THE LIMITS OF BAIL REFORM: A QUASI-EXPERIMENTAL ANALYSIS

ROY B. FLEMMING*
C. W. KOHFELD
THOMAS M. UHLMAN

This study of bail reform in a large urban court employs an interrupted time-series research design spanning a six-year period, and examines case data for over 38,000 defendants. Two reforms—creation of a pretrial release agency and adoption of deposit bail—are evaluated to determine whether they had statistically significant impacts on the likelihood of pretrial release, the probability of financial losses for defendants, and the avoidance of bondsmen in the pretrial process. Weighted regression analysis was used to compensate for the possibility of autocorrelation in the time-series data. The reforms proved a mixed success. Explanations for these findings emphasize the role of previous policy patterns and procedural rules in restraining the scope of bail reform and the political forces encouraging the use of bail for preventive detention.

Guilt is the link between crime and punishment. Before someone accused of a crime can be denied freedom or forced to bear other penalties, the state is expected to establish legal culpability (Packer, 1968). Prior to conviction this link is formally absent, yet large numbers of criminal defendants suffer significant pretrial punishment. They undergo incarceration, incur financial losses, and find their liberties abridged in various ways through court proceedings intended only to assure their appearance in court. Concern over fundamental injustices in pretrial defendant treatment has spurred widespread reform in bail practices over the past 15 years.

Since an apparently auspicious beginning with the Manhattan Bail Project in New York City in the early 1960's, these bail reforms have attempted to change the ways criminal defendants are released prior to trial and lessen the costs associated with financial bail. Most large cities have

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established some form of pretrial service reform modeled along the lines of the original Vera Project. Several states, starting with Illinois, as well as a few local courts, have adopted rules allowing the use of 10 percent deposit bail rather than traditional surety bonds when financial restraints are deemed necessary to assure a defendant's appearance in court. Efforts aimed at documenting the effectiveness of these reforms dominate the literature, but unfortunately the findings are inconclusive largely because of inadequate research designs and methodologies (National Center for State Courts, 1975).

This study employs a quasi-experimental research design and large data base to measure the impact of recognizance and deposit bail reforms in a metropolitan criminal court. In this regard it follows the traditional concerns of the literature, although it tries to avoid the methodological weaknesses of previous research. However, it also raises new questions about bail reform. The findings suggest that by failing to consider the conditions under which their proposals are most likely to succeed, reformers have overlooked the very real limits of the reforms and the extent to which arguments for change obscure the political realities inhibiting reform.

I. BAIL REFORM IN METRO CITY

Metro City, located in the northeastern region of the United States, has a unified two-tiered court system.[1] An arraignment court sets initial bail in all criminal cases, has jurisdiction over misdemeanor and minor felony charges, and conducts preliminary hearings for felony cases with maximum sentences exceeding two years. When bound over for prosecution, more serious felonies are heard in the trial court, which also reviews and hears petitions for bail changes.

Eight months after creating a pretrial release agency in June 1971, the Metro City court instituted a 10 percent deposit bail option for defendants. The pretrial release reform followed the standard Vera format of verifying defendant backgrounds, recommending those who were qualified for recognizance, and assuring their appearance in court by maintaining telephone or mail contact with defendants during the pretrial period. The deposit bail reform, however, was unique. Surety bonds were not eliminated by legislative edict; nor were judges allowed to decide when defendants could post bail through the court.

[1] Guarantees of anonymity were required by court officials before the data analyzed here were released. For a more general description of both the city and the court, see Uhlman (1979: 27-44).

Instead, if financial bail was required as a condition for release from custody, defendants were given the choice of either depositing 10 percent of the bail amount with the court (with the provision that 90 percent of it would be refunded after final deposition if they faithfully appeared in court), or paying a nonrefundable premium to a bondsman to post a surety bond for their release.

Several factors contributed to these reform efforts in Metro City. During the early 1960's, an investigation revealed that some arraignment court judges were being recompensed by bondsmen for steering defendants to them. An activist public defender organization took advantage of the scandal to push for bail reform. Overcrowded jails, a riot, and subsequent litigation gave additional impetus to their efforts, and the court's procedural rules were flexible enough to allow both reforms to be phased in without excessive delay.

The goals of the reforms were discussed by the court's former chief judge in a recent interview[2]:

> These reforms were intended, in part at least, to increase the number of defendants released prior to trial. This was a question of simple fairness since we had an acquittal rate of 40 percent or more. You wanted to keep people out of jail who shouldn't be there or people who would eventually be acquitted. Bail reform was also intended to remove bondsmen from the court; they were scum. Finally, even though it wasn't number one on my hit parade, the reforms were expected to reduce the cost of release. Overall, I think the program was very successful. I'm very proud of it.

II. RESEARCH DESIGN AND METHODOLOGY

The bail reforms in Metro City are evaluated using data drawn from the trial court's computerized case histories of the entire felony population over a six-year period extending from July, 1968 through June, 1974. After omitting cases with coding errors, inconsistencies, or missing data for relevant variables, the final data set included 38,158 defendants whose cases were disposed of in the trial court.[3]

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[2] Personal interviews lasting approximately one-half to one hour each were conducted during the summer of 1978 with several key officials in Metro City including the former chief judge, former arraignment court administrator, the head and his assistant of the pretrial services division, the master appointed to oversee the jails, and the chief public defender. Interviews with the judge, prosecutor, public defender, and pretrial personnel assigned to conduct bail hearings during this time, along with observations of the proceedings, also were carried out. Finally, newspaper reports from the court, prosecutor's office, and pretrial release division for the six-year period were thoroughly reviewed.

[3] The Metro City court has one of the most sophisticated computer-based recordkeeping and case retrieval systems in the country. While the files are still active they are coded and rechecked several times so that mistakes are corrected and missing data problems are minimized. As a result only about 6 percent of the total number of defendants processed by the court over the six-

The basic research approach adopted is the "single group-multiple intervention" design (Glass, *et al.*, 1975: 121). The two reforms are treated as separate interventions in an ongoing policy process in which the measured observations over time are the outcomes of this process for defendants. Any abrupt change in the level or slope of these observations occurring shortly after the reforms is considered, at least initially, to be the result of their intervention (Campbell and Stanley, 1963; Cook and Campbell, 1976; Glass, *et al.*, 1975).

The individual case histories are aggregated by month and quarter to generate two time series with 72 and 24 observations, respectively. The larger monthly series includes all crimes, while the quarterly series is composed of nine separate felony crime types. Quarterly groupings are necessary because, even with this large data set, relatively small numbers of defendants appeared in individual crime categories each month.[4] Two problems with this second time series are that it is more difficult to distinguish the impact of the two reforms when they are separated by only three time periods and the number of observations falls short of the minimum of fifty recommended by Glass, *et al.* (1975: 112). Since the monthly time series is not

year period were deleted because of coding errors or inconsistent and questionable codes. Since the omitted cases were randomly distributed over time and across various charge categories, this procedure did not produce systematic bias in the final data set. It should be noted that missing data problems occurred regarding bail amounts for 7,024 defendants, all of whom were detained at the time of disposition. Bail amounts, however, were available for another 7,634 similarly situated defendants. Cases without bail data were only slightly more serious than those with this information coded. The missing data do not create problems in assessing the effects of reform on the release rates. Nor is using bail amount as an independent variable risky, since charge severity differences between the two prisoner groups are small and the missing data are randomly distributed over time.

[4] The court administrator's definitions were used to group specific criminal charges into nine general crime categories to obtain workable samples. Some sensitivity was lost by grouping related, but not identical, felonies. The following table indicates the total number of defendants accused of crimes within each of the nine major categories and the monthly and quarterly average number of cases.

Crime Category	Total Defendants	Monthly Average	Quarterly Average
Assault	5,756	80	240
Burglary	7,419	103	309
Drug	6,054	83	252
Emb./Fraud/Forgery	1,337	18	56
Homicide	1,517	21	63
Larceny	6,878	95	287
Rape	890	12	37
Robbery	5,207	71	217
Weapons Offenses	3,100	43	219
	36,158	529	1,589

burdened by these problems, it forms the foundation for the study.

Five dependent variables, expressed as rates of release, parallel the chief judge's expressed goals for the reforms: increasing the likelihood of pretrial freedom, minimizing the probability of permanent financial losses for defendants, and eliminating bondsmen from the pretrial process. Table 1 shows the operational definitions of these variables. The effects of reforms are determined by their influence in altering or shifting the time series of these variables.

Table 1. Operational Definitions of Variables

	Dependant Variables	Definition
Y_{1t}	Overall Release Rate	Proportion of total number of defendants released at o_i
Y_{2t}	Surety Release Rate	Proportion of total number of defendants released on surety bail at o_i
Y_{3t}	Cash Bail Release Rate	Proportion of total number of defendants released on cash bail at o_i
Y_{4t}	Financial Bail Release Rate	Proportion of total number of defendants released on surety or cash bails at o_i
Y_{5t}	Recognizance Release Rate	Proportion of total number of defendants released on nonfinancial bail (recognizance) at o_i
	Reform Related Variables	
X_{1t}	Time Variable	Counter variable for monthly observations takes on values 1-72, measures pre-reform slope
X_{2t}	Pretrial Release Reform	Dummy variable where o_i prior to reform are scored as 0 and after reform as 1, measures short-term effects—level changes
X_{3t}	Long-Term Effects of Pretrial Release Reform	Scored as 0 prior to reform and 1, 2, . . .n for all months after reform measures post-reform slope change
X_{4t}	Deposit Bail Reform	Dummy variable where o_i prior to reform scored as 0 and after reform as 1, measures short-term effects—level changes
X_{5t}	Long-Term Effects of Deposit Reform	Scored as 0 prior to reform and 1, 2, . . .n for months after reform, measures post-reform slope change
	Control Variables	
Z_{1t}	Charge Severity	Mean maximum possible sentence of the primary charge against each defendant at o_i
Z_{2t}	Bail Amount	Mean financial bail amount for each defendant regardless of pretrial status at o_i

The overall release rate measures the proportion of defendants freed at the time of case disposition irrespective of type of release. In constructing the other variables, seven release options used with varying frequency by Metro City are collapsed into three general pretrial release methods. The surety release rate indicates how often bondsmen participated in the release of defendants. The cash bail release rate includes three release options (of which deposit bail was the most common) that permit defendants to post nonsurety bonds with the court, thus eliminating both the need for bondsmen and the loss of bail premiums.[5] These two rates are then combined to form the financial bail release rate to measure the proportion of defendants released on financial bail of any type. Finally the recognizance release rate indicates the proportion of defendants freed through bail options involving either no monetary requirement or only a token amount.[6]

The basic questions this study seeks to answer are: Did the institution of pretrial release and deposit bail significantly change the pretrial status of felony defendants in Metro City? Were changes the result of the reform or merely due to chance? More formally, were changes in the level and slope of defendant outcomes after the reforms statistically significant? The following basic model from Cook and Campbell (1976) is used to address these questions.

$$Y_{it} = b_o + b_1X_{1t} + b_2X_{2t} + b_3X_{3t} + e_t \quad (1)$$

where:

Y_{it} = monthly (quarterly) observations of the various pretrial status variables

X_{1t} = a counter for months (1-72) or quarters (1-24), the number of observations

X_{2t} = a dichotomous variable scored 0 for observations before the reform and 1 for observations after the reform

X_{3t} = a counter of months (quarters) which has the value of 0 for observations before the reform and 1, 2, 3, . . . for observations after the reform

e_t = error

The parameters to be estimated are b_i. Parameters b_o and b_1 estimate respectively the level and slope of the pretrial status variable over time before reform. Reform effects are

[5] Of the three bail options in this category, 5,687 (80.6 percent) were released under the deposit bail plan. Another 1,077 (15.3 percent) posted the full face value of the bail, and 286 defendants (4.1 percent) received a "real estate bail" in which the value of real property was pledged by the defendants to obtain their release.

[6] In this category 1,506 defendants (37.7 percent) were coded by the court as released on their own recognizance, with 2,024 (50.7 percent) released on "nominal bail" ($1) and 468 (11.7 percent) who "signed their own bail." Although the last type of release may involve a nontrivial bail amount in the event of a forfeiture, no transfer or pledge of money was required for the defendants to obtain their release.

observed in the estimates for b_2 and b_3. Parameter b_2 estimates change in the level of the variable after reform, and b_3 estimates post-reform change in the slope. Significant changes in these parameters, therefore, indicate short-term and long-term effects of reform.[7] The appropriate null hypothesis of no change is $b_2 = b_3 = 0$.

Defendant outcomes are likely to be sensitive to factors such as the bail amounts set by the court or the types of cases appearing on the docket. For example, changes unrelated to the reforms may occur over time in the gravity of crimes with which defendants are charged. Changes in these factors may confound the influence of the reforms. Such fluctuations represent "change[s] in experimental unit composition" (Glass *et al.*, 1975: 62) and require control. This is done by incorporating the appropriate control variables described in Table 1 into the basic model which then takes the following form:

$$Y_{it} = b_o + b_1X_{1t} + b_2X_{2t} + b_3X_{3t} + b_4Z_{1t} + b_5Z_{2t} + e_t \quad (2)$$

where:

Z_{1t} = mean maximum possible sentence of primary charge against each defendant at o_i (charge severity)

Z_{2t} = mean financial bail amount for each defendant at o_i

The coefficients of interest are still b_2 and b_3. The null hypothesis of no change remains $b_2 = b_3 = 0$ where significant changes in the coefficients indicate the extent of reform effects after controlling for confounding variables.

Autocorrelation frequently plagues interrupted time-series analyses, resulting in inefficient coefficient estimates if ordinary least squares estimation is used. Durbin-Watson tests for autocorrelation proved inconclusive for these data. The consequences of incorrectly accepting the null hypothesis of no autocorrelation, however, are much more serious than if it were incorrectly rejected (Johnston, 1972: 258). Therefore it was rejected *a priori*, and an alternative estimation technique was used.

A first-order autocorrelation disturbance was assumed and a simple two-stage procedure adopted (Johnston, 1972: 258-266; Hanushek and Jackson, 1977: 150-157). The model incorporates no lagged endogenous variables; therefore, ordinary least squares produces a usable estimate for the autocorrelation

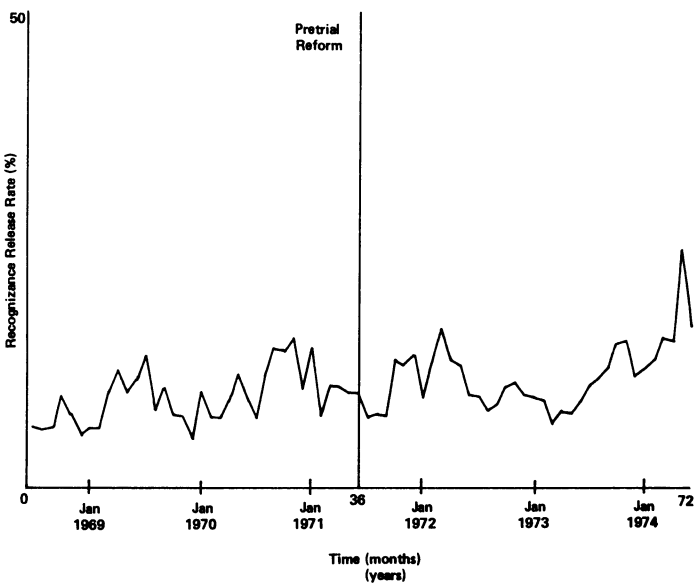
[7] It should be pointed out that the model was tested for multicollinearity by excluding time counters from the model and comparing the results. Only when the time counters were quarters rather than months did the results differ enough to suggest they were the artifacts of multicollinearity. For this reason the model was not used to determine the effects of the reforms for individual charge categories.

coefficient ρ. The original observations are transformed by appropriate use of the known parameter ρ, and then ordinary least squares estimation procedures are applied to the transformed data. This method, sometimes called weighted regression, is simple because it can be done using a standard computer program. Its estimates are equivalent to those produced using the preferable generalized least squares procedures. The results reported in this study are based on weighted regression procedures. T-tests are used to determine the statistical significance of the coefficients.[8]

III. THE FIRST REFORM: PRETRIAL SERVICES AND RECOGNIZANCE RELEASE

Metro City's release-on-recognizance rates over 72 months are plotted in Figure 1. The trend line indicates that the pretrial services reform produced no visible change in pretrial release rates. The figure also shows that recognizance release was not unknown in Metro City before the reform. While release rates fluctuated and were relatively modest, they nonetheless averaged nearly 10 percent a month preceding the creation of the pretrial services agency. After this reform was instituted, rates dipped briefly, then rose, only to decline again

Figure 1. Change in Recognizance Release Rates: July 1968-June 1974

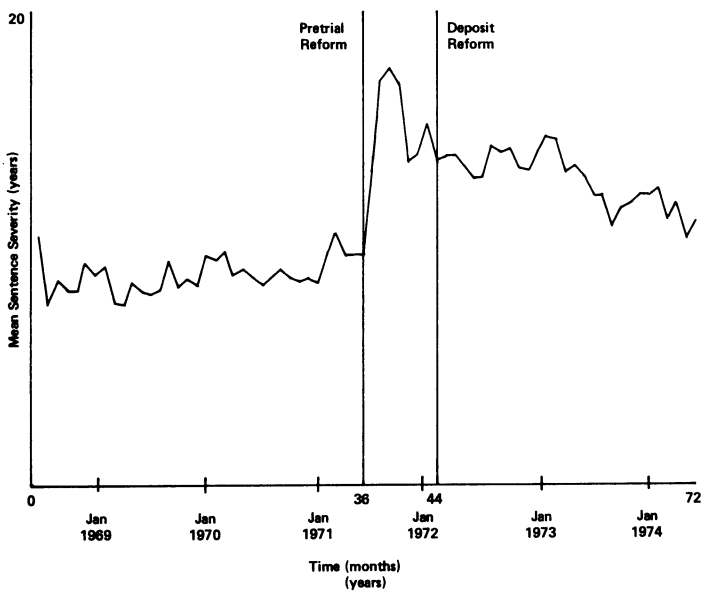


[8] The coefficients reported are the adjusted coefficients obtained from the autoregressive procedure in SAS.

before climbing during the final thirteen months of the time series. For the post-reform period the monthly rate was 11.6 percent—an increase of just 2 percent over the pre-reform average.

Appearances, of course, can be deceiving. The apparent absence of reform effects may be misleading if other conditions influencing the court's bail decisions changed so as to mitigate or obscure the impact of reform. Charge severity, measured as mean maximum possible sentence for charged offenses, is such a factor.[9] Court officials, responding to increasingly serious docketed offenses, may have granted recognizance release less often because defendants were viewed as having stronger incentives to avoid prosecution, as constituting graver threats to the community, or possibly both. Figure 2 indicates that changes in charge severity did occur in Metro City during this time.

Figure 2. Change in Mean Sentence Severity Measured in Years: July 1968-June1974



The mean possible maximum sentence for the defendants on each month's docket fluctuated little before the pretrial reform, but shot up abruptly right afterward, peaked, and slowly fell throughout the post-reform period. No upward movement coincided with the beginning of the second reform,

[9] Charge severity is based on an average of the maximum possible sentences of the criminal charges originally lodged against defendants each month or quarter, not the sentences actually meted out upon conviction. Legislated maximum sentences are used. The approach to determine the principal charge in multiple-charge cases is discussed in Uhlman (1979: 39-40).

however. A comparison of mean charge severity for the periods before and after the pretrial services reform indicates the magnitude of this change. The post-reform average exceeded the pre-reform average by 50 percent (13.2 years *v.* 8.8). Near the end of the time series, however, this difference shrank to the point that the average was roughly comparable to the mean shortly before the pretrial services reform.

The fact that after the first reform the average case was much more serious could be an unanticipated consequence of reform, but temporal sequence is not conclusive evidence of causality. In fact, the relationship in Metro City is coincidental. For a number of years the trial court had been burdened by a growing backlog of untried cases. To correct this problem, the jurisdiction of the arraignment court was expanded to include felony cases with maximum sentences from two to five years; heretofore these cases had been prosecuted before the higher bench.

This change took place during the summer of 1971, and as a result the trial court docket, while smaller, now included a larger proportion of defendants accused of the most serious felonies. The monthly data directly reflect this change. But in shifting cases to the arraignment court, the already heavy workload of the lower court in handling less serious cases and arraignments became even more onerous. Furthermore, convicted felony defendants who were displeased with their sentences in the arraignment court retained the right to *de novo* appeal in the trial court, and they began to exercise this option with increasing frequency. As a result, the docketing change broke down and the higher court unofficially resumed its responsibility for less serious felony cases. Again the data show the effects of this second shift as the monthly mean maximum sentences began to fall soon after the ill-fated change was instituted.[10]

In equation (3) the recognizance release rate is the dependent variable with the pretrial services reform incorporated in the model as described in equation (1). In equation (4) charge seriousness (mean maximum sentence) is included as a control variable.

$$\begin{array}{lcccccccc} Y_{5t} & = & 7.2 & + & .125X_{1t} & - & 2.45X_{2t} & + & .017X_{3t} \\ & & (4.56^{**}) & & (1.69) & & (-1.26) & & (.16) \end{array} \qquad (3)$$

$R^2 = .15 \quad \rho = .48$

[10] It was not feasible to analyze only those charges unaffected by the jurisdictional change. To do so would have reduced prohibitively the size of monthly case samples at several points and the quarterly samples throughout the time series.

$$Y_{5t} = \underset{(4.6^{***})}{12.9} + \underset{(2.3^{*})}{.16X_{1t}} + \underset{(.24)}{.54X_{2t}} - \underset{(-.65)}{.067X_{3t}} - \underset{(-2.4^{**})}{.70Z_{1t}} \quad (4)$$

$R^2 = .23 \quad \rho = .45$

$^{*}p < .05 \quad ^{**}p < .01 \quad ^{***}p < .001$

The values below the parameter estimates are t-ratios; R^2 is the coefficient of multiple determination; and ρ is the first-order autocorrelation coefficient.

The results of equation (3), which tests the effects of reform, confirm visual inspection of the data. Creation of the pretrial services agency only marginally affected recognizance release rates. Indeed the short-term drop in release rates after reform, indicated by the coefficient of X_{2t} (-2.45), is the opposite of what bail reformers might expect. However, the coefficient is not statistically significant. The short-term effect becomes positive when charge severity is controlled in equation (4) (.54). The change in slope measuring the long-term effect of the reform (coefficient of X_{3t}, .017) is positive in equation (3) but again not statistically significant. When the control variable is included, the sign for this coefficient becomes negative, meaning that release rates after reform were not increasing as fast as they had previously. On the basis of these findings the slight improvements in recognizance release rates after the creation of the pretrial services agency could have occurred by chance.

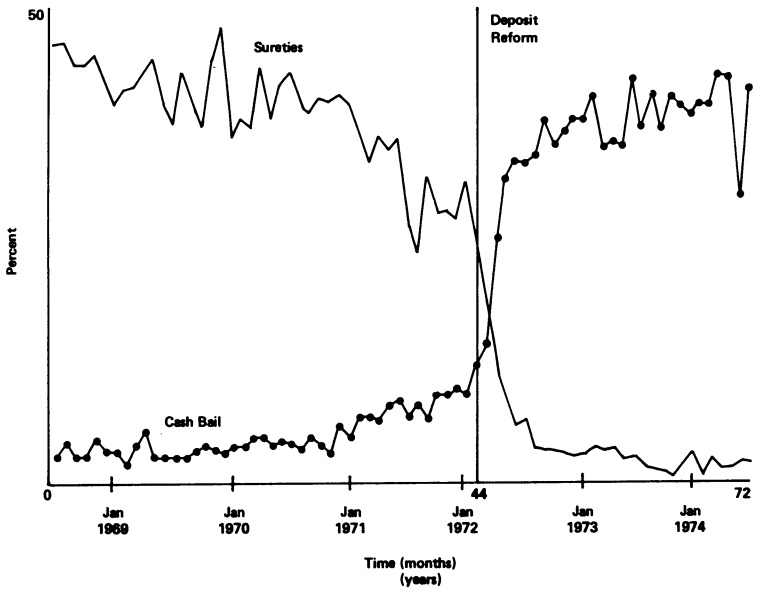
IV. DEPOSIT BAIL REFORM IN METRO CITY

Two key factors can be expected to determine the success of Metro City's unusual deposit bail reform. First, defendants could benefit from the reform only if bondsmen were dislodged from their intermediate position between detention and pretrial liberty. This is a necessary but not sufficient condition dependent on the choices made by defendants. Even if they unanimously chose to purchase their freedom through the court, reform effects could be blunted by changes in the bail amounts defendants were required to post, and these decisions remained in the hands of the courts. Analysis of this reform focuses first on the decisions made by defendants after they were given the opportunity to post bail with the court, second on the court's decisions regarding bail amounts, and third on the question of how the reform affected the release rates for defendants required to post financial bail as a condition for their release.

The Demise of the Bondsman

When given a chance to post bail with the court, defendants overwhelmingly chose the court as their "bondsman." Figure 3 displays the monthly proportions of defendants released through sureties or through one of the court's cash bail options which after reform was usually 10 percent deposit bail.

Figure 3. Change in Use of Bondsmen and Cash Bail: July 1968-June 1974



Patterns for the two release rates provide striking evidence of the reform's impact. Surety release rates plummeted after reform, while the cash bail rate soared. Six months after its start, defendants had almost eliminated the bondsman from the pretrial release process. Prior to the reform, 46 percent of the defendants on average had gained their freedom through the intercessions of bondsmen, compared to only 6 percent on various nonsurety bail options. These proportions were promptly reversed after the reform; only 6 percent of the defendants continued to employ a bondsman, while 43 percent took advantage of the deposit bail option.

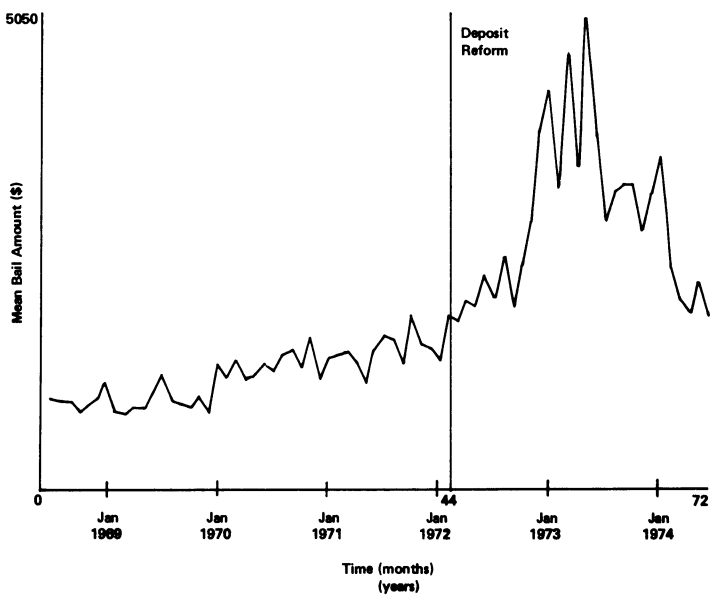
Clearly the first condition for success came about. This change was a major accomplishment in and of itself, since defendant monetary losses regularly incurred under the traditional surety bail system were drastically curtailed.

The Price of Pretrial Liberty

Regardless of the type of financial bond required for pretrial liberty, a defendant's chance for release still greatly depends on the size of the bond, since he or she must be able to raise the money for the premium or deposit. If this prerequisite cannot be met, the difference between bail types matters little. Consequently, the effect of the second reform on release rates hinges on whether bail amounts remained stable or perhaps declined.

In the period prior to the second reform, monthly mean bail amounts for all defendants with some form of financial bail, whether released or jailed, averaged about $1,200. But Figure 4 also shows a gently rising trend as the average rose from $1,000 in the first month of the time series to $1,500 by the time the reform took effect. This trend continued after the reform, but suddenly changed in late 1972. During the first six months of 1973, bail averages fluctuated wildly between $3,000 and $5,000. As abruptly as they rose, the amounts started to decline. By the end of the time series in 1974, they had returned to the level observed at the time of the deposit reform. For the entire post-reform period, the average for the monthly means was $2,820 or 135 percent above the earlier level.

Figure 4. Mean Bail Amount in Dollars: July 1968-June 1974



To what extent can these changes be attributed to the deposit reform itself? Or are they due to changes in the

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severity variables? A comparison of Figures 2 and 4 suggests that changes in bail amount are not explained by changes in charge severity. Mean maximum sentences rose seventeen months before the abrupt increase in mean bails. Moreover, by the time bail amounts started to rise, mean maximum sentences were fairly stable, and afterwards as the amounts rose and then fell, mean sentences declined. The following equations support this visual evidence:

$$Z_{2t} = 874.8 + \underset{(1.62)}{84.6}Z_{1t} \qquad (5)$$

$R^2 = .04$

$$Z_{2t} = 328.3 + \underset{(1.22)}{61.8}Z_{1t} + \underset{(.90)}{12.9}X_{1t} + \underset{(2.0^*)}{790.9}X_{4t} + \underset{(.17)}{4.82}X_{5t} \qquad (6)$$

$R^2 = .36 \quad \rho = .61$

$^*p < .05$

Charge severity is associated with only 4 percent of the variance in bail amounts over time, and the t-test fails to reach statistically significant levels. Of greater interest is the strong relationship between the reform variables (X_{4t}, X_{5t}) and bail amounts while controlling for mean sentence severity (Z_{1t}). The significant short-term effect, indicated by the bail increase of $791 after reform, strongly suggests these increases reflected the negative response of judges, particularly those in the arraignment court, to the reform. Comments by the chief judge provide some insight into this reaction. When asked why the average bail amounts rose after the reform, he commented, "I think it was a knee-jerk reaction of the arraignment court judges who didn't like the 10 percent reform thrust down their throats in the first place." Though he was not completely certain, he went on to speculate that the six- to nine-month delay in escalating bail amounts might have been due to an initial attempt to limit the 10 percent deposit reform. It originally included only drug charges and was later extended to all felony offenses. "We watched it closely the first couple of months and that may be the reason for the delay," he remarked. According to the former administrator of arraignment court the reason for the jump in bail amounts was not complex:

> It's very simple. Judges did not want certain defendants to get on the streets. This is especially true after the preliminary hearing where the judge was aware of the crime and had a good idea whether he thought the defendant was dangerous . . . . It was clear to judges that the new reform made bail easier to raise for defendants, and if a judge wanted

to keep a certain defendant in jail he had to raise bail and many judges did that.

But why did bail amounts begin to fall after their steep rise? We suspect that in large measure it was due to the support the higher court gave the deposit reform, plus the fact that defense attorneys and the pretrial services agency petitioned the trial court to review bails set in the lower court shortly after arraignment. The number of petitions filed "where bail is considered unreasonably high," according to the court's annual report, averaged 78 per month in 1973 but dropped to 44 a month during the first half of 1974 as bail amounts declined. The number of petitions reached its high water mark of 183 in May, 1973; a year later it shrank to 35. While information for 1972 was not available, efforts by arraignment court judges to prevent the release of defendants by setting higher bail amounts apparently were mitigated to some degree by the trial court.

Deposit Bail and Financial Release Rates

Deposit bail reform achieved its goal of removing bondsmen from the pretrial release system. At the same time bail amounts rose dramatically. Given these divergent patterns, the relationship between reform and release rates for defendants with financial bail is not likely to be clear-cut. In fact, two contradictory results might be expected: (1) generally the financial bail release rate should increase with elimination of the bondsmen; but (2) in the short run the release rate may decline or fail to increase very much because of the jump in bail amounts. Equation (7) lends some support to these expectations.

$$\begin{aligned} Y_{4t} &= \underset{(42.5^{***})}{58.6} - \underset{(-4.9^{***})}{25X_{1t}} - \underset{(-.16)}{.33X_{4t}} + \underset{(3.91^{***})}{.39X_{5t}} \\ &\quad - \underset{(-.70)}{.00056Z_{2t}} \qquad (7) \\ R^2 &= .43 \quad \rho = .09 \\ ^{***}p &< .001 \end{aligned}$$

The trend before reform was slightly negative with proportionately fewer defendants released through financial, mostly surety, bail as indicated by the coefficient of X_{1t} (-.25) which is statistically significant though very small. Also slightly negative (and insignificant) is the short-run effect of deposit bail reform (coefficient of X_{4t}, -.33). Over the longer term, however, the reform's effect is positive and significant as shown by the coefficient of X_{5t} (.39). When the coefficients for the slopes (X_{1t}, X_{5t}) are added, the overall trend for the time

series is slightly positive. Surprisingly, equation (7) indicates that bail amounts had almost no impact on release rates.

Figure 5. Financial Release Rate: July 1968-June 1974

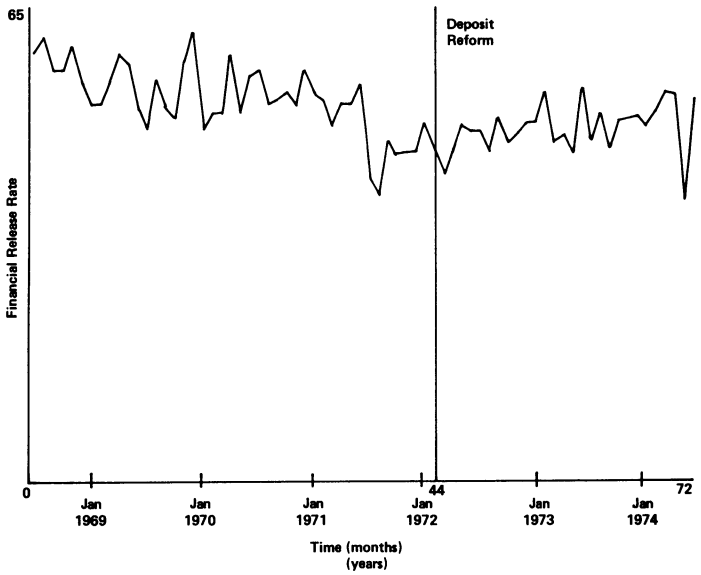

Figure 5 displays financial release rates plotted over time. Comparing it with Figure 4 shows that the change in release rates did not coincide with fluctuating bail amounts as equation (7) also suggests. On balance it appears that deposit reform barely improved the opportunities of defendants to gain their release from custody and that variable bail amounts had little effect on changing release rates.

This curious, but not totally unexpected, situation has no ready-made explanation. For many defendants, particularly the indigent and those with few social ties in the city, the key to pretrial liberty may have been lower bail amounts, which, of course, did not occur. Thus, reform meant little to them. On the other hand, deposit bail may have facilitated the release of some defendants able to afford the surety premiums whom bondsmen otherwise would have refused to take as customers. As will be discussed shortly, this was indeed the case for defendants charged with certain crimes. It is possible that the negligible relationship between bail amounts and release rates means that defendants who customarily can post financial bonds are able to pay various prices for their liberty. Although the post-reform average bail amount was about $1600 higher than the pre-reform average, the amount defendants had to deposit with the court was 10 percent of this difference, or

approximately $160. For many defendants this additional sum may not have been any more prohibitive than previous bail levels. Thus they were able to gain their release at rates roughly comparable to those prevailing before the sudden jump in the amounts of bail.

V. THE IMPACT OF BOTH REFORMS ON THE OVERALL RELEASE RATE

No surprising results are expected when examining the overall release rate, since it is simply a composite of the financial and recognizance rates. However, this brief analysis is valuable in emphasizing the marginal impact of both reforms.

Figure 6. Overall Release Rate: July 1968-June 1974

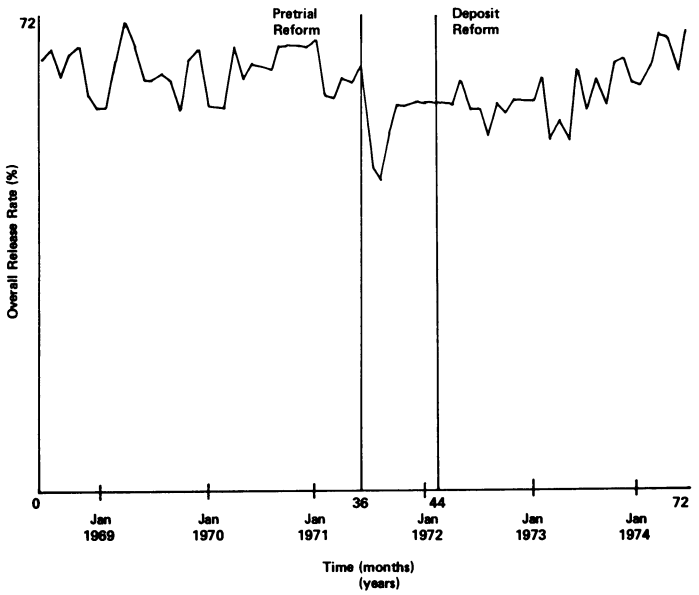

Figure 6 reveals almost no improvement in the overall chances for pretrial freedom in Metro City after its reforms. Release rates prior to the first reform fluctuate considerably, but no trend is apparent. The drop in rates following the first reform probably reflects the change in court jurisdiction discussed earlier. During the year following the second reform, the rates show some stability, but they are still lower than during the pre-reform period. Coincident with the gradual decline in bail amounts and the increase in recognizance release during the final months of the time series, the overall

release rate haltingly climbs and ultimately reaches the level that existed during the three years *before* both reforms.

Regression equation (8) adds further weight to the conclusion that the reforms did little to improve pretrial release opportunities in Metro City.[11]

$$\begin{aligned} Y_{1t} = {} & \underset{(22.4^{***})}{78.6} + \underset{(.92)}{.057X_{1t}} - \underset{(-1.06)}{3.36X_{2t}} + \underset{(1.73^{*})}{.96X_{3t}} - \underset{(-.90)}{2.52X_{4t}} \\ & - \underset{(1.43)}{.83X_{5t}} - \underset{(-3.87^{***})}{1.55Z_{1t}} - \underset{(-2.67^{**})}{.0020Z_{2t}} \end{aligned} \qquad (8)$$

$R^2 = .55 \quad \rho = .13$

$^{*}p < .05, \ ^{**}p < .01, \ ^{***}p < .001$

The short-term effects of both reforms (coefficients of X_{2t}, -3.36, and X_{4t}, -2.52) are negative and not statistically significant when controlling for charge severity (Z_{1t}) and bail amounts (Z_{2t}). The net long-term effect (adding coefficients of X_{3t} and X_{5t}) is slightly positive (.13), which means that after the short-term decreases there is a very modest improvement in the proportion of released defendants. Simply stated, Metro City's oft-praised and much-heralded reforms failed to significantly improve the chances for pretrial freedom of felony defendants in this city.

VI. THE DISTRIBUTIONAL IMPACTS OF THE REFORMS

Until now the possibility that reform effects were distributed unequally among felony defendants has been ignored. Yet it is possible, and perhaps even likely, that one or both reforms benefitted certain defendant groups more than others. Persons accused of homicide or robbery, for example, might have been able to take advantage of the deposit bail reform while those charged with less serious felonies were better able to exploit new recognizance release opportunities. These distinctions were ignored initially so that general patterns could be observed and also to insure that each monthly observation would have a reasonably large number of defendants. Quarterly observations are now required in order to analyze individual crime categories.

In reducing the number of observations, however, a price is paid. The gap of eight observations in the monthly time series between the two reforms is sliced to only three quarterly observations, and disentangling the individual reform effects becomes more difficult. Accordingly, this within-crime analysis focuses on each reform and its relevant release rates. The reduction in the total number of data points, moreover, means

[11] Results similar to those in equation (8) were observed where the two reform effects were tested separately.

that estimates of model parameters become less efficient. Thus very straightforward difference-of-means tests before and after the reforms are used to test for significant changes in release rates in each crime category. Because of these problems, the following analysis should be considered exploratory.

An examination of recognizance release rates before and after the pretrial service reform for the nine felony crime types reveals statistically significant increases for the following six crimes: burglary, embezzlement/fraud/forgery, homicide, larceny, robbery, and weapons offenses (see Table 2). For robbery and homicide, however, few defendants were granted this form of release either before or after the reform. As a result, slight absolute increases in the rates show up as relatively large (and hence statistically significant) proportional increases. For the other crime types—burglary, embezzlement/fraud/forgery, larceny, and weapons—reform had both substantive and statistically significant effects. In burglary cases recognizance release rates rose an average of 7 percent after the first reform. Embezzlement/fraud/forgery cases show an increase of 12 percent, and larceny and weapons cases increased an average of 5 percent and 9 percent, respectively (all statistically significant at the .001 level).

Table 2. Mean Recognizance Release Rates Before and After Pretrial Service Reform By Crime Categories (in percent)

Crime Category	Mean Release Rate Pre-Reform	Mean Release Rate Post-Reform	% Difference
Assault	16.6	16.3	−0.3
Burglary	4.4	10.9	6.5***
Drug	10.8	11.0	0.2
Em/f/forgery	12.7	24.5	11.8***
Homicide	0.9	2.8	1.9*
Larceny	11.4	16.4	5.0*
Rape	1.8	2.7	0.9
Robbery	1.3	3.9	2.6***
Weapons	10.5	19.9	9.4***

*t-test statistically significant .05
***t-test statistically significant .001

The pretrial services reform, then, appears to have aided some defendants more than others. In general, the impact centered on property crimes and weapons cases, while for felonies involving personal injury or its threat the reform had a minimal effect or none at all. The major reason for this difference rests in the fact that defendants charged with murder, rape, robbery, aggravated assault, and assault with intent to kill were not considered eligible for recognizance by

pretrial services and were not recommended for release to the court.

Bondsmen may have been a major barrier blocking the release of defendants accused of serious crimes in Metro City. If they held personal aversions to defendants charged with particular crimes or demanded collateral in these cases, the offender's opportunity for pretrial freedom would be narrowed. Because reform swept away these hurdles, the financial release rate would be expected to increase as long as defendants with serious felonies could make bail. Statistically significant increases in financial release rates are observed for defendants indicted for homicide, rape, and robbery. Particularly noteworthy is the rise in financial release rates for these three crime types even though their mean bail amounts leapt upward following the second reform. Table 3 displays the change in average quarterly mean bail amounts and the differences in financial release rates in each crime category.

Table 3. Changes in Bail Amounts and Financial Release Rates by Crime Type

	Mean Bail Amounts			Mean Financial Release Rate		
Crime Category	Pre-Reform	Post-Reform	Change (%)	Pre-Reform	Post-Reform	Change (%)
Assault	$1151	$2219	93%***	58.5	58.4	−0.1
Burglary	$1175	$1777	51%**	44.5	46.2	1.7
Drug	$1140	$3172	178%***	61.6	64.5	2.9
Em/F/F	$ 928	$1064	15%	56.6	51.7	−4.9*
Homicide	$3706	$9154	147%**	34.0	44.2	10.2***
Larceny	$ 761	$1362	79%***	51.6	44.6	−7.0*
Rape	$2451	$4288	75%**	50.9	56.3	5.4*
Robbery	$2375	$4367	84%***	30.0	37.5	7.5***
Weapon	$ 845	$1856	120%***	69.0	52.8	−16.2

*t-test statistically significant .05
**t-test statistically significant .01
***t-test statistically significant .001

One of the most striking relationships in this table is the 10 percent increase in the homicide release rate despite a 147 percent jump in average quarterly mean bail amounts. Rape and robbery cases display similar patterns, although to a slightly lesser degree. The rape release rate rose by 5 percent as the average quarterly bail amount increased by 75 percent. Mean bail amounts in robbery cases went up by 84 percent, while the corresponding release rate moved upward by 8 percent. Another interesting finding is that embezzlement/fraud/forgery, larceny, and weapons rates

declined after the second reform with weapons cases experiencing an especially sharp drop of 16 percent.

These findings suggest two major conclusions. First, bondsmen evidently refused to accept as customers defendants charged with homicide, rape, or robbery at rates commensurate with their ability to pay their surety premiums since, despite sharply higher mean bails during the post-reform period, financial release rates for these defendants nevertheless increased. Removal of the bondsmen, then, eliminated a middleman whom Metro City judges, particularly lower court judges, correctly assumed would leave certain defendants in jail although they had the financial wherewithal to post surety bonds.

The second conclusion pertains to the drop in release rates for the embezzlement/fraud/forgery, larceny, and weapons crime categories. It is important to recall from Table 2 that these crime types benefitted from the pretrial release reform. Recognizance reform frequently encounters the complaint that its criteria for release select only the "cream" of defendants (Thomas, 1976: 147-150). While the study data do not squarely address this criticism, the drop in financial release rates (second reform) and the rise in recognizance rates (first reform) for these crimes may indicate that as more defendants were released on recognizance those not recommended for this release were less financially able to post a cash bail, particularly when faced with the mounting bails required by the court.

VII. DISCUSSION

Did bail reform fulfill its promise in Metro City? Answers to this question vary with the perspective used to evaluate the reforms. From one vantage point, deposit bail reform was a success. The legitimacy and fairness of financial bail in Metro City improved considerably with the demise of bondsmen as brokers of pretrial freedom. Bondsmen, according to the chief judge, were "scum" who represented a legacy of corruption from the bail scandal in the early 1960's. Inauguration of the refundable deposit bail program ended the financial losses suffered by defendants when posting surety bonds. With a single stroke this reform made pretrial release in Metro City less corruptible and less costly for felony defendants.

From another perspective, the conclusions are far from sanguine. No sizable increases in recognizance release followed the advent of pretrial release reform. Nor did deposit

bail produce much improvement in the release of defendants with financial bails. Both of these rates and the corresponding overall rate of release showed a stubborn and perturbing resistance to rise after the reforms.

The aggregate analysis supporting these conclusions, however, concealed several important consequences of the reforms. First, reform benefits in terms of release rates were distributed unevenly across crime categories. Pretrial release tended to favor less serious felonies, such as larceny, while defendants with more serious charges, like homicide, benefitted from the deposit bail program. Second, for certain crime categories the first reform interacted with the second to produce declines in financial release rates.

Crime severity strongly affected the use of recognizance release in Metro City, primarily because pretrial services excluded from consideration for recognizance those defendants charged with serious crimes against persons. The effects of this rule show up in the low recognizance rates for crime types corresponding to this list of offenses (with the exception of the burglary category which includes both residential and commercial burglaries). When recognizance reform first began in New York City, this "excluded offense rule" was intended to protect the fledgling reform from politically motivated accusations that released defendants would avoid prosecution and endanger the community. The rule's survival and popularity since 1961 (Thomas, 1977: 74) suggest that it continues to perform this defensive function. Little empirical evidence, however, supports the notion that defendants charged with excluded offenses are either less likely to appear in court or more likely to commit additional crimes if they are released (Thomas, 1976: 220-248). Indeed, an evaluation of Metro City's program, done at the agency's request in 1977, concluded:

> Many defendants are now denied eligibility for consideration for release on recognizance because of the type of charge. However, the analysis performed through this evaluation suggests that the charge is not a factor in predicting bail risk (failure to appear).

Nor has the issue of whether such defendants recidivate more often during the pretrial period been settled (see, for example, Angel *et al.*, 1971). Empirical answers, however, may be less crucial than political realities. As the chief judge recalled, Metro City residents were worried that bail reform would jeopardize their safety. Furthermore, shortly after the start of the pretrial services program, its director emphasized to the local media that the program would not put dangerous

criminals back on the streets. The excluded offense rule, then, helped assuage public apprehensions and reduced some of the political risks attending the release of felony defendants on their own recognizance.

Another consequence of the pretrial service reform, in this instance stemming from its selection criteria, is that it reduced the financial release rates for particular crime categories, especially larceny and weapons offenses, even though it increased their recognizance rates. Clearly this conclusion is confounded by changing bail amounts. Still, if defendants who failed to meet the agency's recognizance criteria had weaker social and economic resources, they also may have been less capable of coping with the alternative, deposit bail. An apt example of this problem can be found in the embezzlement/fraud/forgery crime category. Bail amounts here were more stable than in other crimes, rising from $928 to $1,064, a comparatively modest 15 percent. Yet the average financial release rate for defendants with these charges dropped from 57 percent to 52 percent at the same time that the recognizance rate climbed from 13 percent to 25 percent. Thus, to the degree that recognizance standards emphasize employment history, length of residence, and social ties (which is the case in Metro City and other cities), they tend to screen out precisely those defendants who are most likely to have difficulty making financial bail in the first place.

The negative consequences of the first reform could have been mitigated if judges had set bail amounts within the reach of defendants not recommended for recognizance release. However, the chief judge admitted that reducing the cost of release did not have a high priority; it was not "number one" on his "hit parade." Bail reductions, of course, did not take place. Instead substantial increases occurred soon after the second reform (see Figure 4). Bail petitions and pressure from the higher court finally brought soaring bail amounts under closer control, because arraignment court judges had violated traditional norms defining "reasonable bail." In addition, the decline in financial release rates undoubtedly aggravated the overcrowding problem in the city's jails.

A possible explanation for the limited effect of reform on overall release rates is in order at this point. Metro City historically has been a "low bail-high release" city. In 1962 it had one of the largest proportions of cash bails set under $1000 and one of the highest felony release rates of the large cities in Silverstein's (1966) national survey of local pretrial practices.

A recent study by Thomas (1976: 38) confirms Silverstein's 1962 findings and also shows that in 1971 Metro City continued to have one of the lowest detention rates of the twenty cities in his study. Moreover, although bail amounts varied greatly between 1968 and 1974 as shown earlier, Metro City's overall mean bail of about $1,800 in 1972 compares very favorably with Baltimore's mean of $7,292, Chicago's average of $3,244, and Detroit's mean of $2,812 for this same year (Eisenstein and Jacob, 1977: 197). Regardless of the factors behind Metro City's "low bail-high release" tradition, it poses a major problem for testing the efficacy of bail reform.

Perhaps neither reform could increase overall release rates because the maximum feasible level of release already had been reached. Detained defendants include many who are too poor to post any kind of money bail, the presumed clientele of bail reform. But they also include possibly many more who are likely to be too dangerous in the court's eyes to release and who are bad recognizance release risks from the pretrial agency's viewpoint. In short, the city's previously high release rates may have left less room for change than proponents of reform anticipated—unless, of course, the reforms also sharply altered definitions of what constituted a "poor" defendant, a "dangerous" person, or a "bad risk." But these changes do not appear to have occurred. Consequently, as long as these definitions went unchallenged, the most that could be expected is that reform would only alter the traditional ways of releasing defendants who normally would have been freed anyway. The data strongly support such an argument. Despite the reforms, little improvement in overall release rates took place, although the form of release changed dramatically with deposit bails replacing surety bonds.

VIII. CONCLUSIONS

Bail reform in Metro City was neither a complete success nor a total failure. How can we explain these mixed results? To what extent are the findings affected by the likelihood that Metro City may not be typical or representative of other large urban courts? Equally important, how does this study of reform illustrate the more general problems of legal change in criminal courts? Answering these questions would be simpler if the phenomenon of planned legal change, encompassing the related processes of innovation, implementation, and institutionalization, were clearly understood. Regrettably it is not. As a consequence, we will rely greatly on Nimmer's (1978)

conceptual framework for investigating reform and system impact in criminal courts to guide our discussion. According to Nimmer (1978: 27):

> An implemented reform stimulates change by altering aspects of the judicial process. The extent and nature of any resulting change will vary according to the characteristics of both the reform and the system to which the reform is applied.

Two key characteristics of legal reforms are the extent to which a constituency promoting change is involved in the judicial process and whether the reforms are optional or mandatory. The second characteristic is a question of how much discretion judges have regarding decisions that are the target of planned changes (Nimmer 1978: 47-48).

When reforms are legislatively mandated or the result of appellate court rulings, the existence of a change-oriented constituency in local courts is problematic and often critical to whether the reform produces any impact at all. A major feature of most bail reform efforts, particularly recognizance reform because of the example set by the Manhattan Bail Project, is that they are often adopted and implemented by individual courts or through what Nimmer calls the "bureaucratic mode." When reform takes place in this way, the odds are much greater that an active constituency is available to support the reform, improving the chances for the reform's success.

In Metro City the chief judge pushed for adoption of the two reforms, but took care to introduce them in an incremental manner by starting with the pretrial release reform. He then introduced a limited version of the deposit bail plan for minor drug offenders and later expanded it to include all felony defendants. The intent behind this strategy, aside from fending off adverse political reactions, may have been to build and expand the constituency for reform among the lower court judges. But, as noted earlier, these judges were reluctant supporters of the second reform. When they began to increase the size of cash bails, the higher court in tandem with defense attorneys and the pretrial release division found it necessary to take steps to bring the bails back to their customary levels.

Obviously the presence of a reform constituency is a necessary, but not sufficient, condition for successful legal change. Wice (1974) and Dill (1972), for example, have documented the weaknesses and failures of pretrial release reforms in jurisdictions where the innovation was adopted by local court officials. Leadership and a willingness to follow through after adoption of a reform to see that it is implemented

also would seem to be necessary. But it is important to note that in Metro City the response of the reform constituency was triggered by a violation of court norms regarding appropriate bail amounts and not by a desire to reduce or lower these amounts. Aside from this, it may be argued that if the Metro City reform constituency had been broader or the consensus on goals deeper, the reforms would have been more successful. There is no adequate way of responding to this argument, but Pressman and Wildavsky's (1973) analysis of implementation provides a useful illustration of an instance where a project failed despite considerable initial agreement and enthusiasm about it, as disagreements began to surface over time. In other words, the reform constituency and consensus regarding goals must be maintained during the implementation process, which is often a difficult task as other problems and matters begin to intrude on attention, time, and resources, and as the unfolding of events reveals antagonisms and covert conflict neither seen nor anticipated at the start of the reform. As Pressman and Wildavsky put it (1973: 91): "The interests and organizations . . . thought they wanted to do something, but experience . . . teach[es] them that they had been mistaken."

The lower court reaction to the deposit bail plan exemplifies this problem. At the same time it illustrates another issue with a direct bearing on the second characteristic of legal reforms. Nimmer (1978: 175) suggests that a reform "must disrupt the prior balance and accommodation of interests sufficiently [in the judicial process] to induce the participants to engage in a new pattern of behavior." In Metro City the mixed results of reform largely reflect whether the reform was optional or mandatory for the judges. Only in the case of the deposit bail plan where they no longer could choose between surety or deposit bails was reform clearly successful. In other jurisdictions where reform leaves this choice with judges, deposit bails are rarely or infrequently used (Thomas 1976; Flemming, 1977). Mandatory reforms which limit or remove judicial discretion, then, are more likely than optional reforms to satisfy Nimmer's conditions.

Mandatory reforms, of course, are not invariably successful, if only because discretion in other decision-making areas may be used to undermine or elude the restrictions imposed by the reform. The difficulty with mandatory reforms, besides this problem, is that they may raise the issue of conflicting goals more starkly than do optional reforms. Thus it

is important in the case of Metro City to consider the goals of the second reform and why it was not optional.

The reasons behind this decision are not entirely clear, but if the chief judge's primary purpose was to rid the court of bondsmen or, at a minimum, break the link between judges and bondsmen that created the conditions for scandal, then allowing defendants to make the choice between surety and deposit bail was an effective (and shrewd) means of achieving these ends. At the same time the chief judge saw no need for reducing bail amounts, which suggests that he preferred not to challenge the purposes of judges when they required cash bails. Rather than address the ultimate purposes of bail, the reform was sold as a way of removing the opportunities for corruption while simultaneously making bail fairer—goals far less controversial than prohibiting preventive detention by refashioning bail rules.

Optional reforms, such as the pretrial release reform, may fail for a number of political, institutional, or administrative reasons (see Wice, 1974). What is often overlooked when these reasons are enumerated is that the premises of the reforms may well limit their effectiveness. Pretrial release or recognizance reforms, for example, have at least two premises that Nimmer has labeled "technocratic" and "resource." The technocratic premise assumes that judges supplied with greater and more reliable information about the backgrounds and community ties of defendants by a pretrial release organization will rely on this information to make better decisions and presumably release more defendants on their own recognizance.

This clearly was the major hypothesis of the Manhattan Bail Project which attempted to substantiate it through one of the few field experiments in this policy area (Ares *et al.*, 1963). The data from Metro City, however, indicate the weakness of this premise and lend support to Nimmer's statement that other than making time and information available, reforms resting on this premise generally provide no inducement for behavior change and thus cannot grapple with the fact that the "same individuals whose prior actions and decisions constituted the problem continue to make the decisions" (Nimmer 1978: 21).

The resource premise rests on the hypothesis that if defendants are supervised and notified of their court dates, judges will be more willing to free them on their own recognizance or on nonfinancial bail. The agency as an

administrative resource for the court thus makes recognizance a viable or feasible alternative to surety bail. The mere existence of a resource does not mean it will be used, of course. Again the study data lend support to Nimmer's skepticism that the key actors in a policy arena will use increased resources to achieve the goals of reform.

The major premises of pretrial release reform, then, do not provide a foundation for designing programs to directly confront the issue of how reform might alter the incentives shaping the pretrial release decisions of judges. These premises, however, are consistent with the fact that the reform is optional and that judges will retain discretion as to whom to release prior to trial and how.

Until this point, the characteristics of reform have been emphasized, but legal change, to the extent that it occurs, also reflects the system or court in which it takes place. No claims can be made that Metro City is typical of other large urban courts in every respect. Metro City's historically low bail-high release policies for felony defendants may limit its value in learning when bail reform is most likely to be effective.

It is precisely this seeming weakness, however, that makes this city an excellent site for testing the efficacy of bail reform. Many legal reforms are marketed on the basis of experience in a single jurisdiction with the expectation that they will work elsewhere. A central assumption of bail reform largely rooted in the Manhattan Bail Project is that previous policies are sufficiently harsh or discriminatory to have created a large "clientele" or defendant population that will benefit from reform. Metro City, at least in terms of detention rates, did not meet these conditions; yet the methods and criteria used in the Manhattan project were adopted.

Perhaps the central problem in Metro City was not the wholesale incarceration of defendants who because of their community ties could be safely released, but rather the use of bail as a means of preventive detention. Its low bail and high release policies and the limits they placed on the reforms may well be a harbinger of what other courts will face or face at present if, as Thomas found, the trend toward more liberalized pretrial release practices between 1962 and 1971 has continued apace over the past decade. In this sense Metro City is an example of what may be an emerging need to examine whether explicit preventive detention policies might be better than sub rosa practices that, as Thomas states, "are a formidable barrier

to significant expansion of nonfinancial releases" (Thomas 1976: 246).

The limits of bail reform, like legal change generally, are partly and fundamentally political in that a constituency for reform must be created before it can be implemented with any degree of success. But perhaps the central and major limitations are in the premises of the reforms themselves. This is not solely a question of how the reforms are designed, but how the problems which reform is supposed to solve are defined. It may be that the problems associated with traditional bail practices in the sixties are no longer as acute or as widespread as they once were and that the solutions of the sixties are less compelling today.

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