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Selection of Topics and Methods for Law and Social Sciences Research

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II. THE SELECTION OF TOPICS AND METHODS FOR LAW AND SOCIAL SCIENCES RESEARCH

A. *Presentations*

PROFESSOR WALKER: It gives me pleasure to introduce our first speaker, Professor Hans Zeisel of the University of Chicago Law School.

PROFESSOR ZEISEL: The lead-off man in vaudeville, in baseball, or in a conference has the "tough spot." This is especially true in a conference with a topic as difficult as this one. We select our research topics as we swim or ride a bicycle; we do it, but we don't give much thought about how we do it. Yet, one of the major responsibilities of a scholar is to decide what he wants to study, and this conference is a useful and challenging opportunity to reflect on this rather important issue.

In trying to find out what a good selection is, I went over three sources. One was the projects which the National Science Foundation had asked me to review. I reread my memoranda in which I tried to be explicit about the merits and shortcomings of each proposal. My second source was my own research; I tried to recall how I selected my topics. Finally, I looked back at all the published investigations in our field to determine which of them, in retrospect, were successful and which were not.

From these sources I tried to formulate some general thoughts about what makes a topic well chosen. Such discussion involves value judgments. To avoid, however, too personal a view, I propose to begin by recalling some of the studies that have left a major impact on our efforts and some that have failed in this respect. From this review, we can perhaps learn from our joint experience what to watch out for.

Let me begin with a series of early studies that I have always considered eminently important and successful: the American Bar Foundation series on the various phases of the law enforcement process. Under the editorship of Frank Remington they have become standard works in their respective fields.[2] Their research design was simple, almost naive: the investigator and his helpers went into the field, watched policemen, prosecutors, and judges in their daily routine, talked to them, and eventually recorded the multitude of motives and considerations behind the crucial decisions of arrest, pleading guilty, and sentencing. Why are these volumes so important? They were important because every one of them took on a topic about which we knew almost nothing until these studies appeared. They threw first light into some of the many dark corners of our law enforcement process. Their pioneering showed those who came later what to look for and, if they wanted to progress into the sphere of quantitative analysis, what to count. Thus if a dark corner promises to be interesting, first explorations, however simple, have an ineradicable charm and usefulness. Now another dark area currently being explored is the entire field of administrative decision-making. Although we teach administrative law, we know little about how in fact administrative decisions are being made.

At the other end of the spectrum are studies that aim at answering very narrow and precise questions of legal interest. I am thinking, for instance, of Thorsten Sellin's careful compilation and analysis of data on the deterrent effect of the death sentence.[3] He compared neighboring states with and without the death penalty and attempted to trace the effects of its abolition and of reintroduction. I mention

2. [Ed.] AMERICAN BAR FOUNDATION, ADMINISTRATION OF CRIMINAL JUSTICE SERIES (F. Remington ed.).
3. [Ed.] T. SELLIN, THE DEATH PENALTY (1959).

this study here because it was one of the few in our field that has powerfully aided law reform. First in Great Britain by Sellin's testimony before the Royal Commission, and later here in the United States, his study is invariably in the forefront whenever the capital punishment issue is discussed. In passing, let me say that this type of research into secondary sources is relatively inexpensive and none the poorer in quality for it.

Let me mention in this context another set of investigations that had, and perhaps will continue to have, influence on our legal system precisely because they addressed a narrow question. The question was whether jurors who are against capital punishment are less likely to convict a defendant, compared to jurors who are in favor of capital punishment. The former, until the United States Supreme Court's decision in *Witherspoon,*[4] had been excluded from juries in capital cases. Even after *Witherspoon* jurors who are absolutely opposed to the death penalty are not allowed to sit on such cases. These studies, now numbering about half a dozen,[5] are interesting for another reason. Because of the difficulties of real experimentation, all but one of these studies were forced to proceed largely under simulated conditions—a clear drawback. But all six studies, although different in method and approach, confirmed the existence of the relationship between approval of capital punishment and propensity to favor the prosecution. Thus by triangulation each study supports the others, jointly creating a high level of confidence. This is still a rare pattern in our field but one that is commonplace in other, more developed sciences: duplicating studies designed to confirm earlier findings or to detect error, whatever it may be. Since the resolving power of the social sciences is, on the whole, small, the need for duplication and control is therefore great. This should be another consideration in choosing a topic.

The mushrooming of studies on the effect of reducing the size of juries from twelve to six is another more recent example of desirable duplication.[6] The impetus toward duplication came from two

4. [Ed.] Witherspoon v. Illinois, 391 U.S. 510 (1968).

5. [Ed.] *E.g.*, Bronson, *On the Conviction Proneness and Representativeness of the Death-Qualified Jury: An Empirical Study of Colorado Veniremen*, 42 U. COLO. L. REV. 1 (1970); Goldberg, *Toward Expansion of Witherspoon: Capital Scruples, Jury Bias, and the Use of Psychological Data to Raise Legal Presumptions*, 5 HARV. CIV. RIGHTS-CIV. LIB. L. REV. 53 (1970).

6. [Ed.] *E.g.*, Pabst, *What do Six-Member Juries Really Save?*, 57 JUDICATURE 6 (1973); Pabst, *Statistical Studies of the Costs of Six-man Versus Twelve-man Juries*, 14 WM. & MARY L. REV. 326 (1972); Rosenblatt & Rosenblatt, *Six Man Juries in*

sources. First, the problem fits into an important social science tradition—small group research—and allows the small group investigators, perhaps for the first time, to deal with groups answering real and serious questions instead of game-questions or questions contrived for the occasion. The second impetus comes from an intense legal interest in this particular issue.

There are other studies, many of modest size, which took on urgent problems and eventually produced reform. I am thinking, for instance, of Marvin Wolfgang's pioneering attack on the insufficiencies of the FBI's Uniform Crime Reports.[7] Crime statistics are an important yardstick of our social well-being, and such research efforts aimed at immediate improvements are often highly desirable.

Then there is the New Jersey pretrial experiment, conducted by Professor Rosenberg, designed to find out whether pretrial conferences in civil litigation help to settle claims.[8] The experiment showed that the optional pretrial conference eliminates just as many cases from trial as the obligatory conference and requires less court time. It dealt with an urgent issue, and its result proved that the majority view was wrong. Somehow, such a result is always more interesting although it should not matter where the truth lies. This study was also the first controlled experiment in our field. The opportunity for performing a controlled experiment should always be attractive: it is the most perfect instrument in our tool chest, and it is important that we try to expand its application in studying the legal system.

In a way, the Jury Project of the University of Chicago Law School was also a controlled experiment: we observed the differences in the outcome of trials depending on whether they were tried before a jury or before a judge. But since a real case can be tried only once, we obtained the comparison by asking the presiding judge in each trial how he would have decided the case if it had been a bench trial. The judge provided a meaningful standard of comparison because under our laws he is the only realistic alternative to the jury.

The Jury Project was a large enterprise in many meanings of the term. It was concerned with an important legal institution; it was op-

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Criminal Cases: Legal and Psychological Considerations, 47 ST. JOHN'S L. REV. 615 (1973); Note, *The Effect of Jury Size on the Probability of Conviction: An Evaluation of Williams v. Florida,* 22 CASE W. RES. L. REV. 529 (1971).

7. [Ed.] Wolfgang, *Uniform Crime Reports: A Critical Appraisal,* 111 U. PENN. L. REV. 708 (1963).

8. [Ed.] M. ROSENBERG, THE PRETRIAL CONFERENCE AND EFFECTIVE JUSTICE (1964).

erating on a major grant; and it extended over many, too many, years. It is only fair to say that although it has been a seminal study, it has had some impact. There was hardly a decision of the United States Supreme Court dealing with the jury that did not refer to *The American Jury*[9]—mostly in the dissent, I should add. In one case, to our embarrassment, or perhaps pride, we were cited in both the opinion and the dissent.

Let me now turn to studies which I believe to have been poor choices. When social sciences research into the legal system had just begun, Underhill Moore began to investigate with a formidable apparatus the question of how people will react to variations in fines for illegal parking.[10] It was undertaken at the time when psychologists were much interested in learning theory, and the law, it was felt, provided a good context for studying it. When it was all done, the study showed the speed and the extent to which car owners responded to these variations in deterrence. It has been a study that influenced neither psychology nor the law; it fell between the chairs.

Other studies that, at least in retrospect, have failed are the many ambitious efforts to search for the causes of crime. The point came to light when in the early Thirties the Carnegie Foundation considered large-scale financing of criminological studies and commissioned two distinguished scholars, Jerome Michael of Columbia University, and Mortimer Adler of the University of Chicago, to survey the field. The resulting book, *Crime, Law and Social Science*,[11] offered a devastating review of failure. Since not much has been added to our knowledge in this area in the intervening forty years, we may conclude that the question perhaps is too big for the tools in our possession. A very important problem may be a poor choice if the expectation of solving it is minimal.

Let me try to formulate some general conclusions. The first conclusion is that the distinction between basic research and applied research is not very relevant to our field. What matters is that we have an important question or at least an interesting one. Then we must ask: "Important and interesting to whom? At the very least it should be interesting and important to the legal system. It should be interesting to the scholar because unless he is deeply engaged, he will do a poor job.

9. [Ed.] H. KALVEN & H. ZEISEL, THE AMERICAN JURY (1966).
10. [Ed.] U. MOORE & C. CALLAHAN, LAW AND LEARNING: A STUDY IN LEGAL CONTROL (1943).
11. [Ed.] J. MICHAEL & M. ADLER, CRIME, LAW AND SOCIAL SCIENCE (1933).

The second conclusion is that the question to be investigated ought to have an answer. One of the facts of life in social sciences research is that the bigger the question, the more unlikely you are to come up with the answer. It would be nice to know, for instance, how to avoid war, or how to abolish crime, but it isn't possible. The tools of social science research are geared to modest questions, to the middle range of questions, not to the big questions.

Social science methodology has greatly improved during the last thirty years. If forty years ago a social scientist told a judge, "Since so many of your decisions are based on what you believe their effect will be, you should let us social scientists help you," the judge could have rightly replied, "Do you think because you call yourself a social scientist you know more about society than I, who sees society daily before my bench?" The situation has changed. Most of our research instruments have been sharpened, and many new ones have been added to the tool chest. The great advance came primarily from the advance in statistics.

This advance in methods, however, has not been an unmixed blessing. Sometimes we seem to forget why we count or make statistical models. This is perhaps less true for our own little field than for social and political science in general. Today the emphasis is all on quantification and model-building; simple, clear description has gone out of fashion. Browsing the other day through recent volumes of one of our political science journals, I could not help comparing nostalgically what I saw with what I remembered as one of the great studies in that field, Lord Bryce's *American Commonwealth*.[12] To be sure, I had read it at a poignant moment of my life, when I first came to these shores in 1938. A friend had given it to me "to read on the boat." If I am not mistaken, we have lost somewhat our respect for the magnificence of a broad descriptive canvas.

Nevertheless, the great improvement of empirical research came from the development of statistics. To see this you just have to look into any journal in the social sciences, anthropology, economics, sociology, or history. Forty years ago it was a rare case that you found any statistical table. Today it is all the reverse; there is hardly a piece without some statistics.

In applying all of these precious tools to the law, there is a difficulty. The legal system does not always like to be studied at close

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12. [Ed.] J. Bryce, The American Commonwealth (2 vols. 1888).

range. You all have encountered, I am sure, the difficult negotiations with judges, policemen, and lawyers to arrange for study and observation. The natural hesitation of a power system to allow others to pry into its hidden machinery is reinforced by the very real limitations imposed on research by the constitutional guarantees woven into the system.

It might be highly desirable, for instance, to find out whether a private defense attorney is more successful than a legal aid attorney or an assigned counsel. Any good student could easily design the appropriate experiment. All that is necessary is throw dice as the defendants come up at arraignment and say, "You may hire your own lawyer, and we will pay him; and you get a lawyer from the legal aid only" Clearly this would not do; certain rights are guaranteed and cannot be abridged, and therefore such experiments cannot be made.

The third conclusion I will make about what is a good topic reminds me of a remark made by a physician who is a friend of mine, "It is not difficult to cure a patient," he said, "to do it quickly and with little expense, is the doctor's true art." In a way, these are also the traits of a good research proposal. There should be a balance between the question and the answer and the amount of money to be spent, and since our questions are as a rule modest ones, there is a special charm about projects that don't cost too much money. The balance of what we hope to find out and the money expended is of interest. Nevertheless, it is not decisive. I would be the last one to say that, having partaken of the magnificent grant for our jury study. I only say that it is one of the points to consider.

When all is said and done, the point to remember is that we are engaged in a very special and precise task. Traditionally, lawyers and legal scholars read constitutions, statutes, and cases, and out of these elements they build their learned edifices. What we are attempting to do is to add a new dimension to the realm of legal studies. Law givers and lawyers have been trained to make learned assumptions about the foundations and effects of the rules and laws they deal with, and therefore they are pretty good at making them. We are engaged in putting these assumptions to the test so that the legal system will learn more about its functioning and its accomplishments. This broad vision of our purpose should guide our selection of research topics.

In this effort it is important that we overcome our academic departmentalization. The other day a graduate sociology student from one of our neighboring universities came to see me to talk about possible research themes in the area of sociology of law. We discussed a number of topics I thought would be suitable. But after a few days he called back with regrets: his professor thought none of the topics would be, as he put it, "sociologically interesting." We should cease asking whether a question is a psychological one, or an economic one, or a sociological one. What should matter is whether the answer will be legally interesting. If it is, and if we are reasonably certain we can find the answer in the course of our investigation—whatever its special discipline—then we can be sure that we have chosen a good topic.

I believe this is a good thought to end on for a conference, such as this, where scholars from many disciplines and fields have come together to explore the promise of social science in the field of law.

PROFESSOR WALKER: Thank you. Our second speaker this evening is Dr. Alice Padawer-Singer of Columbia University.

DR. PADAWER-SINGER: The subject for discussion tonight, the selection of topics and methods in law and social science research, may be considered from at least two angles. First, *what* topics and methods *ought* to be selected for law and social science research? Secondly, *how* does a researcher go about selecting these topics and methods? I will touch upon both aspects in describing the selection process.

First, I would like to make some general observations. I postulate that there is a strong relationship between the characteristics of topics, methods, and researchers. In addition, the selection process is influenced by public awareness of problems, available funding, and institutions such as law schools and research centers which facilitate the development of research. In general, researchers gravitate to certain topics, although occasionally there may be an element of chance or practicality (such as whether the research will be funded) which leads to the choice of a particular subject for research. Charcteristics of researchers such as academic training, life history, interests, values, and exposure to ideas of other researchers greatly determine the selection of topics and methods.

Often topics selected for research have the characteristics of high visibility and importance to the public, but sometimes the topic does not need to be prominent. Here the notion of "accidental visibility" or partial visibility must be introduced. A topic may neither "exist" nor be "visible" to the public at large but may become "visible" or known