5-2013

It's Not So Simple: The Role of Simplicity in Science and Theory.

C. R. Gregg
East Tennessee State University

IT'S NOT SO SIMPLE:
THE ROLE OF SIMPLICITY IN SCIENCE AND THEORY

Thesis submitted in partial fulfillment of Honors

By

C. R. Gregg
Philosophy
Honors In Discipline Program
East Tennessee State University

May 3, 2013
(Updated) May 7, 2013

Dr. David Harker, Faculty Mentor

Dr. Jeffrey Gold, Honors Coordinator

Dr. Gary Henson, Faculty Reader

Dr. Allen Coates, Faculty Reader

Paul Tudico, Faculty Reader

ACKNOWLEDGMENTS

I would like to sincerely thank:

David Harker, my mentor and friend.

Paul Tudico, who is largely responsible for my choice to pursue Philosophy.

Leslie MacAvoy, without whom I would not be an HID student.

Karen Kornweibel, for believing in me.

Rebecca Pyles, for believing in me.

Gary Henson and Allen Coates, for offering their time to serve as readers.

Marie Graves, for her tolerance throughout the innumerable years of my education.

IT'S NOT SO SIMPLE:
THE ROLE OF SIMPLICITY IN SCIENCE AND THEORY

C. R. Gregg

May 3, 2013

TABLE OF CONTENTS

1. Introduction and Motivation

Occasionally a term so integrates into our way of thought that we take its implications for granted. Years of use and abuse muddle its meaning and fray the edges of its semantic boundaries. *Simple* is one such term. Subjectively, what appears simple to one is complex to another; Einstein certainly found particular equations simple that I have not. Simplicity sometimes entices, as with the *simple solution*, but other times marks incompetence—nobody wants to be *called* simple. Sometimes simplicity is termed *parsimony* and other times *elegance*. Additionally, it is often unclear whether *simple* maintains connotation in cross-disciplinary dialogue, or whether each camp employs some proprietary notion. Part of the project of philosophy is to bring clarity to confusion; this paper, in a small way, contributes to that cause.

2. Historical Treatments of Simplicity

Certainly, the most widely known formulation of a principle of simplicity[1] is Ockham's Razor. Though the nomenclature is spurious, given that there is no evidence that Ockham put forth his own axiom of parsimony,[2] he did nonetheless espouse a version of simplicity something like:

> Plurality is not to be assumed without necessity.
>
> Or
>
> What can be done with fewer [assumptions] is done in vain with more. (Barnes 2000)

[1] Unless as noted in the discussion, I use *simplicity* and *parsimony* interchangeably.

[2] Perhaps some find this assertion as surprising as I did, so a note is in order. I came across several sources in my research that make this claim or allusions to the same; for more information see the endnotes of Barnes (2000, 371) where he refers to Edward's (1967, 307) discussion of the issue in *The Encyclopedia of Philosophy.*

This idea, now attributed to Ockham, has had several proponents, each with their own proprietary formulation.[3]

Aristotle writes:

> We may assume the superiority [all other things being equal] of the demonstration which derives from fewer postulates or hypotheses (Posterior Analytics 25)

Aquinas echoes this, saying:

> If a thing can be done adequately by means of one it is superfluous to do it by means of several; for we observe that nature does not employ two instruments where one suffices. (1945, 129)

As does Kant, in his *Critique of Pure Reason*:

> Parsimony of principles is not merely an economical principle of reason, but an essential law of nature. We cannot understand, in fact, how a logical principle of unity can of right exist, unless we presuppose a transcendental principle, by which a such a systematic unit—as a property of objects themselves—is regarded as necessary a priori. . . . That the same unity exists in nature is presupposed by philosophers in the well-known scholastic maxim, which forbids us unnecessarily to augment the number of entities or principles . . . (Kant 633-36)

The greats of practical science usurped this idea, as seen in Newton's *Principia Mathematica*:

> We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances. (Barnes 2000)

And

> Nature is pleased with simplicity, and affects not the pomp of superfluous causes. (Newton 1964, 398)

Galileo and Lavoisier both echo this same sentiment in various ways, as does Einstein in saying:

> The grand aim of all science . . . is to cover the greatest possible number of empirical facts by logical deductions from the smallest possible number of hypothesis or axioms. (Baker 2010)

[3] Alan Baker (2010) offers a wonderfully concise historical survey of the development of Ockham's Razor in his entry on simplicity for the *Stanford Encyclopedia of Philosophy*. Much of my introduction unabashedly follows his work.

So, it can be said that both philosophers of science and practicing scientists have warmly welcomed the principle of simplicity. One finds its synonyms bolded in classroom texts for logic, science, speech and rhetoric; it is ubiquitous. This ubiquity contributes to the difficulty of defining simplicity.

2.1 The Salient Features of Simplicity Traditionally Understood

In beginning to parse the issue, we can reflect on the quotes just offered and notice some trends. Among them are: mentions of necessity, adequacy, and superfluousness; allusions to economy; appeals to nature; considerations of quantity; and a suggestion that simplicity ought to guide inquiry (be it scientific or otherwise). These themes are indicative of how philosophy has traditionally defined simplicity, the salient features of which we can enumerate as:

1. Necessity: *S(x)*[4] *is the bare minimum of reality*. This could be taken metaphysically to mean that S(x) is a minimal condition of being. Or, one can understand it materialistically to mean that S(x) is the smallest unit of matter (an atomistic notion).

2. Adequacy: *S(x) offers enough explanation*. The premises of an argument or postulates of an explanation should be robust enough to satisfactorily prove conclusions or account for phenomena, respectively. The simple explanation does not leave one wanting.

3. Non-Superfluousness: *S(x) doesn't offer extra*. Just as Adequacy suggests that S(x) doesn't leave one wanting, it likewise doesn't leave one overly burdened; there are no extra explanatory bits that profit nothing.[5]

4. Naturalism: *S(x) accurately aligns with the world*.[6] Newton, Galileo, and Lavoisier invoke this notion in saying that the world operates simply. Simple explanations can be

[4] S(x) being *that which is simple*.
[5] Naturally, more on this later in my paper. It is admittedly ambiguous here, as *profiting nothing* may mean "does nothing to explain" or "does not help make predictions," among other things.

said to mimic the simplicity of the world itself. This is like Necessity, but is notably different: Necessity argues that the world is fundamentally comprised of simple things, Naturalism maintains that nature operates simplistically.

5. Conservatism. *S(x) is quantitatively sparse*. Simple theories are like the simple machines of physics. Simple things have fewer constituent parts; simple theories have fewer constituent premises. This is like Non-Superfluousness in that it quantitatively limits premises, but is less particular because it does not appeal to qualitative features. In other words, while Non-Superfluousness relates to bits that profit nothing, Conservatism relates to bits, period.

In addition to these 5, there are 2 other considerations that are frequently associated with simplicity, like beauty and ease of understanding. The first 5 may be viewed as more formal treatments of parsimony, while these 2 are more informal but just as pervasive—perhaps more so, since these connotations extend outside of the philosophical and scientific community.[7] So, we add:

6. Beauty: *S(x) is aesthetically appealing*. This is not to be taken absolutely; Gothic architecture and Baroque music both serve as counterexamples. However, many traditions (like Zen, for one) view less as more (beautiful). The currently circulating magazine *Real Simple* attests to this, featuring product displays with clean, geometric lines and primary color palettes.[8]

---

[6] This statement is admittedly akin to a Pandora's box of epistemic questions. In other words, take this claim lightly. All I mean to say here is that simplicity respects experience insofar as S(x) accurately articulates observations.

[7] In my research, I often felt as though parsimony had become so elevated a concept as to become divorced from how *simple* is so often used. Some philosophers may have dismissed pedestrian definitions of simplicity in favor of the analytical, ostensibly to differentiate folk simplicity from philosophical parsimony. But when investigated, this often proves a false dichotomy.

[8] Amusingly, the subtitle of *Real Simple* is *Life Made Easier*. Perhaps.

7. Cognoscibility: *S(x) can be better understood*. Elementary school math is *prima facie* simpler than calculus; John Q. Public understands addition while astrophysics is reserved for the few. This relates to Conservatism in that children's books, for example, appeal to a smaller active vocabulary. However, Cognoscibility emphasizes the qualitative experience we have in coming to understand (e.g., learning a foreign language is hard not because it uses *more* words, but because it uses *different* words.)

Lastly, there has been a recent push to understand parsimony in light of Bayesian analysis and various statistical models. So, we add two final features:

8. Probability: *S(x) is the most probable of given options*. This understands simplicity as an impetus towards truth, guiding science to the most likely of theories.

9. Accuracy: *S(x) is the most accurate of given options*. Given an array of data, simplicity helps discern the noise from the signal, and generates a efficacious and precise practice.

Before we begin examining each of these features in depth, it may help to arrange them into a conceptual schema. I suggest:

Evaluative Criteria *broadly construed as:*	**Assessment** *assesses S of S(x) in regards to:*	**Feature** *salient feature that determines its category:*
Quantitative		Conservatism
Qualitative	Appeal	Beauty
		Cognoscibility
	Ontology	Necessity
		Naturalism
		Adequacy
		Non-Superfluousness
Pragmatic		Probability
		Accuracy

This provides a roadmap by which to address each feature, and generally proceeds, going top to bottom, from what I contend are the most spurious or clouded notions of simplicity towards ones with most viability and clarity.

3. Quantitative Assessment

Evaluations of S(x) in quantitative terms refer to the number of its constituent parts. This type of assessment equally applies to tangibles and intangibles. A truly quantitative analysis of simplicity concerns itself only with number, and not with kind. It is an enumerative analysis. To evaluate S(x) enumeratively is to count its parts and regard the total as a measure of its simplicity. Though this may seem unsophisticated *prima facie*, it is common practice. We learn early in our study of sciences that a *simple machine* alters a force (its direction or magnitude) without recourse to complex mechanisms. That is, the six simple machines—lever, wheel and axle, pulley, screw, wedge, and inclined plane—effect change of force in ways we perceive as simple. After all, several are ordinary items like crowbars, doorstops, and knives.

Colloquially, we refer to devices as simple and complex in terms of their moving parts. What more is a Rube Goldberg machine other than a complicated way to do a simple task? We likewise are quick to say something "looks complicated" because it has many parts to be dealt with—the more parts a puzzle has, the longer it takes to complete; the more steps required in assembling the baby's new bassinet, the more the father laments.

Upon reflection, we see this to be a capricious way to talk of simplicity. The Chinese game Go has but three parts: white stones, black stones, and a playing grid. Yet, volumes of intricate game theory abound, spelling out tactics both technical and psychological. The simple appearance of Go belies the experience of its play. Carnivals have exploited this principle for ages, enticing suckers to "simply" ring a bottle or topple milk cans. With education, we come to understand these games as "rigged" or "unfair"—

and we say this to convey that the objectives of the game are extremely difficult to achieve.

3.1 CONSERVATISM: S(X) IS *QUANTITATIVELY SPARSE*.

Conservatism is a squarely quantitatively parsimonious principle. E. C. Barnes offers an account of how quantitative simplicity has traditionally been justified (2000, 356). He describes 5 such justifications:

1. *The general background knowledge justification*: This turns on the belief that nature itself operates simplicity and exhibits a penchant for paucity. This is what I term Naturalism.
2. *The pragmatic justification*: Our preference for simplicity is due to purely aesthetic or pragmatic concerns; the first justification is what I term Beauty.
3. *The unification justification*: Unified theories are confirmed by a greater number of phenomena than less unified ones. I contend this is an argument not of quantity but of evidential relation, and hence it belongs in our later discussions on Adequacy and Non-Superfluousness.
4. *The anti-free parameters justification*: Theories with fewer adjustable parameters (that must be fixed to yield a result) are required to make fewer assumptions and thus are preferable.
5. *The local background knowledge justification*: This argument denies that simplicity is globally justified and favors understanding S(x) in relation to particular applications or specializations. This justification has my sympathies, and will be invoked several times in the latter sections of this paper.

As noted, I address 1-3 and 5 in upcoming sections. As to 4—the anti-free parameters and local background knowledge justifications—let us briefly turn to an astute observation by Elliot Sober (2002, 8):

Take two equations, one linear, and the other parabolic:

(LIN) $y = a + bx$

(PAR) $y = a + bx + cx^2$

To decide which is more conservative, one can either assess these equations in terms of their free parameters or their entailed assumptions. To the first, (LIN) is simpler, since it has only two adjustable parameters.[9] However, in terms of assumptions made, we see that (PAR) is simpler, since (LIN) assumes that $c = 0$ while (PAR) does not; (LIN) entails more background assumptions. As such, it is difficult to decide *what* to enumerate when conducting an enumerative evaluation of simplicity. Do we count assumptions, postulates, parameters, or something else?

This issue of free parameters and background assumptions will reappear when we discuss parsimony's role in Accuracy. For now, I will only say that principles like Conservatism (and other quantitative modes of assessment) often collapse into considerations better articulated differently. The allure of quantitative sparseness, for example, can be understood as a principle of beauty. The pragmatic payoff of having fewer adjustable parameters is a gain of accuracy, and thus should be found in discussions of efficacy. All of this is to say that qualitative parsimony for the sake of

[9] Sober adopts this position in some of his writings (cf. Forster and Sober 1994, Sober 1996). But, as we will come to see, Sober argues elsewhere that simplicity only makes sense given background knowledge—that is, given a particular, localized realm of application (biology, physics, mathematics, etc.)—hence the *local background justification* listed above as (5).

qualitative parsimony makes little sense; one finds value in sparse enumeration due to its effects, not the sparseness itself.

4. Qualitative Assessment

Evaluations of S(x) in qualitative terms means assessing not (only the) number of constituent parts, but their kind. It applies equally to tangibles and intangibles.

4.1 Appeal

Appeal is a vague notion that Quine briefly notes in *Word and Object*:

> [The] supposed quality of simplicity is more easily sensed than described (1960, 19).[10]

This favoring of simplicity seems intuitive in light of the previous discussion of Conservatism; often we traverse the path of fewest steps or purchase the bike requiring least assembly. We saw that these enumerative considerations collapse into affect: the time, labor, or effort involved;[11] quantitative concerns often reduce to qualitative ones.

> Whatever simplicity is, it is no casual hobby. As a guide of inference it is implicit in unconscious steps as well as half explicit in deliberate ones. The neurological mechanism of the drive for simplicity is undoubtedly fundamental though unknown, and its survival value overwhelming (Quine 1960, 20).

Saying that simplicity appeals to us only leads to questioning *why* it appeals or *what qualities* we find appealing. I would like to offer two possible ways in which S(x) appeals to us: aesthetically and cognitively.

[10] From §5 of *Word and Object*, entitled "Evidence." The passage that follows the one quoted here explains this "sense of simplicity" as "perhaps . . . in many cases just a feeling of conviction attaching to the blind resultant of the interplay of chain stimulations in their various strengths."

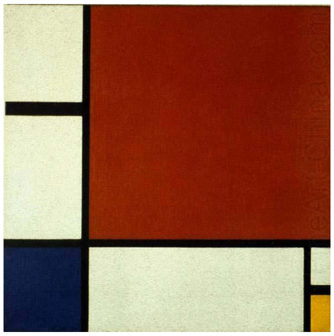
[11] One could argue that time, labor, or effort could still be quantitatively assessed in some metric, i.e., seconds or joules. But here I am asserting that the motivation for choosing S(x) is a qualitative notion; we don't *desire* to dispose of the time or expend the effort. As such, it belongs here in my discussion of qualitative assessment.

4.1.1 Aesthetic Appeal: Beauty: *S(x) is aesthetically appealing.*

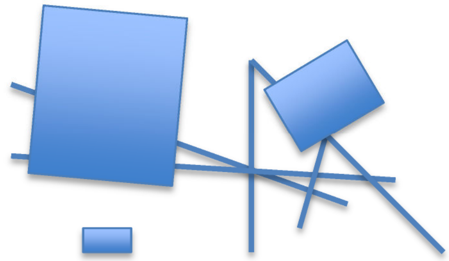
Aesthetic philosophies are notoriously elusive because of the notion that aesthetic preferences, i.e., matters of taste, are highly subjective and hard to codify. Whether this is a justified belief is irrelevant at this time, and we will take as a loose working definition of aesthetic criteria as those that pertain to sensory experience. Understanding what senses are engaged when we are drawn to a particular option among an array of options is sometimes difficult; it is not always apparent why we prefer one thing to another. In regards to simplicity, we may feel that S(x) is simple but remain unable to articulate why. When this is coupled with a notion of sensual attraction, we can employ talk of Beauty.

S(x) might be said to be beautiful by virtue of its simplicity. Or, S(x) might be said to be simple by virtue of its beauty. Both conceptions are nebulous. The first statement asserts that a thing is beautiful because it is simple; this does not help, since a beautiful thing might not readily be thought simple (like, as mentioned earlier, Gothic architecture or Baroque music). The second formulation asserts that a thing is simple because it is beautiful. I suspect we are immediately we are ready to dismiss this second statement out-of-hand. We do not, philosophically or colloquially, readily refer to beautiful things as simple. Nonetheless, we do sometimes conjoin (and perhaps conflate) simplicity and beauty. Consider two examples.

Firstly, the most obvious place to turn when discussing beauty and simplicity is to the art world. Consider:



Composition II in Red, Blue, and Yellow,
Mondrian



A Bunch of Junk,
Gregg

The painting on the left is by renowned artist Piet Mondrian, and the one on the right is by yours truly. Both contain 5 lines and 3 rectangles. Yet, the one on the left (at least, according to authorities on such things) conveys a sense of perfection and beauty that the one on the right does not. Mondrian's works might be referred to as simple works of beauty, or simply beautiful, or beautiful simplicity. In fact, the simplicity of his work gives impetus to some curmudgeonly critics that say, "Psh, I could do that!"

This is a silly example that proves a substantive point. While S(x) might also be beautiful, much of its beauty derives from the relations of its parts, embedded significance, or social connotation, and not just its constituent parts.

Mathematicians might say a given proof is beautiful or elegant when its premises prove its conclusion as swiftly as possible, without meandering or unnecessary steps. A proof might also be termed *clever* in light of how its premises unfurl, or because its proponent deftly performs an unanticipated maneuver (much like an overlooked mate in chess).

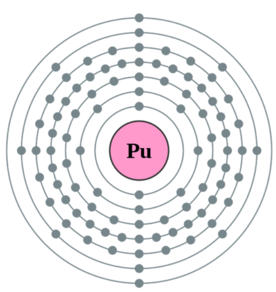
We do venerate tidy proofs, but it's not their beauty we seek. Instead, we want proofs to be elegant in light of other criteria—ease of use, pedagogical utility, clarity of concept—qualities that I have relegated to other salient features of simplicity. Beauty only hitches a ride on simplicity, it doesn't drive the cart.

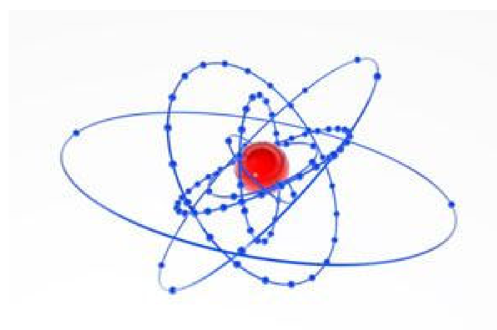
4.1.2 Cognitive Appeal: Cognoscibility: *(S)x can be better understood.*

Much like how aesthetic criteria relate to the sensual experience, cognitive criteria relate to non-sensual experience, insofar as we respect a division between thinking and feeling.[12] Here we briefly consider that the appeal of simplicity may relate to our rational faculties.

I will take it as self-evident that one can remember a shorter list of items better than a longer one. I also take it on intuition that we generally regard simpler things as easier to comprehend. This is, of course, why children's books vary greatly from adult novels, and why 3rd grade math is several steps away from calculus.

One might argue that some theoreticians are engaged in the parsing and arranging of information to make concepts easier to understand and that scientists do the same when presenting their conclusions. For example, a chemist might employ either of the models below in representing plutonium.





Both convey information regarding the atomic makeup of Pu. However, do we all agree which model is simpler? I suspect this is a matter of taste, and this is analogous to any other sort of theoretical abstraction or conceptual modeling in science; given any

[12] This is a division that I am unsure of myself. But, whether the two can be regarded as exclusive is not important here; I am seeking clarity between how S(x) appeals to us in an emotive, affective manner (Beauty) in contrast to its appeal in a reasoned, rational manner (Cognoscibility). My suspicion is this line is indeed not so clear.

phenomenon, it is difficult to determine which is the simpler-to-comprehend abstraction between two competing abstractions.[13]

A geneticist may have an easier time understanding genetic theory than a physicist, who in turn may more aptly understand current research of sub-atomic particles than the geneticist. But to say that genetics is more or less simple than particle physics is nonsensical. The explanation for ease of cognoscibility is found in the background knowledge of the scientists, not the simplicity of the theories themselves.

It is not a stretch to suspect that empirical studies may yield trends suggesting most people more easily understand some concepts than others. I suspect that the general public, if polled, would more readily comprehend a story about Frank buying a hot dog than one about conjugating amino acids. But this is easily explained in that most people are not as familiar with amino acids as they are buying hot dogs—again, background knowledge.

Perhaps tests that (ostensibly) do not rely on prior knowledge or schooling, like the Stanford-Binet test of intelligence, might offer some benchmark of simplicity in terms of cognoscibility, but defining simplicity in this way presents several problems. Firstly, there is a question of vagueness regarding how to index simplicity to cognoscibility—Given a simple concept, to what percentage of the population should it be understandable? Of course, S(x) defined in any manner will always face questions of vagueness and arbitrariness, so this point is uninteresting.

Secondly, creating simplified models (like the Bohr model, above) often demands that information be blunted or "dumbed down."[14] As such, it makes little sense that

---

[13] See 3 paragraphs later for a caveat to this.
[14] In that many details are exempted.

cognitive simplicity should serve as a methodological guide to (philosophical and scientific) practice. If simplicity is a virtue of practice, either as an end to itself or a means to an end, then practice should proceed towards simplicity or through simple means, respectively. Cognoscibility defines S(x) in terms of understanding. But, to conduct (scientific or philosophical) inquiry with the goal of comprehensibility preferences form over content. Should not (something like) accuracy of concept be the guiding principle when drawing conclusions, and simplicity of presentation a secondary consideration? Cognoscibility is a helpful pedagogical criterion but fails to obtain as a broad methodological virtue.

4.2. Ontology

Thus far, our discussion has focused on definitions of S(x) referent to form; Conservatism, Beauty, and Cognoscibility all place emphasis on the manner in which postulates are arranged or posited. However, a large part of the literature on simplicity concerns itself with *ontological* simplicity. This distinction places emphasis not on the number or manner of postulates but rather their kind (ontic significance). S(x) now becomes a question of how we speak of the world rather than just how we speak. The simplicity of *modus ponens* is intuitively conservative, beautiful, and cognizable. For

$$\begin{array}{c} P \supset Q \\ \underline{P} \\ \therefore Q \end{array}$$

means little until we define our variables. Eyebrows rise once we discover that P stands for unicorns or faeries or the like; the argument's validity stands but its soundness is thrown into question.

This highlights a common distinction between two types of simplicity: *parsimony* and *elegance*. Our *modus ponens* example is elegant but not necessarily parsimonious.

Elegance was mentioned earlier during the discussion of Beauty, but it need not always be construed as a purely aesthetic distinction—it might be instead taken as a linguistic phenomenon.[15] As germane to science, however, parsimony is the greater concern, since science is in the business of explaining the world. The pursuit of ontological clarity is a philosophical project, and marks a turn in our discussion from more folkish notions of simplicity towards the more esoteric, highly relativized ones. Quine remarks in *Theories and Things*:

> The common man's ontology is vague and untidy in two ways. It takes in many purported objects that are vaguely or inadequately defined. . . . The boundary between being and nonbeing is a philosophical idea, an idea of technical science in a broad sense. Scientists and philosophers seek a comprehensive system of the world, and one that is oriented to reference even more squarely and utterly than ordinary language. Ontological concern is not a correction of a ay thought and practice; it is foreign to the lay culture, though an outgrowth of it. (1981, 9)

Quine notes this to give impetus to his call for theoretical regimentation, a demand for more particular and logically consistent language in which to conduct scientific inquiry. His goal is not merely to tighten our prose, but also to reduce the ontological commitments of our theoretical language, i.e., ontological reductionism.

Our concern as to what role simplicity should play in scientific theory and practice amounts to questioning the limits of ontological licensure we give to science. If we posit ontological simplicity as a precept of scientific practice, then we effectively draw lines not around how we speak of the world but the world itself; in saying a postulate violates the rules of ontological simplicity, we reject both the postulate and its

[15] Here I have in mind some arguments regarding the structure of observation sentences and the like. I do not have time to develop them here.

referent.[16] The rejection of a postulate because it violates ontological simplicity is not a rejection of form (as with elegance) but of content: the postulate's positing of an entity in the world.[17] As such, we can say that assessing S(x) in ontological terms has to do with how its parts (say, its hypotheses) relate to things in the world, i.e., evidence.

A thorough discussion of what counts as evidence is beyond the scope of this paper. For our purposes, we can regard evidence very generally as observed phenomenon. One can set aside, for now, debate as to what is actually observed as opposed to inferred, since evidence construed as either is sufficient to count as evidence in our discussion.

The idea of assessing S(x) in terms of evidential warrant means determining if S(x) aligns with the world in a distinct manner. At question is how to understand *distinct*. It could be taken to mean that evidence necessitates each and every postulate of a theory such that simple theories contain only the postulates necessitated by evidence. Or, it may mean that the postulates of a simple theory adequately explain the given evidence, thus there is no need for more. The next few sections grapple with clarifying these notions.

4.2.1 Ontological Minimalism: Necessity: *S(x) is the bare minimum of reality.*

Simplicity can be conceptualized as a continuum from most simple to most complex, even if it is unclear what constitutes each extreme. It follows, then, that S(x) would be found at the lowest (most simple) end of the continuum. Taken as an ontological claim, this renders S(x) as the most basic constituent of reality—as an atomic

[16] It is hard to discuss this point without addressing many linguistic issues that may beg the question. Here, my use of referent implies a word-object semantics that I do not necessarily wish to imply. Take referent to mean only, and loosely, an object of experience or phenomenon of scientific study.
[17] Whether the rejection is to the postulate or the posited entity itself is an interesting question and begs for clarity. As this paper examines simplicity as a precept of methodology, I lean towards the former. However, it stands to reason that if one objects to the postulate Px, one might be doing so either in objection to P or x.

unit. I have employed the term *Necessity* to convey that positing any possible world likewise posits its simplest components;[18] simple things are the ones that must exist for a world to exist. One could interpret this metaphysically (S(x) as an ontic unit of being) or more concretely (S(x) as a the basic force, matter, or law of the universe). The former interpretation is dangerously metaphysical, and I have little to say regarding units of being. The second idea, however, is germane to our discussion.

If S(x) is regarded as fundamental,[19] then much of science (and philosophy) is ostensibly engaged in trying to uncover the simplest bits of the universe. But to think of S(x) as the "most basic" constituent of reality in this way so restricts what can be said of simplicity as to render it methodologically impotent. That is, if one can only rightly call the fundamental things of the universe "simple," then we shall indeed be calling very few things simple. Furthermore, science is in a constant state of reform regarding notions of elementary particles; at one time, atoms were considered indivisible, but particle physics has sense challenged this notion. Reserving the label of simple for elementary particles effectively takes simplicity off the table for most practicing scientists.

This is why *simple* must be regarded as adjectival and not nominal,[20] with theory and practice being shaped by concerns for simplicity without seeking simplicity itself. To define simplicity nominally—and thus reify it—posits an end goal of investigation that we cannot obtain. This is because 1) it is unclear in what sense one unit[21] is more elementary than another (Why preference indivisibility as the criterion by which to

[18] One can argue that possible worlds are not a realized worlds, thus they are unpopulated, empty, or nonexistent. Take my term *positing* to mean *realizing*, hence putting something somewhere.
[19] Given theoretical primacy in some way.
[20] Literally in the sense of a noun, being a person, place, or thing. I employ "nominal" not to align with the metaphysical school of nominalism, but to contrast it with "adjectival;" it is a rhetorical device.
[21] Any "thing" of reality.

evaluate the elementaryness of a particle?) and 2) we cannot determine when we have hit rock bottom (the epistemic problem highlighted by the historical shift from atom to particle as elementary) and 3) we cannot seek to find nominal simplicity without first knowing what we are seeking (a different sort of epistemic problem). In contrast, adjectival simplicity can 1) reach comparative conclusions between options, describing S(x) as simple without saying it is *the* simple and 2) continue to work as an evaluative criteria even when science contends to have hit pay dirt[22] and 3) direct practice in seeking parsimonious explanation without presupposing what is ultimately parsimonious.

However, dumping a nominal notion of simplicity does not estrange simplicity from nature. Many scientists contend just the opposite—that nature reveals its simplicity to us—as our the next section shows.

4.2.2 NATURALISM: *S(X) ACCURATELY ALIGNS WITH THE WORLD.*

Many of the greats of philosophy and science have expressed belief that nature itself operates according to simple principles. Consider the following quotes, some of which were introduced earlier.

Aquinas:

> [W]e observe that nature does not employ two instruments where one suffices (1945, 129).

Newton:

> Nature is pleased with simplicity, and affects not the pomp of superfluous causes (1972, 398).

Galileo:

[22] As with many times in the history of science when beliefs taken as fundamental are later superseded.

> Nature does not multiply things unnecessarily; that she makes use of the easiest and simplest means for producing her effects; that she does nothing in vain, and the like (1962, 397).

While the last notion of ontological simplicity we examined, Necessity, seeks to discover the simple constituents of nature, Naturalism contends that the *operations* of nature are simple. Thus, in developing theory with simplicity in mind, we are mirroring natural processes; our explanations are simple because they accurately align with the simplicity inherent in the universe. Leonard Nash says:

> [There are] regulative principles, with implications for the conduct of science; and a much larger group of substantive principles constituting "established knowledge" for the scientists of an age. . . . [I]n effect, the regulative principles assert something about the optimal construction of science, whereas the substantive principles assert something about the actual construction of the world. To be sure, this distinction is not perfectly clean-cut. If we accept a particular regulative principle, and seek to build scientific knowledge in certain ways, we do so only because we also accept a certain conception of the object of knowledge: the construction of the world. (1963, 170)

And echoing this, Hugh Gauch:

> Clearly, if parsimony is a sound epistemological principle, that must be because more fundamentally parsimony is also a realistic ontological principle. Simplicity of theories gets its value from simplicity of nature; otherwise, parsimony is ajar with reality and hence senseless. (2003, 332)

And to this I say, no. I need not necessarily claim that nature is simple to justify simplicity of method; I can gain purchase by appealing strictly to pragmatics. A plain retort to both Nash and Gauch is that I adopt simplicity because it nets me practical gain without making any sort of ontological claim whatsoever. Gauch continues:

> So, is nature simple? Or more precisely, is nature simple at least in some significant sense that underwrites the methodological principle of parsimony? The answer to that question is, and must be, yes. But this answer is surpassingly difficult to explain satisfactorily. (332)

In what way *must* the answer be yes? Again, my methodological principle need not necessarily be underwritten by a particular ontology; what is required to substantiate a methodology is contingent on what one wishes to do with that methodology. But Gauch offers this for consideration:

> [S]implicity does pervade nature. For starters, understand that the reality check is itself a *simple* theory about a *simple* world. It declares that "Moving cars are hazardous to pedestrians." This is simple precisely because it applies a single dictum to all persons in all places at all times. The quintessential simplicity of this theory and its world, otherwise easily unnoticed, can be placed in bold relief by giving variants that are not so simple. For example, were nature more complex than it actually is, more complicated variants could emerge, such as "Moving cars are hazardous to pedestrians, except for women in France on Saturday mornings and wealthy men in India and Colorado when it is raining." Although there is just one simple and sensible formulation of the reality check, obviously there are innumerable complex and ridiculous variants. Regarding cars and pedestrians, a simple world begets a simple theory. Or, to put it the other way around, a simple theory befits a simple world. . . . [P]arsimony [is] everywhere in the world—in iron atoms that are all iron, in stars that are all stars, in dogs that are all dogs, and so on. (323)

This is wrong and patently absurd. I admit I'm somewhat uncertain how to read Gauch's claim, but I have included it here because I believe it echoes the unspoken sentiments of many scientists, namely, that nature is simple because it looks simple! I offer two quick points:

Firstly, there are plenty of instances in nature where, as with his moving cars example, $x\ is\ y\ except\ when\ z$. Consider, for example, that every object in a state of uniform motion tends to remain in that state of motion unless an external force is applied to it; $x\ remains\ y\ except\ when\ z$, where x can be any massive body, and z any number of forces. This entails infinite other statements like "a bowling ball will continue moving forward until the lane slows it, or it strikes a pin, or goes into the gutter, or encounters a walrus . . .” Nature gives us only countless variants upon variants, and not the laws themselves.

Secondly, and at the risk of sounding unsophisticated (perhaps I’m simple!), nothing strikes me as inherently simple about genetic replication, or meiosis, or nuclear fission. As such, I haven’t the slightest notion what it means to say that nature is simple. Nonetheless, it does seem that many people smarter than I have embraced this view, and thus we will examine it now.

The history and development of early astronomy is often cited as an example of Naturalism at play. While both Galileo and Kepler espoused a heliocentric, Copernican-style model of the solar system, their motivations for doing so were different. Kepler believed the cosmos operated in accordance with principles of universal harmony, and sought to unite the mysticism of astrology with the science of astronomy (Barker and Goldstein 2001). In contrast, Galileo held that the phenomena of the world operated under uniform principles (Drake 1999, 343). So, both thought that nature was arranged in an orderly schema. At question is what role this belief in natural simplicity played in their scientific pursuits.

Kepler originally maintained that planets moved in circular orbits, in accord with the Ptolemaic model of celestial bodies. He later changed his position and espoused an elliptical model of planetary motion; this became Kepler’s first law of planetary motion: the orbit of every planet is an ellipse with the sun at one of the two foci. But Kepler’s acquiescence to elliptical motion did not align with his pseudo-metaphysical beliefs that the world operates harmoniously—a position better justified through Ptolemaic orbits of perfect, concentric circles.[23] As such, Kepler's methodological guide—celestial harmony—was undermined by the conclusions of his own observational practice.

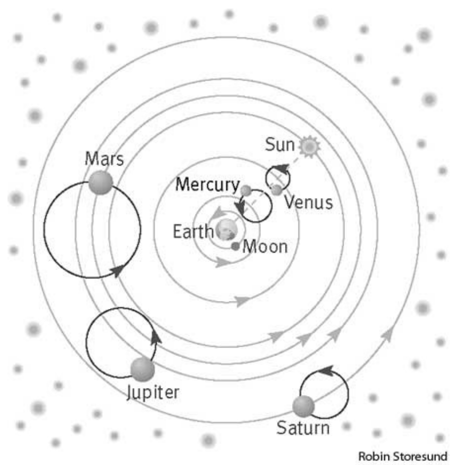
Galileo, in studying tidal forces, reasoned that solely terrestrial motion was responsible for the ebb and flow of oceans. This was in stark opposition to Kepler's assertion that the moon was responsible for the motion of the tides via a lunar force. To Galileo, "the hypothesis of an attraction of the kind suggested by Kepler was equivalent to the invoking of an occult quality" (Drake 1999, 343). Galileo position is now considered wrong; his penchant for uniformity and simplicity may have erroneously influenced his dismissal of Kepler's lunar explanation. Galileo considered Kepler's position to "[create a] kind of influence or power that had no application except to the very phenomenon it was invented to explain" (343). Galileo's methodological guide—parsimonious uniformity—was in this instance an obstacle that retarded understanding tidal motion. However, Galileo's same notions of simplicity did lead him to develop theories of falling bodies (e.g. his observations that a light and heavy rock fall to earth at the same speed) and overturn prior Aristotelian beliefs (that lighter objects fell more slowly).

These phenomena—planetary motion, tidal patterns, and free fall—were once seen as unrelated but are now all explained according to principles of universal gravitation, which amusingly can be said to be both uniformly simple and universally harmonious. Thus, both Galileo and Kepler can rest easily. But as to what role respect for

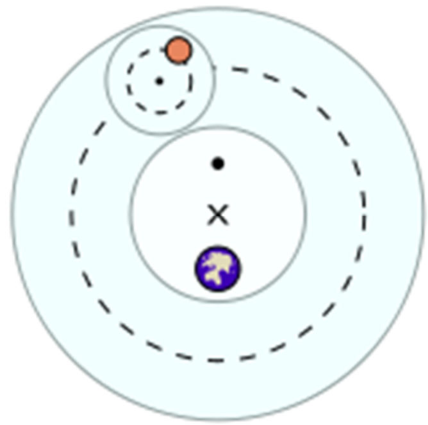
[23] Kepler believed that the celestial bodies were spaced in accordance with various Platonic geometric solids. So, while ellipses may seem benign to the modern reader, it amounted to an upheaval of universal order for Kepler; his own beliefs regarding the nature of the universe were (successfully) challenged by observation and scientific reasoning.

simplicity actually played in reaching this milestone is unclear, since (as shown) it was at times a hindrance and other times an asset.

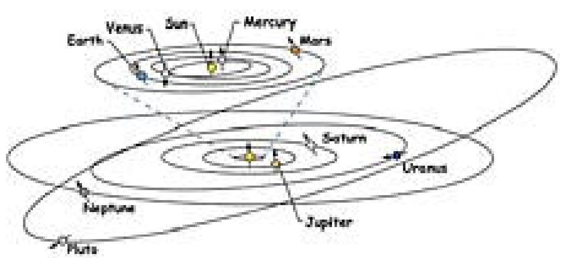
Here we should take a moment to step back and examine our general project. Our current discussion is of qualitative simplicity in regards to ontological consequence. Specifically, we are seeking to understand how Naturalism, i.e., the belief that nature is simple, guides science to conduct business parsimoniously.



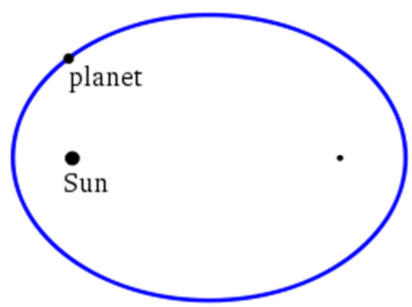
A Ptolemaic model of our solar system.



Ptolemy posited that planets moved in epicycles throughout their orbit; this partially accounted for apparent retrograde motion.



A current celestial model featuring elliptical orbits as proposed by Kepler.



Kepler argued that planets move eliptically around the sun and another focus.

In the examples just discussed, we see that both Galileo and Kepler maintained certain ontologies (though slightly different) that held that nature operates according to ordered relations between bodies. In some instances these ontologies lead toward currently held scientific beliefs (that we regard currently as true) and at other times did not. Both belief systems presupposed an order to nature that was not derived from observational data.[24] Galileo's supposition that nature is simple helped guide him towards a theory of falling objects; Kepler's faith in astronomical harmony helped him recognize the moon's involvement with tides. Newton's similar Naturalistic sentiments lead him to develop a theory of universal gravitation. Their paradigms shaped their interpretation of the evidence such that they saw simplicity; it was not given to them from observation alone.

This touches on a well-known problem in philosophy of science—what Quine recognizes as the problem of underdermination of theory by evidence. We cannot derive a principle of simplicity solely from observed phenomena.

> If all observable events can be accounted for in one comprehensive scientific theory . . . then we may expect that they can all be accounted for equally in another, conflicting system of the world. We may expect this because of how scientists work. For they do not rest with mere inductive generalizations of their observations: mere extrapolation to observable events from similar observed events. Scientists invent hypothesis that talk of things beyond the reach of observation. The hypotheses are related to observation only by a kind of one-way implication; namely, the events we observe are what a belief in the hypotheses would have led us to expect. These observable consequences of the hypotheses do not, conversely, imply the hypotheses. (Quine 1975, 313)

Quine recognized that our theoretical presuppositions about the nature of a thing lead to how we understand a thing—how we conceptualize it and claim to know it—and hence our scientific data is not a set of benign observations. Instead, they are theory-laden

[24] Whether one takes these as *a priori* suppositions or summations made *a posteriori* is of no consequence here. I only mean to highlight that simplicity did not "come from" the evidence, but was rather "read into" it.

postulates.. S(x) is S not because x gives us its simplicity via observation, but because our notion of simplicity informs our conception of x.

While science may purport to bear witness to the simple patterns of nature,[25] it more aptly can be said to interpret data collected from nature *as patterned*. There is nothing in the observation of two falling objects that leads one to believe they are both affected by the same force—the idea of a universal explanation for both is a consideration of generalizability, or economy, or both. Regardless, it (the belief in a universal cause) is not one forced upon us by nature itself; Naturalistic ontology is not a guide to theory, but the result of theory.

> The world with its quarks and chromosomes, its distant lands and spiral nebulae, is like a vast computer in a black box, forever sealed except for its input and output registers. These we directly observe, and in the light of them we speculate on the structure of the machine, the universe. Thus it is that we think up the quarks and chromosomes, the distant lands and the nebulae; they would account for the observable data (Quine 1978, 13-14).

4.2.3 ADEQUACY: *S(X) OFFERS ENOUGH EXPLANATION.*

A third way in which a theory can be evaluated ontologically is in regards to Adequacy: does a given theory account for all available phenomenon? A theory that can account for only part of a set of observations is deemed incomplete, and leaves one wanting for either a secondary theory to conjoin to the one given, or, as science prefers, a more generalizable theory that covers all available evidence. Hence, there is a limit to the how closely we can trim a theory since whittling away too much will result in a weakened model incapable of accounting for all phenomena.

[25] It is not my contention that contemporary science makes this claim; I suspect most working scientists worry little as to the ontological implications of their work. And for scientists who do concern themselves with such things, it is erroneous to assume they ascribe to a Naturalistic account of simplicity. However, this comment is appropriate to many early scientists that emerged from Aristotelian physics and concerned themselves with the theological and metaphysical implications of their work.

Philosophers and scientists alike have expressed this notion:

Kant:

> The variety of entities should not be rashly diminished. (1950, 538)

Karl Menger:

> Entities must not be reduced to the point of inadequacy.
>
> And
>
> It is vain to try to do with fewer what requires more. (Maurer 1984, 466)

Einstein:

> The basic concepts and laws which are not logically further reducible constitute the indispensable and not rationally deducible part of the theory. It can scarcely be denied that the supreme goal of all theory is to make the irreducible basic elements as simple and as few as possible without having to surrender the adequate representation of a single datum of experience. (Einstein 1934, 165)

This concept is sometimes called an *anti-razor*, and Walter of Chatton, a contemporary of Ockham, had a particularly interesting take on the theme. He wrote:

> If three things are not enough to verify an affirmative proposition about things, a fourth must be added, and so on (Maurer 1984, 464).

To put this principle in action, we can examine a debate between Ockham and Chatton regarding causality.[26] Take A and B, where A is said to cause B (or B is taken as the effect of A). Chatton maintained that to show A is truly the cause of B, one must posit C, the causal relationship itself, as a third entity. This is because A and B are not sufficient to justify a causal relationship—their existence alone cannot establish such an interaction (thus correlation but no causation). For example, if I perceive (P) an object (O), I cannot rightly say that O causes P (given the possibility of hallucinations, as an extreme example) without their being a "real relationship" of C between the two. Chatton likewise

[26] See Maurer's *Ockham's Razor and Chatton's Anti-Razor* for further exposition and related notes.

applied this reasoning to explain how one object imparts motion to another (Maurer 1984, 467-68).

Chatton assumes that an unverifiable proposition can be made verifiable by continuing to posit entities. This is *prima facie* problematic, given that one could go on a positing binge, conjuring up unicorns and leprechauns and the like. Ockham understood this, and responded by arguing that one must also consider whether "the assumption of entities is reasonable, in line with experience, or justified by competent authority" (471). Whether these criteria hold water is a point of debate; the gist of the argument is that we must defer to evidence when deciding to posit more entities. In relation to causation, then, even if one were to posit C as the "real relationship" between A and B, we are now left wanting regarding how C interacts with A and B; thus we now posit D (between A and B) and E (between B and C), like this:[27]

A↔B, and we call that causal relationship (↔) C, then;
A↔C↔B, and we call these causal relationships (↔↔) D and E, respectively, then;
A↔D↔C↔E↔B, *ad infinitum* . . .

And *ad absurdum* at that! We simply continue to beg for justification.[28]

What *can* be gleaned from Chatton's queer anti-razor, however, is a realization that determining when theory adequately accounts for data is difficult. When do we decide to accept an explanation (or theory, or hypothesis) as adequate and justified? I content that this can be answered pragmatically; we effectively deem a theory adequate when it is adequately effective. That is, once we have established a working explanation that yields profitable results, we accept it. I will return to this notion later, as I think it's key in understanding what value simplicity *does* have.

[27] Do not take ↔ as a biconditional but only as a relationship.
[28] A well-known problem of justification of belief in epistemology.

Evaluating S(x) in terms of Adequacy is also problematic in that often multiple theories are able to explain phenomena equally as well.[29] This is a reiteration of the earlier discussion regarding underdermination of theory by evidence. But, laying aside Quine's concern, let us consider an example from biogeography. In 1761, Count de Buffon[30] proposed that:

Areas separated by natural barriers have distinct species.

This is now referred to as Buffon's Law (BL). One explanation of BL, termed the Darwin-Wallace hypothesis, said that species disperse into new areas via migration and then diversify as a result of natural selection. Since this diversification occurs differently depending on the area of dispersal, distinct species develop. However, BL had noted exceptions, such as the same species being present on two unconnected continents. D-W proponents accounted for this by reference to temporary conditions—ocean currents, winds, ice barges, etc.—that had since dissipated (Baker 2000).

In 1950, Léon Croizat proposed an alternative theory that explained BL through tectonic shift (continental drift). Proponents of this account likewise had to refer to once-extant formations, like land bridges, to account for anomalies.

Croizat's theory won out, but certainly not on parsimonious merit, since his explanation demanded the positing of extra entities like tectonic plates. Darwin even raised this criticism, suggesting that it required one to make unreasonable deductions (Baker 2000). Both theories were adequate in accounting for observations, and there were no obvious empirical means by which to test the competing theories, given that they

[29] As evaluated in relation strictly to adequacy; in reality, theories often vary in other salient respects, as will be seen with this example.
[30] Georges-Louis Leclerc, Comte de Buffon.

would need millions of years of evolution for verification.[31] As such, this serves as an instance when simplicity did not sink the putt. Instead, it was the less parsimonious theory that was eventually adopted.

Croizat's positing of tectonic plates was not parsimonious but still helped bolster the adequacy of his theory. In weighing how much fat to trim, so to speak, we must not undercut our theories as to render them incapable of explaining phenomena. But how do we determine what is just enough and what is too much? We now turn to considering the latter.

4.2.4 Non-Superfluousness: *S(x) doesn't offer extra*.

Concerns of adequacy center on worries that the postulates of a theory ignore or fail to account for phenomena; conversely, concerns of superfluousness arise from worries that phenomena fail to account for postulates. That is, S(x) contains only the necessary bits and nothing more. In regards to theory, this means trimming off extra postulates that are not needed to establish a relationship to evidence. This relationship, I believe, is one of explanatory power. A postulate is warranted if and only if it adds explanatory power to its theory in a relevant way.

E. C. Barnes discusses the idea of explanatory excess in his paper *Ockham's Razor and the Anti-Superfluity Principle* (2000). In it, he divides simplicity into two distinct but related concepts: the Anti-Quantity Principle (AQP) and the Anti-Superfluity Principle (ASP). The AQP is akin to my earlier exposition of conservative enumeration; it is a principle of elegance. In contrast, the ASP states that theories ought to postulate only insofar as each postulate helps to explain relevant phenomena. We shall focus on the qualitative notion of ASP and how it avoids superfluousness.

[31] Barring computer simulation, which could still be arguably non-empirical in the relevant ways.

Barnes further divides ASP into two regulative types: anti-idleness and anti-overdetermination. A postulate is said to be idle when “the state of affairs the [postulate] asserts literally does nothing in the way of participating in the causal structure that produces the world’s observable phenomena” (Barnes 2000, 359). An idle postulate lacks explanatory teeth in that it fails to correlate to causal evidence; these idle bits are dumped as science progresses and are replaced by more relevant causal accounts. In contrast, a superfluous postulate of the overdetermining sort is not replaced per new evidence and instead remains compatible—although explanatorily unneeded—with emerging science.

Barnes provides two historical examples of the idle-ASP. Firstly, in the 1900s it was widely believed that there was an invisible substance, luminiferous aether, through which waves propagated; its existence was posited to explain how light and electromagnetic waves travel through space. Einstein challenged this notion, however, and showed that waves can travel through vacuums, and thus a medium is not necessary. As a result, aether was no longer needed to explain wave propagation and was tossed. Similarly, the mysterious substance phlogiston, said to be a fire-element released during combustion, was dismissed once Lavoisier successfully argued that oxygen could account for the same.

To illustrate overdetermining-ASP, Barnes cites Boyle’s work with vacuums. Prior to Boyle, Aristotelian teachings held that nature had an aversion to vacuums. Boyle offered an account of vacuums in physical terms, but his explanation did not directly contradict Aristotelian teachings. That is, one could believe both in Boyle’s mechanistic explanation of vacuums while simultaneously believing that nature had a grudge against them, so to speak. In this way, since there was no direct contradiction, the Aristotelian belief was said to be overdetermining (Barnes 2000, 358-60).

When a postulate, T, comes under attack as superfluous under the ASP criterion due to new evidence, N, Barnes says its proponents have three options:

1. Reject N.
2. Accept N and concede that T is explanatorily idle but not refuted.
3. Accept N but affirm T as an overdetermining cause.

For example, consider the diversification of species. Evolutionary theory challenged long-held explanations of the diversity of species as resultant of independent origin and divine creation. Consequently, theists[32] have chosen to:

1. Reject evolution, or
2. Accept evolution but argue that it does not refute the claim of God's existence,[33] or
3. Accept evolution but argue that God is an overdetermining cause of diversification, that is, argue that though God did not act, he was ready and capable of acting in the case that evolution went astray.

We must note that hybrid positions like "God diversified species *through* evolution" collapse into option 1 since what is entailed by "evolution" here is "change of species over time without reference to a deity." If a theist *does* accept evolutionary accounts as satisfactory in explaining the diversity of species, then at question is why (s)he would maintain the existence of God referent to its explanation; God is superfluous. (S)he might want to hang on to God for other reasons—to help give her life meaning, or the like—but that says nothing of the superfluousness of God in the relevant way.

Given the three options Barnes proposes above, we must ask: What justifies taking one option over another?[34] Option 1 does seem *prima facie* a poor response;

[32] That hold God as creator.
[33] Since God had been posited as a cause of the phenomena in question; the explanatory power of God regarding diversification is predicated upon his existence.

presumably, we do not want science determining what data to ignore on the basis of its practitioners' particular theistic beliefs. But 2 and 3 are less easily rejected out-of-hand. What motivation does the theist have to reject the existence of God since it does not directly contradict evolutionary theory? We have already examined several possible justifications of elegance and parsimony (appeal to background assumptions, etc.), all of which Barnes deems *extra-empirical*. However, Barnes argues that ASP is empirically justified; it is uniquely intra-empirical (362). If this proves to be the case, it marks a great breakthrough in discussions of parsimony since it sidesteps several of the objections already discussed throughout this paper.

Consider:

E: a set of statements describing observations or data that need explaining[35]

T: a theory (conjunction of components) that entails E and explains E if true

ts: a hypothesis not included in T that describes a state of affairs not causally linked to E *or* is a state of affairs that is overdetermining in accounting for E given that T

So,

$$P(ts|T\&E)=P(ts|T)$$

The probability of *ts* (being true) is the same given the truth of T *and* E, or just T. This holds since we know T to entail E—hence, its conjunction adds nothing beyond T only. Since the probability of *ts* remains constant in both cases, we see that there is no evidential support[36] for *ts*; E offers nothing to justify belief in *ts* beyond what T already offers.

[34] Implicit in Barne's argument is the idea that the justification of ASP also motivates adoption of ASP; I find this erroneous. And, given the plethora of theists admitting to evolution, so do many theists.
[35] Barnes is unclear here. Are we to take E as a set of Quinean observation sentences, or take it more loosely as "observations," or take it quantitatively as data devoid of qualitative information? This ambiguity plays into my critique that follows.
[36] Insofar as we are counting evidence as statements describing observations.

Barnes says that between two competing theorists, one espousing T and the other T&*ts*, we can argue that *ts* has no evidential support and that T is sufficient on its own in explaining the phenomena in question. He notes that this does not necessarily disprove *ts*, since other evidence not included in E (or entailed by T) might independently confirm it.

But this is not remarkable; it is widely accepted that A is always more probable than A&B. But Barnes says this is an *a priori* justification that misses the thrust of ASP, namely, that superfluous components are deemed as such because they lack evidential support. To explain, consider two theories T1 and T2 that are empirically equivalent such that

$$P(E|T1)=P(E|T2)$$

where

$$T1=\{t_1 \ldots t_n\} \text{ and } T2=\{t_1 \ldots t_n \;\&\; ts\}$$

such that it can be said that T2 entails T1. In this case, we know *a priori* that

$$P(T1) > P(T1\&ts)$$

But this does not invoke ASP, which says that the *reason* for dismissal is that E fais to support *ts.* Barnes warns that solely *a priori* justification for dismissing *ts* commits the fallacy of division, that is, believing that if a whole possesses certain properties, then the members of that whole likewise possess those properties. When given two theories that are empirically equivalent, like T1 and T2, we cannot assume that their constituent postulates are all empirically equivalent. As such, dismissing *ts* on *a priori* grounds motivated by probability theory is erroneous. Barnes refers to this as naïve conformational holism (366).

To review, we have examined how S(x) can be said to be non-superfluous. The ASP deems a postulate superfluous when it lacks certain causal connections to available evidence. Therefore, ASP reaffirms that simplicity must be understood qualitatively—in relation to kinds of evidence—and not as an *a priori* game of probabilities. When we do encounter two empirically equivalent theories,[37] we must attend not to the *number* of their postulates, but how those postulates *explanatorily relate to evidence*.

Simply saying that a postulate "must explain evidence" or that "evidence must corroborate hypotheses" is not particularly helpful; this is already widely accepted in science, lest all experimentation be for naught! To flesh out this vague ideological goal of advancing empirically (evidentially) sound methods that razor away excess, we need an actionable methodology. This is the realm of pragmatics.

5. Pragmatic Assessment

To address pragmatics outside of the quantitative and qualitative headers may be misleading, but it is offered here as a separate assessment to highlight its emphasis on the efficacious advancement of scientific practice rather than on ideological justification. After all, there is a division (even if only perfunctory) between science and philosophy. We now turn to see how simplicity can help marry the two for the greater good of both.

5.1 Probability: *S(x) is the most probable of given options.*

A well-established and highly popular method of assessing the probability of theory in light of evidence is Bayesian analysis. One selling point of Bayesian analysis is that it updates the probability of a hypothesis as new evidence is introduced; this of course occurs frequently in science. Bayes' theorem may be written as:

[37] Though literature on extra-empirical criteria make use of this hypothetical often, I wonder if it ever obtains. I have yet to see evidence in the history of science, but then again, I am not a historian or a scientist.

$$P(H|E) = \frac{P(E|H)}{P(E)} \cdot P(H)$$

Where

- H is a hypothesis under consideration
- E is the available evidence

And

- P(H|E) is the *posterior probability*, the probability of H given that E. This is what we're after, the probability of hypothesis H given that we have evidence E.
- P(E|H) is the *likelihood*, the probability that we will observe evidence E given that our hypothesis H is true.
- P(E) is the *evidence*, the probability of the evidence being extant regardless of whether the hypothesis is true or not; it's the sum of both scenarios. It can be represented as: P(E|H)P(H)+P(E|~H)P(~H)
- P(H) is the *prior probability*, the probability of H being true before we are presented with any evidence.

So

$$(posterior) = \frac{(likelihood)}{(evidence)} \cdot (prior)$$

such that the posterior probability is a combination of considering how evidence matches with our hypothesis (the likelihood) and how probable the hypothesis is more generally speaking (the prior).

We should pursue theories that are more probable; it is efficient and effective to do so. In choosing between two theories, Bayesian analysis can help guide our choice according to which competing theory is more probable because of its likelihood and prior probability.

At question for our project is how simplicity impinges on a theory's probability. In chapter seven of Elliot Sober's *From a Biological Point of View* (1994), he notes that simplicity is germane to Bayesian theory in two ways: by influencing the prior probabilities and the likelihoods (141). To demonstrate this, he invokes two examples from science: debate over group adaptation and phylogenetic modeling.

5.1.1 Group Adaptation v Individual Selection

In biology, a *group adaptation* (GA) refers to a characteristic that exists because it is beneficial to a group. Such an adaptation is also said to be *altruistic* if it benefits an organism's community but not the organism itself—generally, altruism is invoked to explain behaviors that are detrimental to the individual.[38] GA accounts stand in contrast to individual selection (IS) accounts that explain characteristics in terms of lower-order[39] selective processes. Evolutionary biologist G. C. Williams is a famous proponent of IS in part because it is a simpler (more parsimonious) account than GA.

As an example, consider how one might explain the behavior of musk oxen. When under threat from wolves, musk oxen will "wagon-train" by having the males form a protective ring around the females and young, who retreat to the inside of the ring. This behavior is ostensibly an example of GA, given that wagon-training is beneficial for the herd at large but detrimental to the males who confront the wolves. GA reasons that groups that adopted this characteristic were more likely to survive, hence the persistence of wagon-training behavior.

Williams rejects this by arguing that prey are programmed, so to speak, to either fight or flee a predator, contingent on the size of the predator. As such, wolves are of the right size to cause the stronger male oxen to stand and fight but also cause the weaker females and youth to run for cover. Williams argues that this explanation is more plausible because it is more parsimonious, but he fails to develop an argument explaining

[38] A GA *may* be altruistic, but not necessarily so. Additionally, altruistic characteristics may be of various cost to the individual, from no consequence to dire consequence.
[39] E.g., for example, at the genetic level.

as much. Sober generously takes up Williams' argument and defends it via the reasoning that follows (Sober 1994).

Recall that GA relies on altruistic characteristics winning out over more selfish ones. However, selfish characteristics always benefit individuals more than altruistic ones. In an isolated population, selfish organisms will overtake altruistic ones such that eventually the entire group will be composed of selfish members. For altruistic characteristics to evolve—the crux of the GA account—several factors must obtain. Sober argues:

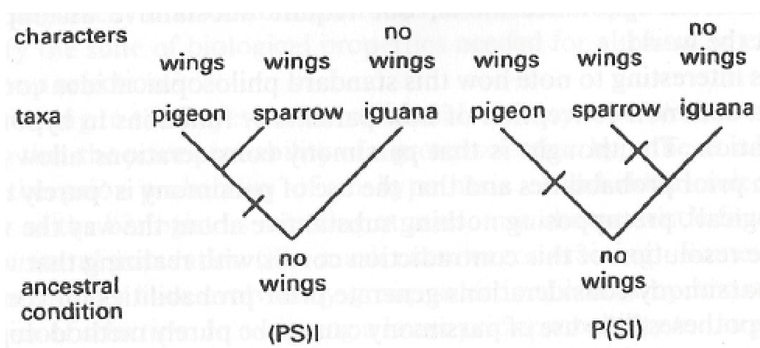
> For altruism to evolve and be maintained by group selection, there must be variation among groups. An ensemble of populations must be postulated, each with its own local frequency of altruism. Groups in which altruism is common must do better than groups in which altruism is rare. . . . [But] if each group holds together for a sufficient length of time, selfishness will replace altruism within it. . . . If the groups hold together for too long, altruism will disappear before the groups have a chance to reproduce. This means that altruism cannot evolve if group reproduction happens much more slowly than individual reproduction. (Sober 1994, 145)

These parameters—conditions that must obtain for altruism to evolve, and hence for GA to occur—comprise a body of assumptions more burdensome (i.e., less parsimonious) than the assumptions required for an IS account.

Williams' argument is tantamount to arguing that the prior probability, P(H), of IS is higher than that of GA because "natural systems rarely exemplify the suite of biological properties needed for altruism to evolve by group selection" (146). What is important to note is that this consideration of parsimony is prior but not *a priori*. That is, it is prior in the Bayesian sense because it fixes the probability of a given hypothesis prior to introducing evidence of a particular sort. (Here, it affects the probability of an IS account being more likely than a GA account prior to knowing whether we are talking about musk oxen or a bacterial colony, for example.) But this probability is not *a priori* because it rests on certain assumptions about the world—like those given above—as well as methodological assumptions like Williams' that lower-order explanations are more simple than higher-order ones.[40]

5.1.2 PHYLOGENETIC MODELING

The second way in which Sober says simplicity can affect Bayesian analysis is through exerting influence on the likelihoods. To demonstrate, he considers the modeling of phylogenetic trees. The following diagrams taken directly from Sober's *From A Biological Point of View* (1994, 148-9):



Since pigeons and sparrows have wings, while iguanas do not, we assume that the former are more closely related to one another (in evolutionary terms) than the latter. The phylogenetic trees above entail two assumptions. Firstly, that all three organisms come from a common ancestor, and secondly, that this common ancestor was devoid of wings. This means that having wings is a derived characteristic.

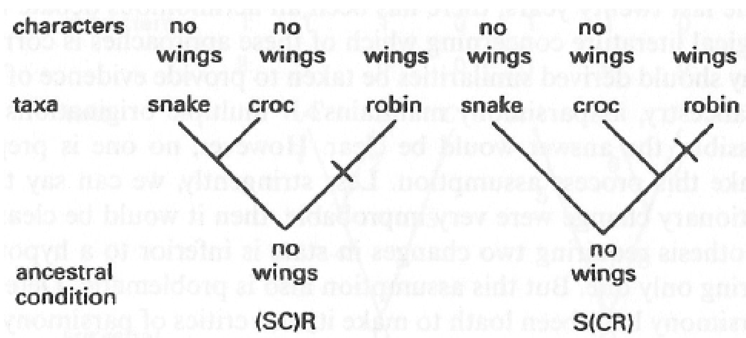
Of debate is at what point during evolution wings were derived; this is denoted on the tree by a slash mark. The left tree, (PS)I, posits that the derivation was present in a common ancestor of pigeons and sparrows. The right tree, P(SI), posits that the derivation

---

40 Per Morgan's Canon: In no case is an animal activity to be interpreted in terms of higher psychological processes if it can be fairly interpreted in terms of processes which stand lower in the scale of psychological evolution and development (Morgan 1903, 59).

occurred independently from one another. The left tree describes having wings as a *homologous* relationship—that is, that pigeons and sparrows have wings because their most recent common ancestor did. In contrast, the right tree shows a *homoplastic* relationship—that is, that pigeons and sparrows have wings derived from independent origins. The left tree, the one that posits only one evolutionary point of derivation, is *prima facie* more simple.

Now consider this diagram:



We are still talking about wings, but this time we are examining snakes, crocodiles, and robins. We make the same assumption as before: that they share a common ancestor devoid of wings. What differs now is that we are attempting to explain the *absence* of wings in snakes and crocodiles. Since the common ancestor now shares the characteristic of the organisms under consideration, we say that the characteristic is *ancestral* (in contrast to the prior example in which the characteristic is *derived*). In both (SC)R and S(CR), we are able to explain robin's wings in terms of one derivation, i.e., one slash; neither diagram can be said to be *prima facie* more simple than the other. As such, simplicity does not help direct us in our choice between (SC)R and S(CR).

This is because:
Derived similarity is evidence of [closeness] of descent.

Ancestral similarity is *not* evidence of [closeness] of descent (149).

But why? Counting only derived characteristics (and not ancestral ones) would be justified if, for example, multiple originations were impossible. That is, if wings could *not* have originated independently (homoplastically), then similar characteristics would *have to be* taken as evidence of a common ancestor. Alternatively, if evolutionary changes were highly improbable, we would be justified in trying to minimize positing such changes in our phylogenetic models. But neither of these restrictions are straightforward; both assumptions are up for debate and are problematic.

Here's what we glean from this: in an attempt to use simplicity as a methodological guide in helping to fix likelihood, (the probability of observing phylogenetic evidence given that our hypothesis—a particular taxonomic tree—is correct), we find that our understanding of which models are more simple rely on background assumptions. "The method does not have an *a priori* and subject matter neutral justification" (Sober 1994, 152). We must utilize auxiliary assumptions if we are to evaluate how a hypothesis stands in relation to given phenomena.

Thus, for the Bayesian, fixing the priors or setting the likelihoods both require we adopt a particular body of background knowledge. Simplicity can help in choosing our paradigms, but only in a highly relativized way.

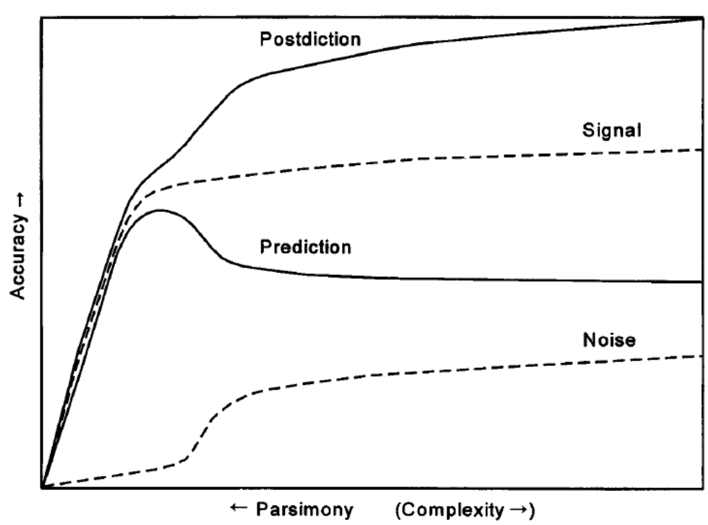
5.2 ACCURACY: S(X) IS THE MOST ACCURATE OF GIVEN OPTIONS.

Another concern of practicing scientists is maximizing the accuracy of explanatory and predictive models. Data trends are often interpreted through the employment of curvilinear modeling. Fundamentally, this practice attempts to depict interactions between variables, e.g. to establish relationships of dependency or correlation. A measure of an accurate statistical model (such as an equation that generates a line of regression) is one that is said to correctly map on to relevant data while ignoring irrelevant data. Relevant data is termed *signal* and irrelevant data, *noise*. For example, in an experiment that attempts to find correlations between an instructor's nationality and her students' test scores, the *signal* would represent student response (the dependent variable) in relation to the nationality of the teacher (the independent variable). In contrast, *noise* would consist of data points that are results of uncontrolled variation, such as student intelligence or socioeconomic class.

There is some hope that justification for simplicity as a methodological guide might be found in demonstrating how simplicity produces more accurate statistical models. In particular, simplicity has been said to help prevent over-fitting in curvilinear modeling—that is, fitting to every data point, including noise. This results in poor goodness-of-fit since it renders no predictive trend.

I would like to now return to the work of Cornell's Hugh Gauch to examine his discussion of simplicity and modeling efficacy in *Scientific Method and Practice* (2003), as I believe he offers a thorough treatment of the issue.

Firstly, Gauch offers us the following diagram for consideration (278):



This shows the predictive and postdictive accuracies of various models in relation to parsimony, with parsimony presented as a continuum from simple to complex, left to right, respectively. This continuum can be understood as moving from linear to quadratic to cubic equations, for example. As models grow in complexity:

- *Signal* is quickly recovered at first but then is slowly recovered.
- *Noise* is slowly recovered at first, then quickly, then slowly.
- *Predictive* accuracy, that is, the model's ability to account for data of the entire population from which a sample was taken, grows quickly and then declines (as noise is recovered more rapidly).
- *Postdictive* accuracy, that is, the model's ability to account for data of the sample itself continually increases.

Scientists give primacy to predictive accuracy in selecting a model, since we want models that account for trends of an entire population, not just a sample—after all, a sample is supposed to be indicative of the general population from which it is sampled. From the above diagram, we see that at some point, the predictive power of a model reaches an apex and then declines, and this decline is inversely related to an increase in complexity. As such, we may conclude that simplicity is an important consideration when attempting to maximize the predictive power of a model.

Gauch discusses several examples[41] of how pursuing parsimony in modeling increases the efficiency of models by increasing their accuracy. A model's statistical efficiency is a measure of how accurately it accounts for the given data (i.e., fits the data), and also represents the model's economy[42]. That is, a model with an efficiency of 1 is said to yield accuracy equal to its data, but a model with an efficiency of 3 means that the model yields a level of accuracy that would be achieved with three times the data. An

[41] Mendel's experiments with peas, cubic vs. quadratic modeling, measuring equivalent conductivity, and measuring crop yields.
[42] Its predictive power in relation to the amount of sampled data.

efficient model, then, does more with less, since it increases accuracy as though more data were collected without actually collecting the data (288).

Gauch offers three explanations of why parsimonious models gain accuracy. For these gains to obtain, we must make five assumptions about the data (312):

1. The data must be noisy, which allows for improvement.
2. The data must contain some signal that is not desperately small.
3. The objective must be prediction and not postdiction.[43]
4. The data must be related.
5. The structure of the signal must be relatively simple compared with the noise.[44]

And one assumption about the model:

6. The model must be suitable, capable of recovering much signal in few parameters compared with the large number required for noise.

Given these restraints, he offers his three explanations of how simplicity leads to a gain in accuracy (312-16):

1. *Signal-Noise Selectivity*: Simpler models (towards the left of the simple-complex continuum) tend to capture more signal and less noise compared to other candidate models.
2. *Variance-Bias Trade-off*: Simpler models strike a balance between variance (variability of a predicted model point) and bias (difference between predicted value and actual value).
3. *Direct-Indirect Information*: Simpler models exploit more data, both direct and indirect, and thus can be "more accurate" than their data.

These explanations do indeed paint a rosy picture for simplicity insofar as simplicity leads us to create models that are more efficient and have higher predictive power. But we should note here that thus far 1) the discussion of simplicity's role in statistical modeling has been a purely pragmatic one, 2) like the discussion of Bayesian analysis earlier, accuracy can only be assessed upon adopting a set of background assumptions,

[43] Since postdictive accuracy always increases as models become more complex.

[44] ""The signal must be relatively simple compared with the noise, ordinarily because the signal has few major causes but the noise has numerous little causes."

and 3) it has been assumed that some sorts of equations (like quadratic equations) are *prima facie* simpler than others (like cubic ones).

What is know as the parsimony-accuracy trade off problem has long been regarded as the Achilles' heel of statistical justifications for simplicity. At question is how to establish the proper balance between simplicity (underfitting) and accuracy (overfitting) when constructing a model. Gauch does address this problem by arguing that this dilemma hinges upon whether we are looking for postdictive or predictive success in our models. In drawing a postdictive model, parsimony and accuracy are inversely related until the bitter end—postdictive success obtains when the model is fit to every data point available, at least in theory. However, Gauch says that we are usually after predictive power such that our model applies to a broad population. He notes:

> [Per the earlier diagram,] predictive accuracy increases until the peak of Ockham's hill[45] is reached, after which neither accuracy nor parsimony is promoted. Hence, the tension between parsimony and accuracy does not last to the end, but rather lasts only until this peak is reached. Consequently, the ordinary solution to [the dilemma] is simply to pick the model at the peak of Ockham's hill. This solution is admirably objective, wonderfully easy, and clearly meaningful in that it optimizes a specific and important model property, predictive accuracy. . . . So the primary resource for resolving the parsimony-accuracy trade-off is to distinguish prediction from postdiction. (319)

Firstly, this "solution" says nothing more than "if one wants to maximize predictive efficiency, then use *x*." It's a practical solution but fails to address the broader methodological problem of justifying simplicity.

Secondly, and more importantly, Gauch speaks of prediction as though it is a transparent concept—a method that hits upon the reality of the data without bias towards a given end. Yes, we want our theories to have wide applicability to members beyond just our sample, hence why we worry so about choosing our samples. However, predictive

[45] This is the "hump" seen in the earlier diagram at which point predictive accuracy declines.

models do not foretell future data—that is, they are not crystal balls that reveal actual phenomena-to-come. Instead, they are predictive only of narrowly-defined trends; they predict with their ends in mind, laden with bias.[46] One must establish signal and noise before there can *be* signal and noise. You must know the needle before looking through the haystack.

A complete exegesis of this problem is obviously beyond the scope of this paper, but we can briefly expound on this issue with the help of J. W. McAllister (2007). McAllister argues that generally, data manifest a multiplicity of patterns, and which pattern is emphasized is a function of what we wish to do with the data. That is, we bring significance *to* the data rather than receiving it *from* the data.

McAllister cites three examples of data sets exhibiting multiple patterns (887-90):

1. *Atmospheric temperature measurements:* The data of all temperature measurements ever recorded manifest three pattern types: highly regular cyclical, less regular cyclical, and noncyclical. *Highly regular cyclical* include those due to Earth's rotation (daily) and Earth's orbit around the sun (yearly); and those reflecting sunspot cycles and earthly precession (epochal). *Less regular cyclical* includes those due to weather systems (e.g. El Niño) and geothermic events. *Noncyclical* includes those due to major geographical changes, rate of Earthly rotation, and anthropogenic changes.
2. *Microwave intensity readings of the sky:* Including patterns due to astronomical radio sources, decoupling of matter in the early universe, motion of the earth, radio emissions of the Milky Way, inhomogeneity in matter distribution, and the (presumably) flat universe.
3. *Cortisol levels in the blood:* Including patterns due to increase in mean cortisol levels over the human lifespan and much smaller (in duration) ones due to 24-hour wake and sleep cycles.

All of these patterns are "equally real" insofar as the data confirms all of them. The choice as to which pattern to highlight, and thus which model to select, is a purely pragmatic one. "Depending on the focus of his or her research, a scientist is fully justified

[46] Along the lines of the Duhem-Quine thesis that states hypothesis are not testable in isolation. My concern, however, is more aptly expressed through McAllister's observations that follow.

in choosing any of these models as the one closest to truth" (888). Regarding (1), meteorologists will pick out smaller cycles than geologists. As to (2), cosmologists will focus on whichever pattern confirms their particular area of interest. And with (3), a geriatrician will be much more interested in modeling her patient's lifetime cortisol increase and ignore patterns of daily cycles.

These patterns are coexistent and non-exclusive. Each of these patterns manifest themselves at different noise levels. A pattern seen over a short time span (like daily temperature fluctuations) comes with less noise than one seen over tens-of-thousands of years.

> Models of data that pick out patterns exhibited with different noise levels should thus not be placed in a single ranking with the aim of determining which one is best. . . . For any quantitative technique for choosing among models of a data set to be adequate to this scenario, it must incorporate conceptual resources to take account of the noise levels with which different patterns are exhibited in a data set. It is clear that a unique model can be identified as "the best" only if a certain desired noise level is specified . . . (893)

McAllister concludes from his argument that many popular model selection techniques (like the Akaike technique[47]) are inadequate given that they fail to contain a provision for specifying desired noise level.

In relation to our discussions of parsimony, then, this means that justifying model selection by its predictive power only raises the subsequent question, "In predicting

[47] McAllister explains: This technique makes use of the concept of a family of curves. Curves belong to the same family if their equations contain the same freely adjustable parameters: examples of families of curves are straight lines and parabolic curves. We first estimate the "expected predictive accuracy" of each family of curves. This is equal to the degree of fit of the curve in each family that best fits the data set, minus the number of adjustable parameters of curves in that family. The best model of the data set is the model corresponding to the best-fitting curve that is a member of the family with the highest expected predictive accuracy (Forster and Sober 1994). For example, the Akaike technique suggests that the Copernican model is a superior model of data on planetary motions than the Ptolemaic model: although the two models fit the data approximately equally well, the Copernican model has a smaller number of adjustable parameters, and thus scores more highly on the Akaike criterion (886).

what?" Those who try to side-step the parsimony-accuracy dilemma by passing it off as a simple matter of minimizing adjustable parameters and maximizing predictive outcome fail to acknowledge that experimenter model selection heavily influences what pattern will be said to be "true of the data."

We are once again directed to address our background assumptions when speaking of simplicity, just as we were when trying to fix the Bayesian priors and likelihoods. This is one more nudge towards simplicity as a relativized notion—that is, as a criterion that is locally applicable to a particular project or discipline—and a step away from simplicity as an *a priori*, purely logical, strictly methodological, or globally justified principle.

6. Conclusions

A science that proceeds simplistically is one that does so efficiently and with great pragmatic payoff, wielding great predictive power and placing bets on the most probable of outcomes. Simplicity increases the ease with which we calculate, the efficiency with which we sample, and the accuracy with which we predict. But these things are good only insofar as we desire them; science pursues what we want it to pursue, and does so by employing the means we deem suitable.

Attempting to gain a global, objective justification for simplicity is fruitless. Given the increase in quality of life that science has afforded, is an extra-pragmatic justification needed? ? There is no need to reify simplicity and elevate it to a global position when it profits nothing.

> [T]he implausibility of postulating a global criterion has two sources. First, there are the 'data'; a close attention to the details of how scientific inference proceeds in well-defined contexts of inquiry suggests that parsimony and plausibility are connected only because some local background assumptions are in play. Second, there is a more general framework according to which the evidential connection

> between observation and hypothesis cannot be mediated by logic and mathematics alone. (Sober 1994, 154)

Quine said that "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (1953, 41). Such is the case with what we capriciously call extra-empirical criteria. When we look to justify our methods without recourse to their effects, we find *only* effects; the entire body of scientific knowledge is saturated through and through with the very methodology we wish to critique. Yet, this is no reason to despair. Simplicity remains meaningful even if its meaning is highly relativized and, at times, simply enigmatic.

> Aristotle accused Plato of hypostatizing The Good. . . . Following Aristotle, we should hesitate to conclude that . . . there must be some single property of parsimonious hypotheses in virtue of which they are good. (Sober 1994, 153)

References

Aquinas, Thomas. 1945. *Basic writings of St. Thomas Aquinas*, trans. A.C. Pegis. New York: Random House.

Baker, Alan. 2012. Simplicity. *The Stanford encyclopedia of philosophy.*

Barnes, E. C. 2000. Ockham's razor and the anti-superfluity principle. *Erkenntnis* 53: 353-74.

Barker, Peter and Bernard Goldstein. 2001. Theological foundations of Kepler's astronomy. *Osiris* 16: 88-113.

Drake, 1999. Stillman. *Essays on Galileo and the history of philosophy of science.* University of Toronto Press.

Forster, M. and Elliot Sober. 1994. How to tell when simpler, more unified, or less ad hoc theories will provide more accurate predictions. *British journal for the philosophy of science* 45: 1-35.

Galileo, Galilei. *Dialogue concerning the two chief world systems,* trans. Stillman Drake. Berkeley.

Gauch, Hugh. 2003. *Scientific method in practice.* Cambridge University Press.

Kant, Immanuel. 1950. *The critique of pure reason*, trans. Kemp Smith. London.

Maurer, Armand. 1984. Ockham's razor and chatton's anti-razor. *Mediaeval studies,* 46: 463-75.

Nash, Leonard. 1963. *The nature of the natural science.* Little, Brown, and Company.

Newton, I. 1964. *The mathematical principles of natural philosophy*. New York: Citadel Press.

Quine, Willard V. O. 1981. *Theories and things.* Harvard.

Quine, Willard V. O. 1978. *The web of belief.* McGraw-Hill.

Quine, Willard V. O. 1975. On empirically equivalent systems of the world. *Erkenntnis,* 9: 313-28.

Quine, Willard V. O. 1960. *Word and object*. Massachusetts Institute of Technology.

Sober, Elliot. 2002. What is the problem of simplicity? In *Simplicity, inference, and eononometric modeling,* ed. H. Keuzenkamp, M. McAleer, and A. Zellner.

Sober, Elliot. 1996. Parsimony and predictive equivalence. *Erkenntnis* 44: 167-97.

Sober, Elliot. 1994. *From a biological point of view.* Cambridge University Press.

APPENDIX

In a small pilot survey of teaching faculty from a regional university, a survey was distributed to gain insight into practicing scientists and their views regarding parsimony. The data was collected anonymously through the use of electronic survey software.

The survey was non-scientific. As such, no statistically significant conclusions can be drawn from the data collected. However, I include it here only as an impetus to further study.

Question 1 was short answer, the remaining questions required the respondent to rank their response from 1 (weak) to 7 (strong).

The next page shows the survey as it was distributed, followed by the raw data collected.

SURVEY: SIMPLICITY IN PRACTICE

This survey should be completed by persons holding PhD degrees in their field.

---

1. What best describes your area of expertise?

- ○ Anthropology
- ○ Astronomy
- ○ Biology
- ○ Biostatistics
- ○ Chemistry
- ○ Economics
- ○ Epidemiology
- ○ Geoscience
- ○ Mathematics
- ○ Physics
- ○ Psychology
- ○ Sociology
- ○ Statistics
- ○ Other, please specify... ☐

The grand aim of all science is to cover the greatest possible number of empirical facts by logical deductions from the smallest possible number of hypotheses or axioms.

2. How strongly do you agree with the above statement?

3. In your opinion, are simpler theories generally more plausible (i.e., more likely to be true)?

4. In your opinion, how important is the simplicity of a theory when determining its viability?

5. In your own research, to what degree does a preference for simple theories influence your hypotheses and explanations?

6. When evaluating the work of colleagues and others, to what degree does a preference for simple theories influence your opinion (in reviewing it favorably)?

7. When considering a particular theory, to what degree does its simplicity (its requiring few calculations, equations, or postulates) influence your choice to adopt or accept it?

For most of history, geocentric astronomical models were favored. However, some time after publication of the work of Copernicus, the heliocentric model became preferred. Though alternative celestial models have been offered, heliocentrism has gained mainstream acceptance.

8. In your opinion, to what degree did the simplicity of the new (heliocentric) model advance its acceptance?

- OR -

☐ I chose not to answer this question

Charles Darwin has been profoundly influential on evolutionary biology. One central tenet of his work is that distinct species are descended from a common ancestor. This idea is now widely accepted. However, prior to the Darwinian revolution, few people supported the theory of common descent.

9. In your opinion, to what degree did the simplicity of Darwin's explanation advance its acceptance?

- OR -

- ☐ I chose not to answer this question

19th century theories of light were committed to the existence of an invisible and undetectable substance, "aether," through which light travels. This was challenged in the 20th century by the theory of special relativity. Both proponents of aether theory (ET) and special relativity (SR) could account for experimental results at the time, so there was no empirical evidence that preferenced one theory over the other. A main difference between the theories was that ET affirmed the presence of an invisible, undetectable aether through which light travels, while SR did not.

10. In your opinion, does positing an undetectable (or yet-to-be detected) entity like aether violate the principle of preferring simpler theories?

- OR -

- ☐ I chose not to answer this question

DATA COLLECTED

1	2	3	4	5	6	7	8	9	10
Astronomy	3	5	5	5	5	5	2	4	2
Astronomy	4	5	4	5	5	3	4	5	3
Astronomy	5	5	3	3	3	3	2	2	5
Biology	7	2	5	6	7	7	7	7	6
Biology	4	2	2	5	2	2		2	5
Biology	2	6	1	5	1	1	2	5	3
Biology	4	3	3	3	3	2		2	4
Biology	2	5	6	5	3	2	2	3	6
Chemistry	2	3	3	2	3	2		1	5
Chemistry	1	2	1	1	1	1	1	1	7
Economics	2	3	5	4	4	5		5	3
Economics	1	7	7	7	7	7	7	7	1
Geoscience	4	6	5	4	3	4		6	
Geoscience	5	4	5	5	3	3	4	4	3
Geoscience	1	6	6	5	6	6	6	4	5
Geoscience	7	3	3	3	1	1	4	4	2
Mathematics	6	4	5	3	6	5	7	6	6
Mathematics	5	5	6	7	7	5	5	5	
Mathematics	6	5	5	4	3	4	5	5	5
Mathematics	5	7	7	2	4	4	6	6	4
Psychology	2	2	2	2	2	3	3	2	6
Psychology	6	4	5	5	5	2	3	5	1
Psychology	7	4	7	7	7	7	2	7	7
Psychology	5	2	4	3	3	3	5	2	3
Psychology	5	5	3	6	4	4		6	6
Psychology	1	5	3	5	3	4	6	2	7
Sociology	4	3	3	2	2	1	3	2	4
Sociology	7	4	2	4	4	2	2	2	1
Statistics	7	4	5	5	4	4		5	4
Statistics	5	5	2	5	5	5	5	5	4