

FINAL REPORT

THE EFFECTS OF INSTITUTING MEANS-BASED FINES IN A CRIMINAL COURT: THE STATEN ISLAND DAY-FINE EXPERIMENT

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September 1991

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**U.S. Department of Justice
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Prepared under Grant No. 87-IJ-CX-0001 from the National Institute of Justice, U.S. Department of Justice.

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I. INTRODUCTION

A. The Fine as an Intermediate Sanction

Intermediate sanctions are not new in American sentencing and fines in particular are a very ancient and widely used criminal penalty in our courts. What is new is the increased enthusiasm for the systematic incorporation of these primarily nonincarcerative sanctions into sentencing systems. In the last decade there has been an explosion of experimental alternative sanctions including community service orders, house arrest, electronic monitoring, intensive probation, boot camps and more. This development is in direct response to the pressing fiscal and justice concerns that have arisen from the uniquely American reliance on imprisonment as the primary means of punishing criminal behavior. Continued concern about our financial capacity to use incarceration to deliver fair and just punishment has spurred interest in creating a graduated progression of intermediate penalties, permitting imprisonment to be reserved as a response to violent, predatory crimes.

Fines have many characteristics that lead them to be used more widely as a criminal penalty in American courts, as well as across northern Europe and elsewhere, than is commonly recognized. These same characteristics make fines especially well-suited to systematic application as an intermediate penalty.

The fine is unmistakably punitive and deterrent in its aim, fitting well into current trends toward retribution and deterrence in sentencing philosophy. It stresses offender accountability by demanding the offender pay his or her debt to society, and permits the size of that debt to be scaled to reflect the severity of a particular offense across an almost unlimited range of criminal behavior. This flexibility also extends to adjusting the size of the offender's fine to his or her income so that equal punishment can be administered across offenders with vastly different financial circumstances who are convicted of the same crime. Fines can also be enforced relatively easily and inexpensively, and the offender can remain

in the community. Despite the widespread belief in American courts that fines cannot be collected, the track record of courts provides ample support for viewing properly set criminal fines as enforceable.

Finally, the fine is already part of the sentencing repertoire of most American courts, large and small, urban and rural, and the structures to administer it effectively are generally in place. In addition, unlike other intermediate penalties, fines generate revenue. They can, therefore, be financially self-sustaining and possibly even provide revenue for other related purposes such as victim compensation.

Despite these obvious advantages, the fine is only now becoming developed as an intermediate penalty in the United States. Although criminal fines are common in limited and general jurisdiction courts in this country, their use is highly variable. Fines are typically imposed for less serious offenses or combined with other non-custodial sanctions rather than allowed to stand alone as the sole punishment. This is in stark contrast to the use of fines in much of Western Europe where they are imposed, as sole sanctions, as the sentence of choice in most criminal cases, including crimes against persons and offenses equivalent to some American felonies.

Until recently, the fine had not come into similar prominence as an intermediate penalty in the United States because of the deep skepticism among American criminal justice practitioners about the ability of judges to set fine amounts that are large enough to punish and deter, yet collectible and fairly imposed across offenders with vastly different economic circumstances. This skepticism, however, is beginning to dissipate, as more American courts explore fining systems that vary fine amounts systematically in relationship to the means of the offender as well as the severity of the offense -- systems with which European courts have long experience.

B. Day fines in Europe and America

The variable fine systems used in Europe (typically called "day fines" because some portion of an offender's daily income is used in calculating the fine amount) are very

different from the typical fixed fine systems used in most American courts. American judges generally apply "going rates" for fines that are based upon (usually informal) understandings that the same or similar amounts will be imposed on all defendants coming before the court convicted of a particular offense. Such tariff systems, however, have limited the usefulness of the fine as an intermediate sanction in the United States. Because the tariffs tend to be set to reflect the lowest common economic denominator of offenders coming before the court, fine amounts are depressed and the range of offenses for which judges will view a fine as an appropriate sole sanction becomes constricted.

Some American courts have begun experimenting with the European day-fine model. The first of these was a pilot project planned and implemented in the Criminal Court of Richmond County (Staten Island), New York, between 1988 and 1990. This project was a joint effort by the court and the Vera Institute of Justice, and funded by the National Institute of Justice (NIJ) and the City of New York. It consisted of an 18-month planning period, involving the Staten Island Criminal Court bench, prosecutors, public and private defense attorneys, court administrators, and planners and researchers from the Vera Institute in New York City; a one-year test period in which day fines were substituted for fixed fines on a regular basis; and a quasi-experimental evaluation of the pilot year carried out by the Vera Institute's Research Department.

The Staten Island day-fine pilot project was a product of ten years of policy research, supported by the National Institute of Justice. As described below, this was a successful pilot; it demonstrated that the day-fine concept could be implemented in a rather typical American court, that day fines could substitute for fixed fines, that fine amounts increase for more affluent offenders under day fines, that overall revenues increase, and that high rates of collection can be sustained (and possibly improved) despite the higher day-fine amounts. Indeed, based on the model developed in the Staten Island pilot, there has been continued adaptation of the concept to the American context in jurisdictions outside New York.

The Superior Court of Maricopa County (Phoenix, Arizona) and its Adult Probation Department are implementing a pilot that will extend day fines into the felony range (Pilcher and Windust, 1991). In addition, three sites will shortly be selected by the Bureau of Justice Assistance to develop day fines as part of a national demonstration to be evaluated by NIJ (BJA, 1991). This effort will provide an important test of the capacity of American courts not only to develop day-fine sentences, but also to place them within an array of intermediate penalties that provides an alternative to imprisonment.

Finally, two states that have already moved significantly in the development of structured sentencing schemes (Minnesota and Oregon) are beginning a process to integrate nonincarcerative penalties into a graded progression of sanctions. In 1990, the Minnesota legislature directed its sentencing guidelines commission to establish a system of day fines as part of this effort, and in Oregon the sentencing guidelines council is moving forward on a parallel path.

It remains to be seen, however, whether these or other American jurisdictions will move in the direction suggested by Norval Morris and Michael Tonry, in their pathbreaking book on intermediate punishments, to make "the fine the basic coin of punishment, ...the preferred sanction in all cases where a prison sentence of two years or less is to be imposed" (1990, pp. 123-124).

C. The Current Report

This report of the evaluation of the Staten Island day-fine pilot project is the last in a series of major reports based on studies funded by NIJ and produced by the Research Department of the Vera Institute documenting the evolution of this significant criminal justice reform. In addition to the work of the Vera Institute, several other organizations and scholars have made major recent contributions to the development of knowledge in this field, several with support from NIJ.

The next section of this report will briefly review the development of this reform effort as a context for the full discussion of the evaluation findings which comprises the

remainder of this monograph. Interested readers are encouraged to review the documents and publications referred to in the text, and in the bibliography, for all the rich detail they contain about the status of the fine as a criminal penalty in the United States and abroad; about collection techniques and their outcomes; about the underlying jurisprudence and structure of the Staten Island day-fine model; and about planning and implementation strategies.

Following the review section (II) is a brief summary of the findings of the evaluation of the Staten Island pilot (section III). This section focuses on the major outcomes of the pilot in the area of sentencing patterns, fine amounts, compliance/collection rates and displacement effects. It is intended to provide a complete, but short and non-technical, review of the evaluation results.

In the last section of this report (IV), these same findings are presented with much greater technical detail, and can be passed over by all but the professional researcher and more statistically trained reader. This level of detail is included in the report because this project reflects the first attempt to study systematically the impact of introducing day-fines into an American sentencing scheme; it will not, however, be the last. Research in this area is likely to increase in the next several years, including the NIJ evaluation of the BJA national demonstration, and Vera researchers wanted to ensure that the research methods and statistics used in this study were recorded for others to build upon and replicate.¹

¹ Appendix A also contains a detailed description of the research design, including sample selection, data sources, follow-up periods, major analytic variable definitions, and the overall structure of the analysis. Appendix B contains a full set of all the technical tables referred to in the text.

II. FINES AND DAY FINES: EVOLUTION OF A SENTENCING REFORM

A. Filling the Gap in Empirical Information about Fines

In 1979, the NIJ released a request for a proposal on the use of fines as a criminal sanction, calling attention to its neglect in American empirical policy research (and in American jurisprudence) and noting the widespread use of fines in Western Europe.²

Vera Institute researchers had already become interested in the same phenomenon, both as a result of research on New York City's courts which revealed far more extensive use of fine sentences in the lower courts than was traditionally recognized and as a result of action-research in England where fines had been a mainstay of the court's sentencing repertoire for many years. In collaboration with colleagues from the Institute for Court Management (now part of the National Center for State Courts) and with funds from NIJ (and from the City of New York and the German Marshall Fund), Vera researchers did an extensive review of the use and collection of fines in criminal courts across the United States.

Published in 1984 (Hillsman, Sichel and Mahoney), this first empirical monograph on criminal fines and the ten Working Papers compiled during the research³ indicated that fines were more widely used in both limited and general jurisdiction courts than was commonly acknowledged in the policy and sentencing literature, and that collection was better, at least in some places, than was recognized. Nevertheless, fine use was extremely variable across courts, collection rates were not uniformly high, judges and court administrators were often lacking in information about the fine situation in their own courts,

² At this time, virtually the only favorable commentary in American journals about the possible expansion of fine use in the United States, and noting the English experience, was an article in Judicature by Carter and Cole (1979).

³ See the bibliography.

and relevant information for monitoring fine use and collection and for policymaking was inadequate in most jurisdictions.

The report also documented the quite different situation in Western Europe, where fines were the sentence of choice in most criminal cases and in some countries were used as a major alternative to incarceration.⁴ The authors concluded that fine use and compliance with fine sentences could be significantly improved in the United States if courts attempted to do what the Europeans did so successfully -- set fine amounts routinely and systematically in relation to both the severity of the offense and the means of the offender -- that is, adapt the European day fine to American courts.

In 1986, Vera researchers in New York and London completed the second monograph based upon research on fine collection in four English magistrates' courts (Casale and Hillsman). Supported by NIJ, this study attempted to explore what collection and enforcement techniques and overall strategies were most successful in securing high levels of compliance and revenue collection. Detailed case studies revealed the intimate relationship between how fines were set (that is, what fine amount the judge set, whether it was combined with other monetary penalties such as restitution, and what terms for payment were established for the total amount due) and the success of fine collection and enforcement efforts.

The report described in considerable detail the various techniques used in the English magistrates' courts to secure payment. It concluded that strategies emphasizing routine notification and close monitoring of payments from the date of sentence were successful in securing high levels of compliance, especially when the amounts set by the

⁴ Research on West Germany by economist Robert Gillespie also provided empirical support for the effectiveness with which fines and day fines were used in Europe (1980, 1981). Similarly, the study of the German day-fine system by Hans-Jorg Albrecht at the Max Planck Institute was most instructive (1980).

sentencing court bore some relationship to the means of the offender. Even high fine amounts imposed on relatively poor offenders could be collected, if close monitoring was combined with credible threats of more coercive action (especially seizure of property and imprisonment for default). However, if fine amounts and financial capability were too discrepant, collection became increasingly difficult and imprisonment for default became a more likely, and sometimes unjust, outcome.

The authors, therefore, again called for testing the feasibility of adapting the European day-fine concept to American courts, and suggested it be combined with improved, individualized methods of monitoring collection.⁵

B. Judicial Attitudes toward Fines

These two studies had made substantial headway filling the gap in empirical information about fining systems in the United States and Europe and developing policy recommendations about how the American process of implementing and administering fine sentences might be improved. Judicial views about current fining practices (both sentencing and collection) and the extent to which judges were open to innovation in the use of fines, however, remained opaque. In 1987, therefore, researchers at the University of Connecticut and the Institute for Court Management of the National Center for State Courts released the results of a national survey of judges in both limited and general jurisdiction trial courts, that had been funded by NIJ (Cole, Mahoney, Thornton, and Hanson).

The survey confirmed much of the earlier research, but added considerable depth to our understanding about when judges use fines, with what other sanctions they combine them, how they take means into account and what they know (and, more often, don't know)

⁵ The lessons from these two studies on criminal fine collection and enforcement strategies and from the experiments undertaken by several court administrators to improve collections in traffic cases were compiled in a special issue of The Justice System Journal (Volume 13 [1], Spring 1988); citations are in the bibliography below.

about the fine collection process post-sentence. The study's findings emphasized the limited amount of information on financial circumstances routinely available to sentencing judges and their hesitation about using a fine as the sole sanction in a more serious case, even when the offense was a property crime. In addition, however, the survey revealed that many judges in general as well as limited jurisdiction courts were interested in the day-fine concept, and many judges expressed willingness to experiment with it in their own courts.⁶

C. Planning a Pilot Test of the Day-Fine Concept in an American Court

Policy research then shifted significantly into planning for a one-site day-fine demonstration project. In 1986, Vera's Director of Court Programs made a three-week trip funded by the German Marshall Fund to several European countries to examine first hand the operation of their day-fine systems (Greene, 1987). The Richmond County Criminal Court bench and the county's District Attorney expressed enthusiasm for a collaborative planning process with the Vera Institute that would enable them to test the first American day-fine model. And NIJ was willing to continue its support of this overall effort by providing a planning grant.

A detailed description of the planning process and the components of the day-fine model developed for Staten Island was published by the Vera Institute at the end of 1987 (Hillsman and Greene).⁷ This report described the architecture developed by the joint court-Vera planning group for the day fine, an amalgam of the West German day-fine model and the Swedish model. It documented the various strategies that were tested by the planning group to establish the number of day-fine units to be used for each common

⁶ A detailed summary of the research and literature on fines and day fines was prepared for volume 12 of Crime and Justice : An Annual Review, published in 1990 (Hillsman).

⁷ A summary of this report was also published in Judicature in 1988, along with a commentary by one of the Staten Island judges in the planning group (McBrien).

offense sentenced in the Staten Island Court and to establish the value of each unit for offenders of differing means, and discussed the rationale for the specific approaches chosen.

The planning project report also outlined changes in the court's collection system to be implemented as part of the pilot project (e.g., more individualized notification and monitoring in lieu of arrest warrants; greater use of non-custodial options in the face of non-willful default [e.g., community service]), and it provided a detailed research design for an evaluation to accompany the pilot project.

D. Implementing an American Day-Fine Model

With continued funding from NIJ and from the City of New York, the Staten Island day-fine experiment began in August 1988 in the Richmond County Criminal Court. Judge Rose McBrien imposed the first American day fine on August 12th, culminating nearly a decade of research and more than a year of planning and development. A report of the implementation process during the first year was prepared in August 1990 by Vera's Director of Court Programs and is scheduled to be published by NIJ as part of its Issues and Practices series (Greene).

In brief, the report describes a highly successful implementation process.⁸ Judges were able to obtain the means information they needed promptly, without disrupting the rapid flow of cases. Once trained to use the day-fine workbook they had helped develop, judges found the mechanics of computing a day-fine sentence easy to use. No conflicts of principle arose from prosecutors or from either the private or public defense bar, and the local press as well as the New York Times had favorable coverage of the project (Hurley, 1988; Gerstel, 1988; Brozan, 1988), including a Times editorial (1988).

⁸ In addition to descriptive materials on the process of implementation, this report contains statistical comparisons, prepared by program staff, between the early day-fine pilot cases and a sample of cases from the previous year. Similar (but not identical) data were collected by Vera Research Department staff as part of the evaluation, and are presented in detail below.

The project's individualized collection procedures, using a micro-computer to track day-fine cases post-sentence and to provide automatic payment notification and warning letters worked smoothly, as similar collection procedures have in other jurisdictions. Some of the defendants who did not pay in full because of changed financial circumstances (lost jobs, ill health, etc.) were returned to court where, using information from the day-fine officer's collection records, the sentencing judge was able to make an informed judgment about the defendant's previous efforts to comply with the court order and his current circumstances, and to re-sentence appropriately.

The only implementation problem encountered was one anticipated by the planners. Statutory fine maxima in New York State are very low, not having been increased since 1965 despite inflation. As a result, in a significant number of cases, the day-fine amounts calculated by judges for more affluent offenders, especially for offenses at the more serious end of the spectrum, were in excess of the statutory limit. In these instances, the sentencing judge recorded the day fine as calculated, then sentenced the offender to the statutory maximum, providing a record for later use in requesting legislative changes in the cap.

Finally, the introduction of day fines resulted in a slight increase in collection rates (above the already high level) and in a significant increase in fine revenues (and especially in anticipated revenues, if the fine caps are raised in the future).

E. Evaluating the Day-Fine Pilot

Goals of the evaluation. The evaluation developed by the Vera Institute Research Department was designed to test what impact the introduction of day fines had on the sentencing patterns of the court (e.g., whether their supposed complexity decreased the use of fines and whether fine use shifted from one type of offense to another). The research was also design to test whether, on average, day-fine amounts were higher than fixed fines and, if so, whether this had any negative consequences for the already high collection rate charac-

teristic of this court.⁹ In addition, the research was designed to test what impact the introduction of day fines per se had on collection, what impact the new collection techniques alone had, and what impact the combination of the two had on the proportion of fined offenders paying in full and the proportion of the fine amount that was collected. Finally, the research design was constructed to measure whether the introduction of day fines encouraged judges to displace any other type of sentence (e.g., probation, jail) in favor of a fine, or only to replace existing fixed fines with day fines.¹⁰

The evaluation design did not include recidivism measures because of time constraints.¹¹ However, just prior to the Staten Island day-fine pilot, NIJ had also funded a study of the recidivism rates of offenders sentenced to monetary penalties by the Los Angeles Municipal Court (Glaser and Gordon, 1988, 1990). The findings of this study indicated that the recidivism rates of fined offenders compared favorably with those of offenders sentenced to probation and to jail. These American findings parallel earlier findings for offenders sentenced to fines and other sanctions in England and Germany

⁹ One reason for selecting the Staten Island court as the site for the pilot was its good collection record. Although some aspects of the court's traditional collection process were cumbersome and inefficient, and the pilot project sought to improve them, it was considered important to pick a well-administered and already relatively successful court for the initial test of whether day fines could be implemented at all in an American court.

¹⁰ The pilot project did not attempt directly to encourage judges to displace jail sentences with day fines. Although this issue was discussed by the planning group, and judges, prosecutors and defense attorneys had many ideas about when this might be an appropriate outcome, during the pilot year, Vera planners and day-fine project staff did not advocate for this change in sentencing. It was considered sufficient to ensure the day-fine mechanisms worked appropriately, to encourage the judges (and other participants in the adjudication and sentencing process) to become comfortable with the new fining process, to monitor their use of the day-fine methodology to ensure it was correctly employed, and to implement the new collection techniques. While some planners and project staff hoped judges would, over time, begin to displace at least some short jail sentences with day fines, no planning was done to achieve this particular outcome.

¹¹ The pilot was twelve months and the follow-up period, to measure collection rates, required another twelve months; to have collected recidivism data for a follow-up period after completion of the fine sentence would have required too long a research period.

(McClintock, 1963; Davies, 1970; Softley, 1977; McCord, 1985; Albrecht and Johnson, 1980).

The evaluation design.¹² The design chosen was a pre/post comparison of penal law felony and misdemeanor arrests disposed in the Staten Island Criminal Court prior to the introduction of day fines and during the day-fine project's pilot year. The post-test sample also contained a randomized sub-experiment to test the impact of the new collection procedures introduced as part of the day-fine pilot.

The pretest sample consisted of all penal law cases disposed from April 1, 1987 through March 31, 1988 (N=4461), prior to the start of the day-fine pilot. The post-test consisted of all cases disposed from September 1, 1988 through August 31, 1990 (N=4883), during the pilot year. The timeframe for the pre-test sample was determined by the need to pick a sample as close to the introduction of day fines as possible, which still provided sufficient time for most fine enforcement activities to have been completed before the day-fine pilot began. The post-test sample coincided with the pilot's first year, allowing three weeks for project start-up and training.¹³

All post-test cases sentenced to a day fine were randomly assigned by docket number into two groups after sentencing: one group was assigned to the day-fine pilot collection program (experimentals); the other was assigned to the traditional post-sentence collection process administered by the court (controls).

¹² For a full, technical description, see Appendix A.

¹³ It is important to note that the research posttest sample is different from the sample of cases followed by program operators/planners during the pilot's first year. Data from the program's own sample of cases, and presented in the descriptions of program implementation and operations written by Hillsman and Greene (forthcoming), began with the date of program inception (August 12, 1988). Thus, the program sample included cases from the last several weeks in August 1988 which aren't included in the research sample; in addition, because the program sample continued for one year, it ended several weeks earlier than did the research sample period. As a result, the research posttest does not include fines imposed during the first several weeks of pilot operations, but does include fines from the last several weeks of August 1989.

By comparing collection outcomes for the day-fine experimental cases with those for all pretest fine cases, the effect of introducing the new day-fine system (day fines plus new collection procedures) could be tested. By comparing outcomes for the day-fine control cases with those with the pretest fines, the independent effect of introducing the day-fine sentences could be measured (factoring out the effects of new collection techniques). Finally, by comparing collection outcomes for the day-fine experimentals with those for the day-fine controls, the effect of the new enforcement procedures alone could be measured.

Data sources. Official information on all sample cases was collected from the computerized databases of the New York City Criminal Justice Agency (CJA), the New York State Office of Court Administration (OCA), the New York Division of Criminal Justice Services (DCJS) and from the manual record systems of several other agencies. Information on all court appearances through disposition and sentence (including bail and custody status pretrial, sentencing outcomes, charges, etc.) was obtained. In addition, all post-sentence court appearance data were secured, including number of scheduled appearances, partial and full amounts paid, arrest warrants ordered, and re-sentences. Data on income were obtained from CJA pretrial interviews. Prior criminal histories were also secured in both detailed and summary form. Finally, court papers and the day-fine pilot project's microcomputer-based information system were used to determine whether a case was a fixed-fine or day-fine and if a day-fine, what the calculated and imposed amounts were.¹⁴

¹⁴ The official computerized databases only recorded the sentence as a "fine" and identified the dollar amount imposed; if the fine was a day fine, however, the court papers contained a record of the number of day-fine units imposed by the judge, the value of the day-fine units he or she calculated, and the total amount of the day fine resulting from the multiplication of these numbers; if the day-fine amount exceeded the statutory maximum, the actual amount imposed by the judge (the statutory cap) was also recorded on the court papers as the official sentence.

Post-sentence follow-up periods. The structure of the samples resulted in different follow-up periods for each case. Twelve to 24 months of follow-up were available to track collection outcomes for cases in the pretest sample; this was reduced to two to 14 months for the posttest sample. As a result, all measures involving post-sentence court appearances, amounts paid, and final payment status, were calculated for each month post-sentence. Comparisons, therefore, could be made for up to 14 months post-sentence, but the number of cases in each sample declined overtime.

Additional post-sentence information from court records was manually collected on all fine cases by day-fine project staff in February 1991; these data were merged into the research database, providing data on the posttest sample for up to 29 months post-sentence. This longer timeframe enabled researchers to create a smaller sub-sample of fined cases (containing all those sentenced within a seven month period) for both the pretest and posttest that had exactly the same number of follow-up months -- from 17 to 23 months post-sentence. Because these sub-samples (referred to in the text as the "seven month sub-samples") had the longest comparable follow-up period, they were used primarily to measure whether the introduction of the day fine had any impact on the final status of the case, and on the total dollar amounts collected over the long run.

Analyses.¹⁵ For most analyses of the impact of day fines on sentencing patterns, comparisons were made between the pretest and posttest using percentage distributions.¹⁶ To measure displacement effects, however, a model predicting sentencing outcomes during

¹⁵ Before any comparisons of the pre- and post-tests could be used to measure the impact of the introduction of the day fine, the comparability of the two samples on arraignment charge was assessed; the two samples were found to be statistically equivalent with respect to the mix of cases coming into the court.

¹⁶ Chi square and Cramer's V are presented to test for significant differences and for the strength of the relationships between variables.

the pre-day fine period was constructed, and then applied to the posttest fine sample. This enabled researchers to determine whether posttest fines were predicted to receive a fixed fine before day fines were a sentencing option, or whether they would have received (i.e., were "displacing") probation, jail or other previously used sentences. The method used in the modeling (multinomial logistic regression) is statistically complex, and is discussed in detail in Appendix A.

Analyses were carried out to determine whether the average (mean) fine amount actually ordered by the judge (as well as the uncapped day-fine amount calculated by the judge, if it was above the statutory maximum) differed before and after the introduction of the day fine. Two-way analyses of variance were used to determine whether there were any significant differences observed in the average fine amounts between the two samples, and whether the impact of arraignment charge or the prior records of defendants varied for the two samples.

Analyses were also performed to see if day-fine amounts were more dispersed than fixed fines (as they should be if day-fines were calibrated according to offenders' incomes as well as to offense severity and fixed fines were set using general fine tariffs). Final total revenues were also compared between the two samples.

As indicated above, collection rates (the proportion who paid in full at sentence, who ever paid in full, and the fine dollars collected as a percent of the amount originally ordered) were compared for both pretest and posttest, and the experimental day-fine cases and controls, in order to measure the separate and combined effects of the day-fine sentence and the new collection method. In addition, analysis of variance was used to examine differences in the average number of days it took for full payment to be made, the number of post-sentence appearances, and the number of arrest warrants ordered. In examining these outcomes, the analysis of variance also measured the independent effect of the fine amount, because day fines were significantly higher overall than were fixed fines.

Finally, analyses using multiple regression were carried out to address the issue of whether income had more effect on fine amount under the day fines, and whether day fines, generally higher than fixed fines, had a negative effect on those with less income, or whether equity was improved. Fine amounts (capped and uncapped) were analyzed using arraignment charge severity, income, and sample year (pre or posttest) to determine what factors, independently and together, affected the size of the fine: In addition, the final status of the case (i.e., full payment versus resentence/warrant) was examined using income and fine amount in order to determine whether the generally higher day fines were affecting the collection success rate for offender at different income levels.

* * * * *

The next section of this report (III) summarizes the findings of the evaluation. The remainder of the report (section IV and the appendices) are a technical presentation of these same findings.

III. SUMMARY: FINDINGS OF THE STATEN ISLAND DAY-FINE PROJECT EVALUATION

A. Comparability of the Pretest and Posttest Samples¹

The two samples compared by the research to measure the effects of introducing the day fine into the Staten Island Criminal Court were comparable with respect to the types of cases coming into the court.

B. The Substitution of Day Fines for Fixed Fines

Day fines were successfully introduced into routine sentencing in the Staten Island court; they replaced two-thirds of the fixed fines in penal law cases sentenced by the judges during the pilot year. The remaining one-third of the fines were fixed fines imposed by non-Staten Island judges sitting temporarily in the court to cover for vacationing or sick colleagues; these judges had not been trained to use day fines and, therefore, used the traditional tariff system to set the fine amount.

All the judges trained to use day fines did so consistently throughout the pilot year without tying up their calendars. Therefore, the mechanics of imposing a day fine (establishing the number of day-fine units based upon the offense, and estimating net daily income and number of dependents to calculate the monetary value of each unit) were not too complex or time-consuming to be applied routinely in a relatively fast-paced criminal court.

C. The Impact of Day Fines on Sentencing Patterns

The introduction of day fines did not meaningfully affect judges' sentencing decisions during the pilot year. Even when prior conviction record and arraignment charges were controlled, sentencing patterns were stable during the initial period in which day fines were introduced into this first American court.

¹ These summary findings parallel the more detailed technical discussion in Section IV below, following the same alpha-numeric sub-sections, beginning on page 31.

The only small but noticeable change in sentencing patterns was an increase in jail sentences for some drug cases, a change that occurred during the height of the crack cocaine "epidemic" in New York City. While it is apparent that the introduction of day fines did not create this sentencing shift, it is also apparent that the availability of the new day fine did not counter it.

D. The Impact of Day Fines on Fine Amounts

1. Mean fine amounts. After introduction of the day fine, average (mean) fines imposed for penal law offenses rose 25%, from an average of \$205.66 during the pretest period to an average of \$257.85 in the posttest period. In addition, had the larger day fines not been capped by the statutory fine maxima, fine amounts during the pilot period would have increased even more dramatically. The average day fine calculated by the judges during the posttest (the uncapped fine) was \$440.83, more than twice the average pretest fine amount (\$205.66). Even when these uncapped day fines were combined with the lower-amount fixed fines also imposed during the posttest period, average fine amounts overall would have risen 84% under the new system (from \$205.66 to \$378.19) if the judges had been freed from the statutory caps.

2. The dispersion of fine amounts. As expected, calculating fine amounts using the day-fine system (with the benchmarks to reflect crime severity and net daily income to reflect means) resulted in greater variation in the size of individual fines; that is, the judges relied substantially less on the traditional "tariffs" (e.g., \$25, 50, or \$100) and calculated fines in uneven amounts, many of which (as noted above) fell above the statutory fine maxima.

3. Total fine dollars ordered (potential revenue). Even constrained by the legislative fine caps, the total dollar amount of the fines imposed by the court in penal law cases increased by 14% during the day-fine pilot (from \$82,060 to \$93,856). However, the impact of day fines on potential revenue was even more dramatic when uncapped fines were examined. In the absence of the caps, the fine dollars ordered by judges in the posttest period would have been almost 50% higher than the capped fines actually ordered (rising to

\$137,660 from \$93,8556); this was a 67% increase over the fine amounts ordered during the period before day fines were introduced.

4. Factors affecting fine amounts. Fine amounts, whether tariffs or day fines, increased as the severity of the arraignment charge increased and, as expected, the nature of this relationship did not change with the introduction of the day fine. Therefore, while day fines increased fine amounts across the range of offenses, they incorporated rather than changed the relative rank order of offenses of different severity previously reflected in the court's sentencing decisions.

Similarly, the introduction of the day fine had no impact on the effect of prior conviction record on the fine amount. Neither before nor after the day fine did judges take prior record into account in setting the amount of the fine, although they may well have taken it into account in deciding whether or not to impose a fine.²

Thus, the day fine, as a new fining mechanism, showed potential for generating substantially higher revenues, so long as collection rates did not decline with the higher individual day fine amounts. As the next set of findings suggests, however, one advantage of the day fine demonstrated by the Staten Island pilot is that, by setting the fine amount according to an offender's means, a financial obligation is imposed that is collectible as well as proportionate to the severity of the offense.

E. The Impact of the Day-Fine Pilot on Collection and Enforcement Outcomes

1. Structure of the analyses. The Staten Island day-fine pilot project introduced two types of changes into the Criminal Court. First, it introduced the day fine itself -- a new method for determining the amount of a fine. Second, it introduced a more individualized collection system. Using the pre/post comparisons as well as the experimental design

² This confirmed the decisions made by the planning group, which developed a day-fine system incorporating prior record into the sentencing decision but not into the fine valuation process.

embedded in the overall research design, the impact of each of these changes, as well as their combined effect, was measured for several outcomes: collection rates, collection patterns, and final enforcement status.

2. Collection rates. Despite the substantial increase in average fine amounts, introduction of the day-fine system did not undermine the court's already high rate of success at ensuring offenders paid their fines in full.³ Eighty-five percent of the day-fine experimental cases (those subject to the new collection strategy) eventually paid in full compared to 76% of the pretest fine cases. This is not a statistically significant difference; therefore, the day-fine system did not improve the court's already high level of success securing full payment. However, day-fine cases that did not pay in full were significantly more likely than pretest fine cases to pay something toward the fine amount owed (9.4% compared to 1.7%) and less likely to pay nothing at all (5.7% compared to 22.2%). The collection picture overall, therefore, improved somewhat.

Introduction of the day-fine method per se (i.e., day fines without the new collection techniques) did not diminish the court's high collection levels, again, despite substantially higher day-fine amounts. (Equivalent proportions of the day-fine control cases [71.4%] and pretest fine cases [76.1%] were collected in full.) However, adding the new collection techniques did improve collection rates for the higher day fines: 84.9% of the day-fine experimental cases paid in full compared to 71.4% of the day-fine control cases, although this difference was not statistically significant. Furthermore, significantly fewer day-fine experimentals paid nothing toward their fines (5.7%) compared to day-fine controls (25.7%).

These data suggest, therefore, that the new enforcement procedures independently improved collection rates for those sentenced to the higher day-fine amounts, but that the higher day fines per se did not make collection more difficult for the court.

³ Recall that the Staten Island court was selected for this demonstration in part because it had a history of successful fine collection.

3. Collection patterns. While the introduction of day fines did not diminish the court's successful collection efforts (and improved it somewhat), the day fines took longer to collect than the pretest fixed fines. This pattern was closely related to the higher average day-fine amounts. Day fines, both with and without the new collection techniques, took longer to collect than the pretest fines. The mean number of days to pay in full was significantly less for the pretest fines (55 days) than for either the day-fine experimentals (114 days) or controls (119 days). This longer collection period is not surprising in light of the substantially higher fine amounts imposed after introduction of the day fine.

Despite longer times to full payment and higher fine amounts, the pilot project (day fines combined with the new collection techniques) did not increase the number of post-sentence court appearances. As designed, the new collection approach kept fined cases off the court calendar until the end of collection and enforcement activities. (Pretest fines had an average of 1.96 post-sentence appearances and day-fine experimentals had 1.76.) However, in the absence of the new collection approach, the higher day-fines did result in more court appearances (2.66) for the day-fine control cases. Without the more individualized collection strategy built into the pilot project for the experimental cases, therefore, the higher day-fine control cases were brought back to court more frequently to secure payment than either the smaller pretest fines or the day-fine experimentals.

While during the pretest period, the data suggest that the number of post-sentence court appearances increased with the size of the fine. However, this was not found for the day fines. It would appear that introducing a means-based method for setting fine amounts mitigated the possible impact of higher fine amounts on the number of appearances needed for collection. The number, therefore, is equalized across offenders with different means and different fine levels.

Parallel to these finding is the finding that the day-fine pilot significantly reduced the number of post-sentence arrest warrants for failure to appear. Whereas the pretest fine cases averaged 0.55 warrants, the day-fine experimental cases (day fines plus the new collection techniques) averaged 0.26 warrants. As with post-sentence appearances, however, the day-fine control cases had the highest average number of warrants (.83). This suggests that, when the court used the traditional collection techniques, it had to rely more heavily on warrants to enforce fine collections, especially for the larger day fines which took longer to collect.

4. Final enforcement outcomes. The day-fine cases subject to the new individualize collection strategy had a higher proportion who paid in full (84.9%, compared to 76.1% for the pretest cases and 71.4% for the day-fine controls), and the lowest proportion who absconded (5.7%, compared to 10.9% and 14.3%) or who had their case returned to court (5.7% compared to 14.3% and 14.3%).⁴

Despite significantly higher amounts and longer collection periods, therefore, day fines were collected in full at high rates, rates that were as high as those for much smaller fixed fines. This appears to be a function of taking the means of the offender into account: even the day-fine control cases (who were subject to the traditional collection techniques of the court) had substantial rates of full compliance (slightly over 70%).

Further, the potentially negative impact on collection and enforcement of raising fine amounts by introducing day fines (particularly in jurisdictions whose existing collection strategies are not as successful as those in Staten Island) can probably be minimized by the new collection techniques. While the individualized collection strategy allowed fined offenders to take longer to pay the larger day fines in full, it required fewer costly court appearances and warrants than did the traditional collection system.

⁴ The Chi square for these percentages is not statistically significant because there were so many cases that paid in full and so few in the other categories; these results, therefore, need confirmation from research on other day-fine programs.

Without the new collection procedures, those jurisdictions with collection systems similar to Staten Island who introduce day fines can expect comparable collection rates to those under their fixed fine systems; however, they may require somewhat more resources for the additional court appearances and warrants that result from the higher day-fine amounts. Thus, individualized collection systems, if economically feasible (which they may be when viewed from the perspective of potentially higher fine revenues), would probably be the best approach for continued implementation of the day-fine approach in American courts.

F. The Impact of the Day-Fine Pilot on Equity

One goal of introducing a means-based system of setting fine amounts is to ensure equity. The research addressed the question of whether day fines were set more in accordance with an individual's ability to pay than were fixed fines. In addition, because day fines were clearly higher on average than fixed fines (especially uncapped day fines), the research addressed the question of whether individuals of differing means were more or less able to pay the fines under the new system as compared to the old system.

1. The relationship between income and fine amount. As expected, the fine amounts imposed by the court increased as income rose, for both fixed fines and capped day fines. However, the relationship was much stronger for the day fines when uncapped amounts were used. As expected, therefore, a person's income appears to have played a more significant role in determining the fine amount under the day-fine system, even when other factors (e.g., charge severity) were controlled.

The relationship between income and uncapped fine amount also varied for the pretest fines and for day fines: for the day fines, there was more spread at the lower end of the income scale and less spread at higher income levels. It appears, therefore, that having

more means information and being guided by the day-fine benchmarks helped judges differentiate among offenders with regard to fine amount. At lower income levels, this prevented fine amounts from being tied to the lowest common economic denominator, thus increasing the spread. For higher income levels, the distance between fine amounts appears to have been narrowed.⁵ Further research, particularly on felony courts, with their wider range of criminal jurisdiction, would be useful to understand these patterns more fully.

2. The relationship between income and enforcement outcomes. The data show no difference in successful case outcomes (i.e., full payment) for the day fine cases when compared to the pretest cases, regardless of income level, although these numbers are small and this finding needs additional research confirmation. This suggests, therefore, that poor people did no better and no worse complying with fine sentences under the day-fine system, despite the significantly higher average day-fine amounts. Similarly, for high fines as well as low fines, the offenders sentenced to day fines generally did as well paying the fine as those who received fixed fines. Therefore, the higher fine amounts under the day-fine system did not appear to have a deleterious effect on offenders' ability to comply with the sentence.

Although the proportionate increase in the uncapped amount for different income levels varied somewhat less for day fines than for pretest fixed fines, it would appear that implementing the day-fine pilot standardized much of the decision-making that was already in place in the court. Staten Island judges not only set fine amounts in relation to the severity of the offense before day fines were introduced, but they also took the offender's income into account, at least to some degree. Under day fines, however, this "calculus" appears to have been made more overt, resulting in greater uniformity in sentencing.

⁵ The reasons for this are not clear. It may have to do with the structure of the Staten Island benchmarks, with the small number of cases in the sample who had both high incomes and severe offenses, or the constriction in the severity range for cases in this lower court's jurisdiction.

G. The Impact of the Day-Fine Pilot on Sentence Displacement

With the introduction of day fines, what sentences might have been displaced; that is, what sentence would have been given in the absence of the day fine? Might day fines have displaced fixed fines or were other sentencing options potentially displaced? The pilot project did not actively encourage judges to use day fines to displace non-fine sentencing options, although some members of the court planning group hoped to see this occur. The primary goal of the pilot, however, was to demonstrate that a day-fine model for an American court could be designed and successfully implemented. Measures of success focused primarily on operational matters intrinsic to the effective use of fine sentences (e.g., maintaining high collection rates, increasing revenues) and to ensuring equity. If day fines proved successful in these terms, planners considered it possible that they could be used as an intermediate penalty to displace other sentencing options, including short jail sentences, as suggested by Morris and Tonry (1990).

A complex predictive model was developed to measure displacement (for details, see section IV-G below). Factors predicting sentencing outcomes for pretest cases were assessed and the resulting model was applied to the posttest fine cases to estimate what the sentence would have been in the absence of the day fine.

As expected, the model indicated that none of the fine cases during the day-fine pilot project would have been sentenced to jail prior to introducing the day-fine option. However, 28% of the relatively few fined cases in the posttest period would have been dismissed outright in the absence of a day fine. This suggests that additional means information and a judicial focus on monetary penalties may have encouraged judges to substitute a punitive monetary sanction for no sanction at all in at least some cases. Possibly, therefore, a means-based fine may substitute for a dismissal agreed to for lack of a suitable punishment option.⁶

⁶ The data do not permit us to know for sure whether the day fine displaced dismissals resulting from a lack of evidence or for lack of a suitable alternative; one would hope the latter.

The model also indicates that 31% of the small number of fines would have received an adjournment in contemplation of a dismissal (ACD), some of which would have been conditioned upon payment of restitution or completion of a community service order.

However, restitution and community service orders were generally increasing as sentencing options during the implementation of the day fine. These data suggest that while day fines may have substituted for some of these sentences, restitution and community service orders were being added to ACDs and conditional discharges in many other cases. The introduction of day fines, therefore, with the accompanying expansion of available means information and greater sensitivity to its use, appears to have encouraged judges to fine tune their imposition of monetary sanctions and work options, and well as increase the use of both.

In summary, the displacement analysis provides some interesting insights into how the introduction of day fines fit into general sentencing patterns established in the Staten Island court. An earlier finding (C above) indicated that overall sentencing patterns did not change appreciably. During the day-fine pilot, fine sentences in penal law cases remained small and stable (about 8-9%); restitution and community service sentences edged up somewhat; and other major sentencing options remained relatively stable.

The displacement analysis adds nuance to this broad picture. Relatively little displacement apparently occurred with the introduction of day fines; fines used in penal law cases has drifted downward over the last decade as other non-custodial options became available in the Staten Island court (including restitution and community service).

However, day fines do seem to have helped the court fine tune their sentencing decisions. Judges may have become more comfortable with monetary penalties whose amounts could be adjusted to individual cases (e.g., fines and restitution, in contrast to fixed fees and surcharges) and with work options when they felt they had sufficient information on

offenders' economic circumstances to make appropriate distinctions among them. Judges in the Staten Island court may (and the evidence is not conclusive) have shifted some community service orders and probation sentences to fines, because the day-fine system permitted them to recognize even a poor offender's ability to pay something and to set a reasonable amount (small but collectible) that was also proportionate to the offense. If so, they did this in the context of a noticeable upward trend in their overall use of both community service and restitution.

IV. TECHNICAL PRESENTATION: FINDINGS OF THE STATEN ISLAND DAY-FINE PROJECT EVALUATION

A. Comparability of the Pretest and Posttest Samples

As indicated in Section II above, the basic evaluation design involved a comparison of samples before and after introduction of day fines. Before examining issues such as whether sentencing patterns altered as a result of day fines, it was necessary to rule out any confounding effects that might result if there had been independent changes in the mix of cases coming into the court over the two and a half year period covered by the two samples. In order to ensure the two samples were comparable, therefore, the distribution of arraignment charges in the pretest and pretest were compared. Table IV-1 compares the two samples, collapsing combinations of arraignment charges by type and severity. (Appendix B, Table 1 presents the same data, using a highly detailed categorization of arraignment charges based upon specific penal law sub-section.)

As Table IV-1 illustrates, there were some, but only small, shifts in the distribution of arraignment charges over the two time periods. A close examination of the detailed charge breakdowns in Appendix Table 1 shows some categories experienced a change of at least one percent between the pretest and posttest. For example, both felony and misdemeanor assaults increased slightly as did misdemeanor judicial misconduct (e.g., criminal contempt, bail jumping, tampering with witness); petit larceny, however, decreased. For drug offenses, there were both increases and decreases. Felony possession/sale went up slightly, as did misdemeanor criminal possession (seventh degree), although third degree felony sale and marijuana possession went down. There was also a slight increase in misdemeanor criminal possession of drugs.

In general, therefore, during the posttest there was a slight increase in the number of both felony and misdemeanor crimes against persons, a slight decrease in misdemeanor property and drug crimes, while most felony drug crimes remained stable. These shifts were less than two percent, however, and were not considered meaningful for subsequent

Table IV-1

Comparison of Pretest and Posttest Samples by Arraignment Charge
(type and severity combined)

<u>Arraignment Charge</u>	<u>Pretest</u>		<u>Sample</u>		<u>Posttest</u>	
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>
Crimes vs. Persons, felony	547	12.8	669	14.3	669	14.3
Crimes vs. Persons, misd	618	14.4	733	15.7	733	15.7
Weapons crimes, felony	35	0.8	35	0.8	35	0.8
Weapons crimes, misd	82	1.9	103	2.2	103	2.2
Property crimes, felony	480	11.2	513	11.0	513	11.0
Property crimes, misd	767	17.9	762	16.3	762	16.3
Drug Crimes, felony	176	4.1	206	4.4	206	4.4
Drug Crimes, misd	460	10.7	435	9.3	435	9.3
Drug Crimes, violation	68	1.6	32	0.7	32	0.7
Forgery/Fraud Crimes, felony	55	1.3	50	1.1	50	1.1
Forgery/Fraud Crimes, misd	60	1.4	57	1.2	57	1.2
Miscon Crimes, felony	4	0.1	20	0.4	20	0.4
Miscon Crimes, misd	558	13.0	586	12.5	586	12.5
Miscon Crimes, violation	177	4.1	230	4.9	230	4.9
Obstruct Justice, felony	3	0.1	11	0.2	11	0.2
Obstruct Justice, misd	191	4.5	242	5.2	242	5.2
Obstruct Justice, viol	1	<0.1	-	-	-	-
Total	4282	100.0	4684	100.0	4684	100.0

$\chi^2 = 53.77$

DF = 16

p = <.001

Cramer's V = 0.09

assessments of the impact of introducing day fines. Although the chi square was statistically significant for these distributions,¹ Cramer's V (a measure of the strength of the association between sample year and arraignment charge that is interpreted like a correlation coefficient) indicates that there is only a very weak relationship between arraignment charge and sample. Therefore, the samples from the two years are viewed as statistically equivalent in terms of the mix of charges coming into the court.

B. The Substitution of Day Fines for Fixed Fines

Day fines were successfully introduced into routine sentencing in the Staten Island court during the pilot year: day fines replaced two-thirds of the fixed fine sentences in penal law cases sentenced by the judges.² Examination of the remaining one-third of the fine sentences that remained fixed fines indicated that the majority were imposed by non-Staten Island judges, temporarily sitting on the bench to cover for vacationing or sick colleagues. Unfamiliar with the day-fine pilot project and untrained in calculating a means-based fine, these new judges continued to follow the conventional tariff methods used previously by the Staten Island bench.

There were also a few instances in which the day-fining judges in Staten Island imposed a fixed fine. Interviews and court papers indicated that this occurred either because a plea bargain had been agreed to by an assistant prosecutor that specified a fixed fine amount or because the offense was sufficiently rare that it had not been included in the original day-fine

¹ This statistic is highly dependent upon sample size: with large sample sizes such as these, even small differences can produce significant chi squares; Cramer's V, therefore, is the important statistic.

² Vehicular Traffic Law (VTL) cases (e.g., criminal cases involving driving while under the influence [DWI], driving with a suspended/expired license, etc.) were not part of the day-fine pilot project or the research. This is because New York State law provides fixed minimum fine amounts for these offenses and, therefore, they could not be subject to the day-fine methodology. Thus only offenses charged or disposed under the state's penal law were part of the demonstration (i.e., if a case was either initially charged as a penal law offense but disposed as a VTL, or arraigned as a VTL, it was excluded).

benchmarks. As the pilot year passed, these offenses were added to the sentencing benchmarks and on-going training of assistant prosecutors made them more familiar with the day-fine method of setting a fine amount.

These implementation findings suggest that fixed fines can be replaced by day fines (so long as fixed fine amounts are not mandatory) and that the only impediment, readily overcome by training, is unfamiliarity with the system. The mechanics of imposing a day fine, therefore, are not so complex or time-consuming that judges will easily slip into the routine of past convention.

C. The Impact of Day Fines on Sentencing Patterns

The evaluation sought to assess whether the introduction of day fines, and their implementation to the extent indicated above, had any impact on sentencing patterns. That is, was this intervention process accompanied by a change in the distribution of sentences between the pretest and posttest periods, looking first at the sentence types categorized at the most general level and then exploring a more detailed breakdown to determine if more subtle changes can be detected. The specific posttest sentencing distribution was also compared to the pretest controlling for prior convictions and arraignment charges. Together, these analyses indicate a considerable degree of stability in sentencing patterns during the initial period in which day fines were introduced into this first American court.

Table IV-2 reveals only small differences in the distribution of sentences comparing the pretest and posttest. The proportion of Fines, Adjournments in Contemplation of Dismissal (ACD),³ and Conditional Discharges (CD) as sole sanctions decreased somewhat (1.5%, 2.7%,

³ Under the New York Penal Law, a case can be adjourned for a period of six months, after which time the prosecutor may move to dismiss the charges if the defendant has not been rearrested or violated a condition set by the court; these are called ACDs and are distinguished from immediate dismissals only by the waiting period. In Staten Island, ACDs can also be combined with community service orders and restitution orders, so they are conditional dismissals and prosecution can be restored if the condition is not fulfilled.

Table IV-2

Comparison of Collapsed Sentencing Distribution for
Posttest versus Pretest

<u>Collapsed Sentence</u>	<u>Pretest</u>		<u>Sample Posttest</u>		<u>Difference in % and direction of Posttest from Pretest</u>
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	
Fines	399	9.2	364	7.7	-1.5
ACD	1124	26.0	1095	23.3	-2.7
ACD/CSS	358	8.3	424	9.0	+0.7
CD	344	8.0	330	7.0	-1.0
CD/CSS	91	2.1	249	5.3	+3.2
Dismissals	1230	28.5	1402	29.8	+1.3
Jail	637	14.8	711	15.1	+0.3
Probation	134	3.1	132	2.8	-0.3
TOTAL	4317	100.0	4707	100.0	

$X^2 = 79.88$
DF = 7
p = <.001
Cramer's V = .09

and 1.0%, respectively) during the posttest, and sentences to a CD combined with a Community Service Sentence (CSS) and outright dismissals increased (3.2% and 1.3%). Incarcerative sentences and probation, however, remained stable.

These shifts are highlighted in Appendix Table 2, in which the sentence combinations are presented in greater detail. For example, ACDs combined with restitution ("VSA")⁴ and with CSS both increased somewhat, as did CDs with one or the other (or both). However, these changes in sentencing patterns are not large enough to be meaningful; while the chi square is statistically significant, the Cramer's V indicates a very weak relationship. Introduction of day fines, therefore, did not meaningfully affect the judges' sentencing decisions during the pilot year.

This general conclusion does not change after further analyses controlled for prior record (number of prior felony convictions, prior misdemeanor convictions, and total number of convictions). Appendix Tables 3 through 5 reveal the same trends noted above, irrespective of the number of prior convictions, except when the number of prior felony convictions increases to two or more. For these more frequent offenders, the court used jail more often during the posttest, although fine sentences remained stable, and the results were not statistically significant.

The final analysis to explore the impact of the day-fine pilot on sentencing patterns controlled for arraignment charge. Program implementation data had indicated that during the day-fine project's first year, the arraignment charge composition of fined cases appeared to undergo some change, with fewer drug charges appearing among the fined cases than previously (Greene, 1990). The question arises, therefore, as to whether the implementation of this new fining mechanism had impact on a change in the "going rate" (i.e., the sentence seen as appropriate for a given charge group) for drug cases or any other category of charges.

⁴ The Victim Service Agency (VSA) has responsibility for the collection of restitution payments in the Staten Island court.

The relevant data are presented in Appendix Table 6. For most arraignment charge groups that contained a sufficient number of cases to assess statistical significance,⁵ there was no evidence of change between the pretest and posttest. The only exception was cases arraigned on misdemeanor drug charges (9.3% of the sample) which experienced a statistically significant decrease in the proportion of fine sentences given during the posttest year, and a concomitant increase in ACD and CD sentences combined with CSS, and in jail sentences.

Interviews with judges, prosecutors and defense attorneys, however, suggest that the implementation of the day-fine project had nothing to do with this change. Instead, there was an increase in public concern with drug offenses during this period (the height of the crack cocaine "epidemic" in New York City). In Staten Island, the court's reaction to the public demand to "get tough on drug crimes" appears to have been to give fewer fines and ACDs alone without CSS, and more jail sentences and non-custodial sentences containing community service orders. Because the types of charges coming into the court remained stable over time (section A above), this sentencing change seems to be responsible for the shift in the charge composition of the fines cases observed by project staff during the pilot. While it is apparent that the introduction of day fines did not create this change in sentencing, it is also apparent that the availability of the new day fines did not counter it.

D. The Impact of Day Fines on Fine Amounts

The evaluation sought to determine whether the introduction of a means-based fining system -- the day fine -- which was accompanied by an increase in the availability of financial information about defendants, would increase fine amounts overall and increase the extent of their dispersion.⁶ (In a latter section [F] the relationship between fine amount and income is

⁵ Based on a power analysis, a sample size of 150 cases was viewed as the minimum number for assessing significance; this decision rule dropped seven of the 17 charge type categories from the analysis.

⁶ One additional issue was whether, given the greater availability of means information, there would be an increase in the use of restitution orders. If so, the research would also have to consider whether the amount of total financial penalties increased as well as

explored.) To accomplish this, the research examined the average fine amounts imposed by judges before and after introduction of the day fine, and the average amounts they would have imposed using the day-fine method if no statutory caps had been in effect.

1. Mean fine amounts

Initially, differences in the mean fine amount imposed by judges were calculated, comparing pretest fines (which were, of course, all fixed fines) with all posttest fines (which included, however, both day fines [66%] and fixed fines [33%]). In addition, however, the average amount of the posttest day fines only was compared to the average pretest fine amount.

Similarly, differences in the mean "uncapped" fine amount (substituting the day-fine amounts calculated by judges and recorded on the court papers but not imposed because they were in excess statutory fine maxima) were also calculated, first for all posttest fines compared to pretest fines, and then for day fines only versus the pretest fixed fines.

As Table IV-3 indicates, after introduction of the day fine, average (mean) fines for penal law offenses rose 25 percent, from an average fine of \$205.66 during the pretest period to an average of \$257.85 in the post-test period. Because day fines comprised 66 percent of the posttest fines, the average day fine imposed in the posttest period (\$258.31) was not much greater than the posttest average of day fine amounts combined with fixed fine amounts (\$257.85). (For full distributions, see Appendix Tables 7 and 8.)

However, the impact of the statutory fine caps on fine amounts is vividly illustrated by examining data on the uncapped day fines judges calculated at sentence during the pilot

whether fine amounts increased. However, the number of restitution orders was stable across the pre and post samples, and accounted for a relatively small number of cases (2.6% [114 cases] in the pre and 4.5% [204 cases] in the post). Thus, total financial penalty was not considered an outcome variable in the analysis.

Table IV-3

Comparison of Pretest and Posttest
Fine Amounts Ordered

<u>P.L. Fines</u>	<u>Pretest</u>	<u>Posttest</u>	
		<u>All Fines (Day Fines & Flat Fines)</u>	<u>Day Fines Only</u>
<u>Capped Fines</u>			
Mean Fine Amount Ordered	\$205.66	\$257.85	\$258.31
Total Revenue Ordered	\$82,060	\$93,856	\$61,994
<u>Uncapped Fines</u>			
Mean Fine Amount Ordered	\$205.66	\$378.19	\$440.83
Total Revenue Ordered	\$82,060	\$137,660	\$105,798

project, fines they would have imposed in the absence of the caps. The average day fine calculated by the judges during the posttest pilot period was \$440.83, more than twice the average pretest fine amount (\$205.66). Even when these uncapped day fines are combined with the fixed fines imposed during the posttest period, average fine amounts under the new system would have risen 84 percent if the judges had been able to remove the caps.

2. The dispersion of fine amounts

Fine amounts not only increased on average, but the dispersion of individual fine sentence amounts around the mean is also greater. The full distribution of day-fine amounts shown in Appendix Tables 7 and 8 reveal that, as expected, calculating fine amounts using the benchmarks to reflect crime severity and net daily income to reflect means, results in judges relying substantially less on the traditional "tariffs" (i.e., \$25, \$50, \$75, \$100, etc.). Instead, the proportion of the posttest fines that are at these tariff points is less than those during the pretest year, with a concomitant increase in fines falling between them. (For example, in Appendix Table 7, less than 1% of the pretest fines were ordered between \$150 and \$200, as compared to 3% of the capped posttest fines.) Furthermore, of those posttest fines falling between the tariff points, most are day fines.

In addition, as Appendix Table 8 indicates, this dispersion is increased when the distribution of uncapped fines is examined. Not only do the day fines compose a substantial proportion of the amounts lying between traditional tariff points, but many day fines would be in amounts above the statutory maxima (e.g., over \$1000).

3. Total fine dollars ordered (potential revenue)

Finally, when individual fine amounts are summed, providing a measure of the total dollar amount of fines ordered during a sample period, the effect of introducing day fines on potential fine revenues can be measured. Even with the legislative caps in effect, the total fine dollars ordered by the court in penal law cases increased during the day-fines project by 14 percent, from \$82,060 to \$93,856 (Table IV-3).

The increase is even more dramatic when uncapped amounts are considered (Table IV-3). The total fine dollars which judges would have ordered if the legislative caps had not been in place is almost 50 percent higher than the capped amount they actually ordered (rising to \$137,660 from \$93,856). This is a 67 percent increase over the fine amounts ordered during pretest year. Thus, in the event that very low legislative caps are removed or raised, the day fine, as a new fining mechanism, has the potential for providing substantially higher revenues, so long as the higher day-fine amounts are collected at a rate that is the same or greater than the rate under the tariff or fixed-fine system.

4. Factors affecting mean fine amounts

In order to see what factors affected these changes in fine amounts, the mean amounts (capped and uncapped) were analyzed using analysis of variance (ANOVA).⁷ The first model included arraignment charge as well as sample year (pre or post sample overall or day fines only for the posttest), and the second included number of prior convictions as well as sample year.⁸ The ANOVA models using arraignment charge and sample are presented in Appendix Tables 9 through 12; those using prior convictions are found in Appendix Tables 17 through 20.

The first set of ANOVA models (Appendix Tables 9 through 12) are significant: both arraignment charge type and sample year have an independent effect on the size of the fine amount, although the interaction of the two is not significant. That is, regardless of how the samples are defined (whether pre/post samples overall are compared or day fines only are used

⁷ ANOVA, or analysis of variance, is a statistical test used to determine whether there are significant differences among the means of various groups.

⁸ During the planning phase of the Richmond project, decision-makers made a conscious choice not to include prior record into the fine valuation system, but rather relegated prior record into the sentencing decision (that is, to fine or not). If the judges followed the system correctly, then there should be little or no impact of prior record on the mean fine amount ordered.

for the posttest), or how fine amount is characterized (whether the amount ordered is used or the uncapped amount), fine amounts differ after introduction of the day-fine pilot (during the posttest). And, while fine amounts independently vary in relation to the type and severity of the arraignment charge, they did so in a similar manner both before and after day fines were imposed -- the relationship between the mean differences of fine amounts (capped or uncapped) for arraignment charges of varying severity does not change between the pretest and posttest periods.

The effects of arraignment charge and sample year on fine amounts are displayed in the tables of means and standard deviations found in Appendix Tables 13 through 16. As expected, felony arraignment charges are routinely associated with higher fine amounts than are misdemeanors.⁹ The charge categories with the highest fine amounts overall are, in order, felony misconduct; felony forgery/fraud; felony weapons offenses; and felony drug offenses. This rank order remains the same both before and after introduction of the day fine although the charge differences are more dramatic when the uncapped amounts are examined.

Thus, the significant differences in fine amounts among different charge levels do not vary substantially for the posttest as compared to the pretest -- the same four charge categories have the same ranking on fine amount for both the pretest and the posttest; as a result, the interaction effect was not statistically significant. The development of the day-fine benchmarks and their implementation, therefore, increased fine amounts across the range of offenses but, as intended, they incorporated rather than changed the relative rank order of various offense types previously reflected in the court's sentencing outcomes.

The next set of ANOVAs measured the independent effect of number of prior convictions on the mean fine amounts (capped and uncapped), and whether that effect varied by

⁹ While the mean fine or uncapped amounts for violations are, as expected, always lower than for misdemeanors, the number of cases is small, and these results should be confirmed with additional research.

sample year. These models are presented in Appendix Tables 17 through 20 and the relevant tables of means are Appendix Tables 21 through 24.

In these analyses, while sample year had an effect on fine amount when examining uncapped fines (with the posttest fines being higher), prior conviction did not have an effect. In addition, while the overall models examining uncapped amount are significant at less than .001, the models looking at mean capped fine amount are either not significant (posttest day fines versus pretest fines on mean fine amount ordered), or marginally significant at only the .05 level (posttest dayfines and fixed fines versus pretest fines on mean fine amount. However, irrespective of the model, any significant differences found is a result of the effect of sample: while sample year always has an effect on either fine amount or uncapped amount, total prior convictions never does.

Thus, as the tables of means indicate, while fine amounts are routinely higher during the posttest as compared to the pretest, there is little variation across the categories of numbers of total prior convictions. As expected, then judges in this lower court followed the fine setting structure developed by the planning group and did not take the severity of an offender's prior criminal record into account when setting the amount of a day-fine; they may well have taken it into account in deciding whether or not to fine the offender. Instead, they relied on the arraignment charge in determining the size of the fine.

In conclusion, successful introduction of the day-fine system into the Staten Island court had the impact of dispersing the range of individual fine amounts, and increasing the average amounts around which fines were arrayed. This in turn increased the total fine dollars imposed by the court and the potential revenue derived from fines. These changes in amounts, however, occurred without changing the existing relationship between offense charge severity and relative fine level in the court's sentencing activities.

E. The Impact of the Day-Fine Pilot on Collection and Enforcement Outcomes

As noted above, the Staten Island Day-Fine Pilot Project introduced two types of changes into the Criminal Court. First, it introduced a new method for determining the amount of the fine -- the two-step day-fine process. Second, it introduced a more individualized system for setting the terms of payment (the installment amounts to be paid and the frequency of those payments), for tracking the fined offender post-sentence, and for notifying and warning him or her about payments due (see Greene, 1990).

The task of the evaluation, therefore, was to determine not only whether the implementation of the full day-fine pilot project (the day-fine system introduced into the court) affected the collection process, but also to assess the extent to which observed changes in collection outcomes were the product of the new fining setting mechanism (the day fine per se), the new collection procedures, or the combination of the two.

1. The structure of the analyses

The analysis used three sets of comparisons to measure these three possible effects. Recall that all day-fine cases in the posttest sample were randomly assigned to two groups: day-fine experimental cases were taken off the court calendaring method of setting payment schedules and applying enforcement techniques and instead were supervised by the day-fine pilot project post-sentence, and day-fine control cases were subject to the same calendaring and post-sentence collection and enforcement that had been used previously by the court.¹⁰

¹⁰ The traditional collection process involved placing each fined cases that did not pay in full at sentencing on the court calendar for subsequent appearances at four-to-six week intervals. If the offender failed to appear, a warrant for his or her arrest was issued. The warrant was processed by the police department but no personal service was carried out; instead, a mailed notification was sent from the police to the offender's address, indicating that a warrant had been issued for the failure to appear and exhorting the offender to voluntarily return to court. If no appearance occurred, the warrant remained open, unless the offender was arrested on a subsequent charge at which time the open fine case would be resolved along with the new case. If the fined offender returned to court voluntarily, however, calendaring for payment would continue unless the judge determined a re-sentence was appropriate in light of the offender's explanations as to the reasons for non-payment and non-appearance.

Therefore, (1) the effect of introducing the new day-fine system (the new fine setting as well as the new collection techniques) was assessed by comparing the day-fine experimental cases with pretest fines; (2) the effect of introducing the day fine per se (the new fine setting method) was assessed by comparing the day-fine control cases with the pretest fines; and, finally, (3) the effect of introducing the new collection and enforcement procedures was assessed by comparing the day-fine experimental cases with the day-fine control cases.

These comparisons were used to measure the effect of the day-fine pilot on several collection and enforcement outcomes. First, the impact on **collection rates** was examined by comparing the proportion of offenders who paid in full at sentence, the proportion who ever paid in full, and the proportion of fine dollars collected as a percentage of dollars imposed by the court. Second, the impact on **collection patterns** was measured by comparing the mean number of days to full payment (for those who made full payment) and the mean number of full payers, of post-sentencing court appearances, and of warrants ordered at nine-months post-sentence. Finally, to measure the impact on **overall enforcement outcomes**, the last known status of the case (paid in full, absconded, resentenced or jail alternative executed, case continued) was examined for the longest comparable follow-up period available in the research dataset.¹¹

¹¹ For the first set of analyses, the "seven month sub-samples" discussed in Section II-E above were used to make the three sets of comparisons, using chi square and Cramer's V. These sub-samples represent the longest comparable follow-up period (from 17-23 months post-sentence for individual cases).

For the second set of analyses, first the seven month sub-samples were used; a general linear model (GLM) or ANOVA was employed examine differences in the mean number of days to full payment for each of the three comparisons, using sample and the fine amount ordered as explanatory variables. Additionally, the full year samples were analyzed for each of the three sets of comparisons to test whether there were significant differences in the mean number of full-payers, total hearings or numbers of warrants ordered at nine months post-sentence, including initial fine amount ordered as an explanatory variable.

For the final set of analyses crosstabulations were carried out on the seven month sub-samples.

2. Collection rates

Table IV-4 summarizes the more detailed data in Appendix Tables 25 through 27 on collection rates for each set of comparisons.

The comparisons under (A) in Table IV-4 indicate that the full day-fine model (the day-fine method of setting fine amounts combined with the new collection techniques) had no statistically significant effect on the proportion of fined offenders who paid in full on the date of sentence, although the observed decrease for the day-fine experimentals (16.1% for the pretest and 7.6% for the posttest experimentals) is probably due to the substantially higher average day-fine amounts.¹²

Similarly, the comparisons under (A) also indicate that introduction of the day-fine system did not undermine the court's already high rate of success at ensuring offenders paid their fines in full. (Recall that one of the reasons this court was chosen for the pilot project was its successful fine collection history.) Indeed, 84.9 percent of the day-fine experimental cases eventually paid in full compared to 76.1 percent of the pretest fine cases. While the day-fine model collection rate is higher, the difference is not statistically significant; this indicates that while introducing the day-fine model did not diminish the court's successful collection rate, despite the substantial increase in average fine amounts, it did not significantly improve the court's overall success. However, when the amount paid as a percentage of the amount due is examined, day-fine cases that did not pay in full were significantly more likely than pretest fine cases to pay something toward the fine amount owed (9.4% compared to 1.7%) and less likely to pay nothing (5.7% compared to 22.2%).

The comparisons under (B) in Table IV-4 are not significant, indicating that introducing the day fines alone (without introducing new collection techniques) did not diminish

¹² Not surprisingly, therefore, the comparisons under (B) and (C) in this table show no independent effect on those who pay in full immediately for either the day fine per se or for the new collection model.

Table IV-4

Collection Outcomes (Seven Month Sub-Samples)*

	<u>A. Full Day-Fine Pilot Project</u>		<u>B. Day Fine Model Alone</u>		<u>C. Enhanced Collection Model Alone</u>	
	<u>Pretest</u>	<u>Post-test Experi- mentals</u>	<u>Pretest</u>	<u>Post-test Controls</u>	<u>Post-test Exp'ls</u>	<u>Post-test Controls</u>
% Paid in Full @ Sentence	16.1	7.6	16.1	17.2	7.6	17.1
% Paid in Full	76.1	84.9	76.1	71.4	84.9	71.4
Amount Paid as % of Amount Due:						
% Paid 0	22.2	5.7 ^a	22.2	25.7	5.7	25.7 ^b
% Partial Payment	1.7	9.4 ^a	1.7	2.9	9.4	2.8 ^b

* Follow-up period: 17-23 months post-sentence.

^a Comparison of Pretest vs. Day-Fine Experimentals significant.

^b Comparison of Day-Fine Experimentals vs. Day-Fine Controls significant.

the court's high collection levels, despite the increase in fine amounts. Equivalent proportions of the day-fine control cases and the pretest fine cases (71.4% and 76.1% respectively) were collected in full, even without adding new collection techniques.

In contrast, implementing the new collection techniques appears to have improved collection outcomes when comparing day-fine cases (as shown in section [C] of Table IV-4), although the differences do not always reach statistical significance. While the day-fine experimental cases did better in terms of the percent who paid in full than did the day-fine control cases (84.9% compared to 71.4%), this was not statistically significant. However, significantly fewer day-fine experimentals paid nothing toward their fines (5.7%) compared to day-fine controls (25.7%), and more paid something (9.4% compared to 2.8%).

When these results are viewed in relation to the earlier finding that collection rates for day-fine controls (day fine cases without new collection techniques) were no better than for the pretest fine cases, they indicate that the new enforcement procedures independently improved collection rates for those sentenced to the higher day fines, and that the higher day fines per se did not make collection more difficult overall.

3. Collection patterns

Length of time to full payment. While the introduction of day fines did not diminish the court's successful collection efforts (and improved it somewhat), the day fines took longer to collect. This pattern was closely related to the higher average day-fine amounts. Appendix Table 28 presents an ANOVA (analysis of variance) examining how long it took those fined offenders who eventually paid in full to pay their fines, for each of the comparisons.¹³

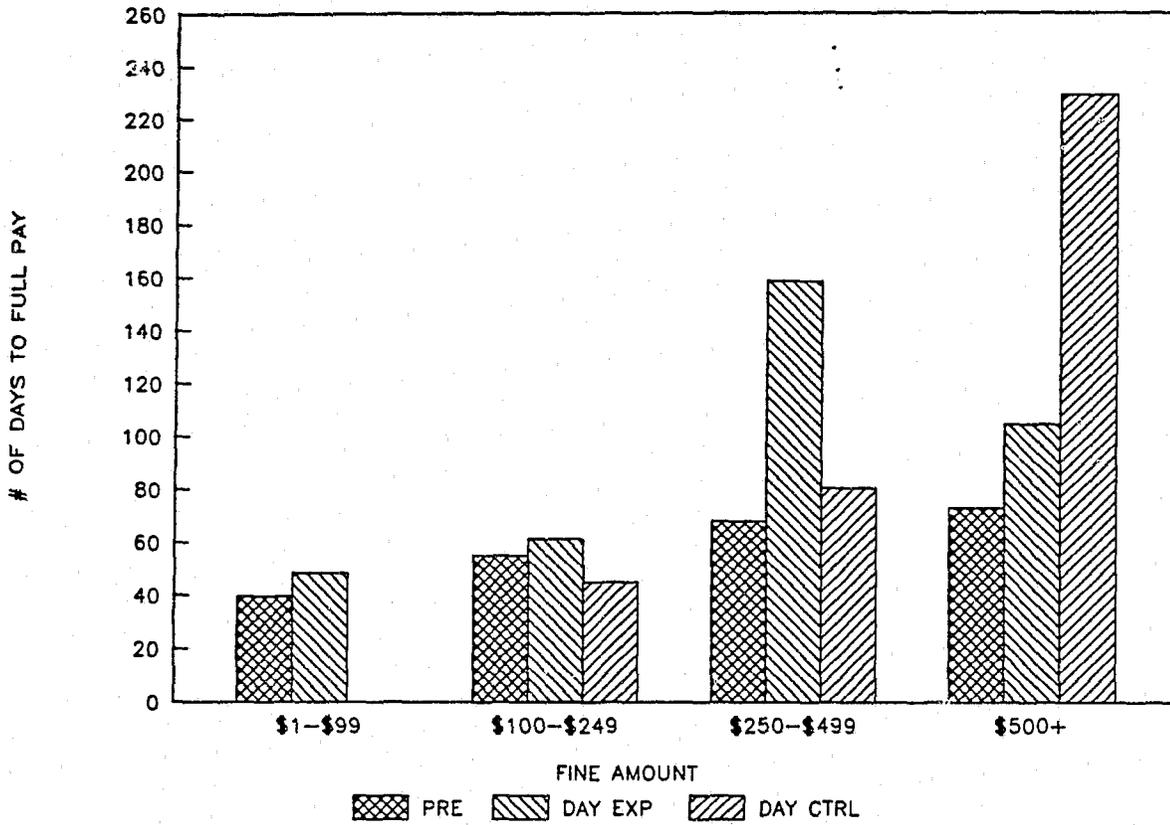
¹³ When the day-fine experimentals and the day-fine controls are categorized by fine amount for these ANOVAs, some of cell sizes are quite small; each group has one cell with under 10 cases (6 and 8). Therefore, the results discussed here should be viewed as preliminary, requiring confirmation by subsequent research.

Comparing the full day-fine model (the experimentals) with the pretest fines, both sample year and fine amount, as well as the interaction of the two, have a significant impact on the number of days to full payment. These same factors are significant when the day fines without the new collection techniques (the day-fine controls) are compared with the pretest fines. However, when the day-fine experimentals are compared to the day-fine controls, there is no significant difference in length of time to full payment (although they both took longer than the pretest cases), indicating that the new collection techniques did not independently affect this pattern.

The effect of these factors on the length of time to full payment can be seen in the means presented in Appendix Tables 29 through 31. The mean number of days to pay in full was significantly less for the pretest fines (55 days) than for either the posttest day-fine experimentals (114 days) or controls (119 days). This is not surprising in light of the substantially higher fine amounts that were imposed after introduction of the day fine. Indeed, the analysis indicates that for pretest fines and day-fine controls (those without the new collection techniques), the higher the fine, the longer it took on average to pay in full; for the pretest, there was a fourteen day or less difference between each fine level, see Chart IV-1 below). While the incremental differences in mean number of days to full payment between each level of fine amount show the same order for the posttest controls as for the pretest fines, the size of the differences was substantially larger. In contrast, for the day-fine experimentals, it was the mid-range fines (\$250 - 499) rather than the highest fines (\$500 and over) that took the longest to collect.

Number of post-sentence court appearances (at nine months). The introduction of day fines also had some effect on the number of post-sentence appearances scheduled by the court for collection purposes. Appendix Table 32 presents the ANOVA for the total number of appearances at nine months post-sentence, and the means are found in Appendix Tables 33 through 35.

CHART IV-1



Despite higher day fine amounts, the pilot project did not increase the number of appearances. When the mean number of appearances are compared for the pretest fines and for the posttest experimentals (combining day fines with the new collection techniques), there is no significant difference despite the higher day fines. The pretest fines had an average of 1.96 post-sentence appearance and the day-fine experimentals had 1.76.¹⁴

However, there is a significant difference in the number of appearances between the pretest and the day-fine controls (1.96 and 2.66), suggesting that the higher day-fine amounts, without the new collection techniques, resulted in more court appearances. The effect of the new collection techniques to off-set the impact of the higher day fine amounts on number of appearances is confirmed by the significant difference between the day-fine experimental and day-fine control cases (1.96 and 2.66). This is consistent with the finding above on the number of days to full payment. Without the individualized collection strategy built into the pilot project, the day-fine control cases, with their higher amounts, were brought back to court more frequently than either the smaller pretest fines or the day-fine experimentals.

The ANOVA also shows the expected positive relationship between fine amount and the number of court appearances (as fine amount increases, so do court appearances) when the pretest fines are compared with either the experimental or control day fines. However, this relationship is not found when the experimental and control day fines are compared with each other. This suggests that introducing a means-based method for setting fine

¹⁴ Recall that the day-fine experimental cases were subject to the new collection techniques which involved removing these cases from the normal calendar and placing them on an inactive calendar while the day-fine project staff supervised post-sentence collection. Therefore, we would expect fewer court appearances for these cases; this result occurred as expected even though the higher fines resulted in much longer collection periods than previously. In effect, therefore, the new collection techniques succeeded in collecting the higher fines without clogging the calendars and using valuable judicial time.

amounts offsets the possible negative impact of higher fine amounts on the number of court appearances needed to ensure compliance. This number, therefore, is equalized across offenders with different means and different fine levels.¹⁵ (See Chart IV-2).

Number of post-sentence warrants ordered (at nine months). The analysis of the number of arrest warrants ordered for failure to appear by nine months post-sentence (the ANOVAs in Appendix Table 36, and the tables of means in Appendix Tables 37 through 39) shows similar results. With respect to warrants, the day-fine pilot had a significant effect. Whereas the pretest fine cases averaged 0.55 warrants, the day-fine experimental cases (day fines plus the new collection techniques) averaged 0.26 warrants. Further, as with post-sentence appearances, the day-fine control cases had the highest average number of warrants (0.83).

In addition, the ANOVAs indicate that only when pretest fines and day-fine controls are compared is there a significant relationship between fine amount and the number of warrants ordered. As Chart IV-3 illustrates, this relationship appears to be curvilinear, with the mid-range fines having the lowest number of warrants ordered. Again, however, the number of cases with the highest fines is quite small. Given the lack of significance in the other two comparisons, it is likely that fine amount alone has only a limited effect on the number of warrants ordered.

Nevertheless, relying on the traditional collection techniques used in the absence of the new individualized collection strategy, the court relies more on warrants to enforce both

¹⁵ As seen in the Appendix tables of means (Tables 33-35), the number of appearances does not vary much by amount of the fine for these two samples. The number of cases in the highest fine groups for both the day-fine experimentals and controls is quite small, however, and these findings should be seen as preliminary and subject to further research. There is also no significant interaction effect for any of the three models.

CHART IV-2

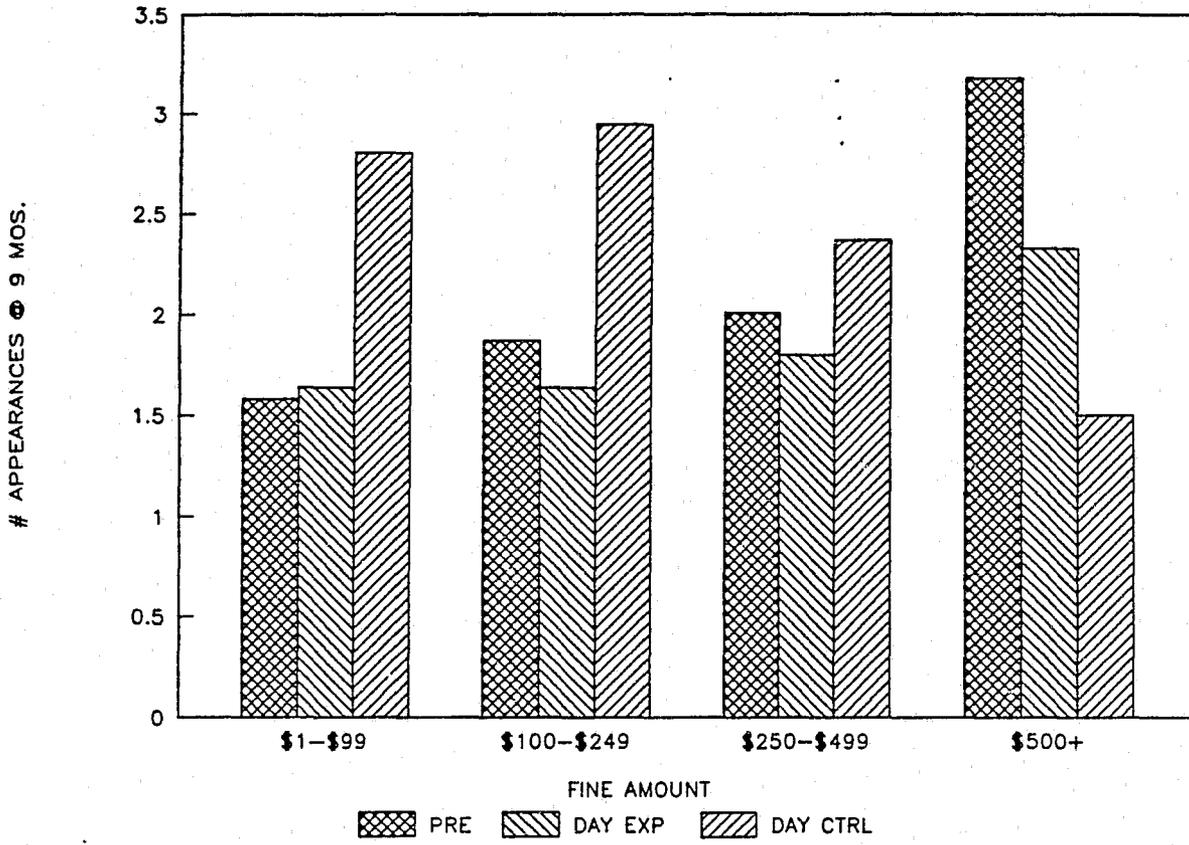
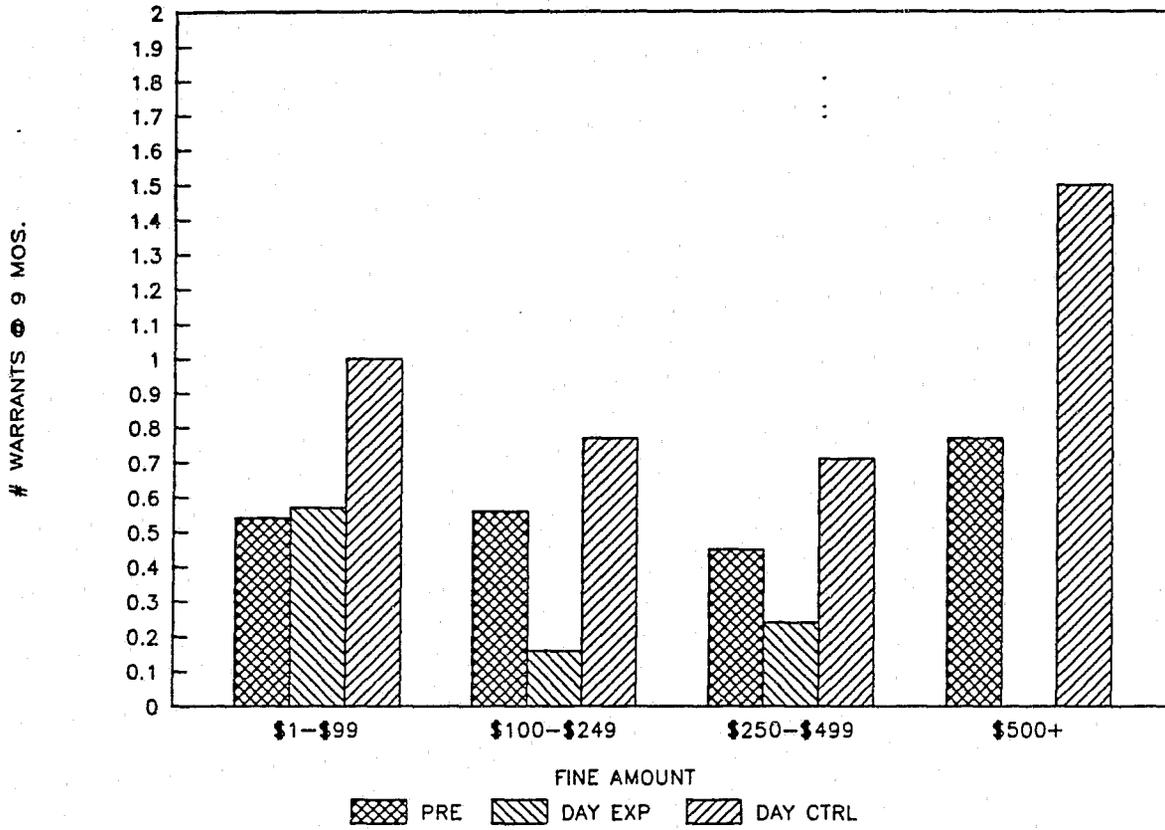


CHART IV-3



the day-fine control cases and the pretest fines. The day-fine experimentals, as expected, are the least subject to the use of warrants to ensure compliance.

Number who paid in full post-sentence (at nine months). The ANOVAs in Appendix Table 40 (and the tables of means in Appendix Tables 41 through 43) provide no surprises given the data above on the length of time fine cases take to full payment. Given the smaller fine amounts, the pretest sample had the largest proportion who paid in full by nine months post-sentence (72%). In contrast, with the larger day-fine amounts, the longer time to full payment is reflected in the 37 percent and 40 percent who paid in full among the day-fine experimentals and controls during the same timeframe.

Although fine amount was significantly related to average length of time to full payment in the ANOVA presented above (and in Appendix Table 28), it is not related to the proportion who pay in full within the initial nine months (Chart IV-4).

4. Final enforcement outcomes

Table IV-5 presents the final status of the sample cases at the conclusion of the longest comparable time period for the various groups.¹⁶ The table compares the three seven-month sub-samples with regard to the proportion who paid in full, who absconded, who were resentenced (or had the jail alternative to the original fine sentence executed), or whose case was still continued for payment.

Because such a large proportion of each sample paid in full during this 17 to 23 month period post-sentence, the other cells are small; as a result the chi square is not significant, but it is also likely to be unstable. The trends in the data, however, are illustrative and should be compared to findings from studies of other jurisdictions.

¹⁶ Recall, this is for the seven-month subsamples and reflects follow-up periods for individual cases ranging from 17 to 23 post-sentence.

CHART IV-4

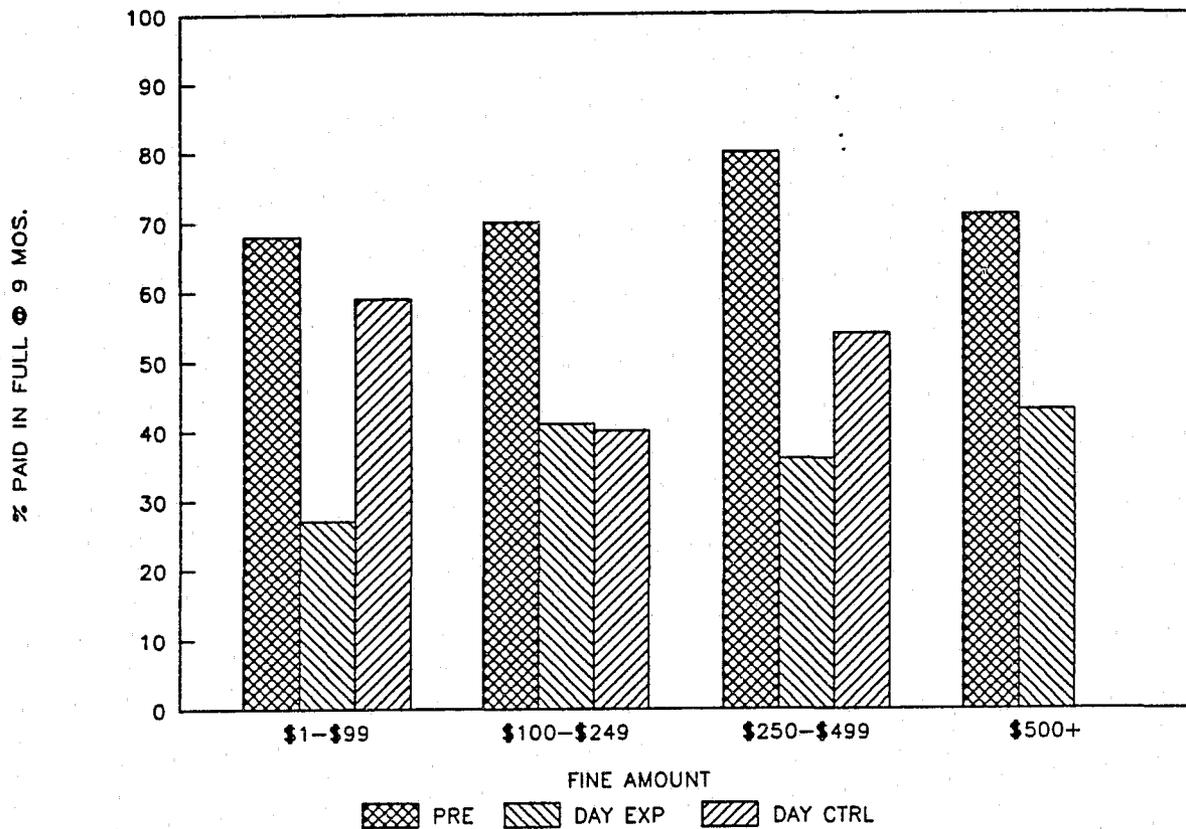


Table IV-5

Comparisons of Case Outcomes on Seven-Month Subsamples

	<u>Pd. in Full</u>		<u>Absconders</u>		<u>Resentenced or Execute Sentence</u>		<u>Continued</u>		<u>Total</u>	
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>
<u>Pre vs. Full</u>										
<u>Day-Fine Model:</u>										
Pretest	175	76.1	25	10.9	23	10.0	7	3.0	230	100.0
Day-Fine Exper.	45	84.9	3	5.7	3	5.7	2	3.8	53	100.0
[X ² = 2.57; DF = 3; p = NS; Cramer's V = .10]										
<u>Pre vs. Day Fine</u>										
<u>w/o Enforcement:</u>										
Pretest	175	76.1	25	10.9	23	10.0	7	3.0	230	100.0
Day-Fine Control	25	71.4	5	14.3	5	14.3	-	-	35	100.0
[X ² = 1.99; DF = 3; p = NS; Cramer's V = .09]										
<u>Day-Fine Exper. vs.</u>										
<u>Day-Fine Controls:</u>										
Day-Fine Exper.	45	84.9	3	5.7	3	5.7	2	3.8	53	100.0
Day-Fine Control	25	71.4	5	14.3	5	14.3	-	-	35	100.0
[X ² = 5.25; DF = 3; p = NS; Cramer's V = .24]										

The day-fine experimentals (who were subject to both the day fine and the new collection techniques) had the highest proportion who paid in full (84.9%, compared to 76.1% for the pretest cases and 71.4% for the day-fine controls), and the lowest proportion who absconded (5.7%, compared to 10.9% and 14.3%) or who had their case returned to court (5.7% compared to 14.3% for both of the other two groups).

The major finding is clear: day fines, despite significantly higher amounts, can be collected in full at high rates, rates as high as those of much smaller fixed fines; this appears to result largely from the fact that day fines take into account the means of the offender. Even the day-fine controls (who were subject to the traditional -- but relatively successful -- collection techniques of the court) had substantial rates of full compliance (slightly over 70%). The new, more individualized collection techniques, however, also appear to counteract some of the negative (though marginal in this jurisdiction) enforcement effects which might result from higher day-fine amounts. Thus, the collection outcomes for day-fine control cases, with their higher average amounts, tend resemble most closely the pretest fine cases, as opposed to the day-fine experimentals; they have a lower (although not statistically significant) proportion of full-payers (71.4% compared to experimentals' 84.9%) and a higher proportion of absconders (14.3% compared to 5.7%).

Table IV-6 summarizes all the findings presented above. In general, it appears that the use of the individualized collection system has the anticipated consequence of improving the collection rates for the day-fine experimentals -- they perform significantly better than either the pretest or the controls when the proportions who either paid in full or paid something are compared. Further, when patterns of collection are reviewed, this group is less costly of judicial resources, having fewer court hearings than the controls, and fewer warrants ordered than either the pretest or the control cases.

However, one unanticipated consequence of the individualized collection is a substantial increase in the length of time to full payment, with a concomitant decrease in the

Table IV-6

Summary of Results for Collection Rates, Collection Patterns, and Enforcement Outcomes for the Pretest, Day-Fine Experimentals, and Day-Fine Controls

	<u>Pretest</u>	<u>Day-Fine Experimentals</u>	<u>Day-Fine Controls</u>
% Paid in Full @ Sent.:	16%	8%	17%
% Ever Paid in Full:	76%	85%	71%
% Paid More than 0:	77%	95% ^a	74% ^{bc}
Mean # Days to Full Pay:	55 days	114 days ^a	119 days ^c
Mean # Total Appearances:*	1.96	1.76	2.66 ^{bc}
Mean # Warrants Ordered:*	.55	.26 ^a	.83 ^{bc}
% Paid in Full @ 9 Months:*	72%	37% ^a	49% ^{bc}
Enforcement Outcomes:			
% Paid in Full:	76%	85%	71%
% Abscond:	11%	6%	14%
% Res/Exec.	10%	6%	14%

* These variables reflect information as of nine months post-sentence based on the full-year's samples. All others used the seven-month subsets.

^a Comparison of Pretest vs. Day-Fine Exper. significant.

^b Comparison of Pretest vs. Day-Fine Controls significant.

^c Comparison of Day-Fine Experimentals vs. Controls significant.

proportion who have paid in full at nine months. Further, the control cases, with no individualized collection, seem to resemble the pretest in terms of collection rates, but fare somewhat worse than either the pretest or the experimentals when patterns of collection are examined -- they have the highest number of hearings and warrants ordered.

Additionally, while the length of time to full-payment is approximately the same with or without the individualized collection schedules (although substantially longer than the pretest), without the individualized collection the length of time to full payment is potentially much longer for fines above \$500.¹⁷ Because the number of fines in this category (i.e., \$500 or more) would increase if the legislative caps are removed by the New York State Legislature, these data suggest that introduction of the more individualized collection techniques used in the State Island pilot could help mitigate extreme delays in full payment.

Thus, the potentially negative impact on collection and enforcement of raising fine amounts by introducing day fines (particularly in jurisdictions whose existing collection strategies are not as successful to start with as were those in Staten Island) can probably be minimized by the new collection and enforcement techniques. While they allow fined offenders to take longer to pay the larger day fines in full, they require fewer court appearances and warrants than the traditional collection system. Without the new collection procedures, those jurisdictions with collection systems similar to Staten Island who introduce day fines can expect similar collection rates to those under their fixed fine systems; however, they may require somewhat more resources for additional court appearances and warrants. Thus, if it is economically feasible to have individualized collection as well as day fines, this would be the best approach for continued implementation of the day-fine approach to fining in American courts.

¹⁷ This finding relates to the mean number of days to full payment for the controls with fines of \$500 or more -- 229 days. However, the number of cases in this group is relatively small, so these results should be seen as preliminary.

F. The Impact of the Day-Fine Pilot on Equity

One goal of introducing a means-based system of calculating fine amounts is to ensure equity. The research, therefore, sought to address the issue of whether the day fines set in Staten Island during the pilot were more in accordance with an individual's ability to pay than were fixed fines. In addition, because day fines were clearly higher on average than fixed fines (especially uncapped day fines), the issue was raised as to whether individuals of differing means were more or less able to pay the fines under the new system as compared to the old system.

These questions were explored through several analyses of the pre and post samples.¹⁸ First, bivariate correlations were calculated between income and fine amount (both capped and uncapped) for the pretest fines and the posttest day fines. Second, a multiple regression was performed, using arraignment charge severity (distinguishing felonies and misdemeanors), sample (pre-post) and income, to determine if these factors had a significant independent effect on fine amount (capped and uncapped). The interaction of income and sample was then included in this regression to assess whether the effect of income on fine amount changed for the posttest day-fines as compared to the pretest fines. Finally, the final enforcement status of all cases was examined, first by income level and then by fine amount, in order to determine how people at different income and fine levels fared under day fines as compared to the pretest fixed-fine system.

¹⁸ All cases in both samples were used in these analyses, where there was a pre-arraignment interview conducted by the New York City Criminal Justice Agency (CJA, the city's pretrial services agency). CJA staff interview all persons summarily arrested, but do not interview individuals whom the police issue summonses to appear in court for arraignment. During the day-fine pilot, means data were collected on these summons cases prior to arraignment to ensure the judges had the same financial information for summons cases as they had for arrested cases. However, because income data were not available for pretest summons cases, only the CJA-interviewed summary arrest cases were used for this particular set of analyses.

1. The relationship between income and fine amount

As expected, when the actual (capped) fine amount imposed was examined, the correlation between income and fine amount was positive and moderate. In addition, the correlation coefficients were not substantially different when the pretest fine sample was compared with the posttest day-fine sample: for the pretest, the correlation was .22; for the day fines it was .20.

However, for uncapped fine amounts, the correlation coefficients with income were considerably different and stronger for the day fines (.35 as compared to .22 for the pretest cases). Thus, when the pilot day-fine system is examined, unfettered by the statutory fine caps, income is more strongly related to the amount of the fine than is the case for the fixed-fine system. As expected, therefore, a person's income appears to play a more significant role in determining the fine amount under the day-fine system.

To apply a more rigorous test to this analysis multiple regression was used to determine if income had a unique, independent effect on capped or uncapped fine amount (the dependent variables) after controlling for other factors deemed important in predicting fine amount. The factors included here were arraignment charge severity and sample, that is, whether the fines were set during the pretest period or during the posttest day-fine pilot. The results of this analysis are presented in Appendix Table 44, and the means and standard deviations for the independent variables are presented in Appendix Table 45.¹⁹

The overall models for both capped and uncapped versions of fine amount are both statistically significant; however, when the dependent variable is the uncapped fine amount, the variance explained by the three independent predictors is almost twice that of the explained about for the capped fine model (22.1% as compared to 11.4%). This reinforces

¹⁹ For ease of presentation, income is presented in the tables as a categorical variable; thus, the different fine amounts for different levels of income are readily displayed.

the finding above that the day-fine system enhances the impact of these factors. Arraignment severity, income and sample all have unique effects on both capped and uncapped fine amounts, and the relationships are generally in the expected direction. Fine amounts (capped and uncapped) increase as cases become more severe (from misdemeanors to felonies), as individuals' incomes increase, and when the fines are part of the day-fine sample as contrasted to the pretest sample.

However, the relative effects of these factors on fine amount differ when capped and uncapped fines are studied. The severity of the arraignment charge has the strongest independent effect on capped fine amount, but whether a case is a pretest fine or a day fine is the most important factor affecting the uncapped fine amount. The regression coefficients in Appendix Table 44 also indicate a significant and meaningful correlation between income and both capped and uncapped fine amounts, after controlling for the effects of arraignment charge severity and sample.

These analyses establish the independent effect of income on fine amount, and that fine amount is independently related to sample (pretest or day fine). They also verify that these relationships remain when the potentially confounding effect of arraignment charge type is introduced. The question that remains is whether the relationship between income and fine amount depends, at least in part, on whether cases are in the pretest or a posttest day fine sample.

To answer this question, an interaction variable was introduced into the regression model.²⁰ The inclusion of a variable measuring the interaction between income and sample

²⁰ An interaction variable is a new variable computed to measure the degree to which the effect of one variable depends on another variable. Here, the variable measured whether the effect of income on fine amount varied for the posttest as compared to the pretest; that is, whether income had a different relationship to fine amount under day fines than was the case during the pretest period.

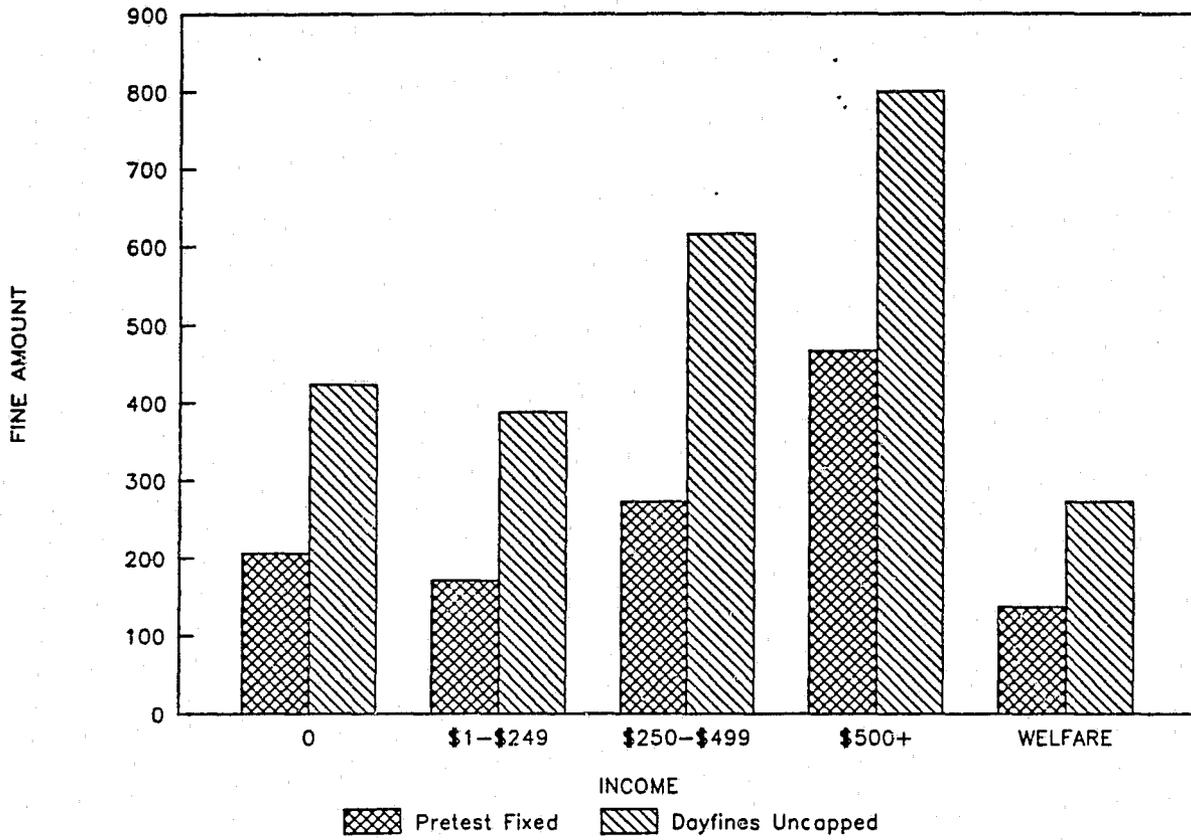
is included in the regression model measuring uncapped fine amounts in Appendix Table 46. The analysis indicates that independent effect of income is no longer significant. This table also reveals that income is dependent on sample; that is, the effect of income on uncapped fine amount differs between the pretest fixed fines and posttest day fines.²¹ This conditionality of the effect of income is displayed by the table of means (Appendix Table 47) which is illustrated below in Chart IV-5.

In this analysis, income is collapsed and compared across different levels for pretest fines and posttest day fines. For the pretest cases, the increase in fine amount between the moderate income level (\$250-499) and the high income level (\$500+) was 71 percent; for the uncapped day-fine cases, this increase was 30 percent.²² However, for the day-fine cases, the increase in uncapped amount for those reporting no income as compared with those earning between \$1 - \$250 was 45 percent; in the pretest this difference was 32 percent. Thus, the relationship between income and fine amount varies for the two sample periods: for the uncapped day fines, there is more spread at the lower end of the income scale and less spread at higher income levels.

²¹ The situation was different, however, for the model measuring capped fine amount. The inclusion a variable measuring the interaction between income and sample did not increase the predictive strength of the model; because the overall R² of this model increased by just .03 percent, the model is not presented.

²² It should be noted that, contrary to expectations, those reporting no income do not have the lowest fine amounts. Instead, it is those with a very small reported income who have the lowest fines. It appears, therefore, that when offenders report they are working, judges use their reported earnings as the basis of the day fine. However, qualitative data from observations at the bench also suggest that, for some offenders reporting no income (e.g., non-working spouses) judges use estimates of family income, and for others (e.g., non-working youth) the judges use estimates of potential earnings that could be obtained from employment in readily available minimum wage jobs to set the day-fine amount. In addition, some "unemployed" offenders are viewed by judges as likely to be engaged in illegal employment; in these situations, if the judge decides to impose a fine, he or she will estimate "income."

CHART IV-5



It is likely that having more means information (as well as being guided by the day-fine benchmarks) has helped judges differentiate among offenders with regard to fine amounts. At the lower income levels, this has prevented fine amounts from being tied to the lowest common economic denominator, thus increasing the spread.²³ For the higher income levels, however, the benchmarks and unit-value formula appear to have had narrowed the distance between fine amounts. The reason for this is not entirely clear; it may have something to do with the structure of the benchmarks or, more likely, the relatively few number of cases in which individuals have both high incomes and serious offenses. Recall that in this lower court, the severity of most offense types is limited, even if the arraignment charge is a felony. Further research, particularly on a felony court, would be useful to explore this issue with a wide range of offense severity as well as offender means.

2. The relationship between income and enforcement outcomes

The last analysis regarding income was carried out to explore whether poor people do better or worse complying with fine sentences under the day-fines system, particularly given the higher average day-fine amounts. Table IV-7 presents case outcomes (measured in terms of either full payment or resentenced/out on a warrant) for different income levels, comparing day-fine cases with pretest cases.

The only income level with a sufficient number of cases to assess statistical significance (those with no reported income) shows no difference in case outcome between the day-fine sample and the pretest; "poor" offenders, therefore, did no worse under the day-fine system than they did under fixed fines despite higher amounts. Similarly, the data for other income levels indicate the same results, although the numbers are too small to test for statistical significance. Therefore, there appears to be no difference in successful case

²³ Recall the analysis of the dispersion of fine amounts under the day-fine system discussed above.

Table IV-7

Comparison of Case Outcomes for Day Fines versus Pretest Fines,
Controlling for Income

	<u>Pretest*</u>		<u>Posttest*</u>	
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>
<u>Income = \$0:</u>				
Paid in Full	55	69.6	13	72.2
Resent/WO	24	30.4	5	27.8
TOTAL	<u>79</u>	<u>100.0</u>	<u>18</u>	<u>100.0</u>

[X² = .05; DF = 1; p = NS; Cramer's V = -.02]

<u>Income = \$1-249:</u>				
Paid in Full	21	84.0	6	85.7
Resent/WO	4	16.0	1	14.3
TOTAL	<u>25</u>	<u>100.0</u>	<u>7</u>	<u>100.0</u>

[X² = NA]

<u>Income = \$250-499:</u>				
Paid in Full	19	90.5	17	85.0
Resent/WO	2	9.5	3	15.0
TOTAL	<u>21</u>	<u>100.0</u>	<u>20</u>	<u>100.0</u>

[X² = NA]

<u>Income = \$500+:</u>				
Paid in Full	6	85.7	3	60.0
Resent/WO	1	14.3	2	40.0
TOTAL	<u>7</u>	<u>100.0</u>	<u>5</u>	<u>100.0</u>

[X² = NA]

*The seven-month subsamples are being used in this analysis.

outcomes for the day-fine cases when compared to the pretest cases, regardless of income level.

Because there are many cases missing income information, and because it is known that day fines are routinely higher than fixed fines, an additional analysis was done to test whether those offenders sentenced to higher fine amounts did better or worse complying with the fine order comparing day fines with pretest cases. These data, presented in Table IV-8, generally confirms the findings above -- for high fines as well as low fines, the offenders receiving day fines did as well paying the fine as those who received fixed fines. Therefore, although fines were generally higher under day fines, there was no deleterious effect on offenders' ability to comply with the sentence.

To summarize, the data clearly show that both income and arraignment charge severity had an independent effect on fine amounts in the expected direction. Additionally, income had an even more substantial effect on uncapped fines than on fine amounts capped by the statutory maximum fines. Although the proportionate increase in the uncapped amount for different income levels varied somewhat less for day fines than for pretest fines, it would appear that implementing the day-fine pilot standardized much of the decision-making that was already in place in Richmond County. Staten Island judges not only set fine amounts in relation to the severity of the offense before day fines were introduced, but they also took the offender's income into account, at least to some degree. Under day fines, this "calculus" appears to have been made overt, resulting in greater uniformity in sentencing.

An immediate effect of the day fine has been to reduce the extent to which fines are set in relation to tariffs that reflect the lowest common economic denominator in the offender population by enhancing the effect of the offender's own income. This process has dispersed fine amounts and has raised them for all levels of income. However, even with these higher amounts, there appears to be no change in "the success rate" either for offenders with differing income levels, or those with differing fine amounts. Recall that the

Table IV-8

Comparison of Case Outcomes for Day Fines versus Pretest Fines,
Controlling for Fine Amount

	<u>Pretest*</u>		<u>Posttest*</u>	
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>
<u>Fine Amount = \$1-99:</u>				
Paid in Full	59	72.0	6	54.6
Resent/WO	23	28.0	5	45.4
TOTAL	<u>82</u>	<u>100.0</u>	<u>11</u>	<u>100.0</u>

[$X^2 = 1.40$; DF = 1; p = NS; Cramer's V = .12]

Fine amount = \$100-249

Paid in Full	51	82.3	15	83.3
Resent/WO	11	17.7	3	16.7
TOTAL	<u>62</u>	<u>100.0</u>	<u>18</u>	<u>100.0</u>

[$X^2 = .01$; DF = 1; p = NS; Cramer's V = -.01]

Fine amount = \$250-499

Paid in Full	40	83.3	32	86.5
Resent/WO	8	16.7	5	13.5
TOTAL	<u>48</u>	<u>100.0</u>	<u>37</u>	<u>100.0</u>

[$X^2 = .16$; DF = 1; p = NS; Cramer's V = -.04]

Fine amount = 500+

Paid in Full	25	80.7	17	85.0
Resent/WO	6	19.3	3	15.0
TOTAL	<u>31</u>	<u>100.0</u>	<u>20</u>	<u>100.0</u>

[$X^2 = NA$]

*The seven-month subsamples are being used in this analysis.

sample sizes here are very small, which suggests the need for further research to validate these findings.

G. The Impact of the Day-Fine Pilot on Sentence Displacement

The final component of the evaluation of the Staten Island day-fine pilot was to estimate its potential "displacement effect" on sentencing outcomes. In other words, the question for the research was what types of sentences would have been imposed on the day fine cases had day fines not been an option during the posttest sample period? Would the day-fines cases have received fixed fines, or would they have received (i.e., "displaced") other sentencing options?²⁴ To answer this question a model was constructed to predict sentencing outcomes for the pretest sample cases; then this model was applied to the posttest sample cases.

1. Developing a model of sentencing outcomes

The sentencing outcome model was built using multinomial logistic regression, used when the dependent variable has multiple categories. Sentencing outcome (the dependent variable) contained six categories: dismissal, ACD, conditional discharge, fine, probation and imprisonment.²⁵ Because this prediction analysis required more information on each case than was collected for other aspects of the evaluation (e.g., full criminal history records), sub-samples were selected from the full pretest and posttest samples for these analyses.

²⁴ As noted earlier, the pilot did not seek to encourage displacement of other sentences; its main purpose was to demonstrate that a day-fine model for an American court could be crafted by a court-based planning group, and that that model could be successfully implemented. Measures of successful implementation, however, included such things as maintaining high collection rates, increasing revenue, and ensuring equity; these have been discussed above. Although some members of the planning group, including both judges and prosecutors, thought it desirable that day-fines be used as an alternative to short terms of custody for some offenders, pilot project staff did not undertake to transform sentencing patterns during the first pilot year.

²⁵ Other outcomes, such as community service or restitution, are usually imposed as either a condition of a discharge, ACD or probation and were not included directly in this analysis.

For each sentencing category, 156 cases were randomly sampled from the full pretest and posttest samples. This number was selected initially because it was the number of cases in the sentencing category with the fewest cases. However, because of adjustments to the datasets while they were being prepared for analysis, the final number of cases changed in some sentencing categories.²⁶ Table IV-9 lists the final size for each pre and post sentence category included in our modeling exercise.

For the posttest sample, the fine category contains both flat fines and day fines. Because the samples were drawn from computerized databases that did not record the distinction between different types of fines, this information had to be added from manual records after the sub-sampling was done.²⁷ Of the 141 cases included as fines, 67% were day fines and the rest were flat fines. Thus the total number of day fine post-sample cases used in the predictive analysis is adequate, but lower than is desirable.

To develop the list of predictor variables for inclusion in the sentencing model, case and court appearance data were used from official court records and detailed prior record data were obtained from official criminal history records ("rap sheets"). Some of the

²⁶ Between the time of the initial sampling and the final analysis, various changes were made in the both sample databases. Some cases were added and some removed from the original samples because an error was found in the court dates of some cases used to specify the sampling parameters (e.g., some cases in the original data file fell outside the time frame set for the pre and post sample periods). This situation resulted in some sentencing categories used for the modeling having fewer cases than initially anticipated (e.g., probation). Changes were also made in the sentencing categories of some court cases in both samples because manual examination of court records during the course of the pilot project revealed coding errors in the court's computerized files that were used to create the original databases.

²⁷ Although the post-sample fine category theoretically should contain all day fine cases, approximately one-third of the cases are flat fines. The reason for this occurrence is discussed in the section B above (The Substitution of Day Fines for Flat Fines). It is likely that if a Staten Island judge (rather than a temporary replacement judge) had been on the bench, that case would have received a day fine rather than a flat fine.

Table IV-9

Sentence Category and Sample Size
for Pretest and Posttest Cases

<u>Sentence Category</u>	<u>N for Pretest</u>	<u>N for Posttest</u>
Imprisonment	155	155
Dismissal	156	156
Probation	131	131
Cond. Discharge	156	147
ACD	156	156
Fine	145	141

variables created from these records include the amount of bail set at arraignment, whether the person was detained at arraignment, the type and severity of the charge at both arrest and conviction, and number and severity of prior convictions.

Appendix Table 49 presents a list of all variables initially included in the modeling. As we can see from this table, the variables from the rap sheets have a lower number of valid cases than the court record ("CJA-OCA") created variables, for both pre and post periods.²⁸ This discrepancy in the proportion of cases with relevant data on criminal history information in the two samples resulted from the high number of no returns when the rap sheets were requested, primarily due to missing NYSID numbers (the New York State Criminal Identifier) in the research computerized database. Lack of available NYSIDs, in turn, occur because many of the court cases in Staten Island either had no prior records or the defendant was issued a summons to appear in court (a DAT or desk appearance ticket) rather than being held in custody prior to arraignment. Because individuals with DATs are not interviewed by the pretrial agency (no bail application is needed), there was no NYSID number with which to link the automated criminal history information when it was requested. Most of the cases missing criminal history data, therefore, are the DAT cases.

The absent criminal history data items for many cases in the samples (whether there is really no prior record or whether that cannot be confirmed because the information was not matched) created a problem for the modeling. To predict sentencing outcomes, the model should include as many of the sample cases as possible. However, because the modeling technique relies on the listwise deletion of cases (i.e., the exclusion of a case if any

²⁸ For example, the proportion of non-missing cases for the criminal history variables in the pre period ranges from .47 to .57, while this range is quite high for the CJA-OCA variables (excluding the number of open cases variable) -- .7 to 1 (.82 to 1.00 if the bail at arraignment variable is excluded).

variable in the prediction equation is missing), including criminal history variables would reduce the number of cases to unacceptably low levels for both the pre and post periods.

Nevertheless, several preliminary models were run for these cases (i.e., the more serious non-DAT cases in the lower court sample) that included prior record to see what effect the criminal court variables might have on the predictive models and how significant a problem the sample loss was. The conclusion of these analyses was that prior record, for these misdemeanor cases, was not a particularly significant determinant of sentencing outcome; other variables were either more significant or were so highly correlated with prior record that they overwhelmed the independent effect of those variables. For example, the model including prior imprisonment predicted the proportion of correct sentencing outcomes at about the same rate as did the model which excluded this variable entirely. Furthermore, the models including number of prior misdemeanor and felony convictions also found these variables to be non-significant, thereby dropping them out for further iterations of the model.²⁹

The CJA-OCA variables listed in Appendix Table 49, therefore, absent criminal history variables, are the basis for the final prediction model developed. To be included in the final prediction equation, a variable had to meet the following three conditions: a significant relationship with the dependent variable; a low level of collinearity, or redundancy, with other predictor variables; and a significant contribution to the predictive strength of the overall model.

Normally, to meet the first condition, the probability of a zero-order correlation between the dependent and independent variables must be equal to or lower than five

²⁹ It is possible that prior record is more significant a determinant of sentence outcome for the least serious cases in the lower court -- i.e., those receiving DATs for which our database lacked criminal history data. If so, the model which follows is deficient; further research should explore this issue.

percent. However, because the dependent variable in this analysis was measured on a nominal scale, this simple Pearson correlation between the dependent and independent variables is meaningless. Instead, a set of dummy dependent variables was created out of the nominal variable and each was correlated with the independent variable.³⁰ If the probability of any these zero-order correlations between the set of dichotomized dependent variables and an independent variable was significant at the .05 level, then that independent variable meet the first criterion of selection.

A low degree of redundancy, or collinearity, was also required before a variable was included in the prediction model. One way to determine the extent of collinearity is to examine the correlation matrix of these variables for any relatively large zero-order correlations. A value of 0.4 was used to establish redundancy among variables.³¹

The final condition for inclusion was met when each individual variable in the model, as well as the model overall, achieved statistical significance. However, variables not meeting this latter condition could still be included if they added to the predictive strength of the model without substantially reducing the number of valid cases in the estimating technique.

2. The pretest sentencing model

Appendix Table 50 presents the best model developed from this process for predicting sentencing outcomes for the pretest period. Because six different sentencing

³⁰ For each sentencing category, a dummy variable that distinguished between that category and all the others was created. Each dummy variable then measures a particular quality of the dependent variable.

³¹ Although a correlation of 0.4 is an admittedly arbitrary value for establishing collinearity, its use assists in finding the most parsimonious solution possible without lessening the predictive power of the model.

These variables were also checked for collinearity through a second procedure. Each variable was alternatively regressed on the other variables and the resultant R² is then examined. A high value is a sign of high collinearity among the variables in the regression, and dropping the variable with the highest value solves the problem of redundancy among the predictor variables. This procedure overcame one of the shortcomings of looking solely at the bivariate correlations. An independent variable may approximate a linear combination of all the other independent variables, an occurrence that is not necessarily revealed by the examination of bivariate correlations.

outcomes were being predicted, multinomial logistic regression selected one of these sentencing outcomes to be a "reference" category; it then predicted the logits of the remaining sentencing categories when each was contrasted to the reference category. In this model, imprisonment was designated as the reference category. Thus, as can be seen in Appendix Table 50, five different sets of logit coefficients estimated the likelihood of a specific sentencing outcome against the likelihood of imprisonment.

Although many of these individual coefficient estimates in Appendix Table 50 are not significant at any of the accepted critical values, it is not this probability level that was used to assess the significant contribution of a variable. Rather, it was the joint impact of a variable on sentencing outcome across all five of the prediction equations. This chi square statistic, presented toward the bottom of Appendix Table 50, reveals that all five independent variables -- white, charge reduction, conviction severity, arraignment type, and detention -- were significantly related to the sentencing outcome of a case for the pretest period. A test for the overall fit of the model is also presented at the bottom of Appendix Table 50.³²

More importantly, this model was used to arrive at predicted probabilities for the six different sentence categories; that is, each prediction equation was transformed to arrive at the estimated probability for the six dependent categories.³³ An individual case thus had six

³² This test estimates the extent to which the designated model varies from a saturated version using the same variables. If our estimated model is a good fit, it should not significantly differ from the unrestricted model. Since this model does not, it can be concluded that the model is an appropriate fit.

³³ The multinomial probabilities for a particular sentence category is given by:
 $P_j = 2.71828^{**} (INT_j + ((X1 * B_j) + (X2 * B_j) + (X3 * B_j) + (X4 * B_j) + X5 * B_j))$,
where j = the sentence category predicted

INT = the intercept
X1 = white
X2 = charge reduction
X3 = conviction severity
X4 = arraignment type
X5 = detention at arraignment
B = the estimated coefficient

continued next page.../

predicted values for sentencing outcome and the category with the highest value was selected as the predicted sentence. This prediction would then be compared to the actual sentencing outcome for the case to ascertain its accuracy.

Applying this logic to the pretest cases, the model predicted the correct sentence category for 35.2 percent of the observations (N=742). Although not especially high, this rate of prediction is better than what could be attained relying only on the distribution of the dependent variable. For example, to estimate what the sentencing outcome would be without benefit of the information from these predictor variables, a common approach would be to predict the sentence category that results in the fewest errors. Using the distribution of actual sentences (Appendix Table 51), dismissal would be the appropriate prediction. Predicting dismissal for each case, however, would result in a correct choice 18.5 percent of the time; conversely, this would be an 81.5 percent error rate which is substantially in excess of the 64.8 percent rate for the model.³⁴

Similarly, when the model developed from the pretest cases was applied to the posttest cases for validation purposes, the same rate of prediction was found -- a correct rate of 32.8 percent. Once again, looking at the distribution of sentences for the posttest sample (Appendix Table 52), indicates that the correct prediction rate would be substantially lower (18.5%) if the only information used was from the dependent variable. Thus for both pre and post cases, the model was quite consistent in its prediction rate.

The probability of the sixth sentence category -- imprisonment -- is calculated by adding the predicted probabilities of the other five sentence categories and subtracting this total from unity.

³⁴ Indeed, if the PRE (proportion in the reduction of error) formula of $(E1-E2)/E1$ is used, where E1=the original error in prediction without the model and E2=the prediction error using the model, there is a 20 percent reduction in the original prediction error using the model.

3. Sentence displacement

Applying the model to the fine posttest cases will provide an estimate of the sentences that would have been given to day fines if that had not been a sentencing option, that is, what the potential displacement effect of day fines on the other sentencing categories might be. Of our posttest cases, 92 individuals actually received fines.³⁵ Using the model developed from the pretest cases, what the sentencing outcomes would have been for the 92 cases, in the absence of the day fine, was predicted. The results of this estimation are presented in Table IV-10.

Jail sentences. As expected, the model suggests that none of the relatively small number of posttest fine cases (7.7% of all sentences for penal law cases during the posttest period) would have received a sentence of imprisonment had they been sentenced in the pretest period. Thus, it does not appear that day fines would have displaced jail sentences. However, since that was never an explicit objective, this is consistent with project expectations.

Dismissals. According to the model, however, 28 percent of these fined cases in the posttest period would have been dismissed outright had day fines not been introduced. This suggests that additional means information and a judicial focus on monetary penalties may have encouraged judges to substitute a punitive monetary sanction for no sanction at all in at least some cases. This is interesting, however, because dismissals represent about 30 percent of all penal law case outcomes in the Staten Island court irrespective of sample

³⁵ Although we originally had 141 fined cases in the posttest period, listwise deletion of case in the modeling process left only 92 valid fine cases.

Because the number of day-fine cases as opposed to fixed fine cases remaining after listwise deletion was smaller than desirable, and because the primary reason that the fixed fines given during the posttest were not day fines was lack of judicial training, rather than qualitative differences in the cases, the predictive model was applied to all posttest fines (including fixed fines) as if they were day fines.

Table IV-10

Predicted Sentence Categories of Posttest Fine Cases

<u>Predicted Sentence Category</u>	<u>N</u>	<u>Percent</u>
Imprisonment	0	0
Dismissal	26	28.2
ACD	29	31.4
Cond. Discharge	5	5.4
Fine	13	14.1
Probation	19	20.7
	<hr/>	<hr/>
TOTAL:	92	99.8%

year; means-based day fines, therefore, may be a substitute in some cases not being dismissed for lack of evidence, but for lack of a suitable punishment option.

ACDs and conditional discharges. The model also indicates that an additional 31 percent of the small number of fined cases would have received an ACD in the pre-day fine period which, in Staten Island is often a dismissal conditional upon the payment of a restitution (4.7% in the pretest period) or completion of a community service order (24% in the pretest period). Another five percent of the fined cases in the day-fine period would have received some form of conditional discharge in the pretest period (8.7% of which in the pretest period were conditional upon a restitution payment, and 20.4% on community service).

This model suggests, therefore, that day fines might have been used in place of at least a few ACDs, some of which would have contained a community service or a restitution order. However, although the proportion of ACDs remained relatively stable across this period (declining slightly from 34.3% to 32.2%, the proportion of ACDs combined with community service and/or restitution increased during the day-fine period). Whereas during the pretest period, 24 percent of all ACDs had community service orders, 27.8 percent had such a condition in the posttest period; 4.7 percent had restitution orders in the pretest period compared to 8.9 percent in the posttest period.

Therefore, restitution and community service orders were generally increasing as sentencing option during the implementation of the day-fine pilot.³⁶ While day fines may have been substituted for a small number of these sentences, restitution and community service orders were clearly being added to ACDs (and CDs) in many other cases. This

³⁶ Similarly, conditional discharges increased slightly from 10.1 percent during the pretest period to 12.3 percent of the posttest cases. As with ACDs, however, the proportion of these CDs that were combined with restitution rose (from 8.7% to 11.2%) and the proportion combined with community service doubled (from 20.9% to 43%).

suggests that judges under the day-fine system, with more means information available at the time of sentencing and greater sensitivity to its use, were probably fine-tuning their imposition of monetary and work options.

Probation and flat fines. Finally, approximately 21 percent of this small number of fined cases were predicted to receive probation in the pretest period, and 14 percent of those actually receiving a day fine might have been sentenced to flat fines had there been no day fines. The new day fines might have substituted for flat fines (during a period in which fines were a small but relatively stable sentencing option for penal law cases, declining slightly from 9.2% of all sentences to 7.7%). They also may have substituted for a few sentences to probation (which also remained stable during this period at about 3% of all sentences).

Fine tuning rather than displacement. In summary, the displacement analysis, while merely suggestive, given the small number of cases actually involved in the modeling, provides some interesting insights into how the introduction of day fines might fit into sentencing patterns in the Staten Island criminal court.

An earlier discussion of the impact of day fines on sentencing patterns (section IV C above), which was based upon comparing the proportion of different sentencing options imposed by judges during the pretest and posttest period, indicated that the pilot, as expected, did not affect sentencing patterns overall. The data indicated that fine use in penal law cases was small and relatively stable (at around 8-9%), that the use of restitution and community service edged up somewhat, and that other major sentencing options remained relatively stable. The only noticeable change was a slight increase in the severity of sentencing options used in drug cases which appeared to have no relation to the day-fine pilot.

This outcome is counter to that which might be expected given the results of the predictive work. If the model were perfect and the day-fine option were not available, one would have expected marginal increases in non-fine dispositions and sentences (e.g., ACD,

dismissal) and a substantial decrease in fines overall. This is because only a small proportion of actual fines were predicted to have been fines. But it is unreasonable to believe that there would have been virtually no fines in the absence of the day-fine option. Therefore, it is reasonable to assume that the predictive model fails to capture the complex reality of the dispositional and sentencing process.

Thus, the displacement analysis adds nuance to the broad overview of dispositional and sentencing outcomes. Relatively little actual displacement apparently occurred following the introduction of day fines into the Staten Island court; in fact, fine use in penal law cases has drifted downward over the last decade (Zamist, 1986 revised) as other non-custodial options have come on the scene (including restitution and community service). Furthermore, sentencing patterns in most courts tend to be relatively stable over the short, and even long run, unless powerful advocacy is introduced into the negotiations (McDonald, 1986; Hillsman, 1982).

However, day fines quite possibly have helped the court fine-tune their sentencing decisions; this is at least a plausible interpretation of these data, which will surely be subject to further study as other jurisdictions introduce day fines into misdemeanor jurisdictions, similar to Staten Island, and into felony jurisdictions (such as Phoenix, Arizona), and then subject their innovations to rigorous research. In Staten Island, for example, a few offenders who once received no sanction from the court, may now be receiving a fine when judges have greater access to means information and a greater capacity to use it fairly.³⁷ If used properly, this substitution may have positive effects on the capacity of the courts to deter relatively new offenders. As alluded to in the introduction to this report (Section II-E, p. 18), research evidence on the deterrent value of monetary penalties, especially as an

³⁷ One hopes, but cannot know for sure, that when these fines are replacing outright dismissals, that they are doing so in cases where the evidence is sufficient for conviction, if only on a minor charge, and not as an extension of punishment where a case is not provable.

alternative to no court response for youthful offenders, is encouraging (Glaser and Gordon, 1990; McCord, 1985).

More likely, perhaps, judges may become more comfortable with monetary penalties generally (especially fines and restitution whose amounts can be adjusted to individual cases, in contrast to fees and other fixed "taxes" on offenders) and with work options (such as community service) when they feel they have sufficient information on offenders' economic circumstances to make appropriate distinctions. Judges in the Staten Island court may (the evidence is far from conclusive) have shifted some community service orders (CDs and ACDs with this as the condition) and a few probation sentences to fines, because the day-fine system permitted them to recognize even a poor offender's ability to pay something and to set a reasonable (small but collectable) amount that was proportionate to the offense. If so, they did this in the context of a noticeable (if not statistically significant) upward trend in their overall use of both community service and restitution.

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APPENDIX A
RESEARCH DESIGN AND METHODS

I. Introduction

A. Goals of the research

The research on the Staten Island day-fine pilot project was designed to accomplish two major goals. Specifically, the research aimed to measure the effects of the implementing the day-fine system, that is to assess the impact of the change from a fixed-fine to a day-fine system. The impacts focused on by the research included changes in sentencing outcomes (such as increases or decreases in the use of fines); in fine amounts; in collection rates and revenues; and in enforcement patterns (such as length of time to full payment).

Additionally, the research sought to assess what sentencing options day fines might have displaced during the pilot year. The research sought to explore whether day fines might have been used by the court in lieu of the traditional fixed fines, whether they might have been used in place of less punitive sanctions such as conditional and unconditional discharge, or whether they might have been used at all as an alternative to short jail sentences or to probation (sanctions generally regarded as more punitive than current fine sentences).

B. Research Design

To accomplish these goals, a pretest/posttest design was developed in order to provide comparisons before and after the pilot project began, that is, before and after the introduction of day fines into the court's repertoire of sentencing options. Although an experimental design with true random assignment to the experimental condition -- sentencing without the possibility of day fines and sentencing with that option -- was considered, the structure of the court in which the pilot was to be carried out made such a design difficult, perhaps impossible, to implement.

The court was a three-judge bench, all of whom had participated closely in the design of the day-fine system. Thus, even though cases could be randomly assigned to these judges prior to sentencing, the behavior of the judges themselves would be "contaminated" by their prior detailed knowledge of and support of the means-based fining concepts that underlies the day-fine system. Individual judges could not be expected to impose the logic of a day-fine system on randomly selected days or weeks and then switch back to a tariff system of fixed fines during other periods; alternatively, a specific judge could not be expected to use the old system while his or her colleagues were using the new system they had collaboratively devised using a logic which seemed preferable to the older sentencing option.

Nevertheless, the pre/post design used for the research was both a feasible and acceptable alternative. In addition, the overall pre/post design incorporated an experimental design to examine the impact of the new collection and enforcement system put in place as part of the day-fine pilot. All cases sentenced to a day fine during the posttest period were randomly assigned to either an experimental group (new enforcement/collection techniques) or to a control group (traditional enforcement/collection). The comparisons made among these various sub-groups are discussed below.

Overall, therefore, the research component of the Staten Island demonstration project, the results of which are presented in the body of this report, is to be viewed as an implementation evaluation of the day-fine pilot effort. In conjunction with other published documents, noted in the introduction to the report, the evaluation is designed to describe the day-fine system developed by the Staten Island court planning group, to examine how it was introduced into the system, and to assess its consequences along certain key dimensions of the court's operations. The various products of the demonstration project, including this research report, will be made available to practitioners and policymakers in other jurisdictions, as well as to scholars and researchers, so they may review the steps that were necessary to design and implement the first day-fine system in an American court as well as understand the issues that arose and the outcomes, intended and unintended, that resulted.

II. Key Research Questions

To accomplish these research tasks, five general research questions were posed about the introduction of day fines:

1. What changes occurred in sentencing patterns?
 - a) Did the proportions of different types of sentences imposed overall shift after the day-fine program was implemented?
 - b) What was the proportion of day fines among all fines; that is, did day fines replace fixed fines?
 - c) Did the types of sentences typically imposed for various types of offenses (charge categories) change after the program was implemented?
2. What changes occurred in fine amounts?¹
 - a) Were there significant changes in fine amounts ordered by the court after the introduction of day fines?
 - b) When the statutory caps on fine amounts are ignored, were day-fine amounts (which would have been imposed in the absence of the caps) significantly different from previous fixed-fine amounts?
3. What changes occurred in fine collection and enforcement outcomes?
 - a) What was the effect of introducing the day-fine pilot (the day-fine system including both new sentencing and new collection procedures) on

¹ One of the original concerns of the researchers was whether, through the increased availability of means information, there might be an increase in the use of restitution orders either alone or in combination with fines. If that had been the case, not only would changes in fine amounts be of interest, but so would changes in total financial penalties ordered. However, the number of restitution orders was relatively stable over time, and accounted for a small number of cases. (There were 114 cases in the pretest and 204 in the posttest; in combination with fines the Ns were 5 in the pretest and 6 in the posttest.) Thus, total financial penalty as an outcome variable was dropped from consideration.

collection and enforcement?

b) What was the effect of introducing the day fine per se on collection and enforcement (i.e., the effect of only the new sentencing procedures)?

c) What was the effect of introducing new enforcement procedures on collection and enforcement?

4. What changes occurred in the equitableness of financial penalties?

a) Were the day fines more closely related to income than fixed fines?

b) Was income less important in determining ability to pay under the day-fine approach?

5. What sentencing options did the day fines displace?

a) Did the same types of offenders receive day fines as had previously received fixed fines?

III. Sample Selection and Data Sources

A. Sample Selection

As indicated above, the structure of the day-fines pilot in Staten Island was appropriate for a pre/post design. Two samples of cases were selected that were composed of all penal law felony and misdemeanor arrests disposed in the Richmond Criminal Court: the pretest sample consisted of cases disposed from April 1, 1987 through March 31, 1988 (N = 4461), while the posttest consisted of cases disposed from September 1, 1988 through August 31, 1989 (N = 4883).²

The pretest was structured to ensure that the data reflected sentencing behavior prior to, but as close as possible, to the beginning of the project year. In addition, to avoid

² Only penal law cases (not criminal violations of the Vehicle and Traffic Law) were eligible for sentencing by a day fine because the VTL provides for mandatory fixed fine amounts; cases originating as VTL offenses, therefore, were excluded from the samples.

contaminating the comparison cases, the pretest sample had to be selected so that most of the fine enforcement activity for these cases had been completed before the project began. The time-frame for the posttest sample was determined by the project itself, allowing three weeks for project start-up and training to occur before the sampling process began.³

Among the posttest fines cases, several distinctions needed to be made for research purposes. First, the posttest fines could be either day fines or fixed fines.⁴ All day fines imposed by the court were randomly assigned using the docket number (even/odd) into one of two groups. The even-number dockets were assigned to the day-fine experimental group and subject to the pilot project's new collection and enforcement activities; these cases were taken off the normal court calendar, monitored post-sentence by the day-fine pilot staff, and returned to the calendar at the conclusion of collection activity. The day-fine control cases (odd-numbered dockets), however, remained as usual on the court's calendar and were subject to the same post-sentence collection activity as fines in the pretest period (and as any fixed fines imposed during the posttest period).

B. Data Sources

There were seven sources of data used to create the analytic databases for the two sample periods. Court appearance information was obtained from the computerized

³ It is important to note that the research posttest sample is different from the sample of cases followed by program operators/planners during the course of the pilot's first year. Data from the program's own sample of cases, and presented in descriptions of program implementation and operations written by Vera's Director of Court Programs, Judith A. Greene (1990), and by Hillsman and Greene (forthcoming), began with the date of program inception (August 12, 1988). Thus, this program sample includes cases from the last several weeks in August 1988 which are not included in the research pretest; in addition, because the program sample continued for one full year, it ended several weeks earlier than did the research sample period. As a result, the research posttest does not include fines imposed during the first two weeks of pilot operations, but does include fines from the last two weeks of August 1989.

⁴ Judges participating in the day-fine pilot were free to use the day fines or not, as they saw fit. One research issue was whether the judges would, in fact, use day fines or whether they would revert to traditional sentencing behavior. In addition, judges not trained in the use of day fines occasionally sat in the Staten Island court to handle the calendars when the court's regular judges were on vacation or ill. Thus, researchers needed to keep careful track of what kind of fine was imposed during the posttest when a fine was the sentence.

database of the New York City Criminal Justice Agency (CJA, the city's pretrial services agency); this included both pre- and post-sentence information. For any case not found in the CJA system, court record data was obtained from the New York State Office of Court Administration (OCA). The format of the information received from OCA, however, differed from that received from CJA; the OCA information was obtained in summarized form (for key milestone events such as arraignment, disposition, and sentencing), while the CJA information was for each specific court appearance. The OCA information available was for arrest, arraignment, disposition, and final sentence; however, if there was a resentence (for example, after a fine default), the original sentence was not available). In addition, no post-sentence (collection/enforcement) information was available from OCA; these data, as well as information on the original sentence for resentence cases, were obtained from the original court papers and hand-coded.

One drawback to the computerized court records of both OCA and CJA is that they do not contain information as to the conditions placed upon the offender at sentence when a conditional discharge or other types of sentences are imposed. This was relevant to this research because both community service sentences and monetary restitution are always imposed as conditions in the Staten Island court. Therefore, to collect supplemental sentencing information on the two samples (either community services sentences [CSS], which are administered by the Probation Department, or monetary restitution, which is administered by the Victim Services Agency [VSA]), the paper files of these two agencies were searched for all cases sentenced during the sample time periods. The appropriate supplemental sentence data were manually coded and added to the research database for each sample case.

The CJA pre-arraignment interview, commonly known as the "gold form," served as the source for income information on cases in the pretest and posttest samples (most of this

information is not computerized). CJA interviews defendants who are detained prior to arraignment, in order to provide information for use by the court in making release decisions.⁵ The gold form, and hence income information, however, was not available for individuals issued summons (Desk Appearance Tickets, or DATs) by the police. DAT cases in the research samples, therefore, could not be used in those analyses requiring income data.

For some analyses (specifically those involving displacement), information on prior criminal record was required. New York State official criminal history records (RAP sheets) were manually collected and hand-coded for all fine cases in the research samples as well as all cases in the sub-samples selected for the sentencing displacement analysis. For all other cases, the summary prior conviction information routinely collected by CJA at the time of their pretrial interview was available.⁶

Finally, additional information on posttest fine cases was collected by program staff and researchers and stored in the pilot project's computer; this information was retrieved at the end of the posttest period and merged with the research database. This information included whether a fine sentenced imposed on a posttest sample case was a day fine or fixed fine; whether a day-fine case was assigned to the experimental or control group for collection purposes; what values were used by the sentencing judge in calculating the day fine (information retrieved from the court papers); and final collection and enforcement data to supplement that supplied by CJA.

⁵ This interview schedule contains information on living situation, number of dependents, employment status, and income, as well as other information used by the court and CJA in the release decision. CJA verified this information to the extent possible by telephone.

⁶ The prior record information routinely available from CJA is number of prior felony convictions, number of prior misdemeanor convictions, and number of open cases; the CJA record also indicates if the instant case was a first arrest.

IV. Defining the Main Analytic Variables

Three general categories of variables were used in the analyses -- court case variables, program variables, and fine collection and enforcement variables. The definition of each is as follows.

1. Court case variables:

(a) Charge: Charge was defined as arraignment charge, because this charge was used by judges in setting the day fine amount (i.e., selecting the number of day-fine units).⁷ Arraignment charge was categorized in several ways. The most detailed version of this variable delineated specific penal law (PL) section-subsections which accounted for at least two percent of the data (e.g., 120.25); less frequent PL numbers were aggregated by section (e.g., all 120s) when the combinations accounted for at least two percent; the remaining PLs were grouped according to combinations of type (e.g., offenses against persons; property offenses) and severity (felony or misdemeanor). The other way in which arraignment charge was categorized for analysis was by seventeen general type-severity combinations. In a few analyses, type categories only or severity levels only (with A through E felonies, A and B misdemeanors, and violations specified) were used.

(b) Bail Amount and Release Status: These variables refer to the conditions of release set by the judge at arraignment. Bail amount is the "effective" bail amount, that is, cash amount needed to be put up to secure release or, if no cash amount was set, the face amount of the bond. (Cash rather than

⁷ There are extensive charge as well as sentence negotiations in the Staten Island Criminal Court; in the pretest, close to one-third (27% or 978 of the 3613 cases disposed as either misdemeanors or violations) were arraigned on more serious charges.

bond is most typical in New York City.) The release status variable reflected whether a defendant was in or out of custody at the conclusion of the arraignment hearing.

- (c) Sentence: This variable was defined first using the most detailed set of categories available in the raw data, including all the combinations of main sentences (probation, conditional discharge, jail, etc.) and all conditions added (e.g., CSS and monetary restitution [VSA]). Second, a more general set of categories were defined for the sentence variables; this included fine, adjournment in contemplation of dismissal (ACD), dismissal, conditional discharge (CD), probation, or jail, with only CSS as an additional sentence where appropriate (thus, additional categories were ACD plus CSS; CD plus CSS).
- (d) Prior record: The main variables used were number of prior felony convictions, misdemeanor convictions and the total of all criminal convictions. For some sub-sets of cases, prior record was further specified as the number of prior violent felony, general felony, and misdemeanor arrests; violent felony, general felony and misdemeanor convictions; prior sentences by type; and mean yearly arrest rate.
- (e) Income: This variable recorded the weekly net income given by defendants during the CJA pretrial interview. It was used both as a continuous and categorical variable. When used as a categorical variable, those on welfare were classified separately, with the other categories being 0 (no income; \$1-249; \$250-449; \$500 and more. In the multiple regression analyses, the welfare cases were dropped from the analyses.⁸

⁸ The welfare cases were dropped from the regression analysis because their income level as coded by CJA was "0," but they were receiving some (unrecorded) income. Thus, combining them with the "0" or no income category would have contaminated the meaning of that group.

(f) Fine amount ordered: This variable refers to the amount the offender was actually ordered to pay by the judge; these amounts are capped by statutory limits on fines. For day fines, therefore, fine amount ordered refers to the ordered amount, even if the computed amount of the day fine was greater than the cap.

(g) Uncapped fine amount: This amount refers only to day fines, and reflects the amount calculated by the judge using the day-fine workbook: the number of day-fine units from the benchmark scales multiplied by the unit value calculated using the valuation schema. The uncapped amount value was constructed by replacing the actual amount ordered with the calculated day-fine value for those day fines in the posttest where the actual amount ordered by the judge equalled the legislative cap, but the computed amount exceeded the cap. Otherwise, for analytic purposes, the uncapped amount equalled the original fine amount ordered.

2. Program variables.

(a) Study group: For the posttest sample, there were three groups by which fines were delineated -- day-fine experimentals (N = 133); these were the day-fine cases randomly selected for the pilot project collection process monitored by the pilot staff); day-fine controls (N = 107); these were day fines randomly selected for normal court collection and calendared post-sentence in the normal fashion; finally, fixed fines (N = 124) were cases for which the court papers showed no indication of a day-fine calculation.

3. Fine collection and enforcement variables.

(a) The variable "paid in full at sentence" was available for all fines cases.

- (b) The variable "amount paid as a percentage of the total amount ordered" was available both by month post-sentence and at the end of the data collection period (which is defined below).
- (c) The following variables were available for each month post-sentence:
 - Number of (post-sentence hearings) at which warrants were ordered;
 - number of hearings at which no payments were made (continuances);
 - number of hearings at which partial payments (or the final payment if it was made post-sentence) were made; number of total post-sentence hearings; number of "full-payers" (i.e., those who paid 100% of the amount fine ordered).
- (d) The following variable was available at end of the data collection period only: Final status of case (paid in full; failure to appear in court to pay and arrest warrant ordered; default, execute jail days alternative or resentence; case continued for payment).

V. Post-Sentence Follow-up Time Periods for Collection

When the research was initially designed, CJA's court appearance data was seen as the primary source of data for post-sentence collection and enforcement information, including data on the number of court appearances, amounts paid, and final case status. The dates specified for receipt of the CJA follow-up data were 3/01/89 for the pretest, and 10/31/89 for the posttest. This follow-up timeframe allowed for 12 through 24 months of follow-up for the pretest cases, but two through 14 months for the posttest cases.⁹ Because of the differences in follow-up period between the pretest and posttest, it was necessary to

⁹ These timeframes were selected on the bases of the original estimate of how long the research would take to complete; the research has taken longer to complete than originally anticipated because of problems associated with collection of the raw data. Nonetheless, the follow-up timeframes are approximately as originally designed.

compute the variables comparing the number of post-sentence hearings, as well as all the collection variables computed from the CJA data comparing amounts paid and final status, for each month post-sentence.

In addition, because of the pilot program in Staten Island continued beyond the end of the first demonstration, project staff periodically updated the case status and amounts paid for all day fines and fixed fines in the pilot program's own dataset; the most recent update was done in February of 1991. These data were merged with the research dataset, thereby extending the follow-up period for posttest cases from 17 to 29 months on the final outcome and final amount paid variables.

Although this number of months was longer for the posttest than for the pretest cases, it was still advantageous to use the longest follow-up time period possible. Therefore, a sub-sample of cases with seven months of equivalent follow-up (17 through 23 months) were isolated in both the pretest and the posttest samples and used in the analyses of the final case status and the amounts paid. The pretest seven month samples were April through October 1987; for the posttest the months were February through August 1989. These are not the precise same months in the calendar year, but because there is only a two month difference (one begins in February while the other begins in April) and because both sub-samples contain summer months they are seen as comparable. In the report, these sub-samples are referred to as the seven month sub-samples.

VI. The Structure of the Analyses

A. Assessing the Comparability of the Pretest and the Posttest

In the context of the pre/post design, one of the first issues to be addressed by the research was whether the two samples were comparable with respect to the types of cases coming before the court in the years before and during the implementation of the pilot project. Because this design did not examine the same cases before and after an intervention, it was important to determine if there were any differences in the mix of cases (specifi-

cally, charge types) coming into the court during the posttest year as compared with the pretest. Such differences would produce a confounding effect on any analysis attempting to assess the impact of introducing the day fine.

Thus, analyses were performed to assess the comparability of the two samples in relation to arraignment charge. Arraignment charge was presented first with the more detailed distribution, and then with the more general format. Comparability was assessed not only with the chi square statistic but also with Cramer's V. Because chi square is sample-size dependent, small changes in distributions can create statistical significance which is not conceptually meaningful; Cramer's V, on the other hand, provides an indication of the strength of the relationship between the two variables (like a correlation coefficient, with ranges from 0 to +1.00) with unity indicating a perfect relationship, can thus help elaborate on the substantive meaning of shifts in distributions.

B. Impact of Day Fines on Sentencing Patterns

To assess whether the introduction of day fines affected sentencing patterns, the distribution of sentences for the posttest was compared to the distribution for the pretest using the most general sentence categories; then the more specific sentence variable was examined to determine where the broader shifts actually occurred. Further, in order to control for any possible effects of changes in the mix of prior record, the specific posttest sentencing distribution was compared to the pretest by specific categories of prior conviction (first by felony convictions, collapsed into 0, 1, 2 or more; misdemeanor convictions were combined into 0, 1, 2, 3 or more, as was total prior convictions). Chi square and Cramer's V were used to assess the significance of any differences in the distribution of variables between pre and post.

Another key analysis was carried out to determine whether there were differences in the types of sentences received by different categories of arraignment charge during each sample. If there were shifts in the mix of cases coming into the court over the sample time periods, those shifts, rather than the implementation of the program, could be responsible

for any changes in sentencing outcomes observed. The main variables used in this section of the research were arraignment charge (presented using the type-severity combination), sample year and sentence.

C. Impact of Day Fines of Fine Amounts Ordered

For this part of the analyses, there were several ways in which the data were manipulated. First, the actual amount of the fine ordered was examined, comparing the entire posttest fines sample (which included both day fines [66%] and fixed fines [33%]) to the pretest fixed fines, and then comparing the posttest day fines only to the pretest fines. Second, the uncapped fines, or what the fines would have been had there been no legislative caps (i.e., limits on the amounts imposed), were analyzed, first for the posttest fine cases overall compared to the pretest, and then just for the day-fine cases compared to the pretest fines.¹⁰

There were several statistical techniques used to explore whether the mean fine amount (or uncapped amount) was significantly different for the various samples or sub-samples. First, two-way analyses of variance (ANOVA) were performed to determine if sample, or arraignment charge, or both, significantly affected mean fine amount. This analysis determines if mean amounts differ for the posttest as compared to the pretest, if they differ for the various types of arraignment charge categories, and if the effect of arraignment charge on fine amounts differs for the posttest as compared to the pretest. This same type of analysis was also performed using total prior convictions instead of arraignment charge, to ascertain what effect, if any, prior convictions might have on the

¹⁰ It should be recalled that when the value of the uncapped fine was created, the amount of the actual fine ordered was replaced only for those day fines where the ordered amount was equal to the legislative cap, and the calculated amount was greater than the cap; otherwise, the original amount ordered was retained. Thus, all cases have, for analysis purposes, an "uncapped amount" which is either the fine as ordered or the calculated amount.

mean fine or uncapped amounts set. Arraignment charge was categorized in the type-severity combination, and total prior convictions was categorized 0, 1, 2, 3 or more.

In addition, the total revenues potentially generated by each type of fining mechanism were calculated (i.e., the sum of all fines ordered under each system). Finally, the distribution of specific fines amounts was compared pre- and posttest to determine whether the diversity of fine amounts increased as hypothesized under day fines.

VII. The Impact of Day Fines on Collection and Enforcement Outcomes

The analysis of collection outcomes used three sets of comparisons that examined three different effects: (a) comparing collection rates for only the day-fine experimental cases with all pretest fines measured the effect of introducing the new day-fine system (i.e., the combined effect of changing both the court's sentencing procedures and how collection and enforcement are carried out); (b) comparing the day-fine control cases with the pretest fines measures the effect of introducing the day fine per se as a new sentencing option; and (c) comparing the day-fine experimentals with the day-fine controls measures the effect of introducing new collection and enforcement procedures.

Several different types of analyses were carried out in making these comparisons. First, to examine collection rates, significant differences in the proportion of offenders who paid in full at sentence, who ever paid in full, and the proportion of fine dollars collected as a percentage of the amounts imposed were assessed for the seven month sub-samples for each set of comparisons, using chi square and Cramer's V.

To assess changes in **collection patterns**, analysis of variance was used to examine differences in the mean number of days to full payment for each of the three comparisons, again using the seven month sub-samples, with sample and fine amount ordered introduced as explanatory variables.¹¹ In addition, to test whether there were significant differences in

¹¹ Typically, analysis of variance is robust enough to handle situations where the sample distributions are unbalanced. However, in two of the comparisons, the distributions were so unbalanced that the ANOVA algorithm used by SAS computed the sums of squares for the

the mean numbers of full payers¹², in the total number of hearings and in the number of arrest warrants ordered for each month post-sentence, the full year samples were analyzed for each of the three sets of comparisons, with the fine amount ordered introduced as an explanatory variable.¹³

Finally, **enforcement outcomes** were analyzed through a series of crosstabulations, focusing on final case outcome for the seven month sub-samples for each of the three sets of comparisons.

VIII. The Issues of Equity

Equity, that is, whether day fines were set more in accordance to income than were fixed fines, was explored in several ways. First, a simple correlation between income and fine amount was run for the pretest, the posttest day fines, and post-test fines overall to determine the relationship between income and fines for each sample. A multiple regression was then performed, using arraignment charge severity (i.e., felony versus misdemeanor), sample, and income, to assess the independent effects of these variables on fine amount. Additionally, the interaction between income and sample year was entered into the

individual effects incorrectly. When this occurred, the General Linear Model procedure (GLM) was used to compute the analysis of variance, and the algorithm that computes the sums of squares sequentially was specified. GLM was used for the first two comparisons -- for the day-fine experimentals and the pretest fines (Ns = 45 vs. 175), and for the day-fine controls and the pretest fines (Ns = 25 vs. 175).

¹² Because this variable was scored '0' if the offender had not paid in full by a given month post-sentence, and '1' if he had, the mean is interpreted like a percentage. Thus, if for Month 7, the mean number of full-payers was .67, this means that 67% of the sample had paid in full by that month.

¹³ Recall that, for the posttest, 100% of the sample had up to three months of follow-up; a declining percentage had a longer follow-up period. Thus, 95% of the sample had four months of follow-up, 93% had five months, 86% had six months, and so on. It was appropriate to do these analyses only through the first nine months post-sentence, because after that point, fewer than 64% of the posttest (76 experimentals and 70 controls) were available for analysis.

equation. Introduction of this interaction term indicated whether the effect of income on fine amount varied by sample.

Finally, the final payment status of cases was analyzed by income level and then by fine amount, in order to determine how different income levels or fine levels fared with regard to fine payments under the day-fine system as compared to the pretest fixed fine or tariff system.

IX. The Issue of Sentence Displacement

An important part of the research was the determination of what the sentence would have been for posttest fine cases had no day fines been instituted in the Staten Island System.¹⁴ Would those cases have received flat fines, or would they have received another type of sentence? To accomplish this task, a model that best predicted sentencing outcomes among the pretest cases was built using multinomial logistic regression. Predictions of sentencing outcomes for the posttest fines cases were then estimated from the results of the model developed from the pretest period.

When a dependent variable is nominal, the traditional techniques of classical regression prediction can not be used. Instead, the specification of nonlinear probability models is an often used alternative. Of these techniques, logistic regression is generally selected as the method of choice. Although best understood when the dependent variable is binary, this method can also be applied to nominal variables having more than two categories. In the dichotomous case, the probability of a positive or negative outcome is calculated by transforming the values of the dependent variable into logits and estimating the parameters of the model using maximum likelihood techniques. The same process is

¹⁴Because the only reason that fixed fines rather than day fines were given during the posttest was lack of judicial training rather than qualitative differences among the cases, all posttest fines (both day fines and fixed fines) were considered appropriate for inclusion in the modeling exercise.

done in the multinomial situation, except that there are the same number of estimating equations less one, as there are categories of the dependent variable. One category of the dependent variable is arbitrarily selected to serve as a reference and is compared to each of the other dependent categories. The equations can then be used to estimate the probabilities of an observation falling into one of the dependent categories. The general form of these equations is

$$P_j = 2.71828^{INT + B_{kj} X_{kj}}$$

where j = the sentence category predicted

P = the probability of a case for a predicted sentence category

INT = the intercept

B = unknown coefficient

X = value of a case for an independent variable

k = independent variable

This procedure was used in predicting what the sentencing category of a posttest fine case would have been had day fines not been instituted.

For inclusion in the final model, a potential independent variable had to meet the following three conditions: a significant relationship with the dependent variable; a low level of redundancy with other predictor variables; and a significant contribution to the predictive strength of the general model. In order to assess the first condition with a nominal dependent measure, a set of binary variables was created from the nominally-scaled dependent variable. These dichotomized variables were then individually correlated with each of the potential independent variables. An independent variable met the first condition if any of the subsequent zero-order correlations between it and the set of binary variables was significant at the .05 level or lower.

The condition of redundancy was met by an independent variable if its zero-order correlations with other potential predictor variables did not exceed 0.4. Variables that were included in the final model also contributed to the predictive strength of the model without greatly reducing the number of cases deleted from the final model.¹⁵

The model selected from the pretest period estimated five equations for five of the six categories of the dependent variable. The sixth category was used as a "reference" category, and its selection was arbitrary. The significance of each independent variable and the overall model was determined by the chi square statistic. A chi square for the overall impact of an independent variable on the dependent variable, its impact on only one category of the dependent variable, was calculated. The chi square test of the overall model entailed the degree to which the stipulated model varied from a saturated version using the same variables.

The five equations were then used to predict the probabilities of the six sentencing categories.¹⁶ The category with the highest probability was selected as the sentence that a person would receive, and this outcome was compared to person's actual sentence. A tally of all correct and wrong predictions was made for all pretest cases, and the correct rate of prediction was then calculated. This value was compared to the correct rate of prediction that would have occurred for pretest cases if no model was constructed.¹⁷ This model resulted in a better prediction rate than if no model had been estimated and was thus subsequently applied to the posttest cases to predict sentencing outcome. Results of this

¹⁵ A problem of this analysis was the lack of case information for many of the variables considered, specifically, information regarding detailed prior criminal record. Moreover, this lack of information among the cases was systematic. Many of the individuals not having information on various variables had less severe offenses than those persons who had valid data. Because the modeling estimation procedure used listwise deletion of the cases, the inclusion of these variables would have resulted in us examining a biased representation of cases entering the Richmond court system. Consequently, we made a decision to exclude certain variables from the final model.

¹⁶ Each equation predicted the probability that a case would receive a particular sentence outcome. The probability of the sixth, or 'reference', category was calculated by taking the sum of all five predicted probabilities of a case and subtracting that value from unity.

¹⁷ This latter value was calculated by predicting the sentence outcome with the highest percentage of cases for all persons in the pretest sample.

prediction for posttest fine cases then allowed the estimation of the sentence outcomes that these persons would have received if no day-fine system was in place in the Richmond County Court System.

APPENDIX B

TABLES

Table 1

Comparison of Pretest and Posttest Samples by Arraignment Charge
(specific and general penal law sections)

<u>Arraignment Charge</u> <u>Specific Sections:</u>	<u>Pretest</u>		<u>Sample</u>		<u>Posttest</u>	
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>
120-Assault, felony	163	3.8	227	4.9		
120-Assault, misd	138	3.2	157	3.4		
120-Assault 3°, misd	468	10.9	557	11.9		
140-Burglary 2°, misd	108	2.5	93	2.0		
140-Burglary 3°, felony	86	2.0	59	1.3		
140-Tresp 3°, misd	81	1.9	78	1.7		
145-Crim Misch, felony	50	1.2	59	1.3		
145-Crim Misch, misd	144	3.4	132	2.8		
155-Grand Larc 3°, felony	51	1.2	91	1.9		
155-Grand Larc, felony	108	2.5	115	2.5		
155-Petit Larc, misd	469	11.0	467	10.0		
160-Robbery 1°, felony	62	1.5	70	1.5		
160-Robbery 2°, felony	80	1.9	83	1.8		
160-Robbery 3°, felony	52	1.2	67	1.4		
165-Crim Poss 3°, felony	84	2.0	111	2.4		
165-Crim Poss 4°, felony	61	1.4	45	1.0		
165-Crim Poss 5°, misd	88	2.1	111	2.4		
165-Theft, misd	98	2.3	115	2.5		
205-Escape, misd	143	3.3	128	2.7		
215-Jud. Miscond, misd	34	0.8	90	1.9		
220-Crim Poss/Sale, felony	56	1.3	118	2.5		
220-Crim Sale 3°, felony	110	2.6	83	1.8		
220-Crim Poss 7°, misd	274	6.4	370	7.9		
221-Marij 5°, misd	131	3.1	27	0.6		
221-Marij, violation	68	1.6	32	0.7		
240-Pub order, misd	291	6.8	273	5.8		
240-Harrassment, violation	88	2.1	133	2.8		
240-Pub order, violation	58	1.4	78	1.6		
225-Weapons, misd	82	1.9	103	2.2		

Table 1 continued.../

Table 1 continued:

<u>Arrestment Charge</u> <u>General Types:</u>	<u>Pretest</u>		<u>Sample</u>		<u>Posttest</u>	
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>
Drugs, felony	10	0.2	5	0.1		
Drugs, misd	55	1.3	38	0.8		
Harm to Persons, felony	62	1.5	103	2.2		
Harm to Persons, misd	57	1.3	97	2.1		
Misconduct, felony	4	0.1	31	0.7		
Misconduct, misd	68	1.6	63	1.4		
Harm to Person & Property, felony	10	0.2	12	0.3		
Harm to Property, felony	64	1.5	48	1.0		
Harm to property, misd	56	1.3	67	1.4		
Harm to property, violation	29	0.7	23	0.5		
Theft, felony	55	1.3	52	1.1		
Theft, misd	49	1.1	42	0.9		
Weapons, felony	35	0.8	35	0.8		
Total	4280	100.0	4683	100.0		

$\chi^2 = 246.08$

DF = 41

p = <.001

Cramer's V = 0.17

Table 2

Comparison of Full Sentencing Distribution for
Posttest and Pretest

<u>Full Sentence</u>	<u>Pretest</u>		<u>Sample</u>		<u>Posttest</u>	<u>Difference in % and direction of Posttest from Pretest</u>
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>		
Fines	394	9.1	358	7.6	-1.5	
Fines/VSA	5	0.1	6	0.1	0	
ACD	1065	24.7	980	20.8	-3.9	
ACD/VSA	59	1.4	120	2.6	+1.2	
ACD/CSS	348	8.1	409	8.7	+0.6	
ACD/VSA/CSS	10	0.2	15	0.3	+0.1	
CD	314	7.3	279	5.9	-1.4	
CD/VSA	30	0.7	51	1.1	+0.4	
CD/CSS	83	1.9	235	5.0	+3.1	
CD/VSA/CSS	8	0.2	14	0.3	+0.1	
Dismissals	1230	28.5	1397	29.7	+1.2	
Jail	630	14.6	700	14.9	+0.3	
Jail/VSA	1	<0.1	3	0.1	-	
Jail (Time served)	4	0.1	3	0.1	0	
Jail (Time Served)/CD	2	0.1	4	0.1	0	
Probation	133	3.0	131	2.8	-0.2	
Probation/VSA	1	<0.1	1	<0.1	-	
TOTAL	4317	100.0	4706	100.0		

 $X^2 = 113.42$

DF = 16

p = <.001

Cramer's V = .11

Table 3

Comparison of Sentencing Distribution for Posttest versus Pretest,
Controlling for Prior Felony Convictions

	<u>Fines</u>	<u>ACD</u>	<u>ACD/CSS</u>	<u>Sentence</u>		<u>Dismissals</u>	<u>Jail</u>	<u>Probation</u>	<u>Total</u>
				<u>CD</u>	<u>CD/CSS</u>				
<u>Prior Felony = 0:</u>									
Pre									
N	190	450	113	166	53	567	337	100	1976
%	9.6	22.8	5.7	8.4	2.7	28.7	17.1	5.1	100.0
Post									
N	164	487	165	168	131	695	393	99	2302
%	7.1	21.2	7.2	7.3	5.7	30.2	17.1	4.3	100.0
Diff in %	-2.5	-1.6	+1.5	-1.1	+3.0	+1.5	0	-0.8	

[$X^2 = 38.84$; DF = 7; p = <.001; Cramer's V = .09]

Prior Felony = 1:

Pre									
N	17	43	2	26	-	78	58	7	231
%	7.4	18.6	0.9	11.3	-	33.8	25.1	3.0	100.0
Post									
N	18	44	10	23	9	125	81	7	317
%	5.7	13.9	3.2	7.3	2.8	39.4	25.6	2.2	100.0
Diff in %	-1.7	-4.7	+2.3	-4.0	-	+5.6	+0.5	-0.8	

[$X^2 = 16.15$; DF = 7; p = <.05; Cramer's V = .17]

Prior Felony = 2+:

Pre									
N	9	24	2	10	2	50	34	-	131
%	6.9	18.3	1.5	7.6	1.5	38.2	26.0	-	100.0
Post									
N	11	11	5	10	4	74	57	1	173
%	6.4	6.4	2.9	5.8	2.3	42.8	33.0	0.1	100.0
Diff in %	-0.5	-11.9	+1.4	-1.8	+0.8	+4.6	+7.0	-	

[$X^2 = 12.88$; DF = 7; p = <.10; Cramer's V = .20]

Table 4

Comparison of Sentencing Distribution for Posttest versus Pretest,
while Controlling for Prior Misdemeanor Convictions

	<u>Fines</u>	<u>ACD</u>	<u>ACD/CSS</u>	<u>Sentence</u>		<u>Dismissals</u>	<u>Jail</u>	<u>Probation</u>	<u>Total</u>
				<u>CD</u>	<u>CD/CSS</u>				
<u>Prior Misdemeanor = 0:</u>									
Pre									
N	145	416	113	133	44	466	141	79	1537
%	9.4	27.1	7.4	8.7	2.9	30.3	9.2	5.1	100.0
Post									
N	148	452	164	136	118	610	196	92	1916
%	7.7	23.6	8.6	7.1	6.2	31.8	10.2	4.8	100.0
Diff in %	-1.7	-3.5	+1.2	-1.6	+3.3	+1.5	+1.0	-0.3	

[$\chi^2 = 32.78$; DF = 7; $p = <.001$; Cramer's V = .10]

Prior Misdemeanor = 1:

Pre									
N	32	51	2	26	2	100	65	16	294
%	10.9	17.4	0.7	8.8	0.7	34.0	22.1	5.4	100.0
Post									
N	23	50	7	32	16	114	72	9	323
%	7.1	15.5	2.2	9.9	5.0	35.3	22.3	2.8	100.0
Diff in %	-3.8	-1.9	+1.5	+1.1	+4.3	+1.3	+0.2	-2.6	

[$\chi^2 = 17.68$; DF = 7; $p = <.05$; Cramer's V = .17]

Table 4 continued.../

Table 4 continued:

	<u>Fines</u>	<u>ACD</u>	<u>ACD/CSS</u>	<u>Sentence</u>		<u>Dismissals</u>	<u>Jail</u>	<u>Probation</u>	<u>Total</u>
				<u>CD</u>	<u>CD/CSS</u>				
<u>Prior Misdemeanor = 2:</u>									
Pre									
N	17	21	1	16	5	42	46	5	153
%	11.1	13.7	0.7	10.5	3.3	27.5	30.1	3.3	100.0
Post									
N	8	21	4	13	5	62	67	3	183
%	4.4	11.5	2.2	7.1	2.7	33.9	36.6	1.6	100.0
Diff in %	-6.7	-2.2	+1.5	-3.4	-0.6	+6.4	+6.5	-1.7	

[$X^2 = 11.01$; DF = 7; p = <NS; Cramer's V = .18]

Prior Misdemeanor = 3+:

Pre									
N	22	29	1	27	4	87	177	7	354
%	6.2	8.2	0.3	7.6	1.1	24.6	50.0	2.0	100.0
Post									
N	14	18	5	21	5	108	196	3	370
%	3.8	4.9	1.4	5.7	1.4	29.2	53.0	.8	100.0
Diff in %	-2.4	-3.3	+1.1	-1.9	+0.3	+4.6	+3.0	-1.2	

[$X^2 = 12.36$; DF = 7; p = <NS; Cramer's V = .13]

Table 5

Comparison of Sentencing Distribution for Posttest versus Pretest
while Controlling for Prior Convictions

	<u>Fines</u>	<u>ACD</u>	<u>ACD/CSS</u>	<u>Sentence</u>		<u>Dismissals</u>	<u>Jail</u>	<u>Probation</u>	<u>Total</u>
				<u>CD</u>	<u>CD/CSS</u>				
<u>Prior Total Convictions = 0:</u>									
Pre									
N	138	392	111	113	44	423	120	64	1405
%	9.8	27.9	7.9	8.0	3.1	30.1	8.6	4.6	100.0
Post									
N	130	426	156	121	113	533	167	85	1731
%	7.5	24.6	9.0	7.0	6.5	30.8	9.7	4.9	100.0
Diff in %	-2.3	-3.3	+1.1	-1.0	+3.4	+0.7	-1.1	+0.3	

[$X^2 = 29.58$; DF = 7; p = <.001; Cramer's V = .10]

Prior Total Convictions = 1:

Pre									
N	29	49	2	38	2	101	59	25	305
%	9.5	16.1	0.7	12.5	0.7	33.1	19.4	8.2	100.0
Post									
N	35	60	12	34	17	133	70	14	375
%	9.3	16.0	3.2	9.1	4.5	35.5	18.7	3.7	100.0
Diff in %	-0.2	-0.1	+2.5	-3.4	+3.8	+2.4	-0.7	-4.5	

[$X^2 = 22.33$; DF = 7; p = <.01; Cramer's V = .16]

Table 5 continued.../

Table 5 continued:

	<u>Fines</u>	<u>ACD</u>	<u>ACD/CSS</u>	<u>Sentence</u>		<u>Dismissals</u>	<u>Jail</u>	<u>Probation</u>	<u>Total</u>
				<u>CD</u>	<u>CD/CSS</u>				
<u>Prior Total Convictions = 2:</u>									
Pre									
N	17	24	1	17	4	52	49	7	171
%	9.9	14.0	0.6	9.9	2.3	30.4	28.7	4.1	100.0
Post									
N	9	27	4	17	6	76	72	3	214
%	4.2	12.6	1.9	7.9	2.8	35.5	33.6	1.4	100.0
Diff in %	-5.7	-1.4	+1.3	-2.1	+0.5	+5.1	+4.9	-2.7	

[$X^2 = 10.6$; DF = 7; p = <NS; Cramer's V = .14]

Prior Total Convictions = 3+:

Pre									
N	32	52	3	34	5	118	201	11	456
%	7.0	11.4	0.7	7.5	1.1	25.9	44.1	2.4	100.0
Post									
N	19	28	8	29	8	152	222	5	471
%	4.0	5.9	1.7	6.2	1.7	32.3	47.1	1.1	100.0
Diff in %	-3.0	-5.5	+1.0	-1.3	+0.6	+6.4	+3.0	-1.3	

[$X^2 = 21.21$; DF = 7; p = <.01; Cramer's V = .14]

Table 6

Comparison of Sentencing Distribution for Posttest versus Pretest,
Controlling for Arraignment Charge
(type and severity combined)

	<u>Fines</u>	<u>ACD</u>	<u>ACD/CSS</u>	<u>Sentence</u>		<u>Dismissals</u>	<u>Jail</u>	<u>Probation</u>	<u>Total</u>
				<u>CD</u>	<u>CD/CSS</u>				
<u>Crimes against Person, Felony:</u>									
Pre									
N	32	61	17	40	7	240	50	31	478
%	6.7	12.8	3.6	8.4	1.5	50.2	10.5	6.5	100.0
Post									
N	37	91	20	39	30	297	67	29	610
%	6.1	14.9	3.3	6.4	4.9	48.7	11.0	4.8	100.0
Diff in %	-0.6	+2.1	-0.3	-2.0	+3.4	-1.5	+0.5	-1.7	

[$X^2 = 13.61$; DF = 7; $p = <.10$; Cramer's V = .11]

Crimes against Persons, Misdemeanor:

Pre									
N	12	266	32	60	7	205	29	5	616
%	2.0	43.2	5.2	9.7	1.1	33.3	4.7	0.8	100.0
Post									
N	30	254	48	55	24	270	38	7	726
%	4.1	36.0	6.6	7.6	3.3	37.2	5.2	1.0	100.0
Diff in %	+2.1	-7.2	+1.4	-2.1	+2.2	+3.9	+0.5	+0.2	

[$X^2 = 22.30$; DF = 7; $p = <.001$; Cramer's V = .13]

Table 6 continued.../

Table 6, page 2

	<u>Fines</u>	<u>ACD</u>	<u>ACD/CSS</u>	<u>Sentence</u>		<u>Dismissals</u>	<u>Jail</u>	<u>Probation</u>	<u>Total</u>
				<u>CD</u>	<u>CD/CSS</u>				
<u>Weapons Crimes, Felony:</u>									
Pre									
N	7	1	-	1	-	17	2	2	30
%	23.3	3.3	-	3.3	-	56.7	6.7	6.7	100.0
Post									
N	6	-	1	1	-	15	4	4	31
%	19.4	-	3.2	3.2	-	48.4	12.9	12.9	100.0
Diff in %	-3.9	-	-	-0.1	-	-8.3	+6.2	+6.2	

[X² = .NA]

Weapons Crimes, Misdemeanor:

Pre									
N	9	30	7	6	4	14	8	2	80
%	11.3	37.5	8.8	7.5	5.0	17.5	10.0	2.5	100.0
Post									
N	17	28	13	6	2	27	7	2	102
%	16.7	27.5	12.8	5.9	2.0	26.5	6.9	2.0	100.0
Diff in %	+5.4	-10.0	+4.0	-1.6	-3.0	+9.0	-3.1	-0.5	

[X² = 6.62; DF = 7; p = NS; Cramer's V = .19]

Property Crimes, Felony:

Pre									
N	24	58	35	55	26	121	67	64	450
%	5.3	12.9	7.8	12.2	5.8	26.9	14.9	14.2	100.0
Post									
N	31	66	40	52	47	125	88	46	495
%	6.3	13.3	8.1	10.5	9.5	25.3	17.8	9.3	100.0
Diff in %	+1.0	+0.4	+0.3	-1.7	+3.7	-1.6	+2.9	-4.9	

[X² = 5.84; DF = 7; p = NS; Cramer's V = .08]

Table 6 continued.../

Table 6, page 3

	<u>Fines</u>	<u>ACD</u>	<u>ACD/CSS</u>	<u>Sentence</u> <u>CD</u>	<u>CD/CSS</u>	<u>Dismissals</u>	<u>Jail</u>	<u>Probation</u>	<u>Total</u>
<u>Property Crimes, Misdemeanor:</u>									
Pre									
N	62	198	119	65	20	157	131	12	764
%	8.1	25.9	15.6	8.5	2.6	20.6	17.2	1.6	100.0
Post									
N	74	175	117	44	45	151	134	14	754
%	9.8	23.2	15.5	5.8	6.0	20.0	17.8	1.9	100.0
Diff in %	+1.7	-2.7	-0.1	-2.7	+3.4	-0.6	+0.6	+0.3	

[X² = 16.40; DF = 7; p = <.05; Cramer's V = .11]

Drug Crimes, Felony:

Pre									
N	18	13	2	8	2	92	20	5	160
%	11.3	8.1	1.3	5.0	1.3	57.5	12.5	3.1	100.0
Post									
N	14	11	4	10	8	109	27	14	197
%	7.1	5.6	2.0	5.1	4.1	55.3	13.7	7.1	100.0
Diff in %	-4.2	-2.5	+0.7	+0.1	+2.8	-2.2	+1.2	+4.0	

[X² = 8.15; DF = 7; p = <.05; Cramer's V = .11]

Drug Crimes, Misdemeanor:

Pre									
N	115	94	26	31	11	127	48	5	457
%	25.2	20.6	5.7	6.8	2.4	27.8	10.5	1.1	100.0
Post									
N	59	83	45	28	30	116	66	4	431
%	13.7	19.3	10.4	6.5	7.0	26.9	15.3	0.9	100.0
Diff in %	-11.5	-1.3	+4.7	-0.3	+4.6	-0.9	+4.8	-0.2	

[X² = 35.47; DF = 7; p = <.001; Cramer's V = .20]

Table 6 continued.../

Table 6, page 4

	<u>Fines</u>	<u>ACD</u>	<u>ACD/CSS</u>	<u>Sentence</u>		<u>Dismissals</u>	<u>Jail</u>	<u>Probation</u>	<u>Total</u>
				<u>CD</u>	<u>CD/CSS</u>				
<u>Drug Crimes, Violations:</u>									
Pre									
N	6	25	2	2	-	30	3	-	68
%	8.8	36.8	2.9	2.9	-	44.1	4.4	-	100.0
Post									
N	2	18	6	1	-	3	2	-	32
%	6.3	56.3	18.8	3.1	-	9.4	6.3	-	100.0
Diff in %	-2.5	+19.5	+15.9	+0.2	-	-34.7	+1.9	-	

[X² = NA]

<u>Forgery/Fraud, Felony:</u>									
Pre									
N	17	3	4	9	1	11	4	3	52
%	32.7	5.8	7.7	17.3	1.9	21.2	7.7	5.8	100.0
Post									
N	17	12	3	3	1	9	1	3	49
%	34.7	24.5	6.1	6.1	2.0	18.4	2.0	6.1	100.0
Diff in %	+2.0	+18.7	-1.6	-11.2	+0.1	-2.8	-5.7	+0.3	

[X² = NA]

<u>Forgery/Fraud, Misdemeanor:</u>									
Pre									
N	14	17	7	4	1	13	4	-	60
%	23.3	28.3	11.7	6.7	1.7	21.7	6.7	-	100.0
Post									
N	11	19	5	4	4	6	8	-	57
%	19.3	33.3	8.8	7.0	7.0	10.5	14.0	-	100.0
Diff in %	-4.0	+5.0	-2.9	+0.3	+5.3	-11.2	+7.3	-	

[X² = NA]

Table 6 continued.../

Table 6, page 5

	<u>Fines</u>	<u>ACD</u>	<u>ACD/CSS</u>	<u>Sentence</u>		<u>Dismissals</u>	<u>Jail</u>	<u>Probation</u>	<u>Total</u>
				<u>CD</u>	<u>CD/CSS</u>				
<u>Misconduct Crimes, Felony:</u>									
Pre									
N	4	-	-	-	-	-	-	-	4
%	100.0	-	-	-	-	-	-	-	100.0
Post									
N	6	-	-	11	-	2	-	-	19
%	31.6	-	-	57.9	-	10.5	-	-	100.0
Diff in %	-	-	-	-	-	-	-	-	-

[X² = NA]

Misconduct Crimes, Misdemeanor:

Pre									
N	39	179	45	28	8	69	188	-	556
%	7.0	32.2	8.1	5.0	1.4	12.4	33.8	-	100.0
Post									
N	29	139	52	34	34	103	192	2	585
%	5.0	23.8	8.9	5.8	5.8	17.6	32.8	0.3	100.0
Diff in %	-2.0	-8.4	+0.8	+0.8	+4.4	+5.2	-1.0	-	-

[X² = 31.73; DF = 7; p = <.001; Cramer's V = .16]

Misconduct Crimes, Violations:

Pre									
N	8	95	20	9	-	33	12	-	177
%	4.5	53.7	11.3	5.1	-	18.6	6.8	-	100.0
Post									
N	9	100	32	14	6	50	18	-	229
%	3.9	43.7	14.0	6.1	2.6	21.8	7.9	-	100.0
Diff in %	-0.6	-10.0	+2.7	+1.0	-	+3.2	+1.1	-	-

[X² = 8.20; DF = 7; p = NS; Cramer's V = .14]

Table 6 continued.../

Table 6, page 6

	<u>Fines</u>	<u>ACD</u>	<u>ACD/CSS</u>	<u>Sentence</u>		<u>Dismissals</u>	<u>Jail</u>	<u>Probation</u>	<u>Total</u>
				<u>CD</u>	<u>CD/CSS</u>				
<u>Obstructing Justice Crimes, Felony:</u>									
Pre									
N	-	-	-	-	-	1	2	-	3
%	-	-	-	-	-	33.3	66.7	-	100.0
Post									
N	-	3	1	2	-	3	-	1	10
%	-	30.0	10.0	20.0	-	30.0	-	10.0	100.0
Diff in %	-	-	-	-	-	-	-	-	-

[X² = NA]

Obstructing Justice Crimes, Misdemeanor:

Pre									
N	24	60	31	14	2	30	28	1	190
%	12.6	31.6	16.3	7.7	1.1	15.8	14.7	0.5	100.0
Post									
N	22	62	29	16	17	57	34	3	240
%	9.2	25.8	12.1	6.7	7.1	23.8	14.2	1.3	100.0
Diff in %	-3.4	-5.8	-4.2	-1.0	+6.0	+8.0	-0.5	+0.8	

[X² = 15.91; DF = 7; p = <.05; Cramer's V = .19]

Table 7

Comparison of Pretest and Posttest on
Fine Amount Ordered (Capped)

	Pretest		Posttest			
	N	%	Day Fines & Flat Fines		N	Day Fines Only*
			N	%		% of Total Posttest
\$1-24	1	.3	1	.3	1	100.0
\$25	33	8.3	4	1.1	3	75.0
\$26-49	-	-	4	1.1	4	100.0
\$50	69	17.3	29	8.0	12	41.4
\$51-74	2	.5	6	1.6	4	66.7
\$75	34	8.5	15	4.1	10	66.7
\$76-99	-	-	9	2.5	8	88.9
\$100	78	19.5	38	10.4	22	57.9
\$101-149	1	.3	15	4.1	14	93.3
\$150	14	3.5	18	4.9	11	61.1
\$151-199	3	.8	11	3.0	10	90.9
\$200	22	5.5	18	4.9	11	61.1
\$201-249	8	2.0	9	2.5	6	66.6
\$250	79	19.8	124	34.0	84	67.7
\$251-499	8	2.0	12	3.3	8	66.7
\$500	22	5.5	22	6.0	12	54.5
\$501-999	4	1.0	7	1.9	6	85.7
\$1000	21	5.3	22	6.0	14	63.6
TOTAL	399	100.0	364	100.0	240	65.9

* This percentage was calculated for each fine amount ordered, by dividing the day-fine N by the total posttest N.

Total Revenue Ordered:	\$82,060.00	\$93,856.00	\$61,994.00
Mean Amount Ordered:	\$205.66	\$257.85	\$258.31

Table 8

Comparison of Pretest and Posttest on
Uncapped Fine Amount Ordered

	<u>Pretest</u>		<u>Day Fines</u>		<u>Posttest</u>	
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>Day Fines Only* % of Total Posttest</u>
\$1-24	1	.3	1	.3	1	100.0
\$25	33	8.3	4	1.1	3	75.0
\$26-49	-	-	4	1.1	4	100.0
\$50	69	17.3	29	8.0	12	41.4
\$51-74	2	.5	6	1.6	4	66.7
\$75	34	8.5	15	4.1	10	66.7
\$76-99	-	-	9	2.5	8	88.9
\$100	78	19.5	38	10.4	22	57.9
\$101-149	1	.3	15	4.1	14	93.3
\$150	14	3.5	18	4.9	11	61.1
\$151-199	3	.8	11	3.0	10	90.9
\$200	22	5.5	18	4.9	11	61.1
\$201-\$249	8	2.0	9	2.5	6	66.6
\$250	79	19.8	56	15.4	16	28.6
\$251-499	8	2.0	37	10.2	33	89.2
\$500	22	5.5	17	4.7	7	41.2
\$501-999	4	1.0	37	10.2	36	97.3
\$1000	21	5.3	16	4.4	8	50.0
\$1001+	-	-	24	6.6	24	100.0
TOTAL	399	100.0	364	100.0	240	65.9

* This percentage was calculated for each fine amount by dividing the day-fine N by the total posttest N.

Total Revenue Ordered:	\$82,060.55	\$137,660.00	\$105,798.00
Mean Amount Ordered:	\$205.66	\$378.19	\$440.83

Table 9

Analysis of Variance of Mean Fine Amount Ordered for Posttest Fines (both day fines and fixed fines) versus Pretest Fines, Arraignment Charge (charge type-severity combination) and the Interaction of Pre/Post and Arraignment Charge

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pre vs. Post	1	475,274.16	475,274.16	11.24	<.001
Arraign. Type-Sev.	14	11,238,860.97	802,762.93	18.98	<.001
Pre/Post * Arrg. T-S	14	573,684.68	38,406.05	.91	NS
Error	725	30,662,542.44	42,293.16	-	-
Total	754	42,914,182.24	-	-	-

Table 10

Analysis of Variance of Mean Fine Amount Ordered for Posttest Day Fines versus Pretest Fines, Arraignment Charge (charge type-severity combination) and the Interaction of Pre/Post and Arraignment Charge

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pre vs. Dayfines	1	381,882.78	381,882.78	8.44	<.01
Arraign. Type-Sev.	14	7,054,140.81	503,867.20	11.14	<.001
Pre/Day * Arrg. T-S	14	1,060,644.98	75,760.36	1.68	NS
Error	601	27,182,410.09	-	-	-
Total	630	35,679,078.67	-	-	-

Table 11

Analysis of Variance of Mean Uncapped Fine Amount for Posttest Fines (both day fines and fixed fines) versus Pretest Fines, Arraignment Charge (charge type-severity combination) and the Interaction of Pre/Post and Arraignment Charge

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pre vs. Post	1	5,483,370.40	5,483,370.40	49.03	<.001
Arraign. Type-Sev.	14	13,607,285.37	971,948.96	8.69	<.001
Pre/Post * Arrg. T-S	14	300,000.82	21,428.63	.19	NS
Error	725	81,070,507.80	111,828.29	-	-
Total	754	100,466,164.40	-	-	-

Table 12

Analysis of Variance of Mean Uncapped Fine Amount for Posttest Day Fines versus Pretest Fines, Arraignment Charge (charge type-severity combination) and the Interaction of Pre/Post and Arraignment Charge

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pre vs. Day Fines	1	8,086,883.74	8,086,883.74	65.87	<.001
Arraign. Type-Sev.	14	10,000,598.76	714,328.48	5.82	<.001
Pre/Day * Arrg. T-S	14	1,286,148.23	91,867.73	.75	NS
Error	601	73,790,286.36	122,779.18	-	-
Total	630	93,163,917.10	-	-	-

Table 13

Table of Means and Standard Deviations of Fine Amount Ordered for Posttest Fines (both day fines and fixed fines) versus Pretest Fines by Arraignment Type-Severity Combination

<u>Arraign. Type - Sev.</u>	<u>Pretest Fine Ordered</u>			<u>Posttest Fine Ordered</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
Crimes vs. Persons, Fel	32	215.16	227.83	37	292.16	270.07	69	256.45	252.53
Crimes vs. Persons, Misd	12	125.00	76.87	30	215.00	114.43	42	189.29	111.98
Weapons Crimes, Fel	7	509.29	464.96	6	500.00	273.86	13	505.00	373.32
Weapons Crimes, Misd	9	183.33	109.69	17	197.36	108.87	26	192.50	107.16
Property Crimes, Fel	24	267.71	250.49	31	277.10	275.21	55	273.00	262.34
Property Crimes, Misd	62	101.37	76.65	74	175.74	155.31	136	141.84	130.69
Drug Crimes, Fel	18	340.28	379.06	14	297.50	150.55	32	321.56	297.93
Drug Crimes, Misd	115	203.04	188.31	59	205.09	129.65	174	203.74	170.31
Drug Crimes, Viol	6	145.83	114.48	2	175.00	106.07	8	153.13	105.59
Forgery/Fraud, Fel	17	527.94	391.80	17	704.35	337.83	34	616.15	371.19
Forgery/Fraud, Misd	14	180.36	247.91	11	127.73	63.42	25	157.20	188.88
Miscond Crimes, Fel	4	800.00	564.21	6	741.67	400.52	10	765.00	442.88
Miscond Crimes, Misd	39	176.92	151.68	29	266.72	269.76	68	215.22	213.22
Miscond Crimes, Viol	8	106.25	91.37	9	238.89	159.64	17	176.47	145.09
Obstruct Just, Misd	24	86.46	59.88	22	196.23	103.24	46	138.96	99.40
TOTAL	391	207.63	238.21	364	257.85	236.54	755	231.84	238.57

Table 14

Table of Means and Standard Deviations of Fine Amount Ordered for
 Posttest Day Fines versus Pretest Fines by
 Arraignment Type-Severity Combination

<u>Arraign. Type - Sev.</u>	<u>Pretest Fine Ordered</u>			<u>Day Fine Ordered</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
Crimes vs. Persons, Fel	32	215.16	227.83	26	311.92	315.86	58	258.53	272.66
Crimes vs. Persons, Misd	12	125.00	76.87	21	216.19	129.20	33	183.03	120.20
Weapons Crimes, Fel	7	509.29	464.96	2	250.00	0	9	451.67	418.58
weapons Crimes, Misd	9	183.33	109.69	12	231.67	100.96	21	210.95	104.97
Property Crimes, Fel	24	267.71	250.49	21	296.19	317.51	45	281.00	280.76
Property Crimes, Misd	62	101.37	76.65	53	188.58	173.70	115	141.57	137.16
Drug Crimes, Fel	18	340.28	379.06	11	331.82	145.38	29	337.07	307.90
Drug Crimes, Misd	115	203.04	188.31	40	216.88	143.28	155	206.61	177.45
Drug Crimes, Viol	6	145.83	114.48	1	25.00	-	7	160.71	111.67
Forgery/Fraud, Fel	17	527.94	391.80	7	757.00	334.31	24	594.75	383.74
Forgery/Fraud, Misd	14	180.36	247.91	8	138.13	56.12	22	165.00	198.82
Miscond Crimes, Fel	4	800.00	564.21	3	483.33	448.14	7	664.29	504.74
Miscond Crimes, Misd	39	176.92	151.68	17	315.00	322.34	56	218.84	224.11
Miscond Crimes, Viol	8	106.25	91.37	3	283.33	202.07	11	154.55	144.40
Obstruct Just, Misd	24	86.46	59.88	15	214.33	107.45	39	135.64	101.96
TOTAL	391	207.63	238.21	240	258.31	234.72	631	226.91	237.98

Table 15

Table of Means and Standard Deviations of Uncapped Fine Amount for Posttest Fines (both day fines and fixed fines) versus Pretest Fines by Arraignment Type-Severity Combination

<u>Arraign. Type - Sev.</u>	<u>Pretest Uncapped Fines</u>			<u>Posttest Uncapped Fines</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
Crimes vs. Persons, Fel	32	215.16	227.83	37	430.18	584.07	69	330.46	464.68
Crimes vs. Persons, Misd	12	125.00	76.87	30	396.06	406.45	42	229.33	361.78
Weapons Crimes, Fel	7	509.29	464.96	6	681.66	332.89	13	588.85	402.82
Weapons Crimes, Misd	9	183.33	109.69	17	461.00	788.15	26	364.89	647.73
Property Crimes, Fel	24	267.71	250.49	31	354.00	479.68	55	316.35	395.50
Property Crimes, Misd	62	101.37	76.65	74	233.15	259.22	136	173.07	208.16
Drug Crimes, Fel	18	340.28	379.06	14	545.64	338.78	32	430.13	371.99
Drug Crimes, Misd	115	203.04	188.31	59	411.14	415.83	174	273.60	301.83
Drug Crimes, Viol	6	145.83	114.47	2	200.00	141.42	8	159.38	113.34
Forgery/Fraud, Fel	17	527.94	391.80	17	726.71	369.69	34	627.32	388.42
Forgery/Fraud, Misd	14	180.36	247.91	11	276.82	537.25	25	222.80	394.90
Miscond Crimes, Fel	4	800.00	564.21	6	1053.33	967.71	10	952.00	802.18
Miscond Crimes, Misd	39	176.92	151.68	29	284.31	275.07	68	222.72	218.02
Miscond Crimes, Viol	8	106.25	91.37	9	269.44	186.15	17	192.65	167.42
Obstruct Just, Misd	24	86.46	59.88	22	265.09	255.00	46	171.89	200.79
TOTAL	391	207.63	238.21	364	378.19	448.00	755	289.87	365.03

Table 16

Table of Means and Standard Deviations of Uncapped Fine Amounts for
 Posttest Day Fines versus Pretest Fines by
 Arraignment Type-Severity Combination

<u>Arraign. Type - Sev.</u>	<u>Pretest Uncapped Fines</u>			<u>Day Fines Uncapped Fines</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
Crimes vs. Persons, Fel	32	215.16	227.83	26	508.35	682.62	58	346.59	504.22
Crimes vs. Persons, Misd	12	125.00	76.87	21	436.29	470.57	33	323.09	404.42
Weapons Crimes, Fel	7	509.29	464.96	2	795.00	572.76	9	572.78	468.00
Weapons Crimes, Misd	9	183.33	109.69	12	605.17	907.62	21	424.38	709.68
Property Crimes, Fel	24	267.71	250.49	21	409.71	568.70	45	339.98	430.05
Property Crimes, Misd	62	101.37	76.65	53	268.74	294.43	115	178.50	222.95
Drug Crimes, Fel	18	340.28	379.06	11	647.64	305.81	29	456.86	379.04
Drug Crimes, Misd	115	203.04	188.31	40	520.80	463.49	155	285.05	316.41
Drug Crimes, Viol	6	145.83	114.47	1	300.00	-	7	167.86	119.65
Forgery/Fraud, Fel	17	527.94	391.80	7	811.29	403.94	24	610.58	408.24
Forgery/Fraud, Misd	14	180.36	247.91	8	343.12	625.92	22	239.55	418.40
Miscond Crimes, Fel	4	800.00	564.21	3	1106.67	1527.30	7	931.43	981.62
Miscond Crimes, Misd	39	176.92	151.68	17	345.00	374.80	56	227.95	229.49
Miscond Crimes, Viol	8	106.25	91.37	3	375.00	238.49	11	179.55	181.60
Obstruct Just, Misd	24	86.46	59.88	15	315.33	292.75	39	174.49	215.57
TOTAL	391	207.63	238.21	240	440.83	513.21	631	296.33	384.55

Table 17

Analysis of Variance of Mean Fine Amount Ordered for Posttest Fines (both day fines and fixed fines) versus Pretest Fines, Total Prior Convictions, and the Interaction of Pre/Post and Total Convictions

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pre vs. Post	1	381,496.37	381,496.37	5.90	<.01
Total Priors	3	313,412.86	104,470.95	1.62	NS
Pre/Post * Tot. Priors	3	248,641.44	82,880.48	1.28	NS
Error	401	25,921,983.86	64,643.35	-	-
Total	408	26,865,534.52	-	-	-

Table 18

Analysis of Variance of Mean Fine Amount Ordered for Posttest Day Fines versus Pretest Fines, Total Prior Convictions, and the Interaction of Pre/Post and Total Convictions

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pre vs. Day Fines	1	320,090.43	320,090.43	4.90	<.05
Total Prior	3	226,345.09	75,448.36	1.15	NS
Pre/Day * Tot. Priors	3	174,624.41	58,208.14	.89	NS
Error	333	21,774,654.59	65,389.35	-	-
Total	340	22,495,714.52	-	-	-

Table 19

Analysis of Variance of Mean Uncapped Fine Amount for Posttest Fines (both day fines and fixed fines) versus Pretest Fines, Total Prior Convictions, and the Interaction of Pre/Post and Total Convictions

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pre vs. Post	1	4,077,598.06	4,077,598.06	30.46	<.001
Total Priors	3	688,614.88	229,538.29	1.71	NS
Pre/Post * Tot. Priors	3	183,372.08	61,124.03	.46	NS
Error	401	53,681,785.03	133,869.79	-	-
Total	408	58,631,370.06	-	-	-

Table 20

Analysis of Variance of Mean Uncapped Amount for Posttest Day Fines versus Pretest Fines, Total Prior Convictions, and the Interaction of Pre/Post and Total Convictions

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pre vs. Day Fines	1	6,116,691.60	6,116,691.60	43.35	<.001
Total Priors	3	675,168.02	225,056.01	1.60	NS
Pre/Day * Tot. Priors	3	438,821.17	145,273.72	1.03	NS
Error	333	46,981,890.01	141,086.76	-	-
Total	340	54,209,570.80	-	-	-

Table 21

Table of Means and Standard Deviations of Fine Amounts Ordered for Posttest Fines
(both day fines & fixed fines) versus Pretest Fines
by Total Prior Convictions

<u>Total Priors</u>	<u>Pretest Fine Ordered</u>			<u>Posttest Fine Ordered</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
0	138	248.62	282.85	130	279.69	236.61	268	263.69	261.43
1	29	201.21	241.75	35	378.69	329.97	64	298.27	304.41
2	17	204.41	227.29	9	308.89	263.79	26	240.58	240.62
3+	32	191.88	147.72	19	203.95	137.62	51	196.37	142.77
TOTAL	216	230.37	257.27	193	291.55	252.63	409	259.24	256.61

Table 22

Table of Means and Standard Deviations of Fine Amounts Ordered for Posttest Day Fines
versus Pretest Fines by Total Prior Convictions

<u>Total Priors</u>	<u>Pretest Fine Ordered</u>			<u>Day Fine Fine Ordered</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
0	138	248.62	282.85	85	286.41	239.61	223	263.03	267.27
1	29	201.21	241.75	22	382.00	349.37	51	279.20	303.60
2	17	204.41	227.29	4	245.00	10.00	21	212.14	203.98
3+	32	191.88	147.72	14	215.36	148.93	46	199.02	146.83
TOTAL	216	230.37	257.27	125	293.95	253.14	341	253.68	257.22

Table 23

Table of Means and Standard Deviations of Uncapped Fine Amounts for Posttest Fines (both day fines and fixed fines) versus Pretest Fines by Total Prior Convictions

<u>Total Priors</u>	<u>Pretest Uncapped Fines</u>			<u>Posttest Uncapped Fines</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
0	138	248.62	282.85	130	435.15	458.48	268	339.10	389.01
1	29	201.21	241.75	35	475.63	546.23	64	351.28	453.83
2	17	204.41	227.28	9	544.89	404.98	26	322.27	335.90
3+	32	191.88	147.72	19	260.26	236.78	51	217.35	186.62
TOTAL	216	230.37	257.27	193	430.38	458.28	409	324.75	379.08

Table 24

Table of Means and Standard Deviations of Uncapped Fine Amounts for Posttest Day Fines versus Pretest Fines by Total Prior Convictions

<u>Total Priors</u>	<u>Pretest Uncapped Fines</u>			<u>Day Fine Uncapped Fines</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
0	138	248.62	282.85	85	524.15	520.78	223	353.65	412.29
1	29	201.21	241.75	22	536.23	646.94	51	345.73	486.41
2	17	204.41	227.29	4	776.00	365.36	21	313.29	338.01
3+	32	191.88	147.72	14	291.79	264.66	46	222.28	193.47
TOTAL	216	230.37	257.27	125	508.31	522.58	341	332.26	399.30

Table 25

Collection Outcomes for Full Day-Fine Enforcement Model
(Day-Fine Experimentals) as Compared with Pretest Fines
Using Seven-Month Subsamples

	<u>Pretest Fine</u>		<u>Post Day-Fine Experimentals</u>	
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>
<u>Paid in Full @ Sentence:</u>				
No	193	83.9	49	92.5
Yes	37	16.1	4	7.6
TOTAL	<u>230</u>	<u>100.0</u>	<u>53</u>	<u>100.0</u>

[$X^2 = 2.54$; DF = 1; p = NS; Cramer's V = -.09]

Ever Paid in Full:

No	55	23.9	8	15.1
Yes	175	76.1	45	84.9
TOTAL	<u>230</u>	<u>100.0</u>	<u>53</u>	<u>100.0</u>

[$X^2 = 1.94$; DF = 1; p = NS; Cramer's V = .08]

Amount Paid as Percent
of Amount Due:

0%	51	22.2	3	5.7
1-99%	4	1.7	5	9.4
100%	175	76.1	45	84.9
TOTAL	<u>230</u>	<u>100.0</u>	<u>53</u>	<u>100.0</u>

[$X^2 = 14.61$; DF = 2; p = <.001; Cramer's V = .23]

Table 26

Collection Outcomes for Day Fines without Enforcement
(Day-Fine Controls) as Compared with Pretest Fines
Using Seven-Month Subsamples

	<u>Pretest Fine</u>		<u>Post Day Fine Only (Controls)</u>	
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>
<u>Paid in Full @ Sentence:</u>				
No	193	83.9	29	82.9
Yes	37	16.1	6	17.2
TOTAL	<u>230</u>	<u>100.0</u>	<u>35</u>	<u>100.0</u>

[$X^2 = .03$; DF = 1; p = NS; Cramer's V = .01]

Ever Paid in Full:

No	55	23.9	10	28.6
Yes	175	76.1	25	71.4
TOTAL	<u>230</u>	<u>100.0</u>	<u>35</u>	<u>100.0</u>

[$X^2 = .36$; DF = 1; p = NS; Cramer's V = -.04]

Amount Paid as Percent
of Amount Due:

0%	51	22.2	9	25.7
1-99%	4	1.7	1	2.9
100%	175	76.1	25	71.4
TOTAL	<u>230</u>	<u>100.0</u>	<u>35</u>	<u>100.0</u>

[$X^2 = .46$; DF = 2; p = NS; Cramer's V = .04]

Table 27

Collection Outcomes for Full Day-Fine Enforcement Model
(Experimentals) as Compared with Day Fines without Enforcement (Controls)
Using Seven-Month Subsamples

	<u>Day-Fine Experimentals</u>		<u>Day-Fine Controls</u>	
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>
<u>Paid in Full @ Sentence:</u>				
No	49	92.5	29	82.9
Yes	4	7.6	6	17.1
TOTAL	<u>53</u>	<u>100.0</u>	<u>35</u>	<u>100.0</u>
[X ² = 1.93; DF = 1; p = NS; Cramer's V = .15]				

<u>Ever Paid in Full:</u>				
No	8	15.1	10	28.6
Yes	45	84.9	25	71.4
TOTAL	<u>53</u>	<u>100.0</u>	<u>35</u>	<u>100.0</u>
[X ² = 2.35; DF = 1; p = NS; Cramer's V = -.16]				

<u>Amount Paid as Percent of Amount Due:</u>				
0%	3	5.7	9	25.7
1-99%	5	9.4	1	2.8
100%	45	84.9	25	71.4
TOTAL	<u>53</u>	<u>100.0</u>	<u>35</u>	<u>100.0</u>
[X ² = 8.04; DF = 2; p = <.01; Cramer's V = .30]				

Table 28

Analysis of Variance on Mean Number of Days to Full Payment for the Three Sample Comparisons, Fine Amount, and the Interaction of Sample and Fine Amount

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pretest vs. Day-Fine Exper:*					
Pre vs. DF Exp.	1	120,480.12	120,480.13	20.40	<.001
Fine Amount	3	73,521.40	24,507.13	4.15	<.01
Pre/DF Exp. * Fine	3	48,300.61	16,100.20	2.73	<.05
Error	210	1,240,315.40	5,906.26	-	-
Total	217	1,482,617.54	-	-	-
Pretest vs. Day-Fine Control:*					
Pre vs. DF Control	1	90,140.68	90,140.68	16.38	<.001
Fine Amount	3	75,046.24	25,015.41	4.55	<.01
Pre/DF Control * Fine	3	99,277.17	49,638.59	9.02	<.001
Error	192				
Total	198				
Day-Fine Exp. vs. Day-Fine Control:*					
DF Exp. vs. DF Control	1	507.93	507.93	.03	NS
Fine Amount	3	132,859.10	44,286.37	2.78	<.05
DF Exp/DF Control * Fine	2	106,188.61	53,094.31	3.33	<.05
Error	62	989,025.60	15,952.03	-	-
Total	68	1,228,581.25	-	-	-

* The seven-month subsets are being used in this analysis.

Table 29

Table of Means and Standard Deviations of Number of Days
to Full Payment for Day-Fine Experimentals
versus Pretest Fines by Fine Amount

<u>Fine Amount:</u>	<u>Pretest Fines</u>			<u>Day-Fine Experimentals</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	59	39.58	52.83	6	48.33	81.74	65	40.83	55.30
\$100 - 249	50	54.82	73.68	9	61.22	74.12	59	55.80	73.14
\$250 - 499	40	67.78	60.55	21	158.52	147.43	61	99.02	107.32
\$500 +	25	72.88	58.80	8	104.62	103.06	33	80.58	71.47
TOTAL	174	55.22	62.81	44	113.80	125.73	218	67.05	82.66

Table 30

Table of Means and Standard Deviations of Number of Days to Full Payment
for Day-Fine Controls versus Pretest Fines by Fine Amount

<u>Fine Amount:</u>	<u>Pretest Fines</u>			<u>Day-Fine Control</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	59	39.58	52.83	-	-	-	59	39.58	52.83
\$100 - 249	50	54.82	73.68	6	45.00	33.09	56	53.77	70.33
\$250 - 499	40	67.78	60.55	11	80.45	106.11	51	70.51	71.69
\$500 +	25	72.88	58.80	8	228.87	201.61	33	110.70	126.86
TOTAL	174	55.22	62.81	25	119.44	151.16	199	63.29	81.68

Table 31

Table of Means and Standard Deviations of Number of Days to Full Payment
for Day-Fine Experimentals versus Day-Fine Controls by Fine Amount

<u>Fine Amount:</u>	<u>Day-Fine Experimentals</u>			<u>Day-Fine Controls</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	6	48.33	81.74	-	-	-	6	48.33	81.74
\$100 - 249	9	61.22	74.12	6	45.00	33.09	15	54.73	59.98
\$250 - 499	21	158.52	147.43	11	80.45	106.11	32	131.69	138.11
\$500 +	8	104.62	103.06	8	228.87	201.61	16	166.75	167.46
TOTAL	44	113.80	125.73	25	119.44	151.16	69	115.84	134.4

Table 32

Analysis of Variance on Mean Number of Appearances at 9 Months Post Sentence
by the Three Sample Comparisons, Fine Amount Ordered, and
the Interaction of Sample and Fine Amount

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pretest vs. Day-Fine Exper:*					
Pre vs. DF Exp.	1	2.39	2.39	1.01	NS
Fine Amount	3	74.52	24.84	10.48	<.001
Pre/DF Exp. * Fine	3	3.05	1.02	.43	NS
Error	393	931.79	2.37	-	-
Total	400	1011.76	-	-	-
Pretest vs. Day-Fine Control:*					
Pre vs. DF Control	1	25.94	25.94	9.63	<.01
Fine Amount	3	62.65	20.88	7.75	<.001
Pre/DF Control * Fine	3	19.28	6.43	2.38	NS
Error	387	1043.00	2.70	-	-
Total	394	1150.87	-	-	-
Day-Fine Exp. vs. Day-Fine Control:*					
DF Exp. vs. DF Control	1	27.03	27.03	6.71	<.01
Fine Amount	3	.98	.33	.08	NS
DF Exp/DF Control * Fine	3	8.50	2.8	.70	NS
Error	126	507.34	4.03	-	-
Total	133	544.34	-	-	-

* These analyses are done on the full year's samples, and excludes those paid in full at sentence.

Table 33

Table of Means and Standard Deviations of Number of Appearances at 9 Months
for Day-Fine Experimentals versus Pretest Fines by Fine Amount

<u>Fine Amount:</u>	<u>Pretest Fines</u>			<u>Day-Fine Experimentals</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	109	1.58	.91	14	1.64	1.08	123	1.59	.92
\$100 - 249	107	1.87	1.37	25	1.64	1.58	132	1.83	1.41
\$250 - 499	76	2.01	1.48	25	1.80	2.16	101	1.96	1.67
\$500 +	39	3.18	2.72	6	2.33	1.51	45	3.07	2.60
TOTAL	331	1.96	1.57	70	1.76	1.71	401	1.93	1.59

Table 34

Table of Means and Standard Deviations of Number of Appearances at 9 Months
for Day-Fine Controls versus Pretest Fines by Fine Amount

<u>Fine Amount:</u>	<u>Pretest Fines</u>			<u>Day-Fine Controls</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	109	1.58	.91	16	2.81	2.01	125	1.74	1.17
\$100 - 249	107	1.87	1.37	22	2.95	2.75	129	2.05	1.72
\$250 - 499	76	2.01	1.48	24	2.37	1.93	100	2.10	1.60
\$500 +	39	3.18	2.72	2	1.50	2.12	41	3.10	2.70
TOTAL	331	1.96	1.57	64	2.66	2.24	395	2.07	1.71

Table 35

Table of Means and Standard Deviations of Number of Appearances at 9 Months
for Day-Fine Experimentals versus Day-Fine Controls by Fine Amount

<u>Fine Amount:</u>	<u>Day-Fine Experimentals</u>			<u>Day-Fine Controls</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	14	1.64	1.08	16	2.81	2.01	30	2.27	1.72
\$100 - 249	25	1.64	1.58	22	2.95	2.75	47	2.26	2.28
\$250 - 499	25	1.80	2.16	24	2.37	1.93	49	2.08	2.05
\$500 +	6	2.33	1.51	2	1.50	2.12	8	2.12	1.55
TOTAL	70	1.76	1.71	64	2.66	2.24	134	2.19	2.02

Table 36

Analysis of Variance on Mean Number of Warrants Ordered at 9 Months Post Sentence
by the Three Sample Comparisons, Fine Amount Ordered, and
the Interaction of Sample and Fine Amount

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pretest vs. Day-Fine Exper.:*					
Pre vs. DF Exp.	1	5.05	5.05	14.34	<.001
Fine Amount	3	1.88	.63	1.78	NS
Pre/DF Exp. * Fine	3	2.83	.94	2.68	<.05
Error	393	138.48	.35	-	-
Total	400	148.25	-	-	-
Pretest vs. Day-Fine Control:*					
Pre vs. DF Control	1	4.06	4.06	9.68	<.01
Fine Amount	3	3.62	1.21	2.87	<.05
Pre/DF Control * Fine	3	.86	.29	.69	NS
Error	387	162.45	.42	-	-
Total	394	171.00	-	-	-
Day-Fine Exp. vs. Day-Fine Control:*					
DF Exp. vs. DF Control	1	10.90	10.90	27.01	<.001
Fine Amount	3	2.89	.96	2.39	NS
DF Exp/DF Control * Fine	3	.93	.31	.77	NS
Error	126	50.67	.40	-	-
Total	133	65.38	-	-	-

* The analyses are done on the full years' samples, and excludes those paid in full at sentence.

Table 37

Table of Means and Standard Deviations of Number of Warrants Ordered at 9 Months for Day-Fine Experimentals versus Pretest Fines by Fine Amount

<u>Fine Amount:</u>	<u>Pretest Fines</u>			<u>Day-Fine Experimentals</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	109	.54	.57	14	.57	.65	123	.54	.58
\$100 - 249	107	.56	.63	25	.16	.37	132	.48	.61
\$250 - 499	76	.45	.55	25	.24	.44	101	.40	.53
\$500 +	39	.77	.81	6	.00	.00	45	.67	.80
TOTAL	331	.55	.62	70	.26	.47	401	.50	.61

Table 38

Table of Means and Standard Deviations of Number of Warrants Ordered at 9 Months for Day-Fine Controls versus Pretest Fines by Fine Amount

<u>Fine Amount:</u>	<u>Pretest Fines</u>			<u>Day-Fine Controls</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	109	.54	.57	16	1.00	.73	125	.60	.61
\$100 - 249	107	.56	.63	22	.77	.75	129	.60	.66
\$250 - 499	76	.45	.55	24	.71	.75	100	.51	.61
\$500 +	39	.77	.81	2	1.50	2.12	41	.80	.87
TOTAL	331	.55	.62	64	.83	.79	395	.60	.66

Table 39

Table of Means and Standard Deviations of Number of Warrants Ordered at 9 Months
for Day-Fine Experimentals versus Day-Fine Controls by Fine Amount

<u>Fine Amount:</u>	<u>Day-Fine Experimentals</u>			<u>Day-Fine Controls</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	14	.57	.65	16	1.00	.73	30	.80	.71
\$100 - 249	25	.16	.37	22	.77	.75	47	.45	.65
\$250 - 499	25	.24	.44	24	.71	.75	49	.47	.65
\$500 +	6	.00	.00	2	1.50	2.12	8	.38	1.06
TOTAL	70	.26	.47	64	.83	.79	134	.53	.70

Table 40

Analysis of Variance on Mean of Full Pay at 9 Months Post Sentence by the Three Sample Comparisons, Fine Amount Ordered, and the Interaction of Sample and Fine Amount

<u>Source</u>	<u>DF</u>	<u>Sum of Squares</u>	<u>Mean Squares</u>	<u>F</u>	<u>P</u>
Pretest vs. Day-Fine Exper.:*					
Pre vs. DF Exp.	1	7.73	7.73	36.99	<.001
Fine Amount	3	.81	.27	1.29	NS
Pre/DF Exp. * Fine	3	.34	.11	.55	NS
Error	460	96.10	.21	-	-
Total	467	104.98	-	-	-
Pretest vs. Day-Fine Control:*					
Pre vs. DF Control	1	3.17	3.17	15.12	<.001
Fine Amount	3	.97	.32	1.54	NS
Pre/DF Control * Fine	3	.85	.28	1.35	NS
Error	454	95.24	.21	-	-
Total	461	100.23	-	-	-
Day-Fine Exp. vs. Day-Fine Control:*					
DF Exp. vs. DF Control	1	.50	.50	2.03	NS
Fine Amount	3	.13	.04	.18	NS
DF Exp/DF Control * Fine	3	1.02	.34	1.37	NS
Error	138	34.02	.25	-	-
Total	145	35.68	-	-	-

* These analyses are done on the full years' samples, and includes those paid in full at sentence.

Table 41

Table of Means and Standard Deviations of Number of Full-Payers at 9 Months Post Sentence
for Day-Fine Experimentals versus Pretest Fines by Fine Amount

<u>Fine Amount:</u>	<u>Pretest Fines</u>			<u>Day-Fine Experimentals</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	137	.68	.47	15	.27	.46	152	.64	.48
\$100 - 249	126	.70	.46	29	.41	.50	155	.65	.48
\$250 - 499	87	.80	.40	25	.36	.49	112	.71	.46
\$500 +	42	.71	.46	7	.43	.53	49	.67	.47
TOTAL	392	.72	.45	76	.37	.49	468	.66	.47

Table 42

Table of Means and Standard Deviations of Number of Full-Payers at 9 Months Post Sentence
for Day-Fine Controls versus Pretest Fines by Fine Amount

<u>Fine Amount:</u>	<u>Pretest Fines</u>			<u>Day-Fine Controls</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	137	.68	.47	17	.59	.51	154	.67	.47
\$100 - 249	126	.70	.46	25	.40	.50	151	.65	.48
\$250 - 499	87	.80	.40	26	.54	.51	113	.74	.44
\$500 +	42	.71	.46	2	.00	.00	44	.68	.47
TOTAL	392	.72	.45	70	.49	.50	462	.68	.47

Table 43

Table of Means and Standard Deviations of Number of Full-Payers at 9 Months Post Sentence
for Day-Fine Experimentals versus Day-Fine Controls by Fine Amount

<u>Fine Amount:</u>	<u>Day-Fine Experimentals</u>			<u>Day-Fine Controls</u>			<u>Total</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
\$1 - 99	15	.27	.46	17	.59	.51	32	.44	.50
\$100 - 249	29	.41	.50	25	.40	.50	54	.41	.50
\$250 - 499	25	.36	.49	26	.54	.51	51	.45	.50
\$500 +	7	.43	.53	2	.00	.00	9	.33	.50
TOTAL	76	.37	.49	70	.49	.50	146	.43	.50

Table 44

Regression Analysis on Fine Amount

	<u>Capped Fine Amount</u>			<u>Uncapped Fine Amount</u>		
	<u>b</u>	<u>p</u>	<u>beta</u>	<u>b</u>	<u>p</u>	<u>beta</u>
Intercept	33,531	-	-	10,100	-	-
Arrestment Charge Severity	11,770	<.001	.23	11,142	<.001	.15
Sample	5,538	<.05	.11	25,345	<.001	.33
Income	19	<.001	.20	35	<.001	.25

N = 328
R2 = .1134
Adj R2 = .1052
F = 13.8; p < .001

N = 328
R2 = .2212
Adj R2 = .2139
F = 30.1; p < .001

Table 45

Table of Means and Standard Deviations on Fine Amount
for Independent Variables

	<u>Capped Fine Amount</u>			<u>Uncapped Fine Amount</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
<u>Arrestment</u>						
<u>Charge Severity:</u>						
Felony	119	333	325	119	412	434
Misdemeanor	209	206	183	209	275	311
<u>Income:</u>						
\$0	124	235	213	124	267	259
\$1-249	60	195	180	60	246	238
\$250-449	88	302	297	88	423	468
\$500	26	418	379	26	595	568
<u>Sample:</u>						
Pretest	214	226	246	214	266	246
Day Fine	114	301	253	114	510	470

Table 46

Regression Analysis on Uncapped Fine Amount with
Sample/Income Interaction Effect

	<u>Uncapped Fine Amount</u>		
	<u>b</u>	<u>p</u>	<u>beta</u>
Intercept	19,516	-	-
Arrestment Charge Severity	10,776	<.01	.14
Sample	17,679	<.001	.23
Income	-18	ns	-.12
Sample/Income Interaction	38	<.01	.42

N = 328
R2 = .238
Adj R2 = .2285
F = 25.2; p < .001

Table 47

Table of Means and Standard Deviations of Uncapped Amount
for Posttest Day Fines versus Pretest Fines by Income

<u>Income:</u>	<u>Pretest Uncapped Amount</u>			<u>Day Fines Uncapped Amount</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
0	89	206.01	184.03	35	423.11	346.08
\$1 - 249	39	170.51	177.32	21	387.29	273.38
\$250 - 449	47	272.34	308.23	41	615.54	550.40
\$500 +	16	467.19	423.94	10	800.00	722.59

Table 48

Table of Means and Standard Deviations of Uncapped Amount for
Posttest Day Fines versus Pretest Fines by Arraignment Severity

<u>Arraignment Severity:</u>	<u>Pretest Uncapped Amount</u>			<u>Day Fine Uncapped Amount</u>		
	<u>N</u>	<u>Mean</u>	<u>SD</u>	<u>N</u>	<u>Mean</u>	<u>SD</u>
Felony	73	304.80	334.25	46	583.02	517.16
Misdemeanor	141	185.53	173.18	68	459.81	432.30

Table 49

Constructed Variables for Possible Inclusion
in Modeling of Sentencing Outcome

<u>Criminal History Variables</u>	<u>Pretest N</u>	<u>Posttest N</u>
Age at first arrest	516	545
Length of arrest record(in days)	516	546
# of prior jailings	520	548
# of prior imprisonments	520	548
# of prior probations	520	548
Probability of receiving a jail arrest	480	512
Probability of receiving a jail conviction	427	373
Probability of receiving a prison arrest	480	512
Probability of receiving a prison conviction	427	373
Probability of receiving a probation arrest	480	512
Probability of receiving a probation conviction	427	373
Total # of arrests	520	548
Total # of convictions	520	548
# of prior arrests for misdemeanors	520	548
# of prior arrests for felonies	520	548
# of prior arrests for violent felonies	520	548
# of prior convictions for misdemeanors	520	548
# of prior convictions for felonies	520	548
# of prior convictions for violent felonies	520	548
# of recent arrests for misdemeanors	520	548

Table 49 continued.../

Table 49 continued:

<u>Criminal History Variables</u>	<u>Pretest N</u>	<u>Posttest N</u>
# of recent arrests for felonies	520	548
# of recent arrests for violent felonies	520	548
# of recent convictions for misdemeanors	520	548
# of recent convictions for felonies	520	548
# of recent convictions for violent felonies	520	548
Seriousness of record	480	512
Imprisoned for most recent conviction	324	292
# of days between present arraignment and most recent disposition date	392	397
Rate of yearly arrest after 16 birthday	487	515
 <u>CJA-OCA Variables</u>	 <u>Pretest N</u>	 <u>Posttest N</u>
White	881	876
Sex	745	784
Arraignment severity	850	855
Arraignment type	850	855
Conviction severity	893	886
Conviction type	893	884
Bail at arraignment	637	675
Detention at arraignment	794	793
Charge reduction	849	853
# of open cases		

Table 50

Multinomial Logistic Regression Analysis
on Day Fines Pretest Sampled Cases

INDIVIDUAL LOGIT COEFFICIENTS:

<u>Effect</u>	<u>Estimate</u>	<u>Standard Error</u>	<u>Chi- Square</u>	<u>Prob</u>
INTERCEPT	1.0996	0.7911	1.93	0.1645
	1.7800	0.8773	4.12	0.0425
	1.6176	0.7480	4.68	0.0306
	3.1805	0.7395	18.50	0.0000
	4.6060	0.7764	35.20	0.0000
WHITE	1.0706	0.3423	9.78	0.0018
	1.1304	0.3477	10.57	0.0011
	0.7068	0.3362	4.42	0.0355
	0.8739	0.3377	6.70	0.0097
	-0.1091	0.3304	0.11	0.7412
CHARGE REDUCTION	-0.4771	0.4684	1.04	0.3083
	-2.0947	0.5855	12.80	0.0003
	-0.0113	0.4282	0.00	0.9790
	0.8997	0.4321	4.33	0.0373
	-1.4356	0.5226	7.55	0.0060
CONVICTION SEVERITY	-0.00563	0.1141	0.00	0.9607
	0.2859	0.1210	5.58	0.0181
	-0.0130	0.1076	0.01	0.9035
	-0.5258	0.1076	23.86	0.0000
	-0.2480	0.1011	6.01	0.0142
ARRAIGNMENT TYPE	0.1279	0.0720	3.15	0.0757
	-0.0456	0.0727	0.39	0.5308
	-0.1086	0.0703	2.39	0.1225
	-0.1191	0.0754	2.49	0.1145
	-0.0800	0.0688	1.35	0.2448
DETENTION	-4.1694	0.5155	65.41	0.0000
	-3.8332	0.4841	62.69	0.0000
	-2.6133	0.3401	59.03	0.0000
	-2.5965	0.3477	55.77	0.0000
	-1.9227	0.3046	39.85	0.0000

Table 50 continued.../

Table 50 continued:

CHI-SQUARE TEST FOR INDEPENDENT VARIABLES:

<u>Variable</u>	<u>DF</u>	<u>Chi-Square</u>	<u>Prob</u>
INTERCEPT	5	48.25	0.0000
WHITE2	5	30.31	0.0000
CHARCHNG	5	44.51	0.0000
CONVTSEV	5	67.68	0.0000
ARRGTYP	5	22.79	0.0004
DETAINS	5	127.50	0.0000

GOODNESS OF FIT FOR OVERALL MODEL:

LIKELIHOOD RATIO	610	616.37	0.4205
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Table 51

Sentence Category and Sample Size for
Pretest Cases Included in the Model

<u>Sentence Category</u>	<u>N</u>	<u>Percent</u>
Imprisonment	100	13.5
Dismissal	137	18.5
Probation	125	16.8
Cond. Discharge	124	16.7
ACD	121	16.3
Fine	135	18.2

Table 52

Sentence Category and Sample Size for
Posttest Cases Included in the Model

<u>Sentence Category</u>	<u>N</u>	<u>Percent</u>
Imprisonment	99	13.1
Dismissal	140	18.5
Probation	125	16.5
Cond. Discharge	127	16.8
ACD	129	17.1
Fine	136	18.0