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The
MAURICE AND MURIEL FULTON
LECTURE SERIES

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***THE AMERICAN JURY PROJECT AND
THE CHICAGO LAW SCHOOL***

FRANKLIN ZIMRING

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THE LAW SCHOOL
THE UNIVERSITY
OF CHICAGO

The American Jury Project and the Chicago Law School

*by Franklin E. Zimring**

The hundred year history of this law school splits into two almost equal chronological segments, with the great divide taking place in 1950 when President Hutchins appointed Edward Hirsch Levi to become its dean. Levi was a graduate of both the college and the Law School of the University of Chicago, the first home-grown dean and the only one until Geoff Stone in the 1980s. Two striking features of the early years of Edward Levi's deanship provide important perspectives on the school's development. The first is that the Levi deanship was part of a much broader demographic shift in leadership at Chicago. No fewer than four Chicago Law graduates from the period 1935-1941 joined (in Levi's case rejoined) the faculty after World War II and stayed on—Levi, Bernard Meltzer, Harry Kalven and Walter Blum—and the collective impact of this group on the trajectory of the Chicago Law School over the next 35 years was enormous. For those who attended the school during its second half-century, it would be difficult to imagine the institution without each of these four as a defining element in its character.

The second allegation I make without fear of contradiction is that the 12 years Edward Levi served as dean at Chicago marked its clear transformation from a very good school into a great one. Frank Ellsworth makes a persuasive case that Chicago was a very strong school the day it opened (Ellsworth 1977). But nothing happening in Hyde Park struck terror in the faculty lounge at Harvard Law School until late in the Edward Levi deanship. By the mid-1960s, the Law School on 60th Street was an intellectual power of the first rank and has remained so ever since.

* William G. Simon Professor of Law and Chair of the Criminal Justice Research Program at the University of California, Berkeley. I thank Shari Diamond, Owen Fiss, Jack Schlegel, and Jan Vetter for comments on an earlier draft of this lecture. Much of my knowledge of the project came from interviews in 1991 and 1992 with Walter Blum, Edward Levi, Bernard Meltzer and Fred Strodbeck.

The tactics that Levi and company used to build both the institution's capacity and its reputation were a mix of the standard and the rather unconventional. The standard method of institution building in the law school world is mid-career theft, hiring away leading faculty from other prominent centers of learning. Chicago did much of this in the early 1950s. Karl Llewelyn and Soia Mentschikoff came together in 1951, the same year that the young Allison Dunham arrived. Francis Allen was recruited twice. Not all the lateral entry hiring was the conventional search for leading lights. There was a risk-taker's preference for eccentric greatness in the profile of established scholars that Chicago recruited. The husband and wife team of Llewelyn and Mentschikoff provided glamour to be sure, but what of the contentious and non-establishment Kenneth Culp Davis, who had made surprisingly few friends while providing a distinctive jurisprudential structure for American administrative law? This was not an appointment made to generate envy at Harvard or Columbia. But the lesson here may be that greatness justified taking risks.

The effort to build a research program in law and the behavioral sciences was a major theme in planning from very early in Edward Levi's decadal career, in large part because Robert Hutchins had reported substantial interest in support for such work from meetings he had with senior Ford Foundation staff.

Hutchins had preached the gospel of social science research into legal issues early in his youthful deanship of the Yale Law School in the late 1920s. William O. Douglas and Charles Clark had launched projects gathering data on case flow and bankruptcy (Schlegel 1993). After Hutchins left Yale, Professor Underhill Moore did embark on an ambitious law and behavioral science project that tried to generalize from observations about the impact of parking signs on the behavior of automobile operators. The Yale program was not considered a success either in the legal academy or in the behavioral sciences (Schlegel 1993). In that respect it is typical of the reputation of other efforts at Johns Hopkins and Columbia.

But the strong interest in law and the social sciences continued

despite the notable absence of proven examples of valuable interdisciplinary work. By 1950, Robert Hutchins had emerged as a major influence at the Ford Foundation, an institution he would move to in 1951 and which would support Hutchins and his agenda for the remaining quarter-century of his life. So the strong interest of Ford in a program of law and behavioral science research was money in the bank, a guarantee of substantial funding for any credible research program that the law school might create. This was viewed as a substantial institution-building opportunity for Edward Levi and the post-war Young Turks at Chicago.

But what topics should be the central concerns for a research program and how might they be studied by an interdisciplinary team based in an American law school? The task of identifying topics and methods was a challenging one. And while the future projects were to be interdisciplinary collaborations, the identification of topics and the organization of a research effort was not interdisciplinary. It was an inside job, the work of a collection of young law professors at a small Midwestern school with little or no formal training in empirical methods. Three topics were identified by the informal planning group as the basis for sustained research programs—the jury, the arbitration process, and citizen perceptions of fairness in taxation. Two other candidates for study early in the process—the youthful offender and obscenity—had dropped out by 1953. The tax topic was an outgrowth of the interests of Harry Kalven and Walter Blum in the jurisprudence of fair taxation. They were collaborating in a short book-length project published as *The Uneasy Case for Progressive Taxation* (1953). A research project on tax would attempt to probe the linkage between public sentiments and various aspects of tax policy.

Arbitration was the most formally structured and commercially prominent system of private dispute resolution to serve as an alternative to litigation in courts in commercial and labor management relationships. It was also a serious interest of Soia Mentschikoff, who was the first and last director of the arbitration project at Chicago.

The jury was widely regarded as a prime candidate for empirical

study. The jury was a distinctive and controversial part of the Anglo-American legal system that had been the subject of endless speculation—particularly in the law of evidence—but virtually no empirical study. While it was the least eccentric of the three research topics put forward in the proposal to Ford, it was also the one topic without an obvious principal investigator on the Chicago faculty. Bernard Meltzer took the lead in identifying the jury as a priority project and chairing the faculty committee assembled to create a research strategy for the jury study (Meltzer 1953). But his contributions to the jury enterprise were more in the spirit of pro bono public service than a bid by Meltzer to start a long career of collaborative empirical research. So this centrally important research program had three different faculty directors in its first two years—Bernard Meltzer, the founder; Phil Kurland, who joined the faculty as the jury project was taking shape, and who would soon make his primary institutional contribution at Chicago by founding and building the *Supreme Court Review*; and finally, Harry Kalven, Jr. By 1953, the law and behavioral science project had a Ford grant of \$400,000 (it would eventually cost about \$1,000,000) and an urgent need to launch. And Edward Levi had taken personal responsibility for the jury project in his reporting to Ford (Levi 1953, 1954, 1958).

What took Harry Kalven so long to come to this project was not any lack of interest on his part in the jury as a legal and governmental institution. Kalven was deeply interested in juries and had a number of creative and original insights about the jury. But Harry Kalven was interested in everything and had an almost embarrassing fountain of fresh ideas on any legal topic he would address. He was also committed to the tax project with Walter Blum until his transition to the jury project marked a quiet and almost costless end to that enterprise.

The major difference between the sort of small-scale “law and” projects that the tax undertaking and the arbitration project were to become and the scale as well as wholly original effort that the jury project would demand at Chicago is ultimately a matter of personnel. The proposal to Ford for all three projects and the longer

research prospectus on the jury were all law school “inside jobs” written by law professors acting alone (Levi 1953). But a major fact-gathering project would require an empirical social scientist with senior professional status who could center his professional life in a law school. The only alternative would have been handing responsibility to a social scientist who would settle comfortably into a department of his disciplinary peers and then commute to the law school from time to time.

The problem with that commuting approach is simple. Once that process gets launched, once the social scientist no longer lives in the law school, the project itself either leaves the law school or the social scientist becomes less centrally involved in the design and execution of the research. But what type of senior methodologist would wish his primary affiliation to be with a law school? And how could the scholar and the school avoid pushing a professional social scientist to the periphery of the law school’s social and intellectual circles?

These problems are critical to the prospects for empirical research in law schools, then and now. To solve them, Edward Levi was hunting for Hans Zeisel, but he didn’t know it yet. One of Levi’s early stops was to offer the jury project job to Paul Lazarsfeld of Columbia University’s Bureau of Applied Social Science. After turning Dean Levi down, Professor Lazarsfeld suggested that his friend and former colleague Hans Zeisel might be perfect for the task. Lazarsfeld and Zeisel had collaborated in Austria on a study of Marienthal published in the early 1930s, before both became refugees from Hitler’s Europe. Zeisel had both a law degree and experience pioneering market research in Europe. He had found his primary professional success as a market researcher in the United States, first with the Tea Council, later as head of a research effort for the Interpublic advertising empire of Marion Harper. But Zeisel had also maintained academic connections and ambitions during the first decade of his American career. In the mid-1940s he had written a small book called *Say It With Figures* that was both a guide to statistical analysis and to statistical presentation. (The book would go through six editions.)

Say It With Figures was a prominent display of the ingenuity, originality, and idiosyncratic creativity that are the trademarks of Hans Zeisel's work. Edward Levi's invitation to join the law faculty and the jury project was accepted with eagerness by Zeisel. Throughout his long Chicago affiliation Zeisel also maintained his New York professional contacts and identity, which on more than one occasion served as an economic and emotional safety valve. But for the next 39 years, Hans Zeisel's intellectual home address was the University of Chicago Law School.

Zeisel was one of two sociologists brought to Chicago with the first Ford grant to contribute to the jury research. The other was Fred Strodbeck, a young Yale product who settled into Hyde Park but developed his primary associations in the Department of Sociology rather than the Law School. He was importantly involved in the jury simulation work that produced, among other publications, *The Jury and the Defense of Insanity* (Simon 1966). Zeisel moved into the Law School, bonded with Harry Kalven, and began work on the formal design of the most ambitious empirical data design and collection effort in the history of fact research in the legal academy.

The first important contribution of Hans Zeisel to the central core of the jury research was a research design using questionnaires to contact trial judges who had presided over jury trials to report on their experiences with the trial and both their personal opinion of the appropriate verdict in the case and of the verdict of the jury. Eventually, more than 500 judges would report on more than 3,600 criminal jury trials in the data set reported in *The American Jury*.

Of course, emphasizing this approach—the central methodology reported in the 1966 book *The American Jury*—understates both the range of the questions the jury project investigated and the methods that Zeisel, Kalven, Fred Strodbeck, Rita James Simon, Thomas Callahan, Dale Broeder, and others used in more than 60 published reports prior to the 1966 jury volume, as well as several jury studies published by Zeisel and others in the two decades after *The American Jury* appeared. Jury simulations resulted in a number of published

studies including Rita James Simon's work on jury responses to different legal standards for the insanity defense. Jurors were the subject of exit interviews much like those used decades later in election studies. Famously, jury proceedings were recorded in Wichita, Kansas, with the consent of trial judges but without the knowledge of the jury. When word of this got out, it resulted in a scandal that produced angry hearings before a U.S. Senate Subcommittee as well as federal legislation and statutes in more than thirty states prohibiting jury-tapping (see Katz 1972). Dean Edward Levi willingly bore the brunt of the Eastland Committee's scorn in the hearings to shield both the project itself and the two principle investigators from further harm.

With all these methodologies and the wide variety of questions investigated, the use of the questionnaire to judges and the comparison of judicial and jury verdicts in jury trials was the central theme in the 1966 study of the criminal jury, and that book is the centerpiece of the jury project. The simple and elegant design Hans Zeisel contributed for this study immediately distinguished the jury project from the sincere but unstrategic efforts of data collection that were characteristic of empirical research efforts at American law schools in the first half of the 20th century. In comparing the judgments of judges and juries, the study was asking a series of questions about the central institutional role of the jury—what difference does it make when juries decide cases rather than judges, in what sorts of cases does it matter most, and why?

While psychologists and students of small groups might be more interested in how juries work, and in how different types of members influence jury outcomes, the basic design of the jury project questionnaire addressed the central difference that the presence of the jury made to the operation of the legal system. This strategy was put in place early in the long research period. It seems to me that this approach made the empirical data from the project into a study of the impact of a legal institution in the way that none of the other behavioral research projects of earlier years could claim.

In emphasizing its research design, I do not mean to pretend that

the jury project was a triumph of central planning. A lack of premeditation was obvious in the schedule and the priorities of the project. The identification of judges who had presided at jury trials and the analysis of the judge's accounts of the trial was a central approach, but resources were expended on a wide variety of other approaches. Many of the research initiatives did not produce useful findings. The simulation exercises under the direction of Fred Strodbeck were not integrated into the substantive discussion of jury behavior even when *The American Jury* was discussing a topic where the simulations had produced a book, such as the insanity defense (see Kalven and Zeisel 1966, Chapter 25). The initial questionnaire effort in 1954-55 had to be supplemented with a second wave of sampled judges in 1958. While some targets of opportunity research were completed fairly quickly—such as the volume on delay in civil cases—the volume on the criminal jury was not published until 12 years after the first sample of cases was collected. Without doubt, the progress of this most expensive single law and behavioral science initiative was trial and error throughout the 15 years from inception to the publication of its culminating volume. Was the project as a whole a success?

Over the 15 years that it operated as a formal program at the University of Chicago, the project produced three books and more than 20 publications reporting original empirical data. But when measured up against the project's announced ambitions, even this scholarly armada fell considerably short of the mark. The three published volumes were three less than had been planned. The volume on the criminal jury took twice as long as had been allotted for it, and the long time in preparation was noted in reviews of *The American Jury* (see Friendly 1966). The civil jury volume was never completed. All of this generated some insecurity about the success of the project 35 years ago, but it seems more than clear in the view from 2003 that the project was a triumph.

When compared to the other efforts to create empirical research programs in law schools, the jury project was a singular success in generating important and methodologically sophisticated research on

questions of basic importance on legal institutions. Where other pioneering efforts spent the majority of their time and treasure gathering vast quantities of descriptive statistics on case flows in legal institutions, business failures, parking behavior, and traffic flows and only then attempted to bring some meaning to their data sets, the jury project was design driven in almost all its research soundings. And the empirical research reported in *The American Jury* is also legally quite sophisticated in both its design and interpretation.

When the natural history of previous attempts at large-scale fact research is consulted, the amazing thing about the jury project is not that it took 13 years to finish its magnum opus, but rather that the two principal researchers had the commitment and tenacity to see the jury project through to its culminating achievement. The jury project went on after the Ford money had run out, fueled by institutional and personal commitments to finish this job. After a brief description of the book that Kalven and Zeisel produced, I will try to explain why perseverance overcame the hazards and frustrations that are the inevitable curse of ambitious law school fact research projects.

The Jury Volume

The central publication of the Chicago jury project was a 500-page account of the questionnaire-to-judges-based study of criminal jury trials conducted in the United States during the 1950s. It is both well written and well organized but can by no means be described as light reading because of the extraordinary detail of the research report. After describing the nature and volume of criminal jury trials and the research methods used in the main questionnaire study, the book first documents the prevalence of agreements and disagreements between judge and jury and then examines the patterns found in different areas of the criminal law, with different characteristics of defendants and their lawyers, and in different evidentiary settings.

The basic findings of the study are straightforward. As the judges tell it, judge and jury agree on the verdict in about three-fourths of all criminal trials. When the judge and the jury disagree, the jury is seven

times more likely to be more lenient than the judge—either acquitting the defendant or convicting of a lesser offense—than to find a verdict that is more severe than the judge would have imposed. Table 12 reproduces one of the two-by-two tables that is the basic method of data presentation in the volume.

Reproduction from *The American Jury*, Table 12
Verdict of Jury and Judge⁶ Consolidated
(Percent of all 3576 Trials)

		JURY		Total Judge
		Acquits	Convicts	
J U D G E	Acquits	14	3	17
	Convicts	19	64	83
Total Jury		33	67	100%

= Judge-Jury Agreement

Even where disagreement takes place, the surrounding circumstances do not show a great gulf between the jury's verdict and the judge's version of a correct legal outcome. Juries are much more likely to reach a verdict contrary to that of the judge when the judge acknowledges that the evidence is close, and the presiding judge

infrequently attributes disagreement with his verdict to the jury's failure to understand the evidence or the law. Cases more likely to produce jury disagreement in a lenient direction are those with a sympathetic defendant, a criminal charge with which the jury is not wholly in sympathy, and superior defense counsel. Typically it is a combination of these jury equities—such as a close case with a sympathetic defendant—that best predicts a divergent jury verdict rather than just one equity factor. But even though the judge and jury are not far apart, the presence of a jury doubles the acquittal rate from what the judges would have produced (see Table 12).

The book's global evaluation of this pattern is the memorable conclusion "that the jury, despite its autonomy, spins so close to the legal baseline" (Kalven and Zeisel 1966, p. 498).

"The jury thus represents an uniquely subtle distribution of official power, an unusual arrangement of checks and balances. It represents also an impressive way of building discretion, equity, and flexibility into a legal system. Not the least of the advantages is that the jury, relieved of the burdens of creating precedent, can bend the law without breaking it" (ibid).

This last and brilliant summation is more than a testament of the verbal genius of Harry Kalven, Jr.; it is also a clear demonstration of how, at nearly every turn in its methodology and analysis, *The American Jury* is legal scholarship, combining a deep understanding of substantive law and procedure with its insights on jury motives and functions. At every level from the choice of the basic issue to be investigated to the particular points of analysis in chapters on self-defense and contributory fault to this concluding tribute to an institution that respects the formal law without being compelled to follow it, *The American Jury* was a volume that could only be written as legal research. When Edward Levi called the jury project "the highest achievement of an approach long advocated but never before accomplished," I think he was referring to a quality of *The American Jury* that transcended social science to become the applied empirical jurisprudence of the American realist ambition.

The Anatomy of Perseverance

The 15-year gestation period of *The American Jury* invites two linked questions: Why did it take so long? And how could the project fight off the frustrations and centrifugal forces that had deconstructed so many other efforts to generate large-scale empirical legal research. The authors of the jury volume addressed the long gestation period in their preface:

“The original program...was to bring together into a working partnership the lawyer and the social scientist; in a phrase we have often used, the hope was to marry the research skills and fresh perspectives of the one to the socially significant problems of the other, and in the end to produce a new scholarship and literature for both....The venture turned out to be more ambitious than we originally anticipated. The collection of the data involved new problems of diplomacy and public relations; the analysis of it was rich in unexpected impasses; and finally, the writing, the development of style to blend legal meaning with statistical measurement proved extraordinarily difficult. In brief, it has taken a long time to get this far” (Kalven and Zeisel 1966, Preface, p. 1).

For the most part, I think this account was plausible in 1966 and remains persuasive a generation later.

A Labor of Love

But why did the collaboration at the center of the jury project persist for the 13 years it took to write the project’s defining book? On a variety of different levels, what held the jury project together was love. The project would not have produced the jury volume unless Hans Zeisel had survived the marginal position of being a social sci-

entist in a law school for well over a decade. How did this happen? It helped that Zeisel thought of himself as a lawyer, but there were still slights both real and imagined that would have tempted this proud and prickly man to escape what might have seemed like second-class citizenship and the frustrations also of being a junior partner in writing the volume. Except that Hans Zeisel adored Harry Kalven and was honored by the gift of his collaboration. For Zeisel, *The American Jury* was literally a labor of love. Kalven enjoyed the challenge of finding coherence in empirical data, and he genuinely loved the jury as an institution, an enthusiasm that was sufficiently contagious to capture Zeisel as well, at least as regards the criminal jury.

There were two other strong bonds that explain the continuing commitment of Kalven and Zeisel to the criminal jury volume. One was the promise of the data. The topic that this volume would investigate was central, and the research was much more fundamental than the other pieces that had been issued by jury project or were scheduled for publication. At least by the early 1960s, the project was energized by the prospect of its potential greatness.

One further loyalty played an important part in the perseverance. Edward Levi had put his reputation on the line for the jury project in the 1955 Senate hearings, in his budgetary priorities, and finally in securing tenure for Hans Zeisel in 1961. One evidence of the strong bond between Kalven and Levi was Edward Levi's heartfelt public memorial to Harry Kalven in 1975, a tribute that ended, "Goodnight, sweet prince." Kalven's loyalty to Edward Levi was unqualified. Zeisel, too, wanted little more than to justify the risks and confidence that Levi had put in the venture.

The jury project was not a peripheral part of the program that Edward Levi had led at Chicago. It was a highly visible gamble on one research project, a project that would be much mentioned in the competitive gossip of American law schools, win or lose. The perseverance that produced *The American Jury* was as much a testament to the social cohesion of the Hyde Park Mafia as to funding streams or the organizational chart of the venture. I know of no other place

and no other time when a major American law school had at its core truly familial relations and loyalties of this intensity. It will probably never happen again.

Legacies and Lessons

A generation has passed since the jury volume was published, sufficient time so that we can trace some of its impact on legal scholarship and the lessons it teaches about the promise and limits of empirical research in the legal academy.

One obvious question is whether the empirical study of jurors and jury behavior that was launched a half century ago has continued. The business of jury research is booming, and our knowledge of jury behavior and the methods to study it have improved in every decade since the jury project launched the enterprise. The majority of modern jury research is done by psychologists, and this has produced more emphasis on simulation and experiment. But legally informed research has also flourished, some of the best of it the product of Hans Zeisel's later collaborations with Shari Seidman Diamond as well as Professor Diamond's more recent work. There is even now a serious effort afloat to repeat an improved version of *The American Jury* research plan. Where legal debates were once embarrassed by a lack of data about the behavior of juries and jurors, modern courts may find themselves embarrassed by convincing empirical evidence about juror attitudes that is inconsistent with the assumptions about jurors that a court majority wishes to make (see e.g. Ellsworth 1988, Zeisel 1973). And jury studies have come full circle from the jury bugging scandal when Shari Diamond and her collaborators recorded Arizona jury discussions with the consent of court and jurors (Diamond, et al 2003).

But what about the empirical study of law and legal institutions in American law schools? On this question, the view from 2003 presents a rather mixed picture. On the one hand, the number of law professors with a sophisticated background in social science research methods has grown explosively over the past three decades. There is

an electronic law school service devoted to empirical studies of various kinds (Arlen) and the *Journal of Legal Studies* has been publishing empirical work of many kinds for 30 years. On the other hand, no law school based research enterprise of the generation just past has approached the scale of the jury product in human and fiscal resources, and none is likely to in the foreseeable future.

The scale of a teaching law faculty and the number of senior researchers required for a major research program in any subject are quite different. A law school faculty is constructed with the demographic profile of Noah's Ark, assemble two (or at most three) experts in every one of the wide range of subject specialties and you have a faculty of 40, but you don't have any clusters of specialists that could be the nucleus of a large research program.

One could of course have non-faculty research specialists as permanent staff in a law school, but in the status hierarchy of the modern law school, the non-faculty fellow is either young and in transition to faculty status or a permanent second-class citizen. Without very large external funding and the equivalent of long term research endowments, the upper limits on the scale of empirical research are pretty strict. There is a glass ceiling to the size of the research enterprise, and it is disturbingly close to the floor.

The only way to gather a critical mass of research scholars in one school for a jury-project-size endeavor would be to center the research activities of many professors who teach at different schools in one central research site (Zimring 1985). Even then, big projects are quite difficult to mount.

Even relatively small scale data gathering is expensive and takes a long time. In every university department, it is much easier to build regression models with available secondary data and crunch numbers than to create data through surveys, through experiments, or through systematic observations of historical records or contemporary events. Further, the social science and humanity departments of research universities are better organized than law schools to support many empirical research efforts of large scale. Is there any reason, then, to

have an empirical research capacity located in the law schools of our major research universities?

Yes there is. The important reason why the research university needs a law school with empirical research capacity is that many research projects require a mix of legal sophistication and empirical research skills. The criminal jury study is Exhibit A on this issue. The judge versus jury comparison proceeds to the heart of the matter, the distinctive functions and values of this nonprofessional group in the criminal trial. *The American Jury* by Kalven and Zeisel is legal research in every detail, the kind of work that cannot be done in a sociology department.

Let me give two further examples from my own Chicago adventures in fact research. Thirty-five years ago, I launched a study of the influence of weapon dangerousness on the death rate from assault when I came across the major sociological book on homicide asserting that the dangerousness of a weapon should not matter much because so many weapons will kill if the attacker really desires his victim's death (Wolfgang 1958). That argument seemed to me a rather heroic assumption to make about homicide because the sociological study had only been examining cases where the victim died. But I also knew that the criminal law standard for malice in murder was quite far below requiring the intent to kill, and instead required only the intention to risk great bodily harm. With that kind of *mens rea* standard, there might well be an extraordinary overlap between attacks that kill and those that do not, in which case the dangerousness of the weapon used could have a large impact on the death rate. That turned out to be the case (Zimring 1968).

A second University of Chicago adventure involved a program that diverted defendants from the early stages of the criminal process into job training and placement efforts. If the defendant was not rearrested, the charges were dropped. The people who designed the program assumed that it was an alternative to a criminal record and punishment, but system insiders know that non-serious criminal charges often produce neither convictions nor punishments. So we tracked

the cases carefully and found out that, for most of the defendants, the program they entered was a larger involvement with the social control system that would have happened if their cases had run the standard route through the system (Zimring 1974).

These case studies are examples of the comparative advantages that the legal scholar brings to empirical research—knowledge of legal process and expertise in the substance of law. This is what makes the legal historian instead of the historian, the legal scholar doing survey research instead of the survey sociologist, and the legal scholar studying deterrent effect (Zimring, Hawkins, and Kamin 2001) into the kind of expert who can ask the right questions and structure the collection of data so that they provide clear answers.

This worldly knowledge of the law professor helps shape empirical research in two important ways. The legal expert can ask the right questions more often than the non-expert, the question that is most important in theory, and most important in practice. This is of critical importance because there is no organized market for legal research that determines questions to investigate or methods to pursue answers. So much that becomes the subject of scholarship, both empirical and theoretical, is idiosyncratic to the tastes and interests of the principle investigator. If Ronald Coase had not left England, nobody would ever have done his studies of payola in the record industry. If Richard Posner hadn't entered the academy, who would have written *Sex and Reason*? The selection of topics for legal research—empirical or not—is a product of a supply-side process rather close to anarchy. The law professor can serve as a good judge of both the relevance and materiality of questions for empirical research.

And that same legal sophistication can inform the methods of empirical research. Here again, *The American Jury* is a stunning example of the right method for the question. The marginal importance of the jury is best approached by comparing outcomes produced by that body with those that would have been delivered by the alternative fact finder provided in the legal system: a judge. Once that

thought experiment is in mind, the questionnaire to judges who happened to preside over trials with juries is a natural and almost inevitable research strategy.

So the good news is that the need for empirical research in legal scholarship is alive and well. The bad news is that the scale of the research enterprise is severely constrained by the small number of specialists of any kind, and the other economic limits of the modern law school. The American law school is an unlikely host to any non-teaching research professor.

So where are the trained empiricists to be found? Increasingly, this question is answered by the broad interdisciplinary training of the modern professor of law. Turn the law faculty upside-down at a modern research university, and more than a handful of professors turn out to have extensive training in history, economics, sociology and other disciplines that have fact gathering traditions. But these law professors are legal scholars as well, with the exposure and training to ask the right questions and often also the ability to design appropriate tests for the critical questions they have posed. In seeking the jointly trained lawyer with capacities for empirical research, we are turning to the combination of attributes that made Hans Zeisel indispensable to the jury project. The empirical scholar with a law degree is no longer a deviant case in the modern law faculty.

And there are a host of questions that provoke empirical study. Are juries where all opponents of the death penalty have been removed more likely to vote for the prosecution? Do the intestate succession rules currently used reflect the distributional wishes of most adults? Did the liberalization of abortion rules contribute to the crime reductions that were a major development in the 1990s? Do laws requiring the safe storage of handguns increase crime rates in states that pass them? Does a joint custody status increase the participation of a father in the lives of his children? These are all the province of modern law-school-based research. Some of the published research is terrific; some of it is terrible. But it is all within the extremely wide mainstream of 21st century research in the legal

academy. To paraphrase Grant Gilmore, we are all empiricists now (Gilmore 1979).

* * *

The jury project was the first great success in empirical research at a law school, but what was its long-term influence on the life and perspective of the Chicago Law School? Certainly it has not had the impact on research that the law and economics paradigm has produced at Chicago or elsewhere, because empirical findings often do not generalize nor do they have pervasive implications for the choice of legal rules. Indeed, belief in empirical research is not really much of an ideology. While empiricism may have once served some of its proponents as a passionate rebuttal to the law library's monopoly dominance of legal research, there is no novelty now in the notion that factual evidence is relevant to legal policy choices. Empiricism is too contingent and too eclectic to serve as an inspiration for partisan academic crusades.

But the values and insights of exemplary empirical research have influenced the generations that grew up in proximity to the jury project. My friend and classmate, John Henry Schlegel, J.D. 1967 at the University of Chicago, wrote a pessimistic history of empirical research in the legal academy that was published a decade ago (Schlegel 1993). I wonder whether his interest in the topic wasn't kindled by his being a law student at Chicago when *The American Jury* was published? If so, his book is circumstantial evidence against its conclusion.

In my time at Chicago, the jury volume was a subtle but important influence on the school's self image. Three such different enterprises as *The Jury Project*, the Center for Studies in Criminal Justice (founded in 1965), and the *Journal of Legal Studies* (founded in 1972) helped each other and also benefitted from many common influences at the Law School. The Chicago perspective of what constituted legal research was broad and varied in 1966, and that is one reason why the range of approved research is broad today every-

where. All of these precedents have also become elements of the research image that young scholars carry away from the school even now, and at many other schools. The jury project provided a broadening of the definition of legal scholarship for many of those who took their training on law here in the shadow of the jury project.

A Bibliographic Icon

But the influence of *The American Jury* has been broader than as an example of empirical research. One cannot discuss the question of the impact of the jury project on this school without addressing the fundamental difference between the Chicago Law School and other schools. The distinctive feature of the Chicago Law School for the last half-century has been its fanatic dedication to the importance of scholarship in the life of a law school. The uneasy role of law schools (and some other professional schools) in research universities was the result of the restricted nature of legal research as well as ambivalence about the importance of scholarship in the professional school. Not at Chicago.

And the commitment to scholarship at Chicago was in no sense a matter of one style of inquiry having hegemony. The religion of the Chicago Law School is scholarship, and excellence in one form of scholarship is complementary to excellence in other forms. The collective commitment to the next book and the next article is what every visitor notices in Hyde Park and what every Chicago veteran misses at other schools. From this perspective, Richard Epstein and Cass Sunstein are not two sides of the same coin, they are the same side of the same coin, united by the enormous energy they invest in scholarship and in the belief that produces such energy, the belief that ideas and arguments and data matter.

The American Jury functions as an iconic book in the one law school on the planet most clearly identified with the supremacy of scholarship. This commitment to scholarship was one reason the book was finished. Its completion added to the growing identity of Chicago as an institution that draws its energy from the ambition to

produce great work. The jury project was both the product of the University of Chicago as a biblio-centric law school and one of its greatest inspirations. *The American Jury* became the most prominent single achievement of Chicago's great faith in the proposition that research matters. At the end of the day, that faith is the genius of the place.

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