Increasing the Cost of Informal Workers: Evidence from Mexico*

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January 14, 2020

Abstract

How do firms respond to increasing the cost of informal employment? What can their responses tell us about why firms hire informal workers? To answer, we link administrative and survey data, tracking formal and informal workers, with over 600,000 random work-site inspections in Mexico. We develop a search model with asymmetrical information across employers where formality is a signal of workers' productivity. Consistent with the model's predictions, workers formalized after an inspection have similar probability of receiving a formal job and comparable starting wages at the next employer than workers formalized without an inspection.

Private sector employers in Mexico must register their workers with the Social Security Institute (IMSS) and pay payroll taxes. Registered (i.e. formal) workers have access to a wide range of social benefits and are protected by various labor regulations. Enforcement

*Previously circulated as: "The Effects of Labor Regulation Monitoring on Hiring, Layoffs, and Wages" *Disclaimer*: This paper uses confidential data from the National Institute of Statistics and Geography (INEGI), the Ministry of Labor and Social Welfare (STPS), and the Mexican Social Security Institute (IMSS). IMSS data was accessed through the Econlab at Banco de México. The EconLab collected and processed the data as part of its effort to promote evidence-based research and foster ties between Banco de México's research staff and the academic community. Inquiries regarding the terms under which the data can be accessed should be directed to: econlab@banxico.org.mx. The views and conclusions presented herein are exclusively the responsibility of the authors and do not necessarily reflect those of Banco de México, IMSS, INEGI or STPS. This project received financial support from the RCA at UCSC and UC MEXUS.

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is limited, however, and over half of the workforce is informally employed. In this paper, we analyze how increasing the cost of employing workers informally, via random inspections, affects firms' and workers' outcomes across various margins, including wages, hiring, separations, and worker formalization. We also examine whether workers are affected after separating from an inspected employer. We use the heterogeneity in firms' responses to test the predictions of a model where employers offer formal or informal jobs based on workers' expected productivity and a heterogeneous cost of informal employment.

Only formal firms (i.e. firms registered with the government) can register workers. Previous literature analyzes firms' decision to register with the government[1]. Instead, we focus on their decision to offer a formal or informal job to a given worker, abstracting from the decision to become a formal firm.[2] Despite the *de-jure* obligation to register all their employees, formal firms can, and do, hire workers "off-the-books" (i.e. informal workers). Using confidential data from a household survey, merged with administrative data, we construct a novel dataset that tracks formal and informal workers across time, formality status, and employers. We find that 1 out of every 4 workers at formal firms is informally employed. These workers account for over half of all informal jobs in Mexico.

This paper sheds light on the trade-off firms face when deciding whether to abide with labor regulation. When offering informal jobs, employers[3] avoid payroll taxes and other regulatory costs, but face a positive probability of getting caught. We examine the effects of increasing firms' costs of informal employment exploiting more than 600,000 random work-site[4] inspections between 2005 and 2016. We argue that inspections raise the probability of a fine from IMSS and heighten employers' awareness of government oversight, increasing the expected cost of keeping informal workers.

We show that inspections have an immediate effect on workers' probability of staying in an informal job. On the quarter of inspection, the average quarterly probability that an informal worker becomes formal at the same work-site increases from 14% to 21%. Increasing the cost of informality through inspections increases within-firm transitions from

[1] For example, De Paula and Scheinkman (2011), Meghir, Narita and Robin (2015), and Ulyssea (2018)

[2] Formal firms are those registered with the government and paying profit taxes.

[3] We use the terms "firm' and "employer" interchangeably.

[4] We use the terms "work-site" and "establishment" interchangeably.

informal to formal jobs, and formal and informal job destruction. As a result, increasing the cost of informal employment reduces firm growth by 2 percentage points.

We find that workers formalized after an inspection have better outcomes with future employers. Relative to workers that were formalized without an inspection ("organically formalized workers"), employees that transition from informal to formal jobs due to an inspection are more likely to be poached. Conditional on separating, the probability of having a formal job at the next employer is higher than that of informal workers at non-inspected establishments and similar to that of "organically formalized" co-workers. Moreover, while wages at the inspected employer are 1.4% higher for organically formalized workers, this wage gap disappears after being poached. These results are consistent with employers using formality status as a signal of worker productivity.

We provide new facts on the prevalence of informal employment within formal firms, how it varies by firm size, and its dynamics.[5] We find that most informal to formal job transitions occur within the same firm while the majority of formal to informal flows are due to separations from the current employer. The probability of transitioning to a formal job at the same employer is highest in the first quarter of employment and decreases with on-the-job tenure and worker's age. Distinguishing between within and across firm changes in formality status is a relevant addition to the literature using microdata to analyze informal labor market dynamics such as Levy (2008), Bosch and Maloney (2010), Bosch and Esteban-Pretel (2012), and Gallardo del Angel (2013). The distinction is relevant for the literature using labor flows to assess the degree of labor market segmentation including Maloney (1999), Pratap and Quintin (2006), Ulyssea (2010), among others.

We also contribute to the theoretical literature. A standard modeling assumption is that the cost of informality is increasing in firm size (either in terms of productivity, capital, or number of employees). This assumption is based on the fact that larger firms are more likely to be formal (i.e. registered with the government), and rationalized by argu-

[5] In a concurrent paper, Bobba et al. (2019) also decompose within and across firms informal-to-formal job transitions. They identify these transitions using worker self-reported tenure data. Tenure data is not collected in ENOE on every quarter and can be subject to bunching so we instead use direct information on whether the worker is with the same employer as in the previous quarter.

ing that the probability of being detected increases with firm size.[6]. Ulyssea (2018) and Bobba et al. (2019) expanded this literature by explicitly modeling firms' decision to formalize some of their workers. In so doing, they kept the previous literature's assumption that the cost of hiring and keeping informal workers was linearly increasing in firm size. We provide empirical evidence of a negative, non-linear, correlation between the share of informal workers and firm size in Mexico.[7] Our findings on firms' responses to random inspections are consistent with larger firms facing higher costs from informal employment.

Our paper is also related to the literature analyzing the effects of enforcing labor regulation using geographical variation in the number of labor inspectors per capita (Ronconi (2010) in Argentina and Almeida and Carneiro (2005) and Almeida and Carneiro (2012) in Brazil) or field experiments (Levine et al. (2012) in the US and de Andrade et al. (2014) in Brazil). Using surveys with administrative data and a very large set of random work-site inspections over a long period of time allow us to have a broader scope. We analyze a wide range of response margins at the firm and worker-levels. We also examine the impact on current and future outcomes for informal employees and their formal co-workers. In so doing, this paper offers insights into how the costs and benefits of monitoring are distributed across different types of workers, and between workers and employers.

The paper is organized as follows. In section 1, we provide a description of the requirement to register workers with IMSS and the accompanying payroll taxes. We then describe IMSS's and STPS's enforcement tools. Section 2 describes the data. Section 3 develops a model with search frictions and asymmetrical information about workers' productivity across current and prospective employers who offer job seekers formal or informal contracts. We list the model's predictions regarding wage changes and transitions between informal and formal jobs within and across employers for workers at inspected work-sites. In section 4 we present empirical findings that support the model's assumptions and test its predictions in survey and administrative data. These results include the effects of monitoring on workers' current and future labor market outcomes, and the effects on inspected and non-inspected firms' size, hiring, poaching, and separations. Section 5 concludes.

[6]Rauch (1991), Fortin et al. (1997), de Paula and Scheinkman (2010), Leal (2014)

[7]This is consistent with Perry et al. (2007) findings that the share of informal workers is highest at firms with 2-5 workers, and rapidly declines with firm size.

1 Formal Jobs, Payroll Taxes, and Monitoring in Mexico

1.1 Registration requirements

In Mexico, employers must register all wage-earning employees with the Mexican Social Security Institute (IMSS) within 5 business days of hiring.[10] Registration with IMSS gives workers access to a set of benefits including public health care and day care services, maternity leave, sick leave, disability insurance, and a retirement fund, among others. IMSS provides these services and collects payroll taxes to finance them. Employers must deduct these taxes from each enrolled worker's paycheck.

Federal Labor Law levies the majority of the payroll tax on the employer.[9] Employers' contribution has a fixed and a variable component. The fixed cost is equal to 20.4% of the daily minimum wage (MW), times the number of days the employee worked in the period. Employers' contributions have an upper bound at a daily wage equivalent to 25 minimum wages (USD$101.45).[8] Due to the fixed fee, as a percent of the wage, employers' contributions as a percent of the worker's after tax wage ranges from 17% for an employee that earns 25 minimum wages to 35% for a minimum wage earner.

Table 1: Contributions to Government Mandated Benefits (per worker/day)

	Employer	Worker	Government	Total
Fixed Fee	20.4% $\times$ MW	0%	13.9%$\times$ MW	34.3% $\times$ MW
Proportional to wage				
If wage$<$ 3 MW	15.15%	2.375%	0.475%	18%
Added % on wage$\geq$ 3 MW	1.10%	0.40%	0%	1.50%
Upper bound	16.394%$\times$ 25MW	2.272% $\times$ 25MW		

Source: Own calculations based on payroll tax rates established in the Social Security Law[10].
MW refers to the daily minimum wage, equal to MXN$73.04 on January 2016.

[8]On January 2016, a constitutional amendment instructed that all references to the minimum wage in all federal and state laws and regulations should be understood as instead referring to the newly created Measurement and Revaluation Unit (*UMA*). When it was first introduced, 1 *UMA* was equal to 1 minimum wage, therefore, the amendment has no effect during our sample period.

In addition to payroll taxes, the Federal Labor Law imposes additional costs on formal employment. These include minimum wages, severance and overtime pay, paid leave and minimum vacation days, workers' right to training, safety and health conditions in the workplace, among others. The Federal Labor Law also requires businesses to distribute a portion of their after-tax profits among their registered employees.[9]

1.2 Monitoring

IMSS and STPS visit firms' work-sites to check whether they comply with labor regulations. STPS has 3 type of inspections: ordinary, extraordinary, and follow-ups. Ordinary inspections are the main focus of this paper and are explained in more detail in section 2.2. Extraordinary inspections are performed after a complaint by a worker, an accident at a work-site, or due to public safety concerns. Follow-up inspections are scheduled to verify whether firms corrected violations detected during previous ordinary or extraordinary inspections. While STPS inspections cover a range of labor regulations, IMSS inspections focus on identifying, and punishing, informal employment.[9]

STPS's self-proclaimed objective is to incentivize compliance, not mainly through fines but by helping employers understand and abide with the law. After each visit, STPS inspectors file a report and give a copy to the employer. The report details the results from the inspection, specifically, whether the inspector identified any violations to labor regulation. Except for dangerous or extreme cases (such as improper management of hazardous waste or use of child labor), the report points out violations, and employers have a pre-specified period of time to comply. STPS schedules a follow-up visit to verify that the corrective measures were implemented and fines the establishment only if any of the originally detected violations are still occurring.[9,37]

We argue that STPS inspections increase the cost of informal employment by raising the probability of a visit, and fine, from IMSS. Even though having informal workers is

[9] The exact parameters that IMSS uses to determine which establishments to inspect are confidential. However, according to IMSS officers in charge of inspections, they take into account firm size, industry, history of previous violations and notifications made to IMSS by STPS when deciding which firms to inspect.

one of the items on the inspection checklist,[10] fining firms for this violation falls outside STPS's jurisdiction. When they detect informal workers, the inspector documents it in the report and sends a notice to IMSS. STPS's notifications are one of the inputs IMSS uses to determine which establishments to inspect. Inspections can increase the cost of informal employment even if IMSS does not receive a notice from STPS, for example, through bribes or via lost output from hiding workers.

The cost of being caught by IMSS with informal workers is high. Employers are fined for each unregistered worker (from 20 to 250 minimum daily wages) and must pay back-due payroll taxes. Employers can also be charged with providing false information which carries an additional fine of 20 to 250 minimum wage for each worker. Fraud against IMSS is punishable with up to nine years in jail.[10] However, employers that register (or terminate) informal workers in the time between a STPS inspection and an IMSS follow-up reduce the likelihood of being fined.

Employers have incentives to formalize (or terminate) their workers promptly after receiving a visit by STPS. If an employer admits to having informal workers before being visited by IMSS, fines are partially, and sometimes entirely, waived. Moreover, employers who come forward with IMSS about their informal employment can request extensions and installment payment plans for their back-due payroll taxes.[11]

2 Data Description

This paper combines 3 datasets in a novel way: the quarterly National Employment and Occupation Surveys (ENOE), the Mexican Social Security Institute (IMSS) administrative employer-employee matched administrative-records, and the Ministry of Labor's Direc-

[10]STPS inspectors make note of how many workers are employed at the establishment and whether the employer provided proof of payroll tax payments to IMSS.

[11]Garcia, Kaplan and Sadka (2012) show that when workers include IMSS as a co-defendant in a labor dispute, firms are more likely to settle. The authors argue that notifying IMSS about the labor dispute raises the stakes for firms, since IMSS is the only entity with the authority to fine firms for not registering their workers. Our hypothesis about STPS inspections increasing the likelihood that firms register or fire workers, before a potential visit from IMSS, is consistent with their findings.

tory of Firms and Inspections Logs.[12]

ENOE's counterpart in the US is the Current Population Survey (CPS). Like the CPS, ENOE is the main source of labor market statistics in Mexico and, since INEGI allows public access to its micro-data, it has been extensively used in research papers. However, the information that allows identifying firms, including the firm's name, is removed from the publicly available data. This paper uses ENOE's employer-side data, which has been so far unexploited due to its confidentiality. Using firms' names and locations, we match ENOE to STPS's Firm Directory and Inspections Logs. Using employers' tax identifiers and establishments' location, we match IMSS administrative records with STPS's Inspections Logs. We next describe each of these datasets and Appendix B provides details on the matching process across them.

2.1 The National Employment and Occupation Survey (ENOE)

The dwelling is the sampling unit in ENOE. It gathers information regarding households' composition and dwelling characteristics, as well as extensive data on each household member such as age, education, gender, labor market participation, and job characteristics. Each quarterly sample is representative of the national labor market and includes 120,260 households and 420,000 individuals on average. Like the CPS, ENOE is a rotating panel: households are interviewed for five consecutive quarters and then replaced.[13]

We focus on wage-earning employees (as opposed as self-employed workers or independent contractors) since these are the workers employers must register with IMSS. We drop individuals in the agriculture sector and domestic workers. We further restrict the sample to individuals employed at firms included in the National Directoy of Firms (DNE) for at least one of the quarters when they participate in ENOE's survey. This guarantees that the sample includes only workers at formal firms that are both able to register

[12]This is, to our knowledge, the first paper to exploit these three sources of data. See Kumler, Verhoogen and Frias (2015) for an example using wage data from IMSS and ENOE to investigate the extent of tax-evasion via wage under-reporting.

[13]The attrition rate is 3% for the first quarter in the sample. 85% of households stay in the sample for a full year.

workers with IMSS[14] and are within the set of firms from which STPS selects randomly for inspections. The average sample size is approximately 26,000 individuals per quarter.

Table 2 shows the predicted quarterly transition rates in formality status within and across employers after controlling for workers' age, tenure, education, gender, firm size, occupation and sector fixed effects.[15] On average, half of all informal employees at a formal firm remain employed at the same establishment the following quarter; 73% of this half remain informally employed while the rest are "promoted" to a formal job. Meanwhile, 81.8% of all formal employees stay at the same establishment with the same formality status. The unconditional probability of separating to a different formal firm is similar for formal and informal employees (7.5% vs. 7.7%). However, workers who leave an informal job at a formal firm are 3 times more likely to again be hired as an informal worker by the new employer than workers that separate from a formal job.

Table 2: Predicted Quarterly Transition Probabilities

		Initial Labor Market Status	
Labor Market Status		Formal Establishment	
Next Quarter		Informal	Formal
Same Formal Firm	Informal	38.5%	1.1%
	Formal	14.2%	81.8%
Separation	New Formal Firm	7.7%	7.5%
	Other*	40.2%	9.1%
Conditional on Separating to a New Formal Firm			
New Formal Firm	Informal	64.5%	30.7%
	Formal	35.1%	69.3%

Source: Own calculations using data from ENOE (2005-2016) and DNE.
We calculate predicted probabilities using a multinomial logit. The sample consists of individuals employed at DNE firms for at least one of the quarters when they participate in ENOE's survey.
* "Other" separations include separating to a job as an employee at an informal firm, becoming self-employed, transitioning to unemployment and movements out of the labor force.

[14] In order to register a worker, a firm needs to itself be registered as a formal firm.
[15] Table 6 in Appendix B presents descriptive statistics for these control variables.

2.2 The Ministry of Labor and Social Welfare's National Firm Directory (DNE) and Inspection Logs

The National Firm Directory is a list of firm's establishments compiled by the Ministry of Labor and Social Welfare (STPS). It includes information on firm's name, unique tax identifier, the date on which the firm was registered in the DNE, and its establishments' addresses. In June 2016, there were 394,651 establishments in the DNE. Firms in the DNE are formal, that is, they are registered with the government and have an employer ID with IMSS and a tax ID with the Mexican Revenue Service (SAT). Since firms do not have a legal obligation to register in the DNE, it is not an exhaustive list of all formal firms operating in Mexico. Most firms enter the DNE after participating in a STPS training program[16] or through extraordinary inspections.

It is STPS's responsibility to visit work-sites to verify that they abide with labor regulations.[39] Between January 2005 and June 2016, STPS performed 620,842 inspections. Inspection bylaws indicate that establishments must be inspected periodically by random selection from the DNE.[38] In appendix C, we examine whether the probability of being inspected is correlated with worker or firm pre-inspection characteristics. We find evidence consistent with random allocation of inspections using both ENOE and IMSS's data. We also provide additional information about the time trend for inspections in the Appendix.

2.3 IMSS Administrative Records

For all formal workers, firms report to IMSS the day the formal employment relation started and the prevailing wage. Employers must notify IMSS whenever the wage changes and when the match is terminated. IMSS data is therefore a complete record of the wage

[16]These programs offer firms training that helps them comply with regulation and bid for government contracts. STPS also provides registered firms free courses and worker training. Firms can also be added to the DNE after a complaint is filed against them. In recent years, STPS has made additional efforts to exchange information with other government entities and private sector institutions that have data on establishments operating in Mexico.

and tenure history for the universe of formal employment relationships. Each observation in the data is an employer-worker-wage match. When a new formal match begins, we can directly observe whether the worker previously had a formal job or not. However, for those workers who did not have a formal job before the new match, we cannot determine whether they were unemployed or informally employed beforehand. IMSS records also include information on workers' date of birth and gender. For each employer, we observe an IMSS employer-specific identifier[17] and their tax ID[18], industry, and location.

For our empirical analyses, we construct two datasets from IMSS administrative records each ranging from January 2005 to April 2019: an employer level one and a worker level one. The employer data is a quarterly panel that tracks employers' size, wage distribution, job creation and job destruction. For job flows, we distinguish between jobs created by poaching workers from other formal firms and new formal jobs (i.e. jobs filled by workers that were not formally employed in the previous quarter). We make an analogous distinction between job destruction where workers separate to another formal job versus separations into informality. We exclude employers that are not included in the DNE.[19]

Our employer-level sample tracks the formal labor market trajectory of all workers who were ever formally employed at a firm included in the DNE. In this dataset, we record the worker's entry and exit date from the DNE firm, as well as her starting and final wages at that firm. We also identify the previous and next firm of formal employment and the wages at these prior and future jobs.

The matched DNE-IMSS employer data includes 23,935 distinct employers and over 950,000 observations. Consistent with previous findings regarding the size distribution of firms in Mexico, most employers in our data are small.[20]. 91% of firms have 5 workers or less. Table 8 in the Appendix B shows descriptive statistics and compares the sectoral composition in our sample and the full universe of IMSS employers.

[17]IMSS assigns an identifier, known as *Registro Patronal*, to each employer. As we discuss in more detail in Appendix B, each firm (or Tax ID) can have more than one employer ID (*Registro Patronal).*

[18]*Registro Federal de Contribuyentes* or RFC.

[19]Firms are identified using names and tax ID's. A firm can have more than one employer ID. Appendix B discusses matching employers to firms, and then firms in the IMSS data to firms in the DNE.

[20]See, for example, Levy (2008) and Hsieh and Klenow (2014)

3 A Model of Asymmetric Employer Learning with Informal and Formal Jobs

In this section, we develop a model to guide our empirical analysis. The goal of the model is to provide testable predictions regarding workers' transitions between informal and formal jobs, and firms' responses to inspections.

3.1 Model setup

The economy is comprised of a continuum of workers and a large mass of employers. Workers differ in terms of their productivity, θ, which is fixed. Firms use the same production technology but the costs of having informal workers varies with firm size, measured in number of formal workers. We consider three size categories: small (S), medium (M), and large (L). Let α_z, $z \in \{S, M, L\}$, denote the employment-weighted firm shares and $P(\theta)$ the distribution of workers' productivity in the economy. Both $P(\theta)$ and α_z are common knowledge.

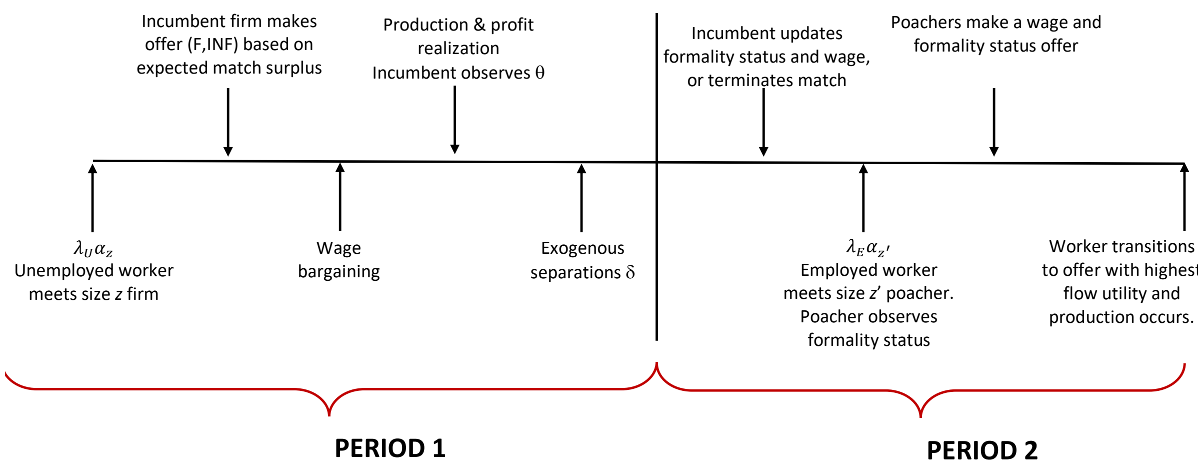
Workers are equally productive in all firms and all jobs, but employers do not observe this productivity before hiring. Upon meeting a worker, employers unilaterally choose whether to offer a formal or an informal job,[21] or to keep searching, maximizing their expected profits.

We consider a two period model. Figure 1 depicts the timing of events. In the first period, all workers are unemployed and firms have symmetric information about the distribution of workers' productivity. At the end of the first period, production occurs and firms that matched with a worker observe her productivity. Matches are destroyed with exogenous probability δ after production occurs.

[21]In a related paper, Bobba et al. (2019) also assume firms there is no bargaining over formality status. In their model, workers learn on the job and endogenous transitions from informal to formal jobs are due to human capital accumulation. In our framework, workers' ability is fixed and transitions instead occur due to employer learning about worker's type. While both types of learning likely play a role in transitions, our finding that within-firm transition from informal to formal jobs are decreasing with workers' age and tenure is at odds with workers on-the-job learning as the main driver.

At the start of the second period, incumbents (i.e. first-period employers[22]) choose whether to endogenously terminate matches, formalize informal workers, or switch formal workers to an informal job, and whether to maintain or increase wages.[23] After incumbents make these decisions, poachers (i.e. unmatched firms) meet an employed worker with probability λ_E and an unemployed one with probability λ_U. Poachers who meet a worker cannot observe the worker's wage or productivity. However, they can use the worker's formality status to update their priors about the expected profits from a match with that worker. We assume poachers can also observe the current employer's size.[24] Workers choose the offer that maximizes their period 2 utility.

Figure 1: Timing



We next describe firms' expected profits (Π^{INF}, Π^{F}). The productivity of a match equals workers' productivity, θ. In formal jobs, firms pay a proportional pay-roll tax τ_p and a fixed-fee τ_f.[25] With informal jobs, firms avoid these taxes but instead pay an informality

[22]We adopt the "incumbents vs. poachers" terminology from Michaud (2018).

[23]In Michaud (2018) downward wage rigidity prevents employers from offering an initial high wage to poach workers and reducing wages afterwards. The timing of offers in our setup, coupled with there only being one production period after wages are set, eliminates this incentive. We maintain the assumption of downward wage rigidity for simplicity.

[24]We believe it is reasonable to assume size is an observable firm characteristic. We can instead assume poachers only know the distribution of firm size in the economy. The main results follow through.

[25]These taxes are meant to reflect the payroll tax structure presented in Table 1.

cost $c(\theta, z)$ that is an increasing function of worker productivity and firm size.[26]

$$\pi^{INF}\left(\theta, w^{INF}, z\right) = \theta - w^{INF} - c(\theta, z) \tag{1}$$

$$\pi^{F}\left(\theta, w^{F}\right) = \theta - w^{F}(1+\tau) - \tau_F \tag{2}$$

Unemployed workers receive flow utility $b_U > 0$. The flow utility of employed workers in equals the wage, w^F and w^{INF}, for formal and informal jobs.

Firms are risk-neutral and can update their offers after observing productivity. Therefore, each period can be treated as a separate maximization problem. Given the information available to them at the beginning of the period, employers offer workers the job that maximizes expected profits in that period. In the next sections, we solve for incumbents' and poachers' optimal offers. We then show how firms' decisions change when they receive an inspection (before and after hiring a worker), and how this affects workers outcomes at the inspected firm and with future employers.

3.1.1 Period 1

Wages at the beginning of the first period are determined via Nash-bargaining over the expected match surplus. We assume workers' bargaining power (β) is homogeneous across workers and job types. Let $\bar{\theta}$ be the average labor productivity in the economy. In the first period, all *z*-sized firms that meet a worker offer the job type where the average worker yields the highest expected profit.[27] Let $\theta_1^{F,z}$ be the value of productivity that makes a *z*-size firm indifferent between a formal and an informal job given Nash Bargaining over wages. Size *z*-firms will offer formal (informal) jobs if $\bar{\theta} \geq (<)\, \theta_1^{F,z}$.

[26] In section 4, we argue that firms' responses to random monitoring are consistent with a positive and non-linear correlation between the costs of informal employment and firm size.

[27] If the average worker in the economy is such that for a *z*-sized firm the expected surplus of both formal and informal jobs is negative, then none of the *z*-sized firms make offers in the first period, and any worker that matches with a *z*-size firm continues on to the next period as unemployed. Under some parameter values, firms could be willing to make an offer to a worker even if the expected profit in period 1 is negative in order to learn about the workers' productivity. Given the values for our calibrated parameters, this does not occur. Firms that match with a worker in the first period always make an offer and poachers partially "free-ride" on incumbents' learning.

Incumbent Maximization Problem. Period 1:

$$\max\left\{\underbrace{\mathrm{E}\left[\Pi^{INF}\left(\theta, w_1^{INF,z}, z\right)\right]}_{\text{hire all informally}}, \underbrace{\mathrm{E}\left[\Pi^{F}\left(\theta, w_1^{F}\right)\right]}_{\text{hire all formally}}, \underbrace{0}_{\text{no hires}}\right\} \qquad \forall z \in \{S, M, L\} \tag{3}$$

where $\mathrm{E}\left[\Pi^{INF}\left(\theta, w_1^{INF,z}, z\right)\right] = \int_{\theta_L}^{\theta^H} \left(\theta - w_1^{INF}\left(\bar{\theta}, z\right) - c\left(\theta, z\right)\right) p\left(\theta\right) d\theta$

$$\mathrm{E}\left[\Pi^{F}\left(\theta, w_1^{F}\right)\right] = \int_{\theta_L}^{\theta^H} \left(\theta - w_1^{F}\left(\bar{\theta}\right) \times \left(1 + \tau_p\right) - \tau_F\right) p\left(\theta\right) d\theta$$

3.1.2 Period 2

Incumbents know that with probability $\lambda_E \times \alpha_{z'}$ their worker will meet a size-z' poacher. They are also aware that poachers are uninformed about workers' productivity, but will make identical inferences about θ given their information set which includes the worker's formality status and current employer's size. Incumbents do not observe poachers' actions but they expect outside offers to reflect poachers' profit maximizing behavior.

Let $w_1^{k,z}$ and $w_2^{k,z}$ be the equilibrium wages in a *k*-type job, $k \in F, INF$ at size-z incumbents in the beginning of periods 1 and 2, respectively. Let $w^{P,z'}\left(k, z\right)$ be the equilibrium offer that a size z' poacher makes if they meet a worker employed in a *k*-type job with a size-z incumbent. The worker accepts the poacher's offer if the wage is higher than with the current employer.[28] If the worker leaves, the incumbent's profits are zero. If the wage offered by the incumbent at the beginning of period 2 is higher than the poacher's offer (or if the worker did not meet any poachers), the worker stays with the incumbent. By increasing wages, incumbents decrease the set of competing poachers but also decrease their share of the match surplus. Equation 4 below presents the maximization problem for incumbent firms at the beginning of period 2.

Taking incumbents' optimal responses as given, poachers update their prior on em-

[28]For simplicity, we assume workers only care about wages. Alternatively, we could assume workers also differ in terms of how much they value a formal job relative to an informal one. The set of poachers an incumbent is willing to compete with would then be defined over wage levels and job types.

ployed and unemployed workers' productivity. Let $\gamma_{k,z}(\theta)$ be the updated probability that the productivity of a worker employed at a k-job with a z-size firm equals θ. Further, let $\theta\left(w^P(k,z)\right)$ be the highest productivity worker that poachers can successfully attract with a wage equal to $w^P(k,z)$. Equation 5 presents the size-z' poacher maximization problem conditional on meeting an employed worker.

Incumbents Maximization Problem. Period 2:

$$\max\Bigg\{ \max_{w_2^{INF,z}} \Big\{ \left(1-\lambda_E\left(1-\sum_{z'}\alpha_{z'}\mathbf{I}\Big[w^{P,z'}(INF,z)<w_2^{INF,z}\Big]\right)\right)\Big(\theta-w_2^{INF,z}-c(\theta,z)\Big)\Big\},$$
$$\max_{w_2^{F,z}} \Big\{ \left(1-\lambda_E\left(1-\sum_{z'}\alpha_{z'}\mathbf{I}\Big[w^{P,z}(F,z)<w_2^{F,z}\Big]\right)\right)\Big(\theta-w_2^{F,z}\times(1+\tau_p)-\tau_F\Big)\Big\},0\Bigg\} \tag{4}$$

s.t. $w_2^{F,z}, w_2^{INF,z} \geq w_1^z$ where $\mathbf{I}\Big[w^{P,z'}<w_2^z\Big]$ is equal to 1 if a z'-size poacher's equilibrium wage offer is lower than than the incumbent's, and w_1^z is the first period wage.

Poachers Maximization Problem

$$E[\Pi^{P,z'}(\theta)|k,z] = \max\Bigg\{ \max_{w^{P,F,z'}(k,z)} \Big\{ \int_{\theta^{k,z}}^{\theta\left(w^{P,F,z'}(k'z)\right)} \Big(\theta-w^{P,F,z'}(k,z)(1+\tau_p)-\tau_F\Big)\gamma_{k,z}(\theta)\,d\theta\Big\},$$
$$\max_{w^{P,INF,z'}(k,z)} \Big\{ \int_{\theta^{k,z}}^{\theta\left(w^{P,INF,z'}(k,z)\right)} \Big(\theta-w^{P,INF,z'}(k,z)-c(\theta,z')\Big)\gamma_{k,z}(\theta)\,d\theta\Big\},0\Bigg\} \quad \forall z'\in\{S,M,L\} \tag{5}$$

3.2 Equilibrium

Strategies for each size-z incumbents and each size-z' poachers are mappings from their available information set to an offer, taking competing employers' actions as given. z-incumbents' strategies are a wage and job type (k) for each type of worker $w_2^{k,z}(\theta)$, z'-

poachers' strategies are a wage and job type (k') for all workers employed in a job k at a z-size incumbent $w^{P,z',k'}(k,z)$, and a strategy for each type of workers is a quit decision $Q\left(w_2^{k,z}(\theta), w^{P,z',k'}(k,z)\right)$. In a Symmetric Perfect Bayesian Equilibrium, strategies are sequentially rational, poachers' beliefs about workers' productivity are consistent with equilibrium distributions, and the productivity distributions for unemployed workers for and employed workers in each job and firm size category are with optimal strategies. We next characterize such an equilibrium.

We first consider incumbents' strategies. Only 4 possible wages can be part of an equilibrium strategy for incumbents: to keep the wage from period 1 unchanged, or to offer a wage equal to one of the offers expected from the three type of poachers, up to the match's profits. An incumbent that increases the wage without matching an outside offer has lower profits if the worker does not meet a poacher (with probability $1-\lambda_E$) and does not keep the worker if an outside offer arrives. Therefore, any increase in wage must at least match the lowest expected outside offer to be an equilibrium strategy.

Incumbents that keep first period wages receive higher profits if the worker does not meet a poacher, but lose the worker with certainty if there is an outside offer. Equations 6 and 7 present the expected change in profits from increasing a formal and informal worker's wage to compete with a size-z' poacher's offer. Incumbents will increase formal (informal) workers' wages to match z'-sized poachers' offer[29] if their productivity θ is such that equation 6 (7) is positive. For workers with lower productivity, incumbents will keep the first period wage unchanged. Then, they will choose the type of job that maximizes their expected profits given these equilibrium wages following a threshold rule: formalize workers with $\theta \geq \theta^{F,z}$, offer informal jobs to workers with $\theta^z \leq \theta \leq \theta^{F,z}$ and terminate matches where $\theta < \theta^z$.[30] Equation 4 determines these thresholds.

[29]Incumbents choose period 2 wages before outside offers arrive. However, employers can rationally predict the poachers' equilibrium wage offers.

[30]Incumbents follow a threshold rule because profits from a formal match are linearly increasing in productivity while the costs of an informal match are increasing and convex on worker productivity.

Change in Incumbent Profits from Competing with Poachers

$$\frac{\delta\, E[\Pi^{F}\left(\theta\right)]}{\delta w = w^{P,z'}\left(F,z\right) - w_1^{k,z}} = \left(1-\lambda_E\right) \underbrace{\left(w_1^{k,z} - w^{P,z'}\left(F,z\right)\right) \times \left(1+\tau_p\right)}_{\text{Loss in Profits if there is no outside offer}}$$
$$+ \lambda_E \times \underbrace{\left(\sum_{z' s.t. w^{P}(F,z) \leq w^{P,z'}(F,z)} \alpha_{z'} \right)}_{\text{Measure of poachers with} \leq \text{offers}} \underbrace{\left(\theta - w^{P,z'}\left(k,z\right)\left(1+\tau_p\right) - \tau_f\right)}_{\text{Gain in profits from competing with poachers}} \tag{6}$$

$$\frac{\delta\, E[\Pi^{INF,z}\left(\theta\right)]}{\delta w = w^{P,z'|INF,z} - w_1^{k,z}} = \left(1-\lambda_E\right) \underbrace{\left(w_1^{k,z} - w^{P,z'|INF,z}\right)}_{\text{Loss in Profits if there is no outside offer}}$$
$$+ \lambda_E \times \underbrace{\left(\sum_{z' s.t. w^{P|INF,z} \leq w^{P,z'|INF,z}} \alpha_{z'} \right)}_{\text{Measure of poachers with} \leq \text{offers}} \underbrace{\left(\theta - w^{P,z'|INF,z} - c\left(\theta,z\right)\right)}_{\text{Gain in profits from competing with poachers}} \tag{7}$$

We next characterize poachers' strategies. A size-z' poacher updates its prior about workers they meet based on the worker's current employment situation. Poachers rationally expect that higher wages will increase the average quality of the set of workers that they can successfully poach. The equilibrium poacher's offer is such that the increase in the expected productivity of the match is equal to the increase in its total cost. Taking the derivative from the poacher's maximization problem in equation 5, their equilibrium strategy is characterized in equation 9. Given these equilibrium wages, poachers then choose the job type that maximizes their profits.

Firms' learning about workers' productivity, and hence about the cost of offering them an informal contract, leads to within-firm changes in formality status. The threat of outside offers is what drives within-firm wage growth. Even though workers are equally productive in formal and informal jobs, and productivity is fixed, workers experience wage growth in both types of jobs. Wage growth is higher for formal workers, both within and across firms, because poachers expect higher productivity from formal workers and incumbents realize that formal workers are more expensive to retain. This leads incumbents to formalize fewer workers than they would if there were no outside offers.

Poachers' Updated Beliefs

$$
\begin{aligned}
E[\theta|F,z] = \overline{\theta^{F,z}} &= \int\limits_{\max\{\theta^{F,z},\theta^{z}\}}^{\theta^{H}} \theta \frac{p(\theta)}{1-P\left(\max\{\theta^{F,z},\theta^{z}\}\right)} d\theta = \int \theta \gamma_{F,z}(\theta)\, d\theta \\
E[\theta|INF,z] = \overline{\theta^{INF,z}} &= \int\limits_{\theta^{z}}^{\max\{\theta^{F,z},\theta^{z}\}} \theta \frac{p(\theta)}{P\left(\max\{\theta^{F,z},\theta^{z}\}\right)-P\left(\theta^{z}\right)} d\theta = \int \theta \gamma_{INF,z}(\theta)\, d\theta \\
&\forall z \in \{S,M,L\} \\
E[\theta|U] = \overline{\theta^{U}} &= \left(1-\lambda_U(1-\delta)\right)\overline{\theta} + \lambda_U(1-\delta)\left(\sum_{z\in\{S,M,L\}} \alpha_z \int\limits_{\theta_L}^{\theta^{z}} \theta \frac{p(\theta)}{P(\theta^{z})} d\theta\right)
\end{aligned}
\tag{8}
$$

Poachers' Equilibrium Wage Offers

$$
\begin{aligned}
w^{*P,F,z'} &= \frac{\left(\theta\left(w^{*P,F,z'}\right)-\tau_F\right)}{1+\tau_p} - \frac{1}{\frac{\delta\theta}{\delta w^{P,z'}}} \frac{\Gamma\left(\theta\left(w^{*P,F,z'}\right)\right)-\Gamma\left(\theta^{F,z}\right)}{\gamma_{k,z}\left(\theta\left(w^{*P,F,z'}\right)\right)} \\
w^{*P,INF,z'} &= \left(\theta\left(w^{*P,INF,z'}\right) - c\left(\theta\left(w^{*P,INF,z'}\right),z'\right)\right) - \frac{1}{\frac{\delta\theta}{\delta w^{P,z'}}} \frac{1}{\gamma_{k,z}\left(\theta\left(w^{*P,INF,z'}\right)\right)}
\end{aligned}
\tag{9}
$$

where $\theta\left(w^{*P,z'|k,z}\right)$ is the productivity threshold above which incumbents are willing to pay a higher wage than $w^{*P,z'|k,z}$ in order to keep the worker. This threshold is determined by the incumbents' maximization problem, and is equal to the value of θ that makes equations 6 and 7 equal to zero.

The purpose of presenting a theoretical model in this paper is not to match worker flows, wage paths, nor the magnitude of firms' responses to enforcement shocks. Instead, we seek to obtain insights into firms' trade-offs between formal and informal jobs that will guide the interpretation of our empirical results. Nonetheless, we do set some parameters to match the data in the sample of non-inspected DNE firms.

First, we set each α_z to match small (1-3 employees), medium (4-10), and large (11+)

firms' employment weighted shares in the economy.[31] For the cost of informality, we assume the functional form $c = (\theta\tilde{z})^{\gamma}$ where $\tilde{z}$ is the size ratio of each firm category relative to the smallest group. We set $\gamma = 2$.[32]. Finally, we set λ_E, λ_U, and δ to match the job-to-job finding rate, the finding rate from unemployment, and the overall separation rate. The taxes in the formal sector, τ_F and τ, are 32% and 20%, respectively. We assume that worker's productivity in the economy follows a Gamma distribution, $\Gamma(8, 20)$.

3.3 Inspections as a shock to the cost of informal employment

In this section, we analyze the effect of inspections on hiring, formalization, and wage setting decisions by incumbents and poachers, when each of them are subject to an inspection. We model inspections as a shock that increases the cost of informal matches such that $c^{INS}(\theta, z) = (1 + x) \times c(\theta, z) \forall z \in \{S, M, L\}$, with $x \in \{0, 1\}$. Inspections arrive at the end of the first period. For incumbents, this is after productivity is observed but before the decision to set wages and formality status for the second period. For poachers, it is before a hiring decision is made. We assume that upon meeting an employed worker, a poacher does not know whether the current employer has been inspected or not.

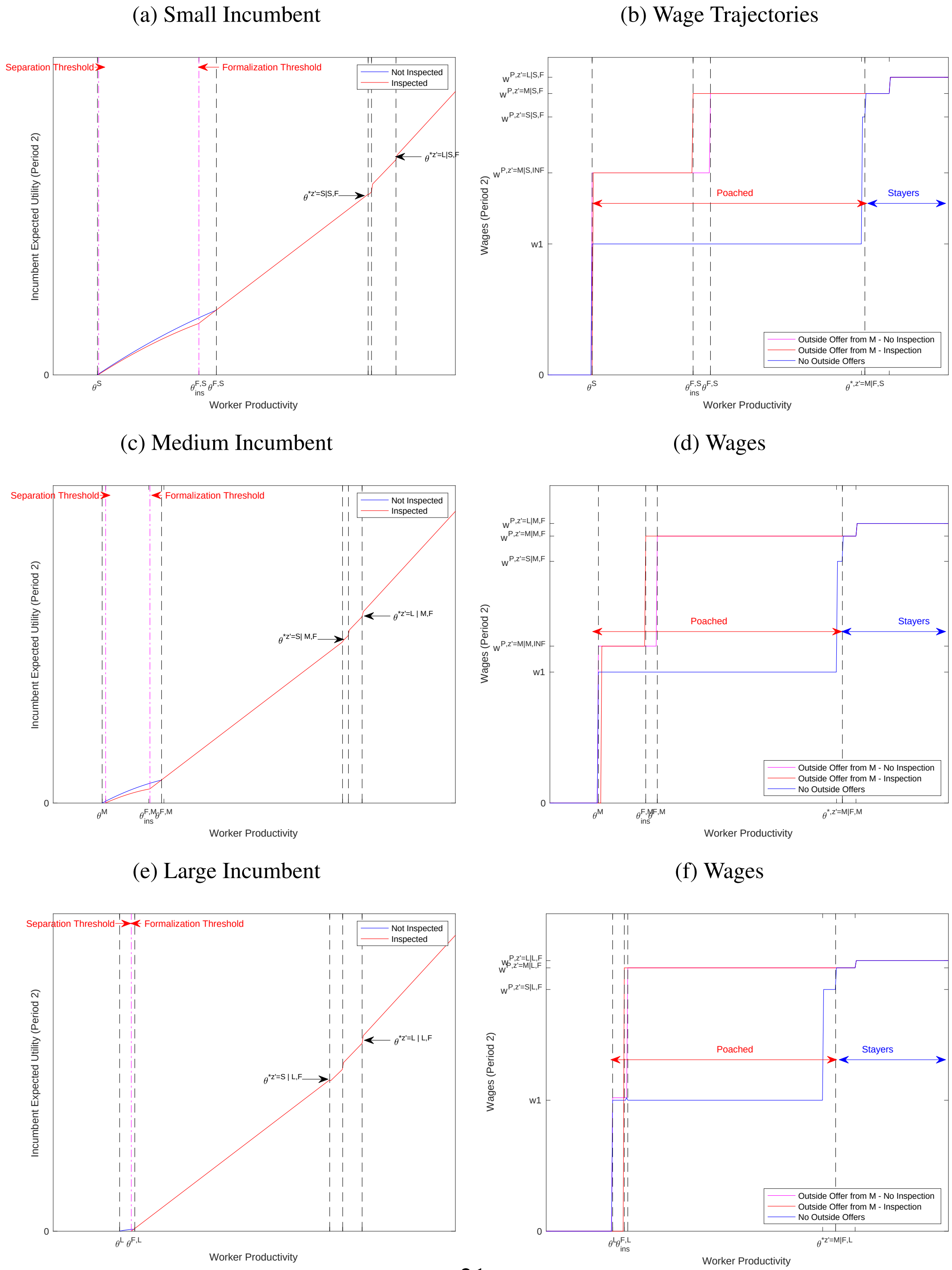
We start with the effects of inspecting incumbents. As shown in Figure 2, inspections increase both endogenous separations (increase the match termination threshold, θ_z) and formalizations (decrease the threshold offer formal jobs, $\theta^{F,z}$) for all firms. The change in the share of formal workers is highest at small firms since larger employers already formalize most of their workers. Because inspection-driven formal[33] workers are indistinguishable from workers that would have been formalized even without an inspection (i.e. workers with $\theta > \theta^{F,z}$), poacher's offers do not change after an incumbent is inspected.

[31] The values of these parameters are consistent with those estimated in Levy (2018)(Levy, 2018).

[32] Appendix D provides evidence consistent with $\gamma > 1$ based on the heterogeneous responses to inspections by firm size.

[33] We will refer to "inspection-driven" formal workers as the set of workers formalized after an inspection who the firm would have been kept as informal in the absence of monitoring ($\theta^{F,z,Inspect} < \theta < \theta^{F,z}$).

Figure 2: Inspecting Incumbents: Effects on Layoffs, Formalization, and Wages



Next, we consider inspections' effects on poachers. Inspections increase the cost of offering informal jobs but do not change poachers' expectations about the productivity of workers they meet. Therefore, an inspection affects the hiring decision only when the expected surplus of the match is higher in an informal job. For these matches, monitoring can lead to either the poacher switching to a formal job offer or to not making an offer at all. Whether poachers' decisions change depends on the magnitude of the rise in the cost of informality caused by inspections.[34] The rows labeled "Min. $\Delta c(\theta, z)$" in Table 3 show the minimum percent increase in the cost of informal employment required to change a poacher's optimal informal offer to either a formal offer or to not making an offer.

Table 3: Inspecting Poachers: Effects on Hiring and Formal Job Offers

			Incumbent Size and Current Formality Status						
			Small		Medium		Large		Unemp.
Poacher Size and Inspection Status			INF	F	INF	F	INF	F	
Small	Not Inspected		INF	F	INF	F	INF	F	INF
	Inspected	Min. $\Delta c(\theta, S)$	84%	0	233%	0	363%	0	1,139%
		New Offer	F	F	F	F	No Hire	F	F
Medium	Not Inspected		F	F	INF	F	INF	F	F
	Inspected	Min. $\Delta c(\theta, M)$	0	0	50%	0	106%	0	0
		New Offer	F	F	F	F	No Hire	F	F
Large	Not Inspected		F	F	F	F	INF	F	F
	Inspected	Min. $\Delta c(\theta, L)$	0	0	0	0	18%	0	0
		New Offer	F	F	F	F	No Hire	F	F

Notes: Each cell in the "Not Inspected" rows shows the optimal job type offered by non-inspected poachers that meet a worker with the current labor/formality status and incumbent size indicated by the column headers. The "min $\Delta c(\theta, z')$ show the minimum percent increase in the cost of informal employment that inspections need to generate for a size-z' poacher to change their offers. The "New Offer" rows indicate optimal poacher's behavior assuming that inspections increase the cost of informal employment by min. $\Delta c(\theta, z')$.

The change in the cost of informal employment that an inspection needs to generate in

[34] We assume throughout that the cost of informal employment after an inspection is $x\%$ higher than without an inspection and the value of x is homogenous across firms of all sizes. Assuming a continuous productivity distribution, for all positive values of x, inspections increase the productivity threshold for separations of informal workers and decreases the formalization threshold for incumbents of all sizes. Whether inspections change poacher's hiring decisions depends on the magnitude of x.

order to change poacher's hiring behavior is decreasing in firm size. A large-sized poacher that meets an informal worker at an equal-sized incumbent would offer the worker an informal job in the absence of an inspection. If inspections increase the cost of informal employment by at least 18%, large poachers will no longer make job offers to workers informally employed at large incumbents. Our model does not track the evolution of firm size, however, table 3 and figure 2 are consistent with larger firms shrinking after an inspection (due to reduced hiring by poachers and increased separations at incumbents).

Table 3 also shows that, conditional on meeting a worker currently in an informal job, the probability that the poacher offers an informal job depends on the incumbent's size relative to the poacher's. Because informal employment is "cheaper" at smaller firms than at larger ones, a formal job at a small incumbent is a stronger signal of high worker productivity that at a larger firm. Conversely, an informal job at a small firm is not necessarily reflective of low worker productivity.

4 Empirical Findings

STPS's goal when monitoring establishments is to increase compliance with labor regulation. In the case of informal employment, inspections can achieve this goal by raising employers' awareness of the government's presence and increasing the expected probability of being fined by IMSS, thus raising the expected cost of having informal workers.

Our model has clear predictions about firms' responses to inspections. First, the share of formal workers increases at firms of all sizes after an inspection through 2 channels: an increase in within-firm informal-formal transitions and an increase in separations for informal workers. Within-firm informal to formal job inflows are largest at small firms. If inspections arrive before a firm and a worker meet, there are fewer hires, more separations, and lower firm growth. Workers formalized after an inspection are poached at higher rates (because incumbents are less willing to compete and poachers cannot distinguish between "inspection-driven" and "organic" formalizations. Conditional on separating from an inspected incumbent, formal workers receive similar offers from poachers, regardless of the motives behind their transition to formality. We test these predictions in this section.

4.1 Transitions out of informality

We begin by estimating the effect of inspections on informal workers' labor market transitions. Between any two consecutive quarters, an informal employee at a formal work-site can either remain informally employed with the same employer or switch to any of six mutually exclusive labor market states: (1) unemployment, (2) formal job with the current employer, (3) formal or (4) informal job with a new formal employer, (5) informal job at an informal firm, and (6) out of the labor force.

We model the probability of transitions out of informal employment into each of these six possible labor market states using the multinomial logit model specified in equation 10. $\Pi\left(TI_{i,j,t}=x\right)$ is the probability that worker i, who is informally employed at firm j in period t, transitions to market state x in period $t+1$ where x is each of the 6 possible transitions listed above, conditional on worker and firm characteristics $X_{i,j,t}$. $V_{j,t}$ is a dummy variable indicating whether STPS detected a regulatory violation during the inspection and $Fine_{j,t}$ is the sum of fines received by j so far (including fines imposed on quarter t). $Inspected_{i,t,q}$ is an indicator variable equal to 1 if in period t individual i was employed at a firm that received an inspection on period $t-q$, $q\in[-3,3]$ and 0 otherwise. The coefficients of interest, β_q^x, capture the time-varying effects of inspections.

$$\Pi\left(TI_{i,j,t}=x\right)=\sum_{q=-3}^{3}\beta_q^x Inspected_{i,t,q}+X'_{i,j,t}\eta^x+\alpha^x V_{j,t}+\gamma^x Fine_{j,t}+t+j+\epsilon_{i,j,t} \quad (10)$$

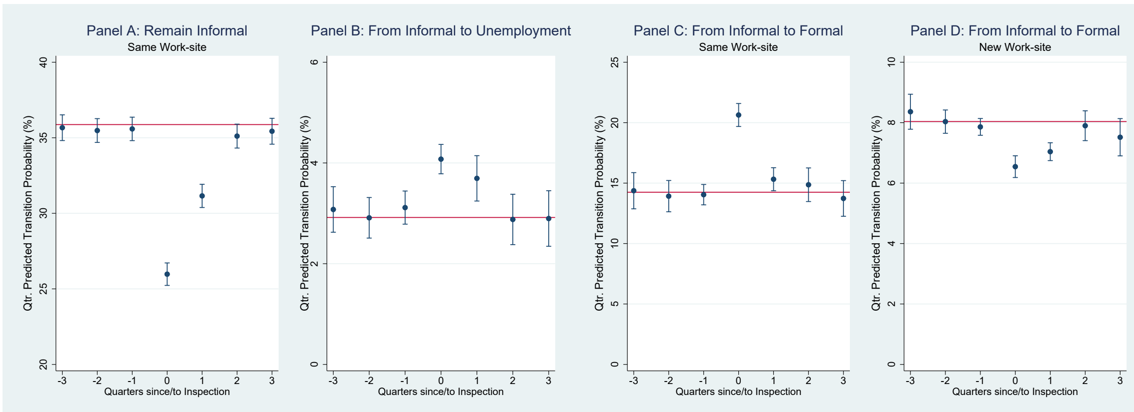
$\forall x\in$ Unemployed, Formal with same employer, Formal and Informal with new formal employer, Informal employer, Out of the labor force.

Panels A to D in Figure 3 below show the dynamic effect of inspections on transitions before and after the inspection occurs (β_q^x). Each panel plots the average quarterly transition probability for informal workers at inspected work-sites (the treatment group) into a different labor market state before and after the inspection.[35] The blue line shows the

[35]Since our model does not capture the labor force participation margin, nor the behavior of informal

average transition probability for the control (informal workers at work-sites included in the DNE that did not receive an inspection within a three-quarter rolling window).

Figure 3: Informal Workers' Transition Probabilities by No. of Quarters since Inspection



Notes to Figure 3 Panels A to D: These figures display the effect of inspections on the probability of transitioning out of informal employment on the treated (inspected) and control groups (not inspected) by number of quarters until and since an inspection occurs. $q = 0$ indicates the quarter of inspection. The sample includes informal workers employed at work-sites included in the DNE between 2005-2015. For each value of q, the treatment group includes all informal workers employed at an establishment inspected q quarters ago. The control group includes informal workers at establishments in the DNE that were not inspected within a [-3,3] quarter window. The solid red line shows the quarterly transition rate dependent mean. The effect on the treated is calculated from the β_q coefficients in the system of equations, (10). Control variables include worker age, gender, education, household size, number of children in day-care age, occupation and industry dummies, i's tenure with employer j, and firm size (as reported by individual i in period t). Errors are clustered at the work-site level.

Informal employees at inspected and non-inspected work-sites exhibit similar average quarterly transition rates out of informality before an inspection occurs, consistent with random work-site selection. This random assignment allows for consistent reduced form estimation of the effect of inspections on labor market flows.[36]

Consistent with our interpretation of inspections as an increase to the expected cost of maintaining informal employees, on the quarter of inspection, the quarterly probability of remaining informally employed at the same work-site decreases from 35% to 26% (Panel

firms, we relegate the results showing inspections' impact on transitions out of the labor force and to the informal sector to Appendix D.

[36]We present additional evidence on random allocation of inspections in Appendix C.

A). This drop is due to a 50 percent increase in the quarterly within-worksite formalization rate (from 14% to 21%) on the quarter of inspection (Panel B) and an increase in the average quarterly probability becoming unemployed from 2.9% to 4.1% (Panel C).

Panels D presents the effect of inspections on informal workers' quarterly transition probabilities to a formal job with a new employer. In our model, inspections affect workers' outcomes with a future employer only through the effect on their formality status at the inspected employer. Panel D shows that quarterly transition rates to formal jobs with a new employer decrease (from 8 to 6.5 percent) on the quarter of inspection. A possible explanation for this is that inspected workers are more likely to experience an unemployment spell between quarterly job transitions that is unobserved to us, but observed by the new employer. Unemployed workers have lower probabilities of receiving formal job offers than informal workers.

The model outlined in section 3 abstracts from informal firms (i.e. firms that are unregistered and can only hire workers informally). The model does predict, however, an increase in separations from formal firms for workers with lower productivity. Panel F in the Appendix shows that informal employees at inspected formal establishments have a 2.3 percentage points higher probability of separation to an informal firm.

4.2 Job creation and job destruction

IMSS data, by definition, does not include informal employment spells. However, whenever a formal spell begins, we can identify whether the worker was previously at a formal job with a different firm or not. Using this distinction, we consider two types of job creation: formalization and poaching. Poaching refers to formal-to-formal job transitions with different employers. Formalizations refer to job creation from outside the formal sector, including worker flows into formal jobs out of non-employment and within-firm informal to formal job transitions. Panel C Figure 3 shows an increase in individual workers' probability of within-firm formalization. We therefore expect to see an increase in in job creation from outside the formal sector in IMSS data. We test this, and other firm-level effects of inspections, using equation 11.

Inspections that occur very close together may have different effects than those that occur infrequently. Frequent inspections may have a cumulative effect on employers' awareness of monitoring activities by the government. Alternatively, multiple inspections within a short period of time might have decreasing effects as employers decrease their share of informal employment or if they increasingly perceive an IMSS follow-up as an empty threat. Equation 11 allows for heterogeneous changes in the expected cost of informal employment by time elapsed between inspections and cumulative total inspections.

$$\begin{aligned}\sinh^{-1} Y_{j,t} =& \sum_{q=-6}^{6} \alpha^{1,q} Inspection^{1}_{j,t,q} + \sum_{n=2}^{N_j} \delta^{v} Overlap_{n,j} + t \times industry + j \\ &+ \sum_{n=2}^{N_j} \left(\alpha^{n,q} Inspection^{n}_{j,t,q} + \sum_{q=-6}^{6} \gamma^{n,q} Inspection^{n}_{j,t,q} \times Overlap_{n,j} \right) + \epsilon_{j,t}\end{aligned} \tag{11}$$

where $Inspection^{n}_{j,t,q}$ is an indicator variable equal to 1 if firm j received its n^{th} inspection[37] q quarters ago.[38] and $Overlap^{n}_{n,j}$ is equal to 1 if inspection n happened up to 12 months after inspection $n-1$, and 0 otherwise.

Figure 4 shows the dynamic effects of the first inspection on firm j establishments' job creation. Panel A shows a small, upward movement in job creation for workers from outside the formal sector around the time of inspection, but the effect is not significant. Poaching from other formal firms (Panel B) declines, significantly and persistently, after the firms' first inspection.

Inspections, on the one hand, increase the minimum productivity threshold to offer new workers informal jobs so inflows to informal jobs decrease. On the other hand, for the stock of informal workers, the productivity threshold to formalize workers decreases and the threshold to separate increases. The combined effect is that outflows from informal jobs increase. The model predicts that smaller firms are more likely to respond to an increase in the cost of informal employment by formalizing workers, while larger firms

[37] n is the cumulative number of inspections that a tax ID (i.e. a firm) has received at any of its registered establishments in the same location (state) up to quarter t.

[38] As before, negative values for q mean that the inspection will occur in q quarters. Positive values mean that the inspection happened q ago.

are more likely to respond by terminating informal matches.[39] To analyze the variation in firms' responses by size, we interact each inspection dummy with firm size. We use the number of formal workers at the establishment two quarters before the first inspection as our measure of employer size and group employers into 6 size categories: 1 to 3 employees, 4 to 10, 11 to 25, 26 to 50, 51 to 100, and more than 100 formal workers.

Figure 5 shows the first inspection's effect on quarterly job creation from outside formality (formalization) by employer size. Consistent with our model, the differences in inspections' effect on formalization between the smallest employers (1 to 3 workers) and the rest is stark and persistent. Formalization rates increase on the quarter of inspection, and every quarter thereafter, at small establishments (1 to 3 employees). Larger employers instead decrease job creation from outside the formal sector after an inspection.

It is important to note that these results are conditional on firm survival. In Appendix D we show that firm exits (from the formal sector) increase on the quarter immediately after an inspection for smaller firms. Some of these exits are due to firms hiding from follow-up inspections by receding into the informal sector. This behavior by small firms endogenously creates size-dependent monitoring. The government can only follow-up to enforce compliance at larger firms.[40]

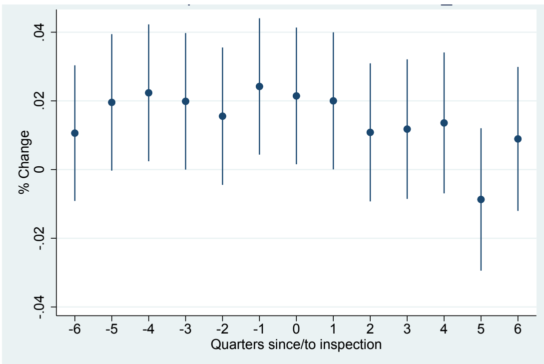
Figure 6 plots inspection effects' on quarterly job destruction and establishment size. Job destruction increases sharply and remains above pre-inspection levels up to a year after STPS's first visit. The increase in separations is driven by higher outflows of workers to unemployment or to informal firms (consistent with panels B and F in figure 3), as well as by higher separations to other formal firms. In the model, the average productivity of formal workers at inspected firms decreases after the inspection (due to a decrease in the formalization threshold). Poachers expectations are unchanged, however, so formal-to-formal job transitions increase. The increase in separations, coupled with the decrease in hiring, leads to lower average quarterly firm growth (Panel 6 B).

[39]In the models' terminology, the productivity threshold to formalize workers decreases more at small firms and the threshold to terminate informal matches increases more at large firms. See panels (a), (c) and (e) in Figure 2.

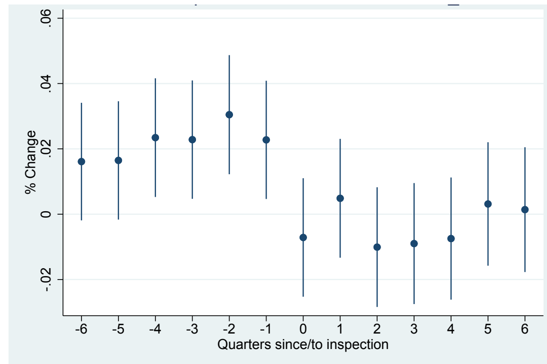
[40]Kaplan and Sadka (2011) find evidence of firms avoiding payments from lost labor court rulings by hiding its assets, relocating, or disappearing.

Figure 4: Firm's First Inspection Effect on Establishment's Job Creation

(a) Formalization



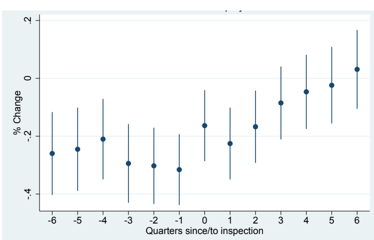
(b) Poaching



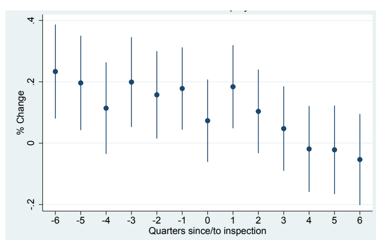
Notes to Figure 4: These figures display the effect of a firm's first inspection on establishments' formal job creation from outside of formality (Panel A) and worker flows from other formal firms. $q = 0$ indicates the quarter of inspection. The sample includes formal establishments' matched in the DNE and IMSS administrative records between 2005-2016. The effect on the treated is calculated from the $\alpha^{1,q}$ coefficients in equation 11. We control for timeXindustry and firm fixed-effects. Errors are clustered at the firm level.

Figure 5: "Formalizations" by No. of Quarters since/to First Inspection

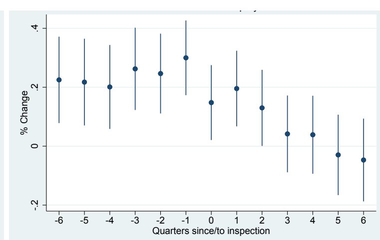
(a) 1-3 Employees



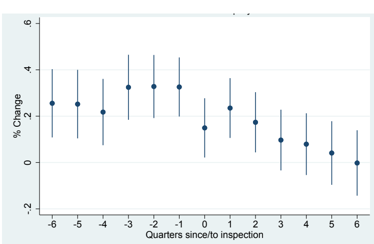
(b) 4-10 Employees



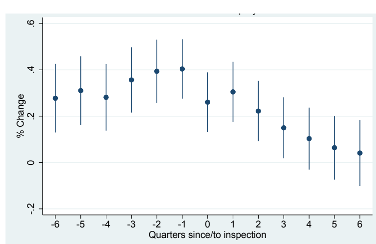
(c) 11-25 Employees Employees



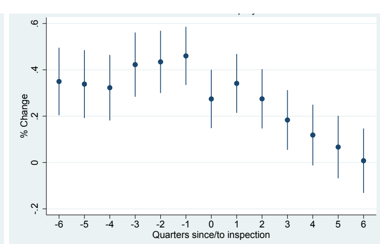
(d) 26-50 Employees



(e) 51-100 Employees Employees



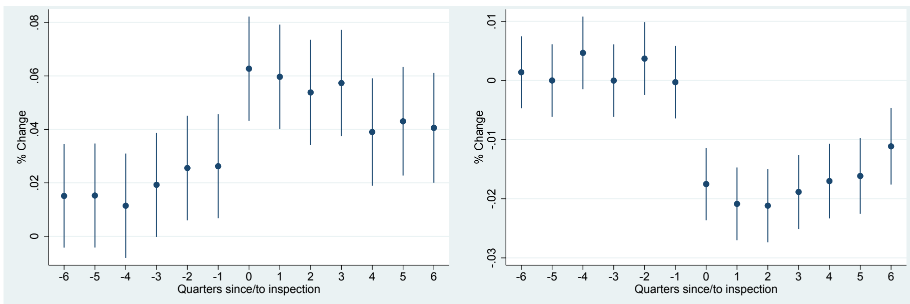
(f) 101+ Employees



Notes to Figure 5: These figures display the effect of a firm's first inspection on establishments' inflows of workers who were not employed at a different formal firm in the previous 6 months. The sample includes formal employers matched in the DNE and IMSS administrative records between 2005-2016. The effect on the treated is calculated from the coefficients from the interaction between firm size and Inspection dummies. We include timeXindustry and firm fixed-effects, and cluster errors at the firm level.

Figure 6: First Inspection Effect on Formal Job Destruction and Firm Size Growth

(a) Job Destruction (b) Quarterly Growth in Establishment Size



Notes to Figure 6: Quarterly growth in establishment size is equal to the change in the number of formal workers divided by the average formal employment in the previous and current quarters. The sample includes formal employers matched in the DNE and IMSS administrative records between 2005-2016. The effect on the treated is calculated from the $\alpha^{1,q}$ coefficients in equation 11. We include timeXindustry and firm fixed-effects, and cluster errors at the firm level.

4.3 Worker Outcomes

In the previous section we shows that inspections increase formalization. Next we use survey and administrative data to examine the effects on wages. We estimate the short-term change in informal workers' wages at inspected firms using ENOE's panel. ENOE allows us to see the worker's wage while informally employed and directly measure whether it experiences any changes immediately after the inspection. We use IMSS data to estimate longer term wage effects for workers at the inspected employer and with future matches.

IMSS data allows us to track workers' wage paths throuhgout their formal employment spells but we cannot observe the wage while informally employed. Moreover, we cannot directly identify within-firm informal to formal job transitions. Given our findings about the increase in within-firm informal to formal transitions on the quarter of inspections, workers starting a formal spell at a firm immediately following an inspection, who were not previously formally employed elsewhere, are more likely to have been informally employed with the same firm before the inspection. We compare workers who start a formal job immediately after an inspection with their coworkers who started immediately before the inspection. We refer to the former as "inspection-driven" and the latter as "organic" formalizations.

4.3.1 Short-term wage effect at inspected employers

We focus on the effects on workers in an informal job on the quarter of inspection. Inspections could change firms' wage setting policies and hence affect formal and informal wages for all workers even if they were not yet employed at the firm at the time of the inspection. We include in our baseline sample only workers employed at firms that either had not been inspected or received their first inspection during the time period when the worker was interviewed for ENOE.[41] For every quarter, the control group is the subset of workers whose work-sites have not yet been inspected. Workers are in the treatment group on the quarter their work-site is inspected and on every subsequent quarter.

ENOE is a 5-quarter rotating panel, so we observe each worker at most 5 times. We will refer to each of these observations as a wave, denoted as $v \in$[1,2,3,4,5]. A worker's firm can receive an inspection in any of these waves so the number of pre and post inspection observations varies depending on the time when the worker entered ENOE's sample. To account for this, we gather workers into cohorts, c, based on the quarter when they enter ENOE's sample. Let $datefirst_j$ be the date of firm j's first inspection. The treatment path for each of j's workers in cohort c, $\{Inspected^c_{j,v}\}^5_{v=1}$, is a non-decreasing sequence of 0's and 1's where the first 1 in the sequence corresponds to the ENOE wave v for cohort c when j receives an inspection.[42]

Figure 7 shows the dynamic effect of inspections on the after-tax wage for informal workers estimated using the log-linear regression model in equation 12. ln $\left(w^c_{i,j,v}\right)$ is the natural log of the hourly after-tax wage for worker i, in cohort c on quarter $t = v + c$

[41] We do not observe inspections before January 2005. Therefore, to limit the possibility of left-side censoring on first inspection dates, we further restrict the analysis to workers that enter ENOE's sample on or after 2007.

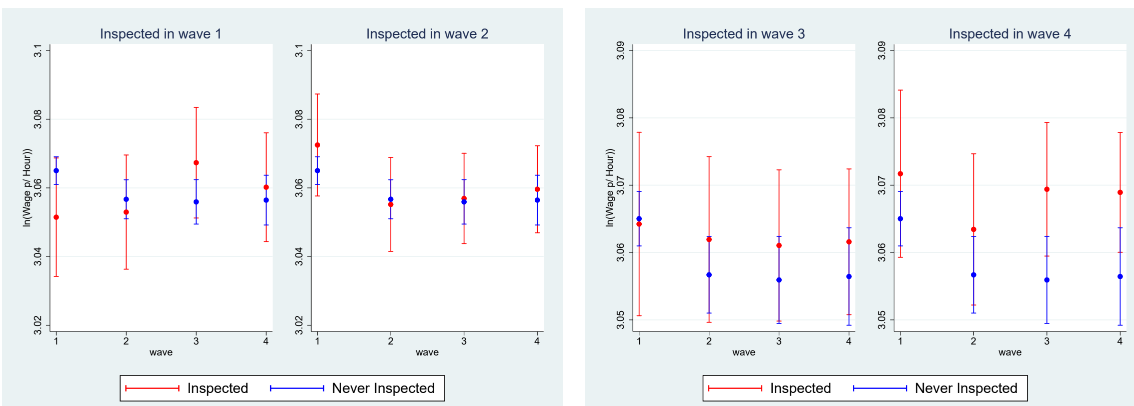
[42] Note that calendar time, t, is equal to the cohort's entry date c plus ENOE's wave, v, $t = c + v$.

employed at firm j, $X_{j,t}$ is a vector of time-varying work-site characteristics. i and t are sets of individual and time fixed-effects.[43].

$$\ln\left(w^{c}_{i,j,v}\right) = \sum_{v=1}^{5} \zeta_v Inspected_{j,c} + \gamma X'_{j,t} + i + t + \epsilon_{i,j,c,t} \tag{12}$$

Average net of tax wages for informal workers at inspected work-sites are not significantly different from those of their counterparts at non-inspected places of work, before or after an inspection occurs.[44] Although inspections increase the probability of transitioning to a formal job, which represents a 30% increase in labor costs for an informal worker earning the average wage, there is no evidence of an effect in short-term wages.

Figure 7: Average Dynamic Inspection Effect on Hourly Wage for Informal Workers



Notes to Figure 7: This figure shows the average treatment effect of inspections on wages for informal employees by timing of treatment using the log linear regression model specified in equation 12. Treatment timing, and hence the number of observed pre and post periods, varies based on the timing of the inspection relative to the quarter when the worker entered ENOE's survey.

[43] Borusyak and Jaravel (2017) show that event studies with two-way (unit and time) fixed-effects and dynamic treatment effects are underidentified. However, in this case, for every cohort entering ENOE's sample there is a well specified control group which can be used to independently identify the time fixed-effects: the set of informal employees at work-sites that have not been selected for a random inspection.

[44] We show in Figure 12 that, conditional on being employed, hours worked are also unaffected.

4.4 Long-term labor market trajectories

In this section, we use IMSS administrative records to test the model's predictions regarding the effect of increasing the cost of informal employment on workers' longer-term labor market trajectories. In the model, the rise in the cost of informal employment reduces the productivity threshold to "promote" workers to formal jobs. Therefore, we expect the average starting wage at formal jobs to be lower after an inspection. Moreover, incumbents realize they face stronger competition for formal workers. Because the average productivity of workers formalized after an inspection is lower, incumbents' willingness to compete with poachers is also lower. As a result, the wage gap between "organically" formalized workers and their "inspection-driven" formal co-workers will increase at the inspected employer. Further, we expect tenure at the inspected employer to be shorter for workers formalized after the inspection.

We compare these outcomes between workers who entered a formal job before and after their employer received an inspection using the specification in equation 13. The baseline sample are workers at any of an inspected firm's establishments who started a formal job within a 6 month window of the firm's first inspection and who were not recently[45] employed at a different formal firm. $Start_{i,j} - FirstInspection_j \in [-6, 6]$ is a dummy variable for the number of months elapsed from the start of a formal match between worker i and firm j to j's first inspection.

$$log\left(Y_{i,j,t}\right) = \sum_{m=-6}^{6} \alpha^m \left(Start_{i,j,t} - FirstInspection_j\right) + \gamma X'_{i,t} + j + t + \epsilon_{i,j,t} \qquad (13)$$

Table 4 shows the estimated α^m coefficients. The omitted category are workers who start a formal job a month before the employer is inspected. Starting wages[46] are similar among workers formalized one, three and six months before an inspection. Meanwhile, starting wages for workers that start a formal job a month after the first inspection are 0.9% lower.

[45] We consider only workers who were not at another formal firm in the previous 6 months. Our sample, therefore, includes workers hired from non-employment or from an informal job at a different firm directly into a formal job, as well as workers who were already at the firm but in an informal job.

[46] It is important to note that "starting" wage here refers to the first observed wage for a worker as a formal employee with a given employer. If a worker was first informally employed, we do not observe their first wage wage at the firm but rather the first wage as a registered, formal worker.

Workers formalized 2 to 3 months after the first inspection still have lower wages than their counterparts hired prior to STPS's visit. Consistent with our previous findings that the increase of within-firm formalizations is concentrated in the first 3 months after an inspection (Figure 3 Panel C), the negative difference in starting wages between workers hired before the inspection is reversed after the first quarter (i.e. $m^{(3,6]} > 0$).

The wage gap between workers hired a month before the inspection and those hired after is higher at the end of their formal spell at the firm. This is seen by comparing the coefficients between in the "Starting Wage" and "Final Wage" columns. For workers hired right after the inspection, the gap increases by half a percentage point, from 0.9% to 1.4%. The "Tenure" column shows the differences in the length of a formal spell for workers by the timing of inspections relative to their formal start date. Workers who start after an inspection have shorter tenures than their workers hired before the inspection.

Table 4: Starting Wages at Inspected Firm

Months from Inspection to Start Date		At Inspected Firm (Incumbent)			At Next Formal Employer (Poacher)
		Starting Wage	Final Wage	Tenure	Pr. of Next Formal Job
Organic	m = (-3,-6]	0.037***	0.041***	0.180***	0.003**
		(0.001)	(0.001)	(0.004)	(0.001)
	m = (- 1,-3]	0.009	0.009***	0.058***	0.000
		(0.001)	(0.001)	(0.004)	(0.001)
Inspection Driven	m = [0,1]	-0.009***	-0.014***	-0.075***	-0.003**
		(0.002)	(0.002)	(0.005)	(0.001)
	m = (1,3]	-0.002***	-0.007***	-0.078***	-0.004***
		(0.001)	(0.002)	(0.004)	(0.001)
	m = (3,6]	0.003**	-0.005***	-0.105***	-0.007***
		(0.001)	(0.001)	(0.004)	(0.001)
N		2,165,139	2,165,297	2,165,297	2,133,772
N firms		31,520	31,520	31,520	31,520
Adj. R^2		0.5766	0.5527	0.2889	0.0665

Conditional on their formality status at the inspected firm, the model predicts workers' outcomes with future firms to be similar regardless of the timing of their formalization relative to the inspection. The last column in table 4 uses a linear probability model, with the same specification as in equation 13 to compare workers' probability of having a formal job with the next employer, conditional on separating from the inspected firm. We find that the probability of being poached into a formal job is between 0.3 and 0.4 percentage points lower for "inspection-driven" formal workers (i.e. those hired up to a quarter after an inspection) relative to their "organically formalized" co-workers.

While this difference in the likelihood of the next job being formal is statistically significant, it is important to put the magnitude of the effect in perspective. Table 2 shows that, conditional on separating to a new formal employer, informal workers at formal firms have a 2-to-1 probability of getting a formal job with their new employer (64.5% vs. 35.1%). The odds are reversed for formal employees (30.7% vs. 69.3%). Finding that the difference in the probability of the next job being formal is 3 percentage points lower is indicative of an important improvement in the odds of future formal employment for inspection-driven formal workers. It is also worth noting that tenure at the inspected firm and worker's age are not positively correlated with the probability of a subsequent formal job. We consider this set of findings as supportive of employers using formality status as a signal of worker productivity.

Finally, we compare starting wages at the new firm, denoted as k, workers who started a formal match at firm j within 6 months of j's first inspection using the specification in equation 14. $Post\_Inspection_{i,j,t}$ is an indicator variable equal to 1 if i's formal match started on or after the date of j's inspection and zero otherwise, t and k are time and next-employer fixed effects, respectively. We find an insignificant coefficient of $\beta = -0.007$[47]

$$log\left(w_{i,j,k,t}\right) = \beta Post\_Inspection_{i,j,t} + \gamma X'_{i,t} + k + t + \epsilon_{i,j,k,t} \tag{14}$$

All these results indicate workers embark on an improved labor market trajectory after being formalized due to an inspection. Inspection-driven formalizations lead to more than a label change at their current employer. They get access to a set of social security benefits, and their future labor path resembles that of other formal employees.

[47] $\sigma\left(\hat{\beta}\right) = 0.005$ The sample size (N = 414,886) is smaller as we condition on separating from the inspected firm. There are 16,831 inspected employers in IMSS data that experienced at least one separation of a worker hired within 6 months of the firm's first inspection. The adjusted R-squared is 0.344.

5 Conclusions

In principle, formal firms must register all their employees with the Social Security Institute and pay payroll taxes. In practice, enforcement is lax and many workers employed at formal firms, of all sizes, have informal jobs. Using inspections at randomly selected formal establishments, we examine the effects from increasing the cost of informal employment on firm and worker outcomes.

Using administrative employer-employee matched data, we find that even if all firms face the same probability of being monitored by the government, smaller firms are more likely to exit the formal sector after an inspection, avoiding follow-ups and punishment. The possibility of "disappearing" when faced with a higher likelihood of being fined leads to an endogenous positive correlation between firm size and cost of informal employment, even if inspections are randomly allocated across firms. Conditional on staying in the formal sector, within-firm informal to formal job transitions increase at smaller firms, while larger firms are more likely to respond to inspections by decreasing hiring and increasing job destruction. As a result, inspections have a negative impact firm's growth, lasting up to 6 quarters after the inspection. These responses are consistent with the cost of informality being increasing in firm size.

We obtain consistent results using household survey data. On the quarter of inspection, informal workers' quarterly probability of remaining informal with the same employer decreases by 9 percentage points. The decrease is caused by a 50% increase in the probability that their current employer formalizes them, and a 40% increase in separation rates.

Future employers treat formal workers similarly regardless of whether they were formalized before or after an inspection. Conditional on separating from the inspected employer, formal workers have similar probability of being poached into a formal job and equivalent starting wages at the new job than their "organically-formalized" co-workers. The asymmetric learning across employers assumption in our model, coupled with poachers using formality status as a signal of workers' productivity, generates predictions that are consistent with these findings.

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Appendix: For Online Publication Only

A Identifying Informal Workers at Formal Firms in ENOE

We follow the 17th International Conference of Labor Statisticians[21] resolution for measuring informality. According to this Resolution, informality has two dimensions. The first dimension refers to employers' characteristics: an employer is categorized as informal when it is not a registered business with tax authorities. The second dimension refers to job characteristics. Informal jobs are those that lack the benefits and institutional protection required by the legal framework in the country. Using this definition of informality, an employee can have either a formal or an informal job at a formal firm depending on whether the employer registers the worker with IMSS or not. All jobs are informal at informal firms in Mexico because only formal employers can register their employees.

The first step to identify informal workers at formal firms is to determine which workers in ENOEs household survey are employed at formal firms.[48] INEGI, and previous research[49], uses data on firms' size and industry to determine whether a firm is formal or not. This classification strategy relies on the assumption that larger firms are more likely to be detected by authorities and hence have a higher risk to informality. Similarly, it assumes that firms in certain industries have more incentives to register with authorities because they either require a larger scale to operate or are more likely to benefit from participating in production networks that require issuing tax deductible sale receipts which are only available to firms registered with the government.

We depart from this strategy to identify formal firms and instead rely on the Ministry of Labor's Firm Directory (DNE). The DNE is a subset of formal firms in the economy. However, our methodology exploits the Ministry's inspections and only firms in the DNE are selected for random inspections. Moreover, we argue that there are several benefits to identifying firms registered with the government directly, instead of inferring formality

[48] All employers and workers in IMSS data are by definition formal.

[49] See, for example, Maloney (1999), Fiess, Fugazza and Maloney (2010), and Alcaraz, Chiquiar and Salcedo (2015) among others.

status from size. First, households' reporting of employers' size might be inaccurate. Using size thresholds to identify formal firms can therefore be problematic. Second, Hsieh and Olken (2014) find no evidence in the distribution of firms in Mexico to support size-based sorting into formality. Third, more than half of all employers registered at IMSS have between 2 to 5 employees. Using size to classify employers could therefore lead to misclassifying a large share of registered employers as informal firms. Fourth, since formal firms can hire workers off-the-books and tax authorities do not share information with IMSS, it is not clear whether the relevant measure to determine risk of getting caught is related to aggregate labor force size, share of non-registered workers or a combination of both.

For analyses based on IMSS administrative data, the baseline sample is all workers who were ever formally employed at a DNE firm. Our baseline sample in ENOE is all individuals employed in a firm included in the DNE for at least one of ENOE's waves. After identifying formal firms, we classify jobs into formal and informal jobs. For this, we follow INEGI's categories which are based on workers' reported access to mandated employer-provided social security benefits. In ENOE's data, transitions across formal and informal jobs refer to changes in workers' reported access to these benefits. In IMSS data, we do not observe these transitions directly. However, for all formal workers in IMSS data, we can observe whether the source and destination of transitions into and out of formal employment are other formal jobs or not.

When using self-reported data on access to social benefits to determine workers' formality status, time-varying misreporting and misclassification can lead us to overestimate transitions rates across formal and informal jobs. While these errors may cancel in aggregate, stock variables, the estimated flow rates between labor market states may be very sensitive to these spurious transitions.Poterba and Summers (1986) This concern might be heightened if individuals' incentives to misreport their access to social benefits is correlated with the timing of inspections.

To address this concern, we first point out that if inspections were only changing reporting behavior but not actual access to social security benefits then we would not see any changes in formal jobs in IMSS administrative data. Second, we implement a conservative correction to transitions. We identify sequential back and forth changes in reported access to social security benefits with the same employer and re-code them as misclassifications. If a worker switches between formal and informal status more than once within a three quarter period with the same employer, we consider the "true" formality status as the job in which the worker spent most time with the employer during the 5-quarter period that ENOE tracks the worker. This correction has a negligible effect on the stocks of informal and formal jobs within formal firms. However, it reduces the rate of transitions from formal to informal jobs and vice-versa by 2.5 p.p. and 2.0 p.p., respectively.[50]

B Merging Datasets

B.1 ENOE and DNE

The National Employment and Occupation Survey (ENOE) interviews 120,260 households every quarter starting in 2005. Among other questions regarding labor market participation, it asks every household member who is employed or involved in any income generating activity the name of the firm, business or institution of employment. ENOE also includes a battery of questions regarding the type of activities performed and goods or services provided by the firm. The Mexican National Institute of Statistics and Geography (INEGI) then uses the answers provided to these questions to classify the firm into one of 178 NAICS industry codes.

DNE is a list of firms' establishments. Each establishment is identified by the firms' "official name" (*razon social*), the establishments' exact address, and for 83% of firms in the directory we also observe the firm's tax ID (*Registro Federal de Contribuyentes*). Meanwhile, in ENOE, workers self-report the name of their employer. Since the dwelling is the unit of observation in ENOE, the survey also includes information on the household's

[50] If establishments register their workers after an inspection to avoid being detected in a follow-up visit by IMSS but then un-register them after the verification takes place, observed informal-formal-informal transitions would not be misclassifications but rather real transitions. However, employers have incentives to avoid this "hiding" practice. Registering and unregistering workers within short periods of time can raise flags with authorities making establishments targets of directed inspection visits.

location, but not that of their place of work.[51] These differences generate two challenges when merging DNE and ENOE. First, due to spelling mistakes, abbreviations, and incomplete name reporting by the workers surveyed in ENOE, the name provided by the worker seldom is an exact match to the official name registered by the firm with STPS. Second, if a firm has more than one establishment in the workers' reported location, we need to make a decision about which establishment to match with the worker.

To match ENOE with the DNE and inspections logs, we first perfom basic name cleaning to standardize workers' reported firm names. This includes removing all punctuation, spacing and accents, eliminating articles, spelling out numbers, and replacing common abbreviations and plural forms. We then want to compare firm names in ENOE and find the closest match in the DNE. We define the closest match using a combination of a soundex algorithm and a Levenshtein distance.

Before implementing our matching algorithm, described in more detail below, we must clean ENOE's names further. In ENOE, employers' names are often reported including the type of establishment or sector in which the firm operates. For example, the answer for a worker employed at a 7-Eleven is at times recorded as "*Autoservicio* 7-Eleven" ("Convenience Store 7-Eleven"), "*Tienda* 7-Eleven" ("Store 7-Eleven"), or with the diminutive "*Tiendita* 7-Eleven". Meanwhile the official name (*Razon Social*) for 7-Eleven, as recorded in the DNE, is "*7-Eleven Mexico, S.A. de C.V.*". In this case, the words "*Tienda*" and "*Autoservicio*" are not actual parts of the firms name so we would like to remove them. However, in other cases, these words are useful to distinguish firms with similar names in different sectors, or are part of the official name. To address this issue, we create a word cloud with the most frequently appearing words in workers' reported employer names. We then reduce these words, and all words with the same root, to the first 5 letters. This procedure reduces the weight given to these words when assessing which name is the closest match in the DNE.

Once we have standardized employers' name in both datasets, we then use a phonetic

[51] These two pieces of information, the household's location and their members' employers' names, are collected and recorded by ENOE. However, due to confidentiality requirements, they are only available through INEGI's microdata lab for research purposes.

algorithm, in Spanish, to reduce mismatches from misspelling and typos.[52] Finally, for each employer name reported in ENOE, we identify the closest match in the DNE using the Levenshtein distance.[53] We consider an ENOE-DNE pair to be a match if the Levenshtein similarity ratio is at least 90% and the worker lived in the same state as the firm's location. For multi-establishment firms, we assume the effect of inspections occurs at the firm-state level. This assumption implies that a worker in state X employed at a firm that received an inspection at any of the establishments in X is coded as inspected.[54]

Figure 8: ENOE Firm Names Word Cloud

Notes: This figure shows a censored version of the word cloud used to standardized employers' names between ENOE and DNE. The goal is to decrease the weight of words frequently used by workers to describe their employers, and inaccurately reported as part of the firm's name, when calculating the Levenshtein distance between ENOE and DNE firm name pairs. Some words were censored to avoid revealing employers' identities.

On average, we match 38,000 workers per quarter (out of an average of 116,000

[52] The algorithm is ouw own implementation of Amon, Moreno and Echeverri (2012)

[53] For each employer name in ENOE, we actually identified the top 5 matches to conduct manual checks and robustness analysis.

[54] We could instead assume that inspections affect firms' decisions for all establishments in a given municipality, or that only the inspected establishment is affected. For the later assumption, we merge workers to the establishment that is closest to their dwelling's location. Our current matching methodology biases our results downwards since we consider workers as "treated" by the inspection even if their specific establishment was not the one targeted for the inspection.

salaried workers at formal firms as defined by ENOE). Table 5 shows the results of the matched inspected and non-inspected workers by formality status. Table 7 compares the share of establishments by industry code in ENOE, the DNE and by inspection status. Consistent with random selection of establishments to inspect, conditional on being part of the DNE list, inspections are similarly distributed across establishments in different industries.

Table 5: Matching by Workers' Inspection and Formality Status

Formal Firms Employees in ENOE's sample (Jan. 2005-Jun. 2016)			
	Matched to DNE		Not Matched to DNE
	Inspected	Not Inspected	
Informal	88,732	317,271	628,558
Formal	336,157	1,033,898	1,620,020

Notes: This table shows the matching rate between ENOE and DNE. The sample are the individuals included in ENOE's sample, between 15 and 80 years old, working as employees at firms that are either part of the DNE and/or that are classified as formal firms using INEGI's methodology.[55]

Table 6: Characteristics of Workers by Firms' and Jobs' Formality Status 2005-2016

	Informal Employees at Formal Establishments	Formal Employees at Formal Establishments	Informal Employees at Informal Establishments	Informal Self-Employed	Formal Self-Employed
Median After-Tax Wage (2014 Pesos p/Hr.)	$18.2	$26.0	$17.4	$20.8	$31.0
Median No. Hours (Weekly)	46	47	47	36	50
Median Tenure (Months)	24	60	24	61	96
% aged 15-24	33	15	33	6	3
% Female	39	39	18	47	31
% Completed 9^{th} grade	71	85	50	50	77
No. Obs:	761,297	2,238,853	483,598	908,157	334,245

Source: Own calculations based on data from the National Employment and Occupation Survey (ENOE)[20] and the National Directory of Firms (DNE)[31]

[55] See INEGI's microdata website[20] for further information on INEGI's methodology to classify formal and informal firms.

Table 7: Establishments in ENOE and DNE by Industry

NAICS Sector	% of establishments in ENOE	% of establishments in DNE	% of inspections
Mining & oil	0.9%	2.7%	2.4%
Utilities	0.6%	5.1%	5.4%
Construction	4.0%	8.8%	9.1%
Manufacturing	18.3%	8.6%	8.0%
Wholesale trade	4.5%	6.1%	6.3%
Retail trade	18.1%	10.3%	9.9%
Transport & warehousing	4.2%	8.5%	9.1%
Finance & insurance	1.9%	9.2%	10.6%
Prof. & technical services	3.0%	1.0%	1.2%
Administrative services	4.1%	1.4%	1.9%
Educational services	10.4%	1.8%	2.4%
Health care & social assis.	5.3%	1.6%	1.5%
Entertainment & rec.	1.5%	1.4%	0.9%
Accomm. & food services	8.6%	9.9%	7.4%
Other services	6.2%	18.8%	19.2%
Public administration	8.5%	4.7%	4.9%
Total	100%	100%	100%

Notes for Table 7: This table shows the distribution of establishments by sector using 2-digit NAICS. The second column shows the distribution in ENOE. It is important to emphasize that this is not the distribution of workers, even though this is a household survey, but the unweighted distribution of establishments. The sample includes all establishments of employment for workers in ENOE, even when the worker did not specify an establishment name. The second column shows the distribution of establishments that could be matched to ENOE by industry. Since matching is based on establishment name, as indicated by the worker, this sample only includes establishments for which the worker provided a name. The last column shows the industry distribution of inspected establishments.

B.2 IMSS and DNE

IMSS employer-employee administrative data and the DNE share some variables that allow us to identify a firms' establishments in the two datasets: the firm name (*"Razon Social"*) and its tax ID (*RFC*). Due to confidentiality restrictions, we were not allowed to work directly with the non-anonymized data. Instead, that staff at Banxico's Econ-

Lab helped us to develop and implement a data cleaning and name matching algorithm to matched IMSS administrative records with the DNE. The EconLab staff is exceptionally well suited for this task as they are extremely familiar with the data and highly skilled in working with big data.

We next describe the steps the EconLab staff implemented to merge these two datasets: 1) tax ID matches, 2) name cleaning and homogeneization, and 3) (direct, phonetic, and closest distance) name matching. We start by matching establishments for which we do have tax ID information. If the tax ID matches perfectly, then we consider these employers as matched and keep them in the sample. For the remaining observations in IMSS data, we follow a similar process than the one used to match ENOE and DNE. First, we clean firms' names in both datasets removing acronyms like Corp., Ltd., Inc., etc. We homogenize capitalization and remove accents. Then we identify the employers in IMSS data that have an identical firm name (letter-by-letter match) as a firm in the DNE. We consider these as matches and continue with the rest of the non-matched employers.

We then perform a soundex algorithm (in Spanish) converting each firm name in the set of unmatched IMSS employers and in the full set of DNE firms to its phonetic equivalent. We then compare the two datasets and, for each firm in the IMSS data, we find the closest phonetic match in the DNE. If the distance between a firm in IMSS data and its closest phonetic match in the DNE is such that the probability of a true match is at least 95%, we consider it a match. Finally, for the rest of the unmatched employers in IMSS data, we calculate how different their firm's name is to each of the firm's names in the DNE using, first, the Levenshtein distance and, second, Jaro Winkler similarity measure. If the nearest match is at least a 95% match then we consider it a match.

Table 8 describes the matched sample and compares it to the universe of firms in IMSS data.

Table 8: IMSS Employers Matched to DNE Firm: Descriptive Statistics 2005-2016

PANEL A: Employer Characteristics			
	Mean	Median	Std. Dev.
Size (No. of Workers)	121.4	30.9	432.2
Starting Daily Wage (2018 Pesos)	$228.0	$165.60	$224.90
Employer Age (quarters)	41.1	47.0	10.5
% Female Workers	40	30	30
No. of Inspections	2	1	3
PANEL B: Share of Employers by Industry			
	Matched IMSS-DNE	IMSS Universe	
Manufacturing	19.3	18.5	
Construction	8.3	10.4	
Prof. and Bus. Services	19.4	22.6	
Other Services	43.5	39.5	
Other NEI	9.5	9.0	
No. of Employers:	23,935	119,489	

Source: Own calculations based on data from IMSS administrative records accessed through Banxico's EconLab Convenio No. 45, the National Directory of Firms (DNE)[31] and STPS Inspection Logs.

Notes: Employer age is measured as the number of months elapsed from the first time an employer ID registers a worker. "Others NEI" refers to all other industries not explicitly listed including government entities, transportation, oil and mining.

C Verifying Random Selection in Inspections

As shown in table 9, in most cases, inspections are closed without there being any reported violations for the items within STPS's enforcement responsibility. Only 10% of all inspections lead to a fine. Between 2005 and 2016, the average fine was MXN$32,194 (USD$1,740) with a maximum fine of MXN$82,569,000 (USD$4,463,189)[56] and a minimum of MXN$20.57 (USD$1.11).

[56] This fine was due to health and hygiene violations in 2013.

Table 9: Distribution of STPS's Inspections by Result (2005-2016)

Result		No. of Inspections	% of All Inspections
Closed without report of violations		266,517	43%
Provided proof of compliance		296,367	48%
Request for time extension granted		184	0%
Fining process started	Fine imposed	23,154	4%
	Fine no yet imposed	34,620	6%

Source: Own calculations using STPS DNE and Inspections logs 2005-2016.[31]
Notes: Excludes violations beyond STPS's jurisdiction, including, those related to informal employment.

Figure 9: Yearly Inspection Probability and Distribution of Establishments by Number of Inspections Received in a Year (Conditional on being Inspected)

Source: Own calculations using the National Firm Directory (STPS) and Inspections logs 2005-2016. (INAI)[31]

The annual probability of inspections increased starting in 2012. Between 2005 to 2011, establishments' annual inspection probability was 9%. In 2013 the likelihood of being inspected increased to 17% and by 2015 it was 29%. Figure 9 shows that the probability of receiving more than two inspections in a given year also increased.

STPS must choose establishments in the DNE at random for ordinary inspections.[37,38] Section 4, where we presented our empirical findings, shows similar trends in the outcomes of interest for inspected and non-inspected establishments in the pre-period. In this section, we conduct additional tests to assess whether establishments are randomly selected.

Let $Z_{i,t}$ be an indicator variable equal to 1 if individual i was employed at an establishment that received an inspection in quarter t and zero otherwise.[57] If inspections are random then the probability of working at an establishment that receives an inspection should be uncorrelated with workers' or establishments' characteristics. Column (1) in table 10 tests this using a linear probability model in ENOE data. Jointly, worker's and establishment's characteristics explain less than 0.1 percent of the variation in inspection probability across establishments within the DNE (joint p-value of 0.595).

To show that we tested inspections' randomness on a relevant set of worker and establishment characteristics, in columns (2) and (3) we show theses covariates do affect workers transition probabilities out of informal employment. In particular, more educated workers and larger employers are more likely to transition to a formal job or separate to unemployment. These are important characteristics that determine workers' outcomes, but do not change the probability of receiving an inspection. Therefore, we conclude that STPS Inspection and Fines System generates a distribution of inspections that is consistent with random selection across workers employed at firms in the DNE.[58]

[57]We also consider different specifications where $Z_{i,t}$ is equal to 1 if in period t, i is employed at an establishment that received an inspection between t and $t+3$. The results are similar.

[58]The similarities on an average quarter between the industry and size distributions for establishments in the DNE and the set of inspected establishment, presented in Appendix B, is also consistent with a random assignment of inspections.

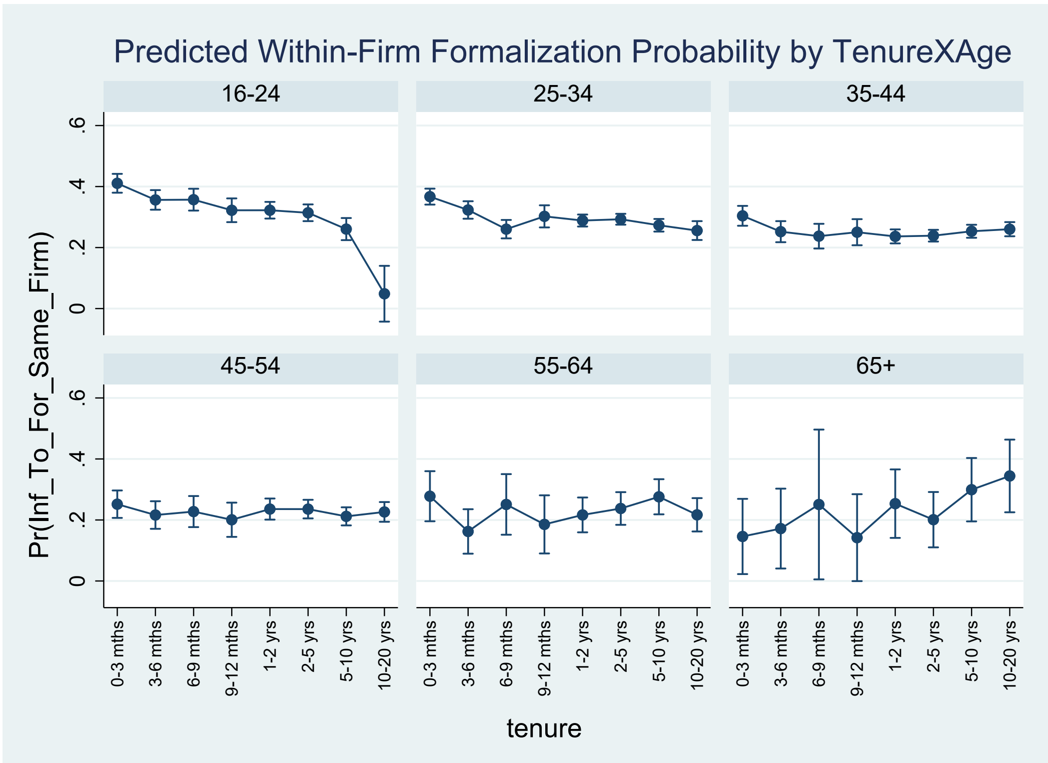
Table 10: Testing for Random Probability of Inspections: Worker-level

	Sample: Informal Workers at Firms in the DNE					
	(1)		(2)		(3)	
	Pr(Inspected)		Pr(Formalized)		Pr(Separated to Unemp.)	
	$Z_{i,j,t}$		$IF_{i,j,t}$		$IU_{i,j,t}$	
	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
Age	-0.001**	(0.001)	0.002	(0.002)	-.005**	(0.002)
Male	0.005	(0.013)	0.068*	(0.041)	-0.229***	(0.057)
Tenure (months)	0.001	(0.001)	0.008**	(0.003)	-0.014***	(0.002)
Education						
Less than HS	0.068	(0.066)	0.261*	(0.149)	0.287	(0.241)
Completed HS	0.043	(0.067)	0.522***	(0.154)	0.559**	(0.247)
Some College	0.075	(0.069)	0.619***	(0.167)	0.650**	(0.264)
College +	0.074	(0.068)	0.642***	(0.156)	0.554**	(0.250)
Children in daycare	0.000	(0.005)	0.050***	(0.015)	-0.784***	(0.270)
Household size	0.003	(0.006)	0.018	(0.017)	-0.008	(0.014)
Establishment size						
6-10	-0.046	(0.025)	0.372***	(0.015)	0.259***	(0.087)
11-15	0.014	(0.029)	0.516***	(0.073)	0.554***	(0.113)
16-50	0.002	(0.021)	0.727***	(0.055)	0.801***	(0.084)
51+	-0.003	(0.019)	0.892***	(0.052)	1.354***	(0.075)
LR	30.44		1,194.36		649.23	
p-value	0.595		0.000		0.000	
No. of Observations: 253,349						

Notes: This table presents evidence of random selection in STPS ordinary inspections. The baseline estimation sample is individuals who are informally employed at an establishment that is included in the DNE between 2005 to 2015. The dependent variables in columns 1, 2 and 3 are, respectively, $Z_{i,t}$ a dummy equal to 1 if individual i was informally employed at an establishment in period t, and 0 if the establishment is included in the DNE but was not subject to an inspection within this time frame; $IF_{i,j,t}$ a dummy equal to 1 if individual i was informally employed at establishment j in quarter t and was then formally employed at the same establishment in $t+1$ and 0 otherwise; and $IU_{i,j,t}$ a dummy equal to 1 if individual i was informally employed at establishment j in quarter t and became unemployed between quarters t and $t+1$ and 0 otherwise. Tenure is the numbers of months employed at the current establishment. Children in daycare are children within the ages of 0 to 4 in the household. All regressions include firm sector, worker occupation, and year fixed effects.

D Additional Empirical Results

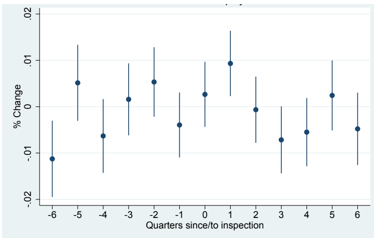
Figure 10: Within-Firm Formalization Probability



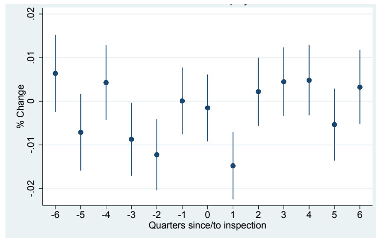
Notes: We calculate the predicted probability of "organic" within-firm informal to formal job transitions using a log-linear probability model with age and tenure groups, an interaction of these two variables, as regressors, We also control for workers' years of education, firm size as reported by the worker, firm and year fixed effects. The figure shows the marginal transition probability for each worker age and tenure groups.

Figure 11: Firm Exit by No. of Quarters since/to Inspection

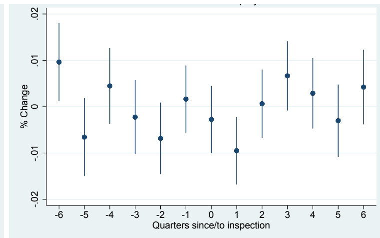
(a) 1-3 Employees



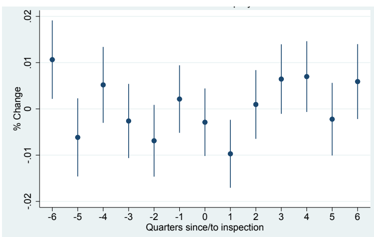
(b) 4-10 Employees



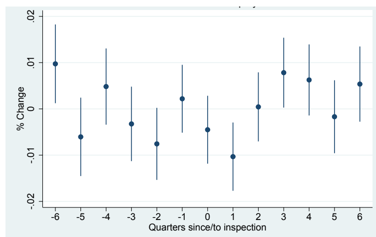
(c) 11-25 Employees Employees



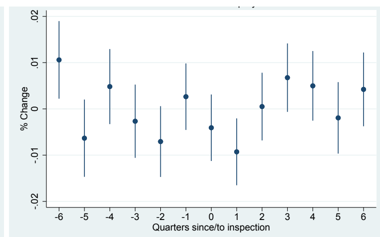
(d) 26-50 Employees



(e) 51-100 Employees Employees

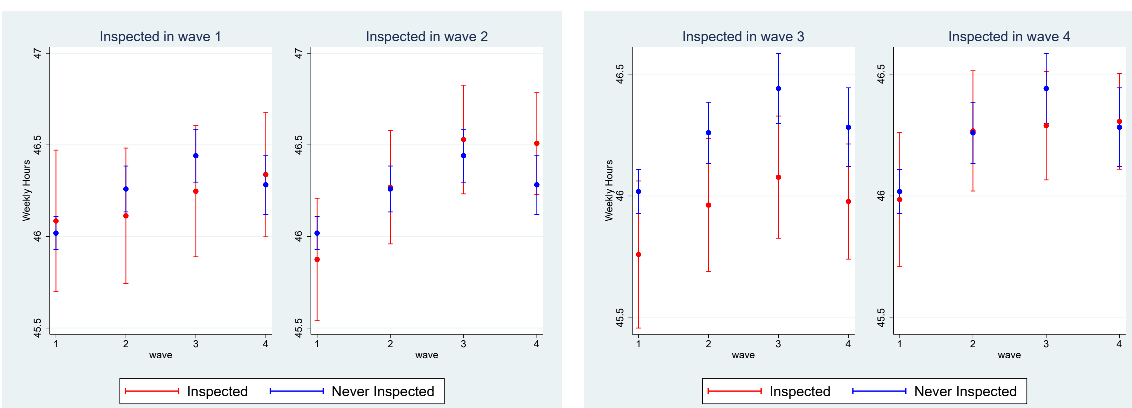


(f) 101+ Employees



Notes: These figures display the effect of inspections on establishments' exit rates from the formal sector. We define exit from the formal sector as firms that go from positive to zero employees. The sample includes formal employers matched in the DNE and IMSS administrative records between 2005-2016. The effect on the treated is calculated from the coefficients from the interaction between firm size and Inspection dummies. We include timeXindustry and firm fixed-effects, and cluster errors at the firm level.

Figure 12: Average Dynamic Inspection Effect on Hours Worked for Informal Workers



Notes: This figure shows the average treatment effect of inspections on hours worked for informal employees, conditional on staying employed, by timing of treatment using the linear regression model specified in equation 12.