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Impact of Federal Audits on Procurement Expenses in São Paulo, Brazil: A Municipal Level Analysis

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Résumé

Cette étude examine l'impact des audits fédéraux sur les dépenses liées aux marchés publics dans les municipalités de l'État de São Paulo, au Brésil, entre 2008 et 2017. En exploitant des données sur les marchés publics au niveau municipal, disponibles exclusivement pour São Paulo, elle permet une évaluation détaillée des effets des audits. Dans les municipalités traitées, seuls des contrats spécifiques sont audités, sélectionnés sur la base de critères sectoriels, formant ainsi le troisième groupe central de l'analyse. En appliquant une méthodologie de Différence-en-Différence-en-Différences (DDD), l'étude examine si l'inclusion de ce troisième groupe modifie les conclusions précédentes, qui avaient systématiquement identifié des impacts négatifs des audits sur les dépenses publiques. Les résultats montrent que les audits réduisent généralement les dépenses dans l'ensemble des municipalités traitées. Toutefois, l'inclusion du troisième groupe révèle un effet positif compensatoire sur les dépenses des catégories de contrats spécifiquement audités, ce qui pourrait refléter une meilleure qualité des contrats ou une priorisation stratégique dans les secteurs ciblés. Soutenues par un examen approfondi de la stratégie anticorruption du Brésil, des critères d'audit de la CGU et du contexte économique et politique de São Paulo pendant la période étudiée, ces conclusions soulignent l'importance de stratégies d'audit adaptées et de réformes complémentaires pour maximiser les bénéfices systémiques des audits dans les marchés publics.

Mots clés : marchés publics, données au niveau municipal, audits de la CGU, dépenses municipales, différence-en-différence-en-différences (DDD).

Abstract

This study examines the impact of federal audits on public procurement expenses in municipalities within the State of São Paulo, Brazil, between 2008 and 2017. Leveraging municipal-level public procurement data, uniquely available for São Paulo, it enables a detailed assessment of audit effects. In treated municipalities, only specific contracts are audited, selected based on sector-specific criteria, thus forming the third group central to the analysis. By applying a Difference-in-Difference-in-Differences (DDD) methodology, the study investigates whether including this third group alters prior findings, which consistently reported negative impacts of audits on public expenses. The results show that audits generally reduce expenditure across treated municipalities. However, the inclusion of the third group reveals a countervailing positive effect in expenses from the specific audited contract categories, potentially reflecting higher-quality contracts or strategic prioritization in targeted sectors. Supported by a detailed review of Brazil's anti-corruption strategy, CGU audit criteria, and São Paulo's economic and political context during the study period, these findings underscore the importance of tailored audit strategies and complementary reforms to enhance the systemic benefits of audits in public procurement.

Keywords: public procurement, municipal-level data, CGU audits, municipal expenses, difference-in-difference-in-difference (DDD).

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List of abbreviations and acronyms

CGU	Comptroller General Office of the Union in Brazil
DiD	Difference-in-Difference
DDD	Difference-in-Difference-in-Difference
FE	Fixed-Effect
GDP	Gross Domestic Product
IBGE	The Brazilian Institute of Geography and Statistics
IV	Instrumental Variables
OLS	Ordinary Least Square
RE	Random-Effect
RDD	Regression Discontinuity Design
TSE	The Superior Electoral Court of Brazil
VIF	Variance Inflation Factor

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Introduction

Unbiased federal audit processes are globally recognized as a crucial mechanism to deter corruption, enhance transparency, and promote cost-effective procurement practices. Their significance lies in fostering accountability and ensuring the efficient allocation of public funds. For instance, audits conducted by the Defense Contract Audit Agency (DCAA) in the United States have led to substantial cost savings in Department of Defense (DoD) procurement contracts (Arnold & Porter, 2022). Similarly, the European Union's procurement audits, initiated in response to the COVID-19 crisis, focused on securing the best value in public procurements while mitigating risks of fraud and corruption (EIPA, 2020). Globally, federal audits have consistently demonstrated their impact in reducing corruption, streamlining procurement processes, and ultimately leading to lower public expenditure.

In Brazil, the role of federal audit programs in reducing corruption and public expenses has been extensively studied. Avis, Ferraz, & Finan (2018) demonstrated how federal audit reports exposing corrupt practices, such as procurement fraud and over-invoicing, effectively reduced inflated pricing and kickbacks. This reduction is attributed to a "disciplining effect," where local mayors face heightened legal and electoral consequences, particularly when audit findings are disseminated through local media. Litschig and Zamboni (2015) further emphasized the importance of judicial accountability, showing that municipalities with active judicial oversight experience significant declines in corruption-related activities, including contract overpricing. Similarly, Ferraz, Finan, and Moreira (2012) highlighted how audits uncover significant misuse of funds, triggering corrective measures that improve resource allocation and curtail inflated contract costs.

Building on these findings, this study utilizes municipal-level procurement data from São Paulo State, Brazil, covering the period from 2008 to 2017, to enhance understanding of the varied effects of audits on the municipal spending across different conditions. The analysis assesses the effects of audits on procurement practices, offering a detailed evaluation through the application of a Difference-in-Difference-in-Differences

(DDD) analysis. By incorporating a third group into the analysis, the study provides novel insights into the nuanced effects of audits, uncovering trends and dynamics that shed light on their broader implications across different municipal contexts.

The dataset encompasses audits applied to specific sectors within municipalities, determined by population size, meaning that not all public contracts in audited municipalities were examined. This design allows for a granular analysis of sector-specific audit impacts. The findings suggest that audits were associated with a 36% increase in the municipal spending in audited sectors of treated municipalities, a result that contrasts with prior findings and provides a unique perspective facilitated by the detailed procurement data from São Paulo. This counterintuitive outcome highlights the importance of sectoral and contextual factors in understanding the broader implications of audit interventions.

The study is organized into three chapters. Chapter 1 introduces Brazil's anti-corruption program, detailing the Comptroller General Office of the Union, or *Controladoria Geral da União* (CGU)[1] audit criteria and providing an overview of the economic and political context during the study period. Chapter 2 describes the dataset and presents descriptive statistics to contextualize the analysis. Chapter 3 outlines the methodological framework, specifying the variables and models employed, and presents empirical results alongside robustness checks. Finally, the study concludes by discussing its findings, and avenues for future research.

This research contributes to the existing literature by expanding the understanding of federal audit impacts and providing actionable insights for policymakers aiming to enhance transparency, accountability, and efficiency in public procurement processes.

[1]The Controladoria-Geral da União (CGU), Brazil's central federal government body, is responsible for safeguarding public assets and promoting transparency in public administration through internal control, public auditing, disciplinary oversight, ombudsmanship, and anti-corruption measures. Additionally, the CGU provides technical supervision and normative guidance to the systems of Internal Control, Disciplinary Oversight (Siscor), Ombudsmanship (SisOuv), and Public Integrity (Sipef) within the federal Executive Branch of Brazil (Controladoria-Geral da União, n.d.).

Literature review

This section reviews previous studies that analyzed the reliability and effectiveness of audits conducted by CGU in reducing corruption in Brazil, highlighting its critical role in addressing governance issues. It also considers studies that explored the impact of federal audits on reducing corruption internationally, providing a broader context. The discussion focuses specifically on the influence of federal transfers on political behavior and corruption in Brazilian municipalities, with particular attention to the effects of CGU audits on public expenses. Additionally, the section outlines the methodologies used in these studies and summarizes their key findings.

Avis, Ferraz, & Finan (2018) examined the impact of CGU audits on corruption using a difference-in-difference (DiD) approach. Their study analyzed changes over time between municipalities that had been audited and those that had not. Municipalities previously audited were classified as the treatment group, while those undergoing audits for the first time formed the control group. By controlling for time-invariant characteristics and common trends, the analysis revealed that audits led to a 7.9% reduction in corrupt practices, such as procurement fraud and over-invoicing, ultimately resulting in lower public expenditures.

Their study also enhances the effectiveness of CGU audits in reducing corruption through increased political and judicial accountability, by causing a disciplinary effect in the local authorities mainly due to the presence of local media, that widely disseminates information revealed in the audits.

Another study that used a similar approach, also with a DiD instrument, was conducted by Litschig and Zamboni (2015). They assessed the effects of CGU audits from May 2009 and May 2010. In May 2009, the CGU informed 120 municipalities that 30 of them would be randomly chosen for an audit in one year.

They aimed to measure the role of judicial accountability in conjunction with CGU audits on reducing corruption. The temporary increase in audit risk led the treatment decreased the proportion of local procurement processes involving waste or corruption by approximately 20%.

Ferraz, Finan, and Moreira (2012) also used a DiD methodology to compare municipalities before and after audits to demonstrate the impact of exposing corruption on the allocation of educational funds. The authors enhanced how CGU audits expose significant mismanagement and embezzlement of funds intended for education, leading to corrective actions and improved resource allocation.

Another similar approach was adopted by Lichand, Lopes, & Medeiros (2016) that also investigate the impact of CGU audits with specific emphasis on the health sector. They employed a DiD analysis to compare changes over time between municipalities that were audited and those that were not and used IV (Instrumental Variables) to address potential endogeneity that might affect the likelihood of being audited but are unrelated to health outcomes.

The study explores a spillover effect, by assessing whether corruption levels are lower in municipalities adjacent to those that had been audited before. They observe a 5.4% reduction in health-related corruption in municipalities neighboring an audited area. They also highlight that this reduction in corruption enhances healthcare delivery and decreases costs associated with medical supplies and services, hence concluding that CGU audits have a broader social benefit, going beyond of just financial savings.

From a different perspective, Colonnelli & Prem (2022) also used a DiD approach to compare changes over time between municipalities that experienced CGU audits with those that did not and concludes that federal audits are effective in reducing corruption and inefficiencies in public procurement, leading to more competitive bidding and lower contract expenses. They study the impact of political alignment between municipal governments and federal authorities on public funding and auditing outcomes in Brazil. They used a RDD (Regression Discontinuity Design) to leverage the exogenous variation in political alignment generated by close elections to study the effect of political alignment on public procurement and audit outcomes.

They found that the political alignment between municipal governments and federal authorities significantly increases the likelihood of receiving federal funds and decreases the likelihood of being audited. They also assessed that the reduction in contract expenditure was particularly significant in municipalities that were previously politically aligned with the federal government, indicating that audits restrict favoritism and collusion. In addition, they noted that after the audits, there is a growth in the number of firms within sectors heavily reliant on government interactions and public procurement, as well as an increase in local real economic activity.

Following the same methodology, Ferraz & Finan (2008) used RDD to explore the random assignment of municipalities to CGU audits, to identify causal effects on electoral outcomes. The authors proved that the disclosure of corruption through audits significantly reduced the re-election probabilities of implicated politicians. Notably, the study shows no evidence suggesting that auditors manipulate audit reports, underscoring the credibility and impartiality of the auditing process. This finding highlights the effectiveness of CGU audits in promoting good governance and electoral accountability.

Another study that sustains the multiple roles of CGU in enhancing democracy and governance in Brazil was provided by Loureiro, Teixeira, & Moraes (2012). Through qualitative analysis and case studies, the authors discuss the impacts of CGU audits on public sector performance and governance by improving accountability and transparency, leading to more efficient public spending.

The autonomy and effectiveness of CGU within Brazil's federal government is highly explored by Bersch, Praça, & Taylor (2016). The study also uses case studies and qualitative analysis to present how the CGU operates as an anti-corruption body and its capacity to function independently of political influences. The study finds that the CGU is one of the most autonomous and least politicized government agencies, which allows it to effectively perform its role in monitoring and auditing public expenditures. The researchers highlight that this high level of autonomy is critical for the CGU's success in uncovering and addressing corruption, thereby contributing to greater governmental transparency and accountability.

Brollo, Nannicini, Perotti, & Tabellini (2013) used RDD to explore discontinuity in the allocation rule for federal transfers to municipalities based on population thresholds. This method helps identify the causal impact of increased transfers on corruption and political behavior by comparing municipalities just below and just above the population threshold, to verify how increased transfers influence the electoral success of incumbent mayors and the subsequent allocation of resources.

The authors conclude that increased fiscal transfers from the federal government to municipalities lead to higher levels of corruption, misallocation of resources, therefore inefficient public spending, thus harming economic development. In addition, they proved that mayors who receive larger transfers are more likely to be re-elected, suggesting that voters reward incumbents for increased spending without necessarily considering the efficiency of that spending.

Outside Brazil Di Tella & Schargrodsky (2003) used DiD to estimate the impact of anti-corruption audits and wage increases on procurement prices of basic inputs in public hospitals in Buenos Aires, comparing the changes over time between hospitals subjected to intensive audits and those that were not. They used a FE (fixed-effect) for hospitals and time periods in their regression models to control for unobserved heterogeneity, and in some specifications the authors use IV to address potential endogeneity issues related to the intensity of audits and wage increases.

They assessed that the prices paid for basic inputs decreased by 15% during the first 9 months of the audits. Prices remained 10% lower even after audit intensity decreased. Higher wages did not significantly reduce input prices during periods of maximal audit intensity but had a noticeable effect when audit intensity was moderate, therefore indicating that both higher wages and auditing are necessary to effectively combat corruption.

Clark, Coviello, Gauthier, and Shneyerov (2018) also used DiD to investigate the effects of anti-collusion measures on public procurement in Quebec's construction industry. The DiD compares the changes in public procurement outcomes before and after the anti-collusion investigation, using regions not affected by the investigation as a control group, and IV to address potential endogeneity by using instruments that affect the likelihood of investigation but are unrelated to procurement outcomes.

The authors showed that the crackdown significantly disrupted the cartel operations, leading to increased competition in public procurement auctions, resulting in a substantial reduction in bid prices for public contracts, hence demonstrating the effectiveness of anti-collusion measures. The study also found that the presence of a cartel deterred new firms from entering the market, whether breaking up the collusion led to increased market competition. The findings highlight the importance of robust anti-collusion policies and enforcement in maintaining competitive public procurement markets and reducing corruption.

Hood et al., (1998) study the impact of audits on public contract expenses. They used a qualitative analysis combined with study cases to assess that audit in various OECD (Organisation for Economic Co-operation and Development)[2] countries, including the UK (United Kingdom) and Australia, have led to significant reductions in procurement fraud and contract expenditure, by enforcing stricter compliance and transparency measures within public sector contracts. Using the same methodology, Jacobs (1998) investigated the impact of value-for-money audits in New Zealand on public sector procurement and pricing, finding that value-for-money audits helped reduce corruption in public contracts by ensuring better oversight and accountability, leading to more competitive bidding processes and lower public expenditures.

These studies collectively highlight the effectiveness of audits and anti-collusion measures in reducing corruption, improving competition, and lowering public expenditures in various contexts worldwide, from Brazil to Canada, Argentina, New Zealand and various OECD countries.

[2] The OECD is an international organization that promotes policies aiming to improve the economic and social well-being of people around the world. Member countries are typically high-income economies with a high Human Development Index (HDI) and are considered to be developed countries.

Chapter 1
CGU Anti-Corruption Strategy and Contextual Overview

This chapter provides a comprehensive examination of Brazil's anti-corruption efforts, focusing on the role of the CGU in combating corruption at the municipal level. It begins by detailing the CGU's initiatives, including the Public Lottery Monitoring Program, a pioneering strategy that combines randomized audits with robust oversight mechanisms to enhance transparency and accountability. The chapter further explores the program's evolution, scope, and significant outcomes, highlighting its role in addressing corruption in public procurement processes.

The second section offers a contextual analysis of the economic and political landscape in Brazil and São Paulo from 2008 to 2017, a period marked by economic volatility, political turbulence, and major corruption scandals. This background establishes a critical foundation for understanding the factors influencing public expenditures and the effectiveness of anti-corruption strategies during this period, with a particular focus on São Paulo's unique position within the national framework.

1.1 CGU and Brazil anti-corruption crackdown strategy program

Corruption in Brazil has been endemic widespread by embezzlement and misallocation of federal funds destined for local development projects. Municipalities receive substantial federal transfers annually to improve public services and infrastructure. However, the decentralized political system and insufficient oversight easily contribute for local authorities to engage in corrupt practices such as procurement fraud, over-invoicing, and fund diversion. Continuous efforts such as the implementation of audits and strengthening of both political and judicial accountability mechanisms have shown promise in reducing corrupt activities and promoting transparency (Ferraz and Finan, 2018).

The CGU was created in 2001 as an anti-corruption agency with an autonomous functionality and attributions formally published by the law 10.683 from May 2003. The same law also launched the Public Lottery Monitoring Program (*Programa de Fiscalização por Sorteios Públicos*), conducted by CGU, consisting of audits in municipalities selected by randomized draws, which followed the same rule of the public lotteries in Brazil to enhance transparency and accountability in the process.

Initially the program aimed to supervise public resources application in municipalities with a population under 500,000 inhabitants, with the exception of state capitals, hence covering 99% of Brazilian municipalities.[3] On this first phase, the program had 40 rounds from April 2003 to February 2015, randomly selecting 2,229 municipalities and following an exception replacement rule: municipalities selected on the three previous rounds should not be eligible for the specific draw.[4]

The initial two rounds of the program selected 5 and 26 municipalities, respectively. From the third to the ninth rounds, 50 municipalities were randomly selected in each draw. Subsequently, from round 10 to round 40, the number of municipalities selected per draw increased to 60. Over time, the frequency of draws per year steadily declined. In 2003 and 2004, the program conducted seven rounds annually, which dropped to five rounds in 2005. Between 2006 and 2010, the program averaged three draws per year, with the exception of 2008, which had only two. Similarly, 2011 and 2012 each featured two draws, while 2013, 2014, and 2015 saw just one draw per year.

From rounds 1 to 22, spanning 2003 to 2006, audits were conducted across all sectors, with the sample size varying based on the population of the municipalities. In April 2005, the CGU introduced a sector-specific approach to its audits, tailoring the selection of public contracts to the population size of the municipalities. These criteria were disclosed only on the day of each draw to ensure transparency and accountability. For example, in Round 23 (May 2007), municipalities with populations under 20,000 had public contracts from all sectors audited. In contrast, municipalities with populations

[3] From a total of 5,570 municipalities in Brazil only 49 are above 500,000 inhabitants, on which 23 are state capitals (Brazil has 26 states), based on the Research Estimated Municipalities Population for 2019 (*Estimativas da População dos Municípios 2019*), published by Brazilian Institute of Geography and Statistics, IBGE (*Instituto Brasileiro de Geografia e Estatística)* in 2020 (Agência Brasil, 2020).

[4] With the exception of round 32 from May 2010, when 30 Municipalities were selected from a control group of 120 that were drawn one year before, in May 2009.

between 20,000 and 100,000 had contracts audited from specific sectors such as education, health, social assistance, public security, and industry. Larger municipalities, with populations between 100,000 and 500,000, were audited only for contracts in education, public security, and industry (Controladoria Geral da União, 2007).

Table A.1, Table A.2 and Table 1 provide a detailed overview of the number of municipalities randomly selected in each round of the CGU Public Lottery Monitoring Program, both across Brazil and specifically within the state of São Paulo. The tables also outline the population size criteria used for sampling and highlight the sectors audited after the program introduced sector-specific criteria in 2005. These criteria varied by population size, with smaller municipalities having audits across all sectors, while larger municipalities were subject to audits in targeted sectors. The progression of rounds reflects adjustments in the program's focus and methodology over time, emphasizing its adaptability in addressing corruption risks in diverse contexts.

The present study focuses on specific public contracts from municipalities in the State of São Paulo during the period from 2008 to 2017, analyzing audits conducted between May 2009 and February 2014. This timeframe encompasses rounds 28 to 39 of the CGU Public Lottery Monitoring Program, as highlighted below in Table 1, totaling 11 rounds. Notably, round 36, scheduled for July 2012, was excluded because all audits in São Paulo municipalities were canceled by a formal decision under *Portaria* 1713, issued on August 8, 2012. The selection of these rounds is particularly significant, as they incorporate both sector-specific and population-based criteria, which were key variables in the study's model. Round 40 was excluded from the analysis, and the municipalities audited in this round were entirely removed from the dataset to accurately assess the impact of audit processes. This round was limited to municipalities with populations under 100,000 and included audits across all sectors, diverging from both the criteria applied in previous rounds and the scope of the study.

TABLE 1: CGU PUBLIC LOTTERY PROGRAM ROUNDS 26 TO 40, SAMPLE CRITERIA AND AUDITED SECTORS.

Round	Total of municipalities selected in Brazil	Total of municipalities selected in São Paulo	Draw Date	Sample criteria based on municipalities population size (% share from total municipalities in São Paulo State)				
				under 20,000 (60%)	from 20,000 to 100,000 (28%)	from 100,000 to 500,000 (11%)	under 50,000 (79%)	from 50,000 to 500,000 (20%)
26	60	5	04/30/2008	all sectors	Education, Health, Social Assistance, Commerce, Services, Agriculture, and Culture.	Health, Commerce, Services, Agriculture, and Culture.		
27	60	5	10/29/2008	all sectors	Education, Health, Social Assistance, Agrarian Reform, Energy and Environment.	Social Assistance, Agrarian Reform, Energy and Environment.		
28	**60**	**5**	**05/12/2009**	**all sectors**	**Education, Health, Social Assistance, Housing, Sanitation, and Urbanism.**	**Education, Housing, Sanitation, and Urbanism.**		
29	**60**	**5**	**08/17/2009**	**all sectors**	**Agriculture, Social Assistance, Commerce, Culture, Education, Health, and Services.**	**Agriculture, Social Assistance, Commerce, Culture, and Services.**		
30	**60**	**5**	**10/05/2009**	**all sectors**	**Public Security, Industry, Science and Technology, Social Assistance, Education, and Health.**	**Public Security, Industry, Science and Technology, and Health.**		
31	**60**	**5**	**03/01/2010**	**all sectors**	**Public Security, Industry, Science and Technology, Social Assistance, Education, and Health.**	**Public Security, Industry, Science and Technology, and Health.**		
32	***60**	**3**	**05/10/2010**	**all sectors**	**Agriculture, Social Assistance, Commerce, Culture, Education, Health, and Services.**	**Agriculture, Social Assistance, Commerce, Culture, and Services.**		
33	**60**	**5**	**07/26/2010**	**all sectors**	**Public Security, Industry, Science and Technology, Social Assistance, Education, and Health.**	**Public Security, Industry, Science and Technology, and Health.**		
34	**60**	**6**	**08/15/2011**	-		-	**Health, Education and Social Development.**	**Education and Social Development.**
35	**60**	**6**	**10/03/2011**	-	-	-	**Health, Education and Social Development.**	**Health and Social Development.**
36	**60	0	07/23/2012	-	-	-	Health, Education and Social Development.	Education and Social Development.
37	*****60**	**6**	**10/08/2012**	-	-	-	**Health, Education and Social Development.**	**Health and Social Development.**
38	**60**	**6**	**03/04/2013**	-	-	-	**Health, Education and Social Development.**	**Education and Social Development.**
39	**60**	**6**	**02/17/2014**	-	-	-	**Health, Education and Social Development.**	**Health and Social Development.**
40	60	6	02/02/2015	all sectors		-	-	-

Round 36 is excluded from the study due to 36 Muncipalities had the audit cancelled by Portaria 1.713, from 10/08/2012, which affected all selected municipalities from São Paulo.
*30 Municipalities were selected from a control group of 120 that were drawn one year before. In São Paulo we have here only the municipalities not included in this control group.
**Audit could not be concluded in one municipality due to further investigation needs.

Source: created by the author based on Controladoria Geral da União (2024)

In August 2015, the program was renamed the Federal Entities Monitoring Program (*Programa de Fiscalização em Entes Federativos*), reflecting an expanded scope that went beyond municipalities to oversee the use of federal funds by various entities within the federation. Since its renaming, the program has adopted three distinct methodologies for selecting entities to audit: Lottery Draw, Census, and Vulnerability Matrix. The Census methodology involves auditing all entities to evaluate the regularity of resource application comprehensively. The Vulnerability Matrix employs data intelligence, analyzing key indicators to identify critical vulnerabilities and strategically select entities for auditing in specific regions. The Lottery methodology, carried over from the original Public Lottery Monitoring Program, continues to rely on randomized selection.

Figure A1 provides an overview of the rounds implemented since August 2015, illustrating the application of the program's three methodologies: Lottery Draw, Census, and Vulnerability Matrix. These methodological advancements highlight the program's continuous evolution in improving its strategies for detecting irregularities and promoting the efficient and transparent allocation of federal resources (Controladoria Geral da União, 2024).

Since its inception in 2003, the program has audited approximately 2,700 Brazilian municipalities, overseeing federal public resources exceeding USD 7 billion[5] (Controladoria Geral da União, 2024). Among the most significant cases uncovered is the Petrobras scandal, a far-reaching corruption investigation that exposed an intricate network of bribery and money laundering involving the state-owned oil company Petrobras. The scheme implicated numerous high-ranking politicians and business executives who orchestrated bribes in exchange for lucrative contracts. The investigation not only revealed systemic corruption but also recovered approximately USD 142 million[6] in misappropriated funds, highlighting the program's critical role in promoting accountability and transparency.

In São Paulo State, one of the most prominent corruption cases is the São Paulo Metro scandal, commonly referred to as the "Trensalão" scandal. This case involved a cartel of multinational companies, including Siemens and Alstom, which colluded to fix prices and pay bribes in exchange for securing contracts related to the construction, maintenance, and refurbishment of São Paulo's metro and commuter rail systems. The cartel manipulated bids for public transportation projects, leading to inflated costs and significant financial losses for the state. The investigation also uncovered a network of corruption implicating high-level officials and politicians from multiple political parties, further highlighting systemic issues within public procurement processes.

These examples underscore the pivotal role of the Public Lottery Monitoring Program in uncovering and addressing corruption at the municipal level, significantly enhancing transparency and accountability in the management of public funds. Bersch,

[5] Original amount R$40 billion (R$ - BRL – Brazilian Real). The exchange rate for 1 Brazilian Real to USD on July 29, 2024, was approximately 5.6551 BRL/USD (Federal Reserve Board, 2024)
[6] Original amount R$800 million (R$ - BRL – Brazilian Real). The exchange rate for 1 Brazilian Real to USD on July 29, 2024, was approximately 5.6551 BRL/USD (Federal Reserve Board, 2024)

Praça, & Taylor (2016) further evaluated the autonomy and effectiveness of the CGU within Brazil's federal government, examining its operation as an anti-corruption agency and its capacity to function independently of political interference. Their study concluded that the CGU is highly autonomous and minimally politicized, emphasizing that this independence is essential for the agency's effectiveness in combating corruption and promoting good governance.

1.2 Economic and Political Background in São Paulo and Brazil (2008-2017)

Between 2008 and 2017, Brazil underwent a period of pronounced instability marked by significant economic fluctuations and a turbulent political landscape. This era was characterized by periods of growth followed by sharp downturns, alongside widespread protests, high-profile corruption scandals, and governmental inefficiency. In contrast, while São Paulo State mirrored the nation's economic volatility, its political scenario was comparatively more stable. The state also played a critical role as a stronghold of opposition to the federal government, further distinguishing its political dynamics from the broader national context.

Figure 1 provides a comprehensive overview of the economic and political landscape in Brazil and São Paulo during the period of CGU audits, spanning from 2003 to 2020. It details GDP (Gross Domestic Product) variations for both Brazil and São Paulo, offering insights into periods of growth and recession. The figure also highlights the political background by presenting the ruling federal and state parties, along with their respective governors, illustrating the shifts in political leadership over time. Additionally, the timeline underscores the significant impact of Operation Car Wash (*Operação Lava Jato*), a landmark anti-corruption investigation initiated in 2014. This operation, which uncovered widespread corruption and scandals like the Petrobras case, had profound repercussions on Brazil's political and economic environment, particularly between 2014 and 2019. By integrating economic performance with political dynamics, Figure 1 underscores the interplay between governance, corruption, and economic stability during this critical period.

FIGURE 1: ECONOMIC AND POLITICAL BACKGROUND IN BRAZIL

Source: created by the author based on Banco Central do Brasil, n.d. and Fundação SEADE, n.d.

Brazil experienced a period of robust economic growth in 2008. However, the global financial crisis of 2008–2009 resulted in a negative GDP variation in 2009. This downturn was largely mitigated by 2010, driven by strong international demand for Brazilian commodities such as iron ore, oil, and agricultural products (Silva & Oliveira, 2019). Despite this initial resilience, Brazil entered an economic recession in the second half of 2013. The recession was primarily triggered by a slowdown in the Chinese economy, one of Brazil's major trading partners, which caused a significant decline in commodity prices. Compounding this, the economic policies of President Dilma Rousseff's administration (2011–2016), including increased public spending and tax cuts intended to stimulate growth, contributed to inflationary pressures and further destabilized the economy (Amadeo, 2023).

Rousseff's administration was defined by widespread public discontent, culminating in massive protests in 2013 against government corruption and inefficiency. This period also saw the emergence of Operation Car Wash (*Operação Lava Jato*), which uncovered extensive corruption networks involving numerous politicians and business executives. The investigation had significant political repercussions, including the release of its first report and the conviction of former President Luiz Inácio Lula da Silva in 2016 and 2017, respectively. These events severely eroded public trust in federal institutions and played a crucial role in Rousseff's impeachment in 2016, which was primarily based on charges of fiscal mismanagement.

Her successor, Michel Temer, introduced measures to stabilize the economy, such as labor reforms and efforts to control public spending. However, his administration was also plagued by corruption allegations, which undermined its credibility and limited the effectiveness of these reforms, resulting in only modest economic growth during this period.

During the study period, the state of São Paulo maintained its position as Brazil's economic powerhouse, contributing approximately 31% to the national GDP. The state's economy diversified significantly, with notable growth in the technology, finance, and service sectors, which helped cushion the effects of the broader national economic downturn. However, São Paulo was not immune to the challenges of the national recession that began in 2013, experiencing a slowdown in economic growth, rising unemployment, and increasing inflation (Holston, 2019).

In response to these challenges, the enforcement of fiscal responsibility laws became stricter starting in 2013. These laws, aimed at curbing public spending and borrowing, required municipalities to adopt austerity measures and reduce expenditures to comply with legal limits. The stricter enforcement of fiscal responsibility laws beginning in 2013 represents a potential confounding policy, as it may independently influence municipal spending patterns during the post-treatment period. While these measures sought to ensure fiscal discipline, they also added pressure to local governments already grappling with economic instability.

São Paulo served as a stronghold for the Brazilian Social Democracy Party (PSDB), which played a pivotal role in opposing the Workers' Party (PT) that dominated federal politics for much of this period. The political dynamic in São Paulo was emblematic of the broader national political landscape, characterized by intense rivalry and ideological tensions between these two major parties. This opposition underscored the state’s unique position in shaping political discourse and reflected the deep divisions that defined Brazilian politics during this time.

Overall, São Paulo's economic and political landscape from 2008 to 2017 was characterized by a dynamic mix of growth, economic diversification, and significant challenges arising from national economic crises and political instability. Despite these obstacles, the state demonstrated resilience through its efforts to manage urbanization, promote economic diversification, and maintain political stability. These efforts underscored São Paulo's pivotal role in shaping Brazil's broader socio-economic trajectory and mitigating the impacts of national turmoil.

Additionally, the significant impact of Operation Car Wash, which exposed widespread corruption in public procurement processes, led to heightened scrutiny, the suspension of public contracts, and the implementation of reforms. These developments not only slowed procurement activities toward the end of the study period but also enhanced the credibility and trustworthiness of the CGU's oversight mechanisms.

At the same time, Operation Car Wash introduces a potential confounding effect in this analysis. Although the investigation was national in scope, its indirect influence likely spilled over to municipal governments, where fear of prosecution or increased public scrutiny could have prompted voluntary adjustments in procurement and spending behavior. As a result, changes in municipal expenditures observed during the post-treatment period may, in part, reflect the effects of Operation Car Wash rather than solely the outcomes of federal audit interventions.

This background review is essential for the analysis, providing valuable context for understanding the variation in municipal expenditures in São Paulo during the study period, particularly across specific sectors. It also highlights the importance of considering potential confounding policies, such as broader fiscal reforms or anti-corruption efforts, that may have independently influenced spending patterns and procurement practices during the same period.

Chapter 2
Data

This chapter provides a comprehensive overview of the dataset and descriptive statistics that form the foundation of the study. It begins by detailing the data sources, the period analyzed, and the criteria applied to construct the final dataset. Additionally, key descriptive analyses of the main variables are presented to highlight the characteristics of the data that support the methodological framework and subsequent results. These analyses include trends in public procurement activities, sectoral distributions, and population-based contract patterns, which together offer valuable context for the interpretation of the findings in later chapters. The chapter underscores how the data preparation and descriptive exploration inform the study's methodological rigor and enhance the robustness of its conclusions.

2.1 Data Description

This study utilized a comprehensive database from the São Paulo State Court of Accounts (*Tribunal de Contas do Estado de São Paulo*), encompassing public contracts formalized through procurement processes across all municipalities in the state of São Paulo, Brazil. The dataset, which is publicly accessible online, provides detailed information on public contract expenditures and the specific types of procurement processes that generated these expenses, with records available starting from 2008 (Tribunal de Contas do Estado de São Paulo, n.d.).

The study also incorporated data from the CGU, encompassing all municipalities audited during Phases 1 and 2 of the CGU rounds (Controladoria Geral da União 2023). Additionally, data from the Brazilian Institute of Geography and Statistics (IBGE) were included, specifically the *Estimates of the Resident Population in Brazilian Municipalities with Reference Date of July 1, 2015* (Instituto Brasileiro de Geografia e Estatística, 2015). The study further utilized IBGE data on the GDP *per capita* of each municipality for the corresponding years (Instituto Brasileiro de Geografia e Estatística, n.d.), which was essential for comparing the economic potential of treatment and control groups. GDP *per* *capita* was also included as a control variable in the model to account for unobserved heterogeneity.

The study also incorporates data on the political party affiliation of municipalities for the corresponding years, serving as complementary information to enrich the analysis. This dataset was obtained from Brazil's Superior Electoral Court (Tribunal Superior Eleitoral, n.d.).

Table 2 provides concise definitions of the main variables in their original form before being transformed for use in the analysis. For example, the "value" variable, representing contract expenses, is described in its original format. However, in the study, a transformed variable, "l_value," was utilized, representing the natural logarithm of the original values. This transformation was deemed essential for the analytical framework and will be further detailed in Chapter 3.

TABLE 2: DATA VARIABLES DESCRIPTION – ORIGINAL FORM

Variables	Description	Origin
year	contract expense year	The São Paulo State Court of Accounts
municipality	municipality name where the contracted expense occurred	The São Paulo State Court of Accounts
fid_cont	awarded contracted (bidding winner) fiscal identification	The São Paulo State Court of Accounts
contracted	awarded contracted (bidding winner) name	The São Paulo State Court of Accounts
date_cont	contract expense date (DD_MM)	The São Paulo State Court of Accounts
value	contract expense value	The São Paulo State Court of Accounts
sector	expense destination sector	The São Paulo State Court of Accounts
origin	resources origin	The São Paulo State Court of Accounts
type_bid	type of procurement process that originated the expense and contract	The São Paulo State Court of Accounts

Variables	Description	Origin
id_action	expense action code, which defines the main reason of the expense	The São Paulo State Court of Accounts
status_exp	expense type	The São Paulo State Court of Accounts
contractor	contracting party or authority responsible for the expense in the municipality	The São Paulo State Court of Accounts
cycle_draw	cycle number of the selected audited municipality	CGU
date_draw	draw date of the selected audited municipality	CGU
pop	population size	IBGE
GDP_pc	Gross Domestic Product *per capita* of the municipality in current prices	IBGE
party_m	municipality government party	TSE
party_f	federal government party	TSE

The dataset focuses on expenses related to civil construction, renovations, works, and maintenance under the direct responsibility of municipal governments between January 1, 2008, and December 31, 2017. The analysis considers audits conducted from May 2009 to February 2014, specifically encompassing rounds 28 to 39. To accurately assess the impact of audit processes, municipalities audited in earlier rounds were excluded from the dataset, as were those selected in round 40 (2015), the first matrix audit in 2015, the random draw in 2016, and the matrix audit in 2017. These exclusions were implemented to eliminate any potential audit effects in the control group during the post-audit analysis period, ensuring the integrity of the comparison.

Municipalities with populations over 500,000 were excluded to align with the criteria of audit rounds 28 to 39, which define the treatment group. This also ensured a more homogeneous comparison between treated and control municipalities. Additionally, municipalities without at least one year of contract expense data were removed, resulting in a balanced panel. The final treatment group includes municipalities with fewer than

500,000 inhabitants that were audited for the first time in rounds 28 to 39. Importantly, none of these municipalities were audited again during the study period, in order to isolate the effect of the specific policy under analysis. The control group consists of similarly sized municipalities that were never audited, either before or during the study period.

Table 3 illustrates the impact of this sample selection applied to the dataset, showing the progression in the number of municipalities remained in the analysis. These selection criteria refined the dataset to exclude municipalities that were previously audited, had populations exceeding 500,000 inhabitants, or lacked contract expense observations for at least one year. The resulting treatment group consists of municipalities audited during the study period, while the control group includes municipalities without audits. Additionally, the column "Total of municipalities in the panel" in Table A3 provides a detailed breakdown of the remaining treated municipalities for each audit round, ensuring the analysis adheres to the specified criteria and maintains consistency across rounds.

TABLE 3: SAMPLE SELECTION IMPACT ON TREATMENT AND CONTROL GROUPS IN THE DATASET

	Treatment	Control	Total
Total number of municipalities in the State of São Paulo	58	587	645
Number of municipalities after excluding the ones already audited.	35	462	497
Number of municipalities after excluding the ones with population above 500,000 inhabitants, and without a contract observation in at least one year.	35	330	365

2.2 Descriptive Statistics

Figure 2 illustrates the trends in key characteristics of public contracts over the analysis period, including total expenses, average expenses per contract, the total number of contracts, and the number of unique awarded firms (referred to as Bid Winners). These metrics provide an overview of the evolution of procurement activity and expenditure from 2008 to 2017.

As discussed in the economic background section, the downturn in expenses and contracts beginning in 2013 can be attributed to a combination of immediate and preceding factors. These include the economic slowdown and the conclusion of federal government stimulus measures, which contributed to inflationary pressures and a decline in tax revenues.

However, expenses per contract experienced a slight reduction initially, followed by an upward trend after 2013. This reflects the stricter enforcement of fiscal responsibility laws introduced that year. Designed to control public spending and borrowing, these laws required municipalities to adopt austerity measures and implement stricter oversight of procurement processes. As a result, the number of contracts and awarded firms decreased, leading to more concentrated spending per contract.

Table 4 provides the main descriptive statistics for treatment and control groups over the entire study period, weighted by municipality and year. Table A4 complements this by presenting the same statistics for three key intervals: the two years prior to the audit period, the middle of the audit period, and the last two years, representing the post-audit period. The post period begins in the year immediately following the respective audit, a detail that will be further explained in Chapter 3 (Methodology).

Table 4 highlights distinct patterns between treatment and control groups. Treatment municipalities represent 9.59% of the total municipalities, account for 7.98% of total contracts, and 9.02% of total expenses. This suggests slightly lower contract engagement but comparable expenditure levels relative to control municipalities. While treatment municipalities have fewer contracts and awarded firms, their average expense per contract is statistically higher (BRL 63.92 thousand) than in control municipalities (BRL 55.92 thousand), indicating potentially costlier contracts, a characteristic that persists throughout the study period, as shown in Table A4.

FIGURE 2: TOTAL EXPENSES, CONTRACTS AND AWARDED FIRMS FROM 2008 UNTIL 2017

Source: created by the author on Stata 17

TABLE 4: EXPENSES, CONTRACTS AND AWARDED FIRMS STATISTICS FROM 2008 UNTIL 2017

	2008 -2017			
	Treatment	**Control**	**Total**	***t*-test diff**
% Total Municipalities	9.59	90.41	100	
% Total Contracts	7.98	92.02	100	
% Total Expenses	9.02	90.98	100	
Avg $ Expenses (BRL M)	13.64	14.60	14.51	-0.040328
Avg # Contracts	213.37	261.06	256.49	-0.035785
Avg #Awarded Firms	33.92	36.66	36.40	-0.104019
Avg $ Exp/Contr (BRL K)	63.92	55.92	56.56	0.0053871
Avg $ Exp/Firm (BRL K)	402.06	398.18	398.53	0.0074352
Avg # Contr/Firm	6.29	7.12	7.05	-130.3981
Avg $ Waived Exp (BRL M)	0.59	1.77	1.68	-45.89593
% Total Waived Contracts	31.36	37.16	36.70	
% Mun Same Party as Fed Gov	10.8	8.79	9.10	
% Transf Federal	78.58	78.65	78.64	
Population (Mean)	24,038	37,449	36,163	

Avg $ Expenses measures the average expenses by municipality, in Brazilian Reais million

Avg # Contracts measures the average number of contracts by municipality

Avg #Awarded Firms measures the average number of awarded firms by municipality

Avg $ Exp/Contr (BRL K) measures the average expenses per contract by municipality, in Brazilian Reais thousand

Avg $ Exp/Firm (BRL K) measures the average expenses per firm by municipality, in Brazilian Reais thousand

Avg # Contr/Firm measures the average number of contracts per awarded firms by municipality

Avg $ Waived Exp (BRL M) measures the average expenses of contracts that got waived from the bidding process, by municipality, in Brazilian Reais million

% Total Waived Contracts represents the relative percentage of contracts that got waived from the bidding process, by municipality

% Mun Same Party as Fed Gov represents the relative percentage of municipalities with the same political party as the federal government.

% Transf Federal represents the relative percentage of contracts that were funded by federal transferred funds

GDPpc (BRL K) represents the GDP *per capita* in current prices of all municipalities in the group for the respective year, in Brazilian Reais million.

Population (Mean) represents the average municipality population size in 2015.

t-test diff reports the t-statistic comparing means between treatment and control groups.

t-tests are not applied to percentage rows, as these reflect sample composition rather than continuous variables.

Additionally, treatment municipalities exhibit higher average expenses per awarded firm, although this metric fluctuates across the years. For instance, in 2012 and 2013, treatment municipalities experienced notable peaks in average expenses (BRL 20.04 million and BRL 14.26 million, respectively), surpassing those of control municipalities during these years.

Tables 4 and Table A4 also underscore the limited representation of municipalities governed by the same political party as the federal government in both groups. They also explore the impact of audits on contracts waived from the procurement process. However, while waived contracts accounted for approximately 35% of all contracts, their associated expenses represented only around 10% of total expenditures. This relatively small share limited their influence on overall spending patterns.

Finally, Table A4 reveals demographic and economic differences between treatment and control groups, with treatment municipalities showing significantly smaller populations and lower GDP *per capita* values in 2015. These differences may influence the study, though subsequent analyses demonstrate a general homogeneity between the groups, supporting the robustness of the comparative framework.

T-tests were conducted to compare average values between treated and control municipalities across total expenses, number of contracts, and number of awarded firms. The results indicate no statistically significant differences for most variables, suggesting that the two groups were comparable in terms of procurement behavior and size-related expenses before the audits. This supports the validity of the identification strategy, particularly in the context of a DiD framework, which relies on the assumption that treated and control groups follow similar trends in the absence of treatment.

Table 5 presents the descriptive statistics for contract expenses over the entire period, weighted by municipality and sector, consistent with the methodology used in the model analysis. Table A5 provides a more detailed breakdown, presenting these statistics for three key intervals: the first two years, the middle of the audit period, and the last two years of the study period.

Table 5 reveals that, on average, treatment municipalities exhibit slightly lower expenditures compared to control municipalities. However, the differences between the median and mean within each group are not particularly pronounced, suggesting a relatively consistent distribution of expenses. Nonetheless, the distribution in the treatment group may still be slightly skewed by a few municipalities with significantly lower or higher spending, which could influence the overall averages. This observation highlights the importance of further analysis to assess potential variations in spending patterns across municipalities and sectors.

TABLE 5: DESCRIPTIVE STATISTICS OF EXPENSES IN TREATED MUNICIPALITIES

$ Expenses (per municipality and sector)	**2008 -2017**		
	Treatment*	**Control**	**Total**
Minimum (BRL K)	0.47	0.43	0.43
Median (BRL K)	525.25	549.90	548.66
Mean (BRL K)	2,284.06	2,506.99	2,485.13
Maximum (BRL K)	121,608.40	225,534.01	225,534.01
Standard Deviation (K)	7,437.85	7,988.82	7,936.58
Total (BRL M)	4,773.68	48,174.40	52,948.08

*Treatment = audited municipalities

Expenses values are expressed in Brazilian Reais thousand, except for the total, that is in Brazilian Reais million

Building on the previous analysis, Table 6 provides further insights into the distribution of contracts by dividing cumulative expenses into deciles, with each representing 10% of the total expense distribution. For example, the last decile (91% to 100%) includes the top 10% of the most expensive contracts in the dataset.

The distribution reveals a slight difference between treatment and control municipalities in the earlier deciles, particularly in the 0%–10% and 11%–20% ranges, where treatment municipalities have a marginally lower percentage of contracts compared to controls. However, the distributions become increasingly similar after the third decile, indicating greater homogeneity in contract allocation patterns for higher expense categories. This pattern supports the observation of comparable expenditure behaviors between treatment and control groups, with minor variations concentrated in the lower expense ranges.

TABLE 6: DISTRIBUTION OF CONTRACTS IN TREATED MUNICIPALITIES BY EXPENSE DECILES FROM 2008 UNTIL 2017

	Treatment*		Control		Total	
Deciles	**%**	**Cum %**	**%**	**Cum %**	**%**	**Cum %**
0% - 10%	71.81	71.81	74.52	74.52	74.30	74.30
11% - 20%	11.84	83.65	10.72	85.24	10.81	85.11
21% - 30%	6.44	90.09	5.70	90.94	5.76	90.87
31% - 40%	4.03	94.13	3.63	94.57	3.66	94.54
41% - 50%	2.58	96.71	2.34	96.91	2.36	96.89
51% - 60%	1.40	98.11	1.45	98.36	1.44	98.34
61% - 70%	0.85	98.95	0.85	99.21	0.85	99.19
71% to 80%	0.61	99.56	0.47	99.68	0.48	99.67
81% to 90%	0.35	99.91	0.24	99.92	0.25	99.92
91% to 100%	0.09	100.00	0.08	100.00	0.08	100.00

*Treatment = audited municipalities

% represents the relative percentage of number of contracts on the respective expense decile

Cum% represents the cumulative percentage across the deciles

Table 7 demonstrates that the distribution of contracts becomes more homogeneous when focusing specifically on audited contracts rather than audited municipalities, while also accounting for sectoral and population breakdowns. Compared to Table 6, the disparities between treatment and control groups have significantly diminished. For example, the difference in the first decile (0%–10%) is smaller, although minor variations persist, particularly in the middle deciles.

Despite these small differences, the overall distribution pattern highlights a predominant concentration of smaller-value contracts across both groups, with higher-value contracts being relatively rare. Independent *t*-tests conducted for related contract and expense variables confirm that the observed differences between treatment and control groups are not statistically significant. This supports the interpretation that higher-value contracts—while financially impactful—are similarly distributed across both groups. These findings reinforce the increased comparability of treatment and control groups when narrowing the analysis to audited contracts and applying relevant selection criteria.

TABLE 7: DISTRIBUTION OF CONTRACTS IN TREATED CONTRACTS BY EXPENSE DECILES FROM 2008 UNTIL 2017

	Treatment*		Control		Total	
Deciles	**%**	**Cum %**	**%**	**Cum %**	**%**	**Cum %**
0% - 10%	76.36	76.36	74.20	74.20	74.30	74.30
11% - 20%	10.96	87.32	10.80	85.00	10.81	85.11
21% - 30%	5.70	93.02	5.77	90.77	5.76	90.87
31% - 40%	3.24	96.26	3.68	94.45	3.66	94.54
41% - 50%	1.95	98.21	2.38	96.83	2.36	96.89
51% - 60%	0.81	99.02	1.48	98.31	1.44	98.34
61% - 70%	0.53	99.56	0.86	99.17	0.85	99.19
71% to 80%	0.32	99.88	0.49	99.66	0.48	99.67
81% to 90%	0.08	99.96	0.26	99.92	0.25	99.92
91% to 100%	0.04	100.00	0.08	100.00	0.08	100.00

*Treatment = audited contracts

% represents the relative percentage of number of contracts on the respective expense decile

Cum% represents the cumulative percentage across the deciles

Another important finding emerges from the analysis of number of contracts and expense values based on sector and population distributions, respectively illustrated in Figures 3 and 4.

Figure 3 illustrates the relative participation of each sector in total expenses and the number of contracts, revealing a significant disparity: over 60% of total expenses are concentrated in "Other Sectors", while more than 50% of contracts are linked to the pre-selected audited sectors. According to Table 1, the participation of "Other Sectors" includes the others non mentioned sectors in municipalities with fewer than 20,000 inhabitants that are part of the audit sample from rounds 28 to 33, as well as non-audited sectors in municipalities with more than 20,000 inhabitants.

FIGURE 3: COMPARISON OF CONTRACT DISTRIBUTION BY SECTOR: PERCENTAGE OF TOTAL EXPENSES vs. NUMBER OF CONTRACTS, FROM 2008 UNTIL 2017

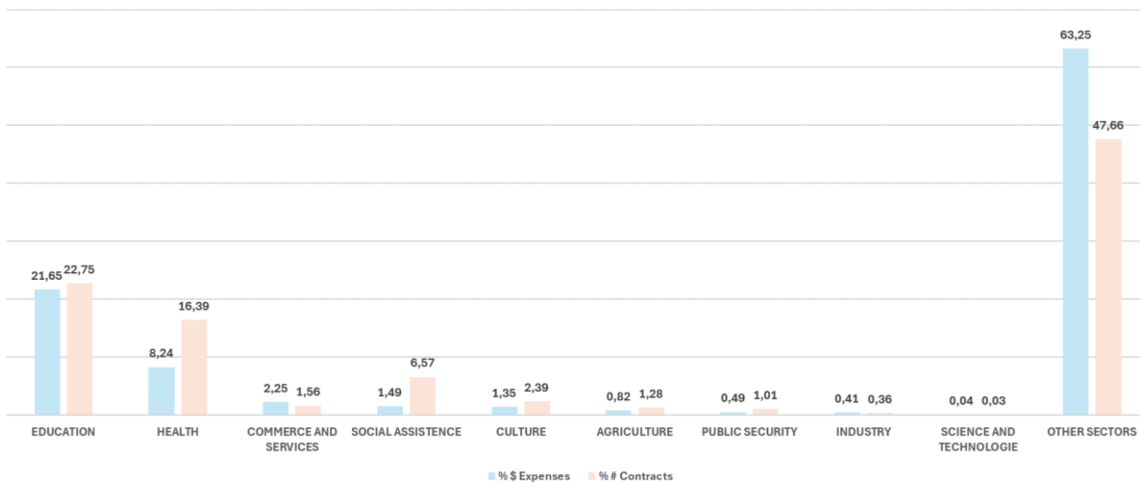
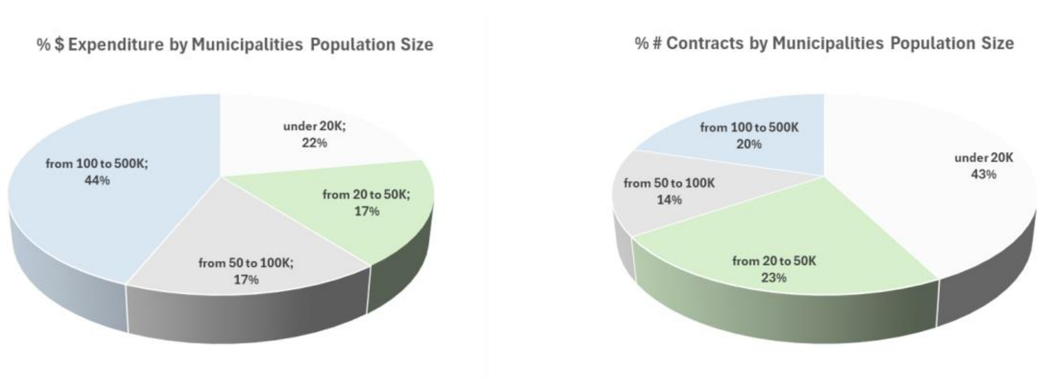


FIGURE 4: DISTRIBUTION OF TOTAL EXPENSES AND NUMBER OF CONTRACTS BY MUNICIPALITIES POPULATION SIZE, FROM 2008 UNTIL 2017



However, as shown in Figure 4, the "Other Sectors" category in Figure 3 reflects only a small contribution from the non discriminated sectors examined in audited municipalities with fewer than 20,000 inhabitants. This is evidenced by their relatively low overall share of total expenses (22%), despite accounting for a higher share of contracts (43%). The discrepancy arises because smaller municipalities tend to have lower-value contracts, which do not significantly influence the dominance of "Other Sectors" in total expenses.

In this context, it is clear that there is a discrepancy between audited and non-audited sectors in terms of expenditure values, which is further supported by Table 8, which compares the descriptive statistics of expenses between audited (treatment) and non-audited (control) contracts. The table reveals that audited contracts have significantly lower expense values, in contrast to Table 5, which focuses on audited municipalities and presents a different pattern. While the difference may initially appear sharp, it is not suspicious in itself. Rather, it reflects both the characteristics of the audit selection criteria and the underlying contract distribution by municipality size and sector. No evidence of data inconsistencies or strategic contract suppression was identified.

Figure 3 also underscores the concentration of expenses and contracts in the Education and Health sectors, which together account for approximately 30% of total expenses and 40% of total contracts. This distribution pattern is examined in greater detail in the subsequent chapter. Additionally, Figure 4 highlights a disparity in contract and expense distributions across population sizes: municipalities with populations under 50,000 account for over 60% of the number of contracts, while municipalities with populations over 50,000 are responsible for more than 60% of total expenses. The next chapter delves into these patterns, emphasizing that most audited contracts are from the Education and Health sectors in municipalities with populations below 50,000.

TABLE 8: DESCRIPTIVE STATISTICS OF EXPENSES IN TREATED CONTRACTS

$ Expenses (per municipality and sector)	**2008 -2017**		
	Treatment*	**Control**	**Total**
Minimum (BRL K)	0.47	0.43	0.43
Median (BRL K)	471.29	553.19	548.66
Mean (BRL K)	1,472.42	2,547.87	2,485.13
Maximum (BRL K)	44,759.43	225,534.01	225,534.01
Standard Deviation (K)	3,651.65	8,123.97	7,936.58
Total (BRL M)	1,830.22	51,117.86	52,948.08

*Treatment = audited contracts

Expenses values are expressed in Brazilian Reais thousand, except for the total, that is in Brazilian Reais million

In conclusion, this chapter has provided a detailed overview of the dataset, the respective sources, and all steps taken to select and refine the data to ensure consistency and relevance for the study. The descriptive analyses have highlighted key patterns and differences across treatment and control groups, including variations in contract distribution, expense values, sectoral participation, and population size. These insights lay the groundwork for the methodological framework and deeper analyses presented in the following chapter, ensuring a robust foundation for evaluating the impact of audits on public procurement outcomes.

Chapter 3
Empirical Analysis

The current chapter outlines the methodological framework employed to evaluate the impacts of audit policies on municipal contract expenses in São Paulo, Brazil, from 2008 to 2017. The study applies DiD and DDD models to a carefully refined dataset of 365 municipalities, ensuring a balanced panel and robust causal inference.

The chapter begins by specifying the models, detailing the application of interaction terms to capture nuanced effects of audits, while addressing potential challenges such as multicollinearity. Key variables, including treatment status, post-audit periods, sector classifications, and GDP *per capita*, are defined and contextualized to reflect the study's objectives. To ensure robust and reliable estimates, FE and RE models are compared using Hausman tests, and rigorous multicollinearity diagnostics are conducted.

3.1 Model Specification

The DiD and DDD models were applied to the entire dataset, comprising 936,183 observations. Each observation represents a public contract for expenses related to civil construction, renovations, works, and maintenance, initiated through a procurement process. These expenses were incurred between January 1, 2008, and December 31, 2017, under the direct responsibility of municipal governments in the state of São Paulo, Brazil, as detailed in Chapter 2. The original form variables associated with these expenses are briefly defined in Table 2.

As outlined in Table 3, the dataset includes a total of 365 municipalities after applying the specified sample selection, essential for creating a balanced panel dataset at the municipality level, ensuring consistency and strengthening the validity of the causal effects estimated in the model.

To evaluate the impact of audits conducted during rounds 28 to 35 (2009–2014), it was crucial to exclude contracts audited outside this period. Including such contracts could introduce potential policy effects into the control group, thereby compromising the validity of the analysis and reinforcing the necessity of excluding these municipalities. Additionally, municipalities with populations exceeding 500,000 inhabitants were excluded, as they were ineligible for the random draws, ensuring that both treatment and control groups shared the same ex-ante probability of being selected for audits. Finally, municipalities without contract observations in all years were excluded to establish the municipality as the panel variable, resulting in a balanced panel dataset of 365 municipalities from 2008 to 2017.

The DiD and DDD model follows the classic structure:

$$Y_i = \alpha + \beta_1 \text{ treatment}_i + \beta_2 \text{ post}_i + \beta_3 \text{ treatXpost}_i + \beta_8 X_i + \varepsilon_i \tag{1}$$

$$Y_i = \alpha + \beta_1 \text{ treatment}_i + \beta_2 \text{ post}_i + \beta_3 \text{ group}_i + \beta_4 \text{ treatXpost}_i + \beta_5 \text{ treatXgroup}_i + \beta_6 \text{ groupXpost}_i + \beta_7 \text{ treatXgroupXpost}_i + \beta_8 X_i + \varepsilon_i \tag{2}$$

The model includes several key variables to identify and estimate the effects of federal audits on municipal spending. The dependent variable is l_value, the natural logarithm of municipal expenditure, aggregated at the municipality-sector-year level. This transformation mitigates the influence of extreme values and enables interpretation of coefficients in percentage terms. The binary indicator treatment equals 1 for municipalities selected for audits between 2009 and 2014, while post indicates the years following each municipality's audit, starting uniformly on January 1st of the year after the draw. The variable group identifies contracts that match the sectoral and population criteria used in the audit selection process. The interaction treatXpost captures post-audit years for all contracts in treated municipalities, regardless of whether the contract itself was directly affected by the audit. treatXgroup further restricts treated municipalities to contracts that fall under the audit-targeted sectors, and groupXpost captures contracts meeting the group criteria executed in the post-audit period. The triple interaction treatXgroupXpost isolates the policy effect by identifying contracts in treated municipalities, within audited sectors, and during the post-audit period. To account for potential unobserved heterogeneity, the control variable GDP *per capita*, calculated at the municipality-sector-year level and expressed in natural logarithm form, is included to improve model robustness and reduce omitted variable bias.

For the validity of causal interpretations derived from these models, two critical assumptions must be satisfied. The first assumption concerns the exogeneity of the treatment and covariates. For the DiD model, equation (1) assumes that $E(\mu_i | \text{treatment}_i, \text{post}_\text{i}) = 0$, ensuring that the unobserved error term is uncorrelated with the treatment and post-treatment indicators. For the DDD model, equation (2) assumes that $E(\mu_i | \text{treatment}_i, \text{post}_\text{i}, \text{group}_\text{i}) = 0$ extending the exogeneity condition to include the group variable. These assumptions are essential for the models to produce unbiased estimates of the audit's effects, as highlighted by Wooldridge, 2011.

The second assumption pertains to parallel trends. For the DiD model, the parallel trend assumption posits that, in the absence of the audit, the average change in the dependent variable would have been identical for the treatment and control groups over time. This condition ensures that any post-audit differences observed between the treatment and control groups can be attributed to the audit rather than pre-existing differences in trends.

In the context of the DDD model, Olden and Møen (2022) argue that the triple difference estimator, while being a combination of two DiD estimators, does not require two parallel trend assumptions. Instead, it relies on the assumption that, in the absence of the policy, the relative outcomes of treatment and control groups for one variable, when the other variable is in the treatment group, must trend similarly to the relative outcomes of the same treatment and control groups when the other variable is in the control group.

Based on their framework, equation (2) assumes that, in the absence of the audit, the relative outcomes of the selected and non-selected groups in the treated municipalities would trend in the same way as the relative outcomes of these groups in the non-treated municipalities.

To ensure the most efficient estimator, both Fixed Effects (FE) and Random Effects (RE) models were initially applied. Hausman tests were conducted to determine whether unobserved effects were correlated with the regressors, addressing potential endogeneity, or if the RE model would yield consistent estimates. These methodological considerations are detailed alongside the presentation of the model results.

The models also incorporate year fixed effects to account for common shocks and trends impacting all municipalities within the same year. Additionally, robust standard errors clustered at the municipality level were employed to correct for within-municipality serial correlation, enhancing the reliability of the estimates.

The identification strategy adopted in this study is strengthened by three key assumptions: no anticipation, no contamination, and no confounding policies. The absence of anticipation effects is supported by the fact that municipalities were not informed in advance about their selection for audits, which were randomly drawn, minimizing the likelihood of behavioral changes prior to treatment. While the results suggest possible spillover effects within treated municipalities, notably reductions in spending even in non-audited sectors, contamination between treated and control municipalities remains unlikely. Audits targeted specific municipalities, and there is no indication that audit effects extended to non-treated municipalities, supporting the validity of the control group as an appropriate counterfactual. Finally, the model controls for potential confounding influences through the inclusion of year-level fixed effects and a logged GDP *per capita* control. While no major concurrent national or state-level policies directly targeting municipal procurement were identified during the treatment window, the possibility of indirect unobserved confounding influences cannot be entirely ruled out, as already mentioned on Chapter 1.

3.2 Variables Specification

Wooldridge (2019, Chapter 7) highlights that while interaction terms are critical for capturing complex relationships in models such as DiD and DDD, they can also increase the risk of multicollinearity. Similarly, Angrist and Pischke (2008, Chapter 5) provide an in-depth discussion on Fixed Effects (FE) and DiD models, emphasizing that interaction terms, particularly when treatment effects are unevenly distributed, may exacerbate multicollinearity concerns.

In this study, model specifications were carefully designed to address multicollinearity, given the uneven distribution of policy effects (audits) across different time periods. To further assess multicollinearity, the Variance Inflation Factor (VIF) was calculated from an Ordinary Least Squares (OLS) regression model. Details of this assessment are provided in the Robustness Checks section to ensure the reliability and validity of the results.

The dependent variables Y_i initially used in the models were l_value, n_contracts and n_firms. The variable l_value represents the natural logarithm of municipal spending, chosen to mitigate the influence of extreme values and allow for a more interpretable analysis. In this form, the coefficients of explanatory variables indicate the marginal effect on the dependent variable as a percentage. n_contracts represents the number of contracts, while n_firms denotes the number of awarded firms. All three dependent variables were aggregated at the municipality, sector, and year levels to align with the study's objectives.

Although the primary estimates for n_contracts and n_firms obtained from the DiD and DDD models are not statistically significant, the models with these variables are included in the study to provide a comprehensive analysis and context for the findings.

The variable treatment is a binary indicator that represents municipalities selected for audits between 2009 and 2014. It includes all contracts from these municipalities, irrespective of whether a specific contract within the municipality was audited.

The variable post is a binary indicator for the period after an audit. To align with the study's goal of capturing effects over time, this variable is defined as starting in the year immediately following the audit draw. For example, the first three draws included in the study (rounds 28, 29, and 30) occurred in different months of 2009: May, August, and October. To ensure temporal consistency, the post period for these draws begins uniformly on January 1, 2010. Consequently, when this variable is not interacted with others in the model, it represents all contracts executed in the dataset from January 1, 2010, onward, as 2009 marks the first year of treatment.[7]

The variable group is a binary indicator equal to 1 when the contract meets the draw selection criteria for audits. These criteria reflect a combination of the selected municipality's population size and the economic sector in which the contract expense was allocated. As detailed in Table A3, each draw applied specific criteria, and the column "Total of municipalities in the panel" lists the number of treated municipalities. To ensure consistency with the model's assumptions and isonomy, only the exact criteria that matched the 35 treated municipalities were considered for this variable. Table A6 provides a detailed list of these municipalities and their corresponding criteria for each draw.

For instance, as shown in Table A6, round 29 randomly selected two municipalities: Presidente Epitácio and Santo Antônio da Alegria. Presidente Epitácio, with a population of 43,535, falls under the population breakdown "from 20,000 to 100,000," and based on this draw's criteria, only contracts related to agriculture, social assistance, commerce, culture, education, and health were audited in this municipality. In contrast, Santo Antônio da Alegria, with a population of 6,739, is classified under "population under 20,000," and all contracts in this municipality were audited. The group variable for this round incorporates the classification "population under 20,000 + population from 20,000 to 100,000 for the specified sectors: agriculture, social assistance, commerce, culture, education, health, and services".

Table 9 provides a summary of the criteria reflected in the group variable for each round, based on the classifications outlined in Table A6. When this variable is not interacted with others in the model, it represents all highlighted groups in Table 9, which are defined by a combination of population breakdown and sectoral criteria. This structure

[7] While 2009 was the year of treatment assignment for the first audit rounds, contracts from that year were retained in the analysis to preserve sample continuity and statistical power. The post-treatment period was uniformly defined as beginning on January 1, 2010, to ensure consistency across treated municipalities, regardless of the month in which the audit was drawn. Excluding 2009 could introduce unnecessary variation in the temporal cutoff and reduce comparability. Moreover, as the audit effects were unlikely to have influenced behavior immediately upon announcement, the risk of contamination in 2009 is considered minimal.

ensures that the variable appropriately reflects the nuanced selection criteria for each treated municipality across the study period.

Figure 5 illustrates the relative percentage contribution of each round's sample selection to the total value of expenses and the total number of contracts. For example, as outlined in Table 9, R28 (round 28) includes contracts from all sectors in municipalities with populations under 20,000, a classification shared by R31 and R32. Collectively, contracts from municipalities under 20,000 inhabitants account for 22.17% of the total expense value and 42.38% of the total number of contracts, aligning with the distribution shown in Figure 4's pie charts. Similarly, rounds R34 and R38 have identical classifications, as do rounds R35, R37, and R39.

TABLE 9: VARIABLE GROUP CRITERION PER ROUND

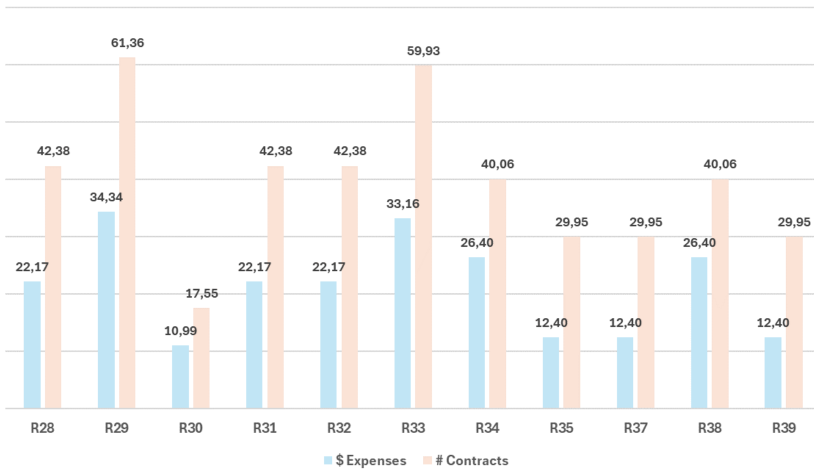
Round	Total of municipalities selected in São Paulo	Total of municipalities in the panel	Draw Date	Sample criteria based on municipalities population size (% share from total municipalities in São Paulo State)				
				under 20,000 (60%)	from 20,000 to 100,000 (28%)	from 100,000 to 500,000 (11%)	under 50,000 (79%)	from 50,000 to 500,000 (20%)
28	5	3	05/12/2009	all sectors	Education, Health, Social Assistance, Housing, Sanitation, and Urbanism.	Education, Housing, Sanitation, and Urbanism.		
29	5	2	08/17/2009	all sectors	Agriculture, Social Assistance, Commerce, Culture, Education, Health, and Services.	Agriculture, Social Assistance, Commerce, Culture, and Services.		
30	5	3	10/05/2009	all sectors	Public Security, Industry, Science and Technology, Social Assistance, Education, and Health.	Public Security, Industry, Science and Technology, and Health.		
31	5	3	03/01/2010	all sectors	Public Security, Industry, Science and Technology, Social Assistance, Education, and Health.	Public Security, Industry, Science and Technology, and Health.		
32	3	3	05/10/2010	all sectors	Agriculture, Social Assistance, Commerce, Culture, Education, Health, and Services.	Agriculture, Social Assistance, Commerce, Culture, and Services.		
33	5	5	07/26/2010	all sectors	Public Security, Industry, Science and Technology, Social Assistance, Education, and Health.	Public Security, Industry, Science and Technology, and Health.		
34	6	3	08/15/2011	-		-	Health, Education and Social Development.	Education and Social Development.
35	6	4	10/03/2011	-	-	-	Health, Education and Social Development.	Health and Social Development.
37	6	3	10/08/2012	-	-	-	Health, Education and Social Development.	Health and Social Development.
38	6	3	03/04/2013	-	-	-	Health, Education and Social Development.	Education and Social Development.
39	6	3	02/17/2014	-	-	-	Health, Education and Social Development.	Health and Social Development.

Round 36 is excluded from the study due to the fact that 36 Muncipalities had the audit cancelled by Portaria 1.713, from 10/08/2012, which affected all selected municipalities from São Paulo.
*30 Municipalities were selected from a control group of 120 that were drawn one year before. In São Paulo we have here only the municipalities not included in this control group.
**Audit could not be concluded in one municipality due to further investigation needs.

Source: created by the author based on Controladoria Geral da União (2024)

Together, these groups represented across the rounds account for 45% of the total expense value and 68% of the total number of contracts. This highlights the significant concentration of contracts in smaller municipalities, particularly in those with populations under 50,000, and underscores the alignment of the variable group classifications with the distribution patterns observed in the dataset.

FIGURE 5: COMPARISON OF CONTRACT DISTRIBUTION PER ROUND: PERCENTAGE OF TOTAL EXPENSES vs. NUMBER OF CONTRACTS, FROM 2008 UNTIL 2017



Based on Table 9 and Figure 3, it is worth emphasizing that certain sectors are represented in multiple rounds, with Education and Health consistently included in all of them. This recurring presence across rounds highlights the significance of these sectors within the dataset. These characteristics of the group variable underscore the importance of carefully specifying interaction terms to ensure accurate estimation of the effects, particularly when addressing the overlapping contributions of sectors across multiple rounds.

The variable treatXpost is a binary indicator that links each treatment municipality to its respective post-audit period, without differentiating the contracts that were specifically subjected to the policy. For example, all contracts executed after 2012 from the treated municipality of Poá (selected in round 34) are included in this variable, even though only contracts in the Education and Social Assistance sectors were audited in Poá. This classification follows the criteria outlined in Table 9 for round 34, as Poá has a population of 113,793 (Table A6). The specific audit effect on these sectors is captured by the interaction variable treatXgroupXpost, which refines the analysis further.

The variable treatXgroup restricts each treated municipality to the corresponding contract sectors that were audited, based on the municipality's population size and the criteria for its respective audit round, regardless of the contract date. For instance, in round

34, the three treated municipalities, Cesário Lange, Santa Albertina, and Poá, have different classifications due to their population sizes. This variable limits contracts from Cesário Lange and Santa Albertina to the sectors Health, Education, and Social Development, while Poá is restricted to contracts in Education and Social Development, as specified in Table A6 and Table 9. The true audit effect is isolated only when these contracts are further limited to the post-audit period, as captured by the interaction variable $\mathrm{treatXgroupXpost}$.

The dummy $\mathrm{groupXpost}$ narrows the criteria from the variable group to align with the corresponding post-audit period for each round. Table 10 details, for each population breakdown, the sectors selected according to the audit round and specifies the post-audit period during which contracts are included. For example, in the post-audit period starting in 2011, all contracts from sectors highlighted in green were considered. This includes all contracts executed after 2011 for municipalities with populations under 20,000, encompassing all sectors based on the criteria of rounds 31 and 32. Similarly, for municipalities with populations between 20,000 and 50,000, contracts executed after 2011 in the sectors of Public Security, Industry, Science and Technology, Social Assistance, Education, and Health were included in this variable.

Table 10 also illustrates the cumulative coverage of the variable in its final column, labeled Variable $\mathrm{groupXpost}$, which consolidates the selected contracts across all rounds and criteria. While the following section reveals that this variable is not statistically significant at the 10% level, it is retained in the model to uphold the formal structure of the study and to provide a comprehensive analysis. This ensures that the potential effects of the audit policy are thoroughly evaluated across all selected sectors and population categories.

Olden and Møen (2022) emphasize the dual benefits of incorporating control variables in the regression formulation of the triple difference model. Firstly, control variables with substantial explanatory power reduce residual variance, thereby enhancing the precision of the estimated causal effect. Secondly, they account for compositional differences between groups, strengthening the credibility of the parallel trend assumption, which is critical for identification. In essence, the inclusion of control variables helps address potential selection issues when the treatment assignment and group composition are influenced by observable characteristics.

TABLE 10: VARIABLE GROUPXPOST CRITERIA

POP	POST DATE START	2010			2011			2012		2013	2014	2015	Variable GroupXPost
SIZE	SECTORS / GROUPS	R28	R29	R30	R31	R32	R33	R34	R35	R37	R38	R39	
UNDER 20K	EDUCATION												
	HEALTH												
	COMMERCE AND SERVICES												
	SOCIAL ASSISTENCE												
	CULTURE												
	AGRICULTURE												
	PUBLIC SECURITY												
	INDUSTRY												
	SCIENCE AND TECHNOLOGIE												
	OTHER SECTORS												
20K UNTIL 50K	EDUCATION												
	HEALTH												
	COMMERCE AND SERVICES												
	SOCIAL ASSISTENCE												from 2010
	CULTURE												
	AGRICULTURE												
	PUBLIC SECURITY												
	INDUSTRY												
	SCIENCE AND TECHNOLOGIE												
	OTHER SECTORS												
50K UNTIL 100K	EDUCATION												
	HEALTH												
	COMMERCE AND SERVICES												
	SOCIAL ASSISTENCE												
	CULTURE												
	AGRICULTURE												
	PUBLIC SECURITY												
	INDUSTRY												
	SCIENCE AND TECHNOLOGIE												
	OTHER SECTORS												
ABOVE 100K	EDUCATION												
	HEALTH												from 2012
	COMMERCE AND SERVICES												
	SOCIAL ASSISTENCE												
	CULTURE												
	AGRICULTURE												
	PUBLIC SECURITY												
	INDUSTRY												
	SCIENCE AND TECHNOLOGIE												
	OTHER SECTORS												

Building on this framework, the control variable GDP *per capita* (X_i) is incorporated into the models to address potential unobserved heterogeneity and other factors that could influence the municipal spendings. Its inclusion aims to mitigate the risk of omitted variable bias, ensuring that the observed effects are attributable to the treatment rather than confounding influences. To maintain consistency and alignment with the model structure, GDP *per capita* was calculated at the municipality, sector, and year levels, and subsequently transformed into its natural logarithm form. This transformation allows for a more interpretable analysis and mitigates the influence of extreme values, ensuring a robust evaluation of its relationship with the municipal spending.

3.3 Results

The estimation strategy follows a DiD and DDD framework, operationalized through interaction terms that capture treatment status, group specific audit targeting, and post-treatment periods. The coefficients of interest correspond to standard DiD and DDD estimators, which isolate the causal effect of the audit policy under study. To clarify the interpretation of each interaction term, the underlying logic is formally presented through Table A7. These expressions represent the group mean structures that motivate the construction of the treatXpost, treatXgroupXpost, groupXpost, and treatXgroup variables used throughout the analysis.

Table 11 displays the estimated results for the dependent variable l_value (natural logarithm of municipal spending) across various model specifications. Each model employs the most efficient estimation instrument, except for the DiD model with the control variable l_gdp_pc (natural logarithm of GDP *per capita*), where both Fixed Effects (FE) and Random Effects (RE) estimations are presented. While the RE model is identified as the most efficient for the DiD specification, the FE estimates are also included to facilitate a comparative analysis with the DDD FE model, which is the most efficient estimation approach for both DDD models.[8]

Robust standard errors clustered at the municipality level are applied to all five models to account for potential within-municipality serial correlation. Additionally, year fixed effects are incorporated to control for common shocks and trends that may affect all municipalities within the same year. This setup ensures the robustness of the estimates and their comparability across different models.

The treatXpost variable, which captures the interaction effect of treatment and the post-audit period, does not yield statistically significant estimates across the DiD models. This lack of significance underscores the critical role of the DDD model in providing more nuanced insights. By incorporating the interaction with the group variable, the DDD

[8] All Robustness Tests are presented on the section 3.4 Robustness Checks

specification allows for a more accurate estimation of the audit's impact, particularly by isolating the differential effects within treated municipalities.

TABLE 11: ESTIMATED RESULTS FOR MUNICIPAL SPENDING

l_value	DiD Fixed Effect Rob Std Er + Year FE		DiD Random Effect Rob Std Er + Year FE	DDD Fixed Effect Rob Std Er + Year FE	
treatxpostxgroup				0.3560651**	0.3611707**
				(0.1718312)	(0.1704398)
treatxpost	-0.1761173	-0.168923	-0.1685423	-0.3871199**	-0.3826604**
	(0.1297353)	(0.1292644)	(0.1293077)	(0.1868886)	(0.1853726)
treatxgroup				-0.4191485**	-0.4227273**
				(0.1719226)	(0.1715785)
groupxpost				-0.0552919	0.0517799
				(0.0780046)	(0.077581)
treatment	0	0	-0.0859217	0	0
	(omitted)	(omitted)	(0.2031628)	(omitted)	(omitted)
post	-0.2807252***	-0.3494206***	-0.7506733***	-0.3044435***	-0.3701549***
	(0.0711879)	(0.0773168)	(0.0855985)	(0.0887755)	(0.0947601)
group				-0.3858529***	-0.3832487***
				(0.1230153)	(0.1227015)
l_gdp_pc		0.1054207*	0.1086383**		0.1044117*
		(0.0550315)	(0.053652)		(0.0534436)
constant	14.87704	13.86629	13.30458	15.15044	14.15045
	(0.0383062)	(0.5242326)	(0.5041594)	(0.0948735)	(0.5138404)
Observations	936,183	936,183	936,183	936,183	936,183
Number of groups	365	365	365	365	365
	F(10,364)=32.66	F(10,364)=29.57	Wald chi2(2) = 333.75	F(14,364) = 24.71	F(15,364) = 22.99
	Prob > F = 0.0000	Prob > F = 0.0000	Prob > chi2 = 0.0000	Prob > F = 0.0000	Prob > F = 0.0000
sigma_u	1.0290348	1.0096956	0.94142069	0.95525978	0.93638303
sigma_e	1.3443784	1.3442032	1.3442032	1.3382475	1.3380751
rho	0.3694398	0.36070512	0.3290835	0.33754231	0.32873201
R-squared: Within	0.0243	0.0245	0.0245	0.0331	0.0334
R-squared: Between	0.0021	0.0969	0.0790	0.2863	0.3508
R-squared: Overall	0.0171	0.0350	0.0358	0.1105	0.1313
corr(u_i, Xb)	0.0142	0.0950	0 (assumed)	0.2817	0.3250

Estimated results with dependent variable l_values (natural logarithym of municipalities spending). Fixed Effect Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the first column (most efficient instrument for the model) and second colum added with natural logarithym of GDP per capita (model added as per comparison motives). Random Effect Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect with natural logarithym of GDP per capita on the third column (most efficient instrument for the model). Fixed Effect Difference-in-Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the forth column and fifth colum added with natural logarithym of GDP per capita (both with the most eficient instrument for the model). Observations per group: Minimum = 245; Average = 2,564.9; Maximum = 16,824.
*Significance at 10%; **Significance at 5%; ***Significance at 1%.
The variable treatment is omitted due to perfect collinearity with municipality fixed effects, as it does not vary over time. Its effect is absorbed by the fixed effects in the model.

The post variable, on the other hand, exhibits statistically significant effects in all model specifications. This significance can likely be attributed to the inclusion of year fixed effects, which mitigate noise and enhance the precision of the estimates. The results indicate a substantial negative effect, suggesting that, on average, the municipality expenses decrease by approximately 30% after 2010.[9] This finding reflects broader trends and reinforces the importance of the fixed-effects approach in capturing temporal dynamics effectively.

The variable treatment is omitted in the FE models due to perfect collinearity with municipality fixed effects, as it does not vary over time. Since fixed effects absorb all time-invariant characteristics at the municipality level, the independent effect of treatment cannot be separately estimated in these models.

The variable group demonstrates that expenditures tend to be, on average, approximately 38% lower in selected groups across time periods and municipalities. This finding highlights that being part of the specific groups selected based on the audit criteria, as detailed in Tables 9 and 10, is associated with lower spendings. These groups are defined by specific population sizes and sectoral compositions targeted during the audit rounds, indicating that the selection process itself is associated with lower expenditure levels.

In contrast, the groupXpost coefficient is not statistically significant, indicating that the change in expenditures over time does not differ significantly between selected and unselected groups. This suggests that, regardless of the municipality, the audit process did not lead to a differential post-audit adjustment in spending for the selected groups compared to the unselected ones. One plausible explanation for the lack of statistical significance is the potential multicollinearity between the year fixed effects and the groupXpost variable. However, on the next section is proved that the degree of multicollinearity does not appear sufficient to substantially distort the coefficient estimate for groupXpost, thus leaving its effect inconclusive within the model.

The interaction variable treatXgroup is both significant and negative, indicating a reduction of approximately 42% in expenditures when comparing selected versus unselected groups within the audited municipalities. This result suggests that, in the

[9] Starting date of the first three rounds, this detailed explanation is illustrated on the previous section 3.2 Variables Specification

municipalities targeted for audits, specific sectors identified by the audit criteria spent less than the sectors not selected for auditing. However, since this reduction occurs irrespective of the time effect, it raises the possibility that pre-existing differences in contract characteristics or unobserved heterogeneity between selected and unselected groups within treated municipalities may also play a role.

As anticipated, the variable treatXpost is significant and negative in the DDD model, indicating that expenses in the 35 audited municipalities decreased by approximately 38% after the audit period. This result aligns with the conclusions of previous studies discussed in the Literature Review, which suggest that audits can lead to reductions in public spending in the audited municipalities. Moreover, this finding, when analyzed together with treatXgroupXpost, highlights the subtle dynamics of audit interventions, suggesting the spillover effects of the audits that impact non-selected groups within the treated municipalities.

In contrast, the interaction term treatXgroupXpost is positive and statistically significant at the 5% level, suggesting a countervailing effect that offsets the negative impact observed in the other interaction terms. Specifically, the results indicate that for the selected group of contracts in the 35 treated municipalities, expenditure increase by approximately 36% after the audits. This finding highlights a nuanced outcome, where the audits may have led to increased spending in certain targeted sectors. While this result might reflect sectoral priorities or strategic shifts post-audit, it warrants further exploration to understand whether the observed increase aligns with improved procurement practices or unintended consequences of audit interventions.

Furthermore, the results reveal a complex redistribution of the audit's impact between the selected and non-selected groups within the treated municipalities. Specifically, the positive and statistically significant interaction term treatXgroupXpost indicates that expenditures for the selected group of contracts in treated municipalities increased by approximately 36% post-audit. However, considering that the overall reduction in the treated municipalities captured by treatXpost amounts to a 38% decrease, this suggests a negative spillover effect of the audits on the non-selected group. This finding highlights how audits may drive divergent spending patterns, with increased spending potentially reflecting sectoral priorities, strategic reallocations, or enhanced procurement practices within targeted contract groups, while generating spillover effects that reduce spending in non-selected areas.

Taken together, these results suggest that in the context of municipalities operating under fixed or rigid budget constraints, audits may trigger a reallocation of spending rather than an overall expansion. The observed increase in expenditures within the audited sectors, combined with a larger overall reduction at the municipal level, points to a possible redistribution effect — where resources are shifted away from non-audited sectors. This dynamic supports the interpretation that spillovers are not merely behavioral, but may also reflect structural budgetary trade-offs in response to audit scrutiny.

The inclusion of the control variable GDP *per capita* (ln_gdp_pc) reinforces the robustness of the findings, confirming the significance of treatXgroupXpost. This suggests that the observed effects are not confounded by economic conditions across municipalities, mitigating concerns about omitted variable bias. However, the relatively weak and marginal significance of GDP *per capita* implies that while economic factors influence expenditure, they are secondary to the primary effects captured by the audit-related variables. This underscores the importance of the audit criteria and sectoral targeting in shaping the observed outcomes.

Table A8 presents the same estimates from Table 11, now including the year fixed effects for reference. The year fixed effect omitted in the DDD models, which serves as the reference year, is 2015. The results reveal significant fluctuations in municipalities spendings across different years. Specifically, there are notable reductions in 2009 and 2017 and increases in 2010, 2012, and 2014 relative to the reference year. These fluctuations align with the broader economic trends discussed in Chapter 1, including the impact of the global financial crisis and subsequent recovery periods. Additionally, the trend aligns with the expense evolution displayed in Figure 2, where fluctuations in total expenses and expenses per contract are evident, as well as with the descriptive statistics from Table A4. Together, these observations underscore the influence of broader economic and policy contexts on municipalities expenditures over time.

The statistical results provide crucial insights into the model's performance and structure. The standard deviations of the municipality-specific effects (*sigma_u*) and the idiosyncratic errors (*sigma_e*) in the DDD models indicate notable variability both across municipalities and within municipalities over time. The intra-class correlation coefficient (*rho*) reveals that approximately 33.8% of the total variance is attributable to differences across municipalities, decreasing slightly to 32.9% after incorporating GDP *per capita* as a control variable. This underscores the importance of accounting for heterogeneity across municipalities in the analysis.

The R-squared values in the DDD models demonstrate that the model is more effective at explaining variations between municipalities than within municipalities over time. This pattern aligns with expectations for fixed-effects models, which often focus on isolating within-entity (municipality) variation (Baltagi, 2008, Chapter 2). The relatively low within R-squared suggests that while the model captures key effects, additional time-variant factors within municipalities, potentially omitted from the model, may also influence the dependent variable. These findings emphasize the need to carefully balance the interpretation of within and between variation in fixed-effects analyses.

In the DDD models, controlling for GDP *per capita* results in a modest increase in the within R-squared, with a more substantial rise in the between R-squared from 0.2863 to 0.3508. This indicates that including GDP *per capita* as a control variable significantly improves the model's ability to explain differences across municipalities. Additionally, the inclusion of GDP *per capita* enhances the overall fit of the model, as evidenced by an increase in the overall R-squared from 0.1105 to 0.1313. These improvements highlight the robustness added by controlling for economic conditions, providing greater explanatory power for both municipality-level and overall model variations.

Finally, the correlation between the unobserved effects and the explanatory variables confirms that the fixed effects are correlated with the regressors, justifying the use of a fixed-effects specification in the DDD models. Moreover, the F-*test* for u_i = 0 is statistically significant, confirming the presence of unobserved heterogeneity across municipalities that must be accounted for. This reinforces the appropriateness and necessity of the fixed-effects approach within this analytical context.

As previously noted, the dependent variables n_contracts (number of contracts) and n_firms (number of awarded firms) yield main estimates from both the DiD and DDD models that are not statistically significant, as illustrated in Table A9 and Table A10. Specifically, none of the key interaction terms, such as treatXpost or treatXgroupXpost, show meaningful effects at conventional significance levels. However, the inclusion of these variables in the study remains valuable for providing a comprehensive analysis of procurement patterns, even if their influence is inconclusive. Notably, the estimates for the post variable and year effects reflect expected trends in procurement over time, reinforcing the broader findings regarding municipality expenses. Given the lack of statistical significance in the main estimates, robustness tests were not conducted for these specific variables, ensuring the focus remains on more substantive findings while retaining these variables for methodological completeness.

3.4 Robustness Checks

In econometric analysis, particularly in complex models like DDD, robustness checks are crucial for validating the reliability of results, especially when dealing with small treatment groups. The sensitivity of such models is further amplified in contexts characterized by economic and political instability, where fluctuating conditions can significantly impact outcomes. Robustness checks help ensure that the observed effects are not artifacts of model specification, sample composition, or external shocks, thereby providing more confidence in the conclusions drawn from the analysis (Angrist & Pischke, 2008; Wooldridge, 2010).

As explained on section 3.1 Model Specification, to obtain the most efficient estimator, both FE and RE models were initially applied with Hausman tests, to detect which one is more appropriate to the data. The Hausman test is commonly used to compare FE and RE models. It tests whether the unique errors (unobserved effects) are correlated with the regressors. If the null hypothesis is rejected, the FE model is preferred because it suggests that the unobserved effects are correlated with the independent variables, making the RE model inappropriate (Wooldridge, 2019, Chapter 14).

Specifically, the coefficient estimates and variance-covariance matrices were extracted from both the FE and RE models. The test statistic, designed to evaluate the consistency of the RE model compared to the FE model, is calculated as follows:

$$X^2 = (b_{FE} - b_{RE})' \,.\, [V_{FE} - V_{RE}]^{-1} \,.\, (b_{FE} - b_{RE}) \tag{3}$$

where X^2 represents the chi-squared test statistic, b_{FE} and b_{RE} denote the coefficient matrices derived from the FE and RE models, respectively, and V_{FE} and V_{RE} correspond to their respective Variance-Covariance Matrices.

The validity of the Hausman test relies on the assumption that both FE and RE are consistent under the null hypothesis, and that only the FE estimator remains consistent under the alternative. It also assumes the variance-covariance matrices are correctly specified and that the difference between the estimators is not driven by inefficiency alone.

For the DiD model without the control variable l_gdp_pc, the Hausman test yielded a chi-squared statistic of 50.68 and a *p-value* of 4.698×10^{-6}. This strongly rejects the null hypothesis, indicating that the FE model is the appropriate specification, as it accounts for the correlation between unobserved heterogeneity and the regressors. However, when the control variable l_gdp_pc is included in the model, the chi-squared statistic drops to 15.28 with a *p-value* of 0.4311. In this scenario, the null hypothesis cannot be rejected, suggesting that the RE model is a valid specification, as the inclusion of l_gdp_pc appears to mitigate the correlation between unobserved heterogeneity and the explanatory variables.[10]

For the DDD model without the control variable l_gdp_pc, the Hausman test produced a chi-squared statistic of 90.33 and a p-value of 1.262×10^{-11}, decisively rejecting

[10] These results were computed on Stata 17.

the null hypothesis and affirming the FE model as the preferred specification. When l_gdp_pc is added, the chi-squared statistic rises significantly to 346.80, with a *p-value* of 4.680×10^{-62}, further reinforcing the suitability of the FE model. These results suggest that unobserved heterogeneity across municipalities continues to influence the explanatory variables, even after the inclusion of l_gdp_pc, making the FE model more robust and appropriate for capturing the effects in the analysis.

As already mentioned, Chapter 5 of Angrist and Pischke (2008) highlights the potential issue of multicollinearity in FE and DDD models, particularly when interaction terms are included, as treatment effects are unequally distributed. This concern is relevant to the study's model, which employs several interaction terms. To evaluate multicollinearity, the Variance Inflation Factor (VIF) is calculated from an OLS regression model, as presented in Table A11.

Table 12 presents the results of the VIF test. A commonly used rule of thumb suggests that a VIF above 10 indicates severe multicollinearity, while some researchers adopt a more conservative threshold of 5 (Gujarati & Porter, 2009; O'Brien, 2007). The mean VIF for the model is 2.99, suggesting moderate levels of multicollinearity. However, the variables post (VIF = 6.60) and groupXpost (VIF = 5.20) stand out with higher VIF values, indicating potential correlations with other predictors in the model.

Although multicollinearity does not bias coefficient estimates, it can inflate standard errors, thereby reducing statistical power (Wooldridge, 2019, Chapter 3). In panel data models with robust standard errors clustered at the municipality level, heteroskedasticity and within-cluster correlation are addressed, but multicollinearity-related inflation of standard errors is not. Consequently, the size of standard errors becomes crucial in evaluating the impact of multicollinearity.

The post variable, despite having a relatively high VIF of 6.60, exhibits a small standard error (0.0947601) and is statistically significant, with a negative coefficient. This indicates a robust and significant effect of the post-audit period in reducing spending. The small standard error suggests that multicollinearity is not significantly affecting the post variable's reliability, reinforcing its importance in the model.

TABLE 12: VARIANCE INFLATION FACTOR TEST RESULTS

Variable	VIF	1/VIF
post	6.60	0.151530
group	3.53	0.283494
treatXpost	2.73	0.366161
treatXgroup	2.45	0.407881
groupXpost	5.20	0.192279
treatXgroupXpost	4.18	0.238990
year		
2009	1.52	0.658824
2010	2.75	0.363057
2011	2.74	0.365384
2012	2.89	0.345765
2013	2.36	0.423604
2014	2.46	0.407009
2015	2.12	0.472542
2016	2.17	0.461759
l_gdppc	1.22	0.818297
Mean VIF	**2.99**	

Similarly, the groupXpost interaction term has a higher VIF (5.20) but a relatively small standard error (0.077581). However, it is not statistically significant due to the small magnitude of the coefficient (0.0517799), rather than issues stemming from multicollinearity. This highlights that while multicollinearity may exist, it does not undermine the interpretability of this variable.

The relatively low standard errors, even for variables with higher VIFs, indicate that multicollinearity is not severely inflating standard errors. This supports the overall robustness of the model's findings. While multicollinearity is present to a moderate degree, it does not appear to compromise the statistical reliability of the model's key estimates.

The combined application of fixed effects estimation, robust standard errors, and multicollinearity checks reinforces the internal validity of the results. The models appear well-specified, and the estimates are not substantially affected by noise introduced by collinearity or unobserved heterogeneity. These diagnostics lend support to the overall robustness of the findings and confirm that the main results are not artifacts of model misspecification or estimation bias.

While the DiD framework is effective in capturing average treatment effects, it may be sensitive to unobserved group-specific trends or structural differences between treated and control municipalities, particularly in contexts where treatment intensity or scope varies across units. The DDD model helps address these limitations by introducing a third dimension—sector or population group—allowing for more refined comparisons and isolating the audit effect within targeted contract groups. This additional layer controls for sector-specific time shocks and helps correct potential biases that arise when the control group is imperfectly comparable.

Moreover, the assumptions underlying the DDD strategy are supported by both the design of the audit policy and the structure of the data. The selection of municipalities and sectors was exogenous to contract execution, based on predetermined audit criteria. No evidence of anticipation or reverse causality was identified, and the staggered implementation across municipalities reduces concerns about confounding shocks. Additionally, the localized nature of audits limits contamination between treated and control municipalities. These conditions strengthen the case that the estimated treatXgroupXpost effect can be interpreted as the causal impact of audits on selected contract groups.

While the composition of the selected group changes across audit rounds due to variation in sectoral and population-based selection rules, the DDD specification remains valid because the group variable is constructed to reflect the exact eligibility criteria applied in each treated municipality. This means that within each round, the comparison between selected and non-selected groups is internally consistent and policy driven. Moreover, the DDD model includes fixed effects and clustered standard errors to account for unobserved heterogeneity and time-specific shocks. Although the group definition evolves across municipalities, it is never endogenously determined by outcomes, preserving the exogeneity required for causal identification.

The alignment between policy design, exogenous treatment assignment, and the strength of diagnostic results provides strong support for the validity of the DDD identification strategy. These conditions reinforce the credibility of the underlying assumptions and justify interpreting the estimated effects as robust measures of the causal impact of audits on targeted municipal expenditures.

Conclusion

This study evaluates the impact of federal audits on procurement expenses in São Paulo municipalities from 2008 to 2017, focusing on incorporating a third group into a Difference-in-Difference-in-Differences (DDD) analytical framework. While audits generally lead to a reduction in expenditures across treated municipalities, the study makes clear that not all contracts within treated municipalities are audited. Instead, the audited contracts form a distinct third group defined by specific audit criteria, including population size and sector focus. This distinction introduces subtle dynamics, as evidenced by positive effects observed within certain audited contract categories, reflecting distinct impacts on the selected municipalities and sectors.

The third group, consisting of contracts meeting the audit criteria, reveals variations in the observed effects over time. These complexities, coupled with challenges like interaction terms and multicollinearity, influenced the significance and magnitude of key variables. The inclusion of year fixed effects and the control variable GDP *per capita* enhanced the model's explanatory power, better capturing variation within municipalities despite the economic and political instability of the study period.

Key findings include the significant and negative impact of audits on expenditures across treated municipalities. However, the analysis also uncovers a countervailing positive effect in the triple interaction term treatXgroupXpost, indicating that audits targeted at specific groups of contracts within treated municipalities may lead to increased expenditures, potentially reflecting higher-quality contracts or prioritization in targeted sectors. This refined finding highlights the importance of considering the specific dynamics of audited contracts and suggests that audits can influence spending patterns in ways that reflect the prioritization of strategic objectives or improvements in procurement processes.

At the same time, the analysis reveals a divergence between selected and non-selected groups within treated municipalities. While the interaction term treatXgroupXpost indicates that expenditures for selected contracts increased by approximately 36% post-audit, the overall reduction of 38% in expenditures captured by treatXpost, suggests a negative spillover effect on non-selected contracts. This finding underscores the importance of distinguishing between audited and non-audited contracts in the audited municipalities and understanding how audits can lead to complex redistributions of resources.

Furthermore, these results emphasize the need to understand the intricate dynamics of audit interventions, particularly how targeted audits influence not only selected groups but also the broader financial ecosystem of treated municipalities. Further investigation is required to determine whether these shifts represent intentional reallocations aligned with policy objectives, improvements in spending efficiency, or unintended consequences of the auditing process. Such insights are crucial for designing audit strategies that maximize effectiveness while minimizing unintended disparities in resource distribution.

Finally, the study underscores the importance of designing audit strategies that account for the interplay of economic, political, and institutional factors. In contexts like São Paulo, characterized by volatility and institutional challenges, targeted audits alone may be insufficient to achieve lasting improvements in public spending practices. Broader reforms aimed at fostering accountability and transparency, alongside tailored econometric models, are critical for addressing inefficiencies in public procurement.

This study reaffirms the critical role of audits as a policy tool for enhancing public procurement efficiency, offering valuable insights into their impacts at the municipal level. By highlighting the importance of distinguishing between audited and non-audited contracts within treated municipalities, it provides policymakers with detailed guidance for designing more effective audit strategies that maximize the benefits of public oversight in dynamic and complex governance settings.

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Appendix

TABLE A1: CGU PUBLIC LOTTERY MONITORING PROGRAM, FIRST 14 ROUNDS AND SAMPLE CRITERIA.

Round	Total of municipalities selected in Brazil	Total of municipalities selected in São Paulo	Draw Date	Sample criteria based on municipalities population size (% share from total municipalities in São Paulo State)				
				under 100,000 (87%)	from 10,000 to 25,000 (53%)	under 300,000 (96%)	from 10,000 to 500,000 (57%)	under 500,000 (99%)
1	5	1	04/03/2003					all sectors
2	26	1	05/12/2003	all sectors				
3	50	3	06/18/2003		all sectors			
4	50	3	07/30/2003			all sectors		
5	50	3	09/03/2003			all sectors		
6	50	3	10/15/2003			all sectors		
7	50	3	11/12/2003			all sectors		
8	50	3	03/30/2004			all sectors		
9	50	4	04/29/2004				all sectors	
10	60	6	26/05/2004					all sectors
11	60	6	06/30/2004					all sectors
12	60	6	08/11/2004					all sectors
13	60	6	10/27/2004					all sectors
14	60	6	11/17/2004					all sectors

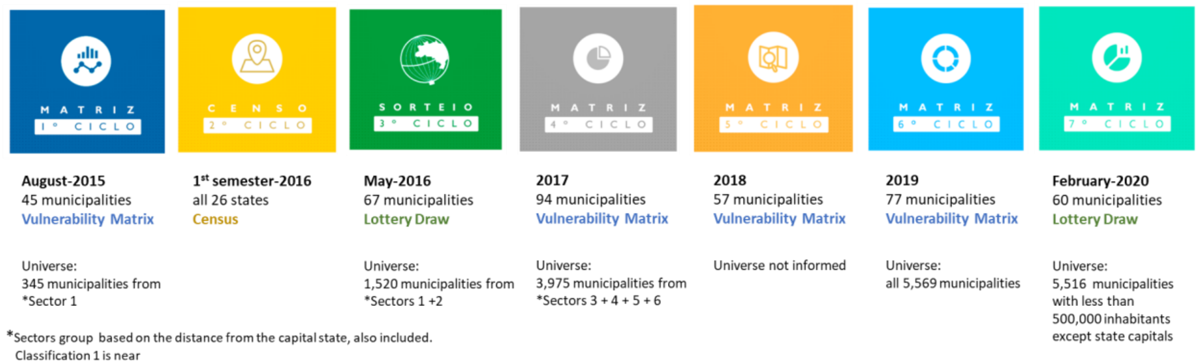
Source: created by the author based on Controladoria Geral da União, 2024

TABLE A2: CGU PUBLIC LOTTERY PROGRAM ROUNDS 15 TO 25, SAMPLE CRITERIA AND AUDITED SECTORS.

Round	Total of municipalities selected in Brazil	Total of municipalities selected in São Paulo	Draw Date	Sample criteria based on municipalities population size (% share from total municipalities in São Paulo State)				
				under 20,000 (60%)	from 20,000 to 500,000 (39%)	under 20,000 (60%)	from 20,000 to 100,000 (28%)	from 100,000 to 500,000 (11%)
15	60	6	04/14/2005	all sectors	Education, Culture, Commerce, and Services.			
16	60	6	06/09/2005	all sectors	Culture, Commerce, Services, Labor, and Social Previdence.			
17	60	6	08/16/2005	all sectors	Social Assistance, Public Security, and Industry.			
18	60	6	27/09/2005	all sectors	Social Assistance, Public Security, and Industry.			
19	60	6	07/11/2005	all sectors	Housing, Sanitation, and Urbanism.			
20	60	6	03/23/2006	all sectors	Housing, Sanitation, and Urbanism.			
21	60	6	06/02/2006	all sectors	Housing, Sanitation, and Urbanism.			
22	60	6	07/19/2006	all sectors	Agrarian Reform, Energy, and Environment.			
23	60	5	05/09/2007			all sectors	Education, Health, Social Assistance, Public Security, and Industry.	Education, Public Security, and Industry.
24	60	5	07/24/2007			all sectors	Education, Health, Social Assistance, Housing, Sanitation, and Urbanism.	Social Assistance, Housing, Sanitation, and Urbanism.
25	60	5	10/09/2007			all sectors	Education, Health, Social Assistance, Housing, Sanitation, and Urbanism.	Education, Housing, Sanitation, and Urbanism.

Source: created by the author based on Controladoria Geral da União, 2024

FIGURE A1: CGU FEDERAL ENTITIES MONITORING PROGRAM IMPLEMENTED ROUNDS.



Source: created by the author based on Controladoria Geral da União, 2024

TABLE A3: CGU AUDITS, ROUNDS 28 TO 39, SAMPLE CRITERIA AND AUDITED SECTORS, WITH NUMBER OF CONTRACTS SELECTED AND AVAILABLE IN THE DATA PANEL.

Round	Total of municipalities selected in Brazil	Total of municipalities selected in São Paulo	Total of municipalities in the panel	Draw Date	Sample criteria based on municipalities population size (% share from total municipalities in São Paulo State)				
					under 20,000 (60%)	from 20,000 to 100,000 (28%)	from 100,000 to 500,000 (11%)	under 50,000 (79%)	from 50,000 to 500,000 (20%)
28	60	5	3	05/12/2009	all sectors	Education, Health, Social Assistance, Housing, Sanitation, and Urbanism.	Education, Housing, Sanitation, and Urbanism.		
29	60	5	2	08/17/2009	all sectors	Agriculture, Social Assistance, Commerce, Culture, Education, Health, and Services.	Agriculture, Social Assistance, Commerce, Culture, and Services.		
30	60	5	3	10/05/2009	all sectors	Public Security, Industry, Science and Technology, Social Assistance, Education, and Health.	Public Security, Industry, Science and Technology, and Health.		
31	60	5	3	03/01/2010	all sectors	Public Security, Industry, Science and Technology, Social Assistance, Education, and Health.	Public Security, Industry, Science and Technology, and Health.		
32	*60	3	3	05/10/2010	all sectors	Agriculture, Social Assistance, Commerce, Culture, Education, Health, and Services.	Agriculture, Social Assistance, Commerce, Culture, and Services.		
33	60	5	5	07/26/2010	all sectors	Public Security, Industry, Science and Technology, Social Assistance, Education, and Health.	Public Security, Industry, Science and Technology, and Health.		
34	60	6	3	08/15/2011	-		-	Health, Education and Social Development.	Education and Social Development.
35	60	6	4	10/03/2011	-	-	-	Health, Education and Social Development.	Health and Social Development.
37	**60	6	3	10/08/2012	-	-	-	Health, Education and Social Development.	Health and Social Development.
38	60	6	3	03/04/2013	-	-	-	Health, Education and Social Development.	Education and Social Development.
39	60	6	3	02/17/2014	-	-	-	Health, Education and Social Development.	Health and Social Development.

Round 36 is excluded from the study due to the fact that 36 Muncipalities had the audit cancelled by Portaria 1.713, from 10/08/2012, which affected all selected municipalities from São Paulo.

*30 Municipalities were selected from a control group of 120 that were drawn one year before. In São Paulo we have here only the municipalities not included in this control group.

**Audit could not be concluded in one municipality due to further investigation needs.

Source: created by the author based on Controladoria Geral da União, 2024

TABLE A4: EXPENSES, CONTRACTS AND AWARDED FIRMS STATISTICS

	2008			2009		
	Treatment	**Control**	**Total**	**Treatment**	**Control**	**Total**
% Total Municipalities	9.59	90.41	100	9.59	90.41	100
% Total Contracts	7.86	92.14	100	8.47	91.53	100
% Total Expenses	8.70	91.30	100	10.11	89.89	100
Avg $ Expenses (BRL M)	14.24	15.84	15.69	10.06	9.49	9.54
Avg # Contracts	306.77	381.21	374.07	224.49	257.38	254.22
Avg #Awarded Firms	43.77	47.90	47.50	34.00	36.42	36.23
Avg $ Exp/Contr (BRL K)	46.40	41.56	41.94	44.83	36.87	37.54
Avg $ Exp/Firm (BRL K)	325.22	330.74	330.25	292.58	260.54	263.45
Avg # Contr/Firm	7.01	7.96	7.87	6.53	7.07	7.02
Avg $ Waived Exp (BRL M)	0.85	2.54	2.38	0.65	2.64	2.45
% Total Waived Contracts	34.04	43.52	42.78	40.45	50.16	49.34
% Mun Same Party as Fed Gov	8.57	9.09	9.04	5.71	8.18	7.95
% Transf Federal	80.01	78.96	79.05	76.54	79.54	79.29
GDPpc (BRL K)	14.08	17.71	17.48	15.14	19.15	18.89
Population (Mean)	24,038	37,449	36,163	24,038	37,449	36,163

	2012			2013		
	Treatment	**Control**	**Total**	**Treatment**	**Control**	**Total**
% Total Municipalities	9.59	90.41	100	9.59	90.41	100
% Total Contracts	7.72	92.28	100	8.20	91.80	100
% Total Expenses	8.55	91.45	100	9.24	90.76	100
Avg $ Expenses (BRL M)	20.04	22.72	22.46	13.69	14.26	14.21
Avg # Contracts	270.20	342.42	335.50	198.09	235.19	231.63
Avg #Awarded Firms	40.97	43.76	43.49	35.74	34.73	34.83
Avg $ Exp/Contr (BRL K)	74.16	66.34	66.95	69.08	60.65	61.34
Avg $ Exp/Firm (BRL K)	489.08	519.22	516.49	314.90	410.68	407.94
Avg # Contr/Firm	6.59	7.83	7.71	4.56	6.77	6.65
Avg $ Waived Exp (BRL M)	0.69	1.84	1.73	0.56	1.74	1.63
% Total Waived Contracts	30.35	34.73	34.39	36.55	38.42	38.27
% Mun Same Party as Fed Gov	5.71	8.18	7.49	14.29	8.48	9.04
% Transf Federal	77.18	77.49	77.47	81.81	79.67	79.84
GDPpc (BRL K)	20.26	27.12	26.69	21.57	29.71	29.19
Population (Mean)	24,038	37,449	36,163	24,038	37,449	36,163

	2016			2017		
	Treatment	**Control**	**Total**	**Treatment**	**Control**	**Total**
% Total Municipalities	9.59	90.41	100	9.59	90.41	100
% Total Contracts	7.95	92.05	100	8.34	91.66	100
% Total Expenses	9.13	90.87	100	9.40	90.60	100
Avg $ Expenses (BRL M)	13.26	14.00	13.93	7.84	8.01	8.00
Avg # Contracts	163.11	200.22	196.67	127.09	148.23	146.20
Avg #Awarded Firms	26.51	29.15	28.90	25.14	24.89	24.91
Avg $ Exp/Contr (BRL K)	81.31	69.91	70.82	61.70	54.05	54.69
Avg $ Exp/Firm (BRL K)	500.20	480.12	481.88	311.88	321.94	320.96
Avg # Contr/Firm	6.15	6.87	6.80	5.05	5.96	5.87
Avg $ Waived Exp (BRL M)	0.49	1.03	0.98	0.28	0.72	0.68
% Total Waived Contracts	23.98	28.70	28.33	29.59	33.01	32.72
% Mun Same Party as Fed Gov	14.29	8.48	9.04	17.14	10.61	11.23
% Transf Federal	81.91	78.67	78.93	86.23	83.76	83.97
GDPpc (BRL K)	25.79	36.16	35.50	26.51	37.82	37.10
Population (Mean)	24,038	37,449	36,163	24,038	37,449	36,163

Avg $ Expenses measures the average expenses by municipality, in Brazilian Reais million

Avg # Contracts measures the average number of contracts by municipality

Avg # Awarded Firms measures the average number of awarded firms by municipality

Avg $ Exp/Contr (BRL K) measures the average expenses per contract by municipality, in Brazilian Reais thousand

Avg $ Exp/Firm (BRL K) measures the average expenses per firm by municipality, in Brazilian Reais thousand

Avg # Contr/Firm measures the average number of contracts per awarded firms by municipality

Avg $ Waived Exp (BRL M) measures the average expenses of contracts that got waived from the bidding process, by municipality, in Brazilian Reais million

% Total Waived Contracts represents the relative percentage of contracts that got waived from the bidding process, by municipality

% Mun Same Party as Fed Gov represents the relative percentage of municipalities with the same political party as the federal government.

% Transf Federal represents the relative percentage of contracts that were funded by federal transferred funds

GDP pc (BRL K) represents the GDP *per capita* in current prices of all municipalities in the group for the respective year, in Brazilian Reais million.

Population (Mean) represents the average municipality population size in 2015.

TABLE A5: EXPENSES DESCRIPTIVE STATISTICS

$ Expenses (per municipality and sector)	2008 Treatment*	2008 Control	2008 Total	2009 Treatment*	2009 Control	2009 Total
Minimum (BRL K)	1.98	1	1	1.68	0.43	0.43
Median (BRL K)	471.29	537.89	530.47	340.79	376.95	372.84
Mean (BRL K)	2,296	2,510.90	2,490.61	1,669.49	1,671.86	1,671.62
Maximum (BRL K)	99,414.38	142,783.6	142,783.6	54,937.57	124,866.8	124,866.8
Standard Deviation (K)	8,343.52	7,881.94	7,925.01	6,195.37	5,241.26	5,343.94
Total (BRL M)	498.23	5,227.69	5,725.92	352.26	3,131.39	3,483.66

*Treatment = audited municipalities

Expenses values are expressed in Brazilian Reais thousand, except for the total, that is in Brazilian Reais million

$ Expenses (per municipality and sector)	2012 Treatment*	2012 Control	2012 Total	2013 Treatment*	2013 Control	2013 Total
Minimum (BRL K)	1.63	0.50	0.50	0.47	0.51	0.47
Median (BRL K)	674.16	687.39	684.53	406.38	544.25	536.71
Mean (BRL K)	3,036.11	3,583.64	3,529.20	2,347.83	2,490.59	2,476.69
Maximum (BRL K)	79,836.09	151,659.4	151,659.4	77,293.01	133,580.01	133,580.01
Standard Deviation (K)	8,285.65	11,108.44	10,858.37	7,205.65	8,097.91	8,013.88
Total (BRL M)	701.34	7,496.98	8,198.32	478.96	4,707.22	5,186.18

*Treatment = audited municipalities

Expenses values are expressed in Brazilian Reais thousand, except for the total, that is in Brazilian Reais million

$ Expenses (per municipality and sector)	2016 Treatment*	2016 Control	2016 Total	2017 Treatment*	2017 Control	2017 Total
Minimum (BRL K)	0.59	0.62	0.59	1.42	0.85	0.85
Median (BRL K)	621.99	621.35	621.67	484.58	381.01	397.54
Mean (BRL K)	2,405.11	2,718.41	2,695.33	1,533.24	1,740.67	1,718.80
Maximum (BRL K)	60,259.93	181,472.10	181,472.10	55,956.80	82.450.84	82,450.84
Standard Deviation (K)	6,489.81	8,679.36	8.480.45	4,914.75	5,341.70	5,297.33
Total (BRL M)	464.19	4,619.20	5,083.39	274.45	2,644.08	2,918.53

*Treatment = audited municipalities

Expenses values are expressed in Brazilian Reais thousand, except for the total, that is in Brazilian Reais million

TABLE A6: TREATMENT MUNICIPALITIES PER ROUND AND GROUP CRITERIA

Round	Municipality	Population Size	Municipality classification according to group criteria
R28	Caconde	18,976	all sectors
R28	Planalto	26,632	all sectors
R28	Lindóia	7,485	all sectors
R29	Presidente Epitácio	43,535	pop bet 20 - 100k + Agriculture, Social Assistance, Commerce, Culture, Education, Health and Services.
R29	Santo Antônio da Alegria	6,739	all sectors
R30	Cerqueira César	19,109	pop bet 20 - 100k + Public Security; Industry; Science and Technology; Social Assistance; Education and Health.
R30	Batatais	60,589	pop bet 20 - 100k + Public Security; Industry; Science and Technology; Social Assistance; Education and Health.
R30	Tanabi	25,467	pop bet 20 - 100k + Public Security; Industry; Science e Technology; Social Assistance; Education and Health.
R31	Viradouro	18,428	all sectors
R31	Ipuã	15,567	all sectors
R31	Jeriquara	3,216	all sectors
R32	Pedregulho	16,517	all sectors
R32	Vargem	9,854	all sectors
R32	São João de Iracema	1,885	all sectors
R33	Piquete	14,123	all sectors
R33	Cristais Paulista	8,260	all sectors
R33	Porangaba	9,299	all sectors
R33	Mirassol	57,857	pop bet 20 - 100k + Social Assistance; Science and Technology; Education; Industry; Health; Public Security.
R33	Lourdes	2,249	all sectors
R34	Cesário Lange	17,163	pop under 50k +Health, Education and Social Development.
R34	Santa Albertina	5,971	pop under 50k +Health, Education and Social Development.
R34	Poá	113,793	pop bet 50 - 500k + Education and Social Development.
R35	São Sebastião da Grama	12,355	pop under 50k +Health, Education and Social Development.
R35	Pontes Gestal	2,593	pop under 50k +Health, Education and Social Development.
R35	Riversul	5,941	pop under 50k +Health, Education and Social Development.
R35	Auriflama	14,961	pop under 50k +Health, Education and Social Development.
R37	Paraíso	6,290	pop under 50k +Health, Education and Social Development.
R37	Patrocínio Paulista	14,093	pop under 50k +Health, Education and Social Development.
R37	Santo Antônio do Jardim	6,053	pop under 50k +Health, Education and Social Development.
R38	Itapecerica da Serra	167,236	pop bet 50 - 500k + Education, Social Development
R38	Fernandópolis	68,120	pop bet 50 - 500k + Education, Social Development
R38	Anhumas	3,999	pop under 50k + Health, Education, Social Development.
R39	Borborema	15,569	pop under 50k + Health, Education, Social Development,.
R39	Pardinho	6,122	pop under 50k + Health, Education, Social Development.
R39	Lavínia	10,590	pop under 50k + Health, Education, Social Development.

Source: created by the author based on Controladoria Geral da União, 2024

TABLE A7: ESTIMATOR EQUATIONS FOR INTERACTION TERMS

The equations below break down how each interaction term in the difference-in-differences (DiD) and difference-in-difference-in-differences (DDD) models is constructed. They show how average outcomes are compared across time, treatment status, and audit-targeted groups to identify the effects estimated in the main analysis.

Difference-in-Differences (DiD) Estimator

$$DiD = (\bar{Y}_1^{T} - \bar{Y}_0^{T}) - (\bar{Y}_1^{c} - \bar{Y}_0^{c}) \tag{A.1}$$

This equation compares changes in the outcome Y over time between treated (T) and control (C) municipalities. It corresponds to the treatXpost coefficient.

Difference-in-Difference-in-Differences (DDD) Estimator

$$DDD = [(\bar{Y}_1^{TG} - \bar{Y}_0^{TG}) - (\bar{Y}_1^{cG} - \bar{Y}_0^{cG})] - [(\bar{Y}_1^{TNG} - \bar{Y}_0^{TNG}) - (\bar{Y}_1^{cNG} - \bar{Y}_0^{cNG})] \tag{A.2}$$

This equation isolates the audit effect on contracts in targeted groups, netting out time and group trends. It corresponds to the treatXgroupXpost coefficient.

Group × Post Interaction

$$Group \times Post = (\bar{Y}_1^{G} - \bar{Y}_0^{G}) - (\bar{Y}_1^{NG} - \bar{Y}_0^{NG}) \tag{A.3}$$

This term reflects sector-specific time trends across all municipalities. It corresponds to the groupXpost coefficient.

Treat × Group Interaction

$$Treat \times Group = (\bar{Y}_1^{TG} - \bar{Y}_1^{cG}) - (\bar{Y}_0^{TG} - \bar{Y}_0^{cG}) \tag{A.4}$$

This equation captures differences between treated and control municipalities in audit-targeted groups across both time periods. It corresponds to the treatXgroup coefficient.

Notation

T: Treated municipalities
C: Control municipalities
G: Audit-targeted groups
NG: Non-audit-targeted sectors
$\bar{Y}$: Average of the dependent variable
Subscripts 1 and 0: Post- and pre-audit periods, respectively

TABLE A8: ESTIMATED RESULTS FOR MUNICIPALITIES SPENDING ADDING YEAR FIXED EFFECTS

l_value	DiD Fixed Effect Rob Std Er + Year FE		DiD Random Effect Rob Std Er + Year FE	DDD Fixed Effect Rob Std Er + Year FE	
treatxpostxgroup				0.3560651**	0.3611707**
				(0.1718312)	(0.1704398)
treatxpost	-0.1761173	-0.168923	-0.1685423	-0.3871199**	-0.3826604**
	(0.1297353)	(0.1292644)	(0.1293077)	(0.1868886)	(0.1853726)
treatxgroup				-0.4191485**	-0.4227273**
				(0.1719226)	(0.1715785)
groupxpost				-0.0552919	0.0517799
				(0.0780046)	(0.077581)
treatment	0	0	-0.0859217	0	0
	(omitted)	(omitted)	(0.2031628)	(omitted)	(omitted)
post	-0.2807252***	-0.3494206***	-0.7506733***	-0.3044435***	-0.3701549***
	(0.0711879)	(0.0773168)	(0.0855985)	(0.0887755)	(0.0947601)
group				-0.3858529***	-0.3832487***
				(0.1230153)	(0.1227015)
2009	-0.4555908***	-0.4661231***	-0.4664351***	-0.4452203***	-0.4556993***
	(0.0590398)	(0.0599979)	(0.0600451)	(0.0597528)	(0.060869)
2010	0.1548725**	0.2003253***	0.6008804***	0.1471568**	0.1920232***
	(0.0664507)	(0.066789)	(0.073854)	(0.0670115)	(0.0677179)
2011	0.1509625*	0.185285**	0.585502***	0.1447236*	0.1785107**
	(0.0772946)	(0.0756455)	(0.0808825)	(0.0789473)	(0.077875)
2012	0.4338211***	0.4557888***	0.85566***	0.4276105***	0.4493475***
	(0.0814497)	(0.081255)	(0.0881503)	(0.0830429)	(0.083167)
2013	0.0191299	0.0331313	0.4327384***	0.017644	0.0315148
	(0.0851989)	(0.0855304)	(0.0889529)	(0.0862828)	(0.086769)
2014	0.298826***	0.3043258***	0.7036506***	0.3007061***	0.3061688***
	(0.0547717)	(0.0547088)	(0.0590974)	(0.0550343)	(0.0550437)
2015	0	0	0.3992299***	0	0
	(omitted)	(omitted)	(0.0550998)	(omitted)	(omitted)
2016	0.0631215	0.0579502	0.4570406***	0.0617056	0.0565823
	(0.045044)	(0.0456134)	(0.0495711)	(0.0451998)	(0.0453786)
2017	-0.3875187***	-0.398823***	0	-0.3925472***	-0.403798***
	(0.0544277)	(0.0551775)	(omitted)	(0.0541275)	(0.054788)
l_gdp_pc		0.1054207*	0.1086383**		0.1044117*
		(0.0550315)	(0.053652)		(0.0534436)
constant	14.87704	13.86629	13.30458	15.15044	14.15045
	(0.0383062)	(0.5242326)	(0.5041594)	(0.0948735)	(0.5138404)
Observations	936,183	936,183	936,183	936,183	936,183
Number of groups	365	365	365	365	365
	F(10,364)=32.66 Prob > F = 0.0000	F(10,364)=29.57 Prob > F = 0.0000	Wald chi2(2) = 333.75 Prob > chi2 = 0.0000	F(14,364) = 24.71 Prob > F = 0.0000	F(15,364) = 22.99 Prob > F = 0.0000
sigma_u	1.0290348	1.0096956	0.94142069	0.95525978	0.93638303
sigma_e	1.3443784	1.3442032	1.3442032	1.3382475	1.3380751
rho	0.3694398	0.36070512	0.3290835	0.33754231	0.32873201
R-squared: Within	0.0243	0.0245	0.0245	0.0331	0.0334
R-squared: Between	0.0021	0.0969	0.0790	0.2863	0.3508
R-squared: Overall	0.0171	0.0350	0.0358	0.1105	0.1313
corr(u_i, Xb)	0.0142	0.0950	0 (assumed)	0.2817	0.3250

Estimated results with dependent variable l_values (natural logarithym of municipalities spending). Fixed Effect Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the first column (most efficient instrument for the model) and second colum added with natural logarithym of GDP per capita (model added as per comparison motives). Random Effect Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect with natural logarithym of GDP per capita on the third column (most efficient instrument for the model). Fixed Effect Difference-in-Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the forth column and fifth colum added with natural logarithym of GDP per capita (both with the most eficient instrument for the model). The year fixed-effect omitted, considered as the reference year, is 2017 on the models with Random Effect instrument and 2015 on the ones with Fixed Effect. Observations per group: Minimum = 245; Average = 2,564.9; Maximum = 16,824. *Significance at 10%; **Significance at 5%; ***Significance at 1%.

TABLE A9: ESTIMATED RESULTS FOR NUMBER OF CONTRACTS

n_contracts	DiD Random Effect Rob Std Er + Year FE		DDD Random Effect Rob Std Er + Year FE		DiD Fixed Effect Rob Std Er + Year FE		DDD Fixed Effect Rob Std Er + Year FE	
treatxpostxgroup			28.05685	26.79392			28.01643	26.71712
			(39.28717)	(39.01339)			(39.282852)	(39.00905)
treatxpost	5.412766	3.72256	-13.73712	-14.84333	5.417817	3.689475	-13.72031	-14.85517
	(25.89438)	(25.62256)	(38.30156)	(38.22428)	(25.89039)	(25.61486)	(38.30002)	(38.22086)
treatxgroup			-63.88026	-62.99382			-63.95098	-63.04022
			(42.9135)	(42.72694)			(42.92537)	(42.74074)
groupxpost			45.24327	46.10926			45.329819	46.22356
			(50.26645)	(49.85867)			(50.25738)	(49.84659)
treatment	-46.7216**	-47.22348**	-8.089301	-9.061549	0	0	0	0
	(23.66574)	(23.77487)	(37.63569)	(37.61429)	(omitted)	(omitted)	(omitted)	(omitted)
post	-132.9859***	-114.1778**	-162.9996***	-143.9211**	-121.6119***	-105.1086**	-152.2533**	-135.5307**
	(35.70792)	(44.60004)	(59.86459)	(69.26547)	(36.38153)	(44.61352)	(60.68358)	(69.05022)
group			-6.220578	-6.876846			-6.087184	-6.74992
			(56.99519)	(56.60128)			(56.99712)	(56.606)
2009	-28.55385	-26.07862	-27.79128	-25.19403	-28.58135	-26.0511	-27.8262	-25.15945
	(41.5611)	(43.73004)	(40.68345)	(42.73672)	(41.55175)	(43.73643)	(40.671368)	(42.74423)
2010	52.53959**	39.19186	54.48036**	40.57098	41.16651*	30.246986	43.68122*	32.26337
	(25.22597)	(27.97054)	(25.51461)	(28.30068)	(24.521957)	(27.407994)	(24.37281)	(27.18196)
2011	61.75981**	51.02814*	63.21995**	52.05775*	50.37867*	42.13309	52.409476*	43.81115
	(27.33401)	(29.08449)	(27.79364)	(29.720274)	(26.87179)	(28.63935)	-2.705.519	-2.886.036
2012	87.16006***	79.33102**	86.64989***	78.47444**	75.77593**	70.49845**	75.83084**	70.29909**
	(32.47389)	(34.14818)	(32.54483)	(34.09182)	(31.55348)	(32.89964)	(31.34576)	(32.55123)
2013	50.21165	44.25433	49.27865	43.05302	38.82412	35.46045	38.45365	34.92372
	(38.26418)	(38.41368)	(38.05815)	(38.13351)	(36.99757)	(37.18933)	(36.76085)	(36.87132)
2014	30.61074	26.65005	29.63744	25.49572	19.24082	17.91955	18.83154	17.44135
	(24.92923)	(25.04533)	(25.17947)	(25.31061)	(24.94185)	(25.00103)	(24.9468)	(24.98858)
2015	11.37645	8.70865	10.81249	8.025254	0	0	0	0
	(18.00761)	(17.81107)	(17.66147)	(17.52413)	(omitted)	(omitted)	(omitted)	(omitted)
2016	8.996214	7.543664	8.467791	6.95074	-2.379611	-1.137278	-2.343618	-1.039816
	(14.49772)	(14.43082)	(14.55701)	(14.51484)	(15.11831)	(14.91705)	(15.002378)	(14.80455)
2017	0	0	0	0	-11.37819	-8.649365	-10.81029	-7.947116
	(omitted)	(omitted)	(omitted)	(omitted)	(18.00659)	(17.81034)	(17.65877)	(17.52133)
l_gdp_pc		-24.76851		-25.87984		-25.32609		-26.57127
		(25.23314)		(23.84609)		(25.44576)		(24.09318 6)
constant	194.7489	428.8267	195.3186	440.3248	289.8463	532.6658	296.8294	551.9894
	(24.49759)	(231.7586)	(56.77244)	(197.1651)	(21.7068)	(233.8381)	(53.60045)	(200.9073)
Observations	936,183	936,183	936,183	936,183	936,183	936,183	936,183	936,183
Number of groups	365	365	365	365	365	365	365	365
	Wald chi2(2) = 55.64	Wald chi2(2) = 63.50	Wald chi2(2) = 69.86	Wald chi2(2) = 72.77	F(10,364) = 2.98	F(11,364) = 4.12	F(14,364) = 2.93	F(15,364) = 3.51
	Prob > chi2 = 0.0000	Prob > chi2 = 0.0000	Prob > chi2 = 0.0000	Prob > chi2 = 0.0000	Prob > F = 0.0013	Prob > F = 0.0000	Prob > F = 0.0003	Prob > F = 0.0000
sigma_u	145.07811	142.35273	132.92254	132.58403	151.15121	154.09655	153.60045	157.19371
sigma_e	244.58964	244.53396	244.2053	244.14399	244.58964	244.53396	244.2053	244.14399
rho	0.2602597	0.25311026	0.22855554	0.2277459	0.27635747	0.28423518	0.28430403	0.29306203
R-squared: Within	0.0258	0.0262	0.0289	0.0293	0.0258	0.0262	0.0289	0.0293
R-squared: Between	0.0223	0.0001	0.0006	0.0142	0.0151	0.0035	0.0016	0.01898
R-squared: Overall	0.0252	0.0124	0.0146	0.0056	0.0187	0.0074	0.0137	0.0048
corr(u_i, Xb)	0 (assumed)	0 (assumed)	0 (assumed)	0 (assumed)	0.0200	-0.0643	-0.0275	-0.1099

Estimated results with dependent variable n_contracts (number of contracts per municipality, sector, and year). Random Effect Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the first column and second colum added with natural logarithym of GDP per capita. Random Effect Difference-in-Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the third column and forth colum added with natural logarithym of GDP per capita.Fixed Effect Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the fifth column and sixth colum added with natural logarithym of GDP per capita.Fixed Effect Difference-in-Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the seventh column and eight colum added with natural logarithym of GDP per capita. The year fixed-effect omitted, considered as the reference year, is 2017 on the models with Random Effect instrument and 2015 on the ones with Fixed Effect. Observations per group: Minimum = 245; Average = 2,564.9; Maximum = 16,824. *Significance at 10%; **Significance at 5%; ***Significance at 1%.

TABLE A10: ESTIMATED RESULTS FOR NUMBER OF FIRMS

n_firms	DD Random Effect Rob Std Er + Year FE		DDD Random Effect Rob Std Er + Year FE		DD Fixed Effect Rob Std Er + Year FE		DDD Fixed Effect Rob Std Er + Year FE	
treatxpostxgroup			1.687215	1.594764			1.542111	1.589714
			(2.971669)	(3.312966)			(3.317166)	(3.312563)
treatxpost	0.6822098	0.7489994	-0.1550569	-0.1118478	0.6823951	0.7471043	-0.1534999	-0.1119216
	(2.030087)	(2.013844)	(2.877174)	(2.879607)	(2.0298853)	(2.013165)	(2.877211)	(2.879076)
treatxgroup			-2.421733	-2.456284			-2.426145	-2.459513
			(3.709224)	(3.695405)			(3.710061)	(3.69613)
groupxpost			0.012897	-0.0211365			0.0185374	-0.0142073
			(3067713)	(3.048507)			(3.067)	(3.047728)
treatment	-2.786169	-2.766722	-1.687215	-1.6494	0	0	0	0
	(1.97203)	(1.980261)	(2.971669)	(2.976003)	(omitted)	(omitted)	(omitted)	(omitted)
post	-9.156192***	-9.899619***	-9.22436***	-9.970228***	-9.00798***	-9.625863***	-9.146911***	-9.759583***
	(2.102669)	(2.539221)	(3.12796)	(3.636544)	(1.91912)	(2.319367)	(3.145242)	(3.563659)
group			3.932653	3.958005			3.942128	3.966409
			(4.076597)	(4.056325)			(4.07665)	(4.056511)
2009	-2.405016	-2.502733	-2.467478	-2.568991	-2.406242	-2.500975	-2.469036	-2.566739
	(2.3081136)	(2.43895)	(2.265666)	(2.390806)	(2.307496)	(2.439186)	(2.264905)	(2.391082)
2010	3.958442**	4.485997**	3.9949**	4.538743**	3.81079***	4.219622**	3.914533***	4.332853***
	(1.666837)	(1.90605)	(1.691482)	(1.921015)	(1.443659)	(1.648383)	(1.48279)	(1.660086)
2011	5.412873***	5.837075***	5.384212***	5.820665***	5.264688***	5.573403***	5.303065***	5.618085***
	-1.981.447	-2.149.539	(2.014189)	(2.179832)	(1.8948316)	(2.016329)	(1.936936)	(2.045653)
2012	5.141799***	5.451266***	5.112693***	5.432368***	4.993727***	5.191316***	5.031327***	5.233996***
	(1.589935)	(1.788782)	(1.583784)	(1.771247)	(1.461183)	(1.598307)	(1.447822)	(1.568702)
2013	3.906217 *	4.141761 *	3.834957 *	4.078419 *	3.75731*	3.883251*	3.752518*	3.881845*
	(2.261132)	(2.343336)	(2.237167)	(2.318177)	(1.995762)	(2.046604)	(1.976083)	(2.022032)
2014	2.438319	2.594818	2.345806	2.507746	2.290895	2.340363	2.264921	2.315854
	(1.74129)	(1.734969)	(1.754352)	(1.750701)	(1.569727)	(1.567363)	(1.574773)	(1.571369)
2015	0.1477797	0.2532114	0.0812604	0.1902516	0	0	0	0
	(1.306055)	(1.30685)	(1.282363)	(1.288268)	(omitted)	(omitted)	(omitted)	(omitted)
2016	0.1451273	0.2025374	0.0895617	0.1488946	0.0028468	-0.0493598	0.0080933	-0.0396746
	(0.9775319)	(0.9685332)	(0.9640827)	(0.9575776)	(0.8950026)	(0.9008582)	(0.8863845)	(0.8932973)
2017	0	0	0	0	-0.1480819	-0.2502491	-0.0812913	-0.1861905
	(omitted)	(omitted)	(omitted)	(omitted)	(1.305979)	(1.307132)	(1.282208)	(1.288568)
l_gdp_pc		0.9790145		1.011919		0.9482096		0.9735012
		(1.980703)		(1.91959)		(1.99974)		(1.942769)
constant	18.28807	9.03654	15.2332	5.653854	26.77883	17.68766	24.27936	14.93097
	(1.602137)	(18.78241)	(3.318629)	(17.17552)	(1.256996)	(19.10241)	(2.97359)	(17.63829)
Observations	936,183	936,183	936,183	936,183	936,183	936,183	936,183	936,183
Number of groups	365	365	365	365	365	365	365	365
	Wald chi2(2) = 37.05	Wald chi2(2) = 37.24	Wald chi2(2) = 41.165	Wald chi2(2) = 42.29	F(10,364) = 3.35	F(11,364) = 3.05	F(14,364) = 2.80	F(15,364) = 2.69
	Prob > chi2 = 0.0001	Prob > chi2 = 0.0002	Prob > chi2 = 0.0003	Prob > chi2 = 0.0004	Prob > F = 0.0003	Prob > F = 0.0006	Prob > F = 0.0006	Prob > F = 0.0007
sigma_u	12.236386	11.850716	11.391061	11.259826	12.601002	12.492353	12.975088	12.853426
sigma_e	19.043261	19.042267	18.99576	18.99471	19.043261	19.042267	18.99576	18.99471
rho	0.29222572	0.27917741	0.26448745	0.26002499	0.30451819	0.30088466	0.31813212	0.3140828
R-squared: Within	0.0202	0.0203	0.0251	0.0252	0.0202	0.0203	0.0251	0.0252
R-squared: Between	0.0174	0.0493	0.0147	0.0020	0.0140	0.0519	0.0236	0.0054
R-squared: Overall	0.02187	0.0326	0.0087	0.0157	0.0166	0.0264	0.0069	0.0132
corr(u_i, Xb)	0 (assumed)	0 (assumed)	0 (assumed)	0 (assumed)	0.0323	0.0783	-0.0636	-0.0184

Estimated results with dependent variable n_firms (number of awarded firms per municipality, sector, and year). Random Effect Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the first column and second colum added with natural logarithym of GDP per capita. Random Effect Difference-in-Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the third column and forth colum added with natural logarithym of GDP per capita.Fixed Effect Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the fifth column and sixth colum added with natural logarithym of GDP per capita.Fixed Effect Difference-in-Difference-in-Difference model with Robust Standard Errors at municipality level and year fixed-effect on the seventh column and eight colum added with natural logarithym of GDP per capita. The year fixed-effect omitted, considered as the reference year, is 2017 on the models with Random Effect instrument and 2015 on the ones with Fixed Effect. Observations per group: Minimum = 245; Average = 2,564.9; Maximum = 16,824. *Significance at 10%; **Significance at 5%; ***Significance at 1%.

TABLE A11: OLS – ORDINARY LEAST SQUARE REGRESSION MODEL

	OLS
treatxpostxgroup	0.172523***
	(0.020608)
treatxpost	-0.0674365***
	(0.0134017)
treatxgroup	-0.1888172***
	(0.012224)
groupxpost	-0.371299***
	(0.0074871)
post	-0.7889943***
	(0.0098054)
group	-1.135898***
	(0.0066147)
2009	-0.4637319***
	(0.0067679)
2010	0.8125816***
	(0.0085903)
2011	0.7647047***
	(0.0084867)
2012	1.03556***
	(0.0082792)
2013	0.6238829***
	(0.0087995)
2014	0.8255811***
	(0.0086572)
2015	0.5521498***
	(0.0091755)
2016	-0.5442553***
	(0.0090788)
l_gdp_pc	0.4719521***
	(0.0028923)
constant	11.08771
	(0.0288026)
Observations	936,183
	F(15, 936167) = 15888.53
	Prob > F = 0.0000
R-squared	0.2029
Adjusted R-squared	0.2029
Root MSE	1.5883

OLS Ordinary Least Square Regression model. Year of 2017 omitted due to collinearity. *Significance at 10%; **Significance at 5%; ***Significance at 1%