Plea Bargaining Policy and State District Court Caseloads: An Interrupted Time Series Analysis

Malcolm D. Holmes Howard C. Daudistel
William A. Taggart

Questions about the relation of court caseloads to plea bargaining practices generate much controversy but little sophisticated empirical research. Here we examine the effects of a 1975 felony plea bargaining ban in the Texas district courts in El Paso. Relying on a quasi-experimental interrupted time series model with annual data for 1968–83, we test the hypothesis that the discontinuance of plea bargaining negatively affects court caseloads, specifically the proportion of cases going to jury trial and the disposition rate. We also examine conviction rate, exploring whether the existence of plea bargaining encourages prosecutors to accept weak cases. We find a considerable increase in the proportion of cases going to jury trial immediately after the ban's implementation and a substantial but gradual decrease in the disposition rate. The jury trial rate contributed substantially to the disposition rate decline. The conviction rate was generally unaffected by the ban, although it became more consistent after the ban. Overall our findings suggest that the ban on explicit plea bargaining did affect the district courts' ability to move the felony docket efficiently.

It is well known that the vast majority of criminal adjudications in the United States result from guilty pleas and that these dispositions are commonly arrived at through plea negotiations. Yet few criminal justice policies are as controversial as plea bargaining. Some critics maintain that defendants are deprived of constitutional due process guarantees because such negotiations occur outside the bounds of formal legal procedure (e.g., Alschuler 1968, 1981; Blumberg 1967; Langbein 1979). Others complain that defendants do not receive appropriately stringent sentences as warranted by their criminal actions (e.g., Callan 1979). Even those who favor the practice acknowledge that some abuses occur but contend that negotiated guilty pleas are on balance an efficient and just means of moving criminal cases through the courts (e.g., Church 1979; Farr

This article is a revision of a paper presented at the 1988 annual meeting of the Academy of Criminal Justice Sciences, San Francisco. We greatly appreciate the helpful comments from the anonymous reviewers of *Law & Society Review*. Thanks also to Vicki Harley-Taggart for graph preparation and Graciela Hernandez for secretarial assistance.

1984; Rosett & Cressey 1976). Regardless of viewpoint, plea bargaining is commonly thought to be inevitable (Heumann 1978)—reliance on adversary proceedings would be prohibitively time-consuming in congested courts. This opinion is officially validated by the Supreme Court's decision in *Santobello v. New York* (1971), which acknowledges that plea bargaining is desirable partly because of the need to manage caseloads.

Despite the pervasiveness of the belief that plea negotiations are essential for controlling dockets, empirical studies provide little support for such a caseload pressure hypothesis. Research often shows that variations in caseloads (cf. Heumann 1975; Nardulli 1979; Wooldredge 1989) and plea bargaining policies (cf. Church 1976; Daudistel 1980; Heumann 1975; Heumann & Loftin 1979; Meeker & Pontell 1985; Rubenstein & White 1979, 1980; Weninger 1987) have little influence on measures of adversariness (jury trial or guilty plea rates). Indeed, the paucity of evidence supporting the caseload pressure hypothesis helped stimulate a search for other explanations of the predominance of plea bargaining and variations in its usage. These alternatives emphasize factors such as the increased specialization and professionalism of police, prosecutors, and defense attorneys (Feeley 1979; Friedman 1979; Padgett 1990), the growing complexity of the criminal trial (Alschuler 1979; Langbein 1979), and the organizational relationships and norms shared by courtroom actors (Eisenstein & Jacob 1977; Nardulli 1979; Nardulli et al. 1988). Such lines of inquiry contribute considerably to our understanding of the historical and organizational factors that explain plea bargaining. Nevertheless, we think that dismissal of the caseload pressure hypothesis is premature; methodological difficulties with existing studies preclude such a definitive conclusion.

One problem is simply that too few jurisdictions have been studied to permit any generalizations in regard to the issue. Certainly it is unlikely that the few localities researchers chose independently for their research comprise a representative sample of the population of jurisdictions in the United States. A second and perhaps more important concern with respect to generalization is a function of the research designs employed previously. Most recent empirical studies examine the effects of policies that reduce court caseloads via jurisdictional change (Heumann 1975; Meeker & Pontell 1985) or that ban plea bargaining (Church 1976; Daudistel 1980; Heumann & Loftin 1979; Rubenstein & White 1979, 1980; Weninger 1987). These investigations rely almost exclusively on the one-group pretest-posttest design, a pre-experimental approach that assesses the change occurring between one time point before and one time point after a policy implementation. Such a research design is inherently weak with respect to causal interpretations (Campbell & Stanley 1963). Finally, no systematic data are available on how disposition rates are affected by jury trials. This lack of attention is noteworthy because modest increases in jury trial rates have been observed after plea bargaining bans (Church 1976; Daudistel 1980; Heumann & Loftin 1979; Rubenstein & White 1979, 1980; Weninger 1987), and even a slight increase in adversariness might negatively affect the efficiency of dispositions.

These limitations suggest that we have substantially less reliable evidence about the "caseload controversy" (Nardulli 1979) than commonly acknowledged. Even the handful of comparatively strong studies provide inconsistent evidence about the relationship of plea bargaining to caseloads (cf. Meeker & Pontell 1985; Nardulli 1979; Padgett 1990; Wooldredge 1989). The study reported here aims to contribute to our knowledge about the issue by examining the effects of a plea bargaining ban implemented in 1975 in the Texas district courts located in El Paso. Specifically, we compared pre- and post-ban patterns in the jury trial rate, the disposition rate, and the conviction rate using annual time series data for felony dispositions for 1968–83. The trial and disposition variables are central to the issue whether plea bargaining is necessary to control court dockets. The conviction rate variable is relevant to discussions about the fairness of bargained justice because evidential considerations apparently influence prosecutorial decisions about negotiations (Adams 1983; Alschuler 1968; Heumann 1978; Mather 1974; Neubauer 1974; Vetri 1964); eliminating the primary means of obtaining convictions in weak cases could reduce the conviction rate. The number of time points available in this study permits the use of an interrupted time series quasi-experiment, a research design that is methodologically stronger than those used in most previous investigations.

The El Paso Plea Bargaining Ban

The plea bargaining ban instituted in the Texas district courts located in El Paso was not the result of a carefully planned policy innovation. Rather, it evolved from a conflict between the district attorney and the district court judges responsible for the felony docket (see Callan 1979; Daudistel 1980). In 1974 the district attorney announced tough new sentencing policies concerning several felony offenses, particularly burglary. Regardless of circumstances, those pleading guilty to the targeted offenses could anticipate a time-to-serve recommendation from the district attorney.

Based on an analysis of several burglary cases, the district court judges responsible for adjudicating alleged felons determined that juries, who may sentence in Texas, were likely to grant probation to first offenders who had not committed violent crimes (Callan 1979). From their perspective, the district attorney's recommendations were impractical and unjust. Yet granting probation in cases where the prosecutor had called for time to serve was a difficult decision for the judges. As elected officials, they were concerned about being portrayed as too lenient, especially after the local press began to publish stories about their frequent rejection of the DA's harsh sentencing recommendations (Callan 1979; Daudistel 1980).

Throughout 1975 the dispute between the judges and the prosecutor escalated. The conflict culminated in December, when the judges announced in a letter to all members of the El Paso Bar Association that they would no longer accept any recommendations from the district attorney. The prosecutor responded by proclaiming that his office would no longer engage in plea bargaining. The joint bans effectively eliminated explicit plea bargaining as a means of obtaining guilty pleas in felony cases (Daudistel 1980).

Nonetheless, the judges still wanted to avoid jury trials in cases lacking a strong defense. To encourage guilty pleas, their letter to the El Paso Bar Association mandated the implementation of a point system. This system was intended to provide a way of measuring a defendant's chance of receiving a probationary sentence. Points were assigned to various factors (e.g., prior record, severity of offense); if a defendant scored below a certain number of points, probation could be expected (see Callan 1979). The judges argued that the point system reflected the sentencing philosophy of local jurors. By granting probation to an offender who would probably be given probation by a jury anyway, they thought the point system would eliminate the attractiveness of trial. Thus the ban on explicit plea bargaining was supplemented by a sentencing policy designed to limit jury trials and to promote efficient dispositions.

Key elements of the plea bargaining ban remained intact throughout the post-ban time period incorporated into the present study.[1] But the effects on court activity occasioned by this long-standing policy are not known. Initial assessments of the impact of the ban are inconclusive because these studies employ simple pre-ban/post-ban comparisons (Daudistel & Holmes 1979; Daudistel 1980; Weninger 1987). Interestingly,

[1] Two of the authors have studied this jurisdiction extensively since 1975, with their efforts including archival data collection, court observations, and interviews with members of the courthouse community. The descriptions of the jurisdiction offered here are grounded in these endeavors, as well as in published accounts of the El Paso plea bargaining experiment (cf. Callan 1979; Daudistel 1980; Daudistel & Holmes 1979; McDonald 1985; Weninger 1987). Changes that occurred during the post-ban period and their relevance to the present investigation are explicated in conjunction with the findings.

these studies show an increase in the jury trial rate and a decrease in the disposition rate. But the number of data points used in these analyses do not allow a determination of whether the observed changes are attributable to the ban, to preexisting trends in court activity, or to random variation around the long-term trend.

Analytical Strategy

When a jurisdiction adopts a policy discontinuing the practice of plea bargaining, it is possible to adopt a simple interrupted time series research design that permits an assessment of the impact of the policy intervention (i.e., the ban) on a series of observations on some dependent variable of interest (e.g., the jury trial rate). This design falls within the category of a quasi-experiment (Campbell & Stanley 1963), and it has been used extensively in research that assesses the effects of legal policies and decisions because it permits a formal statistical test of the impact of a discrete intervention (Cook & Campbell 1979; Meeker & Pontell 1985). The logic of this design is elaborated in the next section, which includes both a graphical and a mathematical treatment of the issues involved. We follow with a description of the data.

Measuring Policy Consequences

The relevant reference point in understanding the consequences of a policy intervention is the expected trend in the post-intervention period, assuming the policy had not been enacted. This hypothetical outcome, referred to as the counterfactual, represents a projection of values on the dependent variable as they existed in the pre-intervention period (Mohr 1988). For instance, if the jury trial rate is relatively constant prior to the plea bargaining ban, the counterfactual for assessing the impact of the policy is the pre-ban level of the dependent variable. This counterfactual, along with two other possibilities, is presented in panel A of Figure 1. In this diagram the intervention is indicated by the line perpendicular to the horizontal time axis. The three lines graphed horizontally represent different counterfactuals based on conditions as they might have existed before the intervention. The line in the middle corresponds to the counterfactual in the example above, while the other two are counterfactuals based on preexisting trends in the data. Should a time series conform to one of these patterns, the null hypothesis of no policy impact would be accepted.

In contrast, some examples of possible policy impacts are presented in panels B and C. The two lines in panel B depict a

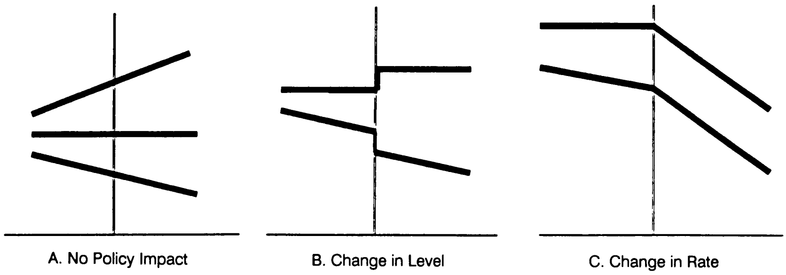

Figure 1. Hypothetical patterns of change in the analysis of impacts

step or change in the level of each series, with the lower line indicating a step function observable even in the presence of a long-term trend. Findings that adhere to the pattern of the top line indicates that the jury trial rate increased following the ban, with the amount of change equal to the difference between the pre- and post-means of the series. The two patterns in panel C indicate a change in the rate of each series, with the upper line showing a downward trend in the post-intervention period after displaying zero change in the pre-policy period. The second line portrays a decreasing trend in both the pre- and post-intervention periods, with the rate of change being greater in the latter time segment. Other impact patterns are possible, including combinations of changes in both the level and rate of an observed time series.

More precise estimates of the possible policy impacts can be obtained by expressing these basic ideas as a mathematical linear function which is amenable to statistical analysis using ordinary least squares (OLS) multiple regression techniques. The intervention model employed here is one of three commonly found in the policy evaluation literature and offers important advantages over other functional forms (see Newcomer & Hardy 1980).[2] Using the year El Paso fully implemented the ban on plea bargaining (1976) as the intervention point, the general model is

$$Y_t = b_0 + b_1\text{PRESLOPE} + b_2\text{STEP} + b_3\text{POSTSLOPE} + e, \tag{1}$$

where Y_t is the observed value of the dependent variable (e.g., the jury trial rate) measured annually ($t = 16$); PRESLOPE equals

[2] Sophisticated statistical techniques designed specifically for the analysis of time series data have been developed (Box & Jenkins 1976), but such methods require a considerably larger number of observations than are available here. The limited number of time points requires the use of OLS regression in the statistical analysis. Autocorrelation tends to be a problem in OLS time series analysis, with its presence affecting model efficiency generally and the accuracy of tests of statistical significance specifically. But this is not an intrinsic problem with the technique (Johnston 1972). An important advantage of the intervention model employed here is that it minimizes autocorrelation (Newcomer & Hardy 1980). It is also preferable to the alternatives because it is characterized by a lower level of multicollinearity (Mandell & Bretschneider 1987; Newcomer and Hardy 1980), a condition that can adversely affect the reliability of parameter estimates (Hanushek & Jackson 1977). Both of these issues are addressed throughout the analysis.

a time counter (starting with one) for years prior to 1976 and which is a constant (the last pre-intervention value) for years following the policy implementation; STEP is a dummy or binary variable coded 0 prior to 1976 and 1 in the post-intervention period; POSTSLOPE is a post-intervention time counter, which is a constant in the pre-intervention period and a time counter starting in 1976; and e represents the residuals unexplained by the statistical model.

In this model, the coefficient b_0 estimates the pre-intervention level of a series (i.e., the starting value of a series or the intercept), b_1 estimates the trend or slope of a series before the plea bargaining ban in 1976, b_2 estimates the level of the series following the ban, and b_3 estimates the trend or slope of the series in the post-intervention period. Put simply, the first two coefficients represent activity occurring before plea negotiations were discontinued. The STEP coefficient indicates whether there was an immediate impact of the ban. The remaining coefficient, b_3, shows whether there was a gradual shift in court activity associated with the ban.

The major advantage of this interrupted time series model is that it represents a quasi-experimental design that eliminates many threats to valid generalizations commonly found in existing studies. The strength of the design is especially apparent in our study. First, the implementation of the plea bargaining ban occurred abruptly, a condition that is advantageous in the use of this design and its corresponding statistical model (Cook & Campbell 1979). Moreover, the ban's formal implementation occurred at the end of 1975, which is important because only annual data are available for analysis. Also the data are available serendipitously for eight years before and eight years after the ban. This distribution is the best possible for assessing trends before and after the policy implementation.

Data

The analysis of trends in criminal court activity is based on data published in the Texas Judicial Council Annual Reports for 1968 through 1983.[3] Annual data were recorded for the number of felony cases on the docket (felony cases pending at year's beginning plus cases docketed during the year), the number of jury trial dispositions in felony cases, the number of convictions (guilty pleas and guilty verdicts) in felony cases, and the total number of felony cases disposed of in the district courts. Data from years prior to 1968 are not employed because they are incomplete; data for years after 1983 are not in-

[3] These reports were prepared under the auspices of the Texas Civil Judicial Council, which in 1974 was renamed the Texas Judicial Council.

cluded because the reporting year changed, producing noncomparable data.

The dependent variables are defined to include: (*a*) the proportion of felony cases disposed of by jury trial (PROJURY), which was calculated by dividing the number of jury trials by the total number of felony cases disposed, (*b*) the disposition rate (PRODISP), which was calculated by dividing the number of felony dispositions by the total number of cases on the felony docket, and (*c*) the conviction rate (PROCONV), which was obtained by dividing the number of felony convictions by the total number of felony dispositions.[4]

Findings

The annual data for the dependent variables are presented in the Appendix. A perusal of them shows clear differences between the pre-ban and the post-ban periods for two of the three series. A comparison of the pre- and post-ban jury trial rate means (.055 and .149, respectively) reveals a marked increase after the ban's implementation. Indeed, the jury trial rate nearly tripled. Still jury dispositions were relatively rare after 1975; only about one of every seven dispositions involved a jury trial. But as we suggested above, it is plausible that even a small increase in the jury trial rate will reduce the disposition rate. Evidence to that effect is found in the distribution of the raw data. The mean disposition rate dropped rather dramatically after the ban (from .662 to .436). As noted, the conviction rate is an indicator of equity in criminal dispositions as plea bargaining permits prosecutors to obtain convictions in cases where juries might acquit. But the raw data do not show that convictions were more difficult to obtain after the ban; rather, the mean conviction rate increased somewhat (from .524 to .636). Although these simple pre- and post-ban comparisons are suggestive, we must turn to the statistical model described above to determine whether the observed differences are actually related to the plea bargaining ban.

Jury Trial Rate

The findings for the jury trial rate are

$$\underset{}{\text{PROJURY}} = \underset{(.021)}{.026} + \underset{(.004)}{.006}\ \text{PRESLOPE} + \underset{(.024)}{.080}\ \text{STEP} - \underset{(.003)}{.002}\ \text{POSTSLOPE}; \qquad (2)$$

$$\text{adjusted } R^2 = .85;\ \text{D.W.} = 2.16.$$

This equation shows that there was no preexisting trend in the jury trial rate before the plea bargaining ban, as evidenced by

[4] Sentencing data, which have often been used to address equity issues in studies of plea bargaining, were not available for our investigation.

the lack of a statistically significant PRESLOPE coefficient ($p>.10$). The positive STEP coefficient ($p<.01$) demonstrates that a marked increase in the jury trial rate occurred immediately after explicit plea bargaining was eliminated. No change in the pre-intervention trend occurred after the policy implementation, as reflected by the nonsignificant coefficient for the POSTSLOPE variable ($p>.10$). The Durbin-Watson (D.W.) test statistic shows that autocorrelation is not a problem in this equation, and elaboration of the analysis likewise reveals that multicollinearity is not a concern.[5]

These results may be compared to the top line in panel B of Figure 1, which depicts the change of level modeled empirically in the equation; that is, there is no trend present in the jury trial rate but an abrupt increase in its level appears immediately after the cessation of plea bargaining. This pattern is also revealed clearly in Figure 2, where the actual jury trial data are presented. Here it may be seen that with the exception of 1975, jury trials represent 6% or less of annual felony dispositions before the ban. Afterward, the rate was at least twice this level in every year except 1979.

The observation that the jury trial rate increased somewhat during the year preceding the ban raises the question whether the findings are attributable to the ban per se or to the

5 As noted, autocorrelation is a statistical phenomenon associated with OLS time series models that must be examined before substantive conclusions can be accepted. With respect to autocorrelation, the D.W. statistic is a small sample test used to evaluate whether the error terms are correlated (Johnston 1972:251). The D.W. statistic is presented in conjunction with each equation reported. (For a discussion of the interpretation of the D.W. test statistic, see Pindyk & Rubinfeld 1981:158–61). The limitation of this test is that it only detects first-order autocorrelation, albeit higher-order autoregressive processes are rarely encountered in the social sciences (McCleary & Hay 1980:59; Pindyk & Rubinfeld 1981:532). Nonetheless, the power of the D.W. test is diminished by virtue of the limited number of time points in our analysis. Accordingly, we also used three other tests for detecting its possible presence. First, the Cochrane-Orcutt procedure was used to estimate the degree of autocorrelation (rho); Monte Carlo simulations show that there is no loss of efficiency with OLS when the value of rho is below .30 (Johnston 1972:265). The estimate for the PROJURY equation is less than .30. Additionally, the error terms from the OLS equation were lagged (e_{t-1}) and regressed on the error terms along with the independent variables in the model; autocorrelation is not a problem if the slope coefficient of the lagged variable is not statistically significant (ibid., p. 313), which proved to be the case here. Lastly, a visual inspection of the OLS residuals plotted over time revealed no "tracking" pattern, which would be observed if autocorrelation is present. Although only the more commonly used D.W. test statistic is reported in the text, each of these procedures was employed for all equations reported throughout the analysis.

Multicollinearity is a second potential problem with the intervention model employed. To assess its possible effects, eq. (2) was reestimated by independently deleting the nonsignificant variables. Deletion of a collinear variable should result in substantively important changes in the parameter estimates for the variables remaining in the equation; however, no such changes were observed. The STEP coefficient remained statistically significant and its estimated value changed only marginally. Although there is no unambiguous test for multicollinearity (Hanushek & Jackson 1977; Lewis-Beck 1980), the findings for the reduced equations suggest that there are no serious specification errors. This procedure of reestimating reduced forms of an equation was also employed throughout the analysis.

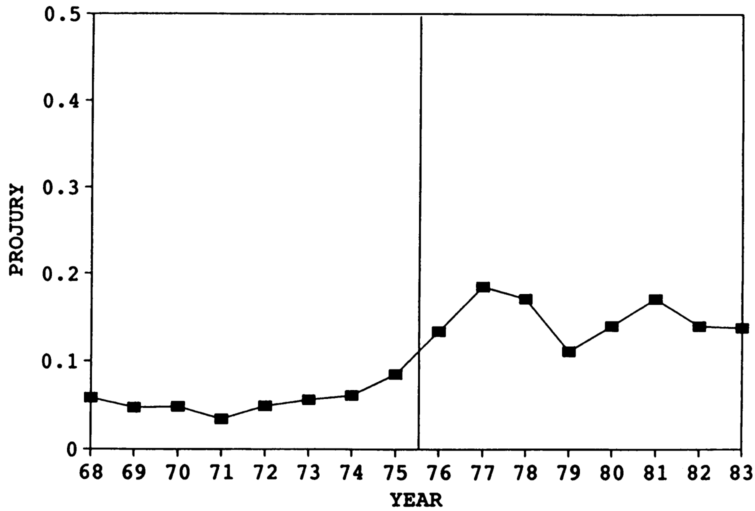

Figure 2. Proportion of Jury Trials (PROJURY) in El Paso District Courts, 1968–1983

interpersonal conflict that produced it. Although an unequivocal conclusion is impossible, two factors support the former interpretation. First, the 1975 data are included in the pre-ban period because the policy was announced in December of that year, but the breakdown in plea negotiations preceded its formal implementation. Thus some post-ban cases are included in the data for that year, at least partially accounting for the higher jury trial rate. Second, all post-ban annual rates are considerably higher than the 1975 rate, even though overt conflict between the judges and prosecutor dissipated during this time. On the other hand, the plea bargaining ban became institutionalized and remained in effect throughout the period. If the findings are attributable to the conflict, the jury trial rate should have decayed during the post-ban era. Conversely, if the ban explains them, the trend in jury trial rates should have remained stable, as the miniscule POSTSLOPE coefficient suggests. An examination of the data confirms the latter, revealing consistently higher rates compared to the pre-ban period, even in 1979, the year after organizational changes designed to facilitate dispositions. The nature of those innovations is elaborated in conjunction with the disposition rate analysis.

Disposition Rate

Here we consider two models. The first examines whether the mean disposition rate declined after the ban's implementation. Thus the form of the first part of the analysis is identical to that of the jury trial rate model. The results are as follows:

$$\text{PRODISP} = \underset{(.095)}{.630} + \underset{(.018)}{.005}\ \text{PRESLOPE} - \underset{(.107)}{.114}\ \text{STEP} - \underset{(.014)}{.028}\ \text{POSTSLOPE}; \tag{3}$$

adjusted R^2 = .60; D.W. = 2.27.

Note that there was no significant trend in the disposition rate before the policy implementation, as indicated by the nonsignificant PRESLOPE coefficient ($p > .10$). That is, the disposition rate was relatively constant before the ban on plea bargaining. The STEP coefficient was not statistically significant ($p > .10$), which means that there was no immediate impact of the plea bargaining ban on the disposition rate. However, the negative POSTSLOPE coefficient ($p < .10$) shows a gradual but marked decline in the disposition rate (.028 annually) after the ban. As a result, there was an overall decline of .224 in the rate during the eight years comprising the post-ban period. The D.W. test again shows that autocorrelation is not a problem, and there is no evidence of a multicollinearity problem (see discussion of procedural details in note 5).

These findings suggest that the ban had a substantial long term negative effect on the disposition rate. The nature of this policy impact is illustrated in the first line of panel C in Figure 1: there is no trend in the disposition rate before the ban, but a negative one appears afterward. This pattern is also apparent in Figure 3, which presents the plotted data. Although there is some variation within the periods, the pre-ban disposition rates are consistently higher than those in the post-ban era. The single exception is the relatively high rate for 1979, an anomaly reflecting organizational changes in the previous year.

Confronting a backlog in the docket and implementation of the Texas Speedy Trial Act, in 1978 the judges divided the criminal docket among all ten district courts in El Paso (Callan 1979). Additionally, administration of the point system was transferred to probation. In the short run, as revealed by the higher disposition rate and somewhat lower jury trial rate in 1979, the organizational changes apparently facilitated case disposition. In the long run, however, it appears the prosecutorial ban on negotiations continued to exert its influence, as the disposition rate subsequently dropped below levels previously seen in either the pre- or post-ban periods. Likewise, Figure 2 shows that the jury trial rate rose after 1979, even though a number of judges not involved in the original conflict were adjudicating cases.

The disposition rate findings are noteworthy because they again indicate that the plea bargaining ban did influence patterns of court activity. They also raise the question whether the immediate change in the jury trial rate influenced the disposition rate. As the ban had a gradual effect on the disposition

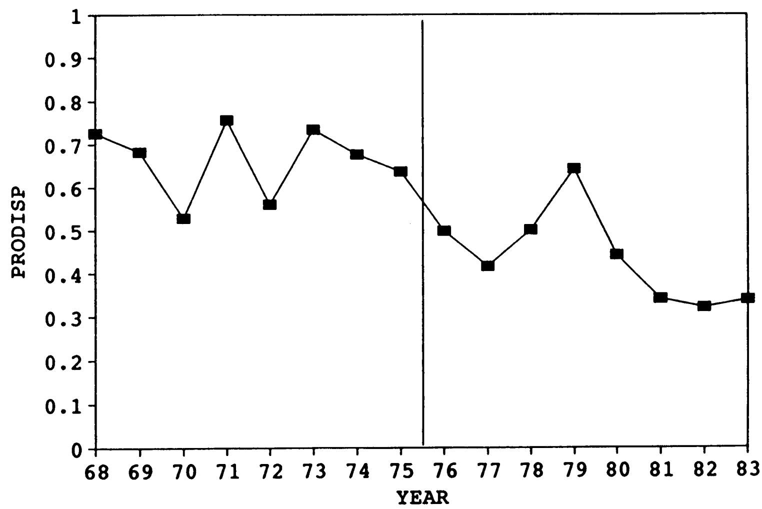

Figure 3. Disposition Rate (PRODISP) in El Paso District Courts, 1968–1983

rate, it can be hypothesized that the change of level in the jury trial rate played a causal role:

$$\text{ban} \xrightarrow{+} \text{jury trial rate} \xrightarrow{-} \text{disposition rate}$$

To address this issue, we modified the original disposition rate equation (3) to include the proportion of jury trials (PROJURY) and the total felony docket (FELDOCKET) as independent variables, along with the POSTSLOPE variable. Given the limited number of time points available for analysis, we deleted the nonsignificant PRESLOPE and STEP variables. We included the jury trial variable to test the hypothesis that the declining disposition rate resulted from the post-ban increase in jury trials. We used the docket variable because the post-ban decline in the disposition rate may have resulted simply from growth in the felony docket (pre-ban $\bar{X}=1{,}733$; post-ban $\bar{X}=3{,}052$). The possible influence of docket size is an important alternative to the jury trial explanation of the declining disposition rate. The modified model is thus defined by

$$\text{PRODISP} = b_0 + b_3\ \text{POSTSLOPE} + b_4\ \text{PROJURY} + b_5\ \text{FELDOCKET}. \qquad (4)$$

In this model, b_0 is the mean value of the pre-intervention trend in the dependent variable. The b_3 coefficient again represents the rate of change in the slope during the post intervention period. The b_4 coefficient represents the effect of the jury trial rate on the disposition rate, and the b_5 coefficient estimates the effect of the felony docket.

The empirical results obtained with this model are

$$\begin{array}{llll} \text{PRODISP} = .784 & -\ .014\ \text{POSTSLOPE} & -\ 1.428\ \text{PROJURY} & -\ .00005\ \text{FELDOCKET}; \\ \quad (.114) & (.022) & (.663) & (.00003) \end{array} \qquad (5)$$

adjusted R^2 = .68; D.W. = 2.33.

The findings from this equation are noteworthy. Most important, the jury trial rate has a significant ($p<.05$) negative effect on the disposition rate. It therefore appears that the declining disposition rate is at least partly attributable to the higher level of jury trials that resulted from the plea bargaining ban. The parameter estimate for the FELDOCKET variable is not statistically significant ($p>.10$), nor is the POSTSLOPE coefficient. An initial assessment of the model therefore indicates that, by virtue of its positive relationship to the jury trial rate, the plea bargaining ban negatively influenced the ability of the courts to manage the criminal docket. The D.W. statistic reveals no problem with respect to autocorrelation, but multicollinearity affects the estimates of the POSTSLOPE and FELDOCKET coefficients. An analysis of the multicollinearity problem shows that the jury trial effect is quite stable over different model specifications, even though the effects of the other two variables cannot be disentangled.[6]

Conviction Rate

One argument against plea bargaining emphasizes that it induces some defendants to plead guilty even though they would probably not be convicted at trial. If this is so, conviction rates should decline when evidentially marginal cases cannot be plea bargained. The conviction rate is therefore analyzed using the policy impact model:

$$\text{PROCONV} = \underset{(.097)}{.534} - \underset{(.018)}{.010}\ \text{PRESLOPE} + \underset{(.109)}{.132}\ \text{STEP} + \underset{(.015)}{.013}\ \text{POSTSLOPE}; \quad (6)$$

$$\text{adjusted } R^2 = .35;\ \text{D.W.} = .72.$$

This equation provides no evidence that the ban had an impact, as none of the coefficients are statistically significant. But the value of the D.W. statistic reveals that autocorrelation is a problem in this equation. Although the parameter estimates are unbiased, significance tests will be inaccurate because the estimated variances are affected. However, the positive auto-

[6] As we are primarily interested in the PROJURY coefficient, the collinearity of the POSTSLOPE and FELDOCKET variable ($r=.87$) is of little concern. The estimates of the latter coefficients are imprecise because their sample variances are inflated by the collinearity between them. However, parameter estimates for variables that are not collinear with other variables ($r<.70$), as is the case with PROJURY, will not be affected (see Hanushek & Jackson 1977:86–96). When eq. (5) is reestimated with only PROJURY and POSTSLOPE, and then with only PROJURY and FELDOCKET, the effect of PROJURY on the disposition rate predictably remains very stable. But the other coefficients are statistically significant when entered separately. Therefore, we cannot be certain whether there is a negative trend in the disposition rate net of the effect of the jury trial rate, or whether the increase in the felony docket explains the component of the trend unaccounted for by the jury trial rate. But there is no doubt that the jury trial rate accounts for a significant part of the decline in the disposition rate subsequent to the ban. Indeed, the PROJURY coefficient is stable and statistically significant even when the nonsignificant PRESLOPE and STEP variables are added to eq. (5).

correlation observed here indicates that the variances are underestimated (Johnston 1972). In other words, the standard errors of the estimated coefficients will increase if the autocorrelation is corrected, which means it is unlikely that a coefficient in the equation is actually statistically significant. In short, the conviction rate appears to be unaffected by the implementation of the plea bargaining ban.

Given that no significant pre- or post-ban trend is evidenced in this intervention model, these results statistically correspond to the counterfactual depicted in the middle line of panel A in Figure 1. However, the plotted data in Figure 4 show a nonlinear trend in the pre-ban era, partly explaining the lack of a statistically significant PRESLOPE coefficient. The conviction rate dropped sharply through 1972, after which it started to increase. Throughout the remainder of the pre-ban era it appreciated to nearly its post-ban level, during which time it remained quite uniform, suggesting a ban-related effect that is not captured in the intervention model.[7]

We suspect that the post-intervention consistency in the conviction rate reflects the case screening procedures put into place after the plea bargaining ban. It appears that cases presented to the grand jury were supported by more evidence after the ban (Daudistel 1980; McDonald 1985; Weninger 1987); weak cases that probably would go to trial were less likely to be accepted. Indeed, recent data compiled by the screening section of the district attorney's office show that half of the cases filed by police are declined for prosecution or are referred to the county attorney's office for prosecution as misdemeanors (see also McDonald 1985). Unfortunately, screening data are not available except for recent years, so the relationship of the ban to prosecution decisions cannot be studied systematically. But it is to be expected that the jury trial rate would have increased even more had less rigorous screening criteria remained in use. Therefore, even though it is not shown in the conviction rate findings, it seems plausible that one dimension of justice improved somewhat because weaker

[7] The curvilinearity in the pre-ban period is at least partially responsible for the significant D.W. statistic reported for the PROCONV eq. (6), which indicates the presence of positive autocorrelation. Autocorrelation is usually traced to the positive longitudinal relationship existing between sequential error terms, but positive autocorrelation can also result from the omission of a relevant independent variable. The parabolic pattern evident in Fig. 4 requires an additional pre-intervention variable—the square of PRESLOPE—to model the conviction rate series more accurately. Inclusion of this variable improved prediction of the pre-ban series, but the D.W. statistic still indicated positive autocorrelation. Thus the equation was reestimated using generalized least squares regression, employing the Yule-Walker estimate of rho (autocorrelation). Coefficients for the PRESLOPE and PRESLOPE squared variables were both significant ($p < .001$), but neither the STEP nor the POSTSLOPE coefficient achieved statistical significance ($p > .10$). Thus the interpretation of the original PROCONV eq. (6) remains essentially unchanged, except that the relative uniformity of the post-ban rates is noteworthy.

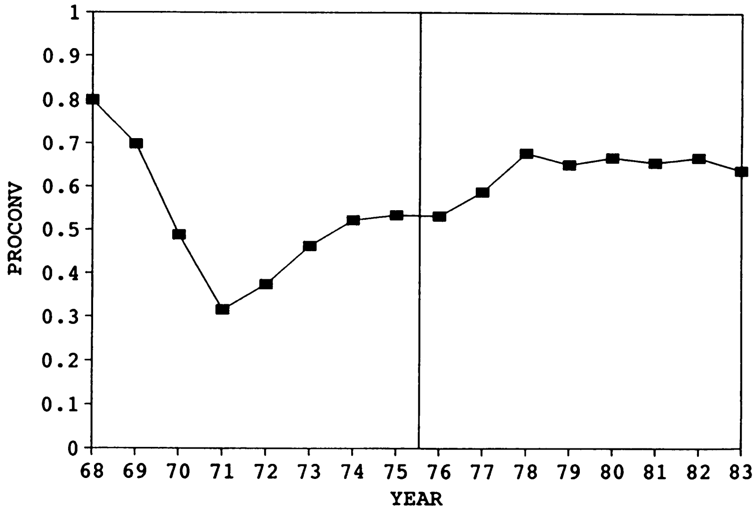

Figure 4. Conviction Rate (PROCONV) in El Paso District Courts, 1968–1983

cases were less likely to be accepted for prosecution after the ban.

On the other hand, research using archival data on felony dispositions during the mid-1970s reveals that juries tended to sentence Hispanic offenders to prison more often than Anglos, regardless of factors such as conviction severity or prior record (Daudistel & Holmes 1979; Holmes & Daudistel 1984; LaFree 1985). If this tendency continued, it is possible that Hispanic defendants were at a disadvantage under the no plea bargaining policy. In this respect, then, the quality of justice may have diminished.

Summary and Discussion

The findings presented above suggest that the plea bargaining ban in El Paso influenced felony caseloads in the district courts. Banning plea bargaining seems to have caused an immediate increase in the level of jury trials and a gradual decrease in the disposition rate. Moreover, it appears that the increase in jury trials helps explain the declining disposition rate. These observations lend support to the caseload pressure hypothesis, although note that most felony adjudications still involved guilty pleas. The conviction rate was unaffected by the ban, except that it became more consistent in post-ban period. Possibly the more stringent case screening procedures instituted after the ban systematically filtered out weaker cases.

An important issue concerning the caseload findings is whether explicit plea bargaining ended completely in El Paso. Research shows that other plea bargaining bans were circumvented, usually because judges became directly involved in sentence bargaining (Church 1976; Heumann & Loftin 1979). To a degree this was also true in El Paso. The judges who created the point system remained committed to it (Callan 1979; Weninger 1987). But, as noted above, in 1978 the judges decided to divide the felony docket more evenly among the ten district courts. Some of the judges who had not previously adjudicated criminal cases apparently participated in *sub rosa* sentencing discussions with defense attorneys (Weninger 1987). But the district attorney's strict ban on plea negotiations remained in effect throughout the 1980s, and wholesale plea bargaining clearly did not reemerge during the post-ban period under study.

Another issue in this regard concerns the reality that the point system provided tacit sentence recommendations designed to encourage guilty pleas. Implicit plea bargaining, in which defendants willingly enter guilty pleas even in the absence of prosecutorial concessions, is a commonly recognized practice (Padgett 1985, 1990); defendants allegedly benefit from these guilty pleas because of the judicial propensity to punish severely defendants who exercise their right to trial in the face of obvious guilt (see, e.g., Heumann 1978; Uhlman & Walker 1980). Indeed, the El Paso judges made it clear that the point system would not be applied mechanically and that facts presented in jury trials would influence sentences (Callan 1979; Daudistel 1980). Moreover, the point system focuses on the decision to probate or incarcerate, and knowing that a serious felony such as burglary will result in probation effectively eliminates many defendants' interest in explicit plea bargaining. The judges were certainly aware that a backlog of cases might develop in the absence of explicit plea bargaining. Although they did not consider it a plea negotiation strategy, the point system was designed as a substitute aimed at avoiding the problem.

Despite the presence of some covert plea negotiations and the implicit guarantees of the point system, the lack of explicit prosecutoral plea bargaining apparently influenced the adversariness and efficiency of felony adjudications. The jury trial rate nearly tripled following the plea bargaining ban's implementation, and the disposition rate declined considerably throughout the post-ban period. This investigation thus provides evidence that explicit prosecutorial plea bargaining helps oil the wheels of justice. At the same time, it is clear that implicit negotiations represent a compensatory mechanism in the absence of explicit bargaining. But implicit bargaining requires a flow of cases that is stronger evidentially than is necessary with prosecutorial forms of plea bargaining (Padgett 1985). Without a high rejection rate by the district attorney's screening section, it seems unlikely that the point system would have controlled the jury trial rate to the degree that it did. Although more consistent with due process standards concerning legal guilt, implicit plea bargaining appears to be less efficient bureaucratically than is prosecutorial bargaining.

The strength of this study lies in its research design. As noted, the interrupted time series design is superior to those used in most existing studies with respect to causal inferences. An important problem, however, concerns its vulnerability to history effects (see Campbell & Stanley 1963; Cook & Campbell 1979; Meeker & Pontell 1985). Events other than the policy intervention under consideration could have influenced the trends observed in the data. But we are unaware of other changes in legal policy or organization that might have influenced trends in court activity in the same direction as predicted by the caseload pressure hypothesis. Other policies of the period, including the point system, the Speedy Trial Act, and the division of the criminal docket should have reduced trials and increased dispositions—the findings show just the opposite.

It must also be emphasized that the external validity of the interrupted time series design remains open to question. Obviously it is impossible to make broad generalizations from this study because of its limited jurisdictional coverage, as had been the case with previous efforts. Further, unlike the plea bargaining bans studied previously, the El Paso experiment did not involve a planned policy change. Yet the courthouse conflict that culminated in the ban dissipated without corresponding changes in court activity, indicating that the observed patterns reflect the ban's impacts. We think dissimilarities in research design are a more likely explanation of the disparate findings reported in the literature than are differences in policy implementation. As mentioned, the majority of studies employ designs that are weak methodologically. Several recent investigations that use stronger designs show caseload pressure effects (Meeker & Pontell 1985; Padgett 1990; Wooldredge 1989). By incorporating additional time points and more sophisticated statistical models, researchers may observe effects that are not detected in simple pre- and post-ban comparisons. As most studies examine policies implemented during the 1970s, sufficient time has elapsed to reexamine them using more rigorous designs.

The major implication of this investigation is that the caseload controversy is not yet resolved. Viewed in light of the existing evidence, which shows at best a modest relationship between jury trial/guilty plea rates and plea bargaining, the results here affirm that jury trials need not become the dominant mode of case disposition in the absence of explicit prosecutorial plea negotiations. Indeed, it would be easy to argue that small increases in jury trial rates are inconsequential if these increases did not in turn affect the ability of the courts to manage dockets efficiently. Our disposition rate findings show something very different; within the eight years following the El Paso plea bargaining ban, this rate dropped to a point far below its pre-ban level. The stoppage of explicit prosecutorial plea bargaining in El Paso apparently had a profound influence on the felony docket, even if the ban created nothing more than a change in the form of negotiations. Yet we must remain cautious about generalizing from these findings. Future efforts along the lines of this study should culminate in a more definitive body of evidence regarding the caseload pressure hypothesis.

Appendix

Dependent Variable Data for Proportion of Jury Trials (PROJURY), Disposition Rate (PRODISP), Conviction Rate (PROCONV), as well as Number of Cases on the Felony Docket (FELDOCKET) in El Paso District Courts, 1968–1983

Year	PROJURY	PRODISP	PROCONV	FELDOCKET
			Pre-Ban	
1968	.058319	.726027	.801029	803
1969	.047308	.682628	.698206	898
1970	.048346	.527871	.488550	1,489
1971	.033742	.756732	.314724	2,154
1972	.049347	.559935	.373004	2,461
1973	.055915	.734544	.460859	2,313
1974	.060842	.676160	.521841	1,896
1975	.084962	.635529	.534410	1,852
			Post-Ban	
1976	.134133	.497289	.532170	1,844
1977	.184923	.414753	.588928	2,047
1978	.171099	.499115	.679078	2,260
1979	.111435	.642911	.652859	2,666
1980	.140122	.440736	.668407	2,607
1981	.171251	.339662	.657498	3,318
1982	.140514	.318854	.669044	4,397
1983	.138453	.338327	.639013	5,273

References

Adams, Kenneth (1983) "The Effect of Evidentiary Factors on Charge Reduction," 11 *J. of Criminal Justice* 525.

Alschuler, Albert (1968) "The Prosecutor's Role in Plea Bargaining," 36 *Univ. of Chicago Law Rev.* 50.

——— (1979) "Plea Bargaining and Its History," 13 *Law & Society Rev.* 211.

——— (1981) "The Changing Plea Bargaining Debate," 81 *California Law Rev.* 69.

Blumberg, Abraham (1967) *Criminal Justice*. Chicago: Quadrangle.

Box, George E. P., & Gwilym M. Jenkins (1976) *Time Series and Analysis: Forecasting and Control*. San Francisco: Holden-Day.

Callan, Sam (1979) "An Experience in Justice without Plea Negotiation," 13 *Law & Society Rev.* 330.

Campbell, Donald T., & Julian C. Stanley (1963) *Experimental and Quasi-experimental Designs for Research*. Chicago: Rand McNally.

Church, Thomas, Jr. (1976) "Plea Bargains, Concessions and the Courts: Analysis of a Quasi-Experiment," 10 *Law & Society Rev.* 377.

——— (1979) "In Defense of Bargain Justice," 13 *Law & Society Rev.* 509.

Cook, T. D., & D. T. Campbell (1979) *Quasi-Experimentation: Design and Analysis Issues for Field Settings*. Chicago: Rand McNally.

Daudistel, Howard C. (1980) "On the Elimination of Plea Bargaining: The El Paso Experiment," in W. F. McDonald & J. A. Cramer, eds., *Plea-Bargaining*. Lexington, MA: Lexington Books.

Daudistel, Howard C., & Malcolm D. Holmes (1979) "Abandoning Plea Bargaining: A Case Study of Nonnegotiated Justice," in P. L. Brantingham & T. G. Blomberg, eds., *Courts and Diversion: Policy and Operations Studies*. Beverly Hills, CA: Sage Publications.

Eisenstein, James, & Herbert Jacob (1977) *Felony Justice: An Organizational Analysis of Criminal Courts.* Boston: Little, Brown.

Farr, Kathryn Ann (1984) "Administration and Justice: Maintaining Balance through an Institutionalized Plea Negotiation Process," 22 *Criminology* 291.

Feeley, Malcolm M. (1979) "Perspectives on Plea Bargaining," 13 *Law & Society Rev.* 199.

Friedman, Lawrence M. (1979) "Plea Bargaining in Historical Perspective," 13 *Law & Society Rev.* 247.

Hanushek, Eric A., & John E. Jackson (1977) *Statistical Methods for Social Scientists.* New York: Academic.

Heumann, Milton (1975) "A Note on Plea Bargaining and Caseload Pressure," 9 *Law & Society Rev.* 515.

——— (1978) *Plea Bargaining: The Experiences of Prosecutors, Judges and Defense Attorneys.* Chicago: Univ. of Chicago Press.

Heumann, Milton, & Colin Loftin (1979) "Mandatory Sentencing and the Abolition of Plea Bargaining: The Michigan Felony Firearm Statute," 13 *Law & Society Rev.* 393.

Holmes, Malcolm D., & Howard C. Daudistel (1984) "Ethnicity and Justice in the Southwest: The Sentencing of Anglo, Black, and Mexican Origin Defendants," 65 *Social Science Q.* 265.

Johnston, J. (1972) *Econometric Methods.* 2d ed. New York: McGraw-Hill.

LaFree, Gary D. (1985) "Official Reactions to Hispanic Defendants in the Southwest," 22 *J. of Research in Crime & Delinquency* 213.

Langbein, John H. (1979) "Understanding the Short History of Plea Bargaining," 13 *Law & Society Rev.* 261.

Lewis-Beck, Michael S. (1980) *Applied Regression Analysis: An Introduction.* Beverly Hills, CA: Sage Publications.

Mandell, Marvin B., & Stuart I. Bretschneider (1987) "Using Linear Trend Models to Analyze Policy Impact," 6 *Policy Studies Rev.* 476.

Mather, Lynn M. (1974) "Some Determinants of the Method of Case Disposition: Decision-Making by Public Defenders in Los Angeles," 8 *Law & Society Rev.* 187.

McCleary, Richard, & Richard A. Hay, Jr. (1980) *Applied Time Series Analysis for the Social Sciences.* Beverly Hills, CA: Sage Publications.

McDonald, William F. (1985) *Plea Bargaining: Critical Issues and Common Practices.* Washington, DC: U.S. Department of Justice.

Meeker, James W., & Henry N. Pontell (1985) "Court Caseloads, Plea Bargains, and Criminal Sanctions: The Effects of Section 17 P.C. in California," 23 *Criminology* 119.

Mohr, Lawrence M. (1988) *Impact Analysis for Program Evaluation.* Chicago: Dorsey.

Nardulli, Peter F. (1979) "The Caseload Controversy and the Study of Criminal Courts," 70 *J. of Criminal Law and Criminology* 89.

Nardulli, Peter F., James Eisenstein, & Roy B. Fleming (1988) *The Tenor of Justice: Criminal Courts and the Guilty Plea Process.* Urbana: Univ. of Illinois.

Neubauer, David W. (1974) *Criminal Justice in Middle America.* Morristown, NJ: General Learning Press.

Newcomer, Kathryn E., & Richard J. Hardy (1980) "Analyzing Policy Impact: Selection of a Linear Trend Model," 8 *Policy Studies J.* 928.

Padgett, John F. (1985) "The Emergent Organization of Plea Bargaining," 90 *American J. of Sociology* 753.

——— (1990) "Plea Bargaining and Prohibition in the Federal Courts, 1908–1934," 24 *Law & Society Rev.* 413.

Pindyk, Robert S., & Daniel L. Rubinfeld (1981) *Econometric Models and Economic Forecasts.* 2d ed. New York: McGraw-Hill.

Rosett, Arthur, & Donald R. Cressey (1976) *Justice by Consent: Plea Bargaining in the American Courthouse*. New York: Lippincott.
Rubinstein, Michael L., & Teresa J. White (1979) "Plea Bargaining: Can Alaska Live without It?" 62 *Judicature* 267.
——— (1980) "Alaska's Ban on Plea Bargaining," in W. F. McDonald & J. A. Cramer, eds., *Plea-Bargaining*. Lexington, MA: Lexington Books.
Uhlman, Thomas, & Darlene Walker (1980) "He Takes Some of My Time: I Take Some of His: An Analysis of Judicial Sentencing Patterns in Jury Cases," 14 *Law & Society Rev.* 323.
Vetri, D. (1964) "Guilty Plea Bargaining: Compromises by Prosecutors to Secure Guilty Plea," 112 *Univ. of Pennsylvania Law Rev.* 865.
Weninger, Robert A. (1987) "The Abolition of Plea Bargaining: A Case Study of El Paso County, Texas," 35 *UCLA Law Rev.* 265.
Wooldredge, John D. (1989) "An Aggregate-Level Examination of the Caseload Pressure Hypothesis," 5 *J. of Quantitative Criminology* 259.

Statute Cited

Speedy Trial of Criminal Cases, Texas Senate, 65th sess., ch. 787, Acts 1977, p. 1970.

Case Cited

Santobello v. New York, 404 U.S. (1971).