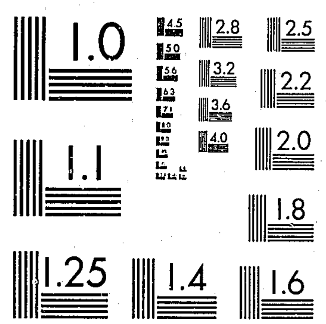

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Days in Court

Patterns of Juvenile Court Response and Their Impact on Arrest Rates

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Preface

This study may be said to have begun with a conversation with John Greacen, then Director of the National Institute of Juvenile Justice and Delinquency Prevention (NIJJDP), in the fall of 1975. We were discussing the distressing state of knowledge about what works in dealing with juvenile offenders. I took the position that the only thing we knew for sure was that incarceration only makes matters worse. Greacen's response was that we didn't even know that--to which I replied that surely the literature had pretty well pinned down at least that one, basic conclusion. I could not cite chapter and verse, but I had a clear image of a large, repetitive literature debunking institutionalization.

Within a few months my colleagues and I had an opportunity to look into the issues for ourselves. The American Institutes for Research (AIR) was awarded a contract to evaluate the Unified Delinquency Intervention Services (UDIS), an experimental program operating in Chicago. The purpose of UDIS was to provide an alternative to incarceration for Cook County's chronic delinquents. Our job was to compare the effects of UDIS with the effects of the traditional institution.

At the outset, we determined to use recidivism as the central measure of impact; for, when we did review the literature on juvenile corrections, we had found a much more scattered and inconclusive body of evidence than we had expected. But we nonetheless went into the study fully prepared to discover that (a) not much effect on recidivism would be produced by either program; (b) UDIS would probably do better than the institutions; and (c) most of our analytic efforts would have to be devoted to making sure that selection artifacts were not the real source of the difference.

We turned out to be half right. When we used "cessation" as the criterion of success--specifically, whether the delinquents were arrested in the year following release--we found that most of the members of both samples failed, with the UDIS group failing slightly but insignificantly less often. Sixty-five percent of the UDIS sample was rearrested within a year after release, compared to 69 percent of the sample sent to institutions.

But then we used a second, less common measure of success, comparing the rate of arrests following release with the rate of arrests in the year prior to intervention. The results were dramatic. Arrests had dropped by proportions ranging from half to more than two-thirds of the pre-intervention rate, for both samples.

We called this reduction the "suppression effect," and wrote up our results in the final report (Murray, Thomson, & Israel, 1978). But even before the report was released, we had obtained support from NIJJDP to extend the study. Finding the suppression effect had been wholly unexpected, and much more needed to be done to understand it.

The results of the follow-on analysis expanded our understanding considerably. The magnitude of the suppression effect for the more restrictive interventions proved to be robust. Whether produced by traditional institutions or out-of-town residential programs conducted by UDIS, the suppression effect could not be attenuated by more than a few percentage points, even accepting some improbable rival assumptions. But the suppression effects for the lesser interventions--supervision, probation, and the at-home UDIS placements--were more sensitive. Applying the same analytic procedures for delinquents of comparable age and with comparable prior arrests, the analyses repeatedly revealed a raw suppression effect for those interventions that was smaller than for the out-of-town residential programs, and one that could be substantially attenuated or even wiped out altogether when tests for artifact were introduced.

The results were given to LEAA with little interpretive embroidery (Murray & Cox, 1979a). We then combined material from the original evaluation with the follow-on work, and wrote a book (Murray & Cox, 1979b), which tentatively identified deterrence as the most parsimonious explanation of our findings. But even then, we limited the interpretive material to a few pages in the last chapter. Put bluntly, we wanted most of all to get people to examine the phenomenon of the suppression effect itself, rather than try to sell a theoretical explanation for it.

But deterrence did have a clear edge over the other candidate explanations. Further, the juvenile corrections literature (including our work) persistently pussy-footed its way around the potential role of deterrence and punishment.

Further yet, we had before us a large, as yet unanalyzed, body of data tracking the arrest-by-arrest police history and appearance-by-appearance court history of more than 1,500 randomly selected Chicago delinquents.

Hence the study that follows. This time, we pursue the effects of court behavior before the delinquent reaches the stage of institutionalization. This time, we bring to the analysis a set of expectations explicitly based on the logic of deterrence. In doing so, we are not proselytizing for a single, simple explanation of delinquency and why it stops or accelerates--we explain only a small portion of the variance. Rather, we apply the assumptions of deterrence in much the same spirit that economists apply assumptions about a perfect market of rational profit-maximizers. Both sets of assumptions simplify their respective worlds, and thereby provide leverage for understanding mechanisms and dynamics.

We begin in Section I with a description of the sample, the study design, and the dependent variable. Section II sets out the backdrop to the study: our view of what we call the "elite wisdom" on juvenile corrections, and a review of the logic of deterrence against which the empirical findings are assessed. Section III is devoted to a discussion of arrest and response up through the first appearance in court. Section IV discusses delinquent response to alternative patterns of court behavior. Section V pulls together the findings and offers some interpretive observations about their implications.

* * *

This marks the fourth time that acknowledgments have been written for the various aspects of our research on Chicago delinquents. To the friends and colleagues who helped with the data collection and the earlier analyses, our previous thanks still hold. A few special mentions about the work for this study are in order.

The study exists at all because of the imagination and determination of Cindy B. Israel. When we set about collecting the data from the court (for other uses), we had planned to gather only the most basic facts about court history: the dates when the subjects were put on supervison and/or probation. Cindy Israel decided that this wasn't good enough; that instead we should reconstruct the entire court history, petition by petition, appearance by appearance. Shortly thereafter, we found what a daunting task that would be, akin to unscrambling, then reassembling, several hundred jigsaw puzzles that had been thrown into the same box. She did it anyway, with precision. It is the study's loss that events took her to new endeavors before we were able to tap the rich data base that she assembled.

During the analysis and the writing of the report, Paul Fingerman and Kristina Peterson reviewed drafts, providing invaluable and occasionally pungent commentary on technical and stylistic lapses. Pamela Swain fended off distractions and suppressed her exasperation at the delays. Joan Flood and Mary Martin deciphered and typed the successive drafts, and gave up asking which would be the last. Finally, Tony Cox, my coauthor from *Beyond Probation* days, who, like Cindy, has gone on to bigger things, had to watch from Cambridge while I got the fun of doing the analysis that he originally planned.

Charles A. Murray

Section I
The Study

Readers may obtain a full discussion of the backdrop to this study in one of the antecedent pieces (Murray, Thomson, & Israel, 1978; Murray & Cox, 1979a; Murray & Cox, 1979b). Below, we provide the essentials about the sample and the design for the current work.

THE SAMPLE

Our sample consists of 1,457 boys who were born in ✓ Chicago during 1960 and who were arrested at least once as juveniles by the Chicago Police Department. They were selected randomly from among all members of the 1960 Chicago birth cohort who were ever arrested.

Arrest Records

The average member of the sample experienced his first arrest at the age of 14 years. The mean number of arrests as juveniles was 3.4. The most common types of offense were some form of theft (29.5%). Violent offenses (armed or strong-arm robbery, homicide, assault and battery, rape or other sexual assault) accounted for 16.0 percent. Criminal damage, trespass, and possession offenses (mostly of stolen goods or firearms) comprised 22.6 percent. Miscellaneous other offenses (primarily disorderly conduct) were 23.3 percent of the total. Status offenses comprised 8.7 percent of the total.

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Court Records

Most boys who were arrested never reached court--the 309 who did reach court represent only 21.2 percent of the 1,457 in the sample. Those 309 appeared in court an average of 6.6 times. As a result of these appearances,

> 60 youths were sent to court but were not put under any restrictions;
>
> 176 youths were sent to court and eventually put on supervision;
>
> 104 youths were were sent to court and eventually put on probation; and
>
> 45 youths were sent to court and eventually sent to either the Unified Delinquency Intervention Services or the Illinois Department of Corrections (Juvenile Division), the most severe sanctions.

The sum of the above exceeds 309 because of dual or triple sanctions in 62 cases. The records of a member of this sample were followed throughout their juvenile careers, but not into the adult system.

DATA COLLECTION

The data analyzed in this study were collected from the Cook County Juvenile Court and the Chicago Police Department.

At the Police Department, we obtained the date and the nature of each arrest. Categorization of arrests is shown in Exhibit 1 on the following page. The codes for "seriousness" (left-hand side) follow Sellin and Wolfgang (1964).

JCCO
CODING
POLICE CONTACT
DATA

1 – 4	AIR ID: pick up four digit number from the roster of names
5 – 10	POLICE FILE NO: pick up six digit number from the Juvenile Record Summary card
11 – 14	VICTIMS: 11 number of victims who received only minor injuries 12 number of victims treated by medical personnel and discharged 13 number of victims hospitalized 14 number of victims killed
15 – 16	RAPE: (if one victim and firearm, score "4", if two victims and firearm, score "5" 15 number of victims forced to engage in a sexual act 16 number of rape victims against whom the offender used a weapon for intimidation
17 – 18	INTIMIDATION: (Do NOT score for rape; if NO firearm, score "1"; if firearm, score "2" 17 offender used physical or verbal intimidation only 18 offender used weapon to intimidate victim.
19	PREMISES: total number of premises forcibly entered by offender during event
20	CARS: number of motor vehicles stolen (recovered and undamaged) by offender during event
21	VALUE (S): monetary value of total property stolen and/or damaged by offender during event. Do NOT score cost of motor vehicle if it is recovered and undamaged. 1 = under 10 2 = 10 – 250 3 = 251 – 2000 4 = 2001 – 9000 5 = 9001 – 30,000 6 = 30,001 – 80,000 7 = over 80,000
22	NATURE OF OFFENSE: 1 = Attempted 2 = Aggravated 3 = More than one charge
23 – 24	OFFENSE TYPE: see list opposite
25	DISPOSITION: 1 = adjusted at station (community adjustment) 2 = referred to court 3 = detained (Audy Home)
26 – 28	DATE: enter month (2 digits in one column), day (2 digits in one column), and the last digit of the year
29 – 30	SEQUENCE: number of events from earliest offense to most recent offense

Exhibit 1

NOTE: An attempt is coded the same as an actual offense (e.g., "attempted armed robbery" and "armed robbery" are both entered as 03). The single exception to this rule is for codes 15 and 16, relating to murder.

ROBBERY AND THEFT	01	Unarmed robbery
	02	Strong-arm robbery
	03	Armed robbery
	04	Other robbery
	05	Purse-snatching
	06	Shoplifting
	07	Auto theft
	08	Larceny
	09	Burglary
	10	Other theft
VIOLENCE-RELATED OFFENSES	11	Intimidation or extortion
	12	Assault
	13	Battery (or assault and battery)
	14	Involuntary manslaughter
	15	Attempted murder
	16	Murder (specify type in margin)
	17	Rape
	18	Deviate sexual assault
	19	Deviate sexual conduct
	20	Contributing to sexual delinquency of a minor
	21	Other sex-related offenses
	22	Battery with robbery or theft
	23	Sexual assault
	24	Other violent acts against persons
	25	Abduction
	26	Kidnapping
"POSSESSION" OFFENSES	31	Possession of heroin
	32	Possession of marijuana
	33	Possession of other controlled substances
	34	Possession of stolen property or receiver of stolen property
	35	Unlawful possession of a weapon
	36	Other possession offense
	37	Sale or delivery of controlled substance
CRIMINAL DAMAGE AND TRESPASS	41	Criminal damage to property or land
	42	Criminal trespass to property or land
	43	Criminal trespass to vehicle
	44	Vandalism
	45	Arson
	46	Other damage offense
	47	Other trespass offense
A MULTITUDE OF SINS	51	Disorderly conduct
	52	Loitering
	53	False fire alarm
	54	Gambling
	55	Riding in a stolen car without knowledge
	56	Contributing to delinquency of a minor
	57	Recruiting gang members
	58	Resisting or obstructing peace officers
	59	Unlawful use of a weapon
	60	Violation of parole or probation
	61	Writ or Juvenile Court warrant
	62	Vice
	63	Prostitution
	64	Escapee
	65	Forgery
	66	Deceptive practice
	67	Paternity
STATUS OFFENSE	71	Underage possession or use of alcohol
	72	Incorrigible or ungovernable
	73	Runaway
	74	Truancy
	75	Curfew violation
	76	Driving underage
	77	Other status offense
TRAFFIC VIOLATIONS	81	Driving without a license
	82	Moving traffic violation
	83	Other traffic violation
CATCH-ALL	91	Other offense not covered above (specify in margin of coding sheet)

At the Cook County Juvenile Court, we obtained the date and disposition of each court appearance. The data collection instrument is shown in Exhibit 2. This seemingly straightforward data collection task turned out to involve an elaborate cross-referencing procedure that linked the police records, hand-written court logs, and the court files for each youth. In a minority of cases, we were unable to reconcile these dual sources: the log would show an entry for which no record could be found in the file folder, or the police records would indicate a referral to court that was not matched by the court records. This produced 58 cases that were incomplete and that consequently were deleted from the sample. The 1,457 cases we analyze in the following chapters represent the net number of complete cases.

THE DEPENDENT VARIABLE: TNA

Recidivism has no single, accepted, operational definition. It has a core meaning--to persist in delinquent or criminal activity--but operationalizing that notion has taken many forms. Three of the most basic types are: binary measures of whether *any* recidivism occurs (cessation), measures of numbers of offenses over a period of time (level of activity), and measures of the speed with which a new offense occurs (velocity). In this study, we use the last of the alternatives, operationalized as *time-to-next-arrest* (TNA), expressed in years and fractions of years.

Interpreting TNA is straightforward: bigger is better. The larger the value of TNA, the longer it took a delinquent to be rearrested. But TNA is not the most obvious of criteria. It does depend on a rearrest--a failure--to be measured, and some elaboration of the uses of TNA and its alternative measures is appropriate.

RESTRICTED INFORMATION / FOR JCCO STAFF USE ONLY — Exhibit 2

Juvenile Court History Coding Sheet

Coder ______________ Date __________ Petition ___ of ___

AIR ID	______
Petition number	___–______
Date petition filed in court	___ ___ ___
• First disposition (code)	____ ____________
Date of first disposition	____ – ____ – ____
• Subsequent disposition (code)	____ ____________
Date of subsequent disposition	____ – ____ – ____
• Subsequent disposition (code)	____ ____________
Date of subsequent disposition	____ – ____ – ____
• Subsequent disposition (code)	____ ____________
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The Cessation Criterion

Historically, research into the effectiveness of juvenile corrections has used cessation as the measure of success. If the delinquent is not rearrested (or reconvicted, or reinstitutionalized, whatever the choice of a "failure" indicator may be) within a given time period, he is considered a success; otherwise, a failure.

We earlier rejected cessation as a useful measure for chronic urban delinquents. As we put it then:

> More generally, it may be argued that the before-and-after comparison is always the appropriate focus of attention for a population consisting of chronic delinquents. We take this position for three reasons. First, the cessation criterion cannot capture certain major changes in behavior. Delinquents who were arrested eight times in the year before intervention and twice in the year after are importantly "different" in the two time periods. But without the preintervention benchmark, they will be classified in the same category with every other delinquent who was arrested twice in the year after intervention.
>
> Second, the cessation criterion is too ambitious. Urban delinquents who reach correctional programs are drawn disproportionately from the inner city. When they leave the correctional program, these youths return to environments that exhibit all of the socioeconomic correlates of crime: high unemployment, low incomes, one-parent or no-parent homes, poor 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 any, must compete with countervailing factors. Some recidivism should not deflect attention from the gains which may have been made.
>
> Finally, the cessation criterion is vulnerable to false readings because of the extra attention that newly released delinquents often face. They return to their communities as highly visible individuals--consequently, neighbors are likely to think of them first when a burglary or a purse-snatching occurs. So are the police. Delinquents who are newly released

from an institution have a prominence as suspects that raises the probability that they will be picked up by the police (Murray & Cox, 1979b: 32-33).

These same reasons apply with only slight qualification to all urban delinquents, not just chronic ones. Since writing that appraisal, however, we have come to the stronger conclusion that *cessation is always unusable as a criterion when working with official arrest data*, for a sample that includes nonchronic delinquents. The reason: the cessation measure is extremely sensitive to error because of the detection problem.

The detection problem lies in the large proportion of offenses that do not result in arrest. Its exact value is unknown, but it is unlikely to be smaller than 80 percent and may be as high as 97 percent (see Murray & Cox, 1979b; Williams & Gold, 1972). Suppose, for purposes of illustration, that it is 90 percent. Now, consider the results of the following hypothetical comparison of interventions A and B, each with 100 cases. We are omniscient, and know as the truth that A is much more effective than B in producing cessation: only 30 of the A delinquents commit a criminal offense during the year following the intervention, compared to 60 of the B delinquents. The true breakdown of arrest records is:

Intervention	No. committing...			Probability of arrest	Likely no. of delinquents arrested at least once
	No offenses	1 offense	2 offenses		
A	70	15	15	.1	4
B	40	30	30	.1	9

Given this situation, using arrest data as the measure and assuming that the laws of probability are working efficiently, the researcher without omniscience will *estimate* the cessation rate approximately 96 percent for program A and approximately 91 percent for program B--in other words, a very large real difference between the two programs will virtually be wiped out by the detection problem.[1] Specifically, the worse program's apparent success rate will be inflated; the worse the program, the greater the inflation. The detection problem arranges a very large fudge factor for hiding failures. Note as well that even large increases in arrest probability do not appreciably diminish the problem. Even if the probability of arrest were as high as .25 in the example given above (almost surely too high), the observed cessation rate would be on the order of 90 percent for program A and 79 percent for program B. The observed difference would be far smaller than the real difference.

Before and After Comparisons of Arrests

The other potential choice of dependent variable is number of arrests in a given time period. This was the basis for calculating the "suppression effect" discussed in the work that led to this study. The rate of arrests in the year prior to intervention was compared with the rate of arrests in the period following release. The suppression effect consisted of the percentage change:

$$\frac{\text{Postintervention Rate} - \text{Preintervention Rate}}{\text{Preintervention Rate}}$$

The gross effects of supervision and probation were analyzed in this fashion (Murray & Cox, 1979b, Chapter 5). But in this study, in which we move to the micro-level of petition-by-petition outcomes, we can no longer use number of arrests during a time period--because the relationship of the court to the delinquent shifts every time that the delinquent goes to court (or, for that matter, is arrested *without* going to court). We are interested not simply in the binary condition of being on or off probation, but in a much finer level of detail. We wish to examine the *next* reaction to the court, and for that we must pair court actions with the very next arrest.

TNA provides the level of detail that is needed. The delinquent's status with regard to the court--both the court's most recent action and the cumulative history of court actions up to that moment--can be associated directly with the TNA for the arrest transition *during which* the appearance occurred. Using TNA also circumvents the detection problem. It remains true that only a fraction of the offenses are detected, but this does not bias the estimate of the sample of offenses that are detected.

Four issues do remain, however, that affect the intepretation of an analysis using TNA as the dependent variable.

Skewness in TNA: To Log or Not to Log

Empirically, TNA is a skewed variable. Of the 3,390 arrest transitions[2] that occurred prior to institutionalization or end of observation (whichever came first), more than half were less than 3 months long, with the other half ranging from 3 to 72 months. Because regression is the principal statistical technique used in this study, and regression assumes normal distribution as one of the properties of the data in computing significance tests, the skewness in TNA presents us with a potential problem.

Statistically, the skewed distribution of TNA can be corrected by using a log transformation of the raw values. But using a log transformation has its disadvantages, despite its statistical properties. The size of the regression coefficients is obscured. Communicating the substantive meaning of the results becomes cumbersome. Most importantly, using a log transformation in the absence of a strong theoretical justification can seduce the analyst into discussing the statistical fit of a variable which is, in effect, a variable that is importantly different from the one that motivated the analysis in the first place. In our view, this is the case when discussing TNA and delinquency.

To illustrate, suppose we are comparing two pairs of arrest transitions: Pair A, 2 months and 2.5 months; and Pair B, 6 months and 12 months. Thinking about these numbers as expressions of real time, what do we make of them? The raw difference between 2 and 2.5 months--about two weeks--is trivial. The difference between six months and a year is not only much longer in an arithmetic sense, it intuitively seems "much longer" in terms of substantive interest. But when we use a natural log transformation, the relationships among the two pairs are altered drastically. In logged terms, Pair A (2 and 2.5 months) becomes .69 and .92. Pair B (6 and 12 months) becomes 1.79 and 2.48. Consider what happens when we try to make statements about the pairs.

Whereas before, we thought that the proportionate increase in Pair B was 12 times greater than the change in Pair A, we now see that the change in Pair B was only three times greater than the change in Pair A. Whereas before we thought that the raw change in Pair B was 6 months, compared to .5 months for Pair A, we now see that the "raw" change was only .69 for Pair B, compared to .23 for Pair A. And, if we decide to translate these changes back into months for comparison purposes, we find that .69 converts into 2.0 months while .23 converts into 1.3 months. In the course of the analysis, it is very hard indeed not to fall into the trap of talking about the change as *being* 2 months versus 1.3 months instead of 6 months versus 2 weeks--by implication if nothing else. The log transformation has greatly attenuated the differences we observed in the raw data.

The point is straightforward: *No theoretical reason justifies weighting the importance of small differences for short TNA, nor for discounting the importance of large differences for long TNA.* On the contrary, an appraisal of the importance of long and short changes in TNA suggests, if anything, that we ought to be weighting the other way around.

Fortunately, regression is a relatively robust procedure, and produces usable significance tests under a variety of violations of its assumptions, including the assumption of normality. We have therefore conducted, and report, regressions using the natural metric for TNA. As a precaution, we replicated several of the analyses using logged forms, and compared the results with the originals. In no case did the reanalysis indicate that we were finding significant results in the unlogged form that were not reproduced in the logged form. In a few instances, it appeared that we might be underinterpreting the data--risking Type II error, in the jargon--but the results in this study already provide enough to chew on, without straining for more.

Protecting Against End-of-Observation Bias

As the followup period comes to an end, the estimated TNA could, given certain assumptions, be an underestimate of the true population mean. Suppose, for example, that the true time to next offense of a given population is .5 years

with a standard deviation of .5. Given a normal distribution and an observation period of exactly one year following each arrest, we can expect that roughly 16 percent of the true time to next *offense* among the population will occur after the end of the year, while our estimate, based on *arrests*, will necessarily include only those offenses that occur within the year.

This bias may or may not exist. A variety of conditions produce situations in which the potential bias is counteracted by other factors (e.g., the large number of "first arrests" during the 17th year). The issue is whether any of the court interventions *systematically tend to occur at the older ages*, thus truncating the postintervention observation period and producing what is known as "a ceiling effect." The basic data are shown in Table 1.1.

TABLE 1.1
The Ceiling Artifact and Alternative Interventions

	N	Mean age at occurence	Mean street-time followup period
Institutionalization	34	16.2	.3 ①
Nonresidential correctional programs	11	16.2	.8
Probation	104	15.4	1.6
Supervision	176	14.0	3.0
First referral to court	309	14.4	2.6

① 14 of the 34 had not been released by the end of observation. Of the 20 released, mean followup was .5 years.

The implication for the analysis is that the post-institutionalization period is likely to show an artifically short TNA, because of the ceiling effect. Nonresidential correctional programs have more leeway (because the youth are on the streets, and the followup period can commence immediately after the disposition), but still can be expected to have some truncation problem. We therefore limit the examination of the effects of either of these types of correction programs to a few brief, illustrative analyses in Section II. Data on TNA for correctional programs (using a follow-up period extending beyond the juvenile history) may be found in *Beyond Probation* (53-57).

Does a residual problem remain even in the case of probation and supervision? The next step was to examine the extent to which TNA shrank as the date of the preceding crime got closer to the cut-off date of observation.[3] But this introduced a problem: From our earlier work, we know that arrest rates tend to increase with age through the 17th birthday, for reasons uncontaminated by artifact (Murray & Cox, 1979b). Further, we know that arrest rates also increased markedly with the sequence number of the arrest transition (Wolfgang et al., 1972; Hamparian et al., 1978; Murray & Cox, 1979b). Given the direct relationship between the cut-off date and age, and the correlation between arrest sequence number and age, we therefore could expect that TNA would indeed tend to shrink as the date of crime approached December 1977--but for substantive reasons admixed with whatever artifact was present. We therefore examined the *pattern* of the relationship of date-of-crime to TNA over varying subsamples. We employed a regression[4] equation of the form:

$$
\begin{aligned}
\text{TNA} = {} & B_1 \text{ (constant)} + \\
& B_2 \text{ ARRESTS} + \\
& B_3 \text{ AGE} + \\
& B_4 \text{ 77CRIMES} + \\
& B_5 \text{ 76CRIMES} + \\
& B_6 \text{ 77CRIMES} \times \text{AGE} + \\
& B_7 \text{ 76CRIMES} \times \text{AGE}
\end{aligned}
$$

where

TNA is time-to-next-arrest in years,

AGE is age in years at the time of the first arrest in a transition pair,

ARRESTS is the sequence number of the first arrest in a transition pair,

77CRIMES is a dummy variable coded "1" for transitions that began during 1977, "0" otherwise, and

76CRIMES is a dummy variable coded "1" for transitions that began during the last half of 1976, "0" otherwise.

To the extent that the truncation problem was a reality for the court interventions, then the dummy variables should significantly add to the variance explained by AGE. A hierarchical regression analysis was conducted, using the following models:

MODEL I: The chronology variables AGE and ARRESTS

MODEL II: The chronology variables plus the end-of-observation dummy variables

MODEL III: Model II plus the interaction terms.

The results indicate that the potential source of bias is not a problem in fact. Model I explained 20.4 percent of the variance ($F = 434.41$, $df = 2, 3400$). With the addition of the dummy variables, Model II explained only another 1/10 of one percent (20.5%). With the addition of the interaction terms, Model III explained an additional 1/100 of one percent (20.51%). Or in other words: the postintervention observation period was not perceptibly skewed by arrests late in the observation period.

Because no pattern was observed, we have chosen to employ the entire data base for the analyses, taking the precaution of always including age at the beginning of an arrest transition as one of the independent variables, and giving special attention to analyses (e.g., of probation) that might yet be special cases.

TNA and the Regression Artifact

In presenting the results that follow, we will encounter an issue that is endemic to longitudinal research and especially before-and-after comparisons: the regression artifact. The logic and mathematics of the issue are discussed at length in the preceding study (Murray & Cox, 1979b, pp. 78-93). Essentially, the phenomenon known as the regression artifact is 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. To what extent is it likely to be a problem in these analyses? To what extent will TNA show an increase, just because the delinquents chosen for intervention typically had an abnormally short arrest transition leading to the intervention?

One safeguard in the analysis is that the length of the preceding arrest transition is used as one of the independent variables. Another safeguard is that the analyses typically compare groups rather than look for a main effect across the entire population. If the length of the preintervention arrest transition is similar for all groups but TNA differs by intervention, the differences among the groups cannot ordinarily be attributed to regression artifact.

But these steps are secondary. A cleaner test is available. Namely: The regression artifact would be a problem if, characteristically, abnormally short prior arrest transitions tended to trigger sanctions. We divide all arrest transitions into two stacks: those that resulted in a sanction (supervision or probation), and those that did not. We then ask whether, when age and the sequence number of the opening arrest are taken into account, we can discriminate between the two stacks. The regression is

$$\begin{aligned} \text{TNA} = \; & B_1 \text{ (constant)} + \\ & B_2 \text{ RESPONSE} + \\ & B_3 \text{ AGE} + \\ & B_4 \text{ ARRESTS} \end{aligned}$$

where

TNA is time between arrests for a given arrest transition,

RESPONSE is a dummy-coded "1" when the arrest transition immediately preceded imposition of supervision or probation, "0" otherwise,

AGE is age at time of the opening arrest of the pair, and

ARRESTS is the sequence number of the opening arrest in the pair.

The results are shown in Table 1.2. In effect, we are conducting an analysis of covariance in which the criterion variable is TNA, the factor is the court's subsequent response, and the covariates are age and arrest sequence number.[5]

As Table 1.2 indicates, sanctions were not being imposed after abnormally short transitions.[6] The coefficient for the

TABLE 1.2
Regression Analysis of the Relationship Between Length of Prior Arrest Transition and Use of Sanctions

Variable	B	Standardized β	Standard Error	$p \leq$
Subsequent sanction (0/1)	–.05	–.03	.03	NS
Age in years	–.15	–.36	.01	.001
Arrest number	–.02	–.15	.00	.001
Constant	+2.82			

Dependent Variable: Time between arrests in years.
Reference group: Transitions which were not followed by a sanction.
$R^2 = .189$ F = 204, 42 (df = 3, 2634)

RESPONSE variable was small in magnitude, and statistically insignificant. Note also that the addition of RESPONSE raised the explained variance insignificantly by only .001 from the R^2 of .188 produced by AGE and ARRESTS alone. The addition of interaction terms in a fully saturated model raised the R^2 by another .001. The effects of the sanctions we shall be describing did not feed off a baseline of easy-to-beat, brief TNAs.

TNA and Dropout: Extrapolating the Findings

Having taken the position that binary cessation measures and numbers of arrests are inappropriate measures for this study, we are left with a problem. By not dealing with the case of the youth who drops out of sight, or whose arrest rate goes to zero, all of the analyses necessarily deal with a form of failure: "time-to-next-arrest," no matter how long it may be, is still, after all, contingent on the occurrence of a next arrest.

We can do little more than acknowledge the situation and duly caution the reader to remember the context in which the analyses are being conducted. The limitations of analyzing nothing but "extents of failure," and never measuring "real success," should not be ignored.

In extrapolating the findings, one has a choice: to assume that findings about the changes in TNA tell us nothing about the effect of the same stimulus on dropout, or to assume that the dynamics that lengthen TNA and those that produce dropouts are similar. We find the latter to be more plausible. If stimulus S_1 produces a length of time-to-failure (TNA) that is double that of stimulus S_2, presumably stimulus S_1 causes more dropouts as well. The alternative is to believe that S_1 can at once be more effective than S_2 in delaying the next arrest, and yet also produce a higher likelihood of *ever* being arrested again. In the context of the analyses that follow, it is not an easily defended logic.

NOTES TO SECTION I

1. These implausibly high success rates are a function of the artificially low limit we put on number of arrestable offenses for illustrative purposes. The bias favoring the worse program is generalizable whenever a cessation criterion is applied in a context of many undetected offenses.

2. "Arrest transition" refers to two sequential arrests and the period they bound.

3. The end of observation was 31 December 1977, the last day on which any of the members of the cohort could have had his seventeenth birthday. Note that actual birthdate was not recorded for this sample. The date of arrest serves as a proxy measure of age at arrest (because all members of the sample were born within the same year). The variable AGE in the following analyses consists of the date of arrest minus 60.5 (the expected mean value of all birth dates in the sample). None of the analyses in the study depends on a more precise date of birth for interpretive purposes.

4. To some extent, the analyses and conclusions of this study can be appraised critically only if the reader has a working familiarity with multivariate regression analysis. But the basic concepts of the procedure are not formidable, and a technically naive reader should be able to read the report and understand the tables. Very briefly: We are presenting equations with one dependent variable--the variable whose values we are trying to "explain"--and several independent variables. We may employ a familiar relationship by way of illustration: Suppose that we are trying to explain income as a function of years of education and age. The regression equation would be

$$\text{INCOME} = (\text{constant}) + X_1\text{EDUCATION} + X_2\text{AGE}$$

X_1 and X_2 are *regression coefficients*--weights--that provide the most accurate (linear) "prediction" of income. If, when age is taken into account, income rises by $1,000 for every year of education, then X_1 would be 1,000.

An *interaction term* is a combination of two or more independent variables. Suppose that income increases spectacularly as the wage earner gets older if and only if s/he has a high level of education. This will show up if we consider age and education in combination--in practice, by multiplying them. The new equation would thus be

$$\text{INCOME} = (\text{constant}) + X_1\text{EDUCATION} + X_2\text{AGE} + X_3(\text{EDUCATION} \times \text{AGE})$$

One important feature of the regression analyses in this study needs mention: *dummy variables*, which are used repeatedly to express the delinquent's status with the court. Suppose, in our running example,

we wanted to analyze the effect of sex on income. Sex is not a quantitative variable, but we can make it into a usable variable for regression analysis by coding it as "0" or "1"--let us say, "1" is women. The hypothetical results are

$$\text{INCOME} = 3000 + (500 \times \text{EDUCATION}) + (100 \times \text{AGE}) + (-2000 \times \text{SEX})$$

The results say that once education and age are taken into account, men tend to earn $2,000 more than women. Evaluating the entire equation for 40 year-olds with 12 years of education, a man is predicted to make $3000 + ($400 x 12) + ($100 x 40) + (-2000 x 0) = $13,000 while women make $3000 + ($500 x 12) + ($100 x 40) + (-2000 x 1) = $11,000. The *reference group* in an equation using dummy variables is the set of cases coded 0 on all the dummy variables--men, in our example.

The above comments all relate to the size of the regression coefficients, not to their accuracy. Even if we were using random data, a regression procedure would crank out coefficients. The issue of accuracy is addressed by *statistical significance*. As a very rough statement: We test the probability that the results could have been produced by chance; and we call the results statistically significant if that chance is no more than 5 percent. It is important to remember that statistical significance does not automatically confer importance on a result. A regression coefficient can be of interest because of its size and direction, even though it is not statistically significant; conversely, a statistically significant result can be of little substantive interest if the regression coefficient is extremely small.

5. Thinking of it as an analysis of covariance is helpful insofar as the usual causal interpretation of a regression equation is not applicable in this instance (RESPONSE, an independent variable, occurs after TNA, the dependent variable).

6. The relationship of two successive times-between-arrest raises the issue of autocorrelation, especially since we use PRIOR, the length of the arrest transition preceding TNA, in virtually every analysis. Because we do not use the entire time series, a full-scale examination of the autocorrelation properties of TNA was foregone. We did examine autocorrelations for the first lag. They were: $r_{12} = .02$, $r_{23} = .08$, $r_{34} = .09$, $r_{45} = .08$, $r_{56} = .19$, $r_{67} = .10$. The subscripts indicate arrest sequence numbers. Autocorrelations of this magnitude, in an analytic approach of the type used in this study, can be expected to have extremely little effect on the results.

Section II
Elite Wisdom and the Logic of Deterrence

THE ELITE WISDOM ON JUVENILE CORRECTIONS

There are two conventional wisdoms about the right way to deal with delinquents: a popular wisdom and an elite wisdom. The popular wisdom can be heard in almost any conversation with almost any group of people not professionally involved in issues of juvenile justice. It holds that kids have no respect for the law any more, because the courts give them a tap on the wrist and send them back on the streets. It holds that we ought to get tough with these delinquents right away. If probation doesn't work, lock 'em up.

The elite wisdom--meaning the wisdom that prevails among the people who shape and implement national policy--is 180 degrees in opposition to the popular wisdom. It holds that punishment of youth is not only regressive but counterproductive. Less is better. The objectives of the juvenile justice system should be to minimize punitive reactions, maximize positive supports. This stance pervades the principal Federal legislation on delinquency, the policy of the Federal agencies that deal with delinquency problems, the efforts of the many lobbies and organizations for youth-related causes, and the practice of many, probably most, of the nation's juvenile courts.

The elite wisdom also has the solid backing of the intellectual establishment. With the exception of a few mavericks like James Q. Wilson or Ernest van den Haag, the precepts of the elite wisdom have typically been criticized only on their peripheries, not challenged on their fundamental validity.

In this study, most of the analyses contradict elite wisdom. So let us state explicitly at the outset what we stated in the preceding studies: Nothing in the data base is anomalous, relative to other comparable data bases. In the *Beyond Probation* analyses, the only other comparable before-and-after comparisons (Empey & Erickson, 1972; Empey & Lubeck, 1971) had found results consistent with ours. In this case, the only comparable analysis using time-to-next-arrest as the dependent variable (and the arrest transition as the unit of analysis) is *The Violent Few* (Hamparian, Schuster, Dinitz, & Conrad, 1978). As it happens, the study provides a close-to-exact parallel with this one: conducted in Columbus, Ohio, it has a large sample, using a birth cohort as the basis for selection. It follows the entire juvenile career (cutting off observation when the youth entered the adult system) arrest by arrest. It devotes extended attention to issues of maturation, spacing of arrests, and number of arrests. And, as noted, it uses TNA (they term it "velocity" of arrests) as a central dependent variable.

It is also an articulate, careful exposition of elite wisdom. It begins from the indispensable perspective of delinquents as victims: "Troubled young people are responding to the troubles of our times." (Hamparian et al., 1978: 1) It points out that violent offenders are a very small fraction of all delinquents. It emphasizes that most delinquents are not heading irretrievably down a road of increasingly serious crime. And, when the authors compare the effectiveness of alternative modes of intervention (no action, court actions, correctional programs) they confirm the tenet of the elite wisdom that getting tough only makes matters worse. As the authors put it:

> What we have to address here is a simple question answerable from the record and not by conjecture. Controlling for such variables as arrest sequence number, type of offense, age, race, sex, and socioeconomic status, how does the sanction imposed for the first of any two pairs of offenses affect the velocity of the commission of the second, given equal street time?...With this constraint, what can be said about the impact of various intervention modalities? *Perhaps the most significant finding in this study is that with all else controlled, there is a moderate to high inverse relationship between the severity of the sanction for the first in every pair of crimes and the arrest for the second in the pair.* (Hamparian et al., 1978: 118-119. Emphasis in the original.)

These results were welcomed by the then Administrator of the principal Federal agency dealing with delinquency (the Office of Juvenile Justice and Delinquency Prevention). He wrote in the Foreword:

> These results...buttress the finding of the Congress that the juvenile justice system overreacts to almost all youth brought before it, and is ineffective in either helping youth or protecting communities from juvenile crime. (John M. Rector, in Hamparian et al., 1978: xvii.)

A more succinct statement of the core belief of the elite wisdom would be hard to find. A more emblematic example of the interlocks among proponents of the elite wisdom--Congress, the Federal agencies, and researchers--would be hard to find.

The point is this: *When we ask the same questions of our data, in the same way, we get the same answers.* The results from Chicago echo the conclusions from Columbus with remarkable fidelity. Let us begin, then, by showing how our data would have supported the accepted truths--if we had stopped with the accepted questions.

The conventional approach has consisted of comparing delinquents who have undergone different correctional "treatments." We shall do so with our data base by comparing TNA for youth at four levels of sanction in ascending order of severity: no sanction (i.e., not even sent to court after an arrest), supervision, probation, and correctional interventions (UDIS or institutions).[1] For these four levels, mean TNA (in years) was:

No sanction	.64
Supervision	.44
Probation	.27
Corrections	.25

The ordering is perfect. Not only do the results suggest that punishment is worse than no punishment, they suggest that the more punishment, the worse matters become. The delinquents on probation recidivated more than twice as fast as the ones without court status; the ones sent to correctional programs recidivated the fastest of all.

These results hold up when examined statistically. As throughout the rest of the study, we employ multivariate regression as the basic tool.[2] The dependent variable is

TNA. The unit of analysis is the "arrest transition," or the variables associated with two sequential arrests. The regression equation employs three sets of independent variables.

The first set consists of three background variables that will prove to be important (see Section III): age at the opening arrest in the transition (AGE), sequence number of arrest, including the opening arrest in the transition (ARRESTS), and time-between-arrests for the immediately preceding arrest transition (PRIOR).

The second set is comprised of three dummy variables, characterizing the most recent "treatment" preceding the close of the transition. CORRECTIONS is coded "1" if the closing arrest of the transition occurred after release from a correctional program. PROBATION is coded "1" if the closing arrest occurred after the youth is on probation (but before going to a correctional program). SUPERVISION is coded "1" if the closing arrest occurred after the youth is put on supervision (but before either probation or a correctional program). The reference group thus consists of all transitions that occurred before any sanction was imposed.[3]

The third set of variables is comprised of three interaction terms, AGE with SUPERVISION, with PROBATION, and with CORRECTIONS, asking whether the effects of the punishments systematically varied as delinquents got older. The regression equation is thus of the form:

$$
\begin{aligned}
\text{TNA} = \; & B_1 \text{ (constant)} + \\
& B_2 \text{ AGE} + \\
& B_3 \text{ PRIOR} + \\
& B_4 \text{ ARRESTS} + \\
& B_5 \text{ SUPERVISION} + \\
& B_6 \text{ PROBATION} + \\
& B_7 \text{ CORRECTIONS} + \\
& B_8 \text{ AGE x SUPERVISION} + \\
& B_9 \text{ AGE x PROBATION} + \\
& B_{10} \text{ AGE x CORRECTIONS}
\end{aligned}
$$

Establishing another pattern that we will follow throughout the study, we conduct a hierarchical analysis. We first let the background variables explain as much variance as they can; then add the treatment variables and ask whether they significantly add to the explained variance; then do the same thing with the interaction terms. The three models tested are:

Model I: The background variables (AGE, ARRESTS, PRIOR)

Model II: The background variables plus SUPERVISION, PROBATION, and CORRECTIONS

Model III: Model II plus the interaction terms.

The results are shown in Table 2.1, and they confirm expectations.

The relationships of all three levels of sanction, probation, and supervision to TNA are negative. Even after taking age, number of prior arrests, and length of the preceding arrest transition into account, the state of being on supervision, on probation, or in a correctional program is

TABLE 2.1
Regression Analysis for Demonstrating the Merits of Minimal Intervention

RESULTS OF THE HIERARCHICAL ANALYSIS

Model	R^2	F	(df)	p ≤	Compared to	R^2, increment	F, increment①	(df)	p ≤
I	.135	141.97	(3, 2729)	.001	—	—	—	—	—
II	.139	73.18	(6, 2726)	.001	I	.004	4.27	(3, 2723)	.01
III	.149	52.85	(9, 2723)	.001	II	.010	10.67	(3, 2723)	.001

PARAMETER ESTIMATES FOR MODEL III

Variable	B	Standardized β	Standard Error	p ≤
Age, in years	− .13	−.31	.01	.001
Prior arrest transition, in years	+ .13	+.15	.02	.001
Number of arrests	− .01	−.16	.00	.001
Supervision (0/1)	−1.23	−.82	.28	.001
Probation (0/1)	−1.27	−.72	.38	.001
Correctional programs (0/1)	−1.56	−.51	.97	NS ②
Interaction, age with supervision	+ .08	+.77	.02	.001
Interaction, age with probation	+ .09	+.76	.02	.001
Interaction, age with correctional programs	+ .10	+.55	.06	NS ②
Constant	+2.35			

Dependent variable = TNA in years
Reference group = subjects who had not yet been (or never were) exposed to a sanction

① Test of significance for improvement of fit over the comparison model.
② Nonsignificant (p >.05).

associated with *faster* subsequent arrest than no sanction at all. Correctional programs did much worse than lesser sanctions. The interaction terms are all positive.

Interpreted in light of the elite wisdom: Do not intervene unless absolutely necessary. If you must intervene, at least wait until the youth is older. On the face of it, our findings could be interpreted using the passage quoted earlier from *The Violent Few*, which also controlled for age

and arrest sequence number, and which also compared "no sanction" with court sanctions and with institutionalization.[4] The replication of results is almost perfect.

Q.E.D. Two major studies, using comparable data analyzed by independent investigators, have both shown that minimal intervention is appropriate. It seldom happens in social science that congruence is so marked.

Obviously, we think there is a catch--namely, that Table 2.1 does not present the end of the analysis, but the beginning. The rest of the study represents an extension of the inquiry.

We begin, briefly, by taking a second look at the results about correctional intervention. The same data, seen from a slightly different perspective, yield dramatically different results.

First, as readers of Chapter I will expect, we worry about the ceiling artifact in considering the poor showing of correctional programs. By stopping the observation period at age 17, we have run the risk of systematically censoring the results of an intervention that ends abnormally close to the 17th birthday.

Apart from that, however, there is a pervasive problem in drawing conclusions from the comparison: the selection process. Judges are basing their decisions on a variety of factors, very few of which are captured by the variables in our equations. It is one thing to say that we have "taken into account" age, number of prior arrests, and length of the preceding arrest transition. This much is possible. To assert that these manipulations have produced comparable subsamples of delinquents is a much stronger, less plausible stance.

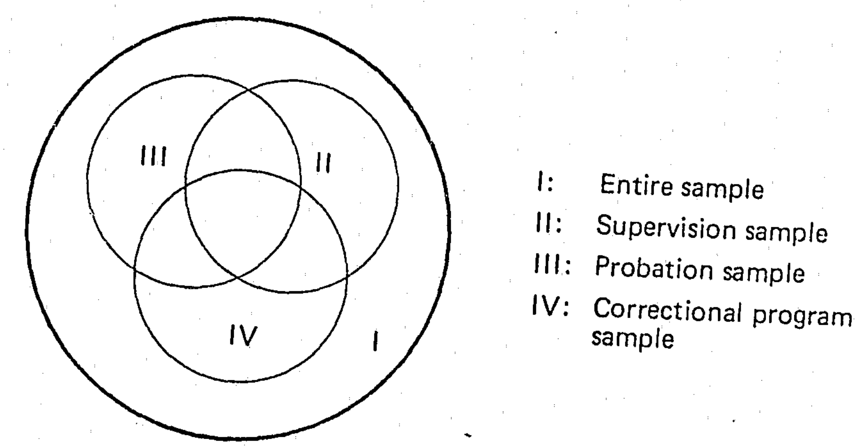
Suppose that we have 100 delinquents, each of whom has committed a burglary. Twenty-five are put in institutions; 75 are not. For some unknown fraction of these 100 cases, the reasons for the institutionalization decision will be captured by our variables--the boy had been in the same courtroom just three days earlier on another charge, or the burglary was the 15th arrest. But for another and presumptively much larger set of cases, the reason will *not* have been captured by these variables. Perhaps the burglary followed three preceding arrests for violent offenses, or the burglary was accompanied by some other characteristic (e.g., malicious destruction) not reflected in the official charge.

These are not irrelevant factors. Even the frequently voiced complaint about some judges that "he will send a kid to jail because he doesn't like his attitude" is not necessarily an indictment of the selectiveness of the selection process. Arguably, *the fact of having been chosen* for institutionalization is one of the most important variables for characterizing the population's preinstitutionalization behavior.

This argument gains force when it is remembered how very little of the variance in judges' decisions is explained by the variables used in the quantitative analysis. If, in the case of our data base, we try to predict whether the judge would institutionalize or not institutionalize on the basis of type of offense, number of prior arrests, age, and length of time since the preceding arrest, we explain a grand total of 1.2 percent of the variance.[5] And yet, *if we are to interpret Table 2.1 in support of the elite wisdom, we must assume that the other 98.8 percent of the variance is unsystematic*--that the judges, without meaning to, were randomly assigning subjects to institutionalization.

If this assumption is too hard to swallow, we may move to an alternative perspective, whereby we assume that the delinquents who reach different stages of intervention are drawn from different populations. The appropriate comparison is not among all delinquents, but among all delinquents who reached the point of sanction X.

We operationalize this by replicating the regression analysis reported in Table 2.1, applying it to four populations, all drawn from the 1960 birth cohort of Chicago delinquents: the entire random sample (as reported in Table 2.1), a random sample of all those who were ever put on supervision, a random sample of all those who were ever put on probation, and all those who were sent to a correctional program.[6] A member of one of those samples may also have been a member of one or more of the others. The potential relationships among the four samples may be depicted as follows:



The material in Table 2.2 demonstrates the dramatic changes introduced by this approach. As the population is successively winnowed, the regression weights of the background variables remain constant (as they should *not* do if

TABLE 2.2
Replication of the Minimal Intervention Analysis, by Samples of Delinquents who Reached Successive Stages of Intervention

	Cohort Sample	SUBJECTS WHO REACHED AT LEAST: Supervision	Probation	Correctional Programs
Age, in years	–.13	–.09	–.07	–.09
Prior arrest transition, in years	+.13	+.15	+.16	+.07
Number of arrests	–.01	–.01	–.01	–.01
Supervision	–1.23	–.64	–.40	+.25
Probation	–1.27	–.42	–.32	+.36
Correctional programs	–1.56	–1.06	–.49	+1.38
Interaction, age with supervision	+.08	+.04	+.03	–.02
Interaction, age with probation	+.09	+.03	+.03	–.02
Interaction, age with correctional programs	+.10	+.07	+.04	–.07
Constant	+2.35	+1.74	+1.32	+1.47

Dependent variable = TNA in years

if these variables were capturing the reasons for judges' decisions). The weights for the treatment variables and the interaction terms do change, consistently and dramatically. *The negative main effects of the treatments and the positive interaction effects with age both diminish, and change signs altogether for the "corrections" group.* Far from hastening arrests, *correctional programs slowed arrests dramatically among the population of delinquents selected for correctional interventions.* These effects were weakened, not strengthened, if the correctional intervention was delayed.

The above is nearly our last mention of correctional programs. The center of our attention is not them, but the earlier interventions--supervision and probation--and with them, at least, it would seem that we have fallen on fallow ground. Even by focusing on the subsamples in Table 2.2, it seems that we are looking at a standard example of "no effect." The main effects for the supervision and probation subsamples continue to be negative, and the small, positive interaction terms can do little more than cancel out the negative main effects.

Again, there is a catch. The above is true if, and only if, we conceive of probation and supervision as relying on *content* for their impact--that is, if we see supervision or probation as consisting of a continuing treatment that will either "take" or "not take." This view of interventions as treatments has dominated the correctional literature. Given that view, the form of analysis employed in Tables 2.1 and 2.2 is appropriate. The tests are interpretable, and the results are negative: supervision and probation do not work, at least in Columbus, Ohio and Chicago, Illinois.

There is, however, an alternative way of looking at court interventions: not as programs laden with content, but as one-time shocks. From this perspective, it is being *put on* probation or supervision that has the impact, not the treatment that the delinquent receives while in that condition. One comes to this view, and asks the questions it raises, by way of the logic of deterrence.

THE LOGIC OF DETERRENCE

The backdrop to this analysis is deterrence. It is one of the most fundamental arguments in favor of punishment, only slightly less ancient than the concept of punishment itself. It is also an unfashionable argument, especially with regard to delinquents. It is so unfashionable, and so widely thought to have been discredited, that the reader may wonder if we are using "deterrence" in a novel sense, or if we are changing some of the usual assumptions. The answer is no. When we use the term, we mean exactly what the reader thinks we mean: namely that, *by and large, people behave in ways that promote gain and avoid loss, and that changes in the calculus of gain and loss produce changes in behavior.* [7] Deterrence is the result of increasing the risk of expected loss, the magnitude of expected loss, or both.

The argument is simple, but widely misperceived as well. The plausibility of deterrence is commonly dismissed because, by everyday observation, we know that people often do *not* calculate gains and losses before acting. People are not even capable of choosing among options, if one accepts a determinist view of the world. Other common objections are drawn on a grand scale (the United States employs longer sentences and imprisons more people per capita than country X, but has much higher crime). Other objections are based on anecdote, perhaps apocryphal (the most threadbare: in old England, pickpockets were most active in the crowds that gathered to witness the hanging of pickpockets).

Yet, taken to its extreme, the deterrence argument has an undeniable force: If a pickpocket could know for a certainty--not a probability, but a rock-solid certainty--that the moment he reached for the wallet his hand would be cut off, the picking of pockets would be reduced to near zero. Deterrence *can* work, ideally, beyond argument. The gap between this situation and deterrence in the real world

lies in the uncertainties that surround detection, punishment, and the likelihood that a potential offender will decide that the game is no longer worth the candle. Let us begin, then, by making explicit the key aspects of the deterrence argument.

Rationality need not be assumed. Rats in psychological experiments are not rational, but they respond to reinforcement schedules. The simpler the schedules, the more predictable the responses. This is not to say that we assume that delinquents are irrational; quite the contrary. But the assumption of rationality is not essential. Van den Haag (1975) explains:

> ...deterrence does not depend on a rationalistic psychology or on the calculations attributed by it to prospective criminals. The behavior of each of us is influenced by the actions of other people. They produce both emotional and material incentives and disincentives for us. People come to work because they are paid and quit, or change, if paid better elsewhere or if they find it more rewarding in some other way. They do avoid unrewarding or painful work or work that pays less than other work they can get. The theory of deterrence rests on these simple observations and not on calculations by its subjects....It is not necessary that the people who are to be deterred calculate, but only that the legislators who want to deter them do. Those who are to be deterred need only respond in predictable ways. People ordinarily do--or else social life would be impossible. (Van den Haag, 1975: 112-113)

Universality need not be assumed. For deterrence to operate, it is not necessary that everyone be equally susceptible to the threat of punishment. Rather, the deterrence argument holds that most people respond most of the time.

Certainty of punishment and severity of punishment interact. A mild punishment is more likely to deter if it has a very high probability of being imposed. A severe punishment is less likely to deter if it is imposed capriciously or very seldom--capital punishment probably being a case in point.

The effectiveness of deterrence depends on perceived reality, not objective reality. To illustrate (and it is not entirely hypothetical), suppose that all judges had a secret threshold that caused them to go from "no punishment" to "severe punishment." Suppose the threshold consists of number of prior arrests. The triggering mechanism may operate perfectly, but if no one knows what it is, and "no punishment" occurs much more frequently than "severe punishment," then the *perceived* reality of the ignorant offender is that the threat is small--even though the *objective* reality may be that the next arrest will inevitably lead to severe punishment.

In the case of a Chicago delinquent, the most plausible assumption is that perceived reality consists of two components: (a) what has happened to him, personally; and (b) what he notices has happened to his friends and acquaintances. This presumably will also weaken whatever deterrent effects we observe in the analysis. Given our data, the only history we can trace is the history of component (a). Component (b) is an unknown for any given delinquent. But, knowing that the practice of the Cook County Juvenile Court is to wait for an average of 13 arrests before imposing institutionalization (Murray & Cox, 1979b), we must assume that the typical experience of the delinquents from observing others has taught them that the court will tolerate a great

deal. The personal history of any one youngster may teach another lesson (e.g., if it has consisted of a steady, brisk escalation of sanctions), but we must expect that, in many cases, the delinquent is saying to himself that the court cannot really mean what it seems to be saying to him, because of the contrary lessons taught through component (b).

Throughout the rest of the study, we keep the logic of deterrence in mind. In some cases, it leads to predictions about what the data should show. In other cases, it generates competing hypotheses, given alternative collateral assumptions. In all cases, the underlying stance toward the data is that the behavior of Chicago delinquents is neither exotic nor aberrant, if one assumes that some important part of their behavior is based on their expectations about getting away with it.

NOTES TO SECTION II

1. "Street time" is used when a residential correctional program is involved. TNA excludes the time spent in the program, while the delinquent was presumably unable (or at least less able) to commit offenses.

2. Those who skipped Section I are referred to note #4 for that section.

3. The categories are mutually exclusive. If more than one sanction was operative during a given arrest transition, priority is given to the one that was imposed during the transition. Thus, for example, the transition for a subject who is on supervision, gets arrested, is put on probation, then is arrested again, would be coded as PROBATION = 1 and SUPERVISION = 0.

4. We emphasize that everything in this and subsequent analyses in this section refers to our data base, not the one used for *The Violent Few*. We do not imply that the same results would obtain if *The Violent Few* data were reanalyzed in the manner that we employ. Given how closely we were able to replicate the results of that study, however, it would of course be of interest to find whether our results could be replicated by the data from Columbus.

5. Hamparian et al. use a more detailed analysis and explain more of the variance, but come to a similar conclusion: "The interaction [with offense type] of other variables, prior record, age, and especially the presentation of self in court make disposition virtually unpredictable." (Hamparian et al., 1978: 106)

6. The sample of youth sent to correctional programs and released by the end of observation (n=20) was too small to yield stable parameter estimates. The population shown as the "correctional program" sample in Table 2.2 is the superset for those 20--the entire 1960 birth cohort of UDIS and institutionalized youth (n=150), who had been released by the end of 1977, drawing from the data base used in *Beyond Probation*. Time-in-program was excluded, observations were cut off at the seventeenth birthday, and all other procedures (including the use of a proxy "age" measure) were identical.

Even with the small released sample of UDIS and institutionalized youth in the random sample, the parameters were close. The regression weights were:

	Random Sample Subset (n=20)	Population (n=150)
AGE	-.08	-.10
PRIOR	+.09	+.07
ARRESTS	-.01	-.01
SUPERVISION	-.03	+.03
PROBATION	+.11	+.12
CORRECTIONS	+.15	+.37

7. In the jargon, we are concerned with "special deterrence"--the deterrence effect on people who have committed crimes--rather than "general deterrence"--the deterrence effect on people who might otherwise commit crimes.

Section III
First Contact with the Court

In most cases, a youngster who is arrested the first time is given a lecture and sent home. In Chicago, he might be given a lecture and sent home--"station adjusted" is the Chicago Police Department's phrase for it--several times.

This practice has a theoretical justification and a practical one. The theoretical justification is based on "labeling": the notion that the youth who is sent to court is labeled as a "delinquent," and thereby starts to think of himself as delinquent, making matters worse. The practical justification is that most juvenile courts are already swamped by work, and do not have the capacity to handle every case that the police could bring them.

THE EFFECT OF STATION ADJUSTMENTS

By the logic of deterrence, the policy of station adjustments should be disastrous. A youngster who has been arrested for the very first time is presumably as impressionable then as he ever will be. If the lesson he takes from that first experience is that being arrested does not necessarily mean punishment--a sanction of some sort--credibility of the *threat* of subsequent punishment is diminished.[1] If he is arrested again--and then a third and a fourth time--without going to court, credibility must continue to suffer. We therefore take as our expectation that those delinquents who are not referred to court will tend to increase their

arrest rate (decrease TNA) as the number of station adjustments increases. As the system continues to cry "wolf," the youth will become a more active criminal, and this will be reflected in a lowered time-to-next-arrest.

Figure 3.1 shows the results, and they are strikingly consistent with the expectation that TNA will decrease the longer that referral to juvenile court is deferred.

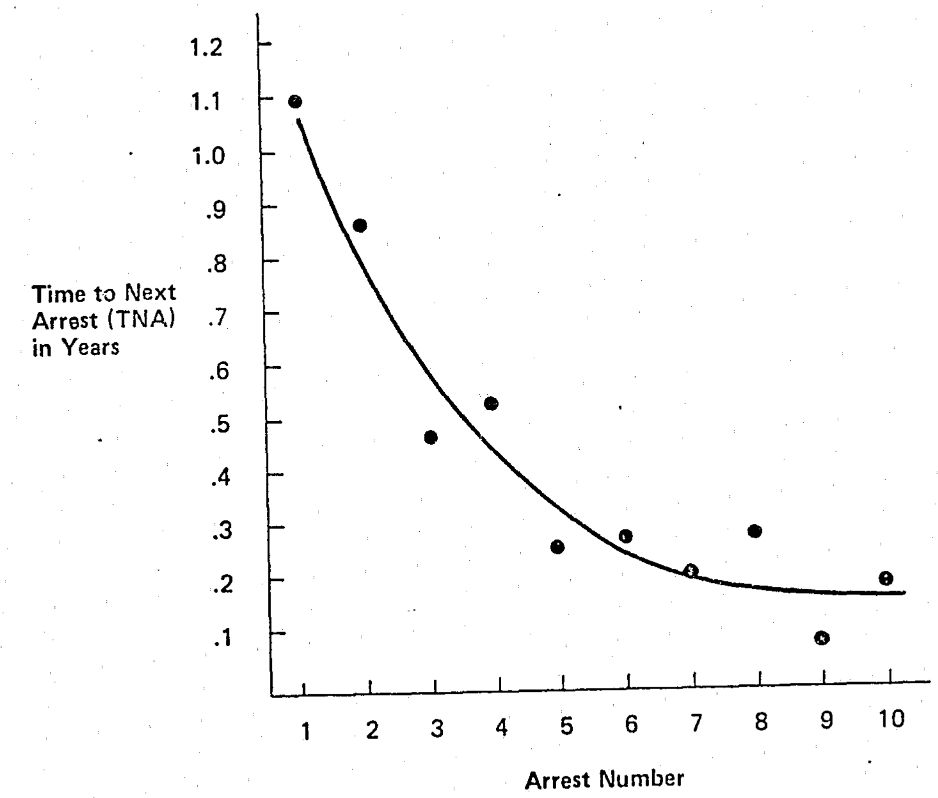


FIGURE 3.1. Time to Next Arrest for Arrests Occurring Before First Referral to Court

The sample exhibits a phenomenon that has been observed elsewhere as well (e.g., Wolfgang et. al., 1972; Hamparian et al., 1978; Murray & Cox, 1979b): TNA drops as the arrest transition increases. We are suggesting an interpretation: the delinquent suffers no sanctions for his behavior, the credibility of sanctions decreases, and therefore the velocity of offenses increases.

THE EFFECT OF THE FIRST COURT APPEARANCE

The next question is: What happens to time-to-next-arrest after the first court appearance?

There are two ways of deciding on expectations. One is to assume that just being referred to court is an escalation in the system's response, no matter what the court decides to do. The delinquent will see that his position has changed, that he is somewhat closer to real punishment of some sort, and that the credibility of punishment is increased. Therefore: some deterrent effect is achieved.

The alternative approach is more hard-headed, based on what actually happens at court. If the court does something that imposes a *sanction*--i.e., puts the youth on supervision or probation--then the credibility of eventual "real" punishment (incarceration) is increased. If, on the other hand, the court dismisses the case, or for some other reason decides to take no action, then the logic we are pursuing suggests that the effect will be worse than not being referred to court at all. The threat of being sent to court, which was repeated at the police station when arrests were being adjusted, has turned out to be no threat at all. The court is as impotent as the police. Credibility of eventual punishment diminishes, the deterrent effect diminishes, offenses become more frequent, and this is reflected in a reduced TNA.

The results for all eligible subjects (those station adjusted at least once, eventually sent to court, and subsequently rearrested) reveal that mean TNA for the arrest transition just prior to being sent to court is .48 years (N=171). Mean TNA for the next transition is .61 years; an increase of 27.1 percent. The evidence suggests that the first court referral in and of itself had a retarding effect of moderate size on arrest rate.

When we turn to the hard-headed approach--it is not just going to court that matters, but whether the court hands down punishment--the results are generally supportive. As Table 3.1 indicates, the increases in TNA were concentrated

TABLE 3.1
Relationship of First Court Action to Time to Next Arrest

		Time to Next Arrest (In Yrs.)		
1st Court Action	N	Arrest Transition immediately preceding first court appearance	Arrest Transition during which first court appearance occurred	Change
No action	113	.48	.47	– 2.1%
Supervision	46	.53	.93	+75.5%
Probation	12	.33	.72	+118.2%
Overall	**171**	**.48**	**.61**	**+27.1%**

among those who were put on supervision or probation, and those increases were large. The average delinquent put on supervision who later recidivated had been arrested about 6.4 months before the court appearances; he would not be arrested again for more than 11 months. Delinquents put on probation had been arrested about 4 months earlier; they would not be arrested again for an average of 8.6 months.

In contrast: the youth who were not subjected to any court action had been arrested about 5.8 months earlier--and would be arrested again in about 5.6 months.

For a statistical examination of the results, we classify the first court response as "Sanction" or "No sanction," combining probation and supervision because of the small sample size (12) of youths put on probation after the first court appearance.[2] We then examine the effect of a sanction in light of two variables: age at time of arrest leading to the first court appearance, and the "baseline" TNA of the subject--that is, the time-to-next-arrest for the arrest transition that led to the first court appearance.

We want to examine additional questions involving interaction effects:

First, *does the effect of a sanction depend on age*? Is it more effective to impose a sanction upon first court appearance for a young, presumably more impressionable youth?

Second, *does the effect of sanction depend on the length of the preceding arrest transition*? The *a priori* argument is that the interaction should be important in one instance: when the prior arrest transition was short, and the court still failed to take action ("It doesn't make any difference how bad I am; the court won't do anything to me").

The regression equation is thus of the form:

$$
\begin{aligned}
\text{TNA} = {} & B_1 \text{ (constant)} + \\
& B_2 \text{ AGE} + \\
& B_3 \text{ PRIOR} + \\
& B_4 \text{ SANCTION} + \\
& B_5 \text{ AGE} \times \text{SANCTION} + \\
& B_6 \text{ PRIOR} \times \text{SANCTION}
\end{aligned}
$$

where

TNA is time-to-next-arrest in years, for the arrest transition during which the court appearance occurred;

AGE is age in years at the time of the arrest that led to the first court appearance;

PRIOR is TNA for the preceding arrest transition; and

SANCTION is a dummy variable coded "1" if the delinquent was put on supervision or probation at the first court appearance, "0" otherwise.

A hierarchical regression analysis was conducted testing three models sequentially: Model I, with the background variables only; Model II, which added the sanction variable; and Model III, which added the interaction terms.

Model II did add significantly to the variance explained by Model I; Model III did not add significantly to Model II. The parameter estimates for Model II are shown in Table 3.2, along with a summary of the results of the hierarchical analysis.

TABLE 3.2
Regression Analysis of the Impact of the First Court Appearance

RESULTS OF THE HIERARCHICAL ANALYSIS									
Model	R^2	F	(df)	$p \leqslant$	Compared to	R^2 Increment	F, increment ①	(df)	$p \leqslant$
I	.103	9.64	(2, 168)	.001	–	–	–		–
II	.140	9.07	(3, 167)	.001	I	.037	7.12	(1, 165)	.01
III	.142	5.46	(5, 165)	.005	II	.002	.17	(2, 165)	NS②

PARAMETER ESTIMATES FOR MODEL II Variable	B	Standardized β	Standard Error	$p \leqslant$
Age, in years	–.13	–.26	.04	.001
Prior arrest transition in years	+.17	+.13	.09	NS②
Sanction (no = 0, yes = 1)	+.33	+.20	.12	.01
Constant	+2.36			

Dependent variable = TNA in years
Reference group = Subjects referred to court, no other action

① Test of significance for improvement of fit over the comparison model
② Nonsignficant ($p > .05$)

The statistical analysis in Table 3.2 results support the inferences drawn from an inspection of the data in Table 3.1: After taking age and the baseline arrest transition into account, the use of a sanction substantially lengthened the time-to-next-arrest--by about .33 years, or four months. Controlling for the effect of the sanction, the "natural course of events" after the first court appearance was for TNA to decrease as a function of increasing age and to increase slightly as a function of the length of the prior arrest. The interaction terms did not add to the explanatory power of the regression equation.

The results are intriguing for two reasons: Sanction did produce a positive effect; referral without sanction produced a smaller and much less reliable effect. We pursue both of these findings in the analyses that follow.

EFFECTS OF DELAYING THE FIRST REFERRAL TO COURT

We have seen (Figure 3.1) that TNA falls dramatically as court intervention is deferred. We have seen (Tables 3.1, 3.2) that a substantial increase in TNA is produced by a court sanction, either probation or supervision, at the first court appearance. Now, the question arises: *to what extent (if at all) do repeated station adjustments diminish the impact of the court sanction when it is finally imposed?* The logic is that repeated station adjustments let the delinquent behavior pattern become set, engender a generally lower level of apprehension about the response that the juvenile justice system is prepared to impose, and thereby lead the delinquent to pay less attention to whatever the court does do when he finally has to appear before it.

Table 3.3 summarizes a comparison of alternative situations. The first column shows the percentage change in TNA from one arrest transition to the next when the delinquent had not yet been sent to court, based on a curvilinear fit of the data in Figure 3.1. This may be interpreted as the baseline "expected" value for i prior arrests. The second column shows the percentage change in TNA between the prior and post arrest transitions when the first court appearance

TABLE 3.3
Effects of Prior Station Adjustments on the Impact of the First Court Appearance

Court Appearance	Change in TNA from the $(i-1)^{th}$ to the i^{th} Arrest Transition (n)		
		Referred to Court	
Number of Arrests (i)	"No referral" expectation	No Action	Supervision or Probation
2	–29%	– 16.3% (31)	+42.4% (15)
3	–27%	– 5.7% (19)	+51.1% (17)
4	–23%	+ 6.0% (15)	+174.6% (11)
5 or more	–16%	+22.3% (48)	+154.3% (15)

took place after the *i*th arrest. The third column shows comparable results when the first court appearance resulted in either supervision or probation. The expectation is that the earlier the intervention, and the more drastic the intervention, the better.

The pattern is unambiguous. Imposition of any sanction, whether supervision or probation, always produced much larger TNA than was observed for the same arrest transition among delinquents who had not yet suffered a sanction. But it does not appear that a greater relative impact is achieved by early *referral*, when no sanction resulted.

A statistical analysis clarifies the situation. We employ as the data base all arrest transitions up through the first court appearance. That is, we consider not only the subsample of youth who were sent to court, but also those who were not; not only the pair of arrest transitions immediately before and during the first court appearance, but all arrest transitions. The unit of analysis is the arrest transition, which is treated as being a function of nine variables.

The regression equation is:

$$
\begin{aligned}
\text{TNA} = {} & B_1 \ \text{(constant)} + \\
& B_2 \ \text{AGE} + \\
& B_3 \ \text{PRIOR} + \\
& B_4 \ \text{1/ARREST} + \\
& B_5 \ \text{REFERRAL} + \\
& B_6 \ \text{SANCTION} + \\
& B_7 \ \text{1/ARREST} \times \text{REFERRAL} + \\
& B_8 \ \text{1/ARREST} \times \text{SANCTION} + \\
& B_9 \ \text{AGE} \times \text{REFERRAL} + \\
& B_{10} \ \text{AGE} \times \text{SANCTION}
\end{aligned}
$$

where

TNA is time-to-next-arrest, in years;

1/ARREST is the reciprocal of the number of the opening arrest in the transition pair;

REFERRAL is a dummy variable coded "1" if the arrest resulted in referral to court *but no sanction*; "0" otherwise;

SANCTION is a dummy variable coded "1" if the court imposed either supervision or probation; "0" otherwise; and

AGE is age at the time of the opening arrest in the transition pair.

The reference group consists of transitions that occurred without referral to court. Note that 1/ARREST is expressed as the reciprocal of the raw number of prior arrests. This transformation serves two complementary purposes. First, it weights the importance of the early arrest transitions (appropriately, as Figure 3.1 suggests). Second, it converts the vector of the variable into one (high is "good," low is "bad") that permits a meaningful interaction term of 1/ARREST with SANCTION, for which high ("1") is also hypothesized to be "good."

The regression analysis is conducted for the 1,417 arrest transitions up through the first appearance at court (for the 309 subjects who were eventually referred to court) or through the end of the observed delinquent career (for subjects who were not referred to court prior to their last arrest before reaching the age of 17). As in all cases when PRIOR is included as a variable, the first arrest transition is omitted from the analysis.

A hierarchical analysis is presented, comparing the following models:

Model I: Background variables (AGE and PRIOR)

Model IIa: Background plus the court action variables (REFERRAL and SANCTION)

Model IIb: Background plus the the number of prior arrests (1/ARREST)

Model III: Background plus the actions plus the number of prior arrests

Model IV: Model III plus the interaction terms

We compared IIa with I, IIB with I, III with IIa, III with IIb, and IV with III, asking in each instance whether the additional variable(s) were significantly adding to the explained variance.

The results are shown in Table 3.4. Both sets of variables--court actions and number of prior arrests--added significantly to the variance explained by the background variables alone. The interactions did not.[3] The parameter estimates for Model III are shown in Table 3.5.[4]

TABLE 3.4
Hierarchical Analysis of Regression Models Testing the Effect of Station Adjustments on Impact of First Court Appearance

Model	R^2	F (df)		$p \leqslant$	Compared to	R^2, increment	F, increment (df)		$p \leqslant$
I	.107	85.07	(2, 1414)	.001	—	—	—	—	—
II a	.116	62.04	(4, 1412)	.001	I	.009	7.47	(2, 1407)	.01
II b	.142	78.01	(3, 1413)	.001	I	.035	58.07	(1, 1407)	.001
III	.151	50.26	(5, 1411)	.001	II a	.035	29.04	(2, 1407)	.001
III	—	—	—		II b	.009	7.47	(2, 1407)	.01
IV	.152	28.05	(9, 1407)	.001	III	.001	.40	(4, 1407)	NS

TABLE 3.5
Regression Analysis of the Effect of Station Adjustments on Impact of First Court Appearance

Variable	B	Standardized β	Standard Error	$p \leqslant$
Age in years	−.11	−.24	.01	.001
Prior arrest transition, in years	+.09	+.11	.02	.001
No. of prior station adjustments	−.21	−.18	.03	.001
Referral to court, no other action	+.01	+.01	.07	NS
Referral to court, with sanction	+.34	+.09	.09	.001
Constant	+ 2.28			

Dependent variable = TNA in years
Reference group = Subjects who were station-adjusted (not sent to court)

The results are noteworthy on several counts:

First, on the question that opened this discussion--does the number of station adjustments have an effect on the impact of the first court appearance--the answer is no. Interactions did not occur consistently enough to be of statistical significance.

Second, the analysis highlights the role of age and of number of prior arrests. Both are associated with powerful negative effects on TNA: The older the delinquent or the more prior arrests, the sooner he can be expected to be arrested again.

Third, a court sanction (supervision or probation) on the first appearance at court did have a major positive effect on TNA--slowed the time-to-next-arrest--but *referral to court without a sanction had no appreciable effect.*

Taken together, the results in Table 3.5 do much to explain the elite wisdom that sanctions do not deter, *from the point of view of a day-to-day observer of a juvenile court*. Two powerful effects--those of age and number of prior arrests--work to accelerate arrest rates. Often, they more than offset the effect of the court sanction. The result: From the judge's perspective, or the perspective of any observer of day-to-day events at court, the effect of the court's action appears to be nil.

We may illustrate this situation graphically. In Figure 3.2 below, we have taken the case of a youth whose last arrest transition was nine months long (the approximate average for the arrest transition leading to the first court appearance). He goes to court. How long can we expect to wait before he is arrested again? If longer than nine months, the court can be encouraged; if shorter than nine months, discouraged. We use the results of the regression model in Table 3.5 to predict the result. The curves represent a solution of the regression equation

$$\begin{aligned} TNA = &+2.28 \\ &-.11\ AGE \\ &+.09\ PRIOR \\ &-.21\ \ln(ARRESTS) \\ &+.34\ SANCTION \\ &+.01\ REFERRAL \end{aligned}$$

under the condition that PRIOR and TNA both equal .75 years. Separate plots are drawn for the case when a delinquent is referred, but no action is taken (REFERRAL = 1, SANCTION = 0) and when a delinquent is put on supervision or probation (REFERRAL = 0, SANCTION = 1).

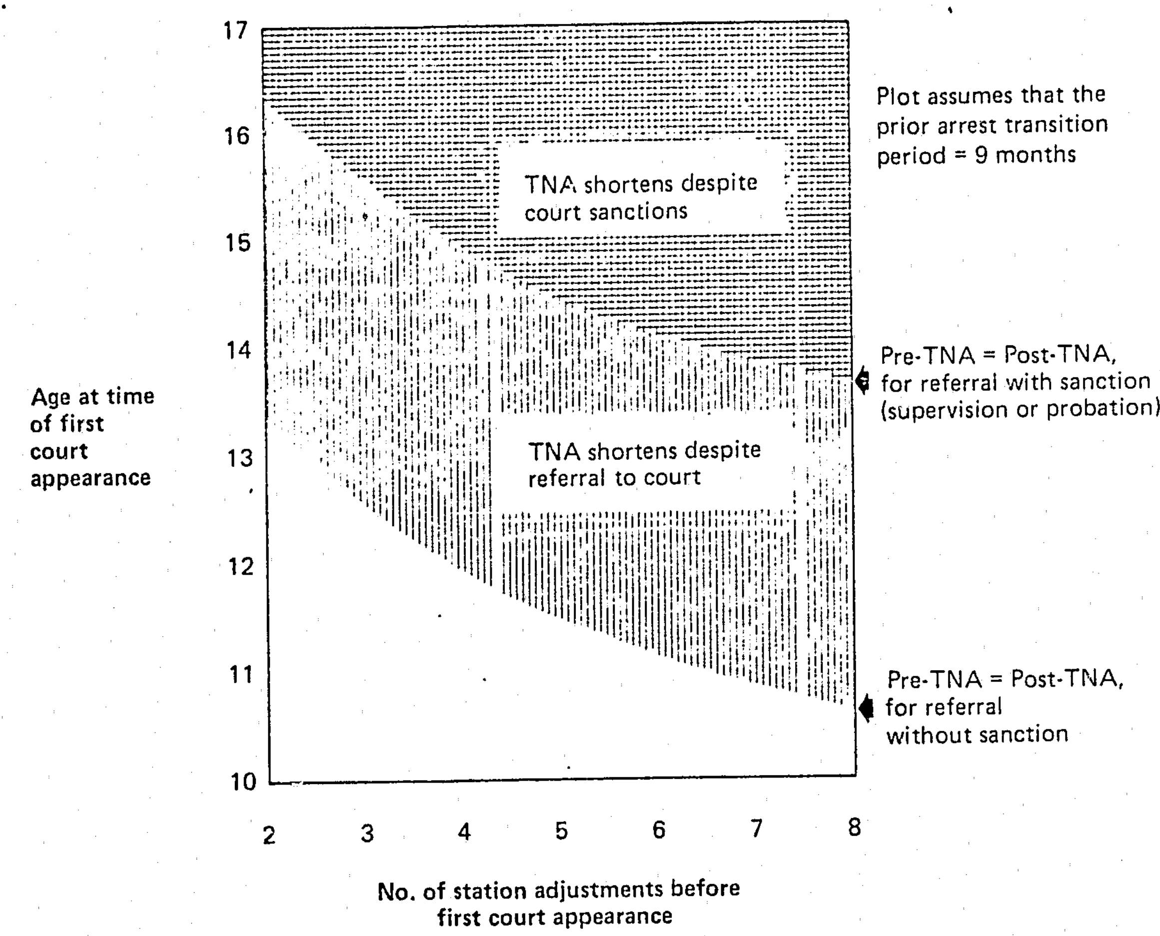


FIGURE 3.2 Indifference Curves: Effect of First Court Appearance

The shaded areas indicate combinations of age and number of station adjustments for which the results--from the court's perspective--look like some degree of failure, not success. As the size of the shaded areas indicates, the court sees apparent ineffectiveness under a wide variety of conditions. The longer the prior arrest transitions, the more frequently that this is the case.

The obfuscation of the impact of sanctions after the first court appearance is abetted by the distribution of court responses. Of the 171 youth who were rearrested after appearing in court, only 34 percent of them had been put on probation or supervision after the first court appearance.

Because referral to court without a sanction does *not* increase TNA, the effect is a situation in which, indeed, most delinquents who pass through the court's hands "get worse."

The Special Case of Court Referral After the First Arrest

To this point, all of the analyses have included only delinquents who were station-adjusted at least once before being sent to court (that being the only way that they could have a value for PRIOR, which was used as an independent variable). Now, we ask about the special case of those youth who were sent to court after the very first arrest. The expectations of deterrence logic are that the effect of a quick sanction (probation or supervision after the first arrest) will be enhanced by early action.

The results are again provocative. As indicated in Table 3.6, the means suggest that use of a sanction does moderately increase TNA over the no-sanction alternative, but that referral to court without taking action is worse than doing nothing at all.

TABLE 3.6
Relationship of Court Action to TNA in the Case of the First Arrest

Action after the first arrest	Mean TNA	N
Not sent to court	1.16 years	646
Referral to court, no action taken	.80 years	38
Referral to court, put on supervision or probation	1.38 years	42

Deterrence logic provides a ready explanation: For the youth who is not sent to court, the threat of court action

(once he is finally sent) is still intact. For the youth who is sent to court, without penalty, the threat of court action is to some degree debunked, and one of the operands in the deterrence calculus shifts to a lower value.

The regression analysis for this issue is based on a hierarchical procedure for the equation

$$\begin{aligned} TNA = {} & B_1 \text{ (constant)} + \\ & B_2 \text{ AGE} + \\ & B_3 \text{ REFERRAL} + \\ & B_4 \text{ SANCTION} + \\ & B_5 \text{ AGE x REFERRAL} + \\ & B_6 \text{ AGE x SANCTION} \end{aligned}$$

where the variables are defined as in the preceding analyses. The main difference is that in this analysis we must drop the "length of the prior arrest transition variable" (PRIOR), because the analysis is limited to the first arrest. The models compared in the hierarchical analysis are

Model I: Background variable (AGE)

Model II: Background plus court action variables (REFERRAL and SANCTION)

Model III: Model II plus the interaction terms

The results are shown in Table 3.7. In this case, the addition of the interaction terms did significantly improve the fit, and we therefore show the parameter estimates for Model III.

Interpreting the coefficients: The reference group in Model III is the set of youth who were arrested for the first time and sent home with no other action--by far the most common outcome, as shown in Table 3.6.

TABLE 3.7
Regression Analysis of the Impact of Alternative Reactions to the First Arrest

RESULTS OF THE HIERARCHICAL ANALYSIS

Model	R^2	F	(df)	$p \leq$	Compared to	R^2 increment	F, increment	(df)	$p \leq$
I	.194	174.54	(1, 726)	.001	—	—	—	—	—
II	.198	59.41	(3, 724)	.001	I	.004	1.82	(2, 722)	NS
III	.205	37.20	(5, 722)	.001	II	.007	3.18	(2, 722)	.05

PARAMETER ESTIMATES FOR MODEL III

Variable	B	Standardized β	Standard Error	$p \leq$
Age, in years	− .30	−.45	.02	.001
Referral to court, no other action taken	−4.24	−.68	1.59	.01
Referral to court, with sanction	+1.02	+.20	1.48	NS
Interaction, age with referral	+ .29	+.63	.12	.01
Interaction, age with sanctions	− .07	−.18	.11	NS
Constant	+5.22			

Dependent Variable = TNA in years
Reference Group = Subjects who were station-adjusted after the first arrest

If instead the youth were sent to court and a sanction (supervision or probation) was imposed, TNA was increased, but the increase is attenuated by an interaction with age. If we take literally the parameters estimated for this data set, the crossover point is reached at 14.6 years--before that time, sanctions after the first arrest increase TNA; after that, they are counterproductive.

To examine this situation further, Table 3.8 below uses the crossover point for both SANCTION and REFERRAL (14.6 years) as a break point, and shows the mean TNA for youth on either side of that line.

The lesson to be drawn from Table 3.8 is far different from the one to be drawn by extrapolation from a linear regression: The variance in effectiveness of alternative reactions to the first arrest is concentrated among the

TABLE 3.8
Mean TNA after the First Arrest, Broken Down by Age of Onset and Alternative Reactions

Age at First Arrest	Station Adjustment	Referral to court, no action	Referral to court, with sanction
Through 14.6 years	1.44 years	.91	1.60
(n)	(458)	(17)	(35)
Older than 14.6 years	.50	.56	.61
(n)	(201)	(10)	(7)

younger subjects; as the subjects get older, TNA drops no matter what court action is taken *if the action is limited to supervision or probation*. We emphasize the last remark, because it ties directly into the finding in our earlier work that a correctional intervention, whether institutionalization or some alternative residential treatment, is highly effective at all ages (Murray & Cox, 1979b: 69-74).

The final interpretation? Although it is true that a sanction after the first arrest is associated with a somewhat higher TNA than station-adjustment, the difference is not dramatic. Station-adjustment after the first arrest is an attractive alternative. The option to avoid is to send the youngster to court after the first arrest, then to do nothing further. This, it appears from the data, is worse than either the less severe or more severe alternatives. It seems especially important that the youth's first lesson from a court appearance not be that the system bluffs.

NOTES TO SECTION III

1. We will subsequently use "sanction" as shorthand for probation or supervision.

2. Probation and supervision are compared in analyses in other sections, when larger sample sizes can be employed.

3. Supplementary analyses confirmed that none of the interaction terms taken individually was significant.

4. Because the interactions of station adjustments with the court actions are not in the model, we code number of station adjustments in its natural direction. We do, however, continue to take the nonlinear characteristics of the variable into account, by using the natural logarithm of number of station adjustments rather than the raw figure. By way of comparison: the R^2 in Table 3.5, using the logged version of station adjustments, is .144; the same equation, using an unlogged version, yields an R^2 of .128.

Section IV
The Court Appearance History

In this section, we turn to the history of court appearances once the station adjustments have been left behind. The issue is whether the delinquent's response to court actions is affected by the patterns of those actions. Two main topics will be examined: *timing of the first court sanction* after the delinquent starts going to court, and the *continuing effects of supervision and probation* after they have been imposed.

THE EFFECT OF REPEATED COURT APPEARANCES BEFORE FIRST SANCTION

We have already observed that the effects of sanctions compete with the effects of increasing age and increasing numbers of arrests up to the point of first referral to court. Both of the latter are associated with subsequent acceleration of the arrest rate, an acceleration that often obscures the slowdown associated with the imposition of the first sanction. Now, we ask two new questions:

- o After the youth has started appearing at court, but before being put on supervision or probation, does TNA stabilize? Continue to decrease? Increase?
- o Does the potency of the eventual sanction diminish the longer the court waits?

We discuss each in turn.

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Pattern of TNA While Waiting for the Court to React

The first appearance in court marks a fundamental change in the youth's relationship with the juvenile justice system. Previously, the "system" from the youth's point of view consisted of the police station. The police could threaten; they could make dire predictions about what would happen; but the police could not *make* anything happen. And the agency that did have that power--the court--was still at one remove. By appearing in court the first time, the delinquent comes under the court's eye. Perhaps as importantly, the court comes into the delinquent's own field of view, quite tangibly. At this point, the issue is *whether the simple state of being in the court's eye tends to have a dampening effect on arrest rate*, because of an increased fear of subsequent punishment.

There is no single *a priori* expectation. Two main branches are possible, depending on the way that the delinquent thinks. This introduces a distinction between two pure types: the farsighted delinquent and the shortsighted delinquent. The farsighted delinquent draws inferences on the basis of long-term and cumulative probabilities. The shortsighted delinquent learns from very recent experience, and projects it into the very near future.

An examination of personality classifications of delinquents (e.g., Quay, 1972; Warren, 1971) quickly suggests that delinquents are more likely to be shortsighted than farsighted. But we spin out the logic for each type anyway, to make two points.

The first point is to show through this exercise how often the conventional approaches to measuring success in

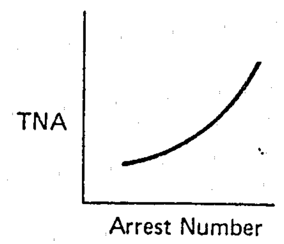
juvenile corrections have implicitly assumed farsightedness, as if that were the only relevant kind of rationality. Our exercise will confirm that, yes, certain types of effects, especially long-term ones, will only be produced by long-term, farsighted calculations. But it is a fallacy to assume that these are the only ways that deterrence can work. The fallacy is a variation on the argument that deterrence relies on rationality at all (Section II). This time, the error lies in the tacit assumption that rational calculation *must* consist of looking ahead further than the very nearest of futures.

The second point is to underscore the difference between shortsighted rationality and impulsiveness. If delinquent acts are wholly impulsive, occurring without regard to any prior incentives or disincentives, then deterrence must indeed fail. But if the delinquent's impulsiveness admits even the most primitive and recent experience into the calculation, then deterrence has a chance.

Let us apply these general remarks to the specific case of the delinquent who has appeared at court, has not been put on supervision or probation, and is now on the streets again. What does he do?

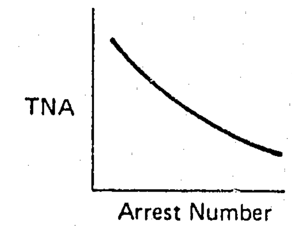
If he is farsighted, using long-term rational calculations, he will expect that the court must finally reach a breaking point and take action. Sooner or later, one more arrest will be one too many. Knowing that, the delinquent who thinks ahead will assume that each new arrest after that first court appearance is eating into the court's tolerance.

If we were dealing exclusively with this type of farsighted delinquent, the plot of TNA against arrest sequence numbers following the first appearance in court might be expected to look something like this:

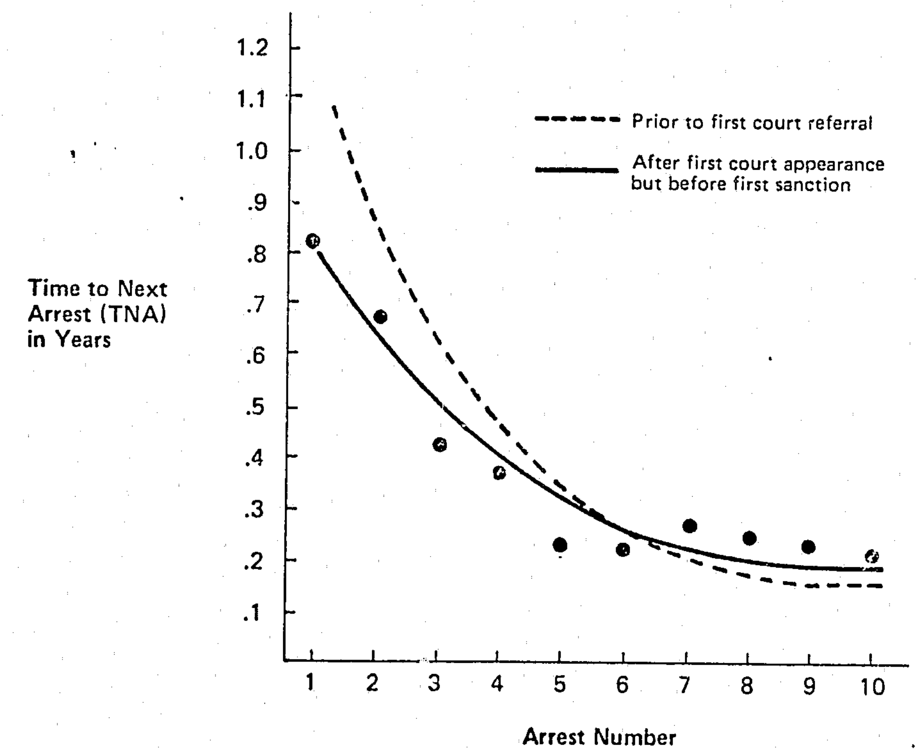


The farsighted delinquent can be reasonably confident that he can get away with one or two, perhaps three or four, more arrests. But the game is akin to Russian roulette: each new arrest uses up one of an unknown number of empty barrels, and makes it that much more likely that the next one contains the bullet. Consequently, the farsighted delinquent slows his offensive activity, and increases his TNA, as the number of arrests following first court appearance mounts.

If he is a shortsighted delinquent, a very different result is produced. Short-term rationality does not see arrests as using up empty barrels. Rather, each new arrest and court appearance uses a new gun, and each court appearance that does not produce a sanction is evidence that the number of barrels in each new gun must be very large. Each new instance is more evidence that there is nothing to worry about. For this type of delinquent, arrest rate should increase as the credibility of eventual punishment continues to diminish, and we should observe a plot very similar to the one that we found for repeated station adjustments (Figure 3.1); i.e.:



The actual results are shown in Figure 4.1, with a replication of the pre-referral trendline from Figure 3.1 (broken line) for purposes of comparison.



NOTE: Arrest number refers to entire arrest history, not to the sequence renumbered after first referral to court.

FIGURE 4.1 Time to Next Arrest for Arrests Occurring After First Referral to Court But Before First Sanction

As the plot makes clear, arrests did not slow as they piled up after the first court referral. The arrest rate increased. The delinquents who had been sent to court without sanction exhibited no evidence that they were anticipating eventual punishment if they persisted in being arrested. Being in the court's eye was not intimidating.

As a sidelight to Figure 4.1, note this additional view of the phenomenon noted in the preceding section, whereby referral to court without sanction at the very early stages of the offensive career is less effective than doing nothing at all. At the left-hand side of the plot, TNA was shorter for the post-referral delinquents than for their station-adjusted counterparts.

Potency of Sanctions After Waiting

Now the discussion shifts to the question: *Does the potency of sanctions diminish the longer that the court waits?* The logic of deterrence again provides two main branches, depending on whether probation and supervision are deterrents because of their *content as punishments* or because of their *symbolic importance* as milestones marking the approach to incarceration.

If probation and supervision are deterrents because of their content, then repeated appearances at court could plausibly have a numbing effect, and diminish the eventual impact of the sanction. The reasoning behind this point of view starts from the observation that probation and supervision are not in truth very punishing. They entail an occasional meeting with an officer of the probation division, and not much else. Repeated appearances at court should tend to familiarize the delinquent with court procedure, teach him how little supervision and probation need be

feared, and thereby defuse the impact of the sanction when it is finally imposed. Given this logic, we should observe that the effect of a sanction is reduced the longer that the court waits.

The alternative argument is that the deterrent effect produced by probation or supervision has nothing to do with their content, but with their symbolic importance. The real threat is incarceration, and the court sanctions produce fear that incarceration is close. In this case, the potency of supervision and probation is not diminished by familiarity with the court (because court is not what worries the delinquent), and the effect of waiting should be small.

Table 4.1 presents some summary data on the issue. The sample consists of all arrest transitions that occurred up through the imposition of a sanction or dropout, whichever came first. These transitions are divided into three subsets: those transitions that led directly to supervision, to probation, or to no court action. In each case, we examine the "before" and "after" TNA for the first court appearance compared to all other repeat court appearances.

There is very little in the breakdown in Table 4.1 to suggest that the potency of the first sanction diminishes with repeated court appearances. In the case of supervision, TNA for the "repeat-appearances" group is somewhat smaller than for the "first-time losers" (.82 versus .95), but the magnitude of the change, compared to the pre-sanction transition, is much greater because of the difference in the baselines. The same phenomenon applies, more dramatically, to the case of probation. TNA for the "first-time losers" is .83, almost double that of the "repeat-appearances" (.42), but again the baselines were quite different. We turn to analytic statistics for elucidation.

TABLE 4.1
Effects of Repeated Court Appearances on the Impact of the First Sanction

Court Action	Timing of First Sanction	N	Arrest transition immediately preceding court sanction	Arrest transition during which sanction was imposed	Change	
Supervision	First Appearance	47	.57	.95	+.38 yrs.	+66.7%
	Repeat Appearance	36	.19	.82	+.63 yrs.	+331.6%
	OVERALL	83	.41	.89	+.48 yrs.	+117.1%
Probation	First Appearance	13	.31	.83	+.52 yrs.	+167.7%
	Repeat Appearance	22	.17	.42	+.25 yrs.	+323.5%
	OVERALL	35	.22	.58	+.36 yrs.	+163.6%
No Action	First Appearance	104	.47	.44	–.03 yrs.	–6.4%
	Repeat Appearance	343	.33	.28	–.05 yrs.	–15.2%
	OVERALL	447	.37	.32	–.05 yrs.	–13.5%

To operationalize a test of the first-time loser hypothesis, we employ a regression of the form

$$\begin{aligned} TNA = {} & B_1 \text{ (constant)} + \\ & B_2 \text{ PRIOR} + \\ & B_3 \text{ AGE} + \\ & B_4 \text{ ARRESTS} + \\ & B_5 \text{ FASTACT} + \\ & B_6 \text{ SLOWACT} + \\ & B_7 \text{ AGE x FASTACT} + \\ & B_8 \text{ AGE x SLOWACT} \end{aligned}$$

where TNA, PRIOR, and AGE are defined as before. ARRESTS again represents number of prior arrests, expressed in its natural metric.[1] The two new variables are as follows:

FASTACT is a dummy variable coded "1" for a transition if the first sanction was imposed during it and that sanction was imposed at the subject's first court appearance

SLOWACT is a dummy variable coded "1" for a transition if the first sanction was imposed during it, but the sanction was imposed during a repeat court appearance.

The reference group is thus the set of arrest transitions that occurred after the first referral to court but before the imposition of a sanction.

In the hierarchical analysis, we begin by using PRIOR, AGE, and ARRESTS as the basic background variables, and test the alternative models as follows:

Model I: Background variables (PRIOR, AGE, ARRESTS)

Model II: Background variables plus the sanction variables (FASTACT, SLOWACT)

Model III: Model II plus the interaction terms.

The results are shown in Table 4.2. They speak to the importance (or lack of it) of acting rapidly.

The results indicate that acting on the first appearance at court has only a slight, statistically insignificant advantage over acting later. In either case, TNA increases by approximately a third of a year. The regression coefficients for FASTACT and SLOWACT are +.38 and +.33 respectively, a difference of only 18 days in TNA. Additional analyses were conducted that treated court appearance number as a continuous variable, rather than using the binary approach of FASTACT and SLOWACT. In no case did the court appearance number add significantly to the variance explained by the background variables and a yes/no sanction variable.

TABLE 4.2
Regression Analysis of the Effect of Repeated Court Appearances on the Impact of Sanctions

RESULTS OF THE HIERARCHICAL ANALYSIS

Model	R^2	F	(df)	p ≤	Compared to	R^2, increment	F, increment	(df)	p ≤
I	.162	39.18	(3, 607)	.001	–	–	–	–	–
II	.207	31.63	(5, 605)	.001	I	.045	17.26	(2, 603)	.001
III	.214	23.52	(7, 603)	.001	II	.007	2.69	(2, 603)	NS

PARAMETER ESTIMATES FOR MODEL II

Variable	B	Standardized β	Standard Error	p ≤
Age, in years	–.12	–.26	.02	.001
Prior arrest transition, in years	+.14	+.12	.04	.005
No. of prior arrests	–.02	–.13	.01	.005
Sanction at first court appearance (Yes = 1)	+.38	+.18	.08	.001
Sanction after repeated court appearances (Yes = 1)	+.33	+.15	.08	.001
Constant	+2.19			

Dependent variable: TNA in years
Reference group: Arrest transitions not resulting in a sanction
Sample: Arrest transitions from first court appearance through first sanction (or end of observation, if no sanction was ever imposed)

The main lesson of the analyses is that sanctions do have an effect regardless of when they occur in the sequence of court appearances. As in the analyses in the preceding chapter, the accelerating arrest rates associated with increasing age and increasing numbers of prior arrests mean that, from an outsider's viewpoint, it will appear that the sanctions do *not* have an effect (analogous to the situation depicted in Figure 3.2).

Interpretively, the results of the analysis are more consistent with the view of probation and supervision as deterrents because of their symbolic content rather than because they are punishing in themselves. It should be stressed that this is an interpretation, not a finding.

Other interpretations are possible; this is the one that seems most plausible to us.

THE CONTINUING EFFECTS OF SUPERVISION AND PROBATION

We have found that the initial imposition of a court sanction has an effect. Does it last?

The Conventional Analysis Revisited

We already know the basic answer from Section II: No. When we treat supervision and probation as continuing states, the results are negative (see Tables 2.1 and 2.2). After the subsequent analyses, we also know that the results in Tables 2.1 and 2.2 are contaminated. They are based on *all* arrest transitions that closed after imposition of the first sanction, including those during which a sanction was first imposed; and those initial arrest transitions appear to have been markedly different from transitions during which a sanction was not imposed. Thus, in the interests of a cleaner analysis, we replicate the analysis presented in Table 2.1, omitting arrest transitions during which either probation or supervision was imposed. The regression equation is

$$\begin{aligned} TNA = \; & B_1 \text{ (constant)} + \\ & B_2 \text{ AGE} + \\ & B_3 \text{ PRIOR} + \\ & B_4 \text{ ARRESTS} + \\ & B_5 \text{ SUPERVISION} + \\ & B_6 \text{ PROBATION} + \\ & B_7 \text{ AGE x SUPERVISION} + \\ & B_8 \text{ AGE x PROBATION} \end{aligned}$$

where

SUPERVISION is coded "1" if the opening arrest occurred after the subject had been put on supervision, and the closing arrest occurred before any other, more severe sanction had been imposed; and

PROBATION is coded "1" if the opening arrest occurred after the subject had been put on probation, and the closing arrest occurred before any other, more severe sanction had been imposed.

All other variables are as previously defined. Three models are tested:

Model I: Background variables (AGE, PRIOR, ARRESTS)

Model II: Background variables plus the treatment variables (SUPERVISION and PROBATION)

Model III: Model II plus the interaction terms

The results are shown in Table 4.3, and they provide ample support for the judges and probation officers who have observed the behavior of the youth who come before them and have reported that supervision and probation are ineffective. Their perceptions are consistent with the facts from Chicago. But let us be very precise about what those facts are. The usual statement is to the effect that

Youth put on supervision and probation are rearrested ster than ones who are not.

native wording is

who are put on supervision or probation slow rrests. If they are subsequently rearrested suffering additional sanctions, they are re- thereafter faster than youth of similar age t history who were not put on supervision or in the first place.

TABLE 4.3

Regression Analysis of the Continuing Effects of Probation and Supervision

RESULTS OF THE HIERARCHICAL ANALYSIS

Model	R^2	F	(df)	$p \leqslant$	Compared to	R^2 increment	F, increment	(df)	$p \leqslant$
I	.139	130.40	(3, 2427)	.001	–	–	–	–	–
II	.140	79.08	(5, 2425)	.001	I	.001	1.42	(2, 2423)	NS
III	.146	59.16	(7, 2423)	.001	II	.006	8.51	(2, 2423)	.001

PARAMETER ESTIMATES FOR MODEL III

Variable	B	Standardized β	Standard Error	$p \leqslant$
Age, in years	–.13	–.30	.01	.001
Prior arrest transition, in years	+.13	+.15	.02	.001
No. of prior arrests	–.02	–.16	.00	.001
On supervision (yes = 1)	–.61	–.37	.36	NS
On probation (yes = 1)	–1.60	–.81	.48	.001
Interaction, age with supervision	+.04	+.36	.02	NS
Interaction, age with probation	+.11	+.86	.03	.001
Constant	+2.36			

Dependent Variable: TNA in years
Reference group: Subjects who had not yet been (or never were) put on supervision or probation.
Sample: All arrest transitions except those during which a sanction was first imposed.

The former statement is the one that could be justified by the analysis in Section II, and is highly supportive of the elite wisdom for dealing with delinquents. The latter is the more precise statement we can make after approaching the data from the perspective of deterrence. Same data, slightly different questions, much different answers. The distinction is between the *event* of being put on supervision or probation and the *state of being on* supervision or probation. The former seems to have some positive effect; the latter seems to have some negative effect.

We now begin to work at a finer level of detail, *limiting the analysis to arrest transitions that occurred after the first sanction was imposed*, in an effort to determine whether, even within the general lack of effectiveness of "being on" supervision or probation, we can isolate some reasons why.

The Effects of Inaction

The most basic of all court responses to arrests after a sanction has already been imposed is another court appearance. We begin by asking what happens when even that most minimal of responses is not forthcoming. The issue is: *What is the effect on TNA when a post-sanction arrest does not result in a new court appearance* (but instead is station-adjusted)?

The expectations from the logic of deterrence are self-evident: one of the best, quickest ways to eviscerate the effect of a sanction should be failure even to take the youth back to court after a subsequent arrest. It is the ultimate statement that the court didn't really mean it. The more shortsighted the delinquent, the greater the negative effect.

The regression analysis consists of our usual three background variables (AGE, PRIOR, ARRESTS) plus a dummy variable (INACTION) coded "1" for arrest transitions during which no court appearance occurred, "0" otherwise. As usual, we also examine the interaction with AGE. The analysis (and the rest of them in this section) is limited to arrest transitions occurring after the imposition of the first sanction.

We test the standard three models (I: Background variables; II: Background variables plus the "treatment" variable; III: Model II plus the interaction term). The results are shown in Table 4.4.

TABLE 4.4
Regression Analysis of the Impact of Court Inaction Following First Sanction

RESULTS OF THE HIERARCHICAL ANALYSIS

Model	R^2	F	(df)	$p \leq$	Compared to	R^2 increment	F, increment	(df)	$p \leq$
I	.136	40.48	(3, 765)	.001	–	–	–	–	–
II	.151	34.03	(4, 764)	.001	I	.015	13.54	(1, 763)	.001
III	.155	28.04	(5, 763)	.001	II	.004	3.61	(1, 763)	NS

PARAMETER ESTIMATES FOR MODEL II

Variable	B	Standardized β	Standard Error	$p \leq$
Age, in years	–.04	–.13	.01	.001
Prior arrest transition, in years	+.15	+.20	.03	.001
No. of prior arrests	–.01	–.15	.00	.001
Court inaction (yes = 1)	–.11	–.13	.03	.001
Constant	+.99			

Dependent Variable: TNA in years.
Reference group: Arrest transitions during which a court appearance occurred.

The expectation is supported by the analysis: The court's failure to act is associated with a significant, negative regression coefficient (–.11), consistent with the causal interpretation that ignoring arrests speeds rearrest.

The interaction term of AGE and INACTION did not quite reach statistical significance (the F for the increment was 3.61; an F of 3.85 would have been significant at the .05 level), and we therefore followed the rules and presented the parameter estimates for Model II. But because the interaction was so close to meeting our criterion of significance, the reader may be curious to know how inclusion of the interaction term affects the pattern of coefficients. The parameter estimates for the background variables are essentially unchanged. The coefficient for the main effect of INACTION is much larger than in Model II: -.70, compared to -.11 in Model II. The coefficient for the interaction term is +.04. An examination of a breakdown of mean TNA for different age groups reveals that the source of the interaction is concentrated almost exclusively among the younger delinquents: inaction apparently has an especially large negative effect on them, whereas it diminished among the older offenders.

In reading Table 4.4, it is important to remember which set of observations is being analyzed. Earlier, we determined that, in comparison to every other set of observations we had examined, the arrest transitions occurring after the imposition of the first sanction looked like undifferentiated failure. Table 4.4 is an example of how the failure was being mediated by events. A first mediator of failure seems to be the court's subsequent lapse into inaction.

The analysis of court inaction is complicated by the activity of some of the delinquents in the sample: some were being rearrested so rapidly that it is not reasonable to expect the court to have acted in the interim. Including the length of the prior arrest transition during which no court appearance occurred (PRIOR) as an independent variable compensates for this artifact. But, as an additional check, it is useful to repeat the analysis. This time, we assume that two weeks is a reasonable time period within which the youth should have been taken to court. Rather than treating PRIOR as a continuous variable, we simply delete all values of less than .0384 years (two weeks). The results are consistent with the analysis presented in Table 4.4. The regression coefficients for the revised model are:

TNA = +1.31 (constant)
-.05 AGE
-.01 ARRESTS
-.08 INACTION,

with all coefficients significant.

Effects of Dismissals

The next candidate explanation for the failure of sanctions to retain a long-term effect is *the role of dismissals*, when a delinquent is brought back to court for a new arrest and the case is then thrown out. We are hampered in this inquiry by our ignorance about whether the dismissal indicates innocence. Presumably the effect of a dismissal when the allegation was genuinely wrong is different from the effect when the allegation was true but the charge was nonetheless dismissed for lack of evidence, procedural errors by the police, or other reasons. We proceed on the assumption that most of the dismissals were for reasons other than innocence, and see what happens; but the confusion in the meaning of "dismissal" remains.

We approach the problem through two analyses. One uses a strict definition of "dismissal," including only judgments which were logged in the court records as "dismissal without prejudice" or "no finding of delinquency." A second analysis uses a more inclusive definition that adds procedural appearances (filing of petitions) and extensions of existing states of affairs (extension of probation or supervision; suspended sentence) to the list of "quasi-dismissals." The variable is called WAFFLE, which suggests the dynamic we are trying to capture.

The analysis of dismissals strictly defined employs the usual three background variables in Model I, adds the "treatment" variable (DISMISSAL) in Model II, and the interaction of AGE with DISMISSAL in Model III. The results are shown in Table 4.5.

TABLE 4.5
Regression Analysis of the Cumulative Impact of Dismissals

RESULTS OF THE HIERARCHICAL ANALYSIS

Model	R^2	F	(df)	$p \leqslant$	Compared to	R^2, increment	F, increment	(df)	$p \leqslant$
I	.094	31.49	(3, 911)	.001	—	—	—	—	—
II	.095	23.78	(4, 910)	.001	I	.001	1.01	(1, 909)	NS
III	.101	20.42	(5, 909)	.001	II	.006	6.07	(1, 909)	.025

PARAMETER ESTIMATES FOR MODEL III

Variable	B	Standardized β	Standard Error	$p \leqslant$
Age, in years	−.08	−.21	.01	.001
Prior arrest transition, in years	+.10	+.10	.03	.005
No. of prior arrests	−.02	−.18	.01	.001
Cumulative no. of dismissals	−1.01	−1.12	.41	.025
Interaction, dismissals with age	+.07	+1.16	.03	.025
Constant	+1.53			

Dependent Variable: TNA in years.

It is an unusual set of results, and we are accordingly restrained about making much of them. Interpreted literally, the results suggest that number of dismissals has no impact on TNA *independently of age*. Without the interaction term (i.e., for Model II), the coefficient for DISMISSAL is only +.03, no larger than its standard error (also .03). Then, when the interaction term is added in Model III, both the main effect of DISMISSAL and the interaction term reach statistical significance, with regression coefficients which are large enough to be of substantive interest as well.

Interpretively, the results from Model III are consistent with other findings in the study. The coefficients indicate that dismissals have a large negative impact on TNA, with the largest occurring among younger delinquents. This is congruent with the earlier finding about the negative effect of "referral without sanction" for the earliest arrests.

We examined a breakdown of mean TNA, and mean changes in TNA, by age and number of dismissals, to see if the bivariate relationship would give us some leads about the reason for the regression results. It did not; to the extent that a relationship exists, its revelation requires the inclusion of the other variables in the regression equation.

When the more inclusive definition of dismissals (WAFFLE) is substituted in a parallel analysis, the same pattern of regression coefficients is obtained (-.16 for the main effect of dismissals, +.01 for the interaction with age), but neither the main effect nor the interaction term add significantly to the variance explained by the three background variables alone. When this is considered alongside the results in Table 4.5, it seems increasingly likely the "significant" interaction of DISMISSAL with AGE is a statistical curiosity. Whatever relationship does exist between dismissals and TNA is tenuous.

The Effects of Pending Business with the Court

The mills of the Cook County Juvenile Court tended to grind slow. A youth might be arrested two or three times while an earlier petition remained unresolved. The question we raise is: *What effects does the existence of "pending" business with the court have on TNA?*

Expectations of the data again take two branches defined by the same "farsighted" and "shortsighted" distinction used in earlier analyses. A farsighted delinquent who has an unresolved petition sitting on a judge's bench could plausibly reason that he had better avoid attracting the judge's attention with a new arrest, lest it tip the balance to a harsh decision. Result: The existence of pending business would be associated with lengthened TNA. A shortsighted delinquent would presumably interpret the court's indecision the same way that he interprets failure to take him back to court after a new arrest, as evidence of the court's toothlessness (see Table 4.3). Result: The existence of pending business would be associated with reduced TNA.

The analysis follows the pattern of the others in this section. The background variables are in Model I; the treatment variable (PENDING) is added in Model II, defined as the number of petitions awaiting disposition at the time of the opening arrest in the arrest transition; the interaction with AGE is added in Model III. The results are shown in Table 4.6.

Both the main effect and the interaction are significant, displaying a pattern which by now is becoming familiar. The "treatment" variable (the number of pending dispositions) tends to depress TNA, and the effect is most dramatic among the younger delinquents. As in the analysis of inaction by the court, the result is obtained from an examination of events--the post-sanction transitions--that look like undifferentiated failure when approached conventionally as a unitary "post-treatment" set. The results are consistent with the expectations of the logic of deterrence, with the expectations of the shortsighted delinquent, and with arguments that delinquents may become less shortsighted as they grow older.

TABLE 4.6
Regression Analysis of the Impact of Pending Business

RESULTS OF THE HIERARCHICAL ANALYSIS

Model	R^2	F	(df)	p ≤	Compared to	R^2 increment	F, increment	(df)	p ≤
I	.094	31.49	(3, 911)	.001	–	–	–	–	–
II	.098	24.77	(4, 910)	.001	I	.004	4.05	(1, 909)	.05
III	.103	20.89	(5, 909)	.001	II	.005	5.07	(1, 909)	.025

PARAMETER ESTIMATES FOR MODEL III

Variable	B	Standardized β	Standard Error	p ≤
Age, in years	–.07	–.20	.01	.001
Prior arrest transition, in years	+.10	+.10	.03	.005
No. of prior arrests	–.02	–.13	.00	.001
No. of pending dispositions	–.70	–.91	.29	.025
Interaction, age with "pending"	+.04	+.85	.02	.05
Constant	+1.44			

Dependent Variable: TNA in years

The Effects of Escalation

Supervision is considered a softer step than probation. A delinquent on supervision who is then put on probation is considered by the court to have taken a step up the ladder of sanctions, and is that much closer to institutionalization. Judges and probation officers typically try to convey that same thought to the delinquent. Do they succeed? *Does escalation of sanctions from supervision to probation reduce the arrest rate?*

Expectations are drawn from the same logic applied to the effect of the first sanction. If the *symbolic* importance of the escalation is important ("The next step is incarceration"), then the escalation ought to have an effect. If the *programmatic content* is important, then an effect is unlikely--

probation is very little different from supervision in its restrictions or demands.

The analysis is limited to the 44 members of the sample who experienced both sanctions. It employs the three background variables plus two dummy variables:

> TOPROBATION, coded "1" for the transition during which the youth went from supervision to probation, "0" otherwise; and
>
> ONPROBATION, coded "1" for transitions after being put on probation, "0" otherwise.

The interactions of these two variables with AGE are also included in the analysis. The reference group consists of transitions occurring while the delinquents were on supervision. For this analysis, four models are tested, so that we can distinguish between the explanatory power of TOPROBATION and ONPROBATION:

Model I:	The background variables
Model II:	The background variables plus TOPROBATION
Model III:	Model II plus ONPROBATION
Model IV:	Model III plus the interaction terms

The results are shown in Table 4.7. Each of the main effects significantly added to the explained variance whereas the interaction terms did not; following the usual practice, we therefore present the parameter estimates for Model III.

The escalation from supervision to probation increases TNA by a third of a year (+.33). Unexpectedly, the post-escalation period is also associated with an increase (+.15 years), though one that is less than half the size of the one-time shock of the escalation transition. This is the

TABLE 4.7
Regression Analysis of the Effects of Escalation from Supervision to Probation

RESULTS OF THE HIERARCHICAL ANALYSIS

Model	R^2	F	(df)	p≤	Compared to	R^2, increment	F, increment	(df)	p≤
I	.086	12.05	(3, 384)	.001	—	—	—	—	—
II	.147	16.48	(4, 383)	.001	I	.061	28.90	(1, 380)	.001
III	.186	17.47	(5, 382)	.001	II	.039	18.48	(1, 380)	.001
IV	.198	13.44	(7, 380)	.001	III	.012	2.84	(2, 380)	NS

PARAMETER ESTIMATES FOR MODEL III

Variable	B	Standardized β	Standard Error	p≤
Age, in years	−.04	−.19	.01	.001
Prior arrest transition, in years	+.12	+.18	.03	.001
No. of prior arrests	−.01	−.18	.00	.005
Escalation transition	+.33	+.30	.05	.001
After escalation	+.15	+.24	.03	.001
Constant	+.82			

Dependent Variable: TNA in years.
Reference group: Transitions occuring while subjects were on supervision.

only analysis in the study that reveals a continued positive effect in the post-sanction period.

Given a deterrence perspective, the most straightforward explanation for the results in Table 4.7 is that the delinquents who experienced the shift from supervision to probation did interpret the experience the way that judges hoped--as an ominous shift in status that put them in immediate danger of incarceration. That the reduction occurred during the one-time escalation transition is consistent with the earlier analyses of the effects of the first sanction. That the post-sanction arrest rate remained depressed suggests the possibility that the experience of escalation from one sanction to another had a quality not shared by the single sanction.

Perhaps the more important aspect of the results in Table 4.7 is their consistency with the preceding ones. In the cases of court inaction and pending business, the logic of deterrence leads to expectations that arrest rate will increase for those delinquents who are basing their calculations on the immediate past. When the data are examined, it is found that the arrest rate does increase. In the case of escalation, the same logic leads to expectations that the arrest rate will decrease. When the data are examined, it is found that the arrest rate does decrease. No inconsistent results emerged from any of the analyses--even the results from the "dismissals" analysis were consistent, though of dubious stability. And all of these results were obtained from the arrest transitions that occurred after the first sanction, and that, under initial examination, appeared to show the failure of sanctions. If no other conclusion is drawn from the exercise, it seems beyond dispute that the conventional approaches to testing for the effects of sanctions have been working at too gross a level of aggregation.

NOTES TO SECTION IV

1. The curvilinear nature of the relationship between arrest sequence number and TNA is concentrated in the first few arrests. In this section, dealing exclusively with transitions after the subject was referred to court, a linear relationship may be assumed without loss in explanatory power.

Section V
Conclusions

We started with the findings from a preceding study that tested correctional programs for juveniles and found that they reduced crime, substantially. The most parsimonious explanation for why they reduced crime was deterrence. The same study found that court sanctions (probation and supervision) had little long-term effect. But we were left with the question: Could deterrence have such a potent impact on institutionalized delinquents and yet be wholly inoperative for lesser sanctions? And so we undertook the investigation reported here, using a closer focus on short-term effects and applying the logic of deterrence as the basis for assessing results.

The findings may be summarized as follows:

1. If court sanctions (supervision and probation) are analyzed as "treatments" extending over a period of time, then the results show them to be ineffective. The state of "being on" probation or supervision is associated with faster rearrest than the un-intervened state.

2. If court sanctions are analyzed as one-time shocks, in which it is the event itself that produces deterrence, then the results show that a short-term effect is achieved. The immediate effect of a sanction is a substantial slowdown in time-to-next-arrest. Probation and supervision seem to work about equally well.

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3. The effects of imposing a sanction are not significantly affected by waiting. Even after several station adjustments, or after previous court appearances that did not result in a sanction, the main effect remains.

4. Being sent to court without a sanction has very little effect on rearrest rate, if any.

5. The effects of imposing a court sanction are well masked. Age and the number of prior arrests are both associated with increases in the arrest rate. From the perspective of an observer of day-to-day events in the court, most delinquents will look like they "get worse" after being put on supervision or probation. Only after the roles of age and prior arrests are factored out of the calculation does the slowing effect of court sanctions become apparent.

6. When the micro-dynamics of "being on" supervision or probation are examined, some differentiation in outcomes can be determined. Failure to take the delinquent back to court after rearrest and delay in reaching disposition of pending petitions are followed by faster rearrest. Escalation of sanctions from supervision to probation is followed by slower rearrest.

Throughout the analyses, the results are consistent with the expectations of the logic of deterrence, *as applied to a shortsighted person who is treating each new event in isolation*. Most of the analyses suggest that delinquents are most shortsighted when they are youngest--an outcome that seems reasonably applicable to adolescents in general. Further, age seems to mitigate the shortsightedness *despite the fact that increasing age is generally associated with increasing arrest rates*--an outcome that is consistent with the image of a delinquent who will get away with it if he can, but who, as he grows older, becomes increasingly calculating about the costs. And, while a review of the state of knowledge about the delinquent personality is beyond the scope of this study, we submit that the image of a delinquent (or any adolescent) who is shortsighted about consequences but not entirely impulsive is a plausible one.

Such are the main lines of the results. What are we to make of them when put in the larger context of causes and cures for delinquency?

We should start with the caution that, in most of the analyses, the variables we used were explaining about 20 to 25 percent of the variance, sometimes less. Or, to turn it around, the variables we used do *not* explain 75 to 80 percent of the variance. One legitimate reaction to this situation is that, as we always knew, delinquency is a very complicated phenomenon. Maybe deterrence plays a role, but it is a small one.

There is another perspective, however. That *any* deterrent effect could be detected for this sample of delinquents is surprising and warrants our intense interest.

Consider the circumstances. During the decade in which these adolescents were coming to the court's attention, institutionalization of juveniles from Cook County was plunging--from more than 1400 in 1966 to fewer than 400 in 1976. The average number of arrests before institutionalization was imposed was more than 13. Whereas the juveniles in our sample could not know these exact figures, they did know from observing their peers and their own treatment that the courts were putting up with a great deal, and acting very slowly.

The climate toward crime, police, and juveniles must also be remembered. The early 1970s were years of rising crime rates and polls that showed regard for the police to

be at an all-time low. Advocacy groups were actively and often successfully seeking to extend protection of the legal rights of juveniles, in the police station and in the courtroom. More generally, it was a time during which the stance that we have labeled "the elite wisdom" was most influential. Nationally, the legislation establishing the OJJDP was passed in 1974, setting out minimal intervention as Federal policy. At the same time in Illinois, key positions in the Department of Corrections, the Department of Children and Family Services, and the Illinois Law Enforcement Commission were filled by nationally known figures with a deep commitment to minimal intervention in general and to deinstitutionalization in particular.

We are not addressing the question of whether these events and attitudes were good or bad, right or wrong. They necessarily undermined the kind of environment that promotes effective deterrence. For, deterrence ultimately relies on trust in the future; specifically, trust that the system will react to wrongdoing with punishment. In the early 1970s, there was considerable debate about whether delinquents should even be said to have "done wrong," let alone whether they should be punished. And, in fact, punishment was very seldom imposed. The more observant and knowledgeable a delinquent was, the less trust he should have had. For deterrence to work in the early 1970s, it had to overcome many countervailing forces.

The policy implications of the findings are oddly obscure. They are obscure first because of the nature of the analysis. Unlike the suppression effect of institutionalization, discussed in the preceding studies, the effects of court sanctions are fragile. The suppression effect (a drop of roughly two-thirds in arrest rates following institutionalization) was large and extremely robust. A major effect remained in the face of a variety of efforts to attenuate it. In contrast, the effects we have discussed in this study are short-lived, and are detected only after controlling for other, competing factors that often drown out a bivariate relationship between the imposition of sanctions and the time to rearrest. Moreover, we approached the analysis from an explicit point of view, seeking to test some hypotheses about deterrence. The analyses do not unambiguously speak for themselves. The interpretations we have developed will be disputed. Major policy changes based on this developing, still-tentative knowledge are inappropriate.

Even if our knowledge were more secure, the policy implications would continue to be uncertain. Putting aside the difficult ethical and legal problems associated with changes in the administration of juvenile justice, large juvenile justice systems have only limited latitude for change. If the Cook County Juvenile Court decided to put all delinquents on probation after the third arrest (for example) it probably would have to increase its probation staff by a factor of two or three. If it decided routinely to institutionalize delinquents after five arrests (again, for example), the Illinois Department of Corrections would need money for a large construction program, along with new staff. A major change in correctional policy for juveniles requires radical and often expensive changes in many interlocking components.

From a policy standpoint, then, the findings of this study do not comprise a call to action. From a research standpoint, however, we hope that the findings will stimulate more work. If this study can claim one unequivocal, established finding, it is that past work on the effects of court sanctions has missed a lot.

Despite the caveats, we expect that subsequent research will tend to confirm and elaborate the dynamics we have identified. The data base from Chicago was large and representative of big-city delinquency; there is no reason to believe that the story it told will be wildly different from the story in other large cities around the nation. If we are right, what are the long range implications?

No matter how much we protest that the findings are not a basis for locking 'em up, the central, inescapable theme of the analyses in this study and its predecessors is that sanctions--punishments--work. The most effective sanction appears to be the most severe--incarceration, either in a traditional institution or some other, perhaps more humane, type of residential facility. The preceding work indicated that the environment need not be harsh, the duration need not be long, but the experience of having been held involuntarily in custody for some appreciable period of time seems to have an effect. Happily, it appears that the *threat* of such a sanction also can be made to work. But if that threat is to be credible, the analyses suggest that the system must clearly, convincingly, consistently, and briskly enforce rules that involve steadily escalating sanctions.

We are at the limit of what data can do to inform the choices. In effect, our message is twofold.

The first message is a pessimistic one: Those who reject punishment as a means *and* want to reduce juvenile crime are going at it the hard way. Many of the current prescriptions for dealing with juvenile crime make very little sense when read side-by-side with the evidence in this and the companion studies. They may make sense as needed services for disadvantaged youth, or as appropriate

legal protections. If implemented on a broad enough scale, long enough, perhaps they can alter some of the conditions that promote juvenile crime. But few of the current prescriptions take advantage of the forces that seem to affect delinquent behavior among youngsters who are standing before the judge's bench right now.

The second message is guardedly optimistic. The findings on the effects of institutionalization that prompted this study have analogs in the findings on the effects of supervision and probation. The data in both instances portray delinquents behaving in a way that most of us recognize in ourselves--responding to carrots and sticks. To that extent, the juvenile justice system has more leverage on the behavior of delinquents than many have thought. To maximize that leverage, the philosophical underpinnings of current "best practice" in juvenile justice would have to be displaced several degrees to the right, but the potential for leverage seems to be there.

If the message is optimistic in its implications for an effective juvenile justice system, it is also optimistic in its implications about delinquents. The news that a delinquent can be affected by the prospect of punishment is really no more than saying that he makes choices about his future. The news may make it more difficult to think of delinquents exclusively as "troubled youth" driven by forces beyond their control. On the other hand, the news makes it easier to treat delinquents more like people with minds of their own and less like hapless wards.

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