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Robert A. Kagan

Berkeley Law

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ROBERT A. KAGAN*

INTRODUCTION

Twenty-one years ago, as the Oxford Centre for Socio-Legal Studies was taking form, I was working on my Ph.D. dissertation in sociology of law. It was a time of intellectual optimism, of hope for collective development of theoretically and empirically grounded understanding of how legal systems really worked. Each new empirical study cast a beam of light into a little-known, often humble province of law’s empire, illuminating relationships between the law in action and the law on the books. The learning curve slanted upward sharply.

These days, on the other hand, I feel a certain frustration. In the last twenty-one years, socio-legal studies have accomplished a great deal. But we are failing, I feel, to keep up with developments in the legal systems that surround us. The problem is this: in contemporary democracies, positive law, the law on the books, proliferates extremely rapidly – so rapidly that it confounds our attempts to find out, in any systematic way, what is actually going on. Like Lewis Carroll’s Red Queen, we seem to run faster and faster only to keep from falling further behind. The ecological outlook in the Amazon may be troubled, but the number of species in the legal rainforest keeps multiplying.

LEGAL PROLIFERATION

The growth of law is proceeding along several dimensions. First, law has become far more ambitious. It attempts to regulate or even extirpate scores of behaviours that it virtually ignored twenty-five years ago: sexual harassment in the workplace, leakage from underground fuel storage tanks, medical malpractice, the incidental taking of birds and marine mammals by fishing boats, the education of handicapped children, the management of

* Professor of Political Science and Law, and Director, Center for the Study of Law and Society, University of California, Berkeley, CA 94720, United States of America
pension plans, treatment of the mentally ill, intellectual property in computer software. The list could be extended for pages.

Second, within each policy domain or traditional subject matter category, law has become more dense, more complex, more politically-charged, and most importantly, more changeable. Prodded by the media and by political controversy, legislators, administrators, lawyers, and judges have become perpetual legal reformers. Criticizing the status quo, they continuously articulate new legal goals and theories. They establish new agencies and techniques for implementing or enforcing the law. Each is consciously designed to improve upon, and hence to differ from, its predecessors. But change stimulates additional legal and political conflict; hence, it also stimulates demands to strengthen, weaken, or revise the recently enacted law as soon as possible. Third, law has spread laterally across the globe, expanding its domain geographically. The collapse of communism has led to the creation of new constitutional courts and legal regimes, raising new questions about the conditions under which law might come to matter, or how law works in societies undergoing radical political and economic change. Finally, law also has spread vertically. Supra-national regulatory regimes and rulings have multiplied, interacting in complex ways with domestic ones.

Amidst these proliferating legal regimes, institutions, and practices, our little community of socio-legal scholars resembles a band of near-sighted detectives, stooping to search for evidence concerning one event while a crime wave is breaking behind our backs. Once we do turn to examine the territory behind us, we look back over our shoulders to find that a new legal structure has been built in the portion of the legal landscape we thought we had figured out. In the years to come, moreover, the gap between socio-legal research and the dynamism of the phenomena to be studied is likely to grow wider. The basic trends that have generated the modern world’s legal explosion are not likely to abate.

WHY HAS LAW PROLIFERATED?

We now dwell (and will continue to dwell) in a world shaped by four powerful, law-generating factors:

(i) more intense international economic competition;
(ii) extremely rapid technological change;
(iii) mounting environmental stress; and
(iv) greater geographical mobility.

These engines of change erode older forms of social control. They batter down tariff barriers and protected monopolies. They undermine the religious authority and the old-boy networks that managed conflict in socially homogeneous, stratified communities. While technological change, competi-
tion, and mobility promote higher aggregate levels of health, wealth, and freedom, they also generate entirely new forms of fraud and new kinds of inequality. They stimulate a constantly changing array of threats to economic security, new fears of environmental degradation, new cultural challenges to traditional community norms. Most distressingly, as Professor Abel reminds us elsewhere in this volume, they also bring us more social disorganization, crime, and ethnic tension.

These recurrent disruptions and tensions all give rise to intense demands for new legal rules and controls. Indeed, the fifth and perhaps the most important law-generating trend of the late twentieth century, I would argue, is occurring in the realm of political culture. One might call it the decline of fatalism. Lawrence Friedman refers to it as rising popular expectations of ‘total justice’ the belief that because sophisticated modern societies can afford to provide compensation for victims of unfair treatment, personal injury, and sudden economic loss, they should enact laws that do so. Because modern governments can provide prophylactic regulatory protections against identifiable sources of harm, against economic insecurity, and against unfair treatment, they should enact laws that do so.

The culture of total justice, moreover, expands the notion of ‘unfairness’. Groups that for generations fatalistically accepted subordinate status now feel justified in demanding legal equality. Spurred by modern communications, individuals throughout the world more often see themselves as rights-bearing, rights-demanding citizens. They demand new legal protections against construction projects that threaten their neighbourhoods. They demand more aggressive legal efforts to combat discrimination. They demand legal rights even for the voiceless, for dolphins, owls, trees, and marshes. Democratic governments tend to supply what the citizens demand. Some citizens, riding the waves of change, demand new rights of inclusion, equality, political access, and control. Others, threatened by change and disruption, demand legal protection from harm and loss of control. Democratic governments pass laws responsive to both.

In all likelihood, therefore, the decades to come will see an accelerated rate of change, disruption, and political demands for more law. New legal rules, institutions, jurisdictions, remedies, and enforcement methods will continue to proliferate, as will political controversy about law. For socio-legal scholars, these developments will be very interesting. But the relentless proliferation of law will make it even more difficult to develop systematic, empirically-validated knowledge and coherent theories of legal processes.

WHAT, THEN, ARE WE TO DO?

When the phenomena in a field of scholarly attention seem beyond the ken of the existing cohort of scholars, ideally they would make concerted choices about which phenomena, which problems, are most important to address.
Scholars being scholars, however, they rarely want anyone else to tell them what is most important. One time-tested response is to clarify the division of scholarly labour, encouraging individual scholars to congregate in the subdivision which they think most important. As new sub-universes within the universe of living things have been discovered, the biology department at the University of California at Berkeley established separate divisions of molecular and cell biology, biochemistry, genetics, immunology, and neurobiology.

Institutes of socio-legal studies are hardly large enough to establish separate divisions. And I surely do not recommend any division of labour that would entail a retreat into separate academic disciplines, for it is precisely the collaboration of sociologists, economists, political scientists, psychologists, and legal scholars that have made socio-legal studies vital and interesting. I do suggest, however, that we should be quite clear about which portion of the legal universe we are working on. Let me attempt to illustrate.

THREE RESEARCH AGENDAS

At a very general level, three research agendas have dominated socio-legal studies. In the first research agenda, law and institutional design are the phenomena to be explained, the ‘dependent variables’. The goal is to analyse the social, political, cultural, and economic forces that shape the formulation of law and the design and function of legal institutions. What political and intellectual conditions account for the formation and maintenance of independent judiciaries? Why do Japanese methods of compensating motor vehicle accident or pollution victims differ from those in the United States of America? What accounts for historical or cross-national variations in the business of courts? Professor Abel’s remarks can be interpreted as urging scholars to analyse the forces that lead to legal policies that bolster (or erode) community cohesion and mutual responsibility, that draw the boundaries of community tightly or loosely, that promote (or discourage) law that guarantees tolerance and compassion.

In the second research agenda, the focus is on the behaviour of legal institutions. One might call it the ‘legal process’ agenda. Its premise is the famous ‘legal realist’ aphorism: ‘Rules don’t decide cases, men do.’ But the question then arises, what influences the men? Thus, United States social scientists code cases and perform statistical analyses to determine whether the policy thrust of judicial decisions is best explained by individual judges’ political party membership, their prior career experience, the status, wealth or race of the parties, and so on. Students of regulation have sought to determine which organizational, political and economic factors induce regulatory law enforcers to be aggressive and legalistic rather than conciliatory or lenient. Numerous scholars have studied how lawyers’ incentives, attitudes, and fees affect the disposition of civil cases.
The third research agenda treats law and legal decisions not as dependent but as 'independent variables'. Its goal is not to explain why laws and legal decisions vary, but to discover and assess what effect legal processes, in all their variations, actually have on social life. We can call that the 'social effects' agenda. Scholars working on this agenda explore the limits of the sociologist William Graham Sumner's overly pessimistic dictum that 'law ways cannot change folk ways.'

As I see it, the socio-legal studies movement, with many individual exceptions, of course, especially at the Oxford Socio-Legal Centre and among law-and-economics scholars, has been far more preoccupied with explaining the provenance of legal institutions and legal decisions than with examining their impact. We have been more concerned with legal process, in short, than with social consequences. This is quite understandable. Socio-legal scholars have been influenced by traditional legal scholarship's focus on the decisions of official legal actors and the ideas that influence them. Even more importantly, it is easier and far less expensive to study a small number of legal decision-makers or the records of their decisions than to study the far-flung individuals and organizations who are the subjects of law.

CONCERNING THE SOCIAL EFFECTS AGENDA

Nevertheless, I do think it would be desirable to intensify our efforts to assess the actual consequences of legal institutions and interventions, to look at law and legal institutions from a 'consumer perspective'. A focus on the consequences of law teaches us about the strengths as well as the limits of legal interventions, while reminding us that law is not the only factor influencing behaviour. It directs us to consider the unintended costs as well as the intended benefits of legal interventions, to look beyond their first order 'effectiveness' and examine their impact on fundamental social values, such as equality and efficiency, co-operation and conflict, security and creativity.

Many of the most useful contributions to our understanding of legal life have explored this terrain. The Oxford Centre's survey of accident victims and similar studies elsewhere revealed the extent to which victims of misfortune fail to invoke the remedies provided by law. Detailed studies of the lawyering costs in tort cases in the United States have exposed how woefully inefficient and inadequate the liability law system is in providing compensation to injured persons. The Kansas City police study showed the limited impact that intensified patrols actually had on crime. The Minneapolis spousal violence study, on the other hand, indicated that when police arrested offenders - rather than mediating - violence was reduced. Research in Maine showed that court orders following adjudication yielded lower subsequent compliance rates than dispositions reached via mediation.
These were all expensive, labour-intensive studies. But their intellectual pay off was great.

Still, it is unfortunate how little really is known about the impact of some of the most controversial legal changes of our time. Has enhanced liability for product-caused injuries produced discernible changes in product design, injury levels, or prices? How do affirmative action or 'positive discrimination' laws affect organizational efficiency and morale? How does medical malpractice liability affect hospital practice, positively as well as negatively? How do police practices change, if at all, when courts are aggressive in excluding illegally seized evidence?

Socio-legal researchers, as far as I know, have hardly begun to research those kinds of questions,\textsuperscript{7} much less provide answers. Work on such a impact agenda, however, would be of great importance both practically and theoretically.

**BEYOND PAROCHIALISM**

By and large, socio-legal studies have been rather parochial, focused on legal processes in a single country. Yet dollar for dollar, scholar for scholar, the greatest contributions to understanding legal processes, in my view, come from cross-national comparative studies. Traditional comparative law scholarship tells us how law differs, or sometimes how legal institutions and legal processes differ. But it rarely tells us how much those differences actually matter and for whom. That is a task for comparative socio-legal studies, which can examine how cross-national differences in law or legal institutions affect relatively similar social functions or types of enterprise.

To provide an example from my own experience, for several years I intensively studied seaports in the United States of America, northern Europe, and China, asking how differences in legal regimes affected port operations. Dutch and American labour law, it turned out, created different incentives for dockworkers' unions. In consequence, stevedoring operations in Rotterdam, compared to American ports, were more efficient, 25 per cent less costly, and resulted in a more equitable sharing of productivity gains.\textsuperscript{8} I learned, too, that United States environmental and land use law slowed port expansion, but without producing greater environmental benefits.\textsuperscript{9} One could employ a similar methodology to examine the social consequences of other kinds of laws and institutions. To what extent do producers change their products (and prices, warnings, and warranties) when they sell them in jurisdictions with weaker (or tougher) product liability laws? How do multi-national manufacturers alter their pollution abatement methods in countries with different environmental laws or enforcement systems?

Economic competition and instantaneous communications have made cross-national differences in law and legal methods much more politically relevant than they used to be. The more we ask law to do in society, the
more it costs. Increasingly, policy-makers want to know 'what works', whether the laws and legal methods used by other nations are more or less effective, more or less efficient, more or less fair. This, in my view, is all to the good. It is more likely to produce a virtuous circle, an emulation of the best, than to produce a vicious circle, a race to the bottom. By responding to this demand, socio-legal scholars can do good while learning more.

Finally, cross-national comparative legal impact studies require cross-national scholarly collaboration. The problems of access and in-depth interpretation always are best dealt with by natives. The often startling insights that come from comparison, on the other hand, flow from intensive interaction between two or more sets of natives. I am gratified by the steps toward cross-national collaboration I see being undertaken by the Oxford Centre, for I am confident that along that path lies the most exciting and meaningful future for socio-legal studies.

NOTES AND REFERENCES


10 This phrase is meant to echo a similar call made over thirty years ago by E. Cahn, ‘Law in the Consumer Perspective’ (1963) 112 Pennsylvania Law Rev. 1.

11 Economists tend to approach law-related behaviour in this way. In their models, markets for goods, services, reputation, and information are at the heart of social life; this provides a starting point for studying what impact variations in law and in enforcement will actually have. There is no a priori assumption that law will matter lot, or that ‘compliance’ is a good thing, or at least a costless thing. Business firms are not treated merely as parties who win or lose cases, but as economic entities through which legal wins and losses are transmitted to the public via price changes or signals for the redirection of investment.

I would not suggest that political scientists and sociologists adopt economists’ methods, especially in so far as they rely on restricted assumptions rather than on detailed data collection. Nor do I think it desirable to focus on economic efficiency as the sole benchmark for assessing outcomes. Indeed, those characteristic limitations make it all the more important for other disciplines to work on the impact agenda.


14 G. Kelling et al., The Kansas City Preventive Patrol Experiment (1974).


17 See, for example, P. Huber and R. Litan (eds.), The Liability Maze (1991); L. Edelman et al., 'Internal Dispute Resolution: The Transformation of Civil Rights in the workplace' (1987) 43 University of Chicago Law Rev. 1016.
