The London School of Economics and Political Science

Essays in Financial Economics

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

Statement of inclusion of previous work

I can confirm that chapter 3 is a result of previous study for an MRes in Finance which I undertook at the London School of Economics and Political Science and completed in 2015.

Statement of conjoint work

I confirm that chapter 2 is jointly co-authored. I contributed 33% of the work for chapter 2.

I declare that my thesis consists of 38,000 words.

Acknowledgement

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Abstract

This thesis consists of three essays in financial economics. In the first chapter, I test for the existence of a new channel through which politicians can exchange favors with campaign donors: different payment periods in procurement contracts. I explore an electoral reform that bans corporate contributions. The reform partially breaks down the relationship between donors and politicians: firms that donate in the previous election can no longer commit to contributing with the same intensity in the next election. Using a within-firm difference-in-differences identification strategy, I find that the payment period to firms that donate to the coalition government increases by five days after the reform. I study the heterogeneity of this effect and find that it is larger in municipalities with low liquidity and for contracts allocated through competitive procurement methods. The results provide an explanation – preferential treatment after the bidding stage – for the persistent evidence of *quid pro quo* even in competitive auctions. Moreover, the results point to the importance of designing rules that curb discretion over payment periods.

In the second chapter, co-authored with Jesús Gorrín and José Morales, we study the trade effects of increases in violence following the Mexican Drug War. A focus on exports allows us to control for demand shocks. We compare exports of the same product to the same country of destination, but produced in municipalities with different exposure to violence after a close electoral outcome. We show that municipalities that are exogenously exposed to the Drug War experience a 45% decrease in export growth on the intensive margin. Large exporters suffer larger effects, along with exports of more complex, capital-intensive, and skill-intensive products. Finally, we provide evidence consistent with violence increasing marginal exporting costs.

In the the third chapter, I study tax and redistributive policies in a dynastic model that features borrowing constraints, occupational choice and preferences for bequests. Borrowing constraints arise from moral hazard. In the absence of taxes, individuals that start with different levels of wealth converge to different steady-states, and poverty traps may occur. The introduction of inheritance taxation creates a trade-off: on the one hand, they tighten borrowing constraints, and thus they de-

plete the short-run aggregate productivity of the economy; on the other hand, they can be an instrument to fight poverty traps, wealth inequality and, in some cases, maximize long-run aggregate productivity.

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Chapter 1

Procurement payment periods and political contributions: evidence from Brazilian municipalities

Bernardo Ricca[1]

1.1 Introduction

Governments have increasingly adopted procurement methods that foster competition.[2] Competitive procurement mechanisms are designed 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. For instance, campaign donors are more likely to win contracts in auctions with multiple tenders (Baltrunaite, 2018). One possible explanation is that some firms receive preferential treatment *after* the tendering process, which gives them an advantage at the bidding stage. I test for the existence of a particular dimension through which politicians can benefit firms that make campaign contributions: shorter payment periods.

[1]I thank Daniel Paravisini, Daniel Ferreira, Vicente Cuñat, Ashwini Agrawal, Juanita Gonzalez-Uribe, Dirk Jenter, Mike Burkart, Moqi Groen-Xu, Flavio Moraes, Lukas Kremens, Jesus Gorrin, Felix Vetter, Alexsandros Cavgias, and seminar participants at the LSE, Insper and Universidad de Los Andes for their helpful comments.

[2]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).

The notion of payment period, defined as the time between the delivery of a good or service 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. 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.[3] The importance of payment terms in the context of public contracts has also been acknowledged by governments. 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.[4] In addition to being relevant to firms, payment periods have an important characteristic: they are 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 unambiguous advantage that can be easily translated into a monetary value.

I study payment periods in public procurement contracts with local governments. In Brazil, government agencies can only pay their suppliers once the object of the 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 in the beginning or at the end of the 30-day period.[5] Late payments are common and suppliers are not properly compensated for delays.[6]

[3]See Barrot (2016), Barrot and Nanda (2018) and Breza and Liberman (2017).

[4]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.

[5]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.

[6]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.

I investigate whether this flexibility in speed of payment is a source of favoritism. It is not clear that this should be the case. In some circumstances, awarding the contract at a favorable price is the obvious way to help a donor. However, in cases in which the tendering process is truly competitive, 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 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 payments can also be interpreted as a haircut. Therefore, governments choose which firms bear the highest haircuts.

Using detailed 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). 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 explore whether political connections, measured by corporate political donations to the coalition government in municipal elections, are associated with speed of payment. The decisions of whether to donate, where to donate, and to which party are endogenous. Therefore, estimates of cross-sectional or panel regressions can only be interpreted as partial correlations.

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.[7] 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 coincide with an increasing anti-corruption sentiment and are initiated by demands of society rather than by politicians. In the case of Brazil, a large anti-corruption probe uncovered a widespread bribery scheme in which companies exchanged donations for public contracts. Several members of the business and political elite were prosecuted. Companies involved in the case had to pay sizeable fines and experienced financial distress. In such situations, stakeholders can withdraw from doing business with firms that are potential targets of the investigation.[8] Therefore, the reforms can coincide with changes in other variables that affect the prospects of politically connected firms. Regressions that try to assess the impacts of the reforms and do not account for time-varying shocks at the firm level can provide biased results.

To overcome this issue, I explore the fact that, in the last elections when donations were allowed, firms chose to donate in some municipalities but not in others. 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. 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. This strategy guarantees that the estimates capture the effect of the change in the relationship with the local politician that is caused by the reforms, and not the effect of changes in other non-observable variables that coincide with the reforms and affect differently donors versus non-donors. I show that, after the reforms, payment periods to connected

[7] 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.

[8] This withdrawal can happen for reputational reasons. For instance, a bank or a supplier does not want to be linked with a firm that could be charged for corruption in the future. But it could also happen for economic reasons. Potential targets of the investigation might have to pay large fines in the future, which could affect their solvency.

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.

I then study the heterogeneity of the effect across municipality characteristics. 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.[9] 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. 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.

[9]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.

This paper relates to a growing literature on trade credit.[10] Barrot and Nanda (2018) 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.[11] 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. He explores a reform that limits payment terms to at most 30 days. In a competitive environment, excess firm-specific liquidity risk cannot be incorporated into prices. Therefore, long payment terms increase liquidity risk to financially weaker firms. He finds that, following the restriction, the probability of default decreases by 25%. Moreover, he finds that the entry of small firms increases. These results, when applied to this setting, imply that more favorable payment terms to donors affect the ability of non-donors to compete. Breza and Liberman (2017) study the effects of a policy that constrains the maturity of trade credit that a subset of suppliers can extend to a large retailer. They find that trade with affected suppliers decreases in comparison to non-affected suppliers, and that vertical integration increases. The effects are larger for financially constrained firms. They also find evidence that firms use longer payment periods to assess the quality of products.[12]

While the trade credit literature studies the effects of changes in trade credit terms, this paper focuses on payment periods in a context where trade credit terms are relatively homogeneous. I highlight, for public procurement, one aspect of the relationship between the transaction parts – connections established though 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

[10]See Biais and Gollier (1997), Burkart and Ellingsen (2004), Costello (2014), Cuñat (2006), Fisman and Raturi (2004), Giannetti, Burkart and Ellingsen (2011), Klapper, Laeven and Rajan (2012), Murfin and Njorge (2015), Ng, Smith and Smith (2002), and Petersen and Rajan (1997), among others. See also Cuñat and Garcia-Appendini (2012) for a review of the literature.

[11]The definition of small varies per industry. In terms of number of employees, the upper limit varies between 100 and 1500 employees.

[12]This fact could challenge the main results in the following sense: after the reforms, when the relationship between donors and politicians breaks down, politicians have to worry about the quality of products supplied by donors and thus use stretched payment terms to assess product quality. However, this is not a major concern in this setting because I measure payment period as the time between payment and the date when the government acknowledges that the good was delivered according to contract terms.

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).[13]

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.[14] Finally, donations are regulated and there is an ongoing debate about the optimal design of such laws. This paper is closely related to Baltrunaite (2018), who explores a ban on corporate donations in Lithuania. She finds that donors' probability of winning contracts decreases by 5%. The effects do not come from sole-bid tenders; they are driven by auctions with multiple tenders. The results suggest that *quid pro quo* is present even in competitive auctions. She proposes two explanations: governments inhibit participation in auctions by imposing restrictions that drive away competitors, or there is leakage of information about competitors' bids. I propose an alternative explanation: preferential treatment in payment periods. I also highlight the necessity of employing within-firm estimations when trying to assess the impact of this type of reform.

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.[15] These benefits are usually associated with an increase in

[13]This reasoning implicitly assumes that it is costly for the politician to form a new relationship with another firm.

[14] The evidence about whether donations are value-enhancing is mixed. See Aggarwal, Meschke, and Wang (2012), Akey (2015), Cooper, Gulen and Ovtchinnikov (2010), and Fowler, Garro, and Spenkuch (2017).

[15]See Khwaja and Mian (2005), Brogaard, Denes and Duchin (2016), Li, Wang and Zhou (2008),

firm value and performance.[16] In the context of Brazil, Claessens, Feijen and Laeven (2008) show that campaign contributions are associated with more bank financing, and Arvate, Barbosa and Fuzitani (2018) show that campaign contributions are linked to more government contracts.

1.2 Empirical setting

1.2.1 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

Schoenher (2018), Goldman, Rocholl and So (2013), Faccio, Masulis and McConnell (2006), and Fisman and Wang (2015).

[16] See Cingano and Pinotti (2013), Acemoglu, Johnson, Kermani, Kwak and Mitton (2017), Amore and Bennedsen (2013), Bunkanwanicha and Wiwattanakantang (2009), Fisman (2011), Faccio (2006), Ferguson and Voth (2008), Duchin and Sosyura (2012), Jayachandran (2006), and Goldman, Rocholl and So (2009), and Tahoun (2014).

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 1.1):

- Authorization: agencies are authorized to commit resources according to the 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.[17] 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).

[17]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.

1.2.2 Public procurement

Government agencies can employ different methods to procure goods and services.[18] 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.[19] The regulation establishes a threshold to define small. For products and services, the threshold is BRL 8,000, while for construction it is BRL 15,000.[20] 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 1.9 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.[21]

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.

[18]Law 8.666 - the Public Procurement Law - contains most of the public procurement regulation.

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

[20]In 2018, the limits changed. Decree 9.412, 18/06/2018.

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

1.2.3 Municipal elections and electoral reforms

Municipal elections are held every four years in Brazil (see Figure 1.2 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. The council is elected in an open-list proportional representation system.[22] 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 de-

[22]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.

clared 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.[23] 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 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 1.3 summarizes the electoral changes.

1.2.4 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 ordi-

[23] Law 13.165, 29/09/2015.

nary commitments. For non-ordinary commitments, I weight each operation by its monetary value (see Figure 1.4 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.[24] 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 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.

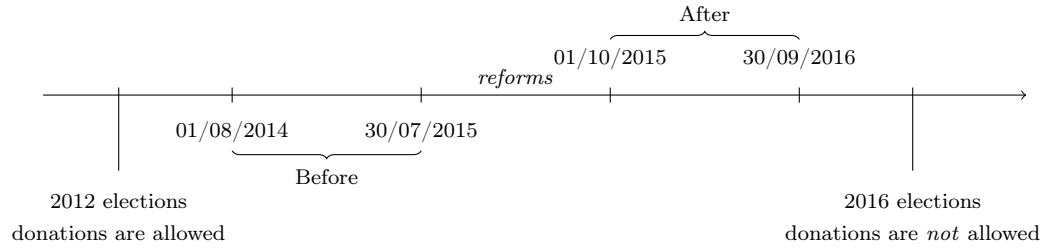
The Brazilian Institute of Geography and Statistics (IBGE) provides municipality characteristics, such as geographical area, GDP, literacy rate and population. The Ministry of Labor provides data on the number of employees at the establishment level.

1.2.5 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[25]:

[24] Balance sheet information as of December 31. The fiscal year runs from January 1 to December 31.

[25] 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.



I implement the following regression specification

$$\Delta y_{fm} = \alpha + \beta \mathbb{1}_{fm} + \alpha_f + \alpha_m + \epsilon_{fm} \tag{1.1}$$

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.[26] The variable Δy_{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. This is a key advantage of the setting. 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.

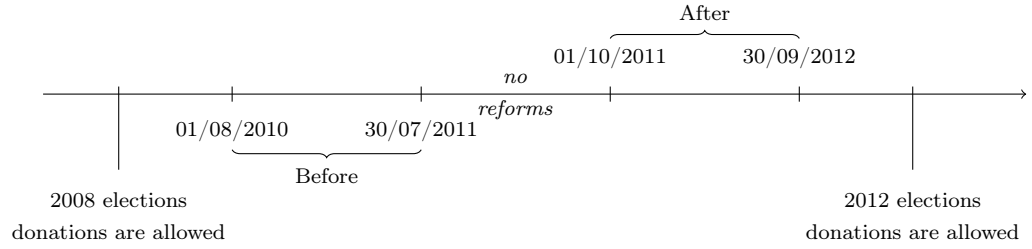
I analyze the data at the firm-municipality level to increase the sample size, as I need to observe contracts of the same firm in a given municipality before and after the reforms. However, this approach does not take into account that firms could select themselves into different procurement methods 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

[26] I also report results for a stronger form of connection: donation to incumbent's party in the previous election.

$$\Delta y_{fmj} = \alpha + \beta \mathbb{1}_{fm} + \alpha_f + \alpha_m + \epsilon_{fmj} \tag{1.2}$$

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.

1.3 Descriptive statistics

First I collapse the data at the municipality-year level using as weights the amount committed.[27] Table 1.1 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. In the end of 2014, the the country entered in 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.[28] The recession has a large

[27]If, in a given year, a municipality m has N_m commitments indexed by $c = 1, ..., N_m$ 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

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

[28]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 govern-

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 1.5, 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 1.12 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 1.10 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.

Next, I collapse the data at the firm-municipality-year-product-procurement method level.[29] 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 1.13 in the appendix for details). Throughout the paper, unless explicitly stated, when I refer to a connected firm it means that it is *connected (be-*

ment 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 that I find in Brazil. See Intrum Justitia (2017).

[29] If a firm f at municipality m, year y, product p, procurement method j, has N_{fmypj} commitments indexed by $c = 1, ..., N_{fmypj}$, each with value C_{fmypjc} and time to pay (or time to deliver) t_{fmypjc}, then

$$t_{fmypj} = \frac{\sum_{c=1}^{N_{fmypj}} C_{fmypjc} \times t_{fmypjc}}{\sum_{c=1}^{N_{fmypj}} C_{fmypjc}}$$

fore). 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 1.2 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 1.3, I present descriptive statistics for competitive and non-competitive procurement. The time between verification an 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 and verification is larger for competitive procurement, possibly reflecting the size of the orders. Figure 1.6 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 1.4 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 1.14 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 1.15, 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.

1.3.1 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):

$$y_{fmpj} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fmpj} \tag{1.3}$$

where $\mathbb{1}_{fm}$ is a dummy variable that takes the value 1 if the 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 1.5 provides the time-series variation of β'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 1.16 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 included, coefficients are not statistically significant and there is no clear change in 2016. Table 1.17 reports results for the *connection (after)* measure until 2012. The magnitudes of the correlation are larger for this measure.

1.3.2 Parallel trends

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

$$y_{tfmpj} = \sum_{t=2009}^{2017} \beta_t \mathbb{1}_{fmt} \times \mathbb{1}_t + \alpha_{ft} + \alpha_{mt} + \alpha_{fm} + \alpha_p + \alpha_j + \epsilon_{tfmpj}$$

where 2008 is the baseline year (the category excluded from the interaction $\mathbb{1}_{fmt} \times \mathbb{1}_t$), y_{tfmpj} denotes time between verification and payment of firm f, in municipality m, through procurement method j, in year t, of product p. The variable $\mathbb{1}_{fmt}$ 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. α_{ft} denotes firm-year, α_{mt} municipality-year, α_{fm} firm-municipality, α_p product, and α_j procurement method fixed effects. Standard errors are clustered at the firm-year and municipality-year levels.

Figure 1.7 shows the results. From 2009 to 2015, the year of the reforms, the β'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.

1.4 Results

1.4.1 Time between verification and payment

Table 1.6 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.[30] 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 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 1.6 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.[31] The effect for municipalities with high liquidity is not statistically significant and similar in magnitude to the effects obtained around the reforms.

Next, I estimate Equation 2, in which I implicitly control for procurement method fixed-effects. Table 1.7 provides the results. In the specification with firm and

[30]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.

[31]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.

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 auctions 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 1.18 and 1.19 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.[32] 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.

[32]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, Malitz and Ravid (1993).

1.4.2 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 committed. I estimate Equations 1 and 2 for changes in log of amount committed. The estimation of the effected 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 1.8 and 1.9 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 1.10 and 1.11 show that estimates are slightly larger around the reform in comparison to baseline estimates. However, in both cases estimates are not statistically significant.

1.4.3 Discussion

What is the economic significance of the results? The largest estimate is 12 days. Barrot and Nanda (2018) find that a 15-day reduction in payment periods causes an increase of 1.7% in employment. 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.[33] Figure 1.8 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.[34] 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,

[33]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.

[34]I perform the calculations with 4 working days.

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. 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 large donations come from firms from other sectors, mainly construction. A possible reason is that it is easier to 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 though 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.

1.5 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.[35] 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.

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

[35]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.

firms could shed light on important elements of their relationship.

1.6 Figures

Figure 1.1: **Budget execution**

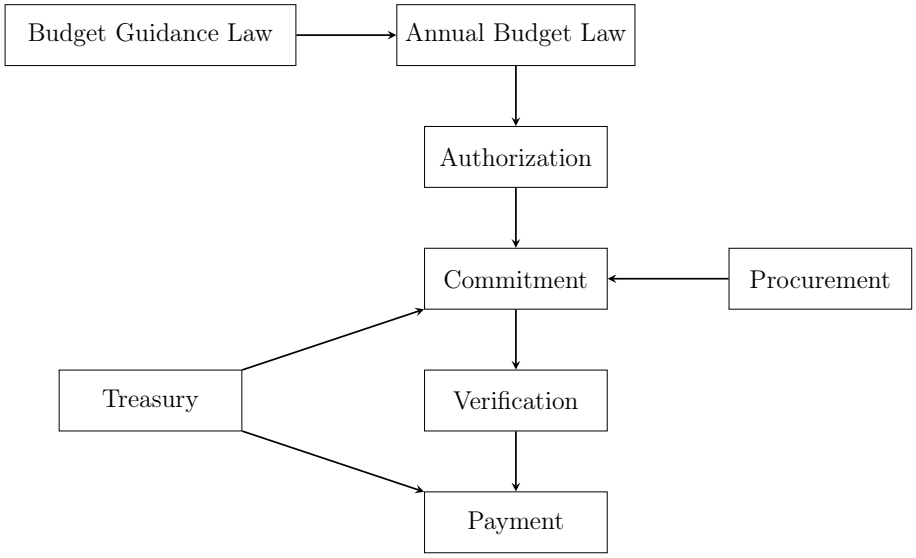


Figure 1.2: **Electoral calendar**

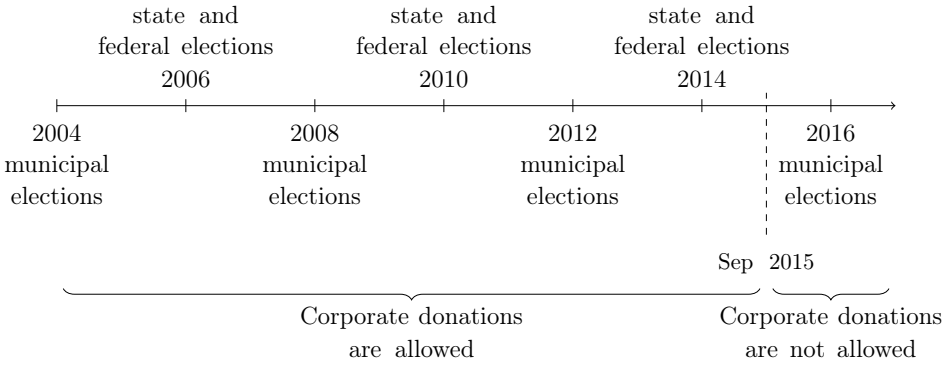
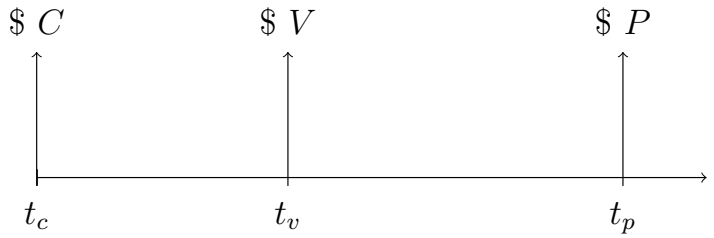


Figure 1.3: **Electoral reform**

	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 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

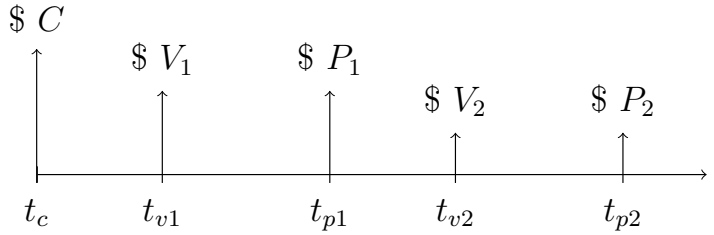
Figure 1.4: **Ordinary and non-ordinary commitment**

Ordinary commitment



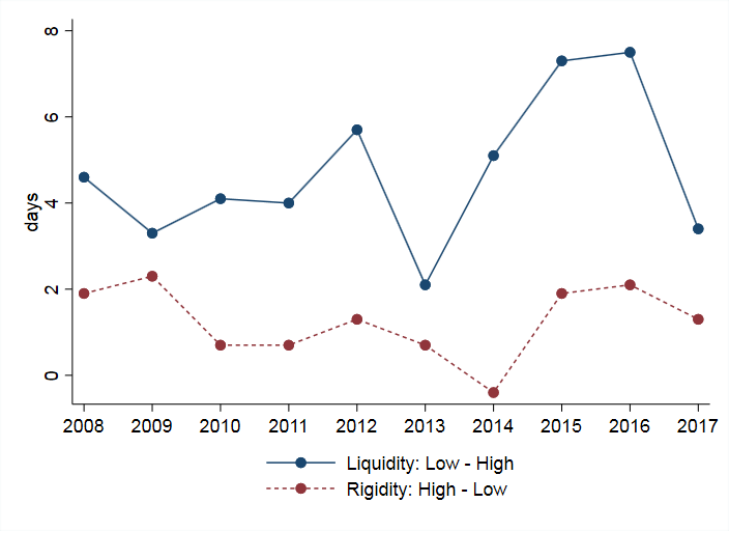
C=V=P
Time to deliver= t_v-t_c
Time to pay= t_p-t_v

Non-ordinary commitment



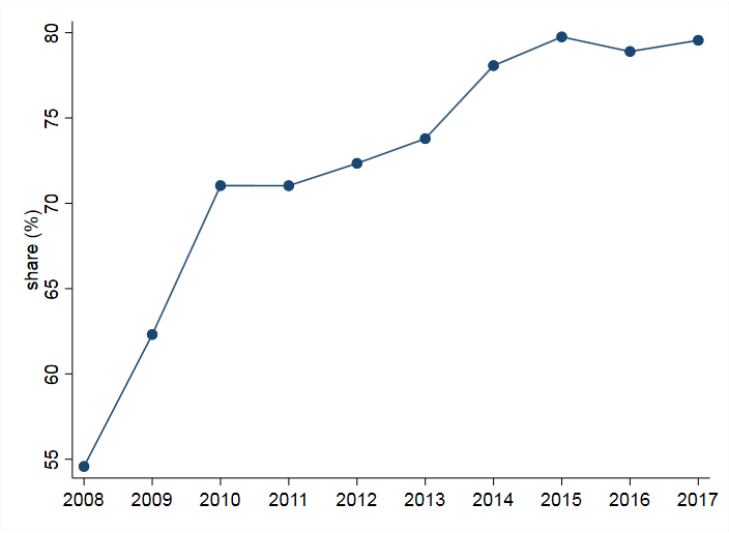
C=$V_1 + V_2$=$P_1 + P_2$
Time to deliver= $= \frac{t_{v1} \times V_1 + t_{v2} \times V_2 - t_c \times C}{C}$
Time to pay= $= \frac{t_{p1} \times P_1 + t_{p2} \times P_2 - t_{v1} \times V_1 - t_{v2} \times V_2}{C}$

Figure 1.5: **Mean of time between verification and payment: relationship with (lagged) fiscal variables**



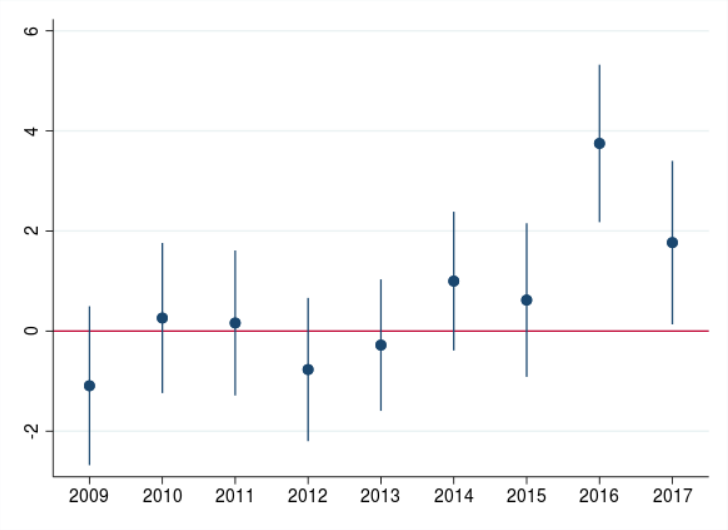
Notes: The data is aggregated at the municipality level using monetary values as weights. For each year, I split the sample into two groups using the medians of the liquidity measure. I repeat the procedure for the budgetary rigidity measure. I then compute the mean of time between verification and payment for each group. I compute liquidity as follows: (cash - accounts payable) / revenues. The budgetary rigidly 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.

Figure 1.6: **Share of amount committed through competitive procurement**



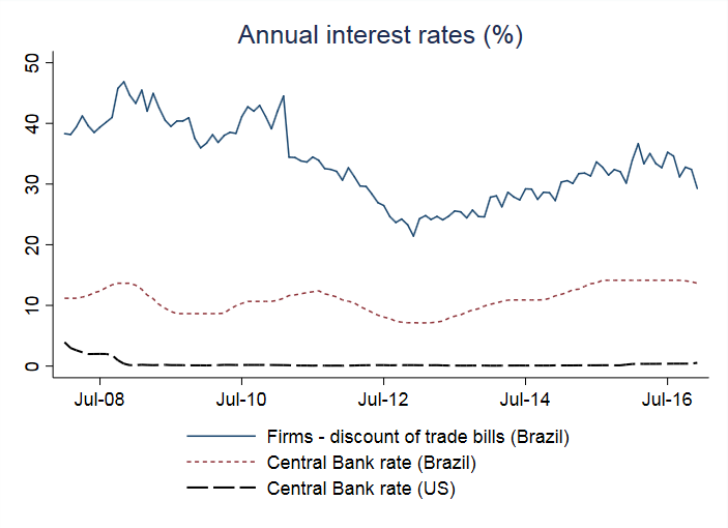
Notes: A procurement method is non-competitive if there is no tendering process (direct contracting). Otherwise, if there is any sort of tendering process, the procurement method is classified as competitive.

Figure 1.7: **Parallel trends, time between verification and payment**



Notes: The figure plots the β_i's and the respective 5% confidence intervals estimated from the following regression: $y_{tfmpj} = \sum_{i=2009}^{2017} \beta_i \mathbb{1}_{fmt} + \alpha_{ft} + \alpha_{mt} + \alpha_{fm} + \alpha_p + \alpha_j + \epsilon_{tfmpj}$. The sample starts in 2008, which is the baseline year in the regression. y_{tfmpj} denotes time between verification and payment of firm f, in municipality m, through procurement method j, in year t, of product p. The variable $\mathbb{1}_{fmt}$ 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 1.8: **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 theses 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.

1.7 Tables

Table 1.1: **Descriptive statistics - municipality variation**

Commitment	GDP	Time between commitment and verification			Time between verification and payment		
year	growth	mean	sd	med.	mean	sd	med.
2008	5.1%	37.2	19.0	37.9	16.8	9.2	15.6
2009	-0.1%	35.5	17.8	36.2	16.9	9.6	15.8
2010	7.5%	38.1	18.9	38.5	18.5	10.4	17.0
2011	4.0%	37.1	19.6	37.2	17.8	9.4	16.6
2012	1.9%	38.1	20.5	39.0	20.3	11.1	19.3
2013	3.0%	35.6	18.8	36.5	17.0	8.3	16.3
2014	0.5%	38.5	20.0	37.9	18.7	10.0	17.7
2015	-3.6%	33.5	19.2	31.9	24.8	12.1	23.6
2016	-3.5%	33.5	18.3	32.6	27.1	13.3	25.5
2017	1.0%	31.7	17.5	29.5	21.77	9.72	20.81
Commitment	Federal primary	Liquidity			Budgetary rigidity		
year	surplus (%GDP)	mean	sd	med.	mean	sd	med.
2008	2.3%	0.13	0.18	0.08	0.44	0.06	0.44
2009	1.2%	0.12	0.19	0.09	0.46	0.08	0.46
2010	2.0%	0.14	0.20	0.08	0.45	0.07	0.46
2011	2.1%	0.13	0.19	0.08	0.46	0.07	0.46
2012	1.8%	0.10	0.20	0.05	0.48	0.07	0.48
2013	1.4%	0.10	0.18	0.06	0.48	0.07	0.49
2014	-0.4%	0.08	0.16	0.05	0.48	0.08	0.49
2015	-2.0%	0.05	0.17	0.03	0.49	0.07	0.49
2016	-2.6%	0.03	0.15	0.03	0.49	0.07	0.49
2017	-1.9%	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 rigidly 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 1.2: **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 1.3: **Descriptive statistics: competitive and non-competitive procurement**

Commit.	Time between verification and payment		Amount committed (in 2017 BRL)		Time between commitment and verification	
year	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 1.4: **Descriptive statistics - donations**

	Mean			Median	
	Non-donor	Donor	P-value	Non-donor	Donor
Panel A: Non-donor vs donor. *Population: suppliers in 2008 or 2012.*					
Number of workers	32.7	177.8	0	3	7
Amount committed (in 2012 BRL)	49013	270359	0	4707	11578
Observations	135259	3944			
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.*					
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			
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.*					
Panel C.1: Donation to incumbent's party?					
Value donation	5786	7950	0.04	853	1746
Amount commited	66557	199554	0	7670	15338
Observations	2194	896			
Panel C.2: Donation to the party of the next incumbent (winner)?					
Value donation	5052	9775	0	900	1500
Amount commited	92000	137506	0.02	8384	11025
Observations	2199	891			
Panel C.3: Donation to a party that has a candidate in the mayoral election?					
Value donation	2272	9054	0	573	1570
Amount commited	59424	134255	0	7241	10336
Observations	1203	1887			
Panel C.4: Party received donation?					
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 1.5: **Partial correlations: cross-sectional regressions**

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Commit. Year	Elect. year	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 β'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 1.6: **Difference-in-differences: time between verification and payment**

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
Panel A: all sample						
Connected	2.1**	1.6	5.0**	-1.1	-1.5*	1.0
	(1.0)	(1.0)	(2.2)	(0.8)	(0.8)	(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
Panel B: Low liquidity						
Connected	3.9**	4.0**	11.7***	-5.1***	-5.3***	-2.5
	(1.7)	(1.6)	(4.4)	(1.6)	(1.7)	(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
Panel C: High liquidity						
Connected	0.7	-0.3	2.7	0.9	0.5	1.9
	(1.2)	(1.1)	(1.7)	(0.8)	(0.7)	(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 Δy_{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 1.7: **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)		
Panel A: all sample						
Connected	1.1	0.8	3.4*	-1.4**	-1.7**	-0.6
	(0.9)	(0.8)	(2.1)	(0.7)	(0.8)	(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
Panel B: Competitive						
Connected	2.1	1.5	11.4***	-1.2	-1.7	-2.5
	(1.8)	(1.8)	(3.6)	(1.5)	(1.7)	(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
Panel C: Non-competitive						
Connected	0.7	0.2	0.3	-1.4*	-1.8**	0.4
	(1.1)	(0.9)	(2.2)	(0.8)	(0.9)	(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 $\Delta y_{fmj} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fmj}$, where Δy_{fmj} 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 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 1.8: **Difference-in-differences: amount committed**

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
Panel A: all sample						
Connected	-0.08	-0.08	-0.17	-0.13*	-0.10	-0.10
	(0.07)	(0.06)	(0.11)	(0.07)	(0.06)	(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
Panel B: Low liquidity						
Connected	-0.08	-0.07	-0.02	-0.24**	-0.24***	-0.41**
	(0.10)	(0.09)	(0.21)	(0.11)	(0.08)	(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
Panel C: High liquidity						
Connected	-0.10	-0.11	-0.25	-0.07	-0.03	0.03
	(0.10)	(0.09)	(0.17)	(0.08)	(0.09)	(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 Δy_{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 1.9: **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)		
Panel A: all sample						
Connected	-0.07	-0.07	-0.14	-0.10*	-0.06	0.07
	(0.06)	(0.06)	(0.11)	(0.06)	(0.06)	(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
Panel B: Competitive						
Connected	-0.02	-0.03	-0.03	0.04	0.04	-0.20
	(0.12)	(0.12)	(0.20)	(0.13)	(0.12)	(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
Panel C: Non-competitive						
Connected	-0.09	-0.09	-0.18	-0.15**	-0.10	0.12
	(0.07)	(0.07)	(0.15)	(0.07)	(0.07)	(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 $\Delta y_{fmj} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fmj}$, where Δy_{fmj} denotes changes in log of amount committed 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 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 1.10: **Difference-in-differences: time between commitment and verification**

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes around the reforms (Sep. 2015)			Baseline estimates (Sep. 2011)		
Panel A: all sample						
Connected	-2.2**	-1.8	1.3	-0.2	0.2	-2.4
	(1.1)	(1.1)	(2.2)	(1.2)	(1.4)	(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
Panel B: Low liquidity						
Connected	-1.4	-1.2	4.3	1.0	1.8	2.1
	(1.7)	(1.5)	(3.8)	(2.0)	(2.4)	(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
Panel C: High liquidity						
Connected	-3.1*	-2.6	-2.1	-1.0	-0.7	-4.8*
	(1.6)	(1.6)	(3.2)	(1.5)	(1.8)	(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 Δy_{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 1.11: **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)		
Panel A: all sample						
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
Panel B: Competitive						
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
Panel C: Non-competitive						
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 $\Delta y_{fmj} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fmj}$, where Δy_{fmj} denotes changes in time between commitment verification 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 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.

1.8 Appendix

1.8.1 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).

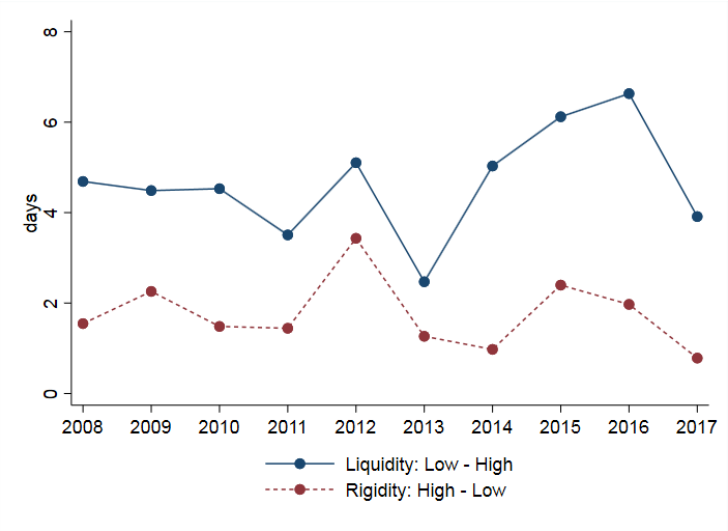
Fiscal variables. There is a change in the accounting reports in 2013. Therefore, I present the variables definitions for two periods, from 2007 to 2013 and from 2014 to 2017. The definitions are such that the variables are as comparable as possible in the two periods given the information available. 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 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); 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").

1.8.2 Figures

Figure 1.9: **Procurement meethods**

Purchasing method	Competitive	Characteristics	Contract size (for products)
Reverse auction *Pregão*	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 *Convite*	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 *Concorrência*	Yes	Open to any interested bidder. One bid per participant.	Any value.
Submission of prices *Tomada de preços*	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 1.10: **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: (cash - accounts payable) / revenues. The budgetary rigidly 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.

1.8.3 Tables

Table 1.12: **Liquidity and rigidity - relationship with observable municipality characteristics**

Commitment	Median split - liquidity			Median split - rigidity		
year	Low	High	H-L	Low	High	H-L
Panel A: Population						
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
Panel B: GDP per capita (in 2017 BRL)						
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
Panel C: Literacy rate						
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 rigidly 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 1.13: **Classification of firms used in Table 1.2**

Commitment	Year of donation (election)	Year of donation (election)	
Year	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 1.2. 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 1.14: **Number of observations**

			Number of firms			
			Connected		Not connected	
Commit.	Number of observations		Contracts	Contracts	Contracts	Contracts
year	Connected	Not connected	1 mun.	$>$ 1 mun.	1 mun.	$>$ 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 1.15: **Number of observations classified as connected, competitive x non-competitive**

Commitment	Number of obs		Amount committed (million BRL)	
Year	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 1.16: **Partial correlations: cross-sectional regressions**

Commitment Year	Election year	(1)	(2)	(3)	(4)
		Time between commitment and verification			
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 β'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 1.17: **Partial correlations: cross-sectional regressions, connection (after)**

Commitment Year	Elect. year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Time between verification and payment				Log (amount committed)			
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 β'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 (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 1.18: **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)		
Panel A: all sample						
Connected	3.5***	2.7**	5.9**	-1.4	-1.5	1.3
	(1.2)	(1.3)	(2.5)	(0.9)	(1.0)	(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
Panel B: Low liquidity						
Connected	6.6***	6.3***	12.0**	-5.1***	-5.3***	-2.6
	(2.0)	(2.0)	(4.7)	(2.0)	(2.0)	(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
Panel C: High liquidity						
Connected	0.6	-0.5	3.5*	0.7	0.6	2.9
	(1.4)	(1.4)	(2.0)	(1.0)	(0.9)	(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 Δy_{fmj} 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 1.19: **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)		
Panel A: all sample						
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
Panel B: Competitive						
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
Panel C: Non-competitive						
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 $\Delta y_{fmj} = \alpha + \beta \mathbb{1}_{fm} + controls + \epsilon_{fmj}$, where Δy_{fmj} 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.

Chapter 2

The impact of the Mexican drug war on trade

Jesús Gorrín, José Morales and Bernardo Ricca[1]

2.1 Introduction

Violence can affect firms in multiple ways: increasing fixed and variable production costs by disrupting input and labor markets, changing the consumption behavior of customers, among others. Violence is also highly regarded as an obstacle for development. Sustainable Development Goal 16 from the United Nations Development Program states:

> *Some regions enjoy sustained peace, security and prosperity, while others fall into seemingly endless cycles of conflict and violence. This is by no means inevitable and must be addressed. High levels of armed violence and insecurity have a destructive impact on a country's development, affecting economic growth and often resulting in long-standing grievances that can last for generations.*

[1]We are grateful to Ghazala Azmat, Vicente Cuñat, Melissa Dell, Daniel Ferreira, Juanita Gonzalez-Uribe, Ricardo Hausmann, Dirk Jenter, Tom Kirchmaier, Horacio Larreguy, Nathan Nunn, Daniel Paravisini, Vikrant Vig, seminar participants at LSE Finance, Warwick Macro and International Workshop, the Growth Lab Seminar at Harvard's CID, the Development Seminar at Harvard Economics, and the Political Economy of Development Seminar at Harvard's Government Department for helpful comments.

Studying how violence affects production is key to understanding its economic impact. This paper focuses on the effects of violence on supply. For this reason, we examine how crime and violence affect trade, and through which margin. In particular, does violence increase variable or fixed costs of exporting? We then analyze the heterogenous effects of violence across different sectors of the economy. Does violence have a stronger impact on sectors that the economic literature has identified as more important for future growth prospects?

A growing literature has documented the economic consequences of violence.[2] Yet little evidence exists on the specific mechanisms through which crime and violence affect economic outcomes. There is also lack of evidence about which sectors of the economy are more vulnerable. It is relevant for researchers to explain how crime and violence affect exports, potentially hampering growth opportunities. Additionally, understanding the relationship between violence and exporting activity is important for the design of policies aimed at attenuating the effects of crime.

A challenge to the existing literature is the endogeneity problem. Crime is correlated with local non-observable economic variables that affect firms' prospects. There is also a reverse causality concern. For instance, researchers have shown that crime reacts to trade-induced shocks.[3] Another issue is measurement error due to underreporting. Since underreporting is correlated with regional characteristics, it can cause biases.[4] We explore a setting that helps us address most of these concerns: the Mexican Drug War.

For several reasons, the Mexican Drug War is an appealing setting for the study of the economic consequences of crime. First, the anti-drug policy, launched in 2006 by president Felipe Calderón, is associated with a dramatic increase in violence. From 2006 to 2011, the homicide rate almost tripled, increasing from 7.9 per 100,000 people in 2007 to 22.9 in 2011. Second, data from surveys indicate that firms were negatively affected. For instance, according to the World Bank Enterprise Survey, between 2006 and 2009 the percentage of establishments paying for security increased from 41.5% to 59%, and the percentage of establishments experiencing losses as a

[2]Most papers focus on violence triggered by political conflicts or terrorism. Fewer papers study the consequences of violence triggered by property and drug-related crimes.

[3]See Dell, Feigenberg and Teshima (2018); and Dix-Carneiro, Soares, and Ulyssea (2018).

[4]See Soares (2004).

result of theft, robbery, or vandalism doubled from 15 to 30%. Third, the war was mainly led by one political party: the National Action Party (*Partido Acción Nacional, PAN*). The deployment of law enforcement tends to be correlated with trends in violence. However, as proposed by Dell (2015), the fact that PAN led the war allows us to employ an empirical strategy that uses close municipal elections as a source of exogenous variation in the intensity of the fight against drugs.

This paper focuses on the effects of violence on trade. Exports are an important part of the Mexican economy and a good measure of economic activity at the local level.[5] Exports are also less likely to be driven by local demand, which could be an additional challenge to the validity of our estimates.[6] We combine a regression discontinuity design using close elections of PAN mayors with controls for foreign demand shocks. By comparing exports of the same product to the same country of destination in municipalities with different levels of exposure to violence, we are able to estimate an effect of violence on the supply of exports that is unrelated to external demand factors.[7]

After electing a PAN mayor in a close election, municipalities experience an average 45% decrease in (log) export growth over 3 years, the duration of the mayoral term. This leads to an annual decrease of 15%. We use firm-level data on exporters from single municipalities and confirm a significant negative effect on export growth.[8] In this sample, annual export growth decreases by 22% at the firm level.

To answer through which mechanism violence affects exports, we differentiate across different margins of adjustment. Changes in trade patterns can emerge in two dimensions: the intensive margin, which consists in pre-existing firms changing their exported amounts, and the extensive margin, which consists in entry or exit

[5]The ratio exports/GDP in Mexico was 30.4% in 2005 (World Bank national accounts data). In the same year, this ratio was 15.2% in Brazil, 23.2% in Argentina, 16.8% in Colombia, 40.2% in Chile, and 26.8% in Peru.

[6]Local demand shocks can affect firms through the internal capital markets channel, that is, firms that sell to the domestic market and are financially constrained might be less able to export. On the other hand, Almunia, Antràs, Lopez-Rodriguez, and Morales (2018) argue that negative local demand shocks can cause an increase in exports because short-term marginal costs decrease.

[7]See Paravisini, Rappoport, Schnabl, and Wolfenzon (2014), who implement a similar strategy of comparing exports of the same product to the same destination to estimate the impact of bank credit shocks on trade.

[8]The micro-data does not allow us to differentiate exports coming from a particular municipality when firms have multiple plants in the same state. This restriction is applied to guarantee the correct assignment of the municipality where exports are produced.

of firms into different markets. If increases in violence manifest in the form of marginal cost increases, we should observe decreases in the intensive margin. If the effect comes from an increase in fixed costs of exporting, this would change exporting decisions at the extensive margin.[9] At the firm level, our results show significant effects only on the intensive margin. The main implication of this finding is that violence only seems to increase marginal costs of exporting.

We also explore the heterogeneity of the effect across product characteristics. We first use the complexity classification defined by Hausmann et al. (2013). We find that exports of more complex products – the ones that require more knowledge and complementary capacities to be produced – are more affected. Export growth of the more complex products drops by 65% over 3 years. The effect is not significant for less complex products. The fact that the effect is concentrated in knowledge- and coordination-intensive products can be relevant for future local economic performance as complexity correlates with future economic growth (Hausmann et al., 2013).

Other results are consistent with the hypothesis that violence has a larger effect on products that require higher specialization. Analyzing firm-level data, we also find larger negative effects of PAN wins on larger exporting firms. Furthermore, building on previous work in the literature, we construct product level measures of capital and skill intensity (Shirotori et al., 2010), and external capital dependence (Kaplan and Zingales, 1997). We show that export growth of products that rely more on long-term capital, skill intensity, and external sources of capital suffer a larger decrease. The types of firms that are more affected – large, capital- and skill-intensive – are also the ones that are more likely to gain from exporting in the first place.[10] We also find suggestive evidence of a decrease in foreign direct investment on municipalities where PAN won close elections.

As a criticism to our identification strategy, it can be argued that the negative effects on exports are caused by the PAN government itself, rather than violence. This is unlikely to be the case. In the absence of the Drug War, municipalities governed by PAN are likely to receive an economic benefit for several reasons. PAN is

[9] See Melitz (2003).

[10] See Mayer and Ottaviano (2008).

deemed a more market-friendly party. The federal administration is likely to benefit PAN municipalities, since they belong to the same party.[11] Potential spillovers to the control group attenuate the effects. All these biases underestimate the hypothesized negative effects of crime and violence on the economy. Because of these reasons, we could theoretically argue that our results offer a lower bound of the negative effects of violence on trade.

Nonetheless, we do not rely solely on a theoretical argument to pinpoint the likelihood that our estimates provide a lower bound. We provide empirical evidence. We perform a series of tests to show the results are unlikely to be driven by other PAN policies besides the war on drugs. First, because at the onset of the war, most of the violence concentrated in the northern regions of the country, we split the sample into two parts: north and south. As *ex-ante* violence is an important determinant of the deployment of law enforcement, a PAN municipality in the South was less likely to be affected by the war, but still experienced the results of policies that are specific to this political party. We find that the effect is only present in the northern regions. In the southern regions, a close PAN win is associated with an increase in export growth. Then, we use data collected by Coscia and Rios (2012) to define municipalities with *ex-ante* drug cartel activity. The drug war explicitly targeted these illegal organizations, and, therefore, was implemented mostly in places with pre-existing high cartel activity. The results are similar to the North vs South split: municipalities with pre-existing cartel presence experience a significant decrease in export growth after a close PAN win. In the absence of pre-existing cartel activity, close PAN wins are correlated with an increase in export growth. Finally, we run placebo tests using previous local elections. We find that, in the absence of the war on drugs, the effect of a PAN win on export growth is smaller and not statistically significant.

Our paper builds on the literature that investigates the negative economic effects of crime, violence and political conflicts. In an interesting contemporaneous paper, Utar (2018) shows that an increase in violence driven by the drug war in Mexico generates a decrease in production to local markets, but not a decrease in exports. Our

[11] Azulai (2017) shows, in the context of Brazil, that partisan connections distort the allocation of public goods towards localities with connected authorities.

main results conflict with this finding. An important difference between the papers is the identification strategy. While Utar (2018) controls for firm-level unobservable characteristics, in that paper the instrument for local violence is the interaction between cartel baseline presence, the choice of the governor to join the drug war, and the estimated price of cocaine. On the other hand, our paper exploits a different source of exogeneity that is unlikely to be related with unobserved local characteristics: close elections. Furthermore, her analysis excludes "maquiladoras", which are concentrated in the areas more likely to be affected by the drug violence (the North of Mexico), and are also an important source of Mexican exports. Moreover, the survey sample in Utar (2018) over represents larger firms. Our paper covers a larger sample of exporters using administrative export data that is more likely to represent smaller firms. On the positive side, because of the level of detail of her survey data, Utar (2018) is able to test for internal trade, and for internal characteristics of the workforce by plants of the same firm across different municipalities. Our paper does not test for heterogeneous effects on the workforce or changes in internal trade.

Other relevant papers in this literature include Ksoll, Macchiavello and Morjaria (2016), who study the effects of electoral violence on exports. They focus their analysis on one product – flowers – and they explore a different shock to violence. They find that export volumes decrease and that worker absenteeism is one of the drivers of the result. Pshisva and Suarez (2010) use firm-level data in Colombia to analyze the impact of kidnappings on corporate investment. They show that firm investment is negatively correlated with kidnappings that target firm owners and managers. Rozo (2018) uses firm level data and heterogeneous provision of government security in Colombia to show that violence decreases production through a decrease in output prices. She also finds negative effects on exit. Abadie and Gardeazabal (2003) explore the unilateral truce declared by ETA in 1998 and find that stocks of firms with a significant part of their business in the Basque Country showed a positive relative performance. Besley and Mueller (2012) find a negative relationship between killings and house prices in Northern Ireland. Similarly, Frischtak and Mandel (2012) provide evidence that the pacification of favelas caused an increase in house prices in Rio de Janeiro, Brazil. Other papers have studied the

negative effects of violence on local labor markets.[12]

We also relate to the literature that explores the effects of the Mexican Drug War. Dell (2015) finds negative effects of the war on local violence levels, while Utar (2018) finds negative effects on production for the national market. Our study advances the literature and finds negative consequences of the Drug War on local exports controlling for external demand factors, hence identifying a shock on the local capacity to supply foreign markets. We also provide further evidence on the mechanism through which the effects may operate. Violence acts as an increase in marginal costs. The negative effects are concentrated on larger exporters, and on exporters in more capital (human and physical) and finance dependent industries. Our suggestive evidence of negative effects of the Drug War on greenfield investments at the municipality level complements the work by Ashby and Ramos (2013), who document a negative relationship between crime and FDI at the state level in Mexico.

Our results suggest that policies that actively engage in violence against drug trafficking can have important unintended negative consequences for the economy. They seem to hamper local export growth of large exporters focusing on complex, capital-intensive, skill-intensive and finance-dependent products. The effect concentrates on the intensive margin, suggesting violence increases marginal exporting costs and not fixed costs of sustaining trading relationships. Finally, violence increases driven by stricter drug policy enforcement seem to hamper the capacity of localities to attract productive investments.

The paper continues as follows: Section 2 presents the empirical setting of the Mexican drug war and outlines our empirical strategy. Section 3 presents descriptive statistics. Section 4 outlines the effects of the Mexican drug war on violence. Section 5 presents results on exports at the municipality level. Section 6 presents results on exports at the firm level. Section 7 presents results on greenfield FDI at the municipality level. Section 8 concludes.

[12]See Adhikari (2013), Clemens (2017), Chamarbagwala and Morán (2011), Ihlanfeldt (2007).

2.2 Empirical setting

2.2.1 The Mexican political landscape and the Drug War

Throughout most of the twentieth century, Mexico experienced a non democratic rule with single party domination. For 71 years, the Institutional Revolutionary Party (*Partido Revolucionario Institucional, PRI*) ruled the country. Elections existed, but they were not competitive. In the 1990s, politicians from different parties started winning local elections, and, in 2000, Mexico elected its first non-PRI president since 1929. Some analysts suggest that, during PRI rule, there was a tacit agreement between the government and the drug traffickers that allowed cartels to operate as long as they complied with some rules (O'Neil, 2009). For example, cartels could not cause major disruptions to civilian life. Importantly, violence was contained. When other parties started winning elections, this relationship was shaken, as cartels had to negotiate with new incumbents from other parties. The election of Vicente Fox (PAN) as president in 2000 triggered some institutional changes. At the time, these changes were limited because the PAN was outnumbered in congress. It was only on 2 July 2006, when Felipe Calderón (PAN) was elected president, that changes intensified. Calderón governed from 1 December 2006 until 30 November 2012. As soon as he took office, he declared the war on drugs, sending the army to several provinces. The policy had tragic consequences. The arrest or assassination of a kingpin can cause a violent dispute for power. Members from the same organization or from rival cartels can exploit the weakening of the leadership to try to gain the control of the organization. Once in charge, new leaders have to assert their authority, in many cases through the use of force. Cartels also retaliated against politicians, police officers, armed forces, and journalists.

Increases in violence also affected civilian life. During Calderón's administration, the number of homicides increased by 160%, from 10,452 in 2006 to 27,213 in 2011 (Figure 2.1). Total homicides between 2006 and 2011 – as well as the absolute increase from the total between 2001 and 2005 – were concentrated in the northern regions of the country, closer to the US border (Figure 2.2). These are the regions where the main cartels operate the smuggling of drugs into the US. In reaction to the

crackdown, there is evidence that cartels began to diversify their activities into other crimes, such as extortion, human trafficking, oil theft, kidnapping, and robbery.

The main strategy of the anti-drug policy targeted cartel leaders. We gathered information for all confirmed deaths and arrests of highly ranked members of nine different Mexican cartels.[13] During the Calderón's presidency, governmental authorities carried out 13 killings and 54 arrests over 49 Mexican municipalities. These operations were mainly organized at the federal level, but coordination with municipal police was important.

Municipal presidents, the Mexican equivalent of mayors, are elected by popular vote. At the time of the war on drugs, Mexico already had competitive elections. All municipalities and states in Mexico control a police force. The municipality has the power to remove or appoint the municipal police chief. According to Article 115 of the Mexican Constitution, the municipal police has the responsibility to provide security and prevent crime. The important role of the mayor in the implementation of the Drug War can also be seen in practice. From 2006 until 2014, organized crime killed 63 former mayors or mayors in office.[14] Furthermore, municipal presidents have denounced extorsion from cartels.[15] Hence, it is reasonable to assume municipal elections are an important source of variation in the way the Drug War policy was implemented at the local level. This assumption is crucial for our identification.

Mexican parties are quite heterogeneous in their preferred social and economic policies. Among the major parties, PAN is more economically liberal and business oriented than its national opponents. As evidence of this, PAN was elected on an economic platform based on globalization and an increase in foreign investment (Krauze, 2006). Its main rival in the 2006 elections, the Party of the Democratic Revolution (*Partido de la Revolución Democrática, PRD*), is suspicious of free markets and globalization. Its other rival, the PRI, is more diverse. However, PAN was also more politically conservative. Especially during Calderón's presidency, PAN pursued heavier enforcement of anti-drug policies.

[13]See "Mexico Drug War Fast Facts" (`https://edition.cnn.com/2013/09/02/world/americas/mexico-drug-war-fast-facts/index.html`) and "Timeline of the Mexican Drug War" (`https://en.wikipedia.org/wiki/Timeline_of_the_Mexican_Drug_War`).

[14]Webpage: https://cnnespanol.cnn.com/2018/04/13/violencia-contra-los-alcaldes-en-mexico-mas-de-100-asesinados-desde-2006/.

[15]Webpage: `http://archivo.eluniversal.com.mx/nacion/165947.html`.

2.2.2 Data

We collect data on local electoral results from the Electoral Tribunals of each state. Local elections are held every three years, and usually elections in different states happen at different times. We focus on municipalities with elections in 2007 and 2008 because the terms of mayors elected in those years started and finished during Calderón's administration. Monthly data on homicides are from the National Institute of Geography and Statistics (*Instituto Nacional de Estadística y Geografía, INEGI*), available since 1990. Data on other types of crimes tend to be noisier due to underreporting. The issue of underreporting is severe in developing countries, where both the police and victims do not report all crimes. The most reliable source of crime data at the municipality level is The National Public Security System (*Sistema Nacional de Seguridad Pública, SNSP*), which started to publish data in 2011. Data on municipality characteristics are from the National System of Municipal Information (*Sistema Nacional de Información Municipal, SNIM*). Municipal data on exports are from the Atlas of Economic Complexity, which was developed at Harvard's Center for International Development.[16] We also use firm level data from the Mexican Tax Administration Service.[17]

2.2.3 Empirical strategy

Governments allocate their enforcement arms to regions where violence is increasing. Therefore a regression of homicides on some measure of law enforcement provides biased results. To address this challenge, we identify the effect of the drug war on violence using heterogeneity in electoral outcomes. First, we use the fact that one party, PAN, implemented stronger actions against the Mexican drug cartels. Thus, following Dell (2015), we use close elections of a PAN mayor as a source of exogenous variation in the intensity of the war on drugs. We focus the analysis on the 2007 and 2008 elections. The administration of mayors elected in those years started at the beginning of the war, and finished around its peak, in 2011. We

[16]Webpage: `http://complejidad.datos.gob.mx`. The original data comes from the Tax Administration Service (*Servicio de Administración Tributaria, SAT)*, Mexican's customs authority.

[17]Micro-level data is not publicly available. We accessed these data at Harvard's Center for International Development.

estimate the following specification

$$y_m = \alpha + \beta PANwin_m + \delta f(Margin_m, PANwin_m) + \epsilon_m \tag{2.1}$$

where m denotes municipalities, $PANwin_m$ is a dummy variable that takes value 1 when PAN wins, and $f(Margin_m, PANwin_m)$ is a polynomial on the vote margin and the dummy of PAN victory. We restrict the sample to municipalities where PAN won or lost by a margin smaller than 5%. Our contribution is to show the negative effect of violence on exports, and potential mechanisms behind this effect. However, since the source of exogeneity is given by the close PAN election, we first need to associate a PAN win with an increase violence. We follow the previous literature that has identified this effect. As in Dell (2015), we test the effect on homicides. Furthermore, following anecdotal evidence that cartels diversified their activities during the war, we also test the effects on other crimes. Because it is likely that crime is under-reported in smaller municipalities, we weight for population size. As suggested by Solon et al. (2015), we always report robust standard errors when weighting.

We then estimate "reduced-form" regressions using trade variables as dependant variables. The trade data are at the municipality-product-country of destination level, which allows us to control for external demand shocks by including product-destination dummies. Regressions take the form:

$$y_{mcp} = \alpha + \beta PANwin_m + \delta f(Margin_m, PANwin_m) + \alpha_{cp} + \epsilon_{mcp} \tag{2.2}$$

where y_{mcp} is the growth over the entire mayoral term (3 years) in exports of product p to country c in municipality m. More specifically, y_{mcp} is the log of the amount exported in the third year of the new administration, divided by the amount exported in the third year of the previous administration, when elections took place.[18] α_{cp} is a set of country of destination-product dummies, which allows us to control for foreign demand shocks, as in Paravisini et al. (2014).

We follow a similar procedure when using firm level data. To guarantee we

[18]The impact on annual log of exports growth is $\beta/3$.

identify the correct municipality from which exports of a given firm are produced, we restrict the data to firms that produce only from one municipality in a given Mexican state.[19] We then estimate a regression analogous to equation 2.2, but using firm-product-destination data. The municipality is identified by the location of the firm's plants.

2.3 Descriptive statistics

Table 2.1 reports summary statistics for municipalities that held elections in 2007 and 2008. Panel A shows socioeconomic characteristics of each Mexican municipality. In terms of population, municipalities are small. They have, on average, 35 thousand inhabitants compared to 100 thousand for the average county in the US. Furthermore, by 2006, compared to the US, Mexico was already a violent country. The American rate of 6 homicides per 100,000 pales in comparison to 11.7 in Mexico. However, compared to some Latin American countries, such as Brazil (26), Colombia (37), Venezuela (49), and El Salvador (58), Mexico's homicide rate was relatively small in 2006 (Berthet and Lopez, 2011). Although PAN was already an important party, only 27% of municipalities had an incumbent PAN mayor. Municipalities that elected PAN mayors (treatment group) are richer, less violent and have a higher share of the population aged between 16 and 29, in comparison to municipalities that did not elect PAN mayors (control group). However, once the sample is limited to municipalities where PAN won or lost by a small margin, the baseline characteristics are not statistically different in the treatment and control groups. Moreover, the loss of power caused by the restriction of the sample does not drive the results. For all significantly different variables in the unrestricted sample, we see smaller differences when we restrict to the 5% spread. The lack of difference on observables provides reassuring evidence in favor of the assumption of random assignment in close PAN victories.

[19]When firms have multiple plants in the same state, but in different municipalities, the data do not allow us to precisely determine which plant produced the exports. Therefore, in our main firm level regressions we decided to apply this restriction. If we do not restrict the sample and use firm level employees by municipalities to pro-rata assign firm exports to a particular municipality, we find consistent results.

Table 2.1 Panel B shows characteristics related to trade. Municipalities where PAN was elected tend to export more *ex-ante*. In general, the differences are not statistically significant for the unrestricted sample; for the sample that is restricted to municipalities facing close elections, all differences are statistically insignificant. In imports we observe a similar pattern.

Panel A of Figure 2.3 shows the geographical distribution of all municipalities in which elections took place in 2007 and 2008, while Panel B shows the geographical distribution of close elections in the same years. In the unconditional sample we can see that, even though PAN wins are not clustered, the losses are. We also see that PAN loses the majority of the municipal elections. However, when we restrict the sample to municipalities with close elections, the distribution of losses and wins are regionally dispersed. This is important for our identification for two reasons. First, this undermines the possibility that regional shocks, and not the treatment, drive our results. Second, it diminishes concerns of spillovers in control municipalities when restricting to the close elections sample.

2.4 Effects on violence

Panel A of Table 2.2 shows the results of estimation of Equation 2.1 when the outcome variable is the annual average of homicides over the new incumbent's term. Regressions are weighted by population size as of 2005. In the baseline WLS regression, a PAN victory causes an increase between 25 and 41 homicides per 100,000 population.

Panel A of Figure 2.4 replicates the finding of Dell (2005), which is crucial for our identification. This graph shows there is a discontinuous and significant effect of a close PAN election on cumulative homicides after the election.[20]

Panel B of Table 2.2 shows that a PAN victory is not associated with any pre-trend increase in homicides: municipalities where PAN won by a close margin do not experience higher homicides rates before the election. Panel C analyses the impact

[20]Our sample of close elections is slightly different from Dell (2015). In Dell's paper there is an additional restriction given by the availability of confidential data on drug transportation routes. In our paper this restriction is not necessary. However, even with this difference, the results are very similar in economic magnitude and in statistical significance.

on the absolute change in homicides: before and after the elections. A PAN win is associated with an increase of 37 in the homicide rate. In Panel D, we use the 2004 and 2005 elections to run a placebo test. Most mayors elected in this period finished their terms before the start of the war on drugs. The regression in Panel D helps us identify the effect of a PAN win on violence in the absence of the war on drugs. The results in this placebo show that close PAN wins are not associated with higher homicides in periods outside the war on drugs. This result suggests that a PAN victory in itself did not cause higher violence at the municipality level. It seems that the main driver of violence was the combination of a PAN victory with the implementation of the war on drugs.

Table 2.15 in the Appendix reports the same regressions when we restrict the sample to municipalities where PAN won or lost by a margin smaller than 3%. The results are consistent. Coefficients increase slightly and remain significant at 5%. Results are also similar when we increase the degree of the RD polynomial (Table 2.16 in the Appendix).

A natural question is whether the incidence of other types of crime also increased. It could be the case that homicides were concentrated in the war between rival cartels and the war between state and cartels. In this scenario, other crimes, such as robbery, kidnapping, and extortion, could remain unchanged. There are some limitations in documenting the effects on other crimes. Data is noisier due to underreporting. Furthermore, the most reliable source started publishing crime statistics per municipality only in 2011. Therefore, differently from homicides where we could test the impact over the whole term, we can only test the impact on the level observed in 2011, and we cannot run a placebo test with previous elections. Table 2.17 in the appendix reports results for six different types of crime. In general, other types of crime also increase, but the effects are not always statistically significant. Effects on extortion and robbery are statistically significant.

2.5 Change in municipalities' exports

2.5.1 Main results

In this section we combine the identification based on close municipal elections with disaggregated trade data at the municipal-product-destination level. Our focus on export growth combined with disaggregated data allows us to concentrate on supply effects.

This approach is different from the rest of the literature studying the effects of violence in the economy. Violence can have effects on both the demand and the capacity of firms to supply goods. For example, violence could potentially affect the economy by diminishing the likelihood or capacity of individuals to consume certain type of goods; it could disrupt production by increasing costs; it could could drive workers out of the affected locality. If we study local production instead of exports, we are not able to disentangle the effects on supply and demand. As we concentrate on exports, we can keep demand factors fixed (or at least exogenous to the local shock) and estimate an effect that is driven by a drop in the growth rate of the production of goods.

Importantly, the disaggregated municipality-product-destination data allows us to control for regional specialization in serving foreign markets. Comparing across the same product-destination diminishes concerns that the effects are driven by particular regions that export certain types of goods.

Regressions on export outcomes follow equation 2.2. We test whether the Drug War affected export growth. For each municipality m, we observe the annual amount (in Pesos) of product p exported to country c. There is one caveat about the data. When a firm has a single plant or all their plants are in the same municipality, the exports reflect directly the municipality. When firms have multiple plants in different municipalities within the same state, then an approximation is made based on the workforce of each plant. We deal with this issue in the next section of the paper, in which we study firm micro level data.

In Table 2.3, we report the regressions of export growth on close PAN wins

using the same weighting by population.[21]. When we control for destination-product dummies, (log of) export growth decrease by 45% over the mayoral term, or 15% annually. These controls also alleviate concerns that differential changes in the terms of trade of certain products drive the result. Therefore, after the implementation of the Drug War, municipalities performed worse in terms of trade even when the more open party, PAN, was elected. Figure 2.5 shows there is a discontinuous and significantly negative effect of a close PAN election on log export growth after the election.

2.5.2 Placebos

Regarding the identification assumptions behind our empirical strategy, random assignment of close PAN wins is not enough to draw conclusions about the actual effects of violence. We need to show that the under-performance was not triggered by the election of the PAN itself and the particular economic policies that the party advocates, but by the propensity to engage in the war on drugs and the ensuing violence that it caused. To address this concern, we provide placebo estimates of the same specification for the 2004-2005 elections. Data is available from 2004, so we take export growth until 2007, the first year of the Drug War.[22] Table 2.4 reports the results. Before the Drug War, the close PAN wins had a negative – 20% over 2 years, or 10% annually – but not statistically significant effect on exports growth.

We also perform two placebo tests during the period of the war on drugs. Ex-ante cartel presence and high levels of violence were drivers of enforcement operations during the war. Therefore, locations with a PAN mayor but no cartel presence and low violence were less likely to be the target of anti-drug operations, but still experienced policies implemented by the PAN. If in those locations a PAN win is not associated with a decrease in exports, then we can conclude that it is not the PAN victory itself that is causing our main result. We thus exploit heterogeneity in the potential intensity of the war on drugs by splitting our analysis in areas that experienced different levels of drug-related activity and violence before the war.

We explore the prevalence of pre-existing violence and cartel activity in the North

[21]Results are robust in the standard OLS regression.
[22]The Drug War started in December 2006.

of the country, close to the US border. Most of the drug-trafficking organizations operate in this region, where the ports of entry to the US (the main consumer market) are located. We split the data into two parts: North and South. We then complement the analysis by using data collected by Coscia and Rios (2012) on cartel presence at the municipal level in Mexico. We split the sample using the presence of any cartel at the beginning of the drug war.[23]

In Table 2.5, Panel A, we show that the effect of a close PAN win on exports is significantly negative in the North of the country. Panel B shows the effect is either positive or indistinguishable from zero in southern municipalities. This supports the interpretation that our estimate is a lower bound of the negative effects on exports. In municipalities where the drug war was less prevalent, PAN had higher export growth on average.

In Table 2.5, Panel C, we show a similar pattern for municipalities with pre-existing cartel presence. The negative effect on export growth is only significant in municipalities with pre-existing cartel presence. In Panel D, when controlling for product-destination fixed effects, PAN wins in municipalities with no cartel presence experienced a significantly positive growth after the election.

2.5.3 Complexity Heterogeneity

We separate the results according to the degree of complexity in different products. We use the Product Complexity Index (PCI) from the Atlas of Economic Complexity developed by Hausmann et al. (2013) to classify products. This measure uses trade data to determine the complexity of a product according to two characteristics: ubiquity and the average diversity of its exporters. In theory, a more complex product is produced by countries that export many products, but it is also produced by few countries. The measure tries to capture how complex it is to produce a given product. Table 2.18 in the appendix shows a list of prod-

[23]Coscia and Rios (2012) collect data from relevant web sources such as newspapers and blogs on Drug Trafficking Organizations (DTO) activities in Mexican municipalities using an automatized system. However, there are some limitations in the ability to collect information since powerful cartels can suppress it (Wainwright, 2016). This problem can be especially prevalent in badly governed municipalities. Using the measure directly to predict violence could introduce a bias. Nonetheless, it is unlikely that this potential bias is correlated with the close election outcomes. This is why the main source of variation that we use is still the close election result.

ucts by their level of complexity in 2007. Complexity correlates with future GDP growth, and complex economies tend to grow more (Hausmann et al., 2013). If the Drug War affected more complex products, then the long term effects could be more pernicious.

In Table 2.6 we report a monotonic pattern in export growth. We divide products into four quartiles depending on how they rank in terms of the economic complexity index. For low complexity the effects on export growth are indistinguishable from zero, or positive if we control for product-destination dummies. The higher the complexity the more negative and significant the effects on export's growth. This result suggests that in the treated municipalities the negative impacts are concentrated in more complex industries.

2.6 Change in firms' exports

In this section we proceed to estimate the effects at the firm level. The nature of the electoral discontinuity allows us to study the economic effects of increased violence at a microeconomic level by identifying firms' municipal locations. A potential concern with the municipality-level export data is that it is constructed assuming a distribution of firms' exports when firms own plants in multiple municipalities. The geographical distribution of exports is assumed to be in line with the distribution of a firm's workforce as expressed in social security records. To verify that this assumption is not problematic, we validate results with firm-level export data for a sample of firms that operate in a single municipality of a state in every given year. Using administrative sources on transaction-level customs data and firm-level social security data, we assess the intensive-margin growth in exports and the extensive-margin disappearance of export relationships at the firm, product and country of destination level, while also allowing for the inclusion of industry-level fixed-effects that control for sector-wide dynamics in firms' main activity.[24]

[24]These anonymized sources were provided by the Mexican Social Security and Tax Authorities as inputs for the development of the Mexican Atlas of Economic Complexity. We worked with this data locally at Harvard's Center for International Development, which partnered with the Mexican government in developing this data visualization tool. Information about the Mexican Atlas of Economic Complexity is available at `http://complejidad.datos.gob.mx`.

At the intensive margin, we estimate the following equation:

$$log\left(\left[\frac{X^{t'}_{fmpc}}{X^{t}_{fmpc}}\right]^{\frac{1}{t'-t}}\right) = \beta_0+\beta_1 PANwin_m+\delta f(Margin_m, PANwin_m)+\psi_{pc}+\kappa_i+\epsilon_{fmcp} \tag{2.3}$$

Where X^t_{fmpc} stands for the exports of firm f of product p to country of destination c, located in municipality m in baseline year t. The dependent variable captures the logarithm of the average yearly growth factor in total exports at the firm, product and country of destination level between years t and t'. β_1 captures the percent difference in the average yearly growth factor of the exports by firm-product-destination for firms marginally exposed to a PAN mayor in their municipality. ψ_{pc} stands for product-country of destination fixed-effects that control for external demand, and κ_i stands for industry fixed-effects that control for factors that are fixed for the main activity of every firm in the data. Standard errors are clustered at the municipality level, which is the level of the treatment. As with the main specifications in the municipality-level analysis, the bandwidth for close elections is 5% and we use linear controls on both sides of the electoral discontinuity.

2.6.1 Main Results

Table 2.7 shows how firm-level specifications largely validate municipality level results at the intensive margin. For the sample of municipalities with close elections in 2007 and 2008, a marginal PAN victory associates with a 21% drop in the annual growth factor of firm exports between 2007 and 2010.

Panel A of Table 2.8 shows that the effect of a Marginal PAN victory in 2007/2008 on future export growth is still observed into 2013, after the US economic crisis had largely subsided. Hence, we believe these results are not contingent on the crisis. Panels B and C of Table 2.8 evaluate the short- and long-term effects of a marginal PAN victory on the probability of a firm to lose an export relationship with a foreign country for a given product. Results largely show a null effect. In the context of the negative and significant effects observed at the intensive margin, the evidence is consistent with firms adapting to the increasingly violent environment by reducing

the intensity of their ongoing export relationships, but not by disproportionately rescinding on these relationships. This finding can be interpreted as consequence of increasing marginal costs of exporting, assuming there exist fixed and sunk costs of developing export relationships.[25] The lack of changes in exit suggests fixed costs of exporting do not change significantly after increases in violence.

2.6.2 Placebos

We repeat the placebo tests that we performed in the municipality regressions. Table 2.9 shows similar specifications applied for a placebo sample of municipalities with close elections in 2004 and 2005. With product-destination fixed effects, a close PAN victory is associated with an increase in exports. However, when we include industry fixed effects, the effect becomes negative, and, even though its magnitude is around half of the magnitude obtained with the 2007 and 2008 elections, it is significant at 10%. These results provide evidence that the effects of a marginal PAN victory are significantly contingent to the period of the war on drugs.

Table 2.10 shows similar regression discontinuity estimates, evaluating how the effects of a marginal PAN victory are contingent on baseline cartel presence. Panel A shows results for the sample of municipalities with close elections in 2007 and 2008. Regressions in columns 1 and 3 show that a marginal PAN win in municipalities with baseline cartel presence associates with a 20% drop in export growth rates between 2007 and 2010, while regressions in columns 2 and 4 show null effects in municipalities without baseline cartel presence. Panel B shows similar estimates for a placebo sample of municipalities with close elections in 2004 and 2005, showing null estimates throughout.

Table 2.11 follows a similar strategy for the north-south split. For Panel A, regressions in columns 1 and 3 show that a marginal PAN win in northern municipalities also associates with a 14% to 26% drop in export growth rates between 2007 and 2010, while regressions in columns 2 and 4 show null effects in southern municipalities. Panel B shows results for the sample of municipalities with close

[25]For a theoretical motivation behind the margins of adjustment see Melitz (2003), and for an estimation on the relevance of each method of adjustment in trade see Helpman, Melitz, and Rubinstein (2008).

elections in 2004 and 2005, before the war on drugs.

2.6.3 Effect Heterogeneity: Size, Product Complexity and Input dependence

There are many possible mechanisms through which violence may disrupt the exporting activities of local firms. Violence may prevent a firm's capacity to source the necessary human capital for its operations; it may hamper the capacity for firms to raise capital and leverage their operations; and it may disrupt the transportation of inputs and outputs. Effects may be more relevant for smaller firms on which the added operating costs of violence may be more onerous, or for larger firms for which parts of the production process may be more exposed to the disruption caused by crime.

Ideally, to assess which of these channels may be operating, we would evaluate how a firm's size and its reliance on human capital, capital, finance and transportation services affect the the impact of a marginal PAN victory on exports. However, given the features of our administrative data, we can only make this assessment directly for the workforce size of exporters. Nevertheless, we also construct metrics of input dependence at the exported product level. We use them to assess whether the negative effects are larger for product groups that disproportionately depend on a given input.

For exporter size, we split the sample of exporters around the median size of the workforce, using the distribution of single-municipality exporters as of 2007. Furthermore, we test for five different product segmentation measures.[26] The measures are suggestive of the channels through which violence might be affecting export growth:

- Product Complexity: This metric from Hausmann et al. (2013) empirically approximates the diversity on the productive capacities required to export a product competitively from a given country. Hence, it can be thought of as a

[26]All input dependence metrics are converted into the 1992 version of the Harmonized System of product classification. Export data for some products cannot be matched to input dependence scores, so that export data for these products cannot be used for the subsequent analyses.

measure of the intensity of input complementarities for the output of a given product.

- Capital dependence: This metric from Shirotori et al. (2010) captures the Revealed Capital Intensity of the product from international trade patterns and national capital endowments of their competitive exporters.

- Human capital dependence: Also from Shirotori et al. (2010), this measure captures the Revealed Human Capital Intensity of the product from international trade and national human capital endowment patterns.

- Finance dependence: This metric from Kaplan and Zingales (1997) and subsequent work by Lamont et al. (2001), measures a product's dependence on external capital for its production. Cash crops with fast turnaround – like tobacco – are in the bottom of the finance dependence list, while sectors that require long-term risky investments and higher working capital – like medicines – are in the top of the list.

- Trucking dependence: We build this metric according to a product's realized dependence on trucking services as measured in the US input-output tables.

Table 2.12 shows the correlations between the product complexity and input dependence scores for long-term capital, human capital, complexity, external finance dependence, and trucking dependence. We can see there is a positive and high correlation between complexity, long-term capital and human capital dependence. This is expected. Complexity is supposed to capture how difficult it is to produce a good, which is correlated to skill dependence.[27] Likewise, the macroeconomics's literature has provided empirical evidence of complementarities between long-term capital and human capital.[28] Therefore, it is natural that both measures are highly correlated.

On the other hand, external capital dependence exhibits lower correlation with other measures. Trucking dependence is not correlated with measures of either com-

[27]See Hausmann et al. (2013).
[28]See Lewis (2011).

plexity, long-term capital, and human capital dependence. It is negatively correlated with external capital dependence.

Table 2.13 shows regression discontinuity estimates of the effects of a PAN victory in a close municipality election in 2007 and 2008 on export growth between 2007 and 2010, conditioning for exporter size groups or for product groups divided by the relevant input dependence measure. For exporter's size we divide the sample into large and small according to the median size of the workforce. The segmentation on high and low levels of complexity or input dependence was divided according to their respective median values in the product distribution. Panel A includes product-country of destination fixed effects, which is the micro data analogous to the regressions we estimate at the municipality level. Panel B adds industry fixed effects.

Results in Table 2.13 suggest that the negative effects of the war on drugs are either contingent to or appear more detrimental for larger exporters. In our preferred specification, Panel A, we observe that a PAN win is correlated with a 27% decrease in export growth at the firm-product-destination only for large exporters. Small exporters suffer no significant change in any of the two specifications.

Firms exporting high complexity products suffer a 27% decrease in their exports located in a municipality with close PAN win in comparison to firms exporting the same product to the same destination in the control group. The results are insignificant for low complexity products. Firms producing capital intensive products suffer a decrease between 32 and 34% in export growth. There is no effect in industries with low capital intensity. Similarly, firms producing products with high human capital dependence and located in a municipality with a close PAN win, experience a decrease of 27% in export growth. There is a negative, but much smaller and not statistically significant effect on products that require low levels of human capital. The results in these product level divisions are consistent with the result that violence affects exports in sectors that require a more complex production process, more capital investments, and more specialized factors of production.

We now turn to the question of financing. The production of certain goods can rely more on the use of external financing, where external financing refers to the use

of funds from outside investors. Even though the estimates are negative and statistically significant only for high finance dependence, they only seem economically different for the Panel B specification that adds fixed effects for the main activity of the firm. Therefore, the evidence on this channel is more mixed.

Another important question is whether violence decreases exports by increasing transportation costs in affected localities. To test this hypothesis we split the sample according to the measure of trucking service dependence. Our results show no evidence of firms with more trucking dependence having a larger decrease in export growth. The results are the opposite. However, this could be partially explained by the fact this the measure is constructed using US data.

The learning from this exercise is consistent with important stylized facts of trade. Exporters tend to be firms that rely more on fixed capital and skill intensity.[29] In our results, the negative effect on exports is more pronounced precisely on these sectors. This is consistent with the findings that violence imposes a cost on exporting. Firms that would gain more from exporting in the first place are more hampered by violence. Moreover, we show that this effect is likely to be driven by increase in the cost of production that requires more physical capital and human capital, rather than by an increase in transportation costs.

2.7 Effects on greenfield investment CAPEX

While the data we have used thus far can help us assess the effects of the war on drugs on export growth and on the disappearance of export relationships, it does not allow us to assess the capacity of a locality to attract new projects from outside investors. For this purpose, we would need a yearly dataset on greenfield investments that identifies the destination municipality and the magnitude invested in the project. To our knowledge, such data is not available in Mexican statistical or administrative sources.

For this reason, we use data from fDi Markets, a service from the Financial Times with a comprehensive database of crossborder greenfield investments covering all countries and sectors worldwide, documenting every investments' capital

[29] See Mayer and Ottaviano (2008).

expenditures. From this investment specific dataset we build an aggregate dataset of the CAPEX received by a Mexican municipality between 2003 and 2006 (pre-treatment), and between 2007 and 2010 (post-treatment). After restricting our sample to municipalities with close elections in 2007 or 2008, we retain CAPEX data for 39 municipalities.[30] Table 2.14 shows that regression discontinuity results. A marginal PAN victory associates with a reduction in CAPEX between 2007 and 2010.

2.8 Conclusion

The Mexican Drug War has drawn the attention of the population, the media and the academia because of the scale of its consequences. We confirm the results in Dell (2015), who provides evidence that homicides increase disproportionately in municipalities where the rollout of the war effort was supported by PAN mayors. We take a step further and assess how the Drug War affected the real economy. We document a negative change in trade patterns, with export growth decreasing significantly after a close PAN win. We argue that a direct, reduced-form approach would yield lower-bound estimates of the negative economic effects of increased violence. To support this assumption we provide placebo estimates on previous elections, regions without ex-ante baseline Cartel presence, and regions facing ex-ante low violence. Our findings support the assumption that the direct negative economic effects of narrow PAN victories only occur in the context of the Mexican Drug War. We interpret our results as evidence of external effects of violence, as effects are not observed outside the temporal and geographic context of the Drug War.

The economic literature has studied the effects of violence on economic outcomes. However, it is difficult to separate the effects on demand and supply. Our paper also contributes to the literature in terms of identification. By combining close elections and comparing exports of the same product to the same destination, we are able to disentangle effects on supply and demand, and study show violence affects the

[30] We expand the electoral bandwidth to 10% in order to gain more observations and reduce the variance in the estimates.

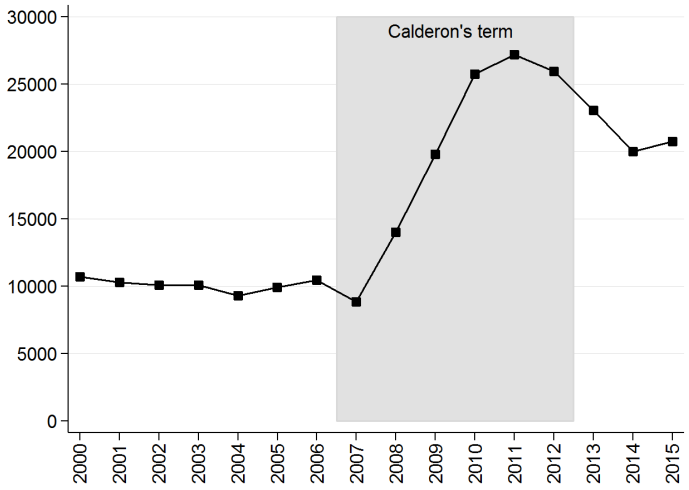
capacity of firms to serve external markets.

We also provide new evidence on the relationship between violence and trade. Using firm-level microdata, we find that firms locating in a municipality that was exposed to a PAN mayor faced lower export growth rates, but we do not find a higher probability of firm exit from product-country markets. This is consistent with the view that violence increases the marginal costs of exporting, but does not affect significantly the fixed costs of sustaining trading relationships. Additionally, we find that the effects are stronger for larger exporters, as well as for exports of more complex, more capital-intensive, more skill-intensive and more finance-dependent products. Therefore, violence affects sectors that are key for future economic opportunities.

The main results suggest that violence can negatively affect trade at the local level. Importantly, the increase in violence was a consequence of government policy. In the case of Mexico, the Drug War policy did not only cost lives, but damaged the export capacity of firms in the most affected locations.

2.9 Figures

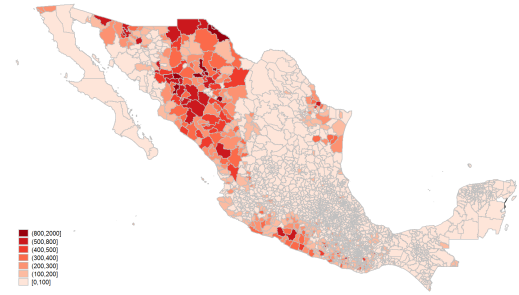
Figure 2.1: **Annual homicides**



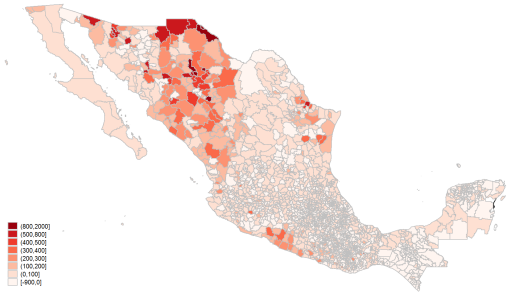
Notes: This figure shows the time series of total homicides in Mexico. The grey area shows total homicides during Calderón's presidency; i.e, during the implementation of the war on drugs.

Figure 2.2: **Spatial distribution of homicides**

Panel A. Total homicides 2007-2011

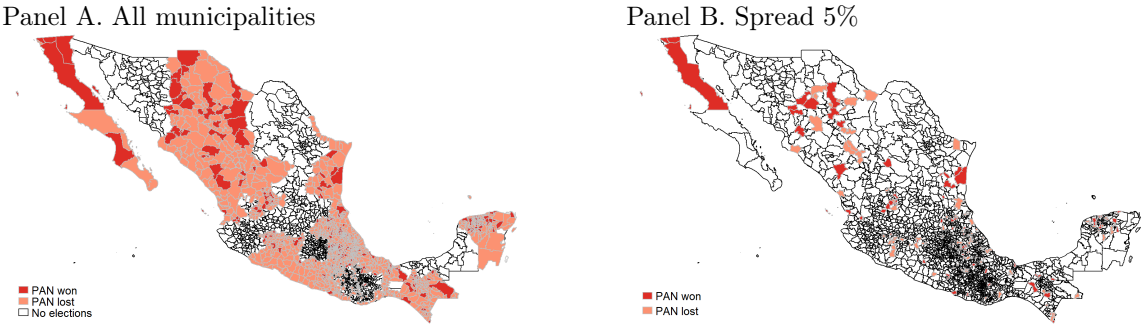


Panel B. Change 2007-2011 and 2001-2006



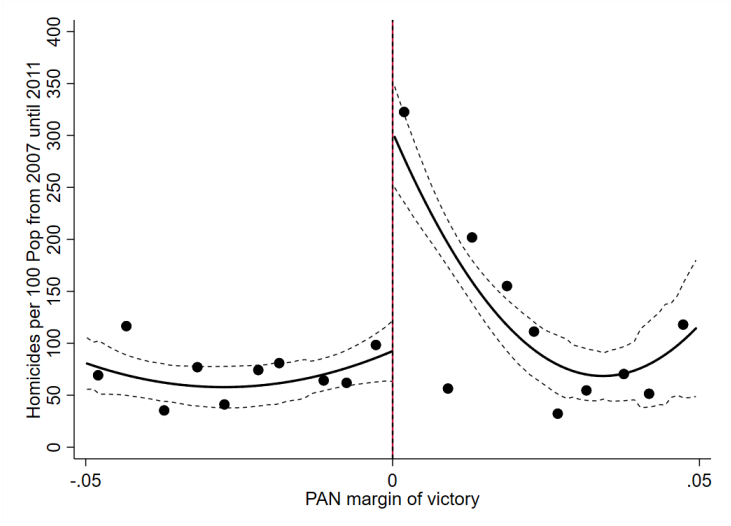
Notes: Panel A depicts the geographical distribution of total homicides between 2007 and 20011 per 100,000 inhabitants. Panel B depicts total homicides between 2007 and 2011 minus total homicides between 2001 and 2006, per 100,000 inhabitants. It is not possible to compute growth rates or logs because many municipalities have zero homicides.

Figure 2.3: **Spatial distribution of of electoral outcomes**



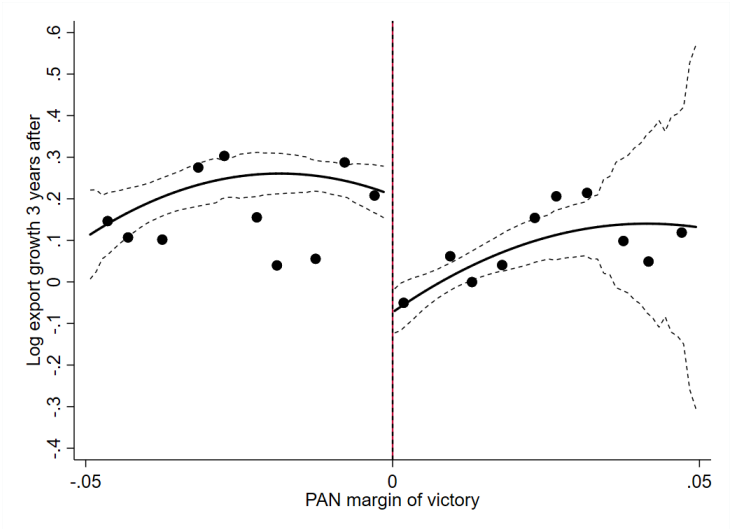
Notes: Panel A depicts the geographical distribution of PAN victories and losses in the 2007 and 2008 local elections. Panel B depicts PAN victories and losses by a margin smaller than 5%.

Figure 2.4: **Cumulative Homicides as a function of PAN electoral share**



Notes: RDD graph on cumulative homicides as a function of direct electoral shares for PAN in a Mexican municipality. The graph weights homicides by Population in 2005. Confidence intervals are presented at a 95% level.

Figure 2.5: **Log Export growth as a function of PAN electoral share**



Notes: RDD graph on log export growth as a function of direct electoral shares for PAN in a Mexican municipality. The graph weights log export growth by Population in 2005. Confidence intervals are presented at a 95% level. The data for exports is formed by triples of municipality, product, and country of destination.

2.10 Tables

Table 2.1: **Baseline Characteristics**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total sample				Spread 5%		
	All	PAN won	PAN lost	P-value means diff.	PAN won	PAN lost	P-value means diff.
Panel A: Characteristics Baseline							
Population 2005	35019	38396	34270	0.54	59232	42934	0.44
	(97487)	(126163)	(89949)		(190580)	(103344)	
Population ages 15-29	25.6	26.2	25.5	0	26.2	25.9	0.33
(% of total)	(2.5)	(2.2)	(2.5)		(2.3)	(2.6)	
Population density, 2005	151.9	162.9	149.4	0.61	209.6	188.14	0.75
	(381.5)	(385.1)	(380.8)		(465.8)	(466.3)	
PAN incumbent	0.27	0.28	0.26	0.49	0.31	0.32	0.84
	(0.44)	(0.45)	(0.44)		(0.47)	(0.47)	
GDP per capita	5740	5996	5683	0.09	6085	6228	0.74
(USD, 2005)	(2678)	(2942)	(2613)		(3360)	(2759)	
Literacy rate ages	95.2	95.6	95.1	0.13	95.5	96.1	0.29
(ages 15-24, 2005)	(4.9)	(4.1)	(5.1)		(4.3)	(3.2)	
Mean years of	5.9	6.1	5.9	0.16	6.1	6.1	0.97
schooling, 2005	(1.4)	(1.4)	(1.4)		(1.4)	(1.4)	
Mean Homicides, 2006	11.77	9.31	12.31	0.04	12.03	12.66	0.86
Per 100 Population	(20.75)	(19.09)	(21.07)		(20.77)	(21.62)	
Observations	1416	257	1159		87	111	
Panel B: Trade Baseline							
Total exports	52.5	81	46.1	0.14	178.6	71.5	0.35
	(340)	(681.6)	(195.7)		(1160.4)	(259.2)	
Exports: number of	19	19.5	18.9	0.71	22.6	22.6	1
countries	(19.9)	(22.5)	(19.3)		(27.2)	(23.6)	
Exports: number of	2.2	2.5	2.1	0.07	3.2	2.6	0.4
products per country	(2.8)	(4.1)	(2.4)		(6.1)	(3.7)	
Total imports	29.7	59.9	23	0.04	147.6	50.5	0.31
	(266.2)	(570.2)	(120.3)		(971)	(229.6)	
Imports: number of	7.7	8.4	7.5	0.45	11.4	10.7	0.82
countries	(16.7)	(20.1)	(15.8)		(27.4)	(19.7)	
Imports: number of	2.8	3.3	2.7	0.11	4.7	3.6	0.35
products per country	(5.5)	(6.8)	(5.2)		(9.6)	(7)	
Observations	1416	257	1159		87	111	

Notes: Columns 1-3 report means for all municipalities in which elections occurred in 2007 and 2008. Columns 5-6 restrict the sample to municipalities where PAN won or lost by a margin smaller than 5%. Columns 4, and 7 report p-values of t-tests on the difference in means between the PAN win and PAN loss sample. Standard errors are reported in parentheses.

Table 2.2: **Effect on homicides, 5% spread**

	(1)	(2)	(3)
Panel A: Average homicide 3 years after election (2007 and 2008 elections)			
PAN win	25.90**	41.22**	41.22*
	(12.65)	(18.98)	(19.79)
Linear polynomial	No	Yes	Yes
Cluster: state level	No	No	Yes
Observations	198	198	198
R-squared	0.172	0.253	0.253
Panel B: Average homicide 3 years before election (2007 and 2008 elections)			
PAN win	3.29	3.76	3.76
	(2.71)	(4.32)	(4.80)
Observations	198	198	198
R-squared	0.030	0.034	0.034
Panel C: Average homicide 3 years after election minus 3 years before election (2007 and 2008 elections)			
PAN win	22.61**	37.47**	37.47**
	(10.80)	(16.62)	(16.81)
Observations	198	198	198
R-squared	0.179	0.301	0.301
Panel D: Placebo, average homicides 3 years after election (2004 and 2005 elections)			
PAN win	-5.08**	-0.81	-0.81
	(2.22)	(3.09)	(2.35)
Observations	247	247	247
R-squared	0.095	0.122	0.122

Notes: Columns 1-3 report standard WLS regressions. Weights are determined by population size in 2005. The dependent variable in panels A and D is average annual homicides per 100,000 population in the three years following local elections; in panel B the dependent variable is average annual homicides per 100,000 population in the three years preceding local elections; and in panel C the dependent variable is the difference between the dependent variables of panels A and B. In panels A, B and C, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections. These are the elections at the beginning of the Drug War (the treatment period). In panels D, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 5% in the 2004 and 2005 elections. These are the elections before the Drug War (a placebo period). Robust standard errors are reported in parentheses.

Table 2.3: **Total exports**

	(1)	(2)	(3)	(4)
Panel A: Exports				
PAN win	-0.24***	-0.54***	-0.56***	-0.45***
	(0.09)	(0.10)	(0.08)	(0.06)
Linear RD Polynomial	No	Yes	Yes	Yes
Country of destination dummies	No	No	Yes	No
Product-country of destination dummies	No	No	No	Yes
Observations	21,435	21,435	21,424	18,267
R-squared	0.00	0.00	0.03	0.58

Notes: Columns 1-4 report weighted regressions. Weights are determined by population size in 2005. Standard errors are clustered at the municipality level. In panel A, the dependent variable is the natural logarithmic of total exports in the final year of the new incumbent's term, divided by total exports in the year when elections took place. The sample is comprised of triples municipality-country of destination-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported for the triple is positive over the new incumbent's term.

Table 2.4: **Placebo**

	(1)	(2)	(3)	(4)
Exports, placebo 2004-2005 Elections				
PAN win	-0.11***	-0.12	-0.13	-0.21
	(0.03)	(0.09)	(0.10)	(0.14)
Linear RD Polynomial	No	Yes	Yes	Yes
Country of destination FE	No	No	Yes	No
Product-country of destination FE	No	No	No	Yes
Observations	17,058	17,508	17,495	14,682
R-squared	0.00	0.00	0.03	0.60

Notes: Columns 1-4 report weighted regressions. Weights are determined by population size in 2005. Standard errors are clustered at the municipality level. The dependent variable is the natural logarithmic of total exports in the second year of the new incumbent's term, divided by total exports in the year when elections took place. We choose the second year because the third year takes place in the middle of the war, which could contaminate the estimates. The sample is comprised of triples municipality-country of destination-product where: (i) PAN won or lost by a margin smaller than 5% in the 2004 and 2005 elections and (ii) the value exported for the triple is positive.

Table 2.5: **Log export growth geterogeneity by municipalities with pre-existing propensity to drug trafficking**

	(1)	(2)	(3)
Panel A: North			
PAN win	-0.62***	-0.63***	-0.40***
	(0.17)	(0.15)	(0.07)
Observations	17,068	17,053	14,120
R-squared	0.00	0.03	0.59
Panel B: South			
PAN win	0.13*	0.14**	0.11
	(0.07)	(0.06)	(0.09)
Observations	4,367	4,349	2,790
R-squared	0.00	0.10	0.80
Panel C: Pre-existing cartel presence			
PAN win	-0.55***	-0.56***	-0.46***
	(0.10)	(0.09)	(0.07)
Observations	16,923	16,910	13,798
R-squared	0.01	0.02	0.42
Panel D: No pre-existing cartel presence			
PAN win	-0.11	-0.13*	0.09**
	(0.08)	(0.07)	(0.04)
Observations	4,273	4,256	3,084
R-squared	0.00	0.08	0.67
Linear RD Polynomial	Yes	Yes	Yes
Country of destination FE	No	Yes	No
Product-country of destination FE	No	No	Yes

Notes: Columns 1-3 report weighted regressions. Weights are determined by population size in 2005. Standard errors are clustered at the municipality level. The dependent variable is the natural logarithmic of total exports in the final year of the new incumbent's term, divided by total exports in the year when elections took place. The sample is comprised of triples municipality-country of destination-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported for the triple is positive over the new incumbent's term. In panels A and B, the sample is divided into two parts using the median of the (average) latitude of the municipalities. In panel A, we report results for the northern municipalities, while in Panel B we report results for the southern municipalities. In Panel C we report effects in municipalities with pre-existing cartel participation (as identified by Coscia and Rios (2012)). In Panel D we report effects in municipalities with no pre-existing cartel activity.

Table 2.6: **Exports per quartile of product complexity**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1st quartile (low)		2nd quartile		3rd quartile		4th quartile (high)	
PAN win	-0.07	0.11	-0.17	-0.32	-0.68***	-0.32***	-0.88***	-0.65***
	(0.25)	(0.34)	(0.14)	(0.23)	(0.06)	(0.05)	(0.29)	(0.11)
Linear RD Polynom.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Destination FE	Yes	No	Yes	No	Yes	No	Yes	No
Product-destin. FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3,899	3,535	3,790	3,220	4,695	4,011	5,306	4,418
R-squared	0.10	0.58	0.06	0.57	0.06	0.60	0.05	0.59

Notes: All columns report weighted regressions. Weights are determined by population size in 2005. Standard errors are clustered at the municipality level. The dependent variable is the natural logarithmic of total exports in the final year of the new incumbent's term, divided by total exports in the year when elections took place. The sample is comprised of triples municipality-country of destination-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported for the triple is positive over the new incumbent's term. Products are divided in 1241 categories. We divide the 1241 products in four groups according to their complexity as defined by the Atlas of Economic Complexity.

Table 2.7: **Firm-level regressions for municipalities**

	(1)	(2)	(3)	(4)	(5)
Log export growth between 2007-2010, Close Elections from 2007/2008					
PAN win	-0.14***	-0.14**	-0.14***	-0.21**	-0.22**
	(0.05)	(0.06)	(0.05)	(0.09)	(0.09)
Observations	17,348	17,348	17,348	17,348	14,647
R-squared	0.00	0.02	0.05	0.15	0.12
Country of destination FE	No	Yes	No	No	No
Product FE	No	No	Yes	No	No
Product-country of dest. FE	No	No	No	Yes	Yes
Industry FE	No	No	No	No	Yes

Notes: Columns 1-5 report OLS regressions. Standard errors are clustered at the municipality level. The sample is comprised of triples firms-country of destination-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported for the triple is positive over the new incumbent's term.

Table 2.8: **Long-term intensive-margin regression and extensive-margin regressions**

	(1)	(2)	(3)	(4)	(5)
Panel A: Log growth factor of exports between 2007-2013, Close Elections 2007/2008					
PANwin	-0.14***	-0.13***	-0.12***	-0.13**	-0.14***
	(0.05)	(0.05)	(0.04)	(0.06)	(0.04)
Observations	14,264	14,264	14,264	14,264	12,223
R-squared	0.00	0.03	0.06	0.17	0.12
Panel B: Relationship disappearance between 2007-2010, Close Elections 2007/2008					
PANwin	-0.04	-0.07	-0.06	-0.07	0.05
	(0.09)	(0.06)	(0.09)	(0.07)	(0.06)
Observations	41,900	41,900	41,900	41,900	34,539
R-squared	0.01	0.12	0.08	0.29	0.29
Panel C: Relationship disappearance between 2007-2013, Close Elections 2007/2008					
PANwin	-0.08	-0.10*	-0.09	-0.08	0.05
	(0.08)	(0.05)	(0.08)	(0.05)	(0.04)
Observations	41,900	41,900	41,900	41,900	34,539
R-squared	0.02	0.10	0.09	0.27	0.28
Country of destination FE	No	Yes	No	No	No
Product FE	No	No	Yes	No	No
Product-country of dest. FE	No	No	No	Yes	Yes
Industry FE	No	No	No	No	Yes

Notes: Columns 1-5 report OLS regressions. Standard errors are clustered at the municipality level. In Panel A, the sample is comprised of triples firms-country of destination-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections. for the intensive margin the second condition (ii) is that the dependent variable for the triple is positive over the period after the election. For the extensive margin regressions – Panels B and C – we show whether a firm stopped exporting afterwards.

Table 2.9: **Firm-level regressions for placebo municipalities with close elections in 2004/2005**

	(1)	(2)	(3)	(4)	(5)
Panel A: Log export growth between 2007-2010, Close Elections from 2004/2005					
PAN win	0.07	0.09	0.02	-0.01	0.15*
	(0.08)	(0.07)	(0.10)	(0.08)	(0.09)
Observations	13,201	13,201	13,201	13,201	11,569
R-squared	0.00	0.01	0.06	0.15	0.12
Panel B: Log export growth between 2004-2007, Close Elections from 2004/2005					
PAN win	0.05**	0.01	0.09***	0.11*	-0.10**
	(0.02)	(0.06)	(0.03)	(0.05)	(0.05)
Obs	16,601	16,601	16,601	16,601	14,113
Rsq	0.00	0.01	0.05	0.15	0.12
Country of destination FE	No	Yes	No	No	No
Product FE	No	No	Yes	No	No
Product-country of dest. FE	No	No	No	Yes	Yes
Industry FE	No	No	No	No	Yes

Notes: Columns 1-5 report OLS regressions. Standard errors are clustered at the municipality level. The sample is comprised of triples firms-country of destination-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported for the triple is positive over the new incumbent's term.

Table 2.10: **Regression in municipalities with and without baseline cartel presence**

	(1)	(2)	(3)	(4)
Panel A: Log growth exports from 2007/2010, Close Elections 2007/2008				
PANwin	-0.216**	-2.976	-0.209**	-0.281
	(0.0885)	(2.825)	(0.0868)	(1.174)
Observations	15939	1409	14170	95
R-squared	0.135	0.899	0.115	0.546
Panel B: Log growth exports from 2004/2007, Close Elections 2004/2005 (placebo)				
PANwin	0.00448	0.346	0.177*	3.776
	(0.0938)	(0.716)	(0.0869)	(2.897)
Observations	12772	429	11340	104
R-squared	0.145	0.771	0.117	0.522
Product-country of dest. FE	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Cartel	Present	Absent	Present	Absent

Notes: Columns 1-4 report OLS. Standard errors are clustered at the municipality level. The sample is comprised of triples firms-country of destination-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported for the triple is positive over the new incumbent's term. We measure cartel presence in before the relevant election using data constructed by Coscia and Rios (2012). The variable cartels is a dummy that determines whether there was cartel presence in a municipality.

Table 2.11: **Regressions in northern and southern municipalities**

	(1)	(2)	(3)	(4)
Panel A: Log growth exports from 2007/2010, Close Elections from 2007/2008				
PANwin	-0.149**	0.870	-0.267**	0.992
	(0.0665)	(2.223)	(0.0966)	(0.625)
Observations	15,682	1,627	14,033	261
R-squared	0.128	0.816	0.114	0.500
Panel B: Log growth exports from 2004/2007, Close Elections from 2004/2005 (placebo)				
PANwin	0.140	0.454**	0.538*	-
	(0.101)	(0.110)	(0.213)	-
Observations	9,479	71	8,197	20
R-squared	0.172	0.931	0.135	-
Product-country of dest. FE	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
North	Yes	No	Yes	No

Notes: Columns 1-4 report OLS regressions. Standard errors are clustered at the municipality level. The sample is comprised of triples firms-country of destination-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported for the triple is positive over the new incumbent's term. To measure North and South we divide municipalities by separating Mexico in two areas using the median latitude. The variable North is a dummy that determines whether the location was above or below the median latitude.

Table 2.12: **Correlation in product-level complexity and input dependence**

	Product Complexity	Capital Dependence	Human Capital Dependence	External Finance Dependence	Trucking Dependence
Product Complexity	1				
Capital Dep.	0.79	1			
Human Capital Dep.	0.71	0.8	1		
External Finance Dep.	0.36	0.28	0.25	1	
Trucking Dep.	-0.05	-0.05	-0.01	-0.38	1

Table 2.13: **Heterogeneity in effects by exporter size and product groups (complexity and input dependence)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Log growth factor of exports between 2007-2010, Close Elections from 2007/2008, Country/Product Fixed Effects												
PANwin	-0.27***	-0.04	-0.27***	-0.12	-0.32***	-0.06	-0.21**	-0.19	-0.27**	-0.12	0.06	-0.38***
	(0.09)	(0.25)	(0.08)	(0.10)	(0.07)	(0.11)	(0.09)	(0.120)	(0.10)	(0.09)	(0.19)	(0.04)
Obs	15,706	1,550	12,523	4,733	11,047	6,209	12,329	4,927	11,658	5,598	6,759	10,497
Rsq	0.16	0.39	0.15	0.17	0.15	0.16	0.16	0.12	0.14	0.18	0.16	0.15
Segment	Large Exporters	Small Exporters	High Complexity	Low Complexity	High Capital Dependence	Low Capital Dependence	High Finance Dependence	Low Finance Dependence	High Human Capital	Low Human Capital Dependence	High Trucking Dependence	Low Trucking Dependence
Panel B: Log growth factor of exports between 2007-2010, Close Elections from 2007/2008, Country/Product and Industry Fixed Effects												
PANwin	-0.23***	0.19	-0.27**	0.01	-0.34**	0.00	-0.23***	-0.13	-0.26***	-0.04	0.18	-0.47***
	(0.09)	(0.35)	(0.12)	(0.14)	(0.13)	(0.14)	(0.08)	(0.15)	(0.09)	(0.17)	(0.13)	(0.10)
Obs	13,284	1,027	10,558	3,959	9,243	5,266	10,307	4,215	9,803	4,709	5,623	8,880
Rsq	0.12	0.35	0.12	0.15	0.11	0.15	0.13	0.12	0.11	0.16	0.12	0.13
Segment	Large Exporters	Small Exporters	High Complexity	Low Complexity	High Capital Dependence	Low Capital Dependence	High Finance Dependence	Low Finance Dependence	High Human Capital	Low Human Capital Dependence	High Trucking Dependence	Low Trucking Dependence

Notes: Columns 1-12 report OLS regressions. The sample is comprised of triples firms-country of destination-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported for the triple is positive over the new incumbent's term. We divide below and above median by product characteristics. Product Complexity: This metric from Hausmann et al. (2013) empirically approximates the diversity on the productive capacities required to export a product competitively from a given country. Capital dependence: This metric from Shirotori et al. (2010) captures the Revealed Capital Intensity of the product from international trade patterns and national capital endowments of their competitive exporters. Human capital dependence: Also from Shirotori et al. (2010), this measure captures the Revealed Human Capital Intensity of the product from international trade and national human capital endowment patterns.Finance dependence: This metric from Kaplan and Zingales (1997) and subsequent work by Lamont et al. (2001), measures a product's dependence in external capital for its production. Trucking dependence: We build this metric according to a product's appeared dependence on trucking services as measured in the US input-output tables.

Table 2.14: **Regression on local greenfield CAPEX**

	(1)	(2)
Greenfield CAPEX (MM US$), Close Elections from 2007/2008		
PANwin	-2,294**	-378.4
	(891.5)	(628.4)
Observations	21	18
R-squared	0.522	0.051
Specification	2007-2010	2004-2007

Notes: Observations are total CAPEX investments by municipality in each period. Columns 1-2 report WLS regressions, where the weight is given by the 2005 Population. Standard errors are robust. Column (1) shows the effect of a close PAN win in the period after the drug war. Column (2) shows the effect of a close PAN win in the elections before (placebo).

2.11 Appendix

Table 2.15: **Effect on homicides, 3% spread**

	(1)	(2)	(3)
Panel A: Average homicide 3 years after election			
PAN win	28.97**	47.91**	47.91**
	(13.87)	(18.87)	(19.36)
Linear polynomial	No	Yes	Yes
Cluster: state level	No	No	Yes
Observations	123	123	123
R-squared	0.185	0.306	0.306
Panel B: Average homicide 3 years before election			
PAN win	4.23	2.40	2.40
	(3.15)	(4.57)	(4.85)
R-squared	0.049	0.057	0.057
Panel C: Average homicide 3 years after election minus 3 years before election			
PAN win	24.74**	45.51***	45.51**
	(11.75)	(17.29)	(18.01)
R-squared	0.182	0.340	0.340

Notes: Columns 1-3 report weighted regressions. Weights are determined by population size in 2005. The dependent variable in panel A is average annual homicides per 100,000 population in the three years following local elections; in panel B the dependent variable is average annual homicides per 100,000 population in the three years preceding local elections; and in Panel C the dependent variable is the difference between the panel the dependent variables of panels A and B. For all regressions, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 3% in the 2007 and 2008 elections. Robust standard errors are reported in parentheses.

Table 2.16: **Effect on homicides, RD polynomials**

	(1)	(2)	(3)	(4)
Panel A: Average homicide 3 years after election, 5% spread				
PAN win	41.22*	52.98***	53.04**	68.11**
	(19.79)	(17.57)	(21.86)	(23.88)
Degree of RD polynomial	1st	2nd	3rd	4th
Observations	198	198	198	198
R-squared	0.25	0.30	0.30	0.33
Panel A: Average homicide 3 years after election, total sample				
PAN win	14.86	24.61**	31.65*	47.36**
	(9.94)	(11.46)	(15.61)	(22.20)
Observations	1,416	1,416	1,416	1,416
R-squared	0.02	0.03	0.03	0.05

Notes: Columns 1-4 report weighted regressions. Weights are determined by population size in 2005. The dependent variable is average annual homicides per 100,000 population in the three years following local elections. In Panel A, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections. In Panel B the sample is comprised of all municipalities in which elections occurred in 2007 and 2008. All standard errors are clustered at the state level.

Table 2.17: **Effect on other crimes**

	(1)	(2)	(3)	(4)
	Panel A: Robbery (business establishments)		*Panel B: Assaults*	
PAN win	46.5	68.5	142.9**	192.8
	(35.284)	(45.864)	(66.821)	(119.079)
Linear polynomial	No	Yes	No	Yes
Observations	139	139	139	139
R-squared	0.106	0.143	0.175	0.235
	Panel C: Extortion		*Panel D: Kidnapping*	
PAN win	1.7	4.7*	0.3	1.4
	(2.189)	(2.646)	(0.643)	(1.026)
R-squared	0.026	0.169	0.006	0.098
	Panel E: Robbery (banks branches, cash-in-transit vehicles)		*Panel F: Robbery (all cases, excluding business and banks)*	
PAN win	1.3	2.8*	455.0	917.1***
	(0.865)	(1.616)	(299.769)	(345.038)
R-squared	0.118	0.323	0.123	0.217

Notes: Columns 1-4 report weighted regressions. Weights are determined by population size in 2005. In all panels the dependent variables are averages of a certain crime type per 100,000 population in 2011. In panel A the dependent variable is robberies that targeted business establishments (including cargo theft); in Panel B, assaults; in panel C, extortions; in Panel D, kidnapping; in Panel E, robberies that targeted bank branches and cash-in-transit vehicles; and in Panel F, robberies (excluding business and banks). For all regressions, the sample is comprised of municipalities where crime data is available and where PAN won or lost by a margin smaller than 5% in the 2004 and 2005 elections. Robust standard errors are reported in parentheses.

Table 2.18: **Product complexity**

Lowest complexity

- Natural rubber, balata, gutta-percha, guayule, chicle and similar natural gums, in primary forms or in plates, sheets or strip
- Cocoa beans, whole or broken, raw or roasted
- Coconuts, Brazil nuts and cashew nuts, fresh or dried, whether or not shelled or peeled
- Bananas and plantains, fresh or dried
- Woven fabrics of jute or of other textile bast fibers of heading 5303
- Jute and other textile bast fibers (excluding flax, true hemp and ramie), raw or processed but not spun; tow and waste of these fibers (including yarn waste and garnetted stock)
- Sisal and Agave, raw
- Coconut, abaca (Manila hemp or Musa textilis Nee), ramie and other vegetable textile fibers,not elsewhere specified or included, raw or processed but not spun; tow, noils and waste of these fibers (including yarn waste and garnetted stock)
- Cassava (manioc), arrowroot, salep, Jerusalem artichokes, sweet potatoes and similar roots and tubers with high starch or inulin content, fresh, chilled, frozen or dried, whether or not sliced or in the form of pellets; sago pith
- Hats and other headgear, knitted or crocheted, or made up from lace, felt or other textile fabric, in the piece (but not in strips), whether or not lined or trimmed; hair-nets of any material, whether or not lined or trimmed

Highest complexity

- Vegetable parchment, greaseproof papers, tracing papers and glassine and other glazed transparent or translucent papers, in rolls or sheets
- Machines and appliances for testing the hardness, strength, compressibility, elasticity or other mechanical properties of materials (for example, metals, wood, textiles, paper, plastics), and parts and accessories thereof
- Machine tools for working any material by removal of material, by laser or other light or photon beam, ultrasonic, electro-discharge, electro-chemical, electron-beam, ionic-beam or plasma arc processes
- Lubricating preparations (including cutting-oil preparations, bolt or nut release preparations, antirust or anticorrosion preparations and mold release preparations, based on lubricants) and preparations of a kind used for oil or grease treatment
- Lathes (including turning centers) for removing metal
- Machining centers, unit construction machines (single station) and multistation transfer machines, for working metal
- Microscopes other than optical microscopes; diffraction apparatus; parts and accessories thereof
- Flat-rolled products of stainless steel, of a width of less than 600 mm
- Photographic plates and film, exposed and developed, other than motion-picture film
- Nickel tubes, pipes and tube or pipe fittings (for example, couplings, elbows,sleeves)

Notes: Using data for 2007, this table reports the 10 products with highest complexity and the 10 products with lowest complexity.

Chapter 3

Inheritance taxation in a model of credit rationing and occupational choice

Bernardo Ricca

3.1 Introduction

Initial wealth plays a key role in models of credit rationing. For instance, Holmström and Tirole (1997) show that, in the presence of moral hazard, individuals must have enough wealth in order to obtain credit and become entrepreneurs; individuals that do not have enough wealth are credit rationed and have to pursue different occupations. This feature of the model renders initial wealth critical. Since, to some extent, initial wealth is determined by bequests, studying the implications of inheritance taxation in this environment may be of interest. The study of inheritance taxation and other redistributive policies has gained relevance as researchers have documented an increase in inequality in a broad set of countries.[1] Concomitantly, there is an intense public debate about the topic.[2]

[1] See, among others, Alvaredo et al. (2013), Atkinson et al. (2011), and Piketty (2014)

[2] In the US, some politicians proposed legislation to repeal the federal estate tax in January 2019 (see `https://www.congress.gov/bill/116th-congress/senate-bill/215/text`). On the other side, also in January 2019, some politicians advocated an increase in this type of taxation, especially for the rich (see `https://www.congress.gov/bill/116th-congress/senate-bill/309/`

I extend the Holmström and Tirole (1997) model to a dynastic model in which parents bequeath to their children and individuals choose their occupation. Apart from heterogeneity in the initial wealth of the first generation, individuals are homogeneous and treat bequests as a consumption good. The combination of borrowing constraints and a fixed investment technology results in conditional convergence: individuals converge to different steady-states depending on their initial wealth. In particular, individuals that start poor cannot access more profitable occupations, and tend to remain poor in the long run, that is, poverty traps may occur.

The introduction of a government that taxes bequests and transfers the proceeds to the population has important implications. On the one hand, bequest taxation affects entrepreneurs' effort choice (moral hazard): they have more incentives to misbehave because, in case of success, outcomes must be shared with the creditors (that have to break even) and the government. Therefore, the initial wealth threshold that determines which individuals have access to credit increases with the tax rate. On the other hand, an individual might receive a lump-sum transfer from the government. Therefore, the government can affect the distribution of wealth and thus the occupations available to each agent. Investors that would not be credit rationed in the absence of taxation can now be credit rationed. If the social planner transfers the taxes to the less wealthy, investors that were credit rationed can now have enough wealth to be granted a loan and become an entrepreneur.

I show that the introduction of taxes creates, at least in the short run, an equity efficiency trade-off. While redistribution reduces inequality, GDP per capita is depleted because taxation increases the threshold required to become an entrepreneur, and thus the number of entrepreneurs decreases. However, in the long run (steady-state) this trade-off is not always present and the policy can achieve both a reduction in inequality and a maximization of aggregate productivity. That is, for some set of parameters, there exists a redistributive policy that generates a steady-state in which all individuals are entrepreneurs, there is no inequality, and GDP per capita is maximized. This outcome would be the optimal policy in a society that only cares about steady-state social welfare. However, this is not the case if the social welfare includes all time periods, as relatively rich individuals are worse off in the short run.

`all-info`).

The optimal policy in this case would depend on Pareto weights.

The interplay between wealth inequality, borrowing constraints and the process of development has been explored by the occupational choice literature in development economics. Even though taxation is not the focus of the early papers in this field, they do analyse the impact of taxes, and policy prescriptions vary according to features of the models. In Banerjee and Newman (1993) and Ghatak and Jiang (2002), individuals have the same preferences and have access to the same technologies, but they start their lives with different levels of wealth. They live for one period and treat bequests as a consumption good. Borrowing constraints arise from imperfect enforceability, and nonconvexities in the production technology result in multiple steady-states, including poverty traps: dynasties that start poor tend to remain poor. Inheritance taxation and transfers targeted at the poor can alleviate the occurrence of poverty traps. However, these papers do not explicitly model taxes and its endogenous implications. In particular, borrowing constraints are not directly affected by taxation. In Ghatak et al. (2001) agents can work and save before becoming entrepreneurs. Labor markets are subject to moral hazard and poor individuals are motivated to work hard when 'young' to build up wealth that would allow them to overcome borrowing constraints and undertake profitable projects when 'old'. Redistributive policies can dampen the incentives of the 'young' workers and result in an decrease in social welfare. In Aghion and Bolton (1997), individuals live for one period, and cannot build up wealth through thrift and hard work. Moral hazard leads to borrowing constraints. They argue that redistributive policies are efficiency-enhancing since they reduce the amount borrowed by the poor, and therefore their incentives to maximize profits are less distorted.

More recently, Cagetti and De Nardi (2009) explicitly study taxation in a model that features occupational choice and borrowing constraints. Their approach differs from Piketty and Saez (2013), who study optimal inheritance taxation in a model where occupation is not a choice variable. Cagetti and De nardi (2009) develop and calibrate a rich model in which individuals are perfectly altruistic and borrowing constraints arise from imperfect enforceability. Individuals have heterogeneous entrepreneurial and working abilities, and when they decide to be entrepreneurs they

choose the size of their firms, in the sense that they can hire an arbitrary number of employers. They find that estate taxation has a small effect on the saving and investment decisions of small business, but it distorts the decisions of larger firms, which has a negative effect on total output.

The empirical literature on this topic is small. The most related paper is Tsoutsoura (2015), who studies a policy that reduces succession taxes in Greece. She shows that, in the context of family firms, succession taxes cause a decrease in post-succession investment. Effects are larger for firms with low asset tangibility and for entrepreneurs that have low income from other activities. This evidence is consistent with the tightening of borrowing constraints due to the taxation.

3.2 Model

3.2.1 Demography, preferences and technologies

I consider an economy populated by a continuum of measure one of infinitely lived dynasties, indexed by $i \in [0, 1]$. In a given period of time, each dynasty is represented by a single individual, indexed by (i, t), who is risk-neutral, lives for one period and has preferences over consumption, bequests received by their children (that is, net of inheritance taxation) and effort. The treatment of bequests as a consumption good is known as *joy-of-giving* or *warm-glow* motive. There are other alternative bequest motives, for instance individuals of one generation may care about the welfare of the next generation (altruistic preferences), or individuals may not derive utility from bequests, but they end up bequeathing their estate because their lifespan is uncertain (accidental bequests). Kopczuk (2013) points out that in practice different bequest motives are not exclusive (e.g. accidental bequests may coexist with joy-of-giving), and that different individuals may have different preferences for bequests. The choice of the joy-of-giving motive is due to tractability, and the results are limited to this case.

At the beginning of their lives, individuals are endowed with one unit of labor and an initial wealth $A_{i,t}$ determined by bequests $b_{i,t-1}$ and lump-sum transfers $g_{i,t}$, that is, $A_{i,t} = b_{i,t-1} + g_{i,t}$. Initial wealth of the first generation, $\{A_{i,0}\}_{i\in[0,1]}$, is

exogenously given.

Following Banerjee and Newman (1993), I assume that all individuals have identical Cobb-Douglas utility functions over consumption and bequests. The disutility of effort is given by a function $h : \{e_L, e_H\} \to \mathbb{R}$. More specifically, utility functional form is given by $u(c, b, e) = \delta^{-1} c^{1-\alpha} b^{\alpha} - h(e)$, where c denotes consumption, b bequests received by the next generation, e effort, $\alpha \in (0, 1)$ and $\delta = \alpha^{\alpha}(1-\alpha)^{1-\alpha}$. The multiplication by δ^{-1} is without loss of generality, and aims at simplifying the indirect utility function. Denoting income realization by y and the inheritance tax rate by τ, each individual chooses c and b by solving

$$\begin{aligned} \max_{(c,b)\in\mathbb{R}^2_+} \; & \delta^{-1} c^{1-\alpha} b^{\alpha} - h(e) \\ \text{s.t.} \quad & c + \frac{1}{1-\tau} b \leq y \end{aligned}$$

where $\frac{1}{1-\tau} b$ is the amount left as bequests, implying that after taxation the next generation receives b.[3] We can view $\frac{1}{1-\tau}$ as the relative price of bequests, which is increasing in τ. The optimal choices of consumption and bequests are given by $c^* = (1-\alpha)y$ and $b^* = \alpha(1-\tau)y$, yielding the following indirect utility function: $(1-\tau)^{\alpha} y - h(e)$. If $\tau = 0$ or $\alpha = 0$ we are back to the set-up of Holmström and Tirole (1997).

There are two production technologies available to all individuals. One technology uses no capital and one unit of labor to produce $q > 0$ units of output, irrespective of the amount of effort. An individual that uses this technology will be dubbed *self-employed*. The other technology is a risky project, and an individual that undertakes it will be dubbed an *entrepreneur*. As in Holmström and Tirole (1997), this project requires $I > 0$ units of capital and one unit of labor. The project yields $R > 0$ in case of success and 0 otherwise. It is subject to moral hazard. An individual can exert high effort (e_H) or low effort (e_L). If she exerts high effort the probability of success is p_H, and the disutility is $h(e_H) = B > 0$; if she exerts low effort the probability of success is p_L, where $p_L < p_H$, and the disutility is $h(e_L) = 0$.

[3] One could think of a tax t that is levied on the amount received by the next generation. In this case, the budget constraint would be $c + (1+t)b \leq y$. We have the following relationship: $t = \frac{\tau}{1-\tau}$.

I assume that the project's net present value is negative when $e = e_L$ and that when $\tau = 0$ the gain (in utility) of becoming an entrepreneur is bigger than the gain of a self-employed worker.

Assumption 1. *$p_H R - I - B > q > 0$ and $p_L R - I < 0$*

If $I > A_{i,t}$, individual i must borrow to undertake the project. Potential lenders cannot observe effort choices, are risk-neutral, behave competitively and have the amount necessary to finance all the projects. Since the project has a negative net present value when $e = e_L$, any contract between a lender and an entrepreneur must be designed in a way that high effort is incentivized. Denoting by R_B the amount received by the borrower in case of success, the incentive compatibility constraint is

$$p_H(1-\tau)^\alpha R_B - B \geq p_L(1-\tau)^\alpha R_B \quad \text{or} \quad R_B \geq R_B(\tau) = \frac{B}{\Delta p(1-\tau)^\alpha} \tag{3.1}$$

where $\Delta p = p_H - p_L > 0$. The introduction of taxes increases the minimum payment required to incentivize high effort, $R_B(\tau)$, which is increasing in both α and τ when $\tau \in (0,1)$, in the sense that its partial derivatives are positive. Even though b^* is decreasing in τ, government's revenue, $\tau b^*/(1-\tau) = \tau\alpha y$, is increasing in τ. Therefore an increase in τ leads to a larger share of R allocated to the government, which disincentivizes high effort. An increase in α, keeping $\tau \in (0,1)$ fixed, means that individuals care more about bequests and results in an increase in b^*. A higher b^* increases the government's stake in case of success, and thus effort becomes less attractive. It must be noted that if τ is very high, R_B will be larger than R, which is not feasible, meaning that the entrepreneur will always choose to exert e_L. Therefore, we have the following constraint for τ:

$$R_B \leq R \iff \tau \leq 1 - \left(\frac{B}{\Delta p R}\right)^{\frac{1}{\alpha}} \tag{3.2}$$

Expected pledgeable income is given by:

$$\mathscr{P} = p_H \left[R - \frac{B}{\Delta p(1-\tau)^\alpha}\right]$$

Lenders must break even, thus it should be the case that:

$$\mathscr{P} = p_H \left[R - \frac{B}{\Delta p (1-\tau)^{\alpha}} \right] \geq I - A_{i,t}$$

or

$$A_{i,t} \geq A(\tau) = p_H \frac{B}{\Delta p (1-\tau)^{\alpha}} - (p_H R - I) \tag{3.3}$$

Thus, only investors with initial wealth greater than $A(\tau)$ can secure financing and undertake the project. $A(\tau)$ is increasing in τ when $\tau \in (0,1)$, that is, taxes reduce the set of individuals that are eligible to borrow. I make the following assumption to ensure that credit rationing takes place even in the absence of taxes.

Assumption 2. $A(0) = p_H \frac{B}{\Delta p} - (p_H R - I) > 0$

Since lenders behave competitively, they make no profits, and the amount R_L that they receive must satisfy

$$p_H R_L = I - A_{i,t} \quad \text{or} \quad R_L = \frac{I - A_{i,t}}{p_H} \tag{3.4}$$

If $A_{i,t} \geq A(\tau)$, $R_B = R - R_L \geq R_B(\tau) = B/[\Delta p (1-\tau)^{\alpha}]$, and the incentive-compatibility constraint is satisfied. To sum up, there are two possible types of occupation: entrepreneurship and self-employment. The realized income of an individual that becomes an entrepreneur and has initial wealth $A_{i,t} \geq I$ is $y = R - I + A_{i,t}$ in case of success and $y = A_{i,t} - I$ otherwise; if $A_{i,t} < I$, $y = R - (I - A_{i,t})/p_H$ in case of success and $y = 0$ otherwise. The realized income of a self-employed individual is $y = q + A_{i,t}$.

3.2.2 Dynamics with no taxes

When $\tau = 0$, Assumption 1 and the fact that lenders behave competitively imply that individuals always prefer entrepreneurship. However, only individuals that have wealth greater than $A(0)$ can choose entrepreneurship, where $A(0) = \frac{B}{\Delta p} - (p_H R - I)$. Recalling that optimal bequests correspond to a share $\alpha(1-\tau)$ of realized income, initial wealth evolves according to

$$A_{i,t+1} = \begin{cases} \alpha(A_{i,t}+q) & \text{w.p. } 1 & \text{if } A_{i,t} < A(0) \\ 0 & \text{w.p. } 1-p_H & \text{if } A(0) \leq A_{i,t} < I \\ \alpha(R-\frac{I-A_{i,t}}{p_H}) & \text{w.p. } p_H & \text{if } A(0) \leq A_{i,t} < I \\ \alpha(A_{i,t}-I) & \text{w.p. } 1-p_H & \text{if } A_{i,t} \geq I \\ \alpha(R-I+A_{i,t}) & \text{w.p. } p_H & \text{if } A_{i,t} \geq I \end{cases}$$

I make the following assumption throughout the paper to make the model more tractable.

Assumption 3. *If entrepreneurs exert high effort, the probability of success is equal to 1,* $p_H = 1$.

Assumption 3 makes the evolution of wealth deterministic, given by

$$A_{i,t+1} = \begin{cases} \alpha(A_{i,t}+q) & \text{if } A_{i,t} < A(0) \\ \alpha(R-I+A_{i,t}) & \text{if } A_{i,t} \geq A(0) \end{cases}$$

Since $\alpha \in (0,1)$ the two equations determining the evolution of wealth have unique stationary points, and any stationary distribution takes the form described below. A mass λ of self-employed individuals with wealth $\underline{A}$ given by

$$\underline{A} = \alpha(\underline{A}+q) \iff \underline{A} = \frac{\alpha q}{1-\alpha},$$

and a mass $1-\lambda$ of entrepreneurs with wealth $\bar{A}$ given by

$$\bar{A} = \alpha(R-I+\bar{A}) \iff \bar{A} = \frac{\alpha(R-I)}{1-\alpha},$$

By Assumption 1, $q < R-I$, and $\underline{A} < \bar{A}$. We have three possible cases:

$$\begin{cases} \lambda = 1 & \text{if } \bar{A} < A(0) & \textit{only self-employed} \\ \lambda \in [0,1] & \text{if } \underline{A} < A(0) \leq \bar{A} & \textit{possibly self-employed and entrepreneurs} \\ \lambda = 0 & \text{if } A(0) \leq \underline{A} & \textit{only entrepreneurs} \end{cases}$$

To ensure that a stationary distribution can have a positive mass of both entrepreneurs and self-employed, it should be the case that $\underline{A} < A(0) \leq \bar{A}$, as described

in Figure 3.1. The following assumption guarantees that this condition is satisfied.[4]

Assumption 4. $q < \frac{1-\alpha}{\alpha}\left[\frac{B}{\Delta p} - (R-I)\right]$ *and* $R > (1-\alpha)\frac{B}{\Delta p} + I$

Assumption 4 implies that all dynasties that start with wealth smaller than $A(0)$ will have wealth $\underline{A}$ in the long run, and λ will be the measure at time 0 of the set $\{i \in [0,1] : A_{i,0} < A(0)\}$. Indeed, if $A_{i,0} < A(0)$,

$$A_{i,t} = \alpha^t A_{i,0} + \sum_{j=1}^{t} \alpha^j q \Rightarrow \lim_{t\to\infty} A_{i,t} = \underline{A}$$

A similar argument shows that all dynasties that start with wealth bigger than $A(0)$ will have wealth $\bar{A}$ in the long run, and the measure of entrepreneurs in the steady-state, $1-\lambda$, is the measure at time 0 of the set $\{i \in [0,1] : A_{i,0} \geq A(0)\}$.

Dynasties that end up with wealth $\underline{A}$ can be said to be trapped in poverty. The technologies available and imperfections in the credit market lead to convergence conditional on the initial wealth of the first generation, and dynasties that start poor tend to remain poor. As pointed out by Ghatak (2015), this result differs from the Solow model, in which no financial frictions and convex technologies result in unconditional convergence: all agents (or countries) converge to the same level of wealth irrespective of their initial wealth. Since Assumption 4 implies that poverty traps occur in the model, tax and transfers policies that aim to eliminate them or, less ambitiously, reduce wealth inequality, may improve social welfare.

3.3 Redistributive taxation

The government collects taxes at the end of a period and makes lump-sum transfers at the beginning of the next period. It balances its budget, in the sense that

$$\int_0^1 g_{i,t+1} di = \int_0^1 \frac{\tau_{i,t}}{1-\tau_{i,t}} b_{i,t} di \quad \forall t \in \{0,1,2,\dots\}$$

[4]Assumptions 1-4 impose a set of restrictions on R. It cannot be very big so that in case of low effort the project has a positive NPV or individuals with no wealth have access to credit, $R < \min\{\frac{B}{\Delta p} + I, \frac{I}{p_L}\}$; but it can also not be very small so that all individuals are self-employed in the steady-state or self-employment yields more utility than entrepreneurship in the absence of taxes, $R > \max\{(1-\alpha)\frac{B}{\Delta p} + I, I + B + q\}$.

where $\tau_{i,t}$ and $g_{i,t}$ are the the tax rate and lump-sum transfer of individual (i,t).

Apart from the benchmark case with no taxation, I consider two possible simple tax and transfer structures, which I call *structure 1* and *structure 2*. *Structure 1* treats all the agents equally, in the sense that everyone pays the same tax rate and receives the same lump-sum grant. *Structure 2* treats agents differently, in the sense that transfers are targeted at the poor, and only the rich individuals pay taxes.

Structure 1. All individuals pay the same tax rate τ and receives the same lump-sum grant, $g_{i,t+1} = g_{t+1}(\tau)$ for all i. The notation emphasizes that the amount transferred at time $t+1$ is a function of τ, but it is worth noting that it also depends on the wealth distribution at the beginning of period t. Government's budget constraint is

$$g_{t+1}(\tau) = \int_0^1 \frac{\tau}{1-\tau} b_{i,t} di \quad \forall t \in \{0, 1, 2, \dots\}$$

In this system, an individual prefers entrepreneurship when

$$(1-\tau)^\alpha (R - I + A_{i,t}) - B \geq (1-\tau)^\alpha (q + A_{i,t}) \quad \text{or} \quad \tau \leq \bar{\tau} = 1 - \left(\frac{B}{R-I-q}\right)^{\frac{1}{\alpha}}$$

By Assumption 1, $R - I - q > B$, which implies that $\bar{\tau} < 1$. Assumption 1 also guarantees that the constraint given by Equation 3.2 is satisfied when $\tau < \bar{\tau}$.

Structure 2. Only individuals with initial wealth greater than $A(\tau)$ pay taxes. Individuals that receive bequests greater than $A(\tau)$ do not receive a transfer; all individuals that receive bequest smaller than $A(\tau)$ receive the same transfer $g_{t+1}(\tau)$. Once all individuals have initial wealth greater than $A(\tau)$, taxes no longer serve a purpose, and I assume that they are set to zero.

Let the function $G_t(a)$ denote the measure of individuals at time t that receive bequest strictly smaller than $a \in \mathbb{R}$. Denoting by Ω_t the set of individuals with initial wealth greater than $A(\tau)$ in period t, the government's the budget constraint is

$$g_{t+1}(\tau) G_{t+1}(A(\tau)) = \int_{\Omega_t} \frac{\tau}{1-\tau} b_{i,t} di \quad \forall t \in \{0, 1, 2, \dots\}$$

Individuals that have enough initial wealth to become entrepreneurs will prefer entrepreneurship when $\tau \leq \bar{\tau}$, as in *structure 1*. I assume throughout the paper that

the social planner always set $\tau \leq \bar{\tau}$.

3.3.1 Dynamics

Structure 1. Initial wealth evolves according to

$$A_{i,t+1} = \begin{cases} \alpha(1-\tau)(A_{i,t}+q) + g_{t+1}(\tau) & \text{if } A_{i,t} < A(\tau) \\ \alpha(1-\tau)(R+A_{i,t}-I) + g_{t+1}(\tau) & \text{if } A_{i,t} \geq A(\tau) \end{cases}$$

Since $\tau(1-\alpha) < 1$, a stationary distribution is characterized by a mass λ_1 of self-employed individuals with wealth $\underline{A}_1(\tau)$, a mass $1-\lambda_1$ of entrepreneurs with wealth $\bar{A}_1(\tau)$, and transfers $g_1(\tau)$ given by[5]

$$\underline{A}_1(\tau) = \frac{\alpha(1-\tau)q}{1-\alpha(1-\tau)} + \frac{\tau\alpha[\lambda_1 q + (1-\lambda_1)(R-I)]}{(1-\alpha)[1-\alpha(1-\tau)]} \tag{3.5}$$

$$\bar{A}_1(\tau) = \frac{\alpha(1-\tau)(R-I)}{1-\alpha(1-\tau)} + \frac{\tau\alpha[\lambda_1 q + (1-\lambda_1)(R-I)]}{(1-\alpha)[1-\alpha(1-\tau)]} \tag{3.6}$$

$$g_1(\tau) = \frac{\tau\alpha}{1-\alpha}[\lambda_1 q + (1-\lambda_1)(R-I)] \tag{3.7}$$

We have the following properties: $\underline{A}_1(\tau) < \bar{A}_1(\tau)$ and when $\lambda_1 \in (0,1)$, $\underline{A}_1'(\tau) > 0$, $\bar{A}_1'(\tau) < 0$. In particular, $\underline{A}_1(\tau) \geq \underline{A}$ and $\bar{A}_1(\tau) \leq \bar{A}$. Even under Assumption 4, there are three possible cases. When there are only entrepreneurs or only self-employed, the stationary points are the same as those in the case without taxation.

$$\begin{cases} \lambda_1 = 1 & \text{only self-employed with wealth } \underline{A}_1(\tau) = \underline{A} \\ \lambda_1 \in (0,1) & \text{self-employed with wealth } \underline{A}_1(\tau) \text{ and entrepreneurs with wealth } \bar{A}_1(\tau) \\ \lambda_1 = 0 & \text{only entrepreneurs with wealth } \bar{A}_1(\tau) = \bar{A} \end{cases}$$

Structure 2. Initial wealth evolves according to[6]

[5]The equations are derived in the Appendix.

[6]Bequests left by self-employed individuals are smaller than $A(\tau)$, and thus their offspring are entitled to receive transfers. To see this note that Assumption 4 can be rewritten as: $\alpha[A(0)+q] < A(0)$. Since $A(0) < A(\tau)$ and $\alpha \in (0,1)$, $\alpha[A(\tau)+q] < A(\tau)$. If $A_{i,t} < A(\tau)$, then $\alpha[A_{i,t}+q] < \alpha[A(\tau)+q] < A(\tau)$. On the other hand, bequests left by entrepreneurs are bigger than $A(\tau)$, and thus their offspring are not entitled to receive transfers. If $A_{i,t} > A(\tau)$ then $\alpha(1-\tau)(R+A_{i,t}-I) = \alpha(1-\tau)(R-I) + \alpha(1-\tau)A_{i,t} = [1-\alpha(1-\tau)]\bar{A}_2(\tau) + \alpha(1-\tau)A_{i,t} > A(\tau)$, as both $\bar{A}_2(\tau)$ and

$$A_{i,t+1} = \begin{cases} \alpha(A_{i,t} + q) + g_{t+1}(\tau) & \text{if } A_{i,t} < A(\tau) \\ \alpha(1-\tau)(R + A_{i,t} - I) & \text{if } A_{i,t} \geq A(\tau) \end{cases}$$

A stationary distribution is characterized by a mass λ_2 of self-employed individuals with wealth $\underline{A}_2(\tau)$, a mass $1 - \lambda_2$ of entrepreneurs with wealth $\bar{A}_2(\tau)$, and transfers $g_2(\tau)$ given by

$$\underline{A}_2(\tau) = \frac{\alpha q}{1-\alpha} + \frac{\tau\alpha(1-\lambda_2)(R-I)}{\lambda_2(1-\alpha)[1-\alpha(1-\tau)]} \tag{3.8}$$

$$\bar{A}_2(\tau) = \frac{\alpha(1-\tau)(R-I)}{1-\alpha(1-\tau)} \tag{3.9}$$

$$g_2(\tau) = \frac{\tau\alpha}{\lambda_2[1-\alpha(1-\tau)]}(1-\lambda_2)(R-I) \tag{3.10}$$

Recalling that when all individuals are entrepreneurs, or all individual are self-employed, taxes are set to zero, the three cases that applied to *structure 1* are also applicable to *structure 2*:

$$\begin{cases} \lambda_2 = 1 & \text{only self-employed with wealth } \underline{A}_2(\tau) = \underline{A} \\ \lambda_2 \in (0,1) & \text{self-employed with wealth } \underline{A}_2(\tau) \text{ and entrepreneurs with wealth } \bar{A}_2(\tau) \\ \lambda_2 = 0 & \text{only entrepreneurs with wealth } \bar{A}_2(\tau) = \bar{A} \end{cases}$$

3.3.2 Social planner

We can think of a social planner that chooses a tax structure, that is, $\{g_{i,t}, \tau_{i,t}\}$, to maximize social welfare of all generations. The choice is made at time zero with full commitment. Denoting Pareto weights by $\omega_{i,t} \geq 0$, the set of individuals that pay taxes by Ω_t, the set of individuals that receive transfers by Γ_t, and realized

$A_{i,t}$ are bigger than $A(\tau)$. Assumption 4 guarantees that there exists a positive number k such that $\bar{A}_2(\tau) \geq A(\tau)$ for all $\tau \in [0,k]$. In the numerical examples of this paper, $\bar{A}_2(\tau) < A(\tau)$ is not a concern.

income by $y_{i,t}$, the social planner problem is

$$\max \sum_{t=0}^{\infty} \int_0^1 \omega_{i,t}[(1-\tau_{i,t})^{\alpha} y_{i,t} - h(e_{i,t})]di$$

$$\text{s.t.} \quad (i) \int_{\Gamma_{t+1}} g_{i,t}di = \int_{\Omega_t} \frac{\tau}{1-\tau} b_{i,t}di \quad \forall t \in \{0,1,2,\dots\},$$

$$(ii) \text{ each individual chooses the occupation optimally,}$$

$$\text{given} \{A_{i,0}\}_{i\in[0,1]}$$

We can also think of a social planner that wants to maximize steady-state social welfare. In this case, there are only two groups of individuals – self-employed and entrepreneurs– with wealth $\underline{A}$ and $\bar{A}$, and Pareto weights ω and $1-\omega$. Denoting by SW_1 the maximum social welfare attainable under *structure 1*, and using the notation $\lambda_1(\tau)$ to make explicit that the proportion of self-employed individuals in the steady-state depends on the choice of the tax rate and the tax structure, we have

$$SW_1 = \max_{\{\tau\}} \Bigg\{ \omega\lambda_1(\tau)\big\{(1-\tau)^{\alpha}[\underline{A}_1(\tau)+q]\big\} + (1-\omega)[1-\lambda_1(\tau)]\big\{(1-\tau)^{\alpha}[R-I+\bar{A}_1(\tau)]-B\big\}\Bigg\}, \text{ given} \{A_{i,0}\}_{i\in[0,1]}$$

where $\underline{A}_1(\tau)$ and $\bar{A}_1(\tau)$ are given by equations 3.5 and 3.6, respectively. Similarly, denoting by SW_2 the maximum social welfare attainable under *structure 2*, we have

$$SW_2 = \max_{\{\tau\}} \Bigg\{ \omega\lambda_2(\tau)\big\{\underline{A}_2(\tau)+q\big\} + (1-\omega)[1-\lambda_2(\tau)]\big\{(1-\tau)^{\alpha}[R-I+\bar{A}_2(\tau)]-B\big\}\Bigg\}, \text{ given} \{A_{i,0}\}_{i\in[0,1]}$$

where $\underline{A}_2(\tau)$ and $\bar{A}_2(\tau)$ are given by equations 3.8 and 3.9, respectively. The dependence of $\lambda_1(.)$ and $\lambda_2(.)$ on the the wealth distribution at time zero makes it difficult to find a general solution to this problem, and I have to resort to numerical examples.

If the social planner maximizes steady-state social welfare, the ideal tax structure

would be such that there are only entrepreneurs in the steady-state, that is, $\lambda_1(\tau) = 0$ in case of *structure 1*, or $\lambda_2(\tau) = 0$ in case of *structure 2*.[7] Importantly, the first-bet does not depend on the Pareto weight ω. This outcome, however, is not always feasible, and a tax structure would only serve the purpose of reducing wealth inequality in the steady-state. However, inequality reduction comes at a cost: since taxes tighten borrowing constraints (the threshold increases from $A(0)$ to $A(\tau)$), they can potentially reduce the mass of entrepreneurs, which negatively affects total output. In this case, the optimal policy will depend on the Pareto weight ω.

It is worth noting that if the social planner cares about the welfare of all generations, a policy that generates a steady-state distribution with a mass one of entrepreneurs might not be optimal, since entrepreneurs experience welfare losses in the beginning, before all agents have enough wealth to become entrepreneurs.

3.3.3 Numerical examples

Example 1. In this example, I consider *structure 1*. I show that the introduction of taxes deplete the productivity of the economy in the short run. This decrease is due to the fact that taxes increase the wealth required to become an entrepreneur, and transfers are not enough to turn a large number self-employed individuals into entrepreneurs. However, if the taxation is large enough, even though the GDP suffers in the short run, transfers are high enough so that the policy can attain a steady-sate in which all individuals are entrepreneurs, and thus GDP per capita is maximized. Figure 3.2 shows how the initial wealth cutoff increases with the tax rate. The trajectory of GDP per capita and the share of entrepreneurs is illustrated in Figure 3.3. For small tax rates, GDP per capita is always smaller than in the benchmark case with no taxes. However, when the tax rate is 25%, a steady-state in which all individuals are entrepreneurs is attained. If the tax rate is 26.5%, the increase in the tax rate is not enough to offset the reduction in the mass of entrepreneurs, and the mass of entrepreneurs in the steady-state is again smaller than 1.

Figure 3.4 shows steady-state values. The relationship between tax rate and GDP per capita has the form of a U-shaped curve. For small values of the tax rate,

[7]Since transfers are the only government expenditures, taxes are set to zero in both structures once all agents have the same occupation.

the policy harms GDP because the mass of entrepreneurs is depleted. However, if the taxes are high enough, transfers allow self-employed individuals to become entrepreneurs, and thus GDP starts to increase. If the tax rate is high enough, all individuals are entrepreneurs in the steady-state. However, after some point, the increase in the minimum wealth to become an entrepreneur is so severe that again the mass of entrepreneurs is smaller than 1 in the steady-state.

In Figure 3.5, I plot the steady-state values of the Gini coefficient. In the case of two levels of income, the coefficient is equal to the proportion of wealth owned by the entrepreneurs minus the proportion of entrepreneurs. Because, for small tax rates, the proportion of entrepreneurs decreases more rapidly than the share of wealth owned by them, the Gini coefficient increases slightly. However, it starts to decrease and reaches zero when all individuals are entrepreneurs. Figure 5 also plots the difference between steady-state wealth of entrepreneurs and self-employed. This measure monotonically decreases with the tax rate, and reaches zero when all individuals have the same wealth.

Example 2. In example 1, the initial wealth is uniformly distributed over [0,10] and the cutoff to become an entrepreneur in case of no taxation is relatively small, 1. Therefore, there is a large mass of entrepreneurs to be taxed, and thus a steady-state with a mass 1 of entrepreneurs can be attained with the appropriate tax rate. However, if the initial distribution is uniformly distributed over [0,3], the initial mass of entrepreneurs is much smaller, and no tax rate can generate a steady-state with a mass 1 of entrepreneurs. In such case, redistribution has only one purpose: reducing inequality. This reduction, however, occurs at the expense of productivity and GDP per capita. Figure 3.6 shows steady-state values of GDP per capita and the Gini coefficient. GDP per capita monotonically decreases with the tax rate, while the Gini coefficient initially increases, but, for appropriate tax rates, it is smaller that the Gini coefficient with no taxation.

Example 3. Now I show that *structure 2* can be a solution in the case in which initial wealth is uniformly distributed over [0,3]. The policy can attain a steady-state with a mass 1 of entrepreneurs for a broad set of tax rates. If the tax rate is too small, such that the mass of entrepreneurs is large but the taxes paid

on each dollar bequeathed are small, or very large, such that the taxes collected on each dollar bequeathed are large but the mass of entrepreneurs is small, the policy cannot attain a steady-state with only entrepreneurs. Figure 3.7 shows the evolution of GDP per capita and the share of entrepreneurs, while Figure 3.8 shows steady-state values. Figure 3.9 shows steady-state values for the Gini coefficient. In this case, taxation always decreases the coefficient in comparison to the case with no taxes.

It is worth noting that, even with *structure 2*, a steady-state with a mass 1 of entrepreneurs is not always attainable. For example, this is the case when initial wealth is uniformly distributed over [0.2]. Apart from tax and transfer structures, policies that aim at improving the functioning of credit markets are also effective. If, for example, we interpret B as private benefits, policies that can reduce B render borrowing constraints less tight, which alleviates the importance of initial wealth in the occupational choice decision. If the reduction in B is such that $A(0) < \underline{A}$ holds, all agents can choose entrepreneurship at some point, and taxes are no longer needed.

3.4 Conclusion

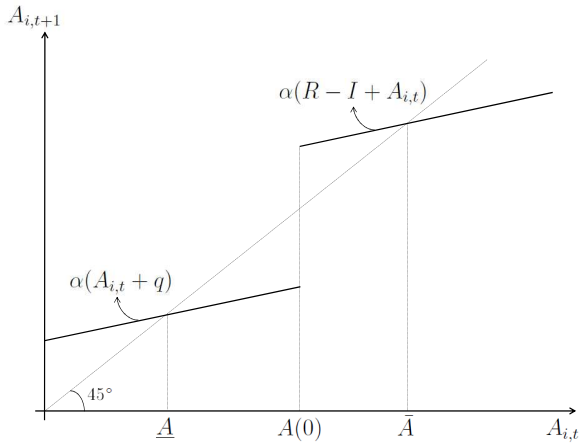
In the presence of borrowing constraints and a fixed-investment technology, dynasties (or individuals) that start with different levels of wealth converge to different steady-states. In particular, dynasties that start poor tend to remain poor. In this case, inheritance taxes and transfers can play an important role. In the model, I endogenize the effect of taxation on credit constraints – there is an increasing relationship between taxes and the initial wealth required to become an entrepreneur. Even though taxes tighten borrowing constraints and thus deplete the productivity of the economy in the short run, they can fight poverty traps and wealth inequality. Importantly, in the long run, for some set of parameters, the trade-off equity-efficiency is not always present. That is, the policy can also attain a situation in which every individual can choose the most profitable occupation and aggregate productivity is maximized.

I studied two tax designs. One design, called *structure 1*, treats people more

equally, in the sense that all individuals pay the same tax rate and receive the same lump-sum grant. The other design, called *structure 2*, treats agents differently, in the sense that the poor receive grants, and the rich pay taxes. Both structures can be an effective way of fighting poverty traps, but in general *structure 2* does a better job. In practice, however, *structure 2* may face more resistance, as favouring specific groups to the detriment of others may be politically costly. When *structure 1* cannot perform well, and *structure 2* is politically unfeasible, policies that aim at reducing credit market imperfections may be useful.

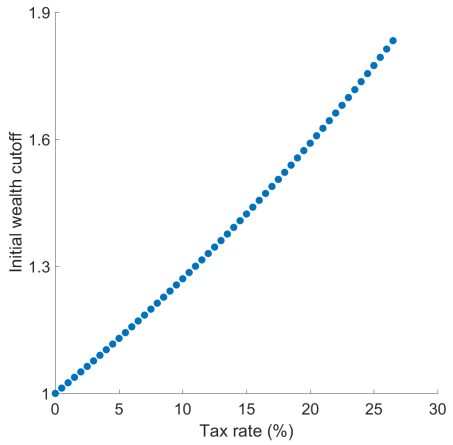
3.5 Figures

Figure 3.1: **Wealth evolution and its stationary points in the absence of taxation**



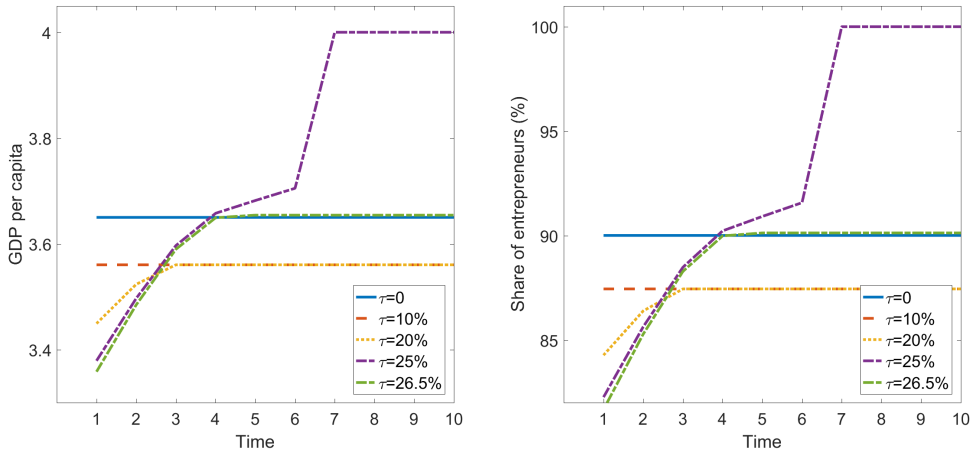
Notes: I assume that Assumption 4 is satisfied. Therefore, $\underline{A} < A(0) < \bar{A}$.

Figure 3.2: **Initial wealth required to become an entrepreneur as a function of the tax rate**



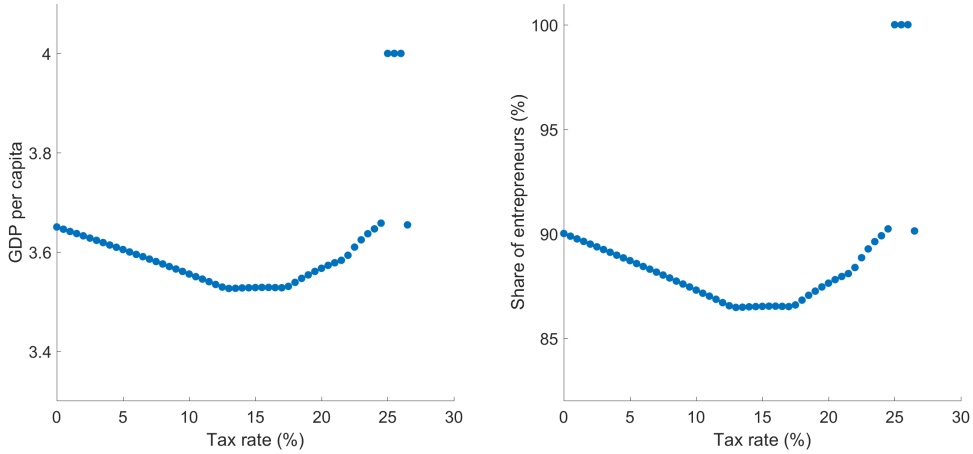
Notes: I assume that $\alpha = 0.5$, $R = 10$, $q = 0.5$, $B = 3$, $I = 6$, and $p_L = 0.4$.

Figure 3.3: **Evolution of GDP per capita and mass of entrepreneurs for different tax rates**



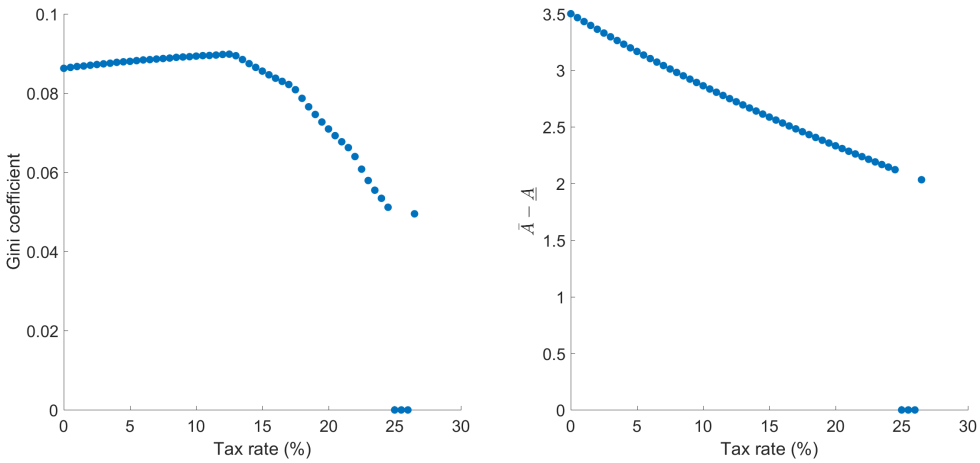
Notes: I assume that the government implements *structure 1*, initial wealth is uniformly distributed over $[0, 10]$, $\alpha = 0.5$, $R = 10$, $q = 0.5$, $B = 3$, $I = 6$, and $p_L = 0.4$.

Figure 3.4: **Steady-state values of GDP per capita and mass of entrepreneurs for different tax rates**



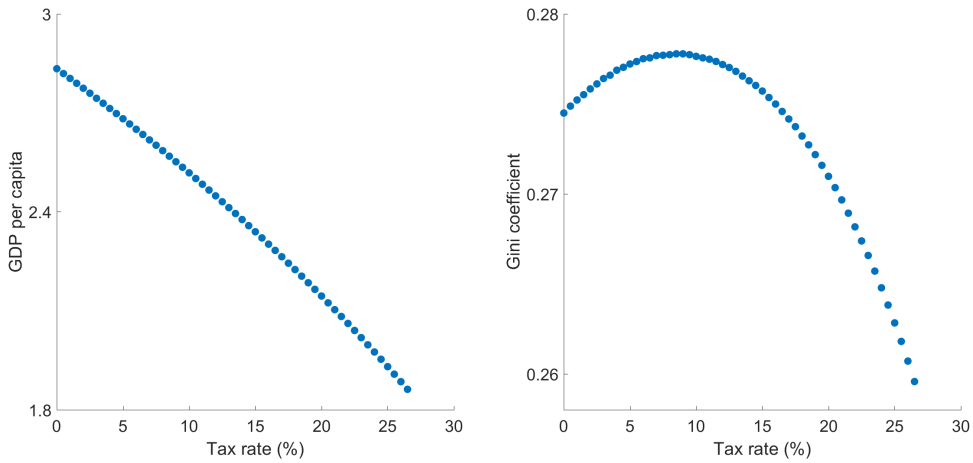
Notes: I assume that the government implements *structure 1*, initial wealth is uniformly distributed over $[0, 10]$, $\alpha = 0.5$, $R = 10$, $q = 0.5$, $B = 3$, $I = 6$, and $p_L = 0.4$.

Figure 3.5: **Evolution of steady-state levels of the Gini coefficient and the difference between the highest wealth level and the lowest wealth level for different tax rates**



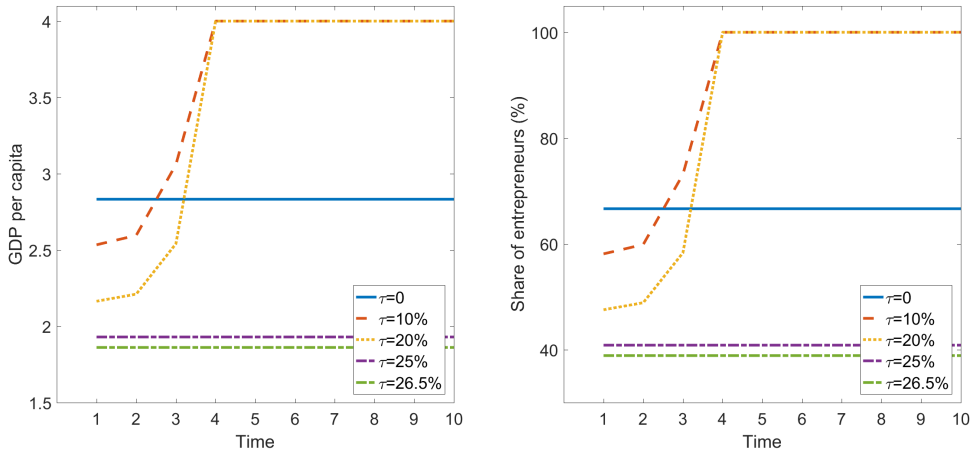
Notes: I assume that the government implements *structure 1*, initial wealth is uniformly distributed over $[0, 10]$, $\alpha = 0.5$, $R = 10$, $q = 0.5$, $B = 3$, $I = 6$, and $p_L = 0.4$.

Figure 3.6: **Evolution of steady-state levels of GDP per capita and the Gini coefficient for different tax rates**



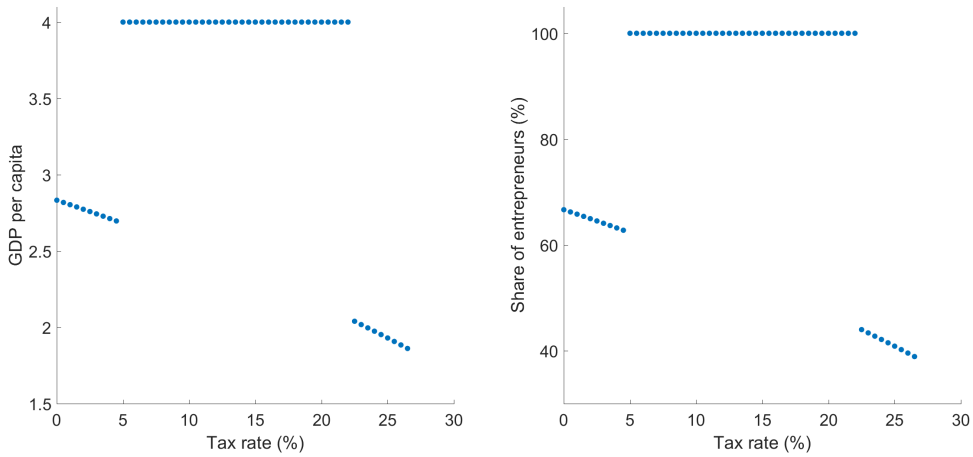
Notes: I assume that the government implements *structure 1*, initial wealth is uniformly distributed over $[0, 3]$, $\alpha = 0.5$, $R = 10$, $q = 0.5$, $B = 3$, $I = 6$, and $p_L = 0.4$.

Figure 3.7: **Evolution of GDP per capita and mass of entrepreneurs for different tax rates**



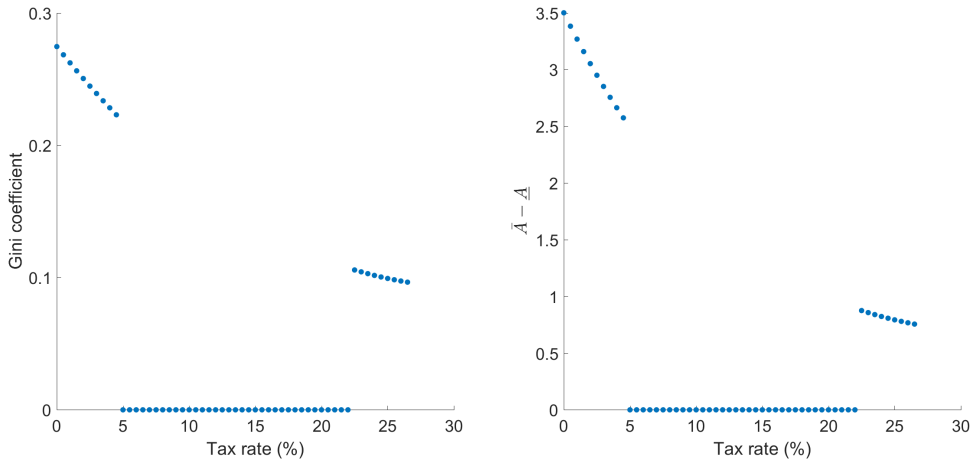
Notes: I assume that the government implements *structure 2*, initial wealth is uniformly distributed over $[0, 3]$, $\alpha = 0.5$, $R = 10$, $q = 0.5$, $B = 3$, $I = 6$, and $p_L = 0.4$.

Figure 3.8: **Steady-state values of GDP per capita and mass of entrepreneurs for different tax rates**



Notes: I assume that the government implements *structure 2*, initial wealth is uniformly distributed over $[0, 3]$, $\alpha = 0.5$, $R = 10$, $q = 0.5$, $B = 3$, $I = 6$, and $p_L = 0.4$.

Figure 3.9: **Evolution of steady-state levels of the Gini coefficient and the difference between the highest wealth level and the lowest wealth level**



Notes: I assume that the government implements *structure 2*, initial wealth is uniformly distributed over $[0, 3]$, $\alpha = 0.5$, $R = 10$, $q = 0.5$, $B = 3$, $I = 6$, and $p_L = 0.4$.

3.6 Appendix

3.6.1 Stationary distributions

Structure 1 $\underline{A}(\tau)$, $\bar{A}(\tau)$ and $g(\tau)$ are given by

$$\underline{A}(\tau) = \alpha(1-\tau)(\underline{A}(\tau)+q) + g(\tau) \Longleftrightarrow \underline{A}(\tau) = \frac{\alpha(1-\tau)q + g(\tau)}{1-\alpha(1-\tau)},$$

$$\bar{A}(\tau) = \alpha(1-\tau)(R+\bar{A}(\tau)-I) + g(\tau) \Longleftrightarrow \bar{A}(\tau) = \frac{\alpha(1-\tau)(R-I) + g(\tau)}{1-\alpha(1-\tau)},$$

$$g(\tau) = \tau\alpha[\lambda(\underline{A}(\tau)+q) + (1-\lambda)(R+\bar{A}(\tau)-I)]$$

Plugging the expressions of $\underline{A}(\tau)$ and $\bar{A}(\tau)$ into the expression of $g(\tau)$, we obtain

$$g(\tau) = \frac{\tau\alpha}{1-\alpha}[\lambda q + (1-\lambda)(R-I)]$$

Now plugging this expression of $g(\tau)$ into the expressions of $\underline{A}(\tau)$ and $\bar{A}(\tau)$, we obtain

$$\underline{A}(\tau) = \frac{\alpha(1-\tau)q}{1-\alpha(1-\tau)} + \frac{\tau\alpha[\lambda q + (1-\lambda)(R-I)]}{(1-\alpha)[1-\alpha(1-\tau)]}$$

and

$$\bar{A}(\tau) = \frac{\alpha(1-\tau)(R-I)}{1-\alpha(1-\tau)} + \frac{\tau\alpha[\lambda q + (1-\lambda)(R-I)]}{(1-\alpha)[1-\alpha(1-\tau)]}$$

Structure 2 Same procedure.

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