THE IMPACT OF CHANGES IN PENALTIES
ON ANTITRUST ENFORCEMENT

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I. Introduction

In 1974 Congress elevated penalties for criminal antitrust offenses from the misdemeanor to the felony level, an action that significantly increased the maximum penalties available to the courts.[1] This legislation is one example of frequent Congressional interventions that either regulate sentencing practices or are intended to raise penalties for particular crimes.[2] The purpose of this article is to analyze the potential effects from such legislation and assess empirically the impact of the felony penalties on antitrust enforcement. The empirical work uses an original data set and a technique that has only recently been applied to analyses of litigation.

In assessing the deterrent effects of the felony penalties, the following questions are relevant: First, how do the courts react to statutory changes in penalties? In this case, the courts followed Congressional intent by sentencing those convicted to substantially longer jail terms and higher fines.[3] Second, and more difficult to answer, does the threat of higher penalties significantly reduce the number and severity of offenses committed? While no direct measures of offender behavior are available, reductions in the number and severity of offenses may be reflected in changes in the set of criminal antitrust cases filed under the felony penalty regime.

Along with the potential deterrent effects, the shift to felony penalties may have further effects on the enforcement process because of the potential for legal error.[4] One consequence of error is that potential and actual defendants will try to avoid conviction. In the context of antitrust enforcement, firms will consider the role of legal error in choosing among business practices and may avoid efficiency-enhancing practices that may be construed as violations. In addition, both offenders and non-offenders will spend resources to reduce the probability of indictment and conviction. An increase in penalties is likely to increase the intensity of these "defensive efforts," which should be viewed as part of overall enforcement costs. A related consequence of error is that the courts, for *efficiency* reasons, may exercise more care in deciding when to impose higher penalties. Thus, even though the courts adjusted

to the antitrust legislation by imposing harsher sentences upon conviction, concerns about legal error might lead the courts increase the burden of proof on prosecutors seeking the stiffer penalties. It is noteworthy that when the misdemeanor penalties applied to criminal antitrust offenses, the courts only rarely imposed the maximum penalty of $50,000 and one year in jail then in effect, indicating a preference for lower penalties.[5]

Together the increase in defensive efforts and the potential resistance on the part of the courts to the higher penalties may make it more difficult for prosecutors to detect and convict those not deterred by the felony penalties. An appreciation of these potential effects along with the deterrent benefits of the felony penalties can provide insights into recent trends in antitrust enforcement. Consistent with the views of many observers, I find a reduction in the Antitrust Division's criminal enforcement output in recent years, measured by the number of significant price-fixing cases filed. I also find evidence of a decline in the number of successful prosecutions. While some have associated the changes with less vigorous enforcement by the Reagan Administration,[6] the lower conviction rates may be the result of the shift to felony penalties, which occurred in the late 1970s. The impact of the felony penalties on Antitrust Division enforcement may also explain the documented decline since then in the number of private antitrust cases that follow government cases.[7]

In the next section, I discuss how the potential for legal error broadens the range of potential effects of a shift to higher penalties, focusing on how defendants and the courts will react to higher penalties. In Section III I discuss how the the insights from this analysis apply to the antitrust enforcement data that I have collected on the number, type, and disposition of criminal antitrust cases filed. In Section IV I report the empirical results concerning criminal antitrust enforcement during the period 1970–85.

II. The Effects of Higher Penalties

The character and accuracy of the information available to the courts will in large part determine the range of potential effects from increasing penalties. When the courts can obtain accurate and irrefutable evidence about defendant behavior, the threat of very large penalties can deter all crime without risking erroneous convictions. In this circumstances, an increase in penalties serves either to further deter offenses or to reduce enforcement costs, leading to the conclusion that the penalty for the most severe crime should equal or exceed an individual's wealth.[8] The same result is obtained if errors in the court's assessments of defendant behavior are bounded, since the courts can set a conviction rule (the standard to which evidence about individual defendants is compared) that prevents erroneous convictions yet convicts offenders with positive probability.[9]

Often, however, the information available to the courts does not permit them to use conviction rules that fully protect non-offenders. Keeping the probability of erroneous convictions at a de minimus level is especially difficult in criminal enforcement of the antitrust laws, where the boundaries between illegal and legal behavior are subject to different interpretations and where criminal intent is an important issue.[10] In the discussion that follows, I consider the broader range of effects of increasing penalties when legal errors may occur and when defendant behavior is not limited to the decision of whether to commit an offense.[11] I proceed by analyzing individual behavior and then evaluate the courts' response to higher statutory penalties.

A. Offender and Non-Offender Behavior

The threat of higher antitrust penalties may affect several dimensions of individual behavior. First, even though the costs and benefits from illegal trade restraints are expected to vary based on a large number of industry and firm characteristics, higher penalties will add to the costs of engaging in crime. The greater costs will deter marginal offenders — those for whom the expected income from various types of collusion is similar to that from legal

activities.

Second, higher penalties will tend to lead intra-marginal offenders to alter their choice of offense severity and defensive efforts.[12] In general detection of severe antitrust offenses is more likely because high markups require an extensive collusive framework and thereby generate more potential evidence of the crime.[13] Despite the legal principle that an agreement to restrain trade in itself constitutes an antitrust offense, the probability of conviction also increases with severity because evidence of substantial illegal profits helps prove the government's allegations. From the offender's point of view, therefore, the optimal severity balances the added income from more severe crimes against the higher expected penalties. When penalties are increased, it follows that offenders will reduce offense severity and forego some illegal income to reduce the expected penalty.

The same logic suggests that when higher penalties apply offenders will spend more resources to reduce the probability of detection and conviction.[14] A higher level of defensive efforts is warranted since a given decline in the probability corresponds to a larger decrease in the expected penalty. In practice, parties to an illegal agreement may exercise more caution in disguising their actions and in limiting the availability of potential evidence. And if detected, the parties will tend to spend more on their legal defenses.

Non-offenders also will react to higher penalties. If, as is usually the case, the probability of erroneous conviction varies among types of legal activity, then non-offenders will substitute away from the more risky activities to reduce the probability of being indicted. Non-offenders who are indicted, like other defendants, will tend to spend additional resources to avoid conviction. It should be noted, however, that while defensive efforts by offenders are intended to increase error, those by non-offenders counteract legal error by reducing the probability of erroneous convictions .[15]

B. The Court's Response to Higher Penalties

When the operative conviction rule does not fully protect legal activities, efficiency-

minded courts will be concerned with the tradeoff between the costs imposed on non-offenders and the gains from stronger deterrence, which include reductions in the level of defensive efforts by those deterred. For the population of non-offenders, enforcement costs include their defensive efforts and inefficient substitution among legal activities. This accounting of enforcement costs can be used to identify the optimal combination of penalty and conviction rule when legal errors may occur, but here the inquiry is more narrow: How will the courts react when the legislature votes for higher penalties upon conviction? Presuming the courts follow Congressional intent and impose higher penalties upon conviction, the relevant question is how do the courts adjust conviction rules.

Since the conviction rule along with the penalty upon conviction influence the decision to offend, a higher minimum standard for conviction weakens deterrence. But a higher minimum offers greater protection to non-offenders and thereby lessens the costs of inefficient substitutions among the set of legal activities and defensive efforts by non-offenders. Presuming that the conviction rule used in conjunction with the misdemeanor penalties balanced deterrence benefits against the costs arising from legal error, it follows that rather than employ the same rule when felony penalties will be imposed, the courts will tend to *offset* the increase in the penalty upon conviction by increasing the standards for conviction.[16] Doing so will limit the increase in the liability for non-offenders and thereby maintain the balance between deterrence and the costs imposed on non-offenders.

It should be noted that if the courts increase the prosecutor's burden of proof in response to the higher penalties, the incentives of intra-marginal offenders to reduce offense severity and increase defensive efforts are not weakened. As a general rule, changes in conviction rules do not affect directly offender behavior because, provided that the probability of conviction is strictly positive, the choice of offense severity and defensive efforts depends on their marginal effects on the probability of conviction.[17] A change in the probability has no predictable effect, for example, on the tradeoff between the gains from a more severe offense and the added expected penalty. Likewise, an adjustment in the conviction rule that lowers the

probability of conviction may either increase, decrease, or leave unchanged the marginal product of defensive efforts. As a result, even though the courts may adjust conviction rules as predicted, a shift to higher penalties will still lead offenders to reduce offense severity and increase defensive efforts, making detection and prosecution of offenders more difficult.

III. Implications for Antitrust Enforcement

According to the analysis in the preceding section, the shift to felony penalties is expected to (i) reduce the severity of offenses committed, (ii) increase defensive efforts by potential and actual defendants, (iii) lead to changes in the conviction rules that place a greater burden on prosecutors, and (iv) reduce the number of offenses committed provided that expected penalties rise on net. In principle, one might consider a large variety of evidence to assess whether these expectations have been borne out. For example, the fact that private plaintiffs have been less likely to follow on government criminal cases since the shift to felony penalties might indicate a reduction in the severity of alleged offenses.[18] One might also argue that the Supreme Court's *Gypsum* decision that specified how prosecutors can prove criminal intent in antitrust cases may represent evidence of an adjustment to the felony penalty regime.[19]

Systematic measurement, however, of the impact of felony penalties on antitrust offenses, defensive efforts, and operative conviction rules is problematic for fairly obvious reasons. Data on the number and severity of offenses and the full range of defensive efforts are not available. In addition, the predicted change in conviction rules will not be reflected in the stated burden of proof, since the "proof beyond a reasonable doubt"standard applies to all criminal cases. Instead, the greater burden might be manifest in jury instructions, rules concerning evidence of prior acts, and other determinants that are difficult to evaluate.

An alternative to direct measurement of the impact of the felony penalties on individual and court behavior is to evaluate the enforcement output of the Antitrust Division before and after Congress increased criminal antitrust penalties. The predicted changes in individual and court behavior should have systematic effects on the cases filed, the plea decisions of

those indicted, and case disposition. As described in the next section, I collected data on criminal cases alleging horizontal violations of §1 of the Sherman Act, which prohibits price-fixing and other restraints of trade. Although increased efforts to avoid detection may confound the issue, the deterrent effects of the felony penalties may be manifest in time-series data on the number of cases filed. The further predictions regarding the severity of offenses, defensive efforts, and possible changes in the conviction rules may be tested with data on defendant choice of pleas and the outcomes of cases in which defendants plead not guilty.

Specifically, the change to felony penalties should lead defendants to plead not guilty with greater frequency. Even with the probability of acquittal constant, the legal resources a defendant must spend to go to trial have a higher private payoff in terms of the penalty that may be avoided, making a not-guilty plea more attractive when felony penalties apply. Moreover, if the felony penalties reduce the severity of most offenses and lead the courts to apply a higher minimum conviction standard, then the probability of acquittal rises, which will further encourage not-guilty pleas. This same line of argument also suggests that when higher penalties will be imposed upon conviction, the probability of conviction conditional on a not-guilty plea will fall.

Of course, whether a greater frequency of non-guilty pleas will be observed depends in part on the selection of cases by enforcers. If prosecutors require that cases under the felony regime meet a higher standard, then fewer cases would be filed and those indicted may not be inclined to force a trial. On the other hand, if prosecutors tend to bring a similar number of cases for prosecution despite a reduction in the number of serious offenses, then fewer of the cases filed will meet even the old conviction standard, which would encourage not-guilty pleas.

Importantly, the prediction regarding lower conviction rates in litigated cases depends both on the selection of cases filed by prosecutors and on the impact of the higher penalties on the frequency of not-guilty pleas. The latter is relevant since defendant plea decisions determine the set of cases that actually go to trial. The tendency toward lower conviction

rates could be mitigated, for example, by a substantial increase in the number of defendants who plead not guilty despite having only a small chance of acquittal. These comments suggest that, to the extent possible, the empirical analysis of litigated outcomes should account for differences in the selection of cases.

IV. Evidence from Antitrust Enforcement

Since the shift to higher actual penalties occurred in the late 1970s, the effects of the change are likely to be reflected in data on criminal cases filed by the Antitrust Division during the period 1970-85. The data I collected for each case alleging a horizontal violation of §1 of the Sherman Act include whether misdemeanor or felony penalties applied, the defendant choice of plea, the outcome of the case, and other relevant variables such as the sales affected by the alleged conspiracy. Appendix A provides more detail about the data. In this section I proceed with a review of the Antitrust Division's caseload and then turn to the regressions that estimate the impact of the felony penalties on defendant plea decisions and on the outcomes of litigated cases.

A. Trends in Enforcement

Table 1 presents various time-series data. The first column identifies total "Bluebook" cases filed,[20] which include civil and criminal price-fixing cases, merger cases, monopolization cases, vertical restraint cases, and other miscellaneous cases. To obtain a measure of criminal cases alleging horizontal restraints of trade, I eliminated other types of cases and then grouped together the remaining Bluebook entries that deal with the same violation. The latter step is necessary because (i) grand juries may indict parties involved in the same conspiracy at different times, generating multiple Bluebook cases, and (ii) while bid-rigging agreements may allocate a large number of government contracts, individual Bluebook cases may refer to only one or two of the set of contracts governed by the conspiracy. Most groupings were straightforward as individual cases relating to the same violation usually are

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filed in the same District Court within a narrow time frame and cite the same or similar geographic and product markets. As reported in column 2 of Table 1, this method yielded 352 criminal cases alleging horizontal restraints during the calendar years 1970–85. The next two columns break down the cases as to whether misdemeanor or felony penalties applied. These data indicate that felony cases outnumbered misdemeanor cases beginning in 1978.

The set of criminal cases I identified with this methodology includes vastly different cases. Prominent cases, such as the cardboard box price-fixing agreement involving nineteen producers with sales of $2.2 billion in 1974, are included along with obviously less significant cases such as bid-rigging on the sale of crushed stones to a single county in Virginia and an agreement among five retail auto body shops to fix prices for reconditioned bumpers in areas of New Jersey and New York.[21] One useful step in counteracting the problem of heterogeneity in the sample is to distinguish *price-fixing* cases, where the buyers of the price-fixed goods include private parties, from *bid-rigging* cases, where the only buyers are government agencies. Price-fixing cases are the core of Antitrust Division enforcement activity, are a key input into private enforcement, and their deterrent effects may account for the greatest welfare gains from antitrust enforcement. Bid-rigging cases usually involve smaller amounts of commerce. In addition, since many of the bid-rigging cases filed are based on direct evidence of conspiracy (usually the testimony of a conspirator), the case outcomes are likely to differ.

Accordingly, the last two columns in Table 1 provide time-series on the number of price-fixing and bid-rigging cases. The average number of price-fixing cases filed per year fell from 14.3 for the period 1970–1977 to 11.5 for the period 1978-1985. This finding, which is not manifest in the aggregate caseload statistics, is consistent with expectations concerning the impact of the felony penalties, but other factors such as changes in the structure of the economy and more intense international competition may be relevant as well. The time-series on bid-rigging cases (most involving highway construction and electrical contracting) show a sharp rise since the shift to felony penalties. A possible explanation for this unexpected finding is that felony penalties help enforcers establish a credible threat that some conspir-

ators eventually will be convicted, and this threat improves grand jury processes that lead to the indictment of individuals engaged in bid-rigging conspiracies.[22]

Table 2 reports data on the dollar volume of sales affected by the alleged conspiracies for both price-fixing and bid-rigging cases. Although the data are flawed, the average sales figures for price-fixing are many times higher than for bid-rigging cases.[23] The data also indicate a sharp decrease in average sales in price-fixing cases after 1979, when the shift to felony penalties was completed.[24] Together, the declines in the number of price-fixing cases and the average sales affected by the alleged restraints are consistent with the propositions that the increase in penalties reduced the number of price-fixing violations and made the detection of conspiracies involving substantial volumes of commerce more difficult.

B. Plea Choice and Case Outcomes

Did the felony penalties increase the frequency of not-guilty pleas? And did the felony penalties lower conviction rates in litigated cases (those in which defendants plead not guilty)? In principle, these questions could be answered by estimating two separate regressions, one for defendant decisions to plead not guilty (as opposed to either *nolo contendre* or guilty), and a second for the outcomes of litigated cases. Separate estimation, however, ignores the sequential nature of the litigation process. Plea decisions at the first stage determine the cases that go to trial. Given that these cases are not randomly drawn from the set of cases filed, observed changes in conviction rates at the second stage can be due either to (i) changes in the set of litigated cases, or (ii) changes in the behavior of the courts and the parties at trial. In this context, if the felony penalties encourage additional defendants to plead not guilty, then differences in the cases going to trial could confound the question of whether the felony penalties induced adjustments in conviction rules and increased defensive efforts at trial.

For these reasons, I used a bivariate probit regression to estimate jointly the plea decisions at the first stage and the litigated outcomes at the second stage. The first stage uses the

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full set of observations and so is equivalent to a univariate probit. However, the second stage regression uses only the observations in which some or all defendants pleaded not guilty. The bivariate probit estimation of the second stage incorporates the effects of the independent variables on the selection of cases, and thereby isolates their impact on the litigation process.[25]

The unit of observation in the bivariate regression is the set of either individual or corporate defendants in a particular case.[26] One drawback to the bivariate probit estimation is that the dependent variables (the decision to plead not guilty and the outcome of litigated cases) must be binary and, as a result, intermediate values cannot be accommodated. To avoid the exclusion of cases in which some (but not all) defendants pleaded not guilty, I defined the dependent variable in the first stage to be one whenever some defendants pleaded not guilty. The regression, however, does exclude cases involving mixed litigated outcomes — where some of the defendants who pleaded not guilty are convicted while others are acquitted.[27]

The independent variables in each stage are (i) FELONY, which equals one for felony cases and zero for misdemeanor cases, (ii) LN SALES, the natural log of the sales affected by the alleged conspiracy in 1980 dollars, (iii) PRICE-FIXING, which equals one for price-fixing cases and zero for bid-rigging cases, (iv) ECONOMY, which equals one if the economy was in recession when the case was filed and zero otherwise, (v) DEFENDANT, which equals one for corporate defendants and zero for individual defendants, and (vi) REAGAN, which equals one if the case was filed by the Reagan Administration. Table 3 reports the bivariate probit results and, for comparison, univariate probit results for the second stage.

The first-stage results indicate that when the felony penalties apply defendants are more likely to plead not guilty and thereby force the government to prove its case. According to the second stage results, the higher penalties also reduce the probability of conviction in litigated cases. Both effects are statistically significant at the 1 percent level and, as discussed below, involve substantial changes in the probabilities of various case outcomes.

The regression results do not reveal significant effects due to the Reagan Administration in defendant decisions to plead not guilty nor in the outcome of litigated cases. Therefore, the effects associated with the felony penalties should not be attributed to changes in administration enforcement policies regarding criminal antitrust violations. However, general economic conditions do appear to influence the enforcement process. Consistent with the theory that business regulation will weigh producer interests (as opposed to consumer interests) more heavily when the economy is weak,[28] defendants in cases filed during recessions are more likely to plead not guilty (statistically significant at the 1 percent level) and these defendants are less likely to be found guilty (significant at the 10 percent level).

The sales variable and the dummy variable for price-fixing can be interpreted as proxies for treble damage liability, as private plaintiffs are more likely to follow on federal price-fixing cases that involve substantial volumes of commerce. How these factors affect the plea choice is not clear cut: An acquittal presumably reduces expected treble damage liability, but a guilty verdict establishes *prima facie* evidence of liability in subsequent private actions. The bivariate probit results, however, sheds some light on these considerations affect defendant behavior. Higher sales increase the probability of a not-guilty plea and decrease the probability of conviction. The dummy variable for price-fixing cases has the same effect, but is not statistically significant. This pattern of effects is consistent with the idea that defendants with large potential treble damage liability will risk litigation if they have a higher chance of acquittal. Note that the univariate probit regression on litigated outcomes, which does not account for this selection criterion, fails to reveal that higher sales significantly reduce the probability of conviction in litigated cases.

Based on the bivariate probit results, one can identify the impact of the felony penalties and other independent variables on the probabilities of (i) a guilty or *nolo contendre* plea, (ii) a not-guilty plea leading to conviction, and (iii) a not-guilty plea leading to acquittal. Table 4 reports baseline probabilities (evaluated at the mean of all of the independent variables) and the estimated changes in these probabilities due to the independent variables. For the

dummy variables, the probabilities are evaluated at values of one and zero. For the sales variable, elasticities are reported.

According to the results in Table 4, the shift to felony penalties decreases the probability that all defendants would plead either guilty or *nolo contendre* from 77.4 percent to 61.0 percent, and increases the probability of a not-guilty plea leading to acquittal from 10.4 percent to 32.2 percent. The probability of a not-guilty plea leading to acquittal is also substantially higher for price-fixing cases and for cases filed during a recession. The elasticities for the sales variable indicate that a 10 percent increase in sales from its mean increases the probability of a not-guilty plea by 1.3 percent and decreases the probability of a conviction by less than 1 percent.

Insights into the sensitivity of the estimated probabilities to case characteristics can be obtained by considering certain case profiles. For example, the probability that corporate defendants in a price-fixing case filed in the absence of a recession plead not guilty is only 20.5 percent when misdemeanor penalties apply and 36.2 percent when felony penalties apply; and the probability of acquittal for these defendants is 8.3 percent when misdemeanor penalties apply, and 28.1 percent when felony apply. Compared to the baseline probabilities, the government faces a substantially more difficult task in price-fixing cases filed against individual defendants during a recession. When felony penalties apply, the probability of a not-guilty plea leading to acquittal is 40.9 percent, nearly equal to the 41.3 percent probability of either a guilty plea or a *nolo contendre* plea.

Overall the regression results on plea choice and case outcomes are consistent with the proposition that the change to felony penalties produced a broad range of effects. Clearly, the more frequent challenges to the government's allegations reflect an increase in greater defensive efforts. But such efforts are more likely to succeed when the evidence of guilt is weak relative to the conviction standard. Thus, the reduction in the probability of conviction associated with the felony penalties probably reflects a combination of adjustments in conviction rules, greater defensive efforts, and changes in the underlying merit of cases that

go to trial.[29]

In contrast, it is difficult to explain the results if higher penalties only influence the decision to commit an offense. Such a view fails to predict, in the absence of changes in the selection of cases filed, that higher penalties would make prosecution of remaining offenses more difficult. Incorporating the additional feature that defendants are more likely to "take a shot at acquittal" when penalties are increased would explain the greater proportion of not-guilty pleas. However, this view predicts a higher conviction rate because the additional cases selected for litigation would tend to be of lower average merit. The actual finding of a lower conviction rate suggests, therefore, some interplay of the factors cited above, including differences in the severity of offenses committed.

One might also consider whether Priest and Klein's model for the selection of cases for litigation in civil settings might explain the findings.[30] If the felony penalties could be interpreted as an increase in the defendant's stakes, then fewer of the cases selected for litigation would be expected to result in convictions. The lower conviction rate in felony cases is consistent with this view, but this approach would not predict the first stage results — the higher proportion of cases in which defendants plead not guilty. Again, to explain the combination of effects attributable to the felony penalties, one would have to consider the factors discussed here or appeal to changes in the selection of cases by prosecutors.

C. The Overall Effects of Felony Penalties on Antitrust Enforcement

Together the higher probability of not-guilty pleas and the lower probability of conviction suggest that the Antitrust Division's success rate has fallen under the felony regime. The percentage of cases in which defendants plead *nolo contendre*, plead guilty, or are found guilty is one such measure since it is in these circumstances that the courts impose penalties. These outcomes also send positive signals, albeit of varying strength, to potential private plaintiffs who may file follow-on cases. Summary data in Table 5 for corporate and individual defendants reveal lower success rates as defined in felony cases, but the percentage differences

are not great. The weak overall results are due to the fact that the Antitrust Division since the late 1970s has filed a higher proportion of bid-rigging cases and cases in which the volume of commerce affected by the alleged conspiracy is low. Since it historically is more successful in these prosecutions, the Antitrust Division has tended to maintain a high overall success rate since the change to felony penalties.

These comments suggest that total sales in successful cases may better measure the importance of the Antitrust Division caseload than either the number of cases won or its success rate. The sum of dollar sales in successful cases for the years 1970–1985 are presented in Table 6. These annual totals, though incomplete due to some missing data, show a striking decline since 1979. In fact the amounts from the most recent years are less than one-fifth of the total in the peak year, 1978.

The data in Table 6 along with other available evidence put the documented reduction in private antitrust enforcement since the mid 1970s in a new light.[31] A large number of factors may have contributed to the decline, including changes in views about the merit of vertical restraint cases as well as the Supreme Court's *Illinois Brick* and *Brunswick* decisions that sharpened the legal definition of antitrust injury and restricted the standing of private plaintiffs to file cases.[32] Indeed, distinguishing the role of each factor is nearly impossible. But the finding that the number of private cases that followed government cases has declined more sharply than independently initiated private cases probably reflects the reduction in federal enforcement output. Since most of the follow-on cases derive from Sherman Act §1 cases filed by the Antitrust Division, the time-series data on the total sales in successful criminal cases suggest that the 1974 Congressional action to increase criminal penalties contributed to the decline in private litigation.

Of course, alternative explanations for the decline in federal enforcement activity can be forwarded. Since 1980 the Antitrust Division's budget has stayed roughly constant in nominal dollars and the attorney staff has been cut by roughly one-third. These cuts no doubt had an impact on some areas of enforcement, but I would argue that because enforcement

of the Sherman Act §1 rules against horizontal restraints is the Antitrust Division's top priority, efforts in this area did not suffer. Another possible explanation, that changes in the economy and increases in foreign competition have made collusion less likely, is more difficult to evaluate.[33]

V. Concluding Remarks

The approach taken in this article, that higher penalties will have several effects on individual behavior, is consistent with the broader law and economics literature on liability rules, which emphasizes that individuals avoid liability by reducing their level of activity and by increasing care. Alternative views of enforcement that emphasize deterrent effects often ignore other dimensions of individual behavior and fail to consider the issue of whether the courts are willing to impose higher penalties. These further considerations are likely to be important when legal activity is threatened by erroneous convictions and when defensive efforts may consume significant resources.

While this approach suggests that penalties should be limited in many circumstances, the discussion has emphasized the empirical implications concerning Congressional actions that are intended to strengthen deterrence. Specifically, statutorily-mandated increases in penalties will tend to make prosecution of remaining offenses more difficult due to increases in defensive efforts, reduced severity of offenses, and unfavorable (from the prosecutor's point of view) changes in conviction rules. The findings that the felony penalties increased the frequency of not-guilty pleas and reduced conviction rates in litigated cases illustrate that these insights are helpful in evaluating enforcement output. Attributing the reductions in the number of convictions to deterrence alone does not appear correct, which in turn suggests that more attention should be focused on how higher penalties affect other dimensions of individual behavior and the enforcement process.

APPENDIX A

Data Description

I collected data on criminal Sherman Act §1 cases alleging horizontal violations during the period January 1, 1970 to December 31, 1985. On file with the *Journal* is a list of the 352 groupings of individual Bluebook cases. Of the total, 334 observations involve corporate defendants and 245 observations involve individual defendants. For each case, I obtained data on real sales, choice of plea, outcomes, and penalties imposed. Most of the sales data are derived from indictments, but I obtained some figures from actual defendants and from other sources such as reports on state highway construction. The sales figures, which proxy for potential private treble damage liability, are for the duration of the conspiracy up to a maximum of four years. The sales data are crude and sometimes are based only on the sales of actual defendants. Often, data were not available for the full period and I estimated total sales. I used the Producer Price Index to deflate figures to 1980 dollars. I used the *Economic Report of the President, 1987*, Table B-2, to determine whether the economy was in a recession when the case was filed.

The Commerce Clearing House *Trade Regulation Reports* provide information on defendant choice of plea and case outcomes. Regarding case outcomes, by definition Antitrust Division successes include guilty pleas, guilty verdicts, and *nolo contendre* pleas. Dismissal, withdrawal of the case, or acquittal counts as an unsuccessful outcome. The two limit Tobit regressions reported in Appendix B use the information on mixed pleas and mixed outcomes. When I encountered mixed pleas, I weighted criminal counts within a case equally. Likewise, I weighted each criminal count equally for cases with mixed outcomes.

APPENDIX B

Two-Limit-Tobit Regression Results

This appendix reports two-limit-Tobit regression results that bear on the question of possible bias due to the exclusion of some cases from the bivariate probit regression. As indicated in the text, the bivariate probit regression only permits binary dependent variables. To avoid excluding cases in which only some defendants pleaded not guilty, I defined the dependent variable in the first stage of the bivariate probit regression as equal to zero if no defendants pleaded not guilty and equal to one otherwise. I did, however, exclude from the regression 37 litigated cases that resulted in mixed outcomes. In contrast, two-limit-Tobit regressions permit intermediate values of the dependent variables and thereby avoid exclusion of these cases.

Table B1 presents two-limit-Tobit results for each stage of the litigation process. The estimated coefficients in the first stage (plea choice) yield a pattern of results in terms of sign and statistical significance that is similar to the first stage of the bivariate probit results reported in Table 3. The only exception is that whereas the positive effect of sales on the likelihood of not-guilty pleas in the bivariate probit regression is significant at the 1 percent level, it is only significant at the 10 percent level in the Tobit regression. It appears, therefore, that the definition of the dependent variable in the first stage of the bivariate regression and the exclusion of some cases does not bias the first stage results.

The second two-limit-Tobit regression on litigated cases, unlike the bivariate probit, does not account for the impact of the independent variables on the cases selected for litigation. Therefore, these results are comparable to the second stage univariate probit results reported in Table 3. The estimated coefficients from the Tobit regression yield the same pattern in terms of sign and significance as the univariate probit regression. In comparing the second-stage Tobit results to the bivariate probit results, the only difference is that LN SALES is not statistically significant in the Tobit regression but is in the bivariate probit regression at

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the 5 percent level.

Footnotes

1. The Antitrust Procedures Act of 1974 increased criminal antitrust penalties from the misdemeanor level, with a maximum $50,000 fine and one year jail sentence per count, to the felony level, with a maximum $1 million fine and three year jail sentence per count.

2. In 1984 Congress passed the Insider Trading Penalties Act to increase penalties and the Comprehensive Crime Control Act to create the U.S. Sentencing Commission, which has issued sentencing guidelines for all federal crimes. The guidelines change average penalties for certain offenses and require judges to impose determinative sentences, meaning the sentence imposed will equal the actual sentence served less a small consideration for good behavior.

3. In partially or fully successful cases filed during the period 1970-85, fines in 1980 dollars averaged $18,872 per defendant in misdemeanor cases and $98,083 in felony cases. These data are based on the amount imposed by the District Court at the time of sentencing, and do not account for motions to reduce or non-payment. Jail sentences averaged 5 days per individual defendant in misdemeanor cases and 37 days in felony cases. Furthermore, the percentage of successful cases which impose jail sentences is higher for felony cases (73 percent) than for misdemeanor cases (64 percent). These data exclude suspended sentences, but do not take into account changes in actual time served due to subsequent motions to reduce sentence and probation.

4. In the words of Polinsky and Shavell, "the occurrence of legal error seems inevitable." A. Michael Polinsky and Steven Shavell, Legal Error, Litigation, and the Incentive to Obey the Law, 5 J. of Law, Econ., and Org. 99 (1989).

5. For historical evidence regarding sentencing practices, see the Case Charts and Tables 1, 15, and 37 in James M. Clabault and Michael K. Block, Sherman Act Indictments: 1955-1980 (1981).

6. For comments on these views, see Edward A. Snyder, New Insights into the Decline of

Antitrust, 7 Contem. Pol'y Iss. 1 (1989).

7. For evidence on the filings of follow-on and independently initiated cases, see Thomas E. Kauper and Edward A. Snyder, An Inquiry into the Efficiency of Private Antitrust Enforcement, 46 Georgetown Law J. 1163 (1986).

8. See Milton Harris and Artur Raviv, Some Results on Incentive Contracts, 68 Am. Econ. Rev. 74 (1978).

9. Gary S. Becker, Crime and Punishment: An Economic Approach, 76 J. Pol. Econ. 169 (1968), advanced this line of argument, stating at 183 that enforcement costs are minimized " ... by lowering p (probability of conviction) arbitrarily close to zero and raising f (fine upon conviction) sufficiently high so that the product pf would induce the optimal number of offenses." For a complimentary analysis, see Robert M. Townsend, Optimal Contracts and Competitive Markets with Costly State Verification, 21 J. Econ. Theory 265 (1979). Several qualifications to this view have been offered. George J. Stigler, Optimum Enforcement of Laws, 78 J. Pol. Econ. 526 (1970), demonstrates that higher penalties must preserve marginal deterrence, meaning more severe crimes must have higher expected costs. Gary S. Becker and George J. Stigler, Law Enforcement, Malfeasance, and Compensation, 3 J. Law & Econ. 1 (1976), note that large penalties for malfeasance will increase detection costs and encourage more resources to be spent in dispute resolution. Richard A. Posner, Economic Analysis of Law, 2nd Ed. (1982) at 167, observes that saving enforcement resources by increasing the fine upon conviction is often unrealistic due to the costs of collecting fines that approach an individual's wealth. For how risk aversion affects the analysis, see A. Michael Polinsky and Steven Shavell, The Optimum Tradeoff Between the Probability of and Magnitude of Fines, 69 Am. Econ. Rev. 880 (1979).

10. It should be noted that even if courts could define rules to fully protect non-offenders, it may not be efficient to do so given (i) the greater enforcement resources needed to maintain a given level of deterrence, and (ii) the possibility that some offenders, with

little effort to disguise their actions, could completely avoid detection.

11. The principal-agent literature has analyzed a similar problem in which principals can-not reliably assess agent's efforts. See, for example, Steven Shavell, Risk Sharing and Incentives in the Principal and Agent Relationship, 10 Bell J. Econ. 55 (1979). The principal-agent framework, however, is not suited for analyzing the effects of higher crim-inal penalties because it ignores the likelihood that individuals will take actions to reduce the probability of being indicted and to influence the court's verification process after in-dictment. As noted by Mirrlees: "The accuracy of the principal's observation ... should depend upon the agent's own efforts to affect the observation; but I shall ignore this, im-portant though it may be." James A. Mirrlees, The Optimal Structure of Incentives and Authority with an Organization, 7 Bell J. Econ. 105 (1976), at 121. In contrast, models of litigation often include litigant efforts to affect outcomes. See William M. Landes, An Economic Analysis of the Courts, 14 J. Law & Econ. 287 (1971).

12. Presumably the decision whether to commit a crime and the choices of offense severity and defensive efforts are made jointly. Assuming risk-neutrality, potential offenders of the antitrust laws would identify the offense severity (the size of the markup, extent of the conspiracy) and the level of defensive efforts that maximize income net of the expected penalty. If at the optimal levels of offense severity and defensive efforts, the expected income exceeds the income from legal activities, then the individual chooses to commit the offense.

13. William M. Landes and Richard A. Posner, Market Power in Antitrust Cases, 94 Harv. Law Rev. 937 (1981), show that a cartel will charge a higher markup the greater its share of industry capacity.

14. Reducing the offense severity and increasing defensive efforts are, however, substitute means of limiting exposure to higher penalties. Therefore, both effects need not occur. Moreover, the effects on offense severity and defensive efforts may interact. For example, the marginal product of defensive efforts may be lower for less severe offenses, in which

case a higher penalty will more strongly decrease offense severity and have only a weak effect on defensive efforts.

15. Therefore, unless there is a basis for discriminating between the efforts by offenders and those by non-offenders, limiting defensive efforts either may increase or decrease the extent of legal error. Further, Rubinfeld and Sappington, demonstrate that permitting individuals to spend resources in their defense may provide the courts with signals about their guilt or innocence. Daniel L. Rubinfeld and D.E.M. Sappington, Efficient Awards and Standards of Proof in Judicial Proceedings, 18 Rand J. Econ. 308 (1987).

16. Judicial interest in following legislative intent may preempt efficiency-motivated adjustments in conviction rules. If so, the deterrent effects of the felony penalties would be more prominent than otherwise. Mitigating actions may also be absent if the courts were in the process of strengthening enforcement, possibly because of the same factors that encouraged the legislation. But in this case there is no evidence that the courts were moving toward stiffer sanctions.

17. Whether the base probability is, for example, 10 percent or 20 percent should not affect affect an offender's decision to forego the benefits from a more severe offense that would increase the probability of detection and conviction by 5 percent. Regarding corner solutions, note that if the probability of conviction is nearly zero, then offenders will choose more severe offenses and lower levels of defensive efforts than dictated by the marginal conditions.

18. See the evidence in Howard P. Marvel, Jeffry M. Netter, and Anthony M. Robinson, Price Fixing and Civil Damages: An Economic Analysis, 50 Stan. Law Rev. 561 (1988).

19. According to the Gypsum decision, which was handed down during the transition to felony penalties, the per se status of certain offenses does not relieve the prosecutor of the burden of proving criminal intent. U.S. v. United States Gypsum Co., 438 U.S. 422 (1978).

20. Bluebook cases refer to the Antitrust Division case citations in Commerce Clearing

House, Trade Regulation Reporter, Volume 4, New U.S. Antitrust Cases, Complaints, Indictments, and Developments (1989).

21. See (i) U.S. v. Keystone Automotive Plating Corp., *et al.* and U.S. v. Tidewater Crushed Stone and Asphalt Co., Trade Cases (CCH), ¶53,637 and ¶53,602, and (ii) U.S. v. International Paper Co. *et al.* and U.S. v. Boise Cascade Corp., *et al.*, Trade Cases (CCH) ¶53,713.

22. The success of a grand jury often depends on "turning" witnesses who, after a plea agreement is reached with prosecutors, offer complete testimony about the alleged illegal conduct. The available data indicate that prosecutors have relied heavily on plea agreements in bid-rigging cases following the shift to felony penalties. In the calendar years 1969 to 1976, only one Bluebook case involved a plea agreement. The number increased substantially thereafter: 2 in 1977; 9 in 1978; 9 in 1979; and 54 in 1980. Bid-rigging cases account for almost all the plea agreements, suggesting that the penalties may have enhanced the ability of prosecutors to prosecute bid-rigging violations.

23. The sales figures are inconsistent because for some cases they account only for the set of firms involved in the alleged conspiracy, while for others they include rivals. Also, the time period for which data are available does not always correspond to the period of the conspiracy. Finally, even if they were accurate and consistent, sales data do not reflect differences in illegal profits.

24. Other evidence indicates that firms indicted for price-fixing during the 1980s tended to have few assets; see Jon Joyce, Effect of Firm Organizational Structure on Incentives to Engage in Price-Fixing, 7 Contem. Pol'y Iss. 19 (1989).

25. For a discussion of the advantages of bivariate probit regression analysis with selection in applications to litigation, see James W. Hughes and Edward A. Snyder, Medical Malpractice Reforms: What Can We Learn from Claims Data?, 7 J. Bus. & Econ. Stat. 423 (1989).

26. Even though the shift to felony penalties led to substantially higher penalties for both

corporate and individual defendants, the effects of the felony penalties may be expected to differ by defendant type since only individual defendants face the higher probability of incarceration. It should be noted that while estimating separate regressions for corporate and individual defendants might be preferred, problems arise from the relatively small number of cases selected for litigation. For example, of the 169 cases in the second stage equation, corporate defendants pleaded not guilty in only two bid-rigging cases when felony penalties applied. Finally, while the ideal unit of observation might be single defendants, no defendant-specific information is available.

27. To check for possible bias from the exclusion of some cases, I ran two-limit-Tobit regressions that include all cases and permit intermediate values of the dependent variables in both the first and second stages. These results are reported in the Appendix B.

28. See Sam Peltzman, Toward a General Theory of Regulation, 19 J. Law & Econ. 211 (1976).

29. In principle a reduction in offense severity due to the increase in penalties is sufficient to explain the observed changes in plea decisions and case outcomes. Given a substantial reduction in the severity of offenses, a higher proportion of those indicted might plead not guilty and the cases going to trial might be of lower merit from the prosecutor's point of view. This explanation for the findings, however, leaves unanswered the question of why the courts would not have applied stiffer penalties before the statutory change. It is more likely that the lower conviction rate also reflects changes in the implied burden of proof on government prosecutors.

30. George L. Priest and Benjamin Klein, The Selection of Disputes for Litigation, 13 J. Legal Stud. 1 (1984), demonstrate that when the stakes for parties engaged in civil litigation are asymmetric (for example, due to reputation effects), the parties for whom the stakes are greater will win more of the cases selected for litigation.

31. See the rules embodied in the Illinois Brick Co. v. Illinois, 431 U.S. 720 (1977) and Brunswick Corp. v. Pueblo Bowl-O-Mat, Inc., 429 U.S. 477 (1977).

32. For a detailed evaluation of recent trends in private antitrust enforcement, see Steven C. Salop and Lawrence J. White, Economic Analysis of Private Antitrust Litigation, 46 Georgetown Law J. 1001 (1986).

33. See Snyder, *supra* note 6 for a discussion of factors that may account for the declines in certain areas of antitrust enforcement.

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TABLE 1.

Antitrust Division Enforcement Data

Calendar Year	Total Bluebook Cases	Sherman Act Section 1 Criminal Cases	Criminal Penalty		Type of Case	
			Misdemeanor	Felony	Price-Fixing	Bid-Rigging
1970	67	8	8	0	8	0
1971	56	7	7	0	7	0
1972	96	17	17	0	15	2
1973	60	14	14	0	13	1
1974	71	18	18	0	13	5
1975	63	21	21	0	19	2
1976	65	21	18	3	21	0
1977	59	19	12	7	18	1
1978	62	28	5	23	22	6
1979	51	15	2	13	11	4
1980	115	24	0	24	14	10
1981	78	19	0	19	5	14
1982	109	23	0	23	3	20
1983	113	39	0	39	12	27
1984	101	54	0	54	13	41
1985	62	25	0	25	12	13
TOTAL	1228	352	122	230	206	146

Source.-- The data on "Bluebook" cases are derived from Commerce Clearing House, Trade Regulation Reporter, Volume 4, New U.S. Antitrust Cases, Complaints, Indictments, and Developments (1989). The Sherman Act Section 1 criminal cases in column 2 combine related Bluebook cases that allege a horizontal restraint of trade.

Notes.-- Misdemeanor and felony cases refer to the potential criminal penalty imposed upon conviction. Three cases combined misdemeanor and felony penalties, but since felony counts dominated, these are classified as felony cases. A case is classified as price-fixing if the goods or services related to the alleged violation were sold to private parties. A case is classified as bid-rigging if the goods or services related to the alleged violation were sold exclusively to government agencies.

TABLE 2.

Real Sales ($M) in Sherman Act Section 1
Criminal Cases Alleging Horizontal Offenses

Calendar Year	Price-Fixing Cases			Bid-Rigging Cases		
	Sum	Cases	Average	Sum	Cases	Average
1970	$2,088	7	$348	$0	0	--
1971	685	7	98	0	0	--
1972	7,397	15	493	13	2	$7
1973	3,277	13	252	14	1	14
1974	5,102	13	392	808	5	162
1975	3,587	19	189	5	2	2
1976	15,130	21	721	0	0	--
1977	2,917	16	182	76	1	76
1978	15,024	22	683	620	5	124
1979	10,207	11	928	99	4	25
1980	1,748	14	125	772	10	77
1981	1,261	5	252	394	14	28
1982	275	3	92	320	19	17
1983	1,328	10	133	755	26	29
1984	85	10	9	59	38	2
1985	774	9	86	79	10	8

Source.-- See Appendix A.

Notes.-- Sales are deflated by the Producer Price Index and are in 1980 dollars. Sales data are not available for 30 of the total 352 cases.

TABLE 3.

Regression Analysis of Plea Choice and Litigated
Outcomes in Criminal Antitrust Cases, 1970-1985

First Stage - The decision to plead not guilty. The dependent variable
equals zero if the set of defendants pleads either nolo
contendre or guilty, and equals one otherwise.

Second Stage - Outcome of litigated case. The dependent variable equals
equals zero if the defendants who pleaded not guilty are
found guilty, and one if they are acquitted.

Independent variables

| | BIVARIATE PROBIT | | UNIVARIATE PROBIT |
	1st Stage	2nd Stage	2nd Stage
FELONY	0.47***	- 0.79***	- 1.00***
	(0.16)	(0.18)	(0.29)
PRICE-FIXING	0.17	- 0.23	- 0.30
	(0.17)	(0.20)	(0.31)
LN SALES[+]	1.11***	- 0.82**	- 0.07
	(0.32)	(0.37)	(0.56)
ECONOMY	0.47***	- 0.28*	0.14
	(0.16)	(0.17)	(0.24)
DEFENDANT	- 0.14	0.29**	0.48**
	(0.12)	(0.14)	(0.21)
REAGAN	0.11	- 0.12	- 0.06
	(0.19)	(0.21)	(0.33)
CONSTANT	- 1.25***	1.66***	0.09
	(0.23)	(0.26)	(0.40)
Number of Observations	504	169	169
Chi-Square Statistic for Likelihood Ratio Test[++]		57.8***	23.4***

TABLE 3. (Continued)

*Significantly different from zero at the .10 level.
**Significantly different from zero at the .05 level.
***Significantly different from zero at the .01 level.

Notes.-- Standard errors are in parentheses. The bivariate probit regressions were estimated using LIMDEP. These regressions exclude 37 observations involving mixed litigated outcomes. See Appendix B regarding the question of selection bias. The univariate probit regression does not account for non-random selection of cases that go to trial. Independent variables are defined in the text.

$^{+}$ The coefficient for the natural log of sales is multiplied by 10.

$^{++}$ The Likelihood Ratio Test statistic is defined by $2[L(B_{ml})-L(B_c)]$ ~ Chi-Square, where $L(B_{ml})$ is the likelihood function using the maximum likelihood estimator and $L(B_c)$ is the likelihood function when the coefficients are contrained to be zero.

TABLE 4.

Impact of Independent Variables
on Estimated Probabilities of Case Outcomes

	Nolo or Guilty Plea	Not Guilty Plea Leading to Acquittal	Not Guilty Plea Leading to Conviction
Baseline Case[+]	.674	.239	.087
Penalty			
Felony	.610	.322	.068
Misdemeanor	.774	.104	.122
Type of Case			
Price-Fixing	.649	.267	.084
Bid-Rigging	.709	.197	.094
Economy			
Recession	.524	.307	.218
No Recession	.704	.217	.079
Type of Defendant			
Corporate	.695	.200	.105
Individual	.644	.291	.045
Reagan Administration			
During Reagan Years	.652	.259	.089
Pre-Reagan Years	.690	.223	.087

Elasticity of Probability of
Guilty Plea with respect to Sales[++]

Baseline Case	0.182
Felony Cases	0.217
Misdemeanor Cases	0.126

Elasticity of Probability of
Conviction with respect to Sales[++]

Baseline Case	- 0.331
Felony Cases	- 0.300
Misdemeanor Cases	- 0.379

TABLE 4. (Continued)

Notes.-- The estimated probabilities are based on the bivariate probit
regression results reported in Table 3. The category "Not Guilty Leading to
Acquittal" means that some, but not necessarily all, defendants in the case
pleaded not guilty and were acquitted. Hence, this category includes cases
in which the government was partially successful.

$^{+}$ The baseline probabilities are evaluated at means of independent
variables.

$^{++}$ The elasticities are evaluated at the means of the other independent
variables.

TABLE 5.

Outcomes of Criminal Cases Alleging
Sherman Act Section 1 Violations, 1970-1985

Corporate Defendants:

	Successful	Unsuccessful	Mixed	Subtotal	Missing	Total
· All Cases	255	21	54	330	4	334
(percent of subtotal)	73.8	6.5	19.8			
Felony Cases	150	16	44	210	4	214
(percent of subtotal)	71.4	7.6	21.0			
Misdemeanor Cases	105	5	10	120	0	120
(percent of subtotal)	82.6	5.0	12.4			

Individual Defendants:

	Successful	Unsuccessful	Mixed	Subtotal	Missing	Total
All Cases	164	25	53	242	3	245
(percent of subtotal)	67.8	10.3	21.9			
Felony Cases	102	23	40	165	3	168
(percent of subtotal)	61.8	13.9	24.2			
Misdemeanor Cases	62	2	13	77	0	77
(percent of subtotal)	80.5	2.6	16.9			

Notes.-- Successes are defined as cases in which defendants plead guilty or nolo contendre or are found guilty. Acquittals, dismissals, and withdrawals account for unsuccessful cases. The difference between the successful and unsuccessful categories in felony and misdemeanor cases for corporate defendants is not significant at the 5 percent level (Chi-Square = 2.44), but is statistically significant for individual defendants (Chi-Square = 8.61).

TABLE 6.

Total Sales (1980 $M) in Successful or Partially
Successful Sherman Act Section 1 Criminal Cases, 1970-1985

Calendar Year	PRICE-FIXING CASES		BID-RIGGING CASES		COMBINED
	Number of cases	Total Sales	Number of cases	Total Sales	Total Sales
1970	8[1]	$2,088	0	0	$2,088
1971	7	485	0	0	485
1972	14	2,028	2	13	2,041
1973	12	3,273	1	14	3,287
1974	13	5,102	5	808	5,910
1975	17	1,670	2[1]	5	1,675
1976	19	12,497	0	0	12,497
1977	18[2]	2,917	1	76	2,993
1978	21[1]	14,562	5	620	15,182
1979	10	10,152	3	96	10,248
1980	11	1,528	9	768	2,296
1981	5	1,261	14	394	1,655
1982	2	113	19[1]	289	402
1983	11[2]	1,325	27[1]	755	2,080
1984	12[3]	83	32[3]	48	132
1985	10[3]	609	12[3]	78	687

Source.- See Appendix A.

[1,2,3] Indicates the number of successful cases with missing sales data.

TABLE B1.

Two-Limit-Tobit Regression Analysis of Plea Choice and Litigated Outcomes in Criminal Antitrust Cases, 1970-1985

First Stage – The decision to plead not guilty. The dependent variable is the proportion of defendants pleading not guilty.

Second Stage – Outcome of litigated case. The dependent variable equals the proportion of defendants who, having pleaded not guilty, are convicted.

Independent variables

	1st Stage	2nd Stage
FELONY	0.53***	− 1.41***
	(0.18)	(0.48)
PRICE-FIXING	0.01	− 0.63
	(0.17)	(0.41)
LN SALES[+]	0.68*	− 0.15
	(0.35)	(0.82)
ECONOMY	0.49***	0.05
	(0.16)	(0.37)
DEFENDANT	− 0.18	0.68**
	(0.13)	(0.33)
REAGAN	− 0.14	− 0.42
	(0.20)	(0.45)
CONSTANT	− 0.88***	0.99
	(0.23)	(0.61)
Number of Observations	541	206
Chi-Square Statistic for Likelihood Ratio Test[++]	24.9***	22.0***

TABLE B1. (Continued)

*Significantly different from zero at the .10 level.
**Significantly different from zero at the .05 level.
***Significantly different from zero at the .01 level.

Notes.-- Standard errors are in parentheses. The second-stage Tobit regression does not account for non-random selection of cases for litigation.

[+] The coefficient for the natural log of sales is multiplied by 10.

[++] The Likelihood Ratio Test statistic is defined by $2[L(B_{ml})-L(B_c)] \sim$ Chi-Square, where $L(B_{ml})$ is the likelihood function using the maximum likelihood estimator and $L(B_c)$ is the likelihood function when the coefficients are contrained to be zero.