Do Set-Aside Auctions Pay Off? Evidence from Brazil *

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Abstract

This paper investigates the impact of Set Aside (SA) auctions for Small and Medium Enterprises (SMEs) in Brazilian public procurement. Exploiting a legal reform that made SA auctions mandatory for contracts below BRL 80,000, we show that restricting competition to SMEs significantly increases their chances of winning contracts by 17.9%, but also raises procurement costs by 12.1%. This implies an annual fiscal cost of approximately BRL 73 million, given the total procurement spending affected by the policy. This result likely reflects the exclusion of larger, potentially more efficient firms. A trade-off emerges when analyzing the effects of mandatory SA auctions. Using a difference-in-differences approach, we find significant positive effects on employment and earnings for SMEs, with an average increase of 0.93 jobs per firm. Our cost-benefit analysis suggests that, despite the additional procurement expenses, the economic gains for SMEs indicate that SA auctions can be a viable policy tool in certain contexts. An estimated cost per job created of approximately USD 2,800.

Keywords: Public procurement, preferential treatment policies,
JEL Classifications: H57, D44, J23, L26, C21

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1 Introduction

Governments often face a trade-off between maximizing efficiency in resource allocation and pursuing broader societal goals of inclusion and equity. This tension is particularly evident in public procurement, which accounts for approximately 15% of GDP and 28% of public spending globally (Bosio et al., 2022). While traditional procurement frameworks prioritize cost minimization, governments increasingly recognize procurement as a powerful tool for inclusive development, particularly through preferential policies towards small and medium enterprises (SMEs) Among these, set-aside (SA) auctions, which reserve certain contracts exclusively SMEs, have gained popularity. The United States has promoted SA auctions for small businesses since 1953 (Albano et al., 2006, p.284), China reserves 30% of its procurement budget for small firms (OECD, 2018, p.88), and Russia has allocated 15% of annual procurement budgets to small businesses since 2006 (Tkachenko et al., 2019).[1]

Despite their popularity, the economic consequences of SA auctions remain poorly understood, especially in developing countries where data limitations and institutional complexity have limited the amount of studies on this matter. Theoretical predictions are ambiguous: SA auctions may raise prices by excluding larger, and potentially more efficient firms and reducing competitive pressure. Alternatively, they might decrease prices by encouraging participation from SMEs who would otherwise have not participated. Similarly, while SA auctions mechanically increase an SME's probability of winning contracts, it is unclear whether this translates into real economic outcomes, such as higher employment and productivity growth, or merely allows firms to capture rents from lower competition without scaling operations.

This paper provides a comprehensive cost-effectiveness analysis of SA auctions for SMEs by leveraging two complementary quasi-experimental strategies made possible by a well-defined institutional reform in Brazil. In 2014, Brazil enacted Supplementary Law 147, which made SA auctions mandatory for all federal procurement contracts below BRL 80,000—up from a previously optional regime under Law 123/2006. This reform creates two distinct sources of identification. First, the sharp BRL 80,000 eligibility threshold allows us to implement a fuzzy Regression Discontinuity Design (RDD) comparing auctions just above and below the cutoff. Second, the shift from voluntary to mandatory SA implementation generates temporal and cross-agency variation, which we exploit in a difference-in-differences

[1] More broadly, 47% of Central Asian countries, 37% of Sub-Saharan African nations, and 26% of Latin American states implement some form of preferential treatment for SMEs (World Bank, 2024).

(DiD) framework that compares agencies with high versus low pre-reform reliance on open auctions.

To estimate the impact of SA auctions on procurement efficiency, we first implement a fuzzy Regression Discontinuity Design (RDD) at the BRL 80,000 threshold, comparing nearly identical goods procured just above and below the cutoff. Our RDD sample includes 950,000 auctions across 30,400 product categories, allowing us to construct quality-adjusted price indices that control for brand, lot size, and product-year fixed effects. We find that restricting competition to SMEs increases procurement prices by 12.1% that persists across raw and quality-adjusted specifications. This price premium implies an annual fiscal cost of BRL 72.6 million, given that BRL 600 million in federal procurement falls under the policy each year. Over our seven-year sample (2014–2021), this accumulates to BRL 508.2 million in additional public expenditure.

We address potential identification challenges to ensure the robustness of our findings. We first document moderate bunching below BRL 80,000, suggesting the possibility of strategic manipulation around the regulatory threshold which could introduce bias in our RDD estimates. To address this, we implement a donut RDD approach that excludes potentially manipulated observations within BRL 2,500 below the threshold. Surprisingly, this more conservative specification yields a slightly larger price effect of 13.3%, suggesting that any manipulation may actually lead to an underestimation of the price premium. We further validate our finding using a difference-in-differences (DiD) design that exploits cross-agency heterogeneity in pre-reform SA adoption. Agencies that previously relied heavily on open auctions experienced a sharper shift toward SA compliance after 2014. Comparing these "treated" versus "control" agencies yields price increases of about 7 to 12% around 2014. These coefficients closely align with our RDD estimates, providing compelling evidence that SA auctions are associated with higher procurement costs.

Moreover, we uncover the mechanism behind the price effect: SA auctions stimulate intense competition among SMEs. We show that auctions below the threshold attract more participants, more bids, and higher bidding intensity (bids per participant). This heightened within-group rivalry partially offsets the efficiency loss from excluding larger firms—explaining why the price increase, while significant, is not catastrophic. Without this competitive response, the fiscal cost could have been far higher.

To assess whether these fiscal costs translate into real economic benefits, we link procurement data to Brazil's universe of formal employment records (RAIS) and implement a firm-level DiD design. We compare government-supplier SMEs (those already participating

in procurement before 2014 and thus directly benefiting from the expansion of SA auctions) with observationally similar non-suppliers, matched on pre-reform employment, wages, industry, and location across 567 industry–state strata. This design isolates the effect of increased access to SA contracts on firm performance.

We find large and statistically significant gains. Treated SMEs increase employment by 0.93 workers per firm on average—equivalent to a 9.2% increase relative to their pre-reform mean of 10.17 employees. They also raise average monthly earnings by 3.9% ($\approx$ BRL 43). These effects grow over time, as shown in event-study graphs with clear parallel pre-trends (2011–2013) and divergent post-reform trajectories. The results are robust to an extensive battery of checks: adding state–year, industry–year, and state–industry–year fixed effects; interacting year fixed effects with pre-treatment firm characteristics (age, tenure, gender composition); and including municipality-level controls (GDP per capita, bank density, etc.). Even in our most demanding specification—which flexibly absorbs differential time trends—the employment effect remains highly significant at 0.77 additional workers.

Given our sample of 33,000 SME establishments, the policy generated approximately 31,000 new formal jobs nationwide. Combining this with the fiscal cost yields a cost per job created of BRL 16,400 ($\approx$ USD 2,800)—a figure that compares favorably to other active labor market interventions in Brazil, such as conditional cash transfer programs that cost USD 8,000 per job (Corbi et al., 2019).

This paper contributes to several strands of literature. First, we contribute to the literature on how auction design and procurement regulations affect market outcomes. A well-established body of work examines the trade-offs between competition, efficiency, and discretionary power in public procurement (e.g., (Decarolis et al., 2020; Bandiera et al., 2021)). Within this, a key question is how policies that restrict bidder eligibility influence prices and competition. Seminal work by (Athey et al., 2013) on US timber auctions found that set-asides for small businesses can increase prices by limiting competition, a finding echoed in studies of highway procurement (Krasnokutskaya and Seim, 2011). However, these analyses often lack the granular data to unpack the mechanisms behind these price effects. While quantitative analyses of mediators and causal pathways remain scarce in the procurement literature (Fazekas and Blum, 2021), we precisely quantify the role of supplier selection and bidding dynamics. We show that the price effect of set-asides is not merely a mechanical result of excluding large firms, but is significantly mediated by a surge in competitive intensity among SMEs, a nuanced channel previously underexplored in this context.

Second, our paper adds to the empirical evidence on the economic consequences of preferential procurement policies for SMEs. While policies like set-asides and bid preferences are common globally (World Bank, 2024), rigorous evidence of their causal effects, particularly from developing countries, is limited. Previous research, often focusing on a single product category or geographically restricted markets, has struggled to generalize findings (e.g., Marion, 2017, 2007). We overcome this limitation by providing evidence from a large developing economy, Brazil, leveraging a nationwide regulatory reform and data on tens of thousands of product categories. This allows us to move beyond case studies and provide generalizable estimates of the fiscal cost of preferential policies, a critical input for policymaking in developing economies.

Third, we contribute to the growing literature on SME growth and development. A vast body of work has focused on relaxing supply-side constraints, such as access to finance and managerial capital (Woodruff, 2018). In contrast, we know much less about the effectiveness of demand-side interventions in promoting firm growth. Our study directly addresses this gap by investigating how government demand, channeled through procurement contracts, influences SME outcomes. We build on and extend a nascent literature that finds winning government contracts can boost employment (Ferraz et al., 2015), firm growth (Gugler et al., 2020), and survival (Cappelletti et al., 2024). Our contribution to this literature is threefold: (i) we identify the specific effect of set-aside auctions as a policy lever to stimulate demand, whereas prior studies often examine procurement wins more broadly; (ii) we provide a unified framework that estimates both the costs (higher prices for the government) and benefits (jobs and earnings for SMEs) of this demand shock; and (iii) by linking procurement and administrative employment data, we provide some of the most comprehensive evidence to date on how preferential access to government demand translates into real economic outcomes for small businesses in a developing country.

The remainder of the paper is organized as follows. Section 2 provides institutional background on Brazil's procurement framework and the implementation of set-aside (SA) auctions. Section 3 describes the data sources and details the construction of the estimation samples. Section 4 presents the empirical strategy and results on procurement outcomes, combining regression discontinuity and difference-in-differences approaches. Section 5 examines the reform's effects on firm-level performance using a difference-in-differences framework. Section 6 evaluates the fiscal cost and cost-effectiveness of the policy. Finally, Section 7 concludes and discusses broader policy implications.

2 Institutional Background

2.1 The Brazilian Procurement System

Auction format. Brazil primarily conducts procurements for standardized goods and services via competitive bidding (Pregão Eletrônico), governed by Federal Law No. 10.520/2002, where the government awards public contracts to the lowest bidder in first-price electronic reverse auctions. Bidders compete in real time by offering successively lower prices for their goods and services, with the lowest price winning the contract. Electronic reverse auctions are a common procurement practice worldwide. The World Bank's Global Procurement Database reveals that 50 out of 152 surveyed countries employ similar systems, including the United States, the United Kingdom, France, Russia, Israel, India, and Mexico. In contrast, countries like Germany, Italy, Canada, South Korea, and Singapore tend to use first-price sealed bid auctions. Evidence suggests these auctions can generate substantial savings: U.S. federal agencies reported 12-14% cost reductions.[2]

Tender vs. auction. An *auction* refers to a single item listed in the procurement system, for which we observe the bidding outcomes. A *tender*, on the other hand, is a broader event grouping multiple auctions under a single public notice, typically issued by a purchasing agency (PA). Each tender consists of separate auctions for each item, with all items sharing a common procurement goal. PAs publish procurement notices and related documents at the tender level. A *bundle* is a set of related goods or services grouped together as a single procurement lot. Each lot has its own auction, but the winner is determined as the one providing the lowest total cost across all items in the lot (i.e., the simple sum of values for all items). PAs typically bundle related products to ensure they are supplied by a single vendor, which can improve compatibility, simplify contract management, and potentially reduce costs.

Organizational units. For our analysis, we define a purchasing agency (PA) as the lowest hierarchical unit within Brazil's procurement management system.[3] These PAs are the actual entities that conduct procurement auctions and make purchasing decisions. Examples include individual public hospitals, schools, universities, and military units. Appendix Table C2 provides a list of selected PAs in Brazil.

[2] See Internet Appendix in Fazio and Zaldokas (2025).

[3] Brazil's procurement system is managed through the *Sistema Integrado de Administração de Serviços Gerais* (SIASG), where purchasing agencies are formally called *Unidade de Administração de Serviços Gerais* (UASG).

Two key bureaucrats play distinct roles in the public procurement process: the auctioneer (*pregoeiro*) and the procurement manager (*homologador*). The auctioneer is responsible for conducting bidding sessions, evaluating proposals, negotiating prices, and selecting the winning bid. However, they do not participate in drafting the tender notice. The procurement manager, in contrast, holds an administrative role within the purchasing agency (PA), overseeing tasks such as market price research and determining the appropriate bidding method.

Bidding Process. The procurement process begins with regulatory approval, followed by the publication of a detailed notice specifying the purchase objective (e.g., acquisition of painting materials), the required items (e.g., paint sealer, acrylic paint), and other procedural details. The procuring institution then collects proposals from potential bidders until the date when electronic bidding takes place. These institutions evaluate the proposals and approved bidders are authorized to participate in the electronic bidding stage, which takes place in the ComprasNet portal. ComprasNet is Brazil's electronic procurement platform, managed by the Ministry of Planning, where all federal procurement auctions are conducted.

Bidders' initial proposals automatically become their first bids when the electronic bidding stage begins. Once the bidding stage starts, participants can submit new bids, which must be lower than their previous bid but can be higher than the lowest bid at that moment. Throughout the process, bidders can observe the current lowest bid in real time but do not have access to bid histories or the identities of other participants. On the other hand, the auctioneer observes the identities of all bidders and has the right to remove suspicious bidders from the auction. Since 2014, the auctioneer also has access to the ultimate ownership structures of the auction participants.

After the bidding ends, the auctioneer declares the lowest bidder the winner, who must then submit documents that prove the firm's going concern status. If approved, both parties sign the public contract. However, if the winner fails to meet requirements—whether by withdrawing, not submitting the required documents, or providing inadequate documentation—they are disqualified, and the second-lowest bidder becomes the new winner. This process continues until the contract is signed and the procurement ends.

Product Classification. Products procured by public agencies follow a standardized classification system based on an official government catalog.[4] Following Fazio (2024), we define

[4]The classification system follows the Catálogo de Materiais (CATMAT), maintained by the Brazilian federal government. The most updated version is available at the link.

a product as the combination of a cataloged product code and its corresponding unit of measurement. Internet Appendix Table C1 provides an example list of products and their corresponding units of measurement, illustrating the variety of items procured — from office supplies to medical and construction materials.

2.2 Set-Aside Auctions for Small Businesses

Set-aside (SA) auctions represent a preferential procurement mechanism that restricts participation to only small and medium-sized enterprises (SMEs). SA auctions are designed to support SMEs, effectively excluding larger competitors from a part of the procurement process. This creates a separate, protected market segment where SMEs compete exclusively among themselves for government contracts. Preferential policies for SMEs are nearly universal across developed and developing countries, with set-aside policies representing one of the most common forms of preferential treatment in public procurement worldwide.

The eligibility criteria for SMEs in Brazil are established under Supplementary Law 123/2006. Eligible firms include Individual Entrepreneurs (MEIs) which are defined as firms with annual gross revenue lower than BR: 81,000), *Microempresas* (ME), annual gross revenue between BRL 81,000 and BRL 360,000, and *Empresas de Pequeno Porte* (EPP) with annual revenues between BRL 360,000 and BRL 4.8 million. Additionally, firms must not be subsidiaries of larger corporations or have foreign ownership exceeding specific limits to qualify for preferential treatment.

When a procurement is designated as set-aside, the entire bidding process, from the initial proposal submission through the final contract award, is restricted to certified SMEs. Larger firms cannot participate even if they offer more competitive prices. If insufficient SME participation occurs (typically fewer than three bidders), the procurement may be reopened to all firms, though this represents an exception rather than the norm.

The implementation of SA auctions in Brazil underwent a significant regulatory change in 2014. Initially, SA auctions were optional for purchases below BRL $80,000$, granting bureaucrats some degree of discretion over their enforcement. Moreover, the total value of contracts awarded through SA auctions could not exceed 25% of the annual procurement budget. This framework changed substantially with the enactment of Supplementary Law 147 of 2014, which introduced two key modifications. First, it made SA auctions mandatory for purchases below BRL $80,000$, eliminating bureaucratic discretion. Second, it removed the 25% cap on the total value allocated to SA auctions, expanding their scope significantly.

Despite this mandatory requirement, Article 49 of the law provided three exemptions allowing public managers to bypass SA auctions: (i) if SA auctions were deemed disadvantageous for public administration; (ii) if they could compromise the supply of goods; or (iii) if fewer than three eligible SME suppliers were available. As a result, even after 2014, SA auctions were still not universally adopted for below BRL 80,000 auctions, as illustrated in Graph (a) in Figure 1.

The regulation does not explicitly specify whether the BRL 80,000 threshold applies to individual auctions or to groups of auctions (bundles or tenders). Given the tendency of Brazilian public managers to adopt more conservative interpretations of regulations, they likely apply the rule at the group level, restricting SA auctions only when the total group value does not exceed the threshold. Importantly, public managers assign the SA treatment at the bundle level, which is consistent with the procurement system's design where bundles require a single supplier across multiple distinct lots. This bundle-level assignment is evident in our data: the variance of the treatment variable is zero for bundles containing multiple auctions below the cutoff, confirming that all auctions within a bundle receive identical treatment.Figure 1 presents the main source of variation used in this study to causally identify the effects of SA auctions for SMEs, which stems from the regulatory change introduced in 2014. Graph **(a)** depicts the share of SA auctions for SMEs across tenders of varying values, highlighting the discontinuity at the BRL 80,000 threshold. Graph **(b)** presents the evolution of SA auction frequency between 2007 and 2021, capturing the sharp increase following the 2014 reform.

Figure 1 illustrates the two main sources of variation in SA auction implementation. Graph **(a)** shows the share of SA auctions across tender values, revealing three key patterns. First, we observe a sharp discontinuity at the BRL 80,000 threshold when using tender value as the running variable, confirming that public managers apply the threshold at the tender (not auction) level. Second, even after the 2014 reform, approximately 10% of tenders above the threshold still use SA auctions, likely due to the regulatory exceptions discussed above. Third, consistent with prior procurement literature (Fazio, 2024; Decarolis et al., 2020), we detect modest bunching just below the threshold, suggesting some strategic tender value manipulation. Graph **(b)** shows the time-series evolution of SA auction frequency. The share of SA auctions increases sharply after 2013, coinciding with when they became mandatory below BRL 80,000. This clear temporal break, combined with the threshold discontinuity, provides the empirical variation we exploit in Section 4.

2.3 Defining Small Business in Brazil

SMEs in Brazil are businesses classified according to Lei Complementar 123/2006 (and its update in Lei Complementar 155/2016) as Individual Microentrepreneurs (MEIs) with revenue up to BRL 81,000, *Microempresas* (micro-enterprises) with revenue between BRL 81,000 and BRL 360,000, and *Empresas de Pequeno Porte* (small enterprises) with revenue between BRL 360,000 and BRL 4.8 million. In Brazilian policy documents these are often referred to as Micro e Pequenas Empresas (MPEs). For consistency with the international literature, we use the term "SME" throughout this paper. However, it is crucial to note that Brazil's definition excludes medium-sized enterprises, which are typically included in OECD, U.S., and EU classifications. As a result, the set-aside policy analyzed here targets only micro and small firms, and our findings are most directly comparable to preferential procurement programs aimed at the smallest formal businesses in other developing economies. This revenue-based classification is not merely administrative: it forms the legal basis for procurement set-aside and preference rules under Articles 42–49 of Lei Complementar 123/2006, making it the relevant definition for analyzing public contracting.[5]

Given that the legal criteria for defining SMEs vary widely across countries (Ayyagari et al., 2007), comparing the type of firms targeted by SA auctions in Brazil with those in the US, EU, and other developing countries is essential to understanding the external validity of our results. While Brazil defines SMEs primarily based on annual revenue, other countries use different criteria, such as the number of employees, turnover, or assets.

The European Union classifies SMEs as firms with fewer than 250 employees, an annual turnover of less than €50 million, or total assets below €43 million (IFC World Bank Group, 2010). In the United States, SME classification depends on industry-specific thresholds for number of employees and annual receipts, as established by the Small Business Administration (Athey et al., 2013).[6] This size comparison suggests our results are particularly relevant for understanding policies targeting micro and truly small enterprises, filling a gap in the literature that often examines larger firms within the SME category. Our findings may have

[5]The Brazilian statistical agency (IBGE) uses an alternative classification based on the number of employees: for example, firms in industry with up to 19 employees and firms in services with up to 9 employees are considered microenterprises. This employee-based scheme is widely used in descriptive statistics but has no relevance for procurement policy, where eligibility depends solely on revenue thresholds defined in Lei Complementar 123/2006 (Instituto Brasileiro de Geografia e Estatística (IBGE), 2023).

[6]See also https://www.sba.gov/federal-contracting/contracting-guide/size-standards. Manufacturing entities generally qualify as small businesses with up to 500 employees, while many service industries use revenue thresholds ranging from USD 750,000 to USD 38.5 million in annual receipts The SBA reviews and adjusts these standards periodically to reflect changing market conditions.

strong external validity for developing countries with similar firm size distributions.

3 Data

3.1 Data sources

Our analysis combines four main data sources that allow us to examine both procurement outcomes and firm-level effects of SA auctions.

ComprasNet. We leverage detailed procurement data from *ComprasNet*, the Brazilian federal government's electronic procurement system. This dataset provides a comprehensive record of procurement tenders, containing the full universe of documents used in federal procurement since 2007.

CNPJ Aberto. We obtain firm-level characteristics from the publicly available *Cadastro Nacional de Pessoas Jurídicas (CNPJ Aberto)*, the Brazilian National Registry of Firms, maintained by the *Receita Federal do Brasil (RFB)*, the country's fiscal authority.[7] This dataset contains rich firm-level characteristics, including registration status, sector classification, and opening and closing dates. Crucially for our analysis, it provides an SME status indicator based on revenue reports to the RFB. Since we use a 2019 version of the dataset, our SME classification follows the revenue thresholds established by Law 155/2016.

RAIS. To track labor market outcomes, we use data from the *Relação Anual de Informações Sociais* (RAIS) from 2011 to 2021. RAIS is an employer-employee matched panel covering the universe of formal employment relationships in Brazil.[8] This dataset allows us to examine firm-level employment dynamics, including workforce size, wages, and job creation patterns.

Regional controls. To account for spatial and regional economic characteristics, we incorporate data from the ESTBAN, a dataset maintained by the Central Bank of Brazil, and the Demographic Census. The ESTBAN dataset provides detailed information on the banking sector at the municipal level, including the density of bank branches and deposit levels. Additionally, we use demographic and economic indicators from the Demographic Census, such as GDP per capita, Gini index, and population, sourced from the *Instituto Brasileiro de Geografia e Estatística* (IBGE).

[7] The most updated version of the CNPJ Aberto is available at the link.

[8] Access to the identified version of the RAIS was granted through an institutional agreement between the *Ministério do Trabalho e Emprego* (MTE) and the *Universidade de São Paulo* (USP).

3.2 Procurement Sample Construction

We construct our procurement analysis sample in two stages: a full sample and a filtered sample, which will later serve as input to select the estimating samples of both our RDD and DiD with procurement outcomes.

Full sample. The full sample comprises 36.2 million purchase acts recorded on ComprasNet between 1997 and 2021. These include 330 thousand distinct product categories (goods and services), organized by 6.7 thousand purchasing agencies into 19.2 million bundles and 5 million tenders.

Filtered sample. We apply four filters to create our analysis sample. First, we retain only the 19.27 million auctions using competitive bidding, as SA auctions only occur in non-discretionary procurement. Second, we exclude 2.6 million auctions from 1997–2006, when the system was still scaling up. Third, we remove outliers where the unit price exceeds ten times or falls below one-tenth of the median price for the same product-year, eliminating 2.0 million auctions (5.3% and 6.7% of remaining observations, respectively). This filter addresses measurement errors in unit coding. Fourth, we exclude 1.4 million service auctions because service classifications lack sufficient precision for cross-product price comparisons.

The filtered sample contains 13.6 million auctions spanning 118.9 thousand product categories, managed by 4.4 thousand purchasing agencies across 408.8 thousand tenders. This sample forms the basis for both our RDD and DID analyses of procurement outcomes.

3.3 Labor Sample Construction

We construct our labor analysis sample through a similar two-stage process: a full sample and a filtered sample. The filtered sample serves as the basis for the propensity score matching that produces our final DiD estimation sample.

Full sample. The full sample is a yearly panel of 5.2 million unique establishments with employment information recorded in RAIS between 2011 and 2020. Of these, 56,561 are government suppliers (GSs)—establishments that won at least one procurement contract during this period.

Filtered sample. We apply three filters designed to isolate establishments most relevant to SA auction policy and ensure valid comparison between GSs and non-GSs.

First, we restrict the sample to SME establishments. Since official SME status is based on firm revenue (which we do not observe annually), we use an employment-based proxy

following Colonnelli et al. (2020). We classify an establishment as an SME if it belongs to one of three size categories based on its modal size during 2011–2013: Micro1 (1–4 employees), Micro2 (5–9 employees), or Small (10–49 employees). This proxy correlates at 0.99 with the official revenue-based classification when both are available in 2018, providing confidence in our measure.[9]

Second, we exclude establishments reporting zero employees or zero wages during 2011–2013 to avoid selection bias from inactive establishments. This filter reduces the sample to approximately 4.8 million establishments, including about 54.8 thousand GSs.

Third, we retain only establishments continuously observed throughout the entire pre-treatment period (2011–2013). This ensures we can properly assess parallel trends and avoid composition effects. This restriction further refines the dataset to around 1.5 million establishments, of which roughly 33.7 thousand are GSs.

After applying these filters, the filtered sample contains 1.48 million establishments, of which 33,731 are GSs. This sample forms the input for our propensity score matching procedure, which produces the final matched sample used in the DiD estimation (described in Section 5).

4 SA Auctions and Procurement Outcomes

This section examines how SA auctions affect procurement outcomes, including prices, participation, and bidding behavior. We address two key questions: First, do SA auctions successfully increase SME participation in government contracts? Second, what is the effect of restricting competition to SMEs on procurement prices?

We exploit two sources of variation in SA auction exposure. First, we use a regression discontinuity design around the BRL 80,000 threshold to estimate the immediate impact on procurement prices and SME participation. Second,we validate these cost estimates using a difference-in-differences approach that exploits the 2014 reform and variation in agency-level SA adoption.

Both empirical strategies, along with their corresponding results, are discussed in detail in the following sections.

[9]Using employment rather than the 2018 revenue classification also avoids sample contamination from firms that changed SME status after the policy period or following the 2016 eligibility revision.

4.1 RDD Estimates: Comparing Outcomes Around the SA Threshold

We construct the RDD estimating sample from the filtered dataset described in Subsection 3.2. To estimate the effect of mandatory set-aside (SA) auctions for SMEs, we apply a sequence of restrictions. First, we exclude about 3.4 million auctions held between 2007 and 2013, when SA auctions for SMEs were optional for bundles below BRL 80,000. Second, we restrict the sample to the 1.1 million auctions with bundle values between BRL 20,000 and BRL 140,000, using a symmetric window to avoid discontinuities around BRL 17,600 (where discretionary procurement becomes more likely) and BRL 180,000 (where SME preference margins apply). Third, we drop roughly 52,000 auctions with missing variables—20,000 with missing bidding information and 32,000 with missing brand identifiers—and 33,000 singleton observations within product-year or brand fixed effects. After these filters, the RDD estimating sample comprises approximately 950,000 auctions, of which 496,000 are SA auctions for SMEs. These auctions correspond to 780,000 bundles from 107,000 tenders, spanning 30,400 product categories procured by 3,300 purchasing agencies.[10]

Main outcomes. Our primary outcomes measure both the effectiveness and cost of SA auctions. First, to assess whether SA auctions expand SME access to procurement contracts, we use an indicator for whether an SME won the auction, $WinnerSME_i$. This captures the mechanical effect of restricting competition.[11] Second, to quantify the effect of SA auctions on procurement costs, we construct two price measures. The main outcome, $PriceIndex_i$, captures the percentage deviation of the winning price from the average paid for the same product that year. This is calculated as the residual from an OLS regression of the log unit price on product-year fixed effects using all auctions from 2013 to 2021.[12] To address concerns that price differences reflect quality rather than costs, we construct $PriceIndexAdj_i$ following Best et al. (2019) and Best et al. (2017); Bandiera et al. (2021). This measure additionally controls for brand fixed effects and lot size, isolating price variation within product-year-brand-size cells.[13]

We also examine three competition measures: number of participants ($NumPart_i$), total

[10] Appendix B provides variable definitions (Table B3) and descriptive statistics (Table B1).

[11] Panel A of Table B3 reports that $WinnerSME_i$ has a mean of 0.85 and a standard deviation of 0.36, suggesting that SMEs win a large share of government auctions in this segment.

[12] The log unit price with product-year fixed effects is less suitable for cost-effectiveness analysis, as product-year averages—and deviations from them—are noisier when based only on auctions near the cutoff.

[13] $PriceIndexAdj_i$ is obtained as the residual from an OLS regression of the log unit price on product-year and brand fixed effects, as well as lot size.

bids ($NumBids_i$), and bids per participant ($NumBidsPart_i$). These help us understand whether SA auctions stimulate or discourage bidding activity. On average, auctions attract 8.3 participants, receive 44.2 bids, and have 5.4 bids per participant.[14]

Identification strategy. We estimate the effect of set-aside (SA) auctions for SMEs on procurement outcomes by exploiting the regulatory threshold at BRL80,000, which defines eligibility for SA auctions. We implement a fuzzy regression discontinuity design (RDD) comparing auctions just below and above this threshold, within fine-grained product categories. Panel A of Figure 1 documents a clear discontinuity in the share of set-aside auctions: on average, 60% of procurements below the threshold are designated as set-asides, compared to about 10% above it. The share is not exactly 100% below the cutoff for two reasons. First, the law permits exceptions when fewer than three qualified SME suppliers exist or when SA auctions would disadvantage public administration. Second, our data include the early months of 2014 when enforcement was still ramping up.

To assess the validity of the RDD identification assumption, we test whether observable characteristics are smooth around the BRL 80,000 cutoff. Specifically, we compare treatment and control observations across a series of predetermined covariates that includes the quantity of products in the lot, quarter-of-the-year dummies, the number of auctions and bundles within each tender, and a set of indicators describing agency attributes—such as region, capital location, legal autonomy, government level, branch of government, and main public function. Figure A5 indicates that, while some covariates show statistically significant differences at the threshold, their magnitudes are economically negligible and do not point to meaningful discontinuities in baseline characteristics. This evidence supports the identifying assumption that potential outcomes vary smoothly around the threshold, reinforcing the credibility of the RD design.[15]

Specification. We estimate a two-stage fuzzy RDD. The first stage relates SA auction status to the threshold indicator:

$$SetAside_i = \delta + \gamma \cdot \mathbf{1}(Value_{j(i)} < 80{,}000) + f(Value_{j(i)}) + \varepsilon_i,$$

where $\mathbf{1}(Value_{j(i)} < 80{,}000)$ indicates that auction i's bundle value is below the threshold, and $f(\cdot)$ is a flexible polynomial in bundle value. The coefficient γ captures the discontinuous

[14] Panel A of Table B3 reports substantial dispersion across auctions, with coefficients of variation exceeding one for all three outcomes, indicating wide heterogeneity in market competitiveness.

[15] Appendix B (Table B3) provides detailed definitions and data sources for each covariate.

increase in SA auction probability at the threshold.

The second stage estimates the effect of SA status on outcomes:

$$Outcome_i = \alpha + \beta \cdot SetAside_i + f(Value_{j(i)}) + \varepsilon_i.$$

The parameter β is our coefficient of interest, representing the local average treatment effect (LATE) of SA auctions for procurements near the BRL 80,000 threshold. We interpret this as the effect of mandatory SA designation on otherwise similar procurements.

Graphical evidence. Figure 2 provides visual evidence of our main findings. Panel A replicates the pattern in Figure 1, showing a marked jump in the probability of set aside auctions below the threshold. Panel B shows a corresponding increase in the likelihood of SME winners, indicating SA auctions achieve their stated goal. Panels C and D reveal higher prices (both raw and quality-adjusted) for auctions just below the cutoff. These patterns suggest that restricting competition to SMEs increases the fiscal cost of procurement, even after controlling for product quality differences.

RDD estimates. Table 1 reports the estimated RD coefficients. First-stage coefficients confirms strong compliance with the threshold rule: auctions below the threshold are 51.2 percentage points more likely to be designated as SA auctions.The reduced-form (ITT) estimates show that crossing below the threshold increases the probability of an SME winner by 9.2 percentage points and raises prices by 6.2%. The second-stage fuzzy RDD estimates indicate that SA auctions increase the probability of an SME winner by 17.9 percentage points, raise procurement prices by 12.1%, and increase quality-adjusted prices by 12.3%.The similarity of these two price effects indicates the cost increase reflects reduced competition rather than quality differences. Taken together, the results highlight an important trade-off: SA auctions substantially expand SME participation—about 24% relative to the control mean of 75.3%—but do so at the cost of a roughly 12% price premium to the government. This increase likely reflects the exclusion of more efficient non-SME firms from competition.

Addressing manipulation. Figure A4 shows the distribution of tender values around the BRL 80,000 threshold that determines eligibility for SA auctions. A moderate excess mass just below the cutoff suggests that some purchasing agencies may have strategically adjusted tender values to remain below the threshold and avoid the SA rule.

To account for this potential manipulation, we implement a *donut* RDD that excludes observations within a narrow window around the cutoff (BRL 77,500 and BRL 80,000),

omitting the region most susceptible to bunching. Table 2 shows the results remain highly robust. The second-stage estimates indicate SA auctions increase SME winners by 20.1 percentage points (SE = 2.8pp) and prices by 13.3% (SE = 4.1pp), both slightly larger than the baseline estimates. This suggests that if manipulation exists, it leads us to slightly underestimate rather than overestimate the true effects of SA auctions.

Placebo tests. We further validate the RDD design, we estimate placebo effects at artificial cutoffs where no policy change occurred. Figure A1 plots coefficients from these regressions using alternative thresholds ranging from BRL 35,000 to BRL 110,000. We find no significant discontinuities in SA probability, SME winners, or prices at any placebo cutoff. This confirms that the discontinuities identified at the true BRL 80,000 threshold likely reflects the causal effect of the reform rather than spurious patterns in the data.

Sensitivity to methodological choices. To assess the robustness of our RDD estimates to alternative modeling choices, we re-estimate both the first- and second-stage regressions using different kernels, polynomial orders, and sets of fixed effects. Figure A2 shows results for SA probability (first stage) and SME winners (second stage), while Figure A3 presents price effects. Across all specifications, the coefficients remain stable and precisely estimated, with magnitudes within 1-2 percentage points of our preferred estimates. This confirms our findings are not artifacts of functional form assumptions or bandwidth selection.

4.2 DiD Estimates: Exploiting a Change in Enforcement of the SA Threshold

To further validate our RDD cost estimates using independent variation, we exploit the 2014 reform that made SA auctions mandatory for contracts below BRL80,000. As shown in Panel B of Figure 1, this reform substantially increased the frequency of SA auctions, providing an alternative source of exogenous variation to identify the effects of restricting competition to SMEs on procurement costs.

Before 2014, the SA rule below BRL 80,000 was weakly enforced, leading to a substantial variation in agency behavior. Some agencies voluntarily set aside most eligible procurements for SMEs, while others relied almost exclusively on open competition. The 2014 reform strengthened enforcement, requiring all agencies to use SA auctions below the threshold. This created differential exposure: agencies with low pre-reform SA rates experienced a sharp increase in SA frequency after 2014, while high-compliance agencies saw little change. We exploit this variation in treatment intensity for identification.

We measure *treatment intensity* as each agency's share of non-SA (open) auctions among eligible procurements (below BRL 80,000) during 2011–2013. Agencies with higher shares of open auctions faced a stronger policy shock, as the reform required them to substitute open auctions with SA auctions after 2014. Our empirical specification thus compares changes in outcomes before and after 2014 between agencies with high and low treatment intensity.

To ensure clean identification, we apply five sample restrictions. First, we exclude 4.5 million auctions conducted between 2007 and 2010, when SA adoption was voluntary and rising, focusing on the period 2011–2020. Second, to avoid contamination from potential manipulation near the SA threshold, we drop 2.14 million auctions with bundle values above BRL 80,000. Third, we exclude 618,000 auctions from agencies inactive during the pre-reform period (2011–2013). Fourth, we remove 192,000 auctions with missing product or brand identifiers. Finally, we drop 250,000 singleton observations across fixed-effects specifications to maintain a consistent sample. The final DiD dataset contains 6.33 million auctions conducted by 4,000 agencies, of which 2.67 million (42%) are SA auctions for SMEs.[16]

We construct two measures of treatment intensity: (i) a continuous variable capturing each agency's pre-reform share of open auctions, and (ii) a binary indicator equal to one for agencies above the median of this distribution. The continuous measure is used in our baseline specifications, while the binary version provides a robustness check.

Empirical specification. We estimate event-study models of the form:

$$Outcome_{i,t} = \alpha_{p(i),t} + \alpha_{a(i)} + \alpha_{b(i)} + \beta \cdot Intensity_{a(i)} \cdot Post_t + \alpha_t \cdot Value_{j(i)} + \varepsilon_{i,t},$$

where i indexes procurement auctions, t denotes years, and $Post_t = \mathbf{1}(t > 2013)$ indicates the post-reform period. The specification includes product-year fixed effects ($\alpha_{p(i),t}$) which absorb all time-varying shocks; agency fixed effects ($\alpha_{a(i)}$), which control for time-invariant differences in agency procurement practices; and brand fixed effects ($\alpha_{b(i)}$), which account for quality differences. We also control flexibly for bundle value via year=specific polynomials ($f(Value_j) \times \gamma_t$).

The coefficient β captures the differential change in outcomes after 2014 for agencies with higher treatment intensity. Identification requires that, absent the reform, outcomes would have trended similarly across high- and low-intensity agencies conditional on the included fixed effects. We assess this parallel trends assumption graphically in Figure 3.

[16] Appendix B (Tables B5 and B6) define all variables and report descriptive statistics.

Outcomes. We examine two primary outcomes. First, to test whether the policy increased SME access to public procurement, we use $WinnerSME_i$, an indicator for whether an SME won auction i.[17] Second, to capture the cost effects of SA auctions for SMEs, we use $LogPrice_i$, the log of the final price paid, with product-year fixed effects ensuring we compare prices within product markets over time.[18]

Results. Panel A of Table 3 shows the 2014 reform significantly increased SME winning probability for agencies that previously used fewer SA auctions. In Column (1), which includes product and year fixed effects, agencies with higher pre-reform shares of non-SA auctions experienced an 8.4 percentage-point increase in the probability of an SME winner after 2014 (SE = 0.009). The estimate remains stable around 7–8 p.p. when progressively adding product-year fixed effects (Column 2), agency fixed effects (Column 3), controls for the running variable interacted with year fixed effects (Column 4), and brand fixed effects (Column 5).

Panel B of Table 3 presents results that are consistent with the 2014 reform increasing procurement prices. The reform increased log-prices by 3.3–4.6% across specifications, with standard errors between 0.012 and 0.017. As before, the inclusion of additional fixed effects barely affects the point estimates, indicating that the price effect is robust to controlling for product-specific time trends, agency heterogeneity, and brand quality differences.

Combining the reduced-form estimates on log-prices (3.3–4.6%) with the first-stage coefficients (0.57–0.61) yields a scaled local average treatment effect of roughly 6–8%. This magnitude represents the price premium associated with SA auctions for *complier agencies* — those that changed their procurement behavior in response to the 2014 reform. The similarity of this estimate to our RDD findings (7–12%) provides strong validation of our main results using an entirely independent source of identifying variation.

Figure 3 complements these estimates by displaying the dynamic treatment effects from the event-study specification. Two key patterns emerge. First, all outcomes exhibit parallel pre-trends, supporting our identification assumption that high- and low-intensity agencies were on similar trajectories before the reform. Second, treatment effects only emerge after 2014 and grow over time, consistent with gradual implementation of the reform. By 2019,

[17] Panel A of Table B6 shows that SMEs won 82% of auctions below BRL80,000 during the pre-treatment period (2011–2013) and 90% in the post-treatment period (2014–2020), suggesting an increase in SME access to procurement caused by the policy.

[18] Panels A and B of Table B6 report descriptive statistics and shows that unit prices vary widely, with a coefficient of variation around one both before (2011–2013) and after treatment (2014–2020).Appendix B provides detailed definitions and data sources for all variables (Table B5).

agencies with high treatment intensity show approximately 40 percentage points higher SA usage (Panel (a)), 8 percentage points higher SME winner probabilities (Panel (b)), and 5-7% higher prices (Panels (c) an d (d)) compared to low-intensity agencies.

"As a robustness check, we re-estimate the model using a binary treatment indicator that compares agencies above versus below the median pre-reform SA adoption rate. Table A1 shows that results remain highly similar: the discrete specification yields effects of 7-8 percentage points on SME winners and 4-5% on prices. This confirms our findings are not driven by the functional form assumption of linear treatment intensity.

The consistency between the RDD and DiD findings reinforces our central conclusion: restricting competition to SMEs increases procurement costs by approximatelly 7-12%. These two approaches exploit fundamentally different sources of variation—the RDD uses discontinuous changes at the value threshold, while the DiD uses differential agency responses to the reform—yet yield strikingly similar cost estimates. This suggests the higher prices reflect a genuine effect rather than bias from any single identification strategy.

4.3 Mechanisms: Entry, Competition, and Bidding Activity

To shed light on the mechanisms behind the effects of Set-Aside (SA) auctions for SMEs, we examine whether the policy altered the degree of competition and bidding activity in public procurement. Figure 4 displays RD estimates for the first stage and intention-to-treat specifications using three outcomes: the number of participants, the total number of bids, and the number of bids per participant. Table 4 reports the corresponding regression results.

Panel (a) of Figure 4 confirms the strong first stage, showing a sharp discontinuity in the probability that an auction is designated as SA below the BRL 80,000 threshold. Panels (b)–(d) reveal a consistent pattern across different measures of competition: auctions just below the cutoff exhibit a higher number of participants, more bids, and greater bidding intensity (bids per participant). These results indicate that SA auctions encourage entry and more active participation by small firms.

The regression estimates in Table 4 corroborate these graphical patterns. In the intention-to-treat specification (Panel A), being below the threshold increases the number of participants by roughly 0.97, the number of bids by about 14, and the number of bids per bidder by 1.38 in the fuzzy second-stage specification (Panel C). Taken together, these effects suggest that SA auctions significantly raise the level of competition among SMEs. Rather than discouraging participation due to restricted eligibility, the policy appears to foster more intense

rivalry among qualified bidders, consistent with the idea that a more homogeneous set of competitors reduces perceived barriers to entry and lowers participation costs for smaller firms.

This increase in within-group competition may help explain why the estimated price effects, though positive, remain moderate despite the exclusion of larger and potentially more efficient firms. In other words, while restricting eligibility could have raised procurement costs sharply, the intensified rivalry among SMEs appears to have partially offset this effect through more active participation and bidding behavior.

5 SA Auctions and Real Outcomes

Our previous results show that restricting competition through set-aside (SA) auctions entails substantial fiscal costs for the government, with estimated price premiums between 7–12% across different empirical strategies. An important question then arises: do these higher procurement costs translate into tangible benefits for small and medium enterprises (SMEs)? While higher prices can increase profits for winning SMEs, these additional resources may not necessarily generate broader economic benefits if they are simply captured as rents by existing owners rather than fostering business expansion. Assessing whether these fiscal costs induce employment growth is therefore crucial for evaluating the developmental rationale of the SA program, which we examine in this section.

Matching Algorithm. To estimate the effect of SA auctions on establishment-level outcomes, we construct a matched sample that ensures comparability between SMEs that gained contracts through SA auctions (GSs) and those that did not (non-GSs). The matching procedure follows three main steps.

First, we stratify establishments into $21 \times 27 = 567$ industry–state cells, combining 21 two-digit industry categories (CNAE 2.0) with Brazil's 27 states.

Second, within each industry–state cell, we estimate propensity scores (PSs) at the establishment level using a logit model. The covariates include pre-reform averages of monthly wages, number of employees, and total payroll (2011–2013), as well as the quintile of the municipality's population distribution (2011). By restricting the PS model to pre-treatment variables, we can later assess matching performance using covariates not included in the estimation, which strengthens the validity of the balance checks.

Third, we match each GS establishment to the nearest non-GS establishment based

on the estimated PS, using nearest-neighbor matching without replacement. The procedure yields minimal attrition: only about 300 of the 33,700 GS establishments remain unmatched. The final matched sample thus comprises approximately 66,700 establishments, evenly split between treated (GS) and control (non-GS) units.

Validation of the Matching Algorithm. We assess the quality of the matching along three complementary dimensions. First, we assess the *common support* assumption by comparing the distribution of estimated propensity scores (PSs) for GS and non-GS establishments. As shown in Figure A6, both the unscaled and standardized PS distributions exhibit substantial overlap, indicating that our matching procedure successfully identifies comparable treated and control units within the same support.

Second, we test whether matching improves *covariate balance* between treated and control establishments. Comparing balance-check statistics before and after matching reveals marked reductions in both the T-statistics and standardized mean differences for nearly all pre-treatment variables, including firm size, wages, and payroll (see Figure A7). This evidence confirms that the matching procedure substantially reduces observable differences between GSs and non-GSs, thereby enhancing the comparability of the two groups for causal analysis.

Although minor level differences remain, the standardized mean differences are small in magnitude. More importantly, relevant to our identification strategy, the parallel trends hypothesis does not require that treatment and control groups be perfectly balanced, making the outcome of the matching algorithm acceptable for our purpose. However, to ensure that such remaining unbalances between GSs and non-GSs do not drive our results, we re-estimate our DiD specification with year fixed effects interacted with pre-treatment variables as controls in Table A2.

Outcomes. We focus on two main outcomes in the DiD analysis with establishment-level data. To assess whether the 2014 reform stimulated labor demand among SMEs, we use the total number of employees in establishment f at the end of year t, denoted $NumEmployees_{f,t}$. To evaluate whether the reform affected labor market prices, we use the average monthly earnings of employees in establishment f and year t, denoted $Earnings_{f,t}$.

Table B4 reports descriptive statistics for the estimating sample used in the DiD analysis, combining pre-treatment (2011–2013) and post-treatment (2014–2020) periods. Before the reform, establishments classified as government suppliers (GSs) and non-GSs exhibited very similar outcomes: GSs employed on average 10.11 workers, compared to 10.23 among

non-GSs, and paid average monthly earnings of BRL 1,105 versus BRL 1,099, respectively. After the 2014 reform, employment declined slightly among GSs (from 10.11 to 10.08) but more sharply among non-GSs (from 10.23 to 9.37), widening the employment gap in favor of treated establishments. Average monthly earnings increased for both groups but grew considerably more among GSs (from BRL 1,105 to BRL 1,582) than among non-GSs (from BRL 1,099 to BRL 1,525). These patterns indicate substantial heterogeneity in firm size and wage levels across establishments and suggest that SMEs benefiting from the reform experienced relatively stronger performance in both employment and earnings, motivating a formal DiD estimation in the next subsection.

Econometric Specification. To quantify the impact of the 2014 reform on employment and earnings, we estimate a DiD model of the form:

$$Y_{f,t} = \alpha + \beta \left(GS_f \times Post_t \right) + \lambda_f + \gamma_t + \varepsilon_{f,t}, \tag{1}$$

where $Y_{f,t}$ denotes either the number of employees or the average monthly earnings of establishment f in year t. The variable GS_f is an indicator equal to one for government suppliers (firms that participated in at least one public procurement auction before the 2014 reform), while $Post_t$ equals one for years after 2014. The coefficient of interest, β, captures the average effect of the reform on treated establishments relative to comparable non-GS establishments. All specifications include establishment fixed effects (λ_f) and year fixed effects (γ_t), so that identification relies on within-establishment changes over time. Standard errors are clustered at the establishment level to allow for serial correlation in outcomes.

We interpret β as the causal effect of the reform on treated SMEs under the standard parallel-trends assumption—namely, that in the absence of the reform, employment and earnings would have evolved similarly across GSs and non-GSs. This identifying assumption is supported by our matching procedure, which balances pre-reform characteristics and outcome trajectories across the two groups. In the next subsection, we provide graphical and regression-based evidence consistent with parallel pre-trends and persistent post-reform differences in outcomes.

We further assess the robustness of our baseline DiD estimates by including time-varying slopes interacted with a set of pre-determined establishment-level controls. These controls are grouped into three blocks.

The first block includes 12 pre-treatment outcomes observed in the three years prior to the reform (2011–2013): number of employees, average monthly earnings, and total annual

payroll. The second block contains 15 pre-treatment firm characteristics, also measured in 2011–2013: firm age, average worker tenure, share of female employees, average worker age, and share of employees with a college degree. The third block captures six municipality-level characteristics that may influence firm dynamics: GDP per capita in 2000, the Gini index of GDP per capita in 2000, total population, the share of GDP from manufacturing, bank branch density, and deposit density.

All these variables are interacted with year fixed effects, allowing for heterogeneous time trends correlated with observable firm and local characteristics. This specification ensures that our estimated effects are not driven by differential pre-existing trends between treated and control establishments. Detailed definitions of all variables used in the DiD estimating sample are provided in Appendix B, Table B2.

We next assess the impact of SA auctions on SME performance using our difference-in-differences framework. Figure 5 displays the dynamic effects of the 2014 reform on employment and wages. The event-study estimates reveal parallel pre-trends in both outcomes prior to the reform, supporting the validity of the DiD identification strategy. After 2014, employment and average earnings among government suppliers (GSs) increase steadily relative to non-GSs, with effects growing over time. Panels (a) and (b) show consistent results for employment when moving from a specification with establishment and year fixed effects to one including state–industry–time fixed effects. Similarly, Panels (c) and (d) indicate robust post-reform gains in average monthly earnings, confirming that the expansion of SA auctions translated into measurable improvements in SME labor outcomes.

Table 5 reports our baseline difference-in-differences estimates across increasingly demanding fixed-effects specifications.

Panel A focuses on employment effects. In the raw specification of Column (1), government suppliers (GSs) increased employment by 0.831 workers after the 2014 reform. The estimated effect rises slightly to 0.939 workers when establishment fixed effects are added in Column (3), indicating that controlling for time-invariant firm characteristics captures part of the underlying heterogeneity. The results remain stable at 0.947 and 0.933 workers as we progressively include state-by-year and industry-by-year fixed effects (Columns 4–5). Relative to the pre-treatment mean of 10.171 employees, our preferred specification in Column (5) implies an average employment gain of approximately 9.2%.

Panel B of Table 5 examines the reform's impact on average monthly earnings. The unconditional estimate in Column (1) indicates a wage increase of BRL 51.0 (standard error = 3.27). This effect declines modestly to BRL 48.5 after including year fixed effects (Column 2)

and to BRL 42.4 with establishment fixed effects (Column 3). The results stabilize around BRL 43 in the more demanding specifications with state-by-year and industry-by-year interactions (Columns 4–5). In relative terms, this corresponds to a 3.9% increase compared to the pre-treatment mean of BRL 1,102.5, suggesting that the expansion of SA auctions modestly improved labor earnings alongside employment growth.

Table A2 provides additional robustness checks by flexibly controlling for potential confounds. For employment (Panel A), adding spatial controls interacted with year fixed effects (Column 2) reduces the baseline effect of 0.939 workers to 0.884 workers. The estimate remains stable at 0.896 workers when we add pre-treatment firm characteristics interacted with year effects (Column 3). Our most demanding specification, which includes pre-treatment outcome controls (Column 4), yields an effect of 0.766 workers that remains highly significant (standard error = 0.096).

The earnings effects in Panel B of Table A2 show similar stability. The effect declines from the baseline 42.352 BRL to 36.038 BRL when adding spatial controls (Column 2), then increases slightly to 37.512 BRL with pre-treatment firm controls (Column 3). Our most demanding specification with pre-treatment outcome controls (Column 4) yields an effect of 37.322 BRL that remains precisely estimated (standard error = 2.754).

The stability of these estimates across increasingly demanding specifications suggests they capture real effects of the reform rather than differential trends across regions or firm types. The combination of employment and wage growth indicates that increased access to government contracts through SA auctions enabled SMEs to both expand their workforce and pay higher wages.

6 Cost-Benefit Analysis

On the one hand, our findings indicate that the policy effectively increased the share of SMEs winning public contracts. On the other hand, it did so at a considerable fiscal cost. In this section, we conduct a back-of-the-envelope calculation to quantify the associated fiscal burden and assess the cost-effectiveness of the policy in terms of employment creation.

Our RDD estimates show that the implementation of set-aside (SA) auctions increased procurement prices by approximately 12.1%. Given that total annual procurement spending affected by the policy amounts to roughly BRL 600 million, this implies an annual fiscal cost of about BRL 72.6 million, and a cumulative cost of BRL 508 million over the seven years

covered in our sample.

At the same time, our difference-in-differences estimates suggest that SMEs benefiting from the reform increased employment by 0.93 workers per firm after the introduction of SA auctions. Given that our sample comprises around 33,000 establishments, this corresponds to the creation of approximately 31,000 new jobs directly attributable to the policy.

Dividing the total fiscal cost of BRL 72.6 million by the estimated 31,000 jobs created yields an approximate cost per job of BRL 16,400 (around USD 2,800). While this figure is not particularly high compared to other job-creation programs in Brazil, it does not account for the potential efficiency losses and opportunity costs borne by larger firms that may have been displaced by the policy.[19]

7 Conclusion

This paper provides a comprehensive cost-benefit analysis of Set-Aside auctions for SMEs in Brazil's public procurement system. Our findings reveal a clear trade-off between procurement efficiency and SME development objectives. On one hand, restricting competition to SMEs increases procurement prices by 12.1%, leading to an annual fiscal cost of approximately BRL 72.6 million. Over the seven years covered in our sample, this translates to a cumulative cost of BRL 508.2 million. On the other hand, increased access to government contracts generates significant benefits for targeted firms, including a 9.2% increase in employment and a 3.9% increase in average wages. The consistency of our estimates across multiple empirical strategies and robustness checks strengthens confidence in these results. Our cost-benefit analysis suggests that the policy costs approximately BRL 16,400 (US$2,800) per job created, a figure that compares favorably to other job-creation policies.

The implications of our findings extend beyond the Brazilian context. As governments worldwide increasingly adopt preferential procurement policies to support SMEs, understanding their economic consequences is critical for evidence-based policymaking. Our results suggest that while SA auctions successfully increase SME participation in public procurement and generate meaningful employment gains, they do so at a measurable fiscal cost. This underscores the importance of carefully designing and targeting such policies to maximize their effectiveness. Future research could explore alternative mechanisms that achieve similar distributive goals with lower efficiency costs, such as bid preferences or capacity-building

[19] Corbi et al. (2019) find that a transfer program in Brazil generated employment at a cost of roughly USD 8,000 per job.

programs for SMEs. Additionally, examining longer-term effects on firm productivity, innovation, and market competitiveness would provide a more comprehensive assessment of these policies' broader economic impact. Ultimately, the decision to implement preferential procurement policies involves balancing efficiency and equity objectives, requiring policymakers to weigh economic trade-offs alongside broader social priorities.

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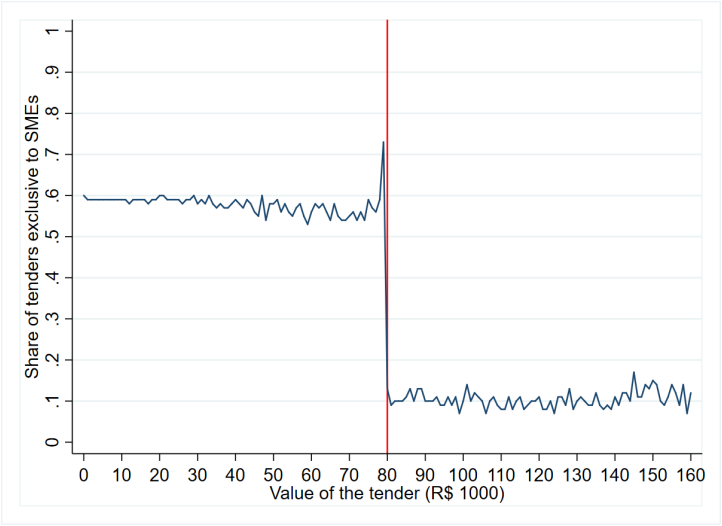
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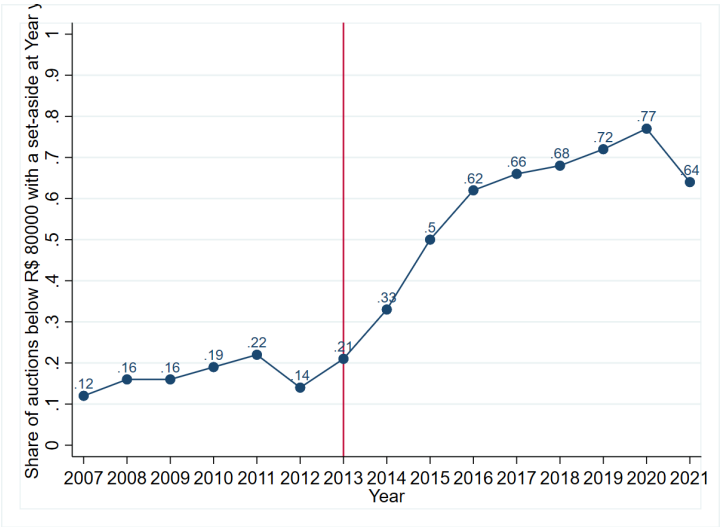
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Figure 1: Identifying Variation: SA Frequency around the Threshold and Over Time
A sharp discontinuity in the share of set-aside auctions below BRL 80,000 and a structural increase after 2014

(a) Frequency of set-aside auctions for SMEs as a function of the value of the lot being auctioned



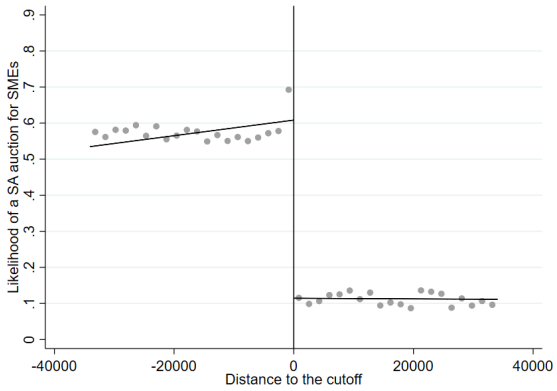
(b) Frequency of set-aside auctions for SMEs across years



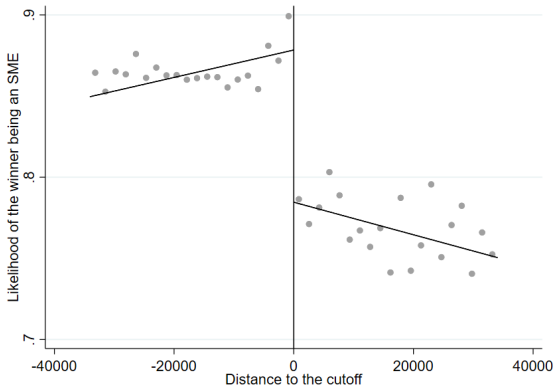
Notes: This figure describes the two sources of variation in set-aside policies exploited by this research project. Graph (a) shows the share of set aside (SA) auctions for Small and Medium Enterprises (SMEs) as a function of the value of the lot for the period 2014 and 2021. It documents a pronounced increase in the frequency of auctions with SA for those lots below 80 thousand reais, the cutoff for mandatory SA. For those auctions below the 80 thousand reais cutoff, Graph (b) displays the share of auctions with SA across years between 2007 and 2021. It shows a substantial increase in the share of auctions with SA auctions a

Figure 2: RD Estimates: Effects of SA Auctions on SME Participation and Prices
A sharp discontinuity at the BRL 80,000 threshold increases SME winning probability and raises prices

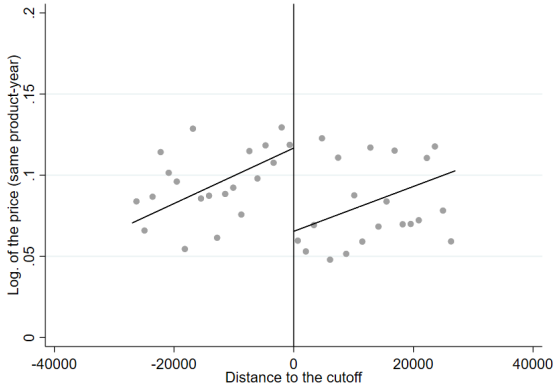
(a) First Stage, Set-Aside Auction



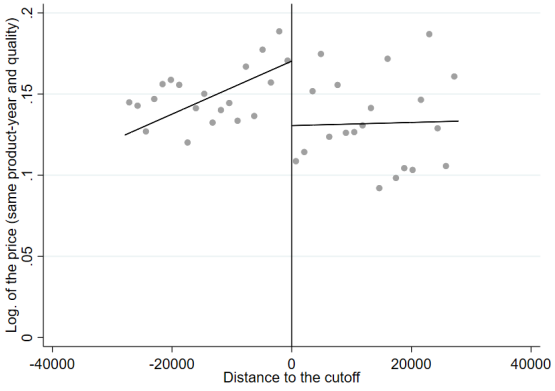
(b) Intention-to-Treat, SME Winner



(c) Intention-to-Treat, Log-Price



(d) Intention-to-Treat, Quality-Adjusted Prices



Notes: This figure shows four RD graphs using procurement data estimated by a Regression Discontinuity Design (RDD) specification using procurement data from 2014 to 2021. All specifications report a bias-corrected RDD coefficient estimated using a linear polynomial, triangular kernel, and MSERD bandwidth selection. Graph **(a)** shows the RDD graph describing one of our three First-Stage (FS) graphs: sharp RDD estimates of the impact of the cutoff indicator on the treatment variable within the bandwidths defined using the SME Winner indicator as an outcome. The FS graphs using Log-Price and Quality-Adjusted Prices as outcomes are similar to graph **(a)** and not reported. Graph **(b)** shows the RDD graph describing the Intention-to-Treat (ITT) graph with the SME Winner indicator as an outcome: sharp RDD estimates of the effect of the cutoff indicator on the likelihood of the auction winner being an SME. Graphs **(c)** and **(d)** replicate Graph (b) but use Log-Price (demeaned within product-year) and Quality-Adjusted Prices as dependent variables, respectively. We estimate the following FS regression model to quantify the effect of the treatment cutoff on the treatment variable $Set_Aside_i = \delta + \gamma \cdot \mathbf{1}(Value_{j(i)} < 80000) + f(Value_{j(i)}) + \epsilon_i$, where i denotes an auction and j is a tender that includes auction i. Set_Aside_i is an indicator taking value 1 when auction i is a SA for SMEs. $f(Value_{j(i)})$ is the RD polynomial. $Value_{j(i)}$ is the value of all products in auctions that belong to tender j. We estimate the following ITT regression model to quantify the effect of the treatment cutoff on our outcome variables $y_i = \alpha + \beta \cdot Set_Aside_i + f(Value_{j(i)}) + \epsilon_i$, where y_i is the procurement outcome of auction i. We cluster the Standard Errors (SEs) at the Purchasing Unit (PU) level.

Table 1: Main Effects of SA Auctions on SME Participation and Prices: RD Estimates
Restricting competition to SMEs substantially increases SME winning probability and moderately raises prices

	(1) MSE Winner	(2) Log(Prices)	(3) Log(Quality-Adjusted Prices)
Panel A: Intention-to-Treat			
RD estimate	-0.092	-0.062	-0.063
	[0.013]***	[0.015]***	[0.018]***
Panel B: First-Stage			
RD estimate	-0.512	-0.512	-0.522
	[0.013]***	[0.012]***	[0.013]***
Panel C: Second-Stage			
RD estimate	0.179	0.121	0.123
	[0.024]***	[0.030]***	[0.037]***
Bandwidth	27034	24006	22487
Observations	521922	516578	479351
N below	366293	362563	336639
N above	155629	154015	142712
Num. of clusters	3714	3677	3652
Outcome mean (control)	0.753		

Note: This table displays the treatment effect of SA auctions for Small and Medium Enterprises (SMEs) estimated by a conventional Regression Discontinuity Design (RDD) specification using the RDD estimating sample described in Subsection 4.1. All specifications report a bias-corrected RDD coefficient estimated with a linear polynomial, triangular kernel, and MSERD bandwidth selection. Panel A reports results from the Intention-to-Treat specification: sharp RDD estimates of the effect of the cutoff indicator on each outcome variable. Panel B reports results from the First-Stage specification: sharp RDD estimates of the effect of the cutoff indicator on the treatment variable within each RD bandwidth defined in Panel A. Panel C reports results from the Second-Stage specification: fuzzy RDD estimates of the effect of the treatment variable on each outcome variable. Column (1) shows RD estimates using the likelihood of an MSE winning a procurement auction, $WinnerSME_i$, as the dependent variable. Columns (2) and (3) replicate Column (1) but use, respectively, the logarithm of the unit price demeaned within product-year, $PriceIndex_i$, and the residual of the logarithm of the unit price regressed on product-year and brand FEs, $PriceIndexAdj_i$. We estimate the following *First-Stage* regression model to quantify the effect of the treatment cutoff on the treatment variable: $SetAside_i = \delta + \gamma \cdot \mathbf{1}(Value_{j(i)} < 80000) + f(Value_{j(i)}) + \epsilon_i$, where i denotes an auction and j a bundle that includes auction i. $SetAside_i$ is an indicator equal to one when auction i is a SA for SMEs. $f(Value_{j(i)})$ is the RD polynomial, and $Value_{j(i)}$ is the value of all products in auctions belonging to bundle j. We estimate the following *Intention-to-Treat* regression model to quantify the effect of the treatment cutoff on the outcome variables: $y_i = \alpha + \beta \cdot SetAside_i + f(Value_{j(i)}) + \epsilon_i$, where $y_i \in \{WinnerSME_i, PriceIndex_i, PriceIndexAdj_i\}$ is the procurement outcome of auction i. The *Second-Stage* RD coefficient in Panel C is the ratio of the *Intention-to-Treat* coefficient in Panel A, β, to the *First-Stage* coefficient in Panel B, γ. Standard errors, clustered at the purchasing agency (PA) level, are reported in square brackets. Coefficients significantly different from zero at the 99% (***), 95% (**), and 90% (*) confidence levels.

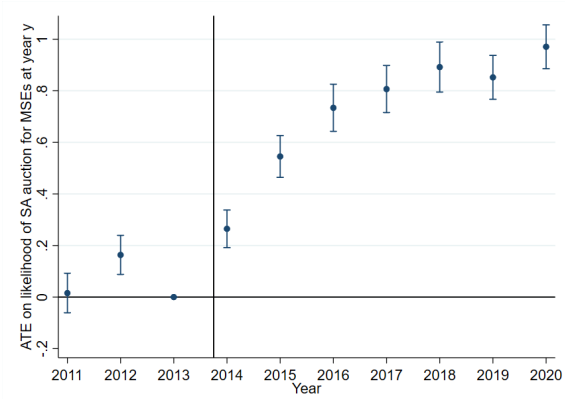
Table 2: Robustness: Donut RD Excluding Potentially Manipulated Observations
Main results remain virtually identical when excluding tenders close to the BRL 80,000 cutoff

	(1) MSE Winner	(2) Log(Prices)	(3) Log(Quality-Adjusted Prices)
Panel A: Intention-to-Treat			
RD estimate	-0.099	-0.063	-0.052
	[0.012]***	[0.010]***	[0.011]***
Panel B: First-Stage			
RD estimate	-0.490	-0.478	-0.478
	[0.013]***	[0.012]***	[0.012]***
Panel C: Second-Stage			
RD estimate	0.201	0.133	0.109
	[0.023]***	[0.023]***	[0.023]***
Bandwidth	32972	28708	30171
Donut hole	[77500;80000]	[77500;80000]	[77500;80000]
Observations	1002873	992357	932967
N below	784848	776576	731339
N above	218025	215781	201628
Num. of clusters	3071	3006	3028
Outcome mean (control)	0.766		

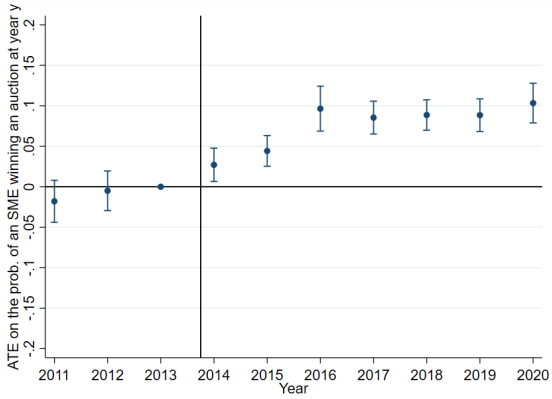
Note: This table displays the treatment effect of SA auctions for Micro and Small Enterprises (MSEs) estimated by a Donut Regression Discontinuity Design (RDD) specification using procurement data from 2014 to 2021 and a one-sided donut hole between BRL 77500 and BRL 80000. All specifications report a bias-corrected RDD coefficient estimated using a linear polynomial, triangular kernel, and MSERD bandwidth selection. Panel A displays results from the Intention-to-Treat specification: sharp RDD estimates of the impact of the cutoff indicator on each of the outcome variables specified in the columns. Panel B displays results from the First-Stage specification: sharp RDD estimates of the impact of the cutoff indicator on the treatment variable within each of the RD bandwidths defined in Panel A. Panel C displays results from the Second-Stage specification: fuzzy RDD estimates of the impact of the treatment variable on each of the outcome variables specified in the columns. Column (1) shows RD estimates using the likelihood of an MSE winning a procurement auction as the dependent variable. Columns (2) and (3) replicate Column (1) but use Log(Prices) (demeaned within product-year) and value for money as dependent variables, respectively. We estimate the following *First-Stage* regression model to quantify the effect of the treatment cutoff on the treatment variable $Set_Aside_i = \delta + \gamma \cdot \mathbf{1}(Value_{j(i)} < 80000) + f(Value_{j(i)}) + \epsilon_i$, where i denotes an auction and j is a tender that includes auction i. Set_Aside_i is an indicator taking value 1 when auction i is a SA for MSEs. $f(Value_{j(i)})$ is the RD polynomial. $Value_{j(i)}$ is the value of all products in auctions that belong to tender j. We estimate the following *Intention-to-Treat* regression model to quantify the effect of the treatment cutoff on our outcome variables $y_i = \alpha + \beta \cdot Set_Aside_i + f(Value_{j(i)}) + \epsilon_i$, where y_i is the procurement outcome of auction i. The *Second-Stage* RD coefficient in Panel C is the ratio between the *Intention-to-Treat* coefficient in Panel A β and the *First-Stage* coefficient γ in Panel B. We display standard errors (SEs) clustered at the Purchasing Unit (PU) level between squared brackets. Coefficients significantly different from zero at 99% (***), 95% (**), and 90% (*) confidence levels.

Figure 3: Effects of the 2014 Reform on SA Auctions, SME Participation, and Prices
Event-study estimates show parallel pre-trends and persistent price increases following the reform.

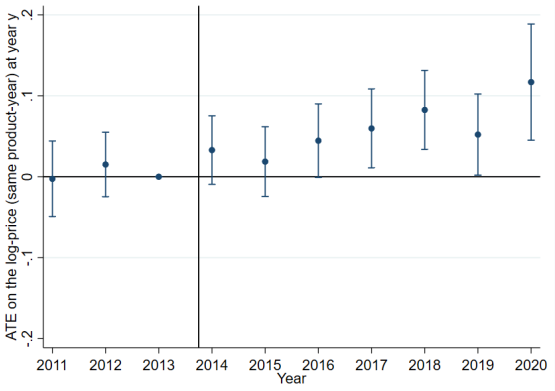
(a) Likelihood of SA auction for MSEs



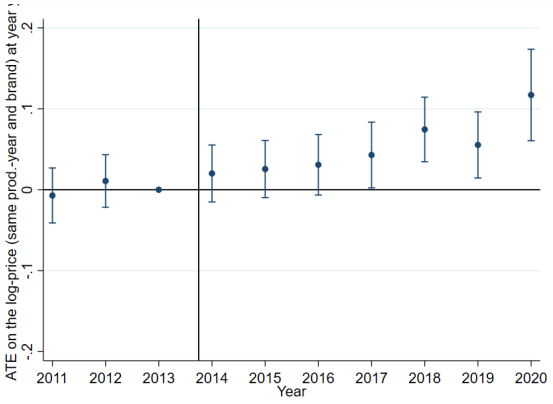
(b) Likelihood of a MSE winning the auction



(c) Log-price, product-year FEs



(d) Log-price, product-year FEs and brand FEs



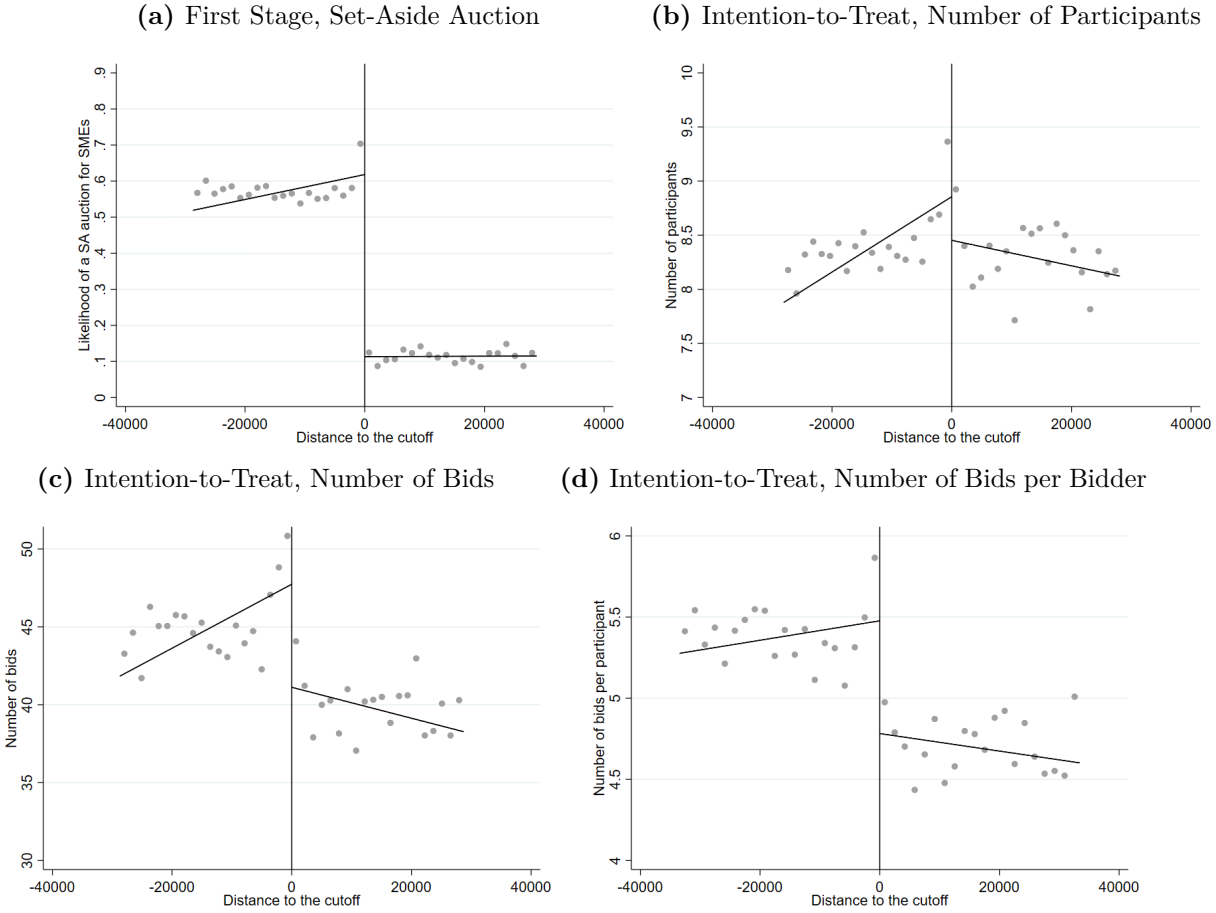
Notes: This figure reports estimates of the reform that made SA auctions for SMEs mandatory for purchases below BRL 80,000 on auction-level outcomes, using a difference-in-differences (DiD) specification and the DiD estimating sample described in Subsection 4.2. Graphs **(a)**–**(c)** show, respectively, the effects on the likelihood of an SA auction for SMEs, the likelihood of an SME winning, and log price (same product category), using a DiD with product-year fixed effects (FEs) and agency FEs. Graph **(d)** replicates the specification in Graph **(c)** with the addition of brand FEs. The DiD model in Graph **(d)** is $y_{i,t} = \alpha_{p(i),t} + \alpha_{a(i)} + \alpha_{b(i)} + \beta \cdot Intensity_{a(i)} \cdot Post_t + \epsilon_{i,t}$, where i denotes an auction and t the year. Respectively, $\alpha_{p(i),t}$ are product-year FEs, $\alpha_{a(i)}$ agency FEs, and $\alpha_{b(i)}$ brand FEs, used as a proxy for quality. $Intensity_a$ is the continuous treatment: the share of non-SA auctions by agency a in the pre-period (2011–2013), when SA auctions for SMEs were voluntary. $Post_t = \mathbb{1}(t > 2013)$ equals one after 2013, the year before the 2014 reform that made SA auctions for SMEs mandatory below BRL 80,000. Standard errors (SEs) are clustered at the establishment level and reported in squared brackets. Coefficients are marked ***, **, * if significant at the 99%, 95%, and 90% levels.

Table 3: Reform Effects: DiD Estimates with Continuous Treatment
The 2014 mandate increased SME participation and procurement prices in line with RD results.

	(1)	(2)	(3)	(4)	(5)
Panel A: Intention-to-Treat: Likelihood of SME winner					
Intensity*Post	0.084	0.077	0.075	0.072	0.072
	[0.009]***	[0.007]***	[0.007]***	[0.006]***	[0.006]***
Observations	6327361	6327361	6327361	6327361	6327361
Num. of clusters	2436	2436	2436	2436	2436
R-squared	0.286	0.352	0.376	0.518	0.518
First-stage coef.	0.578	0.607	0.571	0.570	0.570
First-stage F-stat.	377.548	451.813	369.112	381.687	382.091
Product FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Product × Year FEs	No	Yes	Yes	Yes	Yes
Agency FEs	No	No	Yes	Yes	Yes
Brand FEs	No	No	No	No	Yes
Run. Var. × Year FEs	No	No	No	Yes	Yes
Panel B: Intention-to-Treat: Log-price (same product)					
Intensity*Post	0.033	0.046	0.041	0.039	0.036
	[0.017]**	[0.016]***	[0.014]***	[0.012]***	[0.012]***
Observations	6327361	6327361	6327361	6327361	6327361
Num. of clusters	2436	2436	2436	2436	2436
R-squared	0.809	0.836	0.839	0.873	0.874
First-stage coef.	0.578	0.607	0.571	0.570	0.570
First-stage F-stat.	377.548	451.813	369.112	381.687	382.091
Product FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Product × Year FEs	No	Yes	Yes	Yes	Yes
Agency FEs	No	No	Yes	Yes	Yes
Brand FEs	No	No	No	No	Yes
Run. Var. × Year FEs	No	No	No	Yes	Yes

Note: This table reports estimates of the reform that made SA auctions for SMEs mandatory for purchases below BRL 80,000 on auction-level outcomes, using a difference-in-differences (DiD) specification with a discrete treatment and the DiD estimating sample. Panel A shows effects on the likelihood of an MSE winning; Panel B shows effects on log price (same product). Column (1) presents a DiD with product and year fixed effects (FEs). Columns (2)–(5) add, respectively, product-year FEs, agency FEs, year FEs interacted with bundle value, and brand FEs. The DiD model in column (5) is $y_{i,t} = \alpha_{p(i),t} + \alpha_{a(i)} + \alpha_{b(i)} + \beta \cdot Intensity_{a(i)} \cdot Post_t + \alpha_t \cdot Value_{j(i)} + \epsilon_{i,t}$, where i denotes an auction and t the year. Respectively, $\alpha_{p,t}$ are product-year FEs, α_a agency FEs, and α_b brand FEs. $Intensity_a$ is the continuous treatment: the share of non-SA auctions by agency a in the pre-period (2011–2013), when SA auctions for MSEs were voluntary. $Post_t = \mathbb{1}(t > 2013)$ equals one after 2013, the year before the 2014 reform that made SA auctions for MSEs mandatory below BRL 80,000. $Value_{j(i)}$ is the bundle value that includes auction i, the running variable of the RD. We also report the first-stage coefficient, using Set_Aside_i as the outcome, along with the corresponding F-statistics. Standard errors (SEs) are clustered at the PA level and reported in squared brackets. Coefficients are marked ***, **, * if significant at the 99%, 95%, and 90% levels.

Figure 4: Effect of SA Auctions on Entry, Competitiveness, and Bidding Activity
SA auctions for SMEs increase participation, bidding intensity, and competition, while also raising prices.



Notes: *Notes:* This figure shows RDD graphs displaying the treatment effect of SA auctions for SMEs on mechanism variables estimated by a conventional Regression Discontinuity Design (RDD) specification using the RDD estimating sample described in Subsection 4.1. All graphs report a bias-corrected RDD coefficient estimated with a linear polynomial, triangular kernel, and MSERD bandwidth selection, and robust SEs. Graph **(a)** reports results from the *First-Stage* specification: sharp RDD estimates of the effect of the cutoff indicator on the treatment variable within the bandwidths defined using the number of auction participants, $NumPart_i$, as the outcome. The First-Stage graphs using the number of bids, $NumBids_i$, and the number of bids per participant, $NumBidsPart_i$, as outcomes are similar to graph **(a)** and not reported. Graph **(b)** reports results from the *Intention-to-Treat* specification: sharp RDD estimates of the effect of the cutoff indicator on $NumPart_i$. Graphs **(c)** and **(d)** replicate graph **(b)** but use, respectively, $NumBids_i$ and $NumBidsPart_i$ as dependent variables. We estimate the following *First-Stage* regression model to quantify the effect of the treatment cutoff on the treatment variable: $SetAside_i = \delta + \gamma \cdot \mathbf{1}(Value_{j(i)} < 80000) + f(Value_{j(i)}) + \epsilon_i$, where i denotes an auction and j a bundle that includes auction i. $SetAside_i$ is an indicator equal to one when auction i is a SA for SMEs. $f(Value_{j(i)})$ is the RD polynomial, and $Value_{j(i)}$ is the value of all products in auctions belonging to bundle j. We estimate the following *Intention-to-Treat* regression model to quantify the effect of the treatment cutoff on the outcome variables: $y_i = \alpha + \beta \cdot SetAside_i + f(Value_{j(i)}) + \epsilon_i$, where $y_i \in \{NumPart_i, NumBids_i, NumBidsPart_i\}$ is the mechanism outcome of auction i. Standard errors, clustered at the purchasing agency (PA) level, are reported in square brackets.

Table 4: Effect of SA Auctions on Entry, Competitiveness, and Bidding Activity: ITT, First-Stage, and Second-Stage Estimates

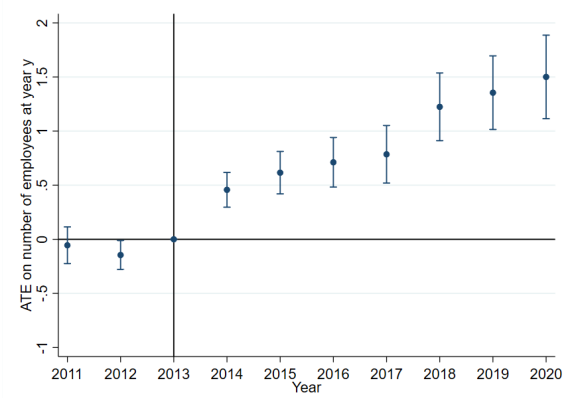
SA auctions for SMEs increase competitiveness, measured by entry, bidding activity, and bidding intensity, while also raising prices.

	(1) Num. Part.	(2) Num. Bids	(3) Num. Bids p/ Part.
Panel A: Intention-to-Treat			
RD estimate	-0.549	-8.038	-0.761
	[0.275]**	[1.422]***	[0.218]***
Panel B: First-Stage			
RD estimate	-0.565	-0.567	-0.553
	[0.011]***	[0.011]***	[0.011]***
Panel C: Second-Stage			
RD estimate	0.972	14.187	1.377
	[0.493]**	[2.492]***	[0.405]***
Bandwidth	28044	28691	33399
Observations	942647	942647	942647
N below	743489	743489	743489
N above	199158	199158	199158
Num. of clusters	2964	2970	3030
Outcome mean (control)	8.311	39.980	4.710

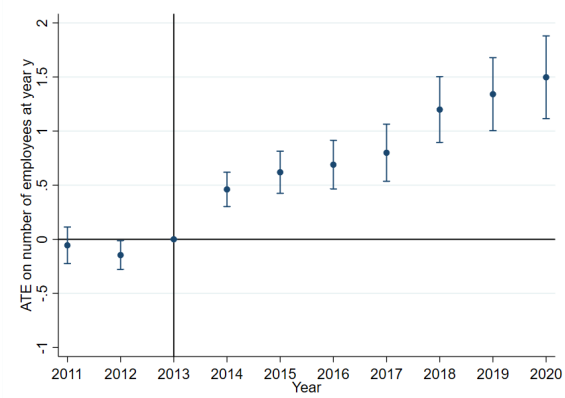
Note: This table displays the treatment effect of SA auctions for Small and Medium Enterprises (SMEs) on mechanism variables estimated by a conventional Regression Discontinuity Design (RDD) specification using the RDD estimating sample described in Subsection 4.1. All specifications report a bias-corrected RDD coefficient estimated with a linear polynomial, triangular kernel, and MSERD bandwidth selection, and robust SEs. Panel A reports results from the Intention-to-Treat specification: sharp RDD estimates of the effect of the cutoff indicator on each mechanism variable. Panel B reports results from the First-Stage specification: sharp RDD estimates of the effect of the cutoff indicator on the treatment variable within each RD bandwidth defined in Panel A. Panel C reports results from the Second-Stage specification: fuzzy RDD estimates of the effect of the treatment variable on each mechanism variable. Column (1) shows RD estimates using the number of auction participants, $NumPart_i$, as the dependent variable. Columns (2) and (3) replicate Column (1) but use, respectively, the number of bids, $NumBids_i$, and the number of bids per participant, $NumBidsPart_i$ as the dependent variable. We estimate the following *First-Stage* regression model to quantify the effect of the treatment cutoff on the treatment variable: $SetAside_i = \delta + \gamma \cdot \mathbf{1}(Value_{j(i)} < 80000) + f(Value_{j(i)}) + \epsilon_i$, where i denotes an auction and j a bundle that includes auction i. $SetAside_i$ is an indicator equal to one when auction i is a SA for SMEs. $f(Value_{j(i)})$ is the RD polynomial, and $Value_{j(i)}$ is the value of all products in auctions belonging to bundle j. We estimate the following *Intention-to-Treat* regression model to quantify the effect of the treatment cutoff on the outcome variables: $y_i = \alpha + \beta \cdot SetAside_i + f(Value_{j(i)}) + \epsilon_i$, where $y_i \in \{NumPart_i, NumBids_i, NumBidsPart_i\}$ is the procurement outcome of auction i. The *Second-Stage* RD coefficient in Panel C is the ratio of the *Intention-to-Treat* coefficient in Panel A, β, to the *First-Stage* coefficient in Panel B, γ. Standard errors, clustered at the purchasing agency (PA) level, are reported in square brackets. Coefficients significantly different from zero at the 99% (***), 95% (**), and 90% (*) confidence levels.

Figure 5: Effects of the 2014 Reform on Employment and Earnings
Event-study estimates show persistent post-reform increases in the number of employees and average monthly earnings among GSs relative to non-GSs.

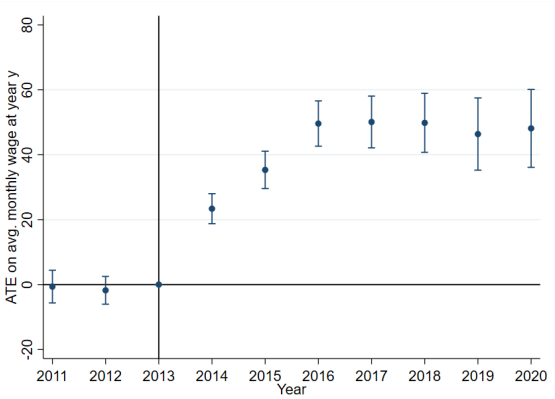
(a) TWFE, Number of employees



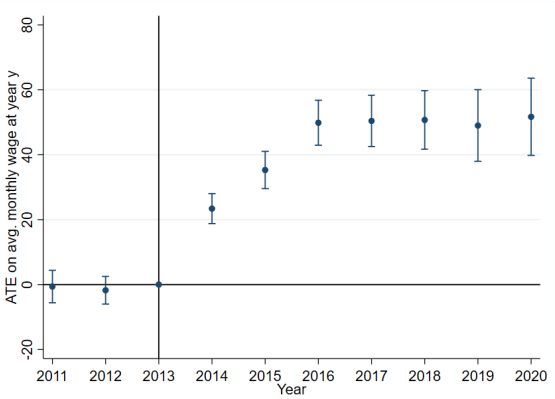
(b) State-Industry-Time FEs, N of employees



(c) TWFE, Monthly earnings



(d) State-Industry-Time FEs, Monthly earnings



Notes: This figure shows four DiD graphs using procurement data estimated by a TWFE specification using establishment data from 2011 to 2020. Each Graph shows estimates of the yearly effects of the reform that made SA auctions for MSEs mandatory for tenders below BRL 80000 on the establishment-level outcomes of MSEs. Respectively, Graphs (a) and (b) display the dynamic effects of the reform on the number of employees and average monthly earnings using the TWFE model $y_{e,t} = \alpha_e + \alpha_t + \beta \cdot GS_e \cdot Post_t + \epsilon_{e,t}$ where e denotes an establishment, and t time, which is the year. α_e captures establishment fixed effects (FEs), and α_t time FEs. $GS_{f(e)}$ is an indicator variable equal to one if the establishment e participated in a procurement auction in the pre-treatment period (2011-2013). $Post_t = \mathbb{1}(t > 2013)$ is an indicator variable that equals one after 2013, the first year before the 2014 reform that made SA auctions for MSEs mandatory for procurement tenders below BRL 80000. Respectively, Graphs **(c)** and **(d)**, mirror Graphs **(a)** and **(b)** but adding State-Industry-Time FEs $\alpha_{s(e),i(e),t}$ to the TWFE regression model. We display clustered standard errors (SEs) at the establishment level between squared brackets. Coefficients significantly different from zero at 99% (***), 95% (**), and 90% (*) confidence levels.

Table 5: Effects of the 2014 Reform on Employment and Earnings
Difference-in-differences estimates show that the expansion of SA auctions for MSEs led to moderate but significant increases in the number of employees and average monthly earnings

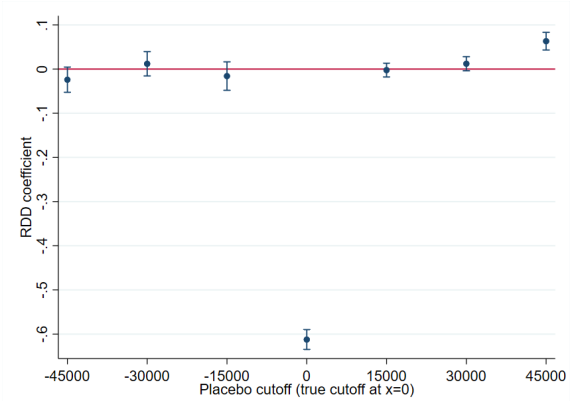
	(1)	(2)	(3)	(4)	(5)
Panel A: Number of employees					
Government Supplier*Post	0.831	0.833	0.939	0.947	0.933
	[0.107]***	[0.107]***	[0.100]***	[0.100]***	[0.099]***
Observations	540756	540756	540756	540756	540742
Num. of clusters	63280	63280	63280	63280	63280
R-squared	0.001	0.001	0.600	0.603	0.609
Outcome mean (pre-treatment)	10.171	10.171	10.171	10.171	10.171
Outcome S.D. (pre-treatment)	11.985	11.985	11.985	11.985	11.985
Year FEs	No	Yes	Yes	Yes	Yes
Establishment FEs	No	No	Yes	Yes	Yes
State × Year FEs	No	No	No	Yes	Yes
Industry × Year FEs	No	No	No	Yes	Yes
State × Industry × Year FEs	No	No	No	No	Yes
Panel B: Avg. monthly earnings					
Government Supplier*Post	51.028	48.504	42.352	43.082	43.178
	[3.268]***	[3.249]***	[2.817]***	[2.764]***	[2.760]***
Observations	540756	540756	540756	540756	540742
Num. of clusters	63280	63280	63280	63280	63280
R-squared	0.115	0.153	0.770	0.774	0.776
Outcome mean (pre-treatment)	1102.494	1102.494	1102.494	1102.494	1102.494
Outcome S.D. (pre-treatment)	466.964	466.964	466.964	466.964	466.964
Year FEs	No	Yes	Yes	Yes	Yes
Establishment FEs	No	No	Yes	Yes	Yes
State × Year FEs	No	No	No	Yes	Yes
Industry × Year FEs	No	No	No	Yes	Yes
State × Industry × Year FEs	No	No	No	No	Yes

Note: This table displays estimates of the effect of the reform that made SA auctions for MSEs mandatory for tenders below BRL 80000 on the establishment-level outcomes of MSEs using a difference-in-differences (DiD) specification and yearly data between 2011 and 2020 from the matched sample. Respectively, Panels A and B display the effects of the reform on the number of employees and average monthly earnings. Column (1) presents the results of a classic DiD specification without fixed effects (FEs). Respectively, Columns (2) to (5) include, respectively, year FEs, establishment FEs, industry-time FEs and state-time FEs, and state-industry-time FEs to the specification in Column (1). The DiD regression model reported in Column (5) has the form $y_{e,t} = \alpha_e + \alpha_{i,s,t} + \alpha_{s,t} + \beta \cdot GS_e \cdot Post_t + \epsilon_{e,t}$ where e denotes an establishment, and t time, which is the year. α_e captures establishment fixed effects (FEs), $\alpha_{s,i,t}$ stete-industry-time FEs. GS_e is an indicator variable equal to if the establishment e participated in a procurement auction in the pre-treatment period (2011-2013). $Post_t = \mathbb{1}(t > 2013)$ is an indicator variable that equals one after 2013, the first year before the 2014 reform that made SA auctions for MSEs mandatory for procurement tenders below BRL 80000. We display clustered standard errors (SEs) at the establishment level between squared brackets. Coefficients significantly different from zero at 99% (***), 95% (**), and 90% (*) confidence levels.

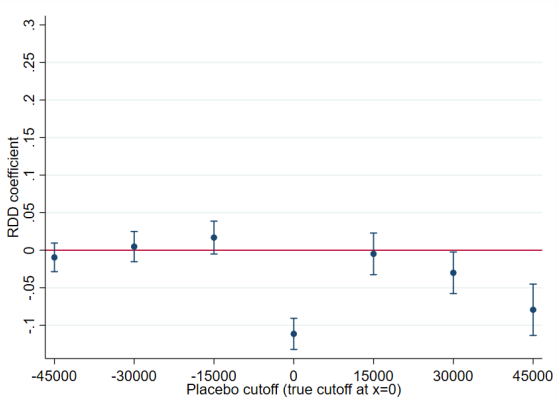
A Additional Figures and Tables

Figure A1: Placebo Cutoffs
Main outcomes show no discontinuities at artificial thresholds.

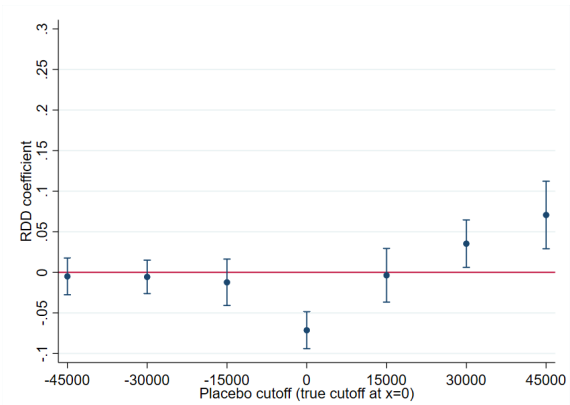
(a) First Stage, Set-Aside Auction



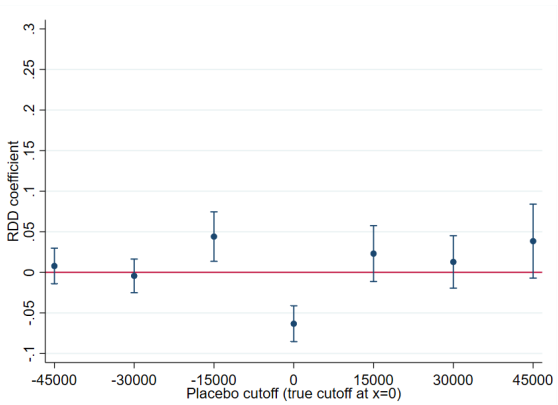
(b) Intention-to-Treat, MSE Winner



(c) Intention-to-Treat, Log-Price



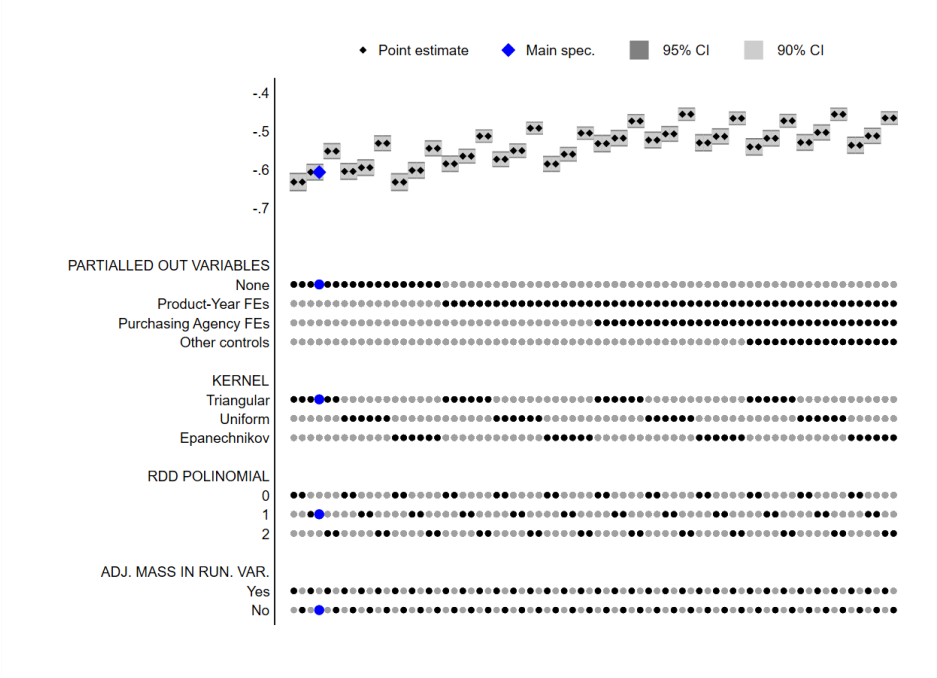
(d) Intention-to-Treat, Quality-Adjusted Prices



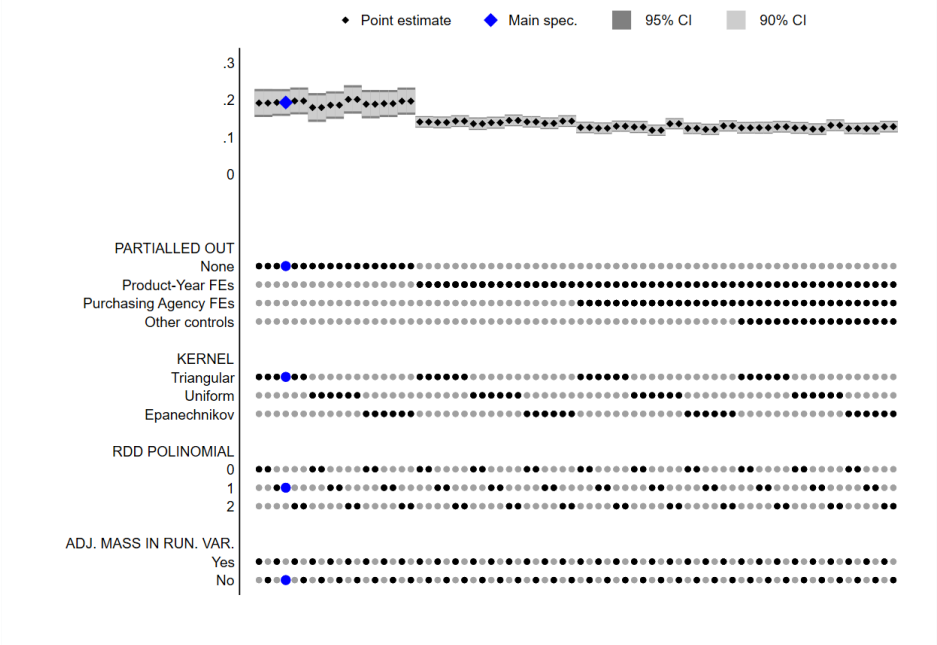
Notes: This Figure displays four graphs with Conventional RDD estimates for six placebo cutoffs. Respectively, Graphs **(a)** to **(d)** display placebo RDD estimates using the outcomes: **(a)** the likelihood of a SA auction for MSEs, **(b)** the likelihood of an SME winner, **(c)** the log-price (same product-year), and **(d)** the log-price adjusted (same product-year and quality). Specifically, for each alternative cutoff $C_k \in \{35000, 50000, 65000, 95000, 110000, 135000\}$, we estimate the first-stage regression model: $y_i = \delta + \gamma^k \cdot \mathbf{1}(Value_{j(i)} < C_k) + f(Value_{j(i)}) + \epsilon_i$ using the subsample of auctions with $Value_{j(i)} < 80000$ if $C_k < 80000$, and the subsample with $Value_{j(i)} > 80000$ if $C_k > 80000$. Then, in each Graph, we plot γ^k on the y-axis and $C_k - 80000$ on the x-axis. i denotes an auction and j is a bundle that includes auction i. y_i is the outcome of auction i. $f(Value_{j(i)})$ is the RD polynomial. $Value_{j(i)}$ is the value of all products in auctions that belong to bundle j. We cluster standard errors (SEs) at the Purchasing Agency (PA) level.

Figure A2: Sensitivity of RDD Estimates to Specification Choices
First and second stages remain stable across kernels, polynomial orders, and fixed effects.

(a) Likelihood of SA auction



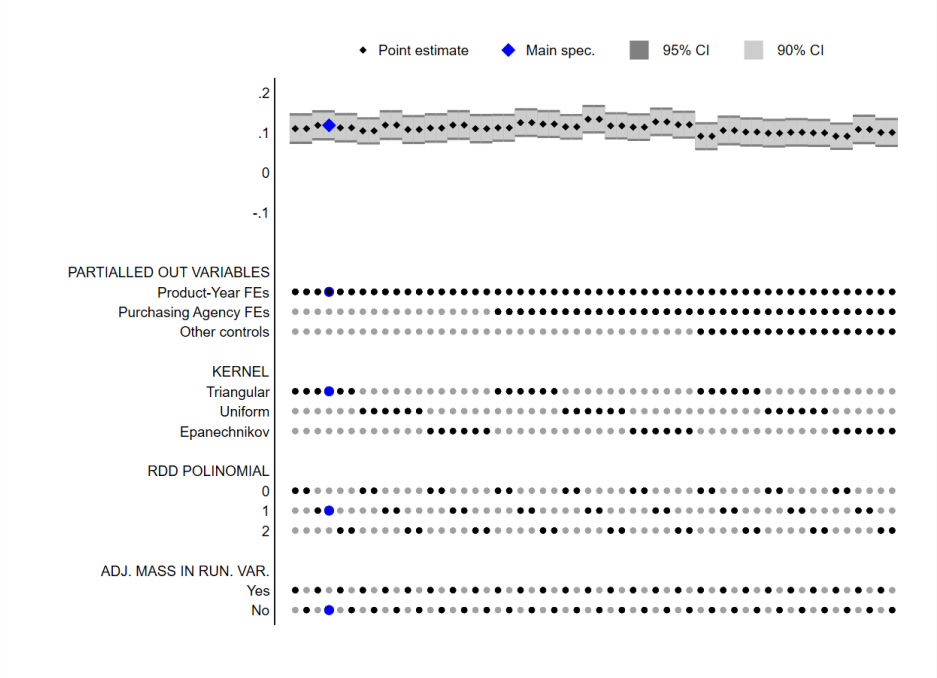
(b) Likelihood of SME Winner



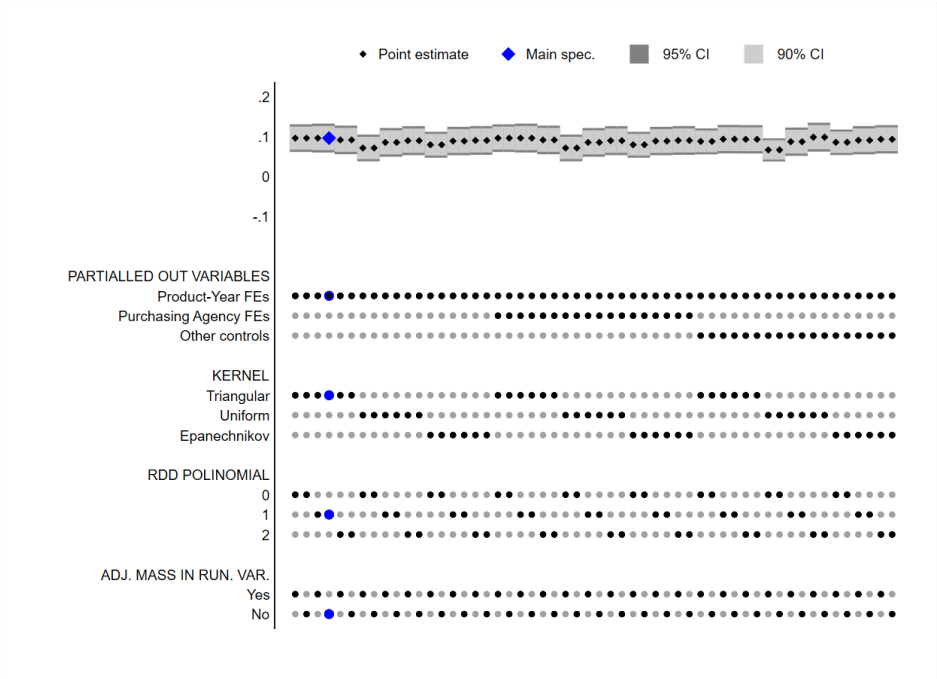
Notes: This figure presents a robustness test for our RD specification from Figure 2. We evaluate robustness by systematically varying key specification characteristics. First, we test three different kernel functions (Triangular, Uniform, and Epanechnikov). Next, we vary the polynomial order used for approximation. Finally, we toggle the mass adjustment on and off. This process results in a total of 18 different estimates, each altering one of these characteristics. Figure A2a displays the robustness results for the likelihood of a set-aside auction, while Figure A2b presents the results for the likelihood of an SME winning the auction.

Figure A3: Sensitivity of RDD Price Estimates
Log-price effects remain stable across kernels, polynomial orders, and control sets.

(a) Log-price (same product-year)

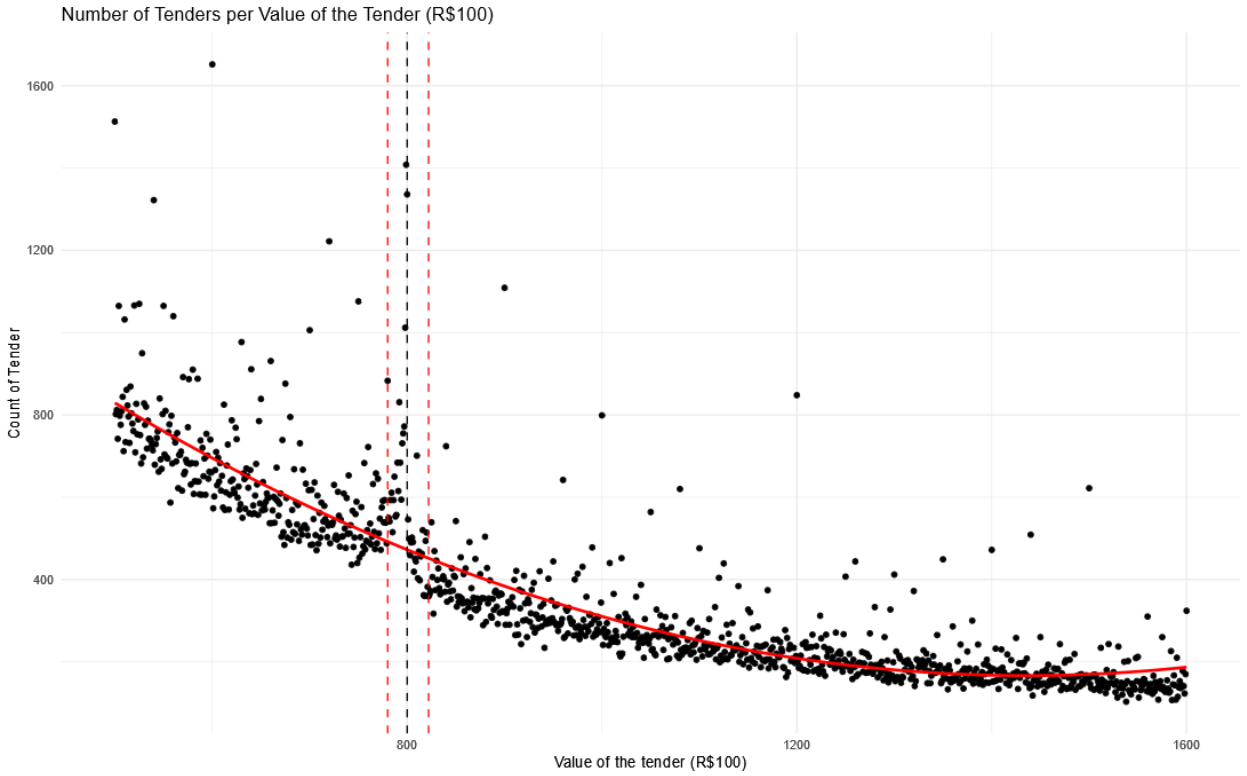


(b) Log-price (same product-year and quality)



Notes: This figure presents a robustness test for our RD specification from Figure 2. We evaluate robustness by systematically varying key specification characteristics. First, we test three different kernel functions (Triangular, Uniform, and Epanechnikov). Next, we vary the polynomial order used for approximation. Finally, we toggle the mass adjustment on and off. This process results in a total of 18 different estimates, each altering one of these characteristics. Figure A3a displays the robustness results for the log price residual, while Figure A3 presents the results for value for money.

Figure A4: Distribution of Tender Values around the BRL 80,000 Threshold
A moderate excess mass just below the cutoff motivates the Donut RDD specification



Notes: This figure shows the frequency distribution of procurement tenders by their total value, with particular focus around the BRL 80,000 threshold that determines whether set-aside (SA) auctions for Small and Medium Enterprises (SMEs) are mandatory. The x-axis represents the tender value in hundreds of Brazilian Reals (BRL), while the y-axis shows the frequency count. The vertical black dashed line indicates the BRL 80,000 threshold. The moderate excess mass observed just below the cutoff suggests strategic manipulation of tender values by purchasing agencies to fall below the threshold, potentially to maintain flexibility in procurement methods.

Table A1: Robustness: DiD with Discrete Treatment

Reform effects on SME participation and prices are consistent with those from the RDD and DiD with continuous treatment.

	(1)	(2)	(3)	(4)	(5)
Panel A: Intention-to-Treat: Likelihood of SME winner					
Intensity*Post	0.032	0.029	0.028	0.026	0.027
	[0.007]***	[0.005]***	[0.005]***	[0.004]***	[0.004]***
Observations	6327361	6327361	6327361	6327361	6327361
Num. of clusters	2436	2436	2436	2436	2436
R-squared	0.285	0.351	0.375	0.518	0.518
First-stage coef.	0.218	0.228	0.210	0.209	0.209
First-stage F-stat.	88.497	106.878	92.937	94.987	95.148
Product FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Product × Year FEs	No	Yes	Yes	Yes	Yes
Agency FEs	No	No	Yes	Yes	Yes
Brand FEs	No	No	No	No	Yes
Run. Var. × Year FEs	No	No	No	Yes	Yes
Panel B: Intention-to-Treat: Log-price (same product)					
Intensity*Post	0.013	0.021	0.018	0.020	0.018
	[0.010]	[0.009]**	[0.008]**	[0.006]***	[0.006]***
Observations	6327361	6327361	6327361	6327361	6327361
Num. of clusters	2436	2436	2436	2436	2436
R-squared	0.809	0.836	0.839	0.873	0.874
First-stage coef.	0.218	0.228	0.210	0.209	0.209
First-stage F-stat.	88.497	106.878	92.937	94.987	95.148
Product FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Product × Year FEs	No	Yes	Yes	Yes	Yes
Agency FEs	No	No	Yes	Yes	Yes
Brand FEs	No	No	No	No	Yes
Run. Var. × Year FEs	No	No	No	Yes	Yes

Note: This table reports estimates of the reform that made SA auctions for SMEs mandatory for purchases below BRL 80,000 on auction-level outcomes, using a difference-in-differences (DiD) specification with a discrete treatment and the DiD estimating sample. Panel A shows effects on the likelihood of an MSE winning; Panel B shows effects on log price (same product). Column (1) presents a DiD with product and year fixed effects (FEs). Columns (2)–(5) add, respectively, product-year FEs, agency FEs, year FEs interacted with bundle value, and brand FEs. The DiD model in column (5) is $y_{i,t} = \alpha_{p(i),t} + \alpha_{a(i)} + \alpha_{b(i)} + \beta \cdot Treatment_{a(i)} \cdot Post_t + \alpha_t \cdot Value_{j(i)} + \epsilon_{i,t}$, where i denotes an auction and t the year. Respectively, $\alpha_{p,t}$ are product-year FEs, α_a agency FEs, and α_b brand FEs. $Intensity_a$ is the continuous treatment: the share of non-SA auctions by agency a in the pre-period (2011–2013), when SA auctions for MSEs were voluntary. $Treatment_{a(i)}$ is the discrete treatment: an indicator equal to one if PA a organizing auction i has $Intensity_{a(i)}$ above the PA-median. Formally, $Treatment_{a(i)} = \mathbf{1}\Big[Intensity_{a(i)} > P_{50}\Big(\{Intensity_{a(i)}\}_{i=1}^{N_a}\Big)\Big]$. $Post_t = \mathbb{1}(t > 2013)$ equals one after 2013, the year before the 2014 reform that made SA auctions for MSEs mandatory below BRL 80,000. $Value_{j(i)}$ is the bundle value that includes auction i, the running variable of the RD. We also report the first-stage coefficient, using Set_Aside_i as the outcome, along with the corresponding F-statistics. Standard errors (SEs) are clustered at the PA level and reported in squared brackets. Coefficients are marked ***, **, * if significant at the 99%, 95%, and 90% levels.

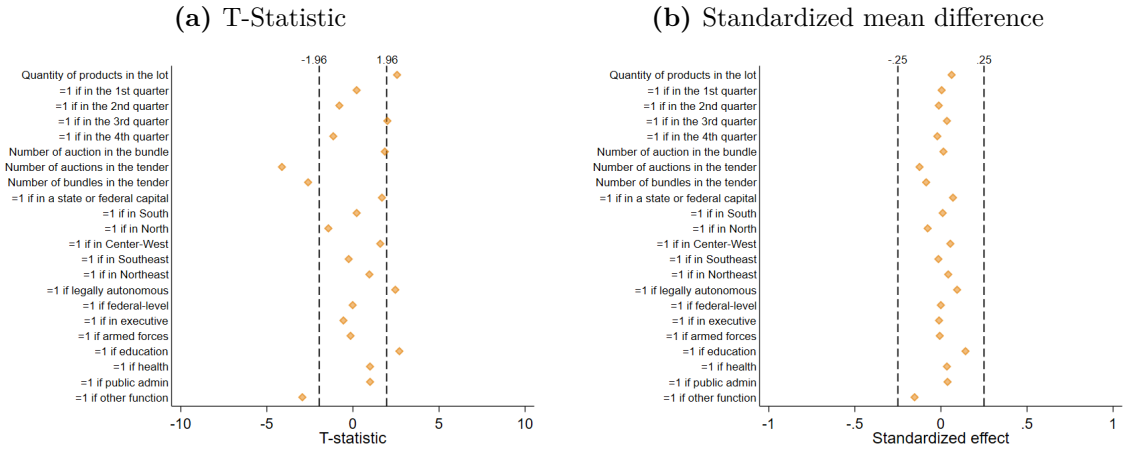
Table A2: Robustness of Employment and Earnings Effects

Estimates remain stable when adding additional firm-level controls and more demanding parametric specifications.

	(1)	(2)	(3)	(4)
Panel A: Number of employees				
Government Supplier*Post	0.939	0.884	0.896	0.766
	[0.100]***	[0.101]***	[0.101]***	[0.096]***
Observations	540756	540756	540734	540734
Num. of clusters	63280	63280	63280	63280
R-squared	0.600	0.603	0.603	0.662
Outcome mean (pre-treatment)	10.171	10.171	10.171	10.171
Outcome S.D. (pre-treatment)	11.985	11.985	11.985	11.985
Year FEs	Yes	Yes	Yes	Yes
Establishment FEs	Yes	Yes	Yes	Yes
Spatial controls × Year FEs	No	Yes	Yes	Yes
Pre-treatment controls × Year FEs	No	No	Yes	Yes
Pre-treatment outcomes × Year FEs	No	No	No	Yes
Panel B: Avg. monthly earnings				
Government Supplier*Post	42.352	36.038	37.512	37.322
	[2.817]***	[2.840]***	[2.827]***	[2.754]***
Observations	540756	540756	540734	540734
Num. of clusters	63280	63280	63280	63280
R-squared	0.770	0.773	0.775	0.791
Outcome mean (pre-treatment)	1102.494	1102.494	1102.494	1102.494
Outcome S.D. (pre-treatment)	466.964	466.964	466.964	466.964
Year FEs	Yes	Yes	Yes	Yes
Establishment FEs	Yes	Yes	Yes	Yes
Spatial controls × Year FEs	No	Yes	Yes	Yes
Pre-treatment controls × Year FEs	No	No	Yes	Yes
Pre-treatment outcomes × Year FEs	No	No	No	Yes

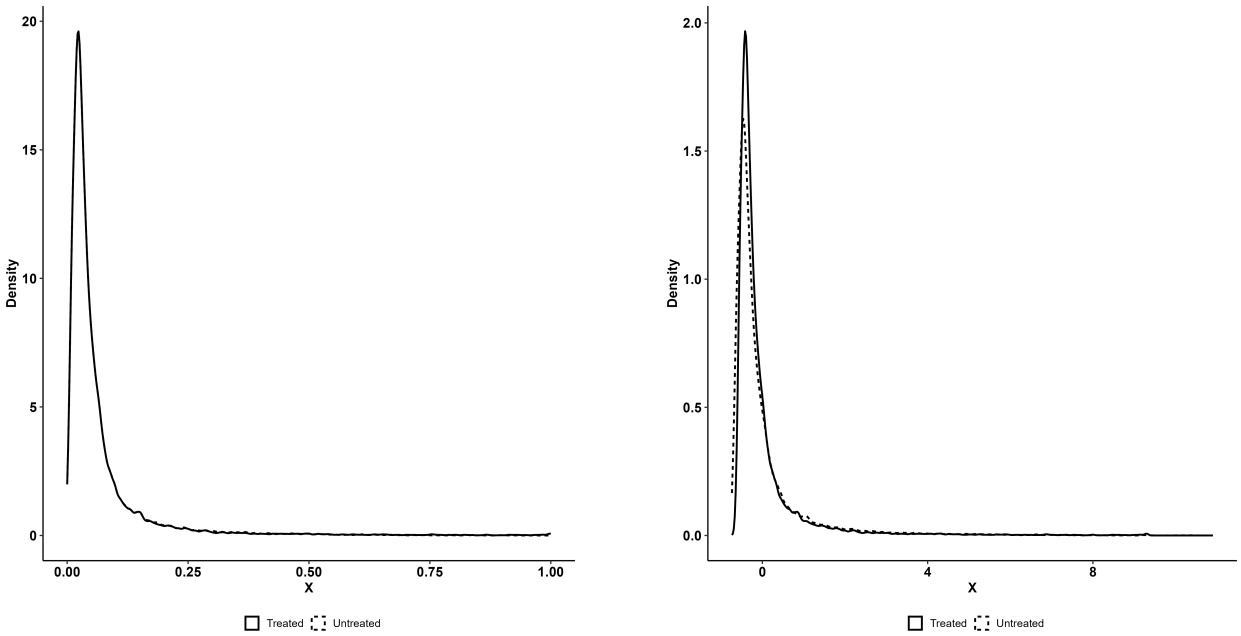
Note: This table displays estimates of the effect of the reform that made SA auctions for MSEs mandatory for tenders below BRL 80000 on the establishment-level outcomes of MSEs using a demanding difference-in-differences (DiD) specification with pre-treatment control variables interacted with the time indicators and yearly data between 2011 and 2020 from the post-matching sample. Respectively, Panels A and B display the effects of the reform on the number of employees and average monthly earnings. Column (1) presents the results from the TWFE regression model with establishment and year FEs displayed in Column (3) of Table 5. Respectively, Columns (2) to (4) include year FEs interacted with pre-treatment outcomes $(\mathbf{y}_{2011,e}, \mathbf{y}_{2011,e}, \mathbf{y}_{2013,e})$, pre-treatment controls $(\mathbf{x}_{2011,e}, \mathbf{x}_{2011,e}, \mathbf{x}_{2013,e})$, and spatial controls $x_{m(e)}$ to the specification in Column (1). The DiD regression model reported in Column (4) has the form $y_{e,t} = \alpha_e + \alpha_t + \beta \cdot GS_{f(e)} \cdot Post_t + \gamma_t \cdot \mathbf{x}_e + \epsilon_{e,t}$, where e denotes an establishment, f firm, and t time, which is the year. α_e captures establishment fixed effects (FEs), α_t year FEs. $GS_{f(e)}$ is an indicator variable equal to if the firm f of which establishment e belongs participated in a procurement auction in the pre-treatment period (2011-2013). $Post_t = \mathbb{1}(t > 2013)$ is an indicator variable that equals one after 2013, the first year before the 2014 reform that made SA auctions for MSEs mandatory for procurement tenders below BRL 80000. $\mathbf{x}_e$ is a vector with our pre-treatment controls. $\gamma_t \cdot \mathbf{x}_e$ are time-specific slopes controlling for the influence of pre-treatment outcomes, establishments' characteristics, and features of the municipality of establishments' location on the change in their outcomes across time. We display clustered standard errors (SEs) at the firm level between squared brackets. Coefficients significantly different from zero at 99% (***), 95% (**), and 90% (*) confidence levels.

Figure A5: Differences in control variables between auctions with and without set-aside for SMEs have a small magnitude.



Notes: This figure reports statistics that check whether treatment and control observations used in the Conventional RDD with procurement outcomes display balance on covariates. Respectively, Graphs **(a)** and **(b)** display the T-statistic and the Standardized mean difference for the 21 control variables in the RDD estimating sample. We standardize the values of each baseline control $x_{i,k} \in \mathbf{x}_i = \left(\mathbf{x}_{1,i}, \mathbf{x}_{2,j(i)}, \mathbf{x}_{3,k(i)}, \mathbf{x}_{4,a(i)}\right)$ by computing $x_{i,k}^{\text{STD}} = \frac{x_{i,k} - \widehat{\mu}_x}{\widehat{\sigma}_x}$, where $\widehat{\mu}_x = \frac{1}{N}\sum_{i=1}^{N} x_{i,k}$ and $\widehat{\sigma}_x = \sqrt{\frac{1}{N-1}\sum_{i=1}^{N}(x_{i,k} - \widehat{\mu}_x)^2}$ are the sample mean and standard deviation, respectively. Then, we estimate our Conventional RDD specification, with a triangular kernel, using each standardized baseline control $x_{i,k}^{\text{std}}$ as the outcome variable: $x_{i,k}^{\text{std}} = \delta + \gamma \cdot \mathbf{1}(Value_{j(i)} < 80000) + f(Value_{j(i)}) + \epsilon_i$, where i denotes an auction and j is a bundle that includes auction i. Set_Aside_i is an indicator taking value 1 when auction i is a set-aside for MSEs. $f(Value_{j(i)})$ is the RD polynomial. $Value_{j(i)}$ is the value of all products in auctions that belong to bundle j. We cluster standard errors (SEs) at the Purchasing Agency (PA) level.

Figure A6: Propensity Score Distributions of Treated and Control Establishments



(a) Propensity Score (unscaled)

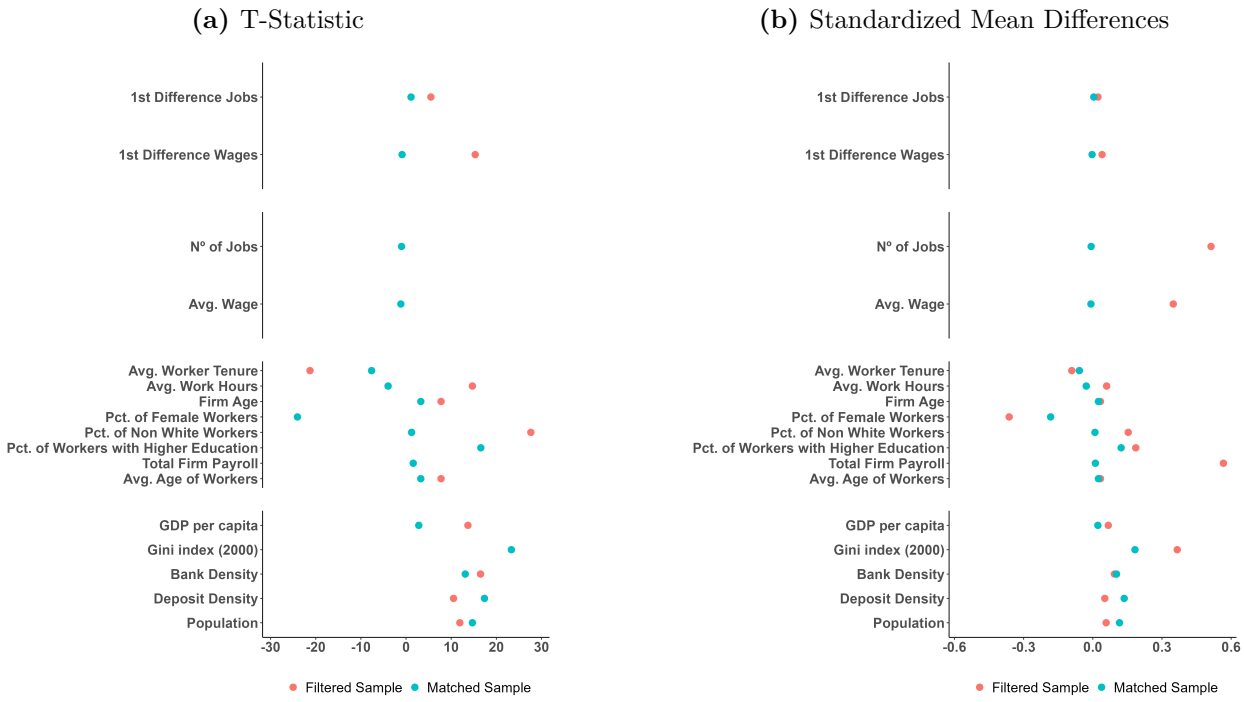
(b) Propensity Score (scaled)

Notes: This figure displays the sample distribution of the Propensity Score (PS) across establishments in the matched sample of treated (GS) and control (non-GS) firms. Panel (a) shows the unscaled PS distributions for treated (solid line) and untreated (dashed line) observations, while Panel (b) shows the corresponding scaled distributions. The scaling is based on a within-group standardization defined as

$$PS_{i,g}^{S} = \frac{PS_{i,g} - \overline{PS}_g}{SD_g(PS_{i,g})},$$

where $PS_{i,g}$ is the propensity score of observation i in group $g \in \{GS, GSC\}$, $\overline{PS}_g$ is the sample mean, and $SD_g(PS_{i,g})$ is the estimated standard deviation of the propensity score in group g. The similarity of the distributions across treated and control groups suggests that our matching procedure ensures a balanced sample that satisfies the common support assumption.

Figure A7: Improved Covariate Balance Between Treated and Control Establishments
Matching substantially reduces pre-treatment differences between GSs and non-GSs



Notes: This figure displays balance-check statistics from the filtered sample (blue dots) and the matched sample (red dots). Graph **(a)** shows the T-statistic for the mean difference between GSs (treatment group) and non-GSs (control group), testing the hypothesis $H_0 : \mu_x(GS = 1) = \mu_x(GS = 0)$, where x represents one covariate, GS is the treatment indicator, and μ_x is the mean of x. Graph **(b)** reports the standardized mean difference $\Delta_x = \frac{\mu_x(GS=1) - \mu_x(GS=0)}{\sigma_x}$, where σ_x is the standard deviation of x. We calculate both statistics by regressing each standardized baseline covariate $\frac{x}{\sigma_x}$ on our treatment indicator $GS_{f(e)}$ using robust SEs clustered at the establishment level.

B Data

B.1 Variable Definitions and Descriptive Statistics

Table B1: Description of additional variables in the RDD estimating sample

Variable	Description	Source
Panel A: Outcomes		
SME winner	Indicator variable equal to 1 if the auction winner is a Small and Medium Enterprise (SME), and 0 otherwise.	CNET
Log-price	Residual from the regression of the logarithm of the unit price on product-year fixed effects. Constructed using the universe of auctions between 2013 and 2021.	CNET
Log-price (adjusted)	Residual from the regression of the logarithm of the unit price on product-year, product quality, and lot-size fixed effects. Constructed using the universe of auctions between 2013 and 2021.	CNET
Panel B: Treatment		
Set-aside	Indicator variable equal to 1 if the auction is a set-aside (SA) for SMEs, and 0 otherwise.	CNET
Panel C: RDD controls		
Below cutoff	Indicator variable equal to 1 if the bundle value is below the BRL 80,000 cutoff, and 0 otherwise.	CNET
Bundle value	Value of the bundle to which the auction belongs. The bundle and auction coincide if the bundle contains a single lot.	CNET
Panel D: Auction level controls		
Quantity	Quantity of products in the auction lot. Winsorized at the 99th percentile.	CNET
Quarter: First	Indicator variable equal to 1 if the auction occurs in the first quarter, and 0 otherwise.	CNET
Quarter: Second	Indicator variable equal to 1 if the auction occurs in the second quarter, and 0 otherwise.	CNET
Quarter: Third	Indicator variable equal to 1 if the auction occurs in the third quarter, and 0 otherwise.	CNET
Quarter: Fourth	Indicator variable equal to 1 if the auction occurs in the fourth quarter, and 0 otherwise.	CNET
Panel E: Bundle level controls		
Bundle size (auctions)	Number of auctions in the bundle. Winsorized at the 99th percentile.	CNET
Panel F: Tender level controls		
Tender size (auctions)	Number of auctions in the tender. Winsorized at the 99th percentile.	CNET
Tender size (bundles)	Number of bundles in the tender. Winsorized at the 99th percentile.	CNET
Panel G: Purchasing agency (PA) level controls		
Capital	Indicator variable equal to 1 if the purchasing agency (PA) is located in a state or federal capital, and 0 otherwise.	CNET
Region: South	Indicator variable equal to 1 if the PA is located in the South region, and 0 otherwise.	CNET
Region: North region	Indicator variable equal to 1 if the PA is located in the North region, and 0 otherwise.	CNET
Region: Center-West	Indicator variable equal to 1 if the PA is located in the Center-West region, and 0 otherwise.	CNET
Region: Southeast	Indicator variable equal to 1 if the PA is located in the Southeast region, and 0 otherwise.	CNET
Region: Northeast	Indicator variable equal to 1 if the PA is located in the Northeast region, and 0 otherwise.	CNET
Legal autonomy	Indicator variable equal to 1 if the PA has legal autonomy, and 0 otherwise.	CNET
Federal	Indicator variable equal to 1 if the PA belongs to the federal sphere, and 0 otherwise.	CNET
Executive power	Indicator variable equal to 1 if the PA belongs to the executive power, and 0 otherwise.	CNET
Public function: Armed forces	Indicator variable equal to 1 if the PA performs armed forces functions, and 0 otherwise.	CNET
Public function: Education	Indicator variable equal to 1 if the PA performs education-related functions, and 0 otherwise.	CNET
Public function: Health	Indicator variable equal to 1 if the PA performs health-related functions, and 0 otherwise.	CNET
Public function: Public Adm.	Indicator variable equal to 1 if the PA performs public administration functions, and 0 otherwise.	CNET
Public function: Other	Indicator variable equal to 1 if the PA performs other public functions, and 0 otherwise.	CNET

Notes: Elaborated by the authors. SME = Small and Medium Enterprise; SA = Set-Aside.

Table B2: Description of the variables in the DiD estimating sample

Variable	Description	Source
Panel A: Outcomes		
Number of employees	Number of active job relations at the establishment level at December 31	RAIS
Average monthly earnings	Average monthly earnings of the active employees within an establishment at December 31	RAIS
Total yearly payroll	Total yearly payroll of the establishment	RAIS
Average monthly working hours	Average hours worked by the active employees within an establishment at December 31	RAIS
Panel B: Treatment		
=1 if estab. is a Government Supplier (GS)	Indicator variable equal to one if the establishment participated in a procurement auction during the pre-treatment (2011-2013)	CNET
Panel C: Employee controls		
Percentage of female employees	Percentage of female employees within an establishment among those active at December 31	RAIS
Percentage of employees with a college degree	Percentage of workers with college or more within an establishment among those active at December 31	RAIS
Average tenure of employees	Average monthly tenure of the active employees within an establishment at December 31	RAIS
Average age of employees	Average age of active employees within an establishment level at December 31	RAIS
Panel D: Employer controls		
Age of the establishment	Age of the establishment	CNPJ Aberto
Panel E: Spatial controls		
GDP per capita of 2000	GDP per capita of the municipality in which the establishment is located	IBGE
Gini index of 2000	Gini index of the GDP per capita of the municipality in which the establishment is located	IBGE
Total population	Population of the municipality in which the establishment is located (in 1000s)	IBGE
Perc. of the GDP from manufacturing	Percentage of the GDP of 2000 from manufacturing of the municipality in which the establishment is located	IBGE
Deposit density	Total deposits divided by the area of the municipality in which the establishment is located (in BRL 1000s)	Estban
Bank density	Number of bank agencies divided by the area of the municipality in which the establishment is located	Estban

Notes: Elaborated by the authors.

Table B3: Descriptive statistics from the RDD estimating sample.

	Mean	Std. dev.	Min.	Max.
Panel A: Outcomes				
SME winner	0.85	0.36	0.00	1.00
Price index	0.09	0.84	-3.71	3.29
Price index (adjusted)	0.14	0.83	-9.41	8.18
Number of participants	8.27	6.42	1.00	76.00
Number of bids	44.23	79.13	1.00	15640.00
Number of bids per part.	5.42	11.36	0.03	1860.25
Panel B: Treatment				
Set-aside	0.49	0.50	0.00	1.00
Panel C: RDD variables				
Bundle value (BRL 1000)	55.26	31.11	20.00	140.00
Below cutoff	0.79	0.41	0.00	1.00
Panel D: Tender-level controls				
Quantity	3928.28	12978.00	1.00	100000.00
Quarter: First	0.20	0.40	0.00	1.00
Quarter: Second	0.28	0.45	0.00	1.00
Quarter: Third	0.22	0.42	0.00	1.00
Quarter: Fourth	0.30	0.46	0.00	1.00
Panel E: Bundle-level controls				
Bundle size (auctions)	7.81	17.00	1.00	96.00
Panel F: Auction-level controls				
Tender size (auctions)	162.30	193.82	2.00	998.00
Tender size (bundles)	113.39	156.23	1.00	820.00
Panel G:				
Capital	0.60	0.49	0.00	1.00
Region: South	0.18	0.38	0.00	1.00
Region: North region	0.17	0.38	0.00	1.00
Region: Center-West	0.16	0.37	0.00	1.00
Region: Southeast	0.27	0.44	0.00	1.00
Region: Northeast	0.22	0.42	0.00	1.00
Legal autonomy	0.35	0.48	0.00	1.00
Federal	0.88	0.33	0.00	1.00
Executive power	0.97	0.17	0.00	1.00
Public function: Armed forces	0.06	0.24	0.00	1.00
Public function: Health	0.12	0.32	0.00	1.00
Public function: Education	0.23	0.42	0.00	1.00
Public function: Public Adm.	0.21	0.41	0.00	1.00
Public function: Other	0.38	0.48	0.00	1.00

Table B4: Pre and Post-Treatment Outcomes by Group

Balance-check statistics from the DiD estimating sample comparing GSs (treatment) and non-GSs (control) establishments before and after the 2014 reform that made SA auctions for MSEs mandatory for tenders below BRL 80,000.

	GSs (treatment)		non-GSs (control)		
Variable	Mean	Std. dev.	Mean	Std. dev.	p-value
Panel A: Pre-treatment outcomes (2011-13)					
Number of employees	10.11	11.66	10.23	12.31	0.15
Average monthly earnings	1105.17	455.74	1098.97	477.21	0.07
Average monthly payroll	8339.59	9790.74	8240.51	9886.79	0.17
Average monthly working hours	43.33	2.58	43.40	2.41	0.00
Panel B: Post-treatment outcomes (2014-20)					
Number of employees	10.08	20.11	9.37	14.47	0.00
Average monthly earnings	1582.46	671.05	1525.43	657.06	0.00
Average monthly payroll	11866.42	15675.69	11125.77	15080.70	0.00
Average monthly working hours	43.02	3.35	43.11	3.10	0.00
Panel C: Treatment (2011-13)					
=1 if estab. is a Government Supplier (GS)	1.00	0.00	0.00	0.00	
Panel D: Employee controls (2011-13)					
Percentage of female employees	32.61	28.16	38.37	34.02	0.00
Percentage of employees with a college degree	7.96	16.97	5.90	15.53	0.00
Average tenure of employees	28.68	24.55	30.50	30.16	0.00
Average age of employees	33.66	6.14	33.51	6.80	0.00
Panel E: Employers controls (2011-13)					
Age of the estab.	6.34	7.75	6.29	8.32	0.40
Panel F: Spatial controls (2011-13)					
GDP per capita of 2000	30.97	16.77	30.69	18.66	0.04
Gini index of 2000	0.59	0.05	0.58	0.05	0.00
Total population (in 1000s)	1796.55	2805.32	1470.68	2616.65	0.00
Perc. of the GDP (of 2000) from manufacturing	18.80	9.57	20.15	11.19	0.00
Deposit density of 2010 (in 1000s)	5015.53	7702.52	4003.04	7211.77	0.00
Bank density of 2010	0.04	0.05	0.03	0.04	0.00

Note: This Table displays balance-check statistics from the DiD estimating sample. Table B2 in Appendix B provides a precise description of each variable in the DiD estimating sample. The second and third columns display the mean and standard deviation for the sub-sample of establishments classified as GSs (treatment group). The third and fourth columns repeat the previous two columns but restrict the sub-sample to establishments classified as non-GSs (control group). In the last column, we plot the p-value associated with the statistical hypothesis $H_0 : \mu_x(GS_e = 1) = \mu_x(GS_e = 1)$, where x is one of our baseline controls described in the first column, and μ_x is the mean of x. We calculate the p-value by regressing each covariate x_e on our treatment indicator GS_e using robust SEs clustered at the establishment level.

Table B5: Description of the variables in the DiD estimating sample (procurement outcomes)

Variable	Description	Source
Panel A: Outcomes		
SME winner	Indicator variable equal to 1 if the auction winner is a Small and Medium Enterprise (SME), and 0 otherwise.	CNET
Log-price	Logarithm of the unit price offered by the awarded firm.	CNET
Panel B: Treatments		
Intensity	Continuous treatment: share of non-SA auctions by agency in the pre-period (2011–2013), when SA auctions for SMEs were voluntary.	CNET
Treatment	Indicator variable for treatment exposure, defined at the auction level.	CNET
Set-aside	Indicator variable equal to 1 if the auction is a set-aside (SA) for SMEs, and 0 otherwise.	CNET
Panel C: Bundle-level control		
Bundle value	Value of the bundle to which the auction belongs. The bundle and auction coincide if the bundle contains a single lot.	CNET
Panel D: Year indicators		
Year: 2011 to Year:2021	Indicator variables for each calendar year.	CNET

Note: Elaborathed by the authors. SME = Small and Medium Enterprise; SA = Set-Aside.

Table B6: Descriptive statistics from the DiD estimating sample with procurement outcomes.

	Mean	Std. dev.	Min.	Max.
Panel A: Outcomes (pre-treatment: 2011-2013)				
Set-aside	0.19	0.39	0.00	1.00
SME winner	0.82	0.39	0.00	1.00
Log. of unit price	2.74	2.12	-4.61	13.95
Panel B: Outcomes (post-treatment: 2014-2020)				
Set-aside	0.58	0.49	0.00	1.00
SME winner	0.90	0.30	0.00	1.00
Log. of unit price	2.86	2.12	-4.61	15.38
Panel C: Treatments (full sample: 2011-2020)				
DiD treatment: continuous	0.81	0.26	0.00	1.00
DiD treatment: discrete	0.57	0.50	0.00	1.00
Panel D: Controls (full sample: 2011-2020)				
Bundle value (BRL 1000)	9.40	15.68	0.00	80.00
Panel E: Year indicators (full sample: 2011-2020)				
Year: 2011	0.13	0.34	0.00	1.00
Year: 2012	0.13	0.34	0.00	1.00
Year: 2013	0.14	0.35	0.00	1.00
Year: 2014	0.12	0.33	0.00	1.00
Year: 2015	0.10	0.30	0.00	1.00
Year: 2016	0.10	0.30	0.00	1.00
Year: 2017	0.09	0.28	0.00	1.00
Year: 2018	0.08	0.28	0.00	1.00
Year: 2019	0.07	0.26	0.00	1.00
Year: 2020	0.03	0.16	0.00	1.00

C Background

C.1 Examples of Procured Items and Agencies

Table C1: Examples of Procured Items

Item Description	Unit of Measurement
Petrol	Liter
Banana	Kilogram
Watermelon	Kilogram
Mineral water (500 ml)	Unit
Protective eyewear	Unit
Nails	Kilogram
USB flash drive (16 GB)	Unit
Ibuprofen (600 mg)	Pill
Rubbing alcohol	Liter
Toilet fixture	Unit
Nasal cannula	Unit
Fluorescent lamp (20W)	Unit
Bleach	Liter
Padlock (35 mm)	Unit
Digital thermometer	Unit

Notes: This table illustrates the heterogeneity of products procured by public agencies in Brazil through electronic auctions. Items range from everyday consumables (e.g., food, cleaning products) to hospital supplies and office equipment. Each item is procured in standardized units to ensure comparability across tenders and agencies. The diversity of goods highlights the broad scope of federal procurement, which spans multiple sectors and administrative levels.

Table C2: Examples of Government Agencies by Function

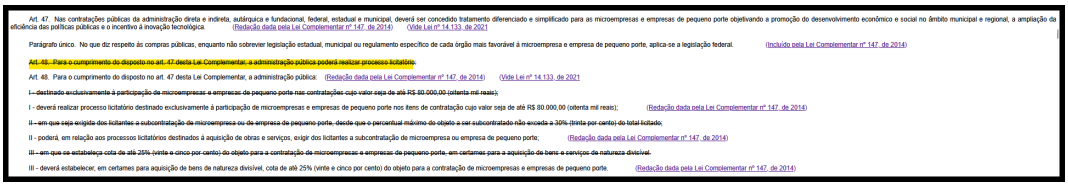
Name of Government Agency	Classification
Universidade Federal do Rio Grande do Sul	Education
Universidade Federal do Pará	Education
Universidade Federal de Pernambuco	Education
Hospital Universitário da UFSC	Hospitals
Hospital Universitário Antônio Pedro (UFF/RJ)	Hospitals
Hospital Universitário Gaffrée e Guinle (UNIRIO)	Hospitals
Grupamento de Apoio de São José dos Campos	Armed Forces
Grupamento de Apoio de Brasília	Armed Forces
14º Grupo de Artilharia de Campanha	Armed Forces
Comissão Nacional de Energia Nuclear	Other
Governo do Estado do Ceará	Other
Departamento de Logística em Saúde	Other

Notes: This table presents examples of federal government agencies (*Unidades Administrativas de Serviços Gerais*, or UASGs) that participate in public procurement. The classification groups agencies according to their main institutional function. Universities and hospitals are among the largest purchasers in education and health, respectively, while military units and other specialized agencies (e.g., energy or logistics departments) represent strategic and operational branches of the federal government. These entities are responsible for implementing electronic auctions and managing procurement processes at the decentralized level.

C.2 Regulatory Framework

The identification of both empirical strategies in this paper—the regression discontinuity design (RDD) and the DiD—relies on an exogenous policy change regulating public auctions for small and medium enterprises (SMEs). The change was introduced through two successive pieces of legislation that modified the rules governing *set-aside* (*pregões exclusivos*) auctions.

Initial Framework (Supplementary Law 123/2006). The original regulation, established by Articles 47 and 48 of Supplementary Law 123/2006, allowed—but did not mandate—public agencies to create *set-aside* lots for SMEs. These auctions could be organized for purchases below BRL 80,000 or limited to a maximum of 25% of the agency's annual procurement volume. This framework created substantial heterogeneity in adoption across agencies, as the use of set-asides was voluntary. The relevant legal paragraphs are reproduced in Figure C1.



Art. 47. Nas contratações públicas da administração direta e indireta, autárquica e fundacional, federal, estadual e municipal, deverá ser concedido tratamento diferenciado e simplificado para as microempresas e empresas de pequeno porte objetivando a promoção do desenvolvimento econômico e social no âmbito municipal e regional, a ampliação da eficiência das políticas públicas e o incentivo à inovação tecnológica. (Redação dada pela Lei Complementar nº 147, de 2014) (Vide Lei nº 14.133, de 2021

Parágrafo único. No que diz respeito às compras públicas, enquanto não sobrevier legislação estadual, municipal ou regulamento específico de cada órgão mais favorável à microempresa e empresa de pequeno porte, aplica-se a legislação federal. (Incluído pela Lei Complementar nº 147, de 2014)

Art. 48. Para o cumprimento do disposto no art. 47 desta Lei Complementar, a administração pública poderá realizar processo licitatório.

Art. 48. Para o cumprimento do disposto no art. 47 desta Lei Complementar, a administração pública: (Redação dada pela Lei Complementar nº 147, de 2014) (Vide Lei nº 14.133, de 2021

I - destinado exclusivamente à participação de microempresas e empresas de pequeno porte nas contratações cujo valor seja de até R$ 80.000,00 (oitenta mil reais);

I - deverá realizar processo licitatório destinado exclusivamente à participação de microempresas e empresas de pequeno porte nos itens de contratação cujo valor seja de até R$ 80.000,00 (oitenta mil reais); (Redação dada pela Lei Complementar nº 147, de 2014)

II - em que seja exigida dos licitantes a subcontratação de microempresa ou de empresa de pequeno porte, desde que o percentual máximo do objeto a ser subcontratado não exceda a 30% (trinta por cento) do total licitado;

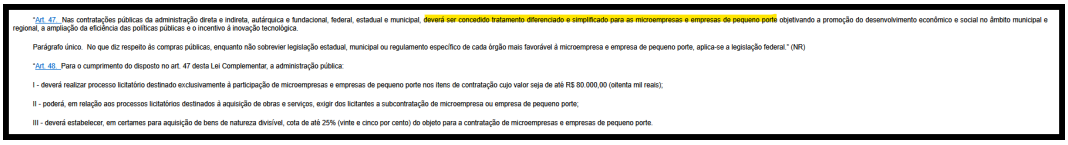
II - poderá, em relação aos processos licitatórios destinados à aquisição de obras e serviços, exigir dos licitantes a subcontratação de microempresa ou empresa de pequeno porte; (Redação dada pela Lei Complementar nº 147, de 2014)

III - em que se estabeleça cota de até 25% (vinte e cinco por cento) do objeto para a contratação de microempresas e empresas de pequeno porte, em certames para a aquisição de bens e serviços de natureza divisível.

III - deverá estabelecer, em certames para aquisição de bens de natureza divisível, cota de até 25% (vinte e cinco por cento) do objeto para a contratação de microempresas e empresas de pequeno porte. (Redação dada pela Lei Complementar nº 147, de 2014)

Figure C1: Supplementary Law 123/2006 – Original Regulation. Articles 47 and 48 established that auctions below BRL 80,000 *could* be reserved for SMEs, up to 25% of total procurement. This provision created voluntary heterogeneity in the use of SME set-asides across public agencies.

Policy Reform (Supplementary Law 147/2014). In 2014, Supplementary Law 147 introduced a major reform by making *set-aside* auctions **mandatory** for all public purchases below BRL 80,000. It also eliminated the 25% annual limit, effectively expanding the scope of the policy to all eligible procurements nationwide. This legislative change represents a clear, discrete shift in the institutional environment, providing the exogenous variation exploited in our empirical designs. The updated legal paragraphs are reproduced in Figure C2.



"Art. 47. Nas contratações públicas da administração direta e indireta, autárquica e fundacional, federal, estadual e municipal, deverá ser concedido tratamento diferenciado e simplificado para as microempresas e empresas de pequeno porte objetivando a promoção do desenvolvimento econômico e social no âmbito municipal e regional, a ampliação da eficiência das políticas públicas e o incentivo à inovação tecnológica.

Parágrafo único. No que diz respeito às compras públicas, enquanto não sobrevier legislação estadual, municipal ou regulamento específico de cada órgão mais favorável à microempresa e empresa de pequeno porte, aplica-se a legislação federal." (NR)

"Art. 48. Para o cumprimento do disposto no art. 47 desta Lei Complementar, a administração pública:

I - deverá realizar processo licitatório destinado exclusivamente à participação de microempresas e empresas de pequeno porte nos itens de contratação cujo valor seja de até R$ 80.000,00 (oitenta mil reais);

II - poderá, em relação aos processos licitatórios destinados à aquisição de obras e serviços, exigir dos licitantes a subcontratação de microempresa ou empresa de pequeno porte;

III - deverá estabelecer, em certames para aquisição de bens de natureza divisível, cota de até 25% (vinte e cinco por cento) do objeto para a contratação de microempresas e empresas de pequeno porte.

Figure C2: Supplementary Law 147/2014 – Revised Regulation. The reform mandated that all public purchases below BRL 80,000 be conducted as SME *set-aside* auctions, removing the 25% cap and making adoption compulsory for all agencies.

Role in Empirical Strategies. This policy shift provides two complementary sources of identification. First, the *RDD* exploits the sharp eligibility threshold at BRL 80,000, comparing auctions just below and just above the cutoff to estimate local treatment effects of the mandatory rule. Second, the *DiD* leverages temporal and cross-agency variation before and after the 2014 reform—capturing how agencies with different pre-reform adoption intensities responded to the policy shock. Together, these two designs allow us to evaluate both the local and aggregate effects of the reform on procurement outcomes and firm performance.