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Just So Stories: Posnerian Methodology

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Hear and attend and listen; for this befell and behappened and became and was...\footnote{RUDYARD KIPLING, JUST SO STORIES 177 (Everyman's Library Children's Classics 1992) (1902).}

INTRODUCTION

At the turn of the last century, Rudyard Kipling spun fantastic yarns that supposedly explained how certain surprising facts about the world came about. The Just So Stories\footnote{According to the editor's notes to the Everyman's Library Children's Classics edition, the title of the collection reflects that Kipling's children, with their infantile love of repetition and precision, always insisted that the stories be repeated word for word—"just so." Of course, the title also suggests that the author was jokingly asserting that the stories were true, and things did, in fact, occur "just so." See id. at Editor's Comments.} drew from sources as diverse as Eastern mythology and Western paleontology to speculate on the causes of not only natural curiosities, such as how the camel got his hump, but also cultural ones, such as how the alphabet was made.

At the turn of this century, Judge Richard Posner, the doyen of the law-and-economics movement, draws from sources as diverse as economics and socio-biology to spin fantastic yarns that supposedly explain not only legal curiosities, such as how the tort system came about, but also social ones, such as why mothers love their children.

Kipling implicitly understood that although "abduction"—the post hoc imagining of explanatory stories—can play an essential role in scientific inquiry, it is merely a means of forming, not proving, hypotheses. One can easily abduct explanations that, while sounding plausible within their context, appear fantastic when examined more fully. Consequently, Kipling created his just so stories as fairy tales
for the amusement of his daughter. In contrast, Posner claims his just so stories are scientific theories for the edification of the legal community. He wants his audience to accept his economic analysis not merely as an account of past legal developments, but as a model for future ones.

Posner’s most recent foray into methodological issues appears in a vociferous reply to the rather modest suggestion by Professors Christine Jolls, Cass Sunstein, and Richard Thaler (“JST”) that legal scholars might wish to consider the implications of certain recent empirical economic research on decision making. Posner accuses JST of a failure to grasp not only the economic theory of rational choice, but also the very nature of scientific theory per se. And yet, Posner’s reply evinces both a highly idiosyncratic definition of rationality and an ad hoc argument that strays far afield from the methodology that usually underlies economics’ claim to scientific status.

This Article is one part of a three-part critique of Posner’s conception of economic rationality. In Rationality in Law and Economics Scholarship, I explore at length the degeneration of Posner’s conception of rationality from the elegant, if simplistic, model drawn from neoclassical economics to its current ad hoc state. Indeed, all that seems to be left of the “rational” component of

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4 Kipling wrote the just so stories for Josephine, his favorite child. The narrator addresses each story to her as his “Best Beloved,” and she is no doubt the model for the “little girl-daughter” heroine of several of the stories. Sadly, she died at the age of six before the stories appeared in print in 1902. This might account for the palpable sense of melancholy that haunts the author’s enigmatic illustrations to what are supposed to be humorous stories.

5 As Posner himself notes, wealth maximization has both positive and normative aspects. The former posits “that the common law is best understood on the ‘as if’ assumption that judges try to maximize the wealth of society.” RICHARD A. POSNER, OVERCOMING LAW 172-73 (1995) [hereinafter POSNER, OVERCOMING LAW]. The latter posits that “judges should interpret...antitrust statutes to make them conform to the dictates of wealth maximization.” Id. at 173; see also Lewis A. Kornhauser, Wealth Maximization, in THE NEW PALGRAVE DICTIONARY OF ECONOMICS AND THE LAW 679 (Peter Newman ed., 1998). Even Posner now admits that “[n]ot all questions that come up in law, however, can be effortlessly recast as economic questions.” POSNER, OVERCOMING LAW, supra, at 22 (discussing abortion specifically); see Kornhauser, supra, at 682 (“In recent years, Posner has weakened his claim from one that asserted that common law courts should be exclusively concerned with wealth maximization to one that asserts that wealth maximization is one of the values that common law courts ought to pursue.”).


7 See infra Part II.A.

Posnerian rationality is the affirmative normative connotations of the term itself. In *Fear of Freedom: A Polemic Against Policy*, I contrast the concept of rationality as used in neoclassical economics with that used in the speculative philosophic tradition. Economic rationality is consequentialist reasoning: choosing an appropriate means to achieve a pre-given end. In contrast, speculative rationality is the process by which a subject determines an appropriate end. Economists assume that rational behavior is predictable and that irrationality is the source of unpredictability. The speculative philosopher believes that irrationality can be rigidly predictable whereas rationality, which is grounded in freedom, is the source of true spontaneity.

In this Article, I examine the methodology that Posner claims to follow and the methodology that he actually follows in developing his account of rationality. Posner presents his methodology as being standard within economics. He states that, in science and economics (and, presumably, law), theories must be instrumental in nature, and that the only appropriate purpose of an economic theory is prediction. I show that not all economists—let alone scientists, philosophers, or legal theorists—agree with the assessment that science should be primarily useful, like engineering, and adopt a number of alternative goals, such as description, explanation, and understanding.

I believe that Posner concentrates on the goal of prediction because he sees law and economics as a form of policy science. That is, he wishes to use the law to manipulate legal and economic subjects to act in such a way in order to further a societal goal, such as wealth maximization. In contrast, speculative theory and critical legal theory seek not so much to predict behavior, but to understand the law. The critical theorist does not identify with the legislator or judge who writes and interprets the law, but with the attorney who advises and represents the individual citizen who is subjected to the law. The critical theorist seeks to understand how she and others fit within the legal system, in order to free herself from its manipulation and, if possible, manipulate the law for her own individual, subjective purposes. I develop this point extensively elsewhere and shall touch on it only briefly in this Article.

Posner identifies his methodology with the widely adopted scientific methodology of falsification. Once again, not all

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philosophers, scientists, or economists (let alone legal theorists) agree that falsification is the only appropriate methodology, or even that it is appropriate at all. Nevertheless, it is certainly a widely accepted one. Posner does not, however, address directly the extensive literature on scientific methodology, but bases his brief methodological discussions on Milton Friedman's notoriously controversial essay, *The Methodology of Positive Economics*. In this Article, I briefly describe Friedman's thesis and discuss the avalanche of criticism it has received. I show that Posner concentrates precisely on the most idiosyncratic and controversial portion of Friedman's work—what Friedman's critics call the F-twist, or the unreality principle. Indeed, Posner gives the F-twist an additional turn of the screw, taking it to a logical extreme that perhaps should be christened the P-twist. Because Posner claims to follow falsification, I give a brief discussion of the standard account of falsification, which was developed by the philosopher of science Karl Popper, and show how Posner confuses falsification with the unreliable methodology of verification. Consequently, although Posner accuses JST of proposing ad hoc nonfalsifiable hypotheses, JST is correct in counteraccusing Posner of precisely the same sin.

I, nevertheless, conclude by suggesting that in his recent work Posner might have stumbled onto the first step in the "sophisticated falsification" methodology explicated by philosopher of science Imre Lakatos and promoted by economic historian Mark Blaug. Posner is correct that a sophisticated falsifier does not abandon the "hard core" of his working hypothesis merely because he observes data that at first blush seem to be inconsistent with it—such as the observations of behavioral economists that seem inconsistent with the rationality postulate of neoclassical price theory. Rather, the sophisticated falsifier first uses "abduction" to attempt to formulate a protective belt of auxiliary hypotheses—or post hoc explanations—that might account for the anomaly and thereby

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11 The preeminence of falsification methodology is probably less widely accepted by scientists and philosophers of science than lawyers seem to assume. See infra text accompanying notes 129-50. In my theoretical work I rely heavily on the theories of G.W.F. Hegel and Jacques Lacan. Although Popper's critiques of Hegelianism and psychoanalysis are notoriously controversial, I believe that he is correct when he says that Hegelian and Lacanian theory are probably not scientific in the sense of being falsifiable. They are not based on inductive reasoning from empirical evidence but on the retroactive deductive method known as the dialectic.


13 See Posner, Behavioral Economics, supra note 6, at 1,560.

buffer the hard core from attack. This is exactly what Posner does; he does not, however, follow through with the implications of this methodology. Mere abduction of auxiliary hypotheses is a necessary, but not a sufficient, methodology. The scientist must further test his auxiliary hypotheses.

I. FRIEDMAN'S CONTROVERSIAL ESSAY ON METHODOLOGY

This, O Best Beloved, is another story of the high and Far-Off Times.15

A. Controversy

Posner's methodology is based largely on Milton Friedman's famous, or infamous, essay The Methodology of Positive Economics.16 Many practitioners of "law and economics" have accepted Posner's claim that this is the methodology generally accepted by economists.17 While Friedman's essay has been highly

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15 KIPLING, supra note 1, at 93.
16 FRIEDMAN, supra note 12. While Posner does not always credit Friedman, his descriptions of "economic" methodology closely track the methodology described in Friedman's essay. See, e.g., RICHARD A. POSNER, ECONOMIC ANALYSIS OF LAW 18-19 (5th ed. 1998) [hereinafter POSNER, ECONOMIC ANALYSIS].


[O]n the rare occasions that practitioners of law and economics attempt to offer more than the most superficial reasons why economics has something to say to law, they tend to rely almost entirely on Milton Friedman's famous essay on the methodology of positive economics, without acknowledging that Friedman's account of economic methodology is highly contested among philosophers of economics.
influential, it is not, as Posner implies, generally accepted among economists. It lies, for example, at the heart of differences not only within neoclassical economics, such as that between Friedman and Paul Samuelson, but also those between the Chicago school of neoclassical economics associated with Friedman and the Carnegie school of behavioral economics founded by Herbert Simon.\textsuperscript{18}

In this Part, I show that on the one hand, certain parts of Friedman's essay can be interpreted to be consistent with standard accounts of falsification, albeit formulated in a highly idiosyncratic and potentially misleading way. On the other hand, Friedman's odd wording is also consistent with a radical nominalism that threatens to divorce theory from truth entirely. It is this latter interpretation that has been dubbed the $F$-twist, or the unreality principle,\textsuperscript{19} by Friedman's detractors.\textsuperscript{20} Even Blaug, who struggles mightily to reconcile Friedman's views with more mainstream accounts of scientific methodology, concludes that "one cannot help being struck by the lack of methodological sophistication . . . displayed" by Friedman and the debate he spawned.\textsuperscript{21} Posner concentrates on probably the most controversial part of Friedman's essay—his peculiar insistence on the unrealistic nature of the assumptions underlying scientific theory.\textsuperscript{22}

1. Instrumentalism

Friedman's methodology is supposed to combine falsification and instrumentalism.\textsuperscript{23} Instrumentalism is the position that a theory

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\textsuperscript{18} See Mark Blaug, The Methodology of Economics, or How Economists Explain (2d ed. 1992) for an excellent introduction to the controversy about economic methodology generally, including about Friedman's article specifically.

\textsuperscript{19} See discussion infra Part I.B.4.

\textsuperscript{20} Blaug suggests that, in context, it is not absolutely clear whether Friedman intended these consequences suggested by Friedman's idiosyncratic language. See infra notes 106-13 and accompanying text.

\textsuperscript{21} Blaug, supra note 18, at 104.

\textsuperscript{22} Posner does admit in at least one place that, although the assumptions underlying a theory must necessarily be unrealistic, it might also be possible for assumptions to be insufficiently realistic. See Posner, Economic Analysis, supra note 16, at 18.

\textsuperscript{23} Herbert Hovenkamp suggests, however, that instrumentalism may be, in fact, antithetical to strict falsification. According to Hovenkamp, the instrumentalist believes: Once someone has formulated a theory, it is not especially important to attempt to falsify it aggressively. Rather, one should simply consider how reliably the theory produces correct answers to a particular question that the researcher wishes to ask. For example, Newtonian physics is perfectly good for the engineer building bridges or the field commander shooting artillery, even though the theory has been falsified by experiments with relativity and might not be the best theory for someone planning an expedition to Mars. Herbert Hovenkamp, Positivism in Law & Economics, 78 Cal. L. Rev. 815, 832 (1990). I basically agree with Hovenkamp's statement but, as I discuss below, I believe that his
is purely a tool for a specific purpose. In Friedman and Posner’s case, theory is primarily a tool for prediction. It is frequently contrasted with “descriptivism”—the view that a scientific theory should accurately describe reality. Instrumentalism should also be contrasted with the use of theory to explain or understand a phenomenon. Because the standard of validity under instrumentalism is whether or not a theory serves its purpose, instrumentalism is agnostic with respect to the truth value of its own propositions.

Instrumentalism is consistent with, but does not require, a radical nominalism. Opinions among scientists as to the epistemological status of science vary widely. To mention two extreme positions, Stephen Hawking is a self-proclaimed “positivist who believes that physical theories are just mathematical models we construct, and that it is meaningless to ask if they correspond to reality, just whether they predict observations.” In contrast, Roger Penrose, Hawking’s frequent collaborator, is a self-described Neoplatonist who believes that mathematical theory is the true reality, which the object world only imperfectly reflects. As David Luban has argued before me, Posner seems to be internally inconsistent with respect to the truth claims of theory. On the one hand, as I shall explore in this Article, in his argument that prediction is the only criterion of a theory’s validity, Posner frequently suggests that the literal truth of a theory is either unknowable, irrelevant, or nonexistent. On the other hand, a complete dismissal of truth claims is hard to reconcile with the basic tenants of falsification, which by positing that hypotheses can be

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24 See Blaug, supra note 18, at 91. Posner does, from time to time, admit that the law has other purposes. For example, in Economic Analysis of Law, he mentions explanation and intervention in the world as plausible purposes. See Posner, Economic Analysis, supra note 16, at 18-19. In context, however, it seems that Posner’s conceptions of explanation and intervention are closely related, if not totally subsumed, into the broader concept of prediction. By explanation he seems to mean: if x occurs then y (i.e., we can predict y from x). Intervention seems to mean the recommendation that someone do x because if she does, then y will occur.

25 See Blaug, supra note 18, at 98-99; see also Posner, Behavioral Economics, supra note 6, at 1560 (distinguishing “between a description and a theory”).


proven to be false implies by negative pregnant that some form of truth is out there, even if we can only indirectly know it.

Karl Popper, probably the foremost proponent of falsification as scientific methodology, tries to sail a middle passage between nominalism and realism. Working within a tradition that reaches at least as far back as Kant, Popper argues that humans can never have direct knowledge of the object world (which he calls world 1), because experience is always mediated through thoughts and images. Nevertheless, humans are not limited to their own subjective interpretations (world 2). We can come to an intersubjective consensus (world 3) about the object world through the application of an agreed upon methodology. Moreover, Popper

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29 As I have elsewhere laboriously stretched a classical allusion with respect to Popper, “To navigate between the Scylla of individualistic solipsism and the Charybdis of false universals, objectivity is defined as the Odysseus of intersubjective agreement.” Jeanne L. Schroeder, Subject: Object, 47 U. MIAMI L. REV. 1, 17 (1992) [hereinafter Schroeder, Subject: Object].

30 The first world is “the world of physical objects or of physical states.” KARL R. POPPER, OBJECTIVE KNOWLEDGE: AN EVOLUTIONARY APPROACH 106 (1972) [hereinafter POPPER, OBJECTIVE KNOWLEDGE].

31 The second world is “the world of states of consciousness, or of mental states, or perhaps behavioural dispositions to act.” Id. at 106. Popper emphasizes the importance of the second world: “The first world and the third world cannot interact save through the intervention of the second world, the world of subjective or personal experiences.” Id. at 155.

32 See id. at 107. Popper states:

Most opponents of the thesis of an objective third world will ... usually say that all these entities [i.e., problems, conjectures, theories, arguments, journals, and books] are, essentially, symbolic or linguistic expressions of subjective mental states, or perhaps of behavioural dispositions to act; ... that is to say, symbolic or linguistic means to evoke in others similar mental states or behavioural dispositions to act.

Against this, I have often argued that one cannot relegate all these entities and their content to the second world.

Id. Popper believes, rather, in an “independent existence of the third world.” Id. He continues: “It seems to me most important to describe and explain the relationship of the three worlds in this way—that is, with the second world as the mediator between the first and third... [T]he mind may be linked with objects of both the first world and the third world.” Id. at 156.

33 Popper follows Kant:

The word “objective” [is used] to indicate that scientific knowledge should be justifiable, independently of anybody’s whim: a justification is “objective” if in principle it can be tested and understood by anybody. “If something is ‘valid’, [Kant] writes, “for anybody in possession of his reason, then its grounds are objective and sufficient.”

Now I hold that scientific theories are never fully justifiable or verifiable, but that they are nevertheless testable. I shall therefore say that the objectivity of scientific statements lies in the fact that they can be inter-subjectively tested.” KARL R. POPPER, THE LOGIC OF SCIENTIFIC DISCOVERY 44 (1968) [hereinafter POPPER, SCIENTIFIC DISCOVERY] (citation omitted); see also POPPER, OBJECTIVE KNOWLEDGE, supra note 30, at 106-08, 152-75; Jeanne L. Schroeder, Abduction from the Seraglio: Feminist Methodologies and the Logic of Imagination, 70 TEX. L. REV. 109, 161-64 (1991) [hereinafter Schroeder, Abduction]; Schroeder, Subject: Object, supra note 29, at 17-29. Although Popper ranted against Hegel in his book, The Open Society and Its Enemies, his theory of the three
claims that the methodology of falsification eliminates hypotheses that are inconsistent with world 1 (the object world). As a result, world 3 (our intersubjective consensus) will over time move further from world 2 (our subjective opinions) and closer to world 1 (the object world). Consequently, Jürgen Habermas has characterized the philosophy of science since Charles Sanders Peirce as being concerned with "a method of arriving at uncompelled and permanent consensus," as opposed to knowledge of existence per se. That is, in Popperian science "methodology replaces ontology and epistemology." I shall return to Popper in the last section of this Article, although a complete discussion of the metaphysical implications of economic theory is necessarily beyond the scope of this Article.

Both Posner and Friedman claim to adopt a definition of rationality as instrumental, or ends-means, reasoning. Such a definition of rationality is neither logically required by an instrumentalist methodology, nor is it inconsistent with a descriptivist one. Nevertheless, it is perhaps not surprising that the concept of rationality as instrumental thinking would appeal to the same type of people who are drawn to an instrumentalist methodology. Indeed, I suspect that by implicitly conflating the term of art "economic rationality" with the colloquial connotations of worlds resembles a simplistic version of Hegelian idealism. Notoriously, Popper seems never to have read Hegel but relied on a book of excerpts and inaccurate second-hand descriptions by critics of Hegel. See K.R. POPPER, THE OPEN SOCIETY AND ITS ENEMIES (1966).

See Schroeder, Abduction, supra note 33, at 162-64. Popper believes that the third world, although by necessity a human creation, is nevertheless objective and autonomous of human consciousness:

The world of language, of conjectures, theories, and arguments—in brief, the universe of objective knowledge—is one of the most important of these man-created yet at the same time largely autonomous, universes.

The autonomy of the third world, and the feed-back of the third world upon the second and even the first, are among the most important facts of the growth of knowledge.

With the evolution of the argumentative function of language, criticism becomes the main instrument of further growth.

Our work is fallible, like all human work. We constantly make mistakes, and there are objective standards of which we may fall short—standards of truth, of content, of validity, and other standards.

Scientists try to eliminate their false theories, they try to let them die in their stead.

POPPER, OBJECTIVE KNOWLEDGE, supra note 30, at 118, 119, 121, 122.

Jürgen Habermas, Knowledge and Human Interests 91 (Jeremy J. Shapiro trans. 1969).

Schroeder, Subject: Object, supra note 29, at 18.

I discuss this extensively in Schroeder, Economic Rationality, supra note 8.
rationality as "sane" or "reasonable"—i.e., rationality is posited not merely as a name for how people hypothetically do act, but as a prescription for or definition of how sane people should act—the proponents might be led to assume that instrumentalism is the only "reasonable" method. That is, if sane people act instrumentally, then theory is only meaningful if it can be used as an instrument. This is a non sequitur.

2. Description

According to Friedman, "[t]he ultimate goal of a positive science is the development of a 'theory' or 'hypothesis' that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed." A theory has two elements: "In part, it is a 'language' designed to promote 'systematic and organized methods of reasoning.' In part it is a body of substantive hypotheses designed to abstract essential features of complex reality." That is, the primary goal of science is prediction. Moreover, the only relevant test of a theory is falsification:

The hypothesis is rejected if its predictions are contradicted ("frequently" or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted; great confidence is attached to it if it has survived many opportunities for contradiction. Factual evidence can never "prove" a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexacty, that the hypothesis has been "confirmed" by experience.

Friedman concludes from this that it is not a criticism of a theory to say that its assumptions are unrealistic. This is so not only because the realism, or lack thereof, of assumptions is irrelevant to the validity of a theory. Rather, Friedman goes further and maintains that "[t]o be important ... a hypothesis must be descriptively false in its assumptions." In other words, Friedman can be interpreted to assert that falsity of assumptions is an affirmatively good thing.

As I shall discuss in greater detail below, Friedman's defenders suggest that, in the immediate context in which these statements are made, Friedman can be read as equating "realism" with a photographic reproduction of all concrete details of a phenomenon

38 See id.
39 FRIEDMAN, supra note 12, at 7.
40 Id. (citation omitted).
41 Id. at 9.
42 Friedman asserts that the "widely held view" that "the conformity of ... 'assumptions' to 'reality' is a test of the validity of the hypothesis" is "fundamentally wrong and productive of much mischief." Id. at 14.
43 Id.
and "unrealism" with abstraction. For example, at one point he states: "A hypothesis is important if it "explains" much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone."\textsuperscript{44}

Friedman also concludes that "the relevant question to ask about... 'assumptions'... [is] whether they are sufficiently good approximations for the purpose in hand"—which is good prediction.\textsuperscript{45} If all Friedman had intended to say—albeit in idiosyncratic and potentially misleading language—was that abstraction is a necessary part of any analytical process, then his essay would never have caused such a sensation.

The controversy arises because other statements made by Friedman suggest that he takes a giant step beyond this relatively uncontroversial view of abstraction and asserts that the assumptions behind a theory have no necessary relationship to "truth" per se. He does not merely identify realism with concreteness and unrealism with abstraction, he suggests further that he might accept as scientific a theory based on assumptions that are either demonstrably false or outright fantastic. That is, he sometimes seems to use the word "unreal" in the colloquial sense of "nonreal" or false. Consequently, Friedman suggests that neoclassical theory need not posit that economic subjects are actually economically rational, or even that they in fact tend to act as if they were economically rational, but that one can make good economic predictions if one assumes that economic subjects act as if they were economically rational, whether or not they actually are or so act.\textsuperscript{46} This interpretation is the F-twist.

3. Explanation

Despite Friedman's assertion, not all scientists, or all economists, accept the proposition that theory is primarily instrumental in nature. Although Samuelson, like Friedman, tries to avoid the inevitable epistemological and metaphysical implications of what it might mean to have knowledge of the object world, he does insist that the point of science is precisely to describe the universe.\textsuperscript{47} Samuelson is an unrepentant descriptivist who asserts that it "is fundamentally wrong [to think] that unrealism in the sense of factual inaccuracy even to a tolerable degree of approximation is

\textsuperscript{44} Id.
\textsuperscript{45} Id. at 15.
\textsuperscript{46} See infra text accompanying notes 154-58.
\textsuperscript{47} See infra text accompanying notes 74-85.
anything but a demerit for a theory or hypothesis...."48 Accordingly, he calls Friedman's methodology "a monstrous perversion of science."49

Samuelson may go too far: many philosophers of science find pure descriptivism to be as naive and as objectionable as pure instrumentalism. Indeed, descriptivism has been described as a "poor man's version of instrumentalism."50

Herbert Hovenkamp offers a more modest, but well-taken criticism of instrumentalism—at least as it is practiced in law and economics:

An important consequence of instrumentalism is to place strict limits on the domain of any particular hypothesis. A hypothesis that has survived testing in a particular context might fare poorly when used elsewhere. Statements within a given economic model are deemed "true" because that model yields better predictions with respect to a certain question than does some alternative model. When the question changes, the model must be reexamined by empirical testing and comparison with alternative hypotheses.51

By the very logic of instrumentalism, then, one cannot conclude from the observation that a set of "assumptions" results in accurate predictions in one context that the same set of "assumptions" will also be a good predictor in another context. This conclusion is, in fact, a new hypothesis that needs to be separately falsified—to accept it as something more is to confuse a new prediction with an assumption.

Although Simon, the father of behavioral economics, agrees with Friedman in the sense that he thinks that economic theory should serve an instrumental purpose, he sharply distinguishes his own methodology from Friedman's. Simon believes that good prediction only flows from the understanding that accurate description brings. That is, according to Simon, if "we want [an] economic theor[y]...to help guide the actual management and operation of firms,"52 then:

[I]t must be a theory that describes [the operations of firms] realistically, not an "as if" theory. In both its descriptive and its normative aspects, it must describe, and prescribe for, the decision making processes of managers with close attention to

49 Id.
50 BLAUG, supra note 18, at 99 (citations omitted).
51 Hovenkamp, supra note 23, at 827.
52 HERBERT A. SIMON, AN EMPIRICALLY BASED MICROECONOMICS 62 (1997) [hereinafter SIMON, MICROECONOMICS].
the kinds of knowledge that are attainable and the kinds of computations that can actually be carried out.\textsuperscript{53} As we shall see, despite some vociferous language, it is not clear whether Friedman—or Posner for that matter—is quite the pure instrumentalist and antidescriptivist he claims to be.

As I mentioned briefly in the introduction and develop at length elsewhere,\textsuperscript{54} whatever the goals of scientific theory, instrumentalism and prediction should not be the only test of legal analysis. Indeed, a fundamental difference in goals is one of the reasons why law-and-economics scholars and their critics consistently fail to communicate. Law and economics is a policy science—it seeks to give advice to legislators, judges, or others in power regarding what the law should be in order to achieve certain societal objectives. As such it must both identify what these goals should be (such as pareto optimality or wealth maximization) and predict how legal and economic subjects will respond to incentives or disincentives created by the law. In other words, the policy scientist seeks to use law to manipulate the behavior of those who are subject to its power. There is nothing wrong about this per se—it is the very nature of legal policy making.

The speculative theorist or critical legal scholar, in contrast, seeks not so much to give advice to the government, but to understand the position of the governed—those who are subjected to the law. The critical position is not that of the legislator or the judge, but of the attorney who counsels and represents clients. By understanding how the law affects her and her client, the lawyer and critical scholar can seek better to integrate the individual legal subject within the legal system—whether with other legal subjects through contract, or with the government through compliance. Through understanding, she seeks to free the legal subject from manipulation by the law. The legal subject who understands the law can now either freely submit to the law, seek to manipulate the law for her own individualistic and subjective goals, or seek to change the law. In the terms of Lacanian discourse theory, the critical scholar speaks a discourse of the Hysteric, whereas the legal economist speaks a discourse of the University.\textsuperscript{55} In the former, one stands in the position of the person subjected to the law who addresses the law itself. In the latter, one stands in the position of the legal expert who addresses the instrumental purpose of the law. As neither addresses the other directly, communication fails. Consequently, the critical scholar who says, in effect, “look what the

\textsuperscript{53} Id. at 63.

\textsuperscript{54} See sources cited supra note 10.

\textsuperscript{55} I explain this analysis in Schroeder, The Four Discourses, supra note 10.
law is doing to me” is never effectively heard by the policy scientist who can only respond “this is the purpose of the law.” For communication to occur, one of the two parties must step out of her own discourse and engage in a linking discourse. Lacan called this third discourse, the discourse of the Analyst.

There are many different types of “explanation” in science and elsewhere. Philosopher of science Ernest Nagel gives ten examples of different types of scientific explanation, which he then groups into four categories: (1) the deductive model; (2) probabilistic explanations; (3) functional or teleological explanations; and (4) genetic explanations. At first blush, the Friedman-Posner methodology would seem to fall within the first, deductive model, which is commonly used in the natural sciences. This type of explanation “has the formal structure of a deductive argument, in which the explicandum is a logically necessary consequence of the explanatory premises.”

Upon closer examination, however, it is apparent that Friedman and Posner’s view of science inverts the deductive explanation. A deductive explanation would posit that rational behavior by economic subjects would result in a downward-sloping demand curve; therefore, the reason the demand curve slopes downward is because economic subjects are rational. That is, economists would initially observe the surprising fact of downward-sloping demand curves and abduct the hypothesis that this could be explained if economic subjects behaved in a certain way (i.e., were economically “rational”). Economists would then test this hypothesis by structuring controlled observations of economic subjects to see if they in fact acted rationally. If they did not (i.e., if the hypothesis is not falsified) then one has reason to continue to accept the hypothesis.

In contrast, Friedman and Posner can be read as saying that, if we assume that economic subjects act “as if” they were rational, then we would predict that demand curves would slope downward. This hypothesis would be tested by engaging in controlled observation of demand curves to see if one could observe any that were upward sloping. Theirs is, however, a “theory” that in fact predicts nothing. It is merely a post hoc explanation of a phenomenon already observed, namely downward-sloping demand curves. Moreover, as

57 Id. at 21. Nagel points out that this “has been widely regarded as the paradigm for any ‘genuine’ explanation and, has often been adopted as the ideal form.” Id. Nevertheless, even in the natural sciences this ideal is often impossible in practice: “[F]ew if any experimental scientists today believe that their explicanda can be shown to be inherently necessary.” Id.
formulated by Friedman and Posner, this theory does not even suggest “why” demand curves slope downward because it does not purport to tell us how economic actors really think or act. As we shall see, it only suggests that downward-sloping demand curves are consistent with the hypothesis that economic actors act “as if” they were rational. But, as Gary Becker among others has suggested, downward-sloping demand curves are equally consistent with certain types of nonrational behavior. Consequently, Friedman and Posner’s methodology gives no way to choose between rival “explanations”—other than aesthetics. As I discuss below, although Friedman and Posner claim to be scientific, from the perspective of the Popperian school of falsification this may, in fact, be an unscientific way of moving from hypothesis formation to “proof.”

B. The Role of Assumptions and the Unreality Principle

1. Unrealistic Assumptions: Abstract or False?

Friedman (and Posner) claim both that theories are predictive, rather than descriptive, and that, therefore, theoretical assumptions are necessarily unrealistic. This claim, however, depends upon questionable definitions that conflate description with both realism and a perfect reproduction of all the details of an empirical phenomenon.

There is more than one way to interpret Friedman and Posner. When they say that theories are not accurate descriptions they might just be using very odd language to make the well-recognized distinction between the abstract and concrete. For example, when Posner speaks of the lack of realism of theories, he cites the failure of theories to “capture the full complexity, richness, and confusion of the phenomena” and refers to realism as “descriptive completeness.” As I have already mentioned in passing, if this were all that they meant, then Posner and Friedman would be relatively noncontroversial. I shall return to this.

Friedman and Posner would also be relatively noncontroversial if they were interpreted as merely describing Popperian

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58 See infra text accompanying notes 154-58.
59 See infra text accompanying notes 260-63.
60 See infra text accompanying notes 127-28.
61 See discussion infra Part II.C.
62 See supra text accompanying notes 39-43.
63 See discussion infra Part I.B.3.
64 POSNER, ECONOMIC ANALYSIS, supra note 16, at 18.
65 Id.
falsification, albeit in a particularly idiosyncratic vocabulary. Friedman’s assertion that the realism of assumptions is irrelevant and that theories are only tested by predictive power, could be merely another way of making the Popperian point that under falsification one does not test the theory directly through verification (i.e., in Friedman’s terms, by testing the “reality” of the assumptions). One can and should, however, test the theory indirectly or negatively by testing the implications of the theory (by trying to observe data inconsistent with the theory). In other words, verification is unacceptable, but falsification is acceptable.

As I discuss below, Friedman and Posner’s language, however, suggests that they go further and divorce theoretical assumptions from any truth claims whatsoever. That is, they might be taking the F-twist: equating “unreal” with “untrue” (or at least with the proposition that the literal truth of assumptions is irrelevant to the validity of a theory). In which case, Friedman and Posner seem to invite us to accept a theory as scientific, even when we know that it is false, merely because it is useful for some purposes.

For example, Blaug characterizes Friedman as asserting that “not only is it unnecessary for assumptions to be realistic, it is a positive advantage if they are not: ‘to be important... a hypothesis must be descriptively false in its assumptions.’” Similarly, Posner says that “lack of realism” is a “precondition of theory.” It is unclear precisely what they mean by this. One can argue that the logical implication of some of Friedman’s statements is that one can ignore the empirical truth value of scientific theories so long as one can use them as predictors. That is, Friedman seems to suggest that he would still adhere to a theory that is based on assumptions that have been shown to be false if it could be used as a predictor. This F-twist is different than the positivism of Hawking, which holds that it is meaningless to ask whether mathematical models correspond to some unknowable external reality, in that it suggests that we should retain a theory even when it can be shown that it does not in fact

67 See infra Part II.B (discussing Popperian falsification).
68 See infra Part I.C.3.
69 BLAUG, supra note 18, at 91 (quoting FRIEDMAN, supra note 12, at 14).
70 POSNER, ECONOMIC ANALYSIS, supra note 16, at 18.
71 In Amartya Sen’s words: “Milton Friedman... in particular, has argued powerfully in favour of departing from truth in describing reality in the context of economic models....” AMARTYA SEN, CHOICE, WELFARE AND MEASUREMENT 434 (1982).
72 See BLAUG, supra note 18, at 97 (“According to Samuelson, the F-twist comes in two versions:... [the] extreme version... ascribes positive merit to unrealistic assumptions on the grounds that a significant theory will always account for complex reality by something simpler than itself.”).
73 See supra note 26 and accompanying text.
match up with the empirical world. Let us stop and examine these arguments in more detail. I shall return to the F-twist below.

2. Descriptivism and Instrumentalism

Despite Friedman and Posner’s dismissal of descriptivism, and despite Samuelson’s agreement that his methodology of descriptivism is indeed distinguishable from instrumentalism, many economists have concluded that in practice there is no material difference between the two approaches. As Amartya Sen has pointed out, part of the reason that theorists like Friedman and Posner reject descriptivism is that they seem to have an extremely odd notion of what description is. They implicitly assume that description is a passive activity of observing and reporting everything that one sees, sparing no detail.\(^{74}\) For example, Friedman and Posner claim that a description of a phenomenon is a reproduction, not an explanation.\(^{75}\) This is, in fact, what Popper condemns as the naive bucket view of observation, which conceptualizes the observer as a passive receptacle of data.\(^{76}\)

In contrast, as Sen points out, a good description requires the “choice of a subset from a set of possible statements.”\(^{77}\) Some perfectly accurate statements about a phenomenon can be “unhelpful, even useless.”\(^{78}\) Consequently, the describer must first identify the purpose of the description. In Popper’s terminology, the scientific observer is a searchlight that actively singles out specific details for examination.\(^{79}\) In other words, observation and description are themselves forms of abstraction.

By asserting that the sole purpose of scientific theory is prediction, Friedman and Posner seem to ignore the possibility that many details that could be included in a description might be irrelevant or harmful for one purpose, although these details might serve some other valid scientific purpose: “Description may well be geared to some objective other than prediction, e.g., normative analysis, or efficient communication, or even satisfying idle curiosity.”\(^{80}\) In other words, one might describe the same

\(^{74}\) See SEN, supra note 71, at 433.

\(^{75}\) In Posner’s words: “A theory that sought faithfully to reproduce the complexity of the empirical world in its assumptions would not be a theory—an explanation—but a description.” POSNER, ECONOMIC ANALYSIS, supra note 16, at 18.

\(^{76}\) See infra note 210.

\(^{77}\) SEN, supra note 71, at 447.

\(^{78}\) Id. at 433. Sen gives the example of an expert answering a question about factory wages in India with the answer, “It varies from place to place.” Id.

\(^{79}\) See infra note 210.

\(^{80}\) NAGEL, supra note 56, at 439.
phenomenon differently depending on one’s purpose, and yet each
description might be “true” within the standards of its purpose.

Once one identifies a purpose, one then needs to choose what
aspects of the phenomenon to be described are relevant to that
purpose. For example, Friedman offers the specious argument that a
“realistic” description of a businessman would include his eye and
hair color and various other personal characteristics that a proper
economic model ignores.\textsuperscript{81} Sen replies:

In assessing this objection it is necessary to consider the
distinction between realism in the sense of “nothing but the
truth” and that in the sense of “the whole truth.” An
assumption can be realistic in that it is true without the claim
being made that it is exhaustive in capturing all aspects of the
reality. Advocates of realism in the sense of “nothing but the
truth” need not demand “the whole truth.”\textsuperscript{82}

A good explanation is one that first determines the purpose of the
description in the sense of what question is being asked; second,
determines what facts about the phenomena to be described are
relevant to that question; and third, abstracts these elements from
the concrete examples of the phenomena actually observed. This
type of description is arguably indistinguishable from falsification.
This seems to be the type of description that Samuelson has in mind.
As I discuss in Economic Rationality,\textsuperscript{83} Samuelson’s model of
economic rationality—revealed preference theory or “RPT”—is
perhaps even more abstract than Friedman’s in that it claims to
eliminate utility (and therefore ends-means reasoning) entirely.

As Sen says of both Friedman and Samuelson’s approach:
“[D]espite some predictive merits [it is] remarkably mute about
human joys and sufferings in which economists used to take a lot of
interest. The result is a descriptive impoverishment from many
perspectives, including—among others—normative relevance . . . .”\textsuperscript{84}
I would argue that this critique is even stronger with respect to
Posner than Friedman, since Posner purports to apply economic
analysis to a wide variety of subjects having great normative import,
such as human sexuality and family relations.

In Sen’s words:

Predicting future choice on the basis of past choice is not in
itself a bad predictive strategy (despite some well-known
problems). But if that is used as the only focus of the theory of
utility, then there is \textit{either} silence on many important issues

\textsuperscript{81} See \textit{Sen}, supra note 71, at 437-38.
\textsuperscript{82} \textit{Id.} at 438.
\textsuperscript{83} See Schroeder, Economic Rationality, supra note 8.
\textsuperscript{84} \textit{Sen}, supra note 71, at 442.
(when "utility" is treated as just another name for a numerical reflector of choice), or there is a good deal of senseless noise.\textsuperscript{85}

3. Abstraction

As I have suggested, one could argue that Friedman's realistic/unrealistic dichotomy might merely be intended to reflect the concrete/abstract distinction. Posner sometimes makes this identification expressly. For example, in a paragraph arguing for the unreality of theories he states that "abstraction is of the essence of scientific inquiry."\textsuperscript{86} In Blaug's words:

The assumptions of economic theory are sometimes said to be "unrealistic" in the sense of being abstract.... [T]his is certainly one of Friedman's meanings: "realistic" assumptions are descriptively accurate in the sense that they take account of all the relevant background variables and refuse to leave any of them out.\textsuperscript{87}

Martha Nussbaum makes a similar point: "Milton Friedman long ago correctly argued that positive economics, like other sciences, can and should use simple assumptions that do not in all respects correspond to the complex phenomenology of real human action."\textsuperscript{88}

It is a very different thing, however, to assert that an "abstract" description is less "realistic" than a "concrete" one. The very nature of abstraction arguably implies some type of truth-statement about the class of things being abstracted. That is, when one abstracts something, one is not asserting that one has identified all characteristics of the thing described, but that one has identified those shared characteristics of a class of objects that are relevant for the purposes at hand. Such description implicitly claims to be "real" in the sense that it identifies the true essence of a phenomenon by winnowing away extraneous detail.\textsuperscript{89} That is, the fact that models are not "the whole truth" does not necessarily mean that they are not "the truth... and nothing but the truth." Consequently, Nussbaum declares: "What is at issue is the question whether the assumptions

\textsuperscript{85} Id.
\textsuperscript{86} POSNER, ECONOMIC ANALYSIS, supra note 16, at 18.
\textsuperscript{87} BLAUG, supra note 18, at 92.
\textsuperscript{88} Martha C. Nussbaum, Flawed Foundations: The Philosophical Critique of (a Particular Type of) Economics, 64 U. CHI. L. REV. 1197, 1201 (1997).
\textsuperscript{89} As Gary Lawson says: "It may seem so obvious that a normative model cannot properly make use of incoherent or empty concepts. To evaluate actions by reference to an incoherent or nonexistent standard borders on a definition of psychosis." Gary Lawson, Efficiency and Individualism, 42 DUKE L.J. 53, 76 (1992). Nevertheless, Lawson suggests that one might carefully use empty concepts for limited purposes. For example, one might use a model as a proxy for a true moral theory, if one thought that the model led to relatively accurate predictions and was easier to apply. See id. at 76-77. Lawson generously interprets Posner as taking this relatively modest approach. See id. at 77.
are too crude, so oversimple that they fail to single out those aspects of the world that are most salient for predictive purposes."\(^{90}\)

Although Nagel does not refer to Friedman directly, he seems to have Friedman implicitly in mind in his discussion of certain methodological problems encountered in economics. One difference between practitioners of the "natural" sciences and most social scientists is that the former try to develop universal laws, while the latter tend to make statistical generalizations.\(^{91}\) Some social scientists, most notably economists,\(^{92}\) have sought to develop a methodology to make their fields more like the natural sciences. This requires an understanding of the nature of scientific laws—a goal that the Friedman article approaches but does not attain.

In Nagel's words:

> [T]he terms used in the universal laws of many branches of science usually have a quite precise connotation, and frequently signify traits that are more or less "idealized" versions of actually observed properties. Each such term is in consequence intended to designate some class of items that are highly homogeneous in certain indicated respects; and a law containing such terms is neither expected to be, nor is it actually, in strict agreement with observed data...\(^{93}\)

That is, although scientific laws require simplified, abstracted concepts, these concepts are nevertheless intended to be "true," at least in the sense of accurate, in that they capture certain shared characteristics of the class of things being studied. The scientist simplifies and idealizes in order to identify the essential aspects of the class of things that is relevant to the task at hand: "[U]niversal laws formulated in terms of distinctions more subtle than is necessary for achieving the objectives of empirical research, may be just so much dead lumber. A high-powered microscope is not an improvement on a simple magnifying glass as an instrument for reading small print."\(^{94}\)

Consequently, when neoclassical economics starts from an assumption that economic actors are "rational," this does not necessarily imply that humans are economically rational at all times, or in all circumstances. It could merely mean that economic rationality is one "true" moment that can be abstracted from human nature. There could be other equally "true" moments. To suggest that there might be an economically irrational as well as an economically rational moment does not necessarily imply that

\(^{90}\) Nussbaum, supra note 88, at 1201.

\(^{91}\) See NAGEL, supra note 56, at 503-05.

\(^{92}\) See id. at 508-09.

\(^{93}\) Id. at 505.

\(^{94}\) Id. at 508.
human behavior is unpredictable, as Posner assumes.\textsuperscript{95} It might be possible to develop a predictive theory of when one aspect of human nature might be expected to prevail and when other aspects might. This is, in fact, the approach of many philosophical systems, most notably Hegelian philosophy and Lacanian psychoanalysis, which I discuss at length elsewhere.\textsuperscript{96}

If one were to take such an approach, then Friedman would be correct in saying that testing for market behavior predicted by the assumption of economic rationality may not necessarily have any implications for the truth of the assumption that human beings are essentially economically rational for all purposes. Indeed, despite Posner’s assertion of the possible unreality of the rationality assumption, the very thing many of Posner’s critics (such as myself) find most objectionable about his theory is precisely that he implicitly concludes from evidence that the rationality postulate is an accurate predictor of some market behavior, that the rationality postulate is a valid description of human nature and a predictor of behavior in other areas. Probably the most notorious example of this is Posner’s analysis of human sexuality. In Nussbaum’s words: “Posner’s descriptions of human sexuality, in Sex and Reason, do not convey the sense that we are looking at sex the way people generally look at it; instead, a perspective of lofty detachment has flattened and simplified things that are usually messy and real.”\textsuperscript{97} As I shall discuss,\textsuperscript{98} despite his claims, Posner usually uses his theory to explain already observed data, rather than predicting unobserved data. Moreover, when he does make predictions he engages in the easy but unreliable methodology of verification—predicting data that would be consistent with his hypothesis—rather than the more difficult and reliable one of falsification—predicting data that would be inconsistent with his hypothesis. In any event, despite his call for lawyers and judges to make empirically based decisions,\textsuperscript{99} Posner does not in fact do the observational work that would either verify or falsify his hypotheses.

The problem of overbreadth that I identify in Posner is precisely one of the dangers that concerned Friedman. Nevertheless,

\begin{itemize}
\item \textsuperscript{95} See Posner, Behavioral Economics, supra note 6, at 1559 (“The rational-choice economist asks what ‘rational man’ would do in a given situation, and usually the answer is pretty clear. . . But it is profoundly unclear what ‘behavioral man’ would do in any given situation. . . He might do anything.”); id. at 1575 (“Behavioral man behaves in unpredictable ways.”).
\item \textsuperscript{96} See Schroeder, Economic Rationality, supra note 8; Schroeder, Fear of Freedom, supra note 9.
\item \textsuperscript{97} Nussbaum, supra note 88, at 1200.
\item \textsuperscript{98} See infra notes 273-89 and accompanying text.
\item \textsuperscript{99} See, e.g., POSNER, OVERCOMING LAW, supra note 5, at 5-8.
\end{itemize}
although Friedman resists, one could certainly conclude that, despite
the fact that the predictions of the rationality postulate have not
been falsified in certain specific circumstances, there exists at least
some evidence that the rationality postulate has something “true” to
say about actual human behavior in these circumstances. In Nagel’s
words: “[I]t is possible in many branches of natural science to state
laws as universally valid under certain ‘ideal’ conditions and for
‘pure cases’ of the phenomena investigated . . . .”

What critics such as Samuelson and myself object to in
Friedman and Posner is that they arguably leap from an appropriate
rejection of the standard of “the whole truth,” to a rejection of
“nothing but the truth,” to a rejection of realism entirely, which
comes very close to holding not merely that truth is not a necessary
condition of a scientific theory, but that fiction has a positive value in
a theory.

That is, Friedman argues not only that accuracy of prediction is
the only test of the validity of a hypothesis, but also that the
conformity of the underlying “assumptions” of the hypothesis to
reality is not an additional test of the hypothesis. Friedman’s critics
have accused him of asserting that the validity of a theory has
nothing to do with the validity of the underlying assumptions (so that
one cannot prove a theory valid by proving the assumptions, and the
fact that a theory is a good predictor does not imply that its
underlying assumptions are true). It is now time to turn to the
interpretation of Friedman’s theory that Samuelson calls the “F-
twist,” and Simon calls the “unreality principle.”

4. Defending Friedman Against the F-twist Accusation

What does Posner mean when he states that the validity of
economic theory is not dependent on the accuracy of its assumptions
as to the rationality of economic actors? Is Posner merely repeating
Friedman, or is he misinterpreting him? Blaug shows that if one
carefully parses Friedman’s language, one can make an argument
that Friedman never quite adopts the $F$-twist (i.e., the proposition that a theory can be valid even if its assumptions are patently false). If Friedman does not adopt the $F$-twist, however, it is far from clear what he is in fact saying. As we shall see, regardless of what Friedman had in mind, Posner accepts the $F$-twist and gives it another turn, arriving at his own $P$-twist.

Blaug tries to partially defend Friedman on the grounds of historical context. If Friedman used extreme language in defending the rationality postulate, this was only because he was responding to a methodological argument current at the time he was writing that was itself worded in absolutist, intemperate language. Early- to mid-twentieth-century critics of neoclassical economics concentrated on attacking the strict accuracy of its underlying assumptions while ignoring its predictive power. That is, they attacked the abstraction of assumptions and demanded photographic concreteness. It is hardly surprising, therefore, that defenders of neoclassical economics would try to draw attention away from the empirical accuracy of the model and toward its usefulness. Unfortunately, the extremist rhetoric of the critics inspired an extremist response:

The style of this criticism, which was invariably accompanied by the crudest of objections to the assumptions of standard theory, paying absolutely no attention to its predictive content, inevitably produced the reaction among defenders of received doctrine that “assumptions are largely irrelevant.” ... Taken with the accusation that no theory with counterfactual assumptions can be taken seriously, the thesis of the irrelevance of assumptions is almost excusable.\(^{104}\)

In other words, Friedman was responding to critics that demanded that a theory be “the whole truth,” in the sense of being a totally accurate description of a phenomenon in all respects—surely an impossibly strict standard that no scientific theory could ever meet.

What then, does Friedman mean when he suggests that the realism of assumptions is irrelevant, or that unrealism of assumptions may be a virtue? First, as Blaug points out, Friedman never quite makes the blunt statement of which Samuelson accuses him: that he is denying the necessity of truth claims in scientific theories. Friedman always modifies his position by saying that the realism of assumptions is “largely” irrelevant.\(^{105}\) Indeed, interpreting Friedman is difficult precisely because he tends to put the word “assumptions” in quotation marks and never defines his terms.\(^{106}\)

\(^{104}\) Id.

\(^{105}\) See BLAUG, supra note 18, at 97.

\(^{106}\) See id. at 94.
Friedman does not even explicitly distinguish between initial conditions, auxiliary hypotheses, and boundary conditions. Assumptions in economics may refer to (1) statements of motivation such as utility and profit maximization; (2) statements of overt behavior of economic agents; (3) statements of the existence and stability of certain functional relationships; (4) restrictions on the range of variables to be taken into account; and (5) boundary conditions under which the theory is held to apply.

Blaug takes Friedman to task for his naive "notion that theories can be neatly divided into their essential components and that the empirical searchlight is to be directed solely at the implications and never at any other parts of the theory...." Indeed, Friedman does argue, in what Blaug describes as "a frequently overlooked section" of his essay, that assumptions can be used as an indirect test of a theory. Friedman's plaint against testing assumptions might be just another form of denying the validity of verification as a methodology. That is, Friedman might be using the term "assumptions" to mean affirmative statements of presumed grounding facts on which a theory is based. To test an assumption might, therefore, be considered an attempt to verify these facts.

Thus we can paraphrase Blaug's attempt to give Friedman the benefit of the doubt and rewrite his reliance on the dubious distinction between the assumptions and implications of a theory as follows. First, when examining a theory, it is not useful to distinguish between what is an assumption and what is a conclusion, let alone between types of assumptions. Consequently, a theory should be tested as a whole, not with respect to its component parts. This is so partly because, in order to theorize, one needs to abstract from concrete, empirical reality. Abstraction is the identification of salient points for a specific purpose, i.e., for use as part of a theory. The test of the appropriateness of an abstraction, therefore, should be a test of how it works in the context of the theory. Friedman believes that the use of economic theory should be prediction of economic behavior. This means that the test of a theory is whether all of its component parts, taken as a whole, serve this purpose (i.e., whether the theory makes accurate predictions).

Second, when Friedman asserts that assumptions may, or must, be unrealistic (or that lack of realism is a virtue in theorization), he is not necessarily suggesting that economists should go out and seek to make their assumptions unrealistic.

107 Id.
108 Id. at 104.
109 Id. at 93.
110 See FRIEDMAN, supra note 12, at 26-30.
Indeed, his description of theory formation has much in common with Popperian methodology. That is, one does not draw one’s starting assumptions out of thin air. One starts with empirical data as the basis of the initial hypothesis—one engages in abduction in order to try to explain a surprising thing. According to Friedman:

Empirical evidence is vital at two different, though closely related, stages: in constructing hypotheses and in testing their validity. Full and comprehensive evidence of the phenomena to be generalized or “explained” by a hypothesis, besides its obvious value in suggesting new hypotheses, is needed to assure that a hypothesis explains what it sets out to explain—that its implications for such phenomena are not contradicted in advance by experience that has already been observed.\(^\text{111}\)

In other words, one’s working assumptions are grounded in empirical observation. In this context, what Friedman seems to be suggesting is that one does not then test the hypothesis drawn from these initial assumptions by seeking to verify the assumptions themselves, but by seeking to falsify the implications drawn from these assumptions.\(^\text{112}\)

The problem with these generous interpretations of Friedman is that they fly in the face of other statements made in his essay. As Blaug points out, Friedman suggests that assumptions underlying theories might be unrealistic in two ways other than abstraction. An assumption might be unrealistic because it does not “ascribe motives to economic actors that we, fellow human beings, find comprehensible.”\(^\text{113}\) That is, as I shall discuss below, Friedman rejects what is known as the Verstehen doctrine, or “methodological individualism.”\(^\text{114}\) Second, assumptions may be unrealistic in the sense that they “are believed to be either false or highly improbable in light of directly perceived evidence . . . .”\(^\text{115}\)

As we shall see,\(^\text{116}\) in at least one example given in his essay, Friedman proposes assumptions that are not merely abstract or partially true, but are completely and irrefutably false. Moreover, Friedman’s methodology should be distinguished at this point from Popper’s. In contrast, Popper only suggested that the initial working assumptions of a proposed hypothesis were “irrelevant” to the scientific process; he did not suggest that the truth or reality of a theory’s ultimate assumptions was irrelevant.\(^\text{117}\) This is because

\(^{111}\) Id. at 12.

\(^{112}\) See id. at 13-14.

\(^{113}\) BLAUG, supra note 18, at 92.

\(^{114}\) See discussion infra Part I.C.4.

\(^{115}\) BLAUG, supra note 18, at 93.

\(^{116}\) See infra notes 174-80 and accompanying text.

\(^{117}\) See infra notes 209-27 and accompanying text.
falsification allows a scientist to eliminate serially all false assumptions. Consequently, if scientists are unable to falsify a theory after repeated experimentation, then one has reason to believe that any assumptions that remain standing after falsification are true. In other words, we can safely ignore the question of the validity of our initial assumptions only if, like Popper, we are confident that all false assumptions can eventually be eliminated through falsification.

In other words, Popperian falsification is a methodology designed precisely to determine the truth or falsity of assumptions. Perhaps the most generous reading of Friedman’s approach is that Friedman retained a healthy skepticism as to Popper’s epistemology and continued to question the assertion that man can achieve metaphysical knowledge through science. If one accepts Popper’s assertion that all scientific theories are fallible and corrigible, then one can never get beyond mere intersubjective consensus to “objective” truth.118

5. The Practicability of Falsifying Assumptions

As we have seen, it is not Friedman’s preference for falsification that is remarkable (since this is a widely, but far from universally, accepted notion of scientific methodology). Rather, it is his suggestion that one cannot also directly test a theory by falsifying (as opposed to verifying) its assumptions, or his implication that the truth of a theory has no relationship to the truth of the assumption. Part of Friedman’s argument seems implicitly to be based on the assertion that we cannot directly test the assumptions of the rationality postulate because it is difficult, if not impossible, to test assertions about the subjective mind-sets of economic actors. Ironically, as I discuss in Economic Rationality, although Samuelson considers himself a critic of Friedman’s methodology, Samuelson’s own theory of revealed preferences (“RPT”) is similarly founded on the alleged problematics of directly observing the subjective preferences that constitute the base assumptions of the rationality postulate.119 But if Friedman’s claim is merely based on an assertion of empirical difficulty, then he should be open to suggestions about how these difficulties might be overcome.120 Indeed, his critics point

118 Consequently, in Popper’s words: “The game of science is, in principle, without end.” POPPER, SCIENTIFIC DISCOVERY, supra note 33, at 53.
119 See Schroeder, Economic Rationality, supra note 8.
120 See BLAUG, supra note 18, at 96. Sen makes a similar point in his critique of the methodology of Friedman’s arch-rival, Paul Samuelson. Samuelson’s theory of revealed preferences is based on the implicit, but untested, assumption that there is no reliable means of testing people’s actual preferences and subjective states. As Sen suggests:
out that the testing of predictions suffers from precisely the same type of empirical difficulties and ambiguities as the testing of assumptions: "[D]irect evidence about assumptions is not necessarily more difficult to obtain than data about market behavior used to test predictions or, rather, . . . the results of examining assumptions are not any more ambiguous than the results of testing predictions . . . ."\textsuperscript{121}

Moreover, his critics assert, if Friedman truly believes that "accurate predictions are . . . the only relevant test of the validity of a theory[,] . . . [then] it would be impossible to distinguish between genuine and spurious correlations . . . ."\textsuperscript{122} Friedman anticipates this criticism and attempts to preempt it with a version of Ockham's razor: "The choice among alternate hypotheses equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant considerations are suggested by the criteria 'simplicity' and 'fruitfulness,' themselves notions that defy completely objective specification."\textsuperscript{123} Elegance and beauty are, indeed, characteristics of many successful scientific theories. Moreover, even arch-falsifier Popper agreed that there is a necessary aesthetic or, in his words, "subjective"\textsuperscript{124} aspect in the choice of one's initial hypothesis. Popper, however, sought to distinguish the subjective moment in all human activity from the objective one of scientific methodology.\textsuperscript{125} As we shall see, Popper argued that his proposed method of falsification was precisely a method of testing one's initial hypothesis in order objectively and scientifically to choose one's final hypothesis.\textsuperscript{126} Ironically, Friedman and Posner wind up promoting not falsification, as they claim, but the theory proposed by falsification's most well-known critic, Thomas Kuhn. Kuhn famously argued that there is no logical way to

\textsuperscript{121} BLAUG, supra note 18, at 96.

\textsuperscript{122} Id.

\textsuperscript{123} FRIEDMAN, supra note 12, at 10.

\textsuperscript{124} POPPER, SCIENTIFIC DISCOVERY, supra note 33, at 31.

\textsuperscript{125} Popper characterized his project as that of "demarcation"—distinguishing what he seemed to consider the uniquely reliable process called "science" from other useful processes such as "common sense." See id. at 19-20, 34.

\textsuperscript{126} See infra text accompanying notes 209-18.
choose between rival paradigms so one must inevitably fall back on
“good,” but nonobjective, reasons like “accuracy, scope, simplicity,
fruitfulness, and the like.” 127 Similarly, Friedman concludes that the
choice between hypotheses is “to some extent arbitrary” but that
“relevant considerations” include “simplicity” and “fruitfulness.” 128

In addition to questioning the empirical difficulty of testing
assumptions, and raising the false correlation problem, Friedman’s
critics have argued:

[T]he attempt to test assumptions may yield important insights
that help us to interpret the results of predictive tests; and . . . if
predictive testing of theories with patently counterfactual
assumptions is indeed all that we can hope for, we ought to
demand that our theories be put to extremely severe tests. 129

C. Arguments Drawn from the “Hard” Sciences

1. Misdescription of Galileo

Friedman tries to defend his assertion as to the irrelevance of
the realism of assumptions by reference to physics. An examination
of his example shows that Friedman’s description is either worded in
a misleading way or evidences a fundamental confusion about the
laws of physics (and, once again, about the concepts of “partial” and
“abstract” on the one hand and “unrealistic” or “nontrue” on the
other). Accordingly, Friedman’s description of the laws of physics
has rightfully become a lightning rod for attacks by his critics.

Friedman notoriously gives the example of the laws of falling
bodies as first proposed by Galileo and developed by Newton. 130
Posner repeats the same example, without attribution. 131 Friedman
states that the law is unrealistic because it “assumes” that bodies fall
in vacuums. 132 “Testing this hypothesis by its assumption presumably
means measuring the actual air pressure and deciding whether it is

127 Thomas S. Kuhn, Reflections on My Critics, in CRITICISM AND THE GROWTH OF
KNOWLEDGE 231, 261 (Imre Lakatos & Alan Musgrave eds., 1970) [hereinafter Kuhn,
Reflections].
128 FRIEDMAN, supra note 12, at 10.
129 BLAUG, supra note 18, at 96.
130 For simplicity, I will follow Friedman in writing as though Newton’s laws of motion
were still the accepted scientific paradigm. It is common knowledge that Newtonian physics
has been supplanted by Einsteinian physics and quantum mechanics. It is not the case that
Newtonian physics is still theoretically true for the macroworld, but Einsteinian physics rules
the microworld. Nevertheless, Newtonian physics approximates the movement of bodies in
the macroworld with sufficient accuracy to serve for law professors to ignore Friedman’s
inaccuracy.
131 See POSNER, ECONOMIC ANALYSIS, supra note 16, at 18.
132 See FRIEDMAN, supra note 12, at 18.
close enough to zero."\textsuperscript{133} It is common knowledge that cannonballs and feathers fall at different speeds in the atmosphere.\textsuperscript{134} Friedman concludes that scientists accept the law of falling objects only with respect to the objects that fall in an atmosphere in a way that is sufficiently close to that predicted in a vacuum to say that the theory "works" for those objects, but not in those circumstances when it doesn't "work."\textsuperscript{135} Consequently, according to Friedman, we accept the theory as "working" for things like cannonballs, but reject it for things like feathers.\textsuperscript{136}

Friedman concludes from this that scientists accept the law of falling bodies for cannonballs despite its "unrealistic" assumption that cannonballs fall in vacuums. Moreover, he asserts: "This example illustrates both the impossibility of testing a theory by its assumptions and also the ambiguity of the concept 'the assumptions of a theory.'"\textsuperscript{137} He rewrites the theory of falling bodies to mean: "[U]nder a wide range of circumstances, bodies that fall in the actual atmosphere behave as if they were falling in a vacuum."\textsuperscript{138}

Before proceeding, we should note that, even though Friedman claims that the assumption that objects fall in a vacuum is "unrealistic," the theory of falling bodies purports to say something about how objects do fall in a vacuum that is "true" in the limited sense that it describes observed motion. Moreover, although it might be correct to suggest that Galileo, Newton, et al. abducted the theory of falling bodies by abstracting from empirical observation of actual bodies falling in the earth’s atmosphere, it is not correct to say that the only way scientists test this theory is by further observation.

\textsuperscript{133} Id. at 16. Posner similarly writes: "Newton's law of falling bodies is unrealistic in its basic assumption that bodies fall in a vacuum . . . ." POSNER, ECONOMIC ANALYSIS, supra note 16, at 18.

\textsuperscript{134} Probably all of us have heard at some point the anecdote that Galileo instituted the era of modern scientific experimentation, which supplanted the earlier Aristotelian speculative science, by demonstrating that objects of different weight fall at the same speed by dropping two objects from the Leaning Tower of Pisa and observing that both hit the ground at the same time. A moment's thought will reveal that this must be a myth because in the earth's atmosphere objects of different weights or sizes usually fall at different speeds. Galileo's new methodology consists not merely of empirical observation, but also of the ability of abstracting generally applicable rules from diverse empirical data.

\textsuperscript{135} FRIEDMAN, supra note 12, at 17. Posner says the law of falling bodies "is still a useful theory because it predicts with reasonable accuracy the behavior of a wide variety of falling bodies in the real world." POSNER, ECONOMIC ANALYSIS, supra note 16, at 18. Friedman does correctly note that one needs to decide what it means to say that something works—that is, what standard to apply. Presumably, given that Friedman's goal is predictability, the answer to this question would be based on what one wishes to predict, and the degree of accuracy one is seeking. A thorough discussion of this topic is beyond the scope of this Article.

\textsuperscript{136} FRIEDMAN, supra note 12, at 17.

\textsuperscript{137} Id. at 17-18.

\textsuperscript{138} Id. at 18.
of objects in the earth’s atmosphere. Nor is it correct to say that scientists who theorize about movement—or even engineers who use this theory for practical purposes—accept the theory for objects for which this result can be replicated and reject it for other objects. Rather, scientists try to replicate in reality the conditions of the theory in its abstract state to see if the theory holds—i.e., they, in fact, test the assumptions. For example, I remember seeing a science film in elementary school in which the scientist (actor) first dropped two objects in a glass jar containing air—demonstrating how differently they fell in an atmosphere—and then pumped the air out of the jar and repeated the experiment. As predicted, the two objects fell at the same speed and hit the ground at the same time. In other words, the theory was shown to be an accurate predictor of movement of both weights and feathers in a vacuum.

Simon aptly criticizes this aspect of Friedman’s theory:

I am not satisfied with the answers to Friedman’s argument that he has as much right as the physicists to make unreal assumptions. Was Galileo also guilty of using the invalid principle of unreality? I think not. I think he was interested in behaviour in perfect vacuums not because there aren’t any in the real world, but because the real world sometimes sufficiently approximates them to make their postulation interesting.\(^{139}\)

Friedman was writing in the mid-1950s when, perhaps, the thought of objects moving in vacuums seemed like a strange starting place for theorization of the laws of motion. However, from the point of view of astrophysics, hypothesizing about how planets, satellites, and other vehicles move in the relative vacuum of space seems completely appropriate. Indeed, I suspect that from the vantage point of the turn of the millennium, after decades of watching space flights on television, the proposition no longer seems strange, let alone unrealistic, to the average American.

In his book on the nature of scientific explanation, Nagel uses the same example to describe classic scientific methodology in a way that is subtly, but decisively, different from Friedman. “We begin by noting the familiar fact that the experimental evidence for the universal laws of physical science is rarely if ever in perfect agreement with them.”\(^{140}\) This certainly is one of the things that Friedman is trying to capture when he says that science does not merely describe phenomena in the sense of ticking off all empirical data. In contrast to Friedman, however, Nagel interprets scientists as very much trying to describe the phenomena to be studied as

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139 SIMON, MODELS, supra note 103, at 370.
140 NAGEL, supra note 56, at 508.
accurately as practicable. In Simon’s words: “[U]nreality of premises
is not a virtue in scientific theory it is a necessary evil—a concession
to the finite computing capacity of the scientist that is made tolerable
by the principle of continuity of approximation.”¹⁴¹ That is, the
scientist understands that a meaningful scientific description is
limited to those aspects of the phenomena germane to the problem
at hand. Indeed, science may be seen as the very act of such
limitation. Nagel continues:

Accordingly, if physicists were to formulate their laws in strict
adherence to what observation establishes about physical
phenomena, those laws would have a statistical rather than a
universal form. For example, had Galileo sought to establish
the laws for freely falling bodies simply by correlating observed
data, he would certainly have found that the velocity of falling
bodies varies with their weight and shape; and he would have
also found that there is only a high correlation rather than an
invariable proportionality between the distances bodies fall and
the squares of the lapsed times of their fall, so that a
generalization based entirely on these findings would have been
statistical in form.¹⁴²

Friedman, therefore, is mistaken when he asserts that a more
complete theory of falling objects has been developed “largely as a
result of attempts to explain the errors of the simple theory.”¹⁴³ As
the passage from Nagel makes clear, the theory that objects fall a
certain way in a vacuum is not “erroneous” because objects on earth
do not usually fall in vacuums. Rather, the theory is “true” precisely
because the necessary corollary to the theory that objects fall a
certain way in vacuums is that they should be expected to fall
differently in different circumstances that deviate from the ideal and
that the deviations should be measurable and predictable. As Simon
states:

Whatever our admiration for Galileo’s law describing the fall of
a body in a vacuum, we do not use it to predict the movement
of parachutes or of meteors in the earth’s atmosphere. If we
wish to test the law, we test it in vacuo or a reasonable
approximation thereto—that is, under conditions where the
assumptions are nearly true, not under conditions where they
are egregiously false. When we apply the theory to real-world
problems, we supply such elaborations, in the form of terms to
take account of air resistance, or friction, or whatever the
sources of complication may be, as may be necessary to fit the
theory to the actual conditions of application. In imagining that

¹⁴¹ SIMON, MODELS, supra note 103, at 371.
¹⁴² NAGEL, supra note 56, at 508.
¹⁴³ FRIEDMAN, supra note 12, at 18 (emphasis added).
theories are used in their simplest idealized form, ignoring the real-world complications, Friedman has drawn a fictitious picture of how theories are actually employed in physical science and engineering and has given bad advice as to how they should be employed in economics.\textsuperscript{144}

Even under the methodology of sophisticated falsification (which I discuss below),\textsuperscript{145} if we assume that Galileo's original formulation of his hard-core hypothesis was merely that objects fall a certain way (i.e., under any circumstances, without limitation to vacuums), the hard core of this theory would not be deemed "erroneous" because we observe that actual objects often fall at a slower speed. Nor is the hard core necessarily falsified by this observation. The differential of the motion of objects as observed and as predicted is, at most, an empirical anomaly that seems inconsistent with the base or hard-core theory of falling bodies. As I discuss below,\textsuperscript{146} pursuant to a falsification methodology, it is the job of the scientist at this point neither to correct "errors" in the theory nor to reject the theory. Rather, the falsifier now tries to abduct auxiliaries to the hard core of the theory that might explain these apparent anomalies. The scientist then develops a research program designed to test the auxiliaries by seeking to observe empirical data inconsistent with them.

As I have already quoted, Nagel states that the methodology of many natural sciences is to develop "laws as universally valid under certain 'ideal' conditions... and to account systematically for any discrepancies between what the laws assert and what observation reveals in terms of more or less well-authenticated discrepancies between those ideal conditions and the actual ones under which observations are made."\textsuperscript{147} In other words, a scientific theory does not ignore empirical reality and make unrealistic "assumptions," nor does it merely exclude empirical phenomena that do not approximate the predictions of the theory, as Friedman suggests. Rather, although a theory in form may apply only to "pure cases" or idealized versions of the phenomena to be studied, it also in practice accounts for empirical deviations. Scientists do not, as Friedman comes close to saying, develop one hard-core theory of objects in a vacuum that applies to objects like cannonballs, and other theories to correct the vacuum "error" that apply to objects like feathers. They, instead, develop an integrated theory that accounts for the

\textsuperscript{144} SIMON, MODELS, supra note 103, at xix.
\textsuperscript{145} See infra text accompanying notes 251-56.
\textsuperscript{146} See infra text accompanying notes 251-56.
\textsuperscript{147} NAGEL, supra note 56, at 508.
movement of all objects given atmospheric pressure and other forms of friction.

At this stage in the argument, Friedman makes another good point, although once again worded in misleading and idiosyncratic terminology. He states:

[T]he difference in shape of the body can be said to make 15 pounds per square inch significantly different from zero for a feather but not for a compact ball dropped a moderate distance. Such a statement must, however, be sharply distinguished from the very different statement that the theory does not work for a feather because its assumptions are false. The relevant relation runs the other way: the assumptions are false for the feather because the theory does not work. This point needs emphasis, because the entirely valid use of “assumptions” in specifying the circumstances for which a theory holds is frequently, and erroneously, interpreted to mean that the assumptions can be used to determine the circumstances for which a theory holds, and has, in this way, been an important source of the belief that a theory can be tested by its assumptions.148

Note, we have just seen that Friedman’s statement that the assumptions of the law of falling bodies are false for a feather is incorrect, because the theory does not posit that objects fall at a certain speed under all circumstances. But, as I have argued, the theory holds that objects fall at a certain speed under ideal circumstances and therefore fall at different speed if actual conditions deviate from the ideal. Friedman’s misleading terminology unfortunately disguises the very good point in this passage. A good theory specifies as part of the theory the conditions under which it is valid. A theory of the movement of falling objects must specify the conditions that affect its movement. The first law of movement specifies that it is universally valid and that it can be directly observed when there are no countervailing forces affecting the movement of an object. Because a vacuum lacks the conditions of friction caused by an atmosphere, good predictions of how objects fall in a vacuum can be based solely on the first law of movement. This does not mean, as Friedman implies, that the law of motion does not “really” apply to objects in an atmosphere, but that in order to predict how empirical objects will actually move in a specific atmosphere we must also consider other factors that affect the movement of objects.

As Nagel notes, scientific theory does not merely note (as Friedman and Posner do) that there is a discrepancy between concrete, empirical actuality and the abstract, theoretical potentiality

148 FRIEDMAN, supra note 12, at 19.
of a hypothesized ideal. Rather, it seeks "to account systematically for any discrepancies" between the actual and the ideal. As a daughter of an aerospace engineer who designed guidance systems intended to deliver missiles launched from the United States to targets in the Soviet Union and China, I know that scientists and engineers who applied their theories believed that they knew how to add the real-world facts of friction, etc., to the abstractions of Newtonian theory to produce deadly accurate results.

2. Inapt Analogy

Even if we were to accept *arguendo* Friedman's misleading terminology—i.e., that Galileo's assumptions are "unrealistic"—the analogy between the "unrealism" of Galileo's assumptions and the "unrealism" of the assumption of economic rationality fails.

As we have seen, physicists working in the Galilean tradition (as subsequently modified over the centuries by Newton, Einstein, et al.) abstract how objects would move in ideal conditions. This is only "unreal" in the sense that, in the real world, the conditions under which objects fall are rarely ideal. However, physicists assert that the theory is nevertheless true: if we were to produce the ideal conditions objects would so fall. And, indeed, by observing the movement of objects in the near-ideal conditions of man-made vacuums and of space, scientists have not been able to falsify the theory (i.e., heretofore, objects have only been observed falling as predicted). The nominalist skeptic might add that all that we can know is that the objects fall "as if" the theory were true—but we in fact observe objects so moving.

This is quite different from Friedman and Posner's assertion, which I discuss in the immediately following section, that markets act "as if” economic subjects were rational. They state that it is irrelevant that this is unrealistic in the sense that people (and firms) frequently, or usually, do not act rationally. Note, this is not saying that economic subjects would act rationally under ideal conditions, which would be the proper analogy to the example drawn from physics. If it were the same, then Friedman and Posner—like physicists—would try to falsify their theory by trying to replicate the ideal conditions of economic rationality and then attempt to observe behavior inconsistent with their predictions. And yet, this is precisely what Posner condemns when he criticizes JST!

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149 NAGEL, supra note 56, at 508.
150 *See infra* text accompanying notes 154-58.
151 *See Posner, Behavioral Economics, supra* note 6, at 1559. Posner states: "The rational-choice economist asks what 'rational man' would do in a given situation, and usually the answer is pretty clear and it can be compared with actual behaviour to see
Nagel does not refer to Friedman by name, but he might have been thinking of Friedman's essay when he criticizes the attempt by economists to develop universal laws, like the natural sciences. If one were to use Friedman's terminology, one might say that Nagel does criticize the assumptions of economics—or more accurately the way economists use these assumptions. Nagel says:

[T]he discrepancy between the assumed ideal conditions for which economic laws have been stated and the actual circumstances of the economic market are so great, and the problem of supplying the supplementary assumptions needed for bridging this gap is so difficult, that the merits of the strategy in this domain continue to be disputed.\textsuperscript{152}

In other words, economists ignore the phenomena they are in fact supposed to be explaining and have not adequately accounted for discrepancies between their ideal or pure cases (i.e., their assumptions) and the empirical world.

3. The “As If” Fallacy

Friedman leaps from the incorrect statement that physicists posit that heavy, compact objects fall in an atmosphere “as if” they are in a vacuum to an assertion that the neoclassical assumption of economic rationality is not necessarily a claim that economic actors are in fact rational, but merely that they act “as if” they are rational.\textsuperscript{153} Friedman uses the example of how an expert billiard

\begin{itemize}
  \item whether the prediction is confirmed. Sometimes it is not confirmed—and so we have behavioral economics.” Id. (citation omitted). Posner criticizes JST’s attempt to take account of observed behavior that does not confirm the predictions of rationality by characterizing it as mere “description” rather than theorization. Posner states: “[I]f rational-choice theory bumps up against some example of irrational behavior, the example can be accommodated by changing the theory to allow for irrational behavior. But there is no greater gain in predictive power . . . , in fact, there is a loss.” Id. at 1560. But, as we shall see, the consistent observation of data inconsistent with a theory’s predictions is the very definition of falsification. The sophisticated falsifier must “deal with” such apparently falsifying observations, either by rejecting the theory as falsified or by adopting auxiliaries to the original theory (in Posner’s words, changing the theory). Moreover, it is hard to see how the predictive power of the theory would be lost by this type of change if, by Posner’s own example, the observation of inconsistent behavior indicates that the current theory does not accurately predict economic behavior. In any event, despite Posner’s criticism of the creation of auxiliaries in the light of inconsistent observations, this is precisely what Posner in fact does.

\textsuperscript{152} NAGEL, supra note 56, at 509.

\textsuperscript{153} For example, he describes the hypothesis that firms are rational profit maximizers:

[Under a wide range of circumstances individual firms behave as if they were seeking rationally to maximize their expected returns . . . [even though] businessmen do not actually and literally solve the system of simultaneous equations in terms of which the mathematical economist finds it convenient to express this hypothesis, any more than . . . falling bodies decide to create a vacuum.

FRIEDMAN, supra note 12, at 21-22.
player makes “his shots as if he knows the complicated mathematical formula” and “could estimate accurately by eye the angles, etc. describing the location of the balls, and could make lightning calculations from the formulas, and could then make the balls travel in the direction indicated by the formulas.” Friedman asserts that the hypothesis that the player acts as if he did go through this process is not dependent on the accuracy of the assumption that he actually did go through this process. He uses this argument to support the proposition that economists can continue to use the hypothesis that businesses act “as if” they rationally seek to maximize profits even though individual “businessmen do not actually and literally solve the system of simultaneous equations in terms of which the mathematical economist finds it convenient to express this hypothesis any more than . . . billiard players explicitly go through complicated mathematical calculations or falling bodies decide to create a vacuum.”

As tempting as it might be to ridicule it, let us leave aside, as a misstatement, Friedman’s peculiar suggestion that Newton’s law of falling objects suggests that objects act as if they create vacuums, and concentrate on his billiard player example. Once again, if all that this example was supposed to illustrate were that, under falsification, one cannot test hypotheses directly by trying to verify what thought process the billiard player used (perhaps by questioning him), but one can test them indirectly by predicting facts that would be inconsistent with the hypothesis and then searching for such inconsistent facts through controlled observation, it would be unremarkable. Nor would it be particularly remarkable if Friedman meant merely to express the common skepticism toward the reliability of testimony about individual mental states and thought processes (either because people frequently lack the self-critical facility necessary for analysis or because they are prone to self-serving statements). Such skepticism would merely suggest that social scientists should put more faith in their subjects’ deeds than in their words, not that economists need not study what their subjects in fact do. But Friedman suggests that he accepts the latter, more radical position; he asserts that either the proof of a hypothesis says nothing about the validity of its assumptions, or that falsifying assumptions might cast doubt on the validity of the theory.

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154 Id. at 21.
155 Id. at 22 (emphasis added).
156 As I discuss in Economic Rationality, such skepticism is common among economists and underlies Samuelson’s revealed preference theory. See Schroeder, Economic Rationality, supra note 8.
Of course, Friedman chooses the billiards example because he assumes that it is obvious that the billiard player is not engaging in the form of calculation described in his theory, but only acting "as if" he did. In Friedman's words: "The billiard player, if asked how he decides where to hit the ball, may say that he 'just figures it out' but then also rubs a rabbit's foot just to make sure.... Upon closer examination, one realizes that Friedman evinces a remarkably unsophisticated view of consciousness and intelligence, as well as of methodology and explanation.

Note that Friedman assumes that the fact that the billiard player has no conscious awareness that he is making a calculation means that in fact his brain is not engaging in some similar cognitive activity. This implies that the only type of thinking is conscious thought. I believe that it is noncontroversial to say that not only psychoanalysts, but most scientists who study intelligence and language, believe that not all thought can be so limited. This is shown not only in the existence of dreams, but in the countless moments of free association we experience everyday. Who has not had the experience of giving up on trying to solve a problem that seems intractable only to have the solution pop into one's head later when one is consciously thinking of something totally different (or not consciously thinking of anything at all, as when one wakes up in the middle of the night with the solution)? The phenomenon of autistic savants, like the character played by Dustin Hoffman in the movie Rainman, also suggests that human beings can engage in mathematical calculations without being conscious of the procedure. Every computer in existence makes complex calculations—indeed they can make much more complex calculations than predicting the movement of a billiard ball in much less time than it takes for a player to make a shot—despite the fact that, as far as we can tell, they lack any capacity for even the most primitive form of consciousness. That is, the ability to calculate—in the sense of processing information—is not necessarily the same thing as conscious thought.

In other words, although Friedman may be correct from the perspective of a falsifying methodology that one cannot prove a theory by verifying (or even falsifying) the existence of underlying factual assumptions, it does not follow that the accuracy of predictions and the truth of assumptions are totally disconnected. Rather, the accuracy of predictions can be evidence that the assumptions are true. The observation that one can predict the movement of a billiard ball through certain mathematical calculations does not mean that the brain is not using a different method of calculation that is not conscious. 

157 Friedman, supra note 12, at 22.
calculations, combined with the fact that people often engage in complex forms of unconscious thought, could lead to the hypothesis that a good billiard player is one who undergoes an unconscious mental process very much like the mathematical calculation. I said evidence, of course, but not proof. This is because the truth of the assumptions is merely an abduction drawn from the accuracy of the predictions (in that it is a story that would make the accuracy of the prediction seem to be in the ordinary course).

4. Verstehen Doctrine

Another criticism of the “as if” fallacy is that it conflicts with the theoretical underpinnings of neoclassical economics. One of the great methodological traditions in the social sciences is the Verstehen doctrine, sometimes known as “methodological individualism.” As its name (German for “understanding”) suggests, Verstehen doctrine posits that a theory of human action can only be understood “from within by means of intuition and empathy.”\(^{158}\) It must constitute “first-person knowledge that is intelligible to us as fellow human beings.”\(^{159}\) Although Verstehen doctrine may not be directly applicable to the physical sciences that deal with such things as subatomic particles that we not only cannot empathize with, but also that, according to quantum mechanics, seem to act in ways that have no analogy in our everyday, macroworld experience (such as being simultaneously located in many probability locations at one time). Proponents insist, however, that “[n]ot only is Verstehen a necessary characteristic of adequate explanation in the social sciences, thus disqualifying such brands of psychology as Skinner’s behaviorism, but it is also the source of unique strength as compared to the outsider’s knowledge of physical scientists.”\(^{160}\)

The primary problems of pure Verstehen doctrine should be obvious, since subjective experience is the criterion of validity: (1) how do we distinguish deception from truth, and (2) more broadly, how does the community of social scientists come to intersubjective consensus?\(^{161}\) Nevertheless, one need not adopt the extreme Verstehen view that first-person understanding is the only criterion

\(^{158}\) Blaug, supra note 18, at 43.

\(^{159}\) Id.

\(^{160}\) Id.

\(^{161}\) In other words, Verstehen may be nothing more than a form of “abduction”—the spinning of a “just so” story that seems to offer a satisfactory account of a surprising thing. As we have seen, abduction is hypothesis \textit{formation}, and one needs another theory or methodology to get from formation to proof. One alternative, of course, is a metaabductive theory explaining why our guesses can be expected to be correct. For example, since the Verstehen doctrine finds validity in empathetic understanding, it might be consistent also to posit that all humans have an empathetic ability to understand other humans so that intersubjective consensus can be achieved purely through empathy.
of validity to conclude that it is a criterion of validity in the social sciences. The failure of a social science theory to be first-person understandable is a reason to reject the theory, because “explanation in social science must run not in terms of physical cause and effect but in terms of the motives and intentions of individuals.” Consequently, Popper, the arch-proponent of the methodology of falsification, was also a defender of methodological individualism—the principle “that explanations of social, political, or economic phenomena can only be regarded as adequate if they run in terms of the beliefs, attitudes, and decisions of individuals.”

Gary Lawson goes so far as to claim:

Methodological individualism is simply a positive statement about the appropriate, indeed the only possible, objects of social scientific study.... [It] is simply recognition of the fact that because the behavior of institutions is really the behavior of individuals in particular institutional settings, “[t]he only way to a cognition of collectives is the analysis of the conduct of its members.”

Lawson goes too far—one should not mistake his confidence in methodological individualism and the repetition of his assertions for a reasoned argument or demonstration. There are many intellectual traditions—Hegelianism for one—that posit that collectives cannot be reduced to a mere aggregation of individuals but have unique characteristics of their own. One cannot dismiss these traditions by mere denial without considering the arguments made in their favor. Indeed, it is precisely the Friedman/Posner theory that markets act “as if” economic subjects are rational that does not require that we show that economic subjects are rational, and that adopts a definition of rationality that neither matches our intuitions of our own behavior nor our observations of the behavior of others, which is implicitly a denial of methodological individualism in favor of methodological collectivism. Nevertheless, Verstehen doctrine’s

162 BLAUG, supra note 18, at 43.
163 Id. at 44.
164 Lawson, supra note 89, at 59-60 (quoting LUDWIG VON MISES, THE ULTIMATE FOUNDATION OF ECONOMIC SCIENCE 81 (1962)).
165 That is, people act differently as part of a collective than they do as separate individuals. Consequently, as in Hegel, the system is holistic and circular—collectives are not mere aggregates, and individuals are not merely parts of collectives, but just as collectives cannot be understood without a theory of the individual, the individual cannot be understood without a theory of the collective. This is the familiar gemeinschaft/gesellschaft dichotomy.

Another example is sociologist Nicholas Luhmann’s theory of autopoesis, which posits how social systems reproduce themselves without direct reference to individual decision making. See generally Arthur J. Jacobson, The Idea of a Legal Unconscious, 13 CARDOZO L. REV. 1473 (1992).
proposition that a theory of human nature should make intuitive sense is very appealing.

Others, more generally, insist that the purpose of a theory is to provide an explanation of a phenomenon understood as a causal mechanism.\(^{166}\) Friedmanian "as if" instrumentalism "not only refuses to offer any causal mechanism linking business behavior to the maximization of returns; it positively rules out the possibility of such an explanation."\(^{167}\) Specifically, Friedman's critics charge him with implicitly accepting the "symmetry" thesis, which has long since been disproved. The notion that explanation is "prediction written backwards" is equivalent to the assertion that "there is a perfect, logical symmetry between the nature of explanation and the nature of prediction..."\(^{168}\) This methodology has been shown to rely entirely on deduction (and, of course, abduction):

The universal laws that are involved in explanations are not derived by inductive generalization from individual instances; they are merely hypotheses, inspired conjectures if you like, that may be tested by using them to make predictions about particular events but which are not themselves reducible to observations about events.\(^{169}\)

Friedman's abandonment of *Verstehen* is particularly remarkable given that Friedrich von Hayek had advocated it as necessarily flowing from the very nature of neoclassical price theory. Price theory is a subset of that branch of social science that seeks to understand a collective or a whole in terms of its individual terms. In this case, the market is explained in terms of the aggregate behavior of economic subjects. Hayek contrasted "methodological individualism" with "methodological collectivism."\(^{170}\) The preference for methodological individualism may derive as much from philosophical as scientific concerns. As its name suggests, this methodology reflects the highly individualistic conception of human nature epitomized by Margaret Thatcher's famous assertion that there is no such thing as society, only individuals and their families.\(^{171}\) Or, in Lawson's words:

[W]hen one studies human behavior, one always studies the behavior of individual humans. There is simply nothing else to study: "[T]here are no such things as ends of or actions by

\(^{166}\) See Blaug, supra note 18, at 98.

\(^{167}\) Id. at 91.

\(^{168}\) Id. at 5.

\(^{169}\) Id.

\(^{170}\) Nagel, supra note 56, at 541 (citing F.A. Hayek, THE COUNTER-REVOLUTION OF SCIENCE (1952)).

'groups,' 'collectives,' or 'States,' which do not take place as actions by various specific individuals." When we speak of the action of a group, as we often do, we speak metaphorically. If we are to understand collectives only as aggregates of individuals, then "on one version of the principle of methodological individualism, the social scientist is to 'continue searching for explanations of a social phenomenon until he has reduced it to psychological terms." It is ironic, therefore, that Friedman and Posner could take a theory rooted in a radical individualistic philosophy and a desire to understand human psychology and purport to remove individual psychology from it.

5. Demonstrably False Assumptions

Perhaps the most telling example of how far Friedman's concept of scientific theory and explanation differs from the norm is his "constructed" theory of leaf density, which he believes is "an analogue of many hypotheses in the social sciences." Friedman asks, how can one account for the fact that trees have more leaves on the south (the sunnier side) than the north? He writes:

I suggest the hypothesis that the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives, given the position of its neighbors, as if it knew the physical laws determining the amount of sunlight that would be received in various positions and could move rapidly or instantaneously from any one position to any other desired and unoccupied position.

He asks the supposedly rhetorical question: "Is the hypothesis rendered unacceptable or invalid because, so far as we know, leaves do not 'deliberate' or consciously 'seek,' have not been to school and learned the relevant law of science or the mathematics required to calculate the 'optimum' position, and cannot move from position to position?" He declares that the hypothesis is not rendered unacceptable because "the contradictions . . . are not within the 'class of phenomena the hypothesis is designed to explain'; the hypothesis

172 Lawson, supra note 89, at 59 (quoting 1 MURRAY N. ROTHBARD, MAN, ECONOMY, AND STATE: A TREATISE ON ECONOMIC PRINCIPLES 2 (1962)). I believe that, given Lawson's unexamined claim that there is nothing else to study other than individuals, Lawson protests too much when he asserts that methodological individualism "most emphatically is not a metaphysical claim about human autonomy, a psychological claim about the formation of preferences, or a normative celebration of separateness." Id.

173 NAGEL, supra note 56, at 541 (quoting J.W.N. Watkins, Ideal Types and Historical Explanation, 3 BRIT. J. PHIL. SCI. 29 (1952)).

174 FRIEDMAN, supra note 12, at 19.

175 Id.

176 Id. at 20.
does not assert that leaves do these things but only that their density is the same as if they did."

Despite Friedman's unsupported assertions to the contrary, I believe most scientists would agree that these contradictions do indeed make the constructed hypothesis unacceptable, which is why it has not been adopted by botanists. I find it patently incredible that any botanist would be satisfied, as Friedman is, with the statement that trees look "as if" their leaves calculated and moved when our everyday experience indicates that individual leaves are neither sentient nor mobile. Rather, I would expect botanists to be interested in learning precisely what process causes leaves to become so clustered.

More important, Friedman's "explanation" is in fact no explanation at all. He observes that leaf density is greater on the sunny side of trees. He asserts that we can predict this behavior if we assume that this is so because leaves move to the sunny side of trees. Not only is this patently false, but the only thing that it predicts is the phenomenon already observed—that leaf density is greater on the sunny side of trees. In other words, this is not a theory that predicts a previously unobserved phenomenon, but merely a patently false ad hoc, and post hoc, explanation.

Friedman's answer might be, in effect, "who cares" whether or not the assumption (in this case of calculation) is literally true or not, so long as the theory predicts the movement of billiard balls, leaf density, and the behavior of businesses. Once again, this response reflects Friedman's extraordinarily narrow view of scientific explanation. Perhaps this seems more obvious from the viewpoint of the turn of the millennium, when we are surrounded by computers and discussions of the possibility of creating artificial intelligence are commonplace. Perhaps Friedman and Posner personally do not care about how economic actors "in fact" make decisions and act, so long as they can predict aggregate market behavior with reasonable accuracy. (Even though, as I discuss in Economic Rationality, behavioral economists claim that evidence shows that the assumption of economic rationality is a relatively poor predictor of market behavior.) Perhaps there are still some radical behaviorists

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177 Id.
178 See Schroeder, Economic Rationality, supra note 8.
179 As Brian Leiter has said:

Philosophers, too, have recently launched a devastating attack on the scientific and cognitive credentials of economics, starting from the observation that "[e]conomic theory is one of the more dismal empirical failures in the history of science." This is widely conceded about the laughably unsuccessful predictions of macroeconomics, but it is only somewhat less true of microeconomics, which
who only study “objective” behavior and claim not to care about (or even to suspect the existence of) subjective experience. \(^{180}\) I doubt it.

For example, to return to Friedman’s leaf example, a more “realistic” hypothesis might be that a greater percentage of the leaves on the shady side of the tree wither and die, and a greater percentage of the leaves on the sunny side of the tree flourish; this would explain the phenomenon observed (relative leaf density) equally well with the advantage not only of being true, but also of suggesting other potential theories or “predictions” to test. That is, it suggests that leaves need a certain amount of light to live (a theory that one can attempt to falsify by placing part of a tree in total darkness and observing what happens), which would in turn lead to other theories about trees and other plants—such as the theory that, unlike we animals who obtain the energy necessary for survival from eating other animals or plants, plants that have leaves are able to obtain energy directly from sunlight (i.e., through photosynthesis).

It should not be surprising that Simon, one of the most vociferous critics of Friedman’s “unreality principle,” was not only the father of behavioral economics, but also a theorist of artificial intelligence.\(^ {181}\) Both behavioral economics and artificial intelligence theory are concerned with how people actually do think and make decisions, and can never be satisfied with assertions that people act as if they thought a certain way. In Simon’s terminology, he is concerned with defining *procedural* rationality (the process of thought), whereas Friedman only concerns himself with *substantive* rationality (the result of the process).\(^ {182}\) The substantive approach seems to reflect a remarkable lack of curiosity about human nature and subjective experience.

II. POSNERIAN METHODOLOGY

A. Posner’s Account

Although Posner’s stated methodology is based on Friedman’s, he does differ in theory by being more extreme; he also violates his own stated precepts in practice. In his reply to JST, Posner repeats

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\(^ {181}\) See *NAGEL*, supra note 56, at 476-80, for a brief description of early behaviorism, which adopted something close to this strict standard.

Friedman’s dichotomy between the descriptive accuracy and predictive power of a potential theory. He declares that behavioral economics is flawed precisely because it is too empirical and is “defined by its subject rather than its method.” “Describing, specifying, and classifying the empirical failures of a theory is a valid and important scholarly activity. But it is not an alternative theory.”

Regardless of whether one accepts Posner’s characterization of JST’s article specially, or of behavioral economics in general, so far, his statements are not that surprising. As already discussed, all theory requires abstraction, and the process of abstraction, by necessity, suppresses many concrete features of specific phenomena. In this sense, the descriptive activities Posner damns with faint praise by labeling them “valid and important scholarly activities” are, therefore, by definition not theorization, because they do not abstract from reality. What Posner underplays, however, is that describing, specifying, and classifying empirical inconsistencies are the primary tools in the process of development, refinement, and testing of a hypothesis known as falsification—the methodology that Posner claims to champion. He also ignores the fact that, as discussed above, observation and description (and, therefore, specification and classification) are themselves forms of abstractions.

According to Posner, “If a theory cannot be falsified, neither it nor its predictions can be validated, for everything that happens is by definition consistent with the theory.” So far, so good. He continues:

JST’s theory seems perilously close to the abyss of nonfalsifiability; perhaps it has fallen in. When people act rationally, JST do not treat this as contradicting the assumption of bounded willpower. When people resist temptations, thus demonstrating strength of will, this is not treated as contradicting the assumption of bounded willpower. And when they act selfishly, this is not deemed a contradiction of the assumption of bounded self-interest. So the question arises, what if any observation would falsify JST’s theory?

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183 See Posner, Behavioral Economics, supra note 6, at 1559 (“[I]n theory-making, descriptive accuracy is purchased at a price, the price being loss of predictive power.”).
184 Id.
185 Id. at 1560.
186 See supra text accompanying notes 86-104.
187 Posner, Behavioral Economics, supra note 6, at 1560.
188 Id. As stated before, my goal is to criticize Posner, not to defend JST. Nevertheless, I will once again point out that Posner is (clearly) mischaracterizing JST’s position. They are not arguing that people are never economically “rational” in the classical sense, nor that they, on the contrary, are always weak of will, altruistic, or whatever. Rather, I believe that they are saying that human nature is neither wholly divine nor wholly bestial. Humans do not act...
Nevertheless, despite his casting stones at JST, Posner can be charged with committing precisely the same sin. In this final Part, I introduce the classic account of falsification as developed by Popper and his school. I then contrast Popperian falsification with Posner’s methodology in theory and in practice. As has been argued by others before me, although Posner calls for an objective, fact-based analysis of legal issues, he in fact rarely, if ever, engages in empirical investigation. In *Economic Rationality* I show that many of Posner’s claimed predictions of unobserved behavior are, in fact, ad hoc explanations of previously observed behavior. In this Article, I further show that when Posner does hazard predictions, he merely uses the vocabulary of falsification while replacing it with the much easier, but less reliable, method of verification. I partially redeem Posner by suggesting that one can interpret him as implicitly adopting the first step (but only the first one) of the methodology that Popper’s student Imre Lakatos called “sophisticated” falsification. This is abduction—the telling of just so stories. Nevertheless, although every journey starts with the first step, Posner remains far from his goal of establishing an appropriate methodology for legal analysis.

**B. Posnerian Methodology v. Popperian Falsification**

*This, O my Best Beloved, is a story—a new and a wonderful story—a story quite different from the other stories . . .*

Friedman and, therefore, Posner claim to be adherents of falsification. The most developed theory that states that scientific methodology should be limited to falsification is that developed by Karl Popper and his school.

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189 See Luban, *supra* note 28, at 1005. Luban describes Posner as arguing that “[d]ecisions at law, judicial or otherwise, must be based on a realistic, empirically informed, unsentimental, preferably quantitative comparison of costs and benefits . . .” *Id.* However, later Luban argues that although economics, one of the three keys to Posner’s legal theories, is “a quantitative and empirical scientific discipline[,]” Posner’s own model “creates too many uncertainties . . . when Posner tries to move from the qualitative to the quantitative, from the imprecise to the precise.” *Id.* at 1024.


191 KIPLING, *supra* note 1, at 199.

192 There are other schools of philosophy of science. Willard Van Orman Quine, for example, rejects Popperian falsification as ad hoc and has developed a coherence theory that
At this point, I should at least note in passing that, although it is commonplace among lawyers and other lay people to follow Popper and identify scientific reasoning with falsification, there is a good deal of disagreement among philosophers of science and practicing scientists whether this is the case, either in theory or practice. Indeed, one can argue that Popperian falsification is obsolete, having been superseded by more recent theories of scientific methodology. What methods scientists actually follow in practice is another question. It is yet another question entirely whether moral and legal questions can or should be decided by "scientific" methods.

Although one need not go as far as Paul Feyerabend, who supposedly asserted that actual scientific practice is "anything goes," it is probably true that even the most ardent believer in falsification must, as a practical matter, combine a number of different methodologies as well as "gut feeling" in scientific investigation. W.V.O. Quine goes further in concluding that supports induction. See W.V. Quine & J.S. Ullian, The Web of Belief 65, 89-91 (2d ed. 1978). Popper wrote his Logik der Forschung in 1934 (published in English as The Logic of Scientific Discovery in 1959), and his theory has arguably been largely superseded in philosophic circles by more recent work. For an introduction to modern philosophy of science since Popper, see David S. Caudill & Richard E. Redding, Junk Philosophy of Science?: The Paradox of Expertise and Interdisciplinarity in Federal Courts, 57 Wash. & Lee L. Rev. 685 (2000). Nevertheless, because Posner claims to adopt the methodology of falsification, I shall limit myself to the theory of Popper and his school.

Most famously, the United States Supreme Court, in Daubert v. Merrell Dow Pharmaceuticals, Inc., 509 U.S. 579 (1993), wrote as though Popperian falsification was virtually universally accepted among scientists and philosophers of science. See id. at 592-95.

Two recent commentators have suggested that the Daubert decision reflects a surprising ignorance of the last forty years of scholarship in this area. See Caudill & Redding, supra note 192; John H. Mansfield, Scientific Evidence Under Daubert, 28 St. Mary's L.J. 1 (1996).

For an interesting account of how one of the most famous textbook examples of the experimental method at work in fact deviated far from the ideal of "falsification," see Oliver Morton, Science in the Dark, WALL ST. J., Aug. 11, 1999, at A18 (discussing theories that purport to explain solar eclipses).

Although Paul Feyerabend has occasionally used this expression, he claims that it was a joke. "Anything goes" is a reductio ad absurdum of his actual position that science, like any other scholarly endeavor, should not and cannot commit itself to any specific method. See PAUL FEYERABEND, SCIENCE IN A FREE SOCIETY 186 (1978). As I have stated elsewhere:

Feyerabend means to suggest that different methods may be more or less adequate for different tasks and that it is very difficult, if not impossible, to define ahead of time what method is best suited to the task at hand. . . . [T]o presume that one knows ahead of time which method will turn out to be the most adequate is to presuppose that one knows the very answer one is seeking.

Schroeder, Abduction, supra note 33, at 152 n.101; see also PAUL FEYERABEND, AGAINST METHOD (1975); PAUL FEYERABEND, FAREWELL TO REASON (1987). In the words of Richard Rorty, "[T]he whole idea of 'being scientific' and of choosing between 'methods' is confused." RICHARD RORTY, CONSEQUENCES OF PRAGMATISM 195 (1986). Although Feyerabend started as a student of Popper, he became one of his most vociferous critics.

As Gregory Crespi suggests:
falsification is incoherent as a methodology. As soon as one admits that one must develop auxiliary explanations to explain why empirical observation deviates from the hypothesis (as it must always do), then one is, in fact, admitting that one is not engaging in falsification. Consequently, despite their logical flaws, abduction and verification may be the only methods available in our imperfect world. The theory of sophisticated falsification developed by Lakatos and championed by Blaug is an attempt to defend the practice of falsification against Quine's charge of ad hocery.

This debate, although extremely interesting, will not be resolved in this Article. Posner and Friedman have already taken sides with the falsifiers so I will in the next section limit myself to an internal criticism from within the falsification school. As shall be evident from the last section of this Article, whatever methodology is appropriate for science, I neither believe that law is or can be a science, nor that scientific methodology is the only or the best methodology for the study of law.

1. The Problem of Induction

Proponents of falsification are reacting to the logical problem of induction. This problem is so familiar as to border on the banal. It is worth revisiting, however, because although it is easy enough to distinguish falsification and verification in theory, scientists frequently are unable to resist the temptation to confuse them in practice, because verification is so much easier to apply than falsification. As soon as one admits that one must develop auxiliary explanations to explain why empirical observation deviates from the hypothesis (as it must always do), then one is, in fact, admitting that one is not engaging in falsification. Consequently, despite their logical flaws, abduction and verification may be the only methods available in our imperfect world. The theory of sophisticated falsification developed by Lakatos and championed by Blaug is an attempt to defend the practice of falsification against Quine's charge of ad hocery.

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falsification. In this section I show that this is an error that Posner routinely makes.

Induction is the derivation of universal rules from specific examples. This is, of course, an extremely common mental process. It characterizes not only the common-law case method but also much of what is known as “common sense.” Unfortunately, in contrast to deduction (the derivation of specific cases from universal rules), one cannot absolutely prove, as a logical matter, the truth of the hypothesis derived from induction.

Through induction, when one observes one thousand white swans and not a single black one, one might conclude that all swans are white and predict that all additional swans one might observe in the future will also be white. Nevertheless, no matter how many additional white swans one observes, this does not logically rule out the possibility that there might be some black swans out there hiding in the shadows. Indeed, it turns out that there is a species of black swans indigenous to (where else?) Australia. In other words, “verification”—defined as the attempt to prove the truth of a hypothesis by searching for observations that are consistent with the predictions drawn from one’s hypothesis—is not a reliable methodology.

Popper famously claimed to have “solved” the problem of induction. Induction can be used negatively, not positively. That is, while no number of consistent observations can prove a hypothesis, even one inconsistent observation can theoretically

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201 This is an accusation that Blaug makes against his fellow economists. See, e.g., BLAUG, supra note 18, at 226 (“Despite continued appeal to the methodological norms of falsificationism, the whole of Becker’s writings are positively infected by the easier option of verificationism . . . ”). However, Blaug notes that Friedman believes that “verification of the postulates or assumptions of economic theory is both unnecessary and misleading . . . .” Id. at 110. Nonetheless, “[t]he prevailing methodological mood is not only highly protective of received economic theory, it is also ultrapermissible within the limits of the ‘rules of the game’ . . . . Modern economists frequently preach falsificationism, . . . but they rarely practice it: their working philosophy of science is aptly described as ‘innocuous falsificationism.’” Id. at 110-11 (citation omitted).

202 To use Charles Sanders Peirce’s example of an inductive syllogism:

Case.— These beans are from this bag.
Result.— These beans are white.
:.Rule.— All the beans from this bag are white.

2 CHARLES S. PEIRCE, Deduction, Induction, and Hypothesis, in COLLECTED PAPERS OF CHARLES SANDERS PEIRCE 373 (Charles Hartshorne & Paul Weiss eds., 1960) [hereinafter COLLECTED PAPERS]; see also Schroeder, Abduction, supra note 33, at 179-80.

203 Peirce’s example of a deductive syllogism is:

Rule.— All the beans from this bag are white.
Case.— These beans are from this bag.
:.Result.— These beans are white.

2 PEIRCE, supra note 202, at 374; see also Schroeder, Abduction, supra note 33, at 179-80.

204 See POPPER, OBJECTIVE KNOWLEDGE, supra note 30, at 8-9; see also Schroeder, Abduction, supra note 33, at 162.
disprove a hypothesis. (I say "theoretically" because I shall leave the pragmatic problems of actually applying falsification as a methodology and the resulting theory of sophisticated falsification to a subsequent section.) Consequently, Popper proposes that the only true scientific methodology is "falsification"—defined as the attempt to disprove a hypothesis through a search for observations that are inconsistent with predictions drawn from the hypothesis.

Note that falsification, like verification, is a form of inductive reasoning. It can, therefore, never be used to prove directly the objective truth of a hypothesis. Popper, nevertheless, argued that falsification can be used to bring science closer to objective truth. By repeated application of the methodology of falsification, scientists can weed out false theories. Eventually, through a process of elimination or "apagogic" reasoning, we will arrive at the "truth" as the only theory left. The rub is that the problem of induction informs us that we can never know for sure when we have arrived at a true theory. No matter how many times we have failed to observe data inconsistent with our hypothesis, there is always the logical possibility that the next observation will falsify our hypothesis. It is reasonable for scientists to tentatively accept a hypothesis that has not heretofore withstood repeated attempts to falsify it. Nevertheless, all such acceptances are merely contingent. To claim scientific status, a hypothesis must always remain fallible and corrigible.

2. Assumptions

Although Posner claims to be engaging in scientific reasoning he, in fact, confuses the first step in scientific reasoning—abduction—with the later steps of induction and deduction—and then further confuses falsification with verification. Let us now consider the process of forming hypotheses so that we can contrast it with the process of proving or demonstrating them.

As I discussed in the previous Part, Friedman and Posner state that the validity of the "assumptions" underlying a hypothesis is

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205 Popper has a very sophisticated—indeed postmodern—conception of "objective" truth. He regards truth as a humanly created, intersubjective consensus that is reached through the application of an agreed upon methodology, as opposed to correspondence with whatever reality exists "out there." Nevertheless, Popper believed (or hoped) that through the scientific methodology of falsification, our objective knowledge might eventually approach external reality. See infra text accompanying notes 209-27.

206 As Popper reminds us, if one does not criticize oneself, one's rivals will be happy to do so. See POPPER, SCIENTIFIC DISCOVERY, supra note 33, at 16. According to Mansfield, Popper de-emphasized the fact that finding facts consistent with a theory does provide some confirmation (but not certainty) of a hypothesis's truth and emphasized falsification "in order to encourage a skeptical attitude toward hypotheses." Mansfield, supra note 194, at 11.

207 See supra text accompanying notes 42-45, 132-57.
irrelevant to the validity of the hypothesis. Popper, in contrast, insists that the first step in any scientific methodology is hypothesis formation—a process that necessarily comprises the development of a set of assumptions. Popper readily admitted that there was an inevitably ad hoc, subjective, “irrational,” and (he thought) unscientific aspect to this necessary starting moment—there is no way logically to determine where to start.²⁰⁸ Although one’s starting hypotheses usually originate in empirical observation, there is no way objectively to choose what facts to start from—observation requires a viewpoint and a viewpoint is always subjective.²⁰⁹

Popper comes to the surprising conclusion that in some sense one’s initial working assumptions are ultimately “irrelevant” to scientific process.²¹⁰ But this is not, as Friedman and Posner suggest, because a valid theory does not require valid assumptions (the F-twist). Rather, it is because it is the assumptions themselves that are the very subject of falsification. In other words, Popper believes that initial assumptions are only irrelevant in the sense that the scientific process of falsification should eventually winnow out all bad assumptions so that the scientific community would approach consensus as to what the appropriate assumptions should be.²¹¹

As I have briefly discussed, Posner identified three “worlds” of truth. First there is the first world of objective reality that exists

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²⁰⁸ See POPPER, SCIENTIFIC DISCOVERY, supra note 33, at 31. Popper states: “The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it.” Id. Consequently, every scientific idea contains an “irrational element” or “creative intuition” that is a matter of “psychology.” Id. at 32. Peirce, of course, would disagree: he thought that an understanding of abduction was necessary for an understanding of science. Part of this disagreement springs from different uses of the word “rational.” Popper limits the term “rationality” to logical necessity.

²⁰⁹ As indicated by the title of his essay The Bucket and the Searchlight: Two Theories of Knowledge, in POPPER, OBJECTIVE KNOWLEDGE, supra note 30, at 341, Popper used the metaphor of the searchlight to describe his theory of science as an active searching starting from a specific viewpoint, and contrasts it with the more common, but naive, “bucket theory,” which sees the mind as a passive receptacle of facts. He states, “In science it is observation rather than perception which plays the decisive part. An observation is a perception, but one which is planned and prepared... An observation is always preceded by a particular interest, a question, or a problem—in short, by something theoretical.” Id. at 342 (citation omitted). Consequently, Popper concludes that “[t]he initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it.” POPPER, SCIENTIFIC DISCOVERY, supra note 33, at 31. The origins of scientific theories contain an “irrational element” or “creative intuition” and are matters of “psychology.” Id. at 31-32.

²¹⁰ He states that “[t]he question how it happens that a new idea occurs to a man... is irrelevant to the logical analysis of scientific knowledge.” POPPER, SCIENTIFIC DISCOVERY, supra note 33, at 31.

²¹¹ Popper says that the origins of a scientific theory are irrelevant, because once formulated, the theory is then “submitted... to logical examination.” Id. at 31. The scientist engages in “subsequent tests” that will determine whether “the inspiration may be discovered to be a discovery, or become known to be knowledge.” Id.
external to human thought. Popper agreed with theorists as diverse as Kant, Peirce, and Lacan that we humans have no direct access to this "thing-in-itself" because our knowledge is always mediated by our thoughts and perceptions. He called our subjective experience of reality the second world. Nevertheless, Popper did not think that each of us is condemned to the ignorance of our individual, idiosyncratic second worlds. Rather, he believed that, by adopting an appropriate methodology, a community could come to an intersubjective consensus, which he called the third world. Popper sought to identify a logical methodology distinguishable from and more reliable than "common sense" that could lead to the favored consensus of science.

Popper sometimes suggested that the third world—the consensus reached by science—could eventually approach a true understanding of the first, or object, world. By proper application of the scientific method of falsification the community of scientists could weed out the bad hypotheses and arrive at a consensus as to which remaining hypotheses seemed promising for future testing. In Popper's terminology, these consensus theses were "objective," but only in the sense of being reached by intersubjective

212 See POPPER, OBJECTIVE KNOWLEDGE, supra note 30, at 106 ("[W]e may distinguish the following three words or universes: first, the world of physical objects or of physical states . . .").

213 In Popper's words, this is "the world of states of consciousness, or mental states, or perhaps of behavioral dispositions to act." Id.

214 This is "the world of objective contents of thought, especially of scientific and poetic thoughts and of works of art." Id.

215 Although Popper argued that the intersubjective third world did not have direct access to the objective first world, he also thought that the subjective second world formed an indirect link between them. See id. at 155-56. Note that although Popper insists on characterizing his third world as "objective," see id. at 108-09, he is using the word in the sense that I have called "Community Objectivity," or intersubjective consensus. See Schroeder, Subject: Object, supra note 29, at 17-28. Intersubjective consensus is objective only in the sense that it is independent of the idiosyncratic subjectivity of any specific member of the community who creates the consensus, not in the sense that it relates to direct access to the external "object" world. In this sense, scientific knowledge is subjectless, as indicated by the title of Popper's essay Epistemology Without a Knowing Subject. See POPPER, OBJECTIVE KNOWLEDGE, supra note 30, at 106.

216 Although Popper recognized the "irrational" moment of theory creation, he argued that this was irrelevant because the methodology of falsification would weed out any purely idiosyncratic theory, leaving only those that are "objective" in the sense of being the subject of intersubjective consensus. See POPPER, SCIENTIFIC DISCOVERY, supra note 33, at 31; Karl R. Popper, Normal Science and Its Dangers, in CRITICISM AND THE GROWTH OF KNOWLEDGE, supra note 127, at 51, 57; see also Schroeder, Abduction, supra note 33, at 163-64.

Note that, from a Popperian standpoint, scientific theories are "value free" or aperspectival... in the sense that they are not dependent on the idiosyncratically held opinions or the viewpoint of any one member of the community but are chosen by, and shared within, the community." Schroeder, Subject: Object, supra note 29, at 20.
agreement. We have no direct way of knowing whether a thesis is "objectively" true in the sense of being absolutely and universally true—because we do not have access to the mind of God. In Kantian terms, as phenomenal creatures we cannot know the noumenon (the thing-in-itself).

Popper describes his notion of science as a search for "verisimilitude" rather than "truth." This does not imply, however, that Popper is indifferent to the truth claims of theory in the way that Friedman and Posner are. Rather, Popper's distinction rests on a very exalted definition of truth, coupled with a modest assessment of human limitations. By "truth" Popper means "the set of all true statements"—that is, perfect knowledge of the object world with absolutely no omissions—which, he admits, is an "unattainable target set." Only God Himself would be capable of such absolute knowledge. Consequently, Popper seeks "a clearer and a more realistic aim than the search for the truth" understood in this universal sense. The goal of verisimilitude is a corollary to the Popperian principle that scientific hypotheses, like all human knowledge, are always fallible and corrigible and, therefore, always "false" to some degree. Consequently, he accepts the fact that "theories retain their interest even if we have reason to believe that they are false." But, once again, this is not, as Friedman and Posner argue, because the only test of theory is prediction. Instead, Popper thinks that a good theory is an "approximation" of the truth, and that the application of scientific methodology can make theories into more and more accurate approximations:

I intend to show that while we can never have sufficiently good arguments in the empirical sciences for claiming that we have actually reached the truth, we can have strong and reasonably good arguments for claiming that we may have made progress towards the truth; that is, that the theory \( T_2 \) is preferable to its predecessor \( T_1 \), at least in the light of all known rational arguments.

Moreover, we can explain the method of science, and much of the history of science, as the rational procedure for getting nearer to the truth.

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217 See Popper, Scientific Discovery, supra note 33, at 44; Schroeder, Subject: Object, supra note 29, at 17-24.
218 Popper, Objective Knowledge, supra note 30, at 57.
219 Id.
220 Id.
221 Id.
222 He states, "Newton never believed that his theory was really the last word, and Einstein never believed that his theory was more than a good approximation to the true theory—the unified field theory which he searched for .... " Id.
223 Id. at 57-58 (emphases added).
Consequently, as Luban has suggested, Posner's self-proclaimed pragmatism with its, at best, agnostic view toward the truth of theories seems at odds with his stated allegiance to the scientific methodology of falsification, which sees truth as the asymptote that scientific theory as verisimilitude approaches, but never reaches.

Once again, I am not arguing that the Popperian notion of science as falsification is itself unproblematic. One nagging problem with Popper's confidence that science can weed out bad initial assumptions (making them "irrelevant") should be obvious. Because falsification can only work negatively and all scientific theories remain fallible and corrigible, we can never know when or if all bad assumptions have been eliminated. Although Popper hoped that the third world of scientific, intersubjective consensus could approach correspondence with the first world of "objective" reality, he understood "that it is not possible to move from a logical conclusion to a statement about the real [i.e., first] world." In other words, even though Popper thought that falsification "would continue the progress of knowledge," the conclusion "that there was any progress at all must finally be a matter of faith."

Consequently, there would seem to be considerable advantages if we could determine somehow what type of assumptions are more or less likely to prove fruitful before embarking on the arduous and unending process of falsification. It might be possible through the study of human thought and behavior to identify systematic prejudices that color our thought processes, which might make us suspicious of certain types of hypotheses and favorably disposed toward others.

3. Abduction

Charles Sanders Peirce, the great philosopher of science and cofounder of pragmatism, tried to give greater rigor to our understanding of the process of determining which assumptions are likely to prove fruitful for inquiry. Following Aristotle, he called this process "abduction" or "retroduction." In contrast to Popper, who only includes deduction and induction in his definition of logic, Peirce insisted that abduction is "a form of logic equal to the other

224 See Luban, supra note 28, at 1005.
225 Mansfield, supra note 194, at 11.
226 Id. at 12.
227 1 CHARLES S. PEIRCE, Lessons from the History of Science, in COLLECTED PAPERS, supra note 202, at 28; see also RICHARD TURSMAN, PEIRCE'S THEORY OF SCIENTIFIC DISCOVERY 13 (1987); Schroeder, Abduction, supra note 33, at 115-17.
forms and absolutely crucial to the growth of knowledge." In other words, to a Peircean, initial assumptions are neither irrelevant nor can they be dismissed as unreal, but are at the heart of scientific methodology.

Abduction is a retroactive attempt to account for a past observation. It is post hoc explanation. Abduction may be described as follows: I observe a surprising thing (i.e., something that is different from my expectations, or an empirical datum that seems anomalous to the result predicted through application of my hard-core theory). I then try to use my imagination to formulate an account that would explain the surprising thing. By explanation, Peirce means: Fact X seems surprising, but if such and such were true, then the fact of X would no longer be surprising, but would seem to be a matter of course.

Peirce describes the difference between the three forms of logic as follows: “Deduction proves that something must be [i.e., necessity]; Induction shows that something actually is operative [i.e., actuality]; Abduction merely suggests that something may be [i.e., possibility].” As Nancy Harrowits explains, “Abduction is a theory developed to explain a pre-existing fact.”

In normal English, abduction is intelligent guessing. One of Peirce’s important points, however, is that abduction alone is merely the generation of potential hypotheses, not a form of scientific proof or even demonstration. In my example, the fact that X exists standing alone does not prove or even give us reason to

228 Schroeder, Abduction, supra note 33, at 119; see also TURSMAN, supra note 227, at 81; Paul Weiss, Charles S. Peirce, Philosopher, in PERSPECTIVES ON PEIRCE: CRITICAL ESSAYS ON CHARLES SANDERS PEIRCE 120, 125 (Richard J. Bernstein ed., 1965).

229 See 2 PEIRCE, supra note 202, at 374; Thomas A. Sebeok, One, Two, Three Spells UBERTY, in THE SIGN OF THREE: DUPIN, HOLMES, PEIRCE 1, 8 (Umberto Eco & Thomas A. Sebeok eds., 1983) [hereinafter THE SIGN OF THREE]; Schroeder, Abduction, supra note 33, at 179-81; see also NORWOOD R. HANSON, PATTERNS OF DISCOVERY: AN INQUIRY INTO THE CONCEPTUAL FOUNDATIONS OF SCIENCE 85 (1958).

230 See CHARLES S. PEIRCE, PHILOSOPHICAL WRITINGS OF PEIRCE 151 (Justus Buchler ed., 1985); see also Umberto Eco, Horns, Hooves and Insteps: Some Hypotheses on Three Types of Abduction, in THE SIGN OF THREE, supra note 229, at 198, 203-04; Schroeder, Abduction, supra note 33, at 179 n.216.

231 5 CHARLES S. PEIRCE, Three Types of Reasoning, in COLLECTED PAPERS, supra note 202, at 106.


233 As I have called it elsewhere. See Schroeder, Abduction, supra note 33, at 180.

234 See id. at 183-84.
think that such and such is in fact true. Abduction is the spinning of a good yarn. It is the telling of just so stories. An abduction that seems plausible, given what other things we know or believe about the world, does, however, constitute a reason for us to further investigate whether such and such is true. To quote Umberto Eco, a theorist of abduction, we need a way of “deciding as to whether the possible universe outlined by our first-level abductions is the same as the universe of our experience.”

Elsewhere, I have suggested that theorists of abduction have offered at least three general ways of moving from abduction to proof. The first would be some form of “logical” or “reasoned” method. As I have already discussed, the Popperian tradition says that we can never, in fact, verify whether or not the new hypothesis is true, but we can develop a research program in an attempt to falsify the new hypothesis. That does not mean that we may not have “good reasons” to either logically or intuitively choose one abducted hypothesis over the other. Obviously, a hypothesis that is consistent with other facts and theories that the scientist already accepts has a competitive advantage over others. For example, even though I enjoy the X-Files, I would not favor an abducted hypothesis suggesting that the reason why so many people behave in ways that seem economically irrational is that they have been abducted by aliens.

This is why we are not tempted to try to test the hypotheses presented in Kipling’s Just So Stories: even though they would explain certain surprising facts if they were true. Given what we know about the world, they are simply not plausible explanations. Indeed, their very absurdity labels them as fantasy and makes them amusing. On the other hand, some brilliant abductions have been accepted by the scientific community despite the fact that they initially seemed to have been widely diverse from accepted knowledge (think, for example of the principles of quantum mechanics).

A second possible way of “proving” a hypothesis is through some “nonscientific,” “nonlogical,” or “irrational” method such as by reference to taste, aesthetics, morality, politics, or faith. By definition, theories of “science” (as opposed to theology or

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235 Eco, supra note 230, at 207.
236 See Schroeder, Abduction, supra note 33, at 183.
237 See id. Note, I am not using the word “irrationality” in the negative sense of being insane, nor do I mean to imply that irrationality is necessarily somehow inferior to rationality. Many very sophisticated thinkers believe that, ultimately, religious faith is the only sure path to truth.
philosophies of morality or aesthetics) have little to say about such methods.

A third approach, one that is occasionally adopted by Peirce and Hegel, may be called a philosophical theory of "metaabduction." This posits that there might be reason to believe that man has some inherent ability to make good abductions (guess correctly) about the object world. A full discussion of Peirce's theory (which is remarkably similar to Hegel's) is beyond the scope of this Article. But, to oversimplify, it is based on the proposition that if we can assume that the human mind participates in universal reality, then there might also be reason to suppose that studying human consciousness might give us knowledge of the universe as well. In Tursman's words:

If nature and thought are parts of the same system, then it may well be the case that whatever constraints apply to the system of thought apply as well to the system of nature. Or, to put it another way, our main goal is to discover whatever constraints obtain on the one system of thought and nature, and if the only way we can observe these constraints on the entire system is by observing the constraints on thought, then that hypothesis is worth pursuing.

As described by Eco, metaabduction requires that one have "the courage of challenging without further tests the basic fallibilism that governs human knowledge."

Peirce and Hegel suggest that metaabduction could only work at the highest level of abstraction—they would never suggest that we can rely on our abductions about most scientific theories, let alone everyday decisions. Most actual abductions have all the independent reliability of just so stories. Consequently, some form of pragmatic reasoning is always a necessary complement to dialectical reasoning.

As we shall see, Posner, however, merely engages in this first step of abduction, but never proposes a research program. As discussed earlier, falsification consists of speculating about what as yet unobserved phenomena would be inconsistent with one's hypothesis, and then attempting through controlled observation to

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238 Eco coined "the term 'meta-abduction' to describe the process of moving from a 'first-level' abductive hypothesis to the conclusion that the hypothesis is true" (that is, "true" in the sense of matching our experience of the world). Schroeder, Abduction, supra note 33, at 183 n.234; see also Eco, supra note 230, at 207.
239 See Schroeder, Abduction, supra note 33, at 184.
240 TURSMAN, supra note 227, at 115.
241 Eco, supra note 230, at 220.
242 See 1 CHARLES S. PEIRCE, Fallibilism, Continuity, and Evolution, in COLLECTED PAPERS, supra note 202, at 70-71; Eco, supra note 230, at 218; Schroeder, Abduction, supra note 33, at 185.
identify the empirical existence of such phenomena. This is the opposite of the methodology that Posner in fact adopts—verification—in which one speculates as to what as-yet unobserved phenomena would be consistent with the hypothesis and then hunting for such verifying phenomena.

4. Sophisticated Falsification

Imre Lakatos reworked Popper’s methodology to develop what I will call “sophisticated” falsification. The problem with the simple statement of falsification given above—that a hypothesis is falsified if scientists observe data inconsistent with the predictions of the hypothesis—should be obvious. No actual hypothesis could ever withstand such strict scrutiny in the messy empirical world. We can always expect to observe some data that at least seems at first blush inconsistent with any theory. Friedman and Posner might be trying to capture this idea in their odd assertion that the assumptions underlying theories are “unrealistic” in the sense of simplified and abstract.

Popper recognizes this problem in part when he insists that falsification requires more than the mere observation of inconsistencies. Rather, these inconsistencies must be consistently observed in reproducible, controlled observations. This only partly solves Popper’s problems.

As Friedman correctly suggests, insofar as all theories are, by necessity, abstractions, it is to be expected that empirical observations will in most (or all) cases deviate from predicted results to some degree. Consequently, a “sophisticated” falsification methodology must distinguish between those deviations that falsify a theory and those that do not.

Popper’s student, Kuhn, famously argued that the implication of Popper’s theory was that there can be no single scientific method for testing hypotheses (or in his terminology, for rejecting an existing scientific paradigm and adopting a new revolutionary paradigm) because methodology itself is always internal to a paradigm or hypothesis. That is, a scientist may very well have “good reasons” like “accuracy, scope, simplicity, fruitfulness, and the like” for shifting paradigms, but never logically necessary ones.

Falsification, therefore, is only a methodology that can be used in refining the details of a specific existing hypothesis (paradigm), but

243 See infra text accompanying notes 86-88. POSNER, ECONOMIC ANALYSIS, supra note 16, at 18 (“But abstraction is of the essence of scientific inquiry, and economics aspires to be scientific.”).
244 See Schroeder, Abduction, supra note 33, at 165-67.
245 See Kuhn, Reflections, supra note 127, at 261.
not to reject it as a whole. Kuhn damned the methodology of falsification with faint praise by labeling it “normal science,”\(^{246}\) to be distinguished from “revolutionary science” that rejects hypotheses (i.e., shifts paradigms).\(^{247}\) As mentioned above, despite Friedman’s claims that he adopts the methodology of falsification, he agrees with Kuhn that, when it comes to deciding between hypotheses, “men can ultimately only fight”;\(^{248}\) and suggests, like Kuhn, that “relevant considerations” might include “simplicity” and “fruitfulness.”\(^{249}\)

Lakatos’s project was to reformulate Popper’s methodology in light of Kuhn’s powerful critique. Lakatos agreed with both Popper and Kuhn that scientists do not (and should not) reject a hypothesis merely because of the observation of apparently inconsistent data. He states, “Contrary to naive falsificationism, no experiment... alone can lead to falsification. There is no falsification before the emergence of a better theory.”\(^{250}\) Rather, the scientist should first identify the “hard core” or theoretically necessary kernel of her theory that can form the basis of a “scientific research program”—Lakatos’s more modest term for a Kuhnian paradigm.\(^{251}\) The scientist then engages in controlled observation (through experiments or otherwise) in order to identify empirical data inconsistent with the hypothesis.

When inconsistencies are observed, the theorist does not immediately reject the theory. Rather, she tries to develop “auxiliary” hypotheses that both explain away the apparent anomaly while remaining consistent with the hard core to serve as a
“protective belt” buffering the core. Most of what Kuhn calls “normal science” would fall within the development of this protective belt. Although to my knowledge Lakatos does not use the term, these auxiliaries are developed through abduction—they are stories that make what was initially surprising (anomalous observations) seem a matter of course. Consequently, since abduction is not proof, the scientist cannot stop when she formulates her auxiliaries. The research program requires that she now try to falsify her auxiliaries (which are now part of her hypothesis). If this leads to the observation of new data anomalous with the auxiliaries, the scientist then develops auxiliaries to her auxiliaries and repeats the process, which could theoretically continue ad infinitum.

One of the most interesting aspects of Lakatos’s methodology is that he seeks to develop a “logical” criteria for shifting paradigms by identifying how a research program degenerates over time. This happens when the hard core becomes too encrusted with an increasingly broad protective belt of ad hoc auxiliaries. In other words, when a research program starts degenerating, the hard core of the theory starts explaining less, rather than more, about the world as the majority of the theory becomes excuses for why the theory does not work in more and more circumstances.

Surprisingly, however, Lakatos does not suggest that a scientist should reject a research program merely because it is degenerating. Rather he maintains that the scientist has no choice but to stick with her hard core until she is able to formulate an alternative hypothesis that has “excess empirical content.” That is, she must formulate a new hard core that explains more of the observed empirical data than the degenerated hard core of the old research program.

The classic example is the replacement of the Ptolemaic theory of the solar system with the Copernican (and the replacement of the Copernican with the Keplerian). As is well known, the Ptolemaic

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252 Lakatos, supra note 250, at 132-38.
253 In Blaug’s words, a scientific research project degenerates if it “is characterized by the endless addition of ad hoc adjustments that merely accommodate whatever new facts become available . . . .” BLAUG, supra note 18, at 33; see also Schroeder, Abduction, supra note 33, at 169-70.
254 Lakatos, supra note 250, at 118.
255 Critics, such as Kuhn himself and Paul Feyerabend, challenge Lakatos’s proposition that excess empirical content constitutes the single “scientific” test for adopting a “revolutionary” research program (paradigm) as either a theoretical or empirical matter. Quine argues that all attempts to formulate a coherent theory of falsification are doomed since the adoption of auxiliary hypotheses is always to some extent ad hoc. Feyerabend goes a step further and contends that in practice actual scientific methodology is “anything goes.” See supra note 196. I discuss the development of falsification methodology extensively in Schroeder, Abduction, supra note 33, at 161-71.

As I discuss throughout this Article, it is standard practice for economists to accuse their critics of ad hocery, while ignoring their own.
theory postulated that the planets (including the sun and moon) all moved in perfect circles around the earth. When astronomers observed that planetary movement deviated from the predictions, they formulated the auxiliary of epicycles—the planets moved in perfect circles within perfect circles. When additional anomalies were observed more and more levels of epicycles were added. Although the astronomers were able to account for most of the movement they observed by multiplication of epicycles, this was at the expense of causing their research program to degenerate. The hard core (the planets moved in a circular orbit around the sun) explained less and less of planetary movement. Copernican theory was adopted because its hard core—the planets (other than the Moon) moved in perfect circles around the sun—explained more of the observed movement of the planets than the hard core of the Ptolemaic. Soon after this, however, anomalies to the Copernican hard core were observed that required auxiliaries. Consequently, the Copernican theory started degenerating and was very quickly replaced with the Keplerian hard core that planets (other than the Moon) moved in elliptical, rather than circular, orbits around the sun.

C. Posner's Methodology in Practice

Although Posner claims to adopt a Friedmanesque methodology based on validation through prediction, he does not practice what he preaches.

1. Assumptions v. Predictions

The Friedman-Posner methodology is based on the distinction between the assumptions underlying a theory and the predictions (or implications) generated by the assumptions. As discussed above, some of Friedman’s critics have charged that his methodology is flawed precisely because he adopts the unsophisticated position that one can easily distinguish between assumptions and predictions and does not offer precise definitions of his terms. If Friedman’s attempt to distinguish between assumptions and predictions is problematic in theory, Posner’s is so in application. One can argue that Posner often labels as untestable assumptions of economic theory that are, in fact, at least partially predictions and should, by his own methodology, be tested. As Blaug points out, this is a sin frequently made by Friedman as well.

The basic assumption of economics that Friedman and Posner discuss is economic rationality—in the marketplace, consumers will act so as to maximize their utility and producers will act so as to maximize their profits. Friedman, and Posner, argue that this
assumption should be maintained regardless of its realism or truth because it has been shown to be a good predictor of market behavior.

Nevertheless, for all his talk about prediction, falsification, and testing, Friedman's argument in favor of the rationality postulate is notoriously lacking in empirical support. For example, Friedman claims that the "maximization of returns" hypothesis is supported by "an important body of evidence" culled from "countless applications of the hypothesis to specific problems and the repeated failure of its implications to be contradicted." Footnote 256 Blaug rightfully describes this as "without doubt the most frustrating passage in Friedman's entire essay because it is unaccompanied by even a single instance of these 'countless applications.'" Footnote 257 Friedman's defense that the "evidence is extremely hard to document [because] it is scattered" Footnote 258 seems half-hearted, at best.

According to Nobelist Gary Becker, however, when economists speak of the predictive power of the rationality postulate they usually have in mind the prediction of downward-sloping demand curves and upward-sloping supply curves. Footnote 259 Despite years of theoretical speculation and empirical research trying to identify meaningful exceptions to these phenomena the observations remains extremely robust. Footnote 260 That is, no economist has convincingly proven the existence of the phenomenon—"giffen goods," or goods with such snob appeal that demand actually increases with price—that would seemingly contradict (and potentially falsify) this prediction. Footnote 261

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Footnotes:

256 Friedman, supra note 12, at 22.
257 Blaug, supra note 18, at 101.
258 Friedman, supra note 12, at 22.
259 Becker claims to show "that negatively inclined market demand curves result not so much from rational behavior per se as from a general principle which includes a wide class of irrational behavior as well." Gary S. Becker, The Economic Approach to Human Behavior 156 (1976) [hereinafter Becker, Economic Approach]. Becker states: "Not only utility maximization but also many other decision rules, incorporating a wide variety of irrational behavior, lead to negatively inclined demand curves because of the effect of a change in prices on opportunities." Id. at 158 (citation omitted).
260 See id. at 156. Becker rhetorically asks, "How can these extensive criticisms [i.e., of the rationality postulate] be reconciled with the fact that the main implication of utility theory—that market demand curves would be negatively inclined—has been consistently verified empirically and found extremely useful in practical problems?" Id.

How can we convince a skeptic that this "law of demand" is really true of all consumers, all times, all commodities? ... Perhaps as persuasive a proof as is readily summarized is this: If an economist were to demonstrate its failure in a particular market at a particular time, he would be assured of immortality, professionally speaking, and rapid promotion while still alive. Since most economists would not dislike either reward, we may assume that the total absence of exceptions is not from lack of trying to find them.
And yet, Becker suggests, this is not necessarily a justification for continued reliance on the assumption of economic rationality. Becker shows that the phenomenon of downward-sloping demand curves does not require the assumption of economic rationality and is consistent with any number of other less controversial assumptions.262

In a footnote,263 Posner acknowledges Becker's analysis but seems to miss the point. Posner correctly states that Becker does not argue that downward-sloping demand curves are inconsistent with the rationality postulate or that most consumers are not in fact rational.264 I read Becker's point, however, to be that insofar as economists like Posner have traditionally tried to defend the hypothesis that individuals are economically rational (despite empirical evidence to the contrary), on the grounds that it accurately predicts downward-sloping demand curves, this defense is weak because other aspects of neoclassical economic theory can predict the same result.265 In Becker's words, the observation that markets act as if they were rational does not require the more controversial hypothesis that individual market participants are themselves rational actors.266 That is, the assumption of the rationality postulate arguably adds nothing to this analysis. Consequently, one cannot rely on the prediction and observation of downward-sloping demand curves as a justification for the rationality postulate.267

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262 See BECKER, ECONOMIC APPROACH, supra note 259, at 161 ("Hence the market would act as if 'it' were rational [i.e., demand curves would be negatively inclined] not only when households were rational, but also when they were inert, impulsive, or otherwise irrational.").

263 See Posner, Behavioral Economics, supra note 6, at 1556 n.11.

264 See id.

265 JST make a similar criticism of Posner's interpretation of Becker. See Jolls et al., Theories and Tropes, supra note 14, at 1598-99.

266 See supra note 260.

267 Becker is a neoclassical economist who does, of course, adopt his own variant of the rationality postulate, but does not seek to justify it by predicting demand curves. Rather, he claims that his "new home economics" is a falsifiable theory that can be tested through analysis of a variety of economic phenomena. Despite these claims, as Blaug notes, Becker commits the usual economist's sin of confusing falsification and verification: that is, he tries to predict and observe behavior that is consistent with his theory, rather than inconsistent. See BLAUG, supra note 18, at 226; Schroeder, Economic Rationality, supra note 8.
Friedman himself notoriously violates his principle that a scientific theory must specify "the circumstances for which [it] holds."268 As Blaug points out:

[H]aving introduced this important methodological clarification, Friedman immediately spoils the point by allowing the theory of perfect competition to apply to any firm whatsoever, depending on circumstances: "there is no inconsistency in regarding the same firm as if it were a perfect competitor for one problem and a monopolist for another."269

This problem is even greater in Posner's analysis than in traditional economics. He takes the empirically questionable assumption of economic rationality—which is not needed to explain the fundamental economic phenomenon of downward-sloping demand curves and is a poor predictor of other behavior—and applies it to any number of legal and social problems.270 As is well known, Posner takes this assumption of economic rationality and uses it to predict a wide variety of behavior, but in doing so, blurs the lines between assumptions and predictions. Specifically, the assumption of economic behavior was originally adopted in order to explain observed behavior in explicit markets. The next step, however, is to assert that consumers are economically rational not only in express markets, but in implicit markets as well. Is this a prediction, a new assumption, or an extension of the old assumption?

Some of Posner's language suggests the former. He concludes from the hypothesis that people act economically rational in market contexts, that we should expect them to act economically rational in other situations.271 This could be read as suggesting that we predict from the observation of economically rational behavior in markets that people will act economically rational elsewhere. If this is a prediction, then by Posner's own methodology he should not accept this prediction but test it.

If it is not a prediction, but just an extension of the original assumption, then he needs to make new predictions to test through falsification. Posner's claims that he does this ring hollow.

268 FRIEDMAN, supra, note 12, at 19.
269 BLAUG, supra note 18, at 93 (quoting FRIEDMAN, supra note 12, at 36).
270 For example, he states: "The basic assumption of economics...is instrumental rationality: the individual chooses the means that are most suitable...to his ends.... The choice of means need not be and often is not conscious.... There is...no paradox in supposing that sexual behavior...may fruitfully be modeled as rational." POSNER, OVERCOMING LAW, supra note 5, at 553.
271 See id. ("Since emotion and reason are not necessarily antagonistic, there is also no paradox in supposing that sexual behavior, despite the intense emotions that precede and accompany it, may fruitfully be modeled as rational.")
2. Abduction v. Induction (Falsification)

As I discuss in *Economic Rationality*, JST point out some facts that seem surprising under the neoclassical rationality postulate—for example, that people value the seemingly altruistic concept of fairness, or that people routinely make the sunk-cost fallacy. Posner then spins a yarn: Once upon a time, about five million years ago, maybe men lived in groups like such and such, and maybe their goals were such and such, and maybe it might make sense to do such and such based on this—and concludes that if this story that I just made up were true, then JST’s facts would no longer look surprising. He suggests that “[w]e need only imagine the kind of cognitive equipment that would be optimal in the prehistoric environment to which early man adapted . . .”

In other words, Posner’s methodology is remarkably like the one adopted by Rudyard Kipling when he wrote his *Just So Stories*. Three of these, *How the First Letter Was Written*, *How the Alphabet Was Made*, and *The Cat that Walked by Himself*, hypothesize about the origins of two fundamental bases of civilization—written language and the domestication of animals. In each story he purports to imagine what life might have been like for the earliest human families living millions of years ago. The result is delightfully amusing precisely because Kipling never meant for anyone to take his fables to be an accurate account of primitive society—his Cro-Magnons are immediately recognizable as a proper middle-class Edwardian family who just happens to live in a cave and wear animal skins.

In contrast, Posner offers his flights of fancy as descriptions of what our ancestors might actually have been like and offers these descriptions as an explanation of modern behavior. Despite his stated commitment to falsification, Posner does not, however, even suggest that he now has the responsibility of demonstrating how this fanciful story of his could be falsified. At most, he offers some speculation as to how it could be verified.

Posner asserts, for example, that he was able to use evolutionary biology “to predict [more accurately] than behavioral economics as conceived by JST” such phenomena as “fairness,” “cognitive quirks,” and “weakness of will.” In fact, Posner does no such thing. He does not start with the theory of evolutionary biology, predict certain forms of behavior, and then look for empirical data that would either falsify or be consistent with his

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274 *Id.* At this stage, Posner does admit that the second two of this triad may not be predictable by traditional rational choice theory alone. See *id.*
predictions. Rather, he takes JST's data (which is surprising under traditional neoclassical rationality theory) and abducts that it might be consistent with some of the hypotheses of evolutionary biological theory. Posner's judgment that evolutionary theory has been successful in other areas may or may not be a "good reason" for Posner to think that his abduction will prove fruitful. It certainly is not, however, proof of the hypothesis abducted. Under Posner's own stated methodology, it is now that Posner needs to make further predictions of human behavior based on his abductions and engage in controlled observations (either of future or past behavior—social science research can be done by historical studies as well as forward-looking ones) to try to falsify it.

Instead, Posner vaguely refers to the fact that rational choice theory has been a good predictor in the past. But this is not a response to JST's assertion that they have made observations that not only were not predicted by the theory but also seem inconsistent with it. That is, behavioral economists think that their evidence indicates precisely that the rationality postulate is a poor predictor of economic behavior.

3. Verification v. Falsification

Posner claims to be following the standard scientific methodology of falsification. In fact, an examination of his application of his methodology shows that he chooses "the easier option of verificationism."275 In Blaug's words:

[W]e begin with the available evidence about human behavior in areas traditionally neglected by economics and then congratulate ourselves that we have accounted for it by nothing more than the application of standard economic logic. . . . Moreover, we hail the economic approach as superior to any available alternative, but we restrict the scope of comparison to our own advantage and we never in fact specify the alternative approaches that we have in mind. Clearly, if these are the rules of the game, we simply cannot lose.276

In other words, Posnerian theory exploits the rhetoric of scientific methodology without adhering to any of its substantive requirements.

An excellent illustration of this is in the introductory, methodological section of Posner's perhaps most controversial book, Sex and Reason.277 In this curious work, Posner purports to offer

275 BLAUG, supra note 18, at 226.
276 Id. In this passage, Blaug is speaking of Becker, another economist who claims to adopt the methodology of falsification yet actually applies that of verification. See id.
277 POSNER, SEX AND REASON 1-12 (1992) [hereinafter POSNER, SEX AND REASON].
economic explanations for, and make legal policy suggestions concerning, a wide variety of sexual laws, customs, and practices. Not surprisingly, as is so often the case, Posner’s single-minded approach just happens to defend the status quo as being almost inevitable. This is probably the inevitable result of his flawed methodology.

Before I go further, I must emphasize that I am not opposed to the proposition that economics can meaningfully add to our understanding of sexuality. Indeed, I have argued extensively elsewhere that Posner’s intuition that sex and economics are linked is consistent with psychoanalytic theory, which holds that both are forms of eroticism.278 This theory, however, suggests that Posner is incorrect in concluding that an analysis of sexuality can be reduced to economics. Rather than sexuality being subsumed into economics, psychoanalysis suggests that markets are one of the simplest, most primitive forms of eroticism. This suggests that we might learn something about erotics by studying markets as its simple form, but that the entirety of complex forms of erotics such as sexuality and love cannot be reduced to the simple model. If, as economists posit, actual markets are characterized by the maximization of enjoyment through instrumental ends-means reasoning, and if economic subjects respond to incentives and disincentives, then we might also expect to see similar behavior in the more obvious erotic relationships of sexuality, as Posner suggests. However, psychoanalysis also posits that erotic subjects often engage both in economically “irrational” behavior calculated either to frustrate enjoyment or bring about pain and as well as in philosophically rational speculation designed to determine the proper ends to pursue, rather than the means to a preexisting end. Consequently, although some erotic behavior may be predictable, eroticism by its very nature contains moments of spontaneity inconsistent with Posnerian prediction. I discuss this at greater length in Economic Rationality.279

Posner states, conventionally, that if his economic analysis of sexuality is to have scientific validity, then it must be able to be subjected to falsification. Posner, however, misstates what this test is. He states: “Another [test of a theory] is its power to generate counterintuitive (hence novel, nontrivial, nonobvious) hypotheses

279 Schroeder, Economic Rationality, supra note 8.
that can be tested empirically and that do not flunk the test. A number of such hypotheses are proposed in this book...  

Posner is correct that a falsifiable theory is one that generates predictions of previously unobserved phenomena. Posner suggests, however, that after making such predictions, the scientist then goes out into the world (or into the lab) and seeks to find examples of the predicted phenomena. This is the easier process of verification—the search for phenomena that are consistent with one's theory.  

Falsification, in contrast, consists of determining what previously unobserved phenomena, were it to exist, would be inconsistent with the hypothesis. The scientist must then try to devise a mode of controlled observation that would enable one to determine whether inconsistent phenomena exist. Under falsification, a hypothesis is never, of course, definitively proved but always remains both fallible and corrigible. Nevertheless, if after repeated attempts scientists fail to find inconsistent phenomena, then we are justified in remaining committed (if only conditionally so) to the theory as being not falsified. If, on the contrary, an apparently inconsistent phenomenon is in fact observed, then the scientist must determine whether his theory is thereby falsified or, under the methodology of sophisticated falsification, whether the hard core of the theory can be protected by the addition of a protective belt of auxiliaries. Accordingly:  

A theory is to be called “empirical” or “falsifiable” if it divides the class of all possible basic statements unambiguously into the following two non-empty subclasses. First, the class of all those basic statements with which it is inconsistent (or which it rules out, or prohibits): we call this the class of the potential falsifiers of the theory; and secondly, the class of those basic statements which it does not contradict (or which it “permits”).

In contrast, when Posner makes predictions, he only identifies the second class of statements that are consistent with the hypothesis. But, according to Popper, “[a]bout the ‘permitted’ basic statements [a scientific theory] says nothing. In particular, it does not say that they are true.” In other words, the fact that a theory accurately predicts certain facts does not prove the theory. Although Posner claims that his theory is falsifiable, his examples do not demonstrate that it is. In Popper’s words, “a theory is falsifiable if the class of its potential falsifiers is not empty.” As I shall discuss shortly, the

280 POSNER, SEX AND REASON, supra note 277, at 5-6.  
281 POPPER, SCIENTIFIC DISCOVERY, supra note 33, at 86.  
282 Id. (citation omitted).  
283 Id.
class of statements that could potentially falsify Posner’s theory of sex is indeed empty.

It is not enough, as Posner implies, that a theory be merely falsifiable. Identifying falsifiability is only the first step in testing a hypothesis. One must actually try to falsify it. A theory is falsified “only if we discover a reproducible effect which refutes the theory.”

The methodology of falsification is the search for such a refuting effect, not the search for a confirming one. In a passage that could have been written with Posner in mind, Popper states:

The fundamental difference between my approach and the approach for which I long ago introduced the label “inductivist” is that I lay stress on negative arguments, such as negative instances or counter-examples, refutations, and attempted refutations—in short, criticism—while the inductivist lays stress on “positive instances,” from which he draws “non-demonstrative inferences,” and which he hopes will guarantee the “reliability” of the conclusions of these inferences.

He continues:

In my view, all that can possibly be “positive” in our scientific knowledge is positive only in so far as certain theories are, at a certain moment of time, preferred to others in the light of our critical discussion which consists of attempted refutations, including empirical tests. Thus even what may be called “positive” is so only with respect to negative methods.

Blaug explains the Popperian method thus: “[T]here is an asymmetry between verification and falsification. From a strictly logical point of view, we can never assert that a hypothesis is necessarily true because it agrees with the facts ….” In other words, in verification, one predicts and looks for data consistent with one’s theory. In falsification, one predicts and looks for data inconsistent with one’s theory. In verification, one feels satisfied when one finds consistent data. In falsification, one is merely temporarily and contingently encouraged by the failure to find inconsistent data.

Consequently, Posner is incorrect to argue that his theory is falsifiable because it is consistent with certain facts that he predicts exist in the world. On the other hand, Blaug states, “we can deny the truth of a hypothesis with reference to the facts … [T]here is no logic of proof but there is logic of disproof.”

284 Id.
285 POPPER, OBJECTIVE KNOWLEDGE, supra note 30, at 20.
286 Id.
287 BLAUG, supra note 18, at 15.
288 Id.
I would suggest that the implicit reason why Posner applies the methodology of verification to his analysis of sexuality (albeit incorrectly labeled as falsification) is that, in fact, his analysis is ad hoc and unfalsifiable.\(^{289}\) Indeed, reading his analysis, I cannot imagine any sexual behavior that could not be explained through an abduction based on his starting "assumptions."

In other words, Posner is guilty of the economist’s sin of preaching falsification but practicing, at best, an "innocuous falsification"\(^{290}\) designed not to challenge and test economic theory, but to protect it from attack.

4. AIDS and Birthrates

For example, in *Sex and Reason*, Posner, writing in 1992, predicted that the AIDS epidemic should result in a reduction in the rate of births to unmarried women.\(^{291}\) This is because a rational cost-benefit analysis would both encourage people to be less promiscuous and encourage people who do choose to have sex to use condoms in relatively "casual" sexual encounters (i.e., sexual relations with persons with whom one does not have a long sexual history or commitment, such as in marriage of other long-term, stable relationships).\(^{292}\)

This prediction of less promiscuity and increased use of condoms is typical of the type of modest contributions economic analysis can make to our understanding of sexual behavior. But this contribution is modest precisely because this type of analysis is hardly new, or unique to economics.\(^{293}\) Indeed, sexual traditionalists have always argued that the harmful potential effects of nonmarital sexuality (for example, through the free availability of contraception) should not be ameliorated precisely because they will

\(^{289}\) See Hovenkamp, *supra* note 23, at 823-24. Hovenkamp suggests: Often . . . the positivist economist relies on theory rather than observation to reach her conclusion about the economic effects of the legal rule. For example, no one has proved by rigorous attempts at falsification that the common law has tended toward efficiency . . . . Actual attempts at falsification play only a small part of economic positivism, even in easy cases.

\(^{290}\) BLAUG, *supra* note 18, at 111.

\(^{291}\) See POSNER, *SEX AND REASON*, *supra* note 277, at 6, 115.

\(^{292}\) See id.

\(^{293}\) As Leiter has accurately stated, the few predictions that economics does successfully make are mundane and banal, at best: "[S]urely we do not need economics—or any putative social science" to draw such simplistic, common-sense conclusions. Leiter, *supra* note 179, at 307 (citation omitted) (referring specifically, in this case, to "Posner's claim that 'an increase in the severity of punishment will (ceteris paribus) reduce the amount of crime'".)
discourage promiscuity. It is the broader prediction that is problematic.

What if, in fact, the rate of nonmarital births increased, rather than decreased, during the height of the AIDS epidemic? Would this falsify Posner's economic analysis of sexuality? Of course not. One could argue that the fear of AIDS encouraged people, rationally, to use condoms. Condoms, of course, have two distinct purposes: protection from disease and contraception. If people started using condoms for the first purpose (protection from disease), one might expect people also to use them for the latter (contraception). Consequently, women who start using condoms might also be expected to stop using other forms of contraception, such as diaphragms and birth control pills. Since condoms are not as reliable a form of birth control as these other two forms, one might expect the birth rate to go up in this population of women.

And, lo and behold, Posner writing in 1996 and 1999 makes precisely this alternative prediction that birth rates will go up and declares it a victory of rational choice theory over behavioral economics. He does not mention his earlier prediction let alone explain why the data do not falsify his theory.

In other words, under Posner's analysis, the existence of the AIDS epidemic is equally consistent with an increase or a decrease in the nonmarital birth rate. The result depends on conditions that are beyond the analysis—i.e., the relative number of women who before the epidemic used no contraception who have been encouraged to use condoms as compared to the number of women who before the epidemic were using another more effective form of contraception who have been encouraged to switch to condoms.

In fact, out-of-wedlock births as a percentage of all births increased throughout the eighties and early nineties—the height of concern over the AIDS epidemic—and began decreasing in the late nineties—after more successful treatments of AIDS with protease inhibitors were introduced. No doubt this neither falsifies nor

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294 This is not merely a traditional Christian analysis. Japan only approved the birth control pill in 1999, and only after protests from feminists against its earlier approval of Viagra. The long delay in the approval process for oral contraceptives is, no doubt, overdetermined. Nevertheless, as reported in the American press, one stated reason by at least some part of the Japanese polity was the usual argument that “the pill would encourage promiscuity and reduce reliance on condoms, leading to the spread of AIDS and other sexually transmitted diseases.” Sheryl WuDunn, Japan, Never on the Pill, Seems Ready to Try It, N.Y. TIMES, June 3, 1999, at A13.

295 See Posner, Behavioral Economics, supra note 6, at 1557 (citing Tomas J. Philipson & Richard A. Posner, Sexual Behaviour, Disease, and Fertility Risk, 1 RISK DECISION & POL'Y 91 (1996)).

verifies either of Posner's two inconsistent predictions, because these statistics might just as well be due to wholly different factors, such as the aging of the population, the healthy economy, and the availability of several new forms of contraception.

5. Sophisticated Falsification Redux

Even if we accept Posner's claims to be able to distinguish between his assumptions and predictions, and to be engaging in falsification rather than verification, Posner is still not following his methodology. He claims that the "realism," or even the literal truth, of his assumptions is irrelevant. Nevertheless, when confronted with evidence that seems inconsistent with his thesis, he, on the one hand, vigorously defends the truth of his hypothesis and asserts the falsity of the alternative and, on the other hand, refuses to find his theory to be falsified. Rather, he engages in a form of ad hoc argument that can be seen as a muddled and flawed attempt at sophisticated falsification.

As I have shown in *Economic Rationality*, when confronted by evidence that seems anomalous to his theory, Posner does not, as Lakatos requires, formulate auxiliary hypotheses to protect his hard core while accounting for the anomalies. Nor does Posner purport to do what Friedman suggests—continue to apply his theory, despite the fact that the conditions of its application might not be met on the grounds that people act "as if" they were rational. Rather, Posner does precisely what he claims he should not do. He rejects his starting assumptions concerning economic rationality and adopts a series of new assumptions concerning human behavior. The only thing that remains the same about Posner's assumptions is that he continues to label them "rationality" even as the substance of the assumptions changes radically!

Indeed, he does not engage in the process of prediction followed by attempted falsification at all. Posner repeats Friedman's claim that the rationality postulate has been shown to be a good predictor of economic activity and stops with that. There are several problems with this—in addition to the obvious one that Posner does not specify what these studies might be.

As I show in *Economic Rationality*, Posner no longer uses, if in fact he ever did, Friedman's assumption of economic rationality. In other words, whether or not empirical investigation has shown that the theory that economic actors act as if they tried to maximize

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298 This charge is frequently made against Friedman as well. A full discussion is beyond the scope of this Article.
their utility based on a well-defined set of preferences and perfect costless information has been a good predictor of market behavior, this same evidence does not support the theory that economic actors act “as if” they were constituted by a warring set of competing subselves, each with its own preferences, or “as if” their genes programmed them in a way that would have been beneficial out in the African plains five million years ago. Once Posner changes his theory (by changing his assumptions), Friedman’s methodology requires that Posner make new predictions, which he would then seek to falsify through controlled observation of previously unobserved data, not by abduction and anecdote.

Many economists dispute the empirical claim about the predictive power of the rationality postulate. This dispute comes in two forms. The most common is the relatively simple assertion that whether or not the rationality postulate has been successful in making certain predictions, it does not accurately predict a wide variety of other economic activity—particularly consumer behavior. Indeed, Blaug questions why so many economists have resisted any critique of the classic rationality postulate given both the observation of anomalies and the existence of alternative theories of economic behavior. 300

Surely Posner must be aware that Simon, probably the foremost proponent of behavioral economics, began his empirical studies precisely because he believed that the traditional theory of economic rationality was an extremely poor predictor of firm behavior. 301 He sought to understand how people actually make decisions not merely to describe them, but also to develop a theory that would more accurately predict behavior and provide the basis for making recommendations about human behavior. And, indeed, Simon offers a theory that combines the concepts of “bounded rationality” and “satisficing” as an alternative to the neoclassical theory of full rationality and maximization. 302

Indeed, the entire behavioral economics movement that Posner damns as untheoretic can be interpreted as showing that the traditional rationality postulate indeed fails to predict consumer

300 See BLAUG, supra note 18, at 233. Blaug states:
We conclude that the classic defense against criticisms of the rationality postulate carries less conviction today than it did in the past. But so what?... We do not discard a research program simply because it is subject to “anomalies” unless an alternative research program is available. However, such alternatives are in fact available....

Id.

301 See supra text accompanying note 52.
302 See BLAUG, supra note 18, at 233. Blaug describes Simon’s theory as an alternate to the classical rationality postulate that can be “described as a non-fully-rational theory of individual action under both certainty and uncertainty.” Id.
behavior (such as the persistence of the sunk-cost fallacy), and the attempt either to develop auxiliary hypotheses to explain these aberrations, or to abduct a new “revolutionary” paradigm of economic behavior, which would then need to be tested further.

Posner does not study economic theory in order to further our knowledge of economics. Rather, he purports to be using economic theory to justify a legal regime consistent with a politically conservative point of view. This justification depends on Posner’s assertion that legal subjects act as economic actors not only in explicit markets, but in all (or almost all) human interactions, which are reinterpreted as shadow or implicit markets. The assertion that a vision of “economic rationality” predicts pricing in markets is used to argue that the same vision of economic rationality will also explain sexual, political, and other activity and, therefore, the law should be such and such. The just so story of Posnerian jurisprudence does not merely try to justify the status quo, as did Kipling’s. It aims also to justify changing the world in accordance with Posner’s personal ideology.

Finally, even if Posner were consistent in his assumptions about economic rationality and if he were also correct that economic rationality has been shown to be an excellent predictor of a wide variety of economic behavior, he still would not be a consistent follower of Friedmaniatical methodology. To repeat: Falsification theory holds that a theory is never finally proved, but always remains corrigible and fallible. When he suggests appropriate legal regimes, Posner engages in explicit and implicit predictions of past and future legal behavior by legal subjects. A truly scientific methodology would require Posner and his followers to then make observations in order to test these predictions. Not only does Posner not do so, he condemns JST’s attempt to do so as being unscientific and untheoretical.

So that’s all right, Best Beloved. Do you see?  

303 KIPLING, supra note 1, at 109 (emphasis added).