RESEARCH INTO VIOLENT BEHAVIOR: OVERVIEW AND SEXUAL ASSAULTS

HEARINGS
BEFORE THE
SUBCOMMITTEE ON
DOMESTIC AND INTERNATIONAL SCIENTIFIC
PLANNING, ANALYSIS AND COOPERATION
OF THE
COMMITTEE ON
SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES
NINETY-FIFTH CONGRESS
SECOND SESSION

JANUARY 10, 11, 12, 1978

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THE PREDICTION AND CONTROL OF VIOLENT BEHAVIOR

Invited Testimony of

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United States House of Representatives
Committee on Science and Technology
Subcommittee on Domestic and International Scientific Planning, Analysis and Cooperation
Rep. James Scheuer, Chair

January 10, 1977
New York, New York

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I. THE PREDICTION AND CONTROL OF VIOLENT BEHAVIOR: INTRODUCTION

Despite William James' (1907) admonition that we cannot hope to write biographies in advance, American society spends a great deal of time, energy, and money attempting to identify today the individual who tomorrow will be violent. This identification of persons who reliably can be predicted to engage in violent or 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).

The purpose of this testimony is twofold: to provide the Committee with an overview of how violence predictions are being used in the criminal justice and mental health systems, and the state of the research on the accuracy of such predictions; and to suggest ways in which the Federal Government might support improvements in research on violent behavior and so form a basis for more informed policy choices. In making these recommendations, I would second Professor James Q. Wilson who urged that we "learn to experiment rather than simply spend, to test our theories rather than fund our fears" (1975, p. 208). If there is any area in the Federal Government where increased efficiency is more important than increased budgets, it is in the study of violent crime.

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 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 (cf Shah, 1976): (1) decisions whether or not to grant bail, and, if bail is to be granted, decisions on the level at which bail is set; (2) decisions whether certain offenders should be transferred from juvenile to adult court for trial; (3) sentencing decisions imposing probation, imprisonment, or death, and if imprisonment is imposed, decisions on the length of imprisonment; (4) parole decisions; and (5) decisions whether to involve special statutes dealing with "dangerous sex offenders," "dangerous mentally ill offenders," or "habitual" criminals (Monahan & 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 predictions of violence are clearly discernable. One is the increased reliance upon the "dangerous standard" as the primary or sole justification for civil commitment in the mental health system, with many states now following 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, Main) recently have passed bills to abolish or limit indeterminate sentences in which a prisoner's release date is determined by a parole board based in part based upon a prediction of his or her 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). This

"just deserts" approach also is incorporated in the Federal Criminal Code Reform Act of 1977 (S. 1437) now before Congress.

III. THE STATE OF THE RESEARCH ON VIOLENCE PREDICTION

It is necessary to understand the four possible statistical outcomes that can occur when one is faced with making a prediction of future behavior. Table 1 displays these outcomes. Once can either predict that the behavior, in this case, violence, will occur ("Yes") or that it will not occur ("No"). At the end of some specified time period one observes whether the predicted

If one predicts that violence will occur and later finds that, indeed, it has occurred, the prediction is called a True Positive. One has made a positive prediction and it turned out to be correct or true. Likewise, if one predicts that violence will not occur and it in fact does not, the prediction is called a True Negative, since one is making a negative prediction if violence and it turns out to be true. These, of course, are the two outcomes one wishes to maximize in making predictions.

If one predicts that violence will occur and it does not, this outcome is called a False Positive. If one predicts that violence will not occur and it does occur, it is called a False Negative. These two outcomes indicate inaccurate predictions, and are what predictors of violence try to minimize. A false positive prediction may result in a person's being confined in a prison or a hospital unnecessarily, while a false negative may mean that someone goes free to commit a violent act.

The eight major research studies which attempt to assess the accuracy of violence prediction are presented in Table 2.

Table 1

Four Possible Outcomes of Predictive Decisions

PREDICTED BEHAVIOR	ACTUAL BEHAVIOR: YES	ACTUAL BEHAVIOR: NO
YES	true positive	false positive
NO	false negative	true negative

Table 2*

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 & Jacoby (1974)	14.0	86.0	438	4
Cocozza & Steadman (1976)	14.0	86.0	96	3

*From Monahan, J. The prediction of violent criminal behavior: A methodological critique and prospectus. In National Research Council (Ed.) Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates. Washington, D.C.: National Academy of Sciences, 1978.

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 which 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 7000 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 assigned to a "potentially aggressive" category, and the rest to a "less aggressive" category. During a one-year follow-up, however, the rate of crimes involving actual violence for the potentially aggressive group was only 3.1 per 1000 compared with 2.8 per 1000 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 4000 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, and 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, Boucher, and Garofalo (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 five 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% of those predicted to be dangerous committed such as 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 2 out of every 3 predictions of dangerousness. (For an analysis of the methodological flaws of this study, see Monahan, 1973b, and the rejoinder by Kozol, Boucher, & Garofalo, 1973).

Data from an institution very similar to that used in Kozol et al.'s study 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 of these patients on the grounds that they were still dangerous and recommend the release of 135 patients as safe. The criterion measure was any new offense (not necessarily violent) appearing on F.B.I. reports during the first three 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 three 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 1000 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 & 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 (Rappport, 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-year follow-up of their transfer. Further, only 3% of Baxstrom patients were sufficiently dangerous to be returned to a hosptial for the criminally insane during four years after the decision (Steadman & 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 2-1/2 years of freedom, only 9 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 & 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:

> For every one patient who was under 50 years old and who had an LDS score or 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

> 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 (pp. 1013-1014).

The Supreme Court's Baxstrom decision promoted 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 & 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 four 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% being predicted to be dangerous and 40% 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% compared with 36%), this relationship was reversed for those rearrested for a crime after their release, with 49% of the dangerous group and 54% of the not-dangerous group rearrested. Predictive accuracy was poorest in the case of rearrest for a violent crime, "perhaps the single most important indicator of the success of the psychiatric predictions." Only 14% of the dangerous group, compared with 16% of the non-dangerous group, were rearrested for violent offenses. While these data

are susceptible to alternative interpretations (Monahan, 1977), the authors believe that they constitute, "the most definitive evidence available on the lack of expertise and accuracy of psychiatric predictions of dangerous" and indeed represent "clear and convincing evidence of the inability of psychiatrists or of anyone else to accurately predict dangerousness."

IV. CONCLUSIONS FROM THE RESEARCH

At least six conclusions from the research on violence prediction appear to be germane to the work of the Committee.

(1) The ability to predict who will engage in violent behavior is very poor.

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 true for both juveniles and for adults. It holds regardless of how well-trained the person making the prediction is--or how well programmed the computer--and how much information on the individual is provided. More money or more resources will not help. Our crystal balls are simply very murky, and no one knows how they can be polished.

(2) It is possible to identify persons who have higher-than-average (but still less-than-even) chances of committing violent crime.

While our ability to predict violence acts is not very good, neither is it completely nonexistent. The research discussed earlier provides us with several factors which, if present in a given person would raise his or her probability of committing a violent act above the base-rate or norm. It should be remembered that if one out of a hundred persons commits a violent act in a given year, a given person could be 49 times more likely than average

to commit a violent crime, and still have less than a 50-50 chance of being violent.

Chief among the characteristics which would affect the probability of a person's being arrested for a violent crime are his or her age, sex, race and socioeconomic status. Also relevant would be educational achievement, IQ, and residential mobility.

(3) The best predictor of future violent behavior is a record of past violent behavior.

If there is any consistency in the research, it is this: The probability of future violence increases with the frequency of past violence. It is certainly true that "not every child who commits an offense is teetering on the brink of a criminal career."[18] Wenk, for example, found that 19 out of 20 juveniles with a violent act in their history did not commit another violent act, at least in the first 15 months after release. It is not that past violence is a good predictor of future violence, it is merely the best predictor available. And if the research suggests that prediction is problematic even in the case of individuals with a history of a violent act, it is emphatic that prediction is foolhardy for persons without violence in their backgrounds. In the words of one psychiatrist who believes that violence can be predicted: "The difficulty invovled in predicting dangerousness is immeasurably increased when the subject has never actually performed an assaultive act . . . No one can predict dangerous behavior in an individual with no hisotry of dangerous acting out" (Kozol et al., 1972). This point can hardly be overemphasized in discussions of public policies to control violent crime.

(4) The poorest predictors of violent behavior are those that relate to psychological functioning.

With the possible exception of IQ, psychological variables have not proven to be particularly useful as prognosticators of violent behavior While Lefkowitz et al. (1977) did find positive correlations between a child's lack of identification with his or her parents, preference on the part of boy's violent television programs and father's upward social mobility, and later violence, these correlations explained only about 10% of the variance of adult aggression.

As Mischel (1968) noted in his classic review of psychological prediction:

> "A person's relevant past behaviors tend to be the best predictors of his future behavior in similar situations. It is increasingly obvious that even simple, crude, demographic indices of an individual's past behaviors and social competence predict his future behavior at least as well as, and sometimes better than, either the best test-based personality statements or clinical judgments."

No psychological test has been developed which can postdict, let alone predict, violence in either juveniles or adults (Megargee, 1970).

(5) Actuarial tables may be superior to clinical judgments in predicting violent behavior.

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. 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 such as age, number of prior offenses, etc., and the criterion of violent behavior. The prediction variables may include clinical diagnoses or scores on psychological tests, but these are statistical 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 Meehls'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.

While actuarial tables have not yet proven their superiority in predicting violent behavior in juveniles, the impression persists that clinicians have "taken their best shot" at prediction and that it has been so wide of the mark that the future lies with actuarial methods. (See below)

(6) One reason clinical prediction persists is that it allows socially sensitive predictor variables to be hidden.

If, after the commission of a violent act, the best predictors of future violence are simple demographic characteristics, and if actuarial tables may be more accurate than expert judgments, then why is there still such reliance upon psychiatric or psychological assessments of violence potential in the criminal justice system? Surely a judge is as capable as a psychologist to check off whether a person is male or female, black or white, 16 or 21 years

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old, rich or poor, how how many times he has moved? Why doesn't he or she just make explicit the variables being considered in the prediction and eliminate the psychiatric middle-man? In all likelihood the judge's prediction would be as good--or as bad--as the "expert's."

The reason that the predictive factors are not made explict seems clear. They are too socially "hot" to handle.

Assume for the moment that the four best predictors of violent behavior, after a violent act has been committed, are age, sex, race, and SES. Assume, that is, that these four factors, which do show up consistently in the research, are not merely artifacts of racist, sexist, ageist, or capitalistic biases in the juvenile and criminal justice systems, although such biases undoubtedly do exist to some extent and to that extent attentuate the strength of the correlation. Assume that, for whatever reason, the relationships still exist when the biases of the system are partialled out.

Can one imagine a judge, presented with two persons, one black and one white, who have committed the same violent act and who are comparable in all other respects, sentencing the black person to a longer period of detention than the white one, and admitting publicly that he or she was doing it because blacks have a higher actuarial risk of violent recidivism than whites? The Supreme Court would be quick to overrule such an appallingly "suspect" and unconstitutional prediction system, even if it could be shown to have some statistical accuracy. The same, one hopes, would be true if the prediction were made on the basis of socioeconomic status, with the poorer person dealt with more harsly precisely because he or she is poor, and poverty is statistically associated with violence.

The case is less clear with sex and age. If two persons comparable in all but their sex came before a judge, could the judge explicitly give more

lenient treatment to the female, because the actuarial table, like the insurance company tables, says that females are much less likely to recidivate than males? Or that 30 year-olds are less likely to commit another violent crime than 17 year-olds?

The "virtue" of clinical prediction is that a judge or parole board does not have to deal with these highly sensitive social questions, but can camouflage the issues by deferring to clinical expertise. The clinician is then free to take all these variables into account--indeed, must take these variables into account if the prediction is to be any good--and no one will be the wiser. The sensitive issues will never be raised because they are hidden in the depths of "professional judgment," while in fact that judgment is made on the basis of the same factors that might be unconstitutional if used in open court. In this sense, clinical prediction represents a "laundering" of actuarial prediction, so that the sensitive nature of the predictor variables cannot be traced.

A related reason for not putting our actuarial cards on the table is that it is unclear which way the deck should be cut. Some of the factors which lead to an increase in predictive accuracy also imply a decrease in moral culpability. If one used poverty or race as variables in a predictive/preventive scheme, for example, one would deal more harsly with the poor and the nonwhite. If, on the other hand, one were attempting to match the sanction not to a utilitarian calculus but rather to the moral desert or culpability of the offender, it could be argued that a history of adversity and discrimination should attenuate rather than exaccerbate the sanction. One cannot, in other words, maximize public safety and moral justice at the same time. The juvenile court is a good example of this. We deal more leniently with a 16 year-old violent offender than with a 50 year-old one, on the moral ground that the older man should know better and is more "deserving" of punishment,

while in fact the chances of violent recidivism are much higher in the 16 year-old. If our primary purpose was to prevent violent acts it is the juvenile, rather than adult, we would subject to lengthy incarceration.

V. WHY ARE PREDICTIONS SO INACCURATE?

To gain an adequate appreciation of the nature of the overprediction of violence by psychiatrists and psychologists, it may be worthwhile to speculate on the factors which lead to this unfortunate situation. Attempts to improve the accuracy of prediction may benefit from an analysis of the processes underlying overprediction. Seven factors are described below which might cumulatively account for the current state of the (in) validity of predictions of violence (Monahan, 1975).

1. Lack of corrective feedback. The legal or mental health official who erroneously assesses violence seldom has a chance to learn of his error and modify his subsequent predictions accordingly. Those predicted to be violent are generally incarcerated on the basis of the prediction, and thus there is little opportunity to confirm or disconfirm the judgment (Dershowitz, 1969; 1970). It is not difficult to convince oneself that the predicted offender would have been violent had the state not preventitively detained him. A lack of violence after release is attributed to the success of "treatment," rather than to the lack of anything to be treated in the first place.

2. Differential consequences to the predictor. If one overpredicts violence, the result is that individuals are incarcerated needlessly. While an unfortunate and, indeed, unjust situation, it is not one likely to have significant public ramifications for the individual responsible for the overprediction. But consider the consequences for the predictor of violence should he err in the other direction--underprediction. The correctional official or mental health professional who predicts that a given individual

will not commit a dangerous act is subject to severe unpleasantness should that act actually occur. Often he will be informed of its occurrence in the headlines ("Freed Mental Patient Murders Mother") and he or his supervisors will spend many subsequent days fielding reporters' questions about his professional incompetence and his institution's laxity (see the case described in Monahan, 1974a). "There may be no surer way for the forensic psychiatrist to lose power than to have a released mental patient charged with a serious crime in the district of a key legislator" (Steadman, 1972). Given the drastically differential consequences of overprediction (i.e., "type 1 errors") and underprediction (i.e., "type 2 errors") for the individual responsible for making the judgment, it is not surprising that he or she should choose to "play it safe" and err on the conservative side.

3. Differential consequences to the subject. The prediction of dangerousness may often be nothing more than a convention to get someone to treatment. If the ticket to secure involuntary treatment is a diagnosis of dangerousness, many psychiatrists and psychologists appear willing to punch it. Once in treatment, the assessment of dangerousness is forgotten (Rubin, 1972). Monahan and Cummings (1974), for example, have demonstrated in a laboratory context that individuals are more likely to be predicted dangerous when that prediction will lead to mental hospitalization than when it will lead to imprisonment. To the extent that states tighten their criteria for involuntary civil commitment from "need for treatment" to "dangerous to others," one should expect predictions of dangerousness to increase. Overprediction, therefore, may be less a comment on any lack of scientific acumen and more a testimony to the ability of officials to subvert

the intent of the law to accomplish what they think is "best" for the patient.

An alternate form of using the prediction of dangerousness as a ploy for other purposes is suggested by the Morris and Hawkins (1970) observation that when dangerousness is invoked, it often is for retributive purposes. There are some, e.g., "mentally disordered sex offenders," for whom the law requires "treatment" rather than "punishment" (Kittrie, 1971). By diagnosing such persons as dangerous, however, one may satisfy tacit retributive demands by insuring that the treatment they receive will involve at least as much incarceration as punishment would have. Foote puts it more stongly: he holds the concept of dangerousness to be "devoid of meaningful content and a convenient handle for political repression" (1970, p. 8).

4. Illusory correlation. An illusory correlation is a type of systematic error of observation in which observers report seeing relationships between classes of events where no relationship actually exists (Chapman and Chapman, 1969). Sweetland (1972) has demonstrated how this phenomenen influences the assessment of dangerousness. Psychiatrists were surveyed to determine which personality traits they considered to be most characteristic of dangerous and nondangerous persons. Following this, naive subjects were asked to examine personality descriptions which were made up of these characteristics and which were paired with the diagnoses "dangerous" or "nondangerous." In one condition of this study, a zero correlation was present between the items designated by the psychiatrists as indicating a dangerous person and the diagnostic formulations with which these items were paired. Subjects were asked after the presentation to describe what they had observed. The results indicated that even when

there was a zero correlation, the subjects responded as if they had observed a relationship in the materials. They consistently recalled that certain of the characteristics had appeared more frequently with the diagnosis of "dangerous," when, in fact, they were uncorrelated. These systematic errors of observation were consistent with the subjects' prior expectations about which characteristics implied dangerousness.

The poor ability of mental health professionals to predict violence, therefore, can be partially explained by their reliance upon stereotypic prior expectations as to what constitutes a predictor of violence, rather than valid correlations. Predictor variables which, in fact, bear no relationship to violence will continue to be used, because those who believe in them will find (illusory) support for their beliefs by selectively attending to the data: they will see only what they wish to see. The relationship between violence and mental illness, for example, appears to be an illusory correlation (see below).

5. Unreliability of the criterion. We have already noted the plethora of definitions which have been advanced for the designation of a violent act. In addition to the handicap of definitional vagary, research on the prediction of violence is actually research on the ppediction of discovered and reported violence. Undetected violence and police discretion in certifying acts of violence necessarily decrease the reliability of the event being predicted. "The problem, then, is this: Most of the violent behavior we would wish to predict probably never comes to our attention, and the part that does is far from a representative sample" (Wenk et al., 1972, p. 401). A prediction of violence may itself be reactive--it may influence the later certification of a violent act. Those at whom a finger has been pointed may be scrutinized more carefully than others, and the prophecy may thus fulfill itself.

6. Low Baserates. A vexing statistical problem further complicates the prediction of violence. The problem has to do with the low baserates

of violence in society, e.g., an annual murder rate of 8.9 per 100,000 (Kelley, 1973).

If the baserate of an event is high, predicting that event without many false positives is relatively easy. If nine out of 10 people commit murder, one could simply predict that everyone will commit murder and be correct 90 percent of the time. As the baserate become lower, however, the problem of false positives becomes more salient. Livermore, Malmquist and Meehl (1968) address themselves to this problem in discussing dangerousness as a criteria for involuntary civil commitment.

> Assume that one person out of a thousand will kill. Assume also that an exceptionally accurate test is created which differentiates with 95 percent effectiveness those who will kill from those who will not. If 100,000 people were tested, out of the 100 who would kill, 95 would be isolated. Unfortunately, out of the 99,900 who would not kill, 4995 people would also be isolated as potential killers. In these circumstances, it is clear that we could not justify incarcerating all 5090 people. If, in the criminal law, it is better that ten guilty men go free than that one innocent man suffer, how can we say in the civil commitment area that it is better that 54 harmless people be incarcerated lest one dangerous man be free? (p. 84)

7. Powerlessness of the subject. Finally, the gross overprediction of violence may be so easily tolerated because those against whom predictive efforts are mounted are generally powerless to resist. Prisoners or mental patients (who became or remained such due to overprediction) are unlikely to arouse a public outcry in their defense. As Geis and Monahan (1976) have recently put it:

> The persons involved as patients or prisoners almost invariably are located in social positions where they do not have adequate political or financial resources to protest effectively against what is being done to them. That is, they lack things such as ready media access and funds to hire good lawyers . . . If society's aim is really to isolate the violent and the violence-prone and protect the innocent, then why are those who allow faulty fuel tanks to continue to be installed in the planes they market, and those who are or ought to be responsible for things such as an unconscionably high national infant mortality rate (Gross, 1967, p. 24) not similarly 'diagnosed' and 'rehabilitated?'

VI. METHODOLOGICAL PROPOSALS TO IMPROVE VIOLENCE PREDICTION RESEARCH

The conclusion that violent behavior is vastly overpredicted is shared by virtually all 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' (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' 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 which, if successfully implemented, might improve the ability to predict violence to a point where it could provide useful information to policy decision-makers. The ensuing discussion will consider the criterion variables which define violent or dangerous criminal behavior and the predictor variables which 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 mulitiple definitions of violence.

Proposal One: Violence should be defined in a hierarchy including (a) the four F.B.I. 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 was a consensus among either the public or professional groups as to what behaviors should be counted as dangerous. Unfortunately, no such consensus exists (Monahan & 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 hierarchial definitions of the criterion, each succeeding one more inclusive than the one which came 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 which 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 (Weiner & 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 truism. 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 initially specify 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 of a somewhat more inclusive 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 F.B.I., is restricted to (a) murder, (b) forcible rape, (c) robbery, and (d) aggravated 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 & 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:

> "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" (1976, p. 5).

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 & 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[5] 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. Russel, 1960), and if the validation researcher is limiting his or her definitions of dangerousness to the F.B.I. 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 defintions 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 predicting violence, or to provide the clinicians with the definitions to be used in the follow-up and have them predict to those definitions. Given the need for consistency across different prediction studies, as well as within each prediction study, the latter alternative would appear 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) one year, (b) three years, and (c) five 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 five studies using a 3-5 year follow up had false positive rates of 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 one year, three year, and five year intervals would appear 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 civil commitment rates to mental hospitals; and (d) all of the above, and 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 which 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 (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 the "clearance rate" of reported crime (i.e., the percentage of reported crime which 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 it's way into police statistics (Kelley, 1974).

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 & 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 (Jacooson, Craven & Kushner, 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 much, 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. Sole reliance 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 which do occur. 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

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with data on civil commitments to mental hospitals, where discretion as to the definition of violence and the procedures for certifying its occurrence is great (Monahan, 1973a; 1973b; in press).

Additional validation procedures are needed which do not rely upon the official statistics which so underrecord violent behavior. One such procedure is self-report. Self-report methodologies have been used extensively in the study of delinquency (Hirshci, 1969) and might fruitfully be applied 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 violence-potential 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 behavior would appear appropriate in future research. A hierarchy of validation procedures beginning with convictions, and then subsequentially adding arrests, mental hospital commitments, 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 such as age, number of prior offenses, etc., 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 four clinical studies have reported substantially better predictions than the three actuarial ones. While several confounding factors make this 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 clincians 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 & Emrich, 1972) rather than 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, and 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, Gottfredson, Wilkins, and Pasela (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 parolled or continued in prison. They found that the correlation between statistical risk estimates based on the actuarial tables and the board's clinical risk estimates was .74 when the actuarial tables were presented to board members before they made their clinical judgments, and .53 when the tables were not provided. The correlation between risk estimates and the outcome of the parole decision was .30 when the actuarial tables were provided and .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, to influence clinical predictions to look more like actuarial ones could result in <u>lowered</u> predictive accuracy in the case of violent behavior. This is especially true in light of the fact that Hoffman <u>et. al.</u> (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 rapproachment between clinical and actuarial prediction lies in the construction of actuarial models of clinical decision-making.

Along these lines, Gottfredson, Hoffman, Sigler, and Wilkins (1975), relying upon a study which 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 developed guidelines concerning the mean sentence served for each severity/risk level. These guidelines were presented to the parole decision-makers, as they were reviewing cases, and they 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% of the parole recommendations were within the guidelines presented.

Creating actuarial models of the clinical decision-making process in prediction of violent behavior could have two advantageous effects: (a) It would make explicit the variables used in clinical decision-making. These variables could then be incorporated in their own account into actuarial models so that their predictive accuracy could be <u>independently</u> assessed; and (b) 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 to always respond to similar information in similar ways:

> "[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." (p. 423).

Goldberg (1970) 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 (determined 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 which 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 at predicting 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 virture 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 & 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 which 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 & Insel, 1974; Stokols, 1977).

The ecological or environmental perspective on human behavior derives in part from a new appreciation of Kurt Lewin's (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, 1971; Monahan & 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' statement that "to adequately predict individual aggressive behavior, one must know something about the environment in which the individual is functioning" (1975a, 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 which 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.

(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 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 would be living, working, and recreating with in his 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 psychosocial prediction according to their 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' 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 non-violent lifestyle?" The personal development dimension might involve items concerning how likely the individual will be to attain a satisfying lifestyle (e.g., as the leader

of a peer group) without resort to violence. System maintenance and system change dimensions might be operationalized by estimates that the individual will be employed in a satisfying job (Cook, 1975; Witte, 1976; Monahan & Monahan, in press).

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 envrionment 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 which relate to the future occurrence of violent behavior. The three non-exclusive approaches to conceptualizing human environments reviewed above could provide a framework for deriving specific predictor items which 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.

There are three topics in the prediction and control of violent behavior which I believe have not yet been sufficiently researched and which hold promise, in my opinion, for increasing our understanding of violence and our policies for coping with it. The topics have to do with the relationship between violent crime and unemployment, with short-term "emergency" predictions of violence, and with violent consequences of corporate decision-making.

VII. RESEARCH PRIORITY I: VIOLENT BEHAVIOR AND UNEMPLOYMENT

As noted above, a prime candidate for a "situational" or environmental variable affecting crime is employment and the lack of it. Support for the proposition that the work environment is related to recidivism comes from several studies. Glaser (1964) interviewed, at monthly intervals, a sample of 135 parolees released from federal institutions in 1959 and 1960. In comparing the job-holding activity of the men who completed parole with that of men returned to prison, he found the eventual successes acquired their first jobs sooner and, during the initial period of parole, earned a higher monthly income than the eventual recidivists.

Cook (1975) studying 327 male felons released from Massachusetts prisons in 1959 found that 65% of those who held a "satisfactory" job (defined as a job which lasted one month or more) during the first three months of parole were eventually successful in completing an 18-month parole period compared with a 36% success rate among those

who did not have a satisfactory job during the first three months. Seventy-five percent of parolees holding a satisfactory job during the second three months of parole were eventual successes, compared with 40% of those who did not hold such a job. Eighty-nine percent of those having a satisfactory job at the end of the first year on parole completed the parole period without revocation, while only 50% of those not satisfactorily employed were successful at doing so.

Cook also found that while steady job-holding was related to parole success, too frequent job-changing increased the likelihood that a parolee would recidivate. The probability of recidivism during the second three months on parole increased monotonically with the number of jobs held during the first three months, from 11% recidivism when one job was held to 43% when five jobs were held.

To control for potential confounding factors in his results, Cook included job satisfaction and numerous other variables in a multivariate regression equation. He found that for the given sample, recidivism depended significantly on the parolee's age and the extent of his criminal record, but not on his race, length of prior prison term, I.Q., education, prior occupation or marital status. In all cases, the probability of recidivism also depended upon job satisfaction, with the size of the relationship varying with the parolee's age and prior record. For offenders with

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one prior prison sentence, aged 26-35, for example, the probability of success during the last six months of parole was .97 if they held a satisfactory job during the previous six months and .44 if they did not.

Reviewing the literature on job discrimination against former convicts, Cook concluded that there was little evidence that ex-offenders cannot find jobs. Rather, the high unemployment rate among parolees seems to be accounted for by their inability to find good jobs. "The labor market severely limits the quality of their opportunities but not the quantity." Cook found that wage rate was directly related to job tenure. "Roughly speaking, it appears that an additional $7 per week in wages is associated with an additional month of job tenure." He concludes:

> Many releasees do not work steadily because they cannot find a job attractive enough to hold them for more than a few months. This suggests that public programs designed to place released offenders in jobs similar to those they can find now -- washing dishes, pumping gas, working as low-wage factory laborers -- are not going to have much impact. . . . Better quality, not quantity, of available jobs may be what is needed. (p. 26)

Such a conclusion might be interpreted as supporting the expansion of vocational training programs in prison.

Yet the experimental evaluations of such programs in terms of their impact upon recidivism have been uniformly negative. These findings, however, do not refute the hypothesized relationship between job opportunities and recidivism, since the manpower training projects typically have failed not only to decrease their clients' recidivism rate, but also failed to increase their employment rate and income. That is, the independent variable -- employability -- has not been successfully manipulated. Most of the trainees did not find the jobs for which they were trained.

Cook notes that the principal alternative to the manpower training approach is "job creation coupled with on-the-job training; instead of using training as an attempt to facilitate job placement, it becomes part of the job itself, and other means are used to facilitate placement," (p. 30) such as special public employment programs or government subsidies to private employers.

The results of one experiment to test the job creation approach are available. In this study, 173 youthful parolees were placed in semiskilled production jobs paying up to $4.17 an hour. In one condition, supervisors were trained in social reinforcement techniques. They were instructed "to acknowledge verbally, in a rewarding manner, any and all improvements in an employee's job performance. Coercive or otherwise negative comments were to be eliminated as much as possible." In the second condition, a counselor provided social reinforcement off the job, while

the parolee worked under an untrained supervisor. A third group received social reinforcement both on and off the job, and a fourth group, the control, received no social reinforcement. As reported by Cook:

> Mentec's principal finding was that [Social Reinforcement] is effective in improving the job performance of parolees when applied by job supervisors (but not when given off the job). When compared with others, the parolees receiving [Social Reinforcement] from supervisors were superior with respect to productivity, quality of work, absenteeism, and tardiness. Compared to the control group, the experimental groups in general were characterized by a substantially higher employment rate and longer job tenure. (p. 31)

Data relevant to the proposition that job experience affects recidivism are also presented by Witte. (1976) She used an econometric model to study the post-release activity of 641 men imprisoned in North Carolina in 1969 and 1971. A mean three year follow-up period was used. Complementing Cook, she found that "higher legal wages tend to decrease the expected number of arrests or convictions," but in her study this effect was significant only for "non-serious income offenses," such as larceny and violations in liquor sale. She hypothesizes that the reason no significant relationship was found between wage level and "serious income crime" (such as robbery) was that the most important

variable determining the arrest and conviction rate for serious income offenders was whether or not they were drug users. "The high correlation between drug use and serious income offenses is well known to criminal justice authorities but makes testing the economic model of crime for this group extremely difficult. Apparently the need for money for drugs largely eliminates the rational decision process which the rational model of crime assumes." Not only wage rates, but the probabilities of arrest, conviction, and imprisonment, and the expected prison sentence fail to significantly affect this group.

Finally, a study recently released by the U.S. Department of Labor (1977) further corroborates the relationship between employment and recidivism. Four hundred and thirty-five males released from prison in Baltimore, Maryland, between 1971 and 1974 were the subjects. While past-release employment showed no relationship to the commission of crimes other than theft, the effect of employment on theft during the first year of release was dramatic: men who worked 10 or more of the first 13 weeks following release had an arrest rate for theft of 19 percent; those who worked 1 to 9 weeks had an arrest rate of 25 percent; and those who did not work at all had an arrest rate of 32 percent. The researchers note the inherent ambiguity in interpreting such correlations: "Do the income and social stability provided by the job remove the incentive to commit crimes of theft? Or are both employment and recidivism related to a third factor--some personal characteristic like 'maturity' which accounts for both?" In the context of the other studies reviewed alone, there is no reason why the former and more optimistic interpretation should not form the basis of policy, at least on a provisional basis.

One might summarize the findings on the effects of job factors on recidivism by the following propositions. There is tentative evidence that (a) job-holding activity is negatively related to the probability of criminal recidivism; (b) the quality of the job held is a major determinant of job tenure; (c) job placement and social support on the job may affect the perceived quality of employment; and (d) the above propositions may not apply to offenders using narcotics.

The data just reviewed point to the importance of an individual's employment situation as a factor predicting his or her return to crime. Data from another source emphasizes the importance of the societal unemployment rate on the prevention of violent behavior.

In testimony before the House Judiciary Committee's Subcommittee on Crime, in September 1977, Professor M. Harvey Brenner documented the relationship between crime and the unemployment rate.

> "Admissions to prisons and the homicide rate . . . vary with unemployment according to several studies. For the period 1926-62, admissions to state prisons and the homicide rate for the entire United States and for New York State were positively correlated with the unemployment rate. In the State of Georgia, the prison population was similarly observed to change with fluctuations in the unemployment rate during 1967-74."

Brenner found that a 1% increase in the 1970 unemployment rate was related to a 4% increase in the homicide rate, a 6% increase in the robbery rate, and a 5% increase in the admission rate to state prisons.

With preliminary data such as these, the relationship of unemployment to violent crime would appear a top priority for further study.

VIII. RESEARCH PRIORITY II: VIOLENT BEHAVIOR AND CIVIL COMMITMENT

Rarely have research data been as quickly or universally accepted by the scholarly community as those supporting the proposition that mental health professionals are highly inaccurate in predicting violence. Since Dershowitz' conclusion in an influential 1969 article that "for every correct psychiatric prediction of violence, there are numerous erroneous predictions" (1, p47), psychiatrists, psychologists, and lawyers have increasingly come to accept the gross inaccuracy of violence prediction as a scientifically established fact (2-6). Indeed, it appears as though the only people who still believe that accurate predictions of violence are possible are those mental health professionals who make their living at such tasks, and the courts which base proceedings for institutionalization on predictive judgments. Even there, the tide appears to be turning.

The conclusion that psychiatrists and psychologists "have absolutely no expertise in predicting violent behavior" (5, p734) has led some to call for the abolition of all forms of preventive intervention which are predicated upon a clinical prediction of future violence (3, 7, 8).

The purpose of this paper is to suggest (a) that a careful reading of the prediction research reported to date does not support the unqualified conclusion that the accurate prediction of violence is impossible under all circumstances, or that psychiatrists, psychologists, or others will invariably overpredict its occurrence by several orders of magnitude; and (b) that there are theoretical reasons why one could expect that one set of circumstances--those which typically apply in the short-term emergency commitment of mentally ill persons predicted to be

imminently violent--may be exempt from the systematic inaccuracy found in the current research. Since there are no data whatever on the accuracy of predictions made in emergency situations, the debate on emergency commitment must proceed on theoretical rather than empirical grounds, until such time as relevant data are available. If this argument has merit, then proposals for "the abolition of involuntary mental hospitalization in any form--long-term or emergency" (7, p.445) cannot legitimately adduce the existing prediction data in their support, and the empirical basis for emergency commitment in an open question, rather than the dead issue many are now presuming it to be.

The basis for the lack of confidence in the ability of psychiatrists and psychologists to predict violence is a large and growing body of research dramatically demonstrating that when a group of prisoners or mental patients who have been predicted to be violent are nonetheless released into the community, the majority and frequently the vast majority of these prisoners and patients are not found to commit the violent behavior expected of them (9-15). The persistence of this finding is itself remarkable: no study has ever found prediction to be more accurate than inaccurate. As I have noted previously, "the literature has been consistent on this point ever since Pinel took the chains off the supposedly dangerous mental patients at La Bicetre in 1792, and the resulting lack of violence gave lie to the psychiatric predictions which had justified their restraint" (16, p.21).

While the major prediction studies differ from each other

in many respects, most (9-13) conform to the following methodological pattern:

(a) Individuals were institutionalized. This could have been on the basis of a criminal or juvenile arrest or conviction or a determination that an individual was a "mentally ill offender," "defective delinquent," or "incompetent to stand trial."

(b) In the institution, predictions were made that a group of these individuals would be violent if released into the community. It is important to note that it was these predictions, made in the institution (jail, prison or hospital), which were being tested in the research, and not the predictions which may have occasioned the original institutionalization.

(c) The group predicted to be violent if released into the community was in fact released into the community. This often occurred by virtue of a judicial order or parole board action. A "natural experiment" was thereby created.

(d) The group predicted to be violent was monitored for a number of years in the community as to their actual performance of violent behavior. This was accomplished by checks of police and (occasionally) mental health records.

(e) Low frequencies of violent behavior were recorded, thereby revealing the inaccuracy of the predictions. No violent behavior was noted on the community records of between 54 and 99 percent of the persons who had been predicted to be violent. Other studies (e.g., 15)

compared groups predicted to be dangerous and those predicted not to be dangerous, and found no differences.

What was tested in these studies? The most reasonable interpretation is that they tested predictions made in an institution of violence to occur in the open community. Persons who, for whatever reason, had been institutionalized for a substantial period of time (a mean of 15 years in one study (9), and not less than several months in any other), were predicted to engage in violent behavior if released into the open community. They were eventually released and most were not violent.

While it is true that some studies (e.g., 15) included violence in the hospital as part of their criterion, the fact that "potentially violent" patients were likely to be medicated makes it unclear whether a lack of violence in the hospital reflects predictive inaccuracy or simply the pharmacological suppression of violent tendencies.

Rather than demonstrating that all forms of violence prediction are "doomed" (as I have previously stated, 16) a more discerning reading of the existing research suggests that it demonstrates the invalidity only of predictions made in one context that an individual will be violent in another, very different, context. The context of prediction in the existing research is a closed institution in which the individual has resided for a significant period of time (several months to several decades). The context of validation is the open community.

There is an enormous body of research which would lead one to expect that the correlation between behavior predicted

in one context and observed in another would be low (17-20). Since Hartshorne and May's finding in 1929 that the assessment of "moral character" was specific to the context in which it was measured, scores of investigations have reluctantly concluded that the cross-situational consistency of any type of behavior rarely exceeds the "sound barrier" (21) of a .40 correlation coefficient. As Mischel has noted, "findings demonstrating the specificy of the interactions between persons and situations constrain how broadly we can generalize from an individual's behavior in any one situation to his reactions under different conditions. . . . Predictive validity tends to decrease as the gap increases between the behavior sampled on the prediction measure and the behavior that is being predicted." (17, p323)

It is precisely this "gap" which exists in the current research on violence prediction. Jails, prisons and mental hospitals in which predictions are made differ in obvious ways from the open community situations which are the truest test of predictive validity. This point is underscored by the fact that institutional performance has little effect on post-institutional (community) behavior (e.g. 9). It is exacerbated by the fact that substantial time periods intervene between the point that the institutional prediction is made and the community validation is undertaken, and/or between the most recent exposure to the community context in which the prediction will be validated and the point at which the institutional prediction is made. In the former case, there is too much opportunity for the individual or the environment to change in unknown ways before the prediction is tested. In the latter case, the information on how the person

behaves in the open community is made obsolete by the unknown changes that have occurred since he or she was institutionalized. As Mischel notes, "the assessor who tries to predict the future without detailed information about the exact environmental conditions influencing the individuals criterion behavior may be more engaged in the process of hoping than of predicting" (17, p140). It is the relative absence of current knowledge about the "exact environmental conditions" which are operating in the community context in which the individual will be functioning which relates long-term institutional predictions to the realm of whimsey.

To be sure, these are not the only reasons why violence has been inaccurately predicted under the circumstances investigated (16). But they may help to account for the degree of inaccuracy which has been observed, and may serve to differentiate the type of prediction that has been tested and found wanting from another type which has yet to be investigated. That type is the prediction of imminent violence typically made in the short-term emergency commitment of the mentally ill.

In emergency commitment, a person residing in the open community is brought to the attention of a mental health professional, usually by a family member, friend, neighbor, or police officer, for a determination of whether he or she is mentally ill and a prediction of whether he or she will engage in violent behavior in the immediate future. A positive diagnosis and prediction results in the short-term "emergency" confinement of the person in a mental health facility.

Note the differences between emergency commitment of this

sort and the kinds of prediction investigated in the research discussed earlier. In emergency commitment: (a) the context of prediction is the same as the context of validation. A prediction is being made in the open community that a person will be violent in that same context. Often a prediction is made in a room in a home that the person will shortly be violent in the same room; (b) the time between the point of prediction and the validation period is very short. Frequently the prediction is that the person will be violent in a matter of minutes or hours; and (c) since the prediction is being made in the same context in which it will be validated, there is little time intervening between the most recent exposure to the context of validation and the point of prediction. The prediction is made immediately after observing how the person behaves in the context in which the prediction would be validated. The information available to the predictor is thus fresh and current.

In emergency commitment, unlike the legal procedures studied in the current research, the situational and temporal "gap" between the behavior used as a predictor and the outcome that is being predicted is small. One is directly sampling actions, e.g., threatening words and gestures, that are "as similar as possible to the behavior used on the criterion measure" (17, p323), e.g., fulfilled threats. In violence as in other areas, it is potentially true that "predictions about individual behavior can be generated accurately from knowledge of the environments in which the behavior occurs" (17, p164).

Given these factors, it would appear that there is a qualitative difference between predictions of violence made in

the community for the prupose of short-term emergency commitment and those reported for longer-term institutionalized patients and prisoners. Research on the abysmal failure accurately to predict violence in the latter situation cannot reasonably be extrapolated to a similar conclusion in short-term emergency commitment cases.

The validity of the existing body of research on violence prediction, or many of the policy implications which have legitimately been drawn from it (e.g., 22-25) are not at issue. There is no question that long-term predictions of violence, or predictions from institutional to community settings, are grossly inaccurate. I am merely restricting the range of predictive decision-making situations to which the currently available research reasonably can be said to apply.

I know of no data substantively relevant to the question of predictive accuracy in emergency commitment situations. The empirical question, therefore, is an open one. It is not capable of being resolved by recourse to the currently existing body of research on violence prediction. There are theoretical considerations, discussed above, which suggest that predictions made under the conditions which typically apply in emergency commitment situations should be better than those made in the institutional settings studied to date. But whether they are in fact better, and, if so, how much better, is not now known. There is no a priori reason to assume that psychiatrists or psychologists would be any better at prediction in emergency situations than other observers or participants (e.g., a police officer or a potential victim). The unresolved question is

whether they would be equally accurate or equally inaccurate (cf. 15). Undoubtedly, some degree of overprediction will inhere in predictive judgment of any sort. In emergency commitment situations, some protagonists will be "bluffing" in their actions or for whatever reason decline to carry through on their threatened violent behavior. Some predictors will rely upon illusory correlations or take extraneous factors, such as a belief that treatment would be beneficial, into account in making their predictions (26).

Indeed, it is difficult to conceive of a design for a study which ethically could put predictions in emergency situations to the test, since the situations are defined as "emergency" ones presumably in need of immediate and decisive action. Could one ethically decline intervention in a random half of the cases in which a mental health professional has both diagnosed mental illness and predicted imminent violence and return the next day for a body-count validation, as has recently been suggested (27)? Lacking such a study, one is left with only prudent judgment to assess the accuracy of short-term predictions of violence in emergency commitment situations.

If research should bear out the theoretical position argued here and reveal emergency predictions to be more valid than those previously studied, what relevance would this have for the legal process in emergency commitment? By the point at which a full hearing is held on the issue of dangerousness (two weeks after initial confinement, according to Lessard v. Schmidt standards), the "emergency" nature of the prediction may well have dissipated. Enough time may have elapsed and the psychological

context may have changed sufficiently by the time of the hearing that the pattern of over predictive inaccuracy repeatedly demonstrated by Steadman and others may have set in. While the position argued here would offer no solice to the psychiatrist or psychologist confronted with predicting dangerousness for the purpose of prolonged institutionalization, it would be relevant to justifying the initial "emergency" period of confinement, that is, the two-week period before the full hearing. Predictive accuracy may end at approximately the same point in time that full legal protection begins. If so, emergency commitment of those diagnosed as mentally ill and predicted to be dangerous may be limited on both empirical and legal grounds to a very brief period.

IX. REASEARCH PRIORITY III: CORPORATE VIOLENCE

Little argument can be mounted against the view that violent street crime represents a serious assault on the right of citizens to be protected from harm. However, the argument can be made that there are neglected forms of violence which also represent a threat to the personal integrity of innocent citizens. People are quite as dead if they are killed by smog, defective automobiles, negligence in the factory, or other forms of industrial and corporate malevolence as they are if done in by an armed robber.

This dictum, patently obvious once stated, nonetheless takes on particular importance when considering the consummate neglect by social scientists of corporate violence by forces and persons otherwise regarded as "legitimate" and "successful" members of the social system. Such neglect has far reaching consequences, including the fact that it perpetuates a growing belief that behavioral science operates in behalf of established

power groups rather than in behalf of an ethic of impartial assessment and scientific objectivity.

The dominant concern in community interventions and experimental studies has been with individual or personal violence. Attention almost exclusively has been focused on violence as a conspicuous transaction between two or more persons. Yet a convincing case can be made that most presentable death and injury in this country is occasioned by actions more subtle than family quarrels or liquor store robberies. It will be argued that violence resulting from corporate decision-making is as much a threat to the public safety as street violence or family violence. Strategies will be proposed whereby social scientists may investigate and consequently affect corporate processes which result in large-scale death and injury.

Corporate Violence

Corporate violence is defined as behavior producing an unreasonable risk of physical harm to consumers, employees, or other persons as a result of deliberate decision-making by corporate executives or culpable negligence on their part. To apply the term "violence" to such acts, as we are doing, is neither an exercise in metaphoric overkill nor a dilution of the concept of violence. Rather, it is an attempt to redefine the boundaries of the term "violence" to include phenomena that properly should come within its purview. Instances of harm-doing by corporate institutions have escaped description as violent for at least three reasons: (1) the anonymity involved in corporate actions, (2) the sequencing of the harm-doing which has sheltered corporate perpetrators from direct association with the injurious consequences of their act, and (3) the fact that corporate violence does not lend itself easily to observation by social

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scientists. In this regard, their exclusion from categorization as violent has been a matter of convenience rather than one of logic.

The traditional behavioral science focus on personal violence is not surprising in view of some of its intrinsic characteristics. It is precisely this kind of violence that the individual citizen fears. Such violence is most commonly associated with an identifiable perpetrator of harm-doing. Corporate violence is more subtle and less conspicuous. As long as violence is defined in terms of conspicuous transactions between persons, the harm-doing performed by social institutions and their agents is obscured.

Corporate violence may be viewed as a form of "white collar crime" (Sutherland, 1949; Geis, 1968; Geis and Meier, in press). Sutherland coined that term in his classic analysis of the history of 70 of the 200 largest corporations in the United States. These companies had been convicted of an average of 14 crimes each, including restraint of trade, infringement of patents, and unfair labor practices. Due to inadequacies in reporting practices, these figures are surely a gross underestimate of corporate crime. Baumhart (1961), in a survey of 1,700 corporate executives, reported that a majority believed that businessmen would violate ethical standards if they thought detection could be avoided. When the respondents were asked to rank each of five factors (company policy, industry climate, behavior of superiors, behavior of equals, and personal codes) for their influence on executive decisions, they were most likely to attribute ethical decisions to personal codes of behavior and unethical decisions to the influence of superiors and industry climate. When asked if they knew of unethical practices in their industry,

four out of five executives affirmed the presence of generally accepted practices which they considered unethical (see also Lane, 1953). While these findings refer to corporate crime in general, rather than specifically to corporate violence, they provide insight into the ethical climate of American industry.

No reliable statistics exist about forms of corporate crime which result in violence. The principle source of crime data in this country, the F.B.I.'s Uniform Crime Reports, ignores corporate violators. It is estimated, however, that two hundred thousand to five hundred thousand workers annually are needlessly exposed to toxic agents such as radioactive materials and poisonous chemicals because of corporate failure to obey safety laws. And an unknown but undoubtedly significant portion of the 2.5 million temporary and 250,000 permanent worker disabilities from industrial accidents each year are the result of managerial acts that represent culpable failure to adhere to established standards (Geis, 1973, p. 183). A likewise unknown portion of the approximately 50,000 deaths each year on the highway are the result of faulty automobile manufacturing.

Virtually the only form of investigation of corporate violence has been the case study. Geis and Monahan (1975) report that between 1964 and 1968, thirteen persons were killed in crashes of a light aircraft whose fuel tank construction was faulty. The aircraft manufacturer was informed of the defect at least three years prior to the first fatal crash. This initial warning was supported by follow-up tests and by customer accounts of fuel mechanism inadequacies that produced hazards in flight. Rather than repair the planes, the company gambled that

crashes would be attributed to pilot error. Similarly, a propellar defect that caused a 1967 crash of an airplane in Ohio killing 38 persons was known to the manufacturer, the Allison Division of General Motors, but the company chose to advise no one of the problem (Johnson, 1972; Mintz & Cohen, 1971). Franklin (1969) reports that the mine involved in the 1968 West Virginia disaster had failed all 24 inspections by the Bureau of Mines in the previous five years, and was cited for 25 safety violations in the two years prior to the tragedy. Despite the profitable technological advances in the extraction of coal, the performance of the coal industry with respect to disaster prevention has been callously inhuman. More than 120,000 men have died violently in coal mines during the last century, excluding deaths from disease. Brodeur (1974) details dozens of other such instances of fatal corporate negligence.

The most famous case study of corporate violence is Ralph Nader's (1965) Unsafe at Any Speed. Nader accused automobile manufacturers of building lethal cars and concealing their knowledge of death-dealing defects from the public. The roster of vehicular defects linked to passenger injuries is indeed appalling. Among the most vicious have been rear wheel tuck-under in the 1960-63 Corvairs, brake failure in the 1953 Buick Roadmaster, weak rear suspension arms in 1965 Fords, faulty steering gear brackets in 1965 Chryslers, and original equipment tires that are highly susceptible to overload. These items are only a portion of a shocking catalogue of hazardous automobile equipment sold to the public.

Even more astonishing have been the nefarious decisions by manufacturers to ignore discoveries by engineering research of hazardous

defects, so that safety concerns become preempted by the exigencies of the marketplace. Manufacturers have both delayed and avoided recalls once an equipment defect has been recognized. The Corvair was marketed for four years before the stabilization problems in its rear suspension were corrected in an improved 1964 design. This delay occurred despite hundreds of consumer complaints regarding the Corvair's instability, repeated criticism from automotive magazines, and numerous instances of the loss of vehicle control by industry test car drivers. As early as 1956, Chevrolet's head of research and development noted in a patent application that the Corvair-type suspension had serious defects relating to the vehicle's tendency to roll over (Nader, 1965). The Corvair's problem was especially dangerous because the hazard materialized suddenly and occurred within normal speed ranges on sharp turns. The design defects were commonly known to highway patrol officers, who over the years had become adept at recognizing gashes in the pavement from the rim of the Corvair's collapsing rear wheel.

Perhaps due to the nature of its product, the automobile industry appears more prone to corporate violence than to "merely" economic forms of corporate crime; or, put less elegantly, it seems particularly likely to produce a higher ratio of killers to thieves than found in other large industries. This is strikingly illustrated in the remarks of Alfred P. Sloan, then President of General Motors, concerning the possible use of safety glass in Chevrolets. Sloan wrote in correspondence submitted as evidence at U.S. Senate Hearings in 1968 and reported in Mintz & Cohen (1971): "Accidents or no accidents, my concern in this problem is a matter of profit and loss . . .Our gain would be a purely temporary

one and the net result would be that both competition and ourselves would have reduced the return on our capital and the public would have obtained still more value per dollar expended . . . you can say perhaps that I am selfish, but business is selfish. We are not a charitable institution—we are trying to make a profit for our stockholders" (p. 258-260). As Mintz & Cohen note, safety glass is one of the most valuable protections ever devised against death and disfiguring injury from automobile crashes.

Factors Underlying the Lack of Psychological Research on Corporate Violence

If corporate violence is responsible for more preventable death and injury than is street violence, why have psychologists chosen to focus their research attention exclusively on street crime? Why has no one tried to investigate the "personality profile" of the corporate offender? Why are "early intervention programs" and "violence clinics" initiated in the ghettoes of Detroit, but not in the nearby corporate headquarters in Pontiac? Several factors appear to be acting in concert:

(1) Definitional boundaries on the concept of violence. Psychologists are subject to the same perceptual biases as other people. The pervasive tendency to fear death by mugging and to be sanguine about death by smogging is not confined to those lacking a Ph.D. We tend to be more sensitive and alert to risks that are immediately identifiable. Corporate violence is not "seen" because definitional boundaries have been limited to personal transactions.

(2) Access to data. Even were psychologists able to transcend cultural tendencies that diminish corporate violence as a social problem,

research on the phenomenon would be stymied by a lack of access to relevant data. One can readily arrange for apprehended street criminals "voluntarily" to take a battery of personality tests or participate in a treatment program. But how does a researcher obtain subjects from a major corporation? Indeed, the very word, "subjects," seems somehow out of place in this context. In addition, companies have a great deal to lose if their harm-doing behavior were identified. They are thus without incentive to cooperate in research which may show them in a negative light. The few corporate violators who are successfully prosecuted are incarcerated for only the briefest time, if at all. University researchers, anxious to do publishable studies, live out the joke of the drunk looking for his car key under the lamp post, not because he lost them there, but because that is where the light is. Psychologists do research on street crime, not because that is where most violence is to be found, but because that is where the subjects are.

(3) Access to funding sources. Like data, sources of funding are much easier to come by if one is interested in "street" rather than "suite" crime. The history of psychological research has largely been the history of funding opportunities (Quinney, 1974). When the Veterans' Administration was a major employer of psychologists, journals were filled with research on back ward schizophrenics. When community mental health centers began to hire psychologists in large numbers, community problems began to receive empirical attention. Prisons hire psychologists to do research on violent inmates; corporations do not hire psychologists to do research on violent executives. Virtually all of the millions of dollars allocated by the Law Enforcement Assistance Administration (LEAA)

have subsidized research on street rather than corporate crime. Other federal, state, and private funding agencies have behaved analogously. Research institutes, which exist only so long as they can solicit grant funds, pursue "fundable" projects. Corporate violence has not been in this category.

(4) Political considerations. Finally, we should note the radical critique of psychological and sociological research in the area of crime (Quinney, 1974). It is not coincidental, state the socialist critics, that a capitalistic society would devote substantial resources to repress violent crime by the poor while winking at death dealt by corporations. To the extent that Calvin Coolidge was right--that "the business of America is business"--one would expect corporate czars to protect their own. Corporate violence, the radical criminologists have it, is part and parcel of the American economic system, and cannot be changed until that system is changed.

The radical critique of behavioral science may well be misreading a general trait of power structures as a characteristic inherent only in capitalistic societies. Virtually all entrenched forces work assiduously to maintain their power; it is only that some are more successful in this endeavor than others. No government system advocates, much less allows, acts deemed to pose a direct threat to its survival, and few power structures are above resorting to violence to "protect" themselves from external or internal threats to their continuance.

In this sense, perhaps the most attractive trait of the American ideology, in theory if not always in practice, is that it provides room for espousal and pursuit of non-establishment-oriented endeavors. The

difficulty observed here is that through preemption, or by a failure of nerve, researchers often impose ideological blinders on their work, failing to ask hard, unpopular questions. Ironically, they thereby do a greater disservice to the values that they allegedly support than those persons who directly oppose such values.

Psychological Research Relevant to Corporate Violence

While psychologists have not provided research insights into the problem of corporate violence, they have, as noted earlier, been prolific producers of research on individual violence and aggression. To the extent that similar factors operate in both situations, examination of selected findings in the study of individual violence can provide hypotheses to be tested in research on corporate decision-making in regard to violence. Four important factors are readily identifiable: (1) conditions of reinforcement, (2) modeling influences, (3) diffusion of responsibility, and (4) depersonalization of victims.

Reinforcement. Aggressive behavior can be shaped and maintained by reward contingencies. The conditions of reinforcement may be either direct (Ceen & Pigg, 1970; Ceen & Stonner, 1971) or vicarious (Bandura, 1965). The instrumentality of aggressive behavior, i.e., its function in obtaining a desired outcome, is a powerful determinant of aggression and its intensity (Buss, 1963; 1966). By systematically rewarding compliance and punishing noncompliance, aggressive behavior can be brought under instructional control (Bandura, 1973). The obedient aggression demonstrated by Milgram's (1963) research followed from the subject's displacement of social values in response to the requests of a

perceived legitimate authority. When the demands of the instructing authority are more immediate and salient than the demands of the victim there is a greater probability of an obedient response (Milgram, 1965). Economic contingencies bearing on decisions within corporations appear to define the situation as one that is especially conducive to the instructional control of behavior. Baumhart (1968) found that the behavior of a person's superiors in the company was ranked as the primary determinant of unethical decisions by executives. As one of his respondents put it, "The constant everyday pressure from top management to obtain profitable business, unwritten, but well understood, is the phrase 'at any cost.' To do this requires every conceivable dirty trick" (p. 132).

Modeling. Bandura's (1973) social learning theory of aggression designates that the acquisition and performance of aggressive behavior is a function of modeling influences which operate through processes of observational learning, disinhibition, and response facilitation. Although instigation to aggression via modeling influences is often demonstrated when subjects have been angered (e.g., Baron, 1971; Baron & Kepner, 1970), instigative effects of modeling do not require emotional arousal (e.g., Hartman, 1969; Bandura, 1973). This is especially relevant to corporate decision-making, where anger is likely to be absent.

Diffusion of Responsibility. To the extent that aggressors can exempt themselves from self-devaluation by displacing responsibility for harm-doing behavior, the probability of aggression and its maintenance is increased. Conditions of justification (Berkowitz & Rawlings, 1963; Brock & Buss, 1964; Meyer, 1972) and diffusion of responsibility (Bandura,

Underwood, & Fromson, 1975) disinhibit the performance of aggressive actions. Corporate organizations appear designed to distribute responsibility in as many directions as possible (Sutherland, 1949). "The large corporation diffuses ever more important collective responsibilities among more and more people and separates ever more acts from consequences--the decision makers from those affected by the decisions. The buck seems to stop nowhere" (Mintz & Cohen, 1971, p. 295).

Depersonalization of Victims. When the victims of aggression are depersonalized, harm-doing actions are facilitated. Milgram (1965) found that subjects are more willing to administer shock when they were less likely to see or be seen by the recipient of the shock. Reducing victim visibility was found to facilitate aggression in a naturalistic setting by Turner, Layton, & Simons (1975). Zimbardo (1969) and a recent study by Bandura, Underwood, & Fromson (1975) found that the dehumanization of victims increased aggressive behavior. To the extent that the consumers of hazardous products are removed in space and time from corporate decision makers, there exists a condition of anonymity that facilitates violence. Furthermore, when the victims are perceived to have voluntarily chosen a hazardous work environment, such as a coal mine, or a hazardous product, such as a Corvair, they can be seen by the decision-makers to have invited their own misfortune.

STRATEGIES FOR RESEARCH ON CORPORATE CRIME

While the literature on aggression supplies some clues concerning the dynamics of corporate violence, strategies must be found to investigate its parameters more directly. At least three methods have promise for

the study of corporate violence: the case study, naturalistic quasi-experimental research, and experimental laboratory simulations.

A. Case Studies

The intensive study of the individual case is one of the oldest research methods in social science. The failings of the case method--its lack of control and susceptibility to unwarranted generalization--are much better known than its contributions. In clinical work (Lazarus & Davison, 1971), as elsewhere, the detailed investigation of a single instance of a phenomenon has provided a wealth of data from which to generate hypotheses for experimental testing, and has put flesh on theoretical abstractions. The case studies of corporate crime and corporate violence cited earlier have contributed substantially to our understanding of the subject. Further case studies are essential.

B. Naturalistic Quasi-Experimental Research

Research strategies which employ naturalistic data in a systematic manner are several steps up the ladder of methodological ascent from case studies. Campbell (1971) has detailed the strengths and the limitations of such approaches. Quasi-experimental designs appear to be particularly suited for studying corporate violence. In regard to the failure to recall defective automobiles, we found that two major Detroit auto manufacturers make recall decisions at the middle-management level, and two make such decisions at the top management level. Data from the National Highway Traffic Safety Administration--the federal agency which monitors recalls--reveals that in 1974, the latest year for which information is available, the two companies who used the middle-management decision makers were audited for safety violations a total of ten times,

while the two companies which make recall decisions at the top management level were audited only once. From this naturalistic data, one cannot infer that middle-managers are more likely to take risks with the public safety than are persons at the top of the corporate structure. Many factors confound such a straightforward interpretation (e.g., differences in sales volume and financial status of the companies). But sophisticated use of naturalistic data in quasi-experimental research eventually may allow inferences on such questions to be drawn with a high degree of confidence.

C. Experimental Laboratory Simulations

As research students know, randomly assigned groups to control for various hypotheses are fundamental to "true" experimental methodology. Unfortunately, the conditions of the real world usually do not lend themselves to assignment by coin-flipping. This is clearly the case with corporate violence: defects and decision-makers cannot be randomly assigned to experimental conditions. But if the laboratory cannot go to the corporation, perhaps the corporation can be brought to the laboratory. The technique of laboratory simulation has proved useful in other realms of psychological research, and may provide a method for studying corporate violence.

Simulation or "role playing" methods have been extensively used in psychological research (Freedman, 1970; Greenberg, 1967; Kelman, 1967). Perhaps the best known recent example of simulation research is the Stanford Prison Experiment (Haney & Zimbardo, 1976). In this study, college students played the role of either a prisoner or a prison guard, and acted out various institutional routines. The study was terminated

earlier than anticipated because participants "got into" their roles so fully that breakdowns and brutality which characterize a real prison began to manifest themselves in the simulation. Research on jury behavior also has relied heavily on simulation methods (Tapp, 1976). While simulation research may at times produce results quite unlike those obtained in the natural situation (Ebbeson & Konecni, 1975) it also may provide a highly useful analogue to situations otherwise immune from experimental intrusions.

Conclusion

Having emphasized scientific approaches to the problem of corporate violence, we will conclude on a literary note. Arthur Miller's play, "All My Sons (1947), has as its protagonist a man who knowingly sold defective cylinder aircraft heads to the Army Air Force in World War II. Numerous plane crashes were caused by his act. Like many corporate offenders, he escaped conviction. At the end of the play, the man's son discovers his guilt, and the father tries to explain himself:

> "I'm in a business, a man is in business; a hundred and twenty cylinder heads cracked, you're out of business; you don't know how to operate, your stuff is no good; they close you up, they tear up your contracts, what the hell's it to them? You lay forty years into a business and they knock you out in five minutes, what could I do, let them take forty years, let them take my life away?"

When told by his father that the cylinder heads were sold so that the business could be preserved for him, the son, who had been a pilot during the war, responds:

> "Where do you live, where have you come from? For me!--I was dying every day and you were killing my boys and you did it for me? What the hell do you think I was thinking of, the gaddamn business? Is that as far as your mind can see, the business? What is that, the world--the business? What the hell do you mean, you did it for me? Don't you have a country? Don't you live in the world?"

And, finally, when his mother asks him, "What more can we be!", the son responds: "You can be better! Once and for all you can know there's a universe of people outside and you're responsible to it."

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END