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2023

Essays on the Labor Market Effects of Public Policies

Szerman, Christiane

Princeton, NJ : Princeton University

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ESSAYS ON THE LABOR MARKET EFFECTS OF PUBLIC POLICIES

CHRISTIANE SZERMAN

A DISSERTATION
PRESENTED TO THE FACULTY
OF PRINCETON UNIVERSITY
IN CANDIDACY FOR THE DEGREE
OF DOCTOR OF PHILOSOPHY

RECOMMENDED FOR ACCEPTANCE BY
THE DEPARTMENT OF
ECONOMICS
ADVISER: ALEXANDRE MAS

MAY 2023

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Abstract

This dissertation investigates the labor market implications of policies tackling inequality.

Chapter 1 investigates the implications of enforcing modest disability hiring quotas for workers and firms. Analysis exploiting variation in enforcement across firms reveals four findings. First, firms hire more workers with disabilities into low-paying jobs. Second, consistent with statistical discrimination, workers with disabilities experience reduced wage growth. Third, the quota does not come at a cost to workers without disabilities in terms of wages or employment, or to firms in terms of closure. Fourth, I estimate that the marginal worker with disabilities hired under the quota has a marginal revenue product close to their wage. Through the lens of a model of enforcement of hiring quotas with imperfect compliance, I show that the policy generates aggregate welfare gains. My findings demonstrate that, in labor markets under imperfect competition, mandating modest increases in employment for the disadvantaged can promote redistribution and improve welfare.

Chapter 2 studies an anti-corruption policy — corporate debarment, or blacklisting — to understand how disclosing illicit corporate practices and the sanctions for these practices affect firm and worker outcomes. Exploiting a policy change in Brazil that imposed stricter penalties for corrupt firms, I find that debarment is associated with a sizable decline in employment and an increase in the probability of exiting the formal sector. I also document that workers' annual earnings fall after debarment. The impacts are driven by lost revenues from government contracts. The results shed light on the costs to workers in weighing the consequences of corruption crackdown.

Chapter 3, joint with Ricardo Dahis, asks how administrative redistricting through the creation of smaller government units shapes local development. Exploiting a reform that led to sharp variations in the number of government units in Brazil, we find that voluntary redistricting spurs public services and economic activity in new local governments without hurting the rest of the country. The gains are concentrated in the public sector and in remote

backwards areas. We provide evidence that these persistent gains are predominantly driven by the decentralization of decision-making power, rather than the increased fiscal revenues.

Acknowledgements

I could not have completed this dissertation without the help and support of many people.

It is difficult to sufficiently express my gratitude for my committee at Princeton. I am deeply indebted to my advisor, Alex Mas, for more reasons than I can fit into this page. His frequent feedback, seemingly endless support, and remarkable creativity have been pivotal to my journey. From weekly virtual meetings during the pandemic to job market adventures, Alex has challenged me, encouraged me, kept me grounded, and articulated my thoughts and ideas. I have learned so much from his guidance.

Since day one of graduate school, Thomas Fujiwara has been incredibly supportive and generous with his time. He has always been available to listen to my rambling thoughts and to provide solutions for my setbacks. Ellora Derenoncourt was the missing piece of my stellar committee. I have benefited so much from her expertise, insightful advice, and detailed notes. Her steadfast encouragement during the last miles of the marathon has given me the necessary confidence. I feel proud to have had Alex, Ellora, and Thomas as my role models, and I hope to emulate them once I have my own students.

I thank participants in the Graduate Labor Workshops, the CHW/RPDE Brown Bags, and the Prize Fellowship in the Social Sciences for suggestions that have improved my work at all stages. My research has especially benefited from comments of many members of the Princeton faculty: Leah Boustan, Janet Currie, Hank Farber, Seema Jayachandran, Ilyana Kuziemko, David Lee, Christopher Neilson, Steve Redding, Mica Sviatschi, and Owen Zidar. I particularly thank Hank for his generous mentorship during the last four years. Gianluca Violante and Laura Hedden have also provided valuable support during the job market.

The Industrial Relations Section at Princeton has been my intellectual home for the last four years. The faculty, staff, students, postdocs, and visitors make it a very special place. I hope many more students share this feeling in the next years.

My gratitude and appreciation also go to the staff of the Economics Department at Princeton. My life would have been much more difficult without their help. I am particularly grateful to Valerie Ching, Gene Coppola, Indy Gaffney, Laura Hedden, Lori Mitrano, Jeannie Moore, Laura Sciarrotta, and Patricia Tracey. I will never forget Laura Hedden's help with my personal matters; Valerie's effort and empathy to find solutions handling mask mandates; and Patti's kindness during the job market.

I further thank my co-authors and friends, Lukas Althoff, Ricardo Dahis, Ana Luiza Dutra, Cecilia Machado, and Valdemar Pinho Neto, for reminding me that research is fun.

I am grateful for the companionship and mutual commiseration of friends and classmates who have made Princeton an outstanding place, including: Patrick Agte, Dorisz Albrecht, Lukas Althoff, Rachel Anderson, Emily Battaglia, Christine Blandhol, Francisco Cabezón, Jiwon Choi, Mateus Dias, Ana Luiza Dutra, Stephanie Hao, Benny Kleinman, Kwok-Hao Lee, Carl Lieberman, Don Noh, Rafael Parente, Carolina Piazza, Pooja Ramamurthi, Ben Scuderi, João Thereze, Carolyn Tsao, and Peng Wang. I especially thank Dorisz and Stephanie for sharing with me the pain of solving first-year problem sets, engaging in long debates about useless topics, and putting up with me; Emily for becoming my academic partner during the second half of graduate school, patiently listening to my endless rants and supporting me during the job market; and Diza and Rafa for making me feel home since my visit day.

Many former mentors and teachers at school, PUC-Rio, Climate Policy Initiative, and EPGE-FGV have provided guidance before graduate school. I am especially indebted to Rogério Werneck, Juliano Assunção, Francisco Costa, and Cecilia Machado. I owe a lot to Cecilia and Francisco for their unwavering support that has led me to Princeton. Francisco has brought me back to the world of research. Cecilia has sparked and nurtured my fascination with research. She has also convinced me to apply to Princeton.

My friends from Brazil, too many to list, have provided a constant source of laughs and fun. Despite having no idea of what my dissertation is about, they have always cheered me

up, and reminded me of who I am and where home is. I also thank Aurora for the joy of being a godmother.

My family has been a constant source of support. Otto revealed his unmatched enthusiasm and companionship every single time I came home. My brother always found his own way of supporting me. Finally, I owe everything to my parents. Even though they were constantly asking "why don't you stop studying", "how are you working if you don't teach" or "why don't you have a job yet", they never failed to show their trust in my choices and to make sure I could always go home when things fell apart. They stood by me with love, care, and encouragement, always.

I thoroughly enjoyed graduate school. I conclude this chapter feeling immense gratitude for my journey at Princeton and for the opportunity of continuing to pursue what I love. I truly hope to pay forward all I have received in the years to come.

In memory of my dearest friends, Mathias Iunes Levy and Tiago Flórido, whose strength, warmth, and brilliance inspire me, always.

In memory of my beloved grandmother, Sura Baila Szerman, whose immense love, kindness, and resilience guide me, still.

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Chapter 1

The Labor Market Effects of Disability Hiring Quotas[1]

1.1 Introduction

More than one billion people, around 16 percent of the population, live with some form of disability worldwide, constituting one of the fastest-growing underrepresented groups (WHO (2022)). Yet, in most countries, their unemployment rates are among the highest, at least

[1]I am deeply grateful to my advisors Alexandre Mas, Thomas Fujiwara and Ellora Derenoncourt for their invaluable guidance and constant support throughout this project. I thank Patrick Agte, Dorisz Albrecht, Sofia Amaral, David Autor, Emily Battaglia, Leah Boustan, Jiwon Choi, Janet Currie, Mateus Dias, Hank Farber, Marcelo Ferreira, Felipe Golfín, Stephanie Hao, Ilyana Kuziemko, David S. Lee, Sophie Mitra, Rafael Parente, Steve Redding, Karl Schulze, Carolyn Tsao, Ana Trindade, Owen Zidar, and participants of Prize Fellowship, CHW and Labor lunches, Development Tea, and Labor Seminar at Princeton University for valuable discussions and suggestions. I have also benefited from comments from seminar participants at Hebrew University of Jerusalem, Tel Aviv University, Insper, Tilburg University, Nova School of Business and Economics, Boston University (Questrom), Federal Reserve Bank of New York, University of Chicago (Harris), Cornell University (SC Johnson), Cornell University (Economics), University of Rochester, Queen Mary University of London, London School of Economics, Carnegie Mellon University (Heinz), Cornerstone, Getulio Vargas Foundation (EPGE), Getulio Vargas Foundation (EESP), University of California (Merced), and European Bank for Reconstruction and Development. I thank Valdemar Pinho Neto and Vladimir Ponczek for kindly sharing auxiliary data. I am deeply indebted to Marcia Jovita and Marcus Novello for spending many hours diligently answering to my questions. I am grateful to many anonymous people for helpful conversations. I thank Oppen Social for the support with the survey. I gratefully acknowledge financial support from the Industrial Relations Section and the Economics Department at Princeton University, the Prize Fellowship in Social Sciences at Princeton University, and the NBER Retirement and Disability Research Center. This chapter served as my job market paper in Szerman (2022). All errors are my own.

twice the rate of the general population (ILO (2007), Mizunoya and Mitra (2013), UN (2018)). Due to firms' unwillingness to hire people with disabilities, even those who can work, policies to boost the demand for these workers are as pivotal as social insurance programs.[2] Implemented in more than 100 countries (see Figure 2.1), disability hiring quotas—whose origin dates back to the World War I when thousands of people were left disabled—are some of the most widespread policy levers available to reduce the unemployment rates and the fiscal burden of social insurance (ILO (2019)).

This paper assesses the implications of enforcing disability hiring quotas for firms and workers. Economists and policy makers have long discussed the merits of affirmative action hiring quotas for disadvantaged groups. In competitive labor markets, the conventional wisdom is that quotas are expected to increase earnings of the disadvantaged group at a potential cost of displacing the non-disadvantaged and reducing firm profits (Welch (1976), Leonard (1984), Griffin (1992), Coate and Loury (1993)). Under these assumptions, the gains for the disadvantaged may be offset by larger welfare losses to other agents. Recent literature, however, has documented extensive evidence of imperfect competition and frictions in the labor market (Card et al. (2013), Lamadon et al. (2022)). In such labor markets, firms may be inefficiently small in equilibrium, and hiring quotas may lead to efficient increases in employment and redistribute economic rents to the disadvantaged (Holzer and Neumark (2000)). This is also true if discrimination is the source of employment gaps.

Despite the extensive coverage of hiring quotas, comprehensive assessments of their labor market impacts are exceedingly scarce. There are at least two main reasons why investigating their consequences empirically, including for other disadvantaged groups, has been difficult. One reason is the limited availability of linked data both containing information on firms who are (or not) under quota regulations and identifying individuals from targeted and

[2]Non-discriminatory factors, such as work disincentives from social insurance, transportation barriers, lack of awareness of disability issues, and lower education levels, contribute to this scenario. Nonetheless, growing evidence from experimental and observational studies show that employers are less likely to hire people with disabilities (Baldwin and Johnson (1994), Ameri et al. (2018)).

non-targeted groups. Second, even when such data are available, the contentious nature of affirmative action and the lack of regulatory incentives in the private sector have made most hiring quota regulations toothless. This has also led governments to introduce reforms for stricter enforcement (ILO (2019)).

This paper overcomes these challenges by exploiting several features of the Brazilian setting. First, the country has a disability quota policy and an enforcement tool. The regulation establishes that firms with at least 100 employees must fill a minimum of 2 percent of their positions with people with disabilities or individuals enrolled in vocational rehabilitation programs. Inspections are the main enforcement tool. Once inspected and found to be delinquent, firms have a grace period to meet the mandated share, or face a heavy fine. Features of the regulation and enforcement structures, along with the high frequency of inspections, create useful quasi-experimental variations for identification. Second, the availability of data at the individual- and firm-levels with detailed information on people with disabilities, including the type of disability, education, hours of work, and occupation, provides a rare opportunity to identify heterogeneous effects across and within groups of workers and to overcome the perennial dearth of data that have long challenged the literature. Third, the large size of the country and its high disability prevalence bring extra statistical advantages when workers with disabilities are vastly underrepresented in the labor market. Fourth, the limited coverage of social insurance for people with disabilities implies that access to disability insurance programs, one often-hypothesized cause of low employment rates, is an unlikely key driver of disability inequality.

In the first part of this paper, I quantify the aggregate impacts of enforcing hiring quotas across local labor markets. I use Census data covering the entire population and exploit the introduction of a new reform in 2000, which established both the quota regulation and inspections as the main tool to enforce compliance. Along with the timing variation from the reform, I use geographic variation in the intensity of potential demand for workers with

disabilities and enforcement of labor regulations. I exploit the interaction between pre-reform firm size distribution and enforcement capacity as the source of cross-sectional variation in exposure to the reform. Pre-reform size distribution predicts differential potential demand for people with disabilities induced by the regulation. Enforcement capacity is proxied by the distance to the nearest labor office (Almeida and Carneiro (2012)).

Consistent with the goal of expanding job opportunities, the market-level results indicate that people with disabilities in labor markets more exposed to the reform experience larger gains in employment and earnings. I document that the people with disabilities from more exposed local labor markets, like the cities of São Paulo or Rio de Janeiro, are 1.1 percentage points, or 10 percent, more likely to become employed in the formal sector ten years after the reform. Along with higher employment, their average earnings increase by around 9.5 percent. I find no evidence that people without disabilities and workers from the informal sector are affected. The aggregate results, albeit informative, are limited in showing how firms adjust to the mandated employment. Understanding the reallocation of firm activities in the cross section of firms is key to quantifying the policy incidence in light of extensive evidence of frictions in the labor market.

The second part of my paper closely examines adjustments within firms. My context permits an event study difference-in-differences design because, once inspected, firms may be assessed for compliance with labor regulations, including but not limited to disability quota regulation. I compare how the outcome variables evolve for inspected firms with pre-inspection employment either above or below the hiring cutoff of 100 workers around the time of inspection. The estimation sample consists of the universe of all firms that were inspected, mitigating concerns related to selection into inspections. The key variation is that only firms with at least 100 employees may be under the disability hiring quota. I show that firms both above and below the mandatory cutoff exhibit similar levels and trends in outcomes prior to the inspection.

Using the universe of inspected firms linked to the matched employer-employee data, I document an increase in employment among people with disabilities following inspections. On the extensive margin, firms under the quota requirement, on average, are 7.8 percentage points more likely to hire an employee with disabilities. On the intensive margin, these firms experience a 41.7 percent increase in the total number of employees with disabilities. The vast majority of the new hires are concentrated in low-paying occupations and have milder disabilities. They also predominantly come from unemployment, instead of being poached from other firms or disability reclassification, indicating potential fiscal savings. At the same time, there is no impact on employment of people without disabilities. I also do not find that firms are more likely to exit the formal sector or experience a decrease in average wages, both imperfect proxies for profits.

Turning to workers' outcomes, I show that workers with disabilities, rather than those without disabilities, are affected. Surprisingly, I find evidence of reduced wage growth of around 6 percent for disabled workers, driven by incumbent workers, at the firms under disability hiring quotas. I find no discernible impact on wage growth for workers without disabilities or new hires. I find little support that workers without disabilities with similar characteristics as workers with disabilities experience wage growth slowdowns. I also document that workers with disabilities experience a decline in promotion rates. My empirical findings point to relevant distributional consequences: although the policy results in an increased representation of workers from the target group, it may also exacerbate workplace inequality in pay between groups.

What can explain the lower wage growth for incumbents with disabilities? I shed light on mechanisms by complementing the administrative data with an original survey conducted with human resources executives and personnel. I argue that several pieces of evidence are consistent with a dynamic statistical discrimination model, in which firms observe signals to infer workers' skills for task assignments and promotions, and are less able to interpret

signals of workers from a minority group. In line with reduced hiring standards, affirmative action hiring quotas reduce the average skill of the disabled group, making their signals more difficult to interpret. Consistent with discrimination being more relevant for workers with weaker signals of their skills, I show that workers without college education and in jobs with less interpersonal relationships with supervisors and peers drive the wage growth slowdowns. I also document that the data reject irreversible fixed costs, compensating differentials, and retaliation due to whistleblowing as alternative explanations.

In the third part of the paper, I evaluate the implications of my findings for aggregate welfare. I develop a simple model of an enforcement of marginal disability hiring quotas under imperfect compliance to provide an assessment of the welfare effects. My model characterizes the changes in profits, surplus of workers with disabilities, and fiscal revenue following an inspection. The effect on profits depends on the wedge between marginal revenue products of labor and wages. To overcome the lack of estimates of marginal revenue product of workers with disabilities in the literature, I propose a discrete choice framework on the decision to comply with the hiring quota following inspections. I find that the data reject that marginal product of new hires with disabilities is lower than their wages. Using a sufficient statistics approach, I also document that increased employment results in gains in surplus of workers with disabilities and fiscal revenue, yielding aggregate welfare gains. Conceptually, these results are consistent with a labor market under imperfect competition characterized by discrimination at the hiring level. This is in line with my experimental survey evidence that firms are significantly less likely to express interest in hiring a worker with disabilities. In such a labor market, a modest rise in mandated employment can achieve redistribution without harming overall efficiency.

This paper contributes to several lines of research. It speaks to a large literature, theoretical and empirical, studying the consequences of affirmative action quota regulations in labor markets on human capital investment and firm output (Welch (1976), Lundberg

(1991), Coate and Loury (1993), Fang and Moro (2011)). Previous studies have focused on the redistribution effects, indicating positive or neutral impacts on the employment of minorities groups, including people with disabilities (Miller and Segal (2012), Lalive et al. (2013), Peck (2017), Miller (2017), Mori and Sakamoto (2018), Prakash (2020)). Other empirical works analyze whether AA policies in educational settings impact economic efficiency through changes in final output, such as educational attainment and income (Bagde et al. (2016), Bleemer (2022), Schaede and Mankki (2022)).[3] My contribution is threefold. First, I contribute to this literature by leveraging unusually rich administrative data and new quasi-experimental research designs to provide novel evidence of the market-level impacts across local labor markets. Second, relative to past literature, I provide richer estimates of the causal impact of hiring quotas on workers' outcomes, including wages, turnover, part-time employment, and promotion. By showing evidence of heterogeneous impacts across worker groups, this work also adds new evidence on the distributional debate about the incidence of this policy. Third, I offer a new comprehensive assessment of the implications for firms and workers, which allows me to speak to the consequences for redistribution and efficiency.

This work also relates to a literature on the consequences of policies targeting people with disabilities. The core of this literature has focused on the supply side, such as the impacts of social insurance (e.g., disability insurance) and anti-discrimination regulations (e.g., Americans with Disabilities Act) on labor supply and consumption (DeLeire (1997), Acemoglu and Angrist (2001), Autor and Duggan (2003), Maestas et al. (2013), Kostøl and Mogstad (2014), Deshpande (2016), Autor et al. (2019), Aizawa et al. (2020)).[4] This

[3] In the context of leadership positions, a handful of papers have found that mandated representation of women on boards is associated with negative to neutral impacts on firm performance (Ahern and Dittmar (2012), Matsa and Miller (2013), Bertrand et al. (2019), Ferrari et al. (2021), Eckbo et al. (2022)).

[4] A common policy among OECD countries is anti-discrimination legislation. The Americans with Disabilities Act, enacted in 1991 in the US, is the most prominent example of an anti-discrimination law aiming to provide better jobs opportunities, prohibit firms from discriminating based on disability, and mandate reasonable accommodations to workers. Several works indicate that the introduction of the ADA increased the costs of hiring workers with disabilities, creating incentives for firms to avoid hiring them (DeLeire (1997), Acemoglu and Angrist (2001)). There is a large literature showing that anti-discrimination legislation does not increase employment in other contexts (Bell and Heitmueller (2009), Lalive et al. (2013)).

paper differs from the prior literature in its focus on a much less studied angle: the demand side (Lalive et al. (2013), Mori and Sakamoto (2018), Aizawa et al. (2020), de Araújo et al. (2021), Palmer and Williams (Forthcoming)). I bring new evidence of the interaction of enforcement and hiring quotas as fundamental policy levers to boost the demand for workers with disabilities.[5] In addition, the lack of evidence that productivity of workers with disabilities falls below their wages suggests scope for demand-side policies incentivizing employers to hire from this group.

At a broader level, this paper builds on a large literature assessing the incidence of regulations. In perfectly competitive labor markets, redistributive regulations cannot be justified on efficiency grounds. In view of a growing number of papers that have documented the existence of employer discrimination (Glover et al. (2017), Benson et al. (2022)) and imperfectly competitive labor markets, in which firms have some power to determine wages and set inefficiently low employment levels (Card et al. (2013), Lamadon et al. (2022)), recent evidence suggests that regulations have the potential to improve efficiency and achieve redistribution (Manning (2011)). Examples of price regulations in labor markets examined in the literature include payroll taxes (Saez et al. (2019)), minimum wage (Harasztosi and Lindner (2019)), wage floor (Card and Cardoso (2021)), and hiring credits (Cahuc et al. (2019)). Wage subsidies are perhaps the most prominent example of regulations with the

[5]In the Brazilian context, Costilla et al. (2002) and de Araújo et al. (2021) show that hiring quotas alone do not increase employment for people with disabilities. Several papers have also used variations in labor inspections to capture change in enforcement of labor regulations (Almeida and Carneiro (2012), Soundararajan (2019), Haanwinckel and Soares (2021), Ponczek and Ulyssea (2021)). On slides available online, de Souza (2020) shows event-study estimates of the effects of labor inspections. He finds that inspections are associated with an increase in employment of people with disabilities. Unlike my study, his results indicate declines in wages and employment of people without disabilities. Differences in identification strategies could explain contrasting findings. My event study difference-in-differences design compares firm with pre-inspection employment slightly above or below the cutoff of 100 workers, before and after the inspection. Only firms with at least 100 employees by the time of the inspection may be assessed for compliance with disability hiring quotas. This allows me to distinguish the effects of enforcing hiring quotas from the effects of enforcing compliance with other dimensions of the labor regulation. Since inspections also target compliance with various dimensions of the labor regulation, including formal registration, minimum wage, and mandated benefits (Almeida and Carneiro (2012)), the identification strategy from de Souza (2020) does not account for this distinction and, therefore, relies on a different counterfactual.

goal of creating job opportunities for the disadvantaged. Most of the evidence, however, indicates that wage subsidies do not increase employment for disadvantaged groups (Katz (1996), Bartik (2001), Huttunen et al. (2013)).[6] This paper focuses on a particular form of quantity regulation that mandates employment for the disadvantaged. My findings indicate that, in labor markets with frictions, enforcing modest quotas can be an efficient pathway for countering the low employment of people with disabilities.

The remainder of this paper is structured as follows. Sections 1.2 and 1.3 describe the institutional context and data. Section 1.4 presents the aggregate impacts of enforcing the quota policy. Section 1.5 analyzes how firms make adjustments and potential mechanisms underlying the earnings effects. In Section 1.6, I discuss implications for welfare. Section 3.8 concludes.

1.2 Institutional Context

1.2.1 Disability Gaps in the Labor Market

In most countries, the employment gap between people with and without disabilities is substantial, leading to high welfare dependency and poverty rates. The unemployment rates among working-age people with disabilities are at least twice the general level (ILO (2007)). Non-discriminatory factors, such as work disincentives from social insurance, transportation barriers, lack of awareness of disability issues, and lower education levels, contribute to this scenario. Employers' willingness to take on workers with disabilities still constitutes a major challenge. Observational studies show that firms are less likely to hire people with disabilities (Baldwin and Johnson (1994)). Randomized audit studies point to lower employer callback rates for applicants with disabilities (Ameri et al. (2018), L'Horty et al. (2022)). In Brazil, my

[6]Despite the limited evidence of effectiveness of wage subsidies for the disadvantaged, a handful of papers have shown that wage subsidies can stabilize labor demand in recessions (Neumark and Grijalva (2017), Schoefer (2021)).

survey results demonstrate that firms are 21 percent less likely to express interest in hiring a candidate with disabilities relative to a similar candidate without disabilities. Workplace accommodation and productivity concerns do not entirely explain the employment gaps.

Like many countries, such as Germany, Hungary, Poland, Sweden, and Turkey, Brazil adopts a medical approach to disability. Disability is defined as an impairment of a physical, mental, intellectual or sensory nature, which, in interaction with other barriers, may hinder the full and effective participation of people with disabilities in society on equal terms relative to others. According to the 2010 Census, 22.4 percent of working-age Brazilian aged between 25 and 54 report living with some form of disability, while 6.7 percent disclose having a severe disability.[7] Even with the challenges associated with defining disability and the lack of standardized disability statistics on a global scale, these numbers are comparable to other countries, such as New Zealand, the US, and the UK (Mitra and Yap (2021)).

Despite their high prevalence, people with disabilities are still vastly underrepresented in the labor market. Records from the 2010 Census data reveal striking disability gaps among the working-age population. Table 1.A.1, Appendix A, shows that nearly 79.4 percent of non-disabled aged between 25 and 54 are economically active. On the other hand, people with some (severe) disabilities are 2.6 (18.2) percentage points less likely to be economically active. Having some (severe) disability is also associated with higher unemployment rates by 2.2 (5.7) percentage points among economically active individuals. Conditional on working, people with disabilities earn about 9.9–19.5 percent less than those without disabilities. These remarkable gaps, which are not explained by differences in location, educational level, potential experience, occupation, or economic sector, illustrate substantial labor market differences even for people with disabilities who can work.

[7]The Census asks whether the respondent lives with severe, some or no difficulties in each of the following activities: seeing, hearing, and walking and climbing stairs. In addition, the Census also collects information on whether the individual has a permanent intellectual disability.

An unconditional cash transfer (*Benefício de Prestação Continuada*) equivalent to a monthly minimum wage (about USD 235) is available to very poor people who have severe disabilities and do not meet household per capita income above one quarter of the minimum wage. The transfer does not require previous contributions to social security. The coverage is quite limited: only the very poor with severe disabilities that hinder independent living and work capacity are eligible for the transfer. In 2010, less than 4 percent of people with disabilities received this transfer, suggesting that work is their main source of income.

1.2.2 2000 Reform

Heeding global efforts to reduce disability inequality, the federal government enacted the Anti-Discrimination Act (National Policy for People with Disabilities or Decree 3,298), which came into effect in 2000 (henceforth 2000 reform). Relevant to the paper, the Act consisted of three main features. First, it provided a legal definition for the term "disability" and a preliminary list of medical conditions required to qualify for disabled status. Examples of disabilities include mobility and physical impairments (e.g., paraplegia, monoplegia, amputation or absence of limb, limbs with congenital or acquired deformity, among others), moderate to profound hearing loss, moderate visual impairment to blindness, and selected mental disorders. Appendix A contains a full list of disabilities recognized by the Act.

Second, the Act regulated the affirmative action quota (henceforth AA quota) by mandating firms in the private sector with at least 100 employees to fill a minimum of 2 percent of positions with people with disabilities or individuals enrolled in a vocational rehabilitation program from the Social Security. The reserved share increases in firm size: firms with 201 to 500 (501 to 1,000) employees must meet a quota of 3 percent (4 percent), while those with more than 1,000 employees have a quota of 5 percent.[8]

[8]The AA quota regulation was first launched in 1991 with the Law 8,213. However, the federal government only formally regulated the AA quota and its functioning after the Anti-Discrimination Act.

The third key element is the utilization of labor inspections as the main enforcement tool to promote compliance with the AA quota. Due to regulatory intricacies, oversight and monitoring of the AA quota through labor inspections became possible after 2003, when the Ministry of Labor started to collect information on workers with disabilities in the formal sector and established administrative fines and penalties for delinquency (Thomasi et al. (2018)).[9] In short, the 2000 reform—which created instruments to define disabilities, mandate reserved jobs through the AA quota regulation, and boost its compliance—represented the first legal framework aimed at increasing employment for people with disabilities.

1.2.3 Enforcement of Disability Hiring Quotas

Brazil has a rigid legal structure with numerous mandated benefits to workers (e.g., minimum wage, unemployment insurance, and severance pay), high dismissal costs, and burdensome tax requirements. Imperfect compliance with labor regulations is quite common. In many cases, firms choose to hire workers without fully complying with several regulations—such as payments to social security and hiring people with disabilities—or even choose to hire informal workers.

The Ministry of Labor is responsible for verifying compliance with labor regulations and uses labor inspections as the main enforcement tool. The enforcement capacity has a decentralized structure: each state has its own state labor office (*delegacia*) located in the state capital, and each state may also have additional local labor offices (*subdelegacias*) in other municipalities, depending on the state's size and economic importance.[10] Inspections can be triggered by random firm audits or anonymous reports from numerous sources, such as

[9]The first rules establishing the formal inspection procedures for the AA quota regulation (Normative Instruction SIT 20) and the administrative fines and penalties for non-compliance (Normative Instruction SIT 36) arose in 2001 and 2003, respectively.

[10]Brazil has 27 states, implying that there are 27 state labor offices (*delegacia*). States with low population density, like Acre, Amapá, and Tocantins, have only one labor office (*delegacias*). More dynamic states, like São Paulo and Minas Gerais, have at least 20 local labor offices (*subdelegacias*) besides their own state labor office (*delegacia*).

workers, labor unions, or the prosecutor's office. In practice, most inspections are triggered by anonymous reports since most labor offices are understaffed (Almeida and Carneiro (2012)).

The technology of enforcement is straightforward. Inspectors are assigned to a specific local labor office and must travel by car to inspect firms. This generates substantial spatial variation, since smaller distances to labor office imply a higher probability of inspection and, as a result, enforcement capacity (Ponczek and Ulyssea (2021)). Once inspected, firms may be assessed for compliance with labor regulations. The key feature, fundamental to my research design, is that *only* firms with at least 100 full-time employees may be required to fill 2 percent of positions with workers with disabilities.

Firms that fail to meet the AA quota may be notified and granted a grace period to comply with the regulation. In case of continued non-compliance, a heavy fine, which typically ranges from 2.5 to 250 monthly minimum wages per missing worker, may be imposed unless the firm shows concrete evidence of effort and failure in hiring workers with disabilities.[11] To qualify for the job reserved to the AA quota, the worker needs to present a medical report containing the type of disability and its code following the International Classification of Diseases (ICD). To minimize moral hazard or frauds in the hiring process, the worker must provide consent in disclosing this information. The penalties for providing fraudulent information can be quite severe.[12]

[11]The final amount of fines is a function of the number of workers with disabilities or rehabilitated workers that firms fail to hire in compliance with the AA quota regulation. It may also depend on numerous factors, such as firm size, recidivism, economic sector, and geographic location. In some cases, firms may be exempted from the hiring quota if they present proofs of attempts to comply with AA quota regulation (e.g., frequent job postings targeting people with disabilities, lack of qualified candidates, etc.). In these cases, the firms may be granted an extended grace period and asked to sign a statement confirming commitment to comply with the AA quota regulation. In addition, several firms (e.g., firms from civil construction and oil and gas industries) can obtain exemption from AA quota via lawsuits alleging that it not possible to guarantee safe work conditions to employees with disabilities.

[12]Informal conversations with inspectors indicate that frauds do not represent a major concern in the Brazilian context because potential financial penalties and reputational damages for fraudulent practices are much more severe than the penalties for non-compliance.

1.3 Data

This paper uses three primary data sources: data on labor inspections provided by the Ministry of Labor, the matched employer-employee records covering the entire formal sector, and the decennial Demographic Census data covering the Brazilian population.

Labor Inspections. The first source of data consists of reports summarizing labor inspections conducted by the Ministry of Labor. The data contain detailed information on tax identifiers, the dates on which the reports are submitted (used as the start dates of inspections), and the labor violations (e.g., formal registration, hours of work, severance pay, minimum wage, etc.) found during inspections.

Firm- and Woker-Level Data. Labor market information are extracted from RAIS (*Relação Anual de Informações Sociais*), a matched employee-employer data from the Ministry of Labor. It provides a comprehensive and high-quality annual overview of the formal sector. Firms report information on their workers, including hiring and separation dates, average annual wages, number of hours contracted, occupation, and demographic characteristics, including gender, age, educational level, race and disability status and type of disability. Because information on whether the worker has a disability and the type of disability only started to be reported in 2003, I link firms who have been inspected to the RAIS datasets from 2003 and 2014, the last year before a deep economic recession hit the country.[13]

[13]Disability status and type of disability are rarely reported in the matched employee-employer data from other countries, limiting progress in research on the role of employers and workplace policies on disability disadvantage in labor markets. This limitation has been described in Baldwin and Johnson (2006): "*there is also a serious shortage of data that support empirical analyses of the labor market experiences of persons with disabilities. (...) The ideal data set includes detailed information on employment, wages, work experience and job characteristics, as well as information on health conditions, functional limitations and disability status.*"

Census Data. The third dataset consists of the decennial Brazilian Demographic Census from 2000 and 2010 sourced from IBGE (Brazilian Institute of Geography and Statistics). The Census contains information on individuals' demographic and socioeconomic characteristics and labor market outcomes, including self-reported degree and type of disability, employment status, and work and non-work (e.g., welfare benefits) incomes. These two waves of Census data allow me to obtain variables of interest before and after the reform. Relative to the matched employee-employer data, the Census has the advantage of providing labor market outcomes outside the formal sector. Due to fundamental methodological differences in questions regarding disabilities from the 1991 and 2000 Censuses, I do not use the Census data from 1991, the first edition with questions on disability.[14]

Other Data. I rely on additional minor sources of data to conduct the main analyses. I gather labor offices' addresses from official sources and link them to their date of creation extracted from Ponczek and Ulyssea (2021) to maintain labor offices created prior to the enactment of the regulation. I also use these information to construct a measure of enforcement capacity by computing the minimum distance between each micro region and the nearest labor office using the Google Maps API.[15] To disentangle several mechanisms behind the results, I also make use of an original pilot survey conducted with human resources executives and personnel. The survey includes vignettes in which disability information is experimentally manipulated, along with questions about affirmative action support, challenges, and

[14]The 1991 Census data has a single question asking respondents if they have one or more of the following conditions: blindness, deafness, hemiplegia, hemiplegia, paraplegia, and intellectual disability. Only 1.23 percent of the working-age population reported having disabilities as opposed to 6.7 (22.4) percent with severe (some) disabilities in 2010. The 2000 and 2010 Censuses ask respondents whether they live with severe, some or no difficulties in seeing, hearing, and walking and climbing stairs separately, and whether they have a permanent intellectual disability. Throughout this paper, I define individuals who report living with some or severe difficulties as persons with disabilities. In addition, as previously mentioned, oversight and monitoring of the AA quota through labor inspections became possible only after 2003. Therefore, I use 2000 as the pre-reform period.

[15]I obtain the distance between the centroid of each municipality and the nearest labor office created prior to 2000s. I then define the minimum distance of the municipalities that belong to each micro region as the minimum distance between each micro region and the nearest labor office, which is used as a proxy for enforcement capacity.

concerns in hiring people with disabilities. Appendix F provides a detailed description of the survey.

1.4 Aggregate Analysis

In this section, I analyze the aggregate effects of the 2000 reform on employment and wages for people with and without disabilities across local labor markets. The next section closely examines how firms respond to the disability quota scheme. The context suggests that labor markets with a high concentration of large plants and frequent labor inspections are more likely to experience an increase in the demand for workers with disabilities after the 2000 reform. To gauge the aggregate effects of the reform on employment and earnings for people with and without disabilities across local labor markets, this section proposes an identification strategy that exploits two dimensions of the reform: the timing of its enactment and the spatial heterogeneity in potential exposure.

1.4.1 Exposure to the 2000 Reform

I construct a measure of local exposure to the reform in three steps. First, I define local labor markets at the micro region level. In Brazil, micro regions consist of an aggregation of economically integrated contiguous municipalities with similar economic and geographic characteristics. These micro regions delineate local economies, similarly to commuting zones in the US. Second, I define potential demand for workers with disabilities across local labor markets as the share of people with disabilities that would potentially benefit from jobs reserved by the AA quota regulation:

$$potential\ demand_r^{pre} = \frac{potential\ jobs_r^{pre}}{total\ PwD_r^{pre}}. \tag{1.1}$$

The numerator, *potential jobs*$_r^{pre}$, represents the total number of jobs in the private sector available to people with disabilities in each micro region r if there is perfect compliance with the AA quota. I calculate this number from the distribution of firm size in 1998, prior to reform, extracted from RAIS data.[16] The denominator, *total PwD*$_r^{pre}$, normalizes potential jobs by the pre-reform number of people with disabilities in each micro region r, calculated from the 2000 Census. The term *potential demand*$_r^{pre}$ ultimately captures pre-reform spatial heterogeneity in potential demand for workers with disabilities across local labor markets and has a median ratio of 5 percent.[17] I then classify micro regions above and below this median as micro regions with strong and weak potential demand. Figure 1.B.1, Appendix B, shows how potential demand varies geographically. The areas with strong potential demand are concentrated in the Southeast and South regions, which are the most developed regions in the country with larger labor markets.

The third step consists of building off a measure of enforcement capacity developed by Almeida and Carneiro (2012) and used in other papers (Haanwinckel and Soares (2021), Ponczek and Ulyssea (2021)) to exploit spatial heterogeneity in enforcement of labor regulation. The Ministry of Labor adopts a straightforward technology of enforcement, requiring only two inputs: inspectors who are assigned to labor offices and travel distance by car between labor offices and inspected firms. In principle, micro regions located farther away from labor offices are less likely to receive inspections. Figure 1.B.4, Appendix B, confirms this negative relationship between the number of inspections per firm and the distance to the nearest labor office. It motivates the use of minimum travel distance to the nearest labor office within each micro region as a proxy for enforcement capacity.[18] To ensure that the

[16] This number is calculated from the RAIS data, which contain information on the total number of employees in each firm and the micro-region where the firm is located.

[17] de Souza (2020) also uses a measure of demand to compare people with disabilities in local labor markets with varying levels of demand generated by hiring quotas between 1991 and 2000. In contrast to de Souza (2020), my measure of potential demand is pre-determined with respect to the reform. In addition, I do not use the 1991 Census data due to changes in the definition of disability between this and the 2000 Census.

[18] For each municipality, I calculate the driving distance from its centroid to the nearest labor office that belongs to the same state. The set of labor offices is restricted to those created prior to 2000. I then take

measure of enforcement capacity does not respond to changes in local labor market conditions and adjustments to the hiring quota regulation, I restrict the analysis to labor offices created before the reform. The median distance is about 60 kilometers (or 66 minutes). I then classify micro regions with pre-determined distances below and above the median as those with weak and strong enforcement capacity. Figure 1.B.2, Appendix B, illustrates the variation in enforcement level across micro regions.

To retrieve a cross-sectional variation, I interact the potential demand and enforcement capacity measures to compute the initial levels of local exposure to the reform. Figure 1.2 plots the geographic variation of the four classifications of the interaction term. Intuitively, persons with disabilities from areas with strong potential demand and enforcement levels should experience larger labor market responses relative to those located in areas with weak potential demand or/and weak enforcement capacity. Important to the empirical strategy, this cross-sectional variation is pre-determined with respect to the introduction of reform.

1.4.2 Empirical Strategy

My empirical strategy for the market-level analysis considers both the timing of the reform and the spatial heterogeneity in exposure to it. Put differently, I estimate the following regression model using the Census data:[19]

$$y_{rst} = \alpha + (\mathbf{1}_r^{SD,SE} \times Reform_t)\beta_1 + (\mathbf{1}_r^{SD,WE} \times Reform_t)\beta_2 + (\mathbf{1}_r^{WD,SE} \times Reform_t)\beta_3 + \alpha_r + \alpha_t + \alpha_s \times t + X_{r,2000}\lambda + \varepsilon_{rst}, \tag{1.2}$$

the minimum of the distances from all municipalities belonging to each micro region r to define the measure of enforcement capacity at the micro region level. The results are robust to alternative definitions, such as maximum and average distances.

[19]The aggregate analysis uses the Census data to provide an overview outside the formal sector. In addition, information on disabilities from the RAIS data started to be collected in 2003, after the reform. In Section 1.4.3, I propose an exercise using RAIS data and find similar patterns.

in which subscripts r, s, and t stand for micro region, state, and time; the indicator variables $\mathbf{1}_r^{SD,SE}$, $\mathbf{1}_r^{SD,WE}$, and $\mathbf{1}_r^{WD,SE}$ represent micro regions with strong potential demand and enforcement capacity, with strong potential demand and weak enforcement capacity, and with weak potential demand and strong enforcement capacity, respectively; $Reform_t$ is an indicator for the period after the 2000 reform; α_r and α_t delineate micro-region and time fixed effects; $\alpha_s \times t$ represents state-specific trends; $X_{r,2000}$ is the vector of baseline characteristics of the micro regions in 2000, including the share of female population, share of population with college education, share of urban population, unemployment rate, income per capita, and total population, all interacted with time fixed effects; y_{rst} is the labor market outcome of interest. Standard errors are clustered at the state level.

The coefficients of interest—β_1 to β_3—capture differential labor market responses across micro regions with distinct levels of exposure relative to micro regions with weak potential demand and enforcement capacity before and after the reform. To assuage concerns related to common shocks affecting micro regions and time-invariant characteristics of micro regions that might be correlated with both the exposure measure and the outcomes of interest, this specification includes both time and micro region fixed effects. I also add state-specific trends to control for policies or unobservable shocks specific to states. The set of baseline controls X_{rt} accounts for heterogeneous initial characteristics that can also influence the labor market outcomes, permitting differential trends across micro regions with heterogeneous initial characteristics.[20]

This empirical strategy relies on the assumption that, conditional on the set of baseline characteristics, the cross-sectional local exposure measure is orthogonal to omitted char-

[20] Although the exposure measure is constructed to be pre-determined with respect to the introduction of the quota regulation, it might be correlated with initial labor market characteristics for people with disabilities across Brazilian micro regions. For instance, micro regions with different initial levels of economic characteristics might undergo different labor market paths, implying that our estimates could capture differential economic trends across micro regions. Including baseline observable characteristics of micro regions in 2000, interacted with time fixed effects, allows for differential trends across micro regions with heterogeneous initial characteristics.

acteristics correlated with differential changes in labor market outcomes for people with disabilities after the reform. While it is not possible to directly test for this assumption, three additional pieces of evidence mitigate concerns related to the empirical strategy. First, the cross-sectional variation is constructed to be pre-determined with respect to the reform's passing. Second, I find no evidence of pre-trends when using alternative data (see Section 1.4.3). Third, I also examine the effects on the informal sector and people without disabilities as placebo tests. One could argue that some omitted characteristics, captured by the exposure measure, may remain unaccounted for. In this context, it is hard to think of omitted variables that would simultaneously lead to an increase in formal employment and no impacts on informality for people with disabilities in response to the reform within the same state and after controlling for the set of baseline controls. In addition, I do not find evidence that workers without disabilities from both the formal and informal sectors are affected by the reform.

Sample and Summary Statistics. Starting with individual-level data from the Census, the sample includes working-age population aged between 25 and 54 from to focus on individuals with strong labor force attachment. For each micro region, I separately compute employment and informality rates and average incomes for people with and without disabilities. Table 1.C.1, Appendix C, displays the summary statistics of the variables used in the aggregate analysis and confirms that people with disabilities have worse labor market prospects than those without disabilities. Most variables display a high dispersion, indicating that micro regions are quite heterogeneous.

1.4.3 Market-Level Results

I begin by documenting the relationship between local exposure measures and changes in employment. Table 1.1 compares micro regions that belong to each of the three groups

of exposure to those with weak potential demand and enforcement capacity. Column (1) indicates that the reform leads to a significant increase by 1.1 percentage points (p.p.) in the share of people with disabilities who are employed in the formal sector exclusively in local labor markets with strong potential demand and enforcement capacity. The magnitude is equivalent to an increase by 9.5 percent relative to the baseline mean of 0.116 in 2000. In other words, moving a region from the lower to the upper median of the distribution of enforcement capacity and potential demand would induce an increase of 9.5 percent of formal employment for people with disabilities.

Column (1) confirms that labor market prospects for workers with disabilities located in areas with either weak potential demand or weak enforcement capacity remain unaffected after the reform, reinforcing the strong complementarities between enforcement and regulation. As falsification tests, Columns (2) to (4) assess whether the exposure measure is correlated with changes in formal employment for people without disabilities or in the informal sector. I find small and statistically insignificant coefficients across all specifications. The lack of changes in informality rates for people with disabilities also suggests that the increase in formal employment comes from non-employment.

Turning to the effects on earnings, Table 1.2 shows that only micro regions with strong potential demand and enforcement capacity display larger impacts on income from work for people with disabilities. The average work income rises by around 34.55 Brazilian *reais*, equivalent to a 9.5 percent increase relative to the baseline mean in 2000. It is a mechanical result as the reform is also associated with higher employment levels for this group. I do not find evidence of spillovers on non-work income (Column (2)) or people without disabilities (Columns (3) and (4)).[21] These findings reveal that the benefits of the reform are

[21] Columns (1) and (3) of Table 1.2 suggest that I cannot reject that the coefficients are different for people with and without disabilities.

mostly accrued by people with disabilities from local labor markets with higher exposure to enforcement capacity and AA quotas.[22]

Robustness Checks. I conduct some additional checks to probe the robustness of the aggregate analysis. First, Table 1.C.3, Appendix C, shows that the main conclusions do not change when considering the mean and maximum travel distances as alternatives to the minimum distance to calculate the exposure measure of enforcement capacity. Second, I use RAIS data to overcome the lack of pre-reform years in the Census. Because information on disability only started to be collected in 2003, I use data from 2003 to 2018 to identify individuals ever reported as disabled to assign retroactive information on disability to individuals found in RAIS between 1997 and 2002. This approach, albeit imperfect, permits an indirect test for pre-trends using the share of people with disabilities in the formal sector as the outcome variable. Figure 1.B.5, Appendix B, corroborates the lack of pre-trends and validates the previous findings. Third, Column 1 of Table 1.C.4, Appendix C, confirms that migration does not drive the increase in employment, mitigating concerns that the reform induced spatial reallocation of people with disabilities to more exposed local labor markets.

1.5 Firm-Level Analysis

The market-level results reveal that the reform induced higher formal employment and earnings for people with disabilities in more exposed local labor markets without generating spillover effects to workers without disabilities or workers in the informal sector. An interesting question is whether and how firms adjust to the mandated employment. Under-

[22]Table 1.C.2, Appendix C, considers work income from the formal sector and work income from the informal sector as outcome variables. These definitions are different from overall work income in Table 1.2. Conditional on employment, I do not find differences in work income in both the formal and informal sectors, corroborating that the baseline gains in earnings are driven by higher employment in the formal sector rather than higher earnings among the employed. Manning (2011) argues that increasing mandated employment does not necessarily translate into an increase in wages. Firms can increase recruitment activity to generate extra supply or reduce worker quality. I document evidence of both strategies in Section 1.5.

standing the reallocation of firm activities in the cross section of firms is key to quantifying the policy incidence in light of extensive evidence on the role of firms in shaping labor market inequalities. To address this question, I combine the requirement for firms with at least 100 employees to have workers with disabilities and the variation in the exposure to the affirmative action quota generated by inspections. The firm-level analysis provides compelling graphical evidence in the short- and medium-terms and examines a broad range of outcomes and mechanisms.

1.5.1 The Employment Effects

Empirical Strategy

In the first part of the firm-level analysis, I estimate the employment effects of the hiring quota. Because the AA quota is rarely enforced without labor inspections and only firms with at least 100 employees may be required to hire workers with disabilities, my empirical strategy exploits both the AA quota requirement and the precise timing of the labor inspections by comparing inspected firms with pre-inspection employment levels above and below the cutoff of 100 workers, which represent treatment and control firms, before and after inspection. I estimate the following event-study model:[23]

$$y_{jt} = \sum_{k=-6}^{k=12} [\beta_k^{Quota} \times \mathbf{1}(t_j = t^* + k) \times Quota_{j,-1} + \theta_k \times \mathbf{1}(t_j = t^* + k)] + \alpha_j + \alpha_t + X_{jt}\gamma + \varepsilon_{jt}, \tag{1.4}$$

[23]In addition to the event-study analysis, I also perform difference-in-differences analyses in which I pool pre- and post-inspection quarters and estimate the average employment changes considering the following model:

$$y_{jt} = \alpha_j + \alpha_t + \tilde{\beta} \times Post_t \times Quota_{j,-1} + X_{jt}\gamma + \varepsilon_{jt}, \tag{1.3}$$

in which subscripts and the set of controls and fixed effects are the same as in Equation (2.1), and $Post_t \times Quota_{j,-1}$ is an indicator variable equal to 1 for all quarters after inspection in firms under the AA quota. As before, standard errors are clustered at the firm level.

in which subscripts j and t stand for firm and quarter-year; $\mathbf{1}(t_j = t^* + k)$ are dummies indicating an event in quarter-year k relative to the quarter-year t^* in which the firm is inspected; $Quota_{j,-1} = \mathbf{1}(\text{Emp}_j \geq 100)$ is an indicator variable for firms with at least 100 employees in the quarter-year prior to inspection, which represent the treated group; α_j are firm fixed effects; α_t are quarter-year fixed effects; X_{jt} are time-varying firm-level controls and include state- and industry-specific trends; and y_{jt} is the employment outcome of interest. Year fixed effects control for common shocks affecting the firms each quarter-year. Firm fixed effects control for time-invariant characteristics of firms that might be correlated with the outcomes of interest and the AA quota requirement. Standard errors are clustered at the firm level.[24]

The post-event coefficients of interest—β_k^{Quota}—capture the dynamics effects of the AA quota requirement relative to the quarter-year before the labor inspection. To mitigate concerns related to selection into labor inspections, the sample contains *all* firms who have been inspected. Once inspected, the firms are assessed for compliance with the main dimensions of the labor regulation, including formal registration, minimum wage, and mandated benefits. The key difference is that *only* firms with at least 100 employees by the time of the inspection may be assessed for compliance with the AA quota targeting people with disabilities, allowing me to exploit the differential impacts based on the regulation threshold. To my knowledge, there is no other firm regulation using the threshold of 100 employees in the country. Identification in Equation (2.1) relies on the timing of labor inspection being uncorrelated with the outcomes of interest, *conditional* on firm and time fixed effects and

[24]One natural candidate for identification in this context is the use of a local regression discontinuity design (RDD). However, this strategy is not compelling because the running variable is rarely well-defined in the data for employment measures and there is some potential firm selection around the threshold of interest. Including firm and time fixed effects, along with industry and location trends, mitigates concerns related to firm selection and allows me to focus exclusively on the variation occurring across quarters and within firms. Another potential candidate is the bunching estimator. As shown in Figures 1.D.1 and 1.D.2, Appendix D, there is no visual evidence that firms bunch below the 100 employees threshold, suggesting that firms do not avoid being subject to the AA quota regulation. These pieces of evidence motivate dynamic difference-in-differences design as my main empirical strategy.

firm-level controls, X_{jt}. The key identifying assumption is that outcomes for treated and control firms would have followed parallel trends in $k > 0$ if no inspection had occurred for treated firms. I test this assumption by assessing whether the pre-event coefficients of interest are statistically indistinguishable from zero.

The estimates are likely to be biased if firms in different size categories had different trends in the absence of labor inspections. For instance, economic shocks might have affected large and small firms differently. I implement several additional steps to assuage these concerns. First, the baseline specification includes a local sample of firms with pre-inspection employment levels between 75 and 125 employees. Second, I probe the robustness of my main results by considering narrower bandwidths around the cutoff of 100 employees and dropping firms very close to it. Third, time-varying controls X_{jt} include state- and industry-specific trends to control for policies or unobservable shocks specific to states and industries.

To capture the employment effects of the AA quota, I consider four complementary outcomes: the extensive and intensive margins of employment responses, measured by an indicator variable for having at least one worker with disabilities and an inverse hyperbolic sine transformation of the number of workers with disabilities, the share of workers with disabilities, defined as total workers with disabilities divided by the total number of workers in each firm, and an inverse hyperbolic sine transformation of total workers without disabilities.[25] In Appendix D, I additionally examine the impacts on hires and separations of workers with and without disabilities.

Sample and Summary Statistics. I take several steps to construct the sample of interest. First, I obtain a list of firms who have been inspected together with the earliest date of inspection reports to avoid duplicated observations. I do not impose any restriction

[25] I apply an inverse hyperbolic sine transformation to handle zeroes in the data. The transformation is given by $asinh(y) = \log(y + \sqrt{y^2 + 1})$. As robustness checks, I also show that my results are robust to adding one before taking the log, using the absolute number of workers with disabilities, and replacing the ordinary least square estimates with a conditional fixed-effect Poisson model.

related to violations brought by inspections. After generating quarterly labor market information from the RAIS data, the second step consists of matching the list of inspected firms to the quarterly data.[26] Third, I limit the sample to firms found in the RAIS data from six quarters before to twelve quarters after the first inspection, allowing me to estimate the dynamic impacts spanning almost five years. Fourth, I categorize firms with less or more than 100 employees as control and treatment groups using information on the total number of employees in the quarter before the inspection since it is the criterion used by inspectors to assess compliance with the AA quota.

Table 1.E.1, Appendix E, presents summary statistics for both groups after aggregating data for the quarters before and after inspections. The numbers confirm that, prior to inspections, control and treatment firms are comparable along observable dimensions, such as the number of workers with disabilities, share of workers with disabilities, log average earnings, location, and sector distribution. The only exception is that, as expected, control firms have, on average, less employees (72.23 employees) relative to the treated firms (93.24). After inspections, employment measures for people with disabilities experience larger increases in treated firms. For instance, the average number of employees with disabilities increases from 0.48 to 0.74. In addition, 25 percent of treated firms report having at least one worker with disabilities after inspections, compared to 12 percent from the pre-inspection period. Control firms experience more modest increases along these dimensions, corroborating that compliance with quota is not required for them after inspections.

Firm-Level Results

This section presents my main results. I start by documenting the impacts of the AA quota on employment. I then investigate whether other firm outcomes, including average earnings and profits, proxied by firm exit, change. I also discuss other margins of firm

[26]Since RAIS data do not contain quarterly information on employment, I combine worker-level information on hiring and separation to transform annual data into quarterly data.

responses by analyzing heterogeneity across occupations, educational level, and type of disabilities. I perform several tests to probe the robustness of my main results.

Effects on Employment. I document strong and persistent increases in employment for people with disabilities resulting from enforcement of the AA quota. Figure 1.3 displays $\hat{\beta}_k^{Quota}$, along with 95 percent confidence intervals, after estimating Equation (2.1) for selected variables. The pre-event coefficients are statistically equal to zero, supporting the assumption that both treatment and control firms have similar pre-inspection trends. Following inspections, there is a sharp increase in the number of workers with disabilities, a pattern that becomes strong and persistent over time.

Panel A of Table 1.3 reports the immediate ($k = 0$), short-run ($k = 6$), and long-run ($k = 12$) impacts, whereas Panel B displays the aggregate impacts. The point estimates for employment for persons with disabilities grow within three years (Column (1) and Figure 2.D.4b). The magnitude of the estimate indicates an increase of 41.7 percent in the number of workers with disabilities after inspections.[27] Considering the extensive margin of employment, Column (2) and Figure 2.D.4d report that treated firms, in the long run, are 7.7 p.p. more likely to have at least one employee with disabilities after inspections. Column (4) and Figure 1.3d show that this increase is not followed by a significant decline in log workers without disabilities. In addition, Appendix D presents the findings for new hires and separations, confirming that adjustments in employment mostly come from higher arrival rates rather than lower departure rates. This suggests that search frictions are unlikely to constitute a major barrier of compliance with the AA quota.

Effects on Other Firm Outcomes. I also investigate other margins of responses to understand whether firms finance new hires through lower average wages or profits. Because

[27]Bellemare and Wichman (2020) derive the elasticity for an arcsinh-linear specification, which is equivalent to $\hat{\beta}x\frac{\sqrt{y^2+1}}{y}$.

I do not have data on firm profits, I use an indicator of exiting the formal sector as an imperfect proxy for profits. Exit is defined as equal to one if the firm does not have any formal employee in a given quarter-year. Table 1.4 indicates no evidence of statistically significant impacts on wages for workers without disabilities, or for firm exit.[28] In addition, I find no evidence for avoidance in the form of bunching below the threshold that would arise in case of costly compliance with the regulation.

Heterogeneity. The level of detail of the RAIS data allows me to scrutinize the extent to which firms respond to the AA quota considering different levels of corporate hierarchy. I have information on occupations, allowing me to categorize each worker into one of the following categories: (i) managers (e.g., manager and director); (ii) high-skill professionals (e.g. researchers, teachers, doctors, nurse, engineers, technicians, architects, mathematicians, and statisticians); (iii) low-skill white collar jobs (e.g., cashier, receptionist, secretary, and library assistant); and (iv) blue collar jobs. Figure 1.D.4, Appendix D, shows that the increase in employment for people with disabilities is concentrated among low-skill, low-paying occupations.[29] Concerning heterogeneity across educational levels, Figure 1.D.4 confirms that firms recruit more from those without college degree.

Another important source of heterogeneity relates to the distinction between different types of disabilities, a unique feature from the Brazilian data. Starting in 2006, employers report whether the worker has one of the following disabilities: physical, hearing, visual, intellectual, or multiple (two or more disabilities). Individuals who received vocational rehabilitation services may also be classified as having disabilities for quota purposes. Figure

[28]Evidence on the impacts of the AA quota on profits is mixed. Consistent with my results, Mori and Sakamoto (2018) find that firm profits are not affected in Japan despite the increase in employment of people with disabilities. Peck (2017) documents opposite findings in Saudi Arabia's *Nitaqat* program, which determined quotas to hire Saudis at private firms. Unlike my context, the Saudi program required aggressive quotas for firms.

[29]Consistent with this result, in the US context, Holzer and Neumark (1999) show that firms under affirmative action are more likely to hire women and minorities with lower levels of education and for jobs with lower skill requirements. The authors do not find evidence of weaker performance of these new hires.

1.D.4 indicates that firms are more likely to hire workers with physical disabilities, followed by hearing and other disabilities, suggesting preferences for milder forms of disabilities.[30]

Robustness Checks. Table 1.E.5, Appendix E, reports additional checks to ensure that my findings are robust to alternative variable, specification, and sample definitions. Column (1) repeats the benchmark specification from Table 1.3. Columns (2) and (3) confirm that conclusions regarding employment of people with disabilities do not change when considering total number of employees plus one as the dependent variable and taking its natural logarithm. Column (4) excludes state- and industry-specific trends and shows similar results. Columns (5) and (6) replace the ordinary least square estimator with a conditional fixed-effect Poisson model to account for count data. The Poisson method corroborates the magnitude of employment effects for people with disabilities (58.9 percent) and the lack of impacts for workers without disabilities. As discussed in Section 1.5.1, Columns (7) and (8) consider more local specifications with closer bandwidths around the threshold of 100 employees, whereas Columns (9) and (10) exclude firms close to it. I find similar estimates across sample restrictions, confirming that the results are not sensitive to measurement error of the treatment definition.

[30]Tables 1.E.2– 1.E.4, Appendix E, display the point estimates for the heterogeneity analysis. Because visual, intellectual, and multiple disabilities represent a small fraction of disabilities, I pool them together. Unfortunately, RAIS data does not contain further details on disabilities. Instead, I use an alternative source of data from the public sector with personnel records from the federal government in 2022 to get a sense of the distribution of disabilities between employed individuals: partial visual impairment (16.72 percent); congenital or acquired deformity (13.11 percent); reduced mobility, permanent or temporary (10.17 percent); partial hearing impairment (8.46 percent); bilateral hearing impairment (5.98 percent); monoparesis (5.33 percent); amputation (5 percent); deafness (4.73 percent); paraplegia (3.66 percent); monoplegia (3.12 percent); blindness (2.82 percent); hemiparesis (1.83 percent); paraparesis (1.70 percent); multiple disabilities (1.20 percent); tetraparesis (0.99 percent); cerebral palsy (0.98 percent); hemiplegia (0.92 percent); intellectual disability (0.71 percent); tetraplegia (0.47 percent); dwarfism (0.35 percent), and others.

1.5.2 The Wage Effects

Empirical Strategy

While the conclusion that AA quotas led to higher employment for the targeted group is consistent with the literature, there is a dearth of evidence of the impacts of quotas on workers' outcomes, including wages. To further understand the effects on workers' outcomes, I estimate the following specification:[31]

$$w_{ijt} = \sum_{k=-6}^{k=12} [\beta_k^{Quota} \times \mathbf{1}(t_j = t^* + k) \times Quota_{j,-1} + \theta_k \times \mathbf{1}(t_j = t^* + k)] + \alpha_j + \alpha_t + X_{jt}\gamma + X_{ijt}\delta + \varepsilon_{ijt}, \tag{1.6}$$

in which subscripts i, j and t stand for worker, firm and quarter-year; $\mathbf{1}(t_j = t^* + k)$ are dummies indicating an event in quarter-year k relative to the quarter-year t^* in which the firm is inspected; $Quota_{j,-1} = \mathbf{1}(\text{Emp}_j \geq 100)$ is an indicator variable for firms under the AA quota for having at least 100 employees in the quarter-year prior to inspection; α_j are firm fixed effects; α_t are quarter-year fixed effects; and the vectors X_{jt} and X_{ijt} represent firm- and worker-level controls. Firm-level controls consist of state- and industry-specific trends. Worker controls include individual characteristics available in the RAIS data, such as gender, race, educational level fixed effects, age, and square age, along with occupation group fixed effects. Standard errors are clustered at the firm level.

Similar to Equation (2.1), the post-event coefficients—β_k^{Quota}—capture the dynamics impacts of the AA quota relative to the quarter-year before the labor inspection. Identification

[31] In addition to the event-study analysis, I also perform a difference-in-differences analysis in which I pool pre- and post-inspection quarters and estimate the change in wages considering the following model:

$$w_{ijt} = \alpha_j + \alpha_t + \tilde{\beta} \times Post_t \times Quota_{j,-1} + \alpha_j + \alpha_t + X_{jt}\gamma + X_{ijt}\delta + \varepsilon_{ijt}, \tag{1.5}$$

in which subscripts and the set of controls and fixed effects are the same as in Equation (1.6), and $Post_t \times Quota_{j,-1}$ is an indicator variable equal to 1 for all quarters after inspection in firms under AA quota. As before, standard errors are clustered at the firm level.

assumptions of Equation (1.6) rely on the timing of labor inspection being uncorrelated with the outcomes of interest, *conditional* on the set of controls. The key identifying assumption is that the wage outcomes for workers in firms with and without AA quota requirement, representing treatment and control firms, would have followed parallel trends in $k > 0$ if no inspection had occurred for firms under the AA quota. I test this assumption by assessing whether the pre-event coefficients of interest are statistically indistinguishable from zero.

Sample and Summary Statistics To examine the wage effects, I use the natural logarithm of hourly wages as the main outcome. This outcome combines information on contracted monthly hours and average wages.[32] In order to obtain the sample of workers, I recover all individuals that worked full time at the same set of firms from the firm-level analysis spanning the period from six quarters before to twelve quarters after the inspection. Using worker-level data is appropriate because the impacts at the firm level could be confounded by compositional changes rather than reflect changes in wages for similar workers. In addition, I construct the worker-level sample at the quarterly frequency, which offers two advantages.[33] This sample is comparable with the firm-level sample, enabling a closer examination of the dynamics effects. The second advantage is to enlarge the sample size, increasing statistical power. Statistical power is an important concern in this context since people with disabilities are vastly underemployed. The interpretation of the results remains unchanged when considering annual frequency.

I present summary statistics during the quarters before and after inspections for workers from both treated and control firms. Table 1.E.6, Appendix E, indicates that both groups of workers are similar along observable characteristics prior to inspections, including wages, disability, gender, race, education, occupation, location, and economic sector. Considering

[32]This measure of wages also contains other forms of monetary work compensation, including overtime premiums, bonuses, commissions, and other benefits mandated by law.

[33]Until 2015, information on wages from the RAIS data are reported at the annual frequency. I transform them into quarterly data by combining information on earnings with hiring and separation dates.

the pre-inspection period, nearly 0.4 (0.3) percent of control (treated) workers have a disability, earn about 11.34 Brazilian *reais* (11.46 Brazilian *reais*) as hourly earnings, 66 (65) percent are male, 67 (69) percent are white, 10 (10) percent have a college degree, and 66 (66) percent have blue collar jobs.

Worker-Level Results

To gauge the impact of the AA quota on workers' wages, I estimate Equation (1.6), which directly compares workers from firms under the AA quota to those unaffected by the regulation, before and after inspection. Figure 1.4 illustrates the dynamics of wages around inspection shocks separately for workers with and without disabilities. The pre-event coefficients are statistically equal to zero, validating my empirical strategy. As expected, the confidence intervals for persons with disabilities are larger due to the smaller sample size.

Table 1.5 displays the immediate ($k = 0$), short run ($k = 6$), long run ($k = 12$) impacts, together with the aggregate impacts. In the first quarters following inspections, the estimates for both groups of workers are statistically insignificant. Over time, the quarterly wage growth does not change for workers without disabilities, which can be interpreted as the policy having limited consequences on non-targeted groups. On the other hand, wage growth becomes slower for people with disabilities at firms under the AA quota. The estimates imply that these workers experience 6 percent slower wage growth relative to the baseline wage growth rates (Column (4) of Table 1.5). Including firm and quarter fixed effects requires firms from treatment and control groups to have at least one worker with disabilities prior to inspections to estimate the coefficient of interest for people with disabilities.

Table 1.5 also investigates whether the results are driven by incumbent workers or new hires (Columns (5) and (6)). Figure 1.D.5, Appendix D, plots the estimates for the subset of individuals who are hired in each quarter-year of the sample and shows no evidence of differential wage growth. Figures 1.4c and 1.4d indicate that incumbent workers with dis-

abilities drive the wage effects.[34] The lack of pass-through to new hires could be explained by labor contract rigidities or labor market institutions, such as wage floors and minimum wage.[35] As such, firms adjust through incumbents' wages from the targeted group with slower wage growth. The results point to an unintended consequence of mandated employment: despite the reduction in inequality at the *hiring* level through higher employment opportunities, firms can adjust to mandated employment through lower wage growth, exacerbating *within*-firm inequality *between* groups and, to some extent, reversing the goal of the policy.

One concern with my baseline estimates is that, on average, workers with disabilities are less educated, less experienced and more likely to be employed in low skill occupations, implying that the wage results could reflect differences along these dimensions and, as a result, in productivity. Including worker controls does not affect the results (Column (2) of Table 1.5). As a sanity check, I also report estimates using two re-weighting methods to reduce observational dissimilarities and allow workers with and without disabilities to be similar along several pre-inspection characteristics: gender, race, age, squared age, education, and occupation. Figure 1.D.6a presents coefficients using inverse propensity score weights computed from this set of characteristics, whereas Figure 1.D.6b shows estimates using the entropy-balancing weights from Hainmueller (2012) to ensure balance across the

[34] In particular, I estimate a modified version of Equation (1.6):

$$w_{ijt} = \sum_{k=-6}^{k=12} [\beta_k^{Quota} \times \mathbf{1}(t_j = t^* + k) \times Quota_{j,-1} + \theta_k \times \mathbf{1}(t_j = t^* + k)] + \alpha_i + \alpha_j + \alpha_t + \varepsilon_{ijt}, \qquad (1.7)$$

in which the subscripts and the remaining variables are the same as in Equation (1.6); and α_i are worker fixed effects. Including worker fixed effects ensures that the coefficients of interest, β_k, capture the impact on wages *within* workers. The findings are robust to restricting the sample to workers in the same firm over the analysis period.

[35] The heterogeneity analysis across different levels of corporate hierarchy indicates that the earnings effects are concentrated in white-collar rather than blue-collar jobs, consistent with blue-collar jobs being more constrained by labor market institutions.

same characteristics. I note that the results are robust to re-weighting methods, alleviating concerns related to dissimilarities in observable characteristics across disability status.

Magnitude. To better understand the magnitude of the reduced wage growth, I compare it with other contexts. For instance, a burgeoning literature studying the relationship between employer concentration and wages has documented that increasing labor market concentration is associated with wage reductions ranging from 2.9 to 26 percent (Arnold (2019), Azar et al. (2022), Benmelech et al. (2022), Prager and Schmitt (2021), Qiu and Sojourner (2019), Rinz (2022)). The displacement literature has documented earnings losses across countries that range from 6 to 42 percent, though estimates from the lower end are far more common (Jacobson et al. (1993), Couch and Placzek (2010), Lachowska et al. (2020), Davis and Von Wachter (2011), Bertheau et al. (2022)). In the Brazilian context, Bhalotra et al. (2021) find a 42 percent decline in work income up to three years after displacement caused by mass layoffs. My estimates are about one-seventh of the expected earnings losses from displacement.

Effects on Additional Worker Outcomes. Figure 1.5 and Table 1.6 display results for other labor market outcomes. I document the impacts on the intensive margin of employment measured by the number of hours specified in the employment contract and the likelihood of part-time employment. Although the RAIS data do not provide the actual number of hours worked, contracted hours and part-time status are informative about adjustments through intensive margins. Figures 1.5a and 1.5b corroborate the lack of evidence supporting such responses.

I also analyze the effects on turnover and promotion. Figure 1.5c shows that the probability of staying at the firm is higher for incumbents with disabilities in firms under AA quota in the first quarters following inspection and then gradually dwindles. To measure the impacts on the probability of internal promotion, I compute the average wages for each

6-digit occupation cell after regressing log hourly earnings on 6-digits occupation and individual fixed effects in a random sample of 20 percent of workers in the private sector. With the occupation effects in hand, I define promotion as an indicator variable equal to one if the worker switches into an occupation that pays higher wages at the same firm relative to the previous quarter. The treatment effect from Column (3) of Table 1.6, albeit relatively noisy, points to a decrease in the likelihood of promotion of 1.4 percentage points for workers with disabilities in firms under the AA quota relative to those in firms without the AA quota. I do not find similar patterns for workers without disabilities.

Heterogeneity and Mechanisms

Using observational data and qualitative evidence from a survey conducted with firms (see Appendix F for further details) together with an analysis of heterogeneous responses, this section provides suggestive evidence pointing to statistical discrimination as a key driver behind the worker-level results on wage effects among incumbent workers with disabilities. I also examine to what extent the main results could be rationalized through alternative explanations, with the limitation that the findings only allow a suggestive glimpse into mechanisms due to the lack of on-the-job data and clean experiments. Which mechanism ultimately explains my findings is hampered by data constraints and left for future work.

Discrimination. A large body of literature has documented employer discrimination against people with disabilities using observational evidence (Baldwin and Johnson (1994), Baldwin and Johnson (2006)) or experiments (Baert (2016), Ameri et al. (2018)). Discrimination can can be taste-based (Becker (1957)) or statistical (Phelps (1972), Arrow (1973), Aigner and Cain (1977)). Becker (1957)'s theory of taste-based discrimination posits that employers have a negative animus towards people with disabilities regardless of productivity considerations. I note that two pieces of evidence seem inconsistent with this theory as an explanation for wage slowdowns. First, an affirmative action policy would make firms hire

more qualified workers with disabilities. On the contrary, I find that firms recruit more workers with lower educational levels. Second, because the empirical findings are driven by incumbents with disabilities, the taste-based theory implies that employers have developed some distaste for workers with disabilities over time. This seems unlikely in light of evidence that tastes are not easily malleable in the short-term (Beaman et al. (2009)).

Models of statistical discrimination assume that employers cannot directly observe workers' skills. Instead, they observe signals to infer workers' skills and are less able to interpret signals of workers from a minority group. In a dynamic setting, Bjerk (2008) shows that, if two groups differ in average skill level or in frequency they can signal their skills at lower level jobs, similarly skilled workers from two groups can display different career progression because members from the minority group need to accumulate more positive signals to get better task assignments. Lehmann (2011) shows that this pattern can be the result of an affirmative action policy, in which firms recruit more workers from the minority group, but require more positive signals for better task assignments.[36]

In my setting, AA quotas induce firms to lower hiring standards to recruit more workers with disabilities, making signals of this group more difficult to interpret (Coate and Loury (1993), Moro and Norman (2003), Fang and Moro (2011)). Indeed, both observational and survey evidence suggest that the average skill level declines with an increase of less educated workers among the group of persons with disabilities. For instance, Figure 1.D.4, Appendix D, reveals that firms under the AA quota hire more disabled workers without a college degree. Survey responses in Appendix F also indicate that firms under the AA quota are more likely

[36]Bjerk (2008)'s "sticky-floor" model was originally used to explain the underrepresentation of women and minorities in top jobs in the absence of discrimination with respect to promotion. In another dynamic model of statistical discrimination without affirmative action, Fryer Jr (2007) theoretically shows that, once members from a group with negative stereotypes overcome discrimination at the initial stage and are hired, the successful members from this group are more likely to be promoted due to a "belief flipping". In this scenario, there is a general pessimism about a group, though optimism about the successful members of this group. This model would explain, under some conditions, overrepresentation of minorities in the highest jobs. Lehmann (2011)'s model explains the increase of black workers in law firms at the hiring stage and their underrepresentation as partners.

to report not being able to find qualified people with disabilities or differences in productivity and management time between workers with and without disabilities as challenges for the company.

A key prediction of dynamic statistical discrimination is that firms under the AA quota require more positive signals from workers with disabilities for better task assignments, retarding their career progression. To indirectly test for it without on-the-job data, I examine whether workers who are less able to send signals of their skills are more likely to experience wage growth slowdowns in firms under the AA quota. I use two dimensions along which workers can send signals of their skills: educational degree and frequency of communication with supervisors and peers. I obtain a measure of frequent communication after matching occupations from RAIS data to O*NET database, which ranks occupations according to on-the-job interactions.[37] Consistent with statistical discrimination being more relevant to workers with weaker signals, Table 1.7 reveals that the wage growth slowdowns are concentrated among disabled workers without college education and in jobs with less interpersonal relationships. Table 1.7 also confirms that workers who can send stronger signals of their skills remain unaffected. Regardless of the final mechanism, the fact that group differences are sustained points to the existence of multiple equilibria under affirmative action (Coate and Loury (1993)).

Alternative Mechanisms. I also investigate other possible mechanisms that could explain the wage results. The goal of this exercise is not to disprove that these other channels play a role in the results. Instead, I provide suggestive evidence that they are unlikely in my setting.

[37]The O*NET database describes and ranks a variety of work activities, including skill requirements. I classify jobs below and above the median of communicating with supervisors and peers, which is defined as "providing information to supervisors, co-workers, and subordinates by telephone, in written form, e-mail, or in person" as those that workers can signal their abilities more frequently. The results are not sensitive to the choice of measure of interpersonal relationships since I find similar results for other work activities.

One possible explanation is that firms may incur sizable and irreversible fixed costs due to the AA quota. For instance, employers may experience increases in workplace accommodation costs (e.g., assistive technologies) or investments in capital (e.g., specialized employment agencies) to improve screening of candidates with disabilities (Miller (2017)).[38] As a result, employers may offset the extra irreversible fixed costs by lowering wage growth for workers with disabilities. Several pieces of evidence reject this explanation. Incumbents were hired before the inflow of new hires, suggesting that, if these costs exist, they were incurred before the new hires. The survey indicates that accommodation costs and screening technologies play a very minor role.[39] The lack of evidence supporting that firms provide accommodations or other amenities also implies that the theory of compensating differentials (Rosen (1974), Rosen (1986)), in which firms may provide lower wages premium to compensate for positive amenity shocks, is an unlikely explanation.

In line with the fixed costs theory, rent-sharing models indicate that the AA quota might affect firm rents, impacting the wages of workers, especially stayers (e.g., Stole and Zwiebel (1996)), Cahuc et al. (2008), Caldwell and Harmon (2019)), Jäger and Heining (2022)). Such models are consistent with lower wage growth. However, ancillary evidence that firm exit

[38]Workplace accommodation costs are frictions that have received a lot of attention in the literature on labor market participation for people with disabilities (Oi (1991), Rosen (1991), Acemoglu and Angrist (2001)). Building on seminal Phelps (1972)' model of statistical discrimination, Miller (2017) argues that an AA quota may induce employers to invest more in screening capital. When subject to an AA quota, employers prefer to hire the most productive candidates from the benefited group. A potential drawback is that employers only observe a noisy signal for each candidate's productivity. The screening model states that firms can invest in screening capital, such as specialized human resources personnel and employment agencies, to improve screening of minority candidates, which can also be interpreted as irreversible fixed costs (Holzer and Neumark (2000)).

[39]Only 8 percent of firms in the survey provided workplace accommodations to the last hire with disabilities. None of the respondents reported that it cost more than hiring someone without disabilities for the same position. As a benchmark, in a survey about workplace accommodations conducted by the Job Accommodation Network (JAN) and sponsored by the Department of Labor in the US since 2004, 56 percent of firms reported that the accommodations needed by employees did not cost anything; 39 percent had a one-time cost; and only 5 percent said the accommodation resulted in annual costs. The median expenditure on accommodations with a one-time cost was $500. When asked about the costs incurred to accommodate an employee with disabilities in addition to what they would have paid for an employee without in the same position, the median answer given by employers is $20 (Job Accommodation Network (2020)).

and wages of workers without disabilities remain unaffected, along with robustness from re-weighting methods, suggests that reduced rents are unlikely drivers of the results.

Retaliation against employees due to whistleblowing could also explain the findings. For example, firms might believe that employees are engaged in complaints against them. First, the administrative data point that 42.07, 19.62, 16.67, and 12.56 percent of the firm-level sample have non-mutually exclusive violations related to formal registration, severance pay, working hours, and days off. These statistics indicate that it is unlikely that firms infer that incumbents with disabilities are the whistleblowers. Second, the survey found that 41.67 percent of respondents thought that inspections were likely triggered by employees. When asked who they think made these complaints, incumbents with disabilities are never mentioned.

1.6 Implications for Welfare

Thus far, the empirical results point to employment and wage consequences of enforcing AA quotas. To explore potential normative implications of my findings for aggregate welfare, I introduce a simple model of an enforcement of AA quotas with imperfect compliance. Following insights from the literature on law enforcement and disability insurance (Diamond and Sheshinski (1995), Burlando and Motta (2016), Haller et al. (2020)), the model allows me to characterize the welfare impacts of increasing enforcement of AA quotas for firms, workers, and government. I then provide more context by discussing the implications for each group, and conditions under which the policy can be welfare enhancing.

1.6.1 Welfare Framework

Model Setup. Consider a simple one-period model with populations of firms and people with disabilities of mass unity. Furthermore, consider the decision to comply with the AA

quota regulation for a representative firm. The firm derives disutility from hiring a worker with disabilities, denoted by $\sigma \sim F(\cdot)$, with pdf $f(\cdot)$, which can also capture taste for non-compliance. If σ is small, the firm fully complies with the AA quota and obtains profit or surplus specific to people with disabilities $MRPL_d - w - \sigma$, in which $MRPL_d$ is the marginal revenue product generated by the employee with disabilities and w is the wage paid to the same employee.[40] If σ is sufficiently large, the firm initially does not comply with the regulation and incurs in the risk of detection by the government. With probability $p(\sigma)$, in which $p'(\sigma) > 0$, the government detects non-compliance. In this case, the firm faces the choice between complying with the regulation and hiring a person with disabilities, $M\tilde{R}PL_d - w - \sigma$, or being sanctioned for delinquency and getting fined by an amount of F.[41] With probability $1 - p(\sigma)$, there is no detection and firm profit does not change.

The firm's choices can be translated into individual payoffs for people with disabilities. When a person with disabilities is employed, she enjoys utility $u(w-\tau)$, in which w represents the wage she earns, τ is the lump-sum tax she pays to the government while employed, and the utility function u is increasing and concave. In case of unemployment, she claims welfare benefits b from the government and gets utility $v(b)$, in which the utility function v is also increasing and concave. Figures 1.G.1 and 1.G.2, Appendix G, illustrate the choices.

In addition, let $\sigma^C = MRPL_d - w + p(\sigma^c)F$ denote the threshold value of σ indicating indifference between compliance and delinquency. Firms with $\sigma \leq \sigma^C$ initially comply with the regulation while those with $\sigma > \sigma^C$ are delinquent. Let $\sigma^F = M\tilde{R}PL_d + F - w$ denote the threshold value indicating indifference between compliance and fine payment conditional on detection. Firms with $\sigma \leq \sigma^F$ prefer to hire a worker with disabilities after detection while those with $\sigma > \sigma^F$ pay the fine. Lastly, assume that $\sigma^F > \sigma^C$ holds.

[40] For simplicity, I take wages as given. This is consistent with institutional constraints, such as minimum wages, wage floors, and on-the-job wage rigidity.

[41] There is a small change in notation in the marginal revenue product of labor to incorporate potential additional disutility from compliance after detection, such as follow-up checks from the government, bureaucratic hassle, etc., or, more broadly, potential costs to accommodate a worker with disabilities.

The government does not observe σ. Instead, the government observes an imperfect enforcement level, σ^*, and attempts to increase employment of people with disabilities by increasing enforcement (e.g., labor inspections), affecting the probability of detection, $p(\sigma^*)$.[42] I next define the surplus levels for firms and people with disabilities and outline the government budget.

Firms. Given w, the producer surplus associated with hiring people with disabilities can be expressed as:

$$\Pi(\sigma^*) = \int_0^{\sigma^c} [MRPL_d - w - \sigma]\, f(\sigma)\, d(\sigma) + \int_{\sigma^c}^{\sigma^F} p(\sigma^*)[M\tilde{R}PL_d - w - \sigma]\, f(\sigma)\, d(\sigma) + \int_{\sigma^F}^{1} p(\sigma^*)[-F]\, f(\sigma)\, d(\sigma) + \int_{\sigma^c}^{1} (1 - p(\sigma^*))[0]\, f(\sigma)\, d(\sigma), \tag{1.8}$$

in which the right-hand side sums up the surpluses under different scenarios: when there is compliance from the beginning (first term); when the firm complies with the AA quota regulation after detection (second term); when the firm decides to pay a fine after detection (third term); and when both non-compliance and non-detection occur (fourth term).

People with Disabilities. Given w, τ and b, the total welfare of people with disabilities can be written as:

$$V(\sigma^*) = \int_0^{\sigma^c} u(w - \tau)\, f(\sigma)\, d(\sigma) + \int_{\sigma^c}^{\sigma^F} p(\sigma^*) u(w - \tau)\, f(\sigma)\, d(\sigma) + \int_{\sigma^F}^{1} p(\sigma^*) v(b)\, f(\sigma)\, d(\sigma) + \int_{\sigma^c}^{1} (1 - p(\sigma^*)) v(b)\, f(\sigma)\, d(\sigma), \tag{1.9}$$

[42] Another policy lever available to a government as part of an enforcement scheme is fines. I assume that the government takes the fine F as given. This assumption is consistent with little variation in the value of fines over time.

in which the right-hand side sums up the utilities of different profiles of people with disabilities integrated over the distribution of σ: employed due to initial compliance (first term); employed due to compliance after detection (second term); recipients of welfare benefits in case of fine payment after detection (third term); and recipients of welfare benefits due to both non-compliance and non-detection (fourth term).

Government. The total revenues raised by the government can be written as:

$$\begin{aligned} R(\sigma^*) = \tau \int_0^{\sigma^c} f(\sigma)\, d(\sigma) + \tau \int_{\sigma^c}^{\sigma^F} p(\sigma^*)\, f(\sigma)\, d(\sigma) + F \int_{\sigma^F}^{1} p(\sigma^*)\, f(\sigma)\, d(\sigma) \\ -b \int_{\sigma^F}^{1} p(\sigma^*)\, f(\sigma)\, d(\sigma) - b \int_{\sigma^c}^{1} (1 - p(\sigma^*))\, f(\sigma)\, d(\sigma), \end{aligned} \tag{1.10}$$

in which the right-hand side accounts for the taxes raised from employment, the fines raised after detection, and the welfare benefits paid to the unemployed. The government also incurs the cost of enforcement, defined as $C(\sigma^*)$.

Welfare Effects of Enforcing AA Quotas. I assume that the government also uses a higher enforcement level to raise additional fiscal revenues $R(\sigma^*)$ (e.g., to increase the provision of public goods). The government sets enforcement level σ^* to maximize the following social welfare function:

$$W(\sigma^*) = \Pi(\sigma^*) + V(\sigma^*) + R(\sigma^*) - C(\sigma^*). \tag{1.11}$$

Under standard regularity conditions, Appendix G shows that the welfare effect from raising enforcement level σ^* can be written as:

$$W'(\sigma^*) = \underbrace{M_C[M\tilde{R}PL_d - w]}_{\substack{\text{marginal firm}\\ \text{cost}}} + \underbrace{M_C[u(w-\tau) - v(b)]}_{\substack{\text{marginal welfare}\\ \text{benefit for PwD}}} + \underbrace{M_C[\tau + b]}_{\substack{\text{marginal revenue}\\ \text{benefit}}} - \underbrace{C'}_{\substack{\text{marginal cost of}\\ \text{enforcement}}}, \tag{1.12}$$

in which $M_C \equiv \int_{\sigma^c}^{\sigma^F} \frac{\partial p(\sigma^*)}{\partial \sigma^*} f(\sigma)\, d(\sigma)$ captures the mechanical increase in employment for people with disabilities due to increased enforcement. Equation (1.12) illustrates four key objects that govern the effects of increasing enforcement on social welfare. First, the change in producer surplus, which depends on the wedge between marginal revenue products of people with disabilities, $M\tilde{R}PL_d$, and their wages, w. Second, the change in surplus for people with disabilities from working, $u(w-\tau)$, relative to their reservation utility from receiving welfare benefit from the government, $v(b)$. Third, when the government has revenue-maximizing reasons, the extra revenues coming from income tax τ and welfare benefits savings b due to higher employment. Fourth, the marginal cost of enforcement (e.g., administrative costs). I note that, since the empirical results show that people without disabilities are unaffected, there is no welfare change for them. By contrast, raising fiscal revenues with increased employment to provide public goods can increase welfare.[43]

This framework abstracts from other factors that can contribute to the aggregate welfare. For instance, I do not account for crowd out from public to private health care (Paim et al. (2011)) or better health conditions (Sullivan and Von Wachter (2009)) and lower criminal involvement (Deshpande and Mueller-Smith (2022)) due to increased employment as potential social benefits. I also do not consider welfare losses to workers without disabilities, distortions in the production function, or moral hazard as potential social costs. Because the

[43]Fines represent a lump-sum transfer from firms to the government. Wage adjustments to incumbent workers, under a linear utility function, also represent a transfer from people with disabilities to firms. In theory, the marginal producer cost can account for potential fixed costs to accommodate workers with disabilities. As discussed in Section 1.5.2, both the administrative and survey data point to little evidence of fixed costs. Therefore, there is no change in the producer surplus due to fixed costs in the welfare evaluation.

reduced-form analysis shows little evidence that enforcement of AA quotas affected workers without disabilities or firm outcomes, the social benefits are underestimated. Another limitation is that the social welfare function assumes that people with disabilities have the same social welfare weights as firms. The fact that quota policies are prevalent across the world suggests that governments put more value on the welfare of people with disabilities (Revillard (2022)). Calculating the social welfare weights is beyond the scope of this paper and an important area for future work.

1.6.2 Implications and Discussion

Firms. The firm-level analysis in Section 1.5.1 finds no evidence that firms under AA quotas are more likely to exit the formal sector, decrease wages for workers without disabilities, or bunch below the regulation threshold. These evidence suggest that firms do not experience lower profits. To further support the lack of changes in profits without additional data on firm outcomes, I propose a simple discrete choice framework that provides a tractable closed-form solution to estimate the marginal revenue product of new hires with disabilities with the available data. Another motivation for this exercise is the lack of evidence of estimates of marginal revenue product of workers with disabilities in the literature.

Conditional on inspection and detection, non-compliant firms have two choices available: they can choose either to pay fines, or abide by the AA quota regulation by hiring additional employees up to the requirement. If it chooses to pay fines, firm i gets utility $U_{i,f} = -F_i$, in which F is the amount of fines. If the firm chooses to hire new workers to comply with the regulation after an inspection, it obtains utility $U_{i,c} = \gamma(MRPL_d - w_i) - \epsilon_i$, in which γ is the number of new hires necessary to become compliant. This number is the gap between the potential number of jobs reserved by the quota regulation, which is calculated from the firm size, and the actual number of employees with disabilities. $MRPL_d$ is the marginal revenue

product of people with disabilities, w_i is the wages paid to disabled employees, and ϵ_i is a distaste parameter for hiring workers with disabilities such that $\epsilon_i \sim \mathcal{N}(0, \Sigma_i^2)$.

Define the probability that a firm chooses compliance after inspection as $P_{i,c} = \Pr(U_{i,c} \geq U_{i,f})$. It can be rewritten as:

$$P_{i,c} = \Phi(\beta_0 + \beta_1 w_i + \beta_2 F_i), \tag{1.13}$$

in which $\beta_0 \equiv \frac{\gamma MRPL_d}{\Sigma_i}$; $\beta_1 \equiv -\frac{\gamma}{\Sigma_i}$; and $\beta_2 \equiv \frac{1}{\Sigma_i}$. Equation (1.13) is a probit model that can be estimated via maximum likelihood. An alternative functional form for the distaste parameter following a logistic distribution leads to similar conclusions. The marginal revenue product can be expressed as a function of estimates of β_0 and β_1 because $\hat{MRPL_d} = -\frac{\hat{\beta_0}}{\hat{\beta_1}}$.[44] Table 1.8 indicates that estimates of the ratio between estimated marginal revenue product of labor and average wages are around 1.10-1.26. At best, these estimates reject that marginal revenue products of people with disabilities fall below their wages.

People with Disabilities. I next examine the changes in surplus for people with disabilities. The job surplus depends on workers' value of being employed under wage contract w, $u(w)$, relative to their unknown value of the outside option, $v(b)$. In competitive labor markets, workers get paid for their marginal product, implying a zero surplus from employment relationships. Nonetheless, the high involuntary unemployment rates (Column (2) of Table 1.A.1, Appendix A) and the presence of binding minimum wages and union wage floors (Engbom and Moser (Forthcoming)) are inconsistent with perfectly competitive labor

[44] I use the fine schedule established in Ordinance 1,199/2003 from the Ministry of Labor to calculate the amount of fines that newly compliant firms would have paid if they choose non-compliance. For the firms that pay fines rather than hire disabled workers, the predicted value of wages is drawn from the distribution of wages of the new hires without disabilities since firms are not allowed to offer distinct wages to new workers from the same position and tenure. In addition, standard errors are calculated using delta method.

markets.[45] Rents from employment relationships are thus likely to be positive for workers with disabilities.

If the surplus from employment is positive for people with disabilities, which labor market frictions can explain that many firms do not voluntarily hire these workers without quotas? Minimum wages and union wage floors are plausible candidates. Figure 1.G.3, Appendix G, indicates that the minimum wage is more binding among workers with disabilities than among workers without disabilities. This discrepancy could hint, for instance, that firms do not hire people with disabilities because their marginal product is below the minimum wage. However, this hypothesis is inconsistent with previous empirical findings.

Instead, the findings are very consistent with discrimination at the hiring level. A large body of literature has documented employer discrimination against people with disabilities using observational or experimental evidence (Baldwin and Johnson (1994), Baldwin and Johnson (2006), Baert (2016), Ameri et al. (2018)). In fictitious audit experiments randomizing information on disabilities that do not limit productivity in administrative positions, Ameri et al. (2018) find that candidates with disabilities receive way fewer callbacks for interviews than similar candidates without disabilities in the United States. The vignette experiment in the survey with HR personnel reveals a similar pattern in Brazil: respondents are significantly less likely to express interest in hiring a candidate with disabilities.[46] Conceptually, discrimination explains why employers do not hire people with disabilities even when their marginal products are equal to or above their wages. Under these circumstances,

[45] In 2010, the unemployment rates for people reporting to live with some and severe difficulty are around 10 and 14.5 percent, larger than the unemployment rate of 6.4 percent for those without disabilities.

[46] In the survey, respondents are assigned to a vignette describing a big fictitious consultancy that would like to hire someone for an entry-level job to do routine clerical and organizational tasks. I introduce a 22-year-old man who finished high school, has flexibility, proactivity, and good organization skills, and interacts well with people. For some respondents, I randomize the information that the man has a bilateral hearing loss. I then ask respondents to rate, on a four-point scale, in which 1 is "unlikely" and 4 is "very likely", how likely they think that the company would be interested in hiring him and that he would accept the job. Table 1.F.1, Appendix F, reports the findings.

hiring quotas can increase employment for people with disabilities without displacing workers without disabilities or shutting firms down.

Without taking a stance on the sources of labor market frictions, I translate the surplus gain of enforcing quotas for people with disabilities into a money metric gain from employment. I err on the side of caution and make additional restrictive assumptions. First, I restrict the gain to occur only in the first year of an employment spell. Second, the marginal welfare benefit is assessed assuming a linear utility function. It implies using the income flow of switching from welfare benefits to employment as the welfare gain. Third, I also account for the opportunity cost of a full-time job due to lost leisure. Following Mas and Pallais (2019), I assume a value of non-work relative to the wages of 0.58. Considering the reduced-form estimates, the (lower bound) estimated marginal welfare benefit of each inspection is net positive: about 372.65 Brazilian *reais*. A detailed description of the calculations can be found in Appendix G.

Government. On the fiscal side, the relevant objects are the marginal cost of enforcement and the marginal fiscal revenue benefit. While there are no data available on detailed spending on each inspection, the average cost of inspections is an upper bound for the marginal cost of enforcement. The data on total expenditures on enforcement capacity reveal that the average cost of a labor inspection is around 99 Brazilian *reais*. Compared to the marginal revenue benefit of 668.34 Brazilian *reais* from each inspection, my results demonstrate unambiguous positive impacts. In addition, disregarding the marginal revenue benefit preserves the conclusion of net positive impacts since the marginal welfare benefit for people with disabilities is almost four times the average cost of enforcement.

Additional Discussion. These results together suggest that, in the presence of imperfect competition and frictions in the labor market, firms may be inefficiently small in equilibrium, and enforcing modest affirmative action hiring quotas redistributes jobs to people with dis-

abilities and induces aggregate welfare gains. I note that, although the data reject that the marginal revenue products of people with disabilities fall below their wages in this context, an important caveat is that I am not able to directly test whether workers with and without disabilities have the same marginal product. Recent literature has found substantial average wage markdowns, with the ratio of workers' marginal revenue product of labor to their wage ranging from 1.40 to 2.13 in other contexts (e.g., Amodio and De Roux (2021), Berger et al. (2022), Yeh et al. (2022)). In Brazil, Felix (2021) finds a ratio of 2 during the 1990s.[47] Comparing my estimates to what other papers have documented, it might be tempting to conclude that people with disabilities are less productive than those without disabilities. The wide range of estimates in the literature, however, suggests great sensitivity to the context. To my knowledge, no recent research has investigated the differences in wage markdowns for workers with and without disabilities, let alone within the same context, and this is an important area for future work.

1.7 Conclusion

This paper provides a novel and comprehensive assessment of the redistributive implications of enforcing affirmative action quotas for one of the most disadvantaged yet understudied groups: people with disabilities. Exploiting the timing of a reform in Brazil that raised enforcement of a new affirmative action hiring quota regulation, my market-level analysis indicates that people with disabilities living in local labor markets more exposed to the reform experience larger increases in formal sector employment and earnings than those in less exposed markets. Leveraging variation in enforcement through inspections across firms, I document that the increase in employment induced by hiring quotas does not come at a discernible cost to other workers in terms of wages or employment or to firms, and raises

[47]Because information on workers with disabilities in the formal sector only started to be collected in 2003, it is not possible to compute average wage markdowns across disability status using Felix (2021)'s approach, which focuses on the pre-liberalization (the 1990s) period.

fiscal revenues. My results indicate that, in labor markets under imperfect competition, enforcing modest hiring quotas generates aggregate efficiency gains and, therefore, constitutes a promising pathway for countering the low levels of employment for the disadvantaged.

This conclusion points to several new directions for future work. First, there might be strong complementarities between quotas and wage subsidies, amplifying the effects of these regulations. Second, aggressive hiring quota regulations and rigid enforcement structures might create different consequences that I do not capture here (e.g., Peck (2017)). Third, I do not exploit the optimal share of mandated employment that maximizes the efficiency gains of quotas in settings with imperfect competition. Each direction invites more research to be done, since improving labor market prospects for the disadvantaged remains a policy priority in many countries.

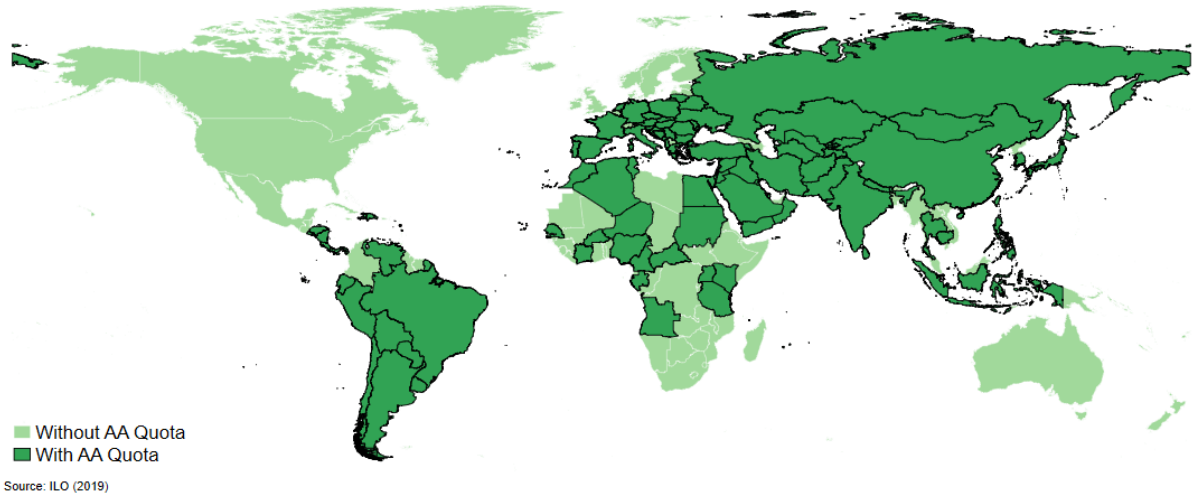
This paper offers several other policy-relevant findings for other contexts. My findings reject that the marginal product of workers with disabilities is lower than their wages. As many businesses struggle to find and retain workers during the post-Covid era, reducing obstacles to employ people with disabilities is a promising approach.[48] In addition, the analysis points to strong complementarities between quota regulations and enforcement capacity. In light of extensive evidence that firms do not actively meet the minimum percentage of quotas, including federal contractors in the US who are required to set a goal of having 7 percent of their workforce composed of persons with disabilities (see Amano-Patiño et al. (2022) for recent evidence for other minority groups), my results suggest that investing in compliance seems attractive from a redistributive perspective. On the other hand, evidence that incumbent workers with disabilities are negatively affected also suggests that firms have some ability to redistribute the costs across and within workers, contributing to the rise of workplace inequality.

[48]Recent articles have highlighted that employment has soared among people with disabilities since Covid: https://www.nytimes.com/2022/10/25/business/economy/labor-disabilities.html and https://www.bloomberg.com/news/articles/2022-10-03/disabled-us-workers-see-highest-ever-employment-figures-from-remote-work.

Disability hiring quotas might also generate other positive externalities beyond the job surplus. For instance, firms fostering an inclusive environment might benefit from higher diversity in teams of workers and complementarities between workers in production. Hiring quotas can promote additional social benefits, such as better health conditions and lower mortality rates due to employment, access to better health services due to crowd out from public to private care, and lower criminal involvement. These possible social benefits are left for future work. Therefore, this paper should be viewed as an initial step toward characterizing the social benefits of hiring quotas.

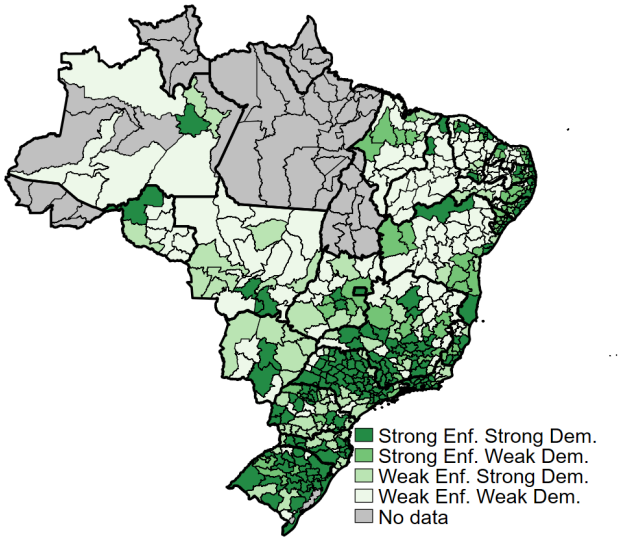
Figures and Tables

Figure 1.1: Countries with Hiring Quotas for People with Disabilities



Note: This map illustrates the prevalence of affirmative action hiring quotas for people with disabilities, which are implemented in more than 100 countries. The United States require federal contractors to set a hiring goal of 7 percent of their workforce with people with disabilities. Source: ILO (2019).

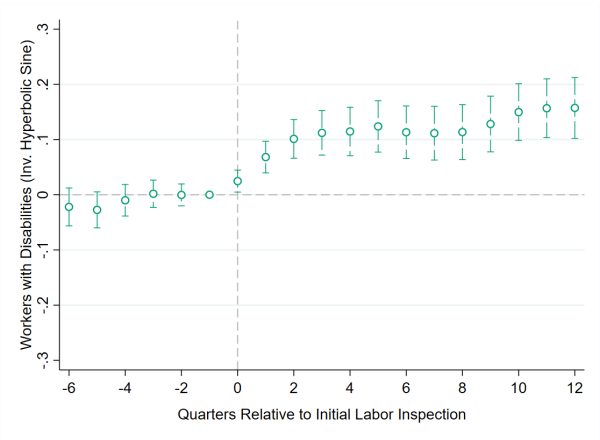
Figure 1.2: Cross-Market Variation: Interaction between Potential Demand & Enforcement



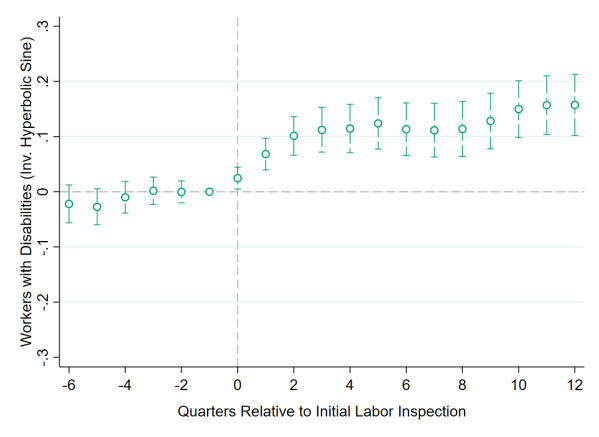
Note: Graph illustrates the geographic variation in interaction between measures of potential demand and enforcement capacity across local labor markets (micro regions) in Brazil. Potential demand is defined as the total number of jobs in the private sector to people with disabilities if there is perfect compliance with the AA quota in 1998, which are calculated from the distribution of firm size, divided by the total number of people with disabilities in 2000. Enforcement capacity is proxied by the minimum distance to the nearest labor office belonging to the same state within each micro region. Sources: 1998 RAIS, 2000 Demographic Census data, and labor offices' addresses.

Figure 1.3: Effects of AA Quota on Employment

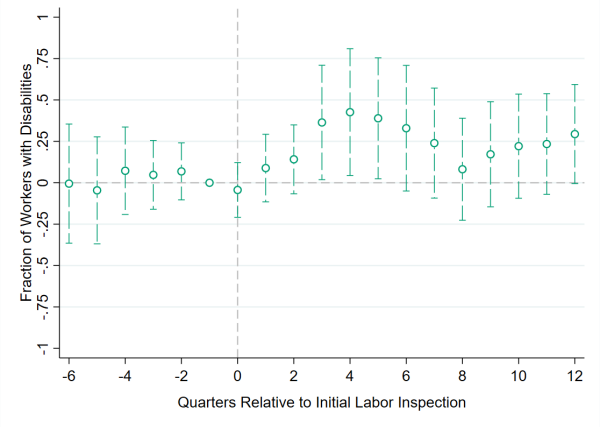
(a) Workers with Disabilities (Inverse Hyperbolic Sine)



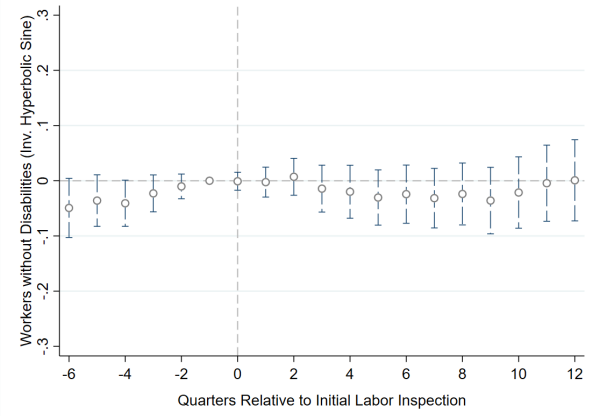
(b) Have at Least One Worker with Disabilities



(c) Share of Workers with Disabilities



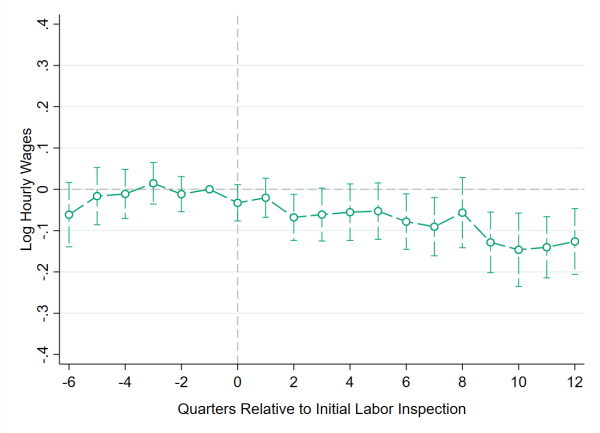
(d) Workers without Disabilities (Inverse Hyperbolic Sine)



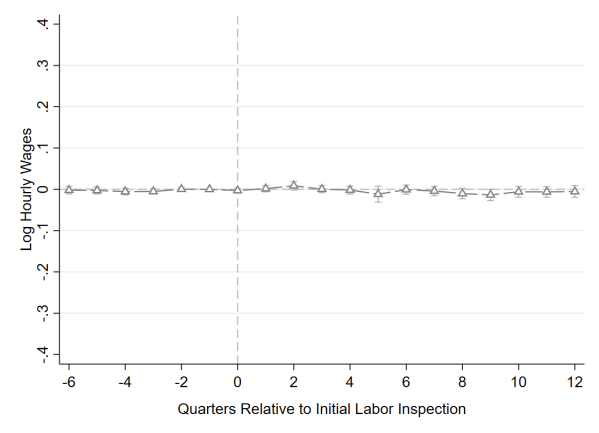
Note: This figure reports point estimates of the quarterly effects of the AA quota on employment considering the intensive margin (inverse hyperbolic sine transformation of workers with disabilities), the extensive margin (indicator for having at least one worker with disabilities), the share of workers with disabilities (defined as total workers with disabilities divided by the total number of workers), and total workers without disabilities (its inverse hyperbolic sine transformation) as outcome variables. The omitted category is the quarter before inspection. More details can be found in Table 1.3.

Figure 1.4: Effects of AA Quota on Wages

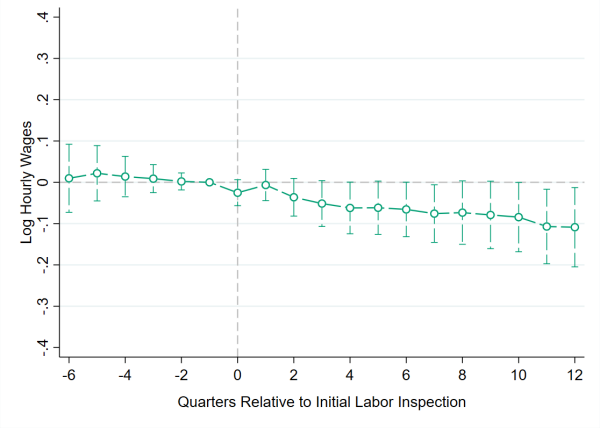
(a) Workers with Disabilities



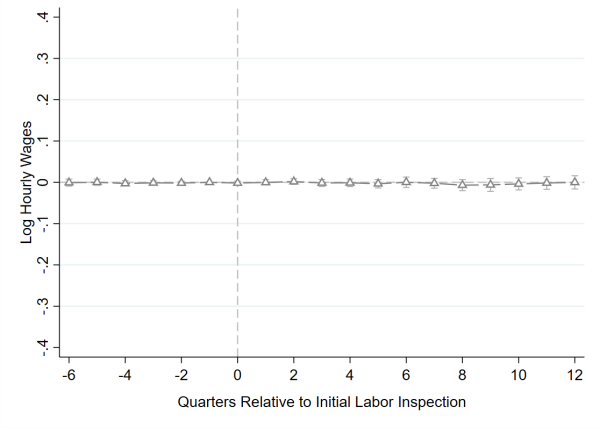
(b) Workers without Disabilities



(c) Incumbent Workers with Disabilities



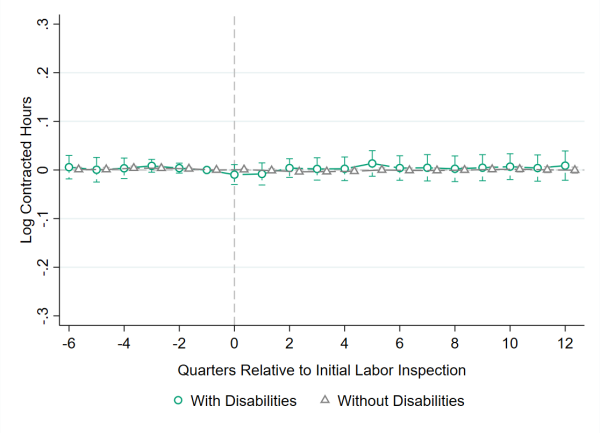
(d) Incumbent Workers without Disabilities



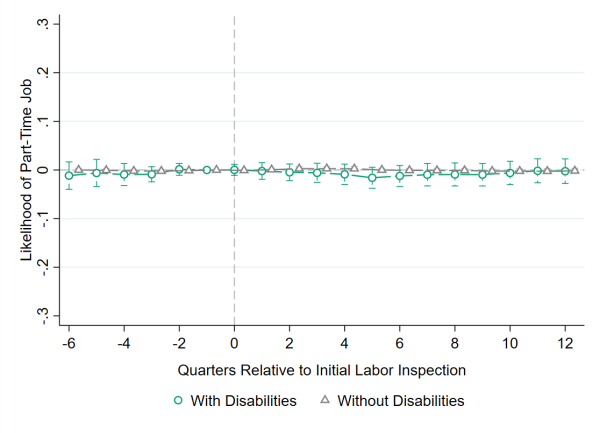
Note: This figure reports point estimates of the quarterly effects of the AA quota on log hourly wages. I consider separately workers with and without disabilities. Figures 1.4c and 1.4d refer to incumbent workers with and without disabilities. The omitted category is the quarter before inspection. More details can be found in Table 1.5.

Figure 1.5: Effects of AA Quota on Other Workers' Outcomes

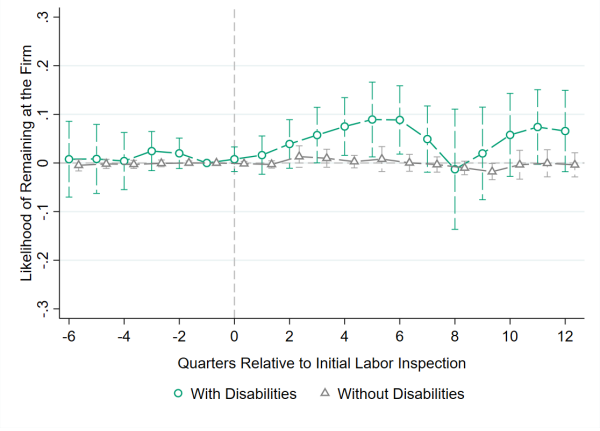
(a) Log Number of Hours



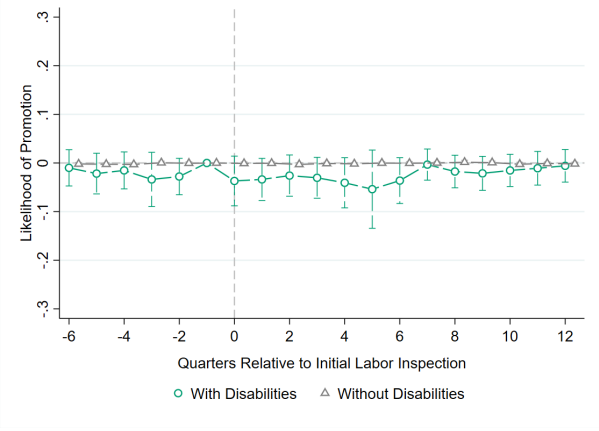
(b) Part-Time Employment



(c) Turnover



(d) Promotion



Note: This figure reports point estimates of the quarterly effects of the AA quota on log number of contracted hours, likelihood of part-time employment, likelihood of staying at the firm, and likelihood of promotion for workers with and without disabilities. The omitted category is the quarter before inspection. More details can be found in Table 1.6.

Table 1.1: Aggregate Analysis: Impacts on Employment

	(1)	(2)	(3)	(4)
	With Disabilities		Without Disabilities	
	Formal Employment	Informal Employment	Formal Employment	Informal Employment
(Strong Demand & Strong Enforcement) × Reform	0.011***	-0.006	0.007	0.002
	(0.003)	(0.003)	(0.004)	(0.004)
(Strong Demand & Weak Enforcement) × Reform	0.003	-0.007	0.004	0.003
	(0.005)	(0.005)	(0.004)	(0.003)
(Weak Demand & Strong Enforcement) × Reform	0.003	-0.006	0.003	-0.003
	(0.003)	(0.004)	(0.003)	(0.004)
Sample Size	1,018	1,018	1,018	1,018
Mean Dep. Var (in 2000)	0.116	0.146	0.191	0.204
Mean Dep. Var	0.143	0.142	0.230	0.182

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of the reform on formal and informal employment rates for people with (Columns (1) and (2)) and without disabilities (Columns (3) and (4)). All columns refer to Equation (1.2). Means of dependent variables are computed from all micro regions in 2000. Standard errors are clustered at the state level.

Table 1.2: Aggregate Analysis: Impacts on Earnings

	(1)	(2)	(3)	(4)
	With Disabilities		Without Disabilities	
	Work Income	Non-Work Income	Work Income	Non-Work Income
(Strong Demand & Strong Enforcement) × Reform	34.55***	-2.08	18.37	-1.60
	(10.71)	(4.60)	(11.79)	(2.85)
(Strong Demand & Weak Enforcement) × Reform	18.16	-2.74	9.67	-2.35
	(23.42)	(6.59)	(10.29)	(3.23)
(Weak Demand & Strong Enforcement) × Reform	-4.08	4.19	6.04	-4.67
	(9.32)	(5.38)	(7.50)	(2.98)
Sample Size	1,018	1,018	1,018	1,018
Mean Dep. Var (in 2000)	363.78	125.33	641.41	74.22
Mean Dep. Var	398.64	149.07	691.32	85.47

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of the reform on earnings, measured as work and non-work income, for people with (Columns (1) and (2)) and without disabilities (Columns (3) and (4)). All columns refer to Equation (1.2). Means of dependent variables are computed from all micro regions in 2000. Standard errors are clustered at the state level.

Table 1.3: Effects of AA Quota on Employment Outcomes

	(1) (IHS) Workers with Disabilities	(2) Have at Least One Worker with Disabilities	(3) Share of Workers with Disabilities	(4) (IHS) Workers without Disabilities
Panel A: Dynamic Impacts				
Immediate ($k = 0$)	0.025**	0.019**	-0.044	-0.001
	(0.010)	(0.008)	(0.084)	(0.008)
Short Run ($k = 6$)	0.113***	0.065***	0.329*	-0.024
	(0.024)	(0.016)	(0.193)	(0.027)
Long Run ($k = 12$)	0.157***	0.078***	0.294*	0.001
	(0.028)	(0.018)	(0.153)	(0.037)
Panel B: Aggregate Impacts				
Post × Quota	0.124***	0.077***	0.192*	0.018
	(0.018)	(0.012)	(0.115)	(0.026)
Sample Size	60,000	60,000	60,000	60,000
Firm and Year FEs	✓	✓	✓	✓
State and Industry Trends	✓	✓	✓	✓
# Firms	3,000	3,000	3,000	3,000
Mean Dep. Var (Control)	0.104	0.084	0.394	4.869

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the firm-level effects of the AA quota on several employment outcomes: inverse hyperbolic sine transformation (IHS) of total number of workers with disabilities, indicator for having at least one worker with disabilities, share of workers with disabilities relative to total number of workers, and IHS of total number of workers without disabilities. All columns refer to Equation (2.1). Means of dependent variables are computed from the control group in the quarterly window [-6, -1] before inspection. Standard errors are clustered at the firm level.

Table 1.4: Effects of AA Quota on Firm Outcomes

	(1) (IHS) Average Wages	(2) Average Wages	(3) Exit
Panel A: Dynamic Impacts			
Immediate ($k = 0$)	-0.0002	1.5002	0.0001
	(0.0045)	(13.155)	(0.0004)
Short Run ($k = 6$)	-0.0151	16.264	-0.0117
	(0.0161)	(20.801)	(0.0086)
Long Run ($k = 12$)	-0.0517	9,012	-0.0058
	(0.0333)	(24.928)	(0.0116)
Panel B: Aggregate Impacts			
Post × Quota	-0.009	14.227	-0.008
	(0.011)	(11.690)	(0.006)
Sample Size	60, 000	60,000	70,300
Firm and Year FEs	✓	✓	✓
State and Industry Trends	✓	✓	✓
Mean Dep. Var (Control)	7.434	1,016.30	0

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the firm-level effects of the AA quota on several firm outcomes: inverse hyperbolic sine transformation (IHS) of average wages of workers without disabilities, average wages of workers without disabilities, and firm exit. All columns refer to Equation (2.1). Means of dependent variables are computed from the control group in the quarterly window [-6, -1] before inspection. Standard errors are clustered at the firm level.

Table 1.5: Effects of AA Quota on Wages

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dynamic Impacts (Disabled) (N = 33,897)						
Immediate ($k = 0$)	-0.023	-0.032	-0.029	-0.033	-0.025	0132
	(0.023)	(0.023)	(0.023)	(0.022)	(0.016)	(0.139)
Short Run ($k = 6$)	-0.097***	-0.076**	-0.094***	-0.078**	-0.066**	0.064
	(0.038)	(0.034)	(0.037)	(0.034)	(0.034)	(0.120)
Long Run ($k = 12$)	-0.123***	-0.125***	-0.128***	-0.126***	-0.109***	-0.031
	(0.043)	(0.042)	(0.042)	(0.041)	(0.049)	(0.104)
Panel B: Aggregate Impacts (Disabled) (N = 33,897)						
Post × Quota	-0.062**	-0.048*	-0.062**	-0.061**	-0.056*	0.002
	(0.029)	(0.029)	(0.029)	(0.029)	(0.032)	(0.074)
Panel C: Dynamic Impacts (Non-Disabled) (N = 7,247,768)						
Immediate ($k = 0$)	-0.002	-0.003	-0.002	-0.002	-0.002	0.007
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.009)
Short Run ($k = 6$)	0.002	0.001	-0.001	0.002	0.001	-0.004
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.012)
Long Run ($k = 12$)	-0.007	-0.006	-0.007	-0.007	-0.000	-0.013
	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.016)
Panel D: Aggregate Impacts (Non-Disabled) (N = 7,247,768)						
Post × Quota	-0.002	0.000	0.002	-0.001	-0.001	-0.006
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)
Firm and Quarter FEs	✓	✓	✓	✓	✓	✓
State and Industry Trends	✓	✓	✓	✓	✓	✓
Individual Controls		✓		✓		✓
Occupation FE			✓	✓		✓
Worker FE					✓	
Sample						New Hires

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the worker-level effects of the AA quota on log hourly wages for workers with (Panels A and B) and without (Panels C and D) disabilities. Panels A and C refer to event-study difference-in-differences from Equation (2.2), while Panels B and D report aggregate difference-in-differences estimates from Equation (1.6). Column (1) refers to a model with firm and quarter fixed effects and state and industry trends. Column (2) adds individual controls (gender, race, educational level fixed effects, age, and square age). Column (3) includes occupation fixed effects. Column (4) refers to the preferred specification described in Equation (1.6). Column (5) includes worker, firm and quarter fixed effects, along with state and industry trends. Column (6) has the same specification as Column (4) with the sample restricted to new hires. Standard errors are clustered at the firm level.

Table 1.6: Effects of AA Quota on Workers' Outcomes

	(1) Log Hours	(2) Part-Time	(3) Turnover	(4) Promotion
Panel A: Dynamic Impacts (Disabled) (N = 33,897)				
Immediate ($k = 0$)	-0.010	0.000	0.008	-0.037
	(0.010)	(0.006)	(0.013)	(0.026)
Short Run ($k = 6$)	0.004	-0.012	0.089**	-0.036
	(0.013)	(0.011)	(0.036)	(0.024)
Long Run ($k = 12$)	0.009	-0.003	0.066	-0.006
	(0.015)	(0.013)	(0.043)	(0.018)
Panel B: Aggregate Impacts (Disabled) (N = 33,897)				
Post × Quota	-0.001	-0.001	0.038	-0.014*
	(0.012)	(0.011)	(0.026)	(0.008)
Panel C: Dynamic Impacts (Non-Disabled) (N = 7,247,768)				
Immediate ($k = 0$)	0.001	-0.001	-0.002	-0.008
	(0.001)	(0.001)	(0.002)	(0.002)
Short Run ($k = 6$)	-0.001	-0.001	0.000	-0.001
	(0.003)	(0.002)	(0.009)	(0.002)
Long Run ($k = 12$)	-0.000	-0.002	-0.004	-0.002
	(0.003)	(0.003)	(0.127)	(0.002)
Panel D: Aggregate Impacts (Non-Disabled) (N = 7,247,768)				
Post × Quota	-0.002	0.004	0.001	0.000
	(0.002)	(0.002)	(0.006)	(0.000)
Firm and Quarter FEs	✓	✓	✓	✓
State and Industry Trends	✓	✓	✓	✓
Individual Controls	✓	✓		✓
Occupation FE	✓	✓		✓
Worker FE			✓	

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the worker-level effects of the AA quota on log contracted hours, likelihood of having a part-time job, likelihood of staying at the firm, and likelihood of promotion. Results are reported for workers with (Panels A and B) and without disabilities (Panels C and D) separately. Panels A and C refer to the preferred specification described in Equation (1.6), whereas Panels B and D refer to Equation (2.2). The only exception is Column (3): the specification for the likelihood of staying at the firm considers worker, firm and quarter fixed effects and state- and industry-specific trends. Standard errors are clustered at the firm level.

Table 1.7: Heterogeneity: Effects of AA Quota on Wages

	(1)	(2)	(3)	(4)
	Educational Level		**On-the-Job Interactions**	
Post × Quota	-0.073*	-0.031	-0.090*	-0.048
	(0.039)	(0.075)	(0.049)	(0.045)
Sample Size	31,177	2,721	15,230	17,958
Sample Restriction	No College	College	Weak Interactions	Strong Interactions
Firm and Quarter FEs	✓	✓	✓	✓
State and Industry Trends	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓
Occupation FE	✓	✓	✓	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the heterogeneous effects of the AA quota on log hourly wages for workers with disabilities. Columns (1) and (2) restrict the samples to workers without and with a college degree. Columns (3) and (4) refer to samples of workers with weak and strong interactions with co-workers, classified as below and above the median of communicating with supervisors and peers. This measure comes from occupations "providing information to supervisors, co-workers, and subordinates by telephone, in written form, e-mail, or in person" as listed in the O*NET database. All columns refer to the preferred specification described in Equation (1.6). Standard errors are clustered at the firm level.

Table 1.8: Discrete Choice Estimates

	(1)	(2)
$\hat{\beta_0}$	0.5544***	1.1726***
	(0.0459)	(0.0888)
$\hat{\beta_1}$	-0.0002***	-0.0006***
	(0.0000)	(0.0000)
$\widehat{MRPL_d}$	2,338.063	2,044.769
	(158.85)	(108.61)
Mean Wages	1,854.74	1,854.74
$\widehat{MRPL_d}/\bar{w}$	1.2606	1.1025
[Lower Bound; Upper Bound]	[1.0927; 1.4285]	[0.9877; 1.2172]
Model	Probit	Logit

Note: This table reports the point estimates after estimating Equation (1.13) via maximum likelihood using normal (Column (1)) and logistic (Column (2)) distributions. $\widehat{MRPL_d}/\bar{w}$ is defined as the ratio between firms' estimated monthly marginal revenue product of labor and average wages. Lower and upper bounds with 95-percent confidence intervals are calculated. Standard errors are computed using the delta method.

Appendices

A Institutional Context and Data

Table 1.A.1: Disability Gaps in the Labor Market

	(1) Economically Active	(2) Unemployment	(3) Informal Employment	(4) (IHS) Work Income
Severe Difficulty	-0.182***	0.057***	0.034***	-0.195***
	(0.008)	(0.003)	(0.002)	(0.010)
Some Difficulty	-0.026***	0.022***	0.018***	-0.099***
	(0.003)	(0.001)	(0.001)	(0.006)
Sample Size	6,406,401	4,752,620	4,372,587	4,372,587
Sample Restriction	-	Econ. Active	Worked	Worked
Mean (Without Disabilities)	0.794	0.064	0.180	7.458
Mean (Severe Difficulty)	0.567	0.145	0.250	7.070
Mean (Some Difficulty)	0.740	0.099	0.215	7.276
Individual Controls	✓	✓	✓	✓
Occup. & Sector Controls		✓	✓	✓

Note: This table displays gaps in labor market outcomes across disabilities using the 2010 Census. The samples include working-age individuals aged 25 to 54. I create indicator variables for whether individuals report having severe and some difficulties in one or more of the following activities: seeing, hearing, walking or climbing stairs. I regress labor market outcomes on having severe and some difficulties. The omitted disability group is no disabilities (no difficulties). All specifications include potential experience, potential experience squared, dummies for educational categories and rural areas, and municipality fixed effects. Columns (2) to (4) additionally include occupation and economic sector fixed effects. The dependent variables are indicators for economically active individuals, unemployed in the last week of July of 2010 and employment in the informal sector, and the inverse hyperbolic sine transformation of work income. In Column (2), the sample is further restricted to economically active individuals. In Columns (3) and (4), the sample refers to individuals who have worked in the last week of July of 2010. Means of dependent variables across disability groups are reported. Standard errors are clustered at the state level.

List of Disabilities

As previously explained in Section 1.2.2, the Anti-Discrimination Act (Article 4) outlines a list of disabilities that qualify for reserved jobs from the AA quota regulation. Throughout the years, this list has been modified to include other disabilities to accommodate decisions made by the Ministry of Labor, Labor Courthouses (*Justiça do Trabalho*), and Supreme Courts. The list of disabilities include:

Physical. Complete or partial alteration of one or more segments of the human body, causing impairment of physical function. It can be paraplegia, paraparesis, monoplegia, monoparesis, tetraplegia, tetraparesis, triplegia, triparesia, hemiplegia, hemiparesis, ostomy, amputation or absence of a limb, cerebral palsy, dwarfism, limbs with congenital or acquired deformity, except for aesthetic deformities and those that do not lead to difficulties. People with reduced mobility also qualify for AA quota.

Hearing. Bilateral (partial or total) loss of 41 decibels (dB) or more, measured by an audiogram test at frequencies of 500HZ, 1000HZ, 2000Hz, and 3000Hz. It is equivalent to moderate to profound hearing loss.

Visual. blindness (visual acuity equal to or less than 0.05 in the best eye with the best optical correction); low vision (visual acuity between 0.3 and 0.05 in the best eye with the best optical correction); cases in which the sum of the visual field measured in both eyes is equal to or less than 60 degrees; simultaneous occurrence of any of the previous conditions; and monocular vision (visual acuity equal to or less than 0.05 in one eye with the best optical correction). Monocular vision was added to the list in 2011 (CONJUR/MTE 444/11).

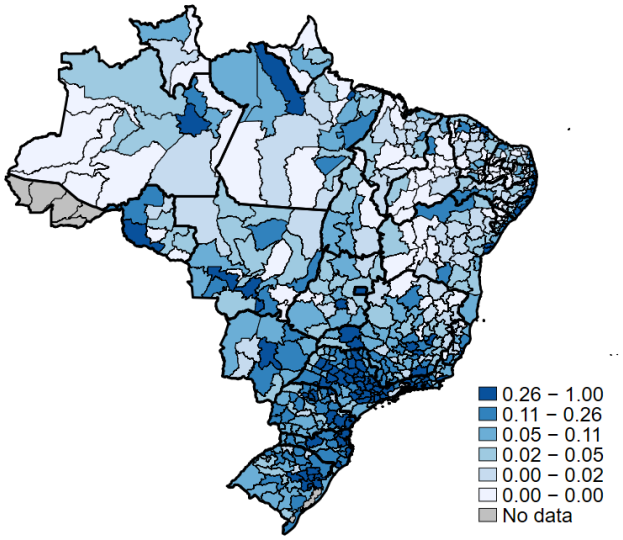
Cognitive and Mental Disorders. Permanent cognitive or mental disorders that create limitations in two or more of the following skills: communication, personal care, social skills,

use of community resources, health and safety, academic skills, leisure, and work. Examples include learning disabilities (e.g., dyscalculia) and autism spectrum disorder. The latter was added to the list in 2012 (Law 12,764).

Multiple. Multiple disabilities encompass two or more disabilities.

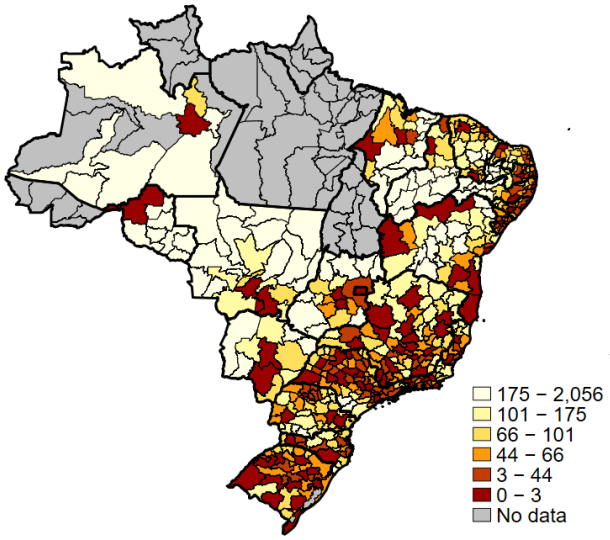
B Figures (Aggregate Analysis)

Figure 1.B.1: Cross-Market Variation: Potential Demand



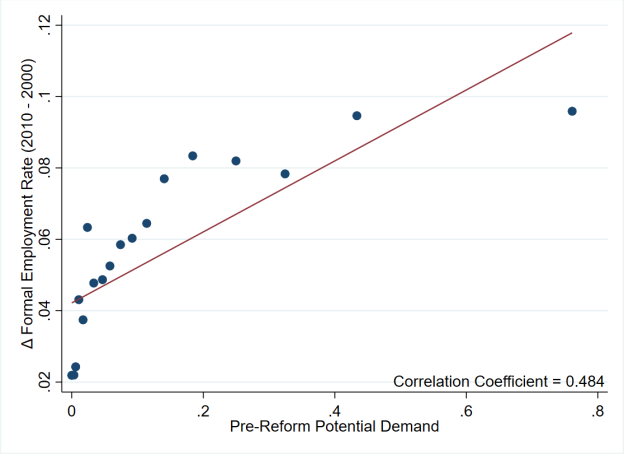
Note: Graph illustrates the geographic variation in potential demand measure across local labor markets (micro regions) in Brazil. Potential demand is defined as the total number of jobs in the private sector that would be available due to the hiring quota regulation in 1998, which is calculated from the distribution of firm size, divided by the total number of people with disabilities in 2000. Sources: 1998 RAIS and 2000 Demographic Census data.

Figure 1.B.2: Cross-Market Variation: Enforcement Capacity



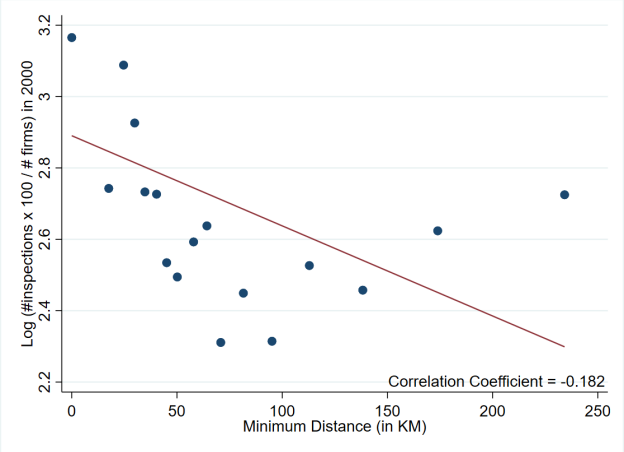
Note: Graph illustrates the geographic variation in enforcement capacity across local labor markets (micro regions) in Brazil. Enforcement capacity is computed by obtaining the driving distance between the centroid of each municipality and the nearest labor office created prior to 2000s. I then define the minimum distance of the municipalities that belong to each micro region as the minimum distance between each micro region and the nearest labor office. Sources: data on labor offices from the Ministry of Labor and Ponczek and Ulyssea (2021).

Figure 1.B.3: Pre-Reform Potential Demand and Actual Employment Rate



Note: This binned scatter plot shows the relationship between the pre-reform potential demand and the increase in employment rate in the formal sector for people with disabilities between 2000 and 2010. The former is defined as the total number of jobs in the private sector that would be available due to the hiring quota regulation in 1998, which is calculated from the distribution of firm size, divided by the total number of people with disabilities in 2000. The latter is defined as the share of formal employment in 2010 relative to 2000. The unit of observation is a micro region. The right hand side variable is grouped into 20 bins. The correlation coefficient is 0.484. Sources: 1998 RAIS and 2000 and 2010 Demographic Census data.

Figure 1.B.4: Pre-Reform Driving Distance to Labor Offices and Frequency of Inspections



Note: Graph illustrates the relationship between distance to labor offices and the number of inspections per firm. Sources: data on labor offices, This binned scatter plot shows the relationship between the pre-reform distance to the nearest labor office and the frequency of labor inspections in 2000. The former is defined as the minimum driving distance between each micro region and the nearest labor office. The latter is defined as log of total number of inspections normalized by the number of firms in each micro region in 2000. The unit of observation is a micro region. The right hand side variable is grouped into 20 bins. The correlation coefficient is -0.182. Sources: data on labor offices from the Ministry of Labor and Ponczek and Ulyssea (2021), and data on labor inspections.

Figure 1.B.5: Impact of Reform on Formal Employment of People with Disabilities Using RAIS



Note: This figure reports point estimates of the annual effects of reform on share of formal employment of people with disabilities using both the RAIS and Census data. The omitted category is micro regions with weak enforcement and weak demand in 2000.

C Tables (Aggregate Analysis)

Table 1.C.1: Descriptive Statistics at the Micro Region Level

	(1) Mean	(2) SD
Panel A: People Without Disabilities		
Formal Employment	0.191	0.099
Informal Employment	0.204	0.048
Work Income (in BRL *reais*)	641.41	318.40
Non-Work Income (in BRL *reais*)	74.22	41.28
Panel B: People With Disabilities		
Formal Employment	0.116	0.065
Informal Employment	0.146	0.041
Work Income (in BRL *reais*)	363.78	211.90
Non-Work Income (in BRL *reais*)	125.32	57.65
Panel C: Demographic and Economic Variables		
Share Female	0.498	0.012
Share College Educated	0.035	0.028
Share Urban	0.679	0.180
Unemployment Rate	0.111	0.039
Income *per capita* (in BRL *reais*)	397.16	211.05
Population	36,334.07	81,152.06
Number of Micro Regions	509	

Note: This table reports descriptive statistics (mean and standard deviation) in 2000 at the micro region level. Shares of formal and informal employment, average work and non-work incomes, shares of female, college educated, and urban population, unemployment rate, average income per capita, and average population are computed using individual-level data and sampling weights from the 2000 Census data.

Table 1.C.2: Aggregate Analysis: Impacts on Formal and Informal Work Income

	(1)	(2)	(3)	(4)
	With Disabilities		Without Disabilities	
	Formal	Informal	Formal	Informal
(Strong Demand & Strong Enforcement) × Reform	11.462***	-4.744	9.442	-2.525
	(4.035)	(3.245)	(7.006)	(2.087)
(Strong Demand & Weak Enforcement) × Reform	10.322	-4.101	14.305**	0.628
	(7.923)	(2.862)	(6.444)	(1.924)
(Weak Demand & Strong Enforcement) × Reform	0.233	-3.641	5.194	-3.636*
	(3.524)	(2.725)	(5.087)	(1.752)
Sample Size	1,018	1,018	1,018	1,018
Control Mean (in 2000)	102.53	43.96	192.71	72.42
Control Mean	134.17	50.32	246.48	78.06

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of reform on formal and informal work income for people with (Columns (1) and (2)) and without disabilities (Columns (3) and (4)). All columns refer to Equation (1.2). Means of dependent variables are computed from all micro regions in 2000. Standard errors are clustered at the state level.

Table 1.C.3: Alternative Exposure Definitions

	(1) Formal Employment	(2) Formal Employment	(3) Work Income	(4) Work Income
Panel A: Mean				
(Strong Demand & Strong Enforcement) × Reform	0.012***	0.006	32.284**	12.522
	(0.004)	(0.005)	(12.193)	(12.154)
(Strong Demand & Weak Enforcement) × Reform	0.002	0.004	25.941	9.682
	(0.004)	(0.004)	(21.884)	(9.784)
(Weak Demand & Strong Enforcement) × Reform	0.003	0.002	3.433	-2.684
	(0.003)	(0.003)	(10.685)	(6.883)
Panel B: Maximum				
(Strong Demand & Strong Enforcement) × Reform	0.013***	0.007	31.731**	19.364
	(0.004)	(0.005)	(15.107)	(11.617)
(Strong Demand & Weak Enforcement) × Reform	0.004	0.003	31.845	7.874
	(0.004)	(0.004)	(19.492)	(10.018)
(Weak Demand & Strong Enforcement) × Reform	0.005*	0.002	9.486	3.590
	(0.003)	(0.003)	(11.046)	(7.636)
Sample Size	1,018	1,018	1,018	1,018
Sample Restriction	Disabled	Non-Disabled	Disabled	Non-Disabled
Control Mean (in 2000)	0.116	0.191	363.76	641.41
Control Mean	0.143	0.229	398.64	691.32

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of reform on employment and work income using alternative definitions of exposure measure. In Panel A, I use the mean, rather than the minimum, of distances from all municipalities that belong to each micro region to define enforcement capacity. Panel B considers the maximum of distances. Columns (1) and (3) restrict the sample to people with disabilities, while Columns (2) and (4) refer to people without disabilities. All columns refer to Equation (1.2). Means of dependent variables are computed from all micro regions in 2000. Standard errors are clustered at the state level.

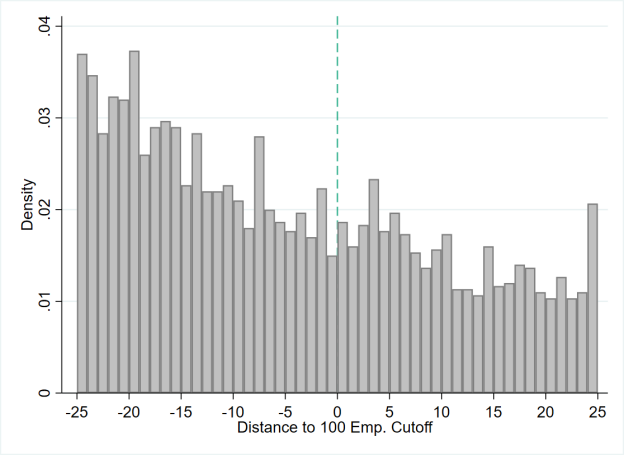
Table 1.C.4: Impacts on Migration and Welfare Program Take-Up

	(1) Migration	(2) Welfare Program
Strong Demand & Strong Enforcement) × Reform	0.016	-0.025***
	(0.016)	(0.008)
(Strong Demand & Weak Enforcement) × Reform	0.022	-0.018**
	(0.013)	(0.007)
(Weak Demand & Strong Enforcement) × Reform	0.011	-0.003
	(0.018)	(0.006)
Sample Size	1,018	1,018
Sample Restriction	Disabled	Disabled
Control Mean (in 2000)	0.297	0.260
Control Mean	0.265	0.311

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of reform on migration rate and likelihood of receiving a welfare program for people with disabilities. Migration is defined as having moved to another state in the last five years. All columns refer to Equation (1.2). Means of dependent variables are computed from all micro regions in 2000. Standard errors are clustered at the state level.

D Figures (Firm-Level Analysis)

Figure 1.D.1: Histogram



Note: Histogram depicts the distribution of firm size, measured by total number of employees, relative to the affirmative action quota cutoff of 100 employees within one point bins. Further details about the sample can be found in Table 1.E.1. Sources: RAIS and data on inspections.

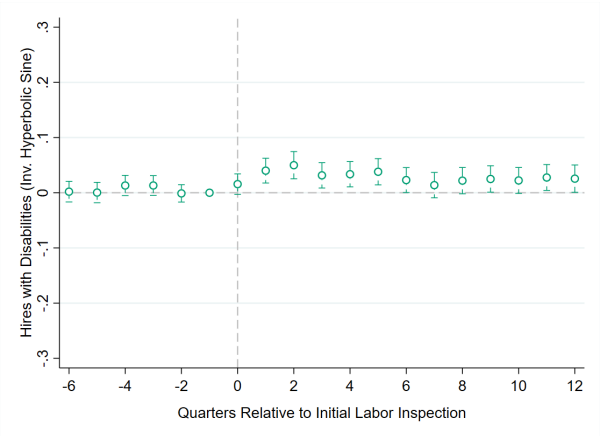
Figure 1.D.2: No Evidence of Bunching



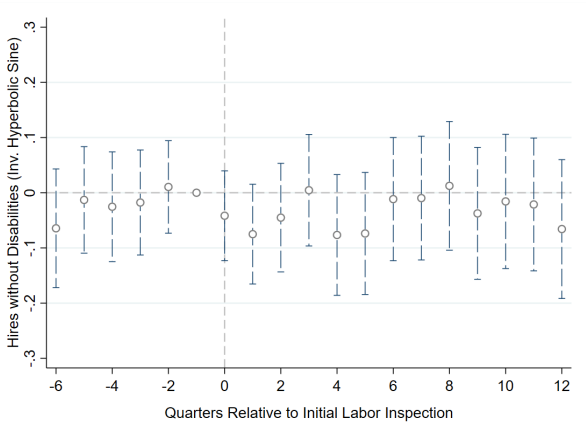
Note: Figure displays the distribution of workers for all firms in the sample before and after the inspections, using a bin size of one. Sources: RAIS and data on inspections.

Figure 1.D.3: Additional Effects of AA Quota on New Hires and Separations

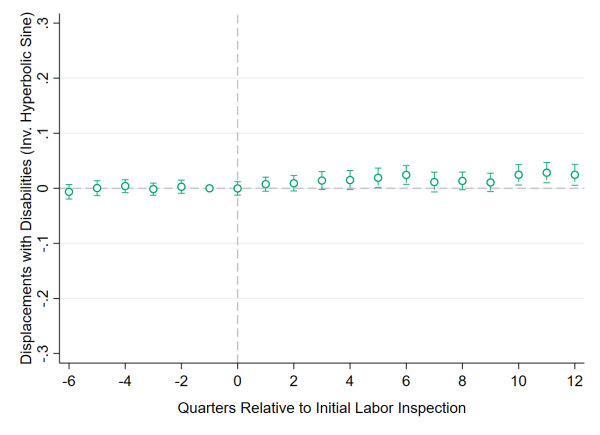
(a) (IHS) Hires with Disabilities



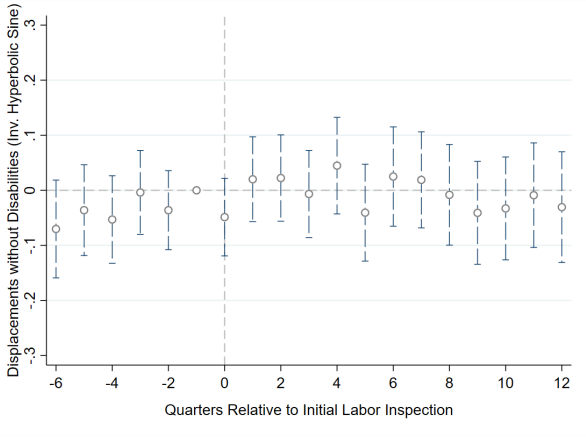
(b) (IHS) Hires without Disabilities



(c) (IHS) Displaced with Disabilities



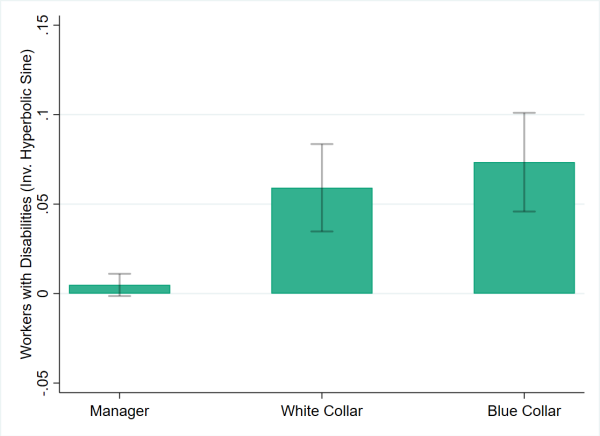
(d) (IHS) Displaced without Disabilities



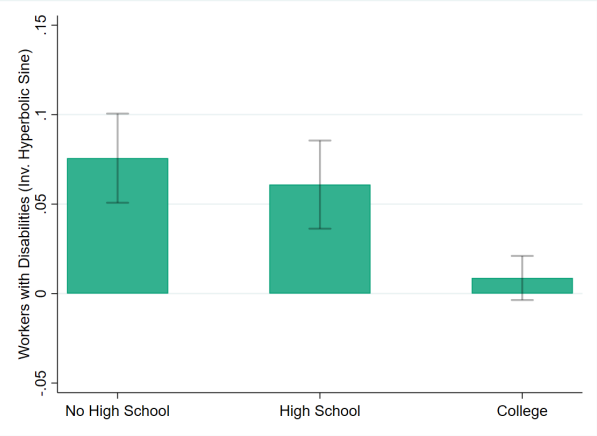
Note: This figure reports point estimates of the quarterly effects of AA quota on hires and separations of workers with and without disabilities. The outcome variables are the inverse hyperbolic sine transformation of these variables. The omitted category is the quarter before inspection. All graphs refer to Equation (2.1).

Figure 1.D.4: Heterogeneous Employment Effects of AA Quota

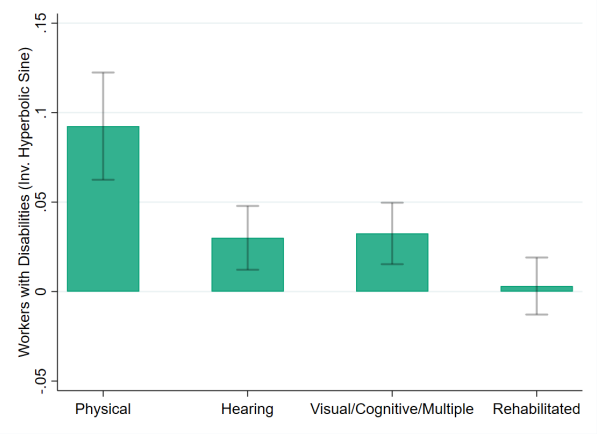
(a) Occupations (DiD)



(b) Educational Levels (DiD)



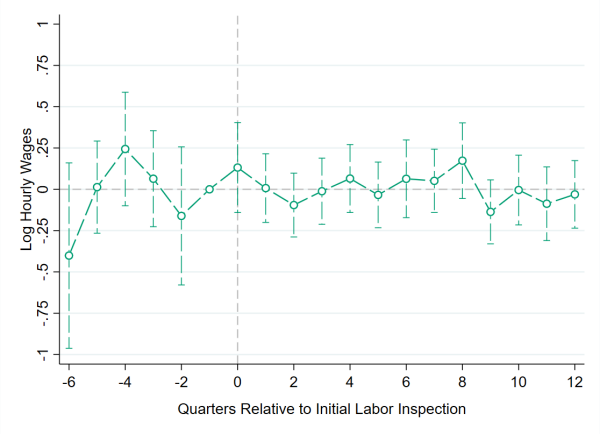
(c) Disabilities (DiD)



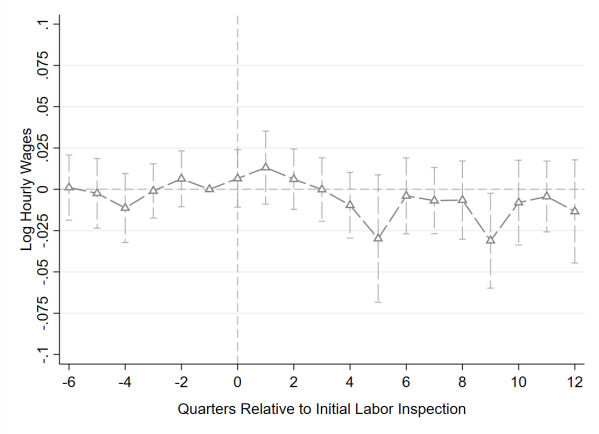
Note: This figure reports aggregate point estimates of the effects of AA quota on employment of people with disabilities considering heterogeneity across occupations, educational levels, and disabilities. The omitted category is the quarter before inspection. More details can be found in Tables 1.E.2–1.E.4, Appendix E.

Figure 1.D.5: Effects of AA Quota on Wages (New Hires)

(a) New Hires with Disabilities



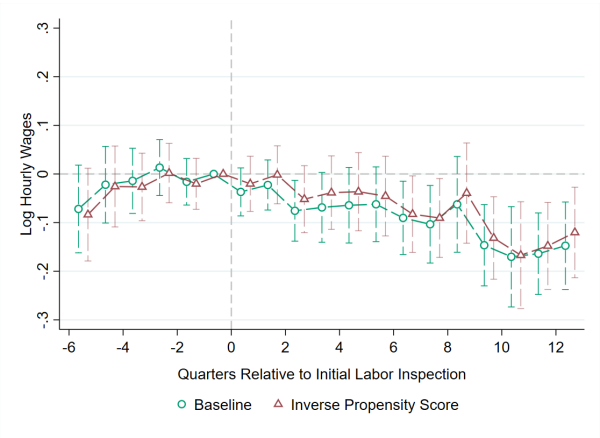
(b) New Hires without Disabilities



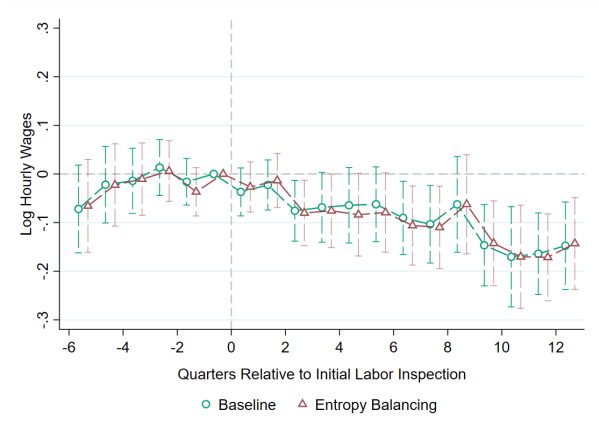
Note: This figure reports point estimates of the quarterly effects of AA quota on log hourly wages for new hires with and without disabilities separately. The omitted category is the quarter before inspection. More details can be found in Column (6) of Table 1.5.

Figure 1.D.6: Robustness of Wage Effects: Re-Weighting Methods

(a) Inverse Propensity Score Weights



(b) Entropy-Balancing Weights



Note: This figure reports point estimates of the quarterly effects of AA quota on log hourly wages for workers with disabilities using re-weighting methods. The green estimates repeat the baseline specification from Column (4) of Table 1.5. Figure 1.D.6a displays coefficients using inverse propensity score weights from the following pre-inspection characteristics: gender, race, age, squared age, education, and occupation. Figure 1.D.6a shows coefficients using entropy-balancing weights from Hainmueller (2012).

E Tables (Firm-Level Analysis)

Table 1.E.1: Descriptive Statistics: Firm-Level Sample

	(1)		(2)		(3)		(4)	
	Control Firms				Treated Firms			
	Before		After		Before		After	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Main Variables								
# Employees without Disabilities	72.23	25.43	97.24	81.45	93.29	37.28	131.21	275.60
# Employees with Disabilities	0.31	3.69	0.48	4.35	0.29	2.69	0.74	4.33
IHS Employees without Disabilities	4.87	0.58	5.09	0.68	5.11	0.62	5.35	0.72
IHS Employees with Disabilities	0.10	0.42	0.19	0.55	0.14	0.44	0.35	0.68
Has at Least One Employee with Disabilities	0.08	0.28	0.14	0.34	0.12	0.33	0.25	0.43
Share Employees with Disabilities (x100)	0.39	4.53	0.44	3.95	0.31	2.91	0.57	3.74
IHS Hires without Disabilities	2.81	1.08	2.60	1.39	3.09	1.10	2.87	1.46
IHS Hires with Disabilities	0.02	0.18	0.03	0.24	0.02	0.16	0.06	0.28
IHS Displaced without Disabilities	2.01	0.96	2.46	0.96	2.24	1.03	2.73	0.99
IHS Displaced with Disabilities	0.01	0.14	0.02	0.16	0.01	0.12	0.04	0.21
Log Average Earnings	7.37	0.51	7.44	0.60	7.38	0.53	7.44	0.71
Location								
Central West Region	0.07	0.25	0.07	0.26	0.05	0.22	0.05	0.22
North Region	0.04	0.20	0.04	0.20	0.04	0.20	0.04	0.20
Northeast Region	0.10	0.31	0.10	0.31	0.11	0.31	0.11	0.32
South Region	0.22	0.41	0.22	0.41	0.26	0.44	0.25	0.44
Southeast Region	0.56	0.50	0.56	0.50	0.54	0.50	0.54	0.50
Sector								
Construction	0.06	0.25	0.07	0.25	0.08	0.27	0.08	0.27
Commerce	0.19	0.40	0.18	0.39	0.17	0.38	0.16	0.37
Transp., Storage & Commun.	0.09	0.28	0.09	0.29	0.08	0.27	0.08	0.27
Transformation Industry	0.30	0.46	0.30	0.46	0.29	0.45	0.29	0.46
Real Estate	0.15	0.36	0.15	0.36	0.16	0.37	0.15	0.36
Services	0.04	0.19	0.04	0.19	0.04	0.20	0.04	0.20
Other Categories	0.17	0.37	0.16	0.37	0.19	0.39	0.19	0.39
N	3,309		3,309		2,013		2,013	

Note: This table reports descriptive statistics for firms using information from RAIS data. The first two columns refer to a sample of control firms. Columns (3) and (4) report summary statistics for the treatment group. Further details on the sample construction are found in Section 1.5. Odd columns indicate summary statistics using the averages in the quarterly window [-6,-1] before labor inspection while even columns refer to the quarterly window [0, 12] following it. The variables are: total number of employees with and without disabilities, log number of employees with and without disabilities, indicator for having at least one employee with disabilities, share of employees with disabilities, log average earnings, indicator variables for whether the establishment is located in Central-West, North, Northeast, South and Southeast regions, average population of the municipality in which the establishment is located, and indicator variables for economic sector the establishment belongs to (administration, construction, commerce, transportation, storage and communication, transformation industry, services, or other sectors). Sources: RAIS and data on inspections.

Table 1.E.2: Heterogeneity in Employment Effects by Occupations

	(1) Manager	(2) Professional	(3) White Collar	(4) Blue Collar
Panel A: Dynamic Impacts				
Immediate ($k = 0$)	-0.000	0.003	0.011**	0.012
	(0.002)	(0.004)	(0.005)	(0.009)
Short Run ($k = 6$)	0.003	0.017*	0.052***	0.062***
	(0.005)	(0.009)	(0.015)	(0.019)
Long Run ($k = 12$)	0.003	0.026**	0.076***	0.089***
	(0.005)	(0.011)	(0.017)	(0.022)
Panel B: Aggregate Impacts				
Post × Quota	0.005	0.013*	0.053***	0.073***
	(0.003)	(0.007)	(0.011)	(0.014)
Sample Size	60,000	60,000	60,000	60,000
Firm and Year FEs	✓	✓	✓	✓
State and Industry Trends	✓	✓	✓	✓
# Firms	3,000	3,000	3,000	3,000
Mean Dep. Var (Control)	0.008	0.018	0.024	0.072

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports heterogeneous effects of AA quota across occupational groups: managerial, high-skill professional, low-skill white collar, and blue collar. All columns refer to Equation (2.1) and use the inverse hyperbolic sine transformation of number of workers with disabilities in occupational groups as main outcomes. Means of dependent variables are computed from the control group in the quarterly window [-6, -1] before inspection. Standard errors are clustered at the firm level.

Table 1.E.3: Heterogeneity in Employment Effects by Educational Levels

	(1) No High School	(2) High School	(3) College
Panel A: Dynamic Impacts			
Immediate ($k = 0$)	0.010	0.012*	0.007*
	(0.008)	(0.006)	(0.004)
Short Run ($k = 6$)	0.072***	0.052***	0.011
	(0.019)	(0.017)	(0.009)
Long Run ($k = 12$)	0.084***	0.099***	0.014
	(0.020)	(0.020)	(0.010)
Panel B: Aggregate Impacts			
Post × Quota	0.076***	0.061***	0.009
	(0.013)	(0.013)	(0.006)
Sample Size	60,000	60,000	60,000
Firm and Year FEs	✓	✓	✓
State and Industry Trends	✓	✓	✓
# Firms	3,000	3,000	3,000
Mean Dep. Var (Control)	0.062	0.045	0.013

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports heterogeneous effects of AA quota across educational levels: without high school degree, with high school degree, and with college degree. All columns refer to Equation (2.1) and use the inverse hyperbolic sine transformation of number of workers with disabilities in educational levels as main outcomes. Means of dependent variables are computed from the control group in the quarterly window [-6, -1] before inspection. Standard errors are clustered at the firm level.

Table 1.E.4: Heterogeneity in Employment by Disabilities

	(1) Physical	(2) Hearing	(3) Vis./Cogn./ Multi.	(4) Rehab.
Panel A: Dynamic Impacts				
Immediate ($k = 0$)	0.029***	-0.004	0.001	-0.007
	(0.008)	(0.005)	(0.004)	(0.007)
Short Run ($k = 6$)	0.085***	0.029**	0.021**	-0.003
	(0.019)	(0.012)	(0.011)	(0.011)
Long Run ($k = 12$)	0.116***	0.028**	0.055***	0.004
	(0.023)	(0.014)	(0.013)	(0.011)
Panel B: Aggregate Impacts				
Post × Quota	0.092***	0.030***	0.032***	0.003
	(0.015)	(0.009)	(0.009)	(0.008)
Sample Size	51,574	51,574	51,574	51,574
Firm and Year FEs	✓	✓	✓	✓
State and Industry Trends	✓	✓	✓	✓
# Firms	3,000	3,000	3,000	3,000
Mean Dep. Var (Control)	0.057	0.027	0.008	0.009

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports heterogeneous effects of AA quota across types of disabilities: physical, hearing, visual, intellectual or multiple, and rehabilitated individuals. All columns refer to Equation (2.1) and use the inverse hyperbolic sine transformation of number of workers with specific disabilities as main outcomes. Means of dependent variables are computed from the control group in the quarterly window [-6, -1] before inspection. Standard errors are clustered at the firm level.

Table 1.E.5: Robustness Checks: Firms-Level Results

	(1) (IHS) employm.	(2) employm. (level)	(3) log employm.	(4) (IHS) employm.	(5) employm. (level)	(6) employm. (non-disabled)	(7) (IHS) employm.	(8) (IHS) employm.	(9) (IHS) employm.	(10) (IHS) employm.
Panel A: Dynamic Impacts										
Immediate ($k = 0$)	0.025**	-0.043	0.019**	0.025**	-0.079	-0.003	0.014	0.004	0.039***	0.035***
	(0.010)	(0.087)	(0.008)	(0.010)	(0.203)	(0.009)	(0.014)	(0.017)	(0.011)	(0.014)
Short Run ($k = 6$)	0.113***	0.397*	0.088***	0.113***	0.484	0.038	0.100***	0.077**	0.141***	0.136***
	(0.024)	(0.209)	(0.019)	(0.024)	(0.378)	(0.056)	(0.032)	(0.039)	(0.028)	(0.033)
Long Run ($k = 12$)	0.157***	0.401**	0.121***	0.158***	0.441	0.031	0.128***	0.127***	0.170***	0.172***
	(0.028)	(0.164)	(0.022)	(0.028)	(0.389)	(0.073)	(0.037)	(0.046)	(0.032)	(0.037)
Panel B: Aggregate Impacts										
Post × Quota	0.124***	0.262**	0.095***	0.126***	0.463*	0.042	0.094***	0.063**	0.152***	0.151***
	(0.018)	(0.117)	(0.014)	(0.018)	(0.281)	(0.054)	(0.024)	(0.029)	(0.021)	(0.024)
Sample Size	60,000	60,000	60,000	60,000	60,000	60,000	34,140	23,420	47,680	36,580
Firm and Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State and Industry Trends	✓	✓	✓	×	×	×	✓	✓	✓	✓
Sample Restriction	75-125 emp.	75-125 emp.	75-125 emp.	75-125 emp.	75-125 emp.	75-125 emp.	85-115 emp.	90-110 emp.	exc. 5 emp.	exc. 10 emp.
Model	OLS	OLS	OLS	OLS	Poisson	Poisson	OLS	OLS	OLS	OLS
# Firms	3,000	3,000	3,000	3,000	3,000	3,000	1,707	1,171	2,384	1,829
Mean Dep. Var (Control)	0.104	0.311	0.082	0.104	0.311	72.27	0.118	0.137	0.097	0.089

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports several robustness checks for the firm-level analysis. Column (1) repeats Column (1) from Table 1.3. Columns (2) and (3) refer to number of employees with disabilities and its natural logarithm as the dependent variables. Column (4) excludes state- and industry-specific trends from the set of firm controls. Columns (5) and (6) estimate a conditional fixed-effect Poisson model using number of employees with and without disabilities as dependent variables. Columns (7) and (8) consider narrower windows around the cutoff. Columns (9) and (10) excludes firms very close to the cutoff. Means of dependent variables are computed from the control group in the quarterly window [-6, -1] before inspection. Standard errors are clustered at the firm level.

Table 1.E.6: Descriptive Statistics: Worker-Level Sample

	(1)		(2)		(3)		(4)	
	Control Workers				Treated Workers			
	Before		After		Before		After	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Main Variables								
Has Disability	0.004	0.06	0.005	0.07	0.003	0.05	0.006	0.08
Log Hourly Earnings	2.19	0.65	2.28	0.68	2.20	0.66	2.28	0.70
Hourly Earnings	11.34	26.39	12.74	28.26	11.46	22.71	12.68	24.06
Contracted Hours (Monthly)	185.43	23.77	184.77	24.14	184.49	24.18	184.05	24.67
Male	0.66	0.48	0.66	0.48	0.65	0.48	0.63	0.48
White	0.67	0.47	0.63	0.48	0.69	0.46	0.65	0.48
Has College Degree	0.10	0.30	0.10	0.30	0.10	0.31	0.11	0.31
Manager	0.03	0.18	0.03	0.17	0.03	0.17	0.03	0.17
Professional	0.14	0.35	0.14	0.35	0.16	0.36	0.16	0.37
Low-Skill White Collar	0.16	0.37	0.17	0.38	0.16	0.36	0.17	0.37
Blue Collar	0.66	0.47	0.65	0.48	0.66	0.47	0.64	0.48
Location								
Central West Region	0.07	0.25	0.07	0.25	0.04	0.21	0.05	0.22
North Region	0.04	0.19	0.05	0.21	0.04	0.19	0.04	0.21
Northeast Region	0.10	0.30	0.11	0.31	0.10	0.30	0.10	0.30
South Region	0.23	0.42	0.21	0.40	0.27	0.44	0.23	0.42
Southeast Region	0.56	0.50	0.57	0.49	0.55	0.50	0.57	0.49
Sector								
Construction	0.06	0.23	0.07	0.26	0.06	0.24	0.08	0.27
Commerce	0.19	0.39	0.18	0.39	0.17	0.37	0.16	0.36
Transp., Storage & Commun.	0.09	0.28	0.09	0.29	0.07	0.26	0.08	0.27
Transformation Industry	0.30	0.46	0.29	0.45	0.29	0.45	0.26	0.44
Real Estate	0.14	0.35	0.16	0.37	0.17	0.37	0.22	0.41
Services	0.04	0.20	0.04	0.20	0.04	0.21	0.04	0.20
Other Categories	0.17	0.38	0.16	0.37	0.20	0.40	0.17	0.38
N	825,152		825,152		633,831		633,831	

Note: This table reports descriptive statistics for workers using information from RAIS data. The first two columns refer to a sample of workers from control firms. Columns (3) and (4) report summary statistics for the treatment workers. Further details on the sample construction are found in Section 1.5. Odd columns indicate summary statistics using the averages in the quarterly window [-6,-1] before labor inspection while even columns refer to the quarterly window [0, 12] following it. The variables are: indicator for having a disability, log hourly wages, absolute hourly wages, monthly number of hours contracted, indicators for male, white, having a college degree, indicators for manager, professional, low-skill white collar, and blue collar positions, indicator variables for whether the establishment is located in Central-West, North, Northeast, South and Southeast regions, average population of the municipality in which the establishment is located, and indicator variables for economic sector the establishment belongs to (administration, construction, commerce, transportation, storage and communication, transformation industry, services, or other sectors).

F Survey Data

Survey Overview. The pilot survey was implemented in collaboration with Oppen Social. The survey instrument included vignette experiments and questions about affirmative action support, along with questions on challenges and concerns in hiring people with disabilities. The sample from the pilot survey has 60 firms from the RAIS data, with 31 firms having more than 100 employees.[49] In June of 2022, the Oppen Social reached out, within a window of seven days, to representatives of Human Resources (HR) departments from 764 and 751 firms found in the RAIS data with less and more than 100 employees. In cases in which the firm did not have a HR department, the interview was conducted with an employee familiar with the recruiting process. The response rate was 4 percent. The low response rate is expected for a 30-minute phone survey conducted with HR representatives.

Around 52 percent of surveyed companies are located in the Southeast region, 19 percent in the Northeast, 17 percent in the South, 9 percent in the Central West, and 3 percent in the North. About 18 percent of firms belong to manufacturing sector, 4 percent to transportation, and 16 percent to wholesale or retail. The distribution of firms in terms of industry and location is similar to the national sample (see Table A2 from Szerman (Forthcoming)).

Survey Questions. The survey is divided into five complementary parts. The first part of the survey has questions related to affirmative action support. I ask respondents to rate on a four-point scale, in which 1 is "disagree" and 4 is "agree a lot", how much they agree that: (i) "women should have preference in hiring in the labor market"; (ii) "black people should have preference in hiring in the labor market"; (iii) "people with disabilities should have preference in hiring in the labor market"; and (iv) "hiring quotas for people with disabilities

[49] The next version of the survey will be conducted with a larger sample. I exclude firms with less than 50 employees from the initial sampling process.

should exist". I randomize the order of questions and the appearance of one of the last two questions.

The second part of the survey consists of questions adapted from Alfaro-Urena et al. (2021). I ask respondents to select up to three alternatives about: (i) "which are the most important aspects your company uses to select highly qualified workers (e.g., managers, engineers, and administrators)"; (ii) "which are the most important aspects your company uses to select less qualified workers (e.g. operators, packers, and janitorial staff)"; and (iii) "which are the most important aspects your company uses to select workers with disabilities". The alternatives are: (a) curriculum vitae; (b) letters of recommendation or references from former employers or teachers; (c) immediate availability; (d) test of cognitive, psychometric, or psychological skills; (e) test of knowledge or professional skills related to the job; (f) evaluation during the probationary period; (g) interviews or tests using English or other languages; (h) criminal records; and (j) availability of infrastructure or assistive technologies. I also randomize the order of questions.

The third part includes a vignette experiment describing a big fictitious consultancy that would like to hire someone for an entry-level job to do routine clerical and organizational tasks. The vignette introduces Rafael, a 22-year-old man who finished high school, has flexibility, proactivity, and good organization skills, and interacts well with people. For some respondents, I randomize the information that this man has a bilateral hearing loss. This disability is chosen because it does not require accommodation costs or interfere in productivity for this position. I then ask respondents to rate on a four-point scale, in which 1 is "unlikely" and 4 is "very likely", how likely they think that the company would be interested in hiring him and how likely they think that he would accept the job.

In the fourth part, adapted from Domzal et al. (2008), I add questions about employer perspective on the employment of people with disabilities. I ask respondents how likely they agree on a four-point scale, in which 1 is "disagree" and 4 is "agree a lot", that the

following issues represent barriers to hire workers with disabilities at the company: (i) lack of knowledge or information about people with disabilities; (ii) accommodations and barriers during the hiring process; (iii) attitudes of co-workers; (iv) attitudes of supervisors; (v) fear of absenteeism; (vi) accommodation costs; (vii) cannot find qualified people with disabilities; and (viii) nature of the work cannot be performed by people with disabilities. In option (vi), I also ask respondents to provide examples of accommodations costs that the company incurred in order to hire a worker with disabilities.

I next describe some concerns about people with disabilities often heard from employers. I ask respondents to rate on a four-point scale, in which 1 is "disagree" and 4 is "agree a lot", how much they agree that each concern is important at the company: (i) supervisors are not comfortable managing people with disabilities; (ii) supervisors are not sure how to evaluate a person with a disability; (iii) co-workers are not comfortable with colleagues with disabilities; (iv) workers with disabilities lack the skills and experience to do their jobs; (v) workers with disabilities might not be as safe and productive as other workers without disabilities; (vi) it costs more to hire a worker with disabilities due to additional management time; and (vii) negative stigma associated with hiring quota for workers with disabilities.

I further outline some strategies that companies use to hire workers with disabilities. I interrogate how much respondents agree on a four-point scale, in which 1 is "disagree" and 4 is "agree a lot", that the following strategies are useful to reduce barriers to hire workers with disabilities: (i) using recruitment agency specialized in people with disabilities; (ii) creating a diversity committee and training existing staff; (iii) increasing availability of assistive technologies; (iv) flexible working hours; (v) fiscal incentives, such as subsidies and tax deductions; and (vi) more labor inspections.

In the fifth part of the survey, I introduce open questions about performance of the last hire with disabilities relative to the peers at the company, whether accommodations

were provided to this worker, and employer perceptions about what and who trigger labor inspections to better understand the mechanisms behind worker-level results.

Survey Responses. I discuss the survey results in two ways. First, I report average means of the responses across control and treatment groups, which represent firms without and under AA quota requirement (i.e., firms with less and more than 100 employees). Second, I present differential responses between these two groups after estimating the following regression:

$$R_{ij} = \alpha + \beta \times \mathbf{1}(\text{Quota}_i \geq 100) + \gamma_j + \epsilon_{ij}, \tag{1.14}$$

in which R_{ij} stands for employer i's answer in questionnaire type j; $\mathbf{1}(\text{Quota}_i \geq 100)$ is an indicator for firms with at least 100 employees; and γ_j is the questionnaire type fixed effects. The latter accounts for the fact that I randomize the order in which several questions are presented. The coefficient of interest, β, captures the average difference in responses between firms without and under the AA quota regulation.

Affirmative Action Support. Panel A of Table 1.F.2 displays results for affirmative action support. Compared to black people and women, respondents are, on average, more likely to support preference in hiring for people with disabilities. There is no significant differences across AA quota regulation requirement as the p-values range from 0.399 to 0.889.

Vignette Experiment. Table 1.F.1 reveals results for the vignette experiment.[50] Respondents are, on average, less likely to express interest in hiring a fictitious candidate with disabilities relative to a similar counterpart without disabilities. There is no differences in the likelihood that fictitious candidate will accept the position.

Most Important Aspects to Select Workers. Table 1.F.3 presents means and point estimates for the most important aspects to select highly skilled, less skilled, and disabled workers. Curriculum Vitae (CV) is the most important factor for these three groups of workers. Some additional patterns emerge. First, the most important factors to hire workers with disabilities resemble more the ones to hire less skilled than factors to recruit highly skilled workers. Second, around 34 percent of employers report the availability of infrastructure or assistive technologies as an important factor. However, there are no significant differences across AA quota groups (p-value = 0.848). Third, firms under the AA quota are more likely to use the probationary period (p-value = 0.003) and less likely to use knowledge tests as factors to select workers with disabilities (p-value = 0.089). Fourth, although larger firms are more likely to use CV to select highly (p-value = 0.021) and less skilled workers (p-value = 0.034), such differential pattern is not evident for people with disabilities (p-value = 0.257).

Employer Perceptions. Panel B of Table 1.F.2 reveals challenges in hiring people with disabilities. On average, the most relevant challenges are the following: lack of knowledge or information about people with disabilities, inability to find qualified people with disabilities, and accommodations and barriers during the hiring process. These patterns seem consistent with employers having more challenges to interpret signals from people with disabilities.

[50] For the vignette experiment, I consider a different specification:

$$R_{ij} = \alpha + \beta_1 \times \mathbf{1}(\text{Quota}_i \geq 100) \times \mathbf{1}(\text{Has Disability}_i = 1) + \beta_2 \times \mathbf{1}(\text{Quota}_i \geq 100) + \beta_3 \mathbf{1}(\text{Has Disability}_i = 1) + \gamma_j + \epsilon_{ij}, \tag{1.15}$$

in which the set of variables is the same as in Equation (1.14) and $1(\text{Has Disability}_i = 1) stands for employer i's question receiving randomized information on disability.$

In addition, as an indirect evidence of lower hiring standards to recruit more workers with disabilities (Coate and Loury (1993)), firms under AA quota are more likely to report that they cannot find qualified workers with disabilities (p-value = 0.090). Respondents rate less attitudes of co-workers and supervisors as challenges, though firms under AA quota are more likely to report attitudes of co-workers and supervisors as challenges (p-values of 0.000 and 0.016).

In Panel C, turning to concerns about workers with disabilities at the company, supervisors who are not sure how to evaluate a person with a disability appear as the most relevant concern, followed by supervisors who are not comfortable managing workers with disabilities. Furthermore, firms under the AA quota are also more likely to report that people with disabilities might not be as safe and productive as other workers without disabilities (p-value = 0.015), workers with disabilities require additional management time (p-value = 0.080), and co-workers are not comfortable with colleagues with disabilities (p-value = 0.056). These pieces of evidence also point to a lower average skill level among workers with disabilities.

Lastly, Panel D illustrates the most relevant strategies to reduce barriers in hiring people with disabilities. Respondents tend to disagree that more frequent labor inspections and flexible working hour would represent important strategies. Interestingly, there is no evidence that firms under the AA quota are more likely to report investments in screening capital (e.g., specialized recruitment agencies) and workplace accommodations (e.g., increasing availability of assistive technologies) as relevant strategies. If anything, firms under the AA quota are *less* likely to agree that specialized recruitment agencies as relevant strategies (p-value = 0.035).

Open Questions. The survey asks how likely respondents think on a four-point scale, in which 1 is "disagree" and 4 is "agree a lot", that inspections are triggered by employees. I find that 41.67 percent of respondents report to be likely or very likely. When asked who they think that made these complaints, incumbents with disabilities are never mentioned.

Table 1.F.1: Survey: Vignettes

	(1) Hire	(2) Accept
Above 100 emp.	-0.093	0.019
	(0.193)	(0.169)
Has Disability	-0.723**	-0.150
	(0.309)	(0.301)
Has Disability x Above 100 emp.	0.403	0.291
	(0.271)	(0.259)
Mean Dep. Var (Control)	3.26	3.19
Mean Dep. Var (Treated)	3.34	3.34

Note: Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports differences in responses in the vignette experiment after estimating Equation (1.15), along with mean survey answers for firms with less and more than 100 employees.

Table 1.F.2: Survey Responses: Affirmative Action Support and Employer Perceptions

	(1) Mean (Control)	(2) Mean (Treated)	(3) Pt. Est. (Std. Err.)
Panel A: Affirmative Action Support			
Women should be given preference in hiring	2.32	2.33	0.005 (0.271)
Blacks should be given preference in hiring	2.19	2.23	0.036 (0.254)
People with disabilities should be given preference in hiring	3.06	2.71	-0.327 (0.382)
Hiring quota for people with disabilities should exist	3.33	3.46	0.125 (0.229)
Panel B: Challenges in Hiring People with Disabilities			
Lack of knowledge or information about people with disabilities	2.59	2.46	-0.126 (0.236)
Accommodations or barriers during the hiring process	2.16	2.41	0.236 (0.262)
Attitudes of co-workers	1.37	2.17	0.812*** (0.202)
Attitudes of supervisors	1.40	2.00	0.580** (0.233)
Fear of absenteeism	1.45	1.76	0.327 (0.204)
Not knowing how much accommodation will cost	1.61	1.62	-0.005 (0.218)
Cannot find qualified people with disabilities	2.22	2.69	0.442* (0.257)
Nature of the work cannot be performed by people with disabilities	1.68	1.76	0.096 (0.210)
Panel C: Concerns for the Company			
Supervisors are not comfortable managing people with disabilities	1.70	2.00	0.312 (0.215)
Supervisors are not sure how to evaluate a person with a disability	2.03	2.03	-0.025 (0.258)
Co-workers are not comfortable with colleagues with disabilities	1.33	1.76	0.400* (0.205)
Workers with disabilities lack the skills and experience to do their jobs	1.35	1.55	0.192 (0.204)
People with disabilities may not be as safe and productive as other workers	1.48	2.00	0.558** (0.222)
It costs more due to additional management time	1.57	1.90	0.366* (0.205)
Negative stigma associated with hiring quota for workers with disabilities	1.74	1.76	0.038 (0.229)
Panel D: Strategies to Reduce Barriers in Hiring People with Disabilities			
Specialized recruitment agencies	3.06	2.62	-0.442** (0.205)
Diversity committee and training existing staff	2.84	2.83	0.029 (0.205)
Increasing availability of assistive technologies	2.84	2.75	-0.108 (0.195)
Flexible working hours	2.32	2.59	0.269 (0.230)
Fiscal incentives, such as subsidies and tax deductions	2.68	2.79	0.120 (0.249)
More labor inspections	2.32	2.28	-0.038 (0.266)
N	29	31	

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports mean survey answers by AA quota regulation requirement (Columns (1) and (2) for firms with less and more than 100 employees) and point estimates after estimating Equation (1.14), along with standard errors (in parentheses).

Table 1.F.3: Survey Responses: Most Important Factors to Hire Workers

	(1) Mean (Control)	(2) Mean (Treated)	(3) Pt. Est. (Std. Err.)
Panel A: Three Most Important Factors to Select People With Disabilities			
Curriculum Vitae	0.63	0.76	0.132 (0.115)
Letters of recommendations or references from former employers/teachers	0.33	0.21	-0.132 (0.121)
Immediate availability	0.10	0.21	0.098 (0.099)
Test of cognitive, psychometric, or psychological skills	0.17	0.10	-0.063 (0.093)
Test of knowledge or professional skills related to the job	0.40	0.21	-0.205* (0.119)
Evaluation during the probationary period	0.37	0.76	0.385*** (0.123)
Interview or test using English or other languages	0.17	0.17	0.010 (0.101)
Criminal records	0.10	0.07	-0.029 (0.077)
Availability of infrastructure or assistive technologies	0.30	0.38	0.073 (0.133)
Panel B: Three Most Important Factors to Select Less Skilled Workers			
Curriculum Vitae	0.74	0.93	0.195** (0.090)
Letters of recommendations or references from former employers/teachers	0.48	0.25	-0.244** (0.119)
Immediate availability	0.39	0.32	-0.083 (0.114)
Test of cognitive, psychometric, or psychological skills	0.16	0.14	-0.010 (0.098)
Test of knowledge or professional skills related to the job	0.23	0.21	-0.024 (0.103)
Evaluation during the probationary period	0.39	0.57	0.190 (0.133)
Interview or test using English or other languages	0.16	0.18	0.024 (0.093)
Criminal records	0.10	0.14	0.044 (0.086)
Panel C: Three Most Important Factors to Select Highly Skilled Workers			
Curriculum Vitae	0.65	0.86	0.240** (0.101)
Letters of recommendations or references from former employers/teachers	0.42	0.65	0.130 (0.117)
Immediate availability	0.13	0.17	0.019 (0.087)
Test of cognitive, psychometric, or psychological skills	0.32	0.38	0.067 (0.124)
Test of knowledge or professional skills related to the job	0.48	0.45	-0.058 (0.134)
Evaluation during the probationary period	0.48	0.28	-0.202 (0.129)
Interview or test using English or other languages	0.16	0.21	0.043 (0.102)
Criminal records	0.13	0.07	-0.058 (0.076)
N	29	31	

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports mean survey answers by AA quota regulation requirement (Columns (1) and (2) for firms with less and more than 100 employees) and point estimates after estimating Equation (1.14), along with standard errors (in parentheses).

G Framework: Welfare Effects of Enforcement of AA Quota with Imperfect Compliance

Decision Trees. Figures 1.G.1 and 1.G.2 illustrate firm and individual choices.

Figure 1.G.1: Decision Tree with Firm Payoffs

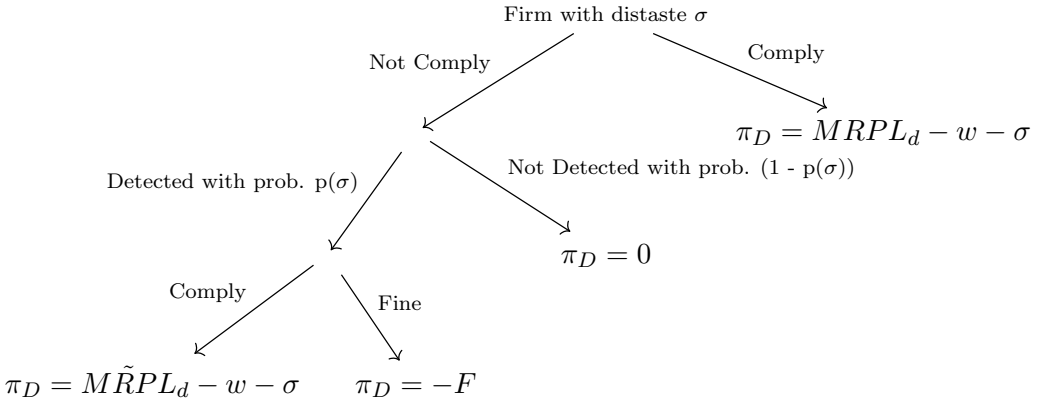
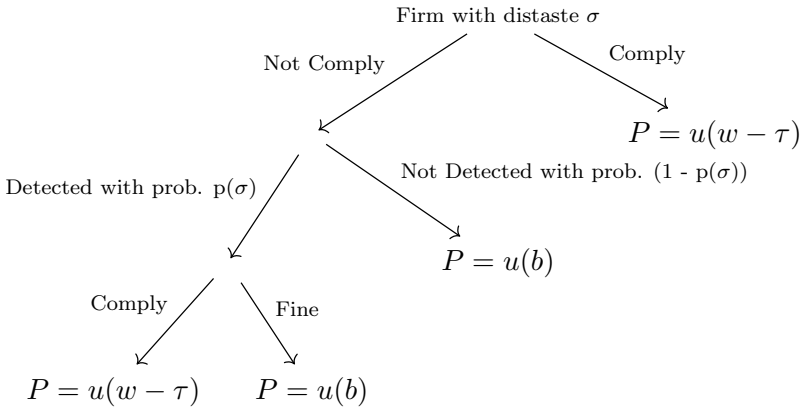


Figure 1.G.2: Decision Tree with Individual Payoffs



Enforcement and Welfare. An increase in the probability of detection $p(\sigma^*)$ impacts the expected payoff associated with non-compliance, thereby affecting the expected utility of inframarginal firms. The welfare impacts of a small change in enforcement can be expressed as:

$$\frac{\partial V}{\partial \sigma^*} = M_C[u(w-\tau) - v(b)], \tag{1.16}$$

in which $M_C \equiv \int_{\sigma^c}^{\sigma^F} \frac{\partial p(\sigma^*)}{\partial \sigma^*} f(\sigma)\, d(\sigma)$ represents the mass of inframarginal firms who choose compliance instead of delinquency due to a small change in enforcement.[51] We note that the welfare impacts summarize the changes in individual surplus of people with disabilities due to the increase of the number of individuals being employed in the formal sector instead of receiving welfare benefits.

Analogously, the impacts on firm profits can be written as follows:

$$\frac{\partial \Pi}{\partial \sigma^*} = (M\tilde{R}PL_d - w)M_C - M_F F, \tag{1.17}$$

in which $M_F \equiv \int_{\sigma^F}^{1} \frac{\partial p(\sigma^*)}{\partial \sigma^*} f(\sigma)\, d(\sigma)$ is the mass of firms that get fined. The impacts on producer surplus incorporate the changes in surplus weighted by the amount of fines paid ($M_F F$) and the difference between the marginal revenue product and wages paid to the new hires with disabilities $(M\tilde{R}PL_d - w)M_C$.

Finally, the effects on revenues can be expressed in the following way:

$$\frac{\partial R}{\partial \sigma^*} = M_C[\tau + b] + M_F F, \tag{1.18}$$

in which the marginal revenue benefit is the revenue raised from higher employment ($M_C[\tau - b]$) and fines ($M_F F$).

[51] The policy has no welfare change from firms that always abstain from compliance regardless of enforcement level. In addition, due to the envelope theorem, the policy also has no welfare change from firms that always comply with the regulation regardless of enforcement level.

Optimal Enforcement Policy. The government sets enforcement level σ^* that maximizes the following social welfare function:

$$W(\sigma^*) = V(\sigma^*) + \Pi(\sigma^*) - C(\sigma^*). \tag{1.19}$$

Under standard regularity conditions, the first-order condition can be written as:

$$\underbrace{V'(\sigma^*)}_{\text{marginal welfare benefit}} = \underbrace{C'(\sigma^*)}_{\text{marginal cost of enforcement}} - \underbrace{\Pi(\sigma^*),}_{\text{marginal producer loss}} \tag{1.20}$$

in which the government chooses enforcement level to set its marginal benefit equal to marginal cost. Put differently, the government trades off the overall marginal private benefits of higher employment of people with disabilities against the marginal cost of conducting inspections and the marginal lost surplus to firms. Because the empirical results indicate that people without disabilities are unaffected, there is no welfare change for them.

If the government also uses higher enforcement level to raise additional fiscal revenues $R(\sigma^*)$ (e.g., to increase the provision of public goods), the first-order condition can be alternatively be written as:

$$\underbrace{V'(\sigma^*)}_{\text{marginal welfare benefit}} + \underbrace{R'(\sigma^*)}_{\text{marginal revenue benefit}} = \underbrace{C'(\sigma^*)}_{\text{marginal cost of enforcement}} - \underbrace{\Pi'(\sigma^*),}_{\text{marginal producer cost}} \tag{1.21}$$

in which the marginal revenue benefit enters in the left-hand side and indicates that the government is willing to tolerate larger welfare loss to firms when setting the optimal enforcement. If the marginal costs exceed the extra revenues raised, the government must weigh the deadweight loss against the marginal benefits of increasing enforcement level.

Plugging Equations (1.16), (1.17), and (1.18) into Equation (1.21), we have that:

$$\underbrace{M_C[u(w-\tau)-v(b)]}_{\text{marginal welfare benefit}}+\underbrace{M_C[\tau+b]}_{\text{marginal revenue benefit}}=\underbrace{C'(\sigma^*)}_{\text{marginal cost of enforcement}}+\underbrace{(w-M\tilde{RPL}_d)M_C}_{\text{marginal producer cost}}. \tag{1.22}$$

All objects can be obtained from the data and the reduced-form estimates. The first term — the marginal welfare benefit — depends on the utility cost of employment. The marginal revenue benefit can be directly obtained from the Census data. The marginal cost of enforcement can be bounded by the average cost of inspections. The marginal producer loss can be computed even without data on firm outcomes through a simple discrete choice framework, which I discuss in detail in Section 1.6.2. I also propose an alternative approach to back out a "break even" producer loss.

Incumbents with Disabilities. Thus far, the model does not distinguish between new hires and incumbents with disabilities and assumes fixed wages. However, the empirical analysis indicates that firms adjust through lower wage growth for incumbent workers. In this setting, with linear utility functions, it represents a simple lump-sum transfer from people with disabilities and government to firms. This implies that the government only needs to trade off marginal benefits and marginal producer loss from new disabled hires when evaluating total welfare. Likewise, revenue from fines represents a lump-sum transfer from firms to the government in linear functional forms.

Marginal Welfare Benefit. To overcome the lack of data on consumption and assets and get a tractable expression, I assume hand-to-mouth agents so that consumption tracks net income. This assumption is reasonable in this context. I also consider the flow of income, implying that my estimates are very conservative and reflect the lower bound of the benefit

side. I take two approaches to derive bounds on the marginal welfare benefit. First, to get an upper bound to the cost of employment, I assume that unemployed individuals do not have marginal disutility from working. In this case, I can approximate the marginal benefit as an extensive margin choice of employment and, therefore, an income flow of a switch from welfare benefits to employment. From the 2010 Census, I calculate that, on average, a worker with disabilities in the formal sector makes 22,660 *reais* every year and, as a result, is subject to an income tax rate of 7.5 percent. Each person with disabilities not employed in the formal sector receives 3,827.16 *reais* in welfare benefits. Given the 41.7 percent increase in the number of employees, together with the baseline average of 0.29, the marginal welfare benefit associated with additional hires is 870.21 *reais*.

Second, to get a lower bound to the cost of employment, I need a parameter representing the utility cost of switching from employment to welfare benefits, which directly depends on the opportunity cost of a full-time job due to lost leisure. In this case, I follow Mas and Pallais (2019) and assume that the value of non-work time relative to labor earnings is 0.58. The estimated marginal welfare benefit is 372.65 *reais*.

Marginal Cost of Enforcement. Because there are no data available on inspection-specific spending, I consider the average cost of inspections as an upper bound for the marginal cost of inspections. According to the Federal Budget Panel (*Painel do Orçamento Federal*), the average annual budget of labor inspected (deflated to 2018 prices) is 27,787,724 Brazilian *reais*. Considering that the total number of inspections per year is about 279,857, each inspection costs around 99.29 *reais*. It is possible to consider alternative enforcement policies to boost compliance with regulation, such as data-driven inspections and threat-of-audit letters, that would target non-compliant firms more directly. While these targeted policies are likely to reduce the costs of inspections in the long term, implementing them would also impose its own costs.

Marginal Revenue Benefit. The marginal fiscal gain is the sum of revenues raised with income tax and saved with welfare benefits from new hires. The marginal revenue benefit is 668.34 *reais*.

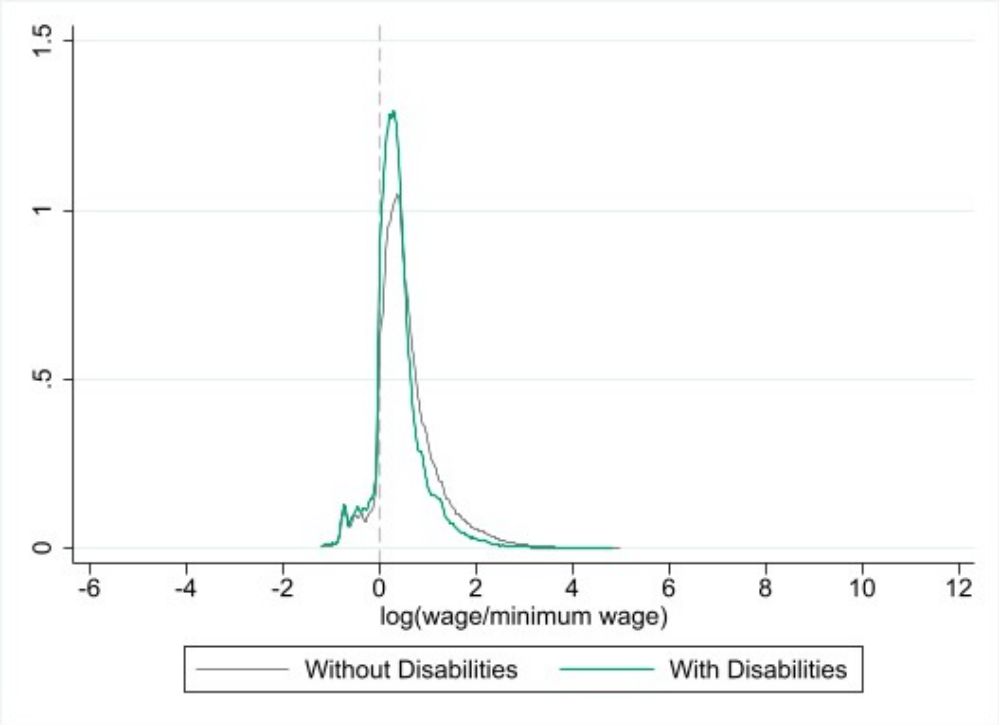
Marginal Producer Cost. Due to lack of data on profits, changes in producer surplus cannot be directly inferred. Instead, I rely on two indirect approaches to construct bounds for the marginal producer loss. First, I use reduced-form estimates to back out a "break even" producer cost that sets marginal welfare benefits equal to marginal costs from Equations (1.20) and (1.22). Table 1.G.1 reports the "break even" values of average ratio between marginal revenue product of labor and average wages considering different scenarios. Each row indicates whether opportunity cost of work and fiscal revenue benefit are accounted for in the calculation. Even in the most conservative scenario, the ratio is below one, pointing to social benefits exceeding social costs. The second approach involves using the discrete choice framework, which is discussed in detail in Section 1.6.2.

Table 1.G.1: Cost-Benefit Analysis

(1) Opport. Cost of Work	(2) Revenue Benefit	(3) Break-Even MRPL/w
No	Yes	0.6563
Yes	Yes	0.4748
No	No	0.7187
Yes	No	0.9002

Note: This table reports a range for "break-even" values of $M\tilde{R}PL$, normalized by average wages, that set marginal welfare benefits equal to marginal costs of increasing enforcement of AA quota from Equation (1.12).

Figure 1.G.3: Monthly Wage Distribution among Workers in the Private Sector



Note: Graph illustrates kernel densities (Epanechnikov kernel with a bandwidth of 0.025) of the log wage-to-minimum wage ratio for people without and with disabilities separately. The sample is restricted to workers in firms with at least 100 employees. Source: RAIS data.

Chapter 2

The Employee Costs of Corporate Debarment in Public Procurement[1]

2.1 Introduction

Corporate debarment or blacklisting is an important anti-corruption measure, providing an instrument for governments to punish companies for corrupt practices. Due to the costs that corruption exerts on economic growth and development (Mauro (1995), IMF (2016)), national governments and international agencies have proposed several anti-corruption strategies. In particular, corporate debarment has been increasingly used as a strategy to target corrupt companies and deter corruption in many countries, such as Brazil, China, France,

[1]This chapter was published at the American Economic Journal: Applied Economics in January of 2023 (Szerman (2023a)). I am extremely grateful to Alexandre Mas and Thomas Fujiwara for their constant guidance and support. Ricardo Dahis has kindly shared some data used here and provided helpful suggestions. I have benefited from comments from Dorisz Albrecht, Natalie Bachas, Leah Boustan, Rebecca Diamond, Hanming Fang, Hank Farber, Mateus Ferraz Dias, Stephanie Hao, Camille Landais, David S. Lee, Benjamin Olken, Micaela Sviatschi, two anonymous referees, seminar participants at Princeton University, and conference participants at the 2020 SBE and 2021 SOLE. I thank *Controladoria Geral da União* (CGU) for providing access to data. Any opinions and conclusions expressed herein are those of the author and do not necessarily represent the views of CGU or Princeton University. All remaining errors are mine.

the U.K., and the U.S. (Zhou et al. (2017), Cerrone et al. (2021)). Despite its popularity, empirical evidence on the consequences of debarment remains scarce.[2]

Debarment is designed to act as a deterrence mechanism by increasing the costs of engaging in corrupt behavior. In principle, debarment results in a severe punishment for corrupt companies by preventing them from obtaining procurement contracts from the public sector for a period of time. However, perhaps surprisingly, the impacts on workers have generally been overlooked.[3] Focusing on debarment, this paper sheds light on how corruption crackdown can have impacts on companies and workers.

I empirically study the labor market consequences of corporate debarment. Brazil provides an ideal setting for studying it for at least three reasons. First, the country is known for the massive burden of government regulation, opening room for corrupt practices by firms. In 2018, Brazil was ranked 136 out of 137 countries for burden of government regulation by the World Economic Forum. Second, Brazil has experienced dramatic changes that increased corporate liability for illicit behavior. In 2013, a large anti-corruption investigation uncovered an unprecedented and extensive kickback scheme to obtain government contracts. In June of 2013, thousands of Brazilians protested against corruption and poor public service provision, prompting the largest protests in two decades. Motivated by general anti-corruption sentiments, two months later, the Congress announced several measures to curb corruption. In particular, the Anti-Corruption Law was enacted, coming into effect at the beginning of 2014, and it substantially increased the costs of engaging in illicit practices for companies. Third, the availability of rich data allows me to characterize the impacts of debarment on firms and workers.

[2]Auriol and Søreide (2017) present a theoretical framework to conclude that debarment curbs corruption under certain conditions. Focusing on the U.S., Cerrone et al. (2021) empirically study how debarment affects procurement auctions by reducing collusion.

[3]Past research has shown the implications of sanctions on firm value using stock market returns (Ahn and Ludema (2020), Draca et al. (2019)) or on political outcomes (Marinov (2005), Allen (2008)). To my knowledge, no work has investigated the impacts of sanctions on labor market outcomes.

This paper takes advantage of important features of the Anti-Corruption Law. Public offices are required to register and update the debarment database, managed by the federal government, the Registry of Ineligible and Suspended Companies (henceforth CEIS). It is a public database that contains sanctions imposed on establishments and individuals convicted of engaging in irregular activities. Irregularities include, for instance, misconduct in bidding procedures, fiscal frauds and frauds in contracts with the public administration. Once the establishment is included in the CEIS data and officially debarred, it is no longer allowed to have contracts with public agencies until the sanctions expire. In some cases, debarred establishments may be required to pay fines. Some administrative sanctions can be disseminated by the mass media, broadening the information shock of debarment.

In the first part of the paper, I estimate the impacts of debarment on establishments. I employ the passage of the Anti-Corruption Law and leverage a unique and novel source of confidential data containing establishments that have been officially debarred due to illicit practices. I link the universe of establishments that are debarred between 2014 and 2016, immediately after the enactment of the new Law, to the Brazilian matched employer-employee data. The debarment data contains several characteristics of sanctions, such as their start and end dates. The labor market data provides complete coverage of all workers and establishments in the formal sector, including establishment size, paid earnings[4], workers' characteristics, dates when workers are hired and fired, and other variables.

To quantify how debarment affects establishments' performance, summarized by selected labor market outcomes, I adopt event study and matched difference-in-differences approaches exploiting variation in the timing of debarment events, defined as the year when establishments are included in the CEIS data, across establishments for identification. Using rich information at the establishment level, I apply a matching algorithm to construct a set of comparable establishments that are not debarred to form the control group. I then compare

[4]Throughout the paper, I use the terms "earnings" and "wages in the formal sector" interchangeably.

how the outcome variables evolve for debarred and control establishments around the time of debarment.

I find that, after debarment, excluded establishments experience, on average, a 47.7 percent decline in total number of employees and are 15.1 percentage points more likely to exit the formal sector. Total monthly payroll also decreases. I show that these effects are robust to alternative specifications and variable definitions. I also document that the employment effect is still negative and persistent for a sample of "establishment stayers", which consists of establishments that survive the debarment shock. I discuss three potential mechanisms that may explain the impacts on establishment aggregates. First, I investigate the role of fines, reflecting on labor market outcomes. I argue that not all debarred establishments are required to pay fines. Even for those that get fined, the maximum cap is too small to entirely explain my results. Second, the public nature of debarment suggests there is scope for reputational damage. Excluded establishments may struggle to retain or attract clients and workers or even secure credit. Third, debarment acts as a negative demand shock since excluded establishments are not allowed to obtain public contracts. Using detailed procurement data, I show that debarment is associated with lower propensity to both bid for and win procurement contracts from the federal government. I also document that the impacts of debarment are larger for establishments more dependent on government relationships.

In the second part of the paper, I use worker-level data to analyze how debarment affects workers' earnings. I implement a matching algorithm based on individual characteristics to create a control group of workers. I then adopt a difference-in-differences design that compares workers from debarred establishments to a matched comparison group of workers before and after debarment. The advantages of using worker-level data are twofold. First, the average earnings at debarred establishments declines in tandem with measures of employment. The aggregate impacts could, for instance, be confounded by compositional changes. Second, debarment may trigger changes in ownership or tax identifiers, making the results

difficult to interpret. The worker-level data mitigates these concerns by allowing me to track the same workers over time.

I document that debarment generates significant earning losses in the formal sector even three years after the event. On average, debarment is associated with a 22 percent decline in earnings. I show that debarment has negligible effects on earnings for workers that remain employed during the analysis period, suggesting that the bulk of earning losses are due to unemployment. While most groups of workers based on observable characteristics, such as gender, educational level or occupation, are negatively affected by the policy, I find some evidence that highly skilled and paid and more tenured workers experience relatively larger unemployment and earning losses.

In the last part of the paper, I scrutinize the role of reputation or information shocks in explaining the findings. Information on excluded establishments is easily accessible in a public and online database. In addition, some sanctions can be disclosed by relevant media, potentially magnifying the information shock of revealing illicit practices. To understand whether unemployment and earning losses are partly driven by reputational effects, I examine the labor reallocation of individuals who are separated from debarred establishments prior to the debarment. I track these workers over time to assess their labor market outcomes after debarment takes effect. This exercise allows me to rule out the direct effect of debarment related to performance and isolate the information shock.

I find some evidence that reputational effects contribute to unemployment and earnings losses. In particular, I find that debarment is associated with a 1.4 percentage point decrease in the probability of being employed and a 12.2 percent drop in earnings. These point estimates, however, are small and noisier relative to the benchmark results, suggesting that reputational damage plays only a secondary role in the findings. Instead, the establishment- and worker-level results can be interpreted as direct consequences of stricter penalties imposed by debarment.

These results have several policy implications. This paper suggests that the costs of debarment extend beyond the main target, the corrupt firms. Corruption crackdown also adversely affects workers, especially those who are highly skilled and paid and have higher tenure and even those who were laid off from the excluded firms before debarment, generating significant and negative impacts on labor market outcomes. This could lead to persistent earning losses in the long run (Lachowska et al. (2020)). While this paper does not quantify the benefits of debarment, it sheds light on the costs that corruption crackdown may induce from the labor market perspective, contributing to the debate over the most promising strategies to fight against corruption. My results underscore the need to fully consider the benefits and costs of these strategies.

This work contributes to three strands of literature. First, there is a large literature in Economics studying corruption (Shleifer and Vishny (1993), Shleifer and Vishny (1994), Svensson (2005), Glaeser and Saks (2006), Olken and Pande (2012)), particularly how corruption affects firm behavior (Mauro (1995), Svensson (1999), Kaufmann and Wei (1999), Fisman and Svensson (2007), Olken and Barron (2009), Sequeira and Djankov (2014), Smith (2016), Bai et al. (2017)). I complement this literature by focusing on workers who remain largely understudied.

Second, there are several papers studying approaches to fight against corruption (Svensson (2005)). Anti-corruption strategies include, for instance, private enforcement of public laws through lawsuits (Hay and Shleifer (1998)), improving citizen access to information and their power to monitor public service quality and officials (Reinikka and Svensson (2005), Reinikka and Svensson (2011)), and "hiring integrity" from the private sector (Yang (2005)). Several works have examined the effects of anti-corruption policies. In China, Lin et al. (2016) analyze how anti-corruption reform impacts shareholder valuations. Zeume (2017) assesses the causal effects of bribery on firm value by exploiting an anti-bribery regulation in the U.K. Karpoff et al. (2017) focus on the enforcement of the U.S. Foreign Corrupt Practices Act

(FCPA), which prohibits U.S. companies from paying bribes to foreign government officials. I build on this literature by tracing the impact of debarment, another popular anti-corruption instrument, on establishments' and workers' outcomes.[5] More broadly, I provide evidence that corruption crackdown affects the labor force.

In the Brazilian context, other studies have focused on another important anti-corruption policy: local government audits (Ferraz and Finan (2011)). Colonnelli and Prem (2017), Lagaras et al. (2017) and Bologna Pavlik and Harger (2018) study how an innovative audit program affects firms and local economies. This program randomly audits local governments to discourage misuse of public funds among public administrators and fostering civil society participation in the control of public resources (Ferraz and Finan (2008)). While the audit reports have information on firms involved in illicit practices with local governments, it does not necessarily imply that these firms are punished. For instance, Colonnelli and Prem (2017) find that corrupt firms listed in audit reports show better performance after the audit: they experience higher employment, sales and investment, supporting the "sand in the wheel" view of corruption.

On the other hand, debarment has a different purpose: consistent with the classical Becker (1968)'s model in which agents weigh the costs and benefits when deciding whether to engage in a criminal activity, debarment acts as a deterrence mechanism by making punishment more severe through exclusion from having contracts with the government or participating in public bidding procedures during a period of time. As a result, debarment is also expected to promote corporate governance and enhance integrity in the relationships between firms and the government.[6] The origin of debarment probably dates back to 1884,

[5]There is also another strand of literature focused on empirical evidence that negative reputation costs can be costly to firms (Karpoff et al. (2008), Murphy et al. (2009), Armour et al. (2017), Akey et al. (2021)), though the shocks studied in these papers are not necessarily related to corruption. In addition, these studies mostly focus on market values, not labor market outcomes.

[6]Conversations with government officials indicate that audit reports are one of the several sources that the federal government relies on to investigate potentially corrupt firms. Other sources to start an investigation include, for instance, media reports and whistleblowing.

when the U.S. Congress required several government contracts to be awarded to the lowest responsible bidder (West et al. (2006)). In 1928, the U.S. Comptroller General admitted the use of debarment as a preventive instrument. Over the next decades, other countries rarely used debarment as a sanction. Starting in the mid-1990s, motivated by the growing international interest in corruption, debarment rapidly gained popularity among national governments and international agencies concerned about suppliers exploiting institutional weaknesses in developing countries (Auriol and Søreide (2017)). Several examples of countries and agencies that adopted debarment policies over the last two decades include the World Bank, Bangladesh, China, Japan, India, Indonesia, Nigeria, Pakistan, and Vietnam. Despite its popularity, there is little evidence on the consequences of debarment. My findings complement the recent literature analyzing the consequences of debarment (Auriol and Søreide (2017), Cerrone et al. (2021)) and, more broadly, the literature on policy instruments designed to reduce corruption.

Third, this paper speaks to a strand of literature related to earnings losses of displaced workers (Jacobson et al. (1993), Sullivan and Von Wachter (2009), Von Wachter et al. (2009), Couch and Placzek (2010), Von Wachter et al. (2011), Lachowska et al. (2020)) exploiting similar empirical methodology. I also provide suggestive evidence that corruption crackdown constitutes an important source of workers' earnings losses through higher unemployment by comparing the estimates to the displacement literature.

This paper is organized as follows. Section 2.2 describes the institutional context, including the creation of CEIS and the Anti-Corruption Law. Section 2.3 outlines the data and the matching algorithm I use to construct the samples of interest. Section 2.4 delineates the empirical strategies and Section 2.5 presents the main results. In Section 2.6, I offer some concluding remarks.

2.2 Institutional Context

2.2.1 The Creation of CEIS Database

Corruption has long been a concern in Brazil. In light of this problem, several anti-corruption measures have been implemented as attempts to halt corrupt practices. In 2003, the federal government created the Office of the Comptroller-General (*Controladoria Geral da União* – CGU), an autonomous federal agency responsible for conducting internal control activities and public audits and implementing corrective and disciplinary measures to prevent and combat corruption, among other duties (Morosini and Vaz Ferreira (2014)).

In December of 2008, following efforts to enhance transparency and improve debarment enforcement in public administration, CGU launched a new and public database, CEIS.[7] It was initially conceived to be a public database compiling information from various sources to disclose establishments who have been sanctioned and debarred for being engaged in illicit practices in bidding procedures, fiscal frauds, or frauds in contracts held with public administration.[8] CGU became responsible for consolidating and disclosing the list of individuals and establishments with valid debarment sanctions imposed by public entities after going through a rigorous process of investigation and condemnation.

Debarment sanctions spanned temporary suspension of participation in bidding, impediment of contracting with public administration and declaration of inability to bid or contract with public administration. In practice, these sanctions implied that punished companies were excluded from having contracts with any public entity or participating in public bidding

[7]The database was launched in December of 2008 to celebrate the International Anti-Corruption Day without any regulation in place. CGU only released the first regulation formally establishing CEIS and its functioning in March of 2010 (*Portaria CGU 516*).

[8]These illicit practices are typically characterized as non-compliance with one of the following regulations: Law 8,666 (enacted in June of 1993), which establishes general rules about public bids and contracts; Law 10,520 (enacted in July of 2002), which regulates the e-procurement system; Law 8,429 (enacted in June of 1992), which targets misconduct in public office; Law 9,504 (enacted in September of 1997), which governs the funding of political parties and campaigns; and Law 8,443 (enacted in July of 1992), which establishes penalties for any damage to the public treasury, like frauds in public tenders.

procedures during the sanction period. Although these debarment sanctions already existed before 2008, the creation of CEIS database disclosing information on punished companies helped to centralize and publicly reveal companies engaged in illicit practices. For simplicity, throughout the paper, I refer to ***corporate debarment or blacklisting*** as an event in which companies are included in the CEIS database.

Until the Anti-Corruption Law was enacted, public entities were not required to keep the database updated. The new legislation was also a watershed in terms of both legal compliance and corporate liability for corrupt practices.

2.2.2 The Anti-Corruption Law

In the first half of 2013, an investigation revealed a large kickback scheme involving private firms and government contracts. Various members of the business and political elite were investigated and convicted for corrupt practices. In June of 2013, Brazil experienced an unprecedented wave of protests against corruption and bad provision of public services (Aguilar and Ferraz (2014)).[9] In response to these complaints, the federal government enacted the Anti-Corruption Law (Law 12,846/2013) in August of 2013, imposing new measures to combat corruption, inspired by the international trend of anti-corruption efforts.[10,11] The new Law came into effect in January of 2014.

Important to the paper, the Anti-Corruption Law mandated that all public entities from all government branches (executive, judicial, and legislative) and levels (federal, state and local governments) must register and update all debarment sanctions in the CEIS database.

[9]Other sources of dissatisfaction included, for instance, police brutality, excess spending on the 2014 World Cup and poor public service delivery.

[10]For instance, the Anti-Corruption Law holds individuals giving out bribes equally responsible as public officials receiving them, recognizing corporate liability for bribery. Companies may be punished, rather than individuals, implying that firing an employee is not enough to rid the company of responsibility. Anecdotal evidence suggests that firms are investing more in compliance.

[11]International examples include stricter enforcement of the Foreign Corrupt Practices Acts (FCPA) in the U.S., as well as countries like China and the United Kingdom enacting new anti-corruption measures or requiring a more stringent enforcement of existing regulations.

Indeed, before 2014, there were very few records from several state governments, such as Distrito Federal, Rio de Janeiro, and Rio Grande do Sul. These states started to actively update the CEIS database with appropriate information only after the Anti-Corruption Law.[12] The range of possible penalties also expanded. Some firms, for example, were required to pay fines. In practice, exclusion from government contracts still constitutes the main punishment. These penalties can be applied through judicial or administrative decisions. In the case of judicial decisions, the judge is expected to send an official letter to CGU requiring both the firm and the sanction to be registered in the CEIS database once the decision becomes final and unappealable.

The administrative process, on the other hand, is conducted in a different way. The first step generally consists of media reports, whistleblowing, audit reports or other sources indicating possible irregularities by a firm or an individual. If there is enough evidence of wrongdoing, there is a thorough investigation and the accused has the right to defend himself against the charge. The main penalty, if applicable, takes force by the end of the short administrative process, which cannot last more than six months. In few cases, firms may also be required to pay a fine up to 20 percent of their gross revenues or up to 60 million *reais* (about USD 15 million) when calculating revenue is not possible, and the sanctions must be disseminated by the mass media, such as government gazettes or relevant newspapers based on circulation. Convicted firms and individuals are then registered in the CEIS database. After serving the sentence, debarment is revoked and the firm may participate in bidding procedures again.

[12]The CEIS database contains the start date of debarment, along with states and government branches and levels, allowing me to notice different patterns before and after 2014. For instance, considering the universe of establishments punished by state governments, Rio de Janeiro accounts for 1.8 (7.73) percent of the cases between 2008 and 2013 (2014 and 2018). I also observe similar changing patterns after the Anti-Corruption Law for other states.

2.2.3 Information Disclosure

The public nature of debarment is an important feature of this paper. Government gazettes and relevant newspapers are examples of media sources through which administrative sanctions are disclosed.[13] Although there is no direct evidence demonstrating that individuals learn about debarment events as the CEIS dataset does not have information on media sources utilized to reveal the sanctions, anecdotal evidence indicates that the information on banned firms is widespread. I include some examples illustrating how this information diffused to the public. For instance, an article from the online portal *O Globo*, part of the biggest media conglomerate in Latin America, states that: "The Company X was registered by CGU in the CEIS database. The company — suspect of participating in the subway and train cartel in São Paulo and Brasília — has been barred from contracting with the federal government due to alleged bribery payments in *Correios*[14] procurements." (*O Globo*, 03/07/2014).

Other prominent examples come from *Folha de São Paulo*, the second most popular newspaper: "The number of ineligible and suspended companies and individuals by CGU increased by 27.3 percent in 2016. The main penalty consists of being prevented from signing contracts with public administration (...). This happens when frauds are detected from their relationships with federal, state or municipal governments or state-owned companies." (*Folha de São Paulo*, 12/22/2016). In March of 2016, another article announced that "CGU published the decision to ban another contractor (...) from contracting with public administration (...). It was proven that Y coordinated actions with other competitors to reduce competitiveness in several bidding processes by combining prices and winners (...). It was

[13]Despite the general requirement that administrative sanctions should be disseminated by the mass media, the legislation provides little guidance on where and how to disclose such sanctions. In addition, the CEIS platform does not require public entities to inform whether and which media was used to reveal the sanctions levied on punished companies. Official government gazettes (e.g. *Diários Oficiais*) and relevant newspapers are some examples of media sources for announcing the sanctions.

[14]*Correios* is a state-owned Brazilian company that operates the national postal service.

demonstrated that the company paid bribes to public agents amounting to 3 million *reais* (equivalent to USD 0.75 million) to sign a contract with *Petrobras*[15] (...)." (*Folha de São Paulo*, 06/09/2016).

These articles from relevant media outlets suggest that information on excluded firms may have reached the public. In addition, this paper focuses on debarment events from 2014 to 2016, a period coinciding with Operation Car Wash, launched in March of 2014 and still ongoing, which uncovered a vast corruption scheme involving corporations and politicians and resulted in unprecedented thousands of arrest warrants against former presidents of the Republic, Congress and Senate members, state governors, and businessmen. Several debarred companies were convicted of corruption charges triggered by this investigation, gaining massive media coverage in Brazil. Google web searches about the corruption scandal indicate that citizen interest in the topic has substantially increased. According to CGU, the number of online accesses to the CEIS platform jumped from 82,837 in January of 2011 to 459,161 in December of 2017, reaching a record high of nearly 600,000 visits in August of 2017.

2.3 Data

This paper uses several data sources. First, I use confidential debarment data provided by CGU. Second, I utilize matched employer-employee records covering the formal sector, the *Relação Anual de Informações Sociais* (RAIS). Third, I gather information on online bids and winners of federal procurement contracts.

2.3.1 The Debarment Data

The confidential CEIS dataset (Controladoria Geral da União (2019)) includes all establishments that have been debarred since its inception. It contains detailed information on

[15] *Petrobras* is a semi-public Brazilian multinational corporation in the petroleum industry.

names and tax identifiers, types of sanction, start and end dates of sanction, and government agencies responsible for applying the penalties.

I make some restrictions to the CEIS dataset. Because debarment sanctions target establishments, not necessarily firms, I begin by restricting the analysis to establishments, as the original data also include punished individuals. To avoid duplicate observations, I maintain the earliest sanction each establishment has. Next, I remove establishments that had cancelled sanctions. Figure 2.1 shows the evolution of the number of debarred establishments. There is a clear increasing trend starting in 2008, when CEIS is created, and reinforced by the Anti-Corruption Law in 2014.

I then limit the sample to establishments whose initial years of debarment are between 2014 and 2016 for three main reasons. First, measures implemented by the Anti-Corruption Law began to take effect in 2014. Prior to this year, public entities were not required to list excluded establishments in the CEIS data, raising concerns about selection bias. For instance, state governments from Distrito Federal, Rio de Janeiro and Rio Grande do Sul only started to update the CEIS database with information on excluded establishments in 2014. Table 2.A.1, Appendix A, displays summary statistics using information from CEIS data and highlights several patters. In addition to some differences in the geographic composition of debarment cases before and after 2014 (Columns (3) and (5)), we notice that the average length of punishment prior to 2014 is 29.13 months, considerably higher than the average of 19.93 months after 2014, indicating selection of more severe sanctions getting reported to the CEIS platform before the Anti-Corruption Law. Second, the institutional context discussed in Section 2.2.3 suggests a higher public awareness of corruption after 2014.[16] Third, data on online bids and winners of federal procurement contracts, essential to probe the mechanisms behind the main results, are only available since 2013. Therefore, to

[16]In a simple Becker (1968)'s framework applied to corruption, the Anti-Corruption Law could deter more establishments from engaging in corrupt practices through more severe or certain punishments or through higher opportunity cost of corruption. By requiring debarred establishments to be included in the CEIS database, public agencies no longer have discretion to choose who gets reported, increasing the likelihood of effective punishment. A higher public awareness of corruption since 2014 may raise the opportunity cost of

facilitate the interpretation of the results, I restrict the sample to establishments that have been debarred since 2014. In Section 2.5.4, I revisit this sample restriction by expanding the analysis to include establishments that have been debarred before 2014.

Because my goal is to track these establishments in labor market data at least two years later after initial debarment and these data are only available until 2018, I drop establishments that have been debarred since 2017. These restrictions leave me with a sample of 6,862 establishments[17] to be matched with employer-employee data through tax identifiers.

2.3.2 Labor Market Data

I extract labor market information from RAIS (Brazilian Ministry of Labor and Employment (2019)), the Brazilian matched employer-employee data that the Ministry of Economy collects each year. It provides a comprehensive and high-quality overview of the entire formal sector (Dix-Carneiro (2014)). To incorporate three years before debarment into the main analysis, I use annual RAIS data for the period between 2011 and 2018, the last year in which the data are available.[18] In all years, each entry consists of detailed worker-level information on demographic characteristics (e.g., gender, age and educational level), average earnings in the year, number of hours worked, occupation, admission date, along with other variables. Establishment-level information include, for instance, industry, establishment size (in terms of number of employees), and municipality and state where the establishment is located.

I have gained access to worker- and establishment-level tax identifiers, which are unique and do not change over time. Identified data has two advantages. First, because the CEIS data also contain establishment identifiers, I can match debarred establishments to RAIS

corrupt practices (e.g. through relatively lower profit streams). Therefore, the degree that debarment can affect establishments may differ during the years before and after the Anti-Corruption Law.

[17]The same sample has 6,823 firms. The data allows me to distinguish between establishments and firms. However, I restrict my analysis to establishments since most firms have only one establishment. In Section 2.5.4, I show that the results are quite robust to the analysis at the firm level.

[18]Section 2.5.4 uses RAIS data for the period between 2002 and 2018.

and recover labor market information. Second, I can track workers over time and across establishments.

Although the RAIS data are incredibly detailed in some regards, there are important caveats to mention. RAIS includes only formal workers, lacking information on employees who are out of the labor force, informally employed, unemployed, or self-employed (Dix-Carneiro and Kovak (2017)). This is an important limitation in a country where about 40 percent of all employed workers were in the informal sector in 2017. It is not possible, for instance, to know whether a missing record is due to unemployment/non-participation or the worker being in the informal sector.

I match the final list of 6,862 debarred establishments to RAIS data from any year between 2011 and 2018. I then drop observations without any employee in all three years before initial debarment, leaving a sample of 3,294 establishments to implement the matching algorithm.[19]

2.3.3 Minor Sources

While the labor market data are the primary source of data, I rely on two minor sources to complete the main analysis. First, for the matching algorithm, I obtain municipal population estimates from the 2010 Demographic Census, carried out by IBGE (Instituto Brasileiro de Geografia e Estatística (2010)). Second, I use information on online bids and winners of federal procurement contracts from *Portal de Transparência* and *API de Compras Governamentais* websites, regularly updated by the federal government, for the years between 2013 and 2018 (API de Compras Governamentais (2019), Portal da Transparência (2019a), Portal da Transparência (2019b)).

[19]Columns (1) to (10) of Table 2.A.1, Appendix A, display summary statistics for establishments facing debarment using information from CEIS. I use four distinct samples: the original sample of debarred establishments (Columns (1) and (2)), after restricting to establishments that have been debarred between 2008 and 2013 (Columns (3) and (4)), after restricting to establishments that have been debarred between 2014 and 2016 (Columns (5) and (6)), after matching post-2014 establishments with RAIS (Columns (7) and (8)), and after implementing the matching algorithm described in Section 2.3.4 (Columns (9) and (10)).

2.3.4 The Matching Procedure

I construct two samples for the main analysis, which I refer as the establishment and worker samples.

Establishment Sample. In order to create a suitable control group for debarred establishments, allowing me to estimate counterfactual trends, I implement a matching procedure using the labor market, municipal population and procurement data. Columns 1 and 2 of Table 2.1, together with Table 2.A.2, Appendix A, indicate sizable differences between debarred and non-debarred establishments. On average, debarred establishments are larger in terms of employment and monthly payrolls[20], are concentrated in specific sectors, such as construction, are more likely to bid for procurement contracts with the federal government, and are located in more populated municipalities.

The first control group uses each of the three years before official debarment — $[t-3, t-1]$ — and matches each debarred establishment to a counterfactual establishment from the same two-digit industry[21] and state with the closest propensity score, in which is estimated by predicting treatment using a linear probability model with the following set of variables: annual distributions of total employment and average earnings for the three years before debarment, whether the establishment has bid for and won procurement contracts with the federal government in the three years before debarment, whether it is a single establishment, and size of municipality where the establishment is located.[22] I compute deciles of the employment and average earnings distributions considering the universe of all

[20]Earnings are adjusted to 2018 Brazilian *reais* using inflation index from Central Bank of Brazil (2021).

[21]Appendix B includes a list of 17 industries generated for the analysis.

[22]I aggregate population information into seven groups: less than 5,000 inhabitants; equal or more than 5,000 and less than 10,000; equal or more than 10,000 and less than 20,000; equal or more than 20,000 and less than 50,000; equal or more than 50,000 and less than 100,000; equal or more than 100,000 and less than 500,000; and more than 500,000. Numbers are drawn from the Demographic Census in 2010, the most recent year. While information on annual population are available, they mostly consist of estimates and are more sensitive to measurement errors. The Census is the most reliable source of population records.

private establishments in the RAIS data in each of the years.[23] In Appendix B, I describe the data and the matching procedure in detail. In Section 2.5.4, I show that the main results are robust to alternative ways of constructing the matching algorithm. In particular, I document that the the main conclusions are similar to matching exclusively on the year before official debarment rather than the pre-event trends, to relying on coarsened exact matching (Iacus et al. (2012)) instead of a propensity score matching, and to allowing multiple control units for each treated establishment.[24]

The matching strategy is similar to other papers using a matched difference-in-differences design (Jäger and Heining (2022), Arnold (2020), He and Le Maire (2020)) and is essential to the first part of the analysis because it recovers a counterfactual group of establishments that have similar pre-event trends but do not face debarment. With the control group in hand, I can document how debarment affects selected outcomes. Starting from a sample of 3,294 debarred establishments, the above matching routine returns 3,179 establishments as the control group to be used in the main analysis.

Worker Sample. I take a few steps to define the sample of workers. First, I recover all individuals that worked in a establishment for the three years ($[t-3, t-1]$) before it was debarred. I apply a tenure restriction to extract workers that are more attached to companies since Brazil has substantial turnover in the labor market. I also find similar results when alternatively considering individuals that have worked in a establishment in the year prior to debarment. The tenure restriction, however, offers an additional advantage of following the displacement literature (Jacobson et al. (1993), Couch and Placzek (2010), Lachowska

[23] I exclude public administration establishments from the sample because they are subject to a different legal framework, and their workers are employed under different contracts and are entitled to special benefits (Arnold (2018)).

[24] Because multiple candidates may arise for each treated unit, I force the main algorithm to generate one candidate for each treated establishment. It avoids weighting issues when splitting the sample to estimate heterogeneous effects. I also ensure that potential control establishments are not associated to more than one debarred establishment. In Section 2.5.4, I relax this restriction by allowing up to three and five control units for each treated establishment and find that the results are not sensitive to choosing one or multiple counterfactual units.

et al. (2020)) and allowing to benchmark my results to this literature. Second, to keep the analysis comparable, the set of debarred establishments to which workers are associated is the same as in the establishment-level analysis. Third, to get candidates to form a comparison group of workers, I generate a sample of workers that have worked at least three years in establishments that have never been debarred.

For each worker in treated establishments, I select the comparison worker with the closest propensity score, which is calculated by predicting treatment using a linear probability model with the following set of worker characteristics in $t-1$: age bins (5 years age bins), indicator for male, indicator for disability, indicator for white, educational level, two-digit industry, and occupation categories.[25] I do not explicitly match workers based on earnings since this is one of the outcomes of interest. Nonetheless, an alternative algorithm that also includes deciles of earnings distributions yields similar results. These restrictions altogether yield a sample with 81,333 workers in the treated group.

2.3.5 Summary Statistics

Establishment Sample. Table 2.1 presents summary statistics for both groups — debarred establishments and the comparison group — using labor market data in all three years prior to debarment. Punished establishments have, on average, 164 employees, while the control group has an average of 114 workers. The average earnings in debarred establishments are similar (1,823 Brazilian *reais*, equivalent to USD 347) to the comparison group (1,958 Brazilian *reais* or USD 373). The two samples are also similar in terms of having bid for a procurement contract from the federal government, location and industry. We notice that the matching algorithm does not fully eliminate differences between debarred and non-debarred

[25]Educational categories are grouped into eleven categories: illiterate, incomplete primary education, primary education, incomplete lower secondary education, lower secondary education, incomplete upper secondary education, upper secondary education, incomplete tertiary education, tertiary education, Master degree, and PhD degree. For occupation, I use the 2002 Brazilian Classification of Occupations (CBO), which classifies jobs based on their skill and task content to construct four occupation categories: managerial, professional, blue collar, and white collar lower level positions.

establishments. My identification strategy, however, does not require balance on covariates. It relies on parallel trends between both groups of establishments before debarment.

Worker Sample. Table 2.2 displays summary statistics for workers with three years tenure in the year immediately prior to debarment for both treated and control groups. Table 2.2 confirms that both groups of workers are comparable along observable dimensions, such as gender, disability, firm size, education, age, occupation, and tenure, reinforcing that the matching algorithm does a good job finding a balanced control group. The main difference comes from earnings. On average, in the year before debarment, workers in punished establishments earn about 2,256 Brazilian *reais* (equivalent to USD 430) per month, which is less than control workers' earnings (2,572 Brazilian *reais* or USD 490). In addition, nearly 62 (63) percent of treated (comparison) workers are male, 83 (81) percent are employed in large establishments with at least 100 employees, 10 (11) percent have a college degree, and 3 percent hold a managerial position.

2.4 Empirical Strategy

2.4.1 Establishment-Level Analysis

In the first part of the empirical analysis, the goal is to estimate the reduced form effects of debarment on establishments. I adopt an event study approach based on when establishments are registered in the CEIS database. For each punished establishment in the analysis sample, I define the year before debarment as $t = -1$, and all remaining years are indexed relative to that year.

To assess the impact of debarment, I estimate the following matched difference-in-difference model using the establishment-level sample:

$$y_{jft} = \sum_{k=-3}^{k=2} [\beta_k \times \mathbf{1}(t_j = t^* + k) \times Debarment_{jf} + \theta_k \times \mathbf{1}(t_j = t^* + k)] + \alpha_j + \alpha_t + \varepsilon_{jft}, \tag{2.1}$$

in which subscripts j, f, and t stand for establishment, firm, and year; $\mathbf{1}(t_j = t^* + k)$ are dummies indicating a debarment event in year k relative to the debarment year t^*; $Debarment_{jf}$ is an indicator variable for debarred establishments; α_j are establishment fixed effects; α_t are year fixed effects; and y_{jft} is the outcome of interest. Year fixed effects control for common shocks affecting the establishments each year. Establishment fixed effects control for time-invariant characteristics of establishments that might be correlated with the outcomes of interest and their inclusion in the CEIS database. Standard errors are clustered at the firm level.[26]

2.4.2 Worker-Level Analysis

To further understand how debarment affects workers' earnings, I estimate the following specification using the worker-level data:

$$y_{it} = \sum_{k=-3}^{k=2} [\beta_k \times \mathbf{1}(t_i = t^* + k) \times Debarment_i + \theta_k \times \mathbf{1}(t_i = t^* + k)] + \alpha_i + \alpha_t + \varepsilon_{it}, \tag{2.2}$$

in which subscripts i and t stand for worker and year; $\mathbf{1}(t_j = t^* + k)$ are dummies indicating a debarment event in year k relative to the debarment year t^*; $Debarment_i$ is an indicator variable for individuals that have worked in debarred establishments in the past three years before the event; α_i are worker fixed effects; α_t are year fixed effects; and y_{it} is the labor

[26]Since the data have both establishment and firm identifiers, I also implement a firm-level analysis. In Section 2.5.4, I show that establishment- and firm-level results generate similar estimates.

market outcome of interest. Standard errors are two-way clustered both at the worker and pre-event firm levels.

From Equations (2.1) and (2.2), the post-event coefficients of interest — β_k — capture the dynamics effects of debarment relative to the year before the event. Identification in Equation (2.1) (Equation (2.2)) relies on the timing of debarment being uncorrelated with the outcomes of interest, *conditional* on establishment (worker) and time fixed effects. The key identifying assumption is that establishments' (workers') outcomes for treated and control establishments (workers) would have followed parallel trends in $k > 0$ if no debarment had occurred for treated establishments (workers). I test this assumption by assessing whether the pre-event coefficients of interest are statistically indistinguishable from zero.

One possible concern with the identifying assumption is that the debarment treatment may be correlated with other unobservable and concurrent shocks, confounding the estimated effects. For instance, debarment may result in a loss in political connections during election years, and thereby affecting the outcomes of interest. The context implies that establishments are debarred at different years in calendar time. Thus, it is unlikely that these shocks only affect debarred establishments and their timing exactly coincide with the timing of debarment, mitigating concerns related to unobservable shocks affecting the coefficients of interest.

2.5 Results

2.5.1 Impacts on Establishments

Figure 2.2 displays $\hat{\beta}_k$, along with 95 percent confidence intervals, after estimating Equation (2.1) for selected labor market variables. The treatment and control groups present similar trends in the years before debarment. Indeed, for all outcomes, the pre-event coefficients are statistically equal to zero, lending support to the parallel trends assumption. The

lack of pre-trends indicates that the matching procedure is effective, since it is designed to mitigate pre-trend differences.

Figure 2.D.4a shows the point estimates for log employment.[27] Immediately after debarment, there is a sharp and significant decline in log employment, a pattern that is even stronger in the subsequent years. In the year of debarment, log employment falls by -0.378 (SE = 0.030). The coefficient grows in absolute value to -0.710 (SE = 0.039) and -0.856 (SE = 0.044) in the next two years. As shown in Column 1 of Table 2.3, the point estimate summarizing the average impact in the first three years after debarment is -0.649 (SE = 0.035), equivalent to a 47.7 percent $((100 * [\exp(-0.649) - 1]))$ decline in employment.[28]

Figure 2.D.4b presents the results when the outcome variable is an indicator for exiting the formal sector, which is equal to one if the establishment does not have any formal employee in the given year. The average impact including the two years after debarment shown in Column 2 of Table 2.3 indicates that punished establishments are 15.1 percentage points (p.p.) more likely to exit the formal sector following debarment.

I also document the event-study estimates for unconditional log earnings in the formal sector. The term "unconditional" means that I use the full sample, regardless of whether the firm exits the formal sector after debarment. Figure 2.D.4c shows that, in the debarment year, average log earnings fall by -0.582 (SE = 0.058) in punished establishments relative to the control group. The coefficient summarizing the decline reaches -1.582 (SE = 0.107) two years later. The average effect on log earnings is -1.147 (SE = 0.078), a 68.2 percent decrease. These findings are unsurprisingly mechanical as debarment is associated with

[27] To deal with zeroes in the data, I add one to the outcome variables so that log is defined for all establishment-year pairs. I also probe the robustness of my results by using the variables in levels and by applying inverse hyperbolic sine transformations to the variables.

[28] To obtain the aggregate estimates, I run the following specification:

$$y_{jft} = \alpha_j + \alpha_t + \beta \times PostDebarment_{jf} + \varepsilon_{jft} \tag{2.3}$$

in which the subscripts and the set of fixed effects are the same as in Equation (2.1), and $PostDebarment_{jf}$ is an indicator variable equal to 1 for all years after debarment in debarred establishments. As before, standard errors are clustered at the firm level.

higher probability of exiting the formal sector. Figure 2.D.4d and Column (4) of Table 2.3 report the findings for log monthly payroll, also suggesting substantial decline.[29]

I next consider an alternative sample with establishment stayers that have at least one employee in all periods in the window of [-3, 2] years around the debarment event and replicate the above exercises. Figure 2.C.2 and Table 2.C.1, both in Appendix C, depict the point estimates. Following debarment, the average log employment, earnings, and monthly payroll decreases by -0.180 (SE = 0.024; about 16.5 percent), -0.066 (SE = 0.016; around 5.92 percent), and -0.264 (SE = 0.034; nearly 23.2 percent), respectively. The impacts are more modest, albeit still large and statistically significant. I interpret these findings as evidence that the overall negative effects still exist even after excluding establishments that exit the formal sector or attempt to escape from sanctions by creating new tax identifiers.

In Table 2.C.2, Appendix C, I implement additional checks to confirm that the results are robust to alternative variable definitions. In Columns (1) and (2), I show that the main conclusions regarding employment effects remain roughly the same when considering total number of employees as the outcome variable and applying its inverse hyperbolic sine transformation. Columns (3) through (6) repeat this exercise by including absolute earnings per worker and monthly payroll as the dependent variables. Columns (1) through (4) of Table 2.C.3, Appendix C, include municipality-specific trends to control for any policy or unobservable shocks specific to municipalities. The point estimates are similar. Columns (5) through (8) alternatively add 2-digit industry-specific trends, and the estimated coefficients do not change. I also document that the aggregate impacts mask substantial heterogeneity across sectors and sizes (in total employment). Tables 2.C.4 and 2.C.5, Appendix C, show that the effects are predominantly concentrated in larger establishments and in some sectors, such

[29]The analysis is restricted to the formal sector, which is a limitation considering the context with a large informal sector. Laid-off individuals are not necessarily unemployed or out of the labor force as some of them may be employed in the informal sector. Unconditional log earnings impute zero earnings to individuals outside the formal sector.

as real estate and construction, though establishments from all sectors and sizes experience significant impacts.

I next discuss three explanations behind the main findings. Disentangling these mechanisms has relevant policy implications and offers insights on the relationship between corruption crackdown and firm performance. First, debarred companies may be financially constrained by the fines imposed on them, reflected in the reduction of tangible costs, such as price and quantity of labor inputs. The data unfortunately has very few observations with information on fines to bring statistical power to investigate this channel.[30] In addition, the maximum cap for fines is set at 20 percent of annual gross revenues. This ceiling may be too low to entirely explain the drop in labor market outcomes. Second, the public nature of debarment may generate an information shock for punished companies. For instance, they may struggle to retain or attract clients and workers or even secure credit after being debarred. I partially test for this mechanism in Section 2.5.3 by focusing on workers, accounting for the limitation that I do not have data on sales performance or loans to observe other margins of response.

Third, government agencies are relevant buyers. In my sample, 62 percent and 51 percent of establishments bid for or won procurement contracts with the federal government between 2013 and 2018, respectively. This high proportion suggests that debarment has the potential to induce a negative demand shock for punished companies. I empirically test for this channel in two ways using the procurement data.[31] First, I estimate Equation (2.1) using indicator variables for whether the establishment bids for and wins a procurement contract with the federal government. Figure 2.C.1 and Table 2.C.6, Appendix C, point to a sizable decline by 4.7 p.p. and 6.1 p.p. in the likelihood of bidding for and winning

[30]Only 0.42 percent of observations from the CEIS data before sample restrictions have information on fines.

[31]The data only covers a subset of government purchases, missing information on purchases from other government agencies, such as state and municipal governments. To my knowledge, there is no centralized data on purchases from all government agencies in Brazil.

contracts following debarment.[32] Second, I test whether the impacts on establishments are stronger among industries with more intense connections with the government. Considering the universe of federal contracts awarded in 2013, the first year of the data, I compute the distribution of total value of contracts by 2-digit industry and define industries above the median as those with stronger connections with the government. I also construct an alternative measure for government dependence for each establishment calculated as the ratio of total revenues obtained from federal government contracts and the annual payroll in 2013. I define establishments strongly dependent on the government as those having at least 25 percent of payroll expenses covered by revenues from federal government.[33] Table 2.C.7, Appendix C, indicates that the estimates are slightly larger among establishments more connected with the federal government. Both Tables 2.C.6 and 2.C.7 provide support for the role of demand shocks in explaining the previous results.

The employment effects shown here are substantially larger than the ones documented by the literature examining the role of demand shocks in firm growth (Ferraz et al. (2015), Pozzi and Schivardi (2016), Atkin et al. (2017), Cho (2018), Gugler et al. (2020)). Debarment constitutes a negative demand shock that is qualitatively different from other positive demand shocks typically documented in the literature in several ways.[34] In addition to the scope for reputation effect in a debarment shock, the procurement market usually has a sizable learning component as firms that win government contracts are likely to participate in future bidding processes with higher values and sell a broader set of products in other markets (Ferraz et al. (2015)), leading to an increasing dependence on government contracts over time. Therefore, the exclusion from government contracts and public bidding proce-

[32]Interestingly, for establishment stayers, Columns (2) and (4) of Table 2.C.6, Appendix C, indicate smaller effects, suggesting that they are less affected by this shock.

[33]The results are not sensitive to this threshold definition.

[34]For instance, Cho (2018) shows that firms who were awarded government contracts, grants, and loans through the American Recovery and Reinvestment Act of 2009 experience a 3.5 increase in total employment. In the context of procurement contracts, Gugler et al. (2020) document that winning a government contract in Austria leads to a 3 percent increase in the workforce. Ferraz et al. (2015) find that winning at least one contract immediately increases firm growth by 2.2 percentage points in Brazil.

dures is expected to generate even larger impacts on firm outcomes in comparison to winning government contracts. Another difference is that corruption may act as a tax on companies to conduct business (Shleifer and Vishny (1993), Olken (2007), Colonnelli and Prem (2017)), and corrupt establishments are likely to have extracted corruption rents prior to debarment to survive and grow. As a result, the exclusion from government contracts forces them to return to levels reflecting their quality in the absence of corruption taxes. Although this can be interpreted as a negative demand shock, it is qualitatively different from other typical demand shocks examined in the literature.

2.5.2 Impacts on Workers

The impact of debarment on workers' earnings is of interest. It is difficult to interpret establishment-level results because companies may alter the composition of their employees in response to shocks. In this case, impacts on earnings may simply reflect compositional changes rather than changes in earnings for similar workers. To circumvent this difficulty, I examine the impacts of debarment on earnings using worker-level data. Instead of using establishments as the unit of analysis, I estimate Equation (2.2), which directly compares workers attached to debarred establishments to a matched control group before and after debarment.

Figures 2.3a – 2.3c present results for the dynamics of workers' employment and earnings around debarment shocks. The pre-event coefficients are statistically equal to zero for all outcomes, validating the event-study strategy. Figure 2.3a depicts the employment estimates, indicating that, in comparison to the control group, workers from debarred establishments experience a sharp decrease in the probability of being employed in the formal sector after debarment, regardless of whether they still work in punished establishments. The average post-event coefficient in Column (1) of Table 2.4 suggests a decline of 3.4 p.p. (SE = 0.014).

Figure 2.3b shows the event-study estimates for log earnings in the formal sector using the full sample. Column (2) of Table 2.4 documents that workers from debarred establishments experience a a relative decline in log earnings by -0.247 (SE = 0.096), equivalent to a 21.9 percent decrease in earnings relative to counterfactual earnings after debarment. This finding is unsurprising as workers are also more likely to not be employed in the formal sector and, therefore, are assigned zero earnings. Conditioning the sample to observations with positive earnings, Column (3) indicates that the point estimate is no longer statistically significant, reinforcing the role of unemployment in the earnings results.[35]

To better understand the magnitude of the earning losses of 22 percent, I compare it to the worker displacement literature. For example, Jacobson et al. (1993) document earnings losses of about 40 percent during the first year of displacement using a sample of displaced workers in Pennsylvania. Couch and Placzek (2010) and Lachowska et al. (2020) find lost earnings of 33 and 45 percent at the time of displacement in Connecticut and Washington State. During the recession in the U.S., Davis and Von Wachter (2011) estimate a 39 percent decline in earnings at the year of displacement. In the Brazilian context, Bhalotra et al. (2021) document a 42 percent decline in labor income up to three years following displacement for workers displaced in mass layoffs between 2012 and 2014. In this paper, I show that the earning losses of 22 percent from debarment are computed from a sample of workers regardless of being displaced from punished establishments and that debarment leads to 47.7 percent decline in employment. Therefore, the earning losses are strikingly comparable to the displacement literature, supporting the interpretation that unemployment plays a major role.

Turning to heterogeneous impacts based on workers' characteristics to understand potential distributional implications, I investigate whether some groups of workers are dispro-

[35]In Table 2.C.8, Appendix C, I perform several robustness checks, confirming that the results are robust to alternative definitions of the outcome variables and additional controls.

portionately affected by this anti-corruption instrument.[36] Table 2.C.9, Appendix C, reports the estimates across different characteristics, including gender, race, skill (measured by level of education), age groups, occupation, tenure, and wage distribution. While all groups of workers are severely hit by debarment, I find some evidence that managers (Panel F), workers with longer tenure (Panel G) and highly paid employees (Panel H) display higher unemployment rates and earning losses, bearing higher costs of corruption crackdown

2.5.3 Isolating the Information Shock Channel

I have shown that debarment is associated with negative effects on employment and unconditional earnings. As discussed in Section 2.5.1, there are at least two reasons that may explain these findings. First, debarment is designed as an anti-corruption policy to punish firms for corrupt practices by excluding them from government procurement processes. This sanction may generate a negative demand shock, especially for firms heavily dependent on public contracts. I refer to this channel as the *direct effect* of debarment. The second explanation comes from the institutional context. Information on excluded companies is listed in a public database and sanctions can be disseminated by the mass media, suggesting a role for *information shock*.

In order to investigate whether the negative impacts of debarment are partly driven by the information shock channel, I scrutinize the labor reallocation of workers separated from employment. In particular, I test for whether there are reputation effects from having worked at least three years in the recent past in a punished company. I create a sample of workers that are laid off *prior* to debarment events from the same set of establishments used in the

[36] To investigate heterogeneous effects based on individuals characteristics extracted from the year before debarment, I estimate the following model:

$$\begin{aligned} y_{it} = \beta_1 \times Post_t + \beta_2 \times Post_t \times Debarment_i + \beta_3 \times Post_t \times Heterogeneity_i + \\ \beta_4 \times Post_t \times Debarment_i \times Heterogeneity_i + \alpha_i + \alpha_t + \varepsilon_{it} \end{aligned} \tag{2.4}$$

in which the set of fixed effects and outcomes is the same as in Equation (2.2). β_4 is the interaction term of interest and is reported in Table 2.C.9, Appendix C.

establishment-level analysis. I track them over the subsequent years, even they are working in a different company, to assess whether there are differences in labor market outcomes after their original firm is debarred. Any difference could be interpreted as the information shock brought by debarment. This strategy allows me to rule out the direct effect and isolate the information shock given the limitation that I do not have data on revenues or loans to observe other margins of response.

I estimate Equation (2.2) using a sample of workers from both treated and control establishments that have been laid off in any of the three years before debarment. Figures 2.4a and 2.4b plot the point estimates for employment and log unconditional earnings in the formal sector. Both outcomes experience similar trends prior to the event, validating the research design. After debarment, there is a negative, albeit small and somewhat noisy, impact. Columns (1) and (3) of Table 2.5 point to a decline by 1.4 (SE = 0.010) p.p. and 0.131 (or 12.2 percent decrease) (SE = 0.072) in employment and log earnings, respectively. When controlling for time-varying worker's characteristics, such as worker's age and age squared, the effect becomes smaller, although still negative.

In Table 2.C.10, Appendix C, I consider separate samples of separated workers depending on whether they have been laid off one, two or three years prior to debarment to examine whether the effect of the information shock is stronger for individuals laid off closer to debarment dates. I do not find evidence of a systematic pattern. Taken together, the findings suggest that debarment also affects workers who were connected to punished companies before the policy shock.

2.5.4 Robustness Checks

Appendix D provides robustness tests for the main analysis. I begin with checks related to the construction of the sample, followed by alternative matching procedures. I also discuss the intensity of treatment and the effects on firms rather than establishments.

Pre-2014 Debarment Analysis. For the reasons outlined in Section 2.3.1, the main analysis is restricted to establishments that have been debarred after the enactment of the Anti-Corruption Law. On the other hand, there is also a set of establishments excluded from public procurement between 2008 and 2013 in the CEIS data, for which I assess the labor market consequences. Tables 2.D.1 and 2.D.2, Appendix D, report the establishment- and worker-level results. Overall, the estimates are in line with the post-2014 analysis, consistent with debarment inducing large and negative impacts on workers.

Alternative Sample Restriction. As described in Section 2.3.2, in order to obtain the final sample to implement the matching algorithm, I drop establishments without any employee in all three years prior to initial debarment. As a robustness check, I relax this restriction by allowing establishments to not have any employee in any of the three years prior to debarment.[37] Table 2.D.3, Appendix D, shows that the impacts on establishments are quite similar to the baseline estimates.

Alternative Matching Algorithms. I also explore the sensitivity of the main results to a number of different matching specifications. First, instead of matching on each of the three years before official debarment ($[t-3, t-1]$), I match only on the level (i.e., the year prior to debarment, $t-1$). It allows me to assess pre-trends in a more flexible way, including whether treated and control establishments were on different trajectories prior to debarment. Second, I change the algorithm to generate up to three and five control units, rather than just one counterfactual unit, for each treated establishment. Third, instead of using propensity score matching to construct a suitable comparison group, I rely on coarsened exact matching

[37] In other words, the establishments need to appear in the RAIS data at least once in three years before debarment. This condition allows me to ensure that the establishments have been operating before debarment and recover some establishment-level information extracted from RAIS for the matching procedure, such as industry and location. This alternative sample restriction produces a sample of 4,649 establishments to implement the matching algorithm, which is similar to the one described in Section 2.3.4, except for a slight modification: when the establishment does not have any employee in any of the three years before debarment, I assign zero value.

procedure (Iacus et al. (2012)). Overall, Figures 2.D.2 to 2.D.4 and Table 2.D.4 show that the main estimates are robust to these alternative matching approaches.

Treatment Intensity. Thus far, all specifications have considered debarment at the extensive margin without accounting for differences in the severity of illicit practices. Because I do not have data on irregularities that have prompted the sanctions, I alternatively utilize the length of debarment as a proxy for the intensity of the punishment. In general, more illicit practices entail longer punishments. I define establishments that are above and below the median of 12 months of punishment as those experiencing a higher and lower treatment intensity. I present the main results in Tables 2.D.5 to 2.D.7, Appendix D. I find that the impacts on establishments and workers as well as the reputation mechanism are substantially stronger among establishments experiencing a more intense treatment, consistent with debarment largely affecting establishments with more severe irregularities.

Firm-Level Results. While the institutional context indicates that the debarment occurs at the establishment level, I also examine the impacts of the policy on firms. Tables 2.D.8 and 2.D.9, Appendix D, show that the magnitudes of the establishment- and firm-level results are quite similar, which is unsurprising given that 88 percent of the debarred sample consists of single-establishment firms (Column (1) of Table 2.1). I also do not find evidence of significant spillovers in other establishments within firms, reinforcing that the results are primarily driven by the policy shock rather than a reallocation of activity across establishments within firms.

2.6 Concluding Remarks

Despite the importance of anti-corruption strategies in curbing illicit practices, their impacts on labor market outcomes remain understudied. Exploiting several features of the Brazilian context, this paper evaluates the effects of an instrument that has also been in-

creasingly adopted by many countries: corporate debarment or blacklisting. I leverage novel data on debarment combined with matched employer-employee data and adopt a matched difference-in-differences approach to investigate how debarment affects establishments and workers.

I find that debarred establishments experience sizable and negative impacts on selected labor market outcomes. On average, they experience a 47.7 percent decline in total number of employees and are 15.1 percentage points more likely to exit the formal sector. In addition, using worker-level data, I show that employees' annual earnings fall by around 22 percent.

Three additional findings are consistent with the results being driven by the unemployment effects. First, I argue that the financial burden due to fines is unlikely to explain the results since the data indicates that very few excluded companies are fined. By law, the maximum cap for fines is 20 percent of annual gross revenues, which may be considered too small to justify the labor market impacts. Second, I provide evidence that relationships with the government are relevant as at least half of the sample has bid for procurement contracts with the federal government. I find a sizable decline in the probability of bidding for and winning procurement contracts after debarment as well as larger impacts for establishments with stronger connections with the government, supporting the prediction that debarment constitutes a severe demand shock. Third, I find small evidence of the role of information shock, although it contributes in some extent to the unemployment effects.

An important implication of my analysis is that anti-corruption policies may generate unintended impacts on other groups beyond the main target. In the case of debarment, it is still unclear what unit should be debarred (e.g., firm, owners or firm division) as the main target can largely vary across countries. In principle, sanctions designed to only affect particular agents, such as owners or a firm division, may be too weak to generate a credible threat as they can avoid such punishments or even redistribute their impacts to less influential sub-groups within the debarred firm (Draca et al. (2019)). On the other hand, debarment

at the firm level has the potential to increase the costs of engaging in corrupt practices and generate larger benefits as its impacts can be easily distributed across both targeted and non-targeted sub-groups within the firm. This paper shows that the combination of demand and information shocks lead to losses for workers connected to punished firms. Although most sub-groups of workers are affected by the policy, I find that highly skilled and paid and more tenured workers experience higher unemployment and earning losses. The findings therefore suggest that policymakers should consider the employee costs created by debarment when considering the potential consequences of this policy instrument, not just the possibility of increasing corporate transparency and integrity. More broadly, in light of the ongoing debate surrounding the policy instruments to combat corruption, any evaluation of the consequences of a corruption crackdown should also consider the employee costs when weighing its costs and benefits.

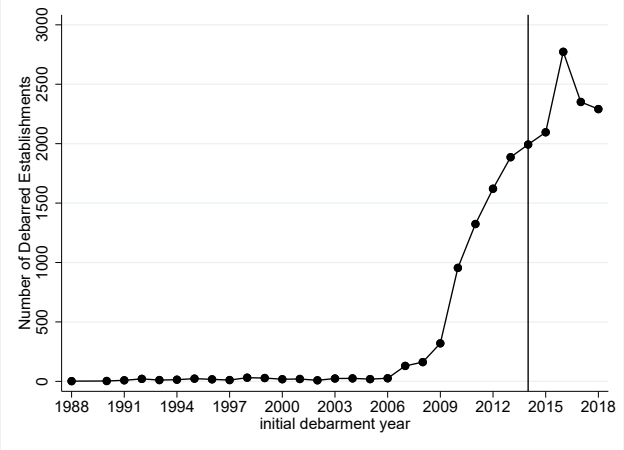
More recently, Auriol and Søreide (2017) has theoretically shown that debarment is only effective when the number of competitor firms in the market is not too large, firms care about future sales streams, and the probability of being caught is sufficiently high. In other cases, debarment may be ineffective. Alternative policy recommendations should also consider coupling debarment with other complementary sanctions, such as fines, prison terms and financial incentives, to generate a larger deterrence effect. Evaluating the effectiveness of these policy instruments is an interesting avenue for future research.

The main limitation of this paper is that I am not able to estimate the benefits of debarment due to lack of additional data. Debarment may also induce substantial reallocation of resources from corrupt to (potentially) non-corrupt firms, and firm-level data on operational and nonoperational costs, revenues, assets, and investment from annual surveys would allow me, for instance, to observe whether corrupt firms make up for losing government contracts and whether debarment has a long-run effect on integrity and productivity or ultimately benefits firms' competitors. Removing competitors may also generate a reduced competition

in the procurement market, resulting lower quality or higher prices, especially in oligopolistic markets (Cerrone et al. (2021)). These effects are the opposite of what procurement contracts are intended to deliver. These possible consequences of debarment are left for future work. Therefore, this paper should be viewed as a first step toward characterizing the impacts of debarment on the economy.

Figures and Tables

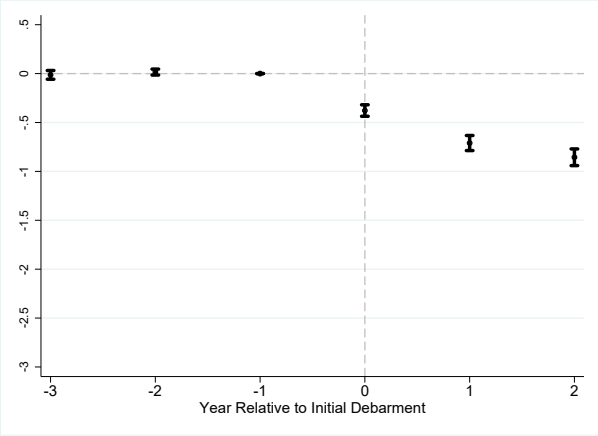
Figure 2.1: Evolution of Debarred Establishments



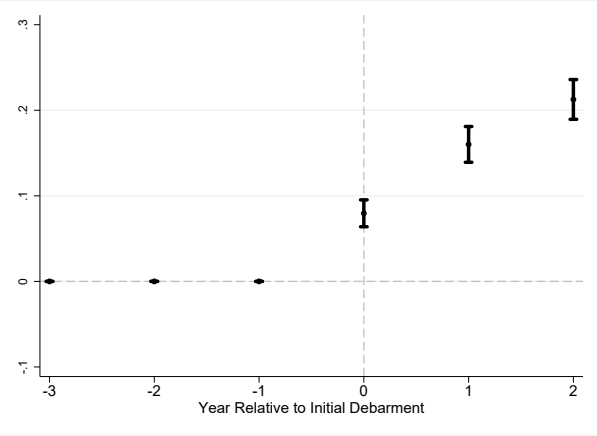
Note: Graph shows, on the left axis, how the total number of debarred establishments has rapidly evolved since 1988. Information is obtained from CEIS data, carried out by CGU.

Figure 2.2: The Effects of Debarment on Establishments' Outcomes

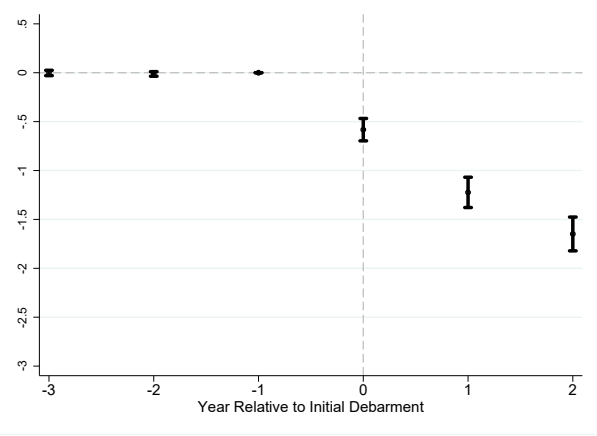
(a) Log Employment



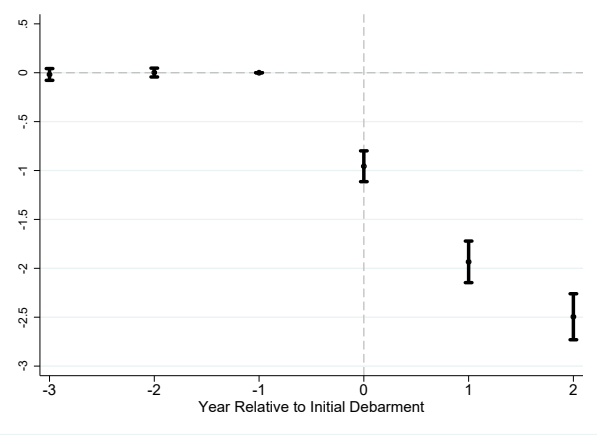
(b) Exit



(c) Log (Uncond.) Earnings



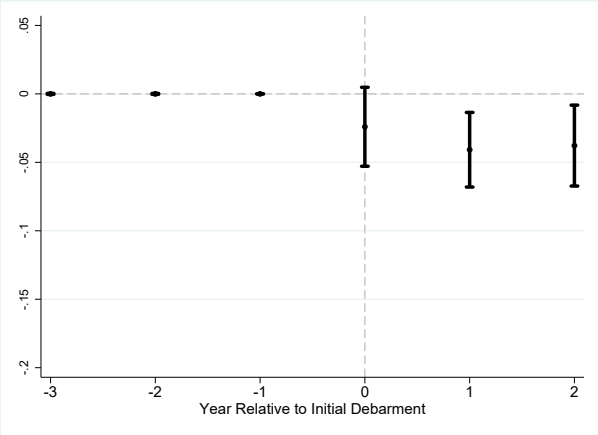
(d) Log (Uncond.) Payroll



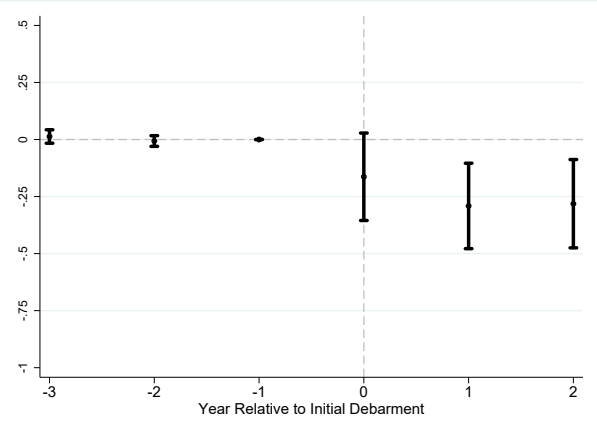
Note: This figure reports point estimates of the annual effects of debarment on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before debarment. More details can be found in Table 2.3.

Figure 2.3: The Effects of Debarment on Workers' Outcomes

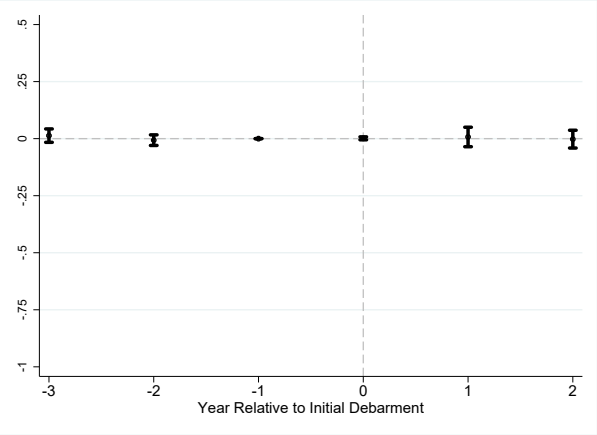
(a) Employment



(b) Log (Uncond.) Earnings



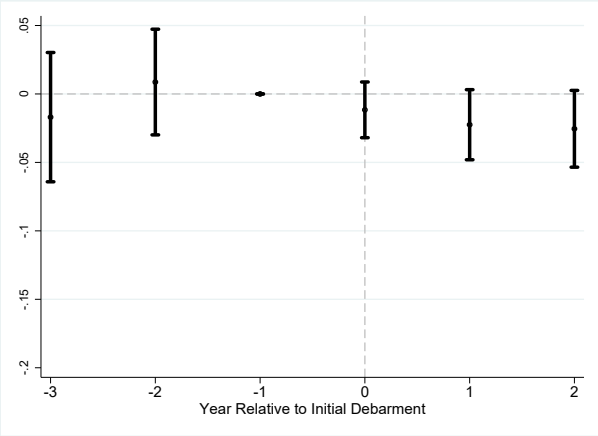
(c) Log (Cond.) Earnings



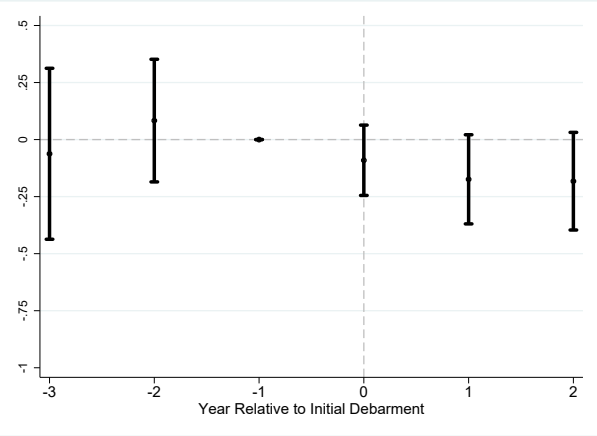
Note: This figure reports point estimates of the annual effects of debarment on different outcomes using the worker-level sample from RAIS data. The omitted category is the year before debarment. More details can be found in Table 2.4.

Figure 2.4: The Information Shock Channel

(a) Employment



(b) Log (Uncond.) Earnings



Note: This figure reports point estimates of the information shock channel of debarment on employment and unconditional log earnings. The omitted category is the year before debarment. More details can be found in Table 2.5.

Table 2.1: Descriptive Statistics: Establishments

	(1)	(2)	(3)	(4)
	Debarred Establishments		**Control Establishments**	
	Mean	SD	Mean	SD
Main Variables				
# Employees	164.49	729.59	113.66	533.50
Log Employees	3.19	1.76	3.14	1.56
Monthly Payroll *(in reais)*	367,709.51	2,543,142.81	247,625.94	1,294,277.79
Log Monthly Payroll *(in reais)*	10.44	2.05	10.44	1.91
Average Earnings per Employee	1,823.04	1,337.89	1,958.27	1,544.03
Log Earnings per Employee	7.38	0.54	7.42	0.62
Bid for Procurement Contract	0.62	0.48	0.62	0.49
Win Procurement Contract	0.51	0.50	0.51	0.50
Single Establishment	0.88	0.33	0.88	0.33
Location				
Central-West Region	0.14	0.35	0.14	0.35
North Region	0.04	0.20	0.04	0.20
Northeast Region	0.15	0.36	0.15	0.36
South Region	0.22	0.41	0.22	0.41
Southeast Region	0.45	0.50	0.45	0.50
Average Municipality Population	2,234,094.66	3,422,168.89	2,255,615.53	3,453,405.58
Sector				
Construction	0.16	0.36	0.16	0.36
Commerce	0.41	0.49	0.41	0.49
Transp., Storage & Commun.	0.05	0.21	0.05	0.21
Transformation Industry	0.13	0.34	0.13	0.34
Real Estate	0.21	0.41	0.21	0.41
Other Categories	0.04	0.20	0.04	0.20
N	3,179		3,179	

Note: This table reports descriptive statistics for establishments using information from CEIS and RAIS data. The first two columns refer to a sample of debarred establishments that are matched to RAIS data. Columns (3) and (4) report summary statistics for the matched control group after implementing the matching algorithm. Further details on the matching algorithm are found in Section 2.3.4. Summary statistics are computed from RAIS data using the averages in the annual window [-3,-1] before debarment. The variables are: average and log number of employees, average and log monthly payroll (expressed in Brazilian *reais*), average and log earnings per worker (also expressed in Brazilian *reais*), an indicator for whether the establishment has bid for and won a procurement contract with the federal government between 2013 and 2018, an indicator for whether the establishment is a single establishment from the associated firm, indicator variables for whether the establishment is located in Central-West, North, Northeast, South and Southeast regions, average population of the municipality in which the establishment is located, and indicator variables for economic sector the establishment belongs to (administration, construction, commerce, transportation, storage and communication, transformation industry, or other sectors).

Table 2.2: Descriptive Statistics: Workers

	(1)	(2)	(3)	(4)
	Treated Workers		**Control Workers**	
	Mean	SD	Mean	SD
Earnings				
Earnings *(in reais)*	2,256.96	2,996.49	2,572.38	3,585.70
Log Earnings	7.29	1.32	7.35	1.44
Gender				
Male	0.62	0.49	0.63	0.48
Disability				
Disabled	0.02	0.13	0.01	0.11
Race				
White	0.46	0.50	0.46	0.50
Firm Size				
0-9 Employees	0.03	0.16	0.02	0.16
10-49 Employees	0.08	0.26	0.10	0.31
50-99 Employees	0.06	0.25	0.06	0.25
100+ Employees	0.83	0.37	0.81	0.40
Education				
Basic Education	0.41	0.49	0.39	0.49
High School	0.50	0.50	0.50	0.50
College	0.10	0.29	0.11	0.31
Age				
less or equal to 25	0.09	0.28	0.09	0.28
26-35	0.32	0.46	0.33	0.47
36-45	0.29	0.45	0.30	0.46
more than 45	0.30	0.46	0.29	0.45
Occupation				
Managerial	0.03	0.16	0.03	0.17
Professional	0.11	0.31	0.13	0.33
White Collar Lower Level	0.18	0.39	0.16	0.36
Blue Collar	0.68	0.47	0.68	0.47
Tenure				
3- years	0.57	0.49	0.53	0.50
4 years	0.14	0.34	0.14	0.34
5 years	0.09	0.29	0.08	0.28
6+ years	0.20	0.40	0.25	0.43
# Unique Workers	81,333		81,333	

Note: This table reports descriptive statistics for workers using information from CEIS and RAIS data. The first two columns refer to a sample of workers from debarred establishments. Columns (3) and (4) report summary statistics for the matched control workers. Further details on the sample and the matching algorithm are found in Section 2.3.4. Total number of unique workers is reported in the last row. The variables are: average and log earnings (in Brazilian *reais*), indicator variable for male worker, indicator variable for disability, indicator variable for whether the race is white, indicator variables for establishment size, indicator variables for whether the worker has basic education, high school and college education, indicator variables for different age groups (less or equal to 25 years old; 26–35; 36–45; above to 45), indicator variables for whether the worker holds a managerial, professional, white collar lower level and blue collar position, and indicator variables for different tenure lengths (three years or less, four years, five years, and six years or more).

Table 2.3: Effects of Debarment on Establishments' Outcomes

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
PostDebarment	-0.649***	0.151***	-1.147***	-1.790***
	(0.035)	(0.009)	(0.064)	(0.092)
Sample Size	38,148	38,148	38,148	38,148
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
# Establishments	6,358	6,358	6,358	6,358
# Debarred Establishments	3,179	3,179	3,179	3,179
Mean Dep. Var (Control)	3.14	0	7.42	10.44

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on several establishments' outcomes restricted to the formal sector: log employment, likelihood of exiting the formal sector, log (unconditional) earnings and log (unconditional) monthly payroll using information from both CEIS and RAIS data. The estimation sample consists of annual window $[-3, 2]$ around debarment between 2014 and 2016. All columns refer to Equation (2.1). Number of establishments and establishment-year pairs is reported. Means of dependent variables are computed from pre-event years $[-3, -1]$ of the matched control group. Standard errors are clustered at the firm level.

Table 2.4: Effects of Debarment on Workers' Outcomes

	(1) employment	(2) log earnings (uncond.)	(3) log earnings (cond.)
PostDebarment	-0.034**	-0.247**	-0.000
	(0.014)	(0.096)	(0.011)
Sample Size	975,996	975,996	883,131
Worker FE	✓	✓	✓
Year FE	✓	✓	✓
# Workers	162,666	162,666	162,666
# Treated Workers	81,333	81,333	81,333
Mean Dep. Var (Control)	1	7.35	7.35

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on workers' outcomes in the formal sector: indicator for employment, log unconditional earnings and log conditional earnings using information from both CEIS database and RAIS data. The estimation sample consists of annual window [-3,2] around debarment between 2014 and 2016. Further details on how the sample is constructed can be found in Section 2.3.4. All columns refer to Equation (2.2). Number of workers, treated workers and worker-year pairs is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.

Table 2.5: The Information Shock Channel

	(1) employment	(2) employment	(3) log earnings	(4) log earnings
PostDebarment	-0.014	-0.012	-0.131*	-0.119*
	(0.010)	(0.009)	(0.072)	(0.068)
Sample Size	586,908	586,908	586,908	586,908
Worker FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Worker Controls	×	✓	×	✓
Mean Dep. Var (Control)	0.71	0.71	5.39	5.39

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table tests the information shock channel by estimating the aggregate effects of debarment on employment and log unconditional earnings in the formal sector using information from both CEIS database and RAIS data. The estimation sample consists of annual window [-3,2] around debarment between 2014 and 2016. Further details on how the sample is constructed can be found in Section 2.5.3. All columns refer to Equation (2.2). In Columns (2) and (4), I add time-varying controls, such age and age squared. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.

Appendices

A Additional Information from Debarment Data

Table 2.A.1: Descriptive Statistics Using CEIS Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Debarred (Initial Sample)		**Debarred Between 2008 and 2013**		**Debarred Between 2014 and 2016**		**Matched with RAIS data**		**Debarred (Final Sample)**	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Government Level										
Federal	0.62	0.48	0.66	0.47	0.65	0.48	0.62	0.48	0.62	0.49
State	0.31	0.46	0.31	0.46	0.30	0.46	0.32	0.47	0.33	0.47
Municipal	0.06	0.24	0.03	0.16	0.04	0.21	0.05	0.22	0.05	0.23
Government Branch										
Executive	0.73	0.44	0.70	0.46	0.71	0.45	0.75	0.43	0.75	0.43
Judiciary	0.20	0.40	0.22	0.41	0.22	0.41	0.19	0.39	0.19	0.39
Legislative	0.02	0.14	0.02	0.13	0.03	0.17	0.02	0.15	0.02	0.15
Others	0.05	0.21	0.07	0.25	0.04	0.20	0.04	0.19	0.04	0.18
Type of Sanction										
Impediment	0.40	0.49	0.32	0.47	0.43	0.50	0.45	0.50	0.45	0.50
Suspension	0.41	0.49	0.46	0.50	0.41	0.49	0.44	0.50	0.44	0.50
Prohibition	0.08	0.27	0.11	0.31	0.08	0.27	0.06	0.23	0.06	0.23
Others	0.11	0.31	0.10	0.31	0.07	0.26	0.05	0.23	0.05	0.22
Length (# months)	21.76	21.69	29.13	24.83	19.95	19.41	18.17	17.22	18.12	17.19
Location										
Central-West Region	0.32	0.47	0.40	0.49	0.34	0.47	0.33	0.47	0.33	0.47
North Region	0.06	0.23	0.05	0.21	0.05	0.21	0.04	0.20	0.04	0.20
Northeast Region	0.16	0.36	0.14	0.34	0.16	0.36	0.15	0.36	0.15	0.35
South Region	0.13	0.34	0.07	0.26	0.14	0.35	0.16	0.36	0.16	0.37
Southeast Region	0.30	0.46	0.28	0.45	0.32	0.46	0.32	0.47	0.33	0.47
N	19,112		6,268		6,862		3,294		3,179	

Note: This table reports descriptive statistics from the CEIS database. The first two columns refer to establishments listed in CEIS before being matched with RAIS data. Columns (3) and (4) (Columns (5) and (6)) consider establishments that are included in CEIS between 2008 and 2013 (2014 and 2016). Columns (7) and (8) consider establishments debarred between 2014 and 2016 that are matched with RAIS data. Columns (9) and (10) describe the sample of establishments debarred between 2014 and 2016 after applying the matching algorithm. The set of variables is the following: indicator variables for whether the establishment is punished by the federal, state or municipal government, indicator variables for which government branch is responsible for the punishment, indicator variables for the types of sanctions, encompassing temporary **suspension** in participation in public bids, **impediment** of bidding and contracting with the public administration, **prohibition** of contracting with the public administration and receiving benefits and incentives, and **other categories**, average length of debarment, expressed in months, and indicator variables for regions where the debarment occurred. The total number of establishments in each sample restriction is reported in the last row.

Table 2.A.2: Descriptive Statistics for All Establishments

	(1)	(2)
	All Establishments	
	Mean	SD
Main Variables		
# Employees	18.27	135.62
Log Employees	1.87	1.11
Monthly Payroll *(in reais)*	38,444.55	446,004.27
Log Monthly Payroll *(in reais)*	8.75	1.61
Average Earnings per Employee	1,526.72	1,271.35
Average Log Earnings per Employee	7.18	0.66
Bid for Procurement Contract	0.01	0.09
Win Procurement Contract	0.01	0.07
Unique Firm	0.85	0.36
Location		
Central-West Region	0.09	0.28
North Region	0.04	0.19
Northeast Region	0.16	0.37
South Region	0.22	0.41
Southeast Region	0.50	0.50
Average Municipality Population	1,662,300.49	3,232,904.87
Sector		
Construction	0.04	0.20
Commerce	0.45	0.50
Transp., Storage & Commun.	0.05	0.22
Transformation Industry	0.11	0.31
Real Estate	0.13	0.34
Other Categories	0.22	0.41
N	4,058,403	

Note: This table reports descriptive statistics for all establishments in the formal sector using information from RAIS data. Summary statistics are computed from RAIS data using the averages between 2011 and 2013, the years before initial debarment in the main analysis. The variables are: average and log number of employees, average and log monthly payroll (expressed in Brazilian *reais*), average and log earnings per worker (also expressed in Brazilian *reais*), indicators for whether the establishment has bid for and won a procurement contract with the federal government between 2013 and 2018, an indicator for whether the establishment is a single establishment from the associated firm, indicator variables for whether the establishment is located in Central-West, North, Northeast, South and Southeast regions, average population of the municipality in which the establishment is located, and indicator variables for economic sector the establishment belongs to (construction, commerce, transportation, storage and communication, transformation industry, real estate, or other sectors).

B Data Appendix

This appendix provides further details on the data sources described in Section 1.3, including subsections on further data description and sample construction.

Data Description

The Debarment Data. The CEIS file contains the following variables: sanction identification number, process number, government agency responsible for the process, government agency's state, level of government agency (federal, state, or municipal), type of punished agent (establishment or individual), tax identifier, name, type of punishment, start and end dates of punishment, total amount of fine, status of punishment, cancellation date, reason for cancellation, reactivation date, and reason for reactivation. The CEIS file is available under a confidentiality agreement with CGU (*Controladoria Geral da União*).

The Labor Market Data. The *Relação Anual de Informações Sociais* (RAIS) is main labor market data source. RAIS is linked employer-employee register with worker and establishment tax identifiers collected by the Brazilian Ministry of Economy and is available under a confidentiality agreement with the agency. The raw RAIS data are mostly provided in state-year files and all variables are standardized across years. I use data spanning the years between 2002 and 2018.

The Procurement Data. Information on all online bidders and winners of procurement contracts obtained with the federal government are extracted from *Portal de Transparência* and *Compras Governamentais* websites, updated by the federal government. Due to data availability, the first year of data used in this paper is 2013.

Data on Municipal Population. Information on municipal population are drawn from the Demographic Census in 2010, the most recent Census data available. Although information on estimated annual population are available, they are more sensitive to measurement

errors. The Demographic Census, sourced from IBGE (*Brazilian Institute of Geography and Statistics*), provides the most reliable information.

Sample Selection and Variable Construction

In order to get the main sample of debarred establishments, I make some restrictions to the CEIS dataset. I begin by restricting the analysis to establishments, as the original data also include punished individuals. To avoid duplicate observations, I maintain the earliest sanction each establishment has. I also remove establishments that had cancelled sanctions or invalid tax identification numbers. Lastly, I delete all sanctions starting before 2014 and after 2016. These restrictions altogether yield a sample of 6,862 establishments to be matched with RAIS data through establishment tax identifiers. When extending the analysis to include establishments that were debarred between 2008 and 2013, I replicate these steps by maintaining the relevant information.

The establishment tax identifier (CNPJ) is unique to a given establishment over time and consists of fourteen digits. The first eight digits correspond to the firm, while the last six digits correspond to an establishment within the firm. Therefore, as a robustness check, I also adopt an alternative sample restriction at the firm level that follows the same steps using the first eight digits of establishment tax identifier instead of all fourteen digits.

I describe here the steps to prepare the sample to be matched with the debarment data and the **variables** used in the establishment-level analysis. I compute the **total number of employees** and **total average payroll** for all establishments every year based on worker-level files. Payrolls are adjusted to 2018 Brazilian *reais*. Each establishment is assigned its modal legal classification, **municipality** (and, therefore, **state** and **region**), industry code, and establishment size group.[38] I then keep establishments from private sector based using legal classification of each establishment. I also remove units from the Brazilian Central Bank and with invalid industry codes. **Average earnings** are defined as total average payroll divided by number of employees. Considering the universe of all establishments that

[38]Size is recorded as a variable with 10 possible values: zero workers, up to 4 workers, between 5 and 9 workers, between 10 and 19 workers, between 20 and 49 workers, between 50 and 99 workers, between 100 and 249 workers, between 250 and 499 workers, between 500 and 999 workers, more than 1000 workers.

survived to former sample restrictions, I compute the **deciles of the employment and average earnings** in each of the years. I also generate an indicator variable for whether each establishment a **single establishment** each year.

Using the first two digits of industry code[39], each establishment is assigned to one of the following 17 **economic sectors (or industries)** (defined as sections by IBGE): agriculture, cattle, and forestry (section A); fishing (section B); extractive industries (section C), transformation industries (section D); production and distribution of electricity, gas, and water (section E); construction (section F); wholesale and retail trade; repair of motor vehicles and motorcycles (section G); accommodation and food service activities (section H); transporting, storage, and communication (section I); financial activities (section J); real estate activities (section K); public administration, defence, and social security (section L); education (section M); health and social services (section N); other services activities (section O); domestic services (section P); and international organizations and other extraterritorial institutions (section Q).

Other variables are also created using procurement and municipal population data, such as **indicator for whether the establishment bids for procurement contracts with the federal government** each year and **population group** for municipality where the establishment is located.[40]

[39]Industry code follows the National Classification of Economic Activities (CNAE), versio 1.0, based on UN ISIC - International Standard Industrial Classification of All Economic Activities.

[40]I aggregate population information into seven groups: less than 5,000 inhabitants; equal or more than 5,000 and less than 10,000; equal or more than 10,000 and less than 20,000; equal or more than 20,000 and less than 50,000; equal or more than 50,000 and less than 100,000; equal or more than 100,000 and less than 500,000; and more than 500,000.

Matching Procedure

I use the labor market, municipal population and procurement data to construct the matching algorithm for the **establishment sample**. For each establishment debarred in a given year of debarment, I consider a set of non-debarred establishments from the same economic sector and state as possible candidates for the control group. The algorithm uses information extracted from RAIS in all the three years before (years $t-3$, $t-2$ and $t-1$). More precisely, for each pair of economic sector and state, I run a simple linear probability model predicting treatment using the following set of variables as regressors: deciles of total employment in years $t-3$, $t-2$ and $t-1$, deciles of annual earnings in years $t-3$, $t-2$ and $t-1$, indicator variables for whether the establishment won procurement contracts with the federal government in years $t-3$, $t-2$ and $t-1$, indicator variable for whether the establishment bid for procurement contracts with the federal government in any of the three years before debarment, whether it is a single establishment in all of the three years before debarment, and population group representing the size of municipality where the establishment is located. For each treated establishment, I keep one control candidate with the closest propensity score. I also ensure that potential control establishments are not associated to more than one debarred establishment. I consecutively repeat the above steps for all years with debarment events.

As robustness checks, I consider the following changes in the matching algorithm. First, instead of considering the previous regressors from all the three years before debarment, I utilize regressors from year $t-1$ and from years $t-2$ and $t-1$ separately. Second, instead of using a simple linear probability model with the closest propensity score, I implement a more restrictive version of matching: one-to-one coarsened exact matching (Iacus et al. (2012)) with the same set of regressors as before (and, again, within pairs of economic sector and state). Third, instead of allowing only one control establishment with the closest propensity score for each treated establishment, I flexibly allow up to three and five control candidates

with the highest propensity scores. I find very similar conclusions. Section 2.5.4 describes the results in detail.

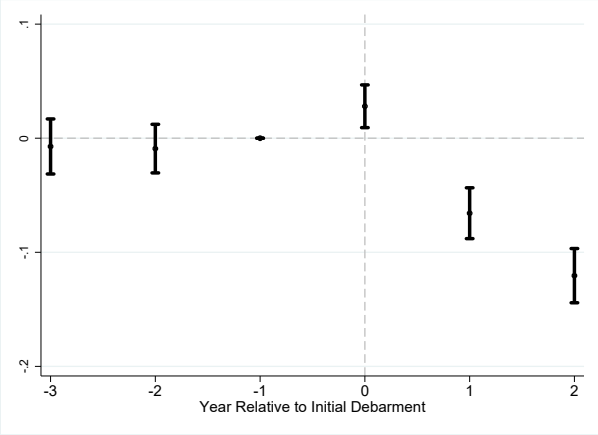
For the worker-level analysis, I construct a **worker sample** also using a matching algorithm. In particular, from the set of matched debarred establishments (i.e., the final debarred establishment sample after the matching steps outlined above), I recover all individuals who worked in these establishments in all three years before they were debarred (years $t-3$, $t-2$ and $t-1$). I repeat this step for the set of matched control establishments. I then recover the relevant characteristics extracted from RAIS in year $t-1$: age groups (I create 5 years age bins), indicator for male, indicator for disability, indicator for white, educational categories (there are 11 educational categories: illiterate, incomplete primary education, primary education, incomplete lower secondary education, lower secondary education, incomplete upper secondary education, upper secondary education, incomplete tertiary education, tertiary education, Master degree, and PhD degree), occupational categories (more precisely, I use the 2002 Brazilian Classification of Occupations (CBO), which classifies jobs based on their skill and task content to construct four occupation categories: managerial, professional, blue collar, and white collar lower level positions), and economic sector. Thereafter, with the sample of workers from matched treated and control establishments in hand, I estimate a simple linear probability model predicting treatment using the above characteristics. For each worker from treated establishment, I keep one comparison worker from control establishment with the closest propensity score.

As a last step, the resulting establishment and worker samples are matched to relevant years of RAIS data (years $[t-3,\, t+2]$) to recover the outcomes of interest.

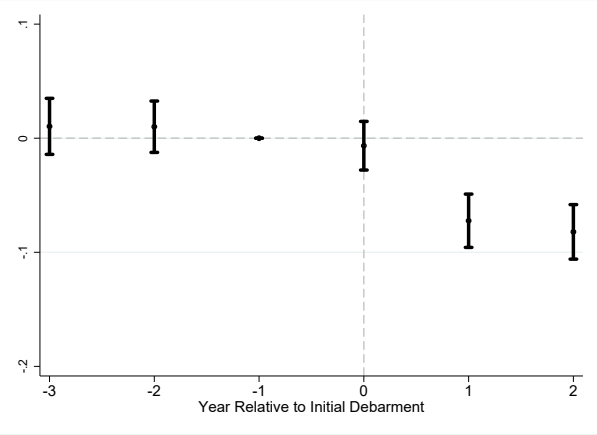
C Additional Results

Figure 2.C.1: Effects of Debarment on Procurement Contracts

(a) Bidding Federal Contract



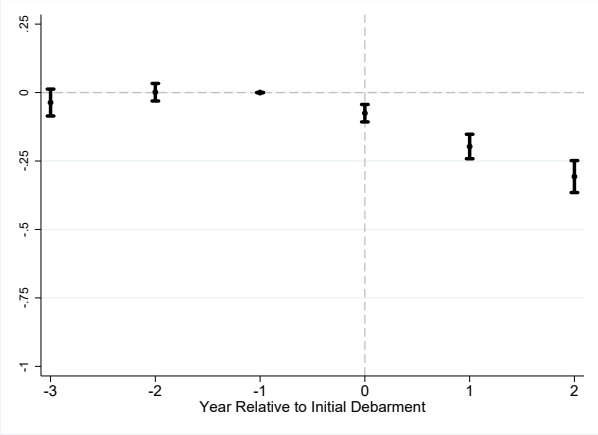
(b) Winning Federal Contract



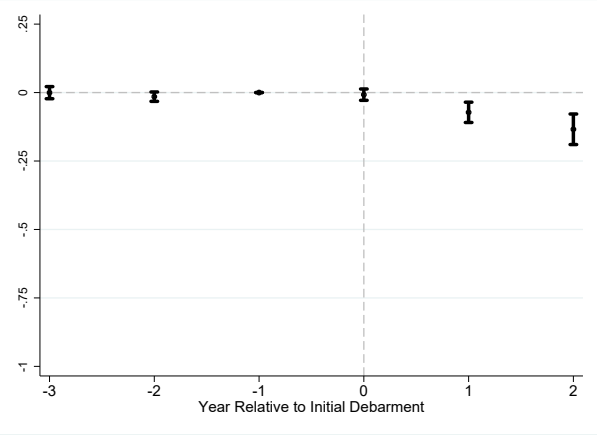
Note: This figure reports point estimates of the annual effects of debarment on the likelihood of bidding for and winning procurement contracts with the federal government. The omitted category is the year before debarment. More details can be found in Table 2.C.6.

Figure 2.C.2: Effects of Debarment on Establishments' Outcomes Using Establishment Stayers

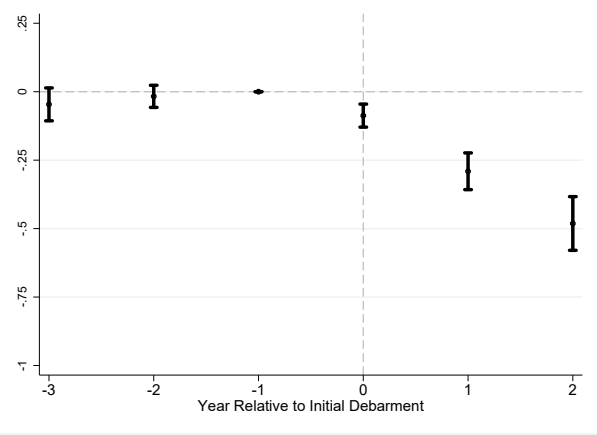
(a) Log Employment



(b) Log Earnings



(c) Log Payroll



Note: This figure reports point estimates of the annual effects of debarment on different establishment-level outcomes from RAIS data. The omitted category is the year before debarment. The sample consists of establishment stayers, that is, active establishments that have at least one employee in all years from the window of [-3, 2] years around the debarment event. More details can be found in Table 2.C.1.

Table 2.C.1: Effects of Debarment on Establishments' Outcomes for Stayers

	(1) log employment	(2) log earnings (cond.)	(3) log payroll (cond.)
PostDebarment	-0.180***	-0.066***	-0.264***
	(0.024)	(0.016)	(0.034)
Sample Size	27,306	27,306	27,306
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
# Establishments	4,506	4,506	4,506
# Debarred Establishments	1,902	1,902	1,902
Mean Dep. Var (Control)	3.25	7.45	10.59

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on several establishments' outcomes: log employment, log earnings and log monthly payroll in the formal sector using information from both CEIS database and RAIS data. The sample consists of active establishments that have at least one employee in all years of annual window [-3,2] around debarment between 2014 and 2016. I refer to them as establishment stayers. All columns refer to Equation (2.1). Number of establishments and establishment-year pairs is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are clustered at the firm level.

Table 2.C.2: Robustness Checks: Establishments' Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Alternative Measures for Outcome Variables					
	employm. (level)	employm. (arc sin)	earnings (level)	earnings (arc sin)	payroll (level)	payroll (arc sin)
PostDebarment	-62.782***	-0.746***	-262.773***	-1.254***	-128,967.369***	-1.897***
	(10.672)	(0.040)	(24.444)	(0.070)	(24,730.618)	(0.098)
Sample Size	38,148	38,148	38,148	38,148	38,148	38,148
Establishment FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Mean Dep. Var (Control)	113.66	3.73	1,958.27	8.11	247,625.94	11.13

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports several robustness checks at the establishment-level using information from CEIS database and RAIS data. Odd columns refer to number of employees, earnings per worker and total monthly payroll as the dependent variables. Even columns consider the inverse hyperbolic sine transformation of these variables as the dependent variables. Number of establishment-year pairs is reported. Means of dependent variables are computed from pre-event years $[-3, -1]$ of the matched control group. Standard errors are clustered at the firm level.

Table 2.C.3: Additional Robustness Checks: Establishments' Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Municipality-Specific Trends				**Industry-Specific Trends**			
	log employm.	exit	log earnings (uncond.)	log payroll (uncond.)	log employm.	exit	log earnings (uncond.)	log payroll (uncond.)
PostDebarment	-0.652***	0.154***	-1.155***	-1.800***	-0.648***	0.149***	-1.138***	-1.782***
	(0.033)	(0.008)	(0.060)	(0.086)	(0.031)	(0.008)	(0.059)	(0.083)
Sample Size	38,148	38,148	38,148	38,148	38,148	38,148	38,148	38,148
Establishment FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Municipality-Specific Trends	✓	✓	✓	✓	×	×	×	×
Industry-Specific Trends	×	×	×	×	✓	✓	✓	✓
Mean Dep. Var (Control)	3.14	0	7.42	10.44	3.14	0	7.42	10.44

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports several robustness checks using establishment-level sample extracted from CEIS and RAIS data. Columns (1)–(4) refer to the same set of outcome variables from Table 2.3 after including municipality-specific trends in the set of controls of Equation (2.1). Columns (5)–(8) add 2-digit industry-specific trends. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are clustered at the firm level.

Table 2.C.4: Heterogeneous Effects by Economic Sector

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
Panel A: Construction (N = 5,976)				
PostDebarment	-0.679***	0.135***	-1.095***	-1.783***
	(0.089)	(0.022)	(0.161)	(0.232)
Panel B: Commerce (N = 15,648)				
PostDebarment	-0.374***	0.150***	-1.104***	-1.455***
	(0.032)	(0.012)	(0.089)	(0.111)
Panel C: Transp., Storage & Commun. (N = 1,824)				
PostDebarment	-0.722***	0.160***	-1.146***	-1.845***
	(0.156)	(0.038)	(0.282)	(0.406)
Panel D: Transf. Industry (N = 5,064)				
PostDebarment	-0.373***	0.084***	-0.639***	-1.014***
	(0.067)	(0.018)	(0.137)	(0.191)
Panel E: Real Estate (N = 8,124)				
PostDebarment	-1.307***	0.213***	-1.606***	-2.921***
	(0.096)	(0.018)	(0.131)	(0.215)
Panel F: Other Sectors (N = 1,512)				
PostDebarment	-0.673***	0.108**	-1.025***	-1.739***
	(0.159)	(0.043)	(0.305)	(0.430)
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports heterogeneous effects of debarment on establishments' outcomes by economic sector using establishment-level sample extracted from CEIS and RAIS data. All columns refer to Equation (2.1) restricted to one of the following sectors: construction, commerce, transportation, storage and communication, transformation industry, real estate or other sectors. Standard errors are clustered at the firm level.

Table 2.C.5: Heterogeneous Effects by Establishment Size

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
Panel A: 0-9 Employees (N = 17,430)				
PostDebarment	-0.289***	0.144***	-1.059***	-1.317***
	(0.030)	(0.013)	(0.092)	(0.113)
Panel B: 10-49 Employees (N = 12,444)				
PostDebarment	-0.606***	0.120***	-0.965***	-1.591***
	(0.051)	(0.013)	(0.097)	(0.142)
Panel C: 50-99 Employees (N = 2,922)				
PostDebarment	-0.999***	0.171***	-1.356***	-2.373***
	(0.137)	(0.026)	(0.198)	(0.324)
Panel D: 100+ Employees (N = 5,352)				
PostDebarment	-1.606***	0.198***	-1.481***	-3.109***
	(0.134)	(0.020)	(0.150)	(0.277)
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports heterogeneous effects of debarment on establishments' outcomes by establishment size using establishment-level sample extracted from CEIS and RAIS data. All columns refer to Equation (2.1) restricted to one of the following sizes: 0-9 employees, 10-49 employees, 50-99 employees, and equal or more than 100 employees. Standard errors are clustered at the firm level.

Table 2.C.6: Effects of Debarment on Procurement Contracts

	(1) bidding contract	(2) bidding contract	(3) winning contract	(4) winning contract
PostDebarment	-0.047***	0.012	-0.061***	-0.007
	(0.008)	(0.010)	(0.008)	(0.010)
Sample Size	38,148	27,036	38,148	27,036
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Sample	All	Stayers	All	Stayers
Mean Dep. Var (Control)	0.36	0.38	0.29	0.31

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on the probability of bidding for and winning procurement contracts with the federal government using CEIS, procurement and RAIS data. In the first column, the sample consists of all establishments from the matching procedure described in Section 2.3.4 considering the period for which procurement data is available. In the second column, the sample is further restricted to establishments that have at least one employee in all years of annual window [-3,2] around debarment, the establishment stayers. All columns refer to Equation (2.1). Means of dependent variables are computed from pre-event years of the matched control group. Standard errors are clustered at the firm level.

Table 2.C.7: Heterogeneous Effects by Intensity of Connection with the Government

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
Panel A: Sectors Less Connected with Government (N = 3,456)				
PostDebarment	-0.703***	0.129***	-1.031***	-1.733***
	(0.119)	(0.026)	(0.194)	(0.292)
Panel B: Sectors More Connected with Government (N = 34,692)				
PostDebarment	-0.645***	0.153***	-1.156***	-1.797***
	(0.034)	(0.008)	(0.062)	(0.088)
Panel C: Lower Dependence on Federal Contracts (N = 28,008)				
PostDebarment	-0.637***	0.135***	-1.040***	-1.677***
	(0.039)	(0.009)	(0.070)	(0.101)
Panel D: Higher Dependence on Federal Contracts (N = 10,140)				
PostDebarment	-0.681***	0.194***	-1.441***	-2.102***
	(0.055)	(0.015)	(0.108)	(0.148)
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports heterogeneous effects of debarment by size on establishments' outcomes by intensity of connection with the government using establishment-level sample extracted from CEIS and RAIS data. All columns refer to Equation (2.1). I use two measures of government dependence. First, considering the universe of federal government contracts awarded in 2013, I create the distribution of total value of contracts by 2-digit industry and divide these sectors into medians. Panels A and B show the results after restricting the sample to sectors below and above the median to represent weaker and stronger connections with the government. Second, I compute the ratio of total revenues obtained from federal government contracts and the annual payroll for each establishment. Panels C and D report the estimates after restricting the sample to establishments with lower and higher dependence on these contracts measured as having less than and at least 25 percent of payroll expenses covered by revenues from federal contracts, respectively. Standard errors are clustered at the firm level.

Table 2.C.8: Robustness Check – Workers' Outcomes

	(1)	(2)	(3)	(4)	(5)
	Other Earnings Measures		**Occupation-Specific Trends**		
	(uncond.) earnings (sin transf.)	(cond.) earnings (sin transf.)	employment	log earnings (uncond.)	log earnings (cond.)
PostDebarment	-0.271**	-0.000	-0.034**	-0.247**	-0.000
	(0.106)	(0.011)	(0.014)	(0.097)	(0.011)
Sample Size	975,996	883,131	975,996	975,996	883,131
Worker FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Occupation-Specific Trends	×	×	✓	✓	✓
Mean Dep. Var (Control)	8.03	8.03	1	7.35	7.35

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports several robustness checks at the worker-level using information from CEIS and RAIS data. Columns (1) and (2) apply the inverse hyperbolic sine transformation on earnings. Columns (3)–(5) include 2-digit occupation-specific trends in the set of controls. Numbers of workers, is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm level.

Table 2.C.9: Heterogeneous Effects by Workers' Characteristics

	(1) employment	(2) log earnings (uncond.)	(3) log earnings (cond.)
Panel A: Gender			
PostDebarment ×**1**(male)	-0.024	-0.137	-0.008
	(0.019)	(0.135)	(0.018)
Panel B: Disability			
PostDebarment ×**1**(with disability)	-0.003	-0.124	-0.078*
	(0.027)	(0.186)	(0.046)
Panel C: Race			
PostDebarment ×**1**(white)	-0.008	-0.128	-0.027*
	(0.021)	(0.140)	(0.016)
Panel D: Education			
PostDebarment ×**1**(High School)	0.004	0.047	0.007
	(0.012)	(0.084)	(0.016)
PostDebarment ×**1**(College)	-0.022	-0.201	-0.013
	(0.022)	(0.159)	(0.027)
Panel E: Age Group			
PostDebarment ×**1**(age ¡ 25)	0.017	0.137	0.062***
	(0.016)	(0.110)	(0.016)
PostDebarment ×**1**($26 \leq age < 36$)	0.013	0.118	0.082***
	(0.014)	(0.093)	(0.013)
PostDebarment ×**1**($36 \leq age < 46$)	0.012	0.110	0.084***
	(0.011)	(0.076)	(0.012)
Panel F: Occupation			
PostDebarment ×**1**(Managerial)	-0.068*	-0.520*	-0.024
	(0.040)	(0.270)	(0.027)
PostDebarment ×**1**(Professional)	-0.053	-0.378*	0.012
	(0.035)	(0.226)	(0.023)
PostDebarment ×**1**(Blue Collar)	-0.031	-0.178	0.015
	(0.037)	(0.229)	(0.018)
Panel G: Tenure			
PostDebarment ×**1**(4 years)	0.021	0.166	-0.003
	(0.035)	(0.233)	(0.018)
PostDebarment ×**1**(5 years)	0.005	0.078	0.020
	(0.018)	(0.124)	(0.021)
PostDebarment ×**1**(6+ years)	-0.036**	-0.235**	0.007
	(0.017)	(0.119)	(0.019)
Panel I: Wage Distribution			
PostDebarment ×**1**(Quintile 2)	-0.043	-0.306	0.054*
	(0.030)	(0.236)	(0.029)
PostDebarment ×**1**(Quintile 3)	-0.022	-0.166	0.053*
	(0.030)	(0.242)	(0.030)
PostDebarment ×**1**(Quintile 4)	-0.036	-0.264	0.067
	(0.030)	(0.242)	(0.030)
PostDebarment ×**1**(Quintile 5)	-0.057*	-0.511**	0.016
	(0.031)	(0.253)	(0.032)
Sample Size	975,996	975,996	883,131
Worker FE	✓	✓	✓
Year FE	✓	✓	✓

Note: This table reports the effects of debarment on several workers' outcomes based on workers' characteristics in the year before the debarment event using information from CEIS and RAIS data. All columns refer to Equation (2.4). Only the estimates for β_4 are displayed. The dependent variables and sample are the same as in Table 2.2. I consider the following characteristics: indicator variable for male workers; indicator variable for disabled workers; indicator variables for educational levels (basic education is the omitted category); indicator variables for age groups (aged above 46 is the omitted category); indicator variables for occupational categories (white collar lower level position is the omitted category); indicator variables for tenure lengths (three years or less of tenure is the omitted category); and indicator variables for wage distributions (first quintile is the omitted category).

Table 2.C.10: The Information Shock Channel by Year of Separation

	(1) employment	(2) employment	(3) log earnings	(4) log earnings
Panel A (Separated One Year Before) (N = 176,112)				
PostDebarment	0.005	0.007	-0.009	0.005
	(0.024)	(0.023)	(0.174)	(0.176)
Mean Dep. Var (Control)	0.78	0.78	5.84	5.84
Panel B (Separated Two Years Before) (N = 226,818)				
PostDebarment	-0.026*	-0.025*	-0.241**	-0.233**
	(0.015)	(0.013)	(0.111)	(0.105)
Mean Dep. Var (Control)	0.70	0.70	5.31	5.31
Panel C (Separated Three Years Before) (N = 183,978)				
PostDebarment	-0.014	-0.012	-0.095	-0.081
	(0.012)	(0.011)	(0.093)	(0.088)
Mean Dep. Var (Control)	0.67	0.67	5.13	5.13
Worker FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Worker Controls	×	✓	×	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table tests the information shock channel by estimating the aggregate effects of debarment on employment and log unconditional earnings in the formal sector. I use information from CEIS and RAIS data. The estimation sample consists of annual window [-3,2] around debarment. All columns refer to Equation (2.1). In Columns (2) and (4), I add time-varying controls, such age and age squared. In Panel A, the sample consists of workers that have been displaced one year before the event. Similarly, Panels B and C consist of workers that have been displaced two and three years before the event, respectively. Further details on how the sample is constructed can be found in Section 2.5.3. Total number of worker-year pairs is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.

D Robustness Checks

In Appendix D, I provide a set of robustness tests for the main results in Section 2.5.

Debarment Prior to the Anti-Corruption Law in 2014

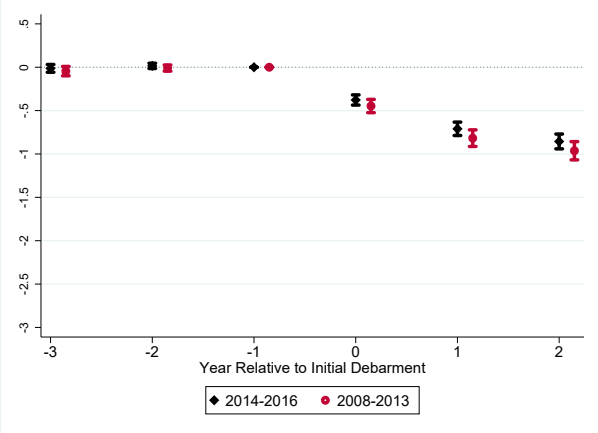
Table 2.D.1: Effects of Debarment on Establishments' Outcomes Prior to 2014

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
PostDebarment	-0.725***	0.132***	-0.952***	-1.685***
	(0.044)	(0.010)	(0.071)	(0.107)
Sample Size	32,136	32,136	32,136	32,136
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean Dep. Var (Control)	3.10	0	7.26	10.24

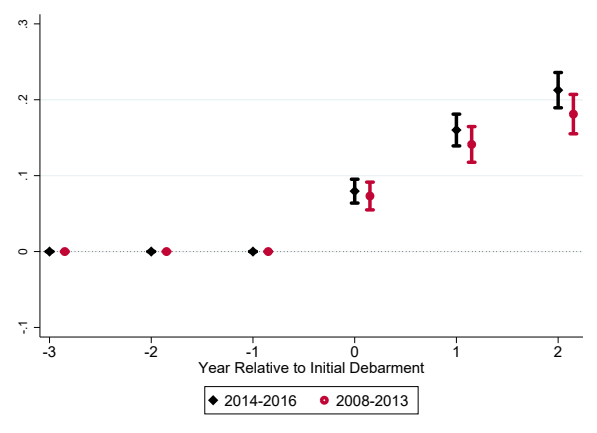
Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on several establishments' outcomes restricted to the formal sector: log employment, likelihood of exiting the formal sector, log (unconditional) earnings and log (unconditional) monthly payroll using information from both CEIS and RAIS data. The estimation sample consists of annual window $[-3, 2]$ around debarment between 2008 and 2013. All columns refer to Equation (2.1). Means of dependent variables are computed from pre-event years $[-3, -1]$ of the matched control group. Standard errors are clustered at the firm level.

Figure 2.D.1: Robustness Check: Debarment Prior to 2014

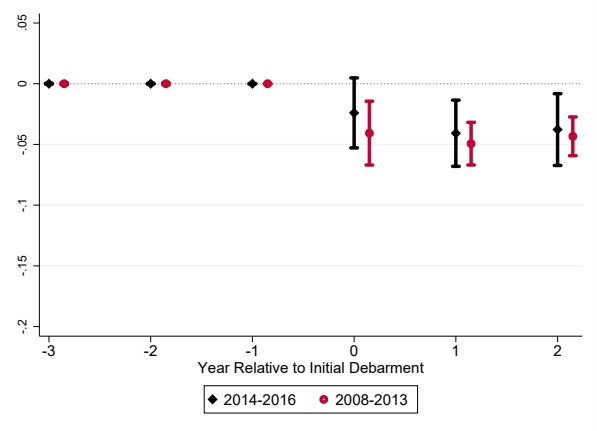
(a) Log Employment - Establishment



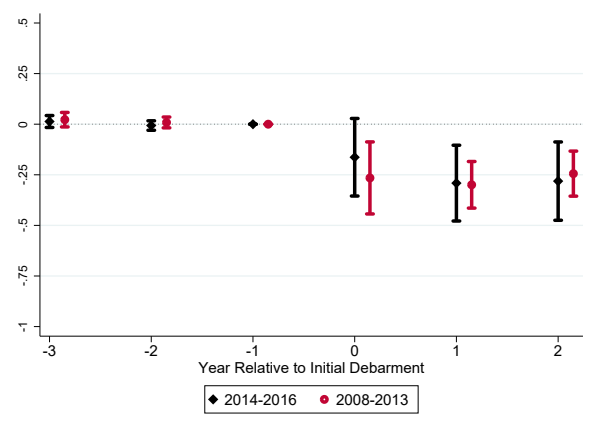
(b) Exit - Establishment



(c) Employment - Worker



(d) Log (Uncond.) Earnings - Worker



Note: This figure reports point estimates of the annual effects of debarment on selected outcomes considering establishments that have been debarred between 2014 and 2016 (in black) and between 2008 and 2013 (in red) separately. The omitted category is the year before debarment. More details can be found in Tables 2.3, 2.4, 2.D.1, and 2.D.2.

Table 2.D.2: Effects of Debarment on Workers' Outcomes Prior to 2014

	(1) employment	(2) log earnings (uncond.)	(3) log earnings (cond.)
PostDebarment	-0.044***	-0.280***	0.014
	(0.009)	(0.063)	(0.012)
Sample Size	474,156	474,156	436,559
Worker FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var (Control)	1	7.19	7.19

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on workers' outcomes in the formal sector: indicator for employment, log unconditional earnings and log conditional earnings using information from both CEIS database and RAIS data. The estimation sample consists of annual window [-3,2] around debarment between 2008 and 2013. All columns refer to Equation (2.2). Number of workers, treated workers and worker-year pairs is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.

"No Employee" Restriction

Table 2.D.3: Effects of Debarment on Establishments' Outcomes: No "Employee" Restriction

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
PostDebarment	-0.507***	0.125***	-0.941***	-1.439***
	(0.030)	(0.008)	(0.062)	(0.085)
Sample Size	55,788	55,788	55,788	55,788
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
# Establishments	9,298	9,298	9,298	9,298
# Debarred Establishments	4,649	4,649	4,649	4,649
Mean Dep. Var (Control)	2.50	0.13	6.40	8.76

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on workers' outcomes in the formal sector: indicator for employment, log unconditional earnings and log conditional earnings using information from both CEIS database and RAIS data. I relax the tenure restriction described in Section 2.3.4 by allowing establishments to not have any employee in any of the three years prior to debarment. Further details on how the sample is constructed can be found in Section 2.5.4. The estimation sample consists of annual window [-3,2] around debarment between 2014 and 2016. All columns refer to Equation (2.2). Number of workers, treated workers and worker-year pairs is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.

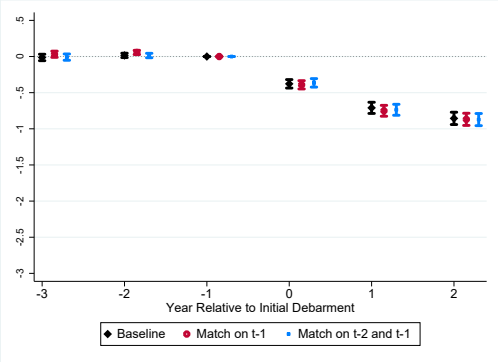
Alternative Matching Algorithms

A. Matching on the Level

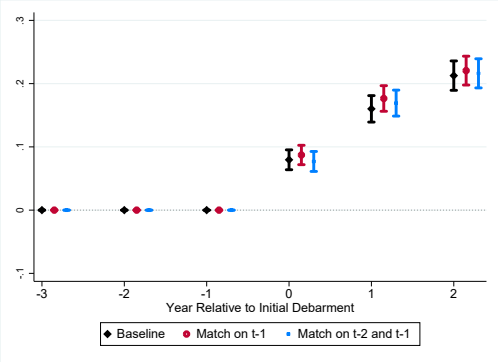
Instead of matching on three years before official debarment ($[t-3; t-1]$), I alternatively implement matching on the level $(t-1)$, considering the year before debarment, and matching on two years before debarment ($t-2$ and $t-1$).

Figure 2.D.2: Robustness Check – Matching on Levels

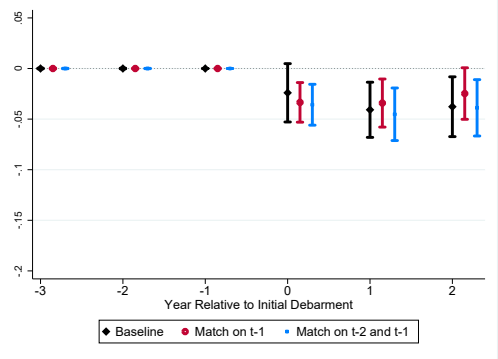
(a) Log Employment - Establishment



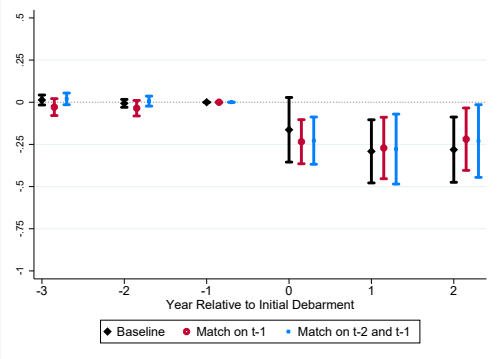
(b) Exit - Establishment



(c) Employment - Worker



(d) Log (Uncond.) Earnings - Worker

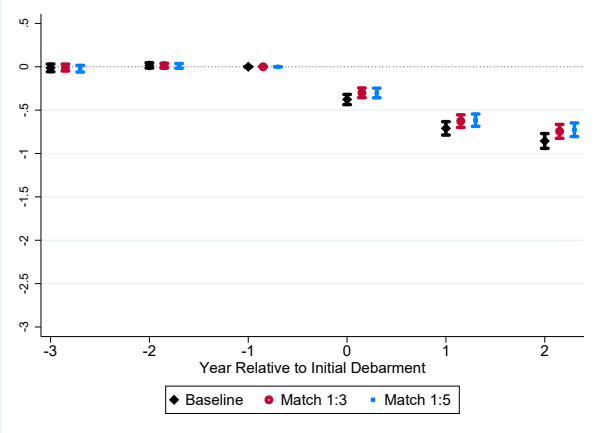


Note: This figure reports point estimates of the annual effects of debarment on selected outcomes considering three different matching algorithms: matching on three years before debarment (in black), matching on the year before debarment (in red), and matching on the two years before debarment (in blue). More details can be found in Section 2.5.4 and in Tables 2.3, 2.4, and 2.D.4.

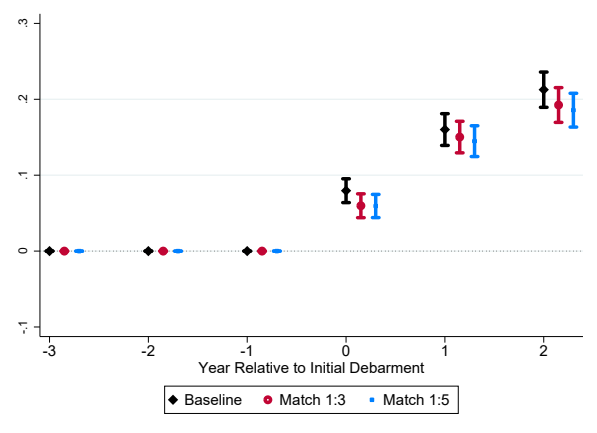
B. Multiple Candidates

Figure 2.D.3: Robustness Check – Multiple Candidates

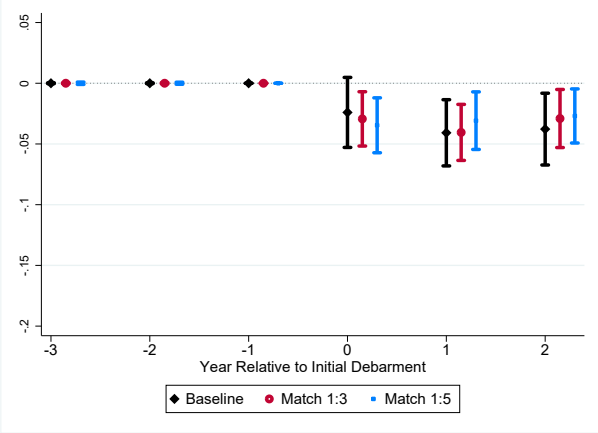
(a) Log Employment - Establishment



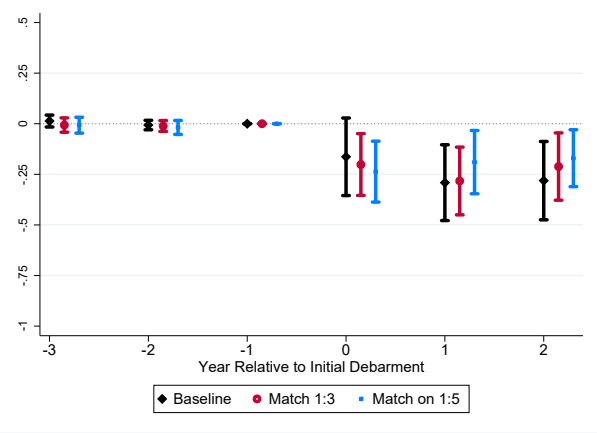
(b) Exit - Establishment



(c) Employment - Worker



(d) Log (Uncond.) Earnings - Worker

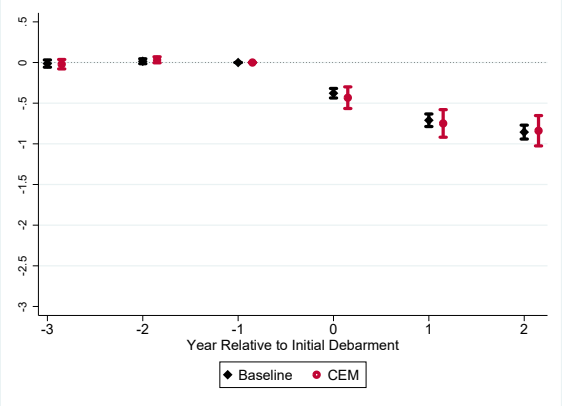


Note: This figure reports point estimates of the annual effects of debarment on selected outcomes considering three different matching algorithms: one-to-one matching (in black), one-to-three matching (in red), and one-to-five matching (in blue). More details can be found in Section 2.5.4 and in Tables 2.3, 2.4, and 2.D.4.

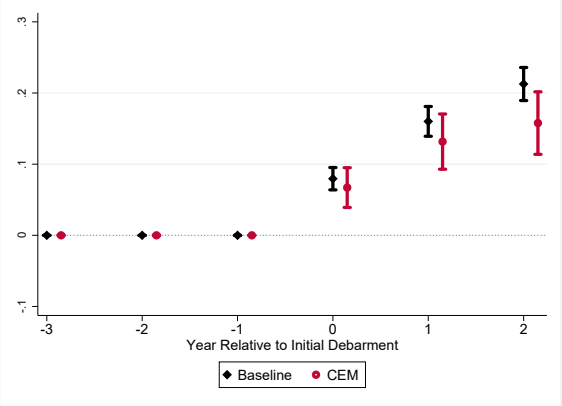
C. Coarsened Exact Matching

Figure 2.D.4: Robustness Check – Coarsened Exact Matching

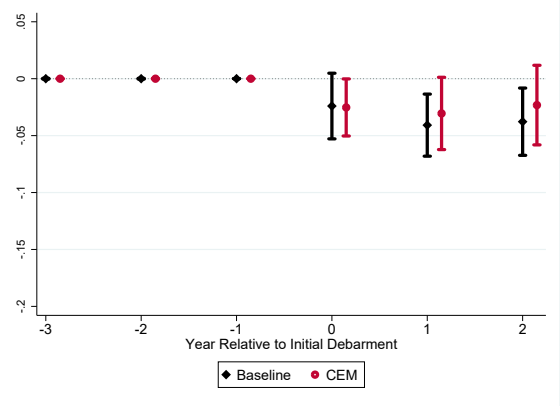
(a) Log Employment - Establishment



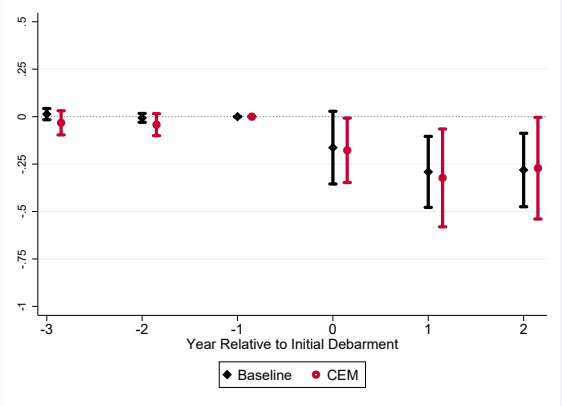
(b) Exit - Establishment



(c) Employment - Worker



(d) Log (Uncond.) Earnings - Worker



Note: This figure reports point estimates of the annual effects of debarment on selected outcomes considering two different matching algorithms: the baseline matching described in Section 2.3.4 (in black) and one-to-one coarsened exact matching (in red). More details can be found in Section 2.5.4 and in Tables 2.3, 2.4, and 2.D.4.

Table 2.D.4: Robustness Checks

	(1) log employment	(2) exit	(3) employment	(3) log earnings (uncond.)
		Panel A: Baseline		
PostDebarment	-0.649***	0.151***	-0.034**	-0.247**
	(0.035)	(0.009)	(0.014)	(0.096)
		Panel B: Match on t-1		
PostDebarment	-0.700***	0.161***	-0.031***	-0.220***
	(0.034)	(0.008)	(0.011)	(0.080)
		Panel C: Match on t-2 & t-1		
PostDebarment	-0.660***	0.154***	-0.040***	-0.254***
	(0.035)	(0.009)	(0.012)	(0.092)
		Panel D: 1:3 Matching		
PostDebarment	-0.557***	0.134***	-0.033***	-0.226***
	(0.033)	(0.009)	(0.011)	(0.079)
		Panel E: 1:5 Matching		
PostDebarment	-0.542***	0.129***	-0.031***	-0.190***
	(0.033)	(0.008)	(0.011)	(0.071)
		Panel F: Coarsened Exact Matching		
PostDebarment	-0.678***	0.119***	-0.026*	-0.232**
	(0.078)	(0.016)	(0.015)	(0.108)
Establishment FE	✓	✓	×	×
Worker FE	×	×	✓	✓
Year FE	✓	✓	✓	✓
Unit	Establishment	Establishment	Worker	Worker

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on selected establishments' and workers' outcomes: log employment, exit, indicator for employment, and log unconditional earnings. Columns (1) and (2) refer to Equation (2.1), while Columns (3) and (4) refer to Equation (2.3). Panel A considers the baseline results after implementing a one-to-one matching on all three years prior to debarment. Panels B and C report estimates after one-to-one matching on the year and on two years prior to debarment. Panels D and E refer to one-to-three and one-to-five matching. Panel F implements a one-to-one coarsened exact matching. Further details can be found in Section 2.5.4. Standard errors are clustered at the firm level.

Intensity

Table 2.D.5: Treatment Intensity: Establishment-Level Analysis

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
Panel A: Less Intense Treatment (N = 20,496)				
PostDebarment	-0.374***	0.104***	-0.793***	-1.154***
	(0.042)	(0.011)	(0.083)	(0.115)
Panel B: More Intense Intense Treatment (N = 16,704)				
PostDebarment	-0.986***	0.208***	-1.585***	-2.575***
	(0.059)	(0.014)	(0.101)	(0.148)
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the heterogeneous effects of debarment on establishments' outcomes by treatment intensity using information from both CEIS and RAIS data. Panels A and B show the results after restricting the sample to establishments with debarment length below and above the median (12 months) to represent less and more intense treatments, respectively. The estimation sample consists of annual window $[-3, 2]$ around debarment. All columns refer to Equation (2.1). Standard errors are clustered at the firm level.

Table 2.D.6: Treatment Intensity: Worker-Level Analysis

	(1) employment	(2) log earnings (uncond.)	(3) log earnings (cond.)
Panel A: Less Intense Treatment			
PostDebarment	-0.016	-0.142	-0.005
	(0.022)	(0.149)	(0.016)
Sample Size	561,372	561,372	510,693
Panel B: More Intense Treatment			
PostDebarment	-0.058***	-0.391***	0.006
	(0.011)	(0.078)	(0.014)
Sample Size	402,636	402,636	361,960
Worker FE	✓	✓	✓
Year FE	✓	✓	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the heterogeneous effects of debarment on workers' outcomes by treatment intensity using information from both CEIS database and RAIS data. The estimation sample consists of annual window [-3,2] around debarment. Panels A and B show the results after restricting the sample to establishments with debarment length below and above the median (12 months) to represent less and more intense treatments, respectively. All columns refer to Equation (2.2). Standard errors are two-way clustered at the worker and pre-event firm levels.

Table 2.D.7: Treatment Intensity: The Information Shock Channel

	(1) employment	(2) employment	(3) log earnings	(4) log earnings
Panel A: Less Intense Treatment (N = 288,252)				
PostDebarment	-0.008	-0.008	-0.109	-0.110
	(0.015)	(0.013)	(0.109)	(0.100)
Panel B: More Intense Treatment (N = 290,280)				
PostDebarment	-0.023*	-0.021**	-0.181**	-0.170**
	(0.012)	(0.011)	(0.090)	(0.085)
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Worker Controls	×	✓	×	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table tests the information shock channel by estimating the heterogeneous effects of debarment on employment and log unconditional earnings in the formal sector by treatment intensity using information from both CEIS database and RAIS data. The estimation sample consists of annual window [-3,2] around debarment. Panels A and B show the results after restricting the sample to establishments with debarment length below and above the median (12 months) to represent less and more intense treatments, respectively. All columns refer to Equation (2.2). In Columns (2) and (4), I add time-varying controls, such age and age squared. Standard errors are two-way clustered at the worker and pre-event firm levels.

Firm-Level Results

Table 2.D.8: Effects of Debarment on Firms' Outcomes Using Firm-Level Sample

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
PostDebarment	-0.635***	0.143***	-1.096***	-1.732***
	(0.035)	(0.009)	(0.064)	(0.091)
Sample Size	38,484	38,484	38,484	38,484
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean Dep. Var (Control)	3.21	0	7.13	10.22

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on several firms' outcomes restricted to the formal sector: log employment, likelihood of exiting the formal sector, log (unconditional) earnings and log (unconditional) monthly payroll using information from both CEIS and RAIS data. The firm-level estimation sample consists of annual window $[-3, 2]$ around debarment. Means of dependent variables are computed from pre-event years $[-3, -1]$ of the matched control group. Standard errors are clustered at the firm level.

Table 2.D.9: Effects of Debarment on Workers' Outcomes Using Firm-Level Sample

	(1) employment	(2) log earnings (uncond.)	(3) log earnings (cond.)
PostDebarment	-0.029***	-0.214***	-0.008
	(0.011)	(0.073)	(0.011)
Sample Size	1,274,652	1,274,652	1,158,547
Worker FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var (Control)	1	7.33	7.33

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on workers' outcomes in the formal sector: indicator for employment, log unconditional earnings and log conditional earnings using information from both CEIS database and RAIS data. The firm-level estimation sample consists of annual window [-3,2] around debarment. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.

Chapter 3

Redistricting, Decentralization, and Local Development[1]

3.1 Introduction

Across the world and throughout history, political boundaries have been relevant drivers of economic development. With the goal of devolving power to local governments, many countries have undertaken changes in political boundaries over the last 30 years, and many more will likely follow suit (Grossman and Lewis, 2014). A large theoretical literature has put forward various competing implications of redistricting for economic development. Proponents argue that the creation of smaller government units encourages competition in the provision of public goods (Tiebout, 1956), policies tailored to local preferences (Oates, 1972;

[1]We thank Seema Jayachandran, Nancy Qian, and Chris Udry for their invaluable advice. We also thank anonymous referees, Lori Beaman, Nicola Bianchi, Prashant Bharadwaj, Leah Boustan, Mariana Carvalho, Alexandra Cirone, Henry Farber, Thomas Fujiwara, David Lee, Riccardo Marchingiglio, Alexandre Mas, Stelios Michalopoulos, Ameet Morjaria, Joris Mueller, Christopher Neilson, Paul Niehaus, Jacopo Ponticelli, Miguel Talamas, Edoardo Teso, Owen Zidar, and participants at the NU Development Lunch, Princeton University, PUC-Rio, New Economic School, Universidad del Rosario, University of Mannheim, EESP, National University of Singapore, Singapore Management University, Brown University, and conferences for comments and suggestions. We thank Renan Holanda and Cláudio Júnior for excellent research assistance, and Rudi Rocha for kindly sharing data. This chapter subsumes our previous version in Dahis and Szerman (2021).

Boffa et al., 2016), and better monitoring of local governments (Besley and Case, 1995). Critics posit that it often comes at the cost of proliferating new government units unable to fund their operations. *Even when these new units have fiscal capacity to self-finance*, the rest of the country may bear the consequences of losses in scale and resources with worse economic outcomes (Alesina and Spolaore, 1997). This paper examines whether this is the case by assessing the long-run impacts of a large episode of redistricting through the creation of new local governments (henceforth "splitting").

Brazil provides an interesting empirical setting for studying the consequences of redistricting. First, the country is composed of municipalities holding substantial administrative, fiscal, and political decision-making power. Municipalities have the same horizontal administrative structure, and contain one or more districts without any decision-making power. Second, Brazil experienced one of the largest within-country splitting episodes worldwide. As a result of generous federal subsidies and flexible redistricting regulations between 1988 and 1996, the number of municipalities jumped from 4,124 to 5,507, a 34 percent increase. Formerly districts, these new municipalities gained power and became responsible for roles entitled to local governments, such as overseeing the provision of several public services, collecting local taxes, and managing fiscal revenues. Heeding concerns about the rapid increase of new municipalities, a reform in 1996 curbed their proliferation. Features of the institutional context create useful quasi-experimental variations for identification. Third, the availability of rich information on public service delivery, economic activity, and fiscal performance provides an unusual opportunity to assess the short- and long-run impacts.

One challenge to quantifying the economic impacts of redistricting is that splits across the world are typically voluntary and reflect choices, and these choices may reflect some underlying characteristics that also affect local development. In our context, areas requesting to split are less developed, neglected by their headquarters. They are also eligible for larger federal transfers once they become municipalities due to a non-linear transfer alloca-

tion mechanism that disproportionately benefits less populated municipalities (Tomio, 2002). Comparing municipalities that split to those that did not unlikely identifies the causal impacts of redistricting, contributing to the dearth of convincing empirical evidence that have long challenged the literature.

To overcome this challenge and to document and characterize the selection into redistricting, we build a new dataset containing areas that requested to split. We collect and classify digitized historical archives of requests covering 63 percent of all split cases between 1988 and 1996. Our difference-in-differences design compares areas with ratified requests to untreated areas whose requests to split were not approved due to political reasons or the 1996 reform. Because *almost split* areas applied and failed to split for reasons unrelated to specific factors that could also affect local development, they form a counterfactual to those that ultimately split. We document that treated and control areas exhibit similar level and trends over various economic outcomes prior to splitting, lending support to the causal interpretation of our difference-in-differences estimates.

We find that redistricting improves public services delivery. Relative to their counterfactual, municipalities that split experience higher capital (e.g., machinery and buildings) and current (e.g., payroll and administrative costs) expenses by around 27 and 15 percent in order to set up new local governments. The growth of the public sector translates into improvements in public services. We find increases in household access to sewage and trash collection ranging from 1 to 4.4 percent. Consistent with higher availability of educational infrastructure, heterogeneity across age from the individual-level Census data also reveals that the positive effects on school attendance and literacy rates are largest for younger and, therefore, more exposed cohorts.

Having demonstrated that redistricting expands bureaucracy and public services, we next examine the effects on economic activity *beyond* the public sector. Turning to matched employer-employee records, we find no evidence that the private sector expands in the same

degree as the public sector. These aggregate impacts, however, mask substantial heterogeneity across economic sectors. We find an increase in new establishments from the retail and services sectors, suggesting some degree of structural transformation towards nontradable sectors. In addition, spatial data from satellite-recorded nighttime lights further confirm that municipalities that split experience an increase in economic activity (Chen and Nordhaus, 2011; Henderson et al., 2012; Pinkovskiy and Sala-i Martin, 2016; Henderson et al., 2018). Exploiting the granularity of the data, we disaggregate the municipality-level effect by estimating the effects on districts that voluntarily and involuntarily split separately. We uncover a relevant distributional incidence: the increase in luminosity is concentrated in districts who voluntarily applied to split, whereas the remaining districts who involuntarily split are unaffected.

Our main findings hold for a variety of robustness checks, including alternative definitions of outcomes, samples, and specifications with a large number of flexible controls to account for observable baseline differences between treated and control areas. We also observe similar patterns when breaking down our results by waves of splitting, strengthening the internal validity of our findings. To address concerns related to unobserved factors influencing our estimates, we propose a novel research design. Prior to 1996, areas requesting to split had to conduct local referenda and obtain approval by simple majority. We leverage this rule in a difference-in-discontinuities design applied to a representative state, Minas Gerais, where referendum results are available. Comparing areas that barely obtained the majority of necessary votes to split to those that did not, we document qualitatively similar results.

What can explain the gains in public services delivery and economic activity? The first possibility is that redistricting leads to larger fiscal revenues. Areas that split experience an average increase in revenues of 14 percent after splitting because the federal government increases transfers to newly created local governments. These extra revenues might be spent on building infrastructure and expanding bureaucracy, generating automatic increases in

public services and economic activity. The second hypothesis is that redistricting implies decentralization of decision-making power to new local governments. With administrative, fiscal, and political autonomy, they might better serve local needs.

We find evidence consistent with decentralization as the main driver of local development. First, improvements in public services after splitting are concentrated in activities exclusively controlled by local governments. We find no such gains for activities shared with federal and state governments. Second, the economic impacts are largest for small, rural, and remote areas that tend to be more captured and neglected by former headquarters. Consistent with administrative remoteness representing an important barrier to state capacity, this result indicates that the benefits of autonomy are stronger for peripheral, remote backward areas. This is one of the major goals of decentralization in developing countries (Bardhan, 2002). Third, turning to historical electoral data, we uncover that, after splitting, new municipalities elect politicians affiliated with different political parties than politicians from their parent municipalities. This suggests that new municipalities are able to implement policies that better reflect the local preferences, achieving one of the premises of decentralization (Oates, 1972). Fourth, two complementary pieces of evidence weaken the role of extra revenues in explaining the main economic effects. Our mediation analysis shows that extra revenues only explain a small part of the increases in public services and economic activity. Additional empirical exercises also reveal that, even though areas that did not voluntarily split experience changes in fiscal revenues, their economic outcomes do not change. Overall, our empirical findings point to decentralization in form of higher decision-making autonomy, rather than extra revenues, as the primary channel through which redistricting spurs local economic development.

A final analysis investigates whether the policy imposes a negative burden to the rest of the country due to losses in resources. Leveraging state-level variation in loss of revenues from federal transfers caused by splitting, we find that municipalities without changes in political

boundaries remain unaffected. Therefore, our results reject the view that redistricting is a "zero sum game" among Brazilian municipalities. Our findings are also consistent with the lost revenues previously subsidizing wasteful expenses, whose marginal value was below the social costs of funds (Liebman and Mahoney, 2017). The reallocation of resources can decrease low-value spending in areas that did not voluntarily split and raise aggregate welfare.

Our findings have relevant policy implications. First, settings with large government units, like the Brazilian case, can benefit from subsidized and voluntary splits. We find that new municipalities drive the gains in public services and economic activity. At the same time, our data reject that the policy is detrimental to the rest of the country. Second, we show that the benefits are largest for peripheral, remote backward areas, suggesting that redistricting through splitting can strengthen state capacity and achieve one of the main goals of decentralization: make local governments more responsive and efficient in promoting long-run development (Bardhan, 2002).

This paper contributes to several lines of research. At a broader level, it speaks to an extensive literature, theoretical and empirical, studying the causes and consequences of decentralization through redistricting.[2] Building on seminal Tiebout (1956) and Oates (1972)' works, Alesina and Spolaore (2005) summarize the main trade-offs of smaller government units: the costs of losing scale and resources to provide public goods may be counterbalanced by the benefits of more homogeneous units and local power. In contrast to a wide range of theoretical implications of redistricting for economic development, empirical evidence on the actual consequences are scarce. Our contribution is threefold. First, rather than leveraging cross-country or cross-sectional variations, we overcome the endogeneity of redistricting

[2]We note that other terms have also been used to study decentralization (Oates, 1972, 1999; Bardhan, 2002; Faguet, 2004, 2014; Barankay and Lockwood, 2007; Treisman, 2007; Gadenne and Singhal, 2014; Mookherjee, 2015), such as size of nations (Bolton and Roland, 1997; Alesina and Spolaore, 1997, 2005; Lassen and Serritzlew, 2011), local government proliferation or fragmentation (Grossman and Lewis, 2014; Pierskalla, 2016; Grossman et al., 2017), border reforms (Coate and Knight, 2007; Boffa et al., 2016; Bazzi and Gudgeon, 2021), amalgamations (Weese, 2015), and municipal cooperation and annexation (Schönholzer and Zhang, 2017; Ferraresi et al., 2018; Tricaud, 2021)

by proposing two complementary quasi-experimental approaches that generate qualitatively similar results. Second, we provide novel evidence on the distributional incidence of redistricting because our granular data allow us to also examine heterogeneous impacts across and within areas involved in border changes, voluntarily and involuntarily. Third, the time horizon of the data permits an assessment of how the local impacts evolve over time. The richness of the data also allows us to conduct a comprehensive overview beyond the programs administrated by local governments, including whether higher public expenditures crowd-in the private sector, and to adjudicate between mechanisms hypothesized by theories of decentralization.[3]

The fact that peripheral, remote backward areas benefit the most from redistricting contributes to a large literature on the effects and spillovers of policies aimed at reducing regional inequality. Examples of policies include public investments and tax incentives towards disadvantaged geographic areas (Busso et al., 2013; Kline and Moretti, 2014; Shenoy, 2018; Slattery and Zidar, 2020), extra grant revenues (Caselli and Michaels, 2013; Litschig and Morrison, 2013; Gadenne, 2017; Corbi et al., 2019), fiscal decentralization (Martínez-Vázquez et al., 2017; Bianchi et al., 2023), and transportation-based policies (Michaels, 2008; Faber, 2014; Banerjee et al., 2020a). Related to the public sector, our context points that administrative remoteness and neglect from the headquarters regions correlate with regional inequality, and that splitting mitigates these frictions and generates subsequent positive and persistent economic consequences to peripheral regions.[4] These findings are also related to the literature studying how geographical isolation and size affect state capacity and devel-

[3]Lima and Silveira Neto (2018) also compare municipalities that split and did not split to examine the impacts of splitting on fiscal outcomes. Relative to this paper, we propose new empirical strategies that corroborate the difference-in-differences approach, examine both the average and distributional implications for local development, and scrutinize the mechanisms. At the national level, our results contribute to the policy debate on the creation of new municipalities, often deliberated upon in the Brazilian National Congress (Tomio, 2002; Mattos and Ponczek, 2013; Lipscomb and Mobarak, 2017).

[4]Outside the public sector, several papers have document that the distance between headquarters and branches in multi-location firms represents an important source of wage inequality across space (Gumpert et al., 2022; Hazell et al., 2022; Kleinman, 2022).

opment (Ashraf et al., 2010; Stasavage, 2010; Nunn and Puga, 2012; Michalopoulos and Papaioannou, 2014; Campante et al., 2019). We advance this literature by evaluating the implications of reducing administrative remoteness.

Our empirical findings build on a range of work investigating the determinants of state capacity (Besley and Persson, 2010; Acemoglu et al., 2015; Gennaioli and Voth, 2015; Johnson and Koyama, 2017). In a seminal work, Besley and Persson (2009) conceptualize state capacity, which also depends on the distribution of economic and political power. Agency problems within government can severely limit state capacity, stymieing the implementation of policies targeted to local conditions (Mookherjee, 2015; Banerjee et al., 2020b). Consistent with the theoretical literature, the within-country policy experiment we study changes the nature of the agency problems and enables us to show how the size of local government influences economic outcomes. Evidence that the creation of new local governments can serve the dual purpose of expanding state capacity, like growing bureaucracy and improving public services delivery, in peripheral regions, and freeing these regions from capture of former governments is a novel result. Interestingly, these benefits do not require increased expenditures from higher levels of government, like the federal or state governments, or shifts in economic activity from one area to another.[5] This is relevant from a policy perspective given that developing countries often need to improve state capacity under severe budget constraints, yet there is little evidence of policies that can achieve similar long-run development in such settings.

The rest of the paper is organized as follows. Sections 3.2 and 3.3 describe the institutional background and the sources of data. Sections 3.4 and 3.5 present the empirical strategy and the main results. In Section 3.6, we analyze potential mechanisms underlying the main effects. Section 3.7 discusses the net effects of redistricting. Section 3.8 concludes.

[5]Our finding that the growing bureaucracy is associated with improvements in public services delivery is also related to a large literature on the personnel economics of the state (Evans and Rauch, 1999; Finan et al., 2017; Dahis et al., 2020; Besley et al., 2022).

3.2 Institutional Background

3.2.1 The Role of Municipal Governments

Brazil has three tiers of government holding administrative, fiscal, and political power: federal, state, and municipal governments. Municipalities are the smallest government units with decision-making power. For organizational purposes, they are divided into districts without any political or administrative autonomy. One municipality may be composed of one or more districts. No district belongs to two different municipalities.

The enactment of the Federal Constitution in 1988 represents the most important step towards fiscal federalism and *vertical* decentralization of administrative, fiscal and political power in the country (Arretche, 2000; Favero, 2004).[6] Since 1988, municipalities are responsible for overseeing the provision of several public services, including primary education, basic health care, sanitation, trash collection, and street lighting services. Municipalities share mandate on provision of certain public services, such as sanitation and health care. For other services, like primary education, they are the sole provider. In addition, municipalities have fiscal autonomy to collect and manage local taxes (e.g., property and service taxes) and to administer their own fiscal revenues (e.g., inter-governmental transfers and local revenues).

Every four years, there are municipal elections in October to elect mayors and municipal councillors.[7] In January after elections, the elected public officials take office.

[6] As opposed to *horizontal* decentralization, which consists of government units breaking into smaller ones with the number of tiers and the allocation of functions held constant, *vertical* decentralization implies the creation of new tiers of government or the transfer of functions from a higher tier to a lower one. In addition, administrative decentralization generally indicates that different government tiers execute various functions and policies. Superior tiers may reverse and overrule decisions made by lower ones. Political decentralization includes *appointment* decentralization, *decision-making* decentralization, or *constitutional* decentralization. Fiscal decentralization involves decision-making decentralization on taxation and expenditure matters (Ebel and Yilmaz, 2002; Treisman, 2007).

[7] In municipalities with fewer than 200,000 voters, there is a single-round system, and the candidate for mayor receiving the majority of votes is elected. Larger municipalities have a two-round system: in case no mayoral candidate gets at least 50 percent of votes, there is a second round with the two most voted

3.2.2 The Creation of New Municipalities

The 1988 Federal Constitution also granted to states the authority to establish their own criteria regarding the creation and amalgamation of municipalities. The requirements, which varied across states, generally involved territorial contiguity, a minimum population, and some level of urban development for new municipalities.

The creation of a new municipality required a multi-stage process: (1) local leaders or state politicians representing an applicant area had to formally request to state assembly the creation of a new municipality; (2) the state legislative committee responsible for the request evaluated and approved it; (3) the state legislature authorized a referendum in the applicant area, although the state governor could veto it; (4) if the majority of voters in the local referendum voted in favor of splitting, the request was put forward for voting in the state legislature; (5) the state and federal governments had to approve or veto the request (Tomio, 2002). In practice, such vetoes were rare. These flexible rules led to a unique episode of *horizontal* decentralization in the first half of the 1990s, with an unprecedented number of districts initiating requests to split and ultimately becoming municipalities.

In light of the rapid rise in number of new municipalities and the subsequent concerns that such splits were inefficient and driven by local patronage, the Congress enacted the Constitutional Amendment 15/1996 (henceforth "1996 CA"). The 1996 CA reassigned to the federal government the authority to regulate over the creation and amalgamation of municipalities. Three major changes stand out. First, districts requesting to split have to conduct a referendum with the entire municipality, not only with the applicant districts, and obtain approval from the majority of voters. Second, the federal government requires evidence of fiscal sustainability from the applicant districts. Third, redistricting would depend on a further, albeit never enacted, legislation by the federal government. As a result, the

first-round candidates. The candidate receiving most votes wins. Municipal councillors are elected through an open-list proportional representation system.

1996 CA induced a *de facto* halt in the creation of new municipalities and left various split requests from districts open and unapproved.

Once the request to split is approved, the applicant district (or group of applicant districts) is established as a new municipality after the ensuing municipal elections, when the elected mayor and municipal councillors take office. Consistent with flexible redistricting regulations, Figure 3.1 displays a 34 percent increase in the number of municipalities between 1989 and 1997, jumping from 4,124 to 5,507. We note two two main waves of splitting before the 1996 CA, both in 1993 and 1997, right after municipal elections.

3.2.3 The Reasons for Splitting

Many factors have contributed to the redistricting process in Brazil. We highlight two factors: neglect from the headquarters and fiscal incentives. Several past studies have put forward large disparities in the provision of public services across districts within a municipality as a key driver of split requests (Cachatori and Cigolini, 2013; Klering et al., 2012). In a survey with mayors in 1992, Bremaeker (1993) confirms that most respondents reported neglect by local governments (63 percent) and large territorial size of local governments (24 percent) as the main reasons for splitting.

Fiscal incentives are also relevant. The creation of new municipalities affects the distribution of the *Fundo de Participação dos Municípios* (henceforth "FPM"), the main fund through which the federal government provides transfers to municipalities. The fund has the following allocation mechanism: every year, 22.5 percent of total revenues from federal income and industrial product taxes are reserved for FPM. Each state receives a block grant to be shared between its municipalities, implying that transfers are zero-sum within state. Each municipality then obtains a share determined by a concave step-wise population-based formula that assigns coefficients to population brackets. In practice, the formula disproportionately benefits small municipalities because transfers per capita are larger in lower

population brackets. Municipalities within the same state and bracket obtain the same amount of transfers.[8] In addition, 15 percent of FPM transfers are earnmarked for education and health each, and the rest is unearnmarked (Brollo et al., 2013). On average, federal transfers account for between 30 and 60 percent, whereas local taxation and fees represent 5 percent of total municipal revenues.

When splits occur and new municipalities are established, they start receiving FPM transfers. Most splits are concentrated in small municipalities, pointing to fiscal incentives as a relevant driver of split requests. We note, however, that the net change in FPM transfers for the headquarters and remaining areas is unclear because it depends on a combination of factors: the allocation of funds within the municipality prior to splitting, the curvature of the FPM curve, and the quantity of splits within the state.

To evaluate the different forces at play and their empirical predictions, Appendix Section A outlines a simple conceptual framework in which the municipal headquarters choose the allocation of public goods across districts within the municipality. The model illustrates two key predictions to motivate our empirical analysis. First, under certain conditions, districts applying to split may benefit more from it. Second, the consequences of splitting for headquarters districts and the rest of the country may be negligible.

3.3 Data

This paper uses a newly collected data on split requests, together with different sources of spatial and administrative data, to capture public service delivery, economic activity, and fiscal performance margins along which redistricting impacts local development.[9]

[8] Litschig (2012); Brollo et al. (2013); Litschig and Morrison (2013); Gadenne (2017) and Corbi et al. (2019) exploit discontinuities in population brackets to estimate the effects of transfers on economic outcomes.

[9] To our knowledge, there is no historical data on housing prices.

Split Requests. We gather information on the official creation dates of municipalities, along with their parent municipalities prior to split, from the Brazilian Institute of Geography and Statistics (IBGE). To catalog split requests, we collect and classify historical archives of such split requests. The availability, level of detail, and quality of these archives largely vary across states because state assemblies set their own redistricting requirements prior to the 1996 CA. The final data include split requests, regardless of their final approval status, initiated by districts from 11 states (Amapá, Espírito Santo, Goiás, Mato Grosso, Minas Gerais, Pará, Paraná, Rio Grande do Sul, Rondônia, Santa Catarina, and São Paulo). This sample represents 41 percent of all states, and covers 58 percent of the Brazilian population and 63 percent of splits between 1989 and 1996. The remaining states do not provide public records on split requests. Section C of the Appendix describes the data collection in detail.

We also scrape legislative reports on referendum results for the state of Minas Gerais. To our knowledge, this is the only state with publicly available records, which include information on turnout and percentage of valid votes in favor of splitting. We validate information from reports by cross-checking them with our data on split requests.

Demographic Census. Information on public service delivery (e.g., household access to trash collection and sewage), along with demographic and socioeconomic characteristics (e.g., population size, urbanization rate, education, health, and income), come from the decennial Brazilian Demographic Census, and are only available at a decadal frequency. We use the 1991, 2000, and 2010 versions sourced from IBGE and the Atlas of Human Development (United Nations Development Program, 2013). For our baseline specification, we aggregate data at the municipality level because district identifiers are not available. We also use individual-level microdata on literacy and school attendance when exploiting variation across birth cohorts.

Formal Labor Market. We draw labor market information from the annual matched employer-employee data, the *Relação Anual de Informações Sociais* (RAIS), carried out by the Brazilian Ministry of Labor. The data cover the entire formal sector between 1995 and 2018, and provide a rich set of worker, job, and establishment characteristics (Szerman, 2023b). We use worker-level data to calculate the total number of employees and establishments in the public and private sectors at the municipality level. We also generate these variables by economic sector (i.e., agriculture, mining, manufacturing, construction, retail, and services) and areas (e.g., education and health). An important caveat is that we are not able to examine the impacts on the informal economy due to the lack of data covering the informal sector prior to 2000.[10]

Night Lights. To measure economic activity, we use satellite imagery of night-time lights organized by the U.S. National Oceanographic and Atmospheric Administration (NOAA) and the National Geophysical Data Center (NGDC).[11] The annual data consist of grids with integer values ranging from 0 (no light) to 63 that record the intensity of lights between 1992 and 2013.[12] Taking advantage of its granularity, we construct district-level information on the intensive and extensive margins of luminosity, measured by the weighted average of lights across grids within a district and whether this average is above zero. To quantify regional inequality, we calculate a luminosity Gini index from variation across pixels within each district.

Local Government Expenditures and Revenues. We collect information on expenditures and revenues at the municipality level from the Brazilian National Treasury. Available

[10]The 2000 Census is the first edition with information on the informal sector. According to the Ipeadata, the informal sector accounted for 56 percent of total employment in 1992.

[11]Intensity of night lights measures both outdoor and some indoor use of lights. Henderson et al. (2012) and Henderson et al. (2018) show that night lights are a good proxy for long-term GDP growth. This is useful in our context because there is no data on economic activity at the district level, and data on electricity consumption are only available for more recent years.

[12]A grid cell captures a 30 arc-second output pixel or 0.86 square kilometers at the Equator.

since 1989, the data details revenue sources (e.g., local taxation and intergovernmental transfers) and expenditure categories (e.g., capital and current expenses).[13]

Other data. We rely on minor sources of data to conduct our analyses. We gather geographic characteristics, such as soil suitability from FAO-GAEZ and terrain ruggedness from Shaver et al. (2019). To disentangle several mechanisms behind the results, we also use municipal-level electoral data from the Superior Electoral Court (TSE). Between 1988 and 1996, we only observe the elected mayor's name and party. Other information, like the list of mayoral candidates and vote shares, started to be reported in 2000.

Municipality-Level Sample. To build our main estimation sample, we begin with 4,298 municipalities from the 1991 Demographic Census and keep those that meet three criteria: (1) municipalities that belong to one of the 11 states with records on split requests; (2) municipalities with either a single split event or with districts having split requests between 1989 and 1996 to avoid multiple events; and (3) municipalities that are not state capitals because they also serve as the headquarters of state governments. The restrictions yield a final municipality-level sample of 448 municipalities.[14]

District-Level Sample. Due to data availability, we perform most of our analysis at the municipality level. We note, however, that the nighttime luminosity information are available at the district level, permitting additional insights on differences within and across municipalities. Starting with 8,855 districts from the 1991 Demographic Census, we apply restrictions similar to before to construct a district-level sample.[15] The restrictions leave us

[13]To our knowledge, there is no data on local tax rates during the 1990s. Anecdotal evidence, however, suggest that changes in local tax rates are uncommon and negligible.

[14]Changes in municipality boundaries are typically not nested. We adopt a standard procedure of harmonizing boundaries between 1991 and 2010 into minimum comparable areas (Lipscomb and Mobarak, 2017; Lima and Silveira Neto, 2018). This approach yields a sample of 4,298 minimum comparable areas, which we refer to as municipalities. We use this approach, instead of the list of 5,565 original municipalities in 2010, to keep the spatial units constant over time. We use the material publicly provided by Ehrl (2017).

[15]We keep districts that meet the following criteria: (1) districts that belong to one of the 11 states with records on split requests; (2) districts that do not belong to state capitals; (3) municipalities with requests

with a final sample of 1,259 districts. We next classify them into three groups: (1) *applicant* districts, which correspond to peripheral districts that requested to split; (2) *remaining* districts that contain peripheral districts that did not request to split but were located in municipalities where some district did so; and (3) *headquarters* districts, which cover districts serving as headquarters in municipalities that have a district requesting to split. Such classification leads to a final district-level sample of 552 applicants, 325 remaining, and 382 headquarters districts.

3.4 Empirical Strategy

We begin this section by characterizing the selection into redistricting. We then explain how we construct a credible counterfactual to areas that split, and discuss the difference-in-differences design we use to examine the effects of splitting.

3.4.1 Who Applies to Split?

To examine how municipalities select into splitting, Table 3.1 presents summary statistics of baseline characteristics in 1991 for the sample at the municipality level. The numbers confirm that, prior to splitting, municipalities with at least one applicant district (Column (1)) are comparable to those without any split requests (Column (3)) in various dimensions, including population composition and income. The exceptions are that municipalities with applicant districts are bigger in population and area, have slightly lower levels of public services, and receive lower share of federal transfers relative to total revenues. In contrast to countries like Indonesia or India (Pierskalla, 2016; Bazzi and Gudgeon, 2021), differences in racial and religious composition are small in magnitude, ruling out social fragmentation as an important driver of splitting.

to split initiated by districts, as opposed to areas smaller than districts, such as neighborhoods or parks; and (4) districts in municipalities with a single split event or with split requests between 1989 and 1996 to avoid multiple events.

We also examine how districts select into applying to split. Our district-level sample corroborates that districts requesting to split are less developed than other parts of the country. Comparing baseline characteristics between applicant (Column 1) and headquarters districts (Column 5), Table 3.2 shows that applicant districts display, on average, worse economic and demographic conditions before splitting. They are also smaller in population and area, less urban, and located farther from their parent town halls.[16]

3.4.2 Identification

Our goal is to examine how redistricting affects economic performance. To mitigate concerns related to selection into splitting, our estimation sample includes municipalities with an application to split. We then define municipalities containing a district that applied and failed to split as the control group. We note that these *almost split* municipalities form a credible counterfactual to those that ultimately split.[17] With initially good chances of approval, their requests were not sanctioned for reasons unrelated to specific factors also affecting economic performance. Reasons include vetoes from state legislative committees or governors, referenda without support from the majority, and the 1996 CA, which left requests initiated in 1994 and 1995 open without enough time to conclude the multi-stage process outlined in Section 3.2.2.[18]

The treatment group consists of municipalities that split. The control group with almost split municipalities includes never-treated units (i.e., units that applied to but never split)

[16]Interestingly, we find that applicant districts are larger and more developed than remaining ones (Column 3). Columns (7)–(10) also display summary statistics for districts outside the estimation sample. Districts who are not involved in splits are similar to those who are in most dimensions excluding population, area, urbanization, and terrain ruggedness.

[17]Figure 3.2 presents a simple diagram that compares split (blue) and almost split (orange) municipalities, as well as illustrates how municipalities are divided into applicant, remaining, and headquarters districts.

[18]Table 5 of Tomio (2005) provides statistics about which stage the requests to split were denied in the state of Rio Grande do Sul. Out of 398 requests, 64 percent of them ultimately passed; 10 percent were still left open; 13 percent were rejected in legislature commissions; 5 percent were rejected by the legislature's plenary; 6 percent were vetoed by the state governor; and 2 percent had the majority of local referendum voting against the split.

and excludes not-yet-treated units (i.e., units that applied to and split after the waves of 1993 and 1997). This division implies that the sample of 448 municipalities contains 324 split units and 124 almost split units. Figure 3.2 plots split and almost split municipalities. We highlight two patterns. First, requests to split are geographically scattered. Second, despite some degree of clustering due to redistricting regulations at the state level, we note a large geographical variation in split and almost split events.

We also apply similar classifications to our district-level sample of 552 applicants (441 split and 111 almost split units); 325 remaining (261 split and 64 almost split units); and 382 headquarters districts (292 split and 90 almost split units). Appendix Table 3.D.1 presents means for districts' baseline characteristics in 1991 across treatment status and split waves (1993 and 1997). Relative their almost split counterparts, districts that split have, on average, smaller population, larger area, and are located farther from their parent town halls. We also observe some degree of negative selection into splitting over time since districts involved in the latter wave have worse economic conditions.

3.4.3 Main Econometric Specification

To estimate the impacts of spitting on municipal outcomes, we estimate the following difference-in-differences specification restricted to the municipality-level sample:

$$y_{mst} = \alpha_m + \alpha_{s(m)t} + \sum_{\tau=-\underline{\tau}}^{\overline{\tau}} \beta_\tau Split_m \mathbf{1}[t - W_m = \tau] + \gamma Post_{mt} + \varepsilon_{mst}, \tag{3.1}$$

in which y_{mt} stands for outcomes for municipality m and state s in time t; α_m represents municipality fixed effects; $\alpha_{s(m)t}$ controls for state-by-time fixed effects; $Split_m$ is an indicator variable for whether the municipality m split; and $\mathbf{1}[t - W_m = \tau]$ are dummies indicating time relative to the wave-year W_m when municipality m split (either 1993 or 1997). Both the start time $\underline{\tau}$ and end time $\overline{\tau}$ depend on the data availability for the outcome of interest y_{mt}. We normalize $\beta_{-1} = 0$ so that our estimates are relative to the year before splitting,

1992 or 1996. The post-event coefficients of interest, β_τ, capture the dynamics effects of splitting relative to that year. The variable $Post_{mt} \equiv \mathbf{1}[t \geq W_m]$ indicates time periods after the municipality's wave-year. Standard errors are two-way clustered both at the state and split wave levels.

The impacts of splitting come from comparing treated municipalities to counterfactual municipalities that almost split and, therefore, are never treated. The inclusion of almost split municipalities assuages concerns related to event-study specifications that only rely on the variation in the timing of treatment (Goodman-Bacon, 2021; Borusyak et al., 2022). In addition, because our data only contain two waves of split, our results are also unlikely to be affected by issues on the variation in the timing of treatment raised by the recent difference-in-differences literature (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021). Indeed, our robustness checks show similar patterns when we break down our results by waves of splitting.

Identification assumptions rely on the timing of splitting being uncorrelated with the outcomes of interest, *conditional* on the set of controls. The key identifying assumption is that outcomes for treated and control municipalities would have followed parallel trends in $\tau \geq 0$ if no splitting had occurred. We test this assumption by assessing whether the pre-event coefficients of interest are statistically indistinguishable from zero.

Even restricting the sample to municipalities that applied to split and attesting parallel pre-trends, one might still be concerned that split and almost split municipalities differ in various dimensions. We address these issues in several ways. First, the inclusion of municipality fixed effects α_m mitigates concerns related to time-invariant characteristics of municipalities that might be correlated with both the splitting event and the outcomes of interest. Second, by adding state-by-time fixed effects $\alpha_{s(m)t}$ to Equation (3.1), we further narrow our comparison to municipalities within the same state. Third, we present year-by-year estimates of outcomes with annual data. Stable pre-trends and sharp effects around the exact time

of splitting provide reassuring evidence that we estimate the impacts of redistricting rather than the impacts of unobservable municipality-specific factors. Fourth, one of our robustness checks accounts for heterogeneous initial characteristics that can also influence the economic performance. We further control for baseline characteristics from Table 3.1 interacted with time fixed effects, permitting differential trends across municipalities with different initial characteristics.

To further rule out unobservable factors influencing our estimates and validate our findings, we leverage an extra feature from the institutional context. Prior to 1996, districts requesting to split had to conduct local referenda and obtain approval by simple majority. As a robustness check, we complement our difference-in-differences approach with a difference-in-discontinuities design exploiting final results from local referenda. Section 3.5.3 shows that both empirical approaches generate qualitatively similar estimates.

3.5 Main Results

We start by documenting how new municipalities set up local governments and to what extent redistricting improves public services delivery. We then show that redistricting has positive economic impacts beyond the public sector. We also find relevant distributional consequences: applicant districts drive the gains in economic activity, whereas the remaining and headquarters districts remain unaffected along this margin.

3.5.1 Setting up New Local Governments: The Effects on Public Services

Bureaucracy in the Public Sector. We examine how new municipalities set up new local governments. Panels (a) and (b) of Figure 3.3 show $\hat{\beta}_\tau$, along with 95 percent confidence intervals, after estimating Equation (3.1) for selected variables capturing local expenses.

Appendix Table 3.D.2 displays the aggregate impacts. The pre-event coefficients are statistically equal to zero, supporting the assumption that both split and almost split have similar pre-split trends. Following splitting, treated municipalities experience a sharp increase in public expenses.

Panel (a) of Figure 3.3 displays results for capital expenditures per capita. These expenses, which account for 16 percent of total municipal expenditures, refer to purchases of machinery, vehicles, buildings, and the like. We find a spike at around 40 percent in the year of splitting, followed by a stable increase of around 27 percent over the next 15 years. Panel (b) reports the results for current expenditures, which represent 84 percent of municipal expenditures and capture maintenance and operation costs of providing public service (e.g., payroll and administrative costs). Following splitting, current expenditures in treated municipalities increase by about 15 percent, a pattern that becomes stable and persistent over time. Lima and Silveira Neto (2018) argue that capital expenditures tend to be initially higher than current expenditures due to installation and entrance costs. Strict rules prohibiting indiscriminate hiring in the public sector also explain the stable trends in current expenditures, mostly payroll costs, after splitting.

We also turn to the richness of the matched employer-employee RAIS data to validate the previous findings. We quantify the impacts on the size of the bureaucracy in the public sector, measured by the number of public employees and total payroll in the local government. Panels (c) and (d) of Figure 3.3 and Appendix Table 3.D.2 report that splitting is associated with an average increase of around 23 percent in both dimensions. At the same time, we find no changes in public employment at the state and federal governments, confirming that the growth of the public sector exclusively comes from new municipalities.

Public Services Delivery. We next investigate to what extent the growth of the public sector influences public services delivery. Although the decennial Census data permit a rich analysis of multiple margins along which splitting affects public services delivery, an

important caveat is that we are not able to directly test for pre-trends because we can only use one data point, the 1991 Demographic Census, prior to the 1996 CA. We note that the lack of pre-trends for other outcomes with higher frequency from alternative data sources, like the RAIS data, helps alleviate this concern.

Figure 3.4 and Appendix Table 3.D.3 report coefficients after estimating Equation (3.1). We document that household access to trash collection and sewage increases by 4.4 and 1 percent (the former is significant at the 10 percent level), whereas we do not find significant impacts on household access to piped water and electricity. Interestingly, the impacts are weaker for public services whose mandate on provision is shared with the state and federal governments, such as water and sanitation sectors. These results are consistent with shared mandates generating lower investments in these public services due to uncertainty about which level of government is ultimately responsible for their provision (Kresch, 2020).[19]

We also employ a complementary empirical approach to estimate additional margins of response. Because pre-split data on public goods from the early 1990s, such as education and health infrastructure, are unavailable, we propose an indirect test exploiting variation in splitting across municipalities and birth cohorts (Duflo, 2001). If splitting causes an increase in the stock of schools, and both the year of birth and municipality of residence determine exposure to it, then younger cohorts more exposed to splitting would experience higher levels of schooling relative to older, less exposed cohorts.

Exploiting the Census data at the individual level and adapting Equation (3.1) to consider heterogeneity by age, we estimate the following specification :

$$y_{imst} = \alpha_{s(m)t} + \alpha_{k(i)m} + \alpha_{k(i)t} + \sum_{\tau=8}^{30} \beta_\tau Split_{mt} \mathbf{1}[k(i) = \tau] + X_i\lambda + \varepsilon_{imst}, \tag{3.2}$$

[19]Using municipal-level Census data, we find that municipalities that split experience higher literacy rates and years of education. We also find increases in preschool and middle school attendance, while we do not observe a similar pattern for high school attendance. We interpret these results as consistent with the division of roles between governments: municipalities are responsible for providing preschool and primary education, whereas state governments are in charge of high schools.

in which y_{imst} represents outcomes for person i in municipality m, state s, and year t; $\alpha_{s(m)t}$, $\alpha_{k(i)m}$, $\alpha_{k(i)t}$ are state-time, age-municipality, and age-time fixed effects, respectively; $Split_{mt}$ is an indicator variable for whether the municipality m split, and takes values equal to zero for $t = 1991$ and equal to one for years $t \in \{2000, 2010\}$ in municipalities that split; and $\mathbf{1}[k(i) = \tau]$ are dummies for each age. The term X_i refers to a vector of individual controls, such as gender, race, religion, and nationality. Standard errors are two-way clustered both at the state and split wave levels. The key identifying assumption is that birth cohorts in split and almost split municipalities would have followed parallel educational trends if no splitting had occurred for treated municipalities.

Consistent with higher investments in educational infrastructure, we document that younger cohorts from municipalities that split experience higher gains in school attendance and literacy rates. Figure 3.5 displays the $\hat{\beta}_\tau$, along with 95 percent confidence intervals. Panel (a) shows that splitting is associated with increases ranging between 2 and 5 percentage points in school attendance, whereas Panel (b) points to increases of up to 5 percentage points in literacy rates for individuals below 16 years.[20] Using RAIS data, Appendix Figure 3.D.3 further shows a crowd-out of employment from non-profit to government organizations in the educational sector, confirming that the higher levels of education come from increased public investments after splitting.

3.5.2 Beyond the Public Sector: The Effects on Economic Activity

Private Sector. We now turn to the economic impacts *beyond* the public sector. Using the near-universe of the private sector from the RAIS data, we estimate Equation (3.1), which directly compares the number of private establishments and jobs in the formal sector in treated and control municipalities, before and after splitting shocks. Panels (a) and (c) of

[20]These effects are not bounded by top censoring of school attendance or literacy rates. To put the numbers into perspective, we plot the average school attendance and literacy rates for different years and ages in Appendix Figure 3.D.2. The rates are far from 100 percent in 1991: school attendance peaks at 82 percent (at the age of 11), whereas the maximum of literacy rate is 87 percent (at the age of 15).

Figure 3.6 and Appendix Table 3.D.4 illustrate the dynamic and aggregate impacts around splitting. The point estimates are positive, but we cannot reject null effects, implying that the private sector does not expand in the same degree as the public sector. The aggregate results, however, mask substantial heterogeneity across economic sectors. Panels (b) and (d) of Figure 3.6 indicate some degree of structural transformation towards nontradable sectors since the majority of new establishments come from the retail and, to a lesser extent, services sectors.

Nighttime Luminosity. Thus far, the empirical results point to positive and persistent economic impacts of splitting. To quantify the impacts on economic activity, which captures the public, private, and informal sectors, we estimate Equation (3.1) with spatial data from satellite-recorded nighttime lights (Chen and Nordhaus, 2011; Henderson et al., 2012; Pinkovskiy and Sala-i Martin, 2016; Henderson et al., 2018). Similar to the previous findings, Panel (a) of Figure 3.7 shows that nighttime luminosity quickly grows in the first five years after splitting. Over time, the growth becomes stable and persistent, with an increase of around 12 percent.

The aggregate results at the municipality level, however, are limited in illustrating the distributional implications of redistricting *within* municipalities. For instance, the gains from redistricting may be uniformly distributed across districts. Or, alternatively, the gains may be asymmetric and exclusively driven by successful applicant districts, whereas other districts remain unaffected. Understanding the overall distribution of economic activity is key to shedding light on the winners and, if any, losers of the policy.

Leveraging the granular structure of the nighttime luminosity data, we estimate the following difference-in-differences specification at the district level:

$$y_{dmst} = \alpha_d + \alpha_{s(d)t} + \sum_{\tau=-\underline{\tau}}^{\overline{\tau}} \beta_\tau Split_{m(d)} \mathbf{1}[t - W_d = \tau] + \gamma Post_{dt} + \varepsilon_{dmst}, \tag{3.3}$$

in which subscripts d, m, s, and t stand for district, municipality, state, and year; and α_d represents district fixed effects. The remaining variables are similar to Equation (3.1) with the exception that, rather than municipalities, the subscripts represent districts. As before, standard errors are two-way clustered both at the state and split wave levels.

Panel (b) of Figure 3.7 plots the dynamics of nighttime luminosity around splitting separately for the applicant, headquarters, and remaining districts, whereas Appendix Table 3.D.5 reports the aggregate estimates.[21] We highlight three main patterns. First, the pre-event coefficients are statistically close to zero, lending credibility to the research design. Second, applicant districts strikingly experience a sharp growth in luminosity right after splitting. The growth peaks at about 40 log points between 5 and 8 years later, and becomes stable, with a 34 log points (or 39 percent) increase 15 years after splitting. Third, the estimates for the remaining and headquarters districts, who are involved in involuntary splits, are statistically insignificant. Exploiting additional margins of luminosity, Panels (c) and (d) of Figure 3.7 indicate a 4 percent increase in lit pixels, which capture the extensive margin of luminosity, and a 3 percent decline in luminosity Gini index for applicant districts. The latter is consistent with a decline in spatial inequality. We find no significant changes in the intensive margin for the remaining districts. If anything, the headquarters districts experience *positive*, albeit much smaller, impacts along this margin. These results indicate no shifts in economic activity from headquarters and remaining districts to successful applicant districts and, therefore, reject that the policy is a "zero sum game" among districts. Instead, they point to aggregate welfare gains.

3.5.3 Robustness Checks

We conduct some additional checks to ensure that our findings are robust to alternative definitions of outcomes, samples, and specifications. Table 3.4 reports the robustness checks.

[21] We add 0.1 to the average luminosity so that its log is defined for all districts.

For brevity, we limit our attention to district-level results from Equation (3.3) for applicant districts. Column (1) of Panel A replicates our benchmark result. In Column (2), we do not add 0.1 to the average luminosity so that its log is not defined for all districts. As an alternative approach to handle zeroes in the data, Column (3) applies inverse hyperbolic sine transformation to the average luminosity. Column (4) presents coefficients only for the 1997 wave to test whether the results are different across waves of splits. Because the process to split is usually lengthy, sometimes taking years, the timing of the 1996 CA is likely to be exogenous to our outcomes of interest for the 1997 wave, whose sample mostly consists of requests initiated between 1994 and 1996. The point estimate is remarkably similar to Column (1). Column (5) controls for trends specific to local economies by adding micro region-by-year fixed effects.[22] Column (6) alternatively controls for baseline characteristics from Table 3.1 interacted with year fixed effects, permitting differential trends across municipalities with different initial characteristics. Panel B further shows that our results are robust to different choices of clustering the standard errors.

Two additional issues could threat our main identification strategy. First, one could be concerned that the splitting treatment may be correlated with other concurrent shocks, confounding the estimated effects. For instance, splitting may result in new government programs, and thereby affecting the outcomes of interest. We are not aware of such shocks in Brazil. We also note that it is unlikely that the exact timing of differential shocks happening in split and almost split municipalities coincides with the timing of the splitting.

The second concern is that selection into splitting along unobservable factors, such as economic growth potential, better organizational capacity, or connections with the state legislative, could bias our estimates. We propose a complementary research design to mitigate this concern. Prior to 1996, districts applying to split had to conduct local referenda and

[22]Micro regions delineate local economies with similar socioeconomic and historical characteristics and are equivalent to commuting zones in the US. In addition, results for remaining and headquarters districts as well as other outcomes are available upon request.

obtain approval by simple majority. We leverage this rule in a difference-in-discontinuities design applied to a large and representative state, Minas Gerais, where referendum results are available, to compare districts that barely obtained the majority of necessary votes to split to those that did not.[23] Appendix E describes the research design in detail and confirms that both the difference-in-differences and difference-in-discontinuities strategies lead to qualitatively similar conclusions, strengthening the validity of our main research design.

3.6 Drivers of Local Development

Our results point that redistricting boosts local development by fueling bureaucracy, public services, and economic activity. We also find evidence that these gains are driven by successful applicant districts. Using observational data and key predictions from a simple model of public goods provision under redistricting outlined in Appendix Section A, this section assesses to what extent our results can be attributed to larger fiscal revenues or to decentralization of decision-making power to new local governments.

3.6.1 The Role of Fiscal Revenues

We scrutinize the sources of financing new local governments by investigating the impacts of redistricting on fiscal revenues, such as federal transfers and tax revenues. Using data on fiscal revenues, we estimate Equation (3.1), which directly compares split and almost split municipalities, before and after splitting. Figure 3.8 shows the dynamics of local revenues around splitting events. The pre-event coefficients are statistically equal to zero, lending support to our empirical strategy. Immediately after splitting, there is a sharp increase in revenues, a pattern that becomes stable over time.

[23] With an area larger than France, Minas Gerais is the second most populous and third richest state in Brazil. The ethnic composition and geography is similar to the rest of the country.

Appendix Table 3.D.6 displays the aggregate impacts. Column (1) indicates an increase of 14 percent in local revenues after splitting. Consistent with the institutional context, Column (2) points that this result is predominantly driven by the increase in federal transfers per capita due to the funding allocation mechanism. Concerning the impacts on tax revenues per capita, Column (3) shows a growth of 11 percent, though we cannot reject a null effect. Along with the lack of increased inflow of population following splitting, this finding indicates a limited role of local taxation as an asset to fiscal capacity in weak states (Balan et al., 2022), reinforcing the importance of non-taxes revenues in building state capacity for peripheral regions.

The fact that splitting leads to increases in federal transfers to newly created municipalities implies an unintentional large subsidy to fund their operations, like infrastructure and bureaucracy. An important question is to what extent our previous findings on public services and economic activity are driven by the increased fiscal revenues. We propose two exercises. First, we implement a "horse-race" approach, in which we add total expenses to the set of controls from Equation (3.1).[24] By holding expenses fixed when comparing split and almost split municipalities, we test whether the coefficient associated with splitting approaches zero in case the increased transfers explain the positive impacts on economic outcomes. Odd columns of Table 3.5 replicate selected baseline results, whereas even columns report the coefficients after controlling for total expenses. The small changes in the point estimates and in the R-squared values indicate a limited role of increased expenses on explaining our main findings.

The second approach overcomes the lack of information on fiscal revenues at the district level by assuming that, prior to redistricting, municipal expenditures and revenues are proportionally shared among districts based on population. Comparing Columns (1) and (2) of

[24]We control for total expenses, rather than total federal transfers, because there is little variation in transfers among municipalities that almost split, as shown in Appendix Figure 3.D.1d, and expenses strongly correlate with revenues.

Table 3.3, the inclusion of predicted expenditures, rather than actual expenditures, at the district level in the set of controls barely alters the effects on luminosity: the point estimate declines from 0.34 to 0.30. These results together suggest that the increased fiscal revenues do not explain the bulk of the gains in public services and economic activity. Because the applicant districts gain administrative, fiscal, and political autonomy once they secede and become municipalities, we next discuss the role of decentralization of decision-making power in justifying our results.

3.6.2 The Role of Decentralization of Decision-Making Power

In line with the new municipalities obtaining *de jure* decision-making power, Section 3.5 highlights that the *de facto* gains in public services are concentrated in activities for which local governments are expected to provide oversight, like trash collection and primary education. In addition, we find no evidence that such gains of splitting extend to activities also under the influence of federal and state governments, such as electricity, sanitation, and high school education. Although the main contribution of the article is the reduced-form estimates of splitting on local development, we also test several theories of decentralization positing its implications for economic development, with the limitation that this exercise only allows a suggestive glimpse into theories due to data constraints.

The Mechanism of Curtailing Capture and Neglect. A key source of inequality across space is that the decision-making process regarding the allocation of resources and burdens usually reflects the preferences of a few elite groups and the lack of policy priorities from local authorities, ultimately promoting capture and neglect. This is supported by a survey with Brazilian mayors in 1992 confirming that neglect by parent local governments and geographical distance to the headquarters are the most common motivations for splitting

(Bremaeker, 1993).[25] One of the premises of decentralization is to curtail the influence of capture and neglect in peripheral regions, advancing policies better aligned with local needs (Oates, 1972; Bardhan, 2002; Mookherjee, 2015).

One challenge to investigating whether this happens is that capture and neglect are difficult to measure. We propose an indirect test that examines whether the gains in economic activity are stronger in areas with higher propensity to capture and neglect prior to splitting. Because they tend to be greater in areas that "are remote from centers of power; have low literacy; are poor; or have significant caste, race, or gender disparities" (Mansuri and Rao (2012), p.5), we examine heterogeneity in luminosity across different dimensions. In line with decentralization benefiting vulnerable areas, Columns (3) and (4) of Table 3.3 reveal that the gains in luminosity accrue to peripheral areas previously located farther from their parent town halls and with lower urbanization rates in the baseline period. These findings suggest that this policy can serve the dual purpose of expanding the public sector in peripheral regions, and freeing these regions from capture and neglect of former governments.

The Mechanism of Politics. One argument against decentralization is the lack of policy coordination across jurisdictions, which can be detrimental when externalities fail to be internalized (Lipscomb and Mobarak, 2017). However, decentralization can also be beneficial in terms of increased political accountability, since local governments have incentives to tailor policies aligned with the local needs, increasing social welfare and influencing electoral outcomes (Seabright, 1996). Because elections are imperfect instruments of political accountability, we can assess the role of politics in explaining our findings (Bordignon and Minelli, 2001).

[25]Other works have shown that administrative remoteness in form of geographical distance to the headquarters limits the provision of public services and local development more broadly (Krishna and Schober, 2014; Asher et al., 2018). For instance, it may reduce the amount and the quality of information about local needs available to the headquarters, leading to fewer public investments (Oates, 1999). High transportation costs and information frictions may also restrict the flow of services as bureaucrats may travel less to remote areas and be less prone to observing citizens' preferences.

Leveraging limited information from the electoral data during the 1990s, we scrutinize the electoral results across applicants and headquarters districts. Figure 3.9 shows that, in most times, the applicants and headquarters districts elect mayors from *different* parties following redistricting. Immediately after splitting, we observe this divergence for about 75 percent of results, and this pattern grows to nearly 85 percent two decades later. This finding speaks to the literature studying the politics of decentralization (Grossman et al., 2017; Pierskalla, 2016), especially to basic models of representative politics, in which elected officials reflect local preferences for policies (Persson and Tabellini, 2002).[26,27]

3.7 The Net Effects

Thus far, our results indicate that subsidized voluntary splits bring positive economic impacts for new municipalities. At the same time, we do find evidence that other areas who involuntarily split are also affected along these margins. An important question for the distributional consequences of this policy is whether the municipalities that did not split ultimately experience negative impacts due to losses in fiscal revenues resulting from the allocation mechanism of the federal transfers.

To account for spillovers to the rest of the country, our first exercise exploits variation in the number of municipal splits within states. In principle, states with more splits are more likely to experience larger changes in federal transfers. This fact motivates the correlation test between changes in federal transfers and selected outcomes, including public and private

[26]For instance, Myerson (2006) illustrates that decentralization may increase yardstick competition between jurisdictions, raising the quality of politicians and improving the chances of selecting capable administrators. Boffa et al. (2016) argue that centralization has the advantage of combining regions with diverse informed voters, which increases the average level of information and limits rent-seeking.

[27]Unlike our context, in which districts can unilaterally request to split, Hassan (2016) and Gottlieb et al. (2019) model splitting as an endogenous distributive policy chosen by the incumbent politician. Because local elections are single-district, incumbent politicians may benefit from splits only to the extent that voters within the applicant district are in the opposition. We are unable to directly test these theories due to the lack of historical electoral data with information on vote shares and on the level of electoral competition.

jobs, number of establishments, and average luminosity.[28] Table 3.6 and Appendix Figure 3.D.4 indicate no evidence that lower revenues are associated with worse measures of local development. The only exception is a decline, although small in magnitude, in the number of establishments.

What can explain the lack of spillover effects onto municipalities that did not split? We argue that the findings are very consistent with a model with decreasing returns to spending, in which local governments engage in wasteful expenses whose marginal value is below social cost of funds (Liebman and Mahoney, 2017). Even when the redistricting process implies less funds available to municipalities that did non split, they may hold back low-value spending and manage these funds more efficiently. Because the social cost of these lost funds exceed their social value, the spending cuts may not be large enough to worsen economic outcomes.

Our second exercise is a back-of-the-envelope calculation asking what the return-per-dollar of transfers should to be for municipalities that did not split to generate a positive social value of redistricting and the subsequent reallocation of federal transfers. The change in social value can be written as:

$$\begin{aligned}\Delta V &= \Delta V_R + \Delta V_N + \Delta V_{NS} \\ &= \Delta T_R \times R_R \times \alpha_R + \Delta T \times (R_N \times \alpha_N - R_{NS} \times \alpha_{NS}),\end{aligned}$$

in which subscripts R, N, and NS stand for the groups of remaining and headquarters districts that split, new municipalities, and non-split municipalities, respectively; α_i represents population shares for each group $i = R, N, NS$; ΔT denotes the change in percentage points in the reallocation of federal transfers by the time of splitting; ΔT_R captures the

[28] Appendix Figure 3.D.1 shows that, on average, a one-percentage-point increase in population residing in new municipalities implies that non-split municipalities experience a 2.1 percentage points decrease in federal transfers.

change in federal transfers for remaining and headquarters districts; and R_i indicates the total returns-per-dollar of federal transfers for each group i.

The reallocation of federal transfers is still beneficial if $\Delta V > 0$. To uncover the maximum value of returns for non-split municipalities, R_{NS}, needed to justify the redistricting process and the resulting reallocation of federal transfers, we map the returns-per-dollar in remaining and headquarters districts, R_R, and in new municipalities, R_N, into the district-level effects on luminosity from Section 3.5.2. We obtain the population shares, α_i, and the change in transfers, ΔT, from the data. We back out a "break-even" value R_2 lower than or equal to 6.2 percent that still leads to positive aggregate benefits, $\Delta V > 0$.[29] An alternative approach using the effects on school attendance and literacy rates points to a maximum value of returns, R_{NS}, of 13 percent.

We then compare the "break-even" values to the *actual* values of R_{NS} in Brazil from the literature. For instance, Corbi et al. (2019) find that a one-percentage-point increase in federal transfers generates around 0.18 and 0.21 percent increases in the total public and private employment. In a similar empirical approach, Litschig and Morrison (2013) document returns of 0.35 percent for school attendance and 0.2 percentage points for literacy rates. Benchmarking the returns from the literature to the "break-even" values, our findings point to positive impacts of the redistricting episode on the aggregate social value.

3.8 Conclusion

This paper provides a comprehensive evidence of the short- and long-run impacts of one of the largest voluntary redistricting episodes worldwide (Grossman and Lewis, 2014). Leveraging rich administrative and spatial data, along with reforms generating sharp variations in the number of municipalities in Brazil, we find that redistricting through subsidized

[29]We use the following numbers: $R_R \approx 0$, $\alpha_R \approx 22.2$ percent, $\Delta T \approx 5.5$ percentage points, $R_N \approx 34/32 = 106.25$ percent, $\alpha_N = 4.3$ percent, and $\alpha_{NS} = 73.4$ percent.

splitting generates positive impacts on the size of bureaucracy, public services delivery, and economic activity for new municipalities, without worsening economic outcomes for the rest of the country. These impacts are driven by applicant districts who voluntarily secede into new municipalities, and are largest for remote districts that tend to be neglected by their former local headquarters. Our findings indicate that splitting promotes decentralization and enables peripheral regions to develop.

This paper offers new policy-relevant insights for countries debating on how much autonomy and resources to devote to peripheral, poor and remote areas as part of their decentralization reforms. The gains in fiscal revenues for new municipalities allow us to shut down one often-hypothesized pitfall of redistricting: their limited fiscal capacity to self-finance. We also do not find evidence that the resulting losses in scale and revenues impose a visible burden to the rest of the country, likely due to the decreasing returns to spending. Therefore, our context of subsidized and voluntary splits shines a positive light on decentralization reforms, often classified as "cautionary tales" (Kremer et al., 2003).

One limitation of this paper is that we are not able to estimate the costs of redistricting due to the lack of additional data. For instance, the literature has documented that revenue windfalls undermine government monitoring, exacerbate political corruption, and deteriorate the quality of politicians (Brollo et al., 2013; Boffa et al., 2016). Understanding whether this happens in the context of redistricting would shed light on its pitfalls. Quantifying the economic costs of redistricting would also advance our understanding of its equity-efficiency trade-off embedded in a general equilibrium framework for a welfare analysis. Lastly, fleshing out exactly how governments are formed in new municipalities, what specific promises and investments they make, and how splitting affects political yardstick competition and representation is a next step worthy of its own paper. We view these examples as promising directions for future research.

Tables and Figures

Table 3.1: Baseline Descriptive Statistics at the Municipality Level

	Contains Applicant		Rest		*Differences*	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Diff. (5)	p-value (6)
Number of Districts	3	1.8	1.6	1	1.4	<0.01
Population (000's)	40.5	71.8	21.7	75.3	18.8	<0.01
Area (000's km2)	2.5	10.3	.9	2.7	1.6	<0.01
% Urban Population	58.5	23.8	59.2	22.8	-.7	0.54
% Population 14-	22.9	3.1	22.2	2.9	.7	<0.01
% Population 15-24	19.4	1.4	19.3	1.4	.1	0.34
% Population 25-34	15.8	1.9	15.9	1.8	-.1	0.55
% Population 65+	4.9	1.4	5.4	1.5	-.5	<0.01
Years of Education	8.8	1.4	8.8	1.4	.1	0.41
% Literacy 11-14	91.6	8.9	92.3	8	-.7	0.12
% Literacy 25+	74.7	12.9	74.2	10.6	.5	0.43
Preschool Attnd.	13.1	9.7	17.4	14	-4.3	<0.01
Middleschool Attnd.	88.1	10.7	89.7	11.5	-1.7	<0.01
High School Attnd.	28.1	14.4	28.3	14.1	-.1	0.87
Life Expectancy	66.8	2.7	66.8	2.6	0	0.83
Child Mortality 1-	32.3	9.7	32.3	9	0	0.92
Child Mortality 5-	38.6	12.8	39	11.8	-.4	0.55
% Piped Water	71.2	24.2	74.9	21.8	-3.7	<0.01
% Trash Collection	63.5	27.3	67.3	29.4	-3.8	0.01
% Electricity	81.3	20	83.8	18.9	-2.5	0.01
% Sewage	96.1	7.7	96.8	8	-.7	0.12
HHI Race	64.3	13.9	62.2	14.9	2	<0.01
HHI Religion	75.8	12.2	79.3	12	-3.5	<0.01
Log Distance to State Capital	5.4	.8	5.3	.8	.1	0.09
Log Income p.c.	5.7	.5	5.6	.4	0	0.23
% Extreme Poverty	19.6	14.9	17.6	13.6	2.1	<0.01
% Poverty	42.8	20.6	42.3	19.2	.6	0.59
% Federal Transfers	37.2	17	43.6	18.5	-6.4	<0.01
	N = 448		N = 1925			

Note: This table reports descriptive statistics in 1991 at the municipality level. We use information from the 1991 Demographic Census and the Brazilian National Treasury data. See Section 3.3 for further details on data and sample construction of the municipality-level data.

Table 3.2: Baseline Descriptive Statistics at the District Level

	In Sample						Rest				*Differences*			
	Applicant		Remaining		Headquarters		Periphery		Headquarters		(1)-(3)		(1)-(5)	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Diff.	p-value	Diff.	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Population (000's)	5.4	12.4	3.2	5.5	31.7	64	3.6	13.1	17.9	48.9	2.6	<0.01	-25.8	<0.01
% Urban Population	.4	.3	.3	.2	.7	.2	.3	.2	.6	.2	.1	<0.01	-.3	<0.01
% Male	.5	0	.5	0	.5	0	.5	0	.5	0	0	<0.01	0	<0.01
% Literacy	.7	.1	.7	.1	.7	.1	.6	.1	.7	.1	0	.3	0	<0.01
% Piped Water	.5	.3	.5	.3	.5	.4	.5	.3	.5	.4	0	.53	-.1	<0.01
% Sanitation	.6	.4	.7	.3	.6	.4	.6	.3	.6	.4	0	.05	0	.45
% Trash Removal	.1	.2	.1	.2	.3	.3	.1	.2	.3	.3	0	.03	-.2	<0.01
Avg. Luminosity	1.8	5.8	1.4	5.2	3.1	6.5	1.9	8.1	2.5	7.3	.5	.19	-1.2	<0.01
Area (000's km2)	.5	1.5	.3	.5	.9	2.5	.3	.9	.6	1.5	.3	<0.01	-.4	<0.01
Log Distance to Parent Town Hall	3	.6	2.8	.6	1.5	1	2.7	.6	1.4	.9	.1	<0.01	1.4	<0.01
Log Distance to State Capital	5.5	.8	5.4	.7	5.4	.8	5.2	.9	5.3	.8	.1	.09	0	.35
Log Maize Suitability	8.7	.3	8.7	.3	8.6	.3	8.5	.3	8.5	.2	0	.98	0	.06
Log Wet Rice Suitability	8.6	.8	8.6	.5	8.7	.5	8.6	.9	8.6	.8	0	.58	0	.42
Log Soybean Suitability	7.7	.4	7.7	.2	7.7	.2	7.6	.8	7.7	.7	0	.47	0	.67
Log Wheat Suitability	6.5	2.9	6.8	2.7	6.6	2.8	6.5	3	6.5	2.9	-.2	.35	0	.85
Terrain Ruggedness	83.2	78.2	72.8	68.5	76.2	72.7	68.6	71.7	68.7	71.4	9.7	.06	6.9	.17
	N = 552		N = 325		N = 382		N = 916		N = 1772					

Notes: This table reports baseline descriptive statistics at the district level.from the 1991 Demographic Census, and the 1992 night lights, MapBiomas, FAO-GAEZ soil suitability, and terrain ruggedness data. See Section 3.3 for further details on data and sample construction of the district-level data.

Table 3.3: Heterogeneous Effects of Redistricting on Economic Activity

	Log Luminosity			
	(1)	(2)	(3)	(4)
Post x Split	0.34***	0.30***	1.71***	0.89*
	(0.03)	(0.04)	(0.27)	(0.46)
Log Expenditures		0.07**	0.07**	0.07**
		(0.04)	(0.03)	(0.03)
Post x Split x Log Population in 1991			-0.04	0.01
			(0.04)	(0.04)
Post x Split x Log Area			-0.14***	-0.28**
			(0.04)	(0.08)
Post x Split x Urbanization Rate in 1991			-0.53**	-0.51**
			(0.20)	(0.19)
Post x Split x Log Distance to Parent Town Hall				0.26*
				(0.15)
Post x Split x Log Distance to State Capital				0.06
				(0.07)
Observations	9,821	9,821	9,821	9,821
R-squared	0.96	0.96	0.96	0.96
State-Year FE	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean Pre-Split	-0.77	-0.77	-0.77	-0.77
SD Pre-Split	1.52	1.52	1.52	1.52

Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports heterogeneous effects of redistricting on log luminosity. Column (1) considers the benchmark specification in Equation (3.3), whereas Column (2) adds log expenditures to the set of controls. In Column (3), we include interaction terms with log population in 1991, log total area, and urbanization rate in 1991. Column (4) further adds interaction terms with log distance to the parent town hall and log distance to the state capital. Standard errors are two-way clustered both at the state and split wave levels.

Table 3.4: Robustness Checks: Applicant Districts

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Specifications						
Post x Split	0.34***	0.48***	0.10***	0.33***	0.25***	0.25***
	(0.03)	(0.05)	(0.03)	(0.04)	(0.07)	(0.02)
Observations	10,122	9,616	10,122	4,920	10,122	10,122
R-squared	0.96	0.95	0.97	0.97	0.98	0.97
Choice	Benchmark	Log Lumin.	(IHS) Lumin.	1997 Wave	Micro region FE	Controls
Panel B: Standard errors						
Post x Split	0.34***	0.34***	0.34***	0.34***		
	(0.03)	(0.05)	(0.05)	(0.03)		
Observations	10,122	10,122	10,122	10,122		
R-squared	0.96	0.96	0.96	0.96		
Std Error Clustering	State-Split Wave	Municipality	Micro region	State		
Number of Clusters	20	422	194	11		

Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports several robustness checks. Panel A shows that the results are robust to different choices of specifications, dependent variables, and samples. Column (1) repeats the benchmark specification from Equation (3.3). Column (2) does not add 0.1 to the average luminosity, whereas Column (3) applies inverse hyperbolic sine transformation to the average luminosity. Column (4) further restricts the sample to districts involved in the 1997 wave. Column (5) adds micro region-by-year fixed effects to the set of controls. Column (6) controls for baseline characteristics from Table 3.1 interacted with year fixed effects. Panel B shows that the results are robust to choices of clustering the standard errors. Column (1) refers to the standard choice of two-way clustering both at the state and split wave levels. Columns (2), (3), and (4) consider clustering at the municipality, micro region, and state levels, respectively.

Table 3.5: The Role of Fiscal Revenues

	Log Municipal Jobs		% Trash Collection		% Sewage		Log Luminosity		Log Private Establishments		Log Private Jobs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Split	0.24***	0.21***	4.50*	3.39	1.11***	0.67	0.12**	0.11**	0.06	0.06	0.07	0.06
	(0.04)	(0.04)	(2.33)	(2.09)	(0.39)	(0.41)	(0.04)	(0.04)	(0.07)	(0.07)		(0.06)
Log Expenditures		0.20***		5.76**		2.27**		0.07***		0.04		0.04
		(0.02)		(2.25)		(0.96)		(0.01)		(0.03)		(0.04)
Observations	7,179	7,179	1,324	1,324	1,324	1,324	8,106	8,106	7,229	7,229	7,229	7,229
R-squared	0.85	0.86	0.87	0.87	0.89	0.89	0.98	0.98	0.99	0.99	0.97	0.97
State-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean Pre-Split	5.980	5.980	63.73	63.73	96.03	96.03	-0.050	-0.050	5.390	5.390	7.160	7.160
SD Pre-Split	1.350	1.350	27.35	27.35	7.780	7.780	1.590	1.590	1.490	1.490	2.020	2.020

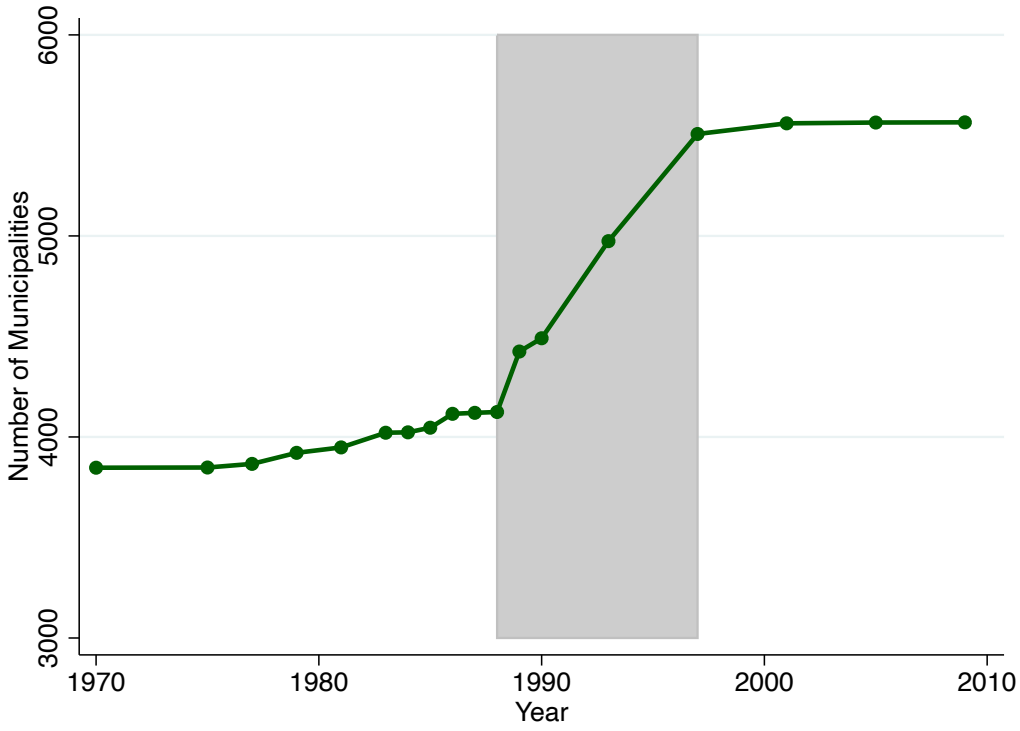
Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports the aggregate estimates of redistricting on selected outcomes representing public sector, economic activity, and private sector. Odd columns replicate baseline results (further details can be found in Appendix Tables 3.D.2 to 3.D.4), whereas even columns control for log total expenditures. Standard errors are two-way clustered both at the state and split wave levels.

Table 3.6: Spillover Effects of Changes in Federal Transfers on the Rest of the Country

	Public Jobs	Private Jobs	Establishments	Luminosity
	(1)	(2)	(3)	(4)
Federal Transfers	-5.86	-9.73	-4.00**	1.23
	(5.83)	(6.08)	(1.66)	(1.00)
Observations	25	25	25	25
R-squared	0.38	0.48	0.71	0.54
Region FE	✓	✓	✓	✓
Split Wave	1997	1997	1997	1997
Mean	226.7	366.4	189.8	117.4
SD	403.9	442.5	133.7	59.62

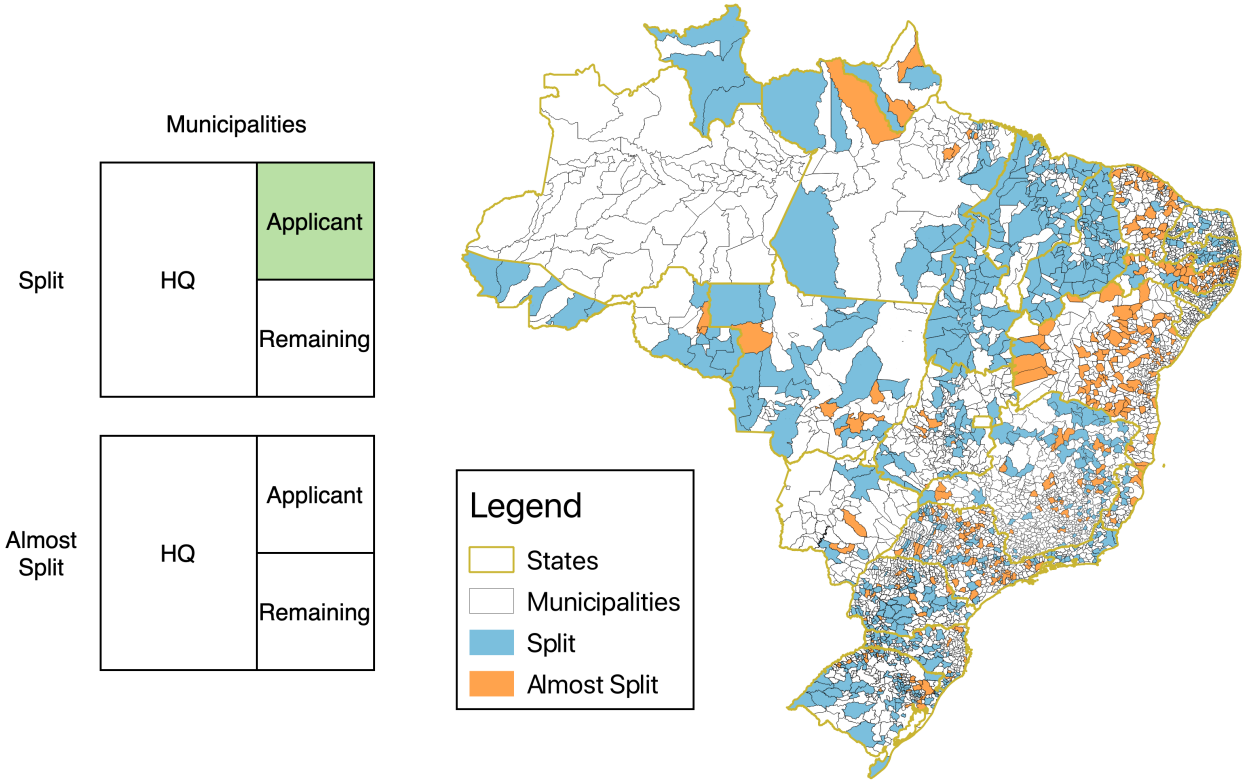
Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports correlations between changes in federal transfers (in percentage points) and selected outcomes (in percentage points) for municipalities that did not split. To capture changes in federal transfers, we exploit variation in the number of splits within states after residualizing for region dummies. Outcomes of interest are percentage changes in total number of public jobs, number of private jobs, number of establishments and average luminosity 15 years after splitting. We exclude Distrito Federal and Roraima from the final sample.

Figure 3.1: Evolution of Total Number of Municipalities



Note: This figure shows the evolution of the total number of municipalities in Brazil between 1970 and 2010. New municipalities are established in the beginning of election terms after obtaining approval to split. The grey area highlights our period of study: the period between the enactments of the 1988 Federal Constitution and the 1996 Constitutional Amendment. Information on splits are obtained from the Brazilian Institute of Geography and Statistics (IBGE).

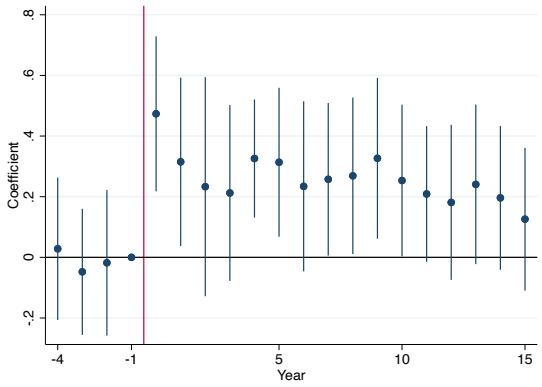
Figure 3.2: Diagram Illustrating Splits and Map of Brazil



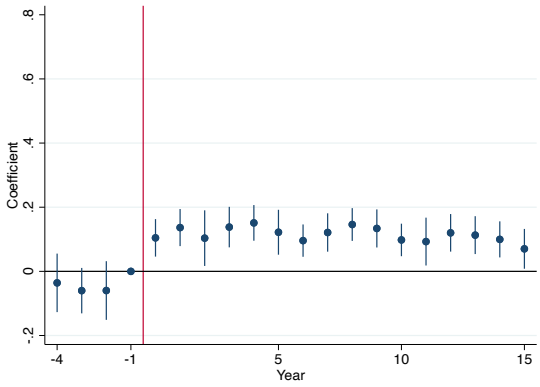
Note: On the left, the diagram illustrates the structure of split requests from our sample. Municipalities are divided into applicant, remaining and headquarters districts. The green color highlights applicant districts that succeed at splitting. On the right, the map represents Brazil in 1991. Municipalities that split are colored blue, while municipalities that almost split are colored orange. More details can be found in Section 3.3.

Figure 3.3: Effects of Redistricting on Bureaucracy in the Public Sector

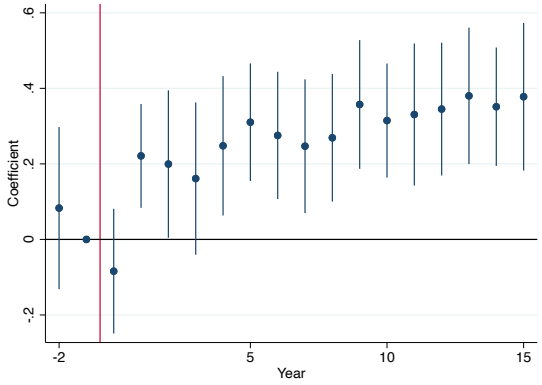
(a) Log Capital Expenditures p.c.



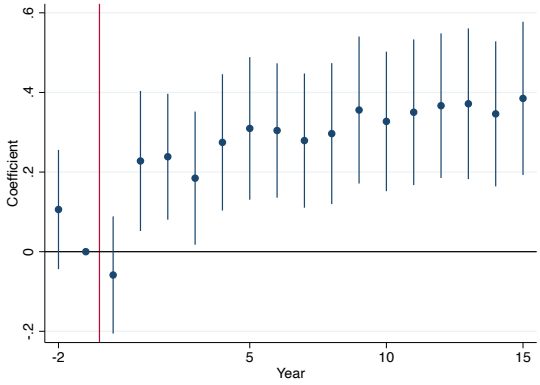
(b) Log Current Expenditures p.c.



(c) Log Municipal Jobs

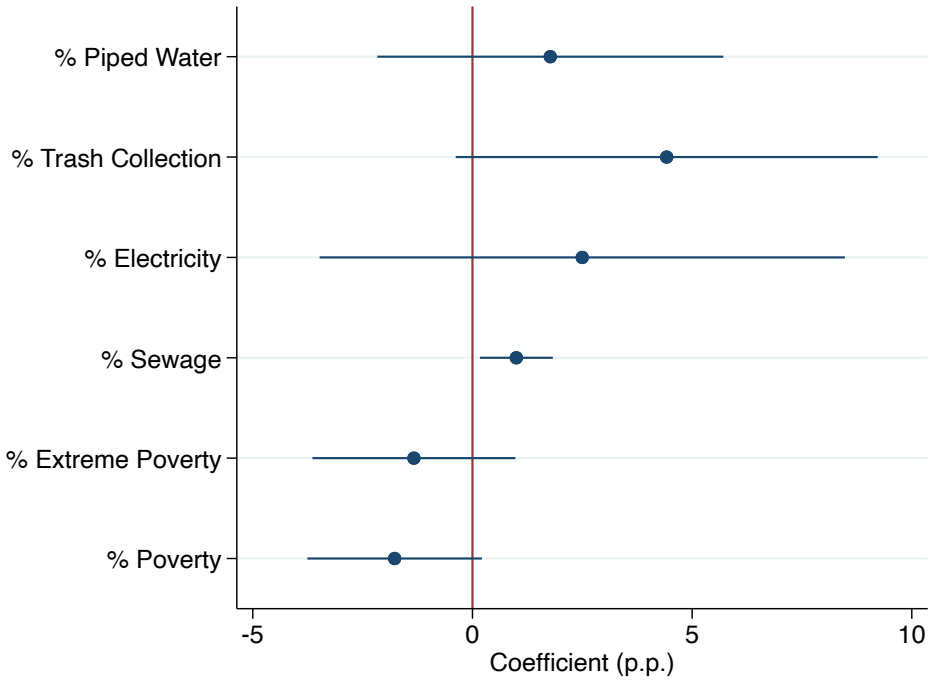


(d) Log Municipal Wages



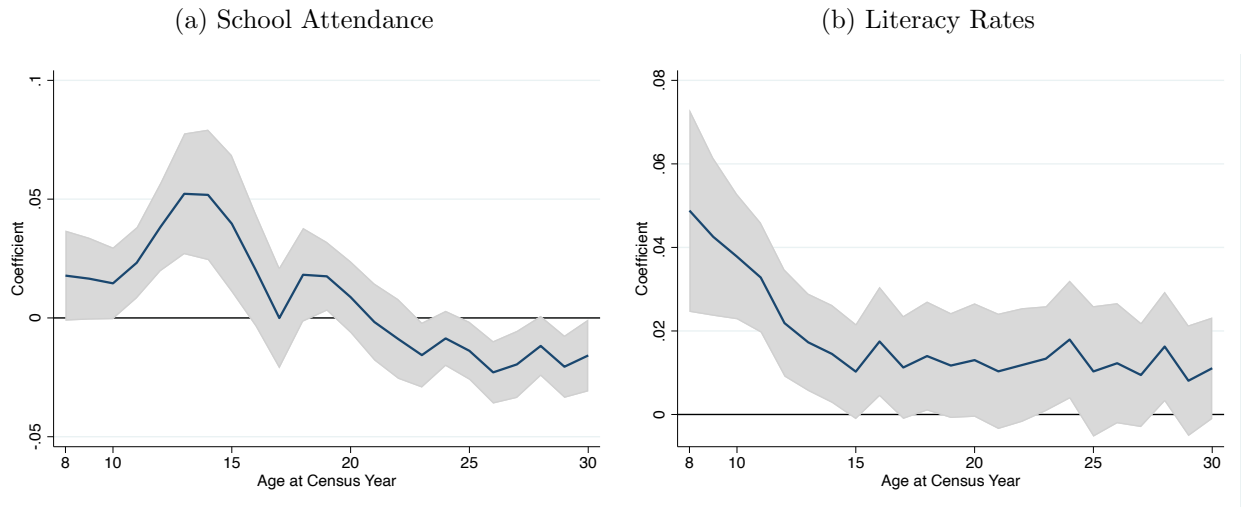
Note: *** p<0.01, ** p<0.05, * p<0.1. This figure reports the annual effects of redistricting on the setup of new local governments after estimating Equation (3.1). We consider the following dependent variables: log municipal capital expenditures per capita, log municipal current expenditures per capita, log total number of municipal jobs, and log municipal wages. The main data sources are the Brazilian National Treasury between 1989 and 2018 and the annual RAIS data between 1995 to 2018. We use information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO) to classify jobs and economic sectors. The omitted category is the year before splitting. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Table 3.D.2.

Figure 3.4: Effects of Redistricting on Public Services and Poverty



Note: This figure reports the aggregate effects of splitting on public services and poverty measures after estimating Equation (3.1). We consider the following dependent variables: household access to piped water, trash collection, electricity, sewage, extreme poverty and poverty rates. The main data sources are the decennial Demographic Census of 1991, 2000 and 2010. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Table 3.D.3.

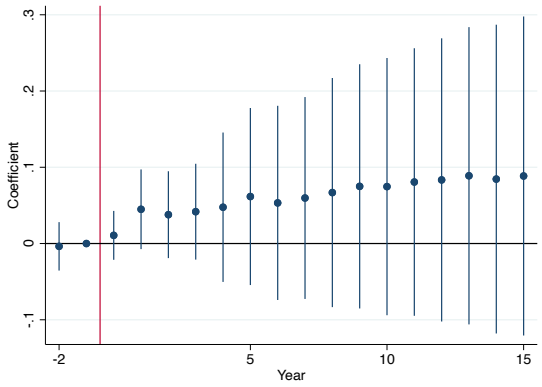
Figure 3.5: Heterogeneous Effects of Redistricting on Education Outcomes Across Age



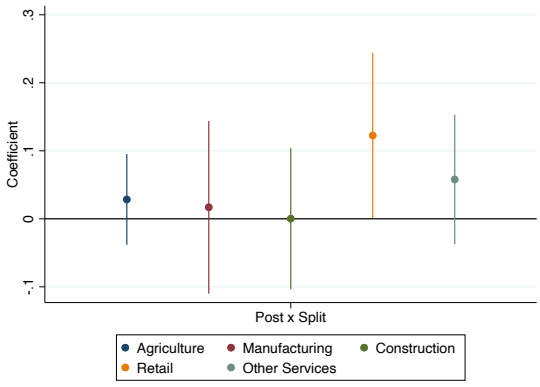
Note: This figure reports heterogeneous effects of redistricting on school attendance (Panel (a)) and literacy rates (Panel (b)) after estimating Equation (3.2). The main data sources are the decennial Demographic Census microdata from 1991, 2000 and 2010. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Figure 3.D.2.

Figure 3.6: Effects of Redistricting on the Private Sector

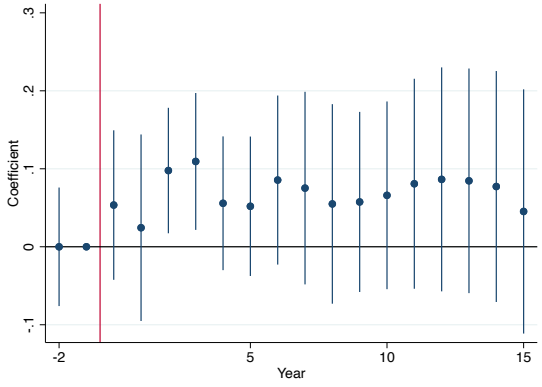
(a) Log Private Establishments



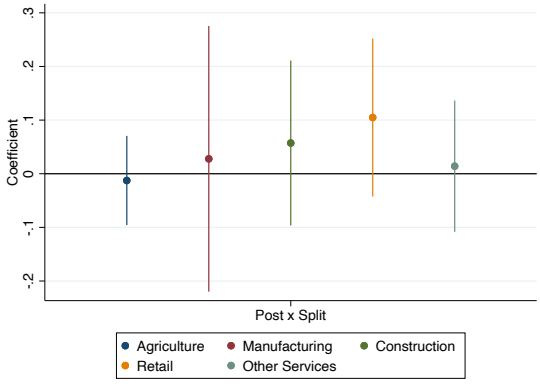
(b) Log Private Establishments



(c) Log Private Jobs



(d) Log Private Jobs

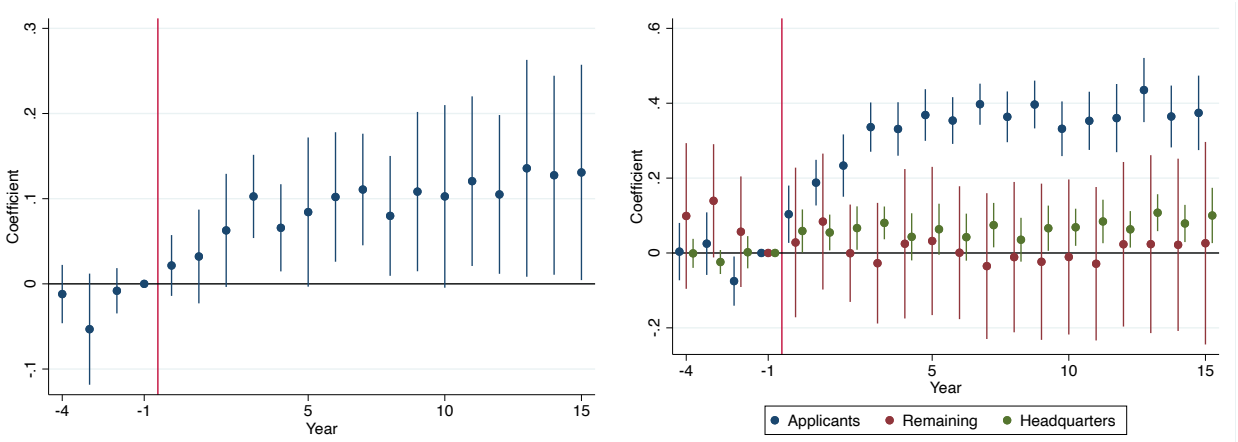


Note: Panels (a) and (c) of this figure report the annual affects of redistricting on log total number of establishments and log jobs in the private sector after estimating Equation (3.1). Panels (b) and (d) report the aggregate effects of redistricting for the same outcomes across economic sectors. The main data sources are the annual RAIS data between 1995 to 2018. We also use information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO) to classify jobs and economic sectors. The omitted category is the year before splitting. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Table 3.D.4.

Figure 3.7: Effects of Redistricting on Economic Activity

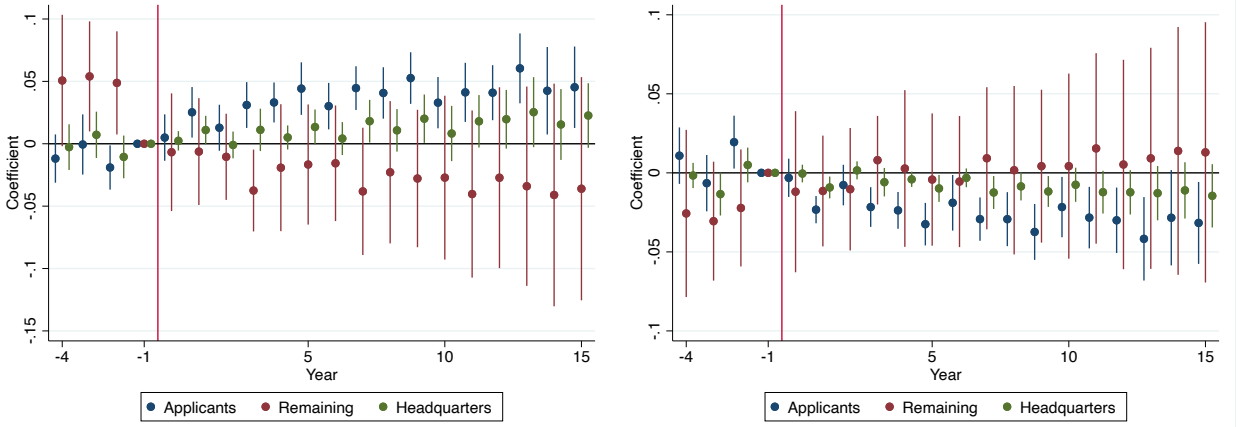
(a) Log Luminosity: Municipality Level

(b) Log Luminosity: District Level



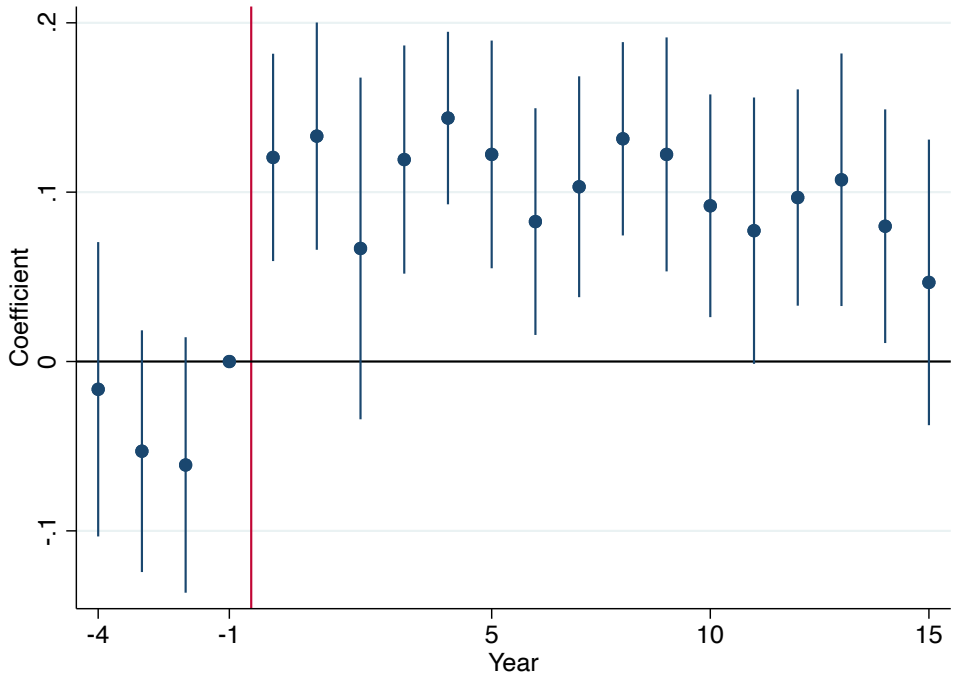
(c) % Pixels Lit: District Level

(d) Luminosity Gini: District Level



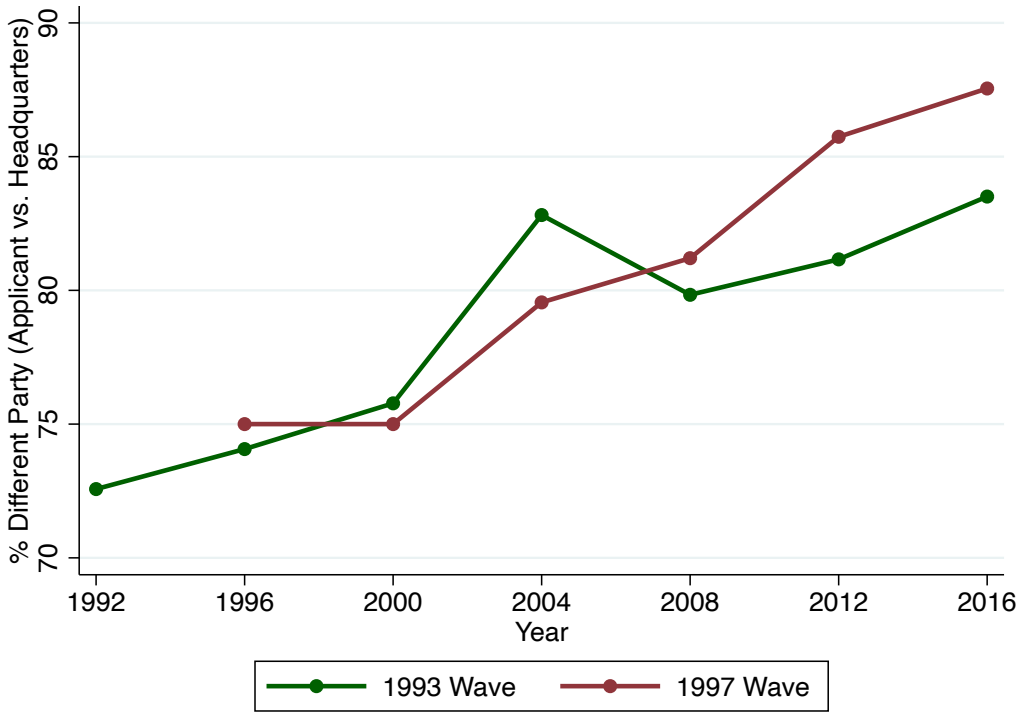
Note: This figure reports the annual effects of redistricting on economic activity. Panel (a) refers to point estimates after estimating Equation (3.1) with log luminosity as the dependent variable. Panels (b), (c) and (d) display point estimates after estimating Equation (3.3) separately for applicant, remaining, and headquarters districts, considering log average luminosity, indicator variable for whether the average is above zero, and luminosity Gini index as the dependent variables. The main data sources are the nighttime luminosity data between 1992 to 2013. The omitted category is the year before splitting. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Table 3.D.5.

Figure 3.8: Effects of Redistricting on the Local Revenues



Note: This figure reports the annual effects of redistricting on log local revenues per capita after estimating Equation (3.1). The main data sources are the Brazilian National Treasury data between 1989 to 2018. The omitted category is the year before splitting. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Table 3.D.6.

Figure 3.9: Divergence in Political Preferences After Redistricting



Note: This figure plots the percentage of municipalities in which the applicant and headquarters districts elected mayors from different parties after splitting. Because data on elections are only available at the municipality level, we only plot trends for municipalities that ultimately split.

Appendices

A Conceptual Framework

We sketch a simple framework to illustrate how redistricting through splitting affects the provision of public services. Our model incorporates several features from our context and highlights the scope for several mechanisms studied in the paper, including neglect from the headquarters and fiscal incentives (Bolton and Roland, 1997; Dur and Staal, 2008). Appendix Section B presents further details and proofs.

We work with a one-period model. We assume that a municipality, which we refer as municipality 1, is composed of two districts, A and B. The municipal population is immobile, and districts A and B have population α_A and α_B. There is no income heterogeneity within the district, implying that all residents have income per capita y. Two sources of municipal revenues finance public goods g: income taxes τ and federal transfers $T(\cdot)$. Consistent with the institutional context described in Section 3.2, $T(\cdot)$ depends on the population size. We also assume that $T(\cdot)$ is weakly increasing and concave, whereas federal transfers per capita are weakly decreasing and convex in population size. The utility takes a quasi-linear form, $U_i = \theta_i \ln(g_i) + (1-\tau)y_i$, in which θ_i captures local preferences for public goods in district i. We normalize the price of public goods to one.

District A contains the municipality headquarters and, for this reason, holds decision-making power, including regarding the allocation of public goods. When districts A and B form together a single municipality, district A chooses the levels of public goods in districts A and B, g_A^U and g_B^U, that maximizes a Pareto weighted sum of utilities subject to a budget constraint. In other words, district A solves the following maximization problem:

$$\max_{g_A, g_B, \tau} \quad (1-\lambda)\alpha_A U_A + \lambda \alpha_B U_B \quad \text{subject to} \quad g_A + g_B \leq \tau y + T(\alpha_A + \alpha_B), \tag{3.4}$$

in which $y \equiv \alpha_A y_A + \alpha_B y_B$, and λ is the intra-municipality Pareto weight capturing the relative welfare strength of the two districts in deciding over the amount of public goods.

In case of splitting, district B becomes a municipality and obtains decision-making power over its level of public goods, g_B^S. The maximization problem can be written as:

$$\max_{g_B,\tau} \quad \alpha_B U_B \quad \text{subject to} \quad g_B \leq \tau \alpha_B y_B + T(\alpha_B), \tag{3.5}$$

in which $T(\alpha_B)$ represents the amount of federal transfers the new municipality receives. The parent municipality, which now only consists of district A, chooses g_A and τ from an analogous maximization problem.

Comparing solutions of the maximization problems, we have:

Proposition 1. *The benefits of splitting for district B are larger if:*

1. ***(Capture and Neglect)*** *Its welfare was captured and neglected by the headquarters (lower λ);*

2. ***(Fiscal Incentives)*** *It is small in population size (lower α_B) and has:*

   (A1) a high comparative gain in transfers if split $\left(\frac{T(\alpha_A+\alpha_B)}{y} \leq \frac{T(\alpha_B)}{\alpha_B y_B}\right)$*; and*

   (A2) a high comparative tax base $\left(\frac{\theta_B}{\theta_A} \leq \frac{y_B}{y_A}\right)$.

Proof. See Appendix Section B. □

To understand the distributional effects of splitting, we extend our framework to introduce a second municipality with population α_2. This municipality represents the rest of the state. To capture the reallocation of federal transfers after a split, define T_i^U as the amount of transfers that area i receives when municipality 1 is integrated; and T_i^S as the amount of transfers that area i receives when municipality 1 splits.

Consistent with the Brazilian context, transfers are "zero sum game", always summing to a constant $\overline{T}$. We also assume that $T_A^S + T_B^S \geq T_{A+B}^U$ and $T_2^U \geq T_2^S$. We define the indirect

utility of transfers for each area i when integrated as V_i^U and when split as V_i^S. We can express the changes in indirect utility for area i after a split as $\Delta V_i \equiv V_i^S - V_i^U$. Our next proposition details how welfare changes after a split.

Proposition 2. *If district B is relatively small* $\left(\frac{\alpha_B}{\alpha_A} \to 0\right)$ *and neglected by its parent district* $(\lambda \to 0)$*, and if municipality 2 is relatively large* $\left(\frac{\alpha_2}{\alpha_A+\alpha_B} \to \infty\right)$*, then (i)* ΔV_A *is small, (ii)* ΔV_B *is positive and large, and (iii)* ΔV_2 *is negative and small.*

Proof. See Appendix Section B. □

The intuition behind Proposition 2 is straightforward. Because of decreasing returns to spending, for a given configuration of population sizes and neglect by the headquarters district, the transfers moved from municipality 2 to district B may do little harm to the former and create substantial benefits to the latter. Welfare of district A changes little, either positively or negatively, depending on whether its transfers change or not.[30] We directly test these predictions in Section 3.5 by separately evaluating the consequences of splitting for headquarters and non-headquarters districts.

We highlight that, despite being outside the scope of this paper, the model can be extended to incorporate specific features from other contexts. For example, it is possible to allow for individuals "voting with their feet" (Tiebout, 1956), with adjustments in population shares after policy choices. Ethnic divisions between areas (Alesina et al., 2004; Pierskalla, 2016; Bazzi and Gudgeon, 2021) or municipality mergers (Weese, 2015; Blom-Hansen et al., 2016) are also potential extensions of the model.

[30]We abstract away from agglomeration effects. In a setting accounting for them, this result could be further exacerbated (Kline and Moretti, 2014).

B Proofs of Propositions

Proof of Proposition 1

Proof. To approximate the Brazilian context, we assume throughout that $\lambda \leq 0.5$, $\alpha_B < \alpha_A$, and $y_B < y_A$. We also highlight two conditions which come up in the proofs below:

(A1) a high comparative gain in transfers if split $\left(\frac{T(\alpha_A+\alpha_B)}{y} \leq \frac{T(\alpha_B)}{\alpha_B y_B}\right)$; and

(A2) a high comparative tax base $\left(\frac{\theta_B}{\theta_A} \leq \frac{y_B}{y_A}\right)$.

From the integrated policy choice problem (3.4), assuming there exists an interior optimum, we can solve the first-order condition:

$$\frac{g_B^U}{g_A^U} = \frac{\lambda}{1-\lambda}\frac{\alpha_B}{\alpha_A}\frac{\theta_B}{\theta_A} \tag{3.6}$$

The agent's private spending is $c_i = (1-\tau)y_i$. We can solve for a closed-form levels of public good provision and taxation under integration:

$$g_A^U = (1-\lambda)\alpha_A\theta_A\frac{y}{\overline{y}} \qquad g_B^U = \lambda\alpha_B\theta_B\frac{y}{\overline{y}} \qquad \tau^U = \frac{\overline{\theta}}{\overline{y}} - \frac{T(\alpha_A+\alpha_B)}{y} \tag{3.7}$$

where $\overline{y} \equiv (1-\lambda)\alpha_A y_A + \lambda\alpha_B y_B$, $y \equiv \alpha_A y_A + \alpha_B y_B$, $\overline{\theta} \equiv (1-\lambda)\alpha_A\theta_A + \lambda\alpha_B\theta_B$, and $\theta \equiv \alpha_A\theta_A + \alpha_B\theta_B$.

Similarly, for Problem (3.5), we can show that:

$$g_A^S = \alpha_A\theta_A \qquad g_B^S = \alpha_B\theta_B \qquad \tau_A^S = \frac{\theta_A}{y_A} - \frac{T(\alpha_A)}{\alpha_A y_A} \qquad \tau_B^S = \frac{\theta_B}{y_B} - \frac{T(\alpha_B)}{\alpha_B y_B} \tag{3.8}$$

District B unilaterally chooses to split if $U_B^S \geq U_B^U$. Substituting in Equations (3.7) and (3.8), we can express the surplus condition as:

$$\begin{aligned} G(\lambda, \alpha_A, \alpha_B, \theta_A, \theta_B, y_A, y_B, T) &\equiv U_B^S - U_B^U \\ &= \theta_B[\ln(g_B^S) - \ln(g_B^U)] + (\tau^U - \tau_B^S)y_B \\ &= \theta_B \ln\left(\frac{\overline{y}}{\lambda y}\right) + \left(\frac{\overline{\theta}}{\overline{y}} - \frac{\theta_B}{y_B} + \frac{T(\alpha_B)}{\alpha_B y_B} - \frac{T(\alpha_A + \alpha_B)}{y}\right) y_B \\ &\geq 0 \end{aligned} \tag{3.9}$$

We can show that:

1. $\frac{\partial G}{\partial \lambda} = -\frac{\alpha_A}{\lambda \overline{y}^2}[(1-\lambda)\alpha_A \theta_B y_A^2 + \lambda \alpha_B \theta_A y_B^2] \leq 0.$
2. $\frac{\partial G}{\partial \alpha_B} = -y_B \left[\frac{(1-2\lambda)\alpha_A \theta_B y_A}{\lambda y \overline{y}} + \frac{(1-\lambda)\lambda \alpha_A(\theta_A y_B - \theta_B y_A)}{\overline{y}^2} + \frac{T'(\alpha_A+\alpha_B)y - T(\alpha_A+\alpha_B)y_B}{y^2}\right] + \frac{\alpha_B T'(\alpha_B) - T(\alpha_B)}{\alpha_B}$

   After more algebra we conclude that $\frac{\partial G}{\partial \alpha_B} \leq 0$ if conditions (A1) and (A2) hold.
3. $\frac{\partial G}{\partial \theta_A} = \frac{(1-\lambda)\alpha_A y_A}{\overline{y}} \geq 0$
4. $\frac{\partial G}{\partial \theta_B} = \ln\left(\frac{\overline{y}}{\lambda y}\right) - \frac{(1-\lambda)\alpha_A y_A}{\overline{y}} \lesseqgtr 0.$
5. $\frac{\partial G}{\partial y_A} = -\frac{\alpha_A y_B}{y^2 \overline{y}^2}[\overline{\theta} y[(1-\lambda)y - (1-2\lambda)\alpha_B \theta_B] - T(\alpha_A + \alpha_B)\overline{y}^2] \lesseqgtr 0$
6. $\frac{\partial G}{\partial y_B} = \frac{\alpha_A y_A}{y^2 \overline{y}^2}[y((1-\lambda)\overline{\theta} y + (1-2\lambda)\alpha_B \theta_B) - T(\alpha_A + \alpha_B)\overline{y}^2] \lesseqgtr 0$

To further understand how choices of public goods and local taxation change after a split, we derive similar calculations for g_B and τ_B. If district B splits, it increases its provision of public goods ($g_B^S \geq g_B^U$) if, and only if

$$\begin{aligned} H(\lambda, \alpha_A, \alpha_B, \theta_A, \theta_B, y_A, y_B) &\equiv g_B^S - g_B^U \\ &= \alpha_B \theta_B - \frac{\lambda \alpha_B \theta_B y}{\overline{y}} \\ &= \frac{(1-2\lambda)\alpha_A \alpha_B \theta_B y_A}{\overline{y}} \geq 0 \end{aligned} \tag{3.10}$$

We can show that:

1. $\frac{\partial H}{\partial \lambda} = -\frac{\alpha_A \alpha_B \theta_B y_A y}{\overline{y}^2} \leq 0$

2. $\frac{\partial H}{\partial \alpha_B} = -\frac{(1-2\lambda)\theta_B y_A [\lambda \alpha_B^2 y_B - (1-\lambda)\alpha_A^2 y_A]}{\overline{y}^2} \geq 0.$

3. $\frac{\partial H}{\partial \theta_A} = 0$

4. $\frac{\partial H}{\partial \theta_B} = \frac{(1-2\lambda)\alpha_A \alpha_B y_A}{\overline{y}} \geq 0.$

5. $\frac{\partial H}{\partial y_A} = \frac{(1-2\lambda)\lambda \alpha_A \alpha_B^2 \theta_B y_B}{\overline{y}^2} \geq 0.$

6. $\frac{\partial H}{\partial y_B} = -\frac{(1-2\lambda)\lambda \alpha_A \alpha_B^2 \theta_B y_A}{\overline{y}^2} \leq 0.$

District B changes local tax rates from τ^U to τ_B^S after a split. This is equivalent to:

$$\begin{aligned} \tau_B^S - \tau^U &= \frac{\theta_B}{y_B} - \frac{\overline{\theta}}{\overline{y}} + \frac{T(\alpha_A + \alpha_B)}{y} - \frac{T(\alpha_B)}{\alpha_B y_B} \\ &= \frac{(1-\alpha)\alpha_A \alpha_B y [\theta_B y_A - \theta_A y_B] + \overline{y}[\alpha_B y_B T(\alpha_A + \alpha_B) - y T(\alpha_B)]}{\alpha_B y_B y \overline{y}} \end{aligned} \tag{3.11}$$

We conclude that local tax rates after a split are lower than when districts are integrated (i.e., $\tau_B^S \leq \tau^U$) if conditions (A1) and (A2) hold. □

Proof of Proposition 2

Proof. If district B is relatively small $\left(\frac{\alpha_B}{\alpha_A} \to 0\right)$ and captured and neglected by its parent district ($\lambda \to 0$), and municipality 2 is relatively large $\left(\frac{\alpha_2}{\alpha_A + \alpha_B} \to \infty\right)$, we have that:

$$\Delta V_A = \theta_A \ln\left(\frac{\overline{y}}{(1-\lambda)y}\right) + \left(\frac{\overline{\theta}}{\overline{y}} - \frac{\theta_A}{y_A} + \frac{T(\alpha_A)}{\alpha_A y_A} - \frac{T(\alpha_A + \alpha_B)}{y}\right) y_A \tag{3.12}$$

$$\Delta V_B = \theta_B \ln\left(\frac{\overline{y}}{\lambda y}\right) + \left(\frac{\overline{\theta}}{\overline{y}} - \frac{\theta_B}{y_B} + \frac{T(\alpha_B)}{\alpha_B y_B} - \frac{T(\alpha_A + \alpha_B)}{y}\right) y_B \tag{3.13}$$

$$\Delta V_2 = \frac{T^S(\alpha_2) - T^U(\alpha_2)}{\alpha_2} \tag{3.14}$$

Given our assumptions, one can show that $\Delta V_A \to 0$, $\Delta V_B \to \infty$, $\Delta V_2 \to 0$. □

C Data Construction

Splitting Requests

This appendix contains a detailed description of the data on split requests used in this paper. From historical archives, we construct a novel dataset that contains all requests to split initiated by districts between 1989 and 1996. Prior to the 1996 CA, each state assembly had discretion to set its own regulation over splitting, leading to substantial variation in records on split requests.

Brazil has 26 state legislative assemblies. For each state assembly, we search for digitized historical records on split requests during the first half of the 1990s. We find records for twelve states: Amapá, Amazonas, Espírito Santo, Goiás, Mato Grosso, Minas Gerais, Pará, Paraná, Rio Grande do Sul, Rondônia, Santa Catarina, and São Paulo. The availability and quality of the data widely vary across states. Figure 3.C.1 provides an example of the material available online, whereas Figure 3.C.2 depicts the distributions of request and split years among applicant districts. We list the variables we construct from the records for each state below:

Amapá: Indicator for whether district has requested to split; indicator for whether district has the request approved; identification number of the split process; start date of the process; approval date of the referendum; and result of the referendum.

Amazonas: Indicator for whether district has requested to split; indicator for whether district has the request approved; and result of the referendum.

Espírito Santo: Indicator for whether district has requested to split; indicator for whether district has the request approved; start date of the process; approval date of the referendum; and result of the referendum.

Goiás: Indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; identification number of the split process; approval date of the referendum; and result of the referendum.

Mato Grosso: Indicator for whether district has requested to split; indicator for whether district has the request approved; identification number of the split process; start date of the process; and result of the referendum.

Minas Gerais: Indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; date when the request was archived; identification number of the split process; start date of the process; approval date of the referendum; and result of the referendum.

Pará: Indicator for whether district has requested to split; indicator for whether district has the request approved; identification number of the split process; start date of the process; approval date of the referendum; and result of the referendum.

Paraná: Indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; identification number of the split process; start date of the process; and result of the referendum.

Rio Grande do Sul: Indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; identification number of the split process; start date of the process; approval date of the referendum; and result of the referendum.

Rondônia: indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; date when the request was archived; identification number of the split process; approval date of the referendum; and result of the referendum.

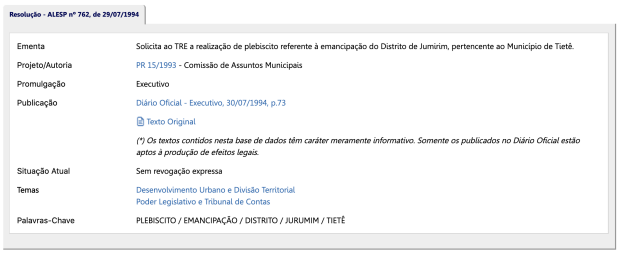
Santa Catarina: Indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; date when

the request was archived; identification number of the split process; start date of the process; approval date of the referendum; and result of the referendum.

São Paulo: Indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; identification number of the split process; start date of the process; approval date of the referendum; and result of the referendum.

Figure 3.C.1: Examples of Raw Material of Split Requests

(a) São Paulo



(b) Rio Grande do Sul

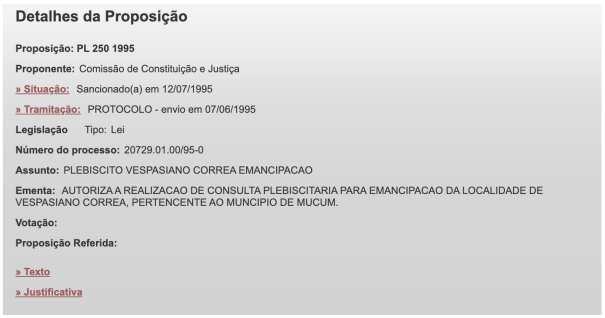
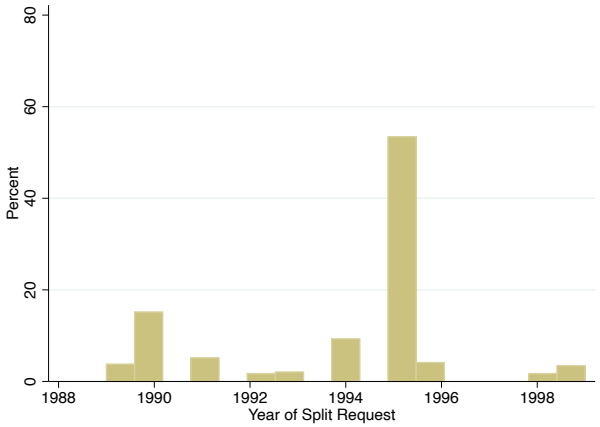
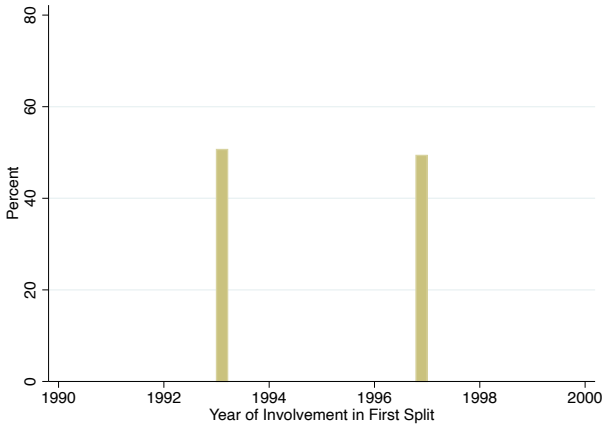


Figure 3.C.2: Histograms of Request and Split Years

(a) Year of *Request* among Almost Split Applicants



(b) Year of *Split* among Applicants



D Additional Results

Table 3.D.1: Descriptive Statistics at the District Level by Split Wave

	Applicants		Split		Almost Split		(2)-(1)		(4)-(3)		(5)-(3)		(6)-(4)	
	1993	1997	1993	1997	1993	1997								
	Mean	Mean	Mean	Mean	Mean	Mean	Dif.	p	Dif.	p	Dif.	p	Dif.	p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Population (000's)	5.73	4.95	4.94	4.32	9.8	6.88	-.78	.46	-.62	.27	-4.86	.03	-2.56	.05
% Urban Population	.42	.34	.4	.33	.54	.38	-.08	0	-.07	0	-.14	0	-.05	.17
% Male	.52	.52	.52	.52	.52	.52	0	.73	0	.77	0	.64	0	.78
% Literacy	.69	.62	.68	.61	.7	.66	-.06	0	-.07	0	-.02	.2	-.05	.02
% Piped Water	.45	.45	.5	.43	.17	.52	0	.92	-.07	.02	.34	0	-.08	.07
% Sanitation	.58	.63	.65	.63	.22	.64	.06	.06	-.02	.56	.43	0	-.01	.76
% Trash Removal	.1	.1	.1	.09	.05	.14	0	.76	-.02	.38	.05	.07	-.05	.08
Avg. Luminosity	1.97	1.51	1.23	.73	5.74	3.87	-.45	.36	-.49	.17	-4.52	0	-3.14	0
Area (000's km2)	.5	.61	.57	.69	.14	.35	.11	.4	.12	.43	.43	.03	.34	.19
Log Distance to Parent Town Hall	2.97	2.96	3.05	3.05	2.56	2.69	-.01	.92	.01	.93	.48	0	.36	0
Log Distance to State Capital	5.49	5.45	5.5	5.6	5.42	5.02	-.04	.59	.09	.17	.08	.46	.58	0
Log Maize Suitability	8.64	8.69	8.68	8.69	8.45	8.67	.04	.05	.01	.58	.23	0	.03	.46
Log Wet Rice Suitability	8.57	8.68	8.56	8.68	8.64	8.71	.11	.08	.12	.14	-.09	.58	-.03	.23
Log Soybean Suitability	7.7	7.74	7.7	7.73	7.71	7.76	.04	.27	.03	.46	-.01	.83	-.03	.69
Log Wheat Suitability	6.56	6.52	6.45	6.66	7.13	6.12	-.04	.88	.2	.48	-.67	.14	.54	.21
Terrain Ruggedness	86.16	79.44	95.21	83.22	39.84	67.99	-6.72	.32	-11.99	.12	55.37	0	15.23	.18
	N = 306	N = 246	N = 256	N = 185	N = 50	N = 61								

Note: This table reports descriptive statistics at the district level by split wave. We use information from 1991 Demographic Census, 1992 night lights, MapBiomas, FAO-GAEZ soil suitability, and terrain ruggedness data. The variables are: total population (in thousands), shares of urban and male population, literacy rates, share of households with access to piped water, sanitation and trash removal, average luminosity, total area (in thousands km^2), log distance to town hall, log distance to state capital, log soil suitability for different crops (maize, wet rice, soybean and wheat), and log terrain ruggedness. See Section 3.3 for further details on data and sample construction of the district-level data.

Table 3.D.2: Effects of Redistricting on Bureaucracy in the Public Sector

	Log Expenditures p.c. (1)	Log Capital Expenditures p.c. (2)	Log Current Expenditures p.c. (3)	Log Municipal Jobs (4)	Log Municipal Wages (5)
Post x Split	0.15***	0.27***	0.15***	0.23***	0.23***
	(0.03)	(0.08)	(0.03)	(0.07)	(0.07)
Observations	8,110	8,105	8,109	7,270	7,270
R-squared	0.97	0.71	0.93	0.85	0.90
State-Year FE	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports the aggregate effects of redistricting on the setup of new local governments. We consider the following dependent variables: log total municipal expenditures per capita, log municipal capital expenditures per capita, log municipal current expenditures per capita, log total number of municipal jobs, and log municipal wages. The main data sources are the annual Brazilian National Treasury data between 1989 to 2018 and the annual RAIS data between 1995 to 2018. We also use information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO) to classify jobs and economic sectors. Standard errors are two-way clustered both at the state and split wave levels.

Table 3.D.3: Effects of Redistricting on Public Services and Poverty

	% Piped Water	% Trash Collection	% Electricity	% Sewage	% Extreme Poverty	% Poverty
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Split	1.77	4.42*	2.50	1.00**	-1.33	-1.77*
	(1.89)	(2.31)	(2.87)	(0.40)	(1.11)	(0.96)
Observations	1,344	1,344	1,344	1,344	1,344	1,344
R-squared	0.89	0.87	0.83	0.89	0.89	0.94
Mean	71.18	63.51	81.33	96.10	19.62	42.81
SD	24.17	27.35	20.03	7.66	14.86	20.60

Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports the aggregate effects of redistricting on public services and poverty. We consider the following dependent variables: household access to piped water, trash collection, electricity, sewage, extreme poverty and poverty rates. The main data sources are the decennial Demographic Census from 1991, 2000 and 2010. Standard errors are two-way clustered both at the state and split wave levels.

Table 3.D.4: Effects of Redistricting on the Private Sector

	Log Private Establishments (1)	Log Private Jobs (2)	Log Private Wages (3)
Post x Split	0.06	0.07	0.08
	(0.07)	(0.06)	(0.05)
Observations	7,348	7,348	7,348
R-squared	0.99	0.97	0.97
State-Year FE	✓	✓	✓
Municipality FE	✓	✓	✓
Year FE	✓	✓	✓

Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports the aggregate effects of redistricting on the private sector. We consider the following dependent variables: log total number of establishments, log total number of jobs, and log wages, all measured in the private sector. The main data sources are the annual RAIS data between 1995 to 2018. We also use information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO) to classify jobs and economic sectors. Standard errors are two-way clustered both at the state and split wave levels.

Table 3.D.5: Effects of Redistricting on Economic Activity: Heterogeneity Across Districts

	Panel A: Log Luminosity		
	Applicants	Remaining	Headquarters
	(1)	(2)	(3)
Post x Split	0.34***	-0.05	0.07***
	(0.03)	(0.10)	(0.02)
Observations	10,122	5,893	6,947
R-squared	0.96	0.96	0.98
Mean Pre-Split	-0.76	-0.86	0.18
SD Pre-Split	1.52	1.43	1.43
	Panel B: % Pixels Lit		
	Applicants	Remaining	Headquarters
	(1)	(2)	(3)
Post x Split	0.04***	-0.06**	0.01
	(0.01)	(0.02)	(0.01)
Observations	10,122	5,893	6,947
R-squared	0.96	0.94	0.97
Mean Pre-Split	0.18	0.16	0.24
SD Pre-Split	0.29	0.26	0.29
	Panel C: Luminosity Gini		
	Applicants	Remaining	Headquarters
	(1)	(2)	(3)
Post x Split	-0.03***	0.02	-0.01
	(0.01)	(0.02)	(0.01)
Observations	9,616	5,052	6,918
R-squared	0.96	0.93	0.97
Mean Pre-Split	0.83	0.84	0.84
SD Pre-Split	0.24	0.21	0.18

Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports the aggregate effects of redistricting on luminosity separately for three groups of districts: applicant, remaining, and headquarters. We consider the following dependent variables for each panel: log average luminosity, indicator variable for whether the average is above zero, and average luminosity Gini index. Standard errors are two-way clustered both at the state and split wave levels.

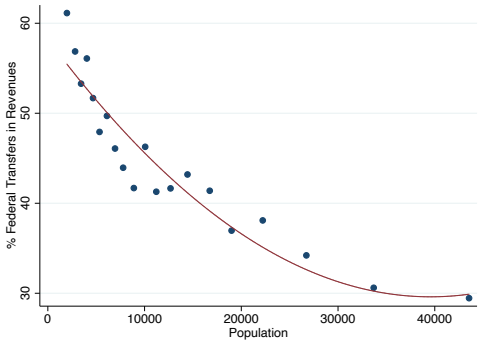
Table 3.D.6: Effects of Redistricting on Local Revenues

	Log Revenues p.c. (1)	Log Federal Transfers p.c. (2)	Log Local Taxation p.c. (3)
Post x Split	0.14***	0.32***	0.11
	(0.03)	(0.02)	(0.10)
Observations	8,110	8,086	8,109
R-squared	0.97	0.90	0.90
State-Year FE	✓	✓	✓
Municipality FE	✓	✓	✓
Year FE	✓	✓	✓

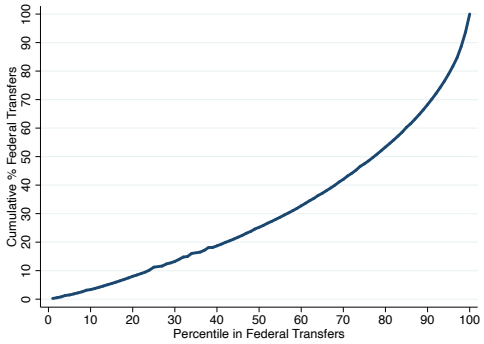
Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports the aggregate effects of redistricting on local revenues. We consider the following dependent variables: log revenues per capita, log federal transfers per capita, and log local taxation per capita. The main data sources are the annual Brazilian National Treasury data between 1989 to 2018. Standard errors are two-way clustered both at the state and split wave levels.

Figure 3.D.1: Distribution of Federal Transfers

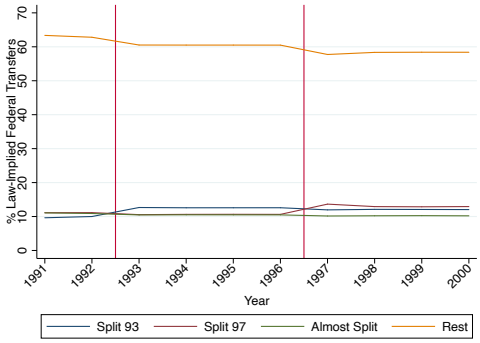
(a) Share of Federal Transfers Relative to Municipal Revenues in 1991



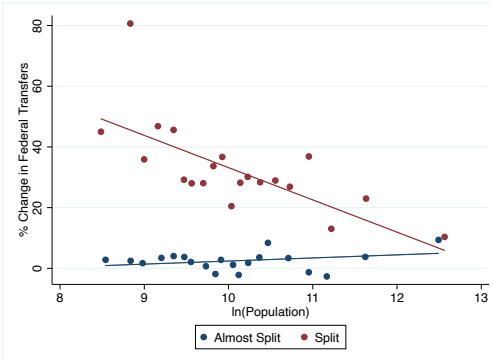
(b) Cumulative Distribution of Federal Transfers in 1991 (Bottom 50% Get ≈ 26%)



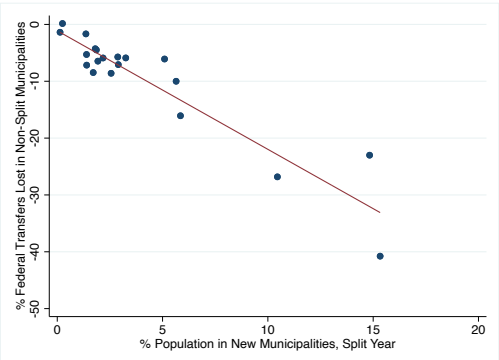
(c) Group Shares After Split Waves



(d) Change in Federal Transfers Across Groups



(e) Within-State Change in Federal Transfers for Non-Split Municipalities

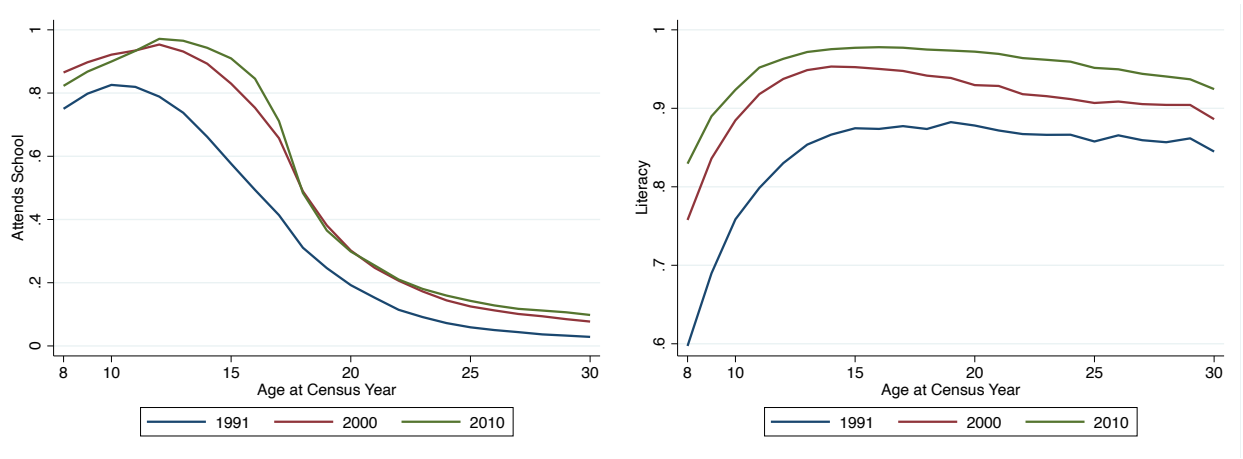


Note: This figure reports the patterns of revenues from federal transfers (FPM) over time, as described in Section 3.2. Panel (a) describes the share of municipal revenues from federal transfers for small municipalities in 1991. Panel (b) plots the distribution of federal transfers in 1991. Panel (c) plots the reallocation of federal transfers after the 1993 and 1997 split waves implied by the transfer allocation mechanism. Panel (d) illustrates how the gains in revenues from federal transfers accrue particularly to new municipalities with smaller population. Panel (e) shows the within-state reallocation of federal transfers for municipalities that did not split after the 1997 split wave.

Figure 3.D.2: Education Outcomes: Raw Data

(a) School Attendance

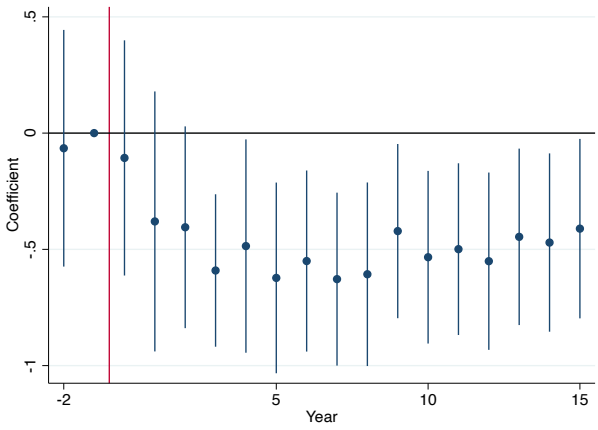
(b) Literacy Rates



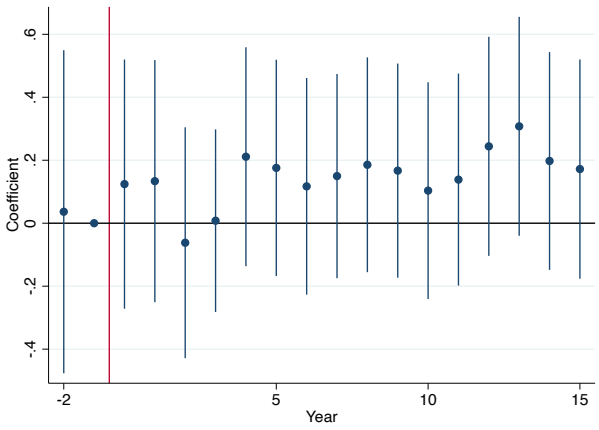
Note: This figure displays the raw data for school attendance and literacy rates from the sample described in Figure 3.5, by year and age. The main data sources are the decennial Demographic Census microdata from 1991, 2000 and 2010.

Figure 3.D.3: Crowd-Out Effects on Jobs in Education

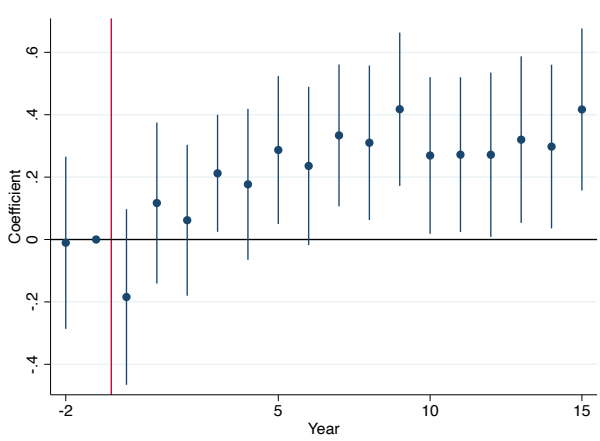
(a) Log Nonprofit Jobs



(b) Log Private Sector Jobs



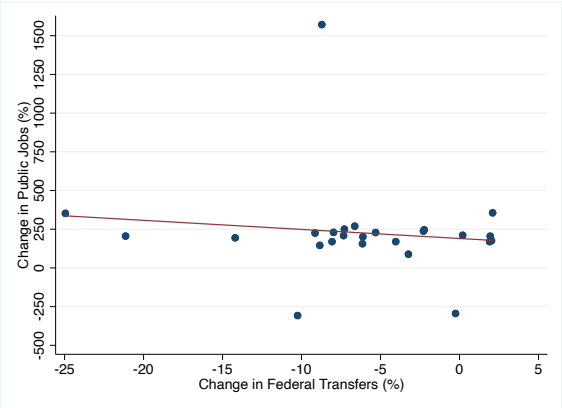
(c) Log Public Sector Jobs



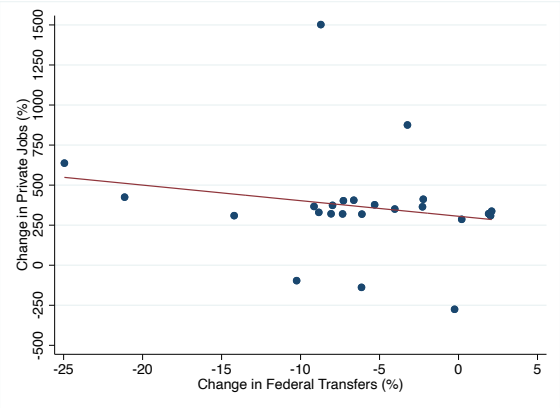
Note: This figure reports annual crowd-out effects of redistricting on jobs in the nonprofit, private, and public sectors in the education area after estimating Equation (3.1). The omitted category is the year before splitting. The main data sources are the annual RAIS data between 1995 to 2018. We also use information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO) to classify jobs and economic sectors. Standard errors are two-way clustered both at the state and split wave levels.

Figure 3.D.4: Spillover Effects of Changes in Federal Transfers on the Rest of the Country

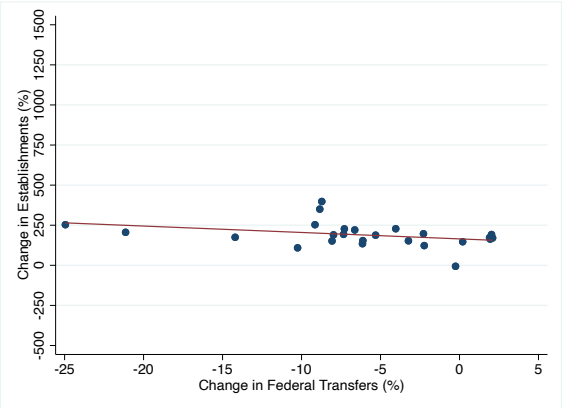
(a) Public Jobs



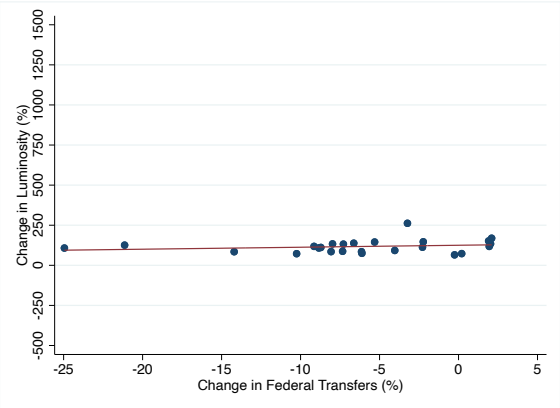
(b) Private Jobs



(c) Establishments



(d) Luminosity



Note: This figure reports correlations between changes in federal transfers (in percentage points) and selected outcomes (in percentage points) for municipalities that did not split. To capture changes in federal transfers, we exploit variation in the number of splits within states after residualizing for region dummies. Outcomes of interest are percentage changes in total number of public jobs, number of private jobs, number of establishments and average luminosity 15 years after splitting. We exclude Distrito Federal and Roraima from the final sample. Further details can be found in Table 3.6.

E Details for Difference-in-Discontinuities in Luminosity

Econometric Specification. Using the nighttime luminosity data at the district level, we estimate the following difference-in-discontinuities model in two stages:

$$Split_{m(d)} = \psi + \phi \mathbf{1}[RV_d \geq 50\%] + \kappa g(RV_d) + \eta_d \tag{3.15}$$

$$y_{dt} = \alpha_d + \alpha_t + \beta Split_d Post_{w(d)} + \gamma g(RV_d) Post_{w(d)} + X_{dt}\lambda + \varepsilon_{dt}. \tag{3.16}$$

From the first-stage Equation (3.15), we have that $Split_{m(d)}$ is an indicator variable for whether the municipality m with district d split after the referendum; RV_d represents the referendum vote share in favor of splitting in district d; $g(RV_d)$ is defined as a linear distance from the cutoff; and $\mathbf{1}[RV_d \geq 50\%]$ is an indicator for whether district d obtained at least half of votes in the referendum. The second-stage Equation (3.16) includes district and year fixed effects, α_d and α_t; and $Post_{w(d)}$, which is an indicator variable for the years after the wave-year w of splitting request. To account for fewer observations on the left side of the cutoff, our preferred specification considers a 15 percent bandwidth. The coefficient of interest, β, captures the effect of splitting. To support the validity of the research design, Appendix Table 3.E.1 shows no evidence of discontinuities in pre-referendum characteristics at the district level around the cutoff.[31]

Panel (a) of Appendix Figure 3.E.1 provides a visual evidence of the first-stage, confirming that reaching the simple majority determines splitting. Comparing applicant districts that barely obtained the majority of necessary votes to split to those that did not, Panel (a) of Appendix Figure 3.E.2 displays a clear jump on the growth of log luminosity around the cutoff. In terms of magnitude, Columns (1) and (3) of Appendix 3.E.2 point to the Wald estimate of 23 percent (= 0.22/0.96). This effect is close to the difference-in-differences

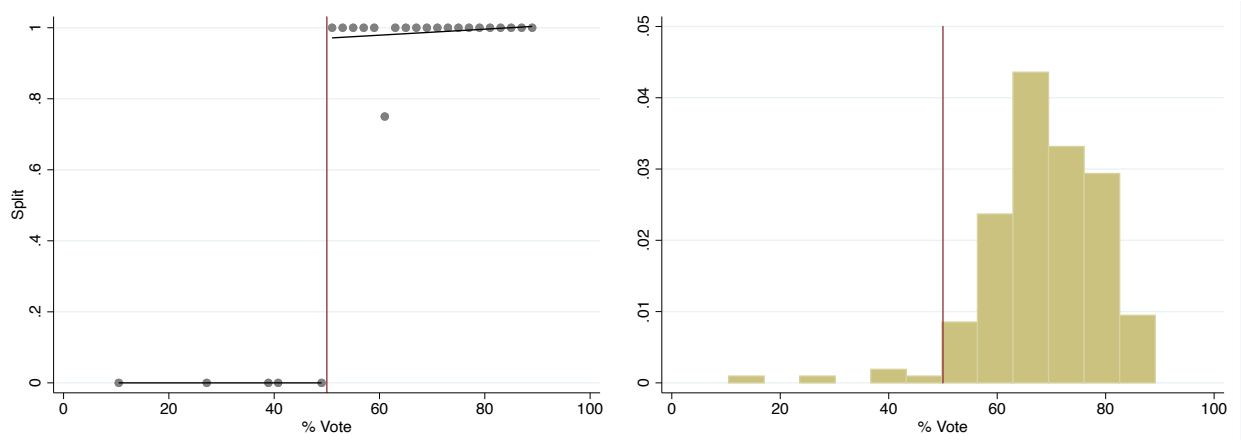
[31] We use baseline characteristics from 1991. Panel (b) of Appendix Figure 3.E.1 depicts the distribution of vote shares around the 50 percent cutoff and points that there are fewer districts with less than half of voters.

estimate restricted to the state of Minas Gerais (Column (4)). Concerning heterogeneity across districts, Panel (b) of Appendix Figure 3.E.2 shows that the gains are driven by applicant districts.

Figure 3.E.1: Referenda in Minas Gerais

(a) First Stage

(b) Histogram of Vote Shares



Note: This figure describes the referendum data from the state of Minas Gerais. Panel (a) plots the first stage of referendum votes on the likelihood of splitting. Panel (b) plots the frequency of referendum vote shares. As described in Section 3.2, districts are required to obtain at least 50 percent turnout and votes in favor of splitting in the unilateral referendum as one of necessary steps to become a municipality.

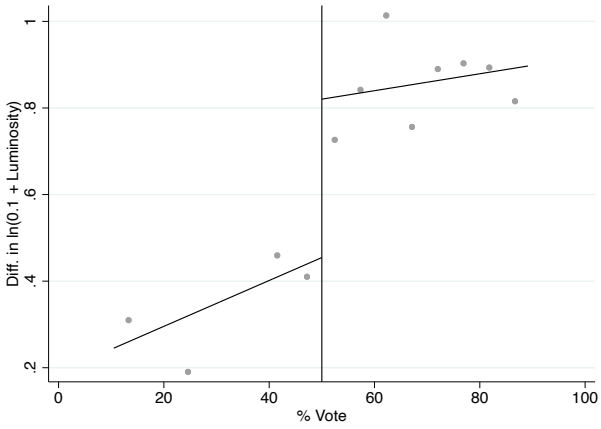
Table 3.E.1: Discontinuity Test on Pre-Referendum Characteristics

	(1) Log Population	(2) Log Area	(3) Log Luminosity	(4) Log Distance Town Hall
Referendum Vote $\geq 50\%$	0.81***	0.18	0.58	-0.05
	(0.28)	(0.34)	(0.37)	(0.30)
Observations	50	50	50	50
R-squared	0.38	0.23	0.40	0.13
Mean	3.12	5.71	-1.43	3.12
SD	0.63	0.95	1.54	0.63

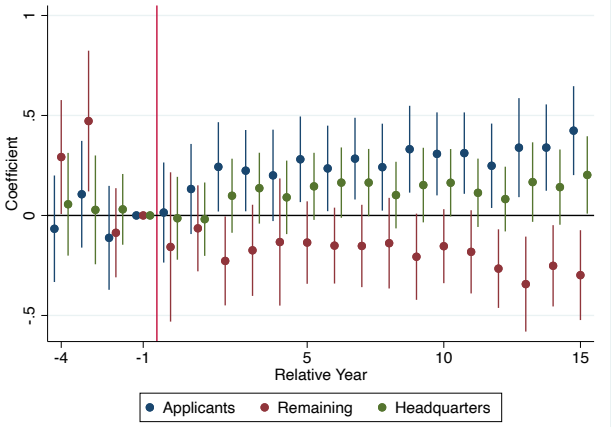
Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports point estimates from modified versions of Equation (3.15) to test for discontinuities in district-level characteristics prior to the referendum. We use the following pre-referendum characteristics: log total population, log total area, log average luminosity, and log distance to the parent town hall.

Figure 3.E.2: Effects of Redistricting on Log Luminosity: Difference-in-Discontinuities

(a) Log Luminosity Growth



(b) Log Luminosity: RD-DD Event-Study



Note: This figure reports results from specifications described in Appendix Section E. Panel (a) plots the growth in log luminosity for applicant districts with share of votes from local referendum in favor of splitting below and above the approval cutoff of 50 percent. Panel (b) plots point estimates of the difference-in-discontinuities from Equation (3.16) for the applicant and headquarters districts separately. The omitted category is the year before splitting.

Table 3.E.2: Effects of Redistricting on Log Luminosity

	First Stage	Reduced Form	Second Stage	DD
	(1)	(2)	(3)	(4)
Referendum Vote $\geq 50\%$	0.96***			
	(0.03)			
Post x Referendum Vote $\geq 50\%$		0.13**		
		(0.06)		
Post x Split			0.21***	0.26***
			(0.05)	(0.03)
Observations	50	985	985	2,422
R-squared	0.64	0.98	0.98	0.98
District FE	-	✓	✓	✓
Mean	0.88	-0.82	-0.82	-0.60
SD	0.39	1.83	1.83	1.94

Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports estimates from the difference-in-discontinuities specification applied to the luminosity data. Column (1) refers to Equation (3.15), whereas Column (2) reports the reduced-form impacts. Column (3) refers to Equation (3.16). Column (4) speaks to the difference-in-difference estimates from Equation (3.3) restricted to the state of Minas Gerais. Except for Column (1), in which the dependent variable is an indicator variable for whether there is a split, the dependent variables are log luminosity.

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