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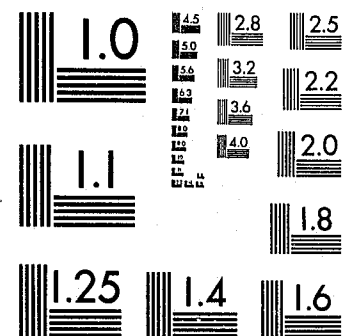
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The Prediction of
Violent Criminal Behavior:
A Methodological
Critique and Prospectus

JOHN MONAHAN

I. OVERVIEW

The identification of persons who reliably can be predicted to engage in dangerous behavior has been called "the greatest unresolved problem the criminal justice system faces" (Rector 1973) and "the paramount consideration in the law-mental health system" (Stone 1975). It is the purpose of this paper to suggest how the problem of predicting dangerous behavior might be clarified by improved methods of empirical research. Current public policies that rely upon the prediction of violence will be briefly reviewed, the empirical data to date will be summarized, and hypotheses will be offered to account for the obtained findings. Following this, five general recommendations for future research in violence prediction will be presented, each with a specific proposal for implementation.

John Monahan is Assistant Professor, Program in Social Ecology, University of California, Irvine.

II. CURRENT POLICY USES OF VIOLENCE PREDICTION

The task of identifying violence-prone individuals has been allocated to the criminal justice and mental health systems. In both systems, predictions of violence[1] are variables in decision-rules relating to who should be institutionalized and who should be released from an institution—the institution being a jail, prison, civil mental hospital, or hospital for the criminally insane.

In the criminal justice system, predictions of violence may be introduced in at least five stages of the judicial process (compare Shah 1976): (a) decisions whether or not to grant bail, and, if bail is to be granted, decisions on the level at which bail is set; (b) decisions whether certain offenders should be transferred from juvenile to adult court for trail; (c) sentencing decisions imposing probation or imprisonment or death[2], and, if imprisonment is imposed, decisions on the length of imprisonment; (d) parole decisions; and (e) decisions whether to invoke special statutes dealing with "dangerous sex offenders," "dangerous mentally ill offenders," or "habitual" criminals (Monahan and Hood 1976).

In the mental health system, predictions of violence are employed primarily in terms of decisions regarding civil commitment to a mental hospital and release from such commitment.

Two recent and contradictory trends in public policies involving the prediction of violence are clearly discernible. One is the increased reliance upon the "dangerousness standard" as the primary or sole justification for civil commitment in the mental health system; many states now follow California's 1969 lead in rewriting commitment laws to emphasize the role of violence prediction (*Harvard Law Review* 1974). The second trend is the decreased reliance upon predictions of violence in determining release from prison in the criminal justice system. Several state legislatures (e.g., California, Maine) have recently passed or are now considering bills to abolish indeterminate sentences

in which the prisoner's release date is determined by a parole board and based in part upon a prediction of his potential for future violence, in favor of sentences of a more definite length set by the judge (cf. Morris 1974; Twentieth Century Fund 1976; von Hirsch 1976).

III. SUMMARY OF VIOLENCE PREDICTION RESEARCH

The eight major research efforts attempting to validate predictions of violence are summarized in Table 1.[3]

Wenk et al. (1972) report three massive studies on the prediction of violence undertaken in the California Department of Corrections. In the first study, a violence prediction scale that included variables such as commitment offense, number of prior commitments, opiate use, and length of imprisonment was able to isolate a small group of offenders who were three times more likely to commit a violent act than parolees in general. However, 86 percent of those identified as violent did not, in fact, commit a violent act while on parole.

In the second study, over 7,000 parolees were assigned to various categories keyed to their potential aggressiveness on the basis of their case histories and psychiatric reports. One in five parolees was as-

TABLE 1[a]   Research Studies on the Prediction of Violence

Study	% True Positives	% False Positives	N Predicted Violent	Follow-up Years
Wenk et al. (1972) Study 1	14.0	86.0	?	?
Wenk et al. (1972) Study 2	0.3	99.7	1630	1
Wenk et al. (1972) Study 3	6.2	93.8	104	1
Kozol et al. (1972)	34.7	65.3	49	5
State of Maryland (1973)	46.0	54.0	221	3
Steadman (1973)	20.0	80.0	967	4
Thornberry and Jacoby (1974)	14.0	86.0	438	4
Cocozza and Steadman (1976)	14.0	86.0	96	3

[a]Updated from Monahan (1976).

[3]This section draws heavily from Monahan (1975, 1976) and Monahan and Cummings (1976).

signed to a "potentially aggressive" category, and the rest to a "less aggressive" category. During a 1-year follow-up, however, the rate of crimes involving actual violence for the potentially aggressive group was only 3.1 per 1,000, compared with 2.8 per 1,000 among the less aggressive group. Thus, for every correct identification of a potentially aggressive individual, there were 326 incorrect ones.

The final study reported by Wenk et al. (1972) sampled over 4,000 California Youth Authority wards. Attention was directed to the record of violence in the youth's past and an extensive background investigation was conducted, including psychiatric diagnoses and a psychological test battery. Subjects were followed for 15 months after release, and data on 100 variables were analyzed retrospectively to see which items predicted a violent act of recidivism. The authors concluded that the parole decision maker who used a history of actual violence as his sole predictor of future violence would have 19 false positives in every 20 predictions, yet "there is no other form of simple classification available thus far that would enable him to improve on this level of efficiency" (p. 399). Several multivariate regression equations were developed from the data, but none was even hypothetically capable of doing better than attaining an 8-to-1 false-to-true positive ratio.

Kozol et al. (1972) have reported a 10-year study involving almost 600 offenders. Each offender was examined independently by at least two psychiatrists, two psychologists, and a social worker. A full psychological test battery was administered and a complete case history compiled. During a 5-year follow-up period in the community, 8 percent of those predicted not to be dangerous became recidivists by committing a serious assaultive act, and 34.7 percent of those predicted to be dangerous committed such an act. While the assessment of dangerousness by Kozol and his colleagues appears to have some validity, the problem of false positives stands out. Sixty-five percent of the individuals identified as dangerous did not, in fact, commit a dangerous act. Despite the extensive examining, testing, and data gathering they undertook, Kozol et al. were wrong in two out of every three predictions of dangerousness. (For an analysis of the methodological flaws of this study, see Monahan 1973b, and the rejoinder by Kozol et al. 1973.)

Data from an institution very similar to that used by Kozol et al. have recently been released by the Patuxent Institution (State of Maryland 1973). Four hundred and twenty-one patients, each of whom received at least three years of treatment at Patuxent, were considered. Of the 421 patients released by the court, the psychiatric staff opposed the release of 286 on the grounds that they were still dangerous and

recommended the release of 135 as safe. The criterion measure was any new offense (not necessarily violent) appearing on FBI reports during the first 3 years after release. Of those patients released by the court against staff advice, the recidivism rate was 46 percent if the patients had been released directly from the hospital, and 39 percent if a "conditional release experience" had been imposed. Of those patients released on the staff's recommendation and continued for outpatient treatment on parole, 7 percent recidivated. Thus, after 3 years of observation and treatment, between 54 and 61 percent of the patients predicted by the psychiatric staff to be dangerous were not discovered to have committed a criminal act.

In 1966, the U.S. Supreme Court held that Johnnie Baxstrom had been denied equal protection of the law by being detained beyond his maximum sentence in an institution for the criminally insane without the benefit of a new hearing to determine his current dangerousness (*Baxstrom v. Herold*, 1966). The ruling resulted in the transfer of nearly 1,000 persons "reputed to be some of the most dangerous mental patients in the state [of New York]" (Steadman 1972) from hospitals for the criminally insane to civil mental hospitals. It also provided an excellent opportunity for naturalistic research on the validity of the psychiatric predictions of dangerousness upon which the extended detention was based.

There has been an extensive follow-up program on the Baxstrom patients (Steadman and Cocozza 1974). Researchers find that the level of violence experienced in the civil mental hospitals was much less than had been feared, that the civil hospitals adapted well to the massive transfer of patients, and that the Baxstrom patients were being treated the same as the civil patients. The precautions that the civil hospitals had undertaken in anticipation of the supposedly dangerous patients—the setting up of secure wards and provision of judo training to the staff—were largely for naught (Rappaport 1973). Only 20 percent of the Baxstrom patients were assaultive to persons in the civil hospital or the community at any time during the four years following their transfer. Furthermore, only 3 percent of Baxstrom patients were sufficiently dangerous to be returned to a hospital for the criminally insane during 4 years after the decision (Steadman and Halfon 1971). Steadman and Keveles (1972) followed 121 Baxstrom patients who had been released into the community (i.e., discharged from both the criminal and civil mental hospitals). During an average of 2½ years of freedom, only nine of the 121 patients (8 percent) were convicted of a crime and only one of those convictions was for a violent act. The researchers found that a Legal Dangerousness Scale (LDS) was most

predictive of violent behavior. The scale was composed of four items: presence of juvenile record, number of previous arrests, presence of convictions for violent crimes, and severity of the original Baxstrom offense. In subsequent analyses, Cocozza and Steadman (1974) found that the only other variable highly related to subsequent criminal activity was age (under 50 years old). In one study, 17 of 20 Baxstrom patients who were arrested for a violent crime when released into the community were under 50 and had a score of 5 or above on the 15-point Legal Dangerousness Scale. Yet the authors conclude (pp. 1013-1014)

For every one patient who was under 50 years old and who had an LDS score of 5 or more and who was dangerous, there were at least 2 who were not. Thus, using these variables we get a false positive ratio of 2 to 1. . . . Despite the significant relationship between the two variables of age and LDS score and dangerous behavior if we were to attempt to use this information for statistically predicting dangerous behavior our best strategy would still be to predict that none of the patients would be dangerous.

The Supreme Court's Baxstrom decision prompted a similar group of "mentally disordered offenders" in Pennsylvania to petition successfully for release in *Dixon v. Pennsylvania*, 1971. The results of the release of 438 patients have been reported by Thornberry and Jacoby (1974) and are remarkably similar to those reported by Steadman. Only 14 percent of the former patients were discovered to have engaged in behavior injurious to another person within 4 years after their release.

Finally, Cocozza and Steadman (1976) followed 257 indicted felony defendants found incompetent to stand trial in New York state in 1971 and 1972. All defendants were examined for a determination of dangerousness by two psychiatrists, with 60 percent being predicted to be dangerous and 40 percent not so. Subjects were followed in the hospital and in the community (if they were eventually released) during a three year follow-up. While those predicted to be dangerous were slightly but insignificantly more likely to be assaultive during their initial incompetency hospitalization than those predicted not to be dangerous (42 percent compared with 36 percent), this relationship was reversed for those rearrested for a crime after their release, with 49 percent of the dangerous group and 54 percent of the not-dangerous group rearrested. Predictive accuracy was poorest in the case of a rearrest for a violent crime, "perhaps the single most important indicator of the success of the psychiatric predictions." Only 14 percent of the dangerous group, compared with 16 percent of the not-dangerous group, were rearrested for violent offenses. While these data are susceptible to alternative interpretations (Monahan, in press[a]), the authors believe that they

constitute "the most definitive evidence available on the lack of expertise and accuracy of psychiatric predictions of dangerousness" and indeed represent *"clear and convincing evidence* of the inability of psychiatrists or of anyone else to accurately predict dangerousness."

The conclusion to emerge most strikingly from these studies is the great degree to which violence is overpredicted. Of those predicted to be dangerous, between 54 and 99 percent are false positives—people who will not, in fact, be found to have committed a dangerous act. Violence, it would appear, is vastly overpredicted, whether simple behavioral indicators or sophisticated multivariate analyses are employed and whether psychological tests or thorough psychiatric examinations are performed.

Several factors have been suggested that might account for the great degree of overprediction found in the research (Monahan 1976).

1. *Lack of corrective feedback to the predictor.* The individual is usually incarcerated on the basis of the prediction and so it is impossible to know whether or not he actually would have been violent (Dershowitz 1970).

2. *Differential consequences to the predictor of overpredicting and underpredicting violence.* False negatives lead to much adverse publicity, while false positives have little effect on the predictor (Steadman 1972).

3. *Differential consequences to the individual whose behavior is being predicted.* A prediction of violence may be necessary to ensure involuntary treatment (Monahan and Cummings 1975).

4. *Illusory correlations between predictor variables and violent behavior.* The often cited correlation between violent behavior and mental illness, for example, appears to be illusory (Gulevich and Bourne 1970, Sweetland 1972).

5. *Unreliability of violence as a criterion event.* There is little consensus as to the definition of violence, and great unreliability in verifying its occurrence (Monahan and Geis 1976).

6. *Low base rates of violence.* The prediction of any low-base-rate event is extremely difficult (Rosen 1954).

7. *Low social status of those subjected to prediction efforts.* Overprediction may be tolerated in part because of class biases in the criminal justice and mental health systems (Geis and Monahan 1976, Monahan *et al.* in press).

IV. FUTURE RESEARCH DIRECTIONS IN THE PREDICTION OF VIOLENCE

The conclusion of Wenk and his colleagues (1972) that "there has been no successful attempt to identify, within . . . offender groups, a subclass whose members have a greater than even chance of engaging again in an assaultive act" is widely shared by researchers in the field (e.g., Stone 1975, Megargee 1976). There is no consensus, however, on the implications of this conclusion for future research. Some agree with Wilkins's (1972) assessment of a major California prediction study that "research along these lines does not seem worthwhile to press. Perhaps this study should be 'the last word' for some time in attempts to 'predict' violence potential for individuals." Others side with Halatyn (1975) that the empirical studies to date "reflect data and design limitations which should stimulate rather than stifle further research."

While the future may bear out Wilkins's pessimistic judgment, we shall proceed here in the spirit of Halatyn's remarks and assume that the last word on violence prediction has yet to be uttered. A series of research priorities shall be articulated that, if successfully implemented, might improve the ability to predict violence to a point at which it could provide useful information to policy decision makers. The ensuing discussion will consider the criterion variables that define violent or dangerous criminal behavior and the predictor variables that attempt to forecast it. In each of these categories, several recommendations will be made to improve the quality of research in the prediction of violence, and specific proposals for research projects will be offered.

Recommendation One: Research on violence prediction must employ multiple definitions of violence.
Proposal One: Violence should be defined in a hierarchy including (a) the four FBI *violent index crimes of murder, forcible rape, robbery, and aggravated assault, and (b) all assaultive acts against persons.*

The choice of a definition of violence for research purposes would be made more simple if there were a consensus among either the public or professional groups as to what behaviors should be counted as dangerous. Unfortunately, no such consensus exists (Monahan and Hood, in press). Given this fact, the appropriate research strategy would seem to lie in the direction of multiple definitions of violence. Research on violence prediction should use several hierarchical definitions of the criterion, each succeeding one being more inclusive than that before it.

This would have two substantial advantages over the current proliferation of studies employing a single arbitrary definition of violent or dangerous behavior:

1. It would allow a greater degree of comparability across studies. As things stand now, it is very difficult to compare the results of prediction research projects that use different criteria. Even projects as similar as Kozol *et al.* (1972) and state of Maryland (1973) did not use similar criteria. Kozol *et al.* defined their criterion as "serious assaultive acts," while at Patuxent, the definition was "any new offense, not necessarily violent."

2. It would facilitate policy implications being drawn from the research. Violence, as Skolnick (1969, p. 4) notes "is an ambiguous term whose meaning is established through political processes." If researchers could present policy makers with a series of plausible definitions of violence, each with attendant empirical data with regard to predictability, the final choice of definition could be left in the political arena (Heller and Monahan 1977).

In establishing multiple definitions of violence, it should be noted that the more inclusive the definition, the greater the predictive accuracy: Large targets are easier to hit than small ones. The data bear out this axiom. One attempt to predict "assaultive behavior" had 16 percent true positives when the criterion was defined as "homicide, all assaults, attempted murder, battery, forcible rape and attempt to rape"; 22.6 percent true positives when the criterion was expanded to include "other sex offenses and kidnapping"; and 53 percent true positives when assaultive behavior was construed still more loosely to encompass "all of the above plus robbery, all sex offenses, weapon offenses and disturbing the peace" (cited in Halatyn 1975). While predictive accuracy is indeed increased as definitions of violence expand, there comes a point at which it is arguable whether one is studying violence or simply any kind of lawbreaking. Including "disturbing the peace" as violent, for example, would seem to stretch the concept to its breaking point.

It would be reasonable to specify initially that at least two levels of the criterion must be identified in future research. One level should be violence in its most strict construction, and the other should be somewhat more inclusive in nature. The narrowest definition of violent crime in common use is that employed by the Federal Bureau of Investigation (e.g., Kelley 1976). Violent crime, according to the FBI, is restricted to (a) murder, (b) forcible rape, (c) robbery, and (d) aggra-

vated assault. There would seem to be little disagreement that these four acts are indeed violent ones.

At the more inclusive level, the kinds of acts referred to by Cocozza and Steadman (1974) and Rubin (1972) as "assaultive behavior against persons," or more formally by Megargee (1976) as "acts characterized by the application or overt threat of force which is likely to result in injury to people" appear reasonably to be definable as violent. According to Megargee (p. 5):

this use of the term [violent] includes, but is not restricted to, such criminal acts as homicide, mayhem, aggravated assault, forcible rape, battery, robbery, arson, and extortion. Criminal behavior not likely to result in injury to people, such as noncoercive thefts or vandalism, are excluded, as are business practices which, although injurious to people, do not involve the application of force.

It is not possible to list precisely all the crimes to be included in this second-level definition of violence, since the categorization of crimes differs from state to state and since many violent acts will result in civil commitment rather than arrest (Cocozza and Steadman 1974). Yet the thrust of defining violence in terms of "assaultive acts against persons" could be captured in future research studies and could add substantially to our ability to compare various prediction efforts and draw policy-relevant information from them.

In research on clinical predictions[4] of violence, it would also appear necessary to achieve a consistency between the "working definitions" of violent behavior employed by the individuals making the predictions and the definitions used in the follow-up research. If a psychiatrist considers "writing a bad check" to be a sufficiently dangerous behavior to justify institutionalization to prevent its occurrence (*Overholser v. Russell*, 1960), and if the validation researcher is limiting his or her definitions of dangerousness to the FBI violent index crimes and assaultive behavior against persons, it is not surprising that overprediction would be reported. Rather than overprediction, however, this would more properly be a case of unsynchronized definitions. Even if the predictions were perfectly accurate—if those predicted to write bad checks actually wrote them—the follow-up researcher using less inclusive definitions of violence would report them as false positives. The two ways in which this inconsistency could be resolved are to match the follow-up criteria to the working definitions used by the clinicians

[4]See the discussion of Recommendation 4 in Section IV for a discussion of clinical and actuarial prediction.

predicting violence, or to provide the clinicians with the definitions to be used in the follow-up and have them predict according to those definitions. Given the need for consistency across different prediction studies, as well as within each prediction study, the latter alternative would appear to be preferable.

Recommendation Two: Research on violence prediction must employ multiple time-periods for follow-up validation.
Proposal Two: Studies should report follow-up results at (a) 1 year, (b) 3 years, and (c) 5 years after release.

The empirical attempts to validate predictions of violence have used a follow-up period of from 1 to 5 years (Table 1). It is self-evident that the longer the follow-up period, the more likely one is to find high rates of true positives, due to the fact that each individual has more opportunity to commit a violent act. Given the difficulty of predicting low-base-rate events, lengthening the follow-up period will have the effect of increasing the base rate, and hence lowering the probability of false positives. The data bear this out. The two studies employing a 1-year follow-up had false positive rates of 99.7 and 93.8 percent, while the six studies using a 3- to 5-year follow-up had false positive rates of 86.0, 86.0, 86.0, 80.0, 65.3, and 54.0 percent.

As with the definition of the criterion, the specification of the follow-up period is not a case of choosing the "best" way to do research. Multiple follow-up periods would serve the same function as multiple definitions: They would increase comparability between studies and facilitate the generation of policy-oriented knowledge. As an attempt at this needed "standardization" of research studies, the reporting of follow-up results at 1-year, 3-year, and 5-year intervals would appear to be both reasonable and feasible.

In the case of predictions by mental health professionals, it would seem that a specification of the duration of the follow-up periods should be made at the time of the original predictions. It would then be possible for different predictions to be made for each of the follow-up periods. For example, a psychiatrist could predict that a given offender or patient had a 30-percent probability of committing a violent act within 1 year after release, a 60-percent probability within 3 years, and an 80-percent probability within 5 years.

Recommendation Three: Research on violence prediction must employ multiple methods of verifying the occurrence of violent behavior.
Proposal Three: Verification methods should be employed in a

hierarchy including (a) conviction rates; (b) conviction rates and arrest rates; (c) conviction rates, arrest rates, and rates of civil commitment to mental hospitals; and (d) all of the above plus self-report.

In the prediction studies to date, police arrest rates have been the primary means of verifying whether or not a violent act has occurred during the follow-up period. For at least two reasons, however, arrest rates are inadequate methods of verification: Most violent behavior is never reported to the police, and the violent behavior that is reported often does not lead to the recording of an arrest.

On the first point, a recent victimization study in eight major American cities found that only 40 to 50 percent of all violent crime was reported to the police. The reporting rate for simple assault ranged from 27 to 39 percent (U.S. Department of Justice 1974). While the reasons for not reporting a crime are varied (e.g., embarrassment, fear of retaliation, low opinion of police effectiveness), the result of underreporting is surely to reduce the usefulness of arrest records as a means of verifying the occurrence of violent behavior (Halatyn 1975).

Added to this is the fact that the "clearance rate" of reported crime (i.e., the percentage of reported crime that results in an alleged offender being charged and taken into custody) is far from perfect. While the clearance rate for murder is reasonably high (79 percent), the clearance rates for forcible rape (51 percent), aggravated assault (63 percent), and robbery (27 percent) are such that a large portion of the violent crime that is reported never finds its way into police statistics (Kelley 1976).

In addition to the standard reasons given to account for the low clearance rates for violent crime (e.g., unidentified offenders, lack of evidence, unwillingness of the victim to press charges, etc.), one factor especially relevant to validation studies of the prediction of violence is that mental hospitalization is often used by the police as an alternative to arrest. As Cocozza and Steadman (1974, p. 1013) noted in their follow-up of the "criminally insane" Baxstrom patients, "some of the patients were rehospitalized for behavior very similar to that displayed by other patients who were arrested for violent crimes." One Los Angeles study found that 33 percent of police referrals to a medical center psychiatric unit had as their primary precipitating incident "some degree of aggressive behavior." In none of these cases was an arrest made (Jacobson et al. 1973).

When these limitations on the use of official crime statistics are taken in concert, they suggest that many persons classified as false positives in prediction research actually may be leading active careers in violent

crime but simply have not yet been apprehended and charged or, if they have been apprehended, they have been diagnosed as "dangerous to others" and processed through the mental health rather than the criminal justice system.

If it is violent behavior, rather than arrests for reported violent crime, that prediction researchers are really interested in, they would do well to broaden their procedures for verifying its occurrence. Criminal justice statistics are estimates of the amount of violent behavior occurring in a given group predicted to be violent. As such, they should be used along with other indicators of violent behavior to arrive at the most reliable estimate possible.

Each estimate of violent behavior will have its own error costs. Reliance solely upon conviction rates for violent crime to verify the occurrence of violent behavior would tend to avoid the erroneous recording of events as violent, but at an enormous cost in the non-recording of violent events that do occur.[5] Arrest records likewise will underestimate crime to the extent that it is unreported or uncleared, but against this underestimation there must be a consideration of those innocent persons who are arrested and later acquitted or have the charges dropped. This is even more true with data on civil commitments to mental hospitals, in which discretion as to the definition of violence and the procedures for certifying its occurrence is great (Monahan 1973a, 1973b, 1977a, 1977b).

Additional validation procedures are needed that do not rely upon the official statistics that so underrecord violent behavior. One such procedure is self-report. Self-report methodologies have been used extensively in the study of delinquency (Hirschi 1969) and might be applied fruitfully to the study of adult violence. In this regard, Toch (1969) has developed a "peer interview" technique whereby parolee research assistants interview other parolees regarding instances of violent behavior. With appropriate guarantees of confidentiality, such methods may provide an extremely valuable addition to the use of official statistics to validate predictive judgments. A representative sample of a cohort of ex-prisoners or ex-patients whose potential for violence is being assessed could be interviewed by other ex-prisoners or ex-patients at 1-, 3-, and 5-year intervals to obtain data on actually committed, but not recorded, violent behavior.

As with the definition of violence and the duration of the validation period, multiple methods for verifying the occurrence of violent behav-

[5]It should be clear that the use of estimates of criminality other than conviction is for research purposes only, since due process considerations preclude their use in the disposition of individual cases.

ior would appear appropriate in future research. A hierarchy of validation procedures beginning with convictions then sequentially adding arrests, mental hospital commitments,[6] and self-reports might be a viable approach. Such a tack, as earlier, should increase comparability across prediction studies and facilitate the derivation of policy implications from the data.

Recommendation Four: Research on violence prediction should stress actuarial rather than clinical methods.

Proposal Four: Actuarial models of the clinical decision-making process should be constructed.

The two generic methods by which violent behavior (or any other kind of event) may be anticipated are known as clinical and actuarial prediction. In clinical prediction, a psychologist, psychiatrist, parole board member, or other person acting as a "clinician" considers what he or she believes to be the relevant factors predictive of violence and renders an opinion accordingly. This was the method used in the Kozol, Steadman, Thornberry and Jacoby, and Patuxent studies reviewed earlier. The clinician may rely in part upon actuarial data in forming the prediction, but the final product is the result of an intuitive weighting of the data in the form of a professional judgment. Actuarial (or statistical) prediction refers to the establishment of statistical relationships between given predictor variables (e.g., age, number of prior offenses) and the criterion of violent behavior. This method was used in the Wenk *et al.* series of studies. The prediction variables may include clinical diagnoses or scores on psychological tests, but these are statistically weighted in a prediction formula.

One of the "great debates" in the field of psychology has revolved around the relative superiority of clinical versus actuarial methods. It is one of the few such debates to emerge with a clear-cut victor. With the publication of Paul Meehl's classic work in 1954 and its many subsequent confirmations (Sawyer 1966), actuarial methods have come to be recognized as the generally superior way of predicting behavior.

At first glance, the research reviewed above on the prediction of violence would appear to constitute an exception to this rule. The five clinical studies have reported substantially better predictions than the three actuarial ones. While several confounding factors make this

[6]By commitment here is meant commitment to a mental hospital through the police power rather than the *parens patriae* power of the state (Kittrie 1971, Shah 1977). Thus, in California, a civil commitment as "dangerous to others" should be counted in validation studies, while commitment as "gravely disabled" (which is defined as an inability to feed, clothe, or house oneself) should not.

comparison problematic (e.g., the base-rate for violent behavior was higher, and the follow-up period longer for the clinical than for the actuarial studies), it would at least be fair to conclude that the actuarial method has not shown the same superiority over the clinical method in the case of violence as it has with the prediction of other behaviors.

Two conflicting interpretations might be drawn from a comparison of the clinical and actuarial studies. One is that clinical prediction methods really do constitute the best way to predict violent behavior, and that future research should focus on improving the predictive accuracy of clinicians. The other is that actuarial methods have not yet lived up to their potential, judging from their performance in other areas, and that a priority for future research should be the development of more sophisticated actuarial models. We shall argue for the latter interpretation.

While it is undoubtedly true that much can be done to improve the accuracy of clinical predictions of violence—including the multiple definitions, validation periods, and methods of verification mentioned earlier and the inclusion of situational variables, to be discussed below—the impression persists that clinicians have taken their best shot at predicting violence and that future improvements will not drastically alter the two-to-one false positive ratio reported so consistently. The Kozol and Patuxent studies, for example, both involved extensive multidisciplinary examinations over a lengthy period of observation in nationally recognized institutions. The base rates for violence in their populations were high, the follow-up periods long, and the criteria generous. Still, a majority of the predictions were erroneous in both cases.

Actuarial studies, on the other hand, have often been based on "general purpose variables" (Wenk and Emrich 1972) rather than on theoretically derived predictors and have been employed with short follow-up periods on populations with very low base-rates of violent behavior. There have been few actuarial studies of any sort, and all have relied on data from a single source (the California Department of Corrections). It would seem that actuarial methods need to be pursued with more vigor before an exception is declared to the general superiority of actuarial over clinical prediction.

But perhaps too much has been made in the past of distinguishing actuarial and clinical methods, and not enough of how each might contribute to the other. Clinical predictions, as was noted, may take into account actuarial tables, and actuarial prediction may incorporate clinical judgments. Two possible strategies for cross-fertilization, therefore, suggest themselves. One is to provide clinicians with as

much actuarial information as possible, to see if this affects their predictions. The other is to construct actuarial models based upon the variables used in the clinical decision-making process.

On the first point, Hoffman *et al.* (1974) presented actuarial prediction tables to parole board members reviewing the files of adult male inmates for parole consideration. The board members were then asked for their own clinical predictions and for a decision on whether the inmates should be paroled or kept in prison. They found that the correlation between statistical risk estimates based on the actuarial tables and the board's clinical risk estimates was 0.74 when the actuarial tables were presented to board members before they made their clinical judgments, and 0.53 when the tables were not provided. The correlation between risk estimates and the outcome of the parole decision was 0.30 when the actuarial tables were provided and 0.18 when they were not. The provision of actuarial data, therefore, affected both the clinical judgments of the parole board and its parole decisions in the predicted direction.

The difficulty with this strategy is that it is, in effect, matching clinical judgments to actuarial ones. This will result in improved predictive accuracy only to the extent that the actuarial predictions are, in fact, better than clinical ones would be. In the prediction of violence, however, actuarial predictors have not yet shown their superiority. Based on the results reviewed earlier, influencing clinical predictions to look more like actuarial ones could result in lowered predictive accuracy in the case of violent behavior. This is especially true in light of the fact that Hoffman *et al.* (1974) found that actuarial data were more likely to result in increasing clinical predictions of unfavorable parole outcome (when the actuarial data suggested such an unfavorable outcome) than they were to result in decreased predictions of unfavorable outcome (when the actuarial data were in the favorable direction). This would mean even more false positives if such a strategy were applied to the prediction of violence.

The other possible rapprochement between clinical and actuarial prediction lies in the construction of actuarial models of clinical decision making. Along these lines, Gottfredson *et al.* (1975), relying upon a study that found that the primary variables influencing parole decision making were severity of offense, "parole prognosis," and institutional behavior, developed systematic decision-making guidelines to be fed back to the parole board members from whom the factors were originally derived. They operationalized severity of offense on a 6-point scale and parole prognosis on an 11-point "salient factor" actuarial table, and they developed guidelines concerning the mean sen-

tence served for each severity/risk level. These guidelines were presented to the parole decision makers, as they were reviewing cases, who were asked to record their reasons if their recommended sentence in a given case was outside the range provided (poor performance in the institution, for example, could be one reason for exceeding the guidelines). While no comparison groups were used in this study, the researchers found that 63 percent of the parole recommendations were within the guidelines presented.

Creating actuarial models of the clinical decision-making process in the prediction of violent behavior could have two advantageous effects. First, it would make explicit the variables used in clinical decision making. These variables could then be incorporated on their own account into actuarial models so that their predictive accuracy could be independently assessed. Second, it could increase consistency both between and within individual decision makers, and this increased consistency or reliability could itself lead to improved predictions. As Goldberg (1970) has stated, "linear regression models of clinical judges can be more accurate diagnostic predictors than the humans who are modeled." He goes on to note that a clinician can incorporate and evaluate a great deal of information but that he or she lacks the reliability of a computer always to respond to similar information in similar ways (p. 423):

[The clinician] "has his days": Boredom, fatigue, illness, situational and interpersonal distractions all plague him, with the result that his repeated judgments of the exact same stimulus configuration are not identical. He is subject to all those human frailties which lower the reliability of his judgments below unity. And, if the judge's reliability is less than unity, there must be error in his judgments—error which can serve no other purpose than to attenuate his accuracy.

Goldberg took a subsample of psychologists' judgments on predicting psychosis from psychological tests and derived a statistical model of their decision-rules. He then had the clinicians and the statistical model of the clinicians compete in predicting psychosis (defined independently) for the rest of the sample. The model won, since it was not subject to the same random errors as were the clinicians from whom it was derived.

It is important to separate the reliability of predictions from their accuracy or validity. Creating statistical models of the clinical prediction process may increase the reliability of the process substantially, but it will increase predictive accuracy or validity only to the extent that some random error is eliminated. Deriving an actuarial model of a clinical prediction process that has low reliability and low validity will

result only in a model with high reliability and almost-as-low validity. The model, in other words, will not be much better than the clinical judgments on which it is based. It may, however, be much quicker and cheaper than human predictions.

Since clinicians do appear to have some (albeit meager) ability to predict violent behavior, a priority for future research should be to create statistical models of the clinical prediction process. The factors obtained could themselves be used in a prediction model (as in Goldberg 1970), or they could be fed back to the clinical decision makers in a systematic fashion to see if they would make more consistent judgments when presented with, in effect, their own preferred data base (as in Gottfredson et al. 1975).

Recommendation Five: Research on violence prediction should include situational as well as dispositional predictor variables.

Proposal Five: Situational variables should be derived from conceptions of human environments in terms of (a) personal characteristics of the environment's inhabitants, (b) reinforcement properties of the environment, and (c) the psychosocial climate of the environment.

After one has defined the criteria, specified the validation periods, selected the methods of verification, and decided upon a clinical or an actuarial prediction format, it remains to choose the variables upon which one will base the prediction effort. Ideally, these predictor variables should be related to the criterion variables by virtue of their causal implication in some theory of violent behavior. Yet unlike theories of aggression (e.g., Bandura 1973), theories of human violence have not generated a great deal of scholarly interest (Megargee 1969). This has left the person who would predict violence with only his or her own implicit theory of violence to guide in the selection of predictor variables.

As it happens, since many of the individuals involved in violence prediction efforts have been mental health professionals or others who have adopted a "mental health ideology," almost all of the variables that have been investigated as predictors of violence have been dispositional variables. That is, they have referred to fixed or relatively enduring attributes or traits of the person under study, such as age, sex, race, prior criminal record, or psychiatric history and diagnosis. This reliance upon dispositional variables or personal traits has characterized not only the prediction of violence but the prediction of all types of behavior. The result has been the same in each case: low correlations between predictor and criterion variables (Mischel 1968; cf. Bem and Allen 1974). In this regard, Arthur (1971), reviewing studies of the

prediction of military performance, has stated that a prediction "sound barrier" exists, since "no matter how much information about the individual one adds to the predictive equation, one cannot bring the correlation coefficient between individual characteristics and prediction criteria much above about .40" (p. 544). This "sound barrier" remains unbroken by research on the prediction of violence.

An alternative to the dispositional or trait perspective in the mental health fields has arisen that offers a possible source of previously overlooked variables to include in prediction research. While the roots of the ecological perspective on human behavior have been planted for some time (e.g., Park 1925), it is only recently that this approach has been taken seriously in psychology (Kelly 1966, Moos and Insel 1974, Stokols 1977).

The ecological or environmental perspective on human behavior derives in part from a new appreciation of Kurt Lewin's (Lewin et al. 1939) dictum that behavior is a joint function of characteristics of the person and characteristics of the environment with which he or she interacts. Until recently, psychological and psychiatric research had focused almost solely on dispositional or person variables. The ecological approach attempts to right this imbalance by an emphasis upon situational or environmental variables, as they interact with personal characteristics. While environmental research of relevance to the topic of violent behavior has been initiated (Newman 1972, Monahan and Catalano 1976), there has as yet been no empirical attempt to apply the ecological or environmental perspective to the problem of prediction. This is despite the fact that there is coming to be widespread agreement with Moos's statement (1975a) that "to adequately predict individual aggressive behavior, one must know something about the environment in which the individual is functioning" (p. 13).

The use of environmental or situational variables in prediction differs from the use of personal or dispositional variables in at least one major way. In the case of dispositional variables, one has only to establish a relationship between the predictors and the criterion. Since the dispositional variables refer to fixed or relatively enduring characteristics of the person, one knows immediately whether any obtained relationship can be applied to a given case: An individual subject will not change from white to black, from male to female, or from 45 to 25 years old over the duration of the follow-up. In the case of situational predictors, however, one must establish both a statistical relationship between a given situation and violent behavior and the probability that the individual will in fact encounter that situation. One might, for example, predict with a high degree of accuracy that a given class of offenders

will resort to violent behavior when confronted with a situation they interpret as a challenge to their masculinity. To predict the actual occurrence of violent behavior, however, one would then have to perform a separate prediction concerning whether they will encounter such situations during the period under investigation.

It can be argued that the inclusion of situational variables is the most pressing current need in the field of violence prediction research. The principal factor inhibiting the development of situational predictors of violence is the lack of comprehensive ecological theories relating to the occurrence of violent behavior.

Moos (1973) has identified six different ways of conceptualizing human environments that have been used in previous research:

1. *Ecological dimensions,* including meteorological, geographic, and architectural variables;

2. *Dimensions of organization structure,* including staffing ratios and organization size;

3. *Personal characteristics of milieu inhabitants,* implying that the character of an environment depends upon the characteristics (e.g., age, sex, abilities) of those who inhabit it;

4. *Behavior settings,* defined by Barker (1968) as units with both behavioral and environmental components (e.g., a basketball game);

5. *Functional or reinforcement properties of environments,* suggesting that people vary their behavior from one setting to another principally as a function of the reinforcement consequences in the different environments; and

6. *Psychosocial characteristics and organizational climate,* in which the characteristics of an environment, as perceived by its members, are measured on various psychosocial scales.

Of these six extant conceptualizations of human environments, two (ecological dimensions and dimensions of organizational structure) appear not to be relevant to the prediction of individual violence, and another (behavior settings) is in an insufficient state of development to allow for its current application to the topic of prediction. The remaining three all provide guidance for the formation of environmental predictors of violence.

Conceptualizing environments in terms of the personal characteristics of milieu inhabitants might lead a researcher to inquire of the about-to-be-released prisoner or mental patient who he or she would be living, working, and recreating with in the post-release environment. The pooled base-rate probabilities of violence for these individuals

(given their age, sex, and prior history of violence, for example) should, according to this approach, relate significantly to the probability of violent behavior being committed by the ex-prisoner or ex-patient who enters the environment.

Emphasizing the functional or reinforcement properties of environments would lead the researcher to a behavioral analysis of the reward contingencies operating in the environments in which the predicted individual would be functioning. If, in a given environment, desired rewards (e.g., material goods, peer approval, self-esteem) can be obtained only by committing violent behavior, then the probability of violence in this environment would be high, according to reinforcement theory.

Finally, environments may be conceptualized for the purpose of prediction according to their psychosocial characteristics and organizational climate. According to Moos, this "social climate" perspective "assumes that environments have unique 'personalities' just like people. Personality tests assess personality traits or needs and provide information about the characteristic ways in which people behave. Social environments can be similarly portrayed with a great deal of accuracy and detail" (1975a, p. 4). He has devised a series of scales to measure the perceived social climates of prisons, hospital wards, community-based treatment programs, classrooms, military units, and families (1975a, 1975b). Common to all these scales are three basic dimensions of the environment: (a) relationship dimensions, such as the degree to which the environment is supportive and involving; (b) personal development dimensions, such as the degree of autonomy the environment provides; and (c) system maintenance and system change dimensions, including the degree to which the environment emphasizes order, organization, and control.

Drawing from Moos's extensive body of research, scales might be derived to describe the psychosocial environment in which a prisoner or mental patient is likely to return when released from an institution. For example, the relationship dimension could be operationalized in terms of items such as, "Is the individual likely to be returning to a parent or spouse, or will he or she be living alone? If the individual will be living with someone else, how likely is that other person to be supportive of a nonviolent lifestyle?" The personal development dimension might involve items concerning how likely the individual will be to attain a satisfying life-style (e.g., as the leader of a peer group) without resort to violence. System maintenance and dimensions of system change might be operationalized by estimates that the indi-

vidual will be employed in a satisfying job (Cook 1975, Witte 1976, Monahan and Monahan 1977).

It should be clear that these three methods of describing environments overlap greatly and that some situational predictor items would fit equally well under any of the three rubrics. It should also be clear that situational variables are being proposed for use in addition to, rather than instead of, dispositional variables in actuarial or clinical prediction schemes. It is the interaction of dispositional and situational variables that holds the greatest promise for improved predictive accuracy. Ideally, it eventually might be possible to make differential predictions of the sort that an individual with dispositional characteristics of type N would have X probability of violent behavior if he resided in environment type A, and Y probability if he resided in environment type B. But in order to reach this nirvana of prediction, it will be necessary for researchers to begin the arduous task of compiling and verifying a catalog of situations that relate to the future occurrence of violent behavior. The three nonexclusive approaches to conceptualizing human environments reviewed above could provide a framework for deriving specific predictor items that could then be applied to a cohort of prisoners or mental patients about to be released from institutions and validated during follow-up periods by the multiple methods specified previously.

V. CONCLUSION

We have examined the research to date on the prediction of violent criminal behavior and suggested several ways in which research in the future might improve upon it. The prediction of violence is an area of intrinsic scientific interest and policy importance as well.[7] In the latter regard, it is well to keep in mind that improvements in prediction technology can inform but not determine public policy. The risks must be borne by the false positives who languish in institutions and the victims of false negatives who lie in the streets. It is a rare prisoner who will accept with equanimity the explanation that he must be denied parole because the odds are one-in-three that he will be violent upon release. It is an even rarer victim of violent crime who will care to listen

[7]The policy implications of prediction research have been addressed in von Hirsch (1972), Dershowitz (1973, 1974), Wilkins (1975), Shah (1976, 1977), Wexler (1976), Fagin (1976), Dix (1976), and Monahan (in press [b]) in addition to the references cited previously.

to a treatise on the difficulty of predicting low-base-rate events. The task of research is to provide the most accurate estimates possible of the relative risks to the individual and to society of various procedures for predicting violence. Their weighting remains, as it must, in the political process.

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