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Book Reviews

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operators, was unconstitutional, but the court found that the class defined was a reasonable one, not violating the equal protection of the laws.

The next attack on the Board's refusal to grant a license complained that failure to pass an examination was not one of the grounds specifically set out for denying a license, but the court held that since the statute provided that "a license may be refused if the applicant demonstrates unworthiness or incompetence" this ground was broad enough to reach a failure to pass because a "study of the subject act clearly impels the motive of the legislature as being to require a minimum standard of efficiency in polygraph examiners".

Appellant's last contention was that the Board was not legally constituted to prescribe and grade polygraph examinations for the reason that members of the Board had not passed the examination to determine their competency. The court held however, that if the legislature had intended that Board members themselves had to take and pass

examinations prescribed by the Board, the Board could never come into existence and the act would be a nullity.

Paraffin Tests Are Not Equivalent To Testimonial Compulsion—*People v. Simpson*, 146 N.W.2d 828 (Mich. 1966). The defendants were convicted of breaking and entering with intent to commit larceny. Among their grounds on appeal was the contention that taking of paraffin tests amounted to testimonial compulsion in violation of the fifth amendment. The Michigan Supreme Court disagreed stating:

We cannot subscribe to this view. The paraffin test to detect the presence of nitrate on the hands is a recognized process requiring no invasion of the body or its substances and is not in *pari causa* with stomach-pumping. *Rochin v. California*, 342 U.S. 165 (1952). The process more nearly equates with fingerprinting or photographing, both sanctioned procedures.

BOOK REVIEWS

Edited by
C. R. Jeffery

THE AMERICAN JURY. By *Harry Kalven, Jr.*, and *Hans Zeisel*. Boston: Little, Brown and Company. 1966. Pp. 559. \$15.00.*

Many who rely on the title of this study will find it misleading. Contrary to its title's implication, this first major product of the University of Chicago Jury Project is not a full-scale examination into the American jury system at all, but a survey restricted to criminal jury trials.¹ Moreover, even those who shake off the feeling that they have been promised more than is delivered may feel that the actual delivery is of something less than they had expected from so elaborate and highly publicized a

project. It is inevitable that experienced criminal trial lawyers will consider the book anticlimactic, and even those for whom experience has not blunted reaction may feel that *The American Jury* is an exercise in expanding molehills into mountains. Such responses will be only partly justified. This study has intrinsic importance and it is an exciting harbinger of things to come. Surprisingly enough, it is also an almost unmitigated delight to read because, serious as their purposes are, the authors have given some rein to their well-tuned senses of humor. They thus are careful to remark that their sample of criminal jury business "ranges . . . from first degree murder all the way down to a prosecution for the illegal keeping of a mud turtle."²

* This review is a modification of one by the same reviewer which appeared in the March-April, 1967 issue of the *Northwestern University Law Review*.

¹ The authors plan to publish a later study of the civil jury in about one year.

ZEISEL, KALVEN & BUCHHOLZ, *DELAY IN THE COURT* (1959), contained some data generated by the Jury Project. At pp. 541-45 of *THE AMERICAN JURY* is a complete bibliography of the numerous articles setting out Jury Project findings.

² Pp. 66-67. The puckishness of the authors is also reflected in the fact that the reader will find buried in this weighty work two thumbnail movie reviews: "Twelve Angry Men"—"brilliantly realized," p. 151. "Divorce, Italian Style"—"lighthearted," p. 233. The generally lucid and engaging quality of the authors'

The American Jury is a statistical study and the chosen universe is criminal jury trials in the United States in the mid-fifties. It was not the purpose of Kalven and Zeisel to decide whether the criminal jury is a good institution, although their study tends rather convincingly to demonstrate that it is far from being an altogether bad one. The authors appreciate that most misgivings about the jury are stirred by its use in civil cases and that, like it or not, the criminal jury is here to stay. Their ultimate purpose was to investigate, in scientifically valid fashion, the way criminal juries actually perform in the decision-making process. To accomplish this the authors decided to measure jury-performance against judge-performance, asking how frequently jury and judge disagree on the outcome of criminal cases and why. This choice of comparison is more than merely sensible; no standard of perfect justice exists. As their raw data Kalven and Zeisel had what they call "a massive sample of actual criminal jury trials."³ To be more specific, they had a total sample of 3,576 cases. This sample probably qualifies, comparatively, as "massive" if a characterizing label is necessary, since nothing on a like scale has ever been done, and the authors do not suppress the fact that as far back as 1955 this country apparently tried to a verdict about 60,000 criminal jury cases in one year.⁴ Data was assembled from the answers of 555 co-operating judges⁵ to two somewhat different sets of questionnaires; the first set covered 2,395 trials conducted in 1954 and 1955, and the second set involved 1,191 trials had in 1958. Fifty percent of the total case-sample came from only 15 percent of the responding judges.

The authors of this study know that the goal of sampling is a random cross-section of the relevant universe—for them, American criminal jury trials. They know also, of course, that this is not what they got: "For a number of reasons . . . we were unable to utilize the preferred sampling techniques in this study."⁶ What they got was a sort of "Kinsey sample" supplied by volunteers, and they are frank to say that this characterization damns with faint praise. The researchers, realizing the potential

prose style outweighs their repetition of such favorite terms as "bland," "heroic," "rich" and "*gestalt*."

³ P. 10.

⁴ P. 12.

⁵ The authors solicited the co-operation of some 3,500 judges on a list that included all United States District Court judges and all state judges of courts possessing criminal jurisdiction; only 555 co-operated "effectively." Pp. 35-36.

⁶ P. 35.

for poor focus inherent in any self-selected sample, were at pains to test and double-test the spread and validity of the questionnaire responses they obtained. Their defensive arguments march pretty well in most instances. For example, they demonstrate quite effectively that regional bias did not significantly warp their research pattern⁷ and that their "co-operative" judges probably had no greater propensity either to agree or disagree with their juries than would those judges who failed to respond to the authors' mailings.⁸

I am not knowledgeable, let alone expert, in the field of sampling techniques. It therefore is with timidity that I inquire whether Kalven's and Zeisel's use of two unidentical questionnaires is not a defect in technique that compounds the problem posed by the smallish size and spread of their sample. The authors nowhere allay my fears; they unsettle me all the more by announcing that "[W]e had the rare chance of designing a second questionnaire after long experience with the first one."⁹ This sounds very much like brave whistling in the dark. It does not mean, of course, that the first questionnaire was worthless, any more than it implies that the second one was perfect. But it does mean that, where a question in the second questionnaire had no true counterpart in the first, the sample is reduced to some unit contained within 1,191 rather than 3,576. In one instance where the "desperately small" sample produced what the authors concede is "weak proof," the sample was in this way reduced to 10 cases.¹⁰ One cannot help wondering, too, whether the authors, in their zeal to mine the last nugget from a fairly small vein, do not divide and subdivide their sample to and perhaps beyond the point of diminishing returns. Being unsure of my competence to answer these questions, I leave them to others and pass to what will surely be the most commonly voiced reservation about this study's worth: Does it tell anyone anything he did not already know?

⁷ Pp. 40-42.

⁸ Pp. 42-44.

⁹ P. 47. Elsewhere on the same page the authors point out that the initial questionnaire "was a relatively open, unstructured effort," while the second one, "building . . . on experience with the first," was "more tightly controlled and more detailed." *Ibid.* It is admirable to improve on the basis of experience. The trouble here, if I am right, is that the authors really ended up with two different samples—different in size and time, different from the standpoint of the questions posed—and yet, understandably unwilling to abandon the product of their first effort and thereby reduce the size of their sample, they frequently lumped together what sometimes were apples and oranges.

¹⁰ Pp. 277-78.

The heart and core of Kalven's and Zeisel's study is in its finding that judge and jury agreed as to the outcome in 75.4 percent of the sample cases¹¹ and that jury disagreement with the judge lay on the side of leniency.¹² The balance of *The American Jury* is devoted to detailed assessments of the reasons for judge-jury disagreements. Having told Kalven and Zeisel the facts of each sample case and whether he agreed or disagreed with the disposition reached in each by the jury, the sample-judge then described what he felt were the reasons prompting the jury's action. Kalven and Zeisel, who have great faith in judges' ability to know what motivates jurors, classify these narrations. They also set up matrices involving myriad factors in an effort to measure their contribution to judge-jury disagreements.

Five main disagreement-producing factors are considered: evidence factors; the impact of facts known only to the judge; disparity of counsel; jury sentiments about the particular defendant; jury sentiments about the applicable law. These principal factors are broken down into subfactors. For example, the large sub-factors under the heading "Sentiments About the Individual Defendant" are personal characteristics; social status; family; appearance in court; occupational record; and special circumstances. Each sub-factor is further compartmentalized: "Personal Characteristics of Defendant" subsumes youth, old age; woman; attractive woman; mother; war widow; cripple; ill health; sympathy in general. "Family" takes account, among other things, of "pregnant wife in court." "Court Appearance" includes "Crying, collapsed." Two "Special Circumstances" are "Kind to victim" and "In no position to repeat crime."

Those who suggest that Kalven and Zeisel have by this process occasionally subdivided their sample to death are probably right. But there are other critics, already vocal, who are not altogether right. Those who neither read this book with care nor fully grasp its manifold purposes will smilingly declare—extremely and unfairly—that Kalven's and Zeisel's work proves nothing more than that a jury is likely to be lenient toward an accused who is an attractive but crippled and destitute war widow of good family who is charged with littering, especially if she has no previous criminal record, takes the stand, cries and collapses.¹³

It can be granted that the study demonstrates the force of obvious factors, but it also reveals some far less obvious ones.¹⁴ For some instances, this survey indicates persuasively, if it does not prove, that jurors have a sometimes surprising tendency to consider certain sorts of interdicted conduct *de minimis*,¹⁵ that in some cases they seem very much concerned with whether the victim was an adult or a minor;¹⁶ that they are not easily or often confused by the evidence and the arguments;¹⁷ and that when it comes to issues of credibility they are probably no more gullible than most judges.¹⁸

Some sections of *The American Jury* do not strain at erecting neat statistical proofs. The authors, fortunately, have included in this volume a substantial amount of data that is not in the direct line of their inquiry. While some of it approaches the trivial, much of it is both interesting and illuminating. Their data on the insanity defense is no more adequate a survey than would be any brief discussion of four trials¹⁹ but the material on intoxication is valuable.²⁰ The written law is against voluntary intoxication as a defense and I think it a fair generalization that defense lawyers have rarely considered it an impressive factor in serious cases unless strong psychiatric undertones were also present. Kalven's and Zeisel's response on the issue was, as they candidly put it, "narrow and subtle"²¹ and yet it sheds some light. In fact, their data suggest that "the jury is aware of the [voluntary intoxication] problem and is moved to leniency if the defendant was drunk."²² Experienced litigators might also be proved off-base were they asked, prior to a look at *The American Jury*, to give the class of sex offenses least likely to induce leniency in juries. We might anticipate a virtual consensus

distrust the validity of the Jury Project finding giving rise to the statement.

¹⁴ This stems in part from the fact that few, if any, lawyers have had experience with every sort of case analyzed in *The American Jury*. I have had fairly extensive criminal trial experience but I have never prosecuted or defended a person charged with the unlawful possession of a mud turtle—or, for that matter, mail theft, prostitution or violation of the Migratory Bird Act. Moreover, the Bar is not populated exclusively by experienced litigators. What may be obvious to the veteran trial lawyer is often not nearly so transparent to the tyro and herein lies one of the merits of this book.

¹⁵ Ch. 18.

¹⁶ Pp. 102, 245 n.7.

¹⁷ Ch. 11.

¹⁸ Ch. 13.

¹⁹ Pp. 329–34.

²⁰ Pp. 334–38.

²¹ P. 335.

²² *Ibid.*

¹¹ P. 56.

¹² P. 59.

¹³ One may distrust the statement "Oh, I've always known that!" fully as much as one may occasionally

on child molestation, but this study forcefully suggests that homosexual offenses are at the bottom of the leniency ladder.²³

A natural inquiry by lawyers in connection with any study of how jurors behave is whether disparity of counsel exerts important influence.²⁴ Kalven and Zeisel anticipated this important question and they have devoted a chapter to it.²⁵ Their conclusion is that the "superiority" of one side's legal representative is a minimal factor in judge-jury disagreements.²⁶ They also believe, on the basis of their questionnaire responses, that "in the vast majority of trials, counsel on both sides are evenly matched."²⁷ Regrettably, the foundation for these conclusions is observably shaky, in part because it straddles the authors' dual questionnaire. In their initial questionnaire the two pertinent questions were whether the prosecutor and the defense attorney were experienced trial lawyers. If the answer was "yes" as to one and "no" as to the other, "imbalance of counsel was inferred."²⁸ The propriety of this generalized inference, simplistically equating experience of undisclosed quantity and quality with "superiority," is seriously questionable. In the second questionnaire a more pointed question was put: "Was the case tried equally well on both sides?"²⁹ There are at least two difficulties inherent in the revised question. In the first place, the only important inquiry in a given case is whether the *jurors* thought one lawyer was significantly superior and

one cannot repose abiding faith in the coinciding of a judge's estimate with theirs. Furthermore, nothing in the instructions accompanying the questionnaire precluded a judge-respondent from assessing counsel after rather than before the verdict was in,³⁰ and so we cannot know whether a judge's view of counsel superiority was influenced by the unarguable fact of one side's success. The Jury Project's conclusions regarding the impact of counsel must, I am afraid, be heavily discounted.

What are we left with, then? We are left with a pioneering study that has produced much that is worthwhile. Perhaps its greatest importance resides in the circumstance that it may simply be a prelude to something much more important: the authors' forthcoming examination of the civil jury.

The fact that *The American Jury* is at once fascinating and imperfect should not be permitted to detract from its significance. In this day, when so much that is haphazard and biased passes as scholarship, one can only applaud genuine efforts to enlarge knowledge and understanding. Professors Kalven and Zeisel are embarked upon this demanding task.

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THE VARIETIES OF PSYCHEDELIC EXPERIENCE.

By R.E.L. Masters and Jean Houston. New York: Holt, Rinehart and Winston, Inc., 1966. Pp. 326. \$7.95.

The major premise of the authors is explicit: "We hope to make entirely credible our belief that the psychedelic drugs afford the best access yet to the contents and processes of the human mind." In the epilogue, summing up their arguments they themselves conclude that there can be no "justified doubt" that they have done what they set out to do, to prove that the drug experience "holds out the promise of rewards of incalculable value."

In the book one finds a description of those LSD, peyote, and other hallucinogenic drug effects observed by or reported to the authors in their work, the data base consisting of 206 drug sessions and interviews with 214 impressively literate drug-

²³ Table 19, at p. 70; p. 336 n.15.

²⁴ It is an important question. Claims that trial defense counsel was ineffective or incompetent have been on the rise in post-conviction proceedings ever since the Supreme Court in *Powell v. Alabama*, 287 U.S. 45 (1932), interpolated the adjective "effective" in the 6th amendment's requirement of "the Assistance of Counsel," *ATT'Y GEN. COMM. ON POVERTY AND THE ADMINISTRATION OF CRIMINAL JUSTICE REP.* 44 (1963), and can be expected to proliferate in the wake of *Gideon v. Wainwright*, 372 U.S. 335 (1963). See Lumbard, *The Administration of Criminal Justice: Some Problems and Their Resolution*, 49 A.B.A.J. 840, 845 (1963).

A related question of importance has to do with disparity of counsel's resources in criminal cases. See Waltz, *Inadequacy of Trial Defense Representation as a Ground for Post-Conviction Relief in Criminal Cases*, 59 NW. U.L. REV. 289, 335-41 (1964). Kalven and Zeisel did not address themselves to this vexing issue, probably because they could not hope to secure meaningful data on the issue by means of a mail questionnaire to judges.

²⁵ Ch. 28.

²⁶ Pp. 371-72.

²⁷ P. 372.

²⁸ P. 353 n.3.

²⁹ P. 353.

³⁰ This qualifies as something of a lapse since, in connection with judge-jury disagreements, the authors had scrupulously directed each judge-respondent to mark his hypothetical verdict prior to the jury's return. P. 52. Of course, one of the flaws inherent in the arm's-length questionnaire approach to a study of this kind is that we must assume that the judges obeyed this stricture.

takers. The contents also include a brief review of that scientific evidence which is favorable to the conclusion that the hallucinogens are of unique psychotherapeutic value; a discussion of selected—and sometimes highly questionable—aspects of the history of hallucinogenic drug use, including a commentary on the contemporary Drug Movement; a very considerable body of dramatic personal accounts of hallucinogenic drug experiences; a treatise on the role of the “guide” (sometimes called the “guru” in other manuals) and setting in securing a satisfactory drug experience; and finally what emerges as a theory of drug action. The latter is neither neuropharmacological nor sociopsychological but rests rather on the authors’ conception of self-enhancement obtainable primarily through drug use and their postulation of several levels of experience, psyche and Unconscious which are capable of revelation through LSD. The most fundamental of these—“the most profound and transforming”—is the level of religious-mystical experience. That conviction is reflected in their title paraphrasing William James’ great work. In the course of things the reader is offered a colorful pageant, a catalogue of imagery in the form of subjective reports illustrating the progression of experiences said to await the psychedelic pilgrim. One is reminded, although not by the authors, of similarly vivid and persuasive accounts offered in the past by writers using opium, hashish, alcohol, etc. (See Ebin, *The Drug Experience*, 1961.)

There are a number of positions advocated by the authors which deserve note. They contend (a) that present alarm of LSD risks is exaggerated since use by normals offers no danger if the guide is carefully chosen; psychosis, for example, “rarely ever will occur”; (b) that certain alcoholics, criminals, sex deviates, anxiety neurotics and others can be cured through LSD more effectively and more quickly than by other methods; (c) that hallucinogenic drug use should not be confined to healing efforts since the normal person may expect gains of insight, integration, religiosity, joy, etc.; (d) that the supervision of drug research and ordinary use should not be restricted to physicians (or other scientists); (e) that present FDA restrictions on LSD research are a case of poor medical and political judgment; and (f) that dangers do exist in the improper use of hallucinogens, that some people should not take them (but that even the unsupervised use by the “psychedelic underground” produces fewer casualties than do programs directed by medical and scientific personnel).

Although accepting most claims for LSD-induced enhancement of psychological processes (memory, sensitivity, nonverbal communication, empathy, concentration, etc.), the authors claim only an interested uncertainty as to whether such drugs facilitate mental telepathy and they discount the contention of many users that one becomes more loving or that behavior change necessarily follows even an intense drug experience.

What is one to make of this highly readable dramatic account set within a well-organized argument for the existence of a chemically expandable, complex, potentially religious and necessarily beautiful Unconscious, an account not cluttered by thorough documentation, the expression of serious doubts, nor the conscientious presentation of counter-arguments? It all depends on what the reader seeks. If the reader be a hard-nosed scientist who requires careful support for the conclusions reached and whose training makes him aware of the difficulties in ascertaining objective reality—whether that be therapeutic efficacy or the determinants of introspective reports, and if he requires the exercise of the usual conventions in sampling, measurement, data analysis and reporting, he will be appalled by this work, appalled by its seductively confident style, its errors, and its naivete. If, on the other hand, the reader is a layman seeking accounts of drug experiences that are always vivid and often poetic and moving, accounts presented within a thoughtful framework encompassing provocative philosophical and religious views, he will be pleased. Should that same layman go beyond reading to be persuaded to take LSD and then suffer one of those psychoses which the authors argue is so unlikely, his reaction no doubt will be shocked surprise. If the reader is already a member of the Drug Movement he will enjoy another supportive account, and if he is seeking a manual for the non-medical and non-Leary administration (not necessarily illicit) of LSD, he may find this to be the text he seeks. If the reader be a criminologist interested in any kind of data about illicit drug use, about the social or psychological correlates of membership in the “psychedelic underground”, or in problems of control or rehabilitation of drug offenders, he will not find that data here.

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NARCOTICS. Edited by Daniel M. Wilner and Gene

G. Kassebaum. New York: McGraw-Hill, 1965. Pp. 302. \$13.50.

NARCOTIC ADDICTION. Edited by John A. O'Donnell and John C. Ball. New York: Harper & Row, 1966. Pp. 248.

There is an extraordinary amount of wisdom in the two volumes under review, and the wonder of it is that so much of it remains embalmed in print and so little becomes translated into public policy. Anyone carefully digesting the readings presented by Wilner and Kassebaum and those included in the O'Donnell and Ball book will be quite well informed about the narcotics situation in the United States, at least as it concerns the opiates and marihuana. He will have a solid foundation in pharmacological matters and biochemical concerns. He should be well rid of those myths that he might originally have brought to bear upon the subject, and he should be able to view with a great deal of alarm the tortured and distorted path we have chosen in attempting to deal with our self-defined dilemma.

The Wilner-Kassebaum reader, hard-bound, includes the proceedings of a Spring, 1963 conference at the University of California in Los Angeles. To a reviewer who heard most of the presentation, it is a pleasant reunion. Few persons at the conference are apt to forget the admirable way in which Francis Allen, now Dean of the Law School at the University of Michigan, presented reservations he had regarding civil commitment, and the grudging, yielding manner in which he allowed himself to be pressed into more forceful statements than he said he had intended originally.

I recall that the "hit" of the meetings was the overview of relapse studies, clinically and yet gently dissection by John O'Donnell. The most readable paper certainly, and perhaps that with the most impact, is Louis Lasagna's piece, amiably titled, "Addicting Drugs and Medical Practice: Toward an Elaboration of Realistic Goals and the Eradication of Myths, Mirages, and Half-Truths". It should be the first thing anybody entering the field of narcotics should read. Leslie Wilkins' contribution on the implications of the British experience with narcotics, a presentation that generated heated controversy at the conference, reads quite calmly. One of its major conclusions, that "perhaps a society can effectively control only those who perceive themselves to be members of that society" and that "perhaps the major . . . distinction between a criminal and a sick person is that the sick person can still be identified and identify

himself as within the social system" is an intriguing insight, though the social and personal implications of the "sick" label, as Wilkins well knows, itself needs further scrutiny.

The O'Donnell and Ball volume is a more comprehensive chronological reproduction and review of the literature on addiction. It has deeper historical perspective, though less material on current treatment approaches and current disputation. Especially valuable is its reproduction of the statement by the Council on Mental Health of the American Medical Association, reviewing the operation of narcotics clinics between 1919 and 1923, a document that should put a good deal of nonsense to rest. In addition, by the nature of their assignment, O'Donnell and Ball were able to include much substantial material that has appeared over the years, notably Becker's "Becoming a Marihuana User" and the Volkman and Cressey article on Synanon and rehabilitative theory, a piece which may, with exception of Sutherland's first article on white-collar crime, be the most republished article in the field of social deviance.

Persons interested in the use of narcotics should own both volumes, though the less affluent, faced with a choice, would be well advised to invest in the much cheaper, paperback O'Donnell and Ball book. Together, they present a fine panorama of a field of study and action undergoing intensive scrutiny, as a prelude, probably, to a major policy error—that involved in civil commitment. It will be interesting to note in a decade or so how firmly the idea of addicts as "sick" people takes hold and what its consequences are. Experiments today in the use of methadone and cyclazocine seem to herald deeper involvement of the medical profession in the handling of addicts, and perhaps they herald as well a radical re-definition of the problem. But how the doctors can retrieve the subject from law enforcement and yet keep it out of the realm of personal pathology is a tantalizing dilemma. Both books contribute a good deal of insight into the paths that might be chosen; their neglect will come at the expense of the addicts themselves.

GILBERT GEIS

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ALCOHOLISM: GROUP PSYCHOTHERAPY AND REHABILITATION. By Hugh Mullan and Iris Sanguiliano. Springfield, Ill.: Charles C Thomas, 1966, 325 pp., \$12.00.

An interdisciplinary team approach is the best feature of this newest work on alcoholism. The

chief reason for this approach is that, traditionally and practically, few psychiatrists in private practice undertake to treat alcoholism unless they can hospitalize them in private hospitals, and this is possible for only a minority of patients.

The contributors, other than the co-authors, are Ruth Fox, M.D., Medical Director of the National Council of Alcoholism; Esther W. Griffing, Research Associate, group therapist, and social worker at clinics and hospitals; and Dr. Rose Wolfson, who is associated with research projects on alcoholism. About the qualifications of the writers there can be no doubt; they have all worked with the New York Alcoholism Vocational Rehabilitation Project and their reports about this are valuable. But there can be considerable doubt about the efficiency of group psychotherapy as a modality of treatment of alcoholics.

It is generally known that no single modality of treatment or combination of modalities will help all alcoholics, and the way the authors applied group psychotherapy to their patients is not necessarily the preferred treatment. The authors' claim that "group psychotherapists have reported that patients long experienced in psychoanalytic (one-to-one) treatment very often have great difficulty entering an intensive interactional treatment group" is nowhere supported by source material and I do not think this is a true statement.

One reason for stressing any one modality of treatment for alcoholics may stem from the authors' one working definition of alcoholism: "The 'compulsive need' to drink alcohol." What they mean by the quotation marks around "compulsive need" is not explained, nor is "compulsive" defined.

The authors do not claim that group psychotherapy alone should be utilized with alcoholics. In the chapter *Casework with the Alcoholic*, they say: "When the worker finds himself dealing with the confused, poorly-motivated, more-often-than-not-drunk patient, the use of Alcoholics Anonymous becomes mandatory. The patient will probably say that other approaches to his problem have failed and that even A.A. has been unsuccessful with him". These two sentences, among others, contain dozens of contradictions and irrationalities, and reveal a feeling of impotence on the part of even qualified "workers" to deal with a problem afflicting at least five million known addicts in this country.

This anthology discusses staff adjustment to the alcoholic; aims and goals and directions of treat-

ment; "orientation" of the alcoholic to his illness; testing; vocational counseling; patient selection, preparation and group placement; and various phases of group treatment, complete with dream material. But the most difficult problem of all—how to motivate an alcoholic patient—is side-stepped, probably because of the authors' failure to offer a psychodynamic approach. Motivation calls for psychodynamic principles. I consider this the main failure of the book.

On the other hand, the topics dealt with are well-chosen and significant for the therapist who treats alcoholic addicts. Apart from the ponderous pretense that group psychotherapy is the preferred modality, the book does make a fine contribution to the treatment of alcoholic patients.

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THE ECONOMICS OF DELINQUENCY. By *Belton M. Fleisher*. Chicago, Illinois. Quadrangle Books, Inc., 1966. 127 pages. \$4.50.

"To what extent . . . is that highly unacceptable form of youthful behavior, delinquency, caused by essentially economic factors?" That is the question which Fleisher—an economist, not a criminologist—set out to answer, in the hope that his answer would "suggest how delinquency might be controlled by the manipulation of economic conditions." What follows is an impressive example of rigorous data analysis, carried out within a terminological straitjacket.

Assuming that a great deal of deviant as well as conforming behavior is directed to the pursuit of economic gain—hardly a "novel approach" to the study of human behavior—the writer proceeds to conceptualize delinquency as illegitimate behavior which results from a young person's private economic decisions, in the sense that decisions about how to use the valuable resource of time "profoundly affect their economic circumstances in later life as well as their current welfare". (p. 20) Such decisions may involve either a deliberate choice of a criminal career or drift into such a career. The individual faced with the choice between illegal and legal alternatives is viewed as (as if) deciding "on the basis of their relative costs and benefits and of his own tastes". (21) Short shrift is given the notion that criminal behavior is essentially nonrational; the author simply asserts without elaboration or citation that "many experts would deny this", and that even if a high proportion of criminals are mentally

unsound, "it still makes sense to ask whether changes in economic conditions produce changes in the proportion of the population engaged in illegitimate behavior". (22-23) In any case, it really does not matter, since the tastes-costs-benefits scheme is not being advanced as a new theory of delinquency but as "a logical framework useful for ordering observable phenomena related to delinquent behavior". (23) What we are offered is a methodological rubric in which, as the author himself says, "there is nothing intrinsically economic". (19)

Even so, Fleisher has put much effort into the presentation and justification of his non-theory economic perspective on delinquency, and clearly expects it to be given serious consideration. The question is, is his conceptual framework as useful for us as it undoubtedly is for him?

Although few would deny that much delinquent behavior does pay off directly or indirectly in material benefits, even fewer are likely to buy the strained attempt to impose so limited an analytical scheme upon so complex a phenomenon. The assumption that a lot of delinquency, at least, is understandable through costs-benefits analysis melts into a discussion that sounds as though the notion "some—perhaps most—of the benefits of crime among the young are non-pecuniary" (25) is not to be taken seriously after all. After observing that over 90 percent of those under 25 arrested for FBI index offenses are charged with robbery, burglary, larceny, or auto theft, and that theft is often necessary for young drug users to purchase narcotics, the writer reasons that non-pecuniary returns—such as the "thrill of getting away with a crime" (27)—are likely to be more or less positively correlated with pecuniary returns, largely because the higher the economic or social status of a victim or the greater the value of stolen or damaged property, the greater the risk, and therefore the greater the thrill. The last point is ingenious but empirically dubious; certainly the commonsensical expectation that property crimes will be more frequent "where the market value of the assets of potential victims is higher" (27) is contradicted by the fact that the poor are far more often victimized.

The discussion continues to labor under forced draft when attention turns to the problem of conceptualizing the costs of delinquency. Some of the difficulties here appear to result from assuming instead of examining the proverbial wisdom concerning the impossibility of profit from criminal

activity. (see 20-21) Two kinds of costs are posited: the costs of spending time in violating the law "instead of using it legitimately for productive purposes" (28), and the reduction in "expected lifetime earnings" effected by apprehension, punishment, and having a criminal record. Accepting the objection that young people will probably have insufficient experience with the labor market to be able to know what their legitimate economic futures are likely to be, the author suggests that they probably get a rough notion from "observing the experience of their parents and other adult earners with whom they come in contact". (29) No mention is made of the rather sizeable volume of research demonstrating how very inadequate this notion is for explaining and predicting occupational aspirations and expectations. Regardless, the conclusion is reached that "the family income of potential [?] delinquents may be quite a good indicator of the economic costs of crime to them". (29) This conclusion is immediately qualified by a confused paragraph in which it is noted, first, that one must assume that the expected returns from legitimate economic activity and from criminal activity are unrelated, since "otherwise it will be difficult to identify costs with legitimate earning potential". Next, it is granted that the assumption is to some extent untenable, mostly for the very good reason that the attributes associated with economic success and failure are probably the same for both legitimate and illegitimate enterprises. Then, there is an immediate about-face: it is asserted that many persons whose capacities for legitimate earning have not been realized because of educational deficiencies may succeed illegitimately. "Therefore, it may not be too misleading to assume no co-variation between legitimate and illegitimate earning prospects among the young, especially to the extent that their gains from criminal activity are usually non-pecuniary." (29) In other words, having made an excellent case against the assumption, Fleisher decides to accept it—with an inconsistent appeal to a view which he has previously taken great pains to minimize.

The limitations of his frame of reference are probably most apparent as the author tries to make of the concept of *tastes* something other than a residual. Having decided that sociologists are primarily interested in taste, or non-economic preference variables, Fleisher proceeds to the inevitable conclusion that economists and sociol-

ogists must not ignore each other because "the economic and sociological causes of delinquency are so closely related that to ignore one set of causes in investigating the nature and importance of the other would lead to biased results". (24) The history of social science, however, refutes the assumption that the two fields are differentiated by the kinds of variables with which economists and sociologists prefer to work. The fundamental difference seems instead to be the difference in the range of variables which they seek to incorporate into theories of human behavior: the plain fact is that the more "sociological"—including many economists (the most notable here being Bonger, not even mentioned by Fleisher)—incorporate a far greater range of variables in the course of research on all forms, including economic, of human activity. Apparently without even seeing the need for such a more general, or sociological theory, Fleisher ends by simply tossing all schemes more comprehensive than his own into the category of "tastes" without even looking at them closely. He then fishes out whatever look like "usable variables", by-passing the difficulty of identifying and measuring theoretically significant non-economic variables by using "surrogate variables", or "observable concepts [?] that we believe represent tastes". (30) The non-economic, or surrogate taste variables include (1) proportion of females over 14 and separated or divorced, (2) proportion of the population over 5 resident in a different county five years prior to census, (3) non-white proportion of the population, (4) a southern-other city dummy variable, (5) proportion of dwellings owner-occupied, and (6) median number of years of schooling of adult population. No systematic attempt is made to relate these variables to the criminological or any other literature.

The atheoretical character of the effort to formulate a statement of the problem of disentangling the economic and non-economic results in a variant of multiple-factorism—a classic and severely criticized strategy in criminological research. The consequence is that one does not know quite what to do with the results of the regression analyses. Fleisher argues that although the estimated effects of the taste variables vary greatly, the fact that alternating them in various combinations has no appreciable effect upon the estimated effects of median family income and of male civilian unemployment rate

"suggests that the difficulties in determining the proper specification of tastes and their effects on delinquency may have little practical consequence for a study of the effects of economic factors". (109) On the basis of both time series and cross-sectional analyses of several different sets of data, with delinquency operationalized as arrests or court-appearances, his best estimates of the independent effects of the economic variables are as follows:

(1) The "combined effects [direct plus indirect] of a \$500 increase in income would probably be a reduction of about 5.2 arrests per 1,000 population. In areas of high tendencies toward crime, a 10 per cent rise in incomes might well result in a 20 per cent decline in delinquency". (117)

(2) "The evidence... indicates that on the average a 1 percent change in unemployment will cause a .15 change in the delinquency rate in the same direction." (117)

Thus, we arrive at about the point where Georg von Mayr left us in the last century, with a half-penny change in the price of rye associated with an increase or decrease of one theft per 100,000 persons. Those who hope to alleviate the crime problem by warring on poverty will have to do a lot better if the skeptics are to be won over.

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STREET GANG WORK: THEORY AND PRACTICE. By *Irving Spergel*. Reading, Massachusetts: Addison-Wesley, 1966, pp. xviii, 249, \$5.95.

Dr. Irving Spergel is to be commended for producing a book on street gang work that provides a broad overview of the area and is at the same time specific and direct enough to guide daily practice. It is, in fact, one of the very pleasing characteristics of this book that the principles of community organization, group work, and casework are so skillfully blended with specific suggestions and illustrative excerpts from actual on-the-street incidents. This integration of theory and practice, as the title quite properly suggests, is made possible by proposing, at the very beginning, a typology of delinquent gangs based largely upon the assumption of limited opportunity or insufficient access to the symbols of status of the general culture.

Discussion is limited largely to youths from families with low income in contrast to youths from

middle or upper income families. Three general types of delinquent subgroups are suggested: the racket group, the theft group, and the conflict (fighting) group. These different groups, arising from and supported by different organizational structures of the community, are described in detail with the authenticity of an author "who has been there." This categorization of delinquent groups is not meant to be mutually exclusive and subsequent work may suggest more appropriate or heuristically valuable categories.

The following definition of street gang work is offered: "The practice variously labeled detached work, street club, gang work, area work, extension youth work, corner work, etc., is the systematic effort of an agency worker, through social work or treatment techniques within the neighborhood context, to help a group of young people who are described as delinquent or partially delinquent to achieve a conventional adaptation."

An agency's perception of and methods for dealing with delinquency problems may be used to characterize basic agency orientation. The "treatment agency" generally has a strong professional social work base and a tradition of psychodynamic or therapeutic work with individuals and small groups. The emphasis tends to be upon younger, potentially delinquent youths with emotional problems. The "area agency", usually growing out of a community concern for the control of severely delinquent and aggressive youths, is often neighborhood-oriented and tends to work with "natural" groups in the community. There is also a "balanced agency" combining the "treatment" and "area" approaches. Although the author's description of these types of agencies is compatible with a functional analysis of a Parsonian type, the discussion is not sufficiently extended or detailed enough to satisfy those hoping for a formal sociological consideration of the linkage of street gang work practice to standard community institutions.

The discussion of techniques for initiating the relationship with a gang contains a typical warning concerning gang ambivalence toward the worker and offers an example of the worker casually walking through a neighborhood to make contact. One might wonder, however, whether some of the difficulties workers face in establishing relationships might not result from a failure to directly offer the gangs things they consciously want in a manner that does not embarrass or confuse them. For example, by offering a gang the possibility of par-

ticipating in programs as research subjects or assistants and paying them for this, they may earn money which they want and at the same time be offered a role that enhances their status. By doing this, the worker may make an immediate, direct and positive contact. Secondly, some behavior which may be interpreted as resistance or hostility may instead only reflect an inability to schedule time or follow usual middle class amenities. Attendance and cooperative behavior may need to be taught or "shaped" by the worker using behavior change techniques.

Because I have a personal preference for the use of behavior change techniques based upon learning theory, there is, for me, a somewhat disappointing absence of references to the psychological literature on operant conditioning, prompting, desensitization, contingency management, and modeling. Although there is a brief discussion of the need for concrete learning and reinforcement, the very practical example of dealing with a boy's urinating in a store alcove illustrates the use of punishment rather than reinforcement. On the other hand, without an explicit theoretical formulation of procedures, the author describes very well the use of behavior which competes with antisocial activities and the extinction of discriminative stimuli in controlling aggressive activity. There is also perhaps a bit too great an emphasis upon advice, admonition, ostracism, and use of negative sanctions in contrast to the use of schedules of reward, praise, status enhancement, and the unexpected pleasant surprise (as per Eissler) which might be very appropriately related to opportunity theory. Spengel, however, correctly points out that creativity, flexibility, adventure, and a sense of the dramatic are important elements in street gang work.

The book, in general, is a most pleasing combination of good examples and scholarly references. In it one may find the insights of Cloward, Ohlin, Glaser, Miller, Short, and others skillfully woven into practice. Near the end of the book one may find the statement that the street worker is a bridge between those persons who have resources and do not know how to offer them effectively and those youths who do not have resources but desperately need them. Hopefully, we may also learn how to share honestly our concerns as well as our wealth with these youths and thus develop genuinely effective strategies for welcoming delinquents to broader social and personal perspectives. *Street*

Gang Work goes a long way in this direction and is worthy of careful consideration.

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Book Notes

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Pawel Zakrzewski. Warsaw: Wydawnictwo Prawnicze. 1964. 224 p. \$85, U.S. Currency.
This book is concerned with efforts at prediction of a future behavior of a delinquent and his susceptibility to rehabilitation. A scientific identification of a potential delinquent should be reflected in the administration of justice, application of parole, and treatment of prisoners. Summary in English, p. 221-224: "Problem of Prediction in Criminology".

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