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DOUTORADO EM ADMINISTRAÇÃO

THE EFFECTS OF TRANSPARENCY AND ANTI-CORRUPTION INVESTIGATIONS ON FINANCIAL MARKETS

TESE APRESENTADA À ESCOLA BRASILEIRA DE ADMINISTRAÇÃO PÚBLICA E DE EMPRESAS PARA OBTENÇÃO DO TÍTULO DE DOUTOR

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"THE EFFECTS OF TRANSPARENCY AND CORRUPTION INVESTIGATION ON FINANCIAL MARKETS''.

TESE APRESENTADO(A) AO CURSO DE DOUTORADO EM ADMINISTRAÇÃO PARA OBTENÇÃO DO GRAU DE DOUTOR(A) EM ADMINISTRAÇÃO.

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Dedico esta conquista aos meus pais, que sempre me incentivaram nos estudos e que me apoiaram em meio às inúmeras adversidades encontradas durante a trajetória acadêmica.

Se não fosse por eles, pelo incondicional suporte familiar recebido dos meus falecidos avós antes de partirem e pelo meu companheiro de vida, não teria resiliência e determinação para conseguir este título tão difícil.

RESUMO

Nos três capítulos desta tese, eu proponho que tanto a transparência quanto investigações anticorrupção são importantes mecanismos capazes de reduzir as assimetrias de informação nos mercados financeiros. Neste sentido, o primeiro capítulo visa formalizar a importância da transparência no mercado de ações brasileiro e mostra como os investidores respondem a graus adicionais de informações no longo prazo. Os resultados documentam que padrões mais elevados de governança resultam em retornos anormais de ações significativamente mais elevados no longo prazo, uma vez controlando para efeitos fixos de empresa e tempo. O impacto positivo aumentou após a crise financeira global, a microestrutura do mercado melhorou e o impacto no mercado é mais forte para empresas financeiramente saudáveis. As evidências sugerem que o compromisso com padrões mais elevados de governança corporativa compensou as empresas brasileiras no longo prazo. O segundo e o terceiro capítulos voltam a atenção para uma análise a nível de banco-firma, avaliando como os bancos reagem às investigações anticorrupção de duas maneiras distintas: diretamente às empresas afetadas (segundo capítulo) ou indiretamente às empresas não-afetadas (terceiro capítulo). Em outras palavras, o capítulo dois investiga se e como substanciais investigações anticorrupção envolvendo um conjunto de grandes empresas na economia brasileira afetaram seu crédito bancário e a qualidade de seus empréstimos. Os resultados indicam efeitos adversos no volume geral de crédito, levando a uma contração considerável do crédito e à deterioração dos prazos dos empréstimos para as empresas de construção civil envolvidas em esquemas de corrupção. Mais especificamente, as empresas mais afetadas recebem menos crédito, têm piores *scores* de crédito e exibem maiores provisões para perdas após o início das investigações. Esse efeito é menos pronunciado para empresas mais conectadas ao governo e com mais trabalhadores qualificados, enquanto é mais pronunciado para empresas com mais relacionamentos bancários. Os principais resultados são robustos a diferentes grupos de controle, 2 algoritmos alternativos de *matching* e uma à

implementação de um controle sintético. O capítulo três, por outro lado, aborda potenciais efeitos colaterais no mercado, bem como a realocação direcionada a empresas comparáveis não envolvidas em qualquer investigação anticorrupção. Os resultados mostram que as empresas não afetadas recebem menos crédito dos bancos afetados, ou seja, aquelas instituições que exibem uma maior exposição de crédito às empresas envolvidas em escândalos de corrupção. Essa diminuição no crédito é menor para os tomadores existentes que têm vários relacionamentos bancários e vêm de estados economicamente mais fortes. Essas empresas também têm maior probabilidade de obter empréstimos com maturidades maiores e terem upgrade no *score* de crédito. Em contrapartida, novos tomadores de empréstimos politicamente conectados e do setor de construção têm maior probabilidade de obter crédito, assim como aqueles que são mais jovens e de estados economicamente mais fracos. Para além do exposto, as empresas não afetadas (tomadores de empréstimo existentes) com uma exposição maior aos bancos afetados reduzem o nível de empregabilidade e os salários no período pós-investigações e esse efeito se intensifica com o tempo. Os resultados são robustos a diferentes critérios de seleção para o grupo tratado e a diferentes janelas de eventos em torno do evento.

Palavras-chave: Corrupção; Governança Corporativa; Mercados Financeiros; Crédito Bancário; Realocação de Crédito; Efeitos Reais; Mercados Emergentes; DID

ABSTRACT

In the three chapters of this thesis, I advocate that both transparency and anti-corruption investigations are important mechanisms able to reduce information asymmetries in financial markets. Therefore, the first chapter aims at formalizing the importance of transparency in the Brazilian stock market and to show how investors respond to additional degrees of information commitment in long run. Results document that higher standards of governance result in significantly higher abnormal stock returns in the long run, controlling for firm and time fixed effects. The positive impact has increased after the Global Financial Crisis, market microstructure improved, and the market impact is stronger for financially healthy firms. The evidence suggests that committing to higher standards of corporate governance paid off for Brazilian firms in the long run. The second and third chapters turn the attention to a micro level analysis, evaluating how banks react to anticorruption investigations in two ways: directly vis-à-vis affected firms (second chapter) or indirectly vis-à-vis non-affected firms (third chapter). In other words, chapter two investigates whether and how substantial anti-corruption investigations concerning a set of major firms in the Brazilian economy affected their bank credit and the quality of their loans. Results indicate adverse effects on the credit flow volumes, leading to a considerable credit contraction and loan term deterioration for construction firms involved in corruption schemes. More specifically, affected firms receive less credit, lower credit ratings and display higher loan loss provisions after the investigations have started. This effect is less pronounced for firms which are more connected to the government and have more skilled workers, while it is more pronounced for firms with more bank relationships in the pre period. The main results are robust to different control groups, 2 alternative matching algorithms and a synthetic control approach. Chapter three, on the other hand, addresses potential spillover effects on other firms and inherent reallocation effects to comparable firms not involved in any anti-corruption investigation. Results show that non-affected firms receive

less credit from affected banks, i.e., those who exhibit a relatively high ex ante credit exposure to affected firms. This decrease in credit is smaller for existing borrowers who have multiple bank relationships and come from economically stronger states. These firms are also more likely to obtain longer maturity loans and experience credit rating upgrades. Nonetheless, new borrowers who are politically connected and from the construction sector are more likely to obtain credit, as well as those who are younger and from economically weaker states. Third, non-affected firms (existing borrowers) with a higher exposure to affected banks reduce employment and wages in the post period and this effect intensifies over time. The results are robust to different selection criteria for the treated group and to different event windows surrounding the event.

Keywords: Corruption; Corporate Governance; Financial Markets; Bank Credit; Credit Reallocation; Real Effects; Emerging Markets; DID

CONTENTS

1. INTRODUCTION: THE EFFECTS OF TRANSPARENCY AND ANTI-CORRUPTION INVESTIGATIONS ON FINANCIAL MARKETS

The need for transparency in financial markets is evident: an effective stakeholder protection mechanism that helps shareholders gain a better understanding of firms' executive practices, is usually pleaded as an important apparatus able to mitigate the information asymmetry and agency costs faced in financial markets. In this regard, influenced by the law Sarbanes-Oxley (SOX) and transparency trends around the world, Brazil is actively engaged in improving transparency[1] and providing more information to the public. In recent decades, for example, in addition to the updates implemented in the financial market[2] and the creation of special listing segments with additional disclosure of information, other initiatives such as an anti-corruption program were implemented by the *Controladoria Geral da União* (CGU) – auditing municipalities regarding their use of federal funds, and anti-corruption investigations began to become public – the *Operação Lava Jato* (Car Wash Operation, 2014-2021), for example.

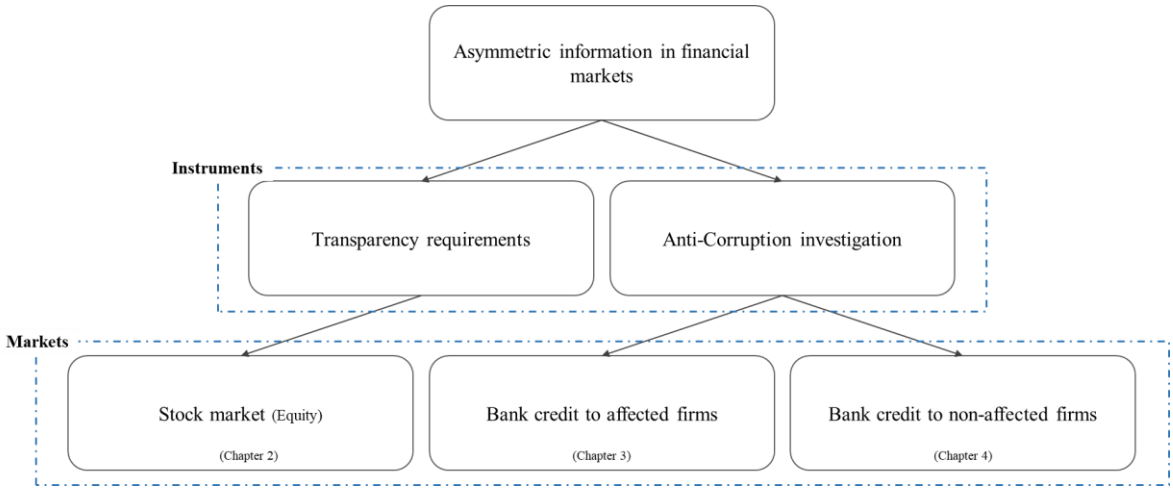
The potential impact of such transparency and anti-corruption initiatives on financial markets is thus quite relevant, and a closer examination is needed to assess these positive effects

1 According to Di Ciommo (2015), who investigates transparency in Brazil, the country has an extensive volume of data and information production provided to the public. Information comprises national public budget, social policies beneficiaries, public contracts and expenditures, demographic information, institutional charts and responsibilities among others. Among the main sources, there are official transparency portals, national census, statistics on congress composition by gender and ethnicity, public servant salaries, data on political candidates' wealth and income, the Brazilian Institute of Geography and Statistics (IBGE), data like the National Research on Households (PNAD), among others.

2 Beyond the extinguishment of the open outcry sessions in 2005, giving place only to the electronic trading system, the Brazilian stock exchange has passed through some mergers in the last decades. One example was the unification of the two stock exchanges in 2008, BM&F and Bovespa, in just BM&FBovespa. Some years later, BM&FBovespa and Cetip (Central of Custody and Financial Settlement of Securities) announced the merger between both entities in 2017, leading to the creation of B3, approved by the Administrative Council for Economic Defense (Cade) and the Brazilian Securities and Exchange Commission (CVM).

(if any) as well as their economic magnitude. Since transparency has been advocated as a solution to corruption (Rose-Ackerman, 2004), the conceptual framework which I propose in this thesis (Figure 1.1) is that both transparency and corruption detection are important mechanisms able to reduce information asymmetries in financial markets (Akerlof, 1978; Stiglitz, 1992; Miller, 2005), consequently impacting enterprises, banks, investors, individuals, as well as their business practices. However, two important assumptions must be made when considering the stock market and credit market herein discussed: the information provided must have a reasonable chance of reaching and being absorbed by the public, and its release and dissemination must provide some sanction mechanisms to curb the behavior of potentially corrupt officials (Lindstedt and Naurin, 2004).

Figure 1.1: Conceptual framework of the thesis



Notes. Transparency requirements of the listing segments and anti-corruption investigations are the main advocated drivers able to reduce asymmetry of information in this conceptual framework, having different consequences depending on the market under investigation. In chapter 1 transparency refers to the disclosure of additional information to the stock market participants according to the segment level of corporate governance, while in chapters 2 and 3 it refers to a widespread corruption detection that affected many firms in the market, probably leading to a credit reallocation in the financial system.

In this sense, the second chapter aims at formalizing the importance of transparency in the Brazilian stock market and to show how investors respond to additional degrees of information commitment, both in the short and long run. The contribution to the literature (Shleifer and Vishny 1997; Becht, Bolton and Röell 2003; Bushman and Smith 2003; Gompers, Ishii and Metrick 2003; Jandik and Rennie 2005; Black, De Carvalho, Gorga 2010) is therefore twofold: first, I analyze long-run stock market performance following firm segment switches. The evidence on short-term effects of market reform is positive, but it is not clear whether improved transparency in Brazil pays off for firms in the long run. Second, to the best of my knowledge, this paper is the first to examine the stock market response to *different levels* of corporate governance and to investigate the incremental value of transparency in the stock market.

For this chapter, we considered Brazilian firms that switched from the traditional segment to the Nível 1, Nível 2 or Novo Mercado since 2000. Results document that higher standards of governance result in significantly higher abnormal stock returns in the long run, controlling for firm and time fixed effects. The positive impact has increased after the Global Financial Crisis, market microstructure improved, and the market impact is stronger for financially healthy firms. The evidence suggests that committing to higher standards of corporate governance paid off for Brazilian firms in the long run.

Analyzing the governance structure is important because financial transparency and information disclosure are critical elements of distinguished corporate governance practices. Beyond the academic contribution, the topic has gained relevance as the countries have committed to the Sarbanes-Oxley (SOX) Act in July 2002, and the Dodd- Frank Act stablished in 2010, both aiming to increase transparency in the financial system. In Brazil, more specifically, the country of which the sample was withdrawn, 85% of the companies that made their IPOs chose to be listed at the Novo Mercado (New Market) since the creation of special

segments of corporate governance, the segment with the highest level of transparency, and more than half of the companies listed in the Brazilian stock exchange already switched to this segment.

The third and fourth chapters turn the attention to a micro level analysis (bank-firm relationships), evaluating how banks react to anticorruption investigations in two ways: directly vis-à-vis affected firms or indirectly vis-à-vis non-affected firms. In other words, chapter 3 investigates whether and how the disclosure of substantial anti-corruption investigations concerning a set of major firms in the Brazilian economy affected their bank credit and the quality of their loans. We find significant adverse effects on the credit flow volumes, leading to a considerable credit contraction and loan term deterioration for construction firms involved in corruption schemes. More specifically, affected firms receive less credit, lower credit ratings and display higher loan loss provisions after the investigations have started. This effect is less pronounced for firms which are more connected to the government and have more skilled workers, while it is more pronounced for firms with more bank relationships in the pre period. Our main results are robust to different control groups, 2 alternative matching algorithms and a synthetic control approach.

Chapter 4, on the other hand, addresses potential spillover effects on other firms and inherent reallocation effects to comparable firms not involved in any anti-corruption investigation. It also derives some implications for the level of economic activity and allocative efficiency. Our results indicate that non-affected firms receive less credit from affected banks, i.e., those who exhibit a relatively high ex ante credit exposure to affected firms. This decrease in credit is smaller for existing borrowers who have multiple bank relationships and come from economically stronger states. These firms are also more likely to obtain longer maturity loans and experience credit rating upgrades. Nonetheless, new borrowers who are politically connected and from the construction sector are more likely to obtain credit, as well as those

who are younger and from economically weaker states. Third, non-affected firms (existing borrowers) with a higher exposure to affected banks reduce employment and wages in the post period and this effect intensifies over time. Our results are robust to different selection criteria for the treated group and to different event windows surrounding the event.

Analyzing the credit reallocation is important because, although the enormous costs incurred in corruption, none of the previous studies herein presented have analyzed whether negative shocks related to corruption influence the way of doing business within a country, potentially affecting the supply of credit through credit market networks. Therefore, this paper fits into a new and growing literature that examines the detrimental impact of corrupt environments and bank lending activity (Beck, Demirgüç-Kunt, Levine, 2006; Charumilind, Kali, Wiwattanakantang, 2006; Barth, Lin, Lin, Song, 2009; Weill, 2011; Park, 2012) and how it is reallocated from corrupt firms towards potentially not corrupt comparable firms (Ding et. al, 2018; Chen and Kung, 2019; Griffin, Liu, Shu, 2018).

Overall, given the broader literature on asymmetric information, the two important mechanisms claimed to reduce it, and the analyses of two important markets devoted in this thesis (stock market, bank credit), I judge two main contributions of this conceptual thesis. The first is a comprehensive analysis of the way (and the consequences) of doing business in Brazil, a country where the law enforcement is weak and courts are slow, inefficient, and congested (Ponticelli and Alencar, 2016). It is important, then, to see how the capital market reacts and how credit networks change when disclosure is enhanced, corruption and crime are inhibited, thus lowering the need for a functioning legal system. The second relates to the novelty the databases of chapters 3 and 4 bring to the academic discussion: because of the rich (matched) data, I will be able to trace the propagation of the shock (anti-corruption investigations) both directly to affected firms and indirectly to non-affected ones, making it possible to identify causal relationships that might be useful for policy debates.

Thus, beyond the contribution to the existing literature, the expectations of this work are substantial. The main goal, however, is undoubtedly to contribute to the discussion on additional informational disclosure at the public and private sectors, identifying means that conciliate incentives between agents and principals, diminish the application of public resources eligible to corrupt practices, and measure its implications for the financial market. In this sense, the three chapters are part of my final thesis, and they are written in the same order previously described in this brief Introduction.

2. DOES GOOD CORPORATE GOVERNANCE PAY OFF IN THE LONG RUN? EVIDENCE FROM STOCK MARKET SEGMENT SWITCHES IN BRAZIL[3]

3 This chapter is based on a published paper by the authors L. Moura (FGV-EBAPE), and L. Norden (FGV-EBAPE) in Brazilian Review of Finance, Vol. 17 No. 3 (2019): July-September, 1-15.

2.1 Introduction

Corporate governance and disclosure of information matter for financial growth and development, helping to reduce agency problems and asymmetric information in financial markets, improve the legal environment, and promote market discipline. The marginal value of good corporate governance is expected to be particularly high for emerging economies where economic and legal institutions are opaque, less developed or plagued by enforcement problems.

In this paper, we investigate the long-run effects of higher standards of corporate governance in the Brazilian stock market, which has expanded strongly in the last two decades. Brazil is on global rank eight in terms of GDP, has the largest stock market in Latin America and accounts for a significant part of the capital invested in the continent. Several changes occurred in the regulatory framework seeking to make the market more accessible to foreign and retail investors. One of the biggest innovations was the introduction of the new listing segments Nível 1, Nível 2 and the Novo Mercado in December 2000 next to the traditional market. In the new segments, listed firms have to adopt higher corporate governance standards than those required by law, as well as the rules of the Sarbanes-Oxley Act (SOX) since July 2002. 46% of the firms listed at the Brazilian stock exchange (and 81% of the IPOs) switched to this segment since its creation.[4] A listing at the Novo Mercado requires the exclusive issuance of common shares (one share, one vote), an annual report of the statutory audit committee, quarterly minutes of the board meetings, and various further strict requirements.

Our focus is on the long-run stock market performance after firms' segment switches. The literature has documented positive short-run effects of the market reform (e.g. Carvalho

4 According to BM&FBOVESPA, among the 142 companies that made their IPOs in a distinct level of corporate governance until January 2017, 115 adhered to Novo Mercado, 20 to Nível 2 and 7 to Nível 1 (since 2004). Likewise, among the 68 that switched to an upper level until the same deadline, 31 migrated to Novo Mercado, 11 to Nível 2 and 26 from the Traditional Market to Nível 1, since the establishment of the distinct segments.

and Pennacchi, 2012), but there is virtually no evidence whether committing to higher standards of corporate governance in Brazil has paid off in the long-run. There are several reasons why the long-run effects might differ from the short-term performance. First, the improved corporate governance and information disclosure due to the market reform represents a promise to investors, but it is unclear to what extent it has been honored by the firms or enforced by investors when it became under pressure.[5] Second, earlier reforms such as the Brazilian Corporate Law reform (*Lei das Sociedades por Ações*, approved by House of Representatives in 2001) that aimed at shaping the legal environment according to the needs of modern capital markets turned out to be insufficient (Carvalho, 2002). Third, firms that switch to the Novo Mercado must cope with non-trivial additional costs[6], making it harder to meet investors' return expectations. Fourth, international experience points at bubbles and bursts in the Novo Mercado's European counterparts, for example the German Neuer Markt, accompanied by insider trading, accounting scandals and corporate fraud. Hence, positive long-run effects of higher standards of corporate governance are by no means guaranteed.[7]

We also investigate the rationale behind switching to the three new segments. If the firms that switch to the Novo Mercado outperform the ones switching to Nível 1 and 2 in the

5 It is well known that the enforcement of shareholder rights and creditor rights in Brazil is relatively weak compared to other countries because of the legal environment, corruption in the private and public sector and congested courts (e.g., La Porta et. 1998, Ponticelli and Alencar ,2016).

6 Differently from the other segments, the firms that switch to the Novo Mercado must set up an audit committee or statutory audit committee, an auditing department, and a compliance, internal controls and corporate risks department. Other examples of direct costs enclose the accounting standards required by the Novo Mercado, such as quarterly consolidated balance sheets, cash statements, and monthly stock positions of the majority and controlling shareholders. Indirect costs include the possibility of greater vigilance on the part of tax authorities, diminishing the possibilities of tax evasion. In addition, since there is greater protection to minority shareholders in the Novo Mercado, it reduces the moral hazard to extract private benefits (other than dividends) from companies, reducing the control value of the company.

7 From 2000 onwards, the fall in the stock prices of the companies in the German Neuer Markt reached more than 80% and many stocks traded at a price below 1 Euro. The same occurred in many other European countries. Consequently, the majority of remaining firms delisted, resulting in a significant reputational damage that eventually led to the failure of most of the New Markets in Europe.

long-run, we can conclude, everything else being equal, that committing to higher standards of corporate governance pays off. However, if their long-run performance is lower than the one of others firms, the positive abnormal returns around the segment switch announcement documented in earlier studies might have been an irrational overreaction and possibly due to an index effect (Harris and Gurel 1986; Chan and Howard 2002; Elliot et al. 2006; Kappou, Brooks and Ward 2009; Kasch and Sarkar 2011)[8] or due to price pressure created by speculative traders around the event.

Next to the impact on returns, listing segment switches might affect the market microstructure of the stocks, leading to higher market quality. The higher standards of the Novo Mercado might help to increase the number of trades and the trading volume after the segment switch. Similarly, the liquidity of stocks might have increased because informational asymmetries have been reduced. Furthermore, considering that high levels of liquidity may reflect improved market expectations, firms' beta coefficients (the sensitivity to the market factor or contribution to systematic risk) are expected to decrease after the segment switch.

Finally, firms' financial health might influence the magnitude of the stock market reaction (e.g., Bebchuk, Cohen and Ferrell, 2009). We expect greater effects of corporate governance the stronger a firm's financial health. The rationale behind is that strong firms are likely to gain when asymmetric information is reduced because they are no longer pooled with weak firms. We measure financial health using Altman's Z-score (Altman, 1968), adapted for the use of financial data made available by Bloomberg. It is a widely employed measure of the corporate financial health and directly related to firms' probability of default.

8 Since the companies listed in the special segments of corporate governance are also included in the IGC-NM (Index of Shares with Differentiated Corporate Governance) as they commit to such levels of corporate governance, we can support our assumptions relying on the literature on index effects.

Based on data from all publicly listed Brazilian firms that switch to a new listing segment during the period 2001-2016, we obtain the following results. We find that firms that switch to the Novo Mercado exhibit significantly higher cumulative abnormal returns than those that switch to the other levels. This result holds for a horizon of up to 1,000 trading days following the segment switch announcement. Furthermore, we find that this effect becomes stronger after the Global Financial Crisis. Measures of market microstructure for firms that switch to the Novo Mercado improve significantly in the long run, especially after the Global Financial Crisis. Finally, the stock market reaction to segment switches is stronger for more financially healthy firms. Our results are robust to alternative event windows, different post-announcement horizons, different models for abnormal returns, alternative sets of control variables, industry fixed effects, year fixed effects, different measures of financial health, and ADR issuance (American Depositary Receipts).

Our study contributes to the literature in two ways. First, we investigate the long-run effects of higher standards of corporate governance in the stock market. Despite the general literature on valuation effects of corporate governance (e.g., Shleifer and Vishny 1997; Becht, Bolton and Röell 2003; Bushman and Smith 2003; Gompers, Ishii and Metrick 2003; Jandik and Rennie 2005; Black, Carvalho, Gorga 2010), there is little evidence on long-run stock market impact of committing to different levels. Some studies analyze the short-term announcement effects after the introduction of the new listing segments in Brazil (e.g., Carvalho, 2002; Aguiar, Corrar and Batistella 2004; Rogers 2006; Procianoy and Verdi 2009; Carvalho and Pennacchi 2012). These studies document significantly positive short-term effects for firms that switch to segments that require higher corporate governance. However, to the best of our knowledge, none of these earlier studies has examined the long-run effects and, and importantly, their consistency with the short-term effects. Second, we investigate the market reaction for each listing segment separately, seeking to understand their incremental values. We

show that firms that switch to the Novo Mercado - but not those that switch to the intermediate segments Nível 1 and 2 - experience significantly positive effects in the long-run. Hence, firms only benefit if they make the full step towards the highest standard of corporate governance and information disclosure.

The remainder of this paper is organized as follows. In Section 2, we describe the institutional background and develop our hypotheses. In Section 3, we describe the data and method. In Section 4, we present the empirical results. In Section 5, we show evidence from further checks and robustness tests. Section 6 concludes.

2.2. Institutional background and hypotheses

2.2.1. Institutional background

In the last three decades, the business environment and the corporate sector in Brazil have undergone major changes that were accompanied by an intense debate of corporate governance and information disclosure. The entry of new investors in the Brazilian market (national and international institutions), the influence of global mergers and acquisitions, the pressure to reduce the cost of capital, and the search for less concentrated corporate control, resulted in a transition from the model of oligopolistic family-owned businesses to a new model with greater participation of institutional investors (Bridger, 2006). In this context, McKinsey and Co. and Standard and Poor's (2001) analyze the corporate governance in Brazil and describe its key features. They show that there was a high concentration of ownership in the largest shareholders, low acknowledgment of minority shareholders' interests, overlap between ownership and board of directors in favor of the majority shareholder's interests, lack of a formal structure of the counsel and external advisors, and insufficient level of transparency for investors. Leal, Silva, and Valadares (2002) examine the control structure of 225 Brazilian firms

and show that the majority of firms are controlled by up to three shareholders. Furthermore, they show that controlling shareholders hold more than the legal minimum investment for publicly traded firms, implying that there exist private control benefits (Carvalho, 2002). Silva (2005) analyzes the control structure of publicly listed firms and shows that for 90% of the firms a share of more than 50% of the voting capital is held by a single shareholder.

Given the political and institutional challenges to promote legal changes that would protect more effectively the minority shareholders and prompted by new listing segments in Europe in the second half of the 1990s[9], voluntary mechanisms to adopt good corporate governance have been developed. Since the Brazilian reform in 2000, there are four listing segments, with the Novo Mercado being the one that requires companies to provide full information disclosure and shareholder protection. Considering that the standards of the Novo Mercado are the most demanding and that many firms would not be able to meet them, the other segments Nível 1, Nível 2 and the Traditional Market were created. The Traditional Market remains the segment for those companies that are not willing to adopt the higher standards.

The rules for switching to Nível 1 are mainly: a minimum free float of 25% of the capital, share dispersion efforts in the public offerings, and a minimum of 3 members in the composition of the Board of Directors (pursuant to Brazilian corporations Law), with unified term of up to 2 years. Firms that switch to Nível 2, beyond the requirements for Nível 1, should provide an annual balance sheet translated into English[10], make a compulsory public tender offer to all outstanding shares at fair value in the event of closing of capital and cancellation of

9 These are: Alternative Investment Market in the United Kingdom, 1995; Nouveau Marché in France, 1996; Neuer Markt in Germany, 1997; Nieuwe Markt in the Netherlands, 1997; and the Nuovo Mercato in Italy, 1999. The only survivor is the Alternative Investment Market (AIM) of the London Stock Exchange. Its success is partly due to its focus, which was (unlike the other new markets) not limited to the technology, internet, and biotechnology sectors. Aside from including a large number of mining companies and alternative sources of energy, it provides significant tax incentives to individual investors and has very flexible listing rules.

10 In accordance with the U.S. GAAP (Generally Accepted Accounting Principles) or with the IASC (International Accounting Standards Committee).

stock trading at that level, and become a member of the Market Arbitration Chamber for the resolution of corporate conflicts. The Novo Mercado, the segment with the highest level of corporate governance and disclosure, only allows the issuance of common shares (one share, one vote), needs internal regulations for the Board of the Directors, the Advisory Committees and the fiscal Council (if there is one) Code of Conduct, and a mandatory setting up of compliance, internal controls and corporate risks department (see Appendix 2.1 for details).

Because of its strict requirements, significant institutional efforts, and complementary legislation for the companies, the implementation of the Novo Mercado has been seen as a success (Matos, 2017), despite a mixed environment of unbalanced corporate governance practices in Brazil (see Black, De Carvalho, Gorga 2010 for a detailed review of corporate governance in Brazil). On the one hand, from the firm's perspective, transaction costs decreased, and the pricing of shares improved. This view relies on the presumption that investors are willing to reward firms that adopt higher levels of corporate governance and disclosure (Aguiar, Corrar and Batistella 2004). On the other hand, investors face lower informational asymmetries and can acquire "a differentiated product", a label used by traders for these stocks.

Similar to their European counterparts, Brazilian firms listed in the new segments Nível 1, Nível 2 and Novo Mercado have to adopt higher standards of corporate governance than in the traditional segment, expanding corporate rights of minority shareholders and increasing the disclosure of information. One important difference between the Brazilian Novo Mercado and its international counterparts is that the former does not focus on specific industries such as information technology, biotechnology, or other growth firms.[11] The new market segments in

11 Another difference is that in Europe firms were not allowed to switch from other listing segments to the new market. In Brazil, firms are allowed to switch to listing segments with higher levels of corporate governance whenever they comply with the rules of that specific listing segment.

Europe in the 1990s targeted firms that were subject to financial constraints and that needed alternative sources of finance to fund their growth. Besides that, the main assumption is that the adoption of higher standards of good corporate governance grants greater credibility to the stock market, consequently, enhancing confidence and the willingness to invest (Aguiar, Corrar and Batistella 2004; Procianoy and Verdi 2009).

2.2.2. Hypotheses

We investigate whether segment switches to higher levels of corporate governance result in a positive long-run stock market reaction. The literature has shown significant positive short-term effects (e.g., Carvalho and Pennacchi, 2012), but no study has investigated the long-run effects after the segment switches. As discussed in the introduction, there is no evidence on the long-run effects and, if there any, it is not clear whether they are consistent with the market expectations implied by the short-term effects. We therefore examine firms' stock market performance over different time horizons following the segment switch announcement. Considering the short-term positive market reaction, we expect a positive long-run reaction, assuming that the same economic reasons apply in the long-run. We hypothesize:

> *Hypothesis 2.1 (Long-run effects).* The cumulative post announcement stock returns of firms that switch to the Novo Mercado are higher than those of the firms that switch to Nível 1 or Nível 2.

We further expect that the long-run effects become more pronounced when corporate governance becomes more valuable, which can be expected in times of increased uncertainty in the market. Research has documented a significant increase in uncertainty and volatility in stock markets during and after the Global Financial Crisis. We therefore hypothesize:

Hypothesis 2.2 (Global Financial Crisis). The long-run effect of the Novo Mercado will be more pronounced after the increase in uncertainty in financial markets due to the Global Financial Crisis.

Moreover, we expect an impact on the market microstructure of the stocks that switch to higher listing segments. Because of the higher corporate governance and disclosure standards, asymmetric information and therefore the difference in the willingness to buy and sell stocks should decrease and other measures of market microstructure should improve. Based on this reasoning, we hypothesize:

Hypothesis 2.3 (Market microstructure). The market microstructure of stocks that switch to the Novo Mercado significantly improves in the long run. The volume of outstanding shares (H3a), the trading volume (H3b), the number of shares traded (H3c) and the market liquidity (H3d) in the Novo Mercado will be higher for these firms than for other firms.

Fourth, we examine whether financial health influences the impact of different corporate governance levels on firms' stock market performance. It is possible that firms prefer higher disclosure of information when they are financially healthy because it would convey good news to the capital market and, consequently, the discounted cash flow of those firms would benefit from this positive information. Under the same reasoning, financially unhealthy firms would prefer to hide information from the public so that lower levels of corporate governance make them better off. Hence, the effect of corporate governance and information disclosure on stock market performance might vary with firms' level of financial health.

Hypothesis 2.4 (Financial health). The impact of corporate governance on stock returns is stronger the more financially healthy a firm.

2.3. Data and method

Our sample comprises daily stock prices and quarterly financial statements of all Brazilian firms that switched to one of the three new listing segments Nível 1, Nível 2 or Novo Mercado during the period from June 2001 to September 2016. We do not consider firms that went public for the first time (IPOs) and also not holding companies that switched at the same time as one of its subsidiaries to avoid double counting.[12] We collect the names of the firms that switched to the new segments directly from the BM&FBOVESPA. The raw sample contains 68 companies. We had to omit 11 firms because there was ambiguity about the announcement date of the segment switch. The final sample contains all 57 companies that switched to a distinct listing segment (see Appendix 2.2).

We define the event day as the date when the announcement of a contract between a firm and the stock exchange to switch to a distinct level was made public for the first time. We searched the leading Brazilian newspapers for the announcements of segment switches between June 2001 and September 2016 and also searched other information sources such as company websites and the BM&FBOVESPA website. We consider the announcement in one of the major online newspapers ("*Valor Econômico*" and "*Infomoney*") and in the "*Boletim Diário do Mercado*" as the main channel of communication between the BM&FBOVESPA and the stock market.[13] The announcement day can be either before the actual segment switch ("Announcement Date"), held by the official registration's date with BM&FBOVESPA, or on the day of the segment switch ("Adhesion Date"). We conducted an electronic search of newspapers, applying the following key words: "company's name", "Corporate Governance

12 We do not exclude financial firms from the sample. Note that we control either for firm and time fixed effects or industry and time fixed effects in all analyses.

13 The CVM (*Comissão de Valores Mobiliários*) is another source of information about listing segments. However, we could not consider this source because the information is not publicly available.

Index", "*IGC*", "*Nível 1*", "*Nível 2*", or "*Novo Mercado*". Appendix 2.3 provides a description of all variables and Table 2.1 reports summary statistics.

Table 2.1: Summary statistics

This table reports summary statistics of the main variables. The whole sample comprises 57 firms that announced the migration to a distinct listing segment of corporate governance. There are 26 switches to the Novo Mercado. We aggregate the switches to Nível 1 and Nível 2 because of the small number of observations (22 firms switch to Nível 1 and 9 firms to Nível 2). The variables Z-score and Size are winsorized at the 5^{th}-95^{th} percentiles.

	All switches					Switches to Nível 1 and Nível 2					Switches to the Novo Mercado				
	Mean	Std. Dev.	Min	Median	Max	Mean	Std. Dev.	Min	Median	Max	Mean	Std. Dev.	Min	Median	Max
Firm size	9,309.33	12,130.00	283.69	4,796.85	30,439.79	12,008.78	14,101.16	721.08	8,741.50	30,439.79	5,530.09	7,586.04	283.69	2,284.13	11,071.53
Firm age	3,933.52	1,596.71	264.00	4,102.50	6826.00	4,122.57	1,660.01	264	4,083	6,826	3,668.86	1,519.35	684	4,192	6,321
Investments/Equity	0.7470	0.6299	0.0090	0.6921	2.1461	0.8518	0.7578	0.0180	0.6765	2.1461	0.6002	0.3635	0.0090	0.7744	1.0319
Z-score	4.85	2.05	1.01	3.40	6.58	1.88	0.81	1.01	5.27	6.81	5.32	2.45	1.01	2.73	7.58

The firms switching to the new listing segments display a relatively good performance in the period before the announcement. Their financial health, as reflected by a mean (median) *Z-score* of 4.85 (3.40), is significantly larger than the cutoff value of 2.6 for the safe zone.[14] The companies that switch to Nível 1 or 2 tend to be financially healthier than the ones that switch to the Novo Mercado.[15] The firms that switch to the Novo Mercado are smaller (mean = 5,530.09), younger (mean = 10.04) and invest less (mean = 0.60) than the ones that switch to the other levels (12,008.78; 11.26; 0.85; respectively). These differences are likely due to the challenges that larger and older firms face to meet the requirements of the Novo Mercado. We control for these differences by including either firm and time fixed effects or industry and time fixed effects in our regression models.

2.4. Results

2.4.1. Long-run effects by segment switches

The initial sample comprises all 57 companies that switch to a distinct level of corporate governance. For these segment switches, we compute the firm-specific stock returns during the whole event window and start comparing the long-run performance by segment[16] to test our Hypothesis 2.1. We compare the cumulative abnormal stock returns before and after segment switch among the distinct levels over different time horizons. Figure 2.1 plots the abnormal cumulative stock returns before and after the announcement of the segment switch.

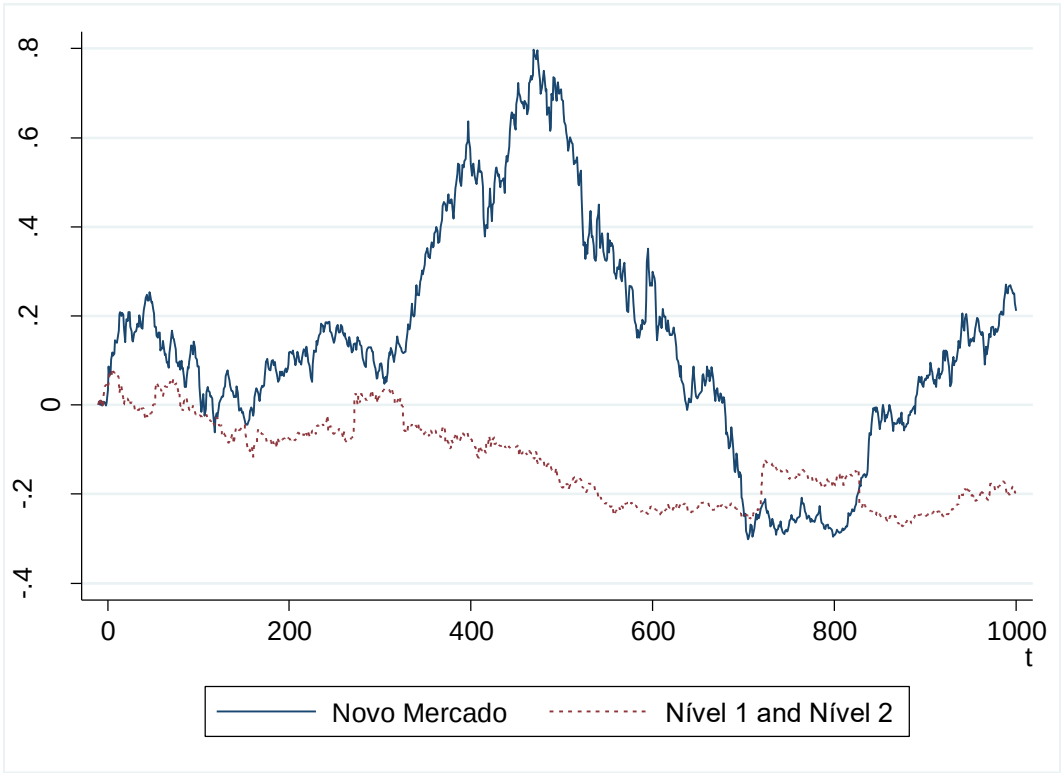
14 According to the Altman Z-score (Altman, 1968), firms classified with a score below 1.1 (the distress zone) are considered as bankrupt. The gray area (indifference zone) has its lower and upper boundaries at the 1.1 and 2.6, respectively. Finally, in the non-bankrupt area (safe zone) are those companies with a score greater than 2.6.

15 Due to the presence of potential outliers on financial statements, we winsorize all quarterly accounting data at the 5th/95th-percentile to compute the Z-score.

16 Due to the small number of firms that switch to Nível 2, we collapsed the first two levels to one category with 28 firms.

Figure 2.1: Cumulative abnormal returns by segment

This figure shows the cumulative abnormal returns CARs [-10, t] before and after the announcement of segment switches for the firms in the distinct listing segments.



There is an increase in the cumulative abnormal return for the firms that commit to higher levels of governance around the event date[17] that persists approximately until 500 trading days after the segment switch. The increase is sharp and strong for switches to the Novo Mercado, while there is a moderate decrease for firms that switch to Nível 1 and 2. On the aggregate level, nonetheless, there is a decrease on the cumulative stock abnormal returns for

17 As depicted in Figure 2.1, there is an upward movement right before the announcement takes place. This is completely reasonable, once there might be rumors in the stock market (Pound and Zeckhauser, 1990) that affect the prices on the event window of 10 trading days before and after segment switch, already accounted in the first analysis. Moreover, we consulted a limited number of sources of information (Valor Econômico, Infomoney, and Boletim Diário do Mercado), so, it might be the case that for some migrations the announcement is made beforehand in another website.

the Novo Mercado until trading day 800, increasing and surpassing the other levels on our long run time frame.[18]

Table 2.2 reports the corresponding multivariate regression results for different post-announcement time horizons (10, 30, 90, 250, 500, 750, and 1,000 trading days). We add firm fixed effects and time fixed effects to the model.

The cumulative abnormal returns for the firms that switch to the Novo Mercado are significantly higher than those of the other firms, as shown by the positive and significant coefficients of this dummy. Moreover, the cumulative abnormal returns increase over long time horizons in all specifications. The coefficient of the interaction term is negative and statistically significant in some specifications (Models 3-5). However, firms that switch to the Novo Mercado still exhibit higher abnormal returns than those firms that switch to the other levels (0.183 + 0.323 – 0.048 = 0.457) during the 500 trading days after the segment switches (column 5). Although the effect slightly decreases for longer horizons, column (7) shows that the cumulative abnormal return of the Novo Mercado is still significantly greater than the other levels up to four years after the segment switch.

18 Since results are aggregated over the event window from t_{-10} until t_{+1000}, no further conclusions can be made about the decrease in cumulative returns from t_{+500} until t_{+800}. While t_{+500} considers the event date (t_0) plus 500 trading days for a firm that switches to the Novo Mercado in 2002, for example, it also considers the event date (t_0) for a different firm that switches in 2011.

Table 2.2: Cumulative abnormal returns before and after the segment switch

This table presents regression results for switches to the Novo Mercado on the cumulative abnormal return CAR [-10, t] with t= 10, 30, 90, 250, 500, 750, or 1,000 trading days after the segment switch. *Novo Mercado* and *Post* are dummy variables that capture the market segment of the security and the period post-announcement, respectively (the baseline category are the firms that switch to Nível 1 and Nível2 and the period before the segment switch). Robust standard errors clustered at the firm level are reported in parentheses. *, **, and *** indicate the coefficients that are significantly different from zero at the 10%, 5%, and 1% levels, respectively.

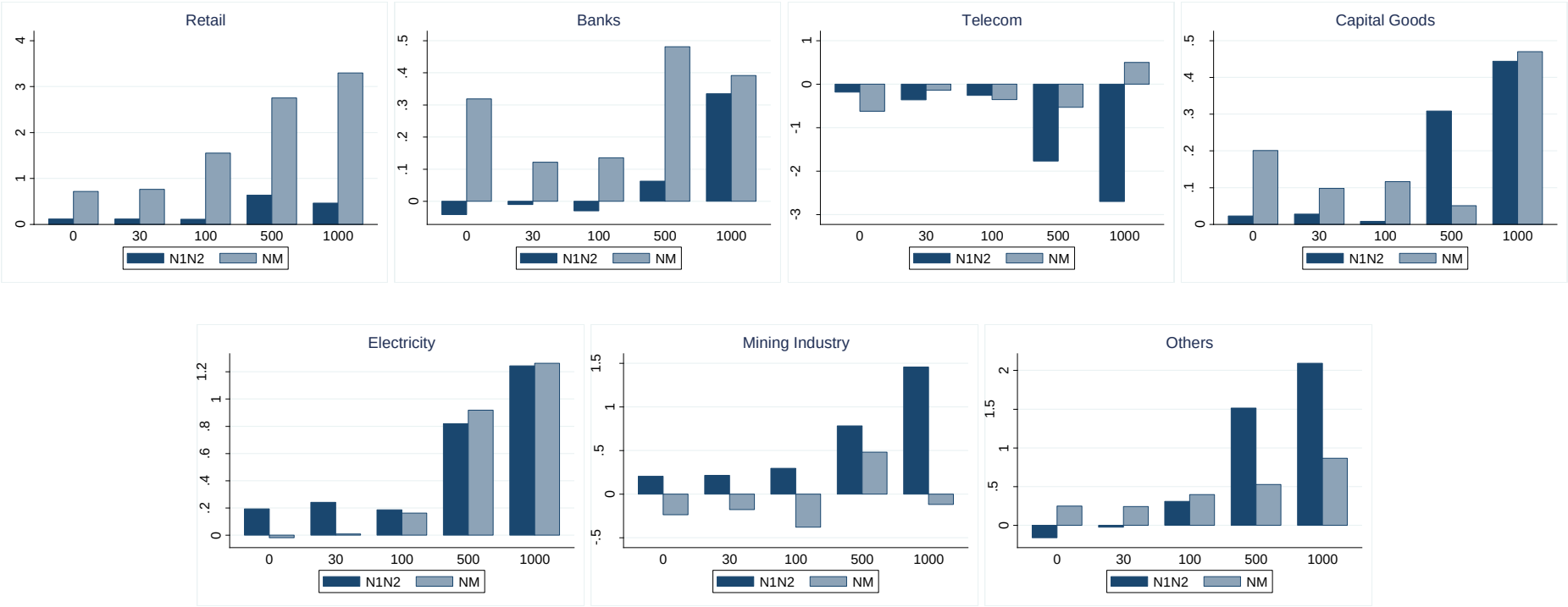
Dep. var.: CAR [-10, t]	(1) 10 trading days	(2) 30 trading days	(3) 90 trading days	(4) 250 trading days	(5) 500 trading days	(6) 750 trading days	(7) 1,000 trading days
Novo Mercado	0.199***	0.198***	0.193***	0.188***	0.183***	0.180***	0.181***
	(0.00957)	(0.0103)	(0.0135)	(0.0161)	(0.0181)	(0.0205)	(0.0236)
Post	0.0568***	0.0542***	0.113***	0.213***	0.323***	0.389***	0.450***
	(0.0149)	(0.0117)	(0.0125)	(0.0138)	(0.0151)	(0.0169)	(0.0194)
*Novo Mercado * Post*	-0.00495	-0.00131	-0.0401***	-0.0527***	-0.0483***	-0.0106	-0.00372
	(0.0194)	(0.0146)	(0.0150)	(0.0160)	(0.0173)	(0.0193)	(0.0221)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,650	6,790	10,210	19,330	33,580	47,789	61,220
Number of firms	57	57	57	57	57	57	57
R^2	0.103	0.103	0.090	0.093	0.093	0.080	0.064

We also analyze the cumulative returns of firms by segment and time horizon. In this additional check, we examine whether the results are driven by certain sectors. As shown in Figure 2.2, in almost all the specifications, the cumulative abnormal returns for the firms that switch to the Novo Mercado are larger than those that switch to the other levels, independent of the time horizon.

We note that some industries display special effects after segment switches. The telecom industry, which is heavily regulated, exhibits predominantly negative long-run performance, but the performance is less negative for firms that switch to the Novo Mercado. Furthermore, the mining industry is sensitive to the small sample size: among the six firms in this industry, only one switched to the Novo Mercado. In the following multivariate analysis, we add firm fixed effects to all regression models, controlling for any observed and unobserved time-invariant heterogeneity (including industry effects) across firms.

Figure 2.2: Cumulative abnormal returns by industry, segment and time horizon

This figure displays the cumulative abnormal returns CAR[-10, t] by industry, segment and for t = 0, 30, 100, 500 and 1,000 trading days. N1N2 refer to firms that switch to Nível 1 or Nível 2, and NM refers to firms that switch to the Novo Mercado.



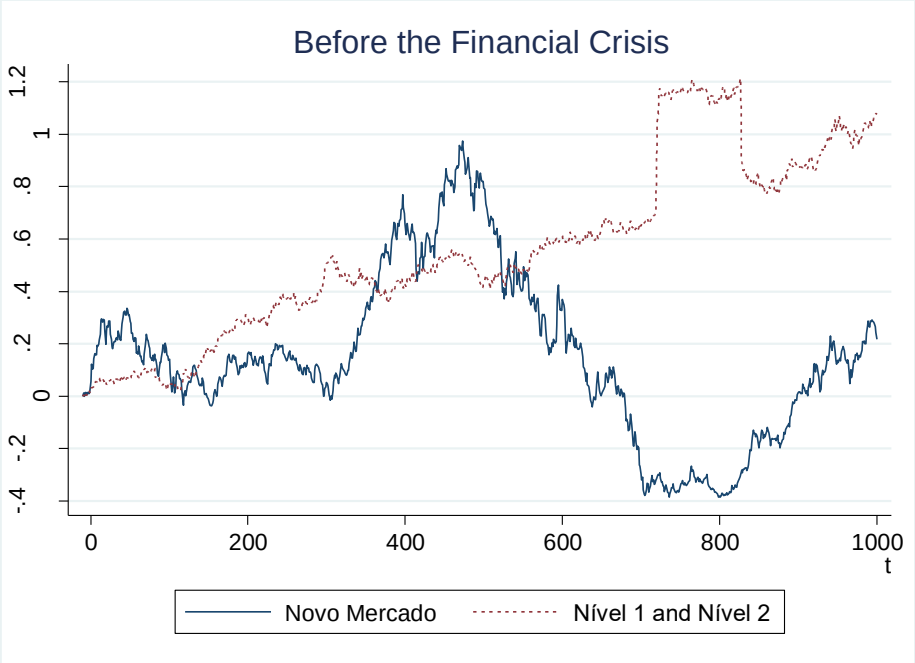
2.4.2. Long-run effects and the Global Financial Crisis

Considering that the positive effect of switching to the Novo Mercado persists over longer horizons, we now test our Hypothesis 2.2 to analyze whether periods of high uncertainty in the financial market, such as the Global Financial Crisis, change the way how investors perceive the additional disclosure efforts by listed firms. Specifically, we compare the cumulative abnormal stock returns over different time horizons for the firms that switched before the third quarter of 2008 (which we consider the start of the Global Financial Crisis) with those that switched afterwards[19]. Figure 2.3 displays the cumulative abnormal stock returns of segment switches in event time that occurred before and during/after the Global Financial Crisis.

19 As shown in Appendix 2.2, only one firm announced the segment switch to an upper level in the around the Financial Crisis. However, considering that this announcement occurred on December 8, we add this segment switch to the category After Crisis.

Figure 2.3: Cumulative abnormal returns and the Global Financial Crisis

This figure shows the cumulative abnormal returns CAR[-10, t] for the segment switches that take place before and after the Global Financial Crisis. Before the financial crisis, 17 firms switch to the Nível 1 and Nível 2, while 16 switch to the Novo Mercado. After the financial crisis, 14 firms switch to the Nível 1 and Nível 2 and 10 to the Novo Mercado.



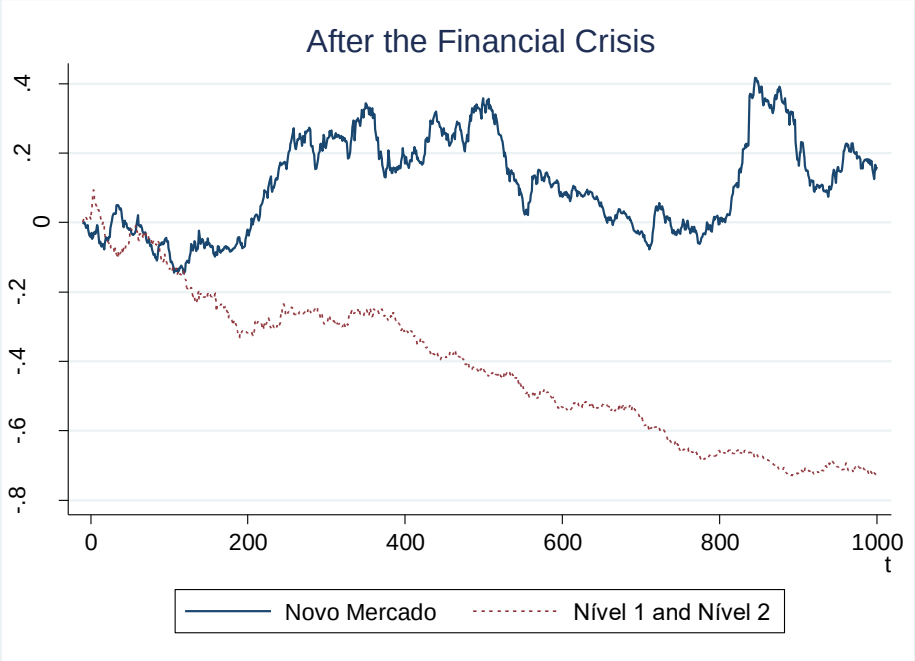


Figure 2.3 shows a clear difference in the performance of firms that switch to Nível 1 and Nível 2 when we compare pre and post crisis times. Cumulative abnormal returns of firms that switched before the crisis increased, while they decreased for those firms that switched after the crisis. This finding is a plausible consequence of the Global Financial Crisis, increasing the uncertainty in financial markets and implying an increased demand for higher standards of corporate governance and information disclosure.

Table 2.3 reports the corresponding multivariate regression results for the triple interaction effect between the firms that switch to the Novo Mercado (*Novo Mercado*), the period after the segment switch (*Post*) and the post crisis dummy (*After Crisis*) for different post-announcement time horizons (10, 30, 90, 250, 500, 750, and 1,000 trading days). We add firm fixed effects and time fixed effects to the model.

Similar to the previous analysis, the significant coefficients of the dummy variables *Post* and *Novo Mercado* indicate that the cumulative abnormal returns increase after the segment switches and are significantly greater for these firms. The coefficient of the triple interaction term is consistent with our Hypothesis 2.1 on the long-run performance. It is positive and statistically significant for longer time horizons (Models 4-7). This finding confirms that the Novo Mercado firms have a positive cumulative abnormal return of around 57.3% higher than the baseline category following the segment switch after periods of high uncertainty in financial markets. The aggregate effect on the cumulative abnormal return on the long run (Model 7) for firms in this category (those firms that switched to the Novo Mercado, after the segment switch and after the financial crisis), for example, is 156% greater than for those firms in the other levels, before the segment switch and before the financial crisis (0,328 – 1,004 + 0,452 + 0,255 + 0,381 + 0,578 + 0,573) as in Model 7.

Table 2.3: Cumulative abnormal returns and the Global Financial Crisis

This table presents regression results for switches to the Novo Mercado on the cumulative abnormal returns CAR [-10, t] with t= 10, 30, 90, 250, 500, 750, or 1,000 trading days after the segment switch. *Novo Mercado*, *After Crisis* and *Post* are dummy variables that capture the market segment of the security, the switches that occurred after the beginning of the financial crisis and the period post-announcement, respectively (the baseline category are the firms that switch to Nível 1 and Nível 2 and the period before the switch and before the Global Financial Crisis). Robust standard errors clustered at the firm level are reported in parentheses. *, **, and *** indicate the coefficients that are significantly different from zero at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. var.: CAR[-10, t]	10 trading days	30 trading days	90 trading days	250 trading days	500 trading days	750 trading days	1,000 trading days
Novo Mercado	0.357***	0.355***	0.347***	0.338***	0.331***	0.327***	0.328***
	(0.0116)	(0.0126)	(0.0167)	(0.0202)	(0.0229)	(0.0259)	(0.0298)
Post	0.0669***	0.0575***	0.140***	0.236***	0.324***	0.397***	0.452***
	(0.0181)	(0.0143)	(0.0156)	(0.0173)	(0.0191)	(0.0214)	(0.0245)
After Crisis	-0.428***	-0.460***	-0.490***	-0.634***	-0.683***	-0.786***	-1.004***
	(0.0614)	(0.0700)	(0.104)	(0.158)	(0.206)	(0.234)	(0.248)
*Novo Mercado * Post*	-0.0208	0.00452	-0.0423**	0.0170	0.0658***	0.143***	0.255***
	(0.0241)	(0.0182)	(0.0189)	(0.0203)	(0.0222)	(0.0248)	(0.0283)
*Novo Mercado * After Crisis*	0.409***	0.407***	0.398***	0.388***	0.384***	0.382***	0.381***
	(0.0187)	(0.0203)	(0.0269)	(0.0325)	(0.0369)	(0.0418)	(0.0480)
*Post * After Crisis*	-0.0126	0.0203	0.0551***	0.205***	0.255***	0.360***	0.578***
	(0.0237)	(0.0168)	(0.0159)	(0.0159)	(0.0168)	(0.0186)	(0.0211)
*Novo Mercado * Post * After Crisis*	-0.0390	0.0117	-0.0153	0.146***	0.252***	0.338***	0.573***
	(0.0376)	(0.0285)	(0.0297)	(0.0321)	(0.0351)	(0.0392)	(0.0448)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,650	6,790	10,210	19,330	33,580	47,789	61,220
Number of firms	57	57	57	57	57	57	57
R^2	0.0332	0.0538	0.0715	0.0863	0.0897	0.0785	0.0633

2.4.3. Segment switches and market microstructure

Given the positive reaction of the stock market to higher standards of corporate governance, we now investigate the market microstructure of the stocks. We consider several variables related to market liquidity before and after the segment switch and the Global Financial Crisis to test our Hypothesis 2.3. Specifically, we consider the following demand-side variables: the number of trades by investors, the number of securities traded by investors, and the stock trading volume. The bid-ask spread, however, is a supply-demand variable. Figure 2.4 displays the results.

Figure 2.4: Market microstructure variables before and after segment switches

This figure shows different variables related to market microstructure of the stocks for the firms that switch to Nível 1, Nível 2 and Novo Mercado. Figure 4a displays the number of trades per share, Figure 4b the number of shares traded, Figure 4c the trading volume and Figure 4d the bid-ask spread.

Figure 4a: Trades per share

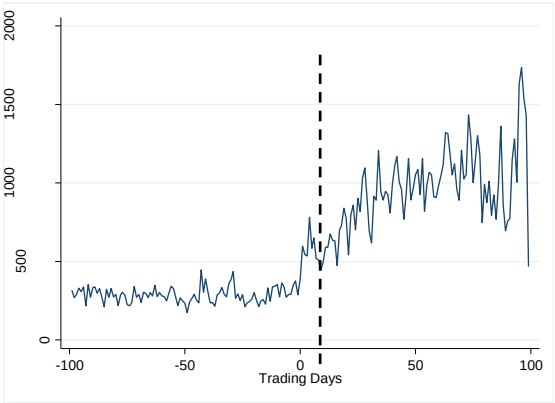


Figure 4b: Number of shares trades

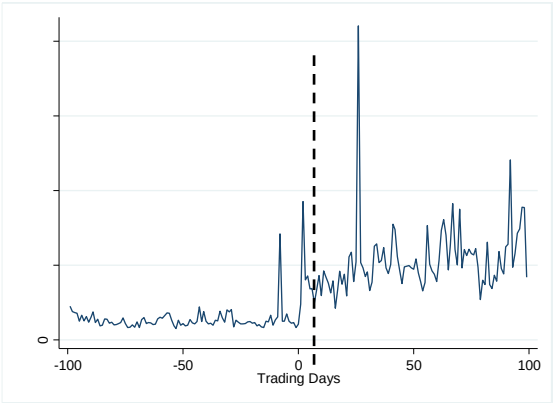


Figure 4c: Trading volume

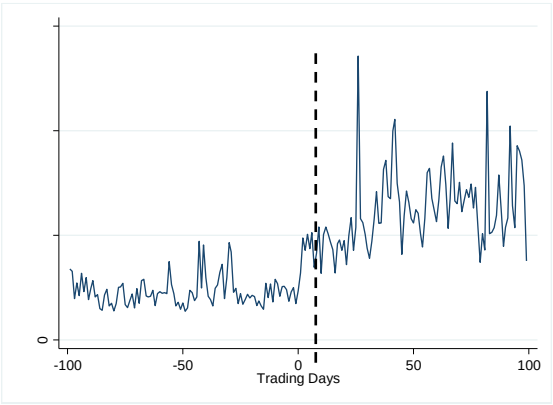


Figure 4d: Bid-ask spread

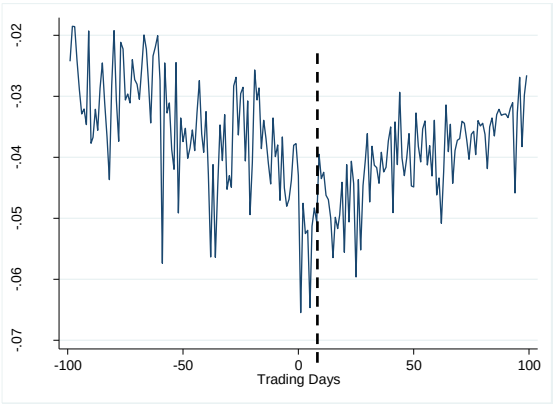


Table 2.4 reports the regression results, further differentiating between the period before and after the Global Financial Crisis. We find evidence suggesting that higher corporate governance significantly improves the market microstructure following the segment switch announcement, especially after the Global Financial Crisis.

Table 2.4. Segment switches, market liquidity and the Global Financial Crisis

This table reports regression results for different measures of market liquidity. In Model 1, the dependent variable is the number of trades made by investors. In model 2, the dependent variable is the number of securities traded by investors. In Model 3, the dependent variable is the financial volume of the securities traded. These 3 models consider 1,000 trading days after the segment switch. In models 4-7, the dependent variable is the Bid-Ask Spread 10, 90,500, and 1,000 trading days after the segment switch. *Novo Mercado*, *After Crisis* and *Post* are dummy variables that capture the market segment of the security, the switches that occurred after the global financial crisis and the period post-announcement, respectively (the baseline category are the firms that switch to Nível 1 and Nível 2 and the period before the switch and before the Global Financial Crisis). Robust standard errors clustered at the firm level are reported in parentheses. *, **, and *** indicate the coefficients that are significantly different from zero at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. var.:	Number of trades	Number of securities traded	Trading volume	Bid-ask spread (10 days)	Bid-ask spread (90 days)	Bid-ask spread (500 days)	Bid-ask spread (1,000 days)
Novo Mercado	-18.70	-90,180	-583,520	-0.00736***	-0.00699***	-0.00651***	-0.00621***
	(44.01)	(102,068)	(614,042)	(0.00231)	(0.00203)	(0.00192)	(0.00184)
Post	172.3***	377,525***	5,296,149***	-0.0111***	-0.00434**	-0.00511***	-0.00561***
	(42.22)	(97,952)	(589,068)	(0.00381)	(0.00196)	(0.00185)	(0.00177)
After Crisis	620.2	-129,893	-4,083,133	0.00164	0.00218	-0.000216	-0.00168
	(409.5)	(608,866)	(4,985,103)	(0.00744)	(0.00540)	(0.00525)	(0.00517)
*Novo Mercado * Post*	-144.2***	-447,088***	-2,621,682***	0.0100**	0.00548**	0.00757***	0.00849***
	(48.95)	(113,547)	(682,926)	(0.00493)	(0.00229)	(0.00214)	(0.00205)
*Novo Mercado * After Crisis*	120.1*	266,740*	1,690,559*	-0.000158	-0.000970	-0.000563	-0.000283
	(65.38)	(151,621)	(912,197)	(0.00343)	(0.00301)	(0.00285)	(0.00273)
*Post * After Crisis*	32.00	-161,246**	2,745,036***	0.00153	0.00137	0.00338**	0.00483***
	(35.41)	(82,116)	(494,062)	(0.00477)	(0.00182)	(0.00157)	(0.00148)
*Novo Mercado * Post * After Crisis*	893.3***	464,039***	6,335,921***	-0.00636	0.000356	0.00362	0.00590
	(71.04)	(164,803)	(991,223)	(0.00736)	(0.00341)	(0.00312)	(0.00398)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	61,220	61,220	61,220	5,650	10,210	33,580	61,220
Number of firms	57	57	57	57	57	57	57
	0.0224	0.0124	0.0313	0.0084	0.0084	0.0094	0.0136

The coefficient of the triple interaction shows that different measures of market microstructure significantly improve for the firms that switch to the Novo Mercado following the announcement date in the period after the global financial crisis. As shown in Models 1-3, the number of trades, the amount of securities traded, and the volume of negotiations are significantly higher than for those firms in the other levels before the segment switch and before the financial crisis. The findings are consistent with the earlier results on Hypothesis 2.2, indicating that the long-run effect of the segment switches became stronger after the Global Financial Crisis. The coefficient of the bid-ask spread, although not statistically significant in the triple interaction, shows that the Novo Mercado firms are subject to higher liquidity and lower informational asymmetries in the stock market.

2.4.4. Segment switches and financial health

Considering that the long-run performance of the firms that switch to the Novo Mercado is significantly greater than the other two levels, we now test our Hypothesis 2.4. We perform a cross-sectional regression analysis that examines the link between cumulative abnormal returns and firm characteristics, testing whether firms' financial health influences the impact of higher standards of corporate governance on stock returns. Table 2.5 reports the results.

We find that neither the standards of corporate governance nor financial health has a direct impact on firms' cumulative abnormal returns (column 1). One plausible reason is the low explanatory power as reflected by the low R^2. However, when we add the interaction term of Novo Mercado and financial health in column (2), the R^2 of the model increases substantially.[20] Importantly, the coefficient of the interaction term is positive significant, indicating that the more financially healthy firms among those that switch to the Novo Mercado

20 We find the same result when we use firms' return-on-equity (ROE) as a measure of financial health instead of the Altman Z-score.

benefit as their cumulative abnormal returns are significantly greater than the ones of other firms ($\beta = 0.0021$, $p < 0.01$). The finding remains robust when we add market- and firm-related controls, year fixed effects and industry fixed effects. The full model (Model 5) confirms that the impact of corporate governance on the cumulative abnormal return is higher for financially healthy companies compared to others. The evidence suggests that stronger firms gain relatively more from higher levels of corporate governance.

Table 2.5. Segment switches and firms' financial health

This table reports the results of a cross-sectional regression for firms' cumulative abnormal returns CAR[-10, 10]. *Novo Mercado* is a dummy variable that equals 1 for firms that switch to the Novo Mercado, and zero otherwise. The models (1)-(5) estimate the impact of the Z-score, Novo Mercado, the interaction terms and market-, and firm-level characteristics on the cumulative abnormal returns. We also included year- and industry-dummies. Robust standard errors are reported in parentheses. *, **, and *** indicate the coefficients that are significantly different from zero at the 10%, 5%, and 1% levels, respectively.

Dep. var.: CAR[-10, 10]	(1)	(2)	(3)	(4)	(5)
Novo Mercado	-0.0172	-0. 0466	-0.0299	-0.0725	-0.00744
	(0.0558)	(0.0551)	(0.0508)	(0.0708)	(0.0747)
Z-Score	-0.0000	-0.0000	-0.0000***	-0.0000	-4.15e-05
	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(4.09e-05)
*Novo Mercado * Z-Score*		0.00219***	0.0023***	0.0023**	0.00224*
		(0.0007)	(0.0007)	(0.0012)	(0.00107)
Return Ibovespa			1.0815**	1.6171**	2.1513**
			(0.4194)	(0.8687)	(1.100)
Firm size			0.0093	0.0283	0.0478
			(0.0141)	(0.0188)	(0.0331)
Firm age			0.0000	0.0000	0.0000
			(0.0000)	(0.0000)	(0.0001)
Investments/Equity			0.00941	0.0479	0.1252
			(0.0512)	(0.0525)	(0.1053)
Year FE	-	-	-	Yes	Yes
Industry FE	-	-	-	No	Yes
Number of observations	57	57	57	57	57
R^2	0.008	0.078	0.091	0.328	0.486

2.5. Further empirical checks and robustness tests

In this section, we summarize findings from further empirical checks and robustness tests. First, we revisit the positive short-term effects of the stock market reform to ensure the comparability with earlier studies (e.g., Carvalho, 2002; Aguiar, Corrar and Batistella, 2004; Rogers, 2006; Procianoy and Verdi, 2009; Carvalho and Pennacchi, 2012). We compute the firm-specific abnormal stock returns (ARs) and use dummy variables for the days in the event window [-10, 10] in the regression model. The coefficients of these dummy variables indicate the abnormal returns on each of the days. We also report the cumulative abnormal returns (CARs). Table 2.6 presents the results.

Table 2.6. Average abnormal stock returns by segment

This table shows the abnormal stock returns by event time *t* for all the 57 companies that switch the listing segment. The announcements are categorized into three groups, according to the level after the segment switch. AR is the average abnormal return for the specified listing segment in event time and CAR is the sample average cumulative abnormal return for the days during the event time window [-10, 10]. Significance is based on two-tailed t-tests. Standard errors are reported in parentheses and *, ** and *** denote significance at the 10%, 5%, and 1% level, respectively.

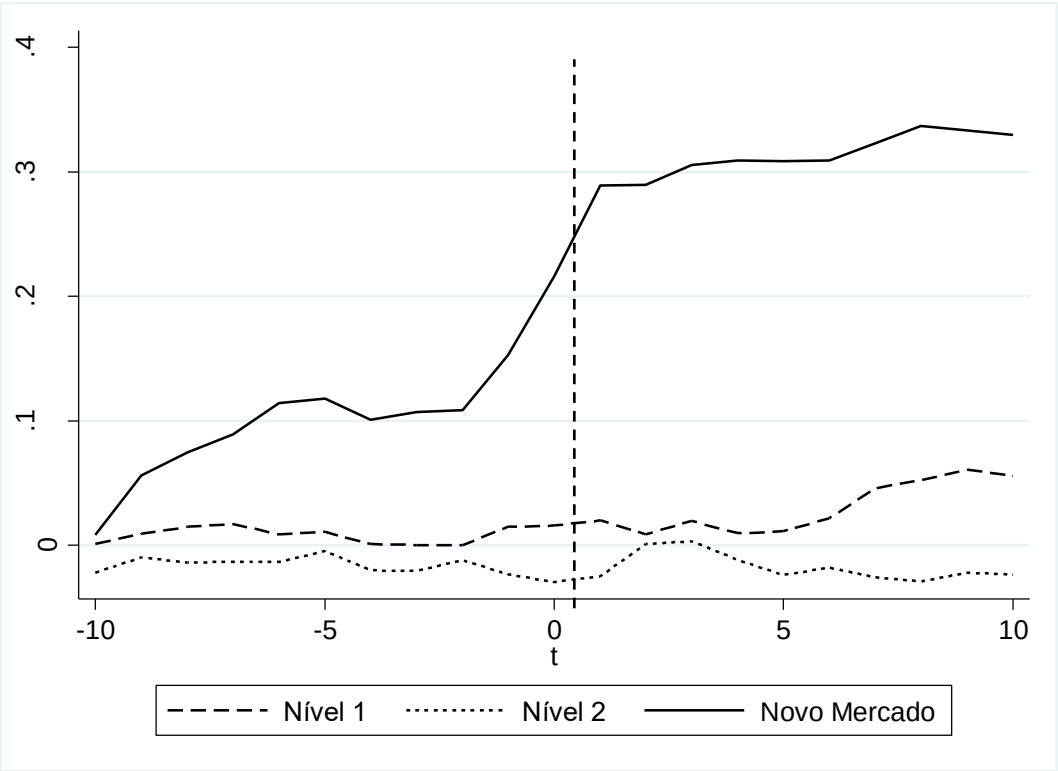
Event time	Switches to Nível 1		Switches to Nível 2		Switches to the Novo Mercado	
	AR	CAR	AR	CAR	AR	CAR
-10	0,0076*	0,0076*	-0,0168*	-0,0168*	0,0134*	0,0134*
-9	0,012**	0,0201***	0,0134*	-0,0034	0,0092	0,0226**
-8	0,0007	0,0208**	-0,0055	-0,0089	0,0184**	0,0411***
-7	0,0039	0,0247**	0,0027	-0,0062	0,0193*	0,0604***
-6	0,0034	0,0282**	-0,0061	-0,0123	0,0249***	0,0854***
-5	0,0011	0,0293**	0,0070	-0,0053	0,0008	0,0861***
-4	-0,0034	0,0260**	-0,0160*	-0,0214	-0,0055	0,0806***
-3	-0,0018	0,0242*	0,0015	-0,0198	0,0132*	0,0938***
-2	0,0012	0,0253*	0,0078	-0,0121	-0,0079	0,0859***
-1	0,0050	0,0303**	-0,0132*	-0,0252	0,0008	0,0866***
0	0,0055	0,0358**	-0,0067	-0,0319	0,0519***	0,1386***
1	0,0025	0,0383**	0,0070	-0,0250	0,0260***	0,1646***
2	-0,0026	0,0357**	0,0297***	0,0048	-0,0172**	0,1475***
3	0,0059	0,0416**	0,0044	0,0092	0,0101*	0,1575***
4	-0,0040	0,0376**	-0,0158*	-0,0067	0,0037	0,1612***
5	0,0061	0,0437**	-0,0104	-0,0170	-0,0088	0,1525***
6	0,0076*	0,0513**	0,0066	-0,0105	0,0020	0,1544***
7	0,0276***	0,0790***	-0,0098	-0,0203	0,0046	0,1590***
8	0,0146***	0,0936***	-0,0031	-0,0234	0,0083	0,1673***
9	-0,0059	0,0877***	0,0058	-0,0176	-0,0074	0,1599***
10	-0,0012	0,0864***	-0,0003	-0,0179	0,0024	0,1624***

The aggregate effect of segment switch announcements at time t=0 is statistically significant only for firms switching to the Novo Mercado. The AR is 5.19% and the CAR is 13.86%. The impact of the segment switch announcement is significant and positive only on the days surrounding the announcement date. The effect does not persist during the days

following the event. Figure 2.5 displays the cumulative abnormal returns over the event window [-10, 10] by segment.

Figure 2.5: Short-term cumulative abnormal returns by segment switch

The figure shows firms' cumulative abnormal stock returns CAR[-10, 10] for the three stock market segments.



Consistent with the regression results, Figure 2.5 shows that the impact of announcement is greater for switches to the Novo Mercado than for switches to the other levels, especially at event time -1 and 0. The evidence shows that the market positively reacts when the firms engage with the highest degree of information disclosure and minority shareholder protection, which is the Novo Mercado. Table 2.6 and Figure 2.5 provide evidence supporting this view. We note that there are many non-significant abnormal returns for the segment switches to Nível 1 and 2. It is possible that the incremental value of these segments is not

particularly high for investors in the short-term, and the best efforts in making available any public offer for the acquisition of company shares, translate all financial reports to English language, and becoming a member of the Market Arbitration Chamber are not as important as they should be. There is also the possibility that the Nível 2 is just seen as a transition stage towards the Novo Mercado, and the companies that switch to this level are still one step away from adopting the highest standard of corporate governance. Consequently, switching to the intermediate levels suggests that the firms are not yet fully prepared to comply with the highest level of governance and information disclosure required by Novo Mercado.

Second, our main specification is based on an event window with 10 trading days surrounding the announcement of migration [t_{-10} - t_{+10}]. We now consider the alternative event window [-1, 1] and obtain similar results. The results confirm that the aggregated announcement effect at t = 0 is statistically significant at the 1% level only for the firms that switch to the Novo Mercado. Similar to our previous analysis, neither the firms that switch to Nível 1 nor those that switch to Nível 2 exhibit a significant stock market response during the shorter event window.

Third, the cumulative abnormal returns of firms that switch to the Novo Mercado might be driven by segment jumps, i.e., firms that switch from the Traditional Level or Nível 1 directly to the Novo Mercado. We find that the significant effect is slightly smaller compared to the one we found previously. Whereas the CAR is 0.162 for the entire set of migrations to the Novo Mercado ($p < 0.01$), the aggregate effect for the cases of jumps is slightly smaller but still highly significant (CAR = 0.149, $p < 0.01$). These results suggest that the market response is not exclusively driven by segment jumps.

Fourth, we investigate whether the segment switches affect firms' cost of capital. The market beta, i.e., firms' sensitivity to the market factor, is expected to decrease after the segment

switch. We analyze the dynamics of the market beta before and after the segment switch using panel data regression models. Table 2.7 reports the results.

Table 2.7. Segment switches and firm beta

This table reports regression results for the firms' market beta. The dependent variable is firms' beta coefficient of the CAPM Model, estimated as a moving coefficient over 5 trading days. Models 1-3 report the main effects of the dummies *Novo Mercado* and *Post* on the beta coefficient using 90 trading days after the segment switches. Models 4-7 report the effect over different time horizons after the segment switch: 10, 30, 60, and 90 trading days after the announcement. Models 8-9 are similar to Model 7, except that they include fixed effects *Novo Mercado* is a dummy variable that equals one for firms that switch to the Novo Mercado, and zero otherwise. *Post* is a dummy that equals one during the period after the announcement, and zero otherwise. Robust standard errors are reported in parentheses. *, **, and *** indicate the coefficients that are significantly different from zero at the 10%, 5%, and 1% levels, respectively.

Dep. var.: β_i	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Novo Mercado	-0.287**	-0.284**	-0.379***	-0.382**	-0.357**	-0.339**	-0.357***	-0.4073***	-0.5336***
	(0.135)	(0.136)	(0.136)	(0.156)	(0.140)	(0.133)	(0.132)	(0.1342)	(0.1905)
Post		0.0535***	0.249***	-0.299**	0.181***	0.203***	0.217***	0.1861***	0.1856***
		(0.0167)	(0.0279)	(0.123)	(0.0660)	(0.0478)	(0.0406)	(0.0450)	(0.0462)
*Novo Mercado * Post*			-0.304***	0.275*	-0.120	-0.186***	-0.192***	-0.1515***	-0.1485***
			(0.0348)	(0.166)	(0.0880)	(0.0629)	(0.0528)	(0.0552)	(0.0554)
Year FE	-	-	-	-	-	-	-	Yes	Yes
Firm FE	-	-	-	-	-	-	-	No	Yes
Number of observations	10,210	10,210	10,210	5,650	6,790	8,500	10,210	10,210	10,210
Number of firms	57	57	57	57	57	57	57	57	57
R^2	0.0760	0.0722	0.0910	0.1110	0.0710	0.0682	0.0802	0.0103	0.0912

We find that firms switching to the Novo Mercado exhibit a reduction in their market beta after the switch. The results shown in column (1) show a negative and significant effect that remains robust in different specifications of the model. In general, the beta coefficient of the firms in the Novo Mercado is 28.7% smaller ($p < 0.05$) than the firms in the other levels, indicating that their cost of capital is lower and their incentives to issue more equity are higher. The coefficient of *Post* in column (3) indicates that the beta increases after the announcement date, while it decreases by 43.4% for the Novo Mercado in the Post period (= -0.379 + 0.249 - 0.304). This effect differs according to the time horizon. Column (4) shows the impact over 10 days following the announcement. For this horizon, *Post* shows a negative coefficient, indicating that the cost of capital of all the firms declines after the segment switch, but being in the Novo Mercado in this post period, increases the beta coefficient. Nonetheless, the overall effect in the post period is still negative and significant in 40.6% (-.382 -.299 + .275). This result is robust for longer time horizons, as we show in columns (5)-(7), and also to the inclusion of year and firm fixed-effects in columns (8)-(9).

Fifth, we investigate whether our results are driven by the Brazilian firms that issue ADRs in the United States. The literature has shown that ADR issuers display higher positive abnormal returns in the short run than other firms (Carvalho, Pennacchi, 2012). Table 2.8 reports the stock market performance of ADR issuers and non-issuers for different time horizons.

Table 2.8. Cumulative abnormal returns by ADR issuance

This table reports cumulative abnormal stock returns CARs [-10, t] of segment-switching firms that issue ADRs (n = 23) and those that do not issue ADRs (n = 34) for different time horizons. The reported values are means with standard deviations in parenthesis. The two bottom rows report the p-values of the t-test and Wilcoxon rank sum test, comparing ADR issuers and other firms.

CAR [-10, t]	t = 0	t = 30	t = 100	t = 500	t = 1000
ADR issuance	0.1808	0.1763	0.3314	0.6348	1.1172
	(0.3128)	(0.3645)	(0.4812)	(1.0172)	(1.2506)
No ADR issuance	0.1487	0.1956	0.2079	0.6042	0.6964
	(0.5645)	(0.5969)	(0.8895)	(1.1777)	(1.6059)
t-test (p-value)	0.8114	0.8936	0.5599	0.9211	0.3259
Wilcoxon rank sum test (p-value)	0.3050	0.7345	0.1123	0.5405	0.1896

We find that the mean (and median) cumulative abnormal stock returns of ADR issuers and non-issuers are not statistically different from each other. Hence, these findings indicate that ADR issuance does not drive our results on the long-run impact of committing to higher standards of corporate governance in the Brazilian stock market.

2.6. Conclusion

In this study, we investigate whether there are long-run effects of higher standards of corporate governance on firms' stock returns in Brazil. The literature has documented positive short-term effects of the major stock market reform in Brazil that took place in the early 2000s, but no study has examined whether these effects persist over a longer horizon. Positive long-run effects are by no means guaranteed in an emerging market such as Brazil because higher corporate governance is costly, might be compromised by weak enforcement, and vary with economic, legal and political uncertainty.

We find that higher corporate governance of Brazilian firms results in significantly better stock market performance in the long run. We also find that this positive impact of higher corporate governance increases during the Global Financial Crisis, market microstructure improves, and the market impact is stronger for financially healthy firms. Several additional empirical checks and robustness tests confirm these findings.

Our results suggest that firms' commitment to higher standards of corporate governance pays off in the long run. We contribute to the literature on corporate governance, disclosure and transparency, providing evidence that companies that commit to higher levels are seen as better investment opportunities. Other emerging economies such as India, Russia[21] and the Philippines that consider improving corporate governance standards, should be encouraged by the positive evidence we provide for Brazilian firms.

21 For example, the Moscow Exchange intended to implement the Novy Rynok, which is similar to the Brazilian Novo Mercado, in 2013. However, besides the expectation of the market, the implementation was not successful and still there are no distinct segments of governance in Russia.

3. ANTI-CORRUPTION INVESTIGATIONS AND CREDIT TO CORRUPT FIRMS[22]

22 This paper is based on an on-going research project by M. Campello (Cornell University), C. Ferraz (University of British Columbia & PUC-Rio), L. Moura (FGV-EBAPE), L. Norden (FGV-EBAPE) and R. Schechtman (Banco Central do Brasil).

3. 1. Introduction

Corruption is an endemic problem of enormous magnitude worldwide, while crucial for debate in developing countries due to an inefficient judicial system (Mauro, 1995), weak prudential regulation (Krueger, 1993; De Soto, 1989), poor development of good governance (Kaufmann, 2004; Wu, 2005) and misallocation of public resources (Easterly, 1993; Restuccia and Rogerson, 2017; Leff, 1964). When pervasive, corruption may hamper effective financial intermediation and force some borrowers without bank relationships to reject sound financial projects, therefore reducing their growth (Beck et al., 2005). In this sense, Brazil, a developing country that has become one of the great emerging markets and highly favored for capital inflows of foreign investors, is still consistently poorly ranked by international organizations such as Transparency International, although many anti-corruption initiatives are being taken aiming to control this malfeasance.

One of the most prominent initiatives, code-named "*Operação Lava Jato*" (Operation Car Wash), aimed at uncovering overbilling of public expenditures and bribes in public contracts, soon turned into one of the largest and most complex anti-corruption investigations in the history of South America. This initiative, headed by the Federal Police and the *Ministério Público Federal*, involves an estimated over 1 billion documents seized, 42 billion *reais* involved in corruption, 6.4 billion bribes paid to public officials and more than 900 search and seizure warrants on the ongoing of the investigations (2014-2021). Political and economic uncertainty became noticeable, many trades were interrupted as the investigations unfolded into something bigger and the fear of a propagation effect of corruption became eminent in financial markets.

Given the economic relevance of these important schemes against corruption, one might infer that the outbreak of substantial anti-corruption investigations in Brazil in recent years

likely affected the way of doing business in the country, probably moving the country from a regime with high corruption and possible misallocations (pre-2014) to a regime with less corruption and less room for misallocation (post-2014). Therefore, in this paper we investigate whether and how major anti-corruption investigations concerning a set of big firms in the Brazilian economy (*Operação Lava Jato*, 2014-2021) affected the bank lending channels.

From the point of view of banks, we acknowledge that the main channel that drives their response is related to financial corporate considerations, such as incentives to risk shifting (Lava Jato firms become riskier after the start of the anti-corruption investigations), market discipline pressures, charter values (Jensen and Meckling 1976; Keeley 1990; Merton, 1977), but mainly, "stigma" that discredits the investigated firms. At the same time, firm heterogeneity might be taken into consideration when assessing their market value and characteristics such as their overall credit risk and pre-existing bank relationships might also play an important role.

There is one main obstacle that researchers face when empirically investigating the bank lending channel towards corrupt firms. Corruption by its inherent secret nature is difficult to measure and endogenously chosen by firms, making it difficult to causally identify its adverse effects. Therefore, we rely on unanticipated anti-corruption investigations against firms to circumvent this challenge. Our setting allows to directly address the issue of endogeneity since the start of the investigations and detection of firms' corruption were exogenously motivated by public oversight bodies and could not be influenced by firms. Exploiting the variation in the credit market towards corrupt firms in our setting can lead us to trace the causal impact of anti-corruption investigations on bank lending, as well as the heterogeneity inherent to this channel.

Using a newly assembled dataset, we combined data from three sources of information. The main source of information is a private dataset owned and managed by the Central Bank of Brazil (SCR – Credit Information System), containing detailed information on all loans at the monthly bank-firm level. The second comes from RAIS (Relação Annual de Informações

Sociais), an administrative data set collected on an annual basis by the Brazilian Ministry of Labor, which covers all former workers in Brazil. The third one comes from TSE (Tribunal Superior Eleitoral), with detailed information on firms' connection to the government through donations in federal elections. Our final sample covers around 17,000 firm-quarter observations referring to new loans to 1,676 different firms with more than 250 employees in the years between 2013 and 2016.

Overall, our findings indicate that the anti-corruption investigations had adverse effects for the quantity and quality of new loans for investigated firms. First, our main analysis shows that Lava Jato firms considerably receive less credit after the ongoing of the investigations, an effect that monotonically increases as time passes by. Furthermore, those affected firms receive lower credit ratings and display higher loan loss provisions after the investigations have started. Second, this effect is less pronounced for firms which are more connected to the government and have more skilled workers, whilst more pronounced for firms with more bank relationships in the pre period. Third, our results do not indicate any clear and robust pattern when considering heterogeneous effects between private and state-owned banks. Our main effect is robust to different control groups including the largest unlisted non-financial firms in Brazil, two different matched samples with similar ex-ante key characteristics and a synthetic control approach created to ascertain parallel trends in the pre period.

Despite the extensive literature on corruption used as background for our paper (Becker, 1968; Rose-Ackerman, 1978; Shleifer, Vishny, 1993; Mauro, 1995; Svensson, 2005; Zeume, 2017; Fisman et al., 2021), there is still no evidence on the consequences of unanticipated anti-corruption investigations against firms, mainly when it comes to detailed bank-firm-level analyses. There are several studies that analyze other anticorruption programs, mostly relying on the audits reports promoted by the *Controladoria Geral da União* (CGU) that took place after 2003 (Ferraz, Finan, 2008; Moreira, 2009; Ferraz, Finan, Szerman, 2016; Avis, Ferraz,

Finan, Varjão, 2019; Lagaras, Ponticelli, Tsoutsoura, 2017; Colonnelli, Prem, 2020). Others rely on indicators of health sector's corruption derived from internal control systems in Buenos Aires (Di Tella, Schargrodsky, 2003); accompany transfers of resources from source to destination in educational programs in Uganda (Reinikka, Svensson, 2004); or evaluate a redistributive program implemented in Indonesia (Olken, 2006).

Although all these studies indicate the enormous costs of public expenditures corroded by corruption (Jensen, Meckling, 1976; Shleifer, Vishny, 1993; Di Tella, Schargrodsky, 2003; Reinikka, Svensson, 2004; Olken, 2006; Zeume, 2017; Avis, Ferraz, Finan, Varjão, 2019) and provide evidence on the aggregate benefits of higher monitoring, none of these studies have investigated the direct impact of corruption detection on the supply of credit through credit market networks. This paper takes further steps on the topic and aims to fill this gap in the literature, which is directly related to the level and allocative efficiency of economic activity.

We relate to two main strands of literature. First, the classic economic literature provides evidence that corruption negatively influences economic growth and development (Shleifer and Vishny, 1993; Mauro, 1995; Svensson, 2005; Robinson et al., 2005). Many scholars have previously highlighted that corruption is not "greasing the wheels" of bureaucracy, focusing either on its macro-economic consequences (Kaufmann and Wei, 1999; Hall and Jones, 1999; Fisman and Svensson, 2007; Olken and Barron, 2009), or in the micro-economic impact (Lui, 1985; Wei, 2000; Bologna et al., 2015; Zeume, 2017; Giannetti et al., 2020; Lagaras et al., 2017) of anti-corruption programs. On the opposite side, nevertheless, there are others that argue that corruption does "grease the wheels" of bureaucracy (Leff, 1964; Leys, 1965; Huntington, 1968; De Rosa et al., 2015), being beneficial in a second-best world due to the distortions caused by ill-functioning institutions[23].

23 Colonnelli and Prem (2020) provide a novel causal evidence against the "grease the wheels" argument. Using a detailed matched dataset, the authors show that anti-corruption investigations in Brazil promoted by the

Other recent studies have advanced our knowledge on the causes and consequences of organized crime and corruption in Italy, a country that has been historically plagued by these phenomena. Calamunci and Drago (2020), using longitudinal firm-level data, estimated the spillover impacts of criminally connected firms on other (legal) firms working within the same market and showed a critical source of allocative inefficiency enforced by organized crime on legal economic activities. Similarly, De Angelis et al. (2020), using a detailed and private dataset on corruption cases across Italian municipalities, show that financial funds got from the European Union increased in 4% the number of white-collar crimes.

Second, we also contribute to a new and growing literature that examines the detrimental impact of corrupt environments and bank lending activity (Beck, Demirgüç-Kunt, Levine, 2006; Charumilind, Kali, Wiwattanakantang, 2006; Barth, Lin, Lin, Song, 2009; Weill, 2011; Park, 2012; Qi, Ongena, 2019). An initial attempt to better understand the relationship between corruption detection and external finance was provided by Beck et al. (2006). In a cross-country analysis with more than 2,500 firms across 37 countries, they found that countries with stronger supervisory agencies[24] tend to have firms that face greater obstacles to obtaining bank loans because of corrupt bank officials than firms in countries where the supervisory agency is less powerful. They also claim that banks do not only allocate capital based on risk-return criteria, rather, when supervisory agencies have the power to influence the distribution of bank loans, then corruption and political ties may shape the allocation of bank credit.

Controladoria Geral da União (CGU) lead to higher levels of economic activity, as observed by the significant increase in the number of firms and business establishments operating in treated municipalities randomly audited from 2003 to 2014. Furthermore, they also document an increase in total sales by local firms, an increase in the total volume of lending and deposits in local banks, and an effect primarily focused on those economic sectors that most rely on government relationships.

24 Countries where supervisory agencies can intervene banks, replace managers, force provisioning, stop dividends etc.

Similar issues are also examined in other developing economies[25] (for a detailed revision of corruption in developing countries, see Wraith and Simpkins, 2010). Weill (2011) analyzes the impact of corruption on bank lending in Russia to understand the causes of financial underdevelopment and finds that corruption hampers bank lending, and the risk-averse banks are the ones lending less. Similarly, Charumilind, Kali, Wiwattanakantang (2006) investigate the lending activity to business connected firms in the presence of corruption in Thailand and find that firms with connections to banks and politicians had greater access to long-term debt than firms without such ties[26]. Many other recent studies delve into anti-corruption investigations around the world with special focus on credit markets and credit reallocation (Ding et. al, 2018; Chen and Kung, 2019; Griffin, Liu, Shu, 2018).

Despite the two strands of literature abovementioned and the progress we have achieved so far, the evidence on how the market participants react to corruption detection is scant. Using the same corruption scandal emerged in Brazil by the *Operação Lava Jato*, Figueiredo (2016) develops a theoretical model to discuss the links between campaign contributions, political exploitation, and overcharged public contracts channeled back to campaigns, politicians, parties and senior bureaucrats. His results show that banning corporate funding for candidates and parties seem to be a fragile (and not necessarily efficient) attempt to curb a bigger problem:

25 For a cross-country analysis that covers firms across 22 transition countries from Europe and address the impact of bribery on credit access from 2007 to 2014, see Qi and Ongena (2019). The firm-level variables were measured through a survey conducted jointly by the European Bank for Reconstruction and Development (EBRD) and the World Bank. Their results show that firms involved in bribery practices have more limited access to bank credit, this impact is mainly driven by supply-side factors, and the effect is less pronounced in localities where there are more foreign banks since they lack the knowledge to distinguish corrupt from not corrupt firms. Partially through the tightening of firms' credit access, bribery hinders firm growth.

26 In a similar setting, Khwaja and Mian (2005) show that politically connected firms – those in which managers are involved in politics and with elections - receive considerable preferential treatment in bank funding using data from 90,000 Pakistani firms over the period 1996-2002. Analogously, Claessens et al. (2008) find that Brazilian firms connected to the government – those that contributed to the presidential campaigns in 1998 and 2002 – received more bank funding than other firms.

institutionalized and systemic corruption[27]. Similarly, Szerman (2020) uses a newly assembled dataset provided by *Controladoria Geral da União* (CGU) to investigate how corporate debarments affect firms and worker outcomes. Results show that establishments included in the federal blacklists experience negative impacts on labor market outcomes and increase their probability of exiting the formal sector, which is driven by the lost revenues from the government contracts. Most of the papers on the topic, albeit novel to the literature, focus on the legal aspects of the investigation herein addressed (Ribeiro, Cordeiro, Guimarães, 2016; Tourinho, 2018; Guimarães, Ribeiro, 2019) or are case studies that target specific companies involved in corruption scandals (Campos, Engel, Fischer, Galetovic, 2021).

The remainder of this paper is organized as follows: the next section provides a brief overview of the characteristics of the *Operação Lava Jato*, the Brazilian institutional context and develop our main hypotheses. Section 3 describes the empirical setting and the different databases we assembled for our analyses. Section 4 shows the descriptive statistics, section 5 presents the results and section 6 concludes.

3.2. Institutional background and hypotheses

3.2.1. Institutional background

This section provides a brief overview of the characteristics of the *Operação Lava Jato*[28,29] and the Brazilian institutional context. In sum, the scandal was about a public

27 Another important and related paper (Oliveira et al., 2015) uses the *Operação Lava Jato* to debate irregularities related to cartel conduct and estimate some impacts on the economy: a retraction of almost R$87 billion in the annual production amount of the Brazilian economy and a reduction of more than 1 million vacancies in the number of jobs.

28 For a detailed description of the *Operação Lava Jato*, see "Inside the Car Wash: The Narrative of a Corruption Scandal in Brazil" (Figueiredo, 2016).

29 The name *Operação Lava Jato* (Car Wash Operation) is due to the use of a gas station to move values of illicit origin, investigated in the first phase of the operation, in which a black-market money dealer was arrested.

procurement endorsed by the state-owned oil firm Petrobras, in which the bidders were construction firms, and the indictments alleged the existence of bid apparatus and bribery schemes. In addition to its economic importance, it involves several important Brazilian politicians and billions of *reais* in corruption schemes folded by the involved companies, being the largest anti-corruption and anti-money laundering investigations in Brazil and the largest detected corruption scandal in the history of Latin America.

Launched in March 2014 and headed by the Federal Police and the Public Prosecution Office, it emitted more than a thousand warrants for search and seizure, temporary arrest, preventive detection and coercive conduct, aiming at investigating a money laundering scheme that moved billions of *reais* in bribes. Essentially, it investigates crimes of active and passive corruption, fraudulent exchange operation, large-scale bribery, kickbacks and an illegal campaign financing scheme. According to inquiries by the *Lava Jato* task force, administrative members of the state-owned oil company Petrobras[30], politicians of the largest parties in Brazil, presidents of the Chamber of Deputies and the Federal Senate, governors of state, as well as executives of large Brazilian companies were involved in such schemes.

The operation had initially focused on agents known as *doleiros* (black market money dealers) who used small enterprises such as gas stations and car washes to lauder the profits of crime. Following with the investigations, prosecutors then found out that those same criminals laundered money for key executives at the state-owned oil giant Petrobras [31], also encompassed

Sequentially, through his confessions, it was verified its connection with the former director of Petrobras, arrested preventively in the second phase.

30 Intriguingly, however, is that Petrobras was previously defined as "the most autonomous and corporately coherent organization within the state enterprise system" (Evans, 1989), an exception if compared to typical glitches of public or state-owned enterprises. According to Almeida and Zagaris (2015), they are commonly captured by politicians in order to maximize their own interests, resulting in discredit because of inefficiency and lack of profitability.

31 According to the investigations of the Federal Public Ministry, witnesses have said that the construction companies formed a multi-year cartel to share out contracts and pad prices, perhaps extending beyond petroleum to highway and hydropower contracts. This cartel scheme of the contractors in works of Petrobras had existed for

in deals with numerous elected officials at the government level in an intricate web of corruption. In November (2014), the operation officially hit large Brazilian construction companies, such as Construtora OAS, Camargo Corrêa and Queiroz Galvão. A few months later (June 2015), other huge players in the segment were also included in the list: Andrade Gutierrez and Odebrecht[32], the Latin America's largest construction conglomerate.

Essentially, overpayments[33] of contracts negotiated between public and private institutions for office construction, drilling rigs, refineries and exploration vessels were diverted into accounts that then shifted the money to political parties and business conglomerates that were part of the schemes. Billions of dollars were dodged through a web layout scheme in which private interests could acquire political concessions, leading participants to bribe officials in 14 other countries in Latin America and Africa[34], concealing their illicit fund in Europe and the United States. The unfolding of the *Operação Lava Jato* outside Brazil, nonetheless, began after the Odebrecht group and Braskem had agreed to a leniency agreement with the United States Department of Justice (DOJ) to pay kickbacks abroad by more than one billion dollars. During the investigations, prosecutors reported crimes committed in several other countries in Europe, Africa and America. Odebrecht alone paid almost US$ 800 million in bribes abroad (Tourinho, 2018; Campos, et al., 2021).

In this sense, in February 2017 the Peruvian court issued an arrest warrant for the former president of the country on charges of having received approximately US$ 20 million to

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at least 15 years. Considering only the decade between 2004 and 2014, though, the companies-maintained contracts with Petrobras, which totaled 59 billion *reais*.

32 For a detailed analysis of the involvement between Odebrecht and the corruption scheme unfolded by the investigations, see Campos, Engel, Fischer, Galetovic (2021).

33 The construction firms and the public counterparts had formed an agreement that ensured guaranteed business on excessively lucrative terms if they agreed to channel a share of between 1% and 5% of every deal into secret slush funds.

34 The unveiled facts directly involve 14 countries, with implications for former or current heads of states of Argentina, Chile, Colombia, the Dominican Republic, Ecuador, Guatemala, Mexico, Panama, Peru, and Venezuela (BBC-Brasil 2017).

facilitate the approval of the construction of the Transoceanic highway linking northern Brazil with Peruvian coast, while in government, from 2001 to 2006. In the same month, executives of the world's fourth-largest offshore service provider – which later became the center of a scandal known as the Panama Papers – were preventively arrested in Panama. Furthermore, according to the Federal Public Ministry of Brazil, 17 countries requested documents for the Brazilian courts in order to steer their own investigations. Besides the United States and Switzerland, which have publicly disclosed their investigations right after cooperation agreements with Odebrecht, authorities in Italy, Denmark, Sweden, Norway and Guatemala have works and businesses in their country under criminal investigation and have already requested evidence from the *Operação Lava Jato*.

The operation had a successful start and worked very efficiently until 2016, then its investigations gradually slowed down, and it has proven to be closer and closer to politics. In 2019, for example, The Intercept Brazil portal disclosed conversations between the then judge Sergio Moro (judge in charge of the investigations) and prosecutors, which indeed questioned the impartiality of the investigations. Then, Lava Jato lost its luster and the *Procuradoria-Geral da República* (Attorney General's Office) announced the dissolution of its original nucleus in the beginning of 2021. It is currently conducted by GAECO (*Grupo de Atuação Especial de Combate ao Crime Organizado*), which is a group part of *Minitério Público Federal* and the operation, as initially conceived, came to an end after around 7 years in effect.

Among the 21 construction firms included in the blacklist of the investigations in the ongoing of the operations, two of them went bankrupt (GDK and Schahin), but the other big majority (12) entered into judicial reorganization process with its creditors (either during our sample period or afterwards)[35]. Furthermore, only 6 of them did not had their CEO or key

[35] The firms that entered in judicial reorganization are the following: Odebrecht, OAS, Queiroz Galvão, UTC Engenharia, Engevix, IESA, Mendes Junior, Galvão Engenharia, GDK, Schahin, Alumini, and Tomé Engenharia.

executives either arrested or with search and seizure warrants by the Federal Police, and no other large company was interested in acquiring them: they are probably affected by stigma and are seen as "zombies" or "pariahs" in the market, which shows that the market did not expect a bright future for them.

The abovementioned corruption scenario has its ground on some historical issues, indeed. Brazil's political setting is remarkably susceptible to corruption. With thirty-three parties and elections at three levels (city, state, and at the federal level) in one of the world's biggest countries, campaigns are harshly costly[36], and it is roughly unattainable for any individual political group to ensure a majority. Achieving power includes winning elections and forming coalitions (which can be achieved though the act of paying other parties), both of which require vast amounts of money. Consequently, one of the greatest advantages in Brazilian government has been the power to designate senior executives at state-owned companies, like Petrobras, because each executive could expect to reap millions in bribes from contracting companies, much of which could be deviated into campaign coffers again.

If one directly considers the financial system, for example, the scandal we consider in this paper has indeed a plausible negative effect: according to the Financial Stability Report (Central Bank, 2015), the exposition of the banks to the companies involved in the *Operação Lava Jato* deserves permanent monitoring. The document, made available in March of that year, pointed out the operation as one of the main factors that could lead to bank defaults. Despite

36 One of the main consequences of this investigation is the Brazilian Supreme Court agreement to prohibit corporate donations to electoral campaigns. Until 2015, firms (primarily from the construction, industrial and financial sectors), were the major political funders and their participation in campaign expenditures in Brazil had increased dramatically from 66% in 2006 to 76% in 2014 (TSE, 2015). The 2016 elections to elicit mayors and local council representatives in Brazil were the first ones, since 1994, that allowed only individual contributions to incumbents and parties. In this sense, according to Nichols (2011), a fundamental purpose of campaign finance regulation was to reduce corruption.

the optimism of the Central Bank regarding the Brazilian financial system strength[37], it is mandatory to consider the impact of a worsening in the constructors' financial situation in case of imposition of penalties to this sector. More than that, it is of paramount importance to understand how the credit market flows in the economy in the eminence of such endemic exposure.

3.2.2. Hypotheses

As a first reaction to the announcement of the anti-corruption investigations carried by the Federal Police, we explore whether banks react by decreasing lending to corrupt firms. It can also be the case that loan terms deteriorated: ratings were downgraded, loan loss provisions increased, maturity decreased as well as interest rates increased. In sum, they are all related to the credit risk of the investigated firms. The hypothesized effects might result from a combination of decreases in credit supply and credit demand[38].

> *Hypothesis 3.1 (Borrower risk).* After the detection of corruption, the amount of credit decreases to firms that are under investigation (H1a). Similarly, loan terms deteriorate: ratings are downgraded (H1b), loan loss provisions increase (H1c), maturity decreases (H1d) and interest rates increase (H1e).

Provided that the detection of corruption might lead to a credit contraction to investigated firms, we investigate the heterogeneity of this finding. It can be the case, for example, that pre-existing firm characteristics are able to distinguish their riskiness in the borrower-lender relationship. It can be the case, for example, that more connected firms,

37 According to the statement of the Supervision Director of Central Bank, Anthero Meirelles, "the financial institutions in Brazil are able to absorb the losses" (Valor Econômico, 2015). "BC says to be alert with Lava-Jato". Printed edition from 20.03.2015.

38 We address the issue of supply and demand in the methodological part of the paper by using different fixed effects specifications.

younger, smaller, with less skilled workers and with less bank relationships are riskier, therefore, leading the effect to be more pronounced for such firms. This hypothesized effect extends H1 by including firm characteristics and leads us to the following:

> *Hypothesis 3.2 (Borrower risk and firms' heterogeneity).* After the detection of corruption, the contraction of credit is more pronounced to firms that are more connected to the government (H2a), younger (H2b), smaller (H2c), with less skilled workers (H2d) and with less bank relationships (H2e).

Finally, complementing the previous step of the analysis, it is important to know whether state-owned banks have different behavior than privately-owned banks when it concerns the overall contraction of credit. It can be the case that the credit granted from these banks serve as a substitute to those firms that face a considerable credit contraction from private institutions. Additionally, state-owned banks might be influenced by politicians that benefit from the underlying corruption scheme and therefore these banks might respond less strongly, later or not at all. This leads us to hypothesize, thus, whether borrowers respond differently to corruption detection as follows.

> *Hypothesis 3.3 (Borrower risk and bank ownership).* After the detection of corruption, the contraction of credit is less pronounced to state-owned banks than it is to privately-owned institutions.

3.3. Empirical analysis

3.3.1 Data

The empirical analyses combine three different data sources. The main source of information comes from the Brazilian Public Credit Register (SCR - Credit Information System), a confidential loan level database owned and managed by the Central Bank of Brazil

(BACEN). It contains detailed information on all loans at the monthly bank-firm level: credit volume, amount of new loans in a specific month, interest rates of the contracts, loan loss provisions, maturity, and firms rating. Borrower-level characteristics[39], however, are very scarce. Therefore, to account for additional heterogeneity in firm characteristics, we use borrower fixed effects in order to control for time invariant unobserved characteristics of the firm.

We also use data from *Relação Anual de Informações Sociais* (RAIS), an administrative data set collected on an annual basis by the Brazilian Ministry of Labor, which covers all former workers in Brazil. The sample we use is restricted to those firms having more than 250 employees to account for the credibility of the comparison between treated (affected firms) and control group (non-affected firms). These firms, identified by their registration number (CNPJ), were merged into the SCR dataset.

Our third dataset has information on firms' connection to the government through donations in the federal elections. Campaign contributions were officially allowed in the Brazilian electoral system since 1997[40], enabling firms to make electoral donations limited to 2% of their gross annual revenues, imposing a linear limit on donations made by individuals up to 10% of the gross income earned by the donor in the year prior to the election, and not restricting limits on self-financing by candidates. The *Operação Lava Jato*, however, has given evidence that the firms' donations for political campaigns were used to fuel corruption schemes of great proportions, reaching politicians of the main Brazilian parties. For this reason, the Brazilian Supreme Court has decided, in September 2015, that corporate donations were unconstitutional from that year's municipal elections on[41].

39 Borrower-level information is restricted to firm's number of employees, wage bill, location, age, industry, firm rating, and whether they are publicly listed or not.
40 Law 9.504/1997, from September 30th 1997 – *Lei das Eleições*.
41 Law 13.165/2015, from September 29th 2015 – *Reforma Eleitoral*.

Historically, the firms that contributed more tended to belong to those sectors in which there were greater government intervention or influence, such as the finance institutions, the capital goods industry or the construction firms (Samuels, 2001; Claessens et al, 2008, Mancuso, 2012). The latter, our selected sector of analysis in this paper, was the main campaign donor in the elections of 2010, representing 24,6% of the total amount donated among the listed firms (Davi, 2016)[42]. These firms, which contributed with almost half of the donations received by the National Directorate during the electoral campaign of the incumbent party in 2010[43], donate in exchange of advantages from elected officials, usually being the ones benefiting from public procurement contracts of large public building constructions[44].

Given that, our dataset provided by the *Tribunal Superior Eleitoral* (TSE) contains the campaign contributions in the federal elections of 2010 and 2014, with detailed information about donors' contributions and recipients. For each candidate, beyond the identification of the parts involved in the contribution, it is possible to identify the political party, the state, position of the candidate (state deputy, federal deputy, senator, governor or president) and the size of the contribution in the specific year of the election. Firms are matched to the public credit register data according to their unique tax-identification numbers (*CNPJ*).

42 This percentage must be underestimated, given that many important firms in the construction sector are not listed in the stock exchange and, therefore, are not included in their analysis.

43 For a detailed media coverage of the donations made by construction firms in the elections of 2010, see https://www.correiobraziliense.com.br/app/noticia/politica/2010/11/09/interna_politica,222331/construtoras-doaram-quase-metade-do-dinheiro-para-campanha-do-pt-ao-planalt.shtml.

44 Using data from the federal elections of 2006, Boas, Hidalgo & Richardson (2014) identified the effect of an electoral victory on government contracts for a candidate's corporate donors: for each real donated, the contractor receives 8,5 times the amount in the form of works contracts chosen by the Workers' Party (*PT*) politicians and included in federal and state budgets, over 33 months after the elections.

The merged dataset comprises free-market credit[45] granted in the period from January 2013 to July 2016[46] of all firms with more than 250 employees and more than R$ 100 million of outstanding debt (to keep the size of the borrowers more homogeneous across banks). State owned banks are excluded from the initial analysis because they might have counter-cyclical behavior in periods of credit shrinkage in the economy (Capeleti, Garcia, Miessi, 2019)[47], as well as financial firms and state-owned firms were also dropped.

Our main identifier of treated units in our baseline regressions is a dummy variable that identifies the 21 construction firms included in the blacklist of the Lava Jato Operation. Our variables related to the bank-firm contracts are separated in two groups. The first refers to the amount of new loans[48]. The second includes loan term characteristics, such as the firms' rating, loan loss provisions, maturity, and pre-fixed interest rates. We aggregate the information at the firm-quarter level[49], but the second group of variables considers the quarterly information as the weighted average according to the representativeness of each loan contract. Firm specific characteristics, such as number of employees, age, and rating were fixed one year after our sample period starts, i.e., 2012.

45 Free market lending refers to the type of credit that does not meet public sector directions to contain demand in times of monetary contraction. On the contrary, earmarked credit includes the credit grant by BNDES and has subsidized interest rates for some sectors of the economy.

46 Our sample period ends before the second semester of 2016 because it was the period of the impeachment of the president Dilma Rousseff, introducing many economic uncertainties in the country.

47 They are included in an additional analysis by the end of this chapter.

48 For the missing new loans, we manually included zeros in case the firm had outstanding credit in that bank-quarter notch.

49 The original data structure is at the bank-firm-month, but variables are aggregated at the quarterly level.

3.3.2 Empirical strategy

We first divide the sample into two periods: one before the start of the investigations (2013) and one after (2014, 2015, and the first 2 quarters of 2016)[50], and then we collapse the data at the firm-quarter level. We blocked the first quarter of 2014 because the beginning of the anti-corruption investigations was in the middle of March 2014, so this quarter might induce noise to the specifications[51]. In order to test the first hypothesis, we estimate the following regression model:

$$\boldsymbol{C_{i,t} = \alpha + \beta_1 Lava\ Jato\ Firm_i + \beta_2 Post_t + \beta_3 (Lava\ Jato\ Firm_i \times Post_t) + \varepsilon_{i,t}} \quad (3.1)$$

In the abovementioned specification, $C_{i,t}$ measures characteristics of the relationship between bank-firms *i* at time *t*: loan volume, ratings, loan loss provisions, maturity and pre-fixed interest rate. *Lava Jato Firm* and *Post* are dummy variables that indicate firms included in the Lava Jato list and the post-period, respectively. Because the same borrower can have contracts with more than one lender, we can control for time-invariant changes in firm's characteristics by including firm fixed effects.

This is a key advantage of the setting: firms that are corrupt might differ from non-corrupt ones[52] due to unobserved characteristics and this might coincide with changes in other variables that impact the bank-firm relationship. As a result, corrupt firms can follow different trends than non-corrupt after the investigations for reasons that are not directly related to the corruption detection. By including firm fixed effects, we are able to control for these changes

50 As a robustness analysis of this project, we also used a symmetric time spam that considers 2 years before Lava Jato (2012-2013) and 2 years after Lava Jato (2014-2015) but results also remain stable.

51 This time frame (2013Q1-2016Q2, except 2014Q1) was considered in all regression specifications of this paper.

52 The firms in the control group are considered not corrupt because they are not included in the blacklist of *Operação Lava Jato*. Nevertheless, we acknowledge that there might be other detected or undetected corruption going on at these same firms.

at the firm-level, and pin down the effects of the corruption detection. We also control for time-invariant bank characteristics by including bank fixed effects, common macroeconomic shocks to firms that may change over time using time fixed effects and the standard error is clustered at the firm level.

Complementing the previous step of the analysis, we considered the heterogeneity of the results and analyzed whether the credit shrinkage is driven by observable firms' that might distinguish the bank-borrower worthiness. This might be important if it is the case that banks distinguish the level of riskiness of Lava Jato firms, even though the treated firms are all part of the same notch of corrupt companies. The sample period is the same used before and, again, the first quarter of 2014 was blocked since it includes the start of the anti-corruption investigations. We implement, thus, the following regression model:

$$C_{i,t} = \alpha + \beta_1 Lava\ Jato\ Firm_i + \beta_2 Post_t + \beta_3 Firm\ Characteristic_i + \beta_4 (Lava\ Jato\ Firm_i \times Post_t) + \ldots + \beta_7 (Lava\ Jato\ Firm_i \times Post_t \times Firm\ Characteristic_i) + \varepsilon_{i,t} \quad (3.2)$$

In the abovementioned specification, we focus on the volume of credit, therefore $C_{i,t}$ measures loan volume. *Lava Jato Firm* and *Post* are dummy variables that indicate firms included in the Lava Jato list and the post-period, respectively. *Firm characteristics* include information such as firm size, mean wage, age, number of bank relationships or volume of campaign contributions made in the federal elections of 2010. This specification also includes a vector of firm, bank, time fixed effects, and clusters the standard error at the firm level.

Then, in the last part of this chapter, we include state-owned banks and test whether they indeed have counter-cyclical behavior in periods of credit shrinkage in the economy. This is relevant for us to distinguish whether state owned banks present different incentives in granting

credit to corrupt firms, or also serve as a buffer to firms that face a considerable credit contraction from private institutions. The regression model is the following:

$$C_{i,t} = \alpha + \beta_1 Lava\ Jato\ Firm_i + \beta_2 Post_t + \beta_3 State\ Owned\ Bank_j + \beta_4 (Lava\ Jato\ Firm_i \times Post_t) + \ldots + \beta_7 (Lava\ Jato\ Firm_i \times Post_t \times State\ Owned\ Bank_j) + \varepsilon_{i,t} \quad (3.2)$$

In this regression specification, since we are interested in the total volume of credit, $C_{i,t}$ measures loan volume. *Lava Jato Firm* and *Post* are dummy variables that indicate either firms included in the Lava Jato list or the post-period, respectively. *State owned banks* is a dummy that stands for financial institutions controlled by the state. This specification also included a vector of firm, bank, time fixed effects, and clusters the standard errors at the firm level.

Following with our line of reasoning, if the credit volume to corrupt firms have shrunk, the anti-corruption investigations might have led to a regime change resulting in a bank buffer that might be used for reallocation and spillover effects in the Brazilian credit market to those firms that are likely not corrupt. Then, given the purpose of this chapter, the reallocation part of the story and the banks reaction to other non-affected firms will be addressed in the following chapter of this thesis.

3.4. Descriptive statistics

The whole sample is composed by 45,345 firm-month observations (highly unbalanced) referring to 109 banks and 1,966 different firms with more than 250 employees. However, after aggregating the information at the firm-quarter level and keeping in our sample only those 50

biggest banks[53], we remain with 20,118 firm-quarter observations referring to new loans. Table 3.1 presents descriptive statistics of the main variables used in our regression specifications, from 2013 to 2016[54]. For distinction issues, we also report statistics for each group separately. Appendix 3.1 contains details about variables definitions.

On average, the firms in our sample borrow R$20,541,316 per quarter in new loans. The treated firms, however, usually borrow slightly less (mean: R$17,981,070) than the control (mean: R$20,575,699), which is plausible given the credit contraction after the ongoing of the investigations that will be displayed in the next session of this chapter. Regarding loan-term characteristics, banks usually set aside 0.68% of their portfolio as provisions to account for future losses on loan defaults (median: 0.49%), albeit the provisions for treated firms are moderately bigger compared to control firms for the whole period 2013-2016.

Along this line, summary statistics of the variable maturity indicates that, on average, the loans are usually paid back in around 7 months, and this characteristic slightly diverge between treated and control units ($mean_{treated}$: 5.07, $mean_{control}$: 7.01, $median_{treated}$: 4.58, $median_{control}$: 6.87). This pattern might indicate two things: first, these short-term loans are usually used for financing working capital demands or other obligations on the short-run and not necessarily capital expenditures related to investment in real assets or other equipment that compose the fixed assets of their balance sheets. Second, given that the treated group is riskier than the control group, banks' credit to these firms also presents shorter maturity since these contracts are considered less risky than the long-term ones.

53 These banks represent 95.26% of the banks total assets in 2012 and have in their portfolio 97.72% of the total amount of new loans conceived for firms with more than 250 employees in the same time period.

54 As previously described, we blocked the first quarter of 2014 because the beginning of the investigations was in the middle of March 2014, so this quarter might induce noise to the specifications. Moreover, in our main specification, we only considered private banks. For this reason, in the summary table we do not consider the first quarter of 2014 and loans from state owned banks, and the sample has fewer observations: 17,809.

Table 3.1: Descriptive statistics

Notes. This table shows the summary statistics for the main variables used in the paper using the period 2013Q1-2016Q2 but excluding information regarding the quarter 2014Q1. We excluded contracts with provisions above 5%, as well as negative interest rates from our sample. All the variables related to loan amount characteristics were winsorized at the 5% and 95% levels due to the presence of outliers in the original distribution. Treated group refers to the 21 construction firms included in the *Lava Jato* list. Control group refers to the other firms not affected by the anti-corruption investigations.

	Panel A: All firms					
Variables	N	Mean	Std. Dev.	P5	Median	P95
Loan amount characteristics						
New loans $_{it}$	17,809	R$20,541,316	R$88,038,727	R$0	R$756,420	R$88,559,120
Loan terms characteristics						
Loan Loss Provision $_{it}$	10,803	.0068	.0084	0	.0049	.0299
Maturity $_{it}$	10,803	6.99	2.92	0	6.85	42.94
Firm rating $_{it}$	10,803	2.0570	.9427	1	2	4
Interest rate $_{it}$	7,976	23.64	26.61	2.02	15	100
Firm characteristics						
Volume of donations (R$)	1,676	.3263	.4690	0	5.631	1
Number of employees	1,676	3,400	12,863	250	1,330	10,714
Mean wage	1,676	R$3,208	R$2,163	R$1,174	R$2,536	R$7,784
Age	1,613	49.52	39.26	5.63	40.38	112.99
Number of bank-relationships	1,574	4.26	3.35	1	3	11

	Panel B: Lava Jato firms (Treated Group)					
Variables	N	Mean	Std. Dev.	P5	Median	P95
Loan amount characteristics						
New loans $_{it}$	236	R$17,981,070	R$38,298,630	R$0	R$0	R$106,706,744
Loan terms characteristics						
Loan Loss Provision $_{it}$	114	.0087	.0099	0	.005	.0299
Maturity $_{it}$	114	5.07	2.83	0	4.58	36.99
Firm rating $_{it}$	114	2.3387	1.0711	1	2.0135	4
Interest rate $_{it}$	78	35.81	34.39	10.02	17.41	100
Firm characteristics						
Volume of donations (R$)	21	.8095	.4023	0	1	1
Number of employees	21	6,243	8,391	1,547	3,994	17,531
Mean wage	21	R$3,728	R$850	R$2,616	R$3,598	R$5,097
Age	21	46.76	37.03	7.83	36.01	112.99
Number of bank-relationships	21	5.76	3.36	1	5	10

	Panel C: Non-Lava Jato firms (Control Group)					
Variables	N	Mean	Std. Dev.	P5	Median	P95
Loan amount characteristics						
New loans $_{it}$	17,573	R$20,575,699	R$88,516,717	R$0	R$782,716	R$88,379,848
Loan terms characteristics						
Loan Loss Provision $_{it}$	10,689	.0068	.0083	0	.0049	.0299
Maturity $_{it}$	10,689	7.01	2.92	0	6.87	43.00
Firm rating $_{it}$	10,689	2.0541	.9408	1	2	4
Interest rate $_{it}$	7,898	23.52	26.49	2.01	15	100
Firm characteristics						
Volume of donations (R$)	1,655	.3202	.4667	0	0	1
Number of employees	1,655	3,364	12,907	299	1,314	10,608
Mean wage	1,655	R$3,201	R$2,174	R$1,169	R$2,518	R$7,819
Age	1,592	49.56	39.30	5.63	40.47	112
Number of bank-relationships	1,553	4.24	3.35	1	3	11

Rating, the only categorical variable in the table, goes from 1 (best rating) to 4 (worst rating) and indicates that the firms usually have a moderate grade of 2, regardless the group we consider. The pre-fixed interest rate, however, clearly indicates that, on average, the treated firms (mean: 35.81%, median: 17.41%) are riskier than the control (mean: 23.52%, median: 15%) given that their interest rate is moderately higher through our sample period.

Even though the sample has a large number of observations, the number of observations from firms classified as connected is small: 619 firms have contributed to politicians/parties in the 2010's elections. Altogether, they donated, on average, an amount of R$0.32/employee (median: R$5.63/employee). Treated firms, however, considerably donate more ($mean_{treated}$: R$0.80/employee, $mean_{control}$: R$0.32/employee), which goes in line that they are more politically connected.

If we consider their number of employees in 2012, the treated firms are usually bigger (mean: 6,243, median: 3,994) than the control (mean: 3,364, median: 1,314). They also use to compensate more their employees, such that the treated firms pay higher salaries (mean: R$3.728/month, median: R$3.598/month) than the control (mean: R$3.201/month, median: R$2.518/month). Furthermore, they are more connected to the financial system than the non-affected firms.

Even though there is suggestive evidence that the corrupt firms and their bank-relationship might differ from the non-corrupt ones, it is important to verify how they behave with the ongoing of the operation and how do groups differ (if so) cross-sectionally over time. We address these issues in the next session of this paper.

3.5. Results

In this section, we provide results of our empirical analyses whether banks react to corruption detection at the firm level. Furthermore, we address the issue of heterogeneity among banks' response according to firm characteristics and according to bank ownership and other incentives concerning their counter-cyclical behavior.

3.5.1. Impact of Lava Jato on firms under investigation

In order to ascertain whether banks react to anti-corruption investigations widely publicized in the market and in the media during the period of the operation, we begin by analyzing whether there is a different behavior among banks to affected firms.

Table 3.2 presents the effect of corruption detection on the total volume of credit (Panel A) and in loan characteristics (Panel B). The unit of observation in all specifications is a bank-firm pair and we control for the unit-fixed effects at the firm-level and at the time-level. To account for any serial correlation across firms, we also cluster the standard errors at the firm level. The dependent variable of Panel A is the logarithm of new loans, which was calculated in order to standardize the variables in a way to make them more comparable across firms regardless the size of the new contract, while the dependent variables of Panel B are borrower rating, loan loss provision, loan maturity and loan interest rate.

Columns 1 and 2, Panel A, show the impact of corruption detection on new loans for the 9-quarters subsequent to the start of the investigations, i.e., comparing the post-investigation (2014Q2 - 2016Q2) with the pre-investigation (2013Q1 - 2013Q4) period. The DID coefficient for the interaction of treated firms after the beginning of the investigations is negative and statistically significant at the 1% level, meaning that corruption-detection significantly decreases the amount of new credit for firms under investigations. Similar results are found in

columns 3 and 4: on average, if we compare one firm caught in corruption with its best counterfactual, corrupt firms receive about 99% ($e^{-4.7}$ - 1 = -0.9909) less credit compared to their matched pair[55] after the start of the investigations. The economic magnitude of the coefficients is quite meaningful, since that the firms involved in corrupt schemes have a contraction of about R$17,801,259 (R$17,981,070 x 0.99 = 17,801,259) in the post period.

Table 3.2: The effect of anti-corruption investigations on credit

Notes. This table shows the regression results of the model $C_{i,t} = \alpha + \beta_1 Lava\ Jato_i + \beta_2 Post_t + \beta_3 (Lava\ Jato_i \times Post_t) + \varepsilon_{i,t}$ where *C* indicates the volume of new loans *Ln(1+New loans)* or other loan characteristics (*Rating, Loan loss provision, Maturity* and *Interest rate*) from firm i at time t. This analysis considers all firms with more than 250 employees and includes the 21 construction firms cited by the Lava Jato investigations. The sample period of this analysis starts in the first quarter of 2013 and go until the second quarter of 2016. *Lava Jato* is a dummy variable that equals one for the 21 construction firms that are subject to anti-corruption investigations and zero otherwise. *Post* is a dummy variable that switches to one in the period after 2014Q1; the first quarter of 2014, in which the Operation Lava Jato started, is omitted. The matching results reported in columns 3, and 4 consider the *nnmatch* algorithm proposed by Abadie et al. (2004) allowing the replacement of the selected units of comparison for either one corresponding match (1:1) (column 3) or four corresponding matches (1:4) (column 4). Robust standard errors (in parentheses) are clustered at the firm level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

Panel A: Loan volume

	(1)	(2)	(3)	(4)
Dep. Var.:		*ln(1+New Loans)*		
Lava Jato × Post	-5.331***	-5.277***	-4.700**	-4.827***
	(1.398)	(1.399)	(1.906)	(1.594)
Firm-FE	Yes	Yes	Yes	Yes
Bank-FE	Yes	Yes	Yes	Yes
Time-FE	Yes	Yes	Yes	Yes
Control group:				
All firms	Yes	No	Yes	Yes
All non-listed firms	No	Yes		
Matched sample (1:1)			Yes	No
Matched sample (1:4)			No	Yes
Number of observations	17,809	16,068	520	1,300
R^2	0.023	0.024	0.165	0.254

55 The matching algorithm considers firm-characteristics measured before our sample period starts, i.e., they were all measured in 2012. They include loans growth, outstanding credit growth, average loan loss provision, average maturity, average rating, number of employees, age, state of location, and the exact match for quarter of comparison.

Panel B: Loan characteristics

Dep. Var.:	(1) *Rating*	(2) *Loan loss provision*	(3) *ln(1+Maturity)*	(4) *Interest rate*
Lava Jato × Post	0.630***	0.00573***	0.0136	7.004
	(0.218)	(0.00200)	(0.276)	(8.388)
Firm-FE	Yes	Yes	Yes	Yes
Bank-FE	Yes	Yes	Yes	Yes
Time-FE	Yes	Yes	Yes	Yes
Control group:				
All firms	Yes	Yes	Yes	Yes
Number of observations	10,803	10,803	10,803	7,976
R^2	0.038	0.046	0.011	0.041

We repeat the analysis using loan term characteristics as dependent variables (Panel B), finding statistically significant estimates for half of the outcomes. On average, mean ratings provided by the banks to firms deteriorate by 63% (Column 1), meaning that the corrupt firms move from category B (mean value = 2.33) to C (mean value = 2.33 x (1 + 0.63) = 3.79) in the post period. Similarly, loan loss provisions significantly increase by 0.573% (Column 2). Although the sign of other coefficients corresponds to our hypotheses, which convey that the riskiness of the transactions increases in the post period, coefficients reported in columns 3 and 4 are not statistically significant. Therefore, we cannot derive further conclusions about their economic implications.

If we decompose the effect in the post period using dummies for each quarter (Table 3.3), we can see that the effect, in general, seems to appear close to the last quarter of 2014, the moment in which the construction firms started being cited by the *Operação Lava Jato*. Firms like Construtora OAS, Camargo Corrêa and Queiroz Galvão, all big players in this industry, and officially identified and deflagrated by the prosecutors, had been included in the *Lava Jato*

list in November 2014. Similar results are found in all the other columns, with the effect monotonically increasing over time. While the loan loss provision, on average, increases 0,573% for the detected firms in the post period (Table 3.2, Panel B, column 2), it increases in the magnitude of 1,36% (Model 3) 6 months after Odebrecht being cited in the investigations (2015Q4). Similar reasoning can be applied to all set of variables.

Table 3.3: The decomposed effect of anti-corruption investigations on credit

Notes. This table shows the regression results of the model $C_{i,t} = \alpha + \beta_1 Lava\ Jato_i + \beta_2 Post_t + \beta_3 (Lava\ Jato_i \times Post_t) + \varepsilon_{i,t}$ where *C* indicates the volume of new loans *Ln(1+New loans)* or other loan characteristics (*Rating*, *Loan loss provision*, *Maturity* and *Interest rate*) from firm i at time t. This analysis considers all firms with more than 250 employees and includes the 21 construction firms cited by the Lava Jato investigations. The sample period of this analysis starts in the first quarter of 2013 and go until the second quarter of 2016. *LJ* is a dummy variable that equals one for the 21 construction firms that are subject to anti-corruption investigations and zero otherwise. *Post* is a dummy variable that corresponds to the quarters after 2014Q1. Robust standard errors (in parentheses) are clustered at the firm level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

Dep. Var.:	(1) *ln(1+New Loans)*	(2) *Rating*	(3) *Loan loss provision*	(4) *ln(1+Maturity)*	(5) *Interest rate*
LJ × Post(2014Q2)	-1.760	0.287	0.00269	0.502*	6.151
	(1.740)	(0.276)	(0.00221)	(0.266)	(9.786)
LJ × Post(2014Q3)	-2.498	0.438	0.00129	-0.198	-15.01
	(1.712)	(0.344)	(0.00203)	(0.379)	(9.765)
LJ × Post(2014Q4)	-4.357**	0.894**	0.00480*	-0.451*	4.443
	(2.171)	(0.385)	(0.00280)	(0.255)	(8.192)
LJ × Post(2015Q1)	-3.230	0.678*	0.00714*	0.0111	4.731
	(2.499)	(0.391)	(0.00428)	(0.381)	(12.39)
LJ × Post(2015Q2)	-6.428***	1.066**	0.0136***	0.0219	20.26
	(2.213)	(0.422)	(0.00514)	(0.370)	(24.76)
LJ × Post(2015Q3)	-8.520***	0.929*	0.0123**	0.289	40.44**
	(2.294)	(0.512)	(0.00494)	(0.447)	(17.41)
LJ × Post(2015Q4)	-8.259***	0.901	0.0124**	-0.485	44.46*
	(1.942)	(0.555)	(0.00569)	(0.666)	(25.22)
LJ × Post(2016Q1)	-9.702***	-	-	-	-
	(1.311)	-	-	-	-
LJ × Post(2016Q2)	-6.266***	1.413***	0.0157***	-0.0866	-8.798***
	(1.752)	(0.259)	(0.00391)	(0.556)	(3.131)
Firm-FE	Yes	Yes	Yes	Yes	Yes
Bank-FE	Yes	Yes	Yes	Yes	Yes
Control group: *All firms*	Yes	Yes	Yes	Yes	Yes
N. of observations	17,809	10,803	10,803	10,803	7,976
R^2	0.446	0.490	0.443	0.486	0.437

It is important to note that coefficients regarding the first quarter of 2016 are omitted in columns 2-5 since there are no new loans for *Lava Jato* firms in this quarter. Thus, observation related to ratings, loan loss provisions, maturity and interest rate are necessarily missing. This pattern is also plotted in Figure 3.1.

Figure 3.1: Graphical analysis of anti-corruption investigations on credit

Notes. These figures display the volume (Panel A) and loan characteristics (Panel B) of new loans to Lava Jato firms (treated group) compared to the control group on the ongoing of the investigations. The vertical red dashed line indicates the first quarter of 2014, the period in which the investigations oficially started.

Panel A

Panel B

As depicted in the abovementioned graphs, the pattern of credit inflows for the control group (blue solid line) is almost stable over time. On the contrary, there is a clear contraction of credit for the treated firms (red dashed line) over time, attenuating around the beginning of the investigations (post-period). This pattern corroborates the regression results and indicates that not only credit shrinkage seems to increase as time passes by, but also loan terms deteriorate when firms are caught involved in corruption.

One might concern that treated firms are not necessarily comparable to control firms for two main reasons. First, albeit we can see a clear parallel trend between both groups, the magnitude of both groups differs before our shock. In general, the new loans granted to the treated group are consistently bigger than the control group. Second, whereas our treated group has only 21 firms, the control group has 1,655 firms. This might induce to distortions when comparing the results. Therefore, we implemented a synthetic control, in which we aggregated the loans at the industry level and then allowed the algorithm to build a control group based on pre-determined firm characteristics[56]. The selected industries according to the algorithm are reported in Appendix 3.2.

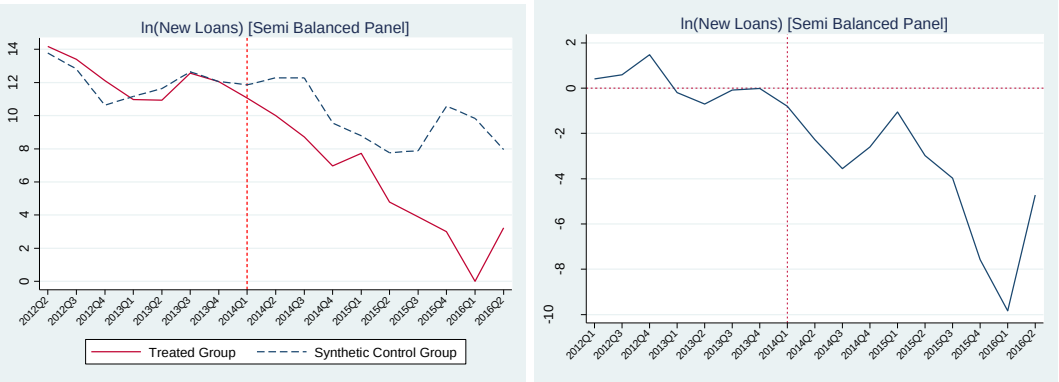
As depicted in Figure 3.2 (Panel A), we can see that both groups are quite similar before the event (which is the default of the algorithm), while there is a sizeable contraction of credit for the treated firms in the post period. Panel B shows a similar analysis, but instead considers the difference between both groups.

56 The characteristics used for the synthetic control algorithm are the same ones used in the matching procedure and include loans growth, outstanding credit growth, average loan loss provision, average maturity, average rating, number of employees, age, and state of location.

Figure 3.2: Synthetic Control of Lava Jato Firms

Notes. These figures display the volume of new loans to Lava Jato firms (treated group) compared to a synthetic control group based on other industries. Panel A considers the volume of credit [ln(1+New Loans)] and Panel B considers the difference between treated and synthetic control group.

Panel A **Panel B**



Overall, the results reported in this section suggest that the detection of corruption leads to an immediate and sizable reaction in the financial system, seeming to persist and increase over time. This gives room for our main story of the paper, which will be further investigated to understand how banks' response can be driven by firms' characteristics and banks ownership. These two complementary stories will be addressed in the following sections.

3.5.2. Heterogeneity of the results among firms under investigation

Given that there exists a clear contraction of credit for companies caught in corruption in the post period, it is important to ascertain whether there exists heterogeneity among banks' response according to firm characteristics. It might be the case that the overall effect for Lava Jato firms is negative, but those bigger firms, older, with more skilled workers or with more

bank relationships are less risky to the banking system and, therefore, less affected overall. We devote this section to investigate this issue.

Table 3.4 portrays that the overall volume of credit decreases for investigated firms in the post period (coefficient *Lava Jato x Post*), which is consistent with the previous part of the results. Additionally, the coefficient *Post x Moderator* conveys that the firms are slightly bigger in the post period, pay higher salaries, older, and have less bank relationships, albeit the magnitude of the coefficient is very small and lacks economic implications for firm size and mean wage.

Table 3.4: Heterogeneous effects of anti-corruption investigations on credit

Notes. This table shows the regression results of the model $Ln(1+New\ loans)_{i,t} = \alpha + ... + \beta_4 Lava\ Jato_i \times Post_t + \beta_5 Lava\ Jato_i \times Moderator_i + \beta_6 Post_t \times Moderator_i + \beta_7 (Lava\ Jato_i \times Post_t \times Moderator_i) + \varepsilon_{i,t}$ from firm i at time t. This analysis considers all firms with more than 250 employees and includes the 21 construction firms cited by the Lava Jato investigations. The sample period of this analysis starts in the first quarter of 2013 and go until the second quarter of 2016. *Lava Jato* is a dummy variable that equals one for the 21 construction firms that are subject to anti-corruption investigations and zero otherwise. *Post* is a dummy variable that switches to one in the period after 2014Q1; the first quarter of 2014, in which the Operation Lava Jato started, is omitted. The moderators are all measured in 2012, except the volume of donations that is related to the federal elections of 2010. Robust standard errors (in parentheses) are clustered at the firm level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

	(1)	(2)	(3)	(4)	(5)
Dep. Var.:	*ln(1+New Loans)*	*ln(1+New Loans)*	*ln(1+New Loans)*	*ln(1+New Loans)*	*ln(1+New Loans)*
Moderator	*Volume of donations*	*Number of employees*	*Mean wage*	*Age*	*More than one-bank relationship*
Lava Jato x Post x Moderator	0.00105**	0.00001	0.00522***	0.0345	-0.838***
	(0.000411)	(0.00007)	(0.00118)	(0.0406)	(0.250)
Lava Jato x Post	-7.678***	-5.344***	-24.79***	-6.750***	0.351
	(1.575)	(1.651)	(4.339)	(2.297)	(1.895)
Post x Moderator	-0.000215	0.000018**	0.000130**	0.0144***	-0.287***
	(0.000331)	(0.000004)	(0.000063)	(0.00337)	(0.0449)
Firm-FE	Yes	Yes	Yes	Yes	Yes
Bank-FE	Yes	Yes	Yes	Yes	Yes
Time-FE	Yes	Yes	Yes	Yes	Yes
Control group:					
All firms	Yes	Yes	Yes	Yes	Yes
Number of observations	17,763	17,763	17,763	17,545	17,277
R^2	0.443	0.443	0.444	0.434	0.446

Heterogeneous effects emerge when considering firm characteristics that may distinguish firms' credit riskiness. As presented in columns 1 and 3, although the overall credit to corrupt firms decreases in the post period, *Lava Jato* firms which are more connected to the government tend to receive an inflow of credit of 0.105% ($e^{0.00105} - 1 = 0.105\%$) in the post period, as well as those with more skilled workers. Nevertheless, contrary to the expectations that bank connections would be favorable to credit supply, Lava Jato firms with more bank relationships in 2012, on average, receive less credit in the post period.

Overall, our results suggest that banks still differentiate the credit worthiness of Lava Jato firms. Although our construction firms included in the treated group might be initially seeing as part of the same group caught in corruption and, therefore, presenting the same level of riskiness, banks' response is driven by pre-existing firm characteristics. Nonetheless, this result is consistently plausible from the hindsight of the risk-return approach and a new insight for the strand of research that related corruption and firm-level characteristics.

3.5.3. Heterogeneity of the results by bank ownership

This section of the paper analyzes whether the credit shrinkage highlighted in the first part of the results differ according to the ownership of the banks. We therefore included state-owned banks in the sample and investigate their response to Lava Jato firms. We also report results for both free market lending and earmarked lending separately, given that the latter is carried out with resources regulated by law or regulations imposed by the government.

Table 3.5 presents the effect of corruption detection on the total volume of free market credit (Panel A) and in earmarked credit (Panel B). The unit of observation in all specifications is a bank-firm pair and we control for the unit-fixed effects at the firm-level and at the time-level. To account for any serial correlation across firms, we also cluster the standard errors at the firm level. The dependent variable in both specifications is the logarithm of new loans,

which was calculated in order to standardize the variables in a way to make them more comparable across firms regardless the size of the new contract.

Table 3.5: The effect of anti-corruption investigations on credit according to bank ownership

Notes. This table shows the regression results of the model $ln(1+New\ Loans)_{i,t} = \alpha + \beta_1 Lava\ Jato_i + \beta_2 Post_t + \beta_3 State\ owned\ banks_j + \beta_4 (Lava\ Jato_i \times Post_t) + \beta_5 (Lava\ Jato_i \times State\ owned\ banks_j) + \beta_6 (Post_t \times State\ owned\ banks_j) + \beta_7 (Lava\ Jato_i \times Post_t \times State\ owned\ banks_j) + \varepsilon_{i,t}$. This analysis considers all firms with more than 250 employees and includes the 21 construction firms cited by the Lava Jato investigations. The sample period of this analysis starts in the first quarter of 2013 and go until the second quarter of 2016. *Lava Jato* is a dummy variable that equals one for the 21 construction firms that are subject to anti-corruption investigations and zero otherwise. *Post* is a dummy variable that switches to one in the period after 2014Q1; the first quarter of 2014, in which the Operation Lava Jato started, is omitted. *State owned banks* is a dummy that equals one for those financial institutions under government control. Robust standard errors (in parentheses) are clustered at the firm level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

Panel A: Free market lending

	(1)	(2)	(3)	(4)
Dep. Var.:		*ln(1+New Loans)*		
Lava Jato × Post	-5.961***	-6.020***	-5.019**	-4.708***
	(1.391)	(1.307)	(1.854)	(1.532)
Lava Jato x State owned banks	0.703	-0.322	-0.885	2.300
	(2.569)	(2.117)	(2.255)	(1.830)
Post x State owned banks	0.510*	0.217	1.385	3.484
	(0.295)	(0.278)	(3.668)	(3.445)
Lava Jato x Post x State owned banks	-0.837	0.287	-0.459	-3.084
	(3.267)	(2.471)	(4.052)	(3.713)
Firm-FE	Yes	Yes	Yes	Yes
Time-FE	Yes	Yes	Yes	Yes
Control group:				
All firms	Yes	No	Yes	Yes
All non-listed firms	No	Yes		
Matched sample (1:1)			Yes	No
Matched sample (1:4)			No	Yes
Number of observations	18,646	16,792	520	1,300
R^2	0.468	0.467	0.166	0.269

Panel B: Earmarked lending

Dep. Var.:	(1)	(2)	(3)	(4)
	ln(1+New Loans)			
Lava Jato × Post	0.0393	-0.214	0.714	-0.323
	(1.025)	(1.052)	(1.684)	(1.707)
Lava Jato x State owned banks	1.622	-1.361*	1.070	0.132
	(1.906)	(0.779)	(2.998)	(2.045)
Post x State owned banks	1.016***	0.760***	-5.331	-6.022
	(0.227)	(0.247)	(5.118)	(3.915)
Lava Jato x Post x State owned banks	-0.654	1.739*	2.732	4.569
	(2.139)	(1.050)	(3.669)	(2.987)
Firm-FE	Yes	Yes	Yes	Yes
Time-FE	Yes	Yes	Yes	Yes
Control group:				
All firms	Yes	No	Yes	Yes
All non-listed firms	No	Yes		
Matched sample (1:1)			Yes	No
Matched sample (1:4)			No	Yes
Number of observations	15,355	13,793	182	455
R^2	0.556	0.556	0.211	0.227

Consistent with previous results reported in section 5.1, in general, Lava Jato firms receive less free market credit from all banks in the post period. The coefficient *Lava Jato x Post* is robust regardless the control group we use. The DID coefficient for the triple-interaction reported in Panel A, our main coefficient of interest, demonstrates by the sign of the coefficient that state owned banks, on average, decrease the supply of free market lending for firms under investigation in the post period compared to private-owned banks after corruption is detected. However, one cannot derive further interpretations given the non-statistical significance in none of the regression specifications.

Furthermore, state owned banks usually lend more in the post period as highlighted by the positive and significant coefficient of the double interaction *Post x State owned banks* in column 1, which might indicate that these banks have counter-cyclical behavior and act as a

buffer in periods of downturn in credit markets. However, this effect is not robust to different control groups, and we cannot ascertain further recommendations based on this finding.

Panel B shows slightly different results given the purpose of the credit analyzed in this sub-section. When it comes to earmarked lending, on average, state owned banks increase lending for firms under investigation in the post period. Result is significant at the 10% level only when considering the control group of other non-listed firms, though (Model 2). By the same column, we can see that, on average, Lava Jato firms have less credit from state owned banks (coefficient *Lava Jato x State owned banks*) and that state owned banks go in the opposite direction of the market, offering more credit in the post period.

In essence, results based on the analysis including state owned banks indicate that their behavior towards corrupt firms is not necessarily countercyclical when compared to private ones. It could be the case that, given the strong credit contraction of credit from private banks to Lava Jato firms, these corrupt firms would substitute their lenders to continue their operations. Results on both types of credit do not support this interpretation in a robust manner, though.

3.6. Conclusion

We investigate the impact of unprecedented anti-corruption investigations on corporate credit in Brazil. Using a dataset provided by the Central Bank that matches quarterly firm-bank new loans with bank characteristics and firm variables, we conduct a difference-in-differences analysis of the impact of unexpected and exogenous anti-corruption investigations on credit to a set of major Brazilian firms included in the blacklist of the Lava Jato Operation during January 2013 to July 2016.

We find consistent evidence that there is a considerable contraction of credit to corrupt firms, in a sense that affected firms receive less credit, lower credit ratings and display higher loan loss provisions after the investigations have started. Furthermore, we find heterogenous results among the contraction of credit, and show that the effect is less pronounced for firms more connected to the government or with more skilled workers, while being more pronounced for firms with more bank relationships. We also differentiate between private and state-owned banks, but results do not indicate any clear and robust pattern when considering free market and earmarked lending.

Our findings suggest that corruption, once detected, limits the ability of firms to raise credit and act as a barrier to entry for new sound projects. This banks' response is as expected and in line with the goal of the anti-corruption investigations. Given the sizable decrease of credit for Lava Jato firms documented in this paper, there is a surplus that banks might use for other incentives other than increasing their liquidity levels. In this sense, the anti-corruption investigations might induce a better allocation of credit, moving the country from a regime with high corruption and more room for misallocation to a regime with less corruption and less room for misallocation. This is the focus of next chapter of this thesis.

4. ANTI-CORRUPTION INVESTIGATIONS, CREDIT REALLOCATION AND REAL EFFECTS[57]

57 This paper is based on an on-going research project by M. Campello (Cornell University), C. Ferraz (PUC-Rio & University of British Columbia), L. Moura (EBAPE-FGV), L. Norden (EBAPE-FGV) and R. Schechtman (Banco Central do Brasil).

4.1. Introduction

Corruption persists as a historical and challenging puzzle that interferes the economic activity in several dimensions. Despite local and international efforts undertaken by national governments and oversight bodies, including Central Banks, OECD interventions and International Monetary Fund initiatives, it results in poverty, greed, unemployment, misallocation of financial resources, lower investment levels, losses in tax revenues, and burden on the quality of infrastructure and public services by distorting the composition of government expenditure. Most of the studies on the topic, however, have devoted their attention mainly to corruption in governments (Ferraz, Finan, 2008; Moreira, 2009; Ferraz, Finan, Szerman, 2016; Avis, Ferraz, Finan, Varjão, 2019; Di Tella, Schargrodsky, 2003; Reinikka, Svensson, 2004; Olken, 2006), yet corporate corruption remains an unsolved challenge that interferes the proper functioning of competitive markets.

A 2017 survey of businesses by the European Commission[58] found that 60 percent of its respondents agreed with the statement that bribery and the use of government connections is often the easiest way to obtain certain public services, and 42 percent of the enterprises think that the only way to succeed in business is by way of political connections. This statement seems not to be different among countries: in underdeveloped countries such as Brazil, most of the donations received by the Federal Government during the electoral campaigns are in exchange of advantages in public procurement contracts from elected officials (Samuels, 2001; Claessens et al, 2008, Mancuso, 2012; Davi, 2016). It makes room for several illicit contracts, bribery, over invoicing of public expenditures and, as a response, leads control bodies to react with audits and anti-corruption investigations, which is the case of the *Operação Lava Jato* (Car Wash Operation).

58 Source: Businesses' attitudes towards corruption in the EU: Flash Eurobarometer 457 (2017), available at http://www.stopcorrupt.lu/wp-content/uploads/2018/02/fl_457_sum_en.pdf.

Therefore, in this paper we investigate whether and how major anti-corruption investigations concerning a set of big firms in the Brazilian economy (*Operação Lava Jato*, 2014-2021) spilled over to other non-affected firms through credit market networks and how it indirectly impacts in the labor market. From the point of view of banks, we acknowledge that the main channel that drives their response is related to financial corporate considerations, such as incentives to risk shifting (Lava Jato firms become riskier after the start of the anti-corruption investigations), market discipline pressures, charter values (Jensen and Meckling 1976; Keeley 1990; Merton, 1977), but mainly, "stigma" that discredits the firms included in the blacklist of Lava Jato. At the same time, firms' heterogeneity might be taken into consideration when accessing their worthiness, and characteristics such as their overall credit risk and pre-existence in the banking system might also play an important role.

There are two main obstacles that researchers face when empirically investigating the bank lending channel involving corrupt firms. First, corruption by its inherent secret nature is difficult to measure and endogenously chosen by firms, making it difficult to causally identify its adverse effects. Second, the mechanisms through which corruption spillovers to other firms are still challenging to measure and hard to advocate. Therefore, we rely on unanticipated anti-corruption investigations against firms to circumvent this challenge. Our setting allows to directly address the issue of endogeneity since the start of the investigations and detection of firms' corruption were exogenously motivated by oversight bodies and could not be enacted by firms. Furthermore, we rely on spillover effects for non-affected firms and explore the variation in the supply of credit from more exposed banks according to their portfolio of credit to corrupt firms before the investigations have started to guarantee exogeneity of our findings. Exploiting this variation can lead us to trace the causal impact of corruption detection on bank lending markets, as well as banks' motivations inherent to this channel.

Using a newly assembled dataset, we combined data from four different sources. The main source of information is a private dataset owned and managed by the Central Bank of Brazil (SCR – Credit Information System), containing detailed information on all loans at the monthly bank-firm level. The second comes from RAIS (Relação Annual de Informações Sociais), an administrative data set collected on an annual basis by the Brazilian Ministry of Labor, which covers all former workers in Brazil. The third one comes from TSE (Tribunal Superior Eleitoral), with detailed information on firms' connection to the government through donations in federal elections. The fourth was publicly collected from IBGE (Instituto Brasileiro de Geografia Estatística) and it contains detailed macroeconomic characteristics at the yearly state-level. Our final sample covers around 25,000 firm-bank-quarter observations referring to new loans to 1,728 different firms with more than 250 employees in the years between 2013 and 2016.

Overall, our results indicate that the anti-corruption investigation led to spillovers through credit market channels, leading to significant adverse effects on the credit flow to non-affected firms. First, non-affected firms that were existing borrowers receive less credit from banks who exhibit a relatively high ex ante credit exposure to affected firms. This decrease is smaller for existing borrowers who have multiple bank relationships, coming from economically stronger states, whilst these firms are also more likely to obtain longer maturity loans and experience credit rating upgrades. Second, new borrowers that are politically connected and from the construction sector are more likely to obtain credit from exposed banks, as well as those who are younger and from economically weaker states. Third, given the overall contraction of credit to existing borrowers, non-affected firms with a higher indirect exposure to affected banks reduce employment and wages in the post period. Our findings are robust to different selection criteria for the treated group and to different event windows surrounding the event.

Despite the extensive literature on corruption used as background for our paper (Becker, 1968; Rose-Ackerman, 1978; Shleifer, Vishny, 1993; Mauro, 1995; Svensson, 2005; Zeume, 2017; Fisman et al., 2021), there is still no evidence on the consequences of anti-corruption investigations against firms, mainly when it comes to firm-level analyses and real effect investigations. There are several studies that analyze other anticorruption programs, mostly relying on the audits reports promoted by the *Controladoria Geral da União* (CGU) that took place after 2003 (Ferraz, Finan, 2008; Moreira, 2009; Ferraz, Finan, Szerman, 2016; Avis, Ferraz, Finan, Varjão, 2019; Lagaras, Ponticelli, Tsoutsoura, 2017; Colonnelli, Prem, 2020). Others rely on indicators of health sector's corruption derived from internal control systems in Buenos Aires (Di Tella, Schargrodsky, 2003); accompany transfers of resources from source to destination in educational programs in Uganda (Reinikka, Svensson, 2004); or evaluate a redistributive program implemented in Indonesia (Olken, 2006).

Although all these studies indicate the enormous costs of public expenditures corroded by corruption (Jensen, Meckling, 1976; Shleifer, Vishny, 1993; Di Tella, Schargrodsky, 2003; Reinikka, Svensson, 2004; Olken, 2006; Zeume, 2017; Avis, Ferraz, Finan, Varjão, 2019) and provide evidence on the aggregate benefits of higher monitoring, allowed with an enforcement system which may deter corruptive practices (Jain, 2001), none of these studies have analyzed whether these negative shocks related to corruption spillover to non-affected firms in the market through the credit market networks, potentially affecting the level and allocative efficiency of economic activity. This paper aims at filling this gap.

With that in mind, our paper relates to three main strands of literature. First, the classic economic literature provides evidence that corruption negatively influences economic growth and development (Shleifer and Vishny, 1993; Mauro, 1995; Svensson, 2005; Robinson et al., 2005). Many scholars have previously highlighted that corruption is not "greasing the wheels" of bureaucracy, focusing either on its macro-economic consequences (Kaufmann and Wei,

1999; Hall and Jones, 1999; Fisman and Svensson, 2007; Olken and Barron, 2009), or in the micro-economic impact (Lui, 1985; Wei, 2000; Bologna et al., 2015; Zeume, 2017; Giannetti et al., 2020; Lagaras et al., 2017) of anti-corruption investigation programs. On the opposite side, however, there are others that argue that corruption does "grease the wheels" of bureaucracy (Leff, 1964; Leys, 1965; Huntington, 1968; De Rosa et al., 2015), being beneficial in a second-best world due to the distortions caused by ill-functioning institutions[59]. Along this line, a more recent cross-country analysis covering 141 economies (Fisman et. al, 2021), show that corruption in general hinder firm growth but indeed can benefit firms in corrupt environments. In this paper we take a step further at resolving this argument, empirically establishing that corruption indeed hampers economic activity, teasing out undesired spillover effects that emerge from this discussion.

Other recent studies have advanced our knowledge on the causes and consequences of organized crime and corruption in Italy, a country that has been historically plagued by these phenomena. Calamunci and Drago (2020), using longitudinal firm-level data, estimated the spillover impacts of criminally connected firms on other (legal) firms working within the same market and showed a critical source of allocative inefficiency enforced by organized crime on legal economic activities. Similarly, De Angelis et al. (2020), using a detailed and private dataset on corruption cases across Italian municipalities, show that financial funds got from the EU increased in 4% the number of white-collar crimes.

59 Colonnelli and Prem (2020) provide a novel causal evidence against the "grease the wheels" argument. Using a detailed matched dataset, the authors show that anti-corruption investigations in Brazil promoted by the *Controladoria Geral da União* (CGU) lead to higher levels of economic activity, as observed by the significant increase in the number of firms and business establishments operating in treated municipalities randomly audited from 2003 to 2014. Furthermore, they also document an increase in total sales by local firms, an increase in the total volume of lending and deposits in local banks, and an effect primarily focused on those economic sectors that most rely on government relationships.

Second, we also contribute to a new and growing literature that examines the detrimental impact of corrupt environments and bank lending activity (Beck, Demirgüç-Kunt, Levine, 2006; Charumilind, Kali, Wiwattanakantang, 2006; Barth, Lin, Lin, Song, 2009; Weill, 2011; Park, 2012; Qi, Ongena, 2019). An initial attempt to better understand the relationship between corruption detection and external finance was provided by Beck et al. (2006). In a cross-country analysis with more than 2,500 firms across 37 countries, they found that countries with stronger supervisory agencies[60] tend to have firms that face greater obstacles to obtaining bank loans because of corrupt bank officials than firms in countries where the supervisory agency is less powerful. They also claim that banks do not only allocate capital based on risk-return criteria, rather, when supervisory agencies have the power to influence the distribution of bank loans, then corruption and political ties may shape the allocation of bank credit.

Similar issues are also examined in other developing economies[61] (for a detailed revision of corruption in developing countries, see Wraith and Simpkins, 2010). Weill (2011) analyzes the impact of corruption on bank lending in Russia to understand the causes of financial underdevelopment and finds that corruption hampers bank lending, and the risk-averse banks are the ones lending less. Similarly, Charumilind, Kali, Wiwattanakantang (2006) investigate the lending activity to business connected firms in the presence of corruption in Thailand and find that firms with connections to banks and politicians had greater access to

60 Countries where supervisory agencies can intervene banks, replace managers, force provisioning, stop dividends etc.

61 For a cross-country analysis that covers firms across 22 transition countries from Europe and address the impact of bribery on credit access from 2007 to 2014, see Qi and Ongena (2019). The firm-level variables were measured through a survey conducted jointly by the European Bank for Reconstruction and Development (EBRD) and the World Bank. Their results show that firms involved in bribery practices have more limited access to bank credit, this impact is mainly driven by supply-side factors, and the effect is less pronounced in localities where there are more foreign banks since they lack the knowledge to distinguish corrupt from not corrupt firms. Partially through the tightening of firms' credit access, bribery hinders firm growth.

long-term debt than firms without such ties[62]. Many other recent studies delve into anti-corruption investigations around the world with special focus on credit markets and credit reallocation (Ding et. al, 2018; Chen and Kung, 2019; Griffin, Liu, Shu, 2018).

Finally, we also relate to the empirical literature examining capital reallocation. Li, Wang and Zhou (2018) make use of the Chinese anticorruption campaign initiated in 2012 by the Communist Party to gauge how the investigations are associated with credit reallocation from state-owned enterprises (SOEs) to non-state-owned enterprises. Contrasting the stylized fact that SOEs may receive preferential treatment in bank lending, they find that there is a reallocation of credit from state-owned enterprises to privately owned enterprises since anti-corruption investigation forces the bank to be more merit based (since non-SOEs experience significant increases in productivity after the staggered investigations). Similarly, Giannetti, Liao, You and Yu (2020) showed that the same anti-corruption campaign leads to better firm performance due to better capital allocation and more efficient labor market: firms face higher growth of sales, lower cost of debt and larger productivity gains. Furthermore, small and young firms benefit more from the lower levels of corruption.

We note that there is also evidence that banks react to partial shocks to their credit portfolios, by reallocating credit away from the affected borrowers and towards (attractive) non-affected borrowers. For example, Bustos, Garber and Ponticelli (2019) use a detailed data on deposit and lending activity at the branch level to access credit reallocation from rural areas of Brazil. Their main results show that, instead of the typical effect that larger agricultural productivity increases the demand for capital in agriculture, rural municipalities increase

62 In a similar setting, Khwaja and Mian (2005) show that politically connected firms – those in which managers are involved in politics and with elections - receive considerable preferential treatment in bank funding using data from 90,000 Pakistani firms over the period 1996-2002. Analogously, Claessens et al. (2008) find that Brazilian firms connected to the government – those that contributed to the presidential campaigns in 1998 and 2002 – received more bank funding than other firms.

savings deposits in local bank branches. Therefore, there is a positive supply effect of agricultural productivity on industrialization, capital outflows from rural areas towards urban regions, and it is then invested in the industrial and service sectors. This effect leads to an increase in employment rates and in the wage bill of those firms that received more credit. In a similar way yet using a different set, this chapter examines whether such reallocation of bank credit occurred after the start of the Lava Jato operation.

This paper proceeds as follows. The next section provides a brief overview of the characteristics of the *Operação Lava Jato*, the Brazilian institutional context and develop our main hypotheses. Section 3 describes the empirical setting and the different databases we assembled for our analyses. Section 4 shows the descriptive statistics, section 5 presents the results and section 6 reports findings of further checks. Section 7 concludes.

4.2. Institutional background and hypotheses

4.2.1. Institutional background

The Lava Jato Operation, one of the largest initiatives to combat corruption and money laundering in Brazil's recent history, began on March 17, 2014. In the first phases of the anti-corruption investigation, a network of people responsible for the movement of public resources was unleashed, acting in several regions of Brazil through shell companies, accounts in tax havens and fictitious import contracts. Among the initial prisoners were Paulo Roberto Costa (former Petrobras director) and Alberto Youssef (an entrepreneur appointed as the money changer responsible for laundering millions of reais), who signed an award-winning whistleblower agreement, contributing to the investigations in exchange for benefits.

According to the *Ministério Público Federal*, the schemes worked by charging bribes to facilitate negotiations between the contractors and Petrobras and the acquisition of bids for

the construction of large public works. The contracts were overpriced, allocating part of the public money to feed the members of the scheme. The bribe was passed on to money changers and lobbyists who paid politicians and civil servants, given that the directors' nominations had, in essence, political-party connections.

With the ongoing of the investigations, a large corruption scheme was discovered involving Petrobras (the largest public company in the country), several politicians in the country, the largest Brazilian construction firms (Odebrecht, Andrade Gutierrez, OAS, Camargo Correia, Queiroz Galvão, Galvão Engenharia, Mendes Júnior, Engevix and UTC) and several companies in other fields (gas stations and car wash chains, hotel chains etc.). The scheme also involved large public infrastructure works, such as the construction of the Angra 3 Nuclear Power Plant and the works carried out for the World Cup (Maracanã reform).

Altogether, more than 1,434 procedures were instituted, with 775 searches and seizures, 210 coercive conducts, 95 preventive arrests, 10 award winning agreements, billions of reais returned to public coffers, and it is estimated that the damage to the nation through these schemes of corruption reach the amount of 42 billion reais. Due to the size of the discovered scheme and the consequences of the investigation for the Brazilian economy and politics, Operation Lava Jato has been considered by many experts as the largest anti-corruption operation in Brazil. The work grew and, due to developments, new investigations were launched in several states during its seven years of operation (March 2014 – February 2021).

The operation had a successful start and worked very efficiently until 2016, then its investigations gradually slowed down, and it has proven to be closer and closer to politics. In 2019, for example, The Intercept Brazil portal disclosed conversations between the then judge Sergio Moro (judge in charge of the anti-corruption investigations) and prosecutors, which indeed questioned the impartiality of the investigations. Then, *Lava Jato* lost its luster and the *Procuradoria-Geral da República* (Attorney General's Office) announced the dissolution of its

original nucleus in the beginning of 2021. It is currently conducted by GAECO (*Grupo de Atuação Especial de Combate ao Crime Organizado*), which is a group part of *Minitério Público Federal* and the operation, as initially conceived, came to an end after around 7 years in effect.

4.2.2. Hypotheses

Provided that the unprecedented anti-corruption investigations in Brazil represent a regime change that likely results in a reallocation of credit in the economy, we investigate spillover effects and reallocations in the credit market to comparable firms. Given the sizable decrease of credit for Lava Jato firms (affected firms) mentioned in the previous chapter of this thesis, there is a surplus that might be reallocated to non-affected firms. It might be the case that the non-corrupt firms receive more credit from those banks that were previously more exposed to corruption, with heterogeneous incentives according to pre-existing relationships (intensive margin) and new bank-firm relationships (extensive margin). We thus hypothesize the following:

> *Hypothesis 4.1 (Credit reallocation at the firm level).* Non-affected firms receive more credit from banks with greater ex ante direct exposure to Lava Jato firms (H1a). The characteristics that drive the reallocation at the intensive margin diverge from the extensive margin (H1b).

We then extended the previous analysis and inspect whether the flow of credit from a lender depends on borrowers' characteristics. We hypothesize, thus, that the credit inflow to firms that are likely not corrupt is concomitantly accompanied by a negative spillover to those more prone to be. This argument has its foundations in the literature that examines corruption in the lending process (Beck, Demirgüc-Kunt, Levine, 2006; Fungácová, Kochanova, Weill,

2015), which states that it can contribute to a reduction in firms' bank debt due to the increasing cost of the loan to the borrower.

In this sense, firms from the construction sector or with connections to the government might proxy for fraudulent borrowers[63,64]. This hypothesis extends H1 by combining the identification of affected banks with characteristics of the borrowers. We also included other firm- and state-characteristics that may lead banks to have heterogeneous effects in the reallocation.

> *Hypothesis 4.2 (Heterogeneous effects of credit reallocation).* Non-affected firms from the construction sector (H2a) or more connected to the government (H2b) receive less credit from banks with greater ex ante direct exposure to Lava Jato firms. Additionally, smaller firms (H2c), with less skilled workers (H2d), younger (H2e), or with just one bank relationship in the pre-period (H2f) also receive less credit from more exposed banks.
>
> *Hypothesis 4.3 (Redistribution of credit).* Non-affected firms located in less developed states receive more credit from banks with greater ex-ante direct exposure to Lava Jato.

Considering that there exists a credit reallocation in the economy and that corruption at the firm level is partially deterred due to the anti-corruption investigations promoted by the *Ministério Público Federal* together with the Federal Police, it is reasonable to assume that loan term characteristics of non-affected firms might become less risky in the post period. This hypothesis follows the idea that more exposed banks reallocate based on the attractiveness of lending opportunities.

63 Albeit similar to those firms included in the blacklist of Lava Jato, corruption has not been detected for these borrowers yet.

64 It can also be the case that political connections may help firms to gain comparative advantages, enhancing firm performance, value and receiving preferential treatment (Fisman, 2001; Khwaja, Mian, 2005; Charumilind, Kali, Witwattanakantang, 2006; Goldman, Rocholl, So, 2009).

Hypothesis 4.4 (Borrower risk of non-affected firms). Non-affected firms have better loan term characteristics from banks with greater ex ante direct exposure to Lava Jato firms. More specifically, they receive best ratings (H4a), shorter loan loss provisions (H4b), bigger maturity (H4c) and smaller interest rates (H4d).

Finally, it's well stablished that corruption impedes firm growth by limiting their access to bank credit (Fisman and Svensson, 2007; Qi and Ongena, 2019). Our context, nevertheless, goes in the other direction: corruption is partially deterred with the ongoing of the anti-corruption investigations and, consequently, there's a subsequent inflow of credit to non-affected firms. This spillover effect might foster firm growth by increasing their employment rates and wages in the post period, which leads us to ascertain the following:

Hypothesis 4.5 (Real effects). Non-affected firms with a higher exposure to affected banks increase employment rates and wages in the post period.

4.3. Empirical analysis

4.3.1. Data

The empirical analyses combine four different data sources. The main source of information comes from the Brazilian Public Credit Register (SCR - Credit Information System), a confidential loan level database owned and managed by the Central Bank of Brazil (BACEN). It contains detailed information on all loans at the monthly bank-firm level: credit volume, amount of new loans in a specific month, interest rates of the contracts, loan loss provisions, maturity, and firms rating.

Borrower-level characteristics[65], however, are very scarce, so that we cannot account for their creditworthiness. Therefore, to account for heterogeneity in firm characteristics, we

65 Borrower-level information is restricted to firm's identity, location, age, sector and firm's rating.

use borrower (alternatively, borrower-bank) fixed effects in order to control for time invariant unobserved characteristics of the firm. Consequently, in the case of borrower-bank fixed effects estimations, we rely only on those firms that have taken out at least two loans from the same bank in that specific period.

We also use data from *Relação Anual de Informações Sociais* (RAIS), an administrative data set collected on an annual basis by the Brazilian Ministry of Labor, which covers all former workers in Brazil. The sample we use is restricted to those non-affected firms, i.e., not included in the blacklist of Lava Jato, and having more than 250 employees. These firms, identified by their registration number (CNPJ), were merged into the SCR dataset.

Our third dataset has information on firms' connection to the government through donations in the federal elections. Campaign contributions were officially allowed in the Brazilian electoral system since 1997[66], enabling firms to make electoral donations limited to 2% of their gross annual revenues, imposing a linear limit on donations made by individuals up to 10% of the gross income earned by the donor in the year prior to the election, and not restricting limits on self-financing by candidates. The *Operação Lava Jato*, however, has given evidence that the firms' donations for political campaigns were used to fuel corruption schemes of great proportions, reaching politicians of the main Brazilian parties. For this reason, the Brazilian Supreme Court has decided, in September 2015, that corporate donations were unconstitutional from that year's municipal elections on[67].

Historically, the firms that contributed more tended to belong to those sectors in which there were greater government intervention or influence, such as the finance institutions, the capital goods industry or the construction firms (Samuels, 2001; Claessens et al, 2008, Mancuso, 2012). The latter, our selected sector of analysis in this paper, was the main campaign

66 Law 9.504/1997, from September 30th 1997 – *Lei das Eleições*.
67 Law 13.165/2015, from September 29th 2015 – *Reforma Eleitoral*.

donor in the elections of 2010, representing 24,6% of the total amount donated among the listed firms (Davi, 2016)[68]. These firms, which contributed with almost half of the donations received by the National Directorate during the electoral campaign of the incumbent party in 2010[69], donate in exchange of advantages from elected officials, usually being the ones benefiting from public procurement contracts of large public building constructions[70].

Given that, our dataset provided by the *Tribunal Superior Eleitoral* (TSE) contains the campaign contributions in the federal elections of 2010 and 2014, with detailed information about donors' contributions and recipients. For each candidate, beyond the identification of the parts involved in the contribution, it is possible to identify the political party, the state, position of the candidate (state deputy, federal deputy, senator, governor or president) and the size of the contribution in the specific year of the election. Firms are matched to the public credit register data according to their unique tax-identification numbers (*CNPJ*).

Finally, our fourth dataset was collected from IBGE (*Instituto Brasileiro de Geografia Estatística*) and has detailed macroeconomic characteristics at the yearly state-level during the years 2012 and 2016. Basically, we have information such as sales volume, nominal revenue, industrial production, unemployment rates and human development index. These characteristics were matched to each firm according to their headquarters' region of location.

68 This percentage must be underestimated, given that many important firms in the construction sector are not listed in the stock exchange and, therefore, are not included in their analysis.

69 For a detailed media coverage of the donations made by construction firms in the elections of 2010, see https://www.correiobraziliense.com.br/app/noticia/politica/2010/11/09/interna_politica,222331/construtoras-doaram-quase-metade-do-dinheiro-para-campanha-do-pt-ao-planalt.shtml.

70 Using data from the federal elections of 2006, Boas, Hidalgo & Richardson (2014) identified the effect of an electoral victory on government contracts for a candidate's corporate donors: for each real donated, the contractor receives 8,5 times the amount in the form of works contracts chosen by the Workers' Party (*PT*) politicians and included in federal and state budgets, over 33 months after the elections.

The merged dataset comprises free-market credit[71] granted in the period from January 2013 to July 2016[72] of all firms with more than 250 employees and more than R$ 100 million of outstanding debt (to keep the size of the borrowers more homogeneous across banks). State owned banks are excluded from the initial analysis because they might have counter-cyclical behavior in periods of credit shrinkage in the economy (Capeleti, Garcia, Miessi, 2019)[73], as well as financial firms and SOEs were also dropped.

For the reallocation of credit addressed in this paper, our main identifier of the treated group is a variable at the bank level that indicates those banks with a higher concentration of total outstanding credit to Lava Jato firms in their portfolio in 2012. The banks in the upper tercile are considered more exposed for corruption in financial markets because they have more concentration on corrupt borrowers on their credit portfolio[74]. Therefore, they are more prone to reallocate credit after the ongoing of the investigations. Another important aspect of the methodology is that the corrupt firms (*Lava Jato* list) serve only to create the bank exposure measure in the pre-period, which means that they are excluded from these analyses.

We used data before our sample period starts to ensure exogeneity of the variable of exposure. It is important that our main argument relies in the fact that banks did not know (ex-ante) that the construction firms caught in corruption were, indeed, corrupt before the outbreak of the investigations. Firm specific characteristics that capture heterogeneous effects of the reallocation, such as number of employees, mean wage, age, and number of bank-relationships

71 Free market lending refers to the type of credit that does not meet public sector directions to contain demand in times of monetary contraction. On the contrary, earmarked credit includes the credit grant by BNDES and has subsidized interest rates for some sectors of the economy.

72 Our sample period ends before the second semester of 2016 because it was the year of the impeachment of the president Dilma Rousseff, introducing many economic uncertainties in the country.

73 They are included in an additional analysis by the end of this chapter.

74 Our metric of corruption considers those firms caught in corruption only if they are cited in the investigations promoted by *Operação Lava Jato*. Nevertheless, we acknowledge that there might be other detected or undetected corruption going on at other non-affected firms and our metric of bank exposure does not fully captures that.

were also considered by the end of the fiscal year of 2012[75]. The same approach was used for macroeconomic characteristics.

Another important aspect of our firm-bank database is that we are able to distinguish the existing borrowers (intensive margin) from the new borrowers (extensive margin) and pin-down whether banks have different incentives to reallocate according to their previous relationship with firms. It might be the case, for example, that banks have more soft information from existing borrowers and, therefore, can increase estimation's precision of borrowers' quality leading these firms to be less risky than new ones.

4.3.2 Empirical strategy

We first divide the sample into two periods: one before the start of the investigations (2013) and one after (2014, 2015, and the first 2 quarters of 2016)[76], and then we collapse the data at the bank-firm-quarter level. For conservative reasons, we blocked the first quarter of 2014 because the beginning of the anti-corruption investigations was in the middle of March 2014, so this quarter might induce noise to the specifications[77].

Given that there's a substantive contraction of credit to corrupt firms included in the blacklist of Lava Jato previously addressed in chapter 3 of this thesis, we can therefore assume that the anti-corruption investigations might have led to a regime change resulting in reallocation and spillover effects in the Brazilian credit market to those firms that are likely not corrupt. Then, we exclude all loan-level information concerning the LJ firms from our database,

75 The only firm-characteristic not taken in the year of 2012 was connection to the government because it was considered the amount of donations to Brazilian federal elections of 2010.

76 In unreported results, we also used a symmetric time spam that considers 2 years before Lava Jato (2012Q1-2013Q4) and 2 years after Lava Jato (2014Q2-2016Q1) but results also remain stable.

77 This time frame (2013Q1-2016Q2, except 2014Q1) was considered in all regression specifications of this paper.

and start by examining the effect of unexpected and exogenous anti-corruption investigations on the reallocation of credit at the firm-level with the following regression model:

$$\boldsymbol{C_{i,j,t} = \alpha + \beta_1 Lava\ Jato\ Exposure_j + \beta_2 Post_t + \beta_3 (Lava\ Jato\ Exposure_j \times Post_t) + \varepsilon_{i,j,t}} \quad (4.1)$$

Where $C_{i,j,t}$ stands either for the credit supply of new loans for firm *i* from bank *j* in quarter *t*, at the intensive margin, or a dummy indicating whether firm *i* received a new loan with bank *j* in quarter *t*, at the extensive margin. *Lava Jato Exposure* is a dummy that indicates the bank *j* with more concentration of credit to corrupt firms in the pre-period, and *Post* refers to the post-period *t*. We saturate the model including a vector of bank characteristics lagged one quarter, and controlling either for Firm-Time-FE and Bank-FE or Firm-Bank-FE and Time-FE.

On the one hand, Firm-Time fixed effects purge all variation in the data that is characterized at the firm-level and captures any determinants of firm credit demand, allowing us to isolate supply factors. On the other, the Firm-Bank fixed effects control for persistent differences in a firm's relation with a particular bank. To account for any serial correlation across banks, we also cluster the standard errors at the bank-time level.

One important aspect for the identification of the treatment effect in our specification, nonetheless, is that we require firms to have at least two different bank relationships in a specific quarter (Khwaja and Mian, 2008). Then, we compare the difference within-firm (from bank A with high exposure to corrupt firms to bank B with low exposure to corrupt firms) cross-sectionally. Credit supply from bank A should be relatively higher than the credit supply from bank B if there is a reallocation effect due to corruption detection or suspicion.

By using Firm-Bank fixed effects in first-differenced data, we compare how the same firm's loan amount from one bank changes relative to another facing a relatively greater exposure to corruption in its credit portfolio. This within-firm comparison absorbs firm-specific

changes in credit demand such that the difference in loan amount can be attributed to differences in banks' exposure.

This strategy has some limitations, though. First, the coefficient of interest can only be estimated in samples of firms with multiple-banking relationships. However, the single-bank firms may be the most prone and sensitive to bank-loan credit shrinkages (Degryse, De Jonghe, Jakovljević, Mulier, Schepens, 2019). Moreover, the estimator does not allow for the identification of the real effects of credit supply, such as employment rates, as there are no multiple observations for the same firm in the same period (i.e., there is only one observation of employment for a particular firm in a given period) (Jiménez, Peydró, Repullo, Saurina, 2019). For this reason, we alternatively isolate the effect from supply and demand using bank-time and firm-time fixed effects to control for (un)observed heterogeneity (see Jakovljević, Degryse, Ongena, 2016).

The abovementioned regressions models, together with the vector of bank controls lagged one period and the fixed effects previously discussed, were implemented to analyze other loan term characteristics of non-affected firms, such as their rating, loan loss provision, loans' maturity and interest rate. Alternatively, we also verified whether these borrowers are more likely to receive a rating upgrade or downgrade in the post period.

We then extended the previous specification by combining the identification of affected banks with characteristics of the firms. It might be the case, for example, that firm characteristics that drive the reallocation at the intensive margin diverge from those driving results at the extensive margin. Therefore, the following model was implemented to test the heterogeneity of credit reallocation among borrowers:

$$C_{i,j,t} = \alpha + \beta_1 Lava\ Jato\ Exposure_j + \beta_2 Post_t + \beta_3 Moderator_i + \beta_4 (Lava\ Jato\ Exposure_j \times Post_t) + \beta_5 (Moderator_i \times Post_t) + \beta_6 (Lava\ Jato\ Exposure_j \times Moderator_i \times Post_t) + \varepsilon_{i,j,t} \quad (4.2)$$

Where all variables are similar to the previous model, except the firm-level moderator. This variable considers ex-ante borrower characteristics such as a proxy for connection with the government (namely, the volume of donations in the federal elections of 2010), a dummy indicating those firms from the construction sector, and other firm-characteristics fixed in 2012, including number of employees, mean wage, age, and number of bank-relationships. This model also includes a vector of bank controls lagged one period, controls for Firm-Time-fixed effects and Bank-fixed effects, and we cluster the standard errors at the bank-time level.

The same regression model mentioned in the previous paragraph is also used for identifying if there exists a re-democratization of credit according to state-level characteristics. Considering that there exists a credit reallocation that may vary according to borrower characteristics, it's plausible to claim that firms located in economically weaker states receive more credit in the post period because there might be growing opportunities in such geographical areas, reducing overall inequality in the allocation of credit. Therefore, we include to the vector of moderators state-level characteristics also fixed in 2012 such as sales volume, nominal revenue, industrial production, and unemployment rate.

For the analysis of real effects, we considered whether those borrowers indirectly more exposed to Lava Jato firms through their banking relationships in the pre-period have a negative impact on their level (and quality) of employment. Therefore, the following regression model is implemented to check whether the credit inflow hypothesized in the first hypothesis foster firm growth by increasing their employment rates, wages, and salaries in the post period:

$$C_{i,t} = \alpha + \beta_1 Firm\ Exposure_i + \beta_2 Post_t + \beta_3 (Firm\ Exposure_i \times Post_t) + \varepsilon_{i,t} \quad (4.3)$$

Where $C_{i,t}$ stands for wage bill, number of employees, and mean wage from firm *i* in year *t*. *Firm Exposure* is a dummy that indicates the borrower *i* with more indirect exposure to affected firms in the previous period[78], assuming the value of one if the firm has a bigger concentration of credit in a bank more exposed to Lava Jato firms in the pre-period. *Post* is a dummy variable that refers to the post-period *t*. This model also includes a vector of firm, state and industry fixed effects, as well as account for any serial correlation across firms by clustering the standard errors at the firm level.

4.4. Descriptive statistics

Our sample is composed by 24,424 firm-bank-quarter observations referring to new loans from those 50 biggest banks[79] to 1,650 different firms with more than 250 employees. However, after including zeros for missing new loans, the final sample of loans has 64,533 firm-bank-quarter observations at the intensive margin, while it has 61,354 firm-bank-quarter observations at the extensive margin[80]. Table 4.1 presents descriptive statistics and variables' definitions of the main variables used in our regression specifications, from 2013 to 2016[81].

78 We used data before our sample period starts to ensure exogeneity of the variable of exposure.

79 These banks represent 95.26% of the banks total assets in 2012 and have in their portfolio 97.72% of the total amount of new loans conceived for firms with more than 250 employees in the same time period.

80 As depicted in Table 1, the number of observations diverge between intensive and extensive margin because we used different approaches to deal with the missing observations: if the firm exists in the pre-period (intensive margin), it includes zeros for missing new loans if the firm has outstanding credit in that bank-quarter; if the firm did not exist in the pre-period (extensive margin), it includes zeros for all missing new loans.

81 As previously described, I blocked the first quarter of 2014 because the beginning of the investigations was in the middle of March 2014, so this quarter might induce noise to the specifications. For this reason, in the summary table we do not consider the first quarter of 2014 and the sample has fewer observations.

Table 4.1: Summary statistics and variable definitions

This table reports the summary statistics and definitions of the variables used in this paper. The variables related to loan characteristics are winsorized at the 1st and 99th percentiles of its distribution. The sample contemplates 1,728 firms, 44 banks and considers all quarters between January 2013 and June 2016, excluding the first quarter of 2014.

Variable	Numb. of observations	Mean	Median	Std. Dev.	P5	P95	Definition
Loan characteristics							
New loans$_{ijt}$	59,580	R$5,370,680	0	29,313,252	0	R$25,119,819	Amount of new loans to firm i from bank j in quarter t. The intensive margin includes zeros for missing new loans if the firm has outstanding credit in that bank-quarter.
New Loans [dummy]$_{ijt}$	57,040	.0573	0	.2326	0	1	Dummy variable that equals one if the firm i took a new loan from bank j in quarter t. The extensive margin includes zeros for all missing new loans.
Maturity $_{ijt}$	19,229	10.79	5.58	14.20	0	37	Number of months until the entire unpaid balance of the loan, including principal and interest, is due and payable.
Interest rate $_{ijt}$	11,178	23.56	14.25	27.89	2	100	Fixed interest rate of the loan.
Rating $_{ijt}$	19,232	2.08	2	1.02	1	4	Credit rating on a scale from AA to C assigned by banks based on the credit quality of borrowers (AA, A, B or C). 1 denotes AA and C denotes 4.

Loan loss provision $_{ijt}$	19,232	.0068	.0049	.0089	0	.0299	Banks' non-cash expense to account for future losses due to loan defaults.
Firm characteristics							
Firm indirect Exposure	1,650	.0181	.0078	.0316	0	.0733	Firm ex ante indirect credit exposure to Lava Jato measured as the weighted average of the exposures of the banks with which the firm has an outstanding credit in 2012 where weights are outstanding credit of each firm-bank relationship in 2012.
Number of employees	1,650	2,938	1,305	5,881	309	10,284	Number of employees at the end of 2012.
Mean wage	1,650	R$3,149.66	R$2,523.39	R$2,058.27	R$1,162.89	R$7,534.67	Firm wage bill divided by the number of employees at the end of 2012.
Age	1,618	49.30	39.93	39.27	5.53	112.99	Firm age at the end of 2012.
Volume of campaign donations	1,650	.3454	0	.4756	0	1	Amount donated by the firm to Brazilian federal elections of 2010 normalized by firms' number of employees.
Number of bank relationships	1,650	4.28	3	3.38	0	11	Average number of bank relationships per firm in 2012.

Construction	1,650	.0656	0	.2476	0	1	Dummy variable that equals one for firms from the construction sector.
Bank characteristics							
Lava Jato Exposure	44	.0261	.0055	.0523	0	.1391	Banks' ex ante credit exposure to Lava Jato firms measured as total outstanding credit to Lava Jato firms in 2012/total outstanding credit to other firms in 2012.
Bank liquidity	514	.2235	.2028	.1381	.0142	.4877	Bank liquid assets over total assets.
Credit/Assets	514	.5287	.5101	.2228	.1382	.9276	Bank outstanding credit divided by total assets.
Total Assets	514	R$72,914,872,139	R$10,927,733,760	R$188,388,419,862	R$2,317,292,800	R$589,418,201,088	Bank total assets.
Capital	514	0.0018	0.0016	0.0007	0.0012	0.0031	Bank regulatory capital over risk-weighted assets.
Non-performing loans	514	.0429	.0388	.0362	0	.0994	Bank loans past-due more than 90 days or with equivalent deterioration of credit quality over total loans.
Return on Assets	514	0.0079	0.0102	0.0198	-0.0197	0.0278	Bank return on assets.

Macroeconomic characteristics							
Sales volume	408	2.9584	3.9833	5.7518	-8.40	10.30	State retail sales volume index (cumulative 12-month variation in %).
Nominal revenue	408	9.1053	9.80	4.8941	-0.20	16.16	State nominal retail sales revenue (cumulative 12-month variation in %).
Industrial production	235	98.23	97.73	7.88	86.53	111.93	State industrial production (yearly comparison).
Unemployment	408	7.88	7.70	2.49	3.90	12.00	State unemployment rate (yearly % change).
HDI	408	0.74	0.75	0.04	0.64	0.81	Municipalities Human Development Index in 2010.

On average, existing borrowers in our sample borrow R$5,370,680 per bank-quarter in new loans and about 5.73% of the new contracts are new loans. As expected, the median value for both the continuous and the dichotomous variables regarding new loans are zero, since we mechanically included zeros for absent new loans in case there exists a stock of credit for that firm-bank notch (intensive margin) or included zeros for all missing new loans (extensive margin).

Regarding loan-term characteristics, the loans are usually paid back in around 10 months (median: 5.58). This pattern might indicate that those short-term loans are usually used for financing working capital demands or other obligations on the short-run and not necessarily capital expenditures related to investment in real assets or other equipment that compose the fixed assets of their balance sheets.

Rating, the only categorical variable along loan characteristics, goes from 1 (best rating) to 4 (worst rating) and indicates that the firms usually have a moderate grade of 2. The pre-fixed interest rate, however, clearly indicates that, on average, the risk of the firms is much bigger than the mean cost of capital during the same period (mean: 23.5%, median: 14.25%). Moreover, banks usually set aside 0.68% of their portfolio as provisions to account for future losses on loan defaults (median: 0.49%)

From firm characteristics, we can see that the firms in our sample are considerably big (mean: 2,938; median: 1,305) and with high skilled workers, given that they compensate their employees (mean: R$3,149.66/month, median: R$2,523.39/month) well above the minimum wage in force in the country[82]. Moreover, they usually are mature firms, have donated an

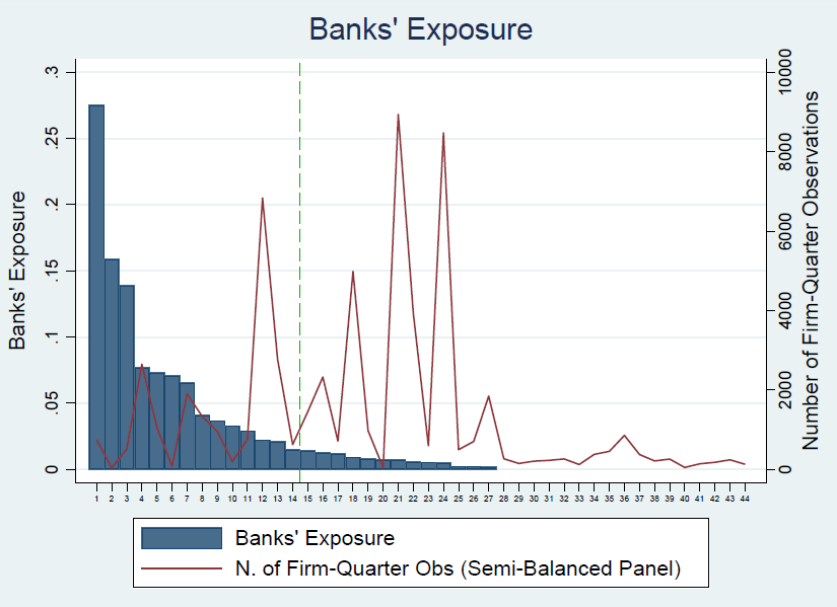
82 The minimum wage in Brazil during our sample period started in R$724, in January 2014, and was R$880, in July 2016.

average of R$0.34 (median: R$0) per employee in the federal elections of 2010, and are well connected in the banking system (mean: 4.28 bank relationships in 2012).

Our main variable of interest at the bank level, Lava Jato Exposure, indicates that, on average, 2.6% of all banks' stock of credit was concentrated on Lava Jato firms back in 2012. This value, however, differs a lot between banks given that banks did not know ex ante that those firms would be caught in corruption in 2014. As depicted in Figure 4.1, the biggest banks in our sample have a moderate level of exposure to Lava Jato firms (the biggest spikes are mainly concentrated in the second tercile of the distribution), while those extreme values of exposure (either the biggest values or the zero-exposure group) are usually smaller banks.

Figure 4.1: Banks' exposure to Lava Jato firms

This figure shows the credit exposure of privately owned banks to Lava Jato firms in 2012 (bars) and the number of firm-quarter observations of each bank (lines). The vertical broken line indicates the tercile split of the sample (T3 vs. T2 and T1).



Given the summary statistics and results reported in the previous chapter of this thesis, it would be interesting to analyze how more exposed banks to Lava Jato firms behave with the

ongoing of the anti-corruption investigations and how do groups heterogeneously differ (if so) cross-sectionally over time. We address these issues in the next session of this paper.

4.5. Results

In this section, we present our results on whether banks react to anti-corruption investigations by reallocating their credit surplus towards firms less exposed to corruption. Furthermore, we address the issue of heterogeneity among banks' response according to firm characteristics and according to the re-democratization of credit across states. Finally, we provide evidence on the borrower risk of non-affected firms and present evidence on the credit-induced real effects of anti-corruption investigations.

4.5.1. Credit reallocation at the firm-level

In order to ascertain whether banks reallocate their surplus of credit to non-affected firms, which are potentially not corrupt and less risky than those included in the blacklist of the investigations, we first start analyzing whether these firms receive a credit inflow from more exposed banks to corruption.

Table 4.2 presents the reallocation effect both at the intensive (columns 1 and 2) and extensive margin (columns 3 and 4). Columns 1 and 2 show the impact of corruption detection on the reallocation of credit to non-affected firms, comparing the post-investigation (2014Q2 – 2016Q2) with the pre-investigation (2013Q1 – 2013Q4) period. The DID coefficient for the interaction of banks more exposed to Lava Jato firms after the beginning of the investigations is negative and statistically significant at the 1% level, meaning that firms which were already existing borrowers in the pre-period, on average, receive less credit from banks more exposed to Lava Jato in the post-period, regardless the fixed effect approach we adopt.

More specifically, we can depict from our regression estimates that the negative coefficient presented in Column 1 captures the difference between more exposed and less exposed bank credit supply for the same firm after the ongoing of the investigations. In other words, when considering the same firm *i* that has relationship with two banks *j*, the more exposed banks' credit shrinkage vis-à-vis less exposed banks is roughly 48.73% ($e^{-0.668} - 1 = -48.73\%$) for the 2-year window following the start of the investigations. Similarly, column 2 also shows a sizable and negative effect: within a firm-bank pair, those banks more exposed, cross-sectionally, cease 64.16% ($e^{-1.026} - 1 = -64.16\%$) of credit in the post period when compared to less exposed banks.

Table 4.2: Bank credit reallocation at the intensive and extensive margin

This table shows the regression results of the model $C_{i,j,t} = \alpha + \beta_1 Lava\ Jato\ Exposure_j + \beta_2 Post_t + \beta_3 (Lava\ Jato\ Exposure_j \times Post_t) + \varepsilon_{i,j,t}$ where $C_{i,j,t}$ denotes either Ln(1+New Loans) or New Loans [dummy]. The analysis of the intensive margin considers only firms that did borrow before 2014Q1, while the analysis of the extensive margin considers only firms that did not borrow before 2014Q1. Both analyses exclude Lava Jato firms. *Lava Jato exposure*, as defined in Table 1, is a dummy that equals one if the bank is in the upper tercile of the distribution of the continuous exposure and zero otherwise (columns 1 and 2); or above the median in the distribution of the continuous exposure (columns 3 and 4). *Post* is a dummy variable that switches to one in the period after 2014Q1; the first quarter of 2014, in which the Operation Lava Jato started, is omitted. Bank controls are *Credit/Assets*, *Ln(Total Assets)*, *Capital*, *Non-performing loans*, *Liquidity* and *Return on Assets* lagged by one period. Standard errors (in parentheses) are clustered at the bank-level. Standard errors (in parentheses) are clustered at the bank-time level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

	(1)	(2)	(3)	(4)
	Intensive margin		Extensive margin	
Dep. Var.:	*ln(1+New Loans)*	*ln(1+New Loans)*	*New Loans [dummy]*	*New Loans [dummy]*
Lava Jato Exposure × Post	-0.668***	-1.026***	0.00786	0.0107
	(0.192)	(0.213)	(0.00867)	(0.0103)
Bank controls $_{t-1}$	Yes	Yes	Yes	Yes
Firm-Time-FE, Bank-FE	Yes	No	Yes	No
Firm-Bank-FE, Time-FE	No	Yes	No	Yes
Number of observations	54,993	59,003	52,482	57,035
Adjusted-R^2	0.168	0.326	0.123	0.145

Next, we considered firms that did not have relationship within a certain bank in the pre period (Columns 3 and 4). In other words, a firm *i* is considered a new borrower from bank *j* if they did not have any relationship in the year of 2013. For this scenario, nevertheless, we are not interested in the volume of credit, but instead on the probability of getting a new loan. Therefore, the dependent variable is a dichotomous variable that indicates whether firm *i* got a new loan with bank *j* or not.

The DID coefficient for the interaction of banks more exposed to Lava Jato firms after the beginning of the investigations, albeit being positive, is not statistically different from zero, regardless the fixed effect approach we adopt. This result, although unexpected, might indicate some heterogeneity of the results, which leads us to the next section of the results.

4.5.2. Heterogeneous effects of credit reallocation and state-level characteristics

Given that the incentives that drive the reallocation highlighted in the previous part seem to be different from existing borrowers and new borrowers, we extend our results by combining the identification of affected banks with characteristics of the non-affected firms. Beyond firm-characteristics, we also included state-characteristics that may lead banks to have heterogeneous effects in the redistribution of credit. Therefore, we have analyzed who are the borrowers that get less credit from affected banks both at the intensive and at the extensive margin.

Concerning the intensive margin, Table 4.3 portrays that, in general, more exposed banks decrease credit to non-affected firms in the post period, which is consistent with previous results presented in the previous section of this paper. Coefficients reported for the double interaction *Lava Jato Exposure x Post* show that the effect is consistently negative and significant at the 1% level in all specifications.

Table 4.3: Heterogeneity of bank credit reallocation at the intensive margin

This table shows the regression results of the model $Ln(1+New\ loans)_{i,j,t} = \alpha + \ldots + \beta_4 Lava\ Jato\ Exposure_j \times Post_t + \beta_5 Lava\ Jato\ Exposure_j \times Moderator_i + \beta_6(Lava\ Jato\ Exposure_j \times Moderator_i \times Post_t) + \varepsilon_{i,j,t}$. The moderators capture heterogeneity by firm characteristics (Panel A) and state-level macroeconomic characteristics inherent to the states or municipalities in which firms have their headquarters (Panel B). The analysis of the intensive margin considers only firms that did borrow before 2014Q1 and excludes Lava Jato firms. *Lava Jato Exposure* is a dummy that equals one if the bank is in the upper tercile of the distribution of the continuous exposure and zero otherwise. *Post* is a dummy variable that switches to one in the period after 2014Q1; the first quarter of 2014, in which the Operation Lava Jato started, is omitted. All continuous moderators were classified as dummies that equal one if the moderator is in the upper tercile of the distribution and zero otherwise. *Construction* and *More than one bank-relationship* are dummy variables that are one for firms from the construction industry, or for firms that had more than one bank relationship in 2012, respectively. Bank controls are *Credit/Assets*, *Ln(Total Assets)*, *Capital*, *Non-performing loans*, *Liquidity* and *Return on Assets* lagged by one period. Standard errors (in parentheses) are clustered at the bank-time level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

Panel A: Heterogeneity by firm characteristics

Dep. Var.:	(1) *ln(1+New Loans)*	(2) *ln(1+New Loans)*	(3) *ln(1+New Loans)*	(4) *ln(1+New Loans)*	(5) *ln(1+New Loans)*	(6) *ln(1+New Loans)*	(7) *ln(1+New Loans)*
Moderator	Volume of donations	Construction	Number of employees	Mean wage	Age	Firm Growth	More than one-bank relationship
Lava Jato Exposure × Post × Moderator	0.0783	0.164	0.412	0.201	0.198	0.300	1.815**
	(0.292)	(0.490)	(0.377)	(0.359)	(0.300)	(0.344)	(0.884)
Lava Jato Exposure × Post	-0.650***	-0.661***	-0.934***	-0.915***	-0.908***	-0.941***	-2.446***
	(0.212)	(0.199)	(0.277)	(0.296)	(0.276)	(0.286)	(0.884)
Lava Jato Exposure × Moderator	0.351	1.720***	-0.423	0.315	-0.302	-0.122	-2.079***
	(0.229)	(0.376)	(0.295)	(0.258)	(0.226)	(0.235)	(0.751)
Bank controls $_{t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time-FE, Bank-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	49,667	54,922	36,851	35,597	36,342	35,990	54,689
Adjusted-R^2	0.169	0.169	0.166	0.170	0.162	0.170	0.167

Panel B: Heterogeneity by state-level macroeconomic outcomes

	(1)	(2)	(3)	(4)	(5)
Dep. Var.:	*ln(1+New Loans)*	*ln(1+New Loans)*	*ln(1+New Loans)*	*ln(1+New Loans)*	*ln(1+New Loans)*
Moderator	Sales volume	Nominal revenue	Industrial production	Unemployment	HDI
Lava Jato Exposure × *Post* × *Moderator*	0.311	0.194	1.184**	-0.00431	-0.543
	(0.444)	(0.473)	(0.556)	(0.362)	(0.443)
Lava Jato Exposure × *Post*	-1.010**	-0.901**	-1.777***	-0.692**	-0.663***
	(0.430)	(0.456)	(0.528)	(0.324)	(0.241)
Lava Jato Exposure × *Moderator*	-0.795**	-0.865**	-0.586	0.0160	0.374
	(0.334)	(0.350)	(0.409)	(0.306)	(0.335)
Bank controls $_{t-1}$	Yes	Yes	Yes	Yes	Yes
Firm-Time-FE, Bank-FE	Yes	Yes	Yes	Yes	Yes
Number of observations	42,890	43,030	35,976	51,828	24,228
Adjusted-R^2	0.169	0.170	0.165	0.167	0.172

Heterogeneous effects emerge when considering firm characteristics that may proxy for their riskiness or state-level macroeconomic characteristics that may distinguish their growth opportunities. As presented by the triple interaction *Lava Jato Exposure x Post x Moderator* in Panel A, we can see that the overall effect is smaller only for those firms which have multiple bank relationships in the 2012. The same pattern appears when considering those growing states with more industrial production in the year of 2012: even though the overall effect is negative (more exposed banks supply less credit to non-affected firms in the post period), those firms located in economically stronger states receive more supply of credit.

When considering the extensive margin, Table 4.4 shows that, in general, more exposed banks do not necessarily reallocate to new borrowers in the post period, as depicted by the mostly non-significant coefficients of the double interaction *Lava Jato Exposure x Post* in Panel A. Nonetheless, new borrowers who are politically connected and from the construction sector are more likely to obtain credit, and, albeit their similarity to those firms caught in corruption by the *Lava Jato*, these characteristics do not necessarily serve as a proxy for fraudulent behavior. Another interesting result is that new borrowers who are younger usually receive a credit inflow from more exposed banks in the post period, indicating that the reallocation do not necessarily occur based on the safety that older firms may convey.

Table 4.4: Heterogeneity of bank credit reallocation at the extensive margin

This table shows the regression results of the model *New Loans [dummy]*$_{i,j,t}$ = α + … + β_4*Lava Jato Exposure*$_j$ × *Post*$_t$ + β_5*Lava Jato Exposure*$_j$ × *Moderator*$_i$ + β_6(*Lava Jato Exposure*$_j$ × *Moderator*$_i$ × *Post*$_t$) + $\varepsilon_{i,j,t}$. The moderators capture heterogeneity by firm characteristics (Panel A) and state-level macroeconomic characteristics inherent to the states or municipalities in which firms have their headquarters (Panel B). The analysis of the extensive margin considers only firms that did not borrow before 2014Q1 and excludes Lava Jato firms. *Lava Jato Exposure* is a dummy that equals one if the bank is in the upper tercile of the distribution of the continuous exposure and zero otherwise. *Post* is a dummy variable that switches to one in the period after 2014Q1; the first quarter of 2014, in which the Operation Lava Jato started, is omitted. All continuous moderators were classified as dummies that equal one if the moderator is in the upper tercile of the distribution and zero otherwise. *Construction* and *More than one bank-relationship* are dummy variables that are one for firms from the construction industry, or for firms that had more than one bank relationship in 2012, respectively. Bank controls are *Credit/Assets*, *Ln(Total Assets)*, *Capital*, *Non-performing loans*, *Liquidity* and *Return on Assets* lagged by one period. Standard errors (in parentheses) are clustered at the bank-time level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

Panel A: Heterogeneity by firm characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.:	*New Loans [dummy]*	*New Loans [dummy]*	*New Loans [dummy]*	*New Loans [dummy]*	*New Loans [dummy]*	*New Loans [dummy]*	*New Loans [dummy]*
Moderator	Volume of donations	Construction	Number of employees	Mean wage	Age	Firm Growth	More than one-bank relationship
Lava Jato Exposure × Post × Moderator	0.0289***	0.0379**	-0.0207	-0.00943	-0.0154*	0.00101	0.000828
	(0.00814)	(0.0181)	(0.0147)	(0.00953)	(0.00930)	(0.00900)	(0.00965)
Lava Jato Exposure × Post	-0.00295	0.00546	0.0201*	0.0109	0.0122	0.00965	0.00622
	(0.00900)	(0.00912)	(0.0104)	(0.0107)	(0.00954)	(0.00875)	(0.0114)
Lava Jato Exposure × Moderator	-0.00421**	-0.0195***	0.00996*	0.000465	0.00263*	-0.00447***	0.00206
	(0.00170)	(0.00646)	(0.00555)	(0.00266)	(0.00146)	(0.00167)	(0.00280)
Bank controls $_{t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time-FE, Bank-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	50,829	52,341	35,056	34,887	34,394	34,663	47,062
Adjusted-R^2	0.124	0.123	0.125	0.115	0.129	0.126	0.119

Panel B: Heterogeneity by state-level macroeconomic outcomes

	(1)	(2)	(3)	(4)	(5)
Dep. Var.:	*New Loans [dummy]*	*New Loans [dummy]*	*New Loans [dummy]*	*New Loans [dummy]*	*New Loans [dummy]*
Moderator	Sales volume	Nominal revenue	Industrial production	Unemployment	HDI
Lava Jato Exposure × Post × Moderator	-0.0256**	-0.0257**	-0.0225	-0.0124	-0.0161
	(0.0118)	(0.0123)	(0.0143)	(0.00901)	(0.0116)
Lava Jato Exposure × Post	0.0347***	0.0349**	0.0314**	0.0183*	0.00424
	(0.0129)	(0.0135)	(0.0152)	(0.0109)	(0.0104)
Lava Jato Exposure × Moderator	0.0111***	0.0111***	0.00794**	0.00383**	0.00306
	(0.00349)	(0.00382)	(0.00334)	(0.00185)	(0.00321)
Bank controls $_{t-1}$	Yes	Yes	Yes	Yes	Yes
Firm-Time-FE, Bank-FE	Yes	Yes	Yes	Yes	Yes
Number of observations	45,381	42,390	35,066	52,462	22,676
Adjusted-R^2	0.126	0.129	0.123	0.123	0.128

Regarding the heterogeneity by state-level macroeconomic outcomes (Panel B), we can see that when considering the subsample of firms included in this analysis, more exposed banks seem to reallocate to new borrowers in the post period, as depicted by the significant coefficients of the double interaction *Lava Jato Exposure x Post.* However, opposite to the results highlighted at the intensive margin (Table 4.3, Panel B), those firms located in more developed states, on average, receive less credit, conveying that there might be a redistribution of credit to firms located in economically weaker states.

In sum, these results suggest that the heterogeneous effects that drive the reallocation might differ when comparing existing borrowers with new borrowers. On the one hand, firms located in economically stronger states receive more credit from affected banks in the post period at the intensive margin. On the other, firms located in economically weaker states and, therefore, with growing opportunities receive more credit from affected banks in the post period at the extensive margin. Furthermore, results give a first impression that banks might reallocate to new firms which are similar from those caught in corruption in the Lava Jato list and this do not necessarily proxy for fraudulent behavior. This result leads us to think about the overall quality of the new loan contracts, a question that will be addressed in the next section of this paper.

4.5.3. Borrower risk of non-affected firms

This section of the paper analyzes whether the overall credit quality of the new loans in the more exposed banks deteriorates/improves in the post period and, therefore, investigates the overall borrower risk of non-affected firms. Given that corruption is partially deterred with the ongoing of the Lava Jato Operation and that our sample do not includes those firms part of this blacklist, it is reasonable to assume that loan term characteristics of non-affected firms might become less risky in the post period.

Table 4.5 presents the loan characteristics of non-Lava Jato firms in the post period. The DID coefficients for the interaction term *Lava Jato Exposure x Post* report that, regardless the significance, when considering the same firm *i* that has relationship with two banks *j*, firms in more exposed banks' have better ratings, smaller loan loss provisions, bigger maturities and smaller interest rates. The sign of the coefficients is in line with previous expectations, in a sense that loan-term characteristics of non-affected firms become less risky in the post period.

Table 4.5: Loan characteristics of non-Lava Jato firms in the post period

This table shows the regression results of the model $C_{i,j,t} = \alpha + \beta_1 Lava\ Jato\ Exposure_j + \beta_2 Post_t + \beta_3 (Lava\ Jato\ Exposure_j \times Post_t) + \varepsilon_{i,j,t}$ where *C* indicates either *Rating, Loan loss provision, ln(1+Maturity)* or *Interest rate*. *Lava Jato exposure*, as defined in Table 1, is a dummy that equals one if the bank is in the upper tercile of the distribution of the continuous exposure and zero otherwise. *Post* is a dummy variable that switches to one in the period after 2014Q1; the first quarter of 2014, in which the Operation Lava Jato started, is omitted. Panel A controls for Firm-Time-FE, Bank-FE; and Panel B controls for Firm-Bank-FE, Time-FE. Bank controls are *Credit/Assets*, *Ln(Total Assets)*, *Capital*, *Non-performing loans*, *Liquidity* and *Return on Assets* lagged by one period. Standard errors (in parentheses) are clustered at the bank-time level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

	(1)	(2)	(3)	(4)
Dep. Var.:	*Rating*	*Loan loss provision*	*ln(1+Maturity)*	*Interest rate*
Lava Jato Exposure × Post	-0.0397	-0.000854*	1.257**	-1.097
	(0.0514)	(0.000476)	(0.526)	(1.593)
Bank Controls $_{t-1}$	Yes	Yes	Yes	Yes
Firm-Time-FE, Bank-FE	Yes	Yes	Yes	Yes
Number of observations	16,933	16,933	16,931	7,585
Adjusted-R^2	0.420	0.326	0.400	0.262

However, one can derive further conclusions only by the significant coefficients reported in columns 2 and 3 since the other two coefficients are statistically not different from zero. Having said that, on average, non-affected firms have -0.09% ($e^{-0.000854} - 1 = -0.09\%$) less

loan loss provisions in more exposed banks vis-à-vis less exposed ones in the post period. Analogous reasoning can be applied for the average maturity of non-affected firms, that significantly increases in the post period for borrowers in more exposed banks.

Given that the overall borrower risk seems to improve in the post period for non-affected firms in more exposed banks, we move one step further and analyzed the variable rating in a different manner. Instead of considering the time-series variation of the variable (as reported in Table 4.5), we took the average rating of each firm-bank pair in the pre-period and compared with the average of the same firm-bank pair in the post period. This strategy is interesting because instead of comparing the same firm *i* in two banks *j* with different levels of exposure, allows us to isolate the effect of comparing the same firm *i* before and after in the same bank *j*, but in the cross-section of the data.

Table 4.6: Rating upgrade/downgrade of non-Lava Jato firms in the post period

This table shows the regression results of the model $C_{ij} = \alpha + \beta_1 Lava\ Jato\ Exposure_j + \varepsilon_i$ where *C* indicates Rating upgrade or Rating downgrade. If the mean of the firm rating from a specific bank in the post period is smaller than the mean in the pre period from the same bank, it is an upgrade. If the mean rating is higher, it is a downgrade. *Lava Jato Exposure* is a dummy that equals one if the bank is in the upper tercile of the distribution of the continuous exposure and zero otherwise. Bank controls are *Credit/Assets*, *Ln(Total Assets)*, *Capital*, *Non-performing loans*, *Liquidity* and *Return on Assets* fixed in 2013. Standard errors (in parentheses) are clustered at the bank-time level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

Dep. Var.:	(1) *Rating upgrade*	(2) *Rating downgrade*
Lava Jato Exposure	0.0895***	-0.00530
	(0.0236)	(0.0258)
Bank Controls [2013]	Yes	Yes
Firm-FE	Yes	Yes
Number of observations	3,538	3,538
R^2	0.014	0.017

Results reported in Table 4.6 are straightforward: on average, non-affected firms usually receive rating upgrades in more exposed banks in the post period. Results, though, are heterogeneous. Albeit the overall coefficient the variable *Lava Jato Exposure* is positive and significant in Table 4.7, the effect denoted by the interaction term *Lava Jato Exposure x Moderator* is smaller for those firms from the construction sector (column 2) and with low ratings in the pre-period (column 7). In opposite line, the effect of rating upgrades is even stronger for bigger firms (column 3) and with more than one bank relationship in the pre-period (column 6), although only significant at the 10% level.

Table 4.7: Heterogeneity of rating upgrade of non-Lava Jato firms in the post-period

This table shows the regression results of the model $C_{ij} = \alpha + \beta_1 Lava\ Jato\ Exposure_j + \varepsilon_i$ where *C* denotes the comparison between the mean of the rating in the post-period for each bank-firm pair, vis-à-vis the pre-period mean for the same bank-firm pair. If the mean of the rating in the post period is smaller than the mean in the pre period, it means an upgrade; otherwise, a downgrade. *Lava Jato Exposure* is a dummy that equals one if the bank is in the upper tercile of the distribution of the continuous exposure and zero otherwise. All continuous moderators were classified as dummies that equal one if the moderator is in the upper tercile of the distribution and zero otherwise. *Construction*, *More than one bank-relationship* and *Low rating* are dummy variables that are one for firms from the construction industry, for firms that had more than one bank relationship in 2012, or firms with the worst ratings (B or C) in 2012, respectively. Bank controls are *Credit/Assets*, *Ln(Total Assets)*, *Capital*, *Non-performing loans*, *Liquidity* and *Return on Assets* fixed in 2013. Standard errors (in parentheses) are clustered at the bank-time level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.:	*Rating upgrade*	*Rating upgrade*	*Rating upgrade*	*Rating upgrade*	*Rating upgrade*	*Rating upgrade*	*Rating upgrade*
Moderator	Volume of donations	Construction	Number of employees	Mean wage	Age	More than one-bank relationship	Low rating
Lava Jato Exposure x Moderator	0.0289	-0.101*	0.0740*	-0.0471	0.0644	0.142*	-0.0784**
	(0.0321)	(0.0528)	(0.0385)	(0.0406)	(0.0395)	(0.0834)	(0.0386)
Lava Jato Exposure	0.0761***	0.0966***	0.0345	0.122***	0.0596*	-0.0502	0.114***
	(0.0283)	(0.0243)	(0.0339)	(0.0364)	(0.0351)	(0.0834)	(0.0283)
Bank Controls [2013]	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,538	3,533	2,398	2,299	2,375	3,487	2,852
R^2	0.014	0.015	0.013	0.023	0.014	0.014	0.018

Altogether, our results reported in this section highlight that the overall credit quality of the new contracts indeed improves in the post period when considering more exposed banks vis-à-vis less exposed ones. More than that, our findings suggest that construction firms and those with low rating in previous bank-relationships are less prone to be upgraded, while bigger firms and with more bank-relationships are more prone to have their credit scores improved. Therefore, we can derive that the overall borrower risk of non-affected firms is superior in the post period, which leaves us with the open question whether there are also positive spillover impacts on the labor market of these firms. The next section addresses this point.

4.5.4. Real effects

Given that banks' supply of credit decreases in the post period when comparing more exposed institutions vis-à-vis less exposed ones, it is important to investigate labor market consequences of these firms indirectly exposed to Lava Jato. It might be the case that this credit shrinkage negatively impacts their level of employment, in the opposite direction to what would happen if there were a credit inflow from more exposed banks to these firms. We devote this section to investigate this issue.

Table 4.8 shows how the indirect exposure to Lava Jato impacts on borrowers' real effects. Results reported in columns 1-3 indicate that those firms indirectly exposed to Lava Jato in the pre period, on average, reduce their wage bill and number of employees in about 15.63% ($e^{-0.173} - 1 = 15.63\%$). Even though the sign of the DID coefficient for the dependent variable points to a negative impact on firms' mean wage too, the coefficient is not statistically different from zero and we cannot derive further conclusions about its magnitude.

Table 4.8: Firms' indirect exposure to Lava Jato and the impact on real effects

Models (1), (2), and (3) show the regression results of the model $C_{i,t} = \alpha + \beta_1 Firm\ Indirect\ Exposure_i + \beta_2 Post_t + \beta_3 (Firm\ Indirect\ Exposure_i \times Post_t) + \varepsilon_{i,t}$ where *C* indicates either *Wage bill*, *Number of employees* or *Mean wage*. *Firm Indirect Exposure* is a dummy that equals one if the firm is in the upper tercile of the distribution of the continuous indirect exposure and zero otherwise. *Post* is a dummy variable that switches to one in the period after 2014. Models (4), (5), and (6) decompose the effect of the post period in 2014 and 2015. Standard errors (in parentheses) are clustered at the firm-level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.:	*Wage bill*	*N. of employees*	*Mean wage*	*Wage bill*	*N. of employees*	*Mean wage*
Firm Indirect Exposure x Post	-0.173***	-0.172***	-15.10			
	(0.0445)	(0.0434)	(36.40)			
Post	0.0169	-0.0773***	299.5***			
	(0.0140)	(0.0145)	(18.04)			
Firm Indirect Exposure x Post [2014]				-0.158***	-0.157***	9.154
				(0.0438)	(0.0440)	(30.17)
Firm Indirect Exposure x Post [2015]				-0.266***	-0.271***	-6.849
				(0.0563)	(0.0529)	(53.53)
Post [2014]				0.101***	-0.0148	308.3***
				(0.0118)	(0.0150)	(17.84)
Post [2015]				0.0448**	-0.109***	516.9***
				(0.0198)	(0.0181)	(22.71)
State-, Industry-, Firm-FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4,816	4,816	4,816	4,816	4,816	4,816
R^2	0.878	0.879	0.942	0.878	0.879	0.942

When decomposing the effect in the post period into two different horizons, we can see that the effect seems to considerably increase as time passes by. This result, albeit different from our previous expectations, is consistent with the previous results presented in the first part of this paper: given that there is less credit from affected banks in the post period, non-affected firms indirectly exposed to Lava Jato consequently face an unexpected credit shrinkage and, therefore, reduce their wage bill and employment rates in the post period, an effect that monotonically attenuates over time.

4.6. Additional checks

4.6.1. Redistribution of credit across municipalities

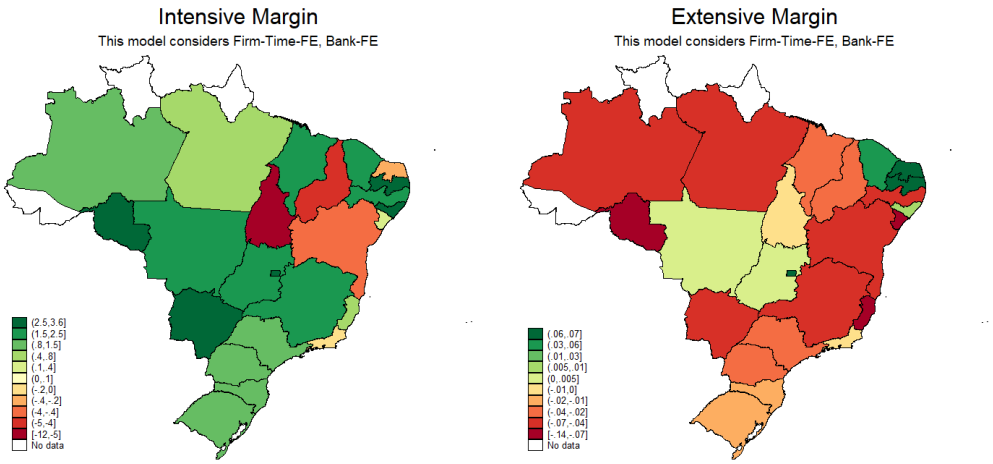
We further investigate the effects of the reallocation and how the heterogeneity of municipalities might give us new insights on the topic. Brazil, a decentralized country with 26 states, 1 federal district, and more than 5,500 municipalities, has a diverse macroeconomic environment. The region Southeast, for example, concentrates around half of the industrial GDP of the country and almost 45% of its population. Agriculture, on the other hand, has an important role in the regional economy of the Midwest region. The Northeast has catching Brazil's economic attention, presenting higher growth rates than the rest of the country.

Therefore, it is legitimate to understand if the banks' incentives to reallocate are also driven by other regional characteristics, not previously addressed in previous parts of this paper. For this reason, to test this regional dependence effect, we consider how the redistribution of credit diverge across firms' municipality. The baseline category for comparison in our specification at the municipality level is Rio de Janeiro, the place where Petrobras, the first company initially hit by the investigations, as well as most of the other firms included in the blacklist of Lava Jato, have their headquarters.

When considering the state-level analysis, results depicted in the heatmaps of Table 4.9 indicate that more exposed banks reallocate relatively more at the intensive margin to all regions of Brazil in the post period, except for firms located in some poor states such as Tocantins, Piauí and Rio Grande do Norte. At the extensive margin, nonetheless, there is relatively more credit to some interior states, such as Mato Grosso and Goiás, and mostly northeastern states, including Ceará, Rio Grande do Norte, Paraíba and Alagoas. The other states, on average, receive less credit from more exposed banks in the post period relatively to Rio de Janeiro.

Table 4.9: Heterogeneity of bank credit reallocation according to firms' state of location

These heatmaps show the regression coefficients of the model $C_{i,j,t} = \alpha + \ldots + \beta_4 Lava\ Jato\ Exposure_j \times Post_t + \beta_5 Lava\ Jato\ Exposure_j \times State_i + \beta_6 Post_t \times State_i + \beta_7 (Lava\ Jato\ Exposure_j \times Post_t \times State_i) + \varepsilon_{i,j,t}$ where *C* indicates either the volume of new loans *Ln(1+New loans)* or whether the firm took a new loan or not, *New Loans [dummy]*. The analysis of the intensive margin considers only firms that did borrow before 2014Q1 and excludes Lava Jato firms. The analysis of the extensive margin considers only firms that did not borrow before 2014Q1 and excludes Lava Jato firms. *Lava Jato Exposure* is a dummy that equals one if the bank is in the upper tercile of the distribution of the continuous exposure and zero otherwise. *Post* is a dummy variable that switches to one in the period after 2014Q1; the first quarter of 2014, in which the Operation Lava Jato started, is omitted. The baseline category for *State of location* is *Rio de Janeiro*. Bank controls are *Credit/Assets*, *Ln(Total Assets)*, *Capital*, *Non-performing loans*, *Liquidity* and *Return on Assets* lagged by one period. Standard errors (in parentheses) are clustered at the bank-time level.



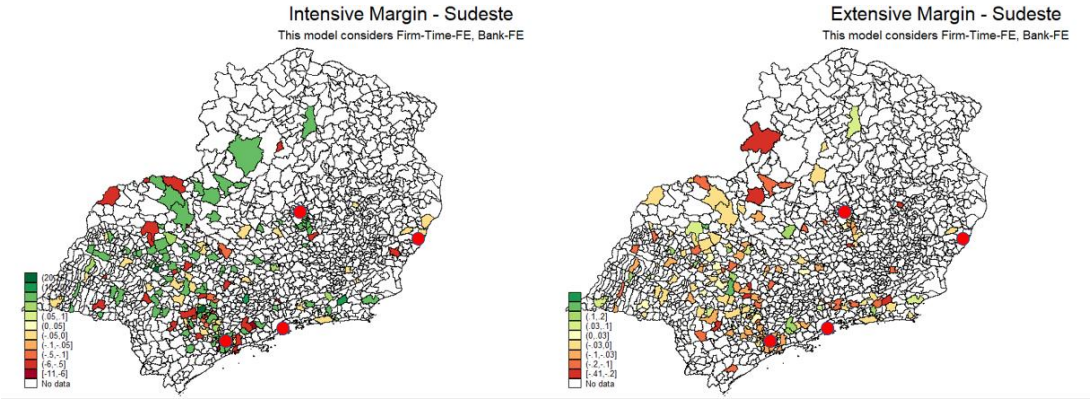
When considering the municipality-level analysis presented in Table 4.10, some very useful insights can be taken. On the one hand, most of the municipalities were painted in a green

pallet at the intensive margin, which means that more exposed banks reallocate relatively more to most of the municipalities of Brazil in the post period. On the other, when it comes to the extensive margin, most of the municipalities were colored in an orange pallet, which means that banks with more exposure to Lava Jato firms reallocate less to most of these municipalities when compared to Rio de Janeiro. Furthermore, while the reallocation seems to occur in the interior of the states in the Southeast and South regions of Brazil, it is concentrated close to the capitals in the other regions of the country, mostly in Northeast of Brazil.

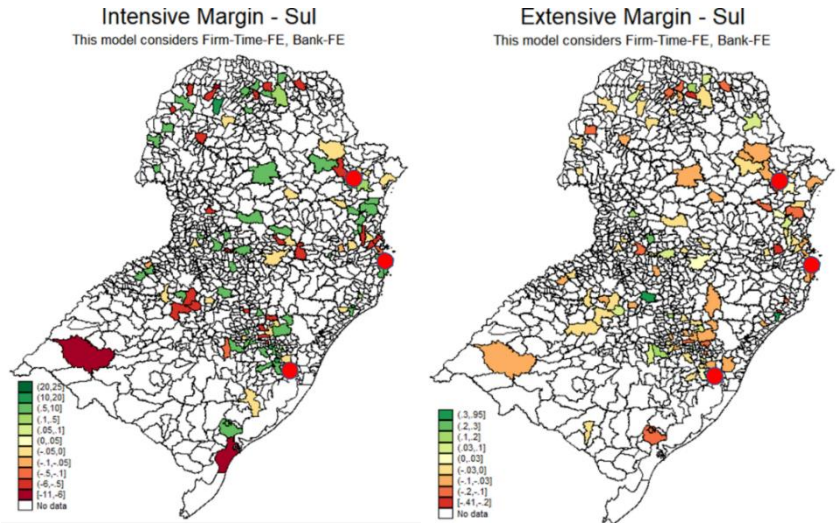
Table 4.10: Heterogeneity of bank credit reallocation according to firms' municipality

These heatmaps show the regression coefficients of the model $C_{i,j,t} = \alpha + \ldots + \beta_4 Lava\ Jato\ Exposure_j \times Post_t + \beta_5 Lava\ Jato\ Exposure_j \times Municipality_i + \beta_6 Post_t \times Municipality_i + \beta_7 (Lava\ Jato\ Exposure_j \times Post_t \times Municipality_i) + \varepsilon_{i,j,t}$ where *C* indicates either the volume of new loans *Ln(1+New loans)* or whether the firm took a new loan or not, *New Loans [dummy]*. The analysis of the intensive margin considers only firms that did borrow before 2014Q1 and excludes Lava Jato firms. The analysis of the extensive margin considers only firms that did not borrow before 2014Q1 and excludes Lava Jato firms. *Lava Jato Exposure* is a dummy that equals one if the bank is in the upper tercile of the distribution of the continuous exposure and zero otherwise. *Post* is a dummy variable that switches to one in the period after 2014Q1; the first quarter of 2014, in which the Operation Lava Jato started, is omitted. The baseline category for *Municipality of location* is *Rio de Janeiro*. Bank controls are *Credit/Assets*, *Ln(Total Assets)*, *Capital*, *Non-performing loans*, *Liquidity* and *Return on Assets* lagged by one period. Standard errors (in parentheses) are clustered at the bank-time level. In panel (A), only those municipalities with significant regression coefficients were painted. In panel (B), non-significant results were set to zero and all regression coefficients that are non-missing were painted. In panel (C), all regression coefficients were painted, regardless the significance of the results. Red dots indicate cities capital.

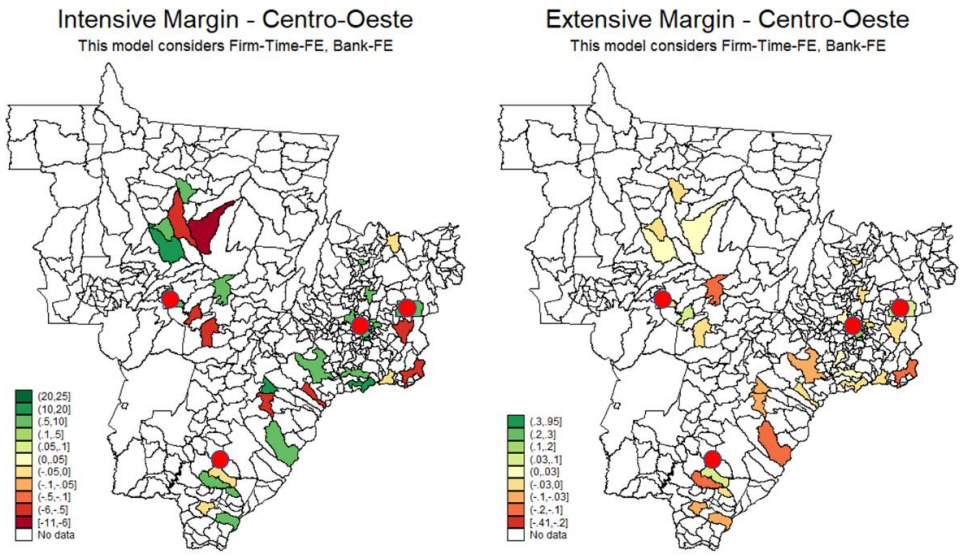
Southeast of Brazil



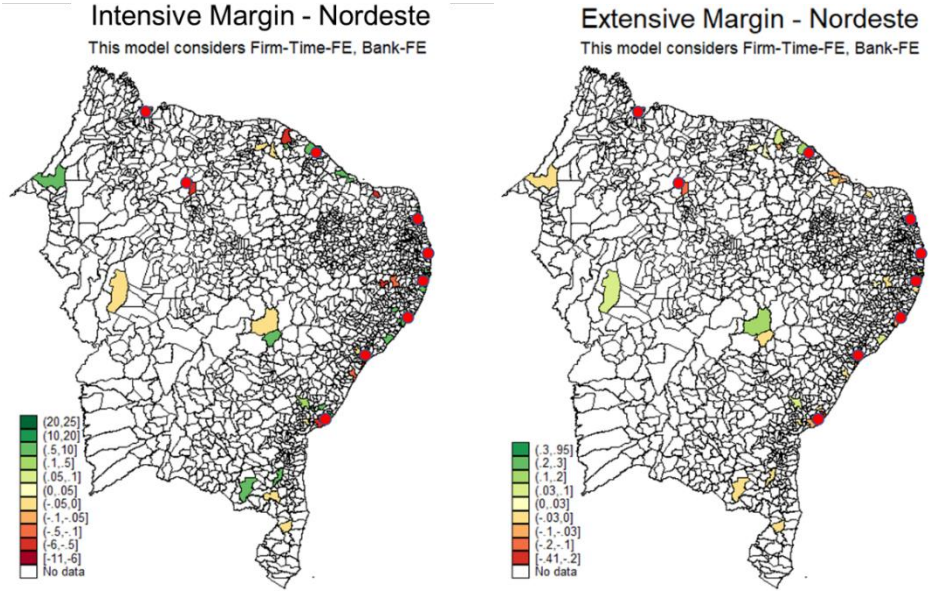
South of Brazil



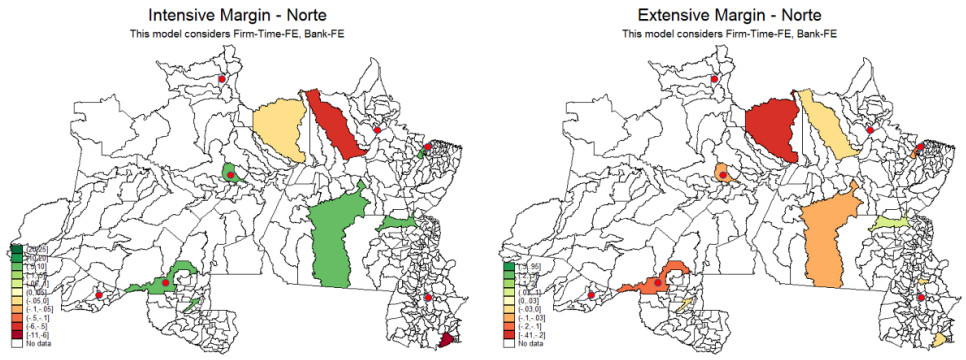
Midwest of Brazil



Northeast of Brazil



North of Brazil



Overall, our results based on firms headquarter location goes in line with previous results: the criteria for more exposed banks to reallocate is far from homogeneous. At a first investigation, while economically stronger states might benefit from the credit reallocation at the intensive margin (Table 4.3, Panel B), economically weaker states receive a credit inflow from more exposed banks at the extensive margin (Table 4.4, Panel B). Our new results

presented in this section extends our findings and show that existing borrowers from different states/municipalities, on average, receive more credit vis-à-vis firms located in the middle of the anti-corruption investigations, Rio de Janeiro. New borrowers, however, don't necessarily have this easiness when it comes to receiving more credit and relatively receive less credit than those southeast new firms.

4.6.2. Spillover effects by bank ownership

Complementing the previous analyses of this chapter, it is important to know whether state-owned banks have different behavior compared to privately-owned banks when it concerns the overall credit reallocation. It can be the case that the incentives that drive the reallocation differ according to bank ownership and credit granted from state-owned banks might serve as a substitute to those firms that face a considerable credit contraction. For this reason, we repeated our main analyses considering all banks (both privately owned and state-owned banks), and both types of credit (free market lending and earmarked lending).

At the intensive margin, results reported in Table 4.11 show a clear contraction of earmarked credit from affected private banks (columns 1 and 2). On average, the more exposed banks' credit shrinkage vis-à-vis less exposed ones ranges from 53.65% ($e^{-0.769} - 1 = -0.5365$) to 73.81% ($e^{-1.340} - 1 = -0.7381$) depending on the fixed effect approach we might consider for the 2-year window following the start of the investigations. Nevertheless, interesting results were found when including state-owned banks in the sample: on the one hand, no significant change was found when considering the earmarked type of credit (columns 3 and 4), and we cannot derive further conclusions on heterogeneity of the results. On the other, opposite behavior was found when considering free market credit from affected state-owned banks (columns 5 and 6): even though there is a significant contraction of credit from more exposed banks to non-affected firms in the post period (interaction *Lava Jato Exposure x Post*), state-

owned banks that are more exposed to Lava Jato firms give more credit to non-affected firms in the post period, which mitigates the overall negative effect of the credit shrinkage and is in line with the countercyclical behavior of state-owned banks.

At the extensive margin, results reported in Table 4.12 indicate that affected banks are more likely to grant credit to non-affected firms in the post period, a result that is robust regardless the usage of only private banks (columns 1 and 2), and the inclusion of state-owned banks considering either earmarked lending (columns 3 and 4) or free market lending (columns 5 and 6). In line with the previous paragraph, nonetheless, we find opposite behavior of free market and earmarked credit from affected state-owned banks, a result that is also robust to both types of credit reported in columns 3-6.

Table 4.11: Bank credit reallocation at the intensive margin

The models (1) and (2) show the regression results of the model $Ln(1+New\ Loans)_{i,j,t} = \alpha + \beta_1 Lava\ Jato\ Exposure_j + \beta_2 Post_t + \beta_3(Lava\ Jato\ Exposure_j \times Post_t) + \varepsilon_{i,j,t}$ Models (3) onwards show the regression results of the model $Ln(1+New\ loans)_{i,j,t} = \alpha + \ldots + \beta_4(Lava\ Jato\ Exposure_j \times Post_t) + \beta_5(Lava\ Jato\ Exposure_j \times State\ Owned\ Banks_j) + \beta_6(Post_j \times State\ Owned\ Banks_j) + \beta_7(Lava\ Jato\ Exposure_j \times Post_t \times State\ Owned\ Bank_j) + \varepsilon_{i,j,t}$. The analysis of the intensive margin considers only firms that did borrow before 2014Q1 and excludes Lava Jato firms. *Lava Jato exposure*, as defined in Table 1, is a dummy that equals one if the bank is in the upper tercile of the distribution of the continuous exposure and zero otherwise. *Post* is a dummy variable that switches to one in the period after 2014Q1; the first quarter of 2014, in which the Operation Lava Jato started, is omitted. Bank controls are *Credit/Assets*, *Ln(Total Assets)*, *Capital*, *Non-performing loans*, *Liquidity* and *Return on Assets* lagged by one period. Standard errors (in parentheses) are clustered at the bank-level. Standard errors (in parentheses) are clustered at the bank-time level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

Dep. Var.:	(1) *ln(1+New Loans)*	(2) *ln(1+New Loans)*	(3) *ln(1+New Loans)*	(4) *ln(1+New Loans)*	(5) *ln(1+New Loans)*	(6) *ln(1+New Loans)*
Lava Jato Exposure × Post	-0.769***	-1.340***	-0.228	-0.452	-0.447**	-0.967***
	(0.293)	(0.261)	(0.282)	(0.300)	(0.180)	(0.200)
Post x State owned banks			1.109**	1.042	-0.401*	-0.373
			(0.494)	(0.645)	(0.238)	(0.238)
Lava Jato Exposure × Post × State owned Banks			0.0374	0.709	1.378*	1.848**
			(0.617)	(0.789)	(0.708)	(0.768)
Bank controls $_{t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time-FE, Bank-FE	Yes	No	Yes	No	Yes	No
Firm-Bank-FE, Time-FE	No	Yes	No	Yes	No	Yes
Earmarked lending	Yes	Yes	Yes	Yes	No	No
Free market lending	No	No	No	No	Yes	Yes
Inclusion of state-owned banks?	No	No	Yes	Yes	Yes	Yes
Number of observations	32,945	37,618	44,502	48,580	69,513	72,917
Adjusted-R^2	0.191	0.354	0.181	0.357	0.182	0.345

Table 4.12: Bank credit reallocation at the extensive margin

The models (1) and (2) show the regression results of the model New Loans(dummy)i,j,t = α + β1Lava Jato Exposurej + β2Postt + β3(Lava Jato Exposurej × Postt) + εi,j,t Models (3) onwards show the regression results of the model New Loans(dummy)i,j,t = α + … + β4(Lava Jato Exposurej × Postt) + β5(Lava Jato Exposurej × State Owned Banksj) + β6(Postj × State Owned Banksj) + β7(Lava Jato Exposurej × Postt × State Owned Bankj) + εi,j,t. The analysis of the extensive margin considers only firms that did not borrow before 2014Q1 and excludes Lava Jato firms. Lava Jato exposure, as defined in Table 1, is a dummy that equals one if the bank is in the upper tercile of the distribution of the continuous exposure and zero otherwise. Post is a dummy variable that switches to one in the period after 2014Q1; the first quarter of 2014, in which the Operation Lava Jato started, is omitted. Bank controls are Credit/Assets, Ln(Total Assets), Capital, Non-performing loans, Liquidity and Return on Assets lagged by one period. Standard errors (in parentheses) are clustered at the bank-level. Standard errors (in parentheses) are clustered at the bank-time level. ***, **, * denote statistical significance at the 1%, 5% or 10% level.

Dep. Var.:	(1) *New Loans [dummy]*	(2) *New Loans [dummy]*	(3) *New Loans [dummy]*	(4) *New Loans [dummy]*	(5) *New Loans [dummy]*	(6) *New Loans [dummy]*
Lava Jato Exposure × Post	0.0196***	0.0223***	0.0236***	0.0294***	0.0379*	0.0404*
	(0.00265)	(0.00321)	(0.00447)	(0.00508)	(0.0207)	(0.0239)
Post x State owned banks			-0.00425	-0.00279	-0.00894	-0.0115
			(0.00529)	(0.00447)	(0.0127)	(0.0155)
Lava Jato Exposure × Post × State owned Banks			-0.0203***	-0.0329***	-0.0633***	-0.0446**
			(0.00683)	(0.00667)	(0.0207)	(0.0225)
Bank controls $_{t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time-FE, Bank-FE	Yes	No	Yes	No	Yes	No
Firm-Bank-FE, Time-FE	No	Yes	No	Yes	No	Yes
Earmarked lending	Yes	Yes	Yes	Yes	No	No
Free market lending	No	No	No	No	Yes	Yes
Inclusion of state-owned banks?	No	No	Yes	Yes	Yes	Yes
Number of observations	59,851	62,244	80,798	80,917	98,313	100,806
Adjusted-R^2	0.121	0.110	0.103	0.110	0.119	0.146

4.7. Conclusion

We investigate the spillover effects of anti-corruption investigations at the firm-level and how it impacts the access to credit of non-affected firms in Brazil. Using a unique dataset provided by the Central Bank that matches quarterly firm-bank new loans with bank characteristics and macroeconomic outcomes related to firms' region of location, we analyze the impact of unexpected and exogenous anti-corruption investigations on the reallocation of credit to other big firms from January 2013 to July 2016.

We find consistent evidence that there is a negative spillover and, maybe, unintended side effects in the credit market. On average, non-affected firms obtain less credit at the intensive margin, while politically connected and from the construction sector are more likely to receive credit at the extensive margin. Furthermore, given the credit shrinkage, firms indirectly exposed to Lava Jato also decrease their employment rates and wage bills in the post period. We also show that the incentives that drive the reallocation may diverge according to macroeconomic characteristics, as well as proximity to big metropolises in more developed areas.

Our findings suggest that corruption spillovers to non-affected firms though credit market channels, negatively impacting their ability to raise capital in financial markets and leading to negative real effects. This double-faced response is probably due to several unconcluded investigations and to the level of uncertainty that considerably increased in banking lending behavior. First, this anti-corruption investigation addressed in this paper had exactly 80 phases and lasted seven years (March 2014 – February 2021). In addition to that, we had two abrupt changes of the President of Brazil, which certainly also corroborated to the high level of political and economic uncertainty in the country. This view is plausible and clear with hindsight from 2021 but was still a blur on the ongoing of the operations.

To conclude, this paper has immediate policy implications for both banks and firms affected by anti-corruption investigations around the world. For banks, we show that it is important that supervisors and regulators acknowledge unintended effects in bank-lending activities like the one herein addressed in this chapter. For firms, our findings highlight various nuances about how spillovers in the market through credit market networks indirectly affects firm performance and labor market outcomes. We also acknowledge that our results are likely to underestimate the full effect. On the one hand, there might be negative multiplier effects through supply chain networks that are partially due to official "corporate debarment" (blacklisting), which pretty much excluded Lava Jato firms from the economy. On the other hand, there might have been positive competitive effects, such as those reported in previous sessions showing that non-corrupt construction firms benefited from the reallocation herein addressed. Nevertheless, further research exploring other spillover effects of anti-corruption investigations and how corruption in firms spillovers to other non-affected firms can help paint a more comprehensive picture needed to evaluate these and other international efforts to curb corruption.

Appendix 2.1. Listing segments in the Brazilian stock market

	Novo Mercado	Nível 2	Nível 1	Traditional
Share Capital	Only common shares	Common and preferred shares (with additional rights)	Common and preferred shares (as per legislation)	Common and preferred shares (as per legislation)
Minimum percentage of outstanding shares (free float)	25% or 15%, if the ADTV (Average Daily Trading Volume) is above R$25 Million	25%	25%	There is no specific regulation
Public Offering of shares	Share dispersion efforts, except for offers pursuant to CVM'S Instruction 476	Share dispersion efforts	Share dispersion efforts	There is no specific regulation
Prohibition to statutory provisions	Voting limitation of less than 5% of the voting capital, qualified quorum and "immutable clauses"	Voting limitations of less than 5% of the voting capital, qualified quorum and "immutable clauses"	There is no specific regulation	There is no specific regulation
Composition of the board of Directors	Minimum of 3 members (pursuant to Brazilian Corporations Law), of which ate least 2 or 20% (whichever is greater) must be independent with unified term of up to 2 years	Minimum of 5 members, of which at least 20% must be independent with unified term of up to 2 years	Minimum of 3 members (pursuant to Brazilian corporations Law), with unified term of up to 2 years	Minimum of 3 members (pursuant to Brazilian Corporation Law)
Prohibition of cumulation of positions	Chairman of the Board of Directors and Chief Executive Officer or Main Officer by the same person. In Case of vacancy the result in cumulation of position, it is obligatory the disclosure of certain information and the compliance with deadline to the regularization	Chairman of the Board of Directors and Chief Executive Officer or Main Officer by the same person (a grace period of 3 years from accession)	Chairman of the Board of Directors and Chief Executive Officer or Main Officer by the same person (a grace period of 3 years from accession)	There is no specific Regulation
Board of Directors Duties	Statement of any public tender offer for the acquisition of shares issued by the company (with minimum requirements, including alternatives to the tender offer available on the market)	Statement on any public tender offer for the acquisition of shares issued by the company (with minimum requirements)	There is no specific Regulation	There is no specific Regulation
Financial Statements	As per legislation in force	Translated into English	As per Legislation in force	As per Legislation in force
Disclosure in English simultaneously with the disclosure in Portuguese	Material information or Benefit distribution information (notice to shareholders or notice to the market) and results press releases	There is no specific regulation besides the financial statements (see item above)	There is no specific Regulation	There is no specific regulation
Annual Public meeting	Public meeting (in-person or by any other means that allow remote participation) must be hold until 5 business days after the disclosure of the quarterly and annual financial statements about the information disclosed	Mandatory (in-person)	Mandatory (in-person)	Optional

Calendar of corporate events	Mandatory	Mandatory	Mandatory	Optional
Disclosure of additional information	Internal regulations of the Board of the Directors, it is Advisory Committees and the fiscal Council (if there is one) Code of Conduct (with minimum requirements) The following policies with minimum requirements (except the compensation Policy: (i) Compensation Policy, (ii) Nomination Policy of the board of the directors, Advisory Committees and Executive Management Board; (iii) Risk Management Policy; (iv) Related Party Transaction Policy; (v)Securities Trading Policy Disclosure of: (i) annual report of the statutory audit committee covering the points contained on the regulation; (ii) quarterly minutes of the Board of Director's meetings, informing the report by the non-statutory audit committee	Securities negotiation policy and code of conduct	Securities negotiation policy and code of conduct	There is no regulation
Tag-along rights	100% of common shares	100% of common and preferred shares	80% of common shares (as per legislation)	80% of common shares (as per legislation)
Delisting from the segment/Public Tender Offer	Compulsory Public Tender Offer, ate least for the fair price, with minimum acceptance quorum of 1/3 (or higher, as established in the bylaws) of the free float shareholders	Compulsory Public Tender Offer in case if registration canceling or segment exit	Not applicable	Not applicable
Becoming a Member of the Market Arbitration Chamber	Mandatory	Mandatory	Optional	Optional
Audit Committee	Mandatory setting up of an audit committee or statutory audit committee in compliance with the requirements set forth in the regulation	Optional	Optional	Optional
Internal Auditing	Mandatory setting up of an auditing department in compliance with the requirements set forth in the regulation	Optional	Optional	Optional
Compliance	Mandatory setting up of compliance, internal controls and corporate risks department. It is not allowed the accumulation of compliance and operational functions	Optional	Optional	Optional

Appendix 2.2. Sample composition

Company	From	To	Switch date	Announcement date	Year	ADRs
Bradesco	Traditional	N1GC	6/26/2001	6/27/2001	2001	Yes
CEMIG	Traditional	N1GC	10/17/2001	9/14/2001	2001	Yes
Gerdau	Traditional	N1GC	6/26/2001	6/25/2001	2001	Yes
Itau Unibanco	Traditional	N1GC	6/26/2001	6/22/2001	2001	Yes
Bradespar	Traditional	N1GC	6/26/2001	4/30/2001	2001	No
Random Participações	Traditional	N1GC	6/26/2001	-	2001	No
Itausa	Traditional	N1GC	6/26/2001	6/22/2001	2001	No
Alpargatas	Traditional	N1GC	7/15/2003	10/01/2002	2002	No
Tran Paulista	Traditional	N1GC	9/18/2002	-	2002	No
Celesc	Traditional	N2GC	6/26/2002	-	2002	No
Marcopolo	Traditional	N2GC	09/03/2002	-	2002	No
Sabesp	Traditional	NMGC	4/24/2002	20/04/2002	2002	Yes
Braskem	Traditional	N1GC	2/13/2003	3/21/2003	2003	Yes
Gerdau Metalúrgica	Traditional	N1GC	6/25/2003	05/09/2003	2003	Yes
Vale	Traditional	N1GC	12/12/2003	12/12/2003	2003	Yes
Cedro	Traditional	N1GC	10/02/2003	-	2003	No
Pão de Açúcar	Traditional	N1GC	4/29/2003	4/22/2003	2003	No
Fras-Le	Traditional	N1GC	11/11/2004	-	2004	No
Eletropaulo	Traditional	N2GC	12/13/2004	12/07/2004	2004	No
Eletrobras	Traditional	N1GC	9/29/2006	9/26/2005	2005	No
Cyrela	Traditional	NMGC	9/21/2005	4/17/2005	2005	Yes
Lojas Renner	Traditional	NMGC	07/01/2005	-	2005	Yes
Tractebel	Traditional	NMGC	11/16/2005	10/07/2005	2005	No
CESP	Traditional	N1GC	7/28/2006	07/07/2006	2006	Yes
Saraiva Livraria	Traditional	N2GC	04/07/2006	3/27/2006	2006	No
Embraer	Traditional	NMGC	06/05/2006	06/02/2006	2006	Yes
Banco do Brasil	Traditional	NMGC	6/28/2006	4/27/2006	2006	No
São Carlos	Traditional	NMGC	12/14/2006	9/29/2006	2006	No
Usiminas	Traditional	N1GC	10/11/2007	3/14/2007	2007	Yes
Banrisul	Traditional	N1GC	7/31/2007	05/07/2007	2007	No
IdeiasNet	Traditional	NMGC	1/15/2008	12/11/2007	2007	No
Indústrias Romi	Traditional	NMGC	3/23/2007	5/15/2007	2007	No
Portobello	Traditional	NMGC	4/30/2008	12/28/2007	2007	No
Copel	Traditional	N1GC	05/07/2008	4/30/2008	2008	Yes
Magnesita	Traditional	NMGC	04/02/2008	-	2008	No
Tarpon Invest	Traditional	NMGC	5/26/2009	-	2009	No
CEEE-D	Traditional	N1GC	12/23/2010	12/22/2010	2010	No
CEEE-GT	Traditional	N1GC	12/23/2010	12/22/2010	2010	No
Eucatex	Traditional	N1GC	07/01/2010	4/19/2010	2010	No

Ferbasa	Traditional	N1GC	01/07/2011	01/05/2011	2011	No
Inepar	Traditional	N1GC	3/31/2011	3/30/2011	2011	No
Forja Taurus	Traditional	N2GC	07/07/2011	07/05/2011	2011	No
Tim Participações	Traditional	NMGC	08/03/2011	05/05/2011	2011	Yes
Metal Leve	Traditional	NMGC	07/05/2011	-	2011	No
Oi	Traditional	N1GC	12/17/2012	7/25/2012	2012	Yes
Tupy	Traditional	NMGC	10/17/2013	1/23/2013	2013	No
Energisa	Traditional	N2GC	7/28/2016	9/22/2015	2015	No
Rossi Residencial	N1GC	NMGC	1/27/2006	12/21/2005	2005	No
BRF	N1GC	NMGC	04/12/2006	2/17/2006	2006	Yes
WEG	N1GC	NMGC	6/22/2007	3/15/2007	2007	Yes
Cia Hering	N1GC	NMGC	5/16/2007	03/09/2007	2007	No
Sofisa	N1GC	N2GC	12/08/2008	12/08/2008	2008	No
IOCHP-Maxion	N1GC	NMGC	3/24/2008	1/17/2008	2008	Yes
Fibria	N1GC	NMGC	5/20/2010	4/30/2010	2010	Yes
Daycoval	N1GC	N2GC	5/16/2013	12/02/2011	2011	No
Indusval	N1GC	N2GC	03/01/2012	12/22/2011	2011	No
Pine	N1GC	N2GC	3/29/2012	12/06/2011	2011	No
Ultrapar	N1GC	NMGC	8/17/2011	04/04/2011	2011	Yes
Paranapanema	N1GC	NMGC	2/15/2012	12/14/2011	2011	No
Klabin	N1GC	N2GC	01/09/2014	06/12/2013	2013	Yes
Eternit	N2GC	NMGC	8/17/2006	06/02/2006	2006	No
Equatorial	N2GC	NMGC	4/23/2008	02/12/2008	2008	Yes
Estacio Participações	N2GC	NMGC	07/11/2008	-	2008	Yes
All	N2GC	NMGC	10/22/2010	09/09/2010	2010	No
Kroton	N2GC	NMGC	12/05/2012	11/13/2012	2012	Yes
Somos Educa	N2GC	NMGC	10/23/2014	08/07/2014	2014	No
Contax	N2GC	NMGC	7/21/2016	1/22/2016	2016	No
Santos Brasil	N2GC	NMGC	8/22/2016	7/27/2016	2016	No

Appendix 2.3. Definition of variables used in chapter 2

Variable	Definition
Daily stock price	Daily closing price of stocks.
AR	Abnormal return = raw stock return adjusted by the expected return calculated from the market model using the Ibovespa return.
Firm size	Logarithm of Total Assets.
Firm age	Number of trading days since the firms' IPO.
Investments/Equity	Capital expenditures (net increase in property, plant and equipment + depreciation) / Equity.
Z-score	Altman Z-score = 6.56*(Working Capital/Total Assets) + 3.26*(Retained earnings/Total Assets) + 6.72*(EBIT/Total Assets) + 1.05*(Market Value of Equity/Book Value of Liabilities).
Novo Mercado	Dummy variable that equals one for the Novo Mercado, and zero otherwise.
Post	Dummy variable that equals one for the post segment switch announcement period, and zero otherwise.
After 2008	Dummy variable that equals one for the period after the Financial Crisis of 2008, and zero otherwise.
Return Ibovespa	Return of the market index Ibovespa.

Appendix 3.1: Definition of variables used in chapter 3

Notes. This table presents the definition of the main variables used in this paper and summarized in Table 3.1.

Variables	Definition
Loan amount characteristics	
New loans $_{it}$	Amount of new loans to firm i in quarter t. The panel includes zeros for missing new loans if the firm has outstanding credit in that bank-quarter.
Loan terms characteristics	
Loan Loss Provision $_{it}$	Banks' non-cash expense to account for future losses due to loan defaults.
Maturity $_{it}$	Number of months until the entire unpaid balance of the loan, including principal and interest, is due and payable.
Firm rating $_{it}$	Credit rating on a scale from AA to C assigned by banks based on the credit quality of borrowers (AA, A, B or C). 1 denotes AA and C denotes 4.
Interest rate $_{it}$	Fixed interest rate of the loan.
Firm characteristics	
Volume of donations (R$)	Amount donated by the firm to Brazilian federal elections of 2010 normalized by firms' number of employees.
Number of employees	Number of employees at the end of 2012.
Mean wage	Firm wage bill divided by the number of employees at the end of 2012.
Age	Firm age (in years) at the end of 2012.
Number of bank relationships	Average number of bank relationships per firm in 2012.

Appendix 3.2: Synthetic Lava Jato donor industries weights

CNAE	Industry	Weight
20	Manufacturing of chemicals	.077
29	Manufacturing of motor vehicles, trailers and bodywork	.087
31	Furniture manufacturing	.146
45	Trade and repair of automotive vehicles and motorcycles	.254
91	Activities related to cultural and environmental heritage	.364
93	Sports, recreation and leisure activities	.072
Sum		1.000

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