Journal of Development Economics 176 (2025) 103516

Contents lists available at ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

Regular article

Enforcement spillovers under different networks: The case of quotas for persons with disabilities in Brazil ☆

Samuel Berlinski [a,b], Jessica Gagete-Miranda [c,*]

[a] Inter-American Development Bank, United States of America
[b] IZA, Germany
[c] Research Institute for the Evaluation of Public Policies, Fondazione Bruno Kessler (FBK-IRVAPP), Italy

ARTICLE INFO

JEL classification:
I38
J68
K31

Keywords:
Enforcement spillovers
Networks
Persons with disability
Brazil

ABSTRACT

This study examines labor law enforcement spillovers in Brazil, focusing on disability quota enforcement for formal firms. New inspection procedures increased compliance through heightened inspections and fines, boosting disability hiring. The largest increase is observed among individuals with mobility impairments, followed by those with visual and cognitive impairments. Most new hires came from outside the formal labor market, while some were poached from other firms. Additionally, job tenure for persons with disabilities in large firms improved. We investigate spillover effects across various firm networks: neighborhood, ownership, and human resources specialists. Results show that spillovers can have up to 3.4 times the impact on disability employment compared to direct fines. These findings highlight the potential for targeted enforcement strategies to amplify policy effectiveness beyond directly affected firms, even in developing economies characterized by low compliance with employment laws.

1. Introduction

Compliance is a crucial issue policymakers face in virtually all legislative spheres, such as tax (Slemrod, 2019), traffic (Lu et al., 2016), environmental (Shimshack, 2014) and labor (Almeida and Ronconi, 2016) regulations. Deterrence achieved through the inspection and punishment of non-compliers is costly and reaches very few individuals and firms in most environments. To improve general compliance, information about the inspection and punishment of a few must travel between individuals and firms (see, Slemrod, 2019; Johnson, 2020). This paper investigates how regulatory enforcement influences compliance and how information on employment fines is transmitted through different firm networks. In particular, we study the case of quotas for persons with disabilities in Brazil (henceforth Quota Law or QL) and explore (i) the impact of new inspection and punishment procedures that increased the regulatory enforcement of the QL; and (ii) how firms learn from different networks about the QL enforcement and their risk of being punished.

The Brazilian Quota Law mandates that companies employing more than 100 workers allocate at least two percent of their employment positions to persons with disabilities. This legal requirement provides an ideal setting for our investigation for several reasons. First, even though the requirement was introduced in 1991, labor regulatory offices only started to effectively enforce it and punish non-compliers about 20 years later. Hence, for a considerable time, firms' prior was that such a *de jure* law was not a *de facto* law, so they would not face the risk of punishment in case of non-compliance. Enforcement became tighter after the introduction of an administrative act in 2012, enabling us to study firms' adjustment processes once a particular law starts to be enforced. Second, the Brazilian context allows us to combine different types of data to investigate how law enforcement spills over through different firms' networks. Third, Brazil exemplifies the challenges developing countries face in providing labor market opportunities for persons with disabilities. The relative employment rate of individuals with disabilities compared to those without ranges from 0.086 to 0.119, depending on age –figures comparable to other

☆ We thank the editor and two anonymous referees for their valuable suggestions. We also acknowledge comments from Matthew S. Johnson, Gabriel Ulyssea, and participants at the 2023 Workshop on Networks and Development, the NEUDC 2023 Conference, and seminars at CAF – Development Bank of Latin America and the Caribbean, the Inter-American Development Bank, University of Milano-Bicocca, University of Pretoria, and the University of Sao Paulo. The opinions expressed in this publication are those of the authors and do not necessarily reflect the views of the Inter-American Development Bank, its Board of Directors, or the countries they represent.

* Corresponding author.
E-mail address: jgagetemiranda@irvapp.it (J. Gagete-Miranda).

https://doi.org/10.1016/j.jdeveco.2025.103516
Received 15 August 2024; Received in revised form 7 April 2025; Accepted 7 April 2025
Available online 22 April 2025

Latin American countries (Berlinski et al., 2021) but significantly lower than the OECD average of 0.6 (OECD, 2003). Finally, Brazil also illustrates the gap between *de jure* and *de facto* laws in developing countries, which reflects endemic problems for governments in enforcing compliance with laws (Acemoglu et al., 2015).

We employ a difference in discontinuity design on matched employee-employer data and document that the 2012 Administrative Act led to an increase in inspections and fines issued due to non-compliance with the Quota Law and, consequently, to an increase in the hiring of persons with a disability after that year. At the margin where causal inference is possible, we estimate an increase of about 7%. The increase is highest for individuals with mobility impairments, followed by those with visual and cognitive impairments, while no effect is observed for individuals with hearing impairments.

We also provide evidence of increased poaching of workers with disabilities from other firms following the 2012 Act. However, the primary effect was a rise in the hiring of individuals with disabilities who were previously outside the formal labor market. Additionally, the Act improved job tenure for persons with disabilities in large firms.

Furthermore, we show non-causal evidence of what might be interpreted as enforcement spillovers. The impact of the 2012 Administrative Act on issuing QL fines decreases as enforcement capacity decreases. However, the impact on hiring persons with disabilities is independent of the local enforcement capacity.

Finally, we present the key contribution of the paper where we test for the presence of enforcement spillovers by employing a stacked differences-in-differences design to understand how the occurrence of a QL fine in a firm's network impacts the likelihood that such a firm will increase its hiring of workers with disabilities. We look at three different networks: *neighbor network* (i.e., firms located in the same zip code of a specific firm), *owner network* (i.e., firms that belong to the same owner of a specific firm or an associate of such an owner) and *HR workers network* (i.e., firms where the HR workers of a specific firm were working before joining such a firm). We compare firms within networks where a Quota Law fine was issued to firms within networks where a Quota Law fine will be issued in the future but have not received one yet.

We find strong evidence of enforcement spillovers when a QL fine happens in the neighbor, owner, or HR workers' networks. If another firm in the network of a firm i receives a QL fine, this increases the number of workers with a disability present in firm i in the following years by 12.3% in the neighbor network, 4.5% in the owner network, and 4.0% in the HR workers' network. A back-of-the-envelope calculation indicates that the total number of workers with disabilities hired due to spillover effects is 3.4 times as large as the direct impact of receiving a QL fine in the neighbor network and 2.4 times as large in the HR workers' network. This figure drops considerably for the owner network, where spillover effects amount to only 20% of the direct impact, due to the smaller size of such networks.

Spillover effects are more pronounced among firms that were further away from the target for compliance with the Quota Law when the fine was issued. For non-compliant firms, the increase in hiring ranges from 20.5% to 26.7%, depending on which network was affected by the QL fine. Among low-compliant firms –firms employing some workers with disabilities but covering less than 50% of the required quota –, the impact is smaller, ranging from 11.2% to 21.7%. As expected, we find no significant effect on high-compliant firms –those meeting at least 50% of the required quota.

We finally show that the likelihood of being inspected does not increase after the occurrence of a QL fine in any of the firm's networks. This suggests that the spread of information and not the *local* increase in enforcement is causing the emergence of spillovers.

This paper contributes to two different streams of the literature. First, it contributes to the literature on regulations to improve the employment opportunities of people with a disability. Title I of the Americans with Disabilities Act (ADA) of 1990 prohibits employment discrimination against qualified individuals with disabilities in hiring, firing, advancement, compensation, and other job-related processes. Evidence on its impact is mixed: some studies find negative effects on labor market participation (Acemoglu and Angrist, 2001; DeLeire, 2000a,b), while others challenge these findings (Hotchkiss, 2004; Jolls and Prescott, 2004).[1] Outside of the United States, quota systems like the one analyzed in this study have been adopted by over two-thirds of OECD countries (OECD, 2003). In Austria (Lalive et al., 2013), Hungary (Krekó and Telegdy, 2022), and Japan (Mori and Sakamoto, 2018), research has found that firms comply with such regulations. Papers that study the quota system in developing countries, where regulatory compliance is arguably worse, usually leverage the role of law enforcement in improving compliance with the quota to calculate its welfare effects, like Szerman (2022) and de Souza (2023), who also study the Brazilian quota system. In this paper, we provide evidence that enforcing the Quota Law improves key labor market outcomes for persons with disabilities, such as the likelihood of formal employment and job tenure. Furthermore, our evidence on spillover effects should be considered in welfare calculations.

Second, this paper contributes to the literature investigating the impacts of law enforcement and, more specifically, the emergence of enforcement spillovers among firms.[2] Inspections, auditing, and fines have been proven effective in increasing regulatory compliance (see Gray and Shimshack, 2011; Levine et al., 2012, for examples on environmental and occupational health and safety regulators). However, developing countries struggle with low enforcement capacity, challenging compliance efforts in many sectors and localities (Almeida and Ronconi, 2016; Ponczek and Ulyssea, 2022). Hence, such countries could significantly benefit from enforcement spillovers since the emergence of such indirect impact would have a multiplier effect on each atomistic enforcement effort. The evidence on enforcement spillovers so far comes from developed countries. In the United States, for instance, Shimshack and Ward (2005) and Evans et al. (2018) provide evidence that enforcing environmental regulations increases future compliance of other firms located in the same state where enforcement occurs but may create negative externalities in areas that are not inspected. Also in the United States, Johnson (2020) shows that publicizing firms' health violations led other firms to comply more with such a regulation. However, the emergence of such spillovers in developing countries is far from obvious due to the weaker enforcement capacity and the larger gap between *de jure* and *de facto* laws. Besides being the first paper to present evidence of enforcement spillovers in developing countries, this paper also contributes to this literature by showing how information about law enforcement flows through different networks connected to the firm, which can help inform policymakers on how to improve targeting to leverage the emergence of spillovers.

2. Background

The Brazilian Quota Law is an important example of the broader emphasis that both developing and developed countries are placing on diversity and inclusion policies.[3] This is an important policy goal for

[1] For a review of the ADA's impact and related policies in the U.S., see Livermore and Goodman (2009).

[2] The literature has shown that there are enforcement spillovers among individuals' networks (family, co-workers, and neighbors) in developed countries for dividend and capital taxation, commuter tax allowances, and TV license payments (e.g., Alstadsæter et al., 2019; Drago et al., 2020; Paetzold and Winner, 2016; Rincke and Traxler, 2011).

[3] For example, the United Nations' Sustainable Development Goals for the year 2030 aspire to: SDG 4: "Ensure inclusive and equitable quality education and promote lifelong learning opportunities for all"; SDG 5: "Achieve gender equality and empower all women and girls"; SDG 8: "Promote sustained, inclusive and sustainable economic growth, full and productive employment,

ethical, efficiency, and redistributive reasons. Berlinski et al. (2021) estimate that in Latin America and the Caribbean (LAC) countries, 88 million people were living with a disability in 2020 (around 15 percent of the population). By 2050, this figure could rise by 60 million. People living with disabilities have lower educational attendance and school completion rates and large gaps in labor market outcomes with respect to those living without disabilities. For example, Berlinski et al. (2021) report that the employment disability gap for people aged 25–34 in the eight national censuses for LAC countries they analyze is, on average, 18.5 percentage points.

Governments often implement employment quotas to promote the hiring of persons with disabilities (see, Mont et al., 2004; Förster, 2007). These quotas are commonly used among OECD and partner countries (Förster, 2007; OECD, 2003). The specific regulations regarding which firms are subject to quotas and the percentage of vacancies they should reserve for persons with disabilities vary across countries. Major corporations are the primary focus of such measures, with the typical proportion of jobs set aside for individuals with disabilities hovering around 4%.

In Brazil, the Quota Law[4] is the most relevant legislation regarding the employment opportunities of persons with disability.[5] It establishes that, respectively, firms with more than 100, 200, 500, and 1,000 employees must fill at least 2%, 3%, 4%, and 5% of their payroll with people with a certified disability.[6]

However, compliance with the Quota Law has historically remained limited since its introduction in 1991. For instance, in 2009, less than 30% of firms with more than 100 workers were employing the minimum number of workers with disabilities established by the law. Compliance has been an issue in other countries as well (see, OECD, 2003) despite some evidence of the effectiveness of quotas at increasing the employment of people with disabilities in high-income OECD countries (Lalive et al., 2013; Mori and Sakamoto, 2018; Krekó and Telegdy, 2022).

One reason compliance has been so low in Brazil is that its enforcement was virtually nonexistent for an extended period after it was put into effect. Even though the law was introduced in 1991, it was only in 2001 that the government established the first administrative act on inspection procedures for labor auditors regarding the Quota Law.[7] Such procedures did increase the labor market opportunity for persons with disability (Szerman, 2022). Nevertheless, they were still quite loose, and firms had much room to bypass them. For instance, the rule used to calculate the precise number of workers with a disability that firms should employ during the year was ambiguous, especially for firms with seasonal hiring patterns. An additional challenge faced by labor inspectors was their limitation to examining companies situated solely within their geographical jurisdiction. This constraint hindered their ability to confirm the employment of workers in companies with multiple facilities. Indeed, between 2003 (the first year of administrative data on fines) and 2005, less than 100 fines were issued yearly despite high levels of non-compliance.

In 2012, the government replaced the 2001 Administrative Act with a new and more stringent resolution.[8] This new act had clearer inspection procedures and guidelines for imposing penalties on non-compliant firms. For example, it established that if a firm has multiple plants, the inspector is allowed to forward the audit regarding the number of workers with a disability to the firm's headquarters in order to calculate the total number of such workers in all the firm's plants. Another crucial change regarding the last administrative act was verifying workers' disability status. While the 2001 Administrative Act did not mention how inspectors should verify such status, the 2012 Administrative Act established that firms should have medical reports detailing whether the worker has a disability and the type of disability. The report needs to be signed by the worker as well, indicating their acknowledgment that they are being employed to fulfill the company's designated quota.

After an inspector confirms a violation related to the Quota Law, the company is required to enter into an agreement that includes a commitment to hiring the specified minimum number of employees with disabilities. Additionally, the company must undertake any necessary accomodations to ensure workplace accessibility. The firm has at most 120 days to comply with this agreement. A fine is issued if the firm refuses to sign this agreement or does not fulfill its requirements within the deadline. In 2018, a US $600 fine was levied for each mandated vacancy not filled with a person with a disability.

Following the enactment of this new administrative act, the number of QL fines experienced a sharp increase. This trend is depicted in Fig. 1, which shows the relative change in the issuance of fines across different labor regulations. While the issuance of QL fines aligned with the trend of other fines until 2012, it diverged thereafter, exhibiting a steep increase.

3. Data

We combine three sources of data. The first is the matched employer–employee data of all formal workers in Brazil (RAIS, in the Portuguese acronym).[9] Formal firms must fill out the RAIS questionnaire once a year, providing details about the firm and all its employees. The Brazilian government uses such information to produce labor market statistics and to determine workers' eligibility for financial benefits.[10] Among the information provided are employee background information (e.g., age, gender, educational background, disability status, and occupation), employment contract information (e.g., start and end dates, type of contract, working hours and earnings), and workplace information (e.g., industry, and location). Whenever a firm has more than one establishment, we consider all establishments in calculating the firm's size and number of workers with a disability.[11] All our analyses are at the firm's level, and we consider their headquarters as the location city.

and decent work for all"; SDG 10: "Reduce inequality within and among countries."; SDG 11: Make cities and human settlements inclusive, safe, resilient, and sustainable; and SDG 16, "Peace, justice and strong institutions," which promotes building effective, accountable, and inclusive institutions at all levels to ensure peaceful and inclusive societies for all. Additionally, this SFD is consistent with the social model of disability embedded in the 2008 United Nations Convention on the Rights of Persons with Disabilities, and it is aligned with ILO Convention 169 and articles 3 and 4 of the UN Declaration on the Rights of Indigenous Peoples, which recognize their right to make autonomous decisions regarding their development priorities.

[4] Art. 93. of Law 8.213/1991.

[5] More recent legislation – especially Law No. 13,146/2015 – establishes additional benefits for persons with disabilities, such as an earlier retirement age (ranging from 55 to 60 years depending on gender and severity of the disability) or reduced contribution time (20 to 33 years). It also prohibits discrimination against persons with disabilities in all aspects of life, including employment, and guarantees accessibility in public spaces, inclusive education, and priority in social assistance programs.

[6] According to the Brazilian Law No. 13,146, of July 6, 2015. "A person with a disability is considered to be someone who has a long-term impairment of a physical, cognitive, intellectual, or sensory nature, which, in interaction with one or more barriers, may hinder their full and effective participation in society on an equal basis with others". Anyone meeting this criterion and having a medical certificate stating their disability is eligible for employment under the Quota Law, regardless of the type or severity of the disability.

[7] Instrução Normativa SIT n° 20 de 26/01/2001

[8] Instrução Normativa SIT n° 98 de 15/08/2012

[9] Formal employment accounted for approximately 50% of the workforce throughout the period under analysis (Firpo and Portella, 2024).

[10] For further details on RAIS, see https://www.gov.br/pt-br/servicos/entregar-a-relacao-anual-de-informacoes-sociais

[11] This is also the aggregation labor inspectors use when analyzing if the firm complies with the Quota Law.

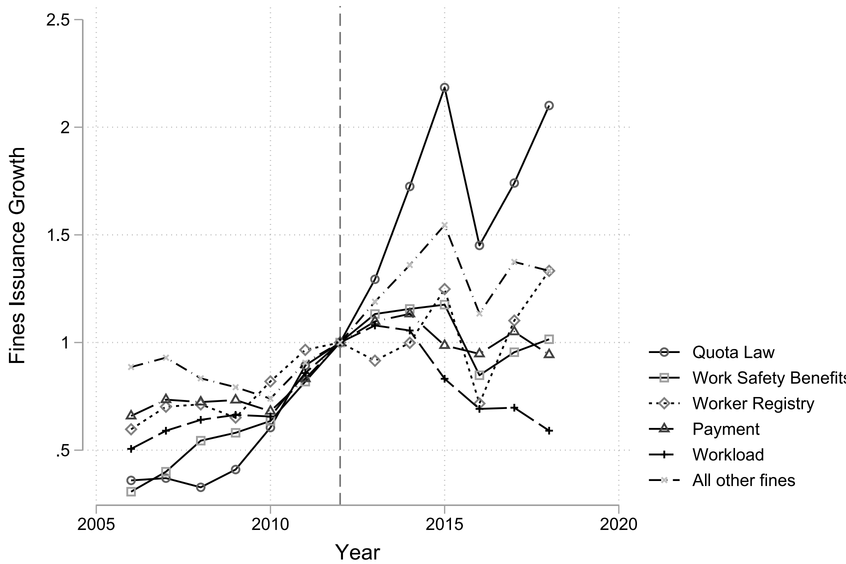


Fig. 1. Relative change in fine issuance across different labor regulations.
Note: This figure presents the relative change in the number of fines issued across different labor regulations, using 2012 as the baseline year.

To calculate firms' size (the number of employees in its payroll), we first restricted the contracts to those considered valid by the Quota Law in computing total employment — for instance, casual workers, apprentices, and independent contractors were dropped from the sample. Then, we summed the workers with an active contract on December 31st of each year, which is the most reliable information on workers' employment status.[12] To increase the tractability of our data, we dropped from our sample firms smaller than 20 workers since they are quite far from the Quota Law threshold of 100 workers.

Information about workers' disability status has been available in RAIS since 2003. However, due to modifications in the imputation method of such a variable, it becomes more reliable after 2007.[13] Hence, we restrict our analysis to the years between 2007 and 2018.

We measure the presence of workers with disabilities by the total number of such workers in the firm (restricting the contracts to those considered valid by the Quota Law). For each year and firm size group (greater or smaller than 100 workers), we excluded observations where the number of workers with a disability exceeded the 99th percentile, in order to remove outliers with exceptionally high numbers of workers with a disability. Due to the highly skewed nature of this variable and its large number of zeros (i.e., many firms do not hire workers with disabilities), we use its hyperbolic sine transformation throughout our analysis. We discuss potential issues with this measure and present robustness checks in Sections 4 and 5.[14]

While RAIS data does not provide information on the severity of a disability, it does indicate the type of disability. Specifically, it distinguishes whether a worker has a mobility, hearing, visual, or cognitive impairment.[15]

Importantly, the government does not directly use RAIS data to issue fines, which minimizes firms' incentives to artificially inflate their numbers of employees carrying a disability when inputting their data. First, the government needs RAIS information to be accurate in both producing useful statistics about the labor market and calculating and distributing financial benefits reserved for formal workers.[16] Consequently, it must ensure that answering RAIS' questionnaire is the most straightforward and the least consequential for firms. Second, issuing a fine due to non-compliance with any labor regulation is more complex than just checking the information provided by firms. It requires inspections, documentation analysis, and agreements involving deadlines for regularization. In the case of the Quota Law, for instance, labor inspectors need to verify the medical report of each person with disability employed by the firm, check if firms' facilities provide an adequate work environment for them, make agreements with non-compliant firms with a deadline to hire the missing persons with disability, among others. Hence, our measure of the presence of persons with disability in the firm is quite accurate.

The second data source we explore is information on labor inspections and fines due to non-compliance with labor regulations. These data come from the Ministry of Labor and bring three pieces of information that are key to this study: (a) the date when a firm was inspected, (b) whether a firm was fined and the date of the fine,[17] and (c) the reason why a firm is fined. We match these entries with RAIS

[12] When inputting workers' details on RAIS, firms include their hiring date and the date of lay-off (if any). Firms must also indicate whether workers' contracts were active by the last day of the year. While the precise dates of hiring and lay-off are subject to errors, the government enforces the accuracy of the information on whether the workers had an active contract by the end of the year.

[13] Before 2006, firms were required to report workers' disability status according to the codes (1 — yes; 2 — no). That year, the codes changed to (0 — The employee does not carry a disability; 1 — Mobility disability; 2 — Hearing disability; 3 — Seeing disability; 4 — Cognition disability; 5 — Multiple disabilities; 6 — Rehabilitated). In 2006, the number of people with hearing and mobility disabilities was considerably higher than in the rest of the historical series. Such a disparity was likely caused by the correspondence between the old codes (1 — yes; 2 — no) and the new codes (1 — Mobility; 2 — Hearing). For more details, see https://sit.trabalho.gov.br/radar/.

[14] While the transformations smooth out outliers, the robustness checks that use non-transformed data require that we eliminate outliers from the sample to improve precision.

[15] RAIS also includes information on workers with multiple disabilities or those classified as rehabilitated—individuals who have undergone a structured process of professional reeducation aimed at identifying their work potential and facilitating their reintegration into the labor market and community life. However, these cases are relatively uncommon, accounting for approximately 10% of workers with disabilities in the years we analyze.

[16] Such benefits are the Brazilian PIS/PASEP salary bonuses, a unique financial benefit provided by the federal government to certain employees, functioning similarly to a targeted economic stimulus.

[17] The date of fine does not necessarily coincide with the date of inspection since there might be some negotiation between the firm and the inspection authority before the fine is issued.

data to recover, for every year, information on the firm size, how many persons with a disability the firm was employing, whether the firm was inspected or fined, and the reason for the fine. We also use information on labor regulator offices location to perform heterogeneous exercises depending on the enforcement capacity level.[18]

Finally, we use data from Brazil's Federal Revenue Administration, which provides information on all business associates linked to a firm. We use this data to build our measure of owner network, decribed below. Since this database is continuously updated and past records are not publicly available, we rely on a cross-sectional snapshot for 2023, excluding firms that do not appear in RAIS during our analysis period (likely because they are newly established). While this prevents us from tracking changes over time, the stability of ownership structures in many firms suggests that the 2023 data reliably capture relevant business relationships.

For our exercise on enforcement spillovers, we define three types of networks through which information about a fine could have spread. The first, called the *neighbor network*, are firms located in the same zip code of a specific firm.[19] The second, called the *owner network*, are firms that either belong to the same owner of a specific firm or a business associate of the firm's owner. Finally, the third network, called the *HR workers network* is the other firms where human resources workers employed by a specific firm in t were working in $t-1$, $t-2$, or $t-3$.

Table 1 presents some descriptive statistics from our data. Panel A includes all the years we analyze, while panels B and C include the years before and after 2012. The table shows how the employment of persons with a disability is remarkably low. First, only 4% of firms with less than 100 workers employ at least one person with a disability. Comparing panels B and C, we conclude that this pattern did not change over time. Second, all firms larger than one hundred workers should employ more than one person with a disability. Still, only 46% of firms had at least one worker with a disability among their employees before 2012. After that year, 62% of firms had at least one worker with a disability but still only 16% of firms fully complied with the Quota Law.[20]

We can also see in the table how the enforcement of the QL increased after the 2012 Administrative Act: while only 3% of firms received a fine due to non-compliance with the QL before 2012, such a figure more than doubled after that year, averaging 7%. In the next section, we will show how the 2012 Administrative Act indeed had a causal impact on the number of persons with a disability hired by firms larger than 100 workers.

4. Impacts of the 2012 Administrative Act

We start our analysis by documenting how the Administrative Act of 2012, which introduced more stringent inspection procedures, impacted firms' hiring. Fig. 2 presents preliminary evidence of that impact. We exploit the Quota Law threshold of 100 workers and perform regression discontinuity estimations before and after 2012 to compute whether surpassing the QL threshold increased the hiring of persons with disability in these two periods.[21] The figure shows no significant discontinuity in the hiring of persons with disability for firms surpassing the QL threshold before 2012, but we see a clear discontinuity after that year. Table A.1 in the Appendix presents RDD estimations such as the ones in Fig. 2, and shows that, after 2012, firms larger than the QL threshold hire about 6% more workers carrying a disability compared to firms smaller than such a threshold.[22]

To understand the magnitude of the changes in the impact of the QL on firms' hiring behavior, we employ a difference in discontinuity design to estimate how the number of persons with disability increased in firms subjected to the Quota Law after the government introduced the new inspection procedures in 2012. This identification strategy was introduced by Grembi et al. (2016), who combined the traditional RD design with ideas from the difference-in-differences design. The authors added a second dimension to the regression discontinuity (RD) design, where a structural change happened between two different periods while a discontinuity holds during both periods. In our case, such a change is the introduction of the new inspection procedures in 2012, while the thresholds defined by the Quota Law remained the same during the whole period. The main idea of the differences-in-discontinuities design is that it takes the difference between the pre-2012 and post-2012 discontinuities in the Quota Law threshold to separate the effect of the Quota Law from the effect of enforcing such a policy through the new inspection procedures.

We identify the effect of introducing the new inspection procedures through the following equation (estimated within the bandwidth proposed by Calonico et al. (2014a,b))[23]:

$$Y_{imt} = \beta_0 + \beta_1 P_{imt} + \beta_2 S_{imt}(\gamma_0 + \gamma_1 P_{imt}) + T[\alpha_0 + \alpha_1 P_{imt} + S_{imt}(\delta_0 + \delta_1 P_{imt})] + \theta_m + \theta_t + \theta_{mt} + \varepsilon_{imt}. \tag{1}$$

Where, Y_{imt} is the outcome of firm i located at municipality m at year t, $S_{imt} = 1(\text{Firm Size} \geq 100)$, and $P_{imt} = \text{Firm Size} - 100$. Moreover, $T = 1(t \geq 2012)$. Our coefficient of interest in Eq. (1) is δ_0. It identifies the impact of surpassing the cut-off threshold after 2012. The estimation includes controls for municipal and year fixed effects (θ_m and θ_t, respectively) and interactions between these two (θ_{mt}). The inclusion of municipality-by-year fixed effects in our estimations addresses two identification threats. First, it ensures that the supply of persons with disability is held constant across municipalities and time. This alleviates concerns that the new inspection procedures might have changed not only firms' demand for workers with disability but also the willingness of persons with disability to look for jobs. Second, it controls for time-specific stringency of labor inspections — for instance, new openings of

[18] We gathered these data from the replication package of Ponczek and Ulyssea (2022).

[19] The Brazilian Zip Code (CEP) is an eight-digit code used to identify regions, municipalities, neighborhoods, and specific streets. The first five digits represent the geographic area, while the last three digits pinpoint a particular address or location.

[20] An important caveat on the measurement of full compliance with the Quota Law is that this variable is subject to considerable measurement error. Since workers are observed only at the end of the year, both firm size and the number of employees with disabilities are noisy measures. As these variables determine the exact number of workers with disabilities a firm should hire, our measure of compliance remains imprecise throughout the year.

[21] We employ the bandwidth selection procedure developed by Calonico et al. (2014a,b). Our running variable, namely firm size, is subject to measurement errors since we observe it only at the end of each year. Hence, we employ a donut ring strategy (see Barreca et al., 2011, for detail), where we exclude firms within two workers from the QL thresholds. Table A.1 and Fig. A.1 in the Appendix present, respectively, RDD estimations before and after 2012 employing different bandwidths selection procedures, and manipulation tests at the 100 threshold.

[22] Fig. 2 shows a difference in both the level and slope of the number of workers with disabilities for firms below the 100-worker threshold. Firm size fluctuates from year to year, and around 11% of firms with more than 100 workers in year $t+1$ had fewer than 100 workers in year t. Before 2012, the hiring of workers with disabilities was low even among firms above the threshold, which explains the flat slope before that year. After 2012, as hiring workers with disabilities became the norm for large firms, the slope to the left of the cut-off increased along with the intercept. This pattern is consistent with the interpretation that enforcement after 2012 had a direct effect on the hiring of workers with disabilities. Additionally, this explains why the jump at the cut-off appears relatively small: firms that fluctuate around the threshold are unlikely to fire workers with disabilities simply because they temporarily fall below it.

[23] Table A.2 in the Appendix presents descriptive statistics for the sample within the bandwidth calculated for the hyperbolic sine transformation of the number of workers with disability

Table 1
Descriptive statistics.

	Firm size: <100	Firm size: ≥100
Panel A: all years (2007–2018)		
N (firm × year)	2,316,302 (83.53%)	456,662(16.47%)
Firm size	38.47 (18.87)	497.29 (1644.09)
At least one worker w/ disability	0.04 (0.21)	0.55 (0.50)
Workers w/ disability (%)	0.00 (0.03)	0.01 (0.04)
Comply with QL	. (.)	0.12 (0.33)
Fine due to non-compliance with QL	0.00 (0.02)	0.05 (0.22)
Some fine	0.06 (0.24)	0.27 (0.44)
Inspection	0.21 (0.41)	0.63 (0.48)
Panel B: Before 2012		
N (firm × year)	967,589 (83.01%)	198,098 (16.99%)
Firm size	38.69 (19.00)	487.04 (1587.39)
At least one worker w/ disability	0.04 (0.20)	0.46 (0.50)
Workers w/ disability (%)	0.00 (0.04)	0.01 (0.06)
Comply with QL	. (.)	0.07 (0.26)
Fine due to non-compliance with QL	0.00 (0.01)	0.03 (0.16)
Some fine	0.06 (0.24)	0.24 (0.42)
Inspection	0.25 (0.43)	0.63 (0.48)
Panel C: After 2012		
N (firm × year)	1,348,713 (83.91%)	258,564 (16.09%)
Firm size	38.31 (18.78)	505.14 (1686.21)
At least one worker w/ disability	0.05 (0.21)	0.62 (0.48)
Workers w/ disability (%)	0.00 (0.02)	0.01 (0.03)
Comply with QL	. (.)	0.16 (0.37)
Fine due to non-compliance with QL	0.00 (0.03)	0.07 (0.26)
Some fine	0.07 (0.25)	0.29 (0.46)
Inspection	0.18 (0.38)	0.63 (0.48)

Note: "Firm size" is the average number of workers with an active contract in the firm in the period considered; Firms smaller than 20 workers are not considered in our analysis; "At least one worker w/ disability" is the average number of firms with at least one person with a disability with an active contract in the period considered; "Comply with QL" is the average number of firms with the minimum number of workers with disabilities established by the QL in the period considered; "Fine due to non-compliance with QL", "Some fine", and "Inspection" is the average number of firms, in the period considered, that received a fine due to non-compliance with the QL, received some fine, and were inspected, respectively.

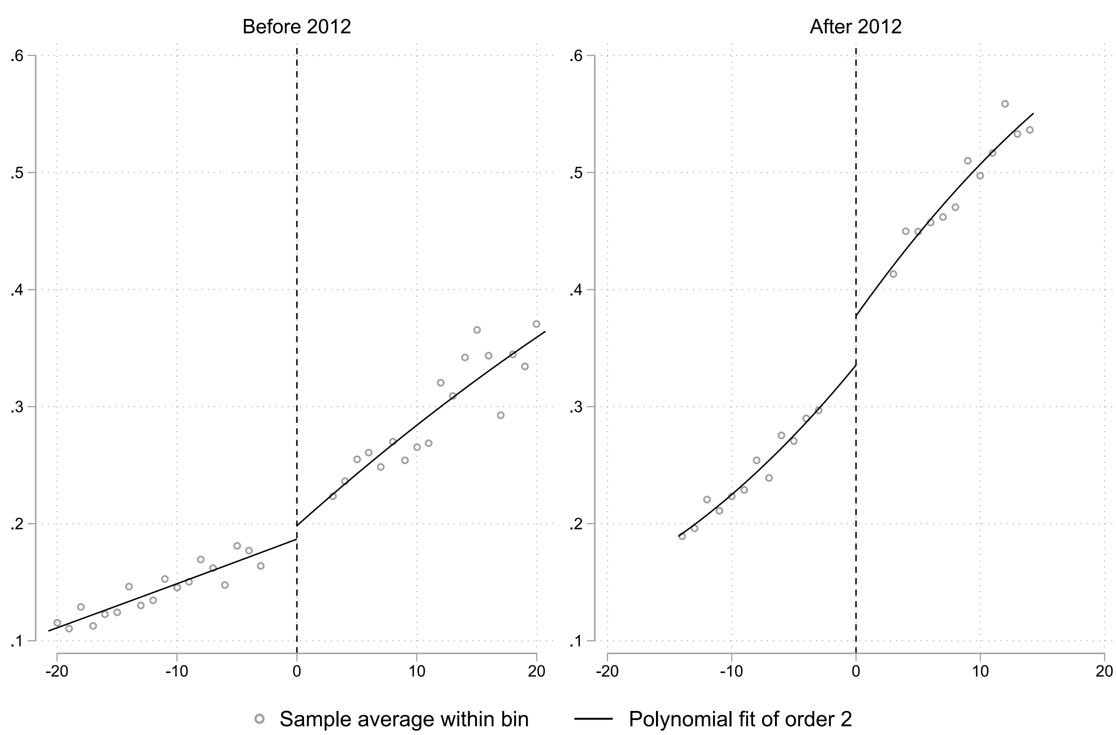


Fig. 2. Regression discontinuity Quota Law threshold.
Note: This figure shows results from local polynomial regressions where we estimate firms' hiring behavior regarding workers with disabilities once they pass the 100 workers threshold established by the Quota Law (see Cattaneo et al., 2019; Calonico et al., 2014a,b, for details on our RDD estimation). The dependent variable is the hyperbolic sine transformation of the number of workers carrying a disability. We employ the bandwidth selection algorithm developed by Calonico et al. (2014a,b). In particular, we adopt the mean squared error (MSE)-optimal bandwidth selector for the sum of regression estimates. Due to measurement errors in the estimation of the firm's size, we exclude firms within a donut ring of size two from the 100 threshold.

labor inspection offices. Standard errors are clustered at the city level to account for the correlation among firms with headquarters in the same city.

Three assumptions need to hold for identification. First, as in the traditional RD design, all potential outcomes should be continuous around the discontinuity each year. Second, similar to the difference-in-differences design, the observations just below and above the discontinuity must follow (local) parallel trends in the counterfactual scenario of no new inspection procedures. Third, the effects of the new inspection procedures should be independent of the thresholds of the Quota Law.

A further concern in our analysis is measurement error in firms' size since our variables are aggregated by year. To address this issue, we implement a donut ring strategy (Barreca et al., 2011), where we exclude firms with sizes within one or two workers from the 100 thresholds. Through such exclusion, we avoid inclusion and exclusion errors where we wrongly consider a firm larger or smaller than the QL threshold. The donut strategy also helps in addressing the issue of potentially endogenous manipulation of the running variable around the threshold, even though Fig. A.1 in the Appendix shows that manipulation is not a concern in our analysis.

Table 2 presents the impact of the 2012 Administrative Act on law enforcement–measured by the likelihood of receiving an inspection (columns (1) and (2)) and of being fined due to non-compliance with the Quota Law (columns (3) and (4))[24] — as well as on the number of workers with disabilities present in the firm (columns (5) and (6)). Columns (1), (3), and (5) present estimations using a donut ring of one, while columns (2), (4), and (6) provide robustness checks implementing a donut ring of two in the firm-size variable.

At the firm-size margins where causal inference is possible, the introduction of the 2012 Administrative Act increased the likelihood of inspection by 2 percentage points, representing a 4.5% increase. The increase in the likelihood of receiving a Quota Law fine, shown in column (3), is even more striking: after the 2012 Administrative Act, this probability rose by 1.3 percentage points, or 54.2%. Finally, the results in column (5) indicate that firms reacted to the increased QL enforcement by hiring more persons with disabilities. Specifically, firms just above the 100-worker threshold increased their number of workers with a disability by 6.4%.[25] The results remain consistent regardless of the donut ring implemented.

Fig. 3 presents the dynamic results of such estimations, where we substitute the binary variable T in Eq. (1) for yearly dummies. The figure shows no difference between firms larger and smaller than 100 workers before 2012. However, we observe an increase in inspections, Quota Law fines, and the number of workers with disabilities for firms larger than 100 workers after that year.[26]

Recent work by Chen and Roth (2024) shows that one should be careful when interpreting results with log or inverse hyperbolic sine transformations, especially if the treatment affects the extensive margins — in our case, the likelihood of a firm passing from having no workers with a disability to having one or more. We present in Table A.3 and Fig. A.3 two robustness checks to deal with this issue. First, we present extensive margin estimations, calculating the likelihood that firms have at least one employee with a disability. Second, we estimate a linear regression model with the number of workers with disability as the dependent variable. Results are the same for the extensive margin estimation and point in the same direction as the main estimation in the regression model with the count variable, even though we do not have enough power to reject the hypothesis of null effects in the last case.

One concern regarding the increase in firms' reported number of workers with disabilities is whether this reflects a real rise in employment or if firms are misclassifying workers without disabilities to meet Quota Law requirements. However, several factors suggest this is unlikely. First, as explained in Section 3, firms have little incentive to misreport data in RAIS, as labor inspections do not rely on it for enforcement. Second, falsifying a worker's disability status would require substantial coordination, including (i) a doctor willing to issue a false disability certificate, (ii) a worker willing to falsely declare a disability and sign an official document confirming their status, and (iii) firm employees either being unaware of or complicit in the fraud. Third, we directly assess in our data whether the post-2012 increase in reported workers with disabilities stems primarily from the reclassification of existing employees (Duryea et al., 2024). Some reclassification is expected even without fraud, as firms may have previously employed workers with disabilities without formally counting them toward Quota Law requirements. However, if the increase primarily reflects new hires rather than reclassification, it would suggest that the policy has effectively generated new employment opportunities for persons with disabilities.

Table A.4 in the Appendix supports this view. Columns (1) and (2) show that roughly one-third of the increase of persons with disabilities comes from the reclassification of existing workers within the firm after the 2012 Act.[27] Columns (3) and (4) find no increase in hiring of workers from other firms who had not been previously classified as having a disability in their past jobs. This suggests that reclassification applies only to employees already working at the firm, likely due to firms paying closer attention to worker classification. Most importantly, Columns (5) and (6) show that our results hold even after excluding reclassified cases from the analysis. This confirms that the majority of the increase in employment of workers with disabilities comes from new hires whose classification has remained unchanged.

We also investigate whether introducing the new inspection mechanisms led to changes in fines unrelated to the QL (non-QL fines) or to outcomes potentially related to firms' profitability. Table A.5 and Fig. A.4 in the Appendix show the result of that analysis. We test for the effects of the new inspection mechanisms on non-QL fines as a placebo exercise since the new administrative act should not interfere with the enforcement of other labor regulations. Indeed, we do not observe any positive impact on the likelihood of receiving fines not related to the Quota Law around the thresholds, which supports our hypothesis that the observed impact for inspections and QL fines is due to the introduction of the 2012 Administrative Act and not to an overall increase in enforcement of labor regulations after 2012. Furthermore, we analyze the impact of higher enforcement on firm closure (we proxied for closure by looking at whether a firm present in the data at time t is not found at $t+1$), firm total wage bill, and turnover rate.[28] We find no impact on these outcomes suggesting that the enforcement did not lead to major profitability issues. These results are in line with evidence on the enforcement of labor regulation in the United States (Levine et al., 2012) and in the Brazilian context (Szerman,

[24] The likelihood of receiving a QL fine is zero for firms smaller than 100 workers since they do not need to comply with the law. Hence, the results in columns (3) and (4) show the difference across time in the likelihood that firms larger than 100 will receive such a fine.

[25] As explained in Section 3, we use the hyperbolic sine transformation of the number of workers with a disability. We include in Table 2 the elasticity of the number of workers with disabilities, using the calculation derived by Bellemare and Wichman (2020).

[26] We also examine whether the 2012 Act increased the likelihood of firms complying with the Quota Law, i.e., hiring the minimum number of workers required by law. The results are presented in Fig. A.2 in the Appendix. However, as we explain in Section 3, this exercise should be taken with a grain of salt since there is a great deal of measurement error in the variable measuring full compliance with the Quota Law.

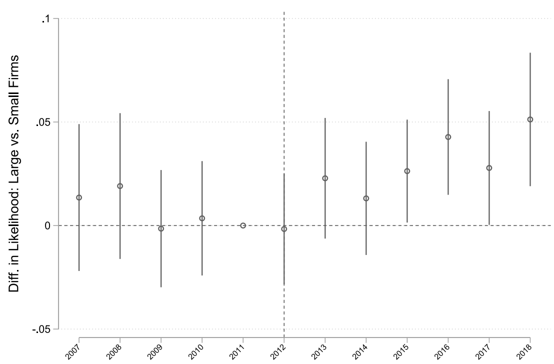
[27] This figure is in line with other contributions measuring reclassification, such as Duryea et al. (2024).

[28] We use separation rate, defined as the number of workers that left the firm at time t divided by all spells present in the firm at t as our measure of turnover rate (Pries and Rogerson, 2022).

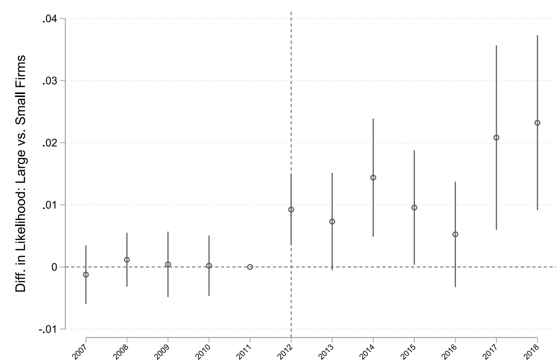
Table 2
The 2012 Administrative Act , law enforcement, and workers with disabilities.

	Inspection		QL fine		Workers w/ disab. (hyp. sine trans.)	
	(1)	(2)	(3)	(4)	(5)	(6)
Year>2012 × Dist . QL threshold>0	0.020***	0.019***	0.013***	0.013***	0.062***	0.068***
	(0.006)	(0.007)	(0.003)	(0.002)	(0.016)	(0.018)
N	446 497	437 528	331 122	322 136	222 233	213 288
Mean Dep. Var.	0.440	0.441	0.024	0.024	0.841	0.855
Elasticity					0.064	0.070
h (left)	33.305	33.305	19.665	19.665	13.910	13.910
h (right)	118.828	118.828	127.249	127.249	66.489	66.489
R2	0.176	0.177	0.106	0.107	0.208	0.210
Donut ring	1	2	1	2	1	2

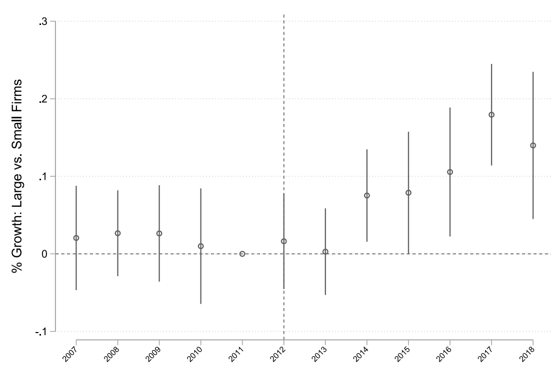
Note: This table presents estimations from Eq. (1). Elasticities of workers with disabilities are calculated based on Bellemare and Wichman (2020). All estimations include city-by-year fixed effects. Significance levels are indicated by ∗ < .1, ** < .05, *** < .01. Standard errors clustered at the city level shown in parentheses.



(a) Inspection



(b) QL fines



(c) Workers with disability

Fig. 3. The 2012 Administrative Act, law enforcement, and workers with disabilities.
Note: These graphs present estimations from a model similar to Eq. (1), where we substitute the post-2012 dummy T with year dummies, leaving 2011 as the benchmark. All estimations include city-by-year fixed effects. 99% confidence interval shown in the graphs.

2022), which also does not find that hiring people with disabilities had negative impacts on firms or workers without disabilities.[29]

Next, we examine key outcomes for workers with disabilities that may have been influenced by the increase in Quota Law compliance. Specifically, we ask: (i) which types of disabilities benefited the most from the 2012 Act, and (ii) how the Act affected job tenure and the likelihood of securing formal employment for persons with disabilities. Additionally, for those already in the formal labor market, we assess whether the Act increased their likelihood of being poached by large firms.

Table 3 addresses the first question by examining how the 2012 Administrative Act influenced the hiring of workers with different types of disabilities. The table presents results for mobility impairments (Column 1), hearing impairments (Column 2), visual impairments (Column 3), and cognitive impairments (Column 4). The findings indicate that the overall increase in the hiring of persons with disabilities was primarily driven by workers with mobility impairments, followed by those with visual and cognitive impairments, while no significant effect was observed for individuals with hearing impairments. Notably, these results do not align with the distribution of disabilities in the

[29] de Souza (2023), in contrast, finds negative effects from the Quota Law enforcement for the employment and wages of workers not carrying a disability in Brazilian firms. A possible reason for such a difference is that we exploit an institutional change, while de Souza (2023) exploits the timing of firm inspections. Receiving an inspection, however, might lead firms to rush into hiring persons with disability, which might decrease their productivity, at least momentarily.

Table 3
The 2012 act and employment by disability type.

	(1) Mobility	(2) Hearing	(3) Seeing	(4) Cognition
Year>2012 × Distance to QL threshold>0	0.044***	0.001	0.017***	0.012***
	(0.010)	(0.010)	(0.006)	(0.003)
N	281 881	256 376	305 474	328 703
Mean Dep. Var.	1.060	0.147	0.081	0.055
Elasticity	0.045	0.001	0.018	0.012
h (left)	17.843	14.616	20.339	20.693
h (right)	99.733	94.617	101.315	126.740
R2	0.928	0.117	0.115	0.106

Note: This table presents estimations from Eq. (1). All dependent variables measure the number of workers in each category and have been transformed using the hyperbolic sine function. Elasticities of workers with disabilities are calculated based on Bellemare and Wichman (2020). All estimations include city-by-year fixed effects. Significance levels are indicated by * < .1, ** < .05, *** < .01. Standard errors clustered at the city level shown in parentheses.

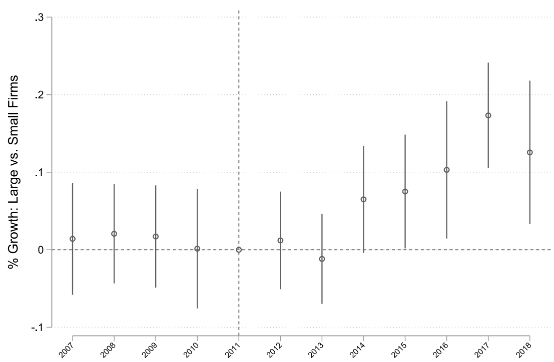


Fig. 4. The 2012 Administrative Act and workers with disabilities – sub-sample: firms that never received a QL fine.
Note: These graph presents estimation from a model similar to Eq. (1), where we substitute the post-2012 dummy T with year dummies, leaving 2011 as the benchmark. We keep in our estimation the sub-sample of firms that never received a QL fine. The estimation includes city-by-year fixed effects. 99% confidence interval shown in the graphs.

Brazilian population. Approximately 15% of Brazilians have a visual impairment, 5% a hearing impairment, 5% a mobility impairment, and fewer than 3% a cognitive impairment (Berlinski et al., 2021). The disproportionate increase in hiring across disability types suggests that firms may prioritize certain groups over others, potentially due to differences in training requirements, workplace accommodation costs, or perceived ease of integration into the workforce.

Table 4 examines the second question by analyzing various job-related outcomes for workers with disabilities. Specifically, we assess whether the 2012 Administrative Act influenced job stability, measured by the number of workers with a disability remaining with the same employer for at least three years (Column 1). For newly hired workers (with less than three years of tenure), we analyze their prior employment status to determine whether firms were recruiting individuals who were previously outside the formal labor market (Column 2), employed in small firms (Column 3), or employed in large firms (Column 4). Importantly, the dependent variable in each specification represents the number of such workers in a given firm, meaning it can take a value of zero if no workers with disabilities fall into that category. The findings reveal an improvement in job stability, as the number of workers remaining with the same firm for at least three years increased. Additionally, while we observe some poaching from both small and large firms, the majority of new hires came from outside the formal labor market. These results suggest that rather than simply redistributing existing workers across firms, the Act played a crucial role in bringing more individuals with disabilities into formal employment and improving their job security.

Overall, these findings demonstrate that the 2012 Administrative Act significantly strengthened the enforcement of the Quota Law, increasing inspection rates and fines for non-compliance. In response, firms expanded their hiring of individuals with disabilities, with those with mobility impairments benefiting the most. Importantly, this policy shift did not negatively affect firms' outcomes such as closure rates, worker turnover, or total wage bills. Finally, the job stability of workers with disabilities improved, and more individuals with disabilities entered the formal labor market. These results underscore the Act's effectiveness in enhancing compliance with the Quota Law and improving labor market outcomes for workers with a disability without imposing undue burdens on businesses.

4.1. Compliance in the absence of direct enforcement

Enforcement spillovers are a powerful tool in achieving general compliance due to the spread of information about law enforcement to agents not directly impacted by it. We next present evidence that firms increased their compliance with the QL after the 2012 Administrative Act, even if located in places with lower enforcement capacity or in cases where they never received a QL fine themselves. In the next section, we directly investigate the emergence of enforcement spillovers.

Previous research on labor regulations highlights the significant impact of enforcement capacity on compliance (e.g., Almeida and Carneiro, 2012; Ponczek and Ulyssea, 2022). However, our analysis reveals that while the issuance of QL fines decreases with reduced enforcement capacity, this does not hold for the hiring of workers with disabilities. In particular, we estimate a model that includes interactions between the variable *Absence of LO* and all elements of Eq. (1). We define *Absence of LO* as a binary variable indicating that there is no labor office in the municipality where firm i is located (i.e., the distance between municipality m where firm i is located and the nearest labor office is greater than zero). The results are presented in Table 5. The impact of the 2012 Administrative Act on inspections does not change depending on the firm's distance to the nearest labor office (see columns (1) and (2)). However, the issuance of fines decreases considerably in places with no labor office (see columns (3) and (4)).

Even with such a decrease in the issuing of fines in the absence of labor offices, the impact of the 2012 Administrative Act on the hiring of persons with a disability is constant across localities, regardless of whether labor offices are present in their municipality or not (see columns (5) and (6)).

Moreover, even in places where enforcement capacity is higher, the number of QL fines issued is relatively small: after 2012, only 8.4% of firms larger than 100 workers located in municipalities with the presence of labor regulatory offices (i.e., closer to the regulation enforcers) were fined due to non-compliance with the QL, even though compliance was only near 20% for these firms during that period. However, even firms that never received a QL fine reacted to the 2012

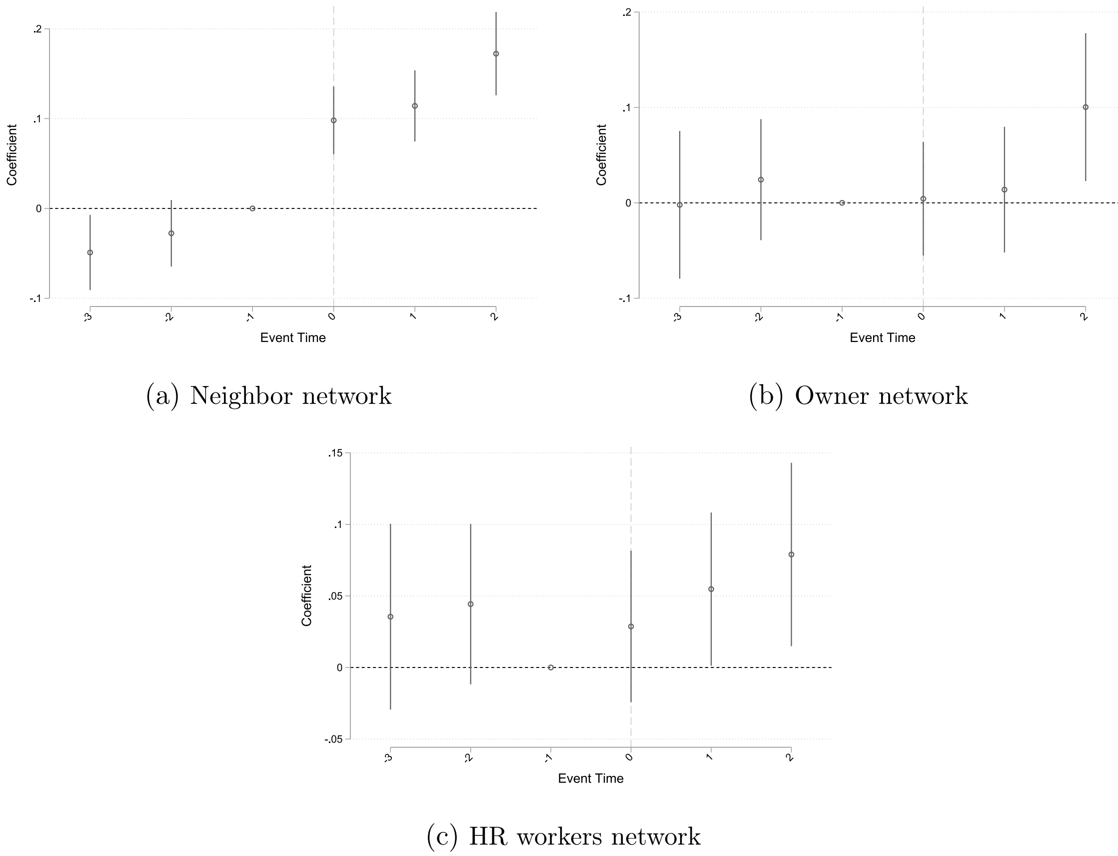


Fig. 5. Law enforcement at firm networks and the number of workers with a disability.
Note: These graphs present estimations from Eq. (2). The dependent variable is the hyperbolic sine transformation of the number of workers with a disability in the firm. The sample comprises firms larger than 100 workers, which did not receive a QL fine in $t = 0$. "Event Time" is the time after the occurrence of the QL fine in the firm's network. All estimations include firm-by-cohort and city-by-time-to-event fixed effects. 99% confidence interval shown in the graphs.

Table 4
The 2012 act and labor market outcomes for workers with disabilities.

		Working at the firm for less than 3 years		
	(1) 3+ yrs in firm	(2) Out of LM before firm	(3) Prev. job: small firm	(4) Prev. job: large firm
Year>2012=1 × Distance to QL threshold>0=1	0.028***	0.031**	0.008***	0.010**
	(0.009)	(0.013)	(0.003)	(0.004)
N	331 946	229 137	448 227	304 372
Mean Dep. Var.	0.319	0.317	0.055	0.117
Elasticity	0.029	0.032	0.008	0.010
h (left)	23.026	13.906	30.131	20.039
h (right)	104.014	77.432	177.368	100.125
R2	0.145	0.152	0.074	0.096

Note: This table presents estimations from Eq. (1). All dependent variables measure the number of workers in each category and have been transformed using the hyperbolic sine function. Elasticities of workers with disabilities are calculated based on Bellemare and Wichman (2020). All estimations include city-by-year fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city level shown in parentheses.

Administrative Act by increasing their hiring of people with disabilities. We show this in Fig. 4, where we reproduce the estimations shown in Fig. 3 restricting our sample to firms that never received a QL fine. As we can see, the impact of the 2012 Administrative Act in this subsample is remarkably similar to the one considering the whole sample of firms.

5. Enforcement spillovers

Having established that introducing new enforcement mechanisms effectively changed firms' behavior regarding hiring persons with a disability, we now focus on the spillover effects stemming from such an increase in the stringency of law enforcement. If firms learn from their networks about the increase in the likelihood of being punished

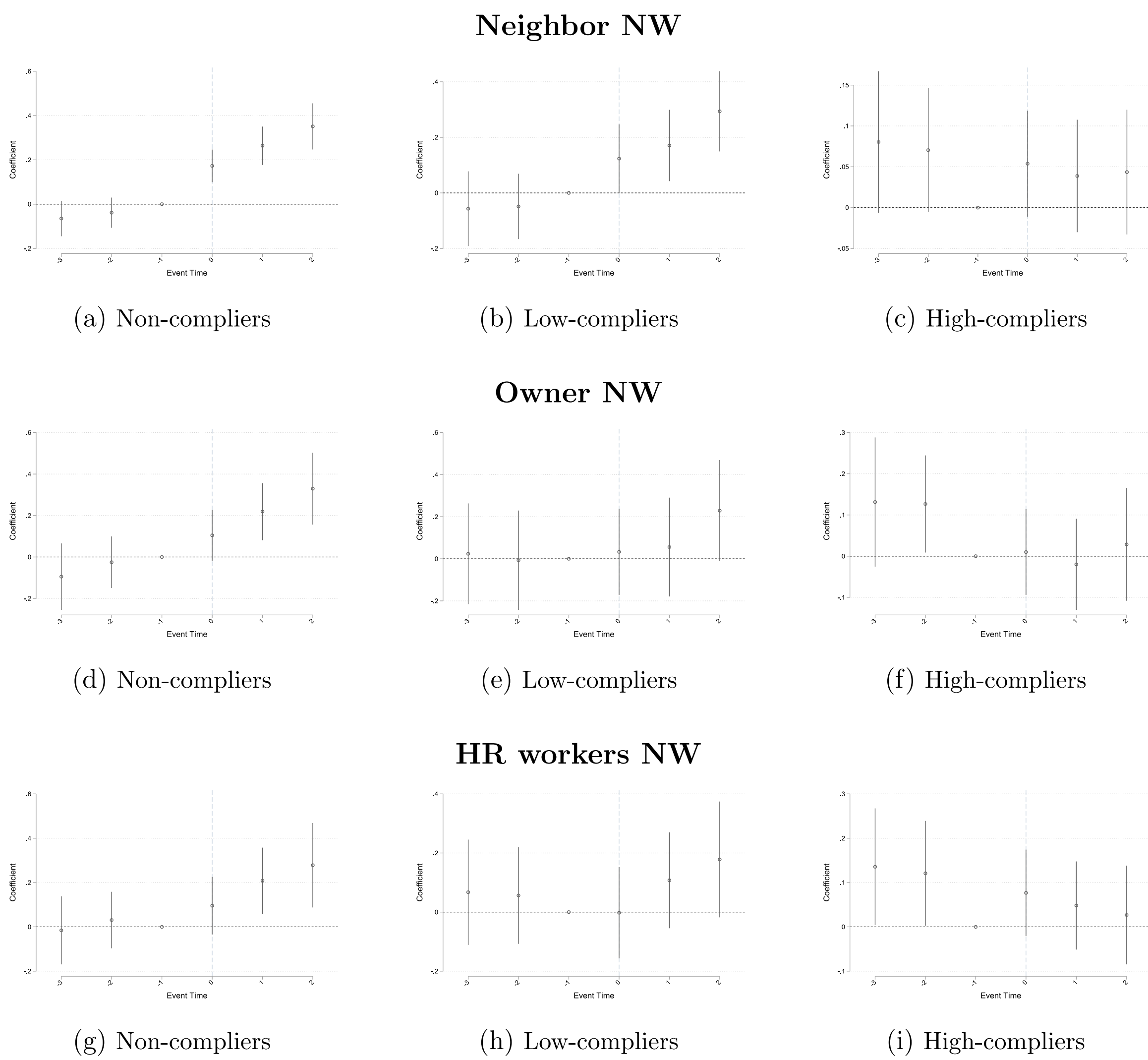
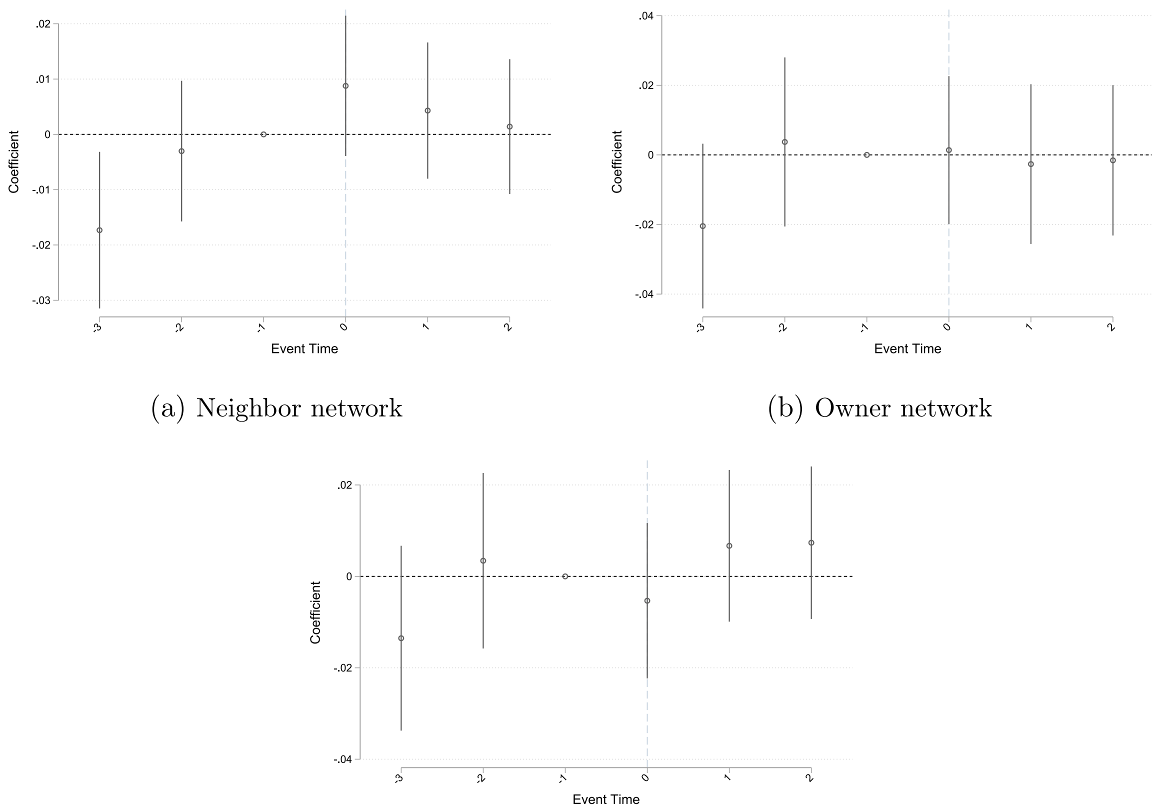


Fig. 6. Heterogeneity by compliance with the QL in $t-1$.
Note: These graphs present estimations from Eq. (2). The dependent variable is the hyperbolic sine transformation of the number of workers with a disability in the firm. The sample comprises firms larger than 100 workers, which did not receive a QL fine in $t=0$. "Non-compliers" are firms that did not employ any workers with disabilities; "Low-compliers" are firms employing some workers with disabilities but less than 50% of the required quota; "High-compliers" are firms meeting at least 50% of the required quota. "Event Time" is the time after the occurrence of the QL fine in the firm's network. All estimations include firm-by-cohort and city-by-time-to-event fixed effects. 99% confidence interval shown in the graphs.

Table 5
Heterogeneous results by enforcement capacity level.

	Inspection		QL fine		Workers w/ disab. (hyp. sine trans.)	
	(1)	(2)	(3)	(4)	(5)	(6)
Year>2012 × Dist . QL threshold>0	0.014*	0.012	0.016***	0.015***	0.062***	0.063***
	(0.008)	(0.009)	(0.003)	(0.003)	(0.017)	(0.020)
Year>2012 × Distance to QL threshold>0 X Absence of LO	0.014	0.017	−0.007**	−0.006*	0.001	0.012
	(0.013)	(0.013)	(0.004)	(0.004)	(0.029)	(0.032)
N	432 081	423 407	320 222	311 529	214 932	206 282
Mean Dep. Var.	0.437	0.438	0.024	0.024	0.465	0.472
h (left)	33.305	33.305	19.665	19.665	13.910	13.910
h (right)	118.828	118.828	127.249	127.249	66.489	66.489
R2	0.175	0.176	0.107	0.108	0.209	0.211
Donut ring	1	2	1	2	1	2

Note: This table presents estimations from Eq. (1). All estimations include city-by-year fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city level shown in parentheses.



(a) Neighbor network

(b) Owner network

(c) HR workers network

Fig. 7. Law enforcement at firm networks and inspection likelihood.
Note: These graphs present estimations from Eq. (2). The dependent variable is a binary variable indicating whether the firm was inspected. The sample comprises firms larger than 100 workers, which did not receive a QL fine in $t = 0$. "Event Time" is the time after the occurrence of the QL fine in the firm's network. All estimations include firm-by-cohort and city-by-time-to-event fixed effects. 99% confidence interval shown in the graphs.

due to non-compliance with the Quota Law, they might change their behavior, even if not directly exposed to law enforcement.

We explore the timing of Quota Law fines and investigate how a firm i reacts when another firm in its network receives such a fine. We look at three different networks from which firm i could learn about the Quota Law enforcement: (i) *neighbor network*, defined as the firms located in the same zip code as firm i; (ii) *owner network* , defined as the firms that belong to the same owner of firm i or firms that belong to a business associate of such an owner;[30] and (iii) *HR workers network*, defined as the firms where human resources workers working for firm i at time t were working up to three years before t.

We implement an event-study methodology where we analyze trends in the presence of workers with a disability before and after the occurrence of a QL fine in a firm's network. We use as control group firms that belong to networks that will receive a QL fine in the future (Deshpande and Li, 2019; Fadlon and Nielsen, 2020). The main identification assumption behind the choice of such a control group is that, while receiving a QL fine might be endogenous to a network, the timing of such a fine can be considered exogenous. Table A.7 in the Appendix shows that the occurrence of a fine in firms' networks already seems quite exogenous, even if we compare them with networks that have never received a fine. Overall, firms' previous characteristics, such as their size or their number of workers with a disability, are not able to predict the occurrence of a QL fine in their network. However, the exogeneity is even more evident when we restrict the comparison group to firms whose networks received a QL fine in the future: the little predictive power that we observe in the even columns of Table A.7 (i.e., the estimation that included never-treated networks) usually vanishes in the subsequent estimations (odd-columns) when we exclude from the estimation firms belonging to networks that have never received a QL fine.

We construct our estimation sample of firms in four steps.[31] First, we take the networks where the QL fine happened at any time after 2012.[32] For instance, to look at the impact of a QL fine in the neighbor network, this means restricting the sample to zip codes where any QL fine happened between 2012 and 2018. The same idea applies to the owner network and the HR workers network. Over this period, some

[30] We define business associates as individuals who share the ownership of a firm.

[31] Table A.6 in the Appendix presents descriptive statistics within the samples we construct.

[32] We focus on the period after implementing the new inspection procedures since the number of fines increased considerably after it.

Table 6
Law enforcement at firm's networks and the number of workers with a disability.

	All firms (1)	Non-compliers (2)	Low-compliers (3)	High-compliers (4)
Panel A: Neighbor network				
Post-event × Treated	0.123***	0.267***	0.217***	−0.010
	(0.015)	(0.026)	(0.038)	(0.020)
N	67 145	25 715	10 422	25 873
N (firms)	10 430	4481	1488	4861
Avg. firm size	383	248	873	328
Mean Dep. Var.	5.538	0.718	9.010	9.283
Elasticity	0.131	0.307	0.243	−0.010
R2	0.856	0.602	0.821	0.892
Panel B: Owner network				
Post-event × Treated	0.045*	0.251***	0.120*	−0.054
	(0.026)	(0.045)	(0.069)	(0.036)
N	24 047	7550	4275	9426
N (firms)	3431	1275	585	1589
Avg. firm size	498	303	917	477
Mean Dep. Var.	8.638	0.939	8.572	15.423
Elasticity	0.046	0.286	0.128	−0.053
R2	0.871	0.633	0.783	0.905
Panel C: HR workers network				
Post-event × Treated	0.040**	0.205***	0.112**	−0.042
	(0.020)	(0.045)	(0.048)	(0.029)
N	25 220	7235	5721	9887
N (firms)	3374	1184	766	1547
Avg. firm size	561	324	1064	457
Mean Dep. Var.	8.769	1.075	10.191	14.005
Elasticity	0.041	0.228	0.119	−0.041
R2	0.867	0.644	0.811	0.898

Note: This table presents estimations from a model similar to Eq. (2), where we substitute the time dummies with a post-event dummy. The sample is composed of firms larger than 100 workers that did not receive an inspection or any type of fine in $t = -1$. "Treated" is an indicator that some type of law enforcement happened in the firm owner's network at $t = 0$. Elasticities of workers with disabilities are calculated based on (Bellemare and Wichman, 2020). All estimations include firm-by-cohort and city-by-time-to-event fixed effects. Significance levels are indicated by * < .1, ** < .05, *** < .01. Standard errors clustered at the city-by-cohort-by-event-time level shown in parentheses.

Table 7
Law enforcement at firm's networks and inspection likelihood.

	All firms (1)	Non-compliers (2)	Low-compliers (3)	High-compliers (4)
Panel A: Neighbor network				
Post-event × Treated	0.004	0.007	0.006	0.004
	(0.004)	(0.008)	(0.007)	(0.006)
N	67 145	25 715	10 422	25 873
N (firms)	10 430	4481	1488	4861
Avg. firm size	383	248	873	328
Mean Dep. Var.	5.538	0.718	9.010	9.283
Elasticity	0.004	0.007	0.006	0.004
R2	0.322	0.323	0.333	0.389
Panel B: Owner network				
Post-event × Treated	0.001	−0.029*	−0.002	0.024**
	(0.007)	(0.015)	(0.012)	(0.010)
N	24 047	7550	4275	9426
N (firms)	3431	1275	585	1589
Avg. firm size	498	303	917	477
Mean Dep. Var.	8.638	0.939	8.572	15.423
Elasticity	0.001	−0.029	−0.002	0.025
R2	0.342	0.344	0.347	0.393
Panel C: HR workers network				
Post-event × Treated	0.011**	−0.008	0.018**	0.008
	(0.005)	(0.014)	(0.009)	(0.008)
N	25 220	7235	5721	9887
N (firms)	3374	1184	766	1547
Avg. firm size	561	324	1064	457
Mean Dep. Var.	8.769	1.075	10.191	14.005
Elasticity	0.011	−0.008	0.018	0.008
R2	0.319	0.325	0.353	0.371

Note: This table presents estimations from a model similar to Eq. (2), where we substitute the time dummies with a post-event dummy. The sample is composed of firms larger than 100 workers that did not receive an inspection or any type of fine in $t = -1$. "Treated" is an indicator that some type of law enforcement happened in the firm owner's network at $t = 0$. Elasticities of workers with disabilities are calculated based on (Bellemare and Wichman, 2020). All estimations include firm-by-cohort and city-by-time-to-event fixed effects. Significance levels are indicated by * < .1, ** < .05, *** < .01. Standard errors clustered at the city-by-cohort-by-event-time level shown in parentheses.

networks are treated earlier than others. Second, at every year, we label time 0 the first time a network receives the QL fine. At that point, the network is considered treated. Third, at every year, any network that receives a QL fine for the first time at least two years in the future is considered a control network. Fourth, we stack for every year a set of treated and control networks repeating this procedure. Every treated and control network has three years of data before and two after the event. To ensure we investigate spillovers from law enforcement and not its direct effect, we drop from our sample the establishments that received a QL fine at time 0.

We estimate the following model using ordinary least squares regressions:

$$Y_{imct} = \delta_0 Treated_{imct} + \sum_{\substack{\tau\neq-1 \\ \tau=-3}}^{\tau=2} D_t^{\tau} + \sum_{\substack{\tau\neq-1 \\ \tau=-3}}^{\tau=2} \delta_\tau (Treated_{imct} \times D_t^{\tau}) + \theta_{mt} + \theta_{ic} + \epsilon_{imct} \tag{2}$$

where Y_{imct} is the outcome of interest (for instance, number of workers with disabilities) for firm i, in municipality m, in the year of treatment (or cohort) c, at event-time t, the D_t^{τ} are indicators equal to one for each event-time window (i.e., τ= -3, …, 0, …., 2), and ϵ_{imct} is a random specification error. We include municipality-by-event-time fixed effects to absorb time-varying local labor market shocks. Moreover, we include firm-by-cohort fixed effects to account for time-invariant firm-specific characteristics within each cohort. We cluster our estimations at the municipality-by-cohort-by-event-time level, as this is the level at which treatment occurs.

The coefficients of interest are the estimates of δ_τ. At every event-time window, they represent the causal impact on the employment of workers with disabilities for firms belonging to a network where another firm received a QL fine. We also present a summary estimate with a post-dummy instead of each event-time dummy.

We estimate equation (2) first for the full sample of firms with more than 100 workers and then separately based on their prior compliance with the Quota Law. Firms are categorized into three groups: (i) *non-compliers*—those that did not employ any workers with disabilities; (ii) *low compliers*—those employing some workers with disabilities but less than 50% of the required quota; and (iii) *high compliers*—those meeting at least 50% of the legally mandated number of workers with disabilities. As noted in Section 3, our measure of compliance should be interpreted with caution due to measurement error, hence the consideration of high compliers as those already employing at least half of the required by the law.

First, firms respond to a Quota Law (QL) fine within their network by increasing the number of workers with disabilities, regardless of which network the fine occurs in. This pattern is evident in Fig. 5, which displays the results from estimations considering all firms with more than 100 workers, for QL fines in the firm's neighbor network (Fig. 5(a)), owner network (Fig. 5(b)), and HR workers network (Fig. 5(c)). Although less evident in the owner network, in all cases a consistent increase in the number of workers with disabilities is observed after a QL fine occurs in the firm's network.

Second, the firms' reactions to QL fines in their networks are inversely proportional to their previous level of compliance with the

Quota Law. Fig. 6 illustrates this result. Overall, the positive effect of a QL fine in the firm's network on the number of workers with disabilities is most pronounced in firms that were not compliant with the QL prior to the fine (Figs. 6(a), 6(d), and 6(g)). Smaller reactions, which are less precisely estimated, are observed in firms with low compliance (Figs. 6(b), 6(e), and 6(h)), while no significant effects are found for highly compliant firms (Figs. 6(c), 6(f), and 6(i)).[33]

Third, one could hypothesize that, after issuing a firm in a particular network, labor inspectors become more likely to inspect other firms in that same network, and this is what drives the increase in the number of workers with disabilities in firms instead of their direct communication with other firms in their network. However, this does not seem to be the case: we can see this in Fig. 7, which presents results of estimations where we investigate whether the occurrence of a Quota Law fine impacts the likelihood of firms being inspected. We estimate models similar to the one in Eq. (2), where now our dependent variable is a binary variable indicating whether the firm was inspected. As shown in the figure, having a QL fine in their network does not increase the likelihood that firms will receive an inspection after such an event.

We summarize these findings in Tables 6 and 7, where we replace the event-time dummies with a binary variable indicating post-event periods. Table 6 presents results on the hiring of persons with disabilities, while Table 7 reports results on inspection likelihood. Overall, the findings indicate that firms respond to a QL fine by increasing their hiring of workers with disabilities. This response is strongest and most precisely estimated in the neighbor network, while the effects are noisier for the owner and HR workers' networks. However, the heterogeneous analysis reveals more consistent patterns across networks: non-compliant firms increase their number of workers with disabilities by approximately 20%–26%, low-compliant firms by 11%–20%, and high-compliant firms show no significant response. Additionally, Table 7 suggests that, in general, fines within a firm's network do not significantly impact the likelihood of inspection. While we detect a small effect in the HR workers' network, the result is marginal and imprecisely estimated, so it should be interpreted with caution.

We provide three robustness checks to our results. First, as mentioned in Section 4, recent work by Chen and Roth (2024) shows that one should be careful when interpreting results with log or inverse hyperbolic sine transformations, especially if the treatment affects the extensive margins. We present in Table A.8 two robustness checks to deal with this issue. First, we present extensive margin estimations, calculating the likelihood that firms have at least one employee with a disability. Second, we estimate a linear regression model with the number of workers with disability as the dependent variable. The results are quite similar to our main estimation.

Second, Callaway and Sant'Anna (2021) propose a strategy different than the stacked differences-in-differences used in this work to identify average treatment effects on treatment units in settings where the treatment happens at different times to different units, with heterogeneous treatment effects. The authors propose a staggered approach, where treated units are compared only to either never-treated or yet-to-be-treated units. Tables A.9 to A.11 in the Appendix present robustness checks implementing Callaway and Sant'Anna (2021).[34] As we can see, the results are quite similar to our benchmark estimates.

Finally, we further test the plausibility of our identification assumptions by showing that firms between 50 and 100 – large enough to be similar to firms subject to the QL but not subject to the law themselves – do not react to the enforcement of the Quota Law in large firms present in their networks (see Table A.12, in the Appendix). This placebo exercise is reassuring since small firms should not react to the occurrence of a QL fine in their network, given that they do not have mandate quotas for persons with disability.

5.1. Discussion on effect size

To contextualize our estimates, we conduct a back-of-the-envelope calculation using the results in Table 6 (Column 1, including all firms). Our goal is to compare the magnitude of the direct effect of QL fines on hiring workers with disabilities (specific deterrence effect) with the indirect effects observed through enforcement spillovers in different firm networks.

Our calculations rely on four key assumptions. First, we use the pre-fine average number of workers with disabilities per firm as the baseline for calculating percentage increases. Second, we approximate the number of firms potentially affected by enforcement spillovers based on the average number of firms with more than 100 workers in each network type. Third, we assume that the estimated percentage increase in hiring applies uniformly across these firms, excluding the fined firm itself. Finally, we estimate that each QL fine leads to the direct hiring of two additional workers with disabilities per fined firm. This assumption is based on non-causal comparisons of workforce composition before and after a fine. This *specific deterrence effect* reflects the direct response of a fined firm to enforcement, whereas the *spillover effects* capture the indirect hiring response in other firms within the fined firm's network.

Table 8 presents all the parameters for our calculation and the comparison between the specific deterrence effect and the spillover effects across different networks. The strongest enforcement spillovers are observed in the neighbor network, where each QL fine leads to an estimated 6.8 additional workers with disabilities being hired in the network, a figure 3.4 times as large as the direct enforcement hires. The HR workers' network also exhibits substantial spillover effects (4.7 hires per fine, 2.4 times larger than the direct enforcement hires). In contrast, the owner network shows a much smaller spillover effect, with only 0.4 additional hires per fine, representing about 20% of the direct enforcement hires.

The differences in spillover magnitudes align with the average size of each network: the HR and neighbor networks contain more firms on average, allowing enforcement effects to propagate more widely. In contrast, the owner network is relatively small, which limits the potential for spillovers.

The figures for the neighbor and HR workers' networks are larger than the ones found in the literature about enforcement spillovers in developed countries. Johnson (2020), for instance, finds that publicizing a facility's violations of safety measures established by the Occupational Safety and Health Administration (OSHA) in the United States had spillover effects in other facilities. Such effects can be up to 50% larger than the specific deterrence effect (i.e., a direct firm inspection by OSHA) for firms closer to a publicized facility. A possible explanation

[33] We employ the methodology proposed by Rambachan and Roth (2023) to assess the sensitivity to parallel trends violations in Figs. A.5 and A.6 (Appendix). Specifically, we allow for differences in linear trends between treated and not-yet-treated and quantify how large any departures from such linearity should be so that we would have null results. We consider a range of values M, where $M = 0$ means no difference in linear trends and $M > 0$ allows for deviations in linearity. If we consider all firms, we do not nullify the results for the neighbor network even for values of M as large as $M = 1$. The breakdown value of M in the worker network is 0.5, and results are already imprecise in the owner network, even for $M = 0$. If we consider non-complier firms, we do not nullify the results for the any network even for values of M as large as $M = 1$.

[34] In our implementation, we pre-demeaned the outcome variable to control for firm-by-cohort and city-by-year fixed effects, thereby isolating the within-firm and within-city-year variation in hiring patterns. We then employed the inverse probability weighting method proposed by Callaway and Sant'Anna (2021) to estimate the treatment effects, comparing treated units only to not-yet-treated firms, effectively excluding those that are never treated. This approach allows us to better account for the staggered nature of treatment timing and heterogeneity in treatment effects.

Table 8
Back-of-the-envelope calculation of enforcement spillovers.

Panel A: Neighbor Network	
Spillover Effect	12.3%
Baseline # Workers w/ Disabilities	5.538
Avg. Network Size (excluding fined firm)	10
Estimated Hires per Firm	0.68
Total Spillover Hires	6.8
Spillover-to-Direct Effect Ratio	3.4
Panel B: Owner Network	
Spillover Effect	4.5%
Baseline # Workers w/ Disabilities	8.638
Avg. Network Size (excluding fined firm)	1
Estimated Hires per Firm	0.40
Total Spillover Hires	0.4
Spillover-to-Direct Effect Ratio	0.2
Panel C: HR Worker Network	
Spillover Effect	4.0%
Baseline # Workers w/ Disabilities	8.769
Avg. Network Size (excluding fined firm)	13.5
Estimated Hires per Firm	0.35
Total Spillover Hires	4.7
Spillover-to-Direct Effect Ratio	2.4

Note: This table summarizes the estimated spillover effects of QL fines on the hiring of workers with disabilities. "Spillover Effect" indicates the percentage increase in hiring; "Baseline # Workers w/ Disabilities" provides a reference point for hiring levels before fines; "Avg. Network Size" refers to the average number of firms with at least 100 workers in each network, excluding the firm which received a QL fine; "Estimated hires per firm" reflects the additional workers hired due to spillovers. "Total spillover hires" aggregates these effects across the entire network; and "Spillover-to-Direct Effect Ratio" is the ratio of spillover hires to direct hires in fined firms, which we estimate to be two workers with disability.

for our relatively large results is that developing countries usually depart from a very low level of regulatory enforcement. Hence, the impact of increasing enforcement might be higher in these countries.

6. Conclusion

This paper examines how the enforcement of disability employment quotas affects firm behavior, both directly and through spillover effects across different firm networks. Our analysis of Brazil's Quota Law yields three key findings. First, the 2012 Administrative Act, which strengthened enforcement procedures, significantly increased the hiring of persons with disabilities in firms subject to the law. The effect was most pronounced for individuals with mobility impairments, followed by those with visual and cognitive impairments. Second, this enhanced enforcement primarily expanded formal employment opportunities for persons with disabilities previously outside the labor market, rather than merely redistributing workers across firms. Third, and most importantly, we document substantial enforcement spillovers, where firms respond to quota law fines received by other companies in their networks by increasing their own hiring of workers with disabilities.

Our findings contribute to the literature on both disability employment policies and regulatory enforcement. While most research on employment quotas has focused on developed economies, our analysis demonstrates that such policies can be effective in developing countries when properly enforced. Additionally, our study provides the first evidence of enforcement spillovers in a developing country context, showing how information about enforcement travels through different firm networks to influence compliance behavior.

These results have important policy implications. Policymakers might enhance the effectiveness of limited enforcement resources by strategically targeting inspections to maximize spillover effects. For example, targeting firms in densely connected business areas could generate larger overall compliance improvements than focusing on isolated companies. Similarly, communicating enforcement actions through channels likely to reach HR professionals across multiple firms might amplify the impact of individual inspections.

Our study has some limitations. While we document evidence of spillover effects, further research is needed to identify the precise mechanisms through which information about enforcement travels between firms. Additionally, our analysis focuses on a specific policy context, and the generalizability of these findings to other regulatory domains or countries requires further investigation. Nevertheless, this paper demonstrates that even with limited enforcement capacity, developing countries can significantly improve compliance with labor regulations by leveraging network effects. The substantial spillovers documented here suggest that strategic enforcement approaches can multiply the impact of regulatory efforts, particularly in contexts where direct monitoring of all regulated entities is infeasible.

CRediT authorship contribution statement

Samuel Berlinski: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Project administration, Methodology, Investigation, Formal analysis, Conceptualization. **Jessica Gagete-Miranda:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Resources, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Declaration of Generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used Chat GPT in order to improve language and readability. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

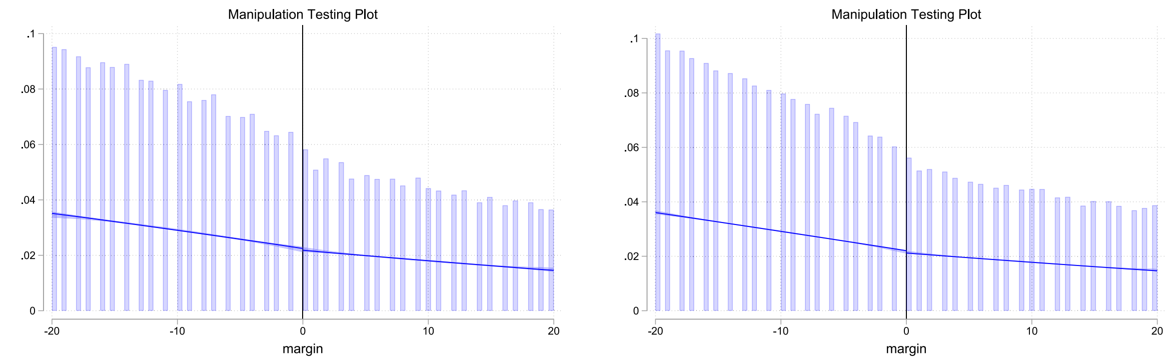
Appendix

A.1. Additional figures

See Figs. A.1–A.6.

A.2. Additional tables

See Tables A.1–A.12.



(a) Before 2012: T stat= 0.2508 ; P>|T|=0.8020 (b) After 2012: T stat=-0.18759 ; P>|T|=0.8513

See Cattaneo et al. (2018) for details about the implementation of manipulation tests.

Fig. A.1. Manipulation test (within RDD bandwidth).See Cattaneo et al. (2018) for details about the implementation of manipulation tests.

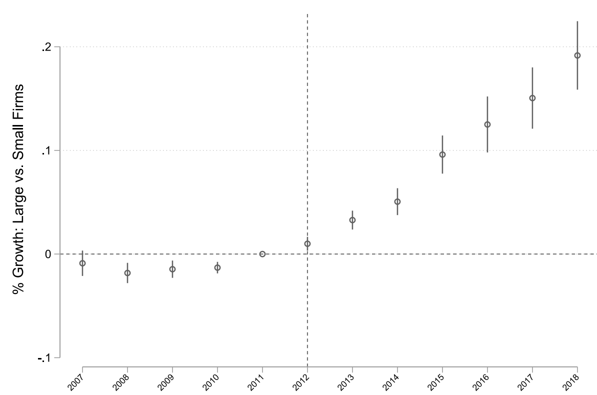
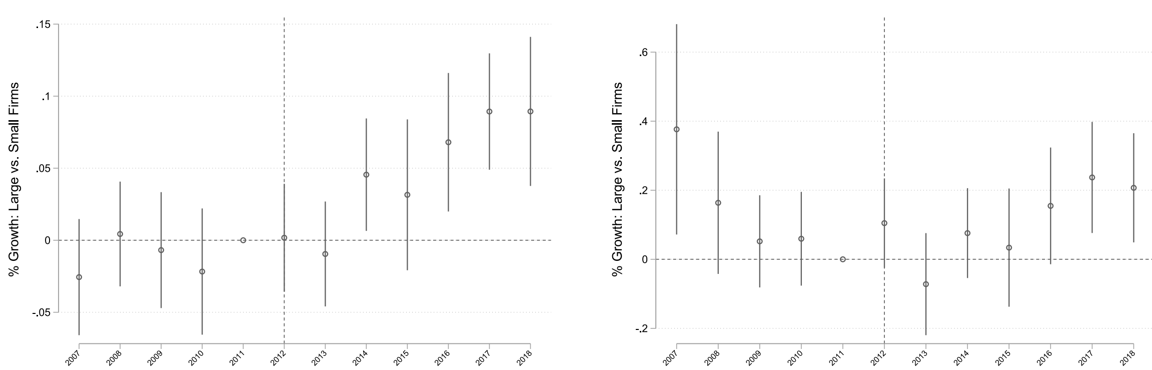
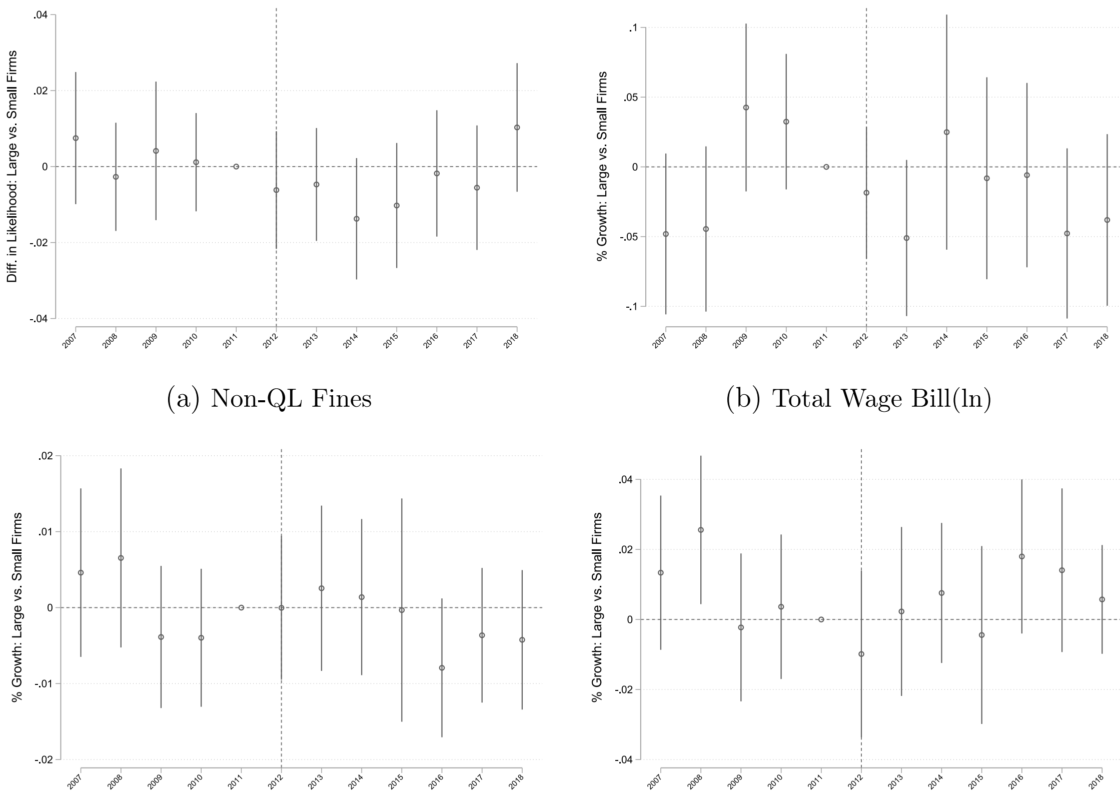


Fig. A.2. The 2012 Administrative Act and the likelihood to comply with the QL.
Note: This graph presents estimates from a model similar to Eq. (1), where we substitute the post-2012 dummy T with year dummies, leaving 2011 as the benchmark. We consider a donut ring of two in the firm-size variable. All estimations include city-by-year fixed effects. 99% confidence interval shown in the graphs.



(a) One or more worker w/ disability (b) Number of workers w/ disability

Fig. A.3. The 2012 Administrative Act and presence of workers with disability: Robustness checks.
Note: These graphs present estimations from a model similar to Eq. (1), where we substitute the post-2012 dummy T with year dummies, leaving 2011 as the benchmark. We consider a donut ring of two in the firm-size variable. All estimations include city-by-year fixed effects. 99% confidence interval shown in the graphs.



(a) Non-QL Fines (b) Total Wage Bill(ln)

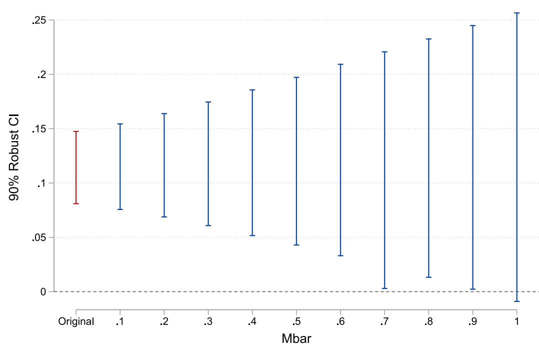
(c) Turnover Rate (d) Firm Closure

Fig. A.4. The 2012 Administrative Act and other firm outcomes.
Note: These graphs present estimations from a model similar to Eq. (1), where we substitute the post-2012 dummy *T* with year dummies, leaving 2011 as the benchmark. The dependent variables are the hyperbolic sine transformation of the variables indicated in each sub-figure. We consider a donut ring of two in the firm-size variable. All estimations include city-by-year fixed effects. 99% confidence interval shown in the graphs.

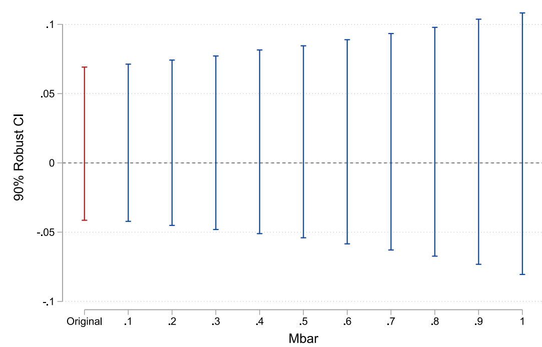
Table A.1
RDD Quota Law threshold: Different bandwidth selection procedures.

	Dep. var.: workers w/ a disability (hyp. sine trans.)				
	(1)	(2)	(3)	(4)	(5)
Panel A: Before 2012					
RD Estimate	0.016	0.041***	0.016	0.016	0.016
	(0.014)	(0.010)	(0.012)	(0.014)	(0.013)
N	991 510	991 510	991 510	991 510	991 510
Mean dep. var. within bandwidth	0.198	0.314	0.195	0.198	0.206
h (left)	16.830	17.820	20.725	16.830	17.820
h (right)	16.830	69.080	20.725	16.830	20.725
Bandwidth selection procedure	mserd	msetwo	msesum	msecomb1	msecomb2
Panel B: After 2012					
RD Estimate	0.057**	0.080***	0.059***	0.057**	0.055***
	(0.025)	(0.017)	(0.018)	(0.025)	(0.020)
N	1 593 962	1 593 962	1 593 962	1 593 962	1593962
Mean dep. var. within bandwidth	0.343	0.548	0.333	0.343	0.371
h (left)	9.844	10.476	14.293	9.844	10.476
h (right)	9.844	52.741	14.293	9.844	14.293
Bandwidth selection procedure	mserd	msetwo	msesum	msecomb1	msecomb2

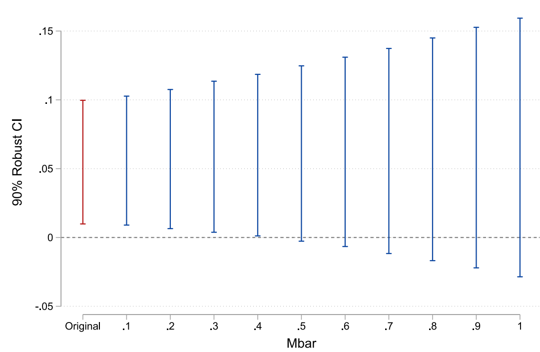
Note: This table shows results from local polynomial regressions where we estimate firms' hiring behavior regarding workers with disabilities once they pass the 100 workers threshold established by the Quota Law (see Cattaneo et al., 2019; Calonico et al., 2014a,b, for details on our RDD estimation). The dependent variable is the hyperbolic sine transformation of the number of workers carrying a disability. Panel A shows estimations for the years before 2012, i.e., before the introduction of the new inspection procedures. Panel B shows estimations for the years after 2012, i.e., after the introduction of the new inspection procedures. Each column of the table shows the results of estimations using different bandwidths optimally computed following the algorithm developed by Calonico et al. (2014a,b). Due to measurement errors in the estimation of the firm's size, we exclude firms within a donut ring of size two from the 100 threshold. Significance levels are indicated by * < .1, ** < .05, *** < .01. Standard errors clustered at the city level shown in parentheses.



(a) Neighbor network



(b) Owner network



(c) HR workers network

Fig. A.5. Pre-trend robustness.
Note: These figures report 90% confidence intervals for different deviations from linear trends between treated and not-yet-treated firms, employing the methodology proposed by Rambachan and Roth (2023) to assess the sensitivity to parallel trends violations. $Mbar = 0$ means no difference in linear trends and $Mbar > 0$ allows for deviations in linearity.

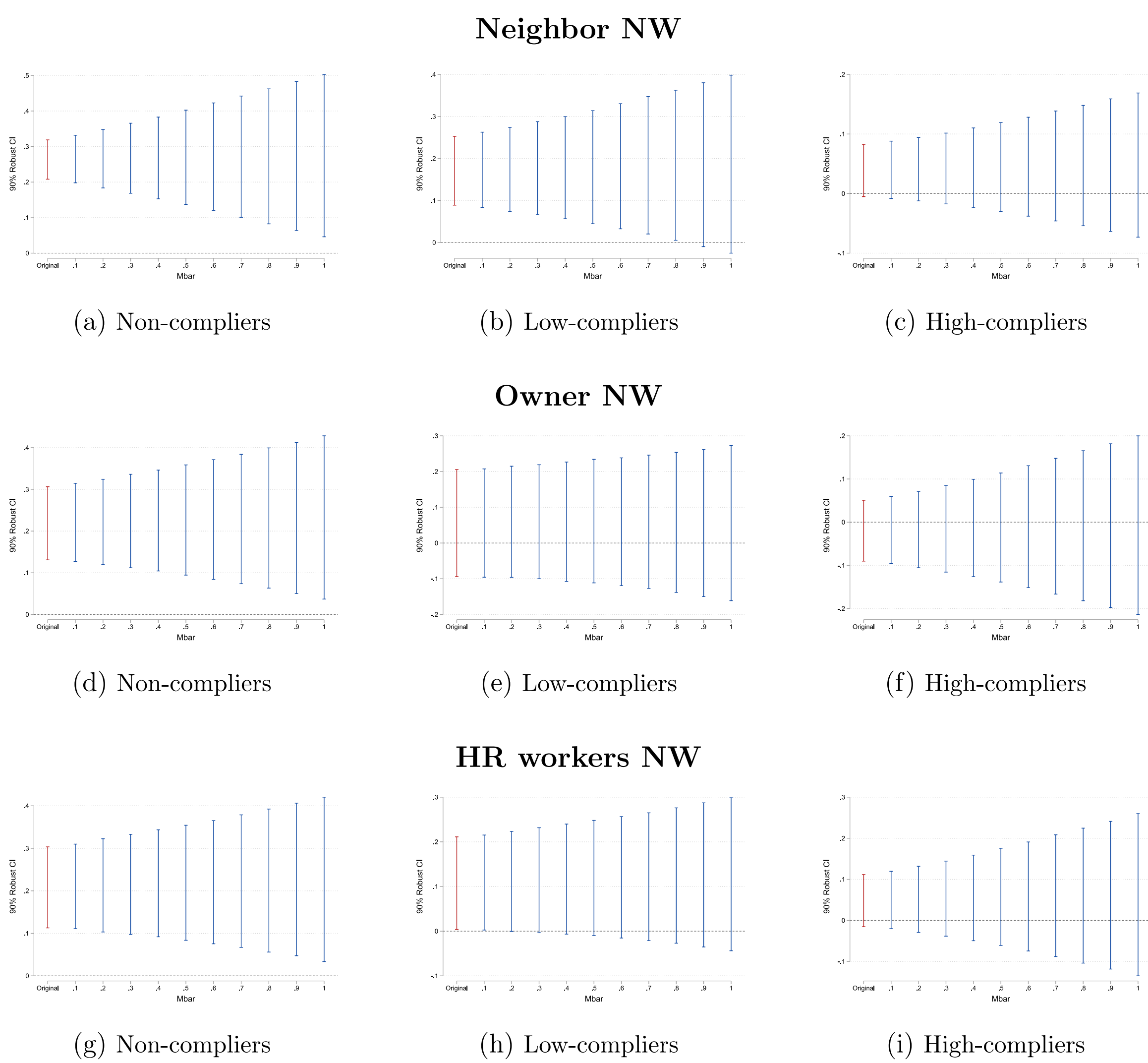


Fig. A.6. Pre-trend robustness — Heterogeneity by compliance with the QL in $t-1$.
Note: These figures report 90% confidence intervals for different deviations from linear trends between treated and not-yet-treated firms, employing the methodology proposed by Rambachan and Roth (2023) to assess the sensitivity to parallel trends violations. $Mbar = 0$ means no difference in linear trends and $Mbar > 0$ allows for deviations in linearity. "Non-compliers" are firms that did not employ any workers with disabilities; "Low-compliers" are firms employing some workers with disabilities but less than 50% of the required quota; "High-compliers" are firms meeting at least 50% of the required quota. "Event Time" is the time after the occurrence of the QL fine in the firm's network.

Table A.2
Descriptive statistics — Firms within diff-in-disc bandwidth.

	Firm size: <100	Firm size: ≥100
Panel A: all years (2007–2018)		
N (firm × year)	70967.00 (31.07%)	157414.00 (68.93%)
Firm size	91.49 (3.66)	126.05 (17.14)
At least one worker w/ disability	0.16 (0.37)	0.37 (0.48)
Workers w/ disability (%)	0.00 (0.02)	0.01 (0.03)
Comply with QL	. (.)	0.15 (0.36)
QL fine	0.01 (0.08)	0.03 (0.16)
Some fine	0.12 (0.33)	0.17 (0.37)
Inspection	0.38 (0.49)	0.50 (0.50)
Panel B: Before 2012		
N (firm × year)	28331.00 (31.33%)	62094.00 (68.67%)
Firm size	91.57 (3.69)	126.23 (17.12)
At least one worker w/ disability	0.12 (0.33)	0.27 (0.44)
Workers w/ disability (%)	0.00 (0.02)	0.01 (0.04)
Comply with QL	. (.)	0.09 (0.28)
QL fine	0.00 (0.04)	0.01 (0.09)
Some fine	0.11 (0.31)	0.15 (0.35)
Inspection	0.40 (0.49)	0.49 (0.50)
Panel C: After 2012		
N (firm × year)	42636.00 (30.91%)	95320.00 (69.09%)
Firm size	91.44 (3.64)	125.93 (17.16)
At least one worker w/ disability	0.19 (0.39)	0.44 (0.50)
Workers w/ disability (%)	0.00 (0.01)	0.01 (0.02)
Comply with QL	. (.)	0.19 (0.39)
QL fine	0.01 (0.10)	0.04 (0.19)
Some fine	0.13 (0.34)	0.18 (0.39)
Inspection	0.37 (0.48)	0.50 (0.50)

Note: "Firm size" is the average number of workers with an active contract in the firm in the period considered; Firms smaller than 20 workers are not considered in our analysis; "At least one worker w/ disability" is the average number of firms with at least one person with a disability with an active contract in the period considered; "Comply with QL" is the average number of firms with the minimum number of workers with disabilities established by the QL in the period considered; "Fine due to non-compliance with QL", "Some fine", and "Inspection" is the average number of firms, in the period considered, that received a fine due to non-compliance with the QL, received some fine, and were inspected, respectively.

Table A.3
The 2012 Administrative Act and presence of workers with disability: Robustness check.

	One or more worker w/ disability		Number of workers w/ disability	
	(1)	(2)	(3)	(4)
Year>2012 × Distance to QL threshold>0	0.049***	0.062***	0.052	0.042
	(0.009)	(0.011)	(0.070)	(0.097)
N	282 594	265 016	282 594	265 016
Mean Dep. Var.	0.328	0.944	0.919	0.944
h (left)	16.175	16.175	16.175	16.175
h (right)	90.242	90.242	90.242	90.242
R2	0.195	0.198	0.108	0.108
Donut ring	1	2	1	2

Note: This table presents estimations from Eq. (1). All estimations include city-by-year fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city level shown in parentheses.

Table A.4
The 2012 Administrative Act and reclassification of disability status.

	(1)	(2)	(3)	(4)	(5)	(6)
	Same firm, not carrying disability in $t-1$		Different firm, not carrying disability in $t-1$		Workers w/ disab. excluding reclassification	
Year>2012 × Distance to QL threshold>0	0.025***	0.026***	0.006	0.007	0.040***	0.043***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.010)	(0.012)
N	309 395	300 429	286 669	277 706	255 616	246 646
Mean Dep. Var.	0.080	0.080	0.069	0.070	0.311	0.315
h (left)	17.429	17.429	14.465	14.465	18.368	18.368
h (right)	118.862	118.862	115.544	115.544	66.863	66.863
R2	0.100	0.101	0.076	0.077	0.174	0.176

Note: This table presents estimations from Eq. (1). All estimations include city-by-year fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city level shown in parentheses.

Table A.5
The 2012 Administrative Act and other firm outcomes.

	Non-QL fines (1)	Total wage bill (2)	Turnover rate (3)	Firm closure (4)
Year>2012 × Distance to QL threshold>0	−0.007*	−0.015	−0.002	−0.003
	(0.003)	(0.013)	(0.003)	(0.005)
N	641 525	298 207	426 112	357 065
Mean Dep. Var.	0.125	12.051	0.314	0.035
h (left)	45.211	22.223	24.076	13.552
h (right)	185.839	82.449	246.241	277.317
R2	0.090	0.440	0.102	0.061
Donut ring	2	2	2	2

Note: This table presents estimations from Eq. (1). All estimations include city-by-year fixed effects. Significance levels are indicated by ∗ < .1, ** < .05, *** < .01. Standard errors clustered at the city level shown in parentheses.

Table A.6
Descriptive statistics — Firms belonging to networks receiving a QL fine.

	Firm size: <100	Firm size: ≥100
Panel A: Neighbor network		
N (firms at event time)	125,407 (84.94%)	22,234 (15.06%)
Firm size	39.48 (19.13)	353.71 (819.52)
At least one worker w/ disability	0.04 (0.20)	0.52 (0.50)
Workers w/ disability (%)	0.00 (0.01)	0.01 (0.03)
Comply with QL	. (.)	0.16 (0.36)
Fine due to non-compliance with QL	0.00 (0.00)	0.00 (0.00)
Some fine	0.04 (0.19)	0.07 (0.25)
Inspection	0.05 (0.21)	0.05 (0.22)
Panel B: Owner network		
N (firms at event time)	14918.00 (67.44%)	7204.00 (32.56%)
Firm size	46.91 (21.68)	463.76 (1027.71)
At least one worker w/ disability	0.07 (0.25)	0.60 (0.49)
Workers w/ disability (%)	0.00 (0.01)	0.01 (0.02)
Comply with QL	. (.)	0.15 (0.35)
Fine due to non-compliance with QL	0.00 (0.00)	0.00 (0.00)
Some fine	0.04 (0.20)	0.08 (0.27)
Inspection	0.07 (0.25)	0.06 (0.24)
Panel C: HR workers network		
N (firms at event time)	6194.00 (47.21%)	6926.00 (52.79%)
Firm size	52.00 (22.17)	503.57 (936.38)
At least one worker w/ disability	0.07 (0.26)	0.65 (0.48)
Workers w/ disability (%)	0.00 (0.02)	0.01 (0.04)
Comply with QL	. (.)	0.13 (0.34)
Fine due to non-compliance with QL	0.00 (0.03)	0.03 (0.16)
Some fine	0.04 (0.20)	0.07 (0.26)
Inspection	0.06 (0.24)	0.04 (0.19)

Note: ‘‘Firm size’’ is the average number of workers with an active contract in the firm in the period considered; Firms smaller than 20 workers are not considered in our analysis; ’’At least one worker w/ disability’’ is the average number of firms with at least one person with a disability with an active contract in the period considered; ‘‘Comply with QL’’ is the average number of firms with the minimum number of workers with disabilities established by the QL in the period considered; ‘‘Fine due to non-compliance with QL’’, ‘‘Some fine’’, and ‘‘Inspection" is the average number of firms, in the period considered, that received a fine due to non-compliance with the QL, received some fine, and were inspected, respectively.

Table A.7
Firms' characteristics in $t-1$ and QL fine in network in t.

	2012		2013		2014		2015		2016		2017	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Neighbor network												
L.hyp_disabled	−0	0	0.001	0.001	−0.002*	−0.004*	0.001	0	−0.001	−0.003	0	−0.001
	(0.001)	(0.003)	(0.002)	(0.004)	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
L.hyp_firm_size_QL	0.001	−0.002	−0	−0.002	0.002	0.003	−0	−0.003	0.001	0.002	−0.001	−0.002
	(0.001)	(0.003)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.001)	(0.003)	(0.001)	(0.003)
Never treated included	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
N	22 744	9926	23 222	10 033	22 962	9927	21 249	9286	19 892	8717	19 822	8145
R2	0.429	0.484	0.357	0.417	0.329	0.403	0.367	0.439	0.407	0.473	0.213	0.329
Panel B: Owner network												
L.hyp_disabled	0.002**	0.003	0	−0.002	0.002	−0.001	0.001*	−0.003	−0.001	−0.008**	0.001	−0.004
	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.005)	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.003)
L.hyp_firm_size_QL	0.001	−0.012**	0.004***	0	0.001	−0.009	0.001	−0.004	0.002	0.002	0.001	−0.007
	(0.001)	(0.005)	(0.001)	(0.006)	(0.002)	(0.008)	(0.002)	(0.009)	(0.001)	(0.006)	(0.001)	(0.007)
Never treated included	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
N	22 744	3719	23 222	3654	22 962	3564	21 249	3361	19 892	3166	19 822	3089
R2	0.055	0.124	0.064	0.125	0.061	0.137	0.071	0.149	0.067	0.138	0.061	0.138
Panel C: HR workers network												
L.hyp_disabled	0.001	−0.002	0.001	−0.002	−0	−0.003	0.001	0.001	0	−0.002	0	−0.002
	(0.001)	(0.004)	(0.001)	(0.003)	(0.001)	(0.003)	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)
L.hyp_firm_size_QL	0.006***	−0.004	0.005***	−0.004	0.002	−0.017***	0.002	−0.014***	0	−0.006*	0.002*	−0.002
	(0.001)	(0.005)	(0.001)	(0.004)	(0.001)	(0.004)	(0.002)	(0.005)	(0.001)	(0.003)	(0.001)	(0.004)
Never treated included	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
N	22 744	4332	23 222	4559	22 962	4652	21 249	4426	19 892	4277	19 822	4194
R2	0.051	0.118	0.054	0.134	0.041	0.117	0.046	0.121	0.062	0.121	0.045	0.110

Note: This table shows estimations of the likelihood that a network receives a Quota Law fine each year, depending on the characteristics of firms belonging to such a network in the previous year. Odd-numbered columns include firms from never-treated networks, that is, that never received a Quota Law fine, while even-numbered columns exclude such firms, restricting the sample to networks used in our analysis. All estimations include city-by-year fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city level shown in parentheses.

Table A.8
Law enforcement at firm's networks: Robustness checks.

	One or more workers w/ disability				Workers w/ disability			
	(1) All firms	(2) Non-compliers	(3) Low-compliers	(4) High-compliers	(5) All firms	(6) Non-compliers	(7) Low-compliers	(8) High-compliers
Panel A: Neighbor network								
Post-event × Treated	0.042***	0.119***	0.047***	−0.005	0.452**	0.885***	1.670**	−0.247
	(0.007)	(0.013)	(0.015)	(0.009)	(0.212)	(0.141)	(0.773)	(0.399)
N	67 145	25 715	10 422	25 873	67 145	25 715	10 422	25 873
N (firms)	10 430	4481	1488	4861	10 430	4481	1488	4861
Avg. firm size	383.499	247.679	873.142	328.402	383.499	247.679	873.142	328.402
Mean Dep. Var.	5.538	0.718	9.010	9.283	5.538	0.718	9.010	9.283
R2	0.734	0.584	0.510	0.751	0.906	0.514	0.866	0.930
Panel B: Owner network								
Post-event × Treated	0.004	0.112***	0.042	−0.030**	1.196***	0.928***	3.038***	0.688
	(0.012)	(0.024)	(0.026)	(0.015)	(0.360)	(0.270)	(0.928)	(0.693)
N	24 047	7550	4275	9426	24 047	7550	4275	9426
N (firms)	3431	1275	585	1589	3431	1275	585	1589
Avg. firm size	497.905	302.858	917.456	477.203	497.905	302.858	917.456	477.203
Mean Dep. Var.	8.638	0.939	8.572	15.423	8.638	0.939	8.572	15.423
R2	0.735	0.609	0.496	0.729	0.938	0.604	0.776	0.955
Panel C: HR workers network								
Post-event × Treated	0.002	0.069***	0.007	−0.007	0.673**	1.691**	2.362***	−0.047
	(0.009)	(0.022)	(0.017)	(0.012)	(0.310)	(0.795)	(0.649)	(0.487)
N	25 220	7235	5721	9887	25 220	7235	5721	9887
N (firms)	3374	1184	766	1547	3374	1184	766	1547
Avg. firm size	561.430	323.973	1064.430	456.828	561.430	323.973	1064.430	456.828
Mean Dep. Var.	8.769	1.075	10.191	14.005	8.769	1.075	10.191	14.005
R2	0.721	0.618	0.505	0.714	0.906	0.324	0.852	0.930

Note: This table presents estimations from a model similar to Eq. (2), where we substitute the time dummies with a post-event dummy. The sample is composed of firms larger than 100 workers, which did not receive a QL fine in $t = 0$. "Treated" is an indicator that some type of law enforcement took place in the firm owner's network at $t = 0$. All estimations include firm-by-cohort and city-by-time-to-event fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city-by-cohort-by-event-time level shown in parentheses.

Table A.9
QL fine in neighbor network: Robustness check using Callaway and Sant'Anna (2021).

	(1) All firms	(2) Non-compliers	(3) Low-Compliers	(4) High-Compliers
Pre_avg	0.004	−0.050	−0.008	0.031
	(0.029)	(0.041)	(0.118)	(0.056)
Post_avg	0.135***	0.505***	0.287	−0.014
	(0.047)	(0.050)	(0.176)	(0.112)
Tm3	0.001	−0.057	−0.045	0.021
	(0.084)	(0.127)	(0.348)	(0.183)
Tm2	0.009	−0.047	0.033	0.018
	(0.055)	(0.069)	(0.192)	(0.123)
Tm1	0.001	−0.045	−0.010	0.056
	(0.049)	(0.049)	(0.170)	(0.113)
Tp0	0.100**	−0.055**	0.162	0.361***
	(0.042)	(0.026)	(0.146)	(0.097)
Tp1	0.137***	0.440***	0.287*	0.001
	(0.050)	(0.053)	(0.174)	(0.114)
Tp2	0.164***	0.731***	0.333	−0.138
	(0.059)	(0.082)	(0.221)	(0.139)
Tp3	0.140*	0.905***	0.366	−0.281
	(0.082)	(0.107)	(0.310)	(0.176)
N	27 067	8044	4176	13 443
N (firms)	7174	2122	903	3537
Avg. firm size	391.717	264.492	816.877	336.116
Mean Dep. Var.	5.772	0.512	7.312	8.615
Elasticity	0.145	0.657	0.332	−0.014

Note: This table presents estimations from Eq. (2), but using the method proposed by Callaway and Sant'Anna (2021) instead of the stacked differences-in-differences used in our main estimations. All estimations include firm-by-cohort and city-by-time-to-event fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city-by-cohort-by-event-time level shown in parentheses.

Table A.10
QL fine in owner network: Robustness check using Callaway and Sant'Anna (2021).

	(1) All firms	(2) Non-compliers	(3) Low-Compliers	(4) High-Compliers
Pre_avg	−0.015	0.083	−0.152	−0.023
	(0.050)	(0.066)	(0.096)	(0.052)
Post_avg	0.138	0.604***	0.336***	−0.059
	(0.091)	(0.080)	(0.111)	(0.094)
Tm3	−0.040	0.227	−0.537*	−0.031
	(0.150)	(0.214)	(0.294)	(0.156)
Tm2	0.057	−0.017	0.082	0.020
	(0.098)	(0.139)	(0.185)	(0.103)
Tm1	−0.060	0.040	0.001	−0.059
	(0.090)	(0.079)	(0.119)	(0.090)
Tp0	0.093	−0.130***	0.162**	0.275***
	(0.084)	(0.049)	(0.081)	(0.068)
Tp1	0.095	0.363***	0.255**	−0.037
	(0.096)	(0.085)	(0.109)	(0.088)
Tp2	0.135	0.776***	0.388***	−0.201*
	(0.113)	(0.123)	(0.129)	(0.110)
Tp3	0.230	1.407***	0.537**	−0.273
	(0.140)	(0.203)	(0.252)	(0.172)
N	7257	1856	1356	3534
N (firms)	1958	505	301	918
Avg. firm size	485.965	272.082	962.272	417.148
Mean Dep. Var.	8.546	0.590	10.684	12.619
Elasticity	0.148	0.829	0.399	−0.057

Note: This table presents estimations from Eq. (2), but using the method proposed by Callaway and Sant'Anna (2021) instead of the stacked differences-in-differences used in our main estimations. All estimations include firm-by-cohort and city-by-time-to-event fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city-by-cohort-by-event-time level shown in parentheses.

Table A.11
QL fine in HR worker network: Robustness check using Callaway and Sant'Anna (2021).

	(1) All firms	(2) Non-compliers	(3) Low-Compliers	(4) High-Compliers
Pre_avg	0.021	0.027	0.016	0.040
	(0.049)	(0.125)	(0.116)	(0.054)
Post_avg	0.106	0.487***	0.221	−0.049
	(0.084)	(0.118)	(0.137)	(0.084)
Tm3	−0.003	0.218	0.015	−0.024
	(0.149)	(0.268)	(0.401)	(0.167)
Tm2	0.047	−0.141	−0.023	0.088
	(0.095)	(0.127)	(0.224)	(0.103)
Tm1	0.018	0.004	0.056	0.055
	(0.082)	(0.084)	(0.190)	(0.075)
Tp0	0.038	−0.176***	0.131	0.281***
	(0.074)	(0.048)	(0.130)	(0.068)
Tp1	0.093	0.310***	0.308*	−0.034
	(0.084)	(0.094)	(0.159)	(0.091)
Tp2	0.125	0.579***	0.199	−0.137
	(0.105)	(0.137)	(0.201)	(0.108)
Tp3	0.170	1.237***	0.246	−0.306*
	(0.147)	(0.304)	(0.229)	(0.165)
N	6347	1680	1263	3102
N (firms)	1622	446	271	774
Avg. firm size	492.806	293.883	995.926	395.695
Mean Dep. Var.	7.778	0.888	7.992	11.694
Elasticity	0.112	0.628	0.247	−0.048

Note: This table presents estimations from Eq. (2), but using the method proposed by Callaway and Sant'Anna (2021) instead of the stacked differences-in-differences used in our main estimations. All estimations include firm-by-cohort and city-by-time-to-event fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city-by-cohort-by-event-time level shown in parentheses.

Table A.12
Law enforcement in firm networks — Placebo (Firms 50–100 workers).

	Neighbor network	Owner network	HR workers network
	(1)	(2)	(3)
Post-event × Treated	0.006	−0.003	0.001
	(0.004)	(0.010)	(0.014)
Sample			
N	132,188	28,615	14,487
N (firms)	20,227	3,991	2,277
Avg. firm size	69.990	74.197	77.937
Mean Dep. Var.	0.180	0.215	0.225
Elasticity	0.006	−0.003	0.001
R2	0.770	0.766	0.760

Note: This table presents estimations from Eq. (1) focusing on a sample of firms sized between 50 and 100 workers, instead of firms larger than 100, as in our main estimations. All estimations include firm-by-cohort and city-by-time-to-event fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city-by-cohort-by-event-time level shown in parentheses.

Data availability

The authors do not have permission to share data.

References

Acemoglu, D., Angrist, J.D., 2001. Consequences of employment protection? The case of the Americans with disabilities act. J. Political Econ. 109 (5), 915–957.
Acemoglu, D., García-Jimeno, C., Robinson, J.A., 2015. State capacity and economic development: A network approach. Am. Econ. Rev. 105 (8), 2364–2409.
Almeida, R., Carneiro, P., 2012. Enforcement of labor regulation and informality. Am. Econ. J.: Appl. Econ. 4 (3), 64–89.
Almeida, R., Ronconi, L., 2016. Labor inspections in the developing world: Stylized facts from the enterprise survey. Ind. Relations: A J. Econ. Soc. 55 (3), 468–489.
Alstadsæter, A., Kopczuk, W., Telle, K., 2019. Social networks and tax avoidance: Evidence from a well-defined Norwegian tax shelter. Int. Tax Public Financ. 26, 1291–1328.
Barreca, A.I., Guldi, M., Lindo, J.M., Waddell, G.R., 2011. Saving babies? Revisiting the effect of very low birth weight classification. Q. J. Econ. 126 (4), 2117–2123.
Bellemare, M.F., Wichman, C.J., 2020. Elasticities and the inverse hyperbolic sine transformation. Oxf. Bull. Econ. Stat. 82 (1), 50–61.
Berlinski, S., Duryea, S., Perez-Vincent, S.M., 2021. Prevalence and correlates of disability in Latin America and the Caribbean: Evidence from 8 national censuses. PloS One 16 (10), e0258825.
Callaway, B., Sant'Anna, P.H., 2021. Difference-in-differences with multiple time periods. J. Econometrics 225 (2), 200–230.
Calonico, S., Cattaneo, M.D., Titiunik, R., 2014a. Robust data-driven inference in the regression-discontinuity design. Stata J. 14 (4), 909–946.
Calonico, S., Cattaneo, M.D., Titiunik, R., 2014b. Robust nonparametric confidence intervals for regression-discontinuity designs. Econometrica 82 (6), 2295–2326.
Cattaneo, M.D., Idrobo, N., Titiunik, R., 2019. A Practical Introduction to Regression Discontinuity Designs: Foundations. Cambridge University Press.
Cattaneo, M.D., Jansson, M., Ma, X., 2018. Manipulation testing based on density discontinuity. Stata J. 18 (1), 234–261.
Chen, J., Roth, J., 2024. Logs with zeros? Some problems and solutions. Q. J. Econ. 139 (2), 891–936.
de Souza, G., 2023. Employment and welfare effects of the quota for disabled workers in Brazil. FRB of Chicago Working Paper.
DeLeire, T., 2000a. The unintended consequences of the Americans with Disabilities Act. Regulation 23, 21.
DeLeire, T., 2000b. The wage and employment effects of the Americans with Disabilities Act. J. Hum. Resour. 693–715.
Deshpande, M., Li, Y., 2019. Who is screened out? Application costs and the targeting of disability programs. Am. Econ. J.: Econ. Policy 11 (4), 213–248.
Drago, F., Mengel, F., Traxler, C., 2020. Compliance behavior in networks: Evidence from a field experiment. Am. Econ. J.: Appl. Econ. 12 (2), 96–133.
Duryea, S., Martínez Alvear, C., Smith, R., 2024. Disability Employment Quotas: Effects of Laws and Nudges. Technical report, IDB Working Paper Series.
Evans, M.F., Gilpatric, S.M., Shimshack, J.P., 2018. Enforcement spillovers: Lessons from strategic interactions in regulation and product markets. J. Law Econ. 61 (4), 739–769.
Fadlon, I., Nielsen, T.H., 2020. Family labor supply responses to severe health shocks: Evidence from Danish administrative records. Am. Econ. J.: Appl. Econ.
Firpo, S.P., Portella, A.L., 2024. The labor market in Brazil, 2001–2022. In: IZA World Labor. Institute of Labor Economics (IZA), Bonn.
Förster, M., 2007. From Disability to Ability. Policy Challenges and Trends in OECD Countries. OECD Directorate for Employment, Labour and Social Affairs. Social Policy ....
Gray, W.B., Shimshack, J.P., 2011. The effectiveness of environmental monitoring and enforcement: A review of the empirical evidence. Rev. Environ. Econ. Policy 5 (1), 3–24.
Grembi, V., Nannicini, T., Troiano, U., 2016. Do fiscal rules matter? Am. Econ. J.: Appl. Econ. 1–30.
Hotchkiss, J.L., 2004. A closer look at the employment impact of the Americans with Disabilities Act. J. Hum. Resour. 39 (4), 887–911.
Johnson, M.S., 2020. Regulation by shaming: Deterrence effects of publicizing violations of workplace safety and health laws. Am. Econ. Rev. 110 (6), 1866–1904.
Jolls, C., Prescott, J.J., 2004. Disaggregating Employment Protection: The Case of Disability Discrimination. Technical report, National Bureau of Economic Research.
Krekó, J., Telegdy, Á., 2022. The effects of a disability employment quota when compliance is cheaper than defiance. IZA Discussion Paper.
Lalive, R., Wuellrich, J.-P., Zweimüller, J., 2013. Do financial incentives affect firms' demand for disabled workers? J. Eur. Econ. Assoc. 11 (1), 25–58.
Levine, D.I., Toffel, M.W., Johnson, M.S., 2012. Randomized government safety inspections reduce worker injuries with no detectable job loss. Science 336 (6083), 907–911.
Livermore, G.A., Goodman, N., 2009. A review of recent evaluation efforts associated with programs and policies designed to promote the employment of adults with disabilities. report. In: Rehabilitation Research and Training Center on Employment Policy for Personswith Disabilities. ERIC.
Lu, F., Zhang, J., Perloff, J.M., 2016. General and specific information in deterring traffic violations: evidence from a randomized experiment. J. Econ. Behav. Organ. 123, 97–107.
Mont, D., et al., 2004. Disability Employment Policy. Social Protection, Labor Markets, Pensions, Social Assistance, World Bank.
Mori, Y., Sakamoto, N., 2018. Economic consequences of employment quota system for disabled people: Evidence from a regression discontinuity design in Japan. J. Jpn. Int. Econ. 48, 1–14.
OECD, 2003. Transforming Disability into Ability: Policies to Promote Work and Income Security for Disabled People. OECD Paris.
Paetzold, J., Winner, H., 2016. Taking the high road? Compliance with commuter tax allowances and the role of evasion spillovers. J. Public Econ. 143, 1–14.
Ponczek, V., Ulyssea, G., 2022. Enforcement of labour regulation and the labour market effects of trade: Evidence from Brazil. Econ. J. 132 (641), 361–390.
Pries, M.J., Rogerson, R., 2022. Declining worker turnover: The role of short-duration employment spells. Am. Econ. J.: Macroecon. 14 (1), 260–300.
Rambachan, A., Roth, J., 2023. A more credible approach to parallel trends. Rev. Econ. Stud. 90 (5), 2555–2591.
Rincke, J., Traxler, C., 2011. Enforcement spillovers. Rev. Econ. Stat. 93 (4), 1224–1234.
Shimshack, J.P., 2014. The economics of environmental monitoring and enforcement. Annu. Rev. Resour. Econ. 6 (1), 339–360.
Shimshack, J.P., Ward, M.B., 2005. Regulator reputation, enforcement, and environmental compliance. J. Environ. Econ. Manag. 50 (3), 519–540.
Slemrod, J., 2019. Tax compliance and enforcement. J. Econ. Lit. 57 (4), 904–954.
Szerman, C., 2022. The labor market effects of disability hiring quotas. Available at SSRN 4267622.