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The Future of Law and Economics: A Comment on Ellickson

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THE FUTURE OF LAW AND ECONOMICS: A COMMENT ON ELLICKSON

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Very well, I shall speak[1]—briefly—to Professor Ellickson's two theses. The first is that the "market share" of law and economics in the legal-education market has stopped growing, partly because the potential applications of economics to law that are accessible to persons who have limited formal training in economics have been made, partly (a related point) because the potential of the neoclassical model of economics to illuminate legal phenomena is nearing exhaustion. The second thesis is that the upward growth of law and economics will resume if economic analysts of law enrich their models with insights from other social sciences, such as psychology and sociology. I shall discuss each thesis in turn.

I.

Ellickson does not argue that the number and the scholarly output of law professors who do economic analysis of law have stopped growing, and I would be surprised if they had. He argues, with some empirical support, that the fraction of legal teaching and scholarship that is economic in character has stopped growing. This judgment may be premature. The evidence that Ellickson is able to marshal for it is not powerful, especially given his decision to confine his search for evidence to four "elite" law schools that, if the truth be told, are increasingly difficult to distinguish from a number of other fine law schools. But assuming that his judgment is correct, I would stress four reasons different from his as to why it is correct. The first is that the relative as distinct from the absolute growth of one component of legal education *must* eventually slow, and then stop. This is a matter of arithmetic. Economic analysis can never constitute more than 100 percent of legal teaching and scholarship, and therefore it cannot grow indefinitely relative to other fields of academic law. Second and related, recent years have seen the

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1. The last paragraph of Professor Ellickson's article, on which I have been asked to comment, reads in its entirety, "Speak, Posner." Ellickson, *Bringing Culture and Human Frailty to Rational Actors: A Critique of Classical Law and Economics*, 65 CHI.-KENT L. REV. 23, 55 (1988) (Professor Ellickson's article appears in this symposium issue).

growth of other interdisciplinary fields of legal studies, such as feminist jurisprudence, law and literature, and critical legal studies; the latter two fields are regarded in some quarters as reactions to law and economics.[2] Room had to be made for these new fields in the curriculum; since the conventional curriculum could give only so far, law and economics may have had to yield some ground to the newcomer fields. Third, law and economics is a victim of its success. Many of its practitioners have achieved a measure of prominence, have experienced enhanced demand for their services, and have been drawn off into other activities—consulting, practice, judging—which have diminished their academic productivity, in some cases to zero. Its acolytes have broader opportunities than other young lawyers. A law school graduate with a command of economics has enormous earnings potential in the private practice of law, and law schools are reluctant to pay the level of salary necessary to attract these graduates into teaching. It is cheaper to fill teaching slots with refugees from the humanities, alumni of public-interest firms, and aspiring constitutional lawyers.

The fourth reason for a possible slowing in the growth of law and economics is the most important. A social science cannot progress far without empirical research to test its theories. This is true of economic analysis of law. It is challenging and fascinating to build models of dispute settlement, optimal deterrence, joint-tortfeasor liability, products liability with risk aversion and incomplete information, and the like, but if the models are never confronted with data, the advance of knowledge is slight. Rules and case outcomes are data, along with the more familiar statistical data; "qualitative data" is not an oxymoron. But the economic analyst of law cannot rest content with offering economic explanations for doctrines and decisions, any more than he (or she) can rest content with proposing formal models that are increasingly complex and therefore increasingly difficult to test with data of any kind. Ellickson points out, and rightly so, that as economics becomes more specialized, the ability of lawyers to contribute to the theoretical part of economic analysis of law diminishes. But a more important point, I believe, is that law professors have neither training nor taste for systematic empirical research, which would inevitably involve statistical analysis. Fortunately, collaboration between lawyers and economists is possible, and in addition more and more practitioners of law and economics have both a law degree and a Ph.D. in economics. Nevertheless it continues to be the case that a

2. *See, e.g.*, Fiss, *The Challenge Ahead*, 1 YALE J. LAW & HUMANITIES viii (1988).

disproportionate share of economic studies of law lack a substantial empirical component.

In emphasizing these explanations for the apparent leveling off of the growth of economics in the handful of elite law schools that Ellickson examined, I do not mean to disparage Ellickson's explanations. It is true that as a field becomes more specialized, nonspecialists are increasingly at a disadvantage; and most law professors are nonspecialists when it comes to economics. It is not so easy to make a splash today with the application of an extremely simple economic model to law as it was twenty years ago. The other side of this particular coin is that simple models didn't seem so simple then as they do now from the vantage point of an increasingly technical discipline, and that it took a certain boldness to apply economics to law twenty years ago which is not required today.

Ellickson is also correct to point out that the practitioners of law and economics have pressed their researches into areas where the simple economic model of rationally self-interested actors seems inadequate. It is indeed frustrating to try to explain judicial behavior by incentives, when the whole object in designing judicial institutions is to divorce judicial decisions from the judges' incentives. The frontier of a discipline can also be a boundary.

II.

Well, what is to be done? An anterior question is, Why should anyone be concerned with *expanding* the role of economics in law schools? Isn't it large enough already? The only warranted reason for answering "no" would be a well-grounded belief that, at the margin, economic teaching and research are more valuable, more promising, than alternative uses of law school resources. I happen to hold this belief, but I am not sure that Ellickson does—or given his views, should—and I am therefore a little puzzled as to why he is concerned about law and economics' "loss of upward trajectory." A reader of his paper might come away with the impression that Ellickson thinks the real growth areas of interdisciplinary legal studies are psychology and sociology rather than economics, and, if this impression is correct, it is not clear why Ellickson wants economically inclined lawyers to borrow insights from psychology and sociology rather than wanting psychologically and sociologically inclined lawyers to barrel full-speed ahead, perhaps picking up some economic insights along the way. But I suspect that what Ellickson actually believes is that the economic model is the most promising starting-point

in the design of models, and that what is needed therefore is not an alternative model but merely a modified or elaborated one.

If this is right—if his suggestion is that law professors having an economic bent not jump the economic ship but merely enrich their economic models with insights from other social sciences, and thus incorporate such concepts as cognitive dissonance, bounded rationality, the divided self, ideology, and altruism—I have the following comments.

First, as Ellickson recognizes, economic analysts of law (himself included) have begun to do this, and in no grudging way either. Altruism and information costs already play an important role in the economic analysis of law, and there is growing attention to the important form of negative altruism that we call vengeance and that has played a shaping role in legal institutions.[3] To some extent, therefore, Ellickson is preaching to the converted. In this connection, his correct emphasis on the gradual erasure of lines among the different social sciences cuts against his recommendations. We can see that much of what passes for sociological or anthropological research on law is implicitly economic.[4]

Second, it is obscure to me how immersion in additional social sciences can be the *lawyer's* answer to the increasingly technical character of economics. Granted, these other social sciences are not yet as mathematical as economics and are therefore more accessible to a person who has only a legal education. But, as I have already intimated, there is a danger of information overload if the economically-minded lawyer feels obliged to become a master of additional social sciences as well.

Third, there is such a thing as an embarrassment of riches. The lawyer who is as conversant with a broad range of social sciences as Professor Ellickson is should be able to explain *any* legal phenomenon by a judicious blending of hypotheses drawn from different social sciences. So eclectic an analyst should be able to construct an explanatory hypothesis for any observation. But, to overstate a bit, if a theory (call it the "enriched" economic theory of human behavior) cannot be falsified by data because it is consistent with all possible observations, it cannot be confirmed either. To put this slightly differently, Ellickson chiefly wants to mine other social sciences for additional *explanations*. It is not clear how

3. *See, e.g.*, R. POSNER, LAW AND LITERATURE: A MISUNDERSTOOD RELATION 25-70 (1988).

4. A striking example is Stewart Macauley's famous article on contract, *Non-Contractual Relations in Business: A Preliminary Study*, 28 AM. SOC. REV. 55 (1963)—in retrospect a prescient anticipation of contemporary economic analysis of self-enforcing contracts. On the parallels between functional anthropology and economic analysis of law, see my article *A Theory of Primitive Society, with Special Reference to Law*, 23 J.L. & ECON. 1 (1980).

this procedure will enlarge our knowledge of the legal system. To do that we need more empirical research.

I can make the same point slightly differently by noting the magpie-like quality of Ellickson's interest in noneconomic social sciences. He does not seem to want to draw from them broad organizing principles that could be used to generate a variety of hypotheses or build new models of the legal system. Maybe they have no such principles. At all events, he seems to view these other fields as sources of random bits of information about human nature or society that the economist can paste into his models, as indeed economists are beginning to do. But these random bits are so numerous that, to repeat a previous point, the danger is acute that the economist will achieve total freedom in the design of his models.

I see this as a movement *away* from the central need in the economic analysis of law, which is not for broader reading in other disciplines but for the use of scientific methods to enlarge our meager knowledge of the legal enterprise. The object of scientific research—and the aspiration of economic analysis of law is to be scientific, whatever the achievements to date—is to increase our ability to predict and control our environment, in this case our social environment. The usual way this is done in science is by advancing hypotheses, preferably bold and counterintuitive ones, and confronting these hypotheses with data. If the data contradict a hypothesis, we have learned a lot. If they don't contradict it (even after repeated testing), the scientific community will be encouraged to propose related hypotheses, designed to test the principles on which the previous hypothesis is thought to rest. *Where* scientific hypotheses come from (Charles Sanders Peirce coined the word "abduction" to describe the process by which scientists decide which of the infinite array of potential hypotheses is worth testing)[5] is a mystery and also a detail, although an important one. The big thing is to come up with hypotheses that have a sufficiently low antecedent probability of being true to be interesting, but that are not so ridiculous that the results of testing them empirically are a foregone conclusion, and to get on with the testing of them. Whether the hypotheses that legal scholars test by confronting them with data on number of cases, frequency and severity of punishments, crime rates, damage awards, judicial affirmances, settlement rates, appeals rates , and the other abundant but neglected data on the legal system come from

5. Pierce, *Abduction and Induction*, in PHILOSOPHICAL WRITINGS OF PEIRCE 151 (J. Buchler ed. 1955).

psychology, sociology, economics, or some other social science is unimportant. The important thing is to get on with the testing.

Nevertheless, since there isn't time to test all the plausible hypotheses about social behavior, it would be a mistake to abandon the economic model—the most fruitful in the history of the social sciences—prematurely in favor of alternative models with an inferior track record. Professor Ellickson doesn't want to abandon the economic model but to improve it, but too many bells and whistles will stop the analytic engine in its tracks. A commitment to a relatively simple economic model, one that does not supply a *facile* explanation for every regularity (or peculiarity) in human behavior, forces the analyst to think hard before discarding the possibility that the behavior under scrutiny may indeed be rational in a straightforward sense. By the same token, a too-great readiness to abandon the simple model in favor of alternative approaches to behavior at the first sign of difficulty carries the risk of overlooking promising avenues for economic analysis. For example, Ellickson suggests that the explanation for the fact (if it is a fact) that long-time tenants are often permitted to renew their leases at below-market rates is cultural. An alternative, economic possibility is overlooked: The long-time tenant is presumably a "good" tenant—otherwise the landlord would not have renewed his lease so many times. Such a tenant imposes fewer costs on the landlord than a "bad" tenant. It makes economic sense, therefore, that he should receive a discount. Similarly, while the reduction in industrial accidents (if there was such a reduction) by reason of adopting workmen's compensation may have been due to cognitive dissonance as Ellickson speculates, instead it may have been due to workers' rational ignorance of industrial hazards, especially at a time when many industrial workers were recent immigrants, not previously employed in the industrial sector, who may have lacked information about such hazards.