

THE UNEXPECTED GUEST: LAW AND ECONOMICS, LAW AND  
OTHER COGNATE DISCIPLINES, AND THE FUTURE OF LEGAL  
SCHOLARSHIP

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INTRODUCTION

Legal scholarship has undergone a number of important changes in the course of the past 100 years. Legal formalism gave way to legal realism; realism then gave way to the legal process school; and then in the 1970s and 1980s a series of “law and” innovations became fashionable in the legal academy. On the one hand, these innovations testify to a flexible discipline looking for important means of advancing legal understanding. Other academic disciplines have undergone similar changes throughout the recent past, although, arguably, those changes have not been as frequent and as thoroughgoing as have those in the law.

On the other hand, the steady stream of innovations in legal scholarship might be taken to be indicative of an almost unseemly striving after fads and fancies. An individual who changed his hairstyle with the same relative frequency with which legal scholarship has changed its method of inquiring into the law would be thought to be flighty or ungrounded. Which of these two, seemingly mutually exclusive views is more accurate—that of a flexible discipline open to new ideas or that of an immature discipline not sure of where it is going or how it should be going there?

My contention in this Article is that the more accurate picture is of a relatively young academic discipline that is just now beginning to get its legs fully underneath it. Law in the academy has always been slightly at war with itself. While trying to train its students for the practice of law, its

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faculty has been trying to establish their credentials as scholars. The preparation of students for the practice of law is at its heart a different enterprise from that of most other graduate school departments in great research universities. Most graduate faculties (with the obvious exception of other professional schools, such as medicine, journalism, business, veterinary medicine, education, social work, public administration, and public policy) are engaged in preparing their graduate students to be scholars and teachers. And without putting too fine a point on the matter, training students to be professionals is a very different undertaking from that of training students to be scholars.

Until relatively recently there was no inherent tension arising from the law professor's roles as a successful trainer of professionals and a successful scholar. To be a successful scholar meant, in most instances, addressing practitioners in a meaningful way, by, for example, reforming the practice of law in a significant manner. So, the successful teacher and the successful scholar addressed the same audience—the bench and the bar.

For a host of reasons, however, this happy coincidence between the teaching and scholarship of the law professor began to come undone in the 1970s.<sup>1</sup> One manifestation of this change was that law professors sought to establish scholarly credentials that withstood comparison to those in other disciplines within the research university. This process is a long and sustained one, and, arguably, a process that has only just begun. With the onset of the desire to establish themselves as having the same claim on scholarly stature as others within the great research universities came also a change in the customs and mores of the internal legal academy. To be brief about that change, legal scholars appear to be adopting the same practices that characterize scholarship in other disciplines: writing for other scholars, sharing work in progress in seminars and workshops, organizing conferences around scholarly topics, reducing their teaching loads so as to give themselves more time to write scholarly articles and books, and so on.

Others have commented on these changes in the legal academy, but few have sought to place them into a coherent pattern that speculates about the recent past and likely future. That is the ambition of this Article. An important observation of this Article is that the central change that is occurring in the legal academy today is the adoption of the scientific method of inquiry that has long characterized other scholarly disciplines in the great research universities. I distinguish this view from one that might characterize the recent developments as being simply the latest in a long stream of

1. I very briefly consider the reasons for this change in the Conclusion.

legal innovations. Rather, I see this most recent change as both fundamentally different from those that have come before and, at the same time, a predictable development of those prior changes. The moving force in this change is not *all* of the “law and” developments of the last twenty years but one particularly—law and economics. I give centrality to law and economics because that field involves importation into the study of law a different *method of inquiry*.

The aspect of that method of inquiry that has excited the most commentary and criticism is its proclivity for theorizing from a sparse set of assumptions.<sup>2</sup> Here I want to draw attention to another aspect of the new method of inquiry—its commitment to empirical investigation. My central contention in this Article is that it is that second aspect of the scientific method of inquiry that is an “unexpected guest” in the legal academy, a guest who will prove, I believe, to be at least as transformative and provocative as the earlier commitment to theorizing.

The Article proceeds as follows. In Section I, I summarize some of the recent developments in legal scholarship that suggest that legal scholarship is becoming more scientific. In Section II, I give some examples of empirical legal scholarship as it has been practiced by those in law and economics and speculate on the future use of empirical research in legal scholarship. In Section III, I very briefly compare the empirical commitment of law and economics to other “law and” innovations of the last thirty or more years and show that among those innovations only law and economics has a commitment to empirical inquiry. In the Conclusion, I summarize the argument and make some predictions about the near-term future of legal scholarship.

#### I. THE TREND TOWARD A MORE SCIENTIFIC LEGAL SCHOLARSHIP

This Section covers three topics: (1) what it means to be a “science” or for scholarship to be “scientific”; (2) some arguments about whether the student of law is or can be scientific; and (3) the role that law and economics is playing in moving legal scholarship toward a scientific method of inquiry.<sup>3</sup> I argue that not only is the study of law well suited to the applica-

2. The heart of the criticisms have had to do with the alleged inaptness, to the analysis of legal issues, of the assumption that legal decision-makers are rationally self-interested. For a summary of this debate, see, e.g., Russell B. Korobkin & Thomas S. Ulen, *Law and Behavioral Science: Removing the Rationality Assumption from Law and Economics*, 88 CAL. L. REV. 1051 (2000). I comment on this debate in Section II below.

3. Some of the material in this Section draws on my article, *A Nobel Prize in Legal Science: Theory, Empirical Work, and the Scientific Method in the Study of Law*, 2002 U. ILL. L. REV. 875 (2002).

tion 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.

### A. Science

Historians generally attribute the great advances in the natural and physical sciences in the last 500 years to the discovery and assiduous application of the scientific method of inquiry.<sup>4</sup>

Those who are within a particular field of study that they believe to be investigating by means of scientific inquiry generally point to three aspects of their procedures that constitute “science.”<sup>5</sup> First, scientists in a particular field share a focus on a particular subject matter—generally, a class of phenomena in the real world. Thus, astronomers focus on heavenly bodies; chemists focus on chemical reactions; animal biologists concentrate on the behavior and other aspects of animals; economists study decision-making by individuals, firms, and other organizations and how broad aggregates in the national, regional, and international economy behave; physicians study the human body and mind; and so on.<sup>6</sup>

Second, those who are united by their study of a particular class of real phenomena generally share, at any given point of time, a theoretical core from which hypotheses about that subject matter may be derived by those learned in the theory.<sup>7</sup> Thomas Kuhn referred to this shared core theory as a “paradigm” and speculated on the general characteristics of the process by which a normal science, as he called it, altered its core paradigm.<sup>8</sup> There are, of course, times in the histories of the natural, physical,

4. See generally ALFRED W. CROSBY, *THE MEASURE OF REALITY: QUANTIFICATION AND WESTERN SOCIETY, 1250–1600* (1997); LISA JARDINE, *INGENIOUS PURSUITS: BUILDING THE SCIENTIFIC REVOLUTION* (2000).

5. See David Goodstein, *How Science Works*, in *REFERENCE MANUAL ON SCIENTIFIC EVIDENCE* 67 (Fed. Judicial Ctr., 2d ed. 2000).

6. Naturally, there are examples of pseudosciences that appear to fit this description—for instance, alchemy, astrology, and Freudian psychology. The lesson to be derived from a consideration of those pseudosciences is, I believe, that new sciences may appear from time to time and seem to be directed at understanding a particular class of real phenomena. But over time the practitioners of those pseudosciences fail to establish that their class of phenomena is real or that there is anything meaningful for serious students to say about that class of phenomena.

7. Goodstein, *supra* note 5, at 72.

8. *Id.* at 71–73; see generally THOMAS S. KUHN, *THE STRUCTURE OF SCIENTIFIC REVOLUTIONS* (3d ed. 1996). Kuhn sought to explain how major changes, which he characterized as “paradigm shifts,” occurred in a science—for example, how astronomers abandoned the Ptolemaic concept of our universe (with the Earth in the center and the Sun and other planets revolving around the Earth) with the Copernican conception (with the Sun in the center and the planets revolving around the Sun). The gist of his hypothesis is that in a normal science, observations pile up that are “anomalous” in the current paradigm. That is, these anomalous observations are difficult, but not impossible, to explain within the

and social sciences when there was no settled paradigm or when there were competing paradigms that seemed to be of equal power and predictability. I mention this last point both because it is historically the case that controversy exists—think of the current debate in physics between the so-called standard model and string theory<sup>9</sup>—and, for my purposes more importantly, to illustrate the fact that a study’s being “scientific” emphatically does not mean that there are definite answers to core questions. Certitude is not one of the hallmark characteristics of science.

Third, scientists of a particular class of phenomena share an agreed-upon technique for determining whether the hypotheses derived from the core theory about the class of phenomena studied are acceptable to those in that field.<sup>10</sup> These experimental and empirical techniques constitute part of the tools with which everyone learned in the field is familiar. Within physics, for example, there is agreement—at least among applied physicists—about which experiments would demonstrate the believability of hypotheses about physical phenomena. Similarly, among economists there is consensus about what constitutes a convincing demonstration of the believability of an assertion about economic phenomena. And physicians look to the highly developed specialty of biomedical statistics for help in assessing the worth of assertions made about the causes of and cures for human illnesses.

The remarkable thing about these three characteristics of science is how commonplace they are. Stripped to their essentials, the characteristics seem to describe the following continuous means of explaining real phenomena: theorizing about those real phenomena so as to derive hypotheses about them; testing to see whether the assertions made are correct; revising the theory in light of the tests; doing a new round of tests so as to assess the revised theory; and so on.

If that is a correct simplification of what it means to be “scientific” in one’s study, then let me draw attention to some implications. First, let me stress again that science is a *process of inquiry* and not a set of conclu-

existing paradigm. Eventually, some scientist proposes a new paradigm that explains these anomalies simply and also encompasses the nonanomalous predictions and descriptions of the older paradigm. For a particularly compelling illustration of this process, see generally THOMAS S. KUHN, *THE COPERNICAN REVOLUTION: PLANETARY ASTRONOMY IN THE DEVELOPMENT OF WESTERN THOUGHT* (1957).

9. For a marvelous explanation for the general reader of the differences between these theories and how the battle between them is being waged, see BRIAN GREENE, *THE ELEGANT UNIVERSE: SUPERSTRINGS, HIDDEN DIMENSIONS, AND THE QUEST FOR THE ULTIMATE THEORY* (1999).

10. See generally Goodstein, *supra* note 5, at 70–71; see also KUHN, *THE STRUCTURE OF SCIENTIFIC REVOLUTIONS*, *supra* note 8, at 10–11.

sions.<sup>11</sup> Thus, to say that an inquiry is scientific does not mean that it has reached clear and concise answers to questions. Rather, it is to say that those in the field have adopted a process by which to answer the questions of moment to them. I cannot stress this point enough because I believe that many of the critics of law and economics and of the changes that are occurring in the legal academy have missed this important difference between a process and a set of conclusions. Law and economics, as I shall shortly show, has brought a process of inquiry to the law, not a settled list of conclusions.

Another aspect of the scientific method to which I want to draw attention is that it is *collective* and *cumulative*. By “collective” I mean that even though there are many lonely hours involved in the scholarly, scientific life, ultimately scholarship in a scientific community is done together. We work on problems together. We help one another refine our scholarship—by rising before learned audiences to present our ideas and to subject them to inquiry, by reading and commenting on one another’s work, and by attending the sessions at which visitors and our colleagues present their scholarly work.

In addition, the scientific method applied by a community of scholars is *cumulative*. What one works on today in one’s scholarship depends in large part on what those who have gone before have worked on—both in the positive sense of dictating what answers they have given in their own inquiries and in the indirect sense of leaving some problems untouched for later generations of scholars to tackle. Very, very rarely does a scholar literally invent a new field of inquiry. In the vast number of instances, he or she is building on the work of others and is engaged with others in an extended dialogue regarding some issue of interest. Isaac Newton, who toiled alone much of his adult life,<sup>12</sup> famously captured this collective and cumulative aspect of the scientific method of inquiry when he wrote to an admiring friend, “If I have seen further it is by standing on the shoulders of Giants.”<sup>13</sup>

11. DAVID L. HULL, *SCIENCE AS A PROCESS: AN EVOLUTIONARY ACCOUNT OF THE SOCIAL AND CONCEPTUAL DEVELOPMENT OF SCIENCE* (1988).

12. See JAMES GLEICK, *ISAAC NEWTON* (2003).

13. *Id.* at 98. Newton used the phrase in a letter to Robert Hooke, with whom he was having a dispute, mediated through the new Royal Society. *Id.* at 217–18. The phrase is famous for its use by Newton, but apparently had long been in use. For an attempt to trace the origins of Newton’s trope, see generally ROBERT K. MERTON, *ON THE SHOULDERS OF GIANTS: A SHANDEAN POSTSCRIPT* (Post-Italianate ed. 1993). Murray Gell-Mann, the famous modern theoretical physicist, parodied Newton’s remark: “If I have seen farther than others, it is because I have been surrounded by midgets.” See <http://www-personal.umich.edu/~mbatalla/quotes.html> (last visited Mar. 30, 2004).

A final aspect of the scientific method that merits attention is that it often succeeds by means of brilliant failures. Naturally, no one sets out to fail. Rather, great scholars often set out to make bold and important claims that may be difficult to defend. They make the most compelling case they can for their outrageous view, and if they are successful, they persuade a skeptical audience of those learned in their subject.<sup>14</sup> If they are not successful, all is not lost—we may have learned something interesting in the course of demonstrating the falsity of the outrageous claim. The Nobel Laureate physicist John Archibald Wheeler drew attention to this little-remarked aspect of science in saying that “[o]ur whole problem is to make the mistakes as fast as possible . . .”<sup>15</sup>

Contrast the importance of brilliant failure in successful science to the attitude prevalent among law professors that there is something suspicious about something that is too brilliant. Indeed, my friend and former colleague Daniel Farber once wrote an entertaining article—*The Case Against Brilliance*<sup>16</sup>—on the extraordinary unlikelihood that anything too brilliant, such as the Coase Theorem, could possibly be true.<sup>17</sup> But, as I hope to show in the next two Sections, this criticism misses the point of the scientific method. Brilliant scholarship, even if dead wrong, may have an important stimulating effect in that it induces others to do superb work that refutes the inaccurate brilliance.<sup>18</sup>

### B. *The Study of Law as Science*

In order to take stock of the possibilities of a legal science, I comment first on the position of law schools within great research universities. I shall contend that when the principal business of law schools was the training of

14. A famous example of this in law and economics is the story that when Ronald Coase first presented his *The Problem of Social Cost* to a seminar at the University of Chicago, almost no one agreed with his characterization of the importance of transaction costs. Indeed, in a formal vote taken shortly after he outlined his central claim, Coase was the only person who voted in its favor. The story holds, however, that when a second vote was taken at the end of the evening, Coase’s view won overwhelmingly. See Ronald H. Coase’s *Autobiography*, at <http://www.nobel.se/economics/laureates/1991/coase-autobio.html> (last modified July 2, 2003), which Professor Coase delivered to the Nobel e-museum after he received the Nobel Prize in Economic Sciences in 1991. See also Thomas W. Hazlett, *Looking for Results: An Interview with Ronald Coase*, REASON, Jan. 1997), available at <http://reason.com/9701/int.coase.shtml>.

15. KARL POPPER, CONJECTURES AND REFUTATIONS: THE GROWTH OF SCIENTIFIC KNOWLEDGE at vi (2002). See also HENRY PETROSKI, TO ENGINEER IS HUMAN: THE ROLE OF FAILURE IN SUCCESSFUL DESIGN (1985), for which wonderful reference I am grateful to my colleague Wayne LaFave.

16. Daniel A. Farber, *The Case Against Brilliance*, 70 MINN. L. REV. 917 (1986).

17. *Id.* at 918–23.

18. All other things equal, surely one prefers *correct* brilliant work. But one should not underestimate the great value—to scholarly work—of brilliant failures.

professionals, there was no tension between the teaching duties and scholarly aspirations of law school faculty, although there was a tension between the goals and aspirations of the nonprofessional scholarly departments of great research universities and law schools. Then I turn to a discussion of the changing nature of legal scholarship to show that the scientific method of inquiry has become more attractive. I also argue that the changing nature of legal scholarship has created a tension between the teaching duties and scholarly aspirations of law faculty. While that has created some unsettling tensions within law schools, it has also brought the legal academy much closer to the nonprofessional, scholarly departments in research universities.

### 1. Doctrinalism: Unity of Purpose in Teaching and Scholarship

The principal task of almost every law school is the training of professional lawyers, not the training of scholars. True, law school graduates are “learned in the law,” but that learning consists mainly of familiarity with a “skill set” for practicing law, not a learning that equips him or her to participate in the scholarly life of a law professor. We train our students in much the same way that driving schools teach their students to pilot cars or carpentry schools teach their students the skills or art of working with wood. We do not create Ph.D.’s.<sup>19</sup> If our students find their way back to the academy, it is by chance, not by our design.<sup>20</sup>

This focus on professional education is the first of two central differences between professional schools and other scholarly units within great research universities. The second is that the faculty in professional schools frequently direct their scholarship at professionals outside the academy rather than towards other scholars within the academy. This fact has meant that the teaching duties and the scholarly work of most law professors were in harmony—both were directed toward the law in action, toward becoming skillful in and having an influence upon the practice of law and the art of judging.

19. Some top-ranked law schools, notably Yale, New York University, and the University of Chicago, are taking steps to identify those of their students who have scholarly aspirations and talents and providing special courses and other supervision tailored toward equipping those students to be productive legal scholars.

20. This characteristic of law schools is precisely the central characteristic of other professional schools that reside within the great research universities, such as those of journalism, education, medicine, veterinary medicine, business, social work, public administration, and public policy. Those units, as well as others, train their graduate students for professions, not for the academy. I strongly suspect that many professional schools have precisely the same uneasy position within the academy as do law schools.

The legal scholarly style that has, until recently, been paramount in law schools is “doctrinalism,” because it is concerned with legal “doctrine.” The goal of much doctrinal scholarship is to bring coherence to the sometimes swirling complex world of law. That coherence is clearly an important social objective. A doctrinal scholar strives to have an impact on judges, lawyers, and, ultimately, to reform the law. He or she wants to influence the doctrine, to persuade those in a position to make law to see things his or her way, to recognize the errors of what previous courts have done, and to look to him or her for guidance about troubling legal matters in the future.

Another implication of doctrinal scholarship is that it is inherently local with respect to subject matter and place. Doctrinalism confines itself usually, but not exclusively, to a particular area of the law, such as tax or bankruptcy. Only rarely does a doctrinalist seek to elucidate commonalities across different areas of the law—to show, for instance, that doctrines regarding state sovereignty have important implications for those in bankruptcy. She does not generally seek to find theoretical commonalities in broad areas of the law or to erect an overarching theory for explaining why the doctrine is what it is.

Similarly, doctrinalism is localized with respect to place. A doctrinal scholar who is studying some legal subject in Moldova has very little of interest to say to a doctrinal scholar studying that same legal subject in New South Wales. Doctrinal scholars—unlike almost anyone else in the nonprofessional departments within a great university—do not have a commonality of scholarly interest with other doctrinalists around the world. It is generally (although not exclusively) the particularities of time and place that interest doctrinal scholarship. It is not the generalities that span time, jurisdiction, and subfield that are of central interest to them. This fact seems to me to encapsulate what is so profoundly different about what has heretofore gone on in law school scholarship from what goes on in the rest of the academy.

While the doctrinal style of scholarship creates a unity of purpose as between teaching duties and scholarly aspiration of law faculty, it created a clear and sometimes festering conflict between what the rest of the university has long expected of its scholars and what professional schools expected of their scholars. Broadly speaking, the nonprofessional units of the great research universities expect their scholars to publish so as to have developed a laudable national and international reputation among other scholars learned in their area. And at evaluation time, those units have consulted other prominent scholars for their candid opinions of the worth of

the candidate's scholarship. Moreover, prior to the evaluation there were numerous other indicia of scholarly worth to which the Ph.D.-granting units could appeal in order to assess faculty quality—publications in peer-reviewed journals, appearance at scholarly symposia, invitations to give seminars on works-in-progress, and so on.

In contrast, the professional schools used a different set of criteria focused on establishing the young scholar's impact on the bench and bar—for example, through citations to the scholar's work by appellate and supreme courts. And the inherent geographical and subject matter locality of the doctrinal work was so different from the national and international character of much other university scholarship that the evaluations of the work of junior doctrinal scholars were bound to be out of sync with what the research universities generally expected of other junior faculty scholars. Most research universities had a strong sense that the professional school indicia were testimony to something other than laudable scholarly reputation, and, as a result, professional school junior faculty have often run into problems at the university level.<sup>21</sup>

I am not denigrating doctrinal scholarship because I recognize its importance to law and am a happy consumer of much of that scholarship. I am simply drawing attention to the fact that doctrinal scholarship typically looks *outside the legal academy* for its indicia of excellence or of impact and that this method of establishing scholarly worth is at odds with what goes on in the rest of a great research university. The parts of the academy whose central business is training scholars looks almost exclusively *inside the academy* for indicia of scholarly excellence.

The unity of purpose in professional teaching and scholarship within law schools made for harmony within the legal academy but for disharmony with the remainder of the great research universities.

## 2. Proto-Science in the Study of Law

Doctrinalism's preeminence in legal scholarship has been waning for a number of years. In the following Section I shall seek to explain the role that law and economics may have had in that development. Here I shall first speculate on why the scientific method did not appeal to doctrinal scholars and then point out some indicators of the change away from doctrinalism as the dominant form of legal scholarship and toward a proto-scientific method of scholarly inquiry about the law.

21. I base this contention on twenty years of observation within the University of Illinois, discussions with faculty members from other departments and other universities, and with former and current administrators at Illinois and elsewhere.

The scientific method of inquiry did not appeal to doctrinal legal scholars, possibly because they perceived no particular advantage to them in adopting it. Neither the theoretical aspect of science nor the empirical testing of those theories had much innate appeal, although, in fairness, it should be said that there was *some* interest in empirical work (but there should have been more).<sup>22</sup>

But there is more to the eschewing by doctrinal legal scholars of the scientific method than mere self-interest. Rather, there seems to be a widely held belief in the legal academy that the scientific method is inappropriate for the study of law.<sup>23</sup> Commentators frequently point to one overriding reason that theorizing about the law is inapposite—that there are vast differences in the institutional, cultural, historical, and social context of every legal system. Perhaps this is meant to indicate that there are so many particularities about the law that they swamp whatever generalities across legal systems there may be. Indeed, doctrinalism takes much of its force from the very careful parsing of details of actual cases or telling hypotheticals. Thus tethered to actualities, it would have taken a heroic leap to get to theoretical generalities.

But this claim that theory is inappropriate for the study of law seems to me to be far fetched. One might make precisely the same claim against theorizing in anthropology, sociology, economics, political science, or any other social science. And yet scholars in those and other social sciences have sought and in most cases found transnational theories.<sup>24</sup> Even if scholars in those areas of social science have not yet succeeded in articulating universal theories, they nonetheless try to do so and, at a minimum,

22. The legal realists had made much of the fact that formalism paid far too much attention to the logical coherence of law and far too little to the real effects of law. *See, e.g.*, NEIL DUXBURY, PATTERNS OF AMERICAN JURISPRUDENCE 38 (1995). In light of that interest, it is curious that there was not much more empirical legal research by the realists.

23. This is, for instance, one of the themes of ANTHONY T. KRONMAN, THE LOST LAWYER: FAILING IDEALS OF THE LEGAL PROFESSION (1993).

24. Indeed, several of those social sciences, such as economics, have gone through a kind of scholarly phylogeny that mirrors the development from a discipline based on contextual particularities to one based on universal generalities. Consider, for example, this story about the development of economics as a scientific study in the early part of the twentieth century:

Giving a lecture before a convention of scientists at Geneva, Pareto was interrupted from the floor by a patronizing cry from Gustav von Schmoller, an economist of the then German Strassburg: "But are there laws in economics?" Schmoller had no personal acquaintance with Pareto at the time. After the lecture Pareto recognized the heckler on the street and sidled up to him in his shabby clothes and in guise of a beggar: "Please, sir, can you direct me to a restaurant where one can eat for nothing?" "Not where you can eat for nothing, my good man," the German replied, "but here is a place where you can eat for very little!" "So there *are* laws in economics!" laughed Pareto as he turned away.

VILFREDO PARETO, THE MIND AND SOCIETY: A TREATISE ON GENERAL SOCIOLOGY at xviii (Arthur Livingston ed., 1935).

share common scholarly values with regard to what should count as valuable research that transcends national boundaries.

If, for reasons that I shall sketch in the Conclusion, legal scholarship is becoming more scientific, then there are, as with any scientific undertaking, two related elements to the inquiry—articulation of a theory with testable hypotheses and their confrontation with data to see the extent to which the world confirms or refutes the hypotheses. Both of these aspects of science are evident in modern legal scholarship. For example, there is an increasing amount of theoretical work in law, at least in that realm informed by economics.<sup>25</sup> And if empirical work follows theory as night follows day, then there should also be an observable increase in the amount of empirical work being published by legal scholars. As, indeed, there is. Professor Robert Ellickson published a statistical study of recent trends in legal scholarship.<sup>26</sup> He sought to compare the number of empirical pieces published in law reviews with the number that expressed skepticism about the value of empirical work during the period from 1982 to 1996.<sup>27</sup> He found that indices of empirical and quantitative work were constant from 1982 to 1996 but that during the period from 1994 to 1996 there were six times more references to empirical work than to work likely to be skeptical of empirical work. Over the same fourteen-year time period, however, Ellickson found that his indices for terms indicating statistical work or statistical significance doubled.<sup>28</sup> He concludes, “The data . . . thus hint that law professors and students have become more inclined to produce (although not to consume) quantitative analyses.”<sup>29</sup>

There are other examples of changes afoot in legal scholarship that make it more science-like. Legal scholars are writing for one another today much more than for practitioners and judges. As a result, citations to one’s work by other scholars now figure much more prominently in faculty evaluations than do citation to that work by courts.

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 en-

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

26. Robert C. Ellickson, *Trends in Legal Scholarship: A Statistical Study*, 29 J. LEGAL STUD. 517 (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. See also William M. Landes, *The Empirical Side of Law & Economics*, 70 U. CHI. L. REV. 167 (2003).

27. Ellickson searched online databases for instances of *empiric!* and *post-modern!*, which latter he took to be a proxy for skepticism about empirical work. Ellickson, *supra* note 26, at 528–29.

28. Ellickson searched online databases for the terms *statistic!* and *significan!*. *Id.* at 528.

29. *Id.*

trants 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 had some experience practicing law before coming to the academy.

And, finally, the extrinsic indicia that the other scholarly units within great research universities have long looked to as indicators of faculty productivity and reputation—publication in peer-reviewed journals, conference organization and presentations, invitations to present works in progress to faculty at other law schools, refereeing grant proposals and manuscript submissions, and more—are all now becoming routine elements in faculty evaluation.

*C. The Role of Law and Economics in Making the Study of Law Scientific*

My assertion is that the revolution that law and economics has brought to legal scholarship is the commitment to the scientific method of inquiry. Economics, whatever faults it may have, is a collective and cumulative enterprise that seeks to achieve its knowledge by means of the scientific method of inquiry. And its use in the study of law has, whatever else it may have done, brought a commitment to the scholarly standards that prevail in the rest of the nonprofessional academy.

Let me be more specific about how law and economics has brought the two related aspects to the scientific method—theorizing and testing—to the study of law. I'll begin with theory. As I mentioned in the previous Section, legal scholars and lawyers have always been skeptical of theory, reluctant to reach too far in justifying a conclusion and, instead, going only far enough to encompass the facts before them.

This skepticism has informed many legal scholars' criticisms of law and economics. The Coase Theorem comes in for much of this criticism but frequently for the wrong reasons.<sup>30</sup> While it is perfectly fair to criticize a theory because it does not comport with one's intuitions or otherwise ring true, the corrective is not to disparage theory as such but rather to provide a better theory. Often the better theory is the result of bringing some empirical work to bear on the unsatisfactory theory. This feedback between theory, intuition, and empirical work is common among those who think themselves scientific, but it is exceedingly rare in the study of law, with the

30. For the theorem's original statement, see R. H. Coase, *The Problem of Social Cost*, 3 J.L. & ECON. 1 (1960). The Coase Theorem holds that where bargaining or transaction costs are very low, an efficient allocation of resources will obtain, regardless of the law. See ROBERT COOTER & THOMAS ULEN, *LAW AND ECONOMICS* 85–91 (4th ed. 2004).

exception of law and economics. Indeed, let me point to two extremely important examples of this mechanism of theory-evidence-refined theory.

Law and economics may reasonably accurately be characterized as the application of rational choice theory to law. What struck many critics as flawed about the entire enterprise was captured in the question, "Who are these rational people you're talking about?" That's a fair question. The right response is not to denigrate theory or to claim that legal decision makers are irrational, which they clearly are not. Rather, the right scholarly response is to take due account of the findings of cognitive and social psychologists who have identified systematic deviations from the predictions of rational choice theory and to explore their applicability to legal analysis. If the results of psychological research are inapposite for the legal analysis at hand, then perform some experiments oneself. A large number of legal scholars are doing just that,<sup>31</sup> and in doing so have provided numerous useful insights into how to incorporate systematic and widespread flaws in judgment and decision making into the analysis of law.<sup>32</sup>

Let me cite one other example to illustrate the value of spectacular failures and of the cycle of theory-empirical work-refined theory. In the mid-1980s, Professor Robert Ellickson sought to test the Coase Theorem by investigating the practices of cattle ranchers and farmers in Shasta County, California, with respect to liability for damage done to farm property by unsupervised cattle.<sup>33</sup> Shasta County presented ideal conditions for a natural experiment of the Coase Theorem because the law of liability for damage done by unsupervised cattle was different in the eastern and western parts of Shasta County. If the costs of bargaining were low, as the Coase Theorem requires, then the actual practices in the two halves of the county would not be different even though the law on the books was different. 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. Those untended cattle sometimes wandered onto farmers' or other private, non-ranch, non-common property.

31. See BEHAVIORAL LAW AND ECONOMICS (Cass R. Sunstein ed., 2000); Korobkin & Ulen, *supra* note 2.

32. See, e.g., Jeffrey J. Rachlinski, *Gains, Losses, and the Psychology of Litigation*, 70 S. CAL. L. REV. 113 (1996), in which he demonstrates that the presence of loss aversion (the finding that most people are risk averse with respect to gains and risk seeking with respect to losses) may be used to criticize the standard account of litigation versus settlement. See COOTER & ULEN, *supra* note 30, at 410.

33. Robert C. Ellickson, *Of Coase and Cattle: Dispute Resolution Among Neighbors in Shasta County*, 38 STAN. L. REV. 623 (1986). Ellickson elaborated on the Shasta County study and other examples in ORDER WITHOUT LAW: HOW NEIGHBORS SETTLE DISPUTES (1991).

To his amazement, Ellickson found that the potential disputants did not know what the law regarding legal obligations for stray cattle was; 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.” Good neighbors, the norm directed, did not sue one another; they helped each other. When a farmer found stray cattle on his property, 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 incurred the costs of repair herself and never asked for indemnification from the cattle owner.

The response among legal scholars (mostly law –and economics specialists) to Ellickson’s article was to alter their contention that individuals assiduously sought to conform their behavior to the dictates of the law or to bargain around the law. Ellickson’s work suggested that, at least in some circumstances, social norms, not law, were a guiding principle of most people’s behavior. And that created an entire new body of scholarly inquiry that sought to elaborate on the positive and normative relations between law and social norms.<sup>34</sup>

Because of its connection to economics and because of economics’ commitment to empirical research, law and economics inevitably brought to the study of law a renewed commitment to testing hypotheses. I intend to elaborate on this and to give a particularly compelling example of the value of empirical work in the next Section. I want here to make another point—that although there has been some fascinating empirical work done in the study of law by doctrinalists, legal scholars have more or less ignored that empirical work. For example, in two studies of the use of promissory estoppel, Professor Stanley Henderson and then Professors Daniel Farber and John Matheson found that over a twenty-year period courts almost routinely enforced promises supported only by reasonable detrimental reliance, even in commercial relationships.<sup>35</sup> Astonishingly, textbooks on contract theory took virtually no notice of these findings. It is simply inconceivable

34. See, e.g., ERIC A. POSNER, *LAW AND SOCIAL NORMS* (2000); Richard H. McAdams, *The Origin, Development, and Regulation of Norms*, 96 MICH. L. REV. 338 (1997).

35. See Daniel A. Farber & John H. Matheson, *Beyond Promissory Estoppel: Contract Law and the “Invisible Handshake,”* 52 U. CHI. L. REV. 903 (1985); Stanley D. Henderson, *Promissory Estoppel and Traditional Contract Doctrine*, 78 YALE L.J. 343 (1969). I elaborate on these studies in Ulen, *supra* note 3, at 902–05.

that in a discipline committed to the scientific method of inquiry that so little attention would be paid to important empirical work.<sup>36</sup>

## II. EMPIRICAL LEGAL SCHOLARSHIP

The case in favor of more empirical legal scholarship is an extremely easy one to make.<sup>37</sup> The received learning in the law is full of implicit and explicit empirical questions—such as, to take but two examples, the extent to which creative activity is affected by intellectual property rights<sup>38</sup> and the circumstances under which and the frequency with which governmental bodies invoke their taking power—and yet there has been, until the last ten or so years, relatively little interest in empirical legal research.<sup>39</sup>

There are two obvious reasons and two not-so-obvious reasons for the paucity of empirical work in the law. The first of the obvious reasons is that to do empirical work requires a facility with statistical techniques—such as hypothesis testing, regression analysis, and ANOVA—in which the typical law professor has had no formal training.<sup>40</sup> That is not to say that law faculty and students cannot learn these statistical techniques because, of course, they can and have. Nonetheless, there is a lot of legal material that the average law professor must master. To warrant giving up time devoted to mastering that material in order to learn empirical techniques requires making a compelling case. Junior law faculty may perceive making a commitment to empirical techniques as a high-risk, and, therefore, unattractive strategy for promotion, and senior faculty may be set in their research techniques.<sup>41</sup>

36. In Ulen, *supra* note 3, at 905–06, I also considered the fact that Professor Douglas Laycock's remarkable empirical findings about the irreparable injury rule had been ignored by those who write about remedies. DOUGLAS LAYCOCK, *THE DEATH OF THE IRREPARABLE INJURY RULE* (1991).

37. Early in 2004, Blackwell will begin publication of a new journal, the *Journal of Empirical Legal Research*, edited by Professors Theodore Eisenberg, Jeff Rachlinski, and Stewart Schwab of the Cornell Law School. For the last several years, the Legal Scholarship Network of [www.ssrn.com](http://www.ssrn.com) has published a periodic group of working papers on empirical legal research under the editorship of Professor Jennifer Arlen of the New York University School of Law.

38. John R. Allison et al., *Valuable Patents*, GEO L.J. (forthcoming 2004), available at <http://papers.ssrn.com/abstract=426020>.

39. A notable exception has been the scholars who characterize themselves as affiliated under the banner of “law and society.” With only slight violence to the facts, one might distinguish this group of scholars as having their university homes generally, though not exclusively, outside law schools. For instance, sociologists have been a particularly active group in law and society. This outsider status—outside, that is, law schools—reinforces my point about the fact that traditional, doctrinal legal scholars have been generally uninterested in empirical research about the law.

40. The scarifying aspect, to law students, of mathematics is hard to exaggerate. Indeed, it is so well known among German academics that there is a Latin phrase to cover it—*iudex non calculat*.

41. One can, of course, overcome these shortcomings by coauthoring with someone who is skilled in statistical matters. But things are clearly changing within law school faculties. My colleague Tom Ginsburg and I invited our colleagues at the University of Illinois College of Law to join us in reading a

The second obvious reason for the relative scarcity of empirical legal research is that the data with which to work is nonexistent or difficult to locate. There is a simultaneity problem here—data tends to get gathered if researchers express a need for that data. To the extent that legal researchers have not cared to pursue empirical research there has not been much of a demand for legal data.

A related but distinct point is that lawyers, when they have thought about empirical research, have tended to focus on data gathered from court reports. There is certainly a great deal to be learned from data about the kinds of complaints filed, their disposition, the amounts in controversy, and the like.<sup>42</sup> But there is a great deal of valuable information—I am tempted to say “more valuable information”—to be gleaned about the law by looking away from filed complaints. First, the vast majority (the usual estimate is 95 percent) of potential disputes do not result in a trial.<sup>43</sup> Second, and more importantly, I believe, many if not most of the effects that law seeks to achieve leave no trace in the court system. Consider, for instance, that if tort law succeeds in inducing cost-effective precaution, as we hope that it does, there may be fewer or less severe accidents and, perhaps as a result, less litigation. What we are seeking is evidence of accidents that did not occur or were less severe than they otherwise would have been. Those are obviously difficult data to unearth, but two things are relatively clear—(1) that clever investigators will find that evidence, and (2) they are unlikely to find it in court records.<sup>44</sup> So, if law faculty are to make a commitment to empirical legal research, that will probably entail a shift in their focus away from cases as the principal source of data.

I said that there were two nonobvious reasons why empirical legal research is less widespread than one might expect. The first of those nonobvious reasons is something I have already stressed—law faculty have for so

statistics and econometrics text. We expected no more than two or three to join us; in fact, twenty have done so.

42. See, e.g., Kevin M. Clermont & Theodore Eisenberg, *Litigation Realities*, 88 CORNELL L. REV. 119 (2002); Kevin M. Clermont & Theodore Eisenberg, *Plaintiphobia in the Appellate Courts: Civil Rights Really Do Differ from Negotiable Instruments*, 2002 U. ILL. L. REV. 947 (2002); Theodore Eisenberg, *Damage Awards in Perspective: Behind the Headline-Grabbing Awards in Exxon Valdez and Engle*, 36 WAKE FOREST L. REV. 1129 (2001).

43. See George L. Priest & Benjamin Klein, *The Selection of Disputes for Litigation*, 13 J. LEGAL STUD. 1 (1984); see also COOTER & ULEN, *supra* note 30, at 414; Robert D. Cooter & Daniel L. Rubinfeld, *Economic Analysis of Legal Disputes and Their Resolution*, 27 J. ECON. LITERATURE 1067 (1989); Marc Galanter, *Reading the Landscape of Disputes: What We Know and Don't Know (And Think We Know) About Our Allegedly Contentious and Litigious Society*, 31 UCLA L. REV. 4 (1983); Lawrence M. Spizman, *The Defense Economist's Role in Litigation Settlement Negotiations*, 5 J. LEGAL ECON. 57 (1995).

44. One should look elsewhere unless the hypothesis is that, as the tort law example suggested, less or a different form of litigation might result.

long been wed to a nonscientific conception of their scholarship that they do not readily and naturally think of the connection between theory and empirical work that the scientific method necessarily implies. I noted in the previous Section that the remarkable empirical work of Professor Stanley Henderson and Professors Dan Farber and John Matheson on promissory estoppel and of Professor Doug Laycock on the irreparable injury rule has had no discernible impact on contract scholarship and teaching or on remedies scholarship and teaching. And I contrasted that non-response to the vigorous response that Professor Bob Ellickson's work on social norms has had. What is the difference? The telling difference that I can see is that those who follow Ellickson's work are, by and large, law –and economics scholars, to whom empirical work seems a natural consequence of any scholarly endeavor and who feel a natural duty to revise their theories in light of well-done, persuasive empirical findings. By contrast, most contracts scholars are doctrinalists for whom empirical work is foreign territory inhabited by dragons.

The second nonobvious reason that law faculty have shied away from empirical legal scholarship is what I perceive to be a fashionable and intoxicating world-weariness among many legal scholars. My strong impression is that for many law faculty the revolutionary headway being made by law –and economics and other “law and” fields in the study of law has produced a kind of counter-reformation whose principal tenets are that the scientific method itself is contextually dependent; theory is politics in fancy garb; and empirical work reaches only the results that the researcher wants it to reach. Set to one side both the profound intellectual laziness and the dazzling contempt for the life of the mind that these views imply. Is there anything serious to be taken from these criticisms? If so, it is simply the pragmatic one that the scientific method rarely results in settled conclusions. But everyone familiar with science knows that it is a *process*, not a formula, for uncovering valuable information and that the process is collective and cumulative.

I have already given some examples of legal questions that might profitably be answered by empirical research. I want to conclude this Section with what I believe to be the most marvelous recent example of empirical research's power to illuminate legal issues—John Donohue and Steve Levitt's article, *The Impact of Legalized Abortion on Crime*.<sup>45</sup>

The rate of non-violent crime in the United States began to decline in the mid-1980s and that decline has continued, though at a decreasing rate,

45. John J. Donohue III & Steven D. Levitt, *The Impact of Legalized Abortion on Crime*, 116 Q.J. ECON. 379 (2001).

until 2002. Property crimes fell 30 percent in the 1990s. Indeed, some of our rates of non-violent crime, such as for auto theft and burglary, are below rates for those crimes in Western Europe. In the early 1990s, a decline in violent crime began that has also continued until the present. Homicide rates, for example, declined 40 percent in the 1990s and are at their lowest levels since the 1930s.

What has caused this dramatic turn of events? There are a multitude of plausible theories. Here are four:

- (1) State and federal criminal sentencing grew more certain and more severe in the 1980s and 1990s with the effect that the prison population in the United States increased by a factor of four between 1980 and 2000. Society got tough on crime, and the predictable result was that criminals were deterred from committing crimes.
- (2) Improvements in police practices, such as community policing and sprucing up public spaces through application of the “broken windows” policy,<sup>46</sup> have greatly reduced the free reign that criminals had over certain neighborhoods in urban areas.
- (3) The robust economic growth of the 1983–2001 period greatly increased the attraction of legitimate employment and raised the opportunity cost of criminal activity.
- (4) The waning of the crack cocaine trade in the early 1990s greatly reduced the violence that came from competition among illegal organizations for the lucrative crack cocaine trade.

All of these are plausible theories, and, importantly, they have very different policy implications for reducing crime. In the absence of careful empirical work, it is unclear which of those different policy implications is most responsible for the decline in crime and which, therefore, we ought to put our trust in to reduce crime in the future or in other settings. It is also possible that there is or are some completely different theory or theories to explain the data.

In 1998, Professor John Donohue of Stanford University and Professor Steve Levitt of the University of Chicago published a startling alternative explanation for the decline in crime—the legalization of abortion in the

46. See GEORGE L. KELLING & CATHERINE M. COLES, *FIXING BROKEN WINDOWS: RESTORING ORDER AND REDUCING CRIME IN OUR COMMUNITIES* (1996). *But see* BERNARD E. HARCOURT, *ILLUSION OF ORDER: THE FALSE PROMISE OF BROKEN WINDOWS POLICING* (2001).

1970s.<sup>47</sup> Their article began from the observation that there just might be a causal connection between the U.S. Supreme Court's *Roe v. Wade*<sup>48</sup> decision handed down in January 1973, that legalized abortion and the decline in violent crime that began exactly eighteen years later. In the United States almost 50 percent of all crime is committed by males under the age of twenty-five.<sup>49</sup> Could it possibly be the case that the legalization of abortion in 1973 led to a significantly smaller cohort of eighteen-year-old males in 1991? And if so, could this decline in the number of eighteen-year-old males in 1991 help to explain the decline in violent crime?

Donohue and Levitt are two of the most careful empirical scholars in the legal academy today; so, their findings must be taken very, very seriously. Their very thorough study suggested that legalized abortion accounted for 50 percent of the decline in crime after 1991. Their study is largely sophisticated econometrics, but I can convey a sense of this marvelous scholarship by citing six reasons that they give in support of their hypothesis:

The number of abortions rose dramatically throughout the 1970s so that by 1980 there were 1.6 million abortions per year—that is, one abortion per two live births. The effect was a significantly smaller population of eighteen-year-olds in the United States in the early 1990s as a percentage of the total population.<sup>50</sup>

Five states legalized abortion in 1970 (three years before *Roe v. Wade*), and those states experienced a decline in crime before the rest of the country did.<sup>51</sup>

“[H]igher rates of abortion in a state in the late 1970s and early 1980s are strongly linked to lower crime” in that state for the period 1985 to 1997.<sup>52</sup>

47. Donohue and Levitt's original article appeared in 1998 as a working paper at [www.ssrn.com](http://www.ssrn.com). It was not published by *The Quarterly Journal of Economics* until several years later. Long before the article appeared in a professional journal, there had been a front-page article about it in the *New York Times* and much online commentary. This is testimony both about the importance of the authors' hypothesis and about the connection between abortion and crime, but it also says a great deal about the changing nature of legal scholarship.

48. *Roe v. Wade*, 410 U.S. 113 (1973).

49. SOURCEBOOK OF CRIMINAL JUSTICE STATISTICS 352–53, 355 (Kathleen Maguire & Ann L. Pastore eds., 30th ed. 2002), available at <http://www.albany.edu/sourcebook/>; Donohue & Levitt, *supra* note 45, at 382.

50. Donohue & Levitt, *supra* note 45, at 380, 381, 384–85.

51. *Id.* at 382, 395–99.

52. *Id.* at 382.

“[T]here is no relationship between abortion rates in the mid-1970s and crime changes between 1972 and 1985 . . .”<sup>53</sup>

Almost all the decrease in crime in the 1990s can be “attributed to reductions in crime among the cohorts born after abortion legalization[;] [t]here is little change in crime among older cohorts” over the last thirty years.<sup>54</sup>

And finally, the decline in crime in the 1990s was nationwide, occurring in cities that had never had a crack cocaine epidemic nor had a reform of their policing practices and in rural areas where urban problems were unknown.<sup>55</sup>

As if that were not startling enough, Donohue and Levitt went further. They identified two components of the effect of abortion on crime—the “cohort size” effect and the “cohort quality” effect. The “cohort size” effect arises from the fact that there were relatively fewer eighteen to twenty-four-year-old males in the U.S. population in the 1990s, and, all other things equal, fewer young males means less crime. But the “cohort quality” effect suggests that the cohort of young men who were born after the legalization of abortion in 1973 were less likely to commit crime than would a similar cohort that would have been born had abortion not been legal. The reason is that “women who have abortions are those most at risk to give birth to children who would engage in criminal activity. Teenagers, unmarried women, and the economically disadvantaged are all substantially more likely to seek abortions.”<sup>56</sup>

Donohue and Levitt suggest that of the 50 percent of the decline in crime that legalized abortion can explain, half of that half is attributable to the “cohort size” effect and half to the “cohort quality” effect.

These are highly controversial results, but can anyone deny that this is a startlingly original insight of great importance? I commend their work to you as a spectacular example of the bright light that careful empirical work can throw on important legal issues.<sup>57</sup>

53. *Id.*

54. *Id.*

55. *Id.* at 380.

56. *Id.* at 381 (citing PHILLIP B. LEVINE et al., *ROE V. WADE AND AMERICAN FERTILITY* (Nat'l Bureau of Econ. Research, Working Paper No. 5615, 1996), available at <http://www.nber.org/papers/w5615>).

57. The authors have recently published another paper with additional evidence on the relationship between the legalization of abortion and the decline in crime. See JOHN J. DONOHUE III & STEVEN D. LEVITT, *FURTHER EVIDENCE THAT LEGALIZED ABORTION LOWERED CRIME: A REPLY TO JOYCE* (Nat'l Bureau of Econ. Research, Working Paper 9532, 2003), available at <http://www.nber.org/papers/w9532>.

### III. LAW AND ECONOMICS AND OTHER LEGAL INNOVATIONS WITH RESPECT TO EMPIRICAL AND EXPERIMENTAL INQUIRY

I mentioned earlier in this Article that law and economics is only one of a relatively large number of “law and” or other innovations in legal scholarship of the past eighty or so years. Clearly some of these innovations have had a greater impact on the legal academy than have others. Legal realism, for example, seems to have had a profound effect on American legal academics (and is, arguably, uniquely American, there being little evidence of similar realist innovations in other educational legal systems of the world).<sup>58</sup> Others, such as critical legal studies, seem to have had virtually no lasting effect on law or legal scholarship. In this Section I want to compare the effect that I think law and economics will have on legal scholarship with the effects that two other successful “law and” innovations might have.

I have argued already that I believe that the most profound effect that law and economics will have on legal teaching and research is its commitment to the scientific method of inquiry. Let me reiterate the point that as important as I think that some of the substantive findings of law and economics are and will be, I believe that the far more profound impact of the economic analysis of law will be its unexpectedly bringing to the study of law the virtues of the scientific method of inquiry. So, it is against that contention that I want to compare law and economics with two other recent “law and” innovations.

Law and society is a movement that has a longer history in the academy than almost any other scholarly innovation of the past half-century. More than forty years ago Professors James Willard Hurst and Stewart Macaulay (and others) began to publish studies of the law in action that should have alerted the rest of the legal academy that what they were teaching about the law and what the law was really achieving were not necessarily the same.<sup>59</sup> The idea is a powerful one and has attracted a large following, which meets annually. Those who characterize themselves as law –and society scholars have done the legal academy the inestimable

58. See generally JOHN HENRY SCHLEGEL, *AMERICAN LEGAL REALISM AND EMPIRICAL SOCIAL SCIENCE* (1995); see also DUXBURY, *supra* note 22; MORTON J. HORWITZ, *THE TRANSFORMATION OF AMERICAN LAW, 1870–1960: THE CRISIS OF LEGAL ORTHODOXY* (1992); LAURA KALMAN, *LEGAL REALISM AT YALE, 1927–1960* (1986). To the extent that legal realism is a necessary precursor of law and economics, the fact that other legal systems may not have experienced a realist movement may explain their slowness to accept law and economics.

59. See, e.g., JAMES WILLARD HURST, *LAW AND THE CONDITIONS OF FREEDOM IN THE NINETEENTH-CENTURY UNITED STATES* (1956); Stewart Macaulay, *Non-Contractual Relations in Business: A Preliminary Study*, 28 AM. SOC. REV. 55 (1963).

service of showing the importance of empirical research for the study of the law.

But law and society has two significant shortcomings as an influence on legal scholarship (assuming, as I do, that the adoption of the scientific method is the correct direction in which the study of the law should be going). The first is that the field has never developed a coherent theory or set of central principles to guide the empirical work. As a result, the theory–empirical work–refined theory cycle that characterizes the scientific method could not take root in law and society. The field is, at best, a collection of interesting empirical findings without a coherent theme. The second shortcoming is that law and society scholars have tended to talk only among themselves and not to the great millrun of legal scholars who do not share their devotion. The field can hope to have a notable impact only if its findings appeal outside its core devotees.

Law and philosophy is another legal innovation whose contributions to substantive law have been broad and long lasting.<sup>60</sup> On the plus side are two factors. First, those learned in jurisprudence have long been comfortable with theorizing and so will continue to have a useful if not pivotal role to play in the theorization of law that will accompany its move to a scientific method of inquiry. Second, those learned in law and philosophy have a deep commitment to the scholarly life and to the values treasured by great research universities. These factors mean that as law schools make a transition to a scholarship that is more congenial to that practiced in the other Ph.D.-granting units in the great research university, law and philosophy will find the transition to be easy and will help law schools make the transition.

On the debit side there are also two points. First, legal philosophers tend to address themselves principally to others learned in jurisprudence and not to a broader congregation, even within law schools. Indeed, my experience is that jurisprudentialists are just as, if not more, comfortable among Ph.D. philosophers as they are among legal scholars.<sup>61</sup> And this is a limitation. All innovations in legal scholarship have a lasting impact to the extent that they say something that appeals beyond those fanatical about the

60. See, e.g., MICHAEL MOORE, *PLACING BLAME: A GENERAL THEORY OF THE CRIMINAL LAW* (1997). For the view that moral philosophy has contributed little of interest or substance to law, see RICHARD A. POSNER, *THE PROBLEMATICS OF MORAL AND LEGAL THEORY* (1999).

61. And in this regard they are very different from legal economists, who are, generally, far more comfortable around lawyers than they are around economists. The reason is that law and economics has generally eschewed the technical aspects of modern economics both because they find it to be unhelpful in looking at many legal issues and because technical details would make their work unintelligible—that is, even more unintelligible—to other legal scholars.

innovation—that is, that appeals to the broad audience of legal scholars. The notion, for instance, of “efficient breach of contract” is not used simply by law and economics scholars; it has become an organizing notion for all contract scholars. While law and economics has made numerous contributions to law generally, law and philosophy has not, apparently, made the same broad appeal to all legal scholars.

Second, law and philosophy, following philosophy in this regard, has not cared very much about empirical research and that fact will greatly limit its appeal to the law. Law is, ultimately, about what works (a fact that makes it congenial to empirical work), and other disciplines are unlikely to have an important impact on law if they bring no compelling evidence to back their innovations.

### CONCLUSION

Before I summarize my findings, I want to comment on two topics that I have heretofore avoided. The first is an explanation of why the particular changes in legal scholarship have been occurring. The second is what these changes might mean for the unity of purpose of teaching and scholarship that characterized the legal academy when doctrinalism was preeminent.

What accounts for this change in legal scholarship away from doctrinalism and toward a more science-like discipline? Judge Richard A. Posner identifies two factors that account, he believes, for the general rise of “law and . . .” scholarship—(1) independent, valuable developments in contiguous disciplines (such as the rise of the analysis of nonmarket behavior in microeconomics and the revival of political philosophy after the publication of John Rawls’ *Theory of Justice* in 1971), and (2) the dramatic increase in the number of law professors over the period 1960 to 2000 (which created, he suggests, pressure for these professors to adopt new forms of scholarship to distinguish their work from that of those who came before and to allow them to compete effectively with their peers for academic favor).<sup>62</sup> Elsewhere, Judge Posner explains the increase in the number of law professors as being due to a general increase in the demand for legal services and the consequent increase in the demand for legal education.<sup>63</sup> Indeed, the number of lawyers doubled between the mid-1970s and the

62. Richard A. Posner, *Legal Scholarship Today*, 115 HARV. L. REV. 1314, 1317 (2002).

63. See RICHARD A. POSNER, *FRONTIERS OF LEGAL THEORY* 3 (2001).

mid-1990s in the United States.<sup>64</sup> Let me say a brief word about these factors.

As to the importance of nonmarket microeconomic theory and the revival of political philosophy, I am not convinced that the important intellectual influences explain, if anything, only law and economics and the parts of the law that would make use of political philosophy. But I'm also not convinced that the elaboration of non-market economics was a necessary precondition for the rise of law and economics. I think a strong case can be made that law and economics arose from the remarkable insights of Ronald Coase, Harold Demsetz, Guido Calabresi, Henry Manne, and Judge Posner.

The second factor to which Judge Posner points is the rapid increase in the number of law professors and their need to distinguish themselves so as to curry academic favor. Why, all other things equal, could it not have been the case that the larger number of law professors advanced their careers simply by each of them doing doctrinal scholarship? Adopting or fostering legal scholarly innovations is not a particularly sensible advancement strategy for junior faculty. It would have been far safer to be original within the well-established confines of doctrinalism. The dynamics of faculty hiring and promotion are, of course, very complex, and so the process by which scholarly innovations come to have a foothold in the legal academy is also complex. My point is simply that there is no obvious reason why an expansion in the number of law professors should, all other things equal, which they arguably were not, necessarily lead to scholarly innovations.

I have already mentioned so much of an alternative theory to Judge Posner's theory as I have—namely, that law and economics appeared as one of a large number of scholarly innovations and, for reasons that still bear examining, caught on in the legal academy to a degree that other innovations have not and (here is the central contention of this Article) unexpectedly brought an entirely new method of scholarship to the study of law.

What about the future of legal education in light of this change in the nature of legal scholarship? If the trends that I've identified are correct, then I think that legal education is in for some serious re-thinking. The unity of purpose that existed between law school teaching and scholarship when doctrinalism was the dominant form of scholarship is gone, replaced by an increasing disharmony between what we teach our students to do and

64. See Richard H. Sander & E. Douglass Williams, *Why Are There So Many Lawyers? Perspectives on a Turbulent Market*, 14 L. & SOC. INQUIRY 431 (1989); see also DEREK BOK, *THE COST OF TALENT: HOW EXECUTIVES AND PROFESSIONALS ARE PAID AND HOW IT AFFECTS AMERICA* (1993).

what we think and talk about among ourselves. This schizophrenia as between one's teaching life and one's scholarly life has heretofore been a minor part of being a law professor and probably characterizes almost all professional education to which there is a scholarly component. But in today's law schools the gap is becoming wider; the schizophrenia is becoming far more evident than in the past.

Judge Harry Edwards, a former law professor, has drawn attention to this "growing disjunction," as he calls it, between the practice of law and legal scholarship.<sup>65</sup> He is, in my view, correct. This is a problem, and I don't know how law schools will deal with this gap in the future. One possibility is that we will more clearly separate the practitioner's and the scholar's degrees. For example, a legal education for those who want to practice may become a two-year course of study, like the MBA for those who want a practical graduate education in business. For those who want to become scholars in the law, we may have a full-blown Ph.D., much as business schools have a DBA degree for scholars of business practices. My prescience is exhausted by those proposals. The changes that are making law schools much more like the rest of the university are ultimately going to necessitate our taking a hard look at the manner in which we educate lawyers and, most likely, making some significant educational reforms.

I believe that there is a clear change afoot in legal scholarship that is making the study of law more science-like. This change has happened because law and economics unexpectedly imported into the study of law a commitment to the same theory-empirical work-refined theory cycle that has characterized the Ph.D.-granting units in the great research universities.

When doctrinalism was the dominant form of legal scholarship, there was a unity of purpose within the legal academy with respect to teaching and scholarship. But there was a notable difference between the scholarly expectations, criteria for evaluation, and customs of other units in the great research universities and the law schools. As law schools unexpectedly move to a greater commitment to the scientific method of inquiry, their relationship with the rest of the university has greatly improved. But the change has meant that the internal harmony between professional teaching

65. Harry T. Edwards, *The Growing Disjunction Between Legal Education and the Legal Profession*, 91 MICH. L. REV. 34 (1992); Harry T. Edwards, *The Growing Disjunction Between Legal Education and the Legal Profession: A Postscript*, 91 MICH. L. REV. 2191, 2191 (1993):

I fear that our law schools and law firms are moving in opposite directions. The schools should be training ethical practitioners and producing scholarship that judges, legislators, and practitioners can use. The firms should be ensuring that associates and partners practice law in an ethical manner. But many law schools—especially the so-called "elite" ones—have abandoned their proper place, by emphasizing abstract theory at the expense of practical scholarship and pedagogy.

2004]

*THE UNEXPECTED GUEST*

429

and scholarship within law schools is fraying and in jeopardy of breaking down completely.

While I regret the rise of disharmony within law schools, I very much welcome the transition to a more scientific method of examining the law. Every change has costs and benefits, and if matters are managed appropriately, the only changes that go forward are those for which the anticipated benefits greatly exceed the expected costs. Particularly within highly competitive enterprises, which U.S. higher and legal education certainly are, misguided change rarely occurs. The current change in legal scholarship, which, I have argued, arose unexpectedly, is almost certainly well worth the short-run costs because the benefits in the form of our understanding of the law will be substantial.