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Preliminary Version

Abstract

This paper investigates informal employment in Brazil's highly regulated labor market, focusing on the intensive margin of informality within formal firms. Using a comprehensive dataset of labor audits conducted from 1997 to 2012, we find that formal firms caught with informal workers face sustained slower growth. Informal workers are found across firms of all sizes, and their characteristics closely resemble those of formal employees. Building on these empirical findings, we develop a dynamic general equilibrium model where firms balance the flexibility of informality against potential costs. Our framework can be used to explore government policy implications and to examine the impact of audit strategies on informality, output, and workers' welfare.

JEL Codes: H2, J1, J2, L1.

Keywords: Informality, labor market regulation, firm dynamics, developing countries.

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1 Introduction

The informal sector accounts for a significant fraction of economic activity in developing countries, where from 20 to 80% of workers and firms operate informally (see Ulyssea (2020) and La Porta and Shleifer (2014) for recent reviews). Despite a growing body of literature, the role of informality and whether and how governments should reduce it remain controversial.

Informal firms do not comply with regulations. They hire workers without registering them in the social security system. The share of workers who work in formal firms but off the books, the so-called intensive margin of informality, is substantial. They account for 40% of informal employment in Brazil and 56% in Mexico (Ulyssea, 2020). Furthermore, productivity distributions of formal and informal firms overlap (Meghir et al. (2015), Allen et al. (2018), Ulyssea (2018)), challenging the classical dualistic view of informality (Rauch, 1991).

Governments in many developing countries impose several forms of regulation in labor markets: payroll taxes, firing costs, etc. From a firm's perspective, informality provides a way to overcome excess regulation in developing countries and contribute to economic dynamism and growth by keeping active firms that would be discouraged otherwise (De Soto, 1989, 2000).

In this paper, we study how the government should monitor (audit) and punish informality, given potential costs (lost tax revenue) and benefits (greater flexibility). To answer this question, we focus on Brazil, a country with a large informal sector and highly regulated labor market, and exploit a unique data set of the universe of audits by the Ministry of Labor between 1997 and 2012.

In Brazil, formal workers are those with an employment contract that is registered into their Labor and Social Security booklet, which records their employment history. Similarly, formal firms have a tax identification number. These firms incur registration costs (time and money), pay payroll taxes, and face firing costs. All workers employed by an informal firm are informal by default. A formal firm, however, can have informal workers, i.e., hire workers without a formal contract. The Ministry of Labor is in charge of auditing formal firms to ensure that they follow the existing labor laws. These labor inspections may happen in all firms without prior notice.

When auditing a firm, inspectors must verify that every worker has a legal con-

tract. Firms caught hiring informal workers must (1) register these workers and (2) pay a penalty. The increase in firm size after an inspection tells us the number of informal workers hired by the formal firms (the intensive margin). If a firm is caught hiring informal workers, it pays a fixed penalty equivalent to the value of one monthly minimum wage for each informal worker. Furthermore, the firm pays another fee equal to three-month salaries plus the payroll taxes of informal workers. Since payroll taxes are very significant, between 68% to 97% of wages, the firm may end up paying five- or six-month salaries (plus the fixed fine), a significant share of total revenue. In the data set, we have detailed information on the outcomes of each audit: no infringement of any law versus infringement due to hiring workers informally. The sample consists of a balanced panel with 381,714 establishments that existed every year between 1996 and 2012. Labor inspection is not rare for those establishments: 209,812 were audited, and 111,368 were caught with at least one informal worker.

Most importantly, for our purposes, we link the data on audits to the universe of the matched employee-employer dataset, RAIS (Relação Anual de Informações Sociais). RAIS follows every formal firm and every formal worker. As a result, we can follow the audited firms before and after their audit.

Several results emerge from the data. First, a successful audit has a sharp and long-lasting effect on firm dynamics. When a firm is caught with informal workers, its growth rate stalls for several years. That is, being caught with informal workers is associated with sustained slower firm growth. This result appears both in descriptive analyses as well as via estimations using state-of-the-art staggered difference-in-difference techniques (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021). Second, informal workers are employed by firms of all sizes. This is not a phenomenon restricted to small firms. Though smaller firms (with less than 10 employees) caught by audits can have 40% or more of their workers off the books, large firms (with 500 employees or more) employ close to 10% of their workforce as informal workers. Third, formal and informal workers exhibit almost identical characteristics. Both groups have similar ages, education levels, and gender compositions. Informal workers earn slightly less. Hence, this is not a story about different types of workers performing different jobs. Formal and informal workers within the same firm are very much alike.

Motivated by these facts, we build and estimate a model economy with for-

mal and informal workers and government audits. Understanding informality requires models where three key agents (firms, workers, and governments) interact in dynamic general equilibrium environments. While a large body of empirical research exists on informality, theoretical models that can be confronted with data fall behind.

We consider an economy populated by heterogeneous firms and homogeneous households. As in Melitz (2003) and Ulyssea (2018), each firm is indexed by a constant productivity level as long as it is active. Labor is the only input for production. Each period, firms decide how many workers to hire. Hiring workers is subject to adjustment costs, given by an increasing and convex function (Merz and Yashiv, 2007; Cooper and Willis, 2009).

Upon hiring a worker, firms decide whether to have them in their books as formal workers or keep them hidden as informal. Why do firms want to employ informal workers? From time to time, firms are faced with growth opportunities. Imagine, for example, a firm might try to enter a new market or develop a new product line. Such growth opportunities arrive stochastically. When a growth opportunity comes, the firm has to allocate workers for this growth (or experimentation) activity. The outcomes of these activities are, however, uncertain. Hence, there is a positive probability that although the firm hires workers to allocate to growth activities, the attempt fails. In that scenario, the firm is left with these extra workers without any change in its growth prospects. As a result, the firm wants to hire at least some of these workers informally. If the experimentation does not work, the firm can fire them, avoiding payroll taxes and firing costs. For informal workers, the firm avoids these costs. The firms weigh this strategy against the probability of being audited and, as a result, have to formalize all their workers and pay the penalty. Furthermore, if an inspection increases the likelihood of future inspections, the firm can be stuck on a no-growth path. Firms trade off the flexibility that informality provides against its potential costs.

Labor markets are competitive, and all workers, formal or informal, are paid the same wage. Hence, we follow the recent literature, e.g., Ulyssea (2018), abstract from non-wage benefits of formal contracts and focus on the firms.

What is the role of government policy in this economy? The government (the regulator) decides on two objects. First, it controls the function that maps firm characteristics (size, growth, and previous audits) into an audit probability. Sec-

ond, the government decides the size of the fine it imposes on firms. Our strategy is to characterize these objects for the benchmark economy, using very detailed observations on audits and their effects on firms. We will then consider alternatives to the existing policies. Should the government target specific firms (large vs. small) or have a non-targeted policy? Should previous records be forgotten or considered for audit decisions? What are the effects of these policies on informality, output, and workers' welfare?

This paper builds on three literatures. The first is the extensive literature on models of the informal economy. See, among others, Amaral and Quintin (2006), de Paula and Scheinkman (2010), Meghir et al. (2015), Ulyssea (2018), Erosa et al. (2023) Cisneros and Ruggieri (2023). The second is the empirical studies on the effects of government policies (lower entry costs, lower regulation costs, or stricter enforcement) on informality. Ulyssea (2010), Almeida and Carneiro (2012), de Mel et al. (2013), and Rocha et al. (2018) are examples from this literature. Within this line, de Andrade et al. (2014) find that stricter enforcement has a large effect on informality. Finally, we bridge these literatures to the large public finance literature on tax evasion and optimal enforcement policies. See Slemrod and Yitzhaki (2002) and Slemrod (2007) for reviews, and Gordon and Li (2009), Kotsogiannis and Mateos-Planas (2019), and Kleven et al. (2011) for recent theoretical and empirical contributions. While these study the role of enforcement in models of tax evasion, our goal is to characterize the impacts of alternative enforcement policies within a quantitative model of informality.

The rest of this paper is organized as follows. Section 2 describes the Brazilian context and the data sets we use. Section 3 discusses our empirical analyses. Section 4 describes a simplified version our framework and Section 5 describes the full model. Finally, Section 6 concludes.

2 Background and Data

2.1 Informality and Labor Inspections

Informality is widespread in Brazil's labor market: informal workers represent a significant proportion of the labor force. Formal workers have an employment contract ("registered wage-earner") and are eligible for benefits such as social security, unemployment insurance, and severance payment. Informal workers

do not have employment contracts nor access to such benefits. In turn, firms incur higher costs when hiring formal workers. These costs include payroll taxes, which correspond to 67.22% or 95.22% of formal workers' wages depending on the labor contract regime (monthly salary or hourly wage, respectively), and firing costs.

One of Brazil's main instruments to enforce the use of formal labor contracts is labor inspection. Labor inspections—regulated by Federal Decree n.4,552/02—may happen in all firms, regardless of sector or size. Labor inspectors—civil servants of Brazil's Ministry of Labor—ensure the application of legal provisions to protect workers in the exercise of labor activity. Labor inspectors have the legal right to enter freely, without prior notice, and on any day and time. When auditing a firm, inspectors must verify compliance with legal and regulatory provisions and verify if every worker is a registered wage earner. Labor inspections may happen because of whistle-blowing or the visit can be defined randomly. In practice, the level of whistle-blowing is high enough such that most inspections are defined by authorities as being informed by others (Cardoso and Lage, 2005).

Establishments caught hiring informal workers must (i) register these workers and (ii) pay fines. There is a fixed fine equivalent to the value of one national monthly minimum wage for each informal worker caught by the inspectors. Besides, firms pay another fine equal to three months salaries plus the payroll taxes that the firm should have been paying during these three months. Since payroll taxes correspond between 68% and 97% of the worker's salary, the firm may end up paying five- or six-month salaries (plus the fixed fine). As a result, the penalty may represent a significant share of total revenue for firms.

Appendix Figure A-1 shows that thousands of establishments are audited and caught with informal workers every year.

2.2 Data

To study the impacts of labor inspections on firms, we use three administrative registries. First, data on employment and other characteristics come from RAIS ("Relação Anual de Informações Sociais"), a matched employee-employer dataset from Brazil's Ministry of Labor. All establishments in Brazil are legally required to submit information to RAIS, so the dataset has information on each

formal worker at each formal establishment in Brazil. Workers and establishment in RAIS are identified with a unique time-invariant identifier, so we can follow them over time. In addition, RAIS provides the exact date of the start and end of each formal worker's contract.

The second data source is the microdata on the universe of labor inspections in Brazil. We use the subgroup of inspections related to informal labor inspections. We have detailed results for each inspection, including the start and end dates and the number of informal workers caught at each establishment (if any were found by inspectors). The labor inspections registry does not provide the ID or name of those informal workers, but we are able to pinpoint those workers by using RAIS. According to the rules governing labor inspections, establishments must formally hire workers after the inspection. We leverage detailed information on (i) the exact labor inspection's start and end dates and (2) the exact timing of the hiring of workers to track and find those (previously informal) workers who were hired because of the inspections.

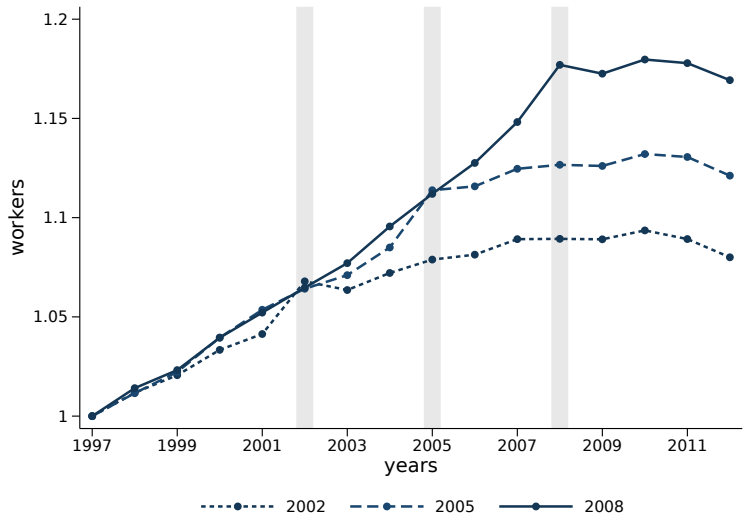
Third, we use the microdata on patent and trademark registration from Brazil's Patent and Trademark Office ("Instituto Nacional de Propriedade Industrial"). The registration dataset provides information on the year of the trademark, innovation patent, industrial design, and utility model registration for each firm in Brazil.

Our period of analysis is 1997–2012 and was chosen due to data constraints—it is the only period where we are able to collect data for the three administrative registries. Our sample consists of a balanced panel with establishments that existed every year between 1996 and 2012 and that were audited at least once in this time frame. A Ministry of Labor audit is not a rare event for those establishments: 209,812 were audited, and 111,368 were caught with at least one informal worker. In the empirical analysis, we work with this sample of 111,368 plants.

3 Empirical Motivation

In this section, we present three facts that motivate our analyses: (i) labor inspections slow down firm's employment growth, (ii) informal workers are found in plants of all sizes, and (iii) formal and informal workers are compara-

Figure 1: Number of Workers and Inspections: Examining the Raw Data



Notes. This figure shows the evolution in the number of workers for three groups of plants: those caught with informal workers in 2002, 2005, and 2008. Vertical bars denote the year in which the plants were inspected. The number of workers was normalized to one in 1997.

ble in many observable characteristics.

We start by examining the effect of labor inspection on plant size. Since the rules governing inspections in Brazil require that establishments detected with informal workers must register all workers, we expect to observe a mechanical increase in the number of formal workers in these companies immediately after the inspection. Over time, establishments will re-optimize, and we assess empirically what happens with these establishments.

Figure 1 shows the evolution in the number of workers for plants inspected and caught with informal workers. The figure focuses on three cohorts: plants inspected and caught with informal workers in 2002, 2005, and 2008. Plant size increases in a similar way for firms of every cohort before they are caught in an inspection. However, after being caught, firms seem to stop growing. Appendix Figure A-2 shows that this pattern is not unique for the three cohorts in Figure 1 but is observed in every single year of our period of analysis. Overall, this descriptive analysis shows an association between being audited and caught with informal workers and growing less. Besides, there is an increase in the number of formal workers in the year of the audit, which is the mechanical effect consistent with the fact that plants must formally hire workers detected as working informally.

To formally check the relationship between inspections and plant size, we use a staggered difference-in-differences design. There are relevant characteristics that select plants into inspections, so we use the staggered design to explore the timing of inspections in the identification strategy. Again, we conduct this analysis using the sample of 111,368 plants that were caught with at least one informal worker between 1997 and 2012.

An establishment is considered "treated" after an infraction is verified; before the inspection, the establishment is considered as "not-yet treated". Since there is heterogeneity in treatment timing across firms, we follow Sun and Abraham (2021) and estimate an event-study specification that takes into account heterogeneous treatment effects across units treated in different years. The comparison group is the group of plants treated in the last year of the period of analysis and that are thus considered not-yet-treated for all other years. The identifying assumption is that in the period of analysis, plants with inspections would have had similar trends in employment compared to the control group in the absence of inspection.

We estimate the following specification:

$$Y_{i,t} = \alpha_i + \gamma_t + \sum_{z=1997}^{2012} \sum_{k=-15}^{14} \mu_{z,k} \mathbf{1}\{E_i = z\} \mathbf{1}\{t - E_i = k\} + \epsilon_{i,t} \ ,$$

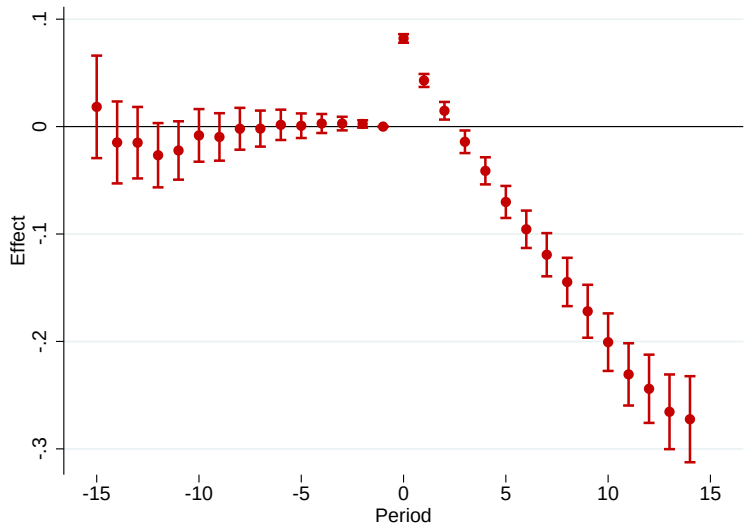
where $Y_{i,t}$ is the log of the number of formal workers, α_i correspond to plant fixed effects, γ_t are time fixed effects, E_i indicates the first year that firm i was treated, and $\mu_{u,z}$ captures the average treatment effect on the treated k years from initial treatment for firms first treated in year z. Standard errors are clustered at the plant level.

We obtain the average treatment effect on the treated k periods from initial treatment (ATT_k) as the weighted average of $\hat{\mu}_{z,k}$, using as the weight the share of units first treated during $E_i = z$:

$$\hat{ATT}_k = \hat{\mu}_{z,k} \hat{Pr}\{Ei = z \mid t - E_i = k\} \ .$$

Figure 2 shows the event-study results. Plants experience a significant increase in the number of formal workers right after the audit takes place. However, the number of formal workers presents a steady decline over time. We also show the lack of pre-trends, as the estimated coefficients are not statistically

Figure 2: Event-Study Analysis: Effects of Labor Inspections on Plant Size



Notes. Estimates from an event-studies analysis using Sun and Abraham (2021). The dependent variable is the log of the number of formal workers in the plant, and the treatment is being caught with an infraction. The sample includes plants caught with infractions between 1997 and 2012. Each point reflects the effect of being detected with infraction k year from the first time the plant was detected with an infraction. Bars indicate 95 percent confidence intervals. Standard errors are clustered at the plant level.

significant for periods $k < 0$, providing support for the identifying assumption.

We present the magnitude of estimates in Appendix Table A-1. A small proportion of inspections (4.60%) starts in one calendar year and finishes in another calendar year. In the baseline event-study analysis, the "first year of treatment" is the year in which the inspection ends. Appendix Table A-2 shows that the estimates are robust to considering the "first year of treatment" as the year that the inspection began. In addition, Appendix Figure A-3 shows that the results are robust to the use of other staggered difference-in-differences estimators: Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfœuille (2020). This exercise shows that the results are robust to alternative control groups—for instance, in Callaway and Sant'Anna (2021), we use the not-yet-treated plants as the control group—and estimation procedures.

Appendix Figure A-4 reports the results of placebo exercises that randomly shuffle the labor inspection dates to "placebo" intervention years instead of actual intervention years. We create three placebo exercises by randomly shuffling 11 times (panels a and b of Appendix Figure A-4, 51 times (panels c and d), 101 times (panels e and f), and 151 times (panels g and h). We then run the

staggered difference-in-differences estimator proposed by Sun and Abraham (2021) with those random labor inspections. Appendix Figure A-4 plots the (i) median coefficient (that should be near zero) and the corresponding standard errors; and (ii) the box plot to describe the entire distribution of coefficients of the placebo exercises. Reassuringly, plant size outcomes do not change after these placebo interventions.

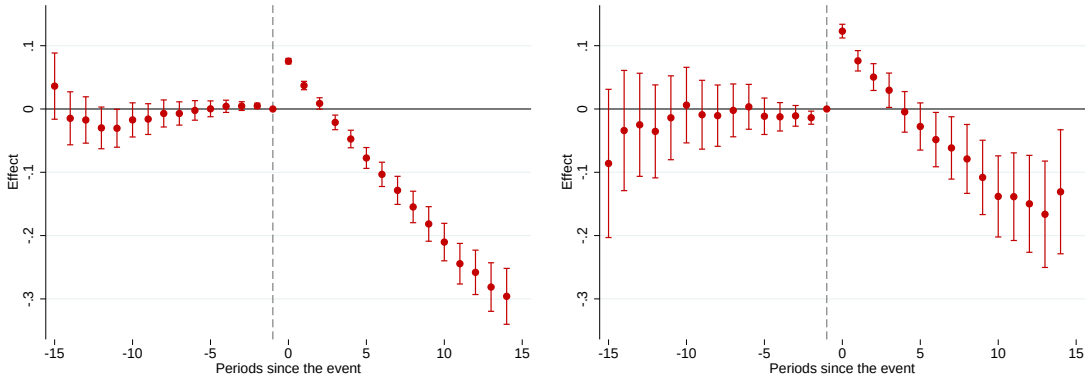
We now turn to exercises that highlight two important features of our model: (i) the threat of future inspections is consequential to firm growth, and (ii) firms start to experiment less after being caught with informal workers. We discuss these potential mechanisms in detail further below in the model.

To provide evidence on the effects of the threat of an inspection, we follow the literature arguing that being near a labor inspection office is associated with more inspections (e.g., Almeida and Carneiro 2012) and use the distance to the nearest Ministry of Labor's office as a proxy for the likelihood of future inspections. Using information from the administrative registries, we create the distance between the plant location and the nearest labor inspection office for each plant in our sample. Figure 3 performs a heterogeneity analysis by splitting the sample of plants into those nearest and farthest from the labor inspection offices. Plants situated closer to a labor office present the greatest decline in formal workers over time.[1]

To assess whether inspections affect experimentation, we run the baseline empirical model with the number of trademarks, innovation patents, industrial design, and utility model registration as the dependent variable. Figure 4 shows that firms caught with informal workers have fewer registrations over time. Panel (a) shows that plants with informal workers present less trademark registration, while panel (b) depicts that plants with information workers present a lower trademark, innovation patent, industrial design, and utility model registration.

[1]Data show that there is an increase in the probability of an audit after a plant is caught.

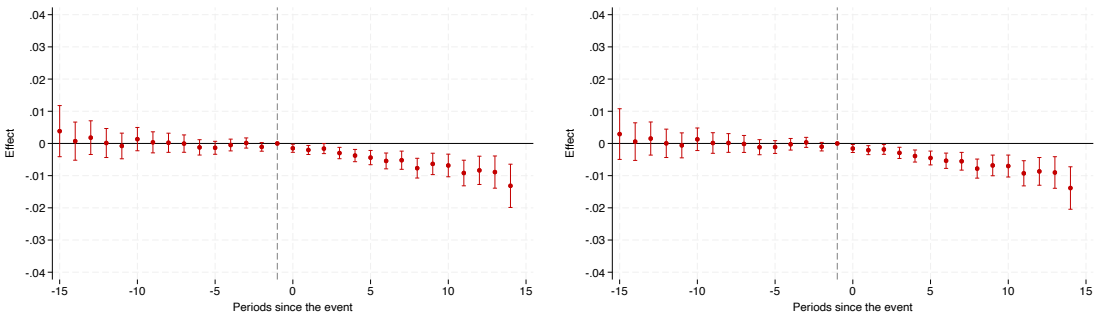
Figure 3: Event-Study Analysis: Distance from Labor Offices



(a) Closer: log(workers) (b) Farther: log(workers)

Notes. Estimates from an event-studies analysis using Sun and Abraham (2021). The dependent variable is the log of the number of formal workers in the plant, and the treatment is being caught with an infraction. Panel (a): plants that are closer to inspection centers (below the median distance from the labor office). Panel (b): plants that are farther away from inspection centers (above the median distance from the labor office). The sample includes plants caught with infractions between 1997 and 2012. Each point reflects the effect of being detected with infraction k year from the first time that the plant was detected with an infraction. Bars indicate 95 percent confidence intervals. Standard errors are clustered at the plant level.

Figure 4: Event-Study Analysis: Effects of Labor Inspections on Innovation



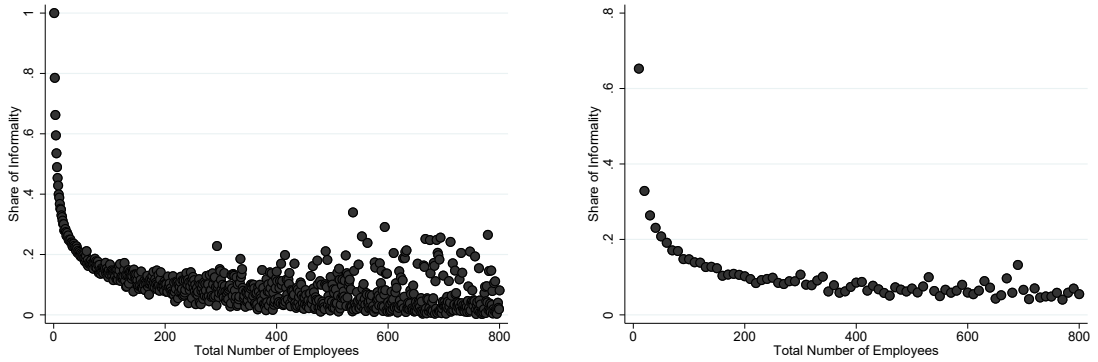
(a) Indicator Variable: Trademark registration (b) Indicator Variable: Trademark, Industrial Design, Patent, and Utility Model

Notes. Estimates from an event-studies analysis using Sun and Abraham (2021). The treatment is being caught with an infraction. The dependent variable in panel (a) is an indicator variable that equals one if the plant has registered a trademark in the year. The dependent variable in panel (b) is an indicator variable that equals one if the plant has registered a trademark, an invention patent, a utility model patent, or an industrial design in the year. The sample includes plants caught with an infraction between 1997 and 2012. Each point reflects the effect of being detected with infraction k year from the first time the plant was detected with an infraction. Bars indicate 95 percent confidence intervals. Standard errors are clustered at the plant level.

We now turn to the relationship between informal workers and plant size. Us-

Figure 5: Share of Informality and Plant Size

(a) % Informal and Size (b) % Informal and Size (bin width = 10)



Notes. The figure plots the average share of informality by firm size of plants caught with infraction between 1997 and 2010. Some establishments are audited more than once in different years. The figure contains information only for the first time that a plant was caught with an infraction. Panel (a) displays the average share of informality for firms with different sizes. Panel (b) aggregates observations in bins and displays the average share of informality for each bin.

ing data on the establishments with infractions, Figure 5 plots the share of informal workers for different plant sizes. The distribution of informal workers varies by plant size: smaller plants have a higher proportion of informal workers. However, the average proportion of informal workers stabilizes around approximately 15% for medium-sized and larger plants. The main implication from Figure 5 is that plants of all sizes are being caught with informal workers.

Finally, we check whether formal and informal workers differ with respect to observable characteristics. Table 1 compares the characteristics of these workers, such as wage, educational attainment, and gender. Column (I) displays the mean characteristics of existing formal workers, while column (II) shows the mean characteristics of informal workers. In column (III), we check if formal and informal workers are balanced across these observable characteristics. We use the standardized difference—which is not influenced by sample size—to assess the difference in the location in the covariate distributions. Imbens and Rubin (2015) suggest that a standardized difference between two groups should be above the threshold of 0.20 to be considered different. According to Table 1, standardized differences remain below 0.2 for all variables but wages. These results suggest that formal and informal workers are comparable in terms of observable characteristics, except for wages.[2]

[2]In Appendix Table **??**, we look at the subgroup of formal workers that were employed for

Table 1: Characteristics of Formal and Informal workers

	Formal Workers (I)	Informal Workers (II)	Standardized Difference (III)
Average Wage	499.543	356.106	-0.232
	(757.781)	(436.595)	
Hours per Week	42.654	42.513	-0.027
	(4.843)	(5.517)	
Age	31.762	29.987	-0.172
	(10.367)	(10.215)	
Male	0.697	0.677	-0.043
	(0.459)	(0.467)	
Foreigner Worker	0.001	0.001	-0.008
	(0.033)	(0.029)	
College Graduated	0.060	0.042	-0.084
	(0.238)	(0.200)	
Illiterate	0.015	0.019	0.028
	(0.123)	(0.136)	
Graduated from Elementary School	0.238	0.246	0.019
	(0.426)	(0.431)	
Graduated from Middle School	0.262	0.281	0.043
	(0.440)	(0.450)	
Graduated from High School	0.336	0.316	-0.042
	(0.472)	(0.465)	
White-Collar, High Skill	0.156	0.126	0.089
	(0.363)	(0.331)	
White-Collar, Low Skill	0.326	0.331	-0.010
	(0.469)	(0.470)	
Blue-Collar, High Skill	0.354	0.376	-0.046
	(0.478)	(0..484)	
Blue-Collar, Low Skill	0.164	0.168	-0.011
	(0.371)	(0.374)	

Notes. Information on the average value for the characteristics of formal - Column (I) - and informal workers - Column (II). Column (III) shows the standardized differences between the average values of the characteristics of informal workers and formal workers.

4 Simple Model

Before moving to the full-blown quantitative model, this section provides a simple toy model that illustrates the main mechanisms that our theory lays out. Suppose firms last for two periods. In the first period, a firm can explore new business opportunities. To explore these opportunities, the firm must employ labor for this activity. Experimentation, however, is risky. If successful, the firm becomes more productive and can employ the newly hired workers to fulfill its now higher labor demand. If the firm does not become more successful, those extra workers are not needed. This riskiness can influence the decision to hire workers formally or informally since firing formal workers implies that the firm must pay firing costs.

Suppose there is a continuum of firms indexed by their productivity z, where $z \sim U(z_{min}, z_{max})$. After successful experimentation, the firm's productivity rises to γz, where $\gamma > 1$. The firm starts the first period with N_1 formal workers. The firm can then decide whether or not to hire more workers to engage in experimentation. If the firm employs N_e workers for experimentation, the probability that it will become more productive is given by $p_e(N_e) = \frac{1}{1+p_1 \exp(-p_2 N_e)}$. These new workers can either be all formal or all informal (in the full model, we will relax this assumption). In the second period, after realizing whether or not it became more productive, the firm can hire or fire workers, $N \lessgtr 0$. Hence, the firm will have $N_1 + N_e + N$ workers for production in the second period. If the firm fires workers, it must pay a firing cost f per formal worker fired, but there is no cost to fire an informal worker. Finally, there is a probability p_A that the firm will be audited. If the firm is audited and caught with informal workers, it must pay a fine χ. The production function is given by $\tilde{z} N_p^{\alpha}$, where $\tilde{z}$ is the productivity level after experimentation and N_p is the number of workers used for production after all hiring and firing takes place.

If the worker decides to hire the new workers formally, it will face the following

at most to 24 months at the time of the inspection. The results indicate that this subgroup of formal workers and informal workers are comparable in terms of observable characteristics.

value functions:

$$\begin{aligned} V^{s,f}(z, N_e) &= \max_N \gamma z(N + N_e + N_1)^\alpha - w(N + N_e + N_1) - f \times \max(0, -N) \\ V^{u,f}(z, N_e) &= \max_N z(N + N_e + N_1)^\alpha - w(N + N_e + N_1) - f \times \max(0, -N) \\ V^f(z) &= \max_{N_e} p_e(N_e)V^{s,f}(z, N_e) + (1 - p_e(N_e))V^{u,f}(z, N_e) \end{aligned}$$

The first equation corresponds to the case in which a firm successfully experiments and its productivity is higher, $\gamma z > z$. Since the firm is hiring formally, it must pay a cost f for each worker it decides to fire. The second line is the analogous case if the firm does not succeed in increasing its productivity. The third line writes the firm's value before experimentation when it is deciding how many workers to hire for this activity.

The corresponding values for the case in which the firm hires informal workers are: If the worker decides to hire the new workers formally, it will face the following value functions:

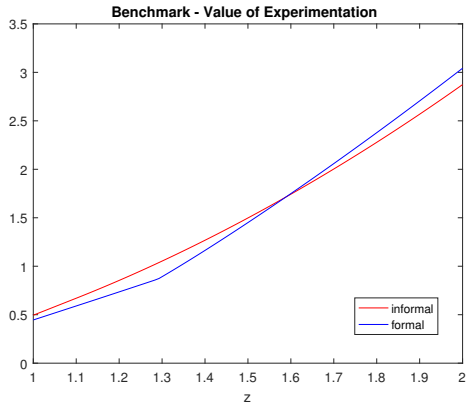
$$\begin{aligned} V^{s,i}(z, N_e) &= \max_N \gamma z(N + N_e + N_1)^\alpha - w(N + N_e + N_1) - f \times \max(0, -N - N_e) - p_A\chi \\ V^{u,i}(z, N_e) &= \max_N z(N + N_e + N_1)^\alpha - w(N + N_e + N_1) - f \times \max(0, -N - N_e) - p_A\chi \\ V^i(z) &= \max_{N_e} p_e(N_e)V^{s,i}(z, N_e) + (1 - p_e(N_e))V^{u,i}(z, N_e) \end{aligned}$$

There are two differences in this case. First, the firm only pays firing costs if it decides to hire all the informal workers (which is costless) and some of the initial formal ones. Second, since the firm employs informal workers, it might be caught by an audit and pay a fine. The expected value of this cost is $p_A\chi$.

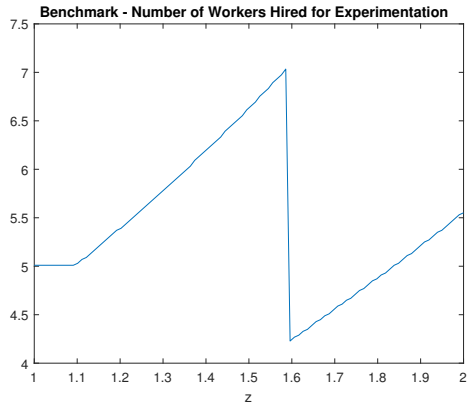
There will be a productivity level z^* such that $V^f(z^*) = V^i(z^*)$. Firms with $z < z^*$ will hire informal workers; see Panel (a) in Figure 6. When a firm hires formal workers, there is a chance that it might end up with too many workers in the second period if it does not succeed in increasing its productivity. Hence, more productive firms that hire workers formally tend to hire fewer workers; see Panel (b) in Figure 6. Since the probability of success in experimentation is an increasing function of the number of workers employed in experimentation, firms that hire informally are more likely to succeed and grow.

The empirical analysis of Section 3 showed that firms caught with informal workers in an audit are more likely to be audited in the future. What happens

Figure 6: Toy Model: Benchmark



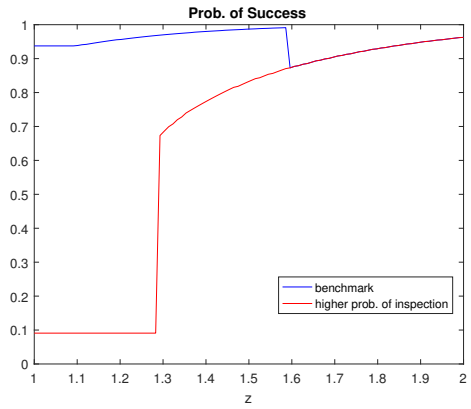
(a) Values for experimentation



(b) Workers hired for experimentation

Figure 7: Toy Model: Experiment with higher audit probability

(a) Workers hired for experimentation



(b) Prob. of successful experimentation

in this simple model if a firm is faced with a higher audit probability? To answer this question, we perform the following thought experiment: increase the audit probability p_A such that no firm finds it profitable to hire informal workers. Panel (a) in Figure 7 shows that, when faced with a higher audit probability, firms do not hire workers informally and end up hiring fewer workers. With fewer workers employed in experimentation, the probability that a firm succeeds in increasing its productivity and growing falls; see Panel (b) in Figure 7.

5 Model

The economy is populated by heterogeneous firms and homogeneous households. Each incumbent firm is indexed by a baseline productivity level z. It also starts with at rung $r = 1$ of a productivity ladder, which it can climb over time. Firms stochastically get an opportunity to increase their productivity by moving up the productivity ladder to $r+1$. Denote the lack of this possibility by the state $\ell = 0$, and by $\ell = 1$ the opportunity to move up. To do so, firms must hire workers to potentially access this new rung of the ladder. If a firm employs N_r workers to work on this enterprise, it will be successful and increase its productivity with probability $p_r(N_r, \ell)$, with $p_r(N_r, 0) = 0$ for all N_r. Denote this successful event by $s = 1$. With the complementary probability, it will remain in the same rung of the productivity ladder, an event denoted by $s = 0$. The firm's productivity will then be $zg(r)$, where r is the rung where the firm is at and $g(r)$ is increasing with $g(1) = 1$.

Labor is the only input for production. Each period, firms decide how many workers to hire. Hiring additional workers, formal or informal, is subject to adjustment costs. Upon hiring a worker, firms decide whether to have them in their books as formal workers or keep them hidden as informal. The firm has to pay payroll taxes and firing costs for formal workers. For informal workers, the firm avoids these costs. But firms are audited by the government, and if caught with informal workers, it has to formalize all its workers and faces a fine. Hence, firms trade off the flexibility that informality provides against its costs.

Labor markets are competitive, and all workers, formal or informal, are paid the same wage. Hence, we follow the recent literature and abstract from non-wage benefits of formal contracts and focus on the firms. Households consume goods and supply labor. We will elaborate on each agent's problem in what follows.

Firms A firm produces output according to the following production function,

$$y = zg(r)N_p^{\alpha}, \text{ with } \alpha < 1,$$

where N_p is the total number of workers the firm employs in production (rather than to try to access a higher rung in the productivity ladder).

Firms hire workers in a competitive labor market. Once a worker arrives at a firm, they can have a formal or informal contract. Let the contract/employment type of a worker be denoted by $e \in \{f, i\}$.

All workers are paid the market wage w. The firm must pay a tax proportional to the wage bill for all formal workers, denoted by τ. To fire a formal worker, the firm must pay a firing cost, denoted by λ. By hiring workers informally, firms avoid labor taxes and firing costs. In order to operate, the firm must pay a fixed cost ξ each period. Each period, a firm might exit with an exogenous probability of δ.

Government Each firm is subject to random audits by the government. The probability that an audit takes place is given by the function $p_A(N_f, N_f', N_i', A)$. The variable A denotes whether a firm has been caught with informal workers in a previous audit. If a firm is in this state ($A = 1$), such flag disappears with probability η and becomes $A = 0$ each period. Besides past audit history, the probability of an audit depends on the number of formal workers in two consecutive periods: N_f and N_f'. This dynamic dependence on the number of formal workers aims to capture the effect of turnover on the probability of an audit. Though auditors do not observe the firm's informal labor force, the audit probability still depends on N_i'. This captures potential "tips" auditors may receive based on informal workers. Moreover, if a firm produces a lot of output with few formal workers, this may lead to suspicion from the authorities, which can result in an audit.

If an audit occurs and a firm is caught with informal workers, $N_i > 0$, it cannot exit, hire, or fire workers during that period. Moreover, all informal workers automatically become formal, and the firm pays a fine $\chi(N_i, A)$.

5.1 Firms' Decisions

Consider a firm that enters the period with N_f formal workers and N_i informal workers so that its total workforce is $N = N_f + N_i$. It can choose to reduce its workforce by choosing a new number $\hat{N}_f$ of formal and $\hat{N}_i$ of informal workers so that $\hat{N} = \hat{N}_f + \hat{N}_i < N$. If this reduces the size of the formal workforce, it has to pay firing costs $\lambda(N_f - \hat{N}_f)$. It can then decide to hire new workers. For H additional workers, it has to pay the increasing and convex hiring cost $h(H/\hat{N})$.

Additionally, it must decide how many of them will be formal (H_f) and how many will be informal (H_i), such that $H = H_i + H_f$.

Let $\beta = (1+\rho)^{-1}$ be the firm's discount factor, where ρ is the interest rate. Denote by $\widehat{V}$ the value of a firm that is able to freely adjust its workforce; i.e., it was not caught by an audit this period. This value can be written as the following Bellman equation, where we use $\mathcal{I}(statement)$ to represent an indicator function that takes the value of 1 if the $statement$ is true and 0 otherwise:

$$
\begin{aligned}
\widehat{V}(N_f, N_i, z, r, \ell, A) = \max_{\widehat{\mathbf{N}}, \mathbf{H}, N'_f, N'_i, N'_p, N'_r} \; & zg(r)(N'_p)^{\alpha} - wN'_i - w(1+\tau)N'_f - \xi \\
& - \lambda(N_f - \widehat{N}_f)\mathcal{I}(\widehat{N}_f < N_f) - h(H/\widehat{N}) + \beta\delta(-\lambda N'_f) \\
& + \beta(1-\delta)p_r(N'_r, \ell)\left\{p_A(N_f, N'_f, N'_i, A)\mathcal{I}(N'_i > 0)\times\right. \\
& \left[\mathbb{E}_{\ell'|\ell, s=1}\widetilde{V}(N'_f + N'_i, z, r+1, \ell') - \chi(N'_i, A)\right] + \\
& \left[(1 - p_A(N_f, N'_f, N'_i, A)) + p_A(N_f, N'_f, N'_i, A)\mathcal{I}(N'_i = 0)\right] \times \\
& \left.\mathbb{E}_{\ell', A'|\ell, A, s=1}V(N'_f, N'_i, z, r+1, \ell', A')\right\} + \\
& \beta(1-\delta)(1 - p_r(N'_r, \ell))\left\{p_A(N_f, N'_f, N'_i, A)\mathcal{I}(N'_i > 0)\times\right. \\
& \left[\mathbb{E}_{\ell'|\ell, s=0}\widetilde{V}(N'_f + N'_i, z, r, \ell') - \chi(N'_i, A)\right] + \\
& \left[(1 - p_A(N_f, N'_f, N'_i, A)) + p_A(N_f, N'_f, N'_i, A)\mathcal{I}(N'_i = 0)\right] \times \\
& \left.\mathbb{E}_{\ell', A'|\ell, A, s=0}V(N'_f, N'_i, z, r, \ell', A')\right\} \qquad (1)
\end{aligned}
$$

subject to

$$
\begin{aligned}
N'_f &= \widehat{N}_f + H_f, \\
N'_i &= \widehat{N}_i + H_i, \\
\widehat{N}_f + \widehat{N}_i &\leq N_f + N_i, \\
N'_f + N'_i &= N'_p + N'_r.
\end{aligned}
$$

The first two lines represent the current profit (including any potential firing costs) and the value of exiting the next period. The remaining lines correspond to situations in which a firm survives. The third and fourth lines represent the case in which the firm is successful in moving up the productivity ladder, but is audited and caught with informal workers. The continuation value, in this case, is given by $\widetilde{V}$ and will be elaborated on next. In this case, the firm must also pay the fine $\chi(N'_i, A)$. The fifth and sixth lines describe the situation in which the

firm was successful in increasing its productivity and was either not audited or audited but did not have any informal workers. The continuation value for this case is V and will be described momentarily. The remaining lines correspond to analogous cases, but when the firm was not successful in moving up the productivity ladder, and remains on the same rung. Appendix B provides the formulas for the expectations in (1). Denote the intra-period profits earned by this firm by $\widehat{\pi}(N_f, N_i, z, r, \ell, A)$.

If a firm is caught with informal workers, it must pay a fine, formalize all of its workers, and cannot exit in this period. Its value $\widetilde{V}$ is thus given by:

$$\begin{aligned}\widetilde{V}(N_f, z, r, \ell) = \max_{N'_p, N'_r} & \; zg(r)(N'_p)^{\alpha} - w(1+\tau)N_f - \xi + \beta\delta(-\lambda N_f) \\ & + \beta(1-\delta)p_r(N'_r, \ell)\mathbb{E}_{\ell'|\ell}V(N_f, 0, z, r+1, \ell', 1) \\ & + \beta(1-\delta)(1-p_r(N'_r, \ell))\mathbb{E}_{\ell'|\ell}V(N_f, 0, z, r, \ell', 1) \qquad (2)\end{aligned}$$

subject to

$$N'_f = N_f = N'_p + N'_r,$$

The first line contains the firm's intra-period profits and the value that it might exit in the next period. The second line writes out the value of a firm that survives and is successful in moving up to the next rung of the productivity ladder ($r+1$). In the third line, the firm survives but does not become more productive. Since the firm was caught in this period's audit, it will have the flag $A = 1$ in the next period as long as it survives. Denote the intra-period profits earned by this firm by $\widetilde{\pi}(N_f, z, r, \ell)$, which might be negative.

If a firm is not audited or is audited but does not employ any informal workers, it is free to adjust the number of its workers (of any type of contract), including firing or hiring new workers. It may also opt to exit. We can thus write the following Bellman equation:

$$V(N_f, N_i, z, r, \ell, A) = \max\left[-\lambda N_f, \widehat{V}(N_f, N_i, z, r, \ell, A)\right]. \qquad (3)$$

That is, the firm may decide to exit (first term) or continue its production. If the firm exits, it must pay the firing costs on all its formal workers, λN_f. If it continues, its value is given by $\widehat{V}$, defined in (1).

Finally, there is a continuum of potential entrants. If a firm decides to enter, it must pay a fixed cost κ. Upon entry, the firm draws a productivity shock from

the distribution $\overline{\Gamma}(z)$ and starts its life at the lowest rung ($r = 1$), at first with no chance to move up the productivity ladder ($\ell = 0$), with no workers and $A = 0$, since it has never been audited. In equilibrium, the following free entry condition must hold:

$$\kappa = \int \widehat{V}(0, 0, z, 1, 0, 0) d\overline{\Gamma}(z). \tag{4}$$

5.2 Households

The economy is populated by a continuum of measure 1 of homogeneous households. Each household consists of a continuum of measure 1 of household members. Each member is endowed with one unit of time that can be divided between hours worked in the market h and leisure $1-h$. The household's utility function is given by:

$$U(c, h) = u(c) - v(h),$$

where $u(c)$ is the increasing and concave utility function c and $v(h)$ is the increasing and convex function representing the disutility of supplying h units of labor. Households cannot save.

Given the description above, the representative household solves the following static optimization problem:

$$\max_{c,h} \left[u(c) - v(h)\right], \tag{5}$$

subject to

$$c \leq w(1 - h) + \Pi,$$

where Π represents the profits distributed from firms to households.

5.3 Equilibrium

We solve for a steady-state equilibrium. Let $M(N_f, N_i, z, r, \ell, A)$ denote the measure of firms with state vector $(N_f, N_i, z, r, \ell, A)$. Let $\mathbf{T}$ denote the operator that maps the current period's distribution into next period's distribution. In a steady state, we must have:

$$M(N_f, N_i, z, r, \ell, A) = \mathbf{T}\left[M(N_f, N_i, z, r, \ell, A)\right]. \tag{6}$$

We can now write the following:

Definition: A stationary equilibrium for this economy consists of value functions, policy functions, a wage rate w, aggregate profits Π, and a distribution M such that:

1. Given w, the firms solve their problems given by (1)-(3);
2. The free entry condition (4) must hold;
3. Given w, the representative household solves its problem given by (5);
4. The goods and labor markets clear;
5. The distribution of firms is stationary; i.e. M solves (6).

6 Conclusion

This study sheds light on the dynamics of informal employment in Brazil, offering valuable insights into the intensive margin of informality within formal firms. The findings reveal that the penalties imposed on formal establishments caught with informal workers have a pronounced and enduring impact on their growth trajectories. The empirical evidence underscores the similarity in characteristics between formal and informal workers, emphasizing the pervasive nature of informal employment across firms of all sizes.

Our dynamic general equilibrium model delineates the strategic choices firms make in navigating the trade-off between the flexibility afforded by informality and the potential penalties incurred. As policymakers grapple with monitoring and punishment strategies, our framework can be used to explore government policies and examine the impact of audit strategies on informality, output, and firm dynamics. Understanding the complexities of informality in the context of a highly regulated labor market contributes essential insights to the debate surrounding government interventions and their implications for economic dynamism in developing economies like Brazil.

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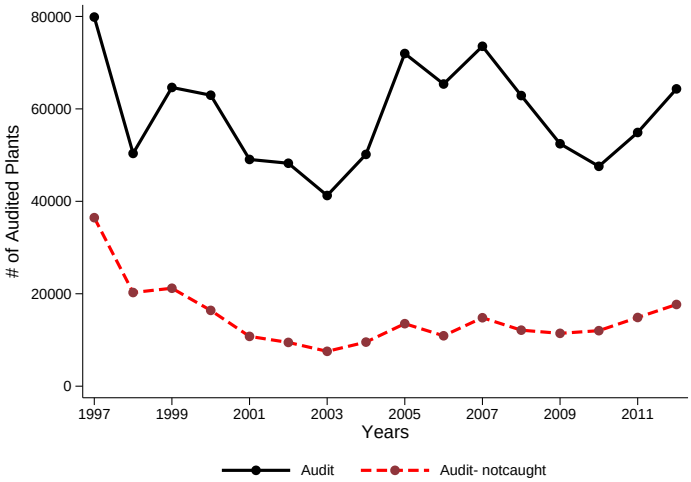
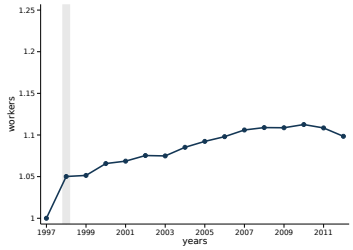
A Appendix - Extra Figures and Tables

Figure A-1: Number of Audited Plants

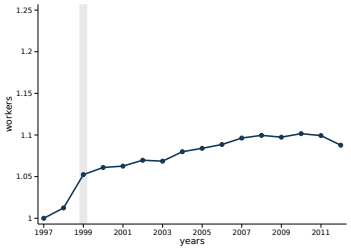


Notes. The figure plots the number of establishments audited per year and the number of establishments caught with informal workers per year.

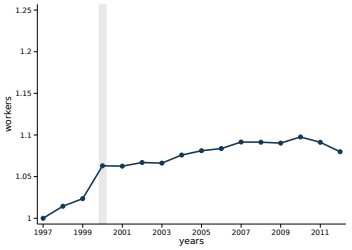
Figure A-2: Number of Workers and Inspections: Raw Data Year-by-Year



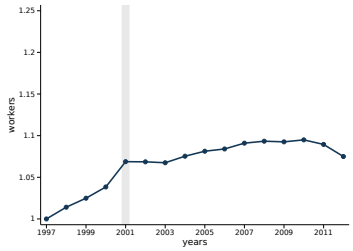
(a) Year: 1998



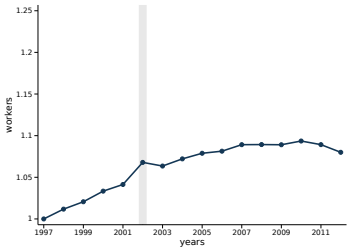
(b) Year: 1999



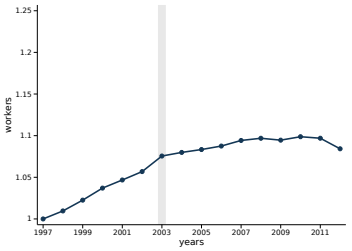
(c) Year: 2000



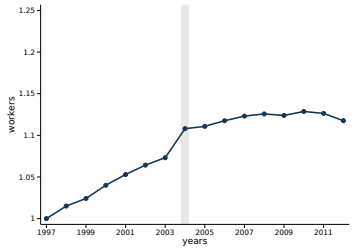
(d) Year: 2001



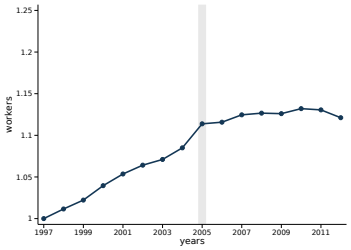
(e) Year: 2002



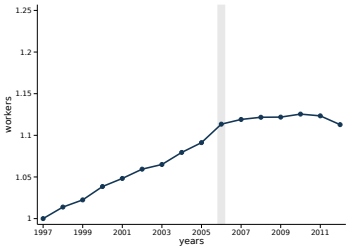
(f) Year: 2003



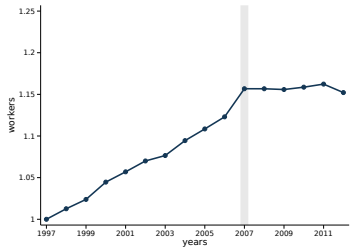
(g) Year: 2004



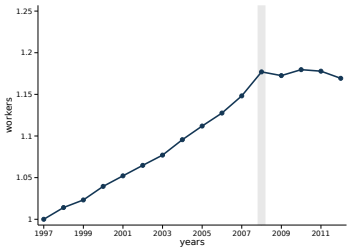
(h) Year: 2005



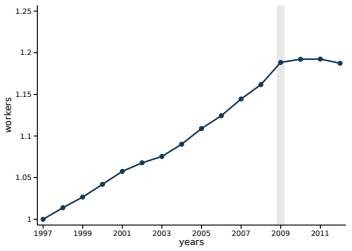
(i) Year: 2006



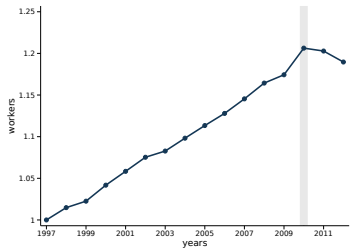
(j) Year: 2007



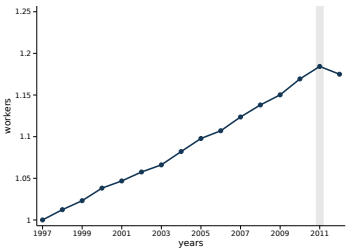
(k) Year: 2008



(l) Year: 2009



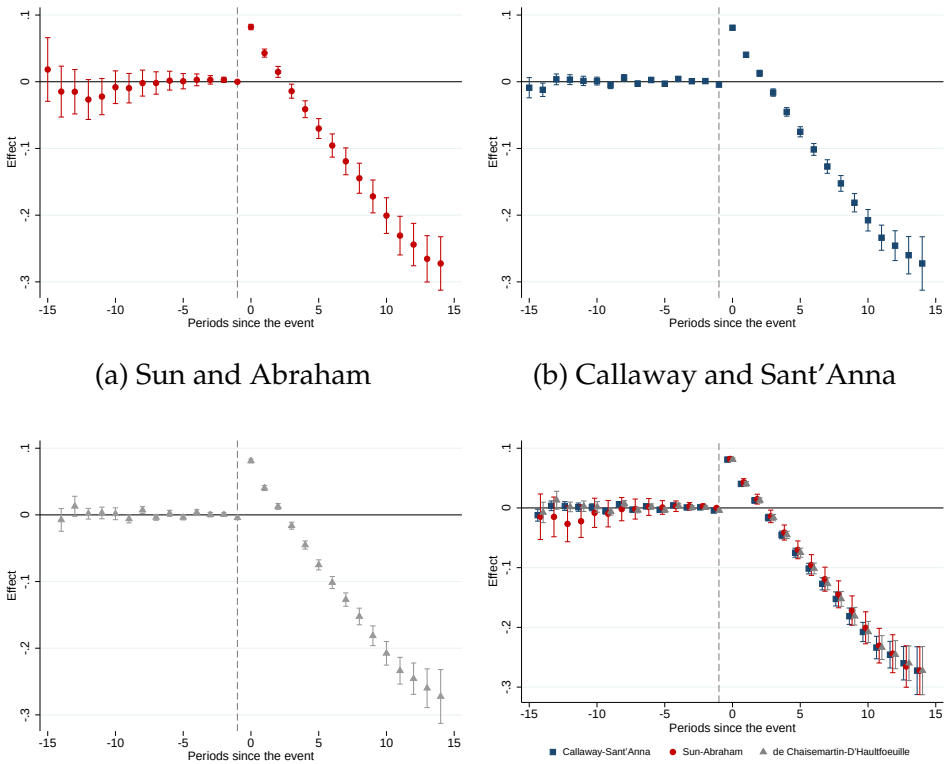
(m) Year: 2010



(n) Year: 2011

Notes. This figure shows the evolution in the number of workers for plants caught with informal workers between 1998 and 2011. Vertical bars denote the year in which the plants were inspected. The number of workers was normalized to one in 1997.

Figure A-3: Staggered Difference-in-Differences: Estimators



(a) Sun and Abraham

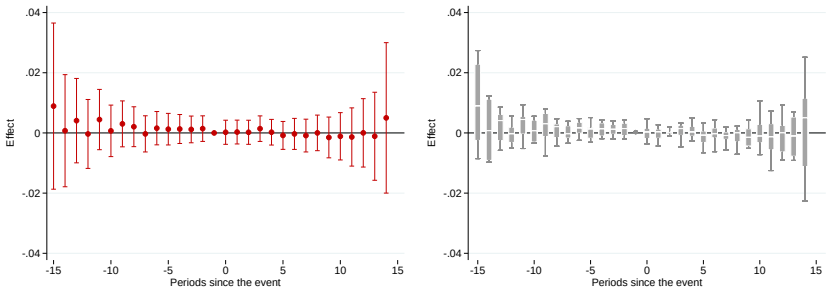
(b) Callaway and Sant'Anna

(c) Chaisemartin and D'Haultfoeuille

(d) All 3 Estimators

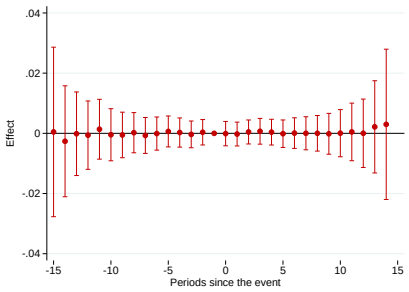
Notes. Estimates from an event-studies analyses. The dependent variable is the log of the number of formal workers in the plant and the treatment is being caught with an infraction. The sample includes plants caught with infraction between 1997 and 2012. Each point reflects the effect of being detected with infraction k year from the first time that the plant was detected with an infraction. Panel (a): Sun and Abraham (2021) estimator. Panel (b): Callaway and Sant'Anna (2021) estimator. Panel (c): Chaisemartin and D'Haultfoeuille (2020) estimator. Panel (d) overlays the plots of all three estimators in the same graph. Bars indicate 95 percent confidence intervals. Standard errors are clustered at the plant level.

Figure A-4: Placebo exercise

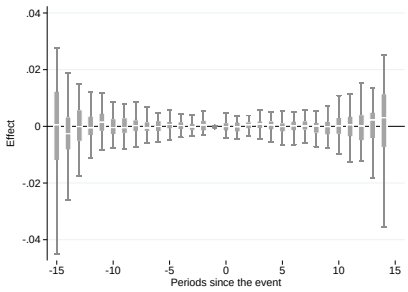


(a) 11 - Median

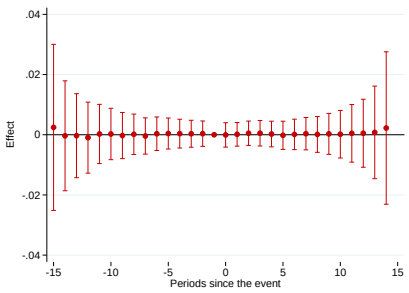
(b) Box Plot - 11



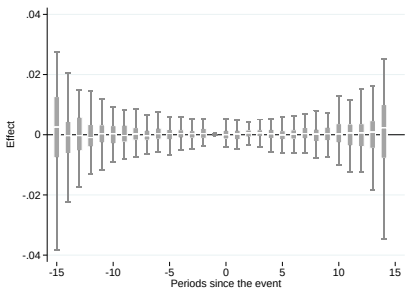
(c) 51 - Median



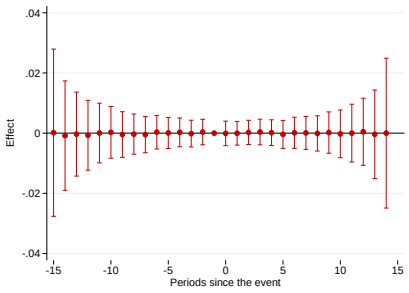
(d) Box Plot - 51



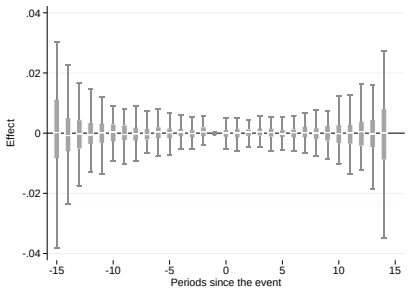
(e) 101 - Median



(f) Box Plot - 101



(g) 151 - Median



(h) Box Plot - 151

Notes. Estimates from an event-studies analysis using Sun and Abraham (2021) and placebo interventions. The dependent variable is the log of the number of formal workers in the plant. The sample includes plants caught with infractions between 1997 and 2012. Placebo exercises randomly shuffle labor inspection dates to create "placebo" intervention years. The figure plots the median coefficient, and the box plot of the distribution of coefficients of the placebo exercises. Panels (a) and (b): randomly shuffling 11 times. Panels (c) and (d): 51 times. Panels (e) and (f): 101 times. Panels (h) and (g): 151 times. Bars indicate 95 percent confidence intervals. Standard errors are clustered at the plant level.

Table A-1: Event Studies Analysis: Effects of Labor Inspection

Period	Coef.	S.E	95% CI	
			Min	Max
-15	0.018	0.024	-0.029	0.066
-14	-0.015	0.019	-0.053	0.023
-13	-0.015	0.017	-0.048	0.018
-12	-0.027	0.015	-0.057	0.003
-11	-0.022	0.014	-0.049	0.005
-10	-0.008	0.013	-0.033	0.016
-9	-0.01	0.011	-0.032	0.012
-8	-0.002	0.01	-0.021	0.017
-7	-0.002	0.009	-0.019	0.015
-6	0.002	0.007	-0.012	0.016
-5	0.001	0.006	-0.011	0.012
-4	0.003	0.005	-0.006	0.012
-3	0.003	0.003	-0.004	0.009
-2	0.002	0.002	-0.001	0.006
-1	0.000	0.000	0.000	0.000
0	0.082	0.002	0.078	0.086
1	0.043	0.003	0.037	0.049
2	0.015	0.004	0.007	0.023
3	-0.014	0.005	-0.025	-0.004
4	-0.041	0.006	-0.054	-0.029
5	-0.07	0.008	-0.085	-0.055
6	-0.096	0.009	-0.113	-0.078
7	-0.119	0.01	-0.139	-0.099
8	-0.145	0.011	-0.167	-0.122
9	-0.172	0.013	-0.197	-0.147
10	-0.201	0.014	-0.227	-0.174
11	-0.231	0.015	-0.26	-0.202
12	-0.244	0.016	-0.276	-0.212
13	-0.265	0.018	-0.3	-0.231
14	-0.272	0.02	-0.312	-0.232

Notes. Estimates from an event-studies analysis. The dependent variable is the log of the number of formal workers in the plant and the treatment is being caught with an infraction. The sample includes plants caught with infraction between 1997 and 2012. Each estimate reflects the effect of being detected with infraction k year from the first time that the plant was detected with an infraction. Standard errors are clustered at the plant level.

Table A-2: Event Studies Analysis: Effects of Labor Inspection Treatment year as the year that audit began

Period	Coef.	S.E	95% CI	
			Min	Max
-15	0.031	0.025	-0.017	0.080
-14	-0.007	0.020	-0.046	0.032
-13	-0.010	0.017	-0.045	0.024
-12	-0.020	0.016	-0.051	0.011
-11	-0.020	0.014	-0.048	0.008
-10	-0.013	0.013	-0.038	0.012
-9	-0.013	0.012	-0.036	0.010
-8	-0.005	0.010	-0.025	0.015
-7	-0.004	0.009	-0.021	0.014
-6	0.003	0.007	-0.012	0.017
-5	0.002	0.006	-0.010	0.014
-4	0.004	0.005	-0.005	0.013
-3	0.004	0.003	-0.002	0.011
-2	0.004	0.002	0.001	0.008
-1	0.000	0.000	0.000	0.000
0	0.086	0.002	0.081	0.090
1	0.046	0.003	0.040	0.052
2	0.018	0.004	0.009	0.026
3	-0.012	0.006	-0.023	-0.001
4	-0.038	0.007	-0.051	-0.025
5	-0.068	0.008	-0.083	-0.052
6	-0.094	0.009	-0.112	-0.076
7	-0.118	0.011	-0.139	-0.097
8	-0.143	0.012	-0.167	-0.120
9	-0.172	0.013	-0.198	-0.147
10	-0.202	0.014	-0.229	-0.174
11	-0.233	0.015	-0.263	-0.203
12	-0.247	0.017	-0.279	-0.214
13	-0.270	0.018	-0.306	-0.234
14	-0.277	0.021	-0.318	-0.236

Notes. Estimates from an event-studies analysis. The dependent variable is the log of the number of formal workers in the plant and the treatment is being caught with an infraction. The sample includes plants caught with infraction between 1997 and 2012. Each estimate reflects the effect of being detected with infraction k year from the first time that the plant was detected with an infraction. In the baseline event-study analysis, the "first year of treatment" is the year in which the inspections ends. In this table, the "first year of treatment" as the year that the inspection began. Standard errors are clustered at the plant level.

Table A-3: Formal and Informal workers with up to 2 years tenure

	Formal Workers (I)	Informal Workers (II)	Standardized Difference (III)
Average Wage	493.36	351.15	-0.233
	(750.931)	(423.446)	
Hours per Week	42.698	42.577	-0.024
	(4.779)	(5.42)	
Age	31.74	30.02	-0.167
	(10.356)	(10.222)	
Male	0.698	0.679	-0.041
	(0.459)	(0.467)	
Foreigner Worker	0.001	0.001	-0.008
	(0.033)	(0.029)	
College Graduated	0.059	0.041	-0.082
	(0.235)	(0.198)	
Illiterate	0.016	0.020	0.030
	(0.124)	(0.139)	
Graduated from Elementary School	0.242	0.250	0.019
	(0.428)	(0.433)	
Graduated from Middle School	0.264	0.283	0.042
	(0.441)	(0.450)	
Graduated from High School	0.330	0.309	-0.045
	(0.470)	(0.462)	
White-Collar, High Skill	0.157	0.126	-0.089
	(0.364)	(0.332)	
White-Collar, Low Skill	0.327	0.332	-0.010
	(0.469)	(0.471)	
Blue-Collar, High Skill	0.354	0.375	-0.044
	(0.478)	(0.484)	
Blue-Collar, Low Skill	0.162	0.167	-0.013
	(0.369)	(0.373)	

Notes. Information on the average value for the characteristics of formal - Column (I) - and informal workers - Column (II) - employed for up to two years prior to the audit. Column (III) shows the standardized differences between the average values of the characteristics of informal workers and formal workers.

B Appendix – Theory

This section provides some missing details for the model described in Section 5.

Equation (1) describes the problem of a firm that was not caught with informal workers this period and can thus freely choose the size and allocation of its workforce. The continuation value for this Bellman equation contains several expectations that take into account the uncertainty the firm faces before next period. We now provide detailed expressions for these expectations. To do this, define the function $p_\ell(\ell, s)$ that gives the probability that the firm will have the opportunity of moving up the ladder in the following period, conditional on having had this opportunity in the current period ($\ell = 1$) or not ($\ell = 0$), and whether the firm was successful becoming more productive ($s = 1$) or not ($s = 0$) in the current period.

$\mathbb{E}_{\ell'|\ell}\widetilde{V}(N'_f + N'_i, z, r+1, \ell')$ captures the value of a firm that successfully moves up the productivity ladder and is audited with informal workers:

$$\begin{aligned}\mathbb{E}_{\ell'|\ell,s=1}\widetilde{V}(N'_f + N'_i, z, r+1, \ell') = p_\ell(1,1)\widetilde{V}(N'_f + N'_i, z, r+1, 1)\\ +[1-p_\ell(1,1)]\widetilde{V}(N'_f + N'_i, z, r+1, 0).\end{aligned}$$

$\mathbb{E}_{\ell',A'|\ell,A}V(N'_f, N'_i, z, r+1, \ell', A')$ represents the value of a firm that successfully became more productive this period and was either not audited or audited and had no informal workers. Next period the firm operates normally. Its value depends on whether the firm currently had a previously caught flag ($A = 1$):

$$\begin{aligned}\mathbb{E}_{\ell',A'|\ell,A=1,s=1}V(N'_f, N'_i, z, r+1, \ell', A') = \eta p_\ell(1,1)V(N'_f, N'_i, z, r+1, 1, 0)\\ +\eta[1-p_\ell(1,1)]V(N'_f, N'_i, z, r+1, 0, 0)\\ +(1-\eta)p_\ell(1,1)V(N'_f, N'_i, z, r+1, 1, 1)\\ +(1-\eta)[1-p_\ell(1,1)]V(N'_f, N'_i, z, r+1, 0, 1),\end{aligned}$$

or did not have the previously caught flag ($A = 0$):

$$\begin{aligned}\mathbb{E}_{\ell',A'|\ell,A=0,s=1}V(N'_f, N'_i, z, r+1, \ell', A') = p_\ell(1,1)V(N'_f, N'_i, z, r+1, 1, 0)\\ +[1-p_\ell(1,1)]V(N'_f, N'_i, z, r+1, 0, 0).\end{aligned}$$

There are similar expressions for the case in which the firm does not move up the productivity ladder. This can happen either because the firm did not have the opportunity to move up ($\ell = 0$) and could not have been successful ($s = 0$) or the firm did have the

opportunity ($\ell = 1$) but was unsuccessful ($s = 0$). $\mathbb{E}_{\ell'|\ell,s=0}\widetilde{V}(N'_f + N'_i, z, r, \ell')$ captures the value of the firm when it was caught with informal workers:

$$\begin{aligned}\mathbb{E}_{\ell'|\ell,s=0}\widetilde{V}(N'_f + N'_i, z, r, \ell') = p_\ell(\ell, 0)\widetilde{V}(N'_f + N'_i, z, r, 1) \\ +[1 - p_\ell(\ell, 0)]\widetilde{V}(N'_f + N'_i, z, r, 0).\end{aligned}$$

Finally, $\mathbb{E}_{\ell',A'|\ell,A,s=0}V(N'_f, N'_i, z, r, \ell', A')$ represents the case in which a firm did not increase its productivity and was not caught with informal workers. This depends on whether the firm had a previously caught flag ($A = 1$):

$$\begin{aligned}\mathbb{E}_{\ell',A'|\ell,A=1,s=0}V(N'_f, N'_i, z, r, \ell', A') = \eta p_\ell(\ell, 0)V(N'_f, N'_i, z, r, 1, 0) \\ +\eta[1 - p_\ell(\ell, 0)]V(N'_f, N'_i, z, r, 0, 0) \\ +(1 - \eta)p_\ell(\ell, 0)V(N'_f, N'_i, z, r, 1, 1) \\ +(1 - \eta)[1 - p_\ell(\ell, 0)]V(N'_f, N'_i, z, r, 0, 1),\end{aligned}$$

or did not have the previously caught flag ($A = 0$):

$$\begin{aligned}\mathbb{E}_{\ell',A'|\ell,A=0,s=0}V(N'_f, N'_i, z, r, \ell', A') = p_\ell(\ell, 0)V(N'_f, N'_i, z, r, 1, 0) \\ +[1 - p_\ell(\ell, 0)]V(N'_f, N'_i, z, r, 0, 0).\end{aligned}$$