THE LEGAL ACADEMY AND THE PROFESSION

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The relationship between the American legal academy and the institutions it studies is a bit estranged, particularly by comparison to where matters stood a few decades ago. There still are direct encounters between academy and profession -- doctrinal scholarship and Restatement projects that lawyers appreciate, and occasional first-hand involvement by academics in legal and political life. But the high-status, high-profile work in the legal academy nowadays tends to be increasingly critical, theoretical, interdisciplinary, interesting to other academics, and useless to courts. Many judges and lawyers understandably are non-plussed by the resulting cascade of literature that has no apparent application to their work; meanwhile academics spend perhaps seven million man-hours annually on scholarship—as much labor each year as it took to construct the Empire State Building—yet usually see little sign that anything they write is valued by the legal system or makes an impact on it. The consequences of this estrangement probably are less important than they appear, but now we are getting ahead of our story.

1. Courts and scholars.

The most natural meeting place for the work of the academy and the work of the profession has long been thought to be the judicial branch, where a judge confronted with a hard case might repair for guidance to scholarship that points the way to its resolution. But only a narrow type of academic work can serve this purpose. It has to be pitched at relatively low level of generality; it has to take a discrete problem that arises in an adversarial setting and produce a recommendation about it; the recommendation has to be presented as way that legal materials can be nudged around within the rules of the judging game, where radical rethinking is discouraged as a matter of norm and practice; and the setting must be one where existing tools for resolving the problem seem unsatisfactory, thus causing a judge to be interested in outside guidance (Cass and Beermann 1993). As a practical matter this generally means that an academic trying to make a direct impact on judicial decisions needs to achieve a particular mix of descriptive and normative elements in his work—largely descriptive and only modestly normative. The best examples of the mix are found in successful treatises and restatements, where recommendations to courts are embedded in large efforts at description and are stated in the language of legal principle, not interdisciplinary talk. Courts welcome the results and often find them useful. Their influence in any given case typically is small but can be large in the aggregate and sometimes very large in particular areas; the American Law Institution's restatements of tort law, for example,

suggested frameworks for several causes of action that took hold in many jurisdictions that had not previously adopted them. Law review articles can strike the same balance and be useful in similar ways, and until perhaps thirty years ago this was the conventional understanding of the primary purpose of legal scholarship: the best of it was most helpful to courts and other legal institutions.

Since around 1970 there has been a gradual shift of the norms within the legal academy about what work counts as valuable. The most prized legal scholarship is now likely to employ tools of economics or other social sciences; more likely to view the legal system from perspectives external to it, such as feminism; and more likely to pitch normative theories at a high level of generality—theories of interpretation or efficiency or equality that are ever farther removed from the practical details of the decisions judges make in their cases. Various reasons for this trend can be offered from the standpoint of intellectual, social, or economic history. The legal realist movement reoriented scholars toward analysis of whether legal doctrines were serving useful purposes; it was only natural for such inquiries to employ tools from other disciplines, as "legal reasoning" is an internal sort of analysis that supplies no answers to questions about the relationship between law and the world it governs. Meanwhile a generation of students dissatisfied with American public institutions in the 1960s and 1970s grew into a generation of academics bored by questions about how they could help courts do their jobs a little better, and more interested in grander critical inquiries into the system's structure and operation.

Then there is the economic account: the legal profession and the wealth attainable by lawyers have both grown dramatically since the 1960s, and this has translated into greater demand for legal education and a corresponding infusion of wealth into the law schools. The wealth in turn has increased the numbers of law faculty, raised their salaries, and made it easier for them to treat scholarship as their only concern. Earlier in the century law professors were much more likely to be part-time practitioners, which increased their likely interest in the same matters that concerned other lawyers and made it less likely that they would have the time or inclination to pursue ambitious interdisciplinary projects. As both a consequence of these developments and a spur to them, Ph.D.s in disciplines other than law became more common on law school faculties. So did academic lawyers without graduate degrees in other disciplines who saw it as their task to combine insights from various social sciences with their own intimate knowledge of legal institutions to create a distinctive intellectual product.

Whatever the cause of these developments, the result is that the standards of value within the academy and outside it have diverged markedly. The law review articles courts cite the most tend to be cited in scholarly work only rarely and are not well-known to academics. Likewise, the legal scholarship most famous and influential within the

academy over the past thirty years generally is of little interest to courts, sometimes being cited hundreds of times by academics but not once in a judicial opinion (Merritt and Putnam (1996)). The trend is illustrated by the Supreme Court's declining interest in citing law review articles. There were .86 such citations per opinion in the early 1970s, but only .47 in the late 1990s; the Harvard Law Review was cited 169 times in 1971-73 (.15 cites per opinion) but only 30 times in 1996-98 (.052 cites per opinion) (Sirico (2000)). The decline has not been met with any particular alarm in the academy. It is a case of mutual indifference. When a recent survey asked academics who they considered the primary audience for their work, the most common answer by a wide margin was other academics (Merritt (1998)). Many of the most illustrious figures in the legal academy have had no perceptible influence at all on the legal system, instead achieving prominence by saying things that impress and influenc other professors.

2. The extent and significance of the disconnection.

Some judges and lawyers infer from the output of leading law faculties that academics lack interest in the practical challenges faced by legal professionals. They long for the days when there was a stronger sense of partnership between academy and profession (Edwards 1991). But the enlargement of the distance is not as consequential as may first appear—or at least not consequential in the *way* that first appears. First, legal academics still produce enormous amounts of doctrinal scholarship. It may be a smaller share of legal scholarship than it used to be (yet even this is not clear; see Gordon (1992)); but there are more than twice as many law professors now as there were in 1970 and more than twice as many law journals, so a declining share need not indicate an absolute decline. Second, it is not clear that the *influence* of legal scholarship on the profession has declined, because that influence never has been great. We saw that the Supreme Court cites the Harvard Law Review less now than it once did, but in the old days, and today as well, most of the citations to it were ornamental anyway—a dash of erudition at the end of a string cite listing cases and, sometimes, articles that discuss the point the Court is dealing with, but with no indication that the discussions cited actually have affected the Court's analysis. Of course scholarship may have stimulated the thinking of judges in ways not visible in their opinions, but then that is still true. And while restatements and treatises have sometimes had considerable influence on courts, that also remains so today.

What may be true is that work toward the doctrinal end of the spectrum has lost prestige within the academy. This takes a psychological toll on the profession's sense of camaraderie with the law schools. Judges and lawyers don't want to feel that the academy has relegated their concerns to a second rank; and meanwhile they look at what academics are writing instead and much of it seems frivolous. These problems do not

seem serious. They are matters of image and pique. The more troubling complaint occasionally offered is that the quality of doctrinal work is slipping precisely because it is no longer the preoccupation of the best minds in the academy. The reasons for this view seem just to be that the work isn't appearing in elite law journals as often as it used to and that elite law schools aren't hiring as many doctrinalists as they once did. But it would be fallacious to infer from this that the quality of doctrinal work is dipping. It may instead be that the academy has redefined the meaning of "best minds" to make producers of useful doctrinal work less likely to qualify—but that the quality of the thinking brought to bear on doctrinal problems is as good as ever. The work of the thousands of doctrinalists in the legal academy should not be disparaged just because their work is less to the taste of the elite law schools than it used to be. Or less to the taste of the elite law reviews, which are almost all edited by students.

Even if it is not as consequential as it appears, however, we might ask whether anything can or should be done about this redistribution of labor and prestige within the academy and the resulting sense of disconnection between academy and profession. The shift represents a trade-off. Whereas doctrinal scholarship has the potential to make a modest but visible difference in the legal system, academics engaging in higher levels of theory are aiming for a larger impact that is less likely to occur or to be clearly visible if it does. They are operating as wholesalers, relying on others to turn their ideas into concrete possibilities for legislation, regulation, or judicial decision. Any single piece of writing in this vein is unlikely ever to make any difference, but it may contribute to a body of work that infects the thinking of other academics, which in turn affects the milieu in which their colleagues talk and their students are trained. Gradually the ideas may be imported into public life by students or colleagues who venture into government service as law clerks, judges, regulators, and so forth. The final impact may be hard to discern or trace to its source, yet still be greater than the impact of an idea retailed directly to judges in a law review article.

It should be stressed that usually the impact of *either* type of work is nil. Doctrinal scholarship specific enough to be useful to courts is the academic equivalent of a scratch-off lottery ticket. Relatively soon the author learns whether he won anything; probably he didn't; if he did, the returns are modest but satisfying. The more theoretical and abstract legal academic writing, like basic research in many fields, is akin to the bigger lotteries where the potential payoff and the odds against achieving it are both larger by orders of magnitude. Failure is the norm either way; nobody has yet found a way to generate good ideas, large or small, except by creating a system that also generates a lot of bad or trivial ones (see R.A. Posner (1995)). Thus most legal scholarship, doctrinal or otherwise, is read by hardly anyone and has no impact on anything. Indeed, many academics abjure any interest in whether their work has a practical impact. They seek to understand the legal system for the intrinsic satisfaction of it;

impact on anyone else is just a happy by-product of their labors. For all that appears, the practical payoff of this approach may end up as high as, or higher than, that of more direct efforts to make a difference.

In any event, there is no question that the creation of the larger theoretical scholarship, however motivated, *can* have a large impact. The law and economics movement has had a few specific triumphs in various areas of doctrine, but its greatest success has been a transformation of the way not only many academics but also many legal officials, especially in the regulatory arena, think about law. Arguments about the efficiency of a rule are more common and influential now than thirty years ago, and this must be attributed in significant part to the rise of law and economics as an analytical approach. In the 1960s it barely existed; by the 1990s it accounted for a substantial share of all legal scholarship. Much of that scholarship seems pointless, just like any other type, but the overall effect of the movement must be counted as high even if little of it has ever been directly useful to courts.

3. Outlook for the future.

Might the gap between academy and profession be narrowed partly by movement on the profession's part? Perhaps judges should be more open to some of the interdisciplinary work academics generate. But the legal system is resistant to the direct influence of such work, and with good reason. The law developed into its current form at times when powerful contributions from the academy weren't available. Think of the legal system, and especially the judicial branch, as if it were a many-brained organism that evolved into its current form by learning to survive on the materials at hand: principles, concepts and customs that judges could use to make satisfactory decisions with little time and information. Constraints on their knowledge caused courts to decide one case at a time, keeping the resulting decisions close to the facts at bar; efforts to do more were to be scorned as judicial legislation. Whatever legal pronouncements courts did make were supposed to be based on cases, statutes or regulations, or the Constitution—not on academic work. Those understandings about the proper size of decisions and about what counts as valid authority persist to this day.

Now that a large legal academy exists with millions of hours of analysis to contribute every year, it might seem appropriate for the legal system to make more room for the contributions academics can make. But the powerful tools of analysis that didn't exist when the law was growing up still don't quite exist. Analysts of law have economic and philosophical tools that were not available one or two or three hundred years ago, but those tools still tend not to produce clear, decisive guidance. They generate disagreement among their users, and when the users do agree, the normative strength of the methods isn't great enough to command deference from judges or legislators. Compared to an academic,

the judge has relatively few hours to spend on a question, but the hours an academic can spend are dwarfed in their own way by the hours of experience and judicial consideration that may be impounded in an old and durable doctrine that the judge in a case is called upon to nudge forward. And judges who do attempt to take academic work into account may find it hard to distinguish the good from the bad, or to apply the good work competently once it is found. A judge who admits that academic scholarship influenced his decision invites ridicule from judicial colleagues, who will cite this as evidence that his position is flimsy; or from the academy, which may be quick to complain if a court relies on work that turns out to be flawed, or inapposite, or that the judge fumbled in trying to apply. The most celebrated use of extra-legal scholarship by an American court came in *Brown v. Board of Education*; while the result of the case is considered a great success, the Supreme Court's use of social science scholarship received a mixed reception (see Cahn (1955)). Most courts would still be more comfortable citing an obscure judicial opinion from another jurisdiction than a hundred-page law review article as support for a holding.

The limits of the direct impact academics usually have on the legal system can be illustrated by considering an exceptional case where the impact has been large: antitrust. A number of important ideas in antitrust law were pressed by scholars in the 1960s and 1970s and then adopted fairly quickly by courts. Maybe the changes would have occurred anyway, but then that can always be said; at the very least the scholarship greatly hastened the advance of certain ideas. But the reasons for the influence of some antitrust scholarship help explain why most other legal scholarship is not so influential. Antitrust is relatively new set of legal doctrines. It does not have centuries of doctrinal development behind it that courts can use as a comfortable foundation for decision. It also is relatively technical, calling on courts to serve as regulators of commercial organization in complex industries. Most judges don't know much about economics, so it is natural for them to welcome guidance from those who do. In addition, antitrust litigation in the 1960s and afterwards became especially complex and time-consuming, justifying investment by the parties in research into any academic work that might be useful. And academics have played a large role in antitrust enforcement, taking tours of duty in the Justice Department and bringing with them various ideas from the academy.

Economic analysis as well as other movements have generated large amounts of scholarship in other areas, too, but usually without making a comparable impact on doctrine. The economic analysis of contract law, for example, has consumed thousands of pages in legal journals over the past twenty years, but a recent study of the influence of those studies on contract law concluded that there had been none (E.A. Posner 2002)). The reasons for this can be seen by looking at the supply and demand sides of the production relationship. Contract law is old, and

mostly was built from customary and fairly simple ideas that were refined in small increments over a long period of time. The resulting edifice is sturdy if imperfect, and seems to serve its purposes tolerably well. Judges feel at home with it and competent to apply it. So when an academic appears with a new argument that some alternative regime of remedies would be more efficient than the existing one, or better from some other perspective, courts pay no attention; academics do not have a comparative advantage that judges feel they need. Thus the big developments in the academic treatment of an area need not, and often do not, bear any connection to developments in the practice of it.

A final point is that the law and economics agenda in antitrust was at least roughly in keeping with the political temper of the times when it took hold; that is one reason *why* it took hold. More commonly, hot academic fads are driven by or accompany a politics unpalatable to the legal system. The figure at the political median of the legal academy is well to the left of the median in the profession, and this is another reason for the distrust of the academy's output by legal professionals (Lindgren 2002): to the extent it is normative rather than informative, it is likely to advance a political vision shared by too few judges and lawyers to be appealing.

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