

## A NOBEL PRIZE IN LEGAL SCIENCE: THEORY, EMPIRICAL WORK, AND THE SCIENTIFIC METHOD IN THE STUDY OF LAW

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*Will there ever be a Nobel Prize in law? Professor Ulen uses this question as a framework for discussing the current state of legal scholarship and the trend toward making legal scholarship more “scientific.” First, Professor Ulen discusses the meaning of “science” and the scientific method, and summarizes the various theories that have developed over time to verify, modify, or reject scientific paradigms. Next, he considers whether or not the study of law is a science. All sciences share core theoretical beliefs that allow for the international study and dissemination of scientific information, and that will produce similar results, regardless of where they are applied. These theories are then examined and tested using empirical research. Although law has no such set of core theoretical beliefs, there is a growing body of empirical research in the law. Professor Ulen believes that interest in empiricism is growing in the legal academy, and that empirical research can be very beneficial to both legal academics and practitioners. As the amount and breadth of legal empirical research increases, Professor Ulen posits that a core set of theoretical beliefs will emerge in the law, and that increased empiricism in the law is vital to the future of the law as a science.*

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## I. INTRODUCTION

In this paper I use the fiction of a Nobel Prize in Legal Science to discuss what I believe to be the predominant direction of change in legal scholarship and specifically, the role of theory and empirical and experimental methods in the study of the law. I advance, with temerity, the hypothesis that legal scholarship is becoming more “scientific.” To say that an academic study is “scientific” is frequently taken to be a way of praising that study and of contrasting it to the “unscientific” methods that may be taken to characterize less praiseworthy scholarship. Although I do have a very high regard for the accomplishments and methods of the sciences, I do not mean to invoke the word “scientific” in a conclusory sense. While I shall only be able to sketch my argument, rather than demonstrate it scientifically, I argue that not only is the study of law well suited to the application of the scientific method but that the academic study of law has for a long time been much more like other scientific pursuits than is commonly thought to be the case. I shall argue, for example, that doctrinal scholarship has a scientific aspect to it that has frequently escaped notice.

In addition, I shall claim that recent scholarly trends in the law show evidence of a movement toward an even more science-like discipline. Although I stress the gradual and continuous change in legal academic work of the past fifty or more years, I do, nonetheless, believe that through the silent accretion of many changes over the last several decades, legal scholarship is on the verge of a dramatically different manner of doing routine legal investigation. Put in a nutshell, that change is to make law much like the other disciplines in the university that believe themselves to be practicing “science” and less like those disciplines that consciously eschew scientific inquiry.

What accounts for this change in legal scholarship? That is a complicated and intriguing question that is beyond my abilities to answer. An important part of the answer will undoubtedly be that the law, like any other discipline, is continuously undergoing change and that the cumulative effect of those many small changes may not be evident until a large number of them have taken place. Others have remarked on the changes occurring in law—some favorably<sup>1</sup> and others unfavorably.<sup>2</sup>

I note the ironic fact that at the same time that legal scholarship is, as I believe, becoming more scientific, there is a generally perceptible decline in the social standing and acceptance of science. Perhaps there was a time when science and its practitioners were universally treated with reverence. That time is past. At least since C.P. Snow’s contention

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1. See Richard A. Posner, *The Decline of Law as an Autonomous Discipline: 1962–1987*, 100 HARV. L. REV. 761, 777–80 (1987).

2. See ANTHONY D. KRONMAN, *THE LOST LAWYER* 165–67 (1993) (arguing that the prominence of economic theory in modern legal scholarship accounts for the decline of the pragmatic counselor).

in the 1950s that there were two cultures—the literary or artistic and the scientific<sup>3</sup>—there has been a clear divide between science and everyone else in the academy. Scientists are suspicious of the other disciplines in the academy as being too “soft.” Those other disciplines have different and less defensible norms, it is argued, for establishing what constitutes an advance in knowledge. Instead, it is sometimes argued, the arts cultivate and refine the emotions or intuitions, leading to different and highly subjective standards of scholarship. For their part, many in the liberal arts believe that science is cold, logical, and calculating.<sup>4</sup> They decry the contention that what science does is objective and, therefore, subject to agreed-upon standards of worthiness. Indeed, one might argue that the development and attraction of post-modernism is fueled in large part by its skepticism of science.<sup>5</sup>

Outside the academy there is also a palpable decline in society’s faith in science.<sup>6</sup> Many seem to hold that science has accounted for as many problems as it has cured, that science is not the wonderful engine for the discovery of truth that some believe it to be, that profit and not the search for knowledge is driving scientific inquiry, and that science has an inexorable march toward uncovering matters that we as human beings are only partially equipped to handle.<sup>7</sup> In light of these criticisms, I recognize the hubris of arguing that the rise of a scientific method of inquiry in law is necessarily a good thing. But while I recognize that every change has costs, I firmly believe that the move toward a more scientific study of law will have greater benefits than costs.

I proceed as follows. In Part II, I briefly explain what is meant by “science” and the “scientific method.” I must also contend with the very strong view, popular in many parts of the legal academy, that law as currently practiced by academicians is not science because it is not “falsifiable.” This is such an important point that I take time in Part III to survey the philosophy of science with a view to seeing whether logical positivism, and its central criterion of falsifiability, still has the cachet that many in legal scholarship believe it to have. Somewhat surprisingly, I find that logical positivism has long since given way within the philosophy of science to alternative views of both scientific method and verification. I also examine the newer views with the intention of showing their applicability to the study of law. In Part IV, I turn to an examination of law as a science. There I claim that law has for a long time been implic-

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3. See C. P. SNOW, *THE TWO CULTURES: AND A SECOND LOOK* 4–5 (2d ed. 1965).

4. Henry Adams captured the perceived difference between science and everything else: “No sand-blast of science had yet skimmed off the epidermis of history, thought, and feeling.” HENRY ADAMS, *THE EDUCATION OF HENRY ADAMS* 90 (1918).

5. See THOMAS NAGEL, *THE LAST WORD* 77–79 (1997).

6. See ROBIN DUNBAR, *THE TROUBLE WITH SCIENCE* 8–11 (1995).

7. This is a common theme, for example, in the debate on the ethics of human cloning. See, e.g., LEON R. KASS & JAMES Q. WILSON, *THE ETHICS OF HUMAN CLONING* 3–24, 61–74 (1998); see also FRANCIS FUKUYAMA, *OUR POSTHUMAN FUTURE: CONSEQUENCES OF THE BIOTECHNOLOGY REVOLUTION* (2002).

itly following the view of science and of verification's role in scientific inquiry as espoused in the modern philosophy of science. I also show that theoretical and empirical work have increased in legal scholarship in the recent past but try to distinguish the kind of theory that is becoming increasingly common in the study of law from jurisprudence or the philosophy of law. Specifically, the theory now gaining favor in the legal academy is less jurisprudential and more like the theories that characterize the natural and other social sciences. The newer theorizing in law tends to make predictions about the real-world consequences of legal rules and standards. Although these newer legal theories must be logically coherent and consistent, ultimately their worth turns on the extent to which they are borne out by careful empirical and experimental work. I conclude with some speculation on whether the trends I have identified will result in a Nobel Prize in Legal Science.<sup>8</sup>

## II. THE SCIENTIFIC METHOD

In order for me to comment on whether the study of law is "scientific," I must first explain what it means for a scholarly discipline to be a "science" and for its practitioners to use the "scientific method."

It is surprising how recently in human history this has been a live question. Until approximately 500 years ago, the emphasis in acquiring knowledge was on constructing logically coherent discussions derived from elementary principles. One model of how to proceed was the method pioneered by Euclid in *The Elements*.<sup>9</sup> From only a few untested but seemingly obvious axioms, Euclid deduced complex theorems about geometry. The inquiries that followed this method were generally directed at that class of questions that can be solved only by logical argumentation. And indeed, many interesting questions are of this sort. Many tend to be philosophical—What is right? What is good?—but there are also inquiries into law that are of this sort. For example, once the legislature criminalizes the use or sale of addictive drugs, questions arise about whether a given substance—such as alcohol, caffeine, or heroin—fits that definition.

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8. My speculation about a Nobel Prize in Legal Science is really just a rhetorical device. Nonetheless, it makes a dramatic and telling point about the extent and pace of the changes occurring in legal scholarship. The analogy I have in mind is between the study of law and the sciences for which the Royal Swedish Academy of Sciences and the Royal Bank of Sweden award Nobel Prizes—physiology or medicine, physics, chemistry, and economics. But several commentators at the Symposium held in Champaign, Illinois, on April 13, 2001, asked if the more appropriate analogy for law might not be the Nobel Prizes in Peace and Literature. Presumably they meant to suggest that the more nuanced, contextual, culturally determined, and case-by-case analysis that may characterize the Peace and Literature prizes might be more apt to a Nobel Prize in Law. I, of course, see the point, but my contention will be that the analogy between law and the natural and social sciences is the more appropriate one. For a short history of the Prizes and the procedures by which the recipients are selected, see the Appendix to this article.

9. See generally EUCLID, *THE ELEMENTS* (Heath ed., 1956).

But there are limitations to inquiring about the world merely through logical argumentation. For example, there are times when the axioms from which that argumentation proceeds demand experimental confirmation. Suppose that one has begun one's inquiry from the assumption that there are four fundamental elements—air, fire, water, and earth—and has derived a set of propositions about the world on the basis of that assumption. Now suppose that someone proposes that there is a fifth fundamental element, phlogiston, and shows that including that element requires altering the conclusions derived from the assumption of only four elements. While it is true that logical argumentation can, in principle, resolve whether there are four or five (or a different number of) fundamental elements, controversy of this sort also can excite a different kind of inquiry—namely, an empirical investigation into the matter.

One can recognize, more generally, that there is a broader class of interesting and important questions—beyond those of coherence and logical deduction—that can only be answered by observation, testing, data collection, and manipulation. Examples include: “What time is it?”; “Are there more females or males in this room?”; and, “When is the next U.S. presidential election?” The law is, of course, familiar with fact-based inquiry: most trials are about the facts in a particular case.<sup>10</sup> But I am seeking to focus on the broader questions of law that are amenable to being answered by empirical or experimental work.

Sometime during the Middle Ages and particularly in the late Middle Ages, the importance of the second class of questions became more pressing—principally in Western Europe.<sup>11</sup> Observers recognized that a new method of acquiring knowledge was becoming common and was showing results.<sup>12</sup> The most striking contemporary articulation of this new method of inquiry was by Sir Francis Bacon, the Lord Chancellor of England, in *Novum Organum*.<sup>13</sup> Bacon held that the scientist should be an unprejudiced observer of nature, collecting observations. Through the collection and organization of facts from the natural world, patterns would emerge, with those patterns becoming scientific knowledge.<sup>14</sup>

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10. The distinction between issues that can be resolved by logical argumentation and those that can be resolved by empirical observation is made in SIMON BLACKBURN, *THINK* 282–87 (1999). Philosophers used to discuss these matters as the difference between necessary and contingent truths. Necessary truths were said to be the particular domain of philosophy, while contingent truth was said to be the domain of empiricism. See Richard Rorty, *An Imaginative Philosopher: The Legacy of W.V. Quine*, *CHRON. HIGHER EDUC.*, Feb. 2, 2001, at B7.

11. Why this should have been the case at that point in human history is a fascinating question, but one that is beyond the scope of this inquiry. See generally ALFRED W. CROSBY, *THE MEASURE OF REALTY* (1997); LISA JARDINE, *INGENIOUS PURSUITS: BUILDING THE SCIENTIFIC REVOLUTION* (1999).

12. See generally CROSBY, *supra* note 11; JARDINE, *supra* note 11.

13. See generally SIR FRANCIS BACON, *NOVUM ORGANUM* (Joseph Devey, M.A. ed., 1902).

14. See David Goodstein, *How Science Works*, in *REFERENCE MANUAL ON SCIENTIFIC EVIDENCE* 69 (Fed. Judicial Ctr. ed., 2000). Cryptically, William Harvey, who discovered the circulation pattern of human blood, is reported to have said of Bacon's contention, “That's exactly how a

Most scientists today, when asked to define “science,” answer by pointing to a method of inquiry as being the essence of science. The “scientific method,” which Bacon and others recognized as having arisen during the fifteenth and sixteenth centuries, consists of a set of procedures for “examining the natural world and discovering important truths about it.”<sup>15</sup> Scientists in a particular field share a focus on a particular subject matter, a theoretical core from which hypotheses about that subject matter may be derived by those learned in the theory, and an agreed-upon technique for determining whether the hypotheses are acceptable to those in that field.

To illustrate, consider modern microeconomics, a field that is contiguous to law and for which there is a Nobel Prize. Those in the field of microeconomics share a body of learning—rational choice theory as applied to consumer decision making, firm organization and internal decision making, decisions about the supply of and demand for inputs into production, and issues having to do with normative evaluation of different economic states of the world. In addition, practitioners in this field recognize that hypotheses about decision making should be derived from the shared theoretical understanding of human decision making, and that there are acceptable techniques—the tools of econometrics—for empirically evaluating (confirming or rejecting) these hypotheses.<sup>16</sup>

The law has, of course, wrestled with the issue of what constitutes science, not for examining its own methods of inquiry but for deciding issues of acceptable evidence. The obvious reason for the connection between law and science is that there are often circumstances in which a legal decision maker must evaluate the credence of testimony whose authority is said to derive from “science.”<sup>17</sup> The simple connection is that if testimony can be shown to be “scientific,” then the fact finder or other decision maker will presumably give the testimony greater weight than if it is merely opinion. One can get a sense for the issues involved by imagining that an astrologer seeks to testify that a timely reading of the plaintiff’s astrological signs would have revealed that she was accident-prone on the date on which, it is alleged, the defendant’s negligence caused the plaintiff’s harm.<sup>18</sup> Should the astrologer’s testimony be allowed to enter

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Lord Chancellor *would* do science.” *Id.* Note that Bacon’s focus was on observation as the principal characteristic of science, not the feedback relationship between empirical and experimental work and theory.

15. *Id.*

16. There is, of course, a great deal of controversy about whether economics is really a “science” in the same sense that the biological, chemical, physical, and natural sciences are sciences. *See generally* JOHN SUTTON, *MARSHALL’S TENDENCIES* (2000).

17. *See generally* DAVID L. FAIGMAN, *LEGAL ALCHEMY* (1999); Ellen E. Deason, *Incompatible Versions of Authority in Law and Science*, 13 *SOC. EPISTEMOLOGY* 147 (1999).

18. Perhaps the testimony seeks to make the plaintiff contributorily negligent for failing to heed the astrological warning, of which he knew or should have known.

and be given weight as being scientific?<sup>19</sup> Should the plaintiff be deemed contributorily negligent for ignoring this astrological information?

The United States Supreme Court has laid out a four-step inquiry regarding whether testimony should be accepted as “science.” In *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, the Court established these four steps:<sup>20</sup>

1. The theoretical underpinnings of the methods must yield testable predictions by means of which the theory could be falsified.
2. The methods should preferably be published in a peer-reviewed scholarly journal.
3. There should be a known rate of error that can be used in evaluating the results of the assertions.
4. The methods should be generally accepted within the relevant scientific community.<sup>21</sup>

These standards have excited some controversy,<sup>22</sup> which, however interesting, are not of central concern to my topic. For present purposes, it is enough to recognize that there is some correspondence, but not an exact correspondence, between the characteristics of science and the scientific method that I championed above and the criteria that the U.S. Supreme Court has instructed federal courts to follow in deciding whether testimony should be entitled to the weight of being “scientific.”<sup>23</sup>

I began this section by noting that there are some questions that might be solved by logical argumentation and others by empirical observation. I then went on to show that one way to characterize the scientific method is as a means of acquiring knowledge about the world through a combination of these two styles of inquiry. Specifically, science seeks to articulate a logically consistent theory about a class of phenomena and

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19. As we shall see in the next section, the logical positivists used alchemy, astrology, and psychoanalysis as examples of pseudosciences, seemingly coherent bodies of knowledge that are distinguishable from true sciences in that they make statements that are not falsifiable. See *infra* notes 25–31 and accompanying text.

20. 509 U.S. 579 (1993).

21. *Id.* at 593–95. It is interesting to ask if these same four criteria apply to the law. Certainly criterion 2 (publication in a peer-reviewed journal) does not. And arguably criteria 1 and 3 do not.

22. See, e.g., David L. Faigman, *The Law's Scientific Revolution: Reflections and Ruminations on the Law's Use of Experts in Year Seven of the Revolution*, 57 WASH. & LEE L. REV. 661 (2000); David L. Faigman, *The Tipping Point in the Law's Use of Science: The Epidemic of Scientific Sophistication That Began with DNA Profiling and Toxic Torts*, 67 BROOK. L. REV. 111 (2001).

23. There are, of course, other important interfaces between law and science. One that has recently been of interest has been the relationship between intellectual property and scientific norms. A central question has been whether the broadening of patentable subject matter since 1980 has adversely impacted scientific inquiry by being at odds with the prevailing norms of science. See, e.g., Rebecca S. Eisenberg, *Proprietary Rights and the Norms of Science in Biotechnology Research*, 97 YALE L.J. 177 (1987); Michael A. Heller & Rebecca S. Eisenberg, *Can Patents Deter Innovation? The Anti-commons in Biomedical Research*, 280 SCIENCE 698 (1998); F. Scott Kieff, *Facilitating Scientific Research: Intellectual Property Rights and the Norms of Science—A Response to Rai and Eisenberg*, 95 NW. U. L. REV. 691 (2001); Arti Rai, *Regulating Scientific Research: Intellectual Property Rights and the Norms of Science*, 94 NW. U. L. REV. 77 (1999).

then to subject that theory to systematic investigation to see if the theory accurately describes and predicts that class of phenomena.

The palpable successes of this method of inquiring into the world are legion. Indeed, there is a famous recent argument that advances the proposition that further knowledge in a wide variety of areas is dependent on application of the scientific method to areas—such as religion—previously thought to be open only to intuition, faith, and logical argumentation.<sup>24</sup> I reserve to Part IV a discussion of whether further advances in law may also necessitate the explicit adoption of the scientific method.

### III. A PRIMER ON THE PHILOSOPHY OF SCIENCE

The previous section laid out a discussion of what constitutes science and the scientific method. Here I move beyond the informality of that discussion to discuss how philosophers of science approach the core issues of what constitutes science and verification.<sup>25</sup>

#### A. *Popper and Logical Positivism*

The most famous modern philosophical theory of science is that of Sir Karl Popper. In *The Logic of Scientific Discovery*,<sup>26</sup> Popper, an Austrian, argued that science proceeds by extracting from the theory prevalent in an area logical but unexpected predictions that can be subjected to empirical verification or refutation. Popper's emphasis on falsification arose from his strong belief that a theory can never be proved correct by agreement with observation. The theory that the sun always rises in the east has been proved correct every morning so far, but according to Popper, that theory might be disconfirmed two weeks from today. However persuasive we may find thousands and thousands of years of consistent observation, strictly speaking yet another piece of confirming evidence cannot prove anything.

In contrast, Popper asserts that disconfirming evidence is particularly powerful because it almost conclusively refutes the hypothesis. For example, the theory that all swans are white is decisively refuted by the observation of a black swan. This deep asymmetry between confirming and falsifying theory is the crux of Popper's famous theory. It has the important consequence of raising skepticism to a very, very high place

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24. See generally EDWARD O. WILSON, *CONSILIENCE: THE UNITY OF KNOWLEDGE* (1998).

25. I am especially grateful to Patrick Maher, Department of Philosophy, University of Illinois at Urbana-Champaign, and Brian Leiter of the University of Texas School of Law for their help with this section. For a general survey of topics and readings in the philosophy of science, see *PHILOSOPHY OF SCIENCE: THE CENTRAL ISSUES* (Martin Curd & J.A. Cover eds., 1998); ALEX ROSENBERG, *PHILOSOPHY OF SCIENCE: A CONTEMPORARY INTRODUCTION* (2000); *THE STRUCTURE OF SCIENTIFIC THEORIES* (Frederick Suppe ed., 2d ed. 1977).

26. KARL R. POPPER, *THE LOGIC OF SCIENTIFIC DISCOVERY* (Karl R. Popper et al. trans., 1959).



among the characteristics valued in science. Every theory can, at best, according to this view, only be contingently and fragilely held because no evidence can conclusively prove the theory true although the next bit of evidence can falsify the theory.

The Popperian theory of scientific discovery and verification was a significant advance in the philosophy of science. And yet, despite its obvious attractions, the theory has been strongly and successfully criticized. There are two central criticisms. First, finding falsifying evidence does not necessarily refute the theory—it certainly tells the investigator that *something* is wrong, but that something is not necessarily the theory.<sup>27</sup> It could, instead, be the data or the methods of verification being used. Moreover, falsification is often not as clear-cut as Popper would have it. In order to derive a falsifiable prediction from a theory, it is often necessary to make additional assumptions beyond those made by the theory. If, having made those additional assumptions, the evidence falsifies the theory, it may well be the case that the falsification points to the inappropriateness of the additional assumptions, not to the believability of the underlying theory.<sup>28</sup>

Second, it seems contrary to common sense to hold that no amount of confirming evidence is enough to induce rational belief in a theory. Consider again the example above about the theory that the sun rises in the east. The fact that since at least the beginning of recorded human history the sun has always risen in the east is not, according to Popperians, enough to confirm that theory. But surely this regularity establishes *some* weight in favor of the theory. Professor David Goodstein also notes that the actual behavior of scientists suggests that they believe that more confirming evidence is a good thing; that is, most scientists act in their professional lives as if they value another result that seems to confirm an unexpected prediction of a theory.<sup>29</sup>

These criticisms have been made so powerfully that even confirmed Popperians have conceded their force.<sup>30</sup> The Popperian view of what constitutes acceptable science may have been discredited by critics and that criticism may have been accepted by Popperians themselves. Somewhat surprisingly, however, the Popperian view is alive and well among legal scholars.<sup>31</sup>

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27. See WESLEY C. SALMON, FOUNDATIONS OF SCIENTIFIC INFERENCE (1967); Wesley C. Salmon, *Rational Prediction*, 32 BRIT. J. PHIL. SCI. 115 (1981).

28. See Goodstein, *supra* note 14, at 71.

29. *Id.* The confirmatory possibility of common, repeated evidence is at the heart of Bayesian inference. See discussion *infra* Part III.D.

30. It is worth noting, however, that logical positivism, although in a modern form, still has important defenders. See, e.g., MICHAEL FRIEDMAN, RECONSIDERING LOGICAL POSITIVISM (1999).

31. A recent attempt to revive some aspects of logical positivism is RICHARD W. MILLER, FACT AND METHOD: EXPLANATION, CONFIRMATION AND REALITY IN THE NATURAL AND THE SOCIAL SCIENCES (1987).

### B. *Kuhn's Theory of Paradigm Shifts*

Almost as famous as Popper's theory of falsifiability is that of the late Thomas Kuhn, a historian of science. In *The Structure of Scientific Revolutions*, Kuhn argued that the central concept in any science is that of "normal science" or the prevailing "paradigm."<sup>32</sup> A "paradigm," according to Kuhn, is the prevailing combination of theory, standards, and acceptable methods of investigation and confirmation in a scientific community.<sup>33</sup> The sense is that at any given time the practitioners of a particular science subscribe to the same paradigm. They undertake their scientific inquiries within the constraints imposed by the paradigm. That prevailing paradigm suggests predictions about relevant phenomena. Investigators then pursue those predictions and through their inquiries amend and elaborate the paradigm. Crucial to the notion of a paradigm is the view that there are, from time to time, anomalous observations that do not seem to fit easily into the prevailing paradigm. Frequently when this occurs, those who practice the paradigm are able to jigger the paradigm so as to accommodate the anomalous observation without having to abandon the paradigm altogether. Advances within the paradigm are made incrementally rather than in leaps and bounds.

However, Kuhn suggested that there are scientific revolutions that occur because sometimes the anomalies pile up to such an extent that it is almost impossible to adjust the prevailing paradigm in a reasonable manner that takes account of the anomalies.<sup>34</sup> Eventually, someone proposes a new paradigm that supplants the existing one by accounting not only for all the phenomena explained without flaw by the older paradigm, but also for all those phenomena that were anomalous under the old paradigm.

Kuhn illustrated his theory in an earlier work on the Copernican Revolution.<sup>35</sup> The Ptolemaic view of our solar system held that the Earth was at the center of the system, and all the other planets and the Sun revolved around the Earth. This theory was consistent with the vast majority of the observations that presented themselves, but there were anomalies. Some planets appeared to have "loops" in their orbits around the Earth. That is, they would, over time, appear to move from, say, left to right across the sky, then appear to stop and go backwards from right to left, then appear to stop again and reverse field again, resuming their passage from left to right.<sup>36</sup> To Copernicus, Johannes Kepler, Tycho

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32. THOMAS S. KUHN, *THE STRUCTURE OF SCIENTIFIC REVOLUTIONS* (3d ed. 1996) [hereinafter KUHN, *SCIENTIFIC REVOLUTIONS*].

33. *Id.* at 10–11.

34. *Id.* at 52–53.

35. THOMAS S. KUHN, *THE COPERNICAN REVOLUTION: PLANETARY ASTRONOMY IN THE DEVELOPMENT OF WESTERN THOUGHT* (10th ed. 1979) [hereinafter KUHN, *COPERNICAN REVOLUTION*].

36. *Id.* at 48–49, 166–67.

Brahe, and others, these loops were anomalous: there was nothing in the Ptolemaic “theory” about planetary movement that would easily explain nonconstant motion. Nonetheless, able astronomers sought to explain the anomalies within the Ptolemaic view of the solar system and did so passably well—that is, successfully enough so that practitioners within the discipline accepted their emendations of the prevailing paradigm.

But Kepler and others felt that there was a simpler explanation for the anomalies—namely, to start from the wholly different view that the Sun was at the center of the solar system and that the Earth and the other planets revolved around the Sun. Among other things, this alternative view explained the “loops” in planetary motion as being due to the fact that the Earth and those seemingly looping planets were both moving around the Sun, with the loops being due to the relative motions of the Earth and those planets.<sup>37</sup>

How, Kuhn asked, were astronomers to choose between the Ptolemaic, Earth-centered view of the solar system and the Copernican, Sun-centered view?<sup>38</sup> Kuhn suggested that that paradigm that is simpler, that offers more fruitful predictions, that is more consistent with the known observations, and that promises greater advances in the future will prevail.<sup>39</sup>

Some commentators on this captivating explanation for scientific advances suggested that an important implication of the Kuhnian theory was that one paradigm is just as good as another. It was said by some that the Copernican view of the solar system is in no sense “better than” or “more real than” the Ptolemaic view; it is simply more economical.<sup>40</sup> Kuhn explicitly rejected this view.<sup>41</sup>

Kuhn’s theory, which is exciting and as initially attractive as Popper’s, also has critics. The central criticism is that Kuhn’s theory posits no way of knowing or of predicting *ex ante* when a paradigm shift is likely to occur. The idea that anomalies pile up within a paradigm, making it ultimately difficult to maintain the paradigm and more economical to adopt a different paradigm, may make sense as a broad description of scientific revolutions, but it is not particularly useful in examining the state of any given paradigm at a particular point in time. Someone observing those who practice within the paradigm would be hard pressed to say whether the number of anomalies at a particular point in time was trivial, of medium size, or so large as to make a paradigm shift imminent.

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37. See *id.* at 209–17.

38. See *infra* Part V.C, on how, according to a modern theory from the philosophy of science, scientists choose between competing explanations for the same evidence.

39. KUHN, SCIENTIFIC REVOLUTIONS, *supra* note 32, at 153–59.

40. Kuhn apparently believed in the idea that scientific revolutions were typically an advance in human understanding. But others have taken a more skeptical view. The view that modern critics have gone too far in criticizing the role of reason in human affairs, including science, is wonderfully made in STEPHEN TOULMIN, THE RETURN TO REASON (2001).

41. See KUHN, COPERNICAN REVOLUTION, *supra* note 35, at 169.

Another point frequently made by Kuhn's critics is that "even when a paradigm shift is truly profound, the paradigms it separates are not necessarily incommensurate," as Kuhn had suggested they would be.<sup>42</sup> Revolutions are not truly revolutionary; rather, they are evolutionary. Scientific progress is not accomplished in well-defined leaps from one distinct paradigm to another, but by discrete, incremental changes that, when observed in retrospect, add up to a dramatic change over time. Scientists rarely describe their own work as revolutionary. It is far more common for them to characterize their work as building on the work of others. Isaac Newton expressed this view famously in a letter to an admirer who had complemented him on his marvelous scientific advances: "If I have seen farther than others, it is because I was standing on the shoulders of giants."<sup>43</sup>

As a further illustration of the Kuhnian theory and one that comes closer to law, consider the prevailing paradigm within microeconomic theory: rational choice theory.<sup>44</sup> Rational choice theory is the paradigmatic theory of human decision making in the social sciences today.<sup>45</sup> It has proved to be so fruitful in examining economic phenomena that it has been used with similar success in disciplines contiguous to economics, such as political science, international relations, and law. That theory posits that human beings are close and accurate calculators of the costs and benefits of their prospective actions—they choose those actions that they expect to give them the greatest net benefits. They almost never make mistakes in these decisions, usually doing so only if they have been systematically misled.

This is the prevailing paradigm in the social sciences, and much important theoretical and empirical work has come from its assiduous application by generations of talented scholars. There has, however, recently been an increasing number of anomalous results that have been found in social science inquiry—results that are difficult to square with rational choice theory. Consider, for example, the finding that individuals frequently take fixed costs into their decisions in ways that rational choice theory says that they will not. Rational choice theory proposes that decision makers will realize that a fixed cost, once incurred, should be forgotten—as in the famous phrase "bygones are bygones." So, for example, once someone has purchased a season ticket to a theater series, her decision to attend any given performance should turn only on the

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42. Goodstein, *supra* note 14, at 73.

43. The view of scientific progress as incremental rather than revolutionary is famously made in ROBERT K. MERTON, *ON THE SHOULDERS OF GIANTS* (1st ed., 1965).

44. As we have seen, there is more to a paradigm than the central theoretical core. Kuhn and others mean the paradigm to include the standards or norms of the scientific community and the methods that are used by those who subscribe to the paradigm.

45. Much of this section is based on Russell B. Korobkin & Thomas S. Ulen, *Law and Behavioral Science: Removing the Rationality Assumption from Law and Economics*, 88 CAL. L. REV. 1051 (2000).

variable costs associated with that performance. She should not, that is, persuade herself to go on the ground that she has purchased the season ticket and should, therefore, get the most for her money. She has bought the season ticket whether she goes to any given performance, to all, or to none. Her decision vis-à-vis any particular production should turn only on her mood that day, the weather, her work obligations the next day, and so on.

As reasonable as this account of decision making involving fixed costs may sound, it is apparently not the way in which most human beings make these decisions. Fixed costs enter into current decision making frequently and decisively.<sup>46</sup>

These and other anomalous results have begun to pile up in the social sciences that rely upon rational choice theory. Following Kuhn's lead, one next asks whether these anomalies are so numerous that there will soon be a paradigm shift. This question turns out to be impossible to answer. The profession must address the anomalies, either by amending rational choice theory to accommodate the anomalous behaviors or by abandoning the theory altogether in favor of another.<sup>47</sup> There is currently a great deal of uneasiness in some quarters of microeconomic theory because of the anomalies,<sup>48</sup> but there is no *ex ante* way to tell whether this will be an example of one of Kuhn's scientific revolutions or merely an incremental advance within the long skein of economic theory over the last three hundred years.

There have been attempts to reconcile Popper and Kuhn, most notably the work of Imre Lakatos. Lakatos suggested that scientific communities engage in research programs that cannot be falsified by one or a small number of results.<sup>49</sup> However, there may be a "degenerating research program" through an accretion of anomalous results and observations. The community can resist falsification of the program through the accretion of anomalies by *ad hoc* alterations in the research program. For example, under the Newtonian theory of planetary movement, there are anomalous movements in the planet Mercury. Newtonians can salvage the theory by positing the existence of "Vulcan," an invisible planet

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46. *Id.* at 1069–70.

47. One frequently hears it said that "it takes a theory to beat a theory." Those who invoke that view are typically doing so in order to justify maintaining the rational choice paradigm until some well-articulated theory comes along to replace it. That is not, however, how scientific advances typically occur. Rather, anomalies arise and are either explained within an amended paradigm or serve as observations that make a case for another paradigm (as yet undefined).

48. There may be uneasiness, but there is also increasing recognition of the importance within the economical profession of providing an account of these anomalies. One testament to this importance is the fact that Professor Matthew Rabin of the Department of Economics, University of California at Berkeley, a prolific scholar in behavioral economics—which focuses on anomalies in the predictions of rational choice theory—recently won the John Bates Clark Medal of the American Economic Association, an award given biennially to the leading economist under the age of forty. Many winners of the Clark Medal have later won a Nobel Prize.

49. See IMRE LAKATOS, THE METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMMES (PHILOSOPHICAL PAPERS, VOLUME I) (G. Currie ed., 1980).

between Mercury and the Sun. Eventually, however, these *ad hoc* alterations must themselves come within the research program—that is, Vulcan must be discovered to exist—or the program must give way to another.

An important element of science and the scientific method that is worth mentioning and that Kuhn stressed is that there are reasonably well-defined norms that prevail within the scientific community about methods of investigation and behavior. This is a very large subject, one that I introduce only to draw attention to the fact that in today's scientific community there is vigorous competition among scientists to advance knowledge in their various areas, that there is a surprising amount of cooperation among scientists with respect to data and results, and that there is a strong ethic of publishing one's results in peer-reviewed journals and participating in periodic gatherings of researchers working in one's area of expertise.<sup>50</sup>

### C. *Bayesian Inference and Scientific Discovery*

Many, if not most, philosophers of science today subscribe to a Bayesian theory.<sup>51</sup> To see how that theory might describe what it is that scientists do, I will first explain Bayesian inference and then show how it might describe the scientific method.

#### 1. *Bayesian Inference Generally*

Bayesian inference is a probabilistic method of systematically incorporating new information into one's prior beliefs.<sup>52</sup> It stands in contrast to classical—sometimes called “Neyman-Pearson”—probabilistic inference. The distinction may be seen in a famous example of inference. Suppose that someone is flipping a coin and counting the heads-and-tails outcomes. The difference between Bayesian and Neyman-Pearson methods has to do with the inferences that the two techniques draw from the record of coin flips.

Neyman-Pearson techniques assume that the coin is fair and that, given a large number of flips, the number of heads and of tails will be equal. A long string of heads will not necessarily cause someone evaluating from the Neyman-Pearson standpoint to question her assumption that the coin is fair. Rather, she is likely to say, “Be patient. If we will continue to flip the coin, there will be an offsetting increase in the number of tails so that over a large number of flips, the proportion of heads to tails will be equal.”

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50. Goodstein, *supra* note 14, at 74–75.

51. See, e.g., COLIN HOWSON & PETER URBACH, *SCIENTIFIC REASONING: THE BAYESIAN APPROACH* (1st ed. 1989); PATRICK MAHER, *BETTING ON THEORIES* (1993).

52. See generally HOWSON & URBACH, *supra* note 51.

In contrast, a Bayesian may, like his Neyman-Pearson counterpart, begin from a presumption that the coin is fair. But if the number of one of the two possible outcomes begins to predominate, the Bayesian will, in the parlance and in light of the accumulating evidence, “update” his “prior belief” of the coin as being fair. The evidence may persuade him that there is an increasingly strong probability that the coin is biased. Someone committed to evaluate the results of the coin flips from a Neyman-Pearson perspective can, of course, also eventually conclude that the coin is not fair. But the distinguishing fact is that Bayesians have a systematic way of reaching this conclusion, while Neyman-Pearson followers must resort to abandoning their assumption of fairness without a systematic means of reaching that abandonment.

## 2. *An Example*

Bayesian inference provides a systematic means of determining the probative value of new information. Moreover, as we shall see, this style of inference provides a means of measuring the degree to which new information ought to alter one’s prior beliefs. Consider an example.

Suppose that you are a physician. A patient comes to your office to tell you that she has discovered a small lump in her breast and is worried that it might be cancerous. You examine her and then tell her—on the basis of everything you know about breast cancer, about this particular patient, about women with her kind of medical history, and other relevant information—that the probability of her having breast cancer is 0.10. That seems like a relatively low probability, and yet to be prudent, you recommend a mammogram. You know that in women of this type who have cancer, the mammogram will correctly detect cancer 90% of the time. In women who do not have cancer, the mammogram will falsely indicate cancer 20% of the time. The mammogram comes out positive—i.e., it indicates that the patient has breast cancer. What is the probability that the woman actually has breast cancer?

Suppose that there are 100 women who have the same medical history and other relevant characteristics as this patient. Suppose further that 10 of them really have breast cancer and 90 do not. Finally, suppose that of the 90 who do not have cancer, 20% will show a positive mammogram—i.e., a false positive. Eighty percent of those 90 will *not* have a positive mammogram—sometimes called a “true negative.” Of the 10 who *do* have cancer, 90% will show a positive mammogram (a “true positive”), and 10% will not show a positive mammogram (a “false negative”).

We can tabulate these results as follows:

TABLE I  
RESULTS OF CANCER SCREENING  
(100 patients screened: 10 truly have cancer, 90 do not)

Result	Percentage Accurate	Number	Characterization
No cancer and a positive mammogram	20	$90 \times .020 = 18$	False positive
No cancer and a negative mammogram	80	$90 \times 0.80 = 72$	True negative
Cancer and a positive mammogram	90	$10 \times 0.90 = 9$	True positive
Cancer and a negative mammogram	10	$10 \times 0.10 = 1$	False negative

The patient has a positive mammogram; therefore, she must fall into one of the two groups that have a positive mammogram. There are 27 patients who have positive mammograms. But of these 27 patients, only 9 really have cancer. Therefore, the probability that the patient has cancer is  $9/27$  or 0.33. It is far more likely that she was one of those who did not have cancer and who had a positive result than that she really had cancer.

### 3. *Bayesian Decision Making and Science*

Bayesian inference has had a direct impact on the philosophy of science. Specifically, this technique has been useful in giving an account of how scientists might choose among competing theories on the basis of evidence. The situation that is of interest in this regard occurs when there are alternative theories for the same natural phenomena and where the question arises as to whether some new evidence tolls more in favor of one theory than the other. Philosophers of science refer to this branch of their study as "confirmation theory."

Confirmation became a central concern for the philosophy of science as the result of Thomas Kuhn's historical studies of paradigm shifts. Some early critics of Kuhn pointed out that when two paradigms were competing (or when any two theories were competing), there had to be some systematic means of the scientific community's determining which of the two paradigms was strengthened and which weakened by new evi-



dence. In seeking to address this question, some philosophers proposed Bayesian inference as a model of how scientists made these confirming and refuting choices.<sup>53</sup>

The application of Bayesian inference to the issue of the confirmation of scientific theories is straightforward. Assume that there is a theory that commands some, but not universal, support among those learned in the field. The probability that the theory is true, or the degree of belief in the theory, constitutes the prior probability in Bayesian inference. Now suppose that some scientist, following the study and publication protocols within his or her field, publishes a new piece of evidence. Should scientists revise their degree of belief in the theory on the basis of the evidence?<sup>54</sup> There is a formal method of explaining the degree to which the new evidence, if probative, should influence one's prior beliefs. Those are the Bayesian equations for updating prior beliefs to become posterior beliefs.

The Bayesian view of scientific confirmation, like the other theories from the philosophy of science that we have surveyed, has critics. The most forceful of them is Professor Clark Glymour.<sup>55</sup> Several of Glymour's criticisms are technical and directed at the underlying assumptions of Bayesian probability and not just at its application to the philosophy of science. For example, Glymour finds difficulties in accepting the view, usually associated with Bayesians, that probabilistic judgments represent degrees of belief, that those degrees of belief necessarily obey the probability calculus, and that degrees of belief alter according to the rules of conditionality captured in the Bayesian formulas.<sup>56</sup> Moreover, he mentions the well-known criticism of Bayesianism that "it fails to provide any connection between what is inferred and what is the case."<sup>57</sup> That is, if prior probabilities correspond to personal degrees of belief, then all that Bayesianism gives us is an account of how rational people ought to revise their personal degrees of belief but no account whatsoever of how rational people come to believe true statements or to eschew falsehoods. A standard reply to this criticism is that even if prior probabilities are merely personal, then over enough time those personal degrees of belief will converge to objective truth for each individual and for all individuals.<sup>58</sup> Another reply that one might make is that when applied to scientific theories, the Bayesian degrees of belief represent not mere personal peccadilloes but the agreed-upon belief of a well-defined and learned scientific community.

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53. See generally HOWSON & URBACH, *supra* note 51; MAHER, *supra* note 51.

54. See Wesley C. Salmon, *Rationality and Objectivity in Science* or *Tom Kuhn Meets Tom Bayes*, in SCIENTIFIC THEORIES, 14 MINNESOTA STUDIES IN THE PHILOSOPHY OF SCIENCE (C. Wade Savage ed., 1990).

55. See CLARK GLYMOUR, THEORY AND EVIDENCE 63–93 (1980).

56. See *id.* at 67–75.

57. *Id.* at 72.

58. *Id.*

Professor Glymour directs his other criticisms specifically at the use of Bayesian inference to explicate science. For example, he does not believe that probabilistic reasoning has been an explicit part of scientific reasoning for very long and certainly not in the grand scientific debates of the sixteenth through the early-twentieth centuries.<sup>59</sup> I do not think this is a particularly forceful criticism. Even if probability theory and especially its Bayesian version are relatively new developments in human history, it does not follow that implicit probabilistic reasoning has not been a part of scientific thinking for much longer than it has been formalized. By analogy, rational choice theory has been formalized only in the last forty years or so, but economists and historians have made great strides in historical explanation from attributing that theory to decisions made centuries before economists formalized the theory.

Glymour makes two additional criticisms of Bayesianism as applied to science. The first is that, even if the account gives a plausible account of confirmation within a scientific community, it fails to explain other fundamental aspects of the confirmation process.<sup>60</sup> For example,

there are a variety of methodological notions that an account of confirmation ought to explicate, and methodological truisms involving these notions that a confirmation theory ought to explain: for example, variety of evidence and why we desire it, ad hoc hypotheses and why we eschew them, what separates a hypothesis integral to a theory from one “tacked on” to the theory, simplicity and why it is so often admired, why “deoccamized” theories are so often disclaimed, what determines when a piece of evidence is relevant to a hypothesis, what, if anything, makes the confirmation of one bit of theory by one bit of evidence stronger than the confirmation of another bit of theory (or possibly the same bit) by another (or possibly the same) bit of evidence.<sup>61</sup>

Second, Glymour contends that the Bayesian view does not really address what we most want to know in scientific debates—namely, what counts as a persuasive argument:

What we want is an explanation of scientific argument; what the Bayesians give us is a theory of learning, indeed a theory of personal learning. But arguments are more or less impersonal; I make an argument to persuade anyone informed of the premises, and in doing so I am not reporting any bit of autobiography. To ascribe to me degrees of belief that make my slide from my premises to my conclusion a plausible one fails to explain anything not only because the ascription may be arbitrary, but also because, even if it is a correct assignment of my degrees of belief, it does not explain why what I am doing is *arguing*—why, that is, what I say should

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59. *Id.* at 91.

60. *Id.* at 63–69.

61. *Id.* at 67–68.

have the least influence on others, or why I might hope that it should.<sup>62</sup>

These are powerful points, but they are not insurmountable. We might answer the first criticism, for example, by noting that it is true that the Bayesian account does not directly address those aspects of the scientific method. But other aspects of the philosophy of science beyond confirmation theory do attempt to give accounts of the questions that Glymour raises about the additional elements of the method. As to the second point, I believe that Glymour misses the fact that Bayesian theory implicitly discusses what counts as a persuasive argument in its discussion of what should count as probative evidence. By implication, evidence that has no probative value will not alter the prior probabilities. So, it is a partial answer to this second criticism to say that Bayesianism suggests that only truly probative evidence counts as an “argument”—that is, as evidence that ought to alter one’s prior probabilities or prior degrees of belief.

The Bayesian view does not resolve some of the thornier core questions in the philosophy of science. But there is one very important advance that comes from the introduction of Bayesianism: the theory seems to give a particularly good view—a positive, not normative, account—of how the scientific community decides to accept new evidence that confirms or refutes an existing theory.

#### IV. LAW AS SCIENCE

Now that we have a sense of what constitutes science and what the modern view of the philosophy of science is regarding science and verification of scientific hypotheses, we are in a position to inquire whether the academic study of law is scientific or is in the process of becoming scientific.

In this section, I first address the question whether the study of law is a science, using the understanding of that word that I developed in Parts II and III. I claim that there are already some elements of the scientific method in the study of law but that there are some important defining elements that are missing—namely, a widespread and commonly accepted theoretical core or paradigm and accepted standards and methods of empirical or experimental verification. I consider the general case of whether doctrinal scholarship had the hallmarks of scientific inquiry and then consider three specific examples that illustrate, I believe, some hopeful signs that the scientific method has long been more implicit in legal scholarship than is generally thought to be the case and is becoming more common in the study of law. I conclude this section with specula-

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62. *Id.* at 74–75.

tion on what more may happen in the near future to increase the scientific nature of legal scholarship.

### A. *Legal Theory*

There are some aspects of the scientific method that the study of law shares with, among other sciences, those for which there is a Nobel Prize; and there are some aspects of that method that the study of law does not share.

The study of law, in whatever society, concerns the same general subject matter—namely, all aspects of the social system of regulation promulgated and enforced by the legitimate organs of government. Included among these aspects are: the determination of what constitutes a legitimate regulation; how those whose behavior might be affected by the regulation might and do, in fact, respond to that regulation; how those who are learned in these legitimate regulations—lawyers—train and operate within the system; how courts, legislatures, and agencies of the executive branch affect the law; and how the relationship between nongovernmental methods of regulation, such as the family and social norms, affect behavior and interact with the legal system.

While there may be agreement among students of the law about the subject matter of their inquiries, there is less agreement about the methods by which these inquiries may acceptably proceed and about the sorts of conclusions by which innovative results are to be found and probed—recall that one of the elements of a science was an agreed-upon method by which to establish and refute claims, even if there was disagreement about the probative value of particular claims. One measure of this lack of an essential element within the scientific study of law is the difficulty of transboundary communication within the study of law. I mean to draw attention to the fact that it is exceedingly difficult for students of law in one country to communicate as meaningfully and forcefully with students of law in another country as scientists in other disciplines do.

To see the difference between law and other disciplines in this regard, consider the situation in medicine or physiology (a discipline for which there is a Nobel Prize). Suppose that a researcher in Moldova publishes a piece that claims to have found some new insight into the successful treatment of prostate cancer on the basis of her studies of patients with that disease in Moldova. First, recognize that there would be general agreement among every scholar who might be interested in this result regarding the methods that the Moldovan researcher should have used to reach her conclusion. There are really two aspects to this general agreement. The subject matter on which the researcher concentrated—“prostate cancer”—must be the same as that understood by other researchers everywhere. And, just as importantly, the research protocols that she used to investigate Moldovan patients must generally be the same as those in use in other studies of the same subject matter. Sec-

ondly, and very importantly, physicians in any country in the world can understand and evaluate the Moldovan study and, if its results withstand scrutiny, apply the insights of the study in their treatment within their own countries. Assuming that the Moldovan study survives careful evaluation by other scholars, no one would claim that that study applies only to prostate cancer patients in the Republic of Moldova.

We can extend these same remarks even beyond the physical, biological, and natural sciences to include statements about some of the social sciences. For instance, consider economics. Generally speaking, economic theory and econometric techniques apply in every country in the world. Microeconomists, for example, share the same core theoretical principles—e.g., the use of rational choice theory to hypothesize about decision making—regardless of where they live and practice. As a result, their hypotheses about how people might behave are, in a very real sense, independent of time and place—they apply to nomads in remote areas of Mongolia as well as to urban residents of Paris and they apply to decisions made by farmers today and to farmers 10,000 years ago. There are differences in the economies of different countries, but those differences can be explained by differences in relative costs, technology, institutions, and tastes. And importantly, there is general agreement among economists in any country about what would constitute a valid test of any hypothesis and the techniques by which those tests should be run. Moreover, an innovation in theory or in practice—for example, a new insight into behavior or a superior econometric method of gathering and testing data—would be potentially useful to any economist studying any economic decision making. Finally, it is worth remarking that a study of Moldovan economic behavior—or of any Moldovan behavior as predicted by rational choice theory—supported by econometric results is of potential interest to economists in any country.

Notice that one might reply that a limitation on the transboundary utility of economics might arise because of the vast differences in local conditions. Some countries are wealthy in natural resources, have a long history of vigorous scientific and technological innovation, and are blessed with pleasant climates and few natural or man-made disasters. Others are poor in valuable resources, have no tradition of entrepreneurship or of scientific and technological innovation, and are so bakingly hot that outdoor work is virtually impossible for large segments of the year. Some societies have governments that facilitate economic growth; others do not. Some have open borders and respond positively to foreign competition; others do not.<sup>63</sup>

And yet, these vast cultural, resource, governmental, and other differences notwithstanding, the same economic theory applies to them all. Nor is it the case that there are a few competing economic theories

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63. For a discussion of these and other factors—particularly the influence of “culture”—in economic development, see DAVID LANDES, *THE WEALTH AND POVERTY OF NATIONS* (1998).

and that various countries today and throughout human history can be grouped into families or typologies explained by one of these alternative theories. The same theory that posits identical decision-making processes and econometric validation techniques also explains the vast differences that exist in economies. So, for instance, differences in the mix of agricultural, manufacturing, and service industries across national borders could be explained in large part by differences in the factor endowments, technologies, institutions, tastes and preferences of the populace, and relative prices within the economy. The theory may also step outside the strict confines of economics and appeal to ad hoc explanations for certain historical—or path-dependent—factors, such as political dispositions against democracy or historical patterns of vulnerability to foreign invasion and exploitation or widespread religious convictions.

The central point of this example is that there is a single, core economic theory of human decision making and of economic development, just as there is a single core theory of physics, chemistry, microbiology, human genetics, and physiology and medicine.<sup>64</sup>

We turn now to law, where the situation stands in very stark contrast to that of the examples of the Moldovan physiologist or economist. Consider what would happen if a student of law in Moldova were to write something about a change in contract law that the courts in that country may have effected. Let us assume, for the sake of argument, that the writing is in English and is lucid. The first thing to recognize is that this scholar's writing is likely to be of interest only to students of Moldovan contract law, including practitioners, but it is highly unlikely that the piece will be of interest to students of law in other countries. The reason, simply, is that legal scholars tend to write scholarly articles only about their own legal systems and only for those acting within that legal system. It is as if the Moldovan physician were to write only about prostate cancer diagnosis and treatment techniques within Moldova or the Moldovan economist had a theory that applied only to decision makers in that Republic.

To put the issue plainly, there is no accepted theory of law that applies to every legal system and to which legal scholars in every country can appeal in explaining the particular institutions or rules of their own systems.<sup>65</sup> That is, legal scholars do not seek to explain either the simi-

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64. I have drawn this stark and only very slight exaggerated portrait of the universality of microeconomic theory not to champion microeconomics per se but to show that there are social sciences that have the same transnational uniformity as do the physical, biological, and natural sciences.

65. I am assured by European legal scholars that theory forms a vital part of their enterprise. Indeed, in many European law schools students take courses early in their legal education on "general legal theory." The two competing general theories of law that command allegiance among continental lawyers are positivism and natural law. I am reluctant to characterize these views hastily, but, having broached the topic, I feel obliged to say something, however briefly. Positivism suggests that the law is to be found only in the legitimate laws of the jurisdiction—e.g., in the statutes duly passed by the legislature. Natural law suggests that there is a prior body of law, one that is given by nature, logic, and,

larities among or the differences between legal systems by appeal to a shared core theory of law.<sup>66</sup>

This observation raises this question: “Why should there be a transnational scholarly community—a shared theoretical core, shared procedures for establishing important statements in the community, and shared standards for important and good work—in the physical, natural, and biological sciences and in several of the social sciences but not in the scholarly study of law?” This is a very large question—one that could occupy scholars for a long time. I do not intend to try to answer that question here, but I strongly suspect that there are two important fundamental points that will form the central part of the answer. First, legal scholars have, until relatively recently, been highly skeptical of grand theorizing and have positively rejected empirical techniques as a standard method of resolving questions about legal issues. Second and relatedly, legal scholars in every country have been closely allied to the practical issues of legal decision making and only distantly interested in studying the legal system as an academic discipline. Of course, a central concern of legal scholars for hundreds of years has been the training of practitioners, and that concern demands that we make familiarizing students with the practicalities of the practice of law a central point of legal education. In the end, I believe that even if the study of law has begun to look more like a science than it has in the past, we are still at a very early point in that development, though signs of the “scientification” of legal scholarship are increasingly evident.

I have said that the explanation for the puzzling lack of theory in law is a topic beyond my scope here, but I do want to make a few more observations about this matter. First, I have found that when discussing the undertheorization of law with colleagues, they often point to two factors as making a universal theory of law unlikely: (1) the vast differences in the cultural context of every legal system, and (2) the historical or path-dependent nature of legal development. It is not clear to me why these points should necessarily lead to a lack of universalizable legal theory. One might make precisely the same claims about anthropology, sociology, economics, political science, or any other social science; societies are so very different from one another that there are few if any unifying

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perhaps, God, to which the earthly makers of law must give way. There are profound practical consequences of these competing views, particularly with regard to how one interprets law. For my purposes, the more important observation is that these competing theories are ones in which disagreements are to be resolved, if at all, by appeal to hypothetical-deductive argument and not by appeal to empirical work. That is, these are jurisprudential or philosophical disputes, which makes them different from science.

66. Notice that I confine my observation to the fact that there is no shared *theory* of law. I do not contend that there is no shared subject matter among students of law, nor that there is a cacophony of procedures for determining valid statements within law. I believe, as I have already asserted, that there is a shared subject matter. With respect to validation procedures, I believe, as I shall elaborate below, that legal scholars have by and large ignored empirical techniques as a method for validating legal claims.

themes that might form the core of a universal theory.<sup>67</sup> And yet, scholars in those and other social sciences have sought, and in some cases found, transnational theories. At a minimum, scholars in those areas of social science where there is no universal theory still share common scholarly values with regard to what should count as valuable research—value that is almost always transnational.

The study of law again looks to be different from the other disciplines that study social arrangements. In principle, there is no reason why one could not articulate a theory that explained the differences (or similarities) in legal systems according to such factors as differences in resource costs, technology, institutions, individual and societal tastes, and the society's history (or path dependence). But there is extremely little of that sort of work going on within the legal academy, as evidence of which I cite the fact that there is no standard, widely accepted theory of legal change or difference.

Second, another explanation that I have sometimes heard for the undertheorization of law is that the central study of any nation's legal scholars has always been the doctrines and practices of that nation's legal community. There are, I believe, two related elements to this claim. One claim—with which I am sympathetic—is that for a very long time the focus of legal scholarship has been turned toward the practice of law. That is, legal scholarship in almost every country, and particularly in the United States, has been a special ancillary of the practicing bar. Some of the most valuable work of the so-called doctrinal scholars was directed at finding unifying patterns in the welter of a jurisdiction's pronouncements on a particular legal topic.<sup>68</sup> The fact that there is, as Judge Harry Edwards has persuasively demonstrated, a "growing disjunction" between the legal academy and the bench and bar is strong testimony to the fact that this process of the theorization of law has recently begun in earnest.<sup>69</sup>

The other claim—with which I am not in sympathy—is that the interesting aspects of any legal system are its particularities, not its generalities. The practicing lawyer may well need to know the particularities of a doctrine in his or her jurisdiction so as to represent a client's interests effectively. But a legal scholar, unless acting as an adviser to the

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67. One could make a similar claim with respect to plant and animal biologists: flora and fauna are highly localized, but the same theories and categorizations apply to *all* plant and animal types, regardless of where in the world they are found.

68. I consider a science-like study within the doctrinal school in *infra* Part IV.C.

69. See, e.g., Harry T. Edwards, *The Growing Disjunction Between Legal Education and the Legal Profession*, 91 MICH. L. REV. 34 (1992) (arguing that the interests of legal academicians and of the practicing bar and judges have diverged, leading to a profound change in the relationship between legal education and the practice of law). Also, Dean Anthony Kronman, in *The Lost Lawyer* (1993), argued strenuously that the practical wisdom that had characterized the great lawyers of a half-century ago has been abandoned in favor of a vulgar scientism that ill suits the law. See also R. George Wright, *Whose Phronesis? Which Phronemoi?: A Response to Dean Kronman on Law School Education*, 26 CUM. L. REV. 817 (1995-96).



practitioner, does not necessarily need to know those particularities. An analogy may help. Just as there are academic economists who study broad generalities of the economy, without particular regard to time and place, there are also economists who work for banks, think tanks, non-governmental organizations, and other entities for whom the central concerns of their professional lives are the particularities of the economy in which they operate. Presumably a core and shared theoretical understanding informs those economists' understanding of the appropriate particularities to which to pay attention. Their relationship to academic economists is, I suggest, like that of practicing attorneys to legal academics.

Naturally, there must be more to the story than these factors. One could, for instance, argue that exactly the same factors to which I have just pointed in explaining the lack of a core theory in law might apply to physiology or medicine: techniques could be entirely local, and scientific scholarship could take a back seat to training practitioners, but that does not appear to be the case in those other disciplines. Indeed, those who intend to teach, research, or practice medicine, economics, or business frequently come to the United States to study prior to returning to their home countries. That also happens in law, but to a somewhat surprisingly smaller extent than is the case in other academic pursuits.

I conclude that there is no persuasive case for "legal exceptionalism"—i.e., for the view that law is inherently different from other academic disciplines that characterize themselves as scientific. The study of law could be a science with shared transnational theoretical understandings and an experimental and empirical method for confirming claims within those understandings. The puzzle is—if it could be a science, why is it not?

### *B. Empirical and Experimental Work in the Study of Law*

In the previous section, I argued that there is a puzzling lack of a core, shared theory across national boundaries in the academic study of law and instead, a focus on contextual particularities. But there is more to my inquiry into why the study of law is not yet a "legal science" for which a Nobel Prize might be awarded. The second absence that strikes an observer of legal scholarship is that of empirical and experimental work—at least by comparison to other academic disciplines studying social phenomena. In this section, I first raise some general considerations regarding the value of empirical work in law.<sup>70</sup> I argue that even if there has been a palpable and quizzical undertheorization of law, there has

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70. I shall hereafter refer only to "empirical" work, but I mean to include under that heading experimental work, whose value in studying law has been illustrated forcefully in Randy Picker's contribution to this symposium and in such work as Russell B. Korobkin, *The Status Quo Bias and Contract Default Rules*, 83 CORNELL L. REV. 608 (1998).

been—at least since the time of the legal realists—a strong demand within the legal academy for empirical verification or refutation of claims about law. Next, I illustrate that point by examining three empirical studies of important legal issues. Part of my reason for looking at these studies is to strengthen my general claim that empirical work is an absolutely vital part of the development of a mature legal science.

1. *The Value of Empirical Work in the Study of Law*

One might argue that the dearth of empirical and experimental work is not independent of the lack of a core, shared theory in the study of law. If an academic discipline has not included theory as part of its canon, then it is unlikely to have a need for techniques of empirical validation of the theory. But with respect to law, the matter is more complicated than that.

A discipline does not typically develop an empirical body of literature if empirical verification is not an important part of the discipline's mission. Philosophy and literature studies are important examples of disciplines where empirical techniques are not highly relevant or valued. The propositions of philosophers, for example, are largely to be resolved only by appeal to hypothetical-deductive methods. In deciding what is right and good, no philosopher makes her appeal to a careful empirical examination of actual behavior in different societies (although they do, somewhat curiously, appeal to widely held but unarticulated “intuitions”). But law is not like that. Although the logical coherence of doctrine is a vitally important element of law, another important element is its effectiveness. There is a transparent value to empirical work to determine how people respond to various legal rules and standards. So, while the absence of theory may explain an absence *among scholars* of a burning desire to validate or refute the theory through empirical work, among *practitioners* of law there ought to be a deep interest in empirical validation.

The puzzling absence of a core theory in law has not, somewhat curiously, led to the total absence of empirical work in law. Indeed, as I shall show shortly, even the doctrinalists had an interest in empirical work, and, indeed, they did some empirical work. The reason for this interest in empiricism in the absence of theory lies, I believe, in the pattern of the development of American jurisprudence in the twentieth century.<sup>71</sup> When legal formalism gave way to legal realism in the 1930s and 1940s, the desire for empirical work was born.

A legal formalist has no particular reason to value empirical or experimental work.<sup>72</sup> As with philosophical arguments, the formalist's test

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71. See generally NEIL DUXBURY, PATTERNS OF AMERICAN JURISPRUDENCE (1995).

72. I argue below that this revival of formalism is to be distinguished from the formalism of the late-nineteenth and early-twentieth centuries. The new formalism is closely allied, I believe, to the

of validation is the hypothetical-deductive method. The doctrines must cohere within the area of particular inquiry and across subdisciplines within law. Presumably, once the formalist achieves that complete logical coherence, her task is over until some random shock disturbs the system, and it returns to coherence equilibrium. But the legal realists found this sterile and preferred to focus on the law's effects and on determining what the law *is* through, as Justice Holmes famously put it, a "prediction of what a court will do."<sup>73</sup> There is an unavoidable empirical thrust to this view of law. To take the most obvious example, you must make some systematic inquiry into a court's decisions in order to make an objectively accurate prediction of how the court will rule in a given dispute. And to extend the view, once the realists suggested that the consequences of a law should form part of the evaluation of the law's worthiness, there inevitably had to be some method of determining a law's consequences. As a result of these and other developments of the past sixty years or so, empirical work has become a part—but only a small part—of legal scholarship.

This ground for performing empirical work has led, until recently, to a particular kind of empirical work. Most of it has been descriptive rather than analytical, directed at counting or tabulating rather than explaining. This should come as no surprise. A discipline that has a core theory undertakes empirical work that is closely tied to probing aspects of that theory. As such, that empirical work usually seeks to explain some proposition derived from the core theory. There may also be important description and tabulation designed to shed light on the subject matter of the discipline, but the explanatory power of empirical work is its highest calling in those disciplines in which there is a core theory.

I want to be clear that I am not disparaging some types of empirical work and exalting others. Rather, I am seeking to draw a distinction between the particular ends that empirical work can serve—descriptive or analytical. And I am drawing attention to the fact that there is a correlation between the kind of empirical work that members of a discipline do and whether that discipline has a shared theoretical core. I illustrate these propositions in the next section.

## 2. *Three Examples of Empirical Work in the Study of Law*

I have argued that at least since the legal realist revolution, legal scholars have valued empirical work and have done important empirical work. I have also asserted that that empirical work would be likely to be descriptive rather than analytical to the extent that there is no shared theoretical core in the study of law. In this section, I shall seek to illus-

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development of a universal legal theory, which, as I have argued, is a very important element of the scientific method.

73. O. Holmes, *The Path of the Law*, 10 HARV. L. REV. 457, 460–61 (1890).

trate these propositions by examining three empirical studies of legal subjects.

a. Promissory Estoppel and Contract Theory

One of the themes that I hope to sound in this article is that there has always been an element of scientific inquiry in legal scholarship. And, moreover, as in the other natural and social sciences, there has been a feedback from empirical observation to theory to new empirical observation. That is, a scholar proceeds by subjecting a prevailing theory to empirical confirmation, discovers that that empirical investigation does not, after all, confirm the theory, and revises the theory in light of the observations. Some subsequent scholar then may undertake empirical work to confirm the revised theory. This prototypical description of the manner in which science proceeds may not apply to all legal scholarship, but, somewhat surprisingly, there has long been an important strand of that form of scholarship within the study of law. But it is a mere strand, not a complete fabric.

To illustrate this scientific trend in the law, I briefly consider two important articles on promissory estoppel: that by Professor Stanley Henderson<sup>74</sup> and that by Professors Daniel A. Farber and John H. Matheson.<sup>75</sup>

The theory upon which a court may enforce a contractual promise on the basis of promissory estoppel is, and has long been, well known.<sup>76</sup> The common understanding of that doctrine is that it is one of two bases of contract enforcement. The more common basis for enforcement is consideration in the context of a bargain promise. Reasonable detrimental reliance by the promisee forms the basis for contract enforcement, it is generally thought, in relatively rare cases.<sup>77</sup>

In the late 1960s Professor Henderson surveyed every case in the United States over the previous ten years in which the plaintiff made a plea for promissory estoppel under Section 90 of the *Restatement of Contracts*. That empirical inquiry caused him to question whether and how courts were enforcing contractual promises under a theory of promissory estoppel based on reasonable detrimental reliance. First, he found it puzzling that courts had not “establish[ed], in Section 90, a separate identity for the reliance principle of promissory estoppel . . . .”<sup>78</sup> Rather,

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74. Stanley D. Henderson, *Promissory Estoppel and Traditional Contract Doctrine*, 78 *YALE L.J.* 343 (1969).

75. Daniel A. Farber & John H. Matheson, *Beyond Promissory Estoppel: Contract Law and the “Invisible Handshake,”* 52 *U. CHI. L. REV.* 903 (1985).

76. See *RESTATEMENT (SECOND) OF CONTRACTS* § 90 (1981) (“A promise which the promisor should reasonably expect to induce action or forbearance on the part of the promisee or a third person and which does induce such action or forbearance is binding if injustice can be avoided only by enforcement of the promise. The remedy granted for breach may be limited as justice requires”).

77. See Farber & Matheson, *supra* note 75, at 945.

78. Henderson, *supra* note 74, at 346.

courts have been so wedded to the notion of bargain promises as the appropriate class of enforceable promises that they have sought to bring the reliance basis of enforcement within the ambit of bargain rather than establish it as a separate basis for enforcement.<sup>79</sup> For instance, “action in reliance is commonly seen as a tool for sorting out motives which bear on the issue of exchange.”<sup>80</sup> This tendency then leads to bargain swallowing reliance as the basis of contract enforcement: “Reliance, under this approach, functions not as a substantive ground for enforcement, but as a vehicle for identification of some other ground for enforcement.”<sup>81</sup>

Second, Professor Henderson’s survey of the cases involving promissory estoppel indicated that in every one of them, the parties were engaged in a commercial negotiation or exchange.<sup>82</sup> This is particularly surprising in view of the origins of the doctrine of promissory estoppel and of the supposed premises of bargain promise enforcement. Students of contract law associate the doctrine of reasonable detrimental reliance with instances of gratuitous promise, as in the famous case of *Hamer v. Sidway*<sup>83</sup>—circumstances, more generally, in which there is no bargained-for performance. In contrast, there was thought to be no need to expand the basis of contract enforcement within bargain promises because consideration served well enough. In light of these well-established propositions, Professor Henderson’s finding that promissory estoppel was invariably associated with instances of commercial bargain or negotiation was puzzling.

Characteristically for the time at which he wrote the article, Professor Henderson, having made these significant discoveries about the actual use of promissory estoppel, was reluctant to draw grand theoretical conclusions from his empirical findings:

Given the vast factual differences among the Section 90 cases, any attempt to dispose of the problems of promissory reliance by a single formula is hazardous because no formula can be comprehensive enough to resolve every case satisfactorily. But the major impact of promissory estoppel in recent years may be that it has made the whole matter of classification or definition less important in the decision of contract cases.<sup>84</sup>

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79. *Id.* at 346–47.

80. *Id.* at 348.

81. *Id.*

82. *Id.* at 352. Professor Henderson reports that there were 100 decisions in his survey in which “promissory estoppel was considered as the ground of decision in a clear bargain transaction.” *Id.* at 352 n.37. He notes further that in one-third of those cases promissory estoppel was the sole or alternative basis of enforcement. Professor Henderson is here making the point that the default method of pleading in the 1960s had become to premise a contract enforcement action on both a theory of consideration and of promissory estoppel simultaneously, “urging the same allegations and evidence in support of both theories.” *Id.* at 352.

83. 27 N.E. 256 (1891).

84. Henderson, *supra* note 74, at 387.

Professor Henderson notes that there is change occurring through the “quiet manipulation of the familiar labels.”<sup>85</sup>

Sixteen years after Professor Henderson’s survey, Professors Farber and Matheson conducted a new survey of actions in which plaintiffs asked for promissory estoppel.<sup>86</sup> Their findings were even more far-reaching than Professor Henderson’s. Farber and Matheson examined every case in which Section 90 of either *Restatement* was cited—over two hundred cases—in the ten years prior to their article. They then drew four important conclusions. First, “promissory estoppel is regularly applied to the gamut of commercial contexts”—to cases involving construction bids, employee compensation, lease agreements, stock purchases, and the like.<sup>87</sup>

Second, “promissory estoppel is no longer merely a fall-back theory of recovery.”<sup>88</sup> That is, courts are now comfortable enough with the doctrine to apply it in virtually any contract dispute. This finding is, of course, at odds with the concept in contracts casebooks that promissory estoppel is inappropriate or unnecessary in bargain promises. But the law in action is apparently different from the law on the books.

Third, “reliance plays little role in the determination of remedies.”<sup>89</sup> This, perhaps, should come as no surprise. The first *Restatement* had held that an innocent party was entitled to full expectation damages even if reasonable detrimental reliance was the basis for enforcement.<sup>90</sup> But some subsequent cases had allowed for the recovery only of reliance expenditures, and the second *Restatement* apparently recognized this development by allowing for partial recovery rather than full expectation damages.<sup>91</sup>

Fourth, and in the opinion of the authors the most important finding, reliance no longer matters very much in determining contractual liability under Section 90.<sup>92</sup> Rather, courts are apparently willing to premise liability on factors other than detrimental reliance, reasonable or not.<sup>93</sup>

I noted above that Professor Henderson had been reluctant to translate his empirical findings into an explicit revision of the theory of contract enforcement. It is an important sign of how much things had changed in the legal academy by the mid-1980s that Professors Farber

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85. *Id.*

86. *See generally* Farber & Matheson, *supra* note 75.

87. *Id.* at 907.

88. *Id.* at 908.

89. *Id.* at 909.

90. RESTATEMENT OF CONTRACTS § 90 (1932).

91. RESTATEMENT (SECOND) OF CONTRACTS § 90 cmt. b (1981).

92. Farber & Matheson, *supra* note 75, at 910.

93. Farber and Matheson give an extended analysis of *Vastoler v. American Can Co.*, 700 F.2d 916 (3d Cir. 1983), as an example of this proposition. Farber & Matheson, *supra* note 75, at 910–12.

and Matheson used their empirical findings to make sweeping revisions in the theory of contract enforcement.

The authors note that there are two factors that characterize the cases that have expanded promissory obligation. First, “the promisor’s primary motive for making the promise is typically to obtain an economic benefit.”<sup>94</sup> Second, “the enforced promises generally occur in the context of a relationship that is or is expected to be ongoing rather than in the context of a discrete transaction.”<sup>95</sup> Farber and Matheson contend that these relational contracts have become much more common and that they require the parties to have a high level of trust in one another. Commitments are therefore made to “promote economic activity and obtain economic benefits without any specific bargained-for exchange.”<sup>96</sup> This leads them to draft a hypothetical Section 71 on “Enforceability of Promises” for the *Restatement (Third) of Contracts*: “A promise is enforceable when made in furtherance of an economic activity.”<sup>97</sup> This is a dramatic revision of traditional contract theory to take account of their empirical findings: “The proposed rule is a major departure from traditional contract law in that it requires neither satisfaction of traditional notions of consideration nor the specific showing of detriment associated with promissory estoppel.”<sup>98</sup> Farber and Matheson argue that this theory fits not only what courts are doing but that it normatively fits the increasing need in a complex and impersonal society for mutual trust.<sup>99</sup>

#### b. The Irreparable Injury Rule

For my second example, I consider another important conclusion in the empirical consideration of contract doctrines—Professor Douglas Laycock’s study of the irreparable injury rule.<sup>100</sup> That rule holds that a court will apply equitable remedies only in the extremely rare instances in which legal remedies—the payment of compensatory money damages—are inadequate.<sup>101</sup> On the theory that the vast majority of plaintiffs who have been wronged can be made whole by the payment of a sum of money, legal remedies are the common remedy. For example, in contract law there is a vast literature to demonstrate the proposition that money damages are adequate in almost every incidence of breach of contract and that specific performance should be reserved for relatively rare incidents of breach.<sup>102</sup>

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94. Farber & Matheson, *supra* note 75, at 925.

95. *Id.*

96. *Id.* at 929.

97. *Id.* at 930–31.

98. *Id.* at 929.

99. *Id.* at 937–38.

100. DOUGLAS LAYCOCK, *THE DEATH OF THE IRREPARABLE INJURY RULE* (1991).

101. *Id.* at 4; *see also* E. ALLAN FARNSWORTH, *CONTRACTS* 854 (1990).

102. There has been theoretical argument for the reverse proposition—namely that specific performance should be the default contract remedy. *See* Alan Schwartz, *The Case for Specific Perform-*

Professor Laycock surveyed over 1400 cases in order to discover whether courts in fact followed this theory of remedies. His finding was that they do not, but with an interesting reservation: courts award equitable remedies only in instances in which damages would be inadequate, but they almost always find that damages would be inadequate.<sup>103</sup>

A remarkable fact about Laycock's findings—and, incidentally, about those of Henderson, Farber, and Matheson—is that they have not been seriously challenged by any scholars and yet they appear not to have had much effect on legal scholarship or on the practice of law. At a minimum, one would have thought that the finding that the irreparable injury rule is dead would work its way into the casebooks or practitioners' manuals in such a way as to lead to an instruction to law students that they could routinely expect to receive equitable relief and did not have to pray for relief in the form of money damages. But no such impact on the teaching of law appears to have occurred.

### c. The Coase Theorem and Social Norms

As my third example of the value of empirical work in the study of law, I cite Professor Robert Ellickson's remarkable study of the Coase Theorem in action.<sup>104</sup> That study represents, just as forcefully as do the previous examples, the great value of subjecting legal propositions to empirical verification. But here the added bonus is that the empirical work concerns the central article in the study of law and economics.

Law and economics can be said, without exaggeration, to have stemmed largely from one article, Ronald H. Coase's *The Problem of Social Cost*.<sup>105</sup> The article may contain the most significant theoretical claim made in the modern law school curriculum—the Coase Theorem.<sup>106</sup> That Theorem holds that when transaction costs are zero or very low, bargaining can lead to the efficient use of resources, regardless of the law. The implications of the Theorem for legal analysis were profound, in two senses. First, the Theorem suggests that there may be a set of circumstances under which, if efficiency is one's legal goal, the achieving of

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ance, 89 YALE L.J. 271, 271 (1980); Thomas S. Ulen, *The Efficiency of Specific Performance: Toward a Unified Theory of Contract Remedies*, 83 MICH. L. REV. 341, 365–66 (1984).

103. “Courts have escaped the irreparable injury rule by defining adequacy in such a way that damages are never an adequate substitute for plaintiff's loss.” LAYCOCK, *supra* note 100, at 4.

104. Robert C. Ellickson, *Of Coase and Cattle: Dispute Resolution Among Neighbors in Shasta County*, 38 STAN. L. REV. 623 (1986) [hereinafter Ellickson, *Dispute Resolution*]; see also ROBERT C. ELLICKSON, *ORDER WITHOUT LAW: HOW NEIGHBORS SETTLE DISPUTES* (1991) [hereinafter ELLICKSON, *ORDER WITHOUT LAW*].

105. Ronald H. Coase, *The Problem of Social Cost*, 3 J.L. & ECON. 1 (1960).

106. I say that the article “may” contain the Theorem because the article contains no explicit statement of what has come to be known as the Coase Theorem. Like any sacred text, there is a great deal of interpretive activity over *The Problem of Social Cost*. See, e.g., *The Coase Theorem*, in 1 THE NEW PALGRAVE DICTIONARY OF ECONOMICS AND THE LAW 270–82 (Peter Newman ed., 1998). For a fuller statement of the theorem and a summary of the subsequent scholarly literature, see ROBERT D. COOTER & THOMAS S. ULEN, *LAW AND ECONOMICS* 83–93 (3d ed. 1999).



efficiency occurs without any help from law (and, by implication, might be impeded or made more costly to achieve by an inefficient law). Second, when transaction costs are high, achieving an efficient allocation of resources may depend crucially on law.<sup>107</sup>

For many years, law-and-economics scholars examined and criticized the Coase Theorem largely through deductive argumentation. Then, in the 1980s, there were two important attempts to subject the Coase Theorem to confirmation through empirical and experimental techniques. In the former, Professor Robert Ellickson examined the practices of cattle ranchers and farmers in Shasta County, California, for harms done by cattle.<sup>108</sup> Ellickson's findings, on which I shall focus in the remainder of this section, not only failed to confirm the predictions of the Coase Theorem but, as we shall see, opened up an entirely new and important area of scholarship. In the latter, then Professor, now President, Elizabeth Hoffman and then Professor, now Dean, Matthew Spitzer conducted a series of experiments designed to see the extent to which bargaining occurred in settings of zero or very low transaction costs.<sup>109</sup> By and large, their experiments confirmed the predictions of the Coase Theorem.

Ellickson decided to investigate the practices of cattle ranchers and farmers in Shasta County, California, to see if the law or private bargaining was the method by which those parties resolved disputes about harms done by cattle to farmers. The prototypical harm occurred when, during the summer months, cattle ranchers herded their cattle up into the foothills of the Sierra Nevada mountains in order to let those cattle forage for food in common areas and those untended cattle wandered onto farmers' or other private, nonranch, noncommon property. The Coase Theorem had famously used a similar hypothetical example of cattle straying onto neighboring grain farms and doing damage to illustrate that when transaction costs were zero, the rancher and the farmer would bargain to a resolution of their conflicting property uses without any regard to whether the farmer had the right to be free from invasion and damage or whether the rancher had no legal obligation to supervise his cattle. Shasta County was a particularly apt place to test the Coase Theorem because the prevailing law on liability for damage done by unsupervised cattle varied across the county.<sup>110</sup>

In a sense, Ellickson found evidence confirming the Coase Theorem in that the practices of ranchers and others with regard to straying cattle were the same throughout the county, regardless of the legal obliga-

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107. See COOTER & ULEN, *supra* note 106, at 101–02.

108. Ellickson, *Dispute Resolution*, *supra* note 104.

109. Elizabeth Hoffman & Matthew L. Spitzer, *Experimental Law and Economics: An Introduction*, 85 COLUM. L. REV. 991 (1985). Hoffman is now President of the University of Colorado, and Spitzer is Dean of the University of Southern California School of Law.

110. See generally Ellickson, *Dispute Resolution*, *supra* note 104, at 662–67.

tions.<sup>111</sup> That is, private ordering by potential disputants trumped the law.

But even more surprising to Ellickson was the fact that the potential disputants did not seem to know what the law regarding legal obligations for stray cattle were; indeed, attorneys in private practice in Shasta County did not know or were mistaken about the law. Apparently, ranchers and others in the county were conforming their behavior not to the law but to a widely respected social norm of “neighborliness.”<sup>112</sup> Good neighbors, the norm directed, did not sue one another; they helped each other. So, if a farmer found stray cattle on his property, he did not call his attorney and commence an action for damages. Instead, the farmer typically called the rancher-owner, informed him that he had his cattle and would feed and shelter them until the rancher could come to pick them up. If the straying cattle had caused damage, the person who had suffered the damage typically took care of it himself and never asked for indemnification. In some instances, weeks passed till the rancher picked up his cattle. And yet almost no one who sheltered cattle, whether for a long or a short time, asked for compensation from the owner.

If there was litigation about damage done by or compensation for feed and shelter provided to stray cattle, one of the parties to the litigation was almost invariably a newcomer to the county or a long-time resident recognized by his neighbors to be churlish, a bad neighbor. Consultation with an attorney regarding these matters and litigation were thought to be a sign that the social norm of “neighborliness” had broken down. The implication was that in the normal course of rancher-farmer affairs, neighbors did not use law to order their affairs and to resolve disputes. They used shared social norms. And they resorted to law only when those norms had broken down.<sup>113</sup>

What is remarkable about this particular piece of empirical work is that it revealed so much about the legal phenomena involved, much of it not the subject of the initial inquiry. Indeed, Ellickson’s article has become part of the law-and-economics canon not for what it set out to find—namely, whether the Coase Theorem applied in a particular setting in California—but for its collateral finding—namely, that people may seek to conform their behavior to social norms rather than to the law. The literature on law and social norms that Ellickson’s study spawned is

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111. *Id.* at 672–73. An additional surprise to most readers of Ellickson’s famous study was that this private ordering occurred despite the fact that the transaction costs of bargaining between ranchers and others were certainly not zero and might have been substantial.

112. *Id.*

113. Among the many interesting lines of inquiry opened by Ellickson’s article were inquiries into the practices of particular organizations to see the extent to which they ordered their affairs by appeal to norms or to law. See, e.g., Lisa Bernstein, *Opting Out of the Legal System: Extralegal Contractual Relations in the Diamond Industry*, 21 J. LEGAL STUD. 115 (1992). An important question raised by Bernstein and others—such as Jody Kraus—is whether law is a complement to or a substitute for social norms.

one of the most significant strands of recent legal scholarship.<sup>114</sup> I can think of no stronger testimony to the value of empirical work.

### C. *The Future of Legal Science*

I have made numerous claims in the sections above, and it behooves me to consolidate them here in order to speculate on the future of legal science. I have argued, first, that the scientific method followed among a community of scholars investigating the same subject matter consists of a widely accepted core theory relevant to explaining and predicting the subject matter and widely accepted procedures for investigating phenomena and for accepting and rejecting contentions made about the subject matter. I then showed, in my brief survey of the philosophy of science, that I find the Bayesian account of acceptance or confirmation to be widely held and congenial to a description of what legal scholars do or might do. Then I argued that what sets the study of law apart from sciences is a lack of a core theory. But there has been, since the 1930s at least, an interest among U.S. legal scholars in empirical work, much of it descriptive rather than analytical.

I believe that I discern in today's legal scholarship a move toward a more scientific method like that prevailing in the other social, natural, physical, and biological sciences.<sup>115</sup> If I am correct, then one should observe the beginnings of an elaboration of a core theory of legal subject matter and increasing attempts to engage in analytical empirical work designed to establish the truth of hypotheses about legal topics derived from that core theory. I think that both these trends are, in fact, distinctly observable in the study of law.

Let me begin with an argument about theory. There seems to be much more of it in legal scholarship than was the case twenty years ago. Probably the most obvious culprit in this regard is law and economics. Whatever else they have done, scholars working from that perspective have assiduously explored the legal system to see the extent to which law is efficient. They have articulated a comprehensive theory—both positive and normative—that asserts that, for example, the rules and standards of property law should (and largely do) foster the efficient use of society's scarce resources, that contract law should (and largely does) efficiently lower the transaction costs of forming and completing consensual agreements, that tort law should (and largely does) efficiently minimize the social costs of accidents, and that potential litigants should (and

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114. ERIC A. POSNER, *LAW AND SOCIAL NORMS* (2000); Richard H. McAdams, *Signaling Discount Rates: Law, Norms and Economic Methodology*, 110 *YALE L.J.* 625 (2001) (reviewing ERIC A. POSNER, *LAW AND SOCIAL NORMS*).

115. Others have discerned a similar trend in legal scholarship. See, e.g., Posner, *supra* note 1, at 766–69 (arguing that law's independence from other, contiguous disciplines was at an end and that modern legal scholarship largely consists of law's adopting the tools of those contiguous disciplines to examine legal issues).

largely do) efficiently choose between settlement and litigation.<sup>116</sup> These same tools—microeconomic analysis of legal decision making on the assumption that everyone involved is a rational actor—has also been extended into the areas of public law, such as corporate law,<sup>117</sup> criminal law,<sup>118</sup> bankruptcy,<sup>119</sup> and family law.<sup>120</sup>

I am certainly not saying that the use of microeconomic theory necessarily completes the business of making the study of law into a science. Rather, I am simply pointing to the great attractions and successes of this particular method of bringing a comprehensive theory to the study of law as evidence of the increasing core theorization of legal scholarship. One could very easily point to other comprehensive theories that have begun to develop of late within the legal academy, even if they do not flourish. Critical legal studies, for example, has the same comprehensive theoretical goal as does law and economics.<sup>121</sup> Similarly, contractarianism, which takes the view that there is much to be gained in understanding law from asking about the ability of private ordering to deal with many social interactions, may be said to be seeking to propound a core theory of law with empirical consequences.<sup>122</sup> One might also argue that law and society is coming at the same general goal of providing a theory of law by using empirical work to examine the real consequences of law in action.<sup>123</sup>

Another example of the core theorization of law that may illustrate science-like developments in legal scholarship comes from corporate law. There have long been only a few models of how to organize business associations. For example, the standard U.S. model—in which the corporation is seen as a nexus of contracts and elements of corporate law seek to maximize shareholder wealth—has long been thought to be particularly well suited to American societal goals and legal norms and not so clearly appropriate to other societies. Other theorists of the corporate form generally focus on two alternatives—the Japanese and the German mod-

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116. See COOTER & ULEN, *supra* note 106, at 99–102, 228–29, 370–71, 484–87.

117. See, e.g., FRANK H. EASTERBROOK & DANIEL R. FISCHEL, *THE ECONOMIC STRUCTURE OF CORPORATE LAW* (1991).

118. See COOTER & ULEN, *supra* note 106, at 506–32; Gary S. Becker, *Crime and Punishment: An Economic Analysis*, 78 J. POL. ECON. 526 (1967).

119. See THOMAS JACKSON, *THE LOGIC AND LIMITS OF BANKRUPTCY LAW* (1986).

120. See MARGARET F. BRINIG, *FROM CONTRACT TO COVENANT* (2000).

121. See, e.g., JACK BALKIN, *CULTURAL SOFTWARE* (1987); MARK KELMAN, *A GUIDE TO CRITICAL LEGAL STUDIES* (1987). Some within the CLS community would dispute my characterization of their subdiscipline as being interested in theorization of the study of law. Instead, they might evidence a deep skepticism about theory per se. Some modern philosophers share that skepticism of theory, preferring pragmatic, limited, and less general considerations to comprehensive theory. See, e.g., Rorty, *supra* note 10, at B7.

122. See, e.g., RANDY E. BARNET, *THE STRUCTURE OF LIBERTY* (1998).

123. One of the most famous articles in this vein is Stewart Macaulay, *Non-Contractual Relations in Business: A Preliminary Study*, 28 AM. SOC. REV. 55 (1963). Without offering a well-developed theory of contracts, Macaulay examined how business associates who were in a contractual relationship actually dealt with one another. They famously found that business people tended to ignore the black letter of the contract and to rely instead on informal methods of resolving any problems that arose in their relationship. *Id.* at 61.

els—and demonstrate that those forms are particularly well suited to those societies.<sup>124</sup>

However, it has been recently asserted that there has been a convergence toward the U.S. model in a variety of different legal systems.<sup>125</sup> The reasons for this convergence are many and powerful—e.g., global, transnational competition—and beyond my current focus. Still, for my purposes, the remarkable aspect of this convergence, if it is in fact occurring, is that it might be said to be one of the first examples of an agreed-upon theoretical development across national boundaries in law. That is, arguably the older argument—namely, that the particular characteristics of some aspect of a national legal system, such as the corporate form of business organization, were the product of path-dependent processes and cultural imperatives that were unique to each national legal system—may have given way to a culturally nonspecific theoretical argument.<sup>126</sup> In this instance, the powerful theoretical arguments of corporate law-and-economics scholars about the superior efficiency of the U.S. shareholder wealth maximization model may have had the practical consequence of causing different legal systems to adopt the U.S. model, regardless of the other differences in their national legal systems.<sup>127</sup>

I am suggesting that the extent to which law is becoming or will become “legal science” depends on its becoming more theoretically sophisticated. Come the millennium of legal science, that theoretical core will be one that is widely shared among legal scholars of all countries. I do not assert that there will be a single theory accepted by all legal scholars. There may be, rather, several competing theories, and scholars may be

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124. See Mark J. Roe, *Some Differences in Company Structure in Germany, Japan, and the United States*, 102 YALE L.J. 1927 (1993); see also Mark J. Roe, *Can Culture Constrain the Economic Model of Corporate Law?*, 69 U. CHI. L. REV. 1251 (2002).

125. See Henry Hansmann & Reinier Kraakman, *The End of History for Corporate Law*, 89 GEO. L.J. 439 (2001); see also RONALD J. GILSON, GLOBALIZING CORPORATE GOVERNANCE: CONVERGENCE OF FORM OR FUNCTION? (Colum. L. Sch., Ctr. for L. & Econ. Stud., Working Paper No. 174, 2000); Bernard Black & Reinier Kraakman, *A Self-Enforcing Model of Corporate Law*, 109 HARV. L. REV. 1911 (1996); John C. Coffee, Jr., *The Future as History: The Prospects for Global Convergence in Corporate Governance and Its Implications*, 93 NW. U. L. REV. 641 (1999). For the view that there is an unavoidable limit to convergence, see Lucian Ayre Bebchuk & Mark J. Roe, *A Theory of Path Dependence in Corporate Ownership and Governance*, 52 STAN. L. REV. 127 (1999). A recent attempt to take account of cultural differences in a unified theory of corporate governance is Amir N. Licht, *The Mother of All Path Dependencies: Toward a Cross-Cultural Theory of Corporate Governance Systems*, 26 DEL. J. CORP. L. 147 (2001).

126. Black and Kraakman are particularly sensitive to this distinction. See Black & Kraakman, *supra* note 125. Nonetheless, they argue—much on the model of a core theory of microeconomics applied to different economies developed in Part IV.A above—that a general model of business associations can help to explain why there might be a different corporate law in different countries. See *id.* For example, their article is an argument for a self-enforcing corporate law system in Russia as opposed to a publicly enforced system. Among the reasons for preferring a self-enforcing system in Russia is that there is a weak tradition of judicial enforcement and there are contextual reasons for not requiring the public disclosure of corporate information that seems to form such an important element of the U.S. system of corporate law. *Id.* at 1296–98.

127. I hope that it is obvious that I am *not* arguing in favor of an economic analysis as *the* universal theory of any area of law, nor arguing that the U.S. model of corporate organization is necessarily the most efficient method of organizing large business firms.

engaged in zealous attempts to persuade the scholarly profession that their particular theory is superior to the alternatives. That process is, of course, precisely the one that characterizes much of the natural, physical, and biological sciences. And, as I argued in Part III.D above, the Bayesian confirmation process is likely to be the one that characterizes this battle among competing theories of legal phenomena.

There is another aspect of this theorization of law that I feel bound to point out—the fact that the increasing theorization of law will almost certainly not be jurisprudential. By “jurisprudential” legal theories, I mean those that are philosophical and that have the characteristic that conflicts among competing jurisprudential theories will be resolvable, if at all, only by hypothetical-deductive argumentation. My sense is that the theorization of law that is occurring and is likely to continue to spread, is positive and normative theorization about the effects of legal rules and standards on behavior and other real phenomena—about, that is, the consequences of law.

If I am correct in asserting that theorization of a particular kind is becoming increasingly common among legal scholars, then I further believe that there is another important consequence for the future of legal science—namely, the increasing use of empirical and experimental methods to examine legal phenomena. Generally speaking, this would be a perfectly pedestrian consequence of the theorization of legal scholarship: every science has a core theoretical and empirical component, and if law becomes more theoretical in the sense that I allege, then it must ineluctably also become more empirical.

My prediction that increasing theorization will bring increasing empiricism is, in fact, borne out in observable patterns of legal scholarship. We know that there has been an increasing amount of theoretical work in law, at least in that realm informed by economics.<sup>128</sup> And if I am correct in my belief that the study of law is becoming more science-like in the degree of core theorization, then there should be a lagged increase in the amount of empirical work being published by legal scholars. And, indeed, there is evidence that this phenomenon of increasing empirical work among legal scholars is already afoot. Professor Robert Ellickson recently sought to do a statistical study of recent trends in legal scholarship and found the following:<sup>129</sup>

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128. See William Landes & Richard A. Posner, *The Influence of Economics on Law: A Quantitative Study*, 36 J.L. & ECON. 385, 387 (1993).

129. Robert C. Ellickson, *Trends in Legal Scholarship: A Statistical Study*, 29 J. LEGAL STUD. 517, 528–29 tbl.4 (2000). Ellickson did a search on Westlaw of all documents containing certain key phrases meant to indicate the style of scholarship that the piece represented.

TABLE 2  
INDEX OF FREQUENCY

	Index of frequency of references, 1994–96 = 100				
	1982–84	1985–87	1988–90	1991–93	1994–96
Empirical:					
<i>empiric!</i>	95	96	86	92	100
<i>quantitat!</i>	86	100	92	97	100
<i>statistic!</i>	55	75	72	83	100
<i>significan!</i>					

With respect to empirical and sociological approaches to the study of law, Ellickson found that from 1994 to 1996 there were six times more references to *empiric!* than to *post-modern!*, which he takes to be a proxy for skepticism about empirical work. However, he found that his indices of empirical and quantitative work were constant from 1982 to 1996, while the indices for the terms *statistic!* and *significan!* doubled over the same time period. He concludes, “The data in Table 4 thus hint that law professors and students have become more inclined to produce (although not to consume) quantitative analyses.”<sup>130</sup>

In the section above on legal theory in the study of law, I speculated briefly on why there is no transnational core theory in law. I might also speculate on why there has been so little empirical work in law.<sup>131</sup> An obvious reason is that there has been so little theoretical work that could excite a demand for empirical work. Such theory as there has been in law has largely been theory that could be attacked and rebutted completely by deductive argumentation. Only since the time of the legal realists and their assertions about the predictability and the consequential nature of laws has there been much call to step outside the hypothetical-

130. *Id.* at 528.

131. There has been a great deal of excellent recent scholarship on the legal realists. Of that writing, I have benefited immensely from John Henry Schlegel’s *American Legal Realism and Empirical Social Science* and am grateful to Michael Heise for bringing it to my attention. Schlegel’s work is a careful examination of the remarkable group of scholars, largely at the Columbia Law School and the Yale Law School after World War I, who sought to create an empirical legal science to stand in juxtaposition to Langdellian legal science, an area which had sought to move from a natural law-based jurisprudence that sought to find law through formal arguments to an empirical science of mining judicial opinions to discover law. To appreciate the connection between my inquiry and that of Professor Schlegel, consider his question at the beginning of his marvelous work: “Why did law not become a scientific study, in the twentieth-century sense of science as an empirical inquiry into a world ‘out there,’ as did all the other disciplines in American academic life that formed in the late nineteenth and early twentieth centuries?” JOHN HENRY SCHLEGEL, *AMERICAN LEGAL REALISM AND EMPIRICAL SOCIAL SCIENCE* 1 (1995). Also on the legal realists, see generally MORTON J. HOROWITZ, *THE TRANSFORMATION OF AMERICAN LAW, 1870–1960: THE CRISIS OF LEGAL ORTHODOXY* (1992); LAURA KALMAN, *LEGAL REALISM AT YALE, 1927–1960* (1986).

On the larger question of why, since the time of the legal realists, there has been relatively little empirical research in law, see Peter H. Schuck, *Why Don’t Law Professors Do More Empirical Research?*, 39 J. LEGAL EDUC. 323 (1989); and Symposium, *Social Science in Legal Education*, 35 J. LEGAL EDUC. 465 (1985).

deductive mode of resolving controversy. Law and society, which has, unfortunately, commanded the allegiance and interest of only a limited percentage of legal scholars,<sup>132</sup> has long focused on empirical studies of legal topics and has generated seminal findings. But only with the rise of more comprehensive theories—such as those of law and economics, contractarianism, and critical legal studies—has empirical work in law been closely connected, as in the sciences, with the evaluation of theoretical results.<sup>133</sup> Surely an additional reason that legal scholars have not done much empirical work is that they are not adept in it. Empirical social inquiry is, after all, a complex special field of scholarly inquiry, and one that lawyers have had neither motive nor opportunity to explore until recently. The number of law school courses devoted to training students to understand and perform quantitative empirical or experimental work must be very, very small, but it is not zero. And those, like I, who welcome and even encourage empirical and experimental work in the study of law have somehow managed to get competent training in those techniques. Just as I argued above that the theorization of law is at an early stage, so it must be true that empiricism and experimentation as standard techniques in the study of law are also at a very early stage of acceptance and development.

## V. CONCLUSION

I began this investigation into the state of legal scholarship by posing a rhetorical question, “Will there be a Nobel Prize in Legal Science?” I am now in a position to summarize the argument and to conjecture an answer.

We have seen that a science or a scholarly discipline that follows the scientific method has the following general characteristics: (i) an agreed-upon subject matter, (ii) considered from a shared, core theoretical perspective with (iii) widely accepted standards and procedures for evaluating (confirming or rejecting) theoretical propositions or descriptions about the field’s subject matter. Scientists in a given field seem to share a paradigm or normal understanding of their field. An important characteristic of the sciences for which Nobel Prizes are awarded is that the scientists in those fields understand and appreciate one another’s work, regardless of the country in which they are conducting their scientific inquiries.

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132. See, e.g., DUXBURY, *supra* note 71, at 454.

133. Recently, Dr. William Meadow and Professor Cass R. Sunstein have argued in favor of using statistical studies in place of expert witnesses to resolve certain factual questions in legal disputes. See William Meadow & Cass R. Sunstein, *Statistics, Not Experts*, 51 DUKE L.J. 629, 631 (2001) (the “legal system should rely, whenever it can and far more than it now does, on *statistical data* about doctors’ performance rather than on the opinions of experts about doctors’ performance . . . our larger claim is that by using statistical data, the legal system will reach far more accurate results.”).



At first blush, I argued that the academic study of law is not a science and does not follow the scientific method—especially in the sense of having an agreed-upon core theory that spans national boundaries and makes studies of legal matters in one country of interest to scholars in another jurisdiction. The academic study of law seems, rather, to be highly contextualized and largely to be of very localized interest. The central reason for this, I suggested, was that there is not yet a core theory of law shared by all academic lawyers regardless of the country in which they find themselves. I argued by analogy to medicine and economics that there are no particularly persuasive reasons for the academic lawyer's lack of a shared core theory and their focus on highly particularized subjects of interest largely within the jurisdiction. In the end, I was left with a puzzle as to why the scholarly study of law has only recently begun to develop the sort of shared core theory that characterizes natural and social sciences.

Despite the lack of a shared core theory among academic lawyers, I did find that there has long been an interest in empirical work in legal scholarship. Indeed, the first doctrinalists, such as Christopher Columbus Langdell, believed that their method of reading judicial opinions so as to discern the doctrinal core within a field of law and a particular jurisdiction was an empirical inquiry along scientific lines. And the legal realists, by focusing in part on the social consequences of law, and by alleging that the law meant an informed prediction of how a court would decide a particular dispute, raised the idea of actually looking at the consequences of any legal rule or standard, or of systematically investigating the holdings of a particular court to more accurately predict how that court would resolve any particular dispute brought to them for resolution. But, somewhat curiously, the legal realists themselves never engaged in much empirical work, even though their core beliefs seemed to push them in that scholarly direction.

I believe that there is a clear change afoot in legal scholarship that is making the study of law more science-like. To be brief, whereas twenty or so years ago legal scholars directed their writing principally toward the practicing bar and judges, today legal scholars often direct their writings principally at other legal scholars who are working in their area.<sup>134</sup> This

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134. Judge Posner argues that one explanation for this increasing turn away from practitioners and towards academics (and the related increase in the number of legal theories) is the great increase in the number of law professors, an increase that arose because of a general increase in the demand for legal services and the consequent increase in the demand for legal education. *See* RICHARD A. POSNER, *FRONTIERS OF LEGAL THEORY* 3–4 (2001). I do not necessarily agree. First, I want to distinguish, as I have tried to do in the paper, between the blossoming of jurisprudential theories (upon which, I think, Judge Posner focuses in his introduction) and legal theory that inherently suggests empirical or experimental work for confirmation. I want to stress the rise of the latter kind of theory. Second, even if there has been a dramatic increase in the demand for legal services (the number of lawyers doubled between the mid-1970s and the mid-1990s in the United States) and therefore, an increase in the number of law professors, I do not think that there is any necessary reason that this

change brings legal scholarship into alignment with other scholars in the academy. University chemists, for example, pursue research and write articles whose principal audience is other university chemists. If it happens that industrial and other commercial chemists find the research of interest, that is a plus, but it is not the effect at which university or research chemists aim. Similarly, a professor of English at a university writes her scholarship principally for other scholars in her area of expertise in English, not for the general public. Indeed, a common method of criticizing a scholar's work is to suggest that it was intended not for other specialists but was for a "popular" audience. So, if an English professor writes a murder mystery for popular consumption, that work will almost certainly not count towards his scholarly reputation. Quite to the contrary, that authorship of a murder mystery may well denigrate that professor's reputation within the academy.

Precisely the same thing may be occurring in legal scholarship. Twenty years ago, prestige and academic acclaim accrued to those legal scholars whose work had a discernible impact on the bench and bar. This was true even if the effect was indirect, as through the authoring of widely used casebooks and treatises. Within the past twenty years or so, however, the sources of academic prestige in the legal academy have changed. The prestige that used to accrue from prominence among practitioners is no longer so valuable and, as a result, is no longer so widely sought. Rather, the ascendant source of prestige among legal scholars today is the high esteem of fellow academics. Legal scholars seek that esteem in different ways from those that prevailed when the esteem of practitioners was a more salient goal among legal academics.

Another indication of the changing nature of legal scholarship is the fact that joint degree holders—those with a J.D. and another degree, usually a Ph.D. in a cognate subject—are among the most highly sought entrants into the legal academy. Yet another indicator of the changing nature of things is the fact that it is no longer mandatory, as it was, say, twenty years ago, that a new assistant professor in law have some practice experience before coming to the academy.

If I am correct that there is a transition underway in legal scholarship, then we may confidently expect there to be some dislocations and protests. Academic interests become no less entrenched in the academy than those special interests that strive so hard for influence in public life. Indeed, there is a quip that holds that "science advances funeral by fu-

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increase should necessitate a change in the style of legal scholarship. Why would the increase not simply lead to more of the same doctrinal scholarship that was the hallmark of pre-1980 legal scholarship?

My sense—for which, I must add, in the spirit of this article, I have no empirical evidence, although I am in the course of gathering it—is that the increasing theorization of and academic emphasis in legal scholarship is due to internal factors having to do with the history of legal scholarship and legal education's being situated within great research universities, not to external or market forces in the legal services market.

neral.”<sup>135</sup> There is no reason to believe that law’s transition from an orderly study, but haphazard science, to a full-blown science, with a shared core theory that suggests empirical tests to confirm or refute hypotheses derived from that theory, will be any less contentious than the scientific transitions from old to new paradigm that Thomas Kuhn so notably chronicled.<sup>136</sup> But the benefits that might follow from a successful crossing to this new method of legal scholarship are many. To take one example, the possibility that legal academics around the world might share a core theory of law and might, therefore, write work that would have a worldwide audience and broaden the horizons of those concerned with finding new understandings and methods of improving the human condition is truly wonderful.

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135. My thanks to Professor Paul Rubin of Emory University for this observation.

136. Edward O. Wilson has argued persuasively in *Consilience* that progress in a large number of disciplines not thought to be scientific will occur only when those learned in those disciplines adopt the scientific method of inquiry into their subject matter. See WILSON, *supra* note 24, at 269. A plea for doing this in an area of scholarship thought to be nonscientific—human history—has recently been made by Professor Jared Diamond. JARED DIAMOND, GUNS, GERMS, AND STEEL: THE FATES OF HUMAN SOCIETIES 420–25 (1997).

## APPENDIX: THE NOBEL PRIZES

Alfred Nobel, a Norwegian, invented dynamite in 1866 and went on to make a fortune producing and selling that and other chemicals. Apparently Nobel felt some regret at having amassed so much wealth by purveying elements of destruction and sought to correct for this perceived shortcoming towards the end of his life.<sup>137</sup> On November 27, 1895, he signed his last will and testament at the Swedish-Norwegian Club in Paris, in which he established the Nobel Foundation and made provision for the annual awarding of prizes to recognize worldwide distinction in international affairs and selected scholarly disciplines. There were five areas mentioned in Nobel's will—Peace, Literature, Physiology or Medicine, Physics, and Chemistry. One prize—that in economics, about which there will be more discussion shortly—has been added since the first Prizes were awarded in 1901. The winners of the Prizes have been announced every Fall for 100 years. Most thoughtful observers consider these prizes to be the greatest achievement to which a person in those fields can aspire.<sup>138</sup>

Nobel's will provides that several different bodies are to serve as the nomination and award committees for the prizes.<sup>139</sup> For example, committees drawn from the Royal Swedish Academy of Sciences are to award the Physics, Chemistry, and Literature prizes, and the Karolinska Institutet in Stockholm is to award the Physiology or Medicine prize. The Peace Prize is to be awarded by a committee of five selected from the Norwegian Parliament to the person who, during the preceding year, "shall have conferred the greatest benefit on mankind."<sup>140</sup> The Committee is to give no consideration to race or nationality so that "the most worthy shall receive it, whether he be Scandinavian or not."<sup>141</sup>

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137. See BURTON FELDMAN, *THE NOBEL PRIZE: A HISTORY OF GENIUS, CONTROVERSY, AND PRESTIGE* 36 (2000).

138. There are, of course, many other prestigious prizes and medals awarded throughout the world. For example, the Wolfenden Prize goes to notable mathematicians—e.g., Andrew Wiles won the prize for his proof of Fermat's Last Theorem. See SIMON SINGH, *FERMAT'S LAST THEOREM* (1997). The German government awards the Leibnitz Prize in a variety of fields, including law; and the American Economics Association has, since 1947, awarded the John Bates Clark Medal biennially to the most outstanding economist under the age of forty. The Templeton Prize for Progress in Religion offers a monetary award that its founder deliberately set so as to exceed the monetary award of the Nobel Prizes. See *Templeton Prize*, at <http://www.templetonprize.org> (last visited Mar. 4, 2002). For a description of these and other commendations, see *WORLD DICTIONARY OF AWARDS AND PRIZES* (1979). See also *AWARDS, HONORS & PRIZES* (19th ed., 2001) [hereinafter *AWARDS*].

139. *AWARDS*, *supra* note 138, at 608–09; see also *The Official Site of the Nobel Foundation*, at <http://www.nobel.se> (last modified Feb. 20, 2002).

140. See *The Official Site of the Nobel Foundation: The Nobel Prize*, at <http://www.nobel.se/peace/index.html> (last modified May 10, 2001).

141. *Id.* The same general criteria regarding the irrelevancy for the awards of the potential recipients' race and nationality and that the recipient be the "most worthy" in the world apply to all the other prizes as well.

In the 100 years of the Nobel Prizes, only one additional prize has been added.<sup>142</sup> In 1968, the Royal Bank of Sweden, as part of a celebration of its 300th birthday, instituted the “Central Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel.” As with the other prizes, winning the Nobel Prize in Economics, as it has come to be called, is the greatest achievement within the economics profession. The Royal Bank of Sweden appoints a five- to eight-person Economics Prize Selection Committee, which commissions studies of the works of the leading candidates.<sup>143</sup> The Prize Committee then presents its report to the Social Science Class of the Royal Swedish Academy of Sciences. The Royal Bank awarded the first Prize in Economics in 1969, to Ragnar Frisch and Jan Tinbergen.<sup>144</sup>

The nomination process has various steps.<sup>145</sup> Each year the selection committees send invitations for nominations to thousands of people—-independent experts, university professors, members of national and international academies, and more—soliciting nominations for each of the prizes for the coming year.<sup>146</sup> The nominations must reach the relevant committee by February 1 of the year in which the prize will be awarded. The committee solicits the help of outside experts in evaluating the nominations and prepares a report for the institution that will award the prize. The members of those institutions then vote on the Laureates. All of the Laureates, except the one for Peace, receive their prizes at the Concert Hall in Stockholm, Sweden, on December 10.<sup>147</sup> The Nobel Peace Laureate receives his or her prize on the same day at the City Hall in Oslo, Norway.

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142. It is worth noting (and perhaps puzzling) that even though there have been remarkable changes in other scholarly disciplines besides economics, there have been no other Nobel Prizes added. Consider, for example, that molecular biology, genomics, and physical chemistry did not exist 100 years ago when the prize categories began. Nonetheless, the Academy has found it possible to recognize those astonishing discoveries within the confines of the categories established in Nobel's will and without having to create new categories.

143. See SYLVIA NASAR, *A BEAUTIFUL MIND* 356–73 (1998) (discussing the nomination process for the economics Nobel).

144. A list of the winners of the Nobel Memorial Prize in Economic Science through Fall 2001, and the cited reason for the award may be found at <http://www.cooter-ulen.com/miscellaneous.htm#Nobelprizewinners>. It is worth noting that some commentators believe that there ought not to be a prize in economics. See, for example, FELDMAN, *supra* note 137, who argues that economics has not yet become as clearly scientific as the other scholarly disciplines for which Nobel Prizes are awarded. *Id.* at 329, 346–48, 352–55.

145. See *The Official Web Site of the Nobel Foundation: Nomination and Selection of the Nobel Laureates*, at <http://www.nobel.se/literature/nomination/index.html> (last modified Dec. 18, 2001).

146. The Special Regulations governing the award of the Literature prize explain who can submit nominations:

The right to nominate candidates for the prize competition shall be enjoyed by members of the Swedish Academy and of other academies, institutions and societies which are similar to it in constitution and purpose; by professors of literature and of linguistics at universities and university colleges; by previous Nobel Prize Laureates for Literature and by presidents of those societies of authors that are representative of the literary production in their respective countries.

See *id.*

147. The ceremony is held on this date to memorialize Alfred Nobel's death of a heart attack at his home in San Remo, Italy, on December 10, 1896.

The Prizes are typically awarded to only two people in each category, and since 1968 the statutes of the Nobel Foundation have restricted the awarding of any of the prizes to no more than three people. Since 1974, the Prize may be awarded to a deceased person only if that person was named as a recipient the previous October but dies before the awards ceremony on December 10. Winners receive a medal, a diploma, and a sum of money, which until relatively recently was free of U.S. income tax but is now taxable.<sup>148</sup> The total amount available through the Nobel Foundation for all prize recipients in 2001 was 10 million Swedish crowns.<sup>149</sup>

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148. 26 U.S.C. § 74 (2000).

149. This is approximately \$971,000, as of April 3, 2002.