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ABSTRACT

Some evaluations have concluded that diversion programs for juvenile offenders (programs intended as community-based alternatives to formal justice dispositions) reduce recidivism only among youths with the least serious offense histories. To investigate the relationship of offense history to program effectiveness, three diversion programs were studied using data from the national evaluation of diversion programs conducted by the Behavioral Research Institute. Offenders were randomly assigned to diversion and control groups. Data on arrest record as well as a broad range of outcome measures collected by personal interviews were used. Analyses revealed no significant relationship of offense history to program effectiveness. Reanalysis of data from earlier research revealed that these findings were insignificant or unconvincing. The success of diversion programs at reducing delinquency appears unrelated to clients' offense histories and suggests that the goals of diversion will best be met by serving offenders who would otherwise have received formal justice dispositions. (Author/JAC)

Offense History and the Effectiveness of Juvenile Diversion Programs

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Abstract

Some evaluations have concluded that diversion programs for juvenile offenders reduce recidivism only among youths with the least serious offense histories. This would place the goal of reducing recidivism at odds with goals of reducing labeling, social control, and justice system costs, all of which require a more serious clientele. The present study investigated the relation of offense history to program effectiveness for a broad range of outcomes related to delinquency. The three programs studied randomly assigned offenders to treatment and control groups. Analysis revealed no significant relation of offense history to program effectiveness. Reanalysis of data from earlier research revealed that these findings were insignificant or unconvincing. We conclude that the success of diversion programs at reducing delinquency is unrelated to clients' offense histories and that the goals of diversion will best be met by serving offenders who would otherwise have received formal justice dispositions.

Offense History and the Effectiveness of Juvenile Diversion Programs

The present study concerns the relation between clients' offense histories and the effectiveness of diversion programs for juvenile offenders. These programs are intended as community-based alternatives to formal justice dispositions, and the type of clients they serve is inextricably related to this mission. As with most social programs, diversion programs originated from a complex set of social forces rather than any single theory or rationale. Nevertheless, Palmer and Lewis (1980) have articulated five goals that summarize the hopes that were held for juvenile diversion programs: a) to avoid labeling, b) to reduce social control and coercion, c) to reduce costs, d) to reduce recidivism, and e) to provide services.

Diversion programs' success at reaching three of these goals is dependent on serving a client population that would otherwise have received formal dispositions from the justice system. The programs cannot possibly avoid the labeling, social control, and costs of justice system processing if their clients were not in jeopardy of such processing. Thus, these goals are not just compatible, but interrelated.

The goal of providing services is somewhat at odds with those three goals. The cost of services reduces potential savings, and any social service may entail some labeling and social control. The premise behind diversion programs is that the costs, labeling, and social control of community-based services will be less than services provided by the

justice system. Furthermore, youths at greater risk of formal dispositions would, presumably, have greater need for services. Thus, choosing such a client population is quite consistent with the fourth goal as well.

Findings from some evaluations of juvenile diversion have, however, raised the possibility that the goal of reducing recidivism is at odds with the others, and that is the reason for the concerns of the present paper. Quay and Love (1977) and Lipsey, Cordray, and Berger (1981) report that the programs they investigated reduced recidivism only for clients with the least prior contact with the justice system. As Lipsey et al document for their program, such clients are precisely the ones who are least likely to penetrate the justice system beyond the point of diversion. Thus, reducing recidivism would mean giving up on reducing labeling, social control, and justice system costs.

There is also evidence that contradicts those two studies. Palmer and Lewis (1980) found diversion to reduce recidivism for offenders with one prior arrest but not for those with either no prior record or at least two prior arrests. On the other hand, there was no relation at all between prior arrests and program effectiveness in a study by Lincoln, Klein, Van Dusen, and Labin (1981).

In addition to its policy implications, there is a theoretical reason to be interested in the relation between offense history and program effectiveness. A primary argument for expecting diversion to affect recidivism is

that stigma from the justice system leads to delinquent behavior that would be avoided by basing services in the community. If this reasoning is correct, diversion programs would have the most impact on offenders who are at greatest risk of being stigmatized. These would be youths whose previous delinquent involvement has not been so serious that it has already provoked stigmatizing responses, but is serious enough that the offender is at risk of formal sanctions.

The present study uses data from the national evaluation of diversion programs conducted by the Behavioral Research Institute, which offers the basis for a much more thorough examination of offense history and program effectiveness than prior efforts. First, youths were randomly assigned to diversion and control conditions, so there is a firm basis for inference about program effects. Second, the study includes programs in three cities, and the justice systems in those cities have very different criteria for case dispositions. Thus, we would be able to determine if local standards for invoking formal sanctions determine which offenders would be most helped by diversion. Third, in addition to arrest records, the data include a broad range of outcome measures collected by personal interviews. Not only does this permit broadening the assessment of outcome beyond recidivism, it also allows us to define the seriousness of prior delinquent involvement in terms of self-reported behavior as well as prior arrests.

Methods

The programs, research design, data collection, and measures are all described in detail in the final report of the national evaluation (Dunford, Osgood, & Weichselbaum, 1981), so our description here will be more limited.

Research Sites

Midwest. A department within city government operated this diversion project in cooperation with the city police department. The project included two separate service programs. The first was administered by the city and employed a case advocacy model. The police department administered the second service program. Professional social workers hired by the police department provided crisis intervention that was limited to 1 month. The second service program used a longer-term case advocacy approach. For both programs, the point of diversion was the juvenile division of the city police department.

Upper South. A private nonprofit organization housed this diversion project. The project functioned as a brokering agency, receiving referrals from juvenile court intake, interviewing youths to determine service placements, and then monitoring service delivery by a variety of community agencies. The most prevalent service was counseling, either individual or family. The next most frequent was recreation, and some clients received employment or educational assistance. Most youths placed in the assignment pool had been arrested for a single felony

theft or multiple misdemeanors.

Lower South. This diversion project was also operated by a private nonprofit organization, and it brokered services through community agencies. Unlike the other diversion projects, each client contracted for a specific amount and type of service, depending only on the service agency chosen. The project placed less emphasis on counseling than the other projects and more emphasis on recreation-oriented service.

The head of the local state attorney's office was responsible for determining which cases would be placed in the pool for random assignment. The pool was limited to first-time misdemeanants, though minor felony cases were occasionally included. It should be noted that most first-time misdemeanants were taken to court in this county.

Data Collection

At each of the four research sites, arrested youths were randomly assigned to either outright release, referral to a diversion program, or normal processing by the justice system. These youths were interviewed at the time of assignment and again twelve months later. Records were obtained of prior arrests as well as arrests up to twelve months subsequent to assignment. A total of over 1300 youths participated in the study. Sample sizes at the three sites ranged from approximately 75 to 200 cases per group. The interview included several measures of perceived stigma (feelings that friends, parents, and teachers considered one

delinquent, emotionally disturbed, or conforming), an extensive measure of self-reported delinquency, and a variety of other measures widely considered relevant to delinquency (e.g., normlessness, attitudes toward delinquency, and delinquency of peers).

The comparability of the groups that resulted from the assignment process has been thoroughly examined by analysis of measures taken at the time of assignment (Dunford et al, 1981). Results indicated that procedures at these three sites resulted in comparable groups. A fourth site was eliminated from the present analysis because it appeared that likely assignment was biased in the direction of referring the more serious cases to normal justice processing.

Results

Our analysis approach was to test for differential effectiveness by the significance of interaction term for offense history and treatment condition (release, diversion or justice processing). There were separate analyses for recidivism and the outcome measures from the interview. In each case, two measures of offense history were used.

The first measure of offense history was prior arrest. At the midwest and lower south sites, respondents were divided into three groups: those with no arrests prior to the offense that led to their participation in the study, those with one prior arrest, and those with two or more prior arrests. Because very few respondents at the lower

south site nad more than one prior arrest, they were divided into those with prior arrests and those without.

Self-reported delinquency for the six months prior to the initial interview was the basis for the second measure of offense history. The self-report method offers a means of assessing delinquent behavior that does not also reflect labeling by the justice system and goes beyond those few offenses that come to the attention of the authorities. Cases at all three sites were divided into three groups on the basis of their involvement in non-trivial and non-drug related offenses. The cutoff points for the groups were chosen to segment the entire sample into thirds, so the proportions for the individual sites varied somewhat.

Recidivism

Log-linear analysis was used to test for an interaction of offense history and treatment condition in their effects on recidivism. We defined recidivism as an arrest for a felony or misdemeanor during the 12 months following random assignment. The results of this analysis appear in Tables 1 and 2.

When offense history was defined in terms of prior arrests the interaction effects approached significance at the midwest site (X^2 = 10.37, df = 6, p = .11) and upper south site (X^2 = 7.93, df = 4, p = .09), but not the lower south site (X^2 = .01, df = 2, p>.50). While such marginal effects hardly allow strong inferences, close examination of Table 1 for possible trends is in order. As the

significance test would lead us to expect, the relation of treatment condition to recidivism at the upper south site is quite constant regardless of prior arrests.

Patterns at the other sites are more complex. At both, recidivism among those with one prior arrest is lowest for the diversion group, but there the similarity ends. For youths with no prior arrests, diversion is the most successful treatment condition at the Midwest site but the least successful condition at the upper south site. This pattern is virtually reversed for youths with two or more prior arrests.

The relative success of diversion for the group with one prior arrest is consistent with the findings of Palmer and Lewis (1980). Nevertheless, the inconsistency of the overall pattern of findings renders this similarity unconvincing. Additional evidence is needed before reaching conclusions.

The second analysis of recidivism used self-reported delinquency as a measure of offense history. The log-linear analysis revealed insignificant interactions between offense history and treatment condition at all three sites (for midwest, $X^2 = 6.41$, $df = 6$, $p = .38$; for upper south, $X^2 = .58$, $df = 4$, $p > .50$; for lower south, $X^2 = .37$, $df = 4$, $p > .50$). A review of Table 2 reveals no striking effects. In fact, the weak trends that appear are quite different from those in Table 1.

The analysis of recidivism offers only very weak

evidence of differential treatment effectiveness. There were no statistically significant interaction effects. The two cases in which interaction effects approached significance are difficult to interpret, but give some indication that diversion may be most effective for youths with one prior arrest. The opposite trend appeared in relation to prior self-reported delinquency.

We are particularly interested in the treatment effects for youths with the least prior delinquency. It was reports of special effectiveness of diversion for that group that indicated a potential conflict between the goal of reducing recidivism and other goals for diversion. Table 1 and 2 reveal that diversion was as often less successful as more successful with this class of offenders.

Other outcome measures

The interview contained a large number of measures covering self-reported delinquency, perceived stigma, and social adjustment. In order to maximize the likelihood of detecting significant effects without capitalizing on chance, analysis was limited to 13 summary measures[1] that most reliably capture the major themes of the interview. Multivariate analysis of covariance was used for significance tests since this technique provides an omnibus test for effects on an entire set of dependent variables. In order to increase the power of the analysis, we also used as covariates pretest measures of all outcome variables as well as age, sex, and ethnicity.[2]

There were no significant interactions between prior arrests and treatment condition in the multivariate analysis (midwest, $p = .42$; upper south, $p = .50$; lower south, $p = .21$). The univariate tests also offer little indication of differential effectiveness. Of the 13 dependent variables there was one for which $p < .05$ at the midwest site, one at the upper south site, and two at the lower south site. All of these cases involved different variables, and there was little similarity in the pattern of the effects.

Results concerning differential effectiveness on the basis of prior self-reported delinquency were essentially the same. None of the multivariate tests for interaction effects were significant (midwest, $p = .69$; upper south, $p = .29$; lower south, $p = .15$). Out of the 39 univariate tests involved, none were significant with $p < .05$.

By broadening the definition of program outcome to include many variables generally thought to be relevant to delinquent behavior, we obtain no additional evidence that diversion programs are more effective for one type of offender than another. For comparison to the results for recidivism, Tables 3 and 4 present the means for self-reported delinquency. Only one nominally significant finding is represented in those tables, the interaction of treatment condition and prior arrests at the upper south sites (Table 3). Any effect there seems attributable to the failure of diversion with more serious cases. The relative success of diversion for youths with one prior arrest that

was suggested in the analysis of recidivism does not appear for self-reported delinquency. Furthermore, there is no support for the contention that diversion is most successful at reducing delinquency for youths with the least serious offense history. If anything, there is particularly little difference in the effectiveness of the treatment conditions for this group.

Discussion

The present study has subjected the issue of offense history and the effectiveness of diversion programs to a much more thorough examination than any prior study and found no convincing evidence or any relation between the two. Our analysis included programs at three sites, all of which used random assignment to assure comparability of treatment groups. While other studies have defined outcomes only in terms of recidivism, we have considered a broad range of measures that bear on delinquency.

Interpreting Prior Research

A review of the three studies that report differential effectiveness is useful to reconcile our findings with theirs. For two of the studies, the reason their conclusions are at odds with ours appears to be the choice of statistical methods. We have relied on tests of interaction effects, which directly reflect the significance of variation in treatment effectiveness across classes of offenders. Lewis and Palmer (1980) and Lipsey et al (1981) reach their conclusions on the basis of testing

effectiveness separately for each class of offenders. This procedure may lead one to conclude that a treatment is effective for one group but not another when there was no significant interaction. In such a case the difference in effectiveness could be attributed to random error. This circumstance is likely when the treatment effect for the entire sample is of borderline significance, which is true of both of these studies.

A reanalysis of data presented in these two studies reveals that there was no significant relation of offense history to program effectiveness in either case. Subjecting Palmer and Lewis's data (1980, p. 95) to a log-linear analysis yielded insignificant interaction effects for non-status offenses ($X^2 = 2.02$, $df = 2$, $p = .36$) as well as for all offenses combined ($X^2 = 4.11$, $df = 2$, $p = .13$). Lipsey et al. (1981) interpret two of their analyses as indicating greater treatment effectiveness for less serious cases. Nevertheless interaction terms from log-linear analyses were insignificant for both their tie-breaking randomization design (p. 293, $X^2 = .82$, $df = 1$, $p = .36$) and their matched groups design (p. 296, $X^2 = .28$, $df = 1$, $p > .50$).

The two remaining studies of offense history and the effectiveness of diversion programs used random assignment and tested for differential treatment effectiveness by interaction terms. Thus, they should be relatively strong studies. Lincoln et al. (1981) found no differential treatment effect, but Quay and Love (1977) did. They found

their program to be much more successful with the least serious offenders. In their case these youths did not arrive at the diversion program as the result of an arrest, but by informal referrals, often from outside the justice system altogether. What is striking about Quay and Love's findings is not that recidivism among informal referrals treated by the diversion program is low (24%); a low rate would be expected for a group with no arrest history. Rather, the recidivism rate for informal referrals in the control condition is remarkably high (64%). It seems unwise to interpret this pattern of effects as a success for the diversion program.

Considering the results from the four prior studies in addition to the present study, there is only one case of a significant effect indicating that the effectiveness of diversion programs depends on clients' offense histories. We find that study less than convincing, and the pattern of results for the remaining studies varies widely. Therefore, we feel quite safe in concluding that the success of diversion programs at reducing delinquent behavior is unrelated to the offense histories of the youths they serve.

Implications of the Findings

Differential treatment effectiveness is an important issue in the evaluation of social services. It would be foolish to presume that the same program would be helpful to everyone. Nevertheless, the search for differential treatment effects must be based on sound statistical

techniques. Otherwise, there is considerable chance that conclusions will be based erroneously on random fluctuations in data.

In the case of offense history and the effectiveness of diversion programs, such conclusions had the profound implication that the goal of reducing recidivism was inconsistent with the goals of reducing labeling, social control, and justice system costs. This is a very serious matter, since research has clearly shown that the vast majority of diversion programs have sacrificed these latter goals in favor of serving a less serious, but more available, population of offenders (Klein, 1979). Our results demonstrate that this sacrifice cannot be excused by greater success at modifying delinquent behavior.

We believe that diversion programs are justifiable only if they serve a population that would otherwise receive formal dispositions from the justice system. This will not impede efforts to reduce recidivism. Indeed, we are not convinced that diversion programs are effective in this regard at all (Dunford, Osgood, and Weichselbaum, 1981). There is, however, evidence that diversion programs are less oriented to social control, coercing and stigma, and more oriented to serving clients' needs than justice processing (Osgood, Dunford, and Weichselbaum, 1982). These benefits can only be obtained if diversion programs are truly used as an alternative to the justice system.

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Footnotes

1 These measures were: self-reported delinquency, self-reported drug involvement, perceived stigma as delinquent, perceived stigma as emotionally disturbed, delinquency of peers, peers' disapproval of delinquency, parents' disapproval of delinquency, personal disapproval of delinquency, normlessness, importance of conventional goals, student-teacher relations, commitment to parents, and social isolation.

2 Prior arrests also appeared as a covariate in the analysis for which it was not an independent variable. Analyses without the covariates were also conducted, with negligible impact on the findings.

TABLE 1

THE RELATION OF TREATMENT CONDITION AND PRIOR ARRESTS TO PERCENT RECIDIVISM

TREATMENT CONDITION	PRIOR ARRESTS			
MIDWEST	NONE	ONE	TWO OR MORE	N
RELEASE	36	53	55	84
DIVERSION	29	30	63	200
JUSTICE PROCESSING	48	41	45	89
UPPER SOUTH	NONE	ONE	TWO OR MORE	N
RELEASE	19	35	48	236
DIVERSION	30	25	40	177
JUSTICE PROCESSING	15	34	27	136
LOWER SOUTH	NONE	ONE OR MORE		N
RELEASE	15	18		147
DIVERSION	18	22		182
JUSTICE PROCESSING	17	21		147

NOTE: ENTRIES ARE PERCENTAGES OF EACH GROUP WHO WERE ARRESTED FOR A NON-STATUS OFFENSE WITHIN ONE YEAR OF ASSIGNMENT TO THE TREATMENT CONDITION.

TABLE 2

THE RELATION OF TREATMENT CONDITION AND
PRIOR SELF-REPORTED DELINQUENCY TO
PERCENT RECIDIVISM

TREATMENT CONDITION	PRIOR SELF-REPORTED DELINQUENCY			
MIDWEST	LOW	MEDIUM	HIGH	N
RELEASE	44	39	56	80
DIVERSION	31	48	46	181
JUSTICE PROCESSING	39	32	58	84
UPPER SOUTH	LOW	MEDIUM	HIGH	N
RELEASE	28	25	36	234
DIVERSION	23	28	37	176
JUSTICE PROCESSING	18	20	31	116
LOWER SOUTH	LOW	MEDIUM	HIGH	N
RELEASE	11	19	19	147
DIVERSION	16	19	24	182
JUSTICE PROCESSING	14	19	24	147

NOTE: ENTRIES ARE PERCENTAGES OF EACH GROUP WHO WERE ARRESTED FOR A NON-STATUS OFFENSE WITHIN ONE YEAR OF ASSIGNMENT TO THE TREATMENT CONDITION.

TABLE 3

MEAN SELF-REPORTED DELINQUENCY BY
TREATMENT CONDITION AND PRIOR ARRESTS

TREATMENT CONDITION	PRIOR ARRESTS		
MIDWEST	NONE	ONE	TWO OR MORE	N
RELEASE	1.01	1.30	1.09	70
DIVERSION	.89	1.52	1.31	151
JUSTICE PROCESSING	1.10	1.19	1.26	71
UPPER SOUTH	NONE	ONE	TWO OR MORE	N
RELEASE	.91	1.12	.71	204
DIVERSION	.89	1.00	1.27	162
JUSTICE PROCESSING	.92	.81	.87	101
LOWER SOUTH	NONE	ONE OR MORE		N
RELEASE	.99	1.10		129
DIVERSION	.89	.94		158
JUSTICE PROCESSING	.38	1.04		117

TABLE 4

MEAN SELF-REPORTED DELINQUENCY BY
TREATMENT CONDITION AND PRIOR SELF-REPORTED DELINQUENCY

TREATMENT CONDITION	PRIOR SELF-REPORTED DELINQUENCY			
MIDWEST	LOW	MEDIUM	HIGH	N
RELEASE	.72	1.04	1.48	71
DIVERSION	.66	.91	1.57	152
JUSTICE PROCESSING	.62	1.21	1.50	72
UPPER SOUTH	LOW	MEDIUM	HIGH	N
RELEASE	.47	.77	1.40	209
DIVERSION	.47	1.07	1.24	162
JUSTICE PROCESSING	.52	.84	1.34	101
LOWER SOUTH	LOW	MEDIUM	HIGH	N
RELEASE	.59	1.15	1.66	129
DIVERSION	.63	.84	1.63	158
JUSTICE PROCESSING	.55	1.15	1.36	118