The Allure of Law and Economics:
Evidence-Based Law
By
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I would like to begin by thanking Professor and Dean Ranita Nagar for her very kind invitation to speak to you at the conclusion of what I am sure has been a very good and a very stimulating conference. I'm sorry that I cannot be there with you, but I'm spending this week taking care of our two granddaughters. But I am very pleased to be able to address you tonight.

My subject today is the allure of law and economics. I have been doing law and economics for nearly 40 years, and I find it just as interesting and exciting today as I did when I first discovered the field in the late 1970s. And I think that many people, including all of you, have noticed that allure yourselves or are discovering that allure now. Why has that allure continue to new generations of faculty and students and why has it commanded the attention of older scholars like me?

There are several reasons. One is the inherent interest of the topic. Another – and, I think, more important reason – is that law and economics keeps changing for the better. It is not the same field today that it was 40 years ago. Nor is it the same field that it was 20 or 10 years ago. There are always new developments by the incredibly talented people who profess law and economics. To give just three examples, consider these: First, during the late 1970s and the 1980s law-and-economics scholars wrote principally about (and elaborated) the theoretical aspects of bringing economic analysis to the study of the law. For instance, they showed how an economic understanding of tort liability might have important answers to open questions in the analysis of tort law (such as the significant differences between negligence liability and strict liability). They also showed that an economic understanding of contract law helped to explain how best to enforce bargain promises and the circumstances under which gift or donative promises might be enforceable.

Second, in the late 1980s and early 1990s, law and economics discovered behavioral economics and began to apply that field to the study of law.[1]And those applications have been transformative. Because behavioral economics focuses on the *actual*observable regularities of human behavior rather than on the normative ideal of the rational decision-maker, behavioral economics was inherently of much greater interest to lawyers and law scholars (who, despite their interest in theory, are practical and pragmatic at heart).

[1] Incidentally, this application of behavioral economics to law occurred before it occurred in economics and much more thoroughly than in economics. I do not know a single law-and-economics scholar who was surprised by the awarding of the 2002 Nobel Memorial Prize in Economic Sciences to the psychologist Daniel Kahneman, but I do know economists who were surprised by that selection.

Third, beginning in the early 1990s and continuing through to today, law and economics has taken a very empirical turn. In part this is because the economic theories of the various areas of the law had, by that time, been so thoroughly elaborated that there was very little more to do in the way of theorizing.

I want to use the remainder of my talk to focus on this ongoing work in empirical legal studies to explain the continuing allure of law and economics.

I shall seek to make four central points about empirical legal studies. First, traditional doctrinal analysis, for reasons about which I shall speculate shortly, was neither capable of doing empirical studies nor, more surprisingly, interested in what those studies might reveal. Second, a widespread interest in empirical work about legal issues blossomed with the spread of law and economics, largely because economics, as a social science, had been deeply interested in confirming or rejecting its theories through empirical work since at least the 1950s. However, and third, even when the desire to do empirical legal studies appeared after the spread of law and economics, it proved difficult for that work to begin because the legal system did not routinely provide data that was well-suited to assessing empirical propositions about the law. That situation induced empirically minded legal scholars to modify their empirical techniques from those that they would have otherwise adopted from economics. Fourth and finally, I will make a modest proposal for a something that you and I can do to make empirical legal studies better and more interesting and persuasive to legal and policy decisionmakers.

Now to some background information.

By "empirical data" I mean systematically collected information about a particular phenomenon or phenomena. Further, when I refer to "empirical data," I mean that someone has arranged those data so that they reveal as much as possible about the phenomenon. These data are typically referred to as "descriptive data" or "descriptive statistics." For example, they show us the observations that are the most common – the mean, median, and mode. They also show us the range of the data – from the lowest number to the highest number; we may have computed the variance or the standard deviation of our data. Finally, we may use these data to perform analysis of the data – for example, to search for correlations among the various elements of the data or to find causal relations through regressions.

Let me give a very brief example. Suppose that the legal phenomenon in which we are interested is crime. So, we collect as much data as we can from a variety of jurisdictions about a set of property and personal, nonviolent and violent crimes over a given time period. We also collect information about a variety of other matters – the number of police in each of the cities; the number of crimes that are solved by an arrest; the number of criminal defendants who are found guilty; and the punishments they are sentenced to. We then compute our descriptive statistics for all these data. We know the numbers for each jurisdiction for each of, say, the last 10 years. We know the average number of

crimes per city per year, and we compute the variance and standard deviation for those crimes within each city over the last 10 years.

Those *descriptive* data can tell us a lot. For example, they can tell us whether crime has been constant over the last 10 years, going up, or going down. They might tell us that crime seems to becoming more serious or less violent. And a large number of other things.

Then we might use sophisticated analytical techniques to discover more. For example, we might be eager to see if stronger and longer punishment leads to (and perhaps *causes*) less crime. We might want to know if more police means less crime and vice versa.

There is one last background concept that I want to discuss at the beginning. This will be important in what comes later. I think that empirical work is an absolutely vital second step in any serious scholarly inquiry. But what is the first step? The absolutely vital first step is to do good theoretical work.[2] By this I mean that it is important to give a general causal account of or meaningful hypothesis about the legal phenomenon that you are discussing. To give an example, consider this hypothesis: Exposure to tort liability for negligently harming another person will lead most people, such as product manufacturers, to take more care than if they were not exposed to tort liability.

There are two implications of this hypothesis. First, if something causes there to be a reduction in the likelihood of an injurer's being held financially liable for harms their negligence causes, then potential injurers will, in the future, take less care. Second, if something causes there to be an *increase* in the likelihood of an injurer's being held financially liable for harms that their negligence has caused, then potential injurers will, in the future, take *more care*.

Those hypotheses seem to be straightforward, and I think that most of us who have studied law and economics find those hypotheses to be much more likely than not to be true.

However, and this is a central point of what I want to say to you, even if a theory or hypothesis seems obviously true, that does not mean that it *is* true. A logically consistent hypothesis could be perfectly wrong! And indeed the history of science – including economics and law and economics – provided plenty of examples of plausible hypotheses that are simply wrong. Consider the hypothesis that the Earth is at the center of our solar system and that the other planets and the Sun orbit around the Earth.

The principal corrective to make sure that we do not put too much of our faith in logically consistent and attractive hypotheses is to do empirical work. That consists of collecting data relevant to establishing the truth or falsity of our hypothesis and then confronting the hypothesis with those data to see whether the world agrees with or rejects our hypothesis.

[2] As I've already indicated, this is precisely the pattern that law and economics has followed: Theoretical work first; empirical work second.

This is particularly important in a field like the law where so much of what we study ultimately results in real actions – that is, in policies and pronouncements about what is right and wrong, what is acceptable and what is unacceptable. And these policies are frequently extremely important, such as whether some important societal values are well-served by taking a life through the death penalty.

That is to say this: We in the academy are not merely a debating society. We are a group of academics and practitioners whose theories have the most important consequences for our societies. We teach those who will guide society in the future. As a result, we have a duty to go beyond theory to provide our students and our societies with *evidence* that our theories are much more likely to be correct than incorrect.

I will argue that we have not – until very recently – fulfilled this duty to back up our legal theories with empirical evidence.

Let me turn now to my four central points.

First, I will argue that prior to the 1990s the academic study of law had almost no interest at all in empirical data about the law. Second, I will suggest that once the law did begin to take an interest in empirical data, that was due to the prior success of law and economics. And third, I will make some observations on some of the impediments to doing more empirical research on legal issues and suggest some modest steps that might make it easier to do that work in the future. Fourth, I'll make a proposal to make empirical legal studies better and more persuasive.

My first point is that prior to approximately 1990, very few practitioners and law professors ever mentioned that there ought to be more empirical work in law. Rather, the complaint was that if there needed to be more of anything from legal scholars, it was more and broader theorizing.

One of the most curious aspects of the pre-1980 period in legal scholarship was that there *was* some empirical legal research being done, but no one seemed to find it interesting.

Let me give you an example. In 1985, Dan Farber and John Matheson, then of the University of Minnesota Law School, published "Beyond Promissory Estoppel: Contract Law and the 'Invisible Handshake,'" 52 *U. Chi. L. Rev.* 903 (1985). The article concerned a principle of contract law called "promissory estoppel." That principle provided a justification for enforcing a contract in which someone promised to give a gift or other benefit – what is called a "donative promise" – to another party without receiving anything explicit back from the other party. (Under central principles of contract law in the common law countries only a "bargain promise" is enforceable. A "bargain promise" is one in which one party promises to do something for another party in exchange for that party's doing something in return. Gift or donative promises are not routinely enforceable.

At least that is what we have taught entering law students for decades and decades. And indeed, still teach them.)

Despite traditional doctrine against the enforceability of a donative promise, most courts in most countries found a means of making a promise to give a gift enforceable – through the doctrine of promissory estoppel. That doctrine held that if the party to whom the promise to give a gift believed the promisor to have been serious about giving a gift, that if believing that promisor was reasonable, and if the promisee incurred detrimental reliance expenditures, the promise became enforceable.

Farber and Matheson looked at every case they could find in the United States in the 10 years before their paper was published in which someone had asked for enforcement of a contract on the basis of promissory estoppel. Standard theory predicted that they would find a pattern of non-enforcement. Instead, what Farber and Matheson found was that in every one of those cases, the promisee was entitled to performance. Why? Because the courts thought that the promise was a serious one upon which a reasonable person would have relied. Surprisingly, the courts did not require actual detrimental reliance. Farber and Matheson said that they believed that this empirical evidence suggested that judges did not distinguish between bargain and gift promises – even though we taught then (*and still teach today*) that only bargain promises are routinely enforceable. But that is apparently *not* what courts in the U.S. are doing. They were prepared to enforce any promise made in furtherance of a legitimate economic goal.

What was the effect of this remarkable empirical finding? Nothing. The legal academy did not change anything at all. I find this astonishing. It is as if a medical study found that a particular medical procedure was completely ineffective but the medical profession continued to use the procedure.

So, to summarize: Prior to 1990 or so, not only was there very little empirical legal work but even when dramatic empirical legal scholarship appeared, no one paid attention.

My second central point is that all this changed around 1990. By that date, law and economics had become a central part of modern legal scholarship. And because law and economics began by bringing the precepts of economics into the study of law and because economics has for the last 80 years made a commitment to do empirical work regarding its theories, the rise of law and economics invariably meant the (eventual) rise of empirical legal studies. That is, the rise of empirical scholarship in the law – not just the work (that was done before) but having the legal profession pay attention to this work – is due to the rise of law and economics.

My third point is that there have been substantial impediments over the past 25 years to doing empirical legal research. The most obvious impediment is that the legal system in almost all countries does not collect data about itself that is well-suited to allowing researchers to describe and explain the extent to which our laws are working well or ill.

Early researchers were ingenious in finding relevant data in archives or creating data through surveys and experiments. But there is only so far that this creativity can take us. Now that a significant portion of the legal academy– if not practitioners, public policy decisionmakers, and the public – is eager to do and report on empirical studies of legal issues, we need to take steps to provide them with data.

The fourth and final point that I would like to make about evidence-based law is this: Because data about the law are not being collected regularly, we in the legal field can all do something to help. We can, for instance, urge the legal system – the legislature, agencies, the courts, private lawyers, and others – to begin to collect systematic data. Perhaps we can help to form a committee with public and private officials that can examine what data could be cheaply but routinely collected and stored in a publicly accessible form. The time to begin that project is yesterday, not today or tomorrow.[3] The sooner we all begin, the greater will be the thanks that future legal researchers, policy students, and everyday citizens will give us.

Thank you for your attention and thank you, Prof. and Dean Nagar, for the invitation to speak.

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[3] There is precedence for this development in the history of economics. When economics developed an interest in macroeconomics, including economic growth, in the 1940s and early 1950s, Simon Kuznets devoted considerable energy to getting the U.S. government to develop national income accounts. Those accounts are now so extensive that they serve as benchmarks of how society is doing with regard to many economic variables, such as growth and unemployment.