When I was invited to join the faculty in 1980, I came as soon as I could. I feared that all the interesting work in law and economics might be done before I got to Chicago. Among other things, this showed how little I understood law and economics. It concerns itself with how changes in the law change the way people behave. As long as legal scholars have to worry about the consequences that a new law brings, we shall call upon the tools of law and economics. This is not to say, however, that law and economics remains the same.

Three decades ago, law and economics was a rough-and-tumble discipline. People were still feeling their way. All presented their arguments with intense passion. Everyone fought for your soul.

Occasionally, you would go to a workshop and see the conventional wisdom in an entire area of the law overturned. But as often, you would see someone swinging for the fences and crash spectacularly. Sometimes an economist would start with an assumption that had the basic legal principle exactly backwards, or someone trained in law would get the economics completely wrong. Only five minutes into the 90-minute seminar, the error would be plain to everyone. Then an awkward silence. At this point, one of my colleagues would take a copy of the draft under discussion, throw it into the air, and say loudly, “Next paper, please!”

Work today is done with greater rigor, and seminars tend to be more civilized affairs. When revolutions succeed, they cease to be revolutions. The days when you could shoot from the hip and sometimes do great work (and more often fail) are gone. Law and economics today requires more discipline and better training.

But opportunities to do great work abound. The future of law and economics turns crucially on whether the next generation can take advantage of the resources available only now.

At its foundation, law and economics is an empirical discipline and always has been. As abstract as the paper might seem, Ronald Coase’s “Nature of the Firm” paper began as an empirical study. Coase saw himself as laying out the conclusions he reached after spending a year visiting the major production plants throughout the United States.
For a long time, however, the empirical tools in law and economics lagged far behind. It was commonly said that there were only two different types of empirical questions—those you could answer and those worth answering. The future of law and economics is bright in large part because this piece of conventional wisdom is no longer true.

Information is accessible in a way that it has never been before. The PACER system allows us to access every document filed in every federal case from our desktops. Google’s digitization project has put nearly everything ever printed at our fingertips. The Social Science Research Network provides everyone with access to everyone else’s work long before it is published.

Moreover, tools exist today to analyze data that simply have not existed before. Multivariate regressions that required weeks or months of computer programming can be done on every laptop in a few minutes. Statistical techniques are available now that can tease out a few kernels of wheat from an enormous amount of chaff.

Such tools can be abused. Data, if tortured long enough, can be made to say anything. But the biggest danger may lie not so much in getting the wrong answers, but in asking the wrong questions.

Law and economics faces the same challenge that the prospect of a comfortable middle age poses for the most successful. After an exuberant and rebellious youth, it is very easy to fall into a complacent middle age. It is too easy to think it enough to say something new and correct. You also have to worry that you are boring, mechanical, and tendentious.

To avoid this danger, the current generation of law and economics scholars needs to be careful not to rest on technical proficiency. It requires retaining the radical and unconventional spirit that has long been part of law and economics at Chicago. The bright future of law and economics lies in the bold questions that still have not been asked.

OMRI BEN-SHAHAR, Frank and Bernice J. Greenberg Professor of Law and the Kearney Director of the University of Chicago Institute for Law and Economics

The most prevailing view among those who predict the future of law and economics is that it will become more technical, more rigorous, and more mathematical. Just like its mother discipline, economics.

It is also a misguided view. It predicts, in other words, that law and economics will become less accessible to its core audience, lawyers and policy makers, and will probably lose its relevance to legal practice (and to most of legal academia).

Because so many people believe that this high-tech trend is the inevitable direction of law and economics, let’s briefly understand the logic and the evidence supporting this prediction. The logic is the law of decreasing marginal returns. Having exhausted the pool of basic legal issues that law and economics can illuminate, scholars in the field now need fancy machinery to reach the higher-hanging fruit. Simple intuition will no longer suffice to harvest new discoveries; state-of-the-art social science is necessary.

There is some evidence consistent with the high-tech trend. For a while, law and economics did become more technical and methodologically sophisticated. More people with PhDs in economics were hired to teach in law schools, and some of the leading journals have gravitated towards scholarship written in math, not in English. The economics discipline has become more rigorous and technical, and as the engine of law and economics, it has been pulling the field to the dizzying heights of advanced math and statistics.

While the high-tech trend has been part of the story, I think the future of law and economics lies in increasing its audiences, not its rigor. The field’s meteoric success since its early days in Chicago is a result of the broad appeal it had among those not formally trained in economics. True, sophisticated economists reinforced the foundations of the field by combing through the earlier discoveries and separating the wheat from the chaff. This growing corps of social scientists will continue to refine and make more credible our body of incrementally growing knowledge.
But the future of law and economics is in taking its mature discipline and stock of ideas and exporting them to new frontiers.

The most important new frontier and the greatest challenge to American law and economics is the crossing of international boundaries. Outside the United States, law and economics is a curious esotery, mostly shunned with distaste by the legal community. Major legal transformations and reforms are occurring in many regions around the world, largely lacking the realism and analytics that good old law and economics would fashion. Seasoned scholars and lawyers view law and economics with anachronistic resentment. Young legal minds are intrigued, but are only minimally exposed to the organized tools of the field. Law and economics is beginning to unfold the map of the world, and it is finding vast opportunities for intellectual arbitrage. Chicago—“the headquarter in this area,” as a Chinese colleague echoed the popular image around the world—is already at the forefront of this imperialistic enterprise.

The other big challenge that law and economics has to conquer is to descend from the sterile academic debate and be more successful in informing actual lawmaking and lawyering—in connecting with audiences that have so far remained outside its scope of influence. It is beginning to expand to areas of law that have largely resisted it. I am thinking, for example, of immigration law, education law, local government, and even areas of international and human rights law. Many areas of recent legal reform—health law, food regulation, consumer law, privacy—have major pockets of laws and rules that are ripe for more informed attention from law and economics.

Not that sophisticated tools are unnecessary. On the contrary, more methods and better methods are likely to emerge. We are witnessing a rise of experimental law and economics, of sophisticated behavioral analysis, and of course of a mature empirical methodology examining a plethora of legal topics. But law and economics—Chicago law-and-economics in particular—has maintained a stronghold on American legal academia for over 30 years by being relevant, accessible, and relentless in luring new audiences.

ANU BRADFORD, Assistant Professor of Law

Over the last two decades, rational choice methods have advanced our understanding on many key international legal issues, including why states make international law and what type of legal instruments they choose to use. Scholars have also been able to explain when and why states comply, or fail to comply, with international law, as well as the tensions inherent in the establishment of international institutions and their ability to constrain state behavior in a world of increasing integration and mutual reliance.

However, the shifting geopolitical landscape is changing how we think about and model these interactions. The most direct implication this will have for international law and economics scholarship is the change in the number and the identity of countries whose preferences matter in interactions. The geopolitical structure of the world has until recently lent itself to simple models that focus on strategic interactions involving a handful of few key actors—the United States, Europe, and, at times, the former Soviet Union or Japan—while generally ignoring the preferences of the rest of the “developing” economies.

Today’s international sphere features a greater multiplicity of relevant actors. Emerging powers such as China and the other BRIC countries are able to advance, increasingly successfully, a much broader and diverse set of preferences in international interactions. This emergence of these new players with standing in the debate forces us to revisit the basic assumptions about countries’ utility functions that underlie all economic analysis of law.

Our understanding of what is meant by “welfare” becomes more elusive. For example, how does China define its fundamental interests in the international order? How does it trade off pursuit of greater wealth and security with a uniquely Chinese desire for social stability, political control, population management, and certain redistributive policies? The utility functions of many emerging actors are less straightforward than those of traditional liberal market-based democracies, and incorporating these into models of
interaction requires more complex analytical frameworks.

This is also true for any public choice analysis. We may continue to assume that all governments seek to maximize their political welfare, but what this entails requires a careful examination of each relevant player’s political system. Thus, the two-level games that capture negotiations taking place simultaneously at the domestic and international levels will call for a more nuanced understanding of what kind of internal pressures different countries with vastly different constitutional systems face.

The heterogeneity characterizing the international system also entails that the pursuit of “grand theories” that can be generalized across countries and issue areas will yield less satisfying insights in the future. To capture the diversity of the strategic interactions, analytical frameworks will be tailored to specific countries and issues involved.

Further, the time and discounting in utility functions of international actors is becoming more difficult to manage. Governments have always struggled to balance their short- and long-term policy objectives, acknowledging the need to temper growth policies with measures that price in the long-term costs and externalities. This intertemporal tension is becoming more acute in issues ranging from aging workforces in both China and the West to energy policy and climate change everywhere. Incorporating these tensions into countries’ preference functions will be vital to understanding optimal legal frameworks.

The increasingly divergent interests among key actors will also raise new questions on how to accomplish mutually beneficial cooperation. Transfer payments will need to evolve to overcome complex collective action problems crucial to global welfare and security. Threats to global order will be more diverse and unpredictable. Economic protectionism will become more subtle and harder to detect under existing WTO frameworks. Multilateral cooperation will become increasingly difficult to achieve as Pareto efficiency will often be unobtainable and much of the bargaining will take place in the second-best world. All of these issues will lead scholars to address more intricate questions on how to design international institutions that can facilitate cooperation in situations where mutual gains may not exist and traditional transfer payment options have been exhausted.

These changes also make the limits of law and economics more pronounced. We are learning that good analytics cannot tell us how states should trade off various goals, but should be used to understand how to optimize across various legal strategies and instruments under various alternative definitions of what welfare maximization entails. Most valuable research will generate and evaluate alternative outcomes based on different possible combinations of preferences, strategic choices, and constraints, exposing the costs and benefits underlying each of these outcomes.

At the heart of these shifts lies the fundamental modeling challenge of balancing complexity and simplicity. The complexity of reality requires a more nuanced understanding of how states form preferences and what drives their behavior. This provides an avenue for richer and deeper, albeit inevitably less certain, insights. At the same time, generating meaningful insights out of the messy reality requires simplicity. Unearthing the very essential of the strategic situation is more important than ever to advance our understanding of the most multifaceted problems of international cooperation. Embracing this tension in a fast-changing global landscape will make international law and economics scholarship an increasingly challenging and, consequently, exciting field for scholars to be working on in the future.

ERIC A. POSNER, Kirkland & Ellis Professor of Law and Aaron Director Research Scholar

The most distinctive and also troubling trend is the division of law and economics into two subdisciplines—an “economics law and economics” and a “law law and economics.” ELE (as I will call it) will be mathematical and descriptive in orientation. LLE will be verbal and normative in orientation. ELE will be practiced by economists and
Law and economics started out as a collaboration between law professors, who supplied the legal knowledge, and economists, who supplied the economic concepts and the mathematical apparatus. Since then, economic ideas have spread through the law schools (some law professors have PhDs or other training), and economists interested in the law now have easier access to legal materials and a law and economics literature to draw on. Because the two groups depend less on each other for each other’s distinctive expertise, they have less reason to collaborate. Isolated in their subcommunities, their methods, jargons, and orientations will drift apart. Those doing ELE in economics departments will find themselves drawn to the questions and methods that economists in other fields use, while those doing LLE in law schools will find themselves drawn to the questions and methods that other law professors use. And so ELE will become increasingly mathematical and empirical, while LLE will become increasingly normative and doctrinal.

This divergence is already evident. To take one of many examples, economists who study contracts are doing something different from law professors who study contract law. Economists take contract law as a given and analyze how rational agents would design optimal contracts. Lawyers focus on how to design optimal contract law, not contracts. The two groups are aware of each other, but they exert less and less influence over each other.

The divergence is also apparent in certain institutional developments. Law and economics seminars are well established in the top law schools, but in recent years some law professors at those schools have peeled off, forming seminars devoted to more mathematical (ELE) law and economics scholarship. The American Law and Economics Association has become increasingly divided between ELE and LLE factions. There is no real hostility between the factions, to be sure, but LLE types have begun dropping out of the annual meeting as ELE types, who enjoy an advantage in numbers, increasingly take over.

This sort of specialization is inevitable in academic scholarship. It is troubling because both fields will suffer. But it may also portend a reintegration of law and economics (that is, LLE) with other fields in legal scholarship, notably public law, where until recently law and economics has made limited inroads. Today, economic thinking dominates contract, commercial, bankruptcy, antitrust, corporate, and securities law and related fields. It is also influential if not dominant in tort, criminal, and property law and civil procedure. It has made less progress in the major fields of public law, including constitutional, immigration, administrative, and international law. These areas of law are less closely connected with commercial behavior than most of the others, and so the off-the-shelf economic models do not as clearly apply to them. Economists have produced a large political economy literature, but the models in this literature are more controversial and less usable than models of commercial behavior. The main problem is that the models are pitched at the wrong scale—analyzing, for example, the differences between democracy and dictatorship, or parliamentary democracy and presidential democracy, but not the costs and benefits of the legislative veto or the preemption doctrine.

But this is changing. In the last few years, a new generation of law and economics (mostly LLE) scholarship has focused on these fields. Scholars see international law as the product of interaction among self-interested states. They analyze administrative law on the basis of agency models that emphasize the divergence of interest between the principal (such as the president) and the agent (such as the bureaucracy). Constitutional law can also be understood using agency models where the “people” are the principal and the government is the agent. Immigration law can be understood using screening models from the economic literature on labor markets.
In the short to medium term, there will be increasing methodological divergence even as the use of economic ideas spreads to the farthest reaches of the law. How these forces will play themselves out in the long term is beyond the ken of my crystal ball.

SAUL LEVMORE, William B. Graham Distinguished Service Professor of Law

Twenty-five years ago, as an inexperienced faculty member, I was astounded to hear the leading law and economics scholar at Harvard assert that within a generation the entire faculty of every major law school would hold PhDs in economics. The prediction seemed (and was) outrageous, self-centered, and misguided. Movements in legal education and scholarship produce countervailing forces. More economics begets more philosophy; more interdisciplinary offerings generate practical legal clinics; more clinical education generates more theory; and more theory brings about more courses in business skills. The driving forces behind these developments include the rewards in the academy for novel, or “cutting-edge,” work, reactions from the bar and donors, and the very nature of academic work.

Similar forces operate within law and economics. The current explosion in empirical work, which is hard to overstate, will bring about its own reduction and renewed interest in modeling or in positive theorizing. (I note that these approaches are hardly dormant. A recent symposium on liquidated damages, for instance, was dominated by work that tried to “explain” cases with economic insights. This kind of work has been the bread and butter of law and economics since its inception.) In the course of the next two decades empiricists will surely expand their domain, favoring other empiricists in the hiring process for example, but eventually the forces already mentioned will take hold. There are other reasons to believe that empirical work will not completely dominate. It is more removed than other forms of law and economics from the practice of law; it creates a large divide—as great as that once observed with regard to critical legal studies—between what faculty members wish to write about and what needs to be taught; and, perhaps most important, there are signs that the judges who have been most interested in citing empirical work are being replaced with, or bolstered by, like-minded judges of similar influence.

Empirical work is likely less valuable in law than in medicine or other disciplines. Results are sensitive to context, and empirical findings in one year are often unlikely to hold true in later years. Contexts change because of new laws, demographic changes, education, and a host of other factors. An optimist would say that this explains why volumes of empirical work about important legal subjects do not seem to change hearts and minds. I am referring here to work on gun control and to work on the deterrent effect of long criminal sentences, as well as the death penalty. (There are counterexamples; important empirical work changed minds in corporate law’s race-to-the-bottom debate.) In contrast, though I concede that it is difficult to know what would make for a fair comparison, empirical work in public health regarding tobacco consumption has had a profound effect on beliefs, laws, and everyday behavior—so it is not as if strongly held views cannot be changed by data.

I turn next to two affirmative predictions about the future of law and economics; one pertains to scholarship and the other focuses on legal education. As we globalize, law and economics will turn with enthusiasm to comparative law. Economists are as mesmerized by the rise of China as anyone. They will turn their attention to the reality of remarkable economic growth in the presence of an unfamiliar conception of eminent domain, a near absence of fee simple ownership of real property, and a very different view of the so-called rule of law. Superficially, modern China is a puzzle to conventional law and economics, but economists love puzzles.

Meanwhile, in our law schools, and especially in the elite schools, law and economics will continue to grow in importance, despite the observations about countervailing forces with which I began. This is because we now have a generation of teachers and students familiar with the toolkit of this interdisciplinary field. Law and economics is now mainstream. When there are ten or more faculty members who think, teach, and write in law and economics terms, as there are now at Chicago, Yale, Harvard, NYU, Penn, and
Second, theoretical work must be better connected to empirical work. Much of the law and economics scholarship in the first few decades (1965–1995) of the field was theoretical. That can be explained by the lack of technology (computers, storage) with which to conveniently conduct empirical research. In the last decade or so, we have seen an explosion of empirical work in law and economics. Unfortunately it is often unconnected to, or only loosely motivated by, theory. Going forward, theory must focus on generating practically testable predictions, and theory must test these predictions to estimate structural parameters from well-defined theoretical models. This process will ensure that the field stays disciplined and keeps making progress.

On the empirical side, law and economics suffers two problems. First, it is narrow. Too often the focus is merely showing that a legal rule affects some outcome, e.g., a three strikes law affects felonies; personal bankruptcy exemptions affect interest rates; tort reforms increase physician labor supply. Insufficient attention is paid to translating that outcome to welfare. What are the costs of enforcing a three strikes law? Do exemptions have some insurance benefits to be balanced against their effect on interest rates? Does equilibrium physician supply even affect consumer or producer surplus? This can partly be remedied by better connecting empirical work to theory.

ANUP MALANI, Lee and Brena Freeman Professor of Law

The future of law and economics is no different than the future of other applied microeconomics fields such as labor, health, and public economics: better identified empirical work with a solid connection to economic theory.

The questions law and economics asks are of course different than other fields. Our focus is on how and how much legal rules and process (whether imposed by courts or legislatures) affect welfare. Do legal rules affect welfare by controlling transactions costs (think optimal default rules)? Or do they do so simply by eliminating uncertainty and facilitating Coasian trade? Do they produce public goods such as information or take advantage of economies of scale such as with the substitution of police protection for self-protection of property? Or do they do so by redistributing income to those with higher marginal utilities? But the theory we employ to model this behavior has much in common with other applied microeconomics fields.

The theoretical challenge going forward is twofold. First, the field must keep pace with advances in modeling from other fields. This includes advances in game theory (including contract theory and mechanism design), general equilibrium models, and assorted other smaller moves from fields such as industrial organization and finance.

Second, empirical law and economics lags behind (as do other fields) labor economics in the skill with which it demonstrates causal connections between legal rules and outcomes. The big problem here is that legal rules are not randomly assigned to populations. They are endogenous, e.g., high crime states tend to pass stricter criminal penalties, states that value insurance generally pass high exemptions, and states faced with physician flight pass tort reforms.

other schools, students cannot avoid learning such basic things as present value, cost-benefit analysis, moral hazards, and simple game theory. In turn, these concepts come into play in most classes in the curriculum, rather than in the occasional antitrust or other class where law and economics seems “natural” and might once have seemed properly isolated. Law and economics is now ubiquitous, and every faculty member learns to incorporate its central concepts by virtue of his or her own education, attendance at faculty workshops, and—at Chicago—immersion in roundtable lunches. (At Chicago, unsurprisingly, every faculty member recognizes the basic law and economics moves, and everyone can ask terrific questions at law and economics workshops, for example. That is not true elsewhere.) The same is true for our students; their facility with the language and ideas of economics has grown considerably over the last decade or two.

Law and economics once seemed rebellious. In the leading law schools, that is certainly no longer the case. In the coming twenty years, we will learn whether the movement and methodology is dominated by empirical work, in which case its primary audience may be regulators, or whether it becomes yet more relevant to judges and to practicing lawyers.

ANUP MALANI, Lee and Brena Freeman Professor of Law

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The theoretical challenge going forward is twofold. First, the field must keep pace with advances in modeling from other fields. This includes advances in game theory (including contract theory and mechanism design), general equilibrium models, and assorted other smaller moves from fields such as industrial organization and finance.
This means that simple correlations between a specific law and outcomes do not imply either that the law caused those outcomes in jurisdictions that already have the law or that the law would similarly cause such outcomes if other jurisdictions adopted that law.

The usual solutions to nonrandom assignment are either to model selection of laws and demonstrate that causal relationship can still be identified or to find instrumental variables (IVs) that causes changes in the law but are otherwise unrelated to the outcome in question. Although we have seen few papers that model both adoption of laws and the effects of those laws, we have seen some neat examples of IVs in use. Hornbeck (2010) uses a technological advance—the invention of barbed wire—to test for the effects of enforceability of property rights on investment and development. Libecap and Lueck (2011) use the allocation of parts of Virginia to Ohio during the US Civil War to study the effects of different methods of drawing property lines (rectangular versus metes and bounds) on economic development.

Even when a legal change is orthogonal to the outcome being studied, other problems frequently arise. One is that the legal change was itself caused by some other legal or nonlegal change that is truly responsible for the observed change in outcome, e.g., a state may see a decline in crime after adoption of a truth-in-sentencing law, but the real cause of the change in crime is a move to a more law-and-order political culture that led to both the specific law studied and a more aggressive prosecutorial office. Another problem is that laws are frequently anticipated, especially in open, democratic societies that debate laws before adoption. In this case, a simple comparison of outcomes just before and just after a law is passed may underestimate the effect of the law, e.g., doctors may decide to retire at a lower rate in a state that is likely to adopt a damages cap in future years. Malani and Reif (2011) extend some techniques from macroeconometrics and empirical finance to show how the anticipation problem can be tackled with panel data.

It is essential that law and economics continue to make advances on the theoretical and empirical fronts I have laid out. They are necessary for the field to attract the brightest and talented new researchers and to remain normatively relevant. I am confident, however, that the faculty in residence at the University of Chicago—including the founding generation of Gary Becker, Bill Landes, and Richard Posner, current leaders of the field such as Saul Levmore and Eric Posner, and a new generation that includes Tom Miles and William Hubbard—and the faculty from other schools who have either trained or developed here (a list that includes such stars as Alan Sykes, Richard Craswell, Richard Brooks, Mark Ramseyer, and Stephen Choi) is equal to the task.

References


1 It is questionable whether the last two questions belong to the domain of law and economics. Certainly scholars working in law and economics have tackled these questions, but an argument can be made that law and economics should focus either on reduction of uncertainty and transactions costs or any value from procedure. The creation of public goods or capturing economies of scale belong to either generic applied microeconomics or defined fields such as public economics.

2 By this I mean instruments for legal change, not legal changes as instruments for nonlegal changes. A great example of the latter is Levitt’s use of prison overcrowding litigation as an instrument for the reduction of prison size.

3 There is also some scope for use of regression discontinuity designs at the borders of jurisdictions with different legal rules. Although communities across the border are exposed to different legal rules, the effect of any one rule can sometimes be identified by looking before and after a change in one particular rule on one side of the border.

THOMAS J. MILES, Professor of Law and an editor of the Journal of Legal Studies

The University of Chicago economist and Nobel laureate George Stigler famously said that the division of labor is limited by the extent of the market. In the “marketplace” of legal scholarship, law and economics has expanded vigorously and continuously since its emergence as a scholarly field in the 1970s. If this growth continues, Stigler’s aphorism suggests that in the future “labor” in law and economics will become more divided. That is, scholarship in law and economics will become more specialized.

But, will law and economics continue to grow? Or, has it reached a mature phase of stability and perhaps retreat? Both the supply and demand sides of the academic market portend continued growth of law and economics. On the supply side, law itself continues to expand its reach and complexity, presenting new questions requiring scholarly analysis. Many legal changes, such as the new financial regulations, seem naturally suited to economic analysis because they involve markets. Other subjects not involving
explicit markets appear at first blush to be ill matched to economic analysis, but precisely because an economic perspective is novel, there are opportunities to make intellectual contributions.

On the demand side, a steady flow of new legal scholars is eager to employ the tools of economic analysis. Some of these scholars are PhD-trained economists who see law schools as an intellectual home because many economics departments have increasingly turned toward abstract theory and away from a nuanced study of institutions. A new cadre of political science PhDs is applying rational choice analysis (which is the essence of the economic approach) to topics in public law that until now have largely escaped the attention of law and economics scholars. For some young professors with more standard backgrounds in law (a JD, then clerkship and a stint in practice), economics is a preferred mode of analysis, and for others, many economic concepts are now a standard part of the legal academic’s analytical toolkit.

As law and economics continues to grow, it will become more specialized, according to the Stiglerian view. When a market is small, a producer must be a jack-of-all-trades, but when it is large, a narrow focus can earn high returns. Also, the acquisition of knowledge incurs a fixed cost, and once acquired, it is efficient to utilize the knowledge as much as possible. This implies that future scholars of law and economics are less likely to be generalists who hopscotch across legal fields applying economics with a broad brush. Instead, they are likely to focus on a limited number of related legal fields, say corporate and securities, and to use economics to understand their legal and institutional intricacies.

The methodological specialization occurring in economics departments reinforces this trend. With few exceptions, graduate students choose relatively early to become theorists (who write formal mathematical models of economic behavior) or empiricists (who test economic predictions against data). Just as PhDs in economics departments specialize in one of these methodologies, so too PhD/JDs in law and economics increasingly devote themselves to a single methodology.

The first decades of law and economics illustrate this pattern. Theoretical contributions dominated the first generation of law and economics. Early theoretical models typically showed how under full information, rational decision making subject to resource constraints could yield efficient outcomes. The next wave of scholarship demonstrated how the introduction of a friction or market failure could qualify this conclusion. With these foundations in place, today’s theorists face a harder challenge. To make a contribution, they must explore the interaction of multiple frictions, increasing the complexity and sophistication of their mathematical models. Expertise has become a necessity.

In the past decade, empirical scholarship in law and economics has surged. With a maturation of theory, evidence confirming or refuting the theoretical predictions was needed. A technological shock also spurred empiricism. Innovations in computing and the rise of the Internet have dramatically lowered the cost of assembling large datasets and conducting statistical analyses. By its nature, empirical work is already relatively specialized, and it is likely to compose a greater share of law and economics scholarship in the future.

For many of us connected to the University of Chicago Law School, the prospect of scholarship becoming ever narrower and deeper is troubling. A great feature of the Law School has historically been its peripatetic intellectualism. More so than in other schools, our faculty teach and write in multiple legal fields, and this has been especially true of our law and economics faculty. The trend toward specialization seemingly presents a risk that single-minded hedgehogs burrowed in their own specialties will replace the nimble and wide-ranging foxes of our faculty. Is the narrowing of law and economics scholarship unstoppable?
Perhaps not. Two other University of Chicago economists, Gary S. Becker (also a Nobel laureate) and Kevin M. Murphy, identified in a 1992 article an important counterweight to specialization: coordination costs. Coordination, the task of combining specialized knowledge, becomes more costly (which is to say, more valuable) as the number of specialties rises. Professors Becker and Murphy presciently noted in 1992, “Economists and lawyers working on the relation between law and economics can coordinate their research, but coordination costs are reduced when economists also become lawyers or lawyers also become economists, as with the increasing number of persons who take advanced degrees in both law and economics.” The increase in PhD-JDs that professors Becker and Murphy predicted has occurred, and Chicago itself has produced a fair number of these new academics.

But, as the richness of law and economics scholarship grows, even a person possessing a PhD and a JD may lack the expertise needed to make a contribution. In Professors Becker and Murphy's words, “limited human capacities tend to make it harder to pack more knowledge into a person without running into diminishing returns.” A solution is to collaborate with another scholar. In a recent study, my colleague Professor Tom Ginsburg and I found that articles containing technical models or empirical studies have in the past twenty years come to compose nearly all of the articles appearing in The Journal of Legal Studies—a marquee journal in law and economics that the Law School has published since 1972 and a bellwether of scholarship in the field. We also found that these articles were far more likely to be coauthored rather than single-authored. These trends suggest that the need for collaboration will prevent law and economics scholars, including those at Chicago, from laboring in isolation in their particular bailiwicks and will prompt them to immerse themselves in the ideas and work of their colleagues. More growth, more specialization, and more collaboration will mark the future of law and economics at Chicago.

David A. Weisbach, Walter J. Blum Professor of Law and Senior Fellow, the Computation Institute of the University of Chicago and Argonne National Laboratory

It is perilous to predict the future. Twenty years from now, perhaps at my retirement party, we can pull out this essay and laugh at how ridiculous my predictions were. Worse, the person who gets it right will be celebrated as visionary even if their predictions were right purely by chance. I might as well buy a lottery ticket—if I lose, well, most predictions are wrong anyway, and if I win, I'll claim it was vision and not luck.

Looking back 20 years, law and economics looked much as it does today. Today we have more economics PhDs, particularly scholars with joint degrees. The field is more empirical and the empirics are more sophisticated. It covers more areas of legal scholarship. But someone time traveling from 20 years ago into a law school today would not notice a lot of difference in the type of work being done.

The easiest prediction, then, is that the trends will continue. We will see more integration with economics departments, more professionalization of the field, better econometric techniques, and expansion into new areas and new legal problems. But things will pretty much continue as they are.

That is my safe bet, and it would not be a bad future. Let me venture out onto a limb, or more likely a twig, and say where I would like things to go and, perhaps being optimistic, where things will go. Law and economics developed as the study of the traditional first-year law school issues of torts, contracts, property, criminal law, and procedure. These are the first-year law school courses because they are the building blocks of other areas of law. It was a smart place to start.

The central problems facing society today, however, go well beyond these building blocks. They are highly complex, structural problems, and knowledge of the building blocks will not be sufficient for addressing them. If we were to list some of the central problems or areas of law facing us today, we would list areas like banking and finance, poverty and inequality, development, education, health care, and energy and the environment.

Studying these fields requires a somewhat different set of tools than most law and economics scholars are currently equipped with. Scholars need a deep understanding of the building blocks, but they also need to understand the institutional, economic, statutory, and political structures of these problems. Because of their complexity, the techniques we use to study them might be different. We
will far more likely to have to work with experts in these fields. Coauthorship with people from other parts of campus may need to become the norm. Models will have to be more complex. Data requirements will be greater. The problems involve less law, in the sense of what courts do, and more policy, in the sense that statutes and

legislatures will be central. There are, of course, people currently working in these areas equipped with all of these tools, but their numbers are limited. If law schools want to contribute to the great problems of the day, scholarship will have to move in this direction.

Let me illustrate with current a project of mine that maybe indicates this trend. (Perhaps this indicates that I can’t see beyond the tip of my nose, because this is my current work. What I’m working on is, of course, indicative of future trends . . . ) The question I wished to address involves climate change: there is strong international pressure for developed countries to start reducing emissions prior to any commitment from developing countries. Developed countries worry, however, that doing so will simply cause energy-intensive production to move abroad. We want to know the parameters of this issue—does it make sense for the developed countries to act alone, and what sort of legal rules might limit the bad side effects?

This problem cannot be understood with conventional techniques. Hard thinking and analytic models can give a sense of the direction but not the size or scope of the problem. Standard econometric techniques are not helpful because the predictions involve situations far from our experience. The solution we (my coauthors and I) turned to was large-scale computation. We simulate the problem with a computational model that allows us to run experiments with different policies to see their effects. It is necessarily interdisciplinary; I have coauthors from a variety of university departments. We are using computers at Argonne National Laboratories to run the model.

There are many criticisms and problems. Computation is not common in economics, not to speak of law and economics. Computational models are hard to understand. The data are uncertain. Results can depend on the model structure, which is driven in part by the modeler’s choices rather than empirics. Subtle and nonobvious changes to the model, such as particular solution algorithms, can change outcomes. There are also solutions. To avoid creating a black box, we use an analytic model to develop economic intuitions that are then tested in the big computer model. The code is open source so that anyone may run it; we also are making simplified versions of the model and code available to help users understand the modeling approach. We use the model to replicate prior studies so that differences in our results and other studies can be understood. To address uncertainty, we engage in a variety of robustness checks, including but not limited to studies of the sensitivity of the results to parameter and model-structure variation.

It is a very different view of legal analysis—it views problems as engineering problems that we model and test. It is empirical, practical, and solution-driven. The role of the legal scholar is to help frame problems, to think about how institutional structures affect the framing, to suggest solutions, and to help interpret and evaluate results.

I don't think computation will become mainstream, although I hope it becomes more common. But interdisciplinary scholarship of this sort, where law and economics scholars work with experts in related fields to think about and devise solutions for the most important problems we face, is one possible future, and one I hope we move toward. It would require a huge shift in the type of things legal scholars do and are able to do. But to address big, structural problems, there is no choice. So there is my lottery ticket, although I would still take the safe bet.
The evolution of law and economics has been shaped by a number of forces: the increased mathematization of economics (including advances in techniques of statistical analysis); the increased availability of statistical data usable in empirical analyses utilizing the latest statistical techniques, as a result of the computer revolution; the broadening of the scope of economics both conceptually (as in the rise of game theory and the advent of behavioral economics—the invasion of economics by psychology) and in the areas of human activity that are studied by economists (marriage and divorce, for example); the increased size and “academification” of the legal professoriat; and, related to a number of these developments, increased specialization of academic law.

The early contributors to the field of law and economics were economists and lawyers—not lawyer-economists—and they tended to write across legal domains. So Becker, for example, studied both racial discrimination and criminal law enforcement, and Coase both tort law and communications regulation, and Baxter both patent law and environmental law. Very little of the work of this early period was either mathematical or statistical (or empirical at all). But beginning in the 1970s economists such as Steven Shavell built increasingly sophisticated mathematical models of legal phenomena. It began to be felt that legal training alone would not enable a lawyer to do sophisticated economic analysis of law, and so economic analysis of law increasingly became the province of law professors who had a PhD in economics, as well as of economists specializing in the application of economics to law who did not have a law degree.

During the 1970s, economic analysis of law began to permeate legal teaching as well as scholarship, and economic
considers and expert witnesses became fixtures of commercial litigation in a variety of fields—in part because lawyers were learning in law school how economics could be used in legal analysis. Most of these consultants and witnesses were not and are not economic analysts of law, but rather analysts of business practices challenged in litigation.

The trend toward increased economic sophistication in the 1970s, which has continued ever since, has had a side effect of increasing the separation between academic economic analysis of law and the practice of law. The two-degree scholars generally don’t have time to engage in law practice to any significant extent (often no more than a one-year clerkship with a judge) before beginning their academic careers. The increased formalization of economics makes it difficult for lawyers who do not have training in economics to collaborate with economists or lawyer-economists. Increasingly, economic analysts of law write for each other, in specialized journals, rather than for the larger profession. Increasingly, indeed, they write not for economic analysts of law as a whole but for economic analysts of the writer’s subspecialty. The expansion of a field leads to the multiplication of its subspecialties.

These developments have increased, and will continue to increase, the rigor of economic analysis of law. The search for new worlds to conquer that is a hallmark of a progressive research program has already paid off. One example is increased attention to the economics of foreign and international law and, concomitantly, increased exploitation of the opportunities that cross-country comparison provides for empirical study. Another example is the empirical study of judicial behavior, where insights from labor economics and the economics of organizations are being used to interpret the large quantities of statistical data that are available (or readily obtainable) concerning the activities of courts and judges.

But the gap between academic law and economics and the law as it is practiced and administered and created and applied is troublesome. Economic analysis of law has intrinsic intellectual interest (like jurisprudence) and is an invaluable component of a modern legal education, but one would also like to see it contribute to the solution of legal problems and the reform of our costly and cumbersome legal institutions. And for that the economic analyst needs to understand law from the inside, which no one, however bright, can do without legal experience (though it might be acquired, on the side as it were, after one had begun an academic career) as well as legal training, for law is like a foreign language. And to avoid the amateurishness of underspecialization, there is a pressing need for greater collaboration between law professors with real legal experience and economists or lawyer-economists who have the analytic tools but not the insider’s understanding of the law in action and in its manifold institutional forms.

There is also need for economic analysts of law, whether they are lawyers or economists or lawyer-economists, to interact with, and sometimes collaborate with, economists in economic departments and business schools who may be interested in law and may have special economic skills to bring to its study, an example being Andrei Shleifer of the Harvard economics department.

The limited amount of such interaction and collaboration is reflected in the slow reaction of economic analysts of law to the financial crash of September 2008 and the ensuing downward spiral of the economy. The legal profession was deeply involved in the creation of the complex financial instruments that crashed and, of course, in the creation, un-creation, and administration of the regulatory laws and institutions governing finance. Yet about these instruments and practices and regulations—the Federal Reserve Act, for example, or the debt ceiling, or the eurozone—economic analysts of law have been largely silent (though not entirely—think of Lucian Bebchuk at Harvard Law School and the finance group at Columbia Law School), even though the economic crisis is about to enter its fourth year (fifth, if we count from December 2007, when GDP first began to dip).

Predictions of the future are almost always just extrapolations. I will conclude in that vein. I expect economic analysis of law to grow in rigor, expand in scope, and becoming increasingly empirical as statistical databases become easier to create and analyze. Up to now the ratio of theoretical to empirical economic analysis of law has been very high, in part because theoretical papers can be produced much more quickly than empirical studies, and (unfortunately) number of papers published is given undue weight in hiring and tenure decisions. That is a concern and another (which is related however to reluctance to undertake empirical studies) is that economic analysis of law may lose influence by becoming too esoteric, too narrow, too hermetic, too out of touch with the practices and institutions that it studies.

**BECKER:** Posner gives an excellent discussion of the evolution of the field of law and economics, with the glaring omission of his own monumental contributions that fundamentally helped define the approaches, techniques, and scope of this field. I will go over some of the same ground as he
The first stage of research on law and economics was mainly theoretical. Economists and lawyers used and adapted concepts and analysis from economics to show that legal rules and doctrines often had a clear economic rationale, and to show how these concepts illuminated how laws and legal systems affect behavior and the efficiency of economic outcomes. These early studies had an enormous influence on how some lawyers and economists began to think about property, contracts, negotiations, trials and settlements, torts, antitrust, corporations and securities, crime, racial and gender discrimination, and other areas of the law. Yet it took a while for these ideas to spread into law schools since academic and other lawyers initially had little exposure to the economic way of thinking. Partly for this reason, considerable opposition developed to many of the ideas espoused by the economists and other pioneers in law and economics. Gradually, however, opposition weakened (although it has not disappeared) as newer generations of lawyers became better qualified to appreciate and evaluate the contributions of this new field.

At the same time, the gain from mainly arbitraging theoretical ideas from economics into the field of law began to lose steam. This was in good part because the early contributions were mainly theoretical, with only occasional support from legal cases, and with still less frequent support from quantitative analysis. Theory alone cannot keep a field vibrant, although it can substantially shift the approaches to different issues.

No field that deals with human behavior has ever remained exciting and innovative by relying on theoretical ideas alone, no matter how valuable these ideas are. A vibrant and creative field requires a continual dialogue between theory and evidence from the real world that not only helps test existing theories, but also suggests new theories that can then also be tested and extended, or rejected.

Fortunately, as the first theoretical stage was slowing down, perhaps because opportunities to arbitrage economic ideas into law were shrinking, a second stage began that collected and analyzed quantitative data. This quantitative approach uses statistical techniques also drawn mainly from economics to analyze antitrust cases, contracts, litigation, intellectual property, divorce, crime, discrimination, and many other areas of the law. Quantitative analysis has become one of the most exciting frontiers in law and economics, since extensive legal data exists, often in rudimentary forms. These data can test, discard, or help in the reformulation of the theories on the effects of laws and legal rulings on behavior.

Most participants in any field, including law and economics, specialize. Some are mainly theorists, while others are mainly empiricists. For a field to remain relevant, however, many researchers must be both, relating theories to real world data. Otherwise, theories become sterile as theorists mainly discuss what other theorists said, and empiricists become mainly number crunchers, with little effort to interpret the data in other than ad hoc ways.

This is why I believe an exciting further development in law and economics will involve extensive interactions between theory and empirical analysis. Some lawyers will cooperate with economists, but even then it would be valuable for the lawyers to acquire not only the rudiments of economic theory, but also basic econometric and other techniques for analyzing data. Economists involved in this research should also acquire some knowledge of legal opinions and how legal systems operate. Indeed, a growing number of individuals with both a law degree and a PhD in economics are beginning to bridge this gap.

The great majority of research in law and economics has been at the “micro” level in the sense of considering the behavior of parties to contracts, torts, crime, and other individual and business behavior. This research has been fundamental, but a newer and also important research focus considers the interactions between legal systems and the macro economy. This research, pioneered by economists Daron Acemoglu and Andrei Shleifer, among others, analyzes the connections between legal systems and long-term rates of growth, the degree of economic inequality, aggregate investments, and other macroeconomic variables. To a lesser extent, this burgeoning literature also analyzes how macroeconomic developments affect the evolution of legal systems.

Scarcity of data often limits how much can be achieved empirically in understanding the macro interactions between laws and economics, although the creation of long time series for many countries on relevant legal and economic variables is widening the database. I expect the macro interaction between law and economics to become another major frontier as the discipline of law and economics pushes its boundaries and insights into uncharted territories.

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1 Becker thanks William Landes and William Hubbard for helpful comments; Posner thanks Landes for helpful comments.