Online Appendix
Making Bribery Profitable Again? The Market Effects of Halting Extraterritorial Accountability for Overseas Bribery

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A Describing repeated FCPA actions

We use Stanford FCPA Clearinghouse data to describe the frequency of repeated actions for past FCPA targets, to substantiate our claim that FCPA histories beget future FCPA risks.

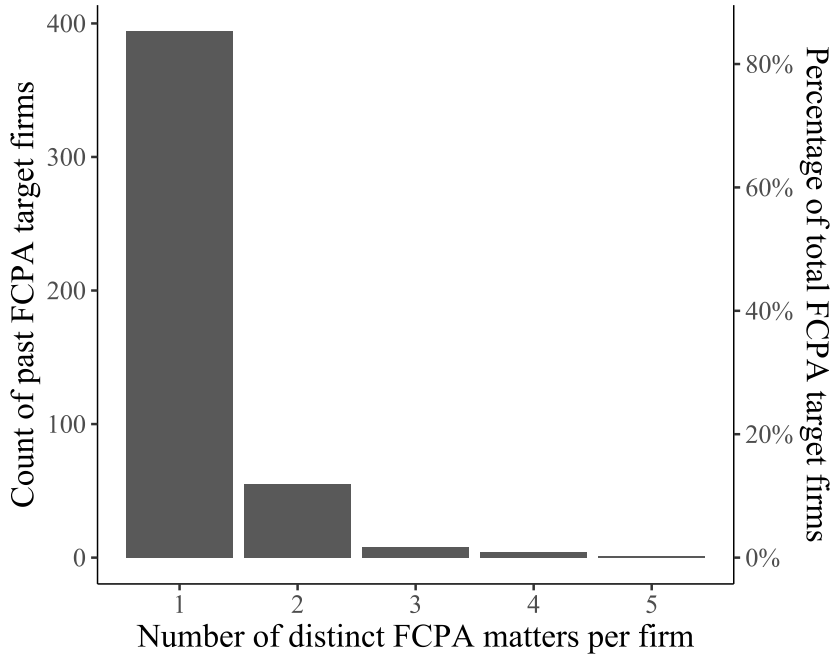

FIGURE A.1: Distribution of the number of distinct FCPA matters that publicly traded and privately owned FCPA targets were involved in

We first describe the number of distinct "FCPA matters" that each FCPA target has been involved in. An FCPA matter is defined by the FCPA Clearinghouse as a distinct (alleged) bribery case. Bribery cases can be significantly complex: they can be articulated in multiple enforcement actions or investigations and can branch into connected schemes that are uncovered as legal actions proceed. This classification thus allows us to distinguish entirely distinct corruption affairs involving the same firm. However, this simplification comes at the cost of ignoring repeated enforcement actions and investigations that occur over multiple years—for instance, because new information on connected bribery schemes emerges—and belong to the same FCPA matter. The number of FCPA matters thus provides us with a lower bound value, and a first approximation, of the repeated exposure of FCPA target firms to FCPA law enforcers.

About 15% of the 462 publicly traded or privately owned[1] firms that have experienced an FCPA action (in the form of an investigation, enforcement, or both) have been involved in more than one FCPA matter. Figure A.1 reports the distribution of the number of distinct FCPA matters that these 462 firms have been involved in. A repeated FCPA matter is not an unlikely outcome for an FCPA target. At the extreme, some firms have been involved in up to five distinct FCPA matters to date. Considering that distinct FCPA matters do not typically emerge because a given investigation, legal action, or unrelated event provides additional information branching into additional anti-bribery actions (these instances would appear under the same

[1] Because in this analysis we are not necessarily concerned with stock market behavior, here we do not limit our focus to publicly traded companies.

FCPA matter), these are remarkable numbers. They provide evidence on a first crucial factor that we advanced in our theory: many past FCPA target firms have an underlying high risk of exposure to bribery, and consequently to future FCPA actions, due to their "type"—for instance, because of the industry they operate in, or their foreign countries of operation, or their corporate culture. Because of such features, firms that are tainted with FCPA histories are likely to experience FCPA actions again in the future.

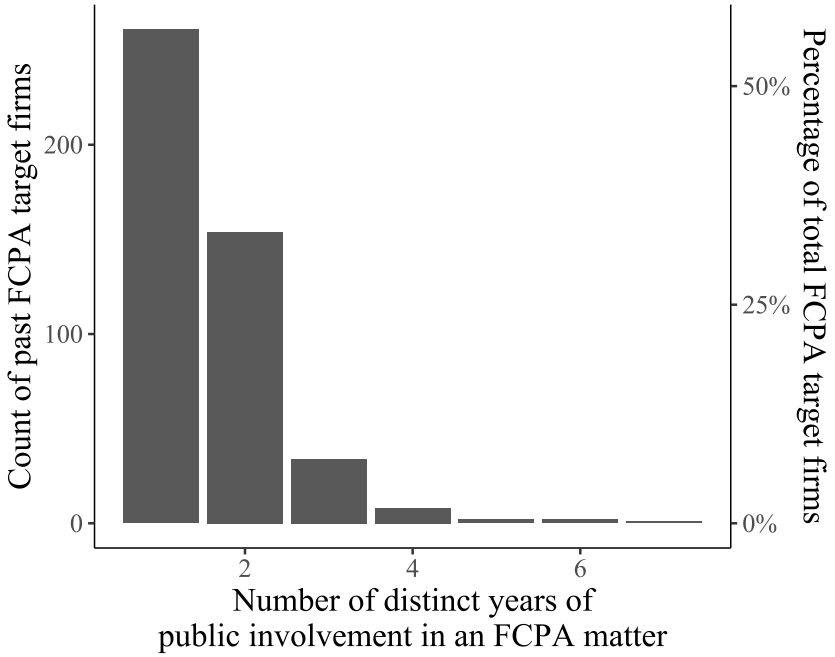

FIGURE A.2: Distribution of the number of distinct years of public involvement in FCPA investigations or enforcement actions for publicly traded and privately owned FCPA targets

By aggregating multiple investigations and enforcement actions under the same FCPA matter, however, these figures conceal significant variation in repeated exposure of past FCPA targets to future FCPA actions. As such, they might even *underrepresent* the extent to which an FCPA past begets future FCPA risk. To better describe that, we first present the number of distinct years in which each past FCPA target has been under the spotlight of FCPA law enforcers, whether under the same FCPA matter or not. We measure this as the number of unique years in which an FCPA target has experienced a publicly disclosed investigation or enforcement action for an (alleged) FCPA violation.

Figure A.2 reports the distribution of our measure for the number of unique years of public involvement in FCPA investigations or enforcement actions for past FCPA target firms. Almost half (44%) of the FCPA targets to date have been involved in publicly disclosed enforcement actions or investigations on repeated occasions, for a minimum of two and a maximum of seven distinct years. This extreme value is assumed by *ABB Limited*, a Swiss multinational electrical engineering corporation which has been involved in a total of four distinct FCPA matters, with investigations and enforcement actions occurring over the years 2003, 2004, 2007, 2009, 2017, 2019, and 2022. Firms that have been under the public FCPA spotlight for six or five years are: *Eni S.p.A.*, an Italian oil major (six years), *Pfizer Inc.*, a US pharmaceutical and biotech multinational (six years), *Halliburton*, a US multinational oilfield service provider (five years),

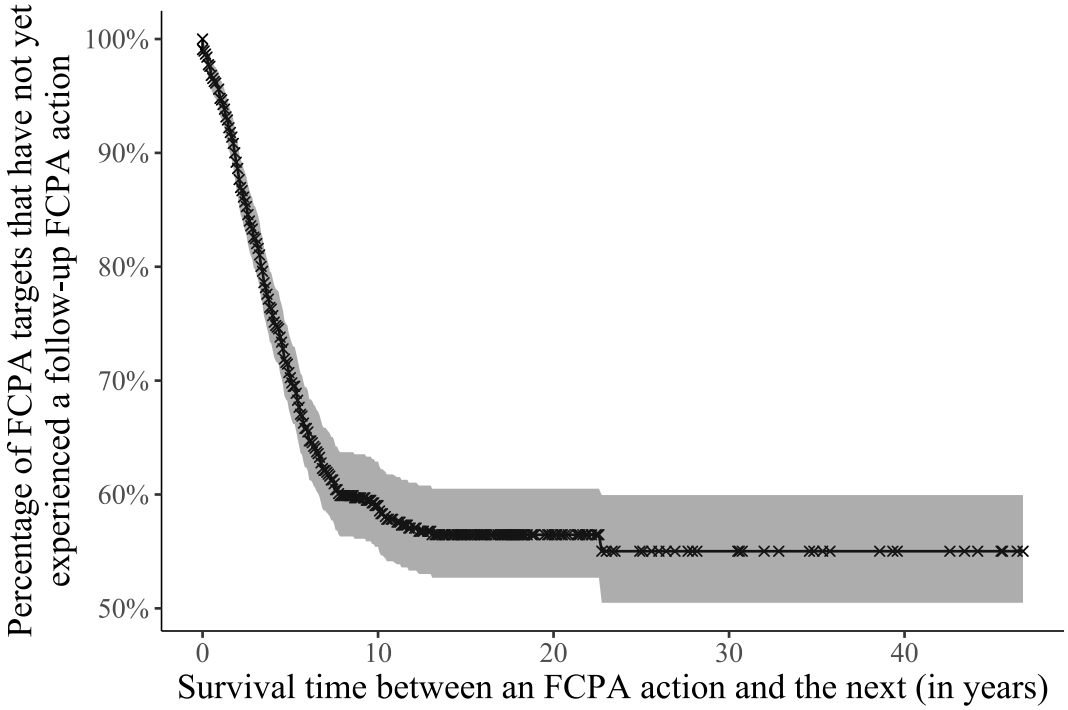

FIGURE A.3: Survival rate of past FCPA targets (publicly traded or privately owned) to a follow-up FCPA action (enforcement or investigation) as time passes. Results from a Kaplan-Meier estimation with no covariates. 95% CIs displayed as shaded area

and *Johnson & Johsnon*, another large US multinational pharmaceutical and biotechnology firm (five years).

We conclude with a simple Kaplan Meier survival analysis to describe the time it takes for an FCPA target to end up again under the spotlight of FCPA law enforcers (either for an enforcement action or an investigation) after an FCPA action. We use FCPA Clearinghouse data to create a data structure where each row represents a unique date in which an FCPA target (publicly traded or not) has been involved in a publicly disclosed investigation or enforcement action. Next, we measure the distance, in months, between the date of the observed investigation or enforcement action and the next, for the same firm. We code cases of "survival" (i.e., those for which an investigation or enforcement action has not yet been followed up with an FCPA action against the same firm) with a binary STATUS indicator and, for them, code the month distance to the date of Trump's executive order (Feb 10, 2025)—the assumption being that no further action will follow for the following 180 days, so we stop our analysis to this day.

Figure A.3 presents the "survival" rate of past FCPA targets to a follow-up FCPA action, as time passes. Results come from a simple Kaplan Meier estimation with no covariates, which we intend purely as a way to describe how the percentage of "surviving" past FCPA targets—i.e., the percentage of firms that have not yet experienced a follow-up action—decreases with time. About 30% of the FCPA targets experience a follow-up enforcement action or investigation within five years from the previous one. After 10 years since the past FCPA event, only about 59% of the past FCPA targets have yet to experience a follow-up action. Confirming what we described earlier, the long-term survival rate of past FCPA targets—i.e., the percentage of FCPA targets that have yet to experience a follow-up action—is about 56%.

Taken together, evidence presented in this section confirms that firms with an FCPA history are at significant risk of future FCPA actions, either for entirely unrelated corruption matters (likely due to firm "type") or because availability of information makes it more likely that follow-up actions on related corruption events will occur.

B FCPA histories and reputational risk rating

Here, we substantiate our claim that FCPA histories cause market analysts to assess firms as reputationally more risky investments. We collected data about market analysts' assessment of firms' exposure to governance risk. We use data from *RepRisk*, a Swiss market analyst that produces an index of firms' reputational risk: the RRI. The RRI ranges from 0 (lowest risk) to 100 (highest risk). The score is built by processing information on firms' respect for various environmental, social, and governance criteria. The RRI is a widely used reputational index, featuring in the analyses made by key investment actors such as *FactSet*, *UBS*, *Dow Jones*, and *JP Morgan*. By focusing on this index, thus, we study a metric that many market analysts evaluate, too, when investing. We obtained RRI data for all firms in our dataset for whom information is available via WRDS (311 companies). Because RepRisk provides information for academic usage with a lag, we are only able to obtain data between January 01, 2007 (when RepRisk began its coverage) and December 31, 2023.

We begin with a cross-sectional analysis where we compare the RRI for past FCPA targets to that for matched placebo firms. We use this analysis to show that past FCPA targets are generally classified as more risky investments than comparable firms without an FCPA history, likely due to the chances of repeated FCPA actions described in Appendix A. Table B.1 shows the results of a linear regression of the latest RRI score available (that on Dec 31, 2023) for the firms in our dataset which report such information. We explain this outcome variable with a binary indicator for whether the firm is a past FCPA target (= 1) or a matched placebo firm (= 0). In column 1, we consider all past FCPA targets for whom RRI information is available. In column 2, we exclude the past FCPA targets that have not been matched to placebo firms due to missing covariates (see Appendix C).

Results indicate that past FCPA targets are classified as more risky investments than comparable firms without an FCPA history. The reputational risk increases by about 4.0 or 4.2 points, on average, over the mean value for matched placebo firms (14.8). That is, market analysts associate past FCPA target firms with a reputational risk that is, on average, 28% higher than for comparable firms. Because this analysis considers the score as of the end of December 2023, in some cases, this difference is observed even years after firms have been FCPA targets.

Can we attribute such differences to the FCPA enforcement itself? In order to answer this question, we move from a cross-sectional analysis to an event-study difference-in-differences. We compare the evolution of past FCPA targets' RRI to that experienced by matched placebo firms, by considering the period before and after their first FCPA enforcement action. Equation 1 represents the model. Because past FCPA targets experience an FCPA action at staggered times, we include a two-way firm and date fixed-effect (α_i and γ_t) and dummies for the relative time-to-treatment with reference point being the day before the FCPA action. The binary variable FCPA indicates whether a firm i is a past FCPA target, r indicates the relative time-to-treatment, and t indexes the date.

Table B.1: Cross-sectional difference in RepRisk reputational risk score for past FCPA targets and matched placebo firms

	All past targets	Only matched targets
	(1)	(2)
Past FCPA target	4.044*	4.235*
	(1.783)	(1.794)
(Intercept)	14.822*	14.822*
	(1.081)	(1.081)
Std.Errors	White-robust	White-robust
Num.Obs.	311	309
R2	0.017	0.018
R2 Adj.	0.014	0.015

* p < 0.05
Linear regression models of the latest available score (Dec 31 2023) proposed by RepRisk to measure reputational risk (RRI). Higher values indicate higher reputational risk. Past FCPA targets and matched placebo firms are considered. "Past FCPA target" is a binary taking value of 1 for firms that have been targets of an FCPA investigation or enforcement action in the past. Heteroskedasticity White-robust standard errors in parentheses.

$$\text{RRI}_{it} = \alpha_i + \gamma_t + \sum_{r \neq t_e - 1} \delta_r \times 1[\text{FCPA}_i = 1] \times 1[t = r + 1] + \varepsilon_{it} \tag{1}$$

We report dynamic ATT results (estimated δ_r), when considering the 30 days before and after the first FCPA enforcement action, in Figure B.1. Each estimate represents by how much more (or less) the RRI changes, between a given day and the day before the FCPA action, for the average FCPA target *vs* for the average placebo firm. We present results when considering the full sample of past FCPA targets (top panel) and when limiting treated firms only to matched placebos (bottom panel). Firms that are "treated" with an FCPA enforcement action increase their RRI, in the days immediately following the enforcement, significantly more than "control" matched placebo firms. The RRI increases by about 7.5 points, on average. In the days leading up to the enforcement, instead, we observe no significant trend, which lends support to the identifying "parallel trends assumption" of a difference-in-differences design.

To confirm that these short-term effects inform long-term differences in reputational scores detected in the cross-sectional analysis of Table B.1, we replicate the event-study by considering the full year before and after the enforcement action (Figure B.2). Even long after the FCPA enforcement action, we observe a significantly larger increase in risk after FCPA enforcement, for firms that experienced it, than for control firms. Estimated ATTs inform us that, one year after the FCPA action, the RRI of an FCPA target has increased by about 4 points more, since the day before the action, than it did for comparable placebo firms. This long-term effect is comparable to what we detected in Table B.1, thus confirming that cross-sectional differences in risk between FCPA targets and similar firms are likely induced by the FCPA enforcement itself.

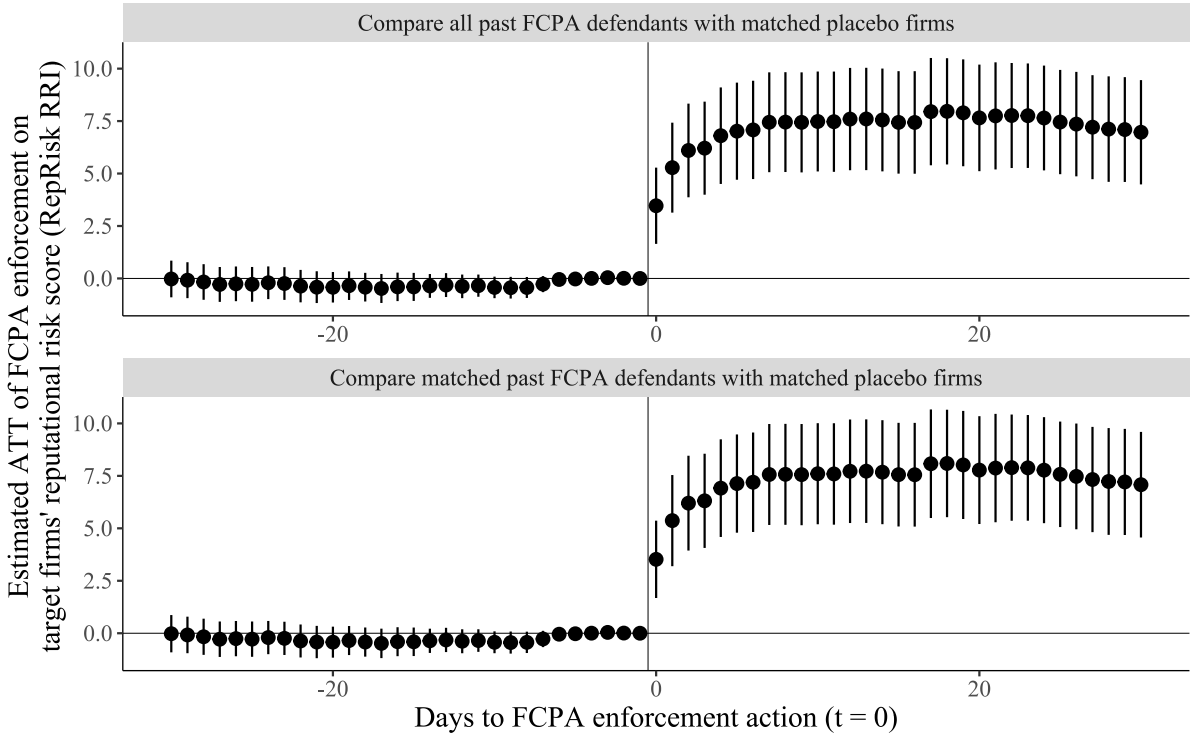

FIGURE B.1: Difference-in-differences event analysis of firms' RRI following an FCPA enforcement action (1 month). 95% CIs displayed as vertical bars

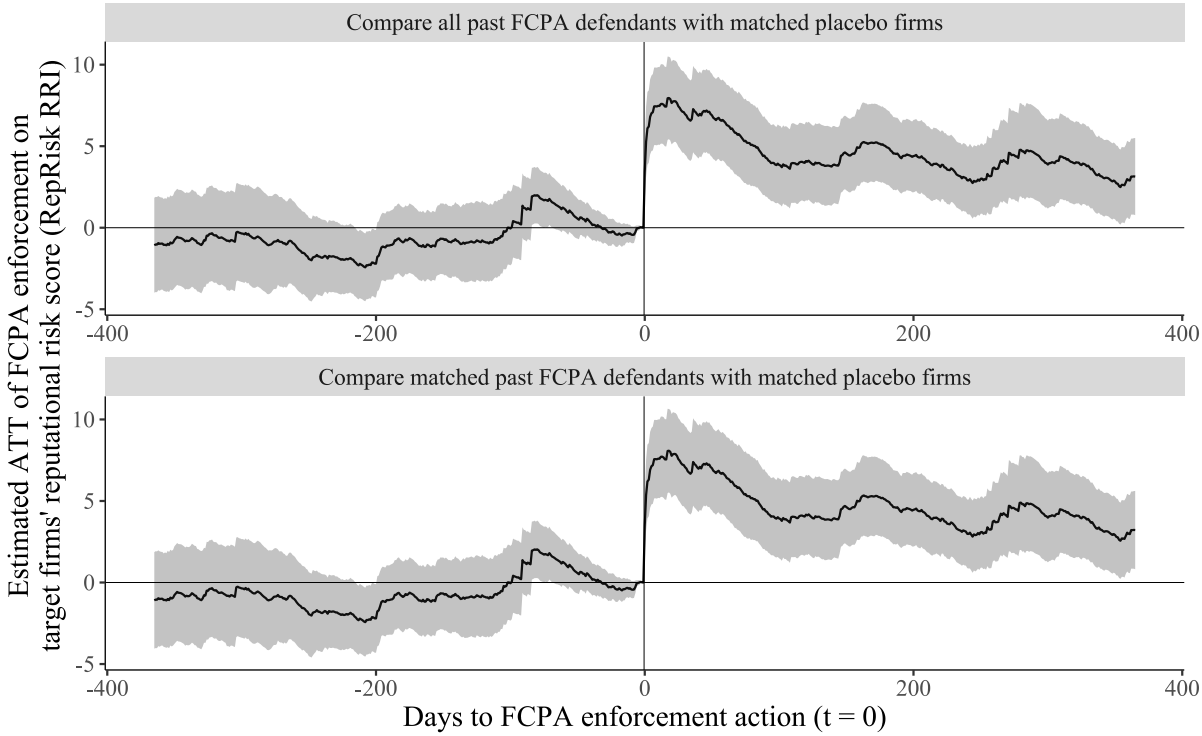

FIGURE B.2: Difference-in-differences event analysis of firms' RRI following an FCPA enforcement action (1 year). 95% CIs displayed as shaded area

C Constructing a placebo set of non-FCPA targets with matching

We constructed a placebo set of non-FCPA targets comparable to past FCPA targets, starting from Compustat firms trading securities in North America. We dropped past FCPA targets. Next, we kept only firms reporting information on covariates of interest (listed below) in the final quarter of the 2024 fiscal year. These selections left us with a pool of 3,408 potential placebo candidate firms.

C.1 Main matching procedure: Propensity score matching

Many of these firms are significantly different from past FCPA targets. To select companies representing a similar portfolio to that of past FCPA targets, we relied on nearest-neighbor matching on the propensity score (PS). We considered the following covariates. First, measures for the firms' activity via subsidiaries. We drew on data from firms' SEC filings (via WRDS) to count the number of foreign countries each firm is present in; the number of subsidiaries in the US; the number of subsidiaries in foreign countries; and the number of overall subsidiaries. Because this information is reported only until 2022, this group of covariates refers to this year only. Next, we considered the following financial fundamentals: whether the firm is an *S&P 500* constituent; the value of total assets; the number of common outstanding shares; and the average stock price at closing over the month of December 2024. We then considered categorical indicators representing which exchange the firm trades securities on (either of: NYSE, NASDAQ, or over-the-counter). And, finally, the 2-digit industry indicator from the North American Industry Classification System (NAICS-2). All these covariates were drawn from Compustat (via WRDS). Only 240 of the past FCPA targets report information on these covariates, so we created a matched sample based on this (slightly smaller) group of firms.

We used propensity score to match the one nearest neighbor potential placebo firm to each of the 240 past FCPA targets for whom we have covariate information, based on the covariates listed above (we match without replacement). The resulting set included 240 placebo firms. Limited trading data for four of the matched placebo firms further restricts this group from 240 to 236 units.

The matched portfolio of placebo firms is significantly similar to the sample of past FCPA targets. Figure C.1 shows standardized mean differences between the sample of past FCPA targets and the matched firms. When compared to the broader sample of potential placebo firms (crosses), past FCPA targets operate in more foreign countries, with more foreign and overall subsidiaries (they have a similar number of US subsidiaries, instead). Past FCPA targets are also more represented among *S&P 500* constituents, have more outstanding shares, and larger asset value. They are more likely to be trading on the NYSE and less likely to trade on the NASDAQ, reflecting their more traditional business sectors of operations. Finally, they are more represented in the primary metal manufacturing industry (NAICS-2 code 33) and less represented in finance and insurance (NAICS-2 code 52). After selecting a limited group of placebo firms with propensity score matching (solid dots), instead, standardized mean differences are more muted and remain within an acceptable range (between -0.25 and $+0.25$). This means that past FCPA targets and selected placebo firms are comparable with respect to their foreign operations, market value and financials, and sectors of operations. Because several of these factors largely increase the firm's exposure to foreign corruption, these two groups are now largely comparable. As reported above, the matched set is based on a subset of the

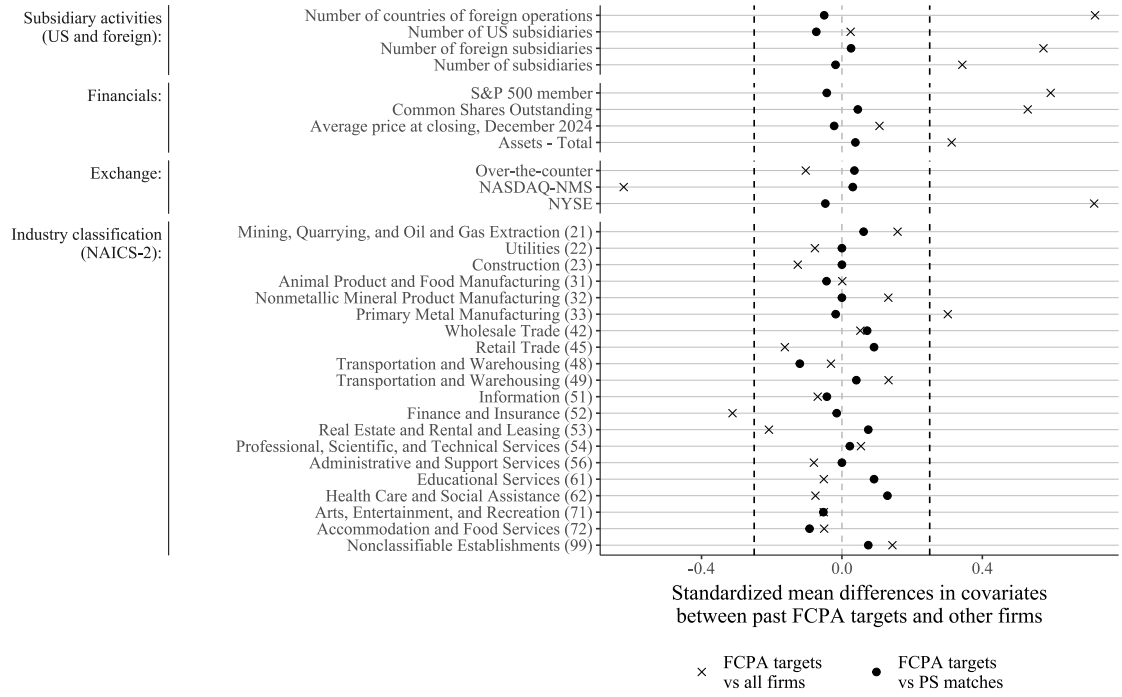

FIGURE C.1: Standardized mean difference in covariates between the sample of past FCPA targets and the matched placebo sample of non-FCPA targets selected with propensity scores (PS) matching

past FCPA targets that report covariate information. In Table I.3, we show that our results are substantively the same if we limit the analysis only to this subset of past FCPA target firms.

C.2 Alternative matching: Coarsened exact matching and entropy balancing

Here, we document how we obtained two alternative placebo sets of non-FCPA targets using alternative matching techniques to PS. We retain PS matching as our primary technique for selecting suitable placebo firms for our analysis, given that we do not use this simple method as our main research design—its purpose is, here, solely to identify a group of firms that could constitute a placebo sample, for which we replicate our event study. However we note that, in recent years, the accuracy of PS as a matching technique has been questioned, with studies showing that it increases imbalance in covariates (King and Nielsen, 2019).

We thus applied two techniques that do not suffer from the shortcomings of PS to construct alternative placebo sets. In Appendix K, we show that we obtain similar results to those of our main text when considering firms in these samples. Our first alternative is coarsened exact matching (CEM, Iacus, King, and Porro, 2012), which does not suffer from PS' shortcomings as it approximates block randomization. Second, entropy balancing (Hainmueller, 2012), an alternative method which is doubly robust to mis-specifications of the outcome and treatment assignment equations (Zhao and Percival, 2017). Differently from selection methods like PS or CEM, entropy balancing is a *reweighting* procedure: it does not select a subgroup of the untreated units to be considered as a comparable control group; rather, it associates each untreated unit to a weight, based on the selected covariates, which generates a balanced comparison between treatment and control group.

We performed CEM and entropy balancing with respect to the same covariates used for PS, described above. For CEM, we matched the one nearest neighbor placebo firm (based on Mahalanobis distance) to each past FCPA target, within each stratum, without replacement. Because CEM discards treated firms without a nearest neighbor in the same stratum, with this technique we match 146 past FCPA targets to 146 placebo firms. With entropy balancing, instead, we compare our set of past FCPA targets to the full set of 3,408 control firms, each associated with its own weight.

Resulting balance in covariates is reported in Figure C.2. Entropy balancing achieves by far the closest balance, returning a placebo group that is very similar to that of past FCPA targets across all observable covariates considered. CEM also performs rather well, particularly with the share of firms by industry codes and trading market, but achieves a more imbalanced placebo group when looking at the value of total assets—past FCPA targets have about $29.555B average asset value, while CEM-constructed placebos $18.185B.

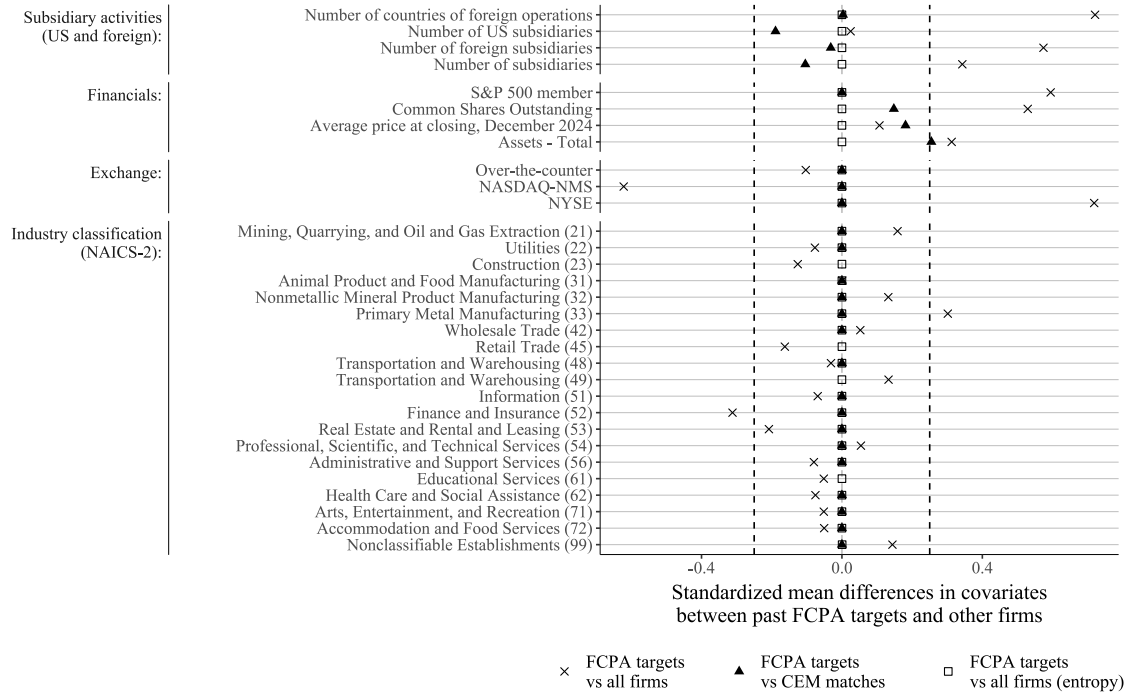

FIGURE C.2: Standardized mean difference in covariates between the sample of past FCPA targets and the placebo sample of non-FCPA targets selected with coarsened exact matching (CEM) or weighted with entropy balancing

Even though entropy weighting achieved a significantly more balanced placebo set than the one selected by propensity score, we stick to PS for our main analysis because it simplifies our next steps substantially (while still achieving a satisfactory balance, see Figure C.1). Otherwise, we would have to estimate one model per each of the 3,408 placebo firms. Considering that we are estimating 15 models per each firm, that would amount to $3{,}408 * 15 = 51{,}120$ models in our estimation stage, just for the placebo firms. That notwithstanding, in Appendix K, we show that our main results do not hinge on the choice of matching technique.

D Estimation window goodness-of-fit

Here, we report goodness-of-fit results from our estimation window phase for the several options of market models we considered. We estimated 15 market models for each firm under study, namely 12 LASSO models using *S&P 500* constituents and 3 OLS models using the aggregate S&P 500 index. Limited trading data for four of the matched placebo firms further restricts this group from 240 to 236 units. Given that we considered 261 past FCPA targets and 236 matched placebos, that amounts to $(261 + 236) * 15 = 7{,}455$ models. The twelve LASSO market models per firm result from the combination of three possible estimation window lengths (starting 30, 90, and 180 trading days before the executive order) and four possible cross-validation (CV) folds (3, 5, 10, and 15). Because OLS does not use CV, the three OLS market models only vary over estimation window lengths (30, 90, and 180 days).

Figure D.1 reports the distribution of market models' R^2 for past FCPA targets. A vertical line indicates the average R^2 for a given type of model. The best performing model, on average, was a LASSO with an estimation window starting 30 days before the executive order and 15 folds of cross-validation, achieving a notable average R^2 of 0.673. For LASSO models, on average, shorter estimation windows and more CV folds yielded better fitting models. LASSO models outperform OLS significantly. The worst performing LASSO model (estimation window starting 180 days before the executive order and 3-fold-CV), on average, yielded an R^2 of 0.445, four times as large as that of the better fitting OLS (one with the same estimation window length, 0.111).

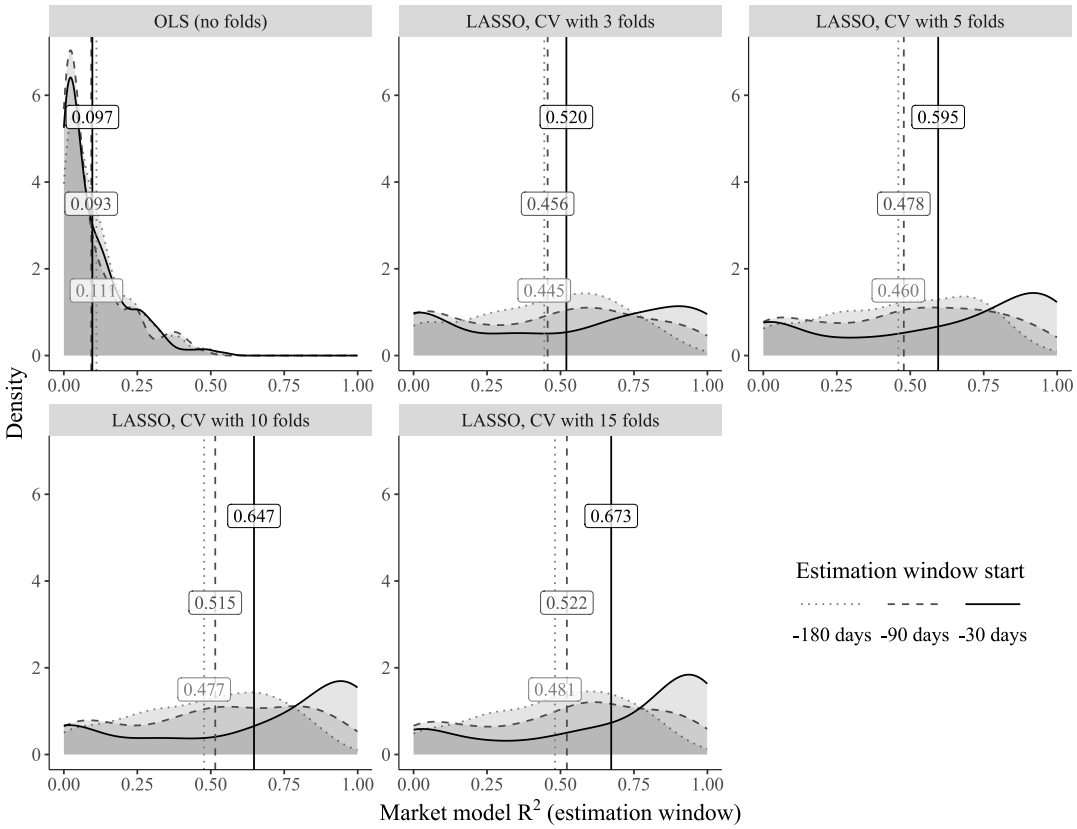

FIGURE D.1: Distribution of R^2 from market models of past FCPA targets

Given their superior fit, we picked the LASSO market models with an estimation window starting 30 days before the executive order and 15-fold CV as our preferred choice. In Appendix H, we show similar results when varying estimation window lengths and CV choices.

In Figure D.2, we show the corresponding distribution of R^2 in the matched sample of placebo firms that were never subject to an FCPA action. In this case, too, the shorter estimation window and higher CV folds yield better fitting models and the LASSO outperforms OLS. As a further confirmation that the matched placebo sample offers a good term of comparison for past FCPA targets, we note that the distributions of R^2 for these placebo firms resemble very closely those obtained for past FCPA targets.

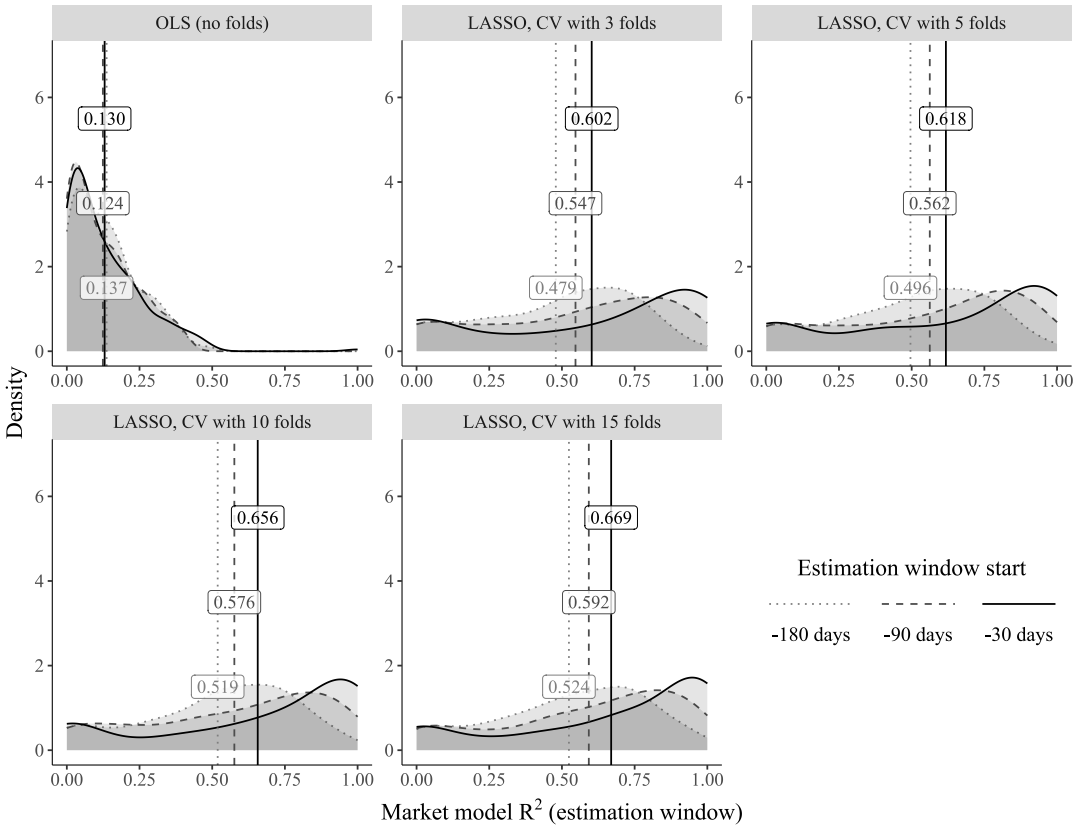

FIGURE D.2: Distribution of R^2 from market models of matched placebo

E Descriptive statistics

Table E.1 reports basic descriptives of the relevant variables: the number of outstanding shares, the daily price at closing, firms' RETURNS, AR, and CAR. We limit our data to the five trading days before and after the executive order.

F Full disclosure of main text results

We present the full estimates underlying Figure 3 in the main text in Table F.1. Columns 5 and 6 also report daily differences-in-means (computed as t-tests) between the averages in columns 1 and 3, and 2 and 4, respectively.

Table E.1: Descriptive statistics of relevant variables

	N	Mean	SD	Min	P25	Median	P75	Max
Outstanding shares (B)	4770	0.914	1.995	0.004	0.071	0.237	0.813	15.728
Closing price ($)	4965	100.745	160.321	0.883	17.690	48.440	119.560	1887.300
RETURNS (%)	4965	0.021	2.694	-37.500	-1.079	0.000	1.153	37.546
AR (%)	4965	-0.033	2.906	-44.093	-1.023	-0.040	1.032	36.129
CAR (%)	2482	0.211	4.527	-25.173	-1.668	0.185	2.147	45.080

Table F.1: The effect of Trump's FCPA halt on firms' AR and CAR

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.406*		-0.569*		0.163	
	(0.079)		(0.088)		(0.118)	
Tue, Feb 04 2025	0.576*		0.035		0.541	
	(0.164)		(0.294)		(0.337)	
Wed, Feb 05 2025	0.326		0.308		0.019	
	(0.167)		(0.185)		(0.249)	
Thu, Feb 06 2025	-0.199		0.022		-0.220	
	(0.272)		(0.211)		(0.344)	
Fri, Feb 07 2025	-0.244		-0.853*		0.609*	
	(0.147)		(0.183)		(0.234)	
Post-event:						
Mon, Feb 10 2025	0.690*	0.690*	0.141	0.141	0.550*	0.550*
	(0.140)	(0.140)	(0.163)	(0.163)	(0.215)	(0.215)
Tue, Feb 11 2025	0.013	0.704*	-0.658*	-0.518*	0.672*	1.221*
	(0.114)	(0.172)	(0.181)	(0.237)	(0.214)	(0.292)
Wed, Feb 12 2025	-0.132	0.576*	-0.205	-0.722*	0.073	1.299*
	(0.204)	(0.264)	(0.192)	(0.307)	(0.280)	(0.406)
Thu, Feb 13 2025	0.371*	0.948*	0.521*	-0.202	-0.149	1.149*
	(0.181)	(0.337)	(0.183)	(0.336)	(0.257)	(0.476)
Fri, Feb 14 2025	-0.073	0.875*	-0.467*	-0.669	0.394	1.543*
	(0.189)	(0.397)	(0.199)	(0.367)	(0.274)	(0.541)
N of firms	261	261	236	236	497	497

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

We present the full estimates underlying Figure 4 in Table F.2. Columns 5 and 6 also report daily differences-in-means (computed as t-tests) between the averages in columns 1 and 3, and 2 and 4, respectively.

Table F.2: Heterogeneous effect on past FCPA targets by whether the investigation is still ongoing

	Ongoing investigations		Non-ongoing investigations		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.415		-0.405*		-0.010	
	(0.259)		(0.082)		(0.272)	
Tue, Feb 04 2025	0.371		0.586*		-0.214	
	(0.372)		(0.171)		(0.409)	
Wed, Feb 05 2025	0.473		0.319		0.154	
	(0.479)		(0.173)		(0.509)	
Thu, Feb 06 2025	-0.081		-0.205		0.124	
	(1.368)		(0.278)		(1.396)	
Fri, Feb 07 2025	-1.074		-0.204		-0.870	
	(0.564)		(0.151)		(0.584)	
Post-event:						
Mon, Feb 10 2025	0.594*	0.594*	0.695*	0.695*	-0.101	-0.101
	(0.247)	(0.247)	(0.146)	(0.146)	(0.287)	(0.287)
Tue, Feb 11 2025	0.742	1.337*	-0.022	0.673*	0.764	0.663
	(0.555)	(0.624)	(0.116)	(0.178)	(0.567)	(0.648)
Wed, Feb 12 2025	0.417	1.754*	-0.158	0.519	0.575	1.234
	(0.401)	(0.872)	(0.213)	(0.274)	(0.454)	(0.914)
Thu, Feb 13 2025	1.889*	3.643*	0.298	0.817*	1.591	2.826*
	(0.846)	(1.240)	(0.184)	(0.346)	(0.866)	(1.287)
Fri, Feb 14 2025	0.591	4.234*	-0.105	0.712	0.697	3.523
	(0.855)	(1.806)	(0.194)	(0.405)	(0.877)	(1.851)
N of firms	12	12	249	249	261	261

* $p < 0.05$

Average AR and CAR to past FCPA targets under ongoing investigations and not per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

G Effect sizes on market capitalization

In Table G.1, we report the estimated AR and gains in capitalization for the 50 past FCPA target firms that recorded the highest abnormal gains on the day of the executive order. Some targets that paid sizable penalties for past FCPA violations (e.g., *Walmart*, *Exxon*, *Siemens AG*, *Abbott*, *Equinor*, *Toyota*...) recorded gains in the order of (tens of) billions of dollars in market capitalization.

Figure G.1 shows the full distribution of estimated changes in market capitalization for past FCPA targets on the day of Trump's FCPA executive order. Past FCPA targets recorded an

Table G.1: Top 50 estimated effect sizes of the FCPA Executive Order among past FCPA targets

Rank	Company name	Ticker	Shares (M)	Price	AR	Capitalization gain (M)
1	WALMART INC	WMT	8,024.000	$101.150	1.458%	$11,836.929
2	MCDONALD'S CORP	MCD	715.200	$294.300	5.247%	$11,043.175
3	MICROSOFT CORP	MSFT	7,435.000	$409.750	0.359%	$10,951.405
4	UBER TECHNOLOGIES INC	UBER	2,107.953	$74.600	5.135%	$8,074.203
5	EXXON MOBIL CORP	XOM	4,353.000	$108.890	1.162%	$5,506.541
6	QUALCOMM INC	QCOM	1,106.000	$167.960	2.217%	$4,118.158
7	ALLIANZ SE	ALIZF	385.919	$318.450	3.233%	$3,972.763
8	ROCKWELL AUTOMATION	ROK	113.073	$268.400	12.270%	$3,723.829
9	SIEMENS AG	SIEGY	1,569.738	$107.298	1.920%	$3,234.245
10	ABBOTT LABORATORIES	ABT	1,731.698	$129.070	1.247%	$2,787.029
11	EQUINOR ASA	EQNR	2,944.733	$23.550	3.848%	$2,668.785
12	ACCENTURE PLC	ACN	626.445	$385.980	1.078%	$2,605.615
13	DEERE & CO	DE	271.414	$465.600	1.964%	$2,481.559
14	LOCKHEED MARTIN CORP	LMT	234.000	$444.390	2.331%	$2,423.815
15	TOYOTA MOTOR CORP	TM	1,309.591	$183.980	0.932%	$2,246.717
16	DEUTSCHE TELEKOM	DTEGY	4,978.615	$34.044	1.271%	$2,154.581
17	LINDE PLC	LIN	473.237	$455.050	0.845%	$1,819.225
18	HONEYWELL INTERNATIONAL INC	HON	649.800	$205.520	1.317%	$1,758.807
19	HSBC HLDGS PLC	HSBC	3,583.641	$53.100	0.833%	$1,584.616
20	NOVARTIS AG	NVS	1,975.089	$106.260	0.750%	$1,573.424
21	NEWMONT CORP	NEM	1,127.000	$44.840	3.092%	$1,562.334
22	CHEVRON CORP	CVX	1,769.012	$152.620	0.540%	$1,457.650
23	BHP GROUP LTD	BHP	2,535.137	$50.350	0.965%	$1,231.854
24	SAP SE	SAP	1,166.589	$279.640	0.359%	$1,170.642
25	SCHLUMBERGER LTD	SLB	1,400.850	$40.160	2.031%	$1,142.322
26	JOHNSON CONTROLS INTL PLC	JCI	660.594	$87.650	1.913%	$1,107.892
27	MEDTRONIC PLC	MDT	1,283.266	$90.010	0.938%	$1,082.899
28	FEDEX CORP	FDX	239.585	$256.080	1.692%	$1,038.390
29	NORTHROP GRUMMAN CORP	NOC	144.952	$468.580	1.446%	$982.120
30	TENARIS SA	TS	554.429	$38.010	4.612%	$971.976
31	BOSTON SCIENTIFIC CORP	BSX	1,474.556	$105.250	0.618%	$958.678
32	TE CONNECTIVITY PLC	TEL	298.766	$146.880	2.046%	$897.982
33	BAKER HUGHES CO	BKR	989.646	$46.200	1.886%	$862.168
34	ASTRAZENECA PLC	AZN	3,101.092	$71.990	0.359%	$801.477
35	PETROLEO BRASILEIRO SA- PETR	PBR	6,444.366	$13.720	0.869%	$768.365
36	EDWARDS LIFESCIENCES CORP	EW	588.600	$70.350	1.806%	$747.623
37	STELLANTIS NV	STLA	2,880.492	$12.940	1.909%	$711.469
38	BROOKFIELD ASSET MANAG LTD	BAM	1,630.525	$57.910	0.702%	$662.699
39	PFIZER INC	PFE	5,667.000	$25.740	0.429%	$625.081
40	MONDELEZ INTERNATIONAL INC	MDLZ	1,317.829	$58.450	0.787%	$605.822
41	GOLD FIELDS LTD	GFI	895.024	$18.680	3.547%	$592.950
42	HP INC	HPQ	944.660	$32.270	1.906%	$581.135
43	GRAINGER (W W) INC	GWW	48.333	$1,035.800	1.139%	$569.996
44	HALLIBURTON CO	HAL	868.000	$25.190	2.587%	$565.628
45	OCCIDENTAL PETROLEUM CORP	OXY	938.458	$46.780	1.239%	$543.768
46	ABB LTD	ABBNY	1,838.192	$54.632	0.495%	$497.487
47	GSK PLC	GSK	2,040.409	$36.040	0.663%	$487.400
48	L3HARRIS TECHNOLOGIES INC	LHX	189.795	$206.130	1.169%	$457.441
49	JOHNSON & JOHNSON	JNJ	2,406.922	$153.120	0.124%	$455.266
50	BANK OF NEW YORK MELLON CORP	BK	717.680	$86.150	0.735%	$454.521

average gain of $160M and an overall gain of $39B. The picture illustrates that several firms recorded gains in the orders of (tens of) billions of dollars.

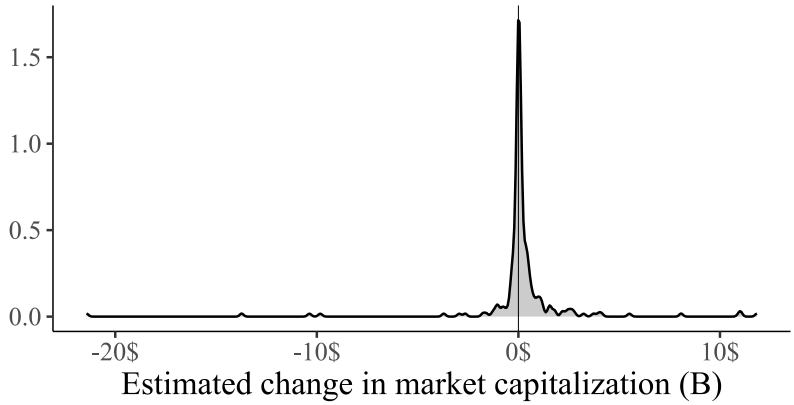

FIGURE G.1: Estimated market gains for firms under study on the day of Trump's FCPA Executive Order

H Robustness to estimation choices

H.1 Change number of LASSO CV-folds

In Tables H.1, H.2, and H.3, we re-estimate our results when varying the number of CV folds employed (we consider 10, 5, and 3 folds, respectively). Estimation windows considered in this section all start 30 days before the executive order, as in our main results. Regardless of the CV hyperparameter choices, we find a significant and positive effect on the day of the executive order on past FCPA targets' AR, comparable to that presented in the main text (ranging from about 0.631 to 0.717 percentage points). The effects on CAR during the trading week are also significant. Consistent with evidence in the main text, we find pre-event significant effects on February 4th and 5th, generally undistinguishable from comparable significant effects experienced by the sample of matched placebo firms. We find no effect on AR and CAR linked to the FCPA Executive Order for the placebo sample.

H.2 Change estimation window length

In Tables H.4 and H.5, we re-estimate our results for both samples of firms when varying the length of the estimation windows considered (estimation windows start, respectively, 90 and 180 trading days before the executive order in the two tables). LASSO market models all employed 15-fold cross-validation, as in our main text. We find positive and significant effects of the executive order on past FCPA targets' AR and CAR, regardless of whether we use an estimation window starting 90 or 180 days before the executive order. Effects are comparable to those detected in the main text, and we note that, with longer estimation windows, CAR effects at the end of the trading week are substantially larger. When considering matched placebo firms that have never been FCPA targets, we detect no effect linked to the FCPA Executive Order. In the previous week, we detected similar statistically significant effects on February 4th and

Table H.1: Varying CV folds of LASSO market models (10 folds)

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.433*		-0.601*		0.167	
	(0.083)		(0.104)		(0.133)	
Tue, Feb 04 2025	0.517*		0.326*		0.191	
	(0.163)		(0.165)		(0.231)	
Wed, Feb 05 2025	0.355*		0.403*		-0.048	
	(0.165)		(0.177)		(0.242)	
Thu, Feb 06 2025	-0.248		0.204		-0.452	
	(0.268)		(0.209)		(0.340)	
Fri, Feb 07 2025	-0.296*		-0.712*		0.415*	
	(0.145)		(0.147)		(0.207)	
Post-event:						
Mon, Feb 10 2025	0.683*	0.683*	0.050	0.050	0.634*	0.634*
	(0.143)	(0.143)	(0.144)	(0.144)	(0.203)	(0.203)
Tue, Feb 11 2025	-0.015	0.668*	-0.610*	-0.561*	0.595*	1.229*
	(0.114)	(0.174)	(0.181)	(0.230)	(0.214)	(0.289)
Wed, Feb 12 2025	-0.157	0.515	-0.288	-0.848*	0.130	1.363*
	(0.204)	(0.267)	(0.184)	(0.291)	(0.275)	(0.395)
Thu, Feb 13 2025	0.320	0.835*	0.469*	-0.379	-0.149	1.214*
	(0.190)	(0.345)	(0.187)	(0.334)	(0.266)	(0.480)
Fri, Feb 14 2025	-0.088	0.747	-0.305	-0.684	0.217	1.431*
	(0.186)	(0.404)	(0.165)	(0.374)	(0.248)	(0.551)
N of firms	261	261	236	236	497	497

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 10-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

Table H.2: Varying CV folds of LASSO market models (5 folds)

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.540*		-0.685*		0.145	
	(0.093)		(0.097)		(0.135)	
Tue, Feb 04 2025	0.546*		0.172		0.374	
	(0.163)		(0.234)		(0.285)	
Wed, Feb 05 2025	0.364*		0.397*		-0.034	
	(0.169)		(0.177)		(0.244)	
Thu, Feb 06 2025	-0.168		0.056		-0.223	
	(0.271)		(0.198)		(0.335)	
Fri, Feb 07 2025	-0.395*		-0.777*		0.382	
	(0.144)		(0.153)		(0.211)	
Post-event:						
Mon, Feb 10 2025	0.631*	0.631*	0.199	0.199	0.432*	0.432*
	(0.140)	(0.140)	(0.150)	(0.150)	(0.205)	(0.205)
Tue, Feb 11 2025	-0.031	0.600*	-0.592*	-0.392	0.561*	0.992*
	(0.111)	(0.168)	(0.175)	(0.229)	(0.207)	(0.284)
Wed, Feb 12 2025	-0.099	0.505*	-0.318	-0.710*	0.219	1.216*
	(0.203)	(0.256)	(0.187)	(0.285)	(0.276)	(0.383)
Thu, Feb 13 2025	0.306	0.811*	0.571*	-0.139	-0.265	0.950*
	(0.178)	(0.327)	(0.190)	(0.323)	(0.261)	(0.460)
Fri, Feb 14 2025	-0.112	0.700	-0.284	-0.423	0.173	1.123*
	(0.184)	(0.391)	(0.175)	(0.376)	(0.254)	(0.542)
N of firms	261	261	236	236	497	497

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 5-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

Table H.3: Varying CV folds of LASSO market models (3 folds)

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.662*		-0.678*		0.016	
	(0.092)		(0.108)		(0.142)	
Tue, Feb 04 2025	0.617*		0.423*		0.194	
	(0.166)		(0.155)		(0.227)	
Wed, Feb 05 2025	0.490*		0.535*		-0.044	
	(0.181)		(0.200)		(0.269)	
Thu, Feb 06 2025	-0.345		0.229		-0.574	
	(0.264)		(0.208)		(0.336)	
Fri, Feb 07 2025	-0.353*		-0.728*		0.375	
	(0.146)		(0.153)		(0.211)	
Post-event:						
Mon, Feb 10 2025	0.717*	0.717*	0.039	0.039	0.678*	0.678*
	(0.133)	(0.133)	(0.150)	(0.150)	(0.200)	(0.200)
Tue, Feb 11 2025	0.057	0.773*	-0.674*	-0.635*	0.731*	1.409*
	(0.107)	(0.163)	(0.174)	(0.226)	(0.204)	(0.279)
Wed, Feb 12 2025	-0.142	0.636*	-0.319	-0.954*	0.177	1.590*
	(0.201)	(0.254)	(0.192)	(0.293)	(0.278)	(0.388)
Thu, Feb 13 2025	0.411*	1.047*	0.628*	-0.326	-0.217	1.373*
	(0.175)	(0.323)	(0.185)	(0.331)	(0.255)	(0.463)
Fri, Feb 14 2025	-0.006	1.041*	-0.261	-0.587	0.255	1.628*
	(0.183)	(0.383)	(0.169)	(0.371)	(0.249)	(0.533)
N of firms	261	261	236	236	497	497

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 3-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

5th as those documented in the main text. These effects are statistically comparable to those experienced by past FCPA targets.

Table H.4: Varying estimation window length of LASSO market models (start 90 days pre-Executive Order)

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.481*		-0.540*		0.059	
	(0.097)		(0.099)		(0.139)	
Tue, Feb 04 2025	0.795*		0.329*		0.466*	
	(0.156)		(0.154)		(0.219)	
Wed, Feb 05 2025	0.377*		0.350*		0.028	
	(0.166)		(0.170)		(0.238)	
Thu, Feb 06 2025	-0.105		0.332		-0.437	
	(0.240)		(0.199)		(0.312)	
Fri, Feb 07 2025	-0.251		-0.510*		0.259	
	(0.144)		(0.148)		(0.206)	
Post-event:						
Mon, Feb 10 2025	0.689*	0.689*	0.107	0.107	0.581*	0.581*
	(0.140)	(0.140)	(0.149)	(0.149)	(0.205)	(0.205)
Tue, Feb 11 2025	0.196	0.885*	-0.527*	-0.420	0.724*	1.305*
	(0.103)	(0.165)	(0.174)	(0.225)	(0.202)	(0.279)
Wed, Feb 12 2025	0.060	0.911*	-0.236	-0.656*	0.296	1.568*
	(0.194)	(0.243)	(0.179)	(0.292)	(0.264)	(0.380)
Thu, Feb 13 2025	0.097	1.008*	0.453*	-0.203	-0.356	1.212*
	(0.184)	(0.303)	(0.184)	(0.337)	(0.260)	(0.453)
Fri, Feb 14 2025	0.061	1.069*	-0.259	-0.462	0.319	1.531*
	(0.176)	(0.351)	(0.162)	(0.361)	(0.239)	(0.504)
N of firms	261	261	236	236	497	497

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 90 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

Table H.5: Varying estimation window length of LASSO market models (start 180 days pre-Executive Order)

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.391*		-0.438*		0.047	
	(0.099)		(0.099)		(0.140)	
Tue, Feb 04 2025	0.748*		0.367*		0.381	
	(0.144)		(0.155)		(0.211)	
Wed, Feb 05 2025	0.487*		0.496*		-0.009	
	(0.153)		(0.170)		(0.228)	
Thu, Feb 06 2025	-0.151		0.320		-0.472	
	(0.229)		(0.195)		(0.301)	
Fri, Feb 07 2025	-0.231		-0.511*		0.280	
	(0.149)		(0.140)		(0.205)	
Post-event:						
Mon, Feb 10 2025	0.667*	0.667*	0.099	0.099	0.567*	0.567*
	(0.136)	(0.136)	(0.146)	(0.146)	(0.199)	(0.199)
Tue, Feb 11 2025	0.102	0.769*	-0.496*	-0.396	0.598*	1.166*
	(0.106)	(0.161)	(0.166)	(0.213)	(0.197)	(0.267)
Wed, Feb 12 2025	0.156	0.899*	-0.038	-0.434	0.194	1.334*
	(0.197)	(0.258)	(0.171)	(0.277)	(0.261)	(0.378)
Thu, Feb 13 2025	0.098	0.998*	0.332	-0.102	-0.234	1.099*
	(0.183)	(0.322)	(0.210)	(0.330)	(0.278)	(0.461)
Fri, Feb 14 2025	0.060	1.058*	-0.209	-0.310	0.269	1.368*
	(0.172)	(0.364)	(0.160)	(0.358)	(0.235)	(0.510)
N of firms	261	261	236	236	497	497

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 180 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

H.3 OLS market models

In Table H.6, we re-estimate our results for both samples of firms when using OLS to estimate market models over the estimation window. In keeping with the idea of selecting the best-fitting market models, even within this choice that yields very poor goodness-of-fit statistics (see Figures D.1 and D.2), we select market models with estimation windows starting 180 days before the executive order. Even with this suboptimal choice of models yielding noisy counterfactual RETURNS, we detect a positive and significant effect on firms' AR on the day of the executive order. The matched sample, instead, experiences no comparable positive effect.

H.4 Estimation window stops one day before the executive order

In Table H.7, we re-estimate our results for both samples of firms using the LASSO with 15-fold CV, and estimation windows starting 30 trading days before the executive order and stopping the day before. That is, we completely absorb the information provided by the Bondi memo in the market models that provide each firm's counterfactual RETURNS. Results are comparable to what was presented above.

I Robustness to exclusion of firms

I.1 Exclude firms with poorly estimated counterfactual

In Table I.1, we exclude firms whose market models result in R^2 smaller than 0.01 (visible in the left tails of densities from Figures D.1 and D.2). These are 33 firms from the sample of past FCPA targets and 28 firms from the matched placebo sample. We find comparable results, among this restricted sample, to those presented in the main text.

I.2 Jackknife test: exclude one firm at a time

In Figure I.1, we exclude one firm at a time and recompute average AR on the day of the executive order for past FCPA targets and the placebo firms, to show that our results do not depend on the inclusion of any single outlier firm. Regardless of the exclusion of any one past FCPA target, the average AR on the day of the executive order is always positive, statistically significant, and comparable to the full-sample estimate (red dot) for this group of firms. Likewise, regardless of the exclusion of any single firm from the sample of matched placebos, the average AR on the day of the executive order is never statistically significant for comparable firms that have never been FCPA targets.

I.3 Exclude top-10 gainers from executive order

In Table I.2, we replicate our analysis after excluding the top 10 past FCPA targets by gains in market capitalization (tickers: WMT, MCD, MSFT, UBER, XOM, QCOM, ALIZF, ROK, SIEGY, ABT). These firms recorded outlier market valuation gains of at least $2.8B, so a fair question might be whether including them significantly drives the positive effect we detect in our main text. In fact, we find that our results are significant to their exclusion, although obviously they become somewhat smaller. This suggests that the gain in market capitalization was experienced by the set of past FCPA targets more broadly, and was not limited to these individual firms that experienced record-high valuations.

Table H.6: Using OLS for estimation of market models

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.535*		-0.586*		0.051	
	(0.118)		(0.120)		(0.168)	
Tue, Feb 04 2025	0.374*		-0.091		0.465*	
	(0.152)		(0.153)		(0.216)	
Wed, Feb 05 2025	0.311*		0.345*		-0.034	
	(0.157)		(0.160)		(0.224)	
Thu, Feb 06 2025	-0.592*		-0.082		-0.510	
	(0.250)		(0.200)		(0.320)	
Fri, Feb 07 2025	0.053		-0.171		0.224	
	(0.138)		(0.134)		(0.192)	
Post-event:						
Mon, Feb 10 2025	0.416*	0.416*	-0.301*	-0.301*	0.717*	0.717*
	(0.142)	(0.142)	(0.152)	(0.152)	(0.207)	(0.207)
Tue, Feb 11 2025	0.172	0.589*	-0.488*	-0.789*	0.660*	1.377*
	(0.102)	(0.169)	(0.167)	(0.221)	(0.196)	(0.278)
Wed, Feb 12 2025	-0.252	0.318	-0.395*	-1.183*	0.143	1.501*
	(0.200)	(0.250)	(0.179)	(0.270)	(0.268)	(0.368)
Thu, Feb 13 2025	0.088	0.406	0.266	-0.917*	-0.178	1.323*
	(0.168)	(0.300)	(0.173)	(0.310)	(0.241)	(0.431)
Fri, Feb 14 2025	0.140	0.546	-0.117	-1.034*	0.257	1.579*
	(0.169)	(0.352)	(0.161)	(0.338)	(0.233)	(0.488)
N of firms	261	261	236	236	497	497

* $p < 0.05$

Average AR and CAR to past FCPA targets per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 180 days before FCPA Executive Order and ends 5 days before it. Market models estimated using OLS and aggregate S&P 500 index as predictor. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

Table H.7: Estimation windows end one day before Executive Order

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.459*		-0.621*		0.162	
	(0.082)		(0.101)		(0.130)	
Tue, Feb 04 2025	0.350*		0.236*		0.114	
	(0.104)		(0.102)		(0.146)	
Wed, Feb 05 2025	0.079		0.179		-0.100	
	(0.101)		(0.102)		(0.143)	
Thu, Feb 06 2025	-0.259		0.004		-0.263	
	(0.185)		(0.126)		(0.224)	
Fri, Feb 07 2025	-0.199		-0.476*		0.276	
	(0.119)		(0.092)		(0.151)	
Post-event:						
Mon, Feb 10 2025	0.637*	0.637*	0.061	0.061	0.577*	0.577*
	(0.146)	(0.146)	(0.142)	(0.142)	(0.204)	(0.204)
Tue, Feb 11 2025	-0.019	0.619*	-0.682*	-0.621*	0.663*	1.240*
	(0.113)	(0.176)	(0.174)	(0.222)	(0.207)	(0.283)
Wed, Feb 12 2025	-0.123	0.467	-0.294	-0.915*	0.171	1.383*
	(0.202)	(0.260)	(0.182)	(0.286)	(0.271)	(0.387)
Thu, Feb 13 2025	0.238	0.706*	0.400*	-0.515	-0.162	1.221*
	(0.190)	(0.339)	(0.181)	(0.328)	(0.262)	(0.472)
Fri, Feb 14 2025	-0.078	0.627	-0.311	-0.827*	0.233	1.454*
	(0.182)	(0.393)	(0.164)	(0.363)	(0.245)	(0.535)
N of firms	261	261	236	236	497	497

* $p < 0.05$

Average AR and CAR to past FCPA targets per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days before FCPA Executive Order and ends 1 day before it. Market models estimated using LASSO and individual S&P 500 constituents as predictor, selected using 15-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

Table I.1: Excluding firms with poorly-estimated counterfactuals

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.411*		-0.491*		0.081	
	(0.075)		(0.080)		(0.110)	
Tue, Feb 04 2025	0.621*		-0.077		0.697	
	(0.152)		(0.329)		(0.363)	
Wed, Feb 05 2025	0.319		0.333		-0.015	
	(0.180)		(0.201)		(0.270)	
Thu, Feb 06 2025	0.009		0.051		-0.042	
	(0.235)		(0.225)		(0.325)	
Fri, Feb 07 2025	-0.193		-0.791*		0.597*	
	(0.134)		(0.191)		(0.233)	
Post-event:						
Mon, Feb 10 2025	0.538*	0.538*	0.190	0.190	0.349	0.349
	(0.130)	(0.130)	(0.171)	(0.171)	(0.214)	(0.214)
Tue, Feb 11 2025	-0.023	0.515*	-0.641*	-0.451	0.618*	0.967*
	(0.116)	(0.177)	(0.196)	(0.250)	(0.228)	(0.306)
Wed, Feb 12 2025	-0.008	0.507	-0.150	-0.601	0.142	1.109*
	(0.227)	(0.294)	(0.174)	(0.317)	(0.286)	(0.432)
Thu, Feb 13 2025	0.426*	0.933*	0.390*	-0.211	0.035	1.144*
	(0.200)	(0.373)	(0.195)	(0.354)	(0.279)	(0.514)
Fri, Feb 14 2025	-0.051	0.883*	-0.295	-0.506	0.244	1.388*
	(0.206)	(0.439)	(0.195)	(0.384)	(0.283)	(0.583)
N of firms	228	228	208	208	436	436

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Excludes firms with market models having R^2 smaller than 0.01. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

Table I.2: Excluding 10 past FCPA targets realizing extreme gains

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.407*		-0.569*		0.162	
	(0.082)		(0.088)		(0.120)	
Tue, Feb 04 2025	0.579*		0.035		0.544	
	(0.170)		(0.294)		(0.339)	
Wed, Feb 05 2025	0.385*		0.308		0.077	
	(0.170)		(0.185)		(0.251)	
Thu, Feb 06 2025	-0.263		0.022		-0.285	
	(0.278)		(0.211)		(0.349)	
Fri, Feb 07 2025	-0.303*		-0.853*		0.550*	
	(0.150)		(0.183)		(0.236)	
Post-event:						
Mon, Feb 10 2025	0.581*	0.581*	0.141	0.141	0.441*	0.441*
	(0.135)	(0.135)	(0.163)	(0.163)	(0.212)	(0.212)
Tue, Feb 11 2025	0.004	0.586*	-0.658*	-0.518*	0.663*	1.103*
	(0.117)	(0.169)	(0.181)	(0.237)	(0.215)	(0.291)
Wed, Feb 12 2025	-0.117	0.473	-0.205	-0.722*	0.088	1.195*
	(0.211)	(0.268)	(0.192)	(0.307)	(0.285)	(0.408)
Thu, Feb 13 2025	0.332	0.804*	0.521*	-0.202	-0.189	1.006*
	(0.186)	(0.343)	(0.183)	(0.336)	(0.261)	(0.480)
Fri, Feb 14 2025	-0.055	0.749	-0.467*	-0.669	0.412	1.418*
	(0.196)	(0.408)	(0.199)	(0.367)	(0.279)	(0.549)
N of firms	251	251	236	236	487	487

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Excludes the top 10 past FCPA targets by gains in market capitalization on the day of the Executive Order. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

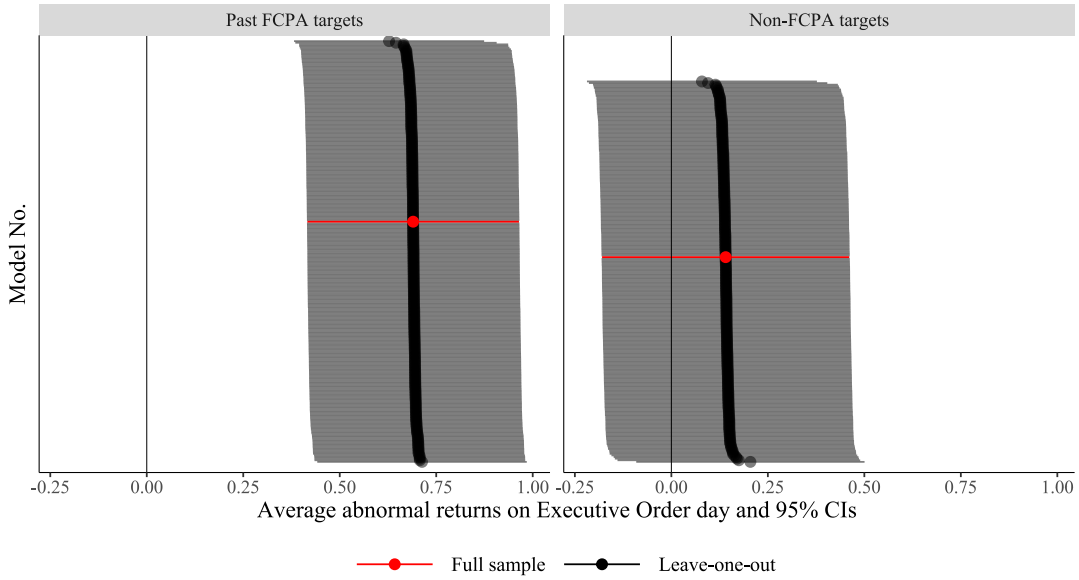

FIGURE I.1: Jackknife test: Average AR on the day of the executive order when excluding one firm from each sample at the time. Red estimates report the average from the full sample for comparison.

I.4 Exclude past FCPA targets that are not matched to placebos

In Table I.3, we replicate our analysis after excluding the 20 past FCPA targets that were not matched to any placebo firms (due to a lack of information on any of the relevant covariates used for propensity score matching, see Appendix C). By limiting the sample of "treated" firms, we are thus making more measured comparisons among the two groups of firms. Results are similar to those found in the main text.

I.5 Exclude companies that trade OTC

In Table I.4, we exclude companies that trade securities over-the-counter (OTC) as opposed to conventional exchanges like the NYSE or NASDAQ. Results are similar to those presented in the main text.

I.6 Exclude past FCPA targets whose ownership history we reconstructed

In Table I.5, we exclude past FCPA targets whose trading history we manually reconstructed (i.e., those that underwent name or ticker changes, or those that merged with other firms). Results are similar to those presented in the main text.

J Robustness to alternative event analysis tests

J.1 Linear regressions (including firm and industry FE)

In Table J.1, we estimate the effect of Trump's executive order by means of linear regressions. We code a binary variable for whether the trading day is the day of the executive order and

Table I.3: Excluding past FCPA targets that miss from matching procedure

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.412*		-0.569*		0.157	
	(0.085)		(0.088)		(0.122)	
Tue, Feb 04 2025	0.659*		0.035		0.624	
	(0.156)		(0.294)		(0.333)	
Wed, Feb 05 2025	0.288		0.308		-0.020	
	(0.181)		(0.185)		(0.259)	
Thu, Feb 06 2025	0.002		0.022		-0.020	
	(0.230)		(0.211)		(0.312)	
Fri, Feb 07 2025	-0.346*		-0.853*		0.506*	
	(0.139)		(0.183)		(0.230)	
Post-event:						
Mon, Feb 10 2025	0.714*	0.714*	0.141	0.141	0.574*	0.574*
	(0.150)	(0.150)	(0.163)	(0.163)	(0.222)	(0.222)
Tue, Feb 11 2025	0.015	0.729*	-0.658*	-0.518*	0.673*	1.247*
	(0.119)	(0.184)	(0.181)	(0.237)	(0.216)	(0.300)
Wed, Feb 12 2025	-0.101	0.629*	-0.205	-0.722*	0.104	1.351*
	(0.223)	(0.287)	(0.192)	(0.307)	(0.294)	(0.420)
Thu, Feb 13 2025	0.371*	0.999*	0.521*	-0.202	-0.150	1.201*
	(0.186)	(0.358)	(0.183)	(0.336)	(0.261)	(0.491)
Fri, Feb 14 2025	-0.147	0.853*	-0.467*	-0.669	0.320	1.521*
	(0.203)	(0.421)	(0.199)	(0.367)	(0.285)	(0.559)
N of firms	236	236	236	236	472	472

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Excludes the past FCPA targets that could not be matched to placebo firms due to missing covariate information. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

Table I.4: Excluding firms trading over-the-counter

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.383*		-0.528*		0.145	
	(0.086)		(0.090)		(0.124)	
Tue, Feb 04 2025	0.693*		0.306		0.387	
	(0.160)		(0.163)		(0.228)	
Wed, Feb 05 2025	0.259		0.313		-0.054	
	(0.172)		(0.181)		(0.250)	
Thu, Feb 06 2025	-0.130		0.104		-0.234	
	(0.225)		(0.207)		(0.306)	
Fri, Feb 07 2025	-0.363*		-0.746*		0.383	
	(0.141)		(0.153)		(0.208)	
Post-event:						
Mon, Feb 10 2025	0.687*	0.687*	-0.043	-0.043	0.730*	0.730*
	(0.153)	(0.153)	(0.148)	(0.148)	(0.213)	(0.213)
Tue, Feb 11 2025	-0.026	0.661*	-0.586*	-0.628*	0.560*	1.289*
	(0.120)	(0.189)	(0.186)	(0.240)	(0.222)	(0.305)
Wed, Feb 12 2025	-0.334*	0.327	-0.292	-0.920*	-0.043	1.247*
	(0.162)	(0.240)	(0.190)	(0.300)	(0.250)	(0.384)
Thu, Feb 13 2025	0.310	0.637*	0.479*	-0.441	-0.169	1.078*
	(0.188)	(0.313)	(0.189)	(0.334)	(0.266)	(0.458)
Fri, Feb 14 2025	-0.194	0.443	-0.243	-0.684	0.050	1.127*
	(0.209)	(0.380)	(0.164)	(0.373)	(0.265)	(0.532)
N of firms	228	228	225	225	453	453

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Excludes firms trading stocks over-the-counter. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

Table I.5: Excluding past FCPA targets whose trading history we reconstructed

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.410*		-0.569*		0.159	
	(0.092)		(0.088)		(0.127)	
Tue, Feb 04 2025	0.535*		0.035		0.500	
	(0.191)		(0.294)		(0.351)	
Wed, Feb 05 2025	0.188		0.308		-0.120	
	(0.173)		(0.185)		(0.253)	
Thu, Feb 06 2025	-0.363		0.022		-0.385	
	(0.326)		(0.211)		(0.389)	
Fri, Feb 07 2025	-0.243		-0.853*		0.610*	
	(0.178)		(0.183)		(0.255)	
Post-event:						
Mon, Feb 10 2025	0.598*	0.598*	0.141	0.141	0.458	0.458
	(0.167)	(0.167)	(0.163)	(0.163)	(0.234)	(0.234)
Tue, Feb 11 2025	0.010	0.608*	-0.658*	-0.518*	0.668*	1.126*
	(0.135)	(0.208)	(0.181)	(0.237)	(0.226)	(0.315)
Wed, Feb 12 2025	-0.256	0.357	-0.205	-0.722*	-0.051	1.080*
	(0.176)	(0.267)	(0.192)	(0.307)	(0.261)	(0.407)
Thu, Feb 13 2025	0.333	0.690*	0.521*	-0.202	-0.188	0.891
	(0.206)	(0.346)	(0.183)	(0.336)	(0.275)	(0.482)
Fri, Feb 14 2025	-0.115	0.575	-0.467*	-0.669	0.352	1.244*
	(0.226)	(0.411)	(0.199)	(0.367)	(0.302)	(0.552)
N of firms	200	200	236	236	436	436

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Excludes past FCPA targets whose ownership history we reconstructed. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

estimate linear models of AR for past FCPA targets, with and without firm fixed effect (FE) and industry FE (models 1, 2, and 3), and for the matched placebo sample (models 3, 4, and 5). Consistent with our main findings, the effect of the executive order on AR is significant for past FCPA targets (models 1, 2, and 3) but not for the comparable placebo firms (models 3, 4, and 5). These findings are not dependent on firm or industry FE.

Table J.1: Linear models of AR

	Past FCPA targets			Non-FCPA targets		
	(1)	(2)	(3)	(4)	(5)	(6)
Executive Order	0.664*	0.684*	0.631*	0.348	0.348	0.348
	(0.160)	(0.166)	(0.164)	(0.208)	(0.208)	(0.209)
(Intercept)	0.026			-0.207*		
	(0.060)			(0.089)		
Firm-FE		Yes			Yes	
Industry-FE			Yes			Yes
Std.Errors	by: firm	by: firm	by: firm	by: firm	by: firm	by: firm
Num.Obs.	2605	2605	2430	2360	2360	2360
R2	0.005	0.110	0.013	0.001	0.155	0.011
R2 Adj.	0.005	0.011	0.005	0.001	0.061	0.004

* $p < 0.05$

Linear models of AR to past FCPA targets and matched placebo firms per day. Standard errors clustered by firm in parentheses. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation.

J.2 Parametric and non-parametric tests

In Table J.2, we focus on the sample of past FCPA targets and apply several parametric tests for event effects proposed by corporate finance studies. In Table J.3, we do the same for the matched placebo firms. We include regular t-test, two tests by Brown and Warner (1980, 1985), Patell (1976), Boehmer, Masumeci, and Poulsen (1991), and Lamb (1995). These tests account for several issues that event studies can experience, for instance, if the event induces changes in the variance of firms' RETURNS within a portfolio of comparable firms. Such contamination effects can yield overestimated effects in normal event studies. Reassuringly, we find that the estimated event effect on AR (0.646, first column) is statistically significant for past FCPA targets across the entire range of considered tests (Table J.2). Instead, we find no consistently significant and positive effect for the sample of placebo firms on the event day that is detected across the full range of parametric tests (Table J.3).

In Table J.4, we apply non-parametric tests for event effects on the past FCPA targets. In Table J.5, we do the same for the matched placebos. Tests considered are those from Boehmer, Masumeci, and Poulsen (1991), McConnell and Muscarella (1985), Corrado and Zivney (1992), Cowan (1992), and Wilcoxon (1992). We find a consistent, positive, and statistically significant effect on the day of the executive order for past FCPA targets (Table J.4). The effect is never statistically significant when considering the placebo sample of non-FCPA targets (Table J.5).

Table J.2: Parametric tests, past FCPA targets

Date	Estimate	BW 1980	BW 1985	T-test	Patell (1976)	BMP 1991	Lamb (1995)
Pre-event:							
Tue, Feb 04 2025	0.516	5.607*	4.278*	2.978*	49.581*	2.406*	4.183*
Wed, Feb 05 2025	0.244	2.650*	2.022	1.400	35.168*	1.069	1.974*
Thu, Feb 06 2025	-0.136	-1.473	-1.124	-0.476	-23.828*	-0.588	-1.096
Fri, Feb 07 2025	-0.000	-0.005	-0.003	-0.003	8.508*	0.420	-0.003
Post-event:							
Mon, Feb 10 2025	0.646	7.021*	5.357*	4.434*	45.292*	2.702*	5.219*
Tue, Feb 11 2025	-0.019	-0.211	-0.161	-0.155	8.911*	0.535	-0.157
Wed, Feb 12 2025	-0.037	-0.398	-0.304	-0.176	9.674*	0.496	-0.265
Thu, Feb 13 2025	0.243	2.638*	2.013	1.282	15.751*	0.856	1.966*
Fri, Feb 14 2025	-0.089	-0.962	-0.734	-0.437	-26.951*	-0.935	-0.694

* $p < 0.05$
Parametric event test results respectively from Brown and Warner (1980, BW 1980), Brown and Warner (1985, BW 1985), regular t-test, Patell (1976), Boehmer, Masumeci, and Poulsen (1991, BMP 1991), and Lamb (1995). Estimation window starts 30 days and ends 5 days before the Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation.

Table J.3: Parametric tests, non-FCPA targets

Date	Estimate	BW 1980	BW 1985	T-test	Patell (1976)	BMP 1991	Lamb (1995)
Pre-event:							
Tue, Feb 04 2025	-0.190	-2.676*	-1.689	-0.472	1.781	0.132	-1.648
Wed, Feb 05 2025	0.223	3.138*	1.981	0.926	-11.176*	-0.731	1.936
Thu, Feb 06 2025	-0.077	-1.081	-0.682	-0.288	9.046*	0.606	-0.588
Fri, Feb 07 2025	-0.659	-9.287*	-5.861*	-2.996*	-9.861*	-0.845	-5.525*
Post-event:							
Mon, Feb 10 2025	0.179	2.517*	1.589	0.895	9.249*	0.672	1.531
Tue, Feb 11 2025	-0.635	-8.936*	-5.640*	-2.962*	-13.543*	-0.459	-5.521*
Wed, Feb 12 2025	0.076	1.068	0.674	0.389	6.649*	0.470	0.660
Thu, Feb 13 2025	0.145	2.047	1.292	0.679	-15.344*	-1.084	1.265
Fri, Feb 14 2025	-0.367	-5.162*	-3.258*	-1.592	-26.335*	-1.543	-3.185*

* $p < 0.05$
Parametric event test results respectively from Brown and Warner (1980, BW 1980), Brown and Warner (1985, BW 1985), regular t-test, Patell (1976), Boehmer, Masumeci, and Poulsen (1991, BMP 1991), and Lamb (1995). Estimation window starts 30 days and ends 5 days before the Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation.

Table J.4: Non-parametric tests, past FCPA targets

Date	Sign test	Gen. sign test	Corrado sign test	Rank test	Mod. rank test	Wilcoxon test
Pre-sanctions:						
Tue, Feb 04 2025	5.261*	5.510*	2.509*	2.773*	2.759*	23438.000*
Wed, Feb 05 2025	0.934	1.181	0.212	0.423	0.423	18396.485
Thu, Feb 06 2025	-0.309	-0.061	-0.079	-0.160	-0.182	16894.000
Fri, Feb 07 2025	-1.052	-0.804	-0.396	-0.119	-0.101	16966.000
Post-sanctions:						
Mon, Feb 10 2025	4.519*	4.767*	2.139*	2.312*	2.330*	22616.000*
Tue, Feb 11 2025	0.309	0.558	-0.079	0.011	0.007	17214.000
Wed, Feb 12 2025	-0.187	0.061	-0.159	-0.572	-0.572	16360.685
Thu, Feb 13 2025	1.805	2.053*	0.744	0.423	0.423	18724.742
Fri, Feb 14 2025	-1.432	-1.185	-0.797	-0.863	-0.863	15126.958

* $p < 0.05$
Non-parametric event test results respectively from a sign test (Boehmer, Masumeci, and Poulsen, 1991), a generalized sign test (McConnell and Muscarella, 1985), a Corrado sign test (Corrado and Zivney, 1992), a rank test (Cowan, 1992), a modified rank test (Corrado and Zivney, 1992), and a Wilcoxon test (Wilcoxon, 1992). Estimation window starts 30 days and ends 5 days before the Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation.

Table J.5: Non-parametric tests, non-FCPA targets

Date	Sign test	Gen. sign test	Corrado sign test	Rank test	Mod. rank test	Wilcoxon test
Pre-sanctions:						
Tue, Feb 04 2025	3.906*	3.889*	2.162*	2.311*	2.311*	17535.000*
Wed, Feb 05 2025	-0.391	-0.408	0.066	0.313	0.313	15174.000
Thu, Feb 06 2025	-0.911	-0.928	-0.459	-0.510	-0.510	13371.000
Fri, Feb 07 2025	-2.083*	-2.100*	-1.310	-1.677	-1.677	10633.000*
Post-sanctions:						
Mon, Feb 10 2025	0.000	-0.017	0.131	0.430	0.430	14849.000
Tue, Feb 11 2025	-2.213*	-2.230*	-1.016	-1.541	-1.541	10712.000*
Wed, Feb 12 2025	-0.260	-0.277	0.000	0.057	0.057	13536.000
Thu, Feb 13 2025	-0.651	-0.668	0.000	0.226	0.226	15162.000
Fri, Feb 14 2025	-2.604*	-2.621*	-0.983	-1.108	-1.108	12092.000

* $p < 0.05$
Non-parametric event test results respectively from a sign test (Boehmer, Masumeci, and Poulsen, 1991), a generalized sign test (McConnell and Muscarella, 1985), a Corrado sign test (Corrado and Zivney, 1992), a rank test (Cowan, 1992), a modified rank test (Corrado and Zivney, 1992), and a Wilcoxon test (Wilcoxon, 1992). Estimation window starts 30 days and ends 5 days before the Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation.

K Robustness to alternative matching methods

Here, we show that our (null) results for the placebo set of firms are confirmed if we select these companies based on matching techniques other than propensity scores. This reassures that our results are not affected by the issues associated with propensity scores matching (King and Nielsen, 2019). First, we show results from a placebo group constructed using coarsened exact matching (CEM, Iacus, King, and Porro, 2012). Second, we re-weight our AR and CAR variables based on weights constructed using entropy balancing (Hainmueller, 2012) to achieve a balanced comparison according to covariates. These alternative matching procedures are described in Appendix C.

K.1 Coarsened exact matching

In Table K.1, we show results if we construct our group of placebo firms by using CEM. We also limit the group of past FCPA targets to those who have found a 1:1 match using CEM. Before the executive order, AR of the two groups moved in a very similar manner (columns 1 and 3), as indicated by the lack of statistically significant difference between the two (column 5). Following the executive order, past FCPA targets recorded significantly positive AR, about +0.821 percentage points higher than expected by market models. The AR for past FCPA targets were significantly higher than for CEM placebos (difference: +0.690). We detect a statistically significant and positive CAR effect for past FCPA targets that lasted until the end of the trading week, indicating persistent abnormal gains.

K.2 Entropy balancing

In Table K.2, we show results obtained when we reweight our outcome variables AR and CAR using weights constructed with entropy balancing, based on observable covariates (see Appendix section C). Here, too, we limit the group of past FCPA targets to those who have been matched (236 firms with covariate information). In this case, too, we notice that AR of the two groups (columns 1 and 3) moved similarly before the executive order, resulting in indistinguishable differences (column 5). Following the executive order, both groups experienced

Table K.1: Building a placebo set of non-FCPA targets with coarsened exact matching

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.395*		-0.796*		0.401	
	(0.123)		(0.294)		(0.319)	
Tue, Feb 04 2025	0.740*		0.640*		0.100	
	(0.200)		(0.192)		(0.277)	
Wed, Feb 05 2025	0.246		0.565*		-0.319	
	(0.239)		(0.208)		(0.317)	
Thu, Feb 06 2025	-0.248		0.008		-0.255	
	(0.317)		(0.257)		(0.409)	
Fri, Feb 07 2025	-0.482*		-0.648*		0.165	
	(0.190)		(0.235)		(0.303)	
Post-event:						
Mon, Feb 10 2025	0.821*	0.821*	0.132	0.131	0.690*	0.690*
	(0.213)	(0.213)	(0.199)	(0.199)	(0.292)	(0.291)
Tue, Feb 11 2025	-0.012	0.809*	-0.343	-0.209	0.330	1.019*
	(0.163)	(0.264)	(0.296)	(0.373)	(0.339)	(0.457)
Wed, Feb 12 2025	-0.097	0.712*	-0.082	-0.291	-0.015	1.003
	(0.179)	(0.299)	(0.215)	(0.429)	(0.281)	(0.523)
Thu, Feb 13 2025	0.679*	1.391*	1.138*	0.839	-0.459	0.552
	(0.265)	(0.410)	(0.486)	(0.633)	(0.555)	(0.754)
Fri, Feb 14 2025	-0.352	1.039*	-0.428	0.414	0.076	0.625
	(0.260)	(0.495)	(0.239)	(0.651)	(0.354)	(0.818)
N of firms	146	146	146	146	291	292

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Matched placebo sample of non-FCPA targets built with coarsened exact matching. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

positive AR, but past FCPA targets did so in a significantly higher manner (difference: +0.465). The detected positive effect cumulated significantly only for past FCPA targets until the end of the trading week.

Table K.2: Weighing placebo set of non-FCPA targets with entropy balancing

	Past FCPA targets		Non-FCPA targets		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.412*		-0.390*		-0.021	
	(0.085)		(0.057)		(0.103)	
Tue, Feb 04 2025	0.659*		0.450*		0.209	
	(0.156)		(0.184)		(0.242)	
Wed, Feb 05 2025	0.288		0.476*		-0.187	
	(0.181)		(0.116)		(0.216)	
Thu, Feb 06 2025	0.002		-0.161		0.163	
	(0.230)		(0.111)		(0.255)	
Fri, Feb 07 2025	-0.346*		-0.605*		0.259	
	(0.139)		(0.113)		(0.179)	
Post-event:						
Mon, Feb 10 2025	0.714*	0.714*	0.249*	0.248*	0.465*	0.466*
	(0.150)	(0.150)	(0.092)	(0.092)	(0.176)	(0.176)
Tue, Feb 11 2025	0.015	0.729*	-0.247*	0.002	0.262	0.727*
	(0.119)	(0.184)	(0.110)	(0.131)	(0.162)	(0.226)
Wed, Feb 12 2025	-0.101	0.629*	0.028	0.029	-0.128	0.599
	(0.223)	(0.287)	(0.125)	(0.210)	(0.255)	(0.355)
Thu, Feb 13 2025	0.371*	0.999*	0.427*	0.449	-0.056	0.550
	(0.186)	(0.358)	(0.116)	(0.250)	(0.219)	(0.437)
Fri, Feb 14 2025	-0.147	0.853*	-0.345*	0.111	0.198	0.741
	(0.203)	(0.421)	(0.131)	(0.269)	(0.242)	(0.500)
N of firms	236	236	3371	3371	3589	3607

* $p < 0.05$

Average AR and CAR to past FCPA targets and matched placebo firms per day. Non-FCPA targets include full set of non-FCPA targets US-traded firms with covariate information, outcomes reweighted with entropy balancing. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

L Robustness to alternative research designs (diff-in-diff and GSC)

L.1 Difference-in-differences

Here, we show that our results do not hinge on the chosen event-study research design. In Table L.1, we study directly firms' observed stock prices at closing in difference-in-differences models. We build a panel dataset considering past FCPA targets as "treated" by the executive order (261 firms). We construct the "control" group using the placebo matched sample (236 firms). The model is estimated by interacting with two binaries: one indicating whether a given trading day follows the executive order, the other indicating whether a given firm is a past FCPA target. The interaction term estimates the difference-in-differences. We consider data on the entire trading week preceding and following the executive order.

Table L.1: Difference-in-differences models of price at closing

	All past targets	Only matched past targets
	(1)	(2)
Post-Executive Order	-1.183*	-1.183*
	(0.421)	(0.421)
Past FCPA target	-8.249	-2.314
	(14.698)	(15.133)
Post-Executive Order × Past FCPA target	1.573*	1.452*
	(0.561)	(0.579)
(Intercept)	105.252*	105.252*
	(11.887)	(11.888)
Std.Errors	by: firm	by: firm
Num.Obs.	4965	4720
R2	0.001	0.000
R2 Adj.	-0.000	-0.001

* $p < 0.05$

Linear regression models to estimate Difference-in-differences of stock price at closing for past FCPA targets and matched placebo firms after the Executive Order. "Post-Executive Order" is a binary taking value of 1 on the day of February 10th 2025 and after. "Past FCPA target" is a binary taking value of 1 for firms that have been targets of an FCPA investigation or enforcement action in the past. The table considers data relative to the entire trading week before the Executive Order and the trading week of the Executive Order itself. Standard errors clustered by firm in parentheses.

Model 1 studies the whole set of firms. Model 2 restricts the sample to the sole treated and control firms that have been matched via propensity score weighting. Results indicate a positive and statistically significant difference-in-differences: Trump's FCPA executive order on Monday 10th of February increased observed stock market price to past FCPA targets by about $1.6 (full set of past FCPA targets) or $1.5 (only matched past FCPA targets) *more* than it did for the sample of comparable firms.

We perform a fully-fledged difference-in-differences event analysis of the models in Table L.1 in Figure L.1. This is done by estimating the following linear model of price at closing to firm i on day t, as a function of a set of binary variables relative to the trading day (with

reference point the day before the executive order, $t_e - 1$) interacted with a binary for whether firm i is a past FCPA target ($\text{FCPA}_i = 1$); firm and day fixed effects (α_i and γ_t) are included:

$$\text{PRICE}_{it} = \alpha_i + \gamma_t + \sum_{r \neq t_e - 1} \delta_r \times 1[\text{FCPA}_i = 1] \times 1[t = r + 1] + \varepsilon_{it} \tag{2}$$

Estimated daily δ_r quantify post-event effects or pre-event existing trends that indicate violations of the parallel trend assumption.

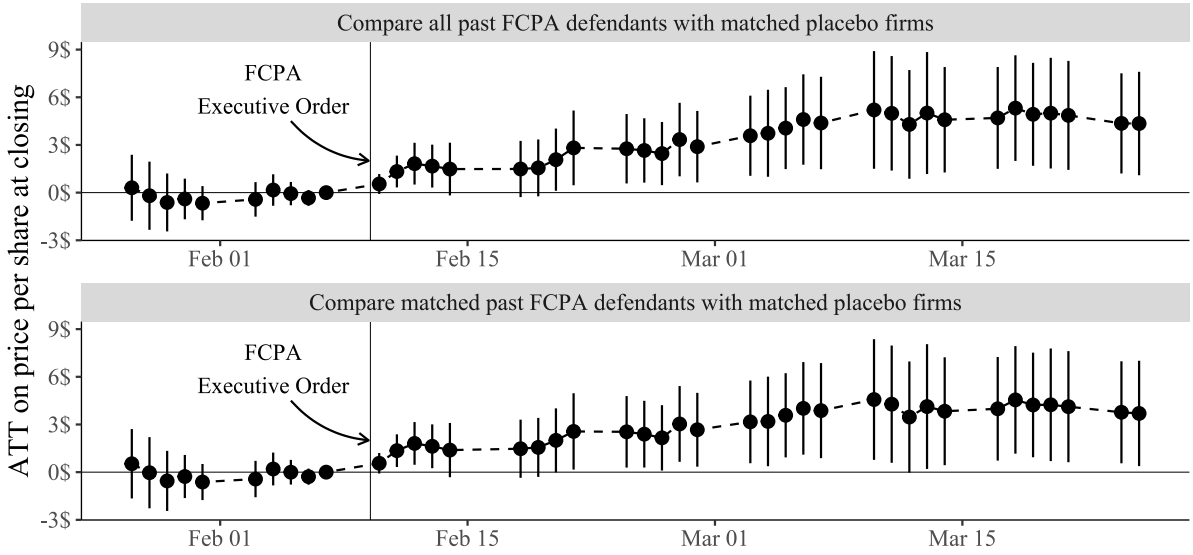

FIGURE L.1: Difference-in-differences event analysis of firms' price at closing following the FCPA Executive Order. 95% CIs displayed as vertical bars

Results of our difference-in-differences event study—in the form of dynamic average treatment effects on the treated (ATTs)—are in Figure L.1. We present results relative to the two trading weeks before the executive order, and up until the end of March 2025. Each coefficient quantifies the ATT on a given day, i.e., it represents the difference between the changes in stock price at closing for treated and control firms, from the reference baseline (the last trading day before the executive order, i.e., Feb 07, 2025) and that time point. Estimated ATTs are significant and positive following the executive order (February 10th, 2025), indicating that past FCPA targets increased their stock prices at closing to a significantly larger degree than control firms, *vis-à-vis* their levels before the Order. We note that this over-time difference in stock prices between past FCPA targets and comparable placebo firms did not reverse as time passed. In fact, more than a month after the executive order, companies that had previously been targeted under the FCPA were trading, on average, about $5 per share higher than similar firms that had not been targeted—relative to the gap between them before the executive order. These effects amount to substantive capitalization gains, as we illustrate in our next section. We do not observe a pre-event significant trend; instead. This final observation reassures us of the internal validity of our results by lending credibility to the identifying "parallel trends" assumption—that treated and control firms would have had similarly trending prices at closing, had it not been for the executive order.

L.2 Generalized synthetic controls

In an effort to further demonstrate that our findings do not depend on applying the classic two-window event analysis from corporate finance, here we show that we can obtain similar results with yet another research design choice: a synthetic control method. The idea of synthetic controls is, in principle, similar to that of the two-window event analysis of our main text: using information from "untreated" firms to impute a counterfactual for "treated" ones. However, the two procedures differ substantially from a mathematical point of view. Here we prove that, that notwithstanding, they yield similar results in our case.

Because the group of firms exposed to the executive order is quite numerous (261), here we cannot resort to the classic synthetic control method that generates one synthetic control per each treated unit (E.g., Abadie, Diamond, and Hainmueller, 2015). Instead, we employ the generalized synthetic control (GSC) proposed by Xu (2017). This design employs interactive fixed effect models to synthesize one control unit per each treated one. The method has a series of advantages, here. First, it estimates individual treatment effects like the two-window event-analysis presented in the main text does. Second, *unlike* that analysis, it allows us to study longer term effects, similar to what the difference-in-differences of Figure L.1 does.

We first apply the GSC estimator to a dataset composed of daily observed prices at closing for the 261 past FCPA targets, from the first of January 2025 until the end of March 2025. Synthetic controls are generated using interactive two-way fixed effects and data from the 236 matched placebo firms. Standard errors are estimated with bootstrapping. Figure L.2 reports the results, displaying both the over-time ATT (top panel) and the average price at closing of the past FCPA targets and synthetic counterfactual firms (bottom panel). Whereas, before the event, the ATT is insignificant (and observed and counterfactual lines run extremely close), the executive order significantly increased the price at closing of past FCPA targets. Even at the end of March 2025, past FCPA targets were trading at about $8 per share more than they would have, based on the synthetic counterfactuals. This translates into a substantial, long-term capitalization gain. GSC estimates that the average past FCPA target realized about $785 million in market capitalization on the very day of the executive order. At the end of March, this gain amounted to about $6.5 billion. This is evidence of a sustained market gain induced by the executive order.

In Figure L.3, we replicate the procedure but limit the sample of "treated" firms exclusively to the past FCPA targets that have found a match with propensity score weighting (that is, we exclude past FCPA targets with no covariate information). Results are comparable to those presented above.

M Heterogeneous effects

M.1 Heterogeneous effects: FCPA enforcement *vs* investigation

In Table M.1, we partition our sample of past FCPA targets based on whether they have experienced an enforcement action in the past (columns 1–2) or whether they have only been investigated for an FCPA violation (columns 3–4). We find that both subgroups of firms experienced positive AR on the day of the executive order. When looking at CAR, we observe a positive and significant estimate on the day following the executive order, but estimates are not significant (although positive) for the rest of the week, likely due to the reduced sample

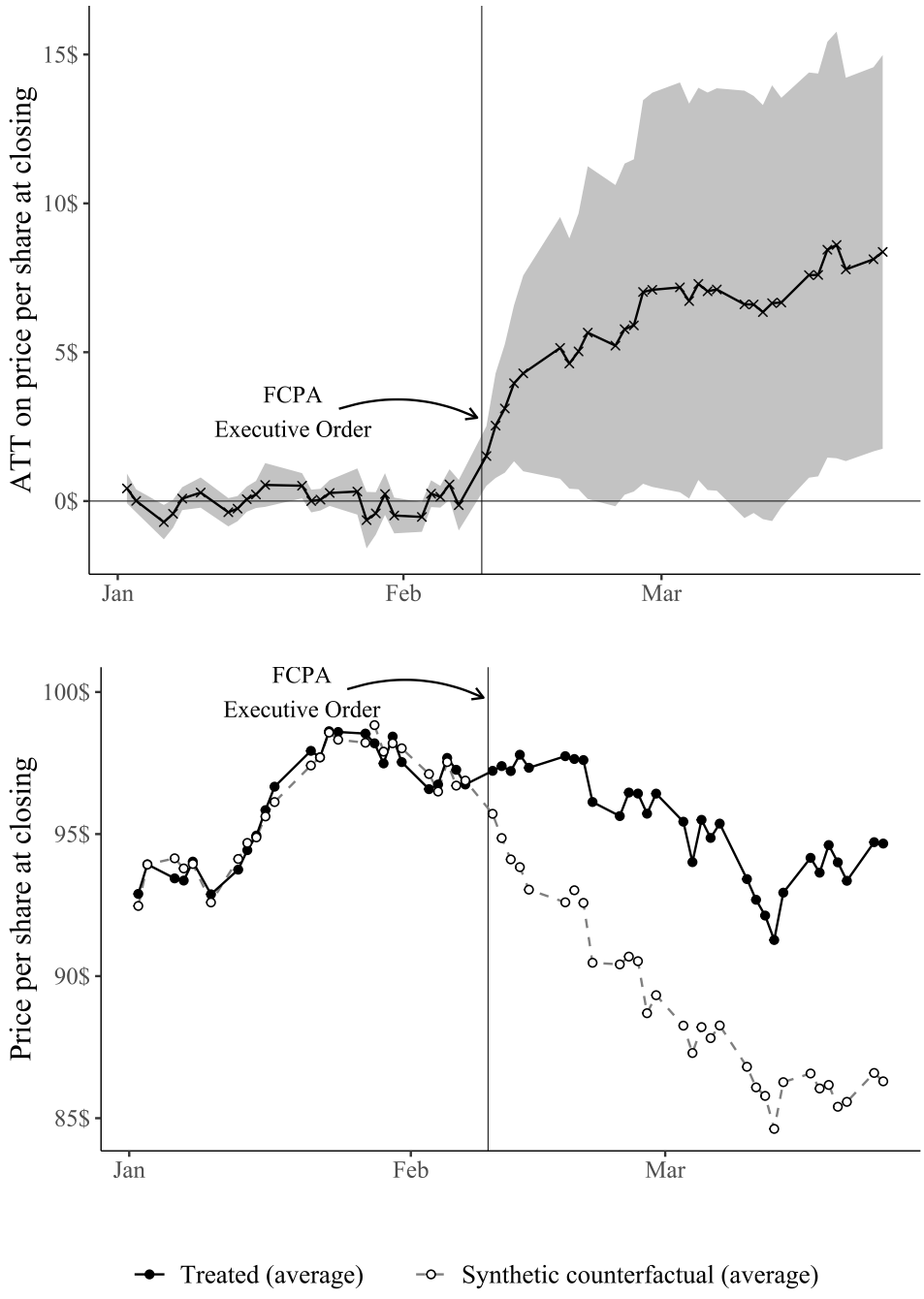

FIGURE L.2: Generalized synthetic control results (full sample). ATT of the executive order on past FCPA targets' price at closing (top panel) and average observed and counterfactual prices (bottom panel). 95% CIs displayed as the shaded gray area.

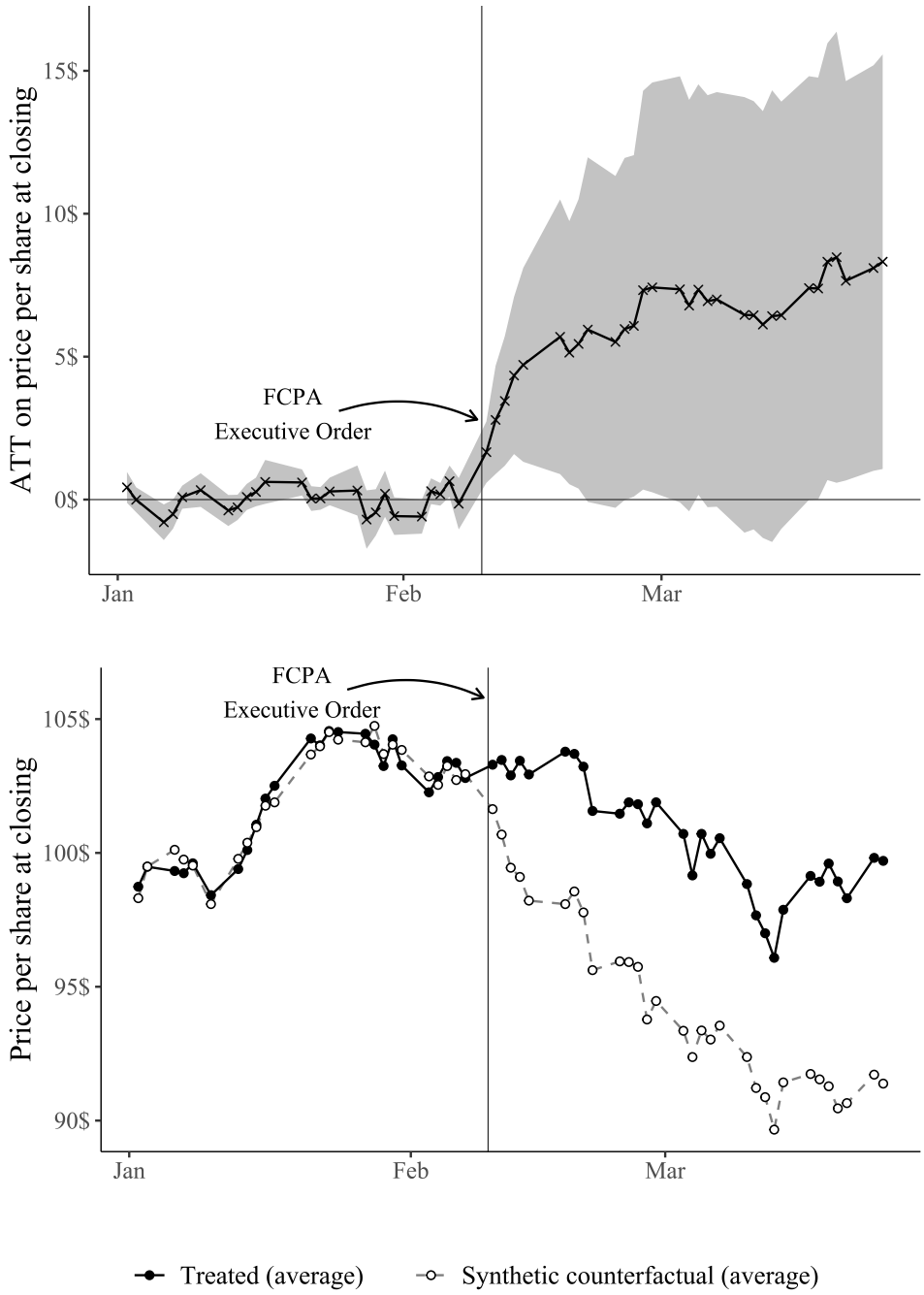

FIGURE L.3: Generalized synthetic control results (only matched past FCPA targets). ATT of the executive order on past FCPA targets' price at closing (top panel) and average observed and counterfactual prices (bottom panel). 95% CIs displayed as the shaded gray area.

size. Columns 5 and 6 confirm that estimates for the two subgroups of firms are statistically indistinguishable.

Table M.1: Heterogeneous effect on past FCPA targets by whether they suffered enforcement action or only investigation

	FCPA Enforcement		FCPA investigation		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.508*		-0.245*		-0.263	
	(0.102)		(0.122)		(0.159)	
Tue, Feb 04 2025	0.621*		0.504		0.118	
	(0.211)		(0.261)		(0.336)	
Wed, Feb 05 2025	0.545*		-0.018		0.563	
	(0.226)		(0.239)		(0.328)	
Thu, Feb 06 2025	-0.294		-0.049		-0.245	
	(0.380)		(0.366)		(0.528)	
Fri, Feb 07 2025	-0.200		-0.313		0.113	
	(0.177)		(0.258)		(0.313)	
Post-event:						
Mon, Feb 10 2025	0.536*	0.536*	0.935*	0.935*	-0.399	-0.399
	(0.165)	(0.165)	(0.249)	(0.249)	(0.298)	(0.298)
Tue, Feb 11 2025	0.197	0.733*	-0.277	0.657*	0.474	0.076
	(0.134)	(0.220)	(0.200)	(0.278)	(0.241)	(0.354)
Wed, Feb 12 2025	-0.083	0.657	-0.209	0.449	0.126	0.208
	(0.275)	(0.340)	(0.298)	(0.422)	(0.405)	(0.542)
Thu, Feb 13 2025	0.293	0.951*	0.494	0.943	-0.201	0.007
	(0.214)	(0.414)	(0.323)	(0.574)	(0.387)	(0.708)
Fri, Feb 14 2025	-0.066	0.884	-0.084	0.859	0.018	0.025
	(0.257)	(0.498)	(0.271)	(0.659)	(0.373)	(0.827)
N of firms	160	160	101	101	261	261

* $p < 0.05$

Average AR and CAR to past FCPA targets that have been under enforcement action or only investigated, per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

M.2 Heterogeneous effects: Time of latest FCPA action

Next, we estimate heterogeneous effects of the executive order by distinguishing when a given FCPA target experienced its last FCPA action—be it an investigation or an enforcement event. This is done to ensure the legitimacy of considering firms that have been targeted with an FCPA action at different times in the same analysis. For the purpose of ensuring statistical power, we distinguish three periods of FCPA actions. First, the earliest years of intensified FCPA enforcement, i.e., before the 2010s; second, the decade that saw the highest increase in enforcement, that of the 2010s; finally, the most recent period (the 2020s). In Figure M.1, we report average AR (and 95% CIs) for past FCPA targets based on their timing of the latest FCPA action. We also report sample sizes, alongside estimated averages. The effect is positive and comparable across the three groups. For firms that experienced the latest FCPA action in the 2020s, the effect is not statistically significant at an alpha of 0.05. However, we note that the three-point estimates are indistinguishable from one another. To demonstrate the statistical equivalence, we report 83.4% CIs as thicker vertical lines. Their overlap allows us to infer that point estimates are indistinguishable at alpha = 0.05. We also report pairwise p-values relative to a test of the difference in average AR across these groups.

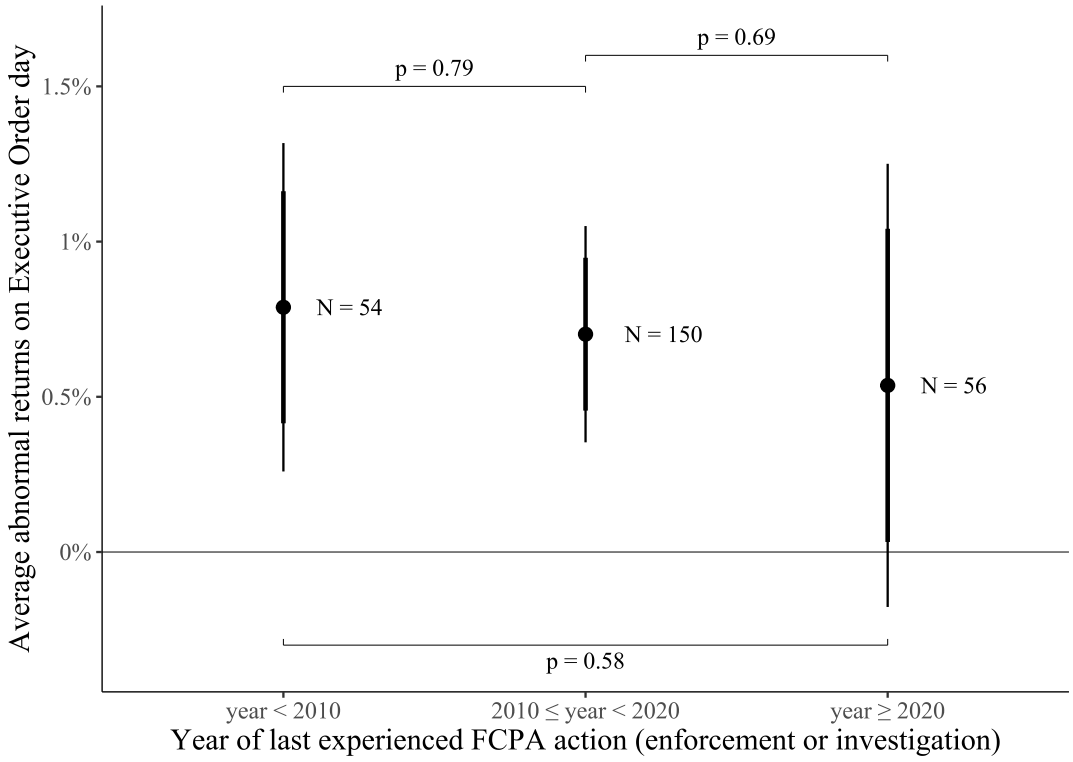

FIGURE M.1: Average AR of past FCPA target firms by the decade in which the firm experienced its last FCPA action (investigation or enforcement). 95% CIs displayed as thin vertical bars and 83.4% CIs displayed as thick vertical bars. P-values from pairwise difference in mean tests reported

We further provide evidence that there is no significantly heterogeneous effect by time of FCPA action with two additional tests. First, we replicate the previous procedure but aggregate the timing of FCPA actions in five-year groups. Results are reported in Figure M.2. To main-

tain the visualization simple, here we do not report pairwise p-values and resort exclusively to 83.4% CI bars to distinguish whether point estimates are statistically distinguishable from one another. We detect a significant effect of the executive order on the AR of firms that have experienced their latest FCPA action in the periods of strongest FCPA enforcement (between 2005 and 2020), particularly between 2005 and 2015. We do not find a significant effect for firms that experienced their latest enforcement in any of the other five-year periods. However, we note that these point estimates are, once again, indistinguishable from one another, as evidenced by the complete overlap between the 83.4% CIs.

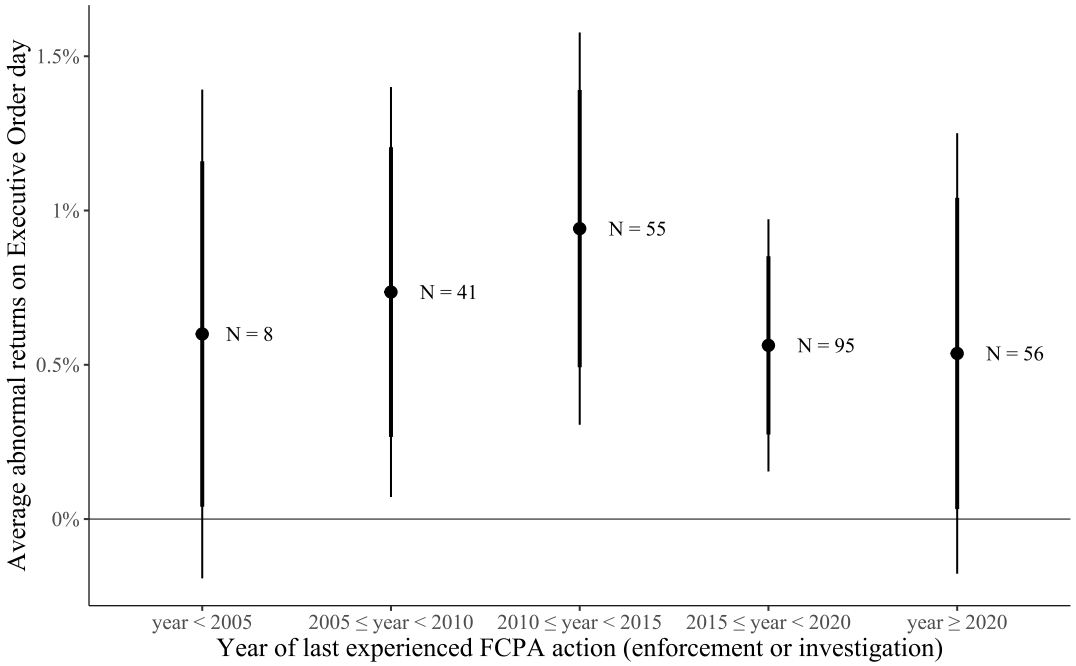

FIGURE M.2: Average AR of past FCPA target firms by five-year period in which the firm experienced its last FCPA action (investigation or enforcement). 95% CIs displayed as thin vertical bars and 83.4% CIs displayed as thick vertical bars

Finally, we propose a much more flexible way of studying heterogeneous effects of the executive order by timing of last FCPA action. We estimate a linear model of past FCPA targets' AR, where the treatment variable is a binary indicator taking value of 1 on the day of the executive order, 0 otherwise. We interact this binary variable with a variable indicating the year of last FCPA action (enforcement or investigation) for each past FCPA target. We consider data relative to the full trading week before the executive order and until the end of the week of its announcement. Standard errors are clustered at the firm-level. We report results in Table M.2. Again, we find no significant difference in effect based on the timing of firms' FCPA exposure, as indicated by the statistically insignificant interaction term (p-value = 0.23).

To interpret these last results substantively and avoid gross extrapolations implied by the model (the coefficient of the un-interacted executive order variable in Table M.2 reports the effect for a firm who experienced its latest FCPA action on the year 0), we report marginal effects of the executive order by year of latest FCPA action in Figure M.3. We also report results obtained when applying a "binning estimator" (Hainmueller, Mummolo, and Xu, 2019) for allowing a more flexible estimation of moderation effects.

Table M.2: Heterogeneous effect of Executive Order by year of last FCPA action

	Past FCPA targets
	(1)
Executive Order × Year of FCPA action	-0.029
	(0.024)
Executive Order	58.919
	(48.494)
Year of FCPA action	-0.001
	(0.009)
(Intercept)	1.834
	(17.256)
Std.Errors	by: firm
Num.Obs.	2595
R2	0.005
R2 Adj.	0.004

* $p < 0.05$

Linear models of AR to past FCPA targets per day. Standard errors clustered by firm in parentheses. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation.

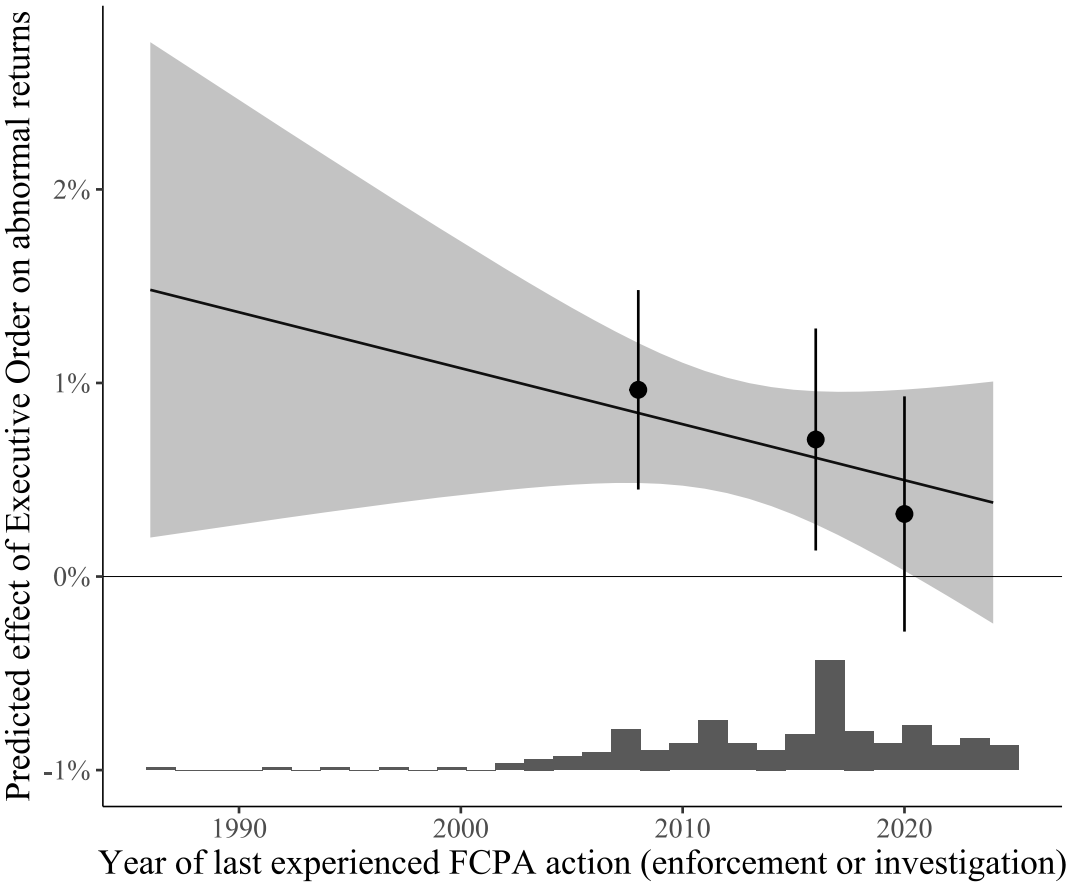

FIGURE M.3: Marginal effect of the FCPA executive order on past FCPA targets' AR conditional on the year of last FCPA action experienced. Results from a linear regression model (straight line) and binning estimator (Hainmueller, Mummolo, and Xu, 2019) (points). All standard errors are clustered at the firm-level. 95% CIs and distribution of the moderator variable are reported.

Across the board, and consistent with evidence provided above, we find a significantly positive effect of the executive order on the AR of firms that experienced their latest FCPA action before the late 2010s, but not after. However, estimated effects are, once again, not statistically distinguishable across the year scale, which leads us to conclude that investors did not use timing of past FCPA exposure as a significant heuristic for inferring contemporary FCPA risk.

M.3 Heterogeneous effects: Change in company name

Here, we show lack of heterogeneous effects also with respect to a further dimension: whether past FCPA targets have changed name. Name changes can occur as a re-branding choice, very rarely also as a response to FCPA actions themselves (to our knowledge, the only case of this kind is represented by the former Italian firm *Finmeccanica S.p.A.*, which renamed itself to *Leonardo S.p.A.* following FCPA enforcement); or, it can occur following organization changes including merger, acquisitions, and spin-offs. It is possible that investors link FCPA histories to the specific names involved in past FCPA actions and, as such, discount the risk for firms that changed names.

To test against this possibility, we split our sample of past FCPA targets by whether they changed their name or not, since their FCPA action. We report results in Table M.3. Against this concern, we find that the two groups (past FCPA targets with or without name change) had very similar trading histories around the executive order, a result which informs us that investors were likely informed of past FCPA histories also for firms that changed names.

Table M.3: Heterogeneous effect on past FCPA targets by whether they changed name since FCPA action

	Changed name		Did not change name		Difference-in-means	
	(1) AR	(2) CAR	(3) AR	(4) CAR	(5) AR	(6) CAR
Pre-event:						
Mon, Feb 03 2025	-0.393*		-0.410*		0.017	
	(0.154)		(0.092)		(0.179)	
Tue, Feb 04 2025	0.709*		0.535*		0.174	
	(0.320)		(0.191)		(0.372)	
Wed, Feb 05 2025	0.779		0.188		0.592	
	(0.431)		(0.173)		(0.465)	
Thu, Feb 06 2025	0.339		-0.363		0.702	
	(0.458)		(0.326)		(0.562)	
Fri, Feb 07 2025	-0.246		-0.243		-0.004	
	(0.240)		(0.178)		(0.299)	
Post-event:						
Mon, Feb 10 2025	0.992*	0.992*	0.598*	0.598*	0.394	0.394
	(0.240)	(0.240)	(0.167)	(0.167)	(0.292)	(0.292)
Tue, Feb 11 2025	0.025	1.017*	0.010	0.608*	0.016	0.409
	(0.204)	(0.279)	(0.135)	(0.208)	(0.244)	(0.348)
Wed, Feb 12 2025	0.274	1.291	-0.256	0.357	0.530	0.934
	(0.652)	(0.713)	(0.176)	(0.267)	(0.676)	(0.762)
Thu, Feb 13 2025	0.498	1.789*	0.333	0.690*	0.166	1.100
	(0.383)	(0.883)	(0.206)	(0.346)	(0.435)	(0.948)
Fri, Feb 14 2025	0.063	1.852	-0.115	0.575	0.177	1.277
	(0.320)	(1.030)	(0.226)	(0.411)	(0.392)	(1.109)
N of firms	61	61	200	200	261	261

* $p < 0.05$

Average AR and CAR to past FCPA targets who did vs did not change name or ownership since FCPA action, per day. Standard errors of the mean reported in parentheses. P-values from a two-tailed test of difference from zero for the average against a standard normal distribution. Estimation window starts 30 days and ends 5 days before FCPA Executive Order. Market models estimated using the LASSO and individual S&P 500 constituents as predictors, selected using 15-fold cross validation. Columns 5 and 6 report the difference in means, respectively, between averages in columns 1 and 3, and those in columns 2 and 4.

N Pam Bondi's memo: FCPA-related paragraphs

The two paragraphs of the Bondi memo pertaining to the FCPA read:

> Foreign Corrupt Practices Act. The Criminal Division's Foreign Corrupt Practices Act Unit shall prioritize investigations related to foreign bribery that facilitates the criminal operations of Cartels and TCOs, and shift focus away from investigations and cases that do not involve such a connection. Examples of such cases include bribery of foreign officials to facilitate human smuggling and the trafficking of narcotics and firearms.
>
> The requirements in Justice Manual § 9-4 7 .110 requiring authorization by the Criminal Division for an investigation or prosecution of a case under the Foreign Corrupt Practices Act and Foreign Extortion Prevention Act, as well as the requirement that such investigations and prosecutions be conducted by trial attorneys of the Fraud Section, are suspended for all matters relating to foreign bribery associated with Cartels and TCOs. U.S. Attorney's Offices shall provide the Foreign Corrupt Practices Act Unit with 24 hours' advance notice of the intention to seek charges and make available to the Unit upon request any existing memoranda relating to the contemplated charges. No new or additional paperwork will be required by the Foreign Corrupt Practices Act Unit in connection with these notices and consultations.

O Describing FCPA enforcement and OECD ABC enforcement

To assess the role of the United States as a global enforcer of the global anti-corruption regime defined by the OECD Anti-Bribery Convention (46 signatory countries to date), we draw on a comprehensive database that includes both FCPA cases and instances of cross-border corruption investigated globally (Crippa, Malesky, and Picci, 2025). According to these data, between 2000 and 2018, investigations were initiated into 1,505 distinct cases of alleged cross-border bribery worldwide. Of these, 584 cases (39% of the total) involved firms headquartered in the United States.

This disproportionate representation does not necessarily indicate a higher propensity of US companies to engage in bribery abroad compared to firms from other countries. Rather, it reflects the particularly proactive enforcement stance adopted by the United States. Notably, 891 cases—amounting to 59% of the global total—were investigated by US authorities during the same period under the FCPA terms, against both US and non-US firms. This assertive approach has included a broad interpretation of jurisdiction, effectively pressuring foreign jurisdictions to enhance their own enforcement efforts (Kaczmarek and Newman, 2011). In particular, we identify 355 cases of alleged corruption involving companies headquartered outside the United States that were nonetheless investigated (most often, non exclusively) by US authorities. These represent 24% of all cases in the dataset and demonstrate the sustained efforts of the United States to encourage stricter enforcement of the OECD Anti-Bribery Convention among other signatory countries.

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