

# Speed of payment in procurement contracts: the role of political connections\*

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## Abstract

We provide evidence of a new channel through which politicians can exchange favors with campaign donors: earlier payment in procurement contracts. We explore an electoral reform that bans corporate contributions and partially breaks down the relationship between donors and politicians. Using a within-firm difference-in-differences identification strategy, we find that the payment period to firms that donate to the coalition government increases after the reform. The effect is larger in municipalities with low liquidity and for contracts allocated through competitive procurement methods. Our results point to the importance of designing rules that curb discretion over payment dates.

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# 1 Introduction

Governments have increasingly adopted procurement methods that foster competition.<sup>1</sup> Competitive procurement mechanisms, such as online auctions, are employed not only to improve efficiency but also to reduce the scope for corruption. Yet, the evidence of *quid pro quo* persists even in competitive auctions.<sup>2</sup> One possible explanation is that some firms receive preferential treatment *after* the tendering process, which gives them an advantage at the bidding stage. In this paper, I test for the existence of a new channel through which politicians can benefit firms that make campaign contributions: shortened payment periods. Exploring a ban on corporate donations, I document an increase in payment terms to campaign donors after the ban, and show that the effect is stronger in municipalities with liquidity problems and in contracts awarded through competitive auctions.

The notion of payment period, defined as the time between the delivery of a good and the payment, is closely related to the concept of trade credit, which describes loans in kind between transaction parts. In this setting, whenever a firm agrees on a contract with the government, it also agrees that it will be paid after the delivery, in effect granting a short-term loan to the government. A recent empirical literature shows that trade credit terms have important real consequences for firms.<sup>3</sup> The importance of payment terms in the context of public contracts has also been acknowledged by policymakers. In recent years, new regulations and initiatives were implemented in an attempt to shorten payment terms, especially to firms that are considered small and financially weaker.<sup>4</sup> In spite of both policymakers and academics regarding speed of payment as an important aspect of trade relationships, there is no study that measures this quantity at a granular level in firm-government relationships.

In Brazil, government agencies can only pay their suppliers once the object of the

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<sup>1</sup>Public procurement is a relevant activity. In 2015, among OECD countries, 29.1% of government expenditure was done through public procurement. In the same set of countries, government procurement amounts to 11.9% of the GDP. See [OECD \(2017\)](#).

<sup>2</sup>For instance, [Baltrunaite \(2017\)](#) shows that political connections distort the outcomes of auctions with multiple tenders.

<sup>3</sup>Stretched payment terms increase the length of time between the transfer of cash to suppliers and the receipt of cash from customers, increasing the working capital needs of firms. Changes in payment terms affect firms' liquidity, employment, trade relationships and probability of default, and effects are larger for firms that are more financially constrained. Changes in payment terms at the industry level affect decisions such as entry. See [Barrot \(2016\)](#), [Barrot and Nanda \(2020\)](#) and [Breza and Liberman \(2017\)](#).

<sup>4</sup>For instance, see the QuickPay initiative, launched in 2011 in the US, and the Regulation 113 of the Public Contracts Regulations, passed in 2015 in the UK.

contract is delivered and verified, that is, once the agency acknowledges that the supplier delivered the good or service according to specifications. Trade credit terms are in general homogeneous: in most cases, agencies must pay within 30 days following the verification. Nevertheless, governments have discretion over the actual timing of payment. The amount due is the same if payment occurs at the beginning or at the end of the 30-day period.<sup>5</sup> Late payments are common and suppliers are not properly compensated for delays.<sup>6</sup>

I investigate whether this flexibility in speed of payment is a source of favoritism. Using administrative data on the budget execution of the municipalities of the state of São Paulo, I am able to observe, at a granular level, amount purchased, payment dates and the dates when the government agencies acknowledge that the object of the contract was delivered (the verification date).<sup>7</sup> I compute payment periods as the time between the payment date and the verification date. Because there is some discretion in the verification of services and construction, I focus the analysis on simple products for which the verification date is a good proxy for the delivery date. I investigate whether political connections, measured by corporate political donations to the coalition government in municipal elections, are associated with speed of payment.

I explore a set of electoral reforms that change the relationship between donors and politicians. In 2015, corporate donations were banned and campaign spending limits were imposed. Firms donate during election campaigns, which take place every four years in Brazil. The electoral changes happened in the middle of the mayoral term, implying that firms that donate in the previous election are not able to donate again in the coming election.<sup>8</sup> In other words, firms can no longer commit to funding political campaigns with the same intensity as before. If politicians' incentives to grant favors to donors depend on the prospect of raising funds from them in future campaigns, the reforms should be associated with less favoritism.

Electoral reforms of this type are not exogenous. They usually follow corruption scandals and coincide with an increasing anti-corruption sentiment, which can add to

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<sup>5</sup>This type of trade credit contract is known as 'net terms' and differs from contracts known as 'two-part terms', in which the seller offers a discount if the payment occurs within a determined period.

<sup>6</sup>After the 30-day period, the amount due can be adjusted by inflation and a late payment interest rate. However, these adjustments are rare, and even when they are paid, they are smaller than the cost of capital for these firms and do not compensate for the liquidity risk they cause.

<sup>7</sup>São Paulo is the most populous state in Brazil, with a population of 45 million. It is also the largest state in terms of economic activity. Its GDP accounts for 34% of the national GDP.

<sup>8</sup>CEOs and board members can donate as individuals, but the donation is limited to 10% of their annual income. Moreover, spending limits were imposed, ruling out the possibility of complete substitution of corporate donations for individual donations.

effect of the reforms.<sup>9</sup> Moreover, in such situations, stakeholders can change the relationship with politically connected firms. For instance, a bank or a supplier does not want to be linked or exposed to a firm that could be charged for corruption and experience distress in the future. In turn, this fact can prompt politically connected firms to increase collection efforts. To account for this time-varying firm effect, I explore the fact that, in the last elections when donations were allowed, firms chose to donate in some municipalities but not in others, forming *local* connections. Thus, in municipalities where a firm donated in the previous election, the relationship with the local politician is shaken after the reforms and the increasing anti-corruption sentiment, while in municipalities where it did not donate the relationship is unchanged. This allows me to use the trajectory of the same firm in a different municipality as a counterfactual. More formally, I am able to control for firm-time fixed effects and implement a within-firm difference-in-differences strategy around the reforms.

I show that, after the reforms, payment periods to connected firms increase by five days. I perform an estimation of the monetary value of this effect. It ranges from 0.31% to 0.61% of the total amount purchased from connected firms. I run the same regression in the previous mayoral term, when there is no change in electoral rules, and show that the effect is not driven by features of the political cycle. I also test for the impact on the amount purchased. Effects are negative and large (decrease of 17%), but not statistically significant. A possible reason for the lack of statistical significance is that the amount purchased is not the ideal variable to test favoritism. Contracts can be awarded before the reform and executed over the next 12 months. Therefore, amount purchased is stickier than speed of payment.

Next, I investigate when this type of favoritism is more relevant. In cases in which the employment of a competitive tendering process is mandatory, the government's commitment to paying more quickly might constitute an important advantage. Favored firms can outbid firms that are otherwise similar, driving away non-favored firms from participating in and winning procurement auctions. Other cases in which the payment period can be relevant are when the firm is in financial distress or when the municipality is liquidity constrained. In the former case, the marginal value of cash can be very large. In the latter case, the municipality has to choose the subset of suppliers that will be paid on time or, more likely, with a smaller delay. Because firms are not fully compensated for payments that take place outside trade credit terms, late pay-

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<sup>9</sup>See, for instance, [Clark et al. \(2018\)](#). The effects of scandals and investigations can also attenuate effects because they change the behavior of firms before the reforms.

ments can also be interpreted as a haircut. Therefore, governments choose which firms bear the highest haircuts. I show that effects are large, around 12 days, in municipalities with lower liquidity. This evidence supports the idea that payment periods are more relevant when municipalities face liquidity constraints.<sup>10</sup> The effect is not statistically significant in municipalities with higher liquidity.

Finally, I study heterogeneity across competitive and non-competitive procurement methods. Competitive procurement methods refer to selection mechanisms that involve a tendering process, while non-competitive methods refer to direct contracting. Effects are large, 11 days, for competitive methods, while for non-competitive methods they are small and not statistically significant. The results suggest that payment periods are an important dimension of favoritism when governments find it less straightforward to benefit donors through the allocation of contracts.<sup>11</sup> Because I restrict the sample to products that have an “off-the-shelf” characteristic, the cost of rigging an auction is high. The effects for amount purchased are negative but not statistically significant for non-competitive procurement. In the baseline estimation, estimated in the previous mayoral term when electoral rules are unchanged, the effects for non-competitive procurement are positive. This result provides some evidence that the amount contracted might be the relevant channel of favoritism for non-competitive procurement.

This paper relates to a growing literature on trade credit.<sup>12</sup> [Barrot and Nanda \(2020\)](#) investigate the effects of the QuickPay reform in the US. The reform reduces from 30 to 15 days the time to pay to a subset of small firms.<sup>13</sup> They find that treated firms increase employment by 1.7%. In a country with more financial frictions, such as Brazil, where firms are credit rationed or pay higher interest rates, effects are likely larger. [Barrot \(2016\)](#) shows that stretched payment terms increase barriers to entry and expose firms to liquidity risk. These results, when applied to this setting, imply that more favorable payment terms to donors affect the ability of non-donors to compete. While the recent empirical literature on trade credit studies the effects of changes in trade credit terms,

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<sup>10</sup>Because liquidity is correlated with other (observable and unobservable) municipality characteristics, other mechanisms could drive the result. For instance, municipalities with lower liquidity can have weaker institutions and higher levels of corruption.

<sup>11</sup>Discretion in allocation contracts does not necessarily have adverse effects on procurement outcomes. See [Coviello et al. \(2018\)](#).

<sup>12</sup>See [Biais and Gollier \(1997\)](#), [Breza and Liberman \(2017\)](#), [Burkart and Ellingsen \(2004\)](#), [Costello \(2014\)](#), [Cuñat \(2006\)](#), [Fisman and Raturi \(2004\)](#), [Giannetti et al. \(2011\)](#), [Klapper et al. \(2011\)](#), [Murfin and Njoroge \(2014\)](#), [Ng et al. \(1999\)](#), and [Petersen and Rajan \(1997\)](#), among others. See also [Cuñat and Garcia-Appendini \(2012\)](#) for a review of the literature.

<sup>13</sup>The definition of small varies per industry. In terms of number of employees, the upper limit varies between 100 and 1500 employees.

this paper focuses on the determinants of the actual timing of payment in a context where trade credit terms are relatively homogeneous. Little is known about this variable, in particular how the nature of the relationship between the transaction parts affects it. I highlight, for public procurement, one aspect of the relationship – connections established through campaign donations – that affects the effective maturity of trade credit contracts. The fact that the effect is larger when the municipality (the buyer) has lower liquidity and therefore is delaying payments squares with the idea that suppliers are better equipped to extend trade credit if the relationship with the buyer is valuable and difficult to replace (Cuñat (2006)).

This paper builds on the literature that studies the effects of political connections. Researchers define connections in different ways. A strand of the literature studies campaign contributions. Connections established through campaign contributions have distinctive features. They are built through a cash donation and the expectation that the relationship will continue in the future. Because it involves a cash disbursement, researchers also investigate whether it is an investment in political capital or a reflection of agency problems.<sup>14</sup> Finally, donations are regulated and there is an ongoing debate about the optimal design of such laws. Another strand of the literature defines connections using different measures: CEOs and politicians have educational, professional or social ties, a large shareholder or officer is a member of the parliament or the executive, a former politician sits on the board of directors, among others. Researchers have uncovered multiple channels through which politicians can benefit connected firms. For instance, politically connected firms have more access to finance, win more government contracts, are more likely to be bailed out or to receive government funds, and are more able to circumvent regulations.<sup>15</sup> These benefits are usually associated with an increase in firm value and performance.<sup>16</sup> In the context of Brazil, Claessens et al. (2008) show that campaign contributions are associated with more bank financing, and Arvate et al. (2016) show that campaign contributions are linked to more government contracts.

This paper is related to Baltrunaite (2017), who explores a ban on corporate donations in Lithuania. She finds that donors' probability of winning contracts decreases after the reform. My results add to the literature in three ways. First, they provide

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<sup>14</sup>The evidence about whether donations are value-enhancing is mixed. See Aggarwal et al. (2012), Akey (2015), Cooper et al. (2010), and Fowler et al. (2017).

<sup>15</sup>See Khawaja and Mian (2005), Brogaard et al. (2016), Li et al. (2008), Schoenherr (2019), Goldman et al. (2013), Faccio et al. (2006), and Fisman and Wang (2015).

<sup>16</sup>See Cingano and Pinotti (2013), Acemoglu et al. (2016), Amore and Bennedsen (2013), Bunkanwanicha and Wiwattanakantang (2008), Fisman (2001), Faccio (2006), Ferguson and Voth (2008), Duchin and Sosyura (2012), Jayachandran (2006), Goldman et al. (2008), and Tahoun (2014).

an explanation for fact that *quid pro quo* is observed even when government agencies employ competitive auctions. Second, the measure of favoritism - speed of payment - is an objective quantity for which measurement is straightforward. It is often difficult to determine the precise economic value of favors that politicians grant to firms. Earlier payment is an advantage that can be easily translated into a monetary value. Finally, I highlight the necessity of employing within-firm estimations when trying to assess the impact of this type of reform.

## 2 Empirical setting

### 2.1 Budget execution

Three laws govern the budget process in Brazil: the Multi-Year Plan (*Plano Pluri-Anual*), the Budget Guidance Law (*Lei de Diretrizes Orçamentárias*) and the Annual Budget Law (*Lei Orçamentária Anual*). The executive branch proposes the laws, and the local legislature amends and approves them. While the Multi-Year Plan (*Plano Pluri-Anual*) covers a period of four years, the other two cover a period of one year. The Multi-Year Plan specifies long-term goals and investments, and projects that will be included in multiple annual budgets. It must be approved in the first year of the mayor's term. The Budget Guidance Law contains the rules that guide the elaboration and execution of the annual budget. It specifies programs that should be prioritized, rules to make budgetary adjustments if realized revenues are smaller than expected, and fiscal targets, including a target for the primary surplus. Once the legislature approves the Budget Guidance Law, the elaboration of the Annual Budget Law commences. The budget details the allocation of predicted revenues to each government agency and program. The budget is comprehensive, that is, an agency can only execute an expense if it is prescribed in the budget. However, the government does not have to execute every expense that is included in the budget. The expenses that have to be executed are called mandatory expenses, while the expenses that might not be executed are called discretionary expenses. When revenues turn out to be smaller than the predicted values used to elaborate the budget, the treasury secretary must limit the execution of discretionary expenses in order to meet fiscal targets.

When the fiscal year starts, the executive branch begins to execute the budget. The budget execution process can be roughly divided into four stages (see Figure 1):

- Authorization: agencies are authorized to commit resources according to the ap-

appropriations defined in the budget and realized revenues.

- **Commitment:** agencies reserve part of their appropriation to purchase the good or service from a previously selected supplier. The amount committed is deducted from the budget allocation.
- **Verification:** government formally acknowledges that the good or service was delivered according to specifications.
- **Payment:** cash is transferred to the supplier.

The length of time between the verification stage and the payment stage is a proxy for the effective maturity of the trade credit that suppliers extend to the government. Physical delivery can precede the verification date, especially for products whose verification is more complex and services that do not have a clear delivery date, such as construction.<sup>17</sup> I restrict the analysis to three classes of products to minimize measurement error.

I classify commitments into two types: ordinary and non-ordinary. The commitment is ordinary when there is only one verification and only one payment. This type of commitment is common in expenses whose value is certain. In this case, the commitment date can be a good proxy for the date of the order. A non-ordinary commitment can be followed by multiple stages of verification and payments. In this case, orders might occur after the commitment date. This type of commitment is employed in expenses whose value cannot be predetermined (for example energy bills, fuel) and in expenses whose value can be predetermined but that occur in instalments (for example, rent).

## 2.2 Public procurement

Government agencies can employ different methods to procure goods and services.<sup>18</sup> In certain cases the government can directly contract with a supplier, that is, tenders can be waived. This happens mainly in two cases: when competition is unfeasible (there is only one supplier) or when the purchase is small.<sup>19</sup> The regulation establishes a

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<sup>17</sup>There is an intermediate stage between the commitment and the verification stages that is known as “in verification”. The date of this stage would be the actual delivery date.

<sup>18</sup>Law 8.666 - the Public Procurement Law - contains most of the public procurement regulation.

<sup>19</sup>The regulation considers other cases, but they are less common. For instance, emergency situations and threats to national security.



threshold to define small. For products and services, the threshold is BRL 8,000, while for construction it is BRL 15,000.<sup>20</sup> I classify the cases without a tendering process as non-competitive procurement.

The other methods involve a tendering process. Invitation to tender and reverse auctions (regular and electronic) are examples. The method itself depends on the scope and value of the purchase (see figure A1 in the appendix for details). Even though these methods can differ in important dimensions, for instance conditions to participate, I classify them in a single group as competitive procurement. The use of electronic reverse auctions has increased over time, especially for simple products.<sup>21</sup>

Government agencies have to pay suppliers within 30 days following the acknowledgement that the object of the contract was delivered. When the purchase is small (same thresholds as for direct contracting), the limit is reduced to 5 days. Payments outside the limits are common. In such cases, the amount due can be adjusted by inflation and a late payment interest rate. However, these adjustments are rare and do not fully compensate firms for their losses and increased liquidity risk. Facing delays, suppliers can take the local government to court. However, in addition to having a cost, this procedure is unlikely to be effective. Courts are congested in Brazil and time in court can be long. Suppliers can also decide to terminate the contract, but this decision is only feasible if payment delays are longer than 90 days. The government is only considered to be in default in this case. Delays smaller than 90 days are not considered a contract breach.

### **2.3 Municipal elections and electoral reforms**

Municipal elections are held every four years in Brazil (see Figure 2 for a description of the electoral calendar). They are held simultaneously, usually in October, to elect the mayor and city councillors, who will serve a four-year term. Contenders that are elected take office on the 1st of January of the coming year. Mayors can run for re-election. However, after the second consecutive term in office, they are not allowed to run again. If they want an additional term, they have to wait until the next election. Members of the local council do not face a limit and can be re-elected indefinitely.

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<sup>20</sup>In 2018, the limits changed. Decree 9.412, 18/06/2018.

<sup>21</sup>Since 2005, the use of electronic reverse auctions to procure standard goods is mandatory for the federal government. Decree 5.450, 31/05/2005.

The council is elected in an open-list proportional representation system.<sup>22</sup> Mayors are elected by absolute majority. In municipalities where the number of voters is bigger than 200,000, there is a run-off if no candidate obtains more than 50% + 1 of the votes in the first round. Because of the large number of parties in Brazil, it is common for parties to form a coalition in elections. Among other benefits, coalitions increase the airtime of TV and radio ads, as they are free in Brazil and proportional to the number of seats that the parties of the coalition have in the federal congress.

Until 2015, campaigns were financed through private donations and public funds. Individuals and firms could donate to political parties or candidates. Firms could donate up to 2% of their total sales, while individuals could donate up to 10% of their annual income. If the individual is a candidate, there is no limit: they can donate as much as they want to their own campaign.

Since 2013, a large anti-corruption investigation revealed a widespread kickback scheme that involved the funding of parties with money obtained from government contracts. Several members of the business and political elite were convicted of corruption charges. Reacting to a growing unrest, institutions – the judiciary and the federal legislature – started to consider measures to deter corruption. In 2013, the Supreme Court began to discuss whether the rules that allowed campaign contributions were unconstitutional. The case was brought by the Brazilian Bar Association. In September 2015, with 8 votes in favour and 3 against, the Supreme Court declared corporate donations unconstitutional. The result was not unexpected: in the beginning of 2014 it was clear that the majority of judges would vote against corporate donations. However, it was not clear when they would finish the trial and, until then, judges could change their votes. Moreover, it was not clear in which elections the new rules would be put into effect. It is not uncommon for the Supreme Court to postpone the implementation of a new rule to allow agents to adapt.

Also in September 2015, the federal congress passed a law that changed political campaigns considerably.<sup>23</sup> Firstly, it established campaign limits. The limits were set at 70% of the maximum amount spent by a candidate in the previous campaign and then adjusted for the accumulated inflation between the last election and the coming election. However, the limit cannot be smaller than BRL 100,000, implying that it is only

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<sup>22</sup>Parties form local coalitions. The number of seats that are allocated to a coalition is calculated as a proportion to the total number of votes that it receives. If there are  $n$  seats available and the total number of votes is  $v$ , the coalition that receives  $v_c$  votes fills roughly  $v_c \times n/v$  seats. Within each coalition, the candidates that receive more votes win the seats.

<sup>23</sup>Law 13.165, 29/09/2015.

binding in larger municipalities where campaign costs are higher. Secondly, the law introduced changes to reduce campaign costs. For instance, it cut by half the duration of the campaigns, from 90 to 45 days. The rules regarding the donations of individuals (whether they are candidates or not) were not changed. Figure 3 summarizes the electoral changes.

## 2.4 Data and construction of variables

The State of São Paulo Court of Accounts (TCE-SP) provides data on the budget execution of the municipalities of the state of São Paulo, excluding the capital (644 municipalities). The TCE-SP provides detailed information on the stages (commitment, verification and payment) of the budget execution. Crucially, it provides the dates and monetary amounts of every commitment, verification and payment, and the identifier of the supplier. The data also contains the procurement method that the government employs to select the supplier and the budgetary classification of the expense. I select three types of expenses for which the verification date is a good proxy for the delivery date: consumption material, material for free distribution and equipment and permanent material. I construct two measures: time between commitment and verification and time between verification and payment. The latter quantity is what I call payment period. Measurement is straightforward for ordinary commitments. For non-ordinary commitments, I weight each operation by its monetary value (see Figure 4 for an illustration). The data contains accounting information that feeds fiscal and accounting reports. It lacks information on prices and quantities. It also lacks details of the tendering process (participants, bids, etc.).

The Superior Electoral Court (TSE) provides data on political campaign contributions and electoral results. I collect information for the 2004, 2008, 2012 and 2016 elections. For each firm, it is possible to observe to which party or candidate it donates and in which municipality.

On a yearly basis, the Ministry of Finance provides aggregate data on the financial situation of the municipalities, including balance sheet, revenue and expenditure.<sup>24</sup> I construct two measures to assess the liquidity and degree of budgetary rigidity of the municipalities (I provide more details on the construction of the variables in Appendix A.1). The liquidity measure is defined as the difference between cash and equivalents and a measure of accounts payable, divided by revenues. The higher this measure, the

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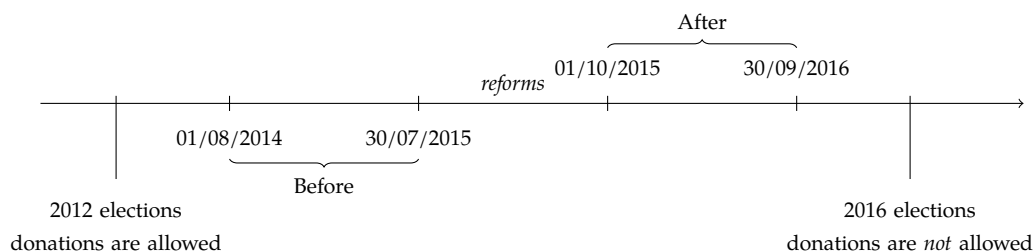
<sup>24</sup>Balance sheet information as of December 31. The fiscal year runs from January 1 to December 31.

more liquid the municipality. The other measure is defined as wage bill divided by revenues. Because it is virtually impossible to fire civil servants (with the exception of misconduct) and wage cuts are not allowed, the higher this measure, the higher the proportion of the budget that cannot be adjusted in case of shocks to the revenue.

The Brazilian Institute of Geography and Statistics (IBGE) provides municipality characteristics, such as geographical area, GDP, literacy rate and population. The Ministry of Labor provides data on the number of employees at the establishment level.

## 2.5 Empirical strategy

I first divide the sample into two periods, one year before and one year after the electoral changes, and then I collapse the data at the firm-municipality level<sup>25</sup>:



I implement the following regression specification:

$$\Delta y_{fm} = \alpha + \beta \mathbb{1}_{fm} + \alpha_f + \alpha_m + \epsilon_{fm} \quad (1)$$

where  $\mathbb{1}_{fm}$  is a dummy variable that takes value 1 if firm  $f$  is connected in municipality  $m$ , that is, if it donates to any party of the coalition government in the 2012 elections.<sup>26</sup> The variable  $\Delta y_{fm}$  measures changes in three variables: time between commitment and verification, time between verification and payment, and amount purchased. Because the same firm can have contracts in more than one municipality, I can control for time-varying changes in firm's characteristics by including firm fixed effects in specification (1). Because the dependent variable measures changes before and after the reform, firm dummies control for firm-time fixed effects.<sup>27</sup> This is a key advantage of this set-

<sup>25</sup>I collapse the data using the monetary value of the operations as weights. I exclude observations whose commitment date is in August or September, because for most of them, payment takes place close to or after the reform.

<sup>26</sup>I also report results for a stronger form of connection: donation to incumbent's party in the previous election.

<sup>27</sup>Throughout the paper, when I refer to firm fixed effects, I am actually referring to firm-time fixed effects.

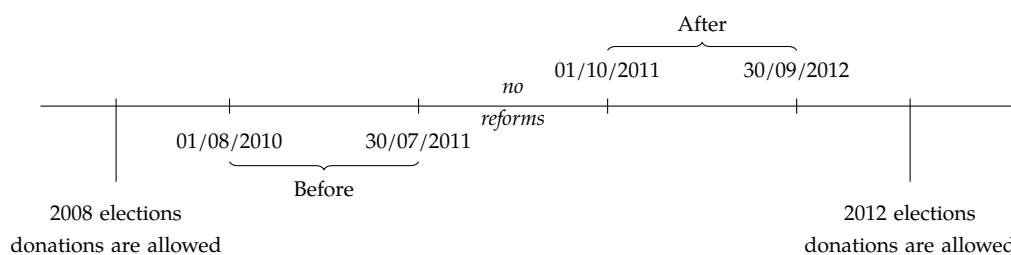
ting. Because electoral reforms of this type are endogenous, they can coincide with changes in other variables that impact firms that have relationship with politicians. As a result, donors can follow different trends than non-donors after the reforms for reasons that are not directly related to the electoral changes. By including firm fixed effects, I am able to control for these changes at the firm level, and pin down the effects of the electoral changes. I also control for time-varying municipality changes by including municipality fixed effects.

I analyze the data at the firm-municipality level to increase the sample size, as I need to observe contracts of the same firm in a given municipality before and after the reforms. However, this approach does not take into account the fact that firms could select themselves into different procurement methods after the reform. Moreover, it is not possible to study the heterogeneity of the effect across procurement methods. Therefore, in a second specification, I collapse the data at the firm-municipality-procurement method level and estimate the following specification:

$$\Delta y_{fmj} = \alpha + \beta \mathbf{1}_{fm} + \alpha_f + \alpha_m + \epsilon_{fmj} \quad (2)$$

where  $j$  denotes procurement method. In this approach, I implicitly control for procurement method fixed effects (not time-varying).

To check whether results are driven by the political cycle, I run a baseline regression around the same period in the previous mayoral term (four years before).



In the baseline regressions, a firm  $f$  is connected in municipality  $m$  if it donates to any party of the coalition government in the 2008 elections. The crucial difference is that firms can donate in the coming elections; there is no law that partially breaks down the relationship between donors and politicians.

### 3 Descriptive statistics

First I collapse the data at the municipality-year level using as weights the amount committed.<sup>28</sup> Table 1 presents means, standard deviations and medians of the main variables, across municipalities, from 2008 to 2017. From 2008 to 2014, the mean of time between verification and payment oscillates between 16.8 and 20.3 days.<sup>29</sup> In the end of 2014, the country entered into a recession that lasted until 2016. With the economic downturn, time between verification and payment jumps to 24.8 days in 2015, and it reaches 27.1 days in 2016.<sup>30</sup> The recession has a large impact on the liquidity of the municipalities. The mean of the liquidity measure drops from 13% in 2008 to 3% in 2016. The budgetary rigidity measure constantly increases throughout the sample period. However, the impact of the recession on this measure is small. In Figure 5, I split the sample into two using the median of the liquidity measure. Municipalities with more liquidity pay between 4 and 6 days earlier. In 2015 and 2016, the difference increases to almost 8 days. The same procedure applied to the budgetary rigidity measure shows that it also correlates (negatively) with speed of payment. However, the magnitude of the difference is smaller (maximum of 2 days). In Table A1 of the appendix, I show that liquidity and rigidity correlate with observable municipality characteristics. For example, more liquidity is associated with larger population, larger GDP per capita and higher literacy rate. The opposite is true for rigidity. In Figure A2 of the appendix, I show that the standard deviation of time between verification and payment is higher in municipalities with lower liquidity. Moreover, it also spikes in 2015 and 2016. The fact that there is more dispersion in payment terms when liquidity is low suggests that the type of favoritism studied in this paper might be more relevant in such case. More budgetary rigidity is also associated with higher standard deviation, but the difference is smaller.

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<sup>28</sup>If, in a given year, a municipality  $m$  has  $N_m$  commitments indexed by  $c = 1, \dots, N_m$  with amount committed and time between verification and payment (or time between commitment and verification) given by  $C_c$  and  $t_c$ , respectively, then time between verification and payment of municipality  $m$ ,  $t_m$ , is given by

$$t_m = \frac{\sum_{c=1}^{N_m} C_m \times t_m}{\sum_{c=1}^{N_m} C_m}$$

<sup>29</sup>At first glance, it is puzzling that municipalities pay in less than 30 days. However, in equilibrium, municipalities that consistently pay earlier should be able to acquire goods at a lower price.

<sup>30</sup>It is hard to obtain this information for other countries. Survey information, collected by Intrum Justitia for more than 10,000 firms across Europe, shows that payment periods in government contracts can be much larger – higher than 90 days – in countries such as Portugal, Italy and Greece. In many other countries, the magnitudes are similar to those I find in Brazil. See [Intrum Justitia \(2017\)](#).

Next, I collapse the data at the firm-municipality-year-product-procurement method level.<sup>31</sup> To describe the data, I classify firms into four groups. A firm is considered *connected (before)* at a given municipality when it donates to any party of the coalition government in the previous elections, or *connected (after)* if it donates to any party of the coalition government in the coming election. Because the donations were banned in 2015, the classification of *connection (after)* is unfeasible from 2013 on (see Table A2 in the appendix for details). Throughout the paper, unless explicitly stated, when I refer to a connected firm it means that it is *connected (before)*. The *connection (after)* definition captures the importance of future donations for the relationship between donors and non-donors. I divide non-connected firms into two groups. A non-connected firm is a *donor* if it donates to any political party in any of the three elections in which donations were allowed (2004, 2008 and 2012), and a *non-donor* otherwise. Table 2 provides summary statistics across the three groups. Donors and connected firms have larger contracts. The difference is larger when I use the *connected (after)* classification. The large difference in contract size could be explained by the fact that firms that donate are larger, more efficient or simply share an agenda the incumbent. The allocation of contracts to firms that have a close relationship with incumbents can also be an efficient solution to issues like moral hazard or adverse selection. Alternatively, donors could have larger contracts because they donate in the previous election and commit to donating in the next election. The time between commitment and verification is larger for donors and connected firms. The larger size of the contracts could explain this difference if we interpret this measure as the time between order and delivery. *Connected (before)* firms are paid on average 17 days after the verification stage. This number is slightly smaller than the average for donors (19.3 days) and for non-connected firms (18.3 days). *Connected (after)* firms are paid 15.7 days after the verification.

In Table 3, I present descriptive statistics for competitive and non-competitive procurement. The time between verification and payment is larger for competitive procurement. Consistent with the legislation, which says that one of the uses of non-competitive procurement is to purchase small amounts, the average amount committed is much larger for competitive procurement. Finally, the time between commitment

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<sup>31</sup>If a firm  $f$  at municipality  $m$ , year  $y$ , product  $p$ , procurement method  $j$ , has  $N_{fmyppj}$  commitments indexed by  $c = 1, \dots, N_{fmyppj}$ , each with value  $C_{fmyppjc}$  and time to pay (or time to deliver)  $t_{fmyppjc}$ , then

$$t_{fmyppj} = \frac{\sum_{c=1}^{N_{fmyppj}} C_{fmyppjc} \times t_{fmyppjc}}{\sum_{c=1}^{N_{fmyppj}} C_{fmyppjc}}$$

and verification is larger for competitive procurement, possibly reflecting the size of the orders. Figure 6 shows that the share of amount committed through competitive procurement has increased over time. It increased from 55% in 2008 to almost 80% in 2014. This reflects the efforts of the executive branch and controlling agencies, such as state courts of accounts, to increase the adoption of competitive methods, especially electronic reverse auctions, in the purchase of off-the-shelf products.

Table 4 provides descriptive statistics for the decision to donate. I restrict the sample to firms that have contracts in 2008 and 2012 (election years). Panel A explores firm characteristics for the firms that donate and firms that do not donate. Firms that donate are larger in terms of number of employees and have larger contracts. Panel B explores the locality of the donation. Firms have more workers and larger contracts in the municipality where they donate. The municipalities where they donate are more populous and have a higher GDP per capita, population density and literacy rate. However, they are similar in terms of liquidity and budgetary rigidity. They are also similar in terms of electoral competitiveness, which I measure by the difference in the share of votes between winner and runner-up. Panel C explores the choice of the party that receives a contribution. Incumbents and winners receive larger donations, and the firms that donate to them have larger contracts. The parties that receive donations obtain a higher percentage of votes.

Even though the sample has a large number of observations, the number of observations from firms classified as connected is small. Table A3 in the appendix presents the number of connected observations. Since I use firm-time fixed effects, I also report the number of firms that have contracts in more than one municipality. The small number of connected firms that have contracts in other municipalities limits the use of some empirical strategies, for instance the restriction of the sample to municipalities with close elections. In Table A4, I split the number of observations marked as connected into two groups: competitive and non-competitive procurement. In general, competitive procurement represents less than 25% of the number of observations. However, despite the smaller number, they account for the majority of the amount committed and the share increases over time, reaching more than 80% in 2016.

### **3.1 Time series variation of partial correlations**

In an attempt to understand the impact of connections throughout the political cycles and before and after the reforms, I run the following cross-sectional regressions



(per year):

$$y_{fmpj} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fmpj} \quad (3)$$

where  $\mathbb{1}_{fm}$  is a dummy variable that takes the value 1 if firm  $f$  is connected in municipality  $m$ , that is, if it donates to any party of the coalition government in the previous elections when current incumbents were elected. The subscripts  $p$  and  $j$  denote product and procurement method, respectively. Because connections are formed endogenously, we cannot interpret the magnitude and sign of the coefficients: they should be interpreted as partial correlations. The idea is to study the evolution of the correlations over time.

Table 5 provides the time-series variation of  $\beta$ 's. Without controls, the correlation between time between verification and payment and connection is negative and, in most years, statistically significant. I then include firm fixed effects to control for unobservable firm characteristics, such as the ability to produce goods and to collect payments from clients. I also include municipality fixed effects to control for omitted variables such as the ability of the municipality to pay on time. When I add these fixed effects, the magnitude decreases and in most years there is no statistical significance. The exception is the year 2016, when coefficients are larger (4.4 days) and statistically significant. Because the electoral changes take place at the end of 2015, this is suggestive evidence that they could be one of the reasons behind this change. The year 2016 is the final year of the mayor's term and also an election year. I do not observe the same effect in years that share those characteristics, 2008 and 2012. The inclusion of product and procurement method fixed effects does not change the results. In one specification, I include firm-party fixed effects to compare the same firm in municipalities governed by the same party. This inclusion controls for a common agenda between incumbents and donors. Despite the loss of power, the pattern remains the same: in 2016 the coefficient becomes positive and statistically significant.

I also report results for amount purchased. Without controls, the correlation between amount contracted and connection is positive and statistically significant. Once I include firm and municipality fixed effects, the magnitudes decrease but remain statistically significant in most years. From 2014 on, magnitudes start to decrease and lose statistical significance.

Table A5 in the appendix reports results for time between commitment and verification. Without controls, coefficients are in general positive and statistically significant, possibly reflecting the larger size of the orders. However, once fixed effects are in-

cluded, coefficients are not statistically significant and there is no clear change in 2016. Table A6 reports results for the *connection (after)* measure until 2012. The magnitudes of the correlation are larger for this measure.

### 3.2 Parallel trends

Before proceeding to the main results, I estimate the following panel regression:

$$y_{t f m p j} = \sum_{t=2009}^{2017} \beta_t \mathbb{1}_{f m t} \times \mathbb{1}_t + \alpha_{f t} + \alpha_{m t} + \alpha_{f m} + \alpha_p + \alpha_j + \epsilon_{t f m p j}$$

where 2008 is the baseline year (the category excluded from the interaction  $\mathbb{1}_{f m t} \times \mathbb{1}_t$ ),  $y_{t f m p j}$  denotes time between verification and payment of firm  $f$ , in municipality  $m$ , through procurement method  $j$ , in year  $t$ , of product  $p$ . The variable  $\mathbb{1}_{f m t}$  takes value 1 if firm  $f$  is connected in municipality  $m$  in year  $t$ , and zero otherwise. The variable  $\mathbb{1}_t$  takes value 1 when the year is  $t$ . A firm is classified as connected if it donates to any party of the coalition government in the previous elections.  $\alpha_{f t}$  denotes firm-year,  $\alpha_{m t}$  municipality-year,  $\alpha_{f m}$  firm-municipality,  $\alpha_p$  product, and  $\alpha_j$  procurement method fixed effects. Standard errors are clustered at the firm-year and municipality-year levels.

Figure 7 shows the results. From 2009 to 2015, the year of the reforms, the  $\beta$ 's are small in magnitude and statistically indistinguishable from zero. This pattern changes in 2016, when the reform took effect: the magnitude of the coefficient increases to 4 days and becomes statistically significant.

## 4 Results

### 4.1 Time between verification and payment

Table 6 reports the coefficients of Equation 1 with the firm-municipality aggregation. Connected firms are paid 5 days later once I include firm and municipality fixed effects.<sup>32</sup> Effects increase to 11.7 days when I restrict the sample to municipalities with low liquidity, where low is defined as below the 2015 median. In this specification, I

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<sup>32</sup>The estimates without firm fixed effects are smaller. One possible reason is that reforms coincide with a smaller provision of bank or trade credit to campaign donors. This reduction increases the marginal value of cash for these firms and thus they make large efforts to collect payments from clients. A regression that does not account for this time-varying effort underestimate the results.

compare the same firm, before and after the reforms, in municipalities with low liquidity where it is connected versus in municipalities with low liquidity where it is not connected. The reason for this cut is that arguably payment periods are an important dimension when municipalities experience a cash shortage. In such cases, governments have to choose which firms are paid in accordance with the trade credit terms or which firms face smaller delays. If liquidity shocks to the municipality coincide with liquidity shocks to the firm, this type of favoritism is even more relevant as it takes place when the marginal value of cash is high. The favor, in this case, would have an insurance characteristic. Stretched payment periods would be less of a problem if governments have enough cash to pay every supplier on time. Indeed, for municipalities with high liquidity, effects are not statistically significant. However, because liquidity is an endogenous variable that correlates with observable and unobservable characteristics that could also affect favoritism, we cannot conclude that it is the only driver of the results.

Table 6 also provides the estimates for the baseline estimation. I run the same regressions around the same point of the previous mayoral term, when electoral rules are unchanged. Estimates are not significant, providing evidence that the effects are not driven by characteristics of the political cycle. When I restrict the sample to municipalities with low liquidity, where low is also defined as below the 2015 median, the effect is negative, albeit not statistically significant.<sup>33</sup> The effect for municipalities with high liquidity is not statistically significant and similar in magnitude to the effects obtained around the reforms.

In table 7, I show how the size of the donation affects the magnitude of the estimates. The effect is only present for firms whose donation is above the median. In this case, the effects increases to 8.5 days. For firms whose donation is below the median the effect, is not statistically significant.

Next, I estimate Equation 2, in which I implicitly control for procurement method fixed-effects. Table 8 provides the results. In the specification with firm and municipality fixed effects, estimates are smaller, 3.4 days and only significant at the 10% level. Effects are larger when I only consider competitive procurement, 11.4 days. In this specification, I compare the payment patterns of contracts awarded in competitive auc-

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<sup>33</sup>I still use the 2015 median in the baseline regressions because in 2011 the country was in a different point of the business cycle. GDP growth was 7.5% in 2010 an 4% in 2011, while it was 0.5% in 2014 and -3.6% in 2015. Therefore, the median of the liquidity measure in 2011 is considerably higher than the median of 2015. If I use the 2011 median as a cutoff, I could classify as illiquid a municipality that in fact has high liquidity.

tions by the same firm, before and after the reforms, in municipalities where it is connected versus in municipalities where it is not connected. In the baseline estimations, effects are negative and not statistically significant. For non-competitive procurement, effects estimated around the reform and in the previous mayoral term are not statistically significant and similar in magnitude. The results suggest that speed of payment is an important way of distorting public procurement when it is more difficult to simply award contracts to connected firms.

Tables A7 and A8 in the appendix provide the estimates of equations 1 and 2 for a stricter measure of connection. I define a firm as connected in a given municipality if it donates to the incumbent's party in the previous election. Results are similar and estimates are in general slightly larger. For instance, the main effect increases from 5 to 5.9 days. Only for competitive procurement the magnitude of the effect decreases to 7.7 days. However, it remains statistically significant at the 1% level.

Breza and Liberman (2017) show that buyers use trade credit to assess the quality of the products.<sup>34</sup> Therefore, there is a concern that the effect is driven by the fact that, after the breakdown of the relationship, governments have to spend more time assessing the quality of products delivered by connected firms. If this is the case, the effect is driven by the fact that there is more uncertainty about product quality, and not because donors no longer receive favors in terms of payment terms. However, this is not a major concern in this setting because I measure payment period as the time between verification and payment. In the verification stage, the government acknowledges that the object of the contract was delivered accordingly. I return to this point when I discuss time between commitment and verification.

## 4.2 Other outcome variables

For non-competitive procurement methods, the allocation of a contract is arguably the first-order channel through which politicians can favor connected firms. The breakdown of the relationship between donors and politicians would be followed by a smaller amount committed. In competitive procurement, the government's commitment to pay earlier enables connected firms to outbid non-connected firms that are otherwise similar. Therefore, amount committed and payment periods are jointly determined and an increase in payment periods would also be followed by a decrease in amount com-

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<sup>34</sup>The idea that delayed payments can be used to mitigate concerns about product quality dates back to Smith (1987), Lee and Stowe (1993) and Long et al. (1993).

mitted. I estimate Equations 1 and 2 for changes in log of amount committed. The estimation of the effect for amount committed is hindered by the fact this variable is stickier than the payment period variable. The length of the contracts can be as large as one year, especially for large amounts, and commitments after the reform could refer to contracts awarded before the reform. Tables 9 and 10 present the results. In general, estimates are negative – decrease in growth of amount committed – but not statistically significant. For the regressions that use the entire sample, the estimates are larger (in absolute values) than the baseline estimates. Consistent with the idea that the amount committed is the relevant channel in non-competitive procurement, around the reform the estimate is negative, -18%, while the baseline estimate is positive, 12%. Effects are not statistically significant though. For competitive procurement the decrease around the reform is smaller in magnitude than the baseline estimates. This result is in contrast to the idea that larger payment periods result in fewer contracts. However, to test this hypothesis properly, I would have to look at changes in the probabilities of winning new contracts.

Even though I restrict the sample to simple products that have a clear delivery date and are easy to verify, there is still the possibility that connected firms are benefited through a more timely verification. Alternatively, because of the continuing nature of the relationship between donors and politicians, issues like adverse selection are not present, and governments can spend less time assessing the quality of the products delivered by connected firms. I test this hypothesis using time between commitment and verification as an outcome variable. The results should be interpreted with caution because the reforms can affect characteristics of the orders that impact the outcome variable. For example, an increase in time between commitment and verification for connected firms can be offset by the fact that orders are smaller (and thus the supplier can deliver more quickly) after the reform. Tables 11 and 12 show that estimates are slightly larger around the reform in comparison to baseline estimates. However, in both cases estimates are not statistically significant.

### 4.3 Discussion

What is the economic significance of the results? The largest estimate is 12 days. [Barrot and Nanda \(2020\)](#) find that a 15-day reduction in payment periods causes an

increase of 1.7% in employment.<sup>35</sup> In Brazil, because financial frictions are higher, effects could be larger. However, in monetary amounts, effects are not large. I compute the monetary benefit to connected firms using the interest rate on loans that have trade bills as collateral.<sup>36</sup> Figure 8 plots the time series of the average interest rates on these loans. Throughout the sample period, the average annual interest rate is 33.5%, the maximum is 44.9%, and the minimum is 21.4%. The total amount committed to connected firms in 2014, before the electoral reforms, is BRL 103 million (in 2017 BRL). It represents 1.68% of the total amount committed on the three classes of products of the sample, BRL 6,132 million. Using the estimate of the effect from Equation 1, 5 days, and the minimum and maximum interest rates, the monetary benefit for connected firms ranges from BRL 0.32 million to BRL 0.63 million.<sup>37</sup> As a percentage of the amount committed to connected firms, it ranges from 0.31% to 0.61%. In terms of the total amount committed, the effects range from 0.005% to 0.01%. In comparison to the amount that connected firms donate, effects are also small. Connected firms donate BRL 7.75 million to parties of the coalition governments in the 2012 elections. If we include the donations to parties that are not part of the coalition, the amount is BRL 10.63 million. Even the largest (annual) estimate, BRL 0.63 million, received over the entire mayoral term, 4 years, would be smaller than the amount that firms donate.

The benefits do not seem to be large. However, because firm owners can still donate as a natural person (or even illegally), the breakdown of the relationship is only partial. It is difficult to assess to which extent the relationship was broken, but we can interpret the magnitudes as a lower bound of the effect in the case of a complete breakdown. I also show cases in which the effects are more relevant, competitive procurement and illiquid municipalities. Moreover, in this paper, I focus on simple products. The firms that sell this kind of products likely operate at low margins. Thus, the effect, as percentage of the margin, can be quite significant. Finally, possibly because it is more difficult to distort procurement of these goods, few firms actually donate. Only 21% of the donations in the 2012 elections come from firms that are in the sample and have contracts over the entire mayoral term (from 2013 to 2016). The bulk of donations come from firms from other sectors, mainly construction. A possible reason is that it is easier to

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<sup>35</sup>Barrot and Nanda (2020) compute this estimate in the context of the 2007-2008 financial crisis. The same estimation at another point of the business cycle, when firms are less constrained, would likely yield a smaller magnitude.

<sup>36</sup>For a firm that has excess cash and does not need to borrow to finance its production process, the opportunity cost of capital would be the short-term rate that they obtain on their investments.

<sup>37</sup>I perform the calculations with 4 working days.

rig auctions for construction services. The selection of the supplier is not only based on price in these cases, but also on technical capability. However, favoritism through payment periods could still be important. The reason is as follows: because it is more difficult to verify the object of the contract and there is no clear delivery date, there is one extra dimension to favour firms through payment period: the verification stage. By postponing the certification that the object of the contract was executed according to specifications, agencies can delay payment. The discretion over the verification and payment stages enables a larger benefit through payment terms. The same argument is valid for services.

#### **4.4 Causal interpretation**

We estimate the regressions around a reform that bans corporate donations and changes electoral rules. However, as pointed out throughout the paper, this type of reform is not exogenous. It is accompanied by a public outcry over corruption practices, a large anti-corruption investigation and other electoral reforms. Therefore, it is difficult to claim that the results are caused only by the ban on donations. This limitation precludes policy recommendations regarding corporate donations. Instead, I focus the interpretation of the results on the partial breakdown of the relationship between firms and politicians. From a policy perspective, the message that regulators should pay attention to discretion over payment periods remains valid, especially when the government is having liquidity problems.

## **5 Conclusion**

This paper provides evidence that payment periods to campaign donors change after an electoral reform that bans corporate political contributions. The firms that donate in the previous elections can no longer commit to donating in the coming elections, partially breaking down the relationship between them and politicians. The changes are more pronounced in municipalities with lower liquidity and in contracts awarded through competitive procurement methods. The results draw attention to a new channel through which politicians can distort public procurement even when the use of competitive auctions is mandatory. Preferential treatment in terms of payment speed might affect the ability of non-connected firms to win contracts, especially if these firms are financially constrained. The findings help to explain the fact that donors are more

likely to win competitive auctions.<sup>38</sup> The paper also sheds light on the informal relational contract between politicians and donors. In particular, it highlights the fact that the prospect of receiving future donations is a key incentive for politicians to grant favors. From a policy perspective, the results call for rules that curb discretion over payment periods and properly compensate firms for late payments. As an example, in 2016, the Brazilian federal government established that payments should be settled on a first-come first-served basis and government agencies should publicize the order of creditors based on the date of the verification.<sup>39</sup>

The results also stress the importance of using within-firm estimates to assess the impacts of electoral reforms. This type of reform is particularly endogenous and likely correlates with changes in other variables that affect firms that have close relationships with politicians. As a result, the trajectory of non-donors is not a good counterfactual for the trajectory of donors. A difference-in-differences estimation that does not account for time-varying shocks at the firm level would provide biased results. I explore the fact that the same firm has relationships of different intensity with local politicians across municipalities. Therefore, the reforms affect the relationship in some municipalities but not in others. This heterogeneity allows me to include firm-time fixed effects and provide more credible estimates. This inclusion guarantees that the results are driven by the shock to the relationship with politicians and not by changes in other variables that coincide with the reforms and affect differently donors versus non-donors.

The paper also contributes to the understanding of determinants of actual payment periods in trade credit contracts and how the nature of the relationship between transaction parts can affect them. The literature on trade credit usually studies trade credit terms, which differ from the effective time to pay. I study this measure in the context of contracts between firms and the government and show that campaign contributions (or connections more broadly) are an important determinant of this variable. Studies of the same measure in contracts between private firms could shed light on important elements of their relationship.

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<sup>38</sup>In this paper I focus on one type of preferential treatment after the bidding stage that increases the competitiveness of donors. However, there are other possible explanations. Politicians can commit to smaller execution costs (less paperwork, etc.). In cases in which there is uncertainty about execution costs, as in infrastructure projects, renegotiations are common and politicians can commit to renegotiating at better terms.

<sup>39</sup>See Normative Instruction 2, 06/12/2016 (<http://pesquisa.in.gov.br/imprensa/jsp/visualiza/index.jsp?data=08/12/2016&jornal=1&pagina=87>).



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# Figures

Figure 1: **Budget execution**

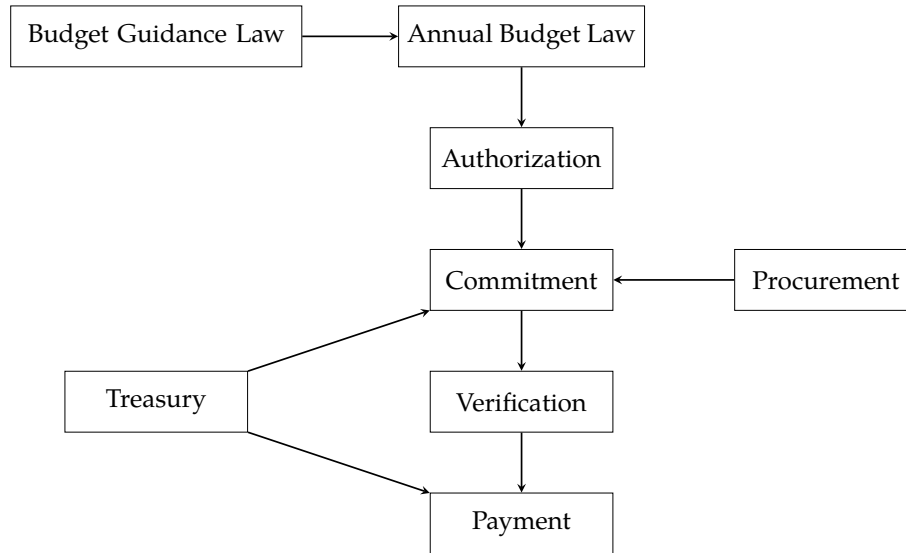


Figure 2: **Electoral calendar**

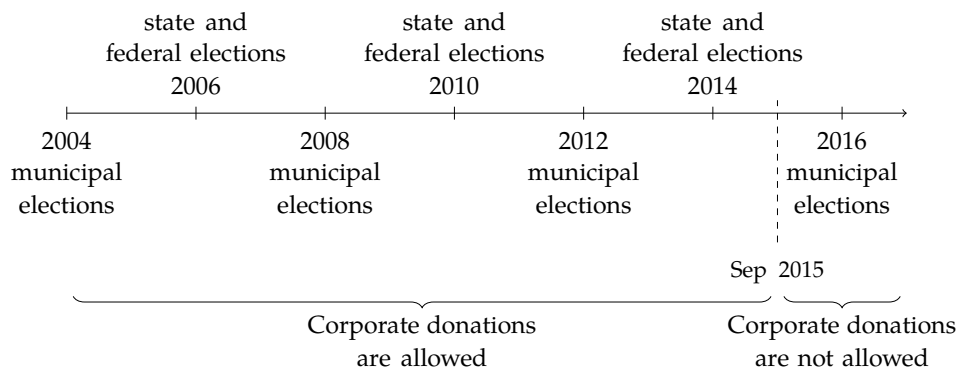
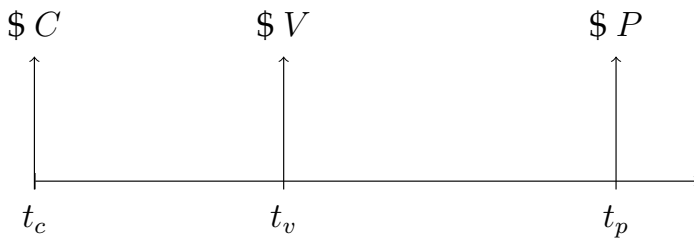


Figure 3: Electoral reform

	Before	After
Campaign duration	90 days	45 days
Campaign spending limits - Mayor	No limit	First round: up to 70% of the mostly expensive campaign in the previous election, in the case of a one-round previous election; or 50% in the case of a two-round previous election Second round: 30% of the most expensive campaign in the previous election. The limit cannot be smaller than R\$ 100,000.
Campaign spending limits - members of local parliaments	No limit	Up to 70% of the mostly expensive campaign in the previous election The limit cannot be smaller than R\$ 10,000.
Donation - legal person	Firms could donate up to 2% of their total sales.	Unlawful
Donation - natural person	Individuals could donate up to 10% of their annual income.	Unchanged
Donation - candidates	No limit, as long as the total campaign cost comply with the limits.	Unchanged

Figure 4: Ordinary and non-ordinary commitment

Ordinary commitment

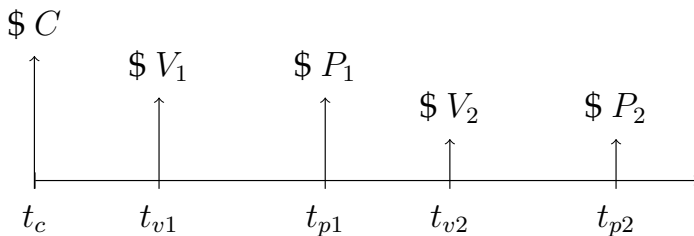


$$C=V=P$$

$$\text{Time to deliver} = t_v - t_c$$

$$\text{Time to pay} = t_p - t_v$$

Non-ordinary commitment

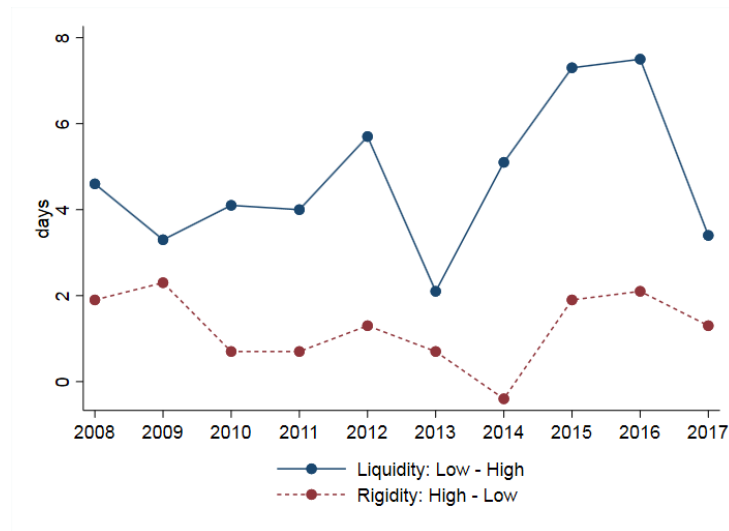


$$C=V_1 + V_2=P_1 + P_2$$

$$\text{Time to deliver} = \frac{t_{v1} \times V_1 + t_{v2} \times V_2 - t_c \times C}{C}$$

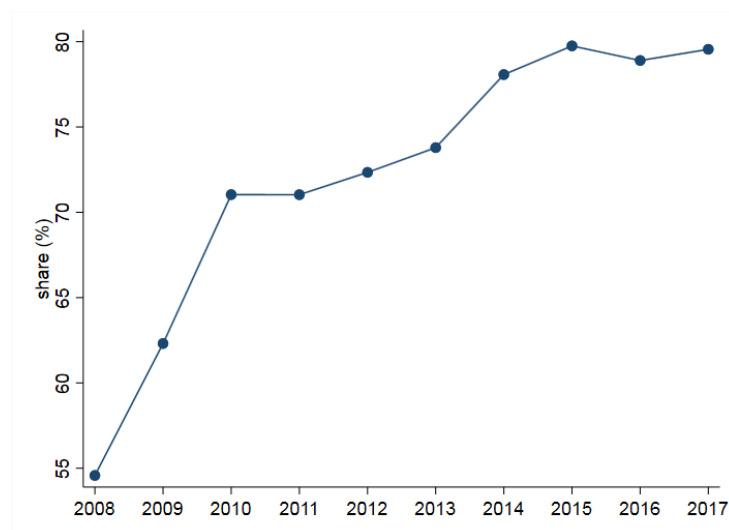
$$\text{Time to pay} = \frac{t_{p1} \times P_1 + t_{p2} \times P_2 - t_{v1} \times V_1 - t_{v2} \times V_2}{C}$$

Figure 5: Mean of time between verification and payment: relationship with (lagged) fiscal variables



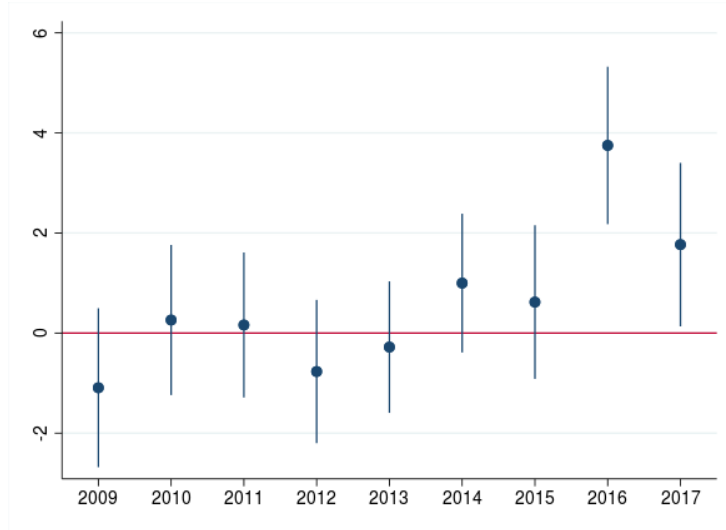
Notes: The data is aggregated at the municipality level using monetary values as weights. For each year, I split the sample into two groups using the medians of the liquidity measure. I repeat the procedure for the budgetary rigidity measure. I then compute the mean of time between verification and payment for each group. I compute liquidity as follows:  $(\text{cash} - \text{accounts payable}) / \text{revenues}$ . The budgetary rigidity measure is defined as  $\text{wage bill} / \text{revenues}$ . The construction of both measures is explained in detail in the appendix A.1. In 2013 there is a reclassification of accounting variables, which might affect the values of fiscal variables.

Figure 6: Share of amount committed through competitive procurement



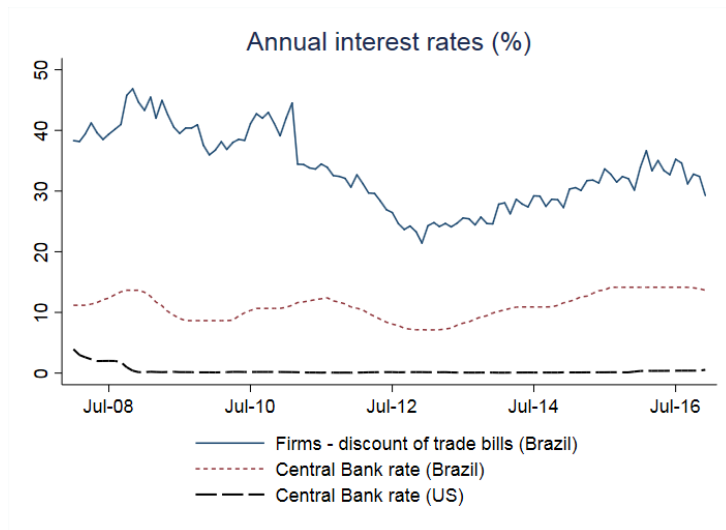
Notes: A procurement method is non-competitive if there is no tendering process (direct contracting). Otherwise, if there is any sort of tendering process, the procurement method is classified as competitive.

Figure 7: Parallel trends, time between verification and payment



Notes: The figure plots the  $\beta_i$ 's and the respective 5% confidence intervals estimated from the following regression:  $y_{t f m p j} = \sum_{i=2009}^{2017} \beta_i \mathbb{1}_{f m t} + \alpha_{f t} + \alpha_{m t} + \alpha_{f m} + \alpha_p + \alpha_j + \epsilon_{t f m p j}$ . The sample starts in 2008, which is the baseline year in the regression.  $y_{t f m p j}$  denotes time between verification and payment of firm  $f$ , in municipality  $m$ , through procurement method  $j$ , in year  $t$ , of product  $p$ . The variable  $\mathbb{1}_{f m t}$  takes value 1 if firm  $f$  is connected in municipality  $m$  in year  $t$ , and zero otherwise. A firm is classified as connected if it donates to any party of the coalition government in the previous elections. Standard errors are clustered at the firm-time and municipality-time levels.

Figure 8: Interest rates: Brazil and US



Notes: Annual (nominal) Central Bank interest rates. It also presents the average interest rate of new operations that involve the discount of trade bills. The Brazilian Central bank describes these operations as the “advance of funds to non-financial corporations based on future cash flows linked to trade bills or other receivables, except checks and credit card bills”. The average is weighted by the value of the operations.



## Tables

Table 1: Descriptive statistics - municipality variation

Commitment year	GDP growth	Time between commitment and verification			Time between verification and payment			Liquidity			Budgetary rigidity		
		mean	sd	med.	mean	sd	med.	mean	sd	med.	mean	sd	med.
2008	5.1%	37.2	19.0	37.9	16.8	9.2	15.6	0.13	0.18	0.08	0.44	0.06	0.44
2009	-0.1%	35.5	17.8	36.2	16.9	9.6	15.8	0.12	0.19	0.09	0.46	0.08	0.46
2010	7.5%	38.1	18.9	38.5	18.5	10.4	17.0	0.14	0.20	0.08	0.45	0.07	0.46
2011	4.0%	37.1	19.6	37.2	17.8	9.4	16.6	0.13	0.19	0.08	0.46	0.07	0.46
2012	1.9%	38.1	20.5	39.0	20.3	11.1	19.3	0.10	0.20	0.05	0.48	0.07	0.48
2013	3.0%	35.6	18.8	36.5	17.0	8.3	16.3	0.10	0.18	0.06	0.48	0.07	0.49
2014	0.5%	38.5	20.0	37.9	18.7	10.0	17.7	0.08	0.16	0.05	0.48	0.08	0.49
2015	-3.6%	33.5	19.2	31.9	24.8	12.1	23.6	0.05	0.17	0.03	0.49	0.07	0.49
2016	-3.5%	33.5	18.3	32.6	27.1	13.3	25.5	0.03	0.15	0.03	0.49	0.07	0.49
2017	1.0%	31.7	17.5	29.5	21.8	9.7	20.81	0.06	0.17	0.04	0.50	0.07	0.50

*Notes:* The data is aggregated at the municipality level using monetary values as weights. I compute means, standard deviations and medians across municipalities. I calculate liquidity as follows: (cash - accounts payable) / revenues. The budgetary rigidity measure is defined as wage bill / revenues. The construction of both measures is explained in detail in the appendix A.1. In 2013 there is a reclassification of accounting variables, which might affect the values of fiscal variables.

Table 2: Descriptive statistics

		Connected (before)			Connected (after)		
		Non-connected			Non-connected		
		Non-donor	Donor	Connected	Non-donor	Donor	Connected
Time between verification and payment	mean	18.3	19.3	17.0	17.4	18.6	15.7
	sd	18.3	18.8	18.3	17.4	18.2	15.7
	p75	26.1	27.3	24.0	25.5	27.0	22.1
	p50	14.0	15.0	11.7	13.4	14.7	11.5
	p25	5.8	6.0	4.7	5.0	5.8	5.0
Time between commitment and verification	mean	22.8	24.8	25.1	22.5	25.5	26.1
	sd	26.5	28.4	28.6	26.4	29.1	28.6
	p75	31.0	34.0	33.1	30.8	35.0	34.8
	p50	14.7	16.0	16.0	14.3	16.8	17.6
	p25	4.3	5.0	6.0	4.1	5.0	7.0
Amount committed (in 2017 BRL)	mean	17316	43036	46503	17150	41626	59671
	sd	135950	302369	224192	133381	268318	329909
	p75	8042	12839	13423	8897	13904	17915
	p50	2327	3633	3142	2476	4052	4345
	p25	618	926	666	652	1018	866
N		2372390	250802	16834	1136944	131283	10506

Notes: The data is aggregated at the firm-municipality-product-year-procurement method level. The sample comprises observations from 2008 to 2017. *Connected (before)* means that the firm donates to any part of the coalition government in the previous election. More specifically, a firm is *connected (before)* in 2008 if it donates to the coalition government in the 2004 elections; a firm is *connected (before)* in 2009, 2010, 2011 and 2012 if it donates to the coalition government in the 2008 elections; and a firm is *connected (before)* in 2013, 2014, 2015 and 2016 if it donates to the coalition government in the 2012 elections. Because donations are not allowed in the 2016 elections, a firm is *connected (before)* in 2017 if it donates to the coalition government in the 2012 elections. *Connected (after)* means that the firm donates to any party of the coalition government in the next election. Since the last election in which donations were allowed occurred in 2012, the sample contains observations from 2008 to 2012. More specifically, a firm is *connected (after)* in 2008 if it donates to the coalition government in the 2008 elections; and a firm is *connected (after)* in 2009, 2010, 2011 and 2012 if it donates to the coalition government in the 2012 elections. Monetary amounts are in 2017 values. I use the consumer price index (Índice Nacional de Preços ao Consumidor Amplo, IPCA) to adjust the values.

**Table 3: Descriptive statistics: competitive and non-competitive procurement**

Commitment year	Time between verification and payment		Amount committed (in 2017 BRL)		Time between commitment and verification	
	Non-compet.	Compet.	Non-compet.	Compet.	Non-compet.	Compet.
2008	16.0	17.6	10167	67737	17.8	41.8
2009	16.6	17.8	7952	66009	17.5	41.9
2010	18.0	18.8	7381	75670	18.0	44.0
2011	17.1	18.6	7675	70538	18.5	43.2
2012	18.6	19.8	7684	71440	18.8	44.3
2013	16.7	17.0	6824	68818	18.6	41.5
2014	17.9	18.8	6214	67854	18.4	41.9
2015	19.5	23.3	5474	58455	17.4	37.8
2016	20.7	25.3	5954	56828	17.2	38.7
2017	17.5	20.9	4965	51050	16.2	35.9

*Notes:* The data is aggregated at the firm-municipality-product-year-procurement method level. A procurement method is non-competitive if it does not involve any tendering process (direct contracting). Otherwise, if there is any sort of tendering process, the procurement method is classified as competitive. Monetary amounts are in 2017 values. I use the consumer price index (Índice Nacional de Preços ao Consumidor Amplo, IPCA) to adjust the values.

Table 4: Descriptive statistics - donations

	Mean			Median	
	Non-donor	Donor	P-value	Non-donor	Donor
<i>Panel A: Non-donor vs donor. Population: suppliers in 2008 or 2012.</i>					
Number of workers	32.7	177.8	0	3	7
Amount committed (in 2012 BRL)	49013	270359	0	4707	11578
Observations	135259	3944			
<i>Panel B: Locality of the donation. Population: donor-municipality pairs with a contract in in 2008 or 2012. The supplier must be a donor in at least one municipality.</i>					
Share number of workers	1.54	79.65	0	0	100
Amount committed (in 2012 BRL)	63092	109682	0	7413	8926
Population	88851	141906	0	32824	59183
Population density	420.2	683.4	0	78.1	142.6
Literacy rate	98.8	98.9	0	98.9	99.0
GDP per capita	22809	23527	0.08	18405	19238
Ratio (cash-accounts payable)/ revenues	0.13	0.13	0.42	0.09	0.08
Ration wage bill / revenues	0.45	0.45	0.15	0.45	0.45
Average margin of election winner	25.9	25.7	0.79	16.2	16.4
Observations	7816	2405			
<i>Panel C: Party selection. Population: supplier-municipality pairs with a contract in 2008 or 2012. The supplier must be a donor in the municipality of the pair.</i>					
<i>Panel C.1: Donation to incumbent's party?</i>					
Value donation	5786	7950	0.04	853	1746
Amount committed	66557	199554	0	7670	15338
Observations	2194	896			
<i>Panel C.2: Donation to the party of the next incumbent (winner)?</i>					
Value donation	5052	9775	0	900	1500
Amount committed	92000	137506	0.02	8384	11025
Observations	2199	891			
<i>Panel C.3: Donation to a party that has a candidate in the mayoral election?</i>					
Value donation	2272	9054	0	573	1570
Amount committed	59424	134255	0	7241	10336
Observations	1203	1887			
<i>Panel C.4: Party received donation?</i>					
Percentage of votes	31.6%	38.5%	0	32.0%	39.8%
Observations	2955	924			

Notes: Panel A compares, among the suppliers in 2008 and 2012, firms that donate and firms that do not donate. In Panel B, I restrict the sample to municipality-firm pairs in which: (i) the firm donates in at least one municipality and (ii) the firm has contracts in the municipality. Then I compare municipality characteristics. Monetary amounts are in 2012 values. I use the consumer price index (Índice Nacional de Preços ao Consumidor Amplo, IPCA) to adjust the values.

Table 5: **Partial correlations: cross-sectional regressions**

Commitment Year	Election year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Time between verification and payment				Log (amount committed)			
2008	✓	0.3	0.6	0.5	0.7	0.61***	0.19	0.18*	0.27*
2009		-1.9***	-0.9	-0.8	-0.4	0.45***	0.19**	0.23***	0.21
2010		-0.9**	0.3	0.4	-1.3	0.43***	0.04	0.11	-0.12
2011		-0.0	0.9	1.0	-1.7	0.37***	0.14*	0.21***	0.12
2012	✓	-1.8***	-0.4	-0.4	1.1	0.25***	0.13	0.22**	0.08
2013		-1.5***	0.8	0.8	1.0	0.28***	0.16*	0.22**	0.38***
2014		-1.4***	1.8*	1.8*	2.3	0.27***	0.06	0.12	0.19
2015		-3.1***	0.5	0.5	1.4	0.16***	0.08	0.14*	0.16
2016	✓	-0.9	4.4***	4.4***	5.6**	0.20***	0.06	0.08	0.11
2017		-1.0**	3.4***	3.4***	4.9**	0.07	-0.03	-0.00	-0.01
Firm FE			✓	✓			✓	✓	
Munic. FE			✓	✓	✓		✓	✓	✓
Prod. FE				✓	✓			✓	✓
Procur. FE				✓	✓			✓	✓
Firm-party FE					✓				✓
Cluster Firm & mun			✓	✓	✓		✓	✓	✓

Notes: The data is aggregated at the firm-municipality-product-year-procurement method level. The table presents  $\beta$ 's of the following regression specification (estimated per commitment year):  $y_{fmpj} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fmpj}$ . The dummy  $\mathbb{1}_{fm}$  takes the value 1 if firm  $f$  is connected at municipality  $m$ , that is, if the firm donates to the any party of the coalition government in the previous election. More specifically, a firm is connected in 2008 if it donates to the coalition government in the 2004 elections; a firm is connected in 2009, 2010, 2011 and 2012 if it donates to the coalition government in the 2008 elections; and a firm is connected in 2013, 2014, 2015 and 2016 if it donates to the coalition government in the 2012 elections. Because donations are not allowed in the 2016 elections, a firm is connected in 2017 if it donates to the coalition government in the 2012 elections.

Table 6: **Difference-in-differences: time between verification and payment**

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
<i>Panel A: all sample</i>						
Connected	2.1** (1.0)	1.6 (1.0)	5.0** (2.2)	-1.1 (0.8)	-1.5* (0.8)	1.0 (1.8)
Observations	81,593	81,593	53,686	81,212	81,212	52,421
R-squared	0.000	0.123	0.309	0.000	0.111	0.318
<i>Panel B: Low liquidity</i>						
Connected	3.9** (1.7)	4.0** (1.6)	11.7*** (4.4)	-5.1*** (1.6)	-5.3*** (1.7)	-2.5 (4.0)
Observations	39,486	39,486	22,288	24,319	24,319	11,296
R-squared	0.000	0.119	0.339	0.000	0.112	0.410
<i>Panel C: High liquidity</i>						
Connected	0.7 (1.2)	-0.3 (1.1)	2.7 (1.7)	0.9 (0.8)	0.5 (0.7)	1.9 (2.1)
Observations	38,357	38,357	21,636	56,777	56,777	34,310
R-squared	0.00	0.13	0.37	0.00	0.11	0.33
Firm FE			✓			✓
Mun. FE		✓	✓		✓	✓

*Notes:* In columns 1-3, I divide the sample into two periods, one year before and one year after the electoral changes, and then I collapse the data at the firm-municipality level. In columns 4-6, I repeat the same procedure in the previous mayoral term, when there is no change in the electoral rules. Regressions take the form  $\Delta y_{fm} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fm}$ , where  $\Delta y_{fm}$  denotes changes in time between verification and payment of firm  $f$  in municipality  $m$ . The variable  $\mathbb{1}_{fm}$  takes value 1 if firm  $f$  is connected in municipality  $m$ , and zero otherwise. In columns 1-3, a firm is classified as connected if it donates to any party of the coalition government in the 2012 elections. In columns 4-6, a firm is classified as connected if it donates to any party of the coalition government in the 2008 elections. In panel A, I include the entire sample. In Panel B, I restrict the sample to municipalities whose liquidity is below the median of the liquidity measure as of December 2015. In Panel C, I restrict the sample to municipalities whose liquidity is above the median of the liquidity measure as of December 2015. Standard errors are clustered at the firm and municipality levels.

Table 7: **Difference-in-differences: time between verification and payment  
High donation versus Low donation**

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
Connected * High donantion	3.8*** (1.4)	3.9*** (1.3)	8.5*** (2.5)	-0.5 (1.1)	-1.0 (1.2)	3.2 (2.5)
Connected * Low donation	0.4 (1.4)	-0.7 (1.3)	-1.6 (3.5)	-1.8* (1.1)	-2.0** (0.9)	-2.3 (2.1)
Observations	81,593	81,593	53,686	81,212	81,212	52,421
R-squared	0.00	0.12	0.31	0.000	0.111	0.318
Firm FE			✓			✓
Mun. FE		✓	✓		✓	✓

Standard errors clustered at firm & municipality

*Notes:* In columns 1-3, I divide the sample into two periods, one year before and one year after the electoral changes, and then I collapse the data at the firm-municipality level. In columns 4-6, I repeat the same procedure in the previous mayoral term, when there is no change in the electoral rules. Regressions take the form  $\Delta y_{fm} = \alpha + \beta_h \mathbb{1}_{fm} \times \mathbb{1}_{high} + \beta_l \mathbb{1}_{fm} \times \mathbb{1}_{low} + controls + \epsilon_{fm}$ , where  $\Delta y_{fm}$  denotes changes in time between verification and payment of firm  $f$  in municipality  $m$ . The variable  $\mathbb{1}_{fm}$  takes value 1 if firm  $f$  is connected in municipality  $m$ , and zero otherwise. In columns 1-3, a firm is classified as connected if it donates to any party of the coalition government in the 2012 elections. In columns 4-6, a firm is classified as connected if it donates to any party of the coalition government in the 2008 elections. The dummy  $\mathbb{1}_{high}$  (high donation) takes value 1 if the donation of firm  $f$  in municipality  $m$  is above the median of the variable amount donated. The dummy  $\mathbb{1}_{low}$  (low donation) takes value 1 if the donation of firm  $f$  in municipality  $m$  is below the median of the variable amount donated. Standard errors are clustered at the firm and municipality levels.

Table 8: **Difference-in-differences: time between verification and payment**  
*(firm-municipality-procurement method regressions)*

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
<i>Panel A: all sample</i>						
Connected	1.1 (0.9)	0.8 (0.8)	3.4* (2.1)	-1.4** (0.7)	-1.7** (0.8)	-0.6 (1.7)
Observations	84,586	84,586	60,379	84,955	84,955	60,301
R-squared	0.000	0.121	0.321	0.000	0.114	0.332
<i>Panel B: Competitive</i>						
Connected	2.1 (1.8)	1.5 (1.8)	11.4*** (3.6)	-1.2 (1.5)	-1.7 (1.7)	-2.5 (3.9)
Observations	23,505	23,502	18,576	18,921	18,918	14,478
R-squared	0.000	0.203	0.326	0.000	0.185	0.352
<i>Panel C: Non-competitive</i>						
Connected	0.7 (1.1)	0.2 (0.9)	0.3 (2.2)	-1.4* (0.8)	-1.8** (0.9)	0.4 (1.8)
Observations	61,081	61,081	36,504	66,034	66,034	40,582
R-squared	0.00	0.11	0.35	0.000	0.111	0.359
Firm FE			✓			✓
Mun. FE		✓	✓		✓	✓

*Notes:* In columns 1-3, I divide the sample into two periods, one year before and one year after the electoral changes, and then I collapse the data at the firm-municipality-procurement method level. In columns 4-6, I repeat the same procedure in the previous mayoral term, when there is no change in the electoral rules. Regressions take the form  $\Delta y_{f_m j} = \alpha + \beta \mathbb{1}_{f_m} + controls + \epsilon_{f_m j}$ , where  $\Delta y_{f_m j}$  denotes changes in time between verification and payment of firm  $f$ , in municipality  $m$ , through procurement method  $j$ . The variable  $\mathbb{1}_{f_m}$  takes value 1 if firm  $f$  is connected in municipality  $m$ , and zero otherwise. In columns 1-3, a firm is classified as connected if it donates to any party of the coalition government in the 2012 elections. In columns 4-6, a firm is classified as connected if it donates to any party of the coalition government in the 2008 elections. In panel A, I include the entire sample. In Panel B, I restrict the sample to competitive procurement methods. In Panel C, I restrict the sample non-competitive procurement methods. Standard errors are clustered at the firm and municipality levels.



Table 9: Difference-in-differences: amount committed

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
<i>Panel A: all sample</i>						
Connected	-0.08 (0.07)	-0.08 (0.06)	-0.17 (0.11)	-0.13* (0.07)	-0.10 (0.06)	-0.10 (0.12)
Observations	81,593	81,593	53,686	81,212	81,212	52,421
R-squared	0.000	0.038	0.254	0.000	0.024	0.255
<i>Panel B: Low liquidity</i>						
Connected	-0.08 (0.10)	-0.07 (0.09)	-0.02 (0.21)	-0.24** (0.11)	-0.24*** (0.08)	-0.41** (0.20)
Observations	39,486	39,486	22,288	24,319	24,319	11,296
R-squared	0.000	0.039	0.281	0.000	0.031	0.335
<i>Panel C: High liquidity</i>						
Connected	-0.10 (0.10)	-0.11 (0.09)	-0.25 (0.17)	-0.07 (0.08)	-0.03 (0.09)	0.03 (0.16)
Observations	38,357	38,357	21,636	56,777	56,777	34,310
R-squared	0.00	0.03	0.28	0.00	0.02	0.27
Firm FE			✓			✓
Mun. FE		✓	✓		✓	✓

*Notes:* In columns 1-3, I divide the sample into two periods, one year before and one year after the electoral changes, and then I collapse the data at the firm-municipality level. In columns 4-6, I repeat the same procedure in the previous mayoral term, when there is no change in the electoral rules. Regressions take the form  $\Delta y_{fm} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fm}$ , where  $\Delta y_{fm}$  denotes changes in log of amount committed of firm  $f$  in municipality  $m$ . The variable  $\mathbb{1}_{fm}$  takes value 1 if firm  $f$  is connected in municipality  $m$ , and zero otherwise. In columns 1-3, a firm is classified as connected if it donates to any party of the coalition government in the 2012 elections. In columns 4-6, a firm is classified as connected if it donates to any party of the coalition government in the 2008 elections. In panel A, I include the entire sample. In Panel B, I restrict the sample to municipalities whose liquidity is below the median of the liquidity measure as of December 2015. In Panel C, I restrict the sample to municipalities whose liquidity is above the median of the liquidity measure as of December 2015. Standard errors are clustered at the firm and municipality levels.

Table 10: **Difference-in-differences: amount committed**  
*(firm-municipality-procurement method regressions)*

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
<i>Panel A: all sample</i>						
Connected	-0.07 (0.06)	-0.07 (0.06)	-0.14 (0.11)	-0.10* (0.06)	-0.06 (0.06)	0.07 (0.10)
Observations	84,586	84,586	60,379	84,955	84,955	60,301
R-squared	0.000	0.039	0.251	0.000	0.028	0.257
<i>Panel B: Competitive</i>						
Connected	-0.02 (0.12)	-0.03 (0.12)	-0.03 (0.20)	0.04 (0.13)	0.04 (0.12)	-0.20 (0.26)
Observations	23,505	23,502	18,576	18,921	18,918	14,478
R-squared	0.000	0.072	0.232	0.000	0.042	0.229
<i>Panel C: Non-competitive</i>						
Connected	-0.09 (0.07)	-0.09 (0.07)	-0.18 (0.15)	-0.15** (0.07)	-0.10 (0.07)	0.12 (0.11)
Observations	61,081	61,081	36,504	66,034	66,034	40,582
R-squared	0.00	0.05	0.31	0.000	0.038	0.311
Firm FE			✓			✓
Mun. FE		✓	✓		✓	✓

*Notes:* In columns 1-3, I divide the sample into two periods, one year before and one year after the electoral changes, and then I collapse the data at the firm-municipality-procurement method level. In columns 4-6, I repeat the same procedure in the previous mayoral term, when there is no change in the electoral rules. Regressions take the form  $\Delta y_{f m j} = \alpha + \beta \mathbb{1}_{f m} + controls + \epsilon_{f m j}$ , where  $\Delta y_{f m j}$  denotes changes in log of amount committed of firm  $f$ , in municipality  $m$ , through procurement method  $j$ . The variable  $\mathbb{1}_{f m}$  takes value 1 if firm  $f$  is connected in municipality  $m$ , and zero otherwise. In columns 1-3, a firm is classified as connected if it donates to any party of the coalition government in the 2012 elections. In columns 4-6, a firm is classified as connected if it donates to any party of the coalition government in the 2008 elections. In panel A, I include the entire sample. In Panel B, I restrict the sample to competitive procurement methods. In Panel C, I restrict the sample non-competitive procurement methods. Standard errors are clustered at the firm and municipality levels.

Table 11: **Difference-in-differences: time between commitment and verification**

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
<i>Panel A: all sample</i>						
Connected	-2.2** (1.1)	-1.8 (1.1)	1.3 (2.2)	-0.2 (1.2)	0.2 (1.4)	-2.4 (2.3)
Observations	81,593	81,593	53,686	81,212	81,212	52,421
R-squared	0.00	0.05	0.25	0.00	0.04	0.23
<i>Panel B: Low liquidity</i>						
Connected	-1.4 (1.7)	-1.2 (1.5)	4.3 (3.8)	1.0 (2.0)	1.8 (2.4)	2.1 (6.8)
Observations	39,486	39,486	22,288	24,319	24,319	11,296
R-squared	0.00	0.06	0.29	0.00	0.05	0.31
<i>Panel C: High liquidity</i>						
Connected	-3.1* (1.6)	-2.6 (1.6)	-2.1 (3.2)	-1.0 (1.5)	-0.7 (1.8)	-4.8* (2.9)
Observations	38,357	38,357	21,636	56,777	56,777	34,310
R-squared	0.00	0.03	0.27	0.00	0.03	0.24
Firm FE			✓			✓
Mun. FE		✓	✓		✓	✓

*Notes:* In columns 1-3, I divide the sample into two periods, one year before and one year after the electoral changes, and then I collapse the data at the firm-municipality level. In columns 4-6, I repeat the same procedure in the previous mayoral term, when there is no change in the electoral rules. Regressions take the form  $\Delta y_{fm} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fm}$ , where  $\Delta y_{fm}$  denotes changes in time between commitment and verification of firm  $f$  in municipality  $m$ . The variable  $\mathbb{1}_{fm}$  takes value 1 if firm  $f$  is connected in municipality  $m$ , and zero otherwise. In columns 1-3, a firm is classified as connected if it donates to any party of the coalition government in the 2012 elections. In columns 4-6, a firm is classified as connected if it donates to any party of the coalition government in the 2008 elections. In panel A, I include the entire sample. In Panel B, I restrict the sample to municipalities whose liquidity is below the median of the liquidity measure as of December 2015. In Panel C, I restrict the sample to municipalities whose liquidity is above the median of the liquidity measure as of December 2015. Standard errors are clustered at the firm and municipality levels.

Table 12: **Difference-in-differences: time between commitment and verification**  
*(firm-municipality-procurement method regressions)*

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
<i>Panel A: all sample</i>						
Connected	-1.9*	-1.6	1.0	0.2	0.2	-0.7
	(1.0)	(1.0)	(2.0)	(1.0)	(1.2)	(2.1)
Observations	84,586	84,586	60,379	84,955	84,955	60,301
R-squared	0.00	0.05	0.26	0.00	0.04	0.25
<i>Panel B: Competitive</i>						
Connected	-2.4	-2.7	4.9	1.4	1.2	3.2
	(2.4)	(2.6)	(5.0)	(3.0)	(3.5)	(6.0)
Observations	23,505	23,502	18,576	18,921	18,918	14,478
R-squared	0.00	0.09	0.26	0.00	0.08	0.25
<i>Panel C: Non-competitive</i>						
Connected	-1.6	-1.2	0.9	-0.2	-0.0	-1.7
	(1.0)	(0.9)	(2.0)	(1.0)	(1.1)	(1.8)
Observations	61,081	61,081	36,504	66,034	66,034	40,582
R-squared	0.00	0.07	0.33	0.00	0.05	0.33
Firm FE			✓			✓
Mun. FE		✓	✓		✓	✓

*Notes:* In columns 1-3, I divide the sample into two periods, one year before and one year after the electoral changes, and then I collapse the data at the firm-municipality-procurement method level. In columns 4-6, I repeat the same procedure in the previous mayoral term, when there is no change in the electoral rules. Regressions take the form  $\Delta y_{f_m j} = \alpha + \beta \mathbb{1}_{f_m} + controls + \epsilon_{f_m j}$ , where  $\Delta y_{f_m j}$  denotes changes in time between commitment verification of firm  $f$ , in municipality  $m$ , through procurement method  $j$ . The variable  $\mathbb{1}_{f_m}$  takes value 1 if firm  $f$  is connected in municipality  $m$ , and zero otherwise. In columns 1-3, a firm is classified as connected if it donates to any party of the coalition government in the 2012 elections. In columns 4-6, a firm is classified as connected if it donates to any party of the coalition government in the 2008 elections. In panel A, I include the entire sample. In Panel B, I restrict the sample to competitive procurement methods. In Panel C, I restrict the sample non-competitive procurement methods. Standard errors are clustered at the firm and municipality levels.

## A Appendix

### A.1 Data sources, sample selection and variables

*Data sources.* The budget execution data is from the São Paulo Court of Accounts (TCE-SP) and can be downloaded at <http://transparencia.tce.sp.gov.br>. The electoral data (election results and campaign contributions) is available at the website of the The Superior Electoral Court (TSE), <http://www.tse.jus.br/>. The Ministry of Finance provides data on the balance sheet, revenues and expenses of municipalities (available at <https://www.tesouro.fazenda.gov.br/contas-anuais>). Finally, municipality characteristics (population, geographical area, literacy rate, GDP) are available at the website of the Brazilian Institute of Geography and Statistics (IBGE), <https://www.ibge.gov.br/>.

*Sample selection.* The budget execution data is available from 2008 on and it includes all municipal expenses (salaries, pensions, interest payments, machines, equipment, food, office material, construction, consultancy services, IT services, etc.). There are 92 classes of expenses. I select three classes that involve contracts with private suppliers and for which the verification date is arguably a good proxy for the delivery date: consumption material (current expenditure, class 30); material for free distribution (current expenditure, class 32); and equipment and permanent material (capital expenditure, class 52). The variable time between verification and payment is winsorized at the 99% level. The TCE-SP only aggregates the data. The municipalities collect and treat the information and send to the TCE-SP on a yearly basis. Therefore, the quality of the data varies across municipalities. To avoid using poorly constructed data-sets, I exclude municipality-year pairs where more than 80% of commitments are verified on the same day of the commitment, or paid on the same day of the verification. When this happens it suggests that the dates of the the budget execution stages were incorrectly recorded. The data only includes commitments that are fully executed (committed, verified and paid) within the fiscal year. Commitments that are executed in a different fiscal year are not available (this includes commitments that are verified but not paid, and commitments that are not verified).

*Fiscal variables.* There is a change in the accounting reports in 2013. Therefore, I present the variables definitions for two periods, from 2007 to 2013 and from 2014 to 2017. The definitions are such that the variables are as comparable as possible in the two periods given the information available. From 2007 to 2013, I define *cash* as the sum of cash, plus deposits in banks plus short-term financial applications (“caixa + bancos

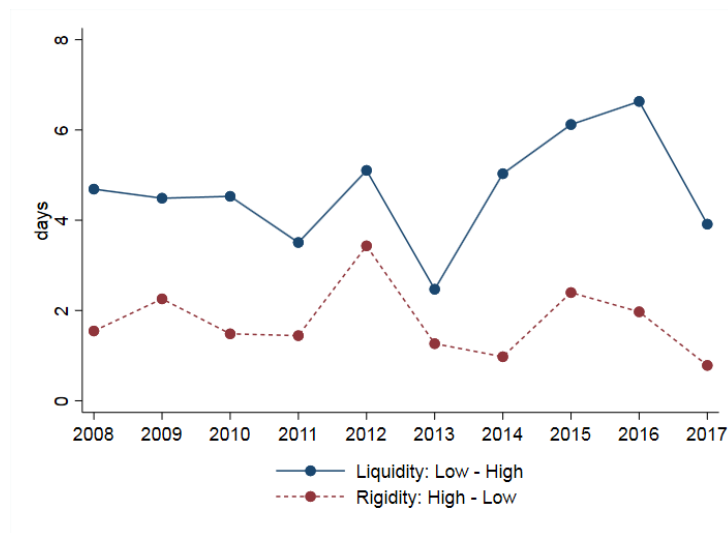
+ aplicações financeiras”); *accounts payable* as expenses verified but not paid (“restos a pagar processados”); *revenues* as current revenues (taxes, contributions, transfers from federal and state governments) minus contributions by pensioners and other deductions (“receitas correntes - contribuições sociais - deduções da receita corrente”); and *wage bill* as salaries, pensions and other benefits minus non-recurring expenses (such as the payments of compensations in disputes involving employees) (“pessoal e encargos sociais - sentenças judiciais - indenizações restituições trabalhistas”). From 2013 to 2017, I define *cash* as cash and equivalents (“1.1.1.0.0.00.00: caixa e equivalentes de caixa”); *accounts payable* as suppliers, wages and other benefits to be paid (“2.1.1.0.0.00.00: obrigações trabalhistas, previdenciárias e assistenciais a pagar a curto prazo + 2.1.3.0.0.00.00: fornecedores e contas a pagar a curto prazo”); *revenues* as current revenues (taxes, contributions, transfers from federal and state governments) minus contributions by pensioners and deductions (“1.0.0.0.00.00.00 - receitas correntes - 1.2.1.0.00.00.00: contribuições sociais - deduções da receita”); and *wage bill* as salaries, pensions and other benefits minus non-recurring expenses (“3.1.00.00.00.00: pessoal e encargos sociais” - 3.1.90.91.00.00: sentenças judiciais - 3.1.90.94.00.00: indenizações e restituições trabalhista”).

## A.2 Figures

Figure A1: Procurement methods

Purchasing method	Competitive	Characteristics	Contract size (for products)
Reverse auction <i>Pregão</i>	Yes	Reverse auction, open to any interested firm. Online or in-person. Off-the-shelf goods. Multiples bids per participant.	Any value.
Waiver (direct contracting)	No	Small purchase	Up to BRL 8,000.
Invitation to tender <i>Convite</i>	Yes	Participants are invited. Minimum of 3 bidders. Uninvited firms are allowed to participate. One bid per participant.	Up to BRL 80,000.
Competitive bidding <i>Concorrência</i>	Yes	Open to any interested bidder. One bid per participant.	Any value.
Submission of prices <i>Tomada de preços</i>	Yes	Bidder must be previously registered. One bid per participant.	Up to BRL 650,000.
Not required (direct contracting)	No	There is only one supplier.	-
Contest	Yes	Artistic, scientific or technical works.	-

Figure A2: **Standard deviation of time between verification and payment: relationship with (lagged) fiscal variables**



*Notes:* The data is aggregated at the firm-municipality-procurement-product level using monetary values as weights. For each year, I split the sample into two groups using the medians of the liquidity measure. I repeat the procedure for the budgetary rigidity measure. I then compute the mean of time between verification and payment for each group. I compute liquidity as follows:  $(\text{cash} - \text{accounts payable}) / \text{revenues}$ . The budgetary rigidity measure is defined as  $\text{wage bill} / \text{revenues}$ . The construction of both measures is explained in detail in the appendix A.1. In 2013 there is a reclassification of accounting variables, which might affect the values of fiscal variables.

## A.3 Tables

Table A1: Liquidity and rigidity - relationship with observable municipality characteristics

Commitment year	Median split - liquidity			Median split - rigidity		
	Low	High	H-L	Low	High	H-L
<i>Panel A: Population</i>						
2008	57016	78789	21773	89977	44003	-45975
2009	55962	80821	24858	78978	57685	-21293
2010	53095	79233	26138	89446	42883	-46563
2011	46936	81509	34573	93530	34915	-58615
2012	54020	71047	17027	71665	53402	-18263
2013	43049	87394	44346	85566	47746	-37820
2014	64387	73214	8827	67450	56826	-10624
2015	64320	65970	1650	72391	50236	-22155
2016	60641	68155	7514	74599	48349	-26249
2017	75496	56318	-19178	69405	55761	-13645
<i>Panel B: GDP per capita (in 2017 BRL)</i>						
2008	25438	30542	5103.9	30255	25530	-4724.8
2009	27631	32342	4710.9	31344	28609	-2735.4
2010	31162	33315	2152.9	35148	29329	-5818.7
2011	30330	35547	5217.1	37595	28283	-9312.4
2012	32480	35213	2733.4	37572	30121	-7450.7
2013	30701	38145	7444.7	39744	29400	-10344.2
2014	34785	37490	2704.8	40551	30749	-9801.5
2015	30946	34489	3542.4	36281	30301	-5979.7
<i>Panel C: Literacy rate</i>						
2008	98.73	98.78	0.05	98.79	98.71	-0.08
2009	98.68	98.78	0.10	98.73	98.73	0.00
2010	98.71	98.76	0.05	98.77	98.69	-0.08
2011	98.67	98.77	0.10	98.80	98.64	-0.17
2012	98.58	98.79	0.21	98.77	98.60	-0.17
2013	98.64	98.77	0.14	98.77	98.65	-0.12
2014	98.65	98.78	0.13	98.73	98.64	-0.09
2015	98.66	98.73	0.07	98.75	98.63	-0.12

*Notes:* The data is aggregated at the municipality level using monetary values as weights. For each year, I split the sample into two groups using the medians of the liquidity measure. I repeat the procedure for the budgetary rigidity measure. I then compute the mean of the variables for each group. I compute liquidity as follows: (cash - accounts payable) / revenues. The budgetary rigidity measure is defined as wage bill / revenues. The construction of both measures is explained in detail in the appendix A.1. In 2013 there is a reclassification of accounting variables, which might affect the values of fiscal variables.



Table A2: Classification of firms used in Table 2

Commitment Year	Year of donation (election)		
	2004	2008	2012
2008	connected (before)	connected (after)	
2009		connected (before)	connected (after)
2010		connected (before)	connected (after)
2011		connected (before)	connected (after)
2012		connected (before)	connected (after)
2013			connected (before)
2014			connected (before)
2015			connected (before)
2016			connected (before)

Notes: This table details the classification of firms used in Table 2. If the donation year is before the commitment year, the firm is classified as connection (before), that is, the firm donate to the coalition government in the previous election. Otherwise, if the donation year is after the commitment year, the firm is classified as connection (after): it will donate after the election. The same firm can be classified as connected (before) and as connected (after).

Table A3: Number of observations

Commitment year	Number of observations		Number of firms			
			Connected		Not connected	
	Connected	Not connected	Contracts in 1 mun.	Contracts in > 1 mun.	Contracts in 1 mun.	Contracts in > 1 mun.
2008	1122	238362	205	202	51620	19,846
2009	1999	242353	491	309	52116	19,759
2010	1849	250690	408	313	52884	20,134
2011	1801	262829	343	303	47124	20,997
2012	1766	275962	329	275	45565	21,390
2013	2033	279543	372	362	46931	21,501
2014	1888	285051	357	336	46233	21,626
2015	1557	254626	331	264	43822	19,662
2016	1465	258380	312	258	44309	19,981
2017	1354	275396	279	234	45912	21,105

Notes: I collapse the data at the firm-product-municipality-procurement method level. A firm is connected if it donates to any party of the coalition government in the previous election. More specifically, a firm is connected in 2008 if it donates to the coalition government in the 2004 elections; a firm is connected in 2009, 2010, 2011 and 2012 if it donates to the coalition government in the 2008 elections; and a firm is connected in 2013, 2014, 2015 and 2016 if it donates to the coalition government in the 2012 elections. Because donations are not allowed in the 2016 elections, a firm is connected in 2017 if it donates to the coalition government in the 2012 elections.

Table A4: Number of observations classified as connected, competitive x non-competitive

Commitment Year	Number of obs		Amount committed (million BRL)	
	Compet.	Non-compet.	Compet.	Non-compet.
2008	213	909	17.3	14.9
2009	350	1,649	29.3	18.7
2010	420	1,429	33.5	13.2
2011	393	1,408	38.7	15.6
2012	369	1,397	38.2	10.8
2013	443	1,590	78.9	13.0
2014	449	1,439	73.5	11.5
2015	359	1,198	63.8	10.1
2016	345	1,120	64.7	13.3
2017	277	1,077	43.9	6.3

Notes: I collapse the data at the firm-product-municipality-procurement method level and restrict the sample connected firms. A firm is connected if it donates to any party of the coalition government in the previous election. More specifically, a firm is connected in 2008 if it donates to the coalition government in the 2004 elections; a firm is connected in 2009, 2010, 2011 and 2012 if it donates to the coalition government in the 2008 elections; and a firm is connected in 2013, 2014, 2015 and 2016 if it donates to the coalition government in the 2012 elections. Because donations are not allowed in the 2016 elections, a firm is connected in 2017 if it donates to the coalition government in the 2012 elections.

Table A5: Partial correlations: cross-sectional regressions

Commitment Year	Election year	Time between commitment and verification			
		(1)	(2)	(3)	(4)
2008	✓	4.1***	-0.8	-0.8	-1.4
2009		2.3***	1.9*	2.4**	0.7
2010		2.2***	-2.1*	-1.9*	-2.5
2011		3.6***	-0.1	0.4	0.7
2012	✓	1.3*	-1.5	-1.1	-0.2
2013		3.4***	-0.0	-0.1	3.0
2014		2.0***	-0.9	-0.9	0.3
2015		1.6**	-1.8	-1.7	-0.1
2016	✓	0.2	1.9	1.8	0.9
2017		-0.4	-0.3	-0.3	0.2
Firm FE			✓	✓	
Munic. FE			✓	✓	✓
Prod. FE				✓	✓
Procur. FE				✓	✓
Firm-party FE					✓
Cluster Firm & mun			✓	✓	✓

Notes: The table presents  $\beta$ 's of the following regression specification (run per commitment year):  $y_{fmpj} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fmpj}$ . The dummy  $\mathbb{1}_{fm}$  takes the value 1 if firm  $f$  is connected at municipality  $m$ , that is, if the firm donates to the any party of the coalition government in the previous election. More specifically, a firm is connected in 2008 if it donates to the coalition government in the 2004 elections; a firm is connected in 2009, 2010, 2011 and 2012 if it donates to the coalition government in the 2008 elections; and a firm is connected in 2013, 2014, 2015 and 2016 if it donates to the coalition government in the 2012 elections. Because donations are not allowed in the 2016 elections, a firm is connected in 2017 if it donates to the coalition government in the 2012 elections.

Table A6: Partial correlations: cross-sectional regressions, connection (after)

Commitment Year	Election year	(1) Time between verification and payment	(2)	(3)	(4)	(5)	(6) Log (amount committed)	(7)	(8)
2008	✓	-1.2***	-1.4	-1.4	-1.0	0.68***	0.26**	0.23**	0.25*
2009		-2.3***	0.1	0.1	1.8	0.43***	0.24***	0.29***	0.36***
2010		-2.4***	-0.4	-0.3	0.8	0.48***	0.23***	0.28***	0.30**
2011		-1.3***	0.5	0.5	0.4	0.44***	0.16*	0.23***	0.29**
2012	✓	-2.3***	-0.5	-0.5	0.1	0.52***	0.30***	0.38***	0.55***
Firm FE			✓	✓			✓	✓	
Munic. FE			✓	✓	✓		✓	✓	✓
Product FE				✓	✓			✓	✓
Procurement FE				✓	✓			✓	✓
Firm-party FE					✓				✓
Cluster Firm & mun			✓	✓	✓		✓	✓	✓

Notes: The table presents  $\beta$ 's of the following regression specification (run per commitment year):  $y_{fmpj} = \alpha + \beta \mathbb{1}_{fm} + \text{controls} + \epsilon_{fmpj}$ . The dummy  $\mathbb{1}_{fm}$  takes the value 1 if firm  $f$  is connected (after) at municipality  $m$ , that is, if the firm donates to the any party of the coalition government in the coming election. More specifically, a firm is connected in 2008 if it donates to the coalition government in the 2008 elections; and a firm is connected in 2009, 2010, 2011 and 2012 if it donates to the coalition government in the 2012 elections. and a firm is connected in 2013, 2014, 2015 and 2016. Because donations are not allowed in 2016, I only report results from 2008 until 2012.

Table A7: **Difference-in-differences: time between verification and payment (alternative connection measure)**

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
<i>Panel A: all sample</i>						
Connected	3.5*** (1.2)	2.7** (1.3)	5.9** (2.5)	-1.4 (0.9)	-1.5 (1.0)	1.3 (2.1)
Observations	81,342	81,342	53,489	81,212	81,212	52,421
R-squared	0.00	0.12	0.31	0.00	0.11	0.32
<i>Panel B: Low liquidity</i>						
Connected	6.6*** (2.0)	6.3*** (2.0)	12.0** (4.7)	-5.1*** (2.0)	-5.3*** (2.0)	-2.6 (4.2)
Observations	39,486	39,486	22,288	24,319	24,319	11,296
R-squared	0.00	0.12	0.34	0.00	0.11	0.41
<i>Panel C: High liquidity</i>						
Connected	0.6 (1.4)	-0.5 (1.4)	3.5* (2.0)	0.7 (1.0)	0.6 (0.9)	2.9 (2.7)
Observations	38,106	38,106	21,459	56,777	56,777	34,310
R-squared	0.00	0.13	0.37	0.00	0.11	0.33
Firm FE			✓			✓
Mun. FE		✓	✓		✓	✓

*Notes:* In columns 1-3, I divide the sample into two periods, one year before and one year after the electoral changes, and then I collapse the data at the firm-municipality level. In columns 4-6, I repeat the same procedure in the previous mayoral term, when there is no change in the electoral rules. Regressions take the form  $\Delta y_{fm} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fm}$ , where  $\Delta y_{fm}$  denotes changes in time between verification and payment of firm  $f$  in municipality  $m$ . The variable  $\mathbb{1}_{fm}$  takes value 1 if firm  $f$  is connected in municipality  $m$ , and zero otherwise. In columns 1-3, a firm is classified as connected if it donates to the incumbent's party in the 2012 elections. In columns 4-6, a firm is classified as connected if it donates to the incumbent's party in the 2008 elections. In panel A, I include the entire sample. In Panel B, I restrict the sample to municipalities whose liquidity is below the median of the liquidity measure as of December 2015. In Panel C, I restrict the sample to municipalities whose liquidity is above the median of the liquidity measure as of December 2015. Standard errors are clustered at the firm and municipality levels.

Table A8: **Difference-in-differences: time between verification and payment**  
**(alternative connection measure)**  
*(firm-municipality-procurement method regressions)*

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
<i>Panel A: all sample</i>						
Connected	1.8*	1.5	4.2**	-1.6*	-1.8*	-0.9
	(1.1)	(1.1)	(2.1)	(0.8)	(0.9)	(1.8)
Observations	84,340	84,340	60,176	84,955	84,955	60,301
R-squared	0.00	0.12	0.32	0.00	0.11	0.33
<i>Panel B: Competitive</i>						
Connected	1.9	1.6	7.7***	-2.7	-1.9	-5.8
	(2.1)	(1.9)	(2.8)	(1.7)	(1.9)	(3.9)
Observations	23,456	23,453	18,547	18,921	18,918	14,478
R-squared	0.00	0.20	0.33	0.00	0.19	0.35
<i>Panel C: Non-competitive</i>						
Connected	1.7	1.0	2.8	-1.2	-1.6	1.5
	(1.3)	(1.3)	(2.6)	(1.0)	(1.2)	(2.1)
Observations	60,884	60,884	36,359	66,034	66,034	40,582
R-squared	0.00	0.11	0.35	0.00	0.11	0.36
Firm FE			✓			✓
Mun. FE		✓	✓		✓	✓

*Notes:* In columns 1-3, I divide the sample into two periods, one year before and one year after the electoral changes, and then I collapse the data at the firm-municipality-procurement method level. In columns 4-6, I repeat the same procedure in the previous mayoral term, when there is no change in the electoral rules. Regressions take the form  $\Delta y_{f_mj} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{f_mj}$ , where  $\Delta y_{f_mj}$  denotes changes in time between verification and payment of firm  $f$ , in municipality  $m$ , through procurement method  $j$ . The variable  $\mathbb{1}_{fm}$  takes value 1 if firm  $f$  is connected in municipality  $m$ , and zero otherwise. In columns 1-3, a firm is classified as connected if it donates to the incumbent's party in the 2012 elections. In columns 4-6, a firm is classified as connected if it donates to the incumbent's party in the 2008 elections. In panel A, I include the entire sample. In Panel B, I restrict the sample to competitive procurement methods. In Panel C, I restrict the sample non-competitive procurement methods. Standard errors are clustered at the firm and municipality levels.