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Developments in Law and Social Sciences Research

Laurens Walker

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Assessment Conference
DEVELOPMENTS IN LAW AND
SOCIAL SCIENCES RESEARCH

Laurens Walker,† Director

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I. FOREWORD

For at least fifty years scholars have urged the application of scientific method to the investigation of legal problems. Until recently, however, research projects adopting this approach have failed to exploit its potential. There were notable exceptions, but, in general, relatively few research projects were carried out, and fewer still could be described as successful. Now there is evidence of change. According to a survey conducted late in 1971 by the Council on Law-

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Related Studies,¹ approximately one-sixth of all full-time law teachers at least occasionally conduct "non-traditional" research, most of which involves some use of the scientific method.

From the beginning those interested in promoting law and social sciences research have well understood that any substantial program in that area would be a radical departure from tradition. Most legal research to date has been "doctrinal" in nature: scholars typically organize their study around legal propositions and then use appellate court reports and other printed materials readily accessible in law libraries as their principal, if not their sole, source of data.

The reasons for this situation are complex and much debated. In general, it is asserted that the practice results from a heavy emphasis on classroom teaching in law schools, which restricts time available for research. Some claim that the problem arises because few law professors are trained in the techniques of empirical research, and the common segregation of law faculties from other faculties on the university campus has made it difficult to correct this deficiency by even informal means. Furthermore, it has been pointed out that law students are not required to produce an original piece of research as a prerequisite for graduation, and, consequently, close student-faculty research collaboration is rare.

Against this background, the recent introduction of substantial law and social sciences research programs suggested the need to assess this new development. One particular group of investigators was a likely source of useful information. On September 1, 1971, the National Science Foundation established for the first time a program in law and social sciences. In the first ten months of the new venture the Foundation funded twelve research proposals at a projected cost of approximately 650,000 dollars, and since that time the program has continued to provide a similar level of support.

The first group of funded proposals attacked a wide variety of problems. These proposals included, for example, "The Role of the Cognitive Process in Determining the Demand for Legal Services" (Philip R. Lochner, State University of New York at Buffalo), "Law and Professional Sports" (John D. Morris, Arizona State University), and "Comparative Study of Trial Courts" (Lawrence M. Friedman, Stanford University).

1. Cavers, *"Non-Traditional" Research by Law Teachers: Returns from the Questionnaire of the Council on Law-Related Studies*, 24 J. LEGAL ED. 534, 543 (1972).

The proposals also revealed a variety of methods. For example, the project, "Human Behavior and the Legal Process" (Laurens Walker and John Thibaut, University of North Carolina at Chapel Hill) emphasized the use of laboratory experimentation. "Social-Economic Impact of Legal Practice" (Earl Johnson, University of Southern California) generated an analytical cost-benefit model, and "Research on the Merits and Demerits of the Traditional Voir Dire System" (Hans Zeisel, University of Chicago) used experimental design in a field setting.

Several different methods for organizing interdisciplinary work were adopted. "Human Behavior and the Legal Process" was highly integrated; one principal investigator was legally trained, the other was a social psychologist. The same integration was present in "Custody Investigations in Divorce Cases" (Robert J. Levy, law professor, and Julie Ann Fulton, sociologist, University of Minnesota). Other projects such as "Social-Economic Impact of Legal Practice" and "The Deployment Process and the Implementation of Legal Policy" (Marc Galanter, State University of New York at Buffalo) made extensive use of consultants from other disciplines at stated points in the development of the research project.

The NSF projects also contemplated a variety of applications for their findings: "Analysis of an Experimental Study of Free-Press—Fair Trial" (Alice Padawer-Singer, Columbia University) provided for the publication of a book, the presentation of papers, and periodic meetings with judges, lawyers, newsmen, and social scientists for the purpose of discussing results and making recommendations. "The Role of the Cognitive Process in Determining the Demand for Legal Services" provided for dissemination of its results, in part, through a law school seminar and a conference of interested scholars. The results of "Comparative Study of Trial Courts" may be used in large part to develop a more comprehensive study.

The scale and variety of these first NSF projects suggested that the principal investigators of those studies were a relatively small group of scholars likely to have considerable knowledge of the developing solutions to the problems of carrying out law and social sciences research. It was therefore proposed that these investigators be brought together to examine those solutions and to consider their effect on the future of law and social sciences research. In addition, it was proposed that a few persons with special knowledge in the area be invited to provide useful perspectives on the investigators' reports.

A conference was thought necessary to obtain the developing information since published research reports typically focus on the results of the project carried out and do not discuss, for example, such important questions as how the topic area was selected or what other methods were considered. These typically unreported "results" can probably only be obtained by bringing experienced investigators together for a brief period of structured discussion.

The proposal was supported by the National Science Foundation (Grant No. GS 36143), and the Conference was held April 11-13, 1973, at the University of North Carolina at Chapel Hill. Four working sessions were conducted, each devoted to one of the following topics: (1) "The Selection of Topics and Methods for Law and Social Sciences Research," (2) "The Organization and Management of Law and Social Sciences Research," (3) "Evaluation, Dissemination and Application of Law and Social Sciences Research," (4) "The Future of Law and Social Sciences Research." Each session began with several prepared presentations related to the selected topic, and a period of discussion followed.

The entire proceedings were taken down by court reporter, and edited versions of the presentations and discussions are presented in this issue of the *North Carolina Law Review*. In preparing these materials for publication an effort was made to preserve as nearly as possible the informal and lively character of the Chapel Hill meeting. Therefore the conference is reported in a proceedings format rather than as a series of formal papers with comments. Footnotes have been added only where absolutely necessary to clarify references to individuals or publications. Those who made the presentations were given the opportunity to suggest editorial changes in their talks, but the editing of the discussion was done by myself and the editors of this review.

The proceedings themselves will provide the best measure of the worth of the conference, but perhaps it is appropriate to suggest here that because of the broad experience, the hard work, and the enthusiasm of the participants, the goals of the conference were realized. It is hoped that the following materials will bear out this suggestion and will be a useful resource for all scholars engaging in law and social sciences research.

LAURENS WALKER

Participants

Cavers, David F., Fessenden Professor of Law, Emeritus, Harvard University. L.L.B., Harvard University. President, Council on Law-Related Studies.

Christian, Winslow, Associate Justice, California Court of Appeal. L.L.B., Stanford University. Director, National Center for State Courts, at the time of this conference.

Friedman, Lawrence M., Professor of Law, Stanford University. J.D., M.L.L., University of Chicago.

Fulton, Julie Ann, Research Fellow, University of Minnesota. M.A. (Sociology) University of Minnesota.

Galanter, Marc, Professor of Law, State University of New York at Buffalo. M.A. (Philosophy), J.D., University of Chicago.

Hines, Howard H., Division Director for Social Science, National Science Foundation. Ph.D. (Economics), Harvard University. Former Professor of Economics, Iowa State University.

Huszagh, Fredrick W., Visiting Associate Professor of Law, University of Montana. J.D., L.L.M., J.S.D., University of Chicago. Program Director for Law and Social Sciences, National Science Foundation, at the time of this conference.

Johnson, Earl, Jr., Associate Professor of Law, University of Southern California. J.D., University of Chicago; L.L.M., Northwestern University. Former Deputy Director, Office of Economic Opportunity Legal Services Program.

Konopka, Arthur F., Program Manager for Law, Science and Technology, Research Applied to National Needs Division, National Science Foundation. L.L.B., Columbia University.

Levy, Robert J., Professor of Law, University of Minnesota. J.D., University of Pennsylvania.

Lochner, Phillip R., Jr., Assistant Professor of Law, State University of New York at Buffalo. L.L.B., Yale University; Ph.D. (Political Science) Stanford University.

Morris, John P., Professor of Law, Arizona State University. J.D., Northwestern University.

Padawer-Singer, Alice M., Senior Research Associate, Bureau of Applied Social Research, Columbia University. Professor of Education, Long Island University. Ph.D. (Social Psychology), Columbia University.

Probert, Walter, Program Director for Law and Social Sciences, Na-

tional Science Foundation. J.D., University of Oregon; J.S.D. Yale University. At the time of this conference and now on leave, Professor of Law, University of Florida.

Radloff, Roland, Program Director for Social Psychology, National Science Foundation. Ph.D. (Psychology), University of Minnesota.

Raiser, Thomas, Professor of Law, Justus Liebig University of Giessen. Dr. iur., Eberhard Karl University of Tübingen; Dr. iur. habil., University of Hamburg.

Rowen, Henry H., Professor of Economics, Stanford University. B. Phil., Oxford University. Former President, Rand Corporation.

Walker, Laurens, Professor of Law, University of North Carolina at Chapel Hill. J.D., Duke University; S.J.D., Harvard University. Director of this conference.

Wheeler, Stanton, Professor of Law and Sociology, Yale University. Ph.D. (Sociology), University of Washington. Consultant, Russell Sage Foundation.

Zeisel, Hans, Professor of Law and Sociology, University of Chicago. Dr. Jur., Dr. Pol. Sc., University of Vienna.

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-

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

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 pos-

sible 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. Characteristics 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

to the researcher and to a relatively small number of individuals. For instance, in 1958 because of exposure to Dr. Nyswander and other researchers involved in finding ways to combat drug addiction, I became interested in this area of research. I discussed various ideas with Dr. Nyswander, and she encouraged me to conduct research on that subject and offered me access to study fifty patients in the hospital. Back at the university I found little awareness of the growing problem of drug addiction, let alone official encouragement and support. I was even told that drug addiction was not a topic of social psychology! Being a graduate student at the time, I had to turn to other research: an experimental study of decisions in negligence cases.¹³ When topics are not "in fashion," when they are not recognized by the academic community or by funding agencies as being within the provinces of their interest or as "important" or "pertinent" to particular disciplines, researchers have great difficulty in pursuing their interests. "Importance" attributed to a problem depends not only on the extent of the problem but also on public knowledge and concern.)

The interdependence of topics, researchers, methods, and public awareness and need for guidelines can be clearly seen in the Free Press-Fair Trial Project.¹⁴ Primarily, the free press-fair trial issue seemed to me to constitute an important problem and a conflict highly visible to the public. The issue involves the conflict between two constitutional guarantees: the right of defendants to a fair trial and freedom of the press. This issue has been repeatedly covered in the press. Claims have been made that certain kinds of trial publicity and press coverage affect the fairness and outcome of a trial.

The public has the need and the right to know something about the conduct of trials. The question is, what? Do the public and potential jurors need to know before a trial that a defendant has a criminal background or that he is alleged to have made and retracted a confession? Do certain types of information in the news media prejudice jurors and lead to a greater probability of guilty verdicts or ver-

13. A. Padawer-Singer, *Anchor Effects on Mean Judgments and Latitudes of Acceptance Under Varying Conditions of Involvement and Discrepancy*, 1965 (unpublished dissertation in Columbia University library).

14. The Free Press-Fair Trial Project has been conducted by Dr. Alice M. Padawer-Singer and Professor Allen H. Barton at Columbia University in cooperation with Professor Maurice Rosenberg of the School of Law and Professor W. Phillips Davison of the Graduate School of Journalism, and with the support of Judge Bernard S. Meyer of the Supreme Court of the State of New York, Nassau County.

dicts which are not based only on facts brought out in a trial? If such prejudices are demonstrated, what measures can be taken to ensure the two constitutional guarantees? These are some of the elements which I wanted to study. In addition, I wanted to study the role of the news media as an ally of the law in combating crime and corruption and in ensuring fair trials.

Choosing to study the dimensions of the free press-fair trial issue was a natural outcome of my background. After graduating from college, I worked as a journalist-editor for a French newspaper, and then entered graduate school at Columbia University where I received an M.A. in experimental psychology and a Ph.D. in social psychology. In addition, I am married to a lawyer and have been exposed to frequent discussions about law with lawyers and judges in my social and private life.

The background, the experiences and the exposure of the investigator influence the development of research. For example, the idea of conducting the Free Press-Fair Trial Project originated when a practicing judge, Bernard S. Meyer, then of the Supreme Court of the State of New York, Nassau County, was alarmed by the lack of empirical evidence on the issue. He discussed the possibility of developing empirical studies with Professor Maurice Rosenberg of the Law School at Columbia University. Professor Rosenberg then contacted the Director of the Bureau of Applied Social Research at Columbia, Professor Allen H. Barton, who was one of the committee members for my doctoral dissertation. Dr. Barton asked me whether I wished to continue jury and other law-related studies. When I indicated my interest, he asked me to design and direct research on the free press-fair trial issue. In summary, the judiciary turned to the law professor in the law school who turned to the research bureau at the university. In other cases, a judge may turn to a bar institute or to a law research institute for the development of empirical studies. Thus research is facilitated by structures inside and outside the university.

Topics selected for research must have characteristics which lead to questions that can be formulated into hypotheses to be subsequently tested through the study of independent and dependent variables. Often as I listen to legal problems, I wonder, "How can they be translated into research? Are there some elements in these problems which could be studied with interdisciplinary legal and social science research methods?" The free press-fair trial issue can be conceptualized as a study of attitude change, a study of messages received

from various sources such as the news media, lawyers, judges and as a study of group dynamics and decision making in the jury. Social psychologists have, in other contexts, studied the effects of messages from various sources, the development of group pressure, and the phenomenon of conformity. Some tools were therefore available to study the issue of free press-fair trial.¹⁵

Sometimes data on a particular topic may have already been collected. If this is the case, the decision to continue research on that topic is made after consideration of the characteristics of the existing data: validity, completeness, representativeness, generalizability, conclusiveness, and realism of previous studies.

When we conduct research designed to lead to answers that are applicable to problems of the real world, we must select methods of study and collect data in such a manner that the results of the study will be seriously weighed and regarded as important and worthwhile by the "consumers" of the data, such as the news media, the judiciary, the legal profession, the public, and the policy makers. In the selection of topics and methods we should therefore take into consideration the basic and applied goals of the research. We must be cognizant of the possible or probable impact of the study. Although laboratory studies are considered by a great number of social psychologists as valid and useful research, they have scarcely received overwhelming acceptance by the judiciary and the bar. In addition, laboratory studies have been the subject of re-examination by psychologists.¹⁶ In

15. See GROUP DYNAMICS (3d ed. D. Cartwright & A. Zander eds. 1968); C. HOVLAND, I. JANIS & H. KELLEY, COMMUNICATION AND PERSUASION (1953); C. HOVLAND, A. LUMSDAINE & F. SHEFFIELD, EXPERIMENTS IN MASS COMMUNICATIONS (3 Studies in Social Psychology in World War II 1949); THE ORDER OF PRESENTATION IN PERSUASION (1 Yale Studies in Attitude and Communication C. Hovland ed. 1957); C. SHERIF, M. SHERIF, & R. NEBERGALL, ATTITUDE AND ATTITUDE CHANGE (1965); Asch, *Effects of Group Pressure upon the Modification and Distortion of Judgments*, in GROUPS, LEADERSHIP AND MEN 177 (H. Guetzkow ed. 1951); Hovland & Weiss, *The Influence of Source Credibility on Communication Effectiveness*, 15 PUB. OPINION Q. 635 (1951); Manis & Blake, *Interpretation of Persuasive Messages as a Function of Prior Immunization*, 66 J. ABNORMAL SOCIAL PSYCH. 225 (1966); Schachter, *Deviation, Reflection and Communication*, in GROUP DYNAMICS, *supra* at 165; Weiss, *Effects of the Mass Media of Communications*, in 5 THE HANDBOOK OF SOCIAL PSYCHOLOGY 77 (2d ed. G. Lindzey & E. Aronson eds. 1969); Zimbardo, *Involvement and Communication Discrepancy as Determinants of Opinion Conformity*, 60 J. ABNORMAL SOCIAL PSYCH. 86 (1960).

16. Orne, *On the Social Psychology of the Psychological Experiment: With Particular Reference to Demand Characteristics and Their Implications*, 17 AMERICAN PSYCHOLOGIST 776-83 (1962); Rosenthal, *On the Social Psychology of the Psychological Experiment*, 51 AMERICAN SCIENTIST 268-83 (1963). These articles deal with the effect of the setting and the experiment situation on the responses of the subject. The

the Free Press-Fair Trial Project, I looked for methods which would be "acceptable" not only to social scientists but also to the news media, the judiciary, and the bar. It became evident that such research needed to be conducted under controlled conditions and with utmost realism. This was the first interdisciplinary experimental research of free press-fair trial to be conducted in the authentic setting of the court with a population of jurors selected through *voir dire* examination. We received the cooperation of court personnel and access to the New York State Supreme Court in Nassau and Kings (Brooklyn) Counties. In Nassau County, we conducted ten jury experiments in the court. After our work in Nassau County, we conducted twenty-five experiment in the court in Kings County. We selected jurors by *voir dire* examinations with the cooperation of lawyers from the legal aid society and the district attorney's office and with the help of the administrative judge of the court.

This is the kind of study in which much time and energy is invested and in which the next day's activities are uncertain. Because we were "guests" of the court, we could have been told at any time, "We are sorry; you cannot have access to jurors anymore." We were always "on the edge of a precipice" wondering what tomorrow would bring. We tried to gather as much data as possible for fear that we might not be back again in court. We have a large body of data now.

The ambitious scope and risky nature of this project illustrate that the values of researchers determine largely the topics they will study. Some researchers are attracted to high-risk studies, longitudinal studies, studies of difficult problems in the field, studies which require a long time, many negotiations and disappointments, and much energy in gaining access to data. Such studies take away from writing and publishing, longitudinal studies, and studies of difficult problems in the field. Other researchers prefer low-risk, short-term studies, or studies in laboratories. My feeling is that although a problem may

study has received the constant support of Judge Bernard S. Meyer of the Supreme Court of the State of New York, Nassau County; the cooperation of Judge Frank Crelotta of the Supreme Court of the State of New York, Nassau County, who welcomed the first phase of the study in Mineola; of the staff of the Supreme Court of Mineola; of Mr. William Wells at the office of the Mayor, University Relations, New York City; of Judge Arthur Hirsch, Director of the Administration of the Courts, State of New York, Second Judicial Department; of Mr. Leland Tolman, Judicial Department, Appellate Division Court; of Judge Miles McDonald, Supreme Court of the State of New York, Kings County, who welcomed our study in the court; of Mr. Eugene Gold, District Attorney of Kings County, and Mr. Caesar Cirigliano of the Kings County Legal Aid Society's staff who conducted *voir dire* examinations.

be difficult and vast, it still should be studied. Even if answers are not easily and readily accessible, attempts should be made to conduct research in order that the next study benefit from the outcome of the first. The benefits come because of the identification of proper questions and methods and the isolation of useless procedures. For instance, I believe that broad studies should be undertaken which adopt different ways of looking at the phenomenon of crime. We have called crime what we have defined as crime. We would not jeopardize the interests of society by relabeling certain problems and decriminalizing certain activities. They should be studied from various vantage points with varied methods.

After gathering and analyzing data, the researcher has to keep from being grandiose, from making far-reaching recommendations that might be harmful to society or to a segment of society. The data must have generalizability. In reporting the analysis of data, we ought to look for ways of translating research and policy matters to the public to make sure that we carefully and clearly delineate the consequences of alternative modes of action and refrain from recommending change for its own sake. We ought to analyze the consequences of suggested alternatives jointly with the "consumers" of the data. Otherwise we miss great opportunities to involve large segments of society in public affairs.

Some of the remarks I have made have implications not only for the education and training of students and researchers but also for funding policy. It seemed important to me that the Free Press-Fair Trial Project not be funded under one aegis. Early funding had come from the Walter E. Meyer Research Institute of Law. I turned to the Columbia Broadcasting System and to the New York Times for funds because I felt that this research ought to be funded either by an entirely neutral foundation or by foundations representative of the parties that would consider the data eventually. Because our funds were meager, we turned to the legal aid society and the district attorney's offices for help. We received great cooperation. Nevertheless, it was difficult to coordinate the time of three volunteer lawyers who were overworked. Although it was exhilarating at times to feel that separate organizations cooperated to make this study possible, adequate funding would have facilitated and shortened the time needed for research. I don't believe that "starving is good for the soul."

What recommendations can I make from my experience? In respect to education and training in research, I would like to expose

more students in high school and college and in law schools and graduate schools to concepts of law and social science. I would like to see the development of simultaneous studies on particular topics in the laboratory and in the field with different populations so that we might determine the extent of the correspondence between the two methods. I still believe that there is a need to study the actual population and preferably in the actual setting in order to be able to trust the value of the data. Students may not behave nor think the way our jurors did. Our jurors came from a variety of backgrounds and occupations: telephone employees, nurses, postal employees, and businessmen. The relation of the college sophomore to the professor is different from the relation of the jurors to the research staff and the setting of the university is different from that of the court. These differences may well influence the outcome of experiments.

Finally, I would like to recommend that we try to create a setting of mutual acceptance between law and social science research. We need to have more interdisciplinary meetings and workshops. Legal authorities, funding agencies, and social scientists should cooperate to facilitate the research process by ensuring ready access to courts, to the field, and to data.

B. *Discussion*

PROFESSOR WALKER: Thank you very much. Now let me invite comments and observations.

DR. HUSZAGH: I guess I was more impressed with the absence of specific notions about topic selection than I was with positive statements. I don't mean that to be a criticism but rather an observation.

To take Dr. Zeisel as a point, my notion of his reputation, which obviously has built into it a lot of error, is that he has an uncanny sense for picking a topic and matching it with a method. Yet, although he laid down some criteria, he really didn't provide a "hot little list" which one could hold in his hand and say, "Ah ha! I have a formula!" Another notion that was raised was that selecting a topic and matching it with a method is largely a reflection of heredity and previous experience. This doesn't ring untrue with what I have observed, but I think it does cast a serious doubt on this facet of the research process.

Let me put it in a little perspective. If I were to design some kind of mechanism for selecting topics and matching methods after this

session, I would say, "Well, first of all, we had better not let the same person do everything. We had better open a competition to find people who have creative insights into certain kinds of problems and the ability to determine why certain statements are important. Once that competition is concluded, we should open up another competition on the matching of methods. Once we have finished that, we should open a third competition which would be necessary to pick bodies to do the research." But would such a process be desirable? Would the experience develop any really useful personnel? I would like Dr. Zeisel and Dr. Padawer-Singer to tell me if that sort of summation is totally off base, and if so, why.

PROFESSOR ZEISEL: I think I made it clear that I would be unable to teach how to do research. I merely gave a half-hour talk about it. I think thirty-five minutes would have put me into some difficulties. Although I don't like to make such general statements, I think some subjects are not teachable. Maybe somebody has thought more about it than I, but I think that you are asking too much.

Your idea of fragmentation brings to mind a trial in which I was a juror. A witness was called to the stand, and the lawyer asked, "Well, tell us how did this telephone conversation go?" The witness said, "Well, I said to him 'how are you, Joe,' and he answered" "Objection!" And so this poor fellow was limited to saying what *he* said on the phone. Then an hour later we had an opportunity to listen to the other end of the conversation.

I don't think that your fragmentation would really work very well. I think that research functions should remain together because what one can't teach is how to go about solving a certain problem. I don't think that one can even teach how to find a good problem. I appreciate that you would like to have a guide on what is important, and then instead of writing to us what do you think of a problem, you could just look in your little booklet. But I don't think I would know how to make such a guide. Perhaps Dr. Padawer-Singer could.

DR. PADAWER-SINGER: I must agree with Dr. Zeisel. It is very difficult to find the topic characteristics that would make the selection of good topics easier.

But in order to be able to study a particular topic, a topic has to have features from which the researcher can abstract certain elements that can be studied. The researcher must then be able to translate these elements into hypotheses and to test those hypotheses in research. Another problem for the researcher is to determine the

parameters or dimensions of the problem and to decide what elements in that topic should be examined. Most of the time an investigator can't study every element. He has to ask himself: "Which elements are the most important to me and (or) to the public?" "Which elements will lend themselves best to research?" "What methods can I use?" From the methods that can be used, he must ask, "Which ones have been used already and with what kind of results?" "What is left that could add some light?" "Can it be done with methods that have been used previously but perhaps not as effectively as they should have been?"

The researcher really must make a series of decisions all along the line. A method used in the first experiment may not be exactly the same as the one adopted in the third or fourth. That's why very often we like to do some pilot studies or some pre-testing. But I think the process is really a series of decisions depending on the researcher's experience as the research proceeds from "A" to "B" to "C," and on his previous experience in dealing with other problems and on the state of research and knowledge at any particular time.

PROFESSOR LEVY: I thought I heard both speakers say that topic selection should be more a function of the scholar's academic interest than of what might be called the current fads in the research funding organizations—more of a function of individual scholarship than academic entrepreneurship or "grantsmanship."

To give you an example of a topic that is currently very fashionable, I find that there is all sorts of money around to study everything about children. But if you are interested, as I happen to be, in families and what happens to children as the result of divorce, as soon as husbands and wives enter into the inquiry, people are very uninterested.

I wonder whether one obvious consequence of the variation in fund-givers' interest from time to time is that scholars have a tendency to manipulate at least the expression of their research into avenues that are currently fashionable. I wonder if either of you have any comments about the implications and results of grantsmanship manipulation, and whether such manipulation should be indulged in or not.

PROFESSOR ZEISEL: I hope that I made it clear that it depends on what is more important to you, the money or your integrity. I have no doubt what one should choose. I would only say that not everybody is engaged in Dr. Huszagh's difficult task of evaluating every project on its merits. After all, one could give grants to an insti-

tute and say, "You make what you want with it," or one can give a lifetime grant to a scholar and say, "You make what you want with it." On the whole I would think that these are better grants because although I have not been privy to the decision process except very occasionally, I think that it is almost impossible to ask the National Science Foundation to sort out projects on their merits. I think it is easy to sort out bad projects which won't answer what they claim to answer. But once you have projects of some merit, how do you decide between them?

How do you decide, for instance, the merits of the following project: I am now interested in problems of sentencing. Why am I interested? I am now interested because I can't help seeing that in the United States the sentence is, in the first instance, decided by the judge. Because this is so, the sentence stands since, as a rule, there is no appeal from the sentence unless the verdict is attacked. Differences in sentencing result because of the individual judges' idiosyncrasies. Where I come from in Austria, and, incidentally, in Canada, the situation is quite reversed. Both the prosecutor and the defendant can appeal the sentence independently of the verdict. Perhaps as a result, the incredible differences in a judge's sentencing different types of criminal defendants for the same crime are reduced. So I am mulling over some ideas about how one could study the various means of avoiding these various idiosyncratic and very undesirable differences which result from the sentencing procedures of our federal courts and some of our state courts. But how is anybody to decide whether this is important or not? I can say I am interested and obviously, the question of sentencing is of some general importance, but this hardly suffices.

Or take as an example something quite different. (I like these comparative law examples because that is how I was forced to look at things.) In the United States the standard way of remunerating a lawyer is through contingent fees. Across the border, and in almost all European countries, a lawyer is disbarred if he accepts contingent fees. In Canada, they think it is a crime; we think that it is the best solution. Nobody has ever studied this problem. (Incidentally, we have related the problem to court congestion.) And so how is Mr. Huszagh ever going to come to a conclusion? It is an impossible task.

DR. PADAWER-SINGER: I think that I would agree with Dr. Zeisel about the integrity of the researcher. As I was listening to Professor Zeisel, I couldn't help but think again that his research topics

are very much a product of his experience and his exposure to various legal systems. His life history and his academic training are very intimately tied to his choice of topics. To me, this has direct implications on the education that we should give our students in law schools and universities. We should expose the students to comparative law and comparative ways of court administration.

MS. FULTON: I would like to respond to what I hope I didn't hear. I thought I heard Professor Zeisel say that if there is no answer to the problem, it is not worth researching, or something along those lines, and I became immediately defensive. Also I thought I heard him say that our tool chest is now so full that we should be able to approach almost any problem. I again felt immediately defensive.

First of all, I do feel that there are many issues about which you have to say: "Are they of interest?" I feel that so many of the problems in the world today are termed "social problems" but are indeed legal problems. If we could just think about things in terms of talking about the issues rather than talking about what is researchable, we could then have innovative and interested minds coming up with research topics. Once this happens we could then see if there are methods to approach them. But I think that there is a lack of thinking about these problems in terms of their interdisciplinary nature. Consider for instance the problems of the equality of women. It is both a social problem and a legal problem. The impact of abortion is a moral problem, a legal problem, and a social problem. We should start thinking in terms of the interrelationship of things that we think of as personally interesting. The topic doesn't have to be "legally" interesting, or "sociologically" interesting, or "psychologically" interesting to be worth researching.

The next thing that I would have to say is that there is a trend toward talking about *theories* of the middle range, *issues* of the middle range, *questions* of the middle range. I think that to be realistic we should also talk about *answers* of the middle range because we do not really have tools that are applicable to all of these problems or that will give us an answer. For some of the problems I don't believe there is an answer. I think that lawyers who are beginning to accept science and economics say, "All right, we are ready to hear what you have to say. Give us the answer now that we are open." It isn't possible. It simply isn't possible.

I think that each of these disciplines has developed to the point of recognizing that the negative findings that they are coming up with

are data. They are finding just as many non-answerable questions as positive results. It has taken some development of intellectual understanding within each of the disciplines to accept that negative findings are important findings also. To expect that the available tools might be applied to all the questions that we might like to raise is probably to expect too much. So many of these tools and techniques are based on mathematical foundations in which certain assumptions are necessary for their applicability. There are now ventures into discovering whether or not you can disregard certain of these assumptions and still apply the techniques and come up with results in which you can have confidence. But until there is more research, we should probably be a little careful. Many techniques, which are in fact "tossed about" so freely, are based upon levels of measurement which are not really meaningful or even possible in some of the areas of sociology, psychology, and law. They are primarily useful in areas in which you have interval data or ordinal data. In most of our work, we don't have these kinds of data. We have nominal data, or rank-order data, or something of this nature. We have tools which are applicable to the less interesting questions, and very interesting questions for which we don't have the tools.

PROFESSOR RAISER: In this discussion of "importance" as a criterion for selecting research, it seems to me that there is one point that is very significant that we have not discussed. That is the question: important to whom? Is it importance to the intellectual ideas and ideals of the scholars, on the one side, or importance to the foundations, the people who have to decide about the money? Neither of these really constitutes the right group.

When we say "important" we should mean: Important to *public interest*. There are a lot of legal questions on which the courts and legislatures must act. If in these questions there is any factual background that is not clear, it would be important to public interest to determine these facts. So it is not the intellectual interest of the scholar, the development of sociology, or the knowledge of science that is the right criterion to use in deciding the question of importance. The relevant inquiry should be clearly the public interest of the question.

PROFESSOR ZEISEL: There I have an opinion that is really counter to yours. First of all, I am really convinced that the distance between what social scientists find, however important it might seem to us in terms of the public interest, and what happens in the real world is immensely remote. So many students go into social sciences

or into the law because they think that they are going to change society. No error can be greater. If they want to change society they shouldn't become students—they should become politicians. Who is the man who made the famous experiment which was to become a famous footnote?¹⁷ Kenneth Clark.¹⁸ It is as if Dr. Clark, who made experiments with white and black balls and found that separate but equal is really not good enough, thinks that he has affected American history by having influenced Supreme Court Justices. That is about as grotesque as somebody thinking that he can do something in the real world as a professor.

Let me give you an example. Our *American Jury*¹⁹ has been quoted about eight times by the Supreme Court. But if one reads these opinions carefully, it is quite clear that we have merely provided handy evidence for a law clerk to put into his Justice's opinion to give it a more substantial foundation. The idea that we were close to influencing the Justices is just fallacious.

Therefore, quite frankly, in response to what Mr. Levy said, and also to what you said, I would reply: For God's sake, do what you think is right! If the foundations don't "play ball," never mind them! Get other foundation directors.

DR. HUSZAGH: You really want to influence the public, don't you?

PROFESSOR ZEISEL: No—but really, science is the scientist. When Eisenhower was President of Columbia, the anecdote is told that there was a faculty conference, and he listened to it, and when it was over, he said: "Well, gentlemen, the University will take what you said into consideration." And a professor got up and said, "Mr. President, I think you are mistaken. *We are the University.*" You see, it is that serious. There is no way out. Help make the selection process of professors better. The whole hope of good science rests on the ability to judge. Whether a person can or cannot teach how to judge or how to select topics is really a secondary question.

PROFESSOR CAVERS: It seems to me that you presuppose

17. [Ed.] *Brown v. Board of Educ.*, 347 U.S. 483, 494 n.11 (1954), *alia* K.B. CLARK, EFFECT OF PREJUDICE AND DISCRIMINATION ON PERSONALITY DEVELOPMENT (Midcentury White House Conference on Children and Youth 1950).

18. [Ed.] Kenneth B. Clark is Distinguished University Professor of Psychology at City College of New York, and is a past president of the American Psychological Association. His publications include *Desegregation: An Appraisal of the Evidence* (1953), *Prejudice and Your Child* (1955), and *Dark Ghetto* (1965).

19. [Ed.] See note 9 *supra*.

that you would have an investigator who has a very clear concept of what he wants to do and what he wants to assert. Unless he is able to pursue that particular objective, he ought to stand in a dark corner and gnash his teeth. I don't think that is necessarily descriptive. I think that there are a good many researchers who have areas of interest rather than a specific objective that is frustrated if the foundation doesn't come through with a particular grant for that particular target. For them the fact that a governmental body needs to have a particular matter looked into is something to which they can respond not because they have been burning to do that particular job but because the support that the body can give will make it possible for them to pursue an acceptable goal.

I think that we tend to get into a rather poetic frame of mind sometimes about the relation of the particular subject to the particular scientist. I don't think that every bit of commissioned research that is done represents a scientist who has sacrificed his integrity in order to carry out the concerns of the body who is producing the funds.

PROFESSOR GALANTER: I was interested in Professor Zeisel's latest comment depicting an autonomous scientific enterprise, and I had some difficulty putting that together with his earlier dismissal of the notion that what is sociologically interesting is a valid criterion for selecting research topics. It strikes me that, however misplaced and crudely applied, this question has at times some germ of merit in it in that, presumably, it represents some kind of commitment to seek out a general understanding of law as a social process. I would have thought that one of the criteria for an interesting research topic is some criteria of generalizability. That is, is it a topic that will somehow give you a lever for coming up with interesting generalizations that apply to more than a specific set of facts? Perhaps this was taken for granted or implicit in what was said. But I was left, at least from the opening remarks, with a feeling that both speakers seemed to despair entirely of any kind of generalized scientific understanding of the legal process, and were, as I heard it, telling us to go out and find problems that were of interest to people out in the practical world. I hope that I will be corrected in that understanding.

PROFESSOR ZEISEL: Of course, one of the desirable features of research is that it isn't too parochial and too geared to one project and that it leads to projectability and to some broader insights. There is no doubt about that. But once I have said that, you tell me what broad insights social science has produced? Tell me one.

PROFESSOR GALANTER: I would say that the Schwartz paper comparing the social control mechanisms in the kibbutz²⁰ reaches exactly the kinds of broad generalization applicable throughout many realms of legal phenomena. I think that Don Black in several recent articles²¹ has pointed out a whole variety of instances in which what might be called the "Schwartz Proposition" has in fact been found—

PROFESSOR ZEISEL: You have got me at a disadvantage. I haven't seen that paper! However, I still think that for the next twenty years findings will be specific and not general, and I find that the general findings are, on the whole, less interesting. I usually find them in the last sentence of an article that I have read, and then I have invariably the feeling that the sentence should have been left out. But I may be wrong. I don't want to quarrel with you. You are quite right in appealing to my major proposition that every scholar should find his own way to salvation.

I don't mean that my own particular ideas are better than anybody else's. I have sufficient respect for my colleagues not to think that. But, for instance, Ms. Fulton said she was shocked at the idea that one shouldn't ask questions if one doesn't know how to go about getting the answers. I have no objection to that. But I wouldn't know how to make the second step. I am willing to go the hard way, but unless I know how to make the second step, willingness is almost meaningless. I have many interesting proposals. But unless I have a way of at least attacking the problem and finding something, my proposal is not going to mean much.

The problem is this: there is an old division between the humanities and the sciences. In Germany, they call it the *Geisteswissenschaften* and the natural sciences. All of this is humbug! If you have a question, if you have a way of finding an answer, then you have a scientific enterprise. If you have only a question and no way of finding the answer, well, you may be a philosopher, you may be a poet, but you are not a scientist. Of course, there are examples that may be so interesting to you that they almost imply the answer. But generally speaking, I am for shedding all—and I am very serious—all the departmental distinctions and looking at the question to see whether it can be answered. If it can be and someone is interested in it, he should go ahead and do it.

20. [Ed.] Schwartz, *Social Factors in the Development of Legal Control: A Case Study of Two Israeli Settlements*, 63 YALE L.J. 471 (1954).

21. [Ed.] See, e.g., Black, *The Mobilization of Law*, 2 J. LEGAL STUDIES 125 (1973); Black, *The Social Organization of Arrest*, 23 STAN. L. REV. 1087 (1971).

DR. PADAWER-SINGER: In order for research to have any impact, in order for any research to be done on topics not simply of importance to the researcher or to the public alone, but to the researcher *and* to the public, in order for research to bring about some body of knowledge, some answers which can be generalized to a larger context, the researcher cannot stay just at his desk or in his lab. He has to go out in the world, and he has got to get the cooperation not only of the people he is going to study but the people who are going to be affected by his work. In policy matters, I believe sociologists can change parts of society and change them for the better. I think that a researcher can change them in a constructive manner and can influence policy makers. But he cannot influence policy makers if he stays in his corner. He can interest policy makers only if he works with them.

PROFESSOR ROWEN: There are different styles of working, and I think that it would be a mistake for us to work exclusively on the level that I think perhaps Professor Zeisel is suggesting, which is a version of a classical scientific model. But I think with respect to policy-related studies, at any rate, it is not uncommon at all for a study that ends up really being very fruitful to begin with neither the question, nor the methodology, nor the data. In effect, a study may be initiated because there is a policy that seems to be in trouble and a researcher who says, "Well, I am interested enough to explore it." Well, he explores it, and grabs hold of it, and sees something of interest and then sort of "messes around in it," and generates some data that looks interesting, or has some wild notion. It is not clear what the method is. The question changes, and frequently, what seems to be a precise question as posed initially may not be the right one. It gets redefined. It may get asked and be abandoned, and something else gets asked.

The research process is really very peculiar. People have to follow their instinct. Their instinct is enormously important. One of the things that is really clear about this business is that you can teach people tools, but you can't teach people research sense. That's really what we have been talking about in part: how does one convey research sense? What is important? What are the methods appropriate to the task? How does one tell whether the question is interesting, whether it is important? It is virtually impossible to generalize beyond saying, as I would, that one should be very catholic. Be sure that there are criteria qualifying certain things or that there are scientific

scales. But with respect to research method, that is another matter. I think that I would be very inclined to dispute the view that there is a rather well-defined set of methods that one can use.

PROFESSOR ZEISEL: I have no quarrel with you whatsoever. I would allow for any latitude in saying, "Let's make a first step, and let's see how the land lies." I didn't mean at all that such an approach was unacceptable especially if one goes into a topic that no one has ever looked at, or only looked at very casually. All I am saying is one must know the next step.

PROFESSOR ROWEN: No, not necessarily.

PROFESSOR ZEISEL: Forgive me, but can you give me one example where you don't know the next step at least in a rudimentary way?

PROFESSOR ROWEN: Well, it is a semantic question. One isn't necessarily terribly clear about where the next step is. Maybe the next step is a misstep.

PROFESSOR ZEISEL: Well, that's all right too.

PROFESSOR ROWEN: You go back to step one, and you begin again. People make all sorts of mistakes on next steps.

PROFESSOR ZEISEL: I am in complete accord.

MR. KONOPKA: I work with Rick [Huszagh] but in a new division of the National Science Foundation called Research Applied to National Needs [RANN]. I see many of the things that we are trying to talk about here. Many of the disciplinary questions that ascribe importance to discipline itself, like sociology, are often raised in the section over which Rick [Huszagh] presides. Questions which are raised with us are only problematic. That is to say, we have people come to us with problems, and we allow these problems to be solved by creating a sort of contract relationship between ourselves and researchers in which there is a third-party beneficiary who could be a legislator, a city planner, or a county planner—someone who has to act, who has a problem, who is probably making faulty, inaccurate decisions. I think the Foundation has to recognize the need for both applied and basic research, and it has to make this option open. What distresses me is the fact Dr. Zeisel raises that there is a world of distance between the social researcher and the people who are out in the real world like the legislator. We in the RANN program are trying to bridge the gap every day, and it is a difficult task. We have a series of programs that we are experimenting with, and I hope that

some of you who have already developed, or will develop, enthusiasm for the principles with which you are working, will be finally driven to help us bridge the gap between your research and the decision makers.

DR. HUSZAGH: Could I ask one very specific question? Should projects not be funded if the sociologist says, "This is an interesting question which may give us some insights in our own disciplinary framework, but it is not an important question for the purposes of the law"? In other words, it is a nice, powerful mirror for readjusting the disciplinary framework. How would you rate that kind of project?

PROFESSOR ZEISEL: I would give complete equality to the sociologists. Why should a lawyer's point of view be more important? The question is: is it an interesting question? In whatever general form, is it an interesting area to explore? If somebody tells me, "Wouldn't it be interesting to explore the age structure of judges?", I would say, "Well, mildly interesting." If someone raises the question concerning how judges are selected in Cook County as compared to the federal judiciary that would interest me more. I think, Marc [Galanter], you too must have a very clear preference for what interests you and what does not interest you. As for the question of money, that is a recent question in social science. When we studied the unemployed community in 1931,²² sixteen people were working for a whole year, and the whole study cost something like three thousand dollars. The money question is incidental.

DR. PADAWER-SINGER: I think that whether a question is worth studying is often as much a question of the values of the directors of the National Science Foundation as it is of the researchers. Some researchers are interested in things which would be interesting and applicable to sociology only. Others would be more interested in research which would perhaps benefit the public or the quality of life of various segments of the public. I think that some of the issues that we are discussing tonight are not just issues of research. We have all, I think, evaded the issue of values, of our own personal values, which guide our research. Are we interested in public interest kinds of studies that have as a goal the improvement of the quality of life or are we interested in exploring a problem that would fit very well

22. [Ed.] M. JAHODA, P. LAZARSFELD & H. ZEISEL, *MARIENTHAL; THE SOCIOGRAPHY OF AN UNEMPLOYED COMMUNITY* (1971).

in the context of a certain social theory? You [Dr. Huszagh] as a director of the Law and Social Sciences Program have to decide, and perhaps give rather equal value to these two occasionally conflicting claims, or decide between them. I don't think that we can tell you how to decide.

III. THE ORGANIZATION AND MANAGEMENT OF LAW AND SOCIAL SCIENCES RESEARCH

A. *Presentations*

PROFESSOR WALKER: Let me call to order this second session of the Conference. The topic for this morning is The Organization and Management of Law and Social Sciences Research. Our first speakers are Professor Robert Levy and Ms. Julie Ann Fulton of the University of Minnesota. They are co-principal investigators in a project which is supported by the National Science Foundation.

PROFESSOR LEVY: I thought what we would try to do is talk of our experiences doing an empirical study of child-custody adjudication in divorce actions. We propose to describe briefly the background of our study and our reasons for choosing the topic; then we will try to generalize from our experience and to suggest—without claiming any great originality for our insight—some of the problems posed by large-scale empirical research of law and its administration.

Custody adjudication has long had special interest for me. First of all, it interests me because a large majority of what are formally and theoretically judicial dispositions are in fact private decisions by the parties to the litigation. Thus it has been known for some time that most custody awards are arranged by the husband and wife with only pro forma review of their decision by the trial judge. Yet these adjudications also involve a substantial amount of authoritarian decision-making by judicial officers which is subject to only vague and inconsequential statutory or common-law constraint. When the husband and wife cannot agree, the dispute is governed by a legal standard—"the best interests of the child"—which provides guidance only by assuring the trial judge that the chances of reversal on appeal are minimal. Moreover, the issues are extremely "relevant" in a period of great social change. As women seek "liberation" from housekeeping and child-caring responsibilities, it is likely that more men will be interested in obtaining custody of their children. If the popular journals can be believed, the traditional preference for the mother as cus-

todian of children after divorce is already on the wane. The issues are also interesting because they tap deep emotions. It was natural, then, that my interest in empirical research would focus on custody adjudication.

The immediate impetus to our research project came from my experience as Reporter of the Uniform Marriage and Divorce Act. A group of highly competent and sophisticated lawyers with extensive experience in divorce litigation were unable to agree on what really happens in custody adjudication, much less on the policy judgments that should properly guide nationwide legislative action. Unless the facts were responsibly gathered and reported, rational policy compromises would never be possible.

It was fortunate, yet debilitating, that about the same time a private foundation was coincidentally interested in giving money to the University of Minnesota Law School for empirical research. It was fortunate because without that help no project would have been begun; it was debilitating because the project was launched with a budget carved and scraped to fit the amount expected rather than derived from any independent judgment about what a useful empirical research project would actually cost. We started with a "sum certain" in the bank. Rightly (if the proper question is whether we are going to report any information worth knowing) or wrongly (if the proper question is how quickly *any* research results giving due credit to the Foundation will be published), we eventually decided to design a study that would cost considerably more than that amount to execute. Thus even as we thought through the issues, designed questionnaires, began to hire, and tried to supervise staff, we continued in the "ivory hunting" business. As our presence at this conference indicates, we were successful in persuading the National Science Foundation to provide some additional funding—once more on a "sum certain" basis—the portion is not in fact segregable—that is, the portion can be reported only as a product of the larger endeavor; and we had to pay a commission for the grant—by agreeing to expand the scope of our sample from three to five counties (at considerable expense). Moreover, twenty-eight percent of the NSF grant had to be devoted to university overhead costs. Our foundation benefactor managed to have its entire grant devoted to the subject—not necessarily because it has more leverage with the university than the federal government, but the fact is nonetheless striking. For reasons that will become clear, we are still in the fund raising business.

Before we generalize about empirical research in law, Julie will describe our research design and the implementation difficulties we have faced (and only occasionally solved satisfactorily).

MS. FULTON: When we first discussed a research plan, we decided to take the traditional approach to constructing a study design: we planned to examine "all" existing literature and research on the subject of divorce and custody adjudication, in order to formulate questions that we wished to answer, and to combine these elements—the existing data and our questions—into hypotheses to be tested by whatever applicable, sophisticated techniques were available. Intention is one thing; implementation is another. We soon discovered that there is very little research literature on the subject of divorce, with the exception of census-type tabulations.²³ There is even less on the subject of custody adjudication. Without information about who is divorcing today, why they are divorcing, how they are divorcing (that is, the judicial processes through which the couple must go in order to be divorced), and how the divorce affects the parties financially, socially, and psychologically, the formulation of specific hypotheses with regard to nuances of custody adjudication seemed a bit premature.

We decided, therefore, to use the money that we were given to find out about the area of divorce in general in order to answer some of our own questions about divorce from that information, and to allow other people to use the information to ask and answer some of their questions. I guess our decision was what Dr. Singer categorized as a high-risk decision, but we were imbued with great curiosity about this area.

Divorce can be analyzed dynamically as a process, or statically as a fact (as is done in divorce-rate research). Divorce is, after all, a process or, as current sociological terminology would describe it, a "system" with numerous in-puts, feedback, and decision-points, at which the participants take one path or another.

We opted for a more dynamic approach in our research than mere statistical tabulation of custody arrangements and alimony awards. Within limits, we decided to take a systems approach and examine for a particular divorce case as many relevant pieces of information as we could obtain. A "danger" for us at that point, and sometimes even now, was the impulse to construct too ambitious a model for our

23. See H. CARTER & P. GLICK, *MARRIAGE AND DIVORCE: A SOCIAL AND ECONOMIC STUDY* (1970).

research design. Ideally, it would be appropriate in each divorce case to gather court records, to interview both husband and wife, to obtain whatever ancillary court records are available (for instance custody investigations and financial investigations), to interview all lawyers, referees, and judges, and, possibly, to interview the children of the couple and their school teachers. If this sounds as if we did not manage to resist the temptation to over-construct our model, bear in mind that we had pared it down considerably by excluding friends, clergymen, and counselors consulted by the couple in their decision-making process, and by excluding interviews with the social workers who did the custody and financial investigations in particular cases. Even so, our ideal plan seemed too ambitious. We decided to start by gathering court records, custody investigations and interviewing the couples, with the intention of adding lawyer interviews in the "next" (as yet unfunded) phase of the research.

We began by establishing a sample. The task was actually more complicated and much more time-consuming than might at first appear. I think it is particularly important, as Dr. Singer said, to be able to generalize findings to the population of interest. This is overly ambitious obviously, but what we are attempting to do is to set up as accurate a model of this process as possible, use the model to answer certain of our questions, and provide data for others. You must have not only a representative sample of the population but also a sample of adequate size to meet the statistical tests of significance. Yet you must know what you are sampling. This means knowing a little bit about the area, which you can't know if there are no data. Thus we faced a vicious circle. If we had simply drawn a random sample of all divorces within a particular year, we could expect, by chance, to obtain cases representing many kinds of divorce processes; but unless our sample was extremely large, we could not necessarily expect to obtain sufficient numbers of each type of divorce process to permit statistical testing by type. Our first step, therefore, before drawing a sample, was to examine as many court records as we could and to talk to as many practicing divorce lawyers as possible in order to establish a preliminary taxonomy of divorce processes. I say a "preliminary" taxonomy because in a field of research as new as legal-social science research, it will undoubtedly be necessary to revise and expand preliminary findings many times.

We decided that there were at least three categories of divorce process which differed enough from one another to merit separate

analysis: the "consensual" divorce, in which it appeared from the court records that the couple and their lawyers were able to arrive at the terms of the divorce without outside intervention; the "contested" divorce, in which the issues in dispute had to be finally resolved in trial before a family court judge; and the "contested-stipulation" divorce, in which the court records indicated that the case would be fully contested but a last minute (for our purposes, twenty-four hours before the hearing) stipulation was achieved—presumably, we felt, with *some* outside intervention (what kind of intervention, we hoped to find out). In the course of our study we have expanded our categories to eight, each of which differs sufficiently from the others to be considered separately.

Our initial concern was with how custody decisions are made and how custody arrangements affect the children. It seemed obvious that a consensual divorce as compared with a contested divorce implied different decision-making processes. But just how they differed and why one couple came to be divorced in one way rather than another was not clear. We felt that there were probably differentiating factors which influenced the direction of the divorce process; in addition, the subsequent adjustment of the parties and their children would probably differ in accordance with the type of divorce experience.

In our conception of the research, then, the type of divorce became our main variable. I say "main" because as we formulated our questions it became clear that we viewed the type of divorce as a link variable, that is, one that links other variables and is at the same time both dependent and independent.

Having classified our main variable, "type of divorce process," we had to determine how many of each kind of divorce occurred in 1970—the year we had chosen to study. I go through this much explanation because it turned out to be important not to have drawn a sample before this point. We found that we had to take the entire group of cases for 1970 for all but the consensual group of divorces in order to have sufficient numbers for significance testing; in the consensual group we were able to draw a random ten percent sample. We arrived at a total sample of 585 cases from the five Minnesota counties we were studying.

Once our sample was drawn, we were able to gather the court records for each case in the sample. We found that the county records clerks were generally cooperative but at the same time unwilling to allow our law student researchers free access to their records. In two

of the three urban counties, the records clerk insisted that he personally find and sign out to the student each file even though our students worked at a table within view of the clerk the entire summer. This kind of bureaucratic care undoubtedly facilitates the recordkeeping activities of the court—and insures that few files are irretrievably lost—but such care also significantly slowed down our research effort.

We decided early in the research project to use our time and money primarily for data gathering; the analysis, we felt, could wait. This was probably a wise decision. We knew that a retrospective field study of divorced parents would involve difficulties in locating the parties securing their interest and finally, obtaining interviews. We knew that this would take time and effort (and therefore money), but we had no idea how much. One of our very early “findings” was that people move after a divorce. To be sure, we expected that one of the parties would be gone and living elsewhere, and we also expected that for financial reasons, perhaps, both parties would move from the homestead. But we were truly surprised by the frequency with which recently divorced persons move—sometimes as many as five or six times in a year. We were also surprised by the number of persons who seemed to withdraw from society—who lived without a phone or with an unlisted phone, who cancelled credit accounts, and who moved without leaving a forwarding address.

At the end of a year of determined effort we had located seventy-two percent of the 1170 persons (585 cases) originally chosen for inclusion in our sample. For every five persons we located and contacted, three agreed to be interviewed and two refused. We eventually obtained slightly over five hundred interviews, representing, because of the possibility of one or two interviews per case, just under two-thirds of the cases we had originally selected. We believe that this rate is rewardingly high, given the resistance to research of an over-surveyed population and the personal and emotional content of the interview subject matter.

It is proper, of course, to ask, “How representative are the responses of volunteer subjects?” Here, our utilization of court records is very important. In contrast to studies which rely upon one source of data, we have used a multi-source approach. By comparing the court records for those cases in which we have an interview with the court records for those cases in which we have none, we will have some measure of significant differences between the two groups. If there are no significant differences, there is some basis for confidence in

the representativeness of our interview sample; if there are differences, we will have some indication of at least some of the ways in which they differ—and this information is rarely available in other studies. In addition, we will be able to compare court records for cases in which only one party has agreed to interview with those for which we have both a husband and a wife interview. In this way we will be able to state with some degree of confidence that the responses for a particular group are representative of the whole or else that they represent a group which differs from the rest in a particular way.

We made two major decisions about the interview in advance. First, we decided to take advantage of the "since I'm here" situation and to include many more questions in the interview schedule than we had originally planned to ask. Our experience and the experience of other researchers had convinced us that there is a substantial loss in terms of numbers when you attempt to re-interview respondents. Also, we had two disciplinary interests—law and social science—and the numbers and kinds of questions of interest were inevitably compounded. Finally, a few trial interviews indicated that a lengthy interview neither alienated nor tired our respondents; to the contrary, respondents who agreed to be interviewed were more than willing to cooperate. Our longest interview lasted over five hours! The average interview lasted approximately two to two-and-a-half hours.

Our second major interviewing policy decision was to tape record every interview. We made this decision because of the length of the interview, because of our judgment about proper interviewing technique, and because of the chance of interviewer bias in recording responses. Obviously, this decision cost dearly both in time and money; but we believe that the decision was correct despite its effect on our budget. We spent a substantial amount of time training students in the techniques of good interviewing, one of which is to give the respondent as much undivided attention—and, ideally, eye contact—as possible. We tried to make our interviewers expert in listening carefully to what the respondent said and in phrasing follow-up questions. This is not possible if the interviewer is busily recording the essence of the preceding response. Sometimes, in order to avoid great gaps in the interview, an interviewer will quickly write down a few key words or paraphrase an answer and proceed to the next question. Often in this summarizing process the real meaning of the response is lost. We felt that if we had the entire interview recorded, the interviewer could later transcribe the responses almost verbatim, and the tapes would also allow for

cross-validity checks among interviewers. In addition, interesting comments could be used verbatim in our final report. This decision meant, however, that each interview was gone over at least twice—once in conducting it and again when it was transcribed. At the student rate per hour, this technique proved exceedingly costly.

At the end of each interview we asked the respondent to sign a consent form allowing us to talk to his or her lawyer about the case, to talk to the children's teachers or physicians if we felt it was necessary, and to examine any social or court-associated agency materials which might pertain to their case. For the most part, the respondents were willing to sign this form. Some refused completely, and others gave permission for us to contact only certain persons. A signed consent form later proved invaluable when we met with resistance from certain lawyers whom we had to contact with specific questions about the case, and this form was also of help in overcoming the resistance of judges who were initially unwilling to release confidential custody investigations from court-associated agencies. We were not able to obtain custody investigations for each case in which one was conducted; but again, as with the interview, our success rate was quite high.¹

The first phase of our data gathering is almost finished. We hope to obtain funds to interview the lawyers in each case before too much time passes. In the meantime, while we await the response to our grant application, we are coding and analyzing the court records, the interviews, and preparing for a content analysis of the custody investigations.

Content analysis of official documents is a research methodology which will undoubtedly be used with greater frequency as the field of legal-social science research develops, but at this point it is still rather new and therefore unstandardized and depends heavily on the insight of the particular researcher. Each researcher who attempts to use this technique, moreover, must face the fact that official records are gathered and maintained for official rather than research purposes and that the data are not always in the most useful form. In addition, what is recorded often tells only half the story; sometime in the future of social-legal research it will be important to study what available information is *not* recorded and why—a sort of "non-content" analysis.²

We are interested in the *effect* of different kinds of intervention in the divorce process and in the *effect* of differing kinds of divorce

arrangements on the parties and their children. In social science research, "effect" is a very difficult concept to define much less to "prove." Nevertheless, we felt that there may be different post-divorce situations that could possibly be linked to various antecedent conditions. In order to study these post-divorce situations, however, it was necessary to allow a time lag in order for the "effect" to take place. We wanted information from several points in time—pre-divorce, during the divorce, and post-divorce. Our options were to do either a test-retest study—which would be extraordinarily expensive in terms of time and money and respondent loss—or to do a retrospective study of the kind we undertook. A retrospective study has the disadvantage that respondents may not remember many details about what happened before, during, and right after the divorce and also the disadvantage that files may be lost from lawyers' offices or courts in the meantime. On the other hand, a test-retest study involves the risk that merely by interviewing the respondents at the time of the divorce *you*, the researcher, may become an intervening variable. In a way, we have not completely eliminated this problem. In the interview we ask parents if they are happy with the divorce arrangements and, if they are not happy, whether they have any intention of going back to court to seek a change. We are a little worried that we may prompt some persons to act who might not otherwise have done so. We intend to check court records every year to see how much post-decretal litigation there has been, but it will not be completely possible to factor-out the effect of our interview.

You may be interested in a few, what I suppose Professor Zeisel would call, "middle range" observations drawn from our experience. Part of our research planning time was spent "learning to talk" to one another. In any interdisciplinary project a substantial amount of teaching and learning must be accomplished if ideas are to be communicated between researchers. I, for instance, had to read quite a bit in the field of family law, and Bob had to spend time familiarizing himself with research methodologies and statistical techniques. Of course, one of the benefits of collaboration is that one person need not be proficient in all areas, but it is still necessary to know enough about the area of the other's expertise to explain and understand reasons for doing things. In addition, we had to convey enough of our special interests to one another so that the data gathering instruments and techniques of analysis would actually tap data which satisfied our interests. Sociologists are not interested in precisely the same things

that lawyers are interested in—although my personal observation is that a sociologist can be convinced of a far greater breadth of relevance in data than can the average lawyer.

Because we are not based in a large, on-going research organization we had to rely on part-time student researchers in the data gathering stage. And, since we could not promise long term support for the students we hired, it proved to be a buyer's market. Class assignments and exams took precedence over the research project from their point of view, and we were reluctant to be too rigid in our job requirements because we had an investment of time and money in their training and could not easily replace them. As it turned out, the uncertainties about renewed funding in mid-project meant that we lost nearly one whole staff and then had to recruit and train a completely new one when the monies were forthcoming. I have talked with enough other university-based researchers to know that our experience was not unique. To add to our difficulties, our law student researchers sometimes felt that the tasks of large-scale survey research were beneath them. Because the nature of our research meant that the portion of interest to them—namely the interviewing—came in spurts, we gave them other parts of the research to work on in the meantime, such as coding or locating respondents. These tasks are always tedious and even the most dedicated researcher sometimes wonders why he or she ever became involved. Yet persons trained in social science and particularly in research seem to be more accepting of the things that must be done in order to produce a research report than are law students. Lawyers *could* be trained to respect not only the findings of research but also the tasks of research, but I doubt that very many lawyers are willing to spend the time it would take to gain this appreciation. My guess is that lawyers will probably enter into legal-social science research on primarily a consulting basis for some time to come.

The administration of a research project is a full-time job. The administrator should administer. He should not conceptualize, design research instruments, gather or analyze data. Those are all jobs for other persons on the research staff—or, ideally, they should be. Too often, however, a university research project must "make do" and use one or two persons to do the job of several. Again, the prospect of a short-term project mitigates against finding more than a few qualified persons who would be willing to work full-time without much job security. I believe that a research proposal should include a position

for a research administrator who would be a liaison between the principal investigators and the research staff, especially when the principal investigators are unavailable (as often happens when the research is only part of their professional duties.) He also would be responsible for the smooth functioning of the research once the research plan is laid out.

Many problems of research could be avoided if researchers were more willing to share their failures and errors as well as their successes. Not only should negative findings be published in research reports, but also there should be some way to publish or communicate the techniques and decisions that were part of the research that did or didn't work. There are very few good handbooks on research, and these tell little more than general ways of "how to do it"—almost none tell how *not* to do it! We could write several pages on what we learned about approaching prospective respondents—about the efficacy of telephoned versus mailed approaches—and on the steps we went through to locate people in an urban setting. Some of our efforts were spent in profitable ways, and some were a total waste of time. When our results are finally published, we hope that our editor will allow some of these "findings" to be published as well as the usual kind found in research reports.

PROFESSOR LEVY: Let me turn to some of the "larger" problems of management of what lawyers at least would consider large-scale empirical research. It should come as no surprise that we believe money is the empirical researcher's "devil's rum." Even if funding is not initiated and continued in a "sum certain" fashion, there is no real basis for estimating in advance how much the data gathering is likely to cost. To our great surprise, we had to devote about one year of a highly skilled graduate student's time solely to locating "hard to find" respondents. Whether the expenditure was worth the interviews we obtained is difficult to say, but a decision had to be made, the sample had been established for some time, and we had already relied on the sample for other purposes. This much is certain: funds used for location could not be used for other purposes. Perhaps it is best to assume that even the most carefully budgeted research project will require a large slush fund for unpredictable but vital expenses. But such a slush fund is difficult to obtain when the myth is widespread that legal research is cheap. When funding agencies, which are used to supporting social science research in large quantities, talk to a lawyer, they seem to have a library in mind. I

think that is in part because there is a lack of sophistication about the importance of empirical research in law, and a lack of sophistication about the influence that careful, thorough data about law can have with policy makers.

Nor are all funding agencies available to researchers. We discovered that some funding sources could be called upon only after playing university politics because the university believed in establishing "internal priorities" for seeking money from favored and generous sources. Continuation funding is an especially time-consuming and agonizing task. It is not always easy to account adequately for the results produced by those tedious hours spent by student research assistants. Even if past expenditures can adequately be justified, the researcher has to face what might charitably be called funding agency jurisdictional disputes. "We won't bail out the _____ Foundation," was a response we heard more than once. On one occasion we were informed, "We are the largest Foundation in———; *we* initiate and get sole credit for research *we* support." Moreover, it is often not enough that the funding agency is interested in the topic of the research; the researcher must be attuned to the personal idiosyncrasies of the foundation's executive director, to be able to satisfy his need to feel socially useful, to contribute to his belief in his eventual acceptance into that "Great Data Bank in the Sky." This is what I call the "Foundation Executive Syndrome." (Needless to say, the less complimentary implications of this statement have no relevance to the staff of our financial host at this conference!)

Planning a research design is a vital part of any research project. We spent literally months conceptualizing the problem, and there is no doubt we would have benefited if we had had more time for planning. We were lucky to have had a grant before we had a research design; but we paid a price for that comfort. There were great pressures to get started—for example, to prove that something was being done with the money and to keep research assistants busy as they were hired. Normally, of course, research planning is an unsupported activity and must be accommodated to the normal pressures of law school life and all those institutional cares for which law faculty members typically want to share responsibility personally. We believe that much would be gained if small grants were available from funding agencies for research planning. Obviously, the idea has problems: if the funding process is democratized, money will be wasted; yet meritocratic administration may add to the elitism which some people believe

is already prevalent in legal education and social science research. We suspect that some of these problems could be minimized if the law schools themselves devoted money to such purposes. In addition, funding agencies should be more willing than we think they now are to fund at small cost what might be called pilot or exploratory studies—with an implicit promise that if the results seem promising, a larger study will be funded. Presently, investigators are pressured to make extravagant promises to funding agencies. So long as the costs of research cannot be programmed in advance and the pursuit of complex problems beyond the initial step continues to be so difficult, large-scale empirical research on the social and behavioral consequences of legal institutions will best be encouraged if researchers can begin in a small way with some assurance that their efforts will continue to receive support. It seems to us that funding agencies should add to such grants the services of experienced research consultants for grantees who have undertaken such small studies—not for “site visit” purposes when renewal or expansion is sought, but rather to confer with the grantee and to help him refine and improve his research design.

I should add a personal note about the difficulties of on-going supervision of a large-scale study. During the year that the National Science Foundation paid one-third of my salary, I found it impossible to consider myself a “two-thirds faculty member” although I was teaching one course less than a full load. Committee responsibilities, faculty meetings, informal discussions, and student conferences all continued unabated. My commitment to the institution did not diminish by one-third. Our law school was immersed, it is true, in the process of choosing a new dean—and I could hardly ignore that task; yet I would guess that each budding lawyer-empiricist will find some vital and time-consuming institutional task which he believes it would be irresponsible to ignore. I have heard that “law-types” worry more about their teaching and their school than may be good for them or their institution and that a sabbatical year will inevitably be wasted if you fail to leave the premises. I am sure that these admonitions contain more than a kernel of truth. Yet the problem of sufficiently divorcing yourself from the day-to-day concerns of the law school remains.

We have emphasized the difficulties we have met and the problems we have been unable to solve. Yet because we believe in the project and in empirical research about law and its administration, we do not want to convey the impression that if we had it all to do over

again, we wouldn't. The work has been frustrating, enormously time-consuming, perhaps not as valuable as we hoped it would be (yet surely more valuable than at times we expected it to be) but worth the time and effort to us. We trust that in the not too distant future you will be able to decide for yourselves whether the project served more than our own education.

PROFESSOR WALKER: Thank you very much.

The third speaker this morning is Justice Winslow Christian of the National Center for State Courts.

JUSTICE CHRISTIAN: Professor Walker, and members of this conference, I am, by family tradition, respectful of academic work, and my recent experience as Director of the National Center for State Courts has increased my appreciation of the power that the academic has to influence, by his ideas and his studies, the course of institutional evolution. I therefore regard as so much camouflage, Professor Zeisel's modest statements of last night about the distance between the scholar's study and the world of affairs. Important reforms—for example, changes in the system for personal injury reparation and changes in divorce and family law—have quickly followed the publication of studies by co-workers of yours whose names are known to all of us.

As one whose professional life has been devoted not to scholarship but primarily to action in the fields of politics, administration, and adjudication, I suppose that I speak as a "consumer representative" in addressing the subject: The Organization and Management of Law and Social Sciences Research. Here I must outline briefly the responsibilities and the aspirations of the National Center for State Courts, the institution that I represent. This organization is now two years old, having been created for the purpose of guiding and energizing the reforms that are needed in the judicial branch of state government. The State Courts Center was organized under the direction of officially selected representatives of the court systems of each state. It is governed by a board of twelve active state judges and is funded by grants from the Law Enforcement Assistance Administration and several private foundations; state funds are also being appropriated to this work. Entering its third fiscal year, the Center is operating with an annual budget in the vicinity of three million dollars.

The staff of the Center is organized along functional lines. Two units in the headquarters staff have responsibilities for research in areas that will be of concern to participants in this conference. The first is the Division of Law-Based Research. The second is the

Division of Systems and Technology. The Divisions of Law-Based Research and of Systems and Technology have responsibility for funding, designing, and monitoring research on court problems. That work is conducted both by staff of the State Courts Center and through outside academic researchers.

Since the National Center for State Courts is comparatively new, and came so recently to the business of designing and sponsoring research programs, my comments on the subject of organization and management of research activities must largely take the form of declarations of intention; I cannot yet report to you on the basis of solid experience.

Our responsibilities and aspirations are not primarily academic or intellectual. They are, by analogy to the physical sciences, in the realm of engineering rather than of pure science. We are trying to put into practice in the courts, through action by judges and legislators, ideas derived directly from judicial experience or from academic or other research. Thus the topics of investigation for which we seek support will be selected with strategic insight into the needs of the state court systems; early and significant impact is sought. Let me outline, as an example, a strategy of research designed to improve state judicial machinery. It takes no special knowledge to recognize that a common deficiency in the court systems of the states is inadequate financing. That appreciation suggests a search for a research topic and research tactics, that may arm state leadership, judicial and legislative, to improve the funding of court services. For that purpose, the State Courts Center produced a rough design of a study, selected an academic person as principal researcher, and cooperated with that researcher in the production of a study of the financing of state judicial machinery, considered in the light of the doctrine of separation of powers.

This national survey, now nearing completion, is being conducted by Dr. Carl Baar, a political scientist. The results will make known to the clientele we are trying to assist (state legislators and state judicial leadership) the experience of various state court systems in trying to bring to public and legislative attention the financial needs of the courts. We expect that Dr. Baar's research will begin immediately to influence events. The experience is already quite varied: in Colorado a fully integrated judicial branch budget is prepared by the management of the judicial branch. It is submitted directly to the legislature, while an information copy is given to the budget office of the executive branch. In contrast, the neighboring state of Utah has no centralized

judicial budget. Judicial and court personnel salaries simply appear in the salary structure of the general programs and services of that state. No one thinks in terms of an overall, unified responsibility for court operations.

Another important source of difficulty in the functioning of American judicial machinery is that the courts are expected to handle a great deal of work for which they are not very well fitted. In the area of criminal adjudication, much has been written and much is now being done, without any intervention on our part, to divert from the criminal justice system cases involving alcoholism, drug abuse, and other "victimless crimes." Therefore as important as diversion strategies are in criminal justice, the priority scale does not call for us to adopt this as a principal area of activity. Instead, we have sought funding (governmental and private), to stimulate academic research on the possibilities of diverting from the courts classes of *civil* litigation in which the services of the court are not especially apt. Here again, tactical judgment influences research design. The research proposals that we have developed concentrate on promising areas which have not already had close attention. Thus, much is being done now on no-fault automobile liability insurance. We are disseminating information about no-fault, but we need not sponsor investigation in an arena that is already so active.

Other topics do call for new activity. We are studying the possibilities of extending and spreading to other states Washington State's scheme of "non-intervention wills."²⁴ We are also analyzing, and documenting for other systems, New Jersey's success in developing special procedures for handling medical malpractice. These procedures have promoted settlements, taking these cases very largely out of the court system.

Another area that calls for research is the application of technology to the internal operations of the courts. Recent years have seen the beginning of electronic data processing in court calendaring and in the collection and employment of judicial statistics.

There are dormant areas in which technological resources are apparently applicable. To stimulate the employment of technology, the State Courts Center has an experiment with the employment of video technology in a number of applications potentially useful to the courts. Some possible uses for this technology are: the examination of a

24. WASH. REV. CODE ANN. §§ 11.68.010-.040 (1972).

witness at a distant site by video transmissions; and the use of video tape to pre-record trial testimony, thus allowing a judge, by ruling on objections at a special hearing, to edit the tape and present a clear record to the jury. The Center's project will build on work that has been done in Michigan, Ohio, and other states by demonstrating nine or ten video applications. The results will be published for the use of the legal profession and the courts.

The second stage of the video project will explore the *effects* of video technology on the judicial system. Technological innovation may have effects quite unintended and quite undesirable. If video tapes were to be used as the basic trial record for appellate review, one wonders how well the doctrine will survive that an appellate court should not disturb trial court factual determinations which are supported by substantial evidence. If the appellate court were not looking at a "cold transcript" but were instead viewing the testimony of witnesses and receiving all of those subliminal signals about the personalities of the witnesses that the jury and the trial court saw in the first place, we might undermine unintentionally the integrity of trial court determinations. The inquiry just mentioned is one that lawyers are not preeminently fitted to pursue. Lawyers or judges need to be involved in identifying the issues, but psychology and communications science must be brought to bear. Thus in response to our practical and rather prosaic interests, a research proposal will be developed which will be of interest to scholars in several fields.

You will see that an opportunistic research strategy has been followed: first, we select an important area that invites inquiry; and second, we seek funding to support a set of research projects in that area; finally we try to match the project which we have picked with a qualified investigator.

It would be unsound for the State Court Center to try to build internal research capacity to deal with the great breadth of learned specialties that must be brought to bear on the range of problems I have mentioned this morning. Therefore we have communicated with the Association of American Law Schools in order to find law teachers who may be induced to work on court problems. Yet, research recruiting has depended too much on casual personal scouting. A way must be found to search more comprehensively for the academic researchers who should be undertaking this work.

This afternoon we will discuss the monitoring of commissioned research and the use of the results of such research. These topics

were not assigned to me, but I want to say a word about those matters because the Center's research goals are not merely scholastic; our aim is to achieve changes in institutions. Therefore we monitor rather closely the work which is commissioned. We visit the researcher once in a while to be assured that he has not forgotten what the project is supposed to accomplish. Demonstration projects are subject to the same hazard of getting off the track. We are now conducting in four state appellate courts a demonstration of some ideas concerning staff utilization that are generally known in the trade. Specifically, we are demonstrating the idea that a centrally-managed research staff placed in an appellate court can help the court increase productivity without causing the judges to lose personal grasp of the cases for which they are responsible. The central hypothesis is that if you add staff to the court, you should add to the central staff rather than to the staffs of individual judges. The participating courts are: the intermediate appellate court in Illinois in the district covering Cook County; the Supreme Court of Virginia; the Supreme Court of Nebraska; and the state-wide Appellate Division of the Superior Court of New Jersey. The project director, Professor Daniel J. Meador of the University of Virginia, and I at intervals of a few weeks meet with the staffs and with the participating courts. We find invariably that between visits someone has thought of a new idea that he wants to try out and the whole project is in danger of going off down some byway. It may be a very useful byway, but it is not within the scope of what we promised to do with the funds that were made available to us. Monitoring is essential if the sponsoring agency is to maintain its accountability for results.

Some of the research efforts that I have mentioned will result in clinical demonstrations similar to the appellate justice project that I have just mentioned. If, for example, one can find in the New Jersey experience with the management of medical malpractice cases something that seems to be worth trying out in other high volume jurisdictions, we would propose to go to another jurisdiction, say Los Angeles, and help the trial court there to design a voluntary arbitration scheme based on New Jersey practice. Through demonstration, we can develop useful experience that would support published work to spread useful ideas across jurisdictional lines. That would be a "midstream" effort to implement the results of the research project.

Of course, researchers are encouraged to publish; we have included in several project budgets provisions to subsidize publication of studies that should be preserved in accessible form.

Finally, we propose to take the products of research, when adequately demonstrated, before the newly created Council of State Court Representatives, which is the basic governing institution of the State Court Center. The Council is made up of officially selected delegates, one from the judicial system of each state. We will ask this Council to develop standards having to do with court functioning. These standards would, of course, not be binding, but would be advisory. Advisory standards can have great influence when you are dealing with a fellowship that is small enough so that ideas of emulation and peer approval are significant. Such standards, applicable to the courts, can have a very large impact, translating into actual institutional change the results of the research and the demonstrations that we have been speaking of his morning.

I want to say something about a weakness that I see in some law and social science research proposals. Americans are much enamoured of the research strategy of the physical sciences; a striking example is seen in discoveries of recent years concerning the molecular basis of genetic inheritance. Mendel deduced laws of genetic probability, and Darwin proved that genetic material changes over the course of generations. But the genetic mechanism was not understood until research made known what had always existed, undiscovered, within living cells: helical pairs of chain molecules that carry coded signals governing genetic inheritance. These beautiful discoveries have changed biological science.

I submit that the attempt, through research, to make discoveries of comparable importance in law and the social sciences is illusory. It is simply not true that there exists, undiscovered, a scientific principle that will settle the question whether an urban trial court should employ a master calendar or individual calendar system. The improvement of laws and of legal institutions depends very largely on low-grade tactical insights resulting from the exchange of information concerning random variations of experience in many jurisdictions. I question whether it is useful to hang the trappings of scientific method on inquiries concerning the workings of courts.

What are the prospects for improving funding in the area of court-related law and social science research? The American Bar Association has undertaken an elaborate effort to obtain federal legislation creating a new funding institution, the "National Institute of Justice," which would have some responsibilities in the areas which we have been discussing. An early version of the proposal was developed by the American Bar Association, the Association of American Law

Schools, and the Association of American Law Libraries in 1967.²⁵ The legislation did not succeed.²⁶ In 1972 the general proposal to create a federally funded institution to take the lead in improving the organs of law and justice received wide publicity in the profession through an article published by the Executive of the American Bar Association.²⁷ Thereafter the American Bar Association picked up the proposal officially; current studies, which will lead to a legislative program, got under way. Several law teachers have been active in the effort—most notably Professor Geoffrey C. Hazard of Yale University who has participated in several capacities as consultant or draftsman.

A federally funded institution will doubtless be created whose functions will overlap to some extent the functions of the National Science Foundation and the Law Enforcement Assistance Administration. The Congress would do well to look to the successful experience of the National Science Foundation in designing such a program.

PROFESSOR WALKER: Our last speaker for this morning's session is Professor Henry H. Rowen of Stanford University.

PROFESSOR ROWEN: Thank you. I am going to address my remarks to the organization and management of policy-related research. This is non-traditional research in terms of most research that is done in universities. It is also a type of research with which I have had a good deal of experience. I spent some six years in the federal government dealing with policy matters, have been head of a research organization, and, earlier, was a researcher.

I would first say that the role of law schools in the development of what has become known as the policy sciences has not been as great as it might have been or as it should be. Perhaps in the course of the discussion it would be useful for us to address the reasons why.

Policy-related research is of several kinds. The most obvious is research that directly concerns government programs and policies, such as the head start program, job training programs, and police operations. This research might be in the form of cost-effectiveness or cost benefit studies or might, more generally, consist of the evaluation of possible new or existing programs.

The second category is research on social problems. This cate-

25. See A.A.L.S., PROCEEDINGS: REPORTS OF COMMITTEES AND PROJECTS Part I, Section II at 24 (1967).

26. See, e.g., H.R. 13584, 90th Cong., 1st Sess. (1967).

27. Early, *National Institute of Justice—A Proposal*, 74 W. VA. L. REV. 226 (1972).

gory might include work on juvenile delinquency, or drug addiction or, to mention an area that I have been working on recently, the question of legalizing gambling.

A third category is research on social phenomena, for example, the migration of people. This is an interesting phenomenon in which there may be public policy implications. Another example of a social phenomenon worth researching is early childhood development.

A fourth category is research on methodology. For example, social experimentation as a methodology presents various opportunities and problems. One problem is that in designing and carrying out a social experiment, both ethical norms and government agencies dictate that there can't be losers, even though there are losers in real life. To illustrate, my former organization, Rand, is deeply involved in a national health insurance experiment in which insurance is being provided to a large group of people. The way the insurance policies have to be written, no one can lose. That does raise a question about the validity of the experimental results.

PROFESSOR ZEISEL: Could you elaborate what that means?

PROFESSOR ROWEN: The concept is to test the response of individuals faced with various possible insurance schemes which might be offered in a national system of health insurance but which are not covered by present commercial policies. The idea is also to look at how different suppliers respond to different arrangements. People who participate in the experiment are asked to give up their present health insurance arrangements, if they have any, to enter this experiment. It is a constraint imposed by the federal government that anybody who participated can't be made worse off because of the experiment. I mention this as an example of a methodological problem of possibly serious dimensions.

What are the problems connected with policy-related research? One characteristic difficulty is that existing data are usually not adequate. Cross-sectional data at a point in time are usually not satisfactory, and longitudinal data take a long time and are costly to generate.

In addition to data problems, there are large areas that are extremely important that just haven't been worked on or have been worked on very little. I mention only one particular area: the problems of policy implementation. What are the ingredients necessary to carry out a good decision? Although a great deal of work has been done on how to make a good decision, little has been done on how

to carry it out. Many recent failures of public programs and policies seem to result from very little attention being paid to understanding the instruments by which the "Great Society" programs were to have been carried out. An important gap was in determining who was actually going to do what, and what incentives there were for seeing that services got efficiently delivered once legislation was passed. There were vast numbers of people doing things very inefficiently.

I turn now to the difficulty of doing policy research in the academy. There are organizational dis-incentives within universities; there are dis-incentives to inter-disciplinary work and to policy studies. These dis-incentives vary by disciplines. In some disciplines, the dis-incentives to do inter-disciplinary and policy-related work are pronounced. The advantages of the university as a base for research are well known. It has independence, a high quality of people, and access to students. And the disadvantages in carrying out inter-disciplinary research are also fairly well known: narrowness of focus, the lack of interest in policy, and in some cases, the positive discouragement of policy studies. Policy research might take people away from a concentration on research that in many cases is really essential. In addition, there is the problem of the many distractions from research that were mentioned this morning.

Some of these disadvantages can be overcome in non-university settings, for example, in non-profit research institutes where I have spent most of my professional life. But these suffer from a lack of financial independence and so are less attractive to many of the most able people. They live on contracts and grants and that is a hard life if one does not have the kind of support that the university can provide. They also suffer to some extent from not having as much student involvement. Although students do have a short-term focus, they also provide advantages.

We mentioned the role of government research staffs, which have been developed in the last few years. The federal government is beginning to develop, in a selective and uneven way, some policy research competency. Such staffs have the important advantages of access to decision-makers, but they suffer from short-time perspective. They also suffer from bureaucratic constraints on both the questions and the answers that are acceptable. These are important limitations. These staffs carry out one very important function, however. They act as an intermediary between the research community and decision makers. If they did nothing else that would justify their existence,

they would still be worth having around because this link between the scholar and the decision maker is so crucial. They do more than this, but this they do.

On funding, I think it is important to mention how rapidly funding has increased for policy-related studies. Increasingly, federal mission agencies have been doing in-house research and commissioning outside research in policy-related areas. For example, this is true of the Department of Housing and Urban Development, the Department of Transportation, and the Department of Health, Education and Welfare. In as short a period as six or seven years, some of these agencies have increased research several-fold. In HUD ten years ago research was essentially zero. Now it costs about twenty million dollars a year. Not all of this money is in policy studies; a lot of it is in technology. But they are currently financing a quite interesting social experiment on housing vouchers, that is, on moving away from the notion that the government should be a supplier of housing to the idea of the government providing financial support in the form of vouchers that individuals can take into the market to buy housing services. This idea is also being tried in education. Until recently no federal agency supported such research. It is now being supported by mission agencies, by the National Science Foundation, and by private foundations.

What are some of the other problems in doing policy research? We have discussed difficulties of doing inter-disciplinary studies within the university. There may be ways of changing incentives a little bit within universities if it is sufficiently important to do so. I will come back to this in a minute. There are problems of interaction with policy-makers. If one is doing policy studies, there is a lot to be said for really understanding the problem. This often means dealing directly with the decision-makers or at least their staff people. But this may be difficult and costly to do and it may not help much in getting a publishable journal article. There is also a need for a strong interaction between theory and applications.

As other speakers have mentioned, this research is hard to do well. It is not easy to create an environment in which people are encouraged, taught, and rewarded so that they can do the kind of work I have described—an environment, for example, in which a person who has an idea can look at the literature and do a little data gathering and formulate some theory and then do some more data gathering, and revise the theory, and then invent some objectives, and invent some alternatives, and then try them out on policy makers, and then

go back and revise the theory, and so on. This is a heuristic process. Unless there is adequate support, institutional support, it is going to be very difficult to proceed in this sequential way. It can be a long process, which sometimes takes years. But the payoffs can be large.

There is a class of studies that doesn't proceed in this way, but the results are not as likely to make such difference to policy. This is a different model, one in which the research plan is pretty well developed. First hypotheses are defined, then data gathered and checked out and then results tested. I don't object to this method. Some questions are pretty well defined. But I would like to emphasize that often the most important and interesting areas involve questions that aren't so well defined.

What about the problem of research support for unstructured and policy-related problems? There are big differences within the university in the degree to which the apparatus to support research has been developed. It is interesting and instructive to see how much social experimentation is now being designed and operated by non-academic research institutions. This may be entirely appropriate since the mission of the university may be incompatible with the conduct of such experiments. But I can't help thinking that it also reflects, in part, the underdeveloped condition of most of the academic social science departments.

Take what might seem to be a trivial problem but isn't. There is a need for obtaining cost estimates in much policy research. This is often thought to be neither a very interesting nor important subject. Yet awareness of the costs in getting certain functions performed may be crucial since much research is guided by costs rather than by other, more esoteric considerations. It has been very hard to get people to go out and find out how much things cost. And yet having people who could be taught to do this would often be very useful.

Another common need is having full-time, or near full-time people. It is often difficult to deal with many problems if all of the key people are part-time.

Also, I think there may be organizational implications in the fact that in the policy sciences field, generally, there is far too much attention devoted to analysis and far too little to design and invention. We could do much more to realize ourselves and to convey to others the notion that most of the important things come from inventions: invent-

ing objectives, inventing alternatives, coming up with new ways of doing things. This is really where the big pay-offs come from, not simply in getting a good description of the system, although this can be useful.

What are some things that might be done within the university? My guess is that the pay-off from having a small fund earmarked for exploratory research, particularly in inter-disciplinary collaborative studies, would be very high. There is a lot of money available for policy studies from foundations and the federal government. But it is a lot easier to get that money once some initial work has been done, particularly given the requirement that one have a hypothesis, a plan, and so forth. But there are internal barriers within the university to getting these studies done. These barriers can be overcome to a degree by having funds earmarked for collaborative, inter-disciplinary studies.

With respect to cross-departmental, cross-school centers or institutions, I really have to defer to members of this group. There are a large number of these in existence. Some are well established and permanent; others seem to be sort of shells for receiving money.

A more radical suggestion is that there be semi-autonomous research institutes associated with universities. In a way, I suppose that is what medical schools are. They seem very autonomous; they have their own salary scale; they have their own hiring procedures and operating methods. The notion of proliferating that type of institution in the university should make any university president faint. Are they an adequate model? Perhaps not.

What about the experience with more or less affiliated nonprofit institutes as, for example, Stanford Research Institute, which is related to Stanford University, or the non-profit organizations associated with MIT? My impression is that while these are useful organizations, there hasn't been much interaction with the university in most of these cases.

Finally, I am really struck by the modesty of the scale of research in the field of law and social sciences research. It is in many ways, I suppose, a lot more fun doing research in a field which is not overblown and in which researchers are working on subjects that haven't been looked at at all systematically. It has to be more interesting than it is going to be ten and twenty years from now when a lot of this has been worked over.

B. *Discussion*

PROFESSOR WALKER: Thank you very much. We have ample time now for questions and comments. There has been much to react to this morning. Perhaps it would be better to direct your question to the speaker that you would like to hear from.

DR. HINES: Let me make a fairly mundane and administrative point that would apply to the management of any funding agency. We all are in great sympathy with the notion that some funds be available for working research up to the point where it can provide a convincing appeal for larger funds. But there is a very difficult administrative problem here because there are not enough funds available in the United States. I don't mean in any one agency—I mean in the whole system. There are not enough funds to give everybody who might be able to do some productive thinking enough money to do this. Therefore, you have to decide who is to get the funds and who isn't.

Now in a local situation, say in a university situation, you do this, if you can, by gaining some kind of personal impression of who looks most promising. You try to hire these people and then you bet on them when you hand them funds. This is done more or less easily depending on how much pressure you are going to get from the other people who say "you made a purely personal judgment to give that guy time off, and you didn't give it to me, and what evidence do you have?" So, you may get into trouble: What evidence *do* you have for giving Smith time off and not giving Brown time off?

When you are operating outside of the local situation, you don't have quite the difficulties of dealing with your neighbor down the street, but you have other difficulties. You have some kind of more generalized responsibility to the community as a whole, and, of course, if you are a public agent or a public agency, to the people of the states. People in State "A" pay taxes just as the people in State "Z" do, and so you have to have some valid basis for giving out the money.

And so, I agree very strongly that money spent to support preliminary work would be highly productive to society, but I think that the problem of how actually to conduct such a program is a very difficult one. It is not difficult for just one funding agency: it is a difficult problem for all funding agencies including the universities and the private organizations.

DR. HUSZAGH: I would like to go back to a point that Professor Zeisel made last night. One of the additional dangers of providing

seed money is that if you take two people starting out, one good and one bad, only God knows who is which at the beginning. There is a point at which the money actually permits the bad to look almost as good as the good before the good pulls away. The initial assessment of data makes everybody look knowledgeable. In my own experience, I think that I have found the way to tell the good from the bad. I think that the way a researcher conceptualizes an issue is the strongest tip-off. Maybe the easiest way to help a person who has been mulling around in a field for six years is not to give that person money, if you are in a funding group or central resource, but rather to help perform the communication linkages with people who have rich experience in facets that relate to his work and can very quickly bring things into focus.

PROFESSOR LEVY: I would like to ask both Dr. Rowen and Judge Christian this question. It seems to be true that fund-raising activities are often easier for something labeled a "research institute," whether it is inside the university or independent of it. Yet I have the impression that the phenomenon poses a problem because of the proliferation of research organizations. It's like the problem of having a county government with a metropolitan government super-imposed over it. Judge Christian's organization was created though there already existed the National College of the State Judiciary in Reno, and there will soon be, Judge Christian predicts, another organization called the National Institute of Justice. Each will have its own staff and its own administrative budgetary needs, and each will be competing for funds with individual scholars. I also understand that Judge Christian is planning a series of regional institutes which will have their own staff. I wonder if the proliferation is wise, and, if it is unwise, should it be controlled, and if so, how?

JUSTICE CHRISTIAN: The proliferation exists. I think that it is going to continue, and I don't know how to control it without accepting constraints that would be defeatist. It would have been possible, giving a personal example, for the founders of the State Courts Center to say, "Well, we should not proliferate here and so let's not do anything." This would have left a field for future expansion or activity on the part of the National College, which is a training institution. It has nothing to do with the function that I have been describing.

PROFESSOR LEVY: Excuse me, but they claim to be doing research and that part of their mission is research.

JUSTICE CHRISTIAN: Well, in any case I don't think that there is an actual overlap of function. We have stayed out of continuing education in the court area for the explicit reason that an adequate job is being done by the College and by several others. I really think that the untidiness of having several entities active in a given area of public concern is not a disadvantage that is important. It is a mild nuisance, but it doesn't really hurt anybody.

PROFESSOR ZEISEL: I have a minor thought about the problem that you raise, Professor Levy, when you say that you added other counties to your research for comparison purposes. For what it is worth, my experience and my predilection with such virginal studies is that I would rather go into depth in one county and forget the other counties. If you look, for instance, at Remington's studies,²⁸ his yield does not become notably richer by adding information from Michigan, Minnesota, and the third state. I think that it would have been infinitely more interesting to get into greater depth in one of these states. And I would think that studies that concentrate on the one county and count on the next study to make comparisons have a higher yield.

PROFESSOR LEVY: We initially chose to deal with three urban counties. At the suggestion of the National Science Foundation we included two rural counties and then had a great problem of trying to find ones that were suitably representative. I suppose that I can say for myself that your point is a very telling one; it was consistent with my own judgment. But we wanted to get the extra funding, and the suggestion was made by the National Science Foundation, and we accepted the suggestion,

DR. HUSZAGH: I must say, the gauntlet has been thrown down.

PROFESSOR WALKER: Maybe we had better pick it up quickly.

DR. HUSZAGH: Let me add a short statement. I think that your approach makes an assumption about the variables that you are examining. You may find that a very important break occurs between the urban and rural, and I personally think that has been highly understudied. I would wager to say that the order of magnitude of benefits flowing from the comparison could be much larger than some kind of miniscule exhausting detail of a single case.

MS. FULTON: I believe a tentative conclusion, and one I can't

28. [Ed.] See note 2 *supra*.

support with numbers right now, is that in rural counties more motions by fathers for custody are granted. *Why* this is so is a question we may investigate later, but this is information we would not otherwise have had if our study had been limited to an urban population.

PROFESSOR CAVERS: I wanted to comment on something that was, I recall, mentioned by Professor Levy and I believe has been answered by Judge Christian, namely, the concern about the proliferation of agencies sponsoring or carrying out research in areas that we are concerned with. I think that one consequence of this proliferation is that a very considerable part of research activity will be commissioned research. The commission will always be coming from the agencies and the like. And while I don't think it is a bad thing to have a lot of commissioned research, it does seem to me that it would be unfortunate if in the planning and development of programs for study there wasn't room for the scholar who has got his own ideas of subjects that are worthy of study. In other words, it would seem to me that a desirable feature of agencies, however numerous, would be that they keep a door open for proposals emanating from universities so that researchers would be able to obtain their own funds as a result of their activities.

DR. HINES: In my profession I am necessarily an inveterate rewriter of things. I spend hours and hours rewriting things, even things that I wrote myself. But this morning I was interested in re-writing part of the Levy-Fulton presentation.

It seemed to me that a large part of their presentation could be rewritten to look at their problems from the other side. They were talking about the difficulties of making their study, how everything seemed to cost more than they expected, and how it took more time. "Now the analysis is more complicated" and so forth and so on. I certainly suspect that is an accurate description of what is going on. On the other hand, I was thinking about the other side of it, the symmetry as it were. While they were saying that "everything cost more," they were also really saying that "with funds we were able to do things which we wouldn't have been able to do at all without funds."

I don't know what the proper conclusion is, but I guess it is that if you had more money, you would have been able to do more things. I think that certainly another conclusion is that if you hadn't had any money at all, you wouldn't have been able to undertake most of the research that you undertook. This is not a personal remark, but a

general indication of the problem of trying to see two sides of what is essentially the same phenomenon.

MS. FULTON: That is definitely true; I found myself being a little reactionary when the word "charming" was used last night to describe studies that are done just on the basis of intellectual curiosity with no funding. They are perhaps "charming," but they are very different from studies that are devised with mechanisms for searching out additional areas of concern and exhaustively looking for participants, and being sure of representativeness, things of this nature. You are absolutely right that the money you have to deal with in large part does define the results. I suspect that most scholars would happily give up their claims to a "charming" study in exchange for financial assistance in their endeavors.

PROFESSOR WHEELER: I want to comment on two different things this morning. First just a brief note on the problem of comparative studies. I think that you would probably agree that it is difficult to generalize about what is right and what is not. I think that what is right would have a great deal to do with the area. I think that it is important to distinguish between comparative studies that are explicitly comparative and those that amount to multiple individual case studies. I think that the Remington example²⁹ is really closer to the latter than the former. You could have had better results from that particular study had it been more explicitly studied from the outside.

Secondly, with regard to Bob Levy's and Julie Fulton's remarks, it seems to me that some of them pertain to problems that are true any time you get involved in a research project and some are distinctive to law and social science. It would be interesting if we could try to make up a list of those problems that are really distinctive to the latter brand of interaction. The funding problems on long-term projects are really typical of any large-scale social research. But there are some other problems that are distinct to law and social science research. One of these has to do with the nature of proof, and it crops up very, very frequently when persons are trying to do social research on a problem that will have some impact on litigation or on legislation. Judges and legislators do ask different questions than your colleagues at other universities. In part, it is a question of who you want your audience to be. Your colleagues have different standards for deciding whether you are doing research that is fine, reliable, ac-

29. [Ed.] *Id.*

ceptable and so forth from those who are much more immediately concerned with policy. At any rate, that seems to me to be one distinct problem.

A second has to do with timing. The single thing that most impresses me about the problems of doing social science research within a law school framework is that there is a difference in the sense of urgency that students, faculty, and policy makers are likely to feel about a particular problem. This sense of timing is different from the sense of timing that, fortunately or unfortunately, those in social science tend to bring to a problem. With the former, the pressure of time seems much greater. With the latter the feeling seems to be that "you cannot do research on a time schedule."

And then a third distinct problem, I suppose, is the problem of doing research within the context of a law school itself. I am sympathetic with the problems that Bob [Levy] has mentioned in regard to his duties to students, to the law school, and to the rest. But it seems to me that there is a real difference, which is going to be felt much more strongly as increasing amounts of this kind of work are done, between the tempo of the law school and the tempo of research. Most behavioral science departments in large universities have persons who naturally assume that they are going to be supported in part on research grants. There is going to be a research center or institute. Those units sit very uncomfortably in most law school frameworks. You have got the image of the lawyer as a generalist, and the feeling essentially that everybody should be good at everything. A research institute seems to fight against that image, and for that reason it is a little harder to get an institute established in a law school. It may take a conscious change in the commitment of law faculty members who really want to make empirical research a central part of their life and their career. Something is going to have to give. You can't just merely add it on to everything else, for law professors are teaching at a much higher student-teacher ratio than the rest of the graduate faculties.¹

Well, anyway, I am sure that there are lots of others, but I would like to see us accumulate a list of the problems that are truly distinctive and a list of the problems that everybody faces in trying to do research on a large-scale basis.

PROFESSOR FRIEDMAN: I want to second that and also add the problem of getting people to help on your research. Very few law schools claim any substantial number of graduate students. The

few who have graduate students find that the graduate students are usually not interested in this area. Many of the researchers in the social sciences naturally assume that there will be Ph.D. candidates who will assist on projects, but generally you can't make that assumption in law schools. That has an impact on the scale of projects and the "do-ability" of projects.

Another point which is terribly important is that the culture of legal education is simply inimical or indifferent to empirical research. This creates real pressures, I think, for people without tenure, and it generates more general difficulties. In fact, it affects the kind of climate of support that you are likely to have when you are proceeding in research of this sort.

MS. FULTON: I would like to comment on that briefly. In sitting in on certain law courses, I found myself wanting to make comments about what I know about findings of social research and how I applied them to situations that were being discussed. Each time I attempted to do this, I was met with great hostility by the law students. They didn't want to know. They have little faith in social research findings. I thought, "well, maybe I can get to them from another direction. I will try to use law students in the research process." I found, however, that not only are law students generally hostile toward social science research, but also they find the research process too time-consuming and presumably not worthy of their attention. They say things like, "anybody can interview. I am going to have to interview as a lawyer." From my experience, however, I find that they are generally poor research interviewers. Of course, there are different objectives in the interviewing done by a lawyer and that done by a social scientist: most often the lawyer is seeking information which differentiates this case from others that are seemingly similar, and the social science researcher is usually seeking information to show underlying similarities among specific cases. But there are some principles of interviewing which should be adhered to by anyone who is interested in the quality of the responses elicited. We went through several practice interviews and later, as we analyzed the interview sentence by sentence, I tried to point out how the students—and even Bob [Levy], sometimes—unwittingly created certain responses and subtly directed and controlled the course of the interview. These are things that lawyers should be aware of but which are seldom discussed in law school training.

PROFESSOR LEVY: I would like to say one more short thing about what Stan [Wheeler] suggested. Some of the unique problems

of conducting social science research in a law school context are not only time consuming and difficult but also are very interesting research problems. To take one example—a custody investigation by social workers. I think this also bears on what Professor Zeisel says about using different counties.

In one county, we had the support of the head of the Department of Court Services and the judges of the Family Court, who immediately elicited the support of the whole county judiciary in our study and opened up their files to us. In another county, the Family Court judge was utterly opposed because he thought our study created problems of confidentiality. He got the district judges, who wouldn't let me appear before them, to refuse us access to custody investigations. Subsequently, a new Family Court judge took office for a year, and during that time we worked out a system where I could get some of the investigations by having him send me a letter saying that if both lawyers agreed and both parties signed a consent form, I could get the investigations from the lawyers. In a third county, the judge told me when I first went to him, "You will probably get resistance from the Welfare Department, but if they give you any trouble, tell them that I order them to give you the studies." This didn't appear necessary when I first approached the Welfare Department director but subsequently became very close to being necessary when it came time to turn the studies over.

Those approaches and those administrative difficulties cost a lot of time, but the different responses are data themselves because it is my impressionistic judgment that the judges in the uncooperative county are considerably less confident of the studies than are the judges in the other counties. At the same time, the lawyers are more anxious to cooperate, or at least as anxious as in the other counties, because they share my suspicion of the judge's attitudes.

How can we handle that data? How is it to be analyzed in the light of what we find from the studies themselves? I am not really certain. But the process of doing research when you have to satisfy an audience that is involved in research is a very interesting one.

PROFESSOR WALKER: It seems to me that we are now focusing on the question, how will law schools as institutions have to be changed if they are to provide a base for the kind of research we are talking about? And so for five minutes now, or for a future discussion period, perhaps we should reflect on what new institutions, or what

other changes, must be contemplated if this research which we are now doing is to continue and be fruitful.

PROFESSOR CÄVERS: Could I toss out a problem that seems to be emerging especially in the law schools? The problem is the relationship of action programs to empirical research.

The law schools, I think, are basically action-oriented and concerned with students. Now we see more and more foundations or governmental bodies being concerned with action values. Is it a feasible and fruitful development that research activity can be tied in with action programs, or should we view this as a retrogressive development which is likely to undermine research activities and produce another form of advocacy?

DR. PADAWER-SINGER: I am very happy to see that we are discussing the implications for education and training in law schools and social science departments. I would like to go further and advocate that we train not only pre-law students but all of our students in law as it is practiced in courts and in social science because we would wake up many students who might become very interested. I have found that our undergraduate students are very good research assistants. They are a neglected resource. They are sometimes more enthusiastic than graduate students who feel they are being imposed on and are not really paid well enough for their contributions (very often perhaps that is a true and legitimate feeling).

I would also like to add that I hope we will soon look at the possibilities of working out access to the courts. By engaging the cooperation of the courts and the funding agencies in a joint effort, we may avoid the present situation in which every researcher engaged in court research must make his or her own arrangements for access. For instance, I hope Judge Christian might make some of our proposals possible by showing us where we can have access to courts and which courts might be amenable to our research.

PROFESSOR ROWEN: I would comment on Dr. Cavers' question. There really is a continuum between work which is on basics and work which is on action. I doubt if there is any really clear point at which we can stop and say, "This isn't really suitable for academic purposes, go somewhere else." But there is one difference, not so much with respect to that continuum, but to a process. I don't see anything wrong with what might be called action in research if it is subject to the process of review and criticism. Where that isn't the case, where people go charging off to try to change the world without

that kind of review process, it seems to me that the work really is questionable. That's crucial, it seems to me.

IV. EVALUATION, DISSEMINATION AND APPLICATION OF LAW AND SOCIAL SCIENCES RESEARCH

A. *Presentations*

PROFESSOR WALKER: Our first speaker will be Professor Phillip Lochner of the State University of New York at Buffalo.

PROFESSOR LOCHNER: I want to talk for a few minutes about in what sense it is realistic to speak about the application of social science research, or law and social science research, to the legal process.

The first question is: Why should we concern ourselves with the application of social science research to the law? Don't we talk about being pure researchers, interested in knowledge for its own sake? We do, but at the same time we do carry around notions in our minds that the kinds of things we are researching have some real world effects. Another reason for our concern is certainly, that the funding agencies are going to be increasingly interested in research that can be applied in some practical way.

It is clear that more and more social science research has found its way into the legal process, into briefs, for example. Both attorneys and judges think this research is more relevant now than they may have in the past. In spite of these facts I believe there are some real limits on the application of social science research to the legal process.

In part, the limits arise out of the nature of social science research itself. I don't want to address that issue at this point. I would prefer to focus on the limits that arise out of the nature of the legal process. But just to indicate what I mean as far as the limits arising out of the nature of social science research, I refer you to a book such as Smigel's study, *The Wall Street Lawyer*.³⁰ It is an interesting book but by its very nature it is a book which lawyers and judges simply aren't going to have much professional use for in the specific kind of case-by-case decision-making and advising that they do. There are other kinds of social science research which do have more potential for practical application, for example, the work that Dr. Singer is doing and has done on free press and fair trial. Potentially, her study could

30. E. SMIGEL, *THE WALL STREET LAWYER* (1964).

end up in a brief or in an opinion, and it might alter a judge's or an attorney's way of looking at reality.

Why isn't even this sort of "relevant" law and social science research applied in the lawyer's world? I will divide the answer into areas which relate to what attorneys do, to what judges do, and then if I have time, I will talk about some of the limits that are inherent in the legislative process.

I will begin the discussion about the problems lawyers have using social science research by indicating a few of the kinds of cases in which social science research has been used.

One sort of case is the "our backs are to the wall" kind of case—for example, a Florida case in which a black man was accused of raping a white woman.³¹ The defense raised the issue of a change of venue. The prosecuting attorney brought four witnesses from the community to explain that no one in the community was biased and that there were no pre-judgments. Considering community social pressures, it was apparently difficult for the defense attorney to find his own witnesses on the issue. Instead, he used survey research to try to indicate the presence of bias and pre-judgment. And so that is one kind of situation in which social science might actually be used in law. Surely another—

PROFESSOR ZEISEL: The court turned him down.

PROFESSOR LOCHNER: Indeed it did.

Another type of situation is when a statute mandates it. Section seven of the Clayton Act suggests the relevance of social science research. There is a tradition of social science research in the unfair competition and trademark area. (I think, just incidentally, that a very interesting question to explore would be why that social science tradition grew up in a particular area.) Very often, a court must decide: Is the public likely to confuse two trademarks? The parties very often go out and survey people as to whether or not they would confuse two trademarks.

It seems to me that these kinds of situations are atypical, and the question remains: Why isn't social science used by lawyers for a variety of other legal questions? Some of the answers that I am going to suggest may sound perfectly obvious; but nonetheless, I think that they are important to keep in mind.

31. *Irvin v. State*, 66 So. 2d 288 (Fla. 1953), *cert. denied*, 346 U.S. 927 (1954).

One reason for nonuse of social science research is clearly the ignorance of most attorneys about the social sciences and what they have to offer. I don't mean to sound snobbish. After all, lawyers are not trained to be social scientists. It is unlikely that most would ever have had any kind of prolonged contact with the social sciences. If you take a look at the Bar, nationwide, (300,000 attorneys) how many of those admitted completed a four-year B.A. before beginning law school? How many were law clerks and never went to law school? Considering the nature and quality of legal education, what can one expect in terms of receptivity to new kinds of approaches?

Another problem is that there is simply no easy way for lawyers to get hold of social science research material. There is no West Key Number system; there is no book of "words and phrases." The Index to Legal Periodicals is not helpful. All of the usual tools that lawyers use are not available to lawyers if they are interested in what social science research can tell them.

Surely, in addition to ignorance, there is inertia. Why, after all, should an attorney spend the additional amount of effort and time to dig out social science research when it is likely that the opponent is not going to use this sort of research and the judge isn't likely to respect it? Why not simply continue doing law work in the way it has always been done? And there clearly are risks to the use of social science research. In early survey research cases there was trouble getting survey research admitted into evidence. Furthermore, a judge is more likely to have trouble understanding social science research. If the social science research doesn't turn out very well, it may have been a very expensive experiment. Why take the chance?

There seems to me to be a third limit on the use of social science research by lawyers: the kinds of attitudes and opinions that attorneys have. Even very well educated and knowledgeable attorneys have negative and hostile attitudes toward the social sciences. I am not concerned about the rational basis for that hostility and those negative attitudes. It is clear that those attitudes do form a powerful force inhibiting lawyers from using social sciences research.

Another limit is the nature of the work that lawyers do. After all, a great many attorneys are glorified paper shufflers; what kind of real application of intelligence does it take to produce the standard will, or to do a real estate closing, or something like that? For many attorneys, the kinds of situations in which social sciences might be applied simply do not turn up very frequently.

Attorneys work with all sorts of time constraints which make the use of the social sciences difficult. There is a need for a fast answer for a client. There are closing dates approaching. Even if one gets over those hurdles, there is the matter of cost. Not all social science research is extremely expensive. There may be relevant social science research in the literature that one can pick out and use. But if you look at the kinds of cases where original social science research has been done, the cases are of one basic kind; they are cases in which a large institution has the resources to sponsor that sort of research and has a lot at stake. Therefore, the institutions are willing to go the extra distance; they are willing to use the institution's resources to generate that last possible argument which might change the outcome.

All of these reasons, which limit the lawyer's use of social science research, may also limit its use in large bureaucratic law organizations such as an attorney general's office. In large law organizations there are bureaucratic pressures which incline attorneys toward specialization. Legal work may become routinized very quickly. When a new case or a new problem comes up, there is an inclination to try to fit it into an existing category. Unless there is a change in a statute, lawyers are likely to continue doing things in existing ways. Specialization and routinization are reinforced, in part, by the demands of legislatures. Legislatures use very unsophisticated tests of institutional effectiveness such as: How many successful prosecutions did you get? As a result of legislative prodding, bureaucratic legal organizations want to go the cheapest route to get the most possible successful prosecutions, or whatever. Social science research, by contrast, is costly and time consuming. Furthermore, there are pressures against innovation in any large institution and these pressures may also limit the use of social science research.

I am hesitant to talk about what judges and justices may or may not do and how they approach social science research. There is at least one justice present. But I will throw these thoughts out in the best social science form, as hypotheses. Then when they are refuted, I can say: "Well, that was just heuristic."

Judges as a whole have the same kinds of limitations in their education and the same kinds of preferences for traditional ways of doing things as have lawyers. Probably more important is the way in which judges view their role. Certainly at the trial level, my guess is that judges have very traditional views of their roles. They see themselves

as umpires ready to step in only when there has been some breach of a rule of evidence, or etiquette, or procedure. If they are making law at all, then they are making it only in the tiniest of interstices. If all that is true—if that is the way judges see their role—there isn't much room for social sciences.

Perhaps, also, some limits on the application of social science research arise out of the obligations which judges may see themselves as having. One obligation is to the two parties. Another is an obligation to some kind of wider audience of potential plaintiffs and defendants, and to other judges and attorneys, to set forth some kind of understandable and workable rules. The more the judges concentrate on their obligation to the parties, the less social science will have to offer them. The nature of social science research is to address the wider audience.

Another problem is that judges may see themselves as being involved in a decision-making process where "the facts" have limited relevance. For example, if the question is, "What is due process in this particular area?", the judge may see his role as asking the question: "How ought people behave, how ought government behave." If that is the relevant question then social science research has little to contribute. If a judge is faced with interpreting legislation, he may very well feel obligated to follow a legislative command even if social science research exists which shows that the legislature acted very foolishly indeed.

The nature of the cases that judges decide also constitutes a limit on the application of social science research to law. Some cases are very trivial, and the input of a costly piece of social science research would simply be not worth the cost. Even in non-trivial cases, there may be instances in which it is more important that a decision be made one way or the other than that the decision be "right." For example, whether or not the testator signs his will before or after the witnesses probably makes very little difference. It is more important that attorneys have a clear and workable rule so that they produce wills that will survive probate, than that we know with great precision whether "immediately before" or "immediately after" is better.

There are also cases that may be *too* complex for social science research; for example, the antitrust case. The judge's main problem with that kind of case may be to simplify his decisional task. He may do that by applying a particularly heavy burden of proof or he may do it by deciding on some kind of arbitrary, clear-cut rule. In those kinds

of situations, sophisticated and subtle social science research may be inimical to getting the problem decided.

If one thinks about the legislatures and the bureaucracies, the other areas of the legal process that social science research may be concerned with, many other kinds of constraints arise. Legislatures are, after all, places in which there is a need to generate decisions very quickly; there is a high turn-over in the legislative bodies. I think that we are all aware of the kinds of distortions which can take place within bureaucratic, administrative organizations. The basic point I want to make is that there are very distinct limits to the usefulness of social science research, and we might want to reconsider some course of research, specific kinds of situations, specific kinds of areas, in which the kind of research we do would have the greatest effect.

PROFESSOR WALKER: Our second speaker this afternoon is Dr. Fredrick Huszagh of the National Science Foundation.

DR. HUSZAGH: Last night Dr. Singer observed that a person's research is affected substantially by his or her background. Fortunately, like a bachelor marriage counselor, my perceptions regarding research are not unduly clouded by experience. From this vantage point I make the following brief comments on research evaluation:

The evaluation process can be viewed as composed of at least two distinct phases. First, various parties engage in diverse forms of evaluation prior to commencement of a research project. After the completion of research there is a distinctly different type of evaluation. My comments shall focus primarily on the former phase. As regards the latter type, however, Professor Lochner has implied practical application is the major evaluative criterion. I concur in his judgment as long as it leaves room for other criteria. For example, much excellent law and social science research may be used only by law scholars for the *sole* purpose of generating deeper insights about legal institutions. Only the second effort may eventually prove worthy of practical application, but the former is obviously important and independently worthy of support.

To explore diverse elements involved in pre-research evaluation, I have decided to focus on the significant actors—persons and institutions—in the research process. First, there are the principal investigators (PIs) who propose to execute a project and be responsible for it. There is next the institution or institutions with which the PIs are affiliated. Normally this would be their department at a university or an on or off campus research institute of the "profit" or "not for profit" va-

riety. However, it should never be overlooked that apart from academics there are people within government, law firms or other non-university settings who have unique potential as researcher for the type of work we are discussing here. Aside from meeting requisite standards for intelligence, they are endowed with real life experience that generates perspectives about essential forces within the legal system not available to academics.

The third actor can best be described as the research subject. In the past this actor has frequently been overlooked in the pre-project evaluation process, and even now he seems relevant primarily in matters of subject privacy, informed consent, and so on. As I shall note later, his or her role may be quite a bit more substantive than this. The fourth unit of my constellation of actors is the research consumer, who has been discussed by Professor Lochner, although as I observed earlier, he did not stress as a representative of that group the scholars who view the research as merely a building block for subsequent research that will produce "ultimate statements" for practical consumption or application. Last but not least there is the funding agency for projects.

Having identified these five actor classes, I will now highlight particular characteristics of some of them, which can substantially affect the pre-research evaluation process. Principal investigators, contrary to "streetcorner" belief, are not a monolithic class of scholars. They can easily be stratified as to research maturity, position within their host peer group, and attitudes toward their host institution. Variance on these characteristics among investigators leads to different types of projects on an identical topic. For example, the young investigator, desirous of generally exploring several areas before specializing, may view the project mainly as an opportunity to poke around in a new area. He may not wish to incur the full entry costs of developing a detailed comprehension of the environment embracing the project problem, which is normally needed to efficiently bring a scientific understanding to the subject. In this situation the proposal is unlikely to reflect the investigator's critical evaluation of a task to be undertaken. It more likely will be a statement calculated primarily to elicit support. With this problem in mind funding institutions frequently insist upon elaborate statements of the research project, which embody an assessment of relevant literature, method, management problems, and so on. This makes explicit the investigator's level of understanding of the problem to

be explored. The detailed plan also limits the investigator's freedom to detour his efforts in order to explore interesting, but tangential, matters during the research process. This propensity to chase "butterflies" may have a higher incidence of occurrence among those not frequently involved in funded research or among those who change research topics often rather than explore a single area in great depth over many years.

An individual researcher may also evaluate his project primarily as a vehicle for projecting his skills to his peer group within the host entity or to an even broader community. If this is the dominant motive the project will be optimized to achieve this end, perhaps by use of exotic techniques even though commonplace approach may be more suitable for examining a stated problem.

Like the investigator, the host institution may also evaluate a proposed project from several different perspectives, each one fostering a different type of interaction between the institution and researcher. For example, the institution may wish to foster a particular project in order to increase the level of internal expertise in a subject area. If an investigator can identify this institutional interest and conceptualize a project congruent with it, he may well obtain inordinate allocations of internal resources for his effort. Conversely a bad matching of topic to institutional interest may preclude pre-funding assistance from the institution at critical points where such assistance in even small quantities may spell the difference between the project's life or death.

If the host institution happens to be a law school, peculiar evaluation criteria may be applicable. Since law teaching and evaluation of students require intensive labor, a proposal involving considerable dedication of a professor's time may be viewed by the dean as an inconvenient drain on the school's resources, especially if the individual is responsible for a specialty area where short-term replacements are not easy to find. In such situations the project's intellectual merit is quite irrelevant to the threshold question of how the proposed manpower allocations for the project will interrupt the institution's commitments regarding teaching.

If the research project has potential for disturbing traditional notions of the legal system, its execution within an institution characterized by traditional legal philosophies may be viewed as threatening to the value preferences of the majority and thus potentially disruptive of faculty unity. In this situation one might imagine a negative overall evaluation despite the project's intrinsic merits as a social science

endeavor. This particular point deserves greater elaboration since I believe social science research concerning the legal system does indeed pose a considerable threat to traditional legal notions, but that is a matter worthy of a meeting by itself.

The funding actor in the pre-research evaluation process is frequently most concerned with encouraging high payout projects that properly match problems to methods, personnel, and material resources. An optimal project is usually one in which the following questions can be answered positively: Is the task feasible within the present state of knowledge? Are the timing schedules appropriate? Does the investigator articulate the task in a way that reflects real expertise of method and subject rather than mere mastery of relevant jargon? Does the allocation of funds to personnel, travel, computer time, and so on reflect an understanding of how material and human resources are best mixed? Put another way in a more narrow context, does the investigator allocate particular classes of manpower in a way which will most efficiently accomplish the proposed task? I would become suspicious if a project clearly requires ten units of creativity and ninety units of mundane legwork, but the investigator seeks funding for full release time for himself and little or no funding for graduate assistants.

Unfortunately many proposals are not detailed enough to permit this type of internal validation. In such instances especially, one is tempted to rely heavily on the investigator's existing reputation, that is fund only proven winners. There are obvious merits to this approach in terms of substance and bureaucratic safety, but it deprives potential Einsteins of the resources needed to make rather important findings. Consequently I think the funding actor must occasionally be prepared to bankroll a project on its internal merits despite the investigator's unproven qualities. Conversely the unproven investigator must make sure his proposal provides the quantity and quality of information necessary to enable the evaluator to recommend funding in the absence of a PI track record. When such information is not furnished, then clearly evaluation must rest on both job description and the experience of those intending to fill it.

One final point about funding agency evaluation, especially as regards the Law and Social Sciences Program. We are impressed when an investigator employs established theory developed in one of the major social science disciplines as the major focusing tool for his research on the legal system. These theories are frequently developed

to explain human or organizational behavior and thus have potential application to subclasses of such behavior associated with the legal system. Much research is now conceptualized without reliance on such theory, and this frequently results in mere "case study" empiricism. It is difficult for other researchers to build off efforts that are not couched in a broader theory structure. The use of hunches rather than existing theory with developed infrastructure also makes efficiently allocating research resources difficult.

I close my remarks with some observations concerning a special class of "subject" actors who also are prime users of research. The family courts being studied by Professor Levy are a good example since they are a major source of his research data and hopefully will benefit from his findings concerning the efficacy of particular custody procedures. When a particular court, administrative agency or other entity is the object of research, its cooperation with the investigator may depend largely upon its perceptions of the project's impact on its future operations. Will the results be threatening or useful? If investigators take this into consideration, they can frequently develop a research design that provides positive benefits to the entities under study without jeopardizing availability of data needed to verify the project's hypotheses.

In addition to being sensitive to the subject's perception of end results, the investigator must also be cognizant of whether the project format is congruent with the subject's rules and procedures. Unfortunately many projects both threaten the stability of the subject's future and the efficacy of its procedures. This conflict is not always avoidable. However, some focus on the subject can minimize unnecessary friction and thus make the project's execution more efficient.

I will close by noting the possibility that a project's data collection phase may actually be a gratifying experience for the research subject. It provides the subject with an opportunity to explain its activities in a "show and tell" vein and focuses attention on these activities. Should the attention be viewed as part of an attempt to threaten the subject's existence, the research process will obviously not be gratifying. If it is viewed as an attempt to understand the subject, however, fundamental notions of psychology suggest that the subject may be favorably disposed toward such activities.

The above comments fall far short of a verbal taxonomy of the evaluation process. I have attempted only to expose, in somewhat

Rohrschact form, some matters which I regard as both important and frequently overlooked.

PROFESSOR WALKER: The third speaker is Professor Earl Johnson of the University of Southern California.

PROFESSOR JOHNSON: Last night the members of this panel met and divided the topics as a means of conserving time. I drew the topic of dissemination of research findings. This appears to be a part of the overall problem of securing a proper application of social science research.

At the outset I must confess that I have not yet reached the stage in my own particular NSF research project where I have faced the problems of evaluation, dissemination, application and the like. Thus, such insights as I may have concerning this topic are derived from experiences unrelated to my current NSF project; for example, a time when we on the Office of Economic Opportunity legal services staff were attempting to design a dissemination system for poverty law research. Beyond that, I am speaking as a consumer of social science research; that is, someone who has attempted to make use of such research in the context of test case litigation.

There is great virtue in separating the advocacy function from the research function. At the same time, I think that all of us who are conducting research in the area of law and social science bear a special responsibility for dissemination of our work product. That responsibility is to make our findings more accessible to those who are involved in litigation, legislation, and other decision-making events in the law. This may require that we become advocates of the value of social science research in the abstract even though we don't necessarily advocate a particular position on a particular issue.

How well are we fulfilling that responsibility? Not very well, I'm afraid. If we are trying to sell a product, that is, social science research, to a market of consumers—legal decision-makers—we are doing a poor job of it. In fact, it would be hard to do a worse job. We are dealing with judges, lawyers, and legislators for the most part. When they wish to find out about the prior doctrinal law—the case precedents, statutes, regulations—there are a tremendous number of research aids available. A comprehensive system of reporters, digests, headnotes, looseleaf publications, and so on guide the judge or lawyer to the relevant legal rules and precedents.

As "advocates" of law and social science research, we presum-

ably would like these same decision-makers to take account of the findings and insights our research has unearthed. In essence, social science research is in competition with doctrinal research for the attention of legal decision-makers. Yet we are packaging our product in a way that virtually gives away the market to our competitors.

I will illustrate the seriousness of this problem by relating my own experience as a consumer, or rather an attempted consumer, of our social science research product. I have been involved in some litigation where social science research is of significant potential value. The issue is the right to counsel in civil litigation. Before undertaking representation of any client, several months were spent researching the issue. A half dozen law review students were turned loose in the library. They were asked to locate relevant social science materials as well as doctrinal materials. In particular, we requested that they attempt to determine whether there were any empirical findings that would indicate whether the presence of counsel had any effect on the outcome of civil litigation.

After a few months of research, the students had produced hundreds of pages of memoranda on the right to counsel issue. They had found scores of court precedents and articulated a number of promising doctrinal theories. Yet despite the mandate to locate relevant social science findings and considerable effort in that direction, the students struck out.

I was about to give up the search when I ran into someone at a conference very similar to this one. He told me about a research project in which he had been involved; a very minor segment of that study had turned up evidence that litigants represented by counsel fared much better—by a ratio of about six-to-one—than those who represented themselves. Shortly thereafter, I was reading the current issue of *Transaction* magazine and came across a reference to a two-year-old article in some obscure social science journal, which contained a well-buried finding that was directly relevant to the right to counsel issue.

The point of this case history is not the traditional, "persistence pays off." On the contrary, a very systematic examination of available sources, requiring scores of hours, simply failed. Instead, the discovery of these very relevant social science findings depended upon some very unreliable and informal contacts. Pure luck, in effect, as opposed to systematic research, is what paid off. This strongly sug-

gests that our methods of disseminating law and social science research are woefully unsuited to the needs of policy makers and policy influencers. (By the term policy influencer I mean lawyers, lobbyists and other persons who devote considerable time and energy in an attempt to modify public policy.) Beyond that, our methods probably do not meet the needs of other researchers very well either.

There are reasons for this. Quite frankly, the real motivations for dissemination at the present time are not ones calculated to produce dissemination in forms and to audiences that are most useful in a policy-making sense. Most of our attempts to disseminate probably are motivated by human vanity (which translates either to the respect or envy of one's colleagues) or if we are really lucky, by profit. Not only are the researchers encouraged to respond to such motives, but the organs of dissemination themselves have similar motivations. (Law schools, for instance, publish law reviews primarily for the prestige of the law school.) As a result, most of the information is published in forms that are not geared to the policy-makers and policy influencers, and is buried in obscure places.

There are several basic approaches to the remedy of this situation. The most obvious is to alter people's motivations. Suddenly we could all become very selfless and do things differently; but human history suggests this is not a very promising approach. Another way is to harness these very motivations to restructure the dissemination system. A new journal might be established in which all law and social science research appears. We could make publication in this journal the chief indication of success in this field of research. To state this alternative is to suggest the very real, and probably fatal, practical problems.

No, I suspect that we are fairly well stuck with the existing dissemination system, inefficient as it may be. Our best hope is to superimpose a separate system specially geared to making law and social science research more readily accessible to policy makers and policy influencers.

First, I would suggest the creation of a form of clearinghouse. Such an operation could actively pursue the collection of published materials, unpublished materials, reports, and so on. Its personnel could catalogue not only the major themes of these studies, but the secondary findings and data. Secondly, I suggest that we encourage the publication of a periodical containing abstracts of unpublished materials and of published works that might be relevant to legal policy-making. Thirdly, and this admittedly represents a more expensive

step, we could initiate a digest system which parallels or is integrated with the existing digests of doctrinal materials. This probably would entail the development of a sophisticated and comprehensive topical index. Each piece of research would be analyzed and the various conclusions and data would be summarized in a series of headnotes. These headnotes would be available for incorporation either in the existing digests or in a special law and social science digest.

What I have suggested above is an expensive and long-run solution to the dissemination problem. In the meantime, individual researchers can ease the problem by paying more attention to the full dissemination of their research to relevant policy-makers and policy influencers. They can take steps to insure that the published findings are placed in the hands of legislators, judges, interested lawyers, and so on. More than that, they can also distribute unpublished conclusions and data to those in a position to affect public policy. There now exists a host of advocacy organizations, public interest law firms, poverty law offices and the like, who are potential users of law and social science research. There are legislative committees and subcommittees on almost every imaginable topic. Researchers have a duty to search out policy makers and policy influencers who may have use for their particular conclusions and data, rather than to wait for that information to be "discovered" by some diligent legislative analyst or lawyer. By so searching the researcher serves not only his own intellectual interest but contributes to the public interest as well.

But through all of this, I think that the researcher must maintain some kind of balance. He must avoid becoming the chief spokesman for his particular research, or he loses credibility. This makes it necessary for him to establish some kind of liaison with advocates. It makes no difference whether the advocate is a public interest law firm, or a legislator, or somebody else as long as the researcher himself is not the person who "carries the ball."

PROFESSOR WALKER: The last speaker is Professor John Morris of Arizona State University.

PROFESSOR MORRIS: Thank you. I appreciate this opportunity to describe the design of my research project and how I propose to utilize the information that is collected.

My observations have been shaped by the topic suggested by Professor Walker, the "Evaluation, Dissemination, and Application of Law and Social Sciences Research." I believe that my research project, which

involves law and professional sports, is ideally suited to be analyzed within this framework.

First, I must admit that I agreed with Dr. Singer's assertion last night that one's background influences the way one approaches a problem. My background is action-oriented and my approach reflects that same philosophy. I am interested in seeing that whatever information, data and conclusions my project develops are influential in shaping the attitude of persons involved in controlling the sports industry and I want to project these ideas into that industry immediately.

In order to have an impact, to achieve acceptance of your ideas, the application, evaluation, and dissemination of information must be an ongoing proposition. The design of a proposal must include all three phases from the beginning. In my project this means that as many constituencies of the sports industry as possible must be involved in all phases of the research activity. The constituencies as they interact are the ones that make the ultimate decisions, and they must all be involved in order that each accommodate the other's views and accept the results.

I developed an interest in the regulatory aspects of professional sports because of the dichotomy created by judicial decisions, which are reflective of society's peculiar view of professional athletes and athletics. Somehow society views the professional athlete as being different from other athletes. Over time, it has come to accept the notion that unlike the amateur, the professional athlete perhaps does not eat Wheaties and may not be the all-American boy, and that his sole motivation in life may not be God, country, and motherhood. The sports institutions want to regulate his activities on and off the field. Those institutions are now the sole arbiter of a professional athlete's life, and society accepts that relationship. The standard applied, without notions of due process, is sometimes couched in terms that the athlete may not engage in "conduct which is detrimental to the sport."

The courts have gone along with society's willingness to treat the professional athlete as something unique and have permitted restraints on his freedom not allowable in the "business" world. It may be true that if the courts' views of the sports industry were shaped in accordance with pure antitrust concepts, such a view would be too narrow. But, since contractual restrictions such as the reserve clause and option arrangements would not be tolerated in any other sector of the economy, such restraints should not be permitted unless they are absolutely

necessary to enable each of the sports to function as a viable entity. The social harm and benefit to the athlete and society should be weighed before these restrictive arrangements are legally sanctioned. This needs study.

In addition, the sequential oligopoly created by a judicial tolerance of professional sports cartels, coupled with the exclusive hold of the three major networks on television rights to the sports contests, plus the selling of large blocks of television time to a few major advertisers who are the only ones who can afford to purchase such time, must affect the behavioral characteristics of the athlete. This too needs study.

The financial and social impact of professional sports on states and municipalities is substantial. Basic to a professional sports team franchise being awarded to a geographical area is the necessity to have a stadium or arena in which to perform. Historically, these structures have been financed by states or municipal tax dollars. The municipalities advance large sums ranging up to \$150,000,000 or more to attract such franchises and then exercise no control over the franchise. What goes into the political process that warrants a decision to make such an investment? If, as has happened, the professional franchise subsequently moves to another locale, the municipality is left with a large debt to amortize and no income with which to retire such debt.

This brief description of problems is not intended to cover all phases of the substantive inquiry that I have undertaken but is offered to try to convey to you the extent professional sports activities have a pronounced impact on our social life. Such impact is not limited to any specific community—and such activity at the present time is practically without regulation at any level.

In approaching the problem of developing a regulatory approach to the professional sports industry, one's first inclination is to treat the industry as a whole and develop one formula to fit the entire structure. Baseball, however, may be different from football, basketball, and hockey, off as well as on the playing field. Each may have different driving forces acting upon it to force each segment of the industry to contract or expand. It is necessary to know the common factors, if any, shared by the various sports. Such common factors may be pressure points where overall regulation is feasible. It is important also to determine points of divergence or cleavage in order to be able to exert influence on a single industry without affecting all segments in the same manner. Throughout, one must have a thorough knowledge

of how the regulatory pattern affects the various constituencies that deal with each sports industry.

A central issue which developed early in the design of my project turned on how the material that was developed would be evaluated, applied, and disseminated. Thus, I early discerned that major constituencies in the sports industry would have to be involved in each phase of the research process. They would have to participate in developing data about the sports industry since such information is not found in the reams of material that is published about sports. Each segment of the industry has its own in-house rules, its own sources of information for determining whether to create new franchises or to transfer existing franchises and for determining how cities are selected for such franchises. The owners of the various franchises have developed ideas about the type of person that they would admit into the fraternity as owners of new franchises or as people who purchase existing clubs as soon as the clubs' tax incentives have been used up. Each segment of the sports industry has developed certain rules and regulations concerning its players, which may be based on common assumptions but diverge in terms of implementation. These factors are not public knowledge but certainly are important in the decision-making process.

I am still primarily in the data collection stage although invariably the evaluation process comes into play. The process that has been developed in the collection phase is not innovative. An advisory committee was formed representing various constituencies in each sport selected for analysis. It is composed of owners, players (past and present), coaches, television executives, advertisers, lawyers, judges, legislators in the Senate and House (where committees in the past have dealt with sports problems), and athletic directors in several major universities. This group is a part of my data collection system. It has been helpful in survey type solicitation as well as in furnishing information through reactive measurement.

At the conclusion of the collection phase, I hope to involve the same group in the evaluation process. Several methods have occurred to me ranging from conference type discussions to further refinement of reactive responses into hypotheses. These evaluation techniques using the advisory committee will be worked out in conjunction with the committee.

The dissemination process seems to fall within the same planned structure. I would like to see the results of the research affect the

social structure that is being analyzed. The conclusions together with the supporting data should be brought to bear on the decision-makers to sensitize them to the idea that the conceptual conclusions are concrete facts—that they are facts as real as the biases that now motivate the present decision-making process.

The accepted vehicles for the dissemination of ideas that law professors have traditionally used—basically legal publications—reach only a narrow segment of the society. For the most part, the ideas so presented rarely make their way into the area of public debate. I want to do something different. Such forums as legislative hearings, educational television—with programs structured so as to present the data—articles written for public consumption, lecture series in selected communities, all offer a wider area for the dissemination of the information developed in our activities, and I hope to take advantage of these forums.

B. *Discussion*

PROFESSOR WALKER: We will open the floor now for comments and questions posed to individual speakers and the panel.

PROFESSOR ZEISEL: Let me start from an observation which I hope doesn't offend anybody. I have made the discovery that lawyers, and that includes judges, don't read books or studies unless the works refer to a case that they have to decide or fight. I just suggest this as a true observation, statistically speaking, rather than with reproach because knowing how hard good lawyers and good judges work, there probably is not time for the luxury of reading merely for pleasure. But given the problem that lawyers don't read, you are going to wonder, "Well, how is this effort of ours to add a new dimension to the law going to get into the blood stream of the law?" And as I look back at these last twenty years spent at the University of Chicago, where I think that it is not immodest to say we try very hard to advance the cause of empirical studies, I ask myself: What did happen?

On two levels something happened: One is among my colleagues. I would think, for ten years, half of my colleagues had an almost negative interest in what I was doing. In the beginning, they were even concerned that "we don't harm the reputation of a law school." It was that serious. But by now, to jump twenty years, I would think that even the worst ones (and by "worst" I mean no reflection: worst with respect to what the person thinks of empirical

research) would say: "You know, that would be an interesting question for you to find out."

What a person who says this really means is he sees that there is an empirical question, and he would never dream of finding an answer to it himself. But then some surprises happen. One of the, how should I say, "classic" teachers (that means law teachers in the classic frame), like my colleague, [Kenneth C.] Davis, suddenly gets the idea that we know nothing about how administrators interpret the great discretionary power of administrative laws. And he suddenly wants to find something out about how prosecutors actually exert their discretion, or how the immigration examiner actually does this. And in this way, in his own way, he gets into empirical research and spends now three-fourths of his time quite seriously on this. There is an interesting linguistic phenomenon: Professor Davis would never admit that he does anything but traditional law study; he calls it prose, and so this is what it is to him. But in fact, he does empirical research.

This gets me to the second perspective, namely, that I think the climate of the law has changed a little bit. If you take the courts for example, today there is much more use of empirical evidence. The *American Jury* alone is mentioned in some eight United States Supreme Court cases. In this connection, one must recall the Supreme Court of California case.³² It is the case of a robber, a bearded black man, who was seen stepping into a car—a Buick with a yellow roof—with a Caucasian girl with a pony tail. Those facts led to some statistical observations during the trial. The man was convicted. The California Supreme Court reversed, and lo and behold, there is a three-page statistical appendix to the opinion, done as learnedly as only a statistics professor can do it, because, in fact, one did! But the real point is that the California Supreme Court thought it worthwhile to go into such matters, which I think is important.

As Mr. Lochner has said, there are really areas of litigation now which are dominated by empirical evidence, such as trademark litigation and antitrust litigation. This is striking if you think back to how difficult it was at one time to introduce surveys as legal evidence running against the hearsay rule. Now you have sometimes explicit statements by the courts that it would be improper to try certain cases *without* survey evidence.

32. [Ed.] *People v. Collins*, 68 Cal. 2d 319, 438 P.2d 33, 66 Cal. Rptr. 497 (1968).

So things have really changed. But the real change, of course, must come from the law schools. We have in our law school about 370 or 380 students, and in my course on empirical methods of research, I have usually 30 to 40 students, 120 over three years. I thought that this was very little until I was invited to your alma mater, David [Cavers],³³ where there was a great seminar on empirical research. Twelve students came and only one of them was from the law school; so, we have a long way to go.

That really brings me to the point that I didn't make last night, which I think is very important—that law-related empirical research should be physically located in law schools because that would help toward the end that we all have in mind; that the legal profession become more receptive. I find that even in such a neighborly university as ours, and I don't say this facetiously, the very fact that one goes by people who do empirical studies, be it in the field of criminal law, or civil law, or procedural law, somehow changes attitudes.

MS. FULTON: I have two issues to raise. One is a query and one is a comment. In a 1969 article in *Psychology Today*,³⁴ June Tapp talked about the differences in basic orientation between social science and law. She made the point, however, that problems of communication within each discipline may be as great—if not greater—than those between the disciplines. There are several levels of law and several levels of social science and, within a discipline, a theoretically oriented person may have as much difficulty talking to a "field" practitioner as would anyone else not trained in that discipline. At the same time there seems to be a growing ease of communication between persons in separate disciplines but at similar levels of expertise.

I am wondering if that isn't what you were talking about, Mr. Lochner—the reluctance of the practicing lawyer to make use of some of the findings of research because the practicing lawyer is on a different level from the researcher. But the legal researcher could perhaps make use of the findings of the social science researcher and transmit them to his own discipline (if, indeed, he could talk to a practicing lawyer!). According to Professor Zeisel's remarks, things are in a state of change. I would be curious as to your feelings about this, that is, as to whether these inter-discipline and intra-discipline differences are becoming more or less, marked.

My other comment I would like to direct to Dr. Huszagh. You

33. [Ed.] Professor Cavers graduated from Harvard Law School.

34. [Ed.] Tapp, *Psychology and the Law: The Dilemma*, *PSYCHOLOGY TODAY* 16-22 (1969).

made a statement something like this: "Subjects may find satisfaction in the research project itself." I think that this is true and not true at the same time. I believe there is almost a state of oversaturation with research in our population with the result that people are becoming less willing to participate in any kind of research. I, a researcher, am the first to hang up on those dinnertime telephone surveys that seem to come every second day. I will not participate, and yet I expect others to help me in my research. One thing that might make a difference in attitudes is the technique of data gathering used. I feel, for instance, that questionnaires and interviews may have gone beyond the tolerance level of the average respondent. Justice Christian talked about the use of video tape in research and the uniqueness of this methodology may encourage people to participate—as a sort of game. However, care must be exercised in matching the data gathering technique to the kind of data desired: for some purposes the interview and questionnaire are the only appropriate tools. For this reason, these techniques should not be overused in situations when some other means would do just as well. Otherwise respondents will have no patience left for important research inquiries.

There has been quite a lot of research to determine what kind of person *will* participate in a research project. Martin Orne, for instance, found that certain psychological variables distinguished research volunteers from a normal cross-section of the population.³⁵ Additional research of this sort is needed to facilitate the interpretation of research findings and to assess their generalizability.

PROFESSOR LOCHNER: To address myself to your first comment, Ms. Fulton, it seems to me that clearly you are right; problems of communications exist. In part, they are language problems. Lawyers have their own jargon, and so do social scientists. Various branches within a given discipline get off into their own kinds of language. That is truly one problem. But another one, equally important, is for lawyers to feel they have some incentive to learn social sciences. The question, I guess, that the lawyers ask themselves is: Why bother? What is the benefit to me, to my client? That is an equally important problem.

MS. FULTON: I think that same kind of objection is raised by the theoretical social scientists too. They aren't going to learn a jargon outside of their own discipline.

35. Orne, *On the Social Psychology of the Psychological Experiment: With Particular Reference to Demand Characteristics and Other Implications*. 17 AMERICAN PSYCHOLOGIST 776 (1962).

PROFESSOR MORRIS: I want to comment on something that Professor Lochner said about the use of empirical data in certain types of legal areas such as anti-trust. There are certain kinds of legal disciplines which adapt themselves to the empirical research method, and anti-trust happens to be one because it works with a very fluid statute with no particular historical precedent. The statute was wide open for people to come in and try to develop basic theories to enforce it. At that point they looked to the economists to help fill in the basic underlying theories that might go into the statute. When the court refers to the Sherman Act, it is somewhat like referring to constitutional law. The Act has the same sort of broad scope that in certain aspects is amenable to social science techniques and information. But, as you pointed out, some legal problems are just not adapted to social science research even though techniques could perhaps be developed.

DR. RADLOFF: I am glad that Dr. Zeisel seemed to recommend the long haul. After all, the law is about the third or fourth oldest profession. And one hundred years ago there wasn't even a social science. Fifty years ago, there was very little empirical research.

Since World War II, we have had tremendous advances in the amount of social research but not much was done in the policy area concerning the law particularly. But I think if you look at the conditions under which social research in the law does get into use, we might learn how we can make some preparations for the future, when I think that more extensive use will be made of social research in the law.

Society decides in the long run on the choice of problems, applications, everything. The social scientist can be just as enthusiastic as he wants, but he is not going to get research done unless schools of social science and social science research are funded and subjects, judges, and lawyers cooperate. I think here we are trying to "push" things, but we have to look at the whole aspect of society including the "pull aspect." The times when the "pull aspect" is the strongest is when society has an identified goal that it is pursuing—World War II being the most recent example of that. I think that during World War II, more advance was made in my field, in psychology, and social psychology specifically, than at almost any other time—a real burst of activity in basic as well as applied research.

Historically the military has been a sector of society that has used social science research probably more effectively and has demanded

more of it than almost any other sector. Why? Because they have a unified goal; because they have problems that they know they have to solve, in selection and training particularly. They have to make decisions rapidly. They have to assign men, masses of men, and they need statistical models. They need research. They have to have techniques to do this. During World War I, the Army-Alpha was developed, which is still the standard device of selection and classification. The GATB [General Aptitude Test Battery] and the other tests that developed out of that model are still the basic ones used in the military today.

So there are possible societal conditions which might demand more social science research. William James described the phenomenon as "society requiring the moral equivalent of war to mobilize resources." In connection with social science resources, lawyers could tell us, or perhaps law professors could tell us, whether the law is reaching a crisis situation in which it is going to demand the kind of information that social science can give it. If it is, then we can perhaps see ourselves in a situation where social sciences will develop tremendously.

Social sciences have not yet been utilized effectively, but they are probably ready to be. And social scientists could probably look for ways in which they can become more ready than they now are for demands that might be made upon them, because I think that it is going to be a "pull" effect. It is not going to be a "push." We can try to disseminate the literature as much as we want, but until people feel that they can use it, they are not going to find the time to read it. But when a crisis situation occurs, and lawyers and judges say "yes, we need more and better information than we have," then social science research will probably be used whether it is very good or not. I think that it is up to the social scientist to see that it is the best that we can have at that time.

PROFESSOR FRIEDMAN: I used to be amused when I would look at one of these old fashioned standard legal encyclopedias like *Corpus Juris*. You look up "forest": "Forest is a herbivorous quadrant frequently inhabited by beasts." The footnote would say, "*Smith vs. Glass*." In any event, the only kind of reality that was ever admitted was something in a case. What reminded me of that is that, unless I was dozing off from time to time, all of the people who were talking about application were talking about application in and by courts. The legal system is very vast, very complicated. There is the Food and Drug Administration, there are zoning boards, there are so many

agencies that can use information. I haven't even the vaguest idea which of these agencies are possible customers for social science information. But to measure impact only in terms of persuading courts to change their decisions is, I think, somewhat overdone, or short-sighted, or something along those lines. I think that social science research or information is influencing our society, and our society is a highly-governed society. Therefore, it is influencing law in a very real sense.

But I also want to make another remark: Because most of us are lawyers, when we talk about social science study of the law, we are thinking about its application to the law. But if research is truly interdisciplinary, it might just as well be applied or useful somewhere else. There is a kind of assumption in this discussion that when the lawyer and the sociologist are working together, they are producing something that is useful to the *law*, and the sociologist is some sort of hand-maiden. But it is perfectly possible that they gather together because the social scientist sees something in the law that would be illuminating to the understanding of society. I think that this does go on and should go on.

And then last of all, is basic research out of fashion? Do we have to say we can't try to understand the fundamentals of society and the legal system simply because we can't see an immediate application?

PROFESSOR WALKER: It occurs to me in connection with your remark, Professor Friedman, that the work that I do is regarded in one of the disciplines I work in as rather basic, and in the other discipline as somewhat applied and unbecoming, and I have therefore become very facile in explaining the utility of doing both basic and applied research.

PROFESSOR LOCHNER: I didn't want to indicate that I thought we had to aim all of our research at some specific application. Surely, I think that the research that I am doing has some specific useful output, but I certainly agree that thinking of one's audience as being only judges and lawyers is a very limited area of application. It is far more likely that law and social science research can be used fruitfully in large bureaucratic organizations. I have, I guess, more doubts about the city council as a fruitful forum. There are new problems there because if you look at most city governments, especially in a small town, there are problems of turn-over, no expertise, no money to do or even look at research.

PROFESSOR ROWEN: There are techniques that have become well known for disseminating social science research results to bureaucratic organizations including some previously mentioned. They are not the sort of techniques that appeal to everyone. They are not likely to appeal to the average academic person. For example, such a person is not likely to favor the notion that you are not likely to persuade people by simply writing reports, writing books, because nobody reads them. But that's true. Disseminating information takes an enormous amount of persistence. You have to use direct persuasion, and this takes time. If you get thrown out the front door, you go around and try the back door. If they close the back door, you try coming in through the windows.

For example, we were invited by the mayor to do research into the problems of New York City. That was a necessary but not a sufficient condition to going in and doing anything useful. One of the organizations that we began working with was the police, not because the police commissioner was interested, because he really wasn't, and not because most of the uniformed force was. We gained *entre* because the mayor had enough leverage momentarily to get "our foot in the door." We promptly got it "slammed on us." In the meantime we really did some very simple but useful work. In a year, we were thrown out—just thrown out!

It turned out that most of the work was implemented either immediately or within a couple of years—very fast implementation. But it is also very clear that for that work and other work that was done for New York City by other agencies in New York City, report writing was the least useful mode of persuasion. In fact, reports caused a great deal of embarrassment because they almost always contained something that was disliked or embarrassing to the bureaucrats.

A year or so later, a new police commissioner replaced the one with whom we had been dealing, a different sort of fellow, and he invited us back. We are now back working with the police in New York City. In the meanwhile, something new has happened. Our researchers, or rather, former researchers, are now being hired by the police force. Well, once this sort of person begins to appear, when analytical people are allowed to come in and there is someone who can act as an intermediary, you achieve results much more readily.

One of the things that we insisted on doing was to set up a permanent institute. It remains to be seen how permanent it will be. In any case, it was intended to be permanent and to act as a continu-

ing institution in New York City because it was clear from the beginning that report-writing, book-writing, article-writing simply would not do to disseminate information. To do this required day-to-day working with the police. It required the symbolism of a semi-independent institute. And it required people who were interested in and willing to go through the business of being thrown out and knocking on the door again. That type of activity is just plain different from what most of us do and from what most of us want to do.

PROFESSOR CAVERS: I think that there has been one development that hasn't been touched on here which I think can be and is being an influential customer for research of the kind that we have been discussing. That development is the proliferation of activist organizations in a great many fields. You have people who are often amateurs, who have some clerical staff, but who are educated to the problems of the field, and who can take materials written in social jargon and translate them into ideas that their constituencies can appreciate and then go forward with campaigns. More often these campaigns are conducted at the administrative level rather than in the legislatures and certainly more often than in the courts. I think, though, that in the courts you see some litigation by some organizations using data of the kind that would not ordinarily come out, and the court in the natural course of events must be exposed to this. I think that we do now have a kind of constituency for the products of research that is quite new, that is both professional and on the law side, and that is also active in the development of communication with populations as a whole. I see something of this tendency; I am glad that it is building up; and I recognize that it is of long-range importance.

DR. KONOPKA: I would like to hark back to something that Earl Johnson mentioned, which I haven't heard discussed since he raised it. It is a question with which I am concerned because we fund applied research, and almost nothing but applied research. There still can be basic research done in our projects, but there will ultimately be defined applications somewhere within each project. Professor Johnson points out that where a person has to become the spokesman for the research that he is doing, there is a danger that he may lose his credibility as an investigator or as a scientific researcher. This is something that we are a little bit concerned with because if this is so we are worried that we won't get high caliber researchers to do the work that we need to have done. Because our applications are problem-solving orientated, it turns out that our researchers almost in-

variably have to become spokesmen for that which they have learned.

I would like to get a sense of feeling from some of the people who do work and have to either become spokesmen for it or simply let it sit on the shelf and gain acceptance by its own merit, as to whether or not applied research is going to run into an unsolvable problem when it calls on researchers to become spokesmen for the results of their research.

PROFESSOR WHEELER: It seems to me that it is very clear that you may have to turn to a researcher to understand technical aspects of his research, or some feature of the research, or some feature of the interpretation of the evidence. But I do not think the researcher necessarily has to be turned to with regard to social policy conclusions that could be drawn from his research. Therefore, he does not necessarily have to be turned to as spokesman. So it is not clear to me why it is essential that he be one.

DR. KONOPKA: There are times when a researcher can simply say, "I will investigate the question that you policy makers are considering without any preconceptions as to my results or which side I will support." There are others, I think, where the question defines itself around the need for an answer. The researcher then has to become a persuasive arguer to a policy maker, saying, "If you wish to achieve this stated end, you must go in the following fashion. I really can't give you a set of options." Therefore, if, for example, we are studying the effect of the exclusionary evidence rule on the way police treat suspects and evidence shows that there is more harassment when there is an exclusionary evidence rule because the police have to extract more physical evidence, then I don't think that the researcher can simply leave the option open. He has to become a spokesman.

DR. PADAWER-SINGER: I would like to add an optimistic note to this meeting. I believe that lawyers are becoming more aware of the positive contributions of social scientists. In my personal experience, I have been asked by the American Civil Liberties Union at times to furnish them with some of the social science information that I had, which would help them to prepare a case. I think that there is much more communication and acceptance than we are willing to acknowledge right here. I think that we need a clearing center for information and maybe more such meetings. The consumer is becoming much more aware.

DR. HUSZAGH: I just want to make one more point. I guess it stems from what I would call "charming" research again and some-

thing about meaningless messages. I have a feeling that we can overdo the methodology. Now scientifically, sophisticated methodology tells us better things. But I have noticed that in some courts' opinions, and in a way, antitrust is a good example, the decision-makers start assimilating this data, and the more sophisticated the method gets, the more difficult it is to shove the information through the hole. I mean, the over-sophisticated methodology adds "spikes." I think that this is another communications issue. Maybe there is a virtue in always asking ourselves: Can we, without too much degradation of results, use a method that is simpler, maybe even more expensive, but that is more compatible with the audience's reception?

PROFESSOR ZEISEL: Do you know what the solution is to this? When you explain what you did, instead of saying, "I have invented infinite decimal calculus," say "with the well known methods of the ages, I have determined," and so forth. It is a question of what one does and what one says.

DR. HUSZAGH: I think that it is a little more than that. I have a feeling that some of the decision making bodies, especially at the lower levels, may not have a great ability to articulate and to assimilate certain kinds of information. But their intuition, I think, is quite uncanny, and I just don't think that we ought to underestimate that feature.

V. THE FUTURE OF LAW AND SOCIAL SCIENCES RESEARCH

A. *Presentations*

PROFESSOR WALKER: The topic for this morning is "The Future of Law and Social Sciences Research." The first speaker is Professor Marc Galanter of the State University of New York at Buffalo.¹

PROFESSOR GALANTER: Thank you. I would like to begin this exercise in futurology by addressing what I think is our intellectual problem. When we talk about the "Future of Law and Social Sciences Research," we tend to talk about institutional problems. I would like to address what I consider to be the conceptual or the theoretical aspects of this research.

As I see it, the basic problem of establishing such research as a continuing kind of activity in the American law school or university setting is the poverty of concepts and theories that characterizes this research. We need concepts which could help us to organize what

we know, to ask good questions, and to begin to create a viable social scientific understanding of the legal process.

I am sure that there are many obstacles to the development of better conceptual tools, but the one I would like to take up in my limited time, because I consider it a very central problem, is the relation of this kind of emerging social scientific study of law to the received perspectives that come to us from so-called classical legal scholarship. In discussing this I am going to over-generalize outrageously because of limited time and limited perspicacity. I shall omit all of the disclaimers and qualifications that would persuade you that I am really a very reasonable and sensible fellow.

I don't really have time to elaborate what I mean by the "received perspectives of legal scholarship" but I think that they are quite familiar to all of us. I speak of that set of implicit presuppositions about the nature of the legal system that informs most legal scholarship: views about the centrality of courts and adjudication to the whole process; the importance, the overwhelming importance of rules; the congruence of behavior with authoritative prescription, and so on. I know that we don't believe these things literally, but we tend in our teaching and research to act as if they were the normal, expected way of things legal and to proceed on that basis.

These presuppositions are there underlying most legal research. Legal research reflects them with its very heavy emphasis on the judicial as opposed to other legal agencies; in its concern with doctrine as opposed to other features in the legal process; in its concern with the agencies at the top of the system rather than those at the field level; in its stress on the agencies of the state and in a complementary absence of interest in non-state forms of social ordering; and in the assumption that for the most part rules reflect and control behavior.

Now one might say that we have moved away from doctrinal research or are moving away from it at a rapid rate as Professor Cavers' very valuable survey of the field shows.³⁶ There is considerable movement, and to paraphrase President Nixon, we can say, "Well, we are all legal realists now." Everyone is attuned to the gap between the law on the books and the law in action.

The point is that the ghost of doctrinal research and the assumptions it embodies still haunts our efforts in social research in law; and that these same presuppositions that underlie doctrinal research are

36. Cavers, *supra* note 1.

still around and influence what gets studied and how it gets studied. They shape the kind of questions that we bring to material, the way we interpret it, and lead us to ignore some fruitful subjects for study. The received view shapes what we study not in the sense that we go around deliberately affirming these notions of what the law is like, but largely because of the absence of any richer or more adequate intellectual lenses through which to view the process and approach our material.

The discrepancy between law on the books and the law in action has been discovered innumerable times since Roscoe Pound used the phrase in 1910. I think it is an embarrassment that we have gone so little beyond that formulation, which is still with us today. This gap has been discovered innumerable times. The question is: What do we do with the discovery? I think that depends on our picture of what is normal and typical in the legal system. From within the "received view" each time we discover that gap between the law on the books and the law in action, we see it as an exception, something atypical, something peripheral, something that departs from the normal course of things. And so, awareness of these discrepancies doesn't induce us to relinquish our model of how the legal system works. It spurs us on instead to add some ad hoc explanation to account for those irregularities.

These received views seem to have a great tenacity. They are very powerfully supported by the training and intellectual orientation of the legal profession, training which provides very elaborate categories for organizing and classifying, and remembering rules, but few conceptual tools for perceiving and keeping in view the non-rule features of the legal process. We are left with a kind of learned incapacity to perceive, recall, and compare features of the legal process in operation. This tendency is reinforced by the practical orientation of the profession: We are in the "doing something about it" business. When we discover a gap between norm and practice, this is taken not as a cause for reflection on theoretical models but as a spur to action.

I am not suggesting that legal scholars are unaware of the departures from that which is expected in the received view of the legal process. They are well aware of the prevalence of bargaining, the politics of enforcement, the factors of cost and delay, the disparities of legal services, the pervasive overload of institutional facilities—all factors that might be regarded as anomalies, whose presence makes the received view seriously suspect as a tool for understanding. The

literature is full of sophisticated accounts of the interweaving of these deficiencies with the components that are considered to *really* describe the legal process—courts, rules, compliance and so on.

What I am suggesting is that the received cognitive map tends to inhibit appreciation of the significance and possible explanatory power of all of these discrepancies. By taking the major regularities and relationships as already known, it keeps us from asking some of the large questions about the legal process and the gross relationships between law and other social phenomena.

Instead, we concentrate on this problem of the law on the books and law in action, the failure of practice to conform to the stated objectives of particular institutions or rules. This law on the books versus law in action formulation *appears* to challenge the received view, or at least come to terms with and incorporate all that departs from it; but I submit that in subtle ways, it gives expression to the received view of the legal system and inhibits the development of alternative theoretical formulations. To say, "there is a gap between the law on the books and the law in action," expresses an expectation of harmony and congruence between the authoritative, normative learning on the one hand and patterns of practice on the other. Each instance of the gap is labeled as atypical or deviate; it is a problem and presumably one which can be solved by appropriate purposive manipulation of rules or institutional arrangements. This brings us to the problems in a way that prevents us from entertaining the possibility that other relationships than harmony and congruence between law and behavior are expectable, normal, and natural. That is, obstructs any attempt to build more complex models of the interaction of law and behavior that depart from this notion of one-to-one correspondence that lies in the background of that law on the books-law in action formulation.

It is as if we attempted to understand language behavior by focusing on the difference between written and spoken English, and we assumed that some kind of harmony or correspondence between them was the normal condition and that our job was to explain those special circumstances that led to instances in which there was deviation. But, we have a much more differentiated cognitive map of language and a much greater ability to label, describe, and recall differences among kinds of usage than we have in the case of law. For example, in our every day conception of the English language, we recognize the co-existence of formal literary language with all kinds of local class and ethnic varieties and occupational jargons; not to mention all kinds of

in-group argot; pidgin; slang; deaf-mute sign language; baby talk. We know they are all the English language; and while we retain some notion of their unity as a manifestation of a common underlying code, we can ask about the way that this common code is refracted by different groups in different settings. This differentiated picture permits us to ask questions about the regularities among and transformations between different sectors and groups of users. We would not necessarily expect any extra-linguistic event to affect all sectors of language in the same way. We can imagine ways of asking about change or persistence, and about patterns of mutual influence or lack of it between these sectors.

What I am suggesting is that we need to supply ourselves, at a minimum, with some terms for making a similar kind of differentiated description or mapping of the legal process. We need more from the research enterprise than merely complementing legal learning as it exists by adding an empirical dimension. Instead, we should be interested in the task of theoretical construction or reconstruction of ways of looking at the legal system—of expanding our conceptual apparatus to encompass features and relationships which lie beyond the boundaries of received legal scholarship.

I say this not in any way to denigrate research which attempts to go out and measure the existence of these discrepancies or the scientist who sees his task as the addition of an empirical dimension. The proliferation of studies of this kind has immensely increased the amount of available information and makes possible the consideration of the kind of theoretical reconstruction that I suggest is desirable. It has also led to a great development of sophistication in the application of research methods to legal phenomena. There are many good things to be said about this kind of research, and there is, I am sure, no shortage of people to say them.

So I would like to say a word in behalf of research which attempts some kind of autonomous theoretical reconstruction of our picture of the legal process. This is a more precarious undertaking both intellectually and institutionally and has fewer friends to speak up for it, partly because it is like the young man who continues to be promising for too long. It consists largely of aspirations and is received with the same polite skepticism at this point, and deservedly so. But I think that if one looks at the literature, there is more there than just aspirations.

There are a number of studies which do undertake a search for

new categories that cut across and subsume the received cognitive map of the legal system and which contain very promising leads for conceptual development.³⁷ And, of course, there are an enormous number of studies by a variety of sociologists, anthropologists, and political scientists, which apply to the legal process concepts originating in other areas with results, some more interesting and some less.

I want to emphasize that most inquiry about law should and will continue to be linked to the policy concerns of lawyers. But the question that we are going to be facing in coming years is whether there is a case for some kind of autonomous inquiry as well; that is, an inquiry that explicitly strives for general theory about the relationship between legal and other social phenomenon, which includes both empirical studies and theoretical explorations, which is concerned with cross-cultural verification of hypotheses, and which selects its subject matter and methods not with an eye to solving pressing problems of the day but out of a commitment to the attainment of universal, scientific generalizations about law in society.

The case for such autonomous social research on law need not and should not rest on its practicality. It should be clear at this point that social science does not offer a "rival and superior way of managing practical human affairs"³⁸ as was sometimes asserted by its enthusiasts. Nor should it rest on the notion of a pure science from which some kind of useful engineering applications can be derived. Both those aspirations ignore the problem of the very different orders of generality and precision, and abstractness and complexity, which are involved in different kinds of decision making.

The claim to practicality must be limited to the modest contribution that such critical, autonomous social research in law might make to the problem identification and problem solution by the people working in policy areas. More generally, it may prove stimulating and suggestive for the process of training lawyers and other participants in public affairs.

Beyond this, the case has to rest on its theoretical interest, on the sense of obligation to pursue understanding of a pervasive aspect

37. Abel, *Toward a Comparative Social Theory of the Dispute Process*, 8 *LAW & SOCIETY REVIEW* — (forthcoming 1974); Black, *The Mobilization of Law*, 2 *JOURNAL OF LEGAL STUDIES* 125 (1973); Friedman, *Legal Culture and Social Development*, 4 *LAW & SOCIETY REVIEW* 29 (1969).

38. The phrase, but not the assertion, is from Kalven, *The Quest for the Middle Range: Empirical Inquiry and Legal Policy*, in *LAW IN A CHANGING AMERICA* 59 (G. Hazard ed. 1968).

of human action. The claim is not unlike that to be made for the social-scientific study of religion. Presumably, there is room in the world for a sociology of religion in addition to studies by various votaries interested in reformulating and enriching their particular creeds. Obviously, concepts found in one or another religious tradition may be suggestive to people working in the sociology of religion, and the findings of an autonomous sociology of religion may assist churchmen in their undertakings. But presumably, as an autonomous enterprise, the sociology of religion is not confined to empiricizing religious concepts as they exist in various religious traditions nor to research ostensibly useful to religion. One need not argue that the sociology of religion will contribute to salvation, spiritual elevation, or even building up church attendance.

If one is attracted by the notion of relatively autonomous (I don't like the term, but I don't have a better one at the moment) kind of social research in law, the question is: How do we get there from here?

Obviously, such a body of social research can't be proclaimed into existence. If one agrees that this is a desirable destination, it does have some implications for the present direction of research. It suggests, first of all, that one priority for research should be its promise for conceptual development. This in turn suggests to me that we should give priority to studies that attempt to take the "givens" in the received view of the legal system and make them into variables to be explained. That is, we should be looking for studies that take courts, rules, enforcement, compliance—all of the background assumptions of our everyday views of how the legal system works—and seek to explain their presence and character. This might be done by beginning with studies of the discrepancies and gaps, the so-called anomalies that don't quite fit our view of the normal, typical legal system at work—bargaining, cost barriers, disparities of legal services, institutional overload, and so forth. These can themselves become a central focus for research. They should be treated not as something marginal and abnormal but should be moved to the center of our field of vision and treated as central and pervasive features of the legal process.

I am not suggesting that such studies must start from scratch. One great need at this point is the reanalysis of existing data. For example, there are about thirty studies of plea bargaining that have a great deal of descriptive material in them. I submit that an inter-

pretative piece reanalyzing the findings of these thirty studies would be a much greater service to the development of social research in law at this point than would the thirty-first study.

Secondly, I think that the aspiration for autonomous social research in law suggests that one very promising area for research would be the exploration of the sociology of legal knowledge. How do legal professionals acquire and process information? What kinds of cognitive maps do they have? How do they get these maps? What are the cognitive, political, and social factors that maintain and perpetuate them? How do the literary and pedagogic forms that we use reflect and support this cognitive mapping? What kind of wider impact do these forms have? What is the impact of legal scholarship? What are the effects, really, of all of these policy oriented studies? Considerable disagreement has emerged at this meeting on what kinds of linkages could be expected between policy-oriented research, on the one hand, and policy making, on the other, and it seems to me that this in itself would be a very fruitful topic for research.

Finally, I suggest that the search for more adequate conceptualization might be facilitated if we would consult with scholars engaged in other studies of pervasive, normative orderings. Law isn't the only such ordering in our society. I would like to see us attempt to share things, maybe only share mutual difficulties or learn that we have nothing to share, with social scientific students of religion, language, medicine, and so on.

Having expressed these hopes, let me close by expressing some puzzlement as to where this is going to take place; there is a real problem of an institutional setting. Although the professionally oriented American law school may be opening up to empirical research, it may find social research of the kind that I have been talking about uncongenial. Such research is going to make problematic what is taken as axiomatic in professional life and very largely in professional training, and one would have to be extremely optimistic to think that it would be easy to incorporate it into the law school setting. The problem is one of creating a second milieu in the law school, and whether this can be done remains problematic. One possibility, in some law schools at least, is the development of an undergraduate program, which would provide a base for having faculty with a major commitment to non-professional studies. Autonomous research institutes within law schools are another possibility, and yet a third is that this

isn't going to take place in law schools at all but over in the social science departments or elsewhere.

PROFESSOR WALKER: Thank you very much. The second speaker this morning is Professor Lawrence Friedman of Stanford University.

PROFESSOR FRIEDMAN: My remarks will be somewhat briefer than they otherwise would have been, because a lot of the points that I would have made have been covered quite well by Marc Galanter.

He has, for example, already touched on what has to be the main point in any discussion of institutional aspects of "The Future of Law and Social Sciences Research." That is this: there are good reasons to doubt that law and social science will ever be, in the predictable future, at the core of legal education either as an academic subject or as the focus of research. First of all, it cannot be sustained by the market for its product. There *is* a market in society for what law schools turn out, but that is a market for lawyers. Law schools have succeeded in persuading the bar and society, through one means or another, that they can efficiently turn out those lawyers. Perhaps their real message to the bar has been that they are an *inefficient* way to train lawyers; they guarantee a certain stringency of supply. That is a complex issue, which we need not go into here. Neither do we need to confront the question whether law school trains *lawyers* as such. People think they do, at any rate, and, for our purposes, that is enough.

There is, then, a market out there, which we confront from time to time. When we law professors meet alumni, or hear from them, or feel their vibrations, we get the message that what they basically want is traditional legal education, as they remember it. They are willing to accept a certain number of fringe activities, just as they will accept, at a certain level of affluence, a very expensive rug on their office floor. Obviously, if times got rough, the rug would have to go. The rug has little enough to do with the basic practice of law. Law-and-social-science is one of those fancy, prestigious things that rich law schools indulge in. These schools never for a moment deceive themselves; they know which activities are the luxuries, the rugs, and which are the main show. Moreover, law and social science competes with other rugs—comparative law, legal history, jurisprudence. All claim a share of scarce and jealously guarded resources.

In short, I doubt that law and social science will flourish in law schools except in a limited, marginal way. The area will have to have

money from outside. Law schools will not move by themselves very far in this direction. They will, however, allow themselves to be bought.

But are there buyers? Who will pour vast sums of money into a field which has produced so little and which does not promise very much? We can feel the demand for investment in cancer research, advanced weaponry, and so on. But law and social science? There is some push, perhaps, to put money into law enforcement. For basic research on law and society—even applied research—it is hard to see much demand. No particular demand comes from the academy. More or less traditional-minded scholars still dominate law schools. They remain impervious to criticism and change because they have total control of their institutions. They little care who carps at them from the sidelines.

It is not hard to see why this is so. Law teachers are recruited from law students by and large. The academic profession puts highest value on brilliance, perfection, logic. The model of the great law school is a kind of honeycomb of offices; in each one sits an individual genius, cogitating to himself. The emphasis is not on research but on classroom performance. It is not only possible but common to find endowed professors at the most prestigious law schools, who have produced nothing beyond a handful of articles and a casebook. It is hard to think of another field where one can make so tremendous a reputation by editing materials to serve up to students. As a matter of fact, in some fields a reputation might have to be built in spite of such activities. Of course, it is good that law schools pay attention to teaching; still, this order of prestige discourages research on law and society.

Yet two generations of querulous attack have taken their toll on traditional legal scholarship; and law professors are attracted outside of traditional boundaries. But they are drawn to fields that resemble traditional scholarship in certain ways. One of these fields is the application of economic theory to law. Another seems to be the analysis of legal language and thought in the light of modern philosophy. These two, unless I am mistaken, are among the more vogueish pursuits of young law teachers. They are rigorous, analytic, and require no field research. I hasten to add much of this work is exciting; vogueish is certainly no synonym for bad.

On the other hand, students these days are more or less activist by nature. If they had wanted to do research, they would have gone on for a Ph.D. in some scholarly subject. They have explicitly rejected

this route, and they tend to be somewhat impatient with law and social science. It has rather meager financial returns, and its aims and style are not to their taste.

All these factors do not incline me to total gloom; they do reinforce our earlier prediction. Legal education, in the foreseeable future, will continue more or less along present lines; specifically, law and social science (except possibly for its economic cousin) will not stand at the core of law school education.

Consequently, I agree with Marc Galanter that there is much to be sought from independent institutes or from pursuing our social science brethren into their own departments. Here too problems abound. Legal affairs come clothed in complicated jargon; law is rather repellent and hard to get at for social scientists. This is one of many reasons why scientists, when they turn to law at all, favor criminal justice. This is more accessible, less technical, to them than the other parts of the legal process.

Another problem is that social scientists on the whole take their image of law from lawyers—naturally enough, since lawyers have a monopoly on information about law. Still, social scientists may make incorrect assumptions about law precisely because their information filters through the work and the minds of lawyers.

So much for institutional problems. I do foresee *some* increase in the effort that will go into law and social science in the future. And a couple of areas, so far neglected, will enjoy, unless my antennae are picking up wrong signals, something of a minor boom.

One is psychology and law. This is long overdue. There has been some interest in psychiatry or psychoanalysis and law, but this has diminished. As far as psychology itself is concerned, there has been some interest in the fact that six witnesses at an accident might describe a car as pink, while another five would say it was blue. This did not seem to lead very far. In recent years, however, a few scholars have turned to problems of attitude and behavior rather than perception: compliance and deviance, the relative efficacy of rewards and punishments, application of learning theory to law, and so on. Some of these interests are not absolutely new, but they have gotten a new lease on life. At present, no more than two or three law schools have given joint appointments in psychology and law. There is bound to be more action in the future.

The other field, in which I have something of a personal stake,

is behaviorally-oriented legal history. History has been a dank back-water of legal education at least partly because it was identified with the land law of medieval England, a real mark of Cain. In recent years, legal history has been reinterpreted. The main emphasis is now on American legal history, much of it recent. Students and teachers find this more attractive than ancient land law, which is not surprising. For reasons that are not entirely clear, the message has gotten through that traditional legal scholarship is slightly passé; therefore, as we have already noted, legal scholars thrash around for something else. We have mentioned some of the things they come up with. Legal history is another. Sociology seems out of the question; it involves statistics and requires going outside the law school, which is an unacceptable strategy. History seems safe. On the other hand, once legal scholars get into history they become slightly behavioral. Partly this is because of the tremendous influence of Professor Willard Hurst of the University of Wisconsin, whose legal history stresses the role of law in economic development. Psychology and history, then, look like growth stocks. Economics will continue to hold up strong. Sociology, anthropology, and political science limp along as best they can.

To return to a topic touched on in prior sessions: Does the social study of law provide insights useful in changing or reforming the legal system? At first blush, the pay-off seems small. But it is easy to ignore indirect, long-term effects. Research sometimes has an impact on behavior through dissemination in the air—something like a virus. Consider the ultimate influence of ideas that Freud set loose. Consider the way they caught hold of intellectual life and became ultimately watered down by way of *Readers' Digest* articles, movies, and the like—in the end probably affecting behavior. In this way ideas and discoveries of social science do get disseminated and popularized and do ultimately have an impact on the way people think and behave.

Law is no exception. The climate among judges, lawyers, legislators, and administrators is different from the climate among law-men in the nineteenth century. There are new ideas about the role of law, some of which ultimately stem from or are influenced by findings or concepts or theories of social science. There is a kind of political or theatre effect of social science research. Something quite small or obscure, dust-covered on the shelves, may be picked up or in some way dramatized; it thus finds its way into the blood-stream of the *Zeitgeist*. Even those of us who are whining around the fringes of law schools, running down the halls wringing our hands and the like, may have

an effect on some students. The students go out in the world, and in the long run there may be some sort of ripple effect. It is difficult to measure, but quite likely it is there.

Finally, I would like to comment on the need for more funding of research. There is an assumption that we have something to offer and that resources would be spent wisely, that we have contributed and will continue to contribute something worthwhile.

Is there any real reason to believe that tripling resources and expenditures on research in law and social science will be worth the added cost?

I believe it would be, even though results to date have been meager. In the first place, applied or lower level research, relatively inglorious, can often make a real difference, not to society as a whole, not to the overall distribution of income, not to the quality of justice in America, but to some small corner of life or law. When we consider how many people go through life without making *any* difference, we ought to stop ignoring the results of these micro-studies. It is a magnificent achievement if someone does a careful study, showing that the small claims court of Clackamash County is a disgrace and publishes it and something is done. We need hundreds of studies of this sort,

At the same time, there is a strain toward developing general theory of law and society. General theory does not mean formulations so abstract as to lack practical meaning but induction plucked out of small, disparate studies, generalizations which can then be applied to wider and wider areas of law or human behavior.

For example, there has been centuries of talk about the deterrent effect of punishment and the incentive effect of reward. Much of it is very interesting. But it was wholly non-empirical—indulged in by philosophers among others, a sure sign that no one looked at any data. Now, in the last several years we have begun to get research—a respectable number of articles and studies, even a book or two. Some studies fall short of the mark in one way or another, but it is interesting and important to see scholars strain toward generalizations about conditions under which parts of the public react in different ways to threats or rewards for this or that act. This research can and should be merged with work on the general theory of legal behavior, and beyond that, with work in the science of behavior in general. Perhaps some results may even have predictive value. On these scores I think

we could in fact make use of additional funding. We cannot aspire to take over the law schools. But we have made a promising beginning. We will at least continue to justify our pay.

PROFESSOR WALKER: Thank you very much. The third speaker this morning is Professor Thomas Raiser of the University of Giessen.

PROFESSOR RAISER: Thank you. I want to first give at least a brief explanation of my background in legal studies and sociology. I am a law teacher in Germany. My special subjects are commercial, corporation, and antitrust law. About five years ago I also engaged in the study of sociology of law. I am still in the process of learning and evaluating social results or perhaps empirical results, as well as sociological theories, and classifying them for the use of the discipline of law.

What I want to present today is some small remarks about what has been done in the field of sociological work in Western Germany during the last ten years.

Empirical work had an early start in Germany between 1910 and 1920 through the efforts of Eugen Ehrlich and Arthur Nussbaum. Due to lack of public and monetary support this work remained of quite limited importance, and died soon after that early start. Empirical research was not resumed until about 1955. This delay in scientific progress had several causes which still determine our present situation; we have a comparatively small number of people involved in empirical work. Until 1955, and also during the last ten or fifteen years, most efforts have been directed to the theoretical questions. Perhaps the most important cause for this was the suppression of sociology and the emigration of many of the best sociologists and jurists during the Hitler regime. These interferences not only interrupted scientific development until the end of the war, but, perhaps even more fatal, created a serious shortage of learned sociologists, which has not been fully overcome even today. In addition, the process of intellectual and political regeneration directed attention to other objectives. Problems of family structure, social stratification, and political reorganization seemed to be more urgent. In particular, many sociologists were intellectually involved in the confrontation between capitalist and socialist systems of society, whereas jurists were usually engaged in the reform and stabilization of the social and legal order in Western Germany. Further, it was necessary to establish sociology as a new, separate discipline at the universities and to develop patterns of teaching. Another urgent goal was to review and reactivate the existing wealth of

theoretical ideas and to reintegrate ourselves into the international level of discussion. Last but not least the continuing lack of public interest caused a serious shortage of personnel and money available for field research. So the production of the last twenty-five years has been rather more theoretical than empirical, and if you were to ask me what might be the contribution of post-war German sociology of law, I should have to above all mention these efforts.

Let me give you some names of German theoretical researchers, not all, but some. First of all I should mention Theodor Geiger, who died in 1952, and who was a very important analyst of concepts of legal and sociological theory. He is almost unknown in this country. Others are Ralf Dahrendorf, Helmut Schelsky, Niklas Luhmann, and Ernst E. Hirsch.

As for empirical work, most of the major publications have been done in criminology, but this area lies entirely out of my field, and so I cannot do more than mention this fact. In addition there have been a number of books that deal with a variety of things, for instance, with the possible meaning of legal concepts and standards, such as customs of commerce and fair practice.³⁹ Others deal with questions of mediation and arbitration,⁴⁰ with the practice of managing insurance cases,⁴¹ with the social and legal situation of illegitimate children,⁴² and with the structure of associations.⁴³ All of these studies have been carried out by lawyers having some training in sociological field research and having some support by sociologists. There have been, furthermore, widespread efforts by both lawyers and sociologists, which have been directed toward three particular subject matter areas: First, the social background and attitudes of lawyers, especially judges;⁴⁴ secondly, the structure of police and the attitudes of police-

39. M. REHBINDER, *DAS KAUFRECHT IN DEN ALLGEMEINEN GESCHÄFTSBEDINGUNGEN DER DEUTSCHEN WIRTSCHAFT* (1970); G. TEUBNER, *STANDARDS UND DIREKTIVEN IN GENERALKLAUSELN* (1971); K. STRACHE, *DAS DENKEN IN STANDARDS* (1968); Limbach, *Die Feststellung von Handelsbräuchen*, in *FESTSCHRIFT FÜR ERNST E. HIRSCH* (1968).

40. K. KOHLER, *DIE MODERNE PRAXIS DES SCHIEDSGERICHTSWESENS IN DER WIRTSCHAFT* (1967).

41. H. WEYERS, *UNFALLSCHÄDEN. PRAXIS UND ZIELE VON HAFTPFLICHT UND VORSORGESYSTEMEN* (1971).

42. G. WINTERS, *SOZIALER WANDEL DURCH RECHTSNORMEN, ERÖRTERT AN DER SOZIALEN STELLUNG UNEHELICHER KINDER* (1969).

43. F. NICKLISCH, *DIE INNERE ORDNUNG DER VERBÄNDE* (forthcoming).

44. W. KAUPEN, *DIE HÜTER VON RECHT UND ORDNUNG; DIE SOZIALE HERKUNFT, ERZIEHUNG UND AUSBILDUNG DER DEUTSCHEN JURISTEN, EINE SOZIOLOGISCHE ANALYSE* (1969); W. RICHTER, *ZUR SOZIOLOGISCHEN STRUKTUR DER DEUTSCHEN RICHTERSCHAFT* (1968); W. WEYRAUCH, *THE PERSONALITY OF LAWYERS* (1964); K. ZWINGMANN, *ZUR SOZIOLOGIE DES RICHTERS IN DER BUNDESREPUBLIK DEUTSCHLAND* (1966); Feest, *Die*

men;⁴⁵ thirdly, the problems of legal procedure, particularly delay in the courts.⁴⁶

The judicial background research has resulted since 1960 in at least ten major publications. The most important are the original English volume by Walter Weyrauch on *The Personality of Lawyers*; and two books by Wolfgang Kaupen: *The Guardians of Law and Order* and *Lawyers Between the Authoritarian State and Democracy*.⁴⁷ Kaupen and others point out the bar recruits a very high percentage of middle class people whereas only about five percent have ascended from the lower classes, which comprise more than fifty percent of the entire population. In addition, they show that there has been no significant change in this fact for half a century. That leads them to the question: How does this social recruitment influence the typical attitudes and political preferences of lawyers, the style of jurisprudence, or even single judicial decisions? They suggest that lawyers are mostly conservative, authoritarian and inflexible toward social change and conclude from that result that judicial decision making, or perhaps the entire jurisprudence in our country, shows an inclination toward class justice and authoritarian social and political structures.

I myself doubt very much whether this tells the whole story, but I won't discuss such questions here.⁴⁸ Instead I should like to direct your attention to the problems which arise from these studies and which can properly be subjected to international scientific discussions.

If am right, the social recruitment of the judiciary in the United States does not, in spite of all differences in the process of election

Bundesrichter: Herkunft, Karriere und Selektion der juristischen Elite, in BEITRÄGE ZUR ANALYSE DER DEUTSCHEN OBERSCHICHT (W. Zapf ed. 1964); Richter, *Die Richter der Oberlandesgerichte der Bundesrepublik: Eine berufs- und sozialstatistische Analyse*, in HAMBURGER JAHRBUCH FÜR WIRTSCHAFTS- UND GESELLSCHAFTSPOLITIK (1960); Rüschmeyer, *Rekrutierung, Ausbildung und Berufsstruktur: Zur Soziologie der Anwaltschaft in den Vereinigten Staaten und in Deutschland*, in SOZIALE SCHICHTUNG UND SOZIALE MOBILITÄT (R. König & D. Glass eds. 1961); Zapf, *Die Verwalter der Macht: Materialien zum Sozialprofil der höheren Beamtenenschaft*, in BEITRÄGE ZUR ANALYSE DER DEUTSCHEN OBERSCHICHT 95-113 (W. Zapf ed. 1964).

45. DIE POLIZEI: SOZIOLOGISCHE STUDIEN UND FORSCHUNGSBERICHTE (J. Feest & R. Lautmann eds. 1971); A. GOESCHEL et al., BEITRÄGE ZU EINER SOZIOLOGIE DER POLIZEI (1971); PROFIS, POLIZISTEN UND PROZESSE (E. Blankenburg & J. Feest eds. 1971).

46. BAUMGÄRTEL/MES, RECHTSTATSACHEN ZUR DAUER DES ZIVILPROZESSES. MODELL EINER GESETZESVORBEREITUNG MITTELS ELEKTRONISCHER DATENVERARBEITUNG (2d ed. 1972); R. BENDER et al., TATSACHENFORSCHUNG IN DER JUSTIZ (1972); E. KLAUSA, EHRENAMTLICHE RICHTER (1972); E. Blankenburg, Die Dauer von Zivilprozessen in erster Instanz vor dem Landgericht, 1971 (unveröffentlichtes Manuscript in Freiburg).

47. See authorities cited note 44 *supra*.

48. See T. RAISER, EINFÜHRUNG IN DIE RECHTSOZIOLOGIE (2d ed. 1973).

and appointment, differ so much from Western Germany. If this is true, the question can be raised: Do judges show similar behavior in both countries, or are there significant differences in conduct and value preferences? What is the explanation for both similarities and differences? Or even more important: What is the impact of such characteristics on the process of judicial decision making or the process of legislation? What effect will these characteristics have on society as a whole, on social stability, and on social change? Finally, how do we assess them in the light of political life? Are they or their inherent consequences neutral, or appreciable, or even dangerous? For what reasons? Is it advisable to plead for a change of this recruitment procedure and these features, or is it advisable to keep them? There is a potential gain from widespread international discussion about the results of empirical and social work.

To take another example, the two German studies on the police⁴⁹ reveal, at least in general, a closely related pattern of recruitment, organization, and personal behavior between the police of our countries. The critical problem in Germany, as in the United States, is the tension between the requirements of effective law enforcement and personal freedom. In both countries police are prone to, or at least are in danger of, emphasizing public order at the expense of civil rights. And in both countries the police are likely to develop certain patterns of managing beyond the limits of strict legality.

But social scientists should not confine themselves to pointing out these facts. On the contrary they should also evaluate them to look for the possibilities and consequences of changes. I am aware, of course, of the great discussion on the possibility of value-free scientific experience that, in the succession of Max Weber, troubles the philosophy of science, and I don't believe that we are allowed to blur the change from investigating facts to evaluating them. I also admit that there are situations in which scholars are wise to refrain from expounding any political view or even making personal comment on the effects that they have found. In general, however, my experience as a lawyer teaches me that social scientists, who include lawyers, have failed their mandate whenever they have refrained from reflecting upon the legal or political implications of the results they have found. They lose control of their statements, which are used or misused by social forces without their influence. The sociology of law in this

49. See authorities cited note 45 *supra*.

case becomes either a game for its own sake or an ancillary discipline, the aims of which are proposed and the results applied by lawyers and politicians. Both events in my opinion should be avoided.

I have dealt with this problem at some length because it is the salient point in determining the objectives of future research. As to this, the major questions are: First, what are the criteria of the selection of goals? Second, who has to decide among the possible projects? The answer to the first question must in my opinion clearly be public utility. This excludes both the progress of scientific knowledge for its own sake and the very special and confined interests of the scholar. But public utility is actually a rather vague and flexible formula which must be filled out and completed by specific argument from particular cases. I don't think a more detailed general rule can be stated; it is necessary to make individual decisions on grant proposals.

Consequently, the real crucial question is: Who has to decide whether to support a program? Now, again in theory, anyone may be capable of deciding and assessing the importance and the social utility of projects. Who has an expert knowledge of matters of social order? Certainly scholars do as well as members of legislative or administrative bodies and even foundation executives. And among scholars, sociologists and political scientists are included as well as lawyers.

Again the difficult question is: Who has to decide the particular cases? And again there is hardly a general rule. I don't think that things are satisfactory as they are now, but I think that *if* public utility is accepted as the major criterion, then the process of selection can involve a number of different persons, institutions, or agencies. And in such a situation I contend that lawyers and practitioners are as competent as sociologists to determine matters of utility. I feel myself as a lawyer entitled to make suggestions about what should be the prevalent future efforts of empirical research.

Some remarks on possible themes from my point of view: In Western Germany, until now, sociologists have for the most part failed in investigating the conditions of order and reform in the area of civil law. There are studies of automobile accidents, damage regulations, and on some aspects of corporation law, but we know almost nothing about the reality of contracts such as bargaining, customs, usages, standardized terms, contractual restriction of liability, and its effects—matters which are fundamental for judicial decision of these topics as well as for legal regulations. Similarly, we have no persuasive

studies on the standards of fair or careful behavior which are observed in social life or in particular professional groups—questions which are relevant for judgment on claims for negligence, and so forth. We also do not know enough about the functioning or the tactics of business enterprises upon which to base necessary reforms of corporations or antitrust law. It might be that things are better in the United States, but I don't have the impression that they are really satisfactory.

Considerations like these lead to the last point on which I want to focus—the problems of practical cooperation between lawyers and sociologists. We have at this conference an example of that in the Fulton-Levy collaboration. Theirs was a successful cooperation, but it seems to me that they are single and exceptional. At least, in Germany, both disciplines have restricted themselves to their own perspectives and have even been hostile to each other. The best relationship seemingly reachable is a kind of skeptical tolerance. Although there have been some arguments in support of this reluctance to cooperate, I don't think that any are really satisfactory. For me, it shows a state of immaturity, which both sociologists and lawyers should outgrow.

Actually, in organizing modern society, both disciplines depend on each other. Jurisprudence will no longer succeed without considering the social realities that are investigated by sociologists, and sociology will remain blind unless it is completed by the traditional legalistic point of view. It is a serious defect. As far as I see, there is not enough reflection on the conditions of interdisciplinary cooperation. We are used to considering such methods as entirely separated and self-contained instead of complementary and interdependent.

Thus, as we are here to discuss the future of sociology of law, I suggest that we should above all discuss having another conference on the important aspect of cooperation; not only the institutionalization of organizations of cooperation between lawyers and sociologists, but much more on the interdependence or the complementary character of the methods that are used by sociologists and lawyers.

PROFESSOR WALKER: Our last speaker is Dr. Howard Hines of the National Science Foundation.

DR. HINES: If I tried to summarize every worthwhile thing that this conference has brought out, I would be here a long time; so I am going to leave out most of what others have already summarized. Many of these points have been quite well made.

When I say that the points "have been quite well made" I mean not only that points of substance have been made, but also that I was impressed by the note of caution (I think "sobriety" is the word) in the presentations. I judge that most of those who spoke were encouraged about the general activity in the area of law and social sciences, but I certainly did not detect any tone of arrogance. (I almost said that I didn't hear the arrogance that I expected to hear; but, of course, am too much a gentleman to say such a thing.)

There are three subjects that could be discussed today. First, the intended focus of the conference, as I understood it, was mostly to be on the methods, the processes of research. On that point, my only regret is that I would have liked to have heard even more than we did hear on the question of conducting this kind of research in law schools as distinguished from somewhere else. Professor Friedman discussed the matter this morning, as others had all through our meeting, but perhaps that issue—it is one of degree—is not something that we can settle soon or at all. It is a question that we should continue to keep before us. I did enjoy the presentation by the "Minnesota Twins" on the practical difficulties they experienced in carrying on research in law and sociology.

Secondly, in addition to research methods and processes, all of us are interested, of course, in the substance of research. That's what we are really after; that's why we are going to all of the trouble of training interviewers, and so on. It is also the justification for the third subject which we have discussed, which has to do with the funding and administration of research. This last matter is the least interesting of all of these topics intellectually, but, of course, it makes the rest possible.

What is most important about funding is that it permits investigators to do research in ways that otherwise would not be possible. That is, it enables the undertaking of inquiries to gather facts and then analyze them much more completely than can be done by an isolated scholar sitting alone in his office. In summary, what we hope to achieve are findings of substance; how we do this is by conducting surveys and experiments and other means of social scientific research, and what makes those procedures possible are funds. If in the end we discover matters of substantive importance, then we can hope for additional funds. If we don't, we will find future fund-raising very difficult and justifiably so.

The social sciences and law are both very complex. It is impor-

tant to recall that there are many, many individual facets to be considered. In particular, we mustn't let the public think the only subject in law that we are interested in is trials for criminal offenses. There is a lot more to law than criminal law, and there are many other points of intersection between law and the social sciences than trials. Research on courtroom trials is only a beginning.

Judge Christian, for example, pointed out the contributions that the social sciences might make to administrative arrangements. It seems to me that Professor Morris' project concerning the various restrictions on sports activities could easily lead to the formulation of legislation, which is another area where social sciences can make contributions. And there are such questions as *when* to bring a case. This is a very important matter in antitrust and other areas. The social sciences can help in deciding which sorts of cases are worth pursuing frequently and which are usually best forgotten.

Not only is law a subject with many facets, but, of course, the social sciences are numerous. A pretty clear distinction was made between economics and the others. This seems to happen in all such conferences since the role of economics is clearer. But all of the disciplines have something to contribute to this area of our common interest.

As for how large a fraction of the total law enterprise social science-related work will occupy, I will give my opinion that it is going to be significant, but social sciences will by no means dominate, change, transform, or to use another word, "threaten" the existing law enterprise. For the most part, the law schools will continue to do what they have been doing; but I think most of the major ones will have some stake in the kind of work that we have been discussing at this conference. Of course, how much the social sciences will "invade the law area" depends not only on the acceptability to the law profession of social science work but also—we ought to look at the other side of it—on the quality of the social sciences themselves. Let me say on behalf of the social science community that we too recognize this as something that has to be taken into account. Social sciences do indeed have a great deal to contribute now, but they are by no means as scientifically developed as they need to be.

That leads to another interesting question, which has also been alluded to here, though this conference was perhaps not adequately staffed to discuss it. That is the effect of law-related studies on the social sciences. In addition to watching the sociologists and econom-

ists "climb into the law building," we might also watch them when they come back out. Will the social scientists be changed, will the social sciences be changed, as a result of these experiences? I hope so, and I believe so, because the law, after all, has in its cases, in the experiences of law practice, and elsewhere a tremendous body of information about how human beings behave. While this knowledge is not usually formulated in scientific terms, information is there and can be used for scientific purposes. For example, I know from a bit of personal experience a long time ago that economics has benefited considerably from the interplay of law and economics in relation to antitrust problems. I believe there is good reason to expect other parts of economics and other social sciences can benefit similarly.

In conclusion I shall briefly discuss the National Science Foundation's programs as they concern law and the social sciences although most conference members are already well acquainted with the NSF. I think that the view of the Foundation about law-related research is an optimistic one, although I hope it is also sober, as our discussions have been.

Progress in this area takes patience and persistence. The NSF has taken a long time to get to where it is today, and its program is still relatively modest. Some of its early grants in this general area were made not to lawyers but to sociologists and other social scientists. Meanwhile strong efforts were being made principally by the Russell Sage Foundation, the Walter Meyer Foundation, and some other organizations to encourage research in law and the social sciences especially in law schools.

The NSF started planning whether to have a special program in law and social sciences about four or five years ago. A conference was undertaken for the Foundation by the Association of American Law Schools.⁵⁰ The participants gave advice about what would be appropriate for the NSF to do in this area. About the same time the National Science Foundation Act was amended.⁵¹ There was general interest in expanding the support of legal studies at the Foundation. In fact, in the report accompanying that bill, it was stated that the congressional committee understood that law was included in the social

50. Mr. Michael Cardozo, who at that time was its executive officer, and Professor Alfred Conard of Michigan, who was President of the Association, along with his colleague, Professor Roger Cramton, planned the sessions.

51. [Ed.] Act of July 18, 1968, Pub. L. No. 90-407, 82 Stat. 360 (codified at 42 U.S.C. § 1862(1) (1970)).

sciences. (Sometimes when people ask me if law is a social science, I refer them to that statement; it is convincing evidence to lawyers.) Soon afterward the NSF followed up with support of a portion of the meeting of the Association of the American Law Schools in San Francisco. It helped the Association to bring some non-lawyers to that meeting to discuss this question further. Finally, about three years ago the NSF felt ready to set up a law and social sciences program. So we at the NSF have been working on this program steadily for a long time, and we are going to persevere.

The level of funding through the Division of Social Sciences is about \$900,000 a year at present. I suppose this level could be increased a good deal if we could display "the fruits of the work" to justify our request for further funds to the representatives of the taxpayers, who provide the funds.

In addition to funds from the Division of Social Sciences, other funds are available through the Research Applications Directorate. In the past, quite important funds were available through our Education Directorate, which funded the "SSMILE" (Social Science Methods in Legal Education) operations and also the curriculum development activities at Northwestern, Wisconsin, and perhaps elsewhere. Unfortunately, funds for that purpose are no longer abundant, but the outlook for research support is still quite attractive.

Quick results will help a researcher to obtain more funds for his work because the best argument for putting money into research I have ever heard is that the money put in last year, and the years before, produced significant results. There isn't much you can say about the value of really genuine imaginative research before it is done. You hope that it is going to come out favorably, but you can't know in advance. Consequently you must extrapolate from the experiences of the past. Therefore funding agencies, private or public, in order to satisfy those who provide them with the funds they allocate, have to call upon grant recipients for news of results—if the results are "quick," it will be so much the easier to fund the next round of projects. I fully appreciate the burdens that our requests place upon the investigators. Professor Levy has accurately described that side of the coin. However, the side I have pointed to is also real.

Nevertheless despite the advantages of quick returns, we at the NSF are going to persist in supporting inquiries that can only have long-term payoffs. We believe the country—indeed, all mankind—will gain from the findings of a small but significant number of scien-

tific studies that can only pay out over a long time span. The Foundation is firmly committed to devoting a large part of its resources to carry out its distinctive role as the sponsor of fundamental research that is speculative and may take a long time to conduct, but which will, when successful, have value for many, many years to many, many people.

B. Discussion

PROFESSOR WALKER: Thank you very much, Dr. Hines. Let me now invite comments, and observations.

PROFESSOR LEVY: I would like to address myself to comments made by Marc [Galanter] and Professor Friedman about the marginal qualities, marginal effects, of empirical research in the law schools and the possibility of moving such research outside the law schools because of its current and future marginal status.

If you stop to think about what lawyers and law professors do, there is an enormous amount of empirical work within their intellectual horizons. They count cases; they count trends; they count plaintiffs. Indeed, even the geniuses in their cubicles want to know what's going on in the real world, although it is very true that they don't often want to *count* what is going on in the real world in the way that we "Minnesota Twins" have tried to do. I also think that the "new lenses" idea that Marc Galanter is talking about is part of the legal intellectual tradition. The kinds of traditional law review articles that most law professors tend to rate most highly are the efforts that look at old institutions in completely new ways. (I think immediately of Brainard Currie's work,⁵² and I think of Jaffe's famous article on administrative agencies.)⁵³ The thing that Marc Galanter is talking about is in that tradition, though perhaps more modestly—and so I believe that it would be a very bad mistake to try to take middle-range empirical inquiry, or the kind of inquiry that Marc is talking about, out of the law school because of frustration about its marginal qualities.

One other point that I would like to make is not completely related. It is becoming increasingly evident, at least to me, that in the effort to get facts about the law in action, we have concentrated entirely too much on what courts do and what administrators do and not enough on what lawyers do when they are not in court. In our custody

52. See B. CURRIE, *SELECTED ESSAYS ON THE CONFLICTS OF LAWS* (1963).

53. Jaffe, *The Effective Limits of the Administrative Process: A Reevaluation*, 67 HARV. L. REV. 1105 (1954).

study it has become quite clear to me that for sociological as well as legal reasons we have to find out how lawyers are advising clients and how lawyers conceptualize the problems. Because not only are lawyers conceptually structuring what the courts will eventually do when the courts get the problems, but the lawyers in their offices are establishing and maintaining norms of conduct which are the law in action in a much more real sense than the law in action that we normally refer to. I hope that we will have much more research of that kind even though I believe that is even more difficult than the traditional research of the law in action that Marc referred to.

PROFESSOR PROBERT: I would like to make an appeal tied most closely to the remarks of Marc Galanter, but spurred by Larry Friedman's derogation of language analysis. It is an appeal for what has been called a "New Empiricism." It involves a way of looking at legal phenomena which takes account of behavioral variables most in favor with, say, the sociologists and yet also takes account of the slighted dimension which Marc alludes to, the normative. There needs to be greater concern in the law, of all places, with language behavior, not just language, but language behavior.

The social sciences generally have been deficient in taking account of language behavior. I am not speaking of so-called ordinary language analysis, which I think is perhaps too limited yet, although even that is a development which is not merely vogueish but undoubtedly a response to a felt need. Ordinary language analysis does have the virtue, even though it may not go far enough, of looking at a phenomenon in the legal process which is somewhere between the purely normative and the purely behavioral. It is at least looking at language behavior in some beginning way.

I think this need is best tied to your appeal, Marc, for a kind of reconstruction of theory. I think that in order to engage in my reconstruction of theory, it is necessary to look at this linguistic or communicative phenomenon. It might indeed be worthwhile to ask if there is actually a reconstruction taking place. Now we don't readily see that. I think a shift is taking place as a matter of fact, but we don't see that unless we look, not through language, but at it and through it, to see how people are talking and to take account of that, at least as a part of what is interpreted.

I think we can say, for instance, that legal realism had a fantastic impact on the way legal professionals think and talk law among themselves. The way we talk is itself data. Ultimately the impact is ap-

parent in published material. It might be interesting, then, just for instance, to look at the shifts in ways of talking about law. Certainly in law reviews the way people talk has changed; and I think that the ways of talking in courts have changed.

Now whether the pendulum will swing back in the current era is another interesting question. But I think that these two things are tied together, the idea of a New Empiricism and the idea of a reconstruction of theory. The latter involves in Marc's view a reinterpretation of what has already taken place. The New Empiricism is a little different, involving additionally a significant shift in ways of looking at observables.

MS. FULTON: I want to respond to the statement that the research that is done should be of rather immediate public utility. I appreciate that you, Professor Raiser, added the clause that utility is a vague concept that needs to be interpreted in each case individually. I would hope, however, that the determination of "utility" would be made from an open perspective rather than from a narrowly restrictive viewpoint. There are potential applications for all kinds of social science research and legal social science research findings, and lawyers must be receptive to these. They should be receptive to general information about society, about societal needs, about societal behavior, and be open enough to interpret research findings as having potential utility even when the findings are not of immediate—or even apparent—consequence. For instance (to give a weak example because on the spur of the moment I can't come up with anything more meaningful), we are coming up with data which show that the peak of trauma for the individuals who are undergoing a break-up of their marriage is at the same time that they make their first contact with the lawyer. This finding is contrary to the general assumption that the period following the final decree is the lowest point. What is the public utility of such a finding? How does, or should, this information influence the lawyer-client relationship or the legal divorce procedures? I have never been able to determine to my own satisfaction the direction of influence between law and society. I do not feel, however, that it is a one-directional kind of influence: society influences the law, which in turn molds and changes society. The awareness of this interrelationship should be the responsibility of both lawyers and social scientists.

DR. HUSZAGH: I want to address myself very briefly to what I heard was a call for centers of excellence. I feel it is naive to think that those who are capable of interacting at this higher level will all

optimize "at the same sunny spot," and in the same institutional setting. More importantly, it is naive to think that we can identify those people. There will be no end of self-identification, but the real problem is finding an objective standard. To me, it is much more realistic to feel that helping those people to connect with other people who at that same moment in time happen to be passing through the same intellectual environment, and facilitating their continued communication as long as it is fruitful, is a much better way of approaching the problem of enabling researchers to interact. It requires a lot of sampling and resampling of the law school world. But it will facilitate a much higher level of interchange between doctrinal researchers and the other side because more feed-back is made possible. And I personally am very hopeful that some people who might be typed as very ordinary will have a moment in which they can make a very substantial contribution in part or in whole to some of the conceptual data.

DR. RADLOFF: I was very pleased to hear Professor Friedman talk about the *Zeitgeist* possibility of psychology. I am probably not so sanguine about it as he is but, nevertheless, it is good to hear it coming from a legal scholar. I think psychologists do exist who might be interested. They probably are a little hesitant to approach lawyers. I am trying to encourage them to approach schools of law, medicine, education, and business in order to expand the subject matter. And I certainly appreciate any help from the other disciplines.

Also I think that there is a factor in the *Zeitgeist* which may contribute to this expansion as well. After all, law students come out of *some* background, and in recent years, there has been an explosive growth in the number of psychology majors. The proportion of psychology majors in many universities is above fifty percent. While many of these are probably people in identity crises who are trying to find out about themselves, nevertheless, some do survive Pavlov's dog, Skinner's pigeons, and the physiology of vision to become majors. It is probably a mistake to concentrate on the attitudes of the average law student because he is not the one who is going to change the school anyway. It is going to be the best and the brightest who are going to have an effect on the law school. They are going to be better trained. Some of them may even come in with advanced degrees or take non law school courses at the same time. This is going to be a cadre that we can look for to make the change, rather

than the average law student. Probably the upper five percent will be reached.

PROFESSOR CAVERS: There is a good deal of evidence that, in a gradual way, the opportunities for the type of research that we have been discussing have been broadening and have been finding more hospitable quarters in the law schools than was true not too many years ago. One reason is that legal education itself, an institution that is seemingly solidified with only peripheral changes in its structure, is loosening up in a degree that to my perspective seems startling. The alteration has not been radical. But to the extent to which we are able to accommodate the kinds of studies that we have been discussing here (particularly if those studies can be defined with the breadth that Professor Zeisel suggested), the prospects for continued enlargement of activity in the law schools are reasonably good.

One thing that has contributed to my optimism is the recognition that many of the conceptual contributions that we all agree are needed can be made without too much fresh empirical work. To Lawrence Friedman's list of areas that are opening up one addition might be made. There seems to be a great deal of current interest in decision-making and decision theory. I am not sure that that comes under the psychology tent—I live in dread that before I completely retire, mathematics will rear its ugly head in legal research. So far we have managed to keep reasonably away from it; but I don't see how this will last forever.

One matter that has very seldom come before this forum is the fact that we do have in our law teachers, regardless of their fields of concern, a very high degree of preoccupation with the processes of teaching. And one of the concerns this suggests is whether the experience that Julie Fulton reported of her efforts to introduce some empirical data into her family law class is going to remain characteristic of the reception law students and teachers give to developments in this area. Are we going to find a way of making a better link between the teaching process and the research process than we have thus far? This is one of the questions that we properly haven't spent much time on, but success in that regard would have a very real impact on law teacher's readiness to pursue various modes of empirical study or social science research. It is something that is not wholly beyond the bounds of impossibility. It has been achieved in the area of antitrust, a relatively simpler case than most, but I am sure that there are other directions in which we will see this happen.

PROFESSOR ZEISEL: I don't want to leave this meeting without saying that I really believe that the topic, what is the relationship of the law to the social sciences, is a wrong dichotomy.

If one reads what lawyers say in their articles, in their judgments on the bench, in their writing, one finds them talking about facts—about social facts. But they have so little data. They make assertions, and they make guesses; they make educated guesses. Fifty years ago what the judges thought about reality was at least as good as what sociologists thought about reality because the sociologists hadn't any data at that time either. And so what is really at issue is that these educated guesses and the hunches in the absence of data become a little bit more precise. What is at stake here is a change in the culture of the law, *Zeitgeist*, or the blood stream, or whatever one may call it. For this reason I don't share Larry Friedman's view of the social scientist in the law school being the "rug on the floor," the outcast, the sad colleague, or the comparative lawyer. In one way, these characterizations are true. But in another way, they are quite wrong because, as I have tried to point out, the essential importance of the presence of somebody who looks at social facts in a law school is that his colleagues don't find it as easy any more to make guesses.

I just want to give you one thought which isn't mine, but Edward Levi's,⁵⁴ who really in one way started the renaissance of social research in the law. Whenever people talked about social research and the law, he said, "Why do you say empirical research *and* the law? This *is* research *in* the law." He refused to make the distinction. You see, there is one thing that distinguishes lawyers, I think, from all other people in the academia: if they are presented with a piece of evidence, they have an inspiring way of going to the jugular to see if it is really so and if it is really proved. And this brings up something that we haven't discussed at all, namely, that there is a great amount of bad social science research in the law, which sets us back because the reserved attitudes of our colleagues toward social science get ammunition by such research. It really doesn't matter where we do it or how we do it as long as we go on producing good research.

I think that the future is taking care of itself; and maybe it might be a useful thing to take, let's say, the year 1920 of the *Harvard Law Review*, and the *Chicago Law Review*, and the "*Berkeley*" *Law Re-*

54. [Ed.] Edward Hirsch Levi, President of the University of Chicago and former dean of the University of Chicago Law School.

view, and compare them with the 1970 reviews, and see what difference there is in legal arguing, and see whether the change of the culture we are aiming at isn't already underway.

And since this is the end of our conference, I think that, at least for myself, I would say a word of appreciation for this nice conference, our lovely host, and the many who made it possible.

PROFESSOR WALKER: Thank you very much.

