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WHAT WORKS?

QUESTIONS AND ANSWERS ABOUT PRISON REFORM

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The tremendous increase of crime during the 1960s heralded a shift in the way Americans would come to view criminals in the next decade, leading to a law-and-order movement in the early 1970s. The Martinson Report, a massive study undertaken at that time to determine the most effective means of rehabilitating prisoners, concluded that, "with few and isolated exceptions, the rehabilitative efforts that have been reported so far have had no appreciable effect on recidivism." These words were interpreted to mean that "nothing works" as far as rehabilitating prisoners was concerned and that a new direction needed to be found. In 1974, a summary of the study's findings was presented by Robert Martinson in *The Public Interest*. Other research studies supported Martinson's conclusion and, by the end of the decade, a paradigm shift had occurred in corrections from rehabilitation to deterrence and just deserts. The influence of Martinson's article, the first published account of the aforementioned survey, cannot be underestimated—it was "the straw that broke the camel's back." Support for the rehabilitative ideal and the medical model of corrections decreased substantially after its publication.

In the past several years, American prisons have gone through one of their recurrent periods of strikes, riots, and other disturbances. Simultaneously, and in consequence, the articulate public has entered another one of its sporadic fits of attentiveness to the condition of our prisons and to the perennial questions they pose about the nature of crime and the uses of punishment. The result has been a widespread call for "prison reform," i.e., for "reformed" prisons which will produce "reformed" convicts. Such calls are a familiar feature of American prison history. American prisons, perhaps more than those of any other country, have stood or fallen in public esteem according to their ability to fulfill their promise of rehabilitation.

One of the problems in the constant debate over "prison reform" is that we have been able to draw very little on any systematic empirical knowledge about the success or failure that we have met when we *have* tried to rehabilitate offenders, with various treatments and in various institutional and noninstitutional settings. The field of penology has produced a voluminous research literature on this

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subject, but until recently there has been no comprehensive review of this literature and no attempt to bring its findings to bear, in a useful way, on the general question of "What works?" My purpose in this [chapter] is to sketch an answer to that question.

THE TRAVAILS OF A STUDY

In 1966, the New York State Governor's Special Committee on Criminal Offenders recognized their need for such an answer. The Committee was organized on the premise that prisons could rehabilitate, that the prisons of New York were not in fact making a serious effort at rehabilitation, and that New York's prisons should be converted from their existing custodial basis to a new rehabilitative one. The problem for the Committee was that there was no available guidance on the question of what had been shown to be the most effective means of rehabilitation. My colleagues and I were hired by the committee to remedy this defect in our knowledge; our job was to undertake a comprehensive survey of what was known about rehabilitation.

In 1968, in order to qualify for federal funds under the Omnibus Crime Control and Safe Streets Act, the state established a planning organization, which acquired from the Governor's Committee the responsibility for our report. But by 1970, when the project was formally completed, the state had changed its mind about the worth and proper use of the information we had gathered. The Governor's Committee had begun by thinking that such information was a necessary basis for any reforms that might be undertaken; the state planning agency ended by viewing the study as a document whose disturbing conclusions posed a serious threat to the programs which, in the meantime, they had determined to carry forward. By the spring of 1972—fully a year after I had re-edited the study for final publication—the state had not only failed to publish it, but had also refused to give me permission to publish it on my own. The document itself would still not be available to me or to the public today had not Joseph Alan Kaplon, an attorney, subpoenaed it from the state for use as evidence in a case before the Bronx Supreme Court.[1]

During the time of my efforts to get the study released, reports of it began to be widely circulated, and it acquired something of an underground reputation. But this article is the first published account, albeit a brief one, of the findings contained in that 1,400-page manuscript.

What we set out to do in this study was fairly simple, though it turned into a massive task. First we undertook a six-month search of the literature for any available reports published in the English language on attempts at rehabilitation that had been made in our corrections systems and those of other countries from 1945 through 1967. We then picked from that literature all those studies whose findings were interpretable—that is, whose design and execution met the conventional standards of social science research. Our criteria were rigorous but hardly esoteric: A study had to be an evaluation of a treatment method, it had to employ an independent measure of the improvement secured by that method, and it had to use some control group, some untreated individuals with whom the treated ones could be compared. We excluded studies only for methodological reasons: They presented insufficient data, they were only preliminary, they presented only a summary of findings and did not allow a reader to evaluate those findings, their results were confounded by extraneous factors, they used unreliable measures, one could not understand their descriptions of the treatment in question, they drew spurious conclusions from their data, their samples were undescribed or too small or provided no true comparability between treated and untreated groups, or they had used inappropriate statistical tests and did not provide enough information for the reader to recompute the data. Using these standards, we drew from the total number of studies 231 acceptable ones,

which we not only analyzed ourselves but summarized in detail so that a reader of our analysis would be able to compare it with his independent conclusions.

These treatment studies use various measures of offender improvement: recidivism rates (that is, the rates at which offenders return to crime), adjustment to prison life, vocational success, educational achievement, personality and attitude change, and general adjustment to the outside community. We included all of these in our study; but in these pages I will deal only with the effects of rehabilitative treatment on recidivism, the phenomenon which reflects most directly how well our present treatment programs are performing the task of rehabilitation. The use of even this one measure brings with it enough methodological complications to make a clear reporting of the findings most difficult. The groups that are studied, for instance, are exceedingly disparate, so that it is hard to tell whether what "works" for one kind of offender also works for others. In addition, there has been little attempt to replicate studies; therefore one cannot be certain how stable and reliable the various findings are. Just as important, when the various studies use the term "recidivism rate," they may in fact be talking about somewhat different measures of offender behavior—i.e., "failure" measures such as arrest rates or parole violation rates, or "success" measures such as favorable discharge from parole or probation. And not all of these measures correlate very highly with one another. These difficulties will become apparent again and again in the course of this discussion.

With these caveats, it is possible to give a rather bald summary of our findings: *With few and isolated exceptions, the rehabilitative efforts that have been reported so far have had no appreciable effect on recidivism.* Studies that have been done since our survey was completed do not present any major grounds for altering that original conclusion. What follows is an attempt to answer the questions and challenges that might be posed to such an unqualified statement.

EDUCATION AND VOCATIONAL TRAINING

1. *Isn't it true that a correctional facility running a truly rehabilitative program—one that prepares inmates for life on the outside through education and vocational training—will turn out more successful individuals than will a prison which merely leaves its inmates to rot?*

If this *is* true, the fact remains that there is very little empirical evidence to support it. Skill development and education programs are in fact quite common in correctional facilities, and one might begin by examining their effects on young males, those who might be thought most amenable to such efforts. A study by New York State (1964)[2] found that for young males as a whole, the degree of success achieved in the regular prison academic education program, as measured by changes in grade achievement levels, made no significant difference in recidivism rates. The only exception was the relative improvement, compared with the sample as a whole, that greater progress made in the top 7 percent of the participating population—those who had high I.Q.'s, had made good records in previous schooling, and who also made good records of academic progress in the institution. And a study by Glaser (1964) found that while it was true that, when one controlled for sentence length, more attendance in regular prison academic programs slightly decreased the subsequent chances of parole violation, this improvement was not large enough to outweigh the associated disadvantage for the "long-attenders": Those who attended prison school the longest also turned out to be those who were in prison the longest. Presumably, those getting the most education were also the worst parole risks in the first place.[3]

Studies of special education programs aimed at vocational or social skill development, as opposed

to conventional academic education programs, report similarly discouraging results and reveal additional problems in the field of correctional research. Jacobson (1965) studied a program of "skill re-education" for institutionalized young males, consisting of 10 weeks of daily discussions aimed at developing problem-solving skills. The discussions were led by an adult who was thought capable of serving as a role model for the boys, and they were encouraged to follow the example that he set. Jacobson found that over all, the program produced no improvement in recidivism rates. There was only one special subgroup which provided an exception to this pessimistic finding: If boys in the experimental program decided afterwards to go on to take three or more regular prison courses, they did better upon release than "control" boys who had done the same. (Of course, it also seems likely that experimental boys who did *not* take these extra courses did worse than their controls.)

Zivan (1966) also reported negative results from a much more ambitious vocational training program at the Children's Village in Dobbs Ferry, New York. Boys in his special program were prepared for their return to the community in a wide variety of ways. First of all, they were given, in sequence, three types of vocational guidance: "assessment counseling," "development counseling," and "pre-placement counseling." In addition, they participated in an "occupational orientation," consisting of role-playing, presentations via audiovisual aids, field trips, and talks by practitioners in various fields of work. Furthermore, the boys were prepared for work by participating in the Auxiliary Maintenance Corps, which performed various chores in the institution; a boy might be promoted from the Corps to the Work Activity Program, which "hired" him, for a small fee, to perform various artisans' tasks. And finally, after release from Children's Village, a boy in the special program received supportive aftercare and job placement aid.

None of this made any difference in recidivism rates. Nevertheless, one must add that it is impossible to tell whether this failure lay in the program itself or in the conditions under which it was administered. For one thing, the education department of the institution itself was hostile to the program; they believed instead in the efficacy of academic education. This staff therefore tended to place in the pool from which experimental subjects were randomly selected mainly "multi-problem" boys. This by itself would not have invalidated the experiment as a test of vocational training for this particular type of youth, but staff hostility did not end there; it exerted subtle pressures of disapproval throughout the life of the program. Moreover, the program's "after-care" phase also ran into difficulties; boys who were sent back to school before getting a job often received advice that conflicted with the program's counseling, and boys actually looking for jobs met with the frustrating fact that the program's personnel, despite concerted efforts, simply could not get businesses to hire the boys.

We do not know whether these constraints, so often found in penal institutions, were responsible for the program's failure; it might have failed anyway. All one can say is that this research failed to show the effectiveness of special vocational training for young males.

The only clearly positive report in this area comes from a study by Sullivan (1967) of a program that combined academic education with special training in the use of IBM equipment. Recidivism rates after one year were only 48 percent for experimentals, as compared with 66 percent for controls. But when one examines the data, it appears that this difference emerged only between the controls and those who had successfully *completed* the training. When one compares the control group with all those who had been *enrolled* in the program, the difference disappears. Moreover, during this study the random assignment procedure between experimental and control groups seems to have broken down, so that towards the end, better risks had a greater chance of being assigned to the special program.

In sum, many of these studies of young males are extremely hard to interpret because of flaws in research design. But it can safely be said that they provide us with no clear evidence that education or skill development programs have been successful.

TRAINING ADULT INMATES

When one turns to adult male inmates, as opposed to young ones, the results are even more discouraging. There have been six studies of this type; three of them report that their programs, which ranged from academic to prison work experience, produced no significant differences in recidivism rates, and one—by Glaser (1964)—is almost impossible to interpret because of the risk differentials of the prisoners participating in the various programs.

Two studies—by Schnur (1948) and by Saden (1962)—*do* report a positive difference from skill development programs. In one of them, the Saden study, it is questionable whether the experimental and control groups were truly comparable. But what is more interesting is that both these "positive" studies dealt with inmates incarcerated prior to or during World War II. Perhaps the rise in our educational standards as a whole since then has lessened the differences that prison education or training can make. The only other interesting possibility emerges from a study by Gearhart (1967). His study was as one of those that reported vocational education to be nonsignificant in effecting recidivism rates. He did note, however, that when a trainee succeeded in finding a job related to his area of training, he had a slightly higher chance of becoming a successful parolee. It is possible, then, that skill development programs fail because what they teach bears so little relationship to an offender's subsequent life outside the prison.

One other study of adults, this one with fairly clear implications, has been performed with women rather than men. An experimental group of institutionalized women in Milwaukee was given an extremely comprehensive special education program, accompanied by group counseling. Their training was both academic and practical; it included reading, writing, spelling, business filing, child care, and grooming. Kettering (1965) found that the program made no difference in the women's rates of recidivism.

Two things should be noted about these studies. One is the difficulty of interpreting them as a whole. The disparity in the programs that were tried, in the populations that were affected, and in the institutional settings that surrounded these projects make it hard to be sure that one is observing the same category of treatment in each case. But the second point is that despite this difficulty, one can be reasonably sure that, so far, educational and vocational programs have not worked. We don't know why they have failed. We don't know whether the programs themselves are flawed, or whether they are incapable of overcoming the effects of prison life in general. The difficulty may be that they lack applicability to the world the inmate will face outside of prison. Or perhaps the type of educational and skill improvement they produce simply doesn't have very much to do with an individual's propensity to commit a crime. What we do know is that, to date, education and skill development have not reduced recidivism by rehabilitating criminals.

THE EFFECTS OF INDIVIDUAL COUNSELING

2. *But when we speak of a rehabilitative prison, aren't we referring to more than education and skill development alone? Isn't what's needed [is] some way of counseling inmates or helping them with the deeper problems that have caused their maladjustment?*

This, too, is a reasonable hypothesis; but when one examines the programs of this type that have been tried, it's hard to find any more grounds for enthusiasm than we found with skill development and education. One method that's been tried—though so far, there have been acceptable reports

only of its application to young offenders—has been individual psychotherapy. For young males, we found seven such reported studies. One study, by Guttman (1963) at the Nelles School, found such treatment to be ineffective in reducing recidivism rates; another, by Rudoff (1960), found it unrelated to *institutional* violation rates, which were themselves related to parole success. It must be pointed out that Rudoff used only this indirect measure of association, and the study therefore cannot rule out the possibility of a treatment effect. A third, also by Guttman (1963) but at another institution, found that such treatment was actually related to a slightly *higher* parole violation rate; and a study by Adams (1959b and 1961b) also found a lack of improvement in parole revocation and first suspension rates.

There were two studies at variance with this pattern. One by Persons (1966) said that if a boy was judged to be "successfully" treated—as opposed to simply being subjected to the treatment experience—he did tend to do better. And there was one finding both hopeful and cautionary: At the Deuel School (Adams, 1961a), the experimental boys were first divided into two groups, those rated as "amenable" to treatment and those rated "non-amenable." Amenable boys who got the treatment did better than non-treated boys. On the other hand, "non-amenable" boys who were treated actually did worse than they would have done if they had received no treatment at all. It must be pointed out that Guttman (1963), dealing with younger boys in his Nelles School study, did not find such an "amenability" effect, either to the detriment of the non-amenables who were treated *or* to the benefit of the amenables who were treated. But the Deuel School study (Adams, 1961a) suggests both that here is something to be hoped for in treating properly selected amenable subjects and that if these subjects are not properly selected, one may not only wind up doing no good but may actually produce harm. There have been two studies of the effects of individual psychotherapy on young incarcerated *female* offenders, and both of them (Adams, 1959a; Adams, 1961b) report no significant effects from the therapy. But one of the Adams studies (1959a) does contain a suggestive, although not clearly interpretable, finding: If this individual therapy was administered by a psychiatrist or a psychologist, the resulting parole suspension rate was almost two-and-a-half times *higher* than if it was administered by a social worker without this specialized training.

There has also been a much smaller number of studies of two other types of individual therapy: counseling, which is directed towards a prisoner's gaining new insight into his own problems, and casework, which aims at helping a prisoner cope with his more pragmatic immediate needs. These types of therapy both rely heavily on the empathetic relationship that is to be developed between the professional and the client. It was noted above that the Adams study (1961b) of therapy administered to girls, referred to in the discussion of individual psychotherapy, found that social workers seemed better at the job than psychologists or psychiatrists. This difference seems to suggest a favorable outlook for these alternative forms of individual therapy. But other studies of such therapy have produced ambiguous results. Bernsten (1965) reported a Danish experiment that showed that socio-psychological counseling combined with comprehensive welfare measures—job and residence placement, clothing, union and health insurance membership, and financial aid—produced an improvement among some short-term male offenders, though not those in either the highest-risk or the lowest-risk categories. On the other hand, Hood, in Britain (1966), reported generally non-significant results with a program of counseling for young males. (Interestingly enough, this experiment *did* point to a mechanism capable of changing recidivism rates. When boys were released from institutional care and entered the army directly, "poor risk" boys among both experimentals *and* controls did better than expected. "Good risks" did worse.)

So these foreign data are sparse and not in agreement; the American data are just as sparse. The only American study which provides a direct measure of the effects of individual counseling—a study of California's Intensive Treatment Program (California, 1958a), which was "psychodynamically" oriented—found no improvement in recidivism rates.

It was this finding of the failure of the Intensive Treatment Program which contributed to the decision in California to de-emphasize individual counseling in its penal system in favor of group methods. And indeed one might suspect that the preceding reports reveal not the inadequacy of counseling as a whole but only the failure of one *type* of counseling, the individual type. Group counseling methods, in which offenders are permitted to aid and compare experiences with one another, might be thought to have a better chance of success. So it is important to ask what results these alternative methods have actually produced.

GROUP COUNSELING

Group counseling has indeed been tried in correctional institutions both with and without a specifically psychotherapeutic orientation. There has been one study of "pragmatic," problem-oriented counseling on *young* institutionalized males by Seckel (1965). This type of counseling had no significant effect. For adult males, there have been three such studies of the "pragmatic" and "insight" methods. Two (Kassebaum, 1971; Harrison, 1964) report no long-lasting significant effects. (One of these two did report a real but short-term effect that wore off as the program became institutionalized and as offenders were at liberty longer.) The third study of adults, by Shelley (1961), dealt with a "pragmatic" casework program, directed towards the educational and vocational needs of institutionalized young adult males in a Michigan prison camp. The treatment lasted for six months and at the end of that time Shelley found an improvement in attitudes; the possession of "good" attitudes was independently found by Shelley to correlate with parole success. Unfortunately, though, Shelley was not able to measure the *direct* impact of the counseling on recidivism rates. His two separate correlations are suggestive, but they fall short of being able to tell us that it really is the counseling that has a direct effect on recidivism.

With regard to more professional group *psychotherapy*, the reports are also conflicting. We have two studies of group psychotherapy on young males. One, by Persons (1966), says that this treatment did in fact reduce recidivism. The improved recidivism rate stems from the improved performance only of those who were clinically judged to have been "successfully" treated; still, the overall result of the treatment was to improve recidivism rates for the experimental group as a whole. On the other hand, a study by Craft (1964) of young males designated "psychopaths," comparing "self-government" group psychotherapy with "authoritarian" individual counseling, found that the "group therapy" boys afterwards committed twice as many new offenses as the individually treated ones. Perhaps some forms of group psychotherapy work for some types of offenders but not others; a reader must draw his own conclusions on the basis of sparse evidence.

With regard to young females, the results are just as equivocal. Adams, in his study of females (1959a), found that there was no improvement to be gained from treating girls by group rather than individual methods. A study by Taylor of borstal (reformatory) girls in New Zealand (1967) found a similar lack of any great improvement for group therapy as opposed to individual therapy or even to no therapy at all. But the Taylor study does offer one real, positive finding: When the "group therapy" girls *did* commit new offenses, these offenses were less serious than the ones for which they had originally been incarcerated.

There is a third study that does report an overall positive finding as opposed to a partial one. Truax (1966) found that girls subjected to group

psychotherapy and then released were likely to spend less time reincarcerated in the future. But what is most interesting about this improvement is the very special and important circumstance under which it occurred. The therapists chosen for this program did not merely have to have the proper analytic training; they were specially chosen for their "empathy" and "non-possessive warmth." In other words, it may well have been the therapists' special personal gifts rather than the fact of treatment itself which produced the favorable result. This possibility will emerge again when we examine the effects of other types of rehabilitative treatment later in this article.

As with the question of skill development, it is hard to summarize these results. The programs administered were various; the groups to which they were administered varied not only by sex but by age as well; there were also variations in the length of time for which the programs were carried on, the frequency of contact during that time, and the period for which the subjects were followed up. Still, one must say that the burden of the evidence is not encouraging. These programs seem to work best when they are new, when their subjects are amenable to treatment in the first place, and when the counselors are not only trained people but "good" people as well. Such findings, which would not be much of a surprise to a student of organization or personality, are hardly encouraging for a policy planner, who must adopt measures that are generally applicable, that are capable of being successfully institutionalized, and that must rely for personnel on something other than the exceptional individual.

TRANSFORMING THE INSTITUTIONAL ENVIRONMENT

> *But maybe the reason these counseling programs don't seem to work is not that they are ineffective per se, but that the institutional environment outside the program is unwholesome enough to undo any good work that the counseling does. Isn't a truly successful rehabilitative institution the one where the inmate's whole environment is directed towards true correction rather than towards custody or punishment?*

This argument has not only been made, it has been embodied in several institutional programs that go by the name of "milieu therapy." They are designed to make every element of the inmates environment a part of his treatment, to reduce the distinctions between the custodial staff and the treatment staff, to create a supportive, non-authoritarian, and non-regimented atmosphere, and to enlist peer influence in the formation of constructive values. These programs are especially hard to summarize because of their variety. They differ, for example, in how "supportive" or "permissive" they are designed to be, in the extent to which they are combined with other treatment methods such as individual therapy, group counseling, or skill development, and in how completely the program is able to control all the relevant aspects of the institutional environment.

One might well begin with two studies that have been done of institutionalized adults, in regular prisons, who have been subjected to such treatment; this is the category whose results are the most clearly discouraging. One study of such a program, by Robison (1967), found that the therapy did seem to reduce recidivism after one year. After two years, however, this effect disappeared, and the treated convicts did no better than the untreated. Another study by Kassebaum, Ward, and Wilnet (1971), dealt with a program which had been able to effect an exceptionally extensive and experimentally rigorous transformation of the institutional environment. This sophisticated study had a follow-up period of 36 months, and it found that the program had no significant effect on parole failure or success rates.

The results of the studies of youth are more equivocal. As for young females, one study by Adams (1966) of such a program found that it had

no significant effect on recidivism; another study, by Goldberg and Adams (1964), found that such a program did have a positive effect. This effect declined when the program began to deal with girls who were judged beforehand to be worse risks.

As for young males, the studies may conveniently be divided into those dealing with juveniles (under 18) and those dealing with youths. There have been five studies of milieu therapy administered to juveniles. Two of them—by Lavlicht (1962) and by Jesness (1965)—report clearly that the program in question either had no significant effect or had a short-term effect that wore off with passing time. Jesness does report that when his experimental juveniles did commit new offenses, the offenses were less serious than those committed by controls. A third study of juveniles, by McCord (1953) at the Wiltwyck School, reports mixed results. Using two measures of performance, a "success" rate and a "failure" rate, McCord found that his experimental group achieved both less failure *and* less success than the controls did. There have been two positive reports on milieu therapy programs for male juveniles; both of them have come out of the Highfields program, the milieu therapy experiment which has become the most famous and widely quoted example of "success" via this method. A group of boys was confined for a relatively short time to the unrestrictive, supportive environment of Highfields; and at a follow-up of six months, Freeman (1956) found that the group did indeed show a lower recidivism rate (as measured by parole revocation) than a similar group spending a longer time in the regular reformatory. McCorkle (1958) also reported positive findings from High-fields. But in fact, [as] the McCorkle data show[s], this improvement was not so clear: The Highfields boys had lower recidivism rates at 12 and 36 months in the follow-up period, but not at 24 and 60 months. The length of follow-up, these data remind us, may have large implications for a study's conclusions. But more important were other flaws in the Highfields experiment: The populations were not fully comparable (they differed according to risk level and time of admission); different organizations—the probation agency for the Highfield boys, the parole agency for the others—were making the revocation decisions for each group; more of the Highfields boys were discharged early from supervision, and thus removed from any risk of revocation. In short, not even from the celebrated Highfields case may we take clear assurance that milieu therapy works.

In the case of male youths, as opposed to male juveniles, the findings are just as equivocal, and hardly more encouraging. One such study by Empey (1966) in a residential context did not produce significant results. A study by Seckel (1967) described California's Fremont Program, in which institutionalized youths participated in a combination of therapy, work projects, field trips, and community meetings. Seckel found that the youths subjected to this treatment committed more violations of law than did their non-treated counterparts. This difference could have occurred by chance; still, there was certainly no evidence of relative improvement. Another study, by Levinson (1962–1964), also found a lack of improvement in recidivism rates—but Levinson noted the encouraging fact that the treated group spent somewhat more time in the community before recidivating, and committed less serious offenses. And a study by the State of California (1967) also shows a partially positive finding. This was a study of the Marshall Program, similar to California's Fremont Program but different in several ways. The Marshall Program was shorter and more tightly organized than its Fremont counterpart. In the Marshall Program, as opposed to the Fremont Program, a youth could be ejected from the group and sent back to regular institutions before the completion of the program. Also, the Marshall Program offered some additional benefits: the teaching of "social survival skills" (i.e., getting and holding a job), group counseling of parents, and an occasional opportunity for boys to visit home. When youthful offenders were released to the Marshall Program, either directly or after

spending some time in a regular institution, they did no better than a comparable regularly institutionalized population, though both Marshall youth and youth in regular institutions did better than those who were directly released by the court and given no special treatment.

So the youth in these milieu therapy programs at least do no worse than their counterparts in regular institutions and the special programs may cost less. One may therefore be encouraged—not on grounds of rehabilitation but on grounds of cost effectiveness.

WHAT ABOUT MEDICAL TREATMENT?

4. *Isn't there anything you can do in an institutional setting that will reduce recidivism, for instance, through strictly medical treatment?*

A number of studies deal with the results of efforts to change the behavior of offenders through drugs and surgery. As for surgery, the one experimental study of a plastic surgery program—by Mandell (1967)—had negative results. For nonaddicts who received plastic surgery, Mandell purported to find improvement in performance on parole; but when one reanalyzes his data, it appears that surgery alone did not in fact make a significant difference.

One type of surgery does seem to be highly successful in reducing recidivism. A twenty-year Danish study of sex offenders, by Stuerup (1960), found that while those who had been treated with hormones and therapy continued to commit both sex crimes (29.6 percent of them did so) and non-sex crimes (21.0 percent), those who had been castrated had rates of only 3.5 percent (not, interestingly enough, a rate of zero; where there's a will, apparently there's a way) and 9.2 percent. One hopes that the policy implications of this study will be found to be distinctly limited.

As for drugs, the major report on such a program—involving tranquilization—was made by Adams (1961b). The tranquilizers were administered to male and female institutionalized youths. With boys, there was only a slight improvement in their subsequent behavior; this improvement disappeared within a year. With girls, the tranquilization produced worse results than when the girls were given no treatment at all.

THE EFFECTS OF SENTENCING

5. *Well, at least it may be possible to manipulate certain gross features of the existing, conceptional prison system—such as length of sentence and degree of security—in order to affect these recidivism rates. Isn't this the case?*

At this point, it's still impossible to say that this is the case. As for the degree of security in an institution, Glaser's (1964) work reported that, for both youth and adults, a less restrictive "custody grading" in American federal prisons was related to success on parole; but this is hardly surprising, since those assigned to more restrictive custody are likely to be worse risks in the first place. More to the point, an American study by Fox (1950) discovered that for "older youths" who were deemed to be good risks for the future, a minimum security institution produced better results than a maximum security one. On the other hand, the data we have on youths under 16—from a study by McClintock (1961), done in Great Britain—indicate that so-called Borstals, in which boys are totally confined, are more effective than a less restrictive regime of partial physical custody. In short, we know very little about the recidivism effects of various degrees of security in existing institutions; and our problems in finding out will be compounded by the probability that these effects will vary widely according to the particular *type* of offender that we're dealing with.

The same problems of mixed results and lack of comparable populations have plagued attempts to study the effects of sentence length. A number of studies—by Narloch (1959), by Bernsten (1965),

and by the State of California (1956)—suggest that those who are released earlier from institutions than their scheduled parole date, or those who serve short sentences of under three months rather than longer sentences of eight months or more, either do better on parole or at least do no worse.[4] The implication here is quite clear and important: Even if early releases and short sentences produce no improvement in recidivism rates, one could at least maintain the same rates while lowering the cost of maintaining the offender and lessening his own burden of imprisonment. Of course, this implication carries with it its concomitant danger: the danger that though shorter sentences cause no worsening of the recidivism rate, they may increase the total amount of crime in the community by increasing the absolute number of potential recidivists at large.

On the other hand, Glaser's (1964) data show not a consistent linear relationship between the shortness of the sentence and the rate of parole success, but a curvilinear one. Of his subjects, those who served less than a year had a 73 percent success rate, those who served up to two years were only 65 percent successful, and those who served up to three years fell to a rate of 56 percent. But among those who served sentences of *more* than three years, the success rate rose again—to 60 percent. These findings should be viewed with some caution since Glaser did not control for the pre-existing degree of risk associated with each of his categories of offenders. But the data do suggest that the relationship between sentence length and recidivism may not be a simple linear one.

More important, the effect of sentence length seems to vary widely according to type of offender. In a British study (1963), for instance, Hammond found that for a group of "hard-core recidivists," shortening the sentence caused no improvement in the recidivism rate. In Denmark, Bernsten (1965) discovered a similar phenomenon: That the beneficial effect of three-month sentences as against eight-month ones disappeared in the case of these "hard-core recidivists." Garrity found an other such distinction in his 1956 study. He divided his offenders into three categories: "pro-social," "anti-social," and "manipulative." "Pro-social" offenders he found to have low recidivism rates regardless of the length of their sentence; "anti-social" offenders did better with short sentences; the "manipulative" did better with long ones. Two studies from Britain made yet another division of the offender population, and found yet other variations. One (Great Britain, 1964) found that previous offenders—but not first offenders—did better with *longer* sentences, while the other (Cambridge, 1952) found the *reverse* to be true with juveniles.

To add to the problem of interpretation, these studies deal not only with different types and categorizations of offenders but with different types of institutions as well. No more than in the case of institution type can we say that length of sentence has a clear relationship to recidivism.

DECARCERATING THE CONVICT

6. *All of this seems to suggest that there's not much we know how to do to rehabilitate an offender when he's in an institution. Doesn't this lead to the clear possibility that the way to rehabilitate offenders is to deal with them outside an institutional setting?*

This is indeed an important possibility, and it is suggested by other pieces of information as well. For instance, Minet (1967) reported on a milieu therapy program in Massachusetts called Outward Bound. It took youths $15\frac{1}{2}$ and over; it was oriented toward the development of skills in the out-of-doors and conducted in a wilderness atmosphere very different from that of most existing institutions. The culmination of the 26-day program was a final 24 hours in which each youth had to survive alone in the wilderness. And Miner found that the program did indeed work in reducing recidivism rates.

But by and large, when one takes the programs that have been administered in institutions

and applies them in a non-institutional setting, the results do not grow to encouraging proportions. With casework and individual counseling in the community, for instance, there have been three studies; they dealt with counseling methods from psycho-social and vocational counseling to "operant conditioning," in which an offender was rewarded first simply for coming to counseling sessions and then, gradually, for performing other types of approved sets. Two of them report that the community-counseled offenders did no better than their institutional controls, while the third notes that although community counseling produced fewer arrests per person, it did not ultimately reduce the offenders chance of resuming to a reformatory.

The one study of a non-institutional skill development program, by Kovacs (1967), described the New Start Program in Denver, in which offenders participated in vocational training, role playing, programmed instruction, group counseling, college class attendance, and trips to art galleries and museums. After all this, Kovacs found no significant improvement over incarceration.

There have also been studies of milieu therapy programs conducted with youthful male probationers not in actual physical custody. One of them found no significant improvement at all. One, by Empey (1966), did say that after a follow-up of six months, a boy who was judged to have "successfully" completed the milieu program was less likely to recidivate afterwards than was a "successful," regular probationer. Empey's "successes" came out of an extraordinary program in Provo, Utah, which aimed to rehabilitate by subjecting offenders to a non-supportive milieu. The staff of this program operated on the principle that they were not to go out of their way to interact and be empathetic with the boys. Indeed, a boy who misbehaved was to be met with "role dispossession": He was to be excluded from meetings of his peer group, and he was not to be given answers to his questions as to why he had been excluded or what his ultimate fate might be. This peer group and its meetings were designed to be the major force for reform at Provo; they were intended to develop, and indeed did develop, strong and controlling norms for the behavior of individual members. For one thing, group members were not to associate with delinquent boys outside the program; for another, individuals were to submit to a group review of all their actions and problems; and they were to be completely honest and open with the group about their attitudes, their thinking patterns, their states of mind, and their personal failings. The group was granted quite a few sanctions with which to enforce these norms: They could practice derision or temporary ostracism, or they could lock up an aberrant member for the weekend, refuse to release him from the program, or send him away to the regular reformatory.

One might be tempted to forgive these methods because of the success that Empey reports, except for one thing. If one judges the program not only by its "successful" boys but by all the boys who were subjected to it—those who succeeded and those who, not surprisingly, failed—the totals show *no* significant improvement in recidivism rates compared with boys on regular probation. Empey did find that both the Provo boys and those on regular probation did better than those in regular reformatories—in contradiction, it may be recalled, to the finding from the residential Marshall Program, in which the direct releases given no special treatment did *worse* than boys in regular institutions.

The third such study of non-residential milieu therapy, by McCravy (1967), found not only that there was no significant improvement, but that the longer a boy participated in the treatment, the worse he was likely to do afterwards.

PSYCHOTHERAPY IN COMMUNITY SETTINGS

There is some indication that individual psychotherapy may "work" in a community setting. Massimo (1963) reported on one such program, using what might be termed a "pragmatic" psychotherapeutic

approach, including "insight" therapy and a focus on vocational problems. The program was marked by its small size and by its use of therapists who were personally enthusiastic about the project; Massimo found that there was indeed a decline in recidivism rates. Adamson (1956), on the other hand, found no significant difference produced by another program of individual therapy (though he did note that arrest rates among the experimental boys declined with what he called "intensity of treatment"). And Schwitzgebel (1963, 1964), studying other, different kinds of therapy programs, found that the programs *did* produce improvements in the attitudes of his boys—but, unfortunately, not in their rates of recidivism.

And with *group* therapy administered in the community, we find yet another set of equivocal results. The results from studies of pragmatic group counseling are only mildly optimistic. Adams (1965) did report that a form of group therapy, "guided group interaction," when administered to juvenile gangs, did somewhat reduce the percentage that were to be found in custody six years later. On the other hand, in a study of juveniles, Adams (1964) found that while such a program did reduce the number of contacts that an experimental youth had with police, it made no ultimate difference in the detention rate. And the attitudes of the counseled youth showed no improvement. Finally, when O'Brien (1961) examined a community-based program of group psychotherapy, he found not only that the program produced no improvement in the recidivism rate, but that the experimental boys actually did worse than their controls on a series of psychological tests.

PROBATION OR PAROLE VERSUS PRISON

But by far the most extensive and important work that has been done on the effect of community-based treatments has been done in the areas of probation and parole. This work sets out to answer the question of whether it makes any difference how you supervise and treat an offender once he has been released from prison or has come under state surveillance in lieu of prison. This is the work that has provided the main basis to date for the claim that we do indeed have the means at our disposal for rehabilitating the offender or at least decarcerating him safely.

One group of these studies has compared the use of probation with other dispositions for offenders; these provide some slight evidence that, at least under some circumstances, probation may mate an offender's future chances better than if he had been sent to prison. Or, at least, probation may not worsen those chances.[5] A British study, by Wilkins (1958), reported that when probation was granted more frequently, recidivism rates among probationers did not increase significantly. And another such study by the state of Michigan in 1963 reported that an expansion in the use of probation actually improved recidivism rates—though there are serious problems of comparability in the groups and systems that were studied.

One experiment—by Babst (1965)—compared a group of parolees, drawn from adult male felony offenders in Wisconsin, and excluding murderers and sex criminals, with a similar group that had been put on probation; it found that the probationers committed fewer violations if they had been first offenders, and did no worse if they were recidivists. The problem in interpreting this experiment, though, is that the behavior of those groups was being measured by separate organizations, by probation officers for the probationers, and by parole officers for the parolees; it is not clear that the definition of "violation" was the same in each case, or that other types of uniform standards were being applied. Also, it is not clear what the results would have been if subjects had been released directly to the parole organization without having experienced prison first. Another such study, done in Israel by Shoham (1964), must be interpreted cautiously because his experimental and control groups had

slightly different characteristics. But Shoham found that when one compared a suspended sentence plus probation for first offenders with a one-year prison sentence, only first offenders under 20 years of age did better on probation; those from 21 to 45 actually did *worse*. And Shoham's findings also differ from Babst's in another way. Babst had found that parole rather than prison brought no improvement for recidivists, but Shoham reported that for recidivists with four or more prior offenses, a suspended sentence was actually *better*—though the improvement was much less when the recidivist had committed a crime of violence.

But both the Babst and the Shoham studies, even while they suggest the possible value of suspended sentences, probation, or parole for some offenders (though they contradict each other in telling us *which* offenders), also indicate a pessimistic general conclusion concerning the limits of the effectiveness of treatment programs. For they found that the personal characteristics of offenders—"first offender status, or age, or type of offense"—were more important than the form of treatment in determining future recidivism. An offender with a "favorable" prognosis will do better than one without, it seems, no matter how you distribute "good" or "bad," "enlightened" or "regressive" treatments among them.

Quite a large group of studies deals not with probation as compared to other dispositions, but instead with the type of treatment that an offender receives once he is on probation or parole. These are the studies that have provided the most encouraging reports on rehabilitative treatment and that have also raised the most serious questions about the nature of the research that has been going on in the corrections field.

Five of these studies have dealt with youthful probationers from 13 to 18 who were assigned to probation officers with small caseloads or provided with other ways of receiving more intensive supervision (Adams, 1966 [two reports]; Feistman, 1966; Kawaguchi, 1967; Pilnick, 1967). These studies report that, by and large, intensive supervision does work—that the specially treated youngsters do better according to some measure of recidivism. Yet these studies left some important questions unanswered. For instance, was this improved performance a function merely of the number of contacts a youngster had with his probation officer? Did it also depend on the length of time in treatment? Or was it the quality of supervision that was making the difference, rather than the quantity?

INTENSIVE SUPERVISION: THE WARREN STUDIES

The widely reported Warren studies (1966a, 1966b, 1967) in California constitute an extremely ambitious attempt to answer these questions. In this project, a control group of youths, drawn from a pool of candidates ready for first admission to a California Youth Authority institution, was assigned to regular detention, usually for eight to nine months, and then released to regular supervision. The experimental group received considerably more elaborate treatment. They were released directly to probation status and assigned to 12-man caseloads. To decide what special treatment was appropriate within these caseloads, the youths were divided according to their "interpersonal maturity level classification," by use of a scale developed by Grant and Grant. And each level dictated its own special type of therapy. For instance, a youth might be judged to occupy the lowest maturity level; this would be a youth, according to the scale, primarily concerned with "demands that the world take care of him. . . . He behaves impulsively, unaware of anything except the grossest effects of his behavior on others." A youth like this would be placed in a supportive environment such as a foster home; the goals of his therapy would be to meet his dependency needs and help him gain more accurate perceptions about his relationship to others. At the other end of the three-tier classification a youth might exhibit high maturity. This would be a youth

who had internalized "a set of standards by which he judges his and others' behavior. ... He shows some ability to understand reasons for behavior, some ability to relate to people emotionally and on a long-term basis." These high-maturity youths could come in several varieties—a "neurotic acting out," for instance, a "neurotic anxious," a "situational emotional reactor," or a "cultural identifier." But the appropriate treatment for these youths was individual psychotherapy, or family or group therapy for the purpose of reducing internal conflicts and increasing the youths' awareness of personal and family dynamics.

"Success" in this experiment was defined as favorable discharge by the Youth Authority; "failure" was unfavorable discharge, revocation, or recommitment by a court. Warren reported an encouraging finding: Among all but one of the "subtypes," the experimentals had a significantly lower failure rate than the controls. The experiment did have certain problems: The experimentals might have been performing better because of the enthusiasm of the staff and the attention lavished on them; none of the controls had been *directly* released to their regular supervision programs instead of being detained first; and it was impossible to separate the effects of the experimentals' small caseloads from their specially designed treatments, since no experimental youths had been assigned to a small caseload with "inappropriate" treatment, or with no treatment at all. Still, none of these problems were serious enough to vitiate the encouraging prospect that this finding presented for successful treatment of probationers.

This encouraging finding was, however, accompanied by a rather more disturbing clue. As has been mentioned before, the experimental subjects, when measured, had a lower *failure* rate than the controls. But the experimentals also had a lower *success* rate. That is, fewer of the experimentals as compared with the controls had been judged to have successfully completed their program of supervision and to be suitable for favorable release. When my colleagues and I undertook a rather laborious reanalysis of the Warren data, it became clear why this discrepancy had appeared. It turned out that fewer experimentals were "successful" because the experimentals were actually committing more offenses than their controls. The reason that the experimentals' relatively large number of offenses was not being reflected in their failure rates was simply that the experimentals' probation officers were using a more lenient revocation policy. In other words, the controls had a higher failure rate because the controls were being revoked for less serious offenses.

So it seems that what Warren was reporting in her "failure" rates was not merely the treatment effect of her small caseloads and special programs. Instead, what Warren was finding was not so much a change in the behavior of the experimental youths as a change in the behavior of the experimental *probation officers*, who knew the "special" status of their charges and who had evidently decided to revoke probation status at a lower than normal rate. The experimentals continued to commit offenses; what was different was that when they committed these offenses, they were permitted to remain on probation.

The experimenters claimed that this low revocation policy, and the greater number of offenses committed by the special treatment youth, were *not* an indication that these youth were behaving specially badly and that policy makers were simply letting them get away with it. Instead it was claimed, the higher reported offense rate was primarily an artifact of the more intense surveillance that the experimental youth received. But the data show that this is not a sufficient explanation of the low failure rate among experimental youth; the difference in "tolerance" of offenses between experimental officials and control officials was much greater than the difference in the rates at which these two systems detected youths committing new offenses. Needless to say, this reinterpretation of the data presents a much bleaker picture

of the possibilities of intensive supervision with special treatment.

"TREATMENT EFFECT" VERSUS "POLICY EFFECTS"

This same problem of experimenter bias may also be present in the predecessors of the Warren study, the ones which had also found positive results from intensive supervision on probation; indeed, this disturbing question can be raised about many of the previously discussed reports of positive "treatment effects."

This possibility of a "policy effect" rather than a "treatment effect" applies, for instance, to the previously discussed studies of the effects of intensive supervision on juvenile and youthful probationers. These were the studies, it will be recalled, which found lower recidivism rates for the intensively supervised.[6]

One opportunity to make a further check on the effects of this problem is provided, in a slightly different context, by Johnson (1962a). Johnson was measuring the effects of intensive supervision on youthful *parolees* (as distinct from probationers). There have been several such studies of the effects on youths of intensive parole supervision plus special counseling and their findings are on the whole less encouraging than the probation studies; they are difficult to interpret because of experimental problems, but studies by Boston University in 1966, and by Van Couvering in 1966, report no significant effects and possibly some bad effects from such special programs. But Johnson's studies were unique for the chance they provide to measure both treatment effects and the effect of agency policy.

Johnson, like Warren, assigned experimental subjects to small caseloads and his experiment had the virtue of being performed with two separate populations and at two different times. But in contrast with the Warren case, the Johnson experiment did not engage in a large continuing attempt to choose the experimental counselors specially, to train them specially, and to keep them informed about the progress and importance of the experiment. The first time the experiment was performed, the experimental youths had a slightly lower revocation rate than the controls at six months. But the second time, the experimentals did *not* do better than their controls; indeed, they did slightly worse. And with the experimentals from the first group—those who *had* shown an improvement after six months—this effect wore off at 18 months. In the Johnson study, my colleagues and I found, "intensive" supervision did not increase the experimental youths' risk of detection. Instead, what was happening in the Johnson experiment was that the first time it had been performed—just as in the Warren study—the experimentals were simply revoked less often per number of offenses committed, and they were revoked for offenses more serious than those which prompted revocation among the controls. The second time around, this "policy" discrepancy disappeared; and when it did, the "improved" performance of the experimentals disappeared as well. The enthusiasm guiding the project had simply worn off in the absence of reinforcement.

One must conclude that the "benefits" of intensive supervision for youthful offenders may stem not so much from a "treatment" effect as from a "policy" effect—that such supervision, so far as we now know, results not in rehabilitation but in a decision to look the other way when an offense is committed. But there is one major modification to be added to this conclusion. Johnson performed a further measurement (1962b) in his parole experiment: He rated all the supervising agents according to the "adequacy" of the supervision they gave. And he found that an "adequate" agent, whether he was working in a small *or* a large caseload produced a relative improvement in his charges. The converse was not true: An inadequate agent was more likely to produce youthful "failures" when he was given a *small* caseload to supervise. One can't much help a "good" agent, it seems, by reducing his caseload size; such reduction can only do further harm to those youths who fall into the hands of "bad" agents.

So with youthful offenders, Johnson found, intensive supervision does not seem to provide the rehabilitative benefits claimed for it; the only such benefits may flow not from intensive supervision itself but from contact with one of the "good people" who are frequently in such short supply.

INTENSIVE SUPERVISION OF ADULTS

The results are similarly ambiguous when one applies this intensive supervision to adult offenders. There have been several studies of the effects of intensive supervision on adult parolees. Some of these are hard to interpret because of problems of comparability between experimental and control groups (general risk ratings, for instance, or distribution of narcotics offenders, or policy changes that took place between various phases of the experiments), but two of them (California, 1966; Stanton, 1964) do not seem to give evidence of the benefits of intensive supervision. By far the most extensive work, though, on the effects of intensive supervision of adult parolees has been a series of studies of California's Special Intensive Parole Unit (SIPU), a 10-year-long experiment designed to test the treatment possibilities of various special parole programs. Three of the four "phases" of this experiment produced "negative results." The first phase tested the effect of a reduced caseload size; no lasting effect was found. The second phase slightly increased the size of the small caseloads and provided for a longer time in treatment; again there was no evidence of a treatment effect. In the fourth phase, caseload sizes and time in treatment were again varied, and treatments were simultaneously varied in a sophisticated way according to personality characteristics of the parolees; once again, significant results did not appear.

The only phase of this experiment for which positive results were reported was Phase Three. Here, it was indeed found that a smaller caseload improved one's chances of parole success. There is, however, an important caveat that attaches to this finding: When my colleagues and I divided the whole population of subjects into two groups—those receiving supervision in the North of the state and those in the South— we found that the "improvement" of the experimentals' success rates was taking place primarily in the North. The North differed from the South in one important aspect: Its agents practiced a policy of resuming both "experimental" and "control" violators to prison at relatively high rates. And it was the North that produced the higher success rate among its experimentals. So this improvement in experimentals' performance was taking place only when accompanied by a "realistic threat" of severe sanctions. It is interesting to compare this situation with that of the Warren studies. In the Warren studies, experimental subjects were being revoked at a relatively *low* rate. These experimentals "failed" less, but they also committed more new offenses than their controls. By contrast, in the Northern region of the SIPU experiment, there was a policy of *high* rate of return to prison for experimentals; and here, the special program *did* seem to produce a real improvement in the behavior of offenders. What this suggests is that when intensive supervision *does* produce an improvement in offenders' behavior, it does so not through the mechanism of "treatment" or "rehabilitation," but instead through a mechanism that our studies have almost totally ignored; the mechanism of *deterrence.* And a similar mechanism is suggested by Lohman's study (1967) of intensive supervision of probationers. In this study intensive supervision led to higher total violation rates. But one also notes that intensive supervision combined the highest rate of technical violations with the lowest rate for *new* offenses.

THE EFFECTS OF COMMUNITY TREATMENT

In sum, even in the case of treatment programs administered outside penal institutions, we simply cannot say that this treatment in itself has an appreciable effect on offender behavior. On the other

hand, there is one encouraging set of findings that emerges from these studies. For from many of them there flows the strong suggestion that even if we can't "treat" offenders so as to make them do better, a great many of the programs designed to rehabilitate them at least did not make them do *worse.* And if these programs did not show the advantages of actually rehabilitating, some of them did have the advantage of being less onerous to the offender himself without seeming to pose increased danger to the community. And some of these programs—especially those involving less restrictive custody, minimal supervision, and early release—simply cost fewer dollars to administer. The information on the dollar costs of these programs is just beginning to be developed but the implication is clear: *that if we can't do more for (and to) offenders, at least we can safely do less.*

There is, however, one important caveat even to this note of optimism: In order to calculate the true costs of these programs, one must in each case include not only their administrative cost but also the cost of maintaining in the community an offender population increased in size. This population might well not be committing new offenses at any greater rate; but the offender population might, under some of these plans, be larger in absolute *numbers.* So the total number of offenses committed might rise, and our chances of victimization might therefore rise too. We need to be able to make a judgment about the size and probable duration of this effect; as of now, we simply do not know.

DOES NOTHING WORK?

7. *Do all of these studies lead us irrevocably to the conclusion that nothing works, that we haven't the faintest clue about how to rehabilitate offenders and reduce recidivism? And if so, what shall we do?*

We tried to exclude from our survey those studies which were so poorly done that they simply could not be interpreted. But despite our efforts, a pattern has run through much of this discussion—of studies which "found" effects without making any truly rigorous attempt to exclude competing hypotheses, of extraneous factors permitted to intrude upon the measurements, of recidivism measures which are not all measuring the same thing, of "follow-up" periods which vary enormously and rarely extend beyond the period of legal supervision, of experiments never replicated, of "system effects" not taken into account, of categories drawn up without any theory to guide the enterprise. It is just possible that some of our treatment programs *are* working to some extent, but that our research is so bad that it is incapable of telling.

Having entered this very serious caveat, I am bound to say that these data, involving over two hundred studies and hundreds of thousands of individuals as they do, are the best available and give us very little reason to hope that we have in fact found a sure way of reducing recidivism through rehabilitation. This is not to say that we found no instances of success or partial success; it is only to say that these instances have been isolated, producing no clear pattern to indicate the efficacy of any particular method of treatment. And neither is this to say that factors *outside* the realm of rehabilitation may not be working to reduce recidivism—factors such as the tendency for recidivism to be lower in offenders over the age of 30; it is only to say that such factors seem to have little connection with any of the treatment methods now at our disposal.

From this probability, one may draw any of several conclusions. It may be simply that our programs aren't yet good enough—that the education we provide to inmates is still poor education, that the therapy we administer is not administered skillfully enough, that our intensive supervision and counseling do not yet provide enough personal support for the offenders who are subjected to them. If one wishes to believe this, then what our correctional system needs is simply a more full-hearted commitment to the strategy of treatment.

It may be, on the other hand, that there is a more radical flaw in our present strategies—that education at its best, or that psychotherapy at its best, cannot overcome, or even appreciably reduce, the powerful tendency for offenders to continue in criminal behavior. Our present treatment programs are based on a theory of crime as a "disease"—that is to say, as something foreign and abnormal in the individual which can presumably be cured. This theory may well be flawed, in that it overlooks—indeed, denies—both the normality of crime in society and the personal normality of a very large proportion of offenders, criminals who are merely responding to the facts and conditions of our society.

This opposing theory of "crime as a social phenomenon" directs our attention away from a "rehabilitative" strategy, away from the notion that we may best insure public safety through a series of "treatments" to be imposed forcibly on convicted offenders. These treatments have on occasion become, and have the potential for becoming, so draconian as to offend the moral order of a democratic society; and the theory of crime as a social phenomenon suggests that such treatments may be not only offensive but ineffective as well. This theory points, instead, to decarceration for low-risk offenders—and, presumably, to keeping high-risk offenders in prisons which are nothing more (and aim to be nothing more) than custodial institutions.

But this approach has its own problems. To begin with, there is the moral dimension of crime and punishment. Many low-risk offenders have committed serious crimes (murder, sometimes) and even if one is reasonably sure they will never commit another crime, it violates our sense of justice that they should experience no significant retribution for their actions. A middle-class banker who kills his adulterous wife in a moment of passion is a "low-risk" criminal; a juvenile delinquent in the ghetto who commits armed robbery has, statistically, a much higher probability of committing another crime. Are we going to put the first on probation and sentence the latter to a long term in prison?

Besides, one cannot ignore the fact that the punishment of offenders is the major means we have for *deterring* incipient offenders. We know almost nothing about the "deterrent effect," largely because "treatment" theories have so dominated our research, and "deterrence" theories have been relegated to the status of a historical curiosity. Since we have almost no idea of the deterrent functions that our present system performs or that future strategies might be made to perform, it is possible that there is indeed something that works—that to some extent is working right now in front of our noses, and that might be made to work better—something that deters rather than cures, something that does not so much reform convicted offenders as prevent criminal behavior in the first place. But whether that is the case and, if it is, what strategies will be found to make our deterrence system work better than it does now, are questions we will not be able to answer with data until a new family of studies has been brought into existence. As we begin to learn the facts, we will be in a better position than we are now to judge to what degree the prison has become an anachronism and can be replaced by more effective means of social control.

STUDY QUESTIONS

1. What was the original impetus for Martinson's study of the effectiveness of rehabilitation programs?
2. Discuss the methodology (research design, data collection methods, sample, and measures) used by the author.
3. Many times, authors citing the Martinson Report erroneously state that his conclusion was that "nothing works." What was his actual conclusion? Does this differ from the one just stated?
4. What exactly were the author's conclusions about community treatment programs?

NOTES

1. Following this case, the state finally did give its permission to have the work published; it will

appear in its complete form in a forthcoming book by Praeger.

2. All studies cited in the text are referenced in the bibliography which appears at the conclusion of the article.
3. The net result was that those who received less prison education—because their sentences were shorter or because they were probably better risks—ended up having better chances than those who received more prison education.
4. A similar phenomenon has been measured indirectly by studies that have dealt with the effect of various parole policies on recidivism rates. Where parole decisions have been liberalized so that an offender could be released with only the "reasonable assurance" of a job rather than with a definite job already developed by a parole officer (Stanton, 1963), this liberal release policy has produced no worsening of recidivism rates.
5. It will be recalled that Empey's report on the Provo program made such a finding.
6. But one of these reports, by Kawaguchi (1967), also found that an intensively supervised juvenile, by the time he finally "failed," had had more previous detentions while under supervision than a control juvenile had experienced.

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