

THE IMPACT OF RESTITUTION ON RECIDIVISM OF JUVENILE
OFFENDERS: RESULTS FROM FIVE EXPERIMENTAL STUDIES

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ABSTRACT

One of the major changes in juvenile justice during the past decade has been the increased reliance on restitution as a sanction for juvenile offenders. To assess the impact of restitution on recidivism, six experiments were undertaken as part of the national evaluation of the OJJDP-funded Juvenile Restitution Initiative. This report contains the results from five of those studies.

In four of the studies, youths were randomly assigned into restitution and into traditional dispositions. On the whole, the results are quite encouraging. Youths in the restitution groups never had higher recidivism rates than those in probation or detention conditions. In two of the four studies, the juveniles in restitution clearly had fewer subsequent recontacts with the court during the two to three year followup.

Comparisons of restitution with traditional probation in Clayton County Georgia showed clear and consistent effects favoring restitution. Similarly, the results from Washington D.C. showed that the restitution cases had fewer subsequent offenses than those on probation. In Boise, Idaho the comparison was between restitution and detention. These results were inconclusive as the restitution group generally did better but the differences may have been produced by chance rather than by true program effects. In Oklahoma county, there were no differences between sole sanction restitution, restitution and probation, and traditional probation.

The study in Dane county, Wisconsin demonstrated the clear superiority of a formal restitution program over an "ad hoc" (informal) program both in terms of successful completion and recidivism rates.

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INTRODUCTION

One of the most profound changes in juvenile justice during the past decade has been the increased use of restitution as a sanction for juvenile offenders. Virtually every state now has legislation which permits the use of restitution either as a direct sanction or as a condition of probation and several states require that judges order restitution unless the youth can prove an inability to pay.

In 1976 the Office of Juvenile Justice and Delinquency Prevention (OJJDP) undertook a major initiative to encourage the use of restitution in juvenile courts and to assess the consequences of this disposition on juveniles and victims. Eight-five courts received funds under the federal program and a major national evaluation, separately funded from the National Institute of Juvenile Justice, was given the responsibility for conducting an implementation study, a process evaluation including all 85 sites, and an impact study based on field experimental designs in six locations (1).

One of the primary purposes of the experimental designs was to assess the impact of restitution on recidivism of juvenile offenders. The six experiments differed from one another in terms of the restitution program models that were used and, most importantly, they differed in relation to the comparisons that were made. In all six, however, random assignments were made to treatment and control conditions

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from a group of adjudicated delinquents.

The Experiments

One of the experiments (Boise, Idaho) was structured to provide a comparison of restitution against short-term detention. Youths randomly selected for the treatment group were required to pay monetary restitution to the victims of their crimes or, if there was no outstanding monetary loss, they were required to complete a specified number of community service hours. Juveniles assigned to the control group were sentenced to several successive weekends of detention in a local detention facility. All juveniles were on probation (in addition to their requirements regarding restitution or week-end detention).

The Washington D.C. design provided a comparison of victim-offender mediation restitution against probation for a group of serious offenders. One of the eligibility criteria in Washington, D.C. was that the youths have at least one felony conviction.

In Clayton county, Georgia (near Atlanta) the study involved a four-fold comparison of restitution (with and without a counselling component) against probation (with and without a counselling component). As in the other sites, the restitution disposition could involve either financial restitution or community service.

The study in Dane County Wisconsin (Madison) was designed to compare two different models of restitution

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(programmatic vs. ad hoc) against each other and to estimate the impact of successful vs. unsuccessful completion on recidivism rates. In the programmatic design, restitution was administered through a non-profit organization whose sole purpose was to implement, monitor, and enforce restitution orders. The "ad hoc" restitution was administered by the probation officer assigned to the case.

The fifth experiment was conducted in Oklahoma County, Oklahoma. This one differed from the others in that it included a three-fold comparison with one group being randomly selected for sole-sanction restitution. A second group was randomly selected for restitution plus probation and a third group (control) was assigned to a probation-only condition.

Ventura, California was also included in the experimental study, but the design became so confounded with "crossovers" (i.e., violations of the random assignment) that it was not possible to estimate the effect of restitution on recidivism. The problem was that more than 70 percent of the youths in the control condition also were ordered to pay restitution. Thus, even though comparisons can be made between the program and the control conditions, this did not constitute a test of restitution vs. probation. For this reason, the results from Ventura are not reported in this monograph.

REVIEW OF LITERATURE

Prior to the planned experiments conducted as part of the national juvenile restitution program evaluation, only scanty information was available about the impact of restitution on recidivism and almost no theoretical work had been undertaken concerning the potential impact of restitution on subsequent behavior of juvenile delinquents.

Empirical studies of restitution have been reported only since the late 1970s and most of these defined the effectiveness of restitution in terms of its impact on victims (Galaway and Hudson, 1978; Hudson and Galaway, 1977; Schneider, 1975; Sutton, 1976). The amount of loss returned, the number or proportion of victims provided with restitution, victim satisfaction with the outcome of the case, and victim perceptions of the fairness or "justice" of the sentence were the common performance indicators included in the early empirical studies.

The first two studies which sought to link restitution with reduced recidivism were both conducted with adult parolees after their release from prison. Heinz, Hudson and Galaway in 1976 reported that the restitution group had fewer convictions after release than a matched group of incarcerated offenders. Similar results were found by Hudson and Chesney (1978) in their two-year followup of adult offenders released from the Minnesota Restitution Center.

In a study conducted by Bonta, et. al. (1983), adult

offenders in a restitution program had higher recidivism rates than those in a control group, although the differences were not statistically significant. Both groups were housed in a community resource center and most persons in both groups were employed. The control program permitted offenders to maintain employment by serving their sentences in the community resource center. The authors point out that the restitution group was a higher-risk group than the others prior to the intervention and that this could have diminished the true impact of the program.

The first two tests of restitution's effect on recidivism of juvenile offenders were undertaken by doctoral candidates. In one of these, conducted by M. L. Wax at Washington State, juveniles were randomly assigned into one of three groups: monetary restitution (with the victim present at sentencing), community service restitution, and a control group which had no contact with victims and paid no restitution. No differences in recidivism rates were found to be statistically significant although restitution was observed to have positive effects on some of the psychological tests (Wax, 1977). The size of the sample in this study, however, was so small (36 total) that the possibility of finding an impact, even if one existed, was exceptionally low.

The second doctoral study examined recidivism rates of approximately 250 offenders in the Tulsa county juvenile restitution program (Guedalia, 1979). Variables found to be

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significantly related to reduced recidivism were victim contact and restitution orders of less than \$100. The latter, of course, could simply be a reflection of a less serious immediate offense (hence the lower amount of the restitution order).

Two recent studies of recidivism rates among juvenile delinquents sentenced to restitution reported positive effects. Cannon and Stanford (1981) found a 19 percent rearrest rate among restitution cases over a six month time period compared with a 24 percent rates for the nonrestitution groups. Hofford (1981) reported an 18 percent recidivism rate for youths in the juvenile restitution program compared with a 30 percent rate for those on regular probation.

The results from these studies are instructive although they are far from being definitive. As is the case with virtually all field research, serious methodological problems confound most of the studies making it necessary to rely more heavily on replication of findings than on any single study. With the exception of Wax's study and the adult study by Heinz, et. al., none achieved a satisfactory degree of equivalence between the comparison group and the recidivism group. In the juvenile studies, little information was provided on whether the groups were equivalent and multivariate analysis was not conducted in an attempt to hold constant other differences between the groups that could have produced different recidivism rates.

Because of the paucity of research on restitution, we proposed a wide-ranging set of propositions for potential inclusion in the national evaluation. The basic strategy was to develop tests of restitution vs. traditional dispositions (either probation or incarceration or both) in as many of the six jurisdictions as possible. Additionally, we hoped to have a variety of restitution program models to compare against the traditional dispositions: financial restitution, community service restitution, accountability-based programs, treatment/service oriented programs, victim-oriented programs, mediation programs, and so forth.

THE PROGRAMS AND THE DESIGNS

Selecting the Sites

The restitution program announcement issued by OJJDP in 1977 specified that jurisdictions interested in receiving funds through the national juvenile restitution initiative would be required to cooperate with a national evaluation and that some would become involved in an experimental study (including random assignment). The announcement also informed all applicants that they would be expected to name one person on their staff as the evaluation liaison and that they could budget a half-time position for this purpose.

The co-directors of the evaluation team read all of the proposals received by OJJDP and identified 13 as potential participants in the experimental part of the evaluation.

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The major criteria for inclusion among the final 13 were (a) a viable restitution program model which included characteristics we had identified as high priority for testing, (b) a strong management capacity, and (c) geographical and community diversity. We wanted to test a variety of program models under a variety of community conditions, including regional distinctions as well as community size. Strong management capacity, of course, is essential to the success of an experimental design as well as to programmatic success. Hence, management capacity was one of the three critical variables included in our selection of programs.

The initial 13 programs were ranked and negotiations undertaken with the first eight to determine whether a suitable experimental design could be developed. Each site was given several choices of propositions which could be tested within the framework of their program. The negotiations with the program involved selecting the eligibility criteria for the initial pool from which youths would be randomly assigned into different conditions and negotiations regarding the actual treatment and control conditions.

There were four fundamental principles followed by the evaluators in negotiating the random assignment. One was that the randomization should not violate any principles of fairness nor create any ethical problems for the program. The second was that the randomization should not create any

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"political" or public relations problems for the program. The third was that the case flow had to be sufficient to support the size of groups needed for an adequate test. One county (Adams county, Ohio) was eliminated because its case flow was too low to support the design. And the fourth was that the program model was to be a "natural" one rather than one rigidly defined by the evaluators. We did not prohibit programs from changing their method of operation during the evaluation if it became necessary for them to do so. Our strategy was to measure such changes, if they occurred, and to take them into account during the data analysis phases.

Underlying the negotiating process was the presumption that if the court agreed with the importance of testing a particular proposition, and if we permitted them to operate the program in accordance with their professional standards, then they would become a willing and cooperative partner in the experiment.

The proposition which had the highest priority for inclusion in each site was to test restitution against the "normal" court dispositions which, in most sites, involved a combination of probation and incarceration. The design which we sought to negotiate in each site would involve separate tests of restitution vs. incarceration (from an incarceration pool) and restitution vs. probation (from a probation pool).

The sites were not selected to intentionally vary the type of restitution program which would be tested against normal dispositions and then to make cross-site comparisons

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regarding the efficacy of different restitution program models. With only six to eight programs, it would not be possible to make such comparisons. Rather, each experiment was viewed as a replication of the others. We hoped to establish that different types of restitution programs (all of which arose "naturally" without rigid specifications from the evaluators) did (or did not) have positive effects on victims and offenders under several different conditions, in communities with different characteristics

In each jurisdiction we also sought to negotiate a secondary design which would permit tests of different ways to conduct or administer restitution programs. We were interested in comparing community service restitution with financial restitution; victim offender mediation sessions with the absence of this component; subsidies with nonsubsidies; sole sanction restitution with restitution plus probation; and probation-operated programs with non-profit programs.

Initial arrangements were made for seven experiments: El Paso, TX; Seattle WA; Washington, D.C.; Clayton county, GA; Oklahoma County, OK; Dane county, WI; and Ventura, CA. Of these, El Paso and Seattle were eventually dropped and Boise (Ada county, ID) was added.

The El Paso court was interested in testing diversion restitution against a control condition involving intake adjustment, as well as post-adjudication restitution vs. probation. The OJJDP guidelines, however, prohibited

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diversion cases from being included in the study. The El Paso court was not willing to continue with only the post-adjudication program on a random assignment basis as it appeared this might create a "net widening" effect. The judge feared that youths who previously would have had their cases adjusted would now be adjudicated to get them into the eligible group from which the assignments to restitution or to probation would be made. Thus, El Paso eventually dropped out of the experiment and out of the federal initiative entirely.

The Seattle program was selected because of its neighborhood-based accountability model. In 1977, accountability models were relatively rare with only three clear examples among the programs which submitted applications for the initiative. These were Seattle, Portland, Maine and Oklahoma County. Both Seattle and Portland, Maine had accountability-board models (an approach developed in Seattle) whereas Oklahoma county's accountability model was court-based but operated by the community services volunteer unit rather than by probation. From these three, Seattle and Oklahoma County were both selected.

Unfortunately, the director of the Seattle program resigned and the replacement process stretched out for more than six months. During this time, she did not wish to begin the experiment (or the project) since her predecessor would be selected at any moment. When a new director finally was

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named, he was not interested in implementing an accountability program. Building on his background, as a former drug and alcohol counsellor, he wanted to operate a counselling-based program and was not interested in helping implement the random assignment procedures with the court. We dropped Seattle from the experiment when it became clear that they were not going to implement an accountability model and when it was apparent that little assistance would be forthcoming in setting up the random assignment.

Boise, Idaho was the last site selected and was approached for possible participation after Seattle dropped out. At that time, the Boise program had been operating for several months and appeared to have developed a very workable restitution program. The intake forms being sent on the restitution cases indicated that they were using restitution in conjunction with week-end detention for many juveniles. Subsequent discussion indicated that weekend detention had become a relatively common sanction for delinquent youths.

The Boise court was approached with the idea of identifying the potential group for weekend detention and then randomly assigning some into restitution (only) and others into weekend detention (only). The caseflow was not deemed large enough to also assign some to both conditions or to develop a secondary design.

They readily agreed to the random assignment procedure and the design was implemented there within a few weeks.

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Oklahoma county was selected because they had an on-going, well-managed restitution program which relied heavily on accountability concepts and community volunteers. The court agreed to a random assignment between normal dispositions (commitment to the state for incarceration or probation) and restitution. They also agreed to a trial period of a restitution-only sanction from the same eligible pool used with the first two assignments.

Dane county was selected in order to include a restitution program operated by a nonprofit with a probation (community supervision) restitution program. We also attempted to negotiate a restitution vs. nonrestitution alternative but were not successful. A secondary design was agreed to which involved subsidies vs. nonsubsidies for community service cases. The case flow for this part of the design, however, was low and the experiment had to end before enough cases were received to provide an adequate test.

Clayton county's program included both community service and financial restitution but they also proposed a mental health counselling component. One of the most prominent issues in 1977 was whether restitution could be used alone or whether it would be effective only if combined with other programs. Hence the Clayton county court was approached with the idea of testing the marginal effect of restitution when it was combined only with probation and when it was combined with both probation and mental health counselling.

The Washington D.C. program was selected partially

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because of its intent to handle especially serious offenders but also because it was one of the few proposals received from a large, metropolitan, central-city area. In addition, the program featured victim offender mediation which was one of the restitution components identified as a high priority for including in the study.

Implementing the Designs

All 85 programs in the OJJDP initiative were required to send to the national evaluators an intake form on every case they handled. When the case was closed, they were required to send a closure form. The experimental programs were also expected to send these forms, but all other data collection tasks associated with being in the experimental part of the evaluation were financed by the evaluation grant rather than the program grant.

To carry out this responsibility, the evaluation team in Eugene, Oregon assigned "site managers" to each experimental program. Both principle investigators on the evaluation grant served as site managers as did four other experienced members of the research staff. The site managers studied the program, its proposal, and the local conditions in order to develop several potential designs to be considered by the court. After negotiating the design--and reaching a written agreement on how the random assignment was to operate--the site managers were responsible for monitoring the design and all data collection from the site.

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The evaluation team hired an on-site data coordinator at each of the experimental sites. The local restitution program director served on the recruitment committee for this position and the program (or the court) provided office space. All other costs associated with the on-site data coordinator (including equipment and supplies) were borne by the evaluation grant.

The data coordinator was responsible for identifying the eligible pool from which random assignment would be made, applying the randomization formula to the eligible group, collecting intake and closure data on the control group, and collecting interview and other data on both the treatment and control groups.

The randomization procedure varied from one program to another although in each site the procedure guaranteed that any deviations would be immediately obvious. The formulas were all based on some combination of day-of-birth, a random number starter, and a final assignment which allocated cases into groups in accordance with pre-determined proportions.

The list of numbers and their assignment was kept at IPA to insure that persons responsible for identifying the eligible pool would not have prior knowledge of the group to which the youth would be assigned if he or she were determined to be eligible. Even though most of the programs developed relatively specific and quantitative eligibility criteria, some judgement was involved in screening for eligibility. To protect against bias in the screening

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process, we collected management information system data (intake and closure forms) on all youths declared to be ineligible for the program. These data permitted a continuous monitoring of the eligibility screening. After a youth was determined to be in the eligible pool, the on-site data coordinator contacted IPA to determine which of the treatment or control groups the person was assigned to.

The random assignment formulas were not necessarily set to achieve equal number of cases in the treatment and control conditions. Instead, the proportions were established so that the program could take a sufficient number of cases to fulfill its grant obligations and to achieve as nearly an equal distribution of cases as feasible under the circumstances. In Dane county, for example, the program was permitted to take two cases for every one in the "ad hoc" condition. In Oklahoma county the two restitution conditions (sole sanction and restitution with probation) were each permitted to take more cases than were assigned to the control group. Although the unequal number of cases reduced the power of the analysis somewhat, it was very important to insure that the program had a sufficient case flow to satisfy local decision makers, grant requirements, and to maintain a reasonable cost per case.

The cost per case presented some especially difficult issues. In most instances, the random assignment was not made until after restitution recommendations had been developed. This meant the experimental programs had to

develop recommendations on many cases that eventually wound up in the control group with no restitution ordered. Thus, the costs, per case, in the experimental programs were higher than they would have been otherwise. For this reason, no cost analyses were conducted in the experimental sites.

After the designs were in place, we monitored the number of cases being received to project the length of time the experiment would need to continue in order to achieve an acceptable level of statistical power. We hoped to continue the studies until a five percent difference in recidivism prevalence (e.g., 45 percent with one or more reoffenses vs. 40 percent) would--if it occurred--have an 80 percent probability of being detected in the sample (Medler, Schneider, and Schneider, 1981; Schneider and Darcy, 1984). We also wanted the sample to be large enough so that if a difference of five percent occurred in the sample, it would be statistically significant at the five percent level.

It was apparent after the study began that the case flows would not provide enough cases to meet either of these criteria, but that differences of approximately 10 percent would be needed if we used the .05 significance level as the standard. As it turned out, the use of statistical power analysis to determine the length of time the experiments should operate was abandoned in the face of serious budget cuts and the potential demise of OJJDP. We operated the experiments as long as possible, given the drastic cut in the budget which occurred in late 1981. Generally, however,

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differences of about 10 percent in most of the sites had a .80 probability of being detected.

The Boise Design

The experimental design in Boise was the simplest among the sites and presented fewer problems in implementation and monitoring. The eligible group included all youths referred to court for adjudication on a delinquent offense, except those who were held in detention during their pre-trial period. These cases were excluded from eligibility because the youths had already experienced incarceration and thus would not represent a proper test of the restitution condition if they later were randomly assigned to it.

Restitution plans (regarding the amount and payment schedule) were developed for all of the eligible youths prior to adjudication. After the fact finding hearing, all youths for whom the charges were substantiated were then randomly designated to be placed in either the restitution group or the incarceration group. At the disposition hearing, the probation officer presented the results of the random selection. The judge was able to either follow the recommendation or give a different disposition. In Boise, the assignment was followed for ⁸⁹~~88~~ percent of those recommended for restitution and for ⁹⁷~~95~~ percent of the detention recommendations.

Because the eligibility criteria for the initial pool of juveniles required either a felony conviction or one prior

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offense, this process resulted in a relatively serious group of offenders.

Cases assigned to restitution were given either monetary or community service restitution orders (or both) and a specific time period for completing it. In most instances, the youths were required to complete some community service work (80 percent) but monetary restitution was ordered whenever there was an outstanding victim loss and approximately 40 percent of the youths in the restitution group repaid part or all of the loss.

Case management in Boise was handled by restitution counsellors. The average length of time in the restitution program was two months and the average length of time under court jurisdiction was nine months. Although these youths were technically on probation, there was little if any active supervision by probation officers.

The control group youths were incarcerated for an average of eight days. The incarceration took place in a local detention facility and usually involved being locked up for several successive weekends. After release from the local facility the youth was on probation for an average of nine months.

The Washington D.C. Design

The Washington D.C. program was developed to handle serious offenders from disadvantaged, central-city,

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neighborhoods. The central features of this program were a victim-offender mediation component and a well-developed community service restitution program.

Mediators were recruited to serve as volunteers in the program and each underwent a minimum of 60 hours training before being certified to serve as a mediator. The restitution program developed a wide variety of community service placement positions for youths in the programs but they did not develop paying job positions nor did they use subsidies. About one third of the youths paid financial restitution to their victims but all performed community service work. The amount of time to be spent in community services was determined at the mediation session.

The Washington D.C. design actually incorporated two distinct experiments.

After the presentence investigation had been completed, the probation officers recommended the youths either for incarceration or for probation. This created two distinct eligibility pools and from each of these, youths were randomly selected for restitution. One group, alternatives to incarceration, was to be compared to the incarcerated group and the other group, alternatives to probation, was to be compared to probation. Comparisons were to be made only within each of the experiments.

Shortly after the restitution program and the random assignment began, it was apparent that recommendations for incarceration (which created the eligible pool for the

incarceration part of the experiment) had suddenly declined to only a few cases per month. At that rate, it would take years to accumulated enough cases in the control and treatment conditions for this part of the experiment to have a reasonable chance of producing significant effects. Thus, the incarceration part of the experiment was terminated and we continued on only with the restitution vs. probation study.

The D.C. program rested on the premise that restitution would be effective only if juveniles accepted responsibility for their offenses and were committed to the principle of making amends to the victim. If a youth did not feel responsible for his or her behavior, then restitution was not expected to be effective. Furthermore, serious complications were expected in the mediation sessions if youths were required to participate in this part of the program. Thus, the program wished to permit the youths who had been randomly selected for victim-offender mediation to voluntarily reject their assignment in favor of probation only.

Although this aspect of the program design clearly would create serious problems with the evaluation, it was accepted since it was an integral and "natural" part of their program. A design which did not permit juveniles to choose or reject restitution would not be generalizable after the experiment was over because the program would not continue to operate with youths who had been "coerced" into the mediation process. Furthermore, we found it difficult to envision any

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victim-offender mediation program anywhere in the United States that would require youths or victims to participate. Thus, voluntary participation was essential to the program and to the generalizability of the research results.

Various inducement were attempted throughout the program period to encourage juveniles selected for restitution to actually participate in it. In particular, youths who completed restitution had their probation shortened and, on the average, served only seven months on probation compared with one year for the probation-only group. Nevertheless, there were many refusals--approximately 40 percent. An analysis of the reasons for the refusals indicated that defense lawyers were an important source of information for the youths and that they were the ones usually suggesting that the youths eschew the restitution/mediation program.

As in the other sites which had "crossovers" or other kinds of violations in the random assignment, the analysis was greatly complicated by these deviations.

The Oklahoma County Design

Oklahoma county established its first juvenile restitution program in 1974 as a victim-oriented, accountability program which handled mainly diversion cases.

When federal funds were received, the program continued its emphasis on accountability, but turned its attention to adjudicated delinquents and incorporated the sole-sanction condition as part of the experimental design.

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Consistent with the concept of offender accountability, the Oklahoma county program placed considerable emphasis on the juvenile finding work from which money could be raised to pay restitution. There were no program subsidies, for example, and only six percent of the cases involved any adjustments in the amount of restitution. Restitution counsellors provided assistance in identifying job openings in the private sector as well as with charitable institutions and public agencies, but the program did not have revolving "job slots" and did not "place" juveniles in paying positions.

Eligible cases in Oklahoma county included all adjudicated delinquents except those convicted of murder and rape for whom a monetary value could be placed on victim losses. The program case workers identified all eligible youths and the on-site data coordinator then randomly assigned these cases into one of three groups: the restitution only group; restitution and probation; and a control group which would receive whatever sanction the judge deemed appropriate, so long as it did not include restitution. For those who were to be in either of the restitution groups, program staff developed restitution recommendations which were presented to the judge along with other pre-sentencing information.

Serious problems were immediately encountered in implementing the design. The experiment and the grant seemed to increase the status of the restitution program (which was

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a separate administrative unit) vis a vis probation. This occurred at about the same time the court administrator was seeking to implement tighter control over the probation officers. The probation officers resented the case tracking requirements accompanying the juveniles in the experiment and they resented the implementation of case management and tracking supervision by the court administrator on their other cases as well.

For a period of time, almost no cases were getting the dispositions to which they had been assigned. This was due both to the fact that probation officers were not recommending the disposition into which the youth had been assigned and to the fact that the judge was not following it.

The probation officers eventually forced the resignation of the court administrator and the evaluation team had to renegotiate the design with the judge and the new administrator.

One important compromise reached in the new arrangement was that the judge would be able to sentence youths in any one of the three groups to an incarceration sanction if she deemed this to be necessary. Although this had an adverse impact on the power of the design (when the youths were left in their assigned groups for the analysis) we did not believe the effect would be harmful enough to abandon the experiment. Since the youths were randomly assigned into the three groups, each group would contain youths of approximately equal seriousness and would have an approximately equal

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proportion of their more serious offenders removed for incarceration. This type of "crossover" does not introduce bias into the final groups if the youths are left in their assigned treatment for analysis, but it does reduce the power of the design. Decisions to incarcerate were about equally likely in all three groups: nine percent of the restitution only group were committed to the state for incarceration; ten percent of the restitution plus probation group were given this disposition, and eleven percent of the control group were committed to the state.

After the renegotiation, we started the experiment over and simply discarded all of the cases received up to that time.

Juveniles were randomly assigned to their respective groups after referral from the district attorney's office. Probation officers' pre-sentence report incorporated the randomly selected assignment in their report to the judge who was then expected to sentence the youth to the assigned group unless an exception was made for incarceration. Following the disposition hearing, juveniles in the restitution groups were assigned a restitution counsellor and those who also were on probation were assigned a probation officer. A meeting was held immediately after the sentencing to discuss the restitution requirements and the implementation. Approximately half of the youths were ordered to pay monetary restitution and the others did community work service.

The Clayton County Design

The restitution experiment in Clayton county was designed to compare four distinct treatment strategies: restitution, counseling, restitution and counseling combined, and a control condition which consisted of the normal disposition which could be either probation or incarceration. All youths in the first three groups were on probation. Thus, the actual test was the marginal impact of restitution. An additional feature of this design was the ability to test the marginal impact of counseling.

Cases were randomly assigned into the four conditions through a multi-tiered process. Following adjudication, all cases were screened by probation during the pre-sentence investigation for eligibility to be considered for the experiment. Juveniles were eligible for the experiment if they were 13 or older, had been convicted of an offense with a demonstrable loss, and had not been convicted of murder, attempted murder, rape, or attempted rape. Youths also were screened out as ineligible if they had a serious drug or alcohol problem, were mentally retarded, or were emotionally disturbed. Eligible cases were then randomly assigned by the on-site data coordinator in accordance with a randomization formula.

The actual placement of the youths into the groups was done by the judge at disposition. The judge could overrule the random assignment but this was seldom done. Of the cases which were included in the study, seven percent received an

actual sentence that differed from the randomly assigned one.

The treatments associated with the four groups can be summarized as follows:

Restitution. Youths in this group were ordered at disposition to pay monetary restitution and/or to do community service restitution. Service restitution was more common, involving 60 percent of the youths. Of the 40 percent who paid monetary restitution, slightly more than half found their own jobs and the rest obtained employment through the efforts of the restitution program.

The youths kept some of their earnings--on the average, about 40 percent. There were no program subsidies in Clayton county and youths generally were not permitted to pay the restitution from their savings or to have family members assist in the payment. Restitution cases were monitored by restitution case workers who also were responsible for insuring their compliance with normal probation requirements. The average period of supervision was 3.5 months.

Counseling. Juveniles with a counseling disposition were assigned to a mental health therapist on the county social service staff. The counseling consisted of a diagnostic session followed by assignment to one of several special kinds of therapy: recreational, family, and so forth. The probation requirements for these youths were, at first, handled by the restitution program (to increase the equivalence with the restitution group) but this practice had to be abandoned after the first year of operation because the

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program did not have sufficient staff to monitor the cases. Thereafter, probation monitored the probationary requirements. The average supervision period was 5.6 months.

Restitution and Counseling. For this group, both restitution and mental health therapy were ordered at disposition. Restitution and probation requirements were handled by the restitution counsellors. The restitution requirements were quite similar to those for the restitution-only group: 63 percent were ordered to do community service and 44 percent had monetary restitution requirements. Families were not permitted to pay and most of the youths found employment in private or public sector jobs. These youths were under supervision for an average of 5.8 months.

Control. Any court-approved disposition was considered appropriate for this group and most were placed on probation (78 percent). Only five percent were incarcerated and the remainder either received some other disposition or were dismissed with no sanction.

The Dane County Design

The study in Dane county differed from all of the others in that it was a test of restitution, administered in an "ad hoc" fashion by the Department of Social Services (a state-funded but county-operated agency which also handles Wisconsin's equivalent of probation, referred to as community supervision) against a programmatic approach, administered by a non-profit agency under contract with the court.

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The rationale underlying this choice of design was that restitution almost certainly was going to become a significant dispositional alternative in juvenile courts, regardless of whether it was shown to be effective in reducing recidivism, because of its impact on victims and its overall attractiveness to the professionals in the field. One of the issues that was certain to arise, however, was how restitution should be administered and what the impact would be on youths if judges began ordering restitution without putting into place some type of program to administer this new disposition.

Prior to the development of the Youth Restitution Program in 1977, the Dane County juvenile court had often ordered restitution for juvenile offenders. Youths who were ordered to make restitution, however, were handled in virtually the same way as youths for whom no restitution requirements had been imposed. Following the disposition hearing, juveniles were assigned to the Dane County Department of Social Services and a case worker was appointed to the youth. The case workers almost never assisted the youths in locating employment and, generally, they acted as if restitution was not part of the rehabilitative plan for the youth.

Judges reportedly were often reluctant to order restitution, or to order the full amount, since the state code permitted restitution to be required only if the youth was able to pay.

The "ad hoc" part of the design was intended to resemble the restitution program which had existed in Dane county before 1977. The restitution orders would be handled by DSS case workers but their priority, within the overall framework of the treatment plan for the youth, would remain quite low--just as it had in the past. Rather than shift the focus and philosophy from rehabilitation, based on services and "treatment," to accountability or victim compensation, the youths in the ad hoc program would experience the normal probationary requirements of curfew, school attendance, family counselling and so forth.

The Youth Restitution Program was operated by a non-profit corporation under contract with the county court. It provided assistance to the juveniles in locating paying job slots, arranged for community service work (when this had been ordered by the court) and developed a job-training program to help juveniles learn how to apply for a job, know what an employer expects of them, and so on.

Subsidies also were available from program funds. A youth who could not obtain a paying job with an existing private or public sector employer, could be put to work by program personnel and then paid from the subsidy fund.

Juveniles who had been ordered to pay restitution were randomly assigned either to the ad hoc group or to the programmatic group. The random assignment occurred after the youths had been ordered to pay restitution and all juveniles for whom a restitution order was issued were considered

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eligible for assignment. Because the randomization occurred after disposition, there were virtually no violations of the assignment. Juveniles assigned to the Youth Restitution Program (YRP) also were on probation and were assigned a case worker at the Dane County Department of Social Services.

The DSS case workers were not told which of the youths on their case load were control group cases nor were they told which cases were being assisted by the restitution program. They could, however, have gained the latter information simply by asking the juveniles. Although every effort was made to insure that the study was inobtrusive (case workers, for example, did not fill out any extra forms, were not interviewed by the evaluators until the study was complete, and so on) they undoubtedly were aware that the study was underway.

We were quite concerned that they might make special efforts with juveniles in the control group to insure that they completed their restitution orders, rather than continuing to handle the restitution as an added (and generally unwanted) disposition. To determine whether this problem was occurring, we obtained time series data on the completion rates of cases handled by DSS. This analysis, as well as the results of the experiment itself (which show much higher completion rates for the experimental group) indicate that DSS case workers continued to make few efforts regarding restitution and continued to treat it as an added and generally unwanted disposition.

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Dane county judges initially were reluctant to operate the random assignment in the manner desired by the evaluators because they thought they would be able to order more restitution, for more juveniles, if they knew the youths would have the services of the restitution project to help them make the payments. Thus, they wanted to know in advance which youths were targetted for the Youth Restitution Program. This, of course, could have jeopardized the study (since it was clear the judges believed they could order higher amounts of restitution if the youths were going into the program).

A compromise was reached during the negotiations about the random assignment in which the judges agreed to assume that all the youths would be assigned to the restitution program and that they could order the full amount of restitution that they thought the youth--with the help of the program--could fulfill. In return, the evaluators agreed that the judges would be able to reduce the amount of restitution at a review hearing held three months or longer after disposition, for juveniles in either group. In practice, such review hearings were rarely used and only a small proportion of restitution orders were ever adjusted downward (three percent in the treatment group and five percent in the control).

As in all of the experiments, actual assignment to the ad hoc or programmatic conditions was done at IPA after the on-site evaluation specialist notified us that an eligible

FIGURE . SUMMARY OF TREATMENT AND CONTROL CONDITIONS

SITE	TREATMENT	CONTROL	RESULTS
Boise	(a) Restn. & Probation	(a) Detention & Prob	Inconclusive
Wash. D.C.	(a) Restn. & Probation	(a) Prob. or Incarcern	Restn. Better
Clayton Co.	(a) Restn. & Probation	(a) Prob. or Incarcern	Restn. Better
	(b) Restn., Prob, & Mental Health Counselling	(b) Prob. & MH Counsling	Restn Better
	(c) MHC and Probation	(c) Probation	No Diff
Okla. Co.	(a) Restn (Sole Sanctn)	(a) Probation	No Diff
	(b) Restn (Sole Sanctn)	(b) Restn & Prob	No Diff
	(c) Restn & Prob	(c) Probation	No Diff
Dane County	(a) Program Restn	(a) Ad Hoc Restn	Pro. Better

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case had been ordered to make restitution and needed assignment into control or treatment conditions.

METHODOLOGY

Four of the designs permitted the comparison of restitution with nonrestitution sanctions. In these instances, the proposition tested was whether there were any differences in the recidivism rates of juveniles required to make restitution from those in the control condition. As the reader will recall (see the summary in Figure) the control condition was probation in Washington D.C.; probation or incarceration in Oklahoma County and Clayton County Georgia; and short-term detention in Boise.

In three of the designs, tests can be made of restitution administered in different ways or in combination with other sanctions.

In Oklahoma county, the secondary test was between those who were required to make restitution as a sole sanction and those who were also on probation. In Clayton County, the impact of restitution with and without a counselling component was examined. Two propositions were tested in these designs. One was the impact of restitution, under the different conditions, on recidivism rates. The second was whether the differing conditions had an impact on successful completion of the restitution orders.

In Dane county, the comparison was between restitution

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operated as an integral part of the youth's sanction by a non-profit organization vs. "ad hoc" restitution administered by DSS caseworkers. One proposition tested in this design was the effect on successful completion rates. The second proposition was whether successful completion had an impact on recidivism rates. We anticipated--given the history of the pre-1977 restitution program in Dane County--that there would be a high rate of incompletions among youths in the "ad hoc" program. We also expected that those in the programmatic approach would have very high successful completion rates. Hence, we expected these programs to produce differences in completion rates that potentially could be linked to differences in recidivism.

Measuring Successful Completion

Successful completion was measured in several different ways. One of these was the program decision on whether the youth had completed the restitution successfully. The criterion recommended by the evaluators, and used by most of the programs in the federal initiative, was that the youth had completed successfully if he or she had paid 95 percent or more of the restitution ordered by the court. If community service work had been ordered, successful completion was defined as finishing 95 percent or more of the hours ordered.

Other definitions of successful completion were used in the initial phases of data analysis, but since these

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did not yield any differences in conclusions, they are not reported here. Among the other definitions examined were the proportion of the restitution ordered actually paid, the percentage of youths who paid 100 percent of the restitution order, and the percentage of youths who paid all of the outstanding victim loss.

Measuring Recidivism

Recidivism was defined as crimes committed after entry into the treatment or control conditions which resulted in contact with the county juvenile or adult court, except incidents which were dismissed due to a lack of evidence or those for which the youth was found not guilty. Crimes that were committed after the immediate offense but before entry into the program were counted as "concurrent" incidents and were not included in the analysis.

A complete search of all juvenile and adult court records was undertaken by a team of trained individuals from the national evaluation group. The followup period varied from 22 to 36 months, depending on when the youths entered the program and when the final official records check was conducted.

Multiple measures of recidivism were used to incorporate both the seriousness and frequency of reoffending as well as to minimize possible misinterpretations based on single-indicator analysis. The measures used were:

Prevalence. Prevalence refers to the percentage of

juveniles in each group who committed a subsequent offense which resulted in a referral to adult or juvenile court during the followup period. Excluded from this figure were any recontacts for which the records definitely established that the case was dismissed for lack of evidence or the youth was found not guilty.

Annual Offense Rate. The annual offense (contact) rate was calculated by summing all of the recontacts for the group, dividing by the time at risk (in days), and then correcting to an average annual rate, per youth. Both the pre and post offense rates were calculated.

Recontact Frequency. Recontact frequency was used as an individual-level measure of overall recidivism in the regression analyses. Recontact frequency refers to the total number of recontacts for each youth. This is a badly skewed variable and several transformations were tested in the multiple regression analysis. Although very few differences were noted in the results, the most stable measure was a natural log transformation and this was employed in the analyses reported here.

Recontact Rate. This is also an individual-level measure used in the multiple regression analysis. It was calculated by dividing the total number of offenses for each youth by the total time at risk, thereby creating an individual-level "rate" of recontact. Juveniles with no reoffenses had differing follow-up periods, however, because they entered the programs at different points in time. A

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simple rate involves dividing zero reoffenses by the risk time which, of course, produces a score of zero regardless of whether the youth had six months of time at risk or four years. To distinguish among the non-recidivists so that those with longer periods of time at risk have lower scores, a small constant (.01) was added to the numerator of this measure.

Seriousness Indices. Three variables representing seriousness were used in the initial data analysis. One of these was an ordinally-coded variable representing the most serious offense committed by the juvenile. Violent personal offenses were coded "6" followed by serious property offenses "5", other felony property offenses "4", minor personal offenses "3", minor property offenses "2", and trivial offenses, "1".

The second variable representing seriousness was created by scoring each reoffense in terms of its seriousness and then summing these to obtain an overall measure of frequency and seriousness of reoffenses. The final variable was a seriousness rate in which the overall score for each youth was divided by the amount of time at risk thereby taking into account that youths with longer follow-up periods would be expected to have more reoffenses.

Because these three measures yielded almost identical results and because these results were similar to those produced by the frequency variables, only the last seriousness measure is included in this report.

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In all instances, the rate of reoffending (frequency divided by time at risk) was actually an adjusted rate in which a small constant (.01) was added to the numerator so that the scores of persons who had no recidivist offenses would be scaled in terms of their time at risk.

Establishing Causality

Juveniles were randomly assigned into the program and control conditions in all five sites. In an ideal experiment, the random assignment alone would be sufficient to insure that the statistical measure of program impact was not confounded with other variables. Field experiments, however, seldom meet the rigid requirements of experimental conditions and the ones in this evaluation were no exception.

The major problem was with crossovers--cases which were assigned to one condition but which ended up in the other. In most of the sites, there crossovers constituted fewer than five percent of the total, but in Ventura more than ⁷⁰~~25~~ percent of the cases were crossovers. Ventura was eventually excluded from the recidivism analysis and other impact evaluations because of the crossover problem and other problems with the data.

Washington D.C. also presented a problem. Approximately half of the youths randomly selected for the victim offender mediation/restitution program voluntarily decided not to participate and to accept traditional probation instead.

It is always difficult to know what to do with

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crossover cases but the analysis here follows the common recommendation which is to consider a case in the group to which it was assigned, even if the actual treatment was something different. This is generally viewed as a conservative approach due to the expectation that those who violate the random assignment do so to protect the program by "creaming". Hence, the more difficult cases are directed away from the program leaving only the easier ones. To include all cases, as originally assigned, protects against "creaming."

In the analysis reported below, however, we also took the precaution of testing all the regression models with the cases in the actual, rather than assigned, groups. The results of these analyses in sites with small numbers of crossovers were no different than the results when the cases were kept in their original groups and are not reported. However, in Washington D.C., the crossover group (juveniles who voluntarily selected probation rather than restitution) were such a sizable group that the entire analysis is reported.

For the analysis, bivariate regressions were conducted and, to insure that potentially confounding effects did not interfere with the interpretation, multiple regression analysis also was undertaken in which priors, age, race, and sex, were controlled. In all of the regression tests, the independent variable is the treatment/control condition with restitution being scored "zero" and control scored "one."

FINDINGS

The results from each of the experiments are summarized in this portion of the report. (Complete information on other analyses conducted, but not reported here, is available in other papers which can be obtained from the author of this report)

Boise

Table 1 contains summary data for juveniles in the restitution and the incarceration groups in Boise. There were no statistically significant differences in the background characteristics of youths in the two groups. Most were full-time students (81 and 85 percent, respectively); the average age was just over 15 years, and more than 80 percent were males.

The population from which the random assignment was made included primarily serious offenders. In the restitution group, 66 percent were repeat offenders and 80 percent were repeaters in the incarceration group. This difference, which appears to indicate that the incarceration group contained somewhat more serious offenders than the restitution group, is offset by the fact that 41 percent of the restitution youths entered the program as a result of a felony conviction compared with 32 percent of the incarceration group.

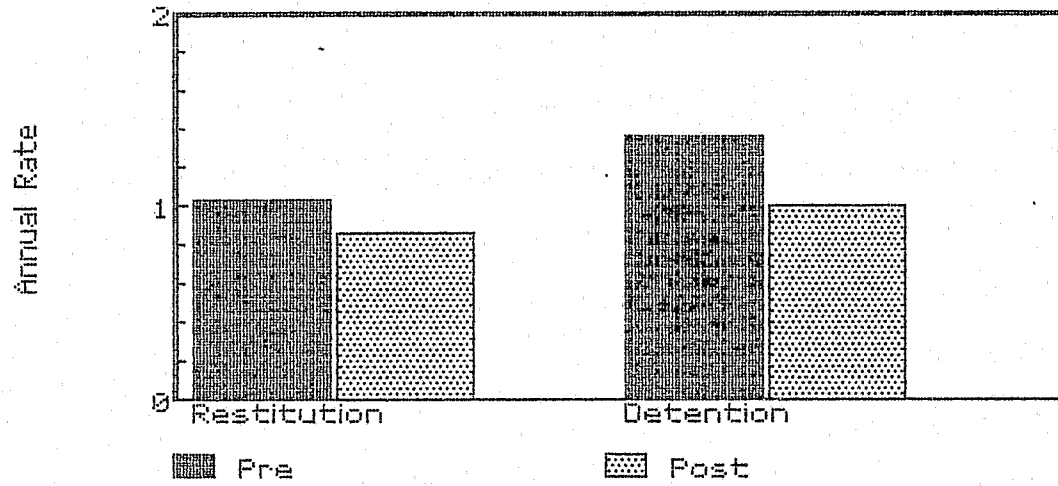
The recidivism analysis suggests that the restitution group did somewhat better (i.e., had fewer reoffenses)

TABLE 1. BOISE, IDAHO: SUMMARY STATISTICS

Characteristic	Restitution	Incarceration
NO. OF CASES	86	95
CHARACTERISTICS OF REFERRALS		
% Full-Time Student	81%	85%
% Minority	5	1
% Male	86	84
% Repeat Offenders	66	80
Avg Age	15.0	15.3
REFERRAL OFFENSE		
% Felonies	41	32
% Misdemeanors	59	67
RECIDIVISM		
Months of Followup	22	22
% With Recontacts	53	59
Annual Offense Rate: Pre Program	1.03	1.37
Annual Offense Rate: Post Program	.86	1.00
Recontact Frequency (beta wt)	.06 n.s.	
Recontact Rate (beta wt)	.02 n.s.	
Seriousness Rate (beta wt)	.04 n.s.	

Positive beta weights indicate the restitution group did better (i.e., had a lower recidivism rate). The observed significance levels were .24, .29, and .25 respectively, on a two-tailed test. Variables controlled in the multiple regression analysis were priors, age, race, sex, and (for recontact frequency) time at risk. A complete set of tables can be found in Griffith, 1983.

BOISE: ANNUAL RECIDIVISM RATES



X Data	Pre	Post
Restitution	1.03	0.86
Detention	1.37	1

but the differences are not very large and they may have been produced by chance. The observed significance levels in most of the analyses, although favoring the restitution group, were between .25 and .30.

Specifically, in the 22 months of followup, 53 percent of the restitution group had one or more subsequent contacts with the court compared with 59 percent of the incarceration group. The post-program annual rate of subsequent contacts was .86 for the restitution group compared with 1.0 for the incarceration group. Differences of this magnitude would be expected, by chance alone, approximately one-fourth to one-third of the time. The multiple regression analysis indicated a difference of about the same magnitude, but it too would occur by chance about 25 to 30 percent of the time. In the multiple regression, priors, age, race, school status, and sex were statistically controlled.

The pre/post comparison shows that the intervention may have slowed the annual offense rate for both groups. The average number of offenses committed, per youth per year, dropped from 1.03 for the restitution group to .86. For the incarceration group, the annual pre-program rate dropped from 1.37 to 1.0.

An examination of the recidivism rates for each of several different types of crimes showed that the restitution group did somewhat better than the incarceration youths, but the differences were not great enough to be statistically significant at the .05 level. For the six types of offenses

examined, the restitution group had fewer recidivists and lower recidivism rates within each.

On balance, the Boise experiment indicates the restitution youths did just as well as those who were incarcerated and that there was a relatively good probability (about two out of three) that the participation in the restitution program actually yielded a slightly lower recidivism rate.

Washington D.C.

The Washington D.C. referrals were among the most serious of all restitution programs participating in the federal initiative. More than 60 percent were repeat offenders and for approximately 60 percent, the immediate incident which resulted in their referral was a felony. (See Table 2). These youths were predominately black (99 percent) and male (95 percent).

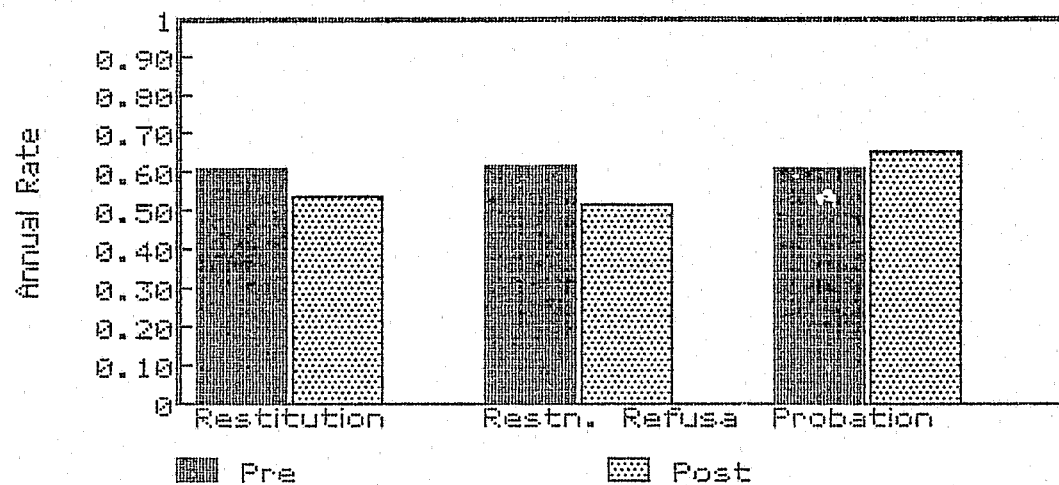
Table 2 shows the characteristics of the restitution group, the restitution refused (crossover) group, and the control group. Of the latter, seven percent actually were incarcerated and the others were on probation. There were no statistically significant differences between the restitution and control groups in terms of background characteristics. This held true when the crossovers were grouped with the restitution youths and when they were kept separate. It should be noted, however, that the crossovers contained substantially more females (13 percent) than either of the

TABLE 2. WASHINGTON, D.C.: SUMMARY STATISTICS

Characteristic	Restitution	Restitution Refused (Crossovers)	Probation (Control)
NO. OF CASES	143	131	137
CHARACTERISTICS OF REFERRALS			
% Full-Time Student	75%	72%	72%
% Minority	99	98	99
% Male	97	87	91
% Repeat Offenders	63	65	61
Avg. Age	15.4	15.5	15.6
REFERRAL OFFENSE			
% Felonies	65	57	59
% Misdemeanors	35	43	41
RECIDIVISM			
Months of Followup	32	31	31
% With Recontacts	53	55	63
Annual Offense Rate: Pre Program	.61	.62	.61
Annual Offense Rate: Post Program	.54	.52	.65
Recontact Frequency (beta weight)		.10*	
Recontact Rate (beta weight)		.10*	
Seriousness Rate (beta weight)		.05 (p=.12)	

Positive beta weights indicate the restitution group did better (i.e., had a lower recidivism rate). The differences on the frequency and recontact rate were statistically significant beyond the .05 level whereas the difference on the seriousness rate had an observed significance level of .12 on a two-tailed test. Variables controlled in the multiple regression analysis were priors, age, race, sex, and (for recontact frequency) time at risk. A complete set of tables can be found in Griffith, 1983.

WASHINGTON D.C.: ANNUAL RECIDIVISM RATES



X Data	Pre	Post
Restitution	0.61	0.54
Restn. Refusals	0.62	0.52
Probation	0.61	0.65

other two groups.

The results of the recidivism analysis are interesting, but perplexing. The restitution group had reoffense prevalence rates considerably below the probation group (53% to 63%)--a statistically significant difference--and annual reoffense rates similarly lower than the control group (.54 to .65). The multiple regression analysis also shows that the restitution youths had lower overall contact (both frequency and rates) after controlling for the number of priors, age, race, school status and sex.

As mentioned previously, substantial treatment contamination occurred in the randomization, with about half of the randomly assigned restitution youth receiving probation instead of restitution. With this issue in mind, three additional sets of multiple regression analyses were conducted. In each instance, different evaluation groups were included or excluded creating different treatment and comparison groups. In each case, the independent (treatment) variable was dichotomous (scored as zero for the control condition and one for the treatment). These results are not presented, but they show the following:

Restitution vs. Restitution Refused (Crossovers). This comparison indicates that the restitution group had slightly fewer recidivist offenses but only for felony reoffenses were the differences statistically significant.

Restitution vs. Control. In this analysis, the crossovers were omitted entirely and the restitution youths

had significantly lower recidivism rates on almost all measures.

Restitution vs. Probation. The crossovers actually received probation and in this analyses were grouped with the probation youths who were randomly assigned. The restitution group had somewhat better scores on most of the indicators but the differences were only marginally significant (e.g., in the .15 to .25 range).

Three major findings stand out. First, youths who were randomly assigned into restitution--whether they actually participated in it or not--had lower recidivism rates than youths randomly assigned probation. Second, those who actually participated in restitution generally had lower recidivism levels than those in probation. And, third, those who participated in restitution never had higher rates than those who participated in probation.

Why then did youth who were randomly selected for restitution but refused (the crossovers) have lower recidivism rates than those on probation? Both actually participated in probation. Differences in the background characteristics do not appear to account for these differences.

Our speculation is that youths selected for restitution were given realistic choices and involvement in the determination of their disposition which the probation youth did not have. All juveniles selected for restitution were presented with two choices of roughly equivalent severity

(probation or restitution) and they were allowed some involvement in the process of determining their disposition. It is possible that this choice component allowed them to select a treatment somewhat better suited to their individual interests and motivations and thus one which was more efficacious in impacting their future behaviors.

Clayton County

Clayton county youths were overwhelmingly white (as is the population in that suburban area near Atlanta), approximately 15 years of age, and predominately male (see Table 3). Between one-fourth and one-half the youths were referred for felony offenses and most of the others had been involved in misdemeanor property crimes.

The restitution-only group was compared with the restitution-counselling group to determine whether the latter produced any improvement in the successful completion rates. As shown in Table 3, the completion rates were very high for both groups and the small difference observed (86% for restitution-only vs. 82% for restitution-counselling) was not statistically significant.

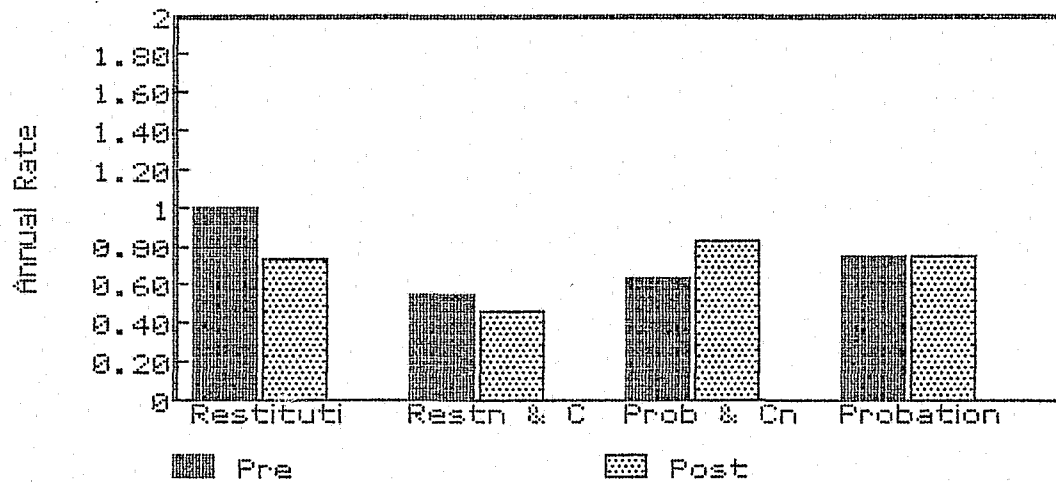
Both restitution groups were somewhat less likely to commit subsequent offenses resulting in court contacts during the three-year followup period as 49 percent of the restitution-only group and 46 percent of the restitution-counselling group were again referred to court. These figures compare with a 60 percent and 52 percent recidivism

TABLE 3. CLAYTON, COUNTY: SUMMARY STATISTICS

Characteristic	Restitution		Probation	
	Restn. Only	Restn. & Counsl.	Prob. & Counsl	Prob. Only
NO. OF CASES	73	74	55	55
CHARACTERISTICS OF REFERRALS				
% Full-Time Student	76%	82%	75%	86%
% Minority	6	4	4	0
% Male	86	80	78	80
% Repeat Offenders	60	43	56	54
Avg Age	15.1	15.2	15.1	15.2
REFERRAL OFFENSE				
% Felonies	24	40	49	30
% Misdemeanors	76	60	51	70
RECIDIVISM				
Months of Followup	35	35	36	37
% With Recontacts	49	46	60	52
Pre-Program Annual Offense Rate	1.01	.55	.64	.75
Post-Program Annual Offense Rate	.74	.47	.84	.75
Recontact Freqn. (beta wt)			.11*	
Recontact Rate (beta wt)			.13*	
Seriousness Rate (beta wt)			.09 (p = .16)	
SUCCESSFUL COMPLETION				
% Successful	86	82		

Positive beta weights indicate the restitution group did better (i.e., had a lower recidivism rate). Recontact frequency and recontact rate were significant beyond the .05 level on a two-tailed test. Variables controlled in the multiple regression analysis were priors, age, race, sex, and (for recontact frequency) time at risk. A complete set of tables can be found in Schneider and Schneider, 1984a.

CLAYTON COUNTY: ANNUAL RECIDIVISM RATES



X Data	Pre	Post
Restitution	1.01	0.74
Restn & Cnsing	0.55	0.47
Prob & Cnsing	0.64	0.84
Probation	0.75	0.75

rates for the two probation groups.

The post-program offense rates also show similar differences with the restitution groups having lower annual offense rates.

The multiple regression analyses, controlling for number of priors, age, school status, race, and sex indicate an effect with a high likelihood of being produced by the program (significance levels of .07, .04 and .16 on a two-tailed test). In these tests, the two restitution groups were compared with the two probation groups.

The group reoffense rates before and after the intervention indicate some interesting differences. For the restitution-only group, the pre-program rate was 1.01 offenses. After the intervention, this dropped to .74. Drops in the post-intervention rates of similar magnitudes were not observed for any of the other groups. The restitution and counselling group had a pre-program rate of .55 which dropped only to .47 afterward. The probation and counselling group actually showed an increase from .64 to .84 and the probation only group showed no change (.75 to .75). Although change in offense rates from pre to post could be impacted by maturation of the youths, the maturation effects should be about the same across all four groups. Hence, the differences in the pre/post rates are of considerable interest.

A second multiple regression analysis was undertaken to determine whether counselling had an impact on recidivism

when contrasted with the non-counselling dispositions. For this analysis, both counselling groups were compared with the non-counselling alternatives and no significant differences were found. A final comparison was between the restitution-only condition and restitution-counselling. Again, no significant differences were found.

The results from Clayton county show that youths required to make restitution to their victims either through community service or monetary payments had lower recidivism rates than those given the more traditional juvenile court dispositions. These results show that restitution works quite well on its own and that it does not need to be combined with mental health counselling.

Oklahoma County

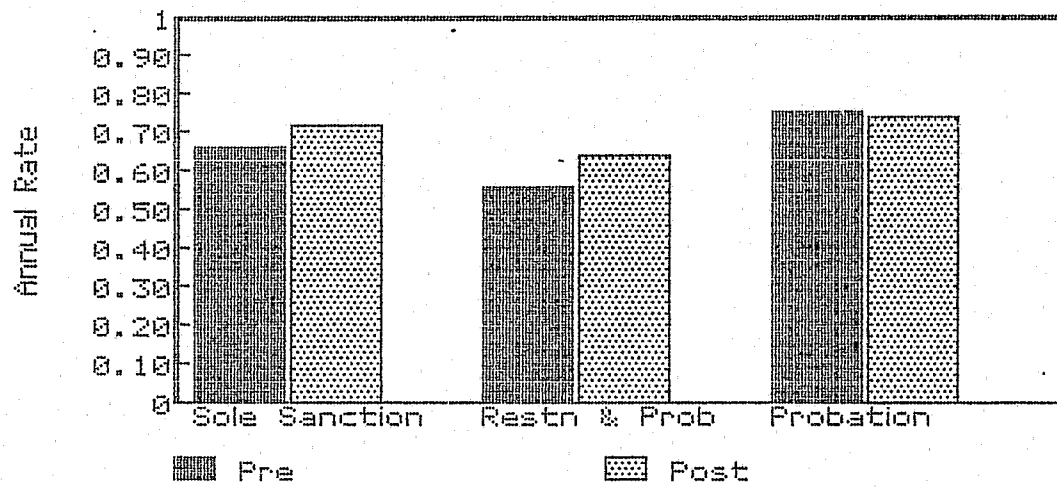
The 298 youths in the Oklahoma county experiment were divided rather evenly among the three groups with 104 in the sole sanction restitution group, 116 in restitution and probation, and 78 in the non-restitution control (see Table 4). Most of the juveniles were relatively serious offenders as 59 percent of the restitution youths had one or more priors and 68 percent of the control youths had a prior record of court contact. Many of the youths entered the experiment as a result of felony convictions: 50 percent of the sole sanction group, 48 percent of the restitution and probation group and 40 percent of the probation only controls.

TABLE 4. OKLAHOMA, COUNTY: SUMMARY STATISTICS

Characteristic	Restitution Sole Sanction	Restn. & Probation	Probation
NO. OF CASES	104	116	78
CHARACTERISTICS OF REFERRALS			
% Full-Time Student	65	69	72
% Minority	34	46	32
% Male	91	87	85
% Repeat Offenders	59	59	68
Avg Age	15.2	15.4	15.5
REFERRAL OFFENSE			
% Felonies	50	48	40
% Misdemeanors	50	52	60
RECIDIVISM			
Months of Followup	23	24	24
% With Recontacts	49	50	52
Pre-Program Annual Offense Rate	.66	.56	.75
Post-Program Annual Offense Rate	.72	.64	.74
Recontact Freqn. (beta wt)			.02 n.s.
Recontact Rate (beta wt)			.01 n.s.
Seriousness Rate (beta wt)			.01 n.s.
SUCCESSFUL COMPLETION			
% Successful	82	88	

Positive beta weights indicate the restitution group did better (i.e., had a lower recidivism rate). The observed significance levels were greater than .80. Variables controlled in the multiple regression analysis were priors, age, race, sex, and (for recontact frequency) time at risk. A complete set of tables can be found in Schneider and Schneider, 1984c.

OKLAHOMA CO.: ANNUAL RECIDIVISM RATES



X Data	Pre	Post
Sole Sanction	0.66	0.72
Restn & Prob	0.56	0.64
Probation	0.75	0.74

On most of the indicators, the juveniles who also were on probation did slightly better in terms of completing their restitution requirements, but the differences were small and may have been produced by chance (see Table 4). In terms of the program's definition of successful completion, 88 percent of the youths who were both on restitution and on probation completed successfully compared with 82 percent of the sole sanction group. There were no differences in the groups in terms of the proportion of the restitution order repaid (92 percent) although slightly more of the probation group paid all of the restitution ordered (91 percent). The sole sanction group was more successful in terms of repaying all of the outstanding victim loss (58 percent compared with 36 percent). The groups were basically alike in terms of their completion of community service hours.

The groups did not differ on recontact with the court, regardless of the method of measuring recidivism or the type of analysis undertaken. On the average, a juvenile in the sole sanction group committed 1.4 offenses per year compared with 1.3 for the restitution and probation group and 1.5 for the controls. These differences are very small and almost certainly were produced by chance variation rather than by program effects.

Comparison of the pre and post rates show that none of the interventions reduced the overall offense rates. In fact, the youths in both restitution groups tended to reoffend slightly more afterward than before whereas the

control group continued at the same rate.

To test the impact of the program on recidivism, multiple regression models were developed in which the program and control condition was the independent variable (scored 0 and 1), the various measures of recidivism were the dependent variables, and age, race, sex, priors, and school status were controlled. The results for each measure of recidivism, testing restitution as a sole sanction against restitution plus probation showed no differences.

Comparisons of both restitution groups (combined) against the nonrestitution controls also showed no effect.

In these analyses, the youths who were committed to the state were left with the group to which they had been randomly assigned. Additional tests, not shown here, were conducted with the incarcerated youths removed from all three groups. This reduced the level of recidivism but did not change the patterns which continued to show almost identical recidivism rates for the three groups.

The findings from this experiment indicate that youths who were given restitution as a sanction, without benefit of probation requirements or probation supervision, were generally as successful as those who recieved probation along with restitution. Furthermore, the results indicate that youths who recieved restitution sanctions did no better and no worse than the control group of probation youths in terms of recidivism.

These results differ slightly from previous findings

based on the full set of data from 85 juvenile restitution programs (Schneider, et al, 1982). In the earlier study, we found that sole sanction restitution requirements appeared to be correlated with higher completion rates and lower recidivism, compared with restitution imposed along with probation. It was pointed out at the time that in the absence of random assignment, a selection bias or "creaming effect" could account for some or all of the apparent relationship. In that respect, the current study is more valid because the design effectively eliminated the possibility that the sole sanction group was comprised of less serious offenders. However, one should be cautious about interpreting the findings from Oklahoma county as the conclusive answer to the question of whether restitution should be imposed as a sole sanction or as a condition of probation. This experiment, although it may have a high degree of internal validity, is only one test, in one place, and replications are needed before final conclusions are drawn.

With regard to the other issue--whether restitution is a more effective sanction than probation--the results in Oklahoma county suggest that recidivism rates were about the same for both groups and that neither type of intervention effectively altered the offense rate of juvenile delinquents.

Dane County

Most of the youths in the "ad hoc" and programmatic

TABLE 5. DANE, COUNTY: SUMMARY STATISTICS I

Characteristic	Programmatic	"Ad Hoc"
NO. OF CASES	165	86
CHARACTERISTICS OF REFERRALS		
% Full-Time		
Student	71%	73%
% Minority	4	10
% Male	84	87
% Repeat Offenders	68	71
Avg Age	16.3	16.3
REFERRAL OFFENSE		
% Felonies	56	52
% Misdemeanors	44	48
SUCCESSFUL COMPLETION		
% Successful	91	45
% Paying All Restn. Ordered	88	40

Additional tables can be found in Schneider and Schneider, 1984b.

conditions were male, fulltime students, white, 16 years of age or older with at least one prior court contact (Table 5). Almost half of the youths in each group had two or more prior contacts. These characteristics indicate that the youths for whom restitution was ordered in Dane county tended toward the more serious offenders. Half of the youths in the programmatic group had been convicted of felony property offenses and 43 percent of the youths in the ad hoc group had been convicted of felonies.

The data clearly establish the superiority of a programmatic approach over an ad hoc approach in terms of successful completion rates (see Table 5). According to the criteria used by the programs and the court, 91 percent of the youths in the programmatic group successfully completed their orders compared with 45 percent of the youths in the ad hoc group.

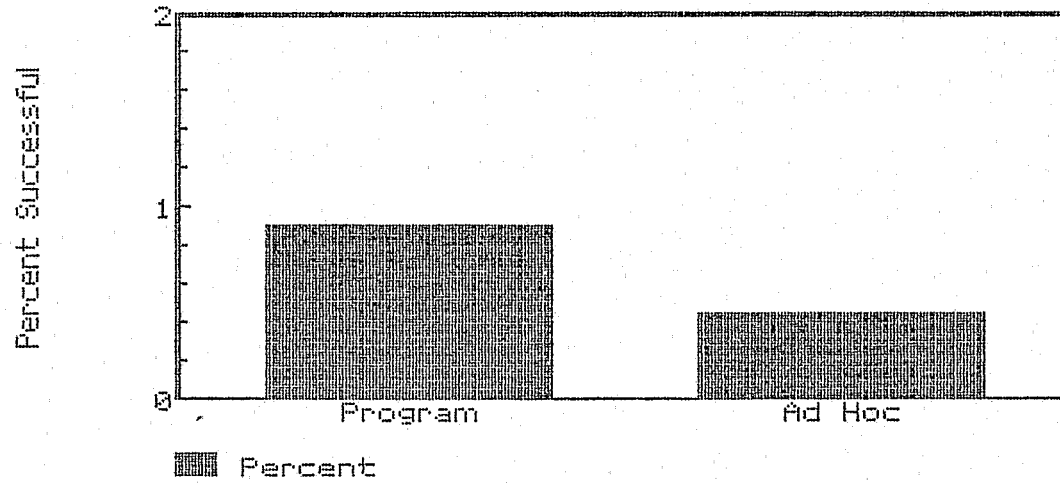
Using the less judgemental standard in which successful completion is defined as paying all of the restitution ordered by the court (or completing all of the community service hours) the data show that 88 percent of the youths in the programmatic group would be judged successful compared with 40 percent in the ad hoc group. Similarly, if a very lenient standard were used (i.e., paying at least some of the restitution) the programmatic approach is considerably better as only two percent of their juveniles failed to pay at least some restitution whereas 37 percent of the juveniles in the ad hoc program did not pay even the first dollar of their

TABLE 6. DANE, COUNTY: SUMMARY STATISTICS II

Characteristic	Unsuccessful	Successful
NO. OF CASES	61	190
RECIDIVISM		
Months of Followup	36	37
% With Recontacts	80	60
Pre-Program Annual Offense Rate	1.40	1.22
Post-Program Annual Offense Rate	1.04	.72
Recontact Freqn. (beta wt)		.08*
Recontact Rate (beta wt)		.10*
Seriousness Rate (beta wt)		.11*

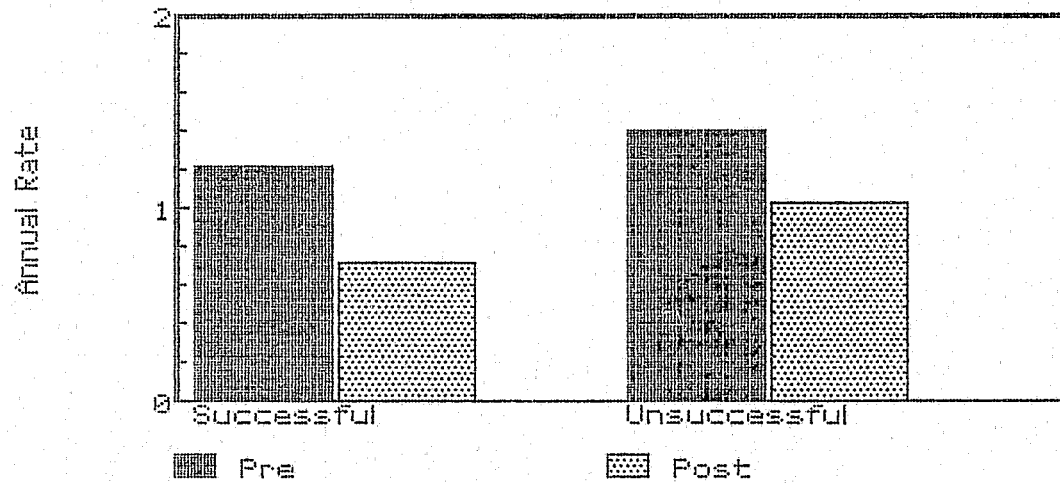
Positive beta weights indicate the successful group did better (i.e., had a lower recidivism rate). The recidivism beta weights were significant beyond the .05 level on a two-tailed test. Variables controlled in the multiple regression analysis were priors, age, race, sex, and (for recontact frequency) time at risk. A complete set of tables can be found in Schneider and Schneider, 1984b.

DANE CO: SUCCESSFUL COMPLETION RATES



X Data	Percent
Program	0.91
Ad Hoc	0.45

DANE CO: ANNUAL RECIDIVISM RATE



higher rates of reoffending before the intervention took place. This underscores the importance of controlling for priors in the multiple regression analysis. The second observation is that the reoffending rate for the successful group dropped tremendously--much more than for the unsuccessful group--after the intervention.

A similar pattern emerges from the multiple regression analysis in which successful vs. unsuccessful completion was used as an independent variable, controlling for age, sex, race, number of priors, and school status.

Successful completion apparently had an impact on recidivism rates independently of other variables as it showed significant effects with all the measures of recidivism used (see Table 5).

The findings from this experiment show, first, that a programmatic approach to restitution in Dane county, Wisconsin clearly increased the likelihood of juveniles repaying victims and increased the amount of restitution paid to victims, in comparison with an ad hoc approach. The implication is that successful completion of restitution is more likely to occur when a greater importance is attached to the restitution requirement and when the juvenile is given additional incentives to comply with this part of the court order.

The second conclusion is that successful completion makes a difference in terms of the likelihood of reoffending and that this effect probably is independent of other

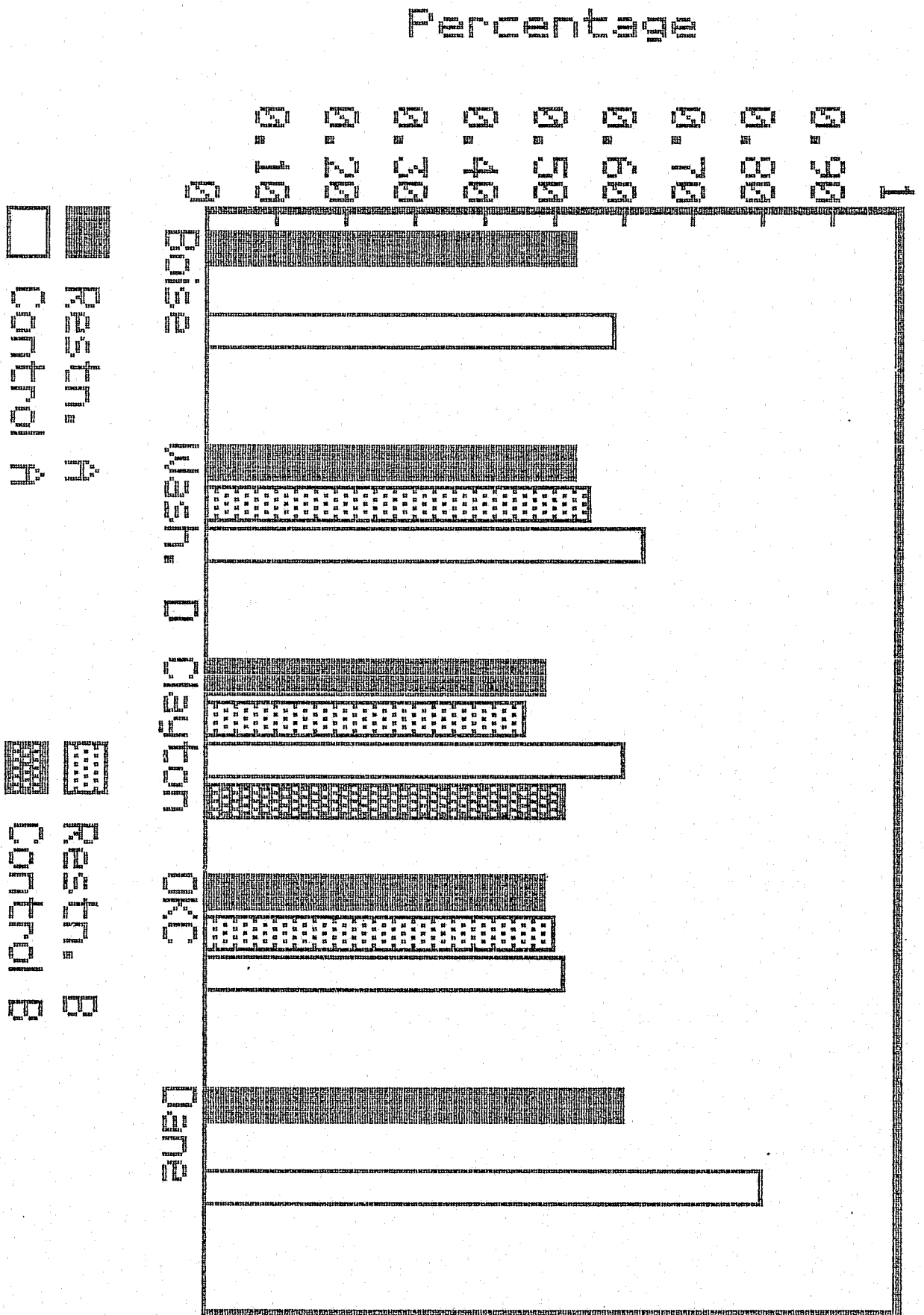
factors. The implication here is that there may be some justification to the notion that courts which implement restitution without simultaneously implementing a program which will place high priority on successful completion (for the purposes of holding youths accountable and repaying victims) may miss the opportunity to have a positive effect on recidivism rates.

The results of this study, however, should not be interpreted to indicate that probation departments cannot or should not operate restitution programs. Too many examples exist of probation departments which effectively transformed themselves into restitution programs to foreclose this possibility. Rather, the critical point is that restitution will be more effective if it is not treated as an "ad hoc" stepchild to the traditional probationary requirements which emphasized curfew, associations, school attendance, and counselling, but, instead, is viewed as an integral part of the juvenile court's approach to delinquency.

CONCLUSIONS AND IMPLICATIONS

The results from the experiments regarding the effect of restitution on recidivism should be viewed as quite encouraging. In two of the four direct comparisons, approximately 10 percent fewer of the restitution group juveniles were recontacted during the followup. An annualized measure of recontact indicated that the

PROPORTION RECIDIVATING



restitution program cases produced almost 10 fewer crimes, per 100 youths, per year, than the controls in these two programs (Washington, D.C. and Clayton county Georgia).. The differences in these two sites were great enough to rule out chance variation as a likely cause of the apparent program effect.

In one other jurisdiction--Boise, Idaho--the program youths did better on both measures of recidivism by six percentage points and an annual rate differential of 14 incidents, per 100 youths, per year. The smaller sample size, however, prevented these differences from achieving statistical significance at the .05 level. It must be noted, that differences of the size observed in Boise have about a one in three probability of being produced by chance and a two in three probability of being produced by true program effects. Thus, there is a fairly reasonable likelihood that these differences also reflect the superiority of restitution over more traditional dispositions.

The study in Oklahoma county revealed no differences among the three groups of sufficient size to merit policy considerations.

These results should not be viewed as inconclusive or as contradictory. Rather, the lesson here is that restitution can have a positive effect on recidivism, but it does not necessarily have this impact under all circumstances.

The results from the Dane county experiment show that successful completion of restitution requirements is crucial

to the avoidance of subsequent offenses. This study further indicated that probation departments which do not have the resources or motivation to implement restitution orders may produce exceptionally low successful completion rates which, in turn, adversely impact recidivism.

The reasons for the success of restitution in reducing recidivism--in those instances when it was successful--remain a matter of speculation and theory. As with any effective intervention, it is reasonable to assume that the intervention must have an impact on one or more variables which influence delinquency. And, since the restitution intervention was directed primarily at the juvenile (rather than his or her parents, friends, or neighborhood) it is reasonable to believe that the effect is transmitted through changes in the juvenile's perceptions or attitudes which, in turn, alter behavior.

Many possible variables might be cited:

1. Positive experiences in "real job" situations which not only provide a positive adult role model but which also instill a sense of confidence that the youth can be successful in non-delinquent situations.
2. Restitution may have a less stigmatizing effect on the youth since it offers the juvenile the potential for "paying the debt" and for being "redeemed" for the offense.
3. The youth may have a more realistic understanding of the actual consequences of crime for victims and for the community as a result of being "held accountable" for

delinquent acts.

4. Restitution is believed by many program managers to break down the post-offense rationalization (e.g., the victim deserved it) and force the offender to confront the true consequences of the crime.

5. It is possible that restitution has a deterrent effect (in the sense of increasing the perception that crimes have consequences which result in costs to the offender).

6. Restitution usually involves a relatively intensive supervision since most of the youths are spending a substantial portion of their free time at work. This may break down the relationship between the youth and other delinquent peers during the supervisory period and perhaps beyond.

Future research needs to focus on the linkages between restitution, attitudes, and subsequent behavior in order to identify how restitution operates to reduce delinquency when, in fact, it has this effect.

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