# Design as a Criterion of Demarcation

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## Abstract

Methodological naturalism, though inexplicit in the denial of purpose, operates exclusively under the tenets of *ontological naturalism*[1] and, therefore, proceeds only by way of the empirical and naturalistic. A more neutral epistemology is less presumptive and would allow science to flourish without the strictures of such a philosophical commitment. The task of divorcing science from methodological naturalism requires the abandonment of the idea that the structure of knowledge or justified belief requires no epistemic foundation[2] and that inferential justification possess a uniquely superior epistemic status in the sciences than that which is non-inferentially known. As I see it, the persistent problem of science and, thus, the criterion of demarcation that undergirds it, is two-fold. First, it is assumed that only inferential knowledge is genuinely justified and, second, that theories must be, at the very least, theoretically falsifiable.

Here I intend to provide a criterion of demarcation of science that is practical and heuristically useful to spur scientific progress. My proposition does not presuppose the causal powers of chance and necessity; instead, it forces the scientist to appreciate the ontological characteristics of nature and leaves the question of causation completely open, thereby, avoiding the pitfalls that ontological naturalism, and its faithful ally, methodological naturalism, habitually impose on science.

**Keywords**: Eutaxiology, design, epistemic justification, foundationalism, criterion of demarcation.

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[1] I will use ontological and metaphysical naturalism interchangeably.
[2] See Neurath, Otto. 1959. *Protocol Sentences*. In Logical Positivism ed. A.J. Ayer Free Press, New York, 199-208.

## Introduction

The history of science is replete with ideas on what science should be and how science should operate; however, delineating what science is has proven to be a difficult task. To be sure, although definitions have not generally interrupted whatever happens in a lab, for knowledge of the world to progress we need a clear distinction between what it means to do science and merely pretending to do science. Clearly, we need to have an understanding of what we are looking for and a methodology of how to look for it. The standard (and dare I say, ambiguous) pronouncement is that science is devoted to solving problems and that it does so by using the observable physical world as the basis for solving them and—in turn—increasing our understanding of the world itself. This is all well, but surely we don't believe that the physical world is our only source of knowledge. The problem is that there seems to be a deep-seated dependence on ontological naturalism to the extent that previous demarcation criteria (which are supposed to be free from ideological bias), as well as the methods of inquiry, are inevitably influenced by it and investigation results ultimately flawed. Ironically, those who fail to see the logical implications assume that inferential justification informs our non-inferential knowledge. In other words, it is assumed that it is the natural world that compels our commitment to ontological naturalism and not the other way around.[3] A shortcoming of this facile rationalization is perhaps that it fails to see the real starting point. Consequently, science cannot do without some ontological commitment,[4] as our observations and methodologies are only as good as our presuppositions.[5] What can we say are our sources of knowledge in interpreting the natural world? Is there enough warrant to believe one frame of reference over another? These questions are loaded with implications and we do not want mere *ideological* commitments to be the gatekeepers of the scientific arena. If we truly want to know what nature is made up of, and indeed what science seeks to unravel, we need to be careful with how we pursue the answers to these fundamental questions.

## From Criterion to Demarcation

In my estimation, the *Problem of Demarcation* in the philosophy of science is closely related to the *Problem of the Criterion*[6] in epistemology. In developing a suitable criterion of demarcation for science, we first need to identify our sources of knowledge and of justified belief. Science is generally thought of as a complete self-sustaining system that depends on nothing more than the so-called scientific method of observation, hypothesis building, making predictions and testing. It is seldom acknowledged that our tools of observation yield representations that demand subjective interpretation. I am not taking a skeptical position here, but I think it is vitally important to the health of science to recognize when we are putting the carriage before the horse, as it were. A criterion that does not meet the

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[3] The idea that science is the final arbiter in ontology is strongly criticized by Philosopher, Yvonne Raley. See for example *Science and Ontology* The Proceedings of the Twenty-First World Congress of Philosophy 12:143-147 (2007).
[4] Quine's 1948 paper entitled *On What There Is* explains the confusions and difficulties in adopting a particular ontology. Descriptions of qualities (such as an object and its representation in our brains) are either true (realist ontology), or they are not (subjectivist ontology).
[5] For Popper, the problem of demarcation was that it appeared to him that "there cannot be any sharp demarcation between science and metaphysics…" See *Realism and the Aim of Science*, Pg. 161.
[6] For a more thorough treatment of this topic, see Roderick M. Chisholm's *Theory of Knowledge*, Pgs. 6ff.

prerequisite of identifying our sources of knowledge is no criterion at all for science; for what does science do without initial statements of fact?

Given our problem of establishing a criterion that could encompass the entire range of scientific disciplines, methodology may be of little use here.[7] How could we apply our criterion to disciplines as diverse as physics and paleontology? The ancient Problem of the Criterion (generally attributed to *Sextus Empiricus*, circa 160–210 AD) stems from our attempt to figure out whether the things we perceive are really as they appear. The problem can be summed up with these two questions:

1. *What* do we know?
2. *How* do we know it?

To understand our perceptions and distinguish true appearances from false appearances, we must employ a criterion (or method) that serves as an aid to distinguish the true appearances from the false ones; however, to develop a criterion we must depend on appearances that we presuppose to be true. The circularity is not difficult to discern. For most things, when we are asked *how* we arrive at certain conclusions, we begin to explain our inferences as they were developed through the experiences that led to them. Seldom do we ever think about the presuppositions implanted before our explanations began to take root. This *method-first* strategy for acquiring knowledge is common practice for empirical science, but is it right? Sir Karl Popper's own strategic moves bypassed or ignored the problem, but in doing so he also excluded genuine science from consideration. I will address this briefly later in the paper.

To resolve the epistemic paradox one could perhaps identify a *particular* instance of knowledge that requires no method for its justification. In other words, we begin with the first question of *what we know*, as opposed to the second question of *how we know it*. When starting with a particular frame of reference, we are not rejecting a criterion for further investigation; we are, in fact, developing it. Do I need additional justification to believe that I am in pain, or that I see light? Do I need a criterion to justify such beliefs? Clearly I don't. In both empirical cases, the subject is *prima facie* justified. However, starting with a particular instance of knowledge, or justified belief, does not itself constitute science. We have indeed identified a source of knowledge, but science requires a criterion that moves instances of knowledge to working hypotheses. Moreover, we need to get from instances of knowledge to structuring that knowledge into a functional criterion that can work across various disciplines.

Arguably, one of the most successful examples of science, the Scientific Revolution, was one that appreciated and exploited the design characteristics of nature to propose theories that, to some extent, still impact us today. Of course, early philosophers were already writing about the design of our universe as self-evident.[8] They recognized that there is a natural

[7] Demarcating science by the "unity of method" remains a mere abstraction that has failed to provide a working criterion.
[8] See, for example, pre-Socratic Greek philosopher, Anaxagoras (ca. 500-428 BC, *Apollodorus* ap. Diog. Laert. ii. 7); Plato (429-347 BC, *Philebus*); Stoic philosopher, Epictetus (55-135 AD, *Discourses* 1.6.1–11); and Saul of Tarsus (a philosopher in his own right c. 5 – c. 67 *Romans* 1:18-20).

epistemic dependence on the order and structure of the world, and thus proposed ideas that cohered with the natural order in the language of mathematics. They acknowledged that it is, in fact, the apprehension of order in nature that determines how nature is understood. Two competing philosophies on how we come to understand the world in the theory of knowledge, rationalism and empiricism, debate whether knowledge could be justified *a priori* or *a posteriori*. Empiricists base knowledge on sense experience and induction, while Rationalists base knowledge on reason and deduction.[9] Interestingly, some Empiricists (namely, *logical positivists*) rejected a realist ontology and opted for a subjective one devoid of any true picture of reality. Our descriptions, they argued, are mere artifacts of human conventions.[10] Nevertheless, no proposition can function without first presupposing other beliefs about reality and one cannot continue *ad infinitum* assuming that all belief is inferential. It is, therefore, logical to propose that all inferential knowledge is subservient to foundational knowledge. This, I believe, is at the core of science. Indeed, when we attempt to answer a question about how we came to a particular conclusion, we want to know the premise on which the conclusion rests. However, any premise that is not basic will suffer under its own need for justification. That is to say, for any subject S to be justified in believing a proposition P on the basis of evidence E, one must be justified in believing $E_1$ on the basis of another proposition $E_2$, and $E_2$ on yet another proposition $E_3$ and so on. If all epistemic justification is inferential, then we wind up with an epistemic regress, or something circular that does not do anything to reinforce our propositions. To illustrate, I may claim to be justified in believing that when I release an apple from my grasp it will not remain suspended in midair, or ascend. I am justified in believing that it will descend because of other known factors, namely, the laws of physics. But to justify my belief in the laws of physics requires that I know something about the inner workings of physics, and that *something* may also depend on something even more fundamental, so that all knowledge is parasitic on how we justify belief. Foundational knowledge, then, serves as the ground on which to build our propositional pillars.

## Design as a Criterion of Demarcation

Design as a criterion of demarcation, the proposition expounded in this paper, affirms that design in nature is a properly basic belief[11] and that in order to do science, one cannot escape its constraints. That is to say that science is confined by the boundaries of patterns, order, structure, and regularity that make up the world. Putting it plainly, design bridges the chasm between ontology (*what is*) and epistemology (*how we know it*). This means that every *a posteriori* inference owes its justification to *a priori* knowledge. To be clear, I am not using the term *design* to mean artificiality, plan, or purpose. I am simply using the term to denote order, function, law, regularity and such characteristics. To use design as a criterion of demarcation of science is simply to let nature's design *characteristics* provide the parameters of investigation. Accordingly, there is no sense (at least for the advancement of knowledge) in asking whether things in nature have the "appearance" of

[9] Popper turned empiricism on its head by proposing that experience does not verify theories, but rather falsifies them.
[10] See, for example, Rudolf Carnap's *The Logical Syntax of Language*.
[11] What I mean by "properly basic belief" is belief that is foundational for knowledge and, therefore, is not dependent on any other epistemic justification. For example, Descartes' own *cogito ergo sum* is a position on what can actually be known from experience, reducing justified belief to the *ego* that is revealed by the *cogito*, and this, doubtlessly avoids an infinite regress of justifications. The concept has been around for some time, but has been noticeably popularized by Alvin Plantinga.

design when it is almost universally explicitly or tacitly acknowledged in the scientific community. This design-centric criterion of demarcation affirms design as an ontological feature[12] of the universe, but it does not presuppose causation. Since this view of science is epistemically pre-theoretical, one might say that it is a eutaxiological[13] philosophy of science. For science to flourish, the question of purpose must remain open, though not presuppositionally affirmed. As such, design as a criterion of demarcation is more concerned with *what is* and not necessarily with particular rules for demarcating science. The only rule (and therefore, our criterion), *which involves the search for the degree of order and complexity of processes or structures*,[14] is established by the coherence in nature's ontological characteristics. In other words, the activity of scientists is distinguished by the incessant search for comprehensibility, for patterns, for things that we recognize immediately without deep ratiocination. *How* these attributes came about is what theories are intended to resolve and, therefore, design is the *sine qua non* of science. As Popper aptly put it, "[The scientific investigator's] aim is to find *explanatory* theories (if possible, true explanatory theories); that is to say, theories which describe certain structural properties of the world, and which permit us to deduce, with the help of initial conditions, the effects to be explained."[15] This way of reasoning puts teleological explanations on an equal footing with teleonomical ones, where both can propose a cause for the effect in question.[16] Design as a criterion of demarcation creates the boundary by which science must operate. It is not to be thought of merely as a theory, but as a determinant from which all theories must operate. The nineteenth century professor of geology and critic of teleological design arguments, Lewis Ezra Hicks, wrote:

> "Physical science is a classified knowledge of external nature; but the possibility of classification, and therefore of science, lies in the fact that there is first a natural, external *order*, whence arises the logical, internal order in the arrangement of facts and principles, which constitutes true science. The external order existed before the science which is based upon it. There was celestial harmony before the science of astronomy was constructed by formulating the laws and principles gathered from observation of the heavens….
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> This eutaxiological argument, then, seems to have no end to it; for order is universal in nature. "[17]
>
> Lewis Ezra Hicks, *A Critique of Design-Arguments,* Pg. 17f.

Here we see how this idea of identifying design and searching for the degree of order and complexity of processes or structures can serve to develop a rigorous scientific research program that isn't committed to either teleological nor teleonomical presuppositions. As such, the different approaches offered by either side of the aisle are welcome. If our

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[12] Ontological design is the contradistinction to ontological randomness and neutral as to the cause of order.
[13] From the Greek word '*eutaxia*', meaning 'good order.'
[14] See Michael Anthony Corey's God and the New Cosmology: The Anthropic Design Argument, pg. 10ff.
[15] See Popper's notes with respect to causal explanations on *The Logic of Scientific Discovery* Pg. 40.
[16] Scientists committed to ontological naturalism have been privileged with monopolizing knowledge without merit and persistently borrow from design to make predictable outcomes.
[17] Hicks' concern was also on the conflation of teleological and eutaxiological design arguments. He did not negate natural order, but the idea that order was indicative of purpose or contrivance.

presuppositions force us to commit ourselves to one perspective—one way to approach the same question—we are no longer doing science but, rather, engaging in the segregation of thought.

Characteristic of previous demarcation criteria is the failure to provide direction and a structure from which alternative methodologies can develop and incommensurability can be avoided. Indeed, they have been more restrictive than progressive in their attempts to protect the enterprise from unwelcome company. For instance, Popper's falsifiability criterion only limits scientists on the types of theories they can subject to scrutiny, but it does not imply that other theories aren't true. This is another way of saying that science must proceed methodologically with testable ideas within the margin they have predetermined. But what does it benefit science if all we know is what can be subjected to agreed upon methods of inquiry? What other guidance does falsifiability offer to the scientist? It seems that the filter of science is being misused. If the purpose of the filter is to sieve empiricism from other frames of thought, then it needs to also provide direction as to what the recipe of science is intended to produce.

**Adoption Of An Ontological Commitment**

I will not engage in the typical mental exercises of philosophers, or visit their wonderlands and attempt to give life to unactualized possibles, Meinong jungles, or other fantasies. Those types of wanderings always seem strange to me as a common sense realist. I am not interested in any form of modal realism nor do I see its usefulness. Indeed, I have exposed my ontological commitment to *what is* and perhaps by extension, *what is not*. But how do we determine if we are committing ourselves to a proper representation of reality? What factors would help us arrive at our conclusions with confidence? When adopting an ontological commitment, we are faced with the decision of either, as Quine put it, "dulling the edge of Occam's razor," or relying on our crudest observations. It may hold that the limitations of language (or metalanguage[18]) have an impact on our descriptions of reality, yet our descriptions continue to produce results because they point to undeniable characteristics of nature. We often use analogies to describe what is it is we are attempting to elucidate, yet our symbolic language does not dictate what reality is. The language of science, whatever it may be, helps us to create mental representations of our observations and, thereby, an ontological representation of reality.

In science, however, it is not enough to identify a correct ontology, but also an adequate one, as that is what will ultimately determine our research design. Recognizing the difference between a correct and an adequate ontology will determine how we proceed from our epistemological questions to our methodological ones. For example, a *realist ontology* may be correct, but it would be inadequate as a criterion because it does not give the sort of information that tells us how to proceed with our investigations. My proposition of design as an ontological feature of the universe takes advantage of the characteristics of the natural world as a means to do science. It does not merely define science; it provides a

[18] There are a wide range of truth theories; here I am thinking of Alfred Tarski's formulation in which truth statements are determined by their correspondence to reality.

foundation for it. What I am attempting to lay out here is a structure of epistemic justification that would lead to doing good science.

In his analysis of determining the best way for conducting social research, Egon Gotthold Guba, outlines three fundamental questions (Fig. 1) that help characterize a paradigm. For our purposes, these are questions we should ask when seeking to justify our theories. The first is an *ontological question*: What is the nature of the "knowable"? Or, what is the nature of "reality"? This is the object that we are to subject to our methods of inquiry. If we don't know what we are studying, then we better hope that we obtain knowledge by stumbling upon it in a blind and random search. The second is an *epistemological question*: What is the nature of the relationship between the knower (the inquirer) and the known (or knowable)? Here, our ontological answer guides our epistemological one. Do we espouse objectivism, or rationalism? Are our senses the only source of knowledge, or can we depend on reason? The third and final question is a *methodological question*: How should the inquirer go about finding out knowledge? Again, the answer to this question lies within the margins of the ontology we accept as true.

**3 Fundamental Questions**

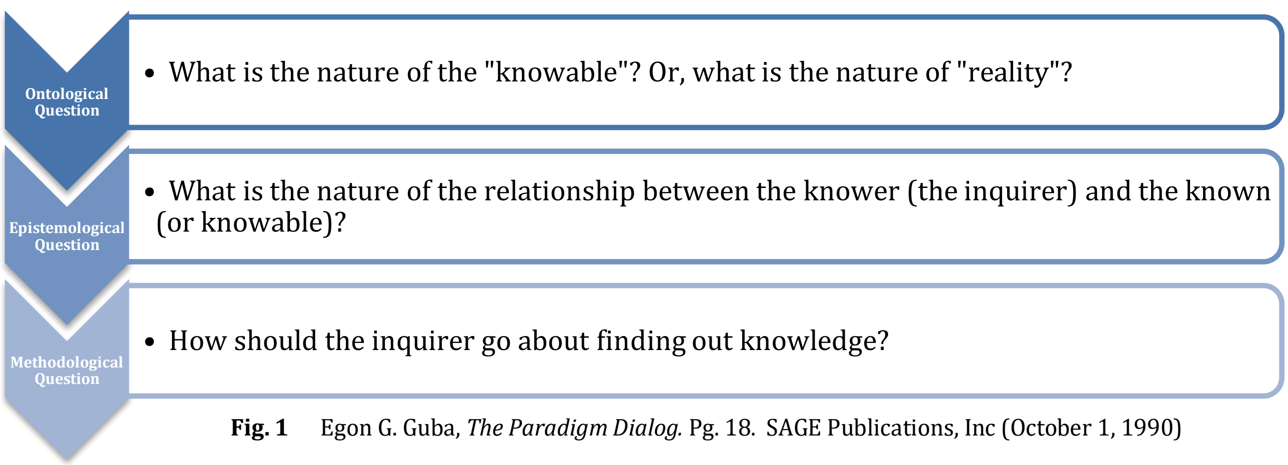


**Fig. 1** Egon G. Guba, *The Paradigm Dialog.* Pg. 18. SAGE Publications, Inc (October 1, 1990)

Design (in the sense that I am referring) is not seen in the work of Sir Karl Popper, as far as I am concerned. As a realist, he really wanted science to progress in understanding the real world, yet without ever having the temerity to make any final pronouncements. He wrote:

> "[T]he system called 'empirical science' is intended to represent only one world: the 'real world' or the '**world of our experience**.'" [Emphasis added]
>
> Karl Popper, *The Logic of Scientific Discovery* p. 16

But what *is* the "real" world? What is "the world of our experience" according to Popper? What sort of questions would we ask if the "real" world were different? Popper was an anti-conventionalist, so he adopted Alfred Tarski's correspondence theory of knowledge. For clarity, the correspondence theory of *truth*, as it is most commonly known, states that the truth of a statement or belief is determined by how it relates to a fact of the world and whether it accurately describes, or corresponds with it.[19] Popper's criterion was intended to correct our interpretations of what is observed. He believed that statements were not merely fallible, but that they are, in fact, theory-laden. A good example of this is philosopher Paul Draper's position on metaphysical naturalism. For the sake of brevity, I will not cover his view at any length here, but want instead to introduce you to the idea of metaphysical naturalism as Draper (a prominent advocate) himself defined it during a 2007 interview for the Future of Naturalism conference at the Center for Inquiry in New York. He said,

> "Metaphysical naturalism is the view that nature is a closed system. That there are no supernatural entities."

Of course, the most obvious problem for the advocate of methodological naturalism is that he arbitrarily defines what nature is. Once more, it is our presuppositions that drive our methods of inquiry, so whatever our starting point, it should help guide us in our attempts to advance knowledge. So, if we assume nature is a closed system, as Draper suggests, what kind of questions are we logically permitted to ask? In our attempt to answer the ontological question, metaphysical naturalism really does not have much to say. In other words, it is not very informative to say that nature is natural, or nature is physical, or that it operates by a nexus of inviolable laws. If we are going to do science, we need to aim our attention on nature's full range of characteristics. In turn, these characteristics should evoke our imaginations as to the methods we use in our pursuit of knowledge. This brings us to another question: How do we trust our cognitive disposition with respect to our perception of the natural world? As alluded to earlier, there are only two ways to answer this question: either we depend on our methods (ignoring their dependence on prior assumptions), or we depend on the reliability of our internal disposition. One could employ an externalist (*reliabilist*) solution and suggest that it is not our *independent* cognitive disposition that we trust, per se, but rather how nature actually works. For example, if nature did not have the type of comprehensibility that we could trust, we would be deluding ourselves to think that we can do science. Only order yields predictable outcomes. Our foundational belief, then, is only true if it corresponds with reality.[20] To be sure, it is not possible for a mind to force structure to an inscrutable or incomprehensible world. Yet, our basic belief is as such because it does not depend on any other knowledge for its justification. It is justified because it is acquired immediately, internally, and objectively. That is to say, we can cogitate upon our direct acquaintance with the structure of the world, which suffices as an objective instance of knowledge or justified belief.[21]

Consequently, the problem we have faced in proposing an adequate ontology, and therefore, a criterion of demarcation between science and non-science is really the problem of determining the sort of explanations we were willing to entertain as we sought

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[19] See Robert C. Solomon & Kathleen M. Higgins, *The Big Questions: A Short Introduction to Philosophy* 9th Edition. *Glossary* Pg. 419

[20] Notice here that I am not saying that our belief is not justifiable independent of experience, but that it is not *true* if it does not correspond with it. This is essentially taking account of the so-called Gettier problem as it related to our mental faculties. My proposition is that our belief is basic and also true and confirmed by our direct acquaintance.

[21] See Roderick M. Chisholm's *Theory of Knowledge*, Pg. 7.

to contribute knowledge. Personally, I don't see how we can develop a criterion without presupposing a source of our understanding of things. In order to solve problems, we need to first understand the world. Therefore, if the aim of science is to describe the *real* structure of the world,[22] our immediate reaction—our intuition—tells us that seeking to unravel its design is the way of science. My foundationalist[23] proposition or view of science depends on the premise that design is self-evident and, therefore, a properly basic belief. This belief, which is formed referentially by direct acquaintance with the natural order is a good starting point for science. As I see it, it is perfectly adequate to "demarcate" science by the very thing science is invested in discovering (i.e. its ultimate design).

My position, with respect to our perceptions of the natural world, differs considerably from that of philosophers, such as Alvin Plantinga,[24] who maintain that teleological design belief is basic. My own position is that only eutaxiological design belief is basic. We may be able to conjoin other basic beliefs about the origin of design, but order, patterns and the such, do not belong to the teleological design classification by default. My opinion is that since ontological questions deal with *what is,* the proper place for design *perception* is on *attributes*, not *causes*. For instance, if I see a Ford Model T, I may immediately intuit that it is the product of mind, not chance or necessity. This is perhaps because I am well acquainted with minds and their artifacts and because I too possess a mind capable of producing artifacts. This is true even if secondary causes were employed. However, I am inclined to think that this is not the case with the natural world. I can appreciate the order, laws, regularities and beautiful structures, but I may form a teleonomic belief of nature's design, most especially if I am already predisposed to that sort of thinking. I think that an ontological commitment to design attributes is less problematic than presupposing causal connections without drawing them by way of inference. Plantinga et al. would have teleological design arguments promoted (or demoted, depending on your attitude toward deductive inferences) to basic beliefs. But I think this is a mistake. Current design arguments have good explicatory power and rightly have a place in science. The problem for design theorists is not so much that they can't make a good case but, rather, that they are bringing their case to the wrong court. If the ontological commitments of science are to metaphysical naturalism, or physicalism, then design in the teleological sense cannot even step into the court. There is no room for it, not even in the jury.

A criterion is a 'means of judging' and thus, can be used as a standard or a characterizing mark from which we can make judgments. So then, developing a criterion of demarcation concerns questions of how to identify sources of knowledge or justified belief. The structure of epistemic justification here proposed, instead of leading to an infinite regress of explanation, inexorably forces us to respond to the ontological question: *What is*? The picture we get of science, then, does not depend first on our observations but, rather, on what sort of ontological commitments predispose our observations to a particular

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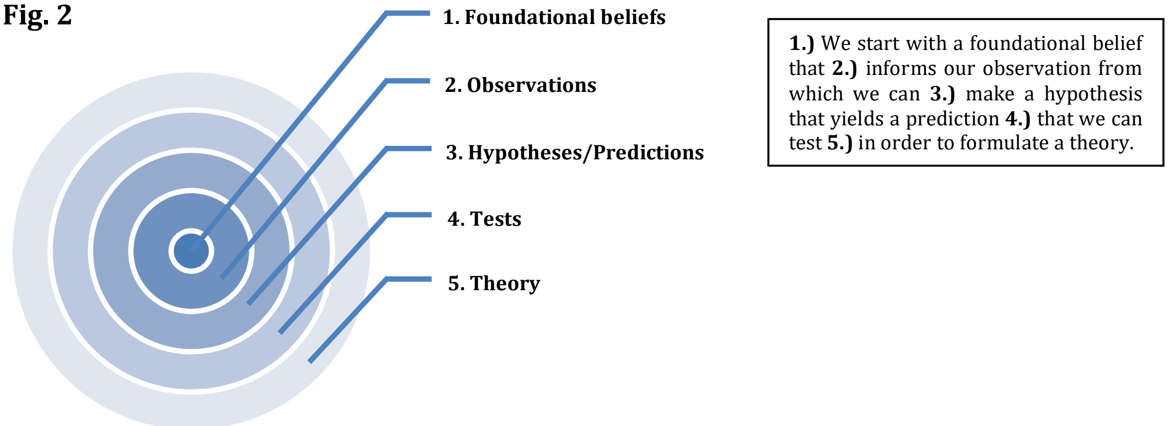
[22] Scientific realism is a position that rejects the idea that the world is really a construct of our fertile imagination. The world, according to metaphysical constructivists, is a mere representation of our theorizing. (See Peter Godfrey-Smith's *Theory and Reality*, Chapter 12).
[23] I am not going to attempt to thoroughly defend foundationalism from previous criticisms here (i.e. the Agrippa/Münchhausen trilemma, The Gettier problem, etc.), as I think vastly more qualified scholars have adequately responded to them already. See, for example, works by Olaf Tollefsen, Michael DePaul, Richard Fumerton, Laurence Bonjour and Timothy McGrew.
[24] Alvin Plantinga. *Where the Conflict Really Lies: Science Religion, & Naturalism*. (2011) Chapter 8, Pgs. 225 – 264.

interpretation. The schematic representation below shows the logical structure of science (Fig. 2) as it actually operates.

**Logical Structure of Science**

**Fig. 2**




Popper believed that all knowledge remains fallible and conjectural.[25] As such, he did not demand the verifiability of statements; instead, he proposed that statements had to have the quality of refutability. He did not believe that we have the capability of giving true descriptions of our observations and, thus, he developed a criterion of demarcating science from non-science by the filtering of falsifiable statements from non-falsifiable ones. In doing so, he thought he avoided an infinite regress of justification and at the same time a way to keep science moving and advancing knowledge.[26] Since the justification of statements were not judged by their verifiability, but by their falsifiability, falsification required "special rules" in order to refute them. He wrote:

> "We must clearly distinguish between falsifiability and falsification. We have introduced falsifiability solely as a criterion for the empirical character of a system of statements. As to falsification, special rules must be introduced which will determine under what conditions a system is to be regarded as falsified.
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> We say that a theory is falsified only if we have accepted basic statements which contradict it (cf. section 11, rule 2). This condition is necessary, but not sufficient; for we have seen that non-reproducible single occurrences are of no significance to science. Thus a few stray basic statements contradicting a theory will hardly induce us to reject it as falsified. *We shall take it as falsified only if we discover a reproducible effect which refutes the theory*. In other words, we only accept the falsification if a low-level empirical hypothesis which describes such an effect is proposed and corroborated." [Emphasis added]
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> Karl Popper, *The Logic of Scientific Discovery*, pg. 66 (Routledge 1992).

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[25] See *Realism and the Aim of Science*, introduction, Pg. xxxv.

[26] See Popper's *The Logic of Scientific Discovery* Pg. 26.

It makes one wonder what Popper meant by stating that a theory is taken as falsified only if "*a reproducible effect which refutes the theory*" is discovered. What sort of effect is reproducible? Is Popper admitting to a necessary condition that must be met before a theory could be refuted? Popper seems to be saying, perhaps inadvertently, that falsifiability is what is minimally needed, and regularity (a design attribute) is what is maximally needed to falsify a theory. Popper avoids the Problem of the Criterion mentioned above as he is not concerned with a particular instance of knowledge; he assumes no knowledge can be gained apart from methodology. By proposing that an empirical scientific system must be refuted by experience, he has given precedence to method over prior instances of knowledge, which are necessary for the development of his criterion in the first place. He goes directly to the second question of the epistemic paradox. We can appreciate the role that experience plays in the justification of statements or claims, but falsifiability imposes an unnecessary burden on science.[27]

It is not difficult to see why falsifiability is not a good criterion of demarcation. Many have been critical of Popper's ideas[28] to various degrees, but my own concern is that his criterion does not do enough to spur scientific progress. The advancement of science (not just its continuance) does not merely require a demarcation that sets the parameters or scope of investigation; it must also guide as to the sort of characteristics that are to be sought in our pursuits. To this end, my criterion of demarcation stresses that every problem of science should be treated as an engineering endeavor. If design as a criterion of demarcation provides the parameters of investigation, design cannot be *imposed*—as in drawing a target around an arrow—but rather it is to be *discovered*, as in structure, regularities, laws, mechanisms and other like attributes. This immediately excludes the usual suspects such as Marxism, psychologism, astrology, multiverse hypotheses and similar mental notions that stem from fertile imaginations as opposed to careful investigation. Seen this way, design can be used as a way of disconfirming imposed renderings of reality, just as falsifiability disconfirms theories through the rules of falsification. Moreover, design as a criterion of demarcation does not constitute the notion that our acquaintance with nature informs us of *all* laws, structure, or regularity. It merely gives us the backdrop and, thus, the confidence to operate within nature, encountering anomalies along the way, but informing us just enough to not hinder that confidence. Anomalies are treated as learning from nature its design and our theories are reworked in the exchange of investigation. This may be taken to mean that science is not merely a systematic way to study what nature readily reveals; it is also a way to understand its secrets and limitations.

## Of Processes and Mechanisms

I submit that conjectures (to use Popper's term) cannot be mere wild speculations, but rather structured inferences aimed at understanding the effect in question.[29] Refutations,

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[27] Hilary Putnam illustrates the burden that falsifiability can put on a theory in his chapter from *The Philosophy of Science* entitled *The Corroboration of Theories, Pgs. 124f.*

[28] Paul Feyerabend, Thomas Kuhn, Imre Lakatos, Max Houck, Larry Laudan, Hilary Putnam, and the Willard Van Orman Quine and Pierre Duhem thesis to name a few.

[29] See Hilary Putnam's criticism in *The Philosophy of Science* by Richard Boyd, Philip Gasper, J D. Trout, Pg. 122.

as noted earlier, only follow when our expectations are shown to be *imposed* on the natural world, as opposed to *discovered* from the natural world. For example, if we begin with the assumption that nature is a closed system (i.e. metaphysical naturalism), then we will only attribute causal mechanisms to every scientific question. But clearly this metaphysical presumption assumes too much and results in confusing processes with mechanisms. Although the terms are used interchangeably, confusing terms is always a hindrance to understanding. To be sure, every event results from a process, but not every event results from a mechanism. More clearly, a mechanism is always a process, but a process is not always a mechanism. In science, it is perfectly admissible to demand processes, but it is not admissible to demand a mechanism, most especially when mechanisms are not causally adequate for the effect in question. A mechanism, a term derived from "machine"[30] (a self-contained apparatus or process), limits our options and is wrongly accredited with every phenomenon we encounter.[31] Of course, this does not imply that we should immediately invoke causes that are of the teleological variety but, rather, that we need to recognize the limitations foisted upon science as a result of a philosophical bias. A great example that sheds light on the difference between a process and a mechanism is seen in the work of bacterial geneticist, James A. Shapiro. His insight on a cell's ability to direct genetic change and repair by means of various very complex strategies is one that can easily be missed if we assume that only mechanistic processes are at play. He wrote,

> "Another common misperception in many conventional discussions of genomic change is that cells cannot avoid the automatic production of mutations in response to DNA-damaging agents such as UV radiation or mutagenic chemicals. This misperception results from ignorance about the sophisticated apparatus that even the smallest cells possess to repair genome damage and a failure to appreciate the power of cellular genome surveillance and response regimes."
>
> James Shapiro, *Evolution: A View From The 21st Century*, pg. 14

The distinction, as illustrated above, shows how easily we can miss the forest for the trees, as it were, if we assume that all cell change is fatalistically determined as Crick[32] and others have believed. A process such as this requires scientists to look past the assumed mechanism and observe what is happening in real time. In a mechanistic type of scenario, all a scientist needs to do is extrapolate from cause and effect assumptions and miss important details as a result. A process that is not mechanistic is lost in history and all that is left is the effect that the process has left behind. Scientists wedded to the idea of a closed system have promoted mechanisms from descriptions of natural phenomena to ultimate causes of all natural phenomena. This attitude, which has exchanged the free enterprise of science for despotism, has limited science in such a way as to create enmity between those that embrace *teleology* and those that embrace *teleonomy*. In order to eliminate debate or confusion over teleology and teleonomy as it relates to design, we must first recognize the

[30] From the Greek *mēkhanē* and the Latin *mechanismus*.

[31] Distinctions between mechanisms and regularities (i.e. a regularity can be statistical as opposed to deterministic) have also been made by philosophers of science, but these nuances are too vast to cover here. See, for example, Benjamin Barros' *Natural Selection as a Mechanism.* 2008.

[32] See Francis Crick's "Sequencing Hypothesis and Central Dogma" in his 1956 paper *On Protein Synthesis*.

difference between a cause and a process. While both are empirically discernable, a process is what we observe and a cause is what we infer. In the case of non-mechanistic processes lost in history (i.e. causes that are out of the reach of direct investigation), the only options for investigation would involve reverse engineering, or inferences drawn from currently known causal processes. This, of course, is problematic for teleological explanations as they are for teleonomical ones. Shapiro's insight shows not only how assuming mechanisms were responsible in cell change can yield erroneous results, but also how, absent a mechanism, teleological assumptions (apart from cell cognition) can be equally wrong. Yet, there are instances in which design inferences do yield knowledge not derived from assumed mechanisms. Take, for example, the case of the so-called "junk DNA." As it turned out, these sequences of DNA that do not code for proteins actually serve other functions (transcription, translational regulation, etc.), but due to a prior commitment to mechanistic processes (as in Crick's Central Dogma), their function had been overlooked and only later proposed by design proponents committed to teleology. DNA is an interesting molecule. Since its structure was identified by Watson and Crick in 1953, and the sequence hypothesis proposed by Crick five years later, scientists have been stumped by its sheer elegance and informational properties. There is simply no known mechanism to explain the information embedded in the molecule along its longitudinal axis. The nucleotide base pairs that are sequenced to specify functional roles within the cell are quite literally arbitrary, as the sequence does not depend on any affinity between the bases.[33] Again, absent a mechanism, teleological design becomes a very attractive alternative.

In the body of this paper I suggest that my demarcation criterion set teleological explanations on an equal footing with teleonomical ones, where both can propose a cause for the effect in question. I wrote this well aware of how I am pitting both law and agency against each other; but I did this only to make a distinction between two modes of explanation to ontological design, that is, primary and secondary causation, both of which may enjoy the benefits of my demarcation criterion for science. In the first type of explanation nature may be worked out rationally (a priori), and science progresses by way of appealing to cause and effect. By contrast, the second type of explanation (a posteriori) deduces knowledge of the natural world from effects to causes.[34] The first may relate laws to mind (top-down), while the second may assume laws to be a mere inherent property of nature (bottom-up). In affirming design as a criterion of demarcation, the distinction set forth here is trivial (not to say superficial). The important thing is that it is laws, affinities, regularities, patterns, etc. that make ontological design self-evident and science possible. Notice here that both terms (teleology and teleonomy) use the prefix *teleo* (from τέλος - télos: end; goal; purpose), and only differ in the suffix, *logy* (from λόγος – logos/logic) and *nomy* (from – νόμος, nómos: law). Design is not some abstract idea that requires elaboration; however, I define it here broadly in order to prevent the stalemates that only serve to stifle scientific progress. In proposing ontological design as a criterion, neither teleonomical nor teleological propositions are to be considered as privileged *explanans* and, in this case, immunity is only reserved for the self-evident *explanandum*. What we do not

[33] Stephen C. Meyer's lengthy book, *Signature in The Cell*, does a great job at shedding light on the problem.
[34] See Karl von Prantl *History of Logic* (iv. 78), concerning German philosopher Albert of Saxony (ca. 1316 – 1390), who made a distinction between *demonstratio a priori* (the proof from what is before), and *demonstratio a posteriori* (proof from what is after).

want to do in science is to marginalize ideas that we disagree with simply because they do not conform to how things are usually done.

## Incommensurability

Science, as we currently know it, is divided not only in focus, in practice, and in language, but also in being able to harmonize natural phenomena across all disciplines. Since there is no single method of science that applies equally to all disciplines, the stratification of science typically depends upon the clear discontinuities that exist in nature (from physics, chemistry, biology, etc.). This expected division makes it difficult to find ways in which all of nature may converge.[35] My proposition is that the one thing that unifies all science is our dependence on a particular attribute of nature; that is, design. It is foundational to every area of science and it is what makes predictions possible. In fact, other criteria of demarcation depend on prior assumptions about the functions and structure of the world also, but the assumptions generally go unnoticed. My proposition of design as a criterion of demarcation also has the benefit of unifying the language of science and resolving incommensurabilities through the sharing of scientific nomenclature with design as its foundation. Language is, more often than not, helpful in converging ideas. Yet in science, where precision is everything, language often becomes an impediment for scientific growth. Since all of science depends on design for understanding and investigating, it also makes sense that my criterion may evoke a unity of scientific parlance not only within specific disciplines, but also across various disciplines with similar objectives.

## Conclusion

We have veered far from pursuing science for the knowledge it contributes and have only managed to amass academic relationships as we avow to remain loyal to the traditional consensus. The aim of my proposition is to bring together ideas that help us better understand the world. Scientists don't often realize that their preferred ontological commitment (perhaps subconsciously) drives their scientific methodology and ultimately the sort of results they get. My criterion of demarcation is a demarcation set by the very attributes of nature, so the ontological commitment is one that corresponds with reality. No matter what other philosophical baggage we may bring, here is one undeniable truism:

> "The best way to account for the coherence of our experience is to suppose that the outside world corresponds, at least approximately, to the image of it provided by our senses."
>
> Alan Sokal & Jean Bricmont. *Fashionable Nonsense* (1998). Pg. 55

If design were not a self-evident attribute of nature, science would simply not be possible. We go about our way without a single thought about what keeps our feet firmly planted on

[35] Physicists seeking a unified "theory of everything" may be on to something, but their focus is typically a reductionist notion that rests on a mechanistic conception of the world. Perhaps design is a theory of everything. That is, if all of nature exhibits the sort of characteristics that are comprehensible, then it may not be that what we are looking for is the unification of laws, regularities, or order, but rather a meta-principle to rule them all.

the ground as we traverse the plains and vastness of time. Our intuitions inform us with enough acuity that we can go with confidence wherever nature leads, to understand her and lay bare her design. This is science.