The Uses of Sociology in the Professions: The Law

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The law, from one view, is a continuous process of synthesizing facts and rules, with new facts at times engendering new rules. The process takes place wherever law is made: in the legislatures, in the administrative agencies, and in the courts.

The facts reach these lawmakers in a variety of forms. The primary source is still the witness who reports on his own, private experience. Occasionally, however, facts are presented as cumulative knowledge, systematically gathered through surveys and most recently also through experiments, methods that are part of the tool chest of the social sciences.

The survey as a source of facts for the law predates modern social science by centuries and constitutes in fact one of its major historical roots. But it is only in recent years that the law has begun to use research operations conducted with technical rigor. The uses the law has made of such systematic investigations differ widely, from simple citation in a brief or opinion to being the decisive ground for a judgment or a legal reform. The great majority of the studies that come before the law raise only private issues, assisting courts and agencies in individual litigation. They may be surveys of the quality of contracted goods, of the geographic range from which a drive-in theater draws its clientele, or the commercial effects of a merger, or of the socioeconomic structure of the jurors in a certain community. But although such studies are at times gems of technical perfection and ingenuity, they will not be discussed here. Their variety is too great, and they seldom reach the higher courts, hence they seldom affect legal rule making; and they have been sufficiently discussed elsewhere.

The studies we shall discuss here deal with more general problems: with substantive rules of law, with procedural rules, or with institutions that are a mixture of both. Some of these studies are broad surveys of a legal institution without more specific focus; they enter the stream of legal resolution only slowly, as but one of the many sources that shape the law. Other studies, in contrast, are designed to illuminate if not to resolve one narrow, crucial issue; these investigations—sometimes they are controlled experiments—are bound to affect the

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the beginning. It should establish a solid basis for confidence in its work by superior craftsmanship in technical and noncontroversial parts of the law. But it should not be too modest. It should hope to become in time a source of perspective and wisdom, and even of bold proposals, on reforms in the law that may take a long time in coming and that may require the kind of perseverance that led to the new Judicial Article. It should expect to produce not only proposed legislation of matters appropriate for immediate action, but thoughtful reports on emerging problems that may not yet be ripe for final recommendations. It should aim to inform, and to elicit wide interest and debate, not only among the members of the profession but also in the public at large. We should judge its success not solely by its batting average in each session of the legislature, but by the extent to which it builds a record of reasoned statements that can constitute a foundation for the progressive development of the law.12

As Sir Leslie Scarman, the chairman of the English Law Commission, has reminded us, law reform is in many respects too serious a matter to be left to the legal profession. The profession must be on guard not to exaggerate its claim of competence to decide what is good for society. But our society has been in the habit of looking to lawyers for guidance. I am sure we believe that the habit should be encouraged and the faith vindicated. In Sir Leslie's words, "The real test for law reform and . . . for the legal profession . . . is whether we as professional men are prepared to lay our hands to the reform of questions of law which profoundly affect interests of great importance to society."18

It would be good to be able to believe that the old suggestion of a Law Reform Commission is an idea whose time has come. Surely there is no project with greater promise of lasting benefit to our legal system than an association such as ours might seek to advance. I think I can assure you that in any such effort this Association would have the warm support and cooperation of the law schools and the academic lawyers of this State.

FOOTNOTES

1. See 48 CHICAGO BAR RECORD 140 (1967).
11. SEE FOR BETTER HOUSING IN ILLINOIS, Report of the Legislative Commission on Low Income Housing, 75th General Assembly (1967).
12. An excellent example of the kind of study that is needed in many areas is the admirable report on Illinois antitrust law prepared by a committee of the Association under the chairmanship of Robert W. Bergstrom, Esq., which laid the basis for a new and greatly improved state antitrust statute. THE LAW OF COMPETITION IN ILLINOIS, A Study by the Special Subcommittee on Illinois Antitrust Laws of the Chicago Bar Association (v. 2 of ILLINOIS LEGAL SERIES, 1962). See also note 1, supra.

(Continued from page 6)

law more directly.

But all the studies on which this essay will report have one thing in common: they were made for the purpose of being used by the law and in many instances have affected its course.4

We shall report these studies in the order of the spectrum that ranges from the narrowly but sharply focused controlled experiment to the broad, diffuse survey.

Controlled Experiment

There is no more powerful tool for assessing a legal innovation, or any innovation for that matter, than the controlled experiment. But since its essence is to apply a rule of law to one group of cases and to withhold it from another, such purposeful discrimination would seem at first to violate the equal protection guaranty of the Constitution.5 Indeed, there are substantive limits to legal experimentation; there can be none that involves withholding of a right, guaranteed under all circumstances. It would be impossible, for instance, to insist that some criminal defendants be tried without counsel in order to find out what effect counsel has. But there are several reasons why, outside this rigidly protected sphere, the law should permit experimentation. First, the experimental discrimination is by definition temporary; second, the discrimination is applied impartially, by lot; third it is the very purpose of the experiment to learn what, if any, effect the discriminating rule would have: hence, at the outset, one cannot even be certain that there is discrimination; and fourth, the ultimate aim of the experiment is to eliminate the rule if it should be found to discriminate unfairly.

Controlled legal experiments, not surprisingly, have largely been confined to rules that convey privileges rather than rights.

The first controlled experiments within the precincts of the law were probably conducted by the Adult Prison Authority of the State of California, which tried to assess the effectiveness of a variety of prisoner treatments. The most daring of these experiments was an effort to deter-
mine what happened if prisoners were released nine months before their appointed time; specifically, whether such a premature release was likely to increase the rate of recidivism.6

Not much came of these experiments, partly perhaps because the recidivism rate is too brittle a measure of effectiveness since, in order to be counted as a recidivist, it is not sufficient to have committed another crime; it is also necessary to be caught and reconvicted. And since the odds of being caught, as revealed by the published statistics, are on the average about one in five, this ratio might well have a great variance and hence be a very unstable measure.7

A controlled experiment that allowed of precise measurement and had an immediate effect on the law was conducted in the state courts of New Jersey.

In most of our courts some or all of the civil suits, before they come to trial, are scheduled for what has become known as pretrial. There, counsel for both sides, occasionally with their clients, meet with the judge to present briefly the issues under dispute and air the possibilities of settlement. Tradition has it that these pretrials, aside from preparing and facilitating the subsequent trial, increase the rate of settlements prior to trial. The institution has, therefore, been considered a most desirable means of reducing the trial load and thereby the intolerable congestion of our metropolitan courts. Since many cases, the trial of which would have lasted two days on the average, are settled during a half-hour pretrial conference, this notion seemed well supported.

But analysis of available statistics made the point doubtful; there were indications that the cases settled at pretrial would have been settled also without it and that the court time spent on pretrying cases might be wasted. The precise answer, it was suggested, could, come only from a controlled experiment which pretrial a random sample of cases and omitted pretrial in a comparable control group.8

At that time the New Jersey courts had a rule that made pretrial obligatory, and the state's distinguished Chief Justice and its Court Administrator, becoming sympathetic to both the query and the proposal, commissioned Professor Rosenberg, then Director of the Project for Effective Justice at Columbia University, to conduct the experiment.9 The design called for random assignment of cases by the clerks of the respective courts to two alternative procedures: to obligatory pretrial in one group of cases, and to optional pretrial in the control group, where it would be held only if one or both of the litigants requested it.10 Accordingly, 2,954 cases were assigned at random alternatingly to the two groups, for which the settlement ratios shown in Table 4-1 emerged.

Table 4-1.

<table>
<thead>
<tr>
<th></th>
<th>CONTROL GROUP: OBLIGATORY PRETRIAL</th>
<th>EXPERIMENTAL GROUP: OPTIONAL PRETRIAL</th>
</tr>
</thead>
<tbody>
<tr>
<td>Suits settled before they reached the trial stage</td>
<td>76%</td>
<td>78%</td>
</tr>
</tbody>
</table>

There was no difference. In addition, the experiment failed to confirm a subsidiary hypothesis, namely, that the pretrial, and hence prepared, cases required a shorter trial time. Thus the conclusion emerged simple and clean: contrary to a widely held belief, obligatory pretrial did not save court time, but in fact wasted it. Persuaded by the experiment, the State of New Jersey forthwith changed its rule and made pretrial optional.11

But the legal experiment that had the most profound and sweeping effect on the law was conducted in the criminal courts of Manhattan. It revolutionized one of the most solid traditions in the criminal law: the practice of setting bail for defendants arraigned in our criminal courts. Bail is set, as a matter of constitutional right, for nearly all defendants; if they can post it, they are set free; if not, they must remain in jail. Whether or not they can post it depends as a rule on the bondsman, who, against a premium of some 10 per cent, will or will not take the risk of providing the demanded bail. Only rarely is a defendant allowed to go free without posting bail.

The system has been heavily criticized because it favors the well-to-do, surrenders the actual decision to the bondsman, and keeps an inordinate proportion of defendants in jail, some of whom are subsequently acquitted. The system, nevertheless, withstood all criticism until the Vera Foundation made known its findings from a unique experiment which it conducted in 1961. With the cooperation of the New York judiciary and the New York University Law School, all defendants arraigned in the felony court of Manhattan were interviewed so as to assess the risk of their failing to appear at their trial, if the court were to free them without requiring bail. On the basis of these interviews the defendants were classified into two groups: those for whom a release without bail could be reasonably recommended to the court and those for whom such a recommendation could not be made. The recommendable group was then divided into two random halves: the experimental group, for which the recommendation to release the defendant without bail was actually transmitted to the arraignment judge, and the control group, with respect to which the judge was told nothing and thereby left to his own traditional mode of making the bail decision. In this latter group only 14 per cent of all defendants were freed without bail, as against 60 per cent in the recommended half. The hypothesis was that at the time of trial, from the group of which 60 per cent had been freed without bail, more
would fail to appear in court than from the group where only 14 per cent were free without bail, and the question was: how many more? When trial time came, only 1 per cent of all defendants released without bail, whether recommended or not, purposely failed to appear in court. The experiment thereby proved that the number of defendants released without bail could be quadrupled without reducing their availability at the time of trial.\textsuperscript{12}

The results of the experiment were stunning. The City of New York took over the interviewing from the foundation and established it as a permanent service. The Attorney-General of the United States convoked a conference on the topic, and today almost all major cities and many rural areas have adopted the Vera procedure, and with it the liberalized practice of release without bail. And the Department of Justice left no doubt as to where the credit belonged: "Of particular significance is the fact that these changes have flowed not out of a crisis . . . but rather from education, through empirical research and demonstration."\textsuperscript{13}

The secret of the success of this experiment was two-fold. First, except for the bail bondsmen, everybody stood to gain from the liberalization: the municipal jails saved money; the defendants themselves were spared unnecessary hardships; and last, but not least, the ends of justice were advanced. Secondly, the numerical result of the experiment was so clear that no probability calculus was needed for its appreciation.\textsuperscript{14}

The Natural Experiment

Sometimes administrative routine will present the investigator with a natural experiment, that is, with an experimental and a control group that were not purposely designed by him. Within limitations, this may be quite satisfactory, as the following case history shows.

Under traditional trial procedure, the plaintiff in a civil case first presents his case both as to liability and size of damages and is followed in turn by the defendant, who presents his side of the case. After both have had their say, the jury retires and decides whether the defendant is at all liable for damages and, if so, how large these damages should be. The question of damages thus becomes relevant only if liability is found. The suggestion was made to split the trial and to limit evidence and argument in the first part of the trial to the liability issue, asking the jury to decide whether the defendant owes anything at all. Only if this decision is affirmative does the trial proceed with the evidence and subsequent verdict on the size of the damages.\textsuperscript{15} Since liability is affirmed in only a little more than half of all cases, this mode of trial was expected to save something like half of the trial time normally spent on damages. The Federal District Court for Northern Illinois was sufficiently intrigued by this split-trial idea to try it out and to ask the University of Chicago Law School to help in assessing the effect of the split-trial rule, as it has come to be called.\textsuperscript{16}

The court had adopted the rule in a form that left it to the discretion of the individual judges whether they wanted to apply it in the particular case. It was from this discretion that, seemingly, the difficulties, but eventually the salvation of the experimental design, arose. If each judge could apply the rule in some cases and not in others, and if he were to select—as in fact he often did—only those cases for split trial which, in his view, promised some gain in time from the application, the cases tried in the regular mode and those tried under the split-trial rule could not be compared. Whatever difference in trial length might be found between the two could not be attributed to the new rule, because the cases were admittedly different to begin with.

At first glance this lack of random assignment would seem fatal. Yet, while it made the analysis more complicated and less powerful, it did in fact make the experiment possible. Since the original assignment of cases to the individual judges was made randomly, the inference was allowed that the cases coming before Judge A did not differ from the cases coming before Judge B. And then something fortunate happened. The discretion of the judges resulted in an effective spread of the experimental stimulus: some used the rule in almost all their cases, some in hardly any, and some in varying proportions between.

If, then, it were true that the application of the split-trial rule saved time, the judges who applied the rule more often should require on the average less trial time than those who applied it less often. This turned out to be true, as Table 4-2 shows.

### Table 4-2. Proportion of Split Trials and Average Trial Time in Personal Injury Cases

<table>
<thead>
<tr>
<th>Judge</th>
<th>Proportion of Cases Tried Under Split-Rule (Per Cent)</th>
<th>Average Length of All Trials Before This Judge (Days)</th>
<th>Number of Trials Before This Judge</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>89</td>
<td>3.2</td>
<td>(26)</td>
</tr>
<tr>
<td>B</td>
<td>51</td>
<td>3.3</td>
<td>(41)</td>
</tr>
<tr>
<td>C</td>
<td>38</td>
<td>3.5</td>
<td>(26)</td>
</tr>
<tr>
<td>D</td>
<td>14</td>
<td>3.8</td>
<td>(22)</td>
</tr>
<tr>
<td>E</td>
<td>7</td>
<td>3.9</td>
<td>(27)</td>
</tr>
<tr>
<td>F</td>
<td>7</td>
<td>4.3</td>
<td>(14)</td>
</tr>
</tbody>
</table>

*Only judges with more than 10 trials are included.*
The regression line based on these data indicated that at the point where a judge conducted all trials under the split-trial rule, his average trial time was about 20 per cent below the point where none of the trials were split. This, then, was the magnitude of the time that could be saved through application of the rule.

But since the variation in the stimulus was not random, but self-selected, it was desirable to provide supporting evidence. It was clear that whatever savings there were must come from the elimination of the damage trial. The frequency of damage trials was, therefore, determined both for the regular and for the split trials. (See Table 4-3.)

<table>
<thead>
<tr>
<th>Table 4-3. Disposition of Regular and Split Trials</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>REGULAR TRIALS</strong></td>
</tr>
<tr>
<td>(%)</td>
</tr>
<tr>
<td>Complete trial on liability and damages</td>
</tr>
<tr>
<td>Trial ended after liability verdict because verdict was for defendant</td>
</tr>
<tr>
<td>because damages were settled after verdict affirmaing liability</td>
</tr>
<tr>
<td>Other dispositions (settlement during trial, directed verdicts)</td>
</tr>
<tr>
<td>100%</td>
</tr>
</tbody>
</table>

The dispositions of the two groups of cases were drastically different: 76 per cent of all regular cases went through a complete trial, against only 15 per cent of the separate trials. As for the latter group, 58 per cent were spared trial of the damage issue because of the intermediate verdict denying liability; in 43 per cent of the cases the trial simply ended when liability was denied, and in another 13 per cent of the cases there was no trial of the damages because they were settled after the jury had affirmed liability.

The split trial is a radical innovation in American law, brought to the fore by the pressures of court congestion. When the study was first published, the Joint Committee on Effective Administration of Justice, on suggestion of its chairman, United States Supreme Court Justice Clark, had copies sent to all trial judges in the United States; as of this writing, several state and federal courts have moved to authorize their judges to use the split-trial procedure, and the institution is likely to gain more ground.17

Experiment Under Seminatural Conditions

If an issue falls into the constitutionally protected area, not even a natural experiment is likely to occur, and a simulated one must suffice. But even then, as many natural components as possible must be retained, as for instance in the following series of experiments concerning the defense of insanity. They were designed to test jury reaction to certain variations in the law. The natural element in these experiments was the juries, summoned by a trial judge from the jury pool of his court with the request to partake in the experiment and to deliberate on the case as if it were a real one.18

In Anglo-American law the defense of insanity has been embodied for more than a century in the so-called M'Naghten rule, which calls for an acquittal if the defendant either did not know what he was doing or did not know that what he was doing was wrong. Recently, the rule has come under criticism, primarily from psychiatrists. In 1954 the Federal Court in the District of Columbia established a new rule in a case in which one Durham was indicted, and subsequently acquitted, on a charge of burglary. The Durham rule considers the defense as established if the criminal act can be shown to be the "product of a mental disease or a mental defect." It became thus a point of major interest for the criminal law to find out what if any difference it made to the outcome of a trial whether insanity was defined under the M'Naghten or under the Durham rule.

The "law" in a criminal jury trial becomes operative primarily through the judge's instruction to the jury before it begins deliberation. In that instruction the judge spells out the circumstances under which the jury may find the defendant insane. In a way, then, the question as to what difference the law makes means what difference it makes to the jury whether it is instructed according to the rule in M'Naghten or in Durham.

To compare the insanity cases in the District of Columbia with cases from a court that operates under the M'Naghten doctrine could provide only unsatisfactory findings, since not only the rule of law but also the cases, the juries, and the judges are likely to be different. And it is obviously impossible to decide the issue through a controlled experiment under completely natural conditions. Therefore, an experiment had to be designed that combined natural with laboratory conditions, sufficiently realistic to justify confidence in its validity.

Two trial records were composed: one, a case of housebreaking, a simplified version of the original Durham trial; the other, an incest case, also an abbreviated version of an actual trial. In both trials the accused's only defense was insanity. The trial evidence was acted out, with the other elements of the trial put on recording tape. Of each case, three main variants were produced.19 The tapes
were identical but for that part of the judges instruction that dealt with the defense of insanity and for the concomitant psychiatric testimony. In one version the instruction and psychiatric testimony were according to M'Naghten; in the second, according to Durham; and in the third, the instruction left it in fact to the jurors own judgment as to whether the evidence in the case supported a defense of insanity, forcing the jury to establish its own law of insanity.

Each of the three versions was then taken into two metropolitan courts and presented in turn to more than a hundred juries. A judge called these jurors into his courtroom and asked them to co-operate in the experiment; by so doing, he advised them, they would oblige the court and also discharge their present turn of jury duty. The jurors then listened to the taped trial and afterward deliberated and arrived at a verdict. Table 44 shows the outcome of the experiment in terms of the jurors' vote on their first ballot, prior to the beginning of the deliberation.20

<table>
<thead>
<tr>
<th></th>
<th>M'NAGHTEN</th>
<th>DURHAM</th>
<th>NO RULE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Incest case</td>
<td>24</td>
<td>36</td>
<td>34</td>
</tr>
<tr>
<td>(240)</td>
<td>(312)</td>
<td>(264)</td>
<td></td>
</tr>
<tr>
<td>Housebreaking case</td>
<td>57</td>
<td>65</td>
<td>76</td>
</tr>
<tr>
<td>(120)</td>
<td>(120)</td>
<td>(120)</td>
<td></td>
</tr>
</tbody>
</table>

In both trials, the Durham rule elicited a higher percentage of acquitals by reason of insanity than the M'Naghten rule. That the percentages under Durham are very close to those obtained under the "No Rule" instruction suggests, furthermore, as indeed it has been argued,21 that Durham comes close to being no rule.

The figures show that the incest case allowed a sharper differentiation between M'Naghten and Durham than the housebreaking case. This difference is instructive beyond the specific issue. The defendant in the incest case was an officer in a city's fire department, with an excellent record, who, except for the crime in question, had never shown any signs of abnormality. The defendant in the burglary case, on the other hand, much like the original Durham, had been in and out of mental institutions and hence had shown, by whatever legal or common-sense rule, signs of insanity. The fireman could be found insane only if the jury was instructed (as Durham allows) to consider the criminal act itself as a symptom of insanity.

The experiment raised two questions of general significance. One applies to the realism of simulation; the other, to the degree of generalizing from experimental findings.

In this experiment there were two simulated elements: the experimental stimulus was greatly reduced (a two-day trial condensed into an hour), and jury deliberation was clearly a mock procedure without consequences in the real world. As to the second point, there was considerable reassurance: these were real jurors called to duty by a real judge; they discharged their responsibilities with such obvious zeal and honesty that deliberations lasted up to ten hours, often engendering high-pitched battles among the jurors who, at times, ended in a "hung jury."

The other point raises a more serious problem. A tape recording of a trial, condensed to about an hour's length, may be something quite different from a full-blown trial, in which the jury not only hears but also sees over a period of many hours real people with all the significant details of their reactions. Without additional research, the point allows of no precise answer.

As to the consequences of the insanity experiments, one can at this stage only venture a guess. On the whole, the experiment should strengthen the hand of those who oppose the Durham rule, simply because its message to the jury is ambiguous. Whatever the shortcomings of the traditional right-wrong test, its criteria are clear and can be applied by the jury.

The jury, in spite of its deep constitutional roots, has been the topic of perennial debate, with little precise knowledge to support it.

Some years ago, the University of Chicago Law School began a large-scale study of the jury system, and one of its key questions was: What difference would it make if all jury cases were tried only by a judge sitting without a jury?

The question would seem to demand a controlled experiment—every case to be tried twice, once with and once without a jury—an obviously impossible solution. Equally impossible it would be to assign cases at random to jury and judge, since this is a choice no defendant must be deprived of. Nor would the simple comparison of actual jury verdicts with actual judge verdicts help, even if limited to trials of the same type of crime, because we know that the cases in which the defendant waives a jury are quite different from those where he wants one.

Curiously enough, the design eventually adopted for the study came close to the ideal design, the controlled experiment. A nationwide sample of trial judges reported for a specified time period on all the jury trials over which they presided. Each judge told us how the jury decided the case and how he, the judge, would have decided it, had he sat without a jury. The design thus made use of the fact that every case is tried twice, albeit simultaneously: once before the jury and once before the presiding judge, who, if there were no jury in the case, would have to render the judgment.

This research design is but a natural controlled experiment of a special order. Experimental stimulus (the jury) and control (the judge) are present in every case, but unlike a planned experiment, there is, except for the
judge who may preside over several trials, no replication. The jury changes from trial to trial; and most important, the case, too, of course, is never the same. The statistical precision of such an experiment is relatively low. But this lack of precision is the price for an unusually broad focus: the study surveys the whole spectrum of cases that come before the American jury. In this sense, the research design may fittingly be called a survey experiment.

From these data it was possible to determine how often judge and jury agree or disagree; and when they disagree, one could trace and count the reasons for their disagreement since through some fifty-odd questions most detailed information became available on every case.

The first part of this study on the role of the jury in criminal trials has just been published. Figure 4-1 reproduces one of its basic findings.

<table>
<thead>
<tr>
<th>Jury</th>
<th>Acquits</th>
<th>Convicts</th>
<th>Hangs</th>
</tr>
</thead>
<tbody>
<tr>
<td>Judge</td>
<td>13</td>
<td>2</td>
<td>1</td>
</tr>
<tr>
<td>Convicts</td>
<td>17</td>
<td>63</td>
<td>4</td>
</tr>
</tbody>
</table>

= Agreement
Number of Trials (3,576)

![Figure 4-1](image)

**Figure 4-1. Agreement and Disagreement between Jury and Judge in Criminal Trials**

Judge and jury agree in \( (13 + 63 =) \) 76 per cent of all cases; and of the 24 per cent disagreement cases, the jury is found on the defendant's side in \( (17 + 4 =) \) 21 per cent and on the prosecutor's side in \( (1 + 2 =) \) 3 per cent of these cases.

The evaluation of the jury as an institution hinges, of course, not only on the extent of its disagreement with the judge but on the reasons that produce these disagreements, and it is the presentation and analysis of these reasons that form the main body of the study. They range from different sentiments on the law which the jury entertains (in spite of what law the judge may give them) through sentiments concerning the particular defendant, different views on the weight of the evidence, and occasionally to an imbalance between the performance of the prosecutor and defense counsel in the trial; a few times it will even happen that the disagreement arises from a discrepancy between what jury and judge know about the case.

These are the five somewhat abstract categories in which the specific causes of the various individual disagreements were ultimately summarized.

At this early point it is not possible to assay the practical consequences of that study. At a minimum, it will lend focus and precision to an important debate in which both sides so far have never been able to draw on more than anecdotal support.

**Secondary Analysis**

Two studies of contemporary legal problems are distinguished by their being primarily reanalyses of data collected in normal administrative routine: one concerned the alleged deterrent effect of capital punishment, the other the problem of court congestion and delay.

In the debate over the merits of capital punishment, the abolitionists had for a long time no good answer to the claim that the death penalty helped to deter would-be murderers. The change came after Thorsten Sellin investigated the problem. He compared homicide rates before and after abolition in some jurisdictions; before and after reintroduction in others; and in jurisdictions that have the death penalty with adjacent jurisdictions that have abolished it. His data made one point clear: whatever other merits the death penalty may have, it has no traceable deterrent effect.

Sellin's data have been quoted wherever the issue is being argued. To what extent they have been an effective cause of abolition is nevertheless difficult to say. Probably they have not been a major cause; growing revulsion from deliberate killing and the actual or near execution of an innocently convicted man have nearly always provided the major impetus. But Sellin's data have helped to silence, if not convince, a special opposition.

The study of court congestion opened insights into a problem that is less dramatic but in the long run perhaps more persistent. It is one of the puzzling aspects of our judicial system that the adjudication of civil claims in most of our metropolitan courts is scandalously delayed. In Chicago, for instance, it takes on the average five and a half years from the date a claim is filed until it can be tried before a jury.

In 1957, the University of Chicago Law School published a study of this congestion problem which had this methodological distinction: its more than three hundred pages of measurement and analysis were based almost entirely on data that had become available in the course of routine housekeeping by the courts in their normal administrative business. From these data a number of measurements and parameters were developed that were to acquire some currency in the administration of the courts: a basic formula for measuring delay was developed, a variety of remedies was evaluated, and, as the case may be, rejected, recommended, or suggested for further investigation. Among the latter were the pretrial and the split trial discussed above.

With the ever mounting costs of securing primary data, this harvest from secondary analysis holds a promise for social research generally.

**Surveys**

Although, as we have seen, the border line between experiment and survey is not a sharp one, it is useful to dis-
tistinguish the two, especially since, in contrast to the experi-
ment, the primary function of the survey is to provide
description. Out of the rapidly growing number of sur-
veys undertaken to give guidance to the lawmakers, only
two will be mentioned here. Both deal with most acute
legal problems; one with the enforcement of civil-rights
legislation, the other with the costs of automobile acci-
dents.

In 1962 Blumrosen and Zeitz began to investigate the
operation of New Jersey's antidiscrimination laws and its
Civil Right Commission. Blumrosen examined all cases
filed with the commission during the fiscal years 1962-
1963, and Zeitz made a survey among Negroes on their
attitudes toward enforcement of these laws.37 The main
finding of their study was that "the laws of New Jersey
against discrimination were not meaningfully and effec-
tively enforced," partly because the Negroes themselves
shied away from individual enforcement and partly be-
cause of shortcomings in the commission itself. The study
had a number of traceable effects: the legislature substan-
tially increased the commission's budget; the commission
itself sharpened some of its policies and much of its mode
of operations; and the state's attorney-general would
speak of his civil-rights division as having been "a shield"
so far but now to become "a sword."38

The survey on the costs of automobile accidents is the
latest on an issue of long-standing concern to the law and
has a distinguished research history. The first major
study was published in 1932 under the auspices of the
Columbia University Committee for Research in the So-
cial Sciences.39 Like the latest, the Michigan study, it too
was a joint effort of lawyers and social scientists. It sur-
veyed broadly, if somewhat haphazardly, the repair
problem caused by automobile accidents. The evidence
was drawn from a variety of sources, but even where sur-
vey data were used, no claim was made to precision and
completeness. Nevertheless, as in many first approaches,
the outlines of the problem and the areas of research
emerged with great clarity and thus marked an important
beginning. The next step came in 1953 when Professor
Adams of the Business School at Temple University
studied the financial and legal history of a random sample
of one hundred automobile accidents in the City of Phila-
delphia and thus established the pattern for later efforts.40
The Philadelphia survey displayed all the glories and
some of the inadequacies of an inspired, pioneering shoe-
string operation. The present study, undertaken jointly
by lawyers and social scientists at the University of Michi-
 gan, is the apex of this development.41

It covers all individuals killed or injured in automobile
accidents that occurred in the State of Michigan during
one calendar year and ingeniously combines two samples
to represent this universe: one taken from the files of the
police, the other from the files of the courts where per-
sonal injury claims are litigated. The difficulty of the yet
unfinished court case was elegantly solved by substituting
the results reached in a comparable group of earlier long-
delayed cases. The major research instrument was a mail
questionnaire to the parties concerned or to their heirs,
thoughtfully supplemented at critical points with per-
sonal interviews, especially with the plaintiffs' lawyers
in the cases.

We now have reliable, precise, quantitative information
on almost every aspect of the injury-reparation process
and hence a sound factual basis for the many debates
which are currently raging over that problem area.32 To
be sure, we have this knowledge only for one year and
only for the State of Michigan. But, the United States
being what it is, one should not be in danger if one
generalizes from these findings. If there are doubts, they
can be removed by duplication of the study elsewhere.

The survey provides, as any good survey should, informa-
tion on both details and broad outlines. Roughly one
of every hundred Michigan residents suffered some loss
in an automobile accident during a year. For over 60 per
cent of the persons involved in accidents, the loss was
below $500; another 30 per cent suffered losses between
$500 and $3,000; and the remaining 2 or 3 per cent suf-
fred losses beyond $70,000.

In terms of all victims or their heirs, 23 per cent re-
ceived no compensation from any source, 37 per cent
received some tort liability settlement, and about half of
the victims received some compensation from loss, colli-
sion, medical care or life insurance. But in terms of the
total dollar amount paid to all victims, almost half of the
total damages remained uncompensated. The sources of
total compensation were tort liability, 55 per cent; loss
liability, 38 per cent; workmen's compensation and social
security, 7 per cent. The surprising finding was the great
role played by loss insurance and the increasing impact
of the social-security laws.

The Trend
The sharp increase in recent years in the number and
quality of social-science investigations of legal institu-
tions was spawned by a number of convergent develop-
ments. There was first a jurisprudential movement, the
Realists, who, beginning in the twenties, asked that the
law in action be explored in contrast to the law on the
books. Strangely enough, their aim remained for a long
time no more than a battle cry. Only now, a generation
later, does it assume substance.33 The second source was
the rapid development of research techniques and a con-
comitant growth of sociological research in general.34

It now appears that we are standing at the beginning of
an era in which the lawmakers will find increasing use
for empirical social-science research in the sound expecta-
tion that it is bound to alleviate their difficult task: of
making good law.
1. In modern times, William Petty’s survey of Ireland, made at the time of the Cromwellian conquest, is perhaps one of the first systematic surveys conducted for the lawmaker. British, and to a much lesser extent also American, legislation has a tradition of relying on systematically collected facts whenever broad legislative issues are at debate. Royal Commissions, Select Committees, and Ad Hoc Committees in the British Empire and Commonwealth, Congressional and other legislative and administrative committees in the United States, have made in their time major contributions to the law and incidentally also the body of social science. See Marie Jahoda, Paul F. Lazarsfeld, Hans Zeisel, Marienthal: new ed. (Allensbach: Verlag für Demoskopie, 1960), Appendix, Zur Geschichte der Soziographie.

2. Ironically, the most famous of these social-science footnotes, in the celebrated decision of the United States Supreme Court in Brown v. Board of Education, the school desegregation case, refers probably to a not very relevant piece of research. It concerned an experiment designed to prove the evil of segregation. The legal scholars seem to be agreed that it did not influence the Court, which was clearly moved by larger, moral considerations; see, for instance, Edmond Cahn, Jurisprudence,” New York University Law Review, XXX (1955), 150. There was even considerable debate about the evidential value of that research. See Kenneth B. Clark, “The Desegregation Cases: Critic of the Social Scientist’s Role,” Villanova Law Review, V (1960), 224, 236; Ernest van den Haag, “Social Science Testimony in the Desegregation Cases—A Reply to Professor Kenneth Clark,” Villanova Law Review, VI (1960), 69; A. J. Gregor, “The Law, Social Science, and School Segregation: An Assessment,” Western Reserve Law Review, XIV (1963), 621-636; Ovid C. Lewis, “Parry and Riposte to Gregor’s The Law, Social Scientist, and School Segregation,” ibid., 637.


4. By way of apology: it is impossible to list all such studies; any offered selection is bound to remain arbitrary.


6. California Board of Correction Monographs, Sacramento.


10. The control group was allowed to have optional pretrial, because it was thought that to simply deprive these litigants of their right to pretrial might engender constitutional difficulties.

11. One may ask why the state did not abolish pretrial altogether. One answer is that the particular experiment answered the issue conclusively only with respect to the option alternative. Moreover, pretrial had deep roots in the state’s tradition, and the limited design of the experiment probably offered a welcome pretext for a compromise.


14. Normally, it is considered essential that in a controlled experiment the experimenter be in direct control of the experimental variable: if two types of fertilizer are to be tested, he must be able to control their assignment to the various plots of land. But in the realm of the law such direct control is rarely feasible. Even in the two experiments discussed so far, the ultimate experimental variable was not under the control of the experimenter: in the bail-bond experiment, the final decision whether or not to release the defendant without bail was up to the judge; and the decision whether or not a pretrial was to be held was, in fact, left to the litigants. Yet because of the prior randomization of the experimental and the control group, these decisions did not invalidate the controlled character of the experiments. For general exposition of this approach, see Irwin Towers, Leo Goodman, and Hans Zeisel, “A Method of Measuring the Effects of Television through Controlled Field Experiments,” Studies in Communication, IV (1962), 87.

15. This suggestion too was made in Zeisel, Kalven, and Buchholz, op. cit., note 5, p. 99.


17. One of these many court decisions had a direct reference to the Zeisel-Callahan paper: Driver v. Phillips et al., U.S. District Court E.D. Pa., December 14, 1964, 36 FRD 261 (1964).


19. There were also two minor variations built into the experiment pertaining to the quality of the psychiatric expert testimony and to the informing of the jury as to the consequences of a finding of insanity.


23. It is, however, possible to cite one early application of findings from the jury study, albeit to a proposed reform of the English jury. The British government has proposed legislation that would allow majority verdicts of 10:2 and 11:1 votes in criminal cases and remove thereby the requirement of unanimity. It was possible to provide the British Home Office with a set of relevant predictions as to what to expect from such a law, because one of the American states, Oregon, allows the majority verdicts Great Britain plans to introduce. The first prediction was that the number of hung juries (juries in which a mistrial is declared because unanimity cannot be reached) would decrease by about 40 per cent. Since about 5 per cent of all trials end in hung juries, this means a change for two out of every hundred trials. The second piece of relevant information was that the presiding judge is by no means always critical of these juries who
remain hung at a 10:2 or 11:1 vote; about one-third of these hung juries are characterized as a result "which a judge too might have come to." Thus, not all of these hung juries are without merit. Thirdly, the Oregon experience suggested that England must henceforth expect some dissent in about one-fourth of all jury verdicts, an experience that might well be shocking in view of the serious sentence that usually follows a verdict of guilty. See H. Kalver Jr. & H. Zeisel, The American Jury: Notes for an English Controversy, Round Table, No. 226, April 1967.


26. Ibid., pp. 6 and 43. It has now been adopted as standard measurement by the Administrative Office of the Illinois Courts.


33. The two most distinguished names in that group were Karl N. Llewellyn, until his recent death professor at the University of Chicago Law School, and Jerome Frank, judge and author.


(Continued from page 1, Col. 1)

as the only area of law which has, to date, gone through successive rounds of codification.

We use the term "commercial law" in a curiously restricted sense. It might be thought that the sale and financing of Blackacre and the multi-million dollar office building which has been, or is to be, built on Blackacre is quite as "commercial" as the sale and financing of an automobile or a piece of industrial equipment. But, in the vocabulary of any lawyer, as in the curriculum of any law school, "commercial law" stops well this side of Blackacre. This usage seems to have established itself more than a hundred years ago. It came in at the time of the first great debate on codification, which occupied the legal profession in this country from the 1820's until after the Civil War. The proponents of codification, who were both numerous and influential, regularly argued that the greatest benefits from a codification of the substantive law would be found in the area of "commercial contracts"—which became a term of art to designate the law of negotiable instruments, sales of goods and, somewhat later, personal property security transactions.

Thus commercial law and codification, even in their origins, were linked together like an odd pair of siamese twins. Both, indeed, were direct legacies from the eighteenth century Industrial Revolution and the banking system which it called into being. The manufacture and distribution of goods became almost overnight central to the proper functioning of the most dynamic and unstable society of which we have record. Almost overnight the relevant, supporting bodies of law were improvised, in an extraordinary outpouring of judicial energy. It is fair to say that there was no commercial law—that is, no body of law which the legal mind felt to be somehow distinct and separate and identifiable—much before 1800. Yet, less than half a century later, Justice Story, surely a qualified observer, could say that the rules of law relating to "commercial contracts"

"have attained a scientific precision, and accuracy, and clearness, which give them an indisputable title to be treated as a fixed system of national jurisprudence."1

Story and others suggested a codification—what he once called a "digest under legislative authority"—of this "fixed system of national jurisprudence" as a solution to two problems which were already bothersome and gave every sign of shortly becoming intolerable.2 One problem was the increasing flood of case reports which, in a society become lawless from a surfeit of law, would soon make it impossible for any judge to declare the law or for any lawyer to advise his clients as to what the law was. The other problem was the progressive tendency toward local fragmentation of the substantive law in a federal system in which there was, it was occasionally declared on high authority, no common law of the United States and in which the United States Supreme Court was to exercise no control over the state courts in their determination of what the local version of the common law was to be.

These were real problems and real evils which required solution and remedy. The conscious thought of the period saw codification as the only way out of the impasse. We might indeed have had a general codification by 1850—in which case we would have inherited a quite different complex of legal traditions from those we have. The law, however, mysteriously provides its own solutions, although it may take us fifty or a hundred years to see that they were solutions. I suggest that three developments combined to make the abrupt remedy of a general codification unnecessary and to make a relatively pure common law system viable for fifty years after it might well have collapsed of its own weight.