Making Bribery Profitable Again? The Market Effects of Halting Extraterritorial Accountability for Overseas Bribery[*]

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Abstract

On February 10th, 2025, US President Trump signed an executive order ceasing initiation of any new investigations and enforcement actions under the Foreign Corrupt Practice Act (FCPA), which made it unlawful for US companies to bribe foreign public officials. We analyze market valuations of publicly traded companies on US financial markets before and after the announcement, finding that on the day of the Executive Order former FCPA targets, whose stocks are publicly traded, experienced returns on equity markets that were about 0.69 percentage points higher than what would have been expected from stock market trends. The effects cumulated substantively, resulting in capitalization gains for the average former target in corporate corruption cases of about $160M and out-sized returns to shareholders. This gain, which materialized for the average past FCPA target in a single trading day, is roughly the same amount as the historical mean of FCPA fines. These results allow us to contribute to long-standing debates about whether the costs firms experience from corruption are due to legal enforcement or the inefficiency and uncertainty it generates for firm operations. When legal enforcement is removed, valuations of firms at risk of corruption rise dramatically, indicating that investors perceive the legal costs to be an important threat to investment in corrupt firms.

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In this research note, we exploit US President Donald Trump's surprising decision to halt enforcement of the Foreign Corrupt Practices Act (FCPA), demonstrating that suspending legal enforcement sharply increased investment in firms previously designated as FCPA offenders compared to non-offenders. The stated purpose of Trump's decision was that stringent regulatory compliance and legal costs of the FCPA disadvantaged American companies in competition with multinational companies (MNCs) from other states. This justification implies that removing the FCPA regulatory burden should have led to investment in all firms operating abroad, regardless of previously corrupt behavior. By contrast, our results indicate that investors primarily understood the Executive Order as reducing the risk of potential enforcement for firms previously flagged as behaving corruptly.

President Trump's Executive Order, signed on February 10th, 2025, mandated that the Department of Justice (DOJ) cease initiation of any new investigations and enforcement actions under the FCPA for 180 days and revise its enforcement priorities (The White House, 2025). Since it first passed the US Congress in 1977, the FCPA made it illegal for US citizens or companies to bribe foreign public officials, subjecting them to punishment in the US for acts committed abroad—a legal principle known as extraterritoriality (Kaczmarek and Newman, 2011). Twenty years later, the FCPA became the cornerstone of the global regime against corporate corruption—centered around a 1997 OECD Anti-Bribery Convention (ABC) (Brewster, 2025). There is substantial empirical evidence that the FCPA has been successful in reducing bribery (Krever, 2007), and investment in and trade with corrupt regimes (Cuervo-Cazurra, 2008; Xu, 2025) both for US firms and foreign firms with activities in the US (McLean, 2011; Christensen, Maffett, and Rauter, 2022). FCPA punishments include colossal fines, such as the $1.6B fee imposed on *Siemens AG* in 2008 (in coordination with German authorities) for bribing public officials in multiple countries (DOJ, 2008) and, in 2020, *Goldman Sachs*' $2.9B assessment for facilitating bribery in Malaysia (DOJ, 2020).

Perhaps because of this success, Trump has long been an opponent of the bill, stating his intention to repeal it and referring to it as a "horrible law" in a television interview during his 2016 campaign (Maddow, 2017). Trump complained that the FCPA placed American compa-

nies at a disadvantage *vis-à-vis* competitor firms who do not face the same onerous regulations during international business transactions.[1] Nevertheless, throughout Trump's entire first term, the FCPA remained untouched and operational. Consequently, the decision to halt the FCPA at the beginning of his second term was unexpected (Salmon, 2025), appearing among a wave of executive orders and actions reshaping US domestic and foreign policy priorities.[2]

In this research note, we study the immediate financial impact of Trump's reversal of US anti-corruption priorities. We analyze the effect of the Executive Order halting FCPA enforcement on stock market evaluations of US-traded companies that have been cited as (potential) FCPA violators in the past. Having been exposed as engaging in past corruption, these companies are at risk of future FCPA actions: our data indicate that more than 40% of the FCPA actions against a target firm are followed up by another FCPA action targeting the same firm within ten years (Appendix A). Market analysts who measure reputational risk exposure register this increased anti-corruption enforcement risk: FCPA targets have, on average, a measured reputational risk that is 28% higher than that of similarly situated firms, a gap that occurs in the immediate aftermath of an FCPA action and remains persistent into the very long term (Appendix B). According to the legal cost hypothesis, we suggest that financial markets welcomed Trump's Executive Order as a signal that corrupt business activities—such as the risky ones entertained by past FCPA targets—would be less costly in the future because the Executive Order reduced the probability of bribery investigations, punishment, and potential fines.[3]

[1] Such criticisms are only partially justified. Under the OECD ABC of 1997, all OECD countries (and eight non-OECD) have adopted similar extra-territoriality legislation and judicial institutions to prevent their firms from bribing abroad. The ABC itself was the result of lobbying by the US, out of concern for competitive damages following the unilateral FCPA adoption (Picci, 2024, 20–23). Thus, many competitors of US firms are subject to the same anti-bribery limitations as US firms in international business. When enforced, this international agreement effectively caused companies to reduce their bribe propensity (Jensen and Malesky, 2018). Although evidence exists that firms from countries outside of the agreement (e.g., China or Singapore) compensate for this reduction in corruption by paying *more* bribes (Chapman et al., 2021), the extent to which anti-bribery laws really put firms at a disadvantage in foreign business competition likely depends on whether corruption operates as an exclusive means of accessing business in foreign countries (Crippa, 2023; Perlman and Sykes, 2017).

[2] To illustrate the surprise that the shock generated, a humorous meme featuring Star Wars' Obi Wan Kenobi made its way around top law firms on the day of the announcement, "I felt a great disturbance in the force... as if a million FCPA attorneys all cried out at once and were suddenly unemployed."

[3] A reasonable counterargument to our expectation would highlight that the FCPA statute of limitations is usually five years. That is, any halted FCPA action can still be enforced under a future US administration that would reverse Trump's Executive Order and restore FCPA enforcement. This is a reasonable argument, but we stress that, from the point of view of a financial investor, the Trump administration's dismissive approach to the FCPA still guarantees (at a minimum) four years of potential profits for investing in tainted companies. These

That is:

H1. *Companies previously targeted by the FCPA will see improvement in their share prices after the Executive Order pausing FCPA implementation, compared to similar-situated firms at less risk of new corruption charges.*

To test this hypothesis, we study the 261 firms with an FCPA history, which we define as having been subject to either FCPA enforcement or investigations in the past—and who trade securities on US stock markets. We leverage the unexpected timing of Trump's announcement and implement an event-study design for causal inference on equity returns of past FCPA targets, which we compare to both the whole market and a portfolio of 236 similarly situated businesses that have never been FCPA targets.

Contrary to President Trump's justification, which should have led to increased investment in all publicly traded MNCs, we find that past FCPA violators recorded positive returns on the day of Trump's Executive Order, which were 0.69 percentage points above what would be predicted based on broader market trends. This effect cumulated substantively over the trading week, resulting in significant capitalization gains for former targets in corporate corruption cases. We calculate a surplus gain above expected performance in market capitalization of $160M for the average past FCPA target on the day Trump signed the Executive Order. This capitalization gain, which materialized for the average past FCPA target on just one trading day, is comparable to the average FCPA fine to date ($150M).[4] However, some of the past FCPA targets recorded gains of (tens of) billions of dollars in capitalization, in some cases larger than the largest FCPA fines in history.[5] All considered, the portfolio of past FCPA targets recorded a net gain in market capitalization of about $39B on the day of the Executive Order. In short, stock market investors welcomed Trump's order halting anti-corruption enforcement as a signal that investing in corrupt enterprises would be less risky and more profitable in the near future.

are very long time horizons on equity markets, given that a stock market transaction—unlike a foreign direct investment—virtually entails no sunk cost for a stock buyer, whose investment can be easily liquidated were FCPA enforcement priorities to change with a future administration.

[4] Authors' own calculations based on data used in this article.

[5] The top-50 gainers are reported in Table G.1. See Appendix G for a further discussion of estimated capitalization gains. For a top ten of FCPA fines, see: https://fcpa.stanford.edu/statistics-top-ten.html.

This generated significant capitalization gains for firms that were at the highest risk of future corrupt activities and corresponding out-sized returns to their investors.

Our results contribute to two additional literatures in political economy and comparative politics. First, we speak to a debate about the costs of corruption. Corruption is costly for firms, as it represents a tax on private initiative which increases inefficiency and uncertainty (Rose-Ackerman, 1975; Dal Bó and Rossi, 2007). The implication is that corrupt business is costly in and of itself through its negative impact on internal controls and strategic planning, raising severe risks for potential shareholders. For instance, budget projections may not fully account for bribery in costing, complicating profit and dividend estimates. Thus, investors should have been reluctant to invest in firms known to bribe abroad, whether or not FCPA enforcement was in effect. Alternatively, others argue that bribery permits firms to gain important first-mover advantages that secure monopolistic positions in foreign markets, with financial gains that can significantly outweigh the inefficiency losses (Malesky, Gueorguiev, and Jensen, 2015; Zhu, 2017). As such, firms would be deterred from bribing only if facing law enforcement costs at least as large as their incentives to bribe (Kaufmann and Vicente, 2011; Banerjee, Gupta, and Krishnamurti, 2022; Lau, Demir, and Bilgin, 2013). Thus far, the literature has struggled to empirically disentangle the costs of corruption in terms of the inefficiency they create and the associated costs of anti-corruption law enforcement. To be clear, enforcement costs differ from the legal, compliance transaction costs that are faced by all foreign operating firms under the FCPA. Here, we refer only to the probability that actual malfeasance will be investigated and punished. By leveraging the fact that Trump's unexpected Executive Order impacted only the enforcement costs of corruption (it could not reasonably make corruption a less inefficient enterprise), our study allows us to separate the two financial drivers. Once enforcement costs are lifted, investors find firms at risk of further corruption to be more profitable. This indicates that many market actors would see corruption as a profitable investment were it not for strong anti-corruption policies. The finding reinforces our understanding that strong anti-corruption mandates are necessary to deter firms from engaging in bribery.

Second, we contribute to work on the benefits that connected market actors can accrue

from sharp changes in economic policies introduced by unconstrained leaders (Fisman, 2001; Fisman et al., 2012; Faccio, 2006; Betz and Pond, 2023), demonstrating their relevance in the second Trump administration. In a recent viral *Substack* post, political scientist Rory Truex (2025) deployed the concept of a tinpot dictator to describe President Trump's inclination to use his office to reward himself and his supporters (Wintrobe, 1990; Olson, 1993). Discussing the numerous changes in trade and financial policies, Truex speculated that anyone with inside knowledge about the timing of these announcements could make a fortune in the market. Our results provide tangible evidence of this possibility by showing the out-sized gains earned by early investors in tainted companies after the Executive Order.

1 Sample construction and data collection

We expect stock market investors to interpret Donald Trump's unexpected halt to FCPA enforcement (on February 10th, 2025) as a sudden lift of law-enforcement-related corruption costs for firms. If investors consider the FCPA costly for its expected enforcement costs, but less so for the burdens of regulation and legal compliance, investing in firms that likely engaged in corrupt activities should have become less risky, and more profitable, after the announcement. If, instead, the regulatory burden of the FCPA hampered the competitiveness of all foreign-operating firms, investment in all MNCs should have increased. This expectation can be empirically tested by considering whether stock symbols traded by "at risk of corruption" firms appreciated after the Executive Order relative to the rest. This would provide evidence that halting FCPA enforcement reduced political risk to these firms by green-lighting corrupt behavior. A general rise in foreign-operating firms after the FCPA would indicate that investors were responding primarily to the legal and compliance costs of corruption. No change in absolute or relative stock evaluation would imply that, instead, investors price corruption as a risky initiative regardless of enforcement costs.

We start by identifying suitable firms that investors would consider "at risk of corruption" (and FCPA action). Measuring foreign bribery risk is complex, as corruption occurs out of the

sight of stock market investors. However, past FCPA enforcement actions or investigations[6] are a valuable heuristic for investors to infer a firm's future corruption risk. Several reasons justify this logic. First, FCPA target firms are often repeat offenders involved in multiple anti-bribery cases over time. To demonstrate this point, in Appendix A we show that more than 40% of all FCPA actions are followed by another action targeting the same firm within ten years. This is explainable both in terms of firms' "type" (e.g., firms operating in certain industries or in certain countries are more likely to be asked for bribes) and also based on the cost—in terms of evidence—required by prosecutors in order to launch new enforcement actions. As Tomashevskiy (2021, 391) notes, investigations into a criminal scheme may uncover new information leading to new enforcement actions. Prosecutors might reduce the evidence costs of enforcement with informational returns to scale from past actions.[7] Similarly, past targets' adoption of compliance and internal monitoring programs often surfaces evidence regarding future FCPA violations. This makes it likely that an FCPA target will risk prosecution or investigation again. In fact, past FCPA histories are also used by market analysts to measure reputational risk exposure. In Appendix B, we show that the measured average reputational risk of a past FCPA target is about 28% greater than that of a comparable non-tainted firm. This increase in risk emerges immediately after an FCPA action but is persistent over the very long term. To the extent that investors consider these ratings to assess corruption risk, a firm's FCPA past begets future anti-corruption enforcement risk.

Based on this logic, our first sample of traded securities comprises firms that faced FCPA enforcement actions or investigations in the past. We obtained the exhaustive list of publicly traded FCPA target firms from Stanford's FCPA Clearinghouse (Stanford Law School, 2025). We manually corrected entries due to name and corporate structure changes since the initial FCPA action.[8] Some firms, such as Norwegian oil major *Statoil* (formerly STO on the NYSE), were later renamed and changed their trading ticker symbols (now *Equinor*, EQNR on the

[6] In our main analysis, we study firms that experienced an FCPA action or an FCPA investigation (followed by no enforcement) in the past, as investors might use both types of FCPA histories as heuristics for future corruption risk. In Appendix L, we show that results are similar when we distinguish among the two groups.

[7] For a similar argument, see Crippa and Kalyanpur (2024).

[8] Table I.5 demonstrates that our findings do not hinge on these corrections.

NYSE). Others underwent mergers and are now part of different corporate groups, such as *Fiat S.p.A.*, now part of *Stellantis N.V.*; or *Zimmer* and *Biomet*, formerly independent (and traded as ZMH on the NYSE and BMET on the NASDAQ, respectively), now *Zimmer Biomet Holdings Inc* (ZBH on the NYSE). We kept firms that had been renamed or merged with other firms but were still publicly traded in our sample. However, we removed cases involving delisted, bankrupt, or defunct firms—such as *Aegerion Pharmaceuticals*, which merged with *QLT Inc* in 2016 to form *Novelion Therapeutics* but filed for bankruptcy in 2019—or firms that have been acquired and are now entirely owned by other companies. Finally, we removed firms whose trading was suspended on US exchanges due to stock market regulations or geopolitical events (e.g., trading of American Depositary Receipts (ADRs) of the Russian *Mobile TeleSystems* was suspended after the invasion of Ukraine in February 2022). The result is a list of 286 publicly traded firms that were involved in at least one FCPA action. Next, we searched through Compustat for firms in the list that publicly traded securities on a US stock exchange.[9] After discarding one penny stock (*Corsa Coal Corporation*) and 24 firms trading on foreign exchanges, we obtained 261 firms that traded securities on a US stock exchange and were targeted in a past FCPA case.

We built a second sample of firms that were *NOT* involved in FCPA actions in the past, which our statistical analysis below employs to discount the effect of Trump's FCPA Executive Order from general market trends or non-FCPA-related shocks. From *Compustat*, we gathered the list of *Standard and Poor's* (S&P) 500 constituents as of February 2025 (i.e., the 500 largest companies by capitalization that trade on US markets). Excluding the 89 *S&P 500* constituent firms that were also involved in an FCPA action, which thus belong to the first sample of companies, this sample comprised a total of 411 firms.

Finally, we built a third "placebo" sample of firms that are comparable to past FCPA targets but have never experienced an FCPA action. We use this placebo portfolio to substantiate our claim that we estimate the effect of the Executive Order on the political risk *of tainted firms*, as priced by investors, rather than the impact of the Executive Order on a generic reduction of

[9] We consider firms trading on the NYSE, the NASDAQ-NMS, or those trading ADRs over-the-counter.

FCPA legal compliance-related costs for similar-situated multinational firms. From the Compustat list of firms trading their securities on North American stock exchanges, we excluded past FCPA targets. From the remaining firms, we used propensity score matching to select a portfolio of 236 firms that are as similar as possible to past FCPA targets, based on observable covariates. Except for not having ever been involved in an FCPA action, this matched placebo sample is significantly similar to past FCPA targets in terms of foreign activity via subsidiaries, financials, and industry characteristics.[10]

For each firm in the three samples, we gathered stock prices at closing on each trading day beginning 180 trading days before Trump's FCPA cessation announcement (i.e., since June 3rd, 2024) until the end of the trading week of the announcement (February 14th, 2025). We use the price at closing on a trading day to compute our outcome variable of interest, measured daily: RETURNS, defined as the percentage change in stock price at closing between two trading days.[11]

2 Research design

We apply the standard two-window event study from corporate finance. Here, we describe our procedure for the sample of past FCPA targets, however, we apply the same design to the matched placebo sample. The goal of the design is to estimate each firm's counterfactual RETURNS after our event of interest (Trump's FCPA Executive Order). We compare observed and counterfactual RETURNS to estimate if (and in what direction) the event affected risk as market participants priced it. To this aim, we divide RETURNS to each firm i into two time windows: an "estimation window" before the event and an "event window" after it. Figure 1 illustrates them. The estimation window spans over the days $t \in [t_0, t_1)$; the event window

[10] We matched on the following covariates. For measuring the activity of the MNC via (foreign or US) subsidiaries: the number of countries of foreign operations; the number of US-based, non-US-based, and total subsidiaries. For financials: whether the firm is an *S&P 500* constituent, the number of common outstanding shares, the average price at closing in December 2024, and the total asset value. We then matched on the exchange market (NYSE, NASDAQ, or over-the-counter). And we matched on the 2-digit industry classification from the North American Industry Classification System (NAICS-2). We describe and document our matching procedure in Appendix C.

[11] We present summary statistics in Appendix E.

covers the days $t \in [t_1, t_2]$ and includes Trump's FCPA order (t_e, February 10th, 2025).

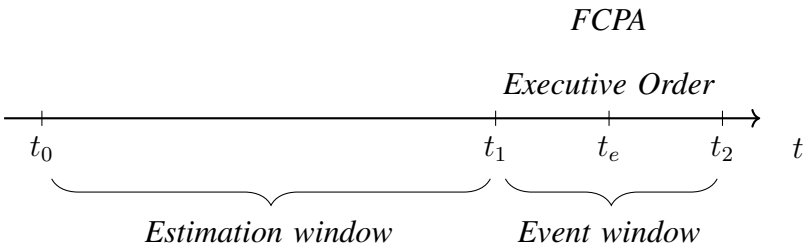

FIGURE 1: Research Design: Estimation and Event Windows

The design estimates counterfactual RETURNS in the event window, for each firm in our sample, using information from the estimation window, which entirely pre-dates the event. Using estimation window data ($t_0 \leq t < t_1$), we estimate a model of daily RETURNS ("market model") for each firm in the sample of past FCPA targets, as a function of predictors ($\boldsymbol{X}$). Once a market model is estimated for each firm, we use it to predict firms' RETURNS over the *full* period in consideration ($t_0 \leq t \leq t_2$). Equations 1 and 2 represent these two steps. When looking at the estimation window ($t_0 \leq t < t_1$), the prediction is useful to assess the quality of our models (e.g., in terms of R^2). When stretched out-of-sample and into the event window ($t_1 \leq t \leq t_2$), this prediction offers RETURNS to a firm i that are counterfactual to the event, under the assumption of no other unrelated shock impacting only the subset of firms of interest.

$$\text{RETURNS}_{it} = \alpha_i + \boldsymbol{X}'_{\boldsymbol{it}}\boldsymbol{\beta}_i + \varepsilon_{it} \mid t_0 \leq t < t_1 \tag{1}$$

$$\widehat{\text{RETURNS}}_{it} = \hat{\alpha}_i + \boldsymbol{X}'_{\boldsymbol{it}}\hat{\boldsymbol{\beta}}_i \qquad \mid t_0 \leq t \leq t_2 \tag{2}$$

Traditional applications use aggregated market indexes—such as the *S&P 500* composite index or the *Dow Jones*—to build a matrix $\boldsymbol{X}$. However, these indices themselves are composed of market movements of companies that might be impacted by Trump's FCPA announcement. For instance, 89 (17.8%) of the firms in the *S&P 500* composite index are themselves past FCPA targets. Therefore, using aggregate indices could bias counterfactual RETURNS. To overcome this, we follow the solution suggested by Wilf (2016). We construct a matrix $\boldsymbol{X}$ using individual RETURNS to each of the 411 *S&P 500* firms that have never been targets

of an FCPA action. Using these 411 stock movements to construct counterfactuals allows us to discount broader market trends that affected traded companies in February 2025 *and* the impact of Trump's FCPA announcement beyond enforcement-risk signals specific to past FCPA targets. Because our estimation has more predictors than observations (we have at most 180 trading days for each market model), we use the "least absolute shrinkage and selection operator" (LASSO, see Tibshirani, 1996) to select which *S&P 500* constituent firms are the most predictive for each past FCPA target. The LASSO excludes non-predictive *S&P 500* firms from each market model.[12] Effectively, for each past FCPA target, we generate a specific index that is the most predictive of stock RETURNS by weighing the *S&P 500* constituents that yield the best fit in the estimation window.

When performing this estimation, we choose to employ the LASSO (and the 411 individual firms' RETURNS as predictors) as opposed to estimating Equation 1 using ordinary least squares (OLS) and an aggregated market index (such as the *S&P 500* composite). We arbitrarily choose the length of the estimation window $[t_0; t_1)$. Also, we choose an arbitrary number of cross-validation (CV) folds when using the LASSO. We tested fifteen possible sets of market models resulting from all combinations of these arbitrary choices—we considered OLS and LASSO, estimation windows of 180, 90, or 30 days, and several LASSO CV folds of 3, 5, 10, or 15—and selected the single combination that yields the best prediction (measured in terms of average R^2): the LASSO implemented on an estimation window starting thirty days and ending five days before the event, with 15-folds CV (average R^2 of these market models is 0.67 for past FCPA targets, 0.666 for matched placebos). In Appendix D, we fully describe our estimation results: we show the distribution of the R^2 yielded by these fifteen combinations and the distribution of the parameters estimated by the LASSO. We also demonstrate that our results are robust to varying these arbitrary choices in Appendix H.

Once LASSO market models are estimated, we focus on an event window data ($t_1 \leq t \leq t_2$) and obtain two measures for each firm: abnormal returns (AR) and cumulative abnormal returns

[12] The LASSO associates a set of non-negative weights to each predictor and selects the single set of weights that maximises the fit through cross-validation. This process generates 95 lambdas (penalty coefficients) that balance the bias-variance tradeoff, and the algorithm chooses the lambda with the minimum mean cross-validated error. The optimal set of weights assigns zero weights to non-predictive *S&P 500* constituents.

(CAR) as shown in Equation 3. AR are daily differences between observed and expected RETURNS, representing the residuals between observations and counterfactuals. CAR are the running daily sum of AR for a firm after the event. They help assess whether a positive (negative) event effect accumulates to a significant gain (loss).

$$\begin{aligned} \text{AR}_{it} &= \text{RETURNS}_{it} - \widehat{\text{RETURNS}}_{it} \quad | \, t_1 \leq t \leq t_2 \\ \text{CAR}_{it} &= \sum_{\tau=t_1}^{t} \text{AR}_{i\tau} \qquad\qquad\qquad\qquad | \, t_e \leq t \leq t_2 \end{aligned} \tag{3}$$

The final step consists of studying the daily evolution of average AR and CAR. We do so by calculating event-window average AR and CAR and testing whether these averages are statistically distinguishable from zero. In the appendix, we complement this approach with firm fixed-effect linear regressions and parametric and non-parametric tests proposed by the corporate finance literature (Appendix J). Across our statistical tests, we impose a level of significance of 0.05 (or, equivalently, we consider whether 95% confidence intervals overlap with 0).

3 Results

We find that investors interpreted Trump's halt to FCPA enforcement as a significant reduction to their risk of investing in companies likely entertaining corrupt behavior. Figure 2 reports average observed and counterfactual RETURNS for past FCPA targets in the estimation window (shaded gray area) and event window. In the estimation window, our market models achieve a reasonable predictive power of 67% (see Appendix D), with counterfactual RETURNS following the observed trend quite closely. In the event window, and following Trump's Executive Order, past FCPA targets realized larger RETURNS than what was expected based on information from just a few days before.

How substantial is the positive effect? In order to answer this question, we estimate daily average AR and CAR. We present estimates for past FCPA targets—and 95% confidence inter-

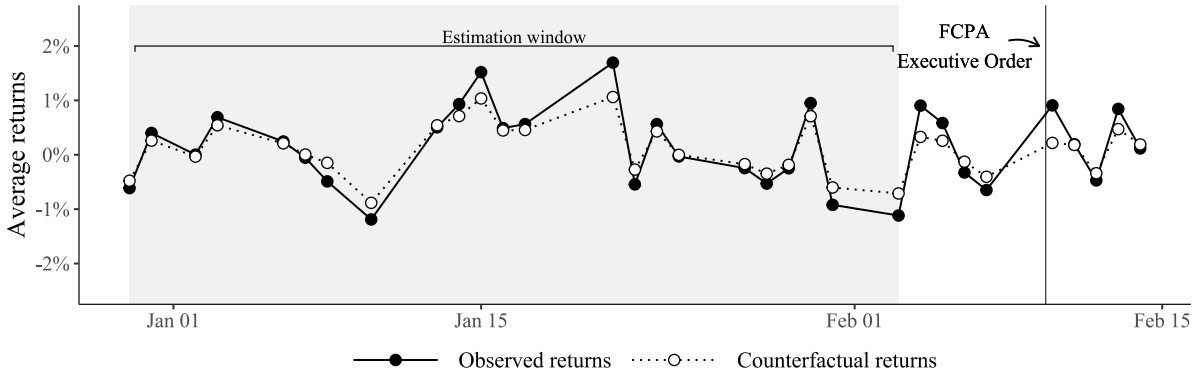

FIGURE 2: Average Observed and Counterfactual RETURNS for Past FCPA Targets before and after Trump's Cessation of FCPA Enforcement. The shaded area indicates the estimation window, and the vertical line is the date of Trump's Executive Order (February 10th, 2025)

vals (CIs)—in the top panel of Figure 3.[13] Before Trump's Executive Order, past FCPA targets recorded returns that were abnormal and statistically significantly above market expectations on February 4th (0.576 percentage points) and February 5th (0.326). They, too, might be related to Trump's lenient approach to corporate corruption. On February 4th, Trump's appointee as an Attorney General, Pam Bondi, took office. Formerly a lobbyist and generally seen as a supporter of corporate interests,[14] on February 5th Bondi issued a memorandum mandating the DOJ to redirect enforcement priorities (including of the FCPA) to focus on the "total elimination of cartels and transnational criminal organizations."[15] Investors might have interpreted this memo as an early deregulatory signal that the FCPA's original mandate was to be weakened, deciding to buy into former FCPA targets. We return to this possibility below.

After Trump's FCPA Executive Order of Monday, February 10th, previous FCPA targets closed their trading days with returns that were, on average, 0.69 percentage points above market expectations, a statistically significant estimate. We calculate that this amounts to an increase in market capitalization for the average past FCPA target of about $160M in just a single day,[16] which is comparable to the average FCPA monetary sanction ($150M). We calcu-

[13] Numeric estimates are reported in Appendix F.
[14] See: https://www.theguardian.com/us-news/2024/nov/21/who-is-pam-bondi-trump-pick-attorney-general.
[15] See: https://www.jdsupra.com/legalnews/us-doj-outlines-new-enforcement-8279099/ and Appendix M.
[16] We compute the change in market capitalization for each firm by multiplying the number of outstanding shares per firm by the price at closing on the last trading day before the Executive Order and by the AR on the day of the event (divided by 100, as AR is expressed on a percent scale). Next, we average the calculated change in capitalization.

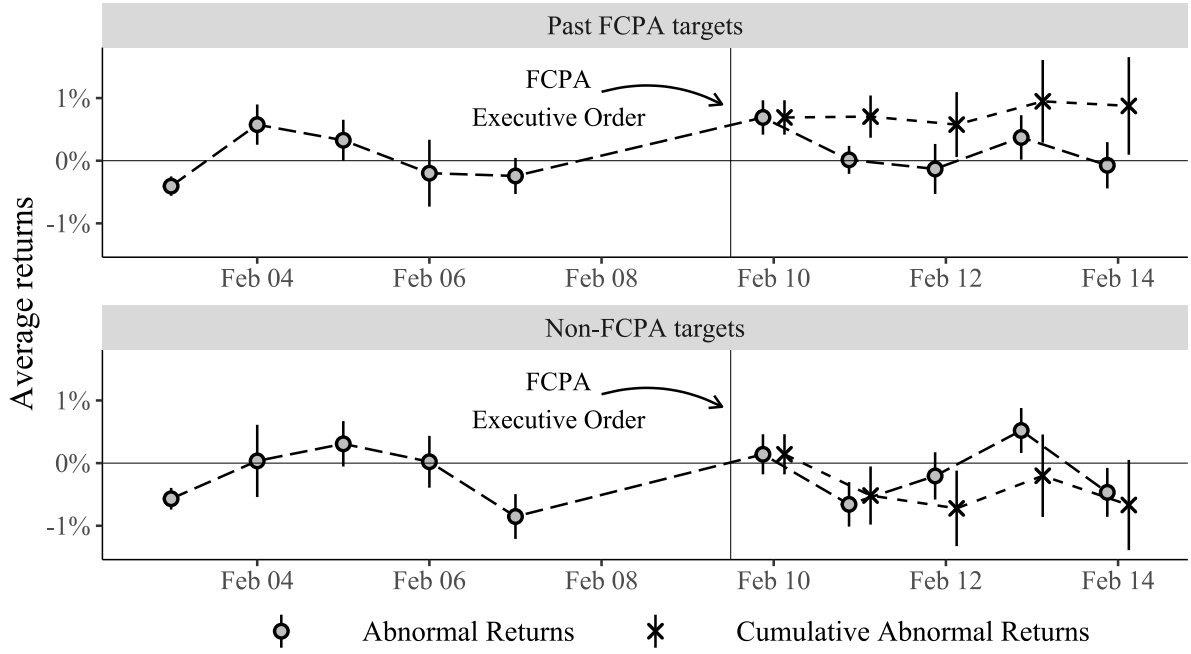

FIGURE 3: Estimated Effects of Trump's Halt to FCPA Enforcement on Average AR and CAR of Past FCPA Targets and Matched Placebo Sample of Firms Never Implicated in an FCPA case. Full estimates in Table F.1. 95% CIs displayed as vertical bars

late that some firms, however, recorded capitalization gains in the orders of (tens of) billions of dollars, gains that are sometimes larger than the largest FCPA fine ever imposed.[17] We report individual estimated effect sizes in Appendix G. When summed up, the portfolio of past FCPA targets recorded capitalization gains of $39B. The effect on AR quickly vanished the following day, consistent with the "efficient market" hypothesis presumption that new price-relevant information is quickly absorbed in a company's stock price (Malkiel, 1989). Nevertheless, the effect cumulated to a substantively positive CAR (reaching a maximum of +0.948 percentage points) that is detected over the entire trading week.

Are we confident that such effects are specific to FCPA-tainted firms? Our research design already discounts broader market trends and shocks (at least to the extent that they affected the 411 *S&P 500* constituents that we use to construct our counterfactual). To further probe the internal validity of our estimates, we turn to our matched placebo sample of 236 comparable firms that have not been involved in a past FCPA action. We report average AR and CAR

[17] For a top ten of FCPA sanctions, see: https://fcpa.stanford.edu/statistics-top-ten.html.

when replicating our procedure for this placebo sample in the lower panel of Figure 3. In this comparable portfolio of firms that were never FCPA targets, we detect an isolated positive effect after February 4th, suggesting that the Bondi memo might have provided price-relevant information for this sample of firms, too, generating a pre-event that is not statistically different from that documented for the FCPA sample (see Table F.1 for estimates of difference in effects). Instead, we detect no effect of Trump's Executive Order on this placebo portfolio of firms, neither on AR nor on CAR, reassuring us of the internal validity of our findings.

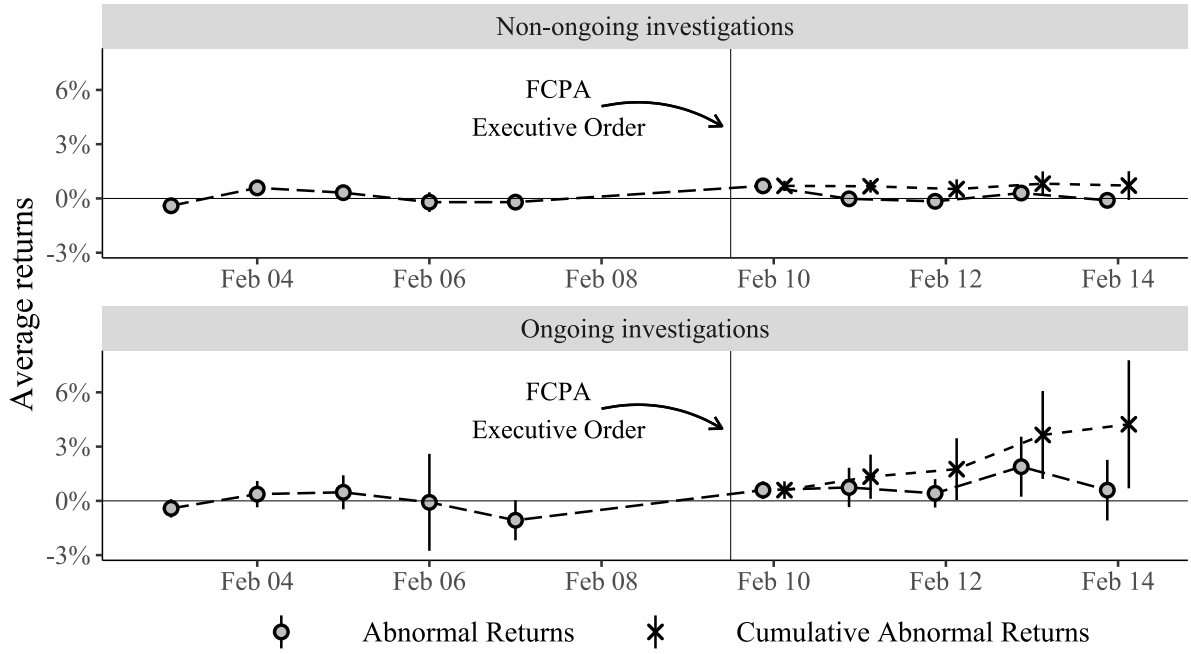

FIGURE 4: Estimated Effects of Trump's Halt to FCPA Enforcement on Average AR and CAR of Past FCPA Targets by Whether Investigation Is Ongoing or Not. Full estimates in Table F.2. 95% CIs displayed as vertical bars

Next, we provide evidence that it was firms that were at the highest risk of legal enforcement that experienced the largest abnormal returns. Firms with ongoing FCPA investigations were those that, directly before the Executive Order, were facing a stronger and immediate FCPA enforcement risk—as opposed to facing a potential future risk informed by past FCPA histories. In Figure 4, we leverage this intuition and partition the sample of past FCPA targets by whether they were experiencing a disclosed ongoing FCPA investigation at the time of the Executive Order (N = 12) or not (N = 249). Despite the smaller sample size, we detect a stronger

effect for the sample of firms experiencing an ongoing FCPA investigation, which cumulates to a 4.234 percentage point increase at the end of the trading week (almost 5 times as large as the largest CAR effect documented in the full sample, Figure 3). Even though firms with ongoing FCPA investigations represent a rather small sample, this cumulative CAR effect is statistically significantly higher than that experienced by the past FCPA targets without a (disclosed) ongoing FCPA investigation (see Table F.2). This finding further confirms our argument. Investors responded to Trump's Executive Order halting FCPA enforcement as implying a lower enforcement risk for firms that were currently at the highest risk of anti-corruption legal enforcement actions.

3.1 Robustness tests

Our results are robust to several tests. In Appendix H, we show that our results do not hinge on the arbitrary choices adopted in our estimation phase (estimation window lengths, number of cross-validation folds, and even the choice of the LASSO over OLS). In Appendix I, we show similar results when we exclude: firms with poorly estimated counterfactuals; one firm at the time from the samples of past FCPA targets and placebo firms; the ten past FCPA targets who realized outlier-sized gains; firms trading securities over-the-counter; past FCPA targets whose ownership history we manually reconstructed; and when we limit the sample of past FCPA targets to those that are matched to the placebo firms (i.e., those for whom we have covariate information). In Appendix J, we show similar findings when adopting linear regression models with firm-fixed effects, industry-fixed effects, or other parametric and non-parametric tests proposed by corporate finance to account for event-induced changes in the variance of RETURNS. In Appendix K, we show that our findings do not even hinge on the chosen research design: we find similar effects when estimating a difference-in-differences of Trump's Executive Order on past FCPA targets' stock price at closing, where we use matched placebo firms as a control group. Finally, in Appendix L we explore heterogeneous effects for the sample of past FCPA targets. We distinguish between whether the past FCPA targets were subject to an enforcement action or just to an investigation. And we study how estimated effects varied based on the

last time a firm experienced an FCPA action. We find little evidence of heterogeneous effects according to these features.

We dedicate a set of final considerations to the significant effect detected on February 4th and 5th, in conjunction with Pam Bondi taking office and circulating a memo to all DOJ employees to redirect enforcement priorities. We argue that this memo, although it provided price-relevant information to investors (hence the early effect detected for past FCPA targets and matched placebo firms on February 4th and 5th), did not clearly anticipate the decision to halt FCPA enforcement in the upcoming Executive Order. The FCPA is only the subject of two paragraphs of the five-page-long memo, which rather aimed at reshaping the entire DOJ priorities (not just FCPA enforcement) to "pursue [the] *total elimination* of Cartels and Transnational Criminal Organizations (TCOs)" (emphasis in the original). In the paragraphs concerning the FCPA,[18] the memo never alludes to the possibility of halting enforcement. On the contrary, Bondi orders the *expedition* of enforcement of the FCPA for cases of bribery associated with Cartels and TCOs, which would now not require anymore authorization for investigation by the Criminal Division or the involvement of the Fraud Section. That is, the memo indicates a shift in priorities (from corporate crime in and of itself to corporate crime linked with TCOs) but does not suggest a halt to enforcement. The positive effect detected on February 4th and 5th for both samples should thus be understood as a result of such a broader de-regulatory approach of the DOJ in conjunction with Bondi taking office, but it does not seem to be plausibly related to an anticipation of FCPA enforcement halt in and of itself. Confirming this interpretation, in our main results and across almost all our robustness tests, we detect a comparable effect for the past FCPA targets and placebo firms on the day the memo was issued. In contrast to the FCPA halt, the Bondi memorandum increased returns for both targeted firms and similarly situated foreign-operating firms without tainted histories.

[18] We report the paragraphs pertaining to the FCPA verbatim in Appendix M.

4 Conclusion

Our analysis demonstrates that President Trump's Executive Order to pause enforcement of the FCPA disproportionally benefited tainted firms, who were targets of previous FCPA investigations or enforcement actions and were at the greatest risk of lapsing back into corrupt behaviors. The ultimate conclusion is robust to alternative specifications and sensitivity tests. In sum, the average benefit for "at risk" firms was $160 million in increased capitalization. A value that matches the size of the average FCPA fine imposed thus far. A few past FCPA targets, however, recorded gains in the order of (tens of) billions of dollars, more than any single FCPA fine previously imposed. And, when taken together, the portfolio of past FCPA targets realized in just one day an abnormal market capitalization gain of $39 billion. Shareholders, who reacted early to this surprising action, banked an enormous single-day return.

The primary alternative explanation can be dismissed by our results. If investors were reacting to the reduction in compliance costs, as Trump suggested, we should not see a difference between the set of tainted firms and the placebo set of firms operating in similar industries with similar global reach. These findings also contribute to a growing literature on the benefits of connections and information in concentrated political systems with limited transparency and unchecked arbitrary policy making (Fisman, 2001; Faccio, 2006). More broadly, they shed light on a theoretical debate regarding the costs of corruption, demonstrating that investors are willing to accept the inefficiencies inherent in "at risk" businesses if they believe that the penalties for corrupt behaviors are minimal (Kaufmann and Vicente, 2011; Banerjee, Gupta, and Krishnamurti, 2022).

While it was beyond the scope of this research note, an immediate next research step is to explore the heterogeneous impact of the pause in FCPA enforcement. Which types of firms saw the greatest benefits? Table G.1 provides an ordered list of the fifty greatest beneficiaries from the Executive Order. Those with the largest abnormal returns appear to be in extraction, construction, transportation, and communications, which are widely believed to be the most corrupt industries in the world because of the necessity of acquiring local permissions and li-

censes, as well as their exposure to arbitrary regulations (Kottasova, 2014). Investors may have sensed that these firms would now have a competitive advantage relative to their competitors in permission and access. In addition, the firms that saw the largest gains in capitalization are truly global operators, such as *Toyota, Walmart, Siemens, HSBC, and Exxon*, with branches, subsidiaries, and outlets all over the world, including locations highly prone to corruption. In these cases, investors were certainly responding to the reduced costs of potential anti-corruption enforcement in their regular operations. At this stage, these patterns are impressionistic. Careful coding and testing are necessary to uncover how the firm-level heterogeneity relates to our larger story.

The findings also raise important new and broader questions for future research. First, what will happen to efforts to internationalize anti-corruption efforts through the OECD ABC, which was strongly advocated for by the United States with the express purpose of expanding extraterritorial enforcement to the companies of major US partners and competitors (Abbott and Snidal, 2002)? The OECD ABC has increased its enforcement and effectiveness over the past decade (Jensen and Malesky, 2018), but the US action could easily spur a wave of retrenchment and reversals among member states, who no longer see the benefits of constraining the actions of their MNCs (Brewster, 2017). Second, what will be the impact on corruption in third-party states, particularly emerging markets, from the US withdrawal and the potential OECD ABC knock-on effects? Previous work demonstrates both a reduction in corrupt activity, but also a reluctance among businesses from restricted states to invest in more corrupt countries and sectors (Cuervo-Cazurra, 2008; Crippa, 2023). Will we see a corresponding increase in corruption in third-party states that lack the capacity or political will to investigate and punish corruption within their borders? If so, how will this affect the governance and welfare of citizens of those countries?

Certainly, these are only a small subset of the questions that will emerge from Trump's Executive Order. The pause of FCPA enforcement has initiated the unwinding of an international political economy architecture that has existed for almost five decades. Governments, firms, and citizens are only now beginning to respond to the dramatic change in incentives that have

been unleashed.

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Online Appendix

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

Contents

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.

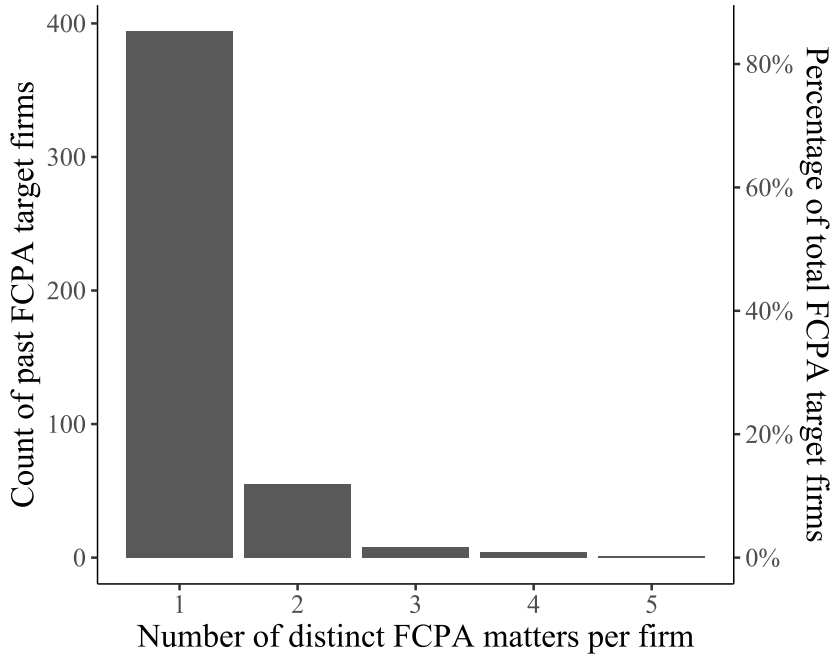

FIGURE A.1: Distribution of the number of distinct FCPA matters that publicly traded and privately owned FCPA targets were involved in

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[19] 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

[19] 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.

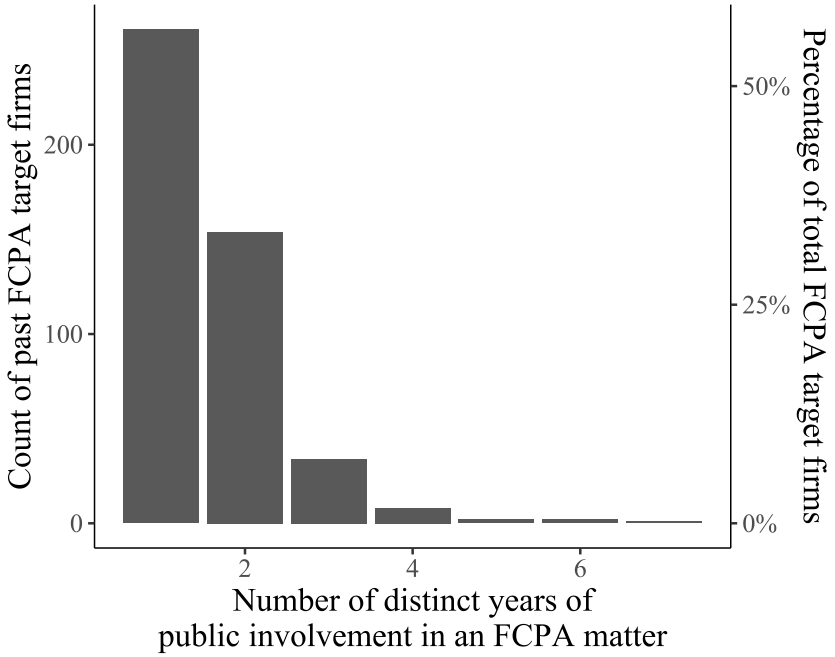

FIGURE A.2: Distribution of the number of distinct years of public involvement in FCPA investigations or enforcement actions for publicly traded and privately owned FCPA targets

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),

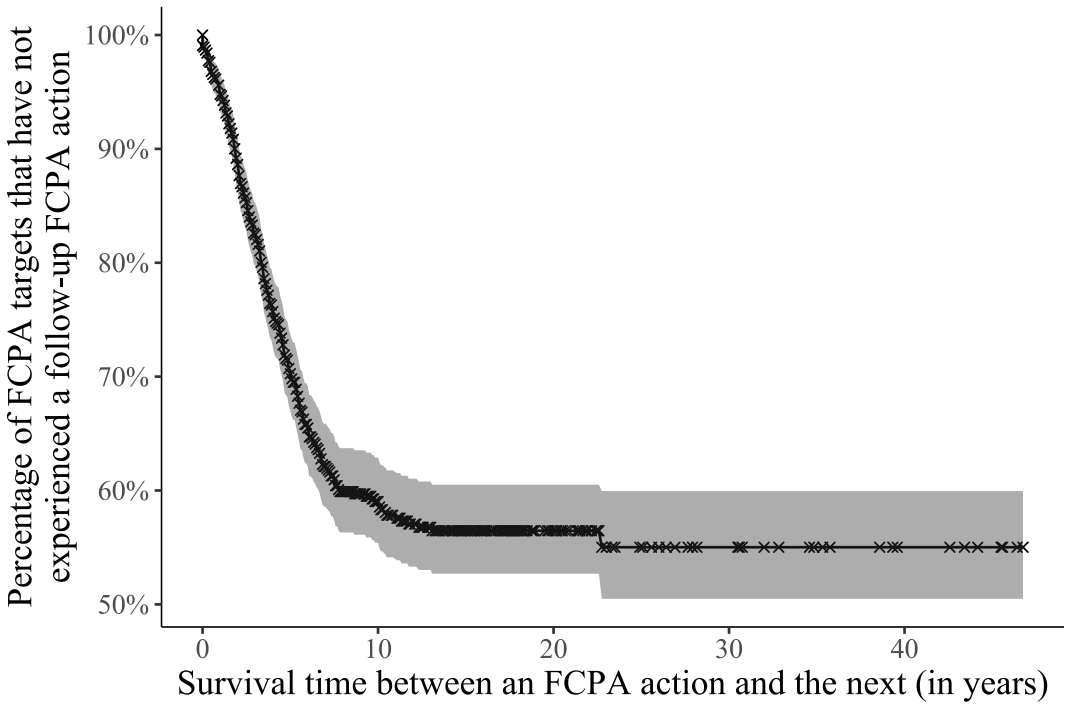

FIGURE A.3: Survival rate of past FCPA targets (publicly traded or privately owned) to a follow-up FCPA action (enforcement or investigation) as time passes. Results from a Kaplan-Meier estimation with no covariates. 95% CIs displayed as shaded area

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). 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 4 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.

$$\text{RRI}_{it} = \alpha_i + \gamma_t + \sum_{r \neq t_e - 1} \delta_r \times 1[\text{FCPA}_i = 1] \times 1[t = r + 1] + \varepsilon_{it} \tag{4}$$

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.

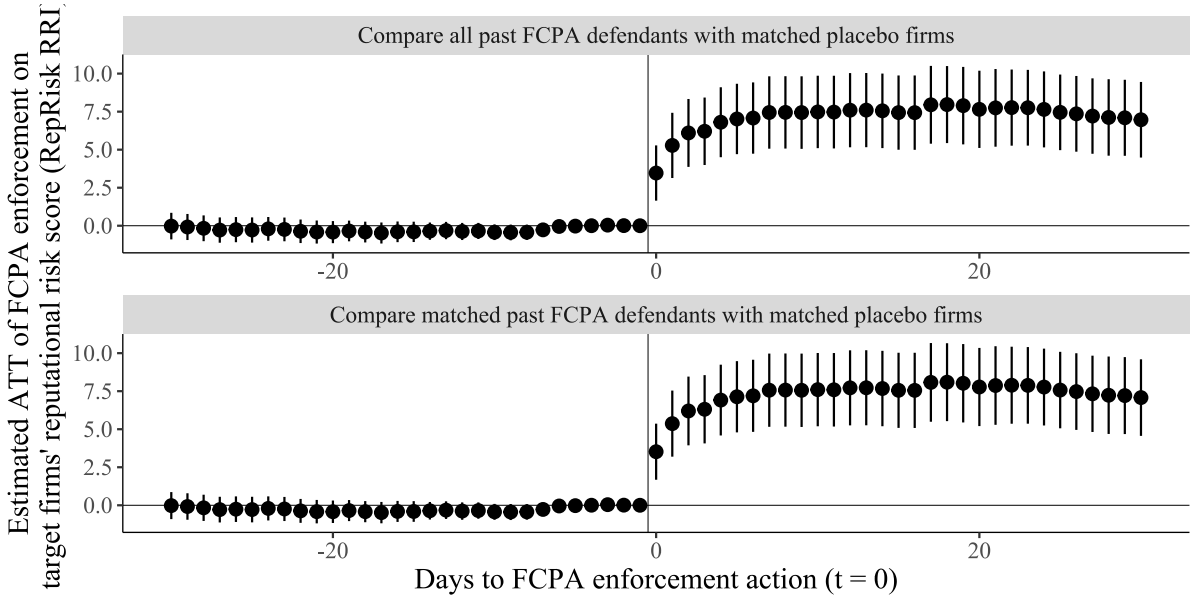

FIGURE B.1: Difference-in-differences event analysis of firms' RRI following an FCPA enforcement action (1 month). 95% CIs displayed as vertical bars

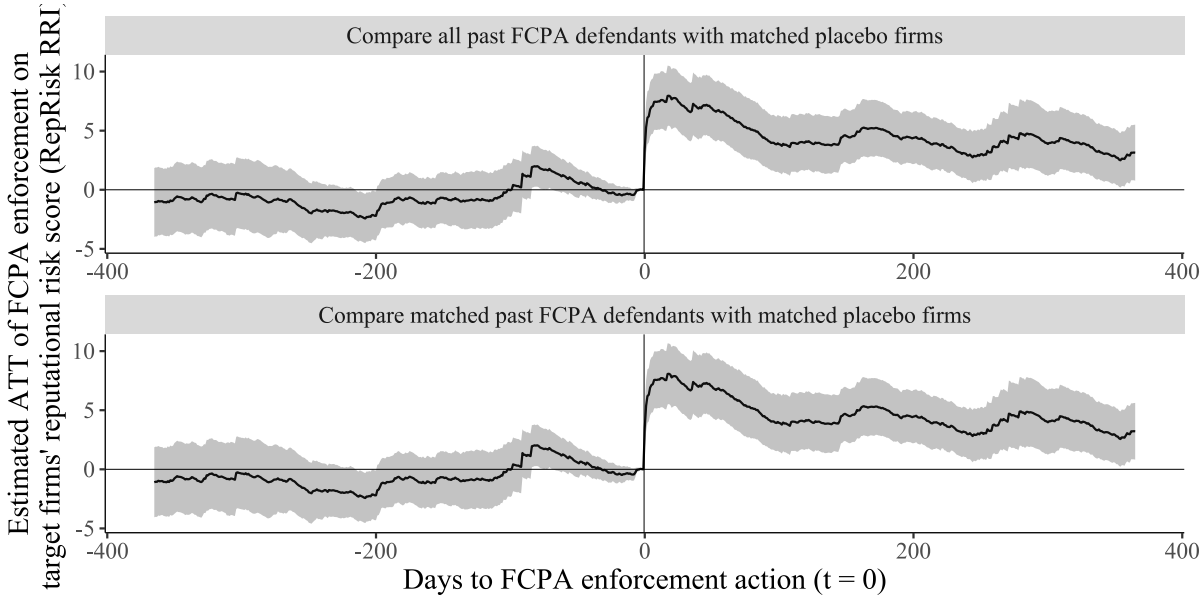

FIGURE B.2: Difference-in-differences event analysis of firms' RRI following an FCPA enforcement action (1 year). 95% CIs displayed as shaded area

C Constructing a placebo set of non-FCPA targets with propensity score 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 3,648 potential placebo candidate firms.

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 propensity score matching. 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 236 of the 261 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 weighting to match potential placebo firms to past FCPA targets, based on these covariates. We matched one control firm per each past FCPA target. The resulting set included 236 placebo firms.

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. 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 $+0.25$).

As reported above, the matched set is based on a subset of 236 of the 261 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.

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

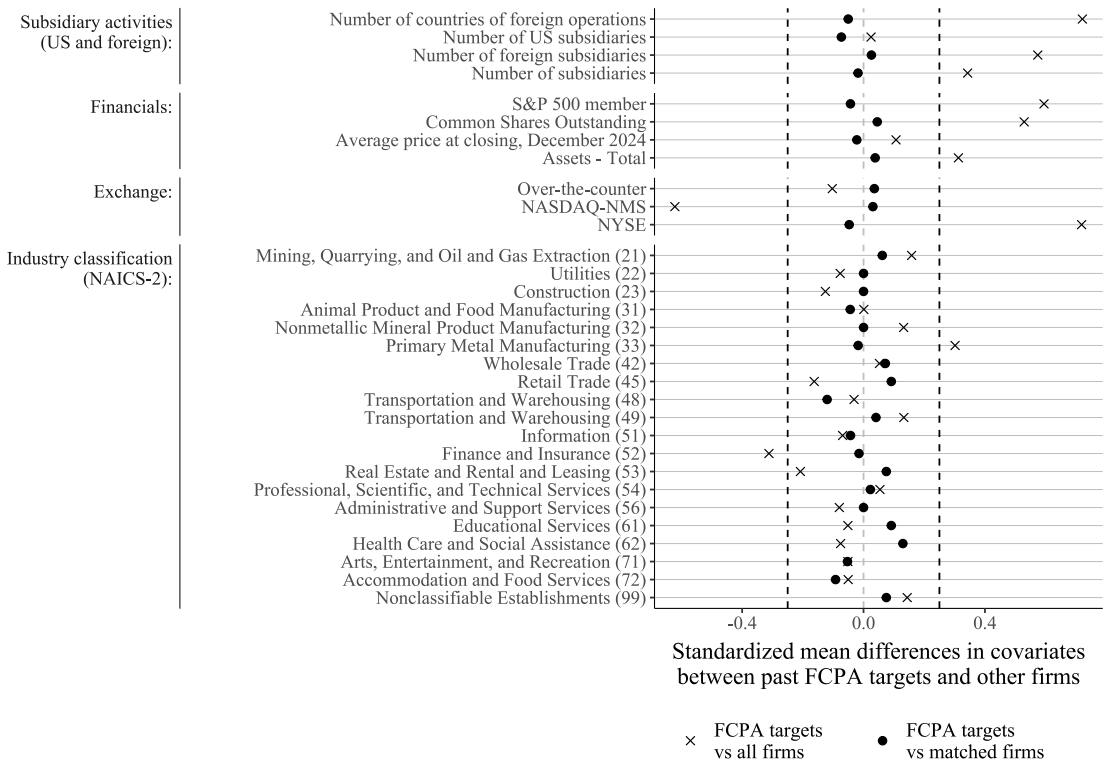

FIGURE C.1: Standardized mean difference in covariates between the sample of past FCPA targets and the matched placebo sample of non-FCPA targets

study, namely 12 LASSO models using *S&P 500* constituents and 3 OLS models using the aggregate S&P 500 index. 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 0.670. 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 0.444, almost four times as large as that of the better fitting OLS (one with the same estimation window length, 0.112).

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

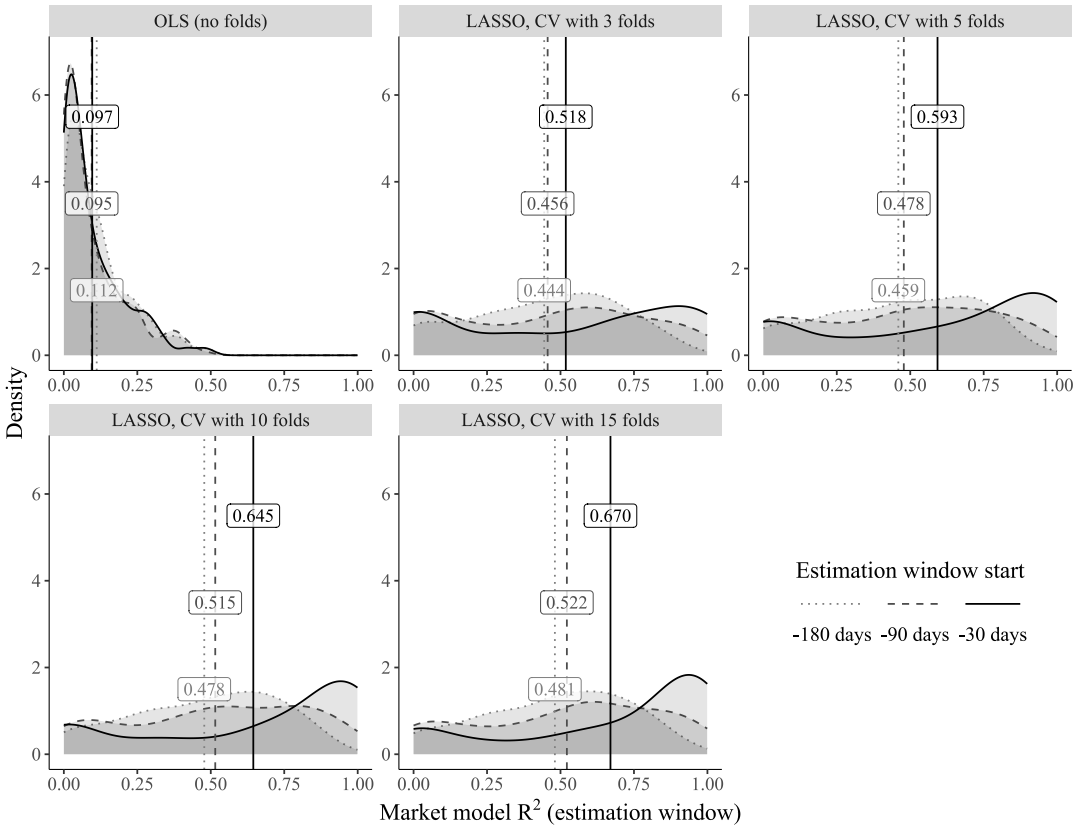

FIGURE D.1: Distribution of R^2 from market models of past FCPA targets

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.

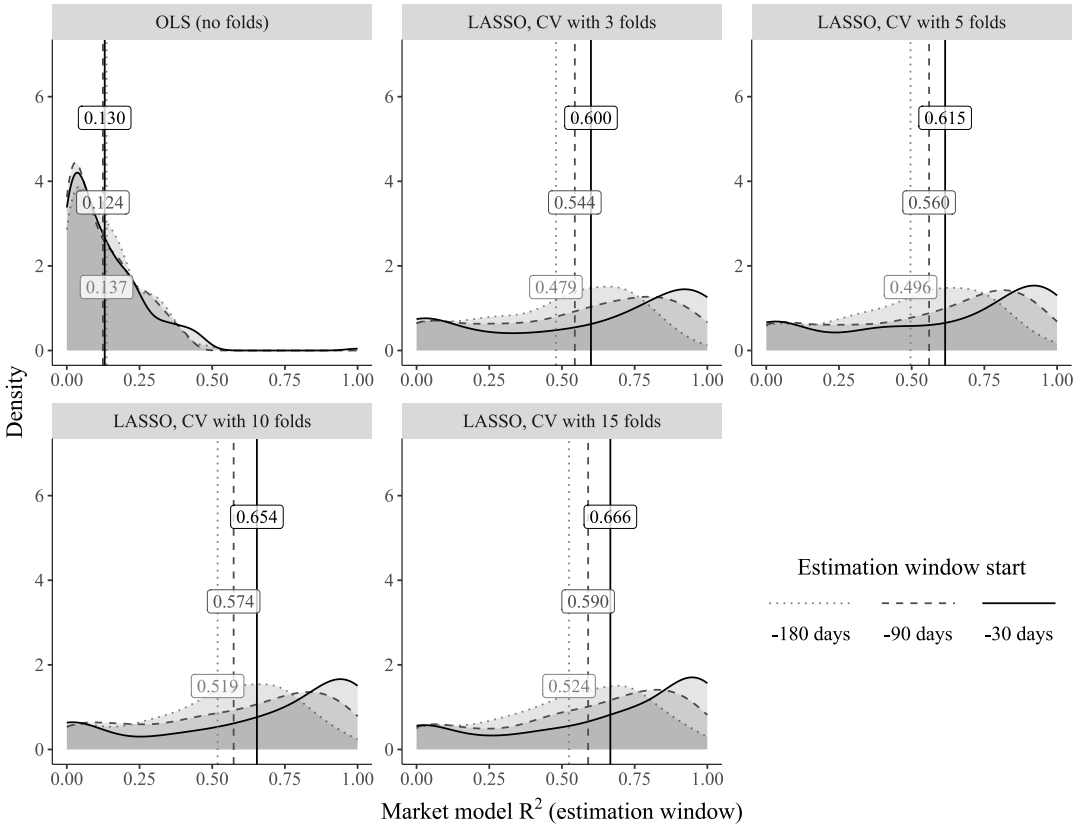

FIGURE D.2: Distribution of R^2 from market models of matched placebo

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.

Table E.1: Descriptive statistics of relevant variables

	N	Mean	SD	Min	P25	Median	P75	Max
Outstanding shares (B)	4770	0.922	1.999	0.004	0.071	0.237	0.833	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

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 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 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.

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

Rank	Ticker	Company name	AR (%)	Capitalization gain (M of $)	Shares (M)	Price ($)
1	WMT	WALMART INC	1.458	11836.929	8024.000	$101.15
2	MCD	MCDONALD'S CORP	5.247	11043.175	715.200	$294.30
3	MSFT	MICROSOFT CORP	0.359	10951.405	7435.000	$409.75
4	UBER	UBER TECHNOLOGIES INC	5.135	8074.203	2107.953	$74.60
5	XOM	EXXON MOBIL CORP	1.162	5506.541	4353.000	$108.89
6	QCOM	QUALCOMM INC	2.217	4118.158	1106.000	$167.96
7	ALIZF	ALLIANZ SE	3.233	3972.763	385.919	$318.45
8	ROK	ROCKWELL AUTOMATION	12.270	3723.829	113.073	$268.40
9	SIEGY	SIEMENS AG	1.920	3234.245	1569.738	$107.30
10	ABT	ABBOTT LABORATORIES	1.247	2787.029	1731.698	$129.07
11	EQNR	EQUINOR ASA	3.848	2668.785	2944.733	$23.55
12	ACN	ACCENTURE PLC	1.078	2605.615	626.445	$385.98
13	DE	DEERE & CO	1.964	2481.559	271.414	$465.60
14	LMT	LOCKHEED MARTIN CORP	2.331	2423.815	234.000	$444.39
15	TM	TOYOTA MOTOR CORP	0.932	2246.717	1309.591	$183.98
16	DTEGY	DEUTSCHE TELEKOM	1.271	2154.581	4978.615	$34.04
17	LIN	LINDE PLC	0.845	1819.225	473.237	$455.05
18	HON	HONEYWELL INTERNATIONAL INC	1.317	1758.807	649.800	$205.52
19	HSBC	HSBC HLDGS PLC	0.833	1584.616	3583.641	$53.10
20	NVS	NOVARTIS AG	0.750	1573.424	1975.089	$106.26
21	NEM	NEWMONT CORP	3.092	1562.334	1127.000	$44.84
22	CVX	CHEVRON CORP	0.540	1457.650	1769.012	$152.62
23	BHP	BHP GROUP LTD	0.965	1231.854	2535.137	$50.35
24	SAP	SAP SE	0.359	1170.642	1166.589	$279.64
25	SLB	SCHLUMBERGER LTD	2.031	1142.322	1400.850	$40.16
26	JCI	JOHNSON CONTROLS INTL PLC	1.913	1107.892	660.594	$87.65
27	MDT	MEDTRONIC PLC	0.938	1082.899	1283.266	$90.01
28	FDX	FEDEX CORP	1.692	1038.390	239.585	$256.08
29	NOC	NORTHROP GRUMMAN CORP	1.446	982.120	144.952	$468.58
30	TS	TENARIS SA	4.612	971.976	554.429	$38.01
31	BSX	BOSTON SCIENTIFIC CORP	0.618	958.678	1474.556	$105.25
32	TEL	TE CONNECTIVITY PLC	2.046	897.982	298.766	$146.88
33	BKR	BAKER HUGHES CO	1.886	862.168	989.646	$46.20
34	AZN	ASTRAZENECA PLC	0.359	801.477	3101.092	$71.99
35	PBR	PETROLEO BRASILEIRO SA- PETR	0.869	768.365	6444.366	$13.72
36	EW	EDWARDS LIFESCIENCES CORP	1.806	747.623	588.600	$70.35
37	STLA	STELLANTIS NV	1.909	711.469	2880.492	$12.94
38	BAM	BROOKFIELD ASSET MANAG LTD	0.702	662.699	1630.525	$57.91
39	PFE	PFIZER INC	0.429	625.081	5667.000	$25.74
40	MDLZ	MONDELEZ INTERNATIONAL INC	0.787	605.822	1317.829	$58.45
41	GFI	GOLD FIELDS LTD	3.547	592.950	895.024	$18.68
42	HPQ	HP INC	1.906	581.135	944.660	$32.27
43	GWW	GRAINGER (W W) INC	1.139	569.996	48.333	$1035.80
44	HAL	HALLIBURTON CO	2.587	565.628	868.000	$25.19
45	OXY	OCCIDENTAL PETROLEUM CORP	1.239	543.768	938.458	$46.78
46	ABBNY	ABB LTD	0.495	497.487	1838.192	$54.63
47	GSK	GSK PLC	0.663	487.400	2040.409	$36.04
48	LHX	L3HARRIS TECHNOLOGIES INC	1.169	457.441	189.795	$206.13
49	JNJ	JOHNSON & JOHNSON	0.124	455.266	2406.922	$153.12
50	BK	BANK OF NEW YORK MELLON CORP	0.735	454.521	717.680	$86.15

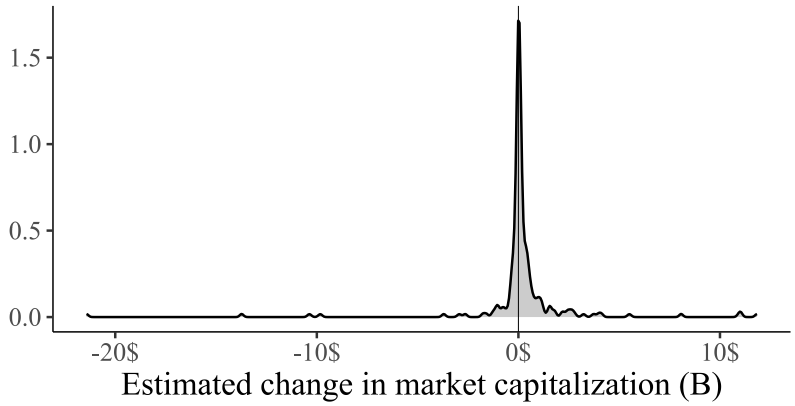

FIGURE G.1: Estimated market gains for firms under study on the day of Trump's FCPA Executive Order

H Robustness to estimation choices

H.1 Change number of CV-folds for the LASSO

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

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.

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	-0.529*		-0.585*		0.056	
	(0.118)		(0.120)		(0.168)	
Tue, Feb 04 2025	0.373*		-0.091		0.464*	
	(0.152)		(0.153)		(0.216)	
Wed, Feb 05 2025	0.312*		0.345*		-0.033	
	(0.157)		(0.160)		(0.224)	
Thu, Feb 06 2025	-0.591*		-0.082		-0.509	
	(0.250)		(0.200)		(0.320)	
Fri, Feb 07 2025	0.061		-0.170		0.230	
	(0.137)		(0.134)		(0.192)	
Post-event:						
Mon, Feb 10 2025	0.416*	0.416*	-0.301*	-0.301*	0.717*	0.717*
	(0.141)	(0.141)	(0.152)	(0.152)	(0.207)	(0.207)
Tue, Feb 11 2025	0.175	0.590*	-0.487*	-0.788*	0.662*	1.379*
	(0.102)	(0.168)	(0.167)	(0.221)	(0.196)	(0.278)
Wed, Feb 12 2025	-0.248	0.324	-0.394*	-1.182*	0.146	1.506*
	(0.200)	(0.250)	(0.179)	(0.270)	(0.268)	(0.368)
Thu, Feb 13 2025	0.085	0.409	0.266	-0.916*	-0.181	1.325*
	(0.168)	(0.301)	(0.173)	(0.309)	(0.241)	(0.431)
Fri, Feb 14 2025	0.142	0.551	-0.116	-1.033*	0.259	1.584*
	(0.169)	(0.352)	(0.162)	(0.339)	(0.234)	(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.

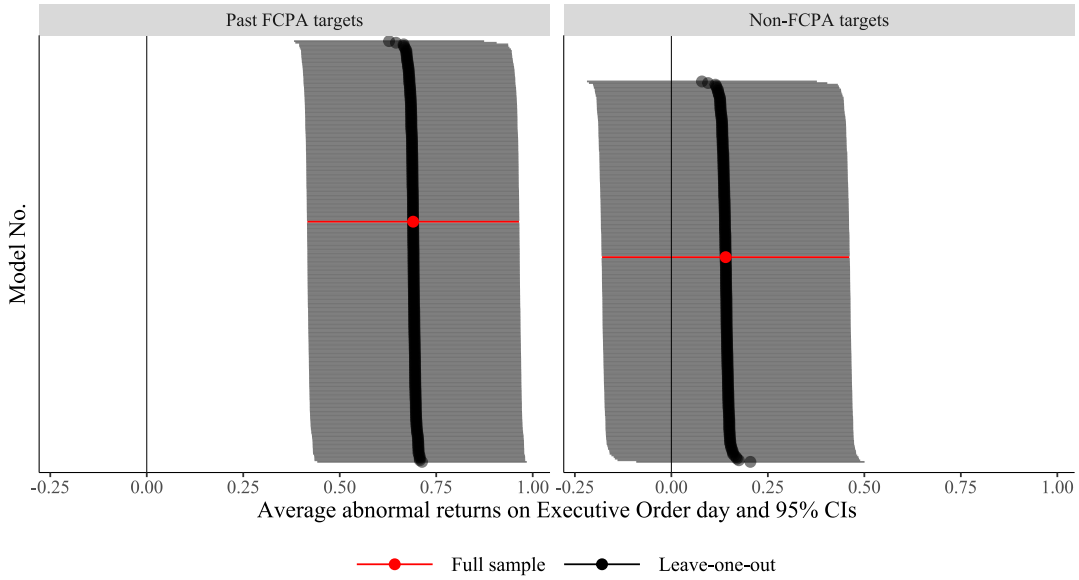

FIGURE I.1: Jackknife test: Average AR on the day of the Executive Order when excluding one firm from each sample at the time. Red estimates report the average from the full sample for comparison.

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 tests by Brown and Warner (1980), Brown and Warner (1985), regular t-test, 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 an alternative research design: Difference-in-differences

Here, we show that our results do not hinge on the chosen event-study research design. In table K.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.

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 K.1 in Figure K.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:

$$\text{PRICE}_{it} = \alpha_i + \gamma_t + \sum_{r \neq t_e - 1} \delta_r \times 1[\text{FCPA}_i = 1] \times 1[t = r + 1] + \varepsilon_{it} \tag{5}$$

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

Results of our difference-in-differences event study—in the form of dynamic average treatment effects on the treated (ATTs)—are in Figure K.1. We present results relative to the two trading weeks before the Executive Order, and up until the beginning of the third week after

Table K.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.

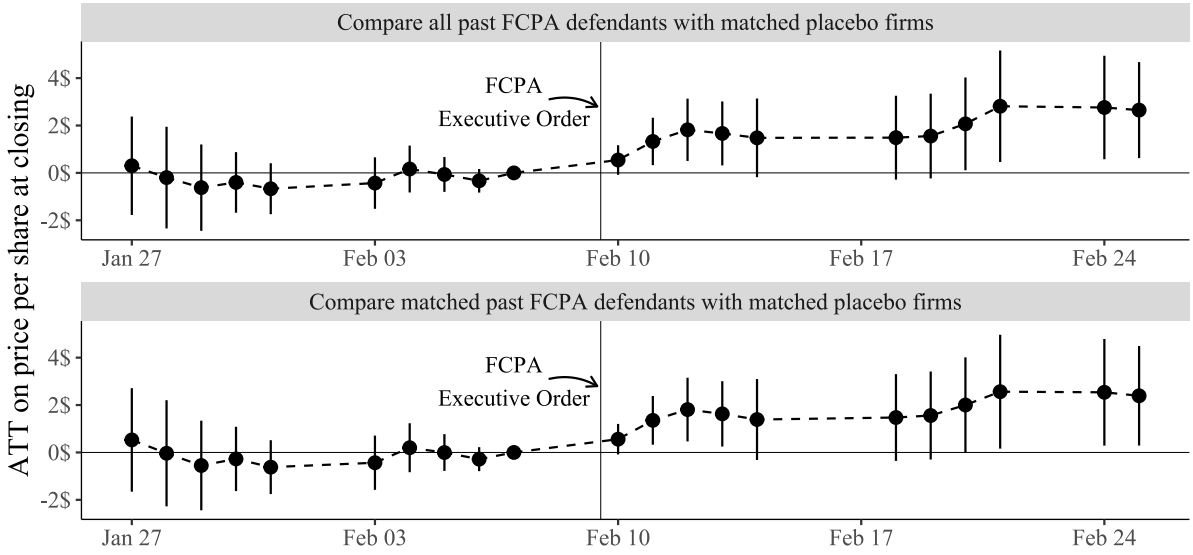

FIGURE K.1: Difference-in-differences event analysis of firms' price at closing following the FCPA Executive Order. 95% CIs displayed as vertical bars

the Order. 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 do not observe a pre-event significant trend; instead. This 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 Heterogeneous effects

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

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 L.1, we report average AR (and 95% CIs) for past FCPA targets based on their timing of 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, but the estimate is indistinguishable from that of the previous time points.

Table L.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.

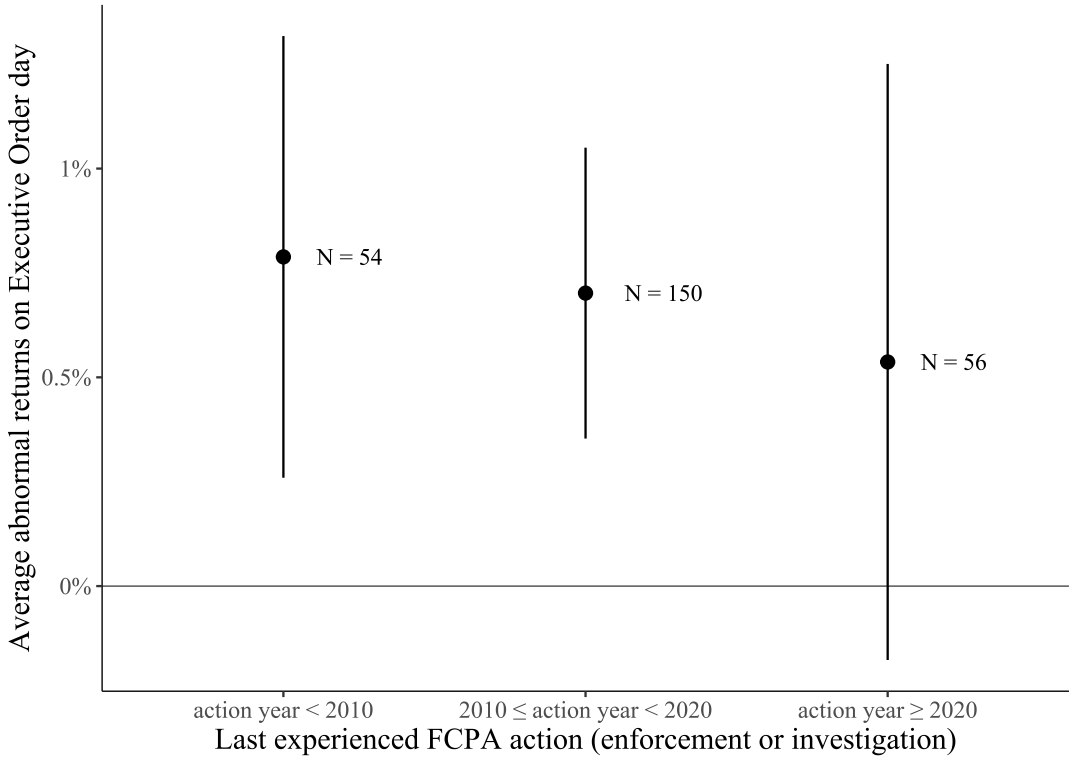

FIGURE L.1: Average AR of past FCPA target firms by the decade in which the firm experienced its last FCPA action (investigation or enforcement). 95% CIs displayed as vertical bars

M 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.