PROJECT CHIP:

A STUDY OF JUVENILE RECIDIVISM

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ABSTRACT

The evaluation of CHIP (Children in Need of Supervision Intervention Project) had three objectives: 1) to discover how the rate of recidivism for project clients compared with that of CHINS offenders who were not diverted, 2) to measure the degree of change in that rate over time, and 3) to identify factors correlated with recidivism. A fifty-percent random sample of CHIP cases, drawn from the universe of CHIP cases completed through May 31, 1977, served as the experimental (treatment) group. The control group was drawn from CHINS cases which were given other informal dispositions by the Juvenile Probation Office (JPO).

Data on the background of the subjects—e.g., area of residence, sex, ethnicity, income, family constellation, prior offense record, previous detention in the Juvenile Home and offense charged—revealed some interesting and important differences between the two samples. The foremost contributors to recidivism were prior record and offense charged, both of which also affected the <u>difference</u> in post-treatment sucess rates of the two groups studied.

Analysis of post-treatment outcome revealed some confounding effects of prior record and high-risk offenses. CHIP had a slightly higher recidivism rate than the control group, primarily because of the greater proportion of recidivism-related factors among CHIP clients. Despite this, 67% of the CHIP cases avoided

subsequent contact with juvenile authorities, a notable figure in light of typically

cited juvenile success rates of 25 to 50%.

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INTRODUCTION

Dealing effectively with CHINS (Children in Need of Supervision) offenders is a major problem of the juvenile justice system. The kinds of offenses with which CHINS are usually charged—running away from home, incorrigibility, truancy, curfew violations, and other status offenses—exacerbate the already weighty and taxing supervision problem of traditional juvenile probation authorities. Moreover, traditional methods of handling CHINS cases is frequently inappropriate because the problems involved often derive more from family and home problems than from the commission of any criminal or delinquent act.[1]

Thus, we typically find that traditional juvenile programs are ill-equipped to deal with the unique and demanding problems of the CHINS offender. Lacking the resources to resolve this particular conflict, the CHINS offender may well find his adolescence punctuated with repeated trips to juvenile court, a condition which diminishes both the effectiveness of probation office services and the juvenile's prospects for successful adjustment.

A number of projects designed to refer these kinds of status offenders away from traditional dispositions and toward special programs providing intensive individual and family counseling have been introduced across the United States.

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[1]Baron, Roger and Floyd Feeney, Juvenile Diversion Through Family Counseling: A Program for the Diversion of Status Offenders in Sacramento County, California, 1976, p. 1.

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A premier program of this type is the Sacramento "601" Project. The Children in Need of Supervision Project (CHIP), recently undertaken in Bernalillo County, is another.

The approach derives from the presumption that CHINS problems, frequently growing out of underlying conflicts with parents or family, are best explored and resolved in the context in which they emerged.[2] Projects using this approach provide counseling to the juvenile and members of his family during and after the conflict or crisis period in an attempt to resolve permanently the problem that caused the referral and to preclude any re-referral of the case to juvenile authorities.

Optimism of program proponents is guarded, however, as they are quick to note the special problems presented by CHINS cases.

> Families with children who run away or who become involved in incorrigible or beyond control behaviors frequently have subsequent problems. Family counseling is more likely than other methods to prevent these kinds of problems, but it is not a panacea. Even with the use of family counseling, therefore, it can be expected that there will be a sizeable number of repeat problems.[3]

Illustrative of the point, a recent study of the Sacramento Diversion Project found recidivism rates on the order of 50% for youths who were elegible for the project. (The counseling concept does appear sound, however, as project cases exhibited a lower recidivism rate (46.3%) than did those in the control group (54.2%).[4]

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[2] Baron and Feeney, op.cit.

[3] Baron and Feeney, op.cit, p. 24.

[4] Baron and Feeney, op.cit., p. 10.

Objectives

The research underlying this report was undertaken during the third year of operation of the Albuquerque CHIP program in an effort to determine the impact of CHIP on juvenile recidivism. This report, embodying the results of that analysis, is presented in three parts. The first describes the research design employed, sampling methodology, data collection techniques, and the rationale behind each. The second relates the socioeconomic characteristics of the two samples, focusing in particular upon the nature of the CHIP population and upon the disproportionate presence of various client attributes—particularly those which proved strongly related to post-treatment success or failure—in either of the two samples. The third engages a more sophisticated and detailed discussion of the comparative recidivism rates of the two samples, the magnitude of change—if any—in recidivism rates over time, and the ecological correlates of recidivism, independent of treatment.

METHODOLOGY AND RESEARCH DESIGN

The straight-forward purpose of this inquiry—i.e., the calculation of recidivism rates for CHIP clients—does not diminish the significance or difficulty of methodological issues which underlies the research enterprise. An investigation of this sort must encompass at least four independent considerations:

1. the sampling frames and sampling process;

2. the criteria for clients success and failure;

3. the time frame, as it relates to both sample selection and to the follow-up

interval;

4. analysis of factors contributing to or "explaining" (in the sense of statistical correlation) success and failure of clients.

Each of these dimensions warrants at least brief discussion.

A Caveat: The Problem of "Specification"

The ideal kind of design for comparing post-treatment effects of different treatment programs would, of course, involve either a matching design wherein experimental "twins" are assigned to different treatments, or a randomized assignment design wherein placement in the treatment groups is completely random. In either kind of design, it is reasonable and appropriate to attribute any and all differences in post-treatment outcome to the treatments themselves, and not to pre-treatment distinctions in clients, which may have pre-disposed certain types of clients to post-treatment failure, irrespective of the treatments to which they were assigned. The problem facing evaluation designs in which assignment to treatment has not been either purely randomized or scrupulously controlled derives from the strong probability that the best (or worst) "risks" have been disproportionately assigned to either the experimental or control group. The direction of the bias is not important; its very existence is sufficient to render one's analysis tenuous or altogether invalid.

Assume, for example, that there are various client-related attributes that are strongly correlated with juvenile recidivism—e.g., a history of referral(s) to juvenile authorities, lack of family cohesion and support, certain kinds of

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offenses (like being a runaway). To the extent that program administrators disproportionately assign—albeit inadvertently—the bad risks to one group or the other, they, in effect, "specify" the results of the experiment. If the bad risks appear more frequently in the control group, the treatment afforded the experimental group will appear all the more effective; on the other hand, if the bad risk clients are disproportionately represented in the treatment group, then the treatment will appear less effective than it actually is. In either case, the "findings" of the evaluation will be misleading.

This evaluation faces precisely the kind of problem discussed above. Assignment to treatment (CHIP) and control (JPO) groups was neither matched nor randomized. Indeed, referral of clients to the CHIP program is both a highly selective and a highly individualized process, administered principally by several JPO intake officers. The criteria for CHIP referral, being unstated, no doubt vary according to the individual decision-maker and, perhaps, over time, as well. Consequently, the problem of statistical "specification" becomes particularly acute.

In ex post facto research designs like this, the specification problem is handled in several ways. First, the researcher selects his control and experimental samples in a fashion that assures a reasonable degree of similarity. Second, the researcher must seek to identify those factors which appear to correlate strongly with client failure. Third, he must determine whether such factors are disproportionately represented in either the treatment or control samples. When such biases are discovered, it is important to expose them, and, where sample size

permits, to statistically control for (eliminate) their effects.

Sampling Frame and Sample Selection

The first task involved selecting samples to represent the project (CHIP) and non-project populations. Since the aim of this study was a comprehensive evaluation of CHIP client success, it was desirable to include clients from all years of CHIP program operation. Such a data base would afford an opportunity not only to assess recent program effects, but also to determine changes in program effect over time.

At the same time, it was necessary to have at least a six-month follow-up period for all cases included in the analysis. Since data collection began in mid-December, 1977, the cut-off date for the follow-up period was set at November 30, 1977; anyone referred to CHIP or the Juvenile Probation Office after May 31, 1977, therefore, was not eligible for inclusion in either sample, since such inclusions would have precluded a full six-month follow-up.

The CHIP file of inactive cases (i.e., not currently under supervision or counseling) maintained at the CHIP program office provided the sampling frame for the CHIP sample. A 50 percent random sample of that file yielded 115 cases that had been referred to CHIP between June 1, 1975 (the approximate time of the program's inception) and May 31, 1977.

Selecting a control sample of JPO clients not referred to CHIP was more difficult. The aim in selecting such a sample from the thousands of annual referrals to the Juvenile Probation Office is to include those individuals who, while not

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referred to CHIP, might just as easily (or likely) have received a CHIP referral as the disposition actually received. On the recommendation of the juvenile intake officer responsible for a substantial number of CHIP referrals, the control sample was limited to referrals who were disposed of without a petition being filed or official action being taken—those who were "counseled and released," "dismissed," "warned or adjusted," placed under "informal supervision," or otherwise informally disposed of. At the same time, the control sample was limited to cases referred to JPO between June 1, 1975, and May 31, 1977, to make the two samples co-terminous. A group of 174 individuals meeting these selection criteria was randomly chosen from Juvenile Probation Office referral files.

Recidivism: Measuring Success and Failure

In an inquiry like this, it is clear that the definition of "recidivism" is of crucial significance. Questions of the appropriateness of various criteria to the definitions of success and failure and the operationalization of those criteria, therefore, become paramount. Consequently, the issue merits discussion.

The definition of "failure" must satisfy the requirements of both validity and reliability. To be valid, the quantitative definition of the concept must conform with general usage of the term; that is, does the researcher's operational definition of "failure" conform with the practitioner's intuition of what constitutes failure? Is the substantive referrent accurately depicted by the quantitative definition? As we shall see, the problem of criterion validity is not as simple as it may first appear. Indeed, one of the principal complaints regarding criminological

evaluation studies is that the criteria which have been employed to measure post-treatment success and failure are frequently invalid—either that they do not successfully and accurately measure what they purport to measure, or that the multiplicity of criteria used are not comparable. Popular definitions of recidivism, for example, range from re-arrest to re-conviction after initial contact with the justice system. More sensitive measures, aimed at gauging the extra-judicial impact of intervention programs, have focused on non-crime-related behavior, e.g., attitudinal and psychological adjustment, employment and family stability, abstention from alcohol. The point is not that one or the other approach is correct; each may be appropriate. The significance of the observation lies, instead, in the unsettling realization that the designated "success" or "failure" of a program may be made to turn on one or another researcher's definition of client "recidivism."

Since the utility of an inquiry like this turns largely on the validity which justice officials are willing to impute to its measures, we conferred with several such officials regarding the propriety of various methods of defining juvenile "recidivism." Inasmuch as the ultimate aim of the CHIP program is to successfully and permanently divert juvenile offenders (primarily "status offenders") from contact with official juvenile authorities (namely, the Juvenile Probation Office), it was agreed that the appropriate measure of client "failure" (for both the experimental and control groups) would be a "re-referral" to the Juvenile Probation Office.

It is important to note that other criteria might have been used. For example, since records of juvenile behavior and misbehavior are maintained by a

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number of agencies other than JPO—e.g., the Juvenile Division of the Albuquerque Police Department, the Bernalillo County Sheriff's Office, and the Family Resource Center, among others—one might have designated post-treatment referral to any of these juvenile control agencies as a kind of client "failure."

Such a strategy was deemed inadvisable for several reasons. First, it would have exacerbated an already extensive data collection task. Second, it would have required filtering through hundreds of inappropriate and perhaps groundless referrals. Third, if the case derived from sufficiently serious misbehavior, it ultimately would have been brought to the attention of juvenile authorities and found its way into official JPO files, anyway. Finally, in a very real sense, as long as juveniles do not return to JPO caseloads, they may properly be thought to have been successfully diverted or treated. Thus, the JPO file of cases referred provides a valid and appropriate touchstone of client success.

The second measurement issue—reliability—is closely related but not identical to the notion of validity. The latter has to do with whether we are actually measuring what we purport to measure; the former relates to the consistency and accuracy of the actual measurement. In short, the question of reliability is concerned with whether our standard and manner of measurement yield the same result each time we take a measurement. Thus, the technique of measurement is said to be "reliable" if and only if repeated measurement of the same or identical items (i.e., clients) yields identical scores (i.e., conclusions about individual client success or failure).

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Having already resolved the validity issue in favor of the JPO referral files, as discussed above, the research team subsequently concluded that this source of data would provide the most reliable measure of success and failure, as well. The chief intake officer of the JPO conspicuously (in red ink) stamps a juvenile's file when he is initially referred to JPO authorities for action. Such a disignation will appear in a juvenile's file each time he is referred or re-referred to the JPO. The designation itself also reflects information about the nature and date of the instant referral, the number of prior referrals to the JPO, prior case disposition, and recommended action for disposition of the instant case. Thus, the measurement of recidivism becomes a task of finding a red box which reflects a referral date subsequent to the date indicated in the box representing the instant referral. This method of measuring recidivism was deemed not only reliable, but easily replicated.

To this point, we have discussed the nature of the data sources upon which this analysis is based, the rationale underlying their selection, and the logic which attended the definition and measurement of recidivism. Now it is useful to review precisely how—and what—data were selected.

The Data Collection Task: Method and Focus

Because data had to be retrieved from two sources, the collection task was bifurcated. On the one hand, the CHIP sample was drawn from CHIP files and information recorded for that sample. Then, names of CHIP clients were cross-checked against JPO files to discover whether those cases had been formally

referred to the JPO subsequent to their involvement with CHIP. At the same time, a control sample, comprised of juveniles who were referred to the JPO between June 1, 1975, and May 31, 1977, and who subsequently received one of the "informal" dispositions mentioned earlier, was also identified. The follow-up methods and evaluative criteria were, of course, identical for the two groups.

The research team encountered a number of problems at this point. First, the data available from the two sources were not comparable. CHIP files were clearly oriented toward family and background characteristics, while the information in JPO records tended to focus heavily on offense, priors, and case disposition. This incomparability of data precluded much important cross-sample analysis and comparison (based on factors like family size, stability, and support; income and employment of parents, and so on). This limitation did not, however, preclude our tabulation and analysis of such factors within either the CHIP or the JPO sample; this latter kind of analysis was conducted, and the results are reported below.

Second, data from each source were incomplete. While we encountered no problems with respect to crucial variables like prior referrals, recidivism, and dates thereof, information regarding a number of practically and theoretically significant factors (like grade in school, number and ages of siblings, age and income of guardians, location of residence) was incomplete; many cases, in fact, reflected little or no information other than the nature and dates of referrals.

Third, perhaps the most troublesome collection problem derived from the incompleteness and inaccuracy of information contained in the JPO "blue

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card" file. The blue cards are meant to provide an historical synopsis of each

case ever referred to JPO, including among other things, information about the

nature, date, and disposition of each referral. A unique identifier on each blue

card refers the investigator to a complete "jacket"—containing investigations,

police and probation reports, petitions, and the like—which is maintained for

each juvenile. A cross-check of blue cards and corresponding juvenile files

("jackets"), however, revealed that information on the blue card was frequently

incomplete, that, in fact, cases having several referrals recorded in their

comprehensive jackets (denoted by the red "stamps" discussed earlier) had

unblemished blue cards, proving the blue card file to be highly unreliable—albeit

seductively convenient—source of data regarding either prior records or re-referrals

of juvenile offenders. This discovery made it clear that reliable evidence of a

juvenile offender's actual referral experiences could derive only from a thorough

search of each juvenile's comprehensive file.

Despite these problems, it was possible to collect some highly useful

and very interesting background data on one or both samples. As we shall see

later, without these background data, our findings regarding recidivism rates for

the two groups studied would likely have proven quite perplexing.

Factors about which data were collected for one or both samples

included the age, race, sex, ethnicity, grade, and residence of juveniles

referred; the number, nature, and dates of the instant, as well as prior and subsequent

referral(s) to juvenile authorities; prior dispositions, including temporary pre-

disposition placement in the Bernalillo County Juvenile Detention Home; and the

age, employment status, and income of the juvenile's legal guardian(s), as well as the blood relationship of the juvenile to his guardian(s).

An interesting but largely unexplored dimension of "recidivism"—besides its substantive referent—is its temporal frame. Although we may eventually agree on the specific behavior that constitutes "failure," the question of how long an individual may (should) remain methodologically "at risk,"—and, thus, eligible for failure—remains at issue. For this analysis, we have generated four separate measures of recidivism, each with a different follow-up interval—6 months, 12 months, 18 months, and 30 months. Earlier, it was noted that the overall "success" or "failure" of a program might vary according to the substantive definition of recidivism employed by the researcher. Furthermore, the designated length of the follow-up period bears comparable significance, since as we extend the period that a person is "at risk," we increase the likelihood of his recidivism.

BACKGROUND CHARACTERISTICS

It is important, for two reasons, to examine the characteristics of both client groups before engaging in a discussion of recidivism. First, client characteristics are especially significant for program planning and administration. Second, they may be independently related to post-treatment success or failure. In such cases, accurate interpretation of any ostensible relation between program assignment and treatment outcome will rest on a clear understanding of differences between the respective client groups.

Sex

As Table 1 indicates, the total sample contained a nearly equal number of males and females.

Table 1: Sex of CHIP and Control Clients by
Sample, 1975-1977*

Sex	CHIP		Control		Total	
	N	%	N	%	N	%
Female	71	61.7	62	35.6	133	46.0
Male	44	38.3	112	64.4	156	54.0
	115	100.0	174	100.0	289	100.0

*The total column percentages in this and subsequent Tables may not actually add to 100 percent due to rounding error.

A look at the individual samples, however, reveals a striking difference. While the preponderance of control clients were male, the opposite is true for the CHIP sample, where girls outnumbered boys nearly two to one. The distinction in client composition is administratively significant, as sex is strongly related to reason for referral, the juvenile's relationship with his or her parents, and other factors which, in turn, bear significantly upon the psychology and effectiveness of the counseling approach.

Ethnic Background

White referrals, which constituted about half of the aggregate sample, were fairly evenly divided between the two samples as we see in Table 2.

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Table 2: Ethnicity of CHIP and Control Clients
by Sample, 1975-1977

Ethnicity	CHIP		Control		Total	
	N	%	N	%	N	%
White	55	47.8	86	49.4	141	48.8
Chicano	48	41.7	48	27.6	96	33.2
Black	2	1.7	5	2.9	7	2.4
Indian	2	1.7	1	0.6	3	1.0
Other	2	1.7	19	10.9	21	7.3
Missing	6	5.2	15	8.6	21	7.3
Total	115	100.0	174	100.0	289	100.0

This was not the case, however, for the next most frequent ethnic class, as we find a disproportionately large percentage of Chicanos being referred to the CHIP program. Specifically, 42% of the CHIP clients, compared to 28% of the control group, were Chicago.

The implications of this distribution are extremely important, as sensitivity to ethnically based perspectives and behavior would appear to be an important ingredient in successfully dealing with a client population that is so disproportionately non-white.

Area of Residence

Despite the expectation that juvenile offenders might be more likely to come from lower income areas of the city, we find in Table 3 that more than 40% of all clients referred came from the northeast heights, a typically middle or upper income area.

Table 3: Area of Residence by Sample,
1975-1977

Area	CHIP		Control		Total	
	N	%	N	%	N	%
Northeast	50	43.5	69	39.7	119	41.1
Northwest	21	18.3	36	20.7	57	19.7
Southwest	23	20.0	25	14.4	48	16.6
Southeast	15	13.0	19	10.9	34	11.8
Missing	6	5.2	25	14.4	31	10.7
Total	115	100.0	174	100.0	289	100.0

In contrast, fewer than 40% of all referrals came from the northwest and southwest sectors, combined, where lower income families tend to reside.

Viewing the groups individually, we find that a slightly greater proportion of CHIP subjects (43.5%) than control group clients (39.7%) resided in the northeast area; this difference, however, was not statistically significant. The only statistically important difference between samples lay in the respective

proportions residing in the southwest; clients from that area were more frequently referred to the CHIP program.

This distribution of the residential source of CHIP referrals could bear importantly on the location of the main and possible branch offices of the project. Currently, the only project office in existence is located in the southeast.

Family Income of Subjects

The family income of juvenile referrals also proved interesting, particularly insofar as it provided a basis for distinguishing the CHIP and control samples.

Table 4: Annual Family Income of CHIP and Control
Clients by Sample, 1975-1977

Income Category	CHIP		Control		Total	
	N	%	N	%	N	%
Public Assistance	14	12.2	3	1.7	17	5.9
0 - $5499	3	2.6	13	7.4	16	5.5
$5500 to $9999	12	10.4	19	10.9	31	10.7
$10,000 to $14,999	15	13.0	25	14.4	40	13.8
$15,000 & up	24	20.9	27	15.5	51	17.6
Missing	47	40.9	87	50.0	134	46.4
Total	115	100.0	174	100.0	289	100.0

Perhaps contrary to our expectations, Table 4 shows that the highest proportion of juvenile referrals in both samples (for whom family income data were available) came from families with annual incomes of $15,000 or more. As income decreases, the number of referrals—and the number of juveniles in both samples—decreases as well (a pattern that might suggest little more than an increasing reluctance of parents to disclose income, as income level decreases).

Differential program assignment of juveniles whose families receive public assistance (nearly 6% of the total) is particularly noteworthy. Nearly everyone in that group found his way into the CHIP program. However, because of the relatively large number of missing values for both samples, conclusions regarding the relation of family income to program assignment must remain tentative.

Family Structure

Family constellation is often cited as an important source of juvenile problems. Specifically, whether or not an adolescent comes from an intact, two-parent home may be related to contact with juvenile authorities.

As we learn from Table 5, fewer than half (48.0%) of the subjects came from intact family units. At the same time, more than one in four (26.3%) were from fatherless homes. A comparison of the two samples revealed only one notable difference: CHIP clients were more than two times as likely as members of the control sample to have come from a family with one natural and one step parent. Such a pattern might forbode (among other things) a greater likelihood of family instability or disharmony among CHIP referrals, thereby exacerbating the counseling task.

Table 5: Family Composition of CHIP and Control
Clients by Sample, 1975-1977

Guardians in Residence with Juvenile	CHIP		Control		Total	
	N	%	N	%	N	%
Both Natural Parents	48	41.7	91	52.3	139	48.0
One Natural and One Step Parent	25	21.7	17	9.8	42	14.5
Mother Only	33	28.7	43	24.7	76	26.3
Father Only	2	1.7	9	5.2	11	3.8
Other relative(s)	5	4.3	6	3.4	11	3.8
Foster Home or Adopted	1	0.9	1	0.6	2	0.7
Missing	1	0.9	6	3.4	7	2.4
Total	115	100.0	174	100.0	289	100.0

Detention Home Placement

Labelling theorists suggest that exposure to formal social control agencies will enhance the chances of repeated subsequent contacts of an offender with such control systems. A stay—regardless of length—in a juvenile detention center, for example, is hypothesized to have just such a deleterious effect. It is important, therefore, to explore the extent to which the samples differ in terms of their clients' prior detention experience.

Table 6: Prior Detention Home Placement of CHIP and
Control Clients by Sample, 1975-1977

D-Home	CHIP		Control		Total	
	N	%	N	%	N	%
Yes	40	34.8 ·	24	13.8	64	22.1
No	60	52.2	150	86.2	210	72.7
Missing	15	13.0	0	---	15	5.2
Total	115	100.0	174	100.0	289	100.0

If the labelling perspective is correct, Table 6 exposes a serious problem
for the CHIP project, in that its clients are more than two and one half times as
likely as the control group to have had a prior detention experience: only about
one in seven (13.8%) of the non-project clients had ever been detained in the
Detention Home; more than one in three (34.8%) of the clients referred to the CHIP
program had had such an experience.

Prior Record

However disheartening for proponents of rehabilitation, it remains a
truism that one of the best predictors of post-treatment failure is a record of prior
offenses. The juvenile cases included in this study prove no exception to this
pattern, as forthcoming discussion of the correlation between prior referral and
recidivism indicates. If we bear this relation in mind, the information in Table 7
is quite instructive.

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Table 7: Prior Referrals of CHIP and Control Cases
to the Bernalillo County Juvenile Probation
Office by Sample, 1975-1977

Prior	CHIP		Control		Total	
	N	%	N	%	N	%
Yes	35	30.4	30	17.2	65	22.5
No	80	69.6	144	82.8	224	77.5
Total	115	100.0	174	100.0	289	100.0

From Table 7 we see that, while only about one in five juvenile referrals had records of prior referral(s) to the Juvenile Probation Office, the distribution of prior offenders between the CHIP and control groups was notably inequitable: juveniles referred to CHIP were nearly twice as likely as their non-project counterparts to have a history of referral(s) (30.4% versus 17.2%, respectively) to juvenile authorities.

Referral Offense

As the strongest single predictors of post-treatment success, the offenses for which juveniles were referred merit special attention. Although not reported in the table below (It is discussed later in the report.), the two offense groups accounting for the most referrals overall bore the strongest relation to post-treatment success and failure, respectively. Table 8 compares the project and non-project samples in terms of the highly disparate, and thus significant,

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distribution of referral offenses between treatment groups.

Table 8: Offenses for Which CHIP and Control
Clients were Initially Referred to the
Juvenile Probation Office by Sample,
1975-1977

Offense	CHIP		Control		Total	
	N	%	N	%	N	%
Shoplifting	12	10.4	53	30.5	65	22.5
Runaway-local	37	32.2	2	1.1	39	13.5
Liquor Violation	2	1.7	18	10.3	20	6.9
Other Juvenile	7	6.1	11	6.3	18	6.2
Incorrigible	5	4.3	12	6.9	17	5.9
Curfew	3	2.6	13	7.5	16	5.5
Truancy	8	7.0	6	3.4	14	4.8
Traffic	0	0	14	8.0	14	4.8
Assault-other	2	1.7	6	3.4	8	2.8
Vandalism	0	0	5	2.9	5	1.7
Possible Marijuana	1	0.9	4	2.3	5	1.7
All Other*	38	33.0	30	17.2	68	23.5
Total	115	100.0	174	100.0	289	100.0

* This category is made up of all the remaining offenses which had fewer than 5
cases and comprised less than one and one-half percent of the total.

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From Table 8, we learn that juveniles referred for shoplifting—the most frequent reason for referral and the offense group exhibiting the lowest overall recidivism rate—are seldom referred to the CHIP program: only 12 of the 65 (18.5%) shoplifting cases in the total sample found their way to CHIP. Conversely, juveniles referred for running away from home—an offense which ranks second in total referrals and which exhibits a strong relation with post-treatment failure—are especially likely to be referred to CHIP. In fact, 37 of the 39 (94.9%) runaway cases in the total sample had been referred to CHIP. The impact of these particular patterns on recidivism will be discussed later in the report.

CHIP Profile: A Summary

In the foregoing discussion, it has been suggested that the CHIP case-load possessed a disproportionate share of attributes that correlate highly with recidivism.

A review of modal categories reveals the following composite of the CHIP referral. She is in the tenth grade, referred for running away from a home which has a 50-50 chance of being intact. While the CHIP client is likely not to have been previously referred to juvenile authorities nor to have ever been held in the Juvenile Detention Center, her likelihood of having had such experiences is greater than that of her non-project counterpart. We will discover in the next section that while she is not likely to be re-referred to juvenile authorities, the probability of recidivism is slightly greater for her than for her hypothetical sister.

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As we examine the comparative success and failure rates of CHIP and control clients, it will be helpful to bear in mind much of the preceeding discussion about the distribution of key client characteristics, since many bear significantly on the crucial issue of post-treatment outcome.

DATA ANALYSIS

Recidivism

It is encouraging to note the comparatively low overall recidivism rate of the two sample groups in Table 9.

Table 9: Recidivism Rates of CHIP and Control
Clients by Sample, 1975-1977

	Recidivism	No Recidivism	Total
CHIP	33% (N = 38)	67% (N = 77)	(N = 115)
Control	21% (N = 37)	79% (N = 137)	(N = 174)
	26% (N = 75)	74% (N = 214)	(N = 289)

$x^2 = 4.405 \ (p < .036);$ $\emptyset = .132$

Of the nearly 300 sample cases handled by the Juvenile Probation Office or diverted to CHIP between June 1, 1975, and May 31, 1977, about one in four (26%) was re-referred to the Probation Office by November 30, 1977, a ratio

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which is half the size of popularly "quoted" recidivism rates of 50 to 75 percent.

We also learn from Table 9, perhaps surprisingly, that the recidivism rate for juveniles diverted to CHIP (33%) is higher than that for juveniles in the control sample (21%). While the difference is statistically significant (i.e., not likely the product of a "chance" distribution), the correlation is low ($\emptyset$ = .132). Moreover, while the numbers reflected here provide cause for concern, that concern is allayed somewhat by closer analysis of the kinds of cases constituting the respective caseloads of the CHIP and control samples.

Before engaging that analysis, however, let us explore some of the other significant aggregate-level findings.

Recidivism as a Function of Time

It was suggested earlier that recidivism would increase as a function of time "at risk." Table 10 not only verifies this hypothesis, it illustrates the approximate function played by time. If we follow each client for only six months after his initial referral, we find an aggregate recidivism ratio of less than one in seven (14.9%). If we extend the follow-up period to November 30, 1977, for everyone in the sample—thereby making the period "at risk" as long as 30 months for some members of the sample and as short as 6 months for others—the overall recidivism rate nearly doubles (26.0%). The lesson is clear: judgments regarding the success or failure of either a client or a program can be radically affected by the length of post-treatment observation.

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Table 10: Recidivism Rates of the Aggregate of CHIP
and Control Samples, by Length of Follow-up
Period,* 1975-1977

Length of Follow-up Period	Number of Recidivists	Percentage of those "at risk" (N = 289)
6 months	43	14.9
12 months	62	21.5
18 months	67	23.2
30 months	75	26.0

*It is important to remember that since eligibility for inclusion in either sample was limited to juveniles with referral dates between June 1, 1975, and May 31, 1977, and since the termination date for all follow-up periods was November 30, 1977, only a small fraction of cases in the combined sample were actually "at risk" for a full 30 months; a slightly larger proportion of cases were "at risk" for 18 months; more than half of the total sample were at risk for at least a year; and everyone in the sample was "at risk" for at least a full 6 months. Thus the extended follow-up periods represent a cumulative rather than an absolute lengthening of the period each client was "at risk." Consequently, it is likely that the recidivism rates for the "extended" follow-up periods are less than they would have been had every person in the respective "time at risk" groups been truly "at risk" for the full period indicated.

Before we leave this discussion, an additional point merits brief attention. Table 10 illustrates how recidivism rates increase as the period "at risk" is extended. From Table 11 we learn that this pattern applies to both the CHIP and the control sample. A closer look at Table 11, however, reveals that the deleterious effect of extending the period that a client is "at risk" is more pronounced for the control group. The recidivism rate for that group doubles with

Table 11: Recidivism Rates of CHIP and Control Samples
by Length of Follow-up Period, 1975-1977

Length of Follow-up Period	CHIP		CONTROL	
	Number of Recidivists	% of Those "at Risk" (N=115)	Number of Recidivists	% of Those "at Risk" (N=174)
6 months	23	20.0	20	11.5
12 months	32	27.8	30	17.2
18 months	34	29.6	33	19.0
30 months	38	33.0	37	21.3

the extended follow-up period (from 11.5% to 21.3%) while the recidivism rate for

the CHIP group increases by only about 50 percent (from 20% to 33%). This pattern

suggests that despite whatever else we might say about the CHIP "treatment," its

beneficial effects would appear to be more permanent than those of the control

group. While such a conclusion exceeds the limitations of the data collected for

this report, the prospect certainly merits additional exploration.

The CHIP Experience from 1975 to 1977: Recidivism over Time

Data on the success of CHIP clients were recorded for a period beginning

with the inception of the program in mid-1975 and ending with mid-1977. The aim

was to enable us to discover the nature and magnitude of any change in program

success rates over time. The data in Table 12 suggest a rather remarkable stability

in success rates for both the CHIP and control samples. Recidivism (using a six-
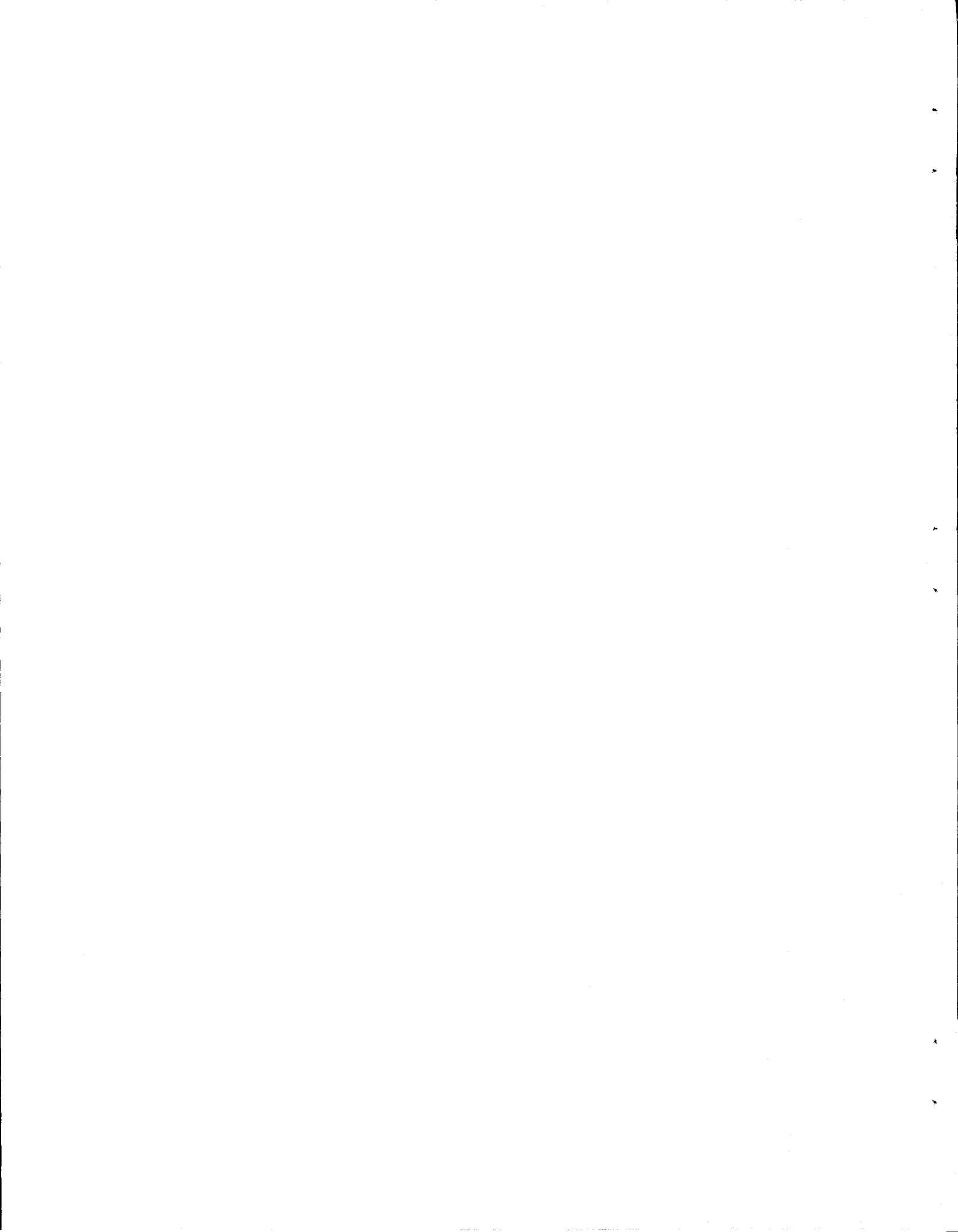
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Table 12. Recidivism Rates (6 month follow-up) for Referrals to
CHIP and JPO in 1975, 1976, and 1977

	1975			1976			1977			Total		
	No. Recid.	No. "at risk"	% Recid.	No. Recid.	No. "at risk"	% Recid.	No. Recid.	No. "at risk"	% Recid.	No. Recid.	No. "at risk"	% Recid.
CHIP	7	40	17.5	11	53	20.8	5	22	22.7	23	115	20.0
JPO	6	47	12.8	10	91	11.0	4	36	11.1	20	174	11.5

month follow-up period) among CHIP clients varied less than six percentage points over the three successive years (from a low of 17.5% to a high of 22.7%). Recidivism rates for the control group varied even less (from a low of 11.0% to a high of 12.8%). The reader will also note a pattern discovered earlier: while the overall recidivism rates are quite low, CHIP clients consistently exhibit approximately twice the likelihood of re-referral to juvenile authorities after initial referral as do members of the comparison sample.
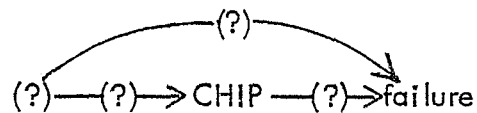
The Correlates of "Failure": De-mystifying a Curious Finding

One is understandably troubled by the patterns exhibited in three of the last four Tables which seem to suggest that clients diverted to CHIP are more likely to "fail" than are those who are handled routinely within the JPO. Since the differences reported are statistically significant, there is little doubt that there is a real difference in outcome for clients in the experimental and comparison samples. Our analysis of the data thus far would suggest some truth to the following conceptual model:

$$CHIP \xrightarrow{\text{(leads to)}} failure$$

$$JPO \longrightarrow success$$
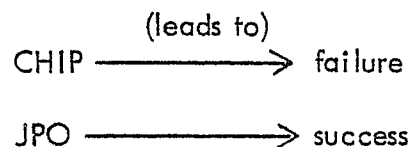
Analysis cannot cease with this discovery, however, inasmuch as earlier discussion has already suggested the possibility that this pattern is a result of some biases in the data. Our task, therefore, was to mine the data for possible interpretations of this model, to look "behind the scenes," as it were, for patterns, correlations, and relationships which might "explain" this curious initial finding,

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to answer the questions implied in the revised model below:

$$(?) \longrightarrow (?) \longrightarrow \text{CHIP} \longrightarrow (?) \longrightarrow \text{failure}$$

with arc labeled —(?)— curving from the first (?) to failure.

Behind the Scene: Rediscovering the Problem of Specification

There are at least two scenarios that fit the hypothetical model above. One involves the "specification" phenomenon discussed earlier. Such a scenario would find a strong correlation between a factor X and post-treatment failure and a coincidental, perhaps accidental, correlation between factor X and assignment to CHIP, such that:

$$\text{factor X} \dashrightarrow \text{CHIP} \qquad \text{failure}$$

with arc curving from factor X to failure.

In such a model, it is clearly the influence of factor X and not the program to which the client is assigned that "causes" the post-treatment failure. It is merely the coincidental association of factor X with one or the other treatment that makes the treatment appear to be the cause of post-treatment success or failure. In short, possession of factor X specifies that a client is more likely to fail, regardless of the treatment to which he is assigned. Thus, if a disproportionate number of clients who possess factor X happen to be referred to one program, that program will "automatically" suffer a higher rate of client failure, a phenomenon which is merely a reflection of its clients' greater "predisposition" to failure.

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Factor X and the specification model illustrate the role played by prior record in this investigation. The next two tables will assist us in exploring the implied relationships.

Table 13: Recidivism by Prior Record,
Aggregate Sample, 1975-1977

	Recidivism	No Recidivism	
Some Prior(s)	38.5% (N = 25)	61.5% (N = 40)	(N = 65)
No Priors	22.3% (N = 50)	77.7% (N = 174)	(N = 224)
	26.0% (N = 75)	74.0% (N = 214)	N = 289

$x^2 = 6.016$ (p < .015); $\emptyset$ = .154

First, in Table 13, we observe the notable correlation between having a prior record and recidivating: the recidivism rate of juvenile offenders who have already been referred to juvenile authorities at least once is 38.5 percent; for first referrals, the failure rate is only 22.3 percent.

The relevance of this correlation becomes particularly apparent when we realize, as Table 14 shows, that the CHIP caseload is characterized by a disproportionate number of juveniles with prior records—to wit, 30.4 percent, as opposed to JPO's 17.2 percent. To the extent, therefore, that one or the other sample is disproportionately "loaded" with individuals who manifest an unusually

Table 14: Number and Percent of CHIP and
Control Clients with a Prior Record,
1975-1977

|	Some Prior(s)	No Priors	
CHIP	30.4% (N = 80)	69.6% (N = 35)	(N = 115)
JPO	17.2% (N = 144)	82.8% (N = 30)	(N = 174)
	77.5% (N = 224)	22.5% (N = 65)	N = 289

$x^2 = 6.177$, $(p < .013)$; $\emptyset = .155$

high probability of post-treatment success or failure, conclusions about the effects
of treatment, per se, must issue with care. We should note before leaving the
issue of prior record, however, that its contribution to the failure rate of CHIP
clients is clear, but minor. While the recidivism rate of the CHIP sample decreases
somewhat when we eliminate the deleterious effects of a prior record, it remains
significantly higher than that of the comparison sample.

Resolving the Dilemma: The Spurious Correlation

The second scenario that fits the conceptual model presented earlier
embodies a spurious correlation. This scenario employs the same elements as the
specification model discussed above; but the elements stand in a slightly different
relation to each other. Like the earlier model, the spurious model requires a

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strong correlation between some factor X and the failure criterion. But, whereas the earlier model required only an accidental or coincidental association between the factor X and the particular treatment program to which a subject is assigned, the spurious model requires that the factor X stand in strong causal relation to program assignment. Thus, both the treatment to which one is assigned and the ostensible outcome of that treatment are, in a sense, "caused" by factor X, as illustrated below:

$$\text{factor X} \begin{array}{c} \nearrow \text{failure} \\ \searrow \text{CHIP} \end{array}$$
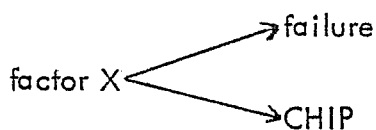
Because both treatment and outcome are so powerfully related to factor X, they appear—all to reasonably, to the uncritical eye—to be strongly and independently related to each other.

Close analysis of the data in this study discloses precisely this kind of spuriousness, giving rise to an illusory correlation between referral to CHIP and post-treatment failure when, in fact, both are largely a function of a third factor—the offense for which the juvenile was initially referred. Indeed, Table 15 is quite enlightening in this regard. It documents two patterns that are highly significant to our discussion. The first relates to runaway referrals, a group comprising 13.5 percent of the total sample. With an overall recidivism rate of 38.5 percent, runaway referrals embody the greatest risk of post-treatment re-referral of all the offenses studied. The significance of these figures comes clearly to light when we note that all but two (94.9%) of these high risk offenders were referred to CHIP,

Table 15: Most Frequent Reasons for Initial Referral to
Juvenile Authorities, by Overall Recidivism
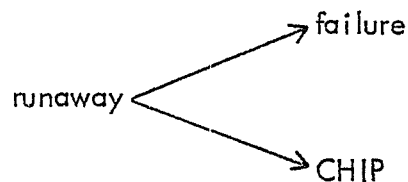Rate and Percent Diverted to CHIP, 1975-1977

	Number of Referrals	% of Total Sample	Overall Recidivism Rate	% of Total Referrals Diverted to CHIP
Runaway	39	13.5%	38.5%	94.9% (N = 37)
Shoplifting	65	22.5%	12.3%	18.5% (N = 53)
Curfew Violation	20	6.9%	20.0%	10.0% (N = 2)

thereby inflating the overall post-treatment failure rate of the CHIP sample by several percentage points. A partial explanation of the comparatively high recidivism rate for CHIP clients, therefore, lies in the fact that CHIP receives a disproportionately large number of bad risks (to wit, runaways) in the first place.
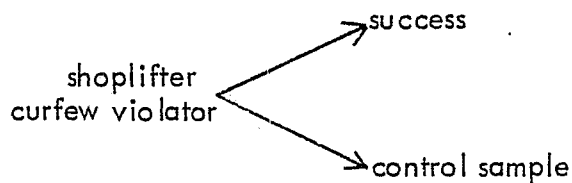
The pattern for shoplifting and curfew referrals is precisely the inverse of that for runaways, but its deleterious effect on the failure rate of the CHIP sample, relative to that of the control group, is identical. That is, shoplifters and curfew violators exhibit among the lowest recidivism rates of all the offender groups (12.3% and 20.0%, respectively). At the same time, such cases seldom find their way into the CHIP caseload; as Table 15 shows, only about one in five (18.5%) shoplifters and one in ten curfew violators were found in the CHIP case-load. Inasmuch as these two offender groups constitute nearly one third of the total sample studied, the impact of their disproportionate assignment to a

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particular treatment cannot be overlooked.

Thus, it becomes clear from Table 15 that the ostensible difference in outcome between the CHIP and control groups is, in some part, spurious. For whatever reason (probation officer frustration with the difficulty of dealing with certain kinds of offenders, perhaps, and the consequent systematic referral of difficult cases to CHIP), it appears that the CHIP sample is overpopulated with unusually bad risks, and the control sample exhibits a highly disproportionate number of particularly good ones. The models which best summarize these relationships are presented below:

```
                                          failure
                                        ↗
                    runaway  <
                                        ↘
                                          CHIP
```

and conversely:

```
                                          success
                                        ↗
        shoplifter
        curfew violator  <
                                        ↘
                                          control sample
```

The confounding effect of this peculiar distribution is an inflation of the failure rate of CHIP clients, coupled with an inflation of the success rate of the control group. The net result is an exaggerated disparity in post-treatment recidivism rates for the two groups.

SUMMARY AND CONCLUSION

A simple comparison of recidivism rates for the samples studied does not favor CHIP. Closer examination, however, discloses a number of factors which bias the analysis by virtually "stacking the deck" against the project.

First, it seems that the especially high risk cases—runaways and clients with records of prior referral(s) to juvenile authorities—are those most likely sent to CHIP.

Moreover, analysis of these kinds of cases, which constitute a major portion of the CHIP caseload, suggests that they would frustrate the most resourceful of interventionists. First, cases which have been referred previously and disposed of unsuccessfully will prove especially difficult to counsel effectively, particularly if—as is likely—the repetition of referrals is symptomatic of an old and enduring conflict area. Second, running away from home—the referral offense for one third of the CHIP caseload—represents precisely this kind of problem, inasmuch as the runaway often perceives his or her flight as a final, desperate attempt to escape what he has come to perceive as an irresolvable problem or an irreconcilable conflict at home. Neither scenario instills optimism about the prospects of a successful resolution. Indeed, that the CHIP success rate is so high (67%) in the face of such odds is laudable.

Exacerbating the difficulty posed by CHIP's receipt of a disproportionately large number of high risk cases, the CHIP program, at the same time, received disproportionately few of a significant number of low risk, e.g., shoplifting, cases. Speculation about why such referrals seldom result in additional contact

with juvenile authorities is not necessary; that they do not revert is evidence enough of their facility.

A Digression on "Diversion"

This discussion leads the writer inevitably to an observation that bears notation. Although casual conversation and thought is likely to label CHIP a "diversion" program, such language or thought is both misleading and unfair. In common parlance and practice, diversion is reserved for the casual offender with no prior record, no serious problems or needs, and who poses no fathomable risk of recidivism.[5] As a result, "diversion programs" are expected to, and usually do, exhibit remarkably low recidivism rates. The rates are usually low, however, not because of the efficacy of any "treatment" offered in conjunction with the diversion, but because the diverted individuals needed no "treatment," because they are less serious and less troubled offenders than the non-diverted group. In accord with this understanding of diversion, one would properly be shocked at discovering suddenly that the recidivism of diverted clients began to outstrip that of their non-diverted counterparts.

The important point to bear in mind is that Project CHIP does not fit this "diversion" model. Contrary to the conventional model, the CHIP caseload was disproportionately populated with high risk cases—repeat offenders, cases

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[5]While it is true that CHIP counseling is a sort of diversion from—i.e., a less serious disposition than—formal probation supervision or institutionalization in a juvenile facility, at the same time, it is also a more serious disposition than a decision by an intake officer to "counsel and release" or to dismiss a referral outright.

involving serious and/or enduring problem areas, etc.—while the inverse was true

of the control sample, as we have repeatedly noted. The project's recidivism

rate simply and candidly reflects these facts. It is essential to remain cognizant

of this if we are to keep the results of this analysis in proper perspective.

A Final Word About Methodology

The importance of the research design cannot be overstated. In an

evaluative design like this, absolute comparability of project and control samples

is a fundamental prerequisite to any definitive conclusions about program "success."

Lacking that comparability in samples, the analyst risks encountering the con-

founding effects of spurious or specified correlations deriving from the dispro-

portionate allocation of key outcome-related attributes across sample groups.

As discussed earlier, the best way to ensure the requisite comparability

is to use a random allocation design, wherein subjects eligible for the experimental

treatment project are randomly assigned either to it or to the control group. This

design literally "randomizes" any biasing factors across the sample groups,

guaranteeing the necessary condition of cross-sample comparability, as comparisons

of post-treatment outcome are made.

Lacking a random allocation design, the only way to adjust for the

inevitable biases that are introduced into the samples is to statistically "control"

for the effects on outcome of all relevant factors that are not distributed equally

across samples. The analysis presented herein proceeded in precisely this fashion,

identifying those factors which were correlated with both program placement and

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treatment outcome, and then controlling for the effects of those factors, one at a time. As discussed, each control tended to reduce sample differences in post-treatment success rates.

The success and rigor with which such multiple controls may be effected, however, is largely a function of sample size: extensive controls require extremely large samples. While the samples drawn by the research team for this analysis were substantially larger than any of those used in previous analyses of Project CHIP, they were still too small to allow the <u>simultaneous</u> control of all the confounding and biasing variables encountered; and while the difficulty of finding a sufficient number of cases (resulting in recidivism) to support this more rigorous analysis testifies to the laudable effectiveness of the program being evaluated, it exacerbates the research task immeasurably. While sample size did not permit verification, there is every likelihood that simultaneous control of the effects of all the biasing high-risk factors would have substantially eroded the difference between the CHIP and control samples in their respective rates of post-treatment failure. These kinds of difficulties, which attend all treatment evaluation designs, underscore the importance of understanding the inherent complexities of social research itself.

> It is essential that judges, administrators, and students should be aware of these problems. . . . [C] onsumers of penological research tend to want results which can be easily and mechanically applied, and they may be disillusioned by findings which are hedged about with

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> many qualifications. But it is in just these circumstances
> that the danger is greatest. [They must persevere.] [6]

No doubt, simplicity has a seductive appeal. But it should come as no surprise

that truth is rarely as simple as we might like. It has been the aim of this report

to present the design, rationale, and findings of research in a clear, but competent

and comprehensive manner. At times, no doubt, detail threatens to tax the weary;

but lack thereof may properly confound the curious. We obviously chose to risk

the former, that we might preclude the latter.

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[6] Wood, Roger and Richard Sparks, Key Issues in Criminology, 1970, p. 172.

END