

# Untying Cerberus: A Gatekeeper's Guide to Economic Evidence

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**Untying Cerberus:  
A Gatekeeper’s Guide to Economic Evidence**

*Michael C.A. Stork*

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## Introduction

...[Alice] went on. “Would you tell me, please, which way I ought to go from here?”  
“That depends a good deal on where you want to get to,” said the Cat.<sup>1</sup>

In *Daubert v. Merrill Dow Pharmaceuticals*,<sup>2</sup> the Supreme Court ruled that judges are the gatekeepers of scientific evidence, thereby bringing the debate about economic methodology to the bench. Debate about the admissibility of scientific evidence, contentious even in the natural sciences, is amplified if the discipline incorporates numerous methodological approaches. In this paper, I will consider three different approaches to economic questions—theory, experiment, and econometrics—and examine how a judge can evaluate these approaches as evidence in the courtroom. The expansion of economic reasoning in law means that this question needs to be answered in a number of areas of law, but to give a thorough examination of the different methodological approaches, this paper will limit discussion to economic evidence in tying law.

An examination of economic methodology in law is distinct from discussions of general economic methodology for a number of reasons. First, while methodological eclecticism may be acceptable within academia, the use of different methodologies is incompatible in the courtroom. As shown in Part II, the three approaches considered in this paper often produce different conclusions. A judge, therefore, cannot give each approach equal weight; in order to reach a conclusion, the judge will need to prefer one type of evidence over the others. Second, although academia can tolerate methodological pluralism, conclusiveness is essential in law. The need for finality in judicial rulings means that a judge cannot wait for further research before ruling in a case: the tying arrangement must either be condoned or condemned. In sum, judges are required to make decisions about what constitutes economic evidence in tying cases. This paper will examine that decision in order to understand how judges can evaluate different types of economic evidence.

In Part I, I will explain what judges are considering when ruling on tying cases, and why economics is useful in tying law. The purpose of establishing a philosophical framework in Part I is to provide a starting point from which judges can consider the different methodological approaches. Part II details the three different methodological approaches, explaining why they are distinct from one another and how they apply to tying law. In Part III and Part IV, I present my conclusions about the preferable methodological approach. Part III argues that theoretical evidence is inappropriate in tying law, drawing both on the current legal standards for scientific evidence and a defense of empiricism in science. Finally, in Part IV, I argue that, in tying cases, econometric evidence is more externally valid than experimental evidence, thereby completing a rationale for judges to choose among these three approaches to economic evidence in tying law.

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<sup>1</sup> Lewis Carroll, *Alice in Wonderland*, 66 (Dover Publications, Inc. 1993).

<sup>2</sup> *Daubert v. Merrell Dow Pharmeceuticals, Inc.*, 509 U.S. 579 (1993).

## I. Consequentialism in Law & Economics

*There is another powerful and great cause of the little advancement of the sciences, which is this: it is impossible to advance properly in the course when the goal is not properly fixed.*

-Sir Francis Bacon<sup>3</sup>

In order to examine how judges make their decisions in tying cases, it is necessary to understand the statutes that regulate tying, as well as how the courts have applied these statutes over the past century. As with most antitrust laws, the Sherman Act and the Clayton Act are the two main statutes concerning tying. Section 3 of the Clayton Act applies only to the tying of commodities, so any cases involving tied services must be brought under Section 1 of the Sherman Act.<sup>4</sup> The term *bundling* is closely related to tying, but bundling refers to the sale of goods or services in fixed proportions. Bundling, therefore, is a subset of tying. Although tying and bundling are different terms, the distinction is unimportant in this paper. The terms have also been used interchangeably by the courts on some occasions.<sup>5</sup>

The way courts have handled tying cases has changed considerably since the early 20<sup>th</sup> century. This change is especially important because the creation of antitrust legal theory is largely a product of the courts, not the legislatures.<sup>6</sup> In the first half of the century, judges generally ruled that tying arrangements were per se illegal, meaning that “their pernicious effect on competition and lack of any redeeming virtue are conclusively presumed to be unreasonable.”<sup>7</sup> Under the classic leverage theory, tying was considered a way for firms with considerable market power to block the entry of new competitors, or for firms to leverage their power from one market to another.<sup>8</sup> This reasoning came under scrutiny beginning in the 1950s, when economists and lawyers began applying economic theory to tying cases. Using the lens of price theory “led the economists and lawyers of the ‘Chicago School’ to the view that there is no economic basis for concern with the exclusionary practices [bundling and tying].”<sup>9</sup> Advocates of the Chicago School approach argued that tying allows firms to price discriminate or lower production costs, both of which benefit consumers.

By the mid-1980s, the Supreme Court was using the rule of reason, not the per se rule, to evaluate tying cases. Using the rule of reason, tying arrangements must be proven to unreasonably restrain trade in order to warrant intervention. “[W]hile the Court has spoken

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<sup>3</sup> Francis Bacon, *Novum Organum*, I, 80. Cited in Mortimer J. Adler and Charles van Doren (ed.), *Great Treasury of Western Thought*, 1110 (R.R. Bowker Company, 1977).

<sup>4</sup> The American Bar Association: Section of Antitrust Law, *Antitrust Law and Economics of Product Distribution*, 217 (American Bar Association, 2006).

<sup>5</sup> Alden F. Abbott and Joshua D. Wright, “Antitrust Analysis of Tying Arrangements and Exclusive Dealing,” 3, in Keith N. Hylton, (ed.), *Antitrust Law and Economics* (Edward Elgar Publishing, 2010).

<sup>6</sup> Robert Bork, *The Antitrust Paradox: A Policy at War with Itself*, 72 (Free Press, 1993).

<sup>7</sup> *Northern Pacific Railway Co. v. United States*, 356 U.S. 1, 5. Cited in Wright, 3.

<sup>8</sup> The American Bar Association, *Antitrust Law and Economics of Product Distribution*, 190.

<sup>9</sup> Richard A. Posner, *Antitrust Law*, 6 (University of Chicago Press, 1978).

of a *per se* rule against tying arrangements, it has also recognized that tying may have pro-competitive justifications that make it inappropriate to condemn without considerable market analysis.”<sup>10</sup> This newly established emphasis on thorough market analysis is another reason why this paper’s evaluation of economic evidence is necessary. The courts have made it clear that rulings in tying cases should be based on sound economic evidence, but, as this paper will show, economic evidence is not uniform. Theoretical, experimental, and empirical research have different methodologies and distinct advantages, so a careful evaluation of economic evidence is essential for effective rulings in tying cases.

An examination of the judicial decision-making process is essential before choosing among the different types of economic evidence. This process can be analyzed by answering two simple questions: 1) what is a judge’s objective? and 2) how can a judge best achieve that objective? While these questions about the adjudication process apply to any area of law, they are especially relevant to tying cases, because antitrust law is inordinately affected by the judiciary. Judge Richard Posner argues, “American antitrust law is far more the creation of judicial decisions than of antitrust legislation: the most important antitrust laws are as skimpy and vague as most provisions of the Bill of Rights. We ought therefore to be interested in how antitrust law has been shaped by the motivations, constraints, and other influences that play on judges.”<sup>11</sup> Because the judiciary has largely created and applied antitrust law, this paper will focus on how tying cases are handled by the judges, with little reference to legislatures.

Before evaluating the three types of economic evidence, I need to defend an answer to the two questions about judicial decision-making. First, judges should adhere to “legal pragmatism” with regard to antitrust law, meaning that a judge’s objective is to create the best consequences with every ruling. Second, judges should be “instrumentalists” when they reference economics in the courtroom, meaning that economics should be used as a tool to create good outcomes. These two approaches—legal pragmatism and instrumentalism—are closely related, so that there is consistent answer to the two main questions about the adjudication process.<sup>12</sup> Together, they constitute a consequentialist framework, meaning that they evaluate choices based on the consequences of the choices in question.

### A. Legal Pragmatism

Put simply, legal pragmatism means that a judge is more focused on the future than on the past. A legal pragmatist’s ruling will be designed to generate the best consequences—weighing both short-term and long-term effects—which may or may not be

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<sup>10</sup> *National Collegiate Athletic Association v. Board of Regents of the University of Oklahoma*, 468 U.S. 85, 104 n.26 (1984). Cited in Wright, 4.

<sup>11</sup> Posner, *How Judges Think*, 5 (Harvard University Press, 2008).

<sup>12</sup> Legal pragmatism is closely related to an instrumental interpretation of law.

consistent with judicial precedent or legislative statutes. “[P]ragmatist judges always try to do the best they can do for the present and the future, unchecked by any felt *duty* to secure consistency in principle with what other officials have done in the past.”<sup>13</sup> Legal pragmatism, therefore, sees judges as less mechanical than other types of jurisprudence. Judges cannot simply apply the law in many cases—often the statute is unclear or past rulings had undesirable consequences—so a pragmatic judge is more willing to acknowledge the need for a judicial intervention. Legal pragmatism can also be understood by contrasting it with legal formalism, which posits that rulings can be based on a priori rules and logical reasoning, while maintaining a commitment to precedent.<sup>14</sup> Legal formalism—associated with the idea that judges should apply law and not make it—may in fact be closer to the average citizen’s conception of an objective, independent judiciary. As Judge Posner notes, “Formalism is the official jurisprudence of lawyers and laypeople alike.”<sup>15</sup> Given this tension between legal pragmatism and legal formalism, why should judges prefer pragmatism?

Within the scope of tying law, there are three reasons to prefer legal pragmatism. First, the Sherman Act and the Clayton Act were not based on modern economic reasoning. Robert Bork writes, “What *is* true is that our ideas are old; they carry whatever credentials time alone can confer. The years 1890 to 1914 witnessed the origin of every major theory that drives and directs the evolution of antitrust doctrine to this day...But it is *not* true, as we trustingly assume, that these ideas were ever demonstrated theoretically or confirmed empirically.”<sup>16</sup> A thorough legislative history is beyond the scope of this paper, but it is clear that economics as a discipline was just emerging when the statutes were written. For example, the Sherman Act was passed in 1890, the same year that Alfred Marshall’s *Principles of Economics* was first published. Given this paper’s subject matter, it is also important to note that Edward Chamberlain did not conduct the first economic experiments until 1948, and econometric techniques were not firmly established until the 1940s.<sup>17</sup>

Given such inauspicious origins, the antitrust statutes are best handled by legal pragmatism, because attempts to apply the statutes according to their original intent would be counterproductive; such formalism may harm both consumers and producers. This is especially true for tying cases, because the statutes and judicial precedent exhibit a “historical distrust of tying arrangements.”<sup>18</sup> Judges, and even economists, had a limited knowledge about the effect of tying in the late 19<sup>th</sup> century and early 20<sup>th</sup> century. The development of tying within academia, from the classic leverage theory to the Chicago

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<sup>13</sup> Posner, *The Problematics of Moral and Legal Theory*, 241 (Belknap Press of Harvard University, 2002).

<sup>14</sup> Originalism, a jurisprudence often associated with Justice Antonin Scalia and former Judge Robert Bork, is a type of formalism that emphasizes a literal interpretation of the law based on its original intent.

<sup>15</sup> Louis Menand, (ed.), *Pragmatism: A Reader*, 419 (Vintage Books, 1997).

<sup>16</sup> Bork, 16.

<sup>17</sup> Mary S. Morgan, *The History of Econometric Ideas*, 2 (Cambridge University Press, 1991).

<sup>18</sup> *Illinois Tool Works Inc. v. Independent Ink, Inc.*, 126 S. Ct. 1281, 1288 (2006).

School and beyond, demonstrates a flaw with legal formalism: simply applying the statutes limits judges to a static level of knowledge. The inevitability of change within a discipline, not only in economics, but also the natural sciences, is well-established.<sup>19</sup> Since our knowledge about the effects of tying will almost certainly change, it is essential to endorse a jurisprudence that can incorporate such change. Legal pragmatism allows judges to include incorporate the dynamism of scientific progress, which will improve the efficacy of judicial rulings.

The second reason to prefer legal pragmatism in tying cases is that the antitrust statutes require significant interpretation. Ambiguous legislation cannot simply be applied, so it is best to acknowledge that a judge must act as lawmaker in tying cases. “Even in statutory fields of law, courts have obligations other than the mechanical translation of legislative will, and these obligations are particularly important with statutes as open-textured as the antitrust laws.”<sup>20</sup> The ambiguity of the antitrust statutes require significant judicial interpretation, maybe to the point of considering the judge a lawmaker, all of which is consistent with legal pragmatism.

The third reason to prefer legal pragmatism is that, within the scope of tying law, it avoids the fundamental criticism of legal pragmatism and consequentialism in general. The major objection to legal pragmatism is that it will lead to inconsistent, arbitrary rulings because there is no consensus about the best outcome in a case. While this may be a valid criticism for legal pragmatism in other areas of law, this critique does not apply to tying law. As antitrust law became more grounded in economics in the second half of the 20<sup>th</sup> century, allocative efficiency became the goal of antitrust laws.<sup>21</sup>

This unanimity about maximizing allocative efficiency, however, does not mean that judges can act as machines. Although judges agree about the goal, and although they agree about the legal test (e.g. using the rule of reason) there is still “no single analytical approach followed by the courts in applying the rule of reason to tying claims.”<sup>22</sup> Having allocative efficiency as a goal, however, does allow judges to avoid the difficult issue of disputing best outcomes. Given the scope of this paper, legal pragmatism also has a fourth advantage: an economist can examine a legal issue like tying without becoming entangled with the type of thorough legal research that legal formalism demands.<sup>23</sup>

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<sup>19</sup> See Part III, Section B. Cited in note 115.

<sup>20</sup> Bork, 72.

<sup>21</sup> Posner, *The Problematics of Moral and Legal Theory*, 229. The distinction between allocative efficiency and consumer welfare as the goal of antitrust laws is important, though not for this paper. What matters is that there is a large degree of consensus about judicial goals in tying cases. For further reading on allocative efficiency, see Dennis W. Carlton, “Does Antitrust Need to Be Modernized,” *The Journal of Economic Perspectives*, Vol. 21, No. 3, 156 and Alison Jones and Brenda Sufrin, *EC Competition Law*, 3 (Oxford University Press, 2008). For reading on consumer welfare, see Bork, 7, and Geoffrey A. Manne and Joshua D. Wright, “A First Principles Approach to Antitrust Enforcement in the Agricultural Industry,” *The CPI Antitrust Journal*, April 2010, 3.

<sup>22</sup> The American Bar Association, *Antitrust Law and Economics of Product Distribution*, 218.

<sup>23</sup> This is not to say that a pragmatic judge needs no legal training. But if the effects of tying are the main concern, then the subject can be reasonably explored without extensive historical research.

Even in recent tying decisions, it is clear that there is tension between legal formalism and legal pragmatism. While judges continue to formally label tying practices as per se illegal, the courts' rulings are often based on the rule of reason.<sup>24</sup> This shows that judges feel compelled to acknowledge precedent, yet understand that precedent may produce undesirable outcomes. Before evaluating economic evidence, judges must have some idea about why such evidence is useful and understand they are trying to achieve. This paper defends using legal pragmatism in tying cases because legal pragmatism is concerned with predictions, not precedent. This focus on prediction is also a central tenet of *economic instrumentalism* which is the approach that answers the second main question: how can a judge pursue the goal of maximizing allocative efficiency?

## B. Economic Instrumentalism

The conflict between instrumentalism and realism is common in both law and economics. In this paper, I will limit this extensive debate by looking only at how the discussion applies to judges in tying cases. I will argue that a judge should use economics when ruling on a tying case because economics is a useful tool for understanding the consequences of tying arrangements. Although a judicial ruling—whether condemning or permitting tying—is assured in every case, each ruling is based on the predicted effects of the tying practice in question. A judge deals with this uncertainty by using economic evidence as a predictive instrument for measuring expected allocative efficiency.

Instrumentalism means that ideas are used as tools for problem solving. “[T]heories are neither true nor false (instruments are not true or false) but more or less adequate, given a particular problem.”<sup>25</sup> As with legal pragmatism, it is also useful to define instrumentalism through contrast. Instrumentalists are unconcerned with whether theories correspond to reality, while scientific realists argue that true theories match objective reality. That is, realists argue that theories are more than useful tools; theories describe how the world actually works. Realists, therefore, set a higher standard for economic evidence. “[I]f we wish our theories to be true as well as predictively adequate, we cannot rely solely on predictive adequacy for evaluating them.”<sup>26</sup> Realists are also more likely to demand that economic evidence have both explanatory and predictive power.

While economic instrumentalism is common, most notably articulated by Milton Friedman in “The Methodology of Positive Economics,”<sup>27</sup> it has also been heavily

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<sup>24</sup> Wright, 3.

<sup>25</sup> Bruce Caldwell, *Beyond Positivism: Economic Methodology in the Twentieth Century*, 178 (Routledge, 1994).

<sup>26</sup> Caldwell, 182.

<sup>27</sup> Although Friedman did not use the term “instrumentalist,” he agreed with such a characterization of his article. See Caldwell, 178. Not surprisingly, Friedman’s views are also propounded by two of the leading antitrust scholars from the University of Chicago: see Richard A. Posner, *Economic Analysis of the Law* (Aspen Publishers, 1998), and Robert Bork, *The Antitrust Paradox*.



criticized.<sup>28</sup> For the purpose of this paper, the most relevant criticism concerns Friedman's emphasis on predictive accuracy. Bruce Caldwell argues, "Philosophers of science since the 1940s have been unanimous in their rejection of the notion that the only goal of science is prediction."<sup>29</sup> Caldwell also notes, however, that instrumentalism is a viable approach in cases where prediction is the primary goal.<sup>30</sup> To defend instrumentalism in tying law, therefore, I only need to show that judges are concerned with predictions, not explanations.

Since there is widespread agreement that antitrust judges should try to maximize allocative efficiency, judges are expected to make predictions—not explanations—about the effects of tying arrangements. Instrumentalism, though unfit for all economists, is still the best philosophy for judges in tying cases. In this way, my claim is less ambitious than Friedman's: instead of arguing that instrumentalism is the correct approach for economists in general, I am arguing that instrumentalism is the correct lens through which judges should view economic evidence in tying cases.

Judges should adhere to instrumentalism because it satisfies a judge's needs without overreaching. Judges should not care whether economic evidence is an accurate representation of reality; they should care whether the evidence provides good predictions about the tying arrangement. The law, like an economic model, can be treated like a black box: as long as it generates good outcomes, it is a reliable tool. In tying cases, an instrumental judge will evaluate the economic evidence based on how useful it is. Since the goal of antitrust law is to maximize allocative efficiency, this means preferring the type of evidence that makes the best predictions about the effects of tying. This does not curtail the scope of economics as a discipline and does not mean that economists cannot be concerned with explanation. But within the legal system, promoting realism or demanding explanation is unnecessary. It is better for a judge to focus on generating good outcomes; seeking explanation and contemplating realism is better left to academia.

Before evaluating theoretical, experimental, and econometric evidence, it is essential to establish a framework by which a judge can evaluate these three approaches. Establishing a philosophical framework is essential because it determines how a judge will evaluate different types of economic evidence. These two philosophies—legal pragmatism and economic instrumentalism—establish a consequentialist framework, meaning that judges are concerned about economic and legal consequences when evaluating different types of economic evidence. Fortunately, antitrust is an area of the law that lends itself to two compatible philosophies. Despite its dubious legislative origins, legal pragmatism and instrumentalism have helped antitrust law become "a success story of which all branches of

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<sup>28</sup> Caldwell notes that Friedman's essay, "has been reviewed often, usually negatively." (178). Daniel Hausman, a philosopher of science, argues, "One finds in Friedman's essay splendid apologetics that, with a bit of confusion, can masquerade as up-to-date respectable quasi-positivist philosophy of science."

<sup>29</sup> Caldwell, 179.

<sup>30</sup> Caldwell, 186.

the law and allied disciplines can be proud.”<sup>31</sup> Having established this framework, then, we can now evaluate theoretical, experimental, and empirical evidence in tying cases.

## II. The Three Approaches

*You know my methods, Watson. There was not one of them which I did not apply to the inquiry.*  
-Sherlock Holmes<sup>32</sup>

### A. Theoretical Evidence

By theoretical evidence, I mean evidence based on purely theoretical or a priori knowledge,<sup>33</sup> that is, evidence that is completely independent of experience or observation. Think of theoretical evidence as armchair theorizing; the problem is conceived, analyzed, and solved without observation or reference to the empirical world. Pure theorists believe that economists can only understand economic problems by relying on a priori knowledge. For example, Ludwig von Mises argues, “In all of its branches, [economics] is a priori, not empirical. Like logic and mathematics, it is not derived from experience; it is prior to experience.”<sup>34</sup>

Theoretical evidence can also be understood by contrasting it with empirical evidence, which is based on a posteriori knowledge. Empirical knowledge is based on experience, and, therefore, it changes with observations of the physical world. In this sense of the term, both experimental economics and econometrics are empirical evidence, because their conclusions rely on observational data.

Theoretical evidence, as I have defined it, encapsulates a diverse range of economists. In the context of tying, however, there are two main approaches that require examination: praxeology and rational choice theory. Although praxeologists and rational choice theorists draw conclusions from purely theoretical evidence, both their fundamental assumptions and methodologies differ significantly.

#### 1. Praxeology

Praxeology (Greek for “the study of human action”) is most closely associated with the Austrian School of economics.<sup>35</sup> There are three key aspects of praxeology: 1) an acceptance of universally true premises, 2) an insistence on verbal logical deduction, and 3) a rejection of empirical testing. As Murray Rothbard describes it, praxeology “builds a system of logical deduction on a few universally-known axioms, and which therefore

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<sup>31</sup> Posner, *The Problematics of Moral and Legal Theory*, 229.

<sup>32</sup> Sir Arthur Conan Doyle, *The Memoirs of Sherlock Holmes*, 416 (Doubleday).

<sup>33</sup> In this paper, the terms “theoretical” and “a priori” will be used interchangeably.

<sup>34</sup> Ludwig von Mises, *Epistemological Problems of Economics*, 12-13 (William Volker Fund, 1960).

<sup>35</sup> Because some Austrian economists, notably Friedrich Hayek, disapproved of the type of radical a priorism associated with praxeology, I will confine my arguments to praxeology and not the Austrian School in general.

arrives by verbal logic at a system of apodictic, absolutely know, truths.”<sup>36</sup> Given that praxeology is a foreign methodology for most students of economics, it will be useful to describe how praxeologists draw conclusions about economic problems.

The starting point of praxeology is a single universal truth: the axiom of human action. By human action, praxeologists mean that all conscious behavior is purposefully directed at some goal. Praxeologists are quick to note that they make no value judgments about the content of the goal or about the actions taken to reach the goal.<sup>37</sup>

Two additional points of clarification are necessary. First, praxeologists are only concerned about conscious human action. Purely reflexive, instinctual behavior, is ignored, because the economic actor does not consciously direct such behavior toward his goal.<sup>38</sup> Second, praxeologists argue that purposeful human action is not the same as rationality, especially as that term is understood in neoclassical economics. Human action may be both irrational and purposeful, for example if the action is ill-conceived or unwise. Also note that because human action is known a priori, it cannot be contradicted by empirical observation. Any results claiming to undermine rationality, namely by psychologists or behavioral economists, do not concern a praxeologist.

Rothbard argues that praxeology is simply working out the logical implications of the fundamental axiom of human action.<sup>39</sup> Given the foundational role that it plays, praxeologists assert that the axiom of human action is absolutely, universally true. Praxeologists hold such confidence because they believe that any attempt to refute the axiom of human action is a self-contradiction.<sup>40</sup> That is, the very act of disproving the axiom is itself a purposeful act. With this reasoning, praxeologists assert that the axiom of human action, “is true for all human beings, everywhere, at any time, and could not even *conceivably* be violated.”<sup>41</sup>

From the axiom of human action, a few other self-evident axioms are derived, such as the diversity of mankind and nature and the conception of leisure as a consumer good. For the purpose of this paper, it is not important to delineate all the subsidiary axioms of praxeology. Instead, the essential point is that the whole of praxeology is based on a priori axioms. After asserting the universal validity of these axioms, the rest of the method follows from logical deduction. A true premise, with valid reasoning, must yield a true conclusion.

The second aspect of praxeology, verbal logical deduction, requires much less explanation than the axiom of human action. Put simply, as mathematics is the language of

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<sup>36</sup> Murray N. Rothbard, “In Defense of ‘Extreme Apriorism,’” *Southern Economic Journal*, Vol. 23, No. 3, 385.

<sup>37</sup> Murray N. Rothbard, “Praxeology: The Methodology of Austrian Economics,” 59. From Rothbard, *The Logic of Action One: Method, Money, and the Austrian School*, 58-77 (Edward Elger, 1997).

<sup>38</sup> *Ibid*, 58.

<sup>39</sup> *Ibid*, 58.

<sup>40</sup> *Ibid*, 68.

<sup>41</sup> Rothbard, “In Defense of ‘Extreme Apriorism,’” 317.

modern economics, logic is the language of praxeology. Praxeologists insist on verbal logic, as opposed to symbolic logic or mathematics, because only words can preserve the meaning of the concept. On this point, Rothbard references a strange bedfellow, John Maynard Keynes, who in *The General Theory of Employment, Interest, and Money* argued that, “Too large a proportion of recent ‘mathematical’ economics are mere concoctions...which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols.”<sup>42</sup> Keynes held that economics was a branch of logic, and that much could be learned by simply working with axioms about economics activity.<sup>43</sup> By insisting on meaningful verbal deduction, therefore, praxeologists argue that the validity embodied in the initial premises is preserved, thus creating a true conclusion.<sup>44</sup>

The final aspect of praxeology, a rejection of empirical testing, places praxeology in fundamental opposition to both experimental economics and econometrics. First, note that from the perspective of a praxeologist, empirically testing a conclusion is simply unnecessary. If a logical proof has a true premise and valid deduction, the conclusion must be true. More contentiously, praxeologists argue that any type of empirical methodology assumes that historical data can express causality. “All that these methods [i.e. logical positivism, institutionalism, econometrics] can establish is history, that is, the description of complex phenomena...it is impossible to derive knowledge that could tell us something about the effects to be expected in the future from the application of definite measures and policies.”<sup>45</sup> The past, a praxeologist argues, cannot be interpreted as a guide for the future.

While the idea of isolating causation through the hypothetico-deductive method works well in the natural sciences, praxeologists argue that it has no relevance in the social sciences. Economists lack any type of controlled testing and therefore need to follow a completely different method of reasoning than the natural sciences. On this point, praxeologists again have the support of John Maynard Keynes. As Keynes argued, “I want to emphasize strongly the point about economics being a moral science...It is as though the fall of the apple to the ground depended on the apple’s motives, on whether it is worth while falling to the ground, and whether the ground wanted the apple to fall, and on mistaken calculations on the part of the apple as to how far it was from the centre of the earth.”<sup>46</sup> Because the social sciences deal with fundamentally different subjects—Mises emphasized free will<sup>47</sup> while Keynes noted the inconsistency and heterogeneity of

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<sup>42</sup> John Maynard Keynes, *The General Theory of Employment, Interest, and Money*, 297-298 in Rothbard, “Praxeology,” 61.

<sup>43</sup> Keynes, in Daniel M. Hausman, *The Philosophy of Economics: An Anthology*, 286 (Cambridge University Press, 1994). This methodological agreement between praxeologists and Keynes is especially noteworthy, given the differences between Austrian Business Cycle Theory and Keynes’s General Theory.

<sup>44</sup> Rothbard, “Praxeology,” 61.

<sup>45</sup> Mises, vii.

<sup>46</sup> Hausman, *The Philosophy of Economics*, 288.

<sup>47</sup> Mises, 11.

economic activity<sup>48</sup>—the very idea of testing theories with empirical data is rejected by praxeologists.

As a final argument against empirical testing, Mises cites a problem all too familiar to critics of economics: two economists using the same data to support contradictory theories. This problem, Mises claims, is indicative of the uselessness of empirical testing. Only by reverting to universally-valid theory can economists hope to resolve such disputes about empirical data.<sup>49</sup> As I will argue in Section B of Part III, however, praxeologists also fall victim to this criticism.<sup>50</sup>

## 2. Rational Choice Theory

While praxeologists are a relative narrow, homogenous group, rational choice theory encompasses a varied and often conflicting group of economists. This general term includes some economists who are sympathetic to the arguments of praxeology—i.e. Lionel Robbins, Friedrich Hayek, and Frank Knight<sup>51</sup>—as well as more recent, mainstream economists, such as game theorists. By rational choice theorist, I mean someone who starts with certain assumptions about human behavior—e.g. utility maximization and transitive preferences<sup>52</sup>—and works out the conclusions using mathematics. It is interesting to note that while utility maximization and transitivity are not among the fundamental axioms for praxeology, praxeologists nonetheless rely on them to deduce the answers to economic questions.<sup>53</sup> Despite these common assumptions (or axioms from the praxeologist’s view) there are two significant differences between praxeologists and rational choice theorists.

First, unlike praxeologists, rational choice theorists accept that their assumptions are unrealistic or simplifying, and, therefore, rational choice theorists do not reject all empirical testing outright. Although based on unrealistic assumptions, rational choice theorists believe that their conclusions are still useful.<sup>54</sup> Although they have no empirical content, a game theorist’s claims may still identify consequences and make predictions.<sup>55</sup> While game theorists may eschew empirical testing, they do not advocate the validity of their initial assumptions. Game theorists do not maintain the universal validity of utility maximization; instead, they see it as a necessary simplifying assumption.

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<sup>48</sup> Hausman, *The Philosophy of Economics*, 287.

<sup>49</sup> Mises, 29.

<sup>50</sup> Cited in note 122.

<sup>51</sup> Caldwell, 105.

<sup>52</sup> Thomas Ulen argues that utility maximization and transitivity are the two basic assumptions economists make when talking about rational agents. Thomas Ulen, “Rational Choice Theory in Law and Economics” 792.

<sup>53</sup> Rothbard, *In Defense*, 318.

<sup>54</sup> Hal R. Varian and Allan Gibbard, “Economic Models,” *The Journal of Philosophy*, Vol. 75, No. 11. 664-677.

<sup>55</sup> Hausman, “‘Testing’ game theory,” *Journal of Economic Methodology*, Vol. 12, No. 2, 213.

Hausman’s article also explains the problems with claiming that experimental economist test game theory in a meaningful way.

Second, rational choice theorists make use of mathematical proofs, not verbal logical. Because this approach is much more familiar than praxeology, I need not explain it in further detail here. I'll save further analysis of rational choice theory for my examination of how it is used in the economic literature on tying.

Before I explain what praxeology and rational choice theory would look like in a tying case, I want to make note of two prominent schools of thought in antitrust analysis, the Chicago School and the Structure-Conduct-Performance School, that preach empiricism but often practice pure theory. These inconsistencies in the Chicago and SCP schools, therefore, show how prevalent a priori conclusions are, even among those schools that guard against them.

Insistence on empirically testing theory is often the defining characteristic of the Chicago School, as is tersely stated by Judge Frank Easterbrook: "No question should be answered without adequate data."<sup>56</sup> Yet Easterbrook's colleague on the 7<sup>th</sup> Circuit Court, Richard Posner, argues instead that using the tools of general economic theory is the basic tenet of the Chicago School of antitrust.<sup>57</sup> Even more revealing is Posner's remark that, "It is a curiosity, and a source of regret, that to this day very few of [Alan] Director's ideas have been subjected to systematic empirical examination."<sup>58</sup>

The Structure-Conduct-Performance School (SCP) was the dominant approach to antitrust analysis before Chicago and was predicated on a case-study method, which used theory to isolate questions and empirics to answer those questions.<sup>59</sup> Scherer described the SCP position on theory this way: "When a tradeoff must be made, the pure theorist will sacrifice some explanatory power for elegance, while the industrial organization specialist tilts in the opposite direction."<sup>60</sup> Still, despite this insistence on empiricism, the SCP approach tended toward premature conclusions regarding potentially anti-competitive practices. As Easterbrook argues, SCP taught that "you could tell whether competition was feasible from the structure of the market."<sup>61</sup> By focusing on the first step, market structure, the SCP approach often ignored the relevant criterion, market performance. Such disregard for practical consequences highlights the gap between articulating and implementing the SCP methodology.

Having outlined the basic approach of praxeology and rational choice theory, we can now examine how these approaches would be used as evidence in tying cases, specifically for a case evaluating *the leverage theory of monopoly*. The leverage theory posits that if a monopolist controls market A, then by tying good A to good B, the

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<sup>56</sup> Frank H. Easterbrook, "Workable Antitrust Policy," *Michigan Law Review*, Vol. 84, No. 8, 1701

<sup>57</sup> Posner, "The Chicago School of Antitrust Analysis," *University of Pennsylvania Law Review*, Vol. 127, No. 4, 925.

<sup>58</sup> *Ibid* 931.

<sup>59</sup> Stephen Martin, *Advanced Industrial Economics*, 4 (Blackwell Publishers Inc., 2002).

<sup>60</sup> F.M. Scherer, and David Ross. *Industrial Market Structure and Economic Performance*. 3rd ed. (Houghton Mifflin, 1990).

<sup>61</sup> Easterbrook, 1696.

monopolist can leverage her monopoly from market A to market B. If the leverage theory is correct, then tying arrangements need to be scrutinized to ensure that monopolists are not curtailing competition in other markets. A priorists line up on both sides of the leverage theory: while praxeologists and Chicago School rational choice theorists reject the leverage theory, game theorists like Barry Nalebuff endorse it. Because theoretical evidence, especially praxeology, is rarely used in tying cases, I will use examples from academia as proxies for theoretical evidence in a legal setting. My purpose here is not to present a thorough example of how a theorist would testify in court, but merely to demonstrate how the different approaches to economic evidence apply to tying law.

First, let me describe a praxeological approach, as presented by Dominick Armentano's in his book *Antitrust and Monopoly*.<sup>62</sup> Because praxeology rejects symbolic logic, Armentano's analysis of tying arrangements is entirely verbal. A praxeological judgment about a tying arrangement relies on two steps. First, Armentano starts from the axiom of human action: "[I]ndividual human action is purposeful and aims at accomplishing selected ends by adopting patterns of resource use (plans) consistent with those ends."<sup>63</sup> Second, Armentano devises an efficiency criterion: "If the means employed in the pursuit of selected ends are consistent with those ends, then the means or plans are said to be efficiently employed."<sup>64</sup> Tying is useless as leverage because the monopolist is already charging a monopoly price for good A, and any attempt to charge more for bundle AB will deviate from the monopoly price—that is, the price will not longer be profit-maximizing.<sup>65</sup> The monopolist, therefore, can do no better selling the tied goods than he can by selling A and B independently.<sup>66</sup> Relying on the axiom that all human behavior must be purposeful, therefore, Armentano shows that because tying cannot increase monopoly profits, monopolists have no incentive to use tying as a type of leverage. Any tying arrangement, therefore, must have an efficiency justification.

This response to the leverage theory, although given by a praxeologist, is known as the single-monopoly-profit theorem and is also associated with the Chicago rational choice approach. Unless the tying is done for efficiency reasons, i.e. price discrimination, then the monopolist derives no additional profits from tying a monopolized good to a non-monopolized good.<sup>67</sup> This argument relies on the premise of human action, or profit-maximization. Because tying cannot logically increase monopoly profits, both schools conclude a priori that the practice is not a threat to competition.

An alternate a priori perspective is given by game theorist Barry Nalebuff in his paper, "Bundling as a Way to Leverage Monopoly." Unlike Chicago theorists, Nalebuff

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<sup>62</sup> Dominick Armentano, *Antitrust and Monopoly* (The Independent Institute, 1990).

<sup>63</sup> *Ibid.*, 29.

<sup>64</sup> *Ibid.*, 29.

<sup>65</sup> *Ibid.*, 200.

<sup>66</sup> Michael D. Whinston, "Tying, Foreclosure, and Exclusion," *The American Economic Review*, Vol. 80, No. 4, 837

<sup>67</sup> Posner, *Antitrust Law*, 173.

does not assume that consumers buy the bundled goods in a fixed proportion.<sup>68</sup> By changing a single assumption of the model, therefore, Nalebuff shows demonstrates a scenario in which the single-monopoly-profit theorem will not hold. Through mathematical proofs, Nalebuff shows that the Chicago single-monopoly-profit theorem is a special case of monopoly pricing. More generally, Nalebuff argues that a monopolist can profit by using a tying arrangement to leverage one monopoly into two monopolies.<sup>69</sup> Despite using rational choice theory like the Chicago School theorists, Nalebuff reaches a different conclusion by altering some assumptions, namely allowing for continuous consumption levels.

In fairness to Nalebuff, he does not hold his paper to be a categorical guide for antitrust policy; he merely argues that economists need to recognize that tying may be used to leverage monopoly.<sup>70</sup> The point, however, is that Nalebuff uses no empirical testing to reach a conclusion about tying arrangements. Nalebuff espouses a theory and a warning about tying that is purely theoretical and based only on a priori reasoning.

Theoretical evidence is preferable in tying cases for a couple of reasons. First, praxeology offers a method to arrive at true conclusions, without becoming ensnared in the problems surrounding simplifying assumptions like rationality or the practical aspects of data collection and verification. Rational choice theory, though it does not claim to arrive at universally true conclusions, is especially applicable in a trial setting because it does not require a historical data set (as econometrics does) or laboratory testing (as experimental economics does).

## B. Experimental

By experimental evidence, I mean the use of laboratory methods to test economic theory. While I am classifying it as an empirical approach, experimental economics cannot exist without theory. Still, it is inappropriate to classify experimental economics as a theoretical approach, because experimentalists insist on empirical testing. As van Fraassen argues, “The real importance of theory, to the working scientist, is that it is a factor in experimental design.”<sup>71</sup> That experimental evidence can draw on theory and not vice versa is not illegitimate; while experimentalists demand a blend of theory and empiricism, a priorists reject the idea of empirical testing. Experimental evidence is distinct from theory because it replaces assumption with observation, and it is distinct from econometrics because laboratory tests generate their own data in a controlled setting.

Vernon Smith describes economic experiments as a three-part framework: 1) an environment that often uses money to motivate exchange, 2) an institution that sets the rules of the game and coordinates communication, and 3) behavior of the subjects, which is the

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<sup>68</sup> Barry Nalebuff, “Bundling as a Way to Leverage Monopoly,” Yale School of Management Working Paper Series ES, 3.

<sup>69</sup> Ibid, 2.

<sup>70</sup> Ibid, 24.

<sup>71</sup> Bass van Fraassen, *The Scientific Image*, 73 (Clarendon Press, 1980).



result of the environment and institution.<sup>72</sup> Simply put, experimentalists attempt to trim the number of assumptions—i.e. rational choice theory—needed to test an economic theory in the hopes of generating more accurate predictions. The goal is to create a laboratory set-up that mimics the characteristics of the market and motivations of market actors while isolating and controlling interesting variables. By following the experimental methodology more characteristic of the natural sciences, experimental economists hope to achieve similar success with regards to predictive accuracy.

Before listing the advantages of experimental evidence, I need to distinguish what I am labeling *experimental economics* from two similar fields. First, experimental economics is separate from behavioral economics, even though both may use laboratory methods can be used for a number of purposes, including testing theories, generalizing theories, and creating new theories.<sup>73</sup> Behavioral economics is more closely associated with psychology than economics, especially with regards to the underlying theory, and places more emphasis on explanation—that is, generalizing or creating new theories. Because this paper is specific to tying law and economic instrumentalism, I use experimental economics to refer to an approach that specifically focuses on testing the predictions of economic theory.

Second, by experimental economics, I mean laboratory experiments, not field experiments. In using this terminology, I am following the convention of Al Roth: “I am speaking of experiments in which the economic environment is very fully under the control of the experimenter, who also has relatively unimpeded access to the experimental subjects. This distinguishes laboratory experiments from 'field' experiments, in which relatively few aspects of the environment can be controlled, and in which only limited access to most of the economic agents may be available.”<sup>74</sup>

An experimental approach to economic evidence has three unique advantages: data generation, control, and replication. Because laboratory experiments generate the relevant data set, there is no need to search for a historical data set and no problems with missing data. Empirical studies are only as good as the underlying data used to make the conclusions, and data availability is never a problem for experimentalists.

Because the data is generated within a laboratory setting, experimentalists also have greater control over the variables, making it easier to isolate and test causal factors. As Talley notes, “[O]utside of the rare natural experiment, it is frequently difficult – perhaps prohibitively so – to make causal inferences from real-world data.”<sup>75</sup> The task of teasing out causal relationships is often problematic for econometricians, a difficulty which is avoided by experimentalists: “[T]here need never be a signal-to-noise problem in experimental

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<sup>72</sup> Vernon L. Smith, “Microeconomic Systems as an Experimental Science,” *The American Economic Review*, Vol. 72, No. 5.

<sup>73</sup> Jennifer Arlen and Eric Talley, “Introduction,” NYU Center for Law and Economics Research Paper Series, Working paper no. 08-30, xxii.

<sup>74</sup> Alvin E. Roth, “Laboratory Experimentation in Economics: A Methodological Overview,” *The Economic Journal*, Vol. 98, No. 393, 974.

<sup>75</sup> Arlen and Talley, xvi.

economics. To put it informally, there need never be a problem of disentangling multiply correlated influences.”<sup>76</sup>

Finally, experimental studies can be replicated, whereas econometric studies are specific to an individual historical data set. By replication, I mean that “other experimenters should be able to employ the same techniques, the same protocols, and the same inducements to similar subject pools to attain comparable results.”<sup>77</sup> Replication not only increases the quantity of data, but it also decreases the likelihood that conclusions are influenced by the economists, whether intentionally or not.

Since their inception, experimental methods have been linked to industrial organization, if not to antitrust law. Even though the first experiments run by Edward Chamberlin examined imperfect markets, experimental methods have not been accepted or used in antitrust cases, let alone in tying cases. To understand what experimental evidence would look like in a tying case, therefore, we must again use a proxy from the academic sphere. A good example of experimental testing of a tying arrangement is provided by Caliskan, et al.<sup>78</sup>

In their paper, Caliskan, et al. first generate hypotheses by using game theory. They make five assumptions: 1) goods A and B are perfectly positively correlated, 2) market A is a monopoly and market B is a duopoly, 3) the marginal cost of production is zero for both firms, 4) a Stackelberg model is used to compute the equilibrium conditions, and 5) demand curves are determined by “high” and “low” gain treatments. After working out the equilibrium conditions, the experimenters generate a number of hypotheses.<sup>79</sup> For example, conjecture 1 states that the monopolist adopts a pure bundling strategy under the low gain treatments.

After the data is generated by the laboratory test, corresponding findings are presented for each hypothesis. For example, the laboratory data show that in a low gain treatment, the monopolist adopted a pure bundling strategy only 10-55% of the time, contrary to the original hypothesis.<sup>80</sup> The paper also details the laboratory environment, explaining what the subjects had control over, how often prices could change, and how data was recorded. I will address these elements of the experiment further in Part IV, Section B.

In sum, this paper shows that although theory is essential for the experiment, the conclusions are based on the empirical data, not the theoretical implications. The experiments conclude, “[O]nly in extreme cases does the bundle pricing strategy employed by the monopolist lead to a decrease in the number of sellers that the monopolist faces in the oligopolistic market...”<sup>81</sup> Though the hypotheses drawn from game theory suggested

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<sup>76</sup> G.M. Swann, *Putting Econometrics in Its Place*, 91 (Edward Elgar, 2006).

<sup>77</sup> Arlen and Talley, xxxvi.

<sup>78</sup> Anil Caliskan & David Porter & Stephen Rassenti & Vernon L. Smith & Bart J. Wilson, “An Experimental Examination of Exclusionary Bundling.”

<sup>79</sup> *Ibid*, 12.

<sup>80</sup> *Ibid*, 16.

<sup>81</sup> *Ibid*, 1.

that the bundling was exclusionary, therefore, the experimental data provide less evidence against the tying arrangement.

### C. Econometrics

Econometric methods are much more familiar to economists and more accepted in the legal sphere as well. Econometric evidence, in fact, has been used in antitrust law since the 1970s.<sup>82</sup> Basically, econometrics means testing economic theory by applying statistical techniques to historical data sets. When talking about econometrics in tying law, I am really talking about a small set of econometric techniques that are well-established in the economics profession, specifically multiple regression analysis. As with experimental evidence, econometrics as defined in this paper is inextricable from some use of theory and can be described as a two-part process: 1) deriving hypotheses from economic theory, and 2) testing these hypotheses using statistical methods. Econometrics, however, is distinct from theoretical approaches because it demands empirical testing of conclusions, and it is distinct from experiments both in how data is gathered and how predictions are tested. While an experimental approach generates and tests data in a controlled laboratory, econometrics accumulates data from past market transactions and tests the data using statistical theory.

Econometrics is useful in tying law, because, as Jonathan Baker argues, “The great promise of statistical methods is that they permit a systematic synthesis of the quantitative evidence, weighting the most informative data points the most heavily.”<sup>83</sup> We can organize the advantages of econometric evidence under two main headings. First, econometric studies use a large data set drawn from the transactions of numerous producers and consumers: “[E]conometric evidence allows for the aggregation of all the material data on a particular issue, and thus avoids the danger of considering only selected data points that may be unrepresentative.”<sup>84</sup>

Second, thorough econometric studies often identify causal links. In the early days of modern econometrics, statisticians were trying to devise testing methods that rivaled the precision of laboratory-controlled tests.<sup>85</sup> In addition to aggregation and control, Rubinfeld argues that, “Multiple regression also may be useful (1) in determining whether a particular effect is present; (2) in measuring the magnitude of a particular effect; and (3) in forecasting what a particular effect would be, but for an intervening event.”<sup>86</sup>

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<sup>82</sup> The American Bar Association, *Econometrics: Legal, Practical, and Technical Issues*, 1 (The American Bar Association, 2005).

<sup>83</sup> Jonathan B. Baker and Daniel L. Rubinfeld, “Empirical Methods in Antitrust Litigation: Review and Critique,” 388.

<sup>84</sup> The American Bar Association, *Econometrics*, 4.

<sup>85</sup> Cartwright, Nancy D. “The Vanity of Rigour in Economics. Theoretical Models and Galilean Experiments.” *The 'Experiment' in the History of Economics*, 2005, P. Fontaine and R. Leonard (eds), pp. 135-153.

<sup>86</sup> Federal Judicial Center 2000, *Reference Manual on Scientific Evidence*, 181 (Lexis Publishing, 2000).

A good proxy for econometric work in tying law is “Foreclosure of Railroad Markets: A Test of the Chicago Leverage Theory.”<sup>87</sup> As with experiments, though theory is essential to an econometric study, the conclusions are based on the data: “[T]he degree to which a firm with a monopoly in one good may extend this power by vertical integration or tying cannot be resolved theoretically... The effects of vertical integration or tying on prices are dependent on a set of empirical parameters particular to a given market.”<sup>88</sup> Using evidence drawn from 395 railroad markets, the authors used least-squares estimation to calculate the effect of a number of explanatory variables, such as the number of interline competitors.<sup>89</sup> The authors conclude that monopoly power may be used to increase profits in a separate market, although tying may not be effective as a foreclosure strategy.<sup>90</sup>

Now that I have defined the three approaches to economic evidence, we are now able to start making conclusions about the preferred approach. The first step is to reject theoretical evidence in favor of empirical evidence—empirical meaning both experimental and econometric. The rationale for rejecting theoretical evidence is based on a defense of empiricism, which I will defend positively, using current legal standards, and normatively, using arguments from the philosophy of science.

While empiricism provides a way to reject theoretical evidence, it does not adequately distinguish between experimental and econometric evidence. The second step, therefore, is to explain why econometric evidence is preferable. In Part IV, I will first show that the general acceptance criterion in *Daubert* provides one reason to prefer econometric evidence, then I will elaborate on the concept of *external validity* to demonstrate why judges should continue to prefer econometric evidence in tying cases.

### III. Rejecting Theoretical Evidence

*Those who have treated of the sciences have been either empirics or dogmatical. The former like ants only heap up and use their store, the latter like spiders spin out their own webs. The bee, a mean between both, extracts matter from the flowers of the garden and the field, but works and fashions it by its own efforts.*

-Sir Francis Bacon<sup>91</sup>

In Part III, I will explain why judges should reject theoretical evidence in favor of empirical—both experimental and econometric—evidence. First, I will explain how current legal standards can serve as a guide about what constitutes “good” evidence, then I will explain why the empirical philosophy in these standards is correct. At the conclusion of Part

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<sup>87</sup> Curtis M. Grimm, Clifford Winston, and Carol A. Evans, “Foreclosure of Railroad Markets: A Test of Chicago Leverage Theory,” *Journal of Law and Economics*, Vol. 25, No. 2.

<sup>88</sup> *Ibid.*, 297.

<sup>89</sup> *Ibid.*, 304.

<sup>90</sup> The American Bar Association, *Antitrust Law and the Economics of Product Distribution*, 217.

<sup>91</sup> Bacon, *Novum Organum*, I, 95. Cited in Adler and van Doren, 1110.

III, I will have shown why judges should prefer empirical evidence, but I will not have shown why judges should prefer either experiments or econometrics.

### A. Legal Standards

When writing about science in the law and specifically about the admissibility of evidence, the main legal standards are found in the case of *Daubert v. Merrill Dow Pharmaceuticals*.<sup>92</sup> *Daubert* was the first time the Supreme Court ruled on what constitutes scientific evidence in court, and the ruling dramatically changed the role of a judge with regards to scientific or expert testimony.<sup>93</sup> Before *Daubert*, the two main standards for a judge were articulated in *Frye v. United States*<sup>94</sup> and in the Federal Rules of Evidence.<sup>95</sup> The criterion in *Frye*—which was the main test from 1923 to 1973—asserts that “expert opinion based on a scientific technique is inadmissible unless the technique is ‘generally accepted’ as reliable in the relevant scientific community.”<sup>96</sup> In 1973, the Federal Rules of Evidence superceded the *Frye* test. According to the Federal Rules of Evidence, “in order to qualify as ‘scientific knowledge,’ an inference or assertion must be derived by the scientific method.”<sup>97</sup> This criterion held for another 20 years, until the Supreme Court ruled on *Daubert*.

*Daubert* was a fundamental change because it moved the judgment of good evidence away from the scientific community and into judges’ hands. The crucial language is that *Daubert* made judges the “gatekeepers” of valid scientific evidence; instead of deferring to the relevant scientific community, judges are now asked to regulate good evidence according to their own standards. A significant limitation, however, is that judges evaluate evidence based only on the methodology, not the conclusions that a certain approach produces.<sup>98</sup> Justice Blackmun, writing for the majority, admits that a judge will have less knowledge about a discipline than a practicing academic, but argues that such a gatekeeping role, “is the balance that is struck by Rules of Evidence designed not for the exhaustive search for cosmic understanding but for the particularized resolution of legal disputes.”<sup>99</sup> Blackmun’s language parallels the point I made about economic instrumentalism: the legal sphere has distinct goals (i.e. resolution) that are distinct from

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<sup>92</sup> Cited in note 2.

<sup>93</sup> John Monahan and Laurens Walker, “Twenty-Five Years of *Social Science in Law*, University of Virginia School of Law Public Law and Legal Theory Research Paper Series No. 2010-09, 8.

<sup>94</sup> *Frye v. United States*, 54 App. D.C. 46 (1923).

<sup>95</sup> Federal Rules of Evidence, 1973.

<sup>96</sup> *Daubert* citing Ninth Circuit, 951 F.2d 1128 (1991).

<sup>97</sup> *Daubert*.

<sup>98</sup> Such a limitation is not contradictory with my defense of legal pragmatism in Part I: how a judge evaluates a tying case need not be the same as how a judge evaluates a piece of evidence. So while a judge should focus on the conclusions in a tying case, she should instead focus on the methodology when evaluating scientific evidence.

<sup>99</sup> *Daubert*.

academic goals (i.e. “cosmic understanding”). Economic evidence, therefore, should be evaluated as a tool of prediction, not a explanation of reality.

Before explaining the criteria in *Daubert* in further detail, it is important to note two other aspects about *Daubert*. First, *Daubert* is the first in a series of three rulings about scientific evidence. The other ruling that is relevant for this paper is *Kumho Tire v. Carmichael*<sup>100</sup>; in *Kumho* the criteria in *Daubert* were explicitly extended to cover all expert testimony, including economic experts. Second, while *Daubert* can be divided into two parts—reliability and relevance—I will only focus on reliability in this paper. Because economics is useful in tying cases as a predictive tool, it is most important to discuss the reliability of economic evidence, not its relevance.

If *Daubert* only assigned a gatekeeping role for the judge, the ruling would be significant, though not very helpful. In *Daubert*, however, the court outlined four criteria for evaluating the admissibility of scientific evidence: 1) empirical testing, 2) peer review, 3) known error rate, and 4) general acceptance.<sup>101</sup> In this paper, I’ll be focusing on only two of these criteria: empirical testing and general acceptance. I will not be addressing peer review or known error rate because they do not provide substantial additional reasons to distinguish between theoretical and empirical evidence (as the empirical testing criterion does) or between experimental and econometric evidence (as the general acceptance criterion does). Also, it is important to note that the Court explicitly avoided presenting a comprehensive list of criteria. These four criteria, therefore, should be read as necessary, though not sufficient, aspects of scientific evidence. As I will show in Part VI, we will need to add the criterion of external validity in order to sufficiently distinguish between experimental and econometric evidence.

Now that I have laid out *Daubert*’s criteria, it is clear why judges should prefer empirical over theoretical evidence. The first criterion, empirical testing, rules out both praxeology and rational choice theory as I explained them in Part II, Section A. Justice Blackmun, writing for the majority, argues that generating hypotheses and testing them is what distinguishes scientific thought from other types of inquiry. Purely theoretical approaches, therefore, should not be considered scientific, and consequently should not be relied upon when ruling on tying cases. Praxeology is clearly non-testable, and it has been shown that game theory is non-testable as well.<sup>102</sup>

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<sup>100</sup> *Kumho Tire Company v. Carmichael*, 526 U.S. 137 (1999).

<sup>101</sup> *Daubert*: “Ordinarily, a key question to be answered in determining whether a theory or technique is scientific knowledge that will assist the trier of fact will be whether it can be (and has been) tested... The fact of publication (or lack thereof) in a peer reviewed journal thus will be a relevant, though not dispositive, consideration in assessing the scientific validity of a particular technique or methodology on which an opinion is premised... Additionally, in the case of a particular scientific technique, the court ordinarily should consider the known or potential rate of error... A reliability assessment does not require, although it does permit, explicit identification of a relevant scientific community and an express determination of a particular degree of acceptance within that community.”

<sup>102</sup> Bruce Kobayashi, “Product Differentiation: Game Theory and Antitrust: a Post-Mortem,” *George Mason Law Review*, Vol. 5 No. 411.

Even before ruling on *Daubert*, the Supreme Court was moving away from relying on theoretical evidence.<sup>103</sup> For example, in *Kodak Co. v. Image Technical Services*, “Kodak’s theory does not explain the actual market behavior revealed in the record...we cannot reach [the petitioner, Kodak Co.’s] conclusions as a matter of law on a record this sparse.”<sup>104</sup> While one could rely on the opinion of the Supreme Court, it is important to go beyond positive evidence to reach a conclusion. So the Supreme Court ruled that empiricism is the foundation of science; why is the Supreme Court correct? An answer to this question requires a more general defense of empiricism.

## B. Why Empiricism?

Empiricism is the belief that experience is the source of knowledge.<sup>105</sup> In this paper, it is important to clarify that what I call empirical approaches—experimental economics and econometrics—are a blend of theoretical and empirical knowledge. That empirical approaches can draw for theory and not vice versa is legitimate; remember that Mises argued that, “In all of its branches, [economics] is a priori, not empirical. Like logic and mathematics, it is not derived from experience; it is prior to experience.”<sup>106</sup> While pure theorists reject any empirical testing, empiricists require an initial framework built from theory.

Empiricism has a long, complex history that runs through Aristotle, Hume, and the logical empiricists, and it connects many distinct and conflicting schools within the philosophy of science.<sup>107</sup> For example, in *Daubert*, Justice Blackmun referenced both Carl Hempel, a logical empiricist, and Karl Popper, who articulated falsificationism as an alternative to logical empiricism. It would be inappropriate to group Hempel and Popper together generally, but both philosophers agreed that a scientific approach requires testing hypotheses. While empiricists may disagree with each other on many issues, they are united by their belief that conclusions must be consistent with observation. The reason judges should rely on empirical evidence in tying cases is the same reason why empiricism is strong in the philosophy of science is the: empirical approaches are more objective than theoretical approaches.

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<sup>103</sup> Charles D. Wellers, “Antitrust economics as science after *Daubert*,” *The Antitrust Bulletin*, Winter 1997, 872.

<sup>104</sup> *Eastman Kodak Co. v. Image Technical Services*, 504 U.S. 451 (1993).

<sup>105</sup> Peter Godfrey-Smith, *Theory and Reality: An Introduction to the Philosophy of Science* (The University of Chicago Press, 2003). Empiricism can also be equated with a posteriori knowledge, the opposite of a priori knowledge. See Godfrey-Smith, 234.

<sup>106</sup> Cited in note 34.

<sup>107</sup> Martin Curd and J.A. Cover, *Philosophy of Science: The Central Issues*, 1296 (W.W. Norton & Company, Inc. 1998). For a history of the term “empiricism,” see van Fraassen, 201-224.

## 1. Data Objectivity

The term “objectivity” carries a lot of baggage in the philosophy of science, often meaning either 1) unbiased or 2) mind-independent.<sup>108</sup> Neither of these two definitions captures what I mean by objectivity in this paper. First, I agree that empirical economic research is not completely unbiased or value-free; the type of absolute separation between positive and normative economics advocated by Milton Friedman in “The Methodology of Positive Economics” has not been fulfilled, if such a separation is even possible.<sup>109</sup> Furthermore, freedom from bias is not feasible in law either, which is part of the rationale for legal pragmatism.<sup>110</sup> Second, I do not mean objective in the sense of mind-independence. On an abstract level, every economist or judge has certain biases that affect how empirical data are interpreted. As Kuhn argued, the prevailing paradigm greatly shapes scientists’ viewpoints and affects their work.<sup>111</sup> Scientists do not constantly test every minute detail about the common assumptions in their discipline, let alone how their society or their language affects their research. Doubt about whether empirical research is mind-independent, however, does not mean that it is futile to distinguish between valid and invalid scientific evidence. It is beyond the scope of this paper to systematically examine the sociological factors in science or arguments about scientific realism, but I do want to emphasize that by objectivity, I do not mean value-free or mind-independent interpretation of empirical data.

So what is objectivity? In this paper, I will use the term *data objectivity* to distinguish from other uses of the word objectivity. Data objectivity is a measure of separation between the economist and the data. In any empirical study, there is necessarily a mix of two elements: a subjective (biased, normative) economist and objective (neutral, positive) data. One could also consider research as a mix between a priori, theoretical evidence and a posteriori, empirical evidence. The conclusions of an empirical study rely not only on the data, therefore, but partially on the economist, who controls the theory, variables, assumptions, etc. For an empiricist, the more the conclusion relies on the data—alternatively, the less power the economist has over the conclusion—the more confident we can be in the conclusion. An empiricist becomes more skeptical about a conclusion when the conclusions are altered by subjective or theoretical elements. The acme of objectivity is

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<sup>108</sup> “Sometimes objectivity is taken to mean the absence of bias; objectivity is impartiality or fairness. But the term “objective” is also often used to express claims about whether the existence of something is independent of our minds.” See Godfrey-Smith, 6.

<sup>109</sup> David Goddard articulates the futility of unbiased science further in “Max Weber and the Objectivity of Social Science” *History and Theory*, Vol. 12, No. 1, 1: “No science is value-free, for all scientific activity (because it is an activity, a human activity) presupposes some framework of meanings or values in terms of which it is judged meaningful, worthwhile, or useful.

<sup>110</sup> See Monahan and Walker on the legal realism movement with Jerome Frank: ““The human element in the administration of justice by judges is irrepressible. A judicial decision is a decision by a human being called a judge, [who has] human prejudices, passions and weaknesses.

<sup>111</sup> Godfrey-Smith, 80.



characterized by the steps in the hypothetico-deductive method: a scientist makes a hypothesis, then the data indicates whether the hypothesis is rejected. Ideally, the conclusion is shown by the data, it is not made by the scientist.

In sum, empiricism is based on letting the data speak for themselves; one's a priori notions should defer to empirical observations from the physical world. Data objectivity, therefore, is the foundation of empiricism, meaning that the subjective economist should defer to objective data. Making conclusions and predictions that are consistent with empirical observation is the whole point of empiricism. As Bernard Diamond argued, "[M]ysticism is the prediction of the future which *is not* based upon empirical observation; science is the prediction of the future which *is* based upon empirical observation."<sup>112</sup> What the data actually say is the crucial point; what the economists wants the data to say should be irrelevant.

Now, certainly data cannot speak for themselves. We need to impose structure—that is, economic theory—to decide what to look at. Even then, there are numerous other assumptions that are usually necessary to test or interpret data. As Bas van Fraassen argues, "[E]xperimental design is exceedingly subtle and complex, so experimental design is exceedingly difficult. Hence the need for the construction of theories..."<sup>113</sup> A valid caution for econometrics, in fact, is that one should not make theory from data; otherwise untangling causation and correlation becomes arbitrary. The insistence of theory as a necessary structure for testing, however, does not invalidate the point that empiricism essentially means relying on the data to make conclusions.

Data objectivity is just one reason to insist on empiricism. Two additional reasons are *progress*, the idea that only empirical approaches change as we update our views of the world, and *resolution*, the idea that approaches—especially in the legal sphere—need to help us decide disputes. Let me begin by explaining why a progressive approach is desirable, which is motivated by the concept of *fallibilism*.

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<sup>112</sup> Bernard L. Diamond, "The Scientific Method and the Law," *Hastings Law Journal*, Vol. 19, No. 179, 181. While I agree with Diamond's basic point, it should be noted that I disagree with his description of social sciences and empiricism. Diamond argues that the social sciences do not made adequate use of either empirical observation or mathematics. See Diamond, 186.

<sup>113</sup> van Fraassen, 73. Van Fraassen goes on to argue that, "Scientists aim to discover facts about the world—about the regularities in the observable part of the world. To discover these, one needs experimentation as opposed to reason and reflection" (van Fraassen, 73).

## 2. Progress<sup>114</sup>

In the philosophy of science, fallibilism is the realization that our current ideas will probably turn out to be wrong.<sup>115</sup> The history of science demonstrates the constant replacement of old theories by new theories, so scientists would be well-advised to anticipate that such a pattern will continue. As Albert Einstein noted, “There are no eternal theories in science. It always happens that some of the facts predicted by a theory are disproved by experiment. Every theory has its period of gradual development and triumph, after which it may experience a rapid decline.”<sup>116</sup> Fallibilism is creative destruction in the marketplace of ideas, and it fosters an intellectual humility regarding our economic theories. The impact of fallibilism can be found in the way that statisticians talk about hypothesis testing; they are careful to either “reject the null hypothesis” or “fail to reject the null hypothesis,” never confirming either way.<sup>117</sup>

Because fallibilism is well-supported in the philosophy of science,<sup>118</sup> it is important for judges to rely on evidence that deals with the problems that fallibilism presents. In sum, judges must prefer empirical approaches because only empirical approaches progress; empirical approaches offer the best chance to avoid applying false theories. If fallibilism is like creative destruction, then empiricism is like the price system; empiricism is in constant flux, rejecting theories that become inconsistent as we gather more data or improve how we make observations. To complete the analogy, theoretical approaches are like a planned economy; a priorism does not respond to observation, meaning that the a priori conclusions are static in spite of any changes or improvements. If the caution of fallibilism is correct, then we should be eager to embrace empiricism—the approach that helps us weed out bad theories.

Now a praxeologist may view this type of constancy as an advantage of a theoretical approach. In fact, a praxeologist would probably point to the constant

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<sup>114</sup> Progress as a criterion for science can be found in Quay, “Progress as a Demarcation Criterion for the Sciences” *Philosophy of Science*, Vol. 41, No. 2: “(1) intrinsic to mature science, there exist at least two aspects of progress, present historically, as well as philosophically significant, which, in combination at least, characterize the progress of no other discipline; (2) the progress of science as a psycho-social phenomenon and as an element in the sciences' pattern of development is inadequate of itself (without some such demarcation criterion as Popper's super-added or presumed) to distinguish mature science from nonempirical disciplines.” (155) and in Paul R. Thagard, “Why Astrology is a Pseudoscience”, in Curd and Cover, 27: a theory or discipline is pseudo-scientific if and only if it meets two criteria: 1) the theory is less progressive—it does not change or improve its explanatory power—than alternative approaches, and 2) the community shows little interest in developing the theory towards solving problems or evaluating the theory in relation to others and is selective about considering confirmations and disconfirmations (32).

<sup>115</sup> The term fallibilism is credited to Charles Sanders Peirce. See Godfrey-Smith, 59.

<sup>116</sup> Albert Einstein in Weller, 881.

<sup>117</sup> This type of language may also be influenced by Popper's falsificationism, which is summarized in Godfrey-Smith, 61.

<sup>118</sup> Godfrey-Smith, 59.

uncertainty in empirical studies as an indication of problems with empiricism.<sup>119</sup> To refute such a claim, one only needs to study the history of an empirical discipline—whether economics or a natural science—and conclude that contemporaneous scientists have more predictive power than earlier scientists. A number of factors, whether technological, cultural, or institutional, affect the stock of scientific knowledge, and it would be radical to argue that human beings have the same understanding of the world today as we had in ancient times. Such a growth in the stock of knowledge is characteristic of empirical, not theoretical, approaches. Because the axiom of human action is true at all times for all people, the same conclusions should hold today and two millennia from now. What empirical economists know today, however, may be only a fraction of what they will know in the future.

To clarify the need for a progressive approach, consider the evolution of tying law over the past century. Initially, tying arrangements were treated as per se illegal by the courts, means that they were considered inherently anti-competitive.<sup>120</sup> As antitrust became more economically-based in the second half of the twentieth century, however, courts began to judge tying arrangements according to the rule of reason, meaning that the consequences of the arrangements had to be considered before a ruling could be made. If the courts had insisted on using the a priori per se rule, there would be no way for an efficient tying arrangement to survive. An arrangement that would benefit consumers, therefore, would be deemed illegal. The advantage of the rule of reason and for empiricism is clear: as judges come to better understand the world, they can adjust their rulings to generate better consequences.

In fact, this type of evolution can happen within a single tying case, as in *U.S. v. Microsoft*.<sup>121</sup> In District Court, Judge Jackson used the per se rule to find that Microsoft's tying arrangement—bundling Internet Explorer with the Windows operating system—violated §1 of the Sherman Act.<sup>122</sup> Furthermore, it is significant that Judge Jackson deemed the tying arrangement illegal, despite the fact that “The government did not present an economic analysis of how Microsoft's inclusion of IE [Internet Explorer] in Windows would reduce consumer welfare.”<sup>123</sup> Judge Jackson's a priori conclusion was overturned by the D.C. Circuit court, which instead applied the rule of reason. The Circuit Court found that Microsoft's justifications for tying—that it benefits consumers and improves the Windows operating system for third-party software—meant that the per se analysis was

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<sup>119</sup> See note 49.

<sup>120</sup> Wright, 3. Especially in earlier antitrust rulings, the per se approach was standard, leading to conclusions like, “it is unreasonable, per se, to foreclose competitors from any substantial market,” (*International Salt Co. v. United States*, 332 U.S. 392, 396 (1947)), and “[t]ying arrangements serve hardly any purpose beyond the suppression of competition.” (*Standard Oil Co. v. United States*, 337 U.S. 293, 305-6 (1949)).

<sup>121</sup> *U.S. v. Microsoft Corporation*, 253 F.3d 34 (D.C. Cir. 2001).

<sup>122</sup> *U.S. v. Microsoft Co.*, COL, 2.

<sup>123</sup> David S. Evans, Albert L. Nichols, and Richard Schmalensee, “An analysis of the government's economic case in *U.S. v. Microsoft*,” *The Antitrust Bulletin*, Summer 2001, 235.

inapplicable. The progression of judicial rulings in the Microsoft case, therefore, shows the friction between a priori and a posteriori conclusions and demonstrates the deficiencies of purely theoretical evidence.

In sum, a lack of progress with a priori evidence is what allows critics to compare economics to religion,<sup>124</sup> and it has persuaded Austrian-inclined economists to move away from the radical apriorism of Mises and Rothbard.<sup>125</sup>

### 3. Resolution

A final reason to prefer empirical approaches is that the legal sphere requires an approach that can resolve disputes. Resolution, meaning that a conclusion can be non-arbitrary and persuasive, is essential in all areas of law, but here I will focus on its use in tying law. Empirical approaches are more conducive to resolution because they rely on foundational assumptions that are more commonly accepted than the foundational assumptions of praxeology or rational choice theory. To motivate the concept of resolution, consider two separate tying cases: in one case, the parties rely on theoretical evidence, and in the second case, the parties rely on empirical evidence.

In the first case, although all parties rely on theoretical evidence, it is unlikely that the parties—even if both are praxeologists—will have the exact same fundamental assumptions.<sup>126</sup> In this example, then, one party will be a praxeologist and the other a rational choice theorist. Even though both parties may have the same goal—to maximize allocative efficiency—it is unlikely they will resolve the dispute. Both parties insist on working deductively from foundational assumptions, but they are working from *different* foundational assumptions. Each party will try to persuade the other, but they cannot convince each other by referencing new evidence or other experiences. Each party is talking within his a priori framework, leaving him incapable of convincing someone who starts from different assumptions. As Posner writes, “[R]easoning to (or from) foundations is convincing only to the already convinced.”<sup>127</sup>

Now consider the second case, where the parties rely on empirical evidence. They all have the same goal, but they put forth different hypotheses. By testing these hypotheses against empirical data, however, they are much more likely to resolve the dispute. It needs to be emphasized, however, that empiricism is a way for parties to resolve disputes, *as long as the principles of testing are accepted*. This last clause deserves emphasis, because it is

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<sup>124</sup> Charles K. Wilber, “The Miseducation of Economists,” *America: The National Catholic Weekly*, Sept. 28, 2009, 12.

<sup>125</sup> Mark Skousen, *Vienna & Chicago: Friends or Foes?* 126 (Capital Press, 2005).

<sup>126</sup> For example, see the debate about the difficulties surrounding “natural” monopolies among praxeologists. See Israel Kirzner, *Competition and Entrepreneurship*, 103, and Murray N. Rothbard, *Power and the Market*, chapter 3, section R.

<sup>127</sup> Posner, “Pragmatism Liberalism versus Classical Liberalism,” *The University of Chicago Law Review*, Vol. 71, No. 2. 659.

not obviously true. While I think it is more likely that two disputing parties will agree to use econometrics than to use praxeology, one must acknowledge that there are significant disputes within any econometric study; while parties can agree on an empirical approach, they may still disagree about what data to use or which econometric techniques are necessary. These are valid concerns, but they are less problematic than the alternative problems with theoretical approaches. Put another way, conflicting theorists are futilely trying to resolve contradictory closed logical systems; as long as your system is internally consistent, you cannot convince someone who rejects your initial premises. Empiricists, on the other hand, are able to make use of the dynamic world of observation and experience. Maybe your opponent isn't convinced today, but the possibility of new data or better testing makes it more likely that she may become convinced at some point in the future. In sum, the problems *within* any given empirical approach are less intractable than the problems *across* theoretical approaches.

The advantages of empirical approaches regarding data objectivity, progress, and resolution, when combined with the legal standards articulated in *Daubert*, explain why judges should prefer empirical evidence over theoretical evidence. While I have rejected one approach to economic evidence in tying cases, I have yet to reach my final conclusion. In Part IV, therefore, I will explain why the concepts of general acceptability and external validity provide the rationale for judges to prefer econometric evidence over experimental evidence.

#### **IV. Rejecting Experimental Evidence**

*[O]ur statements about the external world face the tribunal of sense experience not individually but only as a corporate body.*

-W.V. Quine<sup>128</sup>

##### **A. General Acceptance**

General acceptance criterion means that practicing economists can speak with authority about what constitutes good economic evidence. Disciplines establish their own unique methodology and standards of review, and judges should look at the consensus in the relevant scientific community in order to rule on good scientific evidence. General acceptance, the fourth criterion listed in *Daubert*, is carried over from the ruling in *Frye*, in which it was the dominant criterion of scientific evidence. As Justice Blackmun writes in *Daubert*, "In the 70 years since its formulation in the *Frye* case, the 'general acceptance' test has been the dominant standard for determining the admissibility of novel scientific evidence at trial."

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<sup>128</sup> W.V. Quine, "Two Dogmas of Empiricism," *The Philosophical Review*, Vol. 60, No. 1. 38.

Besides the obvious reason that an industrial organization professor knows more about tying than an average sitting judge, the general acceptance test provides three main advantages<sup>129</sup>: 1) it ensures a minimum number of experts capable of evaluating evidence, 2) scientific communities hold more homogenous opinions than judges, and 3) it preserves legal resources by forcing scientific communities to regulate their own methodologies. In addition to these largely pragmatic benefits, the general acceptance test is implicitly a consensus theory of truth.<sup>130</sup> A consensus theory of truth simply means that what is considered *true* depends on what the majority of people think is true.

Here, let me emphasize that I am not advocating that we rely solely on the consensus within the economics profession to decide what is good economic evidence; such a prescription would destroy any examination of methodology and would lead to a tautological cycle whereby the current methodology supports itself. Still, it is important to remember that economics is useful in tying cases because it is a predictive tool, and if one is deciding which tool is best suited for a job, one would be well-advised to ask other craftsmen.

Consensus is an important test of validity for philosophers, including economists and judges.<sup>131</sup> Deidre McCloskey's influential views on rhetoric in economics, for example, is based on the idea of consensus: "We believe and act on what persuades us...what persuades well-educated participants in our civilizations and justly influential people in our field. To attempt to go beyond persuasive reasoning is to let epistemology limit reasonable persuasion."<sup>132</sup> Likewise, in *Abrams v. United States*, Justice Oliver Wendell Holmes argued that consensus, the power of a thought to get itself accepted into the competition of the market, is the best test of truth.<sup>133</sup>

Using the general acceptance test to prefer econometric evidence, therefore, is supported for two main reasons. First, it was the dominant legal standard for much of the 20<sup>th</sup> century and it remains an explicit criterion in *Daubert*. Second, referencing a consensus theory of truth has support from both economists and jurists. In short, when deciding which method to follow in the marketplace of ideas, it is useful to understand which method is in highest demand.

Having established that general acceptance is a useful criterion, it is evident which approach the economics profession prefers, at least right now. Despite the recent surge in

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<sup>129</sup> Paul C. Gianelli, "The Admissibility of Novel Scientific Evidence: *Frye v. United States*, a Half-Century Later," *Columbia Law Review*, Vol. 80, No. 1197, 1207.

<sup>130</sup> For an overview of one consensus theory of truth, that of Jürgen Habermas, see Mary Hesse, "Habermas' Consensus Theory of Truth," *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, Vol. 1978, 389.

<sup>131</sup> Despite their agreement about gauging consensus, McCloskey and Holmes have different opinions about economic methodology. McCloskey does not favor econometrics, while Holmes does.

<sup>132</sup> Donald N. McCloskey, "The Rhetoric of Economics," *Journal of Economic Literature*, Vol. 21, June 1983, 512.

<sup>133</sup> *Abrams v. United States*. 250 U.S. 616 (1919). Elsewhere, Holmes also writes, "[O]ur test of truth is a reference to either a present or an imagined future majority in favor of our view." ("Natural Law," *Harvard Law Review*, Vol. 32, No. 1, 40).

experimental economic research—accompanied by the publicity of behavioral economics—it is clear that econometric testing still dominates economic research. Although modern econometrics and experimental economics started around the same time, by nearly any measure—journals and citations, academic course offerings, or discipline-wide awards—econometrics dominates experiments. This is especially true in tying cases. Although they are distinct approaches, an approximation about the use of experiments in tying cases may be drawn from a similar examination of behavioral economics in antitrust generally; a 2007 paper by Maurice Stucke shows that no federal court has cited behavioral economic evidence in any antitrust case.<sup>134</sup> On the other hand, the use of econometrics is well-established, especially in antitrust. As Jonathan Baker notes, econometrics is so common that the Federal Judicial Center’s *Reference Manual on Scientific Evidence* has a chapter solely devoted to multiple regression.<sup>135</sup>

While econometrics may currently be the generally accepted method, it is important to note that general acceptance is variable; future economists could come to prefer experiments, institutional case studies, etc. For example, two institutional economists won the 2009 Nobel Prize in Economics, and the 2010 John Bates Clark medal went to an economist for her work with field experiments. Charles Plott, an experimentalist, is optimistic regarding the popularity of economic experiments: “the [economics] profession has tasted the devil’s brew, the use of experimental methods, and likes it.”<sup>136</sup> There is reason to believe that the recent growth in experimental economics will continue, both because of internal factors—economists becoming familiar with experimental methods, and experimentalists becoming more aware of their methodology—and also because of external factors—increasing availability of laboratory test equipment, and test subjects becoming more familiar with computers and digital transactions.

At least at this point in time, however, it is clear that industrial organization, as well as economics generally, remains a non-experimental discipline. The general acceptance test articulated in *Frye* and *Daubert*, therefore, provides one reason to prefer econometric evidence in tying cases.

## B. External Validity

Although general acceptance provides one reason to prefer econometric evidence, there are two reasons to go beyond this single criterion. First, Justice Blackmun explicitly warned against relying solely on the criteria in *Daubert*.<sup>137</sup> Second, by calling judges the

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<sup>134</sup> Maurice E. Stucke, “Behavioral Economists at the Gate: Antitrust in the 21<sup>st</sup> Century,” *The University of Tennessee College of Law Legal Studies Research paper series #12 December 2007*, 515.

<sup>135</sup> Baker, 387.

<sup>136</sup> Charles R. Plott, “Will Economics Become an Experimental Science?” *Southern Economic Journal*, Vol. 57, No. 4, 918.

<sup>137</sup> *Daubert*: “Many factors will bear on the inquiry, and we do not presume to set out a definitive checklist or test.”

“gatekeepers” of scientific evidence, *Daubert* meant to make judges less deferential to scientific communities.<sup>138</sup> A preference for econometric evidence, therefore, should be based on more than just general acceptance. To what other criteria, then, should judges look?

One possibility is to look at the general debates about econometric and experimental methodology. For example, Vernon Smith, who won the Nobel Prize in 2002, criticizes Milton Friedman’s methodology of instrumentalism. Smith argues that Friedman’s methodology ignores the need to explain inaccurate predictions.<sup>139</sup> While Smith’s argument may hold in the academic sphere, it is inconsistent with the view of economics as a tool in the legal sphere. Here again, the goal of prediction in the legal sphere does not necessarily align with the twin goals of prediction and explanation in the academic sphere. But more fundamentally, Friedman is not opposed to testing economic theories by experiments. Friedman’s essay emphasizes testing predictions by empirical means, and he simply does not think that economics is experimental.<sup>140</sup> When he wrote his essay, 1953, Friedman’s lack of faith in experimental economics was justified; the first economic experiments had been run only five years earlier. So while there are differences between prominent econometric and experimental methodologists, these general differences will not help us distinguish between the two approaches in tying cases.

A second possibility is to elaborate on some criticisms about using paid economic experts in litigation.<sup>141</sup> For example, the short time frame of the legal process and the significant compensation may skew the results of the testimony given by economic experts. These criticisms, however, would likely apply equally to both econometric and experimental experts. In general, it is unclear if an econometric study in a tying case would be more thorough than an experimental study, or vice versa, and much would depend on case-specific data availability and legal discovery. Regardless, it would be improper to conclude that one empirical approach is categorically preferable based on the problems inherent to using expert testimony in litigation cases.

A third possibility, the ideal approach, would be to measure the predictive accuracy of econometric studies in tying cases, as compared to the predictive accuracy of experimental studies in tying cases. Remember that in tying cases, the judge uses economic evidence to make predictions about which ruling will maximize allocative efficiency. The reliability of economic evidence—that is, the predictive accuracy of the evidence—is

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<sup>138</sup> Monahan and Walker, 9.

<sup>139</sup> Smith, *Rationality in Economics*, 304 (Cambridge University Press, 2008).

<sup>140</sup> Milton Friedman, in Hausman, *The Philosophy of Economics*, 185: “The inability to conduct so-called ‘controlled experiments’ does not, in my view, reflect a basic difference between the social and physical sciences...”

<sup>141</sup> For critiques about using academics as experts in litigation, see John L. Solow and Daniel Fletcher, “Doing Good Economics in the Courtroom: Thoughts on *Daubert* and Expert Testimony in Antitrust,” 495 and Posner, “The Law and Economics of the Economic Expert Witness,” *The Journal of Economic Perspectives*, Vol. 133, No. 2.



paramount. A measure of predictive accuracy, both for econometrics and experiments, therefore, would be a crucial criterion in deciding which approach to prefer.

Such a measure of predictive accuracy, however, is problematic for two reasons. First, experiments have never been used in tying cases. We could measure the predictive accuracy of econometrics in tying cases, but without a measure of experimental predictive accuracy, we cannot make a valid comparison. It is feasible that such a comparison will be possible in the future, say if courts start using experimental evidence in tying cases. This scenario is also problematic, however, because it turns the legal system into a laboratory to test the two different approaches. While we could feasibly compare the predictive accuracy of the two approaches in the future, such a social experiment would be morally-dubious, especially given the potential consequences for the immediate parties in the tying cases that use the less accurate approach.

The problem, therefore, remains: if predictive accuracy is the ideal criterion, but we cannot measure predictive accuracy, why should judges prefer econometric over experimental evidence? The answer lies in a more thorough examination of experimental methodology. Specifically, judges need to understand the concept of *external validity*<sup>142</sup> and why econometric evidence is more externally valid than experimental evidence. External validity, therefore, when combined with general acceptance, gives judges sufficient reason to prefer econometric evidence over experimental evidence.

External validity asks whether the sample (i.e. the laboratory test or the historical data set) accurately represents the “real world.” If a sample is externally valid, then the conclusions drawn from the sample can be confidently generalized to the population. In tying cases, this means that the conclusions presented in the courtroom generalize to the relevant market in question. As Francesco Guala writes, “[M]ost economists are not particularly interested in what happens when a group of undergraduate students play lottery games. They would like to learn something about the functioning of markets, about economic behavior in the real world.”<sup>143</sup>

In sum, an instrumentalist conception of economic evidence does not allow us to distinguish between experimental and econometric evidence because we do not have the data to compare the two approaches. Instead, we must take a more thorough look at the methodology for each approach in order to decide which approach is more likely to produce conclusions that generalize to the real-world market in question. To make my conclusions more specific to tying law, I will break external validity into two parts, wholism and replication, and show how experimental evidence is deficient in these two areas.

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<sup>142</sup> Francesco Guala, *The Methodology of Experimental Economics*, (Cambridge University Press, 2005). Same basic idea as Smith’s “parallelism.”

<sup>143</sup> Ibid, 141.

## 1. Wholism

For a study to be externally valid, the data in the sample must accurately represent the transactions carried out in the relevant market. This means asking the question: does being in a laboratory alter the way people act? If so, the experiment is not externally valid. A discontinuity between laboratory and real-world behavior is a prevalent concern for social science experiments and is generally known as the *Hawthorne effect*.<sup>144</sup>

The Hawthorne effect is a specific name for a wholistic critique. Wholism is the principle that a part—in tying studies, an economic agent—cannot be understood independently of the whole—the market. Wholism essentially says that there are some characteristics inherent to real-world markets that are lost if data is taken out of the context of the market. From a wholistic perspective, testing economic theory requires sample data that accurately represents the population.

At this abstract level, wholism is an unattainable standard; economists obviously need to narrow their focus somewhat to make economic questions coherent and manageable. When considering evidence in tying law, however, we can use the concept of wholism to motivate a comparison between econometrics and experiments. With laboratory testing, there is a trade-off between wholism and control, a trade-off between data generation and data accumulation. While econometric data is *accumulated* from the relevant market, experimental data is *generated* in the laboratory. As Talley notes, “Notably, both [econometrics]<sup>145</sup> and experimentalism use accepted statistical techniques both to summarize data and to test hypotheses. Their core distinction comes from the distinct underlying sources of that data.”<sup>146</sup> Experimental data, therefore, is more susceptible to a wholistic critique.

Before detailing the problems with experimental data, it is important to keep in mind the scope of this paper. Data generation is often considered an important advantage of experimental economics in general; unlike econometricians, experimentalists never suffer from an incomplete or lacking data set. One could argue, therefore, that even if experimental economics is less wholistic than econometrics, it is necessary to make empirical conclusions. While this argument is valid, it is not applicable to evidence in tying cases.

The econometric data in legal cases is often richer than in academic studies because antitrust enforcement agencies have the clout and private parties have the incentive to accumulate the most complete data sets.<sup>147</sup> Also, as set forth in the Federal Rules of Civil

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<sup>144</sup> Annamaria Fiore, “Experimental Economics: Some Methodological Notes,” *Munich Personal RePEc Archive*, 25.

<sup>145</sup> Talley uses the word “empiricism” here when referring to a method of drawing data from the “real world.” In this paper, I argue that both econometrics and experiments should be considered empirical, so I have changed Talley’s word choice but preserved the basic argument.

<sup>146</sup> Arlen and Talley, lii.

<sup>147</sup> Baker, 430.

Procedure, the legal process of discovery requires that all parties disclose essential documents and data sets.<sup>148</sup> Baker also argues that the use of qualitative data and the goals of litigation econometrics make legal studies more reliable than academic studies.<sup>149</sup> In tying cases, therefore, finding relevant historical data is unlikely to be a problem, rendering one significant criticism of econometrics void.

To clarify the wholistic criticism of experimental data and to apply it to tying arrangements, consider the experiments run by Caliskan, et al.<sup>150</sup> There are five reasons to be skeptical about the laboratory sample's wholistic validity. While these five criticisms could be elaborated further, I'll only give an example from the laboratory experiment to clarify each criticism.

First, the composition of the data set. The human subjects in the experiment were students at George Mason University, and they all signed up to be involved in the economic experiment. The economic actors, therefore, were all college-educated and more likely to have economics training. In the experiment, furthermore, the students acted as the producers, which adds another layer of discontinuity between the market and the laboratory. Second, the incentives of the laboratory do not mirror the market. The average payout was \$25.36 per student, significantly less than actual payout for firm employees.

Third, the experiment imposed informational controls in the experiment that are unlikely to occur in the market. For example, the subjects were given no information on demand, while a real-world firm will likely have data about its customers and their buying habits. Fourth, the experiment imposed assumptions about market structure, namely that demand was computerized to maximize consumer welfare. Computerized demand, although reasonable given the scope of the experiment, is still an artificial assumption that may or may not hold in the actual market. Finally, the experiment lasted only two hours, which raises questions about heuristics. An experiment must strike a balance of time so that subjects can learn from mistakes, but it seems that a real-world actor may put considerably more time into pricing decisions.

Initially, these five wholistic criticisms about experiments seem to echo the criticisms about realistic assumptions of economic models. Instrumentalism, by arguing that we look predictions and not assumptions, is supposed to answer these concerns. But remember that it is impossible to compare the predictive accuracy of experiments and econometrics. A more thorough critique of experimental methodology, therefore, does not

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<sup>148</sup> The American Bar Association, *Econometrics*, 70. For a complete analysis of data discovery in antitrust law, see The American Bar Association, *Econometrics*, 61-95.

<sup>149</sup> Baker, 431.

<sup>150</sup> Cited in note 78 and Anil Caliskan & David Porter & Stephen Rassenti & Vernon L. Smith & Bart J. Wilson, 2007. "Exclusionary Bundling and the Effects of a Competitive Fringe," *Journal of Institutional and Theoretical Economics*, Mohr Siebeck, Tübingen, vol. 163(1), pages 109-132. Though separate experiments, these two papers examine very similar topics. I am considering them together because all the details—subject composition, payout information, length of experiment, etc.—are not provided in either of the two papers separately.

conflict with my defense of instrumentalism earlier in this paper; an additional examination of methodological assumptions is necessary to separate experiments and econometrics.

In sum, the drawbacks of data generation—composition, incentives, information, heuristics, market structure—are tolerable in many experimental studies simply because the necessary econometric data is unavailable. In tying cases, however, econometric data is available, so a reliance on generated data instead becomes a problem for experimental evidence; what the laboratory-generated data gains in availability, it sacrifices in authenticity. The disadvantages of data generation, therefore, make experimental evidence less externally valid than econometric evidence.

## 2. Replication

In addition to asking whether the sample data are authentic, it is important to ask whether the sample data are comprehensive enough to draw conclusions from. Like data generation, replication is often considered an advantage of experimental economics; the conclusions in one experiment can be repeated for different samples to determine how robust the conclusions are. Just as with data generation, however, what is an advantage for experiments generally does not translate to experiments as evidence in tying cases.

Consider the tying experiment in Caliskan, et al., in which the experiment was conducted using 20 undergraduate students. While 20 subjects may seem like a small number, experimentalists are quick to argue that replication of an experiment will expand the sample size so that, at some point, the sample size will be sufficiently large. Experimentalists also note that tests of external validity, even with small sample sizes, show there is no cause for concern. As Al Roth notes, “Do college students behave like real people?... you can never be sure. But for most purposes there is little evidence that they do not, since results initially obtained with college students seem to be robust more often than not.”<sup>151</sup> While there is some evidence that test subjects perform the same as real-world actors,<sup>152</sup> these tests are not specific to tying studies or even to antitrust in general.

The reason that experimental replication is especially invalid in tying law is that tying cases require case-specific market analysis. The consequences of tying arrangements are case-specific and subject to fluctuation if minute details change. For example, the difference between the single-monopoly-profit theorem and the leverage theory of tying is based on the initial assumptions. Merely by assuming scale economies instead of constant returns-to-scale, it can be shown theoretically that a tying arrangement will be anticompetitive.

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<sup>151</sup> Roth, 1022.

<sup>152</sup> Ibid.

Within the academic sphere, the theoretical and econometric literature on the various market conditions is extensive, but still far from exhaustive.<sup>153</sup> Experimental tests of tying arrangements in various market conditions is even less extensive. Just within the academic sphere, therefore, the promise of replication has not held. When examined within the legal sphere, replicable is even less useful.

The rationale for judging tying cases according to the rule of reason is that tying arrangements are extremely sensitive to market conditions. A slight change in market conditions may lead to entirely different consequences, yet experiments rely on time-consuming replication in order to be considered externally valid. If an experiment is to be used as evidence in a tying case, it must both be case-specific and delivered within a relatively short timeframe. This is an intractable situation for an approach that relies on replication for externally valid results. The relatively short term of a lawsuit and the finality of a judge's ruling means that a case-specific experiment cannot be adequately replicated. Because experimental conclusions will necessarily rely on the actions of around 20 self-selected subjects, it is untenable to defend the experiment's conclusions as externally valid.

The criticisms of wholism and replication in experiments, though they provide a rationale to reject experimental evidence, do not obviate the problems in econometric studies. Even when an econometrician gets past data accumulation and model specification, there are still two prominent problems with using econometric evidence. The first problem, termed the *signal-to-noise ratio* by G.M. Peter Swann, regards the noise in explanatory variables caused by measurement error.<sup>154</sup> Swann argues that this ratio becomes more problematic as more explanatory variables are added to the equation, thus creating a trade-off between omitted-variable bias and signal-to-noise ratio.<sup>155</sup> Such a problem has not been adequately addressed by econometric methodologists,<sup>156</sup> which creates concerns about the validity of econometric evidence.

Second, even if an econometric studies is thorough, the principles of statistical testing only generate probable causal links: As Daniel Rubinfeld states, then addresses, the problem, "There is a tension between any attempt to reach conclusions with near certainty and the inherently probabilistic nature of multiple regression analysis...The reality that statistical analysis generates probabilities that there are relationships should not be seen in itself as an argument against the use of statistical evidence. The only alternative might be to use less reliable anecdotal evidence."<sup>157</sup> This criticism is also answered by the instrumental framework laid out in Part I: certainty is too high a threshold for evidence in tying cases, so

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<sup>153</sup> Kobayashi, "Does Economics Provide a Reliable Guide to Regulating Commodity Bundling by Firms? A Survey of the Economic Literature," *Journal of Competition Law & Economics*, Vol. 1, No. 4, 1.

"Thus, this review of the economics literature generally confirms the SG's position in *3M v. LePage's* regarding the underdeveloped state of the economics literature and the wisdom of delaying the promulgation of antitrust standards for bundling."

<sup>154</sup> Swann, 46.

<sup>155</sup> *Ibid*, 52.

<sup>156</sup> *Ibid*, 53.

<sup>157</sup> Federal Judicial Center, 185.

judges should be examining usefulness. As William James said, “All our decisions are bets on what the universe is today, and what it will do tomorrow.”<sup>158</sup>

Despite the problems of signal-to-noise and probabilistic causes, econometrics is still preferable to experimental evidence. The basic reason is that empiricism is the foundation of science, and data objectivity is the foundation of empiricism.<sup>159</sup> By showing that econometric data is more externally valid—and thereby more objective—than experimental data, therefore, judges have a rationale for preferring econometric data in tying cases, despite its imperfections.

The concerns associated with external validity are a constant problem for experimental economics generally. As Loewenstein argues, “[E]xperiments have high external validity if they are intended to represent the behaviour, and consequences of that behaviour, of people operating in highly structured markets. They are much less well suited for testing predictions about the economic consequences of individual behaviour in the 'real world.'”<sup>160</sup> Doubts about the external validity of experiments become even more pressing when we consider their use as evidence in tying cases.

First, econometric evidence is more abundant in the legal sphere than in the academic sphere, so that data generation in experiments becomes a glaring disadvantage. Second, the need to evaluate tying cases individually according to case-specific market conditions make the experimental defense of replication ineffective. In tying cases, therefore, econometric evidence is more externally valid than experimental evidence, giving judges a sufficient rationale to prefer econometrics. Despite the problems associated with econometric studies tying cases, it is the best approach for judges to use when ruling on the legality of tying arrangements. Keeping with the intellectual humility emphasized by fallibilism, judges should reject experimental evidence and fail to reject econometric evidence.

## Conclusion

*For the rational study of law the black-letter man may be the man of the present, but the man of the future is the man of statistics and the master of economics.*

-Justice Oliver Wendell Holmes<sup>161</sup>

Understanding the scope of this paper is essential to evaluating its conclusions. While I conclude that judges should prefer econometric evidence in tying cases, this conclusion may not generalize to economic evidence in other areas of law and certainly does not generalize to the entire economics profession. This paper examines the

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<sup>158</sup> Cited in Menand, xv.

<sup>159</sup> See Part III, Section B.

<sup>160</sup> George Loewenstein, “Experimental Economics from the Vantage-Point of Behavioral Economics,” *The Economic Journal*, Vol. 109, No. 453, F33.

<sup>161</sup> Holmes, “The Path of the Law,” *Harvard Law Review*, Vol. 10, No. 8. 469.

gatekeeping role of a judge within a specific area of law, and my conclusions need to be evaluated within that scope.

The consequentialist framework in Part I relies on the unique characteristics of antitrust law and of economic evidence in the courtroom. The well-established goal of allocative efficiency makes legal pragmatism effective in tying cases, but its efficacy as a comprehensive jurisprudence is left unanswered. Whether judges should be legal pragmatists in tax law or tort law requires additional examination. Likewise, my arguments about economic instrumentalism rely on the courts' use of economics as a tool of prediction and do not generalize to all economic studies. For economics to grow as a science, economists should be encouraged to make use of a wide range of approaches, especially purely theoretical and experimental studies. The legal and academic spheres are distinct, and what is considered scientific evidence in tying cases may not correspond to scientific evidence for academic journals.

Furthermore, the distinctive characteristics of tying arrangements influence my conclusions. For example, the anti-competitive and efficiency consequences of tying arrangements makes empirical approaches preferable, and the variation of consequences with case-specific market conditions favors econometric evidence. The exposition of my conclusions in Parts III and IV, therefore, also need to be evaluated within the scope of tying law.

My goal in this paper is not to present a general theory of economic evidence, but to explain how a judge can discharge his responsibility as a gatekeeper of scientific evidence in tying cases. Contrary to Justice William Rehnquist's caution, a judge need not be an amateur scientist in order to discharge his responsibility as a gatekeeper of scientific evidence in tying cases.<sup>162</sup> With vigilance at the gates of empiricism, judges can realize the capacity of economic science: "[T]o reach knowledge of the law of phenomena, so as to foresee, vary or master phenomena."<sup>163</sup>

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<sup>162</sup> *Daubert et ux., individually and as guardians and litem for Daubert, et al. v. Merrell*, 509 U.S. 579 (1993)

<sup>163</sup> Claude Bernard, *Experimental Medicine*, I, 1. Cited in Adler and van Doren, 1122.