Final Report
78-JN-AX-0014
MFI

# Juvenile Corrections and the Chronic Delinquent

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Prepared for the National Institute for Juvenile Justice and Delinquency Prevention, Law Enforcement Assistance Administration, Washington, DC



March 1979

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ACKNOWLEDGEMENTS

At the top of the list, far in front, is Cindy B. Israel. Coauthor of the original UDIS evaluation and manager of the follow-on research until her departure from AIR in the fall of 1978, her meticulous care in supervising the preparation of this very complex data base was matched by the quality of her insights into its meaning.

Doug Thomson, the other coauthor of the UDIS evaluation, took time from other commitments to help us conduct the survey of literature about juvenile corrections. This task was made easier--made possible--by Judith Wilks and Robert Martinson, who gave us free access to their unique collection of correctional evaluations.

In Chicago, Sidney Buster was our person on the scene, overseeing the day-to-day work in the Chicago Police Department and at the Cook County Juvenile Court. Other staff who contributed to that effort are listed in the description of the project's design. We owe much to each of them.

Old friends from the UDIS evaluation continued to help. We wish to thank especially Commander Harold Thomas and Lieutenant James McGuire of the Chicago Police Department, Samuel Sublett and John Henning of the Department of Corrections, and Edward Nerad and Leonard Holbein of the Cook County Juvenile Court. Richard Sullivan and Anne Tatalovich



of the Illinois Law Enforcement Commission smoothed the way for the follow-on and continued, as during the UDIS evaluation, to be supportive in a dozen ways.

We are grateful to James Howell, Director of the National Institute for Juvenile Justice and Delinquency Prevention, for deciding that the suppression effect demanded more exploration than we could give it the first time around; and to our technical monitor, Pamela Swain, for being unflappable and helpful during the storms that have blown up around the work and its results.

In this regard, we must acknowledge the contributions of those who have been critical of the suppression effect and its interpretation. Andrew Gordon, Richard McCleary, and Jerry Miller in particular stimulated us to approach the material from new perspectives and were a source of encouragement to do a good job.

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# INTRODUCTION

In April 1976, the American Institutes for Research began an evaluation of an experimental program called the "Unified Delinquency Intervention Services"--UDIS, for short. It was intended to be an alternative to incarceration for Cook County delinquents. The Illinois Law Enforcement Commission was providing most of the funds for UDIS, and wanted to know whether it worked. We were assigned the job of finding out.

"It worked" can mean several things for a correctional program. It can mean that the program's clients get the services that the program promised to provide. It can mean that the dollar costs of dealing with chronic delinquents are lowered. It can mean that the delinquents undergo a positive change in their outlooks, or that they go back to school, or that they acquire job skills.

These types of outcomes were to be investigated in the evaluation. But from the outset, it was agreed among ILEC, UDIS and the evaluators that the primary impact measure of success would be recidivism. As we put it in the final report:

> UDIS exists finally to reduce delinquency. Its many other potential values--keeping youth out of institutions and in their communities, providing them with better services, lowering the costs of correctional intervention, and the rest--are important and could even justify the program. But UDIS ultimately reflects important concepts about how to deal with delinquency, and recidivism remains the most direct measure of whether the UDIS idea works. (Murray et al., 1978: 125)

Data analysis began in June 1977. At first, there were few surprises. Recidivism rates were high, consistent with those found in other studies. Almost 70 percent of both UDIS and institutionalized youth were being arrested within a year after release. The differences between the two programs were minor, with UDIS seeming to do slightly better than institutionalization. The differences were probably too small to be of importance.

Then, the analysis started to compare rates of arrests "before" and "after" intervention--the pretreatment/posttreatment comparison that has been a standard part of experimental and quasi-experimental evaluations in most fields, but oddly rare in corrections research. And the results were startling. When arrests in the postintervention period were compared with arrests in a comparable preintervention period, the reductions were very large. Moreover, they were large for both UDIS and the institutionalized youth. The exact magnitude of the reductions varied, depending on the program and on the length of "before" and "after" periods being examined, but the range was always high, showing reductions of 50 to 70 percent. The more closely they were analyzed, the harder it became to explain them away. Apparently we were observing a major behavioral change among a very tough set of delinquents.



The reductions were labelled "the suppression effect" and discussed at length in the final report (Murray et al., 1978). But while we were satisfied that the fact of the reduction could be adequately established with the data at hand, many other natural questions had to go unaddressed. The sample sizes and followup periods prescribed by the design had not been drawn up in anticipation of the finding we had stumbled across.

Within a few months after the report was released, it became clear that what we considered to be ample evidence and analysis of the reality of the suppression effect was not considered adequate by some others. A long and complex dialog began about the statistical properties of the suppression effect. We had known that the suppression effect would be unwelcome news in some quarters; we had drastically underestimated how unwelcome.

Even before the debate erupted, however, it was apparent to us that this important and explosive finding needed further exploration, using larger samples, longer followup, and a data collection strategy explicitly designed to permit answers to the questions raised by the suppression effect. Support for such a follow-on study was sought from the National Institute of Juvenile Justice and Delinquency Prevention. A grant was awarded in February 1978. The following pages report the results.

Our purpose is deliberately limited, and the report must be read with these self-imposed limitations in mind. We do not expound on the more general characteristics of the chronic delinquent, nor on the recent patterns of juvenile crime, nor on the psychological/economic/social implications of alternative interventions. *Such issues are critically* *important to interpreting the policy implications of the suppression effect. We urge that the reader not understand this report too quickly.* But it seems to us that a major obstacle to knowledge-building in the study of delinquency has been that policy implications have been kept too keenly in mind--and, to put it bluntly, that conceptions of what constitutes good p licy have tended to shape the research questions that have been asked. We take a long look at the data in this report, and hope that policy-makers will take an equally long look at the findings in the larger social context in which the delinquency problem is embedded. *The report provides some provocative material for thinking about answers, not the answers themselves.*

The first chapter presents the basics of the suppression effect--how it is defined, the basic sample which is used to analyze it, and its magnitude. Chapter 2 examines the properties of the suppression effect, emphasizing the roles of potential artifacts and confounds. Chapter 3 is devoted to three of the most important of these topics--maturation, the regression artifact, and history. Chapter 4 takes up the question of alternative forms of juvenile corrections, and how they compare with institutionalization. Chapter 5 discusses the impact of lesser interventions, supervision and probation. An afterword points to some of the most basic policy implications.

Four appendixes are included. Appendix A describes the design of the study. Appendix B provides a background description of the UDIS program. Appendix C discusses the relative costs of UDIS and DOC, in terms of dollars and in-program offenses. Appendix D is the bibliography.



# 1. THE SUPPRESSION EFFECT

What defines "success" when measuring recidivism in a delinquency program? Historically, success has been cast in terms of *cessation*: the extent to which the delinquent approached zero offensive behavior after release. The most common measures have been dichotomous ones, asking whether the youth recidivated at all by whatever measure of recidivism was being employed. In these studies, the delinquent either "succeeds" or "fails," and there is no in-between. Success might be defined, for example, as "no new arrests" or "no new conviction" or as "discharge from parole" within a certain time period following release.[1] In rarer cases, the answer has been framed in terms of the degree or quantity of recidivism--*how many* arrests, rather than a simple statement of *whether* an arrest occurred.[2] But both classes observe only the postrelease period, and the degree of success has been measured from the zero-point.

Measured in these terms, delinquency programs (and correctional programs for adults) have looked bad, virtually without exception. One of the more convincingly established findings in the criminal justice literature seems to be that any correctional program experiences a very high proportion of failures. We can "cure" criminality in some people, perhaps, but only in some of the people some of the time.

This study uses an alternative approach. We compare behavior prior to the correctional program with behavior following the correctional program. The question is not whether delinquency is in any sense cured, but whether things get better. The reference point is not zero, but whatever level of activity was occurring in the preintervention phase.[3]

Neither approach has a monopoly on merit. The absolute zero-based measures are appropriate when the question at issue is the degree to which the program approached ultimate success. Correctional programs are finally aimed at cessation of criminal activity, not just reduction, and the absolute measures of success do not require reference to "before" behavior when addressing cessation.

The comparative before-and-after approach is appropriate when the question at issue is whether the community benefits from the correctional program, beyond whatever temporary savings are achieved by taking the delinquent off the streets for a few months. Put in its most elementary terms, the question is: Do these delinquents commit fewer offenses after the correctional program than they would have committed without a correctional program?

An analysis should tell the reader the basic results on both types of measures. But we argue that the before-and-after comparison is the appropriate focus of attention when the population being examined is the institutionalized delinquent, or any population of conspicuously chronic, serious delinquents.

We take this position for three reasons. First, by the nature of the system, almost all of these youth are *chronic*



*offenders*--youth who had been arrested not once or twice, but an average of more than a dozen times prior to commitment. Changes in behavior under these circumstances can be very large even though they do not reach cessation. Second, as we shall discuss shortly, the youth in institutions are drawn disproportionately from the inner city. When they leave the institution, these youth return to environments that exhibit all of the socioeconomic correlatives of crime: high unemployment, low incomes, one-parent or no-parent homes, indifferent schools, negative peer influences, and a variety of other conditions that work against whatever positive effects may have been produced by the correctional program. The impact of the correctional program, if such exists, must compete with countervailing factors. Some recidivism should not deflect attention from the gains which may have been made. Third, they return to their communities as highly visible individuals. Neighbors are likely to think of them first when a burglary or a purse-snatch occurs. So are the police. The delinquent who is newly released from an institution has a prominence as a suspect that raises the probability of arrest, regardless of criminal behavior.

On all of these grounds, we think that zero-based measures of recidivism are of only minor interest when dealing with the delinquents who went to DOC and UDIS. In a research sense, *the chronic delinquent must serve as his own control*, for our system of juvenile justice is so constructed that he has no other. In a substantive sense, the issue is not whether offenses go to zero, but whether they are reduced. The rest of this chapter introduces the reduction--the "suppression effect."

## THE SAMPLE

The sample is composed of 317 Chicago youth who were committed to the juvenile division of the Illinois Department of Corrections during the period 1 October 1974 through 31 July 1976--hereafter designated as "DOC" youth. Other populations of delinquents will be discussed as comparison groups in the following chapters, but these 317 will be the point of departure. They were chosen because they were subjected to the archetypical traditional form of juvenile corrections--a juvenile institution, also known as a training school, reform school, or reformatory. At the time of the research, the Department of Corrections in Illinois operated seven of these institutions. The youth in the DOC sample spent the great bulk of their time (more than 80 percent) in two of them: Saint Charles and Valley View, both moderate-security, campus-type facilities near the town of Saint Charles, roughly a two hour's drive from downtown Chicago.

### Police Record

The arrest records of these 317 youth were impressive. The typical member of the DOC sample experienced his first arrest a few months after his twelfth birthday, and then proceeded to run up another thirteen arrests in the next three and a half years before going to DOC. The typical offense history prior to commitment consisted of 8.2 arrests for what we shall term "index" offenses--in quotation marks, because the correspondence with the definition in the *Uniform Crime Reports* is not exact. The "index" category includes all person-to-person offenses that would be felonies if committed by an adult. All types of theft and robbery are included, along with assault, battery, homicide or attempted



homicide, rape, and other types of sexual assault. In all, the "index" category comprised well over half of all arrests. Of the remaining 5.4 arrests, 1.7 were for damage and trespass offenses; 2.7 were for possession offenses (e.g., possession of narcotics or stolen goods); and 1.0 was for a minor or status offense (e.g., disorderly conduct, traffic offenses, runaway, drinking underage). Note that the last category constitutes only seven percent of the arrests.

The figures for a few specific offenses will indicate the prevalence of the more serious felonies. Prior to being committed to DOC, the 317 youth in the DOC sample had accumulated 718 burglary charges (2.3 per subject), 317 battery, assault, or assault and battery charges (an average of exactly 1.0 per subject), 305 charges of auto theft or criminal trespass to vehicles (about one per subject), 183 armed robbery charges (about three per five subjects), 23 rape charges (about one per 14 subjects), and 14 homicide charges (about one per 23 subjects). It may fairly be said that the sample as a whole meets or exceeds any of the conditions, formal or informal, that have been used in the literature to define either a "chronic" or "serious" delinquent.

All of these arrests had occurred, it should be remembered, before the delinquent had encountered any correctional intervention other than supervision or probation. The typical member of our sample found himself in a DOC institution shortly before he turned 16, and served an average of 10.8 months before his first parole. We observed him for an average of almost 17 months following release. The exact figures are given in Table 1.1.

TABLE 1.1
Basic Career Variables for the DOC Sample (n=317)

| Variable | Mean | Standard Deviation |
|---|---|---|
| | (in years) | |
| Date of birth | 1959.7 | .98 |
| Age at onset | 12.2 | 1.86 |
| Total arrests prior to DOC | 13.6 | 6.96 |
| Age at commitment | 15.8 | .93 |
| Time served before first parole | .9 | .47 |
| Followup period | 1.4 | .82 |

Personal Characteristics

The typical member of the sample was born in late 1959, in the city of Chicago. Racially, 248 youth in the sample were classified as black (78.2 percent) 40 as white (12.6 percent), and 27 as Hispanic (8.5 percent). One was a native American and one was oriental.

Apart from these items, we did not collect background information on the entire DOC sample. Such information was obtained for a randomly selected subsample of 160, through examination of the case files maintained by DOC on each boy committed to the Department. The files were bulky, but information about the variables of interest was often missing or inconclusive; hence the reduced sample sizes in the following discussion. We have no way of knowing whether the sample means derived from this process are systematically biased by the missing cases, or in which direction. In any event, none of these variables is used in the analyses of recidivism.

*Adults in the Home.* Less than one in four of the subjects in the DOC sample (22.5 percent) was living with



both natural parents. More than half were living in a household with only one adult. The actual breakdown among the 160 members of the subsample was as follows:

| | |
|---|---|
| Two natural parents in household | 22.5% |
| One natural parent and one step-parent | 20.0% |
| Other two-adult arrangement | 1.3% |
| One natural parent | 45.6% |
| Other single adult arrangement | 6.3% |
| Foster care or orphanage | 4.4% |

*Parental Employment.* Unemployment in the sense of "unsuccessfully looking for a job" was a problem in fewer than 15 percent of the homes. But the sample of 113 for whom data could be obtained included a large proportion (41.6 percent) of youth whose families included no parents in the labor market. The breakdown was:

| | |
|---|---|
| Resident parent(s) in labor market, regularly employed | 44.3% |
| One parent regularly employed and one parent sometimes or chronically unemployed | 8.9% |
| Both or only resident parent(s) some times or chronically unemployed, despite being in the labor market | 5.3% |
| Both or only resident parent(s) are not in the labor market | 41.6% |

*School Performance.* We did not have access to school records, and the information in the case files was often scanty. Ratings on school performance and behavior were attempted whenever possible, which turned out to be in 66 cases. The results were:

| | |
|---|---|
| No school problems | 1.5% |
| Below average performance or occasional truancy | 24.3% |
| Below average performance and frequent truancy or behavioral problems | 48.5% |
| Failing grades and/or chronic truancy | 25.8% |

*Drug Use.* Two of every 11 DOC commitments had a noteworthy drug or alcohol problem ("sporadic," "chronic," or "clinical"). Whether this is an importantly larger proportion than would be found in the neighborhood high schools is a matter of conjecture. The sample size was 153.

| | |
|---|---|
| No known drug/alcohol use, or minor use | 52.3% |
| Occasional use of alcohol or marijuana | 30.1% |
| Sporadic or suspected use of hard drugs | 4.6% |
| Chronic use of drugs or alcohol | 13.1% |
| Clinical addiction | 0.0% |

*Psychological Problems.* We were unable to construct a rating of emotional and psychological makeup that was reliable across subjects and judges. The manifest variation in the bases that DOC psychologists used to arrive at their assessments was too great to be manageable. Our best estimate from the ratings we employed is that approximately 10 percent of the DOC sample exhibited psychological pathologies severe enough to require professional treatment.

When these background characteristics are combined, the profile that emerges is not markedly different from what the literature would lead one to expect. Socially and economically, the DOC commitments were predominately drawn from



disadvantaged groups. The DOC sample very closely matches the popular conception of the chronic urban delinquent. The question we are now ready to address is what happens to them after they have undergone a correctional intervention?

## ESTIMATES OF THE SUPPRESSION EFFECT

In the year before they were sent to DOC, the 317 DOC subjects were arrested an average of 6.3 times. After they were released, these same 317 boys were arrested an average of 2.9 times during an average followup period of 16.8 months on the street. The second figure is much lower than the first--67 percent lower, when the postrelease figure is converted to an annual rate. And this, in elementary form, is the phenomenon we have called the suppression effect. It can be shown graphically as in Figure 1.1, which plots arrest incidence in the two years preceding and following intervention.

A few general notes about this figure and the ones that follow. Except for the monthly data base (see following), "time" is measured in tenths of a year--computationally a much easier scale to work with than months. But "arrests per month" is much easer to interpret than "arrests per 36.525 days," so the ordinate in the graphs is always scaled in months.

Correctional research usually involves shifting sample sizes as subjects enter and leave incarceration. Figures will include an insert showing shifts in sample size over the period in question. The minimum sample size varies. If none is specified in the accompanying text, the minimum should be understood to be 25.

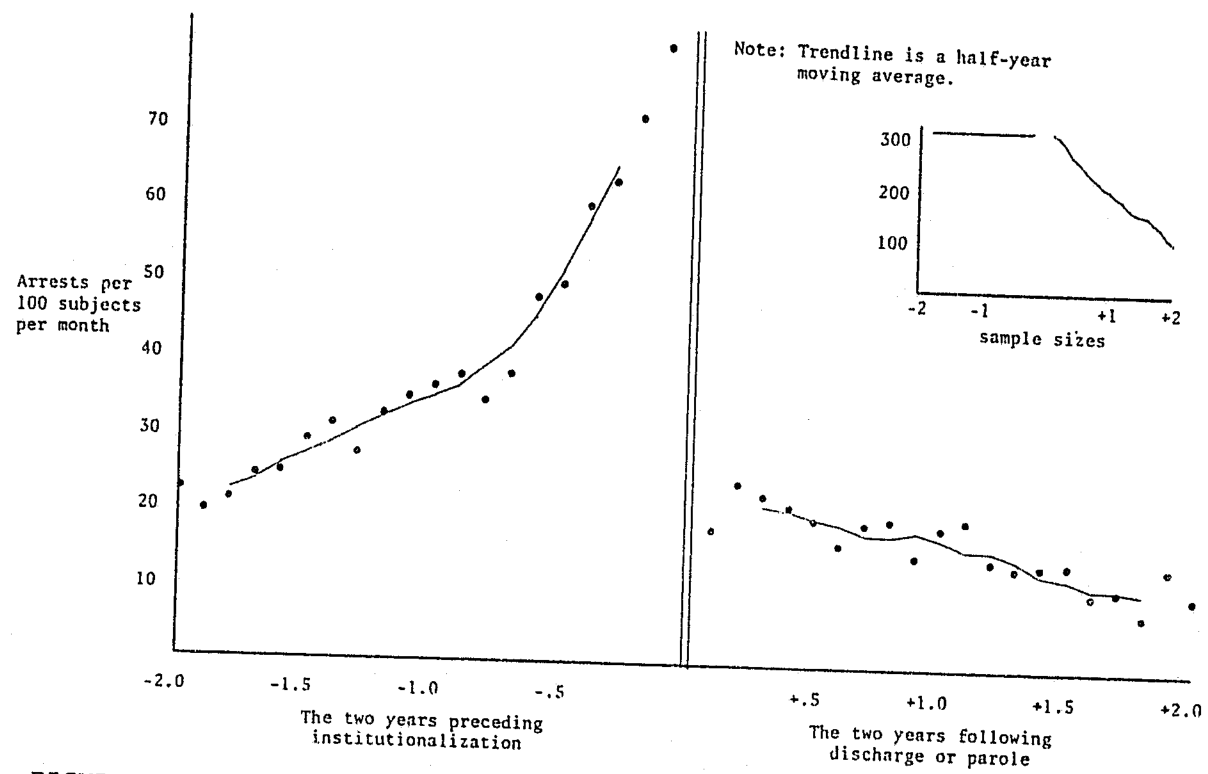



FIGURE 1.1
*Arrests in the Two Years Before and After Institutionalization*

"Suppression effect" refers to the phenomenon of the drop, not to any specific percentage magnitude attached to the drop. Nor is it especially useful from an analytic perspective to compute such a figure. It is partly a function of the periods of "before" and "after" observation that were chosen for the comparison, and singling out any particular number as *the* suppression effect is bound to raise disputes that deflect attention from the main argument--that a real and large reduction in offensive behavior has occurred because of the correctional intervention.

But in discussing the phenomenon, an actual value is useful for purposes of communication, and the range of reasonable values is relatively limited. Below we discuss the parameters of the suppression effect and the range



within which it plausibly falls. We also take this opportunity to introduce the analytic methods which we shall be using throughout the rest of the report.

## Methods of Calculation

The details of the methodology are given in Appendix A. A linear regression model is employed, using two configurations of the data base.

The first and more elaborate data base employs a series of observations for each subject. "Number of arrests" is the dependent variable. The independent variable for expressing "the fact of intervention" is a dummy variable that takes on the value of 0 if the period in question occurred prior to DOC and a value of 1 if the period occurred following release from DOC. The period during confinement is excluded from this analysis.

The data base as a whole contains up to 48 observations for each subject, stretching from two years prior to DOC (for all subjects) through two years following release (remember, omitting the months during institutionalization). If the followup period did not extend a full two years, the missing months are coded as missing data.

The advantage of the time-series model is its capacity to deal with variables that continued to change throughout the delinquent's career; specifically, age, and the number of prior offenses.

Except for the analyses of age and lagged prior offenses (Chapter three), the 48-month data base is collapsed into a simpler, dichotomous repeated-measures data base with two observations for each subject (first-year prior, and the postintervention period). For independent variables that have a single value, the collapsed data base is considerably more convenient and less expensive.

Defects of the collapsed model approach must also be noted. The distribution of the dependent variable in the postintervention observation is skewed about its mean, and the mean is correspondingly difficult to interpret. Part of the reason is that zero is the modal value in the postintervention case. The fact that the suppression effect exists implies that postintervention arrests tended toward the lower bound. The problem is then compounded by a peculiarity of this particular research situation.

In conducting a before-and-after analysis of a correctional program, it is essential that the definition of the follow-up period not exclude those delinquents who were "out" for a short period of time, then reincarcerated because of new offenses. The reason is compelling: if the only subjects permitted in the analysis are those who managed to stay on the streets for a given period of time (say, a year), it is guaranteed that the analysis will include no radically recidivistic subjects. Any such subjects will have been reincarcerated. The requirement for a year of street time will have had the effect of imposing a ceiling on the level of recidivism that will be observed.

Instead, the sample must include (as do all of those analyzed in this report) the reincarcerated failures. Their postrelease rate of annual offenses is weighted by the length of time on the street between first release and reentry into an institution. Thus, for example, a subject



who is released, is arrested twice, and reincarcerated after a month is treated as having an annual "after" arrest rate of 2÷(1÷12), or 24.

Having established the necessity of keeping these subjects in the sample, however, we come up against a dilemma: they cannot be excluded, but *the rate of post-release arrests assigned to them is systematically inflated relative to the method for computing the preintervention rate.*

The situation is analogous to the one that immediately precedes intervention. A delinquent is incarcerated or reincarcerated for the same reason: he has just committed at least one and possibly several offenses in the immediately preceding days or weeks. In the preintervention case, we deal with the situation by lengthening the preintervention observation period to a year. In the postintervention case, this solution is not available. If a delinquent is released from DOC and recommitted after one month, we are stuck with one month as the observation period. If he was arrested twice during that month, the only annual postrelease rate we can attach to him is 24. And to do so is precisely analogous to computing his preintervention rate on the basis of arrests in the one month immediately prior to intervention--which, of course, common sense tells us not to do.

The importance of this factor will become clear when the regression artifact is discussed in Chapter 3. For now, we simply observe that analyses using the collapsed version of the time-series data base inflates the estimate of post-release arrest rate relative to the estimate of preintervention arrest rate, and thereby deflates the estimate of the suppression effect by about 10 to 15 percentage points. This does not interfere with the comparisons among treatment strategies which are the purpose of those analyses.

## The Definition of Observation Periods

As noted, the size of the suppression effect is affected by decisions about the length of preintervention and post-intervention observation periods. We discuss each in turn.

In choosing the *preintervention period*, the objective is to choose one that produces the best estimate of the "true" level of delinquent activity that existed at the time when intervention was imposed. Is the youth who appears before the judge the boy who has just committed two robberies in the last ten days? The one who has committed four robberies since birth? Or something in between?

At one extreme, a period that includes several years is clearly too long. The delinquent activity of a boy committed at age 16 is not usefully estimated by including data about his delinquent activity at age 10, for example. More generally, a juvenile delinquent is by definition in the midst of a fast-changing developmental period of his life. In characterizing the youth's behavior pattern on any dimension (not just delinquency), history becomes a rapidly less reliable guide as it extends back from the here-and-now.

A much more difficult problem arises when we turn to the other extreme, the few months immediately prior to intervention. We know without doubt that part of the observed arrest rate in the second month prior to intervention is artificial. Given the typical delay between arrest and disposition, month "-2" is when the bulk of the "instant offenses"--the offenses that were the immediate reason for institutionalization--occurred. But whether the rate in the other months immediately prior to intervention is artificially inflated is open to dispute. We discuss the problem at length in Chapter 3.



The conservative assumption must be that behavior immediately prior to intervention is generally inflated, and we so assume in calculating the suppression effect. We shall use *the year prior to intervention* as the standard estimate of the "real rate" of delinquent behavior which the correctional intervention is intended to affect. We also compute the suppression effect when two years prior to intervention are included, to obtain a lower-bound estimate.

In choosing the *postintervention period*, there is one essential rule: the period used to compute rates of arrest must be expressed in "street time," rather than total elapsed time since first release. A minimum *qualifying* follow-up period must be expressed in elapsed time, for the reasons already explained, but even then street time is used to compute rates. Once that requirement has been observed, two separate issues are involved: (1) Should a maximum follow-up period be specified? (2) Should a minimum follow-up period be specified?

These decisions were made at the outset of the follow-on research, based on reactions to the UDIS evaluation. Those reactions were: (1) the suppression effect may have been short-term, and a longer follow-up period would diminish it substantially; and (2) the procedures employed did not adequately reflect the records of delinquents who had been released for very short periods. Therefore, before looking at the new data, the conservative approach--the one that would minimize the suppression effect--was thought to be one that incorporated all follow-up periods of whatever length. That remains our preference. But, as revealed in Figure 1.1, a problem has arisen: given the actual data, the measures that were urged do not tend to minimize the suppression effect; they tend to increase it.

To forestall questions about what the suppression effect "would have been" using other computations, we present in Table 1.2 the results which would have been found had other definitions been used. Included are a calculation of the suppression effect using only the first year after release, and an even more restrictive one in which the only members of the sample are those first released at least a year prior to the end of data collection. These are calculated both for the standard one-year preintervention baseline and the two-year "lower bound" period.

Using the year prior to intervention as the basis of the comparison, any of the postintervention approaches yields a suppression effect of at least 64.8 percent. Perhaps a more salient point relates to the computations using the two-year preintervention period. Despite its length, the reductions exceed 50 percent in all cases--an important drop by any definition.

When the collapsed data base is used, with its inflated postintervention measure, the basic comparison--one year prior with the entire postintervention period (expressed as an annual incidence) yields a reduction of 55.2 percent compared to 68.4 percent when the postintervention period is treated in monthly segments. The analyses in Chapter 4, (which use the collapsed data base exclusively) should be read with this discrepancy in mind--the reported reductions are all about 10-15 percentage points lower than a parallel measure produced by the monthly data base, or by a simple ratio based on offenses divided by person-years of street-time. We do not present the unbiased estimates of the suppression effect for these analyses. The comparison of interventions is the focus of attention, not the exact magnitude of the reduction. No matter how it is calculated, it remains large enough to be important.

TABLE 1.2
*The Suppression Effect Computed for Alternative Preintervention and Postintervention Observation Periods*

| Preintervention Period | Postintervention Period | Regression Results* $Y = A$ | $+ B_1 X_1$ | $SE\ B_1$ | Suppression Effect $(B_1/A)$ | Sample Sizes: Months Pre/Post | Sample Sizes: Subjects |
|---|---|---|---|---|---|---|---|
| One year | Up to 2 years | .525 | -.359 | .013 | .684 | 3804/4792 | 317 |
| | Up to 1 year | .525 | -.340 | .016 | .648 | 3804/3034 | 317 |
| | Exactly 1 year | .525 | -.340 | .017 | .648 | 3192/2703 | 266 |
| Two years | Up to 2 years | .400 | -.234 | .011 | .585 | 7608/4792 | 317 |
| | Up to 1 year | .400 | -.215 | .014 | .538 | 7608/3034 | 317 |
| | Exactly 1 year | .396 | -.211 | .015 | .533 | 6384/2703 | 266 |

<u>Notes</u> * In this case, the constant *A* represents the sample mean number of arrests during the preintervention period. The weight $B_1$ represents the reduction attributed to the intervention variable.

If only the traditional, zero-based measures of recidivism had been used, these reductions would have gone unnoticed, and institutionalization in Illinois would have been pronounced the failure that we all "know" institutionalization to be. If the criterion of failure is one arrest in the year following release, fully 82.3 percent of the DOC sample would be considered failures. That many were arrested again. Yet the aggregate number of arrests dropped by about two-thirds.

Are these apparently contradictory results the product of some illusion in the data, or do they indicate that most of our traditional measures have been missing the point? Does the correctional system really have it within its power to importantly reduce arrests among chronic juvenile offenders, or are we dealing with coincidental processes that falsely make the correctional intervention look like the cause?

These are the questions that have dominated the discussion of the suppression effect since the original UDIS evaluation was published. They are the first topic that we take up.



NOTES TO CHAPTER I

[1]For studies conducted prior to 1968, see Lipton, Martinson, and Wilks (1975). Some of the better examples since then of the binary approach to recidivism are Goldman (1970), The Minnesota Governor's Commission on Crime Prevention and Control (1975), Florida Department of Health and Rehabilitative Services (1975), Kawaguchi (1975), Michigan Office of Children and Youth Services (1976), and Minnesota Department of Corrections (1974). For this and subsequent citations of the literature, we greatfully acknowledge the assistance of Dr. Judith Wilks and Dr. Robert Martinson of the Center for Knowledge in Criminal Justice Planning in using the Center's unique collection of correctional studies.

[2]Studies that present data on numbers of postintervention arrests without also giving preintervention information are extremely rare. Examples are McEachern (1967), Warren (1967), and Persons (1967). Sometimes studies present data which lend themselves to varying degrees of interpretation, direct or inferred, about numbers of offenses--Hamparian et al. (1978), for example.

[3]The results of other before-after studies are discussed elsewhere. The leading examples are Empey and Lubeck (1971), Empey and Erickson (1972), Quay (1977), and Sasfy (1975). Only the Empey studies address the effects of institutionalization.

## 2. IT ONLY LOOKS THAT WAY: ALTERNATIVE EXPLANATIONS OF THE SUPPRESSION EFFECT

Upon encountering a large intervention effect contradicting conventional wisdom, the investigator's natural first reaction is suspicion. The potential for illusion in before-and-after comparisons is great. In the case of the suppression effect, the very novelty of the proposition that institutions have an important positive effect on crime rates combines with the many emotional and philosophical issues surrounding the treatment of youth to encourage skepticism. An especially detailed discussion of confounds and artifacts is warranted. In this discussion, we have not limited the analysis to the plausible sources; we have tried rather to be exhaustive. We also adopt a more formal style of exposition, to aid in keeping straight the many interrelationships among the alternative explanations to be examined.

### SKETCH OF THE ARGUMENT

Our objective is to lead the reader to share our conviction that the following interpretation of the suppression effect is correct:

> H: *Intervention causes a large reduction in crime rate,*



with intervention defined for now as institutionalization. We shall refer to this fundamental hypothesis as H.

H cannot be proved directly. Empirical observations disprove false hypotheses; they do not prove true ones. We use the empirical evidence to raise the *probability* that a given hypothesis is correct. In this case, we have inferred H from the following empirical state of affairs:

> E: *Observed arrest incidence increases up to intervention, recommences at a much lower level at the point of release, and decreases thereafter.*

This is the empirical relationship (E, hereafter) described in Chapter 1. To obtain H from E, it is sufficient to make two assumptions:

> A1: *The observations are accurate,*

in the sense that what we observed to be true of the delinquents in our sample is in fact true for the delinquent population at large; and

> A2: *The reduction in observed arrest incidence is caused by intervention,*

and is not merely coincidental with it.

If H is to be refuted, A1 or A2 must be challenged, because together with E (which is indisputably valid) they imply H.

A strategy for using E to establish H is thus suggested: (1) construct an exhaustive list of mutually exclusive hypotheses which, except for H, are generated by systematic denials of A1 and A2. (2) Examine whether any of these rival hypotheses to H can be consistent with E (and with other known truths). (3) If none can be, then H must be correct by process of elimination. We begin with a review of the list of objections we shall be considering.

Assumption A1 is that the observations are accurate. It really entails two separate propositions. First, it asserts that what we observe to be true of our sample is, in fact, true of our sample. This may be abbreviated as the "noiseless observations" proposition. Second, A1 asserts that what we observe to be true of our sample is also true of the population from which it is drawn. We shall call this the "representativeness" proposition.

Each of these propositions may be attacked separately.

Three potential sources of noise threaten the data collected for this study: (1) unobservable behavior, whereby a delinquent could commit an offense yet not be caught; (2) recording error, whereby the behavior recorded in the police files need not be the subject's actual behavior; and (3) transcription error, in the process of collecting the data and preparing them for analysis. The objections are thus

> O1: *A1 is incorrect because offensive behavior was not always detected*; and

> O2: *A1 is incorrect because of errors and omissions in the archival data*

To each of these must be attached the correlative assumptions that the errors in question are pervasive and nonrandom, and that their impact reverses its direction during intervention.



The representativeness proposition may be challenged by four exhaustive objections: the process of observation changes (1) the behavior of the sample; or (2) the behavior of the population; or (3) the relative compositions of the sample and the population; or (4) something else is coincidentally *and differentially* changing the sample and the population. In more elaborated form:

> O3: *A1 is incorrect because the process of observation changes the thing observed. Hence, the observed sample behavior no longer represents the behavior of the population from which it was drawn.*

> O4: *A1 is incorrect because the process of observation changes the things not observed (i.e., the population from which the sample is drawn). Hence, the observed sample behavior no longer represents the behavior of the population from which it was drawn.*

> O5: *A1 is incorrect because the process of observation selects the sample in such a way that it is not representative of the remaining population.*

> O6: *A1 is incorrect because some historical process unrelated to the process of observation coincidentally changed the sample but not the population; changed the population but not the sample; or changed both but in different ways.*

Now we turn to the objections to A2 (that the reduction is *caused* by intervention), which admits of only one, global objection:

O7: *A2 is incorrect because the reduction and the reversal in the trend of observed arrests would have occurred even if intervention had not.*

Three mechanisms by which O7 might come to be true are *maturation*, in which it is hypothesized that delinquents grow out of their criminal behavior; *regression*, in which it is hypothesized that the process of observation selects exactly those delinquents whose crime rate was about to decline anyway; and *history*, in which it is hypothesized that some other contemporaneous phenomenon would have caused arrest rates to behave as they did.

It happens that O7 is the most interesting and most complex of the objections, and that the explication of it provides some of the most important findings in the study. We therefore break the discussion into two chunks. This chapter concludes the examination of objections 1 to 6; Chapter 3 is devoted exclusively to O7.

## OBJECTIONS TO A1
## (THAT THE OBSERVATIONS ARE ACCURATE)

O1: *"A1 is incorrect because offensive behavior was not always detected."*

The probability that a delinquent will be caught, given that he has just committed an offense, is considerably less than 1. This drives a wedge between observed reality and actual reality--in particular, it raises the possibility that whatever reductions we observe involve much larger numbers of offenses than the data indicate. But the failure to detect all offenses does not in itself pose a threat to A1. If, for example, we always observe exactly one half of



the offenses which are committed by the subjects in the sample, then what we observe as increases and decreases in arrests per unit time will be exactly correct. To use O1 as an explanation for E, two postulates are needed:

> O1.1: *The probability of detection systematically increases prior to intervention; hence, apparent crime rate increases.*
>
> O1.2: *The probability of detection systematically declines after release; hence apparent crime rate decreases.*

For the moment, let us evaluate these postulates without reference to changes in the delinquent himself (those will be taken up elsewhere). How plausible are the postulates in reference to the behavior of the detectors (the police and the community)?

The argument that the rate of apprehension increases prior to intervention is consistent with labeling theory. Once a child becomes identified as the neighborhood trouble-maker, he will become an increasingly early suspect for any wrongdoing that occurs. Neighbors will be more likely to remember him when trying to reconstruct the circumstances surrounding a crime; the police will be more likely to pick him up for questioning.

The problem raised by this reasoning is that, by extension, apprehension rates will be at their highest among youth who have been conspicuous--the ones who were institutionalized. The labeling argument can explain a rising crime rate in the preintervention phase, but it backfires when it tries to explain the reduction in the postintervention phase.

O2: *"A1 is incorrect because of errors and omissions in the archival data."*

Reliance on the records of official delinquency is discussed in more detail in Appendix A of this report. Briefly, the records of the Chicago Police Department gave us no reason to believe that the problem of unwarranted arrests influenced the analysis. The accounts in the arrest records tended to be detailed, with specifics (e.g., the youth was caught in the act, or had the stolen merchandise in his possession, or was known by name to eyewitnesses) that provided considerable credibility. Questionable cases were encountered, but the frequency was statistically trivial. Short of assuming that the arresting officers were making up facts on a wholesale basis, the relationship of the allegations to actual behavior appears to have been close.

The procedures for accurately transcribing and preparing the data are also discussed in Appendix A. Cross-checks were employed at all steps in the procedure, from supervision of the data collectors in the police records to entry and conversions of the data in the computer files. It is also worth noting that the data collectors were blind to the date of intervention. They had no way of knowing whether an arrest occurred "before" or "after."

Presumably some noise is present, as in all large data bases. But the nature of the data and the precautions taken in their handling suggest that the number of errors is extremely small. And there is no evidence of any sort, nor any easily conceivable logic, that would encourage the assumptions 1.1 and 1.2, that the errors systematically inflated the preintervention arrest incidence and deflated the postintervention arrest incidence.



O3: *"A1 is incorrect because the process of observation changes the thing observed. Hence, the observed sample behavior no longer represents the behavior of the population from which it was drawn."*

In the terminology of Campbell and Stanley (1963), O3 may be interpreted as a problem with instrumentation.[1] Substantively, O3 raises what shall be called the "getting smarter" hypothesis about the effects of institutionalization.

Our "instrument" for measuring the offensive behavior of the delinquent is the Chicago police force. We have already alluded to one way in which the instrument might change over time--by becoming increasingly aware of the subject. But it is also conceivable that the delinquents get smarter, or warier, as a consequence of intervention. Only the apprehension rate drops, not the real incidence of offenses, and our observations are therefore not consistently representative of the behavior of the sample.

This invokes a popular belief that institutions are "schools for crime"--an assertion that has been repeated so often that it has attained the status of a truism. Insofar as we have been able to determine, the systematic evidence for the proposition is nil. But anecdotes are plentiful, and widely used as a basis for extrapolations. Two quite distinct notions are embedded in the "schools for crime" hypothesis, one much more plausible than the other.

The more plausible notion is that institutionalized youngsters expand their repertoires of potential crimes. It could easily be true. Methods for hot-wiring a car, boosting merchandise from a store, or getting past apartment security systems include gimmicks that are easy to apply, and an institution brings together the youth who are most likely to be familiar with them.

The second notion is less intuitive and more profound. It posits that experience in the institution lowers the chances of getting caught. Note that this potential result is not necessarily complementary to or even consistent with the possibility of learning new ways to commit crimes. Learning new techniques for committing crimes could as easily raise apprehensions as lower them, by several logics (e.g., the newly learned crimes are riskier, or the new knowledge simply creates new opportunities and more offenses).

But the getting smarter hypothesis is not directly falsifiable. Further observations cannot be used to falsify a hypothesis that posits the inaccuracy of such observations.[2] We must rely on indirect evaluations, based on assessments of the assumptions underlying it and the consequences implied by it.

First, consider the assumptions. For example...

Is it true that *large numbers* of the youth in the sample have better access to information on how to get away with crime in DOC than in their own neighborhoods and schoolyards?

Is it true that *large numbers* of these youth are even attentive to crime as a discipline, as a craft?

Is it true that *large numbers* of these boys are not only attentive to the question of professional approach but ready to apply what amounts to a sophisticated learning-application sequence?

Is it true that *large numbers* of these boys want to become more cautious and inconspicuous, rather than to do bigger and more visible crimes?



More generally, we are asking if it is really true that a large sample of delinquents can continue to commit offenses at the same rate as prior to intervention, but reduce the rate of apprehensions by any figure approaching two-thirds (the magnitude of the suppression effect). It would be an impressive achievement.

We find it plausible that some boys consciously did want to become pros, with all that implies about taking on a new state of mind and a detached, calculating approach to crime. It is plausible that some of the boys who wanted to become pros actually managed to change their behavior. It is even possible that some of the boys who managed to change their behavior successfully reduced their apprehension rate. But this subset of a subset of a subset would seem to provide credible explanations for only a small proportion of the reductions that were observed.

Such are the assumptions. For examining the implications, the relevant datum is not number of arrests by all youth who continued to be apprehended, but rather the elapsed time between them--what Hamparian et al. (1978) have termed the "velocity" of the offensive career. More precisely, the phenomenon in question is elapsed time between the first opportunity to commit a new offense, and the next arrest.

It has been known for many years that time-between-arrests follows a predictable pattern (e.g., Wolfgang et al., 1972). Figure 2.1 shows it for the DOC sample.

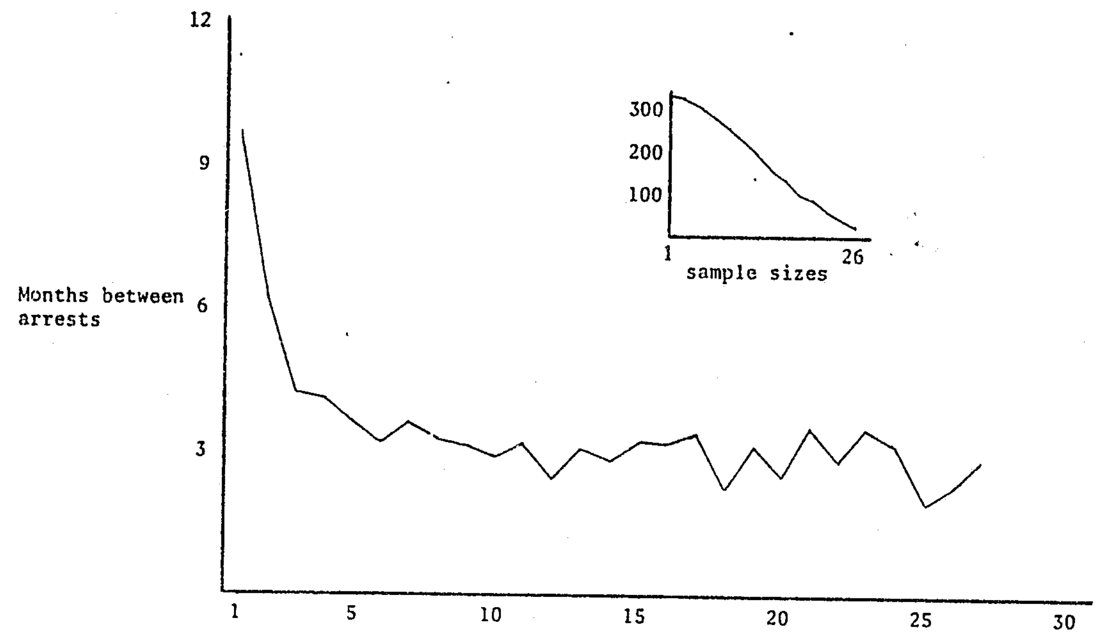



FIGURE 2.1
*Time Between Arrests, Ignoring Intervention Status*

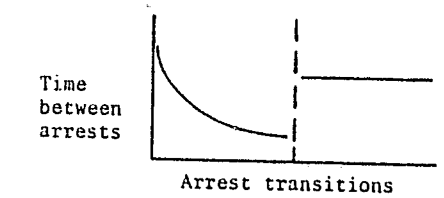
The figure reproduces with remarkable fidelity comparable plots in both of the cited studies. The observed velocity of offenses increases rapidly during the first four or five transitions, then stabilizes.

For the moment, we shall ignore the question of *why* the velocity increases so rapidly in the first few transitions. We simply note that by the fifth transition a flat, predictable line has occurred; and that 293 of the 317 members of the DOC sample (92.4 percent) were arrested at least five times.

The next step is to sort the arrest transitions into two piles: those that occurred prior to institutionalization, and those that occurred after. In the "after" case, elapsed time is measured from date of release, *not* (for obvious reasons) from date of the last preintervention offense. Now, consider the logic of the getting smarter hypothesis and how the patterns should look if it is true.

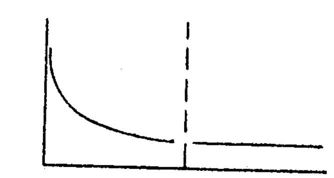


If the institutionalization is a comprehensive learning experience and the youngster emerges with an immediately lowered apprehension rate, one pattern we might observe is this:

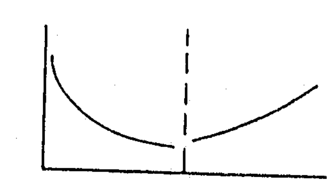



It looks as if a suppression effect has occurred, but in fact we are simply observing a reduced fraction of the same real offense rate that previously existed. The boy is caught less often.

If the boy not only got smarter but increased in real offense rate, the pattern would look something like this. The preintervention velocity would appear to remain constant, but again only because of the change in apprehension rates.



Suppose that the learning in the institution has to be practiced--once out of the institution, the boy is embarked on an increasingly professional career. The pattern would take a shape like this:



As he becomes increasingly practiced, he becomes increasingly difficult to catch.

All three of the above patterns are ambiguous. Besides acting as evidence for changed apprehension rate, the first would be consistent with a genuine suppression effect. The second would be consistent with arguments that the suppression effect is artifact, and represents no real behavioral change. The third would be consistent with a maturation effect. None of them, however, would reproduce the empirically observed postintervention curve, shown as part of Figure 2.2.

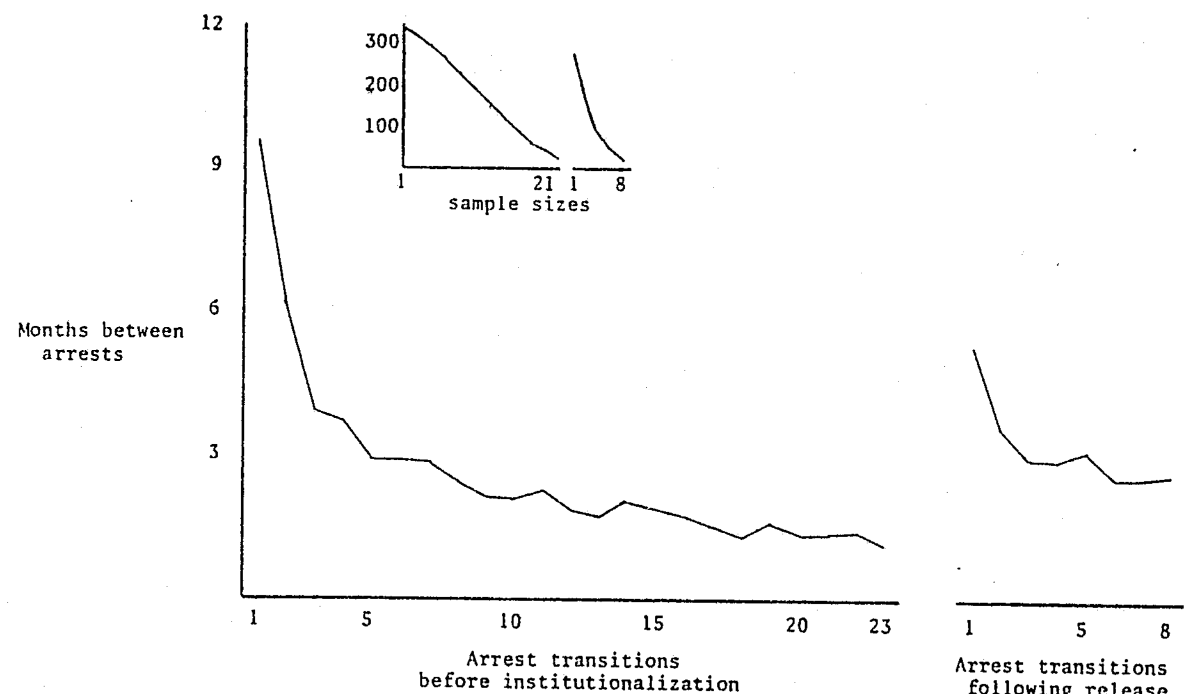



FIGURE 2.2
*Time Between Arrests Before and After Institutionalization*

This curve can be explained by the getting-smarter hypothesis (as, indeed, can any of the possible postrelease plots). But now on top of the other assumptions that have been made, we are forced to add one more: the youth is smart, wary, able to lower his apprehension rate upon getting out. But if he gets caught, and is not reincarcerated, he slips back into his old habits. Once again, as in the case of labeling, the logic usually used to explain away the suppression effect converges with a logic that argues for its reality.



To see this, consider again the steep rise in velocity (or drop in elapsed time between arrest transitions) in the early phases of the delinquent career. What best explains it?

One option is to argue for a weeding-out process. A small core of natural, intractably recidivist delinquents was committing offenses all along, at a constant (high) rate. The other youngsters drop out, and the velocity of the remaining population increasingly reflects the velocity of the core.

The alternative logic invokes a deterrence phenomenon. Interpretively: a juvenile is arrested for the first time. It is a frightening experience. Warnings of the dire consequences of another arrest are communicated. The threats have some credibility. Many youth never reappear in the police station to find out. But for the ones who do test the system again, and are again released, the threat of sanctions has lost some of its credibility. Among a large sample of boys, this means increased offensive activity. Given generally constant probability of getting caught, the mean interval between the second and third arrests shortens. And so on, through the first four or five transitions, by which time the credibility of sanctions has reached a floor. Given the empirical pattern, it appears near the mark to say that credibility of the initial threat has a half life of one arrest--the decay is exponential.

The consistency of this argument with the observed post-intervention pattern is striking. Suddenly, after threats have lost any meaning, the credibility of sanctions has jumped in the only way that it could--by the fact of sanctions having been imposed. Post-release velocity is slowed markedly from its immediate preintervention rate. Then velocity again increases, as arrests occur without resulting in reincarceration.

In a technical sense, the question of what is happening to the real offense rate remains open. The pattern of velocity is consistent with either a suppression effect or with a "getting careless" explanation. But an important new element has to be added to the getting-smarter rationale. In its pure form, that rationale argues that institutions only inspire and educate youth to commit more and better crimes, and that deterrence plays no role. Any claim that a primary motivation for the learning process is to avoid incarceration must be discounted, because *to accept that deterrence has an impact on behavior is to be pointed, in one way or another, toward penalties and enforcement as a method of dealing with delinquency.* Less theoretically and probably closer to the mechanics behind the parallel J-curves in the before and after cases: given the tenuousness of the assumptions underlying the getting-smarter hypothesis, it is easier to see the phenomenon in terms of reduced offenses than as one of reduced apprehensions.

O4: *"A1 is incorrect because the process of observation changes the things not observed."*

O4 raises the possibility that what is true of the sample we studied will not be true of future samples. Suppose, for example, that Cook County Juvenile Court suddenly tripled or quadrupled its use of institutionalization. Suppose further that this had the effect of deterring large numbers of delinquents from further activity. Then it is



plausible to suppose that the youth who *did* continue to be active delinquents, and subsequently became institutionalized, would be of a hardier, less impressionable character than their counterparts who were institutionalized in earlier years. And, in consequence, it might be found that the suppression effect would be far different.

A hypothetical case is necessary because O4 is a problem of the future, not of the present. We acknowledge the possibility that changing treatment philosophies could produce these kinds of changing degrees or types of effect. However, as the discussion of the problem of history will indicate, our findings seem robust.

O5: *"A1 is incorrect because the process of observation selects the sample in such a way that it is not representative of the remaining population."*

The correctional process fosters this artifact in three ways: (1) by keeping some delinquents in institutions for so long that we have no opportunity to measure their postrelease behavior ("missing delinquents"); (2) by reincarcerating them, thereby truncating the measurement of postrelease behavior ("disappearing delinquents"); and (3) by admitting only "unrepresentative" delinquents to institutions in the first place. We discuss each separately, deferring case 3 (the "Regression Artifact") until the next chapter.

## The Missing Delinquents

When the observations ended at the end of February 1978, 49 youth who had entered DOC between 1 October 1974 and 31 July 1976 had not yet been released for any period of time. They represent 13.4 percent of the (317 + 49 = 366) DOC youth for whom we were able to obtain complete court, police, and institutional records.

The set of missing delinquents was top-heavy with youth whose instant offense was a homicide charge--17 of the 49, or 34.7 percent, compared to nine instant-offense homicides among the 317 who had been released. In most of these cases, the youth had been tried as an adult and transferred to an adult institution directly from the Youth Division.

Objection 05 now takes the form of an hypothesis that these 49 are the most recidivistic, most intractable youth, and that they would have increased the aggregate postrelease rate had they been given a chance.

The missing delinquents can be investigated in several ways: the relationship between time-served and subsequent recidivism (slightly, insignificantly negative), the relationship between the number of prior offenses and the suppression effect (effectively nil, as just discussed), and the relationship between commitment for homicide and subsequent recidivism (the homicidal delinquents do slightly better). But the simplest, least ambiguous way to deal with the issue is to break the sample into two cohorts with markedly different mortality rates. The two cohorts are subjects who went into DOC during the first year of observation (10/1/74 - 9/30/75), and those who entered during the final nine months of the observation period (10/1/75 - 6/30/76). The first cohort had more time to get out, and there were correspondingly fewer missing delinquents. To be precise:

| | Aggregate n | % Not Released | Net n | Suppression effect |
|---|---|---|---|---|
| 10/74 - 9/75 Cohort | 248 | 7.7 | 229 | .674 |
| 10/75 - 6/76 Cohort | 118 | 25.4 | 88 | .669 |



The difference in mortality is large; the difference in suppression effect is .005. More to the point, the mortality in the first cohort is so low that even extreme assumptions about the residual incarcerated youth make little difference. Suppose, for example, that the 7.7 percent had gotten out and all showed zero suppression effect--were arrested at the same rate as before incarceration--and were each reincarcerated after only two months out. This would reduce the suppression effect for the first cohort from .674 to .668, or by .006. And so on through successively worse worst-case assumptions, whether for the first cohort or for the combined sample. Even hypotheses of future mayhem by the unreleased fraction fail to shake the degree of estimated sample suppression by more than a few percentage points.

## Disappearing Delinquents

The disappearing reincarcerated delinquents were discussed in Chapter 1. As pointed out, the analyses give full weight to them. The month-by-month data base includes all postrelease months. The collapsed data base assumes that the postrelease activity that led to reincarceration--no matter how short the observation period--would have been representative of behavior had reincarceration not occurred (see Chapter 1.)

06: *"A1 is incorrect because some historical process unrelated to the process of observation coincidentally changed the sample but not the population; changed the population but not the sample; or changed both but in different ways."*

06 is distinct from the confounding role of history in general (discussed in Chapter 3). 06 posits that some outside factor *differentially* affects sample and population.

We include O6 for the sake of exhausting the possible objections to A1. But we have no plausible scenarios whereby O6 could have been valid in the present case. The requirement in O6 that the outside factor have one effect on the sample, another effect on the population, and be unrelated to the process of observation drastically limits the possibilities.



NOTES TO CHAPTER 2

[1]We acknowledge our indebtedness to this famous monograph for much of the discussion in Chapters 2 and 3.

[2]Self-report would seem to be the only technique for a direct test. But that too would pose formidable problems. It would require long-term self-report data and truthful reporting of felonies for which the respondent had not been apprehended.

## 3. THEY WOULD HAVE QUIT ANYWAY: MATURATION, REGRESSION, AND HISTORY

The last of the objections to our interpretation, H, of the suppression effect attacks the central assumption of causality:

> O7: *A2 is incorrect because the reduction and the reversal in the trend of observed arrests would have occurred even if intervention had not.*

The reductions could instead have been caused by *maturation*, by the phenomenon known as *the regression artifact*, or by *historical events* that operated contemporaneously with the period of the study.

### GROWING OUT OF IT: THE ROLE OF MATURATION

Maturation is widely accepted as an explanation of delinquent behavior. The popular argument is that delinquency is largely a developmental phenomenon, increasing in the early phases of adolescence and falling off thereafter. "The best cure for delinquency is growing up," is one catch-phrase in use. The major function of the juvenile justice system, say the proponents of maturation explanations, should be to give the youngster a chance to grow up with as little damage as possible being inflicted from outside.



Accordingly, one of the most common reactions upon first seeing a plot of the suppression effect is to ask whether the drop can be explained by maturation. The youth are arrested at a high point in their activity; cool their heels in an institution for perhaps a year; and then emerge older, less delinquent because they have passed the peak of their propensity to be delinquent, but have not really been affected by the intervention *per se*.

The reality of maturation is far from established in the scientific literature. In fact, persuasive evidence for maturation has yet to be marshaled in any study (including the Gluecks' famous expositions of it). The reader is referred to Hamparian et al. (1978) for a recent review of relevant work (pp. 14-18) and to their analysis of violent offenders and "extinction" of the delinquent career (pp. (72-73, 131-132).

Our task here, however, is not to prove or refute the reality of maturation as it applies to *any* delinquent, but as it applies to the chronic offenders of our study. That task lends itself to some relatively clean, unambiguous analyses.

The implication of the maturation hypothesis is that samples of chronic delinquents of roughly the same age will behave roughly the same way, whether or not they have undergone intervention. And it is this implication that gives us leverage in investigating the degree to which maturation can account for the suppression effect. For, fortunately from a research standpoint, the decision to intervene takes place at various ages and at various points in the delinquent career. Some delinquents go into institutions at age 15; some come out at age 15; some are institutionalized after 5 offenses; some after 10 or 20 or 30. Whether maturation is defined in terms of physical age or point in career, the juvenile justice system provides variance in abundance. We take up each definition in turn.

## Chronological Maturation

The first maturation model takes chronological age as the agent of maturation.

The time-series data base is employed, with "month" as the unit of analysis and *number of arrests* during the month as the dependent variable. The DOC sample is used. The time period encompasses the year prior to institutionalization and the postinstitutionalization observation period up to two years.

The multiple regression equation considers the effects of two independent variables: the subject's *age at the beginning of the month,* and whether the month *occurred before intervention* (coded as 0) *or following release* (coded as 1). The results are shown in Table 3.1 below.

When intervention is taken into account, the relationship connecting age to arrests is overwhelmed by other factors. This is partly because of the pre- and postintervention periods chosen for analysis. As we shall see shortly, arrests are positively related to age before intervention and negatively related after. In the present analysis, the "before" observations comprise slightly less than half of the total (3,804 out of 8,596), and the upward and downward pulls are roughly balanced.

The appropriate next step is to test for the interaction of age with intervention. But as the high (.751) correlation between age and the dummy variable representing intervention



leads one to expect, the interaction term is nearly multicollinear with age and intervention and cannot be used reliably.

TABLE 3.1

*Regression Analysis of Arrests with Age and Intervention*

| | B | SE | p< |
|---|---|---|---|
| I. Intervention and age entered together | | | |
| Intervention | -.355 | .020 | .001 |
| Age | -.002 | .007 | NS |
| Constant | +.552 | | |
| II. Intervention entered alone | | | |
| Intervention | -.359 | .013 | .001 |
| Constant | +.552 | | |
| III. Age entered alone | | | |
| Age | -.089 | .004 | .001 |
| Constant | +1.798 | | |

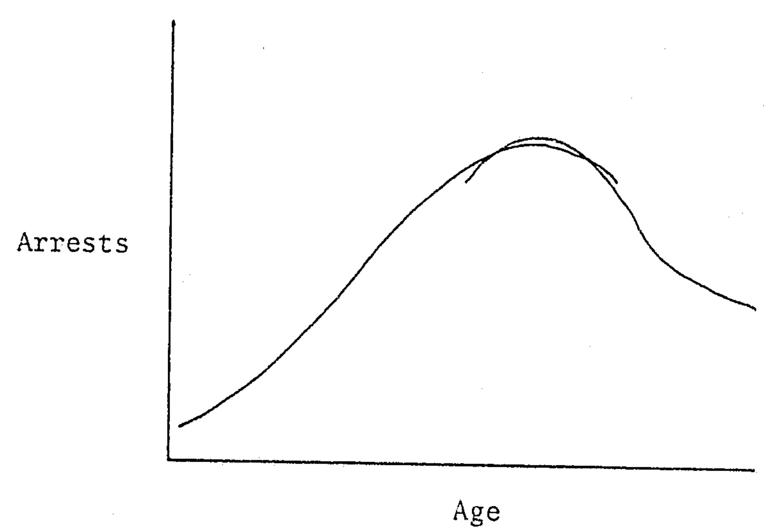
$r_{age.intervention} = .751$
observations = 8,596
subjects = 317

This leads to a more general issue. A correlation of .751 could mean that multicollinearity is leading to unstable estimates of the relative relationship. Taken as the only independent variable, ignoring intervention, age *does* seem to account for some of the suppression effect. Note the regression coefficient of -.089 for model III in Table 3.1. Why not ascribe the cause to maturation?

The problem is not intractable. We move back from the summary statistics of the regression model to the raw data, and visualize how they will look if maturation is the cause, or if intervention is the cause.

We begin with the obvious, simple question: how does arrest incidence vary with age once the fact of intervention is taken into account?

For the answer, we again sort the arrests into two piles, as we did for elapsed time between arrests--that is, arrests occurring prior to institutionalization and arrests occurring following release. We then plot them against age-at-time-of-arrest as the X-axis. To the extent that a maturation phenomenon was setting in "anyway," the plot should look like this hypothetical one:




The overlapping segments of the plot denote samples of boys who were in the pre and post states at the same age. Given no intervention effect, the lines should comingle throughout the overlapping segment.

Figure 3.1 presents the actual data, and they provide a dramatic plot. Visually, a "maturation effect" does appear to exist, but in a radically different form then the bell-shaped curve implied. Arrests increase with age *throughout* the preintervention period; they decrease slightly with age in the postintervention period. And, most conspicuously, arrest rates for postintervention delinquents are much lower than for preintervention delinquents of the same age.



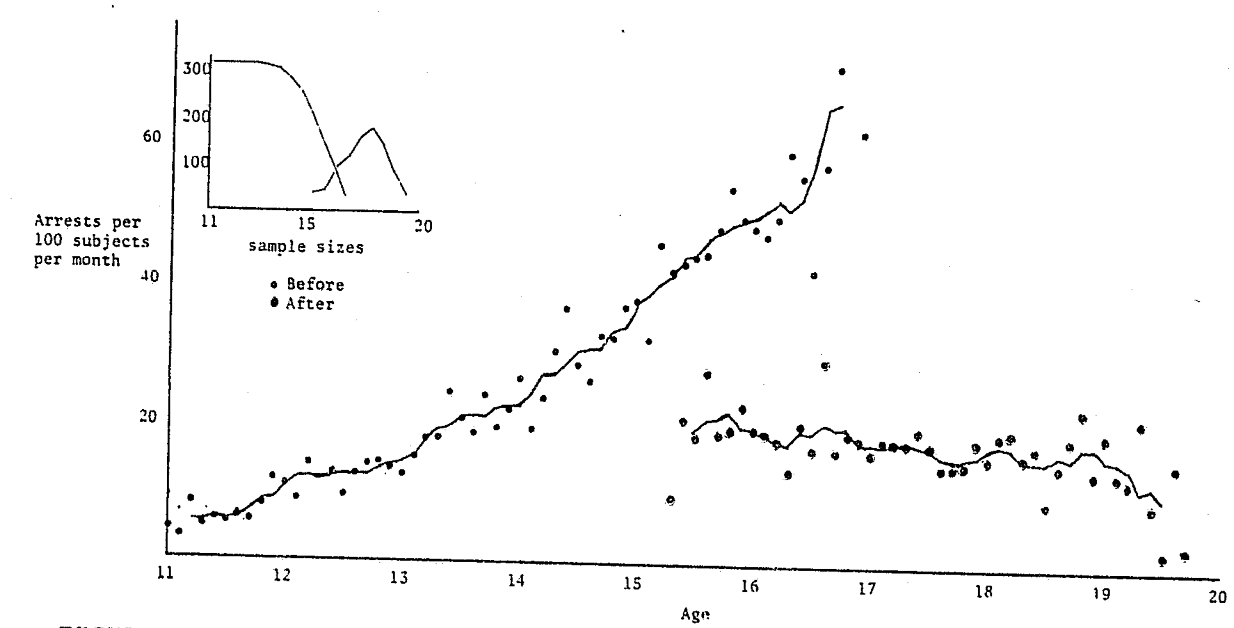



FIGURE 3.1
*Arrests and Age, Before and After Institutionalization*

A first question upon examining the plot in Figure 4.5 is whether the upward trend in the preintervention period is being pushed by a selection artifact. Throughout most of the age periods, up until roughly the sixteenth year, the answer is essentially no. The arrest rates displayed for, say, the 14 year-olds were produced by a mix of boys who be institutionalized any time from the following week to as long as three years later. But as age increases, the proportion of boys who were close to intervention also increases. By the last points on the preintervention plot (the calculations stopped when the number of unintervened boys fell below 25), almost all of the remaining delinquents are within a few months of intervention, and we must presume that the selection artifact again steepens the slope. We therefore computed a parallel plot, shown in Figure 3.2, which avoids this problem.

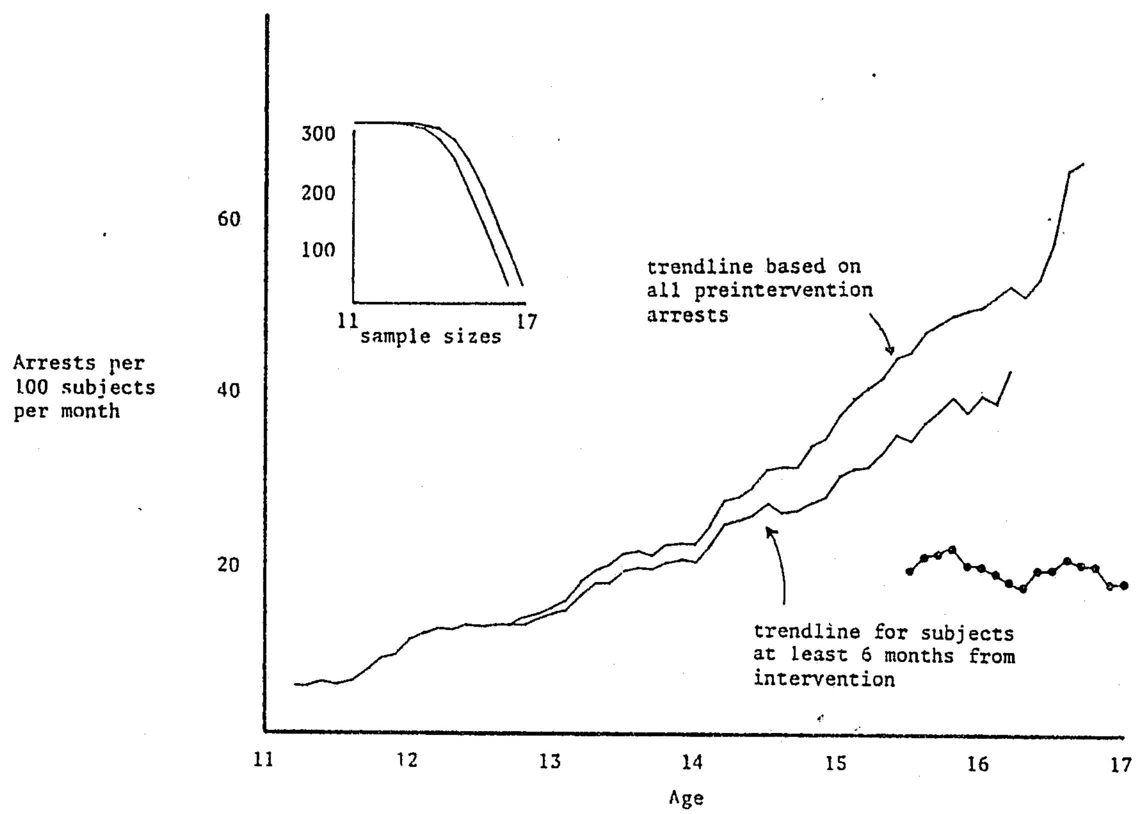



FIGURE 3.2
*Arrests and Age Before and After Institutionalization, Ignoring the Six Months Immediately Preceding Commitment*

In this plot, a youth was dropped from the preintervention sample when he came within half a year of intervention. The comparison on the overlapping segments of the line is thus between postintervention youth and the behavior of delinquents the same age who were still at least six months from being committed. As the figure indicates, the inferences about trendlines are unchanged. We should add that the cutoff can be extended to a year, or even 18 months, without altering these inferences.

It is important to emphasize that these plots cannot be interpreted as evidence that a maturation effect based on chronological age does not exist beyond the age boundary at which we ran out of preintervention subjects. It is possible that a large maturation reduction occurs at ages of 18, 19,



or even 17 1/2. The question is not whether chronic delinquents *ever* grow out of delinquency, but rather (a) whether they are growing out of it during the ages over which the juvenile justice system has jurisdiction; and (b) whether intervention can accomplish reductions that would not have occurred because of age.

## Maturation as Point-in-Career

A second way to view maturation is as a career phenomenon. As noted, the average number of preintervention arrests was 13.6. Perhaps the juvenile justice system picked them for institutionalization at just the time that the delinquent disease had run its course.

The unit of observation remains the month. Intervention is one of the independent variables; the other is cumulative number of offenses prior to the month in question. The results are shown in Table 3.2.

The results again indicate that the "maturation" element--represented by number of prior offenses in this case--exerts no independent downward pull on the arrest rate. Whether entered alone or along with intervention, the regression coefficients are positive. But the interaction of intervention and prior offenses does show a negative effect.

TABLE 3.2

*Regression Analysis of Intervention and Prior Offenses*

| | B | SE | p< |
|---|---|---|---|
| **I. Intervention and prior offenses entered together** | | | |
| Intervention | -.429 | .014 | .001 |
| Prior offenses | +.014 | .001 | .001 |
| Constant | +.434 | | |
| **II. Intervention entered alone** | | | |
| Intervention | -.359 | .013 | |
| Constant | +.525 | | |
| **III. Prior offenses entered alone** | | | |
| Prior offenses | +.002 | | |
| Constant | .311 | | |
| **IV. With the interaction term** | | | |
| Intervention | -.311 | .024 | .001 |
| Prior offenses | +.022 | .002 | .001 |
| Interaction term | -.014 | .002 | .001 |
| Constant | +.373 | | |

| Correlations | 1 | 2 |
|---|---|---|
| 1. Intervention | -- | |
| 2. Prior offenses | .381 | -- |
| 3. Interaction term | .763 | .772 |

Observations: 8,596
Subjects: 317



To translate the size of this negative effect into more readily interpretable form, we divide the sample into four groups, based on number of arrests in the two years prior to intervention: 0 to 5, 6 to 10, 11 to 15, and 16 to 20. This encompasses all but 7 of the 317 members of the DOC sample. The results are shown in Table 3.3.

TABLE 3.3
*Prior Offenses and the Magnitude of the Suppression Effect*

| Total arrests, 2 years prior to committment | N | Total arrests, year prior | Total arrests, postrelease | No. of person years of street time, post-release | Reduction |
|---|---|---|---|---|---|
| 1-5 | 70 | 194 | 135 | 111.04 | -.561 |
| 6-10 | 119 | 653 | 303 | 168.73 | -.673 |
| 11-15 | 91 | 727 | 303 | 118.37 | -.680 |
| 16-20 | 30 | 309 | 126 | 37.43 | -.673 |
| Overall | 317* | 1,990 | 906 | 443.49 | -.675 |

*Seven members of the sample had more than 20 arrests in the two years prior, and are not included in the subgroup.

For youth with five or fewer arrests in the two years before commitment, the reduction was noticeably lower: .561. But, somewhat surprisingly, the reductions among the other three groups were nearly identical, even though the range of preintervention offenses went from 6 to 20.

In some respects, the .561 reduction for the 70 youth with only one to five arrests in the two-years-prior is equally surprising. It may be lower than that of the other three groups--but it is still large. The implications are obvious: if a large reduction had been found to occur only after many preintervention arrests, then the advantages of intervening early are small. If a large reduction results no matter when intervention occurs, then the incentive to intervene early is increased, to maximize the preventive effects of the action.

### Age and Prior Offenses Taken Together

For completeness, we present the results when intervention, age, and number of prior offenses are entered together:

| | B | SE | p< |
|---|---|---|---|
| Intervention | -.409 | .021 | .001 |
| Age | -.009 | .007 | ns |
| Prior offenses | +.014 | .001 | .001 |
| Constant | +.573 | | |

Nothing new is suggested. We may summarize our findings as follows:

There is a maturation effect among the delinquents of this study, but one quite unlike the popular conception of a bell-shaped curve. *In terms of arrest rates, the direction of the maturation effect* before *intervention was up. The direction of the maturation effect* after *intervention is down.* The catalyst that shifts the direction of the maturation effect, at least through the seventeenth year, appears to have been intervention. *Causality did not work the other way around: our alternative proposition that the suppression effect is attributable to age is demonstrably incorrect.*

## THE REGRESSION ARTIFACT

The phenomenon known as the regression artifact is essentially a natural drop (or rise) from an abnormally high (or low) state of affairs. This change is not "caused" by anything except the laws of probability. Thus, a seven-foot man could have a tall son, but it is unlikely that he too will reach seven feet.



In the context of the suppression effect, the logic of the regression artifact is simple. It does require, however, three strong assumptions. These are

- that delinquents have a certain "natural" crime rate, which remains constant throughout the delinquent career;
- that short-run fluctuations occur about the long-run natural rate; and
- that only those delinquents are selected for intervention who have just gone through a fluctuation raising their instantaneous crime rate well above its natural level.

From these assumptions, it follows that those delinquents whom we observe must have recently triggered the juvenile justice system by an atypically high burst of delinquent activity. We may expect such abnormally high levels to return to their natural level, also known as the "expected" or "mean" level. This return is called "regression toward the mean," and is independent of whether intervention has been applied.

What are we to make of this argument? It clearly can account for a simple reduction from the preintervention to the postintervention period. Just as clearly, no amount of fresh observations can discredit this artifactual explanation, because the argument explicitly rests on the premise that the observations themselves are imperfect.

The procedure for examining the regression argument in more detail takes the same form as before. We examine its assumptions and implications, to see what they lead us to expect of the data.

## The Assumptions

The three assumptions listed above are the essence of a regression argument, and they satisfactorily explain a reduction. But they do not fully explain E, our empirical base. Operating only under the three assumptions listed above, the regression artifact argument predicts a high *constant* preintervention aggregate crime rate, followed by a lower, *constant* postintervention aggregate crime rate (e.g., see Maltz and Pollock, 1978, for application to a Poisson process). But the actual data show rising and falling crime rates instead.

The regression artifact can explain the rise and fall in the data only if the argument is reinforced by other, less plausible assumptions. In the Maltz and Pollock discussion, the additional assumption was that judges estimate individual crime rates from a random amount of past behavior. Sometimes a judge will base his decision on what the delinquent has done during the past two years; sometimes on his behavior during the past two months; and *this choice of period will be unrelated to any substantive characteristics of the behavior pattern over that period*. We are unable to devise any other, more plausible assumption which can successfully be combined with the assumptions of the regression artifact to explain E. And yet, while some skepticism about the rationality of the juvenile justice system is warranted, this extreme assumption is nonsensical--libelously so,



juvenile judges would presumably argue. But some assumption of this sort is required to produce the pattern shown in Figure 1.1 that opened the discussion of the suppression effect. The assumptions of the regression argument itself are not enough to produce the upward slope in the preintervention phase.

## The Implications

The most direct way of testing the regression artifact's validity is to find whether the crime rate "falls anyway" in a control group which has not undergone intervention. But this brings us squarely up against the selection argument. If intervention acts as a perfect screen, there can be no control group. The reason, of course, is that any selection artifact depends on the existence of some systematic distinction between those who are selected and those who are not. Thus, those who are not selected cannot serve as a control for those who are: if they were sufficiently similar to constitute an adequate control group, then presumably they themselves would have been selected as well.

The implicit assumption in the "no control group" argument is that delinquents with similar histories are treated similarly. But, as we have just pointed out, this assumption *cannot* be true if the regression artifact is to be used to explain the upward slope in the preintervention period. To save the regression artifact as the explanation for the data, we must postulate that selection is imperfect: some delinquents escape intervention through this "hole in the net." Those who escape (temporarily, at least) form a legitimate comparison group by means of which the regression artifact may be tested.

This modification in assumptions permits the maturation analysis to take on additional analytic burdens. The fact that arrest incidence rises with age throughout the preintervention period, even though the analysis is restricted to events that took place at least six months prior to intervention, strongly suggests that the image of a natural, even rate, punctuated by bursts of activity, does not fit the DOC sample. Given the modification in assumption, these youth may be seen as acting as their own controls--"escaping the net" for a time--and we find no evidence that their activity was going to level out, let alone decline.[1]

Together with the information from the maturation analysis, we may interpret our findings as follows. Before intervention, the "average" natural arrest rate of a set of delinquents increases. Random fluctuations may occur, but they are centered on a mean level that *rises* through time. Presumably this rise would level out eventually. A saturation point of offensiveness must exist. But there is no empirical reason for believing that it will turn downward during the period that the law defines this set of delinquents as juvenile, unless intervention is applied. This conclusion goes directly against the predictions of the regression argument, and specifically denies the assumption of a constant natural rate.

We may summarize the overall conclusion: *We reject the regression artifact as an explanation for H because it postulates a constant mean crime level, whereas the data indicate a rising mean. If the regression artifact argument is modified to account for this rise, the door is opened to interpretation of the maturation data as proof of a genuinely rising mean, which again leads to rejection of the regression artifact as a factor.*



The foregoing discussion has rested on the technical attributes of a regression artifact. Having followed through that analysis, it is still apparent that some artificiality in arrest rate exists in the few months immediately preceding institutionalization--an instant offense had to occur at about that time, else institutionalization would not have. But there is a simple, very powerful test for dealing with that problem: delete those observations from the sample. If the deletion of two months is thought to be unsatisfactory, delete four. If four is not enough, delete half a year. If half a year is not enough, delete a full year. Whatever, the suppression effect remains large. Taking the (absurdly) extreme cases, Table 3.4 shows the results when the half year or year prior to intervention are deleted altogether from the monthly data base.

TABLE 3.4

*Regression Analysis when the 6-month and 12-month Periods Immediately Preceding Intervention are Deleted*

| | B | SE | p< | Reduction |
|---|---|---|---|---|
| Deletion of six months prior to intervention (Sample: Months -7 to -24 with months + 1 to +24) | | | | |
| Intervention | -.147 | .011 | .001 | .468 |
| Constant | .314 | | | |
| Observations = 10,498 | | | | |
| Subjects = 317 | | | | |
| Deletion of the year prior to intervention (Sample: Months -13 to -24 with months +1 to +24) | | | | |
| Intervention | -.108 | .011 | .001 | .394 |
| Constant | .274 | | | |
| Observations = 8,596 | | | | |
| Subjects = 317 | | | | |

The reductions are extremely robust, in the face of what amounted to nonsensical truncations of the preintervention definition of "career." The charactistic pattern of the delinquent behavior was not a sudden burst of activity out of a low base, but a steadily climbing, multiyear growth in activity.

## HISTORY: IS CHICAGO A FLUKE?

The final mechanism whereby O7 could be sustained is history. Maybe Chicago from October 1974 to February 1978 was a fluke. Or, to put it in the form that we have usually heard it: if the drop in delinquent activity is as sharp and consistent as these data suggest, why has it not been common knowledge for years? More to the point, aren't these data contradicted by well-established findings that correctional interventions do not work?

No. The widespread impression that correctional intervention for juveniles has been proved ineffective is an erroneous one. Three characteristics of the existing research literature have led to the misapprehension.

First, the claims are very numerous relative to the body of data. Many of the assertions that correctional interventions fail are just that: undocumented assertions. For example, one finds this confident statement in Schur:

> Much of the disenchantment with current delinquency policy arises from the simple fact that it doesn't work....Neither the treatment reaction nor the reform response has provided any real basis for confidence that our measures are effective in preventing delinquent behavior or rehabilitating youthful offenders. (Schur, 1973: 117)



No citations accompany the text. Or sometimes one undocumented assertion references another. Thus, Clark (1970) quotes Milton Lugar, formerly the Director of the Federal Government's Office of Juvenile Justice and Delinquency Prevention and of New York State's juvenile corrections program, as saying:

> It would probably be better for all concerned if young delinquents were not detected, apprehended, or institutionalized. Too many of them get worse in our care. (Clark, 1970: 240-241)

It is assumed that the expert could have cited the evidence had he wished. It is a characteristic of the literature that the reader may easily check by glancing through influential critiques of institutionalization. The situation is seldom one of ambiguous or weak supporting data; it is almost always one of no supporting data at all. The rhetoric is voluminous; the data are sparse.

The second characteristic of the existing literature is that the results of probationary interventions are often grouped with the results of correctional interventions. For example, the landmark cohort study in Philadelphia observed that "a greater number of those who receive punitive punishment (institutionalization, fine, or probation) continue to violate the law...then those who experience a less constraining contact with the judicial and correctional systems," and concluded that "the juvenile justice system, at its best, has no effect on the subsequent behavior of adolescent boys and, at its worst, has a deleterious effect on future behavior." (Wolfgang, Figlio, and Sellin, 1972: 252) When reading negative assessments of the effects of intervention, a first question to ask is whether lesser interventions are lumped with institutionalization in the intervention. As Chapter 5 of this study indicates, the distinction is a crucial one.

The third and the decisive source of misapprehension about correctional intervention is the reliance on post-program measures alone. Evaluations do exist that compare some form of intensive treatment to ordinary probation, or forms of community-based "milieu therapy" with confinement. But (with two exceptions we shall discuss shortly) these measures of outcome are based instead on the kinds of absolute measures discussed in Chapter 1, such as cumulative percentage of subjects who commit an offense or who are returned to institutions. Using these measures, the DOC sample shows high recidivism rates, comparable to those reported elsewhere. It is these kinds of measures of absolute rates that have filled the evaluation literature and eventually tend to establish in the reader's mind the conviction that, after all, very little can be done to deal with the chronic delinquent.

The trick in interpreting these studies is to keep separate the question, "Does correctional intervention *stop* delinquency?", which the literature does address and answers with a well-documented "no," from the question. "Does correctional intervention *reduce* delinquency?"--which, lacking preintervention data, the literature does not and and cannot address. Before-after comparisons have been nearly nonexistent.

The question remains, however: Is it possible that something was happening in Chicago during 1974-77 that engendered the suppression effort, or at least contributed to it?

There is reason to believe that the suppression effect is remarkably independent of historical considerations. We have been able to locate only two comparable before-and-after studies involving institutionalization, but those two were



both major, both carefully conducted and thoroughly reported, and, best of all, took place in settings and in moments in history very different from Cook County in the mid-1970s. The two studies were *The Silverlake Experiment: Testing Delinquency Theory and Community Intervention*; and *The Provo Experiment: Evaluating Community Control of Delinquency*. The principal author in both cases was LaMar T. Empey, with coauthors Steven G. Lubeck (*Silverlake*) and Maynard L. Erickson (*Provo*).

The Silverlake Experiment was set in California in the mid-1960s. It compared an experimental institutional program with a standard one. In volume of offenses during the 12 months before and after assignment to the programs, the experimental group showed a reduction of 73.1 percent and the control group showed a reduction of 71.1 percent (259-260)--slightly greater reductions than those produced by DOC, but within a few percentage points.

The Provo Experiment was set in Utah in the late 1950s. The one year reduction for the institutionalized sample comparable to our DOC population was 61.4 percent (Table 10.3 p. 211)--slightly smaller than that of DOC, but again within a few percentage points.

The summary statement is: *A suppression effect of the order of magnitude reported for Chicago delinquents in the 1970s was found among Utah delinquents in the 1950s and California delinquents in the 1960s. No before-and-after studies of institutionalization disputes these results. Contrary to popular impressions, the suppression effect is compatible with the existing research literature.*

* * * * *

Having traced through the many potential objections and their correlaries one at a time, a summary estimate may be useful. It is judgmental; the results of the analysis do not add up to a numerical estimate of how much the suppression effect is understated or overstated. But with that caveat, we may state an appraisal.

Objections O1 to O6, discussed in Chapter 2, do not withstand much scrutiny. It was important to explore the possibility that they were valid, but in each instance they were either demonstrably inapplicable or required a set of implausible assumptions.

History also presents few problems. Apart from the fact that there is no reason to think that Chicago in 1974 to 1978 was unrepresentative, the consistent results from Empey's work in very different settings give confidence that we are observing a generalizable phenomenon.

The intriguing, problematic topics are maturation and regression. The maturation analysis persistently indicates that the arrest incidence increases with age, *independently of any bunching effects from selection.* If that is in fact the case--and we have been unable to find any evidence to the contrary--then the regression problem diminishes to one of the instant offense prior to institutionalization. And, more to the point, our procedure for computing the suppression effect has significantly understated its magnitude, because it is based on arrest rates considerably lower than the "real" rate that the delinquent took into the institution.



In view of the concern that led us into the analysis, that the suppression effect is some sort of statistical mirage, it is ironic that the outcome points to the possibility that we are underestimating it. But in the absence of any indication that the arrest rate was going to flatten without intervention, that is where the logic of the problem leads.

That logic suggests that the chronic delinquent may not naturally quit being delinquent. That the apt analogy is not with the other things that adolescents grow out of, like acne or breaking voices, but with behaviors that are reinforcing, like making money or winning status.

These speculations take us into topics that are beyond the scope of the study. For our purposes, it is enough to suggest that the data on maturation and their implications are not inexplicable. One may at least entertain the possibility that committing crimes can be rational.

## NOTES TO CHAPTER 3

[1]But: escaping through the hole in the net once does not mean that the delinquent is forever after immune. Rather, with each subsequent arrest, he must escape again, through holes that shrink. It is impossible to define a "control group" based on the general delinquent population, because the successive nets effectively winnow out offenders. Any uninstitutionalized delinquent with a large number of arrests is conceptually indistinguishable from a member of the DOC sample "the time before" he was committed.

Nonetheless, such samples are often used for comparisons--most recently, in Hamparian et al., (1978), in which the time-between arrests for institutionalized and noninstitutionalized youth were compared. It is guaranteed that such a comparison will favor the youth who slowed enough to avoid being institutionalized.



# 4. INSTITUTIONALIZATION AND LESS DRASTIC ALTERNATIVES

To this point "juvenile corrections" has been equated with institutionalization: the most drastic, most widely criticized form of intervention. Stories of the horrors of the traditional training schools are legion, and have led to a broad consensus that they must go, slowly or quickly, and be replaced with more humane alternatives.

The 1970s have seen the search for alternatives become increasingly energetic. Led by the Massachusetts experience at the beginning of the decade, a number of states have cut back or even tried to eliminate use of juvenile institutions. In Illinois itself, the commitment rate was falling sharply before and during the period covered by this study. Deinstitutionalization was a major purpose of the Juvenile Justice and Delinquency Prevention Act of 1974--the legislation that created the Office of Juvenile Justice and Delinquency Prevention (OJJDP)--and has been the focus of several special initiatives in the Office's grants and programs.

The scientific record comparing institutional and noninstitutional approaches is another question to which we shall return. The point here is not what has been proved, but what has come to be the accepted view. Within the policymaking and academic communities, the proposition that institutions are more expensive *and* less humane *and* less effective than noninstitutional approaches is hardly debated. Here, we reopen the question relative to recidivism.

The vehicle for the comparison is the Unified Delinquency Intervention Services (UDIS) as it functioned in Cook County, Illinois, during the period 1 October 1974 (its inception) through 30 June 1976--the same intake period encompassed in the DOC sample. The mechanics of UDIS and the types of services it offered are described in Appendix B. Briefly, the program took as its client the juvenile who otherwise would have been sent to DOC. It was intended to be, and in large measure actually was, an alternative to incarceration rather than a supplement to probation.

UDIS placed its youth in a variety of service programs, shifting a client from one to another as necessary. Placement decisions were to be guided by "the least drastic alternative principle." Given a choice, UDIS would leave the youth in his own home. Failing that, he would be put in a residential service in his own community. Only when the judge insisted, or because of other exceptional circumstances, would an out-of-town residential program be tried first. Whatever the placement might be, UDIS was supposed to get the youth out of the juvenile justice system fast--within six months if possible.

UDIS was an ambitious, innovative experiment. Its scope, combined with its location in one of the Nation's largest cities, provides a nearly unique opportunity to compare institutionalization with a full range of alternative correctional interventions.



## THE SERVICES TO BE COMPARED

To facilitate the analysis, we divide the placements into levels, based on the degree to which they represent a physical disruption of the youth's normal life (in effect, applying UDIS's own definition of "drastic"). Details on each of the placement categories may be found in Appendix B.

The least drastic group of services (Level 1) embraces services provided while the youth *stayed in the community and continued to live in his own home.* The services included in Level 1 are advocacy, counseling, and educational/vocational. Typically, these services would be provided in combination--a boy would be assigned an advocate and enrolled in a tutoring program; or sent to a family counseling service and to an alternative school. Because they are provided in combination, and the combinations might shift several times, in staggered fashion, it will not be feasible to disentangle (at least quantitatively) the relative effects of the services within Level 1 until UDIS has processed a thousand cases. The cell sizes in the existing sample quickly go to a handful, even when only a few permutations are considered.

The next step up the scale, Level 2, denotes services which were *community-based but residential*, taking the youth away from his home. The two service types included in Level 2 are group homes and foster care. In practice, only six youths in the UDIS sample went to a foster home placement. In the analyses, "Level 2" can be interpreted as synonymous with "group home."

Level 3 refers to *wilderness programs* modeled on the well-known Outward Bound approach. Services in this level act as a bridge between community-based and out-of-town-residential. They were time-lined and usually short; in addition, they could be a reward for progress, not a reaction to problems in the community-based service. It is not at all clear that they were in fact "more drastic" than the Level 2 placements--one of the reasons that we do not treat the levels as an ordered scale in the analyses.

Level 4 is the first of what we term *out-of-town residential* programs. It includes two "camp-like" programs (The Work Camp and Crossroads) that had fixed facilities in a rural setting, and provided their clients with a combination of work, education, and recreational programs.

Level 5 denotes the most drastic of the UDIS placements. It is labeled "*intensive care*," and includes hospitals providing psychiatric services and a residential program using an intensive form of behavior-modification through a positive peer culture approach.

The final level, Level 6, denotes the out-of-town *DOC institution* run by the State of Illinois.

## THE DELINQUENTS BEING COMPARED

The question must always be raised whenever different correctional programs are being compared: to what extent did the different programs get the same delinquent? The understandable suspicion is that the tougher programs got the tougher cases.

The quantitative measures reveal very little difference among the subsamples. The background characteristics showed no major differences (Murray et al.: 58-64). And the prior offense records were remarkably similar. Table 4.1 shows

TABLE 4.1
*Prior Arrest Records, by Level of First Placement*

| Level of First Placement | N | Age at Onset | Mean Number of Arrests | | | TOTAL |
|---|---|---|---|---|---|---|
| | | | "Index" Offenses | Damage, Trespass, & Possession Offenses | Minor & Status Offenses | |
| I Nonresidential services | 157 | 12.1 | 7.9 | 3.9 | .9 | 12.7 |
| II Group homes | 40 | 11.7 | 8.3 | 3.5 | .7 | 12.5 |
| III Wilderness programs | 14 | 12.6 | 6.6 | 4.9 | 1.1 | 12.6 |
| IV Out-of-town residential | 45 | 12.1 | 7.7 | 5.4 | 1.9 | 15.0 |
| V Intensive care | 11 | 11.3 | 8.5 | 4.5 | 3.1 | 16.1 |
| VI DOC institutions | 317 | 12.2 | 8.2 | 4.3 | 1.1 | 13.6 |
| OVERALL | 584 | 12.1 | 8.1 | 4.3 | 1.1 | 13.5 |

Note: The above and subsequent analyses using first-placement as a classifying variable omit 1 UDIS referral who was egressed before being placed with a service and the 2 UDIS referrals who were sent to foster care as a first placement.

the comparative figures by level of the first placement. The column of greatest interest is the one listing mean number of the major "Index" offenses. The differences are small. Only the Level 3 youth showed a noticeably different mean, and that is based on a sample of only 14. Aside from that, the means are bunched between 7.7 and 8.5, showing trivial differences.

The total number of offenses has a wider spread, from a low of 12.5 for Level 2 (group homes) to a high of 16.1 for Level 5 (intensive care). The statistical significance of the spread will be considered shortly.

The next question is more pointed: when the really serious offenses are isolated--the ones that might plausibly indicate the hardcore difficult-to-work-with delinquent--how do the alternative placements compare? Table 4.2 shows the results when four of the most serious violent offenses--homicide, rape, battery, and armed robbery and two of the most serious property offenses (burglary and auto theft) are considered.

The table must be read with the sample sizes in mind--for example, the 18.2 homicides-per-hundred for the Level 5 subsample means only that two of the 11 delinquents placed first in Level 5 had committed a homicide. But a few generalizations may be drawn. Boys who had committed homicides were much more likely to be sent to a residential placement than to a nonresidential placement. Delinquents charged with rape were either sent to a nonresidential placement or to a DOC institution--perhaps reflecting the very different content that might be represented under the label "rape." Armed robbery was more likely to lead to DOC than to UDIS in general.

TABLE 4.2

*Prior Arrests for Selected Offenses, by Level of First Placement*

| Level of First Placement | N | No. of Arrests per 100 Subjects: Homicide | Rape | Battery | Armed Robbery | Auto Theft or CTTV | Burglary |
|---|---|---|---|---|---|---|---|
| I Nonresidential services | 157 | 2.5 | 5.1 | 98.1 | 47.1 | 87.9 | 261.1 |
| II Group homes | 40 | 5.0 | -0- | 115.0 | 37.5 | 72.5 | 330.0 |
| III Wilderness programs | 14 | 7.1 | -0- | 107.1 | 35.7 | 92.9 | 242.9 |
| IV Out-of-town residential | 45 | 4.4 | -0- | 80.0 | 26.7 | 124.4 | 246.7 |
| V Intensive care | 11 | 18.2 | -0- | 100.0 | -0- | 127.3 | 209.1 |
| VI DOC institutions | 317 | 4.4 | 7.3 | 100.0 | 57.7 | 96.2 | 226.5 |
| OVERALL | 584 | 4.3 | 5.3 | 99.1 | 49.5 | 95.0 | 244.5 |

To compare these and other differences among the youth consigned to the various levels of first placement, the five UDIS levels were coded as dummy variables (0/1) and entered as the independent variables in a set of regression equations. The DOC sample was left as the reference group. The dependent variables were: number of arrests in the year prior to intervention, number of arrests in the second year prior to intervention, age at onset (that is, age at first arrest), total number of "index" arrests prior to intervention, and age at intervention. The results are shown in Table 4.3.

The signs and sizes of the regression weights are interpretable, and generally consistent with expectations. But the following comments must be read with the very large standard errors ("SE" in the table) in mind. Only one of the dimensions--number of arrests in the first year prior to intervention--was significantly related to level of first placement. Note in this context that the youth sent first to out-of-town placements showed positive regression coefficients (i.e., tended to have more arrests in the first-year-prior than even the DOC youth).

The other noteworthy feature of the exercise is the *lack* of significance in the equation relating age-at-entry to first placement. Except for delinquents sent to Level 5--the most drastic of the UDIS placements--the largest coefficients was -.242 indicating a mean of roughly three months younger than youth sent to DOC. The larger coefficient associated with Level 5 (-.814, or almost ten months), combined with the large positive weight for first-year-prior arrests, suggests an interpretation: judges confronted with a younger but very active delinquent wanted to avoid sending him to DOC, but also wanted to make sure he was kept off the streets; hence the use of the most secure of the UDIS placement levels.

TABLE 4.3

*Regression Analysis of Preintervention Differences, by Level of First Placement*

| INDEPENDENT VARIABLES | DEPENDENT VARIABLES | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | No. of Preintervention Arrests | | | | | | Age in Years at Program Entry | |
| Level of First Placement | 1st Yr. Prior B | 1st Yr. Prior SE | 2nd Yr. Prior B | 2nd Yr. Prior SE | Total "Index" B | Total "Index" SE | B | SE |
| 1. Nonresidential services | -.547 | .324 | +.037 | .275 | -.229 | .229 | -.134 | .095 |
| 2. Group homes | -.778 | .556 | +.360 | .472 | +.091 | .392 | -.079 | .163 |
| 3. Wilderness programs | +1.294 | .905 | -1.005 | .769 | -.005 | .638 | -.242 | .266 |
| 4. Out-of-town residential | +.789 | .528 | +.665 | .448 | +.266 | .372 | -.169 | .155 |
| 5. Intensive care | +1.450 | 1.016 | -.472 | .863 | +1.339 | .717 | -.814 | .298 |
| Constant | 6.278 | | 3.290 | | 3.934 | | 15.820 | |
| $R^2$ | .022 | | .009 | | .010 | | .016 | |
| F ratio (df: 5,577) | 2.58 (.025) | | 1.02 (ns) | | 1.15 (ns) | | 1.93 (ns) | |

Note: Dummy variable coding. Reference group is DOC.

Overall, the distinctions in offense patterns were less clearcut than might have been predicted. To the extent that the quantitative aspects of the subjects' histories convey the sense of what happened, the youth in the different subsets appear to have been reasonably comparable.

We cannot jump from this to the conclusion that they were truly comparable. As we were repeatedly reminded by probation officers, judges, and UDIS staff, the variables we have examined do not capture important dimensions used in deciding where to send a youngster. Lack of remorse, a probation officer's judgment that a violent offense was likely to recur, or a particularly cruel aspect of an offense could lead to commitment to DOC; factors such as supportive parents, mitigating circumstances surrounding the offense, or a good lawyer could tip the balance toward UDIS.

The selection factors at work undoubtedly included an admixture of irrelevant ones, in a strictly rational sense. But granted that, there is also no doubt that judges and probation officers tried to send the hardest of the hardcore to DOC instead of UDIS. Among the delinquents they did consign to UDIS, they openly insisted on residential placements for the boys they considered to be high risks. In no sense did the placements receive randomly selected subsamples from among a single parent population of boys eligible for commitment to DOC.

We asked 22 probation officers in the Cook County Juvenile Court to respond to this issue. If UDIS had not existed, what proportion of its referrals would the probation officers have tried to send to DOC? The 16 probation officers who felt able to respond had recommended 145 youth to UDIS. They estimated that, of the 145, they would have recommended 88 for commitment to DOC if UDIS had not existed. To



put it another way, the probation officers were suggesting that about 40 percent of UDIS's cases were not candidates for DOC. To the extent that their retrospective was accurate, the selection bias was large.

The importance of the selection biases depends on the results of the comparisons.

*If we find that UDIS does better than DOC*, or that the nonresidential services do better than the residential ones, then the selection biases are crippling. It becomes inadmissible to conclude that the intervention itself accounted for the differences in suppression effect--the selection biases are too plausible as an alternative explanation.

*If we find that DOC does better than UDIS*, or residential services do better than nonresidential ones, the selection bias adds to the confidence that a true difference in effectiveness exists. The selection bias in this case is troublesome mainly because it interferes with arriving at an estimate of the *magnitude* of the difference. The assumption must be that the observed difference underestimates the true difference.

## THE COMPARISON

The "intervention strategy" used by DOC was relatively simple. A boy was placed in one of the seven institutions operating at the time of the study. Usually, commitments from Cook County went first to either Valley View or St. Charles, the two general-purpose institutions closest to the Chicago area. If a youth had problems adjusting to that institution, he might be returned to the Reception Center at St. Charles and then reassigned to another institution. But while the specific site might change, and with it some aspects of the program, the differences among the institutions do not provide clear-cut dimensions on which to discriminate among placement histories. "DOC" is treated as a unitary treatment strategy.

UDIS must be approached otherwise. Not only did UDIS use a variety of placements for the program as a whole; any one referral might also be exposed to a variety of placement types. The permutations were numerous--in all, more than 60 distinct sequences existed within the UDIS sample. But it is questionable how many of those sequences were importantly distinct. Take as an example the youth who started with an advocate (Level 1), then was sent on a short-term, time-lined wilderness program (Level 3) returned to his advocate (Level 1) on schedule, then got in trouble and was put in a group home (Level 2). How different, really, is his experience from that of the youngster who started with an advocate (Level 1), then after a few months got in trouble and was assigned to a group home (Level 2)? The fine-grained distinctions might be of interest if the cell sizes permitted. But, as in the case of the distinctions among Level 1 services, the analyses must wait until UDIS has processed several thousand cases. Some patterning must be imposed on the intervention strategies. We have adopted three characterizations (not mutually exclusive) of the histories of the subjects.

The first analysis employs *first placement* as the discriminating factor. In this comparison, we ignore all that might have come after.

The second analysis characterizes the entire intervention history. The dimension of interest is the degree to which the youth received a *community-based* experience.



Subjects are classified as falling in one of four groups: those who received the "pure form" of entirely at-home services; those who stayed in the community throughout but spent part of their time in a group home; those who received a mix of community-based and out-of-town services; and those who received only out-of-town services.

The third comparison focuses on the first pair of placements, to inform an issue that is much argued within the community of corrections officials: *escalation vs. deescalation*. Is it better to start a youth in a community placement, then shift him to an out-of-town one if necessary? Or is it wiser to "get his attention" with an initial out-of-town placement, then reintegrate him into the community with a community-based one?

## The Analytic Approach

A mixed-model regression design is employed in each case: subjects by groups within conditions. "Subjects" are, of course, the individuals in the samples. "Groups" are defined separately for each analysis. "Condition" refers to the state of being in a preintervention or post-intervention state.

The regressions that subsequently are reported were conducted hierarchically in the order of time (T), groups (G), and the TxG interaction. In companion analyses, G was entered prior to T, to obtain estimates of the sensitivity of the time main effect to order of entry into the equation.

The results as presented include information about control for between-subjects variance. The partitioning of the variance takes $R^2_{Y.S}$ into account, and the regression statistics for estimating statistical significance utilize information on the between-subjects variance. Specifically, the F test for the TxG interaction is computed as

$$F = \frac{R^2_{Y \cdot T}}{1-R^2_{Y \cdot S}-R^2_{Y \cdot T}-R^2_{Y \cdot TxG}} \times (n-g),$$

where

Y is the dependent variable (arrests),
S is subjects,
T is time,
g is groups (e.g., first placement categories), and
n is the number of subjects.

The standard error (SE) for the *i*th independent variable is obtained via

$$SE_{Bi} = \frac{sd_Y}{sd_i} \sqrt{\frac{1-R^2_{Y \cdot S}-R^2_{Y \cdot T}-R^2_{Y \cdot TxG}}{(n-g)(t-1)}} \sqrt{\frac{1}{1-R^2_i}}$$

The notation and procedures used may be found in Cohen and Cohen (1975: 404-406, 412-426). For a discussion of the use of collapsed time periods, see Chapter one and Appendix A of this report.

Each of the analyses was repeated, once with arrest incidence as the dependent variable and once with the natural log of arrest incidence as the dependent variable. The latter analysis provides a perspective that compensates for the problem of the (probably) inflated estimates produced by the algorithm for computing the postrelease arrest incidence of subjects who were released for only a short time, then reincarcerated (see Chapter 1 and Appendix A).



Comparison A: First Placement

This comparison is based on where the youth went first. It is perhaps the most useful single comparison, on three counts. Technically, it is the cleanest of the comparisons--unlike the others, it is not confounded by the in-program experience. It imparts useful information about the Court's outlook on the youth at the outset of the intervention. And, on a practical level, the first placement was typically a dominant placement as well. For DOC youth, the "first placement" level was the *only* placement level. And in UDIS too, the first placement level was the only one for a major proportion of its referrals--105 of the 267 in the sample, or 39.3 percent. Another 72 of the UDIS youth (27.0 percent) went from the first placement level to just one other before leaving the program.

Time is effects-coded (-1 for the preintervention period, +1 for th postintervention period). The levels are dummy-coded (0/1). The omitted level serving as the reference group is Level 6, the DOC institutional experience. The interaction terms are of course the multiple of time and level (TxL).

The results are shown in Table 4.4 on the following page. The focus of interest is the set of interaction terms--the coefficients that point to the effectiveness of the UDIS interventions relative to DOC.

As an inspection of Table 4.4 reveals, the coefficients decrease regularly with increases in the Level number. Thus Level 1 first-placements had the highest coefficient (+.624); Level 5 had by far the lowest (-1.330). To put it plainly, the UDIS out-of-town first placements did better than DOC; the UDIS community-based first placements did worse.

TABLE 4.4
*Regression Analysis of Arrests by Level of First Placement*

Regression Coefficients

| Independent Variable | B | SE | p< |
|---|---|---|---|
| TIME (T). Effects coded (-1/+1) | | | |
| Time: before vs. after | -1.731 | .087 | .001 |
| LEVEL OF FIRST PLACEMENT (L). Dummy coded (0/1) | | | |
| Level 1. Nonresidential services | +.077 | .152 | ns |
| Level 2. Group homes | -.399 | .261 | ns |
| Level 3. Wilderness programs | +1.078 | .424 | .02 |
| Level 4. Out-of-town residential | +.547 | .248 | .05 |
| Level 5. Intensive care residential | +.120 | .476 | ns |
| INTERACTION EFFECTS (TxL) | | | |
| Time x Level 1 | +.624 | .152 | .001 |
| Time x Level 2 | +.378 | .261 | ns |
| Time x Level 3 | -.216 | .424 | ns |
| Time x Level 4 | -.242 | .248 | ns |
| Time x Level 5 | -1.330 | .477 | .01 |
| CONSTANT | 4.546 | | |

Partitioning of the Variance

(Y: arrests. S: subjects, T: time, L: level of first placement)

Between Subjects

$R^2_{Y \cdot S} = .515$ $\qquad$ $R^2_{Y_p \cdot L} = .008$

Within Subjects

$R^2_{Y \cdot T} = .164$ $\qquad$ $R^2_{(Y \cdot S) \cdot T} = .339$

$R^2_{Y \cdot T,L} = .168$

$R^2_{Y \cdot TxL} = .009$ $\qquad$ $R^2_{(Y \cdot S) \cdot TxL} = .018$

F-ratio for TxL as a set = 6.42. df = 5,1160. $p < .001$

Mean Arrests and Suppression Effects by Level of First Placement

| | Before | After | Suppression |
|---|---|---|---|
| 1. Nonresidential services | 5.73 | 3.52 | -.386 |
| 2. Group homes | 5.50 | 2.79 | -.493 |
| 3. Wilderness programs | 7.57 | 3.68 | -.514 |
| 4. Out-of-town residential | 7.07 | 3.12 | -.559 |
| 5. Intensive care residential | 7.73 | 1.61 | -.792 |
| 6. DOC institution | 6.28 | 2.82 | -.551 |



CONTINUED

1 OF 2

TABLE 4.5
*Logged Regression Analysis of Arrests by Level of First Placement*

REGRESSION COEFFICIENTS

| Independent Variable | B | SE | p< |
|---|---|---|---|
| TIME (T). Effects coded (-1/+1) | | | |
| Time: before vs. after | -.681 | .029 | .001 |
| LEVEL OF FIRST PLACEMENT (L). Dummy coded (0/1) | | | |
| Level 1. Nonresidential services | +.079 | .050 | ns |
| Level 2. Group homes | +.008 | .092 | ns |
| Level 3. Wilderness programs | +.390 | .140 | .05 |
| Level 4. Out-of-town residential | -.072 | .082 | ns |
| Level 5. Intensive care residential | -.181 | .157 | ns |
| INTERACTION EFFECTS (TxL) | | | |
| Time x Level 1 | +.180 | .050 | .01 |
| Time x Level 2 | +.076 | .086 | ns |
| Time x Level 3 | +.207 | .140 | ns |
| Time x Level 4 | -.219 | .081 | .05 |
| Time x Level 5 | -.262 | .157 | ns |
| CONSTANT | 4.546 | | |

PARTITIONING OF THE VARIANCE

(Y: arrests. S: subjects, T: time, L: level of first placement)

Between Subjects

$R^2_{Y \cdot S} = .431$ $\quad$ $R^2_{Y_p \cdot L} = .008$

Within Subjects

$R^2_{Y \cdot T} = .250$ $\quad$ $R^2_{(Y \cdot S) \cdot T} = .439$

$R^2_{Y \cdot T,L} = .253$

$R^2_{Y \cdot TxL} = .008$ $\quad$ $R^2_{(Y \cdot S) \cdot TxL} = .015$

F-ratio for TxL as a set = 6.17. df = 5,1160. p < .001



Two of the interaction terms were statistically significant: those for at-home first placements and intensive-care residential first placements. We should also note that the order in which variables were entered made no difference. When the Level variables were entered first, the $R^2$ was .004.

Table 4.5 shows the results when the logged (ln) value of arrests is used as the dependent variable. The explained variance (controlling for the between-subjects variance) rises appreciably from that of the unlogged version, from .339 to .431. Among the interaction terms, the at-home placements continue to produce significantly smaller reductions in arrests than DOC. The coefficient for the wilderness programs (which had a first-placement sample of only 14) shifts signs. The most conspicuous change, however, concerns Level 4, the out-of-town residential "camps." As before, their coefficient is negative. But in the logged version, the standard error for Level 4 is much smaller and the results are statistically significant. It turns out that the Level 4 first-placements had a disproportionate number of the outliers in postintervention offenses. That is, they had a disproportionate number of subjects who were arrested within a few weeks of release then reincarcerated, producing a very large annualized rate in the collapsed data base. The influence of these subjects was substantially diminished by the log transformation.

## Comparison B: Degree of Community Based-ness

We now deal with the entire intervention history, not just the first placement. The set of variables used to characterize it are intended to operationalize as directly as possible the rhetoric of minimal intervention. According to that rhetoric, the best intervention is the least intervention, staying at-home throughout (CB-1 is the label we use for this alternative). The next best is staying in the community throughout (the Level 2 group homes, or a combination of Levels 1 and 2. The label is CB-2). The next best (CB-3) is at least *some* community-based programs, with an admixture of out-of-town placements. And the least desirable strategy is the intervention which takes place entirely out of the community (CB-4).

By using the entire intervention history, we introduce yet another, new bias. This time, the problem is the ability of the youth to demonstrate during the course of the program whether he is a "good risk"--which affects the course of his placement history and serves as a plausible predictor of postrelease performance. In particular, a potential selection bias affects interpretation of the CB-1 group. They were first put in at-home services (itself a positive indicator) and managed to stay there until leaving the program (meaning that they did not get into a lot of trouble during the program).

Given this, the results shown in Table 4.6 are that much more intriguing. For despite the edge which CB-1 should have had, the interaction coefficient for the delinquents who stayed at home throughout their intervention histories is +.640. The standard error is large, and the result does not reach statistical significance ($t$=1.75). The interaction term for the other community-based history (CB-2) was also positive and large. It was significant at the .05 level.

None of the three interaction terms was negative; to put it another way, none of the three alternatives with a community-based component compared favorably with the all-out-of-town histories. The logged version of the analysis showed equivalent results, and added no new information.



TABLE 4.6
*Regression Analysis of Arrests by Community-Based Focus of the Entire Intervention History*

REGRESSION COEFFICIENTS

| Independent Variable | B | SE | p< |
|---|---|---|---|
| Time (T). Effects-coded -1/1 | | | |
| Time: before vs. after | -1.740 | .086 | .001 |
| Community-Based Focus (C). Dummy-coded 0/1 | | | |
| CB-1 At-home services only | -.928 | .259 | .001 |
| CB-2 Community-based only (own & group home) | +.570 | .324 | ns |
| CB-3 Some community-based, some out-of-town | +.972 | .164 | .001 |
| Interaction Effects (TxC) | | | |
| Time x CB-1 | +.640 | .366 | ns |
| Time x CB-2 | +.648 | .300 | .05 |
| Time x CB-3 | +.182 | .164 | ns |
| Constant | 4.478 | | |

PARTITIONING OF THE VARIANCE

(Y: arrests, S: subjects, T: time, C: community-based history)

Between Subjects

$R^2_{Y.S} = .515$ $R^2_{Y_p.C} = .032$

Within Subjects

$R^2_{Y.T} = .165$ $R^2_{(Y.S).T} = .340$

$R^2_{Y.T,C} = .181$

$R^2_{Y.TxC} = .004$ $R^2_{(Y.S).TxC} = .008$

F-ratio for TxC as a set = 4.90. df = 3,1168. $p < .01$

MEAN ARRESTS AND SUPPRESSION EFFECTS BY COMMUNITY-BASED FOCUS

| | Before | After | Suppression |
|---|---|---|---|
| CB-1. At-home services only | 4.65 | 2.45 | -.473 |
| CB-2. Community-based only (own & group home) | 6.14 | 3.96 | -.355 |
| CB-3. Some community-based, some out-of-town | 7.01 | 3.89 | -.445 |
| CB-4. Out-of-town only | 6.22 | 2.74 | -.559 |

Again, the order in which the group and time variables are entered made no difference in the results, in either the logged or unlogged versions. Arrests regressed on the CB variables without the time variable produced an $R^2$ of .016; .008 in the logged version.

Comparison C: Escalation Versus Deescalation

Comparison A looked only at the first placement. Comparison B looked at the entire intervention history. Comparison C examines the first *pair* of placements, with a specific hypothesis in mind, suggested by a qualitative analysis of 102 case histories collected during the original evaluation of UDIS. The out-of-town placements did well--not just in the numbers, but in the opinion of Case Managers who had not yet seen the recidivism results. (Murray et al., 1978: 157-159) But the narratives also suggested that the youth who had been in out-of-town placements did well when they returned to follow-up placements at home. Perhaps, it was thought, the best of all worlds is the combination. The notion was that reintegration into the community following the out-of-town placement might add an important element to the youth's reaction to the correctional experience.

The counter argument is that the proper strategy is escalation. Start with an at-home service, then use the out-of-town placement only when the youth proves himself unresponsive to the less drastic intervention. Whatever virtues (if any) that attach to out-of-town placements will be as effiacious in the second placement as they would have been in the first.

To examine the contrast between these strategies, two subsamples were identified: those who started at-home



(Level 1), then were shifted to a residential out-of-town placement (Level 4 or higher); and the reverse, referrals who started in Level 4 or higher and then went directly to an at-home first placement.

We again face a problem of selection bias. The escalated youth presumably did something negative that got himself transferred to a residential program. Fortunately, the converse is not true--transfer from a residential to nonresidential program does not imply good behavior (on the contrary--the very best behavior ought logically to lead to transfer out of UDIS directly from the residential placement). And we have a counterbalance to the selection bias. The delinquent who started in an out-of-town placement was usually sent there because someone thought he was a high-risk referral. The natural tilt of the analysis, or even whether it has one, cannot be determined. We note the situation as one that the reader should keep in mind when interpreting the results. To facilitate this perspective, the escalation/deescalation issue is put in the context of two other variables. One is the familiar contrast between all-at-home and all-out-of-town. The other is a contrast between variation and no-variation during the placement history.

A contrast coding scheme is employed. The codes are:

| Set | PS-1 | PS-2 | PS-3 |
|---|---|---|---|
| All Level 1 | 1 | 0 | -.5 |
| From Level 1 to Level 4+ | 0 | 1 | +.5 |
| From Level 4 to Level 7 | 0 | -1 | +.5 |
| All Level 4+ | -1 | 0 | -.5 |

TABLE 4.7
*Regression Analysis of Arrests by Escalation Strategy*

REGRESSION COEFFICIENTS

| Independent Variable | B | SE | p< |
|---|---|---|---|
| Time (T). Effects-coded -1/1 | | | |
| Time: before vs. after | -1.608 | .087 | .001 |
| Placement Strategy (PS). Contrast-coded | | | |
| PS-1 All at-home vs. all out-of-town residential | -.300 | .079 | .001 |
| PS-2 Escalation vs. deescalation in 1st placement pair | -.247 | .154 | ns |
| PS-3 Uniformity vs. variety in placement levels | +1.043 | .173 | .001 |
| Interaction Effects (TxP) | | | |
| Time x PS-1 | +.160 | .079 | .05 |
| Time x PS-2 | +.199 | .154 | ns |
| Time x PS-3 | -.057 | .174 | ns |
| Constant | 4.698 | | |

PARTITIONING OF THE VARIANCE

(Y: arrests, S: subjects, T: time, P: placement strategy)

Between Subjects

$R^2_{Y.S} = .505$ $R^2_{Y_p.P} = .032$

Within Subjects

$R^2_{Y.T} = .214$ $R^2_{(Y.S).T} = .433$

$R^2_{Y.T,P} = .227$

$R^2_{Y.TxP} = .002$ $R^2_{(Y.S).TxP} = .003$

F-ratio for placement strategies as a set = 2.89. df = 3,994 $p < .05$

MEAN ARRESTS AND SUPPRESSION EFFECTS BY PLACEMENT STRATEGY

| | Before | After | Suppression |
|---|---|---|---|
| All placements at-home | 5.30 | 2.46 | -.536 |
| Escalation from at-home to out-of-town | 6.41 | 3.54 | -.448 |
| Deescalation from out-of-town to at-home | 7.30 | 3.63 | -.503 |
| All placements out-of-town residential | 6.22 | 2.74 | -.559 |



In examining the results in Table 4.7, our primary point of interest is the interaction term for the escalation variable. The regression coefficient in the unlogged version is +.199. In the contrast coding scheme being employed, this means a net "before versus after" difference of .796 arrests. That is:

| | Before | After | After-Before |
|---|---|---|---|
| Escalation | (-1,+1) .199 | (+1,+1) +.199 | .398 |
| Deescalation | (-1,-1) .199 | (+1,-1) .199 | |
| Net difference | | | .398 |
| | | | .796 |

The contrast is not statistically significant in the unlogged version, and is significant in the logged version ($t$=2.57)--the same Level 4 outliers in Comparison A are involved in this comparison.

The other contrasts which we have examined in related form earlier in this chapter tell the same story as before. They are of interest insofar as they assist the interpretation of the escalation/deescalation interaction. The contrast between a treatment history entirely within the community versus one entirely out-of-town favors the out-of-town alternative (net difference is 4 x .160 = .640). The contrast between heterogeneous and uniform treatment levels during the intervention shows very little difference (net difference is 4 x .057 = .114).

If one assumes that the selection biases going both ways cancel out, these results support the conclusion that escalation is the worst of the alternatives. Interpretively, the logic would be consistent with the arguments against escalation of negative reinforcement in a variety of fields (e.g., in warfare). Slowly turning up the pressure only facilitates increases in resentment and resistance. The very slowness of the escalation obscures thresholds that the subject otherwise would have recognized.

If one does not wish to assume that the selection biases canceled out, the poor showing of the escalation strategy may be seen as one more manifestation of the generally poor showing made by the at-home services. The all-at-home group unquestionably had a *positive* selection factor working in its favor, yet failed to out-perform (or even match) the all-out-of-town group.

The analysis further fails to confirm our hypothesis that the best of the alternatives is the out-of-town placement followed by a reintegrative community placement. The results are clear that it is better from a recidivism standpoint to start a delinquent in an out-of-town placement than to leave him at home. But no advantage accrued to prolonging the intervention by tacking at-home services onto the end of a stay in an out-of-town residential program.

## SUMMARY

A set of alternative correctional approaches has been compared, using institutionalization as the point of reference. The alternatives ranged from services provided while the youth lived at home to residential "intensive-care" facilities. The elements of comparison were first placement, the overall intervention history, and combinations of special interest.



Selection biases were considered. Quantitatively, they were not apparent. Qualitatively, the people who dealt with the youngsters, especially the judges and probation officers, claimed that the DOC youth were thought to be generally more incorrigible than the UDIS youth. Within UDIS, the putatively most difficult or dangerous cases were sent first to residential placements. The selection bias favored community-based programs and specifically the at-home services. But the comparisons showed differences favoring the residential out-of-town interventions over either the at-home or group-home community-based services. The differences were statistically significant and large enough to be of substantive interest.

The best of all results were not shown by DOC, however, but by some of the UDIS residential services. The consensus of observers was that the UDIS residential services were more humane and less punitive than the traditional institutions; in this sense, the analyses do not support simple punitiveness as a basis for the results. They do contradict the hypothesis that "less drastic" approaches are superior. Residential programs produced larger reductions in offenses even though the odds (in the form of selection bias) were loaded against them.

## 5. THE EFFECTS OF COURT INTERVENTIONS

The Cook County Juvenile Court routinely uses two lesser interventions--supervision and probation--in the earlier phases of a youth's delinquent career. This chapter examines their effectiveness when measured on the same recidivism criterion we have used to assess DOC and UDIS.

The DOC and UDIS samples are not a good test of whether they work. Before running any analyses, it can be predicted that data drawn from those samples will show negative results. This is guaranteed. If supervision or probation *had* worked, our DOC and UDIS subjects would not have gotten to DOC or UDIS. It is a classic circularity.

But what about the much larger number of youth who never reach the more drastic interventions? This was among the most tantalizing questions raised by the original analyses of the suppression effect, and the work for this study set out to address it. The hypothesis that seemed most plausible was that we would find a suppression effect at each level of intervention; that different youngsters have different thresholds of response, and that some would find supervision or probation to be enough to trigger observable reductions in subsequent delinquent activity.



To investigate the issue, we collected arrest data for a birth cohort of all Chicago youth born in 1960 who had been arrested as juveniles at least once by the Chicago police. A random sample of 1482 members of this cohort was obtained, as described in Appendix A. Note that adult police records were not examined for this sample. Our data for the 1960 cohort sample capture only arrests that occurred prior to the seventeenth birthday and that were processed through the juvenile justice system. Subsequent plots should be interpreted with that limitation in mind.

### TECHNICAL NOTES

The comparisons employ several plots that were prepared through a common procedure. These technical considerations should be noted.

To facilitate the presentation, the body of the text contains plots of the arrest rate smoothed for seasonal trends, where the smoothed rate for time *t* is

$$Sm(x_t) = \frac{.5x_{t-5}+x_{t-4}+ \ldots +x_{t+4}+.5x_{t+5}}{10}$$

when working in tenths of a year. See, for example, Chatfield (1975) for a discussion of this and other smoothing algorithms for removing seasonal trends.

All incidence data displayed in the figures take subsequent interventions into account. If the topic is the effects of supervision, subjects drop out of the sample at the point when they are put on probation (or sent directly from supervision to UDIS or DOC, as sometimes happened). The reason for deleting them is obvious: after entering a new intervention, subsequent behavior must be interpreted as being in part a function of the effects of the new intervention. Also, of course, the inhibiting effect of incarceration on arrest rates must be considered. Imagine a plot of pre- and post-probation arrests for a program in which all probationers failed very quickly and were institutionalized. If the subsequent intervention were ignored, the program would look spectacularly successful.

The procedures relating to sample size are analogous to those used in the analysis of maturation. When incidence of arrests preceding and following an intervention are being compared, sample sizes change steadily as subjects move from the "before" state to the "after" one. Note, as before, that the two samples have no overlap at any one point. Also as before, the plot begins when sample size is at least 25, and stops when it falls below that number.

In none of the plots is the trendline during the seventeenth year an important factor in reaching an interpretation. We should note, however, that during 1977, all of the members of the sample "disappear" from consideration as they reach 17. By the end of the year, the sample size is zero. But birthdates, which were obtained in full for the DOC and UDIS samples, are incomplete in the cohort sample. This is not a crippling problem insofar as we are interested in aggregate estimates for the population, not individual histories. Births are uniformly distributed throughout the year, and we employed that assumption in estimating sample sizes.[1] During the first half of the year, the arrest rates estimated via this procedure are insensitive (plus or minus a few percentage points) even to gross deviations from a uniform birthdate distribution in the sample population. Sensitivity increases toward the end of the seventeenth year, however--small deviations can produce large swings in estimates. As a precaution, we stopped the calculations of



arrest incidence after the first half of the year, regardless of the estimated residual sample size at that point.

Finally, it should be noted that the rates for the fifteenth and sixteenth years (1976-77) are increasingly understated by the available data. As an adolescent becomes older, an arrest is increasingly likely to be processed by the adult system, even before the seventeenth birthday occurs. Among the 150 members of the DOC and UDIS samples born in 1960 (and for whom adult data were obtained), a total of 143 arrests in 1977 had occurred before these subjects had reached their seventeenth birthday. Of these, the juvenile division recorded 113 (79.0 percent) and the adult division recorded 30 (21.0 percent). In addition, 46 arrests that occurred in 1975 and 1976 were recorded in the adult records, not in the juvenile ones. And the counter-balancing type of error--arrests after 17 recorded in the juvenile records--was weaker. Of 147 arrests that occurred after the seventeenth birthday, only 10 (6.8 percent) had mistakenly found their way into the juvenile records. To the extent that crossover of this sort occurs, some under-estimation of the real arrest rate must be assumed.

## DELINQUENTS IN GENERAL: THE OVERALL PATTERN

*Sample: All members of the 1960 cohort.*

We begin by examining the sample as a whole. Figure 5.1 depicts the arrest incidence for all 1,482 subjects from 1971 (when the boys were in their eleventh year) through the end of 1976. Along with the smoothed trendline, the actual arrest incidence per period is plotted through the first half of 1977. It may be added that the plot takes incarceration into account, but it makes no visible difference. Only 33 subjects in the sample went to DOC, and 11 to UDIS. With 1,482 in the sample, the uncorrected trendline is nearly indistinguishable from the one presented in Figure 5.1.

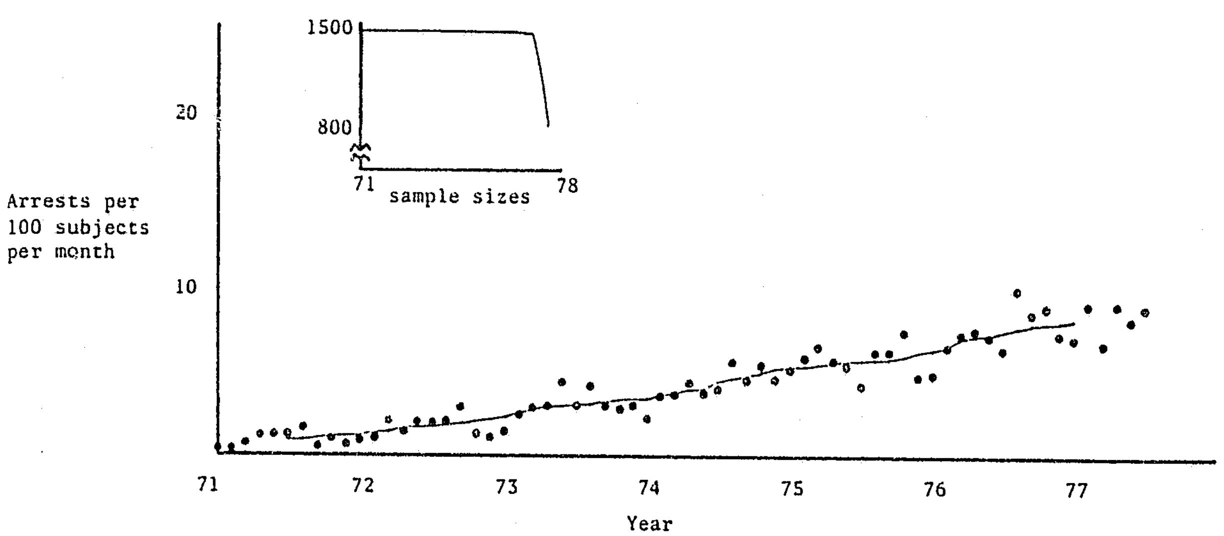



FIGURE 5.1
*Arrest Rates for the 1960 Birth Cohort*

The feature of the plot that first attracts the attention of an audience reared on maturation assumptions is the steady upward slope, continuing to the end of the observations. Nor, beyond the end of the plotted trendline, is there any evidence that the curve fell during the seventeenth year. The evidence suggests the opposite. The number of juvenile arrests during the last five time segments of 1977 were 66, 72, 68, 58, and 29 respectively--indicating a trendline continuing to climb, given plausible (or many implausible) estimates of the residual sample during those time periods.

An important retrospective note about the discussion of maturation among chronic delinquents in Chapter 3. The maturation phenomenon discussed in Chapter 3--constantly



upward until the point of intervention--is consistent with the pre-17 activity of the cohort sample of *all* delinquents.

The plot raises several intriguing questions about when the maturation effect takes place, in the absence of intervention. We remain believers in the essential notion behind the maturation theory. Surely delinquency in the general population is something that is "grown out of," in part. But the reservations we have expressed earlier about the size and nature of that part in the chronic delinquent are compounded by the data in the random sample. The question, "Who grows out of what, when?" has yet to be examined skeptically--for all delinquents, not just chronic ones.

The random sample does considerably more than tell us the shape of the overall delinquency rate in the cohort over time. It also provides an overview of how the Cook County juvenile justice system dealt with the 1960 cohort of delinquents. Where did our UDIS and DOC subjects fit into the larger context of interventions? The breakdown is as follows:

- Of the 1,482 youth in the sample, 179 (12.1 percent) reached the stage of official court supervision. Of these, 127 went no further.

- 109 youth were put on probation--7.4 percent of the sample. Of these, 46 had already been on supervision. Twenty-five of the 109 went on to DOC or UDIS.

- Only 44 out of the 1,482--3.0 percent--reached DOC (33) or UDIS (11).

From one perspective, these figures are reassuring. More than 70 percent of the youth put on supervision stopped and went no further into the system; more than 77 percent of the youth put on probation stopped their somewhat deeper penetration, and avoided DOC. But from another perspective, the figures add fodder to the complaints of those who criticize the system for leniency:

- Of the 3,609 repeat arrests (i.e., not the first arrest), 2,551 (70.7) percent) were committed by youth who at that time still had not experienced an official sanction, of any sort.

- More than half of the arrests--2,890, and 56.8 percent--were of youth who would never encounter an official sanction.

- Youth who eventually went to DOC or UDIS accounted for only 722 (14.2 percent) of the total offenses in the sample.

These statistics do not address the justice or the wisdom of the system, nor do we have any others that do address them. These statistics do not even address the effectiveness of the relative levels of court action. That is the topic next up for discussion. But they do serve as an antidote to the common image of delinquents defiantly committing new offenses in the face of sanctions. More than two out of three repeat arrests occurred before any sanction whatsoever had been imposed.



THE EFFECTS OF SUPERVISION.

*Subsample: All members of the cohort sample who ever reached the point of supervision.*

Like many juvenile courts, the Cook County Juvenile Court may take action without reaching a finding of delinquency. "Supervision" is the label for that lesser step. Supervision may entail a probation officer or other loose constraints. But in general, the intensity and degree of intervention associated with supervision are said to be less than those represented by probation. And, there is a difference in putative labeling. A youth put on supervision has not legally been declared a delinquent.

Of the 1,482 members of the 1960 cohort sample, 179 had been put on supervision. We begin the investigation of its effects by using the same approach taken toward maturation in Chapter 3. We compare arrest incidence among these 179 youth before supervision and after, plotted against age. The procedure (parallel with that described in Chapter 3) is to attach an identifier to each arrest: did it occur after or before the date of supervision? If it occurred "after," did it occur before the youth entered a subsequent intervention (probation or DOC/UDIS)? The denominator of the algorithm consists of sample size during the time period. For the "before" period, it consists simply of all youth who had not yet been put under supervision. For the "after" computation, it consists of all youth had already been put under supervision by that date *and* who had not yet passed on to another intervention.

The results are shown in Figure 5.2. They indicate that, for practical purposes, the trendlines are identical

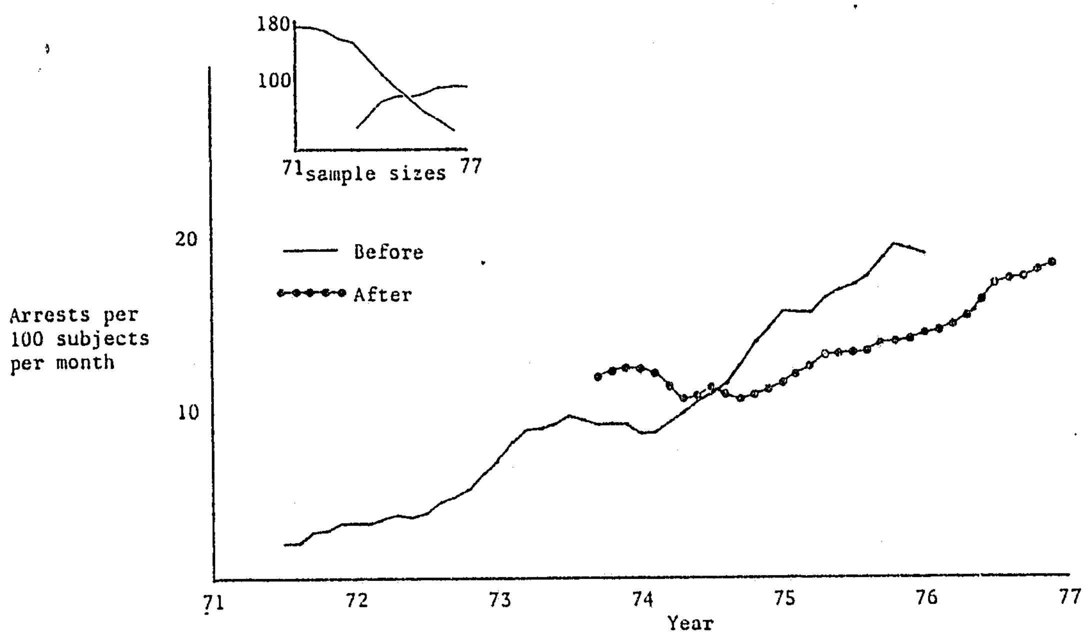



FIGURE 5.2
*Arrest Rates Before and After Being Placed on Supervision*

until the last year or so. By the sixteenth year, a gap does exist--youth who had not yet been put under supervision were committing offenses at a rate of 18.8 arrests per hundred per month (using a smoothed incidence estimate), while those who had already reached supervision were at an incidence level of 14.2. But the reduction is only .245, and its significance is untested.

Tests of statistical significance are hard to come by. Repeating the procedure used for the DOC and UDIS samples, the difference would not only be "not significant;" there would be no difference at all. That is, the procedure used for analyzing the DOC and UDIS before/after results employed a baseline measure that encompassed an entire year's history prior to intervention. As a visual inspection of the plot will indicate, the reduction in the last few months could not tolerate a parallel procedure, if the preintervention rate is computed from anything except the incidence in the last



few months prior to intervention, the analysis yields an estimate of exacerbation, not suppression, of delinquent activity following the imposition of supervision.

The principal alternative approach is a Box-Jenkins time-series model. Resources to conduct such analyses are being sought. They will have to employ some weighting scheme, however, to accommodate the recurring problem of regression artifacts. In the case of supervision (and, subsequently, of probation), the problem seems much more acute than it did in the case of DOC or UDIS.

Remember the discussion of the analogous plot of arrest incidence against age (Chapter 3). One of the virtues of the plot was the degree to which the bunching effects of selection were minimized. But, as a means of further reducing the residual contamination by selection effects, an additional plot was displayed in which the "before" segment included only subjects who were at least half a year from intervention. The procedure was found to make very little difference--arrest incidence still climbed steeply and steadily with age.

We can employ the same procedure to examine the degree to which the arrest trends "before" and "after" supervision are sensitive to the delinquents' behavior immediately prior to the court's action. The results are shown in Figure 5.3.

This time, the procedure alters the picture substantially. Whereas the youngster who went to DOC typically had a long, sustained history of arrests, supervision (which occurs much earlier in the career) did tend to be preceded by a "burst" of arrests. The deletion of the arrest record for the six months immediately preceding supervision lowers the trendline drastically--so drastically, that the post-

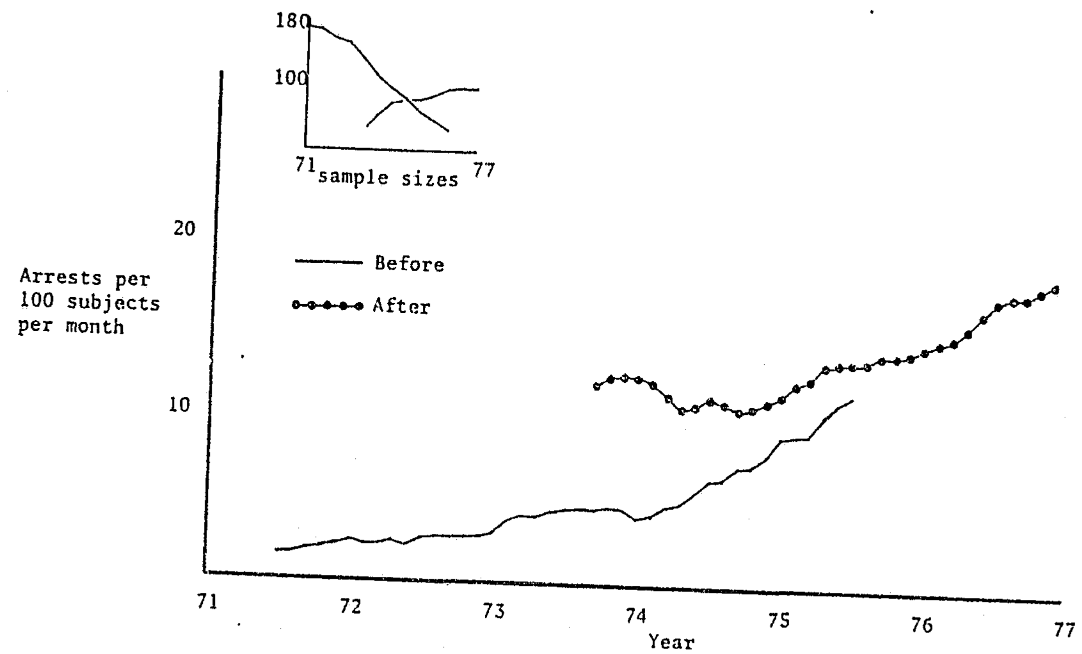



FIGURE 5.3
*Arrest Rates Before and After Supervision, Ignoring the Six Months Immediately Preceding Supervision*

supervision record is markedly worse than the pre-supervision record.

Whether one wishes to argue the case that supervision really makes matters worse depends largely on one's judgment about the meaning of the arrests in the six months preceding an intervention. If one is inclined to discount them as a selection artifact, then supervision can indeed be seen as a "mistake." Either leave the delinquent alone, or do something that will have an effect. If instead it is accepted that the court acts because arrest rates are genuinely increasing, then the easiest case to sustain is that supervision makes very little difference one way or the other.





*Subsample: All members of the cohort sample who ever reached the point of probation.*

Probation is the next step up the ladder of interventions. The youth is legally adjudicated delinquent. He is put on a probation officer's caseload, and some sort of program is established (only on paper, sometimes) whereby the youth will be able to demonstrate his fitness to be discharged from probation.

Of the 1,482 youth in the sample, 109 were eventually placed on probation. We use parallel procedures to plot their arrest incidence before and after probation, as shown in Figure 5.4. This time, we include the trendline produced by our six-month deletion procedure in the same plot with the "pure" preintervention trendline.

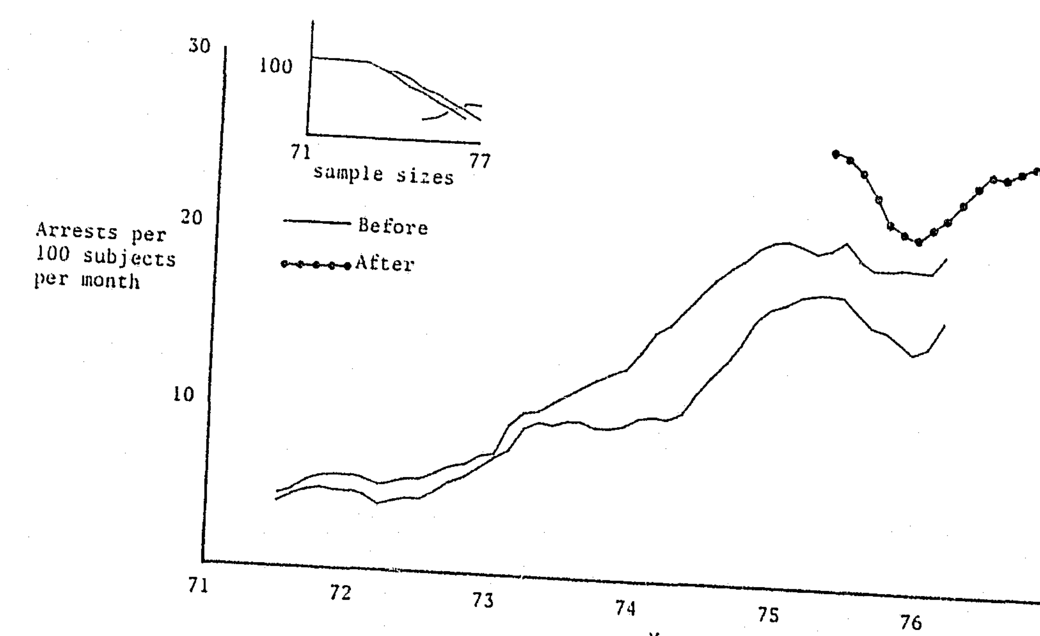



FIGURE 5.4
*Arrest Rates Before and After Probation, With and Without Including the Six Months Immediately Preceding Probation*

As Figure 5.4 indicates, probation fared even worse than supervision in producing a reduction in arrests. Arrest incidence was always higher for post-probation youth than for their counterparts of the same age who had not yet reached probation. And, once again, the discrepancy between the "before" and "after" rates is substantially increased when the six months prior to intervention are ignored. As in the case of supervision, much of the delinquent activity that led to intervention was bunched into the half year preceding the court's action.

We repeat our cautions about jumping to the conclusion that the court's action actually *caused* an increase in offense rate. The nature of the time-series makes it quite possible that the preintervention and postintervention trends are part of the same overall pattern. Resources are being sought to investigate this issue through the appropriate Box-Jenkins analysis.

## A REPLICATION OF THE ANALYSIS FOR DOC/UDIS YOUTH

*Subsample: All subjects in the DOC and UDIS samples who were born in 1960.*

It is instructive to replicate the above analyses for the DOC/UDIS case. Using the same procedures, the same axes, and the same scale as before, we picked out the 1960 birth cohort from among the overall DOC and UDIS samples. Given that the youth born in 1960 behaved like the ones born in 1958 or 1959, and given the plot of arrests against age in Chapter 3, the results can be predicted. They are shown in Figure 5.5.



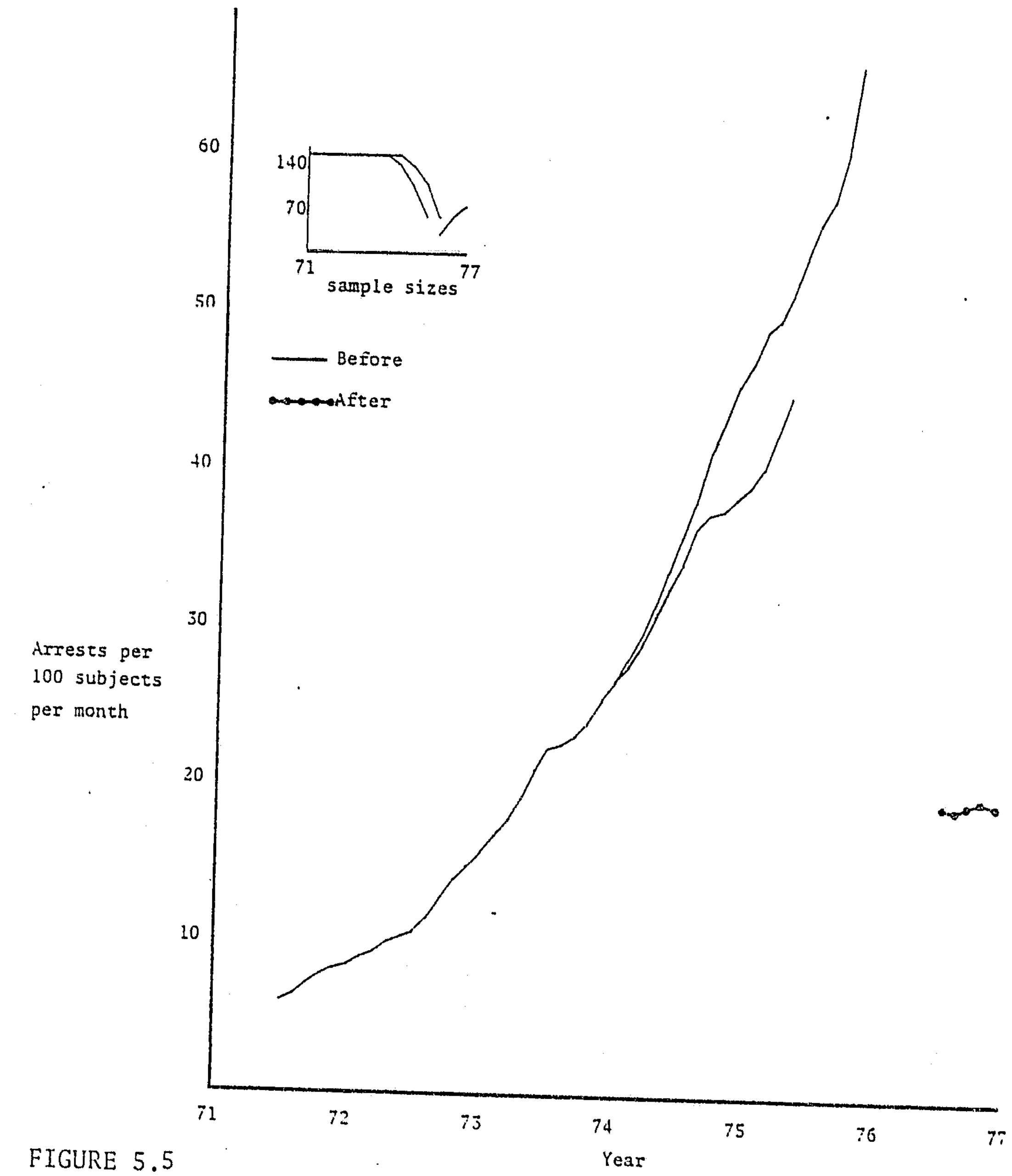



FIGURE 5.5
*Arrest Rates Before and After DOC/UDIS for a Sample Restricted to Subjects Born in 1960, With and Without Including the Six Months Immediately Preceding Intervention*

The contrast with the preceding plots dealing with supervision and probation is striking because of the identical methods used to prepare them. It should be noted that the results look the same if only the DOC/UDIS youth in the cohort sample itself (44 in all) are used.

## SUMMARY

Rationales for minimal intervention have often held that low-level interventions such as supervision and probation do very little good, and may only make matters worse. The findings of this study strongly support that view. The problem, of course, is that the rationales for minimal intervention also hold that the worst, most counterproductive intervention of all is institutionalization, a proposition that is directly contradicted by the findings of this study.

The examination of the 1960 birth cohort is also noteworthy in its failure to produce evidence that the general population of delinquents responds differently to intervention than the special population of chronic delinquents. The expectation taken into the analysis was that each level of intervention would produce its own version of the suppression effect, as subgroups of delinquents encountered the threshold at which they decided the game was not worth the candle. This phenomenon may have been operating at a micro level, but not in a manner that was recognizable over the samples of youth who underwent supervision and/or probation.



## NOTES TO CHAPTER 5

[1]For example, the range in 1975 was from 253,400 (November) to 277,600 (September), when months were weighted for numbers of days. *Vital Statistics of the United States, 1975.* USDHEW, National Center for Health Statistics:Hyattsville, Maryland. 1978, Table I-37.

## AFTERWORD

This is the usual place for the "Conclusions and Recommendations" chapter. We have none. Resources grew scarce, and the analysis came first. Nor are the conclusions ones that can be jotted down in a few hours. No easy policy prescriptions fall out of the data, least of all panaceas based on "lock 'em up."

The data do point unequivocally, we believe, to this: the grounds for debate about juvenile corrections must be shifted. The rhetoric that has guided national legislation and the policies of the Office of Juvenile Justice and Delinquency Prevention is based on the premise that corrections "only makes kids worse." That premise may or may not be true on some dimensions. No one really knows. But in terms of delinquent behavior, corrections does not make kids worse. It makes them better. Much better, from the point of view of the community that must live with them.

In choosing among correctional alternatives, the reflexive assumption that "less drastic is better" needs rethinking. As measured against the recidivism criterion, community-based alternatives were inferior to out-of-town ones. Alternatives that left the youth at home were inferior to residential ones.

Punitiveness as such is not the point. UDIS residential programs that were much less forbidding than some of the DOC



institutions did as well or better on the recidivism measures. Effective juvenile corrections need not be harsh or prolonged.

On the contrary, that juvenile corrections can be of benefit to the community does not require that we abandon the delinquents. That notion has been one of the obstacles to a square look at the problem posed by the chronic delinquent. We have persistently characterized policies as being "for" or "against" youth, then assessed the recidivism effects on a separate dimension. It is a false polarization of choices. Perhaps the major lesson of a program like UDIS is that the two types of benefit--for the youth and for the community--can be reconciled. Federal policy over the past decade has worked steadily to implement half of the lesson, that what is good for the youth is not necessarily bad for the community. Perhaps now we can turn to the other half--that what is good for the community is not necessarily bad for the youth.

A final, more general thought about the program of the National Institute for Juvenile Justice and Delinquency Prevention:

Once it became clear that our findings were putting the study well outside the mainstream of thought on juvenile corrections, it became of major concern to determine whether our data conflicted with the true state of knowledge.

It was only when we set out to examine what is known that the full disarray in the conventional wisdom became apparent. It is riddled with myths--about the role of maturation, about the inability of corrections to reduce offenses, about the virtues of less drastic interventions. On each of these topics, real, interpretable data are virtually nonexistent.

The literature on the correctional process is large. The literature on the psychology and sociology of delinquency is enormous. The literature on absolute measures of correctional outcomes is adequate. But all of these are circuitous approaches to the policy issue arguably of most immediate interest to the public: how to reduce the number of serious offenses committed by juveniles. Our experience suggests that the hard questions have yet to be asked. Basic behavioral analyses of what delinquents *do*--not think, not say, but do--over the course of their careers, under varying social controls and interventions, have yet to be conducted.



APPENDIX A.

## DESIGN OF THE STUDY

The original evaluation of UDIS was conducted from April 1976 through September 1977, under contract to the Illinois Law Enforcement Commission. The American Institutes for Research was the prime contractor. W. V. Rouse & Company, a division of Barton-Aschman Associates, was a subcontractor. The subsequent research reported in this volume was supported by a grant from the National Institute for Juvenile Justice and Delinquency Prevention to the American Institutes for Research, and was conducted from February 1978 through March 1979.

## STAFF

Charles A. Murray was Project Director and Principal Investigator for both segments of the work. Cindy B. Israel was Assistant Project Director for both segments, until her departure from AIR in September 1978. Douglas Thomson, then of W. V. Rouse & Company, now associated with the University of Illinois at Chicago Circle, directed the on-site work during the UDIS evaluation and conducted an analysis of relevant correctional literature for the follow-on study. Louis A. Cox, Jr., joined the project in the fall of 1978 and is coauthor of the present report. Sidney Buster of W. V. Rouse participated in both segments of the research and directed the on-site team during the follow on.

A large supporting group participated at points throughout the project. Additional support during the data collection phase of the original evaluation was provided by Blair Bourque, Ingrid Heinsohn, and Shirley Hines of AIR, and students affiliated with the Northwestern University Center for Urban Affairs. Kathe Serikaku and Susan Adams of Northwestern University jointly supervised the interviews with the UDIS clients. During the follow on, preparation of the data files was conducted by Nancy Cox, Jeanna Faucett, Helen MacKenzie, and Pamela Belluomini.

Wesley Snyder and Paul Fingerman of AIR participated in the design of the original evaluation. Fingerman and Tetsuro Motoyama also provided support in the quantitative data analysis for the follow on. Joan M. Flood and Mary Martin prepared the final report.

## GENERAL DESIGN CHARACTERISTICS

For questions relating to the evaluation of UDIS as a specific program in a specific context, a case study approach was employed. Extensive interviews were conducted with the staff of UDIS and members of the agencies with which it dealt; reports and memoranda, case files, and operating procedures were collected and reviewed. These were subjected to a qualitative review. Sometimes we went as far as to assign frequencies to responses--"17 of the 35 vendors reported that..."--but no analytic statistics were used.

For questions relating to the impact of the program, we sought as rigorous a design as the situation would permit. The basic design was

| | |
|---|---|
| Experimental Group: | $O_1(X_uO_2)O_3$ |
| Comparison Group 1: | $O_4(X_{D1}O_5)O_6$ |
| Comparison Group 2: | $O_7(X_{D2}O_8)O_9$ |

where the "O's" represent observation periods, $X_u$ represents the UDIS intervention, $X_{D1}$ represents the DOC (Department of Corrections) intervention *prior to* the advent of UDIS, and $X_{D2}$ represents the DOC intervention during the same time period that UDIS was observed. The bracketed (XO) combinations denote that during-program observations of the dependent variables were included.

The purpose of observing a sample of youth who went to DOC before the establishment of UDIS was to protect against the possibility of false-positive results for UDIS. We anticipated that judges would send the borderline cases to UDIS and continue to send the hard-core, most intractable delinquents to DOC. Results favorable to UDIS could be interpreted as an artifact of selection. In that event, we planned to try to identify the set of pre-UDIS commitments who were borderline--analogues to the youth who were sent to UDIS when that opportunity opened up--and use them as an additional comparison group. As it turned out, the procedure was superfluous on two counts. The data did not convince us that judges effectively discriminated between borderline and hard-core cases, and UDIS did *not* excel the record of DOC on the recidivism measures. The design that was actually operationalized was the more straightforward one,



| | |
|---|---|
| Experimental Group: | $O_1(X_uO_2)O_3$ |
| Comparison Group 2: | $O_4(X_{D2}O_5)O_6$ |

## THE SAMPLES

### DOC and UDIS Samples

The original UDIS evaluation worked with samples from each population (UDIS, DOC, and the Pre-UDIS Baseline). The follow-on work is based on the entire population of *males with a Chicago address who entered DOC or UDIS from 1 October 1974 through 30 June 1976*. A youth was excluded from the analysis only if data on one of the three key topics (court history, police record, correctional history) could not be obtained, if contradictions existed, or if the record appeared to be incomplete. For police records, we assumed that a record was incomplete if the total number of arrests was three or fewer and none occurred within six months of intervention. Intervention histories were considered incomplete if a month or more of the in-program history could not be accounted for. Court records were considered incomplete if one or more dispositions could not be found for petitions that were known to have been filed.

Sample attrition was as follows:

| | UDIS | DOC |
|---|---|---|
| Total population | 325 | 421 |
| Deaths | 4 | 1 |
| Incomplete intervention data | 9 | 11 |
| Incomplete court data | 6 | 13 |
| Incomplete police data | 19 | 17 |
| Contradictions, ambiquities | 7 | 1 |
| Number with complete data | 280 | 378 |
| DOC to UDIS combination | -- | 12 |
| Not released as of 2/28/78 | 11 | 49 |
| Net recidivism samples | 269 | 317 |

The effects of the missing delinquents--those not released by the end of the observation period--are discussed in Chapter 2.

## The 1960 Birth Cohort Sample

The random sample of the general delinquent population was chosen from among all boys with a Chicago address born in 1960 who were arrested at least once by the Chicago police before their seventeenth birthday. The sample was drawn from the files of the Youth Division of the Chicago Police Department, where all juvenile arrest records are maintained. Four cards were drawn from each drawer. The police employee who assisted us in selecting the sample started at the beginning of the drawer, and took the first subject who met the birth and residence criteria. She then repeated the procedure three times for each drawer, flipping approximately one fourth of the cards to a new starting point for each repetition. A total of 1,515 cards were drawn. Of these, 33 were deleted: 9 because of ambiquities in the police data and 24 because the police data indicated that the youth had been sent to court, but no court record existed. The deletion of the 24 cases with missing court data presumably has led to a slight understatement of the proportion of juvenile arrestees who are referred to court. But police and court sources indicated that to assume all 17 without a court history actually appeared in court could introduce greater error than deleting them; hence the procedure that was adopted.



# ARCHIVAL DATA COLLECTION

Archival data were collected from four major sources: UDIS, the Illinois Department of Corrections, the Chicago Police Department, and Cook County.

## Data Sources

*UDIS.* For the evaluation sample, information was obtained that described both UDIS operations and the individual clients. We reviewed samples of internal communications (intra-office memoranda and minutes of staff meetings), minutes of the Juvenile Justice Policy Board meetings, budgetary materials, and statistical data from the Tracking system operation by the Northwestern University Center for Urban Affairs. For each youth in the evaluation sample, we examined all of the materials contained in the case files. These included:

- UDIS Facesheet: background information on living arrangements, family, school, employment, and court history.
- Probation Officer's Referral Form: background information on the youth.
- UDIS Assessment: psychosocial analysis of the youth's situation and a recommended treatment plan, performed by social workers and psychologists under contract to the UDIS program.
- Social Investigation: descriptive assessment prepared by the Probation Officer of the youth's background, family life, school situation, peer associations, psychological problems, and police contacts. Reports were based on discussions with the youth, his family, school authorities, and any agencies involved in the case.
- Summary Progress Reports: periodic updates of the Social Investigation.
- UDIS Performance Contract: document completed by the Case Manager, the youth, and his parents at the time of program entrance. It stipulates the performance goals to be fulfilled by the youth and UDIS during the program.
- Youth Evaluation Sheet: monthly reports discussing the youth's progress in each program. The reports are completed by the servicing agency.

o Unusual Incident Report: description of any "unusual incident" (police contact, disruptive behavior, runaway) that occurs while the youth is in a placement.

For the follow-on UDIS samples, we were primarily concerned with the placement histories while in UDIS. Two sources were used: the computerized data base maintained by the Northwestern University Center for Urban Affairs and the fiscal records kept by the business manager of UDIS. Both sources provided specific dates of entrance into and exit from UDIS and each individual placement.

Information on UDIS costs were based on aggregate figures. These data were collected from the invoices submitted to UDIS by each service agency, and from overall budget figures compiled by the DOC financial office.

*DOC*. Our data collection efforts at the Department of Corrections paralleled those at UDIS.

The archival data were collected at the Juvenile Division headquarters in St. Charles, Illinois. They included DOC institutional program descriptions, policy statements, summary statistics on juvenile commitments, authorized and unauthorized absences, and financial data. Weekly population summaries were obtained for the period 1 January 1973 through 31 December 1976.

Information on individuals was drawn from two main sources at the Juvenile Division's main record office: summary record cards and case files. The summary cards provided background information (race, sex, age, county), identification numbers (which allowed us to access the numerically coded files), and the institutional movement histories of youth. These were examined for the follow-on sample. The case files contained several documents which provided more specific data on each youth, collected for the evaluation sample only:

o DOC Facesheet: background information on the youth's living arrangements, family, school, police contacts, and court appearances.

o Social Investigation: descriptive assessment prepared by the Cook County probation department of the youth's home and school situation, peer associations, psychological problems, and delinquent activities.



o Social History: descriptive assessment of the youth's background prepared by the Department of Corrections case workers.

o Psychological or Psychiatric Evaluation: psychological workup, diagnosis and recommendations prepared by the DOC staff psychiatrist or psychologist.

o Health Evaluation: results of medical and dental examinations.

o Test Scores: results of an electroencephalogram, IQ, and Stanford Achivement tests.

o Progress Reports: assessments of the youth's progress during commitment prepared by the correctional staff.

Additional materials were found in several of the files. These included court clinicals, school records, and reports from social service agencies.

The adult files of DOC were also reviewed to document commitments to the Adult Division for our sample youth.

*Chicago Police Department*. The records of the juvenile and adult divisions of the Chicago Police Department served as the source for police contact data.

The juvenile records were comprised of juvenile record summary cards and numbered files containing arrest and community adjustment reports. The juvenile record summary cards listed the youth's name, birthdate, and address; the Youth Division Identification Number; and the date, type, and disposition for each offense for which the youth was apprehended. The arrest and community adjustment reports found in the files provided a narrative of each offense, including details about victims, monetary value of property stolen or destroyed, premises entered, and people participating in the incident.

We also obtained access to individual arrest reports for all sample youth who were apprehended and treated as adults by the Chicago Police Department. The adult arrest reports were similar to those filed by the juvenile division, providing the youth's name, birthdate, address, and a description of the offense.

One important limitation should be noted. When a youth reaches 17, it is the policy of the Chicago Police Department to destroy the individual arrest narratives and retain only the summary card. Since virtually all of the subjects had long since reached 17 by the time that the follow-on police data were collected, the information required to determine seriousness scores was thus often unavailable. We have not attempted to present analyses of seriousness based on the partial data we did obtain. For results obtained from the evaluation sample (collected more than a year earlier, when a much higher proportion of the narrative data still existed), see pp. 140-145 of the original evaluation.

*Cook County Juvenile Court.* For the follow-on sample, complete juvenile court histories were obtained. Data obtained on each position included date of filing, date of disposition, and nature of the disposition.

It may be added that these data turned out to be extraordinarily difficult to obtain. The court personnel were most cooperative, but the information had to be pieced together from handwritten logs and individual court records (filed by number, not by name). Gaps in these data account for most of the attrition in the sample, as indicated in the discussion above.

## ANALYTIC ISSUES

Most of the analytic issues are discussed at the appropriate point in the report.

### Reliance on the Records of Official Delinquency

The analysis of recidivism is based wholly on police records. The question often raised is whether officially recorded delinquency is a valid measure of recidivism. These points are pertinent to the analyses in this report.

*The Relationship between Official and Real Levels of Delinquent Activity.* The most commonly cited defect of official data is that only a fraction of offenses result in apprehension and, among juveniles, only a portion of apprehensions reach the stage of documentation in police records--a problem that clearly applies to this study. The reader should in all cases remember that the raw numbers of arrests reflect the offensive activity that has surfaced, not the whole story of the delinquent behavior in which these youngsters have engaged.



What multiplier should be applied? One well-known study (Williams and Gold, 1972) found that among boys with a police contact, less than three percent of all arrestable acts had actually resulted in arrest, implying a multiplier as high as 33 times the official arrest record. But that proportion was based on a very wide range of offenses, including minor ones, for a general population of youth. Presumably the major offenses such as robbery, assault, and burglary that make up the records of the offenders in this study are somewhat more likely to be apprehended than lesser offenses, and certainly more likely to lead to official documentation when they are apprehended. In Chapter 11, evidence from victimization studies and from clearance statistics are used to establish a range of apprehension rates, without regard to the age of the offender. The probable range is estimated to be 10 to 20 percent.

The inability to estimate real levels of offensive activity is a serious obstacle in assessing costs and benefits. A relatively small number of arrests among program participants may represent a much greater level of real delinquent activity. The degree of these "hidden costs," if known, could decisively affect the conclusions.

On the issue of comparative impact of DOC and UDIS, however, the partial count represented by the police records presents more manageable problems. DOC and UDIS youth in the samples came from the same parts of town, with comparable backgrounds. There is no reason to believe that the police were more likely to apprehend one population than another. It is plausible that the apprehensions taken over the groups as wholes reflect comparable sampling distributions of real offenses and that differences between the two groups can therefore be treated as reflective of differences in real levels of offensive activity.

Similarly, we find no persuasive reason to believe that the before-after comparison are contaminated by reliance on official records. If arrests drop, the most parsimonious conclusion is that real offensive activity has dropped. The alternative explanation--that arrests drop because large numbers of the youngsters are getting smarter and are therefore caught less often--requires an elaborate set of assumptions about the way delinquents come to commit delinquent acts, about the proportion of budding professional criminal craftsmen in the population, and about the learning process. We did not find patterns in the data which would be consistent with these assumptions. We found other patterns, especially in the patterns of time between offenses that are not consistent with the getting-smarter hypothesis.

In passing, it can be noted that before-after comparisons using police data have more face validity than a similar comparison using self-report data. The data collectors were blinded as to the timing of the offense relative to the program intervention, and no problems of memory distorted accounts of offenses that occurred many months or years before the data were collected. In contrast, a self-reporting youth would have problems with memory in reconstructing his preintervention career. He would also have incentives to make the before-after comparison of offensive activity fit whatever points he might wish to make about the benefits or harm done to him by the intervention.

*The Relationship of Police Allegations to Actual Behavior*. This study ignores whether the youth was found by a court to have committed the offense for which he was arrested. To what extent might the data be inflated with instances in which the youth was innocent?

In this, then police narratives available during the original evaluation indicate that, at least from the statistical viewpoint, the problem of unwarranted arrests is not likely to have influenced the analysis. In most cases, the apprehension occurred in the act, where the chance of mistake was minimal. In the remaining cases, the collateral evidence tended to be strong; for example, eyewitnesses who knew the youth by name, or stolen property that was found in the youth's possession within a few hours of the offense. There was the occasional police account that lacked specific documentable evidence that the allegation had a strong foundation, but the number of these was statistically trivial. Short of assuming that the arresting officers were making up facts on a wholesale basis, the relationship of the allegations to actual behavior appeared to be close.

## Treatment of Selection Bias in the Comparison Groups

The question of sample bias in the analysis of recidivism is inescapable because of two extremely important factors. First, 33 of the highest-risk UDIS referrals--equal to 18.6 percent of the subjects who had exited from UDIS--were unavoidably skimmed off the top of the UDIS sample. These were the boys who got into sufficient trouble while they were in UDIS to be terminated directly into DOC. Virtually without exception, the trouble consisted of frequent arrests or an arrest for a serious offense, but they have no "after UDIS" record at all, and hence could not be part of a



recidivism analysis.* This selection bias alone stacks the odds substantially in UDIS's favor, and it is intractable to correction.

The second factor is that judges understandably tried to send the most dangerous delinquents to DOC and put safer ones in UDIS. A systematic selection bias was at work that must be presumed to have worked in UDIS's favor if judges had a better than random chance record of identifying "propensity to recidivate." Both points are raised in the discussion of the comparative results of UDIS and DOC.

*They were of course excluded from the DOC sample as well because of the confounding effect of the prior UDIS experience.

APPENDIX B.

## UDIS OPERATIONS AND SERVICES

Originally funded by the Illinois Law Enforcement Commission, UDIS began operations in October 1974 as a demonstration project designed to perform two primary functions.

First, UDIS was to take as its client the juvenile who had gotten into trouble with the law often enough, or for offenses so severe, that he would otherwise have been consigned to one of Illinois' seven correctional institutions for youth. UDIS was to embody the proposition that deinstitutionalization could be extended to the chronic, serious delinquent; that it need not be limited to first-time offenders, to misdemeanants, or to status offenders. UDIS was to be an alternative to incarceration.

The second function of UDIS was to unify resources, as its name implied. In Chicago as in other large areas, the human skills and physical facilities for dealing with youth have been scattered among private agencies, public agencies, store-front operations, church-sponsored groups, school-related institutions, and other decentralized, uncoordinated groups. UDIS was to be a central point at which youth and resource could be brought together.

UDIS brought to these tasks an internally consistent set of principles about juvenile corrections, all of which were argued to represent a less repressive, more supportive approach to correctional intervention than does institutionalization.

"The least drastic alternative principle" was to be the centerpiece of the UDIS approach: Given a choice, UDIS would put the youth in the environment least unlike his normal one. Keeping the youth at home was less drastic than taking him away from home; providing services in the community was less drastic than providing him with services located elsewhere; and letting the youth retain freedom of movement was less drastic than instituting compulsory controls.

Next, the UDIS approach was to get the youth out of the juvenile justice system fast--within six months if possible. Long-term care was not to be UDIS's style. The urgings of service providers that "just a few more weeks" in a program would produce the desired effects were to be resisted.



The third key principle of the UDIS approach was to be individualized programming. Detailed work-ups on each case were to be conducted, going beyond the social and family investigations that the courts or the welfare agencies had conducted. They were also to come at the situation from a fresh perspective: to ascertain the strengths in the youth and the reinforcing elements in his environment, not to catalog deficiencies. Once this was done, the UDIS participant could get the programming he or she required. Progress would be monitored closely by a Case Manager with a relatively small caseload, and it would be possible to make changes quickly when indicated.

### The Mechanics of the Program

A juvenile offender was said to be eligible for UDIS if he had been adjudicated delinquent on two petitions or if he had committed a serious offense and, for either reason, was likely to be committed to DOC. Several directives to probation staff codified these criteria. The system was supposed to operate (and usually did) in the following manner.

A Probation Officer (PO) confronted with a youngster who was a candidate for DOC decided whether, in his or her judgment, the offender could be maintained in a noninstitutional setting through the network of services that UDIS could tap. If so, the PO got in touch with a UDIS Case Manager who then decided whether the case was eligible. If UDIS indicated that it was, the PO requested referral to UDIS at the next court hearing. If the judge consented, the youth was referred to UDIS for a two-week period of assessment. A psychologist or social worker under contract to UDIS then interviewed the youth and prepared an assessment report.

The Case Manager was responsible for developing a program plan jointly with the youth, his family, the PO, and service agencies. A "performance contract" was prepared stating what the youth agreed to do as his part (typical provisions were to "stay out of trouble," and "go to school") and what UDIS would provide as its part of the bargain. On the court date, the assessment and performance contract were presented to the judge who then decided whether to accept the plan, to direct UDIS to rework it, or to commit the youth to DOC.

Once the judge had accepted the proposed program, the youth was entered into the services that had been chosen from among the roster of advocacy, alternative education, family therapy, vocational training, wilderness stress programs, group homes, and psychiatric services with which

UDIS maintained service contracts. Most UDIS clients received services from more than one vendor. The services could be provided on an overlapping as well as on a sequential basis. Changes were decided upon by the Case Manager.

A youth participating in the program remained technically on probation, with continued participation in UDIS stipulated as a condition of that probation. The PO maintained contact with the youth and his family, the Case Manager, and the vendors providing services to the youth. Vendors reported unusual incidents (for example, a police contact, injury, or felonious activity) to the Case Manager who reported them to the PO. The vendor worker also included such incidents in the written monthly report on the youth.

Each UDIS participant received an identification card documenting his status as a UDIS referral. If picked up by the police, a UDIS youth was supposed to show them the UDIS card, in hopes that the officer would get in touch with UDIS. At least one UDIS staff member was on call 24 hours a day.

Case Managers were not direct service workers but instead performed a number of coordinating functions. They brokered services by identifying appropriate resources, arranging for placements, and helping to establish objectives and expectations. They also monitored the youth's progress and the progress of the vendors working with him, and prepared reports on the results for the court.

If there was a major unscheduled change of program or a serious problem, or if the youth was picked up by the police for a new offense, the case was usually brought back before the judge. If the judge determined that the youth had violated the conditions of his probation, the youngster could then be committed to DOC or, depending on the judge's decision, a new program might be developed.

"Egress" was UDIS's term for successful completion of the program. If all went well, a youth was supposed to egress within six months. He could stay in the program longer if the Case Manager judged that the extra time was in the youth's best interest. But after six months the decision to maintain the youth in the program was to be subjected to increasing scrutiny by the case management supervisors.

"Termination" was the word for failure. Termination could be associated with direct commitment to DOC. In



other cases, termination could occur when the UDIS staff decided that their resources had been exhausted, or that the youth was chronically failing to participate in the chosen services. A youth who had been terminated but not committed ordinarily returned to his standard probation status. An egressed youth might or might not return to probation, depending on the circumstances.

## Staff Organization and History

UDIS was a small organization with a simple structure. The central job was that of the Case Manager, each of whom looked after 25 to 30 cases.

There were minor changes in the organizational structure of the UDIS office, but staff responsibilities remained generally stable. The basic administrative staff during the period of the evaluation included an executive director, a fiscal officer and an administrative assistant. The executive director had responsibility for the overall administration of the project. The fiscal officer maintained the financial records for the program and ensured that vendors were paid for services provided to the UDIS clients. The administrative assistant oversaw the office routine.

Program staff included a program coordinator, two case management supervisors, two resource monitors, a court representative, and eight Case Managers. The program coordinator negotiated vendor contracts and assumed responsibility for the program staff. The case management supervisors directed the Case Managers, monitoring progress of their cases and ensuring that the Case Managers were in close contact with their clients and service providers. Resource monitors had responsibility for making monthly on-site visits to all vendors to ensure that vendors were fulfilling contract stipulations. The court representative screened prospective UDIS clients and acted as a liaison with the judges and probationary personnel.

Four support staff completed the UDIS roster, providing secretarial services to both the administrative and program staff.

## Service Resources

The menu of vendor services available to UDIS Case Managers fell into six categories: advocacy, counseling, educational/vocational, group homes/foster care, rural programs, and intensive care. These placement types may be arranged roughly along a continuum ranging from least drastic alternative to most drastic alternative. The fit is not perfect, but advocacy, counseling, and educational/vocational services are generally consistent with maintaining the youngster in the community; group homes/foster care represent a more controlled environment that may or may not be linked with the youth's community; and the rural and intensive care programs are typically residential with no community ties.

A seventh type of setting for UDIS youth was Audy Home, Cook County's juvenile detention center. UDIS referrals were put there out of necessity, not as a "placement," and Audy is therefore excluded from the analyses.

Through mid-1977, UDIS had contracted with 110 vendors. These included the providers of services mentioned above and individual assessors and consultants. A complete list of resources that UDIS has tapped would be larger yet. Contracts were not used for doctors or hospitals nor for providers who billed for no more than $500 of services. Table 1.1 shows contract vendor services available as of July 1976, a typical representation of UDIS services once the program was underway.

Table 1.1 UDIS Service Contracts as of July 1976

| Vendor | Number | Placement Shots Range | Placement Shots Total | Placement Shots Mean | Median Charge |
|---|---|---|---|---|---|
| Advocacy | 12 | 5-40 | 187 | 15.6 | $58.64 per week |
| Counseling | 7 | 5-30 | 85+ | 17.0 | $25.00 per hour |
| Educational/Vocational | 9 | 5-30 | 60+ | 12.0 | $7.38 per hour<br>$48.50 per week<br>$226.34 per month |
| Foster Care | 4 | 5 | 10+ | 5.0 | $136.60 per week |
| Group Homes | 7 | 4-8 | 48 | 6.9 | $36.60 per day |
| Rural Programs | 4 | 8-15 | 42 | 10.3 | $33.00 per day |
| Intensive Care | 3 | 3-6 | 12 | 4.7 | $36.35 per day |



The following descriptions convey the nature of each of the six main categories of service.

## Advocacy

"Advocacy" is a relatively recent concept in juvenile services. It refers to the role of representing the youth in his confrontation with the juvenile justice system or any other of the institutions he might encounter. An analogy is often drawn between this role and that of the typical middle class parent whose child is first picked up by the police. The parent may be angry or may be supportive, but he or she will go to the police station, make the necessary commitments to the authorities, get a lawyer if the offense is a serious one, arrange for treatment services if indicated, and generally run the interference that keeps most middle class delinquents out of training schools. Advocacy services were intended to provide the UDIS youth with a person who could fill a similar role in dealing with the system. As of July 1976, UDIS had contracts with 12 advocacy agencies.

Services focused on intensive contact, with youngsters and advocates spending from 10 to 25 hours a week together. Programs varied, but many provided athletic activities, trips, social functions, and peer group "rap" sessions. Almost all furnished weekly allowances for their clients.

The categories of "advocacy" and "counseling" overlapped, but important differences remained. While advocates sometimes provided counseling services, they attempted to do so outside of traditional counseling settings. They viewed themselves more as representatives for their charges--in the court, the police precinct, the school, and the family. As one advocacy director put it: "Advocates view themselves as 'Philadelphia lawyers,' helping kids to deal with the system and society and being their friends. Counseling is informal, done in the car or during half-time of a basketball game."

The UDIS and advocacy philosophies were compatible: both acknowledged the role of their clients as victims as well as offenders--victims of neglect, poor schools, peer pressures, and institutions of social control. To provide an advocate to represent and support the youth was thus consistent with UDIS's youngster-against-the-world view. But there was also a highly pragmatic reason for the importance of advocacy in the UDIS service roster. UDIS had to demonstrate first that it could offer more intensive personal contact than a probation officer could and, second, that it could provide surveillance while the youth was in the community to protect public safety. The advocacy agencies claimed to fill both of these functions.

Almost any youth who was not placed in a residential setting outside of the community was placed with an advocate. Often, this placement was combined with one in an educational or vocational program.

## Counseling Services

Counseling services were intended to help the youth reconcile the conflicts in his life. Unlike the street-oriented advocacy services that focused on information contact between the paraprofessional worker and the youth, the counseling services offered scheduled weekly or twice-weekly sessions with professional counselors. As of July 1976, UDIS had contracts with seven counseling agencies.

Vendors typically provided three types of counseling: individual, family, and group. Counseling style varied with both the program and the worker. Some used traditional psychoanalytic techniques while others employed reality therapy, parent effectiveness training, or a positive peer culture model. Family counseling sessions offered the youth a structure for confrontation and interaction with all family members in an attempt to remediate deteriorating relationships. The Near North Family Guidance Center specialized in providing treatment to drug abusers and their families.

Referrals to counseling agencies were usually made when a Case Manager felt that his client's conflicts in the home required professional intervention, that a client's problems in social or psychological adjustment required a more therapeutic approach than an advocacy agency could offer.

## Educational/Vocational Training

*Academic Programs.* With only a handful of exceptions, UDIS referrals had school problems. Truancy and poor school performance were nearly universal among the members of the sample, and a number of personal history folders also made



reference to learning handicaps or mental deficiencies. UDIS therefore from the beginning maintained contracts with academic services. In broad terms, these services fell under two headings: tutorial programs, which worked with students on a one-to-one or small-group basis, and alternative schools, which provided a normal class structure with special supplementary resources for problem youth. Both types of services were designed to provide maximum learning in a minimum time--they had the UDIS youngsters for only a few months and were asked to compensate for years of academic deficiencies. In addition, both types of programs sought to foster positive attitudes toward learning that, it was hoped, would survive the short-term intervention. As of July 1976, UDIS had contracts with three tutoring programs and one alternative school.

Discussions with the Case Managers indicated that a youth was likely to be chosen for an educational service for one of four reasons. The first three consisted of special learning needs: functional illiteracy, diagnosed learning handicaps, or pronounced behavioral or motivational problems that prevented the youth from learning in a traditional school setting. The fourth reason was the youth's own interest. If a youngster exhibited an interest in going back to school and the Case Manager felt that the child's public school was not an appropriate setting, a referral was made to one of the educational services.

Tutorial services, while offering instruction in a full range of academic subjects, emphasized the development of reading and math skills. Some programs were designed specifically to prepare older students for the general equivalency degree (GED) examination. Others played a more general catch-up role, helping the youth raise his grade level to the point that he could reenter the public school system.

The chief advantage of the tutorial service was that it permitted a high degree of individualization: one-on-one and small group instruction, periodic testing, individual counseling, and flexible hours of attendance. Recreational and vocational activities were used as incentives and rewards for academic performance.

The individualized nature of the academic programs also tended to produce a spillover function among program personnel. Staff members noted that although advocacy was not explicitly one of their functions, tutors were sometimes asked to take their UDIS clients to the hospital, pick them up at the police precincts, or represent them at court hearings.

As Case Managers frequently pointed out, the tutorial service was for many of the UDIS youth their only opportunity for a positive learning experience, the extent of the child's learning problems often being such that individualized attention was the only way to break through barriers that had been built up over the years. The importance of this advantage of the tutorial programs was reflected in the proportions of UDIS referrals sent to them rather than to the alternative school.

As of July 1976, only one alternative school (CAM Academy) maintained a contract with UDIS. The Academy provided an ungraded year-round program that allowed students to obtain high school diplomas in one to two years. Offerings were diverse, ranging from traditional courses in grammar and simple mathematics to projects in "self-awareness for the future" and "community portrait photography." The program promised to engage the youth in the educational process through informal relationships with staff. Intensive individual and group counseling were said to be a central part of the alternative school program. There was a one-week counseling orientation period as well as frequent group counseling sessions during the school term. Program components included vocational workshops, GED evening classes, and a special educational curriculum for students with severe reading difficulties. UDIS clients were considered "special program students" by the Academy and did not have to meet the usual entrance requirements of graduation from elementary school or qualifying scores on admission tests.

*Vocational Training.* Vocational programs were intended to prepare the older UDIS client for independent living by providing training and exposure to the world of work. The emphasis was on development of employable skills so that the UDIS youth who had egressed could realistically believe that the option of getting and holding a legitimate job was open to him.

Some programs, or some components within programs, required that the youth be over 16 years old or have completed the eighth grade. These restrictions, combined with the general preference of UDIS to return its youngsters to school whenever possible, meant that the youth referred to vocational programs was likely to be older than the average referral. Characteristically, the participant in a vocational program was a high school dropout who had expressed interest in learning a trade.



Four of the five vocational training programs accepted both male and female clients. One--"Pretty Girl," a modeling and self-improvement school--served only female clients. With that exception, the vocational training programs provided structured courses in mechanical and industrial arts: welding, auto mechanics, machine and raw-material processing, and printing. Some participants received on-the-job training with the Illinois Department of Vocational Resources and the Chicago Alliance of Businessmen. Vendors typically tried to locate appropriate placements for clients who successfully completed the training schedule.

As in the educational programs, vocational training services tended to fill advocacy, counseling, and educational functions as well. Three of the vendors offered individual or group counseling for their clients. Two of the programs provided academic tutoring and GED preparation.

## Group Homes/Foster Care

The placement that we call "a controlled environment" was intended to provide structure and supervision away from family tensions, neighborhood haunts and delinquent peers, while still within Cook County. Three types of services fell within the controlled environment category: group homes, foster care, and a Transitional Living Program.

*Group Homes.* Group homes were typically large, old houses with seven to nine adolescents in residence. Some of the homes were overseen by a set of houseparents, usually consisting of a young married couple. Others were staffed by counselors who rotated shifts to provide 24-house supervision. While a few homes offered in-house structured daily activities, others remained primarily living quarters to outside agencies. All the houses placed counseling at the center of their programs. Many scheduled regular group and family counseling sessions, and had staff members trained in crisis counseling. Residents were enrolled in public and alternative high schools as well as GED and vocational training programs. Tutoring was to be provided by house staff when the need arose.

Some of the residents held part-time and summer jobs. Recreational opportunities included memberships in community centers, the YMCA, and recreational programs held at high schools, as well as field trips and special house occasions. The majority of the program personnel interviewed indicated that advocacy was limited to representation in the court or with the police.

The homes were intended to structure their programs around the eventual return of the youth to his family. Parents were usually encouraged to visit the homes at any time. After a brief orientation period, youth often returned to their own homes for overnight and weekend visits. Despite their residential character, the group homes typically were very far from being even a minimum security environment.

A youth was likely to be placed in a group home for one of several reasons: because the judge, probation officer, or state's attorney insisted on a placement outside the home as a precondition for referral to UDIS; because there were severe conflicts between parent and child; because parental instability or deporable physical conditions made continued residence in the home inadvisable; or because negative peer influences and gang activities in the neighborhood indicated a need to relocate the youth.

One of the homes, St. Leonard's House, served the special function of shelter care facility for UDIS youngsters awaiting court hearings after picking up a supplemental petition. Youth were said to be placed at St. Leonard's to "cool off" until suitable residential programs were found.

*Foster Care*. Foster care provided a home-like residence for the youth who could not remain in his own home but did not need the structure of the group home. The supervising agencies recruited and screened all foster parents and additionally trained them to provide individual counseling to their foster children--a function supported by weekly visists of case workers and by frequent family counseling sessions. Most of the placements were temporary, with the youth working toward a return to his natural family or toward independent living.

*Transitional Living Program (TLP)*. TLP was an "emancipation" program; it allowed the adolescent to move away from his family and toward independence without completely discarding adult support. Clients shared an apartment with an advocate who provided counseling for personal, academic, and vocational problems. TLP was run by the Community Advancement Program (CAP). Youth could attend group and individual counseling sessions at the CAP offices where counselors were available for crisis intervention on a 24-hour basis.



Rural Programs

The rural programs provided intensive programming away from the inner city environment where most UDIS referrals originated. Although the programs differed in structure and emphasis, all allowed the Chicago client exposure to a non-urban environment--often, the first exposure. As of July 1976, UDIS had contracts with four rural programs.

Two of the programs, Underway and Darrow Hall, offered time-lined wilderness experiences based on the Outward Bound model. A third, the Work Camp, was an intensive vocational-educational program. Finally, Crossroads was a six-week program set in a combination of rural and city locations.

The two wilderness programs were physically demanding. Youngsters were required to hike, climb, swim, canoe, and learn how to survive in the wild. Each "brigade" included from eight to ten youth, and each program cycle lasted from four to six weeks. While both programs focused on experiential learning, the staff assumed a directive and supportive role, teaching specific wilderness skills and providing counseling on program experiences and problems at home.

The Work Camp, developed specifically for the UDIS client and widely used until its closing in late 1976, required the youth to participate in basic vocational activities such as painting, construction, and food preparation, hoping that he would find work rewarding. The camp also had an alternative school program, preparing some youngsters for return to public schools in Chicago and others for the GED examination. Recreational opportunities included overnight camping, horseback riding, swimming, and gymnastic activities. The typical stay at the Camp ranged from two to four months.

The Crossroads program worked with groups of eight youngsters for a period of six weeks. The program was geared toward rural and city exploration. Clients experienced country life through hiking, camping, and talking with farmers. They re-explored the city through experiences ranging from museum visits to rap sessions with pimps. These experiences were accompanied by formal and informal counseling with emphasis on ways the youth could best survive in his own environment. The program also offered vocational and academic tutoring.

Three of the programs provided follow-up services for their clients for the first few weeks after their return to the community. Although advocacy was not a primary function of any of the programs, a few staff assumed that role informally by attending court hearings and speaking with Case Managers, Probation Officers, and families.

First placements in rural settings were often at the insistence of the judge and not because of the UDIS assessment or the Case Manager's inclination. A first referral might have been made for one of several reasons: seriousness or length of the youth's offense record, conflicts or deleterious conditions in the home, delinquent peers, or gang activities in the neighborhood. Mid-program rural placements, on the other hand, frequently resulted from a supplemental petition or from the failure of the community-based placements to change the attitudes or actions of their clients. In these cases, the rural program became the "least drastic alternative" by default (the remaining alternative being probably commitment to DOC).

Intensive Care

The residential intensive care facilities were designed to deal with the delinquent youth requiring psychiatric or psychological evaluation and treatment. Two types of facilities fell within the intensive care category: hospitals providing psychiatric services, and residential programs using a behavioral approach. UDIS had contracts with three residential programs as of July 1976 and purchased hospital services on an individual basis.

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Hospital services were designed to meet the specific needs of each youngster. The programs were usually supervised by an attending psychiatrist and psychologists and social workers providing therapeutic support. Typically, a youth who was referred for care underwent a psychiatric work-up and evaluation. Individual and group counseling sessions were held on a regular basis. Hospital programs offered daily academic classes as well as tutoring for students who needed additional help. Recreational facilities were sometimes available on the hospital grounds.

The Intensive Care Unit of the Illinois State Psychiatric Institute offered psychological services to its clients. It employed a social work approach rather than the traditional psychiatric model used by the hospitals.

Camelot and Arden Shores were less secure residential facilities offering therapeutic programs based on a behavioral



model. The Arden Shores treatment plan was positive peer culture, emphasizing, in the words of its staff, "the concept of personal responsibility for a student's own behavior and concern for the welfare of his classmates." Formal group counseling sessions were held daily and parental group meetings were held once a week. The program offered daily special education classes on the junior high and high school level as well as an assortment of treatment-oriented recreational activities.

Youth who were conisdered to be "behaviorally dangerous" or who had severe emotional problems were likely to be referred for in-patient psychiatric services. Sometimes a judge would stipulate that a youth with a long and serious offense record be placed in an intensive care facility if he was to be a UDIS referral. Youngsters with behavioral problems who required longer-term care than the rural programs could offer and who were likely to respond well to peer group interactions were referred to the Arden Shores program.

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## APPENDIX C.

### COSTS OF DOC AND UDIS

Attached is a chapter from the UDIS evaluation dealing with the cost of UDIS and DOC. It is included to provide additional bases for assessing the relative merits of institutionalization and deinstitutional alternatives. The principal findings are that UDIS as a program and DOC as a program were equivalent in dollar costs. The at-home services of UDIS were less expensive than the residential ones, and less expensive than institutions. Predictably, UDIS exacted a higher price than institutions in the form of in-program offenses.

The chapter is presented verbatim, as it appeared in Murray, et al. (1978). Since that report was released, it has been determined that UDIS expenditures were overstated by $151,000. Thus the total estimated monthly costs of a program cycle as contained in the chapter are too high by $69. The summary paragraph on page 196 should be changed from

> On a monthly basis, then, DOC was 4.8 percent less expensive than UDIS. Over a typical program cycle, UDIS was 7.0 percent less expensive than DOC, because it held its clients for a shorter time.

to

> On a monthly basis, then, DOC and UDIS were almost identical--UDIS was $3 per month cheaper than DOC. Over a typical program cycle, UDIS was 11.9 percent less expensive than DOC, because it held its clients for a shorter time.

The conclusions in the chapter are not affected by this alteration.



# 11. The Relative Costs of UDIS and DOC

The reductions in delinquent behavior that were produced by UDIS and DOC came with a bill, in tax dollars and in other, nonmonetary costs. This chapter examines the comparative magnitudes of those costs which could be counted.

### COSTS IN TAX DOLLARS

Delinquency is expensive. Apart from the social costs that delinquent behavior may inflict on victims and the community, it uses up a substantial number of tax dollars. Each time an offense committed by a juvenile is reported to the police the expenses begin, regardless of whether the offender is even caught. For any nontrivial report, the minimum cost is the time of a two-person car out of service during the call. If the offense is a major one, followup investigation may be conducted, with all of the detective's costs associated with that investigation. Then, whenever a delinquent is apprehended, a two-person team and a vehicle are taken out of service to transport the youth to the station and transfer him to detention or back home. The Youth Officer, a detective, pursues the matter at the station, trying to call in the parents or guardian if it is an offense that will be referred to court. The paperwork involved in booking and writing up the police report takes the time of both sworn officers and clerical staff, at the precinct and then again at the Youth Division's central office at Police Headquarters.

When the matter is referred to court, attorneys' time is taken up at both the States Attorney's and the Public Defender's offices. If the youth is detained, the cost of keeping him at Audy Home must be added. Probation services are called upon to conduct direct casework with the youth and perhaps sessions with the family, along with completing the collateral paperwork. Finally, there may be all the costs associated with a court hearing.

All of these dollars are spent before the youth is sent to UDIS or DOC. Once there, a whole new set of expenses arise. The support and administrative staff, case workers, service providers, institutional costs, parole personnel--all must be fed into the growing total.

In all, there are three pieces to the total dollar costs of UDIS and DOC: the *direct costs* of the initial program cycle, the *deferred costs* of subsequent institutionalization, and the dollar *costs of processing offenses committed during the program*, in police, court, and detention services. We limit the discussion to the cost component. The postprogram recidivism data in the preceding chapters speak for themselves, and projections of what they might mean in comparative postprogram dollar benefits would be speculative. In this case, we can count current costs much more accurately than we can extrapolate future benefits.

### Direct Costs

For both UDIS and DOC, direct costs are calculated as total operations and administrative expenses divided by the number of client person-days in the program.

*The Numerator.* For UDIS, the numerator of the division consists of the dollar value of the grants expended during fiscal 1976 plus the appropriations from the general fund:

| | |
|---|---|
| Grants expended in FY 1976 | $2,214,722 |
| General fund appropriations, FY 1976 | 151,471 |
| Total | $2,366,193* |

For DOC, the numerator consists of *all* expenditures from appropriated funds--general revenue fund operations, awards and grants, and permanent improvement expenditures

*We wish to acknowledge the assistance of Mr. Maurice Moore of UDIS and of Mr. Marvin Jenkins of DOC in assembling the UDIS cost figures for FY 1976.



of all Illinois juvenile institutions, plus the administrative costs of the central office of the Juvenile Division.* The costs were:

| | |
|---|---|
| Institutions, General Revenue Fund, Operations, FY76 | $16,171,320 |
| Institutions, General Revenue Fund, Permanent Improvements, FY76 | 87,432 |
| Institutions, General Revenue Fund, Awards and Grants, FY76 | 10,531 |
| Juvenile Field Services, Administration, General Revenue Fund, Operations, FY76 | 174,015 |
| Total | $16,443,298 |

*The Denominator.* The denominator of the calculation was the number of days spent "in UDIS" and "in DOC, not on parole" during fiscal 1976. For UDIS, we obtained from the Northwestern UDIS Tracking System a listing of the indates and outdates of all referrals to UDIS.† The days spent in UDIS during fiscal 1976 were determined separately for each referral, then summed. The total for fiscal 1976 was 66,257 person-days (2,178.31 person-months, 181.40 person-years).

For DOC, the weekly population summaries were used to compute client days. The book counts for the weeks in

*This is a considerably more inclusive approach than is generally used. For example, the DOC Fiscal Report for FY 1976 (Petrilli, Fiscal Dept.) associates $16,182,800 with overall costs of institutions, compared to our figure of $16,443,298. The reason for risking an unduly inflated estimate is that our findings tend to contradict the popular belief that community-based corrections are less expensive than institutionalization. Because there are many ways of calculating institutional costs, we have tried to forestall the reply that our findings would have been different if we used another definition. Ours is a high-cost alternative to begin with. Source for the figures is the *Illinois Annual Report, Fiscal Year 1976: July 1, 1975 - June 30, 1976*, prepared by the Comptroller (Michael J. Bakalis), pp. 128-155. The figure for Juvenile Field Services administrative costs reflects operating costs of the St. Charles and Springfield Administrative Offices.

†Updated as of 10/4/77.

fiscal 1976 were summed and multiplied by seven.* The result was 424,522 days (13,956.89 person-months, 1,163.07 person-years).

*Hereby hangs an issue with which the reader should be familiar.

There are two ways of calculating number of days spent in a juvenile institution: "book count" and "head count." Book count includes all boys carried on the books as being consigned to that institution. Thus, it includes boys on temporary furlough (two weeks or less), boys who have been on unauthorized absence for less than 30 days, or boys who are temporarily off the grounds of the institution for a variety of other reasons. Book count does *not* include boys on parole.

The alternative is to use "head count," or "daily population" representing the number of person-days that were actually spent under the roof of a juvenile institution. As discussed subsequently, there is a considerable difference between the "head count" and the "book count." Head count for fiscal 1976 indicates only 273,525 person-days, 35.6 percent fewer than the book count.

From a conceptual standpoint, it makes sense to use book count for several reasons. It conveys a more commonly understood meaning. When it is said that a boy was "in DOC for six months," the sense of the meaning is that he was consigned to an institution for that elapsed period of calendar time, not that he spent 182.5 days on the grounds of an institution, spread over some longer period of calendar time. Also, the size of an institution's staff and its facility costs are relatively insensitive to short-term reductions in population. Finally, using book count is consistent with the approach to calculating UDIS costs, which likewise considered a boy as "in UDIS" on the days he received no services.

So we use book count days instead of head count days as the denominator in calculating monthly direct costs--the "unit cost." But because the cost of institutionalization is so often used as a political football, let this be made explicit: *The method of calculating unit cost has no effect on the estimated costs of a program cycle.* Over a large sample of boys, using book count or head count produces the same answer.

The reason is that the value of the parameters for "days in program" and "cost per day" are both functions of the same definition of what constitutes a DOC day. If book count produces the cost-per-day, then in figuring the costs of a program cycle it is necessary to count days away from the institution as costing money. If head count is used, those days are not counted at all. For example, consider the calculation of expected direct costs for a boy who is paroled after six months in DOC. Using book count, the calculation is the FY 76 budget divided by total days that youngsters were officially consigne to DOC institutions, multiplied by the number of days in six months:



For presentation purposes, we use person-month as the unit. The costs per program-cycle month per youth for the two programs in fiscal 1976 were:

| | | |
|---|---|---|
| Direct costs per month of the program cycle: | UDIS = $1,086 | DOC = $1,178 |

## Deferred Costs

Deferred costs accrue because more than a quarter of UDIS referrals were eventually committed to DOC and more than a quarter of DOC parolees were returned again to the institution. If the question is, "How much will it eventually cost if a boy is sent to UDIS instead of DOC?", the costs of the initial term in each program must be augmented by the costs of the program failures.

Deferred costs for UDIS are based on that fraction of a month that a UDIS referral can eventually be expected to spend in DOC for every month he was in UDIS. For DOC,

(footnote continued)

$16,443,298 ÷ 424,522 × 182.5 = $7,068.90. Using head count, the calculation is the FY 76 budget divided by total days actually spent under the roofs of DOC institutions, multiplied by the expected number of days in six months that a boy actually spent in an institution: $16,443,298 ÷ 273,525 × 117.6 = $7,069.67. The 77 cent difference is a result of rounding error. *The mathematical effect of switching from book count to head count is to increase the unit cost by exactly the same proportion that the day count decreases.*

The very large "annual costs" that are sometimes cited for DOC--the DOC annual report for 1976 gives a figure of $21,988 for FY 1976--are produced by using as a denominator the average daily population in the institutions. The figure has nothing to do with the cost of a year's stay in DOC for the average boy. Rather, it specifies the cost of spending 365 days on the grounds of an institution, which in turn implies consignment to an institution for about 18.6 months--more than a year and a half. Because this is seldom made explicit, most consumers of these annual cost statistics assume that a figure of $21,988 does indeed signify the cost of committing a boy for a year. They are victims of a kind of mental sleight of hand--the calendar definition of "annual" that they naturally use for interpreting the number is subtly different from the one that was used in calculating it. The main point to remember is that it would indeed have cost more than $20,000 in direct costs to have kept a boy in an institution every day of fiscal 1976--but *the program that produced the recidivism results which we have been discussing in this report had annual direct costs of about $14,000, no matter which definition of unit cost is used.*

deferred costs are based on the months spent back in DOC after a failed first parole, compared to the months spent in DOC during the initial stay.

The denominator for the UDIS sample was the mean number of days spent in UDIS. The denominator for the DOC sample was the mean number of days spent in DOC before the first parole. In both cases, the means were based on all subjects who had exited the program prior to 31 December 1976. The numerator for UDIS was mean number of days spent in DOC through 31 December 1976, based on the records of boys who had exited UDIS at least one year prior to the end of data collection. For DOC, a parallel figure was computed using mean number of days spent back in DOC after revocation of the first parole. These proportions are both understated. Neither sample had accumulated its final total of DOC days.* The proportions can be expected to reflect *relative* deferred costs for UDIS and DOC. The figures used in the calculation of deferred costs are shown in Table 11.1.

Table 11.1 Basis for the Calculation of Deferred Costs

| | UDIS | DOC |
|---|---|---|
| Mean number of days, first time in program | 243.9 | 274.7 |
| n | 130 | 95 |
| Mean number of subsequent days in DOC, for boys first released at least a year before the end of observation | 72.8 | 62.1 |
| n | 46 | 45 |
| Proportion of subsequent days to initial days | .298 | .226 |
| Monthly direct cost of DOC | $1,178.15 | $1,178.15 |

* A more natural procedure would have been to use the mean number of subsequent days in DOC based on the sample of boys who had returned to DOC (or who had been sent there from UDIS) and had been released again. But we found that this was eliminating the long-term returnees who had not been released by the end of the observation period even though they had already accumulated more days "back in" than the average of the released-twice sample. Empirically, the procedure we substituted turned out to work to the advantage of UDIS.



The algorithm is proportion of subsequent days to initial days multiplied by the monthly program cost of DOC.

On the basis of the above parameters, the estimated monthly deferred costs for UDIS and DOC was as follow:

| Monthly deferred costs: | UDIS = $351 | DOC = $266 |
|---|---|---|

## External Dollar Costs

External costs are generated by offenses committed while the youth is in the program. We will return to the nonmonetary considerations associated with these offenses in another context. In dollar terms, we attempted to estimate costs of police, court, and detention services required to process the offenses that occurred.

The details of the cost calculations are given in the notes to Table 11.2. As a close reading of that table will make apparent, a great deal of estimation had to go into the exercise. Officials of both the Chicago Police Department and the Cook County Juvenile Court were able, however, to provide estimates based on their experiences, and we present the data with confidence that the costs do usefully cut down the range of guesses.

The costs increase, of course, when the offense resulted in referral to court and are most expensive when referral to court is accompanied by detention at Audy Home.

The sample of UDIS referrals had accumulated 121 person-years in the program by the cutoff of data collection on 31 December 1976. During that time they had committed a total of 361 offenses. The DOC sample had spent 147 person-years "in" institutions--remembering that roughly a third of the time was probably spent on authorized or unauthorized absence--and had committed 57 offenses during

## Table 11.2 The Basis of the Cost Estimates for Apprehended Offenses[1]

| | Hourly wage[2] | Proportion, for cases reaching that phase | Typical time in hours From | Typical time in hours To | Low Estimate | High Estimate | Aggregates | | Best Estimate |
|---|---|---|---|---|---|---|---|---|---|
| **I. COSTS AT THE POLICE STATION[3]** | | | | | | | | | |
| Arresting Officers (2) | 9.88 | 100% | .75 | 2.00 | 7.41 | 19.76 | | | |
| Youth Officer | 10.72 | 100% | .75 | 2.50 | 8.04 | 26.80 | | | |
| Clerical Support | 4.47 | 100% | .25 | .50 | 1.12 | 2.24 | | | |
| Police Investigator | 10.72 | 20% | 1.50 | 5.00 | 3.22 | 10.72 | | | |
| TOTAL DIRECT | | | | | 19.79 | 59.52 | | | |
| General Administrative[4] (8.3% of Total Direct) | | | | | 1.64 | 4.94 | | | |
| Non-personnel costs[5] (6.8% of Total Direct and Administrative) | | | | | 1.46 | 4.38 | | | |
| TOTAL DIRECT and INDIRECT | | | | | | | 22.89 | 68.84 | |
| **II. COSTS AT COURT[6]** | | | | | | | | | |
| Judge | 22.83 | 75% | .33 | 3.00 | 5.65 | 51.37 | | | |
| Probation Officer | 11 02 | 100% | 4.00 | 10.00 | 44.08 | 110.20 | | | |
| State's Attorney | 10.68 | 75% | .50 | 5.00 | 4.01 | 40.05 | | | |
| Public Defender | 12.05 | 75% | .50 | 5.00 | 4.52 | 45.19 | | | |
| Sheriff, Court Clerk | 6.18 | 75% | .25 | 2.00 | 1.16 | 9.27 | | | |
| Arresting Officer (testimony) | 9.88 | 20% | 2.00 | 4.00 | 3.95 | 7.90 | | | |
| Clerical | 5.13 | 100% | 3.00 | 8.00 | 15.39 | 41.04 | | | |
| TOTAL DIRECT | | | | | 78.76 | 305.02 | | | |
| General Administrative[7] (22.1% of Total Direct) | | | | | 17.41 | 67.41 | | | |
| Non-personnel costs[8] (5.6% of Total Direct and Administrative) | | | | | 5.39 | 20.86 | | | |
| TOTAL DIRECT and INDIRECT | | | | | | | 101.56 | 393.29 | |
| **III. COSTS OF DETENTION[9]** | | | | | | | | | |
| Daily cost (44.85)[10] X Mean no. of days (9.0) | | 100% | NA | NA | 403.65 | 403.65 | | | |
| Escort to and from courtroom (sheriff) | 6.18 | 67% | 1.00 | 3.00 | 4.14 | 12.42 | | | |
| TOTAL | | | | | | | 407.79 | 416.07 | |
| TOTAL COSTS. COMMUNITY ADJUSTMENT | | | | | | | 22.89 | 68.84 | 45.87 |
| TOTAL COSTS: REFERRAL TO COURT WITHOUT DETENTION | | | | | | | 124.45 | 462.13 | 293.29 |
| TOTAL COSTS: REFERRAL TO COURT WITH DETENTION | | | | | | | 532.24 | 878.20 | 705.22 |

Notes:

1. Budget Sources George W. Dunne, President, Cook County Board of Commissioners, The Annual Appropriation Bill for the Fiscal Year 1977. 1976 City of Chicago Budget Recommendations: Planned Allocation of Appropriations by Program Element. Appropriations for FY 1976 were used in both cases.
2. All wages are based on median for that job level, assuming a 146-hour month.
3. Description of police procedure was provided by staff of the Youth Division, Chicago Department of Police.
4. The General Administrative loading represents the summed personnel budgets of support functions divided by total police budget. The total police budget was 303,698,697 The following budgets were summed for support functions.

| | | | |
|---|---|---|---|
| Administration-Departmental | 423,451 | Police Document Services | 526,513 |
| Research and Development | 1,163,085 | Administrative-Operational Services | 505,956 |
| Administration-Administrative | 70 700 | Administrative-Patrol Division | 1,558,132 |
| Finance | 568,285 | Administrative-Communication | 59,723 |
| Personnel | 1,273,879 | Administrative-Community Services | 88,724 |
| Training | 1,755,265 | Reproduction and Graphic Arts | 307,648 |
| Data Systems | 1,841,334 | Administrative-Inspectional Services | 184,655 |
| Property Management | 1,078,798 | Internal Affairs | 2,370,370 |
| Records | 159 528 | Inspections | 623,852 |
| Records Inquiry | 1,925,848 | Administrative-Investigative Services | 83,994 |
| Administrative-General Support | 29,380 | Administrative-Criminal Investigation | 1,845,724 |
| Equipment and Supply | 165,533 | Inspection of Entertainment Exhibitions | 148,814 |
| Electronics and Motor Maintenance | 5 592,126 | TOTAL | 25,251,317 |

5. This loading represents non-personnel costs divided by the total police budget. Budgets of the following categories were summed:

| | |
|---|---|
| Contractual services | 7,335,200 |
| Travel | 25,000 |
| Commodities | 6,933,000 |
| Equipment | 5 852,500 |
| Permanent Improvements and Land | 328,000 |
| Specific Items and Contingencies | 110 000 |
| TOTAL | 20,583,700 |

6. Description of Juvenile Court Procedure was provided by staff of the Juvenile Court and In-house study of use of time by probation officers, and Douglas Thomson "Action Taken Regarding 100 Children Referred to Cook County Juvenile Court on Original Delinquency Petitions in January 1973," Cook County Juvenile Court, 1974.
7. This loading represents summed personnel budgets for support functions divided by the total juvenile court budget. Support functions as defined included the following

| | |
|---|---|
| General Administration | 1 249,778 |
| Clinical Services | 324,120 |
| Juvenile Court Committee Staff | 43,320 |
| TOTAL | 1 617,218 |

The total juvenile court budget was 7,328,429.

8. This loading represents nonpersonnel costs divided by the total juvenile court budget. Budgets of the following categories were summed:

| | |
|---|---|
| Impersonal Services | 337,051 |
| Supplies, Materials, and Parts | 20,000 |
| Operation and Maintenance | 37,300 |
| Capital Outlay | 14 400 |
| TOTAL | 408,751 |

9. Source was the FY75 calculation of daily costs and average stay at Audy, taken from the Audy Home's 1976 report on expenditures.
10. Data on daily costs were valid for fiscal 1975. In FY 1974-75, the cost had increased by 47.4 percent, from $24.30/day to $35 88/day. We assume a 25 percent increase from FY75 to FY76.



that time. Table 11.3 shows a breakdown of the offenses by disposition. Rates are expressed in number of occurrences per hundred person-years in the program.

## Table 11.3 Dispositions of Offenses Committed During the Program

| | Number of events per 100 person-years in the program | |
|---|---|---|
| | UDIS (n = 189, person-years in UDIS = 121.05) | DOC (n = 156, person-years in DOC = 147.47) |
| Arrests, no referral to court | 122.3 | 9.5 |
| Arrests with referral to court | 49.6 | .7 |
| Arrests with referral and detention | 126.4 | 28.5 |
| Total number of police contacts | 298.3 | 38.7 |

Not surprisingly, UDIS with its community-based orientation was less effective than institutionalization in incapacitating the youth during the program. UDIS youth were picked up by the police at a rate 7.7 times higher than that of DOC.

Applying the cost figures in Table 11.2 to the disposition figures in Table 11.3, estimated monthly external costs for UDIS and DOC were:*

| | | UDIS | DOC |
|---|---|---|---|
| Monthly estimated costs: | Low estimate | $ 63.49 | $ 12.87 |
| | High estimate | 118.52 | 21.63 |
| | Best estimate | 91.01 | 17.25 |

*Unrounded data were used for the calculations. The "best estimate" is simply the mean of the low and high estimates. See Table 11.2 for details.

Summing across the direct costs, deferred costs, and the best estimate of the external costs, the estimated monthly cost of UDIS and DOC was:

| | | |
|---|---|---|
| **Total estimated monthly costs:** | **UDIS** = $1,528 | **DOC** = $1,462 |

If we adjust these figures for the average length of stay in the two programs, the *estimated costs for the complete intervention cycle* were as shown in Table 11.4.*

Table 11.4 Costs of a Typical Intervention Cycle

| | UDIS | DOC | Dollar Costs UDIS | Dollar Costs DOC |
|---|---|---|---|---|
| • mean initial stay (in months) | 8.02 | 9.03 | $ 8,711.73 | $10,638.69 |
| • expected subsequent institutionalization (in months) | 1.77 | 1.38 | 2,085.33 | 1,625.85 |
| • expected number of police contacts during an average initial term | 1.89 | .29 | 86.69 | 13.30 |
| • expected number of court referrals during an average initial term | 1.12 | .21 | 277.12 | 51.96 |
| • expected number of detentions during an average initial term | .80 | .21 | 329.54 | 86.51 |
| TOTAL COSTS FOR A TYPICAL INTERVENTION CYCLE | | | $11,490.41 | $12,416.31 |

On a monthly basis, then, DOC was 4.8 percent less expensive than UDIS. Over a typical program cycle, UDIS was 7.0 percent less expensive than DOC, because it held its clients for a shorter time.

The conclusion we draw from the exercise is that *there is little basis for choice between UDIS and DOC if the criterion is dollar costs to the taxpayer.* The proposition that community-based corrections is substantially less expensive than institutionalization was not borne out by UDIS experience.†

*Table 11.4 is calculated from the unrounded figures for direct costs (p. 181), deferred costs (p. 183), and the mean of the low and high estimates of external costs for each type of cost (I, II, and III in Table 11.2, p. 184).

†This finding is consistent with other examinations of the costs of deinstitutionalization. The most authoritative example is Lerman's analysis of the California Community Treatment Project (Lerman, 1975).



## THE NONMONETARY COSTS

Dollar costs are probably the least important ones. It happens, as in evaluations of social programs everywhere, that they are the ones we know how to measure most accurately. But there are some underlying "actual" costs of UDIS and DOC, omitted from the dollar cost comparison, that are in some respects much closer to the heart of decisions about the future of policy toward juvenile offenders. Three factors are of special importance.

First, *the calculation of dollar costs has ignored the victim.* One of the most inflammatory issues surrounding deinstitutionalization of chronic delinquents is offenses committed while in the program. Institutions have been widely accepted as "safe" places to keep delinquents; community-based services frequently have not. And the passions aroused by the perceived lack of safety have been fed primarily by concern over the human costs of being a victim, not by the costs of processing them through the arrest or the court appearance.

In truth, the institutionalized youth has not been as thoroughly removed from the streets as most people assume. In 1976, for example, a committed delinquent (*not* paroled) in Illinois was on the streets for more than a third of the time--35.6 percent--that he was officially consigned to an institution: 20.9 percent of the DOC person-weeks for that year was spent on authorized absence and 14.7 percent was spent as runaways. DOC as well as UDIS has been vulnerable to inprogram costs in the form of offenses. But DOC does start with an intrinsic advantage over a program like UDIS. The question is whether the advantage is a significant one.

In the calculation of external dollar costs, it was revealed that the inprogram apprehensions of UDIS youth occurred at a rate 7.7 times that of DOC. Table 11.5 on the following page presents some additional information about the nature of those offenses.

We do not intend to invoke images of widespread mayhem, but, as the table indicates, the incidence rates are of such magnitude that they cannot be disregarded. In addition to the annual rates of the general categories of crime as shown in Table 11.5, we may put the issue in terms of some specific crimes during the average stay in the program. For every eight boys who stayed in UDIS for the typical eight-month period, one was apprehended during that time for an

Table 11.5 Types of Offenses Committed During the Program

| | | Violence-Related | Theft | Damage | Other Non-status Offenses | Status Offenses | Total |
|---|---|---|---|---|---|---|---|
| UDIS | Per 100 person-years | 43.8 | 109.9 | 22.3 | 106.6 | 15.7 | 298.3 |
| | Proportion of total | 14.7% | 36.8% | 7.5% | 35.7% | 5.3% | 100.0% |
| | (n=189, person-years in UDIS = 121.05) | | | | | | |
| DOC | Per 100 person-years | 8.1 | 14.9 | 1.4 | 14.2 | 0 | 38.6 |
| | Proportion of total | 21.1% | 38.6% | 3.5% | 36.8% | 0.0% | 100.0% |
| | (n = 156, person-years in DOC = 147.47) | | | | | | |

armed or strong-armed robbery. One of every eight was arrested for assault or battery. For every four, one was arrested for burglary. For every 48, one was arrested for homicide. These crimes are not status offenses or pranks; they involve serious damage to victims. The costs are not easily quantified, but they are very real.

The implications of these differences are augmented by the second omission in the dollar cost analysis: *the figures on inprogram offenses presumably represent only a fraction of the real level of offensive activity.* Police clear only a small proportion of offenses. Exactly how small a proportion for this population of offenders is unknown. One major study has suggested it to be as small as three percent (Williams and Gold, 1972), but that report dealt with the general population of youth, not the special class in this study.

We may reach an approximation by combining police clearance rates with rates of reporting. In the five largest U.S. cities (Chicago data are not broken out), the percentage of arrests for reported burglaries was 18.3 in 1972 (LEAA/NCJISS, 1975(a), Table 4.17, pp. 353-354). The percentage of arrests for robbery was 31.7, and 16.9 for auto theft. In Chicago, also in 1972, the *reporting* rates of actual victimizations were found to be 53 percent for burglary, 52 percent for robbery, and 78 percent for auto



theft (LEAA/NCJISS, 1975(b), Table 6, p. 61.* By extrapolation, it can be estimated that apprehensions of total offenses were in the region of 10 percent for burglaries, 16 percent for robbery, and 13 percent for auto theft. As a rough estimate, then, perhaps 10 to 20 percent of the offenses being committed by the delinquents in this study were being apprehended. The implication is that UDIS youth were committing on the order of 15 to 30 offenses per person-year while in the program compared to 2 to 4 for DOC. It is not a trivial difference.

Finally, *the calculation of costs has ignored the interests of the youth.* The enumeration of inprogram offenses omitted the unknown number of assaults and other offenses committed *inside* the institution. More generally, institutions have been assessed as being destructive places, even with the best of facilities and staff. The literature on the subject is large, though generally imprecise as to the real extent of damage, psychological or physical, to populations of committed delinquents. Our data do not address this issue except insofar as they serve to disconfirm the proposition that institutions increase the likelihood of delinquent behavior. In making policy decisions, the subjective weight attached to preventing inprogram offenses must be balanced against an equally subjective weight attached to keeping youngsters out of institutions.

## SUMMARY

UDIS and DOC were nearly identical in dollar costs per month of a program cycle. Inprogram nonmonetary costs to the community were much greater for UDIS than for DOC. The raw magnitude of the difference in nonmonetary costs is uncertain, because of our ignorance about the ratio of actual offenses to apprehensions. The nonmonetary costs to the youth were presumably greater for DOC than for UDIS.

We have not attempted to construct a cost-benefit equation. *Given that DOC was equivalent to UDIS in dollar costs, conspicuously less expensive in terms of inprogram offenses, and at least as effective in reducing recidivism,*

*Insofar as the primary population of victims--poor, minority, in the inner-city--are also the least likely to report victimizations to the police, our estimates of apprehension rates based on reporting characteristics of the overall population are likely to be inflated.

*the only factor that could swing the cost-benefit ratio in UDIS's favor is the imponderable personal cost of institutionalization to the youth.* If the issue is immediate costs and benefits to the public, a comparison of cost-benefit ratios would have to favor DOC over UDIS.

These results do not necessarily extend to the question of expansion of services. New institutions require major capital expenditures. So also do certain types of UDIS services (intensive care and other residential services); but others, notably advocacy and counseling services, do not. Especially if UDIS were to concentrate its resources on Level I services, *it can be assumed that expansion of UDIS would require lower front-end expenditures than expansion of DOC.*



## APPENDIX D.

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