Reputational Costs of Revealing Political Connections: Evidence from Electoral Bonds in India

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Why do companies choose to hide potentially beneficial political connections? I show that revealing political activities can hurt a company's valuation in the short run by tainting its reputation. I take advantage of a unique setting where the Election Commission of India suddenly revealed a list of previously-anonymous corporate donors to political parties. Publicly listed companies revealed to be political donors experienced a 1.5% cumulative abnormal underperformance over the next 7 days as a result of this information shock, amounting to an erosion of approximately $9 billion in market capitalization—over 5 times the amount raised by political parties in the first place. The effect is short-lived and is largely driven by companies facing negative press immediately following the reveal.

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1. Introduction

A vast literature finds firms benefit from political connections (Faccio et al. 2006; Fisman 2001; Claessens et al. 2008). And yet firms often try to conceal their political linkages (Jia et al. 2023), masking their identities by funneling financial support through harder-to-trace channels like philanthropies (Bertrand et al. 2020), think tanks (Hertel-Fernandez 2019), or shell entities (Durkee 2017). Why do companies hide their political activities?

A reason may be that political connections can be costly to firm operations. Politically connected firms are vulnerable to external shocks that threaten to rupture fundamental business models that depend on existing political arrangements. These shocks can include the death of a patron (Fisman 2001), electoral turnover (Betz and Pond 2023; Wagner et al. 2018), regime changes (Siegel 2007) etc. Politically connected firms also risk being pressured by politicians to alter business strategy as campaign favors (Bertrand et al. 2018). In experimental settings, researchers find evidence that customers and investors may avoid transacting with and investing in politically connected companies, unsure of their trustworthiness as counterparties (Bhandari 2022; Kubinec et al. 2021).

I argue firms also face a reputational cost that comes from being seen as participating in political corruption. A common view is to equate corporate money in politics with corruption (Nyberg 2021). Public opinion polling routinely finds the vast majority of respondents to be critical of large campaign contributions, and suspicious of businesses' undue influence over the politicians through campaign finance (Cerda and Daniller 2023; Persily and Lammie 2004; Zingales 2017; Haenschen et al. 2024). And so even though most countries do not have bans on corporate donations (Canen and Wantchekon 2022), firms may continue to avoid disclosing campaign contributions fearing reputational taint and would prefer to hide their political activities.

How costly is it for companies to have their political connections revealed? While researchers have found ways to value political *connections*, these studies typically start by observing political connections and then use an external shock (e.g. sudden death, appointment) to value them (Fisman 2001; Acemoglu et al. 2016). I, instead, focus on the process of observing political connections in the first place. I take advantage of a unique setting where companies' previously hidden political activities were suddenly made public, which I use to measure the cost of revealing firms' political connections.

Since 2018, companies in India have been able to make unlimited and anonymous donations to political parties by purchasing "electoral bonds" from the State Bank of India.[1] On March 14, 2024, after a Supreme Court ruling on the illegality of the electoral bond scheme, the Election Commission of India released a previously undisclosed list of all companies that made donations to political parties over the past six years. Out of the

[1] Despite the name, these bonds bear little resemblance to fixed income instruments. They are simply fixed-denomination coupons that are first bought, and then deposited into party coffers at the SBI.

800 or so companies on the list, 88 of those were publicly listed companies with shares actively traded on the National Stock Exchange. In this paper, I track the price action of companies revealed to be on the list of donors versus companies not on the list, and measure the change in value that can be causally attributed to this sharp information shock.

Specifically, I calculate abnormal returns for each stock—based on its historical relationship to market returns—as well as excess returns—measured as deviations in daily returns from the market return. Using an difference-in-difference event study setting, I find that companies revealed as donors experienced a sharp drop in abnormal returns as well as excess returns on the day and immediately following the surprise announcement. I complement this analysis using a synthetic control method which finds similar results in direction, magnitude, and significance. Over seven trading days after the announcement, donor stocks experienced a cumulative abnormal decline of, on average, 1.5% relative to a control group of non-donor stocks. Multiplying this quantity by the market capitalization of donor companies just before the event date, I estimate the value destruction caused by revealing political connections to be in the order of $9 billion.

For context, political parties raised less than $2 billion over 6 years via the electoral bond scheme, meaning this information shock wiped out almost 5 times the total amount raised. For the 88 publicly listed donors themselves, the $9 billion erosion in market capitalization represents over 400 times the total amount contributed via electoral bonds, which summed to 19 billion INR, or around $23 million (as of 2024).

Two mechanisms may be driving this outcome. First, information about companies' political activities may prompt investors to update their beliefs about the companies' fundamental value moving forward. This mechanism would predict heterogeneity in treatment effect by the novelty of information revealed—for instance, if a company was also making disclosed donations to political parties, investors would already be aware of its political activity, and therefore being revealed as a buyer of electoral bonds would not shed new light on the company as politically involved.

Second, given the high publicity and salience of this information release, firms revealed to be on the list were engulfed in a media storm that likely affected public perception. In this case, an immediate drop in value has less to do with the specific information being revealed, and instead reflects media spotlight and reputational hits from being involved in public scandals. This mechanism, therefore, would predict heterogeneity in treatment effect by the level of media attention on donor companies.

To formally test these alternate mechanisms, I split the set of treated firms into various treatment arms. I find little evidence of investors updating their fundamental beliefs about companies' profitability based on new information. On the other hand, I find, first, news coverage about electoral bonds rose sharply after this information was released,

second, this coverage was largely critical of the electoral bond scheme—with high co-occurrence of terms like "corruption", "scam", "extortion", and "bribe"—and finally, that companies that received greater coverage of their electoral bond purchases saw deeper underperformance over the next 7 trading days. In 10 days however, this negative effect was largely wiped out. Together, these results provide strong support for a media scandal theory, rather than investors re-rating companies' fundamentals.

Contributions. The main contribution of this work is to shine empirical light on the theoretical question of why firms conceal their political activity (Jia et al. 2023). The authors theorize that a firm takes into consideration a risk of backlash that might occur if its concealed activities are revealed while deciding its political strategy. While the authors cite studies describing public backlash to corporate politics (King and Soule 2007; King and McDonnell 2012), it does not quite prove the mechanism of reputational cost that firms put on the line when choosing to adopt a more covert political strategy. The sudden and retroactive revoking of an anonymous channel for corporate political activity that I exploit serves as a unique opportunity to show this precise mechanism.

In finding a sharp negative market response to learning about a company's political activity, this work contributes to a small but burgeoning literature that identifies potential *costs* of political connections to firms. Bertrand et al. (2018) and Akcigit et al. (2023) describe how political connections may distort firms operations either through political pressure around election time, or by creating conditions that promote under-investment in innovation. Studies increasingly show how negative attitudes and perceptions around revealed corporate political activity takes a toll on firm performance Katic and Hillman (2023). For instance, one line of studies show how a company's sales may suffer once consumers learn about and disagree with its political leanings (Panagopoulos et al. 2020; Bhandari 2022; Conway and Boxell 2024) In a conjoint experiment setting, Kubinec et al. (2021) finds companies' potential political connections to dissuade hypothetical investors. This paper studies this mechanism outside an experimental setting, with real market reactions rather than reported preferences, and with an ability to measure how long these effects lasted.

Finally, this paper builds on a rich tradition of studies that examine the relationship between political shocks and the stock price of exposed companies. These political shocks include the death of a patron (Fisman 2001; Faccio and Parsley 2009), political appointments (Acemoglu et al. 2016), election turnover (Wagner et al. 2018), and even coups (Dube et al. 2011). But in all these cases, the political connections of firms are known first, and the political shocks changes these firms' proximity to power. In my setting, while the firms are in fact politically connected, the shock is a sudden and highly publicized revelation of these firms' political connections.

This setting is most similar, therefore, to Werner (2017) and Minefee et al. (2021) who exploit a pair of accidental disclosures of corporate donors to Republican party associated nonprofit organizations—one in 2011 and one in 2014, but find different responses. Werner (2017) finds companies *gained* from this information leak, which raises the puzzle of why those companies then chose to hide their activities. On the other hand Minefee et al. (2021) find exposing the political activities cost firms caught in the 2011 leak, which is consistent with my evidence that sudden revelation of a company's political activities hurts company valuations, and explains their choice of seeking anonymity in the first place.

2. Covert Politics as a Reputation Management Strategy

Most studies on corporate political activity use corporate contributions and lobbying reports to examine corporate attempts at influencing the lawmaking process (Lux et al. 2011; Katic and Hillman 2023). But despite the predominance of this approach, these studies face the fundamental challenge that firms will often forge under-the-table channels to actively circumvent public disclosure of political activity (Jia 2018). Recognizing this problem of censored data, scholars find creative and forensic workarounds, like investigating donations to charitable foundations (Bertrand et al. 2020), tracking commodity price movements to infer business political activity (Kapur and Vaishnav 2013; Sukhtankar 2012), or exploiting sudden deaths of top donors to estimate counterfactual political activity (Battaglini et al. 2024). But what explains why firms choose to conceal their political activities at all?

Scholarship across political science, finance, and management offer different reasons for why a firm might want to conceal its political movements, based on *who* the firm is hiding *from*. Political scientists highlight how firms may forum shop for the best venue to extract favorable outcomes (Hacker et al. 2021). In this way, Culpepper (2010) suggests firms may choose a *quiet* venue, hidden away from public scrutiny, to effectively win regulatory concessions. The signal-jamming literature in economics and finance suggest managers may want to selectively block information flow of corporate practices to investors who may punish poor performance or evidence of malfeasance (Holmström 1999; Niessner 2015). Another reason may be that firms want to hide their political activities from other stakeholders for private gain (Jia 2018)—say in a collective action setting, some firms may lobby secretly for private benefit (Posner 1996), or simply to avoid triggering countercontestation by competitors (Dorobantu et al. 2017).

Jia et al. (2023) provide the most thorough conceptual treatment of why a firm may want to conceal its political activities. In their model, at the micro-level, a firm's concealment decision is driven simply by an accounting of the potential benefits from concealment against the cost of concealment. They stress the costs are more than just the administrative costs of making a political contribution under the table, but also the costs of backlash that

might occur if the firm's concealed activities were to be revealed. In this way, the authors highlight the *reputational* damage that would occur if previously concealed activities were to be revealed.

Extensive work has shown how organizations treat reputation as an intangible asset that is crucial for driving shareholder value.[2] External agents often use firm reputation as a signal in noisy informational environments. For example, lawmakers may give preferential treatment and access to companies with better social reputation (Werner 2015), and investors may reward companies that cultivate better public reputation (Raithel and Schwaiger 2015). And so firms will choose to actively manage and protect their reputation as an asset (McDonnell and King 2013).

In the case of non-market strategies, there is likely a relationship between reputation management and political activities. Existing work relating political activities and reputation tends to focus on politically and socially driven public backlash against corporate targets, which then has material consequences on firm performance (Vasi and King 2012; McDonnell and King 2013; McDonnell and Werner 2016), but Shanor et al. (2021) make the connection most explicitly—finding that firms will choose to conceal more of their political activities when they have worse public reputations.

In the case of campaign finance, even if corporate political funding is legal, if the reputational risks of being discovered are too high, companies will choose to take a more concealed path. Specifically with electoral bonds in India, we know firms readily took up this covert channel, and it would appear there are general reputational risks of being involved in the business-politics nexus, with public opinion generally skewed against corporate contributions, lobbying, and the political activities of special interest groups (Zingales 2017; Nyberg 2021).[3] And so, as a theoretical prediction under Jia et al. (2023)'s model, one would expect a sudden revelation of previously-concealed political activities to have a negative effects on the firms revealed as politically active. Given the role of the media as a crucial intermediate channeling information between external stakeholders (the public) and shareholders (King 2008), I expect these effects to be more significantly felt by firms facing greater media scrutiny.

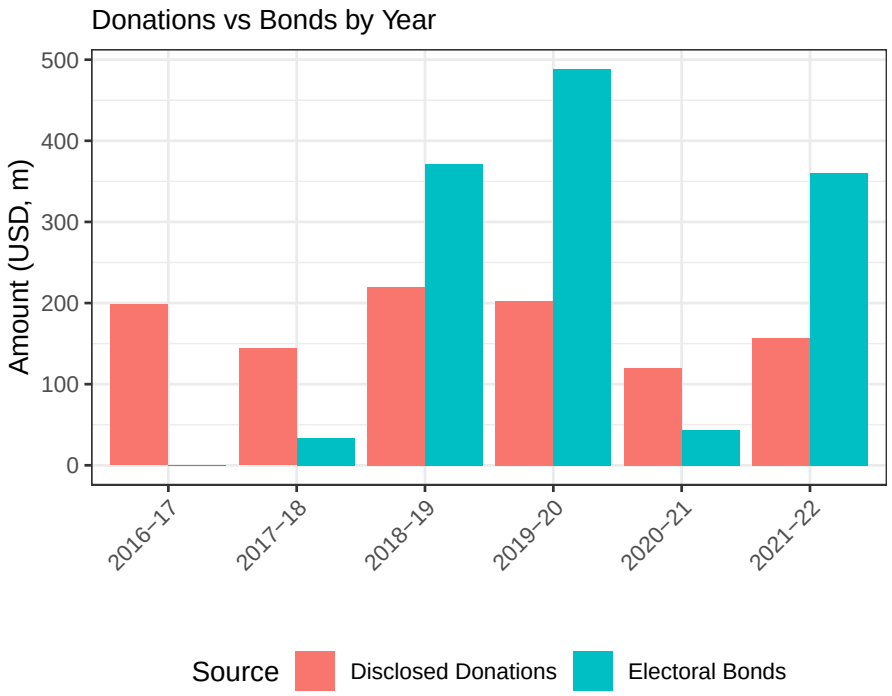
3. Setting

With soaring costs of elections in India, political parties have been turning to companies as an important source of financing. Prior to 2018, companies could legally donate to political parties either directly or via electoral trusts, and contributions above INR 20,000 (approx. 250 USD in 2024) were required to be disclosed by political parties in annual

[2] See, amongst others, Fombrun and Shanley (1990); Deephouse (2000); Mahon (2002); Rindova et al. (2005); Rhee and Valdez (2009); Werner (2012)

[3] For good measure too, as Dixit (1998) describes, corruption and lobbying often go together.

FIGURE 1. Party Finance By Source



Source: ADR, Author's calculations

reports published by the Election Commission (ECI). These donations were tax exempt to encourage disclosed giving, but capped at 7.5% of a company's average net profit over a trailing three-year period.[4]

In the 2017 Finance Bill, the government introduced the Electoral Bond scheme as an alternate mechanism for party financing. In this scheme, individuals or companies could purchase bearer bonds at different denominations, and deposit them into the accounts of political parties at the State Bank of India (SBI). The innovation was twofold: first, the law removed the cap on donations, allowing for a flood of private capital to enter politics; and second, the law allowed for complete anonymity without requiring any disclosures.

Starting January 2018, companies could choose whether to make capped and disclosed donations to parties, or whether to anonymously deposit an unlimited number of electoral bonds in party coffers. Unsurprisingly, parties raised almost twice as much via electoral bonds as from disclosed donations (see figure 1).

Immediately after the electoral bond scheme was announced, civil society organization Association for Democratic Reform challenged the legality of the scheme. In October 2023, the Supreme Court heard oral arguments, and on February 15, 2024—against popular expectations—the Chief Justice of India struck down the scheme as unconstitutional and

[4]See Kapur and Vaishnav (2018) for a thorough treatment of political finance in India.

required the names of all donors to be published on the ECI's website by March 6. After a small delay,[5] at 4pm on March 12, SBI submitted two lists to the ECI: the list of bond purchases with details of the bond purchaser, and the list of deposits into party accounts. On March 14 at 8pm, the ECI published these two lists on its website.

Over the next few days, news outlets and netizens started identifying the largest donors, and find evidence of corrupt linkages. Analysts noticed how companies' donation timing often matched either raids by tax officials or large government contract announcements, suggesting quid pro quo politics at play.[6] The most glaring omission at this point was the two lists could not be combined: it was impossible to identify *which party* each donor was donating to. Upon pressure from the Supreme Court, the ECI released fresh data on March 21 with identifying information across the two lists allowing for analysts to map donations to specific political parties.

4. Data

The main data source for this paper is the list of electoral bond purchases published on the ECI website on March 14, 2024. This list contains information about the date of purchase, name of purchaser, and denomination of bond bought, for almost 19,000 purchases, amounting to over 120 billion INR (approx. 1.7 billion USD) (Traveli and Kumar 2024). I develop and use a web scraping algorithm to link each corporate donor to its unique 21-digit corporate identification number (CIN), which helps resolve cases of ambiguous or duplicated entities. [7] I identify 799 unique corporate entities responsible for over 97% of electoral bond sums raised. 106 (13%) of these companies are publicly listed entities, which are responsible for 20% of the total value raised through the sale of electoral bonds. 88 of these companies are actively traded on the National Stock Exchange (NSE),[8] and serve as the set of companies treated with an information shock on the March 14. Donors make up approximately 5% of the companies listed on the NSE,[9] which is approximately the same share of publicly listed companies that make campaign donations in the US (Fouirnaies and Hall 2018).

I merge this electoral bond data with corporate financial information from CMIE's

[5] On March 4, SBI requested an extension until June 30. But the SC rejected this request and gave a new deadline of March 15.

[6] See, for instance MN et al. (2024)

[7] For instance, the original list reports donations from `Achintya Solar Power Private Limited` and `Achintya Solar Power Pvt Ltd`, which a string matching process would count as distinct entities. My entity resolution algorithm helps recognize these as the same company.

[8] The rest are either do not have actively traded shares anymore, or are listed on the Bombay Stock Exchange. I restrict my analysis to the NSE, which sees up to 14 times higher trading volumes, and is therefore a more ideal venue to observe swift updating on informational shocks.

[9] The NSE reports over 2000 listings, but many of these are not actively traded intraday, and are reserved for either special classes of investors (like foreign institutions), or for special types of investments (like block trades). There are ultimately 1537 stocks available for active trading, and with fully reported data.

ProwessDx and Datastream. I use Datastream to pull daily financial market data—like stock price, volumes traded, and market capitalization—for all individual stocks traded on the NSE and for the Nifty 100, an index of the largest 100 NSE stocks by marketcap, often used as a proxy for the market. Finally, to compare political participation by firms across the electoral bond channel and the disclosed channel, I scrape donation data published on the Association for Democratic Reform's website[10], and raw contribution reports published on the Election Commission of India's website, and use a combination of web scraping and manual matching to link donors to unique corporate identification numbers.[11]

5. Empirical Strategy

Sudden transparency for a select set of publicly listed companies motivates an event study analysis, where the treated firms are those revealed to be donors. For my main empirical approach, I follow principles outlined in MacKinlay (1997), and largely track Acemoglu et al. (2016) who study the effect of Tim Geithner's surprise appointment as Treasury Secretary in 2008 on the stock prices of connected companies.

5.1. Abnormal Returns

My main outcome variable of interest is daily abnormal returns for each firm, which captures deviations from "normal" expected returns. As per MacKinlay (1997):

$$AR_{it} = R_{it} - \mathbb{E}\left[R_{it}|X_t\right] \tag{1}$$

Where AR_{it} is the abnormal returns for firm i on day t, R_{it} is the actual firm returns on day t, and $\mathbb{E}\left[R_{it}|X_t\right]$ is the "normal" expected returns for firm i on day t based on conditioning variables in X_t. "Normal" expected returns could be a number of options, including a simple constant mean, the market return, or an estimate based on a historical relationship between the daily returns of stock of company i and market returns, or key risk factors within the market. Per Acemoglu et al. (2016), for each company i, I estimate a historical linear relationship between a stock's daily returns and the Nifty 100 over a one year period up to 30 days before the event date. I also calculate excess returns, which simply uses market returns as the "normal" expected returns.

Adding up abnormal returns generates a measure of cumulative abnormal returns, which I use to infer the overall effect of sudden transparency over several trading days.

Equation 2 depicts my main empirical specification.

[10] https://myneta.info/party/

[11] See Puri (2024) for more details.

$$y_{it} = \alpha_i + \eta_t + \gamma t X_i + \sum_{s \neq T-1} \beta^s \mathbb{1}[t = s] \times Donor_i + \epsilon_{it} \tag{2}$$

y_{it} is the abnormal daily return, or excess daily return, for firm i on day t. α_i and η_t are company and date fixed effects. $Donor_i$ takes the value 1 if the company is on the list of donors revealed on T = March 14, 2024. tX_i is a vector of firm-specific controls interacted with time dummies, with coefficients γ. $\hat{\beta^s}$ estimates the average effect of being a donor on abnormal daily returns on day s compared to the reference period. I choose to set March 13 as my reference date—one day before the event.

While α_i captures unobserved time-invariant differences in levels of returns across firms, I also include tX_i which is an interaction of firm-level characteristics with time dummies to ensure the estimates do not capture the effect of the shock on other characteristics that may be correlated with the treatment. Following Acemoglu et al. (2016) I include size, leverage, and profitability as firm-specific covariates. I also include indicators for industry groups and a measure of market capitalization a month before the event date. Appendix A shows how the probability of being a donor varies each of these characteristics. Including these interacted fixed effects implies an assumption of parallel trends conditional on these covariates. I attempt to validate this conditional parallel trends assumption visually by including several pre-periods in the event study window, and by running a placebo analysis where I set the reference period to two weeks before the actual event period. I cluster standard errors at the company level.

5.2. Cumulative Abnormal Returns

Market updating based on new information can take a few days, and so in order to estimate an overall effect on treated firms, I calculate the cumulative abnormal returns, which is simply a sum over daily abnormal returns, following MacKinlay (1997)

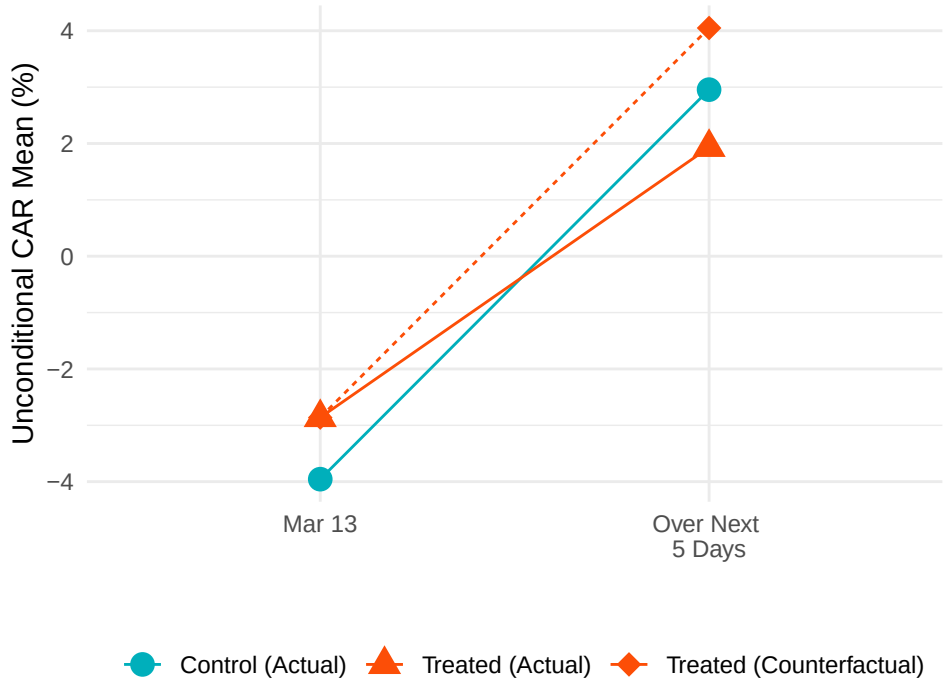
$$CAR_i(\tau_1, \tau_2) = \sum_{\tau=\tau_1}^{\tau_2} AR_{i\tau} \tag{3}$$

CAR is defined for each firm i and over a period of time from τ_1 to τ_2. I study cumulative abnormal returns for firms between 1 and 10 trading days after, and including, the event date of March 14.[12]

I calculate the average treatment effect on the treated (ATT) in a simple two-group, two-period difference-in-difference setting, where the treated group is the set of companies

[12] Note these are trading days, not calendar days. So, for instance, March 16 and 17 are excluded as the NSE is typically closed for trading on weekends.

FIGURE 2. Difference in Difference Visualization



Note: This figure visualizes the two-period two-group difference-in-difference setting I estimate in Equation 4. The pre-period is the cumulative abnormal returns on March 13—one-day before the event date, serving as the reference period. The post-period here is cumulative abnormal returns over five trading days starting March 14. With parallel trend assumptions, the counterfactual unconditional CAR mean for the treated group of companies is traced by the dotted line. The ATT is estimated by the vertical difference between the orange diamond and orange triangle points.

revealed on the list of donors, and the control are all other companies in the NSE. For the pre-period, I use the abnormal returns as of March 13, which is one day before the event date, and serves as the reference period in the event study setting. The post period is between 1 and 10 trading days after March 13. Figure 2 provides visual intuition for this setting.

Formally, I estimate equation 4, where $\hat{\beta}$ estimates the causal effect of this information shock on the cumulative abnormal returns of donor firms, relative to control firms. I include the same set of controls from equation 2: stock (α_i) and period (η_t) fixed effects, along with a vector of company-specific characteristics interacted with the period dummy (tX_i). I cluster standard errors at the stock level.

$$CAR_{it} = \alpha_i + \eta_t + \gamma tX_i + \beta Donor_{it} + \epsilon_{it} \tag{4}$$

The cumulative excess return (CER) measure is defined analogously to CAR, and I follow the same inferential procedure.

5.3. Estimating the Cost of Revealing Political Connections

Multiplying the cumulative abnormal returns with the market capitalization of donor companies immediately prior to the event date estimates the corporate value destroyed by this information release, and therefore quantifies the cost of revealing firms' political connections.

5.4. Synthetic Control Method

As robustness exercise, I implement an alternate procedure, synthetic control method, that compensates for potential violations in parallel trends. A difference-in-difference approach assumes parallel trends between treatment and control groups hold after including covariates and firm-fixed effects to capture unobservable characteristics. The synthetic control method compensates for potential violations of parallel trends by reweighting units to match pre-treatment trends (Arkhangelsky et al. 2021). Developed by Abadie and Gardeazabal (2003); Abadie et al. (2010, 2015), this process involves constructing a synthetic version of each treated unit based on all control unit outcomes, weighted by its fit in the pre-treatment period, and is sometimes used as a robustness test for difference-in-difference procedures.[13]

I follow Acemoglu et al. (2016), given its similar setting of multiple treated units, and aggregate treated firms' actual returns deviations from their synthetic counterfactuals. I estimate confidence intervals using permutation tests as recommended by Abadie et al. (2010); Abadie (2021).

The principle behind this approach is to construct a synthetic match for each treated unit by weighting "donor" (i.e. control) units such that pre-event trends are similar. After constructing such a match, the effect of the event is simply the deviation in behavior between the actual treated unit and its synthetic pair. This calculates a treatment effect for each treated unit individually. I calculate control unit weights over an estimation period of one year, ending one month before the event date.

Formally, I construct a synthetic match for each treated firm i given each control firm j by solving the following optimization problem:

[13] See for instance Acemoglu et al. (2016); Mirenda et al. (2022)

$$\begin{aligned} \arg\min_{w_j^i} \quad & \sum_{t \in \text{Estimation window}} \left[R_{it} - \sum_j w_j^i R_{jt} \right]^2 \\ \text{s.t.} \quad & \sum_j w_j^i = 1 \quad \text{and} \quad w_j^i \geq 0 \end{aligned} \tag{5}$$

where R_{it} is the daily return for treated company i on date t during the estimation window. w_j^i is the weight of control unit j that is calculated in this optimization problem. $\sum_j w_j^i = 1$ and $w_j^i \geq 0$ ensure non-negative weights that sum to 1.

After optimal weights are calculated, I calculate synthetic returns for each treated unit i with the following equation:

$$\hat{R_{it}} = \sum_j w_j^i R_{jt}$$

And Abnormal Returns is simply the difference between actual returns and synthetic returns:

$$AR_{it} = R_{it} - \hat{R_{it}}$$

Estimating the effect of the event requires defining the event window: τ_1, τ_2. Note, aggregating across multiple treated units to calculate an average effect does not change the individual estimation procedures (Abadie 2021). The effect of revealing treated companies names is estimated as:

$$\hat{\Phi}(\tau_1, \tau_2) = \frac{\sum_{i \in \text{Treatment group}} \frac{\sum_{\tau_1}^{\tau_2} AR_{it}}{\sigma_i}}{\sum_{i \in \text{Treatment group}} \frac{1}{\sigma_i}}, \tag{6}$$

where $\frac{1}{\sigma_i}$ is a goodness of fit that overweights better matches (similar in spirit to corrections recommended by Ben-Michael et al. (2021)). $\hat{\sigma}_i$ is defined as:

$$\hat{\sigma}_i = \sqrt{\frac{\sum_{t \in \text{Estimation window}} \left(R_{it} - \hat{R}_{it} \right)^2}{T}} \tag{7}$$

where T is the length of the estimation window (for me, 250 days).

Finally, I take a permutation approach to calculating confidence intervals as recommended by Abadie et al. (2010) which involves calculating a test statistic and comparing how extreme this test statistic is compared to a distribution, and is followed by Acemoglu

et al. (2016). I randomly draw 5000 placebo treatment groups from the set of control units. Each group is the same size as the actual treatment group (88 units). For each placebo group, I calculate a treatment effect over several days starting from the event date of March 14, 2024, $\hat{\phi}(\tau_1, \tau_2)$, and construct a distribution of placebo group effects. I compare the real treatment effect to this distribution. Since the hypothesized effect is negative, I conduct a one-sided hypothesis test where I evaluate if the real treatment effect is significant at 5% if it is below the 5th percentile of the placebo group distribution.

6. Results

6.1. Abnormal Returns

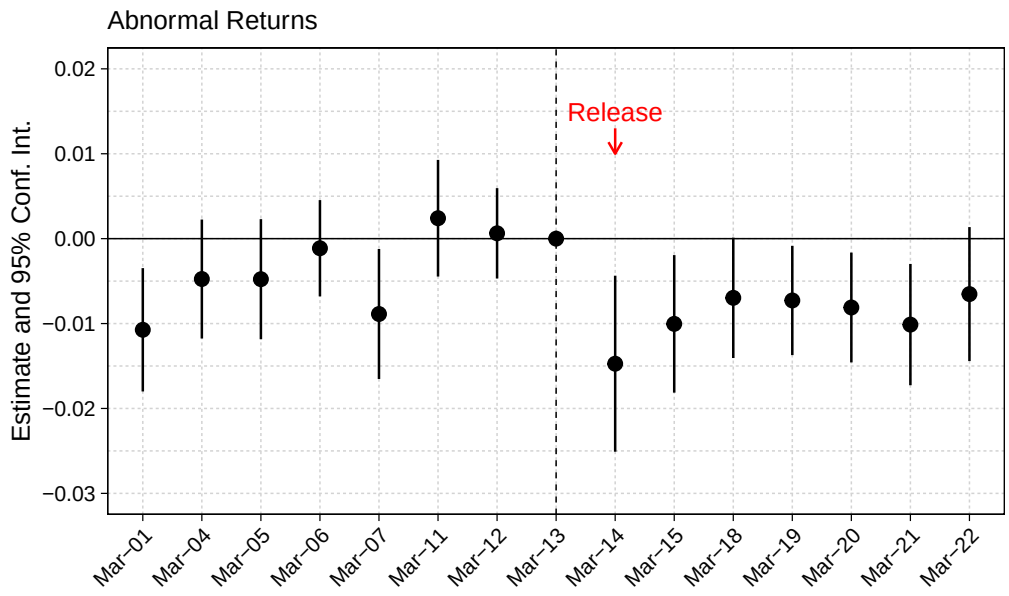
Figure 3 shows the main result of estimating equation 2 on an outcome variable of daily abnormal returns. Companies revealed to be politically active by buying electoral bonds saw a sharp abnormal decline on the day of the release (on average, a 1.5% abnormal decline), persisting for several trading days after. Appendix B.1 reports the results across a range of different empirical specifications, including winsorizing or trimming daily returns to ensure the results are not driven by outliers. The negative effect is consistent in all specifications, with qualitatively similar magnitudes and significance.

The pre-period shows no visually apparent trend, with most estimates in the pre-period generally not significantly separable from zero. Additionally, I run a placebo test where I replace the reference period (March 13, 2024) with one from two-weeks before the event date (February 26, 2024). As shown in appendix B.3, neither the pre-period nor the post-period show a significant and systematic difference between the two groups. These diagnostic tests suggest a strong treatment effect of the information shock, revealed on the event date.

An important detail is that the list of donors was revealed *after* market-hours on the 14th, even though abnormal daily price action is evident on the day itself. The pre-announcement decline reveals the negative effect was driven by not just the release of information, but also the *anticipation* of its release.

Even though the Supreme Court had required the list be revealed by March 6 in its original February 15 ruling, commentators remained skeptical that the State Bank of India would actually adhere to this timeline (Vaishnav 2024). This skepticism was partially vindicated when the SBI requested an extension until June 30. But the Court rejected their request, but revised the release date to be March 15. The threat of information became credible on March 12 at 4:15 pm (after market-hours), when the SBI submitted a password-protected USB drive containing the list of donors to the Election Commission. At this point, it was clear that the ECI was going to reveal the information over the next few days, and some stocks started pricing in what was expected to be in the information release.

FIGURE 3. Event Study: Daily Abnormal Returns



Note: OLS estimation of equation 2. Outcome variable: abnormal returns, defined as the difference between a stock's actual daily returns and predicted returns based on the market's daily return and one year's relationship between the stock and market returns. Error bars represent 95% confidence interval. Size, leverage, return on equity, market capitalization (as of Feb 15, 2024) and industry sections all interacted with time dummies. Names of donors released on March 14.

This market pattern is consistent with empirical studies of *pre*-announcement and *post*-announcement drift around salient market or corporate news events, like FOMC meetings (Lucca and Moench 2015), company earnings, or index inclusions or exclusions (Fernandes and Mergulhão 2016). These studies find equity markets to move in advance of information releases, despite low trading volumes, followed by a bump in trading volumes *after* the information is actually revealed (see appendix B.4)—indicating more of a retail reaction to news (Frazzini and Lamont 2007)—as well as a post-announcement drift, where prices do not correct immediately in one go, but take several days to percolate through.

Some research has tied this pre-announcement drift to private information leaks, for instance between the Fed and market participants (Cieslak et al. 2019). But pre-announcement price action does not necessarily require leaked information and could emerge from the rational behavior from market participants facing uncertainty (Cocoma 2018). More concretely in the case of electoral bonds in India, it was perhaps not a big surprise which companies were likely to be on the list of donors in the first place, with about half of the companies eventually revealed as donors previously giving through

TABLE 1. Conditional ATT on Cumulative Returns

	Cumulative Abnormal Return			Cumulative Excess Returns		
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Donor	−0.015	−0.015	−0.004	−0.014	−0.014	−0.003
	(0.005)	(0.008)	(0.008)	(0.006)	(0.008)	(0.008)
Trading days	1	7	10	1	7	10
Num Stocks	1537	1537	1537	1537	1537	1537
FE: ISIN	✓	✓	✓	✓	✓	✓
FE: Trading Day	✓	✓	✓	✓	✓	✓
R2	0.652	0.649	0.565	0.610	0.642	0.563

Note: OLS estimation based on equation 4, with period dummies interacted with total assets, leverage, return on equity, market capitalization as of Feb 15, and NIC product section for each firm. Firm financial data as of September 30, 2023. Firm-clustered standard errors reported in parentheses.

disclosed channels in any case.[14]

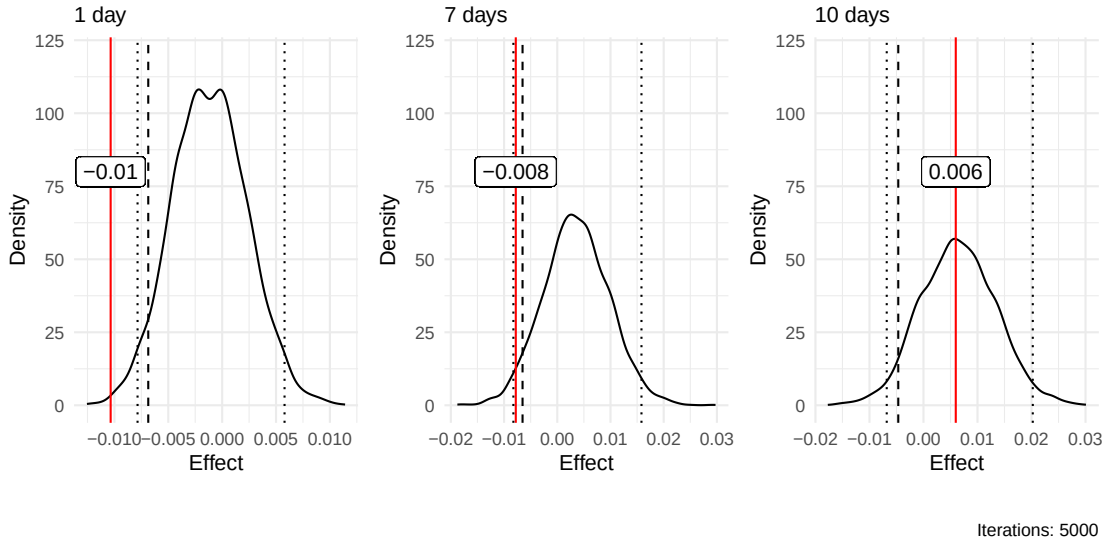
6.2. Cumulative Abnormal Returns

I formally test the effect of this information release on the cumulative average returns of companies revealed to be on the list versus those companies not on the list of political donors by estimating equation 4. Table 1 reports the conditional average treatment effect on the treated (CATT) on cumulative abnormal or excess returns across various windows of trading days after the event date. I find treated firms experience a sharp and significant underperformance on the event day (March 14) and over the following week relative to normal expectations and the overall market. More precisely, over seven trading days after the information release, treated firms experience, on average, a 1.5% cumulative abnormal underperformance versus non-treated firms. This significant negative effect only lasts up to 7 trading days, after which the difference between treated and non-treated firms starts to reduce.

Estimating the cost of revealing political connections. As of March 13, 2024, donors constituted approximately 5% of all publicly listed and actively traded companies on the NSE, but accounted for close to 14% of the overall market capitalization of over $4 trillion, i.e. $609 billion.

[14]For instance, the financial presses reported Adani enterprise stocks experiencing a sharp selloff in anticipation of the release of the electoral bond buyers, with the conglomerate reported to have close ties to the government (Iyengar 2024).

FIGURE 4. Cumulative Abnormal Returns: ATT against placebo



Note: This presents the results of a hypothesis test comparing the real treatment effect against a distribution of 5000 placebo treatment effects across 1, 7, and 10 days. The red line denotes the real treatment effect. The dotted lines demarcates the 2.5th and 97.5th percentile levels for two-sided tests. The dashed line demarcates the 5th percentile for a one-sided test. The results show how the real treatment effect is significant on the event date, as well as upto 7 days after the event, but is not significant after 10 days.

A 1.5% abnormal decline over 7 days on a market capitalization of $609 billion implies a value destruction in the order of $9 billion that can be traced to the sudden revelation of companies' political participation. For context, the entire electoral bonds scheme raised a total of $1.7 billion (Traveli and Kumar 2024), making the short-term cost of revealing political connections more than 5 times the actual benefits transferred to the political parties.

Over the long-run however, the effect is more muted, with cumulative abnormal or excess returns of treated and non-treated firms indistinguishable after 10 days. I explore potential mechanisms that explain these results in the next section.

6.3. Synthetic Control Method

Results from this synthetic control method are similar in direction and significance, but less in magnitude, to the difference-in-difference setting. This method estimates a 0.8% cumulative abnormal underpeformance over 7 days for donor firms compared to non-donor firms, which amounts to an erosion of $4.8 billion in market capitalization, i.e. almost 3 times the amount raised by the electoral bonds channel. Figure 4 presents these findings, and shows how this negative effect persists until 7 days after the event, but dissipates by 10 days after the event, pointing to a short-term effect.

7. Mechanisms

What drives the value destruction that comes from revealing a firm's political connections? I consider two mechanisms: investors re-rating a company's fundamentals in light of new information; or a more reputational story of investors punishing firms for being embroiled in political scandal.

Fundamental Updating. If information about political activities is salient for investors' assessment of the fundamental value of a company, then the effect of the release should depend on how much the information shifts prior beliefs about the company's political activities. Managers may have chosen to suppress this information from investors,[15] and given the scheme required no disclosure either publicly or in annual filings, revealing the list of donors may have had potentially high informational content.

The March 14 release delivered two sets of information: the *fact* that a company made political donations, and the *extent* of its political donations, measured by the total amount donated. Either of these pieces of information could be *surprising*, and prompt an equity analyst to update her beliefs about the health of the company, and its prospects for future cash flow. For instance, learning about a company's political activities might indicate the company's reliance on these electoral bonds to win contracts, which is no longer available. I label this the *fundamental updating* channel.

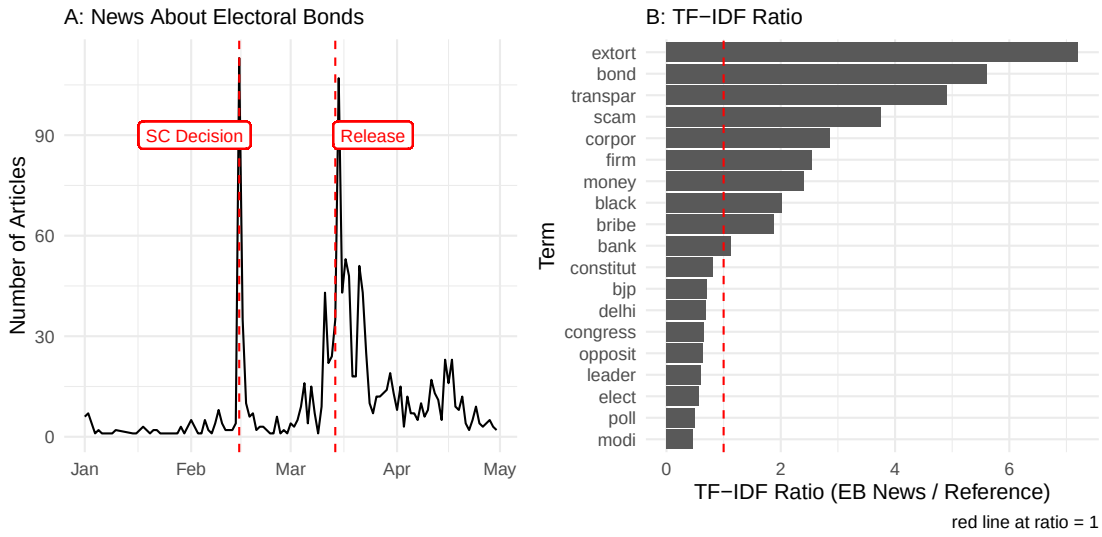
Reputational Cost. Reputation plays an important role in shaping a company's valuation and performance (Tadelis 1999; Lange et al. 2011). Closely related is the role of the media in facilitating an information flow between the general public, investors, and companies (Graf-Vlachy et al. 2020). Financial newsmedia act as monitors of corporate governance, evaluating whether companies run afoul of broad social values (Bednar 2012; Dyck and Zingales 2002), and shapes how the public perceives companies. In turn, a company sees its reputation in the media as a "strategic resource" to manage (Deephouse 2000).

Given a general perception of corporate political activity being synonymous with corruption and special interest (Nyberg 2021), a highly publicized release of political donors would threaten negative news coverage and engulf these companies with air of scandal. Indeed, the days after the event, Indian media outlets were abuzz with coverage of the top donors, and muckraking potential *quid pro quo* arrangements between the state and these donors.

A quantitative examination of the news coverage confirms that the media regarded electoral bonds as a matter of corruption. I use `newsapi.org`, a third party data provider that aggregates news articles from over 150,000 online sources around the world, to gather

[15] "Signal-jamming" in managerial economics describes how managers may manipulate information that investors rely on to price the company (Stein 1989; Holmström 1999; Niessner 2015)

FIGURE 5. News Coverage About Electoral Bonds



Note: A: Count of news articles mentioning electoral bonds, from `newsapi.org`. B: Reports for specific terms the ratio of TF-IDF between the corpus of news mentioning electoral bonds, and a reference corpus of news about elections and India. A ratio of greater than 1 indicates the term being more prominently mentioned in electoral bonds coverage than in general elections-related news in India.

all English-language news reports from India from Jan 1, 2024 to April 30, 2024 about electoral bonds, and a baseline reference of India political news during the same period. I first show in panel A of figure 5 that coverage of electoral bonds spikes around key events: February 15 Supreme Court judgement and March 14 data release. I then examine how the electoral bonds story was covered by comparing how frequently specific terms were used versus the reference period—standard practice for researchers looking to compare two corpora of text.[16] I use term frequency inverse document frequency (TF-IDF) that helps filter out both extremely common and extremely rare words (Gentzkow et al. 2019), and construct a ratio of TF-IDF for each term in electoral bond news to the same in the reference news. Panel B in figure 5 shows how terms like `extort`, `corrupt`, `scam`, `bribe`, and `black` [money] are much more likely to be used in electoral bond coverage. I provide further details and results in online appendix D.1.

7.1. Testing the Mechanisms

In order to directly test each of these alternate mechanisms—fundamental updating and reputational cost—I split the 88 donor firms into treatment arms, leaving the full set of non-donors as controls.

[16] See for example Rayson and Garside (2000); Groseclose and Milyo (2005); Caldara and Iacoviello (2022)

Fundamental Updating. The mechanism of investors updating their beliefs about a company's corporate fundamentals requires separating companies by the *kind* of information that was revealed on March 14. I consider both the extensive margin of political participation (i.e. learning *that* a company is politically active), and the intensive margin of political participation (i.e. learning about the *scale* of its political activities).

Out of the 88 publicly listed companies revealed to be buyers of electoral bonds, I find 48 had also made donations at some point through disclosed channels, while 40 did not.[17] This means for the 48 companies that had at some point made donations through a publicly disclosed channel, learning the fact that they also gave via electoral bonds would not cause investors to update their beliefs about the political participation of these firms. On the other hand, for the 40 companies newly revealed to be politically active, the market may be learning new signals about the company, like the importance of political connections for its business model, or the health of the business that might be driving it to seek out political connections in the first place. I assign the 48 known-donors to the "Known Donors" treatment arm, while the rest to the "New Donors" arm. A mechanism of investors updating beliefs about the fundamental value of the business based on this news would predict a difference in treatment effect across these treatment arms.

The *extent* of political activity may also cause investors to update their fundamental valuation of the company. Being a large contributor might also signal the importance of political connections to a company's operations, and therefore raise concerns about the future viability of the business model after the end of the electoral bonds scheme. I therefore split the treated groups into "High amount" and "Low amount" treatment arms based on the total amount donated compared to the median amount donated by these firms.[18] The expectation here is that firms putting more money into political connections should be more materially affected by the light shone on its past activities and on its business prospects moving forward, predicting a difference in treatment effect across treatment arms.

Reputational Cost. The reputational cost mechanism is less about investors updating their evaluations about a company's business model moving forward, but more a reaction to a high profile political scandal. This may have long-term knock-on effects on the health of the business moving forward, but is more immediately influenced by public attention. I test for this in two ways.

First, for a direct measure of media attention, I count the number of news articles that mentions both donor companies as well as "electoral bonds" in India from March

[17] I search for these companies in the entire period of recorded disclosed donations (2003-2022). Only 30 companies gave through disclosed channels from 2018 onwards, indicating a substitution from the disclosed to covert channels once the electoral bond scheme commenced.

[18] The median amount donated by these 88 firms is 100 million INR ($1.2m at the 2024 exchange rate.)

TABLE 2. Description of Treatment Arms

Mechanism	Treatment Arm	N	Total Donations (INR bn)	Avg Size (INR bn)
Fundamental Updating	New Donor	40	5.0	206
	Known Donor	48	14.6	450
Fundamental Updating	Low Amount	48	2.0	163
	High Amount	40	17.5	550
Reputational Cost	Small Firm	44	5.2	47
	Large Firm	44	14.4	631
Reputational Cost	Low Media Coverage	55	8.2	143
	High Media Coverage	33	11.4	666
	Control	1449	0	317.0

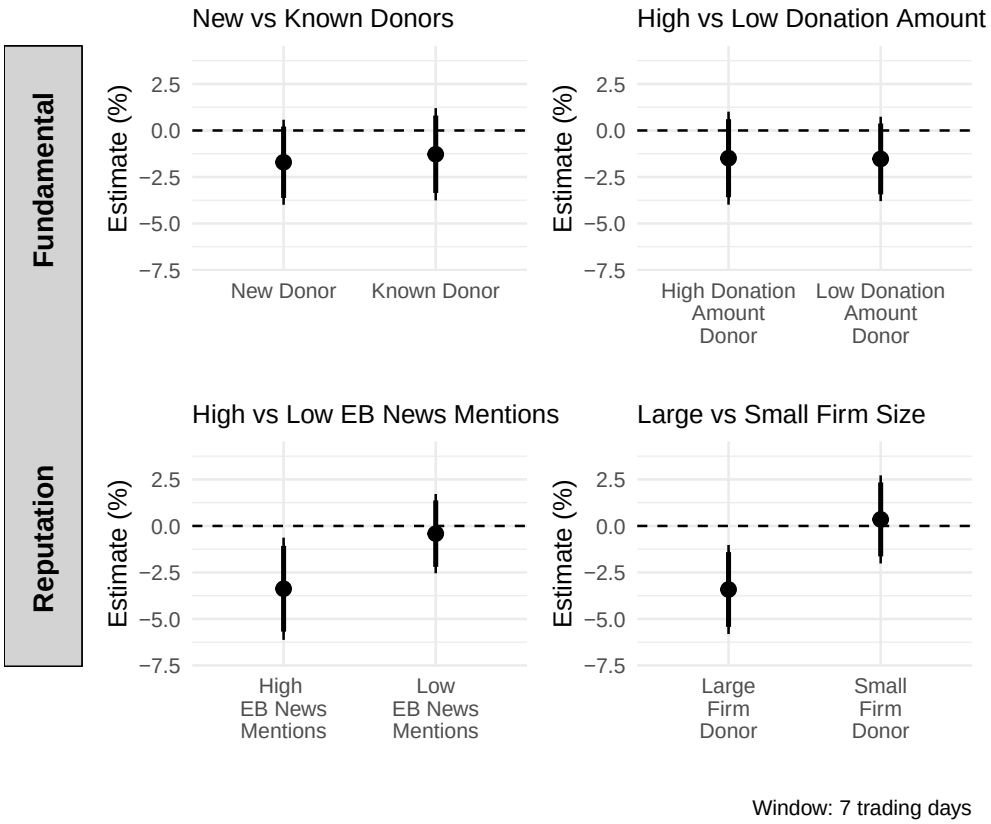
Note: Size: total assets as of September 30, 2023.

14-28, 2024. This data comes from `newsapi.org`. Appendix D.1 presents the distribution of news mentions by donor company. I compare each company's mentions to a median measure, and classify each donor company into a "Low Media Coverage" or "High Media Coverage" treatment arm accordingly.

While this measure of media mentions is precise, it might suffer from false negatives. There may be other sources not covered in `newsapi.org`'s dragnet, especially social media sources, or companies may be mentioned by acronyms and not its official name. Therefore, I consider corporate size as an alternate proxy measure for media attention, and split treated companies by the size of its assets. This channel operates on the assumption that larger firms tend to receive more attention from the media in the first place (Deephouse 2000; Dyck et al. 2008), which makes them more vulnerable to reputational costs that might emerge from a public scrutiny. As a result, larger firms should experience a greater negative effect as a result of a sudden media spotlight on their political activities. I therefore sub-classify treated firms into "Large firm" or "Small firm" treatment arms according to the median firm size by asset value.

Empirical Approach. Appendix table 2 summarizes each of the treatment arms. I then estimate equation 8, which is a straightforward modification of equation 4, for each set of tests, where the outcome variable is cumulative abnormal returns over 7 trading days from the event date. I keep the control group of non-donors constant, and vary the treatment group to be each treatment arm, G, thereby estimating the key coefficient of interest β_G for each treatment arm. So, for instance, in the New vs Known donor treatment condition, I estimate the average treatment effect of being revealed as a donor on newly

FIGURE 6. Conditional ATT: Cumulative Abnormal Returns by Treatment Arm



Note: Outcome variable: 7-day cumulative abnormal returns. Plots present the marginal effect of each treatment arm against a control of non-donors. Thin and thick bars reflect 95% and 90% confidence intervals respectively.

revealed donors ($\hat{\beta}_{G=New}$) and on donors who have been revealed to be donors elsewhere ($\hat{\beta}_{G=Known}$). I cluster standard errors at the firm level.

$$CAR_{it} = \alpha_i + \delta_t + \gamma t X_i + \beta_G Donor_{it}^G + \epsilon_{it} \tag{8}$$

7.2. Results

Figure 6 visualizes the conditional average treatment effect on each treatment arm ($\hat{\beta_G}$). Similar results between New vs Known and Low Amount vs High Amount donors point to limited heterogeneity on these dimensions, and therefore provide limited support to the mechanism of investors learning new information about donors' business models and updating their valuations accordingly.

On the other hand, both the direct measure of media coverage (high media mentions) as well as the proxy measure (large firms) correspond to much stronger effect sizes,

suggesting the overall treatment effect is driven by a firms under greater media scrutiny. In magnitude, companies facing direct media coverage about their electoral bonds saw on average a negative and significant 3.5% abnormal decline over the next 7 trading days, while companies not directly covered by the media experienced approximately null abnormal effects. This result is therefore more consistent with a mechanism of public scandal, where firms suffer short-term reputational damage as a result of being in the public limelight.

Further evidence of this reputational cost mechanism is the fact that the effect is short lived and heterogenous effects largely disappear in 10 days. Ultimately, the actual amounts contributed by these companies is likely too small to be materially consequential for the company's long term operations. For context, the total stock of donations made by the median company in this treatment group of publicly listed donor companies is just over 0.1% of its average *annual* revenue over the period of the electoral bonds scheme (the maximum is donations amounting to 5% of average annual revenue). On the other hand, as panel A in figure 5 shows, revealing the identities of donor companies did capture considerable media attention in the short-run, which would have put these firms' political activities in the spotlight during that period. Taken together, these facts support a more behavioral response where investors react negatively to learning about corporate political activities in the short-run. I replicate the same short-run heterogenous effect dynamics using synthetic control methods as well (see **??**).

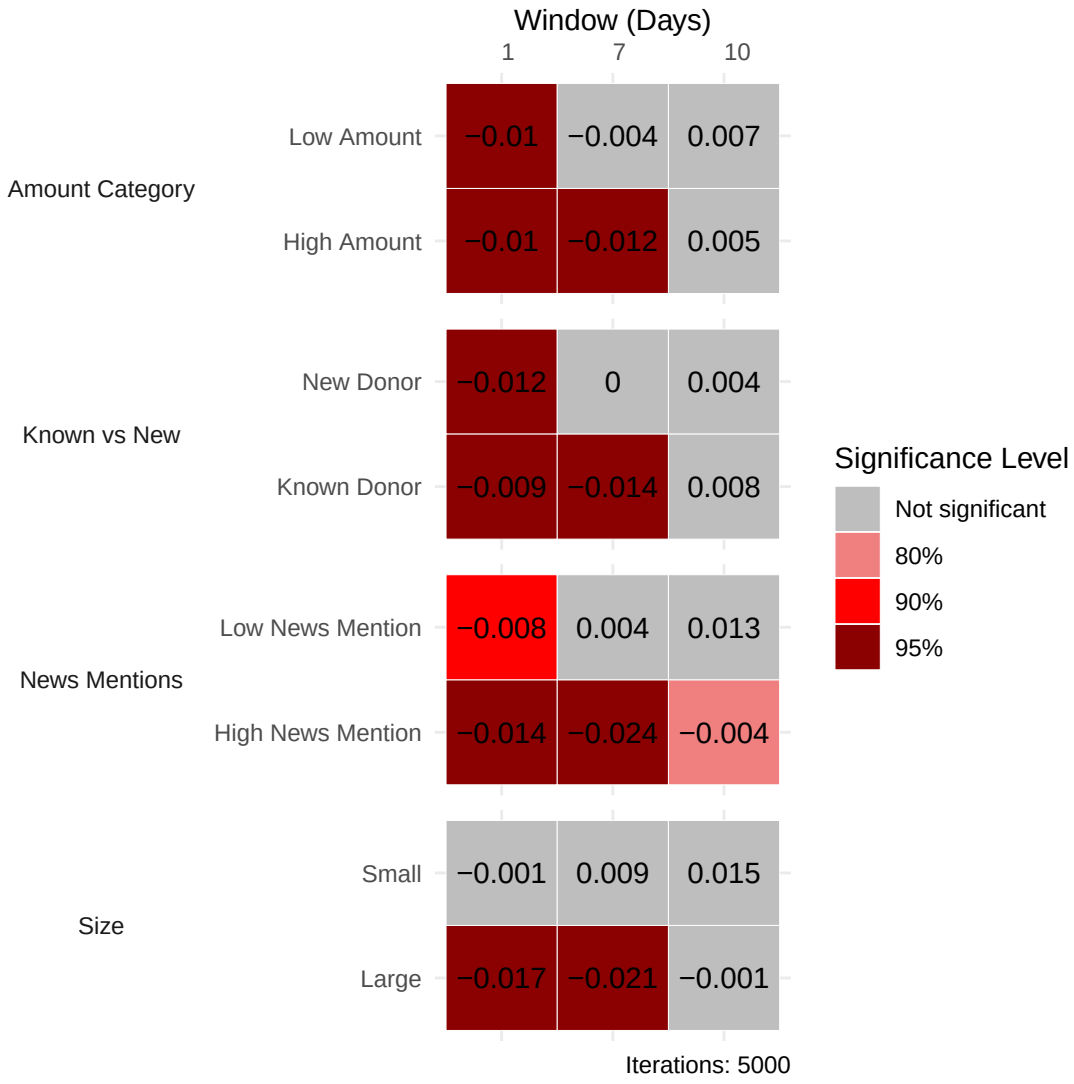
7.3. Synthetic Control Method

I take a randomization inference approach to hypothesis testing heterogenous treatment effects. I split the treatment group into treatment arms, and for each treatment arm, I calculate a test statistic of its real treatment effect, and construct a distribution of treatment effects by drawing 5000 placebo treatment groups from the pool of control units, each of the same size as the treatment group. I carry out a one-sided hypothesis test where the treatment effect is significant at X% if it is below 1 – X% of the distribution. I repeat this procedure for 1 day, 7 days, and 10 days after event onset.

Figure 7 presents the results. Treated units with high news coverage and larger companies tended to see a deeper and more significant abnormal sell-off up to 7 days after the event. Overall effects are largely insignificant 10 days after the event.

The results are broadly similar to the difference-in-difference event study, but an exception is in the "High Amount" treatment arm, which suggests some evidence for a fundamental updating channel. However, ultimately the fact that these heterogenous effects only last a short while suggests a more behavioral reaction than investors re-rating a company's fundamentals.

FIGURE 7. ATT on CAR by Treatment Arms: Synthetic Control Method



Note: Effect size printed inside each grid

8. Conclusion

In this paper, I articulate a puzzle about corporate political activity: if companies benefit from political connections, why do they hide it? While scholars are beginning to shed theoretical light on the question (Jia et al. 2023), empirical studies face the fundamental challenge of trying to observe behavior intended to be concealed.

I take advantage of a unique setting where six years into a government scheme that allowed corporations to make anonymous donations to political parties, the Indian Supreme

Court ruled it unconstitutional, and required all names of donors retroactively be made public.

This highly publicized information shock led to publicly listed donors to underperform their non-donor peers by 1.5% over the next 7 days, implying value destruction of around $9 billion, i.e. 5 times as much as parties raised via the electoral bond scheme in the first place. Several pieces of evidence point to a mechanism of companies being hit with a reputation shock of participating in corrupt political dealings. First, the news coverage was prominent and negative, equating electoral bonds with corruption; second, companies whose electoral bond purchases were caught in the media spotlight did worse; third, even though the causal effect of the news release was large, the actual importance of the electoral bonds channel for these firms was probably low, and so the underperformance was ultimately short lived.

Overall, these findings show how for companies, political connections can be a reputational taint in the short run, which incentivizes companies to seek more concealed channels of political activities.

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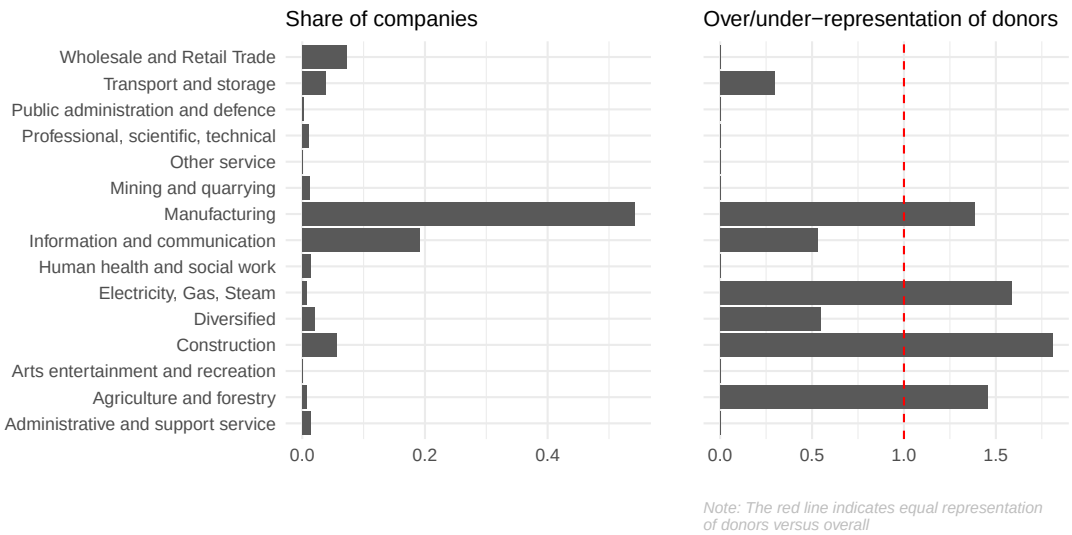
Appendix A. Donor Status Covariates

TABLE A1. Correlations with Donor Status

	(1)	(2)	(3)	(4)	(5)
(Intercept)	−0.195	−0.368	0.052	0.057	−0.331
	(0.031)	(0.049)	(0.009)	(0.006)	(0.054)
log(marketcap)	0.024				0.011
	(0.003)				(0.005)
log(total_assets)		0.025			0.017
		(0.003)			(0.006)
leverage			0.000		0.000
			(0.000)		(0.000)
return_on_equity				0.000	0.000
				(0.000)	(0.000)
Num.Obs.	1537	1537	1537	1537	1537
R2	0.044	0.047	0.000	0.000	0.050

Standard errors in parentheses

FIGURE A1. Sector distribution

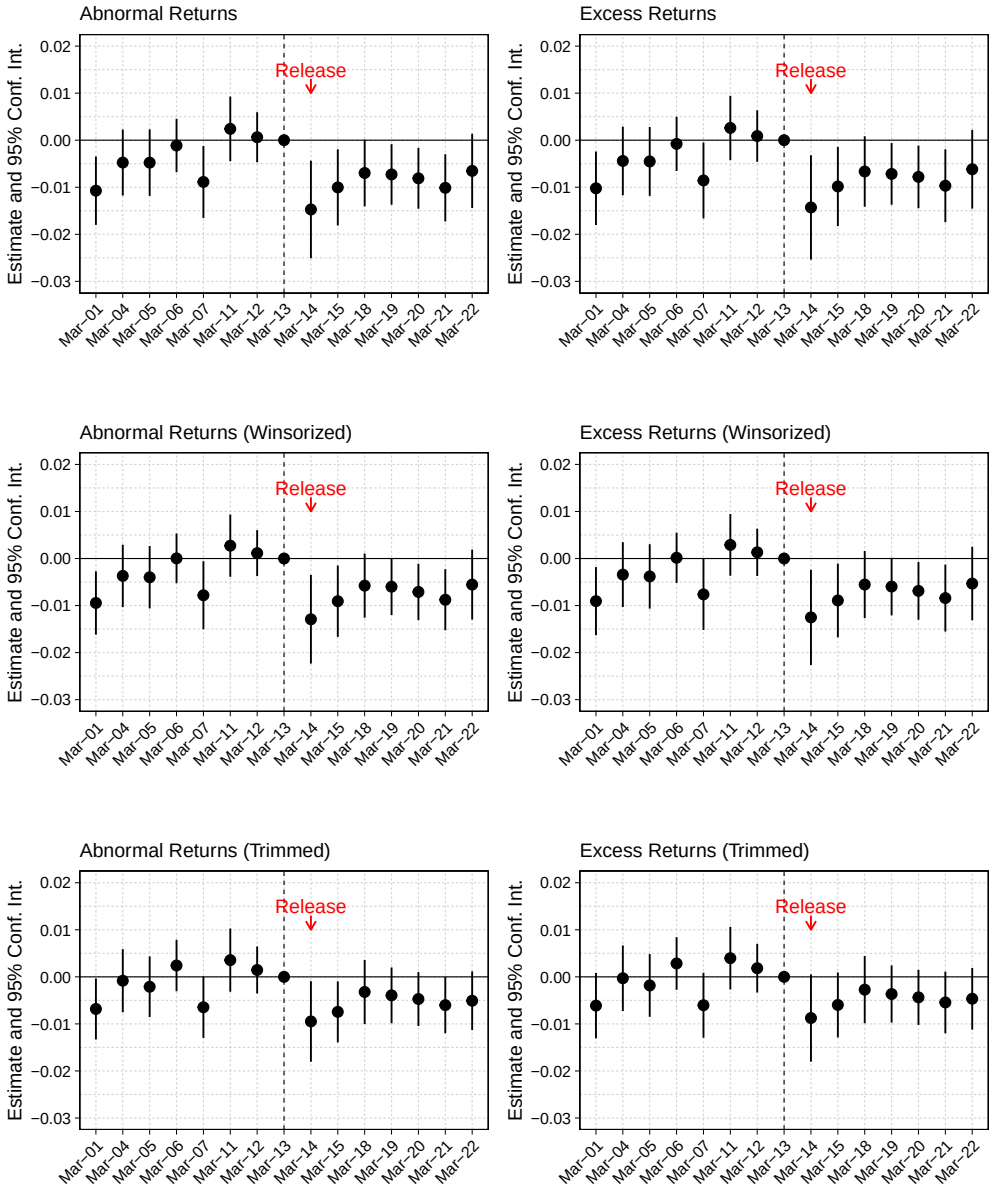


Note: The left panel represents the sectoral distribution of all companies traded on the National Stock Exchange. The right panel shows the sectoral distribution of publicly listed donors relative to the overall sectoral distribution. Manufacturing, utilities (electricity, gas, steam), construction, and agricultural sector companies are overrepresented as donors. Sectors represent NIC "Section" divisions.

Appendix B. Event Study

B.1. Plots

FIGURE A2. Daily Returns Event Study Plots



Note: Winsorizing and trimming daily returns ensures results are not driven by outliers. Winsorized: Daily returns less than the first percentile or greater than the 99th percentile of daily returns capped to the 1st or 99th percentile. Trimmed: Daily returns restricted to 1-99th percentile of daily returns.

B.2. Tables

TABLE A2. Daily Return Event Study

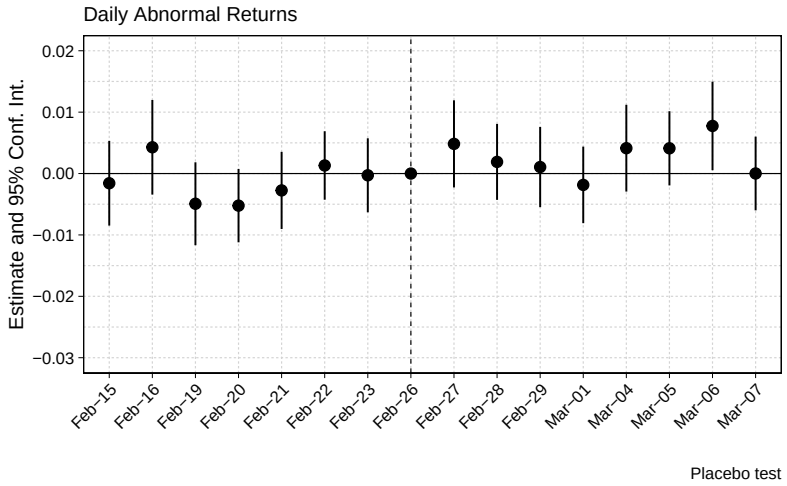
	Abnormal Return			Excess Returns		
	(1)	(2)	(3)	(4)	(5)	(6)
Mar-01 × is_donor	−0.011	−0.009	−0.007	−0.010	−0.009	−0.006
	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Mar-04 × is_donor	−0.005	−0.004	−0.001	−0.004	−0.003	0.000
	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Mar-05 × is_donor	−0.005	−0.004	−0.002	−0.005	−0.004	−0.002
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Mar-06 × is_donor	−0.001	0.000	0.002	−0.001	0.000	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mar-07 × is_donor	−0.009	−0.008	−0.006	−0.009	−0.008	−0.006
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Mar-11 × is_donor	0.002	0.003	0.004	0.003	0.003	0.004
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mar-12 × is_donor	0.001	0.001	0.001	0.001	0.001	0.002
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Mar-14 × is_donor	−0.015	−0.013	−0.009	−0.014	−0.013	−0.009
	(0.005)	(0.005)	(0.004)	(0.006)	(0.005)	(0.005)
Mar-15 × is_donor	−0.010	−0.009	−0.007	−0.010	−0.009	−0.006
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Mar-18 × is_donor	−0.007	−0.006	−0.003	−0.007	−0.006	−0.003
	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Mar-19 × is_donor	−0.007	−0.006	−0.004	−0.007	−0.006	−0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mar-20 × is_donor	−0.008	−0.007	−0.005	−0.008	−0.007	−0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mar-21 × is_donor	−0.010	−0.009	−0.006	−0.010	−0.008	−0.005
	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)
Mar-22 × is_donor	−0.007	−0.006	−0.005	−0.006	−0.005	−0.005
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)
Modification		Winsorized	Trimmed		Winsorized	Trimmed
Num.Obs.	23 055	23 055	22 440	23 055	23 055	22 447
R2	0.317	0.339	0.331	0.301	0.321	0.311
Std.Errors	by: isin	by: isin	by: isin	by: isin	by: isin	by: isin
FE: ISIN	✓	✓	✓	✓	✓	✓
FE: Trading Day	✓	✓	✓	✓	✓	✓

Note: OLS estimation based on equation 2, with period dummies interacted with total assets, leverage, return on equity, market cap, and NIC product section for each firm. Firm financial data as of September 30, 2023. Market cap is as of Feb 15, 2024. Firm-clustered standard errors reported in parentheses.

B.3. Placebo Test

Replace event date with a placebo date from two weeks before the information shock. I choose Feb 26 as the reference date. Figure A3 shows no systematic pattern of difference in abnormal returns between firms revealed on the list of donors and not.

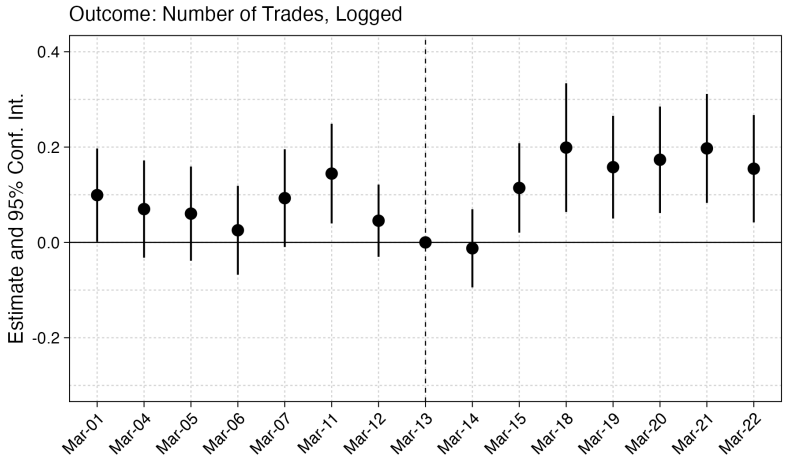
FIGURE A3. Event Study Placebo Test



Note: 95% confidence interval. Size, leverage, profits, market capitalization (as of Feb 15, 2024) and industry sections all interacted with time dummies.

B.4. Trading Volumes

FIGURE A4. Number of Trades



Note: 95% confidence interval. Size, leverage, profits, market capitalization (as of Feb 15, 2024) and industry sections all interacted with time dummies.

Appendix C. Cumulative Abnormal Returns

C.1. CATT on Cumulative Abnormal Returns

TABLE A3. CATT on Cumulative Abnormal Returns

	1	2	3	4	5	6	7	8	9	10
Post x Donor	−0.015	−0.017	−0.016	−0.015	−0.015	−0.017	−0.015	−0.006	−0.005	−0.004
	(0.005)	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
R2	0.652	0.653	0.649	0.644	0.625	0.643	0.649	0.618	0.591	0.565
Trading days	1	2	3	4	5	6	7	8	9	10
Num Stocks	1537	1537	1537	1537	1537	1537	1537	1537	1537	1537

Firm-clustered standard errors in parentheses

Appendix D. Mechanism

D.1. News Mentions

I use `newsapi.org`, which provides a REST (representational state transfer) API to query current and historical news records from over 150,000 news sources from around the world in multiple languages. Each query returns the headline, a snippet, and a description of each article, along with authorship information, source, and language.

I carry out three sets of searches. First, I construct the *electoral bonds news corpus* by searching for the term "electoral bonds" between Jan 1, 2024 and April 30, 2024. This search produces around 1,200 search results all related to India's electoral bonds. Panel A in figure 5 shows how articles that meet this search term spike around key dates: the supreme court decision as well as the release date.

Second, I construct a *reference news corpus* by searching for news articles that contain the terms "elections" and "India" between Jan 1, 2024 and April 30, 2024. I include "India" to restrict the geography to India, as `newsapi.org` does not allow geographic filters in setting search parameters.

Finally, for each treated company revealed to be a donor, I search for news that contains the name of the firm and "electoral bonds", and filter for the period from March 14-28, 2024.

D.1.1. News Text Analysis

I compare the *electoral bonds news corpus* to the *reference news* corpus by computing the term frequency-inverse document frequency (TF-IDF) for key terms in each corpus, and calculating a ratio between each corpus's TF-IDFs.

$TF - IDF_{ij}$ for each term i in corpus j is defined as the product of Term Frequency (TF_{ij}) and Inverse Document Frequency (IDF_{ij}). TF_{ij} is the count of occurrences of i in corpus j. IDF_{ij} is the log of one over the share of documents in corpus j that contain the term i.

Per Gentzkow et al. (2019), TF-IDF filters out both very rare and very common terms because TF_{ij} will be low for extremely rare terms, and IDF_{ij} will be low for extremely common terms that are present in all documents.

The TF-IDF ratio is defined for each term as:

$$\text{Ratio}_i = \frac{\text{TF-IDF}_{i,j=\text{Electoral Bond News}}}{\text{TF-IDF}_{i,j=\text{Reference News}}} \tag{A1}$$

Table A4 presents the statistics.

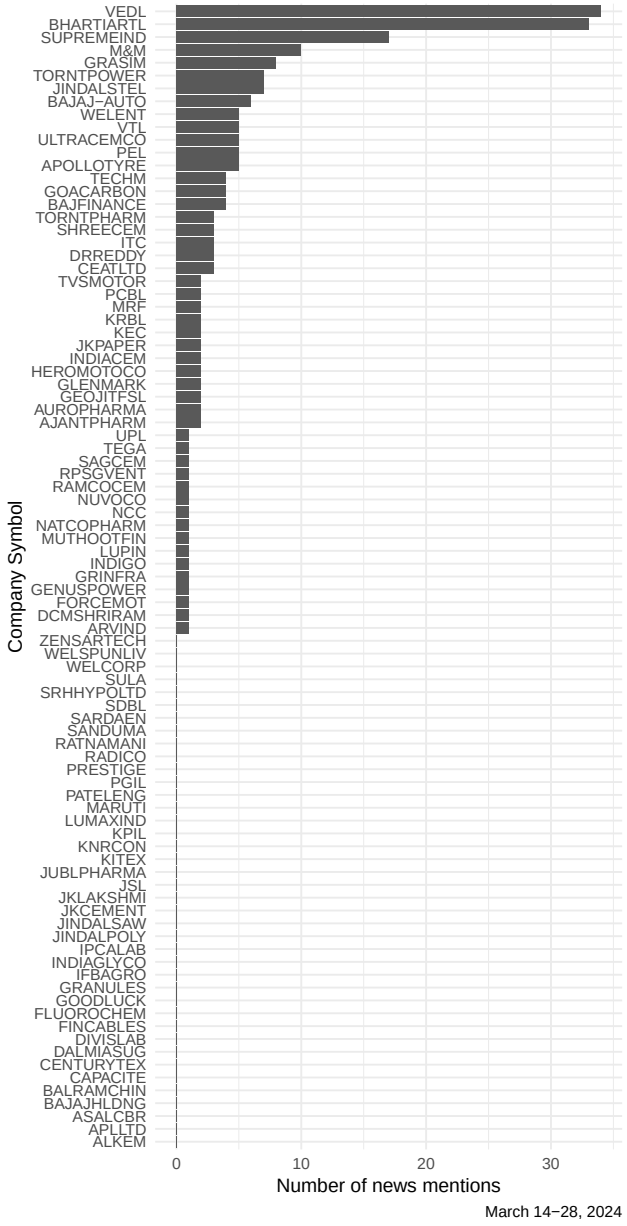
TABLE A4. TF-IDF Comparison

Term	EB News TF-IDF	Reference News TF-IDF	Ratio
extort	61.05	8.47	7.21
bond	325.32	58.00	5.61
transpar	108.47	22.10	4.91
scam	82.94	22.10	3.75
corpor	81.57	28.43	2.87
firm	81.20	31.87	2.55
money	129.58	54.00	2.40
black	58.26	28.78	2.02
bribe	12.46	6.60	1.89
bank	179.57	160.09	1.12
constitut	72.57	88.98	0.82
bjp	299.25	425.54	0.70
delhi	143.57	208.94	0.69
congress	267.01	409.84	0.65
opposit	120.17	187.31	0.64
leader	144.03	240.75	0.60
elect	252.90	451.05	0.56
poll	182.13	359.39	0.51
modi	198.07	429.46	0.46

D.1.2. News coverage for each company

For each donor company, I count the number of articles that contain the name of the company and "electoral bonds" between March 14-28, 2024.

FIGURE A5. Count of news stories about each donor company and electoral bonds



Note: Information from `NewsAPI`. Count of how many news articles were written that contained both the name of the company and included a discussion of electoral bonds between March 14-28, 2024. Source: `NewsAPI.org`

D.2. Heterogeneous Effects

D.2.1. Cumulative Abnormal Returns over 7 Days: Table

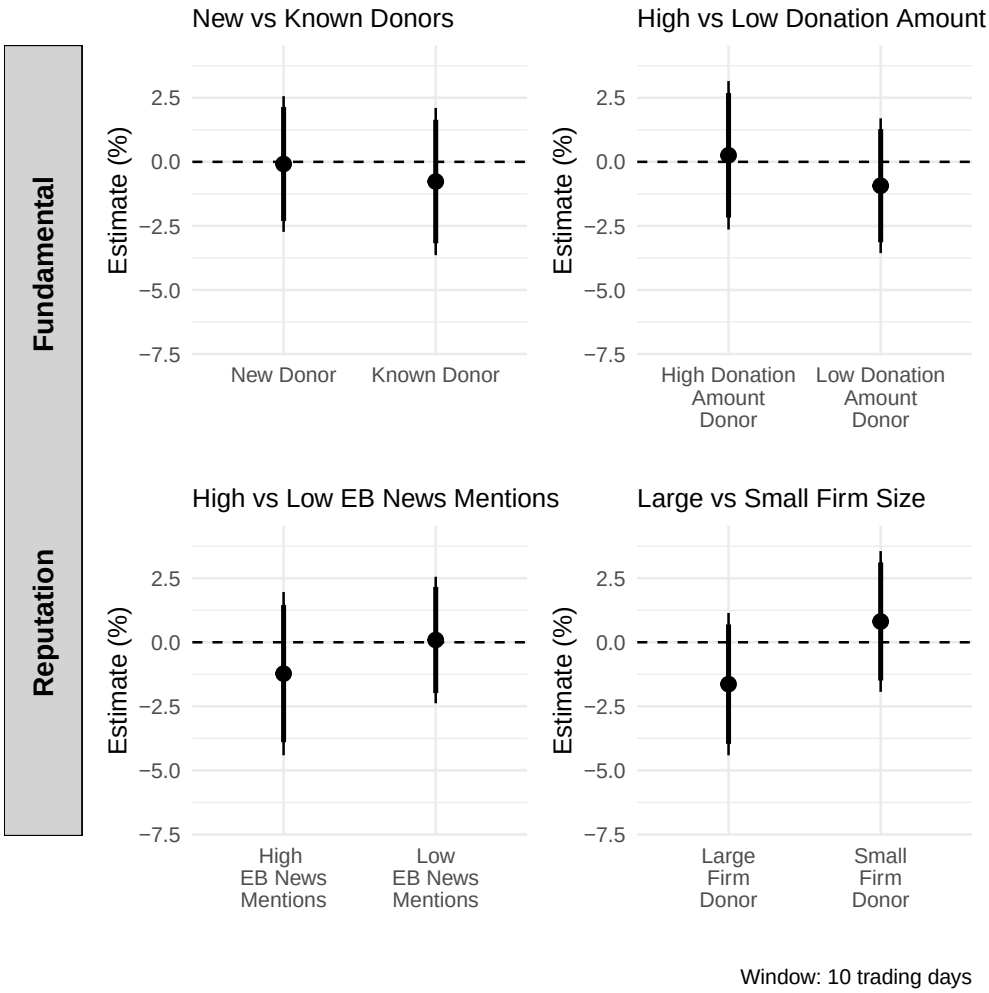
TABLE A5. Heterogenous effect on 7 Day CAR

	New Donor	Amount Donated	Size of Firm	Media Coverage
post × New Donor	−0.013			
	(0.010)			
post × Known Donor	−0.017			
	(0.010)			
post × Amt Donated: Low		−0.015		
		(0.011)		
post × Amt Donated: High		−0.015		
		(0.010)		
post × Firm Size: Small			0.004	
			(0.010)	
post × Firm Size: Large			−0.034	
			(0.010)	
post × News Mentions: Low				−0.004
				(0.009)
post × News Mentions: High				−0.034
				(0.011)
R2	0.649	0.649	0.650	0.650
Num.Obs.	3074	3074	3074	3074

Cumulative abnormal returns over 7 trading days; firm-clustered standard errors in parentheses.

D.2.2. Cumulative Abnormal Returns over 10 Days

FIGURE A6. Conditional ATT: Cumulative Abnormal Returns by Treatment Arm



Note: For 10 days after the event date. Overall effects are negligible, as are heterogeneity across treatment arms.

Reputational Costs of Revealing Political Connections: Evidence from Electoral Bonds in India

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July 25, 2024

Disclosure Statement

I received $450 from my advisor, Ben Ross Schneider, to purchase a one month subscription to `newsapi.org` to fetch news articles around the relevant time periods. I accessed corporate financial data from CMIE's Prowess database and Datastream through MIT Libraries.

Insofar as this project relates to Indian equity markets, I would like to disclose that my father is a board member of one publicly listed company (RBL Bank) and an advisor to another publicly listed company (Vedanta Ltd).

I have no further sources of financial support or potential conflicts of interest to disclose.

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