Campaign money for nothing? Understanding the consequences of a ban on corporate contributions: evidence from Brazil.

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VERY PRELIMINARY.

Abstract

We study the effects of the 2015 ban on corporate contributions in Brazil on the allocation of procurement contracts and the frequency of large personal contributions of corporate members, a channel to circumvent the ban. We use difference in differences regression models that compare outcomes of contributing and non-contributing firms before and after the ban. We document three findings. First, before the ban, contributing firms won 20 to 25% more procurement tenders per year than non-contributing firms. Second, the ban on corporate contributions significantly decreased the number of national procurement contracts won by contributing firms by 2 to 11 percentage points, a magnitude that only partially offset their previous advantage. Third, firms previously making large contributions circumvent the ban in both national and local elections with donations from owners and board members. Together, our findings suggest that a ban on corporate contributions have limited effects on the average influence of contributing firms when there are mechanisms to circumvent the ban.

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"The Brazilian political system is based on campaign financing by large private interests. The law [the ban on corporate contributions] only changed from the CNPJ [corporate fiscal identifier] to the CPF [personal fiscal identifier], the logic remains the same"
Guilherme Boulous, 2018 presidential candidate

1 Introduction

Campaign finance is one of the most debated and controversial issues in contemporary politics (Dawood, 2015). One one hand, advocates of campaign financing regulations claim that campaign contributions increase the influence of interest-groups on public policy and foster corruption. On the other hand, its critics argue that contributions may improve welfare by reducing public campaign spending, increasing political information, fostering political participation, and increasing political competition. Given the ambiguous welfare effects, quantifying the causal effects of campaign financing regulations is crucial to inform such heated debate.

Despite growing evidence documenting that contributing firms receive more government contracts (e.g., Goldman et al., 2013; Tahoun, 2014; Boas et al., 2014; Arvate et al., 2018; Baltrunaite, 2019; Titl and Geys, 2019; Baranek and Titl, 2019) the literature studying political connections is relatively silent about whether and how campaign financing reforms affect the return of contributing firms. While Baltrunaite (2019) shows that the return of contributing firms completely vanishes after a ban on corporate donations in Lithuania, it is still unclear whether a ban on corporate contributions is effective in reducing the return of contributing firms when there are mechanisms to circumvent the ban. In this paper, we help to fill this gap by studying whether and how the 2015 ban on corporate contributions in Brazil changed the allocation of procurement contracts across firms and the distribution of large personal contributions of corporate members.

A clear understanding of the consequences of a ban on corporate contributions attracts considerable policy interest. First, campaign financing regulations are ubiquitous to democracies (Scarrow, 2007). Second, around one-third of the countries ban corporate contributions to either parties or candidates (IDEA, 2020). Third, public procurement is an economically relevant outcome, accounting from 8 per cent (Schapper et al., 2017) to 25 per cent of the worldwide GDP (World Bank, 2017).

The impact of a ban on corporate contributions on procurement outcomes is, *a priori*, ambiguous. On the one hand, it may reduce the influence of corporations in the allocation of government contracts by reducing the amount of money they give to political campaigns. On the other hand, the ban on corporate contributions may be ineffective or even counter-effective in reducing the influence of interest groups. For instance, some corporations may increase their relative influence by finding mechanisms to circumvent the limits, or corporations may keep sending the same amount of money to campaigns but using less transparent forms of contribution.

Ultimately, whether and how a ban on corporate contributions reduces the influence of contributing firms is an empirical question. Ideally, we would like to answer such a question by randomly allocating the corporate ban across electoral districts and comparing their procurement outcomes. However, since this ideal experiment would be politically and ethically problematic, we must rely on non-experimental data to study the consequences of a ban on corporate contributions.

Identifying the effects of a ban on corporate contributions using non-experimental data is challenging. First, as contributing and non-contributing firms have different observable characteristics, omitted variable bias may be a problem because they firms may be differently affected by concurrent events to the ban on corporate contributions such as corruption-scandals and economic recessions. Second, simultaneity bias may be a problem because being a contributing firm may be a consequence of winning procurement contracts with the government. Third, to apply most non-experimental methods that correct for such endogeneity problems, we need to find a setting where both campaign contributions and procurement outcomes are observable before and after the ban, which is rare.

Using Brazil as a laboratory, we solve such challenging inferential problem by estimating a difference in differences estimator that compares procurement outcomes of previously contributing and non-contributing firms before and after the 2015 ban on corporate contributions in Brazil. We provide support to the non-testable identification assumption that outcomes of contributing and non-contributing firms would follow parallel trends *in the absence of the reform* by showing that they follow apparently parallel trends *before the reform*.

Brazil is an ideal laboratory to study the consequences of a ban on corporate contributions. First, corporate contributions were the primary source of funding for expensive political campaigns before being banned, accounting for around three-quarters of campaign revenues in national elections and one-half in local elections before the ban. Second, since contributing firms received more government loans (Claessens et al., 2008; Carvalho, 2014) and win more government contracts (Boas et al., 2014; Arvate et al., 2018) before the ban, we study the consequences of a ban on corporate contributions in a setting where campaign funding was buying influence on the allocation of government resources.

We first use procurement data from the federal government from 2007 to 2018 and data on corporate contributions to 2006, 2010, and 2014 electoral campaigns to estimate the effect of the ban on corporate contributions on the log-number of procurement tenders won in a given year by a given firm. We find that the ban on corporate contributions significantly decreased the number of federal procurement contracts won by contributing firms by 2 to 11 percentage points, a magnitude that only partially offsets the 20 to 25% previous advantage of contributing firms. Results are similar when using the indicator variables for winning a procurement tender in a given year as an outcome.

We then use data on personal contributions from 2008 to 2016, and corporate contributions from 2004 to 2012 to investigate whether large contributing firms use personal contributions of its owners and board members to circumvent the ban. Using a sample of participants in national procurement auctions, we find that contributing firms are 5.4% more likely to have members making a large personal contribution to federal electoral campaigns after the ban, an increase of more than 50% on the pre-ban mean among treated firms. Results are similar when using a sample with all firms in the fifty economic sectors making more corporate contributions and when investigating contributions to local elections, although with smaller magnitudes in the last case.

Our preliminary findings speak to two branches of literature. First, they contribute to the extensive empirical literature quantifying the returns of political connections. Despite growing evidence showing that corporate contributions buy influence in the allocation of government contracts (Goldman et al., 2013; Tahoun, 2014; Boas et al., 2014; Brogaard et al., 2015; Arvate et al., 2018; Baltrunaite, 2019), this literature is relatively silent about wheater and how reforms affect the return of connections. While Baltrunaite (2019) shows that the advantage of contributing firms completely vanished after a ban on corporate donations in Lithuania, we do not know unknown whether a ban on corporate contributions would have the same effect when personal contributions remain unlimited. We complement this literature by documenting that, in contrast to Baltrunaite (2019), a ban on corporate contributions only partially reduced the advantage of contributing firms, suggesting that the impact of a ban on corporate contributions on the influence of contributing firms is limited when there are mechanisms to circumvent the ban.

Second, this paper relates to the literature trying to explain the puzzling modest amounts of money in U.S. politics (Ansolabehere et al., 2003; Bombardini and Trebbi, 2011), a country that banned corporate contributions in 1907. Recent research (Bertrand et al., 2019) shows that U.S. corporations exploit less regulated mechanisms of influence, such as charitable giving, to buy influence in the political process. We produce original evidence that campaign financing reforms led to the rerouting of corporate contributions to other forms of contribution, a hypothesis known as *"hydraulics of campaign finance"* (Issacharoff and Karlan, 1998). The finding of an abnormal increase in less transparent forms of contribution after a regulatory change suggests that tight campaign financing regulations may be part of the reason why we observe a modest amount of official campaign contributions.

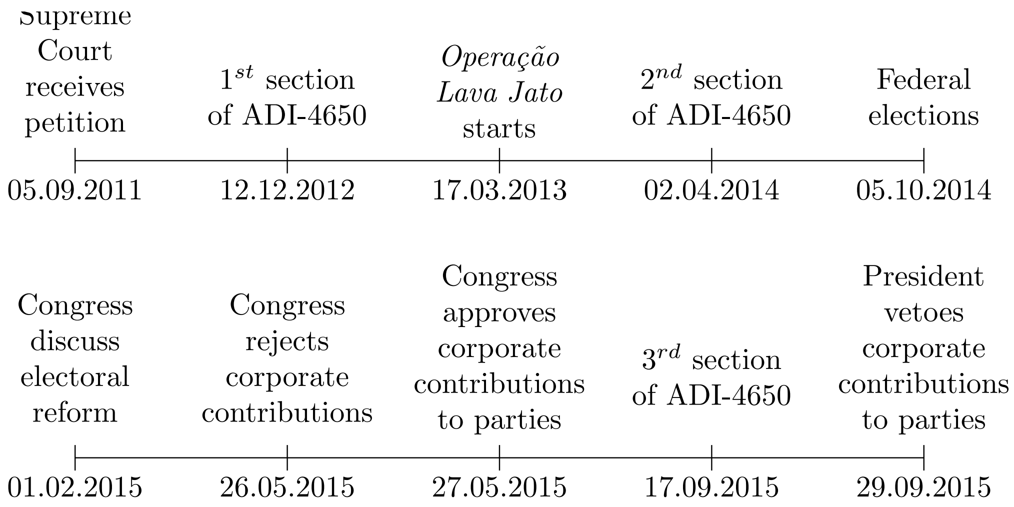
Our findings tell cautionary messages to policy-makers. First, we highlight the risks of changing electoral rules by judicial decisions or without proper legislative discussion. Second, we shed light on the unintended effects of designing campaign financing reforms that affect *only one flow of campaign contributions*.

2 Background

2.1 The ban on corporate contributions.

Figure 1 describe the main events leading to the ban on corporate contributions of 2015. In September of 2011, the *Ordem dos Advogados do Brasil* (OAB) made a petition asking the Supreme Court to judge the legality of corporate contributions to political campaigns arguing that corporations were using them to buy influence. The petition gave birth to the *Ação Direta de Inconstitucionalidade 4650* (ADI-4650), a pronouncement on the legality of the practice. In December of 2013, the rapporteur plus three judges ruled in favour of the petition during the first session of ADI-4650.

Figure 1: Timeline of events leading to the ban on corporate contributions



In March 2014, Brazil's Federal Police started a large-scale investigation of corruption named *Operação Lava Jato*, which later opened the most massive corruption scandal in the country history. The investigation gained ADI-4650 popular support as news broke that corporations were exchanging overpriced contracts with the state-owned oil company Petrobras for campaign contributions.

In April 2014, four additional judges voted for the illegality of corporate contributions during the second session of ADI-4650, ensuring a majority on the matter. However, with the session ending, the last judge to pronounce requested postponement of judgement to revise the process. With the suspension, ADI-4650 could not be concluded before the deadline for electoral reforms and ensured the legality of corporate contributions during the October 2014 federal elections.

In February 2015, the newly-elected congress started discussing a draft of electoral reform that included a provision aimed to legalize all corporate contributions before the conclusion of ADI-4650. On May 26^{th} of 2015, the congress rejected the provision after intense pressure from the society but, in the following day, approved a second provision legalizing corporate contributions to political parties.

In September 2015, ADI-4650 ruled corporate contributions illegal. Upon conclusion of ADI-4650, president Dilma Rousseff vetoed the provision legalizing corporate contributions to political parties thus effectively enforcing a ban on corporate contributions.

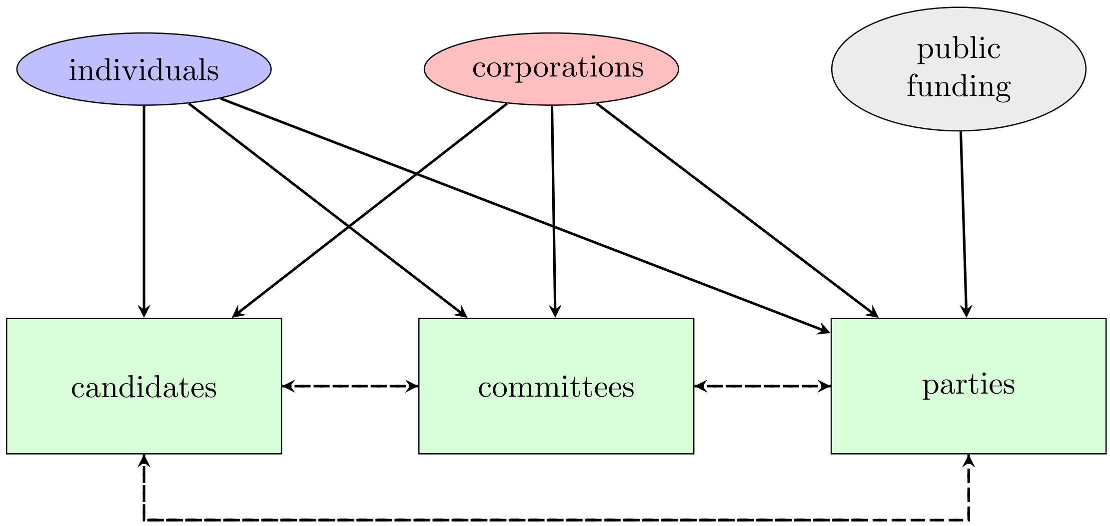
2.2 Electoral campaigns in Brazil.

Elected offices in Brazil are renewed every four years, with municipal elections being held two years after national elections. Presidents, governors and mayors may be reelected only once, whereas members of parliament can be reelect indefinitely, thus creating room for repeated interactions between MPs and interest groups.

Campaign contributions in Brazil are tightly regulated. First, contributions are only allowed during the 90 days of the official electoral campaign. Second, all contributions in Brazil elections must identify donor and receiver and are made public as soon as the regulator receives the information. Third, before the ban on corporate contributions, firms were allowed to legally donate up to 2% of their gross annual revenues to political campaigns. Forth, personal contributors are still allowed to date to legally contribute up to 10% of their gross annual income to political campaigns.

Brazilian campaign financing is a mix of private and public electoral funding. Figure 2 describes the flows of campaign contributions before ADI-4650. The primary sources of campaign revenues were public funding, personal contributions, and corporate contributions. Corporations could contribute to candidates, committees, and parties, which could reallocate the funds by contributing to each other. ADI-4650 abolished the direct flows of money from corporations to candidates, committees, and parties in Figure 2. Relevant to our analysis, owners and board members of corporations are still allowed to date to make personal contributions to political campaigns.

Figure 2: Flows of campaign contributions before ADI-4650



Brazilian electoral rules stimulate intensive political campaigns. First, legislative elections involve fierce competition among a large number of candidates and parties.[1] Second, since legislative elections follow an open-list proportional system, competition for votes is also intense among candidates within the same coalition. Third, candidates travel long distances during campaigns as electoral

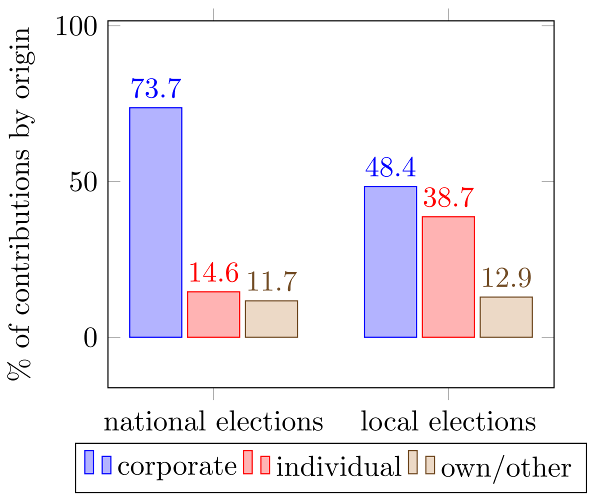
[1]For instance, more than 3 thousand candidates from 35 parties competed in the 2014 federal legislative elections.

districts coincide with the states of Brazil, a vast continental country.

Not surprisingly, electoral campaigns in Brazil are expensive. In the 2014 national elections, the total declared campaign spending achieved R$ 8.46 billion, which correspond to .153 percentage of the 2014 GDP. Legislative elections are particularly expensive. In the same year, the total declared spending of achieved 2.38 billion for federal congress campaigns and R$ 2.55 billion for state congress campaigns. The total campaign spending of all elected federal congressman achieved R$ 733 million in 2014, implying average spending of R$ 1.42 million per congressman.

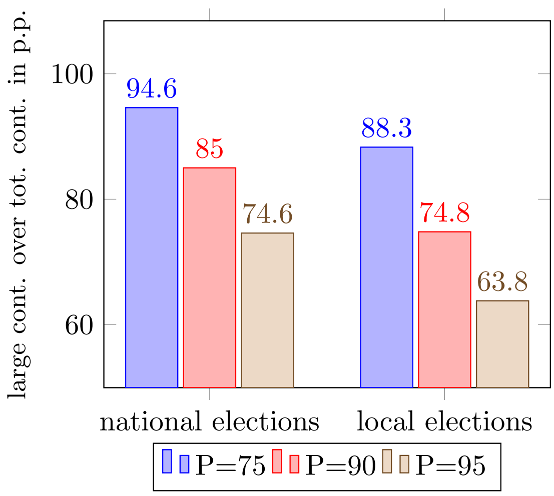
Prior to the 2015 ban, corporate contributions funded a relevant part of such expensive campaigns. Candidates, committees, and parties received R$ 3.04 billion in corporate contributions in 2014, around 36.01% of the total. Figure 3 shows the composition of total campaign revenues by donor type in the 2006, 2014 and 2014 federal elections. Corporate contributions were around five times larger than personal contributions.

Figure 3: Relative weight of corporate, personal and other contributions (own, party, committee) to the 2006, 2010 and 2014 federal elections.



Large contributions account for the bulk of funding. Figure 4 shows the share of campaign funds from donations above the 75th, 90th and 95th percentile for federal and municipal elections between 2006 and 2014. For instance, the top quartile of contributions account for 88 to 95% of the funds funding before 2015. The predominance of large contributions in Brazil contrasts with campaign financing patterns in developed nations such as the United States, Canada, Germany, and the United Kingdom, where small contributions account for a considerable fraction of the total funding (Bouton, Castanheira, Drazen, 2019).

Figure 4: Large contributions were the main source of revenue.



Several factors may explain why large contributions are the primary source of campaign funding in Brazil. First, lax limits on personal donations, and on corporate donations until ADI-4350. Second, even after the ban on corporate contributions, wealthy individuals can still legally fund a substantial fraction of campaign costs because of lenient contribution limits. Third, since lobbying is largely unregulated in Brazil, large contributions may be compensating politicians for past political support or buying future influence.

2.3 Public procurement in Brazil

The Public Procurement Act of 1993 regulates the allocation of government contracts from pubic organizations to firms in Brazil. According to the Act, all organizations must procure off-the-shelf goods using auction-based mechanisms. Federal organizations must conduct electronic auctions using an online platform, ComprasNet.

ComprasNet auctions usually follow a four-step process. First, the organization publicly releases a document describing the lots to be procured, the rules of the tender, and the reservation price of each lot. Second, interested firms submit an opening bid for a given lot. Next, firms compete in a descending online auction starting from the lowest opening bid, and ending randomly between zero and thirty minutes after a warning signal. Finally, the winning firm is awarded the contract when its documentation is approved.

3 Data

In this section, we describe the data sets used in our analysis and discuss how we match them, select the estimation sample, and compute our main variables.

3.1 Sources

We combine information from several sources. First, data on federal procurement come from the SIASG-ComprasNet. Second, data on corporate contributions and personal contributions come from the *Tribunal Superior Eleitoral* (TSE). Third, we identify workers using data from the *Relação Anual de Informações Sociais* (RAIS). Forth, we identify owners and board members using data from *Cadastro Nacional de Empresas* (CNE). In this subsection, we describe our main data sources.

Procurement data. ComprasNet has information about the universe of auctions and procurement contracts from the Brazilian federal government. We obtained these data from the *Conselho Administrativo de Defesa Econômica* (CADE), the Brazilian antitrust authority.[2]

We compute procurement outcomes at the firm-year level from 2007 to 2018. First, we build indicators for participating, bidding, and winning procurement tenders. Second, to gauge procurement outcomes on the intensity margin, we compute the $\log(1+x)$ transformation of the number of procurement tenders in which the firm participated, bid, and won in a given year.

Campaign contributions data. Data on campaign contributions for four three municipal elections (2004, 2008, 2012, and 2016) and four federal elections (2006, 2010, 2014, and 2018) come from the Brazilian Supreme Electoral Court, the *Tribunal Superior Eleitoral* (TSE). Corporate contributors are personal contributors are uniquely identified by the corporate taxpayer identifier, the Cadastro Nacional de Pessoa Jurídica (CNPJ) and the personal taxpayer identifier, the Cadastro Nacional de Pessoa Física (CPF). For the majority of personal donors the full name is available.

Labor market data. We match employers and employees using RAIS, a panel containing information about the universe of formal sector jobs and establishments. [3] Each worker in the dataset is uniquely identified by the Programa de IntegraÃ§Ã£o Social (PIS) and the CPF.

[2]We thank Bruno Duarte Garcia and Felipe Leitão Valadares Roquete for gently sharing updated versions of the SIASG-ComprasNet's data warehouse.

[3]Access to RAIS has been granted by an institutional agreement with the *MinistÃ©rio do Trabalho e Emprego* (MTE), the Brazilian Ministry of Labour.

The *Classificação Brasileiro de Ocupações* (CBO) - the Brazilian taxonomy of jobs - describes the occupation of workers in the RAIS. Using the CBOs, we identify white-collar workers as the ones whose occupation's description has the words "*administrator*" (administrator), "*diretor*" (director), or "*gerente*" (manager).

Firm ownership data. The *Cadastro Nacional de Empresas* (CNE) is a national registry of firms containing information about owners and board members for the universe of Brazilian firms. We download the complete version of the CNE on the website of the *Receita Federal do Brazil* (RFB), the Brazilian fiscal authority. The CNE uniquely identifies firms by their CNPJ and reports the names of owners and board members.

3.2 Matching the data and selecting the sample

Matching the data. We identify contributions of employees by matching RAIS and TSE in the following way. We first select the active RAIS job-relations at the start of each electoral campaign between 2006 to 2018. In case of multiple jobs per worker in a given year, we follow Menezes-Filho et al. (2008); Helpman et al. (2017); Colonnelli and Prem (2017) and keep only the oldest highest-paying job for each worker. Next, we filter personal contributions with a valid CPF and aggregate by contributor-year. We then merge workers data with donations data on the CPF. Finally, we compute statistics about contributions of firm owners and board members collapsing the matched data at the firm-year level.

We identify the contributions of owners and board members by linking the collapsed TSE data with the CNE data using on full names. We mitigate problems of homonymy by partially disambiguating the CNE's records with unique personal identifiers contained the Orbis database.

Selecting the sample. We compile a data set with firm-year level procurement and firm outcomes from 2007 to 2018. Our pre-treatment period goes from 2007 to 2014 and the post-treatment from 2015 to 2018.

We select a panel data of *potential* government suppliers by implementing the following steps. First, we exclude all firms that closed during the period of observation. Second, we exclude firms without workers during at least one year. Third, we exclude all state-own corporations and non-profit organizations. Forth, we exclude all firms which did not participate in at least one ComprasNet auction during the sample period.

We focus on a sub-population of *potential* government suppliers for three reasons. First, we believe it is the most relevant to understand the economic consequences of campaign financing reforms. Second, we increase the strength of the identification by using a sub-sample where contributing and non-contributing firms are less heterogeneous. Third, we increase the precision of our estimates by substantially increasing fraction of contributing firms in the estimating sample.

We construct a balanced panel of active firms before and after the treatment for two reasons. First, by including firms which only participated in ComprasNet auctions after (before) the treatment, we allow changes in the composition of government suppliers to be a mechanism. Second, by inputting zeros to procurement outcomes of firms which shut-down after the treatment, we allow firm-closure to be a mechanism.

4 Methodology

4.1 Effects on procurement outcomes

Main specification. We investigate whether the 2015 ban on corporate contributions affected the allocation of procurement contracts across firms by estimating a difference in differences model that compares outcomes of previously contributing and non-contributing firms before and after the ban on corporate contributions. We estimate the regression model

$$\begin{aligned} y_{f,t} =& \alpha_f + \alpha_t + \beta_1 contributing_f + \beta_2 post_t + \\ &+ \beta_3 contributing_f \cdot post_t + \xi_{f,t} \end{aligned} \tag{1}$$

where $y_{f,t}$ is the procurement outcome of firm f at year $t \in \{2007, ..., 2018\}$, $contributing_f$ is a dummy variable equal to one if firm f donated at any of the three federal elections (2006, 2010, and 2014) before the ban, $post_t$ is a dummy equal to one if $t \in \{2015, ..., 2018\}$ and zero otherwise, α_f captures firm fixed-effects, and α_t captures year fixed-effects.

Our coefficient of interest β_3 measures the change in procurement outcomes of contributing firms relatively to non-contributing ones after the ban on corporate contributions. We provide favourable evidence to the economic hypothesis that the ban on corporate contributions affected the allocation of procurement contracts across firms by rejecting the statistical hypothesis that $H_0 : \beta_3 = 0$.

The causal interpretation of β_3 relies on the non-testable assumption that procurement outcomes of contributing and non-contributing firms would follow parallel trends *in the absence of the ban on corporate contributions*. To access the plausibility of this non-testable assumption in our data, we test whether procurement outcomes of contributing and non-contributing firms follow parallel trends *before* *the ban on corporate contributions*. More precisely, we test the statistical hypothesis $H_0 : \beta_{3,2007} = 0, ..., \beta_{3,2014} = 0$ in the regression model

$$\begin{aligned} y_{f,t} = & \alpha_f + \alpha_t + \beta_1 contributing_f + \beta_2 post_t + \\ & + \sum_{k \in \{2007,...,2018\}} \beta_{3,k} \cdot contributing_f \cdot \mathbf{1}(t = k) + \xi_{f,t} \end{aligned} \tag{2}$$

where $\mathbf{1}(t = k)$ is a dummy variable equal to one in year $t = k$. We provide support to the parallel trends hypothesis by not rejecting $H_0 : \beta_{3,2007} = ... = \beta_{3,2014} = 0$.

Methodological choices We design a *time-invariant* treatment variable $contributing_f$ to capture not only the exchange of present campaign contributions for future contract benefits but also the exchange of future campaign contributions for present contract benefits. In particular, the exchange of future campaign contributions for present contract benefits is particularly plausible in Brazil because legal campaign contributions are only allowed in the three months before each election.

We likely bias our treatment effect towards zero when incorrectly specifying a *time-variant* treatment. First, the treatment group is contaminated with untreated units by assuming that returns from contributions do not vanish after four years, when they do. Second, the treatment group is also contaminated with untreated units when assuming that there is an exchange of immediate benefits for future contributions, when there is none.

In contrast, we may bias our treatment effect in a non-predictable manner when incorrectly specifying a *time-variant* treatment. First, the control group is contaminated with treated units by assuming that the benefits from contributing vanish after four years when they do not. Second, we also contaminate the treatment by assuming that firms do not exchange immediate benefits for future contributions when they do. In both cases, the sign of the bias depends on the sign of the real treatment effect, which is non-observable.

We consider 2015 as a treated year despite ADI-4650 having effect starting from September of 2015 for two reasons. First, happening after September should be affected by the reform. Second, a considerable amount of resources is spent at the end of the year because Brazilian organizations have expiring budgets.

We likely employ a more conservative statistical test by including 2015 in the post-treatment period. We likely bias the treatment effect towards zero by contaminating the treatment with untreated units when assuming that firms were affected in 2015 when they are not. In contrast, when assuming that firms were not affected in 2015 when they are, we may systematically bias our treatment effect by contaminating the control with treated units. In this case, the sign of the bias depends on the sign of the real treatment effect, which is non-observable.

4.2 Effects on personal contributions of firm members

Complementary specification. Next, we test whether owners and board members of former large corporate contributors have higher probability of making large personal contributions after the 2015 ban on corporate contributions. We estimate a difference in differences model that compares the prevalence of large personal contributions among owners and board members of former large corporate contributors and those of the remaining firms before and after the ban on corporate contributions. More formally, we estimate the following regression model

$$\begin{aligned} large\_member_{f,t} =& \alpha_f + \alpha_t + \beta_1 large\_corporate_f + \\ & + \beta_2 post_t + \beta_3 large\_member_f \cdot post_t + \xi_{f,t} \end{aligned} \tag{3}$$

where $large\_member_{f,t}$ is a dummy equal to one if a member (worker, owner, board member) of firm f made an personal donation above the 75th percentile of the national distribution in any of the last two elections before the ban (i.e., 2008 and 2012 for municipal elections, and 2010 and 2014 for federal elections), $large\_corporate_{f,t}$ is a dummy equal to one if firm f made a corporate contribution above the 75th percentile of the national distribution in any of the last two elections before the ban, $post_t$ is a dummy equal to one after the ban (i.e., 2016 for municipal elections, and 2018 for federal elections), α_f captures firm fixed-effects, and α_t captures year fixed-effects.

We use indicators of large campaign contributions instead of the value contributed for two reasons. First, as discussed in Section 2.2, the largest contributions in value were the primary source of campaign financing in Brazil before the ban. Second, we make inference more credible by replacing a very left-skewed outcome - i.e., the value-contributed - with a binary variable capturing a similar event.

The causal interpretation of β_3 relies on the non-testable assumption that the prevalence of large personal contributors among owners and board members of previously large and non-large corporate contributors would follow parallel trends in the absence of the ban on corporate contributions. To access the plausibility of this assumption in our data, we test if the prevalence of large personal contributors among members of previously large and non-large corporate contributors would follow parallel trends *before the ban on corporate contributions*. More formally, we test weather $H_0 : \beta_{3,2010} = \beta_{3,2014} = 0$ in the regression model

$$large\_personal_{f,t} = \alpha_f + \alpha_t + \beta_1 large\_corporate_f + \\ + \beta_2 post_t + \sum_{k \in \{2010,2014\}} \beta_{3,k} \cdot large\_corporate_f \cdot post_t + \xi_{f,t} \tag{4}$$

where $\mathbf{1}(t = k)$ is a dummy variable equal to one in year $t = k$. This regression provides support to the parallel trends hypothesis if we do not reject $H_0 : \beta_{3,2010} = \beta_{3,2014} = 0$.

5 Results

5.1 Effects on the allocation of procurement contracts

In this subsection, we describe the findings of the complementary difference in differences specification discussed in Section 4.1.

Table 1 shows $\widehat{\beta}_1$ and $\widehat{\beta}_3$ for several differences in differences specifications using the log-number of procurement tenders won in a given year as the dependent variable. Column (1) shows the estimates of Equation 1 without fixed effects. Columns (2) through (4) sequentially add year fixed-effects, sector fixed-effects and state of headquarter fixed-effects, and firm fixed-effects. Column (5) further adds a series of indicators for percentiles of baseline variables (firm age, number of employees, average wage, and number of procurement tenders won) interacted with year indicators.[4] We cluster standard errors at the firm-level in all specifications. The first line of Table 1 shows the coefficient of $\widehat{\beta}_1$ described in Equation 1, which measures the relative advantage of contributing firms before the ban. The second line of table 1 show the coefficient of $\widehat{\beta}_3$ described in equation 1, which

[4]We compute vigintiles of the distribution of such variables in 2007, the first-year outside the estimating sample.

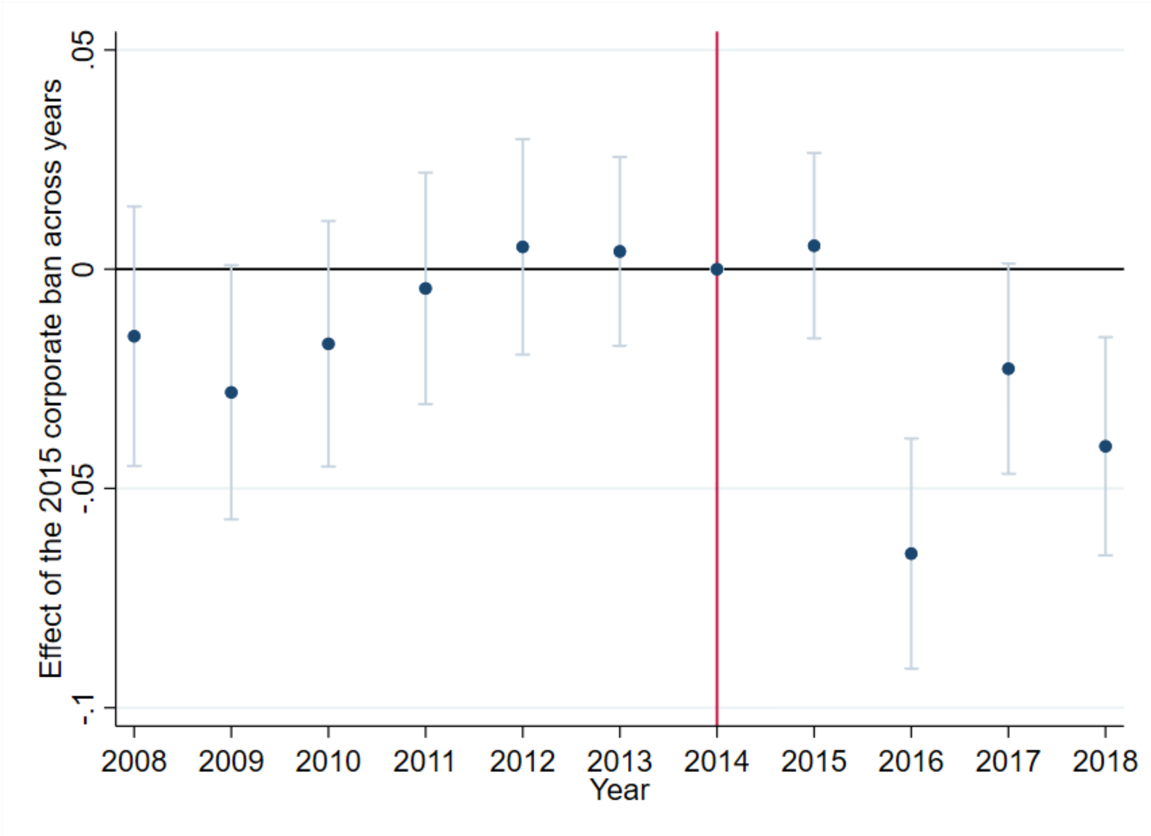
measures the percentage change in the advantage of contributing firms caused by the ban in corporate contributions.

Table 1: Baseline estimates of the effects of the 2015 corporate ban on the log-number of procurement tenders won in a given year

	(1)	(2)	(3)	(4)	(5)
Contributing=1	0.221***	0.221***	0.243***	0.000	0.000
	(0.013)	(0.013)	(0.012)	(.)	(.)
Contributing=1 × Post=1	-0.115***	-0.115***	-0.115***	-0.115***	-0.023***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)
Year FE	No	Yes	Yes	Yes	Yes
Sector (4-digit) FE	No	No	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes	Yes
Controls (vintiles)-year FE	No	No	No	No	Yes
N	1072742	1072742	1072742	1072742	1072379
R^2	0.026	0.037	0.112	0.554	0.606

Firm-level cluster standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 5: Advantage of contributing firms on the log-number of procurement tenders won in a given year contract over the sample years



In Columns (1) to (3) of Table 1, the estimated advantage $\widehat{\beta}_1$ is positive and significant at 1%. According to such estimates, contributing firms won 20 to 25% more procurement tenders than non-contributing firms before the 2015 ban on corporate contributions, which is consistent with the extensive evidence of political favouritism in Brazil. Despite not having a causal interpretation, the magnitude of $\widehat{\beta}_1$ increases when comparing contributing and non-contributing firms in the same economic sectors and state, suggesting that the true advantage of contributing firms could be even larger than estimated.

In Columns (1) to (5) of Table 1, the estimated effect of the ban on the log-number of contracts is negative and significant at 1%, implying the advantage of contributing firms decreased following the ban. The advantage of contributing firms decreased by 2.3 to 11.5 percentage points, depending on the specification. The magnitude of $\widehat{\beta}_3$ decreases substantially when we add vigintiles of baseline variables interacted with year indicators in column (5), suggesting that differential effects to the 2015-2016 economic crisis and-or more intense scarring effects from the *Lava Jato* corruption investigation among contributing firms may explain part of $\widehat{\beta}_3$ estimated in columns (1) to (4).

The results suggest that the ban only partially reduced the advantage of contributing firms. Such pattern contrasts the findings of Baltrunaite (2019), who documents that the advantage of contributing firms in the allocation of procurement contracts completely vanish after a ban on corporate contributions in Lithuania.

Figure 5 plots $\left\{ \left(k, \widehat{\beta}_{3,k}\right) \right\}_{k \in \{2007,\ldots,2017\}}$ and their confidence-intervals estimated in Equation 1 with the log-number of procurement tenders won in a given year as the dependent variable, and with year and firm fixed effects, and percentiles of baseline variables interacted with year dummies as controls. The last full year before the reform (i.e., 2014) is used as the reference-year. As can be seen in Figure 5, we fail to reject $H_0 : \beta_{3,k} = 0$ for years before the reform, supporting the parallel trends assumption and the causal interpretation of the coefficient in the conservative specification in Column (5) of Table 1.

Results are remarkably similar when using procurement outcomes at the extensive margin. Table 2 and Figure 6 have the same structure of Table 1 and 5, respectively, with indicators for winning a procurement tender in a given year. In Table 2, we also estimate a partial reduction of the pre-ban advantage. Moreover, we fail to reject $H_0 : \beta_{3,k} = 0$ for any year before the reform (Figure 6), supporting the parallel trends hypothesis and the causal interpretation of the coefficient in the conservative specification in Column (5) of Table 2.

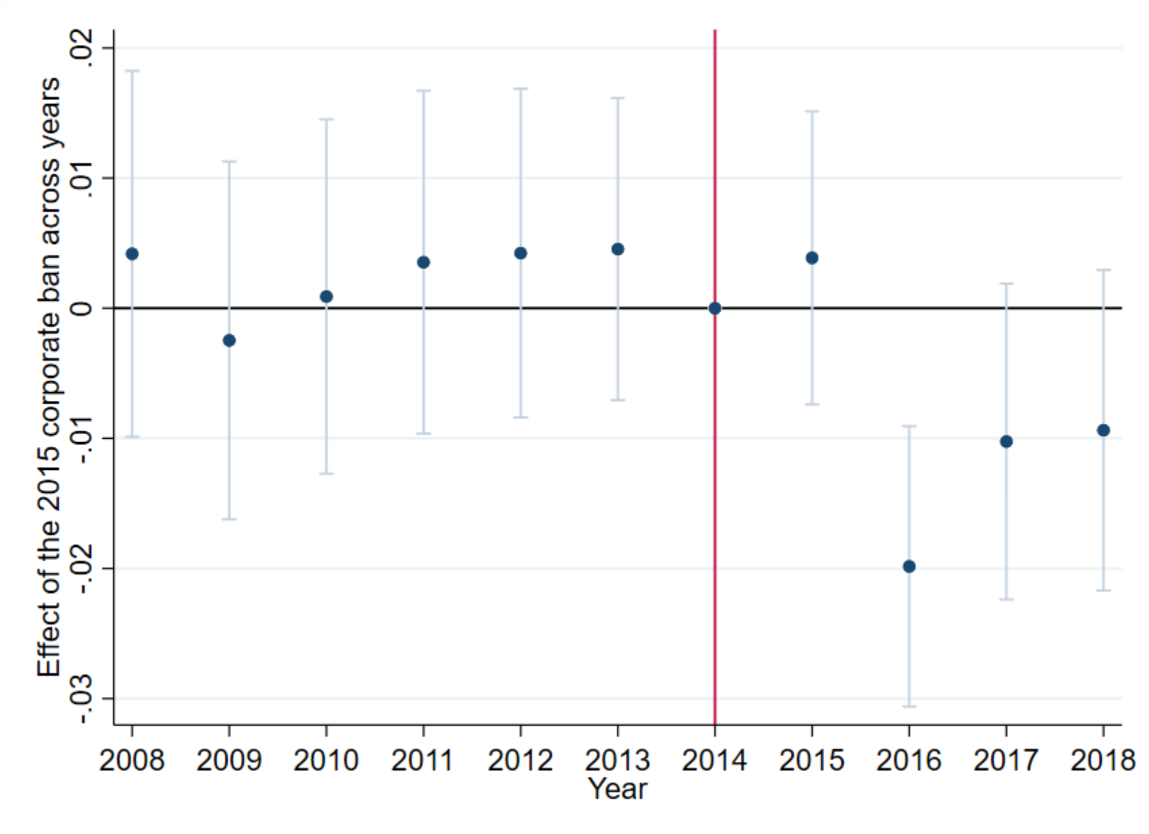
Overall, our results show that the ban on corporate contributions of 2015 decreased the advantage earned by contributing firms in the allocation of government contracts, but only partially. What may explain such limited effects of the 2015 ban on corporate contributions on the allocation of procurement contracts? We speculate that contributing firms managed to circumvent the ban on corporate contributions using the personal contributions of its members. We test such potential explanation in the next subsection.

Table 2: Baseline estimates of the effects of the 2015 corporate ban on the probability of winning a procurement tenders won in a given year

	(1)	(2)	(3)	(4)	(5)
Contributing=1	0.076***	0.076***	0.078***	0.000	0.000
	(0.004)	(0.004)	(0.004)	(.)	(.)
Contributing=1 × Post=1	-0.040***	-0.040***	-0.040***	-0.040***	-0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Year FE	No	Yes	Yes	Yes	Yes
Sector (4-digit) FE	No	No	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes	Yes
Controls (vintiles)-year FE	No	No	No	No	Yes
N	1072742	1072742	1072742	1072742	1072379
R^2	0.035	0.052	0.083	0.356	0.376

Firm-level cluster standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 6: Advantage of contributing firms on the probability of winning a procurement tender over the sample years



5.2 Effects on personal contributions of corporate members

In this subsection, we describe the findings of the complementary difference in differences specification discussed in Section 4.2.

We estimate our complmentary specification using the sample of potential government suppliers defined in Section 3.2 but restricted to the two-hundred sectors by size of corporate contributions before the 2015 ban.[5] We focus on

[5]We make the selection by computing the amount contributed by firms in each 4-digit CNAE during all municipal and federal elections between 2008 and 2014.

this sub-sample for two reasons. First, to reduce the computational burden of the fuzzy merge between the data sets containing the identity of firm members and the universe of personal contributors. Second, to reduce the possibility of measurement error caused by personal contributions of homonyms by focusing on a smaller and more relevant subset of firms. We identify treated firms as the ones making a large corporate contribution (top quartile) in 2010, 2014 or both, and the control firms as the remaining ones.

Figure 7 plots $\widehat{\beta}_{3,2010}$ and its confidence-interval estimated as in Equation 3 but using the probability of firm members making large personal contributions as an outcome, year and firm fixed effects, and the last federal election before the reform (i.e., 2014) as the reference-year. Since the confidence intervals of a $\widehat{\beta}_{3,2010}$ include the zero, we fail to reject $H_0 : \beta_{3,2010} = 0$. This result supports the validity of the parallel trends hypothesis in our comple mentary specification.

Table 3 shows $\widehat{\beta}_3$ for several difference in differences specifications using the probability of a firm member making a large personal contribution as an outcome. Column (1) shows the specification without fixed effects. Columns (2) through (4) progressively add year, firm, and sector-year fixed effects. Standard errors are clustered at the firm level in all specification.

Table 3: The effects of the 2015 corporate ban on the personal contributions of corporate members during federal elections using the sample of potential government suppliers

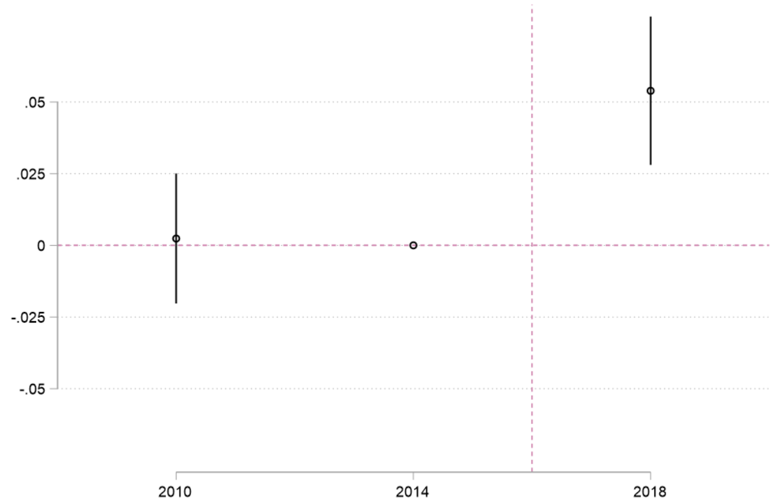
	(1)	(2)	(3)	(4)
Contributing=1	0.082***	0.079***	0.000	0.000
	(0.008)	(0.009)	(.)	(.)
Contributing=1 × Post=1	0.054***	0.057***	0.057***	0.054***
	(0.012)	(0.013)	(0.013)	(0.013)
Year FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Sector-year FE	No	No	No	Yes
N	173,619	173,619	173,619	173,603
R^2	0.011	0.011	0.528	0.533
$\bar{Y}_{Post=0,T=0}$	0.013			
$\bar{Y}_{Post=0,T=1}$	0.095			
$\bar{Y}_{Post=1,T=0}$	0.015			
$\bar{Y}_{Post=1,T=1}$	0.151			

Dependent var: Indicator equal to 1 if personal donation above 75th percentile.

Treatment: having donated above the 75th percentile of corporate donations.

Firm-level cluster standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 7: The effects of the 2015 corporate ban on large (top quartile) personal contributions of owners and board members of firms making large (top quartile) donations in federal elections , accounting for year, firm and sector-year fixed effects (Table 3, column 4). The sample of firms is restricted to potential govenrnment suppliers.



The coefficients reveal an economically meaningful bypass of corporate contributions. The estimated effects are positive and similar across specifications. In the more stringent specification, in Column (4), we estimate $\widehat{\beta}_3 = 0.54$, implying that, following the ban, the probability of previously large contributing firms making a large personal contribution increases by 5.4 percentage points. This is equivalent to an $\frac{.054}{.096} \approx 57\%$ increase from pre-ban levels.

The effect is driven by an increase in the frequency of large personal donations made by firms in the treatment group. This increased from 9.5% to 15.1% . In contrast, the frequency of large personal contributions of owners and board members of control firms barely changed from 1.3% to 1.5%. That is, the frequency of large personal contributions of corporate members grew by 5.6 percentage points in the treatment group and 0.2 in the control group, implying the effect of 5.4 percentage points in Column (1) in Table 3.

We describe results for two additional samples to show that the bypassing patterns are not sample-specific. First, Table 4 and Figure 8 show the results for the set of firms that survive between the 2010 and 2018 federal elections and belong to the fifty sectors that that donated the most before the ban. Similarly, Table 5 and Figure 9 show the results for the set of firms that survive between the 2008 and 2012 municipal elections and belong to the fifty sectors that donated the most before the ban.

Table 4: The effects of the 2015 corporate ban on the personal contributions of corporate members during federal elections using the sample of firms in the fifty sectors with more corporate contributions

	(1)	(2)	(3)	(4)
Contributing=1	0.094***	0.092***	0.000	0.000
	(0.008)	(0.010)	(.)	(.)
Contributing=1 × Post=1	0.066***	0.067***	0.067***	0.059***
	(0.013)	(0.014)	(0.014)	(0.014)
Year FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Sector-year FE	No	No	No	Yes
N	699,753	699,753	699,753	699,753
R^2	0.005	0.006	0.552	0.553
$\bar{Y}_{Post=0,T=0}$	0.010			
$\bar{Y}_{Post=0,T=1}$	0.104			
$\bar{Y}_{Post=1,T=0}$	0.010			
$\bar{Y}_{Post=1,T=1}$	0.170			

Dependent var: Indicator equal to 1 if personal donation above 75th percentile.

Treatment: having donated above the 75th percentile of corporate donations.

Firm-level cluster standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 8: The effects of the 2015 corporate ban on large (top quartile) personal contributions of board members of firms making large (top quartile) donations in federal elections, accounting for year, firm and sector-year fixed effects (Table 4, column 4).The sample of firms is restricted to the 50 sectors that, in aggregate, donated the most before the ban.

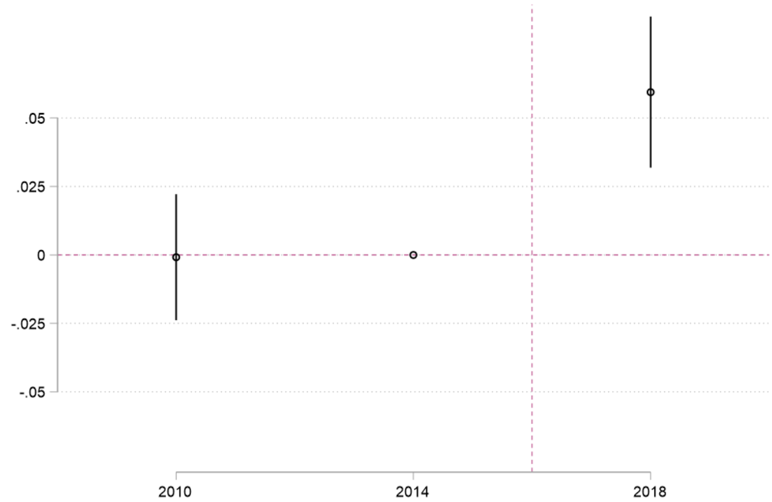


Table 5: The effects of the 2015 corporate ban on the personal contributions of corporate members during municipal elections using the sample of firms in the fifty sectors with more corporate contributions

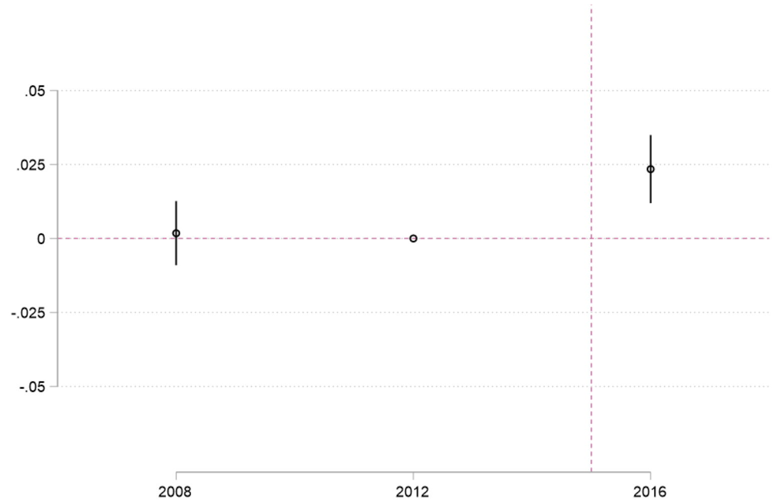
	(1)	(2)	(3)	(4)
Contributing=1	0.101***	0.099***	0.000	0.000
	(0.004)	(0.005)	(.)	(.)
Contributing=1 × Post=1	0.024***	0.026***	0.026***	0.023***
	(0.005)	(0.006)	(0.006)	(0.006)
Year FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Sector-year FE	No	No	No	Yes
N	863,331	863,331	863,331	863,331
R^2	0.009	0.009	0.484	0.484
$\bar{Y}_{Post=0,T=0}$	0.024			
$\bar{Y}_{Post=0,T=1}$	0.125			
$\bar{Y}_{Post=1,T=0}$	0.023			
$\bar{Y}_{Post=1,T=1}$	0.148			

Dependent var: Indicator equal to 1 if personal donation above 75th percentile.

Treatment: having donated above the 75th percentile of corporate donations.

Firm-level cluster standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 9: The effects of the 2015 corporate ban on large (top quartile) personal contributions of board members of firms making large (top quartile) donations in municipal elections, accounting for year, firm and sector-year fixed effects (Table 5, column 4).The sample of firms is restricted to the 50 sectors that, in aggregate, donated the most before the ban.



Results from the additional samples reinforce our findings of an economically meaningful bypass of corporate contributions. Figures 8 and 9 show that, in both samples, mean outcomes in control and treatment groups followed parallel trends before the 2015 corporate ban, suggesting a causal interpretation of the coefficients. Following the ban, the share of personal donations linked to firms that made large contributions to federal electoral campaigns increased by about 6 percentage points, about 63% of pre-ban levels (Table 4). Among firms that made large contributions to municipal electoral campaigns, the share increased by 2.3 percentage points, or about 18% of pre-ban levels (Table 5).

6 Conclusion

In this paper, we study the impact of the 2015 ban on corporate contributions in Brazil on the allocation of procurement contracts across firms and distribution of large personal contributions. We apply a difference in differences methodology comparing treatment and control outcomes before and after the ban on corporate contributions. The strength of our design relies on the fact that control and treatment outcomes follow (apparently) parallel trends before the reform.

We document three findings. First, before the ban, contributing firms won $20-25$ per cent more procurement tenders per year than non-contributing firms. Second, the ban on corporate contributions significantly decreased the number of national procurement contract won by contributing firms in 2 to 11 percentage points, a magnitude that only partially offset their previous advantage. Third, firms previously making large contributions circumvent the ban in both national and local elections with donations from owners and board members.

Together, our findings suggest that a ban on corporate contributions have limited effects on the average influence of contributing firms when there are mechanisms to circumvent the ban.

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