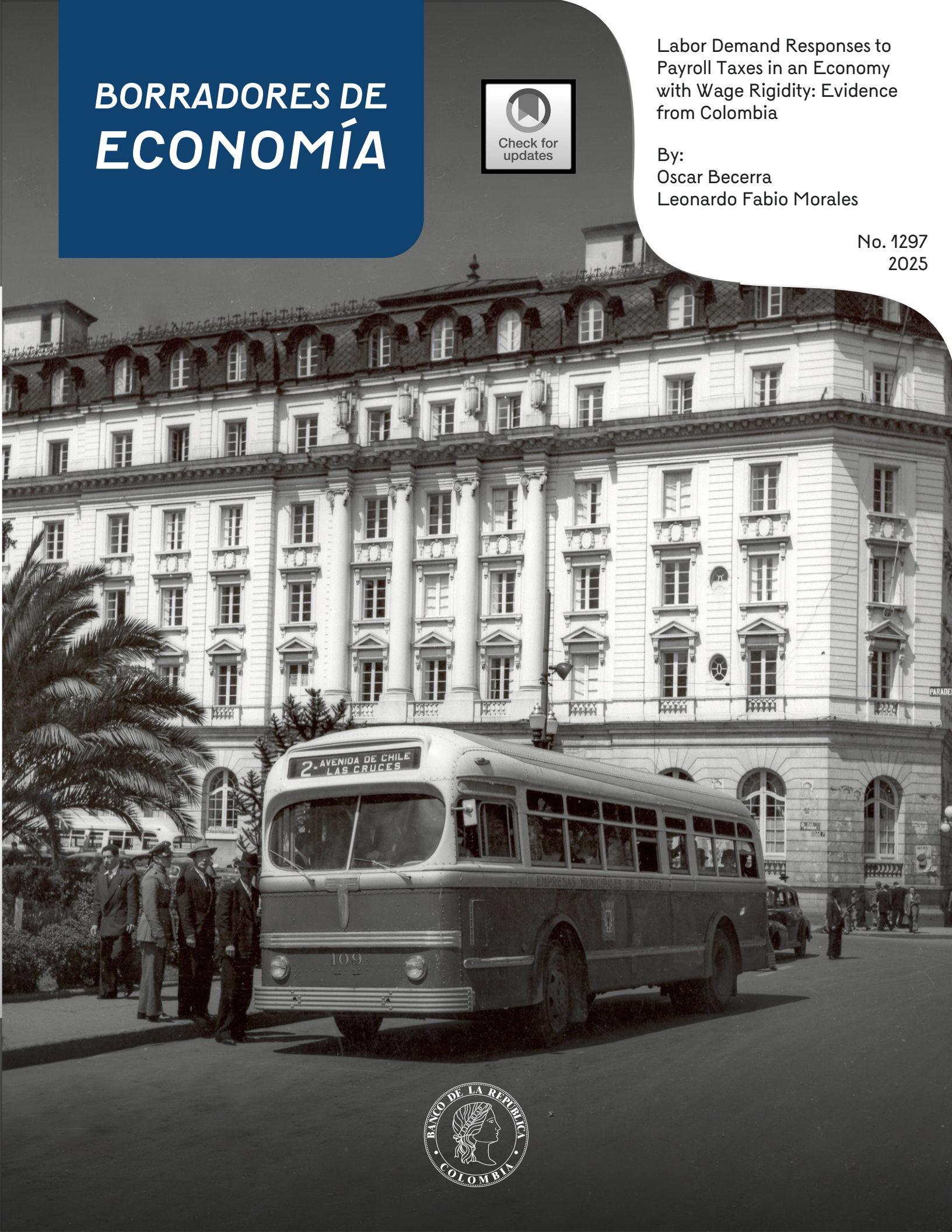
BORRADORES DE ECONOMÍA

Check for updates

Labor Demand Responses to Payroll Taxes in an Economy with Wage Rigidity: Evidence from Colombia

By:
Oscar Becerra
Leonardo Fabio Morales

No. 1297
2025



BANCO DE LA REPÚBLICA
COLOMBIA

Labor Demand Responses to Payroll Taxes in an Economy with Wage Rigidity: Evidence from Colombia*

Oscar Becerra† Leonardo Fabio Morales‡

The opinions contained in this document are the sole responsibility of the authors and do not commit Banco de la República nor its Board of Directors

Abstract

This paper analyzes the effect of payroll taxes on the formal sector labor demand in the presence of wage rigidity. In particular, We study the impact of a reduction of payroll taxes on the creation of formal jobs in Colombia, where about 40 percent of formal-sector workers earn the minimum wage. Using a reform that granted tax credits to firms hiring workers younger than 28 years of age, we obtain estimates of the effect of payroll taxes on formal-sector employment and wages. We show that payroll tax incidence is borne by formal-sector employers. The reduction in payroll taxes increased Formal-sector employment and did not affect wages. Using the estimation results, we recover an estimate of the elasticity of the formal-sector labor demand between -0.53% and -0.87%.

Keywords: Payroll taxes, formal employment, formal wages, labor policy
JEL: E62, H25, J21, J3

*We would like to thank Joshua Gottlieb, Thomas Lemieux, Florian Hoffman, Kevin Milligan, David Green, and Marcela Eslava for their helpful comments and valuable discussions. We also thank the participants at the CEA and LACEA annual meetings, as well as the workshop participants at Banco de la República. Special thanks to Raquel Zapata, Simón Yarce and David Salgado for their research assistance. This work represents the views of the authors and does not necessarily reflect those of Banco de la República. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors. Declarations of interest: none.

†Becerra: Corresponding author. obecerra@uniandes.edu.co. Department of Economics, Universidad de los Andes. Contact Information: Calle 19A No 1-37 Este, W-824. Bogotá, Colombia. Phone: +571 339-4949 ext. 5054.

‡Morales: lmoralzu@banrep.gov.co. Labor Market Analysis Group, Banco de la República, Medellin, Colombia.

Respuestas de la Demanda Laboral a los Impuestos a la Nómina en una Economía con Rigidez Salarial: Evidencia de Colombia*

Oscar Becerra† Leonardo Fabio Morales‡

Las opiniones contenidas en el presente documento son responsabilidad exclusiva de los autores y no comprometen al Banco de la República ni a su Junta Directiva

Resumen

Este artículo analiza el efecto de los impuestos a la nómina sobre la demanda laboral en el sector formal en presencia de rigidez salarial. En particular, estudiamos el impacto de una reducción de los impuestos a la nómina en la creación de empleos formales en Colombia, donde alrededor del 40 por ciento de los trabajadores del sector formal ganan el salario mínimo. Utilizando una reforma que otorgó créditos fiscales a las empresas que contratan trabajadores menores de 28 años, obtenemos estimaciones del efecto de los impuestos a la nómina sobre el empleo y los salarios en el sector formal. Mostramos que la incidencia del impuesto a la nómina es soportada por los empleadores del sector formal. La reducción en los impuestos a la nómina aumentó el empleo en el sector formal y no afectó los salarios. A partir de los resultados de estimación, recuperamos una estimación de la elasticidad de la demanda laboral en el sector formal entre -0.53% y -0.87%.

Palabras clave: impuestos a la nómina, empleo formal, salarios formales, política laboral
JEL: E62, H25, J21, J3

*Queremos agradecer a Joshua Gottlieb, Thomas Lemieux, Florian Hoffman, Kevin Milligan, David Green y Marcela Eslava por sus comentarios útiles y valiosas discusiones. También agradecemos a los participantes de las reuniones anuales de la CEA y LACEA, así como a los participantes del taller en el Banco de la República. Un agradecimiento especial a Raquel Zapata, Simón Yarce y David Salgado por su asistencia en la investigación. Este trabajo representa las opiniones de los autores y no refleja necesariamente las del Banco de la República. Esta investigación no recibió ningún financiamiento específico de agencias públicas, comerciales o del sector sin fines de lucro. Declaración de conflictos de interés: ninguno.

†Becerra: Autor de correspondencia. obecerra@uniandes.edu.co. Departamento de Economía, Universidad de los Andes. Información de contacto: Calle 19A No 1-37 Este, W-824. Bogotá, Colombia. Teléfono: +571 339-4949 ext. 5054.

‡Morales: lmoralzu@banrep.gov.co. Grupo de Análisis del Mercado Laboral, Banco de la República, Medellín, Colombia.

1 Introduction

A major challenge for developing countries is creating a strong social security system while minimizing potential negative effects on the economy (Levy, 2008). Payroll taxation is a key policy instrument in this effort. On one hand, payroll taxes can provide benefits to workers, such as insurance and mandated benefits, and can finance public goods. On the other hand, if formal employers (i.e., those who comply with regulations) bear the burden of payroll taxes, it can increase the cost of labor in the formal sector.[1] Such an increase in labor costs could discourage job creation in the formal sector, reduce demand for formal sector labor, and shift labor to less productive, lower-quality jobs in the informal sector. Empirical evidence from Brazil and Colombia shows that higher payroll taxes have led to increased informal sector employment in both countries (Ulyssea, 2010; Santa María et al., 2009; Bernal et al., 2017). Similarly, studies on Colombia indicate that reducing payroll taxes can stimulate formal job creation and increase wages (Bernal et al., 2017; Fernández and Villar, 2017; Morales and Medina, 2017).

Regarding payroll taxes, the literature identifies three main determinants of their incidence: the tax-benefit relationship of payroll taxes, the elasticity of labor supply, and factors that prevent wages from adjusting, such as minimum wages, nominal rigidities, and equal pay considerations (Gruber, 2000; Saez et al., 2019). If wages are flexible, a one-to-one valuation of benefits funded by payroll tax revenues or an inelastic labor supply will allow payroll taxes to be fully passed through to wages, minimizing employment effects. Ultimately, the incidence of payroll taxes depends on the interaction of these three factors, which is an empirical question.

In this paper, we analyze the incidence of payroll taxes in Colombia. Previous literature has argued that Colombia's labor market institutions are relatively rigid (Flórez et al., 2020), leading to inflexibility and preventing wages from adjusting to changes in payroll taxes. Significant distortions in the wage adjustment process characterize Colombia's economy. For instance, the Colombian minimum wage is binding for a substantial fraction of the population, with about 40 percent of formal sector workers earning the minimum wage (Bell, 1997). Additionally, around 56% of the employed population works in the informal sector (Bonilla et al., 2024). The large informal sector mitigates the pass-through from payroll taxes to wages, as a reduction in formal-sector wages reduces the benefits of working in the formal sector. This wage rigidity suggests that the incidence of payroll taxes is borne mostly by formal-sector employers.

To estimate the causal effect of payroll taxes on the Colombian labor market, we exploit an exogenous reduction in payroll taxes by age. In 2011, the Colombian government introduced the First Job Act, which reduced payroll taxes by 11 percentage points (from 42 percent) for new workers under the age of 28. Since the reduction in payroll taxes did not affect workers' benefits, as the deducted taxes were used to finance public goods, we interpret this reduction as a shock to the demand for labor in the formal sector. Using the exogenous variation caused by the First Job Act, we implement a difference-in-differences identification strategy to identify the causal effect of the policy. We use a new source of administrative

[1] In this context, we use a "legalistic view" to define formal and informal employment (Ulyssea, 2020). Informal employment refers to jobs that do not comply with government regulations, such as paying mandatory contributions and taxes. Companies in the informal sector do not pay taxes, including payroll taxes, but they are subject to fines if inspected. Informal sector workers do not contribute to mandatory benefit and insurance schemes and are not covered by mandated benefits and insurance.

records on formal sector employment and standard household surveys. In our strategy, we compare indicators of formality and participation, employment levels, hiring, and wages for cohorts of workers younger and older than 28 years of age. Consistent with the idea that employers bear the incidence of payroll taxes, we find that the payroll tax reduction brought about by the First Job Act increased formal sector labor demand for young workers by 3.4 percent, while having no significant effect on wages. The estimated effects are similar across firms of all sizes. They are more concentrated among workers with no formal sector experience, men, and workers in less developed regions.

This paper contributes to the literature on the incidence of payroll taxes by examining a context where labor market institutions lead to wage rigidity, causing employers to bear the incidence of payroll taxes. The literature on payroll tax incidence includes several studies focused on developing countries. Two notable papers are by Gruber (1997), who analyzed the impact of payroll tax reductions in Chile in the early 1980s, and Kugler and Kugler (2009), who examined the impact of payroll tax increases in Colombia in the early 1990s. Gruber (1997) found full pass-through from taxes to wages in Chile, while Kugler and Kugler (2009) found partial pass-through and employment effects in Colombia, highlighting wage rigidity as a potential driver of their results. Several studies have also examined the impact of a significant reduction in payroll taxes for all workers earning less than ten minimum wages, implemented in Colombia in 2014. These studies, including Bernal et al. (2017), Fernández and Villar (2017), and Morales and Medina (2017), found positive effects on formal employment. However, identification may be challenging in this case because virtually all workers benefited from the tax break. In our study, using individual-level data allows us to distinguish beneficiaries by cohort and directly analyze the effects of the payroll tax cut. Our empirical approach is similar to that used by Cruces et al. (2010) for Argentina, as we identify the incidence of these taxes by comparing the responses of similar workers facing different payroll tax rates in the same period, an identification strategy often unavailable in previous studies.

As an additional contribution to the literature on the incidence of payroll taxes, we recover the elasticity of labor demand in the formal sector. Based on our estimation results and a standard economic model presented in the appendix of the paper, we recover an estimate of the elasticity of the formal-sector labor demand between -0.53% and -0.87%. Given that the wage distribution of new workers is concentrated around the minimum wage, this elasticity is particularly informative about the labor demand response near the minimum wage. Consequently, our results suggest that a 10% increase in minimum wage would reduce formal sector employment between 5 and 9%. This implication is specific to the Colombian context, where the minimum wage is binding for a large fraction of formal sector workers (about 40%), making it likely that the labor demand in the formal sector drives these results.

The rest of the paper is organized as follows: Section 2 presents the institutional setting of the Colombian labor market, payroll taxes, and the First Job Act. Section 3 describes the data used in the estimation and the identification strategy. Section 4 presents the estimation results, and section 6 concludes.

2 Institutional background

2.1 Colombian payroll taxes and labor market

The labor market institutions in Colombia exhibit characteristics that suggest formal-sector employers bear the incidence of payroll taxes. A weak tax-benefit link, a binding minimum wage, and a large informal sector lead to a potentially large effect of payroll taxes on the generation of formal-sector employment.[2]

Table 1 presents a summary of payroll taxes levied in 2010. It shows the employer and employee tax rates based on contributions and notes whether the contribution rate is applied to the provision of benefits for workers. The total payroll tax rate represents between 46% to 54% of a worker's monthly wage and is divided into three components: insurance, family benefits, and public goods. The insurance component forms the largest part of the payroll tax rate (37 to 45 percentage points). It provides insurance for workers in the event of negative health shocks, old age, disability, and unemployment. Of the 12.5% deducted for health care insurance, two percentage points finance the public health care system. The family benefits component (4 percentage points) goes to Family Benefits funds, which are non-profit organizations responsible for providing benefits to workers, such as child allowances, access to recreation facilities, and housing subsidies. The public goods component of the contribution (5 percentage points) funds a public education institution focusing on technical and vocational education programs (SENA) and the government agency responsible for providing child protection and family services (ICBF). Most of the payroll tax rate is paid by the employer (38 to 46 percentage points).

Although the level of the Colombian payroll tax rate is similar to that of other developed and Latin American economies (Corbacho et al., 2013; OECD., 2016), the features of the payroll tax system may result in a distorting effect on the labor market. The tax-benefit link is weak, given that the benefits deriving from payroll taxes depend on being able to work in the formal sector and personal characteristics, and are not proportional to the worker's contribution. For example, because health care insurance covers the worker and their family, benefits from health care insurance depend on the worker's family size and health rather than their actual contribution. Moreover, unless workers place enough value on the social benefit provided by the public good component of the payroll taxes, they will not give up part of their wage to fund them (Summers, 1989). Previous evidence has shown that the expansion of the non-contributory healthcare system led to an increase of the informal labor market (Camacho et al., 2014).

Additionally, the binding minimum wage implies that the tax incidence – for a significant portion of Colombian workers – is fully borne by employers. In their comparison of wage distribution for Latin American economies, Maloney and Nuñez (2004) show that Colombia has a particularly binding minimum wage. When compared to seven Latin American economies,[3] the wage distribution in Colombia exhibits the second highest minimum wage-to-median-wage ratio, the lowest standard deviation, and the highest skewness coefficient. The results presented by Maloney and Nuñez (2004) indicate that the distribution of wages

[2] In the appendix, we illustrate under a perfect competition market that these three elements (the tax-benefit link, the binding minimum wage, and the large informal sector) are key drivers of the incidence of payroll taxes on labor market outcomes. While there are labor market characteristics that depart from the assumptions of perfect competition, these elements remain relevant in more complex setups.

[3] Argentina, Bolivia, Brazil, Chile, Honduras, Mexico, and Uruguay.

Table 1: Payroll taxes in Colombia, 2010

% of monthly wage	Total	Employer tax rate	Employee tax rate	Benefits for Worker	Benefits for Other
A. Insurance					
Health care	12.5	8.5	4.0	10.5	2.0
Workplace safety	0.4-8.7	0.4-8.7	–	0.4-8.7	–
Pension benefits	16.0	12.0	4.0	16.0	–
Severance savings	8.1	8.1	–	8.1	–
B. Family Benefits funds					
Family benefits	4.0	4.0	–	4.0	–
C. Public goods					
SENA/ICBF	5.0	5.0	–	–	5.0
Total	46.0-54.3	38.0-46.3	8.0	39.0-47.3	7.0

Notes: This Table presents a summary of the payroll taxes paid by Colombian firms and workers in the formal sector. It shows the employer and employee payroll tax rates, and the distribution of the rate between services provided to the worker and the financing of public goods. SENA is a public education institution with a focus on technical programs and training, ICBF is the government agency responsible for providing child protection and family services, and Family Benefits funds are non-profit organizations responsible for providing benefits to workers, such as child allowances, access to recreation facilities, and subsidies for housing.

in Colombia is concentrated around the minimum wage and is more concentrated than in other Latin American economies. The binding minimum wage is confirmed by the administrative records taken from the Social Security system statistics, which show that about 40 percent of formal-sector workers earn the minimum wage.

A third factor preventing wages from adjusting to changes in payroll taxes is the large size of the informal sector. The informal sector is characteristic of the Colombian economy is explained by several factors. Firms are inclined to operate informally given weak enforcement of registration requirements, large differences in costs of labor between the formal and informal sector, and a low valuation given to the benefit of operating in the formal sector (Santa María et al., 2009). As Mondragón-Vélez et al. (2010) show employment in the informal sector accounts for between 50 and 60 percent of total employment in Colombia, mostly less-educated people working as a self-employed or as a salaried worker in a small firm. These patterns are similar to those found in other Latin American countries (Perry et al., 2007; Ulyssea, 2020).

2.2 First Job Act

To encourage the generation of formal-sector jobs, the Colombian government enacted the First Job Act (Law 1429 of 2010). The Act had two objectives: to increase formal-sector employment of workers facing difficulties in finding formal-sector jobs, and to increase the registration rate of small firms.

The first component of the Act provided tax credits to existing firms for hiring workers under the age of 28. Starting in January 2011, employers could deduct 11 percentage points

of the payroll taxes paid for these new workers from their corporate taxes. The temporary tax credits allowed the employer to claim the benefit for up to two years. The deducted contributions correspond to payroll taxes used to fund public goods (SENA, ICBF, and the public health care system) and Family Benefits funds. A worker's eligibility was based on the age at which employment commenced. For example, if the worker was hired when he was 27 years and 11 months old and continued working in the same firm, the firm would still be able to claim the tax credits.[4] Because the Act intended to encourage the creation of new jobs, eligibility for tax credits was conditional on the firm increasing its total payroll by the end of the year.

The second component of the Act provided incentives for new firms employing up to 50 workers. The Act defined new firms as those registered after January 2011 and did not distinguish between new entrants and existing unregistered firms. Newly registered firms were exempt from paying corporate taxes (33 percent), along with 11 percentage points of the payroll taxes of all their workers (SENA, ICBF, Family Benefits, and public health care), and registration fees. The exemptions were temporary, allowing full exemption for the first two years and partial exemptions for the following three years.

The prospect of a reduction in payroll taxes resulted in a positive shock in the demand for workers under 28 years old. After the Act was enacted, the total labor cost (wage plus payroll taxes) of eligible workers declined by 11 percent. In contrast, the Act had a limited effect on labor supply because the reduction in payroll taxes did not affect workers' benefits. Most of the decrease in payroll taxes was associated with contributions used to fund public goods. Additionally, given that firms were still required to contribute to the Family Benefits funds, workers continued receiving the benefits from these contributions.

Since our focus is on identifying the effects of payroll taxes on formal-sector employment, we restrict the analysis to the effect of the reduction of payroll taxes for workers below the age of 28. Although the First Job Act reduced payroll taxes for newly registered firms, it also reduced corporate taxes and registration fees. As a result, we are not able to distinguish the causal effect of the reduction in payroll taxes on employment from the impact of reductions in the corporate tax rate and registration fees on employment for new entrants.

3 Empirical strategy

3.1 Data and sample selection

To investigate the effects of payroll taxes on the formal-sector labor market, we use data from two different sources: the official Colombian household survey (GEIH by its acronym in Spanish) and administrative records from the Social Security System (PILA by its acronym in Spanish). In both cases, we used the data to perform a cohort analysis to assess if cohorts that were potential beneficiaries of the policy showed better performance in terms of labor market results. The advantage of the cohort analysis is that it clearly defines the worker groups that were potential beneficiaries of the policy. Additionally, it allows a

[4]The Act included other groups of eligible workers: women 40 and above without a formal job in the last 12 months; people with disabilities; heads of households eligible for social assistance programs; low-wage workers (up to 1.5 times the minimum wage) who had not worked in the formal sector; refugees; demobilized guerrilla soldiers; and paramilitary members. When a worker met more than one of the eligibility criteria, the exemptions applied once. Due to the lack of information needed to identify these workers, we restrict our analysis to workers under the age of 28.

clean comparison between the results using administrative records and survey data. This is important because the survey data is not a panel, and in the administrative data, the worker-level analysis is limited by the lack of registry for informal or unemployed individuals. Finally, it is a simple and parsimonious method to assess the incidence of payroll taxes on formal-sector employment. As a form of validation of our results, we also estimate the effects of the policy using regression at the firm level with the PILA dataset.

In what follows, we adopt a legalistic view of formality and define formal-sector workers as those who contributed to the Social Security system (Ulyssea, 2020). Regarding household surveys, the GEIH is administered by the Bureau of National Statistics (DANE) and is the official source of employment statistics in Colombia. This comprehensive survey is a repeated cross-section that encompasses both formal and informal workers and serves as the official data source for calculating key labor market indicators. Regarding the administrative records, the PILA is an employer-employee dataset obtained from the system used to collect payroll taxes. From the taxes reported in Table 1, all but the severance savings contributions are collected monthly through the PILA system. The PILA dataset includes information concerning the type of employer (public or private), the kind of worker (self-employed or salaried worker), days worked, job location, and the worker's wage, gender, and date of birth. Since formal-sector employers pay payroll taxes and mandated contributions, the dataset gathers information for the universe of formal-sector workers.

The cohorts for the baseline analysis are constructed based on the quarter of birth. Our study period is between June 2010 and June 2013. We restrict the analysis up to this second date because, at that time, another policy was implemented in Colombia. This new policy, known as the 2012 Colombian Tax Reform, began implementation in June 2013 and reduced payroll taxes for workers regardless of their age. The reduction in payroll taxes was larger than that for the First Job Act. Therefore, the First Job Act effectively ended in the second quarter of 2013. Regarding the selection of workers within each cohort, we restrict the sample to those aged 18 to 38 at the time of the implementation of the First Job Act. This is to mitigate concerns about systematic differences in the time trends of formal-sector employment by age or cohort.

Table 2 presents summary statistics for the cohort estimation sample of employers using GEIH and PILA. We compute job characteristics and aggregates by quarters between the third quarter of 2009 and the second quarter of 2013 and report these aggregated statistics' mean and standard deviation. The average number of formal-sector workers in our cohorts of interest is about 45,000 in both GEIH (using sampling weights) and PILA. Therefore, even though they are data sources of a different nature, the measurement of formality is surprisingly comparable between both sources. On average, between 2010 and 2013, there were 5.9 million workers in PILA and 5.7 million workers in GEIH, aged between 18 and 38 years old. Using the working-age population from GEIH, the formal employment-to-population ratio of the average cohort in our sample is 27%; the participation rate also computed with GEIH data, for the average cohort in our sample, is 82%

The higher employment share belongs to the tertiary economic sector with 80% and 75% of the employment for the average cohort in our sample for PILA and GEIH, respectively. In terms of wages, the measurement is similar between the two data sources: 1.3 million pesos for GEIH and 1.2 million pesos for PILA, which represents around $1,000 US dollars for PPP. Some variables can only be observed in the survey data; for instance, the average years of education for the estimation sample is 9.8, the labor force participation rate is 82%,

and the average formal employment-to-population rate is 27%.

Table 2: Summary statistics

	PILA		GEIH	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**
Formal Employment	45284	12422	45216	10282
Employment to population ratio	-	-	0.27	0.057
Total employment	-	-	122524	14971
Wage	1209972	276157	1350248	362051
Formal Hires	9873	0.01	2820	1660
Participation rate	-	-	0.82	0.062
Total Labor Force	-	-	140111	14873
Male share	0.59	0.01	0.49	0.031
Education (years)	-	-	9.84	0.569
Household head share	-	-	0.32	0.119
Primary economic sector	0.05	0.01	0.11	0.025
Secondary economic sector	0.14	0.01	0.14	0.027
Tertiary economic sector	0.80	0.01	0.75	0.037
Firm size 1: Up to 20 employees	0.18	0.02	0.65	0.054
Firm size 2: between 21 and 50 employees	0.10	0.01	0.06	0.019
Firm size 3: between 51 and 100 employees	0.08	0.01	0.03	0.013
Firm size 4: more than 100 employees	0.64	0.03	0.25	0.046
Firm Age 2: Between 2 and 3 years	0.05	0.02	-	-
Firm Age 3: Between 4 and 6 years	0.06	0.01	-	-
Firm Age 4: more than 6 years	0.86	0.03	-	-

Notes: In this table, we present summary statistics for all quarter cohorts between 18 and 38 years, all during the third quarter of 2009 to the second quarter of 2013.

3.2 Identification strategy

To measure the causal effect of changes in payroll taxes on formal-sector employment and wages, we use the variation across age and time induced by the First Job Act. We implement a differences-in-differences design using a panel by cohort. We identify the labor market effects of payroll taxes by comparing these indicators for cohorts of workers younger and older than 28 before and after the First Job Act. Using our cohort panel for workers in formal-sector firms, we estimate regressions of the form:

$$y_{c,t} = \alpha + X'_{c,t}\beta + \delta \cdot Treat \cdot Post + \mu_{c,t} + \pi_{c,t} + \gamma_{c,t} + u_{c,t} \tag{1}$$

where $y_{c,t}$ is our outcome of interest, $X_{c,t}$ is a vector of control variables, which includes the share of employment in the primary and secondary economic sectors, the share of employment in different age groups, and the share of male workers. In the regressions with GEIH, we control for the average years of education in each cohort. $Treat$ is a variable equal to 1 for cohorts that, by the second quarter of 2013, were less than 28 years old and equal to zero otherwise. $Post$ is equal to 1 after the implementation of the policy. In addition, $\mu_{c,t}$, $\pi_{c,t}$,

and $\gamma_{c,t}$ are cohort, age, and time-fixed effects, respectively. Finally, $u_{c,t}$ is the regression's error term.

We use multiple outcomes of interest, $y_{c,t}$, to estimate the labor market effects of the reduction of payroll taxes caused by the entry of the First Job Act. In the case of the estimations with the PILA dataset, given that we only had access to information about formal-sector workers, we used the log of employment, log of wages, and log of hires as dependent variables. In the case of the GEIH, we use the same variables as the dependent variables and add some additional variables. Taking advantage of the fact that in GEIH we can observe the whole market and not only the formal share, we also use the formal employment-to-population rate and the labor force participation rate as dependent variables. In all the regressions, we weigh by the share of occupied population cells. Under the identification assumption that determinants of formal-sector employment do not change differentially across the treatment and control groups around the reform, δ is the causal effect of the reduction in payroll taxes on formal-sector employment for workers younger than 28.

A necessary condition to interpret δ as the causal effect of the First Job Act on labor market outcomes is that we do not observe differential time trends in the outcomes of interest before the policy was implemented. To provide evidence supporting this assumption, we complement our results by estimating an event study, in which we interact the treatment variable with a set of time dummy variables for each of the study periods instead of a single interaction with the post-treatment variable. In particular, we estimate the equation

$$y_{c,t} = \alpha + X'_{c,t}\beta + \sum_{s=1}^{T} \delta_s \cdot Treat_c \cdot 1_{\{s=t\}} + \mu_{c,t} + \pi_{c,t} + \gamma_{c,t} + u_{c,t}. \tag{2}$$

As before, $X_{c,t}$ is a vector of control variables, and $\mu_{c,t}$, $\pi_{c,t}$, and $\gamma_{c,t}$ stand for cohort, age, and time fixed effects. If there are no differential time trends before the policy was implemented, we should observe that δ_s is equal to zero for each s before the entry of the First Job Act.

Because the minimum wage is binding for a significant fraction of new workers, the expected sign for the employment effect of the reduction of payroll taxes is positive, but the expected sign for the wage effect is ambiguous. To see this, note that if workers' productivity is not constant, reducing payroll taxes has two effects on workers with productivity close to the minimum wage (Kramarz and Philippon, 2001). On one hand, some workers who would have entered earning the minimum wage will receive a higher wage. This effect reduces the fraction of workers at the minimum wage and increases the average wage. On the other hand, the reduction allows the entry of new minimum wage workers who were not productive enough to work at the original labor cost level. This effect increases the fraction of workers at the minimum wage and reduces the average wage. Thus, the overall effect of reducing payroll taxes on wages around the minimum wage depends on the relative magnitude of these offsetting effects.

4 Estimation results

4.1 Baseline results

Tables 3 and 4 report the employment and wage effects of reducing payroll taxes for young workers by estimating equation (1) using the GEIH and PILA datasets. In both tables and for each outcome, the first column reports the results of a model that includes cohort and

time-fixed effects but no additional covariates. The second column includes covariates in addition to cohort and time-fixed effects. The third column includes age-fixed effects in addition to covariates, cohort, and time-fixed effects. In the case of Table 3, GEIH allows the study of the following outcomes: log of formal employment, log of wages, employment-to-population ratio, and labor force participation rate. In Table 4, PILA allows the study of the following outcomes: log of formal employment, log of wages, and log of hires. In all cases, we allow for correlated errors by cohort over time.

The estimates from GEIH in Table 3 indicate that the main adjustment to reducing payroll taxes for new workers younger than 28 was through employment rather than wages. Using the most comprehensive estimation (third column in every outcome), we find a positive and significant effect on employment of 7.6%. We also find positive and significant effects on the employment-to-population rate and participation rates of 1.1 and 1.4 percentage points, respectively. There is a small and insignificant effect on the average wage. A similar picture of the effects of the reform is portrayed by the estimates from PILA in Table 4. The reform has shown an even higher effect on the beneficiary cohorts of 12.8% and an increase in hiring in this cohort of 10%. In both cases, these effects are statistically significant. As in the GEIH case, we find no effects on wages with the PILA dataset.

To provide evidence supporting the assumption that there are no differential trends between the employment outcomes of workers younger and older than 28, we estimate equation (1) and report the estimation results in Figures 1 and 2 for the regression using GEIH and PILA datasets. The figures show the estimated coefficients (δ_s) for formal employment regressions and other relevant outcomes. Before the enactment of the First Job Act (first quarter of 2011), the estimated effects were small and statistically insignificant, confirming the presence of common trends and adherence to model assumptions. In addition, in line with the results of the DID estimations, labor demand and supply indicators show positive and significant effects after some quarters of the implementation of the policy. For instance, in both cases, PILA and GEIH, there is a positive effect of the policy on formal employment, which increases over time and, as in the DID estimation, is of a higher magnitude in the PILA estimations. Similar results are obtained with other outcomes such as employment-to-population rate, hires, and labor participation rate. Finally, as expected from the DID results, there are no positive effects on wages in any quarter after the implementation of the policy, and this result holds for both data sources.

Table 3: DID Regression GEIH

	Employment			Wage			Employment to population ratio			Participation Rate		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
TreatN x Post	0.3133***	0.1524***	0.0768***	0.1058***	0.0510***	0.0061	0.0560***	0.0232***	0.0112***	0.0540***	0.0381***	0.0137***
	(0.0403)	(0.0259)	(0.0216)	(0.0167)	(0.0139)	(0.0194)	(0.0053)	(0.0034)	(0.0038)	(0.0059)	(0.0046)	(0.0046)
Education		0.0799***	0.0540***		0.0616***	0.0538***		0.0148***	0.0145***		0.0250***	0.0214***
		(0.0141)	(0.0109)		(0.0156)	(0.0162)		(0.0025)	(0.0025)		(0.0030)	(0.0024)
Gender		0.0055	0.1451		0.1012	0.1422		0.0602***	0.0562***		0.1172***	0.1236***
		(0.1190)	(0.1027)		(0.1185)	(0.1243)		(0.0213)	(0.0205)		(0.0209)	(0.0194)
Household head		0.3929***	0.3336***		0.2762**	0.2000*		0.1033***	0.0829***		0.1278***	0.1137***
		(0.1194)	(0.1159)		(0.1181)	(0.1186)		(0.0216)	(0.0220)		(0.0230)	(0.0222)
Sector1		-0.0824	-0.2772		0.6200**	0.6294**		-0.1131***	-0.0968**		0.0458	0.0404
		(0.2103)	(0.1854)		(0.2477)	(0.2475)		(0.0392)	(0.0386)		(0.0405)	(0.0379)
Sector3		-1.2322***	-0.8874***		0.3736**	0.4625**		-0.1998***	-0.1874***		-0.3143***	-0.2499***
		(0.1778)	(0.1529)		(0.1839)	(0.1822)		(0.0279)	(0.0286)		(0.0378)	(0.0336)
Firm size: 21 to 50 employees		2.1116***	1.5650***		0.5198**	0.5112**		0.4490***	0.4230***		0.1295***	0.0237
		(0.2552)	(0.1850)		(0.1976)	(0.2171)		(0.0357)	(0.0363)		(0.0437)	(0.0342)
Firm size: 51 to 100 employees		2.2856***	1.6762***		0.4184	0.4729*		0.4110***	0.4072***		-0.0215	-0.1183**
		(0.2908)	(0.2320)		(0.2697)	(0.2767)		(0.0404)	(0.0436)		(0.0602)	(0.0514)
Firm size: 101 employees or more		2.4990***	2.0147***		1.0418***	0.9443***		0.5093***	0.4882***		0.0558**	-0.0341*
		(0.1865)	(0.1281)		(0.0947)	(0.1029)		(0.0225)	(0.0237)		(0.0278)	(0.0187)
Constant	10.5782***	9.8122***	9.9779***	14.0371***	12.6580***	12.7072***	0.2470***	0.0427	0.0527*	0.8066***	0.6764***	0.7046***
	(0.0116)	(0.1770)	(0.1666)	(0.0048)	(0.1991)	(0.1989)	(0.0015)	(0.0304)	(0.0302)	(0.0017)	(0.0416)	(0.0390)
Observations	1,294	1,294	1,294	1,294	1,294	1,294	1,294	1,294	1,294	1,294	1,294	1,294
R-squared	0.6767	0.8149	0.8810	0.7743	0.8022	0.8099	0.7490	0.8914	0.8971	0.8222	0.8676	0.9010
FE Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Period	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Age	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Trend	No	No	No	No	No	No	No	No	No	No	No	No
Employment			45216			45216			45216			45216

Notes: This Table presents the estimation results for the employment, wage, occupation, and participation effects for the sample of cohorts in formal-sector firms between September 2009 and June 2013 (see equation (1)); for this estimation, we use survey data from GEIH. Specification (1) includes cohort and time-fixed effects, specification (ii) includes control variables in addition, and specification (ii) includes control variables, ages, cohort, and time-fixed effects. Employment to population rate is the ratio of employed population to working age population. The labor force participation rate is the ratio of labor force participants to the working-age population. Standard errors are clustered by a combination of cohort-by-age groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

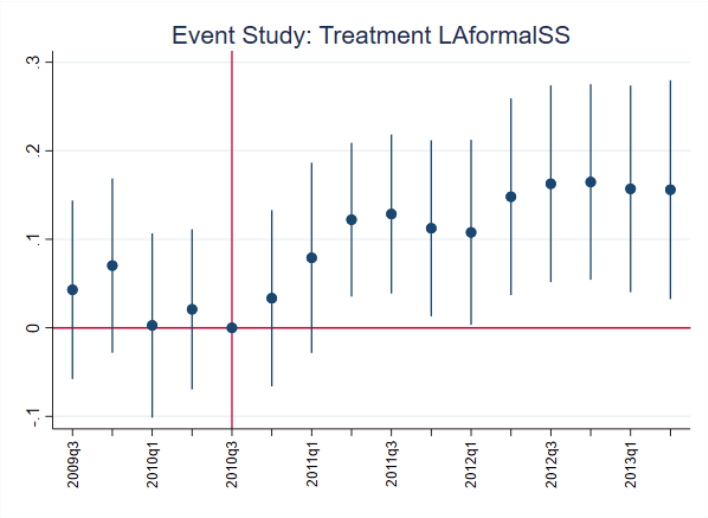
Table 4: DID Regression PILA

	Employment			Wage			Hires		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
TreatN x Post	0.6332***	0.3682***	0.1285***	0.0196	0.0167	-0.0026	0.4872***	0.3058***	0.1000***
	(0.1234)	(0.0958)	(0.0248)	(0.0123)	(0.0151)	(0.0040)	(0.1209)	(0.1053)	(0.0242)
Sexo		-2.8441	-0.8692		0.2904	0.3457*		-4.2683	-1.0153
		(3.3568)	(1.1176)		(0.5841)	(0.1882)		(3.6161)	(1.2137)
Sector1		-10.2293	-2.5215		0.3995	-0.1960		-5.6174	-0.8461
		(18.3330)	(4.7398)		(2.8630)	(0.6860)		(20.2316)	(5.0646)
Sector3		-7.8647**	-3.6704**		-0.2888	0.1718		-6.5352	-1.6322
		(3.9237)	(1.6346)		(0.6102)	(0.2527)		(4.7950)	(1.7863)
Firm age: 2 to 4 years		-23.0554***	1.1097		1.4240	-1.3648**		-28.2394***	1.1519
		(6.9054)	(3.2011)		(1.4102)	(0.5217)		(7.8959)	(3.4615)
Firm age: 4 to 6 years		-13.3514	3.8626		1.4564	-0.9315**		-21.1808*	3.0153
		(9.9779)	(3.3868)		(1.4591)	(0.3976)		(11.4890)	(3.9873)
Firm age: 6 years or more		-12.5564**	1.6520		1.7426	-0.7979*		-22.5372***	-0.3660
		(5.9934)	(2.4714)		(1.0807)	(0.4052)		(6.3517)	(2.9458)
Firm size: 21 to 50 employees		-7.3107	-2.7346		2.4134*	1.2553**		-7.0556	-3.0309
		(8.6686)	(2.0672)		(1.3508)	(0.5503)		(10.3650)	(2.2215)
Firm size: 51 to 100 employees		4.0747	0.4874		0.4156	0.3300		6.6824	1.0792
		(7.9729)	(1.8980)		(1.1987)	(0.4490)		(8.9981)	(2.0333)
Firm size: 101 employees or more		14.8722***	6.1949***		-0.1860	0.7173***		14.9734***	5.4639***
		(3.6321)	(1.3475)		(0.3898)	(0.1980)		(4.1103)	(1.2929)
Constant	10.4108***	22.8000**	8.7270***	13.9593***	12.1892***	13.8409***	8.9579***	29.8562***	7.8182***
	(0.0361)	(9.0936)	(2.0256)	(0.0036)	(1.7806)	(0.4582)	(0.0354)	(10.0054)	(1.8443)
Observations	1,296	1,296	1,296	1,296	1,296	1,296	1,296	1,296	1,296
R-squared	0.7355	0.9152	0.9815	0.9789	0.9836	0.9957	0.6154	0.8615	0.9742
FE Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Period	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Age	No	No	Yes	No	No	Yes	No	No	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Trend	No	No	No	No	No	No	No	No	No
Employment			45284			45284			45284

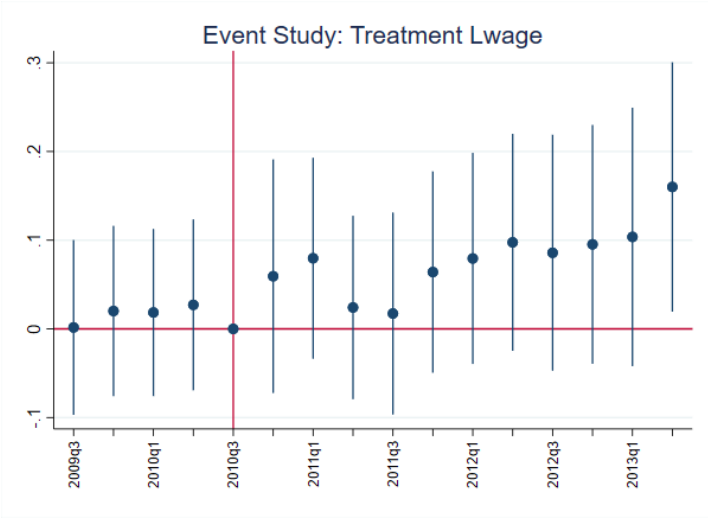
Notes: This Table presents the estimation results for the employment, wage, and hires effects for the sample of cohorts in formal-sector firms between September 2009 and June 2013 (see equation (1)); for this estimation, we use survey data from PILA. Specification (1) includes cohort and time-fixed effects, specification (ii) includes control variables in addition, and specification (ii) includes control variables, ages, cohort, and time-fixed effects. Standard errors are clustered by a combination of cohort-by-age groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Event Study Estimations with GEIH Data

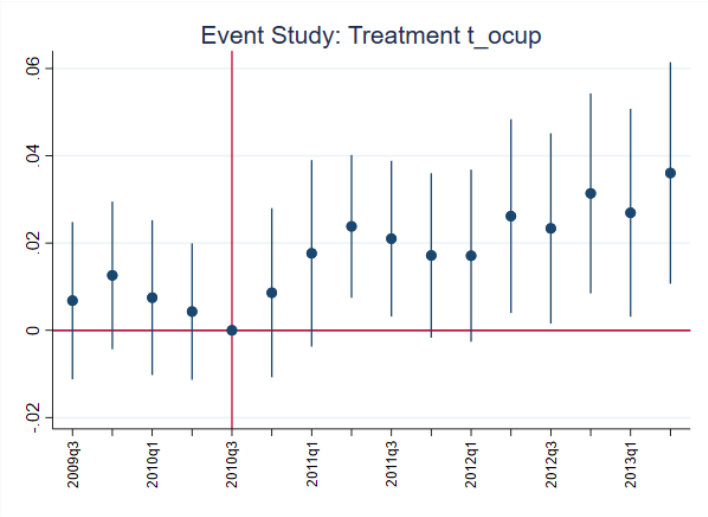
(a) Log of Formal Employment



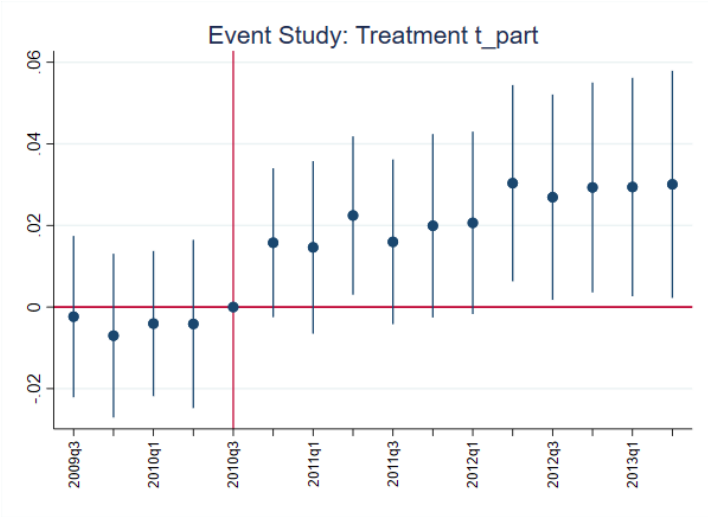
(b) Log of Wages



(c) Employment to Population Rate



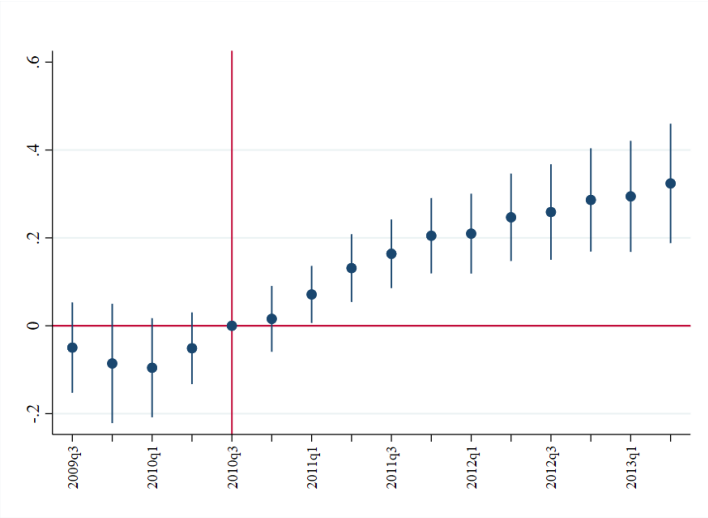
(d) Labor Force Participation Rate



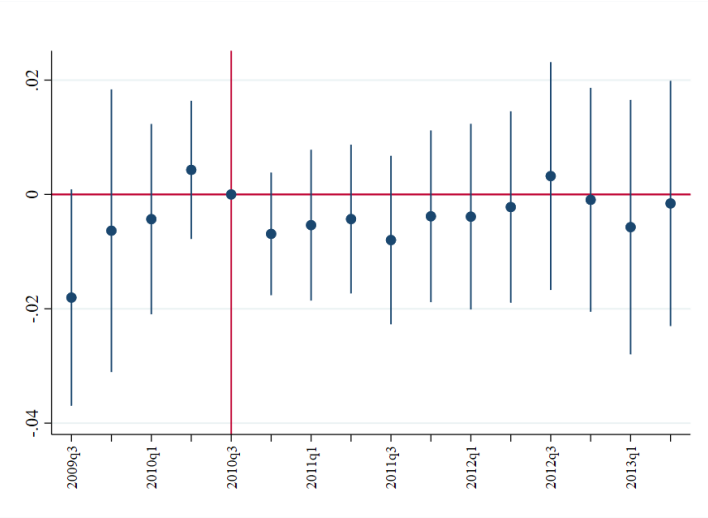
Notes: This Figure investigates the dynamic effects of the First Job Act on employment, wages, employment to population rate, and participation rate (see equation (1)); for this estimation, we use survey data from GEIH. Each point in the graph represents the estimated effect using a differences-in-differences strategy in which we allow the effect to vary by quarter. We use the third quarter of 2010 as a base category. Confidence bands are 95 percent confidence intervals. Standard errors are clustered by a combination of cohort-by-age groups.

Figure 2: Event Study Estimations with PILA Data

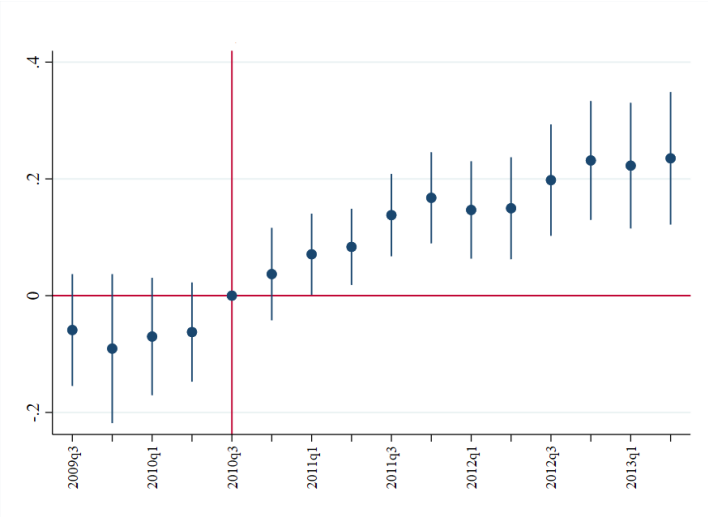
(a) Log of Formal Employment



(b) Log of Wages



(c) Lof of hires



Notes: This Figure investigates the dynamic effects of the First Job Act on employment, wages, and hires (see equation (1)); for this estimation we use survey data from PILA. Each point in the graph represents the estimated effect using a differences-in-differences strategy in which we allow the effect to vary by quarter. We use the third quarter of 2010 as a base category. Confidence intervals are 95 percent confidence intervals. Standard errors are clustered by a combination of cohort-by-age groups.

4.2 Heterogeneity analysis

Our estimation results indicate that reducing payroll taxes increased employment and hires while having negligible wage effects. Next, we analyze whether the effects vary with observable characteristics in the data. In particular, we study the effects of the policy changes by economic sector, firm size, and firm age.

In the Appendix, we present the estimation results of the most comprehensive specification for three different economic sectors, primary sector, manufacture, and services, for both estimations with GEIH (Appendix 1) and PILA (Appendix 2) datasets. According to the results from GEIH estimations, the higher impacts of the First Job Act are concentrated in the service and manufacturing sectors, with the highest impact (13%) in the latter one. Estimations from PILA show relative heterogeneity, and as opposed to the GEIH results, the effect in the primary sector is statistically significant. An explanation for this is that PILA, the formal agribusiness, and agricultural manufacturing are overrepresented; this is because these are the most frequent types of fully formal firms in the primary sector in Colombia.

In Appendix 3 and Appendix 4, we present the estimation of the most comprehensive specification for four different types of firms according to their size: firms up to 20 employees, firms between 21 and 50 employees, firms between 51 and 100 employees, and firms more than 100 employees. As before, we show results using GEIH (Appendix 3) and PILA data (Appendix 4). The results indicate that, regarding the GEIH sample, positive effects on employment are driven by small firms (up to 50 employees) and medium firms (50-100 employees). In the case of PILA estimation, the effects on employment are more homogenous. Nevertheless, their magnitudes are larger for small and medium firms (around 13%) in comparison to larger firms (8%). For medium size firms in both cases, GEIH and PILA, there a positive and significative effect on wages.

Additionally, the PILA dataset allows us to characterize firms by their age; in a final heterogeneity analysis presented in Appendix 6, we show the estimation for four different types of firms according to their age: firms up to 2 years old, firms between 2 and 4 years old, firms between 4 and 6 years old, and firms more than 6 years old. This analysis cannot be done with the GEIH dataset. The results show that even though the effects of the First Employment Act are uniform across these categories, the effects tend to be larger for the youngest firms.

5 Robustness Checks

Our main results hold throughout a series of robustness checks. In the first exercise, we estimate regressions at the firm level to corroborate this study's main two results: (i) the positive and significant effect of the policy on formal employment and (ii) the null effect of the policy on wages. A main concern in this setup is that the policy aimed at boosting employment for young workers; however, an unintended consequence might have been the replacement of formal workers older than 28 years with formal workers younger than 28 years, without increasing overall formal sector employment. Studying the effects of the policy at the firm level will allow us to identify if total employment changes as a result of the policy. Furthermore, we can also assess the effects of the policy on employment for young and older workers; in this way, we can test if any replacement effect in the distribution of workers by age is taking place. Our findings do not support this substitution effect. Instead, we observe

an increase in total employment, employment for those younger than 28 and those older than 28. This suggests that younger and older employment are complementary, as the reduced marginal cost of hiring young workers leads to higher demand for both younger and older formal employees.

Following Saez et al. (2019), we run DID regressions at the firm level, using the variation in the age composition of employment demand at the industry level as a treatment. We estimate a DID regression, where the firms are considered as treated if they belong to an industry that, before the implementation of the reform, had a large share of young employees. More specifically, treated firms belong to an industry in which the percentage of workers younger than 28 years is greater than the median. The estimated regression can be represented as

$$y_{j,t} = \alpha + X'_{j,t}\beta + \delta \cdot Treat_j \cdot Post_t + \mu_{j,t} + \gamma_t + u_{j,t}, \tag{3}$$

where $X_{j,t}$ is a vector of control variables, which include the fixed effect by economic sectors, fixed effects by industries, and the share of male workers. The variable $Post_t$ is equal to 1 after the implementation of the policy. Results for this estimation are presented in Appendix 6. The employment effect on treated firms is positive and significant (1.5%); firms that, before the policy, had a higher percentage of younger workers experienced an increase in employment after the implementation of the policy in comparison with control firms. Regarding heterogeneous effects on employment for young and older workers, we find that the policy had a positive effect on both types of employment, and the magnitudes of the effects are similar. We find no effect on wages in this regression.

We present further evidence in favor of the parallel trend assumption using the methodology developed by Rambachan and Roth (2023). Using this methodology, we analyze inference and sensitivity to violations of the parallel trends assumption during the post-treatment period for the estimated impact of the policy during Q2 2013 (last estimated impact). Moving along the horizontal axis, M-bar values represent the researcher's tolerance for parallel trend violations, ranging from 0 to 2. This framework identifies a "break-down value" beyond which results lose statistical significance (i.e., the confidence interval includes zero). The results remain robust when violations of parallel trends are statistically significant and equal to or greater than the maximum violation observed in the pre-treatment period (i.e., when the M-bar is equal to or greater than one). Our revised robustness checks confirm that the M-bar is at least 1 at a 5% significance level for all the outcomes that show significant effects in the standard DID results. We show these results in Appendix 7.

We also test for heterogeneous treatment effects following de Chaisemartin and D'Haultfœuille (2020). Recent literature has pointed out that the estimated impact of DID design could be biased in the presence of group-specific heterogeneous effects (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon and Marcus, 2020; Imai and Kim, 2021). In the case of the First Job Act, depending upon the definition of the treatment, some cohorts could be eligible for more time during the time window for which the policy was implemented. To minimize this possibility, as explained in section 3.1, we define treated cohorts whose date of birth was after the last quarter of 1983; therefore, the treatment cohorts could be eligible at some point of the implementation window, and the control group is never treated. According to Goodman-Bacon and Marcus (2020), in the case of heterogeneous treatment over time, biases can even result in an estimated treatment effect that is opposite in sign to the actual effect. Specifically, in DID designs with multiple periods, the estimated treatment effect is a weighted average of group-specific effects, where

some weights may be negative. Consequently, it is possible for the overall estimated effect to have an opposite sign, even if the true effect is consistent across all groups.

The diagnostic test introduced by de Chaisemartin and D'Haultfœuille (2020) involves calculating the minimal standard deviation of group-specific Average Treatment Effects on the Treated (ATT) that aligns with a standard DID estimate of the opposite sign to the actual value, considering a reasonable degree of treatment effect heterogeneity. The further this standard deviation is from zero, the lower the risk of a biased standard DID coefficient. In Appendix 9, we present the robust multi-period DID estimation and the diagnostic tests for the baseline and extended study periods for all the estimations of labor demand outcomes and wages, using information from PILA and GEIH. These tables present the robust multi-period DID and the baseline estimation results for comparison. Results show that for all outcomes demand outcomes, the point estimates are very similar between the baseline and the robust estimations; furthermore, in the case of PILA, the effect on employment and hires is significant at the 5% significance level; in the case of the GEIH estimation, the robust effects on employment and employment to populations ratio are statistically significant at the level of 10%. In the case of wages, there is no significant effect on the baseline or the robust estimations.

6 Final remarks

In this paper, we analyze the response of formal-sector labor demand to payroll taxes in an economy with wage rigidity. In developing economies, payroll taxes may have large distortionary effects, given the likelihood that their institutional characteristics will have employers bearing all the incidence of the payroll tax.

In particular, we analyze the incidence of payroll taxes in the formal sector in Colombia. Colombia is an example of an economy with labor market institutions that prevent payroll taxes from passing through wages. On one hand, it has strong wage rigidity. It exhibits one of the most binding minimum wages in the region, and half of the labor force works in the informal sector. On the other hand, most of the payroll tax system has a low tax-benefit link, which leaves workers less willing to give up part of their wage in exchange for access to the benefits from payroll taxes.

We use the First Job Act to estimate the incidence of payroll taxes on the Colombian formal sector. Starting in 2011, the Act reduced payroll taxes for new workers under 28. The Act has two useful aspects for the identification of the incidence of payroll taxes. First, it modified payroll taxes for only a subpopulation of workers, which allows the identification of employment and wage effects by using cohort variation over time. Second, the Act reduced taxes that did not provide a direct benefit for workers, and thus the variation induced by the Act can be interpreted as a shock in the formal-sector labor demand.

We estimate the payroll tax incidence by applying differences-in-differences identification strategies using a new source of administrative data for the formal sector and traditional survey data. We estimate the effects of reducing payroll taxes on formal labor market indicators, including employment and wages. Consistent with the idea that employers bear the incidence of payroll taxes, we find that reducing payroll taxes increased formal-sector demand for cohorts of young workers by 7.6% and 12.8% using the data from surveys and administrative records, respectively. We find no significant effect on wages. The estimated employment and wage effects are consistent across different specifications and subsamples.

The estimated impacts are higher for small-sized and young firms.

Using the estimates from the differences-in-differences strategy and the change in payroll taxes, we find that the implied elasticity of demand in the formal sector is -0.9 (using the effects from GEIH estimates). The results show that changes in payroll taxes are an effective policy tool for generating formal-sector employment when the institutional arrangement prevents the labor market from passing through payroll taxes to wages. The generalization of these results is not straightforward, though, because such generalization would depend on the particular rigidity affecting the labor market. Nonetheless, this paper shows the importance of understanding the wage-setting process to better measure the extent and efficacy of labor market policies.

References

Linda A. Bell. The impact of minimum wages in Mexico and Colombia. *Journal of Labor Economics*, 15(S3):S102–S135, 1997.

Raquel Bernal, Marcela Eslava, Marcela Meléndez, and Alvaro Pinzón. Switching from payroll taxes to corporate income taxes: Firms' employment and wages after the 2012 colombian tax reform. *Economia*, 18(1):41–74, 2017.

Leonardo Bonilla, Catalina Granda, Luz Adriana Flórez, Didier Hermida, Francisco Lasso, Leonardo Fabio Morales, et al. Pérdida de dinamismo en la ocupación y el impacto del programa de certificación de habilidades del sena. *Reporte del Mercado Laboral-No. 29*, 2024.

Brantly Callaway and Pedro H.C. Sant'Anna. Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230, 2021. doi: 10.1016/j.jeconom.2020.12.001.

Adriana Camacho, Emily Conover, and Alejandro Hoyos. Effects of Colombia's social protection system on workers' choice between formal and informal employment. *The World Bank Economic Review*, 28(3):446–466, 2014.

Ana Corbacho, Vicente Fretes Cibils, and Eduardo Lora, editors. *More than Revenue: Taxation as a development tool*. Springer, 2013.

Guillermo Cruces, Sebastian Galiani, and Susana Kidyba. Payroll taxes, wages and employment: Identification through policy changes. *Labour Economics*, 17(4):743 – 749, 2010.

Clément de Chaisemartin and Xavier D'Haultfœuille. Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–96, September 2020. doi: 10.1257/aer.20181169.

Cristina Fernández and Leonardo Villar. The impact of lowering the payroll tax on informality in colombia. *Economía*, 18(1):125–155, 2017.

Luz A Flórez, Leonardo Fabio Morales, Daniel Medina, and José Lobo. Labor flows across firm size, age, and economic sector in colombia vs. the united states. *Small Business Economics*, pages 1–32, 2020.

Luz A Flórez, Didier Hermida, and Leonardo Fabio Morales. The heterogeneous effect of minimum wage on labor market flows in colombia. *Borradores de Economía; No. 1213*, 2022.

Andrew Goodman-Bacon and Jan Marcus. Using difference-in-differences to identify causal effects of covid-19 policies. *Survey Research Methods*, 14(2):153–158, June 2020. doi: 10.18148/srm/2020.v14i2.7723.

Jonathan Gruber. The incidence of payroll taxation: Evidence from Chile. *Journal of Labor Economics*, 15(3):S72–101, 1997.

Jonathan Gruber. Payroll taxation, employer mandates, and the labor market. *Employee benefits and labor markets in Canada and the United States*, page 183, 2000.

Jonathan Gruber and Alan B Krueger. The incidence of mandated employer-provided insurance: Lessons from workers' compensation insurance. In *Tax Policy and the Economy, Volume 5*, pages 111–144. The MIT Press, 1991.

Kosuke Imai and In Song Kim. On the use of two-way fixed effects regression models for causal inference with panel data. *Political Analysis*, 29(3):405–415, 2021. doi: 10.1017/pan.2020.33.

Francis Kramarz and Thomas Philippon. The impact of differential payroll tax subsidies on minimum wage employment. *Journal of Public Economics*, 82(1):115 – 146, 2001.

Adriana Kugler and Maurice Kugler. Labor market effects of payroll taxes in developing countries: Evidence from Colombia. *Economic Development and Cultural Change*, 57(2): pp. 335–358, 2009.

Rafael La Porta and Andrei Shleifer. Informality and development. *Journal of Economic Perspectives*, 28(3):109–26, 2014.

S. Levy. *Good intentions, bad outcomes: Social policy, informality, and economic growth in Mexico*. Brookings Inst Press, 2008.

William Maloney and Jairo Nuñez. Measuring the impact of minimum wages. Evidence from Latin America. In *Law and Employment: Lessons from Latin America and the Caribbean*, pages 109–130. University of Chicago Press, 2004.

William F Maloney. Informality revisited. *World Development*, 32(7):1159 – 1178, 2004.

Costas Meghir, Renata Narita, and Jean-Marc Robin. Wages and informality in developing countries. *American Economic Review*, 105(4):1509–46, 2015.

Camilo Mondragón-Vélez, Ximena Peña, and Daniel Wills. Labor market rigidities and informality in Colombia [with comment]. *Economía*, 11(1):65–101, 2010.

Leonardo Fabio Morales and Carlos Medina. Assessing the effect of payroll taxes on formal employment: The case of the 2012 tax reform in colombia. *Economía*, 18(1):75–124, 2017.

OECD. *Taxing Wages*. OECD Publishing, 2016.

Guillermo E. Perry, William F. Maloney, Omar S. Arias, Pablo Fajnzylber, Andrew D. Mason, and Jaime Saavedra-Chanduvi. *Informality: Exit and exclusion*. World Bank Publications, 2007.

Ashesh Rambachan and Jonathan Roth. A more credible approach to parallel trends. *The Review of Economic Studies*, 90(5):2555–2591, 02 2023. doi: 10.1093/restud/rdad018.

Emmanuel Saez, Benjamin Schoefer, and David Seim. Payroll taxes, firm behavior, and rent sharing: Evidence from a young workers' tax cut in Sweden. *American Economic Review*, 109(5):1717–63, May 2019. doi: 10.1257/aer.20171937.

Mauricio Santa María, Fabián García, and Ana Virgina Mujica. Los costos no laborales y el mercado laboral: impacto de la reforma de salud en Colombia. Working Paper Series 009186, Fedesarrollo, August 2009.

Lawrence H. Summers. Some simple economics of mandated benefits. *The American Economic Review*, 79(2):pp. 177–183, 1989.

Gabriel Ulyssea. Regulation of entry, labor market institutions and the informal sector. *Journal of Development Economics*, 91(1):87–99, January 2010.

Gabriel Ulyssea. Informality: Causes and consequences for development. *Annual Review of Economics*, 12, 2020. ISSN 19411391. doi: 10.1146/annurev-economics-082119-121914.

Appendix

Appendix 1: DID Regression Heteronenous Effects by Sector GEIH

	Sector 1			Sector 2			Sector 3		
	Employment	Wage	Employment to Population Ratio	Employment	Wage	Employment to Population Ratio	Employment	Wage	Employment to Population Ratio
	(4)	(4)	(4)	(4)	(4)	(4)	(4)	(4)	(4)
TreatN x Post	0.0124	-0.0716	0.0023	0.1302***	-0.0047	0.0058	0.0736***	0.0104	0.0122**
	(0.1163)	(0.0664)	(0.0148)	(0.0422)	(0.0313)	(0.0120)	(0.0217)	(0.0166)	(0.0046)
Education	0.0558***	0.0819***	0.0091**	0.0098	0.0919***	0.0078*	0.0563***	0.0539***	0.0167***
	(0.0140)	(0.0175)	(0.0036)	(0.0090)	(0.0116)	(0.0039)	(0.0142)	(0.0161)	(0.0027)
Gender	-0.2745	0.1449	-0.0121	0.0172	0.0069	0.0111	0.2476***	0.1768	0.0883***
	(0.2361)	(0.1239)	(0.0306)	(0.0941)	(0.0576)	(0.0285)	(0.0713)	(0.1044)	(0.0172)
Household head	0.8016***	0.3197**	0.0686***	0.0562	0.1341	0.0229	0.3918***	0.1079	0.0815***
	(0.1639)	(0.1366)	(0.0219)	(0.0884)	(0.0991)	(0.0324)	(0.1322)	(0.1232)	(0.0226)
Firm size: 21 to 50 employees	2.5325***	-0.4072	0.3368***	1.3154***	0.2160	0.5599***	1.6012***	0.4434*	0.4042***
	(0.3661)	(0.3142)	(0.0583)	(0.1335)	(0.1314)	(0.0362)	(0.1923)	(0.2423)	(0.0401)
Firm size: 51 to 100 employees	3.4167***	-0.8700	0.5394***	1.5895***	0.5270***	0.6124***	1.5454***	0.8860***	0.3741***
	(0.4444)	(0.5638)	(0.0802)	(0.1853)	(0.1648)	(0.0407)	(0.2004)	(0.2658)	(0.0349)
Firm size: 101 employees or more	4.3293***	1.1146***	0.6631***	1.8566***	0.5158***	0.7820***	1.7965***	0.8930***	0.3912***
	(0.2203)	(0.1851)	(0.0300)	(0.1109)	(0.1215)	(0.0260)	(0.1475)	(0.0839)	(0.0221)
Constant	6.7783***	13.1672***	-0.0105	8.4053***	12.8065***	0.0661	8.8516***	13.1383***	-0.1313***
	(0.1421)	(0.1580)	(0.0236)	(0.1385)	(0.1191)	(0.0517)	(0.1404)	(0.1676)	(0.0256)
Observations	1,239	1,239	1,239	1,286	1,286	1,286	1,292	1,292	1,292
R-squared	0.4591	0.3355	0.5498	0.6729	0.6177	0.7072	0.8399	0.7782	0.8656
FE Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Period	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This Table investigates whether the employment effect of the First Job Act is heterogeneous by economic sector. This Table presents the estimation results for the employment, wage, occupation, and participation effects for the sample of cohorts in formal-sector firms between September 2009 and June 2013 (see equation (1)); for this estimation, we use survey data from GEIH. Specification includes control variables and age, cohort, and time-fixed effects. Employment to population rate is the ratio of employed population to working age population. Labor force participation rate is the ratio labor-force participants to working age population. Standard errors are clustered by a combination of cohort-by-age groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 2: DID Regression Heteronenous Effects by Sector PILA

	Sector 1			Sector 2			Sector 3		
	Employment (4)	Wage (4)	Hires (4)	Employment (4)	Wage (4)	Hires (4)	Employment (4)	Wage (4)	Hires (4)
TreatN x Post	0.0640***	0.0009	0.0058	0.0457***	0.0111**	0.0395***	0.0481***	0.0094***	0.0107
	(0.0120)	(0.0096)	(0.0183)	(0.0088)	(0.0043)	(0.0123)	(0.0056)	(0.0032)	(0.0068)
Gender	-0.1194	0.1555	0.2829	-0.4465*	0.1279	0.2414	-1.1287*	-0.0433	-0.6105
	(0.1205)	(0.1112)	(0.2190)	(0.2515)	(0.1653)	(0.2944)	(0.6818)	(0.2857)	(0.8104)
Firm age: 2 to 4 years	-0.5287***	0.3725*	-0.5966	-0.4789**	-0.1742	-0.4117	0.4360	0.1526	0.9893**
	(0.1891)	(0.2105)	(0.4729)	(0.2096)	(0.1545)	(0.3389)	(0.4239)	(0.2160)	(0.4819)
Firm age: 4 to 6 years	-0.3421	0.3105	0.0045	0.1553	-0.1890	-0.0189	0.4429	-0.3537	0.7722
	(0.2086)	(0.2430)	(0.4546)	(0.2391)	(0.1510)	(0.3963)	(0.4171)	(0.2594)	(0.5640)
Firm age: 6 years or more	-0.9022***	0.3848**	-1.8621***	-0.2848	-0.0125	-0.9113***	1.4563***	0.0443	1.5635***
	(0.1494)	(0.1640)	(0.3777)	(0.1951)	(0.1445)	(0.3085)	(0.4721)	(0.2275)	(0.5657)
Firm size: 21 to 50 employees	0.2072*	0.2028	0.1334	-0.0976	0.1012	-0.3039*	0.5736**	-0.0494	0.0347
	(0.1235)	(0.1413)	(0.3009)	(0.1007)	(0.0940)	(0.1810)	(0.2677)	(0.1427)	(0.3269)
Firm size: 51 to 100 employees	0.2609*	0.1997	-0.0242	0.1498	0.2850**	-0.3920*	0.2959	0.0581	-0.0116
	(0.1449)	(0.1482)	(0.3602)	(0.1423)	(0.1246)	(0.2332)	(0.3299)	(0.1637)	(0.4305)
Firm size: 101 employees or more	0.2894***	0.5296***	0.1425	-0.0276	0.4956***	-1.0963***	0.2560	0.1822	-0.3057
	(0.1041)	(0.1164)	(0.2622)	(0.1260)	(0.0999)	(0.1733)	(0.3150)	(0.1505)	(0.3219)
Constant	6.9075***	13.5052***	5.5841***	7.8022***	13.8306***	6.7140***	8.0099***	14.0102***	6.2615***
	(0.1521)	(0.1780)	(0.3714)	(0.2977)	(0.1928)	(0.3688)	(0.5209)	(0.2143)	(0.5317)
Observations	2,832	2,832	2,829	2,832	2,832	2,832	2,832	2,832	2,832
R-squared	0.9929	0.9804	0.9613	0.9948	0.9910	0.9869	0.9976	0.9973	0.9970
FE Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Period	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This Table investigates whether the employment effect of the First Job Act is heterogeneous by economic sector. This Table presents the estimation results for the employment, wage, and hiring for the sample of cohorts in formal-sector firms between September 2009 and June 2013 (see equation (1)); for this estimation, we use survey data from PILA. Specification includes control variables and age, cohort, and time-fixed effects. Standard errors are clustered by a combination of cohort-by-age groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 3: DID Regression Heterogeneous Effects by Firm Size GEIH

	Size 1: Up to 20 employees			Size 2: 21-50 employees			Size 3: 51-100 employees			Size 4: More than 100 employees		
	Employment	Wage	Employment to Population Ratio	Employment	Wage	Employment to Population Ratio	Employment	Wage	Employment to Population Ratio	Employment	Wage	Employment to Population Ratio
	(4)	(4)	(4)	(4)	(4)	(4)	(4)	(4)	(4)	(4)	(4)	(4)
TreatN x Post	0.1169*	0.0048	0.0072	-0.0461	0.0564**	0.0015	0.2132**	-0.0136	0.0184	0.0460	-0.0108	0.0130
	(0.0579)	(0.0253)	(0.0065)	(0.0621)	(0.0216)	(0.0315)	(0.0858)	(0.0432)	(0.0259)	(0.0354)	(0.0164)	(0.0079)
Education	0.1023***	0.0582***	0.0127***	0.0245**	0.0693***	0.0185***	-0.0168	0.0753***	0.0060*	0.0310***	0.1224***	0.0093**
	(0.0170)	(0.0112)	(0.0019)	(0.0107)	(0.0104)	(0.0040)	(0.0110)	(0.0077)	(0.0033)	(0.0100)	(0.0114)	(0.0034)
Gender	0.3062	0.0936	0.0525**	0.2432***	0.1962***	0.0658**	0.0624	0.1367**	-0.0018	-0.0837	0.0221	0.0162
	(0.1970)	(0.1290)	(0.0202)	(0.0667)	(0.0528)	(0.0300)	(0.0910)	(0.0530)	(0.0220)	(0.0865)	(0.0528)	(0.0251)
Household head	0.4970**	0.2915***	0.0393	-0.0280	0.0501	-0.0135	0.0447	0.0887*	0.0121	0.1619***	0.1688***	0.0391*
	(0.2119)	(0.0979)	(0.0243)	(0.0891)	(0.0530)	(0.0252)	(0.0818)	(0.0460)	(0.0201)	(0.0567)	(0.0351)	(0.0194)
Sector 1	-0.3196	0.3720**	-0.0552*	0.0921	0.2867**	-0.2993***	0.0466	0.1865**	-0.2965***	0.3250**	0.2635**	-0.1206*
	(0.2302)	(0.1419)	(0.0283)	(0.1085)	(0.1091)	(0.0439)	(0.1406)	(0.0694)	(0.0506)	(0.1459)	(0.1083)	(0.0618)
Sector 3	-0.3349	0.1903	-0.0303	-0.1024	-0.0111	-0.0415	-0.1101	0.0051	-0.0932***	-0.2021***	-0.0588	-0.1337***
	(0.3335)	(0.1206)	(0.0406)	(0.0725)	(0.0576)	(0.0336)	(0.0710)	(0.0418)	(0.0255)	(0.0713)	(0.0736)	(0.0274)
Constant	8.0966***	12.7737***	-0.0076	8.3042***	13.0353***	0.5427***	8.2783***	13.0543***	0.8559***	9.8685***	12.7171***	0.8228***
	(0.3205)	(0.1162)	(0.0394)	(0.1731)	(0.1207)	(0.0472)	(0.1478)	(0.1287)	(0.0517)	(0.1702)	(0.1362)	(0.0549)
Observations	1,289	1,289	1,289	1,278	1,278	1,278	1,263	1,263	1,263	1,291	1,291	1,291
R-squared	0.4696	0.4950	0.3644	0.3707	0.4711	0.2424	0.2397	0.4309	0.1595	0.7480	0.7931	0.2094
FE Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Period	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This Table investigates whether the employment effect of the First Job Act is heterogeneous by firm size. This Table presents the estimation results for the employment, wage, occupation, and participation effects for the sample of cohorts in formal-sector firms between September 2009 and June 2013 (see equation (1)); for this estimation, we use survey data from GEIH. Specification includes control variables and age, cohort, and time-fixed effects. Employment to population rate is the ratio of employed population to working age population. The labor force participation rate is the ratio of labor force participants to the working-age population. Standard errors are clustered by a combination of cohort-by-age groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 4: DID Regression Heterogeneous Effects by Firm Size PILA

	Size 1: Up to 20 employees			Size 2: between 21 and 50 employees			Size 3: between 51 and 100 employees			Size 4: more than 100 employees		
	Employment (4)	Wage (4)	Hires (4)	Employment (4)	Wage (4)	Hires (4)	Employment (4)	Wage (4)	Hires (4)	Employment (4)	Wage (4)	Hires (4)
TreatN x Post	0.1269***	0.0006	0.1118***	0.1299***	0.0122*	0.0881***	0.1229***	0.0125	0.0937***	0.0804***	-0.0110*	0.0682**
	(0.0311)	(0.0046)	(0.0300)	(0.0267)	(0.0069)	(0.0233)	(0.0313)	(0.0103)	(0.0263)	(0.0284)	(0.0063)	(0.0267)
Gender	0.4756	-0.0107	0.7350	0.4499	0.0974	0.3809	0.2297	-0.1203	0.3408	0.0940	0.9518***	-1.6613*
	(1.0232)	(0.0482)	(0.9740)	(0.4718)	(0.0819)	(0.3678)	(0.4086)	(0.1146)	(0.5296)	(0.8062)	(0.2572)	(0.8670)
Sector1	-1.5758	0.1399	-1.0053	-1.7090	0.6065**	-2.5018*	-0.3146	0.3889**	0.8154	9.0916*	-1.6577**	9.5156**
	(1.2206)	(0.1306)	(1.0558)	(1.3466)	(0.2709)	(1.4323)	(0.6474)	(0.1705)	(0.8021)	(5.4551)	(0.7550)	(4.5320)
Sector3	-0.7852	0.0702*	-0.7953	-0.5145	0.0131	-0.7448	0.7527	-0.3536**	0.7124	-0.6529	0.0898	0.2487
	(0.6718)	(0.0396)	(0.8737)	(0.3955)	(0.0638)	(0.4626)	(0.6775)	(0.1553)	(0.7450)	(2.9891)	(0.2407)	(2.3998)
Firm age: 2 to 4 years	0.9610	0.0489	0.9853	-2.4838***	-0.6182**	-2.7349**	0.5720	0.0807	1.4083*	-0.9416	-0.8467	-2.6541
	(0.9320)	(0.0901)	(0.9885)	(0.6990)	(0.2929)	(1.2832)	(0.6192)	(0.1899)	(0.7696)	(7.6946)	(1.4669)	(8.3000)
Firm age: 4 to 6 years	1.0639	0.0089	2.2524*	-0.9200	-0.8134**	-0.2915	-0.9952	0.7221**	0.5112	1.2619	2.0740	-2.9358
	(0.7660)	(0.1005)	(1.2701)	(1.1766)	(0.3315)	(1.7594)	(1.3203)	(0.3599)	(1.1261)	(4.5595)	(1.7049)	(4.6182)
Firm age: 6 years or more	-0.8697	0.0127	-0.9007**	-2.0832***	-0.3158	-2.4610**	-2.1230*	0.6019***	-1.5476	1.5238	-0.4364	-2.0889
	(0.6654)	(0.0870)	(0.4384)	(0.7762)	(0.2939)	(1.2362)	(1.0827)	(0.1728)	(1.2473)	(6.9871)	(1.3226)	(7.3488)
Constant	8.0187***	13.6905***	6.2947***	8.7901***	14.1656***	7.8681***	7.6327***	13.6904***	5.3662***	7.1041	13.8927***	9.2586
	(0.8916)	(0.0831)	(1.0032)	(0.9846)	(0.3081)	(1.1435)	(1.1369)	(0.2059)	(1.3122)	(8.2533)	(1.4293)	(8.2239)
Observations	1,296	1,296	1,296	1,296	1,296	1,296	1,296	1,296	1,294	1,296	1,296	1,296
R-squared	0.9713	0.9901	0.9560	0.9731	0.9821	0.9522	0.9729	0.9675	0.9434	0.9732	0.9929	0.9633
FE Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Period	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This Table investigates whether the employment effect of the First Job Act is heterogeneous by firm size. This Table presents the estimation results for the employment, wage, and hiring for the sample of cohorts in formal-sector firms between September 2009 and June 2013 (see equation (1)); for this estimation, we use survey data from PILA. Specification includes control variables and age, cohort, and time-fixed effects. Standard errors are clustered by a combination of cohort-by-age groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 5: DID Regression Heterogeneous Effects by Firm Age PILA

	Fage 1: Up to 20 years			Fage 2: between 2 and 4 years			Fage 3: between 4 and 6 years			Fage 4: more than 6 years		
	Employment (4)	Wage (4)	Hires (4)	Employment (4)	Wage (4)	Hires (4)	Employment (4)	Wage (4)	Hires (4)	Employment (4)	Wage (4)	Hires (4)
TreatN x Post	0.1655***	-0.0345**	0.0855***	0.1241***	0.0124	0.0948**	0.1211***	0.0036	0.1222***	0.1204***	-0.0012	0.0938***
	(0.0367)	(0.0141)	(0.0305)	(0.0386)	(0.0084)	(0.0407)	(0.0349)	(0.0070)	(0.0350)	(0.0252)	(0.0055)	(0.0262)
Gender	-0.2139	-0.0284	-0.0446	0.2713	0.0036	-0.1010	0.7410	-0.0197	1.1642*	-0.4038	0.8547**	-1.7070
	(0.2719)	(0.0394)	(0.4332)	(0.4729)	(0.0861)	(0.8178)	(0.5729)	(0.0572)	(0.5881)	(1.2998)	(0.3511)	(1.3303)
Sector1	1.0288	0.2144**	0.7963	-1.9378***	0.0381	-3.0031***	-1.4631	0.0033	-0.8186	2.3776	-1.5911*	3.4912
	(0.6525)	(0.0976)	(0.6572)	(0.6368)	(0.0994)	(0.7532)	(0.9866)	(0.0765)	(1.5389)	(2.4511)	(0.8457)	(2.6534)
Sector3	-0.4836	0.1202*	0.0488	-0.5291	-0.0140	-0.6405**	-1.0945	0.0232	-1.0749	-1.3087	0.3469	0.7061
	(0.3737)	(0.0661)	(0.2978)	(0.3190)	(0.0777)	(0.2590)	(0.6590)	(0.0586)	(0.8482)	(0.7946)	(0.4009)	(1.1368)
Firm size: 21 to 50 employees	0.2313	0.0067	0.3499	-0.8105***	-0.2814**	-0.7875**	-0.2357	0.0566	0.6798	-0.5513	1.6806*	-0.9239
	(0.1710)	(0.0640)	(0.2372)	(0.2826)	(0.1256)	(0.3806)	(0.6103)	(0.0714)	(0.7854)	(2.0579)	(0.8693)	(2.4097)
Firm size: 51 to 100 employees	0.7690	-0.0055	0.6959	-0.1308	-0.4523***	0.5950	0.6087	-0.0350	0.7791	1.6249	0.8068	1.0807
	(0.6034)	(0.2147)	(0.5818)	(0.4191)	(0.1632)	(0.5881)	(0.8904)	(0.0709)	(1.0392)	(2.3142)	(0.8667)	(2.3318)
Firm size: 101 employees+	-0.0751	0.1784*	-0.2786	0.2468	-0.0388	0.2062	0.8298**	0.1124**	0.3775	6.9590***	0.9841***	5.7671***
	(0.3286)	(0.0930)	(0.3475)	(0.4587)	(0.0499)	(0.5068)	(0.3339)	(0.0542)	(0.4793)	(1.6927)	(0.2748)	(1.5710)
Constant	5.6361***	13.6201***	4.3687***	6.4412***	13.8530***	5.6210***	6.3075***	13.7535***	4.7163***	5.1071**	12.4598***	3.4650
	(0.3757)	(0.0549)	(0.3737)	(0.3861)	(0.0715)	(0.7185)	(0.8662)	(0.0637)	(0.9382)	(2.1232)	(0.5890)	(2.2684)
Observations	1,296	1,296	1,290	1,296	1,296	1,296	1,294	1,294	1,293	1,296	1,296	1,296
R-squared	0.9508	0.7210	0.9122	0.9572	0.9438	0.9231	0.9646	0.9644	0.9308	0.9832	0.9945	0.9761
FE Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Period	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This Table investigates whether the employment effect of the First Job Act is heterogeneous by firm age. This Table presents the estimation results for the employment, wage, and hiring for the sample of cohorts in formal-sector firms between September 2009 and June 2013 (see equation (1)); for this estimation, we use survey data from PILA. Specification includes control variables and age, cohort, and time-fixed effects. Standard errors are clustered by a combination of cohort-by-age groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 6: DID Regression at the Firm Levels

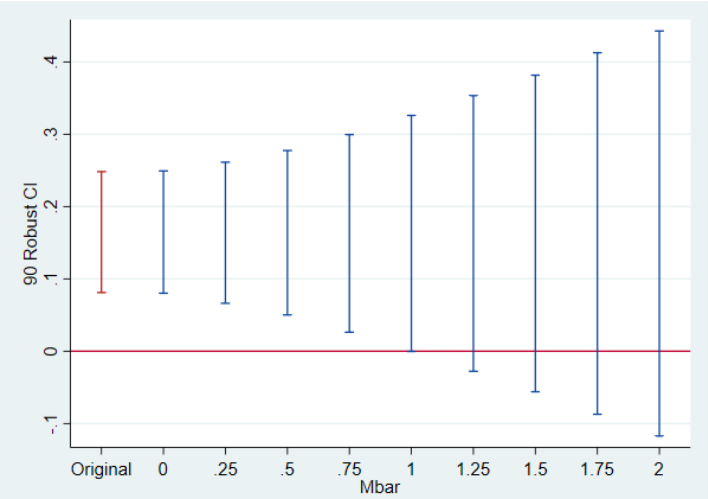
Treatment by CIIU

	Employment						Wages	
	All		<28 years old		>= 28 years old			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat x Post	0.0151***	0.0154***	0.0250***	0.0260***	0.0243***	0.0222***	0.0013	0.0016
	(0.0030)	(0.0030)	(0.0062)	(0.0062)	(0.0035)	(0.0035)	(0.0013)	(0.0013)
2 sector		-0.0320		-0.0963*		-0.0286		0.0019
		(0.0334)		(0.0525)		(0.0358)		(0.0169)
3 sector		0.0300***		0.0240		0.0339***		-0.0109***
		(0.0097)		(0.0198)		(0.0112)		(0.0040)
4 sector		0.0610		0.1542		0.0426		-0.0305
		(0.0456)		(0.0967)		(0.0453)		(0.0187)
5 sector		0.0386***		0.0239		0.0404**		-0.0078
		(0.0148)		(0.0239)		(0.0163)		(0.0052)
6 sector		0.0200**		0.0189		0.0164*		-0.0148***
		(0.0085)		(0.0184)		(0.0098)		(0.0035)
7 sector		0.0137		-0.0151		0.0182		-0.0181***
		(0.0141)		(0.0250)		(0.0155)		(0.0052)
8 sector		0.0219		0.0266		0.0250		-0.0038
		(0.0159)		(0.0298)		(0.0177)		(0.0075)
9 sector		0.0154**		0.0059		0.0132		-0.0164***
		(0.0073)		(0.0171)		(0.0084)		(0.0031)
10 sector		0.0197***		0.0057		0.0204**		-0.0164***
		(0.0071)		(0.0169)		(0.0080)		(0.0031)
% male workers		0.2950***		0.3673***		0.3277***		0.0223***
		(0.0100)		(0.0150)		(0.0118)		(0.0036)
Real GDP departmental		0.0168*		0.0373**		0.0189*		0.0014
		(0.0098)		(0.0158)		(0.0108)		(0.0029)
Constant	2.0238***	1.6661***	1.0632***	0.4210**	1.5709***	1.1565***	13.7472***	13.7327***
	(0.0015)	(0.1099)	(0.0032)	(0.1773)	(0.0017)	(0.1213)	(0.0006)	(0.0325)
Observations	4,105,214	3,804,406	2,326,788	2,318,178	4,019,729	3,745,944	3,801,155	3,801,155
R-squared	0.9177	0.9195	0.8447	0.8481	0.9017	0.9089	0.9131	0.9131
FE Firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Period	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes

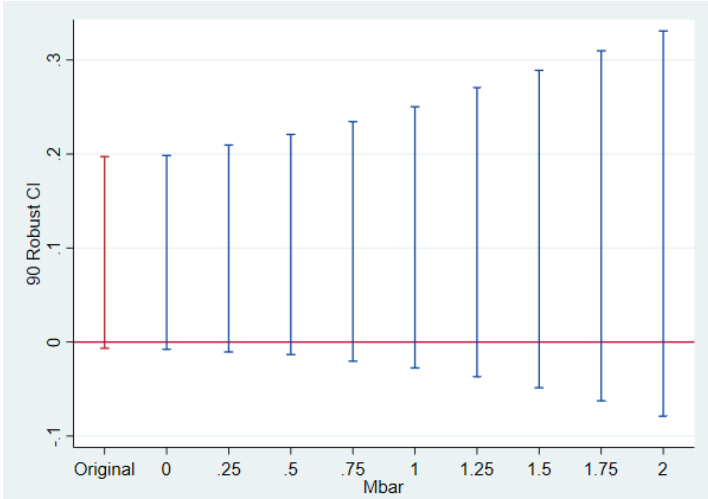
Notes: This Table presents the estimation results for the employment and wage effects for the sample of formal-sector firms between September 2009 and June 2013 (see equation (3)); for this estimation, we use survey data from PILA. Odd specifications include cohort and time-fixed effects; even specifications include control variables as well. Specifications (3) and (4) restrict the sample to workers under 28 years old; Specifications (5) and (6) restrict the sample to workers aged 28 or older. Standard errors (in parenthesis) are clustered at the firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 7: Parallel Trends Sensibility with GEIH Data

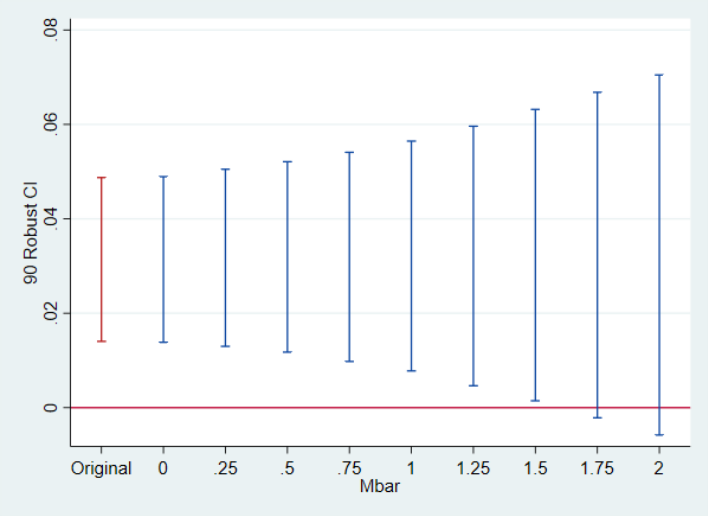
(a) Log of Formal Employment



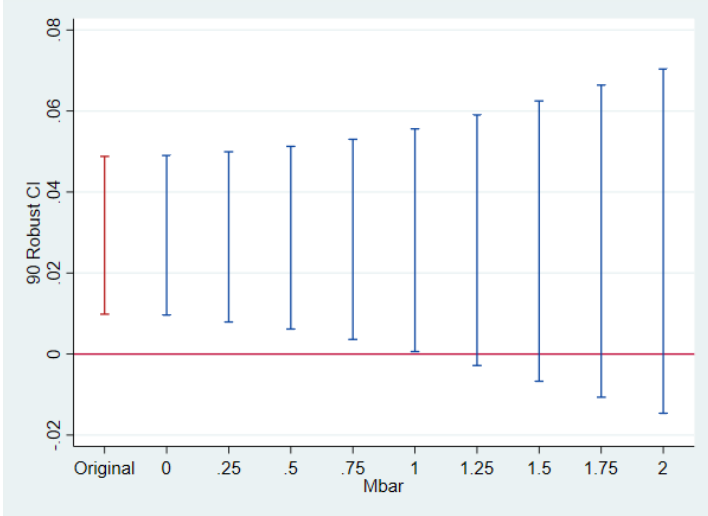
(b) Log of Wages



(c) Employment to Population Rate



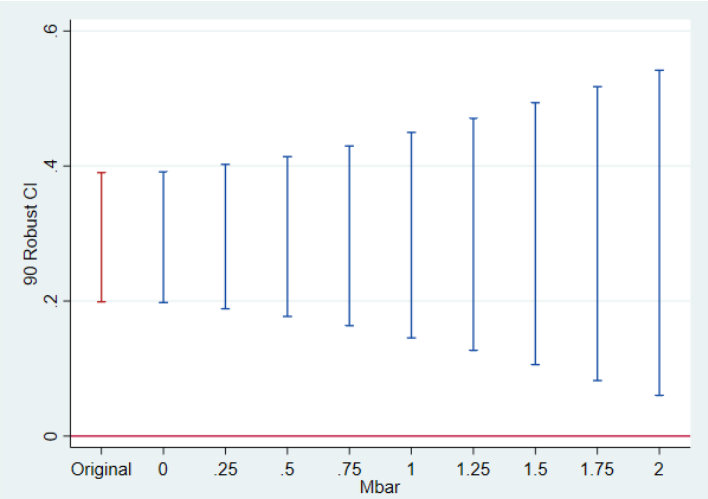
(d) Labor Force Participation Rate



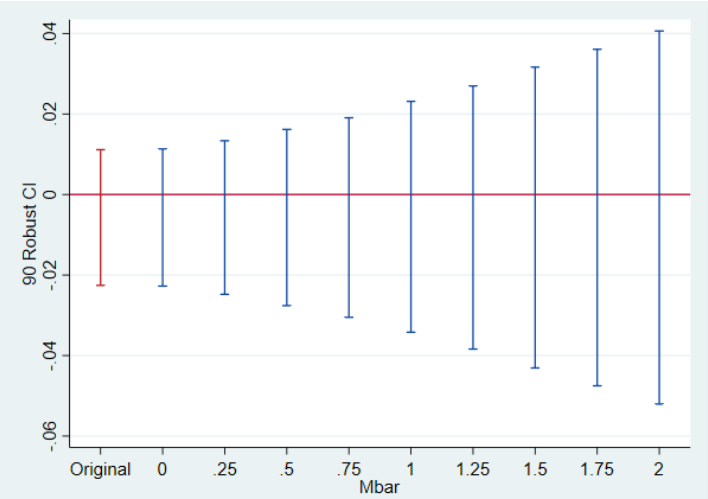
Notes: These graphs show confidence intervals for the estimated impact in Q2-2013 for each outcome, which are robust to violations of the parallel trends assumptions in the post-treatment period of up to M-bar times the maximum difference in trends observed during the pre-treatment period); for this estimation, we use survey data from GEIH. Confidence bands are 95 percent confidence intervals.

Appendix 8: Parallel Trends Sensibility with PILA Data

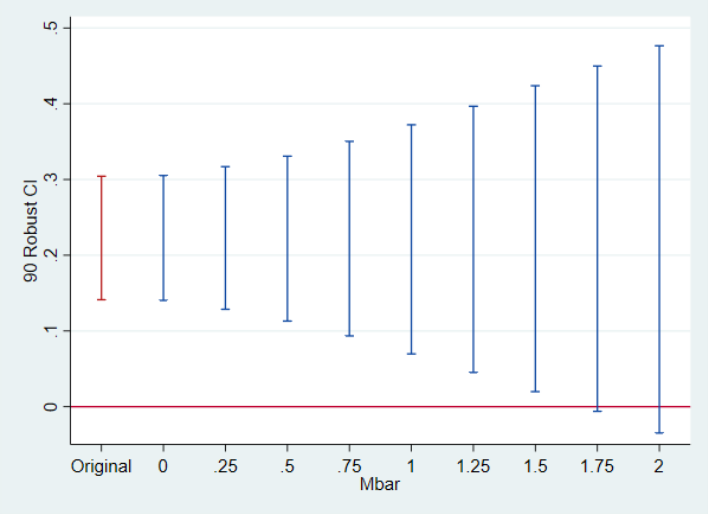
(a) Log of Formal Employment



(b) Log of Wages



(c) Lof of hires



Notes: These graphs show confidence intervals for the estimated impact in Q2-2013 for each outcome, which are robust to violations of the parallel trends assumptions in the post-treatment period of up to M-bar times the maximum difference in trends observed during the pre-treatment period); for this estimation, we use survey data from PILA. Confidence bands are 95 percent confidence intervals.

Appendix 9: Heterogenous Effects DID Regressions

	PILA			GEIH		
	Log Employment	*Log Wage*	*Log Hire*	*Log Employment*	*Log Wage*	*Employment to Population Ratio*
A. Baseline	0.1285**	-0.0026	0.0919**	0.0768**	0.0061	0.0111**
Treatment x Post	(0.0248)	(0.0040)	(0.0242)	(0.0217)	(0.0224)	(0.0041)
TEST	0.1140**	0.0002	0.0891**	0.0766*	0.0143	0.0119*
(did_multiplegt)	(0.0304)	(0.0045)	(0.0396)	(0.0438)	(0.0530)	(0.0072)
Observations	1296	1296	1296	1294	1294	1294

Notes: : Each panel's columns display the baseline specification (3) for each labor demand outcome and wages, as shown in Tables 3 and 4. The term Treatment x Post denotes the standard DID coefficient, while Treatment x Post (multipleg) refers to the DID estimator using the methodology of De Chaisemartin and D'Haultfoeuille (2020). This test calculates the minimal standard deviation of group-specific Average Treatment Effects that aligns with a standard DID estimate of the opposite sign to the actual population effect, considering a reasonable degree of treatment effect heterogeneity. The further this standard deviation is from zero, the lower the concern for a biased standard DID coefficient. Assuming beta represents the DID baseline coefficient, the methodology posits a value B, which, in absolute terms, is greater than the effect in every group and period. If $|beta| < sqrt(3)x$ and $B < sqrt(3)x$, where x is the test parameter value, then x would represent an implausibly high level of treatment effect heterogeneity, potentially causing beta to have a different sign than the true treatment effect. The assumed B value would be several times the estimated beta in all estimations in the table to have a coefficient of the opposite sign to the true value.. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A Conceptual framework

The literature examining the incidence of payroll taxes has a long history. The framework is set out by Summers (1989) and Gruber and Krueger (1991). It emphasizes that the incidence of payroll taxes depends on how much they can be passed through to wages. In particular, if a change in wages offsets a change in payroll taxes, payroll taxes do not create distortions in the labor market. Additionally, the extent to which payroll taxes are passed through to wages depends on the elasticity of labor supply and the tax-benefit relationship, i.e., workers' valuation of the benefits they receive from payroll taxes.

In a competitive labor market with homogeneous agents and employer payroll taxes, the following relationship gives the market equilibrium:

$$D\left(w\left(1+t\right)\right) = S\left(w\left(1+\alpha t\right)\right) \tag{4}$$

where $D\left(w\left(1+t\right)\right)$ and $S\left(w\left(1+\alpha t\right)\right)$ represent aggregate labor demand and supply, w is the equilibrium wage, t is the employer payroll tax rate, and α is workers' valuation of payroll tax-financed benefits. Including the worker's valuation of the benefits implies that the worker's wage and the cost the firm pays for that worker are different because the firm must pay the payroll tax rate t. Similarly, the worker's perceived benefits are higher than the worker's wage, since the worker receives the wage plus the benefits financed by the payroll tax, which the worker values at the rate α. As a result, labor demand is a function of total labor cost $w\left(1+t\right)$, while labor supply is a function of total worker compensation $w\left(1+\alpha t\right)$.

Gruber (1997) shows that, under this setup, a change in the employer payroll tax has effects on the equilibrium level of employment and wages. Using total differentiation on

equation (4), the response of the equilibrium wages and employment to a change in the employer payroll tax is given by:

$$\frac{\frac{dw}{w}}{dt} = \frac{\alpha\,(1+t)\,\varphi - (1+\alpha t)\,\eta}{(\eta-\varphi)\,(1+\alpha t)\,(1+t)} \tag{5}$$

$$\frac{\frac{dD}{D}}{dt} = \eta\left(\frac{\frac{dw}{w}}{dt} + \frac{1}{1+t}\right) = \frac{\eta}{1+t}\left(\frac{\varphi\,(\alpha-1)}{(\eta-\varphi)\,(1+\alpha t)}\right). \tag{6}$$

where, in equations (5) and (6), $\eta = D'\frac{w(1+t)}{D}$ and $\varphi = S'\frac{w(1+\alpha t)}{S}$ stand for the elasticity of labor demand and supply respectively.[5]

From the previous analysis, employer payroll taxes do not have effects on employment (i.e., $\frac{dD}{D}/dt = 0$) as long as the pass-through from taxes to wages is equal to $-\frac{1}{1+t}$. This previous result holds in two cases: First, if labor supply is perfectly inelastic ($\varphi = 0$), then all the incidence of payroll taxes is on workers. Second, if the worker's valuation from the payroll tax equals the cost paid by the employer ($\alpha = 1$), then the tax increase is offset by a proportional wage reduction, leaving employment unchanged (Summers, 1989).

Along with an inelastic labor supply and a one-to-one tax-benefit link, a full pass-through from payroll taxes to wages requires that wages can adjust to changes in payroll taxes. However, this is not always the case in developing economies. In particular, a binding minimum wage and a large informal (unregulated) sector prevent wages from adjusting to tax changes, which generates employment effects even when the labor supply is inelastic.

A binding minimum wage is a common characteristic of Latin American economies, particularly Colombia (Bell, 1997; Maloney and Nuñez, 2004; Flórez et al., 2022). If the minimum wage is binding, payroll taxes cannot pass through to wages, and the incidence of payroll taxes is borne by employers (Gruber, 2000). The extent of the effect of the minimum wage on tax incidence depends on how binding the minimum wage is, a country-specific effect. For example, in Gruber's (1997) examination of payroll taxes in Chile, he argues that the minimum wage is not a relevant factor given that it is relatively low and affects only a small fraction of workers. In contrast, Kugler and Kugler (2009) find a limited pass-through in Colombia, which is consistent with the fact that the minimum wage is binding for a large fraction of Colombian workers (Bell, 1997).

The model presented above predicts that when the minimum wage is binding, the employment effect of an increase in payroll taxes is negative. Using equation (6), the employment effect of a change in payroll taxes would be given by the following expression:

$$\frac{\frac{dD}{D}}{dt} = \frac{\eta}{1+t}. \tag{7}$$

A second characteristic frequently found in developing economies is a formal (regulated) sector co-existing with an informal sector. Typically, the informal sector comprises small firms and self-employed workers that survive in the market by evading taxes and other regulations (La Porta and Shleifer, 2014; Meghir et al., 2015). Most remain unregistered because they are not productive enough to afford the cost of regulation, they are small

[5] Equation (6) is obtained from total differentiation of $D\,(w\,(1+t))$. Equation (5), is obtained from total differentiation on both sides of equation (4), using the equilibrium condition $D\,(.) = S\,(.)$; this equation differs from the equation presented by Gruber (1997) because we assume a positive employer payroll tax and a zero employee payroll tax. We show in Section 2.1 that these assumptions are a good approximation for the Colombian case.

enough to avoid detection by tax authorities, or they do not see the benefit of registering (Maloney, 2004; Perry et al., 2007).

The existence of the informal sector may mitigate the pass-through from payroll taxes to wages. To illustrate the effect of the informal sector on the pass-through from payroll taxes to wages, in this section, we follow Levy (2008) and analyze a two-sector labor market where one sector (the informal) does not comply with labor regulation. We assume that workers do not prefer working in either of the two sectors and do not value the benefits from payroll taxes. As a result, the equilibrium wage is the same for both sectors, and the equilibrium in the labor market is given by

$$D^f\left(w\left(1+t\right)\right)+D^i\left(w\right)=S\left(w\right), \tag{8}$$

where $D^i\left(\cdot\right)$ and $D^f\left(\cdot\right)$ represent the labor demand in the formal and informal sector, and $S\left(\cdot\right)$ is the aggregate labor supply.

Figure **??** presents an example of the equilibrium effect of a reduction of payroll taxes in an economy with an informal sector. We assume that the aggregate labor supply is inelastic and equal to L_m, and that a worker always gets a job in the formal or the informal sector. The formal-sector labor demand is represented by the curve D_0^f (drawn from left to right), while the informal-sector labor demand is represented by the curve D_0^i (drawn from right to left, starting at L_m). The initial equilibrium is denoted by the point A, where the wage received by workers in both sectors is the same $\left(w_0^{*i}=w_0^{*f}\right)$.

A reduction of payroll taxes shifts the formal-sector labor demand curve to the right by $\frac{\eta^f}{1+t}dt$ to D_1^f, where η^f stands for the elasticity of formal-sector labor demand and t is the employer payroll tax. In the new equilibrium, reducing payroll taxes increases wages in both sectors and reallocates employment from the informal to the formal sector (point B). Overall, the effect on formal-sector employment caused by the reduction of the payroll taxes $\left(L_1^{f*}-L_0^{f*}\right)$ is smaller than that observed in a case of a binding minimum wage $\left(\frac{\eta^f}{1+t}dt\right)$, but larger than that observed in a case with full pass-through from taxes to wages and no informal sector (0). The magnitude of the effect depends on the relative elasticity of the formal and informal sector labor demand curves.

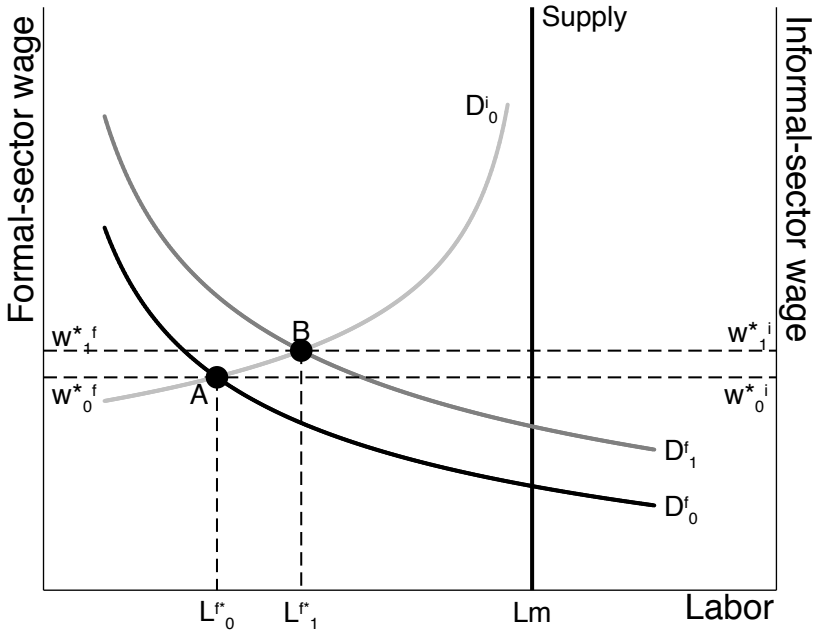
In general, using total differentiation on equation (8), the wage and formal-sector employment effects of a change in payroll taxes are

$$\frac{\frac{dw}{w}}{dt}=\frac{-\delta\eta^f}{\left(\delta\eta^f+\left(1-\delta\right)\eta^i-\varphi\right)\left(1+t\right)} \tag{9}$$

$$\frac{\frac{dD^f}{D^f}}{dt}=\eta^f\left(\frac{\frac{dw^f}{w^f}}{dt}+\frac{1}{1+t}\right)=\frac{\eta^f}{1+t}\left(\frac{\left(1-\delta\right)\eta^i-\varphi}{\delta\eta^f+\left(1-\delta\right)\eta^i-\varphi}\right) \tag{10}$$

where, $\delta=\frac{D^f(w(1+t))}{D^f(w(1+t))+D^i(w)}$ is the fraction of workers employed in the formal sector, and η^f and η^i are the elasticity of labor demand in the formal and informal sectors. Equation (10) implies that payroll taxes have employment effects even with an inelastic aggregate labor supply ($\varphi=0$). Assuming $\varphi=0$, the magnitude of the effect depends on the relative elasticity of labor demand and the size of the formal sector. Payroll taxes do not have an effect on the size of the formal sector if the informal labor demand is relatively inelastic $\left(\frac{\eta^i}{\eta^f}\to 0\right)$ or the size of the informal sector is relatively small ($\delta\to 1$).

Appendix 10: Effect of a reduction of payroll taxes in an economy with an informal sector



Notes: This Figure shows an example of the equilibrium effect of a reduction of payroll taxes in an economy with an informal sector. we assume that the aggregate labor supply is inelastic and equal to L_m, and that a worker always gets a job in either the formal or the informal sector. The formal-sector labor demand is represented by the curve D_0^f (drawn from left to right), while the informal-sector labor demand is represented by the curve D_0^i (drawn from right to left, starting at L_m). The initial equilibrium is denoted by the point A, where the wage received by workers in both sectors is the same $\left(w_0^{*i} = w_0^{*f}\right)$. A reduction of payroll taxes shifts the formal-sector labor demand curve to the right by $\frac{\eta^f}{1+t}dt$ to D_1^f, where η^f stands for the elasticity of formal-sector labor demand and t is the employer payroll tax. In the new equilibrium, the reduction of payroll taxes increases wages in both sectors and reallocates employment from the informal to the formal sector (point B). Overall, the effect on formal-sector employment caused by the reduction of the payroll taxes $\left(L_1^{f*} - L_0^{f*}\right)$ is smaller than that observed in a case of a binding minimum wage $\left(\frac{\eta^f}{1+t}dt\right)$, but larger than that observed in a case with full pass-through from taxes to wages and no informal sector (0).