# How Much Is a Formal Job Worth? Evidence from Mexico *

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**Abstract**

We estimate the value of a formal job in Mexico. For the median household, the value ranges from MXN$610 and MXN$860 pesos (USD$30-42) per month. This valuation encompasses the various social benefits available to formal employees, differences in job stability and career prospects, and any other attributes that make a formal job desirable. We obtain this estimate using a partial equilibrium model of joint labor supply with informal and formal contracts and search frictions estimated using simulated method of moments. We validate our structural estimates for the value of a formal job using an employer-employee-household matched panel dataset and exogenous variation in access to a formal job from over 300,000 work-site inspections. Model and reduced-form approaches yield consistent changes in reservation wages of a spouse after a change in their partner's formal job probability and similar values of a formal job based on compensating income differential estimates. We find substantial heterogeneity in the value of formal employment across income deciles.

*_Disclaimer_: This paper uses confidential data from Mexico's National System of Statistical and Geographical Information, accessed through the National Institute of Statistics and Geography (INEGI) Microdata Laboratory and from the Ministry of Labor and Social Welfare's (STPS) Inspection and Sanctions System. All results and statements reported herein, however, are our own and are not part of INEGI's or STPS's official statistics.

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# 1 Introduction

In Mexico, employers must register their workers with the Mexican Social Security Institute (IMSS) and pay taxes that finance a wide range of social benefits for these employees including pensions, free day care centers, free health care, subsidized mortgages, among others. Nonetheless, half of all non-agricultural sector workers are informally employed, that is, they were not registered with the social security administration as required by law.[1]

Policy makers along with a vast development literature equate these informal jobs with low quality, undesirable jobs that workers would be willing to pay to transition out of, if given the option. In contrast, others argue that informality is an optimal tax-evasion choice and that workers derive little value from formality (Maloney (2004), Levy (2008)). Although the benefits listed are many, few workers actually get to use them and effective elegibility has declined over time. For example, in 2019 the National Commission for the Retirement Savings System estimated that three out of every four formal employees would not meet the minimum contribution requirements to be eligible for a pension.[2] In 2017, only 3.5% of working mothers in Mexico used the government's free day care centers. Among eligible mothers not using the centers, difficulties accessing the service (e.g., wait lists) was the main reason for not using them.[3] Eligibility and use of benefits declines further among low-income households.

In this paper, we estimate the value that households in Mexico place on having a formal job. This measure has eluded estimation due to two main challenges: data availability and endogenous worker flows between formal and informal employment. We address both of these challenges, first, by constructing a new employer-employee-household matched panel that includes workers' formal and informal employment spells. This dataset allows us to track workers and their family members regardless of their employment or formality status. Second, we leverage household-level variation in the probability of receiving a formal job offer, generated by random worksite inspections, to estimate how households respond to changes in formal job finding probabilities. Third, we develop a model of joint labor supply with search frictions, on-the-job search, and formal and informal job offers. We use a simulated

[1]The labor informality rate, excluding agricultural workers, has persistently hoovered around 50 percent since the Mexican Institute of Geography and Statistics began reporting it in 2005. INEGI, InfoLaboral, Tasa de informalidad laboral (TIL2), `https://www.inegi.org.mx/sistemas/infoenoe/Default_15mas.aspx`

[2]Comisión Nacional del Sistema de Ahorro para el Retiro (CONSAR). On July 1997, the Mexican pension system transitioned from a defined benefit to a defined contribution program. The reform also introduced a minimum of 1,250 weeks worked in the formal sector as a requirement to receive a pension upon retirement.

[3]National Employment and Social Security Survey (ENESS) 2017.

method of moments and target households' responses to a worksite inspections to estimate the model's parameters. We use the difference in the net present discounted value of households with an informal employee and households that transition to a formal job due to an inspection to measure the value of formal employment.

Using the novel employer-employee-household panel data, we establish relevant new facts regarding households' joint labor supply. Our sample focuses on households headed by a heterosexual couple. Half of all male household heads are formally employed, while only 13 percent of women have formal jobs. Consistent with assortative matching, the vast majority of formally employed women have a formally employed partner. Formality status is more persistent for men than for women with annual transition rates of 15 and 34 percent, respectively.

Next, we examine households' responses to exogenous changes in formal job finding rates. To do so, we leverage over 300,000 random worksite inspections, which increase informal-to-formal worker flows without a change in after-tax wage or hours worked, as documented by Samaniego de la Parra and Fernandez Bujanda (2024).[4] We estimate the effects pf inspection-driven changes in formality status on spousal[5] labor supply and wages using a difference-in-differences strategy. Consistent with receiving a positive income shock, non-employed spouses are less likely to start a formal job and more likely to remain non-employed after their informal partner's workplace is inspected. Meanwhile, for employed spouses, transitions to a formal job become less likely, but average wages increase.

Conditional on staying employed, mean wage growth is higher for spouses of inspected workers. However, this effect is heterogeneous across education and initial formality status. Wages at informal jobs increase 16 percent for spouses with less than a high-school degree (low education) where the non-inspected partner is initially informally employed. For non-inspected, high education spouses who are initially formally employed wages at formal jobs increase 9 percent after their spouse's establishment is inspected. In low-educated households, the increase in wages is driven by a composition effect: informally employed spouses with lower wages are less likely to remain employed after their partner transitions to a formal job. For high-educated households, most of the wage rise is due to job-to-job transitions.

We then extend Dey and Flinn (2008) partial equilibrium model of joint labor supply with

[4]Samaniego de la Parra and Fernandez Bujanda (2024) find that in the quarter of inspection, the average quarterly probability that an informal worker becomes formal at the inspected employer increases from 14% to 21%.

[5]We use the term couple, spouses or partners interchangeably to refer to a couple that includes the head of household and his or her partner regardless of marital status.

search frictions to incorporate formal and informal contracts and explicitly include households' valuation for a formal job. We use a simulated method of moments to estimate the model and compare the present discounted value of different formality status configurations between the head of household and their partner. This analysis results in three key findings. First, for a median income household, the value of the head of household receiving a formal job when their spouse not employed is $42 USD per month (i.e. 20% of the median monthly wage). This value is 30% higher than what it costs the firm to provide access to the benefits of a formal job for the median worker. Second, the value of formality is decreasing in household income as lower income households have more to gain from a steeper job ladder. Notably, at the estimated value of formal jobs paying the minimum wage, the net value is negative due to a regressive payroll tax. Third, the value of a formal job is much higher for households when it is the female spouse who is formal and the male is not employed.

As a check on the model-based value of formality, we use the reduced-form estimates of the change in spousal labor supply after an inspection to calculate the compensating income transfer that would generate an equivalent response. For the median household, we find that spouse's labor supply response to their partner receiving an inspection is equivalent to receiving a USD$34 income transfer, under a conservative labor income elasticity estimate. This value is 5% higher than the monthly tax costs to a firm of formally employing a worker with the median income[6] The net value of a formal job is negative for households in the lowest income deciles, but as in the model, this is driven by the fixed costs of formalization rather than by low valuations among lower income households. Reassuringly, the value of formal jobs for the median household is similar using the reduced-form, back of the envelope calculation. Moreover, the model replicates the change in wages and consequently in the value of formality for the bottom quartile of the income distribution observed in the data, even though these are not targeted moments.

Our paper contributes to a long-standing debate on the underlying causes of informality.[7] An open question in this literature is the extent to which self-selection versus market segmentation explain the prevalence of informality. Several authors argue that high transition rates

[6]Firms can potentially illegally save on workers' registration and payroll taxes by falsely reporting a lower wage. Comparing administrative data sources against workers' self-reported wages, Kumler et al. (2015)Kumler, Verhoogen and Frias (2015) find extensive under-reporting in Mexico, specially by smaller firms. We abstract from this when calculating firms' costs and assume that firms pay the full required payroll taxes. Under-reporting would decrease the cost to the firm and would also negatively affect the benefits of formality for the worker since severance and pension payments are linked to the reported wage.

[7]See Fields (2007), Fields (2009) for a review of the literature and guidelines for modeling labor markets with informality.

across formal and informal employment and small wage differentials, such as those found in Mexico, provide evidence against the segmented market hypothesis (Maloney (1999), Maloney (2004), and Pratap and Quintin (2006)). Low enforcement, high and regressive taxes along with the development of a non-contributory public system to bridge the gaps in health-care coverage among the low-income population make is such that formal employment offers low added value (Levy (2008), Anton, Hernandez and Levy (2013)). A more conciliatory approach points out that informal workers are heterogeneous and while that voluntary entry to informality co-exists with entry barriers and queueing for formal jobs (Cunningham and Maloney (2001), Fields (2005), Bosch and Maloney (2010), Gunther and Launov (2015)). How much different households value having a formal job is at the core of this debate and yet the existing literature is still missing a value for this parameter.

Our work is related to prior empirical work estimating the value of job characteristics and amenities. Mas and Pallais (2017) document considerable heterogeneity across US workers in valuation of job characteristics. Papers focused in developing countries aim to estimate the willingness to pay for the benefits of a formal job. Based on an efficient market hypothesis and compensating wage differentials, Argente and García (2015) estimate an average value of formal benefits for informal workers equal to 7.9% of their monthly labor income (USD$217). Koettl and Weber (2012) uses synthetic measures of marginal tax rates and benefits to argue that the net benefits of formalization are often negative. Madrigal, Pagés and Suaya (2016) find that self-reported job satisfaction does not increase among workers who transition to a formal job in Mexico. Using the expansion of free clinics in Mexico City as an instrument, Juárez (2012) estimates a 23% informal wage premium in a sample of low-educated female salaried workers. By contrast, Azuara and Marinescu (2013) do not find any significant wage effects across formality transitions with the roll out of non-contributory health insurance. This leads the authors to conclude that the marginal worker in Mexico places little to no value in health insurance access.

We contribute to this literature measuring the value of a formal job in two ways. First, given that the benefits of a formal job extend to workers' families, we consider joint decision by households. We develop a model with households, composed by two working-age individuals who face search frictions in a labor market with formal and informal job offers. Prior work by Dey and Flinn (2008) and Nishiyama (2010) in the US show that ignoring the existence of spouses or dependents can lead to biased estimates of the marginal willingness to pay for health insurance. Second, we combine a structural estimation approach with quasi-experimental variation in entry to formal jobs generated by random work-site inspections.

These inspections increase within-employer informal to formal job transitions (Samaniego de la Parra and Fernandez Bujanda (2024) and outflows from informality. We use households' labor supply responses to this exogenous increase in the probability of receiving a formal job offer to estimate the model and changes in wages for job switchers to validate its results.[8]

Conti, Ginja and Narita (2018) also combines a structural model and a reduced-form approach to estimate the value of non-contributory health insurance in Mexico. Our paper is related to theirs but differs in at least three important ways. First, their main goal is to measure the value of non-contributory health insurance (as in Juárez (2012) and Azuara and Marinescu (2013)) in a context with joint labor supply and informality. Meanwhile, we aim to estimate the present discounted value of formality as a whole, including all benefits that a household perceives it gets from a formal job (pensions, day-care, paid leave, health care, job stability, differences in the job ladder, etc.). Second, we rely on exogenous, household-level variation in the probability of accessing a formal job and target these empirical moments in our structural model. Conti, Ginja and Narita (2018) instead leverage municipality-level variation in the roll out of non-contributory health insurance. Third, we measure the value of formality as differences in the present discounted value across households with different formal status configurations while for Conti, Ginja and Narita (2018) the value of healthcare access is derived from an additive parameter in the instantaneous utility function.[9] Our approach allows for heterogeneity in the value of formal jobs across the income distribution that is not generated exclusively through risk aversion.

The paper is organized as follows. In section 2, we describe the institutional setting including the set of government mandated benefits that accompany a formal job, the taxes that finance them, the worksite inspection process and how it affects worker flows. Section 3 describes the data and reduced-form analyses. Section 4 presents the theoretical framework. In section 5, we describe the estimation procedure and the resulting estimates. In section 6, we compare the model's results to estimates of the value of formality and changes in reservation wages at different points of the income distribution against analogous moments from non-targeted reduced form estimates. Section 7 concludes.

[8]See Mahoney (2022) and Todd and Wolpin (2023) for a description on ways to combine models with descriptive statistics and identified reduced-form estimates.

[9]We allow for a similar parameter in our model to account for potential differences in non-pecuniary preferences for (or against) formality. However, the estimated parameter value is close to zero.

# 2 Formal Jobs, Social Benefits, and Payroll Taxes in Mexico

## 2.1 Formality, Benefits, and Taxes

Mexico's Labor Law requires employers to register all employees with the Social Security Institute (IMSS).[10] Registered workers are eligible for a wide range of social benefits including health care for themselves and their family members, daycare services and childbearing leave, a pension fund to which the employer and the government contribute, sick leave, disability insurance, among other goods and services.[11] Employers must also comply with minimum wage laws, severance payments in case of termination, and every year they must redistribute ten percent of their after-tax profits with their registered employees[12]

These benefits for registered workers and their family members are financed using payroll taxes levied on employers and employees. Once a worker is registered with IMSS, the employer must calculate the total contributions, make the corresponding deductions to workers' earnings, and forward the payments to IMSS each time the employee is paid. Payroll taxes on employers have 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. As a result, the cost of employing a worker formally ranges from 17% of the wage for an employee that earns 25 minimum wages to 35% for a minimum wage earner.[13] Neither workers nor employers can opt-out of the benefits, regardless of whether workers use or want them.

Formal jobs are those for which the employer complies with this registration requirement. Despite Federal Labor Law and the Social Security Law mandating that all wage-earning employees must be registered with IMSS, one out of every four workers employed in tax-paying firms in Mexico is informally employed (Samaniego de la Parra and Fernandez Bujanda (2024)). Formality status, therefore, is not a static worker characteristic, instead, it can vary across time and relates to an employment contract's compliance (or lack thereof) with the requirement to be registered with IMSS. Workers can thus transition in and out of for-

[10] Mandated registration applies to all salaried employees including temporary, part-time, and, up until the 2021 labor reform also outsourced workers. Cámara de Diputados del H. Congreso de la Unión (November, 2015*a*)

[11] Daycare services were only available for working mothers until 2020 when the Social Security Law was amended to extend this benefit to all registered workers regardless of gender.

[12] Newly created firms (operating for less than a year) and those with less than USD$15,000 in annual profits are exempt from the profit-sharing requirement.

[13] There is an upper bound on employers' contributions at a daily wage of MXN$1,826 pesos (USD$101.45) or 25 minimum wages. See table B.9 for additional details on the payroll tax schedule.

mality without changing employers, work schedules, or having any effect on their day-to-day job.

Informal contracts evade payroll taxes but make workers ineligible for contributory social benefits. To bridge the gap in access to health care, in 2004 the government created "Seguro Popular": a parallel system that offered health care services for the low-income population. To the extent that the quality of services provided by this system is comparable to those of IMSS, the rollout would decrease incentives to be formal (Levy (2008)). Prior literature, however, disagrees on whether the marginal worker's decision to be formal was affected by this change (Juárez (2012), Azuara and Marinescu (2013)).

Besides the statutory benefits that accrue to the worker and their family while under a formal contract, formalization may have additional value. Higher separation costs that translate into longer employment spells and reduced income volatility can increase a partner's ability to seek out riskier but more profitable or otherwise rewarding opportunities. Alternatively, under a model with asymmetric information, poaching firms may interpret formality status as a signal of worker quality such that formal jobs are more likely to lead to higher paid, formal jobs in the future.[14]

## 2.2 Enforcement

In Mexico, two distinct agencies are responsible for enforcing labor regulations: the Ministry of Labor and Social Welfare (STPS) and the Social Security Institute (IMSS). STPS visits work-sites to verify whether they comply with labor regulation.[15] Meanwhile, IMSS audits establishments to determine whether they register all wage-earning employees and pay the required social security contributions. Although informal employment is one of the items on the STPS inspection checklist, sanctioning firms for this violation falls outside its purview. When STPS finds informal workers during an inspection, the inspector flags it in their report and sends a notice to IMSS.[16] These notifications from STPS help IMSS identify employers that are more likely to be evading payroll taxes through informal employment (Instituto Mexicano del Seguro Social (2015)).

[14]Michaud (2018) develops such a model in which employment duration is used as a signal for worker type. In an augmented model with informal and formal jobs, where formal jobs have longer durations, formality can be a similar signal. Samaniego de la Parra and Fernandez Bujanda (2024) show that labor market trajectories after an exogenous transition to formality are consistent with formal jobs used as signals.

[15]Reglamento Interior de la Secretaria de Trabajo y Prevision Social, Article 18-VI41

[16]See Samaniego de la Parra and Fernandez Bujanda (2024) for additional information on STPS inspection process.

STPS has three types of inspections: initial, ordinary, and extra-ordinary.[17] Ordinary inspections account for the vast majority of visits on an average year. STPS chooses establishments randomly from a list known as the National Firms' Directory (DNE) for ordinary inspections.[18] After each visit, STPS inspectors file a report detailing the results from the inspection, specifically, whether there were any violations that need correction. The report provides a lists of actions that employes must take to resolve any problems and specifies a deadline to do so. STPS then schedules a follow-up visit to verify that the corrective measures were implemented and fines employers if they were not.[19]

If IMSS audits a firm and finds evidence of informal employment, the employer must pay fines for each unregistered worker and back-due payroll taxes.[20] IMSS may also prosecute employers for social security fraud, punishable by up to nine years in prison. IMSS is lenient towards employers that correct violations before an enforcement action. If an employer registers its workers before IMSS prompts them to, 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. Therefore, since receiving an inspection by STPS increases the probability of an IMSS audit, employers have incentives to change the status of any informal workers promptly after receiving a visit by STPS. Consistent with this change in incentives, Samaniego de la Parra and Fernandez Bujanda (2024) find that workers' probability of transitioning out of informality increases on the quarter when their employer is inspected by STPS.

# 3 Data Description

This paper combines data from a panel household survey with information from the universe of worksites eligible for STPS random inspections and the resulting inspection logs.

[17] Initial inspections are scheduled when an establishment first starts operating. Extra-ordinary inspections are scheduled when there is a report filed against the employer or when idiosyncratic events increase the likelihood of violations or the risks to worker safety in a particular geographical region or industry. For example, during the COVID19 pandemic, STPS scheduled extra-ordinary inspections to verify that social distancing norms in the workplace were being upheld and that non-essential businesses were not operating.

[18] Reglamento General para la Inspeccion y Aplicacion de Sanciones por Violaciones a la Legislacion Laboral40

[19] The Federal Labor Law6 and the Federal Labor Inspector's Manual42 detail the way in which worksites are chosen for inspection, employers' rights and responsibilities, inspectors' instructions, and the overall inspection and sanction processes.

[20] Table B.10 shows the range of fines, in daily minimum wages, that an employer must pay if caught employing workers informally.

Specifically, we use the National Institute on Statistics and Geography (INEGI)'s National Employment and Occupation Survey (ENOE), the National Directory of Firms (DNE), and STPS's Inspections Logs from 2005 to June 2016. We describe each of these datasets and how we use them below.

## 3.1 The Ministry of Labor and Social Welfare's National Firm Directory (DNE) and Inspections Log

The DNE is a list of work-sites registered with STPS. It includes the firms' name, unique tax identifier (RFC), the date when the firm registered with STPS (and hence became part of the DNE), and its work sites addresses. Between January 2005 and June 2016, there were 248,549 firms with 423,129 establishments in the DNE. STPS constructs the DNE through three main sources: 1) STPS labor regulation enforcement efforts; 2) firms' participation in any of STPS's training and government procurement support programs; and 3) information sharing with other government and private sector institutions. Firms do not have a legal obligation to register with STPS, so the DNE is not an exhaustive list of all establishments operating in Mexico. In section 3.3, we discuss how we match the DNE to the household data, how our sample of workers compares to a representative sample of workers in the economy, and how the matching process and the composition of the DNE may affect our results.

STPS chooses work-sites randomly from the DNE for ordinary inspections.[21] STPS uses an electronic system to implement the random allocation of inspections across establishments in the DNE. Samaniego de la Parra and Fernandez Bujanda (2024) test that the allocation of inspections across establishments is uncorrelated with firm and worker characteristics. We reproduce these findings in Appendix A.4.

Between January 2005 and June 2016, STPS performed 620,842 work-site inspections. For each of these, the Inspection Logs list the firm's name, the inspected work-site's address, the inspection date, a short description of the inspections' results, and whether the inspection led to any admonitions or fines. Only 10% of all inspections led to a fine by STPS. Between 2005 and 2016, the average fine was MXN$32,194 (USD$1,740), the highest was for MXN$82,569,000 (USD$4,463,189)[22] and the lowest MXN$20.57 (USD$1.11). Table B.11 provides further detail on the outcomes of STPS's inspections.

[21] Random work-site inspections are required by the Federal Labor Inspector Manual42 and STPS's General Regulation for Inspections and Implementation of Fines for Labor Law Violations40

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

## 3.2 The National Employment and Occupation Survey (ENOE)

ENOE is a quarterly rotating panel and the source for official labor market statistics in Mexico. The survey is ongoing; available starting on the first quarter on 2005 and the latest publicly available data point is for the third quarter of 2024. In each survey round, ENOE gathers detailed information regarding households' composition and dwelling characteristics, as well as extensive data on each household member such as their age, education, gender, labor market participation, wages, access to social security benefits through their employer (i.e., formality status), and other job characteristics.

The dwelling is the sampling unit in ENOE. The survey tracks different families within a dwelling. Families are defined as a group of individuals who share income and consumption. Each quarter, the sample includes 120,260 households and 420,000 individuals on average. Households are interviewed for five consecutive quarters and then replaced, maintaining sample representativeness.[23]

As part of its "Employment and Occupation Module," ENOE asks all household members over 12 years of age about their employment status and, if employed, it collects information about their employer. This module, coupled with ENOE's panel structure allows for direct calculation of individual-level labor market flows. Moreover, the using employers' names we can distinguish between formality transitions that occur within and across employers and jobs. Further, we are able to group workers based on their former, current, and previous employers. Using firms' names and locations, we can match workers in ENOE to STPS's Firm Directory and Inspections Logs to track workers before and after an inspection occurs. The data that allows identifying firms, including the firm's name, is removed from the publicly available data. This paper, and the related work by Samaniego de la Parra and Fernandez Bujanda (2024) are the first to use ENOE's employer-side data.

We use self-reported access to employer-provided IMSS's benefits (i.e., registration with IMSS) to identify formal and informal workers in ENOE's sample.[24] In order to account for misclassification or incorrect reporting across ENOE's waves, we consider consecutive back and forth transitions across formal and informal jobs with the same employer to be

[23] 85% of households remain in the sample for a full year and 77% stay in the sample for the full 5 quarters.

[24] We use the definition of informal employment adopted during the 17th International Conference of Labor Statisticians which sets international standards for measuring informalityInternational Labour Organization (2003).

misreported transitions.[25,26]

## 3.3 Sample Construction

Our sample includes households headed by a heterosexual couple, regardless of marital status, where both partners are between 25 and 59 years of age when they first enter ENOE's sample. We exclude households where either spouse had self-employment spells or owned a business, received welfare benefits, was enrolled in school, retired or disabled. The sample also excludes households with older adults present. These constraints seek to ensure that wages represent the main source of income for households. We focus on households that remain in ENOE's survey for at least four of the five waves and whose 4th interview corresponds to an "extended questionnaire" (19 out of 40 cohorts).[27] Our final sample includes 58,568 households.

## 3.4 Reduced-form Effects of Spousal Transitions Out of Informality

In this section, we first document various stylized facts about households' joint labor supply and wages. Then, we estimate the effect of inspections for one partner's employer on their spouse's labor supply and wages.

Female labor force participation in Mexico remains well below that of men. Between 2005 and 2016, labor force participation rates were 40 percent for women and 80 percent for men. Informality rates are also higher among women (56 versus 46 percent for men).

[25] We consider any worker that experiences a formal-informal-formal transition in consecutive quarters while working with the same employer to be formal throughout the whole nine-month period. Analogously, we reclassify a worker as being informal in three consecutive quarters if we observe an informal-formal-informal transition without a change in employer. 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 and 2.0 percentage points, respectively.

[26] If employers register their workers after an inspection to avoid being detected in a follow-up visit by IMSS but then deregister 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 deregistering workers within short periods of time can raise flags with authorities making their work-sites targets of future inspection visits.

[27] During the first 6 quarters of its existence, ENOE used an extended questionnaire for all rounds of the survey. After the second quarter of 2006, the extended questionnaire was used once a year so that households in the sample answer this version of the survey at least once through their 5-quarter rotation. The extended version of the questionnaire adds questions regarding worker's tenures at their jobs, their employment start date,and for non-employed individuals there are questions on the reasons for separation, current search and former job characteristics.

Table 1 panel A presents the joint distribution of households in our sample by labor market participation and formality status. Given the low levels of female labor force participation, the most common household configuration has an employed male head of household and a non-employed female spouse. This type of household accounts for two thirds of our sample. Within this group, half of the households have a formally employed head, and in the other half the head of household has an informal job. Conditional on the female partner being employed, the male is likely to have the same formality status as the female partner. After-tax average wages are higher for formally employed head of households, regardless of their spouse's employment status. Mean formal wages are higher for head of households with non-employed spouses than with informally employed partners, although the difference is not statistically significant.

We calculate an annual transition matrix across formality and employment status in Table 2. For the head of household, formality status is a highly persistent state. 88 (81) percent of household heads that were formally (informally) employed when they first entered the survey had the same formality status a year later. 15 percent of those who were not employed in the first survey were also not employed a year later.[28] 62 percent of head of households who are not employed during the first wave of the survey, find an informal job by the fourth wave. In contrast, for female partners, non-employment is the most persistent state, followed by formal employment. 88 percent of female partners who are employed on the first survey wave continued to be unemployed a year later. Most of these are prolonged unemployment or non-participation spells as opposed to repeated exits from employment.

Table 3 reports women's outflows from non-employment conditional on their male spouse labor market trajectory. We focus on households were the female partner is not employed when the household is first selected into the survey and the head of household is employed. Despite the low female participation rate in the labor market, transitions into the workforce are more likely among households were the male partner experiences an exit from formal to informal employment. Moreover, the type of jobs that women enter when leaving non-employment varies conditional with their spouse's labor market transitions. The probability of NE-Formal transitions is highest for women in households were the male head of households leaves a formal job to unemployment (6.99 percent). Meanwhile, women in households were the male transitions from an informal to a formal job have the lowest NE-Formal rates (0.73 percent).

[28] Most of these are not continuous unemployment spells. Instead, these are cases of one or more job separations experienced within the year.

The joint labor market transitions presented in Table 3 provide some evidence of joint strategic decision making by households. When the head of households loses a formal job, and consequently the household loses access to the benefits of formality, the female partner is more likely to leave non-employment, in particular, towards a formal job. On the flip side, since the benefits of formality are shared but each formalized employee must pay payroll taxes (i.e., the benefits are public but the costs are private), when a male partner transitions to a formal job then their female partner is less likely to go from non-employment to formality.

While the strategic behavior described here is consistent with the descriptive statistics, these of course cannot be interpreted as causal. Unobserved factors can influence both the head of household's exit from a formal job and their partner's exit from non-employment. To examine how spouses respond to their partners exogenously receiving a formal job offer, we leverage STPS's random inspection schedule.

Inspections are a shock that affects a worker's probability of transitioning out of informality as documented in Samaniego de la Parra and Fernandez Bujanda (2024). To estimate spouses' responses to an inspection shock, we further restrict the sample to households in which at least one partner is employed as a wage-earner at a firm included in the DNE for at least one of ENOE's waves. This guarantees that the sample includes only workers at formal firms eligible for an STPS inspection. We refer to the head of household as spouse 1 and to their partner as spouse 2.

Each household can be characterized by its members joint labor market status defined as the type of current employment (or lack thereof) and its associated wage. Let $\mathbf{LMS}_{i,h,t}[j|\,(k,INF)]$ be an indicator variable equal to if spouse $i \in \{1,2\}$, in household $h$ goes from labor market status $k$ in period $t$ to status $j$ in period $t+1$, while their partner, $-i$, transitions out of informality. We model the probability that spouse $i$ transitions from labor market state $k$ in period $t$ to $j$ in period $t+1$, conditional on the household's joint labor supply in period $t$, as function of spouse's i's characteristics and aggregate labor market conditions in household's $h$ location, as shown in equation 1 where $\eta_{i,h}$ and $\gamma_{l(h),t}$ are spouse and locationXtime fixed effects, respectively.

$$\mathbf{LMS}_{i,h,t+1}[j|\,(k,INF)] = \alpha_i^{j,k}\mathbf{LMS}_{-i,h,t+1}[NotINF|\,(k,INF)] + \eta_{i,h} + \gamma_{l(h),t} + \varepsilon_{i,h,t} \quad (1)$$

If spouse's 1 transitions out of informality were exogenous, we could then interpret the set of $\alpha_2^{j,k}$ coefficients as the effect of spouse's 1 transition out of informality on spouse's 2 probability of transitioning from state $k$ to $j$. However, labor market status transitions are

unlikely to be uncorrelated across spouses and their characteristics. If households are not risk neutral or any of the jobs' amenities or pay are shared among household members, joint decision making offers opportunities for households to smooth income risk and maximize utility (Dey and Flinn (2008), Guler, Guvenen and Violante (2012), Flabbi and Mabli (2018)). The set of acceptable wages and hence the likelihood of accepting any job offer, depends on a partner's current wage and job type. To isolate independent variation in one spouse's transitions, we focus on the case where spouse $-i$ is initially employed in an informal job and use STPS's inspections as a shock to the probability of transitioning out of informality.

Let $Inspected_{-i}$ be a dummy variable equal to 1 if spouse $-i$ is employed at an inspected work-site and 0 if employed at a work-site included in the DNE that has not been inspected. For each treated household, we construct a control group. To do so, we begin with the subset of household with one member who is informally employed at a DNE-registered firm that has not received its first inspection when the household was surveyed. Among this set of non-inspected households, we identify those with identical joint labor market status to the treated household, that enter ENOE's survey at the same point in time, have the same education level, the same number of children under and above 5 years-old, and belong to the same income quartile at the start of the survey. We then randomly select a subset of households from these untreated group and assign them a placebo inspection date equal to the matched treated household's inspection date.[29] We then define variable $Post_{-i,t}$ as an indicator equal to 1 for all survey waves after partner $-i$'s establishment receives an inspection (or placebo inspection, in the case of the control group) and zero otherwise. Equation 2 presents our baseline specification:

$$\mathbf{LMS}_{i,h,t+1}[=]j|(k,INF)] = \beta_i^{j,k} Post_{-i,t} \times Inspected_{-i} + \delta_i Post_{-i,t} + \eta_{i,h} + \gamma_{l(h),t} + \omega_{w(h)} + \varepsilon_{i,h,t} \tag{2}$$

where $\eta_{i,h}$, $\gamma l(h),t$ and $\omega_{w(h)}$ are, respectively, spouse fixed effects, locationXtime fixed effects, and survey-wave fixed effects. We separately estimate equation 2 for each combination of current and future labor market statuses for the non-inspected spouse.

The set of $\beta^{k,j}$ coefficients in equation 2 estimate the average effect of spouse's $-i$ tran-

[29]The household's inspection date refers to the date when spouse's 1 employer was inspected. "Inspected" or "treated" households are those in which the head of household or their spouse was employed at a DNE registered firm that their first ordinary STPS inspection while (i) the household member was informally employed at the firm and (ii) while the household was interviewed for one of ENOE's waves. "Non-inspected" households are those in which the head of household or their spouse was employed at a DNE registered firm during one of ENOE's waves but the household's interviews all occurred before the firm's first ordinary inspection.

sition out of informal employment on spouse's $i$ probability of being in labor market state $j$ in period $t+1$, conditional on being in labor market state $k$ before the arrival of the random inspection. We interpret the estimated $\beta^{k,j}$ coefficients in equation 2 as spouses' elasticity of labor supply from inflows out of informality.

We first estimate equation 2 on the set of households where the non-inspected spouse is female and not employed at the time of the inspection. Table 4 presents the results. After an inspection, non-employed spouses' probability of remaining non-employed increases by 6.6 percentage points (from 68.9% to 73.5%). This increase is due to a 5.9 percentage points decline in the probability of starting a formal job (from 12.7% to 11.9%).[30]

We then analyze the effect of inspections on spousal wages. Table 6 presents the estimated coefficients. We separately measure the effect by household's education. For low education groups where both spouses have an informal job before the inspection, an increase in outflows from informality is associated with an 16 percent increase in informal wages, but has no statistically significant effect on the wages of accepted formal jobs. Meanwhile, within formal-jobs wages increase by 9.1 percent.

In the following sections, we will use the descriptive statistics and reduced-form results discussed above to estimate a model of joint labor supply with formal and informal jobs.

# 4 Model

In this section, we present a partial equilibrium, joint household search model in which each household member randomly receives job offers. It is important to note that our goal is not to explicitly model the structure of the formal sector or to explain the process through which different contracts co-exist among homogeneous workers. The model is neither well-suited nor is it intended for policy counterfactuals that would affect the wage distributions in formal or informal jobs, or firms incentives to create and/or destroy jobs. Instead, the purpose of the model is to build an economy with a distribution of the workforce similar to that observed in Mexico. We will then introduce a shock that changes the outflows from informal jobs and simulate the difference-in-difference exercise produced by inspections in the data.

[30]For individual household members, Azuara and Marinescu (2013) find that access to health care benefits through Seguro Popular did not increase the propensity to participate in the informal market. Consistent with their results, we find that the likelihood of being informal does not significantly increase after an increase in spouses' formalization probability. However, the probability of being formally employed is negatively affected.

Households consist of two spouses who at any point in time can be either formally employed (F), informally employed (INF), or not employed (N). Each spouse receives job offers with a random and exogenous probability ($\lambda^i_{k,j}$ $j \in \{F, INF\}$). A job offer is a characterized by a wage and a formality status. Further, offer arrival rates vary depending on the person's current labor market state $k \in \{N, F, INF\}$, the formality status of the offer $j \in \{F, INF\}$ and across spouses $i \in \{M, Fem\}$.[31] The probability of becoming non-employed if working in a type $j$ job is determined by the involuntary separation rate $s^i_{k,N}$. Job offers are characterized by the offered wage $w$. We assume each spouse draws from different wage distributions, and these further vary across formal and informal jobs. We use $H^i_j(w)$ to denote the wage distribution for jobs of type $j \in \{F, INF\}$ for spouse $i$.

Time is continuous, the horizon is infinite, and households have a homogeneous discount factor $\rho$. Crucially, we allow for endogenous separations and a separation for a spouse can occur simultaneously to a partner transitioning to a different job. Endogenous separations occur in equilibrium when one partner receives an offer such that the marginal utility of income for the household is now lower than that of non-employment, incentivizing the other spouse to quit their current job.

Households receive utility from their labor income ($I = w_M + w_{Fem}$) and leisure when not employed ($\underline{=} b_M + b_{Fem}$). Additionally, we allow for formal jobs to generate an added instantaneous utility that differs for each spouse, $\xi_i$. This parameter can represent the in-kind benefits of formal jobs or a non-pecuniary utility (e.g., workers enjoy/are proud of) from formality.

Let $F_i$ be an indicator function equal to 1 if spouse $i \in M, Fem$ is formally employed. Following Dey and Flinn (2008), we assume households' instantaneous indirect utility function is given by:

$$U_\xi(I, F_M, F_{Fem}) = f(I, b, \xi_M F_M, \xi_{Fem} F_{Fem}) \tag{3}$$

The parameters $\xi_M$ and $\xi_{Fem}$ coupled with household's joint income $I$ or leisure $b$ generate variation across households in the rate of substitution between the benefits provided by a formal job (e.g., government provided health care, day care services, pensions, etc.) and those purchased from the private sector using the household's pooled income. Although we assume public consumption for all goods and benefits within the household, even those households

[31] These stand for Male and Female. Keeping track of spouses' gender is relevant because daycare and other child-caring benefits were only available to formally employed women during our sample period and because of the vastly different patterns in labor force participation across genders in Mexico.

who already have a member with access to a formal job may place value on a second member receiving a formal job offer. Formal jobs can be more attractive than informal ones to the extent that they offer different labor market trajectories (separation rates, on-the-job finding rates, and wage distribution). Moreover, having a second spouse's employer provide social security benefits can be of value to risk averse households in the event that the other partner experiences an involuntary separation.

The state vector characterizing the household decision problem is given by spouses' joint labor market status and the corresponding wages $S = (LMS_M, w_M, LMS_{Fem}, w_{Fem},)$ where $w_i = 0$ if $LMS_i = N$. The value functions for households when none, one, and both spouses are employed are:

- Both spouses are non-employed

$$\begin{aligned}
&\rho V(0,0,0,0) = f(b_1 + b_2) \\
&+ \sum_{\tilde{j}=\{F,INF\}} \lambda^M_{N,\tilde{j}} \int_{w^{\tilde{j}*}_M} \left(V\left(\tilde{j},\tilde{w},0,0\right) - V(0,0,0,0)\right) dH^M_{\tilde{j}}(\tilde{w}) \\
&+ \sum_{\tilde{l}=\{F,INF\}} \lambda^{Fem}_{N,\tilde{l}} \int_{w^{\tilde{l}*}_{Fem}} \left(V\left(0,0,\tilde{l},\tilde{w}\right) - V(0,0,0,0)\right) dH^{Fem}_{\tilde{l}}(\tilde{w})
\end{aligned} \tag{4}$$

Reservation wages for jobs in a given formality status differ across genders due to three different mechanisms: (i) the distribution of wages for a given type of job differs across gender ($H^M_{\tilde{j}} \neq H^{Fem}_{\tilde{j}}$), (ii) the difference in the arrival rate of offers when employed versus when unemployed is also heterogeneous across genders ($\lambda^{Fem}_{N,k} - \lambda^{Fem}_{E,k} \neq \lambda^M_{N,k} - \lambda^M_{E,k}$), and (iii) separation rates from a job with a given formality status differ across gender.

- Both spouses are employed with job types $k_M$, $k_{Fem}$, and wages $w_M$, $w_{Fem}$

$$\begin{aligned}
&\left(\rho + s_{k_{M,N}} + s_{k_{Fem,N}}\right) V(k_M, w_M, k_{Fem}, w_{Fem}) = f\left(w_M + w_{Fem}, \left(\max\{\xi_M \mathbf{I}[k_M = F], \xi_{Fem}\mathbf{I}[k_{Fem} = F]\}\right)\right) \\
&+ \sum_{\tilde{j}=\{F,I\}} \lambda^M_{E,\tilde{j}} \int_{\hat{w}^{\tilde{j}}_M(\ldots)} \left( max\left\{V\left(\tilde{j},\tilde{w},k_{Fem},w_{Fem}\right), V\left(\tilde{j},\tilde{w},0,0\right)\right\} - V(k_M,w_M,k_{Fem},w_{Fem}) \right) dH^M_{\tilde{j}}(\tilde{w}) \\
&+ \sum_{\tilde{l}=\{F,I\}} \lambda^{Fem}_{E,\tilde{l}} \int_{\hat{w}^{\tilde{l}}_{Fem}(\ldots)} \left( max\left\{V\left(k_M,w_M,\tilde{l},\tilde{w}\right), V\left(0,0,\tilde{l},\tilde{w}\right)\right\} - V(k_M,w_M,k_{Fem},w_{Fem}) \right) dH^{Fem}_{\tilde{l}}(\tilde{w})
\end{aligned} \tag{5}$$

- Spouse 1 is employed with job types $k_M$ and wage $w_M$.

$$\begin{aligned}
&\left(\rho + s_{k_{M,N}}\right) V\left(k_M, w_M, 0, 0\right) = f\left(w_M, b_{Fem}\left(\xi_M \mathbf{I}\left[k_M = F\right]\right)\right) \\
&+ \sum_{\tilde{j}=\{F,I\}} \lambda^{M}_{E,\tilde{j}} \int_{\hat{w}^{\tilde{j}}_{M}(k_M,w_M,0,0)} \left( V\left(\tilde{j}, \tilde{w}, 0, 0\right) - V\left(k_M, w_M, 0, 0\right) \right) dH^{M}_{\tilde{j}}\left(\tilde{w}\right) \\
&+ \sum_{\tilde{l}=\{F,I\}} \lambda^{Fem}_{N,\tilde{l}} \int_{\hat{w}^{\tilde{l}}_{2}(k_M,w_M,0,0)} \left( max\left\{ V\left(0, 0, \tilde{l}, \tilde{w}\right), V\left(k_M, w_M, \tilde{l}, \tilde{w}\right) \right\} - V\left(k_M, w_M, 0, 0\right) \right) dH^{Fem}_{\tilde{l}}\left(\tilde{w}\right)
\end{aligned} \tag{6}$$

Four points are worth highlighting from equations 5 and 6:

1. Risk-neutral households ($\delta = 1$) with no non-pecuniary value for formal jobs ($\xi_M = \xi_{Fem} = 0$), maximize welfare with decentralized decision-making. This result holds due to the constant marginal utility of income making any spouse's decisions independent from their partner's current wage or formality status. Although the model does not constrain search frictions to be the same across partner (i.e., job finding and job separation rates differ between men and women), perfect income pooling makes each partner's independent maximizing behavior optimal for the household.

2. The value of a second formal job in the household is not zero even if the non-pecuniary utilities from formality for each spouse are perfect substitutes. Even when one spouse is already formally employed and hence the household already enjoys the "non-pecuniary" utility from formality ($\xi$), the other spouse's reservation wage to transition from an informal to a formal job is below than their current wage if ($\xi > 0$) or if the search environment is better when formally employed, i.e., $V\left(F, w_1, INF, w_2\right) = V\left(F, w_1, F, w_2 - c\right)$ where $c > 0$.[32]

3. Spouses are not indifferent between who holds a formal job, even if households are risk-neutral (i.e. $f(\cdot)$ is linear) and the instantaneous utility derived from formality is the same across households ($\xi_F = \xi_M \neq 0$). With a constant marginal utility of income ($\delta = 1$), the instantaneous utility is the same regardless of who holds the formal job ($w + w + \xi$). However, $V\left(F, w, INF, w\right) = V_M\left(F, w\right) + V_{Fem}\left(INF, w\right) \neq V_M\left(INF, w\right) + V_{Fem}\left(F, w\right) V\left(INF, w, F, w\right)$ because employment trajectories differ by gender and formality status and hence $V_M\left(k, w\right) \neq V_{Fem}\left(k, w\right) \quad k \in \{F, INF\}$.

[32] In the baseline version of the model, heterogeneous separation rates across gender and formality status drive this effect.

4. For all households and for both spouses, if the formality status of a new job offer is the same as their current job's formality status, and their partner's labor market status does not change, then the reservation wage is equal to the current wage.

# 5 Model solution and estimation

The model's solution is characterized by a set of 36 distinct reservation wages (one per each possible combination of formality and employment statuses for each spouse). We assume that the instantaneous utility function is given by:

$$f(I,b,\xi) = \frac{(I+b)^{\delta}}{\delta} + (\max\{\xi_M \mathbf{I}[k_M = F], \xi_{Fem}\mathbf{I}[k_{Fem} = F]\})$$

and that the distribution of offered wages in formal and informal jobs for each spouse are log-normal $H_j^i(\cdot)$ : $\ln(w) \sim N\left(\mu_{i,j}, \sigma_{i,j}^2\right)$ $j \in \{F, INF\}$, $i \in \{M, Fem\}$ with $0 < \underline{w}_F = MW < \overline{w}_F < \infty$ and $0 < \underline{w}_{INF} < \underline{w}_F = \overline{w}_{INF} < \infty$.

We estimate the 21 model parameters using Simulated Method of Moments (SMM) by targeting 29 sample moments. Three of the moments that we target we obtain from the reduced-form estimates of a partner's responses to their informally employed spouse receiving an inspection (i.e., equation 2). We choose the exogenous separation rate parameters using data on involuntary separations to non-employment. Consistent with prior literature, we set the discount rate to 0.02. We jointly estimate the rest of the parameters by minimizing the weighted distance between the moments obtained in the simulated labor market trajectories and the empirical moments.[33]

We simulate 2,000 labor market histories for each parameter vector and estimate each of the targeted moments from the stationary distribution of labor market outcomes and transitions across these simulations. Then, we introduce a one-time unexpected "inspection shock" for a subset of the employed simulations in the stationary distribution. The shock creates a transition from informal to formal employment for men among a share $\upsilon$ of the simulated trajectories and a transition from informal to non-employment for a share $\kappa$. We set $\upsilon$ and

[33]We use the simulated annealing (SA) method to minimize the distance between simulated and sample moments. SA allows for the parameter vector to be estimated while searching for a global minima by systematically explores the parameter space by iteratively generating different parameter combinations. This approach is useful since multiple local minima may exist for an objective function which is non-linear in parameters.

$\kappa$ to 0.18 and 0.08, respectively, to match the increase in the probability that a worker is formalized after an inspection with the same employer and without changing wages and the increase in the probability of unemployment after an inspection, as estimated by Samaniego de la Parra and Fernandez Bujanda (2024). Finally, we estimate the impact of an inspection shock on women's transition probabilities out of unemployment within the simulated labor market trajectories to target the difference-in-differences coefficients from equation 9.

Table 8 presents the estimated parameter values. Table 9 compares the simulated moments and their empirical counterparts. Figure 2 shows the model estimated reservation wages for women for formal and informal job offers when they are non-employed, as a function of their employed spouse's wage and job formality status. Spouse 2's reservation wages are increasing in household income levels. Conditional on their partner's formality status and wage, women's reservation wages for a formal job are higher than for an informal job. Moreover, the reservation wage for informal jobs conditional on their partner being formal is lower than the reservation wage for a formal job when the partner is informal at low income levels. But this relationship is reversed as income increases. The difference in female reservation wages between formal and informal jobs at low levels of income for their spouse and conditional on men's formality status is minimal. However, this gap increases with the male partner's labor income.

Using the reservation wages for individuals that transition from informal to formal jobs, we calculate the value of a formal job. Consider the value function of a household with a non-employed spouse and an informally employed spouse who earns wage $w$: $V(INF, w, NE, 0)$. Suppose the household receives a shock that converts the job into a formal one without a change in wage. If the household sees this is as an improvement then $V(INF, w, NE, 0) < V(F, w, NE, 0)$. The household will be willing to pay for this transition up to the compensating variation $\$C$ where $\$C$ is such that $V(INF, w, NE, 0) = V(F, w - C, NE, 0)$.

Figure 3 shows how the value of a formality for a job varies across the gender of the formalized spouse and by their current wage, conditional on their partner being unemployed. For a household with the median wage (MXN$24,500), the value of the male transitioning to a formal job is approximately MXN$3,400 pesos or 14 percent of the median wage. For women, the value twice as high with willingness to pay of 29 percent of the median wage.

Figure 4 presents each spouse's value of moving from non-employment to a formal job, conditional on their partner already having a formal job with median wage. The value of a second formal job is positive for women, albeit less than half the value of the first job. Among

men, the value of a formal job is negative except for the highest wage levels.

We next decompose the value of the first formal job into the various characteristics that distinguish formal and informal jobs. To do so, we estimate the willingness to pay to transition from non-employment to formality, conditional on their partner being non-employed after homogenizing one-by-one the parameters that distinguish formal and informal jobs. Table 10 shows this results. For men, willingness to pay for a formal job is for the most part driven by the downside risk in the income distribution of informal jobs. This also explains why the value of formality for men increases with income. Differential job finding rates also matter for the men's valuation. In contrast, for women the value of a formal job is entirely driven by differences in separation rates.

# 6 Discussion and Comparison to Reduced-Form Estimates

## 6.1 Value of a Formal Job (Reduced form version)

For spouses of informal employees at inspected work-sites, their partners' transition into a formal job represents an income shock equivalent to the monetary value of the benefits to which they are now entitled. Different households will assign a different value to these benefits depending on family size, age, income, and access to substitutes. Let $\frac{\delta LFP}{\delta F_h}$ be the observed marginal change in labor supply, along the extensive margin, after receiving the benefits of a formal job for a household of type $h$. Let $\eta_h^I$ be the Hicksian income elasticity of labor supply for a household of type $h$. We calculate the average monetary value of the benefits of a formal job $\overline{V}^F$ for household $h$ as:

$$\overline{V}_h^F = \eta_h^I \times \overline{LFP}_h \times \frac{1}{\frac{\delta LFP}{\delta F_h}} \tag{7}$$

Abramovsky and Phillips (2015) calculate a range of values for high and low responsive income elasticities of formal labor supply along the extensive margin. We use these elasticities to estimate the value-to-cost ratio of a formal job for 4 groups of households based on the head of household's average daily wage.[34] Costs are calculated as the sum of employers' and employees' social security contributions required to register a worker with the corresponding

[34]Since this exercise is based on household with an unemployed spouse, the head of household is assumed to be the only earner.

average daily wage as a formal employee. Our estimates, presented in table 7 suggest that the valuation of a formal job is between 4.2 and 1.05 times higher than the fiscal cost of formalization for employers for a household with median earnings in the formal sector. Meanwhile, for households in the lowest quintile of the income distribution the value of a formal job is lower than what it costs employers to register the worker. This negative net valuation is due to the high fixed costs of formality, rather than to comparatively low valuations among low income households.

# 7 Conclusions

In principle, all Mexican workers enjoy health, retirement and saving funds, and other social benefits in return for tax contributions paid by workers and their employers. In practice, enforcement is lax and many workers have informal jobs: they neither pay taxes to support programs that target registered workers nor receive the corresponding government mandated benefits. Moreover, informality is not exclusively a firm-level phenomenon. Formal firms also hire many informal employees. In fact, informal workers account for 25% of all employees at formal firms.

At the household level, when one member receives a formal job, spouses change labor supply decisions in a manner consistent with a positive income effect that increases reservation wages, specially for formal jobs. Non-employed spouses of informal workers who transition to formality because of an inspection are more likely to remain non-employed. Conditional on a new job, they are less likely to be formal but receive higher starting wages than spouses of informal non-inspected workers.

The observed labor supply responses are indicative of a high value of the shared benefits of a formal job for the median household. Households in the lowest income bracket have a negative net value for formal jobs. The higher relative cost of registering a low income worker, due to the regressive payroll tax system, and the availability of lower cost substitutes, like Seguro Popular, make formal jobs less attractive for a segment of the population.

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# Tables

Table 1: Joint Labor Market Status and Conditional Wages by Formality Status

| Panel A: Joint Labor Market Status (% of Households) | | | | | |
|---|---|---|---|---|---|
| | | Spouse's employment status | | | |
| | | Formal | Informal | Not employed | Total |
| Head of Household | Formal | 9.29 | 7.15 | 33.38 | 49.8 |
| | Informal | 2.89 | 8.34 | 31.62 | 42.8 |
| | Not Employed | 0.81 | 1.37 | 5.15 | 7.3 |
| | Total | 12.99 | 16.86 | 70.15 | |
| Panel B: Head of Household's Wage Conditional on Own and Spouse's Formality Status | | | | | |
| | | MXN Pesos | | | |
| | Mean wage in formal jobs | 28,433.05 | 21,370.22 | 24,068.92 | 24,495.79 |
| | | (28,426.26) | (21,363.94) | (24,062.45) | (19,120.16) |
| | Mean wage in informal jobs | 20,654.60 | 15,682.04 | 15,033.46 | 15,538.54 |
| | | (20,649.31) | (15,676.95) | (15,028.78) | (11,626.40) |

Notes: The sample includes households that a) remain in the survey for at least 4 (out of 5) waves ($\sim$70% of households) assigned to "extended questionnaire" on 4th interview, b) are headed by a heterosexual couple between 25 and 59 years old, c) where neither spouse had self-employment spells or owned a business, received welfare benefits, was enrolled in school, retired or disabled. Sample size: 29,284 households.

Table 2: Share of Workers by LMS on 4th Interview Conditional on Status in First Interview

| | | Period 4 Labor Market Status | | |
|---|---|---|---|---|
| | Period 1 LMS | Formal | Informal | Not Employed |
| Head of Household | Not Employed | 22.97 | 62.13 | 14.9 |
| | Informal | 11.44 | 80.67 | 7.89 |
| | Formal | 88.13 | 9.46 | 2.41 |
| HH's Partner | Not Employed | 2.03 | 10.3 | 87.67 |
| | Informal | 5.11 | 53.42 | 41.47 |
| | Formal | 84.36 | 4.69 | 10.95 |

Notes: The sample includes households that a) remain in the survey for at least 4 (out of 5) waves ($\sim$70% of households) assigned to "extended questionnaire" on 4th interview, b) are headed by a heterosexual couple between 25 and 59 years old, c) where neither spouse had self-employment spells or owned a business, received welfare benefits, was enrolled in school, retired or disabled. Sample size: 29,284 households.

Table 3: Women Non-employment Outflow Rates Conditional on Male Partner's Trajectory

| | | % Women from Non-employment to: | | |
|---|---|---|---|---|
| | | Formal | Informal | NE |
| Men's Transitions | INF-NE | 1.49 | 11.67 | 86.84 |
| | INF-INF | 3.20 | 10.59 | 86.21 |
| | INF-F | 0.73 | 11.56 | 87.71 |
| | F-NE | 6.99 | 6.24 | 86.76 |
| | F-INF | 2.64 | 12.75 | 84.61 |
| | F-F | 2.95 | 8.37 | 88.68 |

Notes: The sample includes households that a) remain in the survey for at least 4 (out of 5) waves ($\sim$70% of households) assigned to "extended questionnaire" on 4th interview, b) are headed by a heterosexual couple between 25 and 59 years old, c) where neither spouse had self-employment spells or owned a business, received welfare benefits, was enrolled in school, retired or disabled. We focus on households were women are not-employed on the first interview and an employed male head of household. Sample size: 19,034 households.

Table 4: Spousal Labor Supply Response to Inspection at Spouse's Establishment

(Initially Non-Employed)

| | Daily Wage Quintile (Avg. in USD) | | | | |
|---|---|---|---|---|---|
| | All HH | 1st ($4) | 2nd ($8) | 3rd ($13) | Top 40% ($21) |
| Formal Job | -0.072 | -0.066 | -0.095 | -0.101 | -0.048 |
| | (0.008) | (0.012) | (0.015) | (0.028) | 0.016) |
| Informal Job | -0.010 | 0.005 | 0.008 | -0.020 | -0.016 |
| | (0.010) | (0.013) | (0.014) | (0.013) | (0.019) |
| No. of HH | 24,892 | 4,975 | 4,982 | 5,003 | 9,932 |

Notes: This table shows the effect transitions from informal to formal employment by spouse 1 on transition probabilities for spouse 2 (i.e. the spouse who was not directly affected by an inspection). The baseline sample is non-employed individuals with an informal spouse employed at an establishment in the DNE. The treatment group are individuals whose spouse was informally employed at an establishment at the time of inspection.

Table 5: Spousal Labor Market Status After Partner's Inspection - Initially Non-employed Spouses

| | Low Education | | High Education | |
|---|---|---|---|---|
| | No Children | W/children | No Children | W/children |
| Formal Job | -0.019*** | 0.002 | 0.036*** | 0.019*** |
| | (0.007) | (0.006) | (0.011) | (0.007) |
| Informal Job | -0.003 | -0.022 | -0.024 | -0.016 |
| | (0.021) | (0.023) | (0.022) | (0.016) |
| Non-Employed | 0.016 | 0.020 | -0.012 | -0.003 |
| | (0.022) | (0.024) | (0.023) | (0.017) |
| No. of HH | 6,670 | 4,450 | 5,852 | 7,920 |

Notes: This table shows the effect transitions from informal to formal employment by spouse 1 on transition probabilities for spouse 2 (i.e. the spouse who was not directly affected by an inspection). The baseline sample is non-employed individuals with an informal spouse employed at an establishment in the DNE. The treatment group are individuals whose spouse was informally employed at an establishment at the time of inspection.

Table 6: Spousal Wage Changes - Initially Employed Spouses

| | **Next Formality Status** | | | |
|---|---|---|---|---|
| | Low Education | | High Education | |
| | Informal Job | Formal Job | Informal Job | Formal Job |
| **Initial Status** | | | | |
| Informal Job | 0.160* | -0.009 | -0.246 | -0.227 |
| | (0.085) | (0.071) | (0.331) | (0.222) |
| Mean Wages | 7.570 | 8.437 | 7.732 | 8.554 |
| No. Obs | 1,907 | 2,308 | 181 | 230 |
| Formal Job | 0.815 | 0.064 | 0.125 | 0.091** |
| | (0.738) | (0.050) | (0.168) | (0.036) |
| Mean Wage | 7.647 | 8.397 | 8.340 | 8.682 |
| No. Obs. | 250 | 900 | 131 | 4,131 |

Table 7: Net Value of a Formal Job

| Average Daily Wage | $\frac{\delta LFP}{\delta F_h}$ | Formal LFP Income Elasticity | Value-Cost Ratio |
|---|---|---|---|
| Top 40% ($21) | 0.048 | [0.05 - 0.20] | [4.34 - 1.09] |
| 41st to 60th ($13) | 0.101 | [0.10 - 0.40] | [4.20 - 1.05] |
| 21st to 40th ($8) | 0.095 | [0.15 - 0.60] | [2.28 - 0.57] |
| 1st to 20th ($4) | 0.066 | [0.20 - 0.80] | [0.84 - 0.21] |

Notes: Low and high responsiveness formal labor supply extensive margin income elasticities from Abramovsky and Phillips (2015). Cost of a formal job is calculated as the sum of social security contributions and payroll taxes required from employers and employees for an employee registered with the corresponding average daily wage as indicated in the Social Security Law.

Table 8: Model Parameters

| Parameters | HoH | Spouse |
|---|---|---|
| Estimated | | |
| $\lambda_E^F$ | 0.5059 | 0.3263 |
| $\lambda_N^F$ | 0.6333 | 0.3427 |
| $\lambda_E^{INF}$ | 0.8748 | 0.0505 |
| $\lambda_N^{INF}$ | 0.8823 | 0.0667 |
| $\mu_F$ | 10.5917 | 10.5271 |
| $\mu_{INF}$ | 10.5856 | 10.174 |
| $\sigma_F$ | 0.032 | 0.0137 |
| $\sigma_{INF}$ | 0.0145 | 3.8601 |
| $\xi$ | 0.0498 | 0.0013 |
| $b$ | 0.0006 | 0.0008 |
| $\delta$ | 0.55 | |
| Calibrated | | |
| $\eta^F$ | 0.0185 | 0.0769 |
| $\eta^{INF}$ | 0.0609 | 0.2784 |
| $\rho$ | 0.02 | |

Table 9: Targeted Moments

| | | Model | Data |
|---|---|---|---|
| spouse 1 | formally employed (%) | 0.57 | 0.50 |
| | informally employed (%) | 0.29 | 0.43 |
| | mean formal wage | 10.014 | 10.106 |
| | sd formal wage | 9.92 | 9.858 |
| | mean informal wage | 9.446 | 9.651 |
| | sd informal wage | 9.828 | 9.361 |
| | NE-F | 0 | 0.03 |
| | NE-INF | 0.018 | 0.04 |
| | F-INF | 0.004 | 0.05 |
| | F-NE | 0.012 | 0.03 |
| | INF-F | 0.006 | 0.05 |
| | INF-NE | 0.027 | 0.04 |
| spouse 2 | formally employed (%) | 0.56 | 0.13 |
| | informally employed (%) | 0.07 | 0.17 |
| | mean formal wage | 9.945 | 10.001 |
| | sd formal wage | 9.834 | 9.7 |
| | mean informal wage | 7.476 | 8.103 |
| | sd informal wage | 9.027 | 9.163 |
| | NE-F | 0.04 | 0.02 |
| | NE-INF | 0.03 | 0.07 |
| | F-INF | 0.02 | 0.01 |
| | F-NE | 0.06 | 0.02 |
| | INF-F | 0.006 | 0.01 |
| | INF-NE | 0.04 | 0.07 |
| transition states | spouse 2 (NE-F) and spouse 1 (Inspected, INF) - spouse 2 (NE-F) and spouse 1 (Not Inspected, INF) | -0.023 | -0.07 |
| | spouse 2 (NE-INF) and spouse 1 (Inspected, INF) - spouse 2 (NE-F) and spouse 1 (Not Inspected, INF) | -0.008 | -0.01 |
| joint initial states | spouse 1 is formal and spouse 2 is employed | 0.38 | 0.16 |
| | spouse 1 is informal and spouse 2 employed | 0.16 | 0.11 |
| | spouse 1 is not employed and spouse 2 is employed | 0.09 | 0.02 |

Table 10: Decomposing the Value of a Formal Job

| WMP (% of median wage) | | |
|---|---|---|
| | Male | Female |
| baseline | 14% | 29% |
| $\xi = 0$ | 14% | 29% |
| separation rate | 0.04% | 2% |
| job finding rate | 7% | 26% |
| job-to-job transition rate | 3% | 31% |
| wage (mean) | 13.3% | 29% |
| wage (sd) | 4% | 21% |
| no risk aversion | 2% | 29% |

Table 11: Wage Change After Formality Transition - Model vs. Data

| | Below Median Partner Income | | Above Median Partner Income | |
|---|---|---|---|---|
| | Data | Model | Data | Model |
| Informal - Formal | -0.009 | -0.002 | -0.227 | -0.004 |
| Formal - Informal | 0.815 | 0.005 | 0.091 | 0.006 |

# Figures

Figure 1: Change in the Probability of Holding a Formal Job after an Inspection

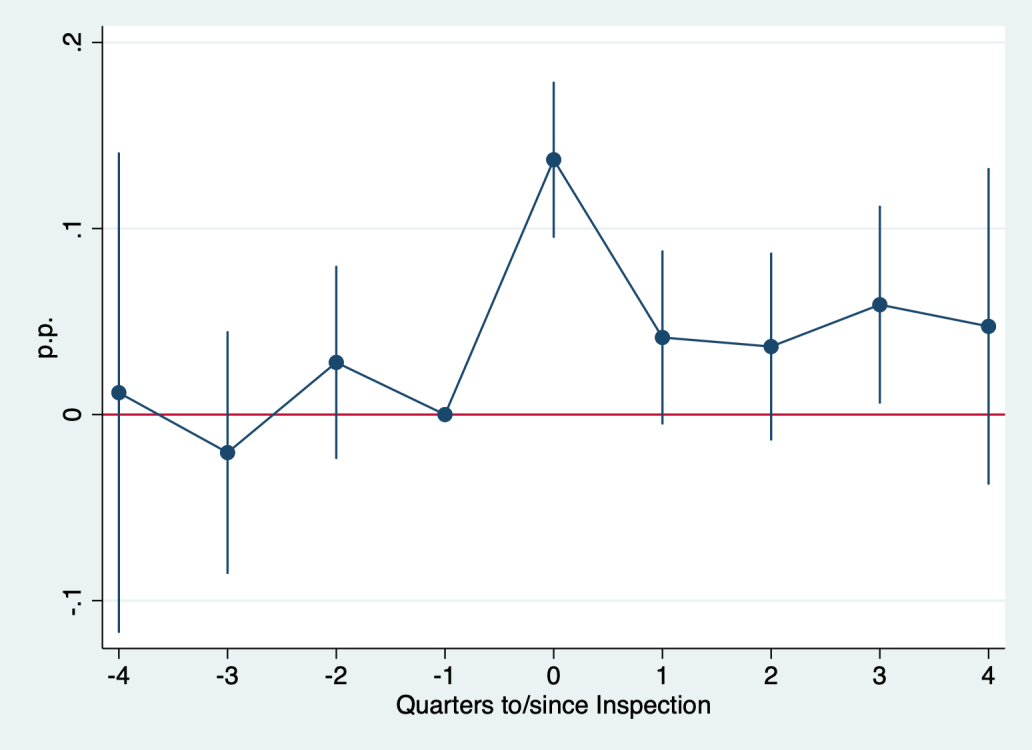


Figure 2: Reservations Wages for Initially Non-Employed Spouses by Head of Household's Wage and Formality Status

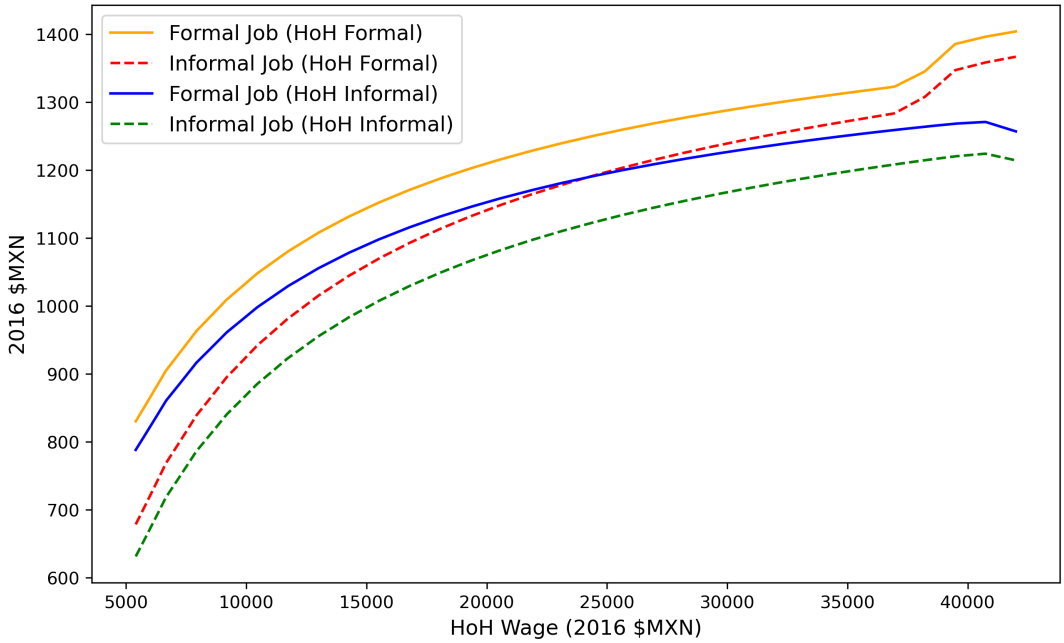



Figure 3: Value of a Formal Job by Wage Paid - WTP Compensating Variation

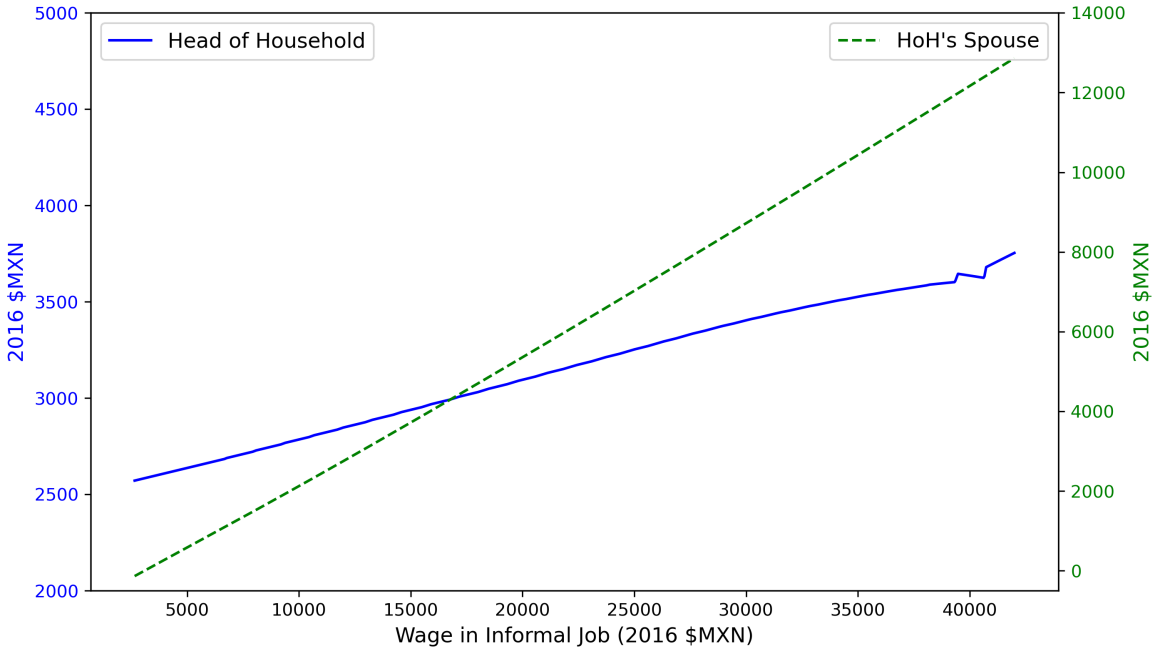
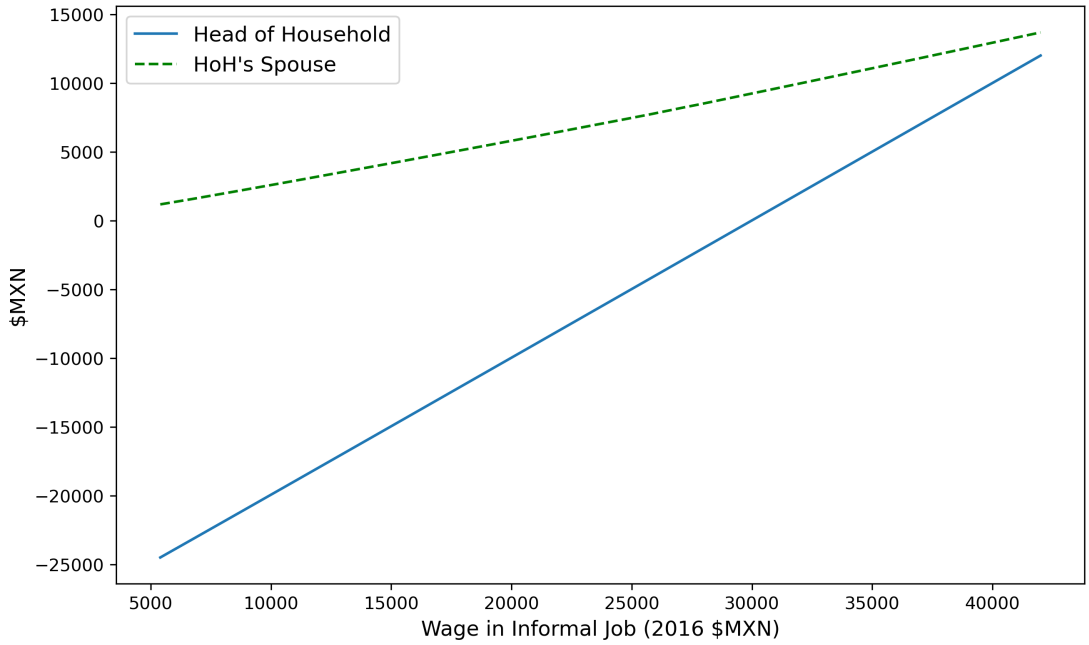


Figure 4: Value of a Second Formal Job by Wage Paid - WTP Compensating Variation



# APPENDIX: FOR ONLINE PUBLICATION ONLY

# A Appendices

## A.1 Dataset Matching and Description

The National Employment and Occupation Survey (ENOE) includes comprehensive data for 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.

Constructing the employer-employee-household dataset requires matching the firm's name (provided by the worker in the household survey) and industry code (determined by INEGI based on responses from ENOE) with the firm's name and industry code included in the Ministry of Labor's (STPS) National Firm Directory (DNE). Due to spelling mistakes and incomplete name reporting by the workers surveyed in ENOE, the name provided by the worker is not always an exact match to the official name registered by the firm with STPS.

To match ENOE with STPS list of firms we use the inexact character variables matching algorithm proposed by Rai (2012).Rai (2012) This algorithm calculates the number of letters that match in the two variables of interest from right-to-left and left-to-right, then it divides the sum of these by the total number of letters in the two strings. This ratio gives a measure of the proportion of common letters between the two strings. According to Rai's (2012)Rai (2012) results, "when the proportion of common letters is more than 0.80 the two names, almost certainly, belong to the same company". We use a more conservative threshold of 0.85 to determine a positive match. Using a fuzzy match algorithm such as a generalization of the Levenshtein edit distance produces more false-positives than this method.

ENOE's questionnaire includes the households' exact address but due to confidentiality concerns we currently only have access to the "Basic Geostatistical Area" (AGEB)[35] where the household dwelling is located. Since workers are not asked about the location of the place of work, for multi-establishments firms, we match workers to the establishment located in the same AGEB where they live. In the cases where there is more than one establishment with the same name and industry code in the same AGEB, We randomly assign workers to each of the establishments. There are also cases where we find more than one match in establishment's name and industry code but none of the establishments are in the same AGEB as the worker's home address. We consider these matches only if the worker and the establishment are in the same municipality, even if not in the same AGEB. If this still leaves more than one match, We randomly assign workers to one of the establishments with the same firm name, industry code and municipality as the worker's home address.[36]

[35] An AGEB is a subdivision for municipalities in Mexico. AGEB's contain between 1 to 50 blocks of a municipality.

[36] The random assignment of workers to establishments when there is more than one match (either within the same AGEB or the same municipality) could lead to incorrectly assigning a worker to the control or treatment

On average we match 38,610 workers per quarter (out of an average of 115,993 employees per quarter in the sample). Table A.1 shows the results of the matched inspected and non-inspected workers by formality status.

Table A.1: 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 my results from matching ENOE's workers with the National Directory of Firms (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.[37]

Table A.2 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 equally distributed across establishments in different industries.

groups. This would only bias down my estimate of the effects of inspections at the firm and household levels.

[37] See *Como se hace la ENOE*Instituto Nacional de Geografía y Estadística (2015*a*) and *Encuesta Nacional de Ocupacion y Empleo. Reconstruccion de Variables*Instituto Nacional de Geografía y Estadística (2015*b*) for further information on the methodology used by INEGI to identify formal firms.

Table A.2: Work-sites in ENOE and DNE by Industry

| NAICS Sector | % of work-sites in ENOE | % of work-sites 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% |

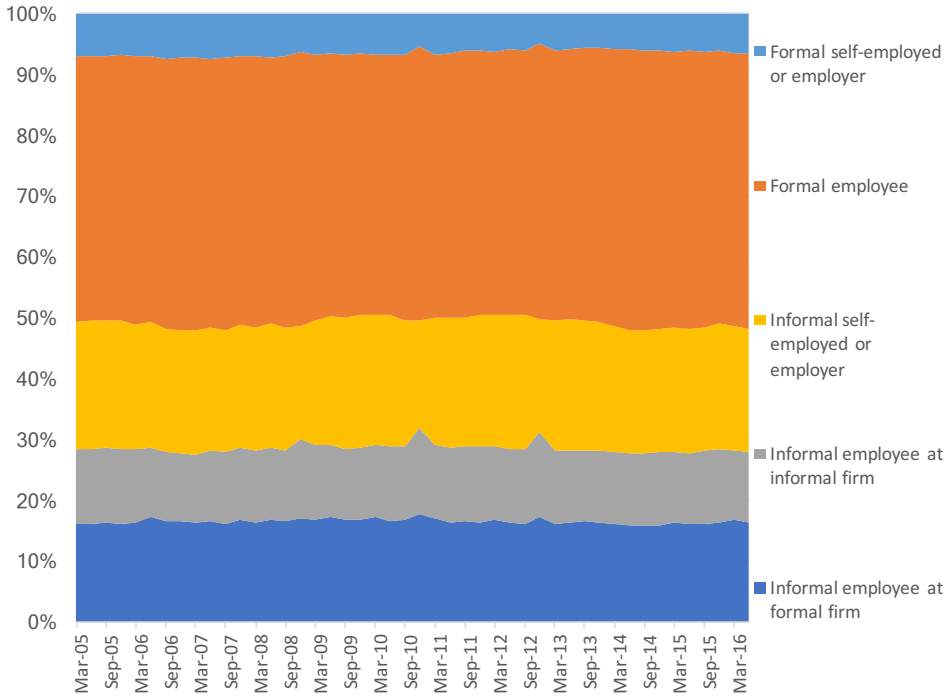
Notes for Table A.2 *(Continued from previous page)*: This table shows the distribution of **establishments** by sector using 2-digit NAICS. The second column shows the distribution in ENOE. It is important to emphasize that this is not the distribution of workers, even though this is a household survey, 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.

Table A.3: IMSS Employers Matched to DNE Firm by Industry

Notes for

Figure A.1 shows the shares of workers by each job category being for every quarter from 2005 to 2016. On average, during the past decade, half of all workers were informally employed. A third of these workers was employed by a registered, or formal, firm.

Figure A.1: Employed Population by Job and Firm Type (2005-2016)




Source: Own calculations based on data from the National Employment and Occupation Survey (ENOE)Instituto Nacional de Geografía y Estadística (n.d.) and the National Directory of Firms (DNE)Secretaría del Trabajo y Previsión Social, Dirección General de Inspección Federal del Trabajo. (2013) 2005-2016.

Table A.4: Predicted 4-Qtr. Transition Probabilities

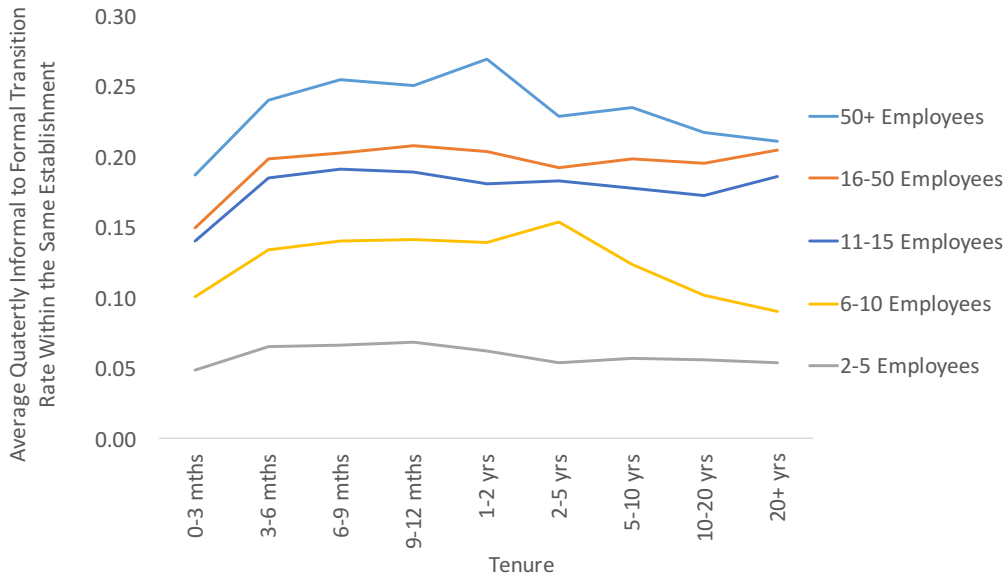
| Labor Market Status Next Quarter | | Initial Labor Market Status | | |
|---|---|---|---|---|
| | | Informal at Formal Establishment | Formal at Formal Establishment | Unemployed |
| Same Formal Firm | Informal | 20.0% | 2.1% | - |
| | Formal | 17.3% | 50.5% | |
| New Formal Firm | Informal | 4.5% | 2.4% | 10.2% |
| | Formal | 0.6% | 10.2% | 24.5% |
| New Informal Firm | | 9.2% | 2.2% | 8.1% |
| Self-Employed or Employer | Informal | 10.0% | 3.0% | 9.8% |
| | Formal | 4.5% | 3.5% | 2.7% |
| Unemployed | | 3.7% | 2.6% | 7.5% |
| OLF | | 31.6% | 23.4% | 37.2% |
| Total | | 100% | 100% | 100% |

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

Table A.4 documents annual transition rates between formal and informal jobs within and across establish-

ments. Although during the first year of employment the average quarterly probability of transitioning from informal to formal status without changing jobs increases, afterwards, this probability stabilizes and even decreases with tenure. Informal workers are likely to remain informally employed a year later, either at the same establishment or with a different employer.

Figure A.2: Average Quarterly Transition Rate from Informal to Formal Job within the Same Establishment by Tenure and Establishment Size




Source: Own calculations using data from the 2005-2015 National Employment and Occupation Surveys (ENOE) and the National Directory of Firms (DNE). Workers' are classified as formal or informal based on their self-reported access to social security services. Tenure is self-reported. Excludes individuals employed in agriculture or as domestic employees.

## A.2 Defining and Measuring Informality

We follow the latest resolution adopted during the 17th International Conference of Labor StatisticiansInternational Labour Organization (2003) which sets international standards for measuring informality. According to this Resolution, informality has two dimensions. The first dimension refers to the characteristics of the economic unit where an individual is employed: a business is categorized as informal when it produces goods and/or services for the market using household resources and lacks basic accounting registries or is not registered with the corresponding 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, regardless of whether the firm or business where the individual is employed is formally registered with the corresponding authorities or not. Using this definition of informality, an employee can have either a formal or an informal job at a formal firm. At an informal firm, all jobs are informal.

Previous literature 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.

There are several problems with this strategy. First, since data is gather at the household level it is unclear whether firm size is accurately represented. Second, Hsieh and Olken (2014)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.

Exploiting new data from STPS and ENOE, we depart from previous literature to identify formal firms. If a firm is included in the DNE then it is a formal firm. It is important to point out that not all formal firms are included in the DNE. We can further identify formal firms using formality status of co-workers or firm and industry categories as previous literature has done. However, since only firms in the DNE are subject to STPS's random inspections, we restrict my analysis to this subset of formal firms.

## A.3 Discussion on Misclassification Error

A possible source of concern with the use of self-reported individual level data to estimate job offer arrival and separation rates in the labor market is that individuals are incorrectly classified in sectors or job types to which they do not belong in one period but not in the next. These misclassification errors lead to an overestimation of labor market mobility. As pointed out by Poterba and Summers (1986)Poterba and Summers (1986), while these errors may cancel in aggregate variables, the estimated flow rates between labor market states may instead be very sensitive to these spurious transitions.

In the case of estimating transitions across the formal and informal sector, this concern might be heightened by the fact that individuals may be more or less prone to lie about whether or not their employment status abides to regulation at different points in time. Therefore, misclassification errors need not be independent across time or throughout the business cycle. Moreover, it can be reasonably argued that worker's propensity to lie about their formal/informal status is dependent on the current authority's policy towards informality which can lead to biased estimates for analysis on fiscal policy enforcement.

Previous literature focusing on gross flows data to analyze labor market dynamics has long since recognized the possibility of these types of response errors and suggested approaches to estimate their incidence and correct for their consequences. Poterba and Summers (1985)Poterba and Summers (1986) use data from a quality check re-interview, in which a small subsample of the individuals originally surveyed are contacted about one week after their initial interview and asked again about their labor market status. Taking the second answer to be an individual's true labor market state, Poterba and Summers calculate the probability of an individual being incorrectly classified in the initial survey when in fact his status hadn't changed. Using this methodology, the authors find that spurious transitions are very large relative to actual observed month to month transition rates reported in the US Current Population Survey (CPS). These results point to the importance of correcting for possible misclassification errors in longitudinal surveys such as ENOE.

Besides offering an approach to estimate the magnitude of classification error in longitudinal surveys, Poterba and Summers also suggest using the estimated error probabilities to correct the observed transition flows in the raw data. Their suggested correction methodology requires making particular assumptions regarding the stochastic process that underlies the appearance of classification errors. In particular, they assume that the probability of misclassification errors is serially uncorrelated, that is, that at any given point in time, the probability an individual is misclassified is the same. Their results suggest that spurious reporting of labor force withdrawal is the most common form of reporting error, with exit rates from the labor force being only one-third the size suggested by the CPS raw data. Further, they argue that under the corrected flows transitions from employment to unemployment are twice as common as movements from employment to out of the labor force.

In the case of ENOE, a re-interview with a smaller batter of questions is conducted in 10% of the households in the original sample after every surveying period. Unfortunately, although the purpose of this second interview is to assess ENOE's database quality, the National Statistics and Geography Institute hasn't made public the corresponding dataset with these re-interviews responses nor any estimates of the magnitude of classification errors. Therefore, the data necessary to estimate error probabilities in the raw data using the Poterba and Summers approach is unavailable.

Given the lack of access to the re-interviews micro-data we propose an alternative, albeit perhaps conservative, method to assess whether misclassification error of individuals are leading to an upward bias in the transitions between the formal and informal sectors for both employees and self-employed individuals. In the case of self-employed individuals, we estimate the incidence of movements back and forth between the formal and informal sectors and consider instances when an individual changes sectors more than once within a three quarter range to be a misclassification error, with the true labor market state being determined by the sector in which the individual spent more time on according to the unadjusted data. For individuals who work as employees, we consider as a misclassification error those cases when a back-and-forth transition across sectors

is observed but the corresponding employment duration is at odds with such a change. That is, if an employed individual reports being in the formal sector in a quarter but not in the next, while his reported duration in the current job is two quarters or more, then his duration and labor market sector responses are inconsistent.

It is important to note that given these assumptions, only a very specific type of misclassification error is accounted for. Only transitions between formal and informal labor market status with inconsistent employment duration within a same type of job position are considered. That is, the only misclassifications considered are those that cause false transitions, as defined above, from formality to informality, or vice-versa, within either the self-employed or employees (but not across these two job positions).

Table A.5: Quarterly Potential Number of Misclassification Cases

| Within-Firm Sequential Transition | Average | Minimum | Maximum |
|---|---|---|---|
| Employee Informal-Formal-Informal | 145,562 | 120,941 | 164,419 |
| Employee Formal-Informal-Formal | 113,962 | 97,672 | 139,768 |
| Self-Employed Informal-Formal-Informal | 123,796 | 97,964 | 154,112 |
| Self-Employed Formal-Informal-Formal | 88,514 | 74,347 | 105,453 |

Table A.6: Quarterly Potential Misclassification Probability

| Within-Firm Sequential Transition | Average | Minimum | Maximum |
|---|---|---|---|
| Employee Informal-Formal-Informal | 1.9% | 1.4% | 2.3% |
| Employee Formal-Informal-Formal | 2.3% | 2.0% | 2.8% |
| Self-Employed Informal-Formal-Informal | 2.2% | 1.4% | 2.6% |
| Self-Employed Formal-Informal-Formal | 3.1% | 2.4% | 3.4% |

Despite the narrowly defined misclassification categories, we find that in any given quarter a relevant number of both employed and self-employed individuals are incorrectly classified as having transitioned across the formal and informal sectors. While the probabilities of these errors vary across quarters, formal employees have a 2.3% probability of being classified as transitioning to informality and back to formal employment while the misclassification probability for an informal employee is 1.9%. Meanwhile, the probability that a formal self-employed individual is classified as changing towards the informal sector and back to formality is 3.1% while the probability that a case of informal self-employment is falsely classified as moving towards formality and back to informality is 2.2%. This falsely induced transitions lead to overstating market mobility across the formal and informal sectors.

Throughout this document, we consider sequential within firm and within position (i.e. self-employment or employees) transitions across sectors to be misclassifications. Table A.6 points to the importance of developing a finer procedure to identify and measure bias generated when calculating labor market flows, not only across the formal and informal sectors, but also for other labor market states and across job positions.

## A.4 Random Selection of Inspected Work-Sites

Let $IF_{i,j,t}$ be a dummy variable equal to 1 if individual $i$ was informally employed at work-site $j$ in quarter $t$ and became formally employed without changing work-sites in period $t+1$ and zero otherwise. Let $IU_{i,j,t}$ be equal to 1 if $i$ was informally employed at $j$ in period $t$ and became unemployed in period $t+1$, and 0 otherwise. Finally, let $Z_{i,t}$ indicate whether individual $i$ was employed at a work-site that received an inspection within the past 3 quarters $[t-3,t]$ and zero otherwise. Finally, let $X_{i,j,t}$ be a vector of worker and work-site characteristics.

Columns (1), (2) and (3) in Table A.7 present the estimates from the following linear probability models:

$$\begin{aligned} E\left(IF_{i,j,t}|X_{i,j,t},t\right) &= \phi\left(\alpha_0+\alpha_1 X_{i,j,t}+t+\varepsilon_{i,j,t}\right) \\ E\left(IU_{i,j,t}|X_{i,j,t},t\right) &= \phi\left(\beta_0+\beta_1 X_{i,j,t}+t+\eta_{i,j,t}\right) \\ E\left(Z_{i,t}|X_{i,j,t},t\right) &= \phi\left(\gamma_0+\gamma_1 X_{i,j,t}+t+\zeta_{i,j,t}\right) \end{aligned} \tag{8}$$

Column (1) uses a linear probability model to test whether worker and firm characteristics are predictive quarterly transitions from informal to formal jobs without changing work-sites[38]. As expected, demographic and skill-related characteristics are highly predictive of whether an informal worker will transition to a formal job at his current place of employment. Workers with higher education are more likely to transition to a formal job. Establishment size is also significantly and positively correlated with this transition probability.

Column (2) shows that tenure, age, and education are negatively correlated with the probability of separating from an informal job into unemployment. Column (3) assesses whether the probability of being subject to an inspection is correlated with firm or worker variables. Jointly, workers and establishments' characteristics explain less than 0.1 percent of the variation in inspection probability across establishments within the DNE (joint p-value of 0.595). Therefore, we conclude that STPS Inspection and Sanction System generates a distribution of inspections that is consistent with random selection across establishments included in the DNE.

[38]We refer to within-work-site or within-firm transitions in formality status as the cases in which a worker experiences a change in access to government mandated benefits without reporting a change in employer's name.

Table A.7: Testing for Random Probability of Inspections

| | (1) Informal to Formal Job ($IF_{i,j,t}$) | | (2) Informal to Unemployed ($IU_{i,j,t}$) | | (3) Probability of Inspection ($Z_{i,j,t}$) | |
|---|---|---|---|---|---|---|
| | Coeff. | s.e. | Coeff. | s.e. | Coeff. | s.e. |
| Worker's Age | 0.002 | (0.002) | -.005** | (0.002) | -0.001** | (0.001) |
| Gender | 0.068* | (0.041) | -0.229*** | (0.057) | 0.005 | (0.013) |
| Tenure (months) | 0.008** | (0.003) | -0.014*** | (0.002) | 0.001 | (0.001) |
| Education | | | | | | |
| Less than HS | 0.261* | (0.149) | 0.287 | (0.241) | 0.068 | (0.066) |
| Completed HS | 0.522*** | (0.154) | 0.559** | (0.247) | 0.043 | (0.067) |
| Some College | 0.619*** | (0.167) | 0.650** | (0.264) | 0.075 | (0.069) |
| College + | 0.642*** | (0.156) | 0.554** | (0.250) | 0.074 | (0.068) |
| Children in daycare | 0.050*** | (0.015) | -0.784*** | (0.270) | 0.000 | (0.005) |
| Household size | 0.018 | (0.017) | -0.008 | (0.014) | 0.003 | (0.006) |
| Work-site size | | | | | | |
| 6-10 | 0.372*** | (0.015) | 0.259*** | (0.087) | -0.046 | (0.025) |
| 11-15 | 0.516*** | (0.073) | 0.554*** | (0.113) | 0.014 | (0.029 |
| 16-50 | 0.727*** | (0.055) | 0.801*** | (0.084) | 0.002 | (0.021) |
| 51+ | 0.892*** | (0.052) | 1.354*** | (0.075) | -0.003 | (0.019) |
| Industry FE | Yes | | Yes | | Yes | |
| LR | | 1,194.36 | | 649.23 | | 30.44 |
| p-value | | 0.000 | | 0.000 | | 0.595 |
| No. of Observations: 406,003 | | | | | | |

Notes: This table displays the results from three linear probability models. The dependent variables in columns 1, 2 and 3 are, respectively, $IF_{i,j,t}$, a dummy equal to 1 if individual $i$ was informally employed at work-site $j$ in quarter $t$ and transitioned to being formally employed at the same work-site in $t+1$ and 0 otherwise; $IU_{i,j,t}$ a dummy equal to 1 if individual $i$ was informally employed at work-site $j$ in quarter $t$ and became unemployed between quarters $t$ and $t+1$ and 0 otherwise; and $Z_{i,t}$ a dummy equal to 1 if individual $i$ was informally employed at an inspected work-site within period $t-4$ and $t$, and 0 if the work-site is included in the DNE but was not inspected within worker's $i$ observed employment period at that work-site. The baseline estimation sample includes individuals who are informally employed at a work-site included in the DNE between January 2005 and December 2015. Workers and work-sites' characteristics are measured before transition or inspection. Tenure is measured in months employed at the current work-site. Children in day-care is the number of children within the ages of 0 to 4 in the household. Gender is equal to 1 for men and 0 for women. Work-site size is the number of employees as reported by the worker. Errors are clustered at the work-site level.

## A.5 Unconditional Effect of Inspections on Spousal Labor Supply

Columns (1), (2) and (3) in Table A.8 show the effects of inspections on spouse's 2 probability of non-employment, starting an informal job, and starting a formal job conditional on being non-employed at the time spouse's 1 establishment is inspected, without controlling for spouse 1 labor market status after the inspection. These effects are estimated from equation 9.

$$\begin{aligned}&\mathbf{E}\left(S^k_{2,h,t+1}|S^{NE}_{2,h,t}=1\ \&\ S^{INF}_{1,h,t}=1\right)= \\ &\phi\left(\beta^k_1 Post_{1,t}+\beta^k_2 Post_{1,t}\times Inspected_1+\beta^k_3+Inspected_1+X'_{2,t}\beta^k_4+t+\eta_{2,h,t}\right) \qquad (9)\\ &\forall k\in[F,INF,NE]\end{aligned}$$

Table A.8: Inspection Effects on Transition Probabilities for Non-Employed Spouses of Informal Inspected Workers

| | New Labor Market Status | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Non-employment | New Informal Job | New Formal Job |
| Dependent Mean Transition Probability | 68.9% | 18.3% | 12.7% |
| PostXInspection | | | |
| Odds Ratio | 1.005* | 1.085*** | 0.901* |
| s.e. | (0.003) | (0.022) | (0.057) |
| No. of Observations | 69,765 | | |

Notes: This table shows the effect of inspections on transition probabilities for spouse 2 (i.e. the spouse who was not directly affected by an inspection). The baseline sample is non-employed individuals with an informal spouse employed at an establishment in the DNE. The treatment group are individuals whose spouse was informally employed at an establishment at the time of inspection. All regressions include individual and time fixed effects. The dependent mean transition probability is calculated as the average probability of each transition out of non-employment with individual characteristics fixed at their mean values. The displayed odds ratios are the exponential of the $\beta^k_2$ coefficients shown in equation 9.

The first row in Table A.8 shows the average probability of staying non-employed, transitioning to informal employment and transitioning to formal employment for non-employed spouses of informal workers. The second row shows the estimated effects of spouse 1 receiving an inspection on each of these transition probabilities. Spouses of inspected individuals are more likely to start an informal job and less likely to start a formal one, conditional on non-employment, compared to spouses of non-inspected workers. After an inspection, the quarterly transition rate from non-employment to a formal job decreases by 9.9% (from 8.7% to 7.7%). Transitions into an informal job instead increase by 8.5%. The probability of remaining non-employed increases slightly by 0.5%. These results are consistent with flows out of informal employment by spouse 1 affecting the type of jobs that spouse 2 accepts, but not significantly changing the overall probability of working.

# B Additional Institutional Detail

Table B.9: Contributions to Government Mandated Benefits (per worker/day)

| | Employer Contribution | Worker Contribution | Government Contribution | Total |
|---|---|---|---|---|
| Fixed Fee | 20.4% × MW | 0% | 13.9%× MW | 34.3% × MW |
| Proportional to wage | | | | |
| If wage< 3 MW | 15.15% | 2.375% | 0.475% | 18% |
| Added % on wage>= 3 MW | 1.10% | 0.40% | 0% | 1.50% |
| Upper bound | 16.4%× 25MW | 2.272% × 25MW | | |

Source: Own calculations based on payroll tax rates established in the Social Security LawCámara de Diputados del H. Congreso de la Unión (November, 2015*a*). MW refers to the daily minimum wage, equal to MXN$73.04 since January 2016.

Table B.10: IMSS Fines Related to Informal Employment (2016)

| Violation | Fine (Daily MW) |
|---|---|
| Not registering workers | 20 - 350 (per worker) |
| Not notifying changes in wages | 20 - 150 |
| Late payment of social security fees | 20 - 75 |
| Providing false information | 20 - 250 |

Source: Social Security Law 2015Cámara de Diputados del H. Congreso de la Unión (November, 2015*a*).

Table B.11: Distribution of STPS's Inspections by Result (2005-2016)

| Result | | | No. of Inspections | % of All Inspections |
|---|---|---|---|---|
| Violation Detected | Closed without report of violations | | 266,517 | 43% |
| | Provided proof of compliance | | 296,367 | 48% |
| | Granted request for more time to comply | | 184 | 0% |
| | Fine process started | Fine imposed | 23,154 | 4% |
| | | Fine not yet imposed | 34,620 | 6% |

Notes: Excludes violations of labor regulations beyond STPS's direct jurisdiction, including, informal employment.
Source: Own calculations using STPS Inspections logs January 2005 to June 2016. Information request no. 0001400017316 National Institute for Transparency, Access to Information and Personal Data Protection (INAI)National Institute for Transparency, Access to Information and Personal Data Protection (2016).

The statistics shows in Table B.11 do not include instances where inspectors detected informal employment

and sent the corresponding notice to IMSS. 42% of inspections were closed without further action by STPS.[39] In 48% of all inspections, employers were able to provide proof of having corrected the irregularities detected after the inspection.

# C Inspections' Effects on Employed Spouses

Section **??** in the main text showed the effect of transitions out of informal employment in households where one spouse is not employed. This section analyzes household labor supply decisions when both spouses are employed at the time of inspection for each of the following cases:

(1) Both spouses are informal at the time of inspection

(2) Inspected spouse is informal and non-inspected spouse is formal

(3) Both spouses are formal

(4) Inspected spouse is formal and non-inspected spouse is informal

Columns (1), (2) and (3) in Table C.12 show the effects of inspections on spouse's 2 probability of non-employment, starting an informal job, and starting a formal job conditional on both spouses being employed at the time of inspection for each of the cases outlined above.

Table C.12: Inspection Effects on Transition Probabilities for Employed Spouses of Inspected Informal Workers

| **Panel A: Both Spouses are Informal at the Time of Inspection** | | | | |
|---|---|---|---|---|
| | Spouse's 2 (Not Inspected) New Labor Market Status | | | |
| | Unemployment or OLF | New Informal Job | New Formal Job | Same Job (Stayers) |
| Dependent Mean Transition Probability | 36.8% | 13.2% | 8.6% | 41.0% |
| PostXInspection | | | | |
| Odds Ratio | 0.990*** | 1.031 | 0.934** | |
| s.e. | (0.003) | (0.041) | (0.027) | |
| No. of Observations | 25,764 | | | |

[39] To be clear, this does not imply that in 57% of inspections there was no informal employment detected. Informal employment, being outside STPS's direct sanctioning jurisdiction, is not part of the "inspection results" variable we received from STPS, although it is one of the items included in inspectors' checklist.

| **Panel B: Inspected Spouse is Informal and Spouse 2 is Formal at the Time of Inspection** | | | | |
|---|---|---|---|---|
| | Spouse's 2 (Not Inspected) New Labor Market Status (from Formal Job) | | | |
| | Unemployment or OLF | New Informal Job | New Formal Job | Same Job (Stayers) |
| Dependent Mean Transition Probability | 17.8% | 2.3% | 5.1% | 74.7% |
| PostXInspection | | | | |
| Odds Ratio | 0.981 | 0.979 | 0.953** | |
| s.e. | (0.075) | (0.016) | (0.021) | |
| No. of Observations | 18,255 | | | |

Notes: This table shows the effect of inspections on transition probabilities for spouse 2 (i.e. the spouse who was not directly affected by an inspection). The baseline sample is employed individuals with a spouse employed at an establishment in the DNE. The treatment group are individuals whose spouse was employed at an establishment at the time of inspection. The dependent mean transition probability is calculated as the average probability of each transition with individual characteristics, for both spouses, fixed at their mean values.

Panels A and B in Table C.12 show the average treatment effect of spouse 1 transitions out of an informal job, instrumented using STPS inspections, on spouse's 2 labor market status conditional on both spouses' formality status at the time of inspection. Panel A shows that if both spouses are informal, inspections on spouse's 1 establishment make transitions into a formal job by spouse 2 less likely. The probability that spouse 2 transitions into non-employment also declines.

Panel B shows the effect if spouse 2 is formal. In this case, transitions out of informal employment by spouse 1 have no significant effect on separations to unemployment by spouse 2. However, job-to-job transitions within the formal sector decrease. Spouse 2 is more likely to keep his current job in the formal sector. This result may appear to contradict the idea that spouses should avoid paying for benefits on both of their jobs. However, it is important to note that:

a) Transitions out of informality by spouse 1 need not be towards formal employment; they can also lead to unemployment;

b) Not all the benefits from formality are perfectly shared across household members (e.g. retirement funds and housing loans), therefore, additional members with formal jobs can have added value for the household;

c) Separation rates to unemployment are much lower in formal jobs (22.6% vs. 10%). If households value this stability they might choose to have two spouses with formal jobs regardless of paying for the shared benefits twice.

These results indicate that spouse's 2 response to spouse's 1 transitions out of informal employment depend on her own formality status. If spouse 2 is informal, job-to-job transitions to a formal job become less likely. If instead she has a formal job, job-to-job transitions within the formal sector also decrease. If spouse 1 is informal, inspections on spouse's 1 establishment increase the probability that spouse 2 stays in the same job.

Panels A and B in Table C.13 show the effect of inspections on spouse's 2 transitions probability conditional on the inspected spouse being formal. In these cases, inspections do not have a significant effect on household turnover rates. This is consistent with the findings from Appendix **??**: inspections do not have a significant effect on formal workers' turnover rates and hence their spouses are similarly unaffected.

Table C.13: Inspection Effects on Transition Probabilities for Employed Spouses of Inspected Formal Workers

| **Panel A: Both Spouses are Formal at the Time of Inspection** | | | | |
|---|---|---|---|---|
| | Spouse's 2 New Labor Market Status | | | |
| | Unemployment or OLF | New Informal Job | New Formal Job | Same Job (Stayers) |
| Dependent Mean Transition Probability | 15.9% | 1.5% | 4.7% | 77.8% |
| PostXInspection | | | | |
| Odds Ratio | 0.959 | 0.851 | 0.983 | |
| s.e. | (0.049) | (0.142) | (0.060) | |
| No. of Observations | 108,533 | | | |

| **Panel B:Inspected Spouse is Formal and Spouse 2 is Informal at the Time of Inspection** | | | | |
|---|---|---|---|---|
| | Spouse's 2 (Not Inspected) New Labor Market Status (from Informal Job) | | | |
| | Unemployment or OLF | New Informal Job | New Formal Job | Same Job (Stayers) |
| Dependent Mean Transition Probability | 34.5% | 11.8% | 11.2% | 42.3% |
| PostXInspection | | | | |
| Odds Ratio | 0.951** | 1.050 | 0.962 | |
| s.e. | (0.021) | (0.096) | (0.108) | |
| No. of Observations | 43,898 | | | |

Notes: This table shows the effect of inspections on transition probabilities for spouse 2 (i.e. the spouse who was not directly affected by an inspection). The baseline sample is employed individuals with a spouse employed at an establishment in the DNE. The treatment group are individuals whose spouse was employed at an establishment at the time of inspection. All regressions include individual and time fixed effects. The dependent mean transition probability is calculated as the average probability of each transition with individual characteristics fixed at their mean values.

Next, we analyze whether starting wages for spouses of inspected workers differ from those of non-inspected workers. If household members take into account the joint access to benefits offered in a formal job, transitions out of informal employment by spouse 1 will affect spouse's 2 reservation wage. This change in reservation wage provides information regarding how much households value a formal job.

As before, let $S^F_{1,h,j,t+1}$ be an indicator equal to 1 if spouse 1 in household $h$ is employed in formal job in establishment $j$ in period $t+1$. Let $Inspected_1$ indicate whether the establishment where spouse 1 is employed

received an inspection. Let $w^s_{2,h,k,t+1}$ be spouse's 2 starting wage in establishment $k$ in period $t+1$. We use the following specification to analyze the effect that transitions out of an informal job by spouse 1 have on spouse's 2 starting wages, conditional on starting a new job:

$$\begin{aligned}&\mathbf{E}\left(S^F_{1,h,j,t+1}|S^{INF}_{1,h,j,t}=1\right)= \\ &\phi\left(\alpha_0+\alpha_1 Post1,t+\alpha_1 Post_{1,t}\times Inspected_1+X'_{j,t}\alpha_2+X_{1,t}\alpha_3+t+\varepsilon_{1,h,t}\right)\end{aligned} \tag{10}$$

$$ln\left(w^s_{2,h,k,t+1}\right)=\beta_0+\beta_1\mathbf{E}\left(\widehat{S^F_{1,h,j,t+1}|S^{INF}_{1,h,j,t}}=1\right)+X'_{k,t}\beta_2+X_{2,t}\beta_3+t+\eta_{2,h,t}$$

Table C.14: Inspection Effects on Spousal Starting Wages
(Conditional on Spouse 1 Having an Informal Job and Spouse 2 Starting a New Job)

| **First Stage:** Spouse's 1 Pr(Transitions Out of an Informal Job) | | | |
|---|---|---|---|
| Variable | Odds Ratio | s.e. | Dependent Mean |
| PostXInspected | 1.489*** | 0.137 | 14.3% |
| | | | |
| **Second Stage:** Spouse's 1 Fitted Probability of Transitioning out of an Informal Job | | | |
| Variable | Odds Ratio | s.e. | Dependent Mean |
| PostXInspected | 1.412*** | 0.145 | |
| | | | |
| **Third Stage:** Spouse's 2 Ln(Starting Daily Wage) Conditional on having an informal partner and starting a new job) | | | |
| Variable | Coeff. | s.e. | Dependent Mean Daily Wage |
| $Pr\left(\widehat{\text{Spouse becomes formal}}\right)$ | 0.034** | (0.012) | MXN$106 |
| No. of Observations | 12,487 | | |

Notes: This table shows the effect of inspections on spouse 2's (i.e. the spouse who was not directly affected by an inspection) starting wages conditional on the inspected spouse being informal and on spouse 2 starting a new job within 3 quarters after the inspection, estimated using equation 10 above. The baseline sample is individuals who started a new job and have an informally employed partner. The treatment group are individuals who started a new job and whose spouse was employed at an inspected establishment (with the inspection occurring within 3 quarters previous to spouse 2 starting the new job). All regressions include time fixed effects and control for relevant characteristics of both spouses such as previous labor market experience, industry and occupation, firm size, household location, education, and gender.

Table C.14 indicates that even though spouse 2 is less likely to transition to a formal job after an inspection on his partner's establishment[40], his starting wage is 3.4% higher.

[40]See Table C.12 Panel B and Table C.13 Panel A