would, on certain parameterizations, fail to account for Mercury's advance, or on other parameterizations, would be attended by additional observational data which was absent.

The arguments against the oblate-sun hypothesis may give examples of both methods just suggested. The oblate-sun hypothesis was in conflict with the interpretation scientists gave to telescopic data involved in describing the sun's shape. This is a case where a

hypothesis may account for a set of data central to certain lines of inquiry, but fail to reconcile with other data to which the scientific community is largely committed which can be seen as a conflict with some version of a unity of science principle. It may also have been that if the sun were oblate rather than prolate then the observed telescopic data would be different than it was.

Scientists also expect scientific theories to be general. What it is for a scientific theory to be general may be subject to some vagueness, but it is clear that scientists expected hypotheses to be involved in theories which account for a broad range of data. While I would not wish to offer my own theory of what it is for a theory to be general, it is possible to point to cases where a value for general theories played a role in scientific methodology. Newton, for example, expected the force law to explain both terrestrial and extraterrestrial motion. Scientists working on the problem of Mercury's perihelion advance expected that uniform laws of friction and gravitation should explain zodiacal light matter interactions with *all* planets.

What this case study has developed so far is the following list of methodological guidelines: that hypotheses account for data, that proposed hypotheses do not imply data which is at odds with other data, that theories are sufficiently general, and that hypotheses are proposed in accordance with a unity of science principle. In addition to these, scientists often expect that the construction and testing of theories ought to involve the addition of as few *ad hoc* hypotheses as possible. Inventive philosophers might not be swayed by the argument that a non-inverse square law gave an odd center of gravity for the moon and therefore ought to be rejected. Perhaps the postulates or axioms of geometry could be

changed. Perhaps hypotheses about the way that light travels between the moon and the Earth or through the lenses of our telescopes could be changed in existing theories to give a coherent story about the motion of terrestrial and extraterrestrial bodies despite Newcomb's argument. Yet for some reason, scientists reject this sort of imaginative tinkering. Scientific theories and methodological principles are overturned by careful and methodical steps. Bode's Law was a widely accepted principle of astronomy despite the margin of error which appeared to be increasing as the distance of planets from the sun increased. Eventually a hypothesis which was constructed and tested using Bode's Law (the Neptune hypothesis) made a significant contribution to the dislodging of the principle from accepted scientific methodology.

Finally, what is interesting to notice about the hypotheses suggested in the study of Mercury is that they are *rejected* rather than accepted on the basis of certain scientific arguments. After a discussion of the problems of induction it is tempting to think that the fundamental puzzle of scientific methodology is how it is that hypotheses that are more likely to be true than their competitors come to be accepted by the scientific community. But the clever scientific arguments given by LeVerrier and Newcomb are arguments telling us which hypotheses ought to be abandoned. None of the reasons given for the abandoning of hypotheses obviously have to do with the relative complexity (or non-simplicity) of the hypothesis, but rather with the methodological values suggested above: hypotheses ought to account for data and ought not to introduce weird data, hypotheses ought to contribute to theories which are general in some sense, the unity of science is a good thing, and *ad hoc* hypotheses are a bad thing.

Swinburne tried to employ a reference to LeVerrier's work to provide an example of how scientists judge that simpler hypotheses are more likely to be true. I cannot see how Swinburne's thesis might be advanced using this example. After all, LeVerrier makes it very clear that he is more committed to his methods than he is to the number of objects that his hypotheses posit. He would rather posit planetary matter to explain the data than overturn Newton's Laws. Some people will leap at the opportunity to point out that LeVerrier is invoking some principle of parsimony with respect to ontological kinds in being committed to there being more of the same *kind* of stuff in his ontology. However, he did not favor his particular hypotheses (Vulcan or Neptune) relative to hypotheses which posited ghosts or effluvia. Were hypotheses positing additional kinds of substance under consideration to explain the orbital data, we *would* have examples where a hypothesis is thought to be more likely than its competitors due to some simplicity judgment about the numbers of kinds of things posited. Instead, LeVerrier wanted to posit more of the same kind of matter rather than to overturn a mathematical theory. To say that hypotheses were compared for their relative simplicity in this case would be to commit a category mistake. LeVerrier is the poster child for a scientific mantra which might go "it is better to endorse a false hypothesis than to change a good methodology."

This story shows the importance of doing scientific work to reject hypotheses and thus to show what remains to be explained. This activity is an important part of the scientific process (of what Kuhn calls *normal science*). *Accepting* hypotheses or theories by an application of a principle of parsimony is not part of this story. So my hope is to point down the path towards greater philosophical focus on the part of science which demon-

strates problems with theories or methods rather than on the part of science concerned with comparing which of several *competing* hypotheses is supposed to be the most likely. Although each of the above hypotheses are competing with the others in the sense that they are each given initially to explain Mercury's advance, they are not compared to one another in the arguments which refute them. So, in some sense, they are never really in competition. Although, in one case, Newcomb mentions what he thought to be the virtue of simplicity exemplified by one hypothesis over another, this serves no role whatsoever in the argument which leads him ultimately to reject the view.

There may be many different ways that simplicity judgments are involved in scientific methodology. Simplicity might be involved in the construction of hypotheses. We might wonder if it was simpler for LeVerrier to posit more of the same kind of matter employed in other scientific explanations than to posit novel kinds of matter for his hypotheses. We should ask what kind of simplicity this is before asking any questions about whether or not principles involving this kind of simplicity are justified.

Perhaps it is simpler, in some sense, to remain stalwart against the temptation to tinker with extremely general and broadly accepted laws. Perhaps it is simpler to have one rather than two or three constants in velocity dependent laws regardless of the sort of phenomena they are posited to explain.

Simplicity might be involved in the construction of arguments which motivate certain lines of inquiry. LeVerrier's argument that Mercury's perihelion advance could not be explained by gravitational interactions with known matter in the solar system involved methods of discrete integration which depend upon certain simplifying assumptions: roughing-

in the first set of calculations as if each problem is a two-bodied problem rather than a multi-bodied problem.

Simplicity, perhaps, is involved in the expectation that our scientific theories be general or that they unify with other theories. Even if the mathematics gets verbose or difficult to use or to comprehend, it still may be simpler to have one law describing the motion of all bodies, or perhaps one constant for velocity-dependent laws than it would be to generate multiple laws.

It might even be that simplicity considerations are shot through several different parts of the scientific methodology, since many methods implicitly involve resisting the addition of *ad hoc* hypotheses to the body of scientific theory. In this way, *the fewest number of ad hoc hypotheses* would be a species of simplicity. However, simplicity judgments in this case are part of the general method of theory construction, and not committed only to a principle of parsimony.

2.7 The Simplicity of the Ancients

In this final section, I discuss the deeply entrenched features of simplicity in philosophy.

The word “simplicitié” probably came into use sometime in the 12th century. The first citation in the *Oxford English Dictionary* is Chaucer’s *Boethius* (“simplicitie”) (1374).²⁶

The suffix “ity” indicates an abstraction from a comparison.

²⁶Simplicity: ‘The state or quality of being simple in form, structure, etc.; absence of compositeness, complexity, or intricacy’ [1]. The word may have earlier Latin ancestors for all I can tell, but etymology is not the focus of this project.

It is important to remember that simplicity is contrasted with complexity perhaps in several senses. There is no upper bound on complexity, but the lower bound on simplicity is *unity*. A methodological principle that promotes unity, is a principle of simplicity. To be clear, we must ask what simplicity criterion is involved in the principle, or what kind of unity the principle promotes. If a simplicity principle does not promote unity (the limiting case of simplicity), then it is a principle that can only be applied in comparative judgments.

Knowing how it is that art, culture, and science reflect one another like a hall of mirrors, it is fun to fantasize that the whole mess about the principle of parsimony was introduced by some early scientist who infected science with some whimsical aesthetic. Swept away by Chaucer and wine this scientist spilled his drink and observed how the chaotic substance resolved itself into a pattern on his pillow. The next morning he returned to his laboratory with a stained face and a new methodological principle. Western thought, ever since, would have inherited a mistake. But I believe that the roots of simplicity in science are far older. Roger Ariew says,

Briefly, the broad historical argument is this: the roots of what is called Ockham's razor can be traced back to Aristotle's *Physica* and *De Caelo*. One can pick up the medieval interpretations of Aristotle's principle in the Latin translations of Averroes' commentaries on the *Physica* and the *De Caelo* or the later Medievals (Aquinas, for example). One can then see how Ockham came about his principle of parsimony from these sources through his immediate predecessors, Peter Auriol and John Duns Scotus.([3] pg.15)

Friedrich Nietzsche had a useful insight when he associated the names of ancient Greek thinkers with archetypal doctrine. This is useful because it helps to organize different kinds of simplicity principles. Additionally, if Nietzsche's account of the thought of the ancient Greeks is correct, then the place of simplicity in science is not merely that it is

a reflection of the values, or errors of Medieval or Early Modern thinkers, but something which is *deeply* embedded in our language and thought. Nietzsche says that, “what they [the Greeks] invented was *the archetypes of philosophical thought*. All posterity has not made an essential contribution to them since.” ([48] pg.31)

I have wondered if Nietzsche is right on the second claim. If so, then we would not learn anything about philosophy by reading contemporary literature in addition to reading the Greeks. I am however, convinced that Nietzsche is right on the first claim.

If I try to fantasize about what the ancient Greek philosophers were talking about, I imagine that they had noticed that there seemed to be order rather than chaos and the game became one of explaining this with as few hypotheses as possible. For the pre-Socratics, the game would have been to give just *one* hypothesis. The ancient Greeks were primarily concerned with cosmogony (the logical evolution of the universe). The fundamental puzzle appeared to be the question of how there came to be a plurality of things and how assertions are possible.

Nietzsche says that the Greeks prior to Plato were pure types and from Plato on philosophers have mixed the pure pre-Socratic views. This is Nietzsche’s useful claim. He is saying that there is a way to think about the philosophical dialectic as several competing hypotheses about unity. He says that it would be

correct and simple to comprehend the latter as philosophic mixed types, and the former as pure types. Plato himself is the first mixed type on a grand scale, expressing his nature in his philosophy no less than in his personality. Socratic, Pythagorean, and Heraclitic elements are all combined in his doctrine of Ideas. ([48] pg.34-35)

Nietzsche says that Thales was *the* notable character at the birth of Greek philoso-

phy. Nietzsche has Thales as a proto-empiricist. Kant seems to suspect this as well, because he mentions that Thales may have been the one who turned geometry into an empirical science.²⁷ It has been said that Thales turned the Greek philosophers *away* from explanations of natural phenomena in terms of anthropomorphic gods and heros. However, it is interesting that Nietzsche also points out the *unity* hypothesis “all things are one” is what is really distinctive of Thales’ Greek philosophy and that Western thought is shot-through with attempts to reinterpret this doctrine.

Greek Philosophy seems to begin with an absurd notion, with the proposition that *water* is the primal origin and the womb of all things. Is it really necessary for us to take serious notice of this proposition? It is, and for three reasons. First, because it tells something about the primal origin of all things; second, because it does so in language devoid of image or fable, and finally, because contained in it, if only embryonically, is the thought, “all things are one.” The first reason still leaves Thales in the company of the religious and superstitious; the second takes him out of such company and shows him as a natural scientist, but the third makes him the first Greek philosopher. Had he said “water turns into earth,” we should have but a scientific hypothesis, a wrong one but difficult to disprove. But he went beyond scientific considerations. By presenting his unity-concept in the form of his water-hypothesis, Thales did not, it is true, overcome the low level of empiric insight prevalent in his time. What he did was to pass over its horizon. The sparse and unordered observations of an empirical nature which he made regarding the occurrence and the transformations of water (more specifically, of moisture) would have allowed much less made advisable, no such gigantic generalization. What drove him to it was a metaphysical conviction which had its origin in a mystic intuition. We meet with it in every philosophy, together with the ever-renewed attempts at a more suitable expression, this proposition that “all things are one.”([48]pg.38-39)

We can see how other ancient philosophers cast, and recast the *unity* hypothesis. Nietzsche says that, “Anaximander takes two steps beyond him [Thales]. For the first he asks himself: How is the many possible if there is there is such a thing as the eternal?.” ([48] pg.49)

²⁷Kant mentions this in the *Preface* to the Second Edition of *The Critique of Pure Reason*.

Alexander Polyhistor, in summarizing the Pythagorean doctrine says that the first principle of all things is the One. From the One came an Indefinite Two (dyad), as matter for the One, which is cause.²⁸ Alexander's discussion of Pythagorean doctrine involves not only the role of the unity in cosmology but also its causal role. Pythagoreans also thought that there was a relationship between mathematical cosmogony and the mathematical basis for music theory²⁹. The history of science is shot through with stories of contributors who cast and recast these Pythagorean values (often in Christian theological frameworks). In general we ought to ask of the role that simplicity has played in reasoning, why it is thought to be related either to causation or to aesthetic values.

We can also imagine the opposing camps occupied by Parmenides and Heraclitus giving their versions of the *unity* hypothesis. Parmenides is thought to have held the view that all things are one and that change is impossible. It is also thought that Parmenides was the primary influence in Plato's division between reality and illusion. Heraclitus held the view of the *unity of opposites*; that everything is changing. By modern categorical labels, Parmenides would be the anti-realist *extraordinaire* and his distinction between reality and illusion would preclude any empiricist commitments. But Heraclitus, who may be thought of as another kind of anti-realist, offered an empirical hypothesis.

I see nothing other than becoming. Be not deceived. It is the fault of your myopia, not of the nature of things, if you believe you see land somewhere in the ocean coming-to-be and passing away. You use names for things as if they rigidly, persistently endured; yet the stream into which you step a second time

²⁸Cornford says that, writing in the first century B.C., Alexander would certainly have been influenced by Plato's *Timaeus* and therefore it is not entirely clear if this is properly a Pythagorean view or if it is just Plato's view. [19](pp.3)

²⁹Aristotle's (Metaphysics v. 285b, 23)

is not the same one you stepped into before. ([48] pg. 52)

I am not sure how the Heraclitian doctrine of the unity of opposites was robbed of its empirical content, but if the thief left any clues he probably left them in the *Timaeus*. It is Plato who tried to take the middle ground between the Pythagoreans who give explanations in terms of what remains the same and the Heraclitian doctrine of change.

Although modern thinkers tend to see Aristotle as rebelling against Plato's division between appearance and reality, Aristotle probably saw himself as the defender of Plato's middle way. Aristotle framed this issue when he promulgated the syntax of scientific discourse in the *Physics*.

Now that we have made these distinctions, here is something we can grasp from every case of coming to be, if we look at them all in the way described. In every case there must be some subject that comes to be [something]; even if it is one in number, it is not one in form, since being a man is not the same as being an unmusical thing. (By 'in form' I mean the same as 'in account'.) One thing [that comes to be] remains, and one does not remain.[4] (*Physics* Book I)

What changes is predicated of the subject which does not change. In order to engage the grammar of science we must establish the vocabulary of science. That is, we must establish which terms are said to change and which are said to stay the same. We must also show which are the law (or law-like) ways in which things come to be. That is, we must generate statements of the form, "it is a law that $Fx \rightarrow Gx$." Unless the Parmenides of Plato's dialog is right that 'there is One, and nothing else can truly be asserted', then some things change and some things stay the same. The scientific language of discourse is committed to some things changing and others staying the same. If it is ever the case

that this is not so, then the physical world will either grind to a screeching halt, or be torn asunder at the joints.³⁰

The difficult questions about parsimony as a methodological principle in science might be seen as a lasting battle between methods that generate hypotheses about *sameness* and hypotheses of *change*. These two general versions of parsimony, in the abstract, are in conflict and yet I see no way to avoid doing science in the basic way outlined by Aristotle. It is as if, in the Fourteenth Century, the appearance of the word “simplicity” in Europe indicated that modernity had finally given up on the Pythagorean ideal. Hume’s critique of induction is the herald of the fact that the empirical hypotheses of Thales and Heraclitus had settled securely into the foundations of scientific methodology, but without their empirical content!

In the introduction to this chapter I promised to show how deeply principles of parsimony are embedded in scientific discourse and I promised to show how the Ancient Greeks can provide for us these very abstract archetypes by which to categorize this discourse. Europeans in the wake of the Copernican Revolution may have given up on the simplicity of ancients because of the straight forward fact that the ancients gave hypotheses about unity, and since there is no upper bound on complexity, the usefulness of the word “simplicity” may indicate that people noticed that the hypotheses of the Ancient Greeks were *too simple* to be employed in the scientific project.

³⁰Plato appears to have the character Socrates defend his merger of the Heraclitian and Pathagorean views by giving an interpretation to Homer saying, “so long as the heavens and the sun continue to move round, all things in heaven and earth are kept going, whereas if they were bound down and brought to a stand, all things would be destroyed and the world, as they say, turned upside down. (*Theaetetus*[49])

Plato mixed the pure hypotheses of the ancients and this shaped modernity. Aristotle followed Plato in mixing the Pythagorean unity of that which is insensible with the empirical unity of Heraclitus. Some things change and some things stay the same and I cannot see how to do science unless we think of it in this very abstract way. The scientific project involves categorizing those things which stay the same and then giving hypotheses about the law (or law-like) ways in which they change. Kant points out that the terms of the science of geometry were fixed before geometry became a science. Similarly, Kant points out that Galileo and Descartes fixed most of the terms of the physical sciences before Newton developed his laws of motion. One of the most important projects involved in science is the project of determining which are to be the signs which stay the same (the predicate terms) for a particular body of discourse. We can think of the Logical Behaviorists as suggesting a massive empirical research program which would result in a tome of operationalized psychological predicates. Another way to think about this part of the scientific project is as a set of questions about where to locate the stay-the-same-structures. Kant locates the structures in the minds of individuals. Chomsky does something similar locating the universal grammar in the human mind. Kuhn locates that which stays the same (at least for a while) in society. This archetype of philosophical thought can even be seen in Russell's work where he mentions a common materialist intuition. "One great reason why it is felt that we must secure a physical object in addition to the sense-data, is that we want the *same* object for different people."³¹

The mixed hypotheses of Plato and Aristotle are useful, but they lead down the

³¹Chapter II of *The Problems of Philosophy*. The emphasis on "same" is Russell's

garden path to skepticism also. Hume noticed this. Hume needed only to reflect on his own experience in order to get a Heraclitean hypothesis of change. Hume finds no *thing* or *self* which stays the same as his thoughts glide by, posturing in this or that way. The problem that arises due to the mixing of the Pythagorean and Heraclitean hypotheses is that the only empirical hypothesis appears to be the Heraclitean one. But the doctrine of flux, by itself, will not do the work of science.

Scientific methodologies will involve several different kinds of simplicity criteria because science is fundamentally committed to a project where some things are said to change and others to stay the same. It is also the case that scientific hypotheses make substantive claims about what there is and about what the fundamental mechanisms of change are. We are faced with a variety of questions about simplicity the moment that we step into the scientific enterprise. The next order of business is to see what might be done by way of organizing and categorizing kinds of simplicity in science.

CHAPTER 3 THE COMPLEXITIES OF SIMPLICITY

The previous chapter introduced several issues. Naïve resistance to the philosophy of simplicity really only serves to motivate it. “Simplicity” is subject to vagueness, and the clarification of simplicity is a philosophical project which is motivated, at the very least, by the fact that we wish to avoid slipping into the equivocal use of terms. It also turns out that the arguments given to clarify simplicity defeat a few naïve dogmatisms. On the one hand, it is not the case that there are no philosophical problems of simplicity in science. Nor is it the case that simplicity reflects *merely* pragmatic or aesthetic values, for this claim is either false or too vague to evaluate. On the other hand, the clarification of simplicity shows that it far from clear that Ockham’s Razor is the final court of appeal in theory construction and selection as many have believed it to be. Again, at the very least, this is because “Ockham’s Razor” is subject to vagueness. There are many razors, not all of which give compatible results. But various principles of parsimony may indeed play important roles in science.

When a principle of parsimony is put to work in science, it is in conjunction with many other considerations. It may not even be possible to imagine cases where all other things are equal and some principle of parsimony, which is specified in a non-arbitrary way, would be the only court of appeal for judging that one scientific theory is more likely to be true than its competitors. The previous chapter’s discussion of Swinburne’s thought experiment shows how a defense of a *scientific* principle of parsimony with a *ceteris paribus* clause can go wrong. Scientists are concerned with satisfying many diverse goals when they construct theories. I hope that I have shown that philosophers must proceed very

carefully if they wish to construct thought experiments in the philosophy of science. The general goals of this chapter are first to categorize simplicity criteria in science, and second to elucidate the relations between these criteria and some of the other goals of science.

I hope that the previous chapter has also shown that it would be misguided to claim that judgments of simplicity play no role whatever in science. The fact that principles of parsimony govern non-deductive inferential judgments (whether they are rationally grounded or not) should be sufficient to show this. In this chapter I hope to explain precisely why it is so difficult to conceive of situations where empirically adequate theories satisfy equally the desiderata of science, and we are left only with simplicity judgments to select one over the other for testing. Once the various features of scientific theories that might be specified by simplicity criteria have been categorized and the other desirable features of scientific theories have been identified, it becomes clear that scientific theory construction involves a delicate act of balancing the various desirable features of theories.

I have also tried to show how it is that the grammar of science is fundamentally committed to at least two very abstract notions of simplicity: the doctrines of sameness and change.¹ One simplicity doctrine is intuitively simpler than two, so in this way the grammar of science is already committed to complexity. Complexity can be expected to increase from this very abstract level as workable theories are constructed and the *things* that stay the same and those that change are defined. Even Aristotle's basic example where a man comes to be musical from having been unmusical involves one subject term and two

¹Plato's mix of the Pythagorean doctrine and the Heraclitean doctrine establishes a pattern which is found repeated throughout Western philosophy, not just in science. See the final section of Chapter One.

predicate terms. Additionally, if we read a bit further in Aristotle's *Physics* we see that Aristotle has at least two different conceptions of necessity that govern change.

Mario Bunge holds that science marches in the direction of complexity, not simplicity. Perhaps this is not at all surprising. It is a deeply entrenched feature of the grammar of science that multiple predicate terms must be defined, that subject terms must be defined, and that mechanisms of change must be posited. In this chapter I present Bunge's detailed analysis of simplicity and synthesize Bunge's arguments about the relationships between the various species of simplicity and some of the other desiderata of science into a new, and I hope, useful form.²

This chapter also includes a discussion of the valuable work done by several other philosophers who join Bunge in criticizing Logical Positivism. The period of the criticism of positivism is a fertile one for philosophy. In this period, we find Nelson Goodman making important contributions to the analysis of simplicity. We also find Carl Hempel and Ernest Nagel laying the ground work for the contemporary discourse on scientific explanation. The analysis of scientific explanation is related to simplicity on several fronts, including the relation of simplicity judgments to the construction of explanations that are general and that are conceptually related to other accepted explanations. This chapter introduces the terminological distinctions and arguments contributed by these philosophers. It is useful to work through a bit of the history of the philosophy of science because these authors contributed to analytic clarity on the issue of simplicity judgments and their rela-

²The title of this chapter is a tribute to Bunge, whose 1962 article is named "The Complexity of Simplicity"

tions to the other desiderata of science. It is also useful to investigate these arguments to show what reasons we have for rejecting the positivist's wholesale hostility towards metaphysics. Although not all simplicity judgments in science make ontological commitments, we find simplicity judgments embedded in a web of interrelated judgments, some of which also make ontological commitments.

In the first section of this chapter I present Bunge's categories of simplicity. Bunge argues that simplicity is not of one kind but of many. Although judgments of simplicity are part of scientific methodology, we should not expect an *overall* measure of the simplicity of theories due to the extreme heterogeneity of simplicity. Bunge also gives arguments which show why it is extremely difficult to give a formal basis for measures of simplicity. I call this the *the gauging problem*. Bunge had several objectives in giving his analysis of simplicity. One goal is to dispel the sorts of dogmatic views mentioned above – that simplicity is hopelessly vague or that simplicity is the final court of appeal in theory selection. Another goal is to show that it is far from clear that judgments of simplicity in science can be reduced to judgments of one kind or to judgments of other kinds. *Ockham's Razor* is mentioned in many places in philosophy, but it can be crippled by vagueness, so philosophers can offer a bit of analytic clarity to this perennial issue. Bunge also makes suggestions about future research programs and I hope to advance his project.

The second section of this chapter is an attempt to synthesize many of the arguments given by Nelson Goodman in the 1940's and 50's with the arguments given by Bunge, and Hempel from the 1960's. *Truth* may be one aim scientists have in constructing and testing

theories³, but if it is, it is one among many aims. Other aims may include: *explanatory depth*, *linguistic exactness*, *accuracy*, *systematicity*, *unity of science*, *representativeness* (Bunge's term), and *testability*. Call these *the desiderata of science*. The desiderata of science are not all logically independent of one another. *Depth*, for example, is related to *testability*, and *testability* is related to *accuracy*. As we shall see *systematicity* is related to *testability*, *unity of science*, and *representativeness*. Each of these terms and their interrelations will be discussed in detail in the second section.

Discussions of the desiderata of science and their relations to one another are distributed throughout many articles and it is very difficult to discover what are the essential features of each and to generate a *comprehensive* view of their relations to one another. Perhaps some people can manage all of this information without the aid of tools. I suspect that Nelson Goodman and Mario Bunge actually did so partly due to the fact that these philosophers had a deep understanding of science. I, however, cannot manage them without the aid of tools. I developed a conceptual map to manage, organize, and analyze the tremendous volume of interrelations between, what Bunge calls the *metascientific criteria*. I present these results in the second section of this chapter. In order to engage this analysis, we must first clarify the various species of simplicity relevant to science and the problems involved in measuring them. Questions about the relation (or relations) between judgments of simplicity and the truth of theories or their relations to ontology are important questions, but I see no way to give a precise formulation of these questions without first clarifying the roles played by various judgments of simplicity in science. We cannot begin to understand

³This should be appropriate to assert, regardless of one's conception of what the truth is.

the roles that simplicity judgments might play in science without first understanding how simplicity judgments are related to one another and to the other aims of science. My hope is that the conceptual map will aid in showing why this is and that it will also provide a useful piece of the dialectic, since it is convenient to critically engage something that organizes terms and relations in this way.

The questions of interest to philosophers about whether or not simplicity is, in any way, a feature of the mind or of the mind-independent world may be of interest across interdisciplinary boundaries. Perhaps these questions are also of interest to physicists, cognitive scientists, and social scientists. But because simplicity is not of one kind but many, a bit of dialectical separation can be kept between questions about the relationship between some judgments of simplicity and the truth and the other important questions about the roles of simplicity in the construction and testing of scientific hypotheses and theories. Perhaps some of the simplicity criteria at work in science are different from those of interest to the classical epistemologist. In the philosophy of science there may also be field-specific questions about how to classify and gauge simplicity.

There are also questions about what roles properly specified simplicity criteria might play in scientific theory construction and testing. We do appear to have theories that are *empirically adequate* in the sense that *up until now* they appear to account for the relevant data. However, theories which compete in the sense that they fit existing data may give different predictions, and competing theories are not *conceptually equivalent* because they posit very different kinds of things and different fundamental mechanisms. Currently, for example, there are many different *interpretations* of quantum physics which are not

conceptually equivalent.⁴ There are also various theories of General Relativity which are not conceptually equivalent.⁵

The criteria which aid in theory construction, testing and selection are called ‘metascientific criteria’. Simplicity criteria are often listed among the metascientific criteria. I follow Mario Bunge in calling the analysis of the metascientific criteria and their interrelations *Metascientific analysis*. Simplicity judgments may contribute to the construction or testing of a theory by aiding in the satisfaction of the other metascientific criteria. Simplicity judgments may also conflict with the other metascientific criteria. In these cases simplicity is usually trumped. Mario Bunge says that

only those simplifications will be admitted in science which render the theory more manageable, more coherent, or better testable: no simplification will be accepted if it severely cuts down either those characteristics or the depth, the explanatory power, or the predictive power of the theory. ([10]pg. 123)

The problem of giving necessary or sufficient conditions for the relation between simplicity criteria and the truth is only *one* of the problems for the philosophy of simplicity. Elucidating the relations between different kinds of simplicity criteria and one another and the other metascientific criteria presents an array of difficult and interesting problems. In addition to these problems there are fundamental problems selecting a formal gauges of simplicity.

The first section distinguishes general species of simplicity criteria and explains the

⁴I do not know if it would be appropriate to say that the different interpretations of quantum theory are themselves competing *theories*. I follow Bunge only to point out that these are not conceptually equivalent.

⁵R.H. Dicke and A.N. Whitehead have given theories which are often cited as theories which compete with Einstein’s General Relativity. [50]

problems involved in gauging simplicity. The second section explains the interrelations between metascientific criteria. The concluding section explains how the analysis contributes to the philosophical dialectic by revisiting some of the issues raised in the previous chapter.

3.1 Categorizing and Gauging Simplicity

Bunge organizes simplicity into five categories: *syntactical simplicity*, *semantical simplicity*, *epistemological simplicity*, *pragmatical simplicity*, and *ontological simplicity* [10]. This section presents Bunge's arguments. Contemporary journal publications regularly feature articles which discuss issues related to simplicity in science [24] [54] [55] [22]. Bayesian curve fitting algorithms have seen a recent spike in academic popularity. Yet, I find Bunge and Goodman too infrequently cited in these articles, because Bayesian curve fitting algorithms depend upon syntactical gauges of simplicity which Bunge and Goodman have shown to have many deficiencies. It is worth giving some careful attention to the articles that served a key role in developing contemporary debates. I also follow Bunge in thinking that this analysis helps to dispel dogmatisms, to avoid unnecessary vagueness, and to promote new research programs.

3.1.1 Ontological Simplicity

Ontological simplicity, in the sense relevant to science, is a comparative measure of the number of kinds of entities or relations posited by a theory.⁶ Questions about simplicity

⁶I say 'in the sense relevant to science', because there may be other issues in ontology where simplicity plays a central role. Questions about whether or not propositions are to be admitted to ontology or whether there are universals or binding relations between universals and particulars are issues which may also involve judgments of simplicity.

are fundamental for ontological inquiries. Recall that when we judge simplicity, there is no upper bound on complexity and the lower limit of simplicity is one (or unity). If all is change, or if there is only one thing and nothing else can be asserted truly, then the correct ontological hypotheses were promulgated long ago by the Pre-Socratics. However, few are convinced of these views. If we think that there is an assertory foundation for science (that scientific statements are either true or false), then we move away from these views and in the direction of complexity, because, at the very least, we are committed to Plato's mixed view: a verisimilar *scientific* theory would be committed to some things changing while others stay the same.

Plato's synthesis of the Heraclitean and Pythagorean hypotheses is adopted by Aristotle and henceforth forms the basis for the grammar of science. Some things change and some things stay the same. As scientists and philosophers we do two projects (often simultaneously). We categorize which things are of the stay-the-same sort and we posit mechanisms by which things change. On the face of it, science is committed to some ontological complexity. This is because science is a mix of the pure doctrines of change and unity. But it is not clear what would count as ontological complexity. It is notoriously difficult to determine how to organize experience so that we could even start to get a measure of the simplicity of things which might correspond to reality. Consider the two sets of objects in figure 3.1.



Figure 3.1: The Counting Problem

If we think about ways to organize the two sets so that we may compare their relative complexities, it is natural to consider various counting strategies. On one way of counting, the two sets are equally simple (or equally complex) because they each contain two objects. On this way of counting, we can say that each set has a complexity of 2. But we also notice that these two sets are different in certain respects. So the identification of the ways in which they differ might be the first step in establishing some basis for comparative judgments. On one way of counting colors, the set on the left counts 1, and the set on the right counts 2 because there is one *type* of color on the left and 2 *types* of colors on the right. But if we count by comparing *types of shapes*, then the left set counts 2, and the right set counts 1.

It is difficult to say what, if anything, about the context of the presentation of these objects could determine how we are to rank the counts given to various types of attributes. Are shapes to be given a more hallowed ontological status than shades are to be? In this case, the problem grows even more puzzling when we consider counting by the number of sides of each shape. Now the circle all by itself, presents a problem. Does the circle have a side-count of 1 or 0 or ∞ ?⁷

⁷Popular author William T. Vollmann said that, “on the subject of what ought to be, let’s remind ourselves that the purpose of conceptualization is to transform reality’s perceptual randomness into patterns. A perfect circle excels in beauty, elegance and mathematical simplicity.”([63] pg.31) Vollmann goes on to claim that there has been psychological research which shows that people remember things as circular even if they are not. He does not cite the source and this point, if true, would be irrelevant to mathematical simplicity because it is not clear that a circle is not just a special case of an ellipse. Vollmann is neither a scientist nor a philosopher. We should not expect this sort of carelessness from scientists or philosophers. I find the quote interesting because it might reflect just the sort of ideas which people of our epoch are conditioned to accept without question. After all, despite Vollmann’s carelessness, he is a very popular award winning author, so perhaps he does reflect popular dogmatisms.

The problem presented here is not a problem judging simplicity. We know how to compare larger numbers with smaller numbers. If we know what to count then we have no problem counting and no problem comparing the results. The problem may be seen as giving us a two part objection strategy: 1) in some situations it is not clear how to *rank* incompatible counts, and 2) in some situations it is not clear how to get started counting in the first place. This problem marks one trail head into the jungle of metaphysics. Richard Fumerton gives a similar example where the difficulty is in deciding which similarities or differences will serve as the basis upon which we organize books on a bookshelf.⁸ Fumerton's point is that radical metaphysical relativism is not the only port in this storm.

The fact that we notice similarities and differences between objects and their attributes in a variety of ways is common dialectical ground held by all parties venturing metaphysical inquiry. The fact that there is a problem ranking incompatible measures of the complexity should not stack the deck for or against any metaphysical positions, because the problem itself favors no position. Still, the suggestion that we count the sides of a circle may appear to be symptomatic of some philosophical sleight of hand. If so, this *lègèretè des mains* has given philosophers to mystify themselves for two and a half thousand years. It is true that in some cases we would be forced to solve deep and enduring issues in metaphysics in order even to propose a measure of complexity. However, this is not always the case. In some cases we can just stipulate, perhaps on pragmatic grounds, the criteria which select the objects to be counted, and we can establish a measure of simplicity. In such cases it is not a problem to count the things specified, and it is not a problem making judgments

⁸Realism and the Correspondence Theory of Truth (2002)

of simplicity. Epistemological questions remain, such as determining which, if either, of two incompatible simplicity judgments is justified. The question of justification will be addressed in the next chapter.

Every species of simplicity may be subjected to this same objection strategy. In some cases it is not clear how to establish a formal measure of simplicity/complexity. In other cases formal measures are, at least in principle, available. When formal measures are available it is very easy to select another simplicity criterion and generate a measure which gives incompatible results (objection strategy (2)).

3.2 The Simplicity of Systems of Signs

Theories are systems of hypotheses with interpreted terms. Systems are interrelated hypotheses, and there are various ways of relating hypotheses. Hypotheses are constructed out of linguistic symbols, and for this reason we can consider the linguistic features of hypotheses which might be specified by simplicity criteria. Bunge says that,

There are basically four types of sign in the field of discourse: terms, propositions, proposals, and theories. Hence, we must begin by studying the formal or structural simplicity of terms (designating concepts), sentences (expressing propositions and proposals), and theories (systems of propositions). In turn, since propositions and proposals are built out of predicates (like ‘between’), names of constants (like ‘Argentina’), and variables (like ‘ x ’), logical constants (like ‘or’), logical prefixes (like ‘all’), and modal prefixes (like ‘possibly’), a methodical study of logical simplicity should begin by examining the formal complexity of predicates.⁹([11] pg.114)

⁹I do not know why Bunge uses the word “proposition” when he wishes to discuss the simplicity of systems of signs. It might seem like “statement” or “expression” might be better since we wish to gauge the complexity of systems of symbols and it is not obvious how to show that propositions are structured in the same way that sentences are because different languages have different rules of grammar. Also, I do not know if Bunge makes a distinction in the uses of words “proposal”, “hypothesis”, and “axiom”. These words have different definitions and it seems like all of them ought to be included in the analysis. Probably, the running together of these terms reflects a positivist

In contrast with the tightly knit systems of signs found in scientific theories, we sometimes find loosely related groups of conjectures in non-scientific discourses. I am often puzzled by what it is that makes some people apparently more successful than others at slinging terms like “upper and lower chakra” in New Age contexts far removed from the ancient Upanishads, that is, torn from their original systematic place. Some of this language, I think, fails to be explanatory. It appears, instead, to be some sort of *magic* for social maneuvering. Part of what makes the use of these terms successful in specific social circles is probably that they are deployed in a deliberately vague way. After all, some kinds of vagueness are favorable to predictions – recall the first chapter discussion of the inferences made by Sherlock Holmes. The contrasting case with non-scientific discourse is useful to consider because it may draw attention to a virtue of scientific theories. Most scientists and philosophers are in agreement about which fields of discourse are at least candidates to fall under the heading of science. While some may disagree about whether or not psychology (for example) is a science, most will agree that it is an important question to ask whether or not it is. However, most scientists and philosophers will agree that it is not worth discussing the scientific merits of New Age babblings, mystical prophecies, or conspiracy theories. Although the so-called pseudo-sciences may have *some* of the characteristics of genuine scientific theories, they are lacking in far too many to be seriously considered scientific. Often vagueness is what takes a discourse far from the path of science. The contrast shows us that we seek a kind of precision in the language of science.

residue because several years passed before the axiomatic account of theories stood sufficiently challenged.

One reason to seek formal mathematical or logical expressions in theory construction is for precision.

We can think of theories as systems of signs. Some of the signs are formal and some of them are the signs of natural language. The formal language in which a theory is codified can contain formal logical signs like “or”, “not” and “all”. They also consist of what are often called *extralogical bases*, and the natural language interpretations of these symbols. The formal language will also have rules that specify which sequences of symbols count as well-formed sentences or formulae. They will also have rules specified which govern the transformation of well-formed-formulae into other well-formed-formulae. These are the transformation rules of the system. The simplicity of extralogical predicates is the first topic of study.

3.2.0.1 The Syntactical Simplicity of Predicates

Consider the various ways in which a predicate may be simple or complex:

- Predicates may be formally simple like the predicate ‘extended’. They may also be complex like ‘extended over a sphere’.
- Predicates may be one-placed or many-placed.
- Predicates may be first-order or second-order.
- Predicates may be dichotomic (presence/absence predicates like “hole”) or metrical

¹⁰This list is quoted from Bunge’s *The Complexity of Simplicity*(1962)

Immediately it is apparent that there are several bases upon which to judge the simplicity of a predicate. Following the pattern suggested by the problem of establishing a basis for judgments of ontological simplicity it is obvious that, here too, our choice of the gauge for the logical simplicity of predicates must be non-arbitrary. This problem is even more difficult than it might first appear to be.

The first problem is that a predicate does not necessarily reveal the complexity of what it denotes. Equally simple signs “*S*” and “*C*” can be used to denote sequences of ideas which are more or less complex. For example, *S* could stand for the class of grade-classes of pupils at Sonora High School and *C* could stand for the class of grad-classes of pupils at Cassina High School. Perhaps both schools have 100 students, but let us say that Sonora has four grades (9-12) and Cassina has only one grade. In this case *S* would denote a class of grade-classes of pupils with the more complex structure¹¹.

Bunge considers an even trickier example. Let us say that we wish to gauge the complexity of a dichotomic predicate like ‘black’. Bunge walks through a proposal about how to measure the complexity of this predicate in order to show what is unsatisfactory about it. We might attempt to measure the complexity *C* of the predicate by adding the number of its atomic constituents *A* to the number of number of places *P* and to its degree *D*.

$$C = A + P + D \tag{3.1}$$

The complexity measure of the predicate ‘black’ would be,

¹¹This sort of example is used by Goodman (1943)

$$C = 1 + 1 + 1 = 3. \quad (3.2)$$

This suggests that the complexity measure may be normalized by dividing by 3. Since simplicity is inversely proportional to complexity we could get a simplicity measure from,

$$S = \frac{1}{C} = \frac{3}{A + P + D}. \quad (3.3)$$

This would give a function that ranges from infinite complexity (simplicity of 0) to perfect simplicity (1). However, this measure is generated from a common sense usage of the term ‘black’. In physics ‘black’ has a special definition involving the extralogical predicates ‘electromagnetic wavelength’, ‘absorbs’ and ‘absorption coefficient x ’. The definition is “an object is black when it completely absorbs all electromagnetic wavelengths.” This might be stated more formally, “ $black(x) =_{df} (y) [electromagnetic\ wavelength\ y \supset x\ absorbs\ y \cdot (absorption\ coefficient\ x = 1)]$.” The atomic value of the predicate ‘electromagnetic wavelength’ is 2 (because it involves a two predicates) and 1 for ‘absorbs’ and for ‘ x ’. Similar to the example above, the predicate ‘black’ would denote a complex structure given its technical definition in physics. The total molecularity is $2 + 1 + 1$. Since ‘absorbs’ is a two place predicate the total number of places is $1 + 2 + 1$ and the total order of degree is $1 + 1 + 1$. This gives,

$$S = \frac{1}{C} = \frac{3}{4 + 4 + 3} = \frac{3}{11}. \quad (3.4)$$

Equations (2.1) and (2.2) give incompatible results. On one proposal about how to measure the complexity of a predicate it is not clear how to establish a non-arbitrary way to select the gauge. Scientific theories contain formal predicates and natural language interpretations of those predicates. It is not clear how we can establish a non-arbitrary formal measure of the syntactical simplicity of predicates which captures both features of a scientific theory and which gives consistent results. The problem is perhaps even worse because even the formal measure of simplicity of the scientific predicate ‘black’ may hide some of its structure. Some metrical predicates like ‘wavelength’ have an infinite range while others, like ‘absorption coefficient x ’ have a finite range (but may take an infinity of values). We might ask what would non-arbitrarily justify either excluding or including these features of the syntactical complexity of predicates in a gauge of simplicity. Additionally, it is not clear that we ought to adopt this proposed measure of predicate simplicity because it involves the linear sum of non-homogeneous predicates. We would need some reason for thinking that non-homogeneous predicates should stand on par in their relative complexity measures.

There may be further problems gauging predicate simplicity. The basic predicates of a scientific theory appear immersed in a system and may be related to other symbols by law statements and perhaps this too ought to fall under the scope of the analysis of the complexity of predicates. Also, many symbols are governed by certain transformation rules. Bunge points out that a symbol which is part of a system may seem to be simple on the face of it, yet by the transformation rules allowed by the system, it may be infinitely complex. For example, the numeral ‘1’ may be written as, $\frac{m}{m}$, $\frac{-n}{-n}$, $i \cdot (-i)$, $(-1)^{2n}$ and so

on. These predicates are not syntactically equivalent, so just by applying the transformation rules specified by a system to some of the predicates we might achieve an *ad hoc* gain in syntactical simplicity.[11] If the transformation rules were given so that each of these symbols is supposed to express the same propositional content, then contradictory measures of simplicity are derivable by application of the transformation rules. This is another example of objection strategy (2).

3.2.1 The Syntactical Simplicity of Formal Expressions

The problems of predicate simplicity are bad enough and formal expressions are strings of symbols. Nevertheless, much of the more recent philosophical literature on simplicity has been dedicated to the syntactical simplicity of formal expressions (or propositions, if we follow Bunge's terminology) than it has been to the syntactical simplicity of predicates. Scientists in many fields employ the results of this trend in analysis when they use computer programs to *fit* functions to data. So the analysis of the syntactical simplicity of formal expressions may be of interdisciplinary interest.

The analysis of syntactical complexity will involve several things. Bunge says that,

syntactical simplicity (economy of forms) depends on (1) the number and structure (e.g., the degree) of the specific primitive concepts (basic extralogical predicates); (2) the number and structure of independent postulates, and (3) the rules of statement transformation. ([10] pg.121)

Early in the twentieth century Dorothy Wrinch and Harold Jeffreys proposed a measure of the syntactical complexity/simplicity of mathematical expressions in a probability text book and a series of articles. If it is assumed that the connectives are all equally simple, then we might attempt to gauge the simplicity of an expression by counting the number of

connectives or the number of independent freely adjustable parameters. This approach does neglect the complexity of the atomic terms discussed above. Jeffreys wanted his simplicity measure to give the prior probability, given by 2^{-k} for physical theories. On the assumption that all physical laws are expressible in differential form, he suggests that we can analyze *families* of equations with the simplicity measure k where $k = \text{order} + \text{degree} + \text{the absolute value of coefficients}$. The order of a differential equation is the highest derivative. The degree of a differential equation is the power of the highest derivative term. Consider the Newtonian equation for position under gravitational acceleration,

$$s = a + vt + \frac{1}{2}gt^2. \quad (3.5)$$

Cleared of roots and fractions 3.3 is expressed in differential form as,

$$\frac{d^2s}{dt^2} = g. \quad (3.6)$$

In this case, Jeffreys' variable k is $2 + 1 + 1 = 4$. This is generalized to *families* of equations. The linear family of curves (LIN) is given by,

$$f(x) = \alpha_0 + \alpha_1x. \quad (3.7)$$

The parabolic family of curves (PAR) is given by,

$$f(x) = \alpha_0 + \alpha_1x + \alpha_2x^2. \quad (3.8)$$

The cubic family of curves (CUB) is given by,

$$f(x) = \alpha_0 + \alpha_1 b + \alpha_2 c^2 + \alpha_3 d^3. \quad (3.9)$$

On Jeffreys' proposal CUB would have simplicity $k = 5$ which is more complex than PAR with simplicity $k = 4$ and LIN with simplicity $k = 3$. If LIN expressed a proposition embedded in a physical theory, then LIN would have the higher prior probability.

The objections to this approach are well known. The first objection is the sub-family objection. If we think about the curves generated by functions as fitting some data then we could achieve syntactical simplicity in an *ad hoc* way. Since each of the functions with a smaller number of adjustable parameters is a sub-family of the families with more adjustable parameters there are infinitely many cases where CUB overlaps with PAR and PAR overlaps with LIN. These are cases where the variable with the highest exponent takes the value of 0. We could get an *ad hoc* gain in syntactical simplicity by fixing the values of a higher dimensional curve at the values which give the best fit (where "fit" involves not merely the fit with the original data but some hindsight judgment about what also would have given the best predictions). In this way the number of adjustable parameters would be reduced and the syntactical simplicity value would be maximized. Forster and Sober [24] suggest that a rule can be added to exclude overlapping cases. However, this solution depends upon an inductive inference that curves which fit data *too well* are too good to be *true*. Since we ultimately wish to investigate whether or not any simplicity criterion is justified, this solution may not be acceptable because it begs the question against the inductive skeptic.

Another objection raised by Hesse [31] is that merely changing the choice of coor-

dinate system will give contrary simplicity measures for equations that generate identical curves. Consider the equation for a circle in Cartesian Coordinates,

$$r^2 = x^2 + y^2. \quad (3.10)$$

A spurious gain in simplicity can come from selecting polar coordinates where the equation for a circle is,

$$r = a. \quad (3.11)$$

If equations (3.8) and (3.9) both express the same proposition, then a contradiction is derivable from Jeffrey's gauge (another example of objection strategy (2)).

Bunge objects that Wrinch and Jeffreys did not analyze predicate complexity and restricted their proposal to metrical predicates, and so the problems of predicate complexity remains on this approach. Also, it is not clear why order and degree are given equal weight. Again it is not clear what would justify the linear addition of the values of heterogenous characteristics. Perhaps we should be suspicious about a measure which makes a 1/2-order variable more simple than a first order variable. Finally, it is not the case that all physical laws are expressible in the form of differential equations ([11] pg.119-120) which is a fundamental assumption made by Jeffreys in his early work. So the applicability of this method is limited.

3.2.2 The Simplicity of Theories

Theories are complex wholes constructed out of postulates, hypotheses and propositions which are themselves constructed with predicates and are embedded in a logical system with transformation rules. At present, no formal gauge of the complexity of theories has been offered. Goodman [27] and Bunge [11] point out that it will do no good to begin counting postulates since all postulates could be turned into one postulate by conjunction.

It is difficult to know how to approach gauging the simplicity of a theory. Bunge says,

That not only the number and complexity of a theory's postulates but also the number and complexity of its transformation rules are relevant to its L-complexity is plain: after all, we are interested in the complexity of systems that, from a semiotic point of view, can be regarded as languages, and the formal complexity of a language is determined by the complexity of both its vocabulary and its grammar ([11] pg.122).¹²

We do not know how to gauge the simplicity of transformation rules themselves and the application of these to a system's basic postulates may give infinitely many results. What matters is the conceptual cohesiveness that the transformation rules (and definitions) contribute to. But, as of yet, there is no measure of conceptual cohesion.

3.2.3 Semantical Simplicity

Semantical simplicity is at least as important as syntactical complexity. There are various theories of meaning. Bunge appears to hold that it might be possible to count the

¹²'L-simplicity' stands for 'logical simplicity'. Bunge uses this term in his general discussion of syntactical simplicity

specifiers of meaning of the basic predicates of a theory. Roughly, the idea is that theories may involve several operational definitions and that we could count the number of basic (unoperationalized) terms. If this view about semantics is false, then the problems involved in selecting a gauge of semantical simplicity may be more serious than the problems Bunge outlined. Perhaps things would be easier if Bunge's view about meaning were correct because we might be able to count specifiers of meaning. Even in that case, Bunge shows how difficult it would be to select a non-arbitrary gauge of semantical simplicity.

Bunge says that,

Semantical simplicity (economy of presuppositions) depends on the number of specifiers of meaning of the basic predicates. Semantical simplicity is valued within limits because it facilitates both interpretation of signs and fresh starts.([10] pg.121) ¹³

The terms 'theory' and 'hypothesis' are of equal syntactical complexity, but 'theory' is semantically more complex than 'hypothesis' because the latter appears in the definiens of the former. The same can, perhaps, be said of 'electron' and 'mass'. However, on Newtonian mechanics 'force' and 'mass' are of equal semantical complexity because they are primitives in that system. Bunge investigates the proposal that we might measure the semantical simplicity S of a term t in a language L as the inverse of the term's extralogical specifiers of meaning M :

¹³I do not know exactly what Bunge means by "fresh starts" but on the basis of some of his other publications, it would appear that he has in mind something like the situations that lead to the construction of revolutionary scientific theories. This is because Bunge holds that scientific theories tend to become increasingly complex until they become untestable or disunified with the other accepted theories in the body of science. Bunge's view shares some features with Kuhn's [41]. To borrow Kuhn's terminology, normal science contributes to the increasing complexity of working theories until *crises* arise. [12]

$$S(t, L) = \frac{1}{M(t, L)}.$$

Bunge points out that several objections arise. In the Peano arithmetic ‘1’ and ‘successor’ are primitives and each would have a semantical complexity measure of 0. Since ‘2’ is the successor of ‘1’ it would have a semantical complexity of ‘1’. However, if we choose ‘0’ as primitive then ‘1’ has a complexity value of 1 and ‘2’ of 2 (objection strategy (2)). Additionally, it is counterintuitive that 10,000 would be 1000 times more complex than 10. This problem might be met by choosing a smoothing function: $\log M(t, L)$. The problem with this proposal is that the semantical complexity of ‘1’ would be 0. The corresponding simplicity value for ‘1’ would be indeterminate. There are infinitely many ways to formulate a gauge of semantical simplicity. It is sufficient for this objection to notice that many alternative smoothing functions are available like: $1 - e^{-M^2}$, $\log(1 + M)$ and $\tanh(M)$. Even if a formal measure of semantical simplicity may be, in principle, possible for formalized languages, it is impossible for natural languages. The analysis may have to encompass the whole of human culture in order to track down the meaning specifiers for terms like “idea”, and culture is a moving target. Finally, if the meaning specifiers for a formal system could be counted, we could not justify giving them all the same weight unless it could first be determined which were conceptually primitive[11].

3.2.4 The Semantical Simplicity of Expressions

Bunge shows us that there is a similar pattern of problems for gauging the semantical complexity of expressions (or propositions). If a measure of the complexity of the

terms of an expression were available, we might be tempted to give a simplicity gauge as the inverse sum of the predicates in the statement. However, in order to do this each predicate would have to be intensionally independent. And this is an unlikely situation for the theoretically embedded statements of science. Bunge suggests that it might be feasible to get a comparative gauge of the semantical simplicity of the expressions of a formal language by counting the number of presuppositions required by a statement. Bunge suggests that by this standard the statements “the Universe exists for all eternity” would be semantically simpler than the statement “the Universe has a beginning” because the latter requires suppositions about creation mechanisms. I suggest that even this example is subject to the same pattern of criticisms which Bunge has raised for the other gauges of simplicity. Cosmological arguments posit mechanisms (or *movers*) for the basic structure of the world regardless of its age. So even this example does not provide a clear case where the counting of suppositions will help to gauge semantical simplicity.

3.2.5 The Semantical Simplicity of Theories

Bunge claims that a gauge of the semantical simplicity of theories would be of great use for scientists. He cites General Relativity as an example of a theory which, although it has empirically equivalent competitors, is preferred for its semantical simplicity of postulates despite its syntactic and psychological complexities (a sub-class of pragmatic complexity). We do not yet know how to gauge the simplicity of terms or postulates, so it is not clear what more can be said here, but Bunge has urged semanticists to work on this project. Interestingly, modern work on semantics has gone a route which involves

ontological proliferation (often positing possible worlds and their inhabitants).

3.2.6 Epistemological Simplicity

Bunge defines epistemological simplicity saying that

epistemological simplicity (economy of transcendent terms) depends on closeness to sense-data. Epistemological simplicity is not desirable in and of itself, because it conflicts with logical simplicity and with depth. ¹⁴([10] pg.121)

The reverse of epistemological simplicity is epistemological complexity or *abstractness*¹⁵. Bunge raises a criticism of phenomenalist languages (to whatever extent these are possible). This criticism is also presented in detail by Cartwright (1983). Bunge says that phenomenalist languages achieve epistemological simplicity at the expense of syntactical complexity and a loss of insight (or at the cost of shallowness). It is not immediately clear why phenomenalist languages would cost insight. This is discussed in more detail in the next section in conjunction with the discussion of explanatory depth. As a matter of fact about the way that scientists actually construct theories, it is the case that they posit ways that the world is, and they posit fundamental mechanisms of change. They do not choose fundamental variables so that their referents (or meaning specifiers) are mere sensations[11].

On the other hand, scientists do not seek epistemological complexity either. They seek to limit the number of transempirical terms to which a theory is committed. Somehow

¹⁴The term “logical simplicity” covers both syntactical and semantical simplicity in this case.

¹⁵It might not matter much if one is a rationalist or a skeptic about sense-data. We can follow Bunge’s project and then attempt to apply it to whatever it is thought might be the atoms of knowledge. Bunge himself is skeptical about sense-data. He mentions this in *The Myth of Simplicity* (1963)

there must be a trade-off between epistemological simplicity and abstractness. Scientific theories do posit *kinds of things* and *mechanisms of causation* and, as we shall see in the next section, this is because such posits contribute to the cohesiveness of science, and to the serendipic power of theories (the power to make novel predictions).

Epistemological simplifications may come about due to judgments not based on the *distance* away from sense experience. If the postulates involved in a theory are not scrutable, then we will endorse an epistemological simplification. Bunge says that

Unnecessarily complicated assumptions and theories should be avoided; that is, hypotheses and theoretical systems employing inscrutable predicates, such as ‘Providence’ and ‘collective unconscious,’ should be shaved with Occam’s razor. Notice, however, that Occam’s razor does not hang in the air, but falls under the more general rule, “Do not propose ungrounded and untestable hypotheses([11] pg.129).”

This, perhaps, suggests conceptually reducing epistemological simplicity to syntactical simplicity with certain metascientific constraints. That is, we would not add to a theory basic predicates if they conflict with the other desiderata of science, like scrutability and depth. The gauging problem arises due to a pattern of objections which show that, in some cases, we do not know what to count, that incompatible measures arise for any criterion and for any gauge by changing one fundamental assumption, that we do not know how to rank the values of a heterogeneous set of objects (objection strategy (1)) or that the formal gauge is arbitrarily selected from many other possible gauges, some of which give contrary measures (objection strategy (2)). Concerning epistemological simplicity, it may turn out to be the case that what might appear to be a simplifying application of Ockham’s Razor is better understood as a judgment based on scientific principles with epistemological relevance. This suggests that perhaps epistemological simplicity can be gauged by the

inverse proportionality of the count of inscrutable predicates in an expression, a hypothesis or a theory. I do not know if any inscrutable predicates ought to be included in a scientific theory. However, scientists do posit causal mechanisms, and Humeans will view the causal necessity involved in these as inscrutable. We might also be suspicious about the scrutability of the posits made by possible world semanticists.

3.2.7 Pragmatical Simplicity

Bunge analyzes pragmatical simplicity into five kinds,

1. Psychological simplicity (intelligibility).
2. Notational simplicity (economy and suggestive power of symbols).
3. Algorithmic simplicity (ease of computation).
4. Experimental simplicity (feasibility of design and interpretation of empirical tests).
5. Technical simplicity (ease of application to practical problems).([10] pg.121-122 and [11] pg.130)

3.2.7.1 Psychological Simplicity

Bunge suggests that psychological simplicity could be measured relative to individuals by the amount of time it takes to learn something, master a skill, or to solve a problem. It also could be measured as the inverse proportion of the number of unsuccessful trials. Perhaps cognitive science will be able to add something to these proposals in the future.

Psychological simplicity is certainly a desirable feature of *scientific practice* although it is not clear how it could be an intrinsic feature of *systems*. Science involves

modeling and modeling involves simplifying assumptions. Recall the first chapter discussion of the orbital calculations made by LeVerrier and Newcomb. [50] Certain assumptions are made in order to begin a systematic inquiry which may be dropped later in the development of the theory. For instance, it would not be uncommon to assume that orbital bodies move free from friction, but the Zodiacal Light Hypothesis may have suggested that such a simplifying assumption would inhibit a high degree of precision in the calculations. There is no general solution to the three-bodied orbital problem in Newtonian Mechanics. Yet, by introducing certain simplifying assumptions we can model the behavior of the solar system with Newtonian Mechanics using the methods of discrete integration. Modeling planetary interactions involves more steps for increasing degrees of accuracy.

Psychological simplicity is also of pedagogical interest. We explain things to others by appealing first to ideas or concepts which are easy to understand and then we aggregate them to bring the student to a new level of understanding. This depends, at least partly, on social and cultural conditions. In vulgar terms, the conscientious instructor is always looking for ways to *connect* with students. What counts as intuitive may shift from year to year and from class to class. Bunge says that,

Psychological simplicity is desirable for both practical (e.g., didactic) and heuristic purposes: if preliminary theoretical models are to be set up, details must be brushed aside, and easily understandable notions (“intuitive” ideas) must be seized upon. The obvious must be tried first, if only to dispose of it early: this is a well-known rule of intellectual work. However, it should be borne in mind that (1) psychological simplicity is culturally and educationally conditioned; i.e., is not an intrinsic property of sign systems; (2) the deliberate neglect of a given factor should always be justified; (3) we must be prepared to sacrifice psychological economy to depth and accuracy whenever the former becomes insufficient—for, whatever science is, it certainly is not a business whose concern is to save experience with the minimum expenditure of work.([11] pg.130)

3.2.7.2 Notational Simplicity

Bunge says that

Notational simplicity, or economy and suggestive power of signs. Thus, vector and tensor representations are notationally simpler than the corresponding “analytica” (expanded) mode of writing, and the symbol ‘ $P(h/e)$ ’ for the probability of the hypothesis h on the evidence e is suggestive, hence easy to retain. Needless to say, compactness facilitates interpretation and favors retention, and well-chosen symbols may be heuristically valuable. ([11] pg.131)

In constructing systems, theorists try to select symbols which are suggestive and therefore easy to remember and they seek a notational basis which is easy to manipulate. On the other hand nominal definitions may hide semantical or syntactical complexities. Just consider the loss of meaning involved in the expediciencies of the current fad known as *text messaging*.

3.2.7.3 Algorithmic Simplicity

Bunge says that,

Algorithmic simplicity, or ease of computation, is something like the reciprocal of the number of steps in logical or mathematical calculation (this number is actually computed in the programming of machines). Algorithmic simplicity depends on logical, semantical, and psychological simplicity, though not in a simple way. Thus the relation ‘ $y < x$ ’ is S-simpler than ‘ $y = x$ ’ in the sense that the equality relation is defined in terms of the two inequality relations. But the corresponding point set $\hat{x}\hat{y}(y < x)$ is algorithmically more complex than $\hat{x}\hat{y}(y = x)$, because finding the frontier of the former set presupposes the determining of the latter set. ([11] pg.131)

In some cases algorithmic simplicity is achieved by a syntactical or semantical complication. For example, some integration techniques involve substituting more complex terms for single variables. Bunge also points out that some theories may have a very sim-

ple notational basis but involve algorithmic complexities. This is acceptable to science when the theory gives an improvement in depth and accuracy.

3.2.7.4 Experimental Simplicity

Bunge says that experimental simplicity is “simplicity in the design, performance, and interpretation of observations or experiments.” He says that experimentalists prefer “hypotheses having the operationally simplest consequents, however complex they may be in all remaining aspects. ([11] pg.131-132)”

3.2.7.5 Technical Simplicity

Bunge uses the term “technical simplicity” to refer to the relative ease of application of theories or practices when the goals are non-cognitive. Time or financial constraints may influence the judgment to put rough or simple theories into practice. Following Bunge’s pattern of analysis, the suggestion might be made that technical simplicity be measured by the relative amount of time or resources saved by selecting one theory or process over another. As long as science is conducted in roughly the same inertial reference frame, then the time intervals between the introduction of a theory or process and its successful application might provide a basis for gauging technical simplicity. It seems natural that the economic notion of *opportunity cost* might give an economic gauge of technical simplicity. However, until the final theory of economics is given, I cannot see how to gauge technical simplicity in purely economic terms. My guess is that scientists and engineers regularly make judgments of technical simplicity on the basis of algorithmic simplicity when computing is involved. This is because an economic value can be given to floating point operations

per second. Scientists may need to run very complex computational models on mainframe computers, and so they must *rent* FLOPS.

3.2.8 Why Categorize Simplicity?

Bunge has given us a way to organize the simplicity in science dialectic. Many different kinds of simplicity criteria are relevant to science. Simplicity criteria in science are heterogeneous because gains in some kinds of simplicity come at the expense of other kinds of simplicity. Given the extreme heterogeneity of simplicity, no *overall measure* is possible. We do not even know how to gauge some species of simplicity. We have only rough sketches of how to measure simplicity of other types.

One reason that Bunge's analysis is useful is because it lays the ground work for seeing how it is that the various species of simplicity are relevant to science. This is something that we should know better when we ask questions about the justification of specific measures of simplicity which are already at work in science.

Clearly, a great deal of work is still needed even to get basic proposals of formal gauges of simplicity of some kinds. Formal gauges of simplicity are of great use in science, both in the early stages of theory development and in error analysis. We also want to know which kinds of simplicity judgments are irrelevant to science.

At the very least Bunge's analysis should put to rest some of the naïve dogmatisms mentioned in the previous chapter which threaten to stop the philosophy of simplicity before it gets started. Ockamists should be able to see that principles of parsimony involving criteria that select ontological objects can be formulated with some clarity and that these

do not play the only role in scientific methodology. Additionally, the Ockhamist may lose some of the confidence which usually accompanies dogmatism when it is noticed that there are serious problems justifying the selection of criteria which select ontological objects and additional problems justifying gauge selection. The naïve pragmatist should be able to see that it is not true that there are no philosophical problems of simplicity in science. It is not the case that all of the kinds of simplicity at work in science reflect merely pragmatic or aesthetic values. In so far as the final justification for desiring science may turn out to be pragmatic, it may also be the case that only pragmatic reasons are available for the *justification* of syntactic or semantic judgments of simplicity in science. But that would not mean that a particular species of simplicity itself is of the pragmatic kind.

3.3 Metascientific Criteria and Their Relations

Mario Bunge's 1961 article "The Weight of Simplicity in the Construction and Assaying of Scientific Theories" yields several results. [11] The general goal is to show that it is misguided to think that simplicity judgments are the final court of appeal in theory construction and selection. On Bunge's view, there are several ways that such a notion might be misguided: 1) Simplicity is not one concept, but many and the extreme heterogeneity of the simplicity criteria employed in science rules out the possibility of any *overall* measure. 2) Simplicity is at best a very weak criterion of theory construction and selection because in many cases where it conflicts with the other desiderata of science, simplicity is trumped. 3) Simplicity is not relevant to many of the desiderata of science and in some cases, due to the fact that different species of simplicity generate competing measures, the relevance to

some of the desiderata of science is ambiguous.

It is not clear how to evaluate Bunge's argument that simplicity criteria are, at best, only weak criteria of theory construction and selection. It may be the case, as Bunge has argued, that simplicity criteria are weak relative to the other desiderata of science. But if any simplicity criterion turned out to be indispensable for science, then we might, by another standard, say that such a criterion is extremely important. Alternatively, even if simplicity criteria in science were justified on purely pragmatic grounds, then their analysis would still be valuable at least for pragmatic reasons. What is of interest is that Bunge has given arguments about the relations between various simplicity criteria and the other desiderata of science. I wish to advance Bunge's project by focusing on developing the analysis of these relations. This project is ambitious enough as it is, so I will omit any discussion of Bunge's arguments regarding the irrelevance of simplicity criteria to the desiderata of science.

To the best of my knowledge, no one has contributed to Bunge's project to give a *comprehensive* analysis to the interrelations between the desiderata of science. Others have focused on bits and pieces of these issues, but never the whole thing. It is natural to focus on bits and pieces of the puzzle because the numbers of interrelations between the desiderata of science get unmanageable after only a few arguments, and each of these issues is deserving of focused discussions. Bunge gives twenty arguments about the interrelations between the desiderata of science. As stated above, my work is not comprehensive either. I synthesize as many of Bunge's arguments as I can into the beginning of a holistic picture of the interrelations between the desiderata of science. Because there are so many features of

this analysis, I have used a conceptual map to organize and present the results of Bunge's arguments.

In order to set up the conceptual map, it must be determined what are the key terms of the discourse. This is very difficult. The reason is that Bunge uses technical terms in a very specific way, although it is not the purpose of his analysis to advance detailed theories about the uses of all of those terms. So, in order to elucidate the desiderata of science it is necessary to consult a few other sources. But it is often the case that the terms are used differently by other authors or there are arguments given that we should conceive of the desiderata of science differently. For example, Bunge uses the term "refutability", and it is not clear in the 1961 and 1962 articles that this term ought to be used as a synonym for "falsifiability" which means only that it must be, in principle, possible to refute a hypothesis.¹⁶ Also, Bunge discusses explanatory *depth* but does not advance a theory of it. Theories of scientific explanation have recently involved discussions of depth and this can be seen in the work of authors such as Michael Strevens, Christopher Hitchcock and James Woodward[57][32]. Extensive work has been done on this topic and there are many different theories of scientific explanation. Not all of them share the same gauges of depth. Still, this recent work builds on a tradition arising from the work done by Hempel and Nagel and we can cull from it some instructive points which contribute to augmenting Bunge's unique project.

The conceptual map program generates a graphic rendering of the metascientific

¹⁶It appears that 'refutability' is the same as 'falsifiability' in Bunge's use. See *The Myth of Simplicity* (pg.91)

criteria and their relations. As the arguments are presented the conceptual map will be updated accordingly. The various simplicity criteria are represented by the image in the figure 3.2.

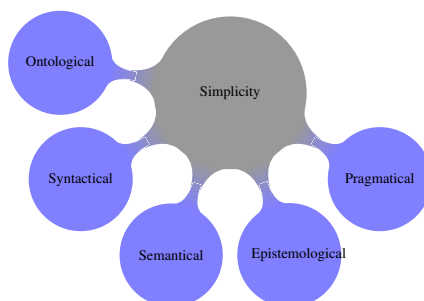


Figure 3.2: Simplicity

3.3.1 Systematicity

Goodman and Bunge have argued that *systematicity* is, for various reasons, a desirable aim for science. Roughly put, systematicity is the connectedness between the basic terms or ideas of an interpreted formal theory. We can think of systematicity as a gauge of how comprehensively a system expresses the interrelations between its fundamental terms or ideas. These are rough characterizations of systematicity because there are different ways to achieve it.

Goodman opens his 1943 paper “On the Simplicity of Ideas” by saying that “The motives for seeking economy in the basis of a system are much the same as the motives for constructing the system itself.”([27] pg. 107)

We wish to have systems that adequately express their subject matter. Bunge included a series of diagrams in his discussion of conceptual connectedness, and it may help

to start with Bunge's discussions about one way to achieve systematicity. However, Bunge does not investigate theories of concepts. Bunge himself has said that no measure of conceptual connectedness is presently known and this may account for why his diagrams are left open to interpretation. Possibly, conceptual connectedness is expressed by the interdefinability of terms, or by partial interpretations. However since these are expressible in formal languages I will save these topics for the discussion of the relation between syntactical and semantical simplicity criteria and systematicity. A further discussion of the definitional structure of terms appears in the subsection on explanatory depth. I can speculate a little about what might be unique about conceptual connections. On one view, concepts can be complex wholes with primitive concepts as their proper parts. On this view, the part-whole relation would account for conceptual connectedness. On another view, concepts are structured just in case they stand in a privileged relationship to other concepts by way of some inferential disposition. On a third view, that concepts could be related is by a subject knowing that two concepts have the same referent but different modes of presentation¹⁷. On a linguistic analysis, this appears as the identification of coreferential terms. I reproduce Bunge's series of diagrams in the following.

If, for heuristic purposes, we assume an axiomatic account of theories of the sort which Bunge appears to embrace in the 1960's, an example of an unsystematic set of mutually independent postulates C_1 through C_6 would be,

$$\dots C_1 \dots, \dots C_2 \dots, \dots, \dots C_6 \dots \quad (3.12)$$

¹⁷See [45] for a detailed discussion of concepts.

in which none of the basic predicates C_1 through C_6 appear in more than one axiom. Let the symbol “—” stand for a conceptual connection of any preferred flavor. A more organized system would be,

$$C_1 - C_2, C_3 - C_4, C_5 - C_6. \quad (3.13)$$

A yet more systematic set would be,

$$C_1 - C_2, C_2 - C_3, C_3 - C_4, C_4 - C_5, C_5 - C_6, \quad (3.14)$$

which is equivalent to,

$$C_1 - C_2 - C_3 - C_4 - C_5 - C_6, \quad (3.15)$$

So one sufficient condition for systematicity is conceptual connectedness. Let the symbol \longrightarrow depict the *sufficient for* relation and update the conceptual map in figure 3.3.

A common view is that concepts have a definitional structure. Perhaps then the definitional power of a term tracks the structure of concepts. This is something which can be analyzed by focusing on linguistic symbols. In the early 1940s Nelson Goodman launched an inquiry into the linguistic structures that contribute to systematicity. Goodman points out that if we can reduce the number of primitives in a system then the system more comprehensively expresses the relationships between the elements of the subject matter. But Goodman is also correct to point out that it will not be satisfactory to seek theoretical simplicity by merely reducing basic predicates. A rough version of Goodman’s argument is that we could get a spurious gain in this sort of *syntactical simplicity* in (3.11) by defining

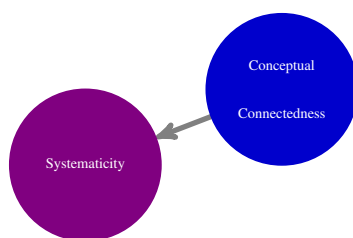
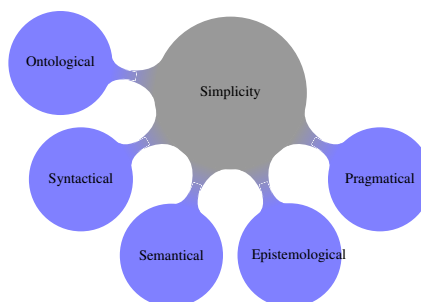


Figure 3.3: Conceptual Connectedness

$C_1 \& C_3$ as one predicate D_1 . This is the sort of argument that we might expect from a nominalist, but even Goodman did not think that we could define terms in any old way we like. The same point can also be made in a different way. Bunge says that the gain in systematicity from (3.10) to (3.13) has nothing to do with the number of basic postulates. All of the sets have the same basic postulates. Conceptual connectedness is necessary for having a system and merely reducing the number of extralogical bases of a system is sufficient systematicity. Conceptual connectedness is sufficient for systematicity and and so is *syntactical simplicity*. However, neither is necessary for systematicity.

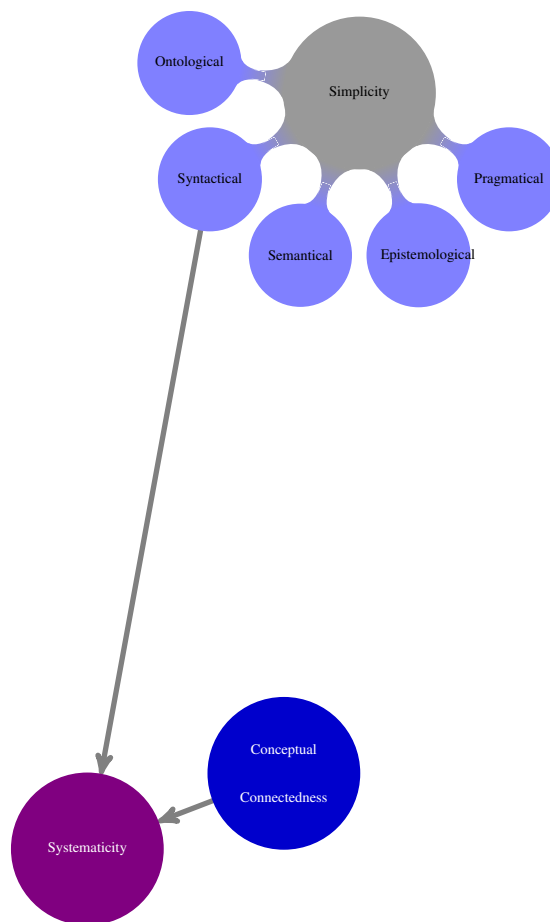


Figure 3.4: Systematicity

Although *syntactical simplicity* is sufficient for systematicity, what we wish to know is whether or not the extralogical bases really are conceptually basic. Because this is problematic, it might be tempting to say that since conceptual connectedness is sufficient for systematicity, we can avoid questions about syntactical simplicity altogether. This is not so easily put into practice. As stated in the previous section, no measure of conceptual connectedness is presently known. Also in the early stages of the development of a theory it may not be clear what the fundamental concepts are, and we also have reasons to seek the exactness of logical or mathematical statements. So pragmatic constraints may make

syntactical simplicity difficult to dispose of in actual practice.

The kind of simplicity that is relevant to systematicity does not depend merely upon the simplicity of the signs used to denote properties or classes of objects. The earlier discussion of syntactical simplicity showed that equally simple signs may denote things which are structured differently even if they pick out the same number of individuals. Scientific theories do not consist merely of formal or mathematical expressions. The signs of the formal theory are given an interpretation. Goodman suggests that we can think about one type of simplicity that is relevant to systematicity by thinking about how the signs of a formal system have their denotations specified by the theory's physical interpretation. Goodman suggests that we might think of the defining power of an idea, not the syntactical structure of a theory, as the basis upon which to gauge the kind of simplicity that is relevant to systematicity. Since predicates might hide the structure of concepts, we can see why authors like Goodman and Bunge, with their views about semantical simplicity, thought that the defining power of terms would be relevant to systematicity. Goodman says that one natural approach might be to try to gauge simplicity in the following way:

If a given idea of basis A were definable solely in terms of B and logic, while B were not definable solely in terms of A and logic, it would be clear that B is the more powerful idea or basis, A the weaker or simpler. ([27] pg.110)

Goodman says that this approach is subject to two objections. One objection is that the approach will not work for cases where neither A nor B are definable in terms of one another. The other objection is that this approach gives counter intuitive results. For example, from the system K consisting of A and B we can get C , D , and E ,

$$C =_{df} (A \cup B), (A \cap B)$$

$$D =_{df} (A \cap \bar{B}), (\bar{A} \cap B)$$

$$E =_{df} (\bar{A} \cup B), (A \cup \bar{B}).$$

Then we can create system K' consisting of C , D , and E . The counterintuitive result is that C , D , and E are definable in terms of A and B but we cannot define A and B in terms of K' . This makes K' weaker than K . Since K' is more complex than K , weakness is not the same as simplicity in the sense relevant to systematicity. What is relevant to systematicity is not weakness but economy.

Goodman points out that in response to the first objection we might try to establish the rule,

if a basis A is definable solely from logic and some basis that is in a specified sense “of the same kind” as B , while B is not definable solely from logic and some basis that is in that sense “of the same kind” as A , then A is weaker than B ([27] pg.110).

But Goodman explains that this solution would decide what are “kinds” independently of defining power, and if we can do this then we do not require this sort of approach to measuring simplicity. After all, if we had some independent way of deciding the questions of ontology, then that would be sufficient for deciding which terms of an interpreted formal theory are basic. Syntactical simplicity is sufficient, but not necessary for systematicity. If we had an independent way to settle the questions of ontology or of the structure of concepts, we would not require syntactical simplicity as a guide to the early stages of theory construction.

3.3.2 Linguistic Exactness

Goodman's discussion illuminates an interesting, and perhaps problematic feature of simplicity in theory construction. When we judge simplicity by comparing the defining power of basic postulates, weakness is inversely proportional to the kind of simplicity that we seek. I will follow Bunge and call one of the desiderata of science *linguistic exactness*. Linguistic exactness is inversely proportional to syntactical simplicity. As we accumulate more and more data, we could always introduce ad hoc data-specific terms to our theories. But we seek economy rather than mere fit with the data. The addition of terms to the postulate basis may involve semantical complexities as well. Goodman and Bunge sought to find what places a constraint on syntactical complexities by analyzing the defining power of terms. Semantical simplicity is shot through with its own problems, and perhaps semantics is not properly analyzed by way of the defining power of terms. What we know is that linguistic exactness is inversely proportional to syntactical simplicity. We do not yet know how, and in how many ways syntactical complexities are constrained. The conceptual map can be updated with the symbol \longleftrightarrow depicting the inverse proportionality relation between syntactical simplicity and linguistic exactness. Let the symbol $\dots \rightarrow$ depict the *would be sufficient for* relation, and this is added between ontological simplicity and syntactical simplicity since Goodman has reminded us that if we could settle matters of ontology then this would settle independently questions of syntactical simplicity.

Goodman published his first proposal about how to gauge the relative complexity of systems in 1943. Over the next decade Goodman published several different proposals. Some of them involve a set-theoretical measure, others a measure of the predicate length

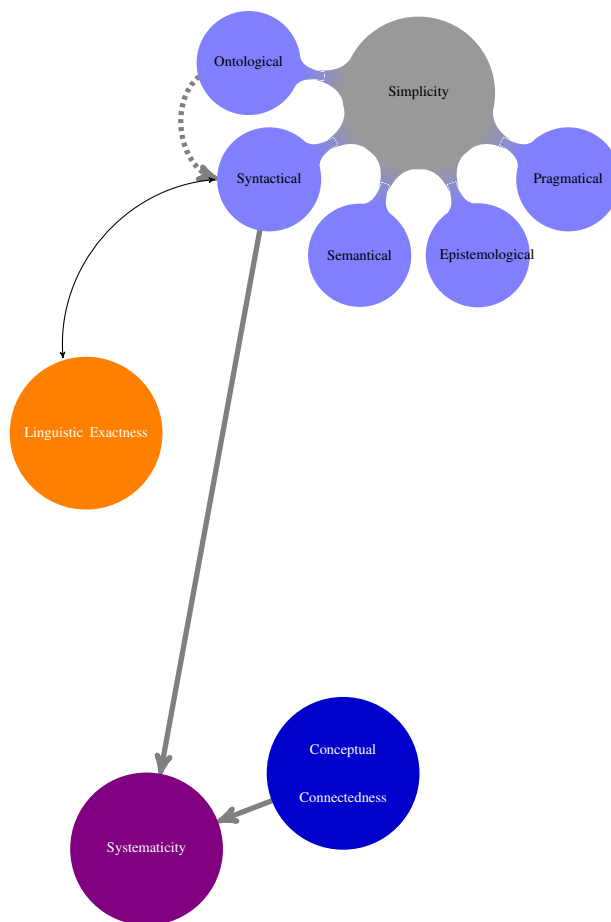


Figure 3.5: Linguistic Exactness

and others involve an axiomatic measurement. Each approach, if successful, would be sufficient for gauging syntactical simplicity in the way relevant to systematicity and since there are many approaches, no one is necessary.

The diagram should help to illustrate one interesting puzzle which scientists are faced with. Both linguistic exactness and systematicity are desirable features of scientific theories, only there is a trade-off between them when systematicity is achieved by way of syntactical simplicity. For this reason Goodman is correct to say that, “it is quite wrong to think of the search for economy as a sort of game, inspired by an abnormal love of

superficial neatness.” ([27] pg.107)

This point, yet again, underscores the importance of rejecting half-baked resistance to a philosophy of simplicity on the grounds that the role of simplicity in science is reducible only to aesthetic values. Pragmatic constraints clearly play a role in theory construction. However, it would be quite wrong to think syntactical simplicity criteria are themselves pragmatic criteria. This would be to conflate the objects specified by the criteria with the justification for defining the criteria.

3.3.3 Testability and Accuracy

Both *systematicity* and *linguistic exactness* are desirable features of scientific theories. Systematicity is desirable for the reason which has already been stated: we wish to have scientific theories which comprehensively express the interrelations comprising their subject matter. Systematicity is also an aim of scientific theory construction because it is related to testability and accuracy. Mario Bunge says that,

A dough of vague assumptions all standing on the same logical level, without strong logical relations of deducibility occurring in its body, cannot be tested the way genuine theories are: since all of the propositions of the pseudotheory are loosely related to one another, every one of them will face separately the trials of logic and/or experience. How could we test the axioms of a factual theory if we cannot spot their logical consequences? A chaotic mass of conjectures lacking logical organization – as is the case with psychoanalysis – cannot be subjected to the test of experience as a whole: experience may at most confirm some of the loosely related conjectures of the pseudotheory, but no evidence will ever conclusively refute the whole set of vaguely stated *ad hoc* hypotheses-especially if they are mutually shielding. And a theory which stands no matter what experience may say, is not an empirical theory.([10] pg.125)

Recall an example in the first chapter where an insane neighbor cannot explain broken glass and a missing sound system, but who thinks that the footprints between the

broken glass and the place where the electronics once were are confirmation for his alien invasion hypothesis. This pseudoscientific theory fails in two ways. It is not systematic in that it does not comprehensively express the relationships between the data. It also fails because it is not falsifiable. Conspiracy theories are like this also. They often consist of vague or mutually shielding hypotheses so that they may be taken to explain any data that the theorist wishes them to explain. Systematicity and linguistic exactness are *necessary for* testability. Now, I suppose that mutually shielding hypotheses do have a kind of relatedness. However, we will not allow the theories of the fortuneteller or the conspiracy theorist to be scientific. Perhaps, then falsifiability provides a necessary constraint on conceptual connectedness. Let the symbol \longrightarrow depict the *necessary for* relation and update the conceptual map with these relations.

As it turns out, the set of relations depicted is not yet precise enough to capture the ways in which the desiderata of science are related. Testability has several components. Theories must be *scrutable*, they must have *explanatory power*, they must *fit* the data, and they must make *predictions*.

To say that scientific theories must be scrutable means that they must be able to be investigated by the public methods of science. This does not mean that all of the terms of a scientific theory must be observable. Rarely is this the case. All it means is that exact connections must be established between the terms of the theory and observable predicates. Bunge says that for this reason terms like 'élan vital' are not suitable for scientific theories. ([10] pg.126) What Bunge has shown is that systematicity is necessary for theories to be scrutable. Additionally, scrutability is, in many cases, inversely proportional to syntactical

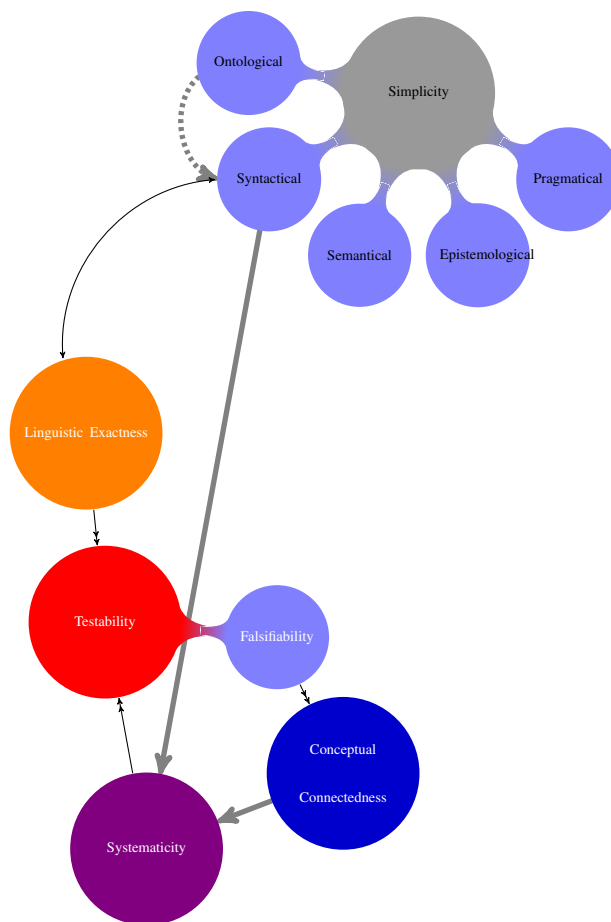


Figure 3.6: Testability

simplicity. Bunge says that,

A theory containing a large number of scrutable predicates will be preferable to another theory containing fewer predicates but all or part of them inscrutable, if only because the former theory will be testable, unlike the latter. The methodological status of the predicate basis is far more important than its logical structure and number. Thus, ‘electrically charged’ is both syntactically and semantically more complex than ‘providential,’ yet it is scrutable and may consequently occur in scientific theory, whereas the latter cannot ([10] pg.126).

The conceptual map is now updated to show that the relevant feature of testability which systematicity and linguistic exactness are necessary for is *scrutability* and to show that scrutability *can be*, under certain circumstances, inversely proportional to *syntactical*

simplicity, that is when the predicate basis is complicated by inscrutable predicates. Let the symbol $\leftarrow - - \rightarrow$ depict what *can be* an inversely proportional relation.

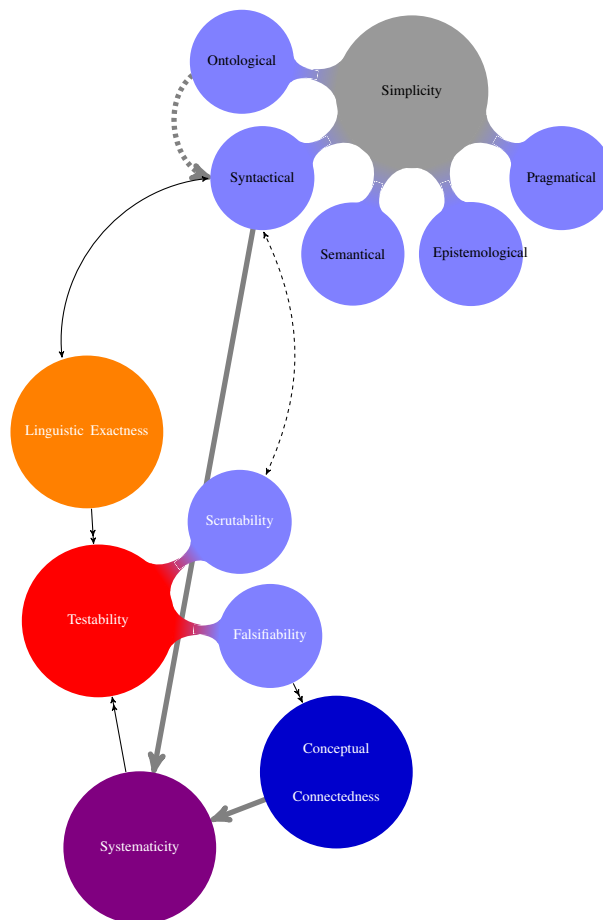


Figure 3.7: Scrutability

3.3.4 Accuracy

When we think about how well a theory fits data and how well it makes predictions we must think about *accuracy*, which is another desideratum of science. This is because accuracy is intimately related with *fit* and *predictive power*. Recall the first chapter discussion of Sherlock Holmes. It was convenient that Holmes omitted certain specifics about

the properties of the keys and coins which were hypothesized to be the causes of some of the watch scratches. Under similar circumstances such specifics might have turned out to be relevant to the truth or falsity of the hypothesis, and such syntactical complexities would also have increased the chances that some of his hypotheses about the history of the watch would turn out to be false. In other words, Holmes could have given more hypotheses or more specific hypotheses but he didn't and this worked out in his favor. The more specific hypotheses are, the more likely they are to be falsified. This shows that accuracy demands linguistic exactness. It also shows that there is an inverse proportion between *fit* and *predictive power* and accuracy. Bunge says that,

The more exact a statement is, the easier it will be to dispose of it; vagueness and ambiguity – the secret of the success of fortune-tellers and politicians – are the best protections against refutation. Now, accuracy demands complexity, both formal and semantic: suffice to compare the simplicity of presystematic, ordinary, discourse with the complexity of scientific discourse; compare 'small' with 'of the order of one atomic diameter', and ' $x > a$ ' with ' $x = a$ '. ([10] pg.126)

We wish to have scientific theories which are, in principle, refutable. In this respect syntactical simplicity is can be both favorable and unfavorable to science. There is some trade-off between syntactical simplicity and testability. How much of a trade-off is allowed depends upon how the other metascientific criteria are satisfied.

The same logical relation holds between accuracy and the relevant features of testability: *fit* and *predictive power*. In this case the only difference between these two features of testability is one of temporal order; either the hypothesis is given after the data are collected or more data are collected after the hypothesis is given. From one perspective we say that a hypothesis fits the data given certain accuracy constraints or we say that it does not fit

the data for some accuracy constraints. From the other perspective we might call a datum confirming if it falls within the accuracy constraints for a specific hypothesis and disconfirming if it falls outside of the accuracy constraints for that hypothesis. For this reason, *fit* and *predictive power* are combined in the conceptual map. Additionally, linguistic exactness is necessary but not sufficient for accuracy. Other factors which contribute to accuracy have to do with what sort of experimental equipment is used, the skill of the experimenter, and the experimental controls involved in a particular experiment. The conceptual map is now updated to show the inverse proportionality relation between *fit/predictive power* and accuracy, and the *necessary for* relation between linguistic exactness and accuracy.

3.3.5 Depth

Despite the importance of elucidating explanatory depth, the discourse still occupies a very focused corner of philosophy. There is at present no *Stanford Encyclopedia of Philosophy* entry on depth and the arguments involving explanatory depth are strewn throughout the literature on scientific explanation and confirmation. Bunge claims that scientists will not make simplifications at the expense of explanatory depth, so it is important to know a bit more about this explanatory virtue.

Depth comes in degrees. Some explanations are deeper than others. Brad Westlake ([64]) credits Hempel with having noticed that explanations may be ranked in order of the range of phenomena that can be explained by the laws involved in them. In some kinds of cases a law which figures in more kinds of explanations than another which explains some of the same phenomena will be the deeper. By this standard Newton's law of gravita-

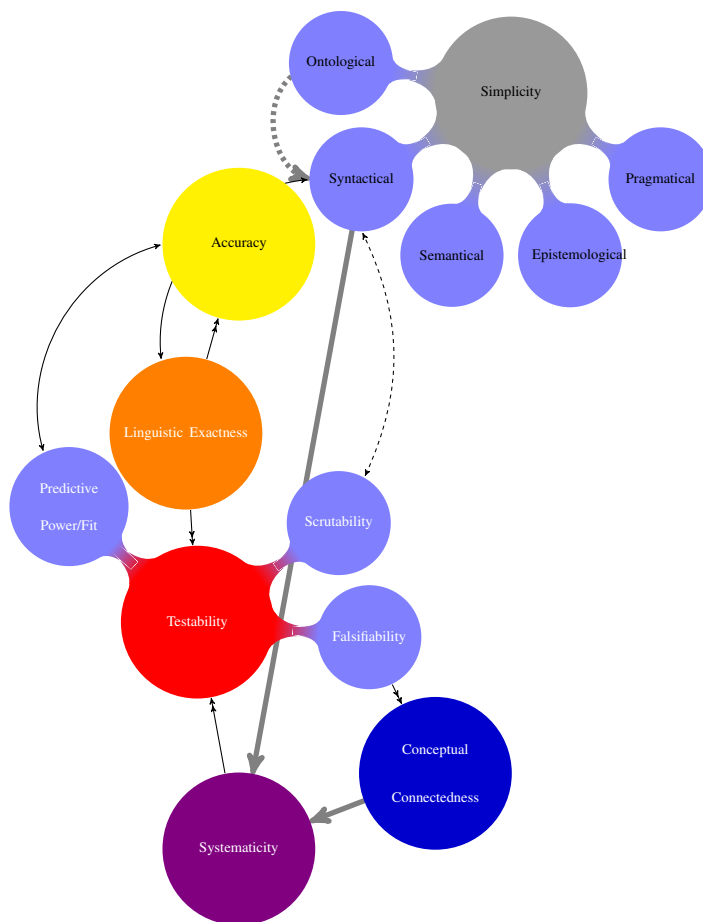


Figure 3.8: Accuracy

tion is deeper than Kepler's laws of motion because Newtonian mechanics applies to both terrestrial and celestial motions. Newtonian mechanics is also thought to be deeper than Galileo's Laws of motion. In one way, this might appear to conform to the same standard because although Galileo was interested in unifying the terrestrial and celestial laws of motion, Newton's mechanics also explains tidal action. However, the reason that Newtonian mechanics explains tidal action has to do with Newton's development of calculus and his treatment of the whole mass of the Earth and the whole mass of the moon as collections of point masses. In this case, the increase in the generality of Newtonian mechanics over

Galilean mechanics has to do with an ontological reduction of wholes to their constituent parts and their relations. So, generality of some sort is a symptom of depth. However, generality can be increased in a number of different ways and what it is that contributes to depth, and thereby gives some increase in generality of some kind depends upon several things including specific notions of scientific explanation and reduction. I wish here only to distinguish several kinds of generality so that some of the symptoms of depth can be identified.

Generality is sometimes referred to as *scope*. A law statement has its scope defined by the number of kinds of properties or entities to which it can be applied. There are several ways to achieve generality in scientific explanations.

Level Generality: a statement expressing a law at the genus level may be expected to have a greater generality than a statement expressing a law at the species level. Laws about the behaviors of the genus of critters *Solenopsis* apply to more kinds of entities than do statements expressing laws about Southern fire ant behaviors only.

Reductive Generality: reductive generality may be achieved by explanations which involve properties had in common by many individuals. Explanations of this kind may also take a form where a part had by particular individuals appears in the postulate basis of a theory because it explains the behavior of a whole class of individuals. This is due to the fact that parts have attributes and structural properties. For example, law statements involving the term “chromosome” makes appeal to a part of particular living organisms in order to explain things about the whole organism. A class of entities have these parts and so this sort of law applies to all entities of this class. Such laws are more general than Mendel’s

Laws involving appeal to *gametes* which Mendel knew to give only species-specific laws.

Unity of Science Generality: Generality of this kind might be analyzable in terms of level generality or reductive generality. However, it should be discussed as a distinct type of generality for three reasons. 1. The unity of science may actually be the primary goal for scientists when they construct some theories. 2. The kind of generality which is relevant to explanatory depth is often relative to a specific account of scientific explanation. 3. Accounts of reduction in science are various, and as we shall see in the next chapter, the word “reduced” has been used in different ways to describe the relationship between two sets of laws, theories, or concepts. The suggestion was made in the previous chapter that LeVerrier and Newcomb were concerned with the unity of science in their investigations of the range of application for Newton’s Laws of motion. Newton’s laws are expressible in terms of momentum which is had by both terrestrial and celestial bodies indicating that Newton’s laws are deeper than the Keplerian laws of motion.

Holistic Generality: If there are laws relating multiply realizable properties then these laws would also achieve a kind of generality. If the color red is thought to be a property of the mind-independent world then it is weakly emergent because it would be realized in many different ways: by emission, absorption, reflection, and transmission. If there are properties that cannot be token-token identified with their compositional properties, then these would defy any kind of reduction. If there are laws relating such properties, then nature would be disunified.

It was stated above that ontological simplicity of some kind may be sufficient for syntactical simplicity. Consider how this might work. It is possible to satisfy several of

the metascientific criteria at once by doing a bit of ontology when we construct theories. Positing, let's say, a causal mechanism will solve some of the problems of systematization because it would establish necessary connections between the terms of the theory. It would also contribute to generality if the mechanism relates several different kinds of properties or if it may be used in many different explanations. Additionally, if the causal mechanism posited by a law represents the way that the world really is (if it is a true posit) then it is natural to expect that the law will exemplify other desirable features: it will have serendipic power and it will support directional counter-factuals. "Serendipic power" is Bunge's term for the power of a theory to make novel predictions and what distinguishes a law from an accidentally true generalization is that a law supports counterfactuals. A causal mechanism posited by a theory will support directional counter-factuals linguistically even if it does not represent a way that the world is, but unless it is true, it cannot be expected to give true predictions.

As a desideratum of science, explanatory depth is not conceptually bound to causation. There are other kinds of explanations. Identities, for example, may be explanatory and they may contribute to depth by making possible the formulation of laws with greater scope. Also, the careful construction of operational definitions can give results which exemplify generality.

It would be extremely difficult to satisfy the conditions of explanatory depth without making ontological commitments. Scientific theories make substantive claims about the way that the world is. Scientists rarely operationalize the theoretical terms of their theories, perhaps for pragmatic reasons or perhaps because they take themselves to be giving

a correct description of reality. The question is whether or not it is, in principle, possible to translate all of the primitive postulates of a scientific theory into phenomenological statements. Hempel's famous 1958 article "The Theoretician's Dilemma" presents arguments which can be employed to show why it is difficult to replace a scientific theory with another theory using only phenomenological terms and yet preserve the features of explanatory depth, no matter how the distinction between phenomenological and theoretical is drawn.

Hempel's dilemma is:

If the terms and principles of a theory serve their purpose they are unnecessary, and if they don't serve their purpose they are surely unnecessary. But given any theory, its terms and principles either serve their purpose or they don't. Hence, the terms and principles of any theory are unnecessary. ([29] pg.49-50)

It is the first sentence that Hempel has pragmatic reasons to reject. So long as we desire depth and predictive power, and so long as we do not wish to systematize by admitting absurdities to the postulate basis, it is necessary that our theories make ontological commitments.

Hempel illustrates this point by explaining that the empirical generalization; "*wood floats on water; iron sinks*" is only applicable to a limited range of situations. Some kinds of wood sink and iron formed into a battleship will float. An operational definition can be constructed which captures the more general facts. The specific gravity of a thing can be defined as the quotient of its weight and its volume, and then a new generalization can be asserted: *A solid body floats on a liquid if its specific gravity is less than that of the liquid* ([29] pg.43-44). Including this generalization in an explanation would improve the

indicator of depth over the previous generalization.

The first part of Hempel's argument involves pointing out how theoretical terms get mixed into general law statements along with observable terms. We can operationalize "specific gravity" by defining it as the quotient of weight and volume, which are *observable*. Nevertheless, when we compare the floatability of two objects we will compare their specific gravities. This is because it is notationally simpler (easier) to express things this way in this sort of case. What should be recognized is that the *theoretical detour* (as Hempel calls it) is conducive to giving systematic connections between observable terms and general laws. Of course the question is, can we operationalize *all* of the terms of any general law statement? Even Carnap begins with the very guarded claim that,

In the case of many words, specifically in the case of the overwhelming majority of scientific words, it is possible to specify their meaning by reduction to other words. ([45] pg.11)

Hempel argues that we have pragmatic reasons not to operationalize all of the terms of our theories. Some of the terms of a theory certainly are definable in terms of the others. However, we must take some terms as primitive. The question then becomes, can we take all and only observational terms as primitives? A problem then arises due to the fact that a definition must give necessary and sufficient conditions for the application of a term. If we attempt to give operational definitions for the primitive terms of a theory by mere conditional statements, a crippling objection arises. Consider a basic logical behaviorist proposal. Suppose that we wished to define Sue's anger by way of a standard conditional statement defining the 'anger' A of the subject x in terms of the observable initial conditions C and the expected behavioral outcome conditions E .

$$Ax \equiv (Cx \supset Ex).$$

The obvious problem with this definition is that the statement is true whenever the antecedent is false. Sue would then be angry in every case where the initial conditions are not met. The only way to preserve this sort of definition and avoid this problem is to bind Cx to Ex nomologically by positing a causal mechanism which would make the conditional express causal necessity. This brings us right back to Aristotle's original view of the *Physics*. A man comes to be musical from having been unmusical when *caused* to become so.

Carnap suggested an alternative which would avoid this difficulty by giving partial specification of meaning or by so-called *reduction sentences*. Carnap's suggestion would replace the above definition with a partial specifier of meaning for A with a definition of the form,

$$Cx \supset (Ax \equiv Ex).$$

This says that under the test conditions C Sue is angry A if and only if she exhibits a behavior of the kind E . Of course this is a *partial* specification of meaning, and to get a full specification of meaning for the general term 'anger' we would have to generate a series of reduction sentences for all of the possible subjects and all of the conditions for which this general term might be applied. This is precisely the situation which undermines predictive force. It does so because the program tends towards both syntactical and semantical complexities which trade-off with predictive power. First, the kind of syntactical simplicities

that *fit* the existing data very precisely will tend to run against predictive power unless accuracy constraints are loosened. Second, the goal to generate partial definitions for all of the terms of a theory will run into a dilemma. On the one hand, if partial definitions are supposed to be analytic truths, then they do not have empirical content. On the other hand, if the wild disjunction of partial definitions for a term are supposed to be related in some way then it is because they get at some underlying law of nature.

We can see how wild the disjunction of reduction sentences for a general term would be if we switch from psychology back to the examples from the first chapter involving physics. LeVerrier and Newcomb were working with the general term 'mass' which figures in both terrestrial and celestial predictive statements. The reduction sentences for objects that are small enough to shake around could be given in terms of the felt resistance to the change in motion of the object. However, we would have to define the mass of Jupiter in some other way. In this case it might be counter intuitive to think that 'mass' really means wildly different things depending on context despite the fact that one and the same general term appears in the predictions of billiard ball movements and of planetary orbits. Finally, it is not clear what sorts of experimental controls would have to be established in order to give empirical interpretations for the theoretical term 'mass' when applied to things like electrons or planets which cannot easily be manipulated. Even Carnap was skeptical that experimental conditions could be established to unambiguously specify the meaning of "the mass of the electron". So the impending project of giving partial definitions for all of the uses of the term 'mass' from electrons to bread boxes to satellites to black holes might give us pragmatic reasons to just take 'mass' as an un-operationalized primitive.

There is one final proposal to consider for the problem of giving necessary and sufficient conditions for the application of a term. It might be suggested that we attempt to give partial definitions in purely empirical terms by appealing to the entire body of scientific empirical research. However, there are two ways to think of this. One way would be to include all of the empirical research *up to now* on the assumption that science is more or less on the right track and that future scientific theories will include the terms science now has. The other approach would be to imagine a completed science and speculate about what the terms of *that* discourse will be. The first proposal is problematic because one thing that we know from the history of science is that revolutions happen, and when scientific revolutions happen we rethink our interpretations of empirical research from the past. After all, the benefit of hindsight shows us that the arguments given by LeVerrier and Newcomb were some of the key arguments which helped to set the stage for a scientific revolution which would tend to make us consider the project of giving operational definitions for ‘absolute time’ and for ‘force’ to be inappropriate. The second proposal is problematic, again, because revolutions happen and we cannot imagine what a future science will involve as far as empirical programs are concerned. Perhaps a future science will find a middle ground between introspective psychology and the experimentalist traditions by including mental predicates as observation terms. It is not clear how to justify settling on one of these two methodological presuppositions.

If we posit causal mechanisms in our scientific theories then the conditional statements of those theories will express causal necessity, and we would solve the formal problems involved in balancing systematicity with depth. Because conditional statements allow

us to derive other statements, systematicity is achieved by deductive logical connections. This would serve well the desire for depth and, if the posits were true, for predictive power.

If we posit primitive terms (like ‘mass’) with instances that fall under the scope of a universal quantifier ranging over a class of entities then we have also given necessary and sufficient conditions for the application of terms which can be the values of the variables bound by this quantifier. This sort of posit too serves well the desire for predictive power and for depth, and it contributes to systematicity by establishing logical connections.

Some things change and some things stay the same, and we could have learned from Hume that we cannot engage the project of explaining such a world without transempirical terms (which suggested to Hume that there is a non-rational foundation for human reasoning). Hume says that,

If reason determin'd us, it wou'd proceed upon that principle, that instances, of which we have had no experience, must resemble those, of which we have had experience, and that the course of nature continues always uniformly the same. (A Treatise of Human Nature 1.3.6.4)

But it is the uniformity of nature principle which Hume claims that we cannot justify by experience.

Why from this experience we form any conclusion beyond those past instances, of which we have had experience? If you answer this question in the same manner as the preceding, your answer gives still occasion to a new question of the same kind, even *in infinitum*; which clearly proves, that the foregoing reasoning had no just foundation. (A Treatise of Human Nature 1.3.6.10)

We also could have learned this just by thinking about Aristotle’s scientific adaptation of Plato’s mixed view. In the *Posterior Analytics* Aristotle says,

We suppose ourselves to possess unqualified scientific knowledge of a thing, as opposed to knowing it in the accidental way in which the sophist knows, when we think that we know the cause on which the fact depends, as the cause of that fact and of no other, and, further, that the fact could not be other than it is.(Book I, Chapter 2)

In so far as we desire our theories to have predictive power and in so far as we desire the features of explanatory depth, we have pragmatic reasons to posit causal mechanisms which govern change and to take as primitives general terms (which stay the same). Systematicity is necessary for testability, and in so far as we wish to have *scientific theories* rather than conspiracy theories or pseudosciences, we have pragmatic reasons to posit the sort of primitives that maximize the logical connections of a theory.

Depth is related to testability because testability is related to systematicity. A theory constructed using the predicate basis suggested by the behaviorist appears doomed either to be crippled by triviality or to become so relativized that it will not make predictions at all, let alone novel predictions. To use a turn of phrase popular in contemporary philosophy of mind debates, disjunctive predicates are not projectible. The logical behaviorist's obsession with scrutability understood in terms of epistemological simplicity undermines explanatory depth, and this undermines predictive power.

As suggested above, positing mechanisms of mental causation (the very thing that the behaviorist wished to avoid adding to the science of human behavior) would fix these problems. Positing mechanisms of mental causation would support non-backtracking counterfactuals (because causation has a direction) and the possibility of generality with respect to other possible properties of the theory. If the causal mechanism is shared by other theories, then it would contribute to this dimension of generality as well (a unity of science

kind of generality). If true then it would be expected to have serendipic power as well. We could, of course, be mistaken about what counts as confirming or disconfirming evidence for theories which posit causal mechanisms.

One final note on depth is that the symptoms of depth are relative to specific accounts of scientific explanation. For example, Woodward and Hitchcock defend what is known as the *interventionist account of explanation*. On this view, there are several indicators of depth; among them is *accuracy*, which I have kept as a distinct desideratum of science. Brad Westlake has also contributed to the project to taxonomize some of the leading accounts of explanation and to show the implications for ontology when depth is gauged in each way [64]. Westlake defends an *abstractionist* account of explanation and this account differs from the Woodward and Hitchcock account mainly due to a difference in views about which direction reductive explanations ought to go in.

Since more work needs to be done on explanatory depth, an exhaustive analysis cannot yet be presented. However, with this minimal understanding of how to gauge depth, we can see some of the ways to construct theories that exemplify some of the symptoms of depth. Operational definitions might turn the trick in some cases, although there are many problems with operational definitions. However, when it comes to the fundamental postulates of a theory, we have pragmatic reasons to take Hempel's *the theoretical detour*.

The conceptual map is now updated to show that a necessary condition constraining judgments of syntactical, semantical, epistemological and pragmatical simplicity in theory construction is that depth not be sacrificed. Obviously, since the positing of causal mechanisms would be one way to satisfy the conditions of explanatory depth, ontological

collapse of the analytic/synthetic distinction and the failures of attempts to give necessary and sufficient conditions for meaning on the verificationist theory. In particular, the positivist's hostility to metaphysics faced serious criticism. Bunge was certainly among those circling the positivist carcass. My guess is that it is closest to Bunge's intention to give the straightforward interpretation to "representativeness": that a theory represents the way that the world really is, rather than taking the basic postulates of a theory to refer to sensations.

Nancy Cartwright says that,

Philosophers distinguish phenomenological from theoretical laws. Phenomenological laws are about appearances; theoretical ones are about the reality behind the appearances. The distinction is rooted in epistemology. Phenomenological laws are about things which we can at least in principle observe directly, whereas theoretical laws can be known only by indirect inference. Normally for philosophers phenomenological and theoretical mark the distinction between the observable and the unobservable.

Physicists also use the terms theoretical and phenomenological. But their usage makes a different distinction. Physicists contrast phenomenological with fundamental. For example, Pergamon Press's *Encyclopaedic Dictionary of Physics* says, A phenomenological theory relates observed phenomena by postulating certain equations but does not enquire too deeply into their fundamental significance.([13] pg.1)

Representative theories posit theoretical entities, and one desideratum of science is to get these posits to represent reality. This section will explain some of the interrelations between *representativeness* and *systematicity*, *depth*, *testability*, and *simplicity*.

Hempel says that,

Scientific systematization is ultimately aimed at establishing explanatory and predictive order among the bewilderingly complex "data" of our experience, the phenomena that can be directly "observed" by us. It is a remarkable fact, therefore, that the greatest advances in scientific systematization have not been accomplished by means of laws referring explicitly to observables, i.e., to things and events which are ascertainable by direct observation, but rather by means of laws that speak of various hypothetical, or theoretical, entities.

i.e., presumptive objects, events, and attributes which cannot be perceived or otherwise directly observed by us.([29] pg.41)

Cartwright distinguishes between the philosopher's uses of the words "phenomenological" and "theoretical", but perhaps philosophers and scientists are talking about the same thing, which is that theoretical statements involve the theoretical detour. Bunge has stated that epistemological simplicity competes with logical simplicity and with depth. He has also said that phenomenalist languages come at the cost of syntactical simplicity and insight. Perhaps now it is clear why he said this. We could have learned this lesson from Berkeley, who attempted to give a radical empiricist interpretation of how God's simplicity is reflected in the created world of sensations [6]. Even Berkeley helps himself to the most exotic theoretical detour, positing *archetypes*, as he struggles to systematize an unwieldy view. On the most charitable of interpretations, Berkeley's suggestion that we operationalize the fundamental terms of science leads to considerable syntactical complexities. Epistemological simplicity leads to syntactical and semantical complexities and it does not reconcile with the features of explanatory depth, thereby undermining systematicity and predictive power.

Now an interesting set of questions arises about to how to update the conceptual map. We have seen that either the positing of fundamental causal mechanisms or the positing of general theoretical terms would be sufficient for satisfying the conditions of explanatory depth which is necessary for predictive power. It has also been shown how the theoretical detour is sufficient for the formal requirements of systematicity. It is tempting, then, to say that ontological proliferation favors *representativeness*. Or, in other words, that

there might be an inverse relation between ontological simplicity and *representativeness*. Yet, recall that the problems (1) and (2) which arise when we try to organize sense experience in an ontologically responsible way do not arise when we just stipulate the kinds of things to be counted. Perhaps we could stipulate simplicity criteria with respect to causal mechanisms on the pragmatic grounds just argued for. Positing one causal mechanism might seem to systematize a theory better than several independent causal mechanisms. Having just one kind of thing is the clearest case of ontological simplicity, and this is what systematizes Newtonian mechanics. Yet it is not clear that positing just one kind of causal mechanism is the best way to achieve systematicity because causal mechanisms could be dependent on one another.

Suppose, for example, that physicists posited one kind of causal mechanism and psychologists posited another kind of causal mechanism which happens to be restricted by the range of what is possible given the physicist's kind of causation. After all, a complete theory of the mental would involve certain physical limits because consciousness can be interrupted by a sharp blow to the jaw, for example. In this kind of situation, psychology would enjoy a certain unification with physics. It could turn out that the theories of psychology would have to be codified in modal systems like S4 or S5 which allow for nested modalities of different sorts. If we have reasons to employ S4 or S5 modal logics it is because they might be thought to adequately express the relationships of theories of this sort *and* because they establish greater systematic connections than do modal systems which do not allow for nested modalities. However, nested modalities may not be the kind of relations that contribute to depth and it would be to endorse an outdated view to sug-

gest that only deductive relations contribute to systematicity. Ontological proliferation may contribute to representativeness, but it is not clear what to say about the relation of causal mechanisms to representativeness. We should accept ontological proliferation only to the extent that such a postulate basis maximizes the other desiderata of science. After all, we should expect some ontological complexities in science, since its very grammar is committed to greater complexities than Parmenides and Heraclitus would have allowed, but the world may have a specific number of kinds of entities, kinds of properties and kinds of relations. We do not know what the upper bound on the complexity of the world is.

We can now update the conceptual map to show that *representativeness* is sufficient for depth. Representativeness is sufficient for systematicity and it stands in an inversely proportional relation with ontological simplicity in the most basic sense that we could easily have learned from Aristotle who followed Plato down the path of the mixed-view. We can also add relations showing that epistemological simplicity is inversely proportional to syntactical and semantical simplicity and inversely proportional to depth (assuming that perhaps losses in depth might come in degrees if we attempt partial specifications of meaning).

3.3.7 The Unity of the Sciences

Some version of *the unity of science* has been taken by many philosophers and scientists to be a goal of science. Bunge has said that "Simplification is sufficient for unification, but it is not necessary to this end and should be minimized in view of the important goals of accuracy." ([12] pg.89)

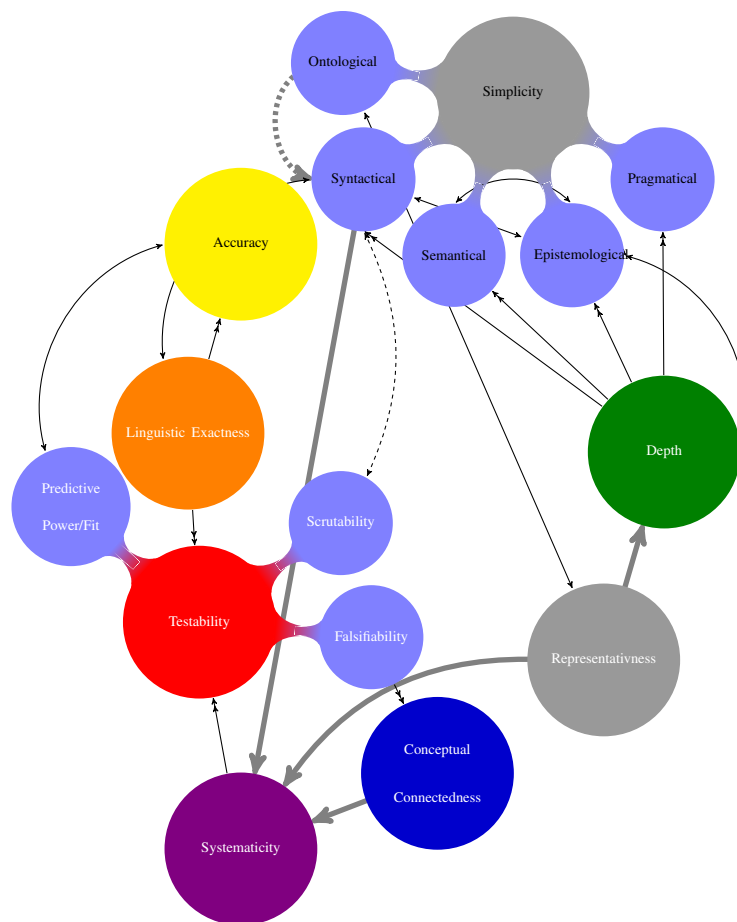


Figure 3.10: Representativeness

In the same article Bunge goes on to say of *World View Compatibility* that,

[it is desired] that the theory be consistent with the prevailing world outlook - a desideratum which, if exaggerated, leads to the banishment of originality. Simplicity is as inconsistent with world-view compatibility as it was found to be with external consistency. ([12] pg.105-106)

By “external consistency” Bunge refers to whatever the features of a scientific theory are that make it compatible with other theories. Scientists had very good reasons to be suspicious of Darwin’s evolutionary theory for many years. The reason is that it suggested that the Earth was millions of years old. This result was incompatible with the results in physics and astronomy. The mass of the sun was known and it had been used in the sorts

of calculations performed by astronomers like those of LeVerrier and Newcomb for many years. The problem was that the only known process by which matter could be converted into heat was by chemical processes. It had been calculated that if the sun converted 100% of its chemical energy into heat it would burn out in a scant few hundred thousand years¹⁸. A theory of atomic energy was needed to reconcile Darwinian theory with physics.

The problem with discussing the unity of science is that it is subject to a great deal of vagueness. Again, all that can be accomplished here is to gesture at some of the features of this goal of science and their relations to the other desiderata of science. Jordi Cat opens the Stanford Encyclopedia of Philosophy article on the unity of science saying that,

The topic of the unity of science includes the following questions: Is there one privileged, most basic kind of stuff, and if not, how are the different kinds of material in the universe related? Can the various physical sciences (physics, astronomy, chemistry, biology) be unified into a single overarching theory, and can theories within a single science (e.g., general relativity and quantum theory in physics) be unified? Does the unification of these parts of science involve only matters of fact or are matters of value involved as well? Moreover, what kinds of unity in the sciences are there: is unification a relation between concepts or terms (i.e., a matter of semantics), or about theories they make up? And is the relation one of reduction, translation, explanation, or logical inference? ([14])

It may already be clear how to extend the discussion of the desiderata of scientific theory construction and testing to the discussions about the unity of science. This section has covered some of the goals and difficulties involved in analyzing conceptual connectedness, of giving reduction sentences, of satisfying the features of scientific explanation and of systematizing under deductive relations. It would be natural to extend these discussions

¹⁸Bunge makes brief mention of the *cool reception given to Darwinian theory in France* in his 1961 article. [10]

further to inter-theoretic relations. It may also become clear how much distance we can get out of the examples raised in the first chapter. The discussion of Plato's mixed view sets the stage for thinking about the problems of induction, for thinking about the grammar of science, and for seeing the basic reasons articulated by Hempel for the theoretical detour. This discussion also sets the stage for thinking about the historical facts about the heritage of proclivities for unity in Western thought. Cat says that,

The general questions should be carefully distinguished from any of the different specific theses addressing them and should be noted as the linking thread of a time-honored philosophical debate. The questions about unity belong to a tradition of thought that can be traced back to pre-Socratic Greek cosmology, in particular to the preoccupation with the question of the one and the many. In what senses are the world and, thereby, our knowledge of it, one? A number of representations of the world in terms of a few simple constituents considered fundamental emerged: Parmenides' static substance, Heraclitus' flux of becoming, Empedocles' four elements, Democritus' atoms, or Pythagoras' numbers, Plato's forms, and Aristotle's categories.([14])

Cat goes on to say that,

With the advent and expansion of Christian monotheism, the organization of knowledge reflected the idea of a world governed by the laws dictated by God, creator and legislator.([14])

Of course the Christian revolution contributed significantly to the thought of the key thinkers of the scientific revolution. Cat says that,

The emergence of a distinctive tradition of scientific thought addressed the question of unity through science's designation of a privileged method, set of concepts and language. In the late 16th century Francis Bacon held that one unity of the sciences was the result of our organization of discovered material facts in the form of a pyramid with different levels of generalities; these would be classified in turn according to disciplines linked to human faculties. In accordance with at least three traditions, the Pythagorean tradition, the Bible's dictum in the Book of Wisdom and the Italian commercial tradition of book-keeping, Galileo proclaimed at the turn of the 17th century that the Book of Nature had been written by God in the language of mathematical symbols and

geometrical truths; and that in it the story of Nature's laws was told in terms of a reduced set of objective, quantitative primary qualities: extension, quantity of matter and motion. In the 17th century, mechanical philosophy and Newton's systematization from basic concepts and first laws of mechanics became the most promising framework for the unification of natural philosophy. After the demise of Laplacian molecular physics in the first half of the 19th century, this role was taken over by ether mechanics and energy physics.([14])

Various projects to classify the parts and the structure of the world or to do the same for human knowledge have fallen under the term "unity of science". It appears that the ancients held metaphysics to have logical priority over epistemology and modern thinkers invert this relation. The ancients wondered how the world and thereby our knowledge of the world was one. Modern thinkers may wonder how our knowledge of the world is one and whether or not that corresponds to the way that the world is. The structure of the dialectic of metaphysics perhaps was mapped onto the dialectic of epistemology in the Early Modern period. We can begin by asking questions about what structures must be in the mind in order for there to be some unity of thought and then ask the question of whether or not any of these correspond to the world to shift the discourse from epistemology to metaphysics, unless of course, we accept Kant's Copernican Revolution in metaphysics where the study of the cognitive structures just *is* the study of metaphysics. For this project I suggest taking the modern approach and asking the epistemological questions first. The arguments for this approach have been given above. When we investigate scientific explanation (epistemology) we end up with pragmatic reasons to take general terms and causal mechanisms as fundamental (ontology).

We have already seen that concepts may be structured definitionally, mereologically, or inferentially. In any case, conceptual unification is sufficient for systematicity. So,

if there is a unity of scientific knowledge then it contributes to conceptual connectedness. Different theories may also be connected by simply sharing fundamental postulates. Also, theories could be unified structurally, as suggested by the example where two theories share a fundamental causal mechanism, so that the operation of one is governed by the range of possibilities of a causal mechanism posited by the other. So, *representativeness* would be sufficient for the unity of science just in case nature turns out to be unified. The next chapter's discussion of scientific reduction explains in more detail how it could turn out that nature is not unified in this sense.

We seek linguistic exactness. This is why it is preferable to codify theories in logical or mathematical systems. A loss in linguistic exactness leads to a loss in accuracy. We now know why Bunge said that simplicity would be sufficient for the unity of science, only we will not accept simplifications which decrease accuracy if the expense involves a loss of depth or testability. A form of ontological simplicity would be sufficient for the unity of science, that is if more than one theory share a fundamental (un-operationalized) postulate. This could well amount to syntactical and semantical simplicity as well. However, it has been shown above that systematicity and depth benefit from ontological complexity so ontological simplifications ought to be accepted for the unity of science only on balance with the other desiderata of science. It has already been shown that some of the desiderata of science impose necessary constraints on several of the species of simplicity.

Depth may be indicated by generality with respect to other systems. Maximizing this feature of depth contributes to a version of the unity of science which Bunge calls "external consistency." Representativeness is sufficient for any version of the unity of science

sketched by Cat. An increase in the versions of the unity of science having to do with the structure of knowledge would be sufficient for conceptual connectedness. Since the focus of my investigation begins with the desirable features of scientific theories expressed in symbols the relevant versions of the unity of science are sufficient for conceptual connectedness.

Before adding these relations to the conceptual map it is worth noting that different scientists may have seen the unity of science as a desideratum which imposes necessary conditions on various goals of scientific theory construction. I will not try to show what necessary conditions might be imposed on the other desiderata of science by a preference for scientific unity because this might involve a serious investigation into the history of science. The first chapter has given good reasons to suspect that scientists like LeVerrier and Newcomb gave a very high degree of priority to scientific unity. When we look at the diagram generated by the conceptual map, we can imagine that some scientists may have held the unity of science to be so important that it may extend an array of necessary conditions to the other desiderata of science. However, because depth is sufficient for scientific unity it cannot be precisely determined if these scientists rejected certain hypotheses because they competed with scientific unity or with some of the features of depth. Perhaps it would be a contribution to the philosophy of science to investigate further how historically influential theories and experiments were constructed with considerations given to depth and to scientific unity.

The conceptual map is now updated to show that depth and representativeness are both sufficient for the unity of science and the unity of science is sufficient for conceptual

connectedness.

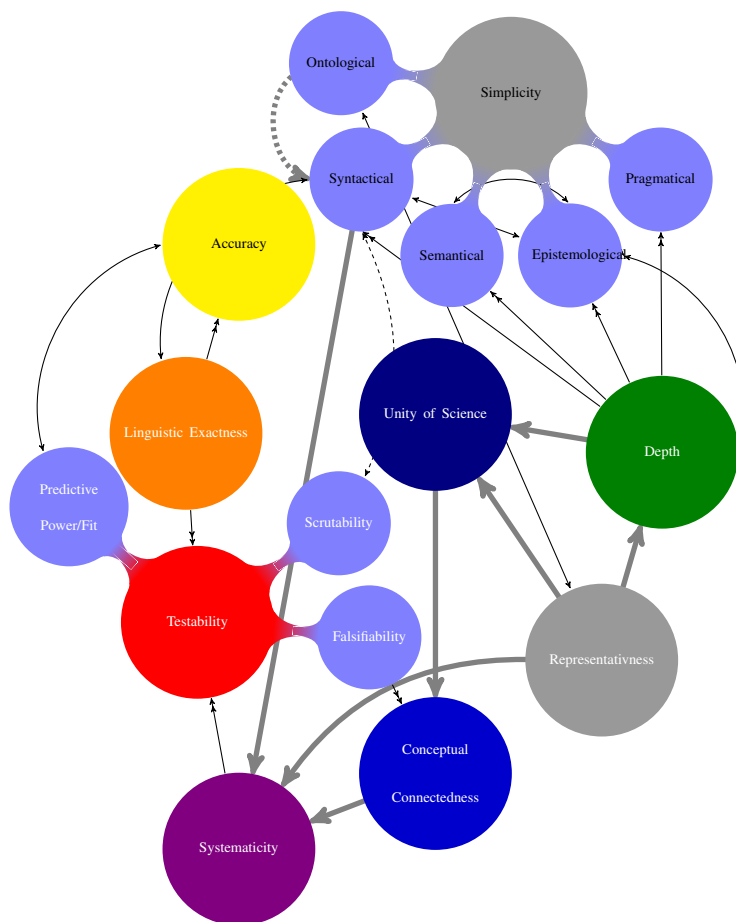


Figure 3.11: Unity of Science

3.3.8 Other Relations

The discussion from the first section showed that pragmatic simplicity often is opposed to syntactical and semantical simplicity. This can occur in situations where judgments of psychological simplicity or notational simplicity lead to complexity of the predicate basis. However we will not accept these simplifications if they render a theory less testable or deep. Pragmatic simplifications are constrained to some extent by some of the

other desiderata of science. It has been argued that there may be cases where pragmatical simplifications will lead to a decrease in accuracy and this would lead to an increase in predictive power. However, what will not be acceptable is a decrease in scrutability or depth. It is important to bear in mind that scrutability should not be interpreted in the phenomenalist's sense as entailing that epistemological simplicity criteria govern selection of the predicate basis. The conceptual map can be updated to show the *inverse proportionality* relation between pragmatical simplicity and syntactic and semantic simplicity and the fact that scrutability provides a necessary condition for pragmatical simplicity.

I have followed in the footsteps of Goodman and Bunge by contrasting scientific theories with pseudoscience and conspiracy theories. We can see that certain types of complexities of the predicate basis which are designed in an *ad hoc* manner to promote the kinds of cohesiveness that will guard against refutation are to be avoided. However, in science the requirements of depth, predictive power, accuracy and linguistic exactness restrict logical complications of this sort. Now we can see that syntactical and semantical simplicity, properly restricted, do contribute to falsifiability. We can add to the conceptual map the necessary constraints that falsifiability places on these kinds of simplicity.

3.4 Evaluation

Several results can be gathered from the discussions of this chapter. We can revisit some of the issues raised in the previous chapter with a finer grained set of terms and distinctions. We can now give an analysis to principles of parsimony and to some of the key roles played by judgments of simplicity in scientific theory construction. We can also



Figure 3.12: Other Relations

establish the foundations for criticisms of the contemporary explosion in the literature dedicated to curve-fitting. I will also make a proposal about how to advance future research programs into the roles of the desiderata of science in theory construction.

3.4.1 Ockham's Razor and Conceptual Analysis

I suggest that we stop using the term "Ockham's Razor" altogether. "Ockham's Razor" has been shown to be subject to vagueness and contemporary uses of the term invite historical confusions. The philosophical problem with Ockham's Razor is that the *ceteris paribus* clause is impossible to satisfy for two different competing scientific hypotheses.

In some cases it may appear that a scientist's actions would be explained if she employed a principle of parsimony in her methodology. However, a closer look at some of these cases reveals that a different sort of reasoning may be involved. Although many have thought that some version of a principle of parsimony served as the final court of appeal in selecting between competing theories, we can now see that some versions of simplicity (especially syntactical simplicity) may just be a symptom of the scientist's desire to maximize depth, predictive power, or the unity of science. In many cases simplicity judgments do play a role in theory construction, it is just that these ought to be kept distinct from principles of parsimony which assert something about the *likelihood* of a theory to be true. Some judgments of simplicity may be reduced to judgments of other sorts; however, given the extreme heterogeneity of simplicity, no overall measure of the simplicity of theories is possible. After all, simplicity is difficult to gauge, and in cases where a clear gauge is available, we have seen that maximizing one species of simplicity leads either to complexity for some other species or to an unacceptable loss in one of the other desiderata of science; we do not, for simplicity's sake, render theories less testable, less deep, or unfalsifiable.

The devastating result of our study of the interrelations between the metascientific criteria is for the family of principles which involve a *ceteris paribus* clause. There are always trade-offs between the metascientific criteria, so other things never are equal. My objection to Swinburne's example in the first chapter is not a case of meager bickering about example choice. First, if one is to be a Kantian about the methodological role of an indispensability principle, then having an example is logically prior to putting the test of indispensability to it to see how experience conforms (or fails to conform) to a concept.

Kant makes it quite clear that the Egyptians *groped about* with mathematics long before some Greek decided to *let the figure inform him*. My best guess is that Swinburne wishes to employ some version of the indispensability principle in the argument for his multifaceted obsidian razor. If I am right about this, then Swinburne must find an unproblematic example.

Secondly, the analysis of simplicity shows us that the *ceteris paribus* clause will be impossible to satisfy in a non-arbitrary way. It is tempting to suppose that scientists like LeVerrier investigated unrestrained conceptual space as they dreamt up hypotheses to explain the phenomena they were interested in, and that here some version of the principle of parsimony helped them to decide which hypotheses deserved further attention. The problems (1) and (2) from the first section show us that this cannot be the correct story to tell about the role of the imagination in *scientific* theory construction. We require some principled way to specify gauges of simplicity in the first place. It is unlikely that LeVerrier's methodological principles were selected arbitrarily. Although I do not yet know precisely what LeVerrier's methods were, we may have a partial guide to determining what some of them were. It is very implausible that LeVerrier could have offered infinitely many competing hypotheses for any of the data that was of interest to him. He took only a very few to be candidates in the first place and then he ruled out each of these by way of arguments that were widely accepted in the scientific community. We can learn something about scientific methodology by investigating the arguments given to rule hypotheses out. But we might also learn something by the ways that hypotheses were constructed in the first place, even if they are constructed only to be ruled out a short while later. It seems to me quite wrong

to suggest that LeVerrier could have given infinitely many competing hypotheses. It seems that he could not. To give any more (or many more) than he did probably would have conflicted with some of his methodological principles. Swinburne appears to think that the relevant question is, why did LeVerrier give only one hypothesis in the case of Neptune? The better question to ask is what LeVerrier's hypotheses had in common such that he only ever offered a handful as candidates to explain some data. After all, two ad hoc hypotheses could be turned into one by conjunction unless this is constrained by a principle of syntactical simplicity as well. In such a case, the hypothesis with conjunctive ad hoc additions would be doubly complex compared to its rival, but not all other things would be *equal*.

Whatever answers suggest themselves to the question of why only a few contenders are allowed into the ring, I do not see how any of them could involve principles with *ceteris paribus* clauses. Suppose that we stipulate the simplicity criterion to play the role in a principle of parsimony with a *ceteris paribus* clause, to select *numbers of ad hoc hypotheses*. At first blush, this might seem like a clear gauge of how hypotheses are generated so that they diverge only a little from the accepted theory. The problem is that scientific hypotheses are *systems of signs* and they have *content*. This means that there will always be another gauge of simplicity available to give a different measure. As we have seen, it is pretty easy to conjure incompatible measures of simplicity by selecting some feature and then trying out different gauges for that feature.

Consider two of LeVerrier's hypotheses about the behavior of Mercury: the Inter-Mercurial planet hypothesis and the Inter-Mercurial ring hypothesis. It might be thought that LeVerrier's hypotheses vary from the accepted theory about the matter in the solar sys-

tem, in each case, by only one *ad hoc* hypothesis. However, these hypotheses are neither conceptually nor empirically equivalent. This shows us that incompatible gauges of simplicity are immediately available. The Inter-Mercurial planet hypothesis might be said to posit just one more thing if we count things as Swinburne does, or perhaps that it posits 0 extra *kinds of things*, a naught difference in complexity from the accepted ontology of Newtonian theory. This hypothesis was also calculated to be very easy (pragmatically simple) to test for and (in an ironic Ockhamian-twist) therefore very unlikely, since the patient eyes of astronomers had not collected data compatible with this hypothesis.

It might also be thought that the Inter-Mercurial ring hypothesis varies by only one *ad hoc* hypothesis from the accepted theory about the matter in the solar system but this hypothesis would count in the millions of things if we count as Swinburne does, or alternatively 0 kinds of things. Finally the initial suggestion of the Inter-Mercurial ring hypothesis was that it would be very difficult to test for (pragmatically complex) if the rings were at the right sort of inclination to be always invisible from Earth.

It appears that LeVerrier rejected these hypotheses because they failed to unify with the accepted body of science and because they could be calculated to give absurd empirical predictions. Testability and unity of science may well have been the criteria which inspired the introduction of these hypotheses in the first place rather than some vague or arbitrary principle of parsimony. LeVerrier sought to give theories which were empirically adequate, which had predictive force, and which unified with the body of science, and then he committed more to discussing what was unlikely rather than what was likely. My objection to Swinburne's version of the principle of parsimony and to any other in that family is that sci-

entific hypotheses have content and this lays open the possibility that incompatible gauges of simplicity are available. But without the *ceteris paribus* clause, a principle of parsimony cannot be the final court of appeal in scientific theory selection because some other principles must guide the non-arbitrary selection of simplicity criteria. Objection strategies (1) and (2) arm us to show that other things never are equal, because both arbitrary and non-arbitrary gauges of the syntactical, semantical, ontological, epistemological or pragmatical features of any two *ad hoc* hypotheses will not give equivalent results.

There may be other versions of the principle of parsimony which do guide theory construction. Ariew interprets Ockham as promulgating one principle about explanations and another about statements or concepts. [3] Newton appears to have promulgated principles about *kinds of causes* and about *kinds of entities*. Properly formulated these principles might actually be a guide to the desirable features of scientific theories, at least in the early stages of theory development. We might plausibly formulate principles of parsimony which select some of the features of scientific theories guided by considerations for depth or systematicity.

Suppose that we follow Newton's apparent principle to limit *kinds of causes* in the postulate basis. This immediately satisfies some of the features of explanatory depth and it contributes to systematicity better than positing several independent kinds of causes. Additionally, this kind of posit will be reflected by semantical gauges of simplicity and perhaps by syntactical simplicity, depending on the number of laws hypothesized and the syntactical gauge selected. Also, if we peer over the cubical to borrow this kind of causal mechanism from a neighboring theorist, we would satisfy the desire for the unity of science.

The analysis of this chapter has been dedicated to the kinds of simplicity that systems of signs might have, but when we begin to think about the unity of science new kinds of simplicity suggest themselves. Perhaps the *degrees of closeness to other systems* might serve as the basis for judgments of simplicity also. The *sharing of basic postulates of the same category* would certainly be a candidate for a gauge of scientific unity. However, these sorts of simplicity gauges are only useful in the preliminary stages of theory development. As a theory marches into what Kuhn calls 'normal science', these principles can be jettisoned. To seek a circulatory theory of government by embezzling the circulatory theory of blood is to seek a fiction. Darwin's theory of natural selection set securely upon its path of normal science only after it joined with the mechanisms of molecule synthesis and replication. One example of this version of simplicity leads in one direction to *unlikeliness* and in the other to complexity. With these reflections, a skeptical eyebrow is raised about how long our most cherished unified scientific theories will remain, conceptually, as they are today. Perhaps more things change than stay the same. Bunge has argued that scientific theories march in the direction of the complexity of the postulate basis as they mature. The analysis of this chapter gives us good reason to believe Bunge on this point. The more over and underpasses that are taken by the theoretical detour, the more our theories will be systematic.

What does this have to do with *likelihood*? Something, I would say, but it does not obviously have to do with the likelihood that a highly systematic theory will be *true*. The more lines that cross the face of a mature theory the more *likely it is to be replaced* by

fresh blood¹⁹. Complexity of the postulate basis marches in the direction of testability, and because linguistically complex theories fit better existing data, in this direction lay future anomalies.

Let us never speak of *ceteris paribus* principles of parsimony again. Nothing that we can imagine satisfies the *ceteris paribus* clause in a non-arbitrary way. Counterexamples will forever be conceivable, and this suggests that as a final court of appeal in scientific theory selection, there is no *Ockham's concept* at all.

3.4.2 Avoiding Obsessions with Syntactical Simplicity

The analysis presented in this chapter allows us to give, at least partial answers to some of the questions raised in the previous chapter. Recall the discussion of the forensic hypotheses offered by Sherlock Holmes. Holmes does conduct some inferences similar the inferences made by scientists. In the first chapter the following questions were introduced:

1. What, if anything, would justify hypotheses which track causation when this form of inference cannot give to us an idea of the necessary connection between causes and effects, but only an idea of their being correlated in the past?

2. What, if anything, would justify selecting just one hypothesis from among infinitely many which would also account for the data?

¹⁹Bunge uses the term 'fresh starts' to describe this situation. [10] It shares some common ground with Laudan's pessimistic meta-induction as well. I would not say that this view is similar to Kuhn's *Gestalt Shifts* partly because Bunge does not agree with this and so, could not have meant it. [41] Also, I would not endorse the *Gestalt Shift* view because Laudan has correctly criticized this notion.[43]

3. What, if anything, would justify a certain degree of inaccuracy in hypothesis formation and what would constrain hypotheses from becoming incorrigible due to vagueness?

We can give pragmatic answers to these questions. Hempel argued that we posit causal mechanisms for pragmatic reasons. We do not need to *know* (in some Cartesian sense) what necessary connection there is between points of data, we only need to posit causal mechanisms to describe them if we wish to have systematic, and therefore testable theories. Our postulate basis may be jettisoned if it fails to give predictive results, or if it comes into extreme dissonance with the accepted body of science, or if it would be impossible to falsify its predictions. None of these problems are serious by the pragmatist's lights. Failures just guide us to select another causal mechanism for the postulate basis of our revised theory. Negative results might lead us to reconsider the way in which we classify the data that we tried to describe with some antiquated theory and with some antiquated causal mechanism, or they might inspire us to peer over the cubical in an attempt to systematize by way of the unity of science.

The second question is too vague to answer as it is. It ought to be made precise by asking, what if anything would justify selecting just one hypothesis in field X from among infinitely many which would also account for the data? If we are talking only about accounting for data and making predictions, then it is obvious that people do not select just one hypothesis to do this. Engineers still use Newtonian Mechanics everyday. It is pragmatically simpler than any other alternative involving quantum physics or some general

theory of gravitation. In solid state physics it is often convenient to model the properties of a semi-conductor by treating atoms as if they conformed to the Bohr model. These pragmatical simplifications might appear to sacrifice depth. My guess is that physicists would scoff at this charge. They would say that in these cases the postulate basis relevant to the focus of their projects are preserved and that calculations made in these ways are approximations which are well within relevant accuracy constraints.

Even if we are not talking about engineering, it does not appear to be the case that theorists themselves generate only one theory. Simon Newcomb was an exemplary scientist and it was not because he advanced and defended a precious theory all his own. It is because he collected many hypotheses from his predecessors and generated a few of his own, painstakingly working out the deductive consequences of each to see if these unified with the accepted body of science or if they gave testable results. Newcomb's contribution to science was to make it quite clear that the accepted body of science in the Nineteenth Century could not explain certain data. This kind of work is extremely important and I think that philosophers tend to forget about it when they get to focussing on the weight of evidence in crucial experiments, about the puzzles of *theory selection*, and about revolutionary theories themselves without thinking about what set the stage for the revolution.

So, one answer to question (2) is that not all scientists do select just one hypothesis from amongst a set of competitors. Another answer is given by the discussion of *ceteris paribus* clauses. It is very difficult to find cases where there are genuine competitors. Empirical adequacy is not the only desideratum of science.

On the other hand, perhaps there are cases where one theory is selected for testing and further research and in these cases again, pragmatic reasons should be easy to give for why there are no competitors in some cases. This must be evaluated on a case-by-cases basis.

The answers to question (3) are the same as the answers to question (2). Certain inaccuracies may be permitted given that scrutability, and falsifiability are not sacrificed. Additionally, questions (2) and (3) are intimately related. Recall that in the previous chapter it is pointed out that Holmes appears to have improved the chances that his retrodictions would be true by not getting too specific about some of the details about the causes of the watch scratches. This point gestures in the direction of the curve-fitting problem. Forster and Sober say that,

Curve fitting is a two-step process. First one selects a family of curves (or the form that the fitted curve must take). Then one finds the curve in that family (or the curve of the required form) that most accurately fits the data. These two steps are universally supposed to answer to different standards. The second step requires some measure of goodness-of-fit. The first is the context in which simplicity is said to play a role. Intrinsic to this two-step picture is the idea that these different standards can come into conflict. Maximizing simplicity usually requires sacrifice in goodness-of-fit. And perfect goodness-of-fit can usually be achieved only by selecting a complex curve. ([24] pg.1-2)

Forster and Sober discuss Akaike's curve fitting algorithm and some of its possible applications in this article. Akaike's idea is that we can use the trade-off between accuracy and goodness of fit and the trade-off between linguistic exactness and syntactical simplicity to fit curves to data. We can do this partly because linguistic exactness places some constraints on accuracy. The gauge of syntactical simplicity used by Akaike is very similar to the one proposed by Jeffreys.

We have already seen how many objections are available for Jeffreys' gauge of simplicity. The application of this gauge is limited to hypotheses which are expressible in a differential form. It is also the case that this must be used in cases where arbitrarily selecting a different coordinate system would be ruled out, perhaps because a mathematical transformation would change the physical meaning of the hypothesis. It must also be the case that some reasons are available to justify giving equal weight to the values of heterogeneous variables and that other kinds of simplicity are ignored. In short, Akaike's curve-fitting algorithms cannot be thought of as general solutions for all cases of curve-fitting in science. Forster and Sober are perfectly aware of these problems.

This view of the curve fitting problem engenders two puzzles. The first concerns the nature and justification of simplicity. What makes one curve simpler than another and why should the simplicity of a curve have any relevance to our opinions about which curves are true? The second concerns the relation of simplicity and goodness-of-fit. When these two desiderata conflict, how is a trade-off to be effected? A host of serious and inventive philosophical proposals notwithstanding both these questions remain unanswered. ([24] pg.2)

Now, I think that some of the recent excitement about Akaike's algorithms arises for two reasons. 1) It is tempting to think that because Akaike's algorithms give a mathematical solution to the curve-fitting problem which means that they add no presuppositions to the scientific basis. 2) Because questions about justification remain, it is also easy to be swept away by the hope that these algorithms do something *magical* and avoiding metaphysics altogether. Both of these views would be mistaken.

It has already been shown that it might be practical to seek some kind of syntactical simplicity in the early stages of theory development. The reasons are that if we can formulate a theory mathematically, then we satisfy the desire for linguistic exactness. If we

gauge the simplicity of a mathematical hypothesis by its syntactical properties then we satisfy a desire for systematicity. When it is possible to use an algorithm involving a gauge of syntactical simplicity we would have good pragmatic reasons to do so. Nevertheless, if the final theory (which may take generations to develop) is deep and gives predictive results, then we will not think that it is because of the syntactical simplicity of its hypotheses, but because of its representativeness and depth.

Secondly, it is not the case that an actual application of one of Akaike's algorithms is completely presuppositionless. Data does not appear from nowhere. Data is collected by skill practitioners who subscribe to a rigorous methodology. This means that their methods are theoretically embedded. Applying Akaike's algorithms to data collected in a theoretically embedded way should only be expected to reveal things about the presuppositions of the theories which were employed in generating the data in the first place. Perhaps curve-fitting algorithms are best used in error analysis and maybe their successful employment helps us to notice things about the relationships between background theories.

The point of all of this is that *mere* syntactical simplicity has not yet answered the Humean skeptic. Why would it? Hume saw that there was something essential about science that forced us to posit transempirical terms. Of course this is the case. It was so by Plato's design. Now, Hume thought that something non-rational happened in the mind when certain *fictions* were posited to solve the problem of induction and to avoid the discomfort of confronting contradictions. I do not know if Hume was right about this. In "Reduction: Ontological and Linguistic Facets" ([30]) Hempel has argued that the "linguistic turn" or the obsession with the linguistic analysis, leaves us still with the problem

of identifying what are the natural kind terms. This analysis shows why this is the case also. Hempel has shown us that we have pragmatic reasons to take the theoretical detour, and it is this that makes our theories deep and this that makes them have predictive power. No matter how we count the syntactical or semantical properties of hypotheses, they have content and so the symbols out of which a theory is constructed reflect the postulate basis and whatever it is (models or axioms) that relate the fundamental postulates. When the analysis reaches certain levels of abstraction, it is easy to forget this, and this is what I believe happens to those who become obsessed with syntactical simplicity gauges as a way to avoid doing metaphysics.

3.4.3 A Modest Suggestion

The arguments of this chapter regarding depth and representativeness have been thoroughly pragmatic. For this reason it is very easy to make another pragmatic suggestion: that we relativize the analysis of simplicity to specific scientific theories or bodies of theories. Plato's mixed view is committed to two abstract kinds of simplicity to start with; a certain complexity on one way of counting. Given the nature of Plato's way of mixing the Pythagorean hypothesis with the Herclitian hypothesis, we are left looking for yet another kind of simplicity to solve the epistemological problems that we have inherited. By taking a relativistic approach to scientific theory construction and evaluation, we can let the abstractness of Plato's mixed view inspire creativity in filling out the details. We can let the extreme heterogeneity of simplicity work for us so long as we remember to make simplicity criteria clear, so that we do not stumble into equivocal or vague uses of the

term ‘simplicity.’ We might, then on pragmatic grounds find non-arbitrary ways to select simplicity criteria which aid in theory construction and the testing of existing theories. I suggest that we try out different gauges of simplicity for different theories to see what sorts of results we get. Failures might show us as much about our theories as do the successes by this method.

I am not sure that mere conceivability will do the work that philosophers intend for it in the philosophy of science. In the Swinburne example I do not know how to imagine Newtonian mechanics to be true and then conceive of the alternative hypothesis he asks us to conjure. In the philosophy of science philosophers run the risk of conceiving of examples which are not scientific. The present discussion should cast this in even better light. The conceptual map program renders a very complicated diagram. It very difficult to think of examples where we change, let’s say, pragmatical simplicity by introducing a hypothesis, and yet retain the proper relations to the other desiderata of science. My project is supposed to contribute to conceptual analysis by creating an image which allows us to consider many relations at once. This would be too great a task for my mind without an aid. Bunge provided all of the parts needed for the whole represented by the diagram which the conceptual map program renders. Synthesizing these arguments into the whole which the conceptual map program produces does not give us something more than the whole, but we may be in a better position to engage these arguments.

Relativizing analysis to systems might have some interesting and helpful results. As a practical matter, scientists and philosophers review theories and articles regularly and it would be helpful if they had a tool which allows them to refer very quickly to the inter-

relations between the desiderata of science. This may help them to evaluate the theories before them. If the arguments which generated this conceptual map program are all sound, then people in these positions may only glance at the diagram and judge whether or not a proposed theory will satisfy the demands of depth, systematicity and testability. If any are unsound, then the arguments given to construct the conceptual map should be criticized and the program updated.

I would think the relativized view would be helpful to pure academic research as well. We should explore different approaches to the development of theories. It might help to think holistically of the desiderata of science and then try out different combinations of simplicity gauges for systems to see both how they cohere with the desiderata of science and to see what sorts of empirical predictions these systems give.

This suggestion is not incompatible with either scientific realism or scientific anti-realism. The anti-realist should be happy to accept the relativized approach and the realist might expect that successful theories generated by the relativized approach will, in the long run, contribute to triangulating the fundamental structure of reality. After all, scientific theories make substantive claims about the way that the world is. This is consistent with the desiderata of depth and representativeness.

CHAPTER 4 SIMPLICITY AND JUSTIFICATION

Like a gemstone, the clearer we make simplicity the more difficult it is to handle. The discussion of the previous chapter helps to clarify issues in the philosophy of simplicity generally. It also contains arguments which help to avoid traditional pitfalls, like the mistake of considering a principle of parsimony to be the final court of appeal in scientific theory evaluation. On the other hand, we have acquired many terms, distinctions and relations which can be difficult to manage. The aim of this chapter is to investigate the question of the justification of simplicity principles in scientific methodology. I will explain how one version of the question of the justification of simplicity is simply confused. Then I will attempt to formulate the questions about justification that may be reasonably asked. I will then organize the types of arguments that might be offered to justify principles of parsimony in scientific theory construction and evaluation. I will show which arguments might bear fruit and apply the results of the analysis of simplicity in science to show which kinds of arguments should be eliminated from this enterprise. My analysis shows first that the problem of the justification of simplicity judgments in science is motivated by the fact that we are still awaiting arguments (rather than mere assertions) that the problems of simplicity can be shuttled off to aesthetics or psychology. Second, I show that some argument forms may be in the running to give this justification although many of them may depend upon solutions to traditional problems in metaphysics and epistemology. Induction, some kind of transcendental deduction, and inference to the best explanation may all be in the running, although each of these, in their own ways, may involve commitments that are

unacceptable to some philosophers.

Bas van Fraassen says that

Studies in philosophy of science divide roughly into two sorts. The first, which may be called foundational, concerns the content and structure of theories. The other sort of study deals with the relations of a theory on the one hand, to the world and to the theory-user on the other. ([61] pg.2)¹

This chapter investigates issues analogous to the second kind of study distinguished by van Fraassen. This study focusses on what types of inferences might be employed to show that there is a relation between a scientific methodology and the scientist or between a methodology and the world. The first and second chapters have shown that it is not merely our theories that make ontological commitments, but our methods as well. When our analysis of methodological principles is sufficiently clear, we see that they involve criteria, some of which select ontological objects. This is unavoidable. Hempel's arguments are typically taken to show that we cannot elucidate scientific theories by reducing all of the terms of science to those which denote only that which is empirically basic. We could have learned a similar lesson from Bunge who has shown that empirical simplicity involves trade-offs with the other desiderata of science – trade-offs which tend toward unacceptable sacrifices in depth and predictive power. It is difficult to see how to balance the meta-scientific criteria

¹The quotation from van Fraassen must not mislead. Van Fraassen is perhaps best known for advancing a particular scientific antirealist view. I cannot do justice to the scientific realism/antirealism controversy. One reason for this is that there may be as many realist and anti-realist views in the philosophy of science as there are philosophers of science. Organizing this discourse would require a book all its own. Another reason is that it might take yet another book to organize the various arguments on meaning and reference which are crucial to this debate. Still, van Fraassen's work plays a key role in several of the issues presented in this chapter. This is because the issue of simplicity and issues involving types of inference have arisen in the debate between van Fraassen and his critics. Fortunately, van Fraassen and his critics are systematic thinkers and we can easily pluck from the debate the issues relevant to simplicity in science.

by an empirical criterion alone. Hempel's arguments give insights beyond just the elucidation of theories. They reveal similar problems for the elucidation of scientific methodology as well.

Why distinguish the philosophy of methodology from the philosophy of theories? The answer is that underdetermination runs both ways – from theory to methodology and from methodology to theory. A popular twentieth century view must be put to rest, namely that we can read off of a theory what are the methods for its construction and evaluation. Bunge's arguments show that a theory may be constructed by using many different criteria of the same species. Consider, for instance, that we may choose infinitely many gauges of syntactical simplicity, many of which will generate the same theories because the transformation rules of the system will allow different expressions of predicates and functional relations. One goal that we might strive for in the elucidation of scientific methodology would be the discovery of which methods are instrumentally reliable – that is, are a reliable guide to the construction and evaluation of the scientific theories that we accept. We might have this aim so that the methods identified could be used to construct entirely new theories. Because they are general in this way, we should expect that any particular methodology (a cluster of methodological principles and criteria) underdetermines any particular theory. Underdetermination runs from methodological principles to theories partly because even clearly defined criteria may be satisfied in many ways and also because *clusters* of criteria are involved in the construction and evaluation of theories. Even if the criterion involved in a popular principle happened to pick out a feature of the world or the mind, that principle could be mixed in with a bunch of other unjustified principles that are unrelated to

the truth, that are themselves false, or that generate false theories. Just imagine the new age religious theorist who constructs her *theory* using *a few* legitimate (and perhaps justifiable) scientific principles, but who should not be taken to have given a *scientific theory* because the methodology is mixed with mistaken principles. Perhaps we can pinpoint her error if she advances an argument that from quantum theory we can deduce that humans have free will. No such deduction is possible, but perhaps humans do have free will and perhaps only random causation can explain quantum tunnelling. Methodological principles committed to free will and random causes² may turn out to be justified. Perhaps this theory leapt the rails of science because it was constructed using some *other* principles that do not involve the satisfaction of meta-scientific criteria. I do not know what draws people into Lucretius' mistake. Perhaps it is the swerve principle: *if determinism is false, then humans have free will*. Even a principle involving a criterion committed to the correct ontology is insufficient for the construction of theories that exemplify depth, fit, predictive power, or systematicity (let alone *truth*). An ontological criterion might be involved in an example like this. It may select kinds of causes. Current interpretations of physical theory are typically committed to at least two kinds of causes: deterministic and random. It looks as if it is true that determinism is false, but even if the swerve principle were constructed using this criterion, it is surely false and its inclusion would pollute a scientific methodology. For these reasons, our epistemological investigation of the methods of science in some respects

²As I understand it, the strong force binds together the nuclei of atoms. But the strong force is a short range force and protons repel one another because they are positively charged. Spectral analysis shows that the nuclei of atoms are sometimes formed in states with energies too low to overcome repulsive electro-magnetic forces. Somehow, protons occasionally *jump* an energy barrier and 'stick' together.

parallels arguments about the ontology of theories, but deserves its own dialectical space anyway.

Some types of arguments may be in the running for showing that there is a relationship between a methodology and the world, and if there is a high degree of continuity between traditional epistemology and the philosophy of scientific methodology, then these are the same arguments that would be in the running for solving the problem of skepticism. Other types of arguments would be of the sort that, at best, they could only be employed to show that there is *some* relationship between a methodology and its user, but taken alone arguments of this type may not specify the nature of that relation. It may seem as if this is well worn philosophical territory, but the analytic treatment of simplicity itself is rare despite the obvious fact that this cuts across the perennial issues in philosophy. Now that we have a sketch about how to clarify simplicity criteria (at least of some sorts), I wish to investigate which argument forms might be employed to justify them.

We have seen that pragmatic justification is easy to come by for specific simplicity principles in specific contexts. This is the case for many kinds of psychological simplicity. For example, the clever choice of a coordinate system and its origin can contribute to algorithmic simplicity, which is useful for solving certain problems. Another kind of simplicity judgment is involved in the methods for estimating discrete integrations where iterated dependencies are expressed by the relevant equations. LeVerrier and Newcomb used methods of this sort to model planetary motions.³ Some problems cannot be solved without simplic-

³Although the example from the first chapter involves Newtonian Mechanics, Monte-Carlo and Runge-Kutta are methods of this sort developed in the Twentieth Century and which are used in a variety of scientific fields and in economics.

ity principles like these that aid in estimating the solutions to complex multi-dimensional integrals (I do not use the word ‘parsimony’ in this context because these judgments would rarely be taken to track ontology). The simplicity criteria for these principles will be chosen within some practical range so that they balance with other meta-scientific criteria like depth and accuracy. The remaining question is whether or not *all* simplicity criteria useable in science can be justified on pragmatic grounds, or if any might be justified in some other way. If the answer to this question is that it is not possible to reduce the justification of every simplicity criterion to pragmatic justifications, then something like a traditional question about justification is motivated. My view is that questions about the relation between simplicity criteria and the truth cannot be reduced to pragmatic justifications without residue. Even if aesthetic judgments play a significant role in scientific theory construction and appraisal, we might wish to know why aesthetic judgments about ontology generate theories that make good predictions.⁴

To show that some principle of parsimony is justified in the sense relevant to this investigation is to show that there is a relationship between some feature of the principle and the truth. Often the chore is taken to be that if it is claimed that some principle of parsimony that involves simplicity criterion λ is *truth conducive*, then it must be shown that the world is simple in λ respect. Put in another way, the traditional question of the

⁴An old engineering principle is, *if it looks good, then it will go fast*. Why do sports cars and fighter jets look like sharks? Probably, some sharks are *on to something* about nature that we could either imitate or discover by an application of the laws of physics. Perhaps we never hear about the companies filled with people who thought that daffodils were beautiful because their fighter jets did not perform very well. Claiming that judgments are aesthetic in nature does not bring an end to our questions for an explanation of why certain judgments generate the results that we desire. Perhaps there are even scientific explanations for why people have some of the tastes that they have.

justification of a principle of parsimony has been taken to be the question of whether the *simplicity criterion* involved in the principle succeeds in picking out a feature of the world.

This formulation of the problem is nearly, but not quite appropriate. Simplicity judgments are comparative, and therefore, it would be to commit a category mistake to ask whether or not *simplicity corresponds to the way that the world is*. In some respects, “simplicity” is like “tall” or “short”. These predicates are comparative and therefore context dependent. So, I would not ask whether simplicity is related to the truth, rather whether some sub-set of the meta-scientific criteria, those which govern ontological simplicity judgments, select objects had by the world. Again, not all simplicity criteria select ontological objects. Some select linguistic, epistemological, semantical or psychological objects for comparison. We wish to formulate the question about the justification of simplicity judgments in science by asking it about the sub-set of the cluster of meta-scientific criteria that select ontological objects.

This formulation at least points in the right direction because it avoids the confusion between comparative predicates and attribute predicates by asking whether or not any of a specific subset of the meta-scientific criteria succeed in picking out kinds of things in the world. Still, perhaps a few too many questions are begged by formulating the problem in the above way. For one thing, we wish to know whether or not *the truth* is even an aim of science (or if it is among the meta-scientific criteria). If the truth is a desideratum of science, then we would need to know what relations it stands in to the other meta-scientific criteria. Questions about what the truth is and how it is related to anything deserve volumes of focused investigation and so are out of range here. Another problem is that this

formulation does not make room for the two possible aspects of our study: the relation between a methodology and the world and the relation between a methodology and its user. If possible, it would be nice to formulate the problem so that it makes room for views that begin and end with the relation between a methodology and the its user.

It will be useful to adopt the terminology that Richard Boyd ([8]) employs to avoid begging any questions in the debate between scientific realists and scientific anti-realists. I shall follow Boyd in calling a scientific theory or hypothesis *instrumentally reliable* when its empirical predictions are approximately true.⁵ I will call a scientific methodology *instrumentally reliable* when it is a reliable guide to the acceptance of *instrumentally reliable* theories or hypotheses.⁶ This still leaves much room for further work on what kind of relation there might be between a principle of parsimony and the truth such that it is truth conducive, how many places the relation has, what the relata of the relation are, or how to account for verisimilitude. At least since Hume, philosophers have recognized how difficult is the problem of showing that there are necessary connections at all and for at least one hundred years the *makes probable* relation has met with similar difficulties.

If a cluster of principles were committed to the correct ontology and their interrelations were also in accordance with the truth (whatever that entails), then it would be no mere accident that these principles were instrumentally reliable. So something stronger than the material conditional would relate true principles to instrumental reliability. Also since something stronger than the material conditional would relate ontological criteria and

⁵This notion of *truth* is whatever the constructive empiricist has in mind when he says that theories are construed as true.

⁶Boyd (1985) *Lex Orandi Est Lex Credendi* pp.4

the interrelations between the metascientific criteria to the truth, something stronger than the material conditional relates ontological criteria and the interrelations between the metascientific criteria to instrumental reliability. Our very difficult problem, then, is to give an argument that there is some stronger-than-material-conditional relation between clusters of metascientific criteria selecting either objects of the mind or of the world and related in specific ways to one another and their instrumental reliability. Presumably, we would have a collection of instrumentally reliable theories that are taken to be paradigmatic of science. Also, we would have the methodological principles used in constructing these theories, testing them, and in preserving them in those cases where competitors were introduced. In order to proceed, I assume that it is possible to accumulate a bunch of paradigmatic instrumentally reliable theories. Perhaps this is not possible. However, many philosophers of science think that this is not only possible, but that they can identify some of these theories. The problem, then, is to show that the methodological principles employed in the construction and appraisal of these theories make these (and future) theories probable. The problem is to give an argument that justifies one of the following kinds of claim:

Principle: 4.0.1. Simplicity criteria Λ related in way R select objects of the world \Rightarrow principles of parsimony Λ, R are instrumentally reliable.

or, perhaps more modestly,

Principle: 4.0.2. Simplicity criteria Λ related in way R select structures that require theories to take certain forms \Rightarrow principles of parsimony Λ, R are instrumentally reliable.

Λ is the set of meta-scientific criteria that make ontological commitments and R

is the specific set of interrelations between the meta-scientific criteria. The relata of R include not only the criteria of Λ but some of the other criteria as well. As we have seen some pragmatical criteria may be indispensable for some methods. The formulation of 4.0.2 is meant to capture the view that some philosophers (like Swinburne) attempt to argue for. The structures that require theories to take certain forms might be found in the mind, in the world, or some combination thereof. The degree to which a philosophical view is an Idealist view might be gauged by the degree to which a view is committed to these structures being in the mind.

There is wide agreement among scientists and philosophers that some principle of parsimony is indispensable for scientific practice. If we are to give a justificatory argument, what we require is an argument that will take us from the truth of the consequent to the truth of the antecedent of either (4.0.1) or (4.0.2). If we are to deny the justification of simplicity principles in science, then arguments would need to be given that the instrumental reliability of theories constructed by methodology $_{\Lambda,R}$ have nothing to do with Λ .

I discuss five general strategies which might engage the question of the justification of (4.0.1) or (4.0.2).

1. Offer a reductive analysis of simplicity principles and show that the reduction bases are relevant to the question of justification.
2. Deny that the way that the world is has anything to do with the instrumental reliability of any principle of parsimony.
3. Offer an inductive argument where the antecedent of either (4.0.1) or (4.0.2) appears

as the conclusion.

4. Offer an argument involving an indispensability principle for the truth of the antecedent of either (4.0.1) or (4.0.2).
5. Offer an argument that the best explanation for the consequent of either (4.0.1) or (4.0.2) is that the antecedent is true.

Initially, it may appear that there is no further discussion to be had on the relation of simplicity criteria to the truth because the question about the justification of simplicity principles might be misconceived from the outset. Unless we have independent grounds upon which to settle the questions of ontology, only comparative judgments of simplicity are possible. Since simplicity judgments are comparative, it would be a mistake to ask whether some particular simplicity criterion is related to the truth, since we need only to apply the same criterion to a different set of theories to get different results. The question that we want an answer to anyway is the question of whether or not the methodological principles at work in science have any correct ontological commitments and whether or not the meta-scientific criteria have been balanced in the appropriate ways. An answer to the question of ontology would settle many other questions raised in the second chapter, like the question of how to systematize theories or how to get predictive success. Goodman pointed out that if only we had some independent way to settle the questions of ontology, many if not all of the puzzles about how to specify and how to gauge simplicity may disappear as well. Our question is whether or not a principle involving simplicity criteria can be a guide to ontology, not whether we end up with simplicity of some form as a result of having settled matters of ontology (supposing any of these can be settled) by some other means. Recall

Lavoisier's claim that a simple principle gave rise to astonishingly simple results. I do not know what Lavoisier's methodological principle was nor do I know why he was astonished. The only situation that would be astonishing would be if one kind of simplicity principle used to construct a theory regularly generated theories exemplifying a completely different kind of simplicity. Although it is surely not what Lavoisier had noticed, it would be astonishing if a principle involving a criterion of psychological simplicity gave ontologically simple results. What we wish to know is what set of relations would make something like that happen and what relation the ontological criteria involved in methodology have to the instrumental reliability of the theories they are used to construct or evaluate.

The complexity of the interrelations between the metascientific criteria raises another sort of question about justification which may still involve simplicity criteria and perhaps this paves the way to formulate the present inquiry. The question is, what if anything explains why a particular set of methods is instrumentally reliable? Clusters of simplicity criteria are involved in theory construction and testing. For a pair of theories (an old one and a newer version or two which compete to explain some data) it may be possible to establish unambiguous and non-arbitrary criteria specifying the objects of comparison and the gauges for their comparison. Simplicity criteria are related to one another and to the other metascientific criteria sometimes in a complementary way and sometimes by inverse proportionality, and sometimes they are not related to one another at all. Of all of the, perhaps infinitely many, clusters of principles that could be employed in scientific theory construction and evaluation, we wish to know if any are justified.

In order to engage this question, the analysis from the previous chapter must be

applied to actual cases of theory construction and testing. However, we can get a rough notion of what sort of cluster of methods might be the subject of justificatory arguments by using some of the results from the first chapter about the history of LeVerrier's hypotheses.

Suppose that a scientist is confronted with LeVerrier's puzzle: how to get the physical theory to give accurate predictions about the positions of celestial bodies. Suppose also that this scientist embraces methodological principles much like those that Newton appears to have promulgated: simplicity with respect to the positing of kinds of causes where systematicity is the aim, simplicity with respect to *ad hoc* causes, and simplicity with respect to kinds of properties and kinds of entities (as opposed to property instances or numbers of individuals). The theories which provide the basis for comparison are T Newtonian Mechanics and T' Newtonian Mechanics augmented with additional hypotheses. This means that the construction of T' would be constrained from moving very far from T in certain respects. By adding no additional kinds of causes or kinds of entities to T the goal of the unity of science will be satisfied and systematicity and depth will be preserved. Such a methodology contributes to pragmatical, epistemological, and semantical complexities because it is difficult to test for inter-Mercurial matter and because semantical complexities will be involved in the introduction of terms denoting additional entities in the solar system. So, we can see that our interest will be in clusters of interrelated principles, because justification is required for constructing theories that on the basis of some kinds of simplicity principles when they are always traded-off with the satisfaction of other kinds of simplicity.

If we were able to justify an instrumentally reliable methodology, and simplicity criteria were part of that methodology, we would have a set of simplicity criteria which could

be put to work in methodological principles for the construction and evaluation of theories. We would have another set of criteria that are not relevant to simplicity because complexities of some kinds are allowed or even to be expected. The justificatory argument would then be expected to give some kind of complex conclusion asserting that $\text{Methodology}_{\Lambda,R}$ is related to the truth. The conclusion may be expected to assert things like that the ontology of the causes of $\text{Methodology}_{\Lambda,R}$ corresponded to the kinds of causes in the world *or* that the properties posited by $\text{Methodology}_{\Lambda,R}$ correspond to the properties of the world *or* the generalizations of the theories generated by $\text{Methodology}_{\Lambda,R}$ express relations between the entities and properties which it posits which are *some* of the relations of the world *and* that the testing procedures used to obtain data and the theories which inform those testing procedures are also justified.

Perhaps we should not expect to get these results from an application of a justificatory argument to the methodology of LeVerrier since one of his aims was to preserve Newtonian mechanics. Hindsight shows us that Newtonian Mechanics could not be preserved and that some of the data that LeVerrier counted in favor of his hypothesis were misunderstood. For this study it may be more appropriate to investigate some of history's most secure changes (if there are any such things), like how it was that Special Relativity unified electro-magnetics and physical dynamics. Even though LeVerrier's endeavors to save Newtonian theory failed, we do have an important glimpse into how a complex set of methodological principles can contribute to the evaluation of theories from the brief historical study in the second chapter. Recall that the arguments given by LeVerrier and Newcomb established that Mercury's perihelion was a phenomenon that required expla-

nation. In the early part of the twentieth century, General Relativity's ability to explain this played a key role in the acceptance of GR. My project cannot triangulate precisely the fundamental principles of science. This project must be left to historians of science or for future inquiries. However, what should be clear is that the justificatory arguments would involve a complex set of issues including ontological justifications for the theoretical postulates of a theory and also justification for inter-theoretical relations. For this project some argument must track backwards from the truth of the consequent of either (4.0.1) or (4.0.2) to the truth of the antecedent of either (4.0.1) or (4.0.2) for whatever the properly specified set of methodological principles are given a particular set of scientific theories. To do this, there are, as noted, five strategies: give a reductive argument, deny that it can be done, give an inductive argument, give an argument involving an indispensability principle or give an argument to the best explanation.

4.1 Reduction

'Reduction' is a term that is probably used far more frequently in the philosophy of science than it is made clear. It is often thought that there is a relation between reduction and ontological simplicity. It is also common to associate successful reductions with increases in explanatory depth. Increases in explanatory depth achieved by some reductions in science are often associated with what is sometimes called *fundamentality*: the idea that quantum physics gives, provisionally, the final ontology of the physical world. However, there is no obvious reason to expect that fundamental physics will give us our only reductive bases. Perhaps multiply realizable properties (like color) are reductive bases also.

Woodward and Hitchcock, for instance, defend the view that ‘temperature’ is basic instead of ‘molecule’ and ‘kinetic energy’. The first part of this section aims to clarify different uses of the word ‘reduction’ in the philosophy of science. I do this by discussing Marshall Spector’s view about how to elucidate reduction. This involves contrasting Spector’s view with other famous attempts to elucidate reduction and also with some very different ways that the word ‘reduction’ is used in various branches of philosophy and physics.

The second part of this section investigates an epistemological motivation for reduction. The question is whether or not reductions may be performed on methodological principles involving simplicity criteria with the result that all concepts of simplicity could be reduced either to one concept of simplicity or to metascientific criteria of other kinds. I will explain why this kind of reduction would be a *conceptual reduction* instead of an *explanatory reduction*.

If all simplicity judgments can be conceptually reduced to one type of simplicity judgment, then our arguments about justification could begin with those reductive bases. Alternatively, if each unique kind of simplicity judgment can be conceptually reduced to a judgment of some sort other than a simplicity judgment then arguments about justification could begin with the reductive bases (whatever these turn out to be). Successful reductions of simplicity might at least provide our epistemological inquiry with some welcomed direction. Ultimately the fantasy might be to diffuse the problems of the justification of simplicity principles in science to other problems of justification or perhaps to draw entirely to a close our discussion of the justification of simplicity criteria by completely reducing simplicity to the other metascientific criteria. The primary argument of this section

is really Mario Bunge's argument recast with some contemporary terminology. Although reductions of specific simplicity criteria to other metascientific criteria may be possible in specific scientific contexts, no overall reduction of simplicity is possible. When this is added to the results from the previous chapter, which show that simplicity criteria can be made clear and that many do play important roles in theory construction and testing, we are forced to take seriously the question about the justification of simplicity.

4.1.1 Clarifying Reduction in Science

Marshall Spector ([56]) has argued that reduction is best elucidated by the notion of a term-by-term replacement involving two different vocabularies. On this view of reduction, metalinguistic functions may define the terms of a reduced vocabulary in terms of a reducing vocabulary. This view is defended on the basis that it avoids several problems faced by the *standard analysis* given by Ernest Nagel. [47] ⁷ Nagel's attempt to elucidate reduction in science involved the explanatory reduction of laws to other laws that explain overlapping sets of data. Spector's approach to reduction is more appropriate than the standard analysis for an application to the metascientific criteria. This is because the metascientific criteria are not any more a part of physical theories than a metalanguage is a part of an object language, and whatever might be explained by a reduction of some set of metascientific criteria to other metascientific criteria should not be understood as having been explained in the same way that a theory is said to explain data or in the same way that one theory explains another.

⁷Nagel's account of theoretical reduction met with many objections. Some of the problems with Nagel's approach will be outlined. See Spector's ([56]) Chapter 3 for a detailed discussion.

Spector traces the genesis of the modern discourse on reduction to “Russell’s Maxim” which was presented in the essay *Logical Atomism* (1924). The maxim states that whenever possible, constructions involving known entities could be substituted for unknown entities. Russell and Whitehead “found by experience”[56] that this principle could be applied to mathematical logic and suggested its application for other areas.

It is fairly easy to understand the motivations for this kind of substitution in some cases involving applied mathematics. Take, for example, imaginary numbers, which regularly appear in physics and engineering contexts. When a convex automobile side-view mirror is designed on paper we find that a virtual image is projected into the complex numbered plane (inside the mirror). It might be thought that it presents a troubling paradox or an outright contradiction to say that imaginary numbers are involved in predictions about the real world – at least in this case, about an image with real numbered dimensions. The idea is that imaginary numbers can be defined using functions that only involve only real numbers. This reduction carries ontological implications since the result suggests that we need not reify imaginary numbers, at least in the case of mirrors.

The motivations for ontological reductions in the philosophy of science are usually a bit different from the motivations in the above example though. One tempting application of this kind of reduction is to reduce “unobservables” to “observables”. Perhaps Berkeley’s view that it is a *manifest contradiction* to assert that what is unobservable exists is a view of this sort. Modern arguments in the philosophy of science have cast serious doubt on whether the observable-unobservable dichotomy can be maintained. One of the reasons for suspicion about the observable-unobservable dichotomy is given in the previous chapter’s

discussion of Hempel's *Theorician's Dilemma*. Deep scientific explanations end up being a mixed bag of predicates denoting bread-box sized objects and theoretical terms.

Having noticed problems of this sort, we might consider ontological reductions. Perhaps we adopt a philosophy of language on which our theoretical terms refer to the most basic entities that we know of in the web of causes and effects and seek reductions that go in a different direction, from macro-objects to micro-objects. This kind of approach to reduction has been investigated for possible application in several adjacent areas in the philosophy of science, including projects to elucidate the scientific progress exemplified by the replacement of older theories with newer ones that account for the same data. This approach to reduction may also cast light on attempts to understand how successful theories are viewed as deep or how theories satisfy the unity of science criterion.⁸ Here, again, the early critics of the Positivist program (Bunge, Goodman, and Hempel) offer useful insights. As Goodman has argued, the notion of simplicity relevant to science is a notion of economy rather than one of mere linguistic simplicity, and this notion is related to systematicity which provides a basic criterion for theory construction. It is tempting, then, to take the fundamental particles and their properties (perhaps protons, neutrons and electrons with parity, spin and charge, or quarks with charm) as our reductive bases. Even so, if the special sciences are autonomous, then no such reduction is possible for every scientific statement.

Perhaps it is now clear why the discourse about reduction in the philosophy of

⁸Reduction (term-by-term replacement), explanation, and the goal to unify scientific knowledge come together in Philip Kitcher's *Explanatory Unification* (1981). Kitcher says that "unification is achieved by using similar arguments in the derivation of many accepted sentences" (pg.519).

science is as complex and diverse as it is. There are different notions of reduction that are motivated in different ways and standing in diverse relations to other issues in philosophy more broadly. It is often, but not always the case that the motivations for reduction are ontological. Accounts of reduction may differ in what is taken to be the reductive basis: micro-physical entities or properties, macro entities or properties, particulars or abstracta.

Perhaps we can also see why confusion about reduction in science is easy to come by. First, there are many distinct ways to consider reduction in science and some of these may be incompatible with one another, depending on what kind of account of explanation is embraced. Second, the word ‘reduce’ is often used differently by philosophers and scientists. Let us consider a few of the very different ways that this term has been used.

It is popular to say that Kepler’s laws of motion were *reduced* to Newton’s laws of motion and it is popular to say that thermodynamics was reduced to classical mechanics. It is true that Kepler’s laws are *derivable* from Newton’s laws *plus simplifying assumptions* (this last part is often omitted, contributing immediately to error). It is not true that Kepler’s laws are strictly derivable from Newton’s laws. This is one of Kuhn’s important insights in *The Structure of Scientific Revolutions*([41]). Kepler’s laws predict elliptical orbits for all of the planets in a solar system and they predict that the planets will sweep out equal areas in equal times. In order to get these results from Newtonian mechanics one must discount several important relations described by Newtonian mechanics including: the gravitational interactions between planets and other planets, and the fact that planets are not perfect spheres with evenly distributed densities.⁹ When our predictions about celestial mechanics

⁹Even though Newtonians model masses as point masses, some very tricky integrals may be

require a high degree of precision these simplifying assumptions are no longer appropriate. This was the situation for LeVerrier and Newcomb given the scientific climate they found themselves in. At a high degree of precision, Newtonian orbits are not Keplerian orbits. Additionally, the first chapter discussion of Newcomb's work explains why it is the case that celestial motions are not derivable from Newtonian mechanics. So if it is the case that the concepts of the large scale phenomena of planetary orbits are reducible to the fundamental laws of Newtonian mechanics then *deducibility* is not the relation upon which this reduction depends.

A similar problem arises for the popular example of the reduction of thermodynamics to classical mechanics *plus molecular theory* (this last part is often omitted, contributing immediately to error). The usual example is that temperature is reduced to a function of the masses and velocities of the molecules of a system or, perhaps more precisely, that temperature is reduced to mean kinetic molecular energy. However, the laws of thermodynamics are not *derivable* from the dynamical laws involving molecules. The laws of thermodynamics apply to systems composed of things smaller than the molecular theory includes (like plasmas or electron gasses). A plasma, for example, has a temperature although it is not composed of molecules. If *molecules* or *atoms* are taken to be in our reductive bases, then temperature is multiply-realizable because systems composed of things much smaller than molecules or atoms have temperatures. Again, perhaps deduction is not the relation essential to reductions.

employed to get very accurate descriptions of some of the behavior of planets, including wobbles (or nutations) in the precession of the planets.

This rough sketch of these examples helps to raise several key points. In the case of the mathematical reduction of imaginary numbers to real numbers the motivations can be made clear. One motivation is ontological. Another motivation is psychological (algorithmic). Hempel has stated that the philosophical motivations for scientific reductions are ontological also. However, when we seek the precision afforded by a linguistic analysis we may say nothing about ontology, rather only something about how it is *possible* to define a set of terms ([30] pg. 195-196). The discussion of Goodman's analysis of the defining power of terms raises the same issue. Both of these authors would probably agree that they have shown that the Positivist's rejection of metaphysics does not end up being justified. I believe that Hempel's arguments against the Positivist's hostility toward metaphysics contributed to a common contemporary view that places metaphysics back at the forefront of inquiry, especially involving the ontology of theories. Similar arguments place metaphysics at the forefront of our investigation into scientific methodology, which I believe deserves some dialectical separation from questions about how to elucidate *scientific progress* or *scientific explanation*. We might also ask what the motivations for a philosophical dialog about reduction in science are. Is it the case that we are convinced that reduction takes place in the sciences and we are searching for paradigmatic examples of it? Is it the case that we think that there is progress in science and we wish to elucidate the notion of *progress*, hoping that some reductive analysis will bear fruit? Do we have a notion of reduction, and we are looking for uses for it in the same way that mathematicians are currently exploring the utility of model theory? These motivations are all very different and so should be kept clear.

At least the leading authors who contributed to the reduction in science dialectic had some specific things in mind. Ernest Nagel sought to elucidate reduction when he gave a detailed discussion of thermodynamics. Nagel held that reduction was explanatory and endeavored to fit the analysis of reduction to the deductive-nomological model of explanation. Spector holds that heterogeneous reductions *can* be explanatory and he employs the above examples to show that reductive explanations need not depend upon deduction. Spector attempts to split the horns of Hempel's linguistic-ontological dilemma by claiming that term-by-term concept replacement is the better way to elucidate reduction than by deductive arguments. On Spector's proposal the ontological impetus for reduction may be preserved, and by expressing the reduction in the metalanguage linguistic precision is preserved. In this way, reductions may be explanatory, but not in the same way that scientific laws explain data.

One final example will help to clarify uses of the word 'reduction' in the philosophy of science discourse. We must not confuse 'reduction' with the phrase 'reduces to'. Physicists say that relativistic physics *reduces to* classical mechanics as $\frac{v}{c} \rightarrow 0$. This kind of reduction would be a homogeneous reduction. This kind of reduction is called *homogeneous* because the reduced theory does not contain different terms than the reducing theory, although the reducing theory may contain different terms than the reduced theory. Deduction may play a role in this kind of reduction if a principle of the following sort is embraced at the methodological level: If error constraint α is not met or exceeded and $\frac{v}{c} \rightarrow 0$, then replace the laws of relativistic mechanics with the laws of classical mechanics. In this sense of the word 'reduce' a similar principle might govern the replacement of the laws

of quantum mechanics with the laws of classical mechanics when values for momentum are much larger than Planck's constant h : $\frac{h}{p} \rightarrow 0$. Useful points can be drawn from this example. Physicists may use the term "reduce" in a way that is quite different from the way in which philosophers use the term and this can contribute to confusion if the two uses are not kept clear. Deduction may play a role in some kinds of reduction. However, the homogeneous kind of reduction given in this example is neither explanatory nor ontological in nature. This kind of reduction is not explanatory because classical mechanics does not *explain* relativistic physics. Classical mechanics is not merely a less accurate way to do relativistic calculations. It is false. The first chapter discussion of Newcomb's work shows why classical mechanics cannot account for some data. This kind of reduction is a pragmatic notion which indicates that approximations are allowed in some contexts. Finally, the key notion involved in this kind of reduction is not a notion involving term-by-term replacement. Rather, the relevant notion involves the replacement of the laws of one theory with the laws of another.

4.1.2 The Conceptual Reduction and Simplicity

There is another application of Russell's Maxim which ought to be considered for the goals of this chapter: the conceptual reduction. We have many examples of this kind of reduction from applied science and engineering where different kinds of systems are modeled using the same mathematics because the descriptions of the systems share certain formal characteristics. It is common to design and diagnose automotive electrical systems on the hydraulic model of current flow. Also, acoustical systems may be modeled using

electrical circuits and disease propagation may be modeled using the same equations that model radioactive decay. In each of these cases two different systems share certain structural features that makes it possible to swap the terms or concepts of one discipline for the terms or concepts of another in the very same equations. This kind of reduction is a *conceptual reduction* rather than an *explanatory reduction* because it is certainly not the case that hydraulics *explains* electricity or that disease propagation explains radioactive decay. Obviously, this kind of reduction is useful. It does not, however, provide any explanation of the sort relevant to scientific explanation. If conceptual reduction provides any explanatory insights they are insights about minds, either human or divine.

Let us now see if simplicity principles in science share sufficient structural similarities with other principles so that a conceptual reduction might be possible. It would be nice if an abstract analysis of the conceptual reduction of metascientific criteria were possible. But it is not because simplicity criteria are radically heterogeneous and they stand in unique interrelations with one another. Conceptual reductions of specific principles to principles involving other metascientific criteria may be possible. Recall that Bunge suggested that in some cases, a principle of parsimony that appears to select ontological objects, or at least to have ontological implications for theory construction, might actually serve an epistemological role. Bunge said that

Unnecessarily complicated assumptions and theories should be avoided; that is, hypotheses and theoretical systems employing inscrutable predicates, such as ‘Providence’ and ‘collective unconscious,’ should be shaved with Occam’s razor. Notice, however, that Occam’s razor does not hang in the air, but falls under the more general rule, “Do not propose ungrounded and untestable hypotheses([11] pg.129).”

I will follow up on Bunge’s suggestion that conceptual reductions of specific prin-

ciples may be possible by reviewing the study of the arguments given by LeVerrier. Recall that LeVerrier constructed several hypotheses initially aimed at explaining Mercury's perihelion. He rejected each because they could not account for Mercury's perihelion without also suggesting other data that was incompatible with the body of collected data. An inter-Mercurial planet and inter-Mercurial asteroids with mass sufficient to account for Mercury's perihelion would be observable. Inter-Mercurial matter either would not account for Mercury's perihelion or would be observable. It seems plausible that these hypotheses were rejected because they did not fit the data. I assume that these hypotheses were *rejected* because they failed to satisfy a straightforward empirical criterion. However, these hypotheses were *constructed* using additional criteria, and this tells us much more about LeVerrier's methodology. After all, LeVerrier did not posit some kind of ether pulling on Mercury, and he did not attempt to alter Newton's law of gravitation. This suggests that LeVerrier may have constructed hypotheses by employing some kinds of simplicity principles.

It seems quite clear that a scientist who constructs an *astroid* hypothesis to explain some data does not include in his methodology a criterion of simplicity with respect to *numbers of entities*. He may have held principles that involved simplicity with respect to *kinds* of substances though. He may also have held a principle of syntactical simplicity which could account for the fact that he did not wish to explain Mercury's perihelion by adding terms to Newton's law of gravitation. However, it is also possible that he had a more fundamental principle that committed him to the unity of science. Perhaps simplicity with respect to kinds of causes and syntactical simplicity can be replaced with a unity of science principle with the same results. My review of LeVerrier's work makes this seem plausible

also. Obviously, settling which, if any of these, was the case is the job for historians of the philosophy of science, but I do think that this shows that conceptual reductions of *some* simplicity principles to principles involving other metascientific criteria are possible. If conceptual reductions of specific methodological principles are possible, we still know nothing about which principles are more fundamental. For this additional arguments are required.

Let us consider a different approach to the conceptual reduction of simplicity criteria. Perhaps the most promising approach is to forget about theories for a moment and to explore the possibility that all kinds of simplicity are constructed in the mind on the basis of one type. It is natural to speculate that people originally get some idea of simplicity through experience and then annex that idea to other cases, thereby acquiring the entire stock of simplicity criteria which govern judgments in the mature mind. Perhaps as a child I lifted two objects and felt that one offered less resistance than the other. The experience might have provided the basic inequality from which the mind abstracts the form of the comparative simplicity relation. This form might be applied to other kinds of cases. Perhaps some skill is more difficult to learn than another or perhaps one sentence consists of more terms than another and in both of these cases the mind borrows the general *less than* relation from previous experience and applies this relation to the new cases. On this story, simplicity criteria would select the objects of comparison for special cases of judgments of inequality. For all I know, this could well be the way that the mind acquires a mixed bag of simplicity criteria.

However, in any particular case of theory construction or evaluation I will have to

call upon several different judgments of simplicity. Some of these criteria are complementary; syntactical simplicity gauged in a certain way may be compatible with ontological judgments of simplicity. But some of these judgments are always incompatible with others. Syntactical simplicity is inversely related to epistemological simplicity. This shows that when we are confronted by any particular case involving simplicity judgments there will be gains and there will be losses in simplicity depending on which criterion is under consideration. Perhaps it is true that simplicity concepts share structural similarities, but when we consider clearly defined simplicity criteria in science, no conceptual reduction of all of the kinds of simplicity criteria to just one type is possible because contradictions will arise.

The question about how human minds come to have their ideas is a different question from whether or not the methods of science are reducible to one another in specific cases. They are both important questions. But when it comes to the methods of science, no conceptual reduction of simplicity criteria is possible. Simplicity, as Bunge has argued, is radically heterogeneous. This fact, the fact that complexity of some sort is always traded with simplicity of another sort, is cast in the very grammar of science as Aristotle advanced it. We may lean a bit more on the Pythagorean notion of simplicity, as Aristotle may have, at the cost of some syntactical and ontological complexities, positing many terms of the “stay-the-same-sort” and several mechanisms of causation. Or we may lean on the Heraclitian notion a bit more like Kuhn and Laudan, holding that more things change than stay the same; where simplicity is gained by positing ‘flux’, we incur syntactical and semantical complexities when we give names to all of the things which change.

Some of the problems for contemporary literature on Bayesian and Akaike information criteria are laid bare by these observations. Some authors are overtaken by the idea that traditional problems of ontology are dissolved by Akaike's equations, which relate accuracy and syntactical simplicity with predictive power (or testability). The database rendering from the previous chapter shows us that it is not surprising at all that these are related to one another. These curve fitting algorithms suggest that we can reduce simplicity judgments in theory selection to syntactical simplicity and epistemological criteria. However, not all of the problems of ontological simplicity in science are eliminated in this way. Syntactical simplicity is not the only kind of simplicity relevant to theory construction and testing. Given the extreme heterogeneity of simplicity concepts, no overall conceptual reduction is possible. It matters not what reductive basis is chosen. There will always be a trade-off between some kinds of simplicity and others. We require some guide to acceptable accuracy constraints in order to know what counts as empirically adequate in any particular case. For this, we must appeal to *depth* and this raises the ontological question again because, as we saw in the previous chapter, must take some un-operationalized primitives in order to satisfy the formal requirements of depth on balance with the other metascientific criteria.

It is intriguing to consider cases where conceptual reductions of specific simplicity principles may be available so that a criterion that makes an ontic suggestion may be stated in other terms. Speculating about this option, I would say that it would probably require a very robust anti-realist account of science, so that the instrumental reliability of the relevant methodological principles could be accounted for in terms of pragmatic, aes-

thetic, and social goals. The reason that this is difficult to imagine at this stage is because well constructed scientific hypotheses satisfy several metascientific criteria at once. It is important to rid ourselves of the Positivist notion that empirical adequacy is the only relevant metascientific criterion. For one thing, *empirical adequacy* does not appear to be a strictly empirical criterion. There is a trade-off between accuracy and predictive power – a trade-off which is balanced by appeal to the other metascientific criteria, some of which suggest ontology. For another thing, hypotheses balance several different criteria: accuracy and predictive power are traded for one another when notational simplicity is adjusted this way or that, and as certain epistemological simplicities are traded for abstractions, and all of this must be achieved without sacrificing depth. It will also be very difficult to find cases where some unity of science goals do not play a significant role in theory construction or testing. It seems, then, that the first salvo in this battle must be fired by the historians of science. In any case, I do not expect examples to arise that change Bunge's insight that simplicity is radically heterogeneous and that no overall reduction is possible.

My strategy, in this section, was to rule out the possibility that the questions about the justification of ontological simplicity criteria can be eliminated by reductive analyses. First, the term 'reduction' is subject to vagueness. Second, it is used in the philosophy of science in several ways. It is only conceptual reduction that might play a role in our present inquiry. Bunge's arguments show that no overall reduction of simplicity in science is possible. However, it might be possible to conceptually reduce some simplicity principles to others or to methodological principles that do not involve simplicity criteria. However, if conceptual reductions of specific principles are possible, we know only something about the

structural similarities of the principles. We do not know which are more fundamental. All of this shows that the *possibility* of conceptual reducing simplicity principles cannot defeat an investigation into the justification of simplicity judgments. However, I believe that the preceding discussions have motivated the question of justification. Hempel's arguments show that we cannot give a purely empirical elucidation of scientific theories. Ontological criteria will serve in principles that make theories deep.

4.2 Denial

I am not sure who would deny that at least some simplicity principles are instrumentally reliable. Even if a mature theory migrates far from its early form by acquiring many additional terms and relations, or by shifts in the semantics, it is still the case that syntactical simplicity, in perhaps various forms, aids in the early stages of theory development by achieving systematicity and testability. It is still the case that scientists seek to balance the trade-offs between notational or algorithmic simplicities and scrutability. It would seem, then, that even the die-hard pragmatist would be inclined to accept that some simplicity principles are instrumentally reliable. The question ought to be as it has been cast here: is the truth related to any of the metascientific criteria?

But there remain two possibilities for denying that arguments can be given to justify simplicity principles. One might take simplicity as a methodological primitive which cannot be justified, or, as suggested in the previous section, perhaps some simplicity principles can be conceptually reduced to other criteria given a sufficiently robust anti-realist account of science. Alan Baker explains these options in the *Stanford Encyclopedia of Philosophy*

entry on simplicity,

Some philosophers have approached the issue of justifying simplicity principles by arguing that simplicity has intrinsic value as a theoretical goal. Sober, for example, writes:

Just as the question ‘why be rational?’ may have no non-circular answer, the same may be true of the question ‘why should simplicity be considered in evaluating the plausibility of hypotheses?’ (Sober 2001, p. 19).

Such intrinsic value may be ‘primitive’ in some sense, or it may be analyzable as one aspect of some broader value. For those who favor the second approach, a popular candidate for this broader value is aesthetic. Derkse (1992) is a book-length development of this idea, and echoes can be found in Quine’s remarks in connection with his defense of Occam’s Razor concerning his taste for “clear skies” and “desert landscapes.” In general, forging a connection between aesthetic virtue and simplicity principles seems better suited to defending methodological rather than epistemic principles.([?])

Baker goes on to say that,

Kuhn (1977) takes this line, claiming that how much weight individual scientists give a particular theoretical virtue, such as simplicity, is solely a matter of taste, and is not open to rational resolution. ([?])

I agree with Baker. It would seem that the reduction of simplicity to aesthetic values would better suited to a defense of methodological principles than epistemic principles. But our previous discussion shows how these views are a bit misguided anyway. Bunge has successfully argued that simplicity is radically heterogeneous. No overall reduction is possible. Even if one kind of simplicity were conceptually fundamental, we would need to know which one this is. So, I cannot see how it would work out that simplicity is conceptually fundamental as Sober suggests that *it* might be. The word ‘it’ does not refer to just one thing in the philosophy of simplicity. The same problem arises for attempts to reduce simplicity judgments to aesthetic values. This reduction is just impossible. We have shown already that many of the simplicity criteria that aid in the construction and appraisal of theories are justified on pragmatic grounds.

Kuhn's claim is not sufficient to deny the justification of all of the different kinds of simplicity principles in science. Probably, Kuhn did not intend to deny the *instrumental reliability* of some methods, at least at certain stages of history. Also, Kuhn was probably thinking only of a kind of ontological simplicity and imagining that scientists reflect only their tastes for certain ontological commitments in theory construction and testing. On Kuhn's account, scientists would acquire these tastes early in their careers from text books and teachers. Kuhn is right to criticize text books that run together different theories from different ages in a misguided attempt to give to students a holistic picture of *progress* in science. My own astrophysics textbook freely switches between Keplerian laws and Newtonian laws of motion without mentioning the almost comic fact that Kepler thought that he was unifying the laws of celestial motion with the laws of musical harmony! I am as unsettled as Kuhn is about this sort of thing. It is not just that no reductive *explanation* will account for the replacement of Keplerian laws of motion by Newtonian laws of motion, but no *conceptual* reduction is possible because Kepler's ancient Pythagorean notions of causation are totally different from Newton's notion of causation. The two theories are totally different and they do not even share structural similarities. I agree with Kuhn that it is either naïve or irresponsible of authors to subject students to this sort of confusion.

Although I share with Kuhn these concerns, his claim that simplicity is solely a matter of taste does not account for the sort of thing that Goodman and Bunge have pointed out, that in the early stages of theory construction, economy of form is desirable because it contributes to systematicity and testability. This is not *merely* a matter of taste and it is open to rational resolution. Really all that Kuhn has pointed out is that there is a problem

justifying how incompatible measures of simplicity are to be weighted. But the gauging problem is different from the counting problem. Each require specific justifications for specific applications. This same point is made by Sober in the article cited by Baker,

Several philosophers have asserted, without providing much of a supporting argument, that the trade-off problem [the problem of how to weight simplicity measures against the other metascientific criteria] has no objective solution. For example, Kuhn (1977) claimed that scientists differ in how much importance they assign to one virtue of a theory as opposed to another, and that this difference is just a matter of taste. One scientist may think that the most important demand on a theory is that it should make accurate predictions; another may hold that the first duty of a theory is that it be elegant and general. Kuhn does not offer much of an argument for this claim; it merely constitutes his *impression* of what would be left open by any compelling and reasonably complete set of epistemological standards. Of course, it is not in dispute that scientists have different temperaments. But from the point of view of normative theory, it is far from obvious that no uniquely best trade-off exists between simplicity and the other factors that affect a theory's plausibility. Once again, this cannot be settled in advance of examining particular proposals [about how to measure simplicity]. ([55])

The better approach is to deny that the world being any sort of way *explains* the instrumental reliability of methodological principles involving simplicity criteria. For this project, I leave it to the defenders of scientific anti-realism to marshal their arguments. It is important to keep in mind the serious challenge faced by those who wish to say anything about the instrumental reliability of simplicity criteria in scientific methodology. The problem is that one method can give different results when a theory *A* is compared to a theory *B* and when the same theory *A* is compared to another theory *C*. Simplicity judgments are comparative. The problem of specifying a suitably abstract methodological principle that may have its instrumental reliability either explained or explained away is faced by both scientific realists and anti-realists. Also, there are two two distinct problems which require justification once they are suitably analyzed: the counting problem and the gauging

problem. These problems have been offered many different rigorous formulations. These are employed by scientists in a wide variety of fields every hour of every day. Typically though, the modern scientist just *points and clicks*, interfacing with some software program, to classify data and fit curves. Rarely does the practicing scientist ask what method was used (nearest-neighbor, decision trees, linear regression, neural net, BIC, AIC, or some combination). Since the scientist rarely asks which method her software program employs, she rarely asks whether or not different results would be obtained by switching methods. Sometimes the results are different, and this question ought to be asked. Once we realize that different methods can give different results the question of what justifies any particular method is once again raised. The “it’s always worked for me”, mantra of normal science, is typically not an inductive justification, but an appeal to pragmatic values aimed at putting to rest the pesky philosopher’s inquiry about justification. However, the mantra is an inhibitor when what Kuhn calls periods of *crisis* demand changes in our methods.

My hope is that attempts to deny the possibility of the justification of simplicity principles in scientific methodology has run its course in philosophy. Perhaps it is natural for this dogma to die a few publishing generations after the death of Positivism. Hempel and others argued that the Positivists could not justify a wholesale hostility to metaphysics. Additionally, the very notion that what distinguishes science from other human activities is its ability to bring about wide-spread agreement between its skilled practitioners inspired attempts to explain or to explain away the idea of progress in science. This idea about science is misguided also. I suggest that the clusters of metascientific criteria that govern activities of various sorts are different and this is sufficient to distinguish activities from

one another. The metascientific criteria differ from the metamusical criteria and this is sufficient to explain the unique paths that different human intellectual endeavors take.¹⁰ Kuhn is trapped in a post-Positivist kind of view, that simplicity reflects ontology. In some cases this is true. In some cases it is not. I wish to revive the idea that arguments are required for our discussion of the justification of simplicity judgments in science. Perhaps arguments can be given to justify this or that kind of simplicity in science. Perhaps, as Sober suggests, some kind (or kinds) of simplicity are conceptually fundamental and we cannot justify them with arguments, but we still require arguments to distinguish the conceptually fundamental kinds of simplicity from those that are not.

Finally, I believe that the analysis of the metascientific criteria and their interrelations suggests something that, for whatever reasons, has not fallen squarely into the limelight. Although several arguments from the twentieth century reignited a discussion about the ontology of theories, the point that we must also discuss the ontology of scientific methods seems to have slipped just below the radar of many (but not all) discussions. Perhaps this is due to the fact that the philosophy of scientific methodology has not been given its own dialectical space. It is clear that Paul Churchland (discussed in the next section) thought of this and employed the idea in an argument against scientific anti-realism. How-

¹⁰Although musicians and composers clearly have their own unique styles, these styles may arise from the various ways that each individual satisfies widely agreed upon metamusical criteria. For example, it may be widely accepted that we prefer non-parallel harmonic motion and that this criterion is only sacrificed when special effects are desired. In other words, there may be a trade-off between the metamusical criteria. We may find that there are syntactical simplicity criteria in music, but it is not likely that the specific gauges of syntactical simplicity in music are the same as those employed in science. A contrasting difference between science and music is probably that depth is not among the metamusical criteria. Although some explanation may be, in principle, available for what makes a half diminished chord *well-placed*, a chord's being well-placed does not explain anything.

ever, this does not show us precisely why considerations of ontology may be unavoidable in the elucidation and justification of methodological principles. The string of reasoning that leads us here is rather complex in its details, but we ought to be in a position to list the relevant general topics. 1) Methodological principles involve criteria. 2) Criteria select objects. 3) The metascientific criteria are radically heterogeneous, so no over all reduction is possible even for a particular genus (say simplicity). 4) The metascientific criteria stand in specific set of complex interrelations. 5) Any attempt to make one genus of the metascientific criteria conceptually fundamental faces either the problem of the irreducibility of the metascientific criteria, or leads to methods that commit us to unacceptable sacrifices in how the others are satisfied. This final point is exemplified in several different problems that have been discussed like the linguistic-ontological dilemma or the problems involved in an obsession with epistemological simplicity. In short, we do not define the metascientific criteria in terms of one another. We balance them against one another, and our methodological choices have ontological consequences.

This is not merely a restatement of Hempel's dilemma. The arguments about the justification of methodological principles is an issue distinct from questions about the ontology of theories. Again, consider that perhaps we can elucidate a unity of science criterion epistemologically - that embracing it involves aiming to unify the encyclopedia of scientific knowledge. Still, theories constructed with a unity of science principle may make ontological commitments (or resist them) like *not* positing invisible frictionless substances or changing the laws of nature. This ought to motivate a genuine discussion about the justification of methodological principles. The study of ontology and the questions of tradi-

tional epistemology do not apply to theories alone, but to methodologies as well. In some cases the questions of traditional epistemology do line up with the questions about the justification of methodological principles because methodological principles often make ontological commitments.

4.3 Induction

In this section, I argue that inductive justifications of simplicity criteria in science do not fail because of any obvious formal problems. Still, I do not favor this approach because I think that it is doomed to fall short of the aims of this project.

Induction was discussed briefly in the first chapter, using an example from a Sherlock Holmes novel. What we might expect for the justification of some specific cluster of methodological principles would be a collection of case studies involving those methods and the instrumentally reliable theories that they were employed in constructing or evaluating.

1. Past Observation Set 1: Methodology $_{\Lambda,R}$ was employed in constructing and evaluating T_1 , and T_1 is instrumentally reliable.
2. Past Observation Set 2: Methodology $_{\Lambda,R}$ was employed in constructing and evaluating T_2 , and T_2 is instrumentally reliable.
3. Past Observation Set 3: Methodology $_{\Lambda,R}$ was employed in constructing and evaluating T_3 , and T_3 is instrumentally reliable.
4. On the basis of uniform past experiences, I have come to expect that Methodology $_{\Lambda,R}$ gives instrumentally reliable theories.

Therefore: It is probable that using $\text{Methodology}_{\Lambda,R}$ to construct and evaluate T_4 will result in T_4 being instrumentally reliable. (just like in every other case that I have observed)

Now, Λ is a set of simplicity criteria. The fourth premise involves the phrase, “on the basis of uniform past experiences” and this involves a *uniformity of nature* criterion which is often noted to be a simplicity criterion. This immediately raises the question of whether or not this argument is circular. It is far from obvious that this is the problem with this argument.

4.3.1 The Question of Circularity

Swinburne raises an objection to the circularity of inductive arguments employed to justify simplicity criteria. Swinburne says that

the claim was only that usually the simplest theory has proved the better predictor, and that would justify the assertion that probably a given simplest theory will prove the better predictor on some future occasion.

However, even this modest claim about the history of science – that usually the simplest theory has proved the better predictor seems very doubtful. In many areas of inquiry the simpler “laws” which served well in the past have been replaced by more complicated laws – the Boyle to Van der Waal’s story is hardly unique. But even if the simplest theories have usually proved better predictors, this would not provide justification for the subsequent use of the criterion of simplicity, for the reason that the justification itself relies on the criterion of simplicity. There are different ways of extrapolating from the corpus of past data about the relative success which was had by actual theories and which would have been had by possible theories of different kinds, if they had been formulated. “Usually simplest theories predict better than more complex theories” is one way. Another way is an extrapolation of the form “Usually theories formulated by the Greeks in the bath, by Englishmen who watch apples drop, or Germans who work in patent offices...etc, which initially appeal to the scientific community predict better than other theories.” An extrapolation of this kind, spelled out at great length, would yield the data of the past history of science just as well as the extrapolation which connects predictive success with simplicity. Of course, this kind of extrapolation is an absurd way of extrapolating from the past data concerning the relative success of theories.

And why is it absurd? Because it is far less simple than the obvious way of extrapolating. We have assumed the principle in providing its justification! Any other purported empirical justification of the use of the criterion of simplicity will prove equally circular. ([59] pg. 52-53)

Swinburne has not yet made his case. Λ is a set of the heterogeneous metascientific criteria specifying a variety of objects for comparative judgments. So long as Λ does not include a unity of nature criterion, this argument does not employ the principle of the uniformity of nature in the making of the inference. Perhaps Λ consists of syntactical and epistemological simplicity criteria. These might specify numbers of logical connectives in a sentence and the degrees of accessibility to a term's denotation. So long as all we wish to give is an argument that past experience makes it probable that Λ will generate instrumentally reliable theories, then it would seem that this is what we have. Swinburne knows that there are many different simplicity criteria. This will be made clear in my discussion of his view in the next section. Since Swinburne knows this, it is not quite clear why he uses the word "criterion" in this passage, although I have some suspicions about it which are my next topic of discussion.

4.3.1.1 Sameness and Change

Although Plato did not wish to do philosophy of nature, probably due to his view that sensations are merely illusory images of what is real, Plato does appear to be the father of the middle way between Pythagoras and Heraclitus. Aristotle founded science upon this middle way and now we are stuck with it. I cannot see how to avoid accepting that some things change and some stay the same. I cannot see how to reconceive of the scientific project involving attempts to give both explanations and predictions about the

regularities in nature. Nietzsche, with his notorious eye for irony, sees *this* pattern and its problems echoed throughout philosophy. Scientists have a serious challenge giving deep explanations about the regularities in nature. Another serious challenge is the one faced by philosophers, and sometimes scientists acting as philosophers, to categorize the things of the stay-the-same sort and the things of the change sort. Perhaps Swinburne's objection reflects the many problems of induction. Swinburne says that "There are different ways of extrapolating from the corpus of past data..."([59] pg.52)

Swinburne may be pointing to the fact that simplicity criteria may be involved in categorizing the relevant terms of the stay-the-same sort. If the principle involved in this is the principle of the uniformity of nature, then indeed this argument is circular. Since we have ways to formally analyze many different kinds of simplicity criteria, it seems to me that we could discover whether this is the case. In order to avoid the circularity, though, all that is required is that none of the criteria used to sort predicates into classes or to govern the inductive inference are members of Λ . This does not mean that induction would justify every principle of simplicity. For all we know, the criteria employed in the sorting of predicates and the criterion of induction could be justified by some other arguments (perhaps some indispensability argument). If that were the case, then the inductive argument would have no formal problems.

Swinburne probably notices that the problems about how to fill out the details of the Pythagorean-Heraclitean mixed principles are fundamental problems. Hume noticed the same thing. I see the problem, but it is far from obvious that an attempt to inductively justify the highly specialized, highly formalized syntactical simplicity criteria employed in

curve-fitting algorithms (for example) would employ those very same criteria to govern the inductive inference. In a sort-of unsatisfactory way, the radical heterogeneity of simplicity comes to the rescue of induction, at least in this very specialized area of inquiry, because it is not obvious that an inductive justification of simplicity principles in science is viciously circular. However, induction itself still depends upon some justification of the simplicity criteria that govern it.

This result is interesting, and perhaps it is a game-changer for some controversies in the philosophy of science. It might be true, as Swinburne expects, that answers to the justification of simplicity in science await the skeptic to be given a final answer. However, we should not expect the arguments meant to justify simplicity in science to be the very same arguments aimed at answering the skeptic. The degree to which the philosophy of science has inferential criteria in common with classical epistemology I will call the *degree of continuity* between these disciplines. I do not know how much continuity to expect, but the radical heterogeneity of simplicity suggests that there is *some* discontinuity. If this is so, then we would not answer the skeptic with the same argument that justified some scientific methodology.

4.3.1.2 Reasons Not to Use Induction

Perhaps my reasons to be suspicious about the possibility of giving successful inductive arguments to justify simplicity principles in science are related to Swinburne's. Induction does not seem capable of telling us what we want to know in this case and I do not see how to reconcile an inductive argument for the justification of simplicity with

changes in science. I do not agree with Swinburne that inductive arguments justifying simplicity in science are circular, but I do agree that they do not get at the epistemologically fundamental issues.

First, let us suppose that the “these methods have always worked for me” claim is the claim of inductive justification. An inductive argument linking methodology_{A,R} to true theories is exactly what one would expect if there were some necessary connection between the use of a specific set of interrelated methodological principles and the generation of true (or nearly true) theories. It seems that giving an inductive justification for the use of simplicity principles in science cannot, by itself, stop me from wondering if there is a reason *why that was possible*. Induction cannot tell us anything about necessary connections, if there are any. Perhaps I have shown that an inductive justification of simplicity principles in science would not fail because of any obvious circularity. I do not know if I have shown that such an argument is possible, and it looks very unlikely that someone will actually give it. For me to be convinced that induction is the alpha and the omega of the justification of simplicity in science, I would have to be convinced that no other argument could do the trick. Only in that case would I let the justification of simplicity live by virtue of its heterogeneity or die by the sins of circularity. I do not believe that all other arguments forms have yet been ruled out.

Second, Swinburne may be on to something when he objects that there are different ways of extrapolating data. One fundamental problem with the argument from induction is that we are as justified in believing that things will change as we are in believing that things will remain the same. Kuhn’s arguments drew attention to the important fact that

revolutions are an important part of science. Laudan holds that the flux of science is more constant than even Kuhn thought that it was. Science changes and we would not wish to load the inductive dice by picking a bunch of methodological principles that generated theories of the same sort that we have generated in the past. Swinburne gives an example of a law of thermal dynamics that was replaced by a more syntactically complex law. The problem with inductive justification is that we would be exactly as justified in believing that complexity favors science as believing that simplicity favors it. At some point, we want to generate entirely new theories in entirely new areas of study. If we wish to justify the methodological principles that will take us into the unknown and guide us through the unexpected, we might wish to know something more than induction can provide. We might wish to know something about the human mind or about the world. One original motivation for looking into the roles of simplicity judgments in science arises directly from the suspicion that complexity might just be the friend of science. Induction, alone, cannot decide between the complexity and simplicity.

4.3.2 Indispensability

Kant suggested that we employ an indispensability principle to solve the problem of induction. This would change the approach to traditional metaphysics which relied mainly on a principle of non-contradiction. The change would be that we might discover something about the mind rather than about the mind-independent world – we might discover which concepts make possible the having of specific experiences. In its strongest form, the indispensability principle states that having some concept is both necessary and sufficient

for experiencing sensations in the unique way that we do.

Unfortunately, Kant did not know that paradigmatic examples would be difficult to come by. Kant thought that he could argue that space and time concepts are indispensable for the kind of knowledge that we get from doing physics. As it turned out, even length and time would be conceived of relative to an inertial frame of reference after the splash made by Einstein's Special Relativity. Science also ended up embracing non-Euclidian geometries, so that even the axioms of extension in space and time cannot easily be shown to be indispensable for the way that a scientist would experience experiments while embracing any particular theory. But this kind of inference did not die along with any hopes for a Copernican Revolution in metaphysics. Some authors still hope that an indispensability principle will solve the traditional problem of showing that our theories are related to the truth.

One approach is to employ the indispensability principle in an argument aimed at showing that theories constructed and evaluated by methodology $_{\Lambda,R}$ will be instrumentally reliable because we have concepts that are filled out when the criteria λ are satisfied (however this works out in experience). On this version of the indispensability argument some concepts are identified as being both necessary and sufficient for the having of some experiences.

Another approach involves embracing a weaker version of the indispensability principle. A weaker form of the principle might state only that some concepts are sufficient (but not necessary) for the instrumental reliability of scientific theories. For our present inquiry, the idea would be to show that the reason that specific theories constructed using a specific

methodology give descriptive and predictive results within acceptable accuracy constraints is that they produce theories with the correct ontology. This argument form should be expected to identify a feature of cognition that makes possible our having experiences of a specific sort – the sort had by those who embrace true theories. Perhaps, moored to a causal theory of reference and with the sorting of some tricky details about the structure of concepts, this argument form would also solve the traditional problem of ontology.

One version of the indispensability argument is as follows:

1. We have good reason to believe the literal truth of our most successful (instrumentally reliable) scientific theories.
 2. Methodology $M_{\Lambda,R}$ is indispensable for scientific practice.
- C: We have reason to believe in the existence of the properties and mechanisms posited by theories constructed using $M_{\Lambda,R}$.

So, if $M_{\Lambda,R}$ is a method which is indispensable for the construction or testing of scientific hypotheses or theories, then we have reason to believe that the properties and mechanisms posited by $M_{\Lambda,R}$ exist ¹¹.

In this section I present and evaluate arguments given by two modern authors which appear to take this form. Richard Swinburne's argument, I think, does properly take the form of this argument. It involves the strong form of the indispensability principle. Paul

¹¹This indispensability argument borrowed from Alan Baker's "Mathematics, Indispensability and Scientific Progress" (2001) where it is argued that that this principle ought to be understood broadly so that it applies not only to current scientific principles and methods, but also to any which history has shown to have played an indispensable role in the development of science. I have altered Baker's argument a bit by including the " Λ, R " subscript. I believe that I have shown that even a methodology committed to the correct ontology had better have principles that stand in the correct relations to one another. I changed Baker's word "abstracta" to "properties and mechanisms".

Churchland's argument appears to take the form of this argument with the weak version of the indispensability principle, although it is possible that Churchland's argument should be analyzed as an argument to the best explanation. Arguments to the best explanation are discussed in the next section.

I raise objections to the arguments given by Swinburne and Churchland. I conclude that Swinburne's approach is more promising than the weak version of the indispensability argument.

4.3.3 Richard Swinburne's Argument

Swinburne's *Simplicity as Evidence of Truth* (1997) is a short and lucid defense of the view that, other things being equal, the simplest hypothesis among competitors is most likely to be true. Swinburne says,

I seek in this essay to show that – other things being equal – the simplest hypothesis proposed as an explanation of phenomena is more likely to be true than is any other available hypothesis, that its predictions are more likely to be true than those of any other available hypothesis, and that it is an a priori epistemic principle that simplicity is evidence of the truth. ([59]pg.7)

We learn a bit more about Swinburne's view when he says that

If one theory is superior to another in yielding the data to a higher degree of inductive probability, or in yielding more data to the same degree of probability, then as such it is more likely to be true, but any greater simplicity of a rival theory is a compensating factor which could lead to equal probability overall or even to the greater probability of the rival. ([59]pg. 19)

Swinburne's defense of principles involving simplicity has many merits. Among them is Swinburne's sensitivity to the fact that the metascientific criteria stand in a tight set of interrelations. We would expect more accurate predictions and perhaps some increase

in the number of data (or, perhaps the kinds of data) predicted by deeper theories and it is likely that Swinburne's arguments reflect sensitivity to the relations between *depth* and the other metascientific criteria. Swinburne's view should also accommodate a variety of views about curve-fitting that are based generally upon the gauge of syntactical simplicity introduced by Jeffreys.

Swinburne knows that we construct and evaluate theories according to some criterion of how well a hypothesis *fits* the data and some criterion of how well a hypothesis makes predictions. He also knows that some criterion of *fit* (in a different sense) with our background knowledge plays a crucial role in (what Kuhn would call) normal science. These notions deserve more elucidation than they have been given in the previous chapter, but Swinburne's sensitivity to these features of science suggests that his view would work nicely with the mixed bag of empirical and non-empirical criteria. Swinburne uses slightly different terminology, but as I have discussed them, his a posteriori criteria would include *predictive power/fit, accuracy, and the unity of science*. Swinburne adds two a priori criteria of theory appraisal, one involving the trade-off between conceptual content and predictive power and the other involving simplicity.¹² Swinburne's sensitivity to the conceptual content of theories reveals his sensitivity to the kinds of issues discussed by Goodman about economy. It is Swinburne's aim to argue only that simplicity is an indispensable a priori criterion of theory evaluation, but I take it to be a merit of his view that he sees that several

¹²Swinburne says that there are four criteria of theory evaluation, two a posteriori and two a priori. I do not see how to do this job with just four criteria. The suggestion that there is a *criterion* of the trade-off between content and predictive power appears to conflate the metascientific criteria with their interrelations.

other criteria are involved in the construction and appraisal of theories.

Another merit of Swinburne's view is that he distinguishes different kinds of simplicity criteria. I have argued that this is where all philosophies of simplicity must begin. I will present Swinburne's view and raise one objection and another important challenge. My objection is that his criteria depend on the *ceteris paribus* clause and that my arguments have shown that no principle of parsimony in science can depend on this. My challenge for proponents of this strategy is to show how the indispensability argument can uniquely select any particular cluster of simplicity criteria as those that are necessary for generating instrumentally reliable theories.

4.3.3.1 Swinburne's Kinds of Simplicity

Swinburne distinguishes five different criteria of simplicity, or as he calls them *facets* of simplicity. Swinburne's simplicity criteria specify numbers of things, kinds of things, a criterion of *graspability* which may involve notions of epistemological simplicity or psychological simplicity (a kind of *pragmatical simplicity*), numbers of independent laws, and a composite criterion involving syntactical simplicity and either *pragmatical simplicity* or *epistemological simplicity*. I will let Swinburne's own words show how he distinguishes simplicity criteria.

Swinburne's first facet is a criterion selecting the numbers of things postulated.

The first facet of simplicity is just a matter of number of things postulated. A theory which postulates one entity (or property of an entity) rather than two, two rather than three, is (other things being equal) simpler. ([59]pg. 29)

His second facet is a criterion selecting numbers of kinds of things postulated.

A theory which postulates three kinds of entities (or properties of entities) is (other things being equal) simpler than one which postulates six, and so on. A theory which postulates three kinds of quark is simpler than one which postulates that quarks have just certain properties, such as spin, is simpler than one which postulates that they have these properties and also charm as well. ([59]pg. 30)

Swinburne's third facet appears to be either a kind of semantical simplicity or a kind of psychological simplicity.

A formulation of a theory which contains a term referring to an entity or descriptive of a property which can only be grasped by someone who grasps some other term will be less simple than an otherwise equally simple formulation of a theory which contains the latter term instead. Thus if "grue" can only be understood by someone who understands "green" but not conversely, then "all emeralds are green" is simpler than all emeralds are grue". ([59]pg. 30)

Swinburne's fourth facet is a criterion selecting numbers of separate laws. It should be noted that Swinburne embraces a causal theory of explanation, so this criterion will select *causal laws* and will be related to kinds of causation in some way. "A formulation of theory consisting of few separate laws is (other things being equal) simpler than one consisting of many laws." ([59]pg. 31)

Swinburne's fifth facet appears to capture some of the things discussed by Goodman about economy of form. The fifth facet involves the fewness of variables related by individual laws and, also, what might be thought of as the defining power of these variables. "A formulation of a theory is simpler in which individual laws relate few variables rather than many."

Two sub-facet are involved in this facet of mathematical simplicity. One is that fewer terms in an equation make it simpler. $y = z + x$ is simpler than $y = z + x + x^2$. Secondly, other things being equal, an equation is mathematically simpler than another in so far as it uses simpler mathematical entities or

relations than that other. A mathematical entity or relation θ is simpler than another one ψ if θ can be understood by someone who does not understand ψ , but ψ cannot be understood by anyone who does not understand θ . ([59]pg. 32)

I believe that Swinburne's argument does take the form of an indispensability argument involving necessity very much like the example that I have given above, although he does not state it this way explicitly. In a few places Swinburne says that a person who does not judge the a priori probability of hypotheses by their simplicity would be said to be irrational, and I infer that what he means is that simplicity principles are indispensable for rational thought. Two passages which support this are also useful for the critical discussion at the end of this section. Swinburne says that,

if there are two theories which yield the observations made so far, one predicts that all life in the northern hemisphere will be destroyed tomorrow and the other predicts that all life in the southern hemisphere will be destroyed tomorrow, and there is no time to test further between them, but the latter is complicated and the former is simple, any northerner would be on the aeroplane to the south tonight and think that they were highly rational to do so. ([59]pg.50)

Perhaps the most telling piece of text evidence that Swinburne's argument takes the form of the indispensability argument with necessity is a passage where he argues against the possibility that inductive justification can be given for simplicity principles.

All such attempts to prove from some theorem of mathematics or logic that the simpler theory is more probably true, as I suggest, doomed to failure. The fact – however unwelcome to many – is that, if the principle of simplicity is true, it is a fundamental a priori truth. If data ever render one theory or one prediction more probable than another, that can only be because there are a priori criteria for extrapolating from the data in one direction rather than another. Yet there is no truth of logic with a consequence about which direction extrapolation yields probable truth. So – if any proposition which is not analytic is synthetic – it is both synthetic and a priori that (other things being equal) a simpler theory is more probably true than a complex one. If simplicity could be justified further, it would derive that justification from some higher a priori criterion, and that one would be fundamental. ([59] pg.55-56)

I conclude that Swinburne's argument is that these five criteria of simplicity are indispensable for the rational enterprise of science.

4.3.3.2 Criticisms of Swinburne's View

My objection to Swinburne's view is that each facet depends on a *ceteris paribus* clause. No simplicity principle in science can depend on this clause. The analysis of the interrelations between the metascientific criteria show us this. There is no way to construct hypotheses that are *different* from one another and have them not differ along several dimensions of the metascientific criteria. For this reason, a simplicity principle cannot involve simplicity criteria that depend upon *ceteris paribus* clauses. Appeal to a *ceteris paribus* clause is an attempt to give analytic treatment to simplicity without settling the difficult matter of weighting the metascientific criteria. We cannot get around this problem which reigns in the freedom of conceptual analysis. If any two hypotheses account for some data equally well, but are truly different hypotheses, then they will differ conceptually. This means that, necessarily, other things never are equal.

It is interesting that Swinburne does not appear to notice this problem. He sees that there is a trade-off between the various simplicity criteria. He says that,

in order to compare theories, we need to compare their simplest formulations. But it is not always clear which is the simplest formulation of a theory. For it is always possible to give a formulation of a theory which makes it simpler in one respect at the cost of loss of simplicity in another respect. We can for example reduce many laws to few by introducing variables with more components (scalars such as mass having only one component, vectors such as velocity in three-dimensional space having three components, tensors having many more), and so compressing the information into a shorter form. Maxwell is known for having propounded four laws of electromagnetism, but Maxwell used only vectors and scalars. Put his theory in tensor form, and you can express it with only two laws. But the gain in simplicity in fewness of

laws is balanced by the loss involved in introducing variables, that is, values of properties, more remote from observation (you cannot grasp the concept of an electromagnetic field tensor without grasping the concepts of electric field and magnetic field, but not vice versa), and more complex mathematical entities. Formulations of theories may or may not become simpler when there is a loss in respect of one facet but a gain in respect of another. ([59]pg. 34-35)

Swinburne is right that it is always *possible* to formulate theories that are simpler in one respect and more complex in another, because it is *always actually* the case that when competing scientific theories are offered they are simpler in some respects and more complex in others. Swinburne is right to make a point very similar to Goodman's, which is that it is possible to achieve spurious gains in simplicity of a particular sort and gauged in a particular way just by defining terms a bit differently. Goodman's point, and, I take it, Swinburne's as well, is that we seek economy rather than mere syntactical simplicity. What deserves to be noticed is that it is not only the case that it is always possible to reformulate a theory, or even that it is always possible to solve the ranking or gauging problems in different ways, but that different theories are *different* and so will either differ along more than one kind of simplicity measure or else they will differ with respect to the ways in which the other metascientific criteria are satisfied. Individual hypotheses H_1 and H_2 may differ by only one term. But this difference will be reflected in the semantics, in algorithmic simplicity, in psychological simplicity, and in epistemological simplicity. Recall that there is a trade-off between syntactical simplicity and epistemological simplicity. Also, two competing hypotheses should not be expected to be equal in their depth, or in the ways that they are thought to unify with the rest of our physical theories. At the very least, it is difficult to conjure an example where two *scientific* hypotheses account for the same data

and both satisfy the other metascientific criteria equally and in the very same ways.

Recall the earlier discussion Swinburne's example that clearly *does not* successfully compare two hypotheses differing in their satisfaction of one criterion of simplicity while holding other things equal. LeVerrier could not have posited seven hundred bodies with a common center of mass to explain the wobbly orbit of Uranus instead of just one because this configuration of bodies would not behave in that way without either changing other data about the orbits of Uranus and the other planets, or without depending upon some very strange physical laws. The seven hundred body hypothesis would change either the expected data or it would depend upon non-Newtonian laws or both. The fact that LeVerrier and Newcomb knew this is reflected in their arguments about Mercury's perihelion advance. LeVerrier, for example, argued that the sun could not have rings that affected Mercury's orbit in a way consistent with the data. The rings would have to be at just the right angle of inclination to be invisible from Earth, but at that angle they would not give Mercury an orbit consistent with the data.

Although I cannot yet accept Swinburne's argument, because it is for a principle of parsimony with a *ceteris paribus* clause, a finer analysis of the interrelations between the metascientific criteria may aid in constructing an argument along similar lines. Swinburne says that "There is this crucial a priori element affecting the probability of [a hypothesis] h is the claim of this paper, which affirms that it is a function of simplicity and (inversely) of content." ([59] pg.60-61)

It is true that if we could find some acceptable way of giving an empirical analysis of both data and auxiliary hypotheses, as Swinburne suggests, perhaps we could put all of the

empirical data predicted by background theories into the evidence, then there is an inverse relation between the content of a hypothesis and its *fit* with the data, only if we assume that *fit* involves predictive power. However, as Goodman pointed out, we are not interested in mere simplicity, but in economy. More precisely, there is an inverse relation between the *accuracy* of a hypothesis and its ability to make correct predictions, and there is an inverse relation between the syntactical simplicity of a hypothesis and linguistic exactness, which places a necessary constraint on accuracy. We have seen that the metascientific criteria are related in many other ways as well. *Depth* provides a necessary constraint on syntactical, semantical and epistemological simplicity criteria also. Swinburne may be a friend of the view that explanatory depth is achieved by getting ontology correct and I would think that this might favor an argument like his because it might aid in triangulating what are the relevant simplicity criteria in different departments. In order to pursue this line, some very difficult work would need to be done to establish which theories and which associated methods were paradigmatic of scientific success and then to give to these the kind of detailed analysis that Bunge has outlined. It seems to me that if the balance between the metascientific criteria employed in the construction of instrumentally reliable theories is more delicate, then the indispensability argument is more insulated from challenges. This would be the case if it could be shown that one and only one set of interrelated criteria are involved in our instrumentally reliable methods. It would be a very difficult case to make, and one that I do not believe has yet been made. The first move in this direction is to review the work of historians of science to identify specific success stories in science and then to see which hypotheses were considered by scientists and what were the arguments by

which each contender was ruled out. In the effort to triangulate paradigmatic instrumentally reliable methods, I suggest that we might have better luck by looking at the cases involving the ruling-out of hypotheses than by looking for cases where entire *theories* were thought to be somehow confirmed. The reasoning in these cases is fairly clear and confirmation is a thorny issue.

My challenge for Swinburne, and for proponents of this kind of argument, is to show that a specific cluster of clearly defined simplicity criteria really are indispensable for the construction and evaluation of our instrumentally reliable scientific theories. Swinburne has made an attempt to give this argument. He says, “to summarise the claims in a nutshell: either science is irrational (in the way it judges theories and predictions probable) or the principle of simplicity is a fundamental synthetic a priori truth.” ([59] pg.61)

It may be only a minor problem to show that some specific cluster of simplicity criteria employed in a methodology are sufficient for the construction of instrumentally reliable theories. The real problem is showing that some cluster is necessary for that end. Kant notices this problem when he outlines a similar style of indispensability argument for the concept from which all moral duties are derived.

For in such a case it is easy to decide whether the action [which accords with duty] was done *out of duty* or for some self-interested goal. This distinction is far more difficult to perceive when the action accords with duty but the agent has in addition a direct inclination to do it. For example, it is certainly in accord with duty that a shopkeeper should not overcharge an inexperienced customer; and, where there is much business, a prudent merchant refrains from doing this and maintains a fixed general price for everybody, so that a child can buy from him just as well as anyone else. People thus get *honest* treatment. But this is not nearly enough to justify our believing that the shopkeeper acted in this way out of duty or from principles of honesty; his interests required him to act as he did. We cannot assume him to have in addition a direct inclination towards his customers, leading him, as it were out of love, to give no one pref-

erential treatment over another person in the matter of price. Thus the action was done neither out of duty nor from immediate inclination, but solely out of self-interest. (*Groundwork for the Metaphysics of Morals*, Chapter One)

The problem, in Kant's example, is that a shopkeeper's fair prices could be established either from selfish ends or from acting on a moral duty. Just by reviewing the datum, we cannot know which is the case. There is a similar problem for an argument that any particular cluster of properly specified metascientific criteria are indispensable for the construction or evaluation of instrumentally reliable theories. It is possible to conceptually reduce *specific* principles of simplicity to other methodological principles. Since the reduction is a *conceptual reduction*, we would know only something about the structural similarities of the reducing and reduced principles, but we would not know which is more epistemologically fundamental. Again, we can see this in the arguments given by LeVerrier and Newcomb when they evaluated and ruled out hypotheses proposed to explain Mercury's perihelion advance. A specific hypothesis about inter-Mercurial matter (recall that there were several of this sort) may have been ruled out because it did not satisfy one of the following criteria: simplicity of entities, simplicity of syntax, semantical simplicity, conformity with the data, or unity with accepted scientific laws. If limited conceptual reductions of specific methodological principles are possible (and I suggest that they are), then the indispensability argument *alone* cannot determine which specific principles are necessary for the construction or evaluation of instrumentally reliable theories.

Now, I suggested that a close look at the work done by historians of science might aid in triangulating indispensable principles and this is where I think that we should look. However, this challenge is more difficult than it may at first appear. The above example

from Kant is employed to show that the problem of induction is unavoidable so long as our investigation involves sensations. Kant concludes that he will never find law-like connections between sensations and therefore, he must investigate the form of the concept alone in order to discover any universal and necessary connections (which Kant says is *easy*). In our present inquiry into the justification of scientific methodologies, we have an empirical component: the instrumental reliability of theories constructed by specific methods. The fundamental problem remains to show that synthetic a priori judgments are possible. For this reason, I cannot yet see how to defend this kind of argument.

4.3.3.3 Piety and Swinburne's Examples

I wish to conclude this subsection by reviewing what may appear to be a challenge to my own project. This challenge arises from the *if our lives depended upon some judgment* kind of case that Swinburne has given. Swinburne gives an example where a scientist must decide whether or not to fly to the southern hemisphere on no further information than that a hypothesis about some northern hemisphere cataclysm is simpler, in some respect, than a competing hypothesis about the destruction of the southern hemisphere. This kind of example appears to confront my project with a dilemma: either accept that scientists are irrational or accept that simplicity judgments are fundamental for rational thought. I reject the dilemma. Although I might be inclined to accept that science is not entirely rational (but not fundamentally irrational), it is not yet clear that these are the only two options.

It is key for this example that the individual deciding to fly to the southern hemisphere is a scientist. The reason is that we wish for the individual making this judgment

to be someone who understands science. What is not clear is whether the principles that govern scientific inferences collapse into the principles that govern the inferences of classical epistemology. The scientist in this example may just give up on knowing anything on the basis of scientific principles in this kind of case and revert to some common human judgment. If human knowledge is possible, then I suggest that scientists are not the only people who have it. If humans have knowledge, then they have it without understanding the specific, and sometime highly technical, esoteric principles of science. Science may be a rational enterprise because it shares some, but not all, of the principles of fundamental rational inference. There may not be a high degree of continuity between classical epistemology and the philosophy of science although the word ‘epistemology’ is correctly applied in both cases. It is possible that classical epistemology and the realism versus anti-realism controversy in the philosophy of science are, to some extent, dialectically independent. I do not see why non-scientists should be thought of as employing Swinburne’s criteria when they make rational judgments, so it is not clear that a scientist who makes a rational judgment (supposing that is what it is in this example) is employing Swinburne’s criteria. Perhaps the metaepistemological criteria and the metascientific criteria partially overlap. Swinburne must first argue that they are identical before he can force us to embrace this dilemma.

4.3.4 Paul Churchland’s Argument

Paul Churchland employs what may be a version of the second kind of indispensability argument in “The Ontological Status of Observables: In Praise of the Superempirical Virtues” [18]. If this is the correct way to understand Churchland’s inference, then

it would be an inference that some concept, or set of concepts are indispensable for the experiences that we do have, but only as a contingent matter of fact. The *superempirical virtues*, Churchland argues, are some of the fundamental cognitive features had by critters who succeed in breeding, reasoning and doing science of the sort that we are familiar with. These map very generally onto the metascientific criteria; including, of course, simplicity.

Churchland's arguments in this article are specifically directed against the constructive empiricism of Bas van Fraassen. Constructive empiricism is committed to the following two claims:

1. The truth of a theory is measured by nothing more than descriptive excellence at the observational level.
2. Acceptance of the theory should not involve commitment to *unobservable* entities.

The thrust of these two claims is that *empirical adequacy* is the only theoretical virtue in science and that in accepting theories as true, we need not reify the terms postulated by the *theoretical detour*. Churchland argues that there are other theoretical virtues: simplicity, coherence, and explanatory power.

Churchland's strategy is to press the Constructive Empiricist's commitment to a distinction between what is *unobservable* and what is *unobserved*. Churchland's claim is that the distinction between *observable* and *unobservable* seems intuitively plausible only because of the contingent state of affairs that human beings find themselves in. Evolutionary events, random and deterministic causation tell the story about how humans came to sense only certain wave lengths of E-M radiation, specific frequencies of sound, and things only of certain sizes or at certain distances. Were we to have our heads fitted with artificial

transducers, or were evolutionary forces to have favored electron microscope eyes (so the argument goes), we would have developed a discourse about the strands of DNA which we would “see”, instead of the breadbox-sized-objects-discourse which we are accustomed to. The constructive empiricist must say that there are unobserved entities, but Churchland’s thought experiment is supposed to show that some of the entities that the constructive empiricist wants to call “unobservable” might just be unobserved. If Churchland’s objection is on point, then the constructive empiricist needs to make a principled distinction between what is supposed to be *unobservable* and what has merely not been *observed*. Churchland reasons that if a principled distinction is not defensible, then the constructive empiricist must either give in to skepticism or else accept the fact that we accept theories on the basis of criteria that cannot be reduced to empirical criteria.

Churchland says that

there is no way of conceiving or representing ‘the empirical facts’ that is completely independent of speculative assumptions, and since we will occasionally confront theoretical alternatives on a scale so comprehensive that we must also choose between competing modes of conceiving what the empirical facts before us are, then the epistemic choice between these global alternatives cannot be made by comparing the extent to which they are adequate to some common touchstone, ‘the empirical facts.’ ([18]pg. 41)

Churchland concludes,

values such as ontological simplicity, coherence, and explanatory power are some of the brain’s most basic criteria for recognizing information, for distinguishing information from noise. ([18]pg. 42)

Added to this is a premise arising from a rich discussion in the philosophy of science: that even everyday observations are *theory laden*. The conclusion is that the constructive empiricist cannot make empirical adequacy the sole theoretical virtue. Hume’s

problem is a problem at the level of medium-sized dry goods as it is at any other scale.

Churchland says,

I assert that global excellence of theory is the ultimate measure of truth and ontology at all levels of cognition, even at the observational level. Van Fraassen asserts that descriptive excellence at the observational level is the only genuine measure of any theory's truth and that one's acceptance of a theory should create no ontological commitments whatever beyond the observational level.

Against van Fraassen's first claim I will maintain that observational excellence or "empirical adequacy" is only one epistemic virtue among others of equal or comparable importance. And against his second claim I will maintain that the ontological commitments of any theory are wholly blind to the idiosyncratic distinction between what is and what is not humanly observable, and so should be our own ontological commitments. ([18] pg.35)

Perhaps Churchland's argument does take the form of the indispensability argument without the necessity claim. I will suppose, for the purpose of analysis, that this is his argument, or at least that someone may give a very similar one intending it to be of this form. If this is the case, then the argument would purport to show that humans have specific concepts that we employ in solving the problem of Humean skepticism both when we bump around the world of sensations and when we do science. If successful, this argument would aim to put van Fraassen in a dilemma: either give a principled distinction between *unobserved* and *unobservable* or admit that the skeptic must be given a final answer before the realism versus anti-realism controversy in the philosophy of science can be settled.

4.3.4.1 Criticisms of Churchland's View

Although I agree with Churchland that empirical adequacy is but one of many meta-scientific criteria, I do not share Churchland's reasons for holding that empirical adequacy is not (cannot be) the sole criterion of theory acceptance. My reason is that empirical adequacy is a composite notion involving both the *fit* of a theory with the existing data and its

ability to *make predictions*. In order to gauge how well a theory satisfies the composite criterion of empirical adequacy, we need to know what the *accuracy* constraints are. We can learn some important things about how accuracy constraints are established by empirical studies, but we cannot learn everything. Empirical studies may tell us about the physiological and psychological capabilities of experimenters or about how experimenters are influenced by social phenomena. However, we cannot give a purely empirical elucidation of how accuracy constraints are determined when we consider how experimental equipment is designed or how experimental controls are established. These things are determined by appeal to *theories*.

For example, the famous Cavendish experiment, which is aimed at measuring the value of the gravitational constant, depends for its construction upon theories about springs and theories about optics¹³. Additionally, this experiment is very sensitive to vibrations and so theories about possible sources of vibration contribute to the experimental controls placed on any particular run. The experiment may be performed on a massive spring loaded table or run only at non-commute hours of the day to reduce the vibrations contributed by cars driving near by. In this way, scientific experiments are said to be *theory laden*. This is because the design of this experiment and the interpretation of its results depend upon theories about the behavior of metals, about inertia, and sources of vibration – theories that are independent of the theory of gravitation. Churchland is quite right to point out that our experiments are theory laden. It is true that scientists who perform this experiment take into

¹³Because the deflection of a set of masses suspended by a spring, in this experiment, is quite small, the experiment typically bounces a beam of light off of a mirror and projects that beam out many feet (or yards) so that small movements can be detected.

account these factors and some of them are reflected in considerations about the accuracy of the results.

Accuracy constraints, however, are also determined by the ways in which they are balanced with many of the other metascientific criteria. Most importantly, *depth* will play a key role. All scientific experiments involve simplifying assumptions. No simplifying assumptions will be allowed at the expense of explanatory depth. I do not believe that a purely empirical elucidation of depth has yet been given. For these reasons, I end up with a conclusion much like Churchland's – that empirical adequacy is not the sole criterion of theory acceptance. I do not end up with this conclusion because Churchland has convinced me that run-of-the-mill rational thought involves precisely the same criteria that scientific thought does.

In any case, it was not Churchland's project to justify any specific principle of parsimony involving a specific criterion of simplicity as it is *in the brain*. His point is to make trouble for a fundamental commitment of constructive empiricism. In this he succeeds.

Not surprisingly, my objections to this kind of inference begin with the clarification of simplicity criteria. We know that there are many ways to categorize and to make precise the fine-grained details about simplicity criteria. There are many different kinds of simplicity criteria and these stand in unique relations with one another and with the other metascientific criteria. No overall reduction of simplicity is possible, so humans do not have just one simplicity concept. In some cases, specific methodological principles may be conceptually reduced to others but successful reductions of this sort leave us, still, with

a radically heterogeneous bag of criteria – some empirical and some non-empirical. Conceptual reductions of specific criteria leave us in the lurch about which are supposed to be epistemologically fundamental.

Again, consider the ways in which the Kantian indispensability argument is challenged. Kant thought that space and time concepts were indispensable for having knowledge. Only, Kant was thinking that Newtonian physics was pretty well the final word on natural philosophy. Alas, but Euclidian geometry would go the way of the dinosaur when Einstein gave a theory about curved space-time and a star was viewed in a way that it should not be, if Newtonian physics were true, during a solar eclipse. The problem arises when many concepts would appear to be sufficient, but no one necessary, for our theories to be instrumentally reliable within specific accuracy constraints.

Similarly, Nietzsche may have Kant's ethical groundwork as a target in *The Genealogy of Morals*¹⁴. If Nietzsche succeeds in showing that moral judgments change in such a radically heterogeneous way that no overall conceptual reduction is possible, then he will have shown that no single concept of morality is *indispensable* for human moral judgments. This would show us that *practical anthropology* is really the final study in the field of ethics and that this shows us something about society, but nothing about the structures of the hu-

¹⁴Nietzsche's primary targets are Spencer and Huxley who published popular books blurting out precisely the sort of thing that one might expect from Englishmen bobbing in the wake of the Christian conceptual revolution – that the term “good” was originally used to compliment unegoistic acts, but after humans *evolved*, the species *forgot* this, but continued to use the term this way. There are many problems with this view. It commits the genetic fallacy. It is not based on research. It appears to depend upon a teleological view of evolution. It would be odd to report something that the species forgot and that is complimented for being good today. Nietzsche probably suspects that the defenders of this view got squeamish about doing any real history because they had a notion that the Christian conceptual revolution was a bloody and ferocious one.

man mind. If we give up the necessity claim of the indispensability argument, we give up the original aim of this study which is to justify the stronger-than-material-conditional relation between the truth of our theories and the instrumental reliability of the methodologies that generated them. This is a problem. The more serious problem is that when we show that many different principles are sufficient, but no one is necessary, for the construction of our instrumentally reliable theories, we notice that an indispensability argument cannot determine anything about the mind. My objection to the indispensability argument minus the necessity claim is that it is *bad cognitive science*. For every reason discussed in this subsection, Swinburne's style of argument would be the less sinful way to go.

Finally, it may not be appropriate to characterize Churchland's actual argument as an indispensability argument at all. Alan Baker says that the modern style of indispensability argument – where *we have good reason to believe X* because of the success of some body of scientific work committed to *X* – is actually an argument to the best explanation *that X can justifiably be reified*. The inference to the best explanation argument strategy is the next topic of discussion.

4.4 Inference to the Best Explanation

In this final section, I wish to investigate the possibility that the justification of simplicity principles in science might be based upon the inference to the best explanation (IBE). Paul R. Thagard has given a thorough defense of IBE as a form of inference in science. Thagard also suggests that his analysis is general enough to be applied to issues in classical epistemology as well. Thagard says that a merit of his analysis of IBE is that

it makes possible a reunification of scientific and philosophical method,

since inference to the best explanation has many applications in philosophy, especially in metaphysics. Arguments concerning the best explanation are relevant to problems concerning scientific realism, other minds, the external world, and the existence of God. ([60] pg.92)

I present Thagard's analysis and attempt to sketch how IBE might be applied to the justification of simplicity principles in science.

I also present Richard Fumerton's argument that IBE cannot be used as an alternative to induction in answering the skeptic. Fumerton's arguments raise interesting issues about the role of simplicity judgments in IBE. I might be convinced not to use IBE to answer the skeptic; however, I do not yet see why IBE suffers from any formal problems if it is employed to justify simplicity principles in science. If the argument from IBE is circular, it is because the principles of simplicity that govern the inference are the same as the principles appearing in the conclusion. For reasons similar to those discussed in the previous sections, the radical heterogeneity of simplicity, perhaps, comes to the rescue when IBE is charged with *circularity*. However, if IBE needs rescuing then this rescue does rely on the notion that if simplicity principles in science were to be justified by IBE, then the skeptic would have received a final answer by way of some other argument.

I do not yet know how the IBE justification of simplicity in science would go in all of its details. Part of this has to do with the same issue that has previously been discussed. It seems that a few enduring controversies in the history of science need to be settled so that we can see which scientific theories and which associated methodologies are paradigmatic of scientific success. What I aim to show is that if IBE is employed to solve the problem of the justification of simplicity in science then 1) it is far from obvious that IBE suffers from

any formal difficulties and 2) it is not yet clear that it conceptually reduces to induction.

4.4.1 How IBE Might Go

Thagard says that, “to put it briefly, inference to the best explanation consists in accepting a hypothesis on the grounds that it provides a better explanation of the evidence than is provided by alternative hypotheses.”([60] pg.77)

In the second chapter, I suggested that IBE would be employed in an argument of the following form,

1. Observation set O .
2. Observation set O' .
3. Observation set O'' .
4. If h were true then it would be better than the disjunction of all of the (perhaps infinitely many) hypotheses which give a unified explanation for the bunch of observations which would otherwise be a totally disconnected set of coincidences.

Therefore: h is probably true.

If this argument form were applied to justify simplicity in science it might go as follows,

1. Methodology $_{\Lambda,R}$ was employed in constructing and evaluating T_1 and T_1 is instrumentally reliable.
2. Methodology $_{\Lambda,R}$ was employed in constructing and evaluating T_2 and T_2 is instrumentally reliable.
3. Methodology $_{\Lambda,R}$ was employed in constructing and evaluating T_3 and T_3 is instru-

mentally reliable.

4. If the world has the features selected by the members of Λ and those features are related in $\text{Methodology}_{\Lambda,R}$ so that it generates instrumentally reliable theories, then this would be a *better* explanation than the disjunction of all of the other (infinitely many) hypotheses which give a unified explanation for the fact T_1 , T_2 , and T_3 are instrumentally reliable, which would otherwise be a totally disconnected set of coincidences.

Therefore: Probably the world has the features selected by the members of Λ and those features are related in ways that $\text{Methodology}_{\Lambda,R}$ tracks such that it generates instrumentally reliable theories.

This formulation still does not look much different from induction due to the fact that each of the premises are uniform cases. The proponent of induction could claim that to get the story right about how the human mind works we should replace (4) with the principle of the uniformity of nature. I wish to see if it is possible to avoid this stalemate by giving IBE a unique form in this context. Perhaps the argument form should go more like this:

1. $\text{Methodology}_{\Gamma,R_1}$ was employed in constructing and evaluating T_1 and T_1 is instrumentally reliable.
2. $\text{Methodology}_{\Delta,R_2}$ was employed in constructing and evaluating T_2 and T_2 is instrumentally reliable.
3. $\text{Methodology}_{\Theta,R_3}$ was employed in constructing and evaluating T_3 and T_3 is instru-

mentally reliable.

4. If analysis reveals that Λ could be a subset of Γ , Δ , and Θ and R could be a subset of R_1 , R_2 , and R_3 , and $\text{Methodology}_{\Lambda,R}$ could play a role in the construction of T_1 , T_2 , and T_3 , then the world has the features selected by the members of Λ and those features are related in $\text{Methodology}_{\Lambda,R}$ such that it generates instrumentally reliable theories, then this would be a *better* explanation than the disjunction of all of the other (infinitely many) hypotheses which give a unified explanation for the fact T_1 , T_2 , and T_3 are instrumentally reliable, which would otherwise be a totally disconnected set of coincidences.

Therefore: Probably the world has the features selected by the members of Λ and those features are related in ways that $\text{Methodology}_{\Lambda,R}$ tracks such that it generates instrumentally reliable theories.

This argument form looks distinct from induction because it attempts to give a unified explanation for a heterogeneous, rather than homogeneous set of observations. But, the argument does attempt to draw something homogeneous out of the observations: that $\text{Methodology}_{\Lambda,R}$ may be seen as playing a role in each case. Perhaps what is unique about this form of argument is that those who give it would not have to wait for historians to sort out every detail about which scientists took which principles to be epistemologically fundamental and then show that they had a few in common. The philosopher of science could attempt to give a conceptual reduction of any principles appearing in the scientist's methodologies to other principles, showing that these reductions do not change the interrelations between the metascientific criteria. If it could be shown that all of the distinct

methodologies *might* share a common subset of principles, then, given the complexity of the metascientific criteria and the tightness of their interrelations, it would be a miracle if anything else accounted for their instrumental reliability than that methodology $_{\Lambda,R}$ was related to the truth.

This project would involve a mountain of analytic and historical work! It seems that identifying a common subset of metascientific criteria and interrelations amongst several heterogeneous methodologies would be a miracle all its own. I cannot give the argument, nor can I spell out any specific details about which sorts of principles might be involved or how they might be related. However, I do think that this is the best case to be made that IBE would give us a form of argument distinct from the inductive argument.

Richard Boyd argues for a version of scientific realism by employing IBE[9]. Boyd's strategy involves first, arguing that the scientific anti-realist has a fundamental epistemological commitment to IBE because science is fundamentally committed to IBE in generating theories from data. Second, according to Boyd, that the only competing hypotheses about the instrumental reliability of scientific methodologies are the realist's hypothesis and the hypothesis that it is a "miracle" (i.e. that there is no explanation other than luck) that our instrumentally reliable methods collect instrumentally reliable theories over time. Third, Boyd's explanation for why the scientific methods employed by the mature sciences collect instrumentally reliable theories over time is in part that the auxiliary hypotheses needed for conducting experiments and interpreting the data are approximately true.

Boyd points out that antirealists themselves account for the projectibility of predicates and degrees of confirmation by appealing to background theories.

What Kuhn and other constructivists insist (correctly, I believe) is that judgments of projectibility and degrees of confirmation are quite profoundly dependent upon the theories that make up the existing theoretical tradition or paradigm. ([9] pg.57)

Boyd argues that scientific judgments about the projectibility of predicates and the degrees of confirmation conferred upon hypotheses by data depend on the inference that background theories are approximately true. He says,

...this conception of the enterprise of science provides the only scientifically plausible explanation for the instrumental reliability of the scientific method. In particular, I argue that the reliability of theory-dependent judgments of projectability and degrees of confirmation can only be satisfactorily explained on the assumption that the theoretical claims embodied in the background theories which determine those judgments are relevantly approximately true, and that scientific methodology acts dialectically so as to produce in the long run an increasingly accurate theoretical picture of the world. ([9] pg.59)

Boyd's conclusion is that the antirealist is committed to IBE in virtue of her commitment to enumerative induction and must justify limiting IBE to observables.

The rejection of abduction or inference to the best explanation would place quite remarkable strictures on intellectual inquiry. In particular, it is by no means clear that students of the sciences, whether philosophers or historians, would have any methodology left if abduction were abandoned. If the fact that a theory provides the best available explanation for some important phenomenon it is not a justification for believing that the theory is at least approximately true, then it is hard to see how intellectual inquiry could proceed. Of course, the antirealist might accept abductive inferences whenever their conclusions do not postulate unobservables, while rejecting such inferences to "theoretical" conclusions. In this case, however, the burden of proof would no longer lie exclusively on the realist's side: the antirealist must justify the proposed limitation on an otherwise legitimate principle of inductive inference. ([9] pg.67)

I wish to make three points about Boyd's argument: 1) it accommodates the fact that science often involves the ruling-out of hypotheses, 2) perhaps the argument could be

employed to justify methodology $_{\Lambda,R}$, and 3) that simplicity criteria of some kinds may be involved in the construction of his argument.

1) The fact that the efforts of LeVerrier and Newcomb to preserve Newtonian dynamics led to the key arguments that helped to overturn Newtonian dynamics may seem to present the scientific realist with a puzzle – how to justify the claim that scientific theories are approximately true, or that their terms succeed in referring, or that scientific progress builds upon earlier theories, when exemplary scientific work sometimes succeeds only in overturning its paradigm. Boyd's view would accommodate this, because his inference would be that the theories needed to collect data about planetary motions are roughly true. In other words, that our theories about optics are roughly true. For all I know, Boyd may be right to claim that some scientific methods depend upon the truth of background theories.

2) What we want is some argument that methodology $_{\Lambda,R}$ is justified. It would appear that the *style* of argument that Boyd gives against the anti-realist might do this. What would explain the instrumental reliability of our methodologies is that the background theories that some of the methods depend upon are approximately true, and also that the methodological principles are truth conducive, in the sense of preferentially selecting theories that are true or that progressively approach the truth. This style of argument would aim to show that the only explanation for the instrumental reliability of the theories of mature science is that methodologies of mature science involve principles that are related to one another in just the sort of way that is conducive to generating approximately true theories, and it would also aim to show that those methodological principles involving criteria that make ontological commitments are committed to the true ontology, or something close to

the true ontology. This argument could be extended to justify the simplicity criteria that are constituents in the methodology of mature science. However, this argument, alone, does not tell us which methodological principles standing in which interrelations are truth conducive. Identifying the specific principles of mature science requires empirical investigation.

3) Various simplicity criteria may be involved in the assignment of prior probabilities to hypotheses that purport to explain the instrumental reliability of the mature sciences. Boyd holds that there are only two hypotheses competing to explain the instrumental reliability of scientific methodology; the “miracle” hypothesis and the realist hypothesis. He concludes that the “miracle” hypothesis does not explain the instrumental reliability of scientific methodology. However, we might ask why Boyd considers only two hypotheses, and if our Boyd-style argument for the justification of methodological principles should do the same. Laudan has argued that there have been many “rogue” theories which enjoyed periods of instrumental success, but are now thought to be false, and to contain theoretical terms that, by the lights of current theories, lack reference. [42] Boyd must have some principled way to show that these sorts of hypotheses have an insignificant prior probability relative to the realist hypothesis and the “miracle” hypothesis. We must get an idea about how this kind of inference goes. In order to answer this challenge so that the Boyd-style argument can be employed to justify $\text{methodology}_{\Lambda,R}$, we need a principled way to distinguish “rogue” theories from the theories of “mature science”. Perhaps arguments are available to the effect that “rogue” theories were not actually more instrumentally reliable (however this is gauged) than the theories of the mature sciences (whatever those are). If

this cannot be done for all “rogue” theories, then this style of argument to the best explanation may still involve judgments of simplicity in the assignment of the prior probabilities. If the notion of *best* in IBE depends upon simplicity judgments, then we must ask two important questions. Would an argument to the best explanation that methodology $_{\Lambda,R}$ is truth conducive be circular? Would the justification of the simplicity criteria involved in IBE depend upon a track-record of giving probably true conclusions?

The remaining problem is, as suggested in the second chapter, how to understand the word “best” involved in IBE. This is where Thagard’s defense of IBE comes in handy.

4.4.2 Thagard’s Analysis

Thagard’s view is that the word “better” or “best” involved in the crucial premise of IBE can be analyzed as a set of criteria which have to be balanced against one another. Those criteria are: *consilience*, *simplicity*, and *analogy*. Thagard says that,

by “criteria” I do not mean necessary or sufficient conditions. We shall see that the complexity of scientific reasoning precludes the presentation of such conditions of the best explanation. A criterion is rather a standard of judgment which must be weighed against other criteria used in evaluating explanatory hypotheses. ([60] pg.79)

The criterion of analogy would be satisfied in the above example by the reductive analysis (supposing that one could be given) that resulted in some common set of inter-related methodological principles in every case. Simplicity and consilience require a bit more detailed discussion. Most of Thagard’s paper is devoted to spelling out the criterion of consilience. He says that,

the notion of consilience is derived from the writings of William Whewell. Consilience is intended to serve as a measure of how much a theory explains,

so that we can use it to tell when one theory explains more of the evidence than another theory. Roughly, a theory is said to be consilient if it explains at least two classes of facts. Then one theory is more consilient than another if it explains more classes of facts than the other does. Intuitively, we show one theory to be more consilient than another by pointing to a class or classes of facts which it explains but which the other theory does not. ([60] pg.79)

I have tried to make an argument form involving IBE capture this feature of consilience. The way that this is supposed to happen is, first by assuming that a conceptual reductions of several principles from several distinct methodologies will arrive at a common set of principles and interrelations, and second, that a commitment to the ontology suggested by this methodology would explain a diverse set of facts. Thagard also says that, “in inferring the best explanation, what matters is not the sheer number of facts explained, but the variety, and variety is not a notion for which we can expect a neat formal characterization.” ([60] pg.83)

In scientific explanations, the focus of Thagard’s paper, we would expect the criterion of consilience to satisfy the aims of *systematicity*, *depth*, and perhaps *the unity of science*. Perhaps, we would expect a criterion of consilience to do something similar in the inference about the justification of simplicity principles in science – it would contribute to the systematicity, unity, and generality of the hypothesis aimed at explaining the instrumental reliability of a methodology. I have suggested that scientific theories succeed in satisfying the metascientific criteria by doing more than we can account for by a mere *fit* with the data and by the systematicity to which syntactical simplicity contributes. This is due to the fact that we do not know everything that there is to know about *accuracy* by appeal to empirical and linguistic criteria alone – we must balance these with *depth*. Thagard

says something similar.

A consilient theory unifies and systematizes. To say that a theory is consilient is to say more than that it “fits the facts”: it is to say first that the theory explains the facts, and second that the facts it explains are taken from more than one domain. These two features differentiate consilience from a number of other notions which have been called “explanatory power,” “systematic power,” “systematicization,” or “unification.” For example, Carl Hempel has given a definition of “systematic power” which is purely syntactic, and hence much more exact than the above definition of consilience. However, it is not applicable to the sort of historical examples I have been considering, since it concerns only the derivation of sentences formed by negation, disjunction, and conjunction from atomic sentences “Pa”; it therefore does not represent the way in which Huygens, Lavoisier, and Darwin systematize by explaining a variety of facts, including those expressed by laws. ([60] pg.82)

I have attempted to formulate this version of IBE for the justification of simplicity in science guarding against IBE collapsing into an indispensability argument. This would happen if the crucial premise said something like,

- 4 If analysis reveals that Λ is the only subset of Γ , Δ , and Θ and R is the only subset of R_1 , R_2 , and R_3 , and Methodology $_{\Lambda,R}$ plays an essential role in the construction of T_1 , T_2 , and T_3 , then the world has the features selected by the members of Λ and those features are related in ways such that Methodology $_{\Lambda,R}$ generates instrumentally reliable theories, because there are no other alternatives and otherwise T_1 , T_2 , and T_3 , would be a totally disconnected set of coincidences.

Additionally, this form of argument might be pliable enough to accommodate the fact that scientists may have in common a few mistaken principles. Analogously with scientific explanation,

According to Wesley Salmon, variety of instances is important in that it helps us to eliminate alternative hypotheses; according to Clark Glymour, variety is needed in order to compensate for cases where errors in one or more hypotheses, or in evidence, may cancel each other out. ([60] pg.85)

Finally, we wish to guard against the objection that the maximally consilient explanation would explain any fact whatsoever. Thagard suggests that the solution to this problem involves balancing consilience with other criteria.

The limit to these adjustments depends on the increase in consilience of the theory being offset by a decrease in satisfaction of other criteria, such as precision and simplicity. ([60] pg.85)

Thagard's view is that simplicity places a constraint on consilience. His simplicity criterion selects *numbers of ad hoc hypotheses*. Thagard says,

a simple consilient theory not only must explain a range of facts; it must explain those facts without making a host of assumptions with narrow application.

An ad hoc hypothesis is one that serves to explain no more phenomena than the narrow range it was introduced to explain. Hence a simple theory is one with few ad hoc hypotheses.([60] pg.87)

This notion is included in the IBE argument for the justification of simplicity. The idea is that we need posit nothing more to explain the instrumental reliability of *methodology_{A,R}* than that it is committed to the correct ontology.

4.4.3 Circularity and IBE

For the very same reasons discussed in the section on induction, it is far from obvious that IBE employed to justify simplicity in science is committed to circularity just because it involves a criterion of simplicity. Thagard's IBE in scientific explanation involves a criterion of simplicity with respect to ad hoc hypotheses. He is probably correct to assert that scientists embrace such a criterion. If IBE had a similar form for the justification of simplicity principles in science, it might also involve a criterion of simplicity with respect to ad hoc hypotheses that would place a constraint on consilience. It is not obvious that the

criterion is the same in both cases. Scientific explanations are often *causal* explanations. The explanation of the instrumental reliability of a methodology is not obviously a causal explanation. Therefore, IBE in scientific explanation could involve a simplicity criterion selecting *numbers of ad hoc scientific hypotheses* and IBE employed to justify simplicity in science could select *numbers of ad hoc metaphysical hypotheses*. So long as IBE uses different criteria, the general argument *form* can be employed to justify the criteria used by that argument form in another field of discourse. Again, the final justification of simplicity in science might wait for the skeptic to be given a final answer. Another way of putting this is, don't wish for science to solve the problems of ontology.

Does that mean that there is really no point in the epistemological study of simplicity in science? After all, everyone is waiting for the skeptic to receive her final answer. I do not see why this is the case. If we learned something about how inferential criteria are balanced by investigating the philosophy of science, then we learned something in general. Perhaps, this lesson was not available by consulting mere imagination alone. If we learned something about how the classical problems of epistemology are different from the problems in the philosophy of science, then we learned something by way of contrast. Anyway, for all I know, an anti-ad hoc-hypothesis criterion of metaphysics might be justified by some transcendental argument, not by IBE. In that case, we really would need two different analyses: one to show how the transcendental argument answers the Humean skeptic and the other to show how simplicity criteria in science are justified.

4.4.4 Fumerton's Challenge

Fumerton has argued that IBE will not serve as a viable alternative to induction in answering the problem of skepticism.[25] The reason is that the inferential criteria, in particular, *simplicity* would have to be justified themselves. It is not clear what could justify constraining consilience with an anti-ad hoc-simplicity criterion except that past experience showed us that this method uniformly generated true hypotheses. Appeal to uniform past experiences is an appeal to induction. Probably, one of the inferences, IBE, induction, or indispensability is fundamental if the skeptic is to receive an answer, but only because an infinite regress would have to be avoided. However, in the philosophy of simplicity in science, I do not see that we have to take any of these argument forms to be fundamental. None appear to share sufficient structural similarities for conceptual reductions to be available.

4.4.4.1 Structural Similarities with Metaepistemology

Our present inquiry shares a few structural similarities with the internalism/externalism controversy in metaepistemology. Fumerton characterizes the form of the controversy in the following way:

Inferential internalism maintains that someone S justifiably infers one proposition P from another E only if 1) S is justified in believing that E confirms or makes probable P and 2) S is justified in believing E. The view is, of course, highly controversial and all well-known versions of externalism deny at least condition 1). ([25] pg.152)

Fumerton says that externalists will have no need of an argument to the best explanation since they have rejected 1), which is the only claim that would require an argument of this form for its justification. In our present investigation, we have slightly different

claims. We make the very plausible assumption that we are justified in believing 2s) that some of our scientific theories are instrumentally reliable and then we wish to see if there are any arguments available to show that 1s) something about our instrumentally reliable methodologies makes probable the production or preservation of instrumentally reliable theories. I have shown that it is, at least, very difficult to reject 2s) and that the usual claims aimed at rejecting 2s) are either false or too vague to get the job done. For this reason we are left with only a few candidate argument forms: (I) induction with the assumption that problems in the philosophy of science are independent of problems in classical epistemology, (TD) a transcendental deduction supposing that i) synthetic a priori knowledge is possible and that ii) paradigm examples of instrumentally reliable methodologies might be given and analyzed in such a way that a set of methodological principles worthy of an indispensability argument could tell us something about the mind, and (IBE) inference to the best explanation.

Fumerton says that

I suggested that one who accepts externalism would probably feel no particular need to try to subsume various fundamental belief-forming processes under some more general pattern of argument like inductive reasoning or reasoning to the best explanation. On the other hand, I should hasten to emphasize that if externalism were true, the philosopher qua philosopher should probably have no very strong opinions on what processes do and do not generate justified beliefs. ([25] pg.155)

Similarly, I say that if simplicity judgments are reducible to psychological states or aesthetic values, then philosophers should have no strong opinions about the methods that bring about instrumentally reliable theories. However, I have argued that an overall reduction of simplicity criteria is impossible. So, the problem of the justification of scientific

methodological principles is motivated, and we have only a few strategies for dealing with this problem.

I made note of the fact that we would need to add some special premises to the argument involving IBE in order for it to look distinct from the inductive argument. When we do this, we introduce a bunch of inferential criteria and these must be justified somehow.

Fumerton says a similar thing.

If it were true in general that plausible reasoning to the best explanation is enthymematic inductive reasoning, it trivially follows that reasoning to the best explanation cannot serve as an alternative to inductive reasoning in bridging gaps between the available evidence and our commonsense beliefs.

One can, of course, reject the suggestion that reasoning to the best explanation typically collapses into inductive reasoning by adding more to the premises of arguments to the best explanation. Thus instead of a conditional simply asserting that our observations would be explained by E, we could have a conditional saying that the observations would be explained best by E where our criteria for comparing explanations do not rely on any inductively supported connections.([25] pg.159)

Fumerton's concern in this paper is in the possibility of employing IBE to solve the problem of skepticism. He concludes that this is not possible because the justification of the inferential criteria will depend upon induction. Fumerton says that,

If we rely on any information about the past at all in justifying our belief that most events have causes, we will need an independent solution to the problem of justifying beliefs about the past, and thus if the preceding argument is correct we will have foreclosed the possibility of using reasoning to the best explanation in an attempt to justify beliefs about the past. If in reaching the conclusion that most events have causes we rely on the fact that our sensations have causes, we will again need a prior solution to skepticism about the physical world and we will be precluded from using reasoning to the best explanation in order to get that solution.([25] pg.162-163)

Fumerton may have successfully intimidated me into avoiding IBE to answer the skeptic, but, in all fairness, the skeptic had me intimidated already. The extreme hetero-

generality of simplicity suggests that it is not obvious that the criteria of IBE employed in some case would be justified by induction rather than IBE with a slightly different set of criteria. There is no circularity here. Justificatory arguments stacked in this kind of way either terminate with an answer to the skeptic or go on infinitely (if there are infinitely many simplicity criteria that would actually constrain consilience in the appropriate ways). If these arguments terminate, then I agree that either IBE, induction, or indispensability is fundamental. If justificatory arguments regress, it is not clear that this is a vicious regress. Perhaps avoiding a vicious regress here depends upon a coherence theory of justification.

4.4.5 The Independence of IBE?

Finally, I wish to show why I am not yet convinced that IBE can be conceptually reduced to some other argument form. However, I hasten to add that I could easily be convinced that IBE does collapse to some other inference, were a few things to fall into place. In my set up of the argument involving IBE, I showed how its form should be distinguished from an indispensability argument. If philosophers could show that some methodology _{Λ, R} was the one and only methodology possibly had in common by several independent paradigmatic cases of successful theory construction and evaluation, then IBE would collapse into indispensability. The problems for this are paramount, including sorting out “paradigmatic cases” and what “successful” amounts to and giving the relevant metascientific analyses.

I am also not yet convinced that IBE conceptually reduces to induction. Fumerton says,

I don't think there is a legitimate form of reasoning to the best explanation

that is distinct from inductive reasoning and thus I don't think reasoning to the best explanation can circumvent the traditional problems involved with finding an inductive bridge between available evidence and commonsense beliefs. ([25] pg.169)

I agree with Fumerton that it is difficult to see how IBE could circumvent the traditional problems of bridging the gap between evidence and common sense beliefs. I do not see that this is because IBE is not distinct from induction. Perhaps this can be seen by noticing that the problems of induction are a bit different from the problems of IBE.

In the first chapter I noted that *if* it turned out that we had some idea of the necessary connection between one cause-effect pair we could reason from cause to effect in that kind of case. However, we still would be unable to reason from effect to cause. It is not clear that IBE and induction share sufficient structural similarities that one could be conceptually reduced to the other because we criticize induction and abduction differently. Both the problems of underdetermination by data and the problem of circularity apply to reasoning from cause to effect. But reasoning from effect to cause is crippled by the underdetermination of theory by data alone. In causal reasoning, there is an asymmetry between the *problems* of reasoning from cause to effect and reasoning from effect to cause. This is because the uniformity of nature principle does not sort out which things are of the stay-the-same sort. IBE includes criteria that help to do this job. Another way to put these criticisms would be that the inductivist must figure out how to distinguish accidental generalizations from laws of nature, but the proponent of IBE must give an account of what the "best explanation" is.

Both forms of inference face the problem of showing that what the stronger than

material conditional relation is between antecedent and consequent. Induction alone just cannot show us this because we would have to rely upon past experience to establish which things stayed the same and which change, but experience is a mix of change and sameness. The argument involving IBE posits some necessary connection or some *makes probable connection* between antecedent and consequent, and it appears to have criteria appropriate for governing this inference. The problem with IBE is that it, alone, cannot tell us precisely what this relation is.

What would it take to talk me into accepting that IBE reduces to induction? I would have to be shown that the crucial premise of IBE could be replaced with the crucial premise of induction and that the criticisms of IBE remain the same. I think that the question of interest is not whether IBE conceptually reduces to induction (or vice versa), but whether or not the justification of IBE's inferential criteria must depend upon induction, not upon IBE using different criteria or upon indispensability. What about the possibility of an infinite regress? I, for one, am not all that concerned. I was suspicious about simplicity judgments in science to begin with. I could easily come around to the view that science is not entirely rational. Unlike some people who are incensed by the suggestion that science is like art in some ways. I am not. Both involve some rationality (because they can be done in very stupid ways). However, it is not yet clear that each is *entirely* rational. I do not know why this is a bad thing, nor why it would involve an impiety to think of the world as being closer to chaos than to simplicity. I could easily be convinced that the terminus of the justificatory arguments for simplicity in science is in the answer to the skeptic or in some non-rational judgments. Again, I say, do not simply assert one or the other. Marshall your arguments.

CHAPTER 5 CONCLUSIONS

My conclusions are that induction, indispensability and IBE are the only remaining contenders for arguments meant to justify simplicity principles in science. So what? Anyone familiar with classical epistemology could have told us that the dialectic long ago revealed this three-way stand-off in the fight for justificatory supremacy. Hold on, I say. Is it right to say that the preceding amounted to common philosophical knowledge? The introduction laid out some very ambitious objectives which I believe have been met.

First I motivate giving analytic treatment to simplicity. It is tempting to lunge at the questions of justification and thereby miss the valuable and difficult project of clarification. Typically, lunges at justification arise in semi-casual philosophical conversations. Some people think that there are no problems of simplicity in philosophy because they have allowed themselves to slip into thinking that simplicity judgments reflect subjective values and that there is nothing much more to say on the matter. Others appear to think that the frequency of the use of terms like “Ockham’s Razor” by legitimate authorities (like Smart) indicates that some experts must know what is going on with simplicity. The appeal to subjective values and the appeal to authority both miss the crucial first step. We must be clear about what we are refuting or defending. Anyway, some philosophers actually do put into print claims intended to slip by the questions of the justification of simplicity. Kuhn’s claim that how much weight scientists give a particular theoretical virtue is a matter of taste cannot be quite right. It could be right to say that how much weight a particular scientist gives to a theoretical virtue on balance with the other virtues is a matter of pragmatic and

aesthetic judgments. My inclination towards this view will increase as problems with the inductive, indispensability and IBE approaches are revealed. Other authors have said things that require a bit of work to clarify. Smart appears to be one of these. It would be nice to find out what Smart thinks that Ockham's Razor is - a principle that scientists do not violate or a useful heuristic. In any case, I believe that I have shown that arguments are required either to reject principles of parsimony or to justify them. We wish to avoid equivocation, so we must begin by analyzing simplicity judgments.

A study of history can supply the basic ideas about the roles played by simplicity judgments generally and in science. Nietzsche had a useful insight. He noticed that the archetypes for the problems of philosophy are to be found in Plato's mixed view. Nietzsche provided a template for understanding many contemporary issues including the problems of skepticism, and perhaps Nietzsche's template also provides a guide by which various realist and anti-realist positions may be categorized.

By way of example, consider once again that Kuhn was critical of the notion that what was distinctive of science was its ability to bring about widespread agreement between its skilled practitioners. He was also critical of the view that scientific progress could be accounted for by showing that older scientific laws could be deduced from newer laws. There are probably many things wrong with these views. However, Kuhn's unique argument is that these views are incompatible with a very important feature of science - the revolution. Applying Nietzsche's insight we see that Kuhn's attempt to elucidate science aimed to balance change and sameness. Kuhn's view about sameness is that somehow the terms and methods of science are established by paradigm theories for periods of time that

involve *normal science* - the working out of the details of paradigm theories. Eventually, changes in experimental equipment and the increasing complexity of theories will lead to a *crisis* - paradigm theories will no longer explain or predict the relevant data. When a crisis arises, the stage is set for an overturning of the old paradigm. Applying Nietzsche's insight again, Larry Laudan argues that change in science is more fluid than Kuhn's view allows for. He says that his argument is "thoroughly Heraclitean" (*Science and Values* pp. 64).

Second, I believe that the conceptual map of the metascientific criteria is valuable in many ways. Although the arguments about the interrelations between the metascientific criteria are not my own (they are mainly Bunge and Goodman's arguments), the development of the conceptual map may be a small contribution to the dialectic. The graphic rendering allows us to have a holistic picture of the interrelations between the metascientific criteria with only a glance. This may be a useful aid for both scientists and philosophers who do not specialize in the metascientific analysis as Bunge and Goodman did. Also, this particular format is chosen so that it will contribute to the philosophical dialectic. Although I have said quite a lot about the interrelations between the metascientific criteria, not nearly enough has yet been said. The open-source LaTeX program may provide a key foundation for future criticism and contributions. Philosophers who wish to criticize the arguments given by Bunge or who wish to contribute arguments about some of the other relations need only to locate the relevant sections of the LaTeX code and make the adjustments that they argue for. We could do the usual thing in philosophy and present a whole bunch of principles with associated arguments. After all, the conceptual map just shows us a bunch of logical relations. The advantage of the conceptual map is that both its rendering and its

code are compact.

Third, I believe that the arguments about “Ockham’s Razor” are decisive. This term is subject both to vagueness and to historical confusions. It is not that philosophers have nothing to say about simplicity or about principles of parsimony. It is quite clear that many kinds of simplicity play roles in science. Some of those roles may be in principles of parsimony. I offer a revisionary thesis. We should not use the words “Ockham’s Razor” or “Occam’s Razor” ever again. In every case, we should formulate the principle and specify the criteria that govern the judgment, then discuss only the principle that we mean to discuss. Revise “Ockham’s Razor”, I say, for clarity and posterity.

There is an even more devastating result for “Ockham’s Razor”. The popular addition of the *ceteris paribus* clause to principles of parsimony cannot be justified in the philosophy of science. Scientific theories must satisfy many different criteria. The meta-scientific analysis shows us that there is always a trade-off between some kinds of simplicity and others. When we construct competing *scientific theories* we cannot hold all other parameters constant while altering just one. Even if we did have two *different* theories that accounted for the same data, and even if they did give the same predictions (which would be rare if even possible), they would not differ by only one kind of simplicity. Suppose that two hypotheses differed in the kinds of causal mechanisms that they posited. This would be reflected in the syntax of the theories and in their semantics as well. If it were possible to give a fully empirical analysis of the other metascientific criteria, then the two theories, so long as they really are *different* would no longer account for the same data, and hence, they would not really be competitors. In any case, I am doubtful that a fully empirical analysis

of the metascientific criteria is possible. The sign suggesting that this path is a dead end reads, "Hempel's Theoretician's Dilemma - Turn Back Now".

Fourth, this project opens the door for future research in the philosophy of science. I would like to apply these results to a critique of confirmation theory. Our brief historical account of the arguments given by LeVerrier and Newcomb show us that simplicity judgments of various sorts may be involved in the construction of hypotheses which are ruled out for various reasons. Now, some of these simplicity criteria are justified on pragmatic grounds, like those involved in the methods of discrete integration. It is likely that these scientists aimed primarily for another kind of simplicity - a simplicity with respect to concepts. It is plausible that LeVerrier and Newcomb valued a kind of unity of science principle. One version of a unity of science principle is that it aims to unify the encyclopedia of human knowledge. Why would we ask what justified their judgments when we now believe that they were endeavoring to preserve a theory that has been replaced? The reason is historical. When General Relativity was published, scientists were prevented from viewing the first possible solar eclipse to see if the light from distant stars was bent near the sun. They were prevented from performing this experiment because of World War I. Einstein's General Relativity was not without competitors. Dicke and Whitehead also gave general theories of gravitation. Einstein's GR was accepted because it could account for Mercury's perihelion advance. The only reason that scientists had for taking this to be a virtue of the theory was that LeVerrier and Newcomb had argued that Mercury's perihelion advance was a genuine anomaly for Newtonian theory. This, I should think, would lead us to ask if the methods employed by LeVerrier and Newcomb were justified. This suggests

that scientific confirmation and disconfirmation are not as cleanly based on evidence as some authors have suggested (see Achinstein, *The Book of Evidence* 2001, for a detailed discussion).

Finally, the metascientific analysis lays the groundwork for epistemological studies and perhaps cues metaepistemological inquiry. It is true that one result is not surprising. Induction, indispensability and IBE are in a stand-off when it comes to the justification of simplicity. The arguments of the third chapter contribute one small thing to this discussion. They show how, and with what care, these arguments must be constructed by those who wish to defend them.

The more subtle insight that we might gain from the discussion of the justification of simplicity in science has to do with the order of intellectual divisions of labor. I do not yet have an idea about how cleanly the problems of classical epistemology map onto the specific problems of the philosophy of science. It is at least worth asking a question about how much continuity there is between classical epistemology and the philosophy of science. We might put this question another way. How many of the metascientific criteria are also metaepistemological criteria? As a first guess, I would say, not many. It seems unlikely that epistemologists actually aim for syntactical simplicity in their theorizing. It seems unlikely that the kind of generality that we would expect from epistemological theories would be gauged in the same way that we would gauge the generality of scientific theories. Epistemologists have been known to make an appeal to causation in their arguments. The positing of causal mechanisms is one way to systematize a theory and achieve explanatory depth - if, of course, the posits are true. However, the epistemologist should be

wary about borrowing causal mechanisms from science to construct her theories. The final justification for the judgments of simplicity that lead scientists to posit the causal mechanisms that they posit may just depend upon answers to questions in classical epistemology.

At least there is good reason to be suspicious about the degree of continuity between classical epistemology and the philosophy of science. Aristotle did make some important changes to Plato's mixed view. Ever since, the project of science has been committed to a delicate balancing act involving constructing theories about the laws that govern how some physical things change while others remain the same. Aristotle's notions of causation must have been different from Plato's. We should ask if the notions of causation in science are still different from those in classical epistemology. I take the most interesting result of the analysis of simplicity in science to be the inquiry that it suggests. This new investigation would be into the degrees of continuity between classical epistemology and the philosophy of science. This investigation makes it look plausible that the ways that meta-criteria are specified and balanced against one another distinguish activities of different sorts and reveal how different activities are related to one another.

APPENDIX A
SHERLOCK HOLMES AND THE SIGN OF THE FOUR

[Watson is the first-person voice, and he addresses Holmes in this spot to put his skills to the test, right after Holmes has performed one of his "deductions".] "In this case it certainly is so" I replied after a little thought. "The thing, however, is, as you say, of the simplest. Would you think me impertinent if I were to put your theories to a more severe test?"

"On the contrary," he answered, "it would prevent me from taking a second dose of cocaine. I should be delighted to look into any problem which you might submit to me."

"I have heard you say it is difficult for a man to have any object in daily use without leaving the impress of his individuality upon it in such a way that a trained observer might read it. Now, I have here a watch which has recently come into my possession. Would you have the kindness to let me have an opinion upon the character or habits of the late owner?"

I handed him over the watch with some slight feeling of amusement in my heart, for the test was, as I thought, an impossible one, and I intended it as a lesson against the somewhat dogmatic tone which he occasionally assumed. He balanced the watch in his hand, gazed hard at the dial, opened the back, and examined the works, first with his naked eyes and then with a powerful convex lens. I could hardly keep from smiling at his crestfallen face when he finally snapped the case to and handed it back.

"There are hardly any data," he remarked. "The watch has been recently cleaned, which robs me of my most suggestive facts."

"You are right," I answered. "It was cleaned before being sent to me."

In my heart I accused my companion of putting forward a most lame and impotent excuse to cover his failure. What data could he expect from an uncleaned watch?

“Though unsatisfactory, my research has not been entirely barren,” he observed, staring up at the ceiling with dreamy, lack-lustre eyes. “Subject to your correction, I should judge that the watch belonged to your elder brother, who inherited it from your father.”

“That you gather, no doubt, from the H. W. upon the back?”

“Quite so. The W. suggests your own name. The date of the watch is nearly fifty years back, and the initials are as old as the watch: so it was made for the last generation. Jewellery usually descends to the eldest son, and he is most likely to have the same name as the father. Your father has, if I remember right, been dead many years. It has, therefore, been in the hands of your eldest brother.”

“Right, so far,” said I. “Anything else?”

“He was a man of untidy habits – very untidy and careless. He was left with good prospects, but he threw away his chances, lived for some time in poverty with occasional short intervals of prosperity, and finally, taking to drink, he died. That is all I can gather.”

I sprang from my chair and limped impatiently about the room with considerable bitterness in my heart.

“This is unworthy of you, Holmes,” I said. “I could not have believed that you would have descended to this. You have made inquiries into the history of my unhappy brother, and you now pretend to deduce this knowledge in some fanciful way. You cannot expect me to believe that you have read all this from his old watch! It is unkind and, to speak plainly, has a touch of charlatanism in it.”

“My dear doctor,” said he kindly, “pray accept my apologies. Viewing the matter as an abstract problem, I had forgotten how personal and painful a thing it might be to you. I assure you, however, that I never even knew that you had a brother until you handed me the watch.”

“Then how in the name of all that is wonderful did you get these facts? They are absolutely correct in every particular.”

“Ah, that is good luck. I could only say what was the balance of probability. I did not at all expect to be so accurate.”

“But it was not mere guesswork?”

“No, no: I never guess. It is a shocking habit – destructive to the logical faculty. What seems strange to you is only so because you do not follow my train of thought or observe the small facts upon which large inferences may depend. For example, I began by stating that your brother was careless. When you observe the lower part of that watch-case you notice that it is not only dented in two places but it is cut and marked all over from the habit of keeping other hard objects, such as coins or keys, in the same pocket. Surely it is no great feat to assume that a man who treats a fifty-guinea watch so cavalierly must be a careless man. Neither is it a very far-fetched inference that a man who inherits one article of such value is pretty well provided for in other respects.”

I nodded to show that I followed his reasoning.

“It is very customary for pawnbrokers in England, when they take a watch, to scratch the numbers of the ticket with a pin- point upon the inside of the case. It is more handy than a label as there is no risk of the number being lost or transposed. There are no

less than four such numbers visible to my lens on the inside of this case. Inference – that your brother was often at low water. Secondary inference – that he had occasional bursts of prosperity, or he could not have redeemed the pledge. Finally, I ask you to look at the inner plate, which contains the keyhole. Look at the thousands of scratches all round the hole – marks where the key has slipped. What sober man’s key could have scored those grooves? But you will never see a drunkard’s watch without them. He winds it at night, and he leaves these traces of his unsteady hand. Where is the mystery in all this?”

“It is as clear as daylight,” I answered. “I regret the injustice which I did you. I should have had more faith in your marvellous faculty. May I ask whether you have any professional inquiry on foot at present?”

“None. Hence the cocaine. I cannot live without brainwork. What else is there to live for? Stand at the window here. Was ever such a dreary, dismal, unprofitable world? See how the yellow fog swirls down the street and drifts across the dun- coloured houses. What could be more hopelessly prosaic and material? What is the use of having powers, Doctor, when one has no field upon which to exert them? Crime is commonplace, existence is commonplace, and no qualities save those which are commonplace have any function upon earth.”

APPENDIX B
“SIMPLICITY” AND “PARSIMONY”

I suggest that philosophers attempt to reserve distinct uses for the words “simplicity” and “parsimony.” Alan Baker points out that the words are often used interchangeably¹. It is true that the words get used interchangeably by news reporters, magazine writers, writers of fiction and many others who employ the word in colorful ways. But as philosophers we may wish to be more careful. I have three reasons for using the words as I do.

1. The definitions of the two words are different in ways which suggest that they refer to different kinds of things.
2. There are two different things which philosophers might like to refer to, one is a methodological principle and the other is a state of affairs or attribute.
3. Some scientists appear to keep the words distinct in the very way that I suggest we do.

The *Oxford English Dictionary* gives the definition of “simplicity”

Definition B.1. *The state or quality of being simple in form, structure, etc.; absence of compositeness, complexity, or intricacy.*

and of “parsimony”

Definition B.2. *Economy in the use of assumptions in reasoning or explaining; esp. in law of parsimony n. (also principle of parsimony) the prin-*

¹[5]

principle that no more entities, causes, or forces than necessary should be invoked in explaining a set of facts or observations (cf. Ockham's razor n.).

Both are nouns and both have adjective forms like “simplex” and “parsimonious.” However “simplicity” is a state of affairs or an attribute and “parsimony” is an economy in use. These appear to be words which refer to different types of things. I suggest that we use “simplicity” in reference to states of affairs or attributes and that we use “parsimony” in reference to a methodological principle. These are two different things which philosophers wish to discuss and these uses are consistent with the definitions.

I am no lexicographer and probably we would do well to consult one on why the definitions of “parsimony” and “simplicity” are different in the ways that they are. I do not know what sort of arguments might be marshalled for the view that the two words actually mean the same thing. For all I know, the word “parsimony” may be like the word “podium” which has morphed due to pervasive misuse. I consulted the Corpus of Contemporary American English and found that the uses of “parsimony” as a synonym for “simplicity” in spoken word, fiction, magazine articles, newspapers and academic journals was considerable. However, one group uses the words in consistently distinct ways: evolutionary biologists.

In the 2008 *Bioscience* article “The Green Algal Underground: Evolutionary Secrets of Desert Cells” Cardon, Gray, and Lewis use “parsimony” to refer to a methodological principle,

Two methods were used to estimate the number of independent transitions to the desert habitat in green algae: with phylogenetic trees obtained from Bayesian phylogenetic analyses, parsimony reconstruction (under optimizations favoring either reversals or parallel changes) and Bayesian mapping led

to a conservative estimate of 14 to 17 independent transitions from aquatic ancestors to the desert habitat.

David Irwin uses the word in the same way in the 2006 *Bioscience* article “Evolution of Hormone Function: Proglucagon-derived Peptides and Their Receptors”

Phylogenies were generated by both parsimony and distance methods. For parsimony methods, the preferred phylogenetic tree would be one that minimizes the number of mutational steps necessary to account for the diversity of amino acid sequences.

Elliott Sober works on the philosophy of evolutionary biology, and he uses the words this way in *Reconstructing the Past, Parsimony, Evolution, and Inference* [54]. Elliott Sober is one of the world’s leading experts on simplicity and parsimony. I suggest that we follow these careful scientists and philosophers in the use of these terms.

APPENDIX C
METASCIENTIFIC CRITERIA CONCEPTUAL MAP

```

%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%
% Author   : Dan Schulz (Jan. 2011)
%
% Title    : Map of the metascientific criteria
%           and their interrelations
%
%           Affiliated with the University of Iowa 2011
% Notes    :In LaTeX 2e with tikz graphics package.
%
% Tags     : simplicity, metascientific criteria, mindmap,
%           layers,
%
\documentclass{article}

\usepackage{tikz,times}

\usetikzlibrary{arrows,positioning}

\usetikzlibrary{mindmap,backgrounds}

\begin{document}

\tikzstyle{root concept}+=[concept color=gray!80, text=black]

```



```

\tikzstyle{level 1 concept}+=[set style={{every child}=[
concept color=blue!50, inner sep=8pt, minimum size=9mm]]

\begin{figure}[h]

\centering\scalebox{.5}{

\begin{tikzpicture}[mindmap,level 1 concept/.append
style={level distance=130,sibling angle=40},
extra concept/.append style={color=blue!50,text=black}]

\begin{scope}[mindmap]

\node [concept] at (0,0) {\small Simplicity}

[clockwise from=-30]

child [concept]{node [concept] (prag)

{\footnotesize Pragmatical}}

child {node [concept] (epi)

{\footnotesize Epistemological}}

child {node [concept] (sem) {\footnotesize Semantical}}

child {node [concept] (syn) {\footnotesize Syntactical}}

child {node [concept] (ont) {\footnotesize Ontological}};

\end{scope}

% Accuracy

\begin{scope}[mindmap]

```

```

\node [concept, concept color=yellow, text=black](acc)
  at (-7.2,-4)
  {\small Accuracy};
\end{scope}

% Linguistic Exactness
\node [concept, concept color=orange, text=white](lin)
  at (-8.6,-9)
  {\small Linguistic Exactness};

% Testability

\begin{scope}[mindmap, concept color=red, text=white]
\node [concept](tes) at (-8.3,-14) {\small Testability}
  child [grow=35]
    {node [concept] (scr) {\footnotesize Scrutability}}
  child [grow=-5]
    {node [concept] (fal) {\footnotesize Falsifiability}}
  child [grow=-222]
    {node [concept] (fit)
      {\footnotesize Predictive Power/Fit}}
\end{scope}

```

```

;

\end{scope}

% Sytematicity

\begin{scope}[mindmap, concept color=violet,text=white]
  \node [concept](sys) at (-7.2,-20) {\small Systematicity};
\end{scope}

% Scientific Unity

\begin{scope}[mindmap, concept color=blue!50!black,text=white]
\node [concept, text=white](uni) at (-2,-8.5)
  {\small Unity of Science};
\end{scope}

% Conceptual Connectedness

\begin{scope}[mindmap,
concept color=blue!80!black,text=white]
\node [concept, text=white](ccd) at (-2.,-18)
  {\small Conceptual Connectedness};
\end{scope}

```

```

% Representativeness

\begin{scope}

  \node [concept, text=white](rep) at (2.4,-15)
    {\small Representativeness};

\end{scope}

% Depth

\begin{scope}[mindmap,
concept color=green!50!black,text=white]

\node [concept](dep) at (4,-9.5) {\small Depth};

\end{scope}

% Relations between metascientific criteria

\begin{pgfonlayer}{background}

\draw [>=stealth', ->, line width=4pt, gray]

(ccd) edge (sys)

(syn) edge (sys)

(rep) edge[bend right=28] (sys)

(rep) edge (dep)

(dep) edge (uni)

```

```

(rep) edge (uni)

(uni) edge (ccd)

;

\draw [>=stealth', ->,
line width=4pt, dashed, gray]

(ont) edge[bend right=60] (syn.west);

\draw [>=stealth', <->, thick,
shorten <=2pt, shorten >=2pt]

(lin) edge[bend left=45] (syn.west)

(fit) edge[bend left=45] (acc.west)

(rep) edge (ont)

(epi) edge (syn)

(epi.north) edge[bend right=45] (sem.north)

(epi.east) edge[bend left=45] (dep.east)

(scr) edge[bend right=35] (prag)

;

\draw [>=stealth', <->, thick, dashed,
shorten <=2pt,
shorten >=2pt]

(scr) edge[bend right=25] (syn.south)

```

```

;

\draw [>=stealth', ->>, thick, shorten <=1pt,
shorten >=1pt]

    (sys) edge (tes)

    (lin) edge (tes)

    (lin) edge (acc)

    (fal) edge (ccd)

    (dep) edge (fit)

    (dep) edge (prag)

    (dep) edge (epi)

    (dep) edge (sem)

    (dep) edge (syn.south)

    (fal) edge[bend left=25] (syn)

    (fal) edge[bend right=25] (sem)

;

\end{pgfonlayer}

\end{tikzpicture}

}

\caption{}

\end{figure}

\end{document}

```

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