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 exploit an electoral reform in Brazil 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 connected firms experience longer payment terms post-reform. The effect is particularly relevant in municipalities with low liquidity, where payment delays are more common, and for contracts awarded through a competitive tendering process. Our results highlight the importance of designing rules that curb discretion over the contract execution process in government purchases.

Keywords: Payment timeliness, public procurement, political connections
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1 Introduction

Government purchases account for a large share of government expenditures and can be a substantial revenue source for firms.[1] To improve efficiency and reduce the scope for corruption, governments have increasingly adopted procurement methods that foster competition and reduce discretion. Yet, even when sellers are selected through competitive procurement methods, the evidence of *quid pro quo* persists (e.g. Baltrunaite 2020). One possible explanation is that some firms receive preferential treatment after the tendering process, which gives them a competitive edge at the bidding stage (e.g. Brogaard et al. 2021).

In this paper, we document a new channel through which politicians may benefit firms that make campaign contributions: more timely payments. Recent research shows that the time elapsed between the delivery of goods and payment is consequential for firms.[2] Consequently, the speed of payment can be used by governments to favor certain suppliers. In light of these considerations, several countries have introduced policies aimed at shortening payment terms and eliminating discretionary payment practices.[3] Exploring a reform that bans corporate donations, we document an increase in payment time to campaign donors after the ban and show that the effect is particularly relevant in municipalities whose governments are more liquidity-constrained and in contracts awarded through competitive methods.

Evidence for this channel of political favoritism has been absent in the literature for a variety of reasons. First, it requires a shock to the strength of firms' political connections. To address that, we explore a set of electoral reforms in Brazil 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 donated in the previous election are not able to donate again in the coming election. Even though the owners of firms could still donate as individuals or even illegally, the ban implies that 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

[1] Public procurement represents 12% of global GDP (Bosio et al., 2022). Ferraz et al. (2021) document how government purchases shape firms' growth.

[2] Payment terms affect firms' liquidity, investment, employment, trade relationships, and probability of insolvency (e.g. Abad et al. 2023, Barrot 2016, Barrot and Nanda 2020, Breza and Liberman 2017, and Conti et al. 2021). Results tend to be more pronounced for financially weaker firms.

[3] Examples include the QuickPay initiative, launched in 2011 in the United States; Regulation 113 of the Public Contracts Regulations, passed in 2015 in the United Kingdom; and Law 21,131 in Chile, enacted in 2019. Brazil's new procurement law (Law 14,133), enacted in 2021, stipulates that payments must be made in chronological order and mandates that all purchasing units publish this order on their websites on a monthly basis.

less favoritism.

Second, electoral reforms of this type are not exogenous. They usually coincide with corruption scandals and an increasing anti-corruption sentiment, which can amplify their effects (e.g. Clark et al. 2018) or attenuate them if firms and politicians change their behavior before the new rules are in place. Moreover, stakeholders can change their relationship with politically connected firms in such situations. For instance, a bank or a supplier might not want to be linked or financially exposed to a firm that could be charged with corruption and experience distress in the future (Ferraz et al., 2023). In turn, this fact can impair the capacity of politically connected firms to supply to the government and to invest in costly inputs (e.g. labor, legal fees) that enable an effective payment collection process. To account for these time-varying firm effects that are not directly caused by the reform, we exploit the fact that firms chose to donate in some municipalities but not in others in the last elections when donations were allowed, 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, while in municipalities where it did not donate, the relationship is unchanged. Our setting allows us to compare within-firm changes in payment delays around the reforms.

Finally, data on supplier-level payment delays are often not available to researchers. We leverage a rich new dataset on public procurement at the municipal level in Brazil to construct measures of payment delays at the municipality-supplier level. As is usually the case in budget execution, governments pay suppliers after the verification stage – the moment they acknowledge that the delivery of goods and services procured are in accordance with contract specifications. We then measure how long governments take to pay suppliers by computing the days between the payment and verification dates. Because the verification of services and construction projects is complex and more prone to discretionary decisions, we focus our analysis on the procurement of goods, for which the verification date is a good proxy for the delivery date.[4]

We show that, after the reforms, the time between verification and payment to connected firms increases by approximately 3 days.[5] The effect amounts to 13% of the average payment time, 20.3 days.[6] We next assess whether the probability of extreme delays is affected, as this may represent the most critical concern for firms. We show that, following the reform, the probability of being paid after more than 60 days increases by 3 percentage points for connected firms. This is a large effect, considering that the unconditional probability of delays exceeding 60 days is 5%. We also test for the impact

[4]Results for services and construction are available in the Appendix.

[5]In the paper, we discuss some factors that can attenuate the magnitude of the effects. One of them is the undeclared donations known as *caixa 2*. This form of funding is widespread in Brazil and remains unaffected by the reform. Business owners can also partially offset the ban by increasing donations as individuals.

[6]Although microdata on payment times are limited, 20.3 days is likely a relatively short period. For instance, survey evidence indicates that payment delays are longer in Europe (Intrum Justitia 2013).

on the amount purchased. Effects are negative (decrease of 10%) 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 five years. Therefore, the amount purchased is more path-dependent than the days between verification and payment variable.

Next, we investigate when favoritism in terms of speed of payment is more relevant. When the municipality is liquidity-constrained, meaning it does not have enough liquid resources to meet its short-term financial obligations, it must determine which expenses will be paid on time or with minimal delay. Because firms are not fully compensated for payments that take place outside the contractual terms, late payments can also be interpreted as a partial default. Therefore, governments choose which firms bear the highest losses.[7] Another case in which the payment timeliness can be relevant occurs when the tendering process is mandatory. In this case, the government's commitment to paying more quickly may represent an important advantage, allowing favored firms to outbid otherwise similar firms and deterring non-favored firms from participating in or winning procurement contracts. (e.g. Colonnelli et al. 2024).

We test whether the effects are more pronounced when municipalities face liquidity constraints. Once reforms diminish the gains of paying connected suppliers more timely, governments may refocus their priority on expenses that generate greater political gains in the post-reform environment.[8] We show that the effects of the reforms on payment dates for connected firms are larger, around 7 days, in municipalities with lower liquidity. The probability of receiving payment after 60 days increases by 7 percentage points for connected firms in these municipalities, compared to the unconditional probability of 8%. These effects are not statistically significant in municipalities with higher liquidity. We then 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 larger, 10 days, for contracts awarded through competitive methods, while for non-competitive methods, they are not statistically significant. The results suggest that payment speed is an important dimension of favoritism when governments find it less straightforward to benefit donors through the direct allocation of contracts.[9] This is particularly relevant in our setting, as we restrict the

[7]Brazilian authorities have expressed concerns about the prioritization of payments to certain firms during periods of government liquidity shortages. We provide further details in the discussion of the institutional setting. Earlier payment is also valuable when the supplier faces financial distress or tight borrowing constraints. In these cases, the marginal value of cash increases.

[8]These expenses might include suppliers in areas not included in the analysis, or even wages and social benefits. For example, it is not uncommon for municipalities to delay salary payments, which is politically costly. After a reform, illiquid governments might prioritize wages over connected suppliers.

[9]Discretion in allocation contracts does not necessarily have adverse effects on procurement outcomes. See Coviello et al. (2018).

sample to products that have an "off-the-shelf" characteristic and, as a result, the cost of rigging an auction is high.

This paper contributes to two broad strands of literature. First, it adds to the substantial literature on the effects of political connections and favoritism (Krueger 1974; Fisman 2001; Faccio et al. 2006). The literature has shown *quid pro quo* happening for politically connected firms through a variety of channels, such as preferential procurement contracting (Goldman et al. 2013; Brogaard et al. 2016; Schoenherr 2019), having more access to finance (Khawaja and Mian 2005; Li et al. 2008), or beneficial regulation (Fisman and Wang 2015).[10] Such effects have generally been shown to increase firm value and performance (Fisman 2001; Jayachandran 2006; Ferguson and Voth 2008; Duchin and Sosyura 2012; Cingano and Pinotti 2013; Amore and Bennedsen 2013; Acemoglu et al. 2016),[11] but at the expense of resource misallocation (Akcigit et al. 2023; Brugués et al. 2024). The paper most closely related to ours is Baltrunaite (2020), which explores a ban on corporate donations in Lithuania. She finds that the probability of donors winning contracts decreases after the reform.

Having defined a political connection as a campaign donation,[12] we document the speed of payment as a novel channel of favoritism.[13] Our results add to the literature in three ways. First, they explain why *quid pro quo* is observed even when government agencies use competitive auctions. Second, the measure of favoritism – speed of payment – is an objective quantity for which measurement is straightforward. Determining the precise economic value of the favors that politicians give to firms is often difficult. Earlier payment is an advantage that can be easily translated into a monetary value. Third, we argue that empirical strategies that use the trajectories of non-donors as a counterfactual for the trajectories of politically connected firms after electoral reforms (e.g. Baltrunaite 2020) can be biased due to the endogeneity of the new legislation (e.g. Besley and Case 2000). We highlight the necessity of employing within-firm estimations when trying to assess the impact of this type of reform. Finally, our paper sheds light on the nature of the relationship between donors and politicians. The relationship is not based on an instantaneous and one-time exchange of favors; rather, these are relationships built on trust, repeated interactions, and enforced by the future value

[10]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 receiving more government contracts.

[11]One notable exception is Fowler et al. (2017), who find no detectable *quid pro quo* in the US.

[12]Because campaign contributions involve a cash disbursement, researchers also investigate whether they are an investment in political capital, a reflection of agency problems, or a form of consumption of business owners with strong political beliefs. See Aggarwal et al. (2012), Akey (2015), Cooper et al. (2010), and Fowler et al. (2017).

[13]Other papers in the literature define political connections in different ways: CEOs and politicians have educational, professional or social ties, a large shareholder or officer is a member of the parliament or the executive (Bunkanwanicha and Wiwattanakantang, 2008; Tahoun, 2014), a former politician sits on the board of directors (Goldman et al., 2008), among others.

of complying with the informal arrangement (e.g. Levin 2003). Our results show that politicians grant fewer favors to donors (i.e. the relationship is weakened) when they cannot benefit from future donations.

This paper also contributes to a growing literature on the importance of payment terms. Barrot and Nanda (2020) investigate the effects of the QuickPay reform in the US. The reform cut the time to pay from 30 to 15 days to a subset of small firms. They find that treated firms increase employment by 1.7% following the 15-day reduction. Conti et al. (2021) show that the EU directive on late payments increased firm survival and Abad et al. (2023) document that the payment of government arrears in Spain increased supplier investment. Barrot (2016) shows that stretched payment terms increase barriers to entry and expose firms to liquidity risk. Payment terms might be even more relevant in a country with more financial frictions, such as Brazil, where firms are credit-rationed or pay higher interest rates. These findings, when applied to this paper's setting, imply that more favorable payment terms to connected firms affect the ability of non-connected firms to compete in government auctions. The fact that the effect is greater when the municipality has lower liquidity and, therefore, is delaying payments provides an additional motivation for the proper management of government arrears, as their existence might lead to rent-seeking behavior (Flynn and Pessoa 2014).

The remainder of the paper proceeds as follows. Section 2 describes the empirical setting, the main data sources, and the empirical strategy. Section 3 overviews some descriptive statistics. Section 4 discusses the main results, heterogeneity, and placebos. Section 5 concludes.

2 Empirical setting

2.1 Budget execution

Two annual laws mainly guide the budget execution of local governments in Brazil: the Budget Guidance Law (*Lei de Diretrizes Orçamentárias*) and the Annual Budget Law (*Lei Orçamentária Anual*). The executive branch proposes the bills, which are discussed, amended, and voted on by the local legislature, and then signed into law by the mayor. 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. After the approval of the Budget Guidance Law, the elaboration of the Annual Budget Law commences. The budget details the allocation of expected revenues to each government agency and activity. The budget is comprehensive, that is, an agency can only execute an expense if it is prescribed in the budget.

When the fiscal year starts, the executive branch begins the execution of the expenses specified in the budget. The budget execution process in Brazil is similar to that in other countries (Potter et al. 1999; Flynn and Pessoa 2014). It consists of three distinct stages:

1. ***Commitment***: The government agency reserves part of a specific appropriation for a supplier that was previously selected in a procurement process. From a budgetary perspective, this is the moment an expenditure is recognized since committed amounts are deducted from the budget appropriation.
2. ***Verification***: The government agency formally acknowledges that the good or service was delivered according to specifications. This is the moment when a debt with a provider is recognized, and it is considered an expenditure from an accrual accounting point of view.
3. ***Payment***: Cash is transferred to the supplier. This is the moment an expenditure is recognized under cash-basis accounting, which is the method used to compute metrics such as the primary surplus.

The number of days between the verification and payment stages is a proxy for the amount of time it takes governments to pay suppliers. 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. We restrict the analysis to goods in order to minimize measurement error and provide results for other categories in the Online Appendix. In 2015, municipalities allocated an average of 12.1% of their total expenses to the purchase of goods. In comparison, expenditures on construction and services provided by firms represented 4.3% and 16.9%, respectively, of total expenses. The payment of salaries constituted the largest share of municipal expenses, accounting for 54.8% of total expenditures on average.

The number of days between the commitment and verification has a less clear interpretation. In some cases, in which a commitment is followed by one verification and one payment, this variable can be viewed as a proxy for the time elapsed between the purchase order and its delivery. However, in many cases, the commitment is followed by multiple verifications and payments, indicating that one commitment might be associated with several purchase orders. In this case, the interpretation of the variable is less clear.

2.2 Public procurement and payments

Government agencies can employ different methods to procure goods and services.[14] 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.[15] The regulation establishes a threshold to define small. During our sample period, the threshold was 8,000 BRL for products and services and 15,000 BRL for construction. We classify the cases without a tendering process as non-competitive procurement.

The other methods involve a tendering process, such as invitations to tender and reverse auctions (regular and electronic). The method itself depends on the scope and value of the purchase. Even though these methods can differ in some dimensions (e.g. conditions to participate), we classify them in a single group as competitive procurement. The use of electronic reverse auctions has increased over time, especially for simple products.

Government agencies have to pay suppliers within 30 days following the acknowledgment 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.[16] Payments outside the limits are common.[17] In such cases, the amount due can be adjusted by inflation and a late payment fee. 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 being costly, 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. In this case, the government is considered to be in default, and delays smaller than 90 days are not considered a contract breach.[18]

Although the procurement law in effect during our sample period included a provision requiring payments to be made in chronological order, this requirement was not observed. Concerns regarding payment discretion as a potential source of favoritism have been raised in various public debates, especially when the financial resources of governments are limited. For instance, the Federal Court of Accounts (*Tribunal de Contas da União*) stated in the report of the Ruling No. 2,360/2018: "Compliance with

[14] During our sample period, the Public Procurement Law (Law 8,666) contained most of the public procurement regulations. The regulation changed in 2021.

[15] The regulation considers other cases, but they are less common. For instance, emergencies and threats to national security.

[16] Article 40 of the Public Procurement Law (Law 8,666).

[17] According to a survey of the National Confederation of Municipalities (*Confederação Nacional dos Municípios*, CNM), 50.2% of the municipalities reported a positive stock of arrears owed to suppliers in 2018 (CNM, 2018).

[18] Article 78 of Law 8,666 describes situations in which the contract can be revoked.

the payment order is important, since agencies do not always have all the financial resources available. Therefore, a payment order aims to prevent a supplier from being overlooked. The advance of certain payment processes may unduly favor a particular supplier and characterize anti-equitable treatment, configuring procedural fraud. These advances, when combined with other evidence or indications, may also characterize a crime of corruption." Similarly, Sarai (2021) argues that the chronological order "(...) represents the application of the principle of impartiality in a potential circumstance of finite resources, in which the Administration either does not have the means to pay all of its suppliers, or, if it can do so, runs the risk of having to delay payments to some while the resources for this purpose are determined." In light of these concerns, the new public procurement law, enacted in 2021 (after our sample period), and the Normative Instrtuction No. 77, of 2022, established that payments should follow the chronological order, which is defined by the verification date, and that government agencies should make the payment order available in their websites.

2.3 Municipal elections and electoral reforms

Municipal elections occur every four years in Brazil. Voters choose the mayor and city councilors, who serve a four-year term. Elected officials take office on the first of January of the coming year. Mayors can run for re-election but are allowed to serve two consecutive terms only. Members of the local council do not face a limit and can be re-elected indefinitely. The council is elected in an open-list proportional representation system.[19] Mayors are elected by absolute majority. In municipalities where the number of voters is larger than 200,000, there is a run-off if no candidate obtains more than 50% 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.[20]

Since 2013, a large anti-corruption investigation revealed a widespread kickback scheme that involved the funding of parties with money obtained from federal and state government contracts. Several members of the business and political elite were convicted of corruption charges. Reacting to growing unrest, the judiciary and the fed-

[19] Parties form local coalitions. The number of seats allocated to a coalition is calculated as a proportion to the total number of votes it receives.

[20] A limit on "self-donations" was only imposed in 2019 through Law 13.878/2019

eral 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. In September 2015, the Supreme Court declared corporate donations unconstitutional.[21]

Also in September 2015, the federal congress passed a law that changed political campaigns considerably.[22] Firstly, it established campaign expenditure limits. The limits were set at 70% of the maximum amount a candidate spent in the previous campaign and then adjusted for the accumulated inflation between the last and coming elections. Secondly, the law introduced changes to reduce campaign costs. For instance, it cut the duration of the campaigns by half, from 90 to 45 days. The rules regarding the donations of individuals (whether they are candidates or not) were not changed.

2.4 Data and construction of variables

We collect data on the execution of municipal budgets from the State Audit Courts (*Tribunais de Contas dos Estados*, TCEs) of the states of São Paulo, Minas Gerais, Rio Grande do Sul, and Paraná (Dahis et al. 2023). These courts are independent institutions that supervise the public finances of the municipalities in their states. The TCEs of these states provide detailed information on the three stages of budget execution. In particular, they provide the dates and monetary amounts of every commitment, verification, and payment, as well as the supplier's tax identifier. For municipalities in the state of São Paulo, the data also contain the procurement method that the government employed to select the supplier.

We 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. We construct two measures: the days between commitment and verification, and the days between verification and payment. The latter is our proxy for the time it takes for the government to pay a transaction. Measurement is straightforward for commitments that have only one verification and one payment. For commitments associated with multiple verification and payment stages, we weigh each operation by its monetary value (see Figure A.1 in the Online Appendix for an il-

[21]The trial started in 2013, and by April 2014, six out of the eleven judges voted against the constitutionality of corporate donations. However, one of the judges requested more time to examine the case, arguing the matter was the prerogative of Congress and not the Supreme Court (see the article "Brazil's top court bans corporate money in election campaigns," published at https://www.reuters.com/article/idUSKCN0RH33A/). Even though a majority was formed at the beginning of 2014, it was not clear when the court would finish the trial. Cases with vast implications, such as this one, can take many years to be fully appraised, and a judge can single-handedly suspend a case indefinitely. Moreover, as long as the case is open, judges could change their votes. Finally, it was unclear in which elections the new rules would be implemented. It is not uncommon for the Supreme Court to postpone the implementation of a new rule to allow agents to adapt.

[22]See Avis et al. (2022) for the effects of this law on political entry and competition.

lustration). Since these are budgetary data, they do not include information on prices and quantities purchased nor details on the tendering process (such as the number of participants or the value of bids).

The Superior Electoral Court (*Tribunal Superior Eleitoral*, TSE) provides political campaign contributions and electoral results data. We collect information for the 2008 and 2012 elections from the Data Basis platform (Dahis et al. 2022). The mayors elected in the 2008 elections were in office from January 1, 2009, to December 31, 2012; mayors elected in the 2012 elections were in office from January 1, 2013, to December 31, 2016 (Figure A.2 in the Online Appendix depicts the electoral calendar during our sample period). We observe to which party or candidate the firms donated and in which municipality.

The Ministry of Finance annually provides aggregate data on the financial situation of municipalities, including balance sheet items, revenues, and expenditures. Balance sheet items are measured on December 31 of each year, while revenue variables refer to the fiscal year, which runs from January 1 to December 31. We construct a measure to assess the liquidity of municipalities. The liquidity measure is defined as the difference between cash and equivalents and a measure of accounts payable, divided by revenues.[23] The higher this measure, the more liquid the municipality, that is, the more liquid reserves a local government has to meet obligations that are due within one year (the current part of the liabilities of a government). We provide more details on the construction of this variable in Section A.1 of the Online Appendix.

The Brazilian Institute of Geography and Statistics (IBGE) provides municipality characteristics, such as geographical area, GDP, literacy rate, and population.

2.5 Empirical strategy

We divide the sample into two one-year windows around the electoral reforms of September 2015 (Figure 1a): one year before (pre-reform) and one year after the electoral changes (post-reform). The budget execution data are at the commitment level. We collapse them to the firm-municipality-time level using the monetary amount of the commitments as weights. Suppose that in period $t \in \{\text{pre-reform}, \text{post-reform}\}$ firm f has C_{fmt} commitments with the government of municipality m. If commitment $c \in \{1, ..., C_{fmt}\}$ has value V_{cfmt} and days between verification and payment (or days between commitment and verification) D_{cfmt}, then our measure of payment time is

$$y_{fmt} = \frac{\sum_{c=1}^{C_{fmt}} V_{cfmt} \times D_{cfmt}}{\sum_{c=1}^{C_{fmt}} V_{cfmt}}$$

[23]Similar measures, such as the current ratio and the quick ratio, are used to gauge firms' liquidity.

For the amount committed, we simply sum all the commitments of firm f in municipality m and period t.

We run the following regression specification:

$$y_{fmt} = \alpha_{mt} + \alpha_{ft} + \beta Connected_{fm} \times Post_t + \gamma Connected_{fm} + \epsilon_{fmt} \tag{1}$$

where $Connected_{fm}$ is a dummy variable that takes the value one if firm f is connected in municipality m, that is, if it donates to the mayor's party in the 2012 elections; y_{fmt} represents the outcomes of interest (days between commitment and verification, days between verification and payment, and the natural logarithmic of the amount committed), which we measure in periods $t \in \{\text{pre-reform}, \text{post-reform}\}$. The dummy $Post_t$ takes the value one when $t = $ post-reform. We control for unobserved time-varying municipality changes, such as a deterioration in the ability to pay suppliers on time, by progressively including municipality-time fixed effects α_{mt}. Because the same firm can have contracts in more than one municipality, we can control for unobserved time-varying changes in firm characteristics by progressively including firm-time fixed effects α_{ft}. We cluster standard errors at the firm and municipality levels.

The possibility of controlling for time-varying changes at the firm level is a key advantage of our setting. Because electoral reforms of this type are usually the politicians' response to corruption scandals, investigations, and anti-corruption sentiment, they can coincide with changes in other variables that impact firms that are connected to politicians. As a result, politically connected firms can follow different trends than non-connected firms after the reforms for reasons unrelated to the electoral changes. For instance, suppose that suppliers, other clients, and banks refrain from doing business with politically connected because they do not want to be financially exposed to them. If the collection process is costly due to the need for skilled lawyers and collection specialists, politically connected firms may face longer payment terms, even in the absence of approved reforms, as their ability to collect payments is compromised. Alternatively, these firms might increase collection efforts because they need to access liquidity more quickly. In this case, connected firms might experience shorter payment terms even if the reforms had not been approved. Although these biases might cancel out each other, it is important to address the possibility that one dominates the other. By including firm-time fixed effects, we are able to control for these changes at the firm level and pin down the effects of electoral changes. The identification hypothesis is that, in the absence of reforms, firms would follow a similar trajectory in municipalities where they are connected and municipalities where they are not.

The downside of the inclusion of firm-time fixed effects is that only firms that transact with more than one municipality end up entering the sample. As a result, even though these fixed effects address omitted-variable bias concerns, they might introduce some sample bias. Moreover, as we cannot observe the main variables when firms do not sell to the local government, we also restrict the sample to firms that sell in all the time periods of the panel. Because many firms do not sell to the same local government repeatedly, the inclusion of more pre-reform periods would render these filters excessively restrictive. Additionally, an expansion of the time window might include the previous mayoral term, during which the connection status of firms might be different. Therefore, we implement the main analysis with two time periods only (pre- and post-reform) and then perform tests to guarantee that effects are not driven by pre-trends or the specific point in the political cycle in which the reforms occur (around fourteen months before the next election).

To provide evidence in support of the lack of pre-trends, we estimate a similar regression using a sample in which the "post" period is the pre-reform period and the "pre" period is the 12-month window that precedes the pre-reform period (Figure 1b). In this exercise, the definition of connected firms remains the same (a firm is connected if it donates to the mayor's party in the 2012 elections), but no electoral changes take place between the two periods. In the Online Appendix, we also estimate a dynamic specification with two 12-month pre-treatment periods.[24] To check whether results are driven by the point of the political cycle in which reforms take place, we run a similar regression but using data from the previous mayoral term (four years before). In these regressions, a firm f is connected in municipality m if it donated to the mayor's party in the 2008 elections. The crucial difference is that firms can donate in the coming elections, that is, no law partially breaks down the relationship between donors and politicians (Figure 1c).

3 Descriptive statistics

Table 1 provides summary statistics for the main variables. We use two samples: the entire data and a restricted version in which firms are required to sell to more than one municipality.[25] The restricted version is our estimation sample, as our identification strategy relies on within-firm variation across municipalities.[26] The average time between verification and payment is 18.5 days in the unrestricted sample and 20.3 days in the restricted sample. Despite a low average, Figure 2 shows that a significant share of commitments are paid outside the 30-day period. The 90th percentile is around 42

[24]A sample with more than two 12-month pre-treatment periods would contain observations of the previous mayoral term.

[25]In Table B.3 of the Online Appendix, we also show the consequences of restricting the sample to firms that sell in both periods. Firms that sell in both periods are larger (in terms of the amount committed) than firms that sell pre-reform only.

[26]The outcomes of firms that sell to one municipality only are fully explained by the firm-time fixed effects.

days in both samples.[27]

The average time between commitment and verification is 20 days in both samples. The average amount committed is 23,133 BRL in the unrestricted sample and 35,766 BRL in the restricted sample, which is consistent with firms that sell to more than one municipality having larger contracts. The distribution is highly skewed: the median amount committed is 3,252 BRL in the unrestricted sample and 5,529 BRL in the restricted sample. The averages for the main variables are similar in the pre- and post-reform periods.

Table 2 provides summary statistics of firms and municipalities. We classify firms into three groups. A firm is considered *connected* at a given municipality if, in the previous elections, it donated to the party of the candidate that was elected. We divide *unconnected* firms into two groups. An unconnected firm is a *donor* if it donated only to the parties of mayoral candidates that did not win the previous elections, and a *non-donor* if it did not make campaign contributions. Unconnected non-donor firms comprise the majority of our sample. Connected firms have larger contracts (as measured by the amount committed) than unconnected donors, which in turn have larger contracts than unconnected non-donor firms. There are several potential explanations for this pattern. Firms that donate could be more efficient and have a larger production capacity. Additionally, connected firms can engage in activities that are part of the incumbent's agenda. For instance, a firm that produces school supplies might donate to candidates who prioritize education expenditures. The allocation of contracts to firms that have a close relationship with incumbents can also be a solution to issues such as moral hazard or adverse selection when the quality of the object of the contract is not readily observable or verifiable. Finally, donors could have larger contracts because of favoritism.

Even though connected firms sell to 13 municipalities on average, they only donate to 1.2 municipalities on average. The amount connected firms donate to winners is around 10,123 BRL, which represents 1% of the average amount committed to these firms. The time between verification and payment for connected firms is about one to two days smaller than for unconnected firms.

In Panel B of Table 2, we show that the average municipality in our sample has 36 thousand inhabitants and a GDP per capita of 22.4 thousand BRL in 2015. We split the sample of municipalities into high- and low-liquidity groups using the median of the liquidity measure in 2015. Municipalities with high and low liquidity display similar GDP per capita and population levels. However, low-liquidity municipalities pay

[27]Reliable information on payment delays across countries is hard to come by, but municipal governments in Brazil seem to be relatively fast payers. According to data from the Contracting with the Government survey of the World Bank, the average payment time for road construction contracts in other countries was 101 days in 2020. See https://archive.doingbusiness.org/en/data/exploretopics/contracting-with-the-government.

suppliers 5.5 days later than high-liquidity municipalities. In Figure 3, we plot the histogram of the days between verification and payment variable for the two groups of municipalities. The mass of commitments paid after 30 days is larger in low-liquidity municipalities, confirming that delays are more common when governments do not have enough liquid resources to pay short-term liabilities. Figure B.1 in the Online Appendix shows how our payment time measure correlates with business cycle fluctuations. It shows that the payment times increase during recession periods. Moreover, it shows that low-liquidity municipalities take longer to pay suppliers in times of boom and bust alike.

4 Results

4.1 Main results

Table 3 reports the coefficients obtained from the estimation of Equation 1. We report results in four combinations of fixed effects. First, we include no fixed effects to allow for the full variation in the data, with firms that sold to governments before or after the reform, or both (Baltrunaite, 2020). Second, we include municipality-time fixed effects to account for municipality-specific trends. Third, we include firm-time fixed effects to account for firm-specific trends. Finally, we include both sets of fixed effects to estimate our most stringent specification discussed in Section 2.

The time between verification and payment for connected firms increases by 4.2 days after the reforms. Once we include firm-time and municipality-time fixed effects, the magnitude of the coefficient drops to 2.7 days but remains statistically significant at 5%. This magnitude is around 13% of the average time between verification and payment variable (20.3 days). In Section 4.6, we provide a more detailed discussion about the economic significance of the effect. In Figure 4, we show that the difference-in-differences effect comes from connected firms being paid earlier than unconnected firms before the reforms; after the reforms, the two groups face similar payment times.

Even though we restrict the sample to simple products that have a clear delivery date and for which the verification is not complex, there is still the possibility that connected firms benefit through a more timely verification. Alternatively, because of the continuing nature of the relationship between donors and politicians, issues like adverse selection and moral hazard are not present, and governments can spend less time assessing the quality of the products delivered by connected firms.[28] We test this hypothesis using the days between commitment and verification as an outcome vari-

[28] Breza and Liberman (2017) show that buyers use trade credit to assess the quality of the products. 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).

able. Panel B of Table 3 shows that connected firms do not experience a change in this variable after the reforms.

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, the amount committed and payment timeliness are jointly determined and a deterioration in payment timeliness would also be followed by a decrease in the amount committed. Panel C of Table 3 shows no effects on the log of the amount committed for connected firms after the reforms. One potential explanation for the absence of an effect is that the amount committed variable displays more inertia than the time variables. The length of the contracts can be as long as five years, especially for large amounts, and commitments after the reform could refer to contracts awarded before the reform.

4.2 Heterogeneity by the liquidity status of the local government

In Table 4, we show how the municipality's liquidity shapes results. We divide the sample into two groups of municipalities (high- and low-liquidity) using the median of the liquidity measure in December 2015.[29] In the specification with municipality-time and firm-time fixed effects, the time between verification and payment for connected firms increases by 6.6 days after the reform in low-liquidity municipalities.

This result is consistent with payment timeliness being a more relevant dimension of favoritism when municipalities experience difficulties in meeting short-term obligations. In such cases, governments must decide which expenses are paid according to contract terms and which may experience smaller delays. Since the electoral reforms reduce the value of relationships with connected firms, cash-constrained governments may shift their priorities, allocating funds to other expenses that are now comparatively more valuable. If liquidity shocks to the municipality government coincide with liquidity shocks to the firms that trade with it, this type of favoritism is even more relevant as it takes place when the marginal value of cash for the firms is high. The favoritism, in this case, would have an insurance aspect: it pays off exactly when its marginal value is higher. Payment timeliness is less of a problem when governments have enough cash to pay all suppliers on time. Indeed, in high-liquidity municipalities, the point estimate is 0.8 days and it is not statistically significant.

[29]Even though the number of municipalities is the same in both groups, the number of observations is higher in the high-liquidity sample. This is because the data is at the firm-municipality-time level, and high-liquidity municipalities are slightly larger and richer than low-liquidity municipalities (Table 2).

The effects on the other variables – days between commitment and verification and log of the amount committed – are not statistically significant in either subsample.

4.3 Extreme delays

It could be that not only the average payment speed changes, but also that extreme delays become more likely. Extreme delays may be a primary concern for suppliers, especially if the costs of delay increase exponentially over time. We assess whether, after the reforms, municipalities are more likely to make payments to connected suppliers 30, 45, or 60 days after the verification stage. We provide descriptive statics of these variables in Appendix Table B.4. In the estimation sample, the probabilities of being paid 30, 45, and 60 days after the verification stage are 21%, 10%, and 5%, respectively. The table also reveals that extreme delays are more common in liquidity-constrained municipalities, where the probabilities of payment times exceeding 30, 45, and 60 days rise to 27%, 14%, and 8%, respectively.

In Table 5, we show that, after the reforms, the probability of being paid in more than 60 days increases by 3% for connected firms, compared to an unconditional mean of 5%. The probabilities of being paid in more than 45 or 30 days also increase by 3% (relative to the unconditional probabilities of 10% and 21%, respectively), but we cannot reject the hypothesis that these effects are different from zero.

4.4 Heterogeneity by competitive and non-competitive procurement

In Table 6, we test whether effects differ by the type of procurement method used. We restrict the data to the municipalities of the state of São Paulo since this information is not available in other states. Competitive procurement methods entail an open tendering process. Non-competitive procurement methods do not involve a tendering process and, as a result, politicians have more discretion in the selection of suppliers.

Effects are larger when we only consider commitments for which the suppliers were selected through competitive procurement: the time between verification and payment for connected firms increases by 9.7 days after the reforms. The effects are not statistically significant for commitments for which suppliers were selected through non-competitive procurement methods. The results suggest that speed of payment is an important way of distorting public procurement when it is more difficult to award contracts to connected firms. We do not find statistically significant effects in the other variables (days between commitment and verification and log of the amount committed). In the case of the amount committed, one possible explanation is the unobserved length of the contracts, which means that commitments after the reform could refer to contracts signed before the change.

4.5 Placebos and pre-trends

In Table 7, we report the results of placebo exercises with two periods. A potential concern is that, since the post-reform period overlaps with the 12-month period before the next election, results could be driven by politically connected firms allowing governments to prioritize the payment of other expenses on time. For instance, as payment delays of some expenditures (such as the salaries of school teachers) are very costly politically and can affect election results, connected firms might put up with delays so that governments can pay these expenses on time. To rule out this channel, we run the same specification at the same point of the mayoral term (that is, fourteen months before the next election) but in the previous electoral cycle, when reforms did not take place (Figure 1c). Estimates are not significant across all variables, which provides evidence that the effects we uncover in Table 3 are not driven by features of the political cycle.

Another potential concern is the existence of pre-trends. As running a regression with multiple periods would be very restrictive, as firms would need to sell to multiple municipalities in multiple periods, we run a regression in a sample with two periods in which the pre-reform period becomes the "post" period. We also do not find statistically significant effects. In Figure B.2 and Table B.1 of the Online Appendix, we report coefficients of a dynamic difference-in-differences model with four 12-month periods: two periods before the reform (2013 and 2014), the reform year (2015), and one year after the reform (2016). The results confirm statistically insignificant pre-trends and a positive significant effect after the reforms.

4.6 Economic significance and factors that can attenuate the effects

What is the economic significance of the results? Estimates of the effect of the reforms on the time between verification and payment range from 2.7 to 9.7 days. Barrot and Nanda (2020) find that a 15-day reduction in payment terms causes an increase of 1.7% in firms' employment. In Brazil, because financial frictions are more severe and access to credit is more restricted and expensive, a similar reduction in payment delays could have even larger effects. However, the effects do not seem to be large in monetary amounts. Assuming that firms finance their working capital needs with bank loans that use receivables as collateral, 2.7-9.7 days represent around 0.28%-0.83% of the amount committed.[30]

Other institutional features can moderate the magnitude of the effects. Because firm owners can still donate as an individual or illegally, the breakdown of the relationship is only partial. In Brazil, illegal contributions, known as *caixa 2*, are common and con-

[30] According to the Central Bank of Brazil, the average monthly interest rate of loans that use receivables as collateral was 2.31% in December 2015.

sist of slush funds used by politicians in their campaigns. Therefore, it is difficult to assess to which extent the reforms broke the relationships between firms and politicians, but we can interpret the magnitudes as a lower bound of the effect in the case of a complete breakdown. We also uncover cases in which the effects are more relevant: commitments awarded through competitive procurement methods and those granted by illiquid municipalities.

Moreover, in this paper, we focus on simple products. The firms that sell this kind of product likely operate at low margins. Thus, the effect can be quite significant as a percentage of the margin. 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 came 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 rig auctions for construction services. Supplier selection is based not only on price in these cases but also on technical capability. However, favoritism through payment terms could still be important. The reason is as follows: because it is more difficult to verify the object of a construction service and there is no clear delivery date, there is one extra dimension to favor firms through the 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 for which the delivery takes place continually and not on a single date. Even though we uncover no statistically significant effects for services and construction in Appendix Table B.2, we should interpret these findings with caution as measuring their speed of payment is more prone to measurement error.

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, we 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 terms to campaign donors change after an electoral reform that bans corporate political contributions. The firms that donated 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.[31]

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 dates and properly compensate firms for late payments.[32] Moreover, the results being larger in municipalities with low liquidity and larger payment delays provide an additional motive for policies aimed at preventing the build-up of government expenditure arrears, as chronic delays might incentivize the rent-seeking behavior we uncover in this paper.

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 with 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. We exploit 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 us 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.

[31] In this paper, we 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, such as in infrastructure projects, renegotiations are common and politicians can commit to renegotiating at better terms.

[32] As an example, a reform to the procurement law passed in 2021 in Brazil established that payments should be settled on a first-come-first-serve basis.

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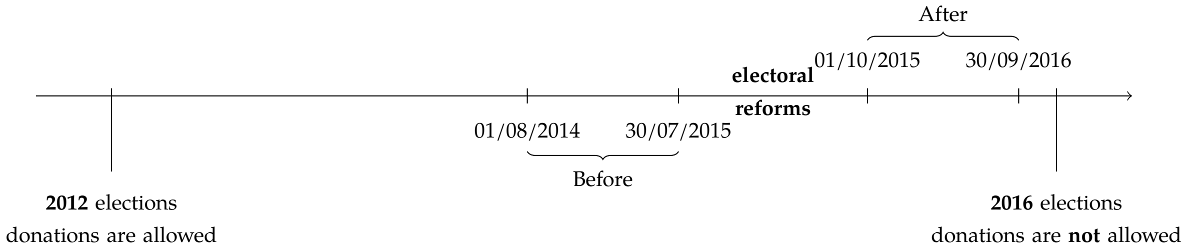
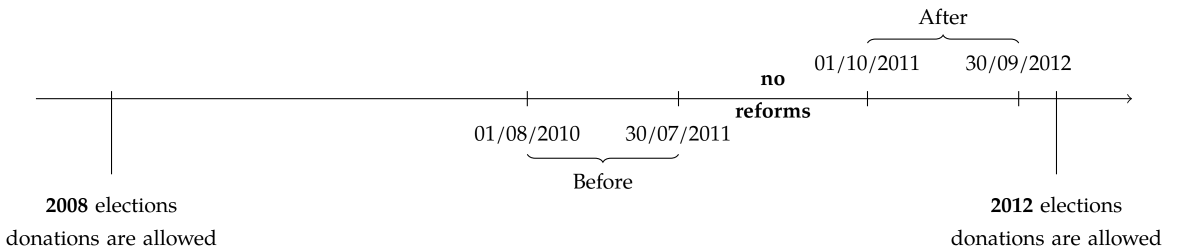
Figures

Figure 1: Estimation strategies



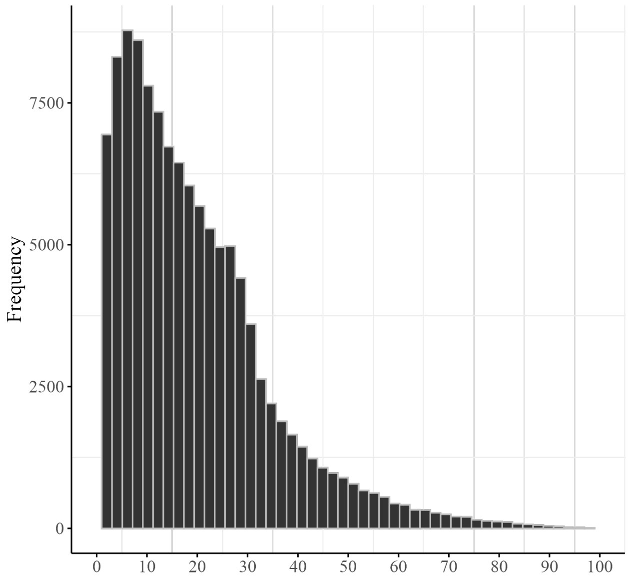
(a) Estimation around the reforms

(b) Estimation before the reforms in the same electoral cycle

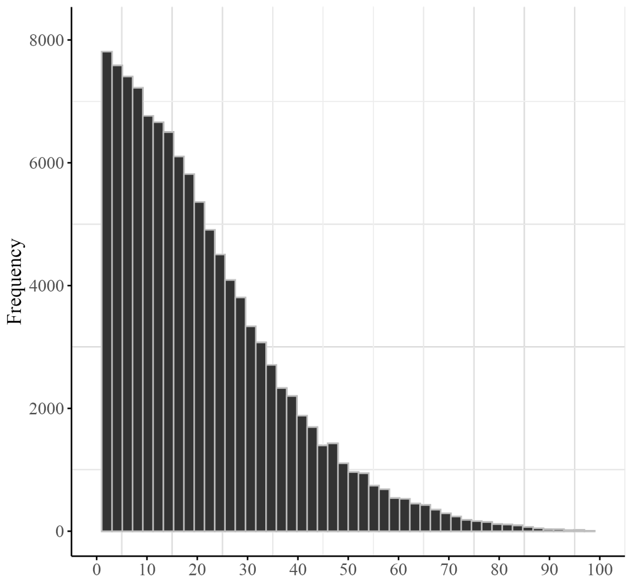


(c) Estimation in the previous electoral cycle

Figure 2: Histograms



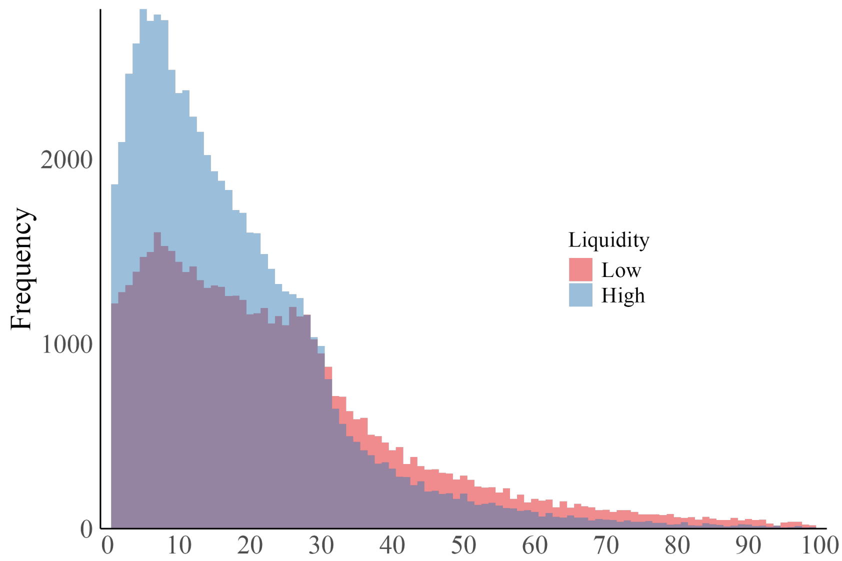
(a) Days between verification and payment



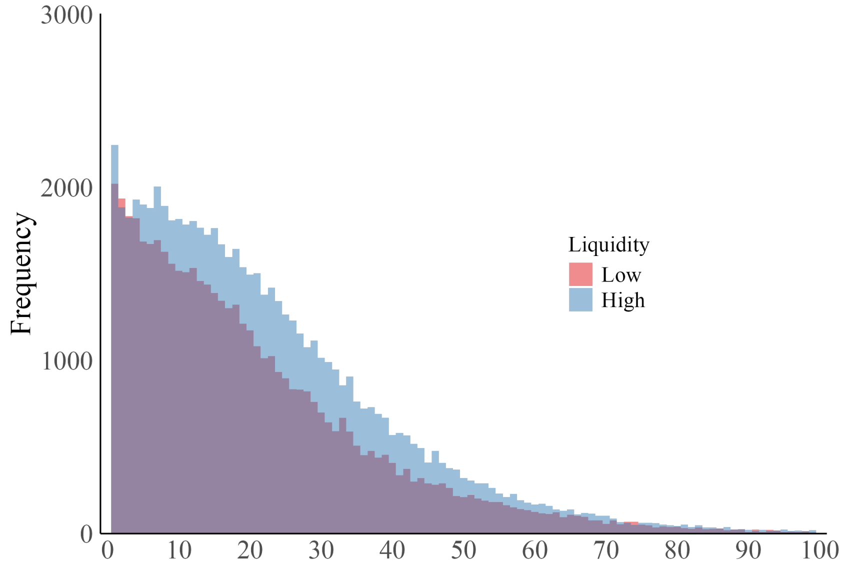
(b) Days between commitment and verification

Notes: The data are at the firm-municipality-time level. The sample is the one used to estimate the difference-in-differences model, where we only select firms that (i) sold to at least two municipalities during the period and (ii) sold in both the pre- and post-reform periods. The time periods are the pre- and post-reform periods.

Figure 3: Histograms: heterogeneity by the liquidity of local government



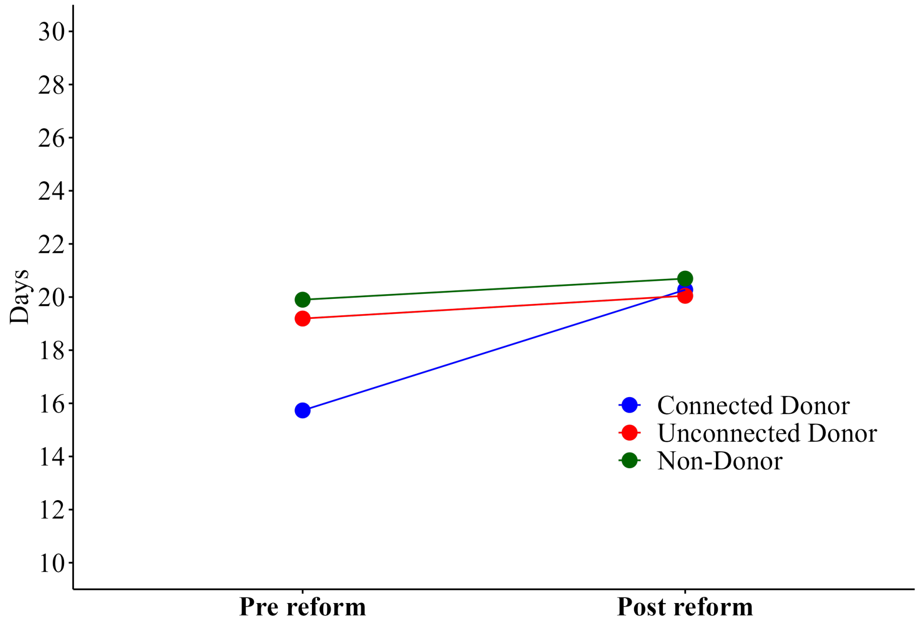
(a) Days between verification and payment



(b) Days between commitment and verification

Notes: The data are at the firm-municipality-time level. The sample is the one used to estimate the difference-in-differences model, where we only select firms that (i) sold to at least two municipalities during the period and (ii) sold in both the pre- and post-reform periods. The time periods are the pre- and post-reform periods. Municipalities with low (high) liquidity are those for which the liquidity measure, as of December 2015, is below (above) the median.

Figure 4: Days between verification and payment averages for different groups, before and after the reform



Notes: Pre- and post-reform averages for the days between verification and payment variable. Connected firms are those that donated to the mayor's party (that is, the party of the candidate that was elected) in the 2012 elections; unconnected-donor firms are those that donated to the party of candidates that were not elected in the 2012 elections; unconnected-non-donor firms are those that do not make campaign contributions.

Tables

Table 1: Descriptive statistics of main variables

		Full sample (1)	Estimation sample (2)	Estimation sample - pre (3)	Estimation sample - post (4)
Amount committed (BRL)	Mean	23,133	35,766	35,863	35,668
	SD	149,524	206,982	218,611	194,660
	p10	269	480	500	458
	p50	3,252	5,529	5,708	5,357
	p90	40,087	64,701	64,956	64,444
Days between commitment and verification	Mean	20.2	20.4	20.5	20.4
	SD	21.1	19.6	19.6	19.5
	p10	0.1	1.0	1.0	1.0
	p50	14.0	15.1	15.2	15.0
	p90	48.7	46.6	46.8	46.3
Days between verification and payment	Mean	18.5	20.3	19.9	20.7
	SD	20.7	20.4	19.9	21.0
	p10	1.0	1.9	1.7	2.0
	p50	12.4	15.0	14.8	15.1
	p90	41.9	43.7	42.7	44.8
Observations		750,621	248,826	124,413	124,413

Notes: The table presents descriptive statistics of the main variables: mean, standard deviation (SD), 10th percentile (p10), median (p50), and 90th percentile (p90). The data is at the firm-municipality-time level. The time periods are: the pre-reform period, which is the 12-month window that precedes the reform (August 1, 2014 - July 30, 2015), and the post-reform period, which is the 12-month window that follows the reform (October 1, 2015 - September 30, 2016). Column (1) refers to the full sample, and column (2) refers to the sample used to estimate the difference-in-differences model, where we only select firms that (i) sold to at least two municipalities during the period and (ii) sold in both the pre- and post-reform periods. Column 3 (4) refers to the sample used to estimate the difference-in-differences model, restricted to the pre-reform (post-reform) period.

Table 2: Descriptive statistics for firms and municipalities

Panel A: Firms	Connected	Unconnected: Donor	Unconnected: Non-donor
Days between commitment and payment	39.2	40.8	40.7
	(25.6)	(26.2)	(27.7)
Days between commitment and verification	21.2	21.2	20.4
	(19.1)	(18.6)	(19.6)
Days between verification and payment	18	19.6	20.3
	(17.7)	(19.9)	(20.5)
Number of municipalities firm sells to	13.3	10.1	5.4
	(39.1)	(24.6)	(12.7)
Total committed (1,000 BRL)	956	494	182
	(3,791)	(1,860)	(1,107)
Number of municipalities firm donates in	1.2	1.1	-
	(0.6)	(0.3)	-
Total donated to losers (BRL)	2,881	8,558	-
	(14,587)	(31,257)	-
Total donated to winners (BRL)	10,123	-	-
	(19,814)	-	-
Number of connections	1.1	-	-
	(0.4)	-	-
Number of firms	346	355	21,593

Panel B: Municipalities	All	Liquidity: Low	Liquidity: High
Liquidity, 2015	0.06	-0.02	0.17
	(0.17)	(0.07)	(0.17)
GDP per capita (1,000 BRL), 2015	22.4	23.4	25.8
	(20.6)	(19.2)	(24)
Population (1,000)	36.1	34.5	36.4
	(251.2)	(126.5)	(93)
Days between commitment and verification	19.1	18.1	20.1
	(10.6)	(10.4)	(10.7)
Days between verification and payment	21	23.8	18.3
	(11)	(11.1)	(10.2)
Total committed (1,000 BRL)	4,347	3,796	4,895
	(11,849)	(12,142)	(11,605)
Number of municipalities	2,778	996	1,037

Notes: This table presents the mean and standard deviation (in parentheses) of firm and municipality characteristics. The firms are those that enter our estimation sample. In Panel A, the data are at the firm level, while in Panel B, the data are at the municipality level. In both cases, the aggregation uses the monetary values of the operations as weights. The pre-reform period spans from August 1, 2014, to July 30, 2015, and the post-reform period spans from October 1, 2015, to September 30, 2016. Connected firms are those that donated to the mayor's party (that is, the party of the candidate that was elected) in the 2012 elections; unconnected-donor firms are those that donated to the party of candidates that were not elected in the 2012 elections; unconnected-non-donor firms are those that do not make campaign contributions. Reported donations correspond to the 2012 mayoral elections. Liquidity is defined as the ratio of (cash - accounts payable) to revenues. Municipalities are split into high- and low-liquidity groups using the median of the liquidity distribution in 2015. GDP per capita is in 1,000 BRL 2015 values.

Table 3: Main difference-in-differences results

	(1)	(2)	(3)	(4)
Panel A: Days between verification and payment				
Connected × Post	4.2***	2.2**	3.4***	2.7**
	(1.0)	(1.0)	(1.2)	(1.2)
Connected	-2.9***	-1.5**	-2.7***	-0.67
	(0.84)	(0.58)	(0.99)	(0.76)
Observations	573,593	573,593	248,808	248,808
R^2	2.6×10^{-5}	0.24	0.27	0.49
Mean dep. variable	19.3	19.3	20.3	20.3
Panel B: Days between commitment and verification				
Connected × Post	-1.2	-1.3*	-0.17	0.11
	(0.90)	(0.80)	(1.2)	(1.2)
Connected	-0.59	-3.5***	3.9***	-0.47
	(0.95)	(0.72)	(1.2)	(1.0)
Observations	573,593	573,593	248,808	248,808
R^2	7.8×10^{-6}	0.26	0.38	0.54
Mean dep. variable	21.6	21.6	20.4	20.4
Panel C: Log of the amount committed				
Connected × Post	0.02	-0.07	-0.08	-0.10
	(0.08)	(0.08)	(0.09)	(0.09)
Connected	1.1***	0.97***	1.0***	0.62***
	(0.11)	(0.10)	(0.10)	(0.10)
Observations	573,593	573,593	248,808	248,808
R^2	0.0008	0.10	0.47	0.53
Mean dep. variable	8.2	8.2	8.6	8.6
Municipality-time FE	No	Yes	No	Yes
Firm-time FE	No	No	Yes	Yes

Notes: The data are at the firm-municipality-time level. Regressions take the form $y_{fmt} = \alpha_{mt} + \alpha_{ft} + \beta Connected_{fm} \times Post_t + \gamma Connected_{fm} + \epsilon_{fmt}$, where y_{fmt} denotes the dependent variable. Connected$_{fm}$ is a binary variable indicating if firm f donated to the winning mayor's party in municipality m in the previous election, $Post_t$ is a binary variable indicating the post-reform period. Columns (1) and (2) restrict the sample to firms that sold in at least two different municipalities, while columns (3) and (4) are restricted to firms that sold in at least two different municipalities before and after the reform. Standard errors are clustered at the firm and municipality levels.

Table 4: Heterogeneity by the liquidity status of the local government

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Low liquidity				High liquidity			
Panel A: Days between verification and payment								
Connected × Post	6.0***	4.5**	6.4**	6.6**	1.2	-0.28	1.2	0.80
	(1.9)	(2.0)	(2.6)	(2.7)	(1.2)	(1.0)	(1.4)	(1.2)
Connected	-3.3*	-1.7	-2.7	-0.04	-2.1*	-0.51	-2.7**	-1.3
	(1.7)	(1.2)	(2.0)	(1.5)	(1.1)	(0.67)	(1.3)	(0.97)
Observations	228,704	228,704	94,624	94,624	289,418	289,418	124,146	124,146
R^2	0.0	0.20	0.31	0.49	0.0	0.28	0.29	0.53
Mean dep. variable	22.7	22.7	24.1	24.1	16.8	16.8	17.6	17.6
Panel B: Days between commitment and verification								
Connected × Post	-1.7	-1.4	-2.5	-2.0	-1.5	-2.0*	2.4	1.9
	(1.7)	(1.6)	(2.4)	(2.4)	(1.3)	(1.1)	(1.7)	(1.7)
Connected	0.45	-2.8*	7.4***	1.8	-0.88	-3.6***	0.48	-2.7*
	(2.0)	(1.4)	(2.1)	(2.0)	(1.2)	(1.0)	(1.9)	(1.4)
Observations	228,704	228,704	94,624	94,624	289,418	289,418	124,146	124,146
R^2	0.0	0.27	0.39	0.56	0.0	0.26	0.40	0.56
Mean dep. variable	21.2	21.2	20.0	20.0	22.9	22.9	21.6	21.6
Panel C: Log of the amount committed								
Connected × Post	-0.18	-0.28*	-0.07	-0.11	0.04	-0.04	-0.19	-0.19
	(0.15)	(0.15)	(0.17)	(0.17)	(0.12)	(0.12)	(0.13)	(0.13)
Connected	1.2***	1.0***	1.3***	0.88***	1.1***	0.88***	0.88***	0.46***
	(0.21)	(0.17)	(0.19)	(0.17)	(0.15)	(0.14)	(0.15)	(0.13)
Observations	228,704	228,704	94,624	94,624	289,418	289,418	124,146	124,146
R^2	0.0006	0.11	0.48	0.55	0.0006	0.11	0.48	0.55
Mean dep. variable	8.2	8.2	8.6	8.6	8.3	8.3	8.7	8.7
Municipality-time FE	No	Yes	No	Yes	No	Yes	No	Yes
Firm-time FE	No	No	Yes	Yes	No	No	Yes	Yes

Notes: The data are at the firm-municipality-time level. Regressions take the form $y_{fmt} = \alpha_{mt} + \alpha_{ft} + \beta Connected_{fm} \times Post_t + \gamma Connected_{fm} + \epsilon_{fmt}$, where y_{fmt} denotes the dependent variable. Connected$_{fm}$ is a binary variable indicating if firm f donated to the winning mayor's party in municipality m in the previous election, $Post_t$ is a binary variable indicating the post-reform period. In columns (1)-(4), the sample is restricted to municipalities whose liquidity is below the median of the liquidity measure as of December 2015. In columns (5)-(8), the sample is restricted to municipalities whose liquidity is above the median of the liquidity measure. In columns (1)-(2) and (5)-(6), the sample is restricted to firms that sold in at least two different municipalities. In columns (3)-(4) and (7)-(8), the sample is restricted to firms that sold in at least two different municipalities before and after the reform. Standard errors are clustered at the firm and municipality levels.

Table 5: Extreme delays

	(1)	(2)	(3)	(4)
Panel A: More than 30 days between verification and payment				
Connected × Post	0.06***	0.03	0.04*	0.03
	(0.02)	(0.02)	(0.03)	(0.03)
Connected	-0.04**	-0.02	-0.04	0.010
	(0.02)	(0.01)	(0.02)	(0.02)
Observations	573,593	573,593	248,808	248,808
R^2	1.5×10^{-5}	0.22	0.24	0.44
Mean dep. variable	0.19	0.19	0.21	0.21
Panel B: More than 45 days between verification and payment				
Connected × Post	0.06***	0.04**	0.04**	0.03
	(0.02)	(0.02)	(0.02)	(0.02)
Connected	-0.04***	-0.02***	-0.03**	0.002
	(0.01)	(0.009)	(0.01)	(0.01)
Observations	573,593	573,593	248,808	248,808
R^2	2.4×10^{-5}	0.16	0.21	0.36
Mean dep. variable	0.09	0.09	0.10	0.10
Panel C: More than 60 days between verification and payment				
Connected × Post	0.03***	0.02	0.04**	0.03*
	(0.01)	(0.01)	(0.02)	(0.02)
Connected	-0.03***	-0.02**	-0.03***	-0.008
	(0.007)	(0.006)	(0.01)	(0.009)
Observations	573,593	573,593	248,808	248,808
R^2	1.7×10^{-5}	0.11	0.19	0.31
Mean dep. variable	0.05	0.05	0.05	0.05
Municipality-time FE	No	Yes	Yes	Yes
Firm-time FE	No	No	No	Yes

Notes: The data are at the firm-municipality-time level. Regressions take the form $y_{fmt} = \alpha_{mt} + \alpha_{ft} + \beta Connected_{fm} \times Post_t + \gamma Connected_{fm} + \epsilon_{fmt}$, where Connected$_{fm}$ is a binary variable indicating if firm f donated to the winning mayor's party in municipality m in the previous election, $Post_t$ is a binary variable indicating the post-reform period. The dependent variable is a binary variable that takes the value one when the time between verification and payment is larger than 30 days (Panel A), larger than 45 days (Panel B), or larger than 45 days (Panel C). Columns (1) and (2) restrict the sample to firms that sold in at least two different municipalities, while columns (3) and (4) are restricted to firms that sold in at least two different municipalities before and after the reform.

Table 6: Heterogeneity by procurement method (São Paulo state only)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Competitive				Non-competitive			
Panel A: Days between verification and payment								
Connected × Post	10.6***	8.8***	10.8**	9.7**	5.4**	1.9	5.7*	4.0
	(3.6)	(3.2)	(4.6)	(4.7)	(2.1)	(2.0)	(3.1)	(2.9)
Connected	-5.9*	-5.2***	-5.4	-3.9*	-1.4	1.2	0.18	3.3*
	(3.1)	(1.9)	(3.5)	(2.2)	(1.8)	(0.95)	(2.6)	(1.9)
Observations	80,376	80,372	39,717	39,712	159,418	159,416	71,610	71,610
R^2	9.9×10^{-5}	0.36	0.15	0.51	4.2×10^{-5}	0.22	0.30	0.49
Mean dep. variable	23.0	23.0	23.4	23.4	20.8	20.8	22.4	22.4
Panel B: Days between commitment and verification								
Connected × Post	3.7	1.4	4.4	4.1	0.19	0.42	1.1	2.0
	(4.0)	(4.0)	(3.5)	(3.3)	(2.1)	(1.9)	(2.4)	(2.2)
Connected	-0.57	-4.4*	2.3	-2.3	2.4	-1.8	5.8***	0.51
	(3.6)	(2.4)	(4.5)	(3.1)	(1.8)	(1.4)	(1.8)	(1.5)
Observations	80,376	80,372	39,717	39,712	159,418	159,416	71,610	71,610
R^2	1.5×10^{-5}	0.25	0.27	0.49	3.9×10^{-5}	0.32	0.40	0.58
Mean dep. variable	31.6	31.6	28.7	28.7	17.0	17.0	16.1	16.1
Panel C: Log of the amount committed								
Connected × Post	0.08	0.02	-0.06	-0.16	0.13	0.02	-0.09	-0.07
	(0.23)	(0.20)	(0.26)	(0.29)	(0.13)	(0.13)	(0.16)	(0.16)
Connected	1.7***	1.2***	0.97***	0.57**	0.60***	0.45***	0.92***	0.55***
	(0.30)	(0.23)	(0.25)	(0.24)	(0.12)	(0.10)	(0.19)	(0.12)
Observations	80,376	80,372	39,717	39,712	159,418	159,416	71,610	71,610
R^2	0.002	0.09	0.34	0.44	0.0004	0.11	0.40	0.48
Mean dep. variable	9.5	9.5	9.8	9.8	7.8	7.8	8.1	8.1
Municipality-time FE	No	Yes	No	Yes	No	Yes	No	Yes
Firm-time FE	No	No	Yes	Yes	No	No	Yes	Yes

Notes: The data are at the firm-municipality-time-procurement level. Regressions take the form $y_{fmt} = \alpha_{mt} + \alpha_{ft} + \beta Connected_{fm} \times Post_t + \gamma Connected_{fm} + \epsilon_{fmt}$, where y_{fmt} denotes the dependent variable. Connected$_{fm}$ is a binary variable indicating if firm f donated to the winning mayor's party in municipality m in the previous election, $Post_t$ is a binary variable indicating the post-reform period. In columns (1)-(4), the sample is restricted to commitments in which the supplier was selected through a competitive procurement method. In columns (5)-(8), the sample is restricted to commitments in which the supplier was selected through a non-competitive procurement method. Standard errors are clustered at the firm and municipality levels.

Table 7: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Analogous regression in the previous electoral cycle				Pre-reform period as the "post" period			
Panel A: Days between verification and payment								
Connected × Post	-1.6	0.24	-1.2	-0.94	-0.23	-0.65	-1.6	-1.1
	(1.1)	(1.0)	(1.4)	(1.3)	(0.70)	(0.67)	(1.0)	(1.0)
Connected	0.58	0.77	1.5	2.3*	-1.6**	-0.85	-0.34	1.1
	(1.3)	(0.91)	(1.6)	(1.3)	(0.75)	(0.55)	(0.97)	(0.83)
Observations	318,672	318,672	142,432	142,432	581,503	581,503	261,614	261,614
R^2	6.6×10^{-6}	0.29	0.28	0.53	2×10^{-5}	0.26	0.27	0.51
Mean dep. variable	17.2	17.2	17.8	17.8	18.2	18.2	19.2	19.2
Panel B: Days between commitment and verification								
Connected × Post	1.6	1.5	0.86	0.97	-1.7**	-1.3*	0.67	0.54
	(1.1)	(1.1)	(1.4)	(1.4)	(0.78)	(0.78)	(1.0)	(1.0)
Connected	2.0	-3.7***	3.1***	-1.9**	1.2	-1.9***	4.9***	0.22
	(1.2)	(0.81)	(1.1)	(0.83)	(0.96)	(0.71)	(1.1)	(0.86)
Observations	318,672	318,672	142,432	142,432	581,503	581,503	261,614	261,614
R^2	5.5×10^{-5}	0.30	0.39	0.56	4.8×10^{-6}	0.28	0.37	0.54
Mean dep. variable	20.6	20.6	19.6	19.6	21.4	21.4	20.8	20.8
Panel C: Log of the amount committed								
Connected × Post	0.11	0.15*	0.26**	0.17	-0.11	-0.13	0.02	0.02
	(0.10)	(0.09)	(0.11)	(0.10)	(0.08)	(0.08)	(0.09)	(0.09)
Connected	1.2***	0.85***	0.92***	0.51***	1.3***	1.1***	1.0***	0.65***
	(0.14)	(0.13)	(0.13)	(0.13)	(0.11)	(0.10)	(0.11)	(0.11)
Observations	318,672	318,672	142,432	142,432	581,503	581,503	261,614	261,614
R^2	0.001	0.11	0.47	0.53	0.001	0.10	0.46	0.52
Mean dep. variable	8.0	8.0	8.5	8.5	8.1	8.1	8.6	8.6
Municipality-time FE	No	Yes	No	Yes	No	Yes	No	Yes
Firm-time FE	No	No	Yes	Yes	No	No	Yes	Yes

Notes: The data are collapsed at the firm-municipality-time level. Regressions take the form $y_{fmt} = \alpha_{mt} + \alpha_{ft} + \beta Connected_{fm} \times Post_t + \gamma Connected_{fm} + \epsilon_{fmt}$, where y_{fmt} denotes the dependent variable. Connected$_{fm}$ is a binary variable indicating if firm f is connected in municipality m, $Post_t$ is a binary variable indicating the "post" period. In columns (1)-(4), the "pre" period runs from August 1, 2010 to July 30, 2011 and the "post" period runs from October 1, 2011 to September 30, 2012 (Figure 1c). In columns (5)-(8), the "pre" period runs from June 1, 2013 to May 31, 2014 and the "post" period runs from August 1, 2014 to July 30, 2015 (Figure 1b). In columns (1)-(4) ((5)-(8)), a firm f is connected if it donated to the winning mayor's party in municipality m in the 2008 (2012) elections. Standard errors are clustered at the firm and municipality levels.

Online appendix to *Speed of Payment in Procurement Contracts: The Role of Political Connections*

A Institutional setting, data and variables

A.1 Data sources, sample selection and variables

Data sources. The budget execution data originally come from State Audit Courts (*Tribunais de Contas dos Estados*, TCEs); the electoral data (including campaign contributions) come from the Superior Electoral Court (*Tribunal Superior Eleitoral*, TSE); and data on the balance sheet, revenues and expenses of municipalities come from the Ministry of Finance. We source the data from Data Basis platform (Dahis et al., 2022).

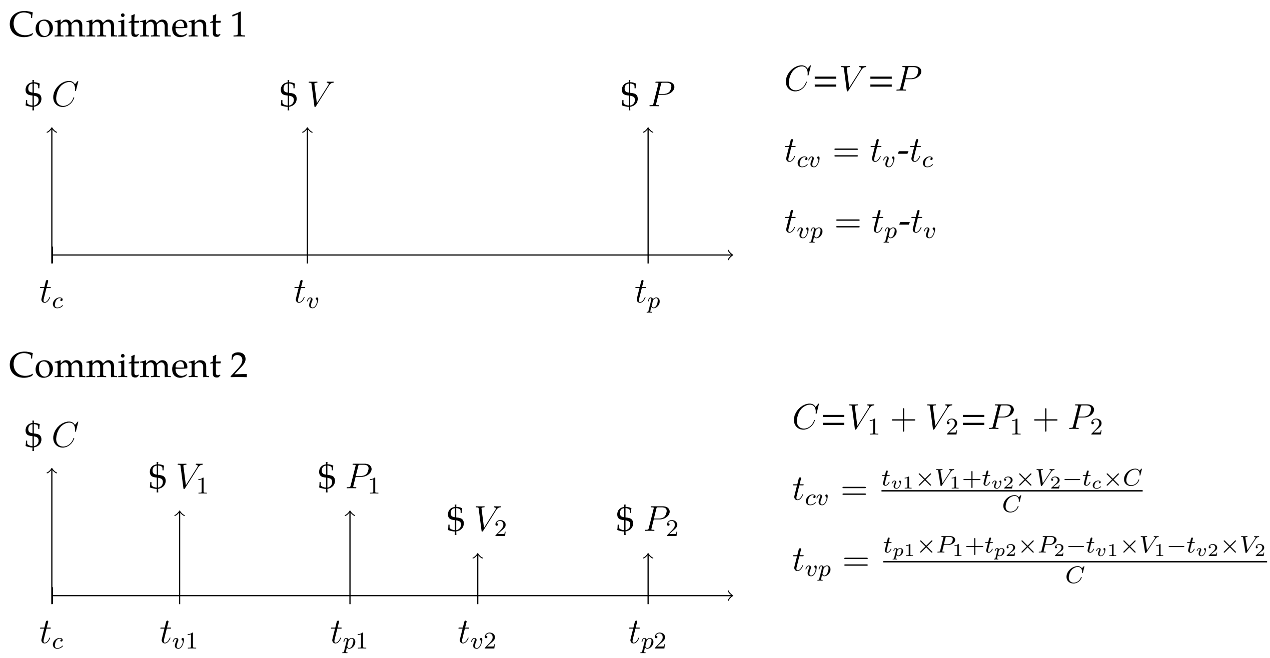
Sample selection. The budget execution data include all municipal expenses (salaries, pensions, interest payments, machines, equipment, food, office material, construction, IT services, etc.). Brazil adopts a budget classification system in which the economic classification of expenses (*elemento de despesa*) is comprised of 69 groups that are identified by a two-digit code. We restrict the data to three groups related to the purchase of goods and materials: consumption material (code 30), material for free distribution (code 32), and equipment and permanent material (code 52). The variable days between verification and payment is winsorized at the 99% level. The municipalities collect and treat the information and send it to the State Audit Courts yearly. As the quality of the data varies across municipalities, we exclude municipality-year pairs in which 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 budget execution stages were incorrectly recorded. The data only include fully-executed commitments, that is, commitments that are verified and paid within the fiscal year.

Fiscal variables. There was a change in the accounting reports in 2013. Therefore, we present the variables definitions for two periods, from 2007 to 2013 and from 2014 to 2016. The definitions are such that the variables are as comparable as possible in the two periods given the information available. From 2007 to 2013, we 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"); textitrevenues 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"). From 2013 to 2017, we 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"); and *revenues* as 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: contribuicões sociais - deducões da receita).

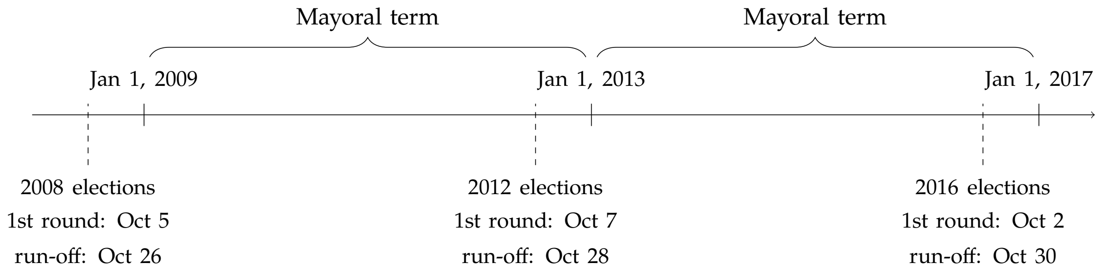
A.2 Days between verification and payment and days between commitment and verification: an example

Figure A.1: Illustration of the computation of the variables days between verification and payment (t_{vp}) and days between commitment and verification (t_{cv})



A.3 Electoral calendar: municipal elections

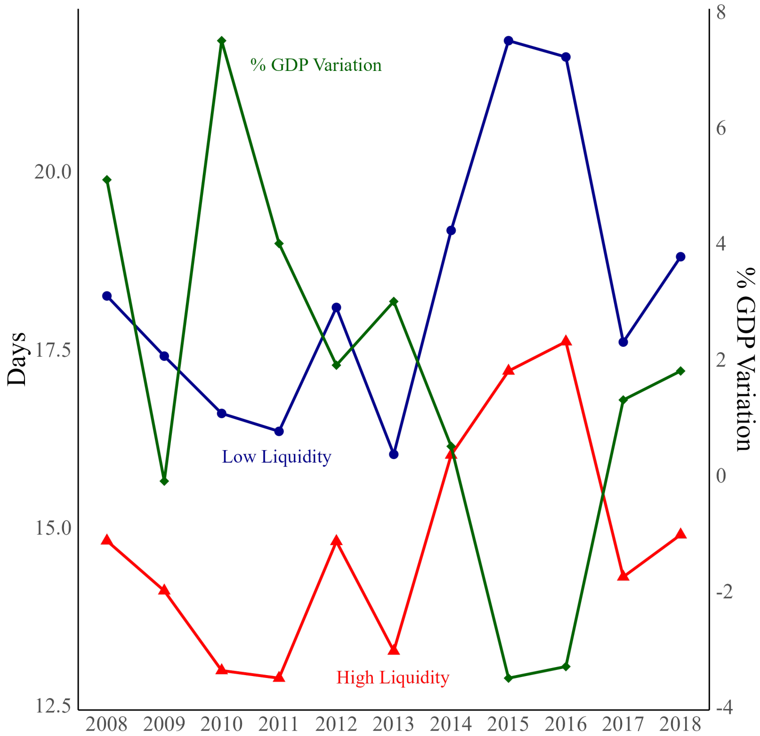
Figure A.2: Electoral calendar



B Additional descriptive statistics and results

B.1 Days between verification and payment: business cycle and high- versus low-liquidity variation

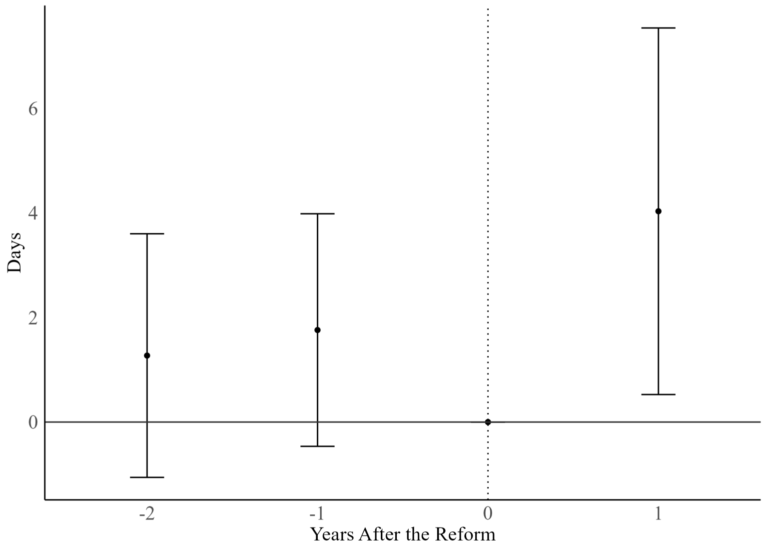
Figure B.1: Days between verification and payment: business cycle and high- versus low-liquidity variation



Notes: The data are at the municipality-year level. We compute averages by liquidity groups. Low-liquidity (high) municipalities are those whose liquidity is below (above) the median of the liquidity measure.

B.2 Days between verification and payment: pre-trends

Figure B.2: Days between verification and payment: pre-trends



Notes: The data are at the firm-municipality-year level. We estimate the following regression: $y_{fmt} = \beta_{-2}Connected_{fm} \times I_{t=2013} + \beta_{-1}Connected_{fm} \times I_{t=2014} + \beta_1 Connected_{fm} \times I_{t=2016} + \gamma Connected_{fm} + \alpha_{mt} + \alpha_{ft} + \epsilon_{fmt}$, where $I_{t=2013}$ denote a dummy variable that takes the value one when $t = 2013$ (analogous definitions for $I_{t=2014}$ and $I_{t=2016}$).

Table B.1: Days between verification and payment: pre-trends

	Time between verification and payment		
	(1)	(2)	(3)
Connected	-3.1***	-1.9***	-0.59
	(1.1)	(0.64)	(0.99)
2013 Dummy	-3.3***		
	(0.25)		
2014 Dummy	-1.3***		
	(0.17)		
2016 Dummy	0.64***		
	(0.18)		
Connected × 2013 Dummy	3.0***	1.5**	1.3
	(0.77)	(0.66)	(1.2)
Connected × 2014 Dummy	1.9**	2.2***	1.8
	(0.84)	(0.68)	(1.1)
Connected × 2016 Dummy	3.6***	2.7**	4.0**
	(1.1)	(1.1)	(1.8)
Observations	1,386,063	1,386,063	1,001,369
R^2	0.005	0.24	0.44
Dependent variable mean	17.4	17.4	18.3
Municipality-time fixed effects	No	Yes	Yes
Firm-time fixed effects	No	No	Yes

Notes: The data are at the firm-municipality-year level. We estimate the following regression: $y_{fmt} = \beta_{-2}Connected_{fm} \times I_{t=2013} + \beta_{-1}Connected_{fm} \times I_{t=2014} + \beta_1 Connected_{fm} \times I_{t=2016} + \gamma Connected_{fm} +$ fixed effects $+ \epsilon_{fmt}$, where $I_{t=2013}$ denote a dummy variable that takes the value one when $t = 2013$ (analogous definitions for $I_{t=2014}$ and $I_{t=2016}$).

B.3 Services and construction

Table B.2: Main difference-in-differences results: Services and construction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Services				Construction			
Panel A: Days between verification and payment								
Connected × Post	-1.1	-0.12	0.38	0.36	0.26	-1.4	1.7	8.0
	(0.93)	(0.83)	(1.3)	(1.3)	(2.7)	(2.7)	(4.0)	(8.4)
Connected	3.6***	3.0***	-1.6	-0.81	0.44	0.87	-0.03	0.09
	(1.1)	(0.88)	(1.4)	(1.3)	(1.5)	(1.9)	(3.7)	(7.3)
Observations	458,950	458,950	205,792	205,792	15,260	14,503	3,484	2,586
R^2	7.3×10^{-5}	0.16	0.40	0.50	1.1×10^{-5}	0.35	0.48	0.87
Dependent variable mean	13.6	13.6	13.4	13.4	12.6	12.7	12.4	12.6
Panel B: Days between commitment and verification								
Connected × Post	0.45	0.71	1.0	1.1	0.46	1.2	3.2	-3.1
	(0.73)	(0.71)	(1.0)	(1.0)	(3.2)	(3.3)	(4.5)	(10.4)
Connected	5.2***	1.4*	1.7*	-0.89	0.30	-2.3	-2.6	-7.9
	(1.0)	(0.83)	(0.99)	(0.94)	(2.6)	(2.2)	(4.3)	(9.3)
Observations	458,950	458,950	205,792	205,792	15,260	14,503	3,484	2,586
R^2	0.0003	0.18	0.40	0.50	5×10^{-6}	0.37	0.51	0.85
Dependent variable mean	13.1	13.1	11.7	11.7	22.8	22.9	20.7	21.5
Panel C: Log of the amount committed								
Connected × Post	-0.24***	-0.13	0.07	0.07	0.29	0.07	0.65**	-0.09
	(0.09)	(0.08)	(0.09)	(0.10)	(0.25)	(0.30)	(0.33)	(0.65)
Connected	1.6***	1.2***	0.78***	0.45***	1.9***	1.2***	0.86**	0.49
	(0.15)	(0.12)	(0.13)	(0.12)	(0.31)	(0.29)	(0.35)	(0.61)
Observations	458,950	458,950	205,792	205,792	15,260	14,503	3,484	2,586
R^2	0.001	0.10	0.57	0.62	0.007	0.45	0.74	0.93
Dependent variable mean	7.8	7.8	8.2	8.2	10.2	10.2	10.8	10.9
Municipality-time FE	No	Yes	No	Yes	No	Yes	No	Yes
Firm-time FE	No	No	Yes	Yes	No	No	Yes	Yes

Notes: The data are at the firm-municipality-time level. Regressions take the form $y_{fmt} = \alpha_{mt} + \alpha_{ft} + \beta Connected_{fm} \times Post_t + \gamma Connected_{fm} + \epsilon_{fmt}$, where y_{fmt} denotes the dependent variable. Connected$_{fm}$ is a binary variable indicating if firm f donated to the winning mayor's party in municipality m in the previous election, $Post_t$ is a binary variable indicating the post-reform period. Columns (1) and (2) restrict the sample to firms that sold in at least two different municipalities, while columns (3) and (4) are restricted to firms that sold in at least two different municipalities before and after the reform. Standard errors are clustered at the firm and municipality levels.

B.4 Sample Bias: estimation sample

Table B.3: Selection bias in difference-in-differences sample

		DiD Sample (1)	Firms that sell pre-reform only (2)
Amount committed	Mean	35,766	13,968
	SD	206,982	73,734
	p10	480	240
	p50	5,529	2,483
	p90	64,701	25,304
Days between commitment and verification	Mean	20.40	23.40
	SD	19.60	23.60
	p10	1	0
	p50	15.10	16.70
	p90	46.60	56.10
Days between verification and payment	Mean	20.30	18.20
	SD	20.40	21.10
	p10	1.90	0
	p50	15	12
	p90	43.70	42
	N	248,826	123,657

Notes: The data is at the firm-municipality-time level. The periods are two: the pre-reform period, which is the 12-month window that precedes the reform (August 1, 2014 - July 30, 2015), and the post-reform period, which is the 12-month window that follows the reform (October 1, 2015 - September 30, 2016). Column (1) refers to the sample used to estimate the difference-in-differences model. For this sample, we only select firms that (i) sold to at least two municipalities and (ii) sold in both the pre- and post-reform periods. Column (2) refers to firms that only sell before the reform, and therefore are not used in the main estimation.

B.5 Extreme delays

Table B.4: Probability of being paid after 30, 45, or 60 days

		Full sample (1)	Estimation sample (2)	Estimation sample - low liquidity (3)	Estimation sample - high liquidity (4)
30+ days	Mean	0.18	0.21	0.27	0.15
45+ days	Mean	0.09	0.10	0.14	0.06
60+ days	Mean	0.05	0.05	0.08	0.03
Observations		750,621	248,808	109,962	137,836

Notes: The data is aggregated at the municipality-time level. The periods are two: the pre-reform period, which is the 12-month window that precedes the reform (August 1, 2014 - July 30, 2015), and the post-reform period, which is the 12-month window that follows the reform (October 1, 2015 - September 30, 2016). Column (1) refers to the full sample. Column (2) refers to the sample used to estimate the difference-in-differences model. For this sample, we only select firms that (i) sold to at least two municipalities and (ii) sold in both the pre- and post-reform periods. Columns (3) and (4) split the sample of column (2) into two according to the median of the (municipal) liquidity measure as of December 2015.