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BOOK REVIEWS

REVIEW ARTICLE

THE IMPACT OF CRIME ON THE DEVELOPMENT OF THE AMERICAN POLICE*

CYRIL D. ROBINSON**

The writing of police history by historians largely dates from Roger Lane's 1967 history of the Boston police, followed by James F. Richardson's history of the New York City police and the 1977 comparative study of London and New York City police, *Cops and Bobbies*, by Wilbur R. Miller. There have also been a number of excellent articles on police history by Mark H. Haller and others. Aside from these works, police history has largely been ignored by historians and has more often than not been the province of sociologists, criminologists and political scientists. This tendency for other disciplines to fill the gap left by historians has not been all bad. One result has been to allow nonhistorians to paint with a broader brush than historians ordinarily would allow themselves. Historians as a precept of their craft generally restrict themselves to a certain time period and subject matter and draw only the most careful and limited conclusions from their materials.

It is perhaps with this perspective in view that Johnson, an historian, in a "bibliographical note" mentions that "scholars dissatisfied with the somewhat mechanistic interpretation which Lane and Richardson use to explain the origins of preventive policing have written studies analyzing the roles of community conflict and ideology in the reform of law enforcement" (at 234-35). Johnson finds that even studies that accept this latter task have the "common failing" of being "case studies" rather than employing a "comparative analysis which would enable us to assess the general trends in the development of the police." He notes that "the administrative history of police departments in the

large cities seems to have followed a similar pattern everywhere" (at 235).

Johnson's book then may be judged by his own criterion: whether it allows us more comprehensively to "assess the general trends in the development of the police." I conclude that Johnson has been either excessively conservative in assessing these trends or has been insensitive to them. Nevertheless, he has provided ample material for readers with another perspective to come to their own conclusions.

The author takes for his objectives two intertwined themes: to show (1) how the criminal underworld shaped police behavior in the nineteenth century and (2) how the theory of crime prevention worked in practice. He concentrates on three categories of unlawful behavior: theft, street crime and illegal enterprise (gambling and prostitution). He is quite right in suggesting that the relationship between criminals and police is important in understanding police practice and further that it has been largely overlooked by police historians.

Although the author competently chronicles events involving the underworld and related police responses, he is unable to place these events in an analytic framework which explains why the "administrative history" in the departments studied "seems to have followed a similar pattern everywhere."

My major criticisms are that Johnson appears to approach his subject matter with no clear hypothesis or theory of historical relationships for the period he is assessing; that though using the *work* of criminals as his organizing factor, he employs no economic analysis; and although occasionally using class terminology, he seems insensitive to class relationships as dynamic forces in social change.

Johnson ties most social change to "urbanization"; yet he completely ignores the dynamic factors of capitalism, which completely reorganized

* A review of *POLICING THE URBAN UNDERWORLD: THE IMPACT OF CRIME ON THE DEVELOPMENT OF THE AMERICAN POLICE, 1800-1887*. By *David R. Johnson*. Philadelphia: Temple University Press, 1979. Pp. viii, 240. \$15.00.

** Center for the Study of Crime, Delinquency and Corrections, Southern Illinois University.

American society in the opening years of the nineteenth century. While he writes of increasing centralization, discipline, changes in neighborhoods, race riots, working-class districts as the arena for gang fights, violent thefts, and vice, and concludes that the function of the police was to supervise these "unruly" groups on behalf of an upper class elite, he can do not better than relate these events to "urbanization." Johnson sees conflict (race and ethnic riots) as arising out of "opposing views" rather than from conflicts over inadequate access to jobs, housing and recreational facilities.

For Johnson, both the criminal and the police form a separate subculture with only the vaguest connection to the rest of society. Though he notes that "prostitution was the only kind of work which offered women high wages," this effect results from "urbanization and industrialization" (at 151). Particularly annoying is his use of the historian's ploy of referring to moving forces in terms of "urban residents," "respectable citizens," "the public," "many city dwellers," "prominent men," and the like.

Apart from these criticisms, this book is a solid piece of scholarship, an important contribution to the growing mosaic of police historical studies.

THE QUESTION OF PAROLE: RETENTION, REFORM OR ABOLITION? By *Andrew von Hirsch* and *Kathleen J. Hanrahan*. Cambridge, Ma.: Ballinger Publishing Co., 1979. Pp. xxx, 178. \$16.50.

In this decade there have been a number of books that can be labelled "classics" in the sentencing literature. *The Question of Parole* will probably be added to that list. This volume is well-written, well-organized and well-documented. It ties together many of the materials and themes found in a variety of other documents. The footnotes indicate that the book is up-to-date in the sentencing literature. Of course, the reader has a head start if he/she has read von Hirsch's *Doing Justice*. The "desert model" spelled out in *Doing Justice* is applied to the parole process in *The Question of Parole*.

Von Hirsch and Hanrahan argue that the duration of confinement should be based on the seriousness (harm of the offense and culpability of the offender) of the criminal conduct. The "desert model" proposes that a prior criminal record affects the offender's culpability. The authors postulate that lack of a previous criminal record decreases culpability. Despite the protests of the authors, those offenders with prior offenses are getting a

relatively more severe penalty than those offenders without prior offenses. The principle of "commensurate deserts" would seem to preclude this difference in penalties. The difference amounts to penalizing again the repeat offender for the offense that has been subjected to a "deserved" penalty in the past.

In considering problems in prison that may require a modification of an early time-fix, the authors maintain that serious crimes should be prosecuted and minor infractions should result in a loss of privileges. The middle range of prison misconduct, "significant disruptions of prison discipline that do not involve major felonies," could result in administrative extensions of the sentence. The authors distinguish what procedures should apply to a "severe sanction of lengthy extra imprisonment" and a "limited stint of extra prison time." The latter does not require full procedural safeguards in their estimation. However, throughout the book imprisonment is characterized as a "severe" penalty and even short periods of imprisonment "may add up to considerable painfulness."

My focus would not be on the label (serious, moderate, petty) of the misconduct or the length of imprisonment, but the penalty. I would argue that any period of incarceration is a severe penalty requiring procedural fairness. An extension of imprisonment for an offense occurring in prison, beyond what is deserved for the original offense, would seem to require the panoply of rights as much as revocation of parole for a new crime.

The focus of the book is parole, and thus, there is only a brief examination of probation. In both *Doing Justice* and *The Question of Parole*, probation receives little attention. The authors are not certain about the "role that probation now plays, and the role that an altered system of community punishments should play in a more desert-oriented system."

Although the authors conclude that there are "important differences" between probation and parole supervision, I find it difficult to differentiate the supervision of parolees from that of probationers. Even though the authors do not consider supervision to be punitive, I would rely on von Hirsch and Hanrahan's opinion on parole supervision and revocation to argue that supervision should be eliminated and that revocation for a new crime should require the same evidentiary level and standard of proof as at trial. More thought should be given to the idea of a "back-up" sanction that is more severe than that which is deserved.

The authors are very concerned over the public's

reaction to an immediate move to real time from dual time. This concern is important in their decision to retain the parole board, at least during a period of transition. The authors acknowledge that the "public has little clear awareness of how sentencing and parole operate." If that is true, then perhaps an education campaign is in order to prevent the escalation of penalties in those states that do abolish dual time.

In conclusion, von Hirsch and Hanrahan present their cogent arguments for an early determination of the release decision. It appears that the reasonableness of this proposition has been gaining ground across the United States. Retention of the parole board to make the decision on duration based on a desert model is also receiving careful consideration. It remains to be seen whether the essential functions the parole board performs will be retained or redefined or perhaps shifted to a sentencing agency.

NICOLETTE PARISI

Department of Criminal Justice
Temple University

BURGLARY: THE VICTIM AND THE PUBLIC. By *Irvin Waller* and *Norman Okihiro*. Toronto: The University of Toronto Press, 1978. Pp. x, 190. \$12.50.

This small book, only 110 pages of text, is the fourth book in the Canadian Studies in Criminology series (and the second book in this series by Waller). It represents an ambitious study in victimology in which the authors claim to address several major questions: How often, where and in what manner do residential burglaries occur in Metropolitan Toronto? What are the reactions and losses of the victims, the public experience with the system of justice and the public's attitude toward crime and punishment? The authors also claim to address what is crime, what is its impact, what are its causes, what does the victim do, how can we prevent crime, what is the fear of crime and why do we desire revenge. Waller and Okihiro claim further that they address the implications of their answers to policymakers, policemen, urban planners, researchers, the security industry, insurers and those who want to understand the public's experience of crime (at 3).

From the quantitative data we learn that burglary in Toronto is, according to the authors, a minor offense committed by an amateur, involving relatively small property loss with rarely any threat or physical harm. We also learn that three residences in one hundred will be burglarized in To-

ronto each year and that one out of three of these burglaries will not be reported to the police. The study also reports that nearly 11% of the sample had committed burglary and that the favorite targets for burglary were unattended affluent dwellings. These findings are interpreted by the authors as being unambiguous and as having implications for the criminological literature on environmental design and defensible space. Recommendations for preventing burglary are accordingly proffered.

These crisp goals, claims, findings and recommendations lose much of their clout, however, upon close scrutiny of the forward, text, tables and appendixes. In fact, the study is an unsuccessful effort to present Toronto as a city with a minor burglary problem when compared to the central cities in the United States. The first indicators of the troubles to follow are located in the forward (at v) where the series editor, G. A. B. Watson, concludes that some of the previous assurances of our modern culture and science have been discredited with the "revelations of the harsh realities in human nature" and the persistence of "bad judgment." Giving credit to the sociohistorical structural characteristics of our society(ies) for the current disillusionments and anxieties is, apparently, outside of the editor's paradigm. "Judgment" is blamed rather than trying to understand the "content" or hegemony of our judgments. Equally troublesome language on the next page demonstrates that Watson is not only inclined to be asociological, but prefers 1920ish biological determinist explanations of "phenomena which instincts seem to tell us are in a fundamental way criminal" (at vi). With this kind of editorial guidance, it is of little surprise that the manuscript is atheoretical.

While Waller and Okihiro wish to provide a systematic analysis of the "experience of the public with residential burglary" (at ix), no effort is made to define "experience," reveal its dimensions or explore in any way the sociocultural reasons why burglary is experienced as it is by its victims in Toronto or the United States. Neither is the reader ever clear about what theoretical notions the authors have in mind when they state that they are concerned with the "real needs of the victim" (at x, 5). This issue is not made clearer when it is presented as the "real" experience of residential burglary (at 6, 33).

The authors' clearest admission of their theoretical weakness, one which contributes to this study's dubious value to criminologists, sociologists of law and jurists, is their reliance upon a diction-

ary definition (Webster, 1976) of the most important concept in the study—"burglary" (at 16). (Webster, 1976, does not appear in the references. A perfectly acceptable scholarly definition is found in Martin's *Annual Canadian Criminal Code*, which does.) This reliance upon an atheoretical definition and the weak reliance upon the relatively new and underdeveloped "defensible space" school of criminological thought allow the study to be easily dismissed and also prevent it from contributing to our knowledge regarding why any act is considered a crime, as well as why and how we react as we do. In the case of burglary this need not be so, especially when the cross-cultural criminal act examined shares the same legal tradition, *i.e.*, common law, and when it is considered that the sociohistorical and legal development of burglary has been articulately addressed in other works.¹ Without such considerations, so-called cross-cultural analysis will remain sterile, and the scholarly abyss among criminologists, sociologists of law and jurists will persist.

It would be acceptable to dismiss the lack of theoretical guidance if the study was presented only as a descriptive exploratory study on burglary and its victims in Canada. (Indeed, the book jacket, the forward and the acknowledgements say nothing about the book being a comparative study). However, once past the introduction, the book becomes a comparative analysis of Toronto and central cities in the United States. This comparison is, at first glance, a helpful means by which to keep in perspective the claim that \$40 million (at 3, 99) in property and cash is stolen each year by burglars in Canada. The source of the \$40 million figure is not identified nor is there any discussion of the influences of inflation on the annual figure. The book leaves the reader with the impression that burglars annually steal \$40 million, regardless of inflation, with an average loss of \$285 (1971), while in the United States the average loss was \$322 (1970).

This type of comparative statistical information is presented throughout the book and is nearly always supplemented by prose "suggesting that property is more sacrosanct in Toronto, that there is a greater belief in police efficiency or that the moral duty to report is higher in Toronto than

Washington" (at 29) and suggesting that the lower crime rate in Toronto is due to a different cultural heritage, the selective immigration policy, high employment and general affluence (at 101). However, these alleged cultural differences are never analyzed.

This shift to the comparative stance raises problems: the cost of burglary in Toronto appears to be nearly 200% greater than for any other Canadian city, and the statistical information about burglary in Toronto is inconsistently interpreted by Waller and Okihiro.

The average high cost of burglary in Toronto as compared to other Canadian cities is not presented forthrightly and has to be teased out of the fact that the alleged annual loss in property and cash was stolen in "400,000 burglary events each year" (at 106). Simple arithmetic reveals the average loss (if one is willing to accept the \$40 million and 400,000 as accurate) to be only \$100 per burglary. Not only do the authors fail to present this gem, they fail to report that Toronto's average loss is 185% higher than the Canadian average loss to burglary. Instead, the authors insist on presenting the Toronto average in comparison to United States cities. For the authors this may be interesting reading, and it might sell books because it presents an image of Toronto as only slightly troubled by burglary. However, when compared to other Canadian cities, Toronto becomes Detroit!

The authors' discussion of the \$40 million loss is equally as suspect and unenlightening as are the figures. For instance, they claim that the victims' loss "does not create a major social problem comparable to inflation, traffic deaths, or even the flu" (at 101). Not only do the authors mislead by comparing crime to noncrime, they fail to inform the reader at what dollar-point they think a major social problem is created, either for the victims or for society.

In view of these basic perceptual problems, one is not surprised to learn that burglary is a common occurrence (at 79), yet only 3% of Toronto's dwellings are burglarized (at 19). This confusion about the frequency and severity of burglary in Toronto is carried through to the conclusion where Waller and Okihiro state "burglary is one of the commonest of serious crimes" (at 99), but "burglary is a relatively rare event, causing little economic or social harm when it occurs" (at 101).

What we have, then, is an inadequate attempt to present an analysis of burglary that is flawed from beginning to end with problems of improper conceptualization, theoretical emptiness, factual

¹ See, *e.g.*, Wright, *Statutory Burglary—The Magic of Four Walls and a Roof*, 110 U. PA. L. REV. 411 (1951); Note, *Rationale of the Law of Burglary*, 51 COLUM. L. REV. 1009 (1951); Note, *Crime of Burglary—A Perennial Metamorphosis*, 4 WILLAMETTE L.J. 285 (1966); Note, *Reformation of Burglary*, 11 WM. & MARY L. REV. 211 (1969).

unsoundness and inconsistent interpretations. What could have been a significant contribution to understanding burglary in Canada actually is a poorly articulated cross-cultural comparative analysis which lacks comparison within Canadian society as a whole and is therefore misleading. Major questions related to the sociohistorical-legal aspects of burglary and the "magic of four walls and a roof" need to be understood before this study or others like it, with all their good intentions and preventive recommendations, produce desired results.

J. ROBERT LILLY
Department of Sociology
Northern Kentucky University

UTILIZATION-FOCUSED EVALUATION. By *Michael Quinn Patton*. Beverly Hills, Ca.: Sage Publications, Inc., 1978. Pp. 304. \$7.95 softcover, \$16.95 hardcover.

One of the characteristics of criminal justice evaluation research is the observed problem that evaluation results have generally not exerted significant influences on program decisions. Another is the lack of well-developed methodological literature specific to the criminal justice field; most of the rapidly growing evaluation research literature has drawn from experiences with evaluation in mental health, education and poverty programs. *Utilization-Focused Evaluation* is no exception to this observation, yet it is worthy of review in this journal because the carryover of observations gleaned largely from evaluation in other fields may help to impact the utilization of research findings in criminal justice.

The evaluation approach described has two fundamental requirements: first, that "relevant decisionmakers and information users must be identified and organized," and second, that "evaluators must work actively, reactively, and adaptively with these identified decisionmakers and information users to make all other decisions about the evaluation" (at 284).

Patton is not the first to talk of such an approach. Nevertheless, he uniquely blends findings and quotations from a study of the utilization of twenty national program evaluations, other case examples, scenarios, parables and children's stories to present an easy-reading reexamination of a number of generally accepted propositions about evaluation research. Among the issues included are the specification of goals and objectives, design rigor and slippage, program implementation, evaluator objectivity, the use of case studies and other less

"scientific" techniques and the very definition of utilization.

This book would be particularly useful as a teaching document for graduate students to supplement technical research design and methods material with applications issues and problems. It is also useful for persons currently involved in evaluation research issues, as the reader has the opportunity to apply personal experience while reflecting on the author's point of view.

The major shortcoming of the book is also its strength—namely its focus on the evaluator in the effort to increase the utilization of evaluation results. As others have noted, the managers of criminal justice programs need to learn how to use information better to make decisions and how to accomplish this within the constraints of the criminal justice environment. While others work with managers on these issues, Patton is articulating a new approach for the evaluators that tries to take into account the problems and limitations of managers while also redefining the evaluator's task. Regardless of whether one agrees with Patton, the value of the book is its capacity to engage the reader in an interesting discussion of some very important issues.

MARILYN C. SLIVKA
Adult Probation Department
Court of Common Pleas
Philadelphia, Pennsylvania

CRIME AND PASSION: MURDER AND THE MURDERER.
By *David Lester and Gene Lester*. Chicago: Nelson-Hall, 1975. \$12.95.

This book attempts to present an overview of the literature on homicide for the student and the layman. Certainly, there is a need for this type of work as there is a surprising lack of such overviews in the literature. *Crime of Passion* seems to be an attempt to find a middle ground between the more academic literature on homicide and the large number of books on homicide that are geared toward laymen. It is a criminological work rather than a criminal justice work in that no attempt is made to deal with the response of the police, prosecutor and courts to the crime of homicide. The focus is on the characteristics of the crime, the victim and the offender and on the various explanations for homicide found in the literature.

Crime of Passion covers the following topic areas: public stereotypes (myths) of homicide, characteristics of the victim, offender and victim/offender relationship, the law of homicide, parents who murder children, children who murder parents,

marital homicide, presidential assassins, threats of murder, murder from cross-cultural perspective, murder and suicide, theories which attempt to explain homicide, and prevention of murder. The book does not present new data but attempts to synthesize the literature on each topical area. Case studies are utilized to illustrate some points made by the authors.

The authors are both psychologists and their work reflects the bias of that discipline. The book focuses on what is thought to be the "underlying" causes of this act of violence. It is assumed that "deep interpersonal stresses" lie behind "every case" of murder and that most murders can be understood if one examines the developmental history of the murderer (*e.g.*, attitudes of mother, consistency and length of close association with parents, physical damage at birth, physical abuse, rigidity of training about impulse control). It is further suggested that what appears to be a simple motive (such as a distraught husband attempting to kill his wife's lover) cannot be understood without examining the unconscious mind of the killer.

Crime of Passion does have a number of weaknesses. The literature review is not comprehensive.

The great bulk of the literature is outdated and involves only the psychological perspective on homicide. Little or no attention is paid to sociological studies of homicide (those patterned after Wolfgang's 1958 study in Philadelphia), to patterns in homicide over time, to the differences between the FBI and the National Center for Health Statistics data bases, to explanations for the correlates of homicide (*e.g.*, such as why Blacks have much higher victimization and offender rates), to the literature on the problems of prediction. However, the discussion of theories of causation and the section on preventative measures are strong.

This book is probably the best available for use by students and laypersons as a textbook on homicide. The literature review is reasonably complete, most important topics are covered, and it is readable. However, sociologists would be disappointed in the lack of attention paid to sociological correlates and explanations.

WILLIAM WILBANKS

Center for the Study of Crime, Delinquency and
Corrections
Southern Illinois University