

BOOK REVIEW

Procedural Justice: A Psychological Analysis. JOHN THIBAUT & LAURENS WALKER. Lawrence Erlbaum Associates, Publishers, Hillsdale, New Jersey, 1975: Pp. viii, 150.

This book is a problem to review.¹ It promises more than it delivers, but it delivers enough to justify its existence. The authors are John Thibaut, a social psychologist on the faculty of the University of North Carolina, and Laurens Walker, a professor in the Law School at that same University. They claim in the first line of the preface that the work is "fully interdisciplinary." I am not sure I know what that means, though I remember reading in various places that it is a virtue. Actually, the book strikes me as being mostly a social science product. The role of the lawyer *qua* lawyer in such an endeavor seems basically preliminary, identifying areas of controversy which might not be apparent to the social scientist untrained or inexperienced in the law, and injecting the laboratory simulations with realism. The experiment design, the statistical analysis of the results, and writing up of the results in a standard and professional way is the domain of the social scientist. Hence, it is not surprising that most of the language in a book, which deals basically with nine social science experiments, appears to be the product of a social scientist. This is not to denigrate Professor Walker's role in the endeavor. The preliminaries are of vital importance. Further, insofar as Professor Walker is an interested intelligent human being, I have no doubt he was capable of playing the functional role of a social scientist to the benefit of the project in other areas. Finally, I am sure that both authors did their share of the large amount of donkey work which must be performed to carry any set of experiments from design to fruition.

Social scientists obtain the information from which they construct theories about human beings from many sources. Like the rest of us, they too are human beings, and they live with human beings. They have themselves to observe and it is inevitable that they will test any

¹ This book was suggested as a possible program topic for the 1976 annual meeting of the Civil Procedure Section of the Association of American Law Schools. I read it first in March of 1976 and wrote substantially all of this review at that time. Another topic was ultimately selected for the Civil Procedure Section meeting and I put away the review, feeling that time would soften my criticisms. In July of this year I was preparing footnotes to an article which, for one reason or another, reminded me of this book and the review. Time, it turned out, did not change my opinions. I offer the review now as a comment not only on this book, but also on some of the problems of applying social science methods to legal concepts generally.

general proposition against themselves to see if it rings true. Of course, they must be aware that they, as individuals, may be significantly different than the run of human kind. Still, they have lived every day of their lives with others and have accumulated the wealth of information which comes from life itself. This they use to judge the contours of their own representativeness. Furthermore, such information will have bearing on their judgment of the likelihood of any given theoretical proposition about humans in general. The dependability of such judgments from the clinic of life hinges upon the richness of the experience of the judge, and the care and sophistication with which that experience is evaluated. To this pool of human information we must add the hearsay we all daily receive, evaluate and utilize, ranging from rumors and anecdotes of others, to all the printed information which comes to us through sources of such varying dependability as the *National Enquirer*, and the *Encyclopedia Britannica*. These sources bring us the bulk of what we know or think we know about the human past, be it yesterday or 1000 years ago.

In the sense that we all have access to such information and utilize it to create some general propositions about humans in which we invest reality, we are all social scientists, though some of us are not very good at it. The more careful we have been trained to be, logically and epistemologically, in handling information generally, the better we will be. At this clinical level some lay persons may be much more incisive than some professional social scientists. But we must realize that, no matter how broad our experience, our personal data is more or less skewed. We have each grown up in a limited surrounding and known a limited set of people. Knowledge of this must make our clinical theories of humanity tentative, though the broader our human experience and the more qualified the proposition under consideration the more we may feel justified in having some confidence of judgment on our clinical experience of life alone. The social sciences as organized disciplines depart from this armchair level by attempting to design ways to get information about human beings which are as free from skew as possible, and about which the potentials of unrepresentativeness are isolated and described to the greatest extent possible.

This is a formidable task. Human beings as individuals and as groups are mind-bogglingly complex, and the presence of the observer creates artifact problems on a Heisenberg principle analogy of monstrous proportions. Still, it is a worthwhile endeavor; the goal is that we may more objectively understand ourselves and correct for the skews of our own experience with insights from better sampled information.

Basically, there have been two major types of social scientists worthy of the name in the last fifty years, describers and experimenters. Describers have sought to organize their own observations of human beings in such a way as to set down as factually as possible complete descriptions of human behavior which they have observed. Field anthropologists and sociologists are good examples. They may rely on teams of observers, but then care is taken to standardize the things to be seen. They may have to rely on the subjects of observation for information on facts or attitudes, but then care is taken to get information from multiple sources and to design the questions to check for internal consistency. Their goal, however, is the description of the human condition as they found it.

The experimenters take human subjects and manipulate them in a controlled way to observe their responses. The experimenters' goal is the identification of common mind patterns that will produce predictable and reproducible results.

To a certain extent, the experimental area is where the intellectual action is today. The pioneering days of gross observation are over, and the embracing of the experimental method puts the social scientist's procedures more recognizably in congruence with those of the natural scientists, a position social scientists have coveted for its perhaps misplaced aura of the high priesthood of immutable truth. There are, however, costs in becoming an experimenter. If the epistemological problems of the social sciences in general are formidable, those of the experimenters are overwhelming. In any human experiment, the complexity of the subject makes isolating variables difficult. Moral and legal restrictions on experimental method make it worse. Basically, all experimental subjects must be volunteers, a factor which becomes impossible to factor out. The largest and most convenient group who will volunteer in significant numbers (for cheap enough compensation) are students, another variable. The design of an experiment with proper controls isolating variables to any acceptable degree often produces an experiment which can only test the most miniscule factor of all the variety of factors which affect human behavior, and deciding what weight it should be taken as having in real world situations among untested variables is impossible to do with any precision. No wonder the results of such experiments are so often tagged with the weak adjective "suggestive." That is the best in most circumstances they can be.

Actually, that should be enough. Driving one empirical nail into the lower siding of the edifice of social science knowledge is an honorable task, even if it leaves other corners of the board flapping. The whole edifice, like the edifice of the natural sciences, will take cen-

turies to near completion, and necessary moral restrictions on human experimentation may mandate that some parts of the facade be left unbuilt. The problem is that everyone wants to believe that what they have just discovered is not a missing nail, but the missing link. People's mental well-being, professional reputation and continued employment often depend on convincing themselves and the world that their nail is at least a golden spike. This tendency is compounded by the existence of occasional sets of experimental data which do have major implications, and do seem to strongly support whole important theories.

Many people have the good sense to see fruitful areas of inquiry, only to find that the question they have asked is a composite of many questions, each being a momentous research task and no one appearing to be of overwhelming general significance. The options then are to pick one question and concentrate on it, to try and answer the whole jumble properly, delaying final synthesis for thirty or so years, or to construct experiments which nibble first at one question, then another, filling in the gaps with speculation. It seems to me that the latter is what the book under review reflects. This approach, however, is a difficult horse to ride. The intermingling of speculation seemingly extrapolated from thin experiments may make the whole endeavor seem like an attempt to palm off the speculation as the product of a more rigorous empiric than the clinical experience of life which is the basis of all social speculation.

There is thus a great temptation to write one's experimental report in cosmic terms, indicating that the experiments were prompted by overwhelmingly important questions of social policy. If one can use a lot of normatively loaded words, thus implying the possibility of an empirical demonstration of the good, so much the better.

The book under review concerns nine experiments which are generally fine, as far as they go. If one were to be grudging, one could say that they suggest that students who will volunteer to participate in social science experiments at a state university in the southern United States for money will at least say that they prefer dispute resolution procedures where control of those procedures is assigned to a very great extent to adversaries in front of a judge figure.² If one is less grudging, one could say that the results indicate that Americans like the adversary system. One is inclined to be less grudging because the suggestions emanating from the results merely

² The authors acknowledge the weaknesses inherent in their methodology. J. THIBAUT & L. WALKER, *PROCEDURAL JUSTICE: A PSYCHOLOGICAL ANALYSIS* 4-5 (1975). Unfortunately, this acknowledgement did not make them cautious in framing their conclusions.

tend to confirm what the clinical judgment of most lawyers would have been without them. This is in no way, I repeat, no way to denigrate the value of the experiments. To provide experimental support for armchair wisdom (which is occasionally shown thereby to be wrong) is a praiseworthy enterprise. These experiments provide competently fashioned, suggestive, nails.³

Yet, it is both an overstatement and a misstatement to imply in the title that these experiments provide even suggestions about the whole topic of procedural justice, a conceit carried forward in egregious fashion in the preface and at various points throughout the book. Consider the following excerpt:

It seems clear that the quality of future human life is likely to be importantly determined by the effectiveness with which disputes can be managed, moderated, or resolved. Procedures or methods that may be put to this task of conflict resolution therefore claim our attention.

We are not concerned with all such possible methods. Aggression, revolution, warfare, and other violent methods of conflict resolution are not discussed in these pages. The procedures we have chosen to study are peaceful—at least in their most characteristic forms.

... Among these institutional modes of conflict resolution the legal process is a salient choice for attention because the courts and similar agencies constitute the principal formal device for the large-scale solution of social disputes. It follows that the procedures of the courts have much potential for creating widespread justice or injustice. However, what procedures are just?

We suggest that the just procedure for resolving the types of conflict that result in litigation is a procedure that entrusts much control over the process to the disputants themselves and relatively little control to the decision maker. There are many correlated and subsidiary elements of procedural justice, but the key requirement for procedural justice is this optimal distribution of control. Our proposal that in formal litigation control over dispute resolution ought to be largely in the hands of the disputants is likely to provoke considerable skepticism. We recognize that the allocation of so much power to self-interested litigants runs contrary to much current thinking, especially among scholars concerned with legal process. Nevertheless, this is the ultimate conclusion we draw from the research reported in this book. We announce this at the outset to encourage the reader to test this statement against the developing evidence.

³ But see Damaska, *Presentation of Evidence*, 123 U. PA. L. REV. 1083 (1975).

More generally, we propose that distribution of control constitutes the basic variable or dimension for analyzing, comparing, and assessing the justice of all forms of dispute resolution, legal and nonlegal.

It is indeed surprising that to date procedural justice has not been the subject of much, if any, social science research.

. . . There have been some noteworthy empirical studies about procedure carried out by legal scholars, as, for example, the jury research of Kalven and Zeisel (1966) and the examination of the pretrial conference done by Rosenberg (1964a), but these and similar investigations have only examined the functioning of particular parts of systems. They do not propose any general criteria for assessment and comparison of procedural systems and, of course, do not propose any general conclusions about systems. Procedural justice has therefore been overlooked by social scientists, perhaps because they have been unaware of its importance, and legal scholars have managed only *ad hoc* investigations, perhaps because they have lacked the integrative and analytic concepts.⁴

The authors have decided that how control of conflict resolution procedures is distributed among contending parties as individuals, and between them as a group and any neutral party which might be involved, is an important thing to examine. No quibble there. That distribution might affect the accuracy of the process in fact reconstruction, if that is one of the functions of the process under consideration. It might affect the perceptions of the parties concerning the fairness of the procedure, and this might be useful to know from a purely utilitarian perspective if one is interested in getting parties to resort to a process and to accept its results freely. But the authors have gone further and equated the question of distribution of control with "procedural justice." This reveals a willingness to tie value loaded words to the empirical process in an inappropriate way, and a willingness to make too much of the thin reed of their experimental results. The general proposition the authors offer as both their hypothesis and their conclusion will not stand scrutiny.

What people prefer, or say they regard as fair, may have something to do with procedural justice, but the connection is neither obvious nor within the scope of empirical demonstration. I personally think the basic rationality of any fact reconstruction process is more central, and readers may supply other factors of their own. But it is important to realize that the social sciences may, at the outside, tell

⁴ J. THIBAUT & L. WALKER, *supra* note 2, at 1-3.

us what is, and perhaps what may be, but they cannot as sciences tell us what ought to be. That does not mean that people who also happen to be social scientists may not put forth valuable insights about justice, but merely that when they provide the normative component of those insights they act in the capacity of philosophers, not scientists, and must be careful to expose rather than to obscure the dual role they are playing. So far this review sounds largely negative, not out of any hostility to the application of social science methods to legal questions to which they are appropriate, but as a caveat. Practitioners of the social science approach face a community of tough critics in the legal world. Many lawyers and legal academics have training in the social sciences. Further, and perhaps more importantly, they are, on the whole, a community of skeptics trained to ferret out overstatement. Finally, they are a community of fairly broad experience in the clinic of life in general and life in the law in particular. They will seize on puffery with a vengeance. My impression of the reputation of the social sciences in these circles is that they are rather generally suspected of habitual puffery. It is not that their observations and experiments are thought inaccurate a priori, or that their speculations either as to nature of reality or proper values are necessarily more suspect in and of themselves than those of other mortals. It is that they are suspected of a convenient and easy overblowing of their hard data to support their pet speculations, with a convenient myopia to the distinction between propositions of fact and propositions of value in the bargain. Only the utmost scrupulousness in these regards will gain the efforts of the social sciences in this area the respect to which they are properly due.

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