

A Critique of the Evaluation of Patuxent Institution, With Particular Attention to the Issues of Dangerousness and Recidivism

ROBERT A. GORDON, PH.D.*

The Patuxent Institution has had its outside audit and has been found wanting. This paper examines the basis for that conclusion.

BACKGROUND FOR THIS CRITIQUE

Nine years ago, I was appointed to the Advisory Board of Patuxent Institution to fill the position allocated, somewhat warily, to a "competent" member of the sociology department of Johns Hopkins University by Article 31B.¹ My professional interests lay in the areas of social psychology and the causes of crime and delinquency, and I knew nothing, at that time, about corrections. I was relieved to find that the Advisory Board benefitted from a mixture of competencies, and that persons knowledgeable in the field of corrections were already represented.

My own orientation in social science was that outcome variables are largely determined by the input or selection of human material, and that intervention or treatment, while by no means hopeless, plays at best a modest role in satisfying the important personnel demands of a complex society. This view, in my opinion, applies in fields as widely different as education, ordinary psychotherapy, and rehabilitation of criminals. Selection includes self-selection, as exemplified in the decision to seek psychotherapy or to cooperate with a psychotherapist once one is available.

From the first, I appraised Patuxent Institution as one whose successful outcomes with inmates must depend far more heavily on screening for release than on any known therapeutic program that could be applied to or imposed upon its entire intake. To the extent to which they were recognizable in advance, Patuxent did not have to parole or release its failures. Because the institution, under the indeterminate sentence law, was not obliged to release inmates just because their sentences had expired, it was in a position to safeguard society against recidivism to whatever degree the responsible citizens on its Institutional Board of Review thought consistent with a reasonably humane policy toward inmates. Those paroled or released could be as highly selected as human judgment permitted, depending on the risks to public safety that the Board of Review chose to

*Dr. Gordon is Associate Professor, Department of Social Relations, Johns Hopkins University. He gratefully acknowledges helpful discussions with James Fennessey and Eileen E. Rudert during preparation of this paper.

tolerate. In practice, it seemed to me, the Board acted as a human valve, controlling the outflow of potentially dangerous offenders according to its collective conscience and the feedback it got from observing consequences of its own decisions. Conceivably, the Board would also be responsive to other considerations, such as effects on the morale of inmates of various parole policies, and community reactions as transmitted through the political process.² But this was the essential model.

The role of the therapeutic regimen, as I saw it, was to provide a means of rehabilitation for those inmates, however few, who could benefit. This prevented the inmate's situation from being a completely hopeless one, and humanized, to an important degree, what otherwise would have been an extremely harsh protective policy by assisting inmates who had the potential to qualify for release to do so. At the same time, the therapeutic relationship would serve — quite properly under the circumstances, I felt — as an important tool for assessing fitness for release.³ In this sense, therapy would also contribute to the selection component of postrelease success, as distinct from the rehabilitation component. Two studies of decision-making on the part of members of the Board of Review in fact agree in finding that "progress in therapy" ranks extremely high among considerations affecting parole and release.⁴

It also occurred to me that the provision for rehabilitation and consequent release blunted, somewhat, civil libertarian criticisms that might otherwise have impeded the establishment of indefinite incapacitation for any class of offenders broader than murderers, rapists, and kidnappers. In retrospect now, it appears clear that in order to regain their original ability to mobilize civil libertarian sentiment against indeterminate confinement, leading civil libertarians would have to concentrate on discrediting the therapeutic program at Patuxent. This is exactly what they have done.

Under the conception of Patuxent advanced here, which I feel is within one or two nuances of the realistic one, the institution's program of therapy would become subject to criticism only if it failed to keep up with the state of the art. Aside from this, the absolute level of rehabilitation, however low, would not be a cause for criticism because it had never been promised that the institution would deliver any particular amount of this rare commodity.

Indeed, the eminent psychiatrists who contributed to the founding of Patuxent apparently shared this same view, for they stated explicitly, "It was the opinion of medical members that most persons in such an institution would be incurable and the primary problem therefore would be one of custody and proper care rather than curative treatment for mental illness."⁵ The recent evaluation report⁶ notes this original cautious assessment of the potential for rehabilitating Patuxent inmates in several places (pp. 3-5, 8). The above passage is even quoted twice (pp. 3 and 76-77).⁷ Given that Patuxent therapists would have to work with the most difficult and intractable of all patient categories, with the possible exception of psychotics, any other view on the part of the founders would have been unrealistic. On the legal side, the position of the legislative researchers at that time was equally clear: "The *primary* purpose of such legislation is to protect society from this segment of the criminal population who probably will again commit crimes if released on the expiration of a fixed sentence;

and thus they should be detained and specially treated unless and until cured" (emphasis added; also quoted on p. 4 of the report).⁸

Against this background, the indeterminate sentence would have been legitimated sufficiently by the protection it provided the general public. True, many therapists would not fail to notice that the indeterminate sentence also constituted an inescapable crisis in the life of an inmate that was analogous in many ways to the crises in family life, job, or school that typically drive persons to seek psychotherapy in everyday life by raising their anxiety to intolerable levels.⁹ Commitment to Patuxent might well represent the first such crisis that the typical defective delinquent ever had to confront without acting out or running away in order to avoid considering his own contribution to the trouble he was in.¹⁰ If true, the therapeutic leverage thus gained over a class of elusive and evasive patients would represent an ancillary benefit of the indeterminate sentence, but it would never be mistaken for the major justification of such sentencing by those who kept the original conception of the institution in clear view. Consequently, refusing to concede the therapeutic advantage of the indeterminate sentence or even the efficacy of therapy itself would not materially improve anyone's case against the sentence or the institution, both of which rested firmly on the bedrock of public safety.

There is still a third class of argument that one encounters concerning therapy and the indeterminate sentence. The form that it takes is popular among nonscientist intellectuals because it often seems to depend on logic alone rather than on empirical evidence. It consists of demonstrating internal contradictions in the opposing position. When the opposing position is purely philosophical, such demonstration is sufficient to demolish it. As applied to the indeterminate sentence, this form of argument maintains that the sentence is actually therapeutically counterproductive (presumably, to a greater degree than it is also productive).

If correct, this last argument would impugn the rationale of the institution regardless of whether its primary mission was to safeguard the public or to rehabilitate defective delinquents, but the more so as the primary mission tended toward the second of these two objectives. In their concrete form, arguments of this type have claimed either that therapy was "impossible" when not entirely "voluntary" or that levels of anxiety supposedly generated by the indeterminate sentence were so high that they impeded therapy (see pp. 13, 15, 35, 43, 66, and 83 of the report).¹¹ Under any circumstances these are empirically testable propositions demanding evidence in the form of controlled comparisons, but it is clear that the questions raised pose more fundamental challenges to the institution the more it depends on therapeutic success to justify its mandate. I have never seen any empirical evidence adduced in support of these contentions, incidentally. Instead, those employing them have rested their claims upon dubious analogies from private practice and from voluntary programs for inmates in quite different institutions, and upon personal authority. At just what point in the concatenation of events leading to constructive participation in therapy it is essential that the decision be "voluntary" is never considered. The fact that the precipitating crisis (or "presenting complaint") is linked in some sense to the helping agency in the case of Patuxent does not necessarily make the

ultimate decision to cooperate in therapy less "voluntary." The old saw, "You can lead a horse to water, but you can't make it drink," cuts both ways. In the final decision, which is the one that counts most for therapy, this old saw shows that voluntarism is preserved for the Patuxent inmate to the same extent as for any other class of patient. Without evidence, and without apparent merit (since therapy obviously did go on within Patuxent, and with about as much success as one could reasonably expect), such claims of counterproductivity should have been easy to brush aside. However, institutions purporting to provide help are far more vulnerable to unsubstantiated criticism than mere prisons, whose task is simpler and thus easier to judge from afar.

It is interesting to note that in its opening pages the evaluation report expresses close agreement with my views of the Institution's purpose and of the implications of straying from that conception by placing greater emphasis on the therapeutic function. Thus, the report states (p. i), "From the start the Institution operated with a dual purpose: to protect society from potentially dangerous offenders by keeping them incapacitated until they were no longer dangerous, and to provide a therapeutic environment which would aid, *where possible*, in their rehabilitation" (emphasis added). A few pages later, the report observed (p. 9), "... opinions of Patuxent's success depend on whether one perceives it mainly as a rehabilitation program or mainly as a method of incapacitation." The report then goes on to note that both supporters and critics of Patuxent agree that it (p. 9) "is better at incapacitation than at treatment; but they differ as to whether Patuxent is a success." In view of the report's appreciation of these implications, as revealed in these statements and in its quotations of intentions from founding documents (pp. 3-5, 8, 76-77), one might have expected it to commit itself cleanly to criteria relevant for evaluating what Patuxent was expected to be, really was, and could only be, despite the other definitions that critics have sought to impose on the institution. Alternatively, the report could have developed two parallel sets of evaluations, each based on one of the two contrasting definitions of the institution, so that policy-makers would have both available to ponder before assuming for themselves the responsibility for deciding just what it was that Patuxent was supposed to be. Ideally, the report would have pursued these parallel evaluations, taken a stand itself as to which was more appropriate, and justified the stand. Had such a choice been one that was solidly defensible, it might have cleared the air for a time. Instead, as we shall see, by maintaining that its incapacitation effort is largely wasted on individuals who are not dangerous at all, the report tends to join those who found it convenient or irresistible to evaluate the institution mainly in terms of its therapeutic success.

As a noted sociologist once remarked, "... evaluation studies usually make the client look bad."¹² The need to compete successfully in order to survive and the necessity to respond to criticism both reasonable and unreasonable produce an inexorable pressure toward overpromising in the institutional life of our society. Commercial advertising and political campaigns are only two of the more commonly recognized cases. Nothing seems sufficiently justifiable in terms of its real function, and hence

everything is vulnerable to criticism. From the start of my tenure on the Advisory Board, I felt that Patuxent staff members tended to overemphasize the success of their therapy program by unconsciously crediting it with the advantages that must be due to selection of inmates after long opportunity for observing them. This tendency seemed like a harmless self-indulgence, typical of any team effort, that was probably good for staff morale and hence possibly good for the program too. However, as it became necessary for the staff to defend Patuxent against the welter of criticisms of recent years,¹³ there was a natural tendency for the claims of successful therapy to assume greater prominence in the course of repeated assertion. Probably no set of professionals likes to think of itself as mainly jailers, and the climate in recent years has not, for the most part, been one to encourage simple restatements of Patuxent's original hard-line mission.

There came a time when I felt obliged to warn the staff that by drifting toward claims of "therapeutic omnipotence" they were playing into the hands of their opponents. I urged that a vigorous restatement of Patuxent's original primary mission be included in the 1973 progress report, and a suitable quotation from the founding documents, one of those that were quoted above, did indeed appear there. This reminder of original priorities was more than offset, unfortunately, by the extremely favorable results of the recidivism study that appeared in the same progress report. The results of this study showed a 7% recidivism rate for inmates released after three years on parole, and this rate was compared to a 65% rate "frequently quoted for adult offenders nationally," and to other intermediate rates for various categories referred to Patuxent but not fully treated there.¹⁴ The seemingly strong results of the recidivism study eclipsed the custodial function once again, gave new life to the myth of "therapeutic omnipotence," and invited further attack from the critics of the institution.

This recidivism study has been criticized, fairly it would seem, for beginning the period at risk for the fully treated group with the completion of parole.¹⁵ I did not realize at the time that much of recidivism occurs during parole itself, or I would have questioned this procedure. I naively imagined recidivists behaving as model parolees and waiting until parole supervision had ended before resuming their criminal careers. Had this assumption been the case, comparisons between Patuxent's recidivism rate and other rates would have been quite appropriate. As it was, the Patuxent staff understandably regarded the parole period as a continuation of treatment. Infractions committed during parole were no different in a sense from crimes committed within the institution itself. There was nothing to prevent other correctional programs from beginning their own recidivism count with the end of parole too, thus bringing their operational definition of recidivism into alignment with Patuxent's for purposes of comparison. The Patuxent staff could not have implemented this definition for the rest of the correctional system in preparing its report, however, because it did not have control over the necessary records or the resources for such a major undertaking. Consequently, the staff constituted comparison groups that were meaningful within the framework of the institution's own existence. The fact that some of these groups exited the correctional system through facilities other than Patuxent meant that recidivism during parole could not

be distinguished from recidivism after parole when following-up individuals in those groups through FBI reports. The parole situations themselves, moreover, are not comparable across institutions. Patuxent provides closer parole supervision, as noted in the evaluation (pp. viii and 190), and its parolees are more apt to be reincarcerated without trial following arrest, consequently accumulating fewer convictions (p. 123). Although these differences could have been resolved by using arrest instead of revocation of parole or conviction as the criterion of recidivism, the solution awaited a more focused statement of the question, as would arise naturally in the course of actually conducting a recidivism study for several institutions at once, as was done for the evaluation. Certainly, there was no impropriety in the definitions chosen by the Patuxent staff in performing its own recidivism study, and the evaluation report makes clear that its different results, based on different criteria, "do not affront" the numbers cited by Patuxent (p. 125).

The "often quoted" recidivism rate of 65% included by Patuxent staff in their 1973 report emerged from earlier consultation with Advisory Board members concerning typical rates.¹⁶ Judging from the five recidivism rates of arrest for various groups in the evaluation report (p. 124), which range from 60.4% to 74.3% and which average 66.5%, the rate discussed with the Board must have applied to arrest rather than conviction. The comparison that eventually found its way into the Patuxent report was, then, unfortunately confused, since conviction, not arrest, was used as the recidivism criterion for ex-inmates of the institution. Although it was not the crucial comparison by any means, and it was set apart in the table in which it appeared, it was misleading. Despite the fact that rates of 46% and 39% for partly treated groups released by the courts did give an indication of recidivism as it would be defined in the evaluation report (see its rate of 54.3% for such a group), the extremely low rate of 7% in the 1973 Patuxent report in combination with the figure of 65% set the stage for exploding a myth of therapeutic potency that it had never been the intention of Patuxent to claim.

Having described the background against which I view the evaluation report, I now turn to the report itself and to its surrounding scientific literature. The evidence dealing with the two definitions of Patuxent in the report is organized around two main issues, one having to do with the prediction of dangerousness, the other with recidivism. These major themes are taken up in the next two major sections.

THE DANGEROUSNESS ISSUE

It is not unusual for perfectly respectable variables in social science to come under heavy and persistent attack. IQ, which is certainly one of the best-measured and most important of all psychological variables, is a case in point, and readers of this journal will be aware of similar attacks on psychotherapy.¹⁷ In recent years, the concept of dangerousness has been subject to such questioning, and the conclusions drawn, rather erroneously in my opinion, have been put to service in the evaluation report, where they play a major role. Let us look briefly at the studies responsible for the new claim that dangerousness cannot be diagnosed or predicted.

Empirical Studies of Dangerousness

The first set of studies concerns what are known as the Baxstrom patients (after the legal decision). As the result of a class action court ruling, these patients were abruptly transferred from high security hospitals for the criminally insane to less secure civil hospitals, and many were soon after released into the community. A sample of 246 of the total group of patients were followed-up by Steadman and his associates. At transfer, the Baxstrom sample had been incarcerated for an average of 15 years, and their average age was 47. In addition, 19.1% of the sample was female, although sex does not seem to have been distinguished in the analyses concerned with later dangerous behavior. Only a minority of the entire sample proved to be troublesome within the follow-up period, either in their new hospital surroundings or in the community. For present purposes, the discussion will focus only on the 98 patients released into the community who were alive at the end of the follow-up period. One statement in the report suggests that as many as 73 of the 98 were males, and perhaps as few as 68. Of the total 98 released, with an average time at risk of two years and a half, 20.4% had been arrested, 11.2% had been convicted of a crime, and 14.3% had committed violent assaultive acts against persons. When the sample released into the community was divided according to both age and score on a Legal Dangerousness Scale (a summary measure of previous criminal activity), it was found that patients less than 50 years old with a score of 5 or more were distinctly more dangerous than the rest. Within this subgroup, 47.2% had been arrested and 30.6% had been involved in violent assaultive acts.¹⁸

As the result of the *Baxstrom* decision, the State of Massachusetts decided to review its own criminally insane patients.¹⁹ Of 266 reviewed, 9 were recommitted to secure facilities, 246 were transferred, 4 were discharged, and 7 were permitted to remain voluntarily where they were. The mean age of the sample on follow-up was 60.4, and the average length of incarceration was 28.7 years. Relatively few of these superannuated patients got into trouble, but the mean age of the 15 who did, following escape or release, was 47.5.²⁰

A study by Koppin attempted to replicate the Baxstrom results in a sample of 111 male offenders found not guilty by reason of insanity.²¹ These patients had been in custody for an average of 4.5 years when released conditionally into the community after treatment at an average age of 33 because they were predicted to be no longer dangerous. When followed-up they had from two to four years' opportunity to recidivate in the community. Their principal differences from Baxstrom patients released into the community were that Koppin's were younger and known to consist of males only. Of the total group, 42.3% were arrested during the two to four years at risk, and 29.7% committed acts considered dangerous to others (such as serious threats of physical violence or actual physical assaults). Following the work of Cocozza and Steadman,²² Koppin constituted a high-risk subgroup consisting of those under age 50 with a Legal Dangerousness Scale score of 5 or more. Within this high-risk group, 66.7% were arrested and 48.3% committed acts dangerous to others.

Kozol, Boucher, and Garofalo studied 592 male convicts, virtually all of

whom had been guilty of serious sex offenses.²³ Force, violence, or threats had been involved in more than half the cases. The mean age of the 435 who were eventually released was 35.6. The psychiatric diagnosis of 304 of these offenders was that they were not dangerous, and so they were released into the community after serving their normal sentences without special treatment. Serious assaultive crimes were subsequently committed by 8.6% of this group. The remaining 131 offenders were diagnosed dangerous and recommended for indefinite commitment to a special psychiatric treatment facility. In 31 cases the court rejected the initial diagnosis of "dangerous" and those individuals were eventually released into the community too. Serious assaultive crimes were committed by 38.7% of this group, one a murder. Note that the rate is 4.5 times greater than in the case of the "not dangerous" group, and the percentage difference is 30.1 points. Another 18 were released by the court after an average length of treatment of thirty months, despite medical insistence that they were still dangerous. Serious assaultive crimes, one a murder, were committed by 27.8% of this group. Finally, 82 patients were recommended for release after an average treatment period of forty-three months, and serious assaultive crimes, one a murder, were committed by 6.1% of this group. The maximum period at risk seems to have been five years, since this is the time at risk cited for those who had not recidivated according to the criterion of serious assaultive crime in the groups released by the court against medical advice.

Wenk and Emrich followed-up 4,146 young males released on parole at an average of 19.4 from the California Youth Authority (CYA).²⁴ They were particularly interested in what data in hand would predict commission of a violent offense during 15 months at risk on parole. In only 250 cases, or 6.0% of the total sample, had a violent offense been responsible for their present incarceration. The rate of violent parole violations in the period at risk was only 2.4% or 2.5% for the entire sample. Certain subgroups of the entire sample had rates of violent parole violation somewhat higher than this. The four subgroups with the highest rates of violent recidivism were those who had been referred for psychiatric evaluation of their potential for violence (6.2%), those with a prior history of actual violence (5.2%), those with four or more prior admissions to the CYA (4.8%), and those with a violent admission offense at the present time (4.8%). (All four of these criteria would apply to most Patuxent inmates conjointly).²⁵ Two other attempts to develop statistical prediction formulas drawing on a wealth of background, personality, and cognitive variables were not especially successful for this sample. One produced a high-risk category in which 5.3% became violent recidivists, the other, a category in which 10.8% did. In another paper, Wenk, Robison, and Smith mentioned that a violence prediction scale devised by the California Department of Corrections was able to isolate a category of offender in which 14% violated parole by a violent or potentially violent discovered act.²⁶ The probability of such violence among parolees in general was only 5%.

All of these authors call attention to the high rates of "false positives" produced by their predictive categories, a result that is almost inevitable when base rates for the phenomenon being predicted are low.²⁷ The false positive rates in all of these studies are simply the difference between the

rate of violent crime in the group predicted dangerous and 100%. For example, Kozol *et al.* indicated that they were aware that 65% of the 49 sex-offenders whose release they had opposed had not committed serious assaultive crimes during nearly five years of freedom.²⁸ This means that 35% of that group did prove dangerous, and 65% were "false positives." Like all of the other authors of these empirical studies, some of which were more successful than others in their particular context of application, Kozol *et al.* were cautious in their conclusions concerning the ultimate policy implications of their first attempts to predict later dangerous behavior.

Further Interpretations of these Findings, and Their Applicability to Patuxent

The secondary literature of commentary on the original empirical studies has not always been so reluctant to draw sweeping conclusions, often lumping together findings from quite different populations and quotations from quite different research contexts for the purpose. For example, the aging Baxstrom patients are certainly a relevant population for those concerned with holding the criminally insane in expensive facilities without periodic redeterminations of their continued risk to self and others.²⁹ To some extent, the Baxstrom studies may also have revealed a tendency toward "excessive clinical conservatism" in the management of such patients.³⁰ The studies do not deal, however, with differences in the valid subjective threat posed by criminally insane patients in contrast to other classes of dangerous criminal. A greater element of unpredictability in the violence of the criminally insane, and hence a greater demand for vigilance, pervading a broader range of situations both real and imagined, might conceivably make their presence in the community more difficult to tolerate than that of psychopaths with higher base rates whose violence is restricted to contexts in which their self-interest is recognizably at stake. This possibility has not been investigated at all, but it could conceivably justify higher false positive rates in the management of the criminally insane.³¹

The studies reported by Wenk and his associates, as discouraging as they may be for prediction among routine populations of young offenders, hardly apply to Patuxent, where 71% of the inmates committed in 1970-72 had been convicted of murder, robbery, assault, or rape.³² This is 11.8 times the violent last-offense rate in the Wenk and Emrich CYA sample. The studies that come closest to approximating the Patuxent situation are those by Koppin and by Kozol *et al.*, the first involving relatively young criminally insane offenders, the second relatively young, antisocial sexual psychopaths (with some psychotics, about 7%, included).³³ These studies also had somewhat longer follow-up periods than 15 months, however appropriate that brief period might have been in the context of the California studies. Especially apropos in her study is Koppin's high-risk group, those under age 50 with Legal Dangerousness Scale scores of 5 or more, because it is easy to demonstrate that virtually all Patuxent inmates would fall within those categories. Between 1955 and 1972, the average age at commitment of Patuxent inmates was 24.6, with 4.9 prior convictions,³⁴ and average age at parole from Patuxent in 1971 and 1972 was 33.³⁵ Profile data for the

average Patuxent inmate indicate that he would score 14 out of a possible 15 on the Legal Dangerousness Scale. This places him, incidentally, at the 78th percentile of the 111 released criminally insane offenders in Koppin's study, and reveals another reason why studies of the Baxstrom type, based on a general cross-section of the released criminally insane, should not be generalized to apply to Patuxent. The few Baxstrom patients who were returned to hospitals for the criminally insane after transfer averaged only 9.2 on the Scale (the 68th percentile), and the remainder of the Baxstrom population averaged 6.0 (the 49th percentile of Koppin's sample). Only 51% of the Baxstrom patients had been convicted of a violent crime, in contrast to the 71% noted above for Patuxent inmates.³⁶

In Koppin's study, 48.3% of the high-risk group proved dangerous (compared to 30.6% of the high-risk Baxstrom patients, who were undoubtedly older); in Kozol *et al.*'s study, 38.7% of the group eventually released by the court without treatment against staff advice committed serious assaultive crimes (apparently a more stringent criterion than Koppin's, which included threats of harm). In comparison to these groups, the dangerousness of the untreated Patuxent inmate can be gauged by looking at the violent arrest recidivism rate for those diagnosed defective delinquent by the staff, but not certified by the court. Violent arrest here means arrest in connection with the violent offenses murder, manslaughter, assault, robbery, kidnapping, and arson. The violent arrest rate for these untreated "defective delinquents" within three years of having been paroled from the regular correctional system was 41.3%.³⁷ It might be worth taking into account that the past records of this group of 46 men were only 62% as violent as those actually certified defective delinquent and later released on parole from Patuxent.³⁸ These data indicate that the Patuxent inmate is as dangerous as the most dangerous categories of offender described anywhere in the literature, if not more so.³⁹

The predictive value of a diagnostic procedure is defined as the percentage of positive predictions that are true positives when applied to the population of interest.⁴⁰ Let us compare the predictive values of the categories established by Koppin, Kozol *et al.*, and Patuxent, as expressed in the triple set of recidivism rates introduced above (48.3%, 38.7%, 41.3%), with the predictive values of categories established in the various studies by Wenk and his associates. Recall that Wenk and Emrich, using their most predictive subgrouping, obtained a violent recidivism rate (after 15 months at risk) of 6.2%. The triplet of figures above represent predictive values that are 7.8, 6.2, and 6.7 times greater, respectively. If compared with the 14% rate achieved by the California Department of Corrections criteria, which represents the highest predictive value mentioned by Wenk and Emrich or by Wenk *et al.*, the two studies of populations that are more like the Patuxent population yield predictive values that are 3.4 and 2.8 times greater, respectively, and the uncertified Patuxent group yields one that is 3.0 times greater. These are not negligible differences, either relatively or in absolute level. Most likely, the improvements are due to the higher base rates of dangerous behavior in the Koppin, Kozol *et al.*, and Patuxent populations, as compared with general offenders in California, and to higher cutoffs.⁴¹

The concern in the secondary literature has been with the actual number

of false positives in any particular context, the implications of incarcerating false positives whatever the number, and the question of the predictability of dangerousness. For example, in his 1976 testimony before the Maryland State Senate committee considering a bill to abolish Patuxent, the criminologist Leslie T. Wilkins referred to the study by Wenk and Emrich discussed above, and asked, "Are we willing to lock up nine false positives (persons incorrectly identified as potentially violent) for every one positive (a truly violent-prone individual)?" Wilkins continued, "I would submit that there is insufficient justification for this so-called experiment. Patuxent . . . is a relic . . . of an old philosophy."⁴² But in view of the low base rate of the population in question, the Wenk and Emrich findings are the least relevant – except for purposes of contrast – of any of the studies known, and the one by Kozol *et al.* that is relevant had been in the literature since 1972. Given a predictive value of 41.3% for uncertified "defective delinquents," which yields a false positive ratio of only 1.42:1, Wilkins' 9:1 ratio is too large for certified Patuxent inmates by a factor of at least 6.3.

Some of the commentaries show no first-hand awareness of the offenders or offenses they consider. Diamond, an M.D. and Professor of Law and Criminology, discussed a follow-up study of 100 "threat-to-kill" admissions to a state psychopathic hospital. Within six years of admission, three of the patients had killed others and four had committed suicide. Diamond acknowledged the possible benefit of intervention here in keeping the rate as low as it was, but our interest at the moment is in another aspect of his remarks. He stated, "Obviously, these seven fatalities are of significance; yet one can hardly justify a preventive detention procedure when over ninety percent of the suspected persons do not commit dangerous acts."⁴³ Surely, our abhorrence of dangerous acts does not originate in any arbitrary proscription of certain behaviors, but rather in the particular subjective consequences that such behaviors have for victims. Once this is recognized, it is easy to see that the subjective consequences of death threats are nearly as noxious as those of death itself. Diamond overlooks that death threats are themselves dangerous acts (as recognized in the Koppin study, above, and in law, where they are considered assaults) and that they entail in addition a prediction by the offender himself as to what he will do – from the horse's mouth, so to speak. In view of this failure to attach more importance to threatening acts, it is not surprising, perhaps, that elsewhere in his article Diamond found "violence itself . . . extremely difficult to define."⁴⁴

The ratios of false positives cited by Wilkins and Diamond are rather wide of the mark with respect to Patuxent. However, intolerance toward almost any attainable level of "false positives" is revealed by Monahan and Cummings, who actually addressed themselves to the rate achieved by Kozol *et al.*, stating that they "were wrong in 2 out of every 3 predictions of dangerousness."⁴⁵ Elsewhere, Monahan declared, "If for every correct psychiatric prediction of dangerousness there are two incorrect ones, the right of the 'false positives' to remain free of unnecessary incarceration becomes a central consideration."⁴⁶ Working with recidivism data from Patuxent, and finding it advantageous further to restrict the criterion and supposed prediction only to "aggravated or violent crime," Sidley concluded that the diagnostic staff was in error the majority of time, and that

"Diagnostic accuracy and precision must be severely called into question by such a large number of false positives."⁴⁷ Of course, there is no requirement in the law that defective delinquency turns only on the commission of violent crimes, and there seems to be no good reason why society should tolerate repeated offenses against property any more than those against persons, since persons are the victims in either case, Sidley's assumptions notwithstanding.

On an even more general level, the supposed impossibility of predicting violence or dangerousness is proclaimed throughout this secondary literature at every turn. Thus, we have Diamond's subheading, "No Means of Prediction Exist";⁴⁸ Monahan and Cummings' title, "... Implications of the Inability to Predict Violence";⁴⁹ criminologist Christie's statement, quoted with approval by Diamond, "There seems to be no convincing study to show that we can predict really dangerous behavior with any amount of acceptability";⁵⁰ various quotations reviewed by Stone (although presented with greater neutrality) such as the following, from Halleck, "Our criteria for predicting who will commit a dangerous act are totally inadequate"; and Stone's own conclusion, "... there is no valid method for predicting dangerousness."⁵¹ One would have thought that the fact that these assertions are so much at variance with common experience would have given the authors pause. Some years ago I noted that "[t]he principal social defense against noxious deviant behavior is physical distance. Generally, it is the potential victim who removes himself from contact with deviants — it is only in extraordinary cases, actually, that we remove the deviant."⁵² In a brilliant review of social control in primitive societies, Colson reports that when individuals are offensive to members of their own community, gossip enters each transgression into a "community dossier." If the offenders do not heed the warning signals, they may find themselves fatally accused of witchcraft when the consensus of opinion against them reaches a critical level. In these societies, "... it is the individual who is being judged not the crime."⁵³ Are these commonsense efforts to no avail? If not, what is wrong with the assertions concerning our supposed inability to recognize danger?

One thing that is wrong is that without reference to particular populations with particular base rates, and to particular cutoffs, such statements are meaningless. (We have already seen, in the case of Wilkins, that it is misleading to generalize experience from California Youth Authority populations to the Patuxent population.) The higher the cutoff, typically, the greater the proportion of true positives out of all predicted positives, and hence the greater the predictive value. In order to know what cutoff one is operating at, it is necessary to know the percentage distribution of offenders with respect to severity or dangerousness. Although I have elsewhere developed some such information for juvenile delinquents,⁵⁴ it is in general practically unknown in criminology, a fact which accounts for much of the floundering around that we observe in discussions such as that concerning Patuxent. Since remote audiences cannot tell readily whether the average Patuxent inmate falls at the 50th percentile ("the boy next door" if the distribution is normal) or at the 99.9th percentile of the general male population with respect to dangerousness, critics are free to exploit the resulting ambiguity — or to be influenced by it themselves.⁵⁵ Before pursuing

this question of relative dangerousness, it is convenient to consider the purely semantic implications of the claim that dangerousness cannot be predicted, since the two matters lead in the same direction.

Dangerousness and Safety: Two Ends of a Single Dimension

From a statistical standpoint, there are several ways of interpreting or construing the term "prediction." The most inclusive sense is measured by the statistical association between prediction and outcome, and is assessed, for example, by a correlation coefficient. Prediction in this correlational sense does not differentiate between types of error (false positives and false negatives). But some of this inclusive sense also carries over even when attention is restricted just to false positive errors; that is, when prediction is assessed by predictive value (as defined earlier) instead of simply a correlation. Especially in the case of correlation, but to some extent also in the case of predictive value, to state that we cannot predict dangerousness is logically equivalent to stating that we cannot predict safety. This equivalence obviously holds in the case of predictive value in all senses when it is at 50% and remains true *a fortiori* as the predictive value rises above 50%. If one judges dangerousness "unpredictable" in these circumstances, then safety is at least as unpredictable. The situation in which predictive value stands at 50% is not an unrealistic one for present purposes, because we have seen that such values verge on 50% for the three populations most similar to certified defective delinquents (e.g., 48.3%, 38.7%, and 41.3%), and critics have already expressed dissatisfaction with two of these values (the rate from Kozol *et al.*, and implicitly, the Patuxent rate). If one cannot differentiate which members of a category are dangerous and which are safe, then for all practical purposes they are equally dangerous or equally safe, and one becomes indifferent to selection among them. The crucial question becomes, how dangerous (or safe) are they? This conclusion, it seems to me, is inescapable.

How Dangerous or Safe is the Patuxent Population?

It is precisely at this point that the critics' failure to specify population characteristics and the critics' claim of unpredictability converge on the same question — the relative dangerousness of the Patuxent population, which, it turns out, has an average score of 14 on the Legal Dangerousness Scale. For the sake of illustration in pursuing this question, let us construe dangerousness to refer to the use of violence, either actual or threatened, although in reality violence represents only an extreme degree of noxious behavior along a continuum of noxiousness, and individuals might legitimately wish to defend themselves against lesser degrees as well.

We have already seen that at time of admission the contemporary Patuxent population is 11.8 times as dangerous as the California Youth Authority population. That is, the Patuxent population has a violent admitting offense rate of 71%, as compared to one of 6.0% for the CYA population. Now it can be added that at time of release a Patuxent-like population is 16.5 times as dangerous during follow-up as the CYA population upon release. This emerges from the violent arrest rate of 41.3%

for uncertified "defective delinquents," as compared to the violent parole violation rate of 2.51% for the CYA wards upon release.⁵⁶ Do the violent admitting offense rates suggest that 71% of Patuxent inmates are equivalent to the most violent 6% of the CYA wards — the top six percentiles? Apparently not, because although the two are equivalent in this one formal characteristic, the Patuxent inmate must be more dangerous in other respects. This conclusion emerges from a comparison of the violent parole violation rate, namely 4.8%, of the CYA subgroup that had violent admitting offenses,⁵⁷ with the violent arrest rate after release of the uncertified "defective delinquents," namely 41.3%. Here, the Patuxent-like population is 8.6 times as dangerous. Plainly, the entire Patuxent population must be equivalent to only a small fraction, perhaps smaller even than the worst six percentiles, of the CYA male population.

How dangerous is the CYA population? Let us employ a rough but convenient comparison. Elsewhere, I have determined that only 1.0% of white males are sent to training schools by age 18.0, and other data that I have seen since support this figure.⁵⁸ Although it is often maintained, as with Patuxent, that training school inmates are incarcerated with little cause, actual familiarity with the reality indicates that only the most severe delinquents are ever incarcerated.⁵⁹ If the very small fraction of juvenile offenders for whom Juvenile Court jurisdiction is waived, who have never been sent to training school before, are added to the 1.0% figure above, it probably would not change perceptibly. If we equate the CYA population of young offenders, average age at admission 19.4, with the training school population in other states, it suggests that the CYA population is roughly equivalent to the most dangerous 1% of the white male population. However, the period of risk extends to age 23.0 for the CYA population, instead of age 18.0, and so it might be wise to consider it as representing the top 2% of the white population in view of the added five years of exposure to risk.⁶⁰ If qualifying for CYA incarceration is defined as the criterion of dangerousness, and anyone less dangerous is defined as "not dangerous," then it could be concluded that the CYA population, 100% of whom qualify, is roughly 50 times as dangerous as the white male population, only 2% of whom qualify, as each enters the period at risk.

These various considerations lead to approximate estimates of the dangerousness of Patuxent inmates as compared to the general population. First, they suggest that the true Patuxent inmate represents the uppermost percentiles (top six or less) of the most dangerous 2% of the white male population, or somewhere on the order of the most dangerous 2 to 12 individuals per 10,000 white males.⁶¹ An independent assessment of the probability of being committed to Patuxent for cohorts of white males in Maryland passing through the period of risk during the years 1969-1971 yields an average figure of 10 chances in 10,000, which falls within the previous range of estimates built up from considerations of dangerousness.⁶² Second, using a crude rate-wise index, and defining commitment to Patuxent as the criterion of dangerousness (which defines everyone else as "not dangerous"), these figures indicate that the Patuxent population is 1,000 times as dangerous, per capita, as the general white male population. Although this comparison is much less satisfactory than other rate-wise ones

I have employed, because we are less willing to grant the definition of nondangerousness in this case, it does have some heuristic value, since we can arbitrarily divide it by 10 and still have an impressive figure. It is no wonder that we are unable to locate in the literature any routinely constituted category of offender that is markedly more dangerous on an *a priori* basis than the Patuxent inmate. In the first place, given that individuals are released from Patuxent, it would take a sizable adult population to generate enough such inmates to justify construction of a separate facility. In the second place, in past times offenders at the next hypothetical order of magnitude of dangerousness would probably have been executed. In short, the Patuxent population is evidently as dangerous as it is possible for an administrative category of offenders to be, and if they are not to be considered dangerous, then nobody is.

If we assume that the CYA population is from 10 to 50 times more dangerous than the average white male over 16.0 (which includes some persons who have been or will be incarcerated; hence the suggested lower bound of 10) and if the Patuxent inmate is more than 10 times as dangerous still, as we have seen from comparing violent offense rates, then he is more than 100 to 500 times as dangerous as the average white male of 16.0. More refined analysis might well increase this estimate by a factor of from 2 to 10 or more, as the unsatisfactory rate-wise comparison above also suggests. Now if we are really unable to predict within this category at all, as critics assert, does it really mean that our ability to predict dangerousness has been discovered to be nonexistent? Hardly. If we cannot differentiate within the category, and if individuals in it are extremely dangerous on the average, then we would predict that *they were all dangerous* — and be right in the actuarial sense that is important to the society as a whole.

The Effect of Homogeneity on Prediction

By examining decision-procedures within ever more homogeneous categories, critics have discovered a wedge that can be driven into any administrative action for the purpose of discrediting it. It is the business of rational bureaucracy or judicial systems to create fairly homogeneous categories. But then further dispositions must inevitably be made within such categories, and decision becomes difficult again and predictions again appear weak. This infinite regress inevitably presents tempting targets to critics, especially as new generations appear who have no memory of the original state of affairs⁶³ The validity of psychiatric testimony would look more impressive if random citizens were included for evaluation of their criminal insanity. Homogeneity is always relative to some framework, and critics of prediction have often narrowed the framework unrealistically, thus creating the appearance of heterogeneity and hence of careless administration. Referrals to Patuxent, for example, are already nearly homogeneous, which makes the diagnostic decisions appear more arbitrary than they are in fact, and decisions concerning commitment or parole also must be made within categories in which the degree of dangerousness has already been held practically constant. The same observation applies to a

recent study by Cocozza and Steadman, who found that psychiatrists could not predict dangerousness accurately among felony defendants already judged incompetent to stand trial.⁶⁴ To claim that we cannot predict dangerousness when the degree of dangerousness has already been controlled is to commit what I have elsewhere called "the partialling fallacy,"⁶⁵ a type of error that is rather common in social science and public debate. In brief, it consists of failing to specify the theoretical context in which one is claiming association or lack of association between two variables (usually, the latter), with consequent confusion on all sides. Associations between predictions and outcomes must similarly be understood with respect to context. This sets in larger perspective my earlier contention, above, that statements by critics concerning the alleged unpredictability of dangerousness were meaningless as they stood. For the empirical record, it might be noted that even within the homogeneous category of 111 criminally insane patients recommended for release in Koppin's study, the combination of age and score on the Legal Dangerousness Scale predicted dangerousness with a correlation (ϕ) of .44, and arrest with a correlation of .53.⁶⁶ These associations are stronger than many of those reported in our social science journals. If 111 60-year-old nuns were added to Koppin's population, thereby increasing heterogeneity, these correlations would rise to .57 and .68, respectively. On the other hand, if prediction were confined to the high-risk category of her study, we would again be at a loss as to how to proceed.

If the critics who have been telling us that dangerousness cannot be predicted in certain populations had added that safety could not be predicted either, and that in fact the populations in question were extremely dangerous on the average, I suspect they would have created less of a stir. For this is what we already knew. The high base rates for safety in certain populations make it possible for individuals to locate themselves successfully in contexts that are predictably highly safe, and to avoid other contexts that are predictably fairly dangerous. Either context, actually, is predicted with equal success, since it is the relation or difference between the two that constitutes the total prediction. However, because outcomes in one context are more homogeneous than outcomes in the other, in view of base rates, one of the choices pays off with greater predictability at the individual level (as distinct from the level of the contexts themselves). In contrast to the ease of moving to a predictably safe environment (*e.g.*, a middle class neighborhood), the low base rates for dangerous behavior cause us to isolate the deviate only under extraordinary provocation, as I noted above, and cause primitive societies to compile a lengthy dossier of provocations before they act, as noted by anthropologist Colson.⁶⁷ Except from the standpoint of the offender, whether the diagnostic label is "witchcraft" or "defective delinquent" is less important than the fact that it enables more effective action finally to be taken.⁶⁸ The dossier in either case is a thick one.

In choosing contexts or categories with an eye to safety, laymen act in accordance with the statistician's and gambler's realization that the probability of an outcome can often be predicted with great success, although within categories of homogeneous or equal probability the individual outcome itself may remain difficult to predict.⁶⁹ Sophisticated persons responsible for personnel selection are also aware of this fact of life

and they adjust to it by recognizing that their obligations are fulfilled when they employ the best decision procedure currently available, even when the correlations in question, as in the case of those cited above, do not attain the high values recommended by textbooks for "individual prediction." Certainly, they do not abrogate their responsibility by refusing to make decisions at all if the decision is one that society has judged necessary despite the going error rate.

Considerations of "Social Cost" and their Relation to Policy Decisions

Implicitly, such mandatory decisions by laymen and their representatives reflect an analysis of the various outcomes in terms of "social cost." Considerations of social cost transform the problem from one of mere prediction, in which the social costs of errors in either direction are deemed equal to each other, to one of attempting to minimize social costs. Although intellectuals often portray such decisions and their underlying value-judgments as highly relativistic, and hence presumably highly idiosyncratic, in practice there is usually considerable consensus behind any long-established, and reasonably public, decision policy, particularly when it is conducted by groups empowered to act on behalf of the public.⁷⁰ In medical diagnosis, for example, the physician usually considers the effects on patients of false positive versus false negative errors, which are not the same for all diseases and treatments.⁷¹ Often, these considerations are so compelling and obvious that fairly uniform policies emerge spontaneously without benefit of prior reflection or coordination. Because of this frequent regularity, it is often possible to reconstruct decision rules, even though they may never have been formulated explicitly in advance.

For example, it might be possible to infer from observing their behavior over a broad range of situations that a parole board implicitly judged the social cost of releasing an inmate who later commits a violent crime to be 50 times greater than that of holding an inmate who will not recidivate violently. This conclusion would follow from the fact that the board tolerated false positive rates up to 98% (49 out of 50), but not higher, for various categories of inmate. Equivalently, it could be said that the board tolerated violent recidivism rates up to, but not including, 2%. Here, it would be clear that in terms of judged social cost the break-even or indifference point in deciding to parole or not to parole inmates occurs when about 49 false positives are found in conjunction with each true positive.⁷² From these data alone, it would be impossible to determine whether the board had performed its social cost analysis implicitly or explicitly. Had it proceeded explicitly, the board would have assigned social costs to all outcomes, including correct predictions. When used in combination with a table of experiences showing probabilities of outcomes for inmates in various categories, such a set of explicit social cost weights would have led to the calculation of total expected social costs associated with every decision to parole or not to parole. This would have made it simple for the board always to choose decisions with the lowest overall social cost for any category of inmate. Just such a model has been presented by the statistician Goodman.⁷³

It should be clear from this brief discussion that although it is possible, and perhaps even desirable, to make decision rules highly explicit by assigning weights to outcomes in advance, it is certainly not necessary that this be done in order for a decision-making body to operate in reasonable accordance with the principle of minimizing social costs. An apparently large ratio of false positives, therefore, may not represent, as Monahan contends, an excessively conservative overprediction of violence so much as a reasoned estimation of relative social costs that Monahan just does not happen to share.⁷⁴ Sidley, for example, attributes "institutional conservatism" in the matter of false positives to sensitivity on the part of Patuxent staff to public indignation over released true positives and false negatives ("Why did they let him out?"), but he apparently refuses to recognize this as the legitimate expression of collective social cost analysis.⁷⁵ Monahan advances a similar model of the psychiatrist as a self-serving decision-maker preoccupied mainly with his reputation, as though his reputation were not also a legitimate function of the social costs of his decisions.⁷⁶ Although a single unfortunate decision to release a violent inmate may lead, as Monahan maintains, to a period of embarrassing adverse publicity, the psychiatrist with a reasonable batting average should be able to weather such a storm with equanimity. Countervailing forces in the form of expert psychiatric testimony for the defense, inmate pressure, and, in the case of Patuxent especially, judicial review, should usually suffice to keep any single psychiatrist from drifting into a position of extreme conservatism. Certainly, the typical situation is far more homeostatic than the one portrayed by critics of prediction, and it should not be regarded as automatically sinister that "psychiatry is intimately connected with the political forces of social control and is influenced by political pressures from legislators and local communities," as Steadman and Cocozza remind us.⁷⁷ What else can one expect from one of the two major modes the community has of gaining relief from exceedingly disruptive and disturbing individuals? The concern should be with due process and not with the inescapable social control function *per se*.

Analysis to this point has indicated that the public is most concerned with relative risks across population categories that differ in probability. Experience reveals that small multiples of even extremely low absolute risks are sufficient to produce an intense public reaction, especially when other utilities are not involved (as they obviously are, for example, in giving up smoking). This sensitivity can be illustrated by the fate of the recent swine flu vaccination program, which was halted when the risk of suffering Guillain-Barré syndrome severe enough to cause permanent injury or death was found to be increased by a factor of 7.5, from perhaps two in a million to perhaps fifteen in a million.⁷⁸ Implicitly, the decision to be immunized or not rested on an analysis of personal costs too, but the swine flu example serves to underscore the direction of comparison with which the public is most concerned, particularly when it feels confident in discounting its own exposure to one of the sets of predictions (*e.g.*, getting severe swine flu in absence of a visible epidemic or being diagnosed defective delinquent⁷⁹). One can imagine, therefore, the attitude of the public toward releasing an inmate who is judged to be, say, 100 times more dangerous than the average white male.

Three More Objections to Claims Concerning Dangerousness

Having set forth the issues at some length, I am now in a position to state three more objections to the general claim that dangerousness cannot be predicted. These three objections are closely intertwined with each other, but since each highlights a particular aspect of the same problem, they are worth stating separately. In addition to failing to be specific as to the context in which prediction was being attempted, the critics have unilaterally converted the problem from one of predicting probabilities to one of predicting individual outcomes; unilaterally altered the usual direction of comparison from that between vertical risk categories (the public's main concern) to that between horizontal cells within a risk category, that is, between false positives and true positives (here, I am imagining the usual table, with different populations in each row and positive or negative outcomes in each column); and, in consequence as well as in addition, have unilaterally determined or reassessed the social cost weights to be assigned implicitly to all of the various possible outcomes, which in turn determine the level of accuracy required in order for predictions of dangerousness to be acceptable. That they have done these last three things in combination with the first, that is, without bothering to be specific as to context and conditions, is perhaps the most damaging feature of their entire argument, for it means that they have retained the capability of adjusting judgments of social cost in any new situation so that those costs will continue to justify their preferred policy recommendations. In short, it suggests that they have given more priority to policy outcomes than to cost outcomes in their thinking, and that the dangerousness issue, therefore, has simply become the useful tool of the moment for effecting the policy outcomes that they favor on some other, *a priori*, basis. It is not surprising, consequently, to find that the critics have not committed themselves explicitly to any particular statement of relative social costs. This leaves them free to emphasize absolute social costs to false positives without ever having taken a precise, and hence challengeable, stand on the relative cost of false negatives *vis-a-vis* false positives. For this reason, the absence of any attempt to determine the average relative dangerousness of false negatives (or released true positives) has not been experienced by some critics as a lack requiring immediate remedy. However, any discussion of the costs of false positives in the absence of such an assessment inevitably presents a one-sided picture.

I think it is fair to say that such one-sidedness is present to an important degree even in the original Baxstrom studies, which provide perhaps the most convincing demonstration, albeit a limited one, of what the authors call "overprediction" of dangerousness. Thus, Steadman and Cocozza inform us repeatedly that the Baxstrom patients were much less dangerous than expected, but this could be a function of the original expectation as well as of the actual outcome.⁸⁰ An exaggerated expectation of dangerousness certainly forms the backdrop for the study, but on whose part? Unreasonable apprehension toward the Baxstrom patients is most convincingly documented in the case of the staffs of the civil hospitals to which the patients were being transferred — *who did not know them*.⁸¹ Once the Baxstrom patients had arrived, hospital staff in their new

surroundings quickly revised their estimations of how dangerous the new patients were, without waiting for the Baxstrom studies to inform them.⁸² Corresponding overestimation of dangerousness on the part of staffs in the original hospitals for the criminally insane is inferred from the continued presence of Baxstrom patients in those facilities, and from remarks by the authors, such as the following: "...if the *Baxstrom* decision had not occurred . . . 199 men would have spent 10 more years of their lives in the criminally insane hospitals."⁸³ This particular dramatic statement encourages the impression that the alternative possibility was complete release into the community, with a consequent avoidance of 2,000 man-years of unnecessary institutionalization. Actually, the real alternative was simply transfer to another hospital for the majority of the group. Only 73, or 36.7%, were ever released to the community, and five of these were rehospitalized and another two were sent to correctional facilities.⁸⁴ Apparently, a central consideration in the continued stay of the Baxstrom patients at the hospitals for the criminally insane was simply that they were still sick, as so many obviously were. Dangerousness certainly entered into this determination, as revealed by the fact that they were denied transfers to civil hospitals,⁸⁵ but only had they not been still sick would the prediction of dangerousness have had to bear the full burden for depriving them of their liberty. A clearcut choice between liberty and confinement would have sharpened the issues, but as things stood the decision was between confinement in one hospital and confinement in another hospital, and the conservative decision of the original physicians to play it safe must be understood for the most part in that context.

Against the Baxstrom authors' backdrop of unrealistic fear, the modest absolute levels of violence actually observed appear in stark contrast, and the false positives appear disproportionately numerous (assuming that the number of released patients, rather than the entire Baxstrom population, is the appropriate denominator for the false positive rate). But over the long run, once the original misconceptions have been dispelled, the impact of the observed rate of violence on the community is what will matter, and there will be little comfort in the thought that however much violence there is, the amount is much less than was once expected. Given the disruptiveness that even nondangerous psychotics can introduce into their surroundings, is it reassuring that (only) 30.6% of the high-risk Baxstrom patients and 14.3% of the group as a whole were involved in violent assaultive acts when released into the community? Steadman and Coccozza are for the most part silent concerning the implied vertical comparison between the meaning of this rate and normal everyday risks, that is, concerning the effect of the new rate on the quality of life in the community.⁸⁶

In view of the ability of the majority of the public often to discount accurately the risk of finding themselves in the position of "false positive," it is certainly appropriate that someone speak up on behalf of the luckless few, since the general public has little incentive for not protecting themselves as fully as possible against false negatives at the expense of high false positive rates. (In reality, there do seem to be countervailing forces usually available, particularly in kinship networks of the putative false positives, but since these would favor my main argument, they can be disregarded for the

present.)⁸⁷ However, speaking up on behalf of false positives ultimately entails making an explicit and defensible comparison of social costs of the two types of error, and this is exactly what I have charged the proponents of the unpredictability of dangerousness with failing to do.

What Is the Basis for Our Attitudes Toward False Positives?

This brings us finally to the question of what our attitude toward false positives should be. We have already seen that an important consideration in the case of offenders is the average dangerousness of true positives, from whom the false positives are indistinguishable prior to release. The average dangerousness of all those in a reasonably homogeneous offender category is simply the average dangerousness of true positives weighted (multiplied) by the predictive value of a positive prediction or diagnosis (the true positive rate for the category). But what irreducible concern should we have toward false positives, quite aside from their relative numbers and the dangerousness of those with whom they are hopelessly confounded — that is, aside from considerations of social cost? Monahan and Cummings, for example, who have objected even to the relatively modest false positive rate of Kozol *et al.*, portray false positives as “innocent men and women” when they state, “We are left with the stark moral issue: how many false positives — how many innocent men and women — are we willing to sacrifice to protect ourselves from one violent individual?”⁸⁸ They go on to liken false positives to individuals who have innocently been convicted of crimes. However, a false positive in Patuxent, who has usually achieved his positive diagnosis by virtue of combining unusual persistence with unusual dangerousness,⁸⁹ is not “innocent” in the same sense as those erroneously convicted of a crime or those who will be the victims of his true positive fellow inmates should we decide to release everyone in his actuarial category.

Consider by way of illustration an individual who periodically rushes into the street and menaces strangers with knives, but who we know from prior experience never commits actual physical harm. Quite apart from the panic he creates, social annoyance with such a false positive would reflect the fact that if allowance for him were to be made, the ability of persons to protect themselves from true positives by reacting promptly in such a critical confrontation would have been impaired. Suppose the individual in question were a practical joker, who lived in Malaysia, where occasionally individuals run amok. Would not our attitude be that he would have to bear the consequences for having made himself indistinguishable from members of a highly dangerous category? Even if he were harmlessly insane, the potential consequences of his actions, such as being shot dead in the street, would be regarded as part of the “tragedy” of his affliction.

Suppose now that the false positive rate were increased, in the one case by formation of a society of practical jokers, in the other by an increase in incidence of a pseudo-amok form of insanity. To whatever extent forbearance developed toward individuals who menaced strangers with knives, would it not be likely to be less in the first case? Normally, it seems to me, assessment of the social cost of false positives takes into account the responsibility of the individual for having made himself indistinguishable

from dangerous true positives in the first place. To the extent that the individual has willfully engaged in actions that contribute to his diagnosis, the social cost to him is discounted accordingly. For example, an individual who presented himself to a psychiatric hospital pretending to have heard "voices" would not be in a favorable position to sue for malpractice if he were diagnosed psychotic.⁹⁰ The critics of the predictability of dangerousness have failed to take this normal social consideration into account, perhaps because much of the impetus for their arguments has derived from studies of the criminally insane, where it is recognized, by definition, that the offender suffers from diminished responsibility. The responsibility we feel for protecting the insane from the consequences of their actions may include the consequences of diagnosis, but there is no such obligation toward criminals who are sane. This conclusion represents another reason for not generalizing the Baxstrom type of situation to Patuxent — the false positives in these cases are not as similar to each other as the abstract label alone seems to suggest.

Just how false is the false positive? This question certainly deserves to be included in any consideration of what our attitude should be. Here I have in mind something other than the validity of the operational measure for detecting recidivism and the problem of length of the follow-up period. These problems should not be forgotten, but there is a still more profound problem that needs to be addressed. Recall that false positives are indistinguishable in advance from true positives, and that a probability of dangerous criminal activity can be assigned to all members of a category. This probability is simply the predictive value of a positive prediction, and since it often combines with a high or low average level of dangerousness for the true positives so as to yield a high or low average level of dangerousness overall, I have argued that there is warrant for treating the entire category in an actuarial sense. The idea of social cost follows naturally from actuarial considerations and provides the intellectual counterpoise to the critics' exclusive emphasis upon costs to false positives.

As just outlined, the model that describes the relation between release outcomes and particular inmate categories can be characterized as *probabilistic*. Tacitly, the model tolerates, but does not endorse, the inference that heterogeneity of outcome implies heterogeneity of input, an inference that might be termed the central assumption of the dangerousness critics. Sidley makes this central assumption quite explicit:

Whatever be the patterns of observation that lead to the conclusion, the diagnosis of defective delinquent must be homogeneous with respect to the outcome, in that persons so diagnosed and not . . . otherwise restrained commit . . . crimes of violence . . . shortly after they are so diagnosed. Furthermore, individuals diagnosed not defective delinquent must be homogeneous with respect to outcome, in that persons so diagnosed and not restrained do not commit crimes of violence.⁹¹

Here, Sidley insists on making this inference so that he can use homogeneity of outcome as a test for the existence of a valid diagnostic category called "defective delinquent," a test that he knows will be flunked because of the

presence of significant numbers of false positives. According to Sidley's reasoning, the failure of some positives to be violent implies that they really were false (*i.e.*, different) initially, as distinct from being simply practically false in the more restricted sense of outcome. Again, it should be emphasized that this inference, which is untestable, is not intrinsic to the descriptive model, however much the critics urge their reasoning upon us. The inference belongs merely to a special case of the model that it is of interest to distinguish from another special case, which I shall identify in a moment. The general descriptive model that subsumes these two special cases is indifferent to the possibility that individuals in the diagnostic category are different from each other — it merely accepts the fact that we cannot tell them apart for purposes of predicting outcome. Hence, the general model can be termed stochastic (or random) at the level of the category. Here, the probability of a true positive, the predictive value, is a parameter (identifying statistic) that is a structural property of the category. Although this parameter can be applied usefully to the individuals in the category, it is clearly not necessarily a structural property of any individual.⁹²

Sidley's special case can be likened to a box with six dice in it, three of which have ones on all six sides, and three of which have sixes on all six sides. It is a parameter of the box that if we draw one die at random and toss it without being permitted to examine it, the probability of getting a six is .5. No single die has this property, since the probability for a single die of rolling a six is either zero or one; that is, once the die has been drawn the exact outcome is completely determined prior to the toss. Hence, the model is restricted to being stochastic at the level of the box (category).⁹³

Let us contrast the preceding example, which we may call the "zero-one" case, with the other special case, in which the model is stochastic at the level of the individual as well. In this case, the box also contains six dice, but each die has ones on three of its sides and sixes on the remaining three sides. The probability of rolling a six after a random draw remains .5. However, this parameter is now a structural property of each die as well as of the box, and the exact outcome is not determined prior to the toss. In this prior sense, whatever the outcome, no positive is intrinsically more false than any other positive, when predicting a six.

On the basis of the available facts concerning predictability of outcomes, it is impossible to tell which of these special cases is applicable to the diagnostic prediction of dangerousness. It is as though we can observe only the upturned face of a die following a toss, and must return it to the box before tossing again, although we are able to keep track of successive outcomes, thereby arriving at the parameter .5. It is possible, however, and instructive, to consider which special case caricatures what we know of reality to a greater degree.

Not many persons would wish to defend the proposition that the probability of an individual's behaving in a violent manner was necessarily either zero or one. Such a proposition contradicts what we know of the complexity of organisms and circumstances.⁹⁴ But any relaxation of the demand that probabilities assume only one of these two values means that one of the two special cases begins to degenerate into the other. Indeed, it is doubtful whether the distinction between cases can be maintained at all in

the face of any compromise, even one designed to make the “zero-one” case just a shade more realistic. If extreme values were retained as the theoretical replacement for the “zero-one” case, the question would arise as to whether those values are not just as unrealistic as zero or one, and wherever the line were to be drawn between cases, there would follow a demand for enunciating the principle underlying its being placed at that particular point. In short, there is no way of justifying a distinction between special cases that assigns to one of them any probability value other than the empirically observed one, unless it is zero or one, when the individuals are truly indistinguishable initially; this empirical value, of course, represents the ultimate degree of degeneration for the “zero-one” case.

It is difficult to see that the alternative special case caricatures reality at all. First, it is widely recognized, from a number of different methodological perspectives, that associations between variables, between predictions and outcomes, or between simultaneous indicators break down as one focuses on ever more homogeneous reference classes.⁹⁵ Second, sophisticated developments in social science, statistics, and philosophy of science over the last twenty years increasingly assign a role to stochastic processes under such circumstances, and regard the determination of a probability for the homogeneous reference class as well as the assignment of individuals to such classes as worthy scientific goals.⁹⁶ Within this framework, “defective delinquent” or “dangerousness” would be regarded as a “disposition concept” that explained a propensity to dangerous behavior, the diagnosis would infer the disposition, and strength of the disposition itself would be measured in terms of a probability.⁹⁷ In this sense, the critics could be said to mistake prediction of a disposition for a prediction of behavior (an error similar to mistaking the prediction of probability for the prediction of individual outcomes⁹⁸). Both disposition concepts and stochastic processes that are not further explainable either in principle or for practical reasons of complexity are quite familiar in modern science. When dealing with radioactive elements, for example, we know better than to attempt to distinguish between atoms that will or will not decay during our lifetime — we segregate them all. Sex determination of children is treated as stochastic for purely practical reasons, on the other hand, and weather reports now give the probability of precipitation for a combination of reasons. Even the dictionary defines “dangerous” as “able or likely to inflict injury,” that is, as a stochastic concept.

The ultimate demonstration of the stochastic model of dangerousness would be realized if we could recycle released inmates and show that the ones who recidivated in any one period at risk were statistically independent of the ones who recidivated in other cycles, although the rate remained constant.⁹⁹ Substantively, enough is known about recidivism and crime to justify stochastic considerations. For example, situational factors, strongly emphasized by Wilkins, and recognized in conjunction with personal factors by Wenk and Emrich, clearly introduce an element of randomness, even if individuals do carry different probabilities of becoming violent.¹⁰⁰ Oddly enough, such situational factors and their interaction with personality are often emphasized by those who wish to obscure the main effect of individual personality differences, such as Wilkins, but actually they lead to the

conclusion that when individual personality is held relatively constant within a diagnostic category, then there may actually be little real difference between "false" positives and "true" positives, the residual outcomes being determined stochastically.¹⁰¹ This conclusion obviously boomerangs against the strong distinction between false positives and true positives that is implied in Wilkins' position concerning the prediction of dangerousness. Whether or not a released inmate recidivates may depend on chance factors such as recalling something his therapist said at the moment of temptation or falling in with the right companions. The aleatory component in crime must never be overlooked.¹⁰²

For some reason, the critics of the predictability of dangerousness and the authors of the evaluation report who have applied their conclusions to Patuxent have ignored these scientific developments and familiar considerations. Consequently, the special stochastic case that I have introduced as an alternative to the special case favored by the critics is never discussed in this literature, although its relevance is apparent. Ironically, in view of misgivings concerning the relevance of the "medical model" to Patuxent in the evaluation report (pp. iii, 6), the critics themselves may be misapplying a medical diagnostic model, appropriate for predicting physical disease, to the problem of predicting dangerous behavior. In medical diagnosis of physical disease, the patient either has the disease or he does not, and so the "zero-one" special case applies.¹⁰³ The diagnostician may not be able to tell right away whether or not the patient has the disease, but time and further investigative procedures will invariably produce evidence that is regarded as conclusive even in occult cases. As a result, a false positive really is false, *ab initio*.

To the extent that individuals are truly different, treating them as equally dangerous raises questions of justice that tend to override all other considerations. Much of the force of the rhetoric of "false" positives and "innocent men and women" derives from the high priority we accord to justice in our society in combination with the presumption that the false and true positives really were profoundly different from each other at the start despite our not being able to tell them apart. In short, justice appears to be a quality with "infinite utility." Because they make a shambles of decision theory, qualities with this property are usually excluded from analysis.¹⁰⁴ It is easy to understand, then, why even relatively modest false positive rates, such as those obtained by Kozol *et al.*, can be brought under heavy fire. However, the issue of justice is usually not as simple as the "zero-one" model would have us believe. I have already pointed out that even under the "zero-one" model, an individual must bear some responsibility for having made himself indistinguishable from truly dangerous others. Just how indistinguishable this can be is witnessed in the case of Patuxent inmates, with 4.9 prior convictions on the average, and hence 4.9 prior releases that are tantamount in our society to predictions of "not dangerous." With respect to those 4.9 prior predictions, the average Patuxent inmate has repeatedly proven himself a false *negative*. Since false negatives are equivalent in outcome to true positives, it is a wonder that such well-established individuals should suddenly be regarded as profoundly different from true positives, whatever the next outcome. In any case, we

now see that stochastic considerations call into question whether false positives are in principle different from true positives at all, except with respect to chance outcome. Obviously, this places the problem of justice in a new light altogether, for now we would be treating as equally dangerous individuals who really were equally dangerous, except for chance factors.

Normative Considerations in the Substantive Interpretation of Abstract Probabilities

With such fundamental prior distinctions between false positives and true positives set aside, only the question of worthwhileness of the prediction remains. The critics have encouraged the view that unless the predictive value is extremely high, so that false positive outcomes are proportionately few, intervention may not be worthwhile. This view deserves further analysis. For the most part, worthwhileness translates directly into social cost analysis, which under the completely stochastic special case is now relieved of the heavy burden of injustice imposed by the assumption that false positives were genuinely different initially. However, there is a residual consideration of justice involved in the notion of confining a number of persons in order to prevent a subset from committing dangerous acts, even though they may all be equally deserving. How large should we expect that subset to be? Sidley, for example, states that if the "cost of preventing certain crimes is the deprivation of fundamental liberties of some people, the cost is too high."¹⁰⁵ This could be read to apply to either special case of the general descriptive model that I introduced in the preceding section. Because of its location in our hierarchy of values, justice must always be given special attention. In the present context, the critics seem to believe that the answer to the problem of justice can be read directly from the proportion of false outcomes for predicted positives, and thus that it can be determined, for example, that two false positives for every true positive is too high a price to pay in justice, even when only the outcome is at issue.

However, there is a scaling problem involved in every probabilistic phenomenon that requires more knowledge than just the probability alone for its solution. To some extent, this problem is related to the vertical comparison of risk-ratios that I discussed earlier. But it goes beyond simple comparisons of relative multiples of risk between actuarial categories, since the solution also depends on knowing the range of probabilities that the phenomenon typically manifests, and multiples of risks do not always convey that information. For example, it is impossible for an ignorant but intelligent foreigner to determine from the absolute level of the probability alone whether a .333 season batting average is good or bad, although he could easily tell that it is only half as good as one of .666. But .666 does not occur as a season batting average. The range of interest of many phenomena is found to lie within only a very narrow segment of the probability continuum, and that segment often contains probabilities that are unimpressively low in absolute value. Batting averages, for example, typically range only between .2 and .4. How could our ignorant foreigner know that a mere .097 probability of hitting a home run described Babe Ruth in his greatest season?¹⁰⁶ Similarly, probabilities for individuals committing

dangerous crimes within three years may seldom range higher than .3 to .5 in our society. When the probability becomes higher than that, it may apply to extremely unusual phenomena, such as armed desperadoes on a killing rampage, or extremely trivial cases, such as bank robbers just before they leave their hideout on the way to a bank. In short, if we inquired into the matter, we might find that probabilities that appear modest in absolute value actually describe the Babe Ruths of dangerousness, and that it is unrealistic to expect values ever to get any higher than that. When they do, the societal reaction may be to shoot first and ask questions later. What this means, then, is that if society is ever to protect itself routinely against individuals that it experiences as the most dangerous of all, it is going to have to do so at probability levels between .3 and .5 or not do it at all. Under such circumstances, as long as the assumption is tenable that the individuals concerned constitute a relatively homogeneous reference class with respect to their individual probabilities, it does not seem appropriate to me to construe the false positive outcome rate as an issue of justice.¹⁰⁷

How the Dangerousness Issue was Applied Against Patuxent

Now let us turn to the evaluation report on Patuxent Institution, where the relatively untried conclusions of the recent literature concerning the prediction of dangerousness have received their first major practical application (pp. iii-iv, vi, 22, 28-30, 43-45, 63-65, 181-183, 188) — with devastating effect. The supposed hopelessness of recognizing dangerousness is perhaps the single most influential assumption underlying the report's recommendations. The report itself adds nothing really new to the empirical evidence for this assumption, relying instead mainly on the literature I have thus far reviewed. I pass over the report's own uncritical review of this literature, and turn instead to passages that illustrate the role of the dangerousness issue in the evaluation and its impact on the final recommendations.

It [was] concluded that on the basis of present knowledge it is impossible to predict dangerousness. To ensure, therefore, that all or most dangerous offenders are incarcerated, far more offenders are included in that category than are in fact potentially dangerous. Thus the indeterminate sentence, whose justification rests on keeping the dangerous offender behind bars until he is no longer dangerous, is actually applied to many non-dangerous offenders as well, and for this reason cannot be considered any more valid than the medical treatment model.¹⁰⁸

Decision making was found to be effective to the extent that those committed to Patuxent did in fact meet the statutory definition of defective delinquent. On the other hand, as a crime control measure the process is seen to be of dubious effectiveness, since it requires the commitment of far more persons than are actually dangerous in order to ensure that those few who are dangerous are removed from society.¹⁰⁹

We have also seen, in the section on predicting dangerousness, that dangerous behavior is greatly over-predicted by treatment staff. This in part accounts for many inmates being incarcerated longer than they would be in the regular correctional system.¹¹⁰

The research . . . shows dangerousness cannot be predicted with any accuracy; this overprediction of dangerousness is common.¹¹¹

Finally, the empirical evidence justifying the indeterminate sentence, namely treatment within a medical model based on the ability to diagnose and predict dangerousness, has been shown to be lacking. This leaves no valid foundation for the indeterminate sentence.¹¹²

The problem of "false positives" in the prediction of dangerousness, discussed in Chapters I and III, is present in the Patuxent setting as well. People are detained at Patuxent in order to prevent them from committing new crimes, but many of those detained do not constitute a risk.¹¹³

But, as indicated earlier, many inmates who are no longer dangerous are retained at Patuxent.¹¹⁴

. . . it is still impossible to predict with any accuracy which inmates still constitute a danger to society.¹¹⁵

Predicting Dangerousness. Considerable disagreement exists about the concept of "defective delinquent" and its congruence to the concepts of "psychopath" or "sociopath." But when it comes to the predictability of dangerousness, the evidence from previous research studies . . . is clear: dangerousness cannot be reliably predicted. This has an important policy consequence: in order to reduce the number of false negatives . . . to a minimum, the number of false positives must be increased to a large number. False positives are those who are predicted to be dangerous, but who are in fact not dangerous. [Note the "zero-one" model implied here.] The evidence is clear on this point as well: dangerousness is greatly overpredicted, as every study reviewed has found.

Indeterminate Sentence. The indeterminate sentence has been shown to result in offenders being incarcerated for longer periods, owing to an inability, thus far, to predict dangerousness accurately and the consequent tendency to over-predict. The indeterminate sentence was designed to make treatment possible; it was also designed to hold the "uncured" to protect society. These activities require accurate distinctions to be made between dangerous and non-dangerous offenders and, as we have seen, such distinctions cannot be made with the present state of knowledge.

Therefore, attempts to validate the model based on the corrections

literature indicate no valid or reliable basis for the indeterminate sentence.¹¹⁶

This reasoning culminated in the recommendation that the indeterminate sentence provision of Article 31B be repealed, with the following comment:

The available evidence indicates that one important basis for the indeterminate sentence, namely, the ability to predict dangerousness, is not valid.¹¹⁷

and in the recommendation that the designation "defective delinquent" be abolished, with this comment:

Legally, the designation "defective delinquent" connotes either past dangerousness (primarily in terms of crimes against the person) or presumed future dangerousness, or both. Since the study team found that dangerousness cannot be accurately predicted, many non-dangerous offenders are unfairly held beyond their original sentences. But since the legal definition of defective delinquency includes the dimension of dangerousness, a practical difficulty exists in separating the defective delinquent from the others.¹¹⁸

Thus, two out of the report's three recommendations were heavily based on the dangerousness issue, and both of these recommendations explicitly or implicitly found fault with Patuxent's performance of its custodial function, which was its major function according to the declared intentions of founding documents.¹¹⁹

Miscellaneous Issues

Minor amounts of additional support for these recommendations against the indeterminate sentence were claimed from two other sources. First, the study team concluded that "the research literature provides little or no justification for the indeterminate sentence as a therapeutic tool" (p. 44). The research literature in question consisted mainly of rather polemical unsubstantiated claims by Prettyman (pp. 35, 42-43);¹²⁰ a general observation by Monahan and Cummings (p. 42) that inmates spend more time in prison under indeterminate sentences than they might otherwise;¹²¹ a similar observation by the study team concerning Patuxent inmates (pp. 63, 121, 182); general conclusions from the broad survey by Martinson that rehabilitation treatments do not reduce recidivism;¹²² and a specific and somewhat irrelevant finding that the length of short indeterminate sentences (ranging between 6 and 14 months) served in institutions for juvenile offenders was mainly a function of bed space (p. 44), the implication apparently being that some inmates were held longer than necessary in order to keep the beds filled. No cognizance was taken of the research literature concerned with anxiety as a motivation for entering therapy, remaining in it, and benefitting from it, that I have cited above.¹²³

But for the dangerousness issue, longer retention of inmates would

normally be looked upon as the expected goal for Patuxent, so it is difficult to see that particular criticism as adding anything to the evidence. The pessimism that many readers have found in Martinson's review of rehabilitation programs might even be invoked in support of the need for indefinite confinement of certain classes of inmates, although Palmer has recently pointed out that Martinson's broader conclusions recognized many isolated exceptions, and hence the implications of his survey for any specific program would have to be traced rather carefully.¹²⁴ A more reasonable interpretation of the "bed space" finding is that although the juveniles would have benefitted from longer stays, some of the institutions could not accommodate them because of budgetary restrictions. This converts the example from one suggesting mindless retention of inmates to one that merely illustrates the consequences of insufficient resources. Cowden and Bassett found that "maturation is a significant factor in the reduction of serious adjustment problems of 'high risk' young offenders and of their propensities toward recidivism. This strongly suggests extension of the institutional period for young offenders. . . ."¹²⁵ Thus, our concern should be directed toward those institutions that released young offenders prematurely because of overcrowding, rather than toward those that held them longer, with implications for Patuxent exactly opposite to those the evaluation team seems to have had in mind.

The second minor source of additional support for the recommendations came from the evaluation's recidivism study, in the form of the following observation (p. 63): "in the study team's opinion, the large number of people released by court action despite a finding by the IBR [Institutional Board of Review] that the person is not a safe candidate for release, who do not commit new crimes of violence — 74.3 percent — or new crimes of any type — 45.7 percent — indicate that the release process . . . is ineffective." By implication, these individuals would be false positives. It should be noted that the rates above are based on conviction rather than arrest as the criterion. When arrest is substituted, thus approximating more closely the kind of criterion used generally throughout the dangerousness literature, the false positive rates drop to 66.7% and 25.7% for violent and all arrests, respectively (p. 124).

The inmates actually released by the Institutional Board of Review had somewhat lower recidivism rates than the ones released by the court. This suggests that the Board was operating in accordance with a social cost analysis different from the ones employed by the courts (assuming they had made one) and by the evaluation team. Had the court-released inmates displayed the lower recidivism rates, it would have suggested not only a different social cost analysis, but perhaps also that the Review Board did not know its business. Obviously, this was not the case.

The violent arrest recidivism rate implicitly "tolerated" by the Review Board in its decisions was 31.2%, which implies that the social cost of violent recidivism was judged not as high as 3.2 times the social cost of confining an inmate in the parolee category (100% divided by 31.2% equals 3.2). If we stipulate that there are no social costs of confinement for true positives, so that costs of confinement would have been borne entirely by the 68.8% who became false positives, then the implied social cost of confining a false

positive would be not less than 45% of the social cost of a violent act leading to arrest (31.2% divided by 68.8% equals 45%). In light of the analysis presented earlier in this paper concerning the meaning of "false positives," and of the nature of the inmates in question, this does not seem to be an unreasonably low appraisal of the social costs of false positiveness. It could even be argued that, in view of the prior records of Patuxent inmates, the Review Board employed a release policy that was rather generous toward potential false positives. In any case, simply gesturing toward false positive rates, as the report does here, confounds the false positiveness of the prediction with the false positiveness of the inmate (thus implying that he was initially different), and tells us nothing about social costs as society in general would perceive them.

Note that the statement quoted above concerning false positive rates among inmates released by the courts tends to emphasize the rate for violent offenders over the lower rate for offenders in general. This is an example of a shift in the meaning of the defective delinquent law that is achieved throughout the report by its preoccupation with physical dangerousness as the predictive criterion of diagnosis as a defective delinquent. Since the problem of predicting dangerousness has arisen out of a literature concerned exclusively with violence to persons, whenever the report makes reference to this problem the reader clearly understands it to refer to violence to persons. The long quotations that I presented, above, to illustrate the role of the dangerousness issue, foster the impression that the function of Patuxent was to safeguard the public from violent behavior only. This tack is also conspicuous in the work of critics of Patuxent, such as Sidley.¹²⁶ Because of this narrow focus, the low false positive rate for general arrests, 25.7%, received little weight in the report's discussion of false positives, as though society did not deserve adequate protection against individuals who commit hundreds of burglaries, for example, when they are at large.¹²⁷ Article 31B had stated that defective delinquents were persons who "demonstrate an actual danger to *society*" (emphasis added), and the fabric of society can be endangered by disrupting property relations as well as by injuring persons physically. "Society" refers to an organization of social relations and expectations, and it certainly seems to have been the intention of framers of the defective delinquent law to protect society in this holistic sense as well as to protect its personnel from physical harm. By subtly restricting the meaning of "society" so that it refers only to persons instead of to an organized form of human action, the critics have beguiled us into judging Patuxent according to a misleading set of standards. Had this redefinition of legislative intent not occurred, the issue of the predictability of dangerousness and its associated literature would have seemed less crucial from the start.

Summary of the Issues Concerning Dangerousness

The evaluation report added nothing new to the dangerousness literature, but simply applied its conclusions to Patuxent, as I have just demonstrated. Consequently, a brief summary of the key points in my review of that literature will suffice to show that the same criticisms apply to the report as

well. We have seen that claims concerning the unpredictability of dangerousness have ignored both important differences between populations in actual degree of dangerousness, and the implications of the degree of dangerousness if indeed we cannot distinguish within a population who is dangerous and who is safe. When such homogeneous populations exhibit a high average level of dangerousness, as in the case of Patuxent inmates, their members are all equally dangerous for practical purposes. The actual degree of dangerousness of Patuxent inmates is never fully conveyed by the evaluation report, even though the evaluation team was familiar with the Legal Dangerousness Scale, on which the average Patuxent inmate scores 14 out of a possible 15. The report fosters the notion that large numbers of Patuxent inmates are not dangerous at all. Yet, the report paradoxically notes that the job of custodial officer at Patuxent is "dangerous" and that the custodial officers are fearful of the inmates (p. 99). Ironically, having deprived themselves of the right to refer to Patuxent inmates as "dangerous," the evaluation team at one point resorts to the term "habitual serious offender" (p. vi). One wonders in what sense an inmate might be "habitual" and "serious" if he is not in fact "dangerous."

In the process of ignoring differences between populations in degree of dangerousness, modest but respectable empirical correlations between predictions and outcomes have been passed over in silence. Considerations of social cost have not received any systematic treatment even in the abstract and, except for occasional acknowledgements of the moral dilemma, have thus been ignored. By default this has given critics a free hand in imposing their own social cost analysis on the discussion covertly. This operation has been characterized by an exclusive emphasis upon implicit social cost comparisons in one direction only, and by a unilateral redefinition of the prediction problem, from one involving probabilities to one involving individual outcomes. The concern with individual outcomes harks back to a medical model of physical disease and its diagnosis, where the structural probability that the patient has the disease is either zero or one. The presupposition of such a "zero-one" probability model for a disposition concept such as dangerousness maximizes the appearance of injustice to false positives. At the same time, the false positive's own responsibility for his plight is overlooked. Finally, an even more plausible model that is stochastic at the individual level has been omitted from consideration entirely, despite the widespread acceptance of completely stochastic models in social and physical science generally. According to this alternative model, false positives are less profoundly different from true positives than the concern of the critics would lead us to believe, and the problem of injustice to false positives is thereby placed in a new light. In connection with this alternative model, it was pointed out that although the probability of a certain outcome can be used to measure the strength of a disposition, the scale of the phenomenon cannot be read directly from the probabilities without additional normative information. Consequently, probabilities that are absolutely rather low may actually represent the most intense manifestations of a disposition phenomenon known to human experience. The most dangerous batters in history, for example, actually had quite modest probabilities associated with their performances at the plate.

THE EVALUATION REPORT'S RECIDIVISM STUDY

The claim that Patuxent was unfairly incapacitating the wrong individuals was used, as we have seen, to support recommendations for abolition of both the indeterminate sentence and the diagnostic category for which it was intended. Although that claim led directly to a judgment of Patuxent in terms of its supposed success in performing its primary function of incapacitation, the argument also touched upon the institution's secondary therapeutic function at several points. These points concerned the role of the indeterminate sentence in motivating inmates to participate constructively in therapy and the accuracy of medical diagnosis for purposes of commitment to and release from therapy (and confinement). I have dealt with these issues as they have arisen.

Now we come to a part of the evaluation report that concerns itself more directly with the therapeutic function alone, as that function was manifested in successful rehabilitation and, to be realistic, selection for release. This part of the report involves the output from treatment and diagnosis as reflected in recidivism. With the incapacitation function supposedly called into question by the dangerousness issue, the effectiveness of Patuxent would come to depend entirely on its therapeutic success with those inmates the institution itself had designated as relatively successful cases by placing them on parole.

Because of their rarity, comparative recidivism studies are always of interest, and the one performed by the evaluation team is no exception. The team constituted five groups for comparison, the last three of which were held in, and then eventually released from, the regular correctional system:

Group 1 — All Patuxent inmates put on parole status in 1971 and 1972.

Group 2 — All Patuxent inmates released in 1971 and 1972 by redetermination hearings or legal technicalities.

Group 3 — All inmates from 1964 through 1972 referred to Patuxent for evaluation and found defective delinquent (DD) by Patuxent staff, but not certified DD by the court.

Group 4 — All inmates referred for evaluation in 1967 and not found defective delinquent by Patuxent staff.

Group 5 — A sample of 100 inmates paroled in 1971 or 1972 from Maryland correctional facilities.¹²⁸

In the evaluation report, data concerning the prior criminal activity of members of these groups (input) and data concerning their criminal activity subsequent to parole (output) were presented in separate tables. No effort was made to meld the input data with the output data so as to obtain summary statistics reflecting all of the information simultaneously. Instead, the report provided a description that took account of both the input and output data verbally — that is, in a nonquantitative manner. The report

concluded: "Overall, Patuxent inmates enter Patuxent with more serious criminal histories than the comparison groups and do about the same or slightly better on all indicators of recidivism" (p. 127). Consequently, readers could easily be left with separate impressions of Patuxent inmates as more severe at input and somewhat less severe at output without it occurring to them that when considered together the two sets of facts might add up to a considerable difference at output. Such a difference would prove important in policy analysis.

Since the individuals in the five groups were neither randomly assigned to the five "experiment treatments" nor matched on all relevant criteria prior to assignment, prior (input) differences between the groups must somehow be taken into account when interpreting their outcomes (output) as a basis for policy decisions. Conceivably, this could have been attempted by introducing certain types of statistical control in the course of the analysis (such as analysis of covariance). Such methods have their problems, but their application in the present circumstances would certainly have had heuristic value, since the results could have been interpreted provisionally on the basis of how the outcomes would appear if matching and other formal assumptions had been met. The aim of such controls is not to increase predictability of subsequent criminal activity, but to enable us to interpret the relative effectiveness of various policies without its being obscured by differences in input, accidental or otherwise.

The evaluation team did not hesitate to employ statistical controls of exactly this type in its cost-benefit analysis (pp. 129-179); hence their omission in the recidivism study cannot be accounted for on the basis of methodological purism. Although the team is to be congratulated for providing descriptive statistics for the five groups at input, their failure to follow through with some kind of statistical control procedure is puzzling.

In Table I, I have brought the input and output data together in distilled form within a single table, so that readers can comprehend both kinds of data simultaneously. The five groups are as defined above. In addition, I have expressed the relevant statistics for each group in terms of their relation to one group, the Patuxent parolees. This facilitates comparisons.

Columns (1) and (2) of Table I show that Patuxent parolees were substantially more severe in terms of their prior record than all other groups. Let us focus on the comparison between groups 1 and 3 for illustration, since group 3, which was diagnosed defective delinquent but not certified by the court, is conceptually the nearest approximation to a matched control group for Patuxent parolees. In column (1), we see that the average of the three ratios for mean prior arrests, mean prior convictions, and mean prior incarcerations was 1.40, which means that Patuxent parolees were 1.40 times as severe, on the average, as uncertified "defective delinquents" at input. When we shift from crimes in general in column (1) to violent crimes only in column (2), the Patuxent parolees are found to be 1.62 times more severe. Now let us follow the comparison of these two groups over to the output stage, in columns (3) and (4). There we find that the Patuxent parolees recidivate .90 times as often with respect to crimes in general and .76 times as often with respect to violent crime. Looking at columns (3) and (4) generally, group 1 does better than all other groups except the relatively

nonviolent prison comparison sample in group 5. This one discrepancy is quite plainly a function of real differences in the composition of these two populations at the start, and it illustrates the problem of evaluating treatment results fairly for policy-making purposes when the material fed through the various treatments is not uniform overall.

In columns (5) and (6) I have created sets of numbers that express the relation of the output to the input. This is for heuristic purposes only, and it should be understood most emphatically that these numbers are not to be interpreted as an indication of the relative magnitudes of the recidivism rates had all groups been equated for input. Such an interpretation would involve the assumption that a doubling of input severity would lead to a doubling of recidivism rates (output severity). The true relation between input and output is certainly not that strong. Doubling input, for example, might result in only a 10% increase in output severity instead of a 100% increase. The numbers in columns (5) and (6) merely serve to indicate that certain important questions about the recidivism study remain unanswered.

In his study of 118 uncertified "defective delinquents," Hodges found that 80.5% were eventually convicted of crimes.¹²⁹ This group corresponds to group 3 in Table 1, which had a conviction rate of only 32.6% (p. 124). Hodges' sample was larger and his follow-up period was not limited to three years. This discrepancy in recidivism rates makes one wonder whether all of the relevant information has been assembled for judging the recidivism rate of Patuxent parolees. Their long-term recidivism rate after parole, for example, might be different from that of other groups. Certainly, the definitive study of recidivism for Patuxent inmates remains to be done — if it

TABLE I
COMPARATIVE RECIDIVISM, BOTH GENERAL AND VIOLENT, BASED ON INDICES OF
PRIOR SEVERITY (Input), INDICES OF PAROLE SUCCESS (Output), AND
THE RELATION BETWEEN THE TWO (Output/Input Index)

Populations compared	Sample size	Input ratios		Output ratios		Output/Input index	
		General ^a	Violent ^b	General ^c	Violent ^d	General ^e	Violent ^f
Patuxent parolees divided by:		(1)	(2)	(3)	(4)	(5)	(6)
1. Patuxent parolees	106	1.00	1.00	1.00	1.00	1.00	1.00
2. Patuxent, partly treated	105	1.39	1.88	.81	.94	.58	.50
3. Staff found DD—not certified	46	1.40	1.62	.90	.76	.64	.47
4. Not DD after evaluation	39	1.96	2.86	.91	.94	.46	.33
5. Prison comparison group	74	1.46	1.88	.95	1.28	.65	.68

Source: Contract Research Corporation: The Evaluation of Patuxent Institution. Belmont, Massachusetts, 25 February, 1977, Exhibits VI-1 and VI-3.

^aThe average of mean prior arrests, prior convictions, and prior incarcerations.

^bThe average of percentages with prior violent arrest and prior violent conviction.

^cPercentage ever arrested within three years of parole.

^dPercentage with violent arrest within three years of parole.

^eColumn (3) divided by column (1). For illustration only. See text.

^fColumn (4) divided by column (2). For illustration only. See text.

is possible to do at all in the absence of strict adherence to principles of experimental design.

One thing that is surprising about the study is that the recidivism rates turned out to be so similar. This similarity suggests that it is psychologically difficult for us to confine other individuals in our society, and that parole boards respond to common pressures with common thresholds for release that are triggered by reasonably compliant behavior over a reasonably long period in the recent past of the inmate, other things being held constant, such as the severity of the last offense.¹³⁰ Such a reluctance to confine would account in part for the "nullification in practice"¹³¹ of existing "three-time loser laws," such as the one recently introduced in Maryland to provide the protection for the public previously afforded by Patuxent and the indeterminate sentence. Such habitual criminal laws center responsibility for their application in just a few individuals, whereas the defective delinquent law diffused this responsibility over the court (which referred for diagnosis), the Patuxent staff, the court again (which certified the diagnosis), and the Institutional Board of Review, which reconsidered each case yearly. By involving so many individuals in the decision, this elaborate set of procedures may have made it possible to confine dangerous offenders longer than would have been possible under habitual criminal statutes, which always have an element of arbitrariness as well since they employ the "law of the act" rather than the "law of the individual offender."¹³²

This greater length of confinement for serious offenders, which is noted at many points in the evaluation report, was probably the defective delinquent law's most valuable function. Because of the false positive issue, however, the report tends to view longer incapacitation as a disadvantage of the law. The incapacitation function is even overlooked in the cost-benefit study, which fails to weigh into the balance crimes which might have been committed by those who were never released, and crimes uncommitted as the result of longer confinement even of those who eventually were released. Let us take this last category as an example. The cost-benefit study projects that within 2.9 years¹³³ of parole, 69% of Patuxent inmates are reincarcerated as opposed to 72% of equivalent inmates sent through the regular correctional system. (The high projected incarceration rate of 69% in this part of the report appears to conflict with the false positive rates and concern over false positives that appear elsewhere in the report.) According to the cost-benefit study, the savings in crimes is only 3 percentage points (72% minus 69%). However, if the average released inmate commits .69 crimes in 2.9 years, then he is committing crimes at the rate of .24 per year at this point in his career. If he was held at Patuxent 2.4 years longer than he would have been at some other facility, as the report maintains, then .57 crimes were postponed or avoided during those extra 2.4 years of confinement. This could boost the savings to .60 serious crimes per inmate, instead of the .03 acknowledged in the report (p. 178), depending on the scenario one adopts for the cycle of later confinements and periods at liberty, and on where in the sequence one terminates the scenario.¹³⁴ Whatever the exact facts concerning these projections, it does seem clear that the cost-benefit study took into account only the reduction in crimes subsequent to release, that is, the reduction that might be ascribed to

rehabilitation and selection. No attention was given to the reduction achieved as the result of longer or permanent incapacitation, which according to historical documents has always been the institution's main objective.

Although the report displays much awareness of this original main objective, it occasionally lapses into clichés of the following sort: "The legislative history of Patuxent Institution and Article 31-B reflect the optimism of the fifties about the contribution that psychiatry can make in dealing with the criminal offender" (p. 6). Certainly we have seen from the quotations that I presented earlier that there was no optimism over prospects for therapy. In fact, in 1958, one of the founders of Patuxent stated quite forcefully: "I do not believe that the work of this Institution to date is to be assessed on the basis of startling and dramatic rehabilitation and parole. Less dramatic but more important is . . . that many sensational headlines have been avoided because Maryland has been able to keep in quarantine many deadly and dangerous convicted criminals . . ." ¹³⁵ Since the Maryland legislature promptly followed the recommendations of the evaluation report by abolishing the indeterminate sentence and the concept of defective delinquency, and since habitual criminal laws have proved ineffective in the past, it may no longer be possible to maintain that quarantine. ¹³⁶

References

1. Annotated Code of the Public General Laws of Maryland. The Advisory and Governing Boards were merged in 1977 as the Board of Patuxent Institution.
2. I refer here mainly to the community making its reactions known through newspaper editorials and letters. At the extreme, the Governor could change the personnel of the Board as their appointments expire. For an unusual example, having nothing to do with Patuxent, in which attempts were made to fire parole board members, see Payson HE: Suicide among males in prison — why not? *Bulletin of the American Acad Psychiatry and the Law* 3:152-161, Sept 1975, p 156
3. Boslow HM, Rosenthal D, Kandel A, Manne SH: Methods and experiences in group treatment of defective delinquents in Maryland. *J of Social Therapy* 7:65-75, No. 2, 1961
4. Cherry CR, Kandel A: Initial survey on the Board of Review of the Patuxent Institution. Unpublished paper, Patuxent Institution. Manne SH, Rosenthal D: Decision-making regarding release programs for committed criminal offenders. *Bulletin Am Acad Psychiatry and the Law*, IV:24-36 (1976). Both of these papers were written about 1976.
5. Report of Commission to Study Medico-Legal Psychiatry, submitted to Governor Wm. Preston Lane, Jr. and The Maryland General Assembly, December 28, 1948, p 22
6. Contract Research Corporation: The Evaluation of Patuxent Institution: Final Report, 25 February, 1977. Belmont, Massachusetts
7. *Ibid.*
8. Reiblich GK: An Indeterminate Sentence Law for Defective Delinquents, Research Report No. 29, Research Division, Maryland Legislative Council, December, 1950, Baltimore, City Hall
9. On external life situations as the source of motivation to seek psychotherapy, see Gurin G, Veroff J, Feld S: *Americans View Their Mental Health*. New York, Basic Books, 1960; Howard KI, Orlinsky DE: Psychotherapeutic processes. *Annual Review of Psychology* 23:615-668, 1972, pp 628, 638
10. See, e.g., Boslow, Rosenthal, Kandel, and Manne, note 3, above; Boslow HM, Manne SH: Mental health in action: treating adult offenders at Patuxent Institution. *Crime and Delinquency* 12:22-28, Jan 1966, pp 26-27; Carney FL: The indeterminate sentence at Patuxent. *Crime and Delinquency* 20:135-143, April 1975, pp 138-139; Rapoport JR: Patuxent revisited. *Bulletin of the American Acad Psychiatry and the Law* 3:10-16, March 1975, p 10. See also note 11, below
11. See, e.g., Monahan J, Cummings L: Social policy implications of the inability to predict violence. *J of Social Issues* 31:153-164, No 2, 1975, p 160. These authors present several quotations from an American Friends Service Committee publication, in which inmates describe the painfulness of the indeterminate sentence. No attempt is made to grapple with the problem of interpreting and weighing such testimony. No cognizance is taken of the psychological literature concerning the relation of such pain to motivation for therapeutic change. But theoretical statements such as the following have long claimed such a relation: "anxiety provides much of the motivation of the

patient seeking psychotherapy," Whitaker CA, Malone TP: *The Roots of Psychotherapy*. New York, Blakiston, 1953, p 122; "The patient's motivation for therapy comes from his misery," Dollard J, Miller NE: *Personality and Psychotherapy*. New York, McGraw-Hill, 1950, p 403; "The individual [seeking therapy] is under a degree of tension arising from incompatible personal desires or from conflicts of social and environmental demands with individual needs. The tension and stress so created are greater than the stress involved in expressing his feelings about his problems," Rogers C: *Counseling and Psychotherapy*. Boston, Houghton Mifflin, 1942, p. 76. Dollard and Miller, above, assert that the prognosis is better if the patient is extremely miserable. Howard and Orlinsky, note 9, above, concluded after an extensive review that "Personal distress is still a critical factor in determining a person's becoming a patient in psychotherapy," p 658. On the purely empirical end, a national survey found psychological anxiety correlated at all educational levels with the decision to seek psychotherapy; see Gurin G, Veroff J, Feld S: *Tabular Supplement to Americans View Their Mental Health*. Ann Arbor, Michigan, Survey Research Center, 1960, Tables C.1.-C.14. Taylor Manifest Anxiety scores were also found related to the decision to seek therapy in Terwilliger JS, Fiedler FE: An investigation of determinants inducing individuals to seek personal counseling. *J of Consulting Psychology* 22:288, August 1958. Once in psychotherapy, those who scored higher on the Taylor Manifest Anxiety scale prior to entering, and who had fewer antisocial acts in their histories to start, were more likely to remain, as reported in Lorr M, Katz MM, Rubinstein EA: The prediction of length of stay in psychotherapy. *J of Consulting Psychology* 22:321-327, Oct 1958. Finally, an extensive review of variables affecting the outcome of psychotherapy found that high initial anxiety level was one of those most frequently linked to therapeutic progress, as reported in Luborsky L, Chandler M, Auerbach AH, Cohen J, Bachrach HM: Factors influencing the outcome of psychotherapy: a review of quantitative research. *Psychological Bulletin* 75:145-185, March 1971, Table 1 and pp 150, 166-167. Thus, actual evidence indicates that anxiety and psychic distress cause individuals to seek therapy, to remain in therapy once they have found it, and to change as the result of therapy. Against this evidence the opponents of the indeterminate sentence have offered only vague assertions.

12. Davis JA: Great books and small groups: an informal history of a national survey, in *Sociologists at Work*. Edited by Hammond PE. New York, Basic Books, 1964, pp 212-234, 217
13. Many examples of this heterogeneous criticism, but by no means all, are included in Chapters 2 and 3 of the evaluation report. See Contract Research Corporation, note 6, above.
14. Patuxent Institution: Maryland's Defective Delinquent Statute: A Progress Report, January 9, 1973. Maryland, Department of Public Safety and Correctional Services, Patuxent Institution, pp 1, 3
15. See, e.g., Sidley NT: The evaluation of prison treatment and preventive detention programs: some problems faced by the Patuxent Institution. *Bulletin of the American Acad Psychiatry and the Law* 2:73-95, June 1974, pp 89-91; Contract Research Corporation, note 6, above, pp 117-118, 125-126; Wilkins LT: Treatment of offenders: Patuxent examined. *Rutgers Law Review* 29:1102-1116, No 5, 1976
16. I distinctly remember a discussion of the 65% rate at a meeting, but the figure also seems to have been present in the minds of staff as early as 1963, for "usually quoted" rates of 65% to 70% were cited by the Director even then in Boslow HM, Kohlmeier WA: The Maryland defective delinquency law: an eight-year follow-up. *Amer J Psychiat* 120:118-124, 1963
17. For an example and refutation of such an attack on IQ, see Gordon RA: Examining labelling theory: the case of mental retardation, in *The Labelling of Deviance: Evaluating a Perspective*. Edited by Gove WR. New York, Wiley, 1975, pp 83-146
18. Steadman HF, Keveles G: The community adjustment and criminal activity of the Baxstrom patients: 1966-1970. *Amer J Psychiat* 129:30-86, Sept 1972; Cocozza JJ, Steadman HJ: Some refinements in the measurement and prediction of dangerous behavior. *Amer J Psychiat* 131:1012-1014, Sept 1974; Steadman HJ, Cocozza JJ: *Careers of the Criminally Insane*. Lexington, Massachusetts, Lexington Books, 1974. The above reports are obscure concerning sex. Sex composition of the released sample can be inferred from a statement that only 73% of the men in the sample were released to the community, and of these 5 had died. Since the 98 released to the community consisted of those still alive, this suggests 68 males. See Steadman and Cocozza, pp 107, 113, 137, 139, and 141, for these various facts.
19. McGarry AL, Parker LL: Massachusetts' operation Baxstrom: a follow-up. *Massachusetts J Mental Health* 4:27-41, Sept 1974
20. I omit discussion here of a similar study of a small group of aged criminally insane inmates in Rubin B: Prediction of dangerousness in mentally ill criminals. *Arch Gen Psychiatry* 27:397-407, Sept 1972. The comment that "the behavior of released patients may say more about institutional inertia than about poor predictions" has been made about studies of the Baxstrom type in Stone AA: *Mental Health and the Law: A System in Transition*. Rockville, Maryland, National Institute of Mental Health, Center for Studies of Crime and Delinquency, 1972, p 31.
21. Koppin M: A validation study of Steadman's Legal Dangerousness Scale with reference to related data. Unpublished paper. Colorado State Hospital, Department of Research and Program

- Analysis, August, 1976. Although not cited in the evaluation report, this study was known to the evaluation team, because I received my copy from them in a mailing of preliminary findings to members of the Board of Patuxent.
22. See note 17, above. The Legal Dangerousness Scale is described in more detail in note 36, below.
 23. Kozol HL, Boucher RJ, Garofalo RF: The diagnosis and treatment of dangerousness. *Crime and Delinquency* 18:371-392, Oct 1972. See also the following exchange: Monahan J: Dangerous offenders: a critique of Kozol *et al.* *Crime and Delinquency* 19:418-420, July 1973; Kozol HL, Boucher RJ, Garofalo RF: Dangerousness. *Crime and Delinquency* 19:554-555, Oct 1973
 24. Wenk EA, Emrich RL: Assaultive youth: an exploratory study of the assaultive experience and assaultive potential of California Youth Authority wards. *J of Research in Crime and Delinquency* 9:171-196, July 1972
 25. By definition, all Patuxent inmates have been referred for psychiatric evaluation, and the component of dangerousness is a prominent consideration in these referrals. The recidivism study of Patuxent parolees in the evaluation report shows that 65.1% had a prior arrest for a violent crime, and 67.9% a violent committing offense. Although these parolees had only 2.6 prior incarcerations (*i.e.*, less than four), they also had only 3.1 prior convictions, as opposed to 4.9 on the average for all committed Patuxent inmates between 1955 and 1972. If incarcerations remain roughly proportional to convictions, this would suggest 4.1 prior incarcerations for the average Patuxent inmate. These data appear in Contract Research Corporation, note 6, above, at pp 120, 122, and Patuxent Institution, note 13, above, Table III.
 26. Wenk EA, Robison JO, Smith GW: Can violence be predicted? *Crime and Delinquency* 18:393-402, Oct 1972
 27. The effects of low base rates on prediction are considered in many publications. For an excellent source, see Galen RS, Gambino SR: *Beyond Normality: The Predictive Value and Efficiency of Medical Diagnoses*. New York, Wiley, 1975
 28. See note 23, above, p 392.
 29. See the comment by Stone, note 20, above, who goes on to state, "the enthusiasm of Rubin, Monahan, and others about the conclusive results of this natural experiment is not entirely justified," p 31
 30. McGarry and Parker, note 19, above, p 28
 31. Psychiatrists rightly regard such fears of laymen as exaggerated, but their own insight into those fears may be limited, since their own contact with psychotic patients on wards provides the security of knowing that other individuals can come to a physician's aid within a minute or two. I have myself witnessed such an emergency mobilization at Phipps Clinic, where everyone not urgently needed elsewhere converges on the scene of an incident at the press of a bell. Moreover, on the wards there is always someone else available to sound the alarm. Hospital personnel are also not obliged to sleep under the same roof with criminally insane patients. As sociologists have pointed out, "Sleep is . . . an insecure and exposed state" and we choose carefully those others in whose presence we dare to sleep; see Aubert V, White H: Sleep: a sociological interpretation. II. *Acta Sociologica* 4:1-16 No 3 1959, p 9. For examples of the kinds of crimes that are least predictable from the layman's perspective, see Finkelstein BA: Offenses with no apparent motive. *Diseases of the Nervous System* 29:310-313, May 1968. Steadman and Coccozza, note 18, above, at p 83, also observe that the label "insane" implies unpredictability.
 32. Patuxent Institution, see note 14, above, p 14
 33. See note 21, above, and note 23, above, pp 379-380, 383.
 34. Patuxent Institution, note 14, above, Table III
 35. Contract Research Corporation, see note 6, above, p 122. This average age of 33 answers a question raised by Stone concerning an earlier recidivism study of Patuxent by Hodges, which did not provide age data. Stone pointed out that if the average age at parole was 50, no one could conclude that it was the treatment that had been effective. See Stone AA: Discussion. *Amer J Psychiat* 128:291, Sept 1971 and Hodges EF: Crime prevention by the indeterminate sentence law. *Amer J Psychiat* 128:291-295, Sept 1971
 36. The Legal Dangerousness Scale accords 8 points for any reasonably severe juvenile record prior to age 17.0 (apparently excluding so-called "juvenile status" offenses *e.g.*, truancy), 4 points if incarcerated twice or more previously, 2 points for any history of prior violent crimes, and 1 point if the present offense was more severe than offenses against morals such as gambling or selling or using drugs. Now consider the average Patuxent inmate. During the period 1955 to 1972 he was 16.6 years old when first convicted, and he arrived at Patuxent with 4.9 prior convictions. The recidivism study in the evaluation report shows that parolees, who average fewer convictions, namely 3.1, already had 2.6 prior incarcerations, so it follows *a fortiori* that the average Patuxent inmate would have had at least two. Since 65.1% of parolees had had a prior violent arrest, a majority clearly meets the third requirement of the dangerousness scale. The percentage suggests a value of 1.3 points for the average inmate out of a possible 2. Finally, 71% of those committed to Patuxent between 1970 and 1972 had been convicted of murder, robbery, assault, or rape, thus easily meeting the fourth requirement for the model inmate. It is reasonable

to expect that the remainder would meet the criterion too, since there are still four unused scale positions before the cutoff at morals offenses. The sources of these data are Patuxent Institution, note 14, above, Table III and p 14, and Contract Research Corporation, note 6, above, pp 119-120. Boslow and Manne, note 10, above, provided a detailed "typical case history" in 1966 that clearly attains a score of 15, the maximum possible, on the Legal Dangerousness Scale. For the violent conviction rate and mean Scale scores of the Baxstrom patients, see Steadman and Cocozza, note 18, above, pp 87, 107.

37. This datum comes from the recidivism study in the evaluation report. See Contract Research Corporation, note 6, above, pp 119, 124. After release, this group had a rate of 17.4% for violent convictions, which is lower than the rate of 28.0% for violent convictions for an identical but larger sample of 118 in an earlier Patuxent recidivism study by Hodges, see note 35, above, Table 3. Again assuming arrests remain proportional to convictions, it suggests that Hodges' data confirm that Patuxent inmates are at least as dangerous as claimed here.
38. This comparison also derives from the recidivism study in the evaluation report. I have expressed the rates of violent convictions and violent arrests in one group as percentages of the same rates in the other group, and then averaged these two percentages to get 62% as a summary figure.
39. This conclusion lends perspective to comments by critics of Patuxent concerning the supposed nondangerousness of its inmates, quite aside from the predictability issue (considered below). For example, Dr. Brian Crowley has often maintained that "Given the wrong turn of events, almost anybody could be adjudicated a defective delinquent," and "The [defective delinquent] law catches in its net a whole cast of characters, a shocking number of men who are not dangerous and should not be locked up;" quoted in Olesker M, Nawrozki J: Patuxent: a nightmarish "total treatment" prison. The News American (Baltimore), June 9, 1974. Crowley may be correct in saying that anyone could be confined at Patuxent, but the facts indicate that it would help if the "wrong turn of events" happened to include 4.9 prior convictions, at least 2.6 prior incarcerations, an average sentence of 10 years, and commission of murder, robbery, assault, or rape as one's last crime.
40. Predictive value of diagnostic procedures is discussed in Galen and Gambino, note 27, above, p 12. Predictive values can be defined for either positive or negative diagnoses.
41. Many illustrations of the effects of base rates on predictive value for the population of interest are given in Galen and Gambino, note 27, above.
42. Quoted in Timberg R: Patuxent called not justified. Evening Sun (Baltimore), February 20, 1976, p C1
43. Diamond BL: The psychiatric prediction of dangerousness. University of Pennsylvania Law Review 123:439-452, Dec 1974, p 441
44. *Ibid.*, p 450. In addition to the Baxstrom studies, Diamond also drew on a study by Rubin, note 20, above, of 17 aging criminally insane patients who had been incarcerated for many years, and stated, "Rubin personally examined each of the seventeen men and found that in no case was the prediction of dangerousness valid" (p 445). The impression conveyed here is misleading, because Diamond does not tell us whether he is referring to the original prediction, made an average of 33.5 years prior to Rubin's examination, which the reader might readily conclude, or to the tacit administrative decision that we infer from the continued presence of these patients in confinement. Seven of the patients had committed murder, and another two had shot someone nonfatally, it might be noted, and all but one, a severe mental retardate, had been psychotic at time of admission. Stone's comment, note 20, above, on Rubin's study is worth noting: "Twenty-five years after a murder the killer may well seem like the kindest and most innocent of men, and the brief charges contained in his file may, when reviewed through his current mental status, seem preposterously weak and exaggerated, as Rubin claims to have been the case in his survey" (p 35). On the basis of four black patients in his Illinois sample, Rubin, note 20, above, also concluded, "From the data it seems likely that the poor, the mentally incompetent, the drifter, and the black are more likely to be labeled in this way for social reasons unrelated to any violent behavior . . ." (p 405). But Stone, in his review, shows that black overrepresentation is relatively slight in facilities for the criminally insane in comparison to other criminal facilities, where it is 3.2 times greater (p 35). The Patuxent population is not disproportionately black for its location, and blacks referred are diagnosed defective delinquent at the same rate as whites. See Patuxent Institution, note 14, above, Table II. Rubin went on to cite familiar labelling theorists in support of his general position. However, recent reviews of the empirical evidence for labelling theoretical claims of invalid bias or aggravation of deviance through labelling reveal that these grandiose claims cannot be supported; these reviews appear in Gove, note 17, above. An excellent critique also appeared in this journal; see Roth LH: Some comments on labelling. Bulletin of the American Acad Psychiatry and the Law 3:123-131, Sept 1975. Roth's comments on the Rosenthal and Jacobson study, which claimed to find IQ increases as the result of labelling, would undoubtedly have been more critical still had he been aware that this particular extraordinary result has failed of replication nine times out of nine; see Baker JP, Crist JL: Teacher expectancies: a review of the literature, in Pygmalion Reconsidered. Edited by Elashoff

- JD, Snow RE. Worthington, Ohio, Charles A. Jones Publishing Company, 1971. pp 48-64
45. Monahan and Cummings, note 11, above, p 156. See also the exchange between Monahan and Kozol *et al.*, note 23, above.
 46. Monahan, note 23, above, p 420
 47. Sidley, note 15, above, p 89. See also pp 81 and 86, where he reinterprets the defective delinquent law to refer only to offenses involving violence or threat of violence. By restricting recidivism of those diagnosed, but not certified, defective delinquent in Hodges' study, note 35, above, to violent crimes, Sidley obtained a figure of 33% instead of Hodges' 81%. This led to Sidley's conclusion that the staff was in error 2 out of 3 times.
 48. Diamond, note 43, above, p 451
 49. Monahan and Cummings, note 11, above, p 153
 50. See note 43, above, p 451
 51. See note 20, above, p 37
 52. Gordon RA: Social level, social disability, and gang interaction. *American J of Sociology* 73:42-62, July 1967, p 52
 53. Colson E: Tradition and Contract: The Problem of Order. Chicago, Aldine, 1974, pp 56-59
 54. Gordon RA: Prevalence: the rare datum in delinquency measurement and its implications for the theory of delinquency, in *The Juvenile Justice System*. Edited by Klein MW. Beverly Hills, Sage, 1976, pp 201-284
 55. See the comments by Crowley, note 39, above.
 56. For the rate of 71%, see note 14, above, p 14; for the rates of 6.0% and 2.51%, see note 24, above, Tables I and VIII. Wenk and Emrich give 2.4% for this last figure, but 104 individuals, p 178, out of 4,146 equals 2.51%. For the rate of 41.3%, see note 6, above, p 124.
 57. See note 24, above, p 183.
 58. Gordon RA: An explicit estimation of the prevalence of commitment to a training school, to age 18, by race and by sex. *J of the American Statistical Association* 68:547-553, Sept 1973. See also Gordon, note 54, above, Table 2.
 59. Gordon, note 54, above, pp 232-234. Wilson JQ: The police and the delinquent in two cities, in *Controlling Delinquents*. Edited by Wheeler S. New York, Wiley, 1968, pp 9-30, at pp 18-21. Glueck S, Glueck ET: Five Hundred Criminal Careers. New York, Knopf, 1930
 60. In actuality the CYA population was only 53.4% white, but this hardly affects the calibration, since, for example, a racially mixed group with IQ 100 would still be at the 50th percentile for whites. The ethnic composition and age range of this sample appear in Wenk EA, Halatyn TV, Harlow N: An Analysis of Classification Factors for Young Adult Offenders, Volume 1. Background of the Study and Statistical Description of the Total Study Population. Administrative Summary. Davis, California, Research Center of the National Council on Crime and Delinquency, Oct 1974, p 14
 61. For simplicity, I consistently refer to the percentile distribution within only one race. The 99th percentile among white males is equivalent to the 96th percentile among black males. See Gordon, note 58, above. Differences between the races in delinquency rates have been shown to be closely commensurate with differences in IQ. See Gordon, note 54, above, Table 7
 62. Work by my student Eileen E. Rudert indicates that this estimate is easily obtained from the incidence of commitment, since repetition of diagnosis as a defective delinquent is practically unknown, and incidence rates are not inflated consequently. One simply multiplies the incidence rate for aptly chosen ages of peak risk by the number of such age groups. The calculation is not very sensitive to the exact age range chosen. I have assumed that the proportion of whites committed in these years is .44, which is given as the proportion white among referrals in 1970-1972. See Patuxent Institution, note 14, above, p 15; numbers found defective delinquent are from Patuxent Institution: Annual Report, 1970, p 31, and Annual Report for the Fiscal Years 1973, 1974, 1975, p 23; and numbers of white males by years of age in Maryland are from U.S. Bureau of the Census: Census of Population: 1970, Volume 1, Characteristics of the Population, Part 22, Maryland. Washington, D.C., U.S. Government Printing Office, 1973, Table 19
 63. For example, no matter what cutoff is chosen, there will be some rejected students who if admitted would have graduated from a selective college. One might consider such students "false positives" for flunking out. Failure to recall why admissions criteria were originally imposed leads to the demand that previously rejected students be admitted too, until the costs of this policy are rediscovered. A recent study found that because of high dropout rates, it cost \$103,061 to graduate one disadvantaged student from public colleges in New York State, as opposed to \$18,570 from private colleges, which had remained more selective toward such students. See Buder L: Graduation rate unaltered by City U. SEEK program. *New York Times*, April 12, 1977, pp 1, 22
 64. Coccozza JJ, Steadman HJ: The failure of psychiatric predictions of dangerousness: clear and convincing evidence. *Rutgers Law Review* 29:1084-1101 (1976). Sidley, note 15, above, insists on judging homogeneity in terms of the residual variation (which always exists), instead of in

terms of the reduction in variation that existed prior to classification. Not only is this completely out of keeping with usual methodological practice, but no metric exists for determining whether the residual variation is large or small, apart from reference to the original amount of variation. Thus, Sidley remarks, "... the diagnosis of defective delinquent must be homogeneous with respect to the outcome, in that persons so diagnosed and not incarcerated ... commit ... crimes of violence ... shortly after they are so diagnosed" (p 86). The insistence on "shortly after" here reveals just how grudging and demanding critics of Patuxent can be. Sidley's lack of sympathy for the problem of discriminating within the rather homogeneous group of court referrals is revealed when he "wonders whether the diagnostic process has done a better job of predicting [violent] recidivism than would a random assignment of DD [defective delinquent] diagnoses," p 89. The diagnosis would predict violent recidivism quite well in comparison to random assignment if performed within a cross-section of the general population. See, for example, the correlations cited further on in connection with Koppin's study. Finally, the ultimate irony occurs when Sidley wonders whether as good or better results, in terms of homogeneity of outcome, would be obtained by using a simple criterion such as five convictions to declare persons defective delinquent. But homogenizing the intake on a simple-minded criterion instead of the ambiguous clinical diagnosis that Sidley denigrates is certainly no guarantee of homogeneity of outcome!

65. Gordon RA: Issues in multiple regression. *American J of Sociology* 73:592-616, March 1968. There is also a resemblance here to the principle of "local independence" in Lazarsfeld's latent class analysis: see, Lazarsfeld PF: A conceptual introduction to latent structure analysis, in *Mathematical Thinking in the Social Sciences*. Edited by Lazarsfeld PF. Glencoe, Illinois, Free Press, 1954, pp 349-387, at pp 368-369. When individuals have been assigned to a latent class on the basis of their similarity with respect to some latent disposition (e.g., political conservatism), the correlations among indicators of the disposition break down, and the responses of the individuals in that class to survey questions, for example, become independent of each other. The extension of the principle of local independence to diagnosis is quite straightforward. For example, a number of symptoms may correlate strongly with schizophrenia, but among individuals with diagnoses of schizophrenia many irregularities in the pattern will be found, unless the diagnostic criteria are made arbitrarily uniform. This principle too would account for the failure of the Patuxent evaluators to find more consistent differences between those diagnosed "defective delinquent" and those diagnosed "not defective delinquent" within the rather homogeneous group (latent class) of court referrals. See Contract Research Corporation, note 6, above, p 76. The examination here involved comparing two sets of 11 files randomly chosen from cases receiving each diagnosis. But consistent differences reported by the staff between these diagnostic categories over the entire history of the institution to 1972, with respect to age (-2.4 years), age at first conviction (-3.6 years), number of prior convictions (+1.5), and sentence (+2.0 years) indicate that reliable distinctions were certainly being made. Here, of course, the enormous sample sizes of 1,163 and 731 help to stabilize the differences. See Patuxent Institution, note 14, above, Table II. It is surprising to me that the evaluation report passed over this larger body of data in this context. Perhaps they construed "diagnosis" as referring only to classic psychiatric criteria. Sidley's suggestion, in note 64, above, would be an example of making the diagnostic criteria arbitrarily uniform.
66. See note 21, above.
67. Practice in our society, for better or worse, is remarkably consistent with Colson's observations. First offenders, for example, especially juvenile ones, are rarely incarcerated even for rather serious offenses. On juveniles, see Gordon, note 54, above, pp 232-234, and Wilson, note 59, above, pp 18-21.
68. Steadman and Cocozza, note 18, above, at pp 8, 184, observe that witchcraft often served as a catchall diagnostic category for mental illness even in European societies of the recent past. In the societies studied by Colson, note 53, above, the term seems broader, and appears to function as a catalyst of stored-up hostility as well.
69. The error of the critics of predictability could be characterized as assuming the prediction in question is of dangerous behavior, when it is really of the probability of dangerous behavior. In the former case, the prediction might seem poor, whereas in the latter case, it might be superb. Just such a point, concerning parole success, is made in Palmer J, Carlson P: Problems with the use of regression analysis in prediction studies. *J of Research in Crime and Delinquency* 13:64-81, Jan 1976, p 79
70. For example, cultural relativistic claims notwithstanding, there is strong consensus among a wide range of population subgroups concerning the relative goodness or badness, severity, or seriousness of crimes, and these judgments correlate highly with those formalized by justice systems. See Gordon RA, Short JF, Jr., Cartwright DS, Strodbeck FL: Values and gang delinquency: a study of street-corner groups. *American J of Sociology* 69:109-128, Sept 1963; Sellin T, Wolfgang ME: *The Measurement of Delinquency*. New York, Wiley, 1964; Rossi PH, Waite E, Bose CE, Berk RE: The seriousness of crimes: normative structure and individual differences. *American Sociological Review* 39:224-237, April 1974. There is even strong

consensus across national cultures, as reported in Newman G: *Comparative Deviance: Perception and Law in Six Cultures*. New York, Elsevier, 1976

71. Galen and Gambino, note 27, above, pp 50-51
72. For these calculations, I have set the social cost of incarcerating a true positive equal to that of a false positive, since both probably treasure their liberty equally.
73. Goodman LA: Generalizing the problem of prediction. *American Sociological Review* 17:609-612, Oct 1952. The social cost of a decision is simply the sum of the costs for each affected category weighted by the numbers of individuals in those categories. One might always wish to retain a subjective or clinical component in establishing the categories, so that inmates are unable to make a mockery of formalistic criteria, and so that pressure is maintained on inmates to meet the more diffuse expectations of society as well as just a few formal ones. Goodman, it is interesting to note, set the social cost of not having paroled a false positive slightly greater than that of a true positive, because in addition to prison costs, the families of the former would protest. In actuality, families of either kind of positive are likely to protest, and that is why I set their costs equal. See note 72, above.
74. Monahan J: The prediction of violence, in *Violence and Criminal Justice*. Edited by Chappell D, Monahan J. Lexington, Massachusetts, Lexington Books. 1975, pp 15-31, at pp 20-22
75. Sidley, note 15, above, p 90
76. Monahan, note 74, above, p 22
77. Steadman and Cocozza, note 18, above, p 110
78. Boffey PM: Guillain-Barré: rare disease paralyzes swine flu campaign. *Science* 195:155-159, Jan 14, 1977
79. See note 39, above.
80. Steadman and Cocozza, note 18, above
81. *Ibid.*, pp 98-99
82. *Ibid.*, pp 101-102, 116-117
83. *Ibid.*, p. 108
84. *Ibid.*, p 137. If the Baxstrom women patients are included, the percentage released is 56% of the sample still alive at the end of the follow-up period. See p 113.
85. *Ibid.*, p 77
86. If the percentage of noxious deviants in the larger community is small (which is becoming less true every day, judging from crime rates) even severe effects will appear miniscule when averaged over the entire population, and any cavalier policy favoring release can be defended successfully. For this reason, it is usually more appropriate to consider effects on the local micro-community immediately surrounding a released person when that person is simply mentally ill (criminals probably have a wider range). There is no justification for imposing burdens on the micro-community that one would not tolerate oneself or would not recommend for the entire community. The assignment of greater weight to the micro-community in considering the release of mentally ill persons would account in part for the influence of accepting or rejecting family members on decisions to release irrespective of the health or dangerousness of the inmate (within certain limits, probably). Since the family represents a large share of the micro-community, its attitude "speaks for" that sector, and its willingness to assume further responsibility is strongly implied. On the influence of the attitude of family members, see Greenley JR: The psychiatric patient's family and length of hospitalization. *J of Health and Social Behavior* 13:25-37, March 1972; Steadman and Cocozza, note 18, above, Chapter 7. The shift in attitudes concerning the sector of the community that physicians were responsible for protecting may account in part for the different release policies applied to criminally insane patients in the Baxstrom study, the original psychiatrists placing more emphasis on the "criminal" label than physicians in the civil hospitals, who were accustomed to dealing merely with "insane" patients. Since criminals presumably perform noxious acts over a wider range of the community than mental patients in general, family members of criminals cannot speak for this wider sector in trying to effect their release. The impact of psychotics and other mentally ill categories on life in the micro-community deserves more attention than it receives in the Baxstrom studies. As I write this, a friend has recently become acutely psychotic, with extensive paranoid ideation. According to reliable reports, he has struck one stranger and casually threatened the life of an acquaintance. He has had his local community of friends and colleagues in a state of desperation for three months, and has reduced his parents to despair. On three occasions he has been hospitalized by police and he may even be a danger to himself. Yet, he resists voluntary hospitalization and four psychiatrists have refused his father's request to commit him involuntarily. Even though I am hundreds of miles away, I myself feel some apprehension — I think with good reason. Had he been hospitalized, it might have prevented his being fired, preserved a network of friends that is now sorely out of patience, and cut short a series of scandalous episodes that will haunt his professional life if he ever recovers. Is the community entitled to no protection from cases like this, not to mention the sick individuals themselves? Abramson has called attention to the fact that such socially disruptive persons are now being subjected to criminal prosecution as a means

- of removing them from the community in the face of more stringent civil commitment criteria (a paper which Steadman and Cocozza took note of). Is that what we want, and if not, how many options can there be? See Abramson MF: The criminalization of mentally disordered behavior: possible side-effect of a new mental health law. *Hospital and Community Psychiatry* 23:101-105, April 1972; Steadman and Cocozza, note 18, above, pp 174-178.
87. The successful internment of Japanese-Americans after Pearl Harbor, with its astronomical false positive rate, may be understood in part as the result of including entire kinship networks in the confinement, leaving no one outside to exert countervailing pressure.
 88. Monahan and Cummings, note 11, above, p 157; also in Monahan, note 74, above, p 20
 89. See note 39, above.
 90. This is reminiscent of the controversial study by Rosenhan DL: On being sane in insane places. *Science* 179:250-258, 1973, which Galen and Gambino, note 27, above, p 19, interpret in the context of low base rates, pointing out the discouragingly high failure rate a psychiatrist would encounter in trying "to discover sanity in an insane environment" even if the sensitivity and specificity of his diagnostic measures were extremely high.
 91. See note 15, above, p 86. Rapoport, note 10, above, p 11, comments that Sidley has "developed a logical concept of diagnosis, treatment and outcome so rigorous that it could be applied to only very few aspects of the practice of all medicine and in all probability to no aspects of psychiatric treatment."
 92. Here, I have adopted Lazarfeld's conceptualization, expressed clearly in his statement: "*The probability is therefore a property of the structure.*" See note 65, above, p 357.
 93. It may be helpful to distinguish the probability for a single die, which is structurally either zero or one, from the probability for a single toss, which is .5, since we never know which die we have. Those familiar with the frequentist theory of probability will recognize the similarity of this example to the problem of defining the probability of a single event, which is solved by assigning the single event to a homogeneous reference class, and assigning the probability of the numerous events in the reference class to the single event. Problems may arise because a single event may be assignable to more than one reference class whose probabilities differ. In the present example, the dice lend themselves, because of their stipulated structure, to assignments that yield different probabilities than the reference class derived from rules governing the toss. This is precisely what makes the example of interest, however, in contradistinction to the other special case, below, where the stipulated structure and the rules lead to reference classes with the same probabilities. See Salmon WC: *Statistical Explanation and Statistical Relevance*. Pittsburgh, University of Pittsburgh Press, 1971, pp 40-44
 94. Sidley, note 15, above, p 82, prefers to read the defective delinquent law in Maryland as requiring virtually a zero or one probability of dangerousness because it contains the phrase in its definition of defective delinquent, "an individual who . . . clearly demonstrate[s] an actual danger to society." Sidley himself rejects the plausibility of the "zero-one" model (at least the "one" part), stating "it is a strongly doubtful assumption that a group of individuals can be discovered whose numbers would virtually *all* commit aggravated crimes *within a reasonable period of time.*" His claim that "the statute presupposes such a high degree of certainty" must be recognized as self-serving, but also as one that the ambiguous language of legislators often invites because they do not know what probability to specify in order to accomplish their purposes.
 95. This recognition goes under various names, according to the context from which it arises. See Gordon, note 65, above, on the "partialling fallacy"; Lazarsfeld, note 65, above, on "local independence"; Thurstone LL: *Multiple-Factor Analysis*. Chicago, University of Chicago Press, 1947, pp 266-270, on the concept of "the vanishing of tetrad differences"; and Salmon, note 93, above, p 41, who notes the conflict between predictive precision, which is increased by narrowing the reference class, and reliability, which is decreased by narrowing the reference class.
 96. This trend is so well known that I give only outstanding examples. See Lazarsfeld, note 65, above; Salmon, note 93, above; Coleman JS: *Introduction to Mathematical Sociology*. New York, Free Press of Glencoe, 1964
 97. Disposition concepts, now generally familiar, were defined in Hempel CG: *Fundamentals of Concept Formation in Empirical Science*. *International Encyclopedia of Unified Science*, Volume II. Chicago, University of Chicago Press, 1952, pp 24-29. They refer to scientific phenomena that are not *directly* observable, such as "gravitational field" and "introversion," but which have observable effects.
 98. As observed in note 69, above
 99. An element of constancy seems to be suggested for transitions from last release back to prison *via* recidivism in data reported for Herstedvester in Denmark, where the proportion readmitted after each successive release remains at a constant 50%. One wonders what relation this intriguing constancy has with age at release. The data are reported in Steadman and Cocozza, note 18, above, p 179.
 100. Wilkins is quoted by Wenk and Emrich, note 24, above, pp 171-172, who express their own view on p 196. In this quotation, Wilkins opts for a totally situationalist theory of violence that regards personality characteristics as irrelevant on the basis of the low predictive values of

- positive predictions in the CYA studies mentioned above. However, the explanatory power of a theory can be quite separate from its predictive power, and hence a theory can tell us much about a phenomenon even if its probability is low. See Greeno JG: Explanation and information, in Salmon, note 93, above, pp 89-104, at p 97
101. See Wilkins, note 100, above, and the extreme interactionist perspective in Mischel W: On the future of personality measurement. *American Psychologist* 32:246-254, April 1977. A vigorous defense of personality trait theory, and the stability of traits, is presented in Hogan R, DeSoto CB, Solano C: Traits, tests, and personality research. *American Psychologist* 32:255-264, April 1977
 102. Strodbeck FL, Short JF, Jr.: Aleatory risks versus short-run hedonism in explanation of gang action. *Social Problems* 12:127-140, Fall 1964
 103. This is quite clear in the work of Galen and Gambino, note 27, above, although outcomes are sometimes stochastic even in physical medicine. An example involving paresis as a purely stochastic outcome of untreated latent syphilis is given in Salmon, note 93, above, pp 56-58.
 104. See, for example, the comment in Jeffrey RC: Statistical explanation vs. statistical inference, in Salmon, note 93, above, pp 19-28, at footnote 3.
 105. Sidley, note 15, above, p 91
 106. This rate is approximate, assuming four official times at bat per game.
 107. Salmon, note 93, above, pp 7, 9, 56, 63-65, has disposed of the notion that an event must be highly probable in order for it to be "explained." This misconception certainly colors our attitude toward the middle-range probability of dangerous acts. See also Greeno, note 100, above.
 108. Contract Research Corporation, note 6, above, pp iii-iv
 109. *Ibid.*, p iv
 110. *Ibid.*, p 43
 111. *Ibid.*, p 44
 112. *Ibid.*, p 45
 113. *Ibid.*, p 63
 114. *Ibid.*, p 64
 115. *Ibid.*, p 65
 116. *Ibid.*, pp 181-182
 117. *Ibid.*, p vi
 118. *Ibid.*, p vi
 119. The third recommendation was that Patuxent be continued as a special treatment facility.
 120. Prettyman EB Jr.: The indeterminate sentence and the right to treatment. *American Criminal Law Review* 11, 172
 121. See note 11, above.
 122. Martinson R: What works? — questions and answers about prison reform. *The Public Interest* 1974:22-54, Spring 1974
 123. See notes 9 and 11, above. The evaluation report does mention articles by Patuxent personnel, such as those in note 10, above, but these are not the main sources of empirical research evidence.
 124. Palmer T: Martinson revisited. *J of Research in Crime and Delinquency* 12:133-152, July 1972
 125. Cowden JE, Bassett HT: A comparison of reformatory inmates committed directly by the court with those transferred from juvenile institutions. *J of Clinical Psychology* 28:214-215, April 1972, at p 215
 126. Sidley, note 15, above, pp 81 and 86. See also note 47.
 127. See, e.g., Howell R: You can't escape break-ins by fleeing the city: 'Hundreds' of break-ins easy, a 'professional' says. *Evening Sun* (Baltimore), July 5, 1977, pp C1, C3
 128. Contract Research Corporation, note 6, above, p 117
 129. Hodges, note 35, above, Table 1
 130. The similarity could also be due to the difficulty of predicting within the present parolee population so as to lower the recidivism rates further. Patuxent's Review Board could always fall back on age if it could find no other predictors, and if the present recidivism rates were decided to be too high.
 131. As noted in Morris N: Psychiatry and the dangerous criminal. *Southern California Law Review* 41:514-547, Spring 1968, p 530; and in Contract Research Corporation, note 6, above, p 63. Maryland's proposed new law allows for early release through Patuxent if the habitual offender is deemed a good risk in the same sense that defective delinquents eligible for parole once were. Otherwise, he serves his full 25-year sentence.
 132. These phrases appear in Godfrey EA, Schulman RE: Age and a group test battery as predictors of types of crime. *J of Clinical Psychology* 28:339-342, July 1972, at p 341
 133. The period 2.9 was employed in the report for technical reasons.
 134. For example, imagine a scenario of later recidivism in which periods of confinement four years long alternate with periods of liberty three years long. The cycle of an inmate sent to Patuxent

begins with seven years in confinement in Patuxent, and the cycle if he were sent to the correctional system begins with four years in confinement in the correctional system. Hence, the cycles run 7, 3, 4, 3, etc. and 4, 3, 4, 3, etc., for the Patuxent and correctional system inmate, respectively. At certain points, but not others, the Patuxent inmate will have had three fewer years at risk. For example, 14 years after the beginning of year one, the Patuxent inmate has had only three years at liberty whereas the correctional system inmate has had six. This amounts to one cycle of crime versus two cycles of crime. However, at 17 years, both have had six years at liberty and two cycles of crime. Clearly, comparative evaluation of the incapacitation function of the two institutions in this context is not only dependent on the time period chosen, but also on events that are remote in time and later than the initial incarceration in Patuxent. From an intellectual standpoint, this is quite unsatisfying. These peculiar properties of the evaluation notwithstanding, the Patuxent inmate is never ahead in the number of subsequent crimes committed, and is usually behind. Since he is always three years older, according to this scenario, and since the probability of recidivism presumably declines with age, inmates initially sent to Patuxent would also commit fewer crimes during each subsequent cycle at liberty, and this difference would also accrue to the advantage of Patuxent over the cycles, however many cycles there were. The potential effectiveness of incapacitation in Patuxent for longer periods at one stage of the scenario argues for return to Patuxent for longer periods at later stages too. This would contribute substantially to the realization of savings all along the line by avoiding crimes rather than merely postponing them. I have enjoyed and benefitted from a stimulating discussion with Howard Bloom, of the evaluation team, in arriving at these conclusions.

135. Robinson J: Address on defective delinquency. Presented at the General Assembly of the States Council of State Governments, Chicago, Illinois, December 5, 1958, at p 6
136. If the National Council on Crime and Delinquency and the critics of dangerousness both prevail, society will have no protection, for the former group advocates that "the nondangerous offender should not be imprisoned." See Board of Directors, National Council on Crime and Delinquency: The nondangerous offender should not be imprisoned: a policy statement. Crime and Delinquency 19:449-456, Oct 1973 and 21:315-322, Oct 1975