Enforcement Spillovers under Different Networks: The Case of Quotas for Persons with Disabilities in Brazil

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Abstract

This study examines labor law enforcement spillovers in Brazil's highly informal economy, focusing on disability quota enforcement for formal firms. New inspection procedures increased compliance through heightened inspections and fines, boosting disability hiring. We investigate spillover effects across various firm networks: neighborhood, ownership, and human resources specialists. Results show that spillovers can have up to twice 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.

JEL Codes: I38, J68, K31

Keywords: Enforcement spillovers, Networks, Persons with disability, Brazil

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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 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 under different firms' networks. Third, Brazil is a good example of 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 by about 7%. 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 7.4% in the neighbor network, 7% in the owner network, and 4.6% 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 in the neighbor and HR workers' networks is about twice as large as the direct impact of receiving a QL fine. This figure decreases considerably for the owner networks (only 30% as large as the direct impact) due to the small size of such networks.

Spillovers are stronger for firms that were not complying with the Quota Law when the fine was issued. We show that the likelihood of being inspected does not increase after the occurrence of a QL fine in any of the firm's networks, which 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 ensures that private employers, state and local governments, employment agencies, and labor unions cannot discriminate against qualified individuals with disabilities during job application procedures, hiring, firing, advancement, compensation, job training, and other aspects of employment. Evidence on its impact is mixed, with some papers showing a negative effect on labor market participation of persons with disabilities (Acemoglu and Angrist, 2001; DeLeire, 2000a,b), while other studies dispute such 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

[1]For a review on the impacts of the ADA and other policies targeting the inclusion of persons with disabilities in the United States, see Livermore and Goodman (2009).

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 generates spillover effects, which should be taken into consideration in any welfare calculation.

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 to 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. For example, the results of a set of inspections could be disseminated through information letters to companies in a firm's network.

[2]The literature has shown that there are enforcement spillovers among individuals' networks (family, coworkers, 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).

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 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 workers 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%.

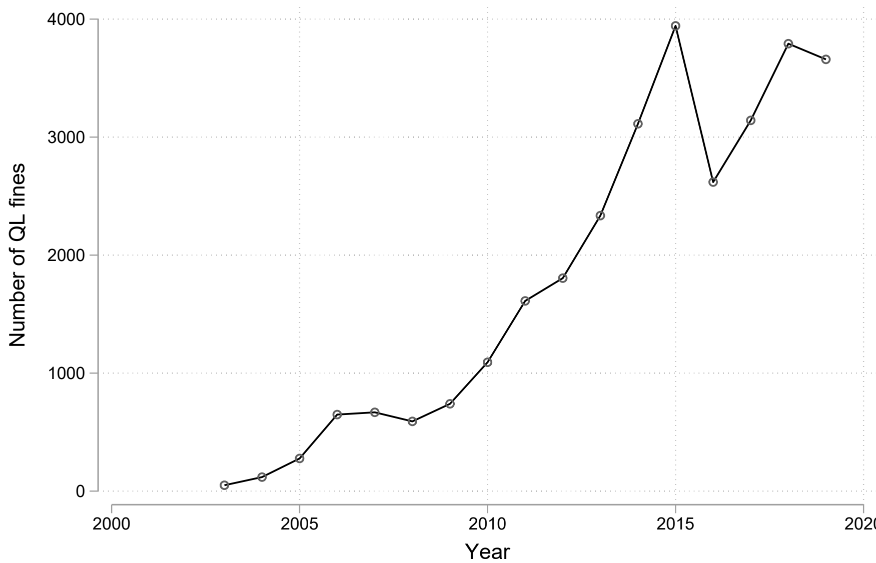
In Brazil, the Quota Law[4] is the most relevant legislation regarding the employment opportunities of persons with disability. It establishes that, respectively, firms with more than 100, 200, 500, and 1,000 employees must fill at least 2%, 3%, 4%, and 5% of their payroll with people with a certified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

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

[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, 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.

Figure 1: Number of QL fines across years



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[5]. 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, as shown in Figure 1, the Administrative Act of 2001 did not increase fines due to non-compliance with the Quota Law. 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.[6] 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

[5]Instrução Normativa SIT nº 20 de 26/01/2001

[6]Instrução Normativa SIT nº 98 de 15/08/2012

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 modifications 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.

After this new administrative act was enacted, the number of QL fines steeply increased (Figure 1). While the yearly number of fines was never larger than 2,000 before 2012, it peaked at around 4,000 in 2015.

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). 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.[7] 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.[8] All our analyses are at the firm's level, clustered within the city of the firm's headquarters.

To calculate firms' size (the number of employees in its payroll), we first restricted the

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

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

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 Dec. 31st of each year, which is the most reliable information on workers' employment status.[9] 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 only became reliable after 2007.[10] 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 (more or less 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.

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.[11] 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

[9] 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.

[10] 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 - Physical disability; 2 - Hearing disability; 3 - Visual disability; 4 - Mental disability; 5 - Multiple disabilities; 6 - Rehabilitated). In 2006, the number of people with hearing and physical 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 - Physical; 2 - Hearing). For more details, see https://sit.trabalho.gov.br/radar/.

[11] 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.

instance, labor inspectors need to verify the medical report of each person with disability employed by the firm, check if firms' installations 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, [12] and (c) the reason why a firm is fined. We match these entries with RAIS 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.[13]

Finally, we use data from Brazil's Federal Revenue Administration that contains information regarding all business associates linked to a firm.

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. 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, firms with less than one hundred workers do not need to comply with the QL, and we can see that only 4% of firms smaller than this threshold 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, even though only 16% of firms fully complied with the Quota Law, the scenario improved: 62% of firms had at least one worker with a disability.

[12]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.

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

Table 1: Descriptive Statistics

	Firm size: <100	Firm size: ≥ 100
Panel A: all years (2007-2018)		
N (firm x 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 x 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 x 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)

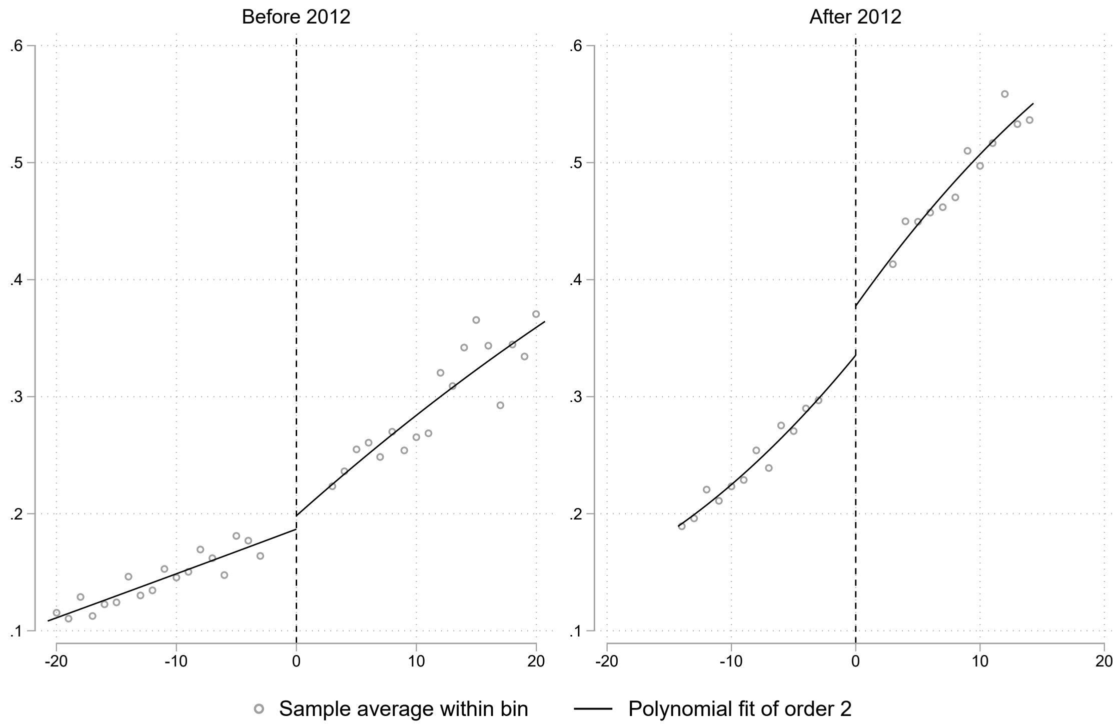
Note: "Firm size" is the average number of workers with an active contract in the firm in the period considered; Firms smaller than 20 workers are not considered in our analysis; "At least one worker w/ disability" is the average number of firms with at least one person with a disability with an active 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.

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 that introduced more stringent inspection procedures impacted firms' hiring. Figure 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

Figure 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.

the QL threshold increased the hiring of persons with disability in these two periods.[14] 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 A1 in Appendix A presents RDD estimations such as the ones in Figure 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.

[14]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 A1 and Figure A1 in Appendix A present, respectively, RDD estimations before and after 2012 employing different bandwidths selection procedures, and manipulation tests at the 100 threshold.

4.1 Main Effects

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)):

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

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 equation 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 labor inspection offices.

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 problem 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 Figure A1 in the Appendix A shows that manipulation is not a concern in our analysis.

Table 2 presents the impact of the 2012 Administrative Act on law enforcement (the likelihood of receiving an inspection in columns (1) and (2) and of being fined due to non-compliance with the Quota Law in columns (3) and (4)[15]), and on the number of workers with disabilities present in the firm (columns (5) and (6)). Columns (1), (3), and (5) present estimations considering a donut ring of one, while columns (2), (4), and (6) present robustness checks implementing a donut ring of two in the firm-size variable.

The introduction of the 2012 Administrative Act increases the likelihood that firms would be inspected by 2 percentage points or a 4.5% increase. The increased likelihood of receiving a Quota Law fine, shown in column (3), is much more striking: after the 2012 Administrative Act, such a likelihood increased by 1.3 percentage points or 54.2%. Finally, the result in column (5) shows that firms reacted to the increase in QL enforcement after 2012 by hiring more persons with disabilities. More specifically, we can see that firms larger than 100 workers increased their number of workers with a disability by 6.4%.[16] The results are quite similar regardless of the donut ring implemented.

Figure 3 presents the dynamic results of such estimations, where we substitute the binary variable T in equation 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.

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

[15] 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.

[16] 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).

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 A2 and Figure A2 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.

Table 2: The 2012 Administrative Act, Law Enforcement, and Workers with Disabilities

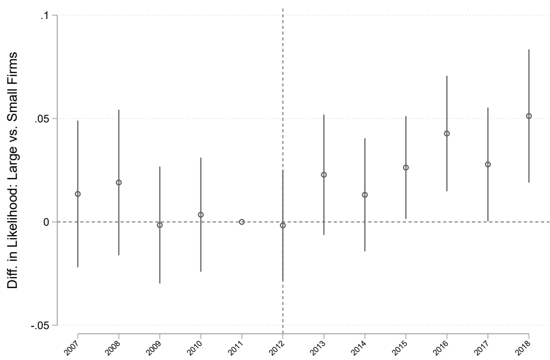
	(1)	(2)	(3)	(4)	(5)	(6)
	Inspection		QL fine		Workers w/ a disability (hyp. sine trans.)	
Year>2012 X Dist. QL threshold>0	0.020***	0.019***	0.013***	0.013***	0.062***	0.068***
	(0.006)	(0.007)	(0.003)	(0.002)	(0.016)	(0.018)
N	446497	437528	331122	322136	222233	213288
Mean Dep. Var.	0.440	0.441	0.024	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 Equation 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.

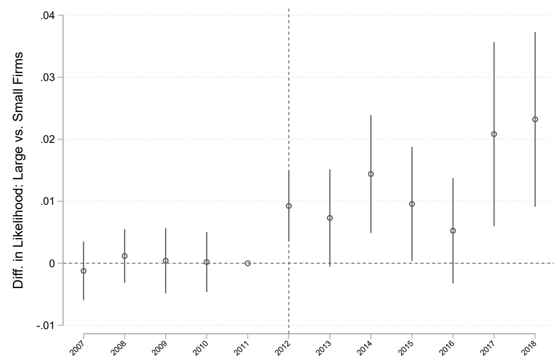
We also investigate whether introducing the new inspection mechanisms led to changes in fines unrelated to the QL (non-QL fines) or to outcomes potentially related to firms' profitability. Table A3 and Figure A3 in the Appendix show the result of that analysis. We test for the effects of the new inspection 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 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 around the threshold 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.[17] 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

[17] 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)

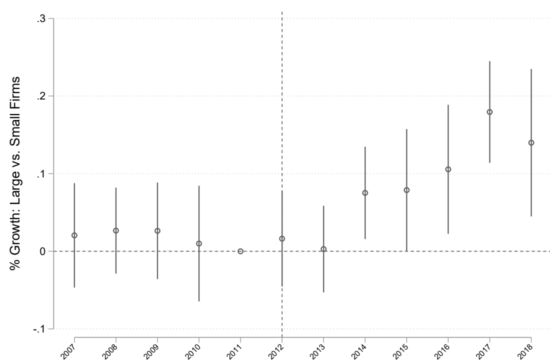
Figure 3: The 2012 Administrative Act, Law Enforcement, and Workers with Disabilities



(a) Inspection



(b) QL Fines



(c) Workers with Disability

Note: These graphs present estimations from a model similar to Equation 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. 90% confidence interval shown in the graphs.

States (Levine et al., 2012) and in the Brazilian context (Szerman, 2022), which also does not find that hiring people with disabilities had negative impacts on firms or workers without disabilities.[18]

A final question we ask is whether enforcing the Quota Law led firms to compete for workers with disability. Hence, we investigate whether the wages of persons with disabilities increased after the 2012 Administrative Act. We present this result in the last column of Table A3 and the last sub-figure of Figure A3 in the Appendix. As we can see, there is no evidence of such a competition for workers with disability.

Overall, these results show that the only remarkable difference after 2012 in the prevalence of law enforcement between firms larger and smaller than 100 was the increase in inspections and primarily the increase in issuing of Quota Law fines. The results also show that firms reacted to such an increase in QL enforcement by hiring more workers with disabilities, and this does not seem to impact other firms' economic outcomes or their competition for the employment of persons with disability.

The impact of the new inspection mechanisms on the hiring of workers with disabilities is particularly noteworthy, especially when considering two factors that could make firms less responsive to the quota law. First, if the costs of hiring a worker with a disability—such as the expenses required to adapt the workplace—are prohibitively high, firms might opt to ignore the quota and pay the associated fines instead. Second, corruption could weaken the effectiveness of these inspection mechanisms, for instance, if firms bribe inspectors to overlook non-compliance. However, our findings suggest that, even when these factors are present, they do not fully counteract the effects of strengthened law enforcement.

4.2 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).

[18] 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.

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 equation 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 3. 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)).

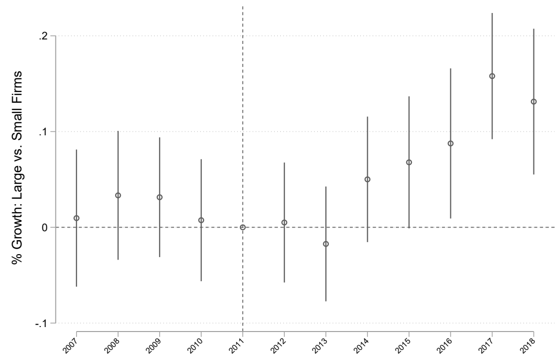
Table 3: Heterogeneous Results by Enforcement Capacity Level

	(1)	(2)	(3)	(4)	(5)	(6)
	Inspection		QL fine		Workers w/ a disability (hyp. sine trans.)	
Year>2012 X 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 X Dist. 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	432081	423407	320222	311529	214932	206282
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 equation 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.

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 Administrative Act by increasing their hiring of people with disabilities. We show this in Figure 4, where we reproduce the estimations shown in Figure 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 sub-sample is remarkably similar to the one considering the whole sample of firms.

Figure 4: The 2012 Administrative Act and Workers with Disabilities
Sub-sample: Firms that Never Received a QL Fine



Note: This figure presents estimation from a model similar to equation 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. 90% confidence interval shown in the graphs.

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 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;[19] 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 A4 in the Appendix shows that the occurrence of a fine in firms' networks already seems

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

quite exogenous, even if we consider 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 A4 (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. First, we take the networks where the QL fine happened at any time after 2012.[20] 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 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:

$$\begin{aligned} Y_{imct} = \delta_0 Treated_{imct} + \sum_{\substack{\tau \neq -1 \\ \tau=-3}}^{\tau=2} D_t^\tau + \sum_{\substack{\tau \neq -1 \\ \tau=-3}}^{\tau=2} \delta_\tau (Treated_{imct} \times D_t^\tau) \\ + \theta_m + \theta_t + \theta_c + \theta_{mct} + \epsilon_{imct} \end{aligned} \tag{2}$$

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. As in equation 1, we include controls for time and municipality fixed effects and the interaction between these two terms (θ_t and θ_m, and θ_{mt} respectively) to control for fixed characteristics of the local labor market which could be correlated with the likelihood of observing a fine, and the presence of labor regulatory offices. Besides, we

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

also include cohort fixed effects θ_c to control for other shocks happening simultaneously to the QL fine.

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 sample of all firms larger than 100 workers and second separately for the sub-sample of firms that were at least partially complying with the QL before the QL fine hit their network and the sub-sample of firms that were not complying with the QL at that time.[21] We hypothesize that firms already complying with the law should not be affected by the information about enforcing such a law in their networks, while non-compliant firms should be the most impacted by the new information.

First, firms react to a QL fine in their network by hiring more workers with disabilities, regardless of the network where such a fine happens. We see such a pattern in Figure 5 that presents the results for estimations considering all firms larger than 100 workers for the event of a QL fine in the firm's neighbor network (Figure 5a), owner network (Figure 5b), and HR workers network (Figure 5c). In all cases, we observe an increase in the number of workers with a disability in the firm after the event of a QL fine in their network. That increase is persistent, and it grows with time in the neighbor and the owner networks while it fades out in the HR workers' network.

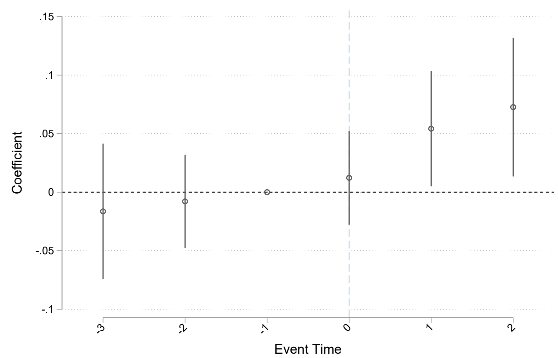
Second, only firms that risk receiving a QL fine – that is, those not complying with the law – react to the occurrence of a fine in their networks. Figure 6 presents such a result. Overall, we see that the positive effect of a QL fine in the firm's network on its number of workers with a disability is concentrated in firms that were not complying with the QL before that event (figures 6a, 6c, and 6e). The result is imprecisely estimated for the HR workers' network (Figure 6e), such that they are not significantly different from zero, even though we observe an increase in the coefficients' size. In turn, firms that were already at least partially complying with the QL do not change their behavior at all when exposed to the event of a QL fine (Figures 6b, 6d, and 6f).[22]

[21] Since fully complying with the QL is a rare event, we consider that a firm partially complies with the law if it has at least 50% of the number of workers with a disability that it should have according to the quota.

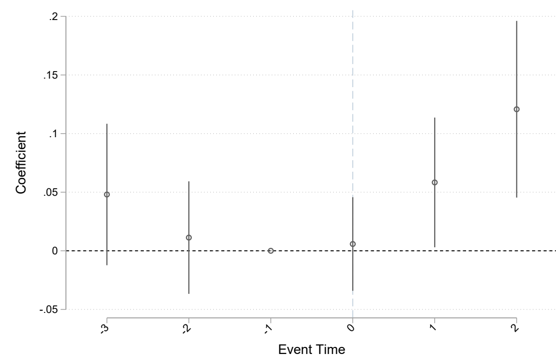
[22] We employ the methodology proposed by Rambachan and Roth (2023) to assess the sensitivity to parallel trends violations in Figure A4 (Appendix A). Specifically, we allow for differences in linear trends between treated and not-yet-treated and quantify how large any departures from such linearity 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 owner network is 0.3, and results are already imprecise in the worker network, even for $M = 0$. If we consider non-complier

Third, one could hypothesize that, after fining 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 in Figure 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 equation 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.

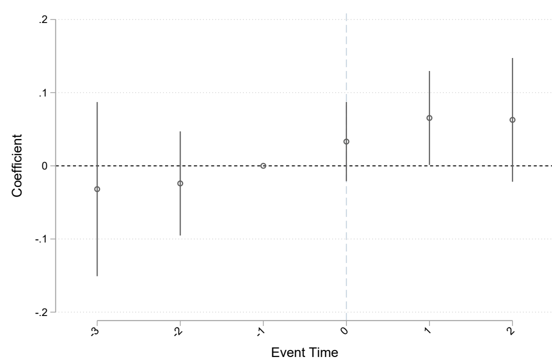
Figure 5: Law Enforcement at Firm Networks and the Number of Workers with a Disability



(a) Neighbor Network



(b) Owner Network



(c) HR Workers Network

Note: These graphs present estimations from equation 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 that 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 cohort and city-by-year fixed effects. 90% confidence interval shown in the graphs.

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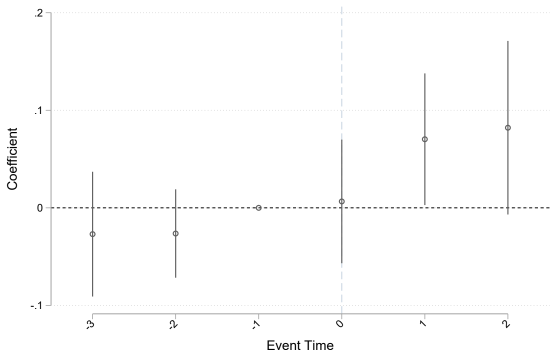
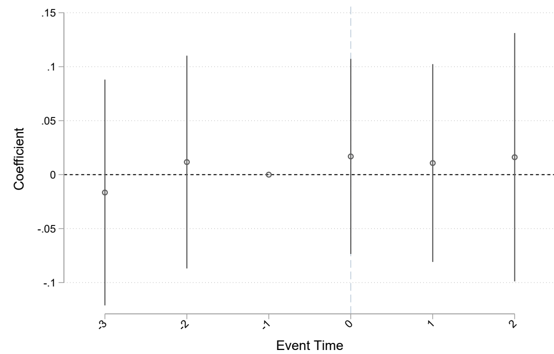
firms, we do not nullify the results for the neighbor and owner network even for values of M and large as $M = 1$, and the breakdown value of M is 0.8 in the worker network.

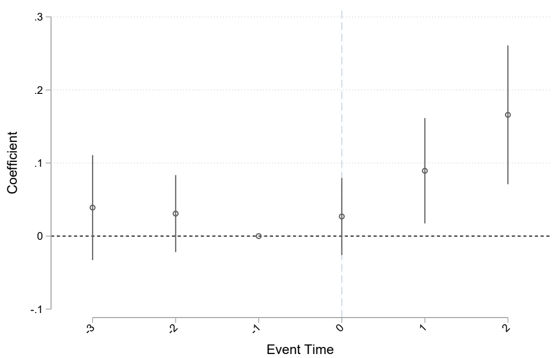
Figure 6: Heterogeneity by Compliance with the QL in $t-1$



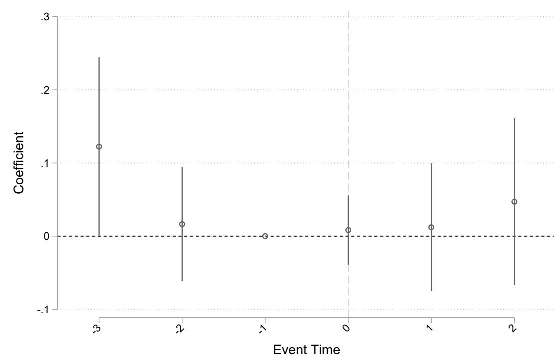
(a) Neighbor NW: Non-compliers



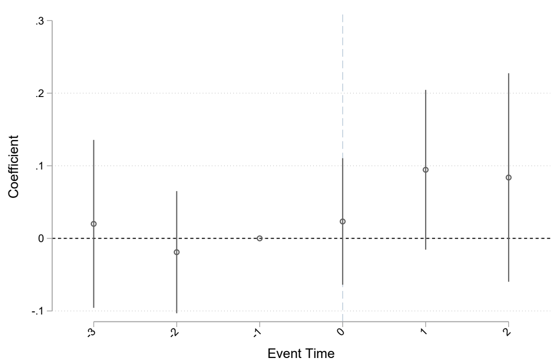
(b) Neighbor NW: Partial Compliers



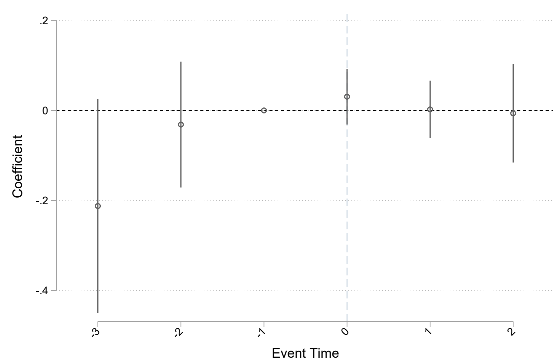
(c) Owner NW: Non-compliers



(d) Owner NW: Partial Compliers



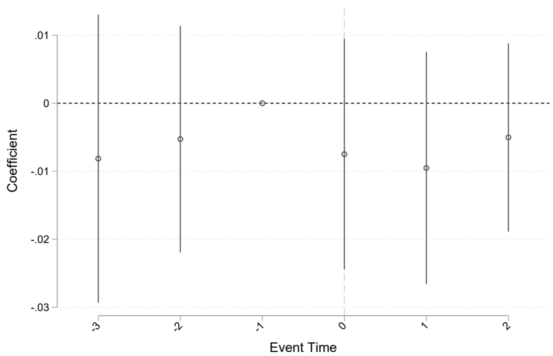
(e) HR Workers NW: Non-compliers



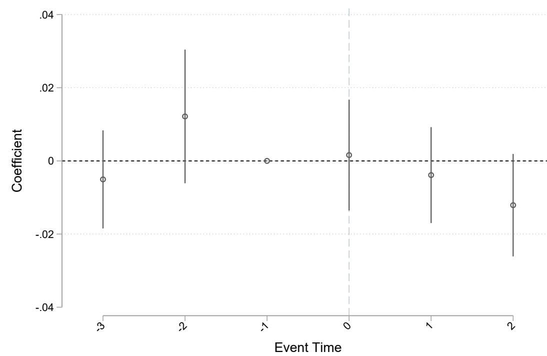
(f) HR Workers NW: Partial Compliers

Note: These graphs present estimations from equation 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 $t = 0$. "Partial Compliers" are firms that had at least 50% of the number of workers with a disability that it should have according to the quota. "Event Time" is the time after the occurrence of the QL fine in the firm's network. All estimations include cohort and city-by-year fixed effects. 90% confidence interval shown in the graphs.

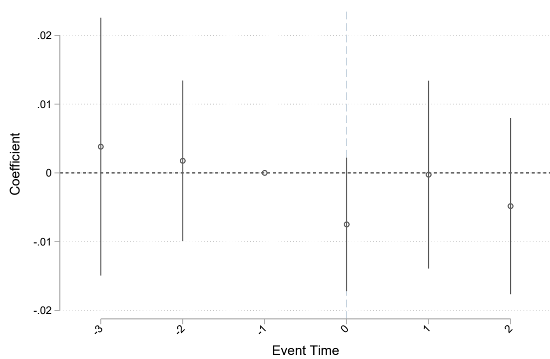
Figure 7: Law Enforcement at Firm Networks and Inspection Likelihood



(a) Neighbor Network



(b) Owner Network



(c) HR workers Network

Note: These graphs present estimations from Equation 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. "Partial Compliers" are firms that had at least 50% of the number of workers with a disability that it should have according to the quota. All estimations include cohort and city-by-year fixed effects. 90% confidence interval shown in the graphs.

We summarize all these results in Table 4, where we substitute the event-time dummies with a binary variable indicating the post-event periods. Overall, these results show that firms react when exposed to a QL fine by hiring more workers with disability. The table also shows that firms' response is larger and better estimated at the neighbor network and the owner network, while the evidence is noisier for the HR workers' network. Moreover, since we do not observe an increase in the firm's likelihood of receiving an inspection after a QL fine event, communication through the firm's networks is likely the mechanism behind the increase in the number of workers with a disability.

Table 4: Law Enforcement at Firm's Networks

	Workers w/ a disability (hyp. sine trans.)			Likelihood of inspection		
	(1)	(2)	(3)	(4)	(5)	(6)
	All firms	Non-compliers	Partial compliers	All firms	Non-compliers	Partial compliers
Panel A: Neighbor network						
Post-event X Treated	0.073***	0.116***	0.017	-0.002	-0.004	-0.001
	(0.016)	(0.023)	(0.028)	(0.003)	(0.003)	(0.004)
N	118160	70940	42603	118160	70940	42603
N (firms)	12248	7861	5610	12248	7861	5610
Avg. firm size	368.404	385.448	351.608	368.404	385.448	351.608
Mean Dep. Var.	5.579	2.607	10.739	5.579	2.607	10.739
Elasticity	0.075	0.123	0.017	-0.002	-0.004	-0.001
R2	0.156	0.189	0.180	0.125	0.158	0.160
Panel B: Owner network						
Post-event X Treated	0.068**	0.096***	-0.004	-0.008	-0.021***	0.001
	(0.030)	(0.036)	(0.055)	(0.005)	(0.007)	(0.007)
N	37049	20846	13948	37049	20846	13948
N (firms)	3794	2322	1729	3794	2322	1729
Avg. firm size	485.848	499.083	492.306	485.848	499.083	492.306
Mean Dep. Var.	8.649	3.983	16.025	8.649	3.983	16.025
Elasticity	0.070	0.101	-0.004	-0.008	-0.021	0.001
R2	0.226	0.285	0.217	0.205	0.259	0.199
Panel C: HR workers network						
Post-event X Treated	0.045*	0.090**	-0.004	0.005	0.001	0.006
	(0.026)	(0.038)	(0.032)	(0.004)	(0.005)	(0.006)
N	37237	22122	13385	37237	22122	13385
N (firms)	3671	2415	1615	3671	2415	1615
Avg. firm size	513.923	575.726	419.657	513.923	575.726	419.657
Mean Dep. Var.	7.698	4.692	12.547	7.698	4.692	12.547
Elasticity	0.046	0.094	-0.004	0.005	0.001	0.006
R2	0.227	0.265	0.249	0.183	0.206	0.208

Note: This table presents estimations from a model similar to equation 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 $t = 0$. "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 cohort and city-by-year fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city level shown in parentheses.

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 A5 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. Table A6 in the Appendix presents robustness checks implementing Callaway and Sant'Anna (2021). As we can see, the results are remarkably similar to our benchmark estimates.

Finally, we further test the plausibility of our identification assumptions by showing that firms with less than 100 workers do not react to the enforcement of the Quota Law in firms with more than 100 workers present in their networks (see Table A7, 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.

To put our estimates in context, we perform a back-of-the-envelope calculation with the estimates in Table 4 (considering Column (1), with all firms). For the neighbor network, we estimate an increase of 7.5% in the hiring of workers with disabilities after the occurrence of a QL fine in the network. Given the baseline average number of workers with disabilities (5.579), this represents an increase of 0.4 workers with disabilities per firm. On average, a firm that receives a QL fine hires two new workers with disability. We use this figure as an estimate of the specific deterrence effect, that is, the direct effect of receiving a QL fine.[23] Conditional on having at least one firm larger than 100 workers in the neighbor network, we have, on average, 11 firms larger than 100 workers in these networks (including the firm that was fined). Hence, each QL fine leads, on average, to hiring two workers with disabilities due to direct law enforcement and four (10*0.4) workers with disabilities due to enforcement spillovers in the network.[24] In other words, the spillover effects from a QL fine increase the number of workers with disabilities twice as much as the specific deterrence effects (i.e., the direct effects from the QL fine occurrence in the firm).

Similar calculations show that 0.6 workers with disabilities are hired due to enforcement spillovers in the owner network (or an impact 30% as large as the specific deterrence effects);[25]

[23]This number should be interpreted cautiously because it does not represent a causal effect. Since the timing of a QL fine event is endogenous, we cannot perform an event study such as the ones we performed to estimate the spillover effects coming from the occurrence of a QL fine in the firm's network.

[24]We always subtract one firm from our calculation to consider the firm that received the QL fine, i.e., direct law enforcement.

[25]For the owner network, we see an increase of 7% in the hiring of workers with disabilities after the

and 4.7 workers with disabilities are hired due to enforcement spillovers in the HR workers network (or an impact 2.4 times as large as the specific deterrence effects).[26] The total spillover effects in the owner network are quite small compared to the neighbor and HR workers' networks. This is because the owner network is usually smaller than the neighbor and HR workers' networks, limiting the spread of enforcement spillover.

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 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

We provide evidence of how information regarding law enforcement disseminates across various firms' networks, resulting in the emergence of enforcement spillovers. Specifically, we examine the Brazilian quota system for persons with disabilities. Our findings demonstrate that intensifying the enforcement of this regulation led firms to enhance their hiring of individuals with disabilities, even among firms that were neither inspected nor fined. Notably, these spillover effects occur when a Quota Law fine is imposed within firms' neighboring, ownership, and HR workers' networks.

Our findings carry significant policy implications. First, while enforcing labor laws can be expensive, particularly for developing countries, our research demonstrates that the returns from enforcement surpass those initially assumed when considering only the targeted firms. Therefore, it is crucial to account for spillover effects when assessing the cost-effectiveness

occurrence of a QL fine in the network. Given the baseline average number of workers with disabilities (8.649), this represents an increase of 0.6 workers with disabilities per firm. Conditional on having at least one firm larger than 100 workers in the owner network, we have, on average, two firms larger than 100 workers in such networks. Hence, each QL fine leads, on average, to hiring 0.6 (1*0.6) workers with disabilities due to enforcement spillovers in the network.

[26] For the HR worker network, we see an increase of 4.6% in the hiring of workers with disabilities after the occurrence of a QL fine in the network. Given the baseline average number of workers with disabilities (7.698), this represents an increase of 0.35 workers with disabilities per firm. Conditional on having at least one firm larger than 100 workers in the HR worker network, we have, on average, 14.5 firms larger than 100 workers in such networks. Hence, on average, each QL fine leads to hiring 4.7 (13.5*0.35) workers with disabilities due to enforcement spillovers in the network.

of enforcement efforts. Second, given that neighbor networks are readily identifiable and yield the most substantial spillover effects, labor inspectors should prioritize firms situated in areas of higher firm density to maximize the impact of each enforcement endeavor.

An important caveat in interpreting our results is that we do not observe negative externalities of enforcement (see Evans et al., 2018). The 2012 Administrative Act did not result in firm closures, reductions in firms' total wage bills, or increased turnover rates. Future studies should explore whether law enforcement in other regulatory contexts would yield similar spillover effects.

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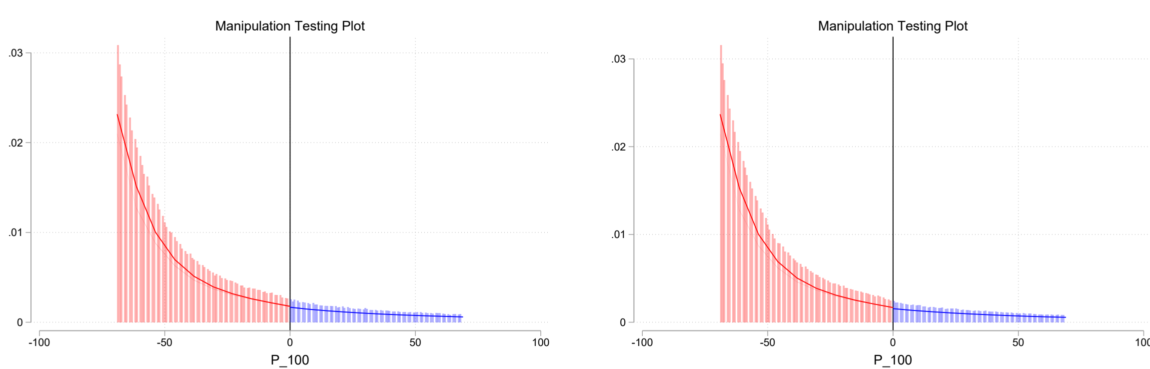
A Appendix

Table A1: 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	991510	991510	991510	991510	991510
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	1593962	1593962	1593962	1593962	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.

Figure A1: Manipulation test



(a) Before 2012: T stat= -0.4719 ; P>|T|=0.6370 (b) After 2012: T stat=-2.7434 ; P>|T|=0.0061

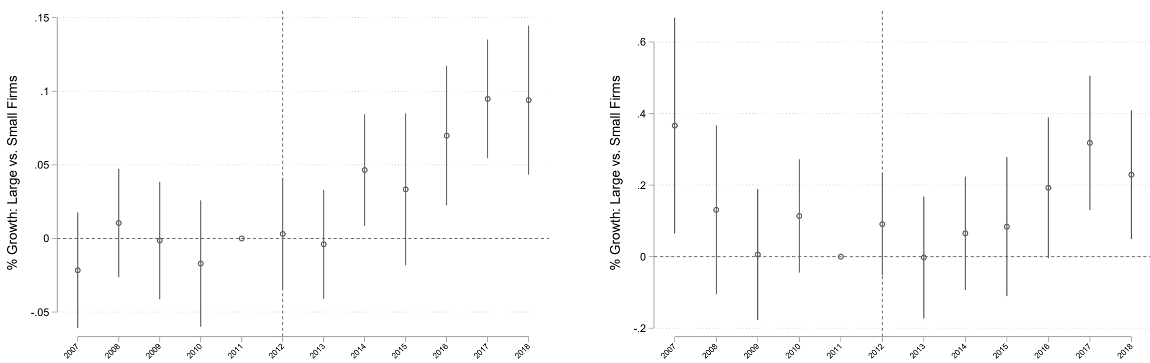
See Cattaneo et al. (2018) for details about the implementation of manipulation tests.

Table A2: 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>2012X Distance to QL threshold>0	0.049***	0.062***	0.052	0.042
	(0.009)	(0.011)	(0.070)	(0.097)
N	282594	265016	282594	265016
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 equation 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.

Figure A2: The 2012 Administrative Act and Presence of Workers with Disability: Robustness Checks



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

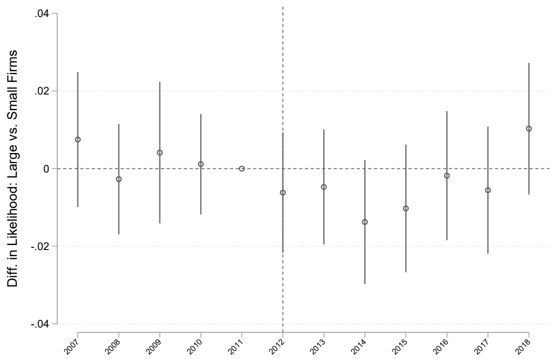
Note: These graphs present estimations from a model similar to equation 1, where we substitute the post-2012 dummy T with year dummies, leaving 2011 as the benchmark. We consider a donut ring of two in the firm-size variable. All estimations include city-by-year fixed effects. 90% confidence interval shown in the graphs.

Table A3: The 2012 Administrative Act and Other Firm Outcomes

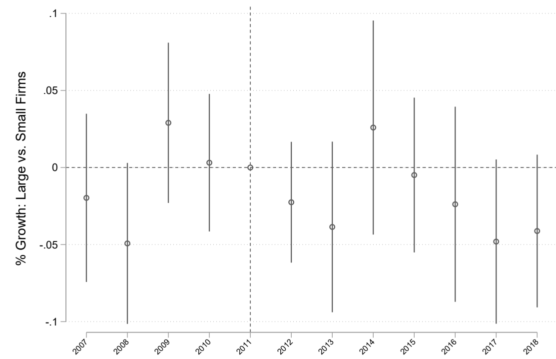
	Non-QL fines	Total wage bill	Turnover rate	Firm closure	Wage workers w/ disability (ln)
	(1)	(2)	(3)	(4)	(5)
Year>2012 X Distance to QL threshold>0	-0.007*	-0.015	-0.002	-0.003	-0.056
	(0.003)	(0.013)	(0.003)	(0.005)	(0.038)
N	641525	298207	426112	357065	113520
Mean Dep. Var.	0.125	12.051	0.314	0.035	7.952
h (left)	45.211	22.223	24.076	13.552	30.213
h (right)	185.839	82.449	246.241	277.317	126.129
R2	0.090	0.440	0.102	0.061	0.267
Donut ring	2	2	2	2	2

Note: This table presents estimations from equation 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.

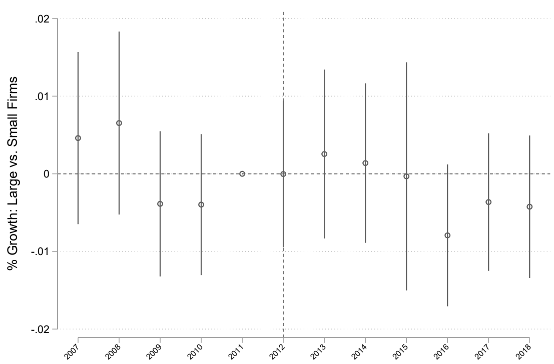
Figure A3: The 2012 Administrative Act and other Firm Outcomes



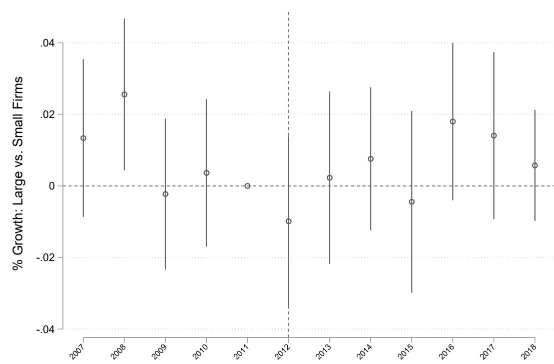
(a) Non-QL Fines



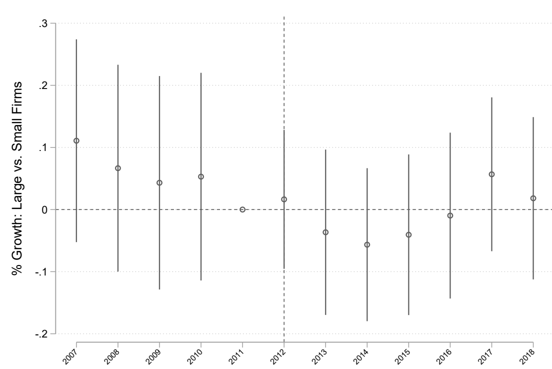
(b) Total Wage Bill



(c) Turnover Rate



(d) Firm Closure



(e) Wage Workers with disability (ln)

Note: These graphs present estimations from a model similar to equation 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. All estimations include city-by-year fixed effects. 90% confidence interval shown in the graphs.

Table A4: 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	-0.000	0.000	0.001	0.001	-0.002*	-0.004*	0.001	0.000	-0.001	-0.003	0.000	-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	-0.000	-0.002	0.002	0.003	-0.000	-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	22744	9926	23222	10033	22962	9927	21249	9286	19892	8717	19822	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	0.000	-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***	0.000	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	22744	3719	23222	3654	22962	3564	21249	3361	19892	3166	19822	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	-0.000	-0.003	0.001	0.001	0.000	-0.002	0.000	-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***	0.000	-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	22744	4332	23222	4559	22962	4652	21249	4426	19892	4277	19822	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 A5: Law Enforcement at Firm's Networks: Robustness Checks

	One or more worker w/ disability			Number of workers w/ disability		
	(1)	(2)	(3)	(4)	(5)	(6)
	All firms	Non-compliers	Partially compliers	All firms	Non-compliers	Partially compliers
Panel A: Neighbor network						
Post-event X Treated	0.023***	0.038***	-0.001	0.432	0.719***	0.046
	(0.007)	(0.010)	(0.010)	(0.508)	(0.275)	(1.399)
N	118160	70940	42603	118160	70940	42603
N (firms)	12248.000	7861.000	5610.000	12248.000	7861.000	5610.000
Avg. firm size	368.404	385.448	351.608	368.404	385.448	351.608
Mean Dep. Var.	5.579	2.607	10.739	5.579	2.607	10.739
R2	0.172	0.193	0.193	0.044	0.101	0.082
Panel B: Owner network						
Post-event X Treated	0.014	0.032*	-0.019	1.279**	1.063**	1.145
	(0.013)	(0.019)	(0.011)	(0.643)	(0.434)	(1.621)
N	37049	20846	13948	37049	20846	13948
N (firms)	3794.000	2322.000	1729.000	3794.000	2322.000	1729.000
Avg. firm size	485.848	499.083	492.306	485.848	499.083	492.306
Mean Dep. Var.	8.649	3.983	16.025	8.649	3.983	16.025
R2	0.254	0.287	0.216	0.140	0.195	0.173
Panel C: HR workers network						
Post-event X Treated	0.003	0.011	-0.003	0.552	1.106**	0.078
	(0.010)	(0.015)	(0.013)	(0.442)	(0.523)	(0.760)
N	37237	22122	13385	37237	22122	13385
N (firms)	3671.000	2415.000	1615.000	3671.000	2415.000	1615.000
Avg. firm size	513.923	575.726	419.657	513.923	575.726	419.657
Mean Dep. Var.	7.698	4.692	12.547	7.698	4.692	12.547
R2	0.218	0.252	0.203	0.218	0.301	0.273

Note: This table presents estimations from a model similar to equation 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 an inspection or any type of fine in $t = 0$. "Treated" is an indicator that some type of law enforcement took place in the firm owner's network at $t = 0$. Elasticities of workers with disabilities are calculated based on Bellemare and Wichman (2020). All estimations include cohort and city-by-year fixed effects. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$. Standard errors clustered at the city level shown in parentheses.

Table A6: QL Fine in Firm's Network: Robustness Check Using Callaway and Sant'Anna (2021)

	Neighbor network	Onwer network	HR workers network
	(1)	(2)	(3)
Pre_avg	-0.022	-0.038	0.011
	(0.030)	(0.030)	(0.022)
Post_avg	0.104***	0.154***	0.102***
	(0.027)	(0.038)	(0.031)
Tm3	-0.067	-0.090	0.011
	(0.067)	(0.081)	(0.066)
Tm2	-0.017	-0.032	0.008
	(0.032)	(0.047)	(0.039)
Tm1	0.019	0.008	0.013
	(0.023)	(0.032)	(0.028)
Tp0	0.050***	0.073***	0.047**
	(0.019)	(0.027)	(0.023)
Tp1	0.125***	0.158***	0.108***
	(0.028)	(0.044)	(0.034)
Tp2	0.138***	0.233***	0.150***
	(0.046)	(0.057)	(0.050)
N	24477	9887	10439

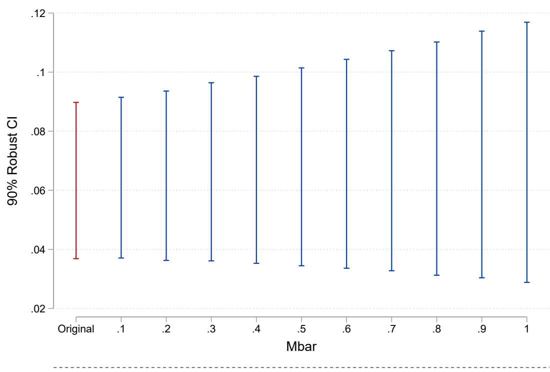
Note: This table presents estimations from equation 2, but using the method proposed by Callaway and Sant'Anna (2021) instead of the stacked differences-in-differences used in our main estimations. All estimations include city-by-year fixed effects. Significance levels are indicated by $* < .1$, $**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

Table A7: Law Enforcement at Firm's Networks: Placebo with Firms Smaller than 100

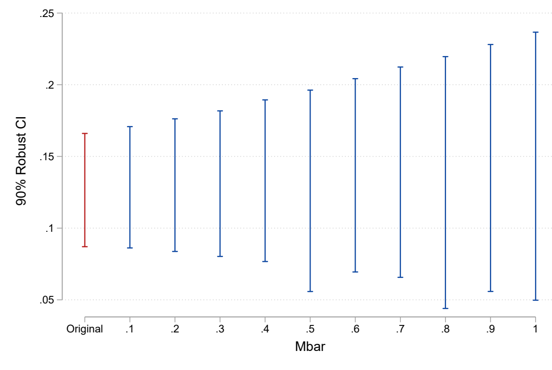
	Neighbor network	Owner network	HR workers network
	(1)	(2)	(3)
Post-event X Treated	0.000	-0.006	-0.014
	(0.002)	(0.010)	(0.012)
N	599867	69379	29689
N (firms)	70133	7952	3614
Avg. firm size	41.717	49.181	54.135
Mean Dep. Var.	0.097	0.167	0.227
Elasticity	0.000	-0.006	-0.013
R2	0.045	0.164	0.194

Note: This table presents estimations from equation 1 focusing on a sample of firms smaller than 100 workers, instead of firms larger than 100, as in our main estimations. 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.

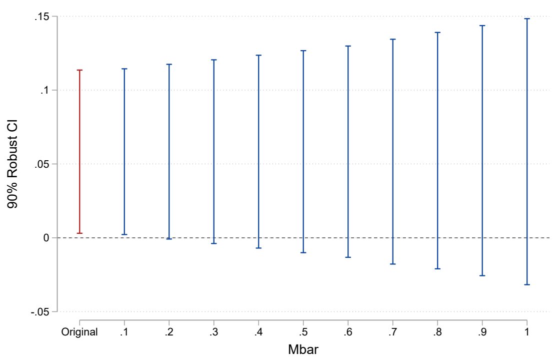
Figure A4: Pre-trend Robustness



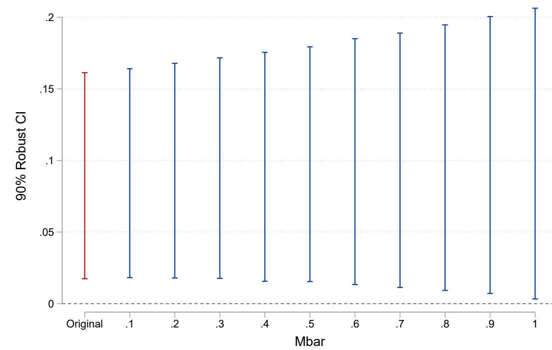
(a) Neighbor NW: All Firms



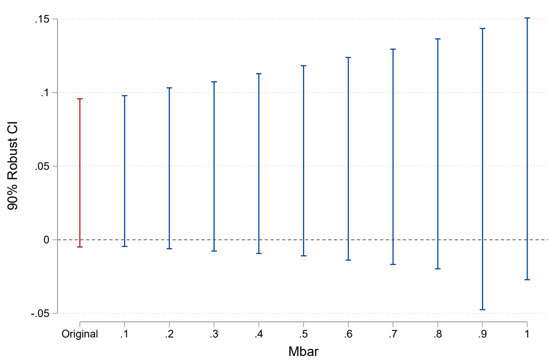
(b) Neighbor NW: Non-compliers



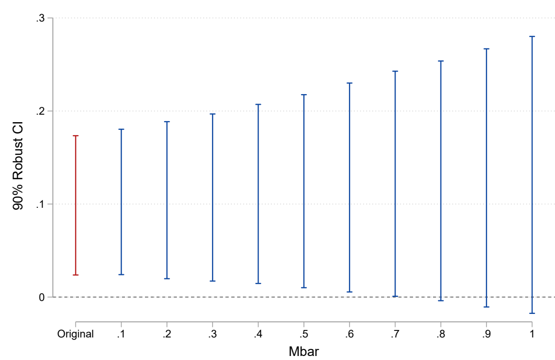
(c) Owner NW: All Firms



(d) Owner NW: Non-compliers



(e) HR workers NW: All Firms



(f) HR workers NW: Non-compliers

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.