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DID THE CORPORATE CRIMINAL SENTENCING GUIDELINES MATTER? SOME PRELIMINARY EMPIRICAL OBSERVATIONS*

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ABSTRACT

This paper presents an empirical analysis of the impact of the federal Sentencing Commission’s 1991 guidelines for imposing criminal sentences on corporations convicted of federal crimes. Despite the Sentencing Commission’s announced intentions of raising and restructuring corporate fines, we generally find no statistically significant change in the level or structure of corporate monetary penalties imposed under the guidelines during 1992–95, as compared with baseline data taken from preguidelines cases sentenced in 1988, after controlling for the harm attributed to the criminal offense. In an extension of that analysis, we find a marginally significant change in the relationship between corporate penalty levels and the presence of individual codefendants charged together with the corporation in the direction of attenuating that relationship in the postguidelines era. We discuss the implications of these findings from the perspective of the limited role played by corporate criminal sentencing determinations in the overall public law enforcement effort.

I. INTRODUCTION

In late 1991, the United States Sentencing Commission promulgated the first general sentencing guidelines (USSG) for the determination of criminal sentences to be imposed on corporations convicted of crimes in the federal courts.¹ According to the commission’s statements at the time, these new

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¹ United States Sentencing Commission, Sentencing of Organizations, as Added by Amendment 422, Effective November 1, 1991, ch. 8, in Guidelines Manual (November 1997). This 1991 promulgation established the first “general” corporate sentencing guidelines, though an earlier 1987 guideline covered organizational fines for antitrust offenses; see USSG § 2R1.1 (effective November 1, 1987).

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guidelines were intended to make a substantial break from past practice in terms of both the level and structure of corporate criminal penalties in the federal courts. In this paper, we report our empirical analysis of the guidelines' impact in practice, generally finding no significant effect on the levels or structure of corporate monetary sanctions actually imposed, after controlling for the harm attributed to the offense.

While this negative finding obviously is not definitive and may be attributable to the poor quality of the data released by the Sentencing Commission for empirical analysis, it is also consistent with the view that corporate criminal sentencing guidelines neither are nor should be a critical factor in federal law enforcement policy, given the inherently limited role that corporate criminal sentencing determinations play in the overall enforcement effort. From this perspective, both judges and policy makers may prefer a nonbinding guideline structure that permits judges to follow the same objective function as the preguideline system, while still satisfying the political market's demand for an enhanced rhetoric of condemnation and allowing more inputs from lawyers representing both prosecution and defense.

II. THE LIMITED ROLE OF CORPORATE CRIMINAL SENTENCING

One of the continuing debates about criminal punishment concerns the extent to which the precise determination of penalties within the criminal sentencing process effectively serves any utilitarian goal of public law enforcement or is merely political theater. Even if we grant the point that some criminal sanction is more useful than none, there remain the questions of whether and when it is worthwhile at the margin to devote resources to refinements in the formal criminal penalty determination system, except perhaps as required to preserve marginal deterrence.

2 Together with its 1991 guidelines, the commission released a document entitled Supplementary Report on Sentencing Guidelines for Organizations (August 30, 1991) (hereinafter cited as Commission Report 1991), in which the commission stated that past practice was "not particularly meaningful" in setting guideline fine levels (id. at 11), predicted that "fines, on average, are likely to be higher under the promulgated guidelines" (id. at 22), and estimated that median "minimum" guideline fines would be approximately double the median fine under past practice, at least for certain categories of offenses (id. at 22–23).

3 This is one implication of Becker's pathbreaking analysis, in which socially optimal penalties may be maximal in amount, and minimal in probability, in order to optimize enforcement costs. Gary S. Becker, Crime and Punishment: An Economic Approach, 76 J. Pol. Econ. 169 (1968). Of course, this result is subject to a number of qualifications introduced by considering marginal deterrence within the penalty framework, as well as the costs of risk aversion, error, and offenders' wealth constraints. See, among others, Michael K. Block & Joseph G. Sidak, The Cost of Antitrust Deterrence: Why Not Hang a Price Fixer Now and Then? 68 Geo. L. J. 1131 (1980); Richard Craswell & John E. Calfee, Deterrence and Uncertain Legal Standards, 2 J. L. Econ. & Org. 279 (1986); Bruce H. Kobayashi & John R. Lott, Jr., Low-Probability–High-Penalty Enforcement Strategies and the Efficient Operation of the
In legal tradition and literature there is a substantial body of practice and opinion suggesting limited utilitarian benefits from refinements in the criminal penalty determination system. The early English common law took this practice to an extreme, establishing essentially a bimodal punishment system, where the choice was between felony, generally punishable by death (both physical and, through forfeiture of property, economic), and misdemeanor, generally punishable by relatively minor punishments (whipping, and so forth), followed by release.\(^4\) For most of this century, criminal sentencing in the United States was oriented away from precise penalty determination and toward "treatment" or "rehabilitation" of the offender under an "indeterminate" sentencing system.\(^5\) In recent years, more emphasis has been placed on maximal incarceration sentences as a means to "incapacitate" repeat offenders.\(^6\) Although utilitarian in some dimension, either rehabilitation or incapacitation would appear to conflict with the penalty structure implied by the utilitarian objective of deterrence, as resting on certain punishments proportioned to the harm of the offense and the probability of punishment. Some criminal sentencing theorists go further, to suggest that the determination of particular sentences should not be utilitarian at all but, rather, formed on the basis of moral "desert" alone.\(^7\)

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\(^4\) Blackstone's Commentaries *94--*98. While a more conventional explanation of this structure is the high cost of imprisonment in a feudal society, this too is another manifestation of the same general point, for the social costs of imprisonment are part of society's transaction costs of implementing a penalty system, as are the costs of refinements to the gradations of penalty, to be balanced against the benefits of measured deterrence. Even Bentham's seminal utilitarian critic of the common law, admitted this concept into his otherwise rigorous insistence on proportionality in punishments. Jeremy Bentham, An Introduction to the Principles of Morals and Legislation 185 (Hafner ed. 1948)(1789).

\(^5\) Dissatisfaction with this system, which emphasized broad discretion of correctional and parole authorities as to the conditions and length of confinement sentences, led to a wave of sentencing reform legislation at both the state and federal levels, including the federal Sentencing Reform Act of 1984, Pub. L. No. 98-473, tit. II, ch. 2, 98 Stat. 1837, 1976, 1987-2040 (1984) (the Act), which was the legislation that created the U.S. Sentencing Commission to promulgate sentencing guidelines for the federal courts.

\(^6\) From "selective incapacitation" in the 1970s and 1980s, this same impulse has developed more recently into the "three strikes and you're out" form of "career offender" statute that has been so popular of late with both federal and state legislators.

\(^7\) This resurgence of interest in a "retributive" and ostensibly nonutilitarian system of punishment arose in the 1970s, in conjunction with dissatisfaction with the preexisting "rehabilitative" system. See Andrew von Hirsch, Doing Justice: The Choice of Punishments (1976); and Andrew von Hirsch, Past or Future Crimes: Deservedness and Dangerousness in
Furthermore, though deterrence-based penalty determination may be preferable in a theory that assumes away adjudication costs, informational and other transaction costs may overwhelm any practical benefit of such a policy. Thus, there is some question whether the institutions of criminal punishment are even capable of refinement to the point of imposing precisely measured penalties to “fit the crime,” both because of inherent limitations in the criminal justice system itself and because that system does not operate in a vacuum. Even where criminal punishment is the only or predominant form of sanction available—as in many “street crimes”—questions have been raised about the efficacy of the criminal sentencing system in achieving predictable results in any utilitarian dimension, whether deterrence, rehabilitation, or incapacitation. When the focus is shifted to “white-collar” offenses, these doubts become more substantial, because criminal penalties are no longer the only nor perhaps even the primary mode of sanction. In the case of the white-collar offender with substantial assets and reputation, the principal sanction may be the conviction itself, with its collateral consequences of reputational loss through “stigma” as well as exposure to noncriminal legal sanctions such as debarment, civil

the Sentencing of Criminals (1985). Following the logic of H. L. A. Hart, which distinguished the “general justifying aim” of punishment from the principles of its “distribution” in individual cases (see H. L. A. Hart, Punishment and Responsibility (1968)), these neoretributivists argued that although the existence of criminal punishment may be justified by reference to utilitarian aims, the determinations of sentences in individual cases should be made by reference to the offenders’ moral “desert.”

8 See Parker, supra note 3.

9 Among other things, the criminal adjudication system is arranged in such a way as to minimize the costs of erroneous convictions, even at the expense of tolerating a relatively high rate of erroneous acquittals. More directly to the current point, the criminal justice system does not appear to be particularly well suited to such determinations as assessing the social harm of a criminal offense, even as compared with other legal institutions, such as civil litigation. These institutional arrangements themselves imply the role of criminal punishment as society’s sanction of last resort. See Jeffrey S. Parker, The Blunt Instrument, in Debating Corporate Crime, at 71–97 (William S. Lofquist, Mark A. Cohen, & Gary A. Rabe eds. 1997).


or administrative penalties, and compensatory and punitive civil damages. Furthermore, the nature and extent of these collateral consequences are largely beyond the control of the criminal justice system.\textsuperscript{12} In this context, the formal criminal sentence itself—as opposed to the conviction and its collateral consequences—may be supplemental only.

These effects become even more pronounced if we now narrow the focus further to criminal sanctions against corporate entities. In this context, the case for any criminal liability at all is highly attenuated.\textsuperscript{13} Unlike individuals, corporations have neither "liberty" nor "wealth" to protect as such. They do, however, often have substantial assets and reputation that can be affected by the collateral consequences of conviction, which appear to be even larger and more robust than in the case of individual white-collar offenders\textsuperscript{14} and are also outside the direct control of the criminal justice system.\textsuperscript{15} Even if we assume that there is a case for entity-level liability, it does not follow that there is a case for any substantial criminal penalty,\textsuperscript{16} and far less that the system for determining such penalties requires a high degree of refinement or regulatory control.\textsuperscript{17} The formal criminal penalty against the corporate entity appropriately—and efficiently—may be relegated to a supplemental if not trivial role in the overall law enforcement system.

If it is true that formal criminal penalties against corporate entities have a minimal role to play in an effective law enforcement system, then sensible public policy toward corporate criminal sentencing presumably would en-

\textsuperscript{12} Even nominally equivalent criminal penalties can have a varying effect, depending on the defendant's wealth, income, and reputation. See John R. Lott, Jr., Should the Wealthy Be Able to "Buy Justice"? 95 J. Pol. Econ. 1307 (1987); and John R. Lott, Jr., Do We Punish High-Income Criminals Too Heavily? 31 Econ. Inquiry 583 (1992).

\textsuperscript{13} See Jeffrey S. Parker, Doctrine for Destruction: The Case of Corporate Criminal Liability, 17 Managerial & Dec. Econ. 381 (1996).

\textsuperscript{14} Jonathan Karpoff & John R. Lott, Jr., The Reputational Penalty Firms Bear for Committing Fraud, 36 J. Law & Econ. 757 (1993).

\textsuperscript{15} Jeffrey S. Parker, Criminal Sentencing Policy for Organizations: The Unifying Approach of Optimal Penalties, 26 Am. Crim. L. Rev. 513, 529–33 (1989). Research by Mark Cohen found in preguidelines practice that the existence of collateral sanctions is empirically significant to the determination of the corporate criminal fine, in terms of both collateral civil or administrative sanctions against the firm and joint penalties against codefendant individuals, thus implying that sentencing judges implicitly coordinated the criminal sentence against the firm with collateral sanctions, some of which were determined outside the criminal justice system. See Mark A. Cohen, Theories of Punishment and Empirical Trends in Corporate Criminal Sanctions, 17 Managerial & Dec. Econ. 399 (1996).

\textsuperscript{16} John R. Lott, Jr., The Level of Optimal Fines to Prevent Fraud When Reputations Exist and Penalty Clauses Are Unenforceable, 17 Managerial & Dec. Econ. 363 (1996).

\textsuperscript{17} Michael K. Block, Optimal Penalties, Criminal Law, and the Control of Corporate Behavior, 71 B.U. L. Rev. 395 (1991).
courage (or at least permit) that result. Are the observed public policies consistent with this hypothesis?

In this respect, we encounter the problem of divergence between political rhetoric and practical effects. Although we have very little knowledge of historical charging and sentencing practices, such evidence as exists suggests that criminal penalties against corporations traditionally were both rare in incidence and modest in magnitude. This observation would be consistent with a judicial recognition that the precise determination of formal criminal penalties against corporate entities was tangential to the overall enforcement effort, as a practical matter. That condition is to be distinguished from a separate demand in the political markets for a rhetoric of condemnation.

Prior to the mid-1980s, the judiciary was relatively unconstrained by legislation in determining corporate criminal penalties. However, at that time, corporate criminal penalties were swept up in the more general movement toward criminal sentencing reform measures designed to shift toward a "determinate" sentencing system of fixed sentences without parole, determined under guidelines binding on the sentencing judges. At the federal level, that movement led to the Sentencing Reform Act of 1984, which abolished parole and created the United States Sentencing Commission to establish a determinate guidelines-based sentencing system for the federal criminal courts, which was implemented for individual defendants in late 1987. Corporate criminal penalties were no more than an afterthought to

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18 Early studies of "corporate crime" by sociologists such as Edwin Sutherland and Marshall Cloward actually reinforce the marginal role of criminal enforcement, as the vast majority of cases studied (84 percent in Sutherland's famous study) involved civil matters, not criminal enforcement. Marshall Cloward, Illegal Corporate Behavior (1979); Edwin Sutherland, White Collar Crime (1949). The first comprehensive studies focused on criminal sanctions against corporations were performed in the late 1980s and were an outgrowth of the U.S. Sentencing Commission's development of corporate sentencing guidelines. See Mark A. Cohen et al., Organizations as Defendants in Federal Court: A Preliminary Analysis of Prosecutions, Convictions, and Sanctions, 1984–1987, 10 Whittier L. Rev. 103 (1988); Mark A. Cohen, Corporate Crime and Punishment: A Study of Social Harm and Sentencing Practice in the Federal Courts, 1984–1987, 26 Am. Crim. L. Rev. 605 (1989); and Mark A. Cohen, Corporate Crime and Punishment: An Update on Sentencing Practice in the Federal Courts, 1988–1990, 71 B.U. L. Rev. 247 (1991); Cohen, supra note 15; Parker, supra note 15. The first of these studies, examining 1938–87 data, showed that the mean corporate fine was approximately $57,000 (Parker, supra note 15, at 528). In a subset of cases for which more data were available and the fine did not appear to be bounded by inability to pay, the mean corporate fine was approximately equal to the loss caused by the offense (Cohen et al., supra at 122, table 9). Median fines were much lower—on average, only 20 percent of the loss caused by the offense (id.).


that reform,\textsuperscript{21} and the commission did not adopt general corporate sentencing guidelines until late 1991.\textsuperscript{22}

The commission's 1991 promulgation was accompanied by some "get tough" rhetoric that was reflected in tough-looking guidelines that the commission advertised as intended to raise corporate penalty levels from past practice.\textsuperscript{23} To some extent, this was merely a continuation of a 50-year effort from various quarters to increase public sensitivities to white-collar crime, generally, and "crime in the suites," particularly.\textsuperscript{24} The main question we examine in this paper is whether the resulting guidelines actually did change the level or structure of corporate penalties.

Certainly, the corporate guidelines on their face purported to both raise the level and radically restructure the determination of corporate criminal penalties in the federal courts. But the commission may well have been responding to a demand for political rhetoric rather than practical change in law enforcement. If the commission recognized that efficient law enforcement favored the preguidelines system but that political markets demanded "tough" rhetoric, then both criteria could be satisfied by tough-sounding guidelines that in operative effect were so permeable that they did not place binding constraints on sentencing judges.\textsuperscript{25}

\textsuperscript{21} The legislative history contains some indication that the Congress was aware of the marginal impact of corporate criminal penalties, including statements suggesting that the Sentencing Commission was not necessarily required to issue binding sentencing guidelines for corporate penalties and might consider issuing nonbinding policy statements instead (Senate Sentencing Report, supra note 19, at 166). The Congress obviously was concerned that some of the new sentencing options provided under the Act—such as the corporate probation sentence—could get out of hand (id. at 68–69, 96–97, 99).

\textsuperscript{22} However, as noted above (note 1 supra), the commission did adopt one specific corporate guideline with its initial promulgation in 1987, covering corporate fines imposed in criminal antitrust cases.


\textsuperscript{24} This movement began with the coining of the phrase "white collar crime" by Sutherland in a 1939 speech (Edwin Sutherland, The White Collar Criminal, 5 Am. Soc. Rev. 1 (1940)) and its popularization with the publication of Sutherland’s book (supra note 18), which primarily focused on corporate "crimes" that actually were not crimes—a usage that continues within sociology and politics to this day. See Parker, supra notes 9, 13.

\textsuperscript{25} In an essay written in early 1993 (before any substantial experience with the new guidelines) Parker criticized the explicit provisions of the guidelines but also offered an alternative hypothesis—based on the relative quiescence of the business community prior to their adoption—of their implicit purpose and effect: "There always has been an important component of political theater to crime and punishment: the morality play of evil receiving its just desert seems to be popular, so long as the price of admission is not too high. When the time comes actually to impose sentences in particular cases, the saving grace of the corporate Sentencing Guidelines may well be their permeability. The Guidelines are a lawyer’s dream of ambiguous phases, complex provisos, and diffuse factual determinations that will defy effective appellate review. Many of the key provisions are deliberately open-ended, thereby allowing sentencing courts considerable discretion in defining the operative rules of law. The incidence of federal prosecution against corporations is so low that, without a major increase, it will
From the judiciary’s point of view, in the absence of a binding constraint in the new guidelines, there is no reason to expect a systematic change in corporate penalty levels or structure. Under the assumption that the guidelines were nonbinding in practice, judges’ incentives would be unchanged. Unlike sentencing commissioners, judges in general would appear to receive no political payoff from a systematic change in corporate criminal penalty levels or distribution. If anything, a unforced change by sentencing courts could impose an additional “tax” on the judiciary through its effect on the supply of corporate criminal prosecutions brought by prosecutors, who presumably would respond to any increase in penalty levels by devoting relatively more of their resources to corporate prosecutions.

On the basis of these considerations, our empirical analysis tested the null hypothesis that the new guidelines, despite their rhetoric, effected no systematic change in the level or structure of corporate criminal penalties. In general, our empirical results failed to reject that hypothesis.

We emphasize that our results are limited in terms of both data and analysis. The quality of data that have managed to filter up through the commission—especially in the postguidelines era—is very poor. If the data systematically exclude an important class of cases, then our results obviously could change, though we ran several empirical tests of hypotheses that the data were truncated or adversely selected, all with no significant effect on our results.26

Furthermore, our analysis looks only to the penalty function of fine levels and structure, given by the relationship of corporate fines to rough estimates of harm that we derived by using amounts of restitution paid and ordered. Therefore, we do not exclude the possibility that the guidelines induced underlying changes in charging practices or corporate compliance behavior.

III. DATA SOURCES AND METHODS

All of the data used in our analyses were drawn from data sets that have been released from the Sentencing Commission through the Inter-university...
Consortium for Political and Social Research (ICPSR) under the title "Organizations Convicted in Federal Criminal Courts" and were downloaded from the ICPSR web site.\(^{27}\)

Our preguidelines baseline was provided by a full-year data set for 1988, consisting of 328 sentencings.\(^{28}\) This base year of 1988 was the first full year of data following statutory changes in authorized fine levels in 1984 and 1987.\(^{29}\) The postguidelines data included all corporate guidelines sentencing data available for the years 1987–95 (the most recent year available), which was a total of 324 cases, though the pre-1992 guidelines cases were confined to antitrust offenses only. Thus, the combined data set included a total of 652 observations.

In order to focus our analysis on the comparison of a guidelines versus a nonguidelines regime, both antitrust and environmental cases were excluded from our analysis.\(^{30}\) These exclusions reduced the total size of the data set to 384 cases.

The overall quality of data released by the commission is poor and reflects excisions of many variables of interest to the researcher, including case docket numbers, reliable means of identifying codefendants, identifications (even anonymous ones) of defendants and sentencing judges, and the like. In addition, the postguidelines data exclude some of the fields included in preguidelines data, making comparative analysis difficult.

Of particular interest to our study, the postguidelines data omitted any field for the sentencing court's estimate of loss or harm caused by the offense, even though the corporate guidelines call for such a determination as one factor in determining the guideline fine.\(^{31}\) We addressed this problem

\(^{27}\) The web site address is icpsr.umich.edu, and the file is ICPSR #9513.

\(^{28}\) The commission also had a partial set of data on preguidelines sentencings in 1989–90 (Commission Report 1991, supra note 2, at 17), which we did not consider in our analysis, primarily because of the absence of a coding book explaining the data fields involved, which differ from the 1988 data.

\(^{29}\) The major change in statutory maximum fines was made in 1984 and revised in 1987 (see Parker, supra note 15, at 547–48). The commission referred to these changes as a basis for a largely ignored earlier data set of corporate sentencings from 1984–87 (Commission Report 1991, supra note 2, at 11), though the effects of changes in statutory maxima could have been estimated and had been modeled by Cohen, Corporate Crime and Punishment: An Update, supra note 18. The 1984–87 data set apparently was never released by the commission. The preexistence of changes in legislatively authorized maximum fines provides one reason why we would expect to find no marginal change from the guidelines. The legislation may have removed an upper-bound constraint, which guidelines could not do.

\(^{30}\) As noted above, the antitrust fine guideline for organizations dates from the initial sentencing guidelines of 1987, and that guideline essentially was carried forward by the 1991 amendments. Similarly, the 1991 guidelines did not specify fines for environmental offenses, which continue to be governed by a preguidelines regime; see USSG § 8C2.1.

\(^{31}\) See USSG § 8C2.4(a)(3).
by using a proxy for harm (HARM) consisting of the sum of fields recording amounts of restitution paid and ordered, which were common to both groups of data. To test that proxy, we compared it within the 1988 data to the separate "loss" field, finding a correlation of 0.91 between them. As the relevant provisions of federal restitution law had undergone no significant change between 1982 and 1995, we have no reason to believe that the relationship between restitution and harm would have changed between 1988 and the postguidelines period. This is, however, a critical assumption of our analysis.

Even by the use of this proxy, many cases reflected zero or missing values for HARM, and these observations were omitted from the analysis, thereby further reducing the total size of the combined data set to 106 cases. From this group, we dropped one outlying observation involving a total monetary penalty of some $19 million, which was six times higher than the next highest figure of $3.1 million, thus reducing the principal data set studied to 105 observations. Within this group, the postguidelines cases were categorized into offense types on the basis of offense code clas-

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The regression result is as follows.

<table>
<thead>
<tr>
<th>Variable</th>
<th>B</th>
<th>SE B</th>
<th>Beta</th>
<th>T</th>
<th>Significance T</th>
</tr>
</thead>
<tbody>
<tr>
<td>HARM</td>
<td>2.823654</td>
<td>.214233</td>
<td>.905825</td>
<td>13.180</td>
<td>.000</td>
</tr>
<tr>
<td>Constant</td>
<td>-89,423.6780</td>
<td>164,757.6274</td>
<td>-1.150</td>
<td>.257</td>
<td></td>
</tr>
</tbody>
</table>

Note.—Dependant variable: CONLOSS = loss caused by offenses of conviction; independent variable: HARM = voluntary + ordered restitution (excluding zero values); multiple $R = .90582$; $R^2 = .82052$.

The basic federal provisions for restitution were added by the Victim and Witness Protection Act of 1982, 18 U.S.C. former §§ 3579–3580 (1976 and Suppl. V), which were carried forward by the Sentencing Reform Act of 1984 in 18 U.S.C. §§ 3663–3664 (1982 and Suppl. 1998). Major amendments were not made to these statutes until 1996, after the period covered by our study.

There is some evidence that the incidence of restitution orders changed from the pre-guideline to postguideline period. However, unless this factor is correlated with some other variable of interest, it would not affect our results, given the absence of any change in the criteria for determining the magnitude of restitution awards. In any case, our tests for selection bias indicate no effect due to this factor (see Section IVB below).

Alternative specifications, in which zero HARM values were replaced by either the median or mean HARM of other nonzero HARM values, did not produce differing results.

As noted above, we also excluded 268 cases involving antitrust and environmental offenses, as neither category of offense was affected by the 1991 guidelines. However, as with the zero HARM values, we also examined alternative specifications, including the antitrust and environmental cases in the model, and they also did not materially affect the results reported below.

Dropping this one grossly outlying observation improved the fit of the modeling to the data available. However, we also tested the hypothesis that the remaining data represented a truncated set (see Appendix), with no effect on our general results.
sifications used in the 1988 data, as follows: (1) currency, (2) food and
drug, (3) fraud, (4) property, (5) tax, and (6) miscellaneous. However, there
were no food and drug offenses represented in the residual group, so only
five types are represented in the analysis.

While the exclusion of many observations for lack of either a direct or
proxy measure for harm is regrettable and obviously limits our analysis, this
was the only means that we could devise for studying the effect of the 1991
guidelines while remaining within the confines of the data sets released by
the commission.

IV. MODELING AND EMPIRICAL RESULTS

Our modeling focused primarily on the question whether the 1991 guide-
lines produced a significant change in corporate criminal monetary san-
tions from preguidelines practice. Accordingly, we tested the effect of the
1991 guidelines essentially as an "event" within a simple preguidelines
model specifying the corporate monetary sanction as a function of offense
type, HARM (as defined above), and the square of harm (HARM^2), as had
been suggested by previous empirical analyses of preguidelines corporate
sentences. We then examined whether the guidelines affected the mean
sanction or its variance or changed the structure of the relationship between
HARM and the sanction. In general, we found no significant effects of the
guidelines on either the mean sanction or its variance, as a function of
HARM, or on the structure of the relationship between HARM and the
monetary sanction.

A. Did the Guidelines Affect Mean Sanction Levels or Variance?

To test the effect of the 1991 guidelines on fine levels, we began with a
simple preguidelines model expressing the total sanction as a linear func-
tion of HARM and its square, as follows:

\[
\text{total sanction} = \text{constant} + b_1 \text{HARM} + b_2 \text{HARM}^2 + \text{error term},
\]

38 For this purpose, corporate criminal monetary sanction equals the sum of the fine and
the restitution ordered by the criminal court.

39 See Cohen et al., supra note 18; Cohen, Corporate Crime and Punishment: A Study,
supra note 18; Cohen, Corporate Crime and Punishment: An Update, supra note 18; Cohen,
supra note 15; and Parker, supra note 15. This functional form provided a better fit with the
data than the alternative of using the logarithm of HARM.

40 As discussed in Section III supra, HARM is defined as the total of restitution previously
paid by the defendant plus additional restitution ordered by the criminal sentencing judge.
### TABLE 1
**AGGREGATED MODEL**

| Variable  | Coefficient | SE     | r-Ratio | Pr(|t| > 0) |
|-----------|-------------|--------|---------|------------|
| HARM      | 1.5675      | .2694  | 5.818   | .00000     |
| HARM²     | −.19818E−06 | .9220E−07 | −2.150 | .03159     |
| CH8GUIDE  | 20,707      | .5378E+05 | .385   | .70024     |
| Constant  | −20,604     | .5026E+05 | −.410  | .68182     |

*Note.*—Dependent variable = total monetary judgment; number of observations = 105; $f = 131.60096$; $R^2 = .79629$; adjusted $R^2 = .79024$; results corrected for heteroskedasticity; Breusch-Pagan $\chi^2$ (df) = 126.490 (3).

or

$$S_i = C + b_1H_i + b_2H_i^2 + e_i.$$  

There were two basic variations of this model, one aggregating all offense types and a second variation including separate dummy variables for each offense type (in which case the constant was omitted). To test the effects of the 1991 guidelines, we added a dummy variable named “CH8GUIDE” (chapter 8 was the guidelines chapter added by the 1991 amendments), taking the value of one for all cases sentenced under the new guidelines and zero otherwise, and performed ordinary least squares regressions. In all instances, both HARM and HARM² are significant, and the chapter 8 variables were insignificant in all cases with the exception of property offenses in one variant of the disaggregated model.

The regression results under the simplest model are reported in Table 1. In this model, the coefficient of the dummy variable for the 1991 guidelines was positive but insignificant.

We also ran two variations of the disaggregated model, separating results by offense type—one variation with a simple dummy variable for the “chapter 8” effect (Table 2) and a second variable that separated the chapter 8 effect by offense type (Table 3). Both produced results very similar to the aggregated model and neither provided substantial additional refinement.

In both of these models, the chapter 8 effects remain insignificant, whether measured in the aggregate or by offense type, with the sole exception of a marginally significant finding ($p = .075$) of a positive effect of

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41 While our dependent variable is censored, using the more complicated Tobit model provided absolutely no change. This is because in our sample of 105 cases, there were only three observations of a total monetary fine of zero. When we ran our analysis on the larger data set, the Tobit model was used to account for the higher proportion of censored data.
TABLE 2

PREGUIDELINES OFFENSES BY TYPE: AGGREGATE CHAPTER 8 EFFECT

| Variable   | Coefficient | SE        | t-Ratio | Pr(|t| > 0) |
|------------|-------------|-----------|---------|------------|
| HARM       | 1.6142      | .2701     | 5.976   | .00000     |
| HARM²      | -.21748E-06 | .9319E-07 | -2.334  | .01961     |
| CH8GUIDE   | -30,657     | .8815E+05 | -.348   | .72800     |
| CURRENCY   | -.12712E+06 | .1333E+06 | -.954   | .34016     |
| PROPERTY   | -60,898     | .8020E+05 | -.759   | .44764     |
| TAX        | -47,712     | .1102E+06 | -.433   | .66499     |
| FRAUD      | 73,428      | .9143E+05 | .803    | .42191     |
| MISC       | -67,826     | .4577E+05 | -1.482  | .13837     |

Note.—Number of observations = 105; \( F = 65.75591 \); \( R^2 = .84431 \); adjusted \( R^2 = .83147 \); results corrected for heteroskedasticity; Breusch-Pagan \( \chi^2 \) (df) = 150.564 (7).

TABLE 3

FULLY DISAGGREGATED BY OFFENSE TYPE

| Variable   | Coefficient | SE        | t-Ratio | Pr(|t| > 0) |
|------------|-------------|-----------|---------|------------|
| HARM       | 1.6339      | .2716     | 6.016   | .00000     |
| HARM²      | -.22342E-06 | .9373E-07 | -2.384  | .01714     |
| CURRENCY   | -.16329E+06 | .9510E+05 | -1.717  | .08597     |
| PROPERTY   | -.17205E+06 | .8792E+05 | -1.957  | .05037     |
| TAX        | -81,749     | .5962E+05 | -1.371  | .17034     |
| FRAUD      | .10758E+06  | .1163E+06 | .925    | .35505     |
| MISC       | -78,603     | .4713E+05 | -1.668  | .09538     |
| PROPC8     | .15210E+06  | .8542E+05 | 1.781   | .07496     |
| FRAUDCH8   | -77,833     | .1221E+06 | -6.637  | .52381     |
| MISCCH8    | 45,714      | .5310E+05 | .861    | .38932     |

Note.—Number of observations = 105; \( F(9, 95) = 44.99 \); \( R^2 = .81 \); adjusted \( R^2 = .79 \); results corrected for heteroskedasticity; Breusch-Pagan \( \chi^2 \) (df) = 158.973 (9).

chapter 8 on property offense penalties under the fully disaggregated model.\(^{42}\)

In the partially disaggregated model (Table 2), the preguidelines offense types themselves lack significance, once we control for HARM and its square, for the offense types studied. However, under the fully disaggregated model (Table 3), several offense types regain significance or marginal significance in the preguidelines data, which is consistent with prior stud-

\(^{42}\) At the most disaggregated level (Table 3), there are insufficient observations to estimate the chapter 8 effect on two of the five offense types (currency and tax).
TABLE 4
AGGREGATED VARIANCE MODEL

<table>
<thead>
<tr>
<th>Variable</th>
<th>B</th>
<th>SE B</th>
<th>Beta</th>
<th>T</th>
<th>Significance T</th>
</tr>
</thead>
<tbody>
<tr>
<td>HARM</td>
<td>-49,971,093.58</td>
<td>8,078,161.948</td>
<td>-0.838022</td>
<td>-6.186</td>
<td>0.000</td>
</tr>
<tr>
<td>HARM\textsuperscript{2}</td>
<td>31,693,846</td>
<td>2,746,077</td>
<td>1.559,846</td>
<td>11.541</td>
<td>0.000</td>
</tr>
<tr>
<td>CH8GUIDE</td>
<td>1.62360E+12</td>
<td>3.7423E+12</td>
<td>0.022710</td>
<td>0.434</td>
<td>0.6653</td>
</tr>
<tr>
<td>Constant</td>
<td>3.04652E+12</td>
<td>3.0790E+12</td>
<td>0.989</td>
<td>0.3248</td>
<td></td>
</tr>
</tbody>
</table>

Note. — Number of observations = 105; \( F = 89.61203; R^2 = .72495; \) adjusted \( R^2 = .71686. \)

As noted above, in one instance (property offenses), the offense-type effect is significant in both the pre-guidelines and post-guidelines data. To investigate this exception further, we reran the fully disaggregated model under an alternate specification in which both HARM and HARM\textsuperscript{2} were allowed to vary with offense type, and in that variation, the chapter 8 effects disappeared, thus suggesting that the guidelines may have had an effect on the structure rather than the level of penalties for that offense type only. We investigate the structural effects further in Section IVB below.

These first three models, like all simple regression models, test only the effects at the mean. However, arguably one of the purposes of sentencing guidelines is to reduce the variance in sentencing outcomes and thereby increase both the certainty and perceived fairness of punishment.\textsuperscript{44} Therefore, we also ran similarly specified models that redefined the dependent variable as variance (computed as the squared difference between the observed and mean fine, computed separately for the 1988 and post-guidelines data sets). Similar results were observed: the 1991 guidelines had no significant effects,\textsuperscript{45} either under the aggregated model or by offense types (see Tables 4 and 5). Like the basic models, variance under these models was not sig-

\textsuperscript{43} Preguidelines study had indicated differences among offense types in terms of the relationship between harm and the sanction (Cohen et al., supra note 18, at 122, table 9). However, the most notable deviation was in the environmental category, which we did not examine.

\textsuperscript{44} This was one of the purposes stated by Congress in the Sentencing Reform Act of 1984 for the Sentencing Commission's guidelines; see 28 U.S.C. § 991(b)(1)(B).

\textsuperscript{45} We received a comment from an anonymous referee that a negative finding on variance is ambiguous, given that the guidelines purported to rest corporate penalties on culpability as well as harm factors, and our model omits culpability. However, our specification here would detect any change in overall variance by consideration of the chapter 8 dummy variables, and we found none. The probability of exactly offsetting effects between an omitted variable and otherwise reduced variance would appear to be very small. As noted in the following section, we also found a negative result under the Chow test for a change in modeling structure. However, this does not exclude a possible change in the structure of variance, albeit at the same magnitude, which is a point that we do not address.
<table>
<thead>
<tr>
<th>Variable</th>
<th>B</th>
<th>SE B</th>
<th>Beta</th>
<th>T</th>
<th>Significance T</th>
</tr>
</thead>
<tbody>
<tr>
<td>HARM</td>
<td>-50,908,190.43</td>
<td>8,517,573.611</td>
<td>-0.932359</td>
<td>-5.977</td>
<td>.0000</td>
</tr>
<tr>
<td>HARM²</td>
<td>31.981174</td>
<td>2.879119</td>
<td>1.608816</td>
<td>11.108</td>
<td>.0000</td>
</tr>
<tr>
<td>CURRENCY</td>
<td>7.67849E+12</td>
<td>8.8505E+12</td>
<td>.047841</td>
<td>.868</td>
<td>.3878</td>
</tr>
<tr>
<td>TAX</td>
<td>5.71570E+12</td>
<td>5.9286E+12</td>
<td>.052821</td>
<td>.964</td>
<td>.3374</td>
</tr>
<tr>
<td>FRAUD</td>
<td>1.91365E+12</td>
<td>5.5113E+12</td>
<td>.039902</td>
<td>.347</td>
<td>.7292</td>
</tr>
<tr>
<td>FRAUDCH8</td>
<td>2.64378E+12</td>
<td>6.3289E+12</td>
<td>.048865</td>
<td>.418</td>
<td>.6771</td>
</tr>
<tr>
<td>PROPERTY</td>
<td>6.32943E+12</td>
<td>1.3527E+13</td>
<td>.039436</td>
<td>.468</td>
<td>.6409</td>
</tr>
<tr>
<td>PROPCH8</td>
<td>-4.32736E+12</td>
<td>1.7421E+13</td>
<td>-0.20885</td>
<td>-2.48</td>
<td>.8044</td>
</tr>
<tr>
<td>MISC</td>
<td>3.4842E+12</td>
<td>3.9662E+12</td>
<td>.052281</td>
<td>.878</td>
<td>.3819</td>
</tr>
<tr>
<td>MISCCH8</td>
<td>-1.79133E+11</td>
<td>1.1650E+13</td>
<td>-8.645E-04</td>
<td>-0.015</td>
<td>.9878</td>
</tr>
</tbody>
</table>

Note.—Number of observations = 105; F = 25.83687; R² = .72910; adjusted R² = .70088.
TABLE 6
Unrestricted Interaction Effects

| Variable      | Coefficient | SE       | t-Ratio | Pr|t| > 0 |
|---------------|-------------|----------|---------|----|-----|
| HARM          | 1.4293      | .5909    | 2.419   | .01557 |
| HARM$^2$      | -.16696E-06 | .2397E-06 | -3.697  | .48602 |
| CH8GUIDE      | -6.321.0    | .5697E+05 | -.111   | .91165 |
| CH8HARM       | .18134      | .6690    | .271    | .78635 |
| CH8HARM$^2$   | -.38972E-07 | .2599E-06 | -.150   | .88080 |
| Constant      | -2.492.2    | .5220E+05 | -.048   | .96192 |

Note. — $F(5, 99) = .7799289E+02$; sum of squares = .8206357E+13; $R^2 = .7975313E+00$; adjusted $R^2 = .7873056E+00$; results corrected for heteroskedasticity; Breusch-Pagan $\chi^2(df) = 138.807 (5)$.

significantly affected by the 1991 guidelines, and, under the disaggregated model, offense types were insignificant both before and after the guidelines.

B. Did the Guidelines Affect Penalty Structure?

A possible objection to our simple model is that it could detect only a general shift in sanction levels and not a shift in the relationship between HARM and the sanction. Thus, although the average sanction may not have changed following the adoption of the guidelines, the relationship between HARM and the sanction ordered may have changed. In order to test for any structural change in the penalty function, we respecified the model to include various interaction terms and then reestimated the aggregated model.

$$F_i = C + b_1 H_i + b_2 H_i^2 + b_3 \text{CH8Dummy} + b_4 \text{CH8Dummy} \times H_i + b_5 \text{CH8Dummy} \times H_i^2 + e_i.$$  \hspace{1cm} (2)

This is our "unrestricted" model, in that although we continue to assume that monetary fines are a function of HARM and HARM$^2$, we permit all coefficients for the postguideline function to differ from the preguideline function. The results of this specification are presented in Table 6. As specified in equation (2), any change in the structural form would manifest itself with a significant coefficient for any of the chapter 8 interaction terms. As Table 6 demonstrates, none of these three coefficients is, standing alone, statistically significant. While none of the three interaction terms is significant standing alone, together they may represent a significant structural change in the penalty structure due to the chapter 8 guidelines. We test for any structural change by comparing the unrestricted model from equation (2) with the restricted model from equation (1). Using the Chow test, we test the null hypotheses that there was no structural change from the guide-
TABLE 7

**RESTRICTED MODEL**

| Variable | Coefficient | SE   | t-Ratio | Pr(|t| > 0) |
|----------|-------------|------|---------|------------|
| HARM     | 1.5720      | .1462| 10.755  | .00000     |
| HARM²    | -1.9939E-06 | .6361E-07| -3.135 | .00225     |
| Constant | -8,507.2    | .3361E+05| -.253  | .80069     |

*Note.* — $F(2, 102) = .19903$; sum of squares = .8267158E+13; $R^2 = .796$; adjusted $R^2 = .792$; results corrected for heteroskedasticity; Breusch-Pagan $\chi^2(df) = 124.087$ (2).

The residual sum of squares from the restricted and unrestricted regressions form the basis for this F-test. The results of the restricted estimation are reported in Table 7.

The F-statistic for testing whether there was a structural change is calculated as $F(3, 99) = 0.2593$. The critical value for 95 percent significance is 2.71; we cannot, therefore, reject the null hypothesis that there was no structural change due to the chapter 8 guidelines.

We also tested for sample selection bias attributable to our use of restitution amounts as a proxy for harm. As discussed in Section III above, our analysis includes only those cases in which restitution was paid or ordered, from which we derived our estimate of harm. We addressed this issue by recomputing consistent estimates within a sample selection model. In order to derive consistent estimates, we first estimate a probit model on the entire data set ($N = 652$), with the dependent variable equal to one if restitution was ordered or paid and zero otherwise. Independent variables offered to explain the restitution decision included the type of criminal offense and the chapter 8 guideline dummy variable. Examination of our probit results did reveal a higher probability of restitution being ordered after the enactment of the guidelines. Calculating and introducing the Inverse Mill’s Ratio into our regression analysis will account for the sample

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47 See William H. Greene, Econometric Analysis 211–12 (1993). In calculating our test statistic, we assume that the disturbance variance did not change with the adoption of the guidelines. Were the disturbance variance to vary, our test statistic would overestimate the significance of the change. See id. at 215. Given that we failed to detect any structural change, we are not concerned that the test statistic may be too high.

48 The model is based on William H. Greene, Sample Selection Bias as a Specification Error: Comment, 49 Econometrica 795 (1981); and G. S. Maddala, Limited-Dependent and Qualitative Variables in Econometrics (1983).
TABLE 8

UNRESTRICTED MODEL AND RESTRICTED MODEL WITH SELECTION CORRECTION

| Variable      | Coefficient | SE   | t-Ratio | Pr(|t| > 0) |
|---------------|-------------|------|---------|------------|
| A: Unrestricted Model: |             |      |         |            |
| HARM         | 1.5804      | .3510 | 4.502   | .00002     |
| HARM²        | -22147E-06  | .1599E-06 | -1.385 | .16930     |
| CH8GUIDE     | -14702E+06  | .9210E+05 | -1.596 | .11363     |
| CH8HARM      | .45070E-01  | .3853 | .117    | .90712     |
| CH8HARM²     | .52859E-08  | .1736E-06 | .030   | .97576     |
| Constant     | .64703E+06  | .2820E+06 | 2.294  | .02390     |
| B: Restricted Model: |             |      |         |            |
| HARM         | 1.5763      | .1444 | 10.914  | .00000     |
| HARM²        | -20390E-06  | .6288E-07 | -3.242 | .00161     |
| Constant     | .28312E+06  | .1591E+06 | 1.779  | .07818     |

NOTE.—Unrestricted model: number of observations = 105; F(6, 98) = 68.88; sum of squares = .7768791E+13; $R^2 = .808$; adjusted $R^2 = .797$. Restricted model: $F(3, 101) = 137.1315$; sum of squares = .7989313E+13; $R^2 = .802886$; adjusted $R^2 = .797031$.

selection bias. The results of this sample selection regression are presented in Table 8.

After having accounted for the sample selection bias, our data still fail to reveal any significant change resulting from the guidelines. And while recomputing the $F$-statistic for a structural change does offer a more significant number, 0.99, it is still insufficient to reject the hypothesis that the guidelines had no structural impact on the relationship between harm and the monetary corporate penalty.

Our results indicate that while there may have been some change in monetary penalty structure occasioned by the 1991 guidelines, it was undetectable in the data provided by the commission. And while we cannot comment on the guidelines' effects on a subset of extreme cases viewed in isolation, we can conclude that for the typical corporate criminal case, the

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49 Adjustment of the standard errors is also required.

50 In addition to trying to adjust the smaller sample to account for the possible selection bias, we also addressed this problem by examining the entire data set rather than just a smaller sample of it. This required generating a different proxy for harm for those cases where no restitution was ordered. Each of those cases was assigned a HARM equal to the median and mean restitution observed for that crime type. Analysis with a Tobit model similarly failed to reveal any change resulting from the new guidelines.

51 For this reason, we do not believe that our results necessarily are inconsistent or incompatible with the similarly titled paper presented in 1997 by Alexander, Arlen, and Cohen, who study the distinct problem of how the corporate sentencing guidelines affected the penalties imposed on a subset of large, publicly traded firms, which are far from the typical corporate defendant in the federal courts. Cindy R. Alexander, Jennifer Arlen, & Mark A. Cohen, Regulating Corporate Criminal Sanctions: Evidence of the Effect of the U.S. Sentencing Guidelines (draft paper presented at the John M. Olin Conference on Penalties: Public and Private, University of Chicago Law School, December 13–14, 1997). Alexander, Arlen, &
guidelines had no visible effect on the penalty imposed, after controlling for the harm caused by the offense.

C. Did the Corporate Guidelines Affect the Joint Determination of Individual Penalties?

One of the major objections to corporate criminal liability is that it may act to shield responsible individuals within the firm from liability.\(^{52}\) The 1991 guidelines dealt with this effect fairly explicitly, with a provision for a limited fine offset for individual penalties imposed on the owners of closely held firms,\(^{53}\) thus implying a policy against such offsets in other circumstances. As prior empirical analyses had found some relationship between individual and corporate sanctions for all types of firms,\(^{54}\) it seems appropriate to investigate whether the 1991 guidelines had a significant effect on that relationship.

Regrettably, such an investigation encounters severe data problems because of what appears to be a deliberate effort on the part of the Sentencing Commission to hide the relationships between individual and corporate penalties. While the commission has released data sets containing both individual and corporate sentences, it is impossible to match those data (for example, by reference to common docket numbers or even an anonymous joint control number) because the commission has removed all such identifiers. Therefore, a rigorous assessment of the relationship between individual and corporate penalty levels is precluded. However, there is still the possibility of indirectly measuring the effect of individual codefendants on the corporate penalty both before and after the guidelines.

Both the 1988 and postguidelines corporate data internally contain minimal information on whether the corporation had an individual codefendant and, if so, how many. We recoded this data as a zero-one dummy variable named INDCODEF and then added this variable to our basic aggregate model, with the results reported in Table 9 below. In this model, the effect of individual codefendants on corporate penalties is negative but insignifi-

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Cohen also asserts that much of the data concerning large fines were excluded systematically from the Sentencing Commission’s data sets. While we have not had access to their evidence of this effect and encountered no evidence in our own study that such an effect exists, we nonetheless assume its existence and analyze the potential effects in the Appendix to this paper, in which we find that, even under extreme assumptions of systematic data truncation, the results reported in our paper are unaffected. However, our paper says nothing about the possible effects of the guidelines on a defined subsample of large, publicly traded firms, which is the topic addressed by Alexander, Arlen, & Cohen.

\(^{52}\) See Parker, supra notes 13, 25.

\(^{53}\) USSG § 8C3.4.

TABLE 9

EFFECTS OF INDIVIDUAL CODEFENDANTS ON CORPORATE PENALTIES

| Variable   | Coefficient | SE     | t-Ratio | Pr|t| > 0 |
|------------|-------------|--------|---------|----|-----|
| HARM       | 1.6140      | .2795  | 5.774   | .00000 |
| HARM\(^2\) | -.21175E-6  | .9522E-07 | -2.224  | .02615 |
| INDCODEDEF | -57.563     | .5051E+05 | -1.140  | 25445 |
| Constant   | 23.544      | .4374E+05 | .538    | .59042 |

Note.—Number of observations = 102; F(3, 98) = 127.8349; R\(^2\) = .7964714; adjusted R\(^2\) = .7902409; results corrected for heteroskedasticity; Breusch-Pagan \(\chi^2\)(df) = 123.903 (3).

TABLE 10

EFFECT OF INDIVIDUAL CODEFENDANTS, DISTINGUISHING NEW GUIDELINES

| Variable   | Coefficient | SE     | t-Ratio | Pr|t| > 0 |
|------------|-------------|--------|---------|----|-----|
| HARM       | 1.6114      | .2796  | 5.764   | .00000 |
| HARM\(^2\) | -.21211E-06 | .9478E-07 | -2.238  | .02524 |
| PREGIND    | -.12890E+06 | .7153E+05 | -1.802  | .07152 |
| CH8IND     | -24.381     | .5249E+05 | -.465   | 64229 |
| Constant   | 23.666      | .4374E+05 | .541    | .58850 |

Note.—Number of observations = 102; F(4, 97) = 97.24342; R\(^2\) = .8004007; adjusted R\(^2\) = .7921698; results corrected for heteroskedasticity; Breusch-Pagan \(\chi^2\)(df) = 129.735 (4).

cant. To investigate whether this effect changed with the 1991 guidelines, we separated the preguidelines and postguidelines effects into two interaction dummy variables, one named PREGIND, which takes the value of one for preguideline cases involving individual codefendants and zero otherwise, and a second variable named CH8IND, which takes the value of one in chapter 8 cases involving individual codefendants and zero otherwise, with the results reported in Table 10. These results are interesting, as they now show a marginally significant (\(p = .07\)) and negative effect of individual codefendants on corporate penalties under the preguidelines regime, after controlling for the effect of the 1991 guidelines.\(^5\) This implies that the presence of individual codefendants reduces the corporate penalty and that the new guidelines may weaken this effect to some extent, though our result was highly insignificant statistically.

If the corporate penalty is reduced by the presence of individual codefen-

\(^5\) Our basic specification uses total corporate monetary sanction rather than fine alone as the dependent variable, which may be thought to affect the individual codefendant results, as individual codefendants may be ordered to share in restitution payments. Accordingly, we ran equivalent regressions on fine alone, with similar results, though the significance level of the preguidelines variable declined to \(p = .17\).
dants, this is some indication that individual penalties are coordinated, at least implicitly, with corporate penalties. Obviously, this gives the firm some incentive to assure that there are individual codefendants. To the extent that this incentive is weakened by the new guidelines, this may be a matter of interest.

Conversely, this penalty structure, if embodying correlative adjustments in individual penalties (a subject that we were unable to study directly because of the lack of matching variables between the commission’s individual and corporate data sets), also gives the individuals incentives to avoid being defendants or, failing that, to assure that their firms also are defendants. To the extent that the trade-off is relatively direct and assuming both that prosecutors maximize punishment and that individuals are more difficult to convict than corporations, the corporate liability and sanctioning structure also may discourage prosecutors from proceeding against individuals operating within the corporate structure.

D. Did the Guidelines Affect Compliance Behavior?

Aside from the advertised changes to penalty levels and structure, the main feature of the 1991 guidelines was the emphasis on corporate “compliance programs”—essentially, a formalized corporate bureaucracy devoted to legal compliance. Under the 1991 provisions, such a compliance program was mandated by the probation guidelines for convicted corporations and, if in existence at the time of the offense and found satisfactory at sentencing, could reduce the fine imposed on the corporation.\textsuperscript{56} Under these circumstances, we may expect to see some change in the incidence of compliance programs in the postguidelines regime. Regrettably, the available data are too few and ambiguous to draw even tentative conclusions. However, the raw data do suggest possible lines of inquiry for further research.

The available data consist of one field in the 1988 data and two fields in the postguidelines data. In the 1988 data, there is a variable, COMPLY, indicating whether the corporation had a compliance program at the time of the offense, with the frequencies shown in Table 11. In the postguidelines data, there are two variables, (1) COMPLYPR, which is the probation officer’s report of whether the corporation had a compliance program at the time of the offense, and (2) COMPLY, which is a new variable indicating whether the offense occurred despite an “effective” compliance, such as to earn the corporation a three-point discount on its “culpability score” un-

\textsuperscript{56} See Parker, supra note 25, at 410–23.
TABLE 11
COMPLIANCE PROGRAM DATA, 1988

<table>
<thead>
<tr>
<th>Code</th>
<th>Value Label</th>
<th>Frequency</th>
<th>Percent</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>No</td>
<td>200</td>
<td>60.9</td>
</tr>
<tr>
<td>1</td>
<td>Yes, but not enforced</td>
<td>12</td>
<td>3.7</td>
</tr>
<tr>
<td>2</td>
<td>Yes, actively enforced</td>
<td>2</td>
<td>0.6</td>
</tr>
<tr>
<td>9</td>
<td>Unknown</td>
<td>114</td>
<td>34.8</td>
</tr>
</tbody>
</table>

Note.—N = 328.

TABLE 12
COMPLIANCE PROGRAM DATA, POSTGUIDELINES

<table>
<thead>
<tr>
<th>Variable and Code</th>
<th>Value Label</th>
<th>Frequency</th>
<th>Percent</th>
</tr>
</thead>
<tbody>
<tr>
<td>A. COMPLYPR, as reported by probation officers:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>0</td>
<td>No</td>
<td>305</td>
<td>94.1</td>
</tr>
<tr>
<td>1</td>
<td>Yes</td>
<td>10</td>
<td>3.1</td>
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<tr>
<td>9</td>
<td>Missing or indeterminable</td>
<td>9</td>
<td>2.8</td>
</tr>
<tr>
<td>B. COMPLY, as computed under the guidelines:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>0</td>
<td>No “effective” program</td>
<td>166</td>
<td>51.2</td>
</tr>
<tr>
<td>8</td>
<td>“Effective” program</td>
<td>27</td>
<td>8.3</td>
</tr>
<tr>
<td>9</td>
<td>Unknown</td>
<td>2</td>
<td>0.6</td>
</tr>
<tr>
<td>98</td>
<td>N.A.: no guideline fine</td>
<td>120</td>
<td>37.0</td>
</tr>
<tr>
<td>99</td>
<td>Missing</td>
<td>9</td>
<td>2.5</td>
</tr>
<tr>
<td>-3</td>
<td>Unknown</td>
<td>1</td>
<td>0.3</td>
</tr>
</tbody>
</table>

Note.—N = 324; N.A. = not applicable.

under the guidelines. If the second variable is intended to be a subset of the first, this would be inconsistent with their relative frequencies, as shown in Table 12. The variable COMPLY, as computed under the guidelines, is shown in section B of Table 12. Unless the data are in error, it would appear that a larger number of firms received discounts for an effective compliance program than were recommended by probation officers. This is not necessarily inconsistent, as the sentencing judge may choose to accept or reject the probation officer’s recommendations.

If the differences between the two postguidelines variables were data error, we can investigate under alternative assumptions about which of the

57 See USSG § 8C2.5(f).
58 COMPLY provides the score for this culpability factor under the guidelines, 98 if the factor is inapplicable to the case, or 99 if the datum is missing from the data gathered by the commission.
postguidelines variables is more accurate. First assuming that COMPLYPR is more accurate and comparable to the 1988 observations, it would appear that the incidence of known compliance programs among convicted firms actually declined slightly in the postguidelines era: from 4.3 percent in 1988 to 3.1 percent in the postguidelines era. However, that change is not statistically significant under the standard test for comparing two proportions.\textsuperscript{59} Of course, even a significant drop would not necessarily reflect underlying compliance behavior: if compliance programs actually work in the sense of preventing offenses, then the incidence of compliance programs among convicted firms may be unchanged or decline, even if more firms are engaging in this behavior, either because of the guidelines or for other reasons.

The alternative assumption that the postguidelines COMPLY variable is more accurate and comparable leads to more provocative implications. Assuming the lowest possible incidence of compliance programs consistent with that variable, 27 of the 324 firms had such a program.\textsuperscript{60} Even that figure would indicate a near doubling of compliance program incidence under the guidelines—an increase from 4.3 percent to 8.3 percent—and that change is significant, producing a two-proportion test statistic of 2.13, which is significant at the 95 percent confidence level. If that turns out to be the more reliable indicator of the guidelines' effect, then, coupled with our other findings, it opens up an interesting new avenue for future investigation.

If one were convinced both that the guidelines produced a significant increase in compliance program incidence among convicted firms and that the guidelines did not significantly raise overall penalty levels, then, ceteris paribus, such findings could supply an empirical basis for determining whether compliance programs—at least those of the sort prescribed by the guidelines—are accomplishing their supposed purpose of reducing the incidence of crimes within corporations.\textsuperscript{61} This implication is suggested by the

\textsuperscript{59} Taking the relative proportions of known compliance programs to the total number of observations and computing the test statistic of

\[ p_1 - p_2 \sqrt{\frac{p(1-p)}{n_1} + \frac{p(1-p)}{n_2}} \]

produces .8013, which is far below the critical values of the Z-tests and t-tests.

\textsuperscript{60} The number could be higher, depending on how one interprets some of the ambiguous coding values in section B of Table 12, as both 9 and -3 both could represent the existence of a compliance program, thus bringing the total to 30, which would further reinforce the possible effects discussed in the text.

\textsuperscript{61} There is a developing theoretical debate on whether this apples-and-oranges mixture of strict liability and negligence systems can provide an optimal penalty structure, for which we compare Block, \textit{supra} note 17; and Parker, \textit{supra} note 25; with Jennifer Arlen, The Potentially Perverse Effects of Corporate Criminal Liability, 23 J. Legal Stud. 833 (1994); and Jennifer Arlen & Reinier Kraakman, Controlling Corporate Misconduct: An Analysis of Corporate Liability Regimes, 72 N.Y.U. L. Rev. 687 (1997).
fact that we are only observing compliance program incidence among convicted firms. However, this implication is subject to the qualifications that there is no overall change in the rates of either violations or compliance activity and that there is no change in the technology of compliance programs, which introduces problems that we could not resolve within the context of this study.\(^{62}\)

If one could control for more general changes in violations and compliance programs, then presumably violating firms would display a lower incidence of compliance program activity than nonviolating firms if, in fact, compliance programs actually had an impact on the probability of a violation. In the pre-guidelines regime, the observed incidence of compliance programs among violating firms could reflect this incentive. However, the new guidelines introduced the confounding change of granting a substantial discount from the corporate fine for compliance programs that by definition did not work in the particular case but nonetheless were deemed “effective” by the sentencing judge. Now firms have some incentive to adopt compliance programs that do not work in terms of reducing violations, simply to earn a discount from the expected fine. If, at that same time, the overall expected sanction level, the technology of compliance, and the supply of offenses all remain unchanged, then firms have no additional incentive to adopt programs actually to reduce the incidence of violation but only to obtain the discount. Under these circumstances, an observed increase in the incidence of compliance programs among convicted firms may imply that compliance programs are being adopted only for the fine discount and not because they actually reduce the incidence of violation.

At this point, our observations on this subject are merely suggestive. Aside from the data problems and inconsistencies, the careful study of compliance effects will require a far more sophisticated modeling of the problem. However, this would appear to be a fruitful direction for future research.

V. Discussion

There is always some trepidation associated with reporting largely “negative” results such as those reported in this paper. Strictly speaking, these results are nothing more than a failure to reject the null hypothesis that the 1991 guidelines had no significant effect on the general level or structure of corporate monetary penalties.

\(^{62}\) We did investigate whether there was any detectable change in the effect of compliance programs on corporate monetary penalties after controlling for harm, but we found that there were too few observations within our study sample of 105 cases for which we had harm estimates.
But plainly there are sound theoretical and empirical bases for questioning whether the entire system of corporate criminal sentencing in the federal courts is worth much effort from the standpoint of public law enforcement policy. Our principal focus in this paper has been on examining whether the system itself behaves as if its role were important, and we found little evidence that it does. Had we found the opposite, however, the more general questions would remain. If the prior research concerning the effects of reputation and other collateral consequences\textsuperscript{63} can be generalized to all corporate sentencing, then we are speaking of a system accounting for only a few percentage points of the overall effect of the public enforcement action. Thus, even if corporate criminal penalties had increased severalfold, that effect alone still would account for only a relatively small percentage of the overall effect. Moreover, given that there appears to be a functioning market in reputational effect, even larger increases in formal criminal penalties may be completely offset by an adjustment in that market response.

This is not to suggest that marginal changes cannot be important. In some cases, they can. For example, if criminal enforcement became relatively more or less attractive to enforcers than civil or administrative enforcement, such a shift could have an important influence on the overall mix of enforcement effort. But under the current state of our knowledge, the welfare implications of such shifts are highly ambiguous. Therefore, any empirical finding within the corporate criminal sentencing system alone also is ambiguous. From this point of view, our "negative" findings, if they hold up, may be a reassuring sign the system does not take itself too seriously.

Nevertheless, empirical findings from within the criminal sentencing system can sometimes provide a window on the wider world. As is suggested by our speculative observations regarding compliance programs, better data from the criminal sentencing system may permit more useful inferences about the advisability of such things as mandated compliance programs and the like without necessarily taking on the far more daunting prospect of seeking to evaluate the optimality of the overall public law enforcement system.

As this example also indicates, the use of the criminal sentencing system to gain a perspective on the law enforcement system requires at the very least consistent and reliable data, which the Sentencing Commission so far seems unable (or unwilling) to release. Regrettably, the most immediate policy implication of our work on this subject is that the Sentencing Commission is not doing a very good job of compiling and releasing usable data.

\textsuperscript{63} See especially Karpooff & Lott, supra note 14, and see generally notes 13–25 and the accompanying text infra.
The commission needs to do a better job if it is to be faithful to the congres-
sional directives that the commission base its sentencing policies on "‘ad-
ancement in knowledge of human behavior as it relates to the criminal jus-
tice process’"²⁴ and that the commission "‘develop means of measuring the
degree to which the sentencing, penal, and correctional practices are effec-
tive,’"²⁵ which together comprise two of the four enumerated purposes for
which the Sentencing Commission was created by Congress. The commis-
sion cannot carry out either of those mandates behind closed doors and
without the assistance of independent outside researchers if the operative
intent is to make the commission accountable for these functions. Of
course, there is the alternative possibility that such is not the operative in-
tent of the legislation.

VI. Conclusion

Did the federal corporate sentencing guidelines matter? The answer de-
pends on the criterion one selects.

Studying a limited data set selected with reference to explicit estimates of
harm (by proxy from restitution), we failed to find evidence of a statistically
significant change in either the levels or the structure of monetary corporate
penalties, after controlling for harm, when comparing the 1991 guidelines
with baseline data from 1988, with the sole exception of our finding of a
marginally significant change for property offenses in a model disaggregat-
ing the guidelines’ effect by offense types. In extensions of that same analy-
sis, we found a marginally significant difference between the effect of indi-
vidual codefendants on corporate penalties as between 1988, when the
presence of individual codefendants reduced the corporate penalty, and the
postguidelines era, when that factor had no significant effect. We were un-
able to determine whether the guidelines had any effect on the incidence of
compliance programs among convicted firms, though that subject deserves
more extensive study in future research.

We are agnostic on the questions whether any of these findings would
withstand a major change of specification (though we investigated several
alternatives, all with similar results) or whether they could be generalized
beyond this limited data set (though we also adjusted our analysis for as-
sumed problems of truncation and selection bias in our working data set).
But even if they were robust, neither these results nor their exact opposites
would tell us very much about the overall properties of the federal public
law enforcement system, given the relatively limited role of formal corpo-

rate criminal penalties in that context and the fact that both legal and marketplace substitutes for criminal penalties may adjust to negate any change within the criminal system. If our results are correct, then they may tell us that the 1991 guidelines "mattered" in one possibly important respect, in that they made no significant change in the mix between criminal sanctions and their substitutes and, therefore, did not appear to require such an adjustment. On the current state of knowledge concerning the corporate criminal sentencing system, that could be the optimal result.

It may be an especially desirable result if, in fact, the 1991 guidelines were never intended to do anything other than meet a demand for political rhetoric rather than law enforcement. The rhetoric is in the guidelines, though its sensibility as policy is open to question. The actual effect of the guidelines may be safely buried forever in the vicissitudes of the Sentencing Commission's data collection policies. If the guidelines' effect actually is zero, this may be the best of all possible worlds.

APPENDIX

ANALYSIS OF ASSUMED DATA TRUNCATION

This Appendix is designed to address the suggestion by Alexander, Arlen, and Cohen's draft paper66 (1997) that the Sentencing Commission's postguideline data set reflects the systematic exclusion of "big" cases involving significant fines against large, publicly traded corporations. We express no opinion on whether such a selection bias does or does not exist in fact. In our study, we encountered no evidence of such a bias, and we have not examined the evidence relied on by Alexander, Arlen, and Cohen to support their suggestion. Nevertheless, for purposes of this Appendix, we assume the problem exists and is severe and show that it has no effect on the principal results reported in the text of our paper.

We refer to assumed sample bias as a "truncation problem," because it assumes that the upper tail of the distribution (with respect to the size of the monetary penalty) of corporate sentencings has been excluded from the Sentencing Commission's postguideline data set. If the cases provided by the commission were truncated, then our analysis could be affected by a mistaken estimation of the OLS regression coefficients.67 The problem is magnified because the assumed truncation occurs, in theory, only with the postguideline cases. Under this hypothesis, the 1988 data represent a random sample, but the postguidelines data are truncated at some upper limit.

We resolve this problem by assuming the worst possible truncation supported by our data and reestimate our model using a truncated regression model with a flexible truncation point that depends on the date of the case. If the case is postguidelines, we assume upper truncation beyond a limit of $3,500,000,68 that is, that every

66 Alexander, Arlen, & Cohen, supra note 51.
67 Maddala, supra note 48.
68 The highest observed postguideline fine in our working data set is $3,100,000. Numerous seven-figure fines below that level are present. The worst possible truncation (in terms
TABLE A1

UNRESTRICTED MODEL WITH TRUNCATION

| Variable   | Coefficient | SE     | t-Ratio | Pr|t| > 0 |
|------------|-------------|--------|---------|----|------|
| HARM       | 1.4293      | .3429  | 4.168   | .00003 |
| HARM²      | -1.6696E-06 | .1572E-06 | -1.062 | .28832 |
| Constant   | -2.492.2    | .5527E+05 | -.045 | .96404 |
| CH8GUIDE   | -6.049.7    | .6954E+05 | -.087 | .93067 |
| CH8HARM    | .17690      | .3792  | .467    | .64085 |
| CH8HARM²   | -3.5256E-07 | .1726E-06 | -.204 | .83811 |
| å           | .27976E+06  | .1934E+05 | 1.465 | .00000 |

Note.—Log-likelihood = -1465.760.

TABLE A2

RESTRICTED MODEL WITH TRUNCATION

| Variable   | Coefficient | SE     | t-Ratio | Pr|t| > 0 |
|------------|-------------|--------|---------|----|------|
| HARM       | 1.5694      | .1452  | 10.807  | .00000 |
| HARM²      | -.19739E-06 | .6418E-07 | -3.075 | .00210 |
| Constant   | -8,333.9    | .3316E+05 | -.251 | .80159 |
| å           | .28074E+06  | .1940E+05 | 14.471 | .00000 |

Note.—Log-likelihood = -1466.160.

case involving a total monetary penalty of $3.5 million or above was systematically excluded from the data set. For preguidelines cases, the truncation point was set at infinity; that is, those cases were assumed to have no truncation problem, which is the worst-case scenario for our analysis. Our truncated regression model derives consistent MLE estimates by adjusting the likelihood function for the truncation problem.69 The results of the truncated regression model are provided in Tables A1 and A2. We estimate the truncated regression model with both the restricted (preguidelines) and unrestricted (allowing for structural change) specification.

As with the results reported in the text of the paper (Section IV), the chapter 8 interaction terms in Table A1 are still individually insignificant, even after we have corrected the estimates and their standard errors for the most severe truncation problem supported by the data, which excludes the possibility that a truncation effect accounts for our finding of no significant change in penalty levels, after controlling for harm. Similarly, when we calculate the sum of residuals for the restricted

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69 See Maddala, supra note 48.
and unrestricted models, and derive the same $F$-statistic to test for any structural change, we calculate a test statistic of 0.26, which is not significantly different from the $F$-statistic reported in the text. Thus, even when we account for the assumed truncation of all postguideline cases with monetary penalties above $3.5 million, the adjusted estimates still reveal no significant change resulting from the guidelines, in either penalty levels or structure.

As we also point out in the text, this result does not imply that our findings necessarily are inconsistent with those reported by Alexander, Arlen, and Cohen, as their analysis focuses on the distinct problem of analyzing the guidelines' effect on a defined subsample of cases involving large, publicly held firms, which are not representative of the average or typical corporate sentencing case in the federal courts.

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