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NOTES ON METHODOLOGICAL ISSUES IN
CRIMINAL JUSTICE SANCTIONS

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Introduction

The Methodological Notes

The following methodological notes were compiled during the coding of experimental studies for the meta-analysis. Therefore, some experiments in the Registry of Randomized Criminal Justice Experiments in Sanctions are not represented here due to the lack of information available.

Contents of the Notes

- A. Flowcharts are presented to highlight what subjects were eligible for the experiment, when randomization took place, and the types of conditions to which subjects were assigned.
- B. Each chart is followed by methodological notes compiled from the published reports. These notes contain study-specific research design concerns and have been consistently updated upon second and third readings.
- C. Citations for the experimental reports and reviews of these studies are set out for each experiment in the Registry of Randomized Criminal Justice Experiments in Sanctions, September 1990.

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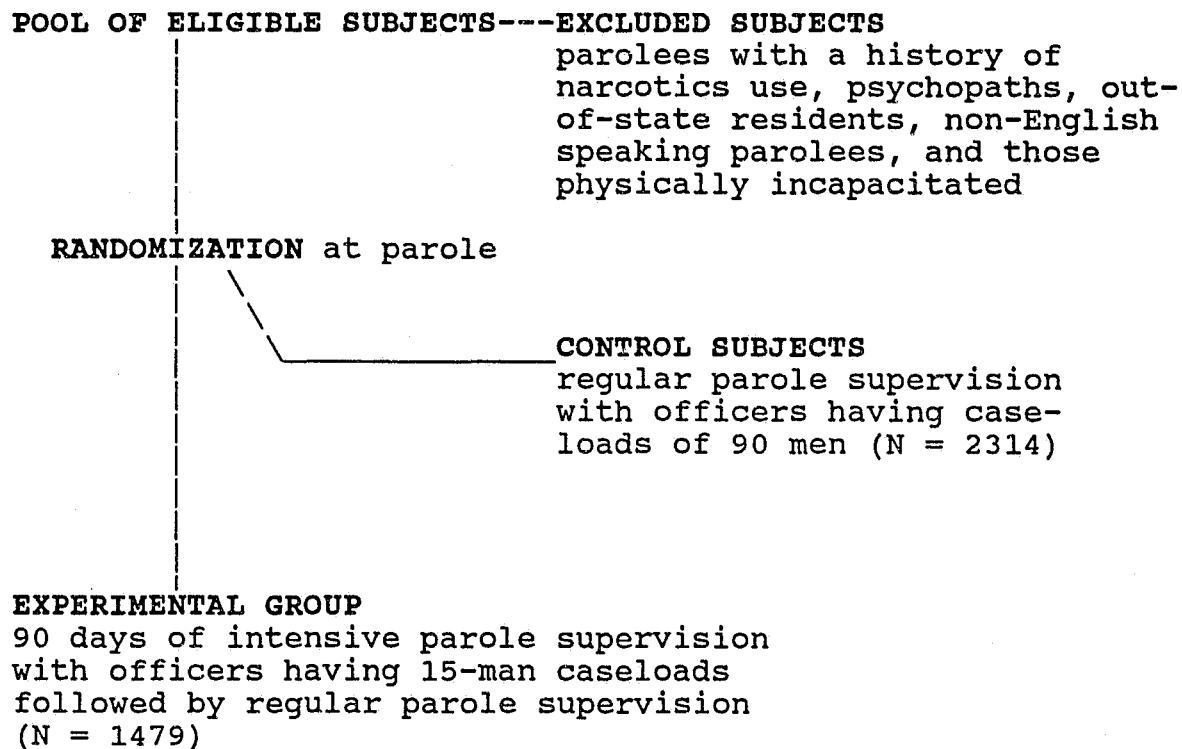
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"A"
Experiments

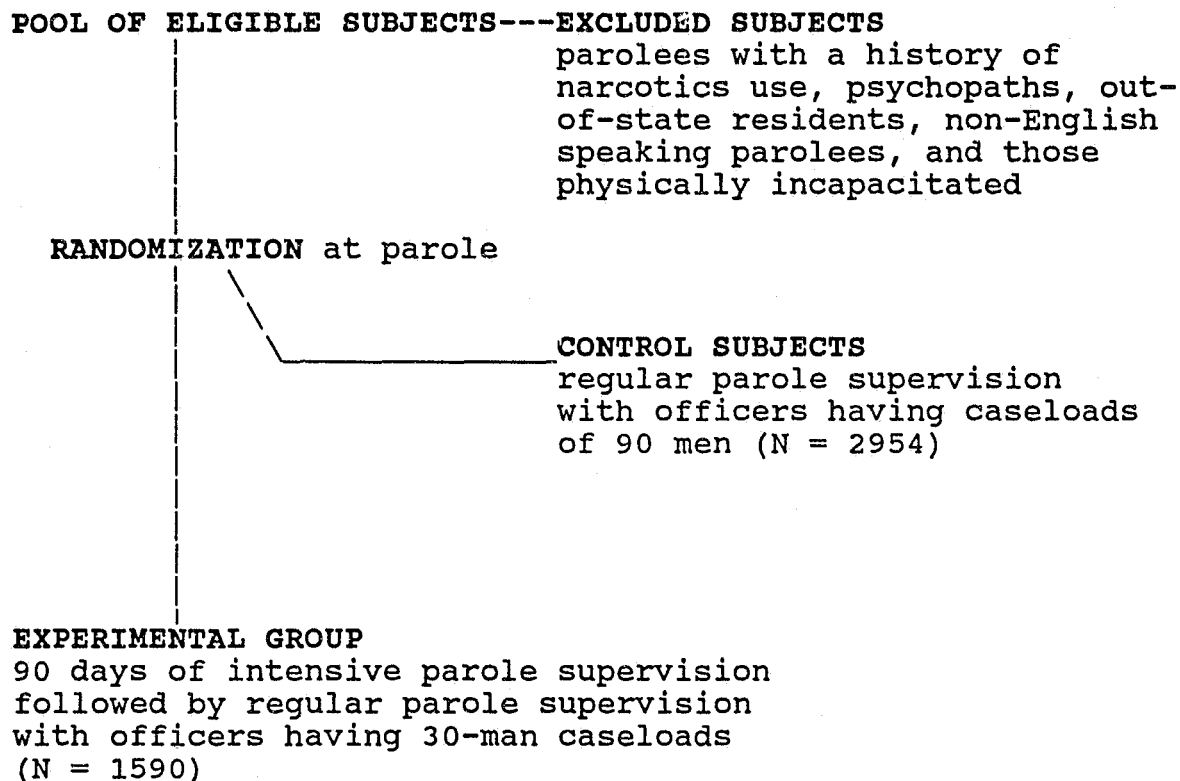
(1953) California Special Intensive Parole Experiment - Phase I - A



Notes

One problem which was noted later in the experimental report was the control group's simulation of the experimental condition -- control parole officers also increased their contacts with parolees during the experimental period. This was offered as a possible reason for the lack of significant effect for the intervention in the last 13 months of the experiment. The PIs stressed the danger of "contamination" within field experiments which administer the experimental and control conditions in close proximity to each other.

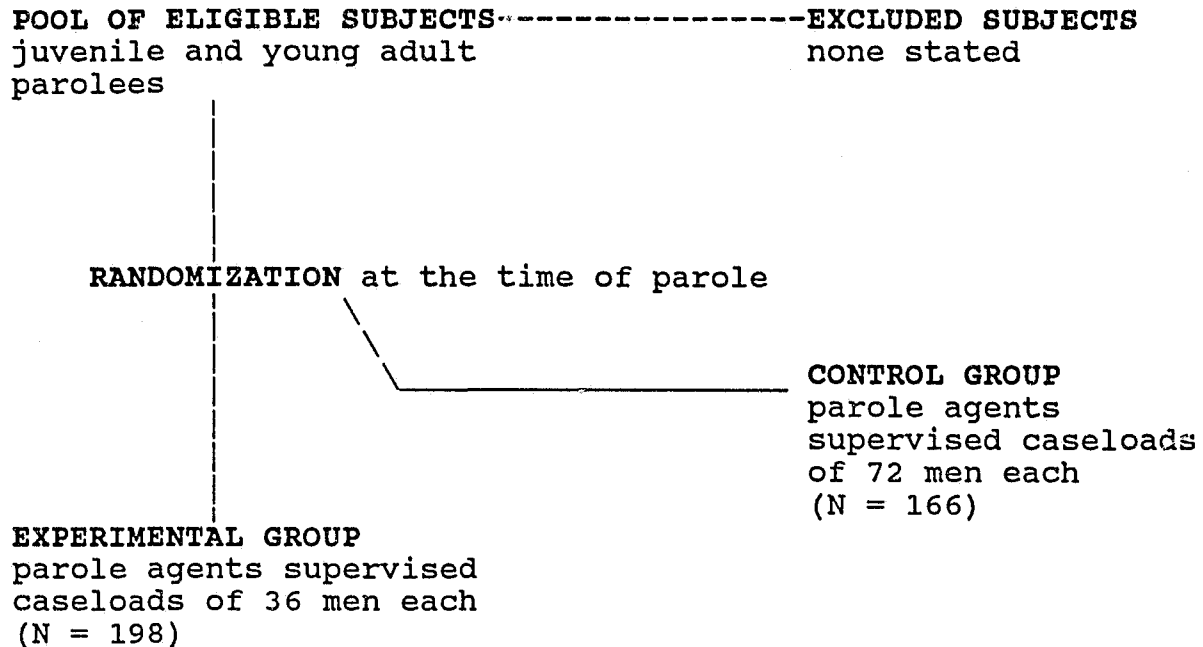
(1956) California Special Intensive Parole Experiment - Phase II - A



Notes

Field experiments which require adjustments in personnel assignment must maintain satisfactory caseflow to insure that the expense in maintaining a program can be justified. In this experiment, randomization was violated at certain times -- with all subjects sent to the experimental condition -- to keep caseload levels at their predetermined numbers.

(1959) California Parole Research Project Experiment - A

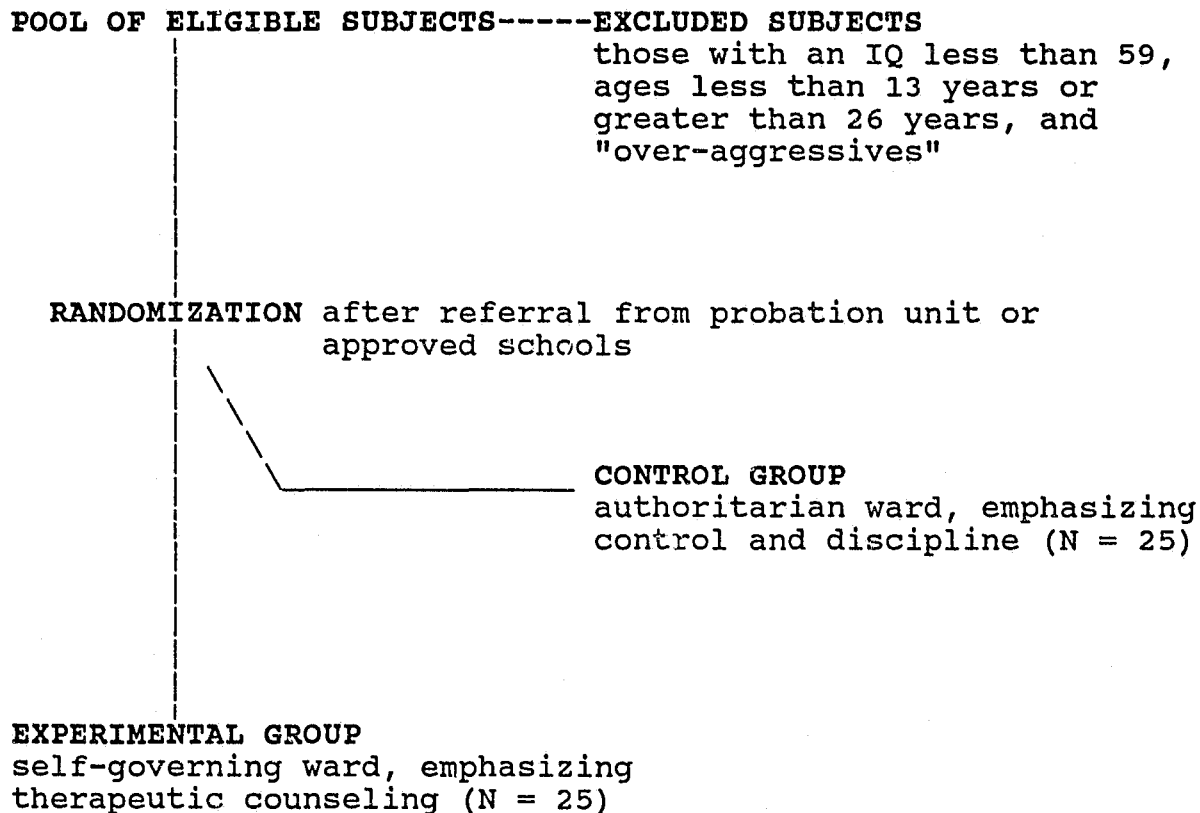


Notes

Differential effects were noted by the PI. First commitments (those on parole for the first time) did significantly better in the intensive group than those with prior parole commitments.

The "strength and integrity" of the supervision was also noted as a factor affecting the outcome variable; as the level of contacts increased, so did the experimental subjects' chances for parole success.

(1959) English Psychopathic Delinquent Experiment - A



Notes

This experiment highlights a significant conflict for criminal justice experiments -- the quest for knowledge versus ethical considerations. Most experiments lack any way to gauge the baseline effectiveness of study conditions. The PIs stated that to know if the wards were actually helping or hurting the psychopathic delinquents was not possible unless the "courts agree[d] to assign eligible youths to a no treatment condition" (Craft et al., 1964: 553). In effect, the authoritarian ward was statistically better on post-release measures than the self-governed ward, but one cannot know if exposure to the authoritarian ward was better than doing absolutely nothing (which ethical considerations prevented these PIs from examining).

Utah Provo Experiment (cont.)

If the dropouts were excluded from the final analysis, one would no longer be confident that these two groups were equivalent. Perhaps there was some common, underlying factor which accounts for the failure of these subjects. The conservative approach would be to analyze the subjects as assigned and count them as failures if they do not meet the success (outcome) criteria -- even though they may not have received the full intervention or stayed for the entire exposure period.

(1961) California Juvenile CTP Phase I Experiments - A
- (includes two experiments) **

Sacramento-Stockton

POOL OF ELIGIBLE SUBJECTS---EXCLUDED SUBJECTS

cases where parole was opposed
by local law enforcement

RANDOMIZATION during institutionalization

CONTROL GROUP

regular institution and parole
supervision (N = 305)

EXPERIMENTAL GROUP

Community Treatment Program featuring
intensive parole supervision after 4
weeks at a reception center (CTP
subjects were assigned according to
their maturity levels to an
appropriate agent.) (N = 396)

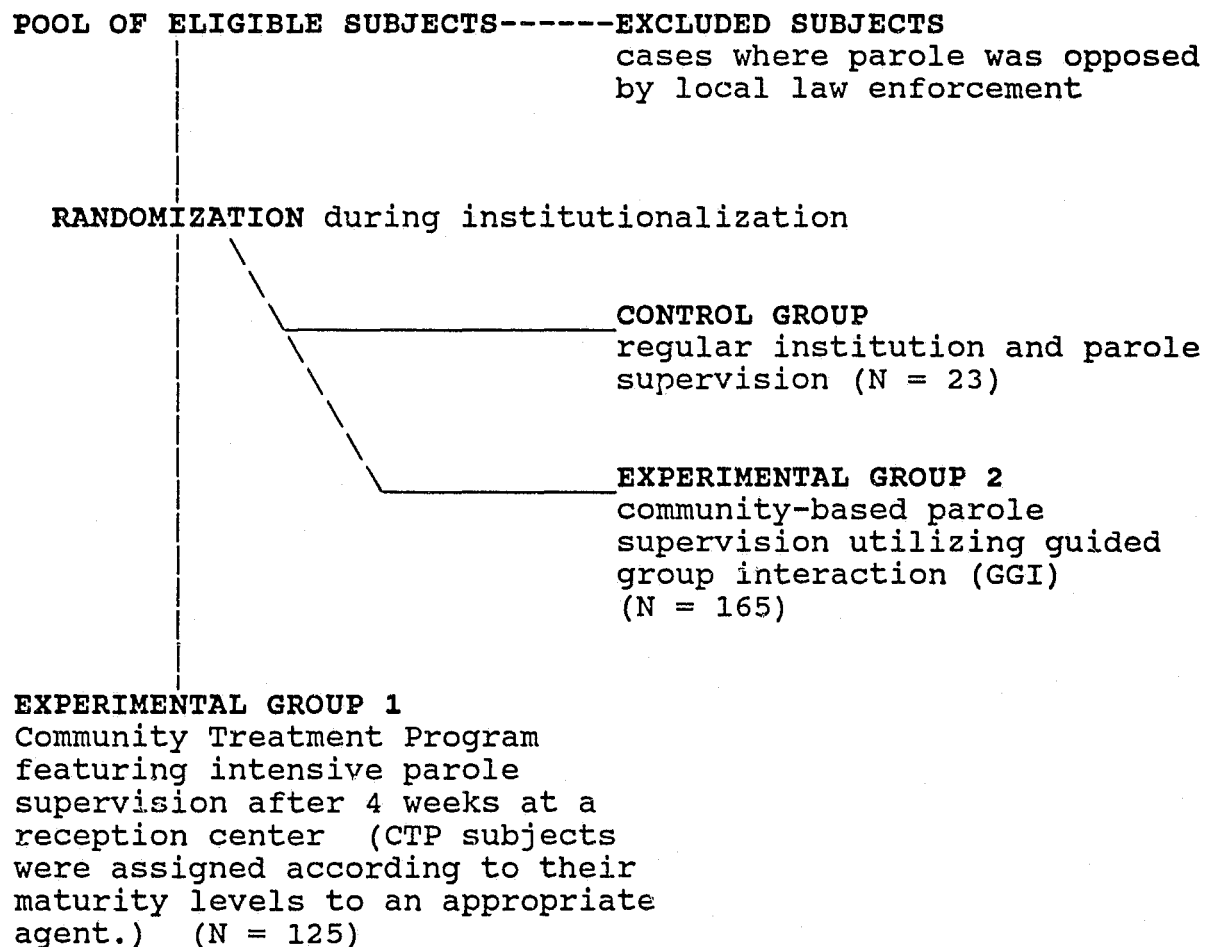
Notes

This study highlights the trouble with non-laboratory experiments. Though the groups were randomized and appeared equivalent, the experimental parole agents responded differently to their subjects than the control agents did. Lerman (1968, 1975) pointed out that the significant effect of the CTP program on parole failure rates was due to experimental parole agents overlooking offenses that normally resulted in revocation. When minor revocation offenses were controlled for, there was no difference between the groups. Farrington (1983) suggested an experiment in which the staff administering the intervention would also be randomly allocated to a group to control for this problem.

** Each of the experiments discussed here was analyzed separately in the data base ("SPSS Data Set - Experiments in Sanctions").

California Juvenile CTP Phase I Experiments (cont.)

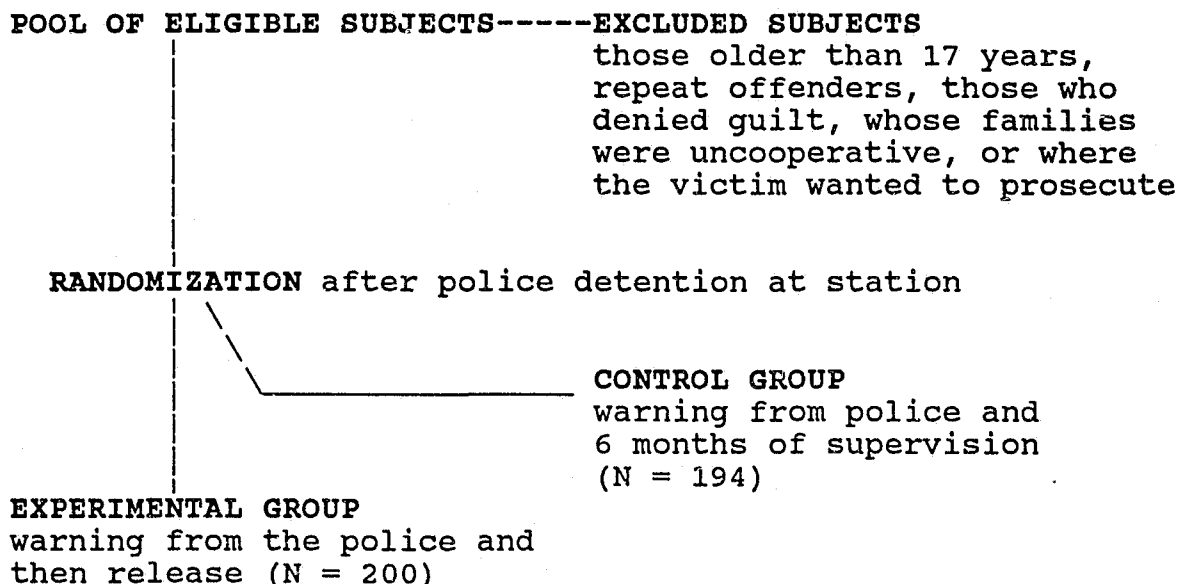
San Francisco



Notes

San Francisco was the other location for the multi-site CTP experiments. In this experiment, only 23 cases were assigned to the control group -- the harsher sanction -- indicating selection bias may have entered into the randomization process. This was not explained in the reports. There was also no explanation of GGI (Guided Group Interaction) or the nature of the supervision provided for the second experimental group.

(1963) English Police Cautioning Experiment - A



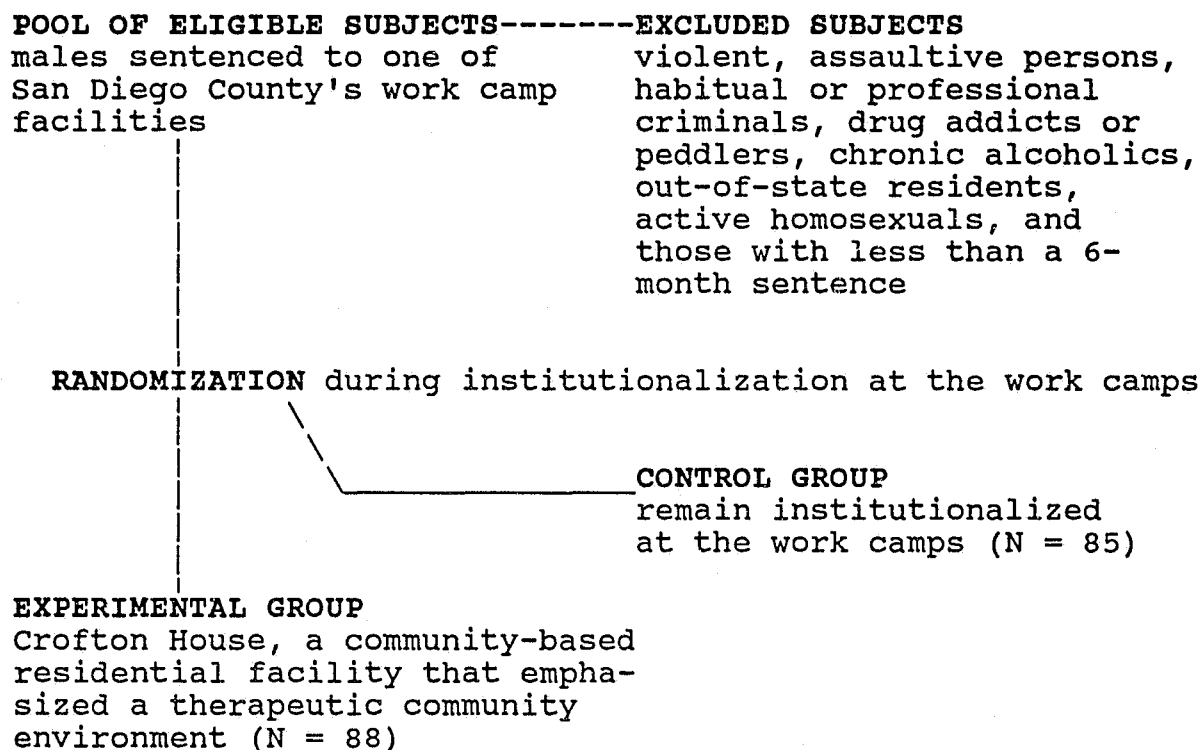
Notes

The Principal Investigators noted that the low-risk nature of the sample (all first-time minor offenders) made the probability of demonstrating a significant result more difficult -- these subjects were unlikely to re-offend regardless of the intervention imposed.

The supervision in this experiment was not observed, leaving the possibility open that the treatments given the two groups may not have been different enough to produce an effect. It is possible that more intensive supervision would have had an effect -- but that was not tested.

The attrition problem in the experiments should be noted here, since 24 experimental cases were lost in the outcome analysis. By attrition, we refer to the loss of subjects after randomization had taken place. This problem can be acute during outcome measurement -- if official records are incomplete. The conservative approach is to check that the attrition rates during outcome measurement are equivalent for the experimental and the control groups. If the loss of subjects in one of the groups was substantial, the benefits of using randomization may be nullified (cf. Provo) because the groups in the final analysis may no longer be equivalent.

(1965) California Crofton House Experiment - A



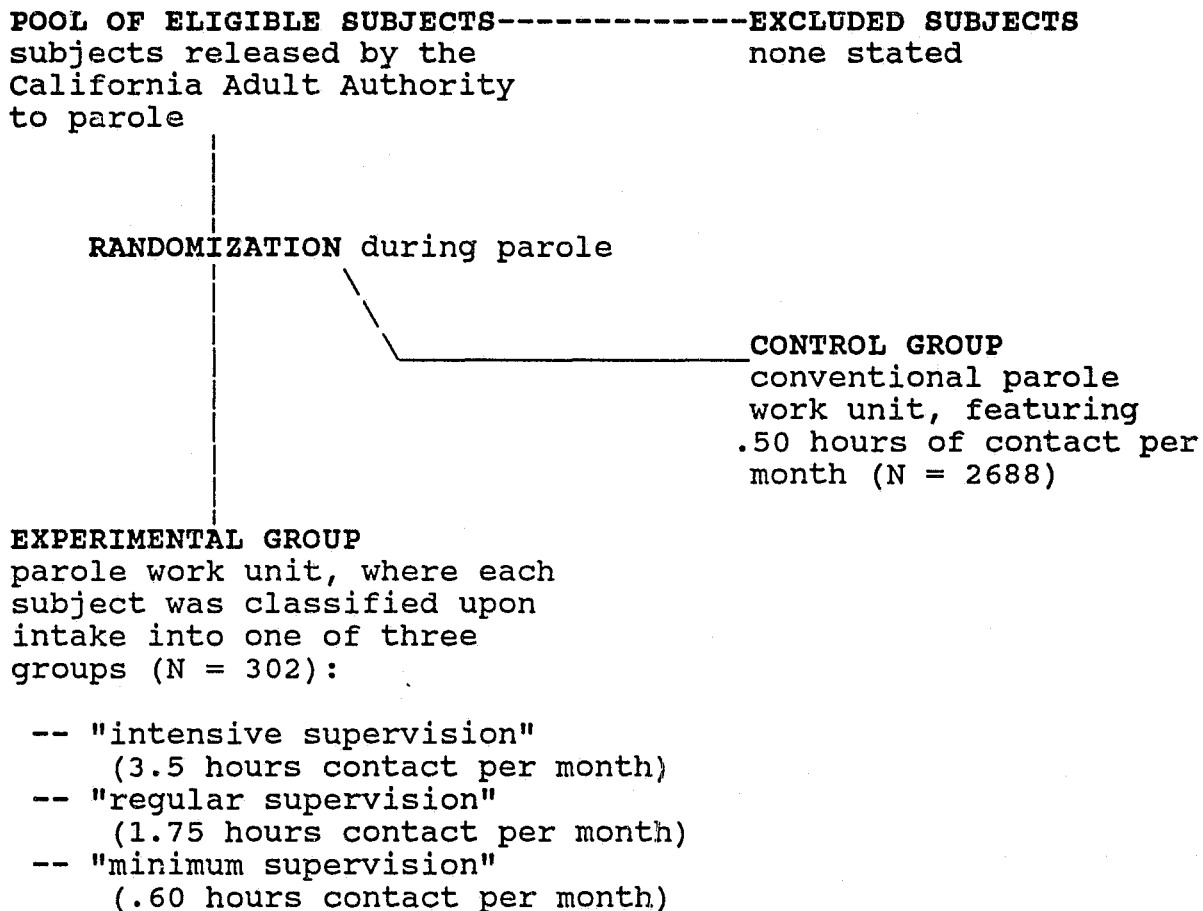
Notes

The exclusion criteria were exercised, leaving only 10% of the work camp population eligible for the study. The eligible population consisted predominantly of repetitive misdemeanant offenders.

No summary statistics were given in the report. Moreover, the outcome measure was simply stated as "post release behavior" -- no information was given to clarify whether arrest or revocation was used.

Comparison statistics between the two study groups would have been useful. Perhaps the eligible population would have improved or failed regardless of the nature of the intervention imposed.

(1965) California Parole Work Unit Experiment - A

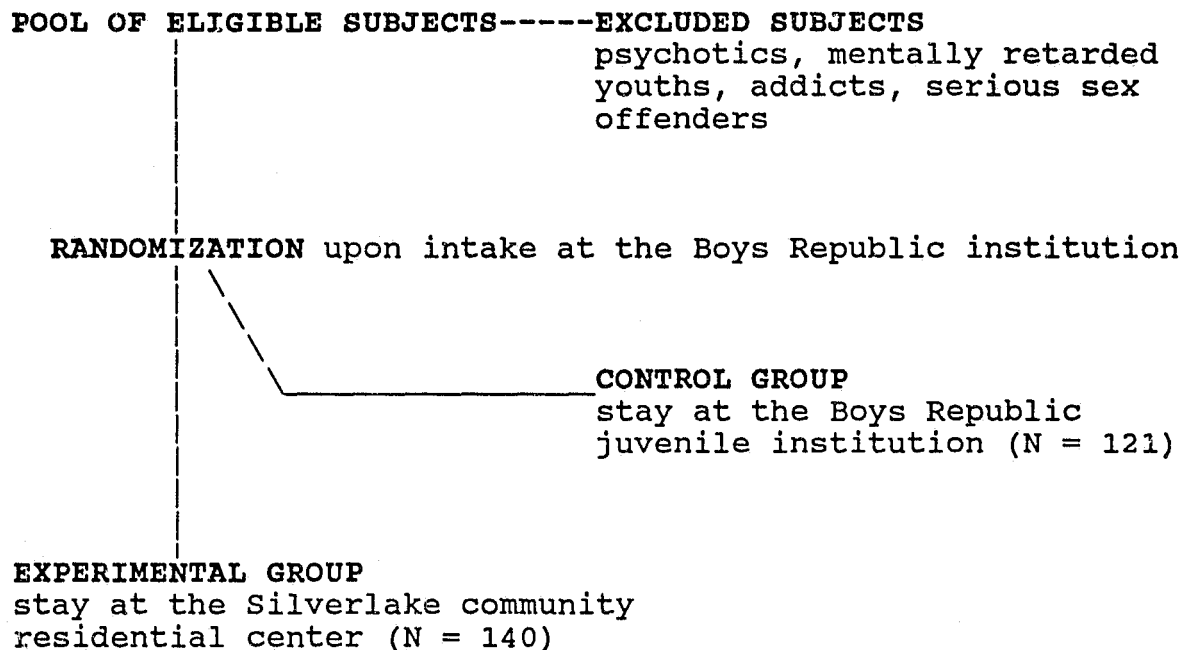


Notes

The experimental evaluation demonstrated no significant difference between the two groups. However, a quasi-experimental follow-up did show that the experimental work-unit group performed significantly better than the control group on the outcome measure of parole failure.

Again, when caseflow problems are experienced, design problems can be encountered. In this study, the PI relaxed randomization when the experimental group was at full capacity. In addition, random assignment proportions fluctuated when adjustments were made for the caseflow problems.

(1965) Los Angeles Silverlake Experiment - A



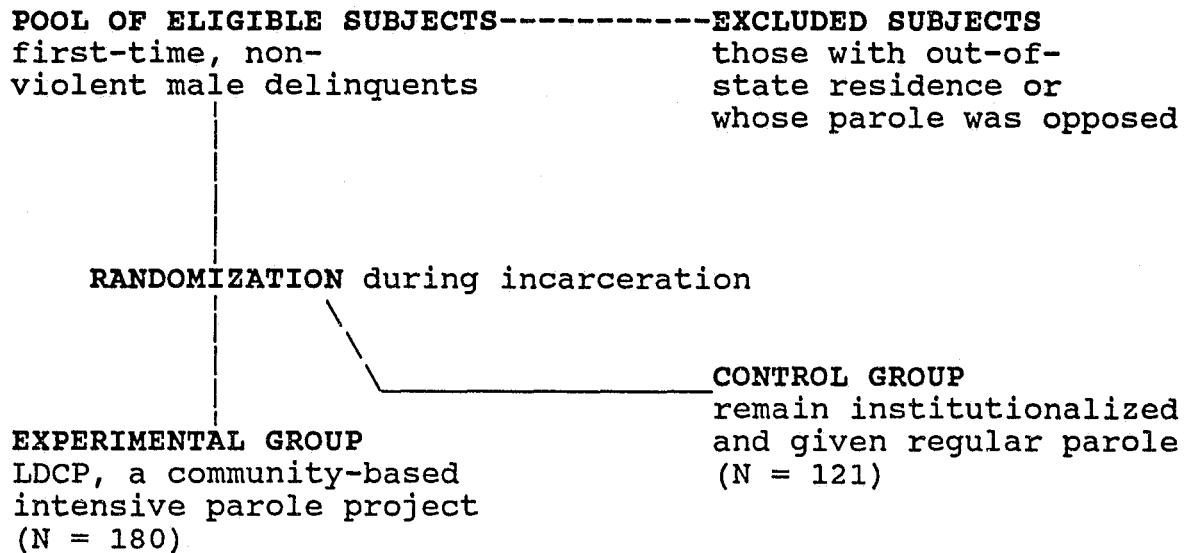
Notes

The number of statistical pretests run increases the probability that the groups will be significantly different on some variables before the intervention starts. Over 100 tests of group comparability were reported by the Investigators (Empey and Lubeck, 1971), with 6 achieving statistically significant differences.

Caseflow requirements forced the Investigators to "relax" randomization in order to fulfill experimental group population quotas (pp. 317-19).

The PIs also noted that there was a financial savings with the Silverlake community center over the traditional institution and suggested that cost is an appropriate outcome measure of an intervention.

(1966) Los Angeles Community Delinquency Control Project
Experiment - A



Notes

Differential attrition proved to be problematic, with the community-based parole group losing 21% of its subjects after original assignment. These losses resulted because subjects assigned to the experimental condition were rejected since it was feared that they posed a risk to the community.

This experiment was designed as a single experiment at two locations -- Jefferson and Watts. As is the case with some experiments, differences in results between sites emerged -- Jefferson subjects performed significantly better than Watts subjects on the outcome variable.

(1969) Denver Drunk Driving Sentencing Experiment - A

POOL OF ELIGIBLE SUBJECTS-----**EXCLUDED SUBJECTS**
those convicted for first-time DWI offenses none stated

RANDOMIZATION at sentencing

EXPERIMENTAL GROUP 1
probation and assorted
driver education and
alcohol counseling
(N = 164)

EXPERIMENTAL GROUP 2
probation with no therapy or
education (N = 157)

EXPERIMENTAL GROUP 3
received a monetary fine
(N = 166)

Notes

Local practitioners (sentencing judges) initiated the experiment. They were determined to see which of the sentencing dispositions was more effective with the first-time DWI offender.

The randomization process was subverted by the judges in at least 50% of the cases. This was due mostly to the urgent plea of defense attorneys, who wanted their clients to receive the less-restrictive fine condition. The breakdown of actual assignment versus random assignment was:

- 1) Fine -- 97% of those randomly assigned to this condition received a fine;
- 2) Conventional probation -- 30% of those randomly assigned to this condition actually received this sentence; and

Denver Drunk Driving Sentencing Experiment (cont.)

- 3) Therapeutic probation -- 35% of those assigned to this condition actually received it.

The PIs analyzed the results as randomly assigned and as actually assigned; neither analysis showed any significant differences between the three groups on the outcome variables. One criticism of the analysis is that neither the published nor the primary report gave actual effect sizes for the groups; only summary chi-square statistics were reported (Ross and Blumenthal, 1974).

(1969) Florida Inmate Work Release Experiment - A

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS
not stated

RANDOMIZATION while institutionalized

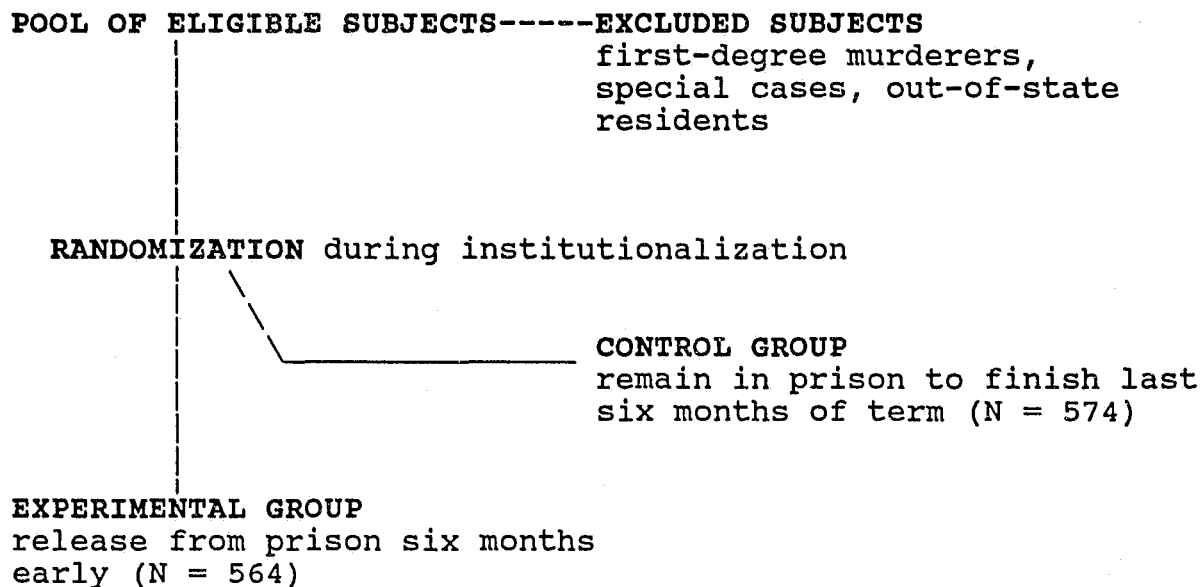
CONTROL GROUP
no work release -- regular
institutional programs
(N = 93)

EXPERIMENTAL GROUP
work release during
institutionalization
(N = 188)

Notes

The PIs used 18 measures of recidivism in an attempt to solve for the outcome measure problems that plague experiments and non-experiments (cf. Waldo and Griswold, 1979). Three different sources for the data were tapped: the F.B.I., the Florida Department of Corrections and self-report questionnaires. No significant differences between the groups were found on any measure or source, providing confidence in the PIs' conclusion that work release, as carried out in this experiment, did not reduce recidivism.

(1970) California Reduced Prison Sentence Experiment - A



Notes

The PIs concluded that inmates could be released six months early from prison without affecting subsequent recidivism levels. This conclusion was based on a chi-square analysis of 3 categories of subjects: "favorable," "unfavorable" and "pending." Cook and Boswick (1988) reanalyzed the experiment by dropping the pending category in a difference of proportions test and found that the prison group was significantly less likely to be returned to prison in the 24-month follow-up.

The problem of maintaining disparate conditions was encountered, as the control group subjects in some instances served less prison time than some experimental subjects (due to pre-parole advancements, etc.).

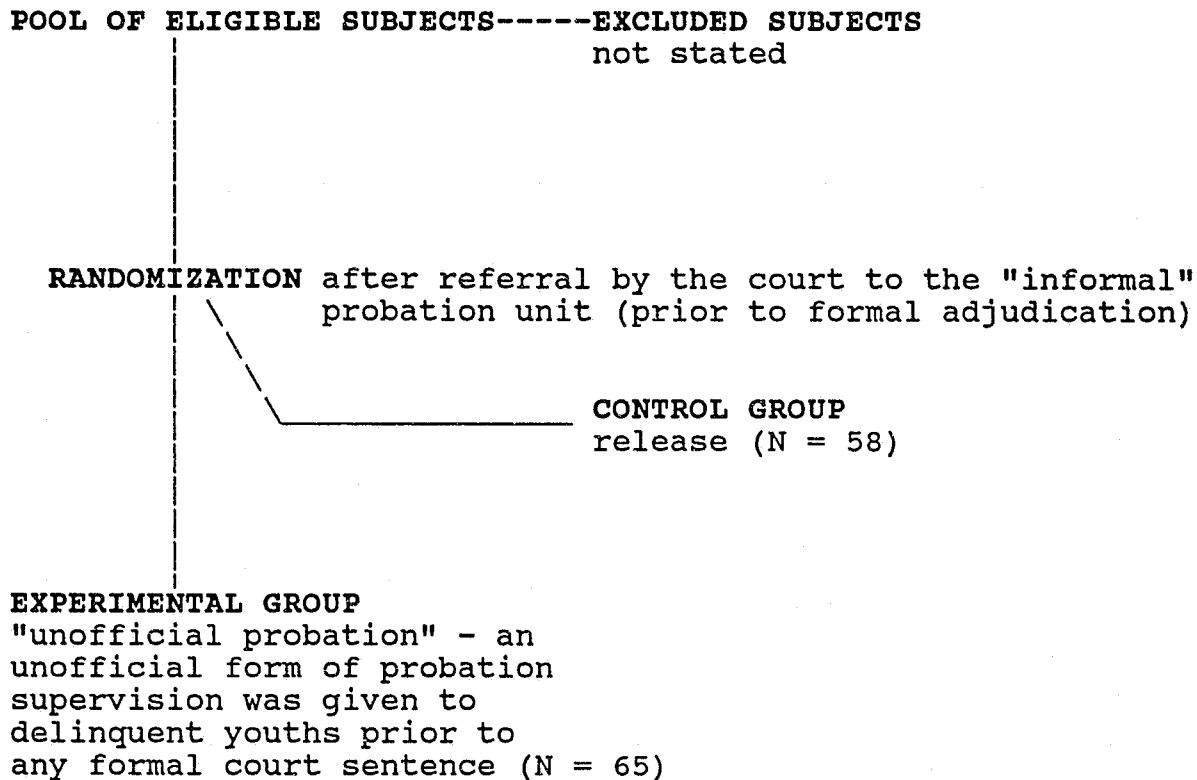
This experiment highlights the problem with generalizing from study findings. Though the experimental and control groups were comparable on the measures of equivalence used, the study sample was significantly different from the non-study prison population on many variables, leading the PIs to suggest

California Reduced Prison Sentence Experiment (cont.)

that selection bias entered into the experiment when eligible subjects were chosen.

The fact that the experiment was being conducted was known to both staff and inmates, but no test of the effect of this knowledge was undertaken.

(1970) California Unofficial Probation Experiment - A

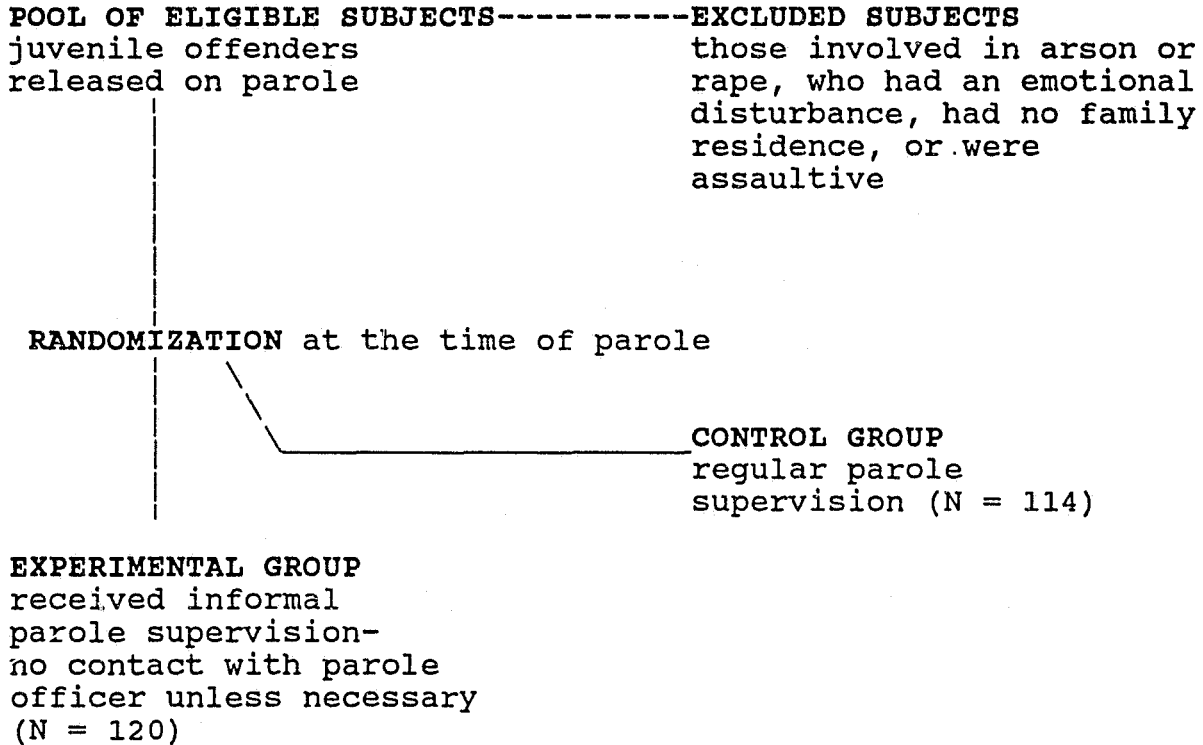


Notes

The number of cases desired for each group (the PI wanted 75 in each) never materialized due to a lower number of eligible youths in the county being assigned to the program than had been expected.

The PI did establish a recording procedure in which the experimental agents were to record their contacts with the youths to determine if the study conditions were in reality different from each other. Non-observational recordkeeping of this kind has its own pitfalls (including being subject to falsification), but it represents an early, rudimentary treatment check.

(1970) Minneapolis Informal Parole Experiment - A



Notes

The PI initially wanted to examine experimentally the different effects that parole supervision had when compared with no parole supervision. However, Minnesota law prohibited the release of any juvenile offender without a parole supervision period being designated. The "informal parole" was a compromise between the research design and the law. It was clear that members of the experimental group understood that "informal parole" meant there was to be no contact with their parole officers (Hudson, 1973:3).

Those supervising the traditional parole group, as expected, did have significantly more contact with subjects than did the informal parole officers. The mean number of contacts per group were 116 for traditional parole and 15 for informal parole subjects.

Minneapolis Informal Parole Experiment (cont.)

Hudson looked separately at the effect of the experimental intervention for male and female subjects. A strong backfire effect was found among the male subjects, while there was no statistical difference between the two groups for the female subjects. When analyzed together, there was a significant backfire effect for informal parole (using a difference of proportions test).

(1970) Sacramento (CA) Juvenile 601 Diversion Experiment - A

POOL OF ELIGIBLE SUBJECTS -----**EXCLUDED SUBJECTS**
runaways, truants, uncon- none stated
trollable youths, and
minor criminal offenders

RANDOMIZATION after referral from police, family,
or school

CONTROL GROUP
enter traditional criminal
justice system -- not further
specified (N = 558)

EXPERIMENTAL GROUP
diversion from the traditional
criminal justice system to receive
the services of Sacramento 601
Diversion Unit (N = 803)

Notes

As in Silverlake, financial cost as an outcome measure strongly supported the Diversion Unit over the traditional criminal justice system response.

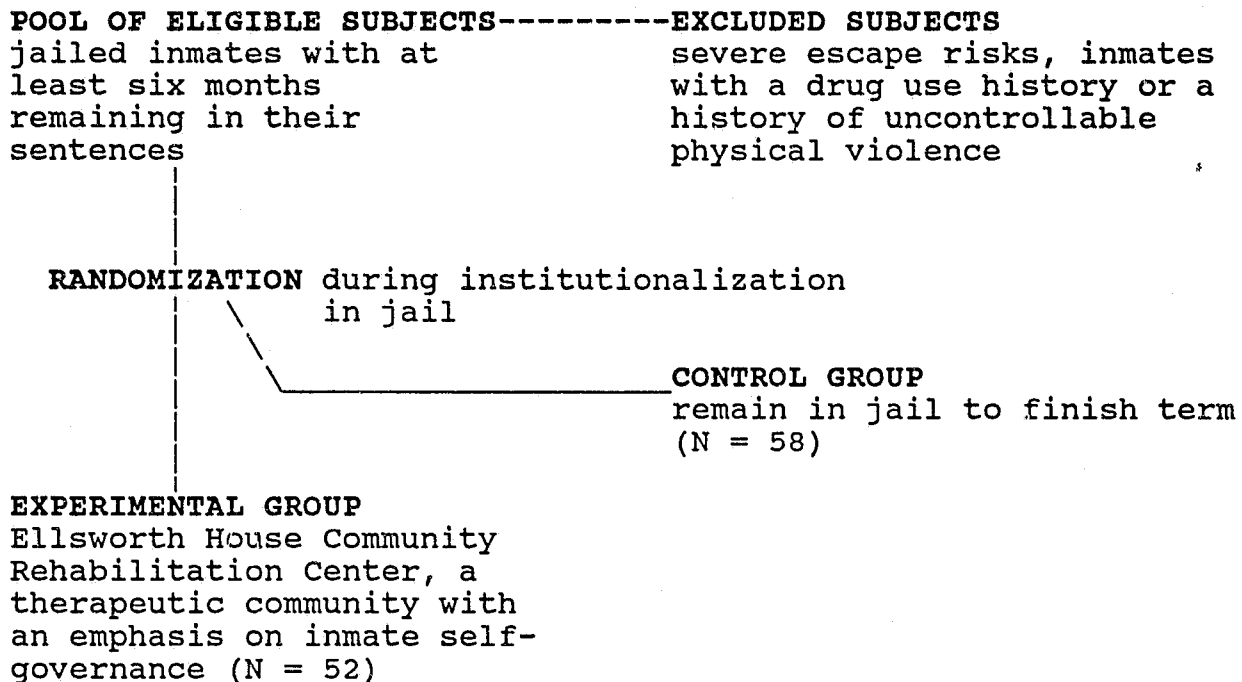
The PIs noted that some contamination took place: a) 45 cases were inadvertently assigned the wrong condition in the experiment; and b) some subjects in the experimental group committed criminal offenses and received the traditional system treatment that the controls had received. The PIs analyzed the subjects according to their original random assignment --not according to the treatment they actually received.

The PIs overlooked the large number of girls in the experimental diversion group as compared to the control traditional system group. A difference of proportions test confirmed that this difference was significant at $p < .05$. This was important -- since girls did much better in the follow-up than did the boys.

Sacramento (CA) Juvenile 601 Diversion Experiment (cont.)

This was a classic case of an experiment influencing policy. Based on the academic evaluations, Sacramento adopted the 601 Diversion Plan for all 601 cases in 1972.

(1971) California Ellsworth House Experiment - A



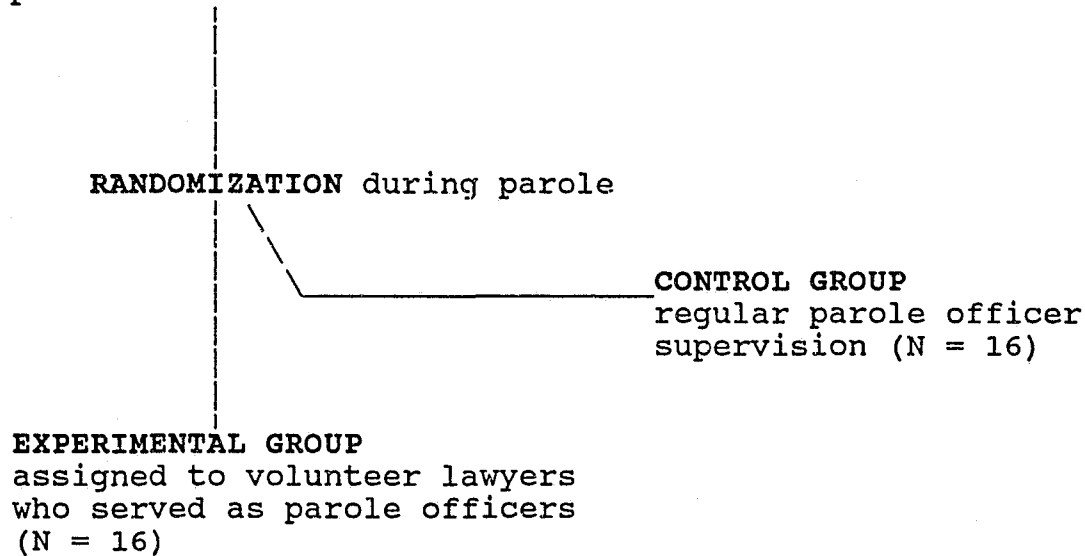
Notes

Some of the problems encountered here may be attributable to problems associated with action research. For example, treatment failure occurred during the early stages of the Ellsworth House experiment. Conditions at the House were extremely lenient -- originally designed rules were not kept in force. The permissive attitude at the House was blamed for the higher (though not significant) recidivism at the 6-month follow-up.

Once the PIs learned of the staff problems, changes were made in the program to see that staff enforced the rules. When recidivism leveled off at the 12-month follow-up (again, no difference between the groups), the PIs suggested it was due to the latter subjects (housed under the improved conditions) performing better upon release.

(1971) Illinois Volunteer Lawyer Parole Supervision
Experiment - A

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS
male offenders already on parole none stated



Notes

Volunteer lawyers were told to befriend and counsel clients and not to police them. The lawyers had no power to arrest. Contact between the lawyers and clients totaled approximately 6 hours per month, which, given the normal extent of parole supervision, could be considered intensive (Berman, 1978).

All of the subjects were on parole 6 months before the assignments were made.

The experimental intervention provided the parolee with a person of status to use as a reference; however, no impact was made on the economic and other negative conditions facing the ex-offender upon release. It was suggested by the PI that the limited scope of the intervention could explain why this intervention did not produce a significant difference on the outcome measure.

(1971) English Intensive Probation Experiments - A
 - (includes four experiments) **

POOL OF ELIGIBLE SUBJECTS----- offenders 17 years of age or older with at least 2 prior confinements or probation orders	EXCLUDED SUBJECTS those under 17 years with fewer than 2 confinements or orders
--	---

RANDOMIZATION after court referral to probation unit
 at one of the four sites

EXPERIMENTAL GROUP
 intensive probation
 supervision, 20-man
 caseload --

Sheffield	(N = 142)
Dorset	(N = 113)
London	(N = 172)
Staffordshire	(N = 101)

CONTROL GROUP
 regular probation
 supervision, 40-man
 caseload --

Sheffield	(N = 136)
Dorset	(N = 111)
London	(N = 141)
Staffordshire	(N = 85)

Notes

This experiment provided strong evidence against the rehabilitative benefit of intensive probation supervision as carried out in England. None of the sites alone showed any reductions in recidivism levels. Nor was any reduction in recidivism shown when the results from all sites were merged. The PIs suggested that treatment may have been carried out poorly or that the intensity of the supervision may have increased legal contacts (i.e., the experimental subjects were under more scrutiny than the subjects in the control condition).

Though these experiments were designed for high-risk male probationers, the PIs noted that this eligibility criterion was often relaxed to fulfill project quotas.

** Each of the experiments discussed here was analyzed separately in the data base ("SPSS Data Set - Experiments in Sanctions").

English Intensive Probation Experiments (cont.)

There were problems noted in obtaining information on the nature of the intensive supervision -- PIs did note that the differences between the groups in the number of contacts made were statistically and practically significant.

A strong methodological feature of this experiment was the refusal of researchers to allow the practitioners to remove a case after randomization had taken place. Any exclusions had to be made prior to random allocation.

(1972) Sacramento (CA) Juvenile 602 Diversion Experiment - A

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED

those charged with petty theft, none stated
possession of narcotics, and
drunk and disorderly conduct

RANDOMIZATION after referral from police, family, or
school

CONTROL GROUP
enter traditional
criminal justice system
-- not further specified
(N = 105)

EXPERIMENTAL GROUP

diversion from the traditional
criminal justice system to receive
the services of Sacramento 601/602
Diversion Unit (N = 111)

Notes

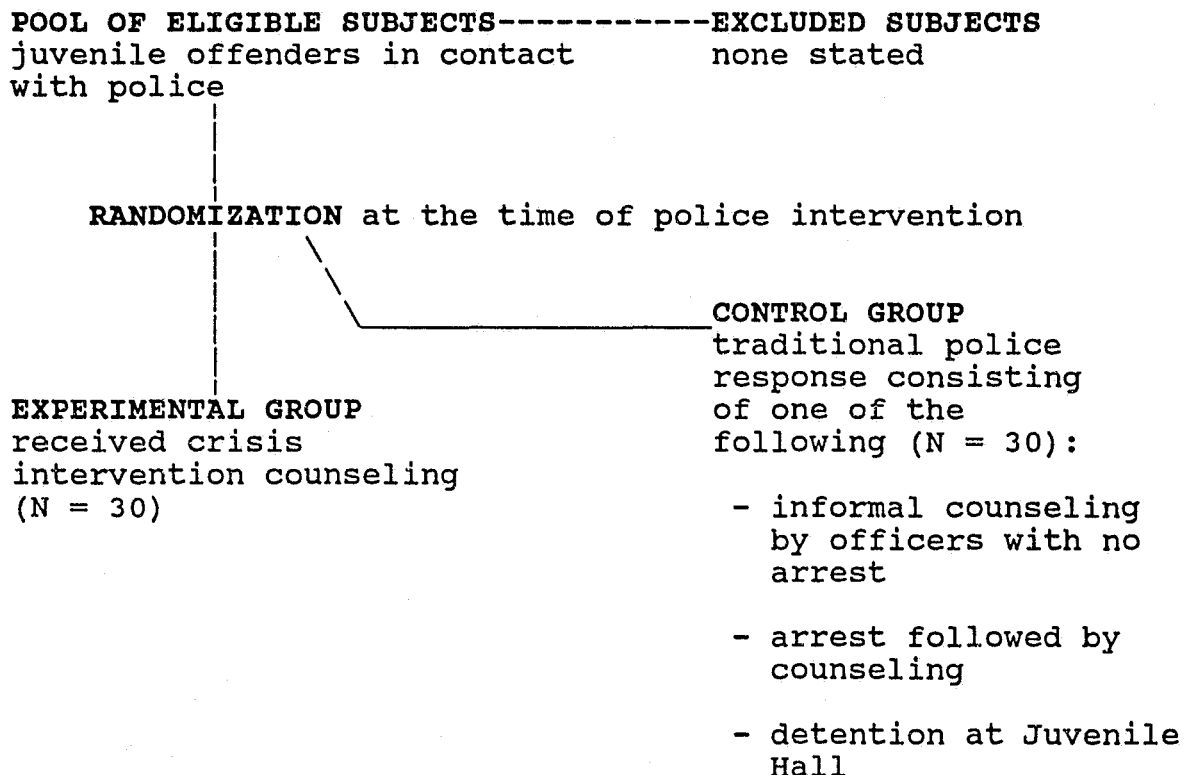
The criminal justice system condition represents a problem in the sanctioning experiments since no further evidence is given to us about the nature of the condition. What did the control groups in this experiment end up with? It could be that the intrusions of the criminal justice system response were less onerous than those of the diversion unit. For instance, it may be that in defining the "harshest" sanction we are bringing our own middle- and upper-class mindsets to the research. Appearance before a juvenile court judge may sound threatening to those who were never in trouble before -- but to the youths in these experiments it may be preferable to having tedious meetings with family counselors and therapists.

The fact that the 601 Diversion experiment had been set up provided the PIs with the necessary foundation to run a second experiment -- this time with juvenile minor criminal offenders. 602 eligibles were referred to the project and

Sacramento (CA) Juvenile 602 Diversion Experiment (cont.)

handled by the same agency staff that were handling the 601 cases. No new site had to be arranged -- nor staff hired. The ease with which the researchers were able to arrange this experiment can be seen when looking at the years the experiment started: 601 Diversion in 1970; 602 Diversion in 1972.

(1973) San Fernando (CA) Juvenile Crisis Intervention Experiment - A



Notes

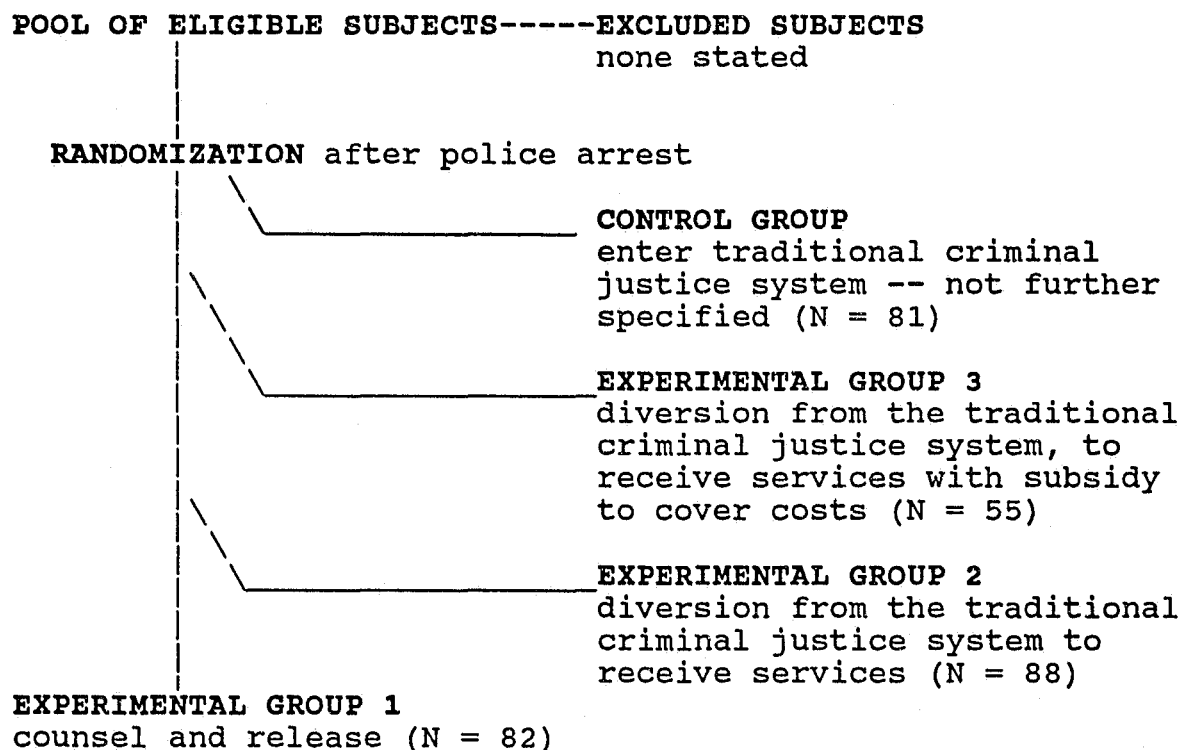
The PI gave the reader a good description of the city where the experiment took place, but not of the subjects in the study. There was no description of how many in the control group received each of the possible police responses. The pretest was done on only 3 variables.

The PI referred to the insufficient power of the experiment. He stated that "...if the proportions were maintained, as the size of the groups increased, there would have been a significant difference in the two groups regarding individual arrest rates" (Stratton, 1975: 11).

A financial cost analysis was done and demonstrated that the crisis intervention approach represented a 50% savings over the traditional approach.

(1974) Juvenile Diversion and Labeling Paradigm Experiment

- A



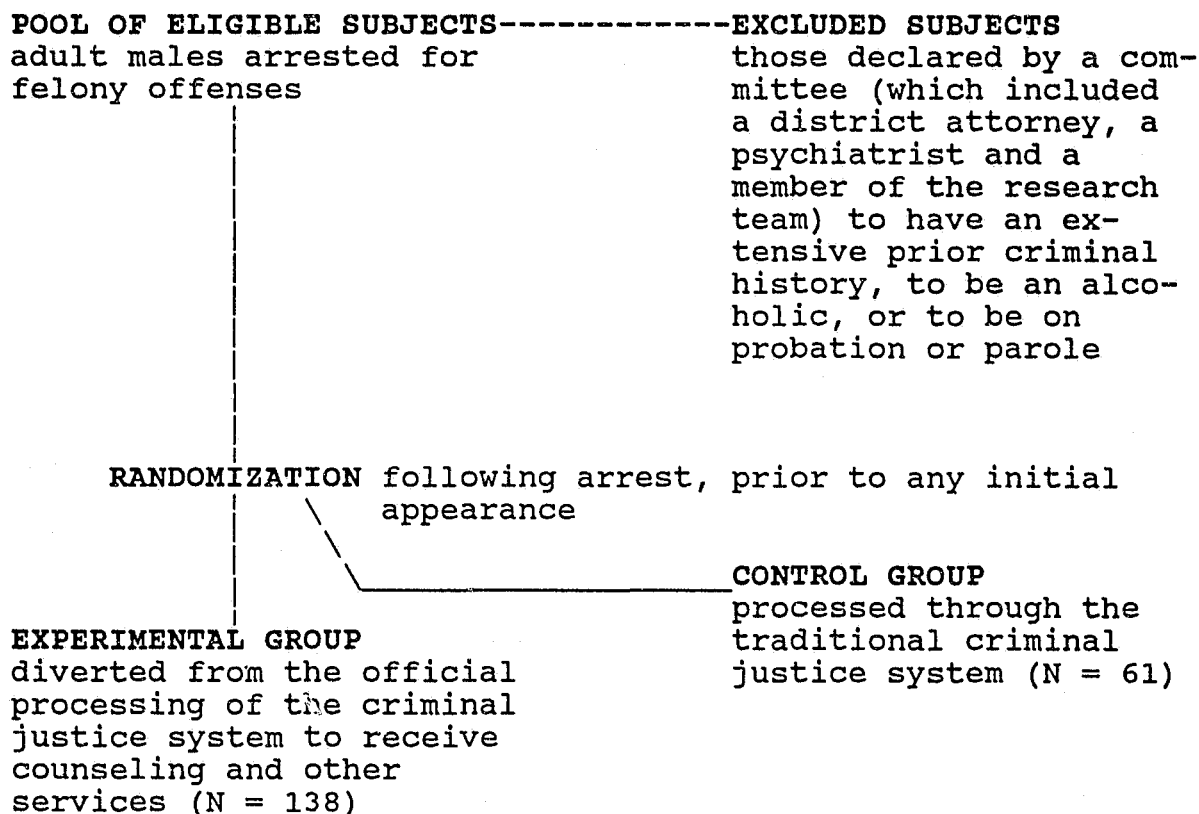
Notes

The field experiment, unlike the laboratory experiment, must be adapted to the changing social environment. In this field experiment, the PIs had to accept a lower number of cases for the subsidy group due to a delay in signing financial contracts.

The police attempted to subvert the randomization process in the initial stages. This was observed and controlled by the PIs.

Two types of outcome data were used to measure recidivism: official police reports and self-reports. The official reporting showed a significant difference between the counsel and release group and the petition group, while the self-report data showed no difference between these two groups. Only 60% of the self-report interviews were completed and the completion rates for each of the groups was not reported.

(1975) San Pablo (CA) Adult Diversion Experiment - A



Notes

Only 54% of the total felon arrest population was eligible for the experiment, with the remaining 46% declared ineligible.

The problem of sanction integrity is evidenced in the diversion experiments. We define the traditional processing as the harshest sanction; however, both groups should be followed. It may turn out -- as in the Vera study -- that the diversion group actually received more intrusive treatment than the control subjects. Did the controls have high rates of dismissed sentences, suspended sentences and fines without having to meet the diversion program requirements and contacts?

San Pablo (CA) Adult Diversion Experiment (cont.)

This issue demonstrates the importance of the Principal Investigator's goal. Here the interest was in showing diversion to be an acceptable alternative to court processing for some arrestees. However, if the PI's goal had been stated as deterrence, control over the independent variable might have meant insuring that traditional processing resulted in punishment more coercive than that in the diversion program.

(1975) Pinellas County (FL) Juvenile Services Program
Experiment - A

POOL OF ELIGIBLE SUBJECTS-----	EXCLUDED SUBJECTS
children legally adjudicated as delinquent, children designated as being in need, and informal referrals from police, family and schools	none stated

RANDOMIZATION after referral to the Juvenile Services Project

CONTROL GROUP
enter traditional criminal
justice system -- not
further specified (N = 132)

EXPERIMENTAL GROUP
diversion from the traditional
criminal justice system to receive
job placement, and vocational and
counseling services (N = 436)

Notes

To satisfy ethical standards, the PIs established a caseload procedure that would proportionately assign more subjects to the experimental group (believed to be more beneficial than the traditional system) than to the control group. The experiment was justified on the ground that more clients were being referred to the Juvenile Services Program (JSP) than could possibly be served and randomization would represent the "fairest" assignment mechanism.

This experiment also illustrated how different breakdowns of rearrest data can have different results. For example, a significant positive effect for JSP was found for in-program and post-program arrest rates when these were examined separately. However, when combined, the differences between the groups were not significant. In addition, in-program arrests are probably best viewed as treatment failures while post-program arrests can be used as a measure of treatment effectiveness for those who completed the treatment.

(1975) Washington, D.C., Pretrial Supervision Experiment - A

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS

pretrial subjects who were
arrested for a first-time
felony offense

none stated

RANDOMIZATION occurred after court referral into the unit

EXPERIMENTAL GROUP 1
intensive pretrial super-
vision, with an emphasis on
continued contact and
notification to increase the
likelihood of appearance in
court (N = 100)

EXPERIMENTAL GROUP 2
Moderate or regular dosage
of pretrial supervision (N =
100)

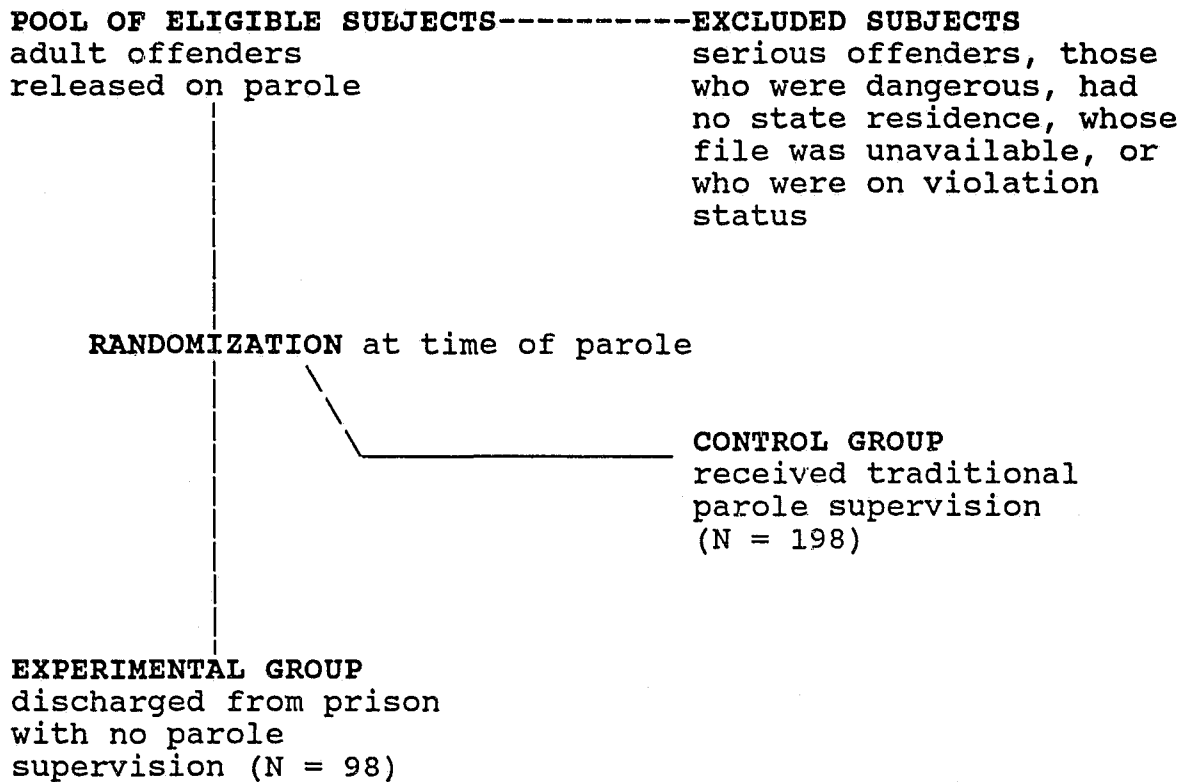
EXPERIMENTAL GROUP 3
passive pretrial supervision with
client-initiated contact (N = 100)

Notes

The Washington, D.C., Bail Agency initiated this experiment. Officially it was not charged with law enforcement responsibilities, however, it was noted that the Bail Agency did monitor conditions of release and notified the court of failures on the part of clients (Welsh, 1978: 138).

"Failure" in this study was defined as: a) rearrest; b) failure to comply with court-ordered conditions of pretrial release; or c) failure to appear as scheduled in court. Any such failure resulted in an arrest warrant being issued. Although higher levels of supervision might be seen as helpful (i.e., more notifications and reminders for court dates given to clients), the increased level of supervision also brings the client into more contact with supervising agents and increases the likelihood of failure.

(1976) California Early Parole Discharge Experiment - A

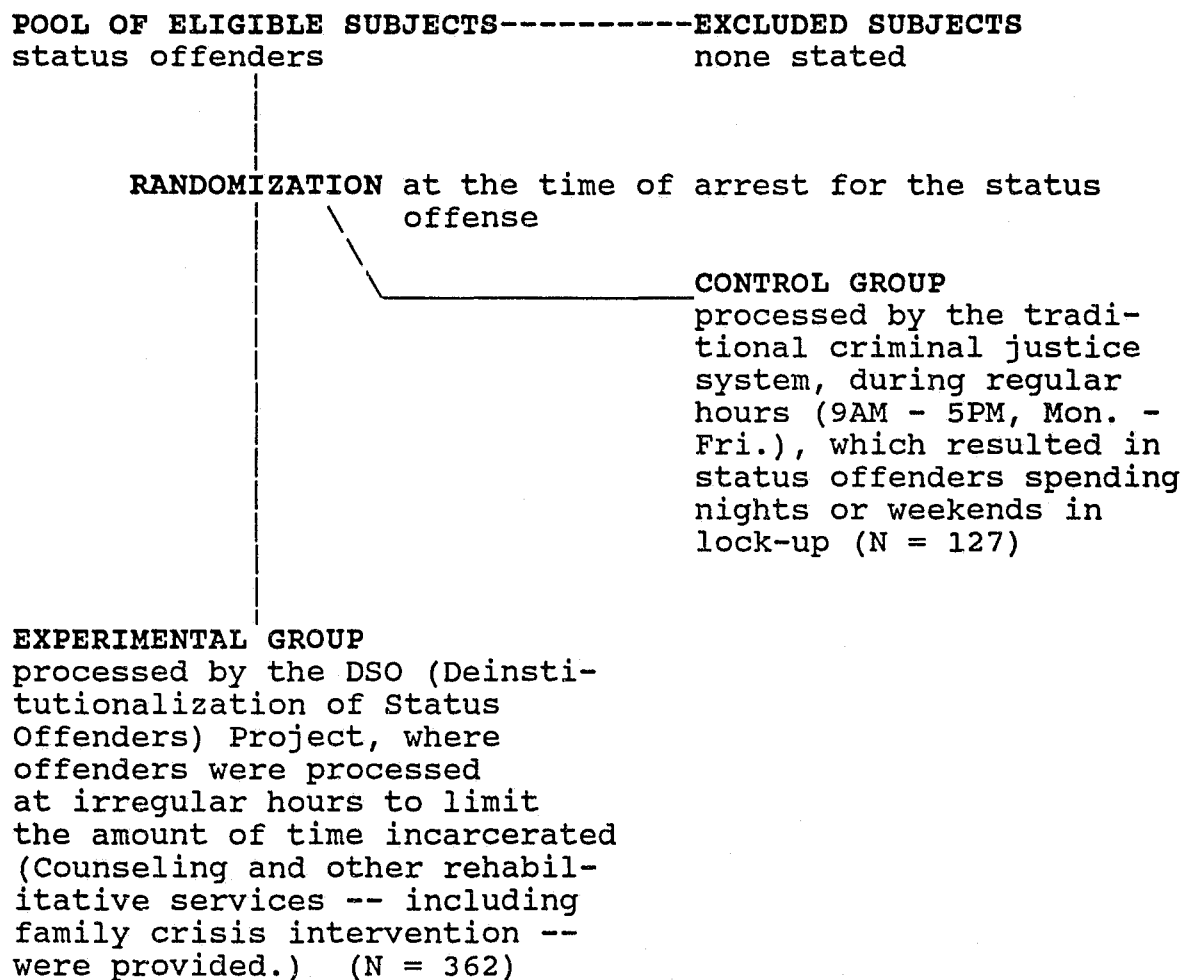


Notes

Those assigned to the traditional parole group committed significantly fewer drug- and alcohol-related offenses. They were, however, charged with significantly more homicide and rape offenses than the discharged group.

Although random assignment to the discharge and regular parole groups was kept intact, a third group was originally designed to be part of the random assignment: voluntary services. This condition was never implemented.

(1976) Clark County (WA) Status Offender Deinstitutionalization Experiment - A



Notes

This experiment was part of the National Evaluation of the DSO Programs. In the overall national report, Kobrin and Klein (1983) claimed that none of the DSO sites was able to implement random assignment.

The power of an experiment to randomly distribute follow-up error was noted by the PI (Schneider, 1980: 132). Any bias

Clark County (WA) Status Offender Deinstitutionalization
Experiment (cont.)

in official processing should be randomly distributed for the experimental and control groups; there should be no systematic bias on the recidivism measure.

The PI defined recidivism as a subsequent court contact for a delinquent or status offense after the instant status offense.

It was not explicitly stated in the publication what types of subjects were ineligible for the experiment -- although it can be inferred that those arrested for "delinquent" offenses were ineligible.

Differential attrition was noted for the different follow-up periods, with a larger percentage of experimental subjects dropped in later analyses (E = 27%, C = 3%). There was also substantial variation in the number of months each individual subject was followed -- no standardized follow-up time was used.

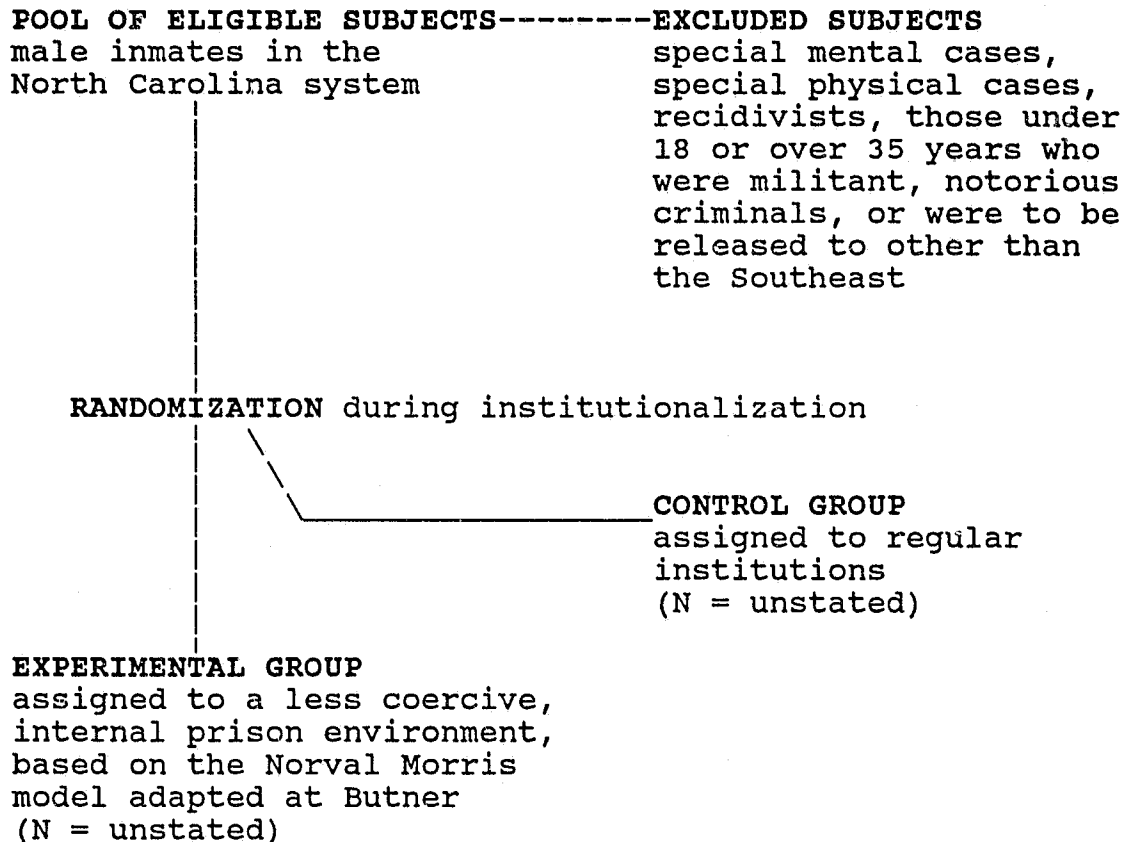
Memphis Drunk Driving Sanctioning Experiments (cont.)

Notes

The experimental reports described this as a very coercive experiment -- those not completing the requirements of the Driver Improvement program (e.g., the educational classes) were subject to a 1-year jail term.

A significant deterrent effect was found in the "problem" drinkers experiment for the probation groups compared to the non-probation groups on the non-DUI rearrest measure. This was masked when both experiments were combined.

(1976) North Carolina Butner Correctional Facility Experiment
- A



Notes

Morris (1974) proposed that the freedom, dignity and individuality of all persons, including prisoners, must be protected. His prison model embraced the following ideas:

- 1) self-help programs were to be offered but not required;
- 2) there was to be a predetermined length of imprisonment;
- 3) prison should be used to test inmate suitability for release; and

North Carolina Butner Correctional Facility Experiment
(cont.)

- 4) life in the prison was to simulate life outside as much as possible, including freedom of movement within the prison and the wearing of civilian clothes.

"Opt-outs" were those assigned to the experimental prison at Butner who were given 90 days to decide if they wished to go back to regular custodial facilities and decided to go back. The primary publication did not clearly state whether these opt-outs were kept in the analysis.

(1977) Memphis Juvenile Diversion Experiment - A

POOL OF ELIGIBLE SUBJECTS-----**EXCLUDED SUBJECTS**
youths who normally
would not be eligible
for diversion (to counter
"net-widening" criticism)

RANDOMIZATION after court referral to Memphis Metro North
Diversion Project

EXPERIMENTAL GROUP 3
enter the traditional
criminal justice system
(N = 377)

EXPERIMENTAL GROUP 2
counsel and release
with no further contact
(N = 475)

EXPERIMENTAL GROUP 1
diverted from the traditional system
to receive the services of the Memphis
Metro North Diversion Project (N = 775)

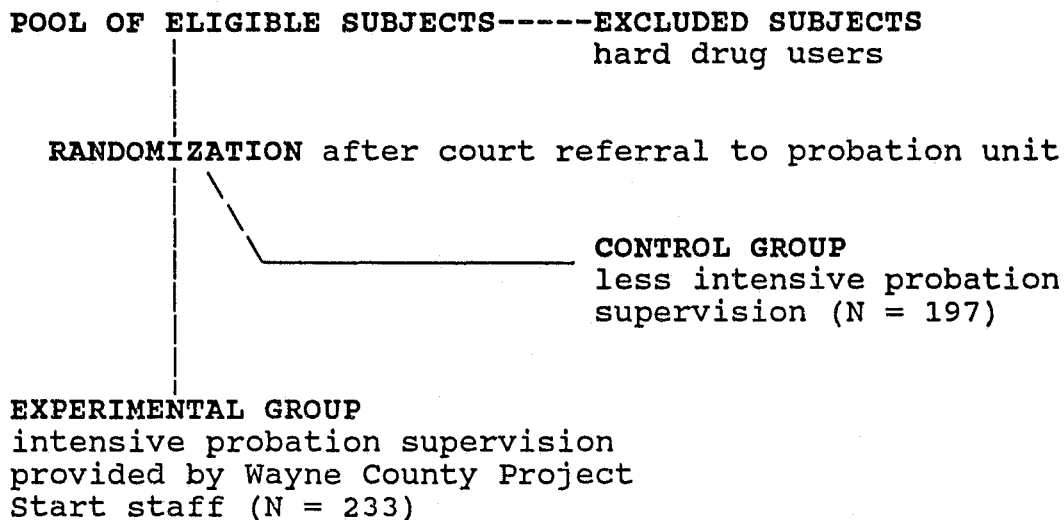
Notes

Juveniles in the experiment could have refused the diversion and release conditions -- and would have received the traditional system response defined here as the harsher sanction. This harsher sanction could not be refused.

The PIs analyzed the groups as randomly assigned and as actually assigned after refusal and found no significant differences between the three dispositions using either method of analysis.

"Net-widening" is a criticism levied at diversion projects -- those who would normally have been set free without diversion are referred to programs and projects. The PIs attempted to counter this criticism by using an eligibility pool that would normally have been processed through the traditional system if Memphis Metro-North had not existed.

(1977) Wayne County (MI) Project Start Experiment - A



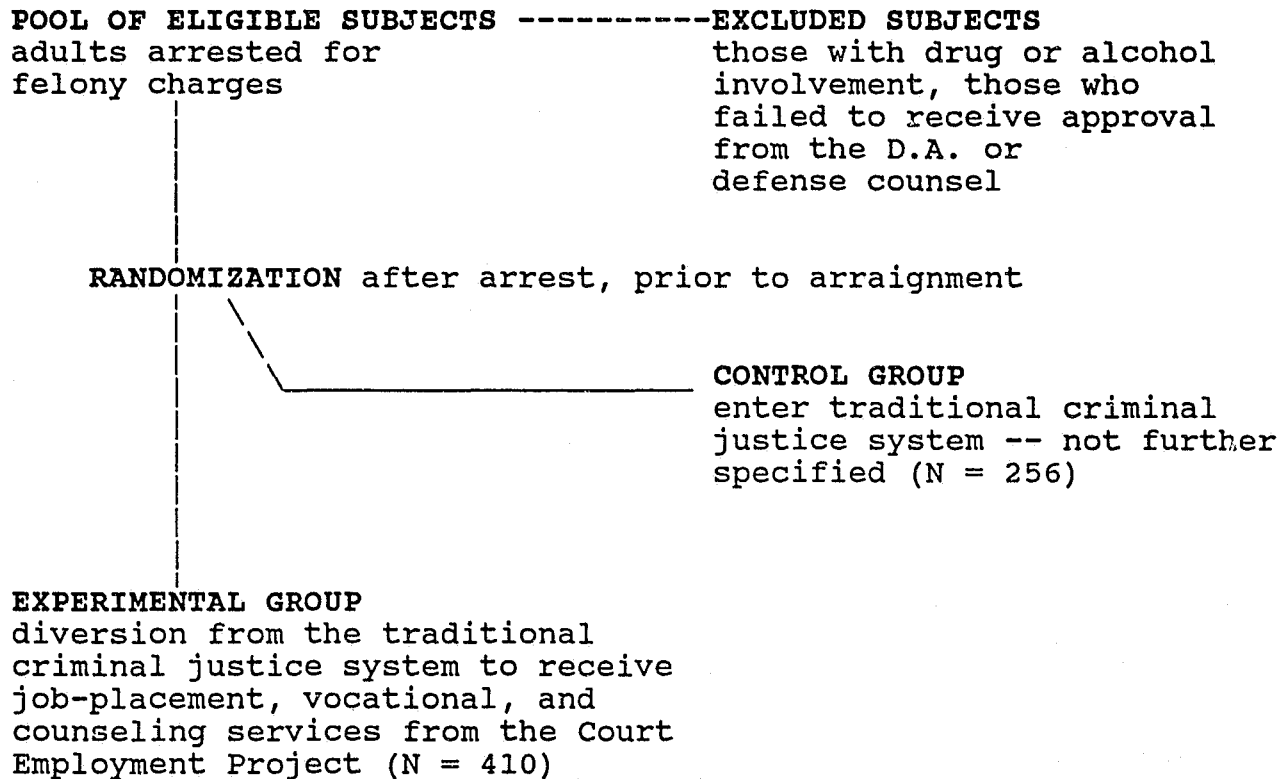
Notes

Random assignment in the experiment started on a 1:1 basis (1 person to the experimental group, the next person to the control group). Low numbers of eligible cases later in the study forced a 2:1 assignment ratio.

The difference between the groups in supervision intensity was statistically significant but not significant when viewed practically. Can we expect that the experimental group's average of 2.44 contacts per month reduces recidivism at a greater rate than the control group's 1.32 contacts per month? The difference in monthly contacts between the groups decreased as the experiment progressed -- perhaps experimental staff began to operate according to "business as usual."

The follow-up period for the outcome measures was not given -- the PIs stated that the exact period of time depended on the date when the probationer began probation (Lichtman and Smock, 1981: 91). No supporting data were given that would assure readers that the follow-ups periods were similar for the two groups; perhaps one group was at risk significantly longer than the other.

(1977) Vera Institute (NY) Pretrial Adult Felony Offender
Diversion Experiment - A



Notes

Ethical questions were handled by assigning more eligibles to the Court Employment Project than the program could divert. Overflow cases were assigned to the traditional system control group. A random-time quota system was used.

Overrides were allowed -- subjects randomly assigned to one group were placed at practitioner's discretion in the other group -- but the analysis was done on the groups as randomly, and not actually, assigned.

The "harshest sanction" group in this case may have received more lenient treatment than the diversion group. 69% of those who went through the criminal justice system received no punishment (46% were dismissed, 23% were discharged). 79%

Vera Institute (NY) Pretrial Adult Felony Offender Diversion Experiment (cont.)

of the diversion group had their charges dismissed or discharged. The coercive threat to satisfy the conditions of the CEP program did not materialize; nearly 40% of the subjects failing to meet the conditions of CEP (e.g., attendance) also had their charges dropped.

The diversion project had an operating assumption: if impact could be made on employment, then positive impact could be made on recidivism. The project did not significantly affect the employment of the experimental group -- therefore it could not have had a corresponding effect on recidivism.

(1977) Hamilton (Canada) Juvenile Services Project
Experiment - A

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS

those older than 14 years,
not living with their own
family, or with fewer than 2
prior police occurrences

RANDOMIZATION after police detention at station

CONTROL GROUP
traditional investigation
by the Youth Bureau Officer
(N = 151)

EXPERIMENTAL GROUP
diversion from the criminal justice
system to receive family crisis
counseling (N = 154)

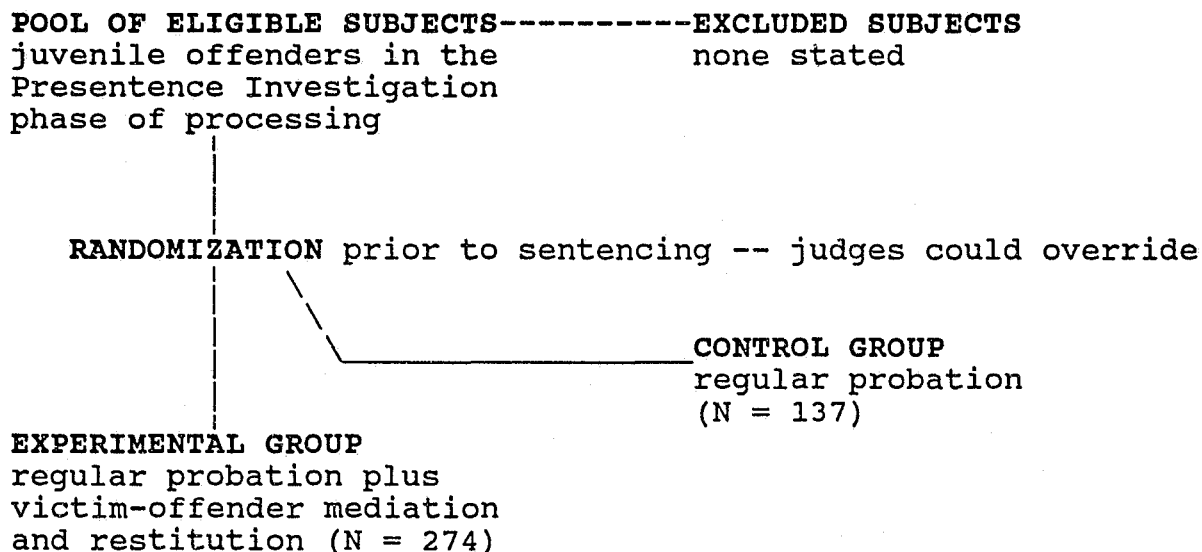
Notes

Only the experimental diversion treatment was observed (the counseling sessions were viewed through a window). There was no way to ascertain the strength of supervision administered by the traditional YBOs.

Only 70 of the 154 families attended the counseling session, highlighting the non-coercive nature of the diversion program.

National Restitution Experiments (cont.)

Washington, D.C.



Notes

There were different follow-up periods used for the experimental (32 months) and control groups (31 months). No explanation was given in the primary report about the effect, if any, that an additional month at risk might have had on the results of the analysis.

The option to refuse the restitution group was given to subjects in the experiment; subjects could choose conventional probation. The PIs analyzed the results for the two randomly assigned groups, finding no differences between them. The PIs also analyzed the results for restitution, probation and the restitution-refused subjects. Again, no differences between the groups were found.

National Restitution Experiments (cont.)

Clayton County, Georgia

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS
juvenile offenders at the none stated
adjudication stage

RANDOMIZATION at adjudication to

CONTROL GROUP
probation (primarily) --
although some subjects
were incarcerated
(N = 55)

EXPERIMENTAL GROUP 3
probation and restitution
(N = 73)

EXPERIMENTAL GROUP 2
probation and restitution
with counseling
(N = 74)

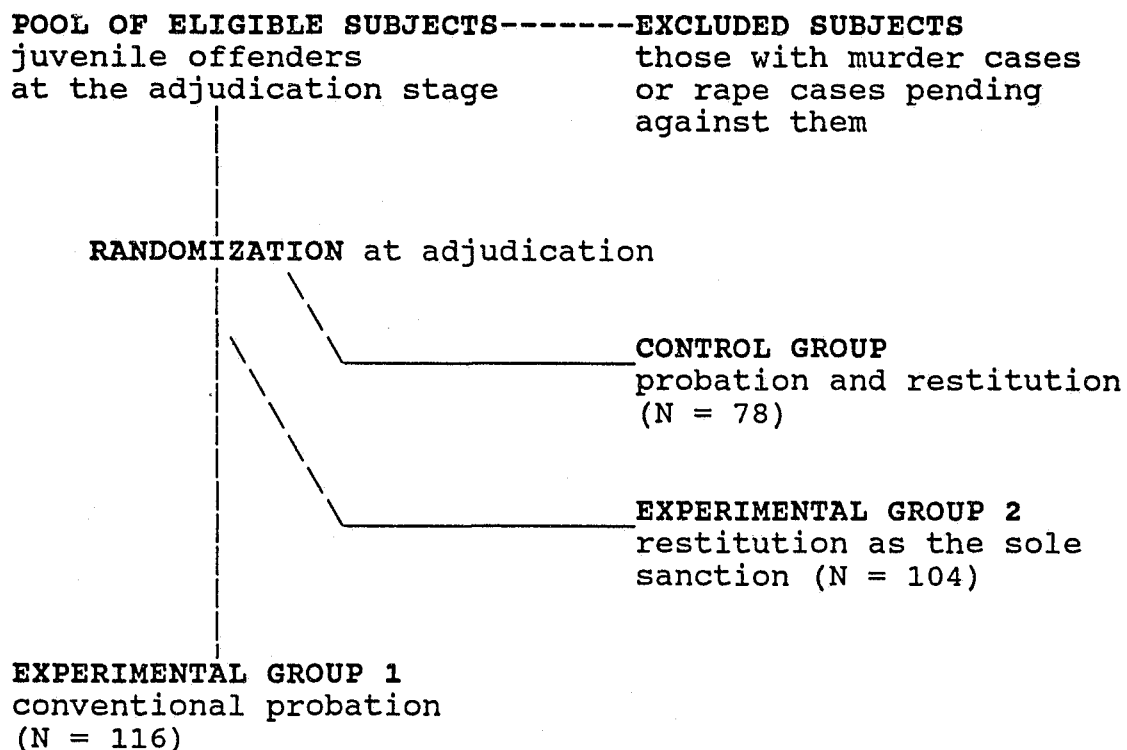
EXPERIMENTAL GROUP 1
probation and counseling
(N = 55)

Notes

Although the control condition consisted primarily of conventional probation, 5% of those in the control condition were incarcerated. This presents difficulty in determining which sanction should be considered as the "harshest" sanction. Since 95% of the group received probation, the present analysis defines this condition as the least harsh intervention.

National Restitution Experiments (cont.)

Oklahoma County, Oklahoma



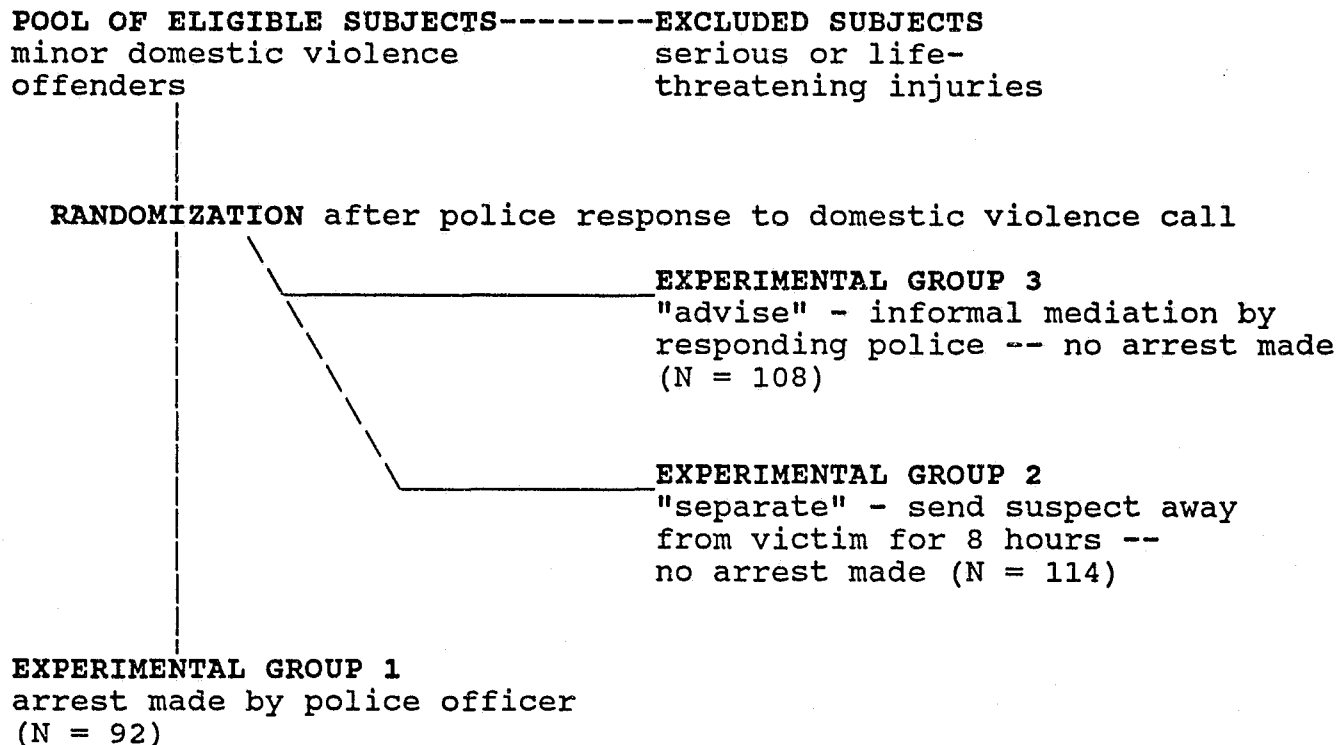
Notes

The judges could override random assignment. The percentages of overrides in each group were:

- 1) sole sanction restitution - 9%;
- 2) restitution and probation - 10%; and
- 3) conventional probation - 11%.

The PIs analyzed the results according to the groups into which the subjects had been randomly assigned.

(1981) Minneapolis Domestic Violence Experiment - A



Notes

The field experiment can also consist of conditions that are not purely "experimental or control." For example, in this experiment, all 3 groups received some form of intervention. (Would a pure control group have consisted of police not responding at all to the call?)

Overrides of random assignment were allowed -- and, to the PIs' credit, were well-documented. It appeared as if the officers were upgrading the less severe sanctions to arrest status. The actual assignment figures were: arrest - 98.9%; advise - 77.8%; and separate - 72.8%. Overrides were allowed if:

- 1) the offender attempted to assault the police;
- 2) the victim demanded an arrest;
- 3) a restraining order was violated; or
- 4) the offender would not leave the premises when ordered to do so.

Minneapolis Domestic Violence Experiment (cont.)

Sherman and Berk (1984) ran a logit analysis on why officers overrode randomization and found that police violated assignment when: a) the officer reported that the suspect was rude; b) it was reported that the suspect tried to assault one of the officers; c) it was reported that a weapon was involved; d) the victim demanded arrest; or e) a restraining order was violated.

Of the 136 suspects who were actually arrested in the study, only 3 received any "formal" sanction from a judge. However, Minneapolis was unique because it kept most domestic violence suspects locked up for one night, leading to speculation that the one night of detention may be the independent variable rather than arrest.

It was not stated in the scholarly report whether tests of group comparability were done. Female suspects were not mentioned -- was the sample 100% male?

This experiment has been misinterpreted. It is true that the "arrest" group had lower recidivism rates than the "separate" group -- but when "arrest" was compared to "advise," there was no significant difference in the recidivism rates for the two groups. "Advise" was also statistically indistinguishable from the "separate" group. However, a difference of proportions test did show a significant effect for "arrest" when compared to the "advise" and "separate" groups combined ($t = 2.46$).

For the self-report data, "arrest" was significantly different from "advise" -- but "separate" was lower than either.

Generalizability is always a problem with experiments. Binder and Meeker (1988) pointed out peculiar aspects of the Minneapolis sample which should have lessened its policy impact. For example:

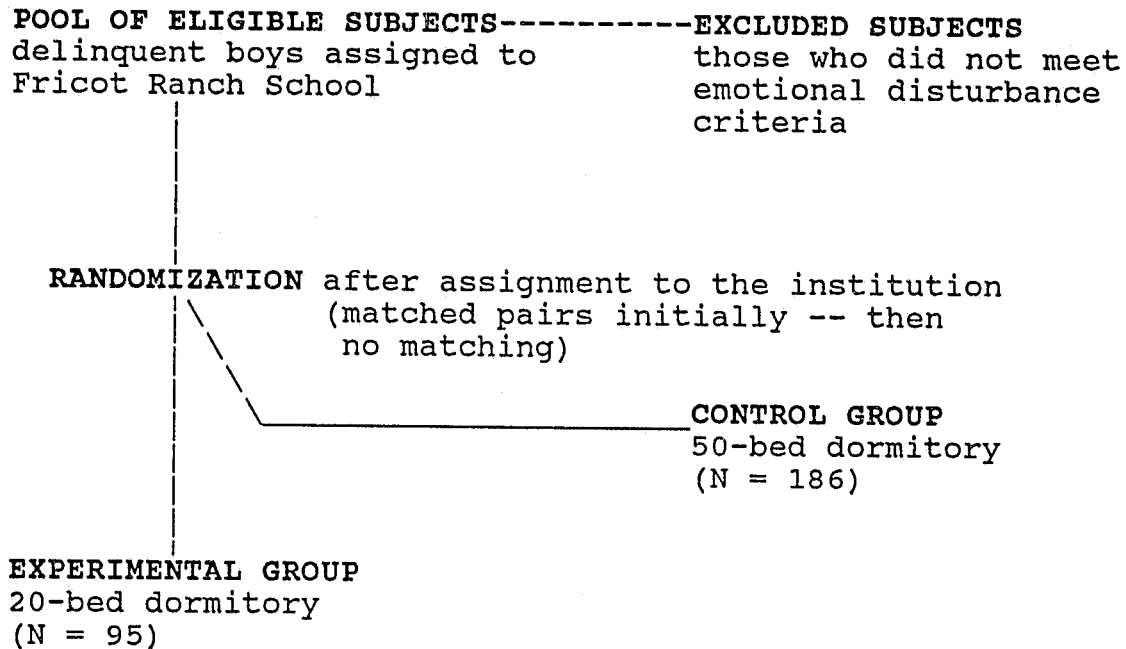
- 1) 45% of the suspects were unmarried male lovers (35% were current husbands);
- 2) 59% of the suspects had been arrested before;

Minneapolis Domestic Violence Experiment (cont.)

- 3) unemployment rates for suspects and victims were both 60%
- 4) approximately 17% of the suspects and victims were Native Americans; and
- 5) there were a large number of racially mixed marriages.

"B" Experiments

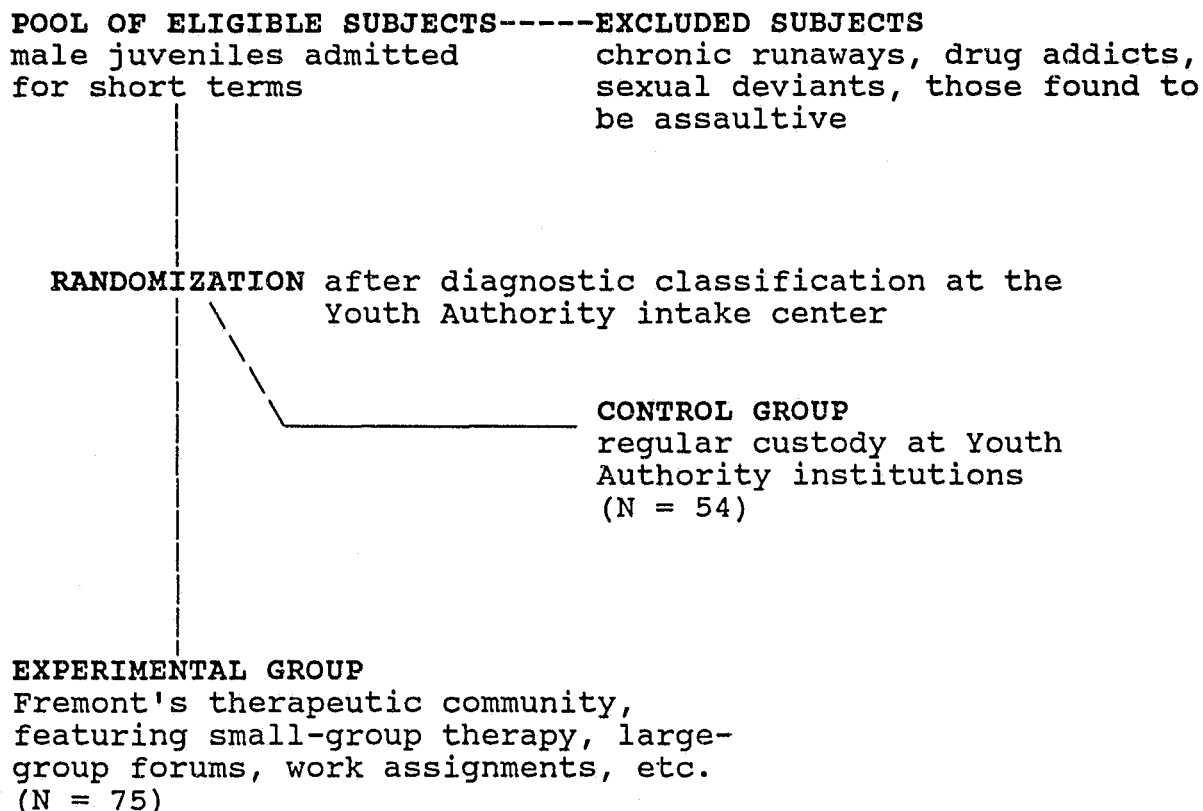
(1957) Fricot Ranch Delinquent Dormitory Experiment - B



Notes

Concern that the treatment conditions for the two groups might have been more alike than different prompted an analysis of the number of contacts made between the subjects and the staff. Charts were kept for a two-year period. Any interaction between staff members and subjects of five minutes or more was recorded. The PI reported that those in the experimental group had nearly five times as much contact with the staff as did those in the control group.

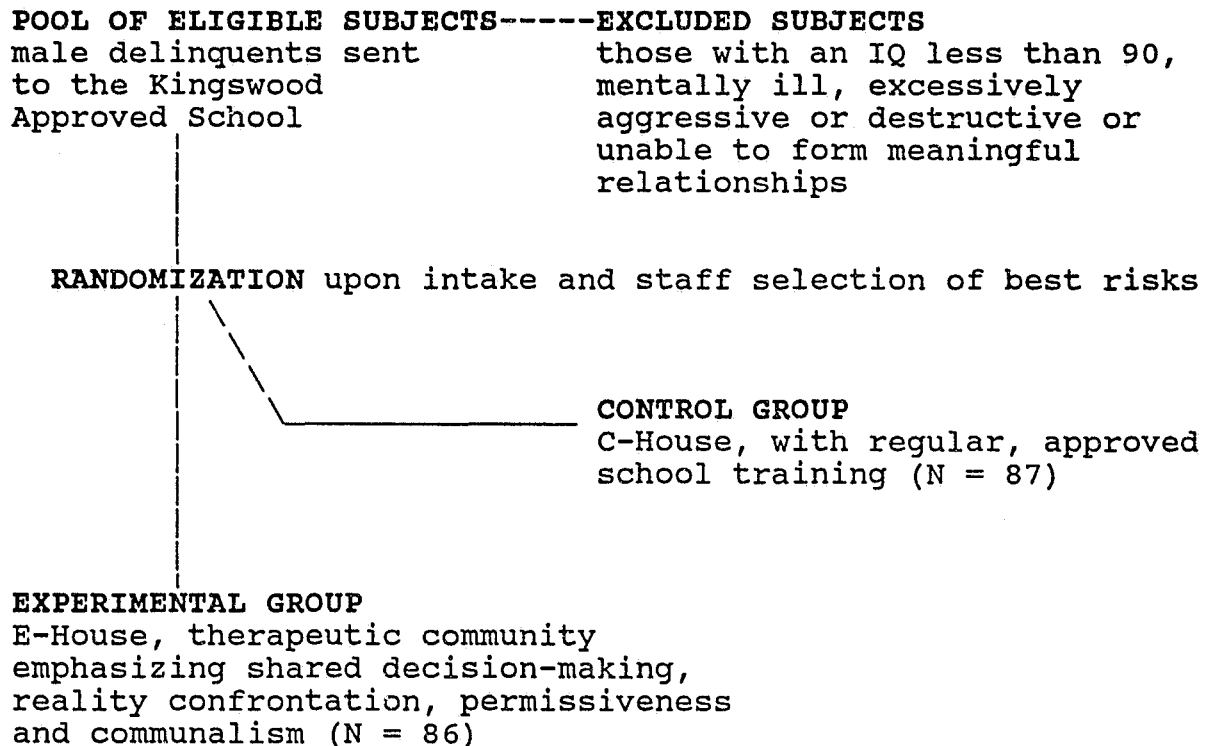
(1961) California Fremont Program Experiment - B



Notes

Though this experiment showed no statistical difference between the groups on the outcome measures, the exposure times were statistically different for the two groups. The control group was exposed to regular custody for an average of 9 months, while the experimental group stayed at Fremont 5.8 months on average. The PI did not attempt any statistical control holding exposure times constant.

(1965) English Juvenile Therapeutic Community Experiment - B



Notes

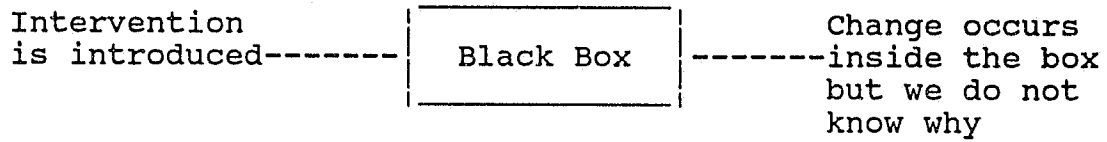
The daily operation of a prison interferes with program evaluation, according to the experimenters. The PIs stated that institutional goals do not include experimental therapy -- custody and security remain the main emphasis (Clarke and Cornish, 1972: 20).

Caseflow problems developed when judges became hesitant to send juveniles to Kingswood for fear they would be randomly assigned to the control condition and not get the treatment they needed.

The PIs maintained that if a statistically significant difference would have emerged, it would have been difficult to determine the causal relationship of the change because of the

English Juvenile Therapeutic Community Experiment (cont.)

changing staff and environment (e.g., the innovations and improvements in the E-House) . This has been referred to by Twain (1980), among many others, as "black box intervention," where:



(1966) California Preston School Typology Experiment - B

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS
boys institutionalized at the Preston School of Industry those with psychiatric treatment assignments or outside work assignments

RANDOMIZATION upon intake at the Preston School

CONTROL GROUP
assigned to one of 5 living units, regular institutional classification and treatment (N = 518)

EXPERIMENTAL GROUP
assigned to one of 5 living units, featuring I-Level Maturity classification and treatment which matches youth's personality to appropriate treatment (N = 655)

Notes

The Preston experiment did not state the percentage of subjects in each group who were transferred after randomization. Future experimental literature should be clear about explaining the loss of subjects from each group at every point in the study. To illustrate:

Ineligibles.....% of those in the general

California Preston School Typology Experiment (cont.)

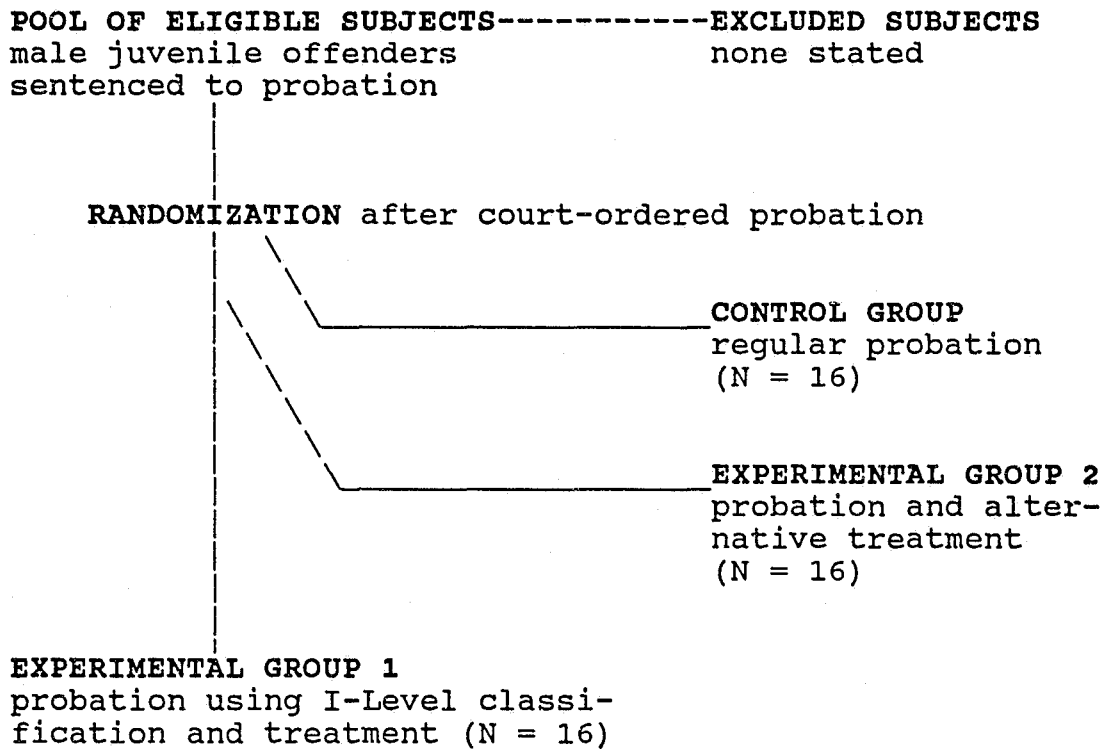
Ineligibles (cont.).....population who were refused
entry into the eligibility
pool

Eligibles not included.....% of eligible subjects who
in the study were not included in the study

Cases lost due to.....% of subjects who dropped out
attrition or were lost for some reason
after randomization occurred,
stated separately for each
group

Cases not included in.....% of subjects who were not
the analysis included in the final statis-
tical analysis, stated
separately for each group

(1973) Canadian I-Level Maturity Probation Experiment - B

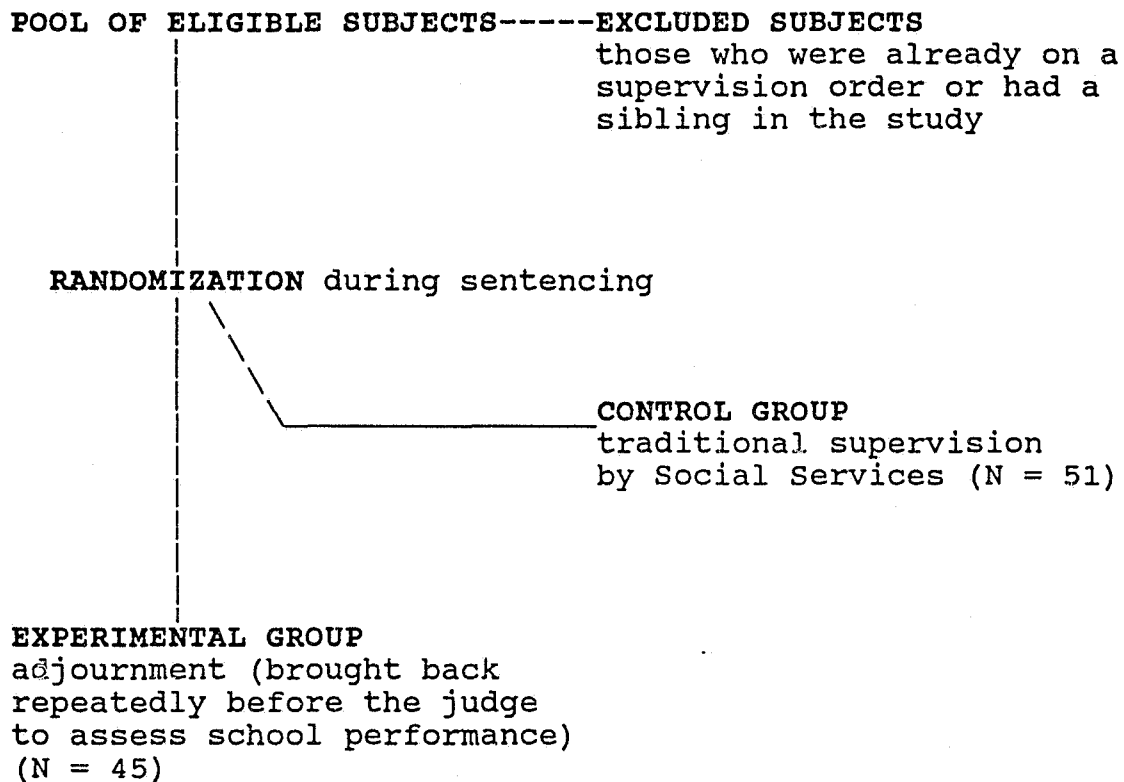


Notes

Recidivism was defined as any subsequent adjudication.

The intensity of the supervision was essentially similar for all groups, although the probation officers supervising the regular probation group did have larger caseloads since treatment was not emphasized.

(1975) Leeds (UK) Truancy Experiment - B



Notes

Overrides were not permitted -- but exclusions from the study by the magistrate were. These exclusions (and those that occurred prior to randomization were not included in the statistical analysis. Three covert overrides (boys assigned adjournment who received supervision) were reported.

This experiment was kept secret from the court and supervision staff until its completion.

"C"
Experiments

(1951) Copenhagen Short-Term Offender Experiment - C

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS
jailed male inmates with none stated
at least 6 months remain-
ing on their sentences

RANDOMIZATION during incarceration

CONTROL GROUP
incarceration with regular
custodial services available
(N = 126)

EXPERIMENTAL GROUP
incarceration with special short-term
counseling and treatment provided (N = 126)

Notes

The treatment services were rendered in an unstructured manner; there was no uniform or consistent pattern of treatment given to the experimental subjects.

The randomization process appeared to have been upheld, highlighting the conflict with using experimental research in the prison. Since the population is a captive one, threats to the experimental design are better controlled than in a less restrictive environment. The ethical question of using inmates in an experiment against their will or without their consent, however, becomes one which is important to address.

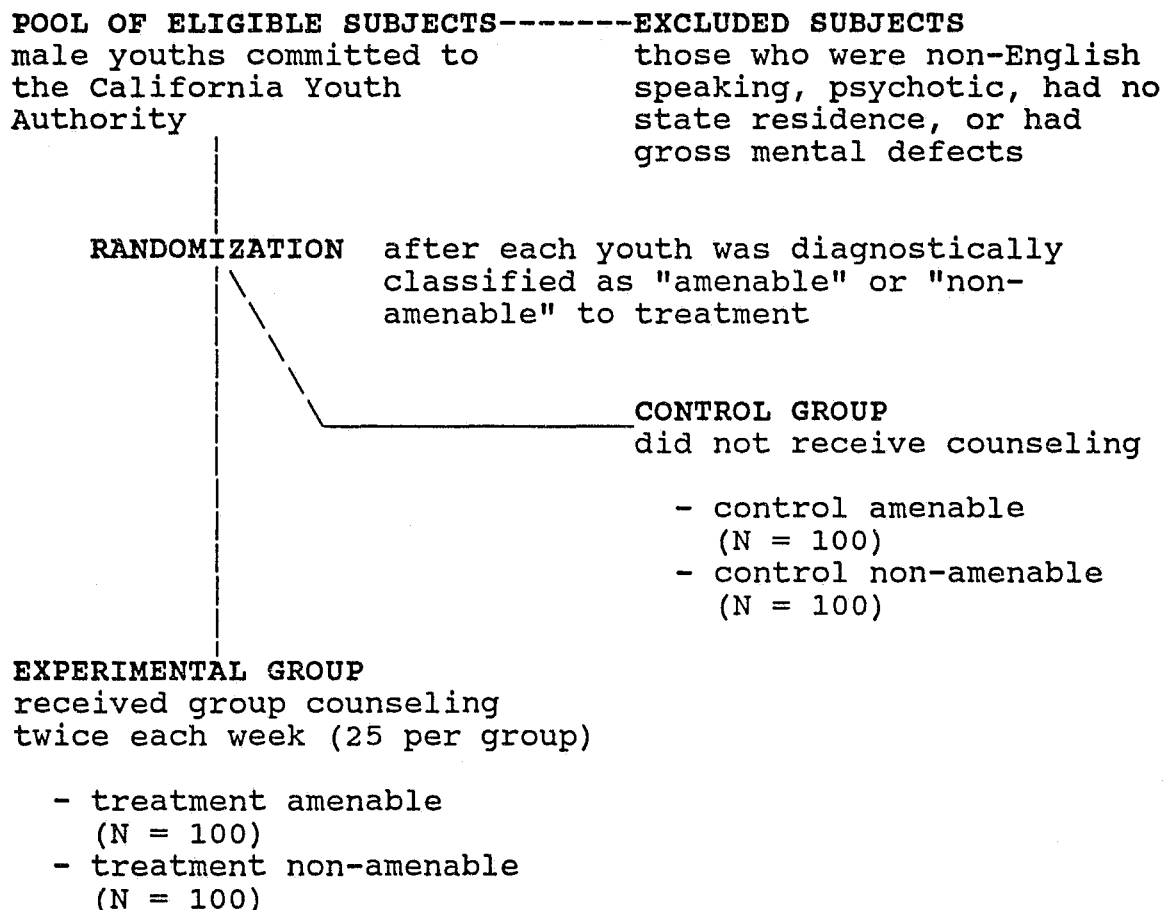
The Principal Investigators developed their own categories of post-treatment recidivism:

	<u>E</u>	<u>C</u>
no subsequent convictions	41.3%	31.7%
fine/detention	17.5%	10.3%
prison sentence	36.5%	52.4%
special measures	4.8%	5.6%

Copenhagen Short-Term Offender Experiment (cont.)

The PIs dropped the fine/detention category in their analysis and found a significant difference in failure rates between the two groups ($p < .01$). Cornish and Clarke (1975) argued that inclusion of the fine and special detention group weakens the PIs' findings.

(1955) California Pico Experiment - C



Notes

The classification of subjects into those who were amenable and non-amenable was done using pooled clinical judgments.

Adams (1970) found a significant effect for the treatment amenable group in comparison with the other three groups, leading to his conclusion that better diagnostic methods, combined with differential treatment, are needed to improve treatment success and have an impact upon post-release behavior.

When the experimental and control groups are looked at without considering amenability to treatment, there is no difference between them in post-release recidivism.

(1959) California Short-Term Psychiatric Treatment Experiments - C - (includes two experiments) **

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS
juvenile males incarcerated none stated
at the Preston School of
Industry or the Nelles School
for Boys who were diagnosed
with psychological problems

RANDOMIZATION during institutionalization

CONTROL GROUP
regular custodial programming
- Preston (N = 109)
- Nelles (N = 61)

EXPERIMENTAL GROUP
psychiatric treatment,
consisting of individual
therapy in conjunction
with regular custodial
programming
- Preston (N = 106)
- Nelles (N = 62)

Notes

No statistically significant difference was noted at the Preston School -- despite an 11% difference between the experimental and control outcomes. This finding provides support for the argument that, by increasing sample size, one increases the chance that a statistically significant effect will be found if one exists.

A backfire effect was noted at Nelles, which supports the contention of Bergin et al. (1968) that psychotherapy can have harmful effects. The PIs noted that the integrity of the treatment might have been negated by staff morale and waning levels of motivation supporting the experiment.

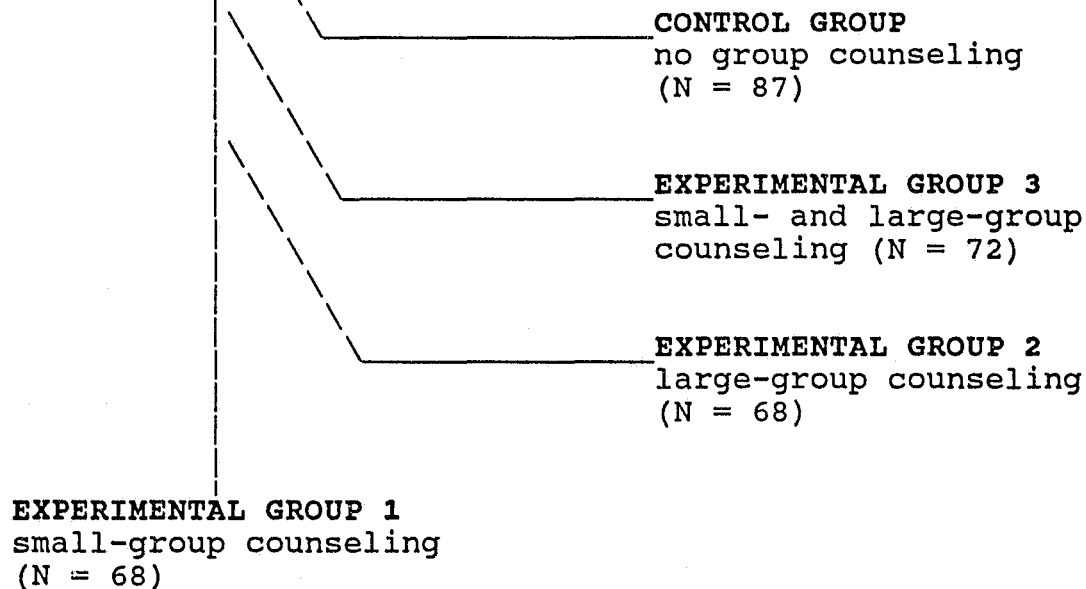
** Each of the experiments discussed here was analyzed separately in the data base ("SPSS Data Set - Experiments in Sanctions").

(1960) California Paso Robles and Youth Training School Group Counseling Experiment - C - (includes two experiments) **

Paso Robles

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS
juveniles institutionalized none stated
at Paso Robles

RANDOMIZATION during incarceration to one of four cottages



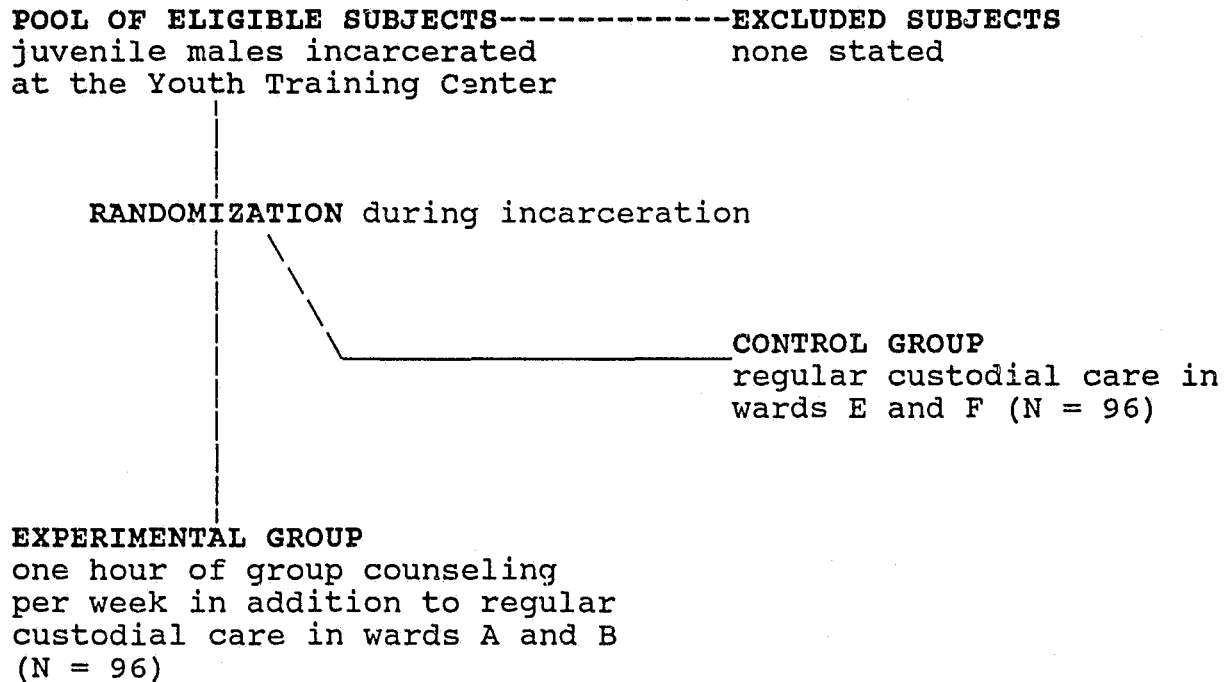
Notes

There was a problem with attrition throughout the project, with 437 subjects originally assigned to the 4 groups but only 295 left in the final analysis. This was due both to transfers within and out of the institution.

** Each of the experiments discussed here was analyzed separately in the data base ("SPSS Data Set - Experiments in Sanctions").

California Paso Robles and Youth Training School Group Experiments (cont.)

Youth Training School



Notes

The PI stated that there were 239 subjects left in the sample following attrition loss and exclusions (Seckel, 1965: 5). The analysis later included only 192 subjects (p. 15).

(1961) California Group Counseling Prison Experiment - C

POOL OF ELIGIBLE SUBJECTS-----**EXCLUDED SUBJECTS**
male inmates housed in medium security prison those over 65 years, arsonists, psychotics, those with disciplinary problems, or terms over 3 years or under 6 months

RANDOMIZATION during imprisonment **

CONTROL GROUP 1
"mandatory" group
(N = 270)

EXPERIMENTAL GROUP 1
"mandatory" large-group counseling (N = 69)

EXPERIMENTAL GROUP 2
"mandatory" small-group counseling (N = 173)

Ethical and practical considerations resulted in the PIs allowing the inmates who refused their respective assignments to enter "voluntary" groups.

CONTROL GROUP 2
"voluntary" group
(N = 176)

EXPERIMENTAL GROUP 3
"voluntary" small-group counseling (N = 278)

** The number of cases shown here is slightly higher than the number of cases recorded in the SPSS data set "Experiments in Sanctions" because cases with missing information on the outcome variable coded there have been included here if their group assignment is known.

California Group Counseling Prison Experiment (cont.)

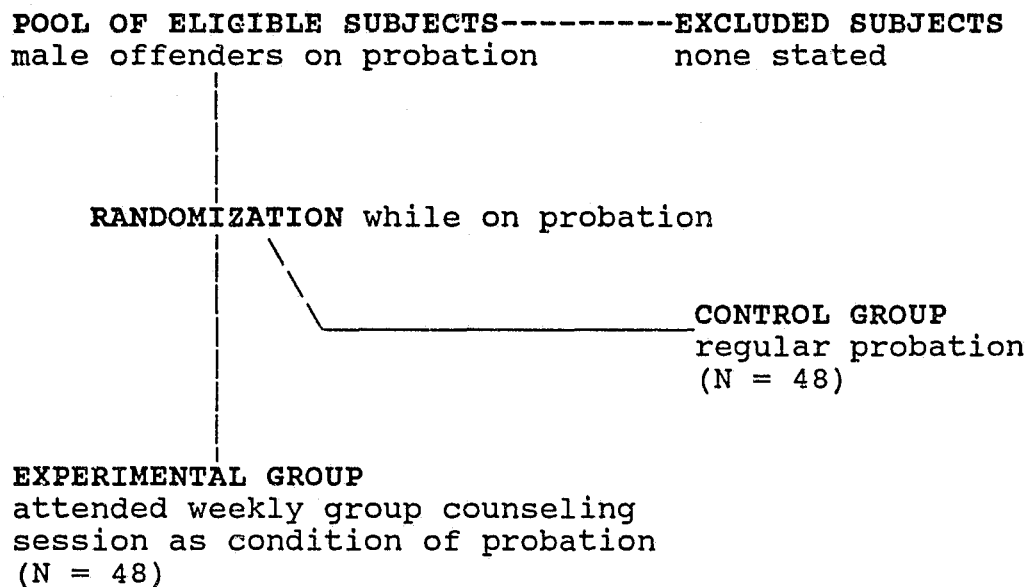
Notes

One of the problems with group counseling is noted by the PIs and echoed by Dennis (1988) -- promoters of group counseling give no reason why it should have an impact upon subsequent recidivism.

In this experiment corrections officers were trained as the counselors, leading Quay (1977) to speculate that group counseling may not have been adequately tested here -- since the staff was poorly trained and did not believe in counseling methods to begin with.

The PIs questioned using a one-year follow-up. Although many offenders will have failed in the one-year period, the lag time in paperwork will inhibit these failures from appearing in official records within that time.

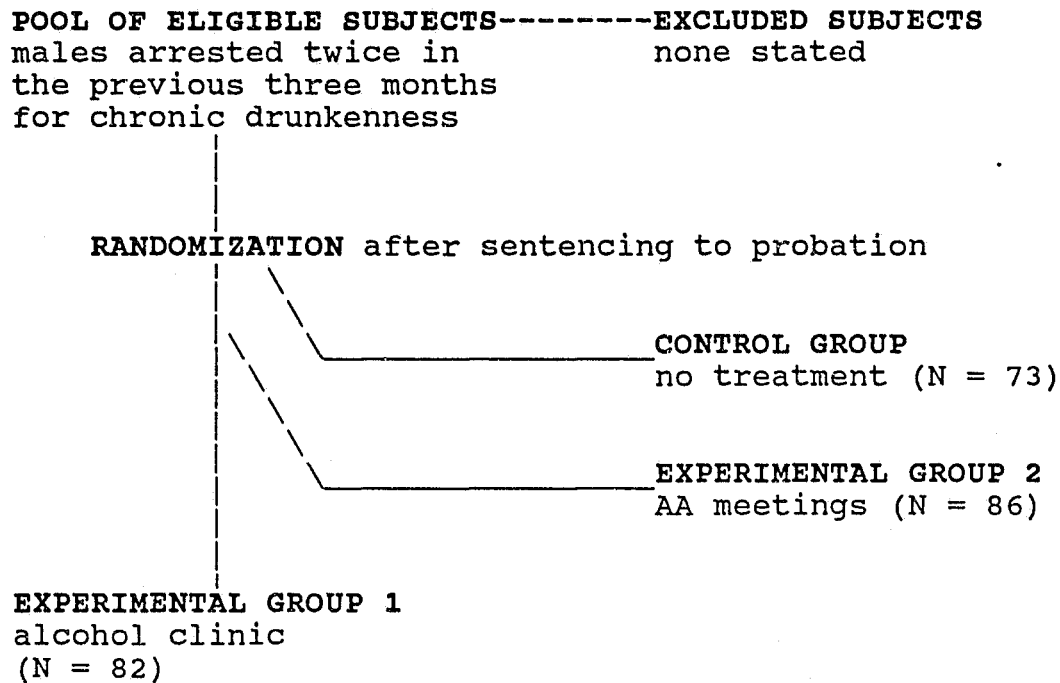
(1963) California Juvenile Probation and Group Counseling Experiment - C



Notes

Coercive control of the experimental intervention was lacking; subjects were resistant to attending the meetings. Warnings were issued that those who failed to attend faced incarceration at Juvenile Hall. Adams (1965) argued that "shock incarceration" should be used to enforce treatment, a view which runs counter to the prevailing argument that treatment is more effective when delivered to voluntary subjects.

(1964) San Diego (CA) Chronic Drunk Offender Experiment - C



Notes

Coercive conditions were operating here since participation in the treatment was a condition of probation and failure to comply would have resulted in an arrest warrant being issued.

Even when the Alcoholic Clinic and Alcoholics Anonymous Groups are combined into one treatment group, the difference between the experimental and control conditions was not significant.

One criticism that can be offered is that one might not expect treatment directed toward alcoholism to radically affect total rearrest rates, the outcome measure used here. Perhaps an outcome measure consisting of subsequent arrests for drunkenness would have been more appropriate.

(1964) Kentucky Village Psychotherapy Experiment - C

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS
female juveniles none stated
institutionalized
at Kentucky Village

RANDOMIZATION during imprisonment

CONTROL GROUP
no psychotherapy
(N = 30)

EXPERIMENTAL GROUP
received weekly psychotherapy
sessions, 4 groups of 10 girls
each (N = 40)

Notes

The PIs used the percentage of time spent out of prison as one outcome measure. The three years prior to the experiment were observed as well:

	% time in prison	
	3 years before experiment	1 year after experiment
Experimental	18%	45.6%
Control	22%	60.0%

Both groups did much worse after the experiment, with those in the psychotherapy group worsening at a lower rate than those in the control group.

(1964) Fairfield School for Boys Experiment - C

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS
institutionalized juvenile none stated
males at the Fairfield School
for Boys

RANDOMIZATION of eligible subjects who had been matched
on socioeconomic and other variables
during institutionalization

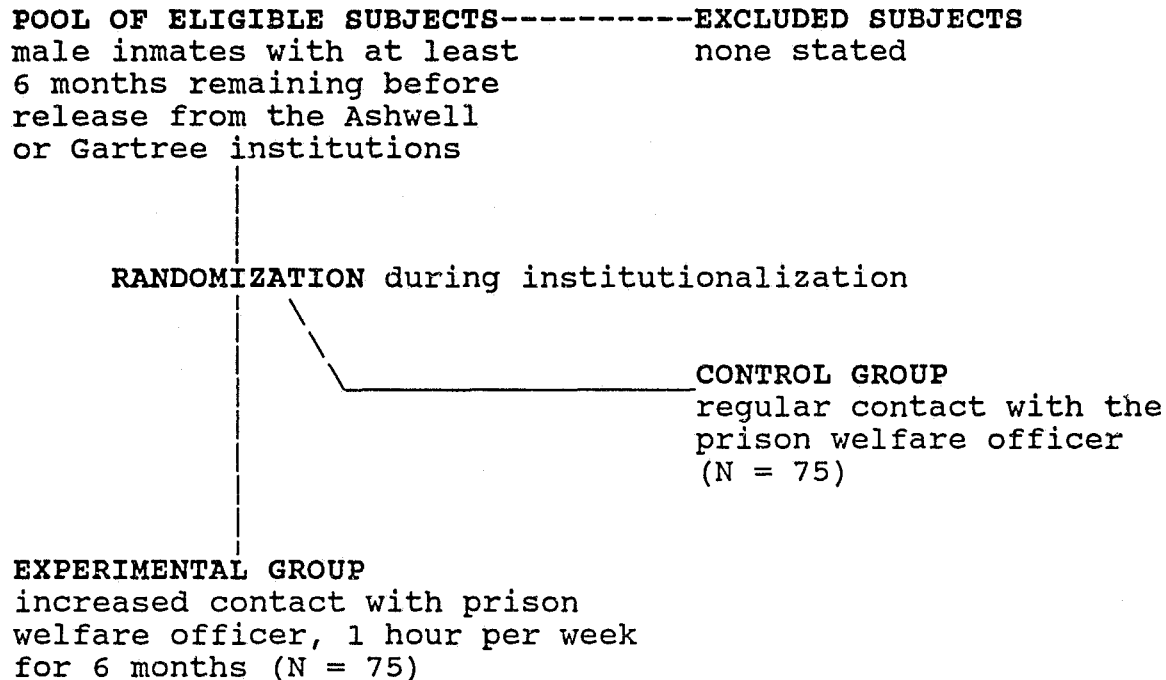
CONTROL GROUP
received regular custodial
care with no psychotherapy
(N = 41)

EXPERIMENTAL GROUP
received psychotherapy while in
custody (Therapy sessions, lasting
1.5 hours, were held twice per week
and continued for a total of
20 weeks.) (N = 41)

Notes

Do institutional measures provide stable predictors of subsequent recidivism? This experiment supports the contention that parole officers -- and others -- should take such measures into account. Persons (1966) initially examined institutional infractions and the time elapsed before passes for free movement as outcome measures, finding significant effects in a positive direction for the psychotherapy group. These results were substantiated in outcome measures of post-release adjustment ("community adjustment"), with the psychotherapy group again performing significantly better than the control group.

(1968) English Prison Intensive Social Work Experiment - C



Notes

One criticism of the study is that there was no explanation given of why the increased contact with the prison welfare officer should have an impact on post-release behavior.

During the first 8 months, no statistically significant difference between the groups emerged. In the 8-to-12-month follow-up, a statistically significant difference between the groups appeared, favoring the experimental group.

Ohio Juvenile Probationer Behavior Modification Experiment
(cont.)

increased and intensive support services of other types (e.g., group counseling, psychotherapy, vocational and educational training).

(1971) Tacoma Juvenile Inmate Modeling and Group Discussion
Experiment - C

POOL OF ELIGIBLE SUBJECTS-----**EXCLUDED SUBJECTS**
male delinquents sentenced to Cascadia Reception and Diagnostic Center none stated

RANDOMIZATION upon intake into the center

CONTROL GROUP
no modeling or discussion
(N = 64)

EXPERIMENTAL GROUP 1
two psychology students served as models for optimal behavior which was performed in front of a class of 4-5 subjects -- discussion with the group followed (N = 64)

EXPERIMENTAL GROUP 2
groups discussed the optimal-behavior scenarios but did not witness any modeling (N = 64)

Notes

The assignment process was described as "essentially random; occasionally influenced by weekly admission rates" (Sarason and Ganzer, 1973: 443). When the experimental groups were full, new admissions were assigned to the control group. This "essentially random" process was enough to cause Farrington (1983) to drop the study from his review of criminal justice experiments.

Treatment duration was 4 weeks, with 4 sessions held per week for a total of 16 sessions.

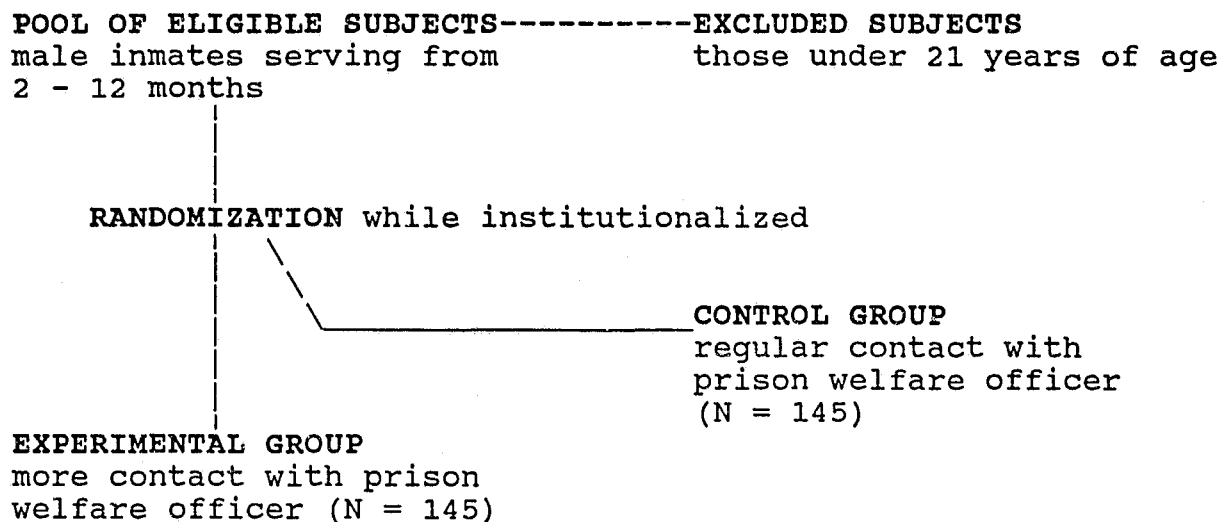
Tacoma Juvenile Inmate Modeling and Group Discussion
Experiment (cont.)

Recidivism was defined by the PIs (1973: 447) as:

- a) a return to a juvenile institution;
- b) conviction in Superior Court resulting in adult-status probation; or
- c) confinement in an adult correctional facility.

The control group did participate in a number of educational, vocational and recreational activities at the center, described by the PIs as a "good" institution (1973: 448).

(1973) English Intensive Welfare Experiment - C



Notes

It was determined from the outset that the Home Office research staff would not instruct the Prison Welfare Officers on the nature of the contacts, although they were told that they should make the experimental treatment as intensive as necessary (Fowles, 1978: 5).

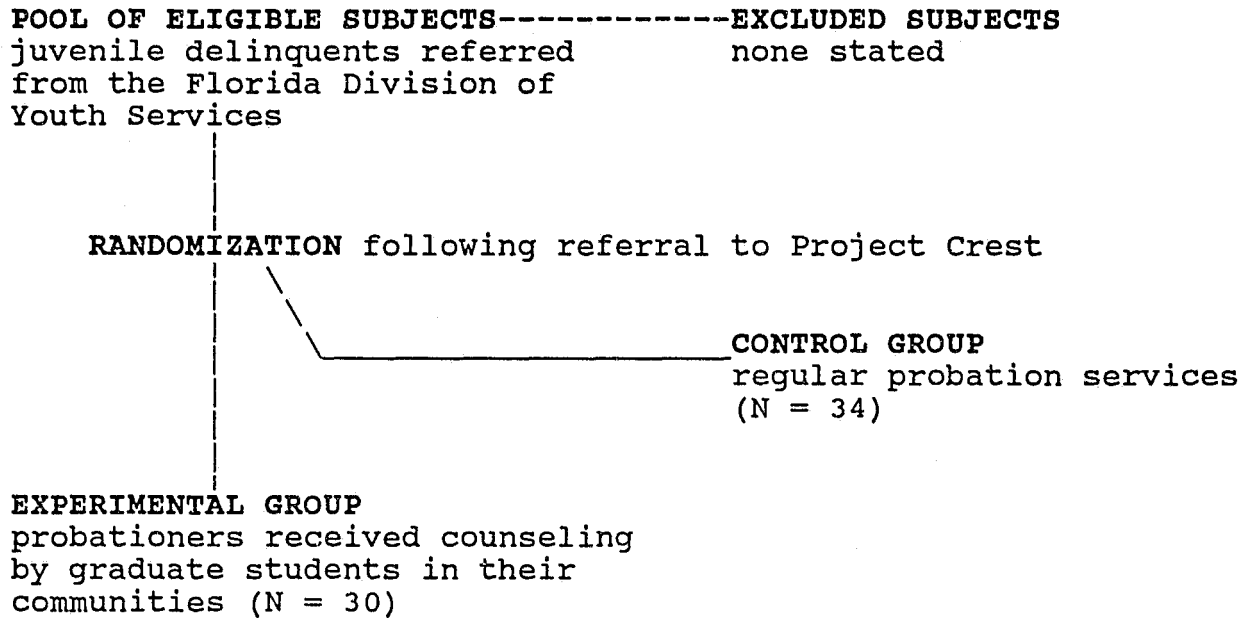
Prisoners could refuse the experimental treatment; they were not excluded from the final analysis.

Experimental prison welfare officers did have significantly more contact with their subjects than control officers.

There were potential "Hawthorne effect" problems, since everyone in the experimental group knew he was part of the study.

The ethical questions associated with having a treatment versus no-treatment experiment were by-passed because the experiment compared regular with more intensive contacts.

(1976) Florida Project Crest Experiment - C

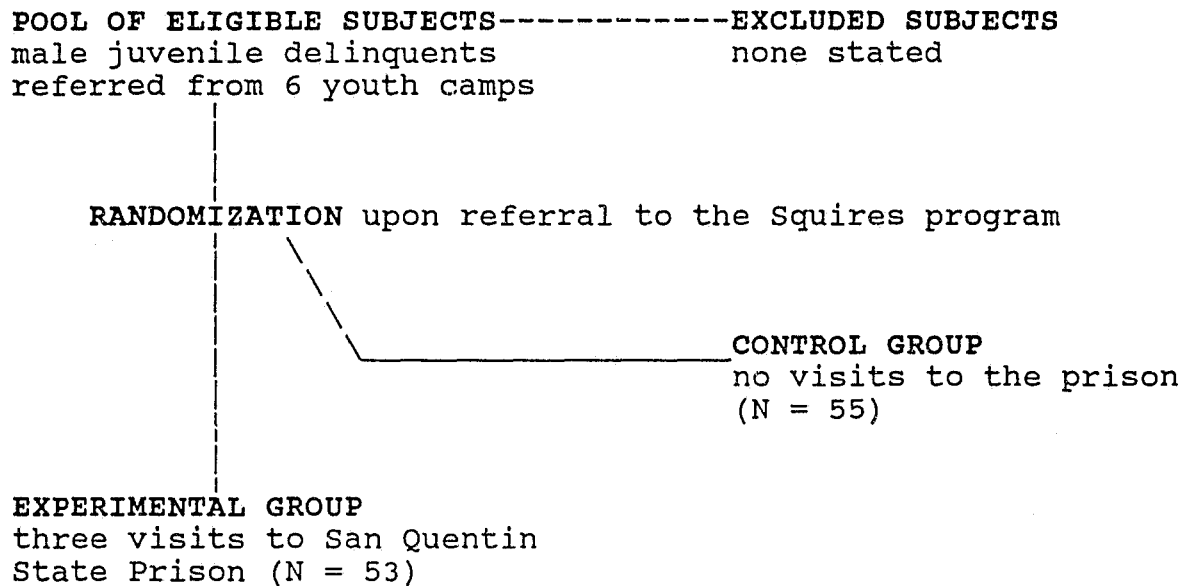


Notes

CREST is an acronym for Clinical Regional Support Teams. It was started in 1972 with LEAA funds. It was not designed to replace probation -- merely to supplement it.

A one-tailed t-test was used by the PIs. Even when the outcomes were reanalyzed with a two-tailed test, the difference between the groups was significant. The decision to use the one-tailed test can be criticized. The PIs assumed that the experimental effect would be in only one direction. Our work on backfires reveals that this may not be a viable assumption.

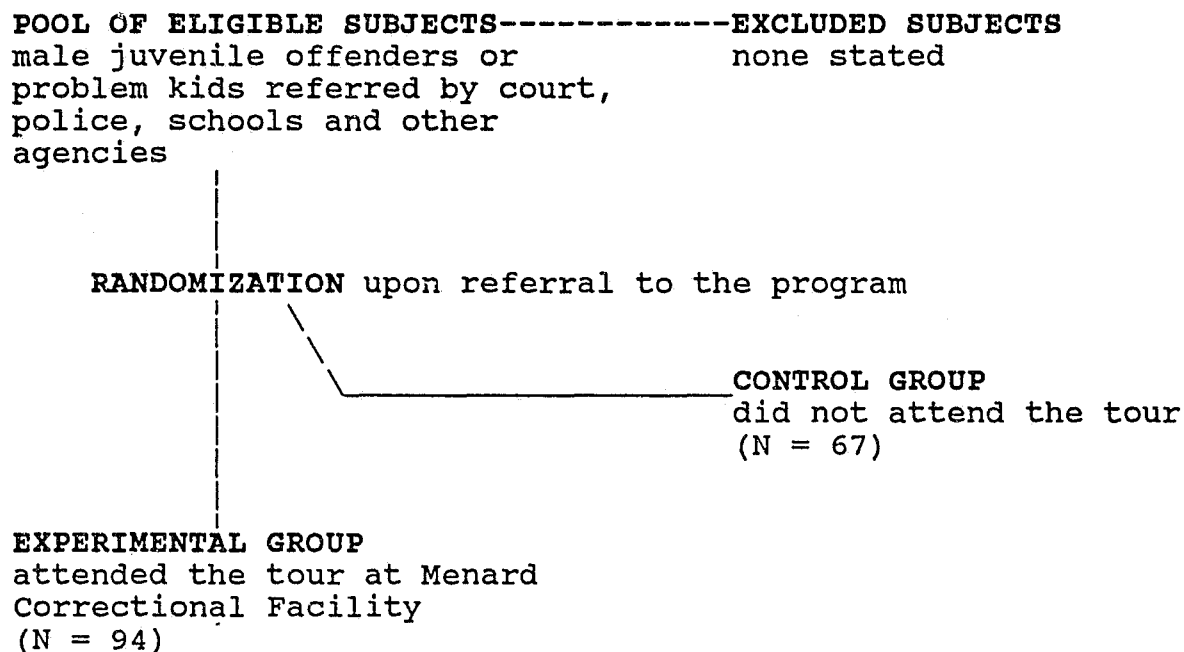
(1977) San Quentin (CA) Squires Program Experiment - C



Notes

Coercive control was maintained more effectively in this "Scared Straight" type of experiment. Attendance was made a condition of the youth camp sentence. Only 1 experimental subject failed to attend the tours.

(1978) Illinois Juvenile Tours Experiment - C



Notes

Three types of juveniles were referred to this tour:

- a) those petitioned to juvenile court;
- b) those contacted by police but not yet referred; and
- c) those never contacted by police.

There were problems in maintaining coercive control; no-shows and cancellations were common.

There were 6 different tour periods, with approximately 15 subjects attending each tour.

Follow-up data for only five months were available for the last tour; time-at-risk was lower for the last tour subjects than for the earlier tour subjects. However, time-at-risk was the same for subjects in the experimental and control groups.

(1978) Michigan Juvenile Offenders Learn Truth (JOLT)
Experiment - C

POOL OF ELIGIBLE SUBJECTS-----
male delinquents arrested or
petitioned for an offense that
would have been criminal if
committed by an adult

EXCLUDED SUBJECTS
females, status
offenders, those with
no legal guardian

RANDOMIZATION upon referral to JOLT

CONTROL GROUP
did not visit the
prison (N = 90)

EXPERIMENTAL GROUP
made two visits in one week to
the Southern Michigan State
Prison (N = 79)

Notes

There was a problem with enforcing attendance coercively --
58 subjects in the experimental group did not attend both visits.
Those who did not attend were dropped from the analysis of outcome
measures.

(1978) New Jersey Juvenile Awareness Program (Scared Straight)
Experiment - C

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS
juveniles referred from none stated
police, schools and social
agencies

RANDOMIZATION upon referral to program

CONTROL GROUP
did not visit prison
(N = 35)

EXPERIMENTAL GROUP
attended prison tour
(N = 46)

Notes

This experiment had severe randomization breakdown and has been characterized by the PI as a "quasi-experiment." When groups were analyzed as actually assigned and not as randomly assigned, the backfire effect was no longer demonstrated.

The kids that were referred were defined as "problem kids" and not as delinquents (as in the Michigan JOLT experiment).

(1981) Ramsey County (MN) Community Assistance Program
Experiment - C

POOL OF ELIGIBLE SUBJECTS-----EXCLUDED SUBJECTS
misdemeanant offenders none stated
brought before the Ramsey
County, Minnesota,
Municipal Court

RANDOMIZATION after court referral, upon intake
into the unit

CONTROL GROUP
regular probation
supervision (N = 54)

EXPERIMENTAL GROUP
probation supervision
and services provided
by the Community Assistance
Project (CAP) (N = 124)

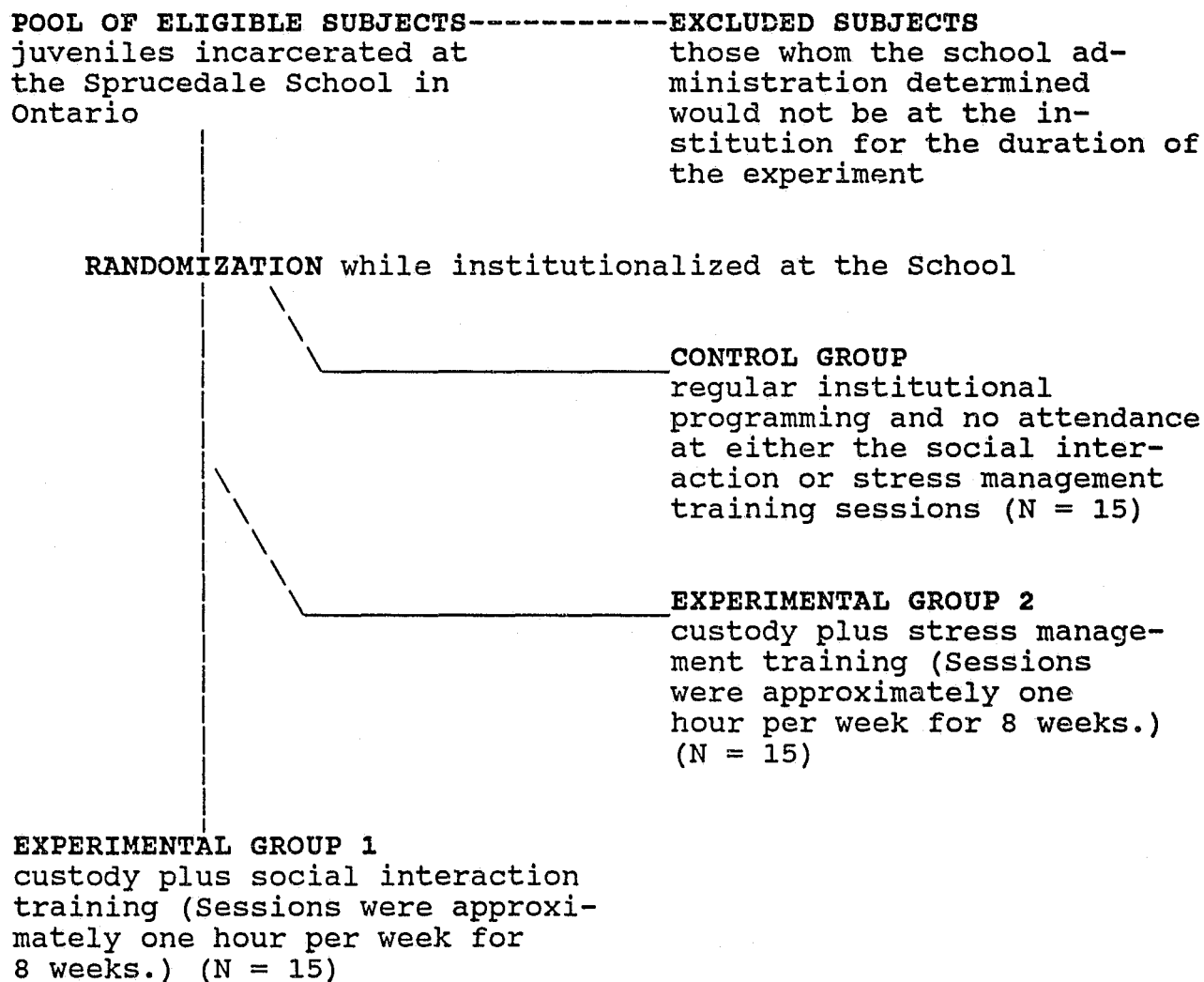
Notes

The cooperation of the judges was necessary for the experiment to be initiated and continued (Owen and Mattesich, 1987:5).

The experimental group was exposed to a wide range of CAP services, including group and individual counseling, educational counseling, financial management, vocational counseling and traffic offender assistance. Due to a lack of coercion, tremendous variation existed among experimental subjects in the types of treatment services received (with some subjects dropping out altogether).

(1984) Ontario (Canada) Social Interaction Training Experiment

- C



Notes

This experiment highlights a potential source of conflict over the outcome measures coded for the data base in the present analysis. While the Weisburd-Sherman design used here concentrates on outcome measures of criminality (in most cases recidivism), other crucial outcome measures might better serve the investigation of deterrence. Perhaps the MMPI and other psychological measurement tests provide better or more scientific evidence of behavioral change than the official reports.

Ontario (Canada) Social Interaction Training Experiment (cont.)

Moreover, this experiment reports a follow-up period of 12-15 months. No standardized follow-up is reported -- though one might assume that everyone in the sample was followed for at least 12 months. It may be that one group (perhaps the no-treatment controls) were predominantly followed for 15 months while subjects in one of the experimental groups were followed for 12 months. Differential periods of months at risk could be the explanatory variable for any differences found between the groups and not the treatments themselves.