Internet Appendix for

"Negation of Sanctions: The Personal Effect of Political Contributions"

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This online appendix contains the following:

IA.1. Endogeneity in Political Contributions and Severity of Penalty

In this section of the Internet Appendix, we describe empirical analyses that consider the issue of endogeneity in political contributions.

A.1.1 The Bipartisan Campaign Reform Act (BCRA)

A firm's or individual's decision to contribute politically is likely endogenous. As such, unobserved factors correlated with both the extent of political contributions and the severity of government sanctions may bias the results. To consider the effect of endogeneity in political contributions, we follow Bradley et al. (2016) and Ayyagari et al. (2019), exploiting the enactment of the Bipartisan Campaign Reform Act (BCRA) as an exogenous shock to political contributions.

In the context of our analysis, this regulatory event, enacted on November 6, 2002, affects our measure of political contributions in two ways. First, it increased the contribution limits for both individuals and firm-sponsored PACs, with some being indexed to inflation.[1] Second, it restricted the ways in which firms or individuals can establish or maintain political connections other than PAC and individual contributions. For instance, it diminished the role of soft money in political campaigns by prohibiting political parties and/or candidates from receiving soft money in federal elections in the United States. It also barred the use of treasury funds for political advertisements by corporations. By raising the cap for PAC and personal contributions while limiting alternative ways in which firms can establish political connections, the enactment of BCRA enhanced both the power and intensity of the channels that remain, such as PAC and personal contributions. At the same time, BCRA was not designed to cater to the severity of government penalties imposed upon fraudulent executives.

[1] Individual contributions changed from $1,000 per candidate per election to $2,000 per candidate per election, with this amount being indexed to inflation. Individual limits to PACs were increased from $5,000 to $10,000 per year.

In a difference-in-differences setting, we estimate how the effect of political contributions on government penalties varies following the enactment of BCRA. The dummy variable for BCRA is set to one if the relevant government agency investigation begins after November 6, 2002. We expect to see a more prominent impact of our proxy for political contributions after the act in influencing government penalties on fraudulent executives.

Table A1 at the end of this online appendix reports the results. We observe that the coefficient for the interaction between political contributions and the post BRCA dummy is always negative. It is also statistically significant (save for the test involving the propensity of criminal investigation). Responding to an exogenous change in political spending brought about by the BRCA that strengthened the intensity and effect of PAC and individual contribution, fraudulent executives receive less severe government penalties. Table A1 thus supports the baseline findings that, by limiting the outcomes of government enforcements, political contributions are beneficial for fraudulent executives.

A.1.2 Entropy Balanced Matching

To further mitigate the concern that observable differences across connected and unconnected firms explain differences in penalties across these two groups, we use an entropy balanced matching approach to form a comparable control group, balancing with respect to the first three moments of observable firm characteristics across connected and unconnected firms.[2] This newly-balanced data structure ensures that the features of firms with and without political contributions are similar in terms of mean, standard deviation, and skewness (Hainmueller 2012). We then re-estimate our regressions in Table 2 using this entropy-balanced matching sample.

[2] Specifically, the matching variables are executive age, leverage, number of accused, a dummy for small firm, damages, fraud duration, time to filing, a dummy for top 5 executive, and fraud type dummies.

Table A2 reports the regression results based on the entropy matched sample. We observe that the effect of political contributions on the severity of government penalties remains robust when we closely match firms that make generous political contributions to those that do not.

IA.2: Alternative Estimation Specifications on Penalty Transfer and Entrenchment of Fraudulent Executives

While the risk of endogeneity driving our results is less obvious in the analyses on penalty transfer and entrenchment, we nevertheless check the robustness of our findings by using procedures typically employed to alleviate such concerns: entropy balanced matching and Abadie-Imbens (2006) matching. These tests provide evidence that our findings are not specific to the sample or the estimation method used in the main text of the paper.

A.2.1 Entropy Balanced Matching

To validate the results on penalty transfer, we re-estimate the analysis in Table 9 using an entropy balanced matching approach (Hainmueller, 2012) to form a comparable control group, balancing observations with respect to the first three moments of observable firm characteristics across connected and unconnected firms.[3] From Panel A of Table A3, we always observe a negative and statistically significant coefficient for the term "*Predicted Severity*", which captures the extent that political contributions reduce the severity of government penalty imposed upon an accused executive. As the severity that a politically connected, fraudulent executive receives falls further, their firm receives a greater fine by the SEC.

Panel B of Table A3 shows that the coefficient on the term, "*Predicted Firm Fine*", is always negatively and significantly linked to the likelihood of these fraudulent executives losing their job.

Overall, using this entropy balanced matching sample, we confirm our main findings in Table 9 that contributing executives are able to shift their penalty to the firm's shareholders and that when they do so, they are able to avoid termination, successfully entrenching themselves.

[3] Specifically, the matching variables are the level of political contributions, executive age, leverage, number of accused, a dummy for small firm, damages, fraud duration, a dummy for top 5 executive, and fraud type dummies.

A.2.2 Abadie-Imbens (2006) Matching

While nearest neighbor matching is among the most common matching methods widely used for causal inference in observational studies, the estimator can be biased in finite samples when the matching is not exact. In this subsection, we adopt the Abadie and Imbens (2006) matching techniques to estimate the treatment effect and validate our findings in Table 9. The Abadie and Imbens (2006) procedure offers two benefits over other matching models: First, it not only allows for the nearest neighbor matching, but also the exact matching within the *same* model. Second, the estimator corrects for the asymptotic bias present in simple matching estimators, which can arise if there is incomplete overlap between the distributions of control variables between the treated and the control groups (Colak and Whited 2007).[4]

Since the treatment variable has to be an indicator variable in the Abadie and Imbens (2006) matching approach, we construct our variable of interest, "*Dummy for Contributors with Low Severity*", in an attempt to describe two different attributes of the data. Specifically, this variable is set to one if a fraudulent executive both contributes politically and receives a penalty with a lower-than-"normal" level of severity. To identify such executives, we regress "*Severity*" on "*PC*" to generate each executive's "*Predicted Severity*", which reflects the level of penalty severity in the presence of political capture. The dummy variable is then set to one if a fraudulent executive contributes politically and whose "*Predicted Severity*" falls below the sample median "*Predicted Severity*".

We then form a matched sample of fraudulent executives using the procedure in Abadie and Imbens (2006), selecting matching variables to mirror the combination of control variables in Table 9 Panel A since we are establishing the validity of these results. Matches are created exactly

[4] See more discussions in Colak and Whited (2007) on the Abadie-Imbens matching procedure, especially with respect to COMPUSTAT data.

on industry × year dummies, executive role dummies, as well as dummy variables for class action in selected specifications. We use nearest neighbor matching for the rest of control variables.

Panel A of Table A4 shows that the coefficient estimate for "*Dummy for Contributors with Low Severity*" is positive and statistically significant for all columns. Overall, the results suggest that cases with contributors that receive low-severity penalties result in higher firm fines, consistent with the evidence on a shift in penalty from fraudulent executives to the shareholders of the firm.

Next, we move to validate the tests for the entrenchment of fraudulent executives following the penalty shift. Again, because the treatment variable used in the Abadie and Imbens (2006) matching approach has to be an indicator variable, we construct our variable of interest, "*Dummy for Contributors with Increased Firm Fine*", as an indicator variable set to one if both a fraudulent executive contributes politically and the firm where he is employed has received a higher-than-"normal" fine by the SEC. For the latter, we first regress "*Firm Fine/All Fines*" on the level of political contributions ("*PC*") to generate the predicted value of firm fine, which reflects the level of firm fine in the presence of political capture. We then set the dummy variable to be one if the value of a firm's "*Predicted Firm Fine*" is greater than the sample median "*Predicted Firm Fine*". In doing so, we aim to capture whether a firm is fined more than the typical level of firm fine.

We construct an Abadie-Imbens (2006) matched sample using the set of matching variables that mirrors the corresponding combination of control variables in Table 9 Panel B. Matches are created exactly on executive roles and industry × year, as well as an indicator variable for class action in selected specifications. We use nearest neighbor matching for the rest of control variables.

Panel B of Table A4 reveals that cases involving fraudulent managers who contribute politically and whose firms were assessed higher than median fines see a significant decrease in the likelihood that these executives are terminated. This echoes the findings in Table 9 Panel B, suggesting that politically connected fraudulent executives are able to entrench themselves after shifting the penalty to shareholders.

Additional Reference:

Colak, G. and T. Whited (2007). Spin-offs, Divestitures, and Conglomerate Investment, *Review of Financial Studies* 20, 557-595.

IA.3: Which Contributions — PAC or Individual Contributions — Are More Impactful?

Our main variable of interest, "*PC*", combines both the firm's PAC contributions and those made by the accused executive. In doing so, we attempt to capture the aggregate impact of political connections, considering that such connections can be established via the venues of corporate or individual contributions. As described in subsection 2.2, while personal contributions may suggest a more aggressive and clear preference of the contributor, it is often limited by a lower cap compared to a firm's PACs, thus may understate the contribution effort. By contrast, PACs not only allow an executive to shift the cost of spending on political contributions to shareholders, but more importantly, offer anonymity for the contributors, which is more relevant to our research question. When a fraud comes to the spotlight, politicians often refund prior contributions made by the accused executives.[5] PAC contributions, on the other hand, are less likely to be refunded due to the indirect link to the fraudulent executives. Arguably for this reason, Richter and Werner (2017) provide evidence that CEOs increase personal giving to specific candidates to substitute for their firm's linked PACs' inability to contribute to these candidates, suggesting that executives may prefer to contribute through their PAC over contribute directly from their own bank accounts.

In this section, we explore which contributions — PAC or individual — are more impactful in affecting sanctions. In Table A5, we split contributions into PAC and individual contributions and re-estimate our baseline regressions. We include the same set of control variables as used in the baseline regressions. In light of Richter and Werner (2017), whose findings suggest a potential substitution effect between the two contribution channels where individual contributions may serve as the second-best option, we control, additionally, the ratio of individual contributions over PAC contributions.

[5] "Why some campaign contributions get refunded." October 10, 2017. Opensecrets.org.

Table A5 reveals that our baseline results are largely driven by PAC contributions.[6] This implies that while fraudulent managers harvest personal benefits from political contributions, shareholders of their firms bear not only the damages from their fraud ex post, but also the cost of political contributions ex ante. Interestingly, for criminal penalties — which often are more detrimental to these executives personally than civil penalties, individual contributions are positively related to probation and prison terms. The differential effects between personal and PAC contributions on criminal penalties corroborate with the findings of Richter and Werner (2017), highlighting — in the context of our analysis — a potential reason for individual contributions being the second-best venue for these executives as well as providing support for our approach in measuring political contributions.

[6] The *F*-statistics testing the difference between the estimates associated with "*PAC Contributions*" and "*Individual Contributions*" in column 2 (officer ban) is 2.11, insignificant at the conventional level.

IA.4: Agency's Discretion in the Outcome

In the main text, we explore two potential channels through which political contributions may help fraudulent executives evade harsh sanctions. In this subsection, we consider another plausible mechanism. When cases are brought to a federal court, the SEC can propose sanctions, but the decision is ultimately left to the court. Often these cases are settled prior to court judgment, in which case the agency has greater control over the penalties. Likewise, cases prosecuted by the DOJ can either result in a court judgment or be settled by the agency through a plea bargain. Penalties determined by courts are usually significantly harsher.[7] In our sample, fraudulent executives face $7.87 million more in monetary penalty and 3.35 more years in prison if penalties are imposed by the court. *Ceteris paribus*, they would prefer settling with government agencies rather than going to the court.

To consider this distinction, which may affect the severity of a government penalty, we hand collect data on the resolution of the case from the SEC, the DOJ, and various other sources. We construct "*Settlement/Plea Bargain*", a dummy variable set to one when the penalty results from a settlement with the SEC or a plea bargain with the DOJ, and zero when the penalty is imposed by the court (either the judge or a jury). In instances where we could not find information as to whether the outcome of the case took the form of a court judgment or a settlement/plea bargain, we assume that a settlement/plea bargain was used. This assumption is based on the fact that the majority of our sample (56% of civil cases and 80% of criminal cases) is resolved through

[7] An example is Citigroup's proceedings. Citigroup and its executives were accused of deceiving investors by betting against more than $1 billion in mortgage-backed securities sold to investors. The SEC offered to settle with Citigroup in exchange for a $285 million penalty, an amount Judge Radkoff referred to as mere "pocket change". ("Appeals court delays SEC Citigroup fraud case", December 27, 2011, Reuters, and "For S.E.C., court ruling on penalties ties a hand", November 30, 2011, The New York Times). Following Judge Radkoff, in a separate matter Judge Rudolph T. Randa requested the SEC explain how the agency's proposed settlement with Koss Corporation was "fair, adequate, and in the public's interest" ("An S.E.C. fraud settlement questioned, gets approved", February 2, 2012, The New York Times).

a settlement or plea bargain. Obviously, it will bias against us finding significant results in cases where a judgment was actually made.

We regress our proxy for the extent of punishment, "*Severity*", on the dummy for settlement with the SEC or plea bargain with the DOJ as well as the control variables and fixed effects included in our base analysis. Columns 1 and 2 of Table A6 confirm that penalties are significantly harsher if they are determined by the courts in our sample, as the coefficient for the dummy variable "*Settlement/Plea Bargain*" is negative and is significant at the 1% level. This implies that all else equal, the accused executives would prefer the penalties set by the government agencies, i.e., a settlement or plea bargain, rather than by the court.

Next, we provide evidence consistent with political contributions affecting the severity of penalties by increasing the propensity to avoid court judgment. We decompose the dummy for settlement and plea bargain into two components: the predicted and residual values from regressing "*Settlement/Plea Bargain*" on "*PC*". By construction, the predicted component, "*Predicted Settlement/Plea Bargain*", captures the extent of the propensity to settle with government agencies that stems from the executive's political contributions. The residual component captures the variations in "*Settlement/Plea Bargain*" unrelated to political contributions.[8]

Columns 3 and 4 of Table A6 reveal that the coefficient for "*Predicted Settlement/Plea Bargain*" is negative and is significant at the 1% level. This suggests that, after controlling for various factors that may affect the severity of penalty, political contributions allow accused

[8] While the procedure that we follow is common in the literature, it is possible that political contributions also affect "*Settlement/Plea Bargain*" indirectly via latent variables. As such, the predicted component of "*Settlement/Plea Bargain*" may under-estimate the real effect of political contributions on the likelihood of settling with government agencies. Nevertheless, this works against finding the results, suggesting that our findings may quantify a lower bound of the extent to which avoiding court-assessed penalty through more political contributions mitigates the severity of penalty.

executive to receive less harsh, and certainly more desirable sanctions by settling with government agencies instead of going to the court.

While the results in Table A6 are consistent with political contributions allowing fraudulent executives to receive more lenient sanctions by settling with government agencies rather than going to court, we shall interpret these findings with caution due to the concern for potential selection. It is possible that the expected penalty from going to court is lower than settling because there is some chance that the manager will win. More importantly, disputes go to court when the parties have substantially different expectations of the outcome; as such, low penalties, settlement, and contributions can be all related to the type of information asymmetries that drive the process by which some disputes go to court.

IA.5: Additional Robustness

In untabulated regressions, we consider political contributions during the five years leading up to the first year of the fraud instead of during the fraud period, and raw contribution dollars instead of log-transformed. We find that our baseline results are invariant to these alternative ways of computing political contributions.

We also test whether industry classification affects our baseline findings (see, e.g., Kahle and Walkling, 1995). Instead of one-digit SIC code to classify a firm's industry, we use Fama-French 49 industries. Columns 1-6 of Table A7 reveal that our baseline results are robust to alternative ways of classifying industries. Using Fama-French 12 industries produces similar findings (untabulated).

Lastly, to take into account the fact that time-varying characteristics could drive both contributions and penalties, we replace industry- and settlement year-fixed effects by industry × year fixed effects. Columns 7-12 of Table A7 suggest that time-varying industry-specific characteristics are unlikely to explain our main findings. Nevertheless, given the small sample size, the inclusion of industry × year fixed effects might seek to identify empirical variation using a very small number of cases, leading to noisy estimators.

Additional Reference:

Kahle, K. and R. Walkling (1996). The Impact of Industry Classifications on Financial Research, *Journal of Financial and Quantitative Analysis* 31(3), 309-335.

Table A1: Political Contributions and the Bipartisan Campaign Reform Act

This table examines how the effect of political contributions on the severity of government penalty varies before and after the Bipartisan Campaign Reform Act (BCRA). The dependent variables indicated on top of each column. "*BCRA*" is a dummy variable set for one if the initiation of the SEC/DOJ investigation is after the enactment of the Bipartisan Campaign Reform Act and zero otherwise. The rest of the variables are defined in the Appendix. All the models include a constant, a set of control variables (executive age, market share, leverage, firm size, dummy for small firm, damages, number of accused executives, fraud duration, time to filing, and board independence) and fixed effects as described in the table, but coefficients are not tabulated. Industry is a firm's 1-digit SIC code. Robust standard errors clustered at the firm level are in square brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Monetary Penalty	Officer Ban	Dum(Criminal Investigation)	Probation	Prison	Severity
	(1)	(2)	(3)	(4)	(5)	(6)
PC × BCRA	**-0.45*****	**-0.91***	**-0.12**	**-1.66*****	**-2.74*****	**-0.59****
	[0.015]	**[0.494]**	**[0.130]**	**[0.109]**	**[0.063]**	**[0.284]**
BCRA	-0.15***	0.31	-0.11	13.66***	14.81***	-0.06
	[0.030]	[0.383]	[0.114]	[0.152]	[0.125]	[0.235]
PC	0.39***	0.45	0.04	0.57***	1.27***	0.29
	[0.015]	[0.493]	[0.128]	[0.109]	[0.063]	[0.293]
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Fraud Type FE	Yes	Yes	Yes	Yes	Yes	Yes
Executive Role FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Settlement Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	411	522	520	490	490	522
(Pseudo) R-squared	0.147	0.085	0.344	0.307	0.271	0.129

Table A2: Political Contribution and Government Sanctions – Entropy Balanced Matching

This table examines how political contributions affect the severity of government penalty using a Hainmueller (2012) entropy balanced matching sample. The dependent variables are indicated on top of each column. All the models include a constant, a set of control variables (executive age, market share, leverage, firm size, dummy for small firm, damages, number of accused executives, fraud duration, and time to filing) and fixed effects as described in the table, but coefficients are not tabulated. Variable definitions are in the Appendix. Industry is a firm's 1-digit SIC code. Robust standard errors clustered at the firm level are reported in square brackets. *, **, and *** indicate significance levels of 10%, 5%, and 1% respectively.

Dependent Variable	Monetary Penalty	Officer Ban	Dum(Criminal Investigation)	Probation	Prison	Severity
	(1)	(2)	(3)	(4)	(5)	(6)
PC	**-0.05*** **	**-0.55*** **	**-0.08** **	**-0.82*** **	**-0.56*** **	**-0.32*** **
	[0.017]	**[0.148]**	**[0.039]**	**[0.043]**	**[0.050]**	**[0.099]**
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Fraud Type FE	Yes	Yes	Yes	Yes	Yes	Yes
Executive Role FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Settlement Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	411	522	520	522	522	522
(Pseudo) R-squared	0.214	0.138	0.323	0.439	0.302	0.231

Table A3: Penalty Transfer and Executive Entrenchment – Entropy Balanced Matching

Panel A: Penalty Shift

This panel examines how the severity of penalty imposed on accused executives affects the fine their firm receives from the SEC using a Hainmueller (2012) entropy balanced matching sample. The linear probability regression estimates are reported in columns 1-2, and tobit regression estimates are in columns 3-6. The dependent variables are indicated on top of each column. We generate "*Predicted Severity*" by regressing "*Severity*" on "*PC*". All the models include a constant, a set of control variables (executive age, market share, leverage, firm size, dummy for small firm, damages, number of accused executives, fraud duration, and time to filing) and fixed effects as described in the table, but coefficients are not tabulated. Variable definitions are in the Appendix. Industry is a firm's 1-digit SIC code. Robust standard errors are clustered at the firm level and reported in square brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Dum(Firm Fine)		Firm Fine/All Fines		Firm Fine/Total Assets	
	(1)	(2)	(3)	(4)	(5)	(6)
Predicted Severity	**-0.05****	**-0.05****	**-0.12*****	**-0.10*****	**-0.19*****	**-0.13*****
	[0.021]	**[0.020]**	**[0.014]**	**[0.015]**	**[0.027]**	**[0.027]**
Delisted		-0.22***		-0.70***		-0.92***
		[0.061]		[0.044]		[0.058]
Class Action		0.14*		0.74***		1.43***
		[0.073]		[0.048]		[0.071]
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Fraud Type FE	Yes	Yes	Yes	Yes	Yes	Yes
Executive Role FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Settlement Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	522	522	522	522	522	522
(Pseudo) R-squared	0.372	0.424	0.412	0.493	0.355	0.421

Table A3 continued.

Panel B: Termination of Fraudulent Executive

This panel reports the linear probability regression estimates relating predicted firm fine on the likelihood of job termination for a fraudulent executive using a Hainmueller (2012) entropy balanced matching sample. The dependent variable is "*Executive Termination*", a dummy set to one for termination of the fraudulent executive, and zero otherwise. We generate "*Predicted Firm Fine*" by regressing "*Firm Fine*" on "*PC*". All the models include a constant, a set of control variables (executive age, market share, leverage, firm size, dummy for small firm, damages, number of accused executives, fraud duration, and time to filing) and fixed effects as described in the table, but coefficients are not tabulated. Variable definitions are in the Appendix. Industry is a firm's 1-digit SIC code. Robust standard errors are clustered at the firm level and reported in square brackets. *, **, and *** indicate significance levels of 10%, 5%, and 1% respectively.

Dependent Variable	Executive Termination			
	(1)	(2)	(3)	(4)
Predicted Firm Fine	**-0.64****	**-0.62****	**-0.69***	**-0.60***
	[0.312]	**[0.298]**	**[0.350]**	**[0.341]**
Executive Ownership	-0.32	-0.50	-0.34	-0.50
	[0.384]	[0.336]	[0.392]	[0.338]
Delisted			-0.04	-0.01
			[0.082]	[0.086]
Class Action			-0.03	0.03
			[0.126]	[0.115]
Control Variables	Yes	Yes	Yes	Yes
Fraud Type FE	Yes	Yes	Yes	Yes
Executive Role FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Settlement Year FE	Yes	Yes	Yes	Yes
Observations	473	473	473	473
(Pseudo) R-squared	0.202	0.349	0.204	0.349

Table A4: Penalty Transfer and Executive Entrenchment – Abadie-Imbens Matching

Panel A: Penalty Shift

This panel relates fraudulent executives that receive low severity to firm fines using an Abadie-Imbens (2006) matched sample. The dependent variable in columns 1-2 is a dummy variable equal to one if the firm receiving a fine from the SEC and zero otherwise, is *"Firm Fine/All Fines"* in columns 3-4, and "*Firm Fine/Total Assets*" in columns 5-6. "*Dummy for Contributors with Low Severity*" is a dummy variable set to one if an executive contributes politically and "*Predicted Severity*", which is obtained by regressing "*Severity*" on "*PC*", is less than the median "*Predicted Severity*". The set of matching variables in each column mirrors the corresponding model in Table 10 Panel A with exact and nearest neighbor matches as described in the table. Industry is a firm's 1-digit SIC code. Variable definitions are in the Appendix. Robust standard errors are clustered at the firm level and reported in square brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Dum(Firm Fine)		Firm Fine/All Fines		Firm Fine/TA	
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy for Contributor with Low Severity	**0.15****	**0.13***	**0.11****	**0.11****	**0.22***	**0.21***
	[0.069]	**[0.071]**	**[0.046]**	**[0.047]**	**[0.119]**	**[0.109]**
Exact Matches:	Industry × Year, Executive Role					
Nearest Neighbor Matches:	Executive Age, Market Share, Leverage, Damages, Number of Executives, Fraud Length, Time to Filing, Fraud Types					
Additional Exact Matches:		Class Action		Class Action		Class Action
Additional Nearest Neighbor Matches:		Delisted		Delisted		Delisted
Number of matches:	3	3	3	3	3	3
Observations	522	522	522	522	522	522

Table A4 continued.

Panel B: Executive Entrenchment

This panel relates the firm fine that has been skewed upward due to political contributions by the fraudulent executive with the likelihood that the executive is terminated using an Abadie-Imbens (2006) matched sample. The dependent variable is "*Executive Termination*", a dummy set to one for termination of the fraudulent executive, and zero otherwise. "*Dummy for Contributor with Increased Firm Fine*" is a dummy variable set to one when fraudulent executives contribute politically, and when the "*Predicted Firm Fine*", which is obtained by regressing "*Firm Fine / All Fines*" on "*PC*", is greater than the median "*Predicted Firm Fine*". The set of matching variables in each column mirrors the corresponding model in Table 10 Panel B with exact and nearest neighbor matches as described in the table. Industry is a firm's 1-digit SIC code. Variable definitions are in the Appendix. Robust standard errors are clustered at the firm level and reported in square brackets. *, **, and *** indicate significance levels of 10%, 5%, and 1% respectively.

Dependent Variable	Executive Termination			
	(1)	(2)	(3)	(4)
Dummy for Contributor with Increased Firm Fine	**-0.16***** **[0.052]**	**-0.16***** **[0.052]**	**-0.15***** **[0.050]**	**-0.15***** **[0.051]**
Exact Matches:	Industry × Year, Executive Role			
Nearest Neighbor Matches:	Executive Age, Executive ownership, Market Share, Leverage, Damages, Number of Executives, Fraud Length, Time to Filing, Fraud Types			
Additional Exact Matches:			Class Action	Class Action
Additional Nearest Neighbor Matches:		Delisted		Delisted
Number of matches:	3	3	3	3
Observations	473	473	473	473

Table A5: PAC versus Individual Contributions

This table relates political contributions to the severity of government penalties, considering contributions at firm level and individual executive level. "*PAC Contributions*" is the natural logarithm of one plus the average annual amount of the firm's PAC contributions during the period of fraud. "*Individual Contributions*" is the natural logarithm of one plus the average annual amount of political contributions made by the accused executive. The dependent variables are indicated at the top of each column. All the models include a constant, a set of control variables (executive age, market share, leverage, firm size, dummy for small firm, damages, number of accused executives, fraud duration, time to filing, and the ratio of individual over PAC contributions), and fixed effects as described in the table, but coefficients are not tabulated. Industry is a firm's 1-digit SIC code. Variable definitions are in the Appendix or in the text. Robust standard errors are clustered at the firm level and reported in square brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Monetary Penalty	Officer Ban	Dum(Criminal Investigation)	Probation	Prison	Severity
	(1)	(2)	(3)	(4)	(5)	(6)
PAC Contributions	**-0.06*** **	**-0.34** **	**-0.08***	**-0.85*** **	**-0.72*** **	**-0.28*** **
	[0.013]	**[0.159]**	**[0.040]**	**[0.074]**	**[0.056]**	**[0.089]**
Individual Contributions	**-0.06**	**-1.33***	**0.02**	**0.89*** **	**1.73*** **	**-0.16**
	[0.045]	**[0.697]**	**[0.143]**	**[0.221]**	**[0.200]**	**[0.450]**
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Fraud Type FE	Yes	Yes	Yes	Yes	Yes	Yes
Executive Role FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Settlement Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	411	522	520	522	522	522
(Pseudo) R-squared	0.140	0.0847	0.323	0.212	0.215	0.127

Table A6: Agency Discretion in the Outcome

This table presents the ordered probit regression results examining how the effect of political contributions on the severity of government enforcement varies with a government agency's discretion. The dependent variable is "*Severity*", a variable equal to 5 if both prison and officer ban are imposed, 4 for prison term, 3 for an officer ban, 2 for probation, 1 if there is monetary penalty, and zero if no penalty is imposed. "*Settlement/Plea Bargain*" is a dummy variable equal to one if the penalty results from a settlement with the SEC and/or plea bargain with the DOJ, and zero if the penalty is imposed by the court (either the judge or a jury). "*Predicted Settlement/Plea Bargain*" is the predicted value from regressing "*Settlement/Plea Bargain*" on "*PC*". Control variables (identical to those in Table 2) and fixed effects (described in the table) are included in estimations, but coefficients are not tabulated. Industry is a firm's 1-digit SIC code. Variable definitions are in the Appendix. Robust standard errors are clustered at the firm level and reported in square brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Severity			
	(1)	(2)	(3)	(4)
Settlement/Plea Bargain	**-1.84*** **	**-1.82*** **		
	[0.287]	**[0.291]**		
Predicted Settlement/Plea Bargain			**-0.83*** **	**-0.82*** **
			[0.129]	**[0.131]**
Delisted		0.16		0.16
		[0.139]		[0.139]
Class Action		0.20		0.20
		[0.154]		[0.154]
Control Variables	Yes	Yes	Yes	Yes
Fraud Type FE	Yes	Yes	Yes	Yes
Executive Role FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Settlement Year FE	Yes	Yes	Yes	Yes
Observations	522	522	522	522
Pseudo R-squared	0.148	0.150	0.148	0.150

Table A7: Alternative Industry Classifications

This table relates political contributions to the severity of government penalties considering different ways to control the effect of industry. The dependent variables are indicated at the top of each column. In columns 1-6, industry fixed effects are based on the Fama-French 49 industries. In columns 7-12, we include industry × year fixed effect, where industry is a firm's 1-digit SIC code. Control variables (identical to those in Table 2) and fixed effects (described in the table) are included in estimations, but coefficients are not tabulated. Variable definitions are in the Appendix. Robust standard errors are clustered at the firm level and reported in square brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Monetary Penalty	Officer Ban	Dum(Criminal Investigation)	Probation	Prison	Severity	Monetary Penalty	Officer Ban	Dum(Criminal Investigation)	Probation	Prison	Severity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
PC	**-0.03****	**-0.40****	**-0.07***	**-0.98*****	**-0.71*****	**-0.27*****	**-0.07*****	**-0.30*****	**-0.10***	**-1.11*****	**-0.85*****	**-0.29****
	[0.012]	**[0.156]**	**[0.042]**	**[0.071]**	**[0.046]**	**[0.095]**	**[0.013]**	**[0.023]**	**[0.053]**	**[0.043]**	**[0.033]**	**[0.145]**
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fraud Type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Executive Role FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No	No
Industry × Year FE	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Settlement Year FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No	No
Observations	411	522	515	522	522	522	411	522	496	522	522	522
(Pseudo) R-squared	0.180	0.0981	0.367	0.292	0.263	0.160	0.221	0.149	0.462	0.400	0.377	0.227