Outsourcing, Employment and Wages: Evidence from a Policy Reform in Mexico*

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Abstract

This paper investigates the impact of Mexico's labor outsourcing partial ban on formal workers' labor market outcomes. Using longitudinal administrative data and an event-study framework, we analyze the evolution of employment and wages before and after the reform. The findings show that post-reform, regularized workers are more likely to retain their positions in the private formal job sector than those who remained with outsourcing employers or were not part of the regularization process. Moreover, wages for workers directly affected by the reform increased by 3-4%, with significant variation across gender, age, industry, firm size, and region, indicating a heterogeneous response to the reform. These findings underscore the varied impacts of outsourcing restrictions on different segments of the labor market, providing valuable insights into the implications of labor market regulations on employment stability and wage growth.

JEL Classification: J31, J38, J46, J81, K31.

Keywords: Outsourcing, Formal Workers, Labor Market, Employment, Wages.

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1 Introduction

Outsourcing labor services is a form of labor relations that has been increasingly used in many economies in recent decades.[1] In this case, an outsourcing firm offers its workers to perform tasks on another firm's premises, the contracting firm. In other words, the employees work for the contractor, who places them at the contracting firm's disposal to perform activities at this firm location. One example of this type of relation occurs when a software firm hires coders from outsourcing firms. In Mexico, some large firms have incorporated firms specifically for human resources management reasons, adopting the form of outsourcing from the perspective of labor regulation.

Some evidence suggests that outsourcing has disadvantaged employees (Autor, 2009; Appelbaum, 2017; Stansbury and Summers, 2020; Felix and Wong, 2021). For instance, wages in outsourced jobs fall relative to similar jobs that are not outsourced (Goldschmidt and Schmieder, 2017; Drenik et al., 2021). For this reason, outsourcing opponents have forcefully contended the need to regulate this form of labor relations and even ban it altogether. One example is Mexico, where Congress enacted a reform that restricts outsourcing to tasks that are not directly related to the firm's primary activities. At the same time, this reform forced an important share of workers to switch employers from outsourcing to contracting firms within three months, which we call regularization. In this paper, we evaluate the effects of this reform on the labor market, exploring how it affects employment and wage levels.

There are several reasons why a firm uses outsourcing of labor services. Outsourcing can assist firms with managing human resources or accessing specialized services to a firm when the contractor has the required ability to do so. In this context, outsourcing is desirable because it allows firms to increase their efficiency and scale up production. For instance, outsourcing firms can assist with workforce recruitment, selection, and training, especially when these firms can exploit efficiencies from specializing in these tasks. It can also assist with payroll, tax payments, social security contributions administration, and the processing of severance payments.

Another application of outsourcing is to supply specialized services to a firm when the contracting firm has the required ability to do so, for example, accounting management, housekeeping services, legal services, and information technology services. On the other hand, under the subcontracting regime, the outsourcing firm can hire workers to liberate other firms from their obligations to the worker, thereby eroding worker rights. Firms may also opt to transfer their employees to a subsidiary company they have created or to a company dedicated solely to human subcontracting,

[1] For Latin America, there is some evidence regarding the impact of outsourcing on labor market outcomes, as demonstrated by the studies conducted by Drenik et al. (2021) and Felix and Wong (2021). However, it is worth noting that both of these studies focus on specific occupations and the legalization of outsourcing. In contrast, our analysis examines the prohibition of outsourcing and its effects on the universe of formal employment.

possibly damaging worker stability and wage fairness.[2] In general, firms use outsourcing to increase profits, although this only seems to happen in larger firms rather than for small firms (Görg et al., 2004).

It is helpful to understand how outsourcing affects employment and wage levels in the relevant occupational labor markets to evaluate the effects of outsourcing on the labor market. To study the impact of outsourcing on formal employment and wages, we use detailed administrative data on the universe of private sector formal workers from the Mexican Institute of Social Security (IMSS from its acronym in Spanish). The main advantage of this data is that it is possible to construct individual workers' employment histories and follow establishments over time. The panel structure of this data set allows us to measure changes in employment and earnings during the year the outsourcing reform went into effect. We apply an event-study design, exploiting exogenous variation in the passage of outsourcing legislation to provide credible estimates of the short-run causal effects of outsourcing on formal workers' employment trajectories and wages. We assume that the workers' jobs are the same before and after the reform. Thus, we can use a suitable control group to estimate the impact of the reform using a difference-in-difference type estimator.

One limitation of this data is that outsourced workers are not directly identifiable in our dataset. Unfortunately, we do not observe occupations, so we cannot implement an approach based on focusing on occupations that are "outsourceable." Instead, we track simultaneous flows in bulk of employees between firms in the Mexican administrative data to identify outsourced workers. Even though this assumption only accounts for a portion of all outsourced workers, it serves as a helpful venue to examine how outsourcing affects employment and wages.

Our findings suggest that, as a result of the outsourcing reform, workers who underwent regularization are more likely to retain their positions in private formal jobs than workers who were not part of the regularization process. Furthermore, our results indicate that, with the enactment of the outsourcing regulation, the wages of Mexico's formal workers affected by the reform increased by 2-6%. In particular, there is a sharp wage increase in the month after the enactment of the reform. Furthermore, the estimated effects of the outsourcing policy on formal wages in the subsequent months remain positive and significant after six months. We find notable variances between age groups, with employees aged 20- 49 experiencing more significant raises than their younger and older counterparts. The reform's impact varied significantly depending on firm attributes, like industry and size, particularly in the initial months after the outsourcing ban was implemented. The results indicate that workers in the manufacturing and trade industries and those employed by large companies (with over a thousand employees) witnessed more significant salary increases.

[2]These schemes may allow firms to evade compliance with employees' labor rights. For instance, firms could avoid paying required profit sharing, workers' social security, housing obligations, compensations in cases of unlawful dismissal, or costs associated with worker seniority.

Additionally, there is geographical heterogeneity in the effect, with workers in the center region experiencing more substantial salary increases than others.

Related Literature. Previous literature indicates that differences in worker productivity are not enough to explain wage dispersion across sectors and firms (Dunlop, 1957; Krueger and Summers, 1988; Groshen, 1991; Gibbons and Katz, 1992; Mortensen, 2003). This wage dispersion arises from pay premia in imperfectly competitive labor markets through different factors such as different collective bargaining agreements (Card et al., 2004; DiNardo and Lee, 2004), search frictions (Burdett and Mortensen, 1998; Hornstein et al., 2011), monopsony (Manning, 2013; Card et al., 2018), or fairness and efficiency wage concerns (Akerlof and Yellen, 1990; Rees, 1993; Card et al., 2004; DiNardo and Lee, 2004; Card et al., 2012). These conditions may result in diverse salaries for observationally equivalent workers.

As a response, firms may find it advantageous to outsource tasks to subcontractors as a cost-minimizing strategy.[3] There is also evidence suggesting that outsourcing may benefit companies by reducing employees' bargaining power. For instance, Basu et al. (2019) show that part of the appeal of subcontracting stems from a downward revision of workers' fair wage demand when producers delegate employment decisions down the supply chain. In this context, nonstandard employment arrangements have been linked to lower earnings and more inequality (Weil, 2014).

The incidence of domestic outsourcing, as a form of alternative employment arrangement, has significantly risen in the U.S. (Katz and Krueger, 2019), Germany (Goldschmidt and Schmieder, 2017), and France (Bilal and Lhuillier, 2021). Several studies have shown that outsourcing generally implies lower wages. Using data from the CPS, Dube and Kaplan (2010) find that the outsourcing wage penalty ranged from 4% to 7% for janitors and 8% to 24% for guards. Moreover, their findings on health benefits mirror those on wages. For Argentina, Drenik et al. (2021) identify outsourcing by looking at "temp" agency work arrangements, finding that "temp" agency workers receive 49% of the workplace-specific pay premia earned by regular user-firm workers. Similarly, for Germany, Goldschmidt and Schmieder (2017) identify outsourcing workers in specific industries and show that outsourcing reduces wages by around 10% across various measures.

Bilal and Lhuillier (2021) use a similar approach to identify outsourced workers. They find that job switchers in an outsourcing event lose 12% of their pre-event wage relative to workers at the firm who are not in the outsourcing event but also switch employers.[4] For Brazil, Felix and Wong (2021) analyze the impact of legalizing outsourcing on employment and wages, finding a persistent

[3] In this context, subcontractors compete for service contracts from firms based on pricing. Because labor expenditures account for a substantial portion of contractors' overall costs, these contractors are pressured to decrease salaries and benefits. This mechanism implies that outsourced work arrangements may erode pay premiums.

[4] However, factors such as concerns about equity (Card et al., 2012; Breza et al., 2017; Dube et al., 2019; Saez et al., 2019) or the unobservable nature of effort (Akerlof and Yellen, 1986; Katz, 1986) may lead to firm-specific pay premia even to outsourced labor.

increase in the total employment of security guards following the legalization of outsourcing, without any significant reduction in their average wages in the relevant occupational markets. However, Bergeaud et al. (forthcoming) find that the effects of outsourcing on wages are heterogeneous in terms of human capital. In particular, they find that workers in high-skill occupations experienced wage gains from being outsourced, while workers in low-skill occupations lost out.

While these papers and the present study share an interest in assessing the effects of outsourcing on employee labor outcomes, no prior research has examined the impact of banning labor subcontracting on the employment and wages of outsourced workers. A notable exception is Estefan et al. (2024), which analyzes the effects of Mexico's 2021 outsourcing ban using data from the Mexican Economic Census and manufacturing surveys. They find that the ban increased wages and reduced monopsony power, though it modestly decreased investment and increased firm exit rates. However, this study is limited to the manufacturing sector, making it difficult to generalize findings across other sectors, and it does not address distributional effects, potentially missing variations in the ban's impact across different worker demographics, firm sizes, or regions.

In contrast to prior studies, our paper overcomes existing limitations by leveraging longitudinal social security data from Mexico's formal labor market, enabling us to capture the reform's effects across multiple sectors. To better understand these impacts, we utilize the exogenous enactment of a law banning the outsourcing of firms' core economic activities in Mexico, paired with administrative employment data that allows for the indirect identification of outsourced workers. Using an event-study design, we provide credible short-run estimates of the reform's effect on formal employment and wages. We also contribute by examining distributional impacts and revealing detailed wage effects by gender, firm size, and region. Thus, our findings offer a more comprehensive view of how the reform influences wage dynamics and labor market outcomes, adding to the limited evidence on the relationship between labor subcontracting reforms and wages in middle-income and developing countries.

Unlike previous literature, which primarily examines the effects of outsourcing adoption on worker and firm outcomes, our study focuses on the impact of an outsourcing ban on employment and wages. Specifically, we investigate the consequences of labor regulation reform, which lead workers under the outsourcing regime to switch employers, potentially transitioning to their direct workplace employers. To identify the workers potentially affected by the reform, we measure the number of employees who shift simultaneously from one employer to another, comprising groups of 20 or more workers, when the reform takes effect. We start by examining some transition features between regimes, such as employed survival with their employer, their survival in formal employment, and worker mobility. Then, we estimate changes in the average wages of this group of workers, which we argue are due to the effects of the reform. In addition, we explore potential heterogeneities based on gender, age, and geographic location.

Finally, another distinctive aspect of our study is that, unlike previous research, we do not limit our analysis to specific occupational groups to understand outsourcing's labor market effects. By examining the reform's impact on the wages of the entire universe of formal workers, this paper expands the literature, documenting the effects of a national outsourcing reform on wage dynamics in a middle-income country

Outline. The rest of the paper is organized as follows: Section 2 summarizes the policy reform in Mexico and describes the data. Section 3 provides a conceptual framework. Section 4 describes the empirical model. Section 5 presents the main findings and a series of robustness checks. Lastly, a conclusion is presented in Section 6.

2 Institutional Context and Data

2.1 Policy Reform in Mexico

Firms in Mexico increasingly used outsourcing of labor services between 2000 and 2020. One source of information that shows the increasing importance of outsourcing is the Economic Census, which collects information on the total number of workers hired directly or indirectly by establishments in the case of the manufacturing sector and firms in the case of the other sectors. These workers include all the individuals who worked during the reference period, whether they were contractually bound to the economic unit or not, as long as they were subject to its direction and control. The Census also collects information on workers hired and provided by other firms. These workers were employed during the establishment's reference period but were contractually dependent on another firm, excluding those involved in security, cleaning, and gardening services.[5] Based on this data source, 6% of the workers employed by the establishments in 2003 were hired and provided by another firm. Fifteen years later, this share increased to 15.2%.[6]

Congress first regulated outsourcing by introducing new provisions to the Federal Labor Law in 2012, even though firms had already used this form of labor relations. In particular, *subcontatración*, the Spanish term for outsourcing, was defined as a situation in which an employer executes work or provides services with his employees to another firm. At the same time, the provisions in the 2012 reform set some limitations. For instance, the activities hired through outsourcing could not cover activities similar to those carried out in the workplace by workers hired by the contracting firm (i.e., the firm that hires the outsourcing firm).

[5] *INEGI* tries to capture the number of workers participating in the tasks directly related to the firm's main activity.

[6] The Economic Census collects basic information on all establishments that produce and commercialize goods and provide services to generate economic indicators for Mexico. It captures basic economic information on the country's economic activities, except for agriculture, livestock, and forestry, which are part of the Agricultural Census. It has taken place every five years since 1989.

In November 2020, the Federal Executive sent Congress an initiative to reform the legal framework that regulated outsourcing. This initiative recognized outsourcing as a new form of labor relation with its specificities in which firms contracted with other firms to execute particular processes or specific projects. On the other hand, the initiative also argued that employers could sometimes use this form of labor relations to promote abusive practices and simulations. For instance, some firms would transfer workers to another firm specifically created for payroll management, with labor conditions that are less favorable than those of the original firm. Even though there is no mention of how pervasive these practices were, the Federal Executive proposed eliminating outsourcing, except for specialized tasks or specific projects, to limit the possibility that firms could adopt practices that were considered abusive. Some examples of these specialized tasks are cleaning, maintenance and repairs, computing services, etc.

The reform proposed by the Federal Executive was finally enacted in April 2021 after a period of negotiation with trade unions and business associations. This reform outlawed outsourcing of labor services for tasks related to the predominant economic activity of the contracting firm. The reform only allowed workers to be outsourced to provide specialized services or execute specialized tasks. Initially, the reform granted companies until the end of July 2021 to regularize outsourced workers; contracting firms had to hire outsourced workers before this date. However, Congress later extended this deadline to September 1 in July 2021, although most of the job-to-job flows resulting from the reform occurred before the first deadline, as shown below.[7]

2.2 Data Description

In this paper, we use matched employer-employee data obtained from the administrative records of the Mexican Institute of Social Security (IMSS from its acronym in Spanish). This data set comprises a universe of all workers with jobs covered by social security provided by this institute.[8] The data exclude most government employees, the self-employed, and workers in informal employment. In particular, employers in the private sector report workers to IMSS to pay the social security tax. The dataset contains information on the employment type (permanent or temporary), the daily contribution wage (SBC from its acronym in Spanish) of all these workers, which comprises all payments made in cash as wages, including bonuses, perceptions, food, room, commissions, and benefits in kind for almost all workers, and some demographic variables (such as gender, and age). Although the wage data is top-coded, the upper bound is above a level that would affect our estimation.

[7] It is worth clarifying that according to the reform, providing or performing specialized tasks or services that are not part of the beneficiary firms' corporate purpose or economic activity will not be considered subcontracting of employees, as long as other legal conditions are followed.

[8] A job enrolled in social security is considered part of formal employment by the national statistical agency.

Employers are uniquely identified through the establishment ID.[9]Every worker also has a permanent ID from the first time they are enrolled at IMSS, even if the establishment that reports them changes or if they leave the formal sector for a period. This information allows us to construct individual workers' employment histories and follow establishments over time. We can obtain this information starting in 2005, the initial data point of the data.[10] To construct these histories, we take one monthly record for each worker. We use the record with the highest wage if a worker has multiple monthly employment records.

We also observe the employer's geographical location at the municipal level and the industry. The frequency of the data is monthly, and the sample period for the regressions is from January 2018 to November 2021 so that we can construct placebos of this reform with the data before 2021. However, we will use the whole data span to generate variables such as the age of the establishment and tenure. We will further restrict our attention to prime-age workers, employees between 25 and 64 years of age.

One limitation of the data is that we cannot directly identify outsourced workers. To study the effects of the reform, we first have to determine which workers were potentially affected: those who worked at an outsourcing establishment by June 2021. Unfortunately, we do not observe occupations, so we cannot implement the same approach as the one used in other papers (e.g., Goldschmidt and Schmieder, 2017). Instead, we track month-to-month worker movements between establishments, noting the number of employees moving between establishments from one month to the next.

In particular, we rely on worker flows between establishments between June and July 2021 and assume that the outsourcing establishments reported, in June 2021, workers who moved between establishments in groups of 20 or more between these two months. We call the movement of 20 or more workers between establishments as bulk flows. We impose the restriction that the establishments where bulk flows originated must have had at least 50 employees to reduce the risk of picking up workers' reorganizations across establishments. In sum, a flow of workers from one establishment - the predecessor - to another establishment - the successor - is defined as a regularization event in July 2021 if the following conditions hold: (i) the size of the flow from the predecessor to the successor establishment is 20 or more workers, and (ii) the size of the predecessor is at least 50 employees. For all flows between establishment pairs identified as regularization events, we call the predecessor establishment the outsourcing establishment and the successor establishment the contracting firm.

[9]Employers are identified by the *registro patronal*, a number assigned to a working site. In this paper, we call a *registro patronal* an establishment. Starting in 2018, we can also observe their anonymized tax ID (*Registro Federal de Contribuyentes*), which might encompass various *registros patronales*. We refer to a tax ID as a firm.)

[10]The firm's and the worker's IDs are masked, so neither the firm nor the worker can be completely identified. However, it is still possible to follow individuals and firms over time.

Workers potentially affected by the reform are those reported by outsourcing establishments in June 2021. Table 1 shows the distribution of jobs at that date according to whether they were potentially affected by the reform and, if they were affected, their status according to the classification described above in July 2021. We noticed that some establishments were origins and recipients of bulk flows of workers at the same time in July. To ensure the accuracy of our treatment group, we decided not to consider workers in these establishments as potentially affected. In other words, we reclassified workers involved in bulk flows of groups of 20 or more workers whose employers were both the origin and the destination of these flows in July to the category "non-affected by the reform."

Table 1. Job Distribution According to Potential Impact of the Reform

	Number of jobs	Percent
Jobs at outsourcing establishments	2,789,460	16.12
Regularized by the reform	2,042,313	11.80
Remained with previous employer	481,763	2.78
Switched to a new employer	155,772	0.90
Out of IMSS dataset	109,612	0.63
Not affected by the reform	14,512,030	83.88
Remained with previous employer	13,350,392	77.16
Switched to a new employer	527,796	3.05
Out of IMSS dataset	633,842	3.66
Total	**17,301,490**	100

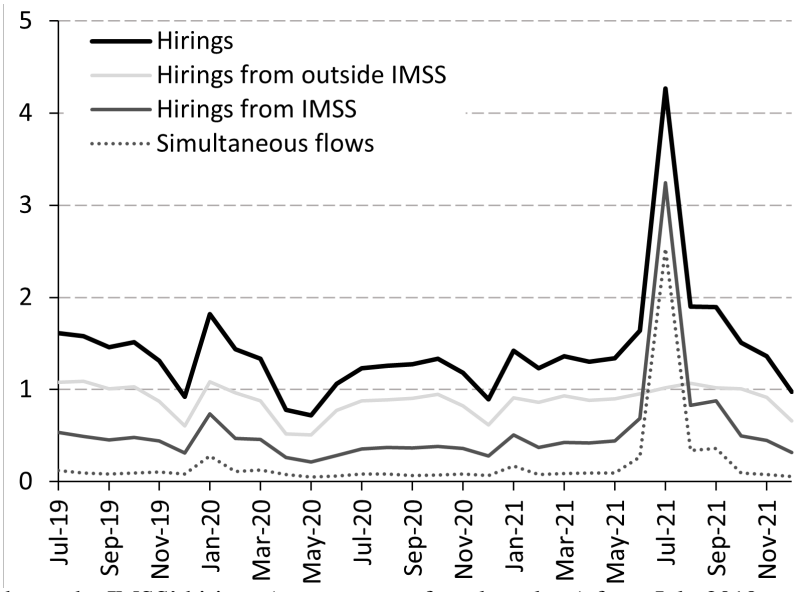
Notes: The table shows the job distribution in relation to the outsourcing reform as of June 2021. A job is a combination of worker I.D. and establishment I.D. The outsourcing establishments are those who reported, in June 2021, workers who moved between establishments in groups of 20 or more between these two months, the bulk flows. Regularized by the reform workers are those workers who participate in bulk flows between June and July 2021. Jobs that stayed with the previous employer are workers that were reported by the same employer in both months. Switched to a new employer are workers that flow to another employer by themselves or in simultaneous flows of less than 20 workers. Out of formality are workers that are reported by no employer in July 2021. Jobs not affected by the reform are the rest of the jobs. Workers employed in these jobs are between 25 and 64 years of age. All the percentages are with respect to the total of jobs.

Out of 17 million jobs in June, 16% were located in outsourcing firms, of which almost three of four were regularized. Only a tiny fraction of the workers in these establishments were lost to other employers or out of formality altogether. In contrast, outsourcing establishments kept 17% of all their workers on their payroll. For the workers not potentially affected by the reform, 92% remain with their previous employer, 4% switched employers, and no employers reported 4% of these workers in July.

Are the size of the bulk flows a regular data feature, or was it unusual around the deadline imposed by the reform? Figure 1 presents the evolution of hirings from July 2019 to November 2021. Hirings in a given month are jobs whose workers are reported to IMSS for the first time by an establishment. There are two kinds of hirings. First, there are what we define FF hirings that

correspond to workers who were reported by a different establishment the previous month (these are equivalent to formal job-to-job flows), and second, OF hirings that correspond to workers that the previous month were not reported in the IMSS database at all. We do not observe the previous employment situation of workers in the OF hirings, except that no employer reported them as private sector formal employed workers. Before appearing in the dataset, they could have been formally employed in the public sector, informally employed, unemployed, or out of the labor force.

Figure 1. IMSS' Hirings



Notes: This figure shows the IMSS' hirings (as a percent of total workers) from July 2019 to August 2021. It shows the distinction between those hirings of workers who were previously IMSS workers (hirings from IMSS) and those who were not IMSS workers (hirings from outside IMSS).Simulatenous flows show the ones that were simultaneous flows (workers that changed firms simultaneously in groups of 20 or more workers).
Source: Author's calculations based on IMSS's data.

Figure 1 shows that bulk flows are a small percentage of FF hirings, except in July 2021, the first deadline for switching outsourced workers to new employers. Usually, 18% of all FF hirings correspond to bulk flows, except in July when 2.4 million workers (81% of all FF hirings) switched employers in groups of 20 or more workers. These workers were regularized after being employed by an outsourcing establishment and are the focus of this paper. In addition, most of the FF hirings occurred in July, which we chose as the base for deciding the treatment time.

2.3 Descriptive Statistics

Table 2 shows the descriptive statistics of all establishments with 50 or more employees in June 2021, distinguishing between outsourcing and non-outsourcing establishments. Out of 50 thousand

establishments, 9.3 thousand are outsourcing establishments. In the comparison between both groups, we observe that outsourcing establishments are larger (295 workers in the outsourcing establishment vs. 227 workers in the non-outsourcing establishment), pay higher wages (MXN$ 553 vs. MXN$ 433), participate more intensely in wholesale and retail trade (34% vs. 18%), and are younger (9.8 years vs. 10.4 years).

Table 2. Establishment Characteristics in June 2021 by Treatment Status

Mean	Non-outsourcing establishment	Outsourcing establishment	Total
Number of employees	226.637	295.360	239.471
Wage	433.517	553.342	455.963
Female Workers	0.363	0.365	0.364
Northern Region	0.266	0.226	0.259
Northern Central Region	0.219	0.201	0.216
Central Region	0.395	0.431	0.402
Southern Region	0.119	0.142	0.124
Agriculture	0.041	0.016	0.036
Mining	0.008	0.012	0.009
Manufacturing	0.255	0.208	0.246
Construction	0.090	0.048	0.082
Electricity	0.012	0.002	0.010
Trade	0.181	0.344	0.212
Transportation	0.082	0.067	0.079
Business	0.236	0.292	0.247
Social	0.095	0.012	0.079
Age (in months)	125.783	118.333	124.392
Observations	40,676	9,341	50,017

Notes: Table shows the mean of each variable. All characteristics as of June 2021. Outsourcing establishments are included if they were the predecessors of the bulk flows between June and July 2021. Excludes establishments with less than 50 employees. The variable *age* corresponds to the number of months elapsed between the first time an establishment appears in the dataset and June 2021.

Table 3 shows the descriptive statistics of all workers divided into the following groups: regularized workers, workers affected by the reform who switched to different employers independently, workers affected by the reform who left formality in the private sector, affected workers who remained in the outsourcing establishments, and non-affected workers. We observe several differences in the comparison between regularized workers and non-affected workers. First, regularized workers earn higher wages. In June 2021, these workers' average daily wage was MXN$ 579, while non-affected workers' average daily wage was MXN$ 446.

Second, regularized workers are eight years younger on average. Third, outsourcing is mainly used in wholesale and retail trade rather than in educational services, healthcare, and social assistance sectors. In particular, 34% of regularized workers are hired by establishments in wholesale

and retail trade, whereas for non-affected workers, this share is 18%. Finally, regularized workers' jobs tend to be located in the Center region, including the Mexico City Metropolitan Area, in contrast to the non-affected workers' jobs more intensely located in the North and Central-North regions. Specifically, the share of regularized workers in the Center region is 47%, whereas this share for non-affected workers is 37%. In contrast, 24% and 17% of regularized workers' employers are in the North and Northern Central regions, respectively. These shares for non-affected workers are 28% and 23%, respectively.

Table 3. Descriptive Statistics

	Outsourcing Establishments										Non-Outsourcing Establisments	
	Total Sample		Switched to new employers		Out of formality		Regularized		Stayed with previous employer		Non-affected	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)	(5)	(6)	(5)	(6)	(5)	(6)
Wage	459.92	450.65	497.02	498.43	348.64	335.71	579.34	506.87	582.88	540.23	446.17	439.07
Age	39.67	10.02	36.99	9.16	36.48	9.59	37.90	9.34	38.39	9.56	40.19	10.09
Men	0.61	0.49	0.63	0.48	0.58	0.49	0.61	0.49	0.58	0.49	0.61	0.49
Women	0.39	0.49	0.37	0.48	0.42	0.49	0.39	0.49	0.42	0.49	0.39	0.49
Industry												
Agriculture	0.03	0.18	0.08	0.27	0.06	0.23	0.02	0.14	0.03	0.16	0.03	0.17
Extractive	0.01	0.08	0.00	0.06	0.01	0.07	0.01	0.11	0.00	0.06	0.01	0.08
Processing	0.27	0.44	0.17	0.38	0.25	0.43	0.31	0.46	0.25	0.44	0.27	0.45
Construction	0.08	0.27	0.05	0.23	0.07	0.25	0.03	0.17	0.05	0.22	0.08	0.27
Electricity	0.01	0.09	0.00	0.02	0.00	0.03	0.00	0.04	0.00	0.02	0.01	0.10
Trade	0.20	0.40	0.23	0.42	0.26	0.44	0.34	0.47	0.23	0.42	0.18	0.38
Transportation	0.06	0.25	0.07	0.26	0.06	0.25	0.06	0.23	0.05	0.22	0.07	0.25
Business Services	0.22	0.42	0.38	0.49	0.28	0.45	0.22	0.42	0.28	0.45	0.21	0.41
Social Services	0.12	0.32	0.01	0.10	0.02	0.14	0.01	0.11	0.11	0.31	0.14	0.35
Region												
North	0.27	0.44	0.19	0.39	0.25	0.43	0.24	0.43	0.26	0.44	0.28	0.45
Center-North	0.22	0.42	0.19	0.39	0.17	0.37	0.17	0.38	0.19	0.40	0.23	0.42
Center	0.38	0.49	0.52	0.50	0.48	0.50	0.47	0.50	0.46	0.50	0.37	0.48
South	0.12	0.33	0.11	0.31	0.10	0.30	0.12	0.32	0.09	0.28	0.12	0.33
Firm Size												
1 worker	0.02	0.13	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.02	0.14
2 to 5	0.07	0.25	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.08	0.27
6 to 50	0.23	0.42	0.02	0.13	0.02	0.13	0.01	0.12	0.00	0.07	0.26	0.44
51 to 250	0.24	0.43	0.35	0.48	0.28	0.45	0.28	0.45	0.20	0.40	0.23	0.42
251 to 500	0.11	0.31	0.18	0.39	0.20	0.40	0.19	0.39	0.16	0.37	0.10	0.30
501 to 1,000	0.11	0.31	0.14	0.34	0.17	0.37	0.17	0.38	0.17	0.38	0.09	0.29
More than 1,000	0.24	0.42	0.30	0.46	0.33	0.47	0.34	0.47	0.46	0.50	0.22	0.41
Observations	17,301,490		155,772		109,612		2,042,313		481,763		13,350,392	

Notes: The table shows the mean and the standard deviation of workers classified by treatment status. Outsourcing establishments are included if they were the predecessors of the bulk flows between June and July 2021. Non-outsourcing establishments are the establishments reporting workers in June 2021 and were not predecessors of bulk flows. Switched to a new employer are workers that flow to another employer by themselves or in simultaneous flows of less than 20 workers. Out of formality are workers that are reported by no employer in July 2021. Regularized workers were those involved in these flows. Jobs that stayed with the previous employer are workers that were reported by the same employer in both months, regardless of another employer also reported them in July 2021. Jobs non-affected by the reform are the rest of the jobs.

There were also more subtle differences between regularized workers and those who stayed working with the outsourcing employers. First, regularized workers earned lower wages (MXN$ 579 vs. MXN$ 583) and were younger (37.9 vs. 38.4 years) on average. Second, regularization was concentrated in manufacturing (31% vs. 25%) and wholesale and retail trade (34% vs. 23%). In contrast, business service, educational services, and healthcare and social assistance employers

kept a larger share of workers, which is consistent with the fact that these employers are more likely to provide specialized services. Finally, regularized workers' employers are smaller on average than the employers of outsourcing workers who stayed with the previous employer.

3 Conceptual Framework

In this section, we introduce a stylized model designed to illustrate the empirical implications of implementing a ban on labor outsourcing following a framework similar to Acemoglu and Autor (2011), Acemoglu and Restrepo (2018) and Acemoglu and Restrepo (2022). Through the development of this simple theoretical framework, we seek to illustrate the potential impact of such a policy change on the labor market, with a particular emphasis on shifts in labor demand and wage adjustments. Our model is a simplified representation that encapsulates the key aspects of labor reallocation and wage determination within the context of labor outsourcing, laying the groundwork for understanding the broader empirical implications of regulatory intervention that ban labor outsourcing.

3.1 Environment

Consider an economy with a single upstream sector and two downstream sectors. The upstream sector generates intermediate business services solely through the use of labor. In contrast, each of the downstream sectors produce a final good. Firms within the downstream sector 1 hire workers to carry out production tasks. However, they also have the option to purchase intermediate services to complete some of these tasks. On the other hand, firms within the downstream sector 2 only use workers for production.

The upstream sector produces intermediate services M using labor as the only input. The production function for this sector is given by:

$$M = A_m L_m \tag{1}$$

where M is the amount of intermediate services produced, A_m is the productivity of labor in the upstream sector, and L_m is the labor input in the upstream sector. The price of intermediate services is denoted as P_m, and the marginal cost of production is given by $MC_m = \frac{w_m}{A_m}$, where w_m is the wage rate in the upstream sector.

Downstream sector 1 produces a final good Y_1 by completing a continuum of tasks indexed by $t \in [0,1]$. These tasks can be performed either by in-house workers or outsourced to intermediate services. While in-house workers have identical productivity in all tasks, outsourcing services are

as productive as in-house workers on tasks in the range $[0, I]$ but have zero productivity at tasks $(I, 1]$. The production function for sector 1, Y_1, can then be written as:

$$Y_1 = \int_0^I A_m L_m \, dt + \int_I^1 A_1 L_1 \, dt \tag{2}$$

Given the task-based structure, we can simplify Eq. (2) to $Y_1 = A_m I + A_1(1-I)L_1$. We assume that the cost of outsourcing is lower than the cost of in-house labor, $P_m < \frac{w_1}{A_1}$. Therefore, tasks in the range $t \in [0, I]$ are outsourced to intermediate services at cost P_m, and tasks in the range $t \in (I, 1]$ are performed by in-house workers at cost $\frac{w_1}{A_1}$. The cost per unit of output in sector 1 is given by:

$$P_1 = I\frac{w_m}{A_m} + (1-I)\frac{w_1}{A_1} \tag{3}$$

where I represents the extent of outsourcing in sector 1. We assume that the markets for both inputs are perfectly competitive. Downstream sector 2 produces final goods using only labor L_2. The production function for sector 2 is given by:

$$Y_2 = A_2 L_2 \tag{4}$$

where A_2 is the technology parameter. In this model, the price of the good produced by sector 2 is set as the numeraire, so $P_2 = 1$. Given that $P_2 = 1$, the wage in sector 2 will be $w_2 = A_2$. The aggregate demand for goods produced by sector 1 is exogenous and given by:

$$Q(P_1) = \kappa P_1^{-\sigma} \tag{5}$$

where κ and σ are parameters that determine the price elasticity of demand. For simplicity, we assume that households consume only the good produced by sector 2. The labor demand in sector 1 depends on the tasks that are performed in-house. Given the aggregate demand for the good produced by sector 1, we can obtain the labor demand by plugging the expression for P_1 into the aggregate demand for good 1. Therefore, the labor demand is:

$$LD_1(w_1) = (1-I)\kappa \left[I\frac{w_m}{A_m} + (1-I)\frac{w_1}{A_1} \right]^{-\sigma} \tag{6}$$

Individuals derive utility from wages in different sectors and have idiosyncratic preferences for where they work. The utility function for an individual i working in sector j is given by:

$$U_{ij} = \beta \ln w_j + c_2 + \varepsilon_{ij} \tag{7}$$

where β is a parameter capturing the sensitivity of utility to wages, c_2 is the consumption of the good produced by sector 2, and ε_{ij} is an idiosyncratic preference shock. The probability that an individual chooses sector j is given by $P_{ij} = \frac{\exp(\beta \ln w_j + c_2)}{\sum_k \exp(\beta \ln w_k + c_2)}$, where k sums over all possible sectors (e.g., 1, 2, m). Note that the baseline utility c_2 is common to all sectors, so it cancels out in the numerator and denominator. Using the property of logarithms, $\exp(\beta \ln w_j) = w_j^\beta$, the expression for the probability becomes $P_{ij} = \frac{w_j^\beta}{\sum_k w_k^\beta}$ Then, the aggregate labor supply for sector 1 (LS_1) is given by the probability of choosing sector 1 P_{i1} by the total labor force, $LS_1 = L \cdot P_{i1}$. Normalizing the total labor force to 1, then, the aggregate labor supply to sector 1 is given by:

$$LS_1(w_1) = \frac{w_1^\beta}{w_1^\beta + w_2^\beta + w_m^\beta} \tag{8}$$

3.2 Equilibrium in the Labor Market with Outsourcing

The equilibrium in the labor market is determined by equating labor supply to labor demand. For our purposes we are interested in the equilibrium in sector 1, which is given by:

$$LS_1(w_1) = LD_1(w_1) \tag{9}$$

Substituting the expressions for labor supply and demand, we get:

$$\frac{w_1^\beta}{w_1^\beta + w_2^\beta + w_m^\beta} = (1-I)\kappa \left[I\frac{w_m}{A_m} + (1-I)\frac{w_1}{A_1} \right]^{-\sigma} \tag{10}$$

Given that $w_2 = A_2$, and $w_m = P_m A_m$, the equilibrium condition becomes:

$$\frac{w_1^\beta}{w_1^\beta + A_2^\beta + (P_m A_m)^\beta} = (1-I)\kappa \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma} \tag{11}$$

This relationship shows that the equilibrium wage in sector 1, $w_1 = w_1(I, P_m, \beta, \kappa, \rho, \sigma, A_m, A_1, A_2)$, is a function of the extent of outsourcing (I), the price of intermediate services (P_m), the productivity of labor in the each sector sector A_i, for $i = m, 1, 2$, and supply and demand elasticity parameters β, κ, and σ.

3.3 Comparative Statics

Effect of Outsourcing on Labor Demand. To examine how changes in I affect the labor demand in sector 1, we need to differentiate the labor demand function LD_1 in Eq. (6) with respect to the

extent of outsourcing I, which gives:

$$\frac{dLD_1(w_1)}{dI} = -\kappa \left(IP_m + (1-I)\frac{w_1}{A_1} \right)^{-\sigma} \left[1 + \sigma \frac{(1-I)(P_m - \frac{w_1}{A_1})}{IP_m + (1-I)\frac{w_1}{A_1}} \right] \tag{12}$$

According to this result, changes in outsourcing has an ambiguous effect on labor demand. Moreover, the sign of this effect crucially depends on the size of the elasticity of labor demand σ.[11] To better interpret this comparative statics, we can take the natural logarithm of the labor demand:

$$\log LD_1(w_1) = \log(1-I) + \log \kappa - \sigma \log \left[IP_m + (1-I)\frac{w_1}{A_1} \right] \tag{13}$$

Then, we differentiate each term separately to get:

$$\frac{d \log LD_1}{dI} = \frac{-1}{1-I} - \sigma \frac{P_m - \frac{w_1}{A_1}}{IP_m + (1-I)\frac{w_1}{A_1}}$$

On one side, we have the displacement effect, which is consistently negative, $\frac{-1}{1-I} < 0$. This effect refers to the phenomenon where outsourcing leads to the reallocation of production tasks to external suppliers, often resulting in reduced demand for certain types of labor within the firm. Conversely, there is the productivity or scale effect, which is positive under the condition that $P_m < \frac{w_1}{A_1}$. This effect refers to the phenomenon where outsourcing reallocates production tasks to external suppliers, often enhancing efficiency and expanding the scale of production. The magnitude of this latter effect is significantly influenced by the elasticity of labor demand, σ. A larger σ results in a stronger productivity effect, potentially outweighing the negative displacement effect.

Effect of Outsourcing on Wages. To analyze the comparative statics of the wage w_1 with respect to the extent of outsourcing I, we need to implicitly differentiate the equilibrium condition in Eq. (11) with respect to I, which give us:

$$\frac{dw_1}{dI} = \frac{-\kappa \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma} \left[1 + \sigma \frac{(1-I)\left(P_m - \frac{w_1}{A_1}\right)}{IP_m + (1-I)\frac{w_1}{A_1}} \right]}{\frac{\beta w_1^{\beta-1}}{\left(w_1^\beta + A_2^\beta + (P_m A_m)^\beta\right)} - \frac{\kappa\sigma(1-I)^2\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma-1}}{A_1}} \tag{14}$$

The numerator of Eq. (14) captures the direct impact of changing the extent of outsourcing on the equilibrium condition. On the other hand, the denominator of Eq. (14) captures the responsiveness of the equilibrium condition to changes in the wage w_1. The exact direction and magnitude of

[11] See the Appendix B for details.

the changes will depend on the relative sizes of these effects.[12] To obtain a clearer interpretation of this comparative statics, we take the natural logarithm on both sides of the wage equation w_1 and then performing the comparative statics of I on $\log(w_1)$. Taking the natural logarithm on both sides give us:

$$\beta \log(w_1) = \log(1-I) + \log(\kappa) - \sigma \log\left(IP_m + (1-I)\frac{w_1}{A_1}\right) + \log\left(w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}\right)$$

Then, we differentiate each term separately to get:

$$\frac{d\log(w_1)}{dI} = \frac{-\frac{1}{1-I} - \sigma \frac{P_m - \frac{w_1}{A_1}}{IP_m + (1-I)\frac{w_1}{A_1}}}{\beta\left(\frac{1}{w_1} - \frac{\sigma(1-I)}{IP_m + (1-I)\frac{w_1}{A_1}}\right) + \frac{\beta w_1^{\beta-1}}{w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}}} \tag{15}$$

In the numerator we have again the displacement effect, which is consistently negative, and the productivity or scale effect, which is positive under the condition that $P_m < \frac{w_1}{A_1}$. As mentioned before the magnitude of this latter effect is significantly influenced by the elasticity of labor demand, σ. In the denominator, the first term reflects the combined effect of the marginal productivity of labor and the elasticity of substitution between outsourced and in-house labor. The term $\frac{1}{w_1}$ represents the inverse of the wage, which decreases as w_1 increases. The second part, $\frac{\sigma(1-I)}{IP_m + (1-I)\frac{w_1}{A_1}}$, captures the sensitivity of the in-house wage to changes in outsourcing, considering the relative costs of in-house versus outsourced labor. The sign of this term depends on the balance between these two components, but it generally suggests that as I increases, the overall impact on w_1 depends on how much the wage w_1 is affected by the relative costs of outsourcing versus in-house production. The second term represents the marginal effect of the wage on the overall labor market equilibrium, considering the productivity of sector 2 and the upstream sector. The numerator $\beta w_1^{\beta-1}$ is positive and reflects how sensitive the wage w_1 is to changes in the equilibrium condition. The denominator $w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}$ captures the combined productivity effects of the different sectors.

Effect of Outsourcing on the Probability of Staying Employed. For simplicity, let's define the probability of staying employed (P_{stay}) as a function of LD_1, representing the ability of the labor market to absorb workers:

$$P_{\text{stay}} = \frac{LD_1}{LS_1} \tag{16}$$

[12] See the Appendix B for details.

Then, if we substitute the expression for the labor demand (LD_1) and labor supply (LS_1) and simplify the expression, we get:

$$P_{\text{stay}} = \frac{(1-I)\kappa \left[\frac{IP_m+(1-I)\frac{w_1}{A_1}}{P_1}\right]^{-\sigma} (w_1^{\beta} + w_2^{\beta} + w_m^{\beta})}{w_1^{\beta}}$$

To analyze how changes in outsourcing (I) affect P_{stay}, we differentiate with respect to I to obtain:

$$\frac{dP_{\text{stay}}}{dI} = \frac{dLD_1}{dI} \cdot \frac{1}{LS_1} - \frac{LD_1}{LS_1^2} \cdot \frac{dLS_1}{dI} \tag{17}$$

From earlier, changes in LD_1 with respect to I include the displacement and productivity effects. For the labor supply (LS_1), we have that $\frac{dLS_1}{dI} = \frac{\beta w_1^{\beta-1} \frac{dw_1}{dI}}{\left(w_1^{\beta}+w_2^{\beta}+w_m^{\beta}\right)^2}$. The probability of staying employed (P_{stay}) is influenced by changes in outsourcing (I) through two opposing effects: the displacement effect, which reduces in-house labor demand as tasks shift to external suppliers, and the productivity/scale effect, which can increase efficiency, expand production, and raise demand for remaining in-house tasks. The net effect on P_{stay} depends on the balance of these forces, shaped by factors like the elasticity of labor demand (σ) and the relative costs of outsourcing (P_m) versus in-house labor ($\frac{w_1}{A_1}$). Additionally, the supply-side response (LS_1) plays a critical role, as it depends on how wages (w_1) adjust to changes in I. Thus, the overall impact of outsourcing hinges on the interaction of the responses to labor demand and wages.

3.4 Discussion

To comprehensively compare the wages and labor allocation before and after the ban on labor outsourcing, we will analyze the equilibrium conditions in both scenarios and discuss the implications for wages and employment.

Before the ban on labor outsourcing, sector 1 operates with a mixed production model, using both in-house labor and outsourced services from the upstream sector. The equilibrium wages in sector 1 (w_1) are influenced by the relative cost of in-house labor versus outsourced services. If outsourcing is more cost-effective ($P_m < \frac{w_1}{A_1}$), firms in sector 1 rely more on outsourced services, which keeps in-house wages lower and reduces the demand for in-house labor. Sector 2, which does not engage in outsourcing, has stable wages and employment based on its internal productivity. Meanwhile, the upstream sector benefits from strong demand for its services, leading to higher wages (w_m) and significant employment (L_m) driven by the extent of outsourcing.

After the ban, sector 1 must fully rely on in-house labor, causing a significant shift in its labor market dynamics. The increased demand for in-house workers drives up wages (w_1) as firms compete to fill positions previously covered by outsourced services. Employment in sector 1 rises sharply, as all tasks now require in-house workers. Sector 2, while not directly involved in outsourcing, may experience upward pressure on wages due to increased competition for labor with sector 1. This could lead to a reduction in employment in sector 2 if it cannot attract enough workers. The upstream sector, which provided outsourced services, faces a severe decline in both wages and employment as the demand for its services evaporates, potentially leading to a significant contraction or collapse of the sector.

The ban on labor outsourcing fundamentally alters the equilibrium conditions across the economy. Sector 1 sees higher wages and increased employment due to the shift to a fully in-house labor model. Sector 2 may experience wage increases but could face employment challenges as it competes for labor with sector 1. The upstream sector, however, suffers a sharp decline in both wages and employment, as its primary demand source disappears. Overall, the labor market undergoes significant adjustments, with labor reallocating from the now-redundant upstream sector to the increased labor needs of sectors 1 and 2, leading to potential disruptions and a reconfiguration of the workforce.[13]

Based on our conceptual framework, the ban on outsourcing is expected to lead to a reallocation of labor and impact wages. However, given that some factors shape outsourcing's effects on employment and wages, the overall impact remains uncertain. This uncertainty underscores the need for empirical investigation. To address this, in the next section, we will use a quasi-experimental design, leveraging a national labor market reform in Mexico that banned outsourcing, along with administrative data, to test these predictions.

4 Empirical Strategy

To estimate the effect of the outsourcing reform on employment in formal private sector jobs and wages, we employ an event study design. We use a similar approach to the one used by Goldschmidt and Schmieder (2017), in which we exploit the panel structure of the dataset. As mentioned above, we distinguish as regularized workers those employees who separated from the same employer in June 2021 and were hired simultaneously by another employer in groups of twenty or more workers in July 2021, the initial deadline. These workers comprise the treatment group.

[13] A simulation of this simple model is included in the Appendix B, demonstrating how varying levels of outsourcing affect wages and employment.

We consider that the relevant time of the event is April 2021, when the reform was enacted, and not when the regularization occurred. Employers may have reacted as soon as they knew the exact content of the reform. We argue that most of these workers change employers due to the reform rather than for any other reason. Even though we have no information about job characteristics, including occupation, it is plausible that these workers kept the same jobs with their new employers. If the workers' jobs are the same before and after the reform, one can use a suitable control group to estimate the impact of the reform by a difference-in-difference type estimator. This framework allows us to observe adjustments in employment and wages each month, capturing any changes in the labor market throughout the analysis period.

Formally, we estimate the following model:

$$Y_{ijt} = \alpha_i + \psi_j + \delta_t + \sum_{k=-6, k\neq -1}^{6} \beta_k (D_i \times I_{t=t^*+k}) + \varepsilon_{ijt} \tag{18}$$

where Y_{ijt} represent the outcome of worker i at firm j in month t (e.g., employment status or wages), α_i are individual fixed effects, ψ_j is the fixed effect for the firm, δ_t are time fixed effects and D_i is an indicator for whether a worker i belongs to the outsourced group. The indicator variable $I_{t=t^*+k}$ is one during the lead or lag k. Each period t corresponds to a month. The parameter of interest are the coefficients β_k for $k \in \{-6, \ldots, -2, 0, 1, \ldots, 6\}$. The coefficient for the period right before the simultaneous flows of employees occurred is normalized to zero (i.e. $\beta_{-1} = 0$). Therefore, we should interpret each coefficient β_k as the effect of a switch in employer on an incumbent outsourced worker k months since the law's enactment relative to their matched pair at a non-outsourcing firm. We cluster standard errors on the level of the establishments.

Since the estimation of this model with all the data is too demanding in terms of computing resources, we draw a random sample of 150 thousand outsourced workers, which is approximately 5% of those affected. Table A1 presents descriptive statistics for the sample and the entire universe of outsourced workers in the IMSS database. To corroborate that this sample is representative of the entire universe of outsourced workers in the IMSS database, we show the summary statistics in Table A1 of some observable characteristics between the sample of workers selected randomly from the treatment group and the population of workers in the IMSS dataset that were directly affected by the regularization mandate. The main contrast between the sample and the population is the proportion of workers in the Center-North region. However, this difference is not economically meaningful, albeit statistically significant. In sum, the results in Table A1 suggest that these two groups are very similar, and therefore, this random sample could be used in the analysis as the treatment group.

To construct our treatment group, we concentrate on "regularized workers." These are employ-

ees who departed from the same employer in June 2021 and were then hired by a different employer in July 2021, as part of a simultaneous hiring group of twenty or more workers. A potential concern is that the assignment of workers to the treatment group—those who changed employers in groups of twenty or more—may not be random. This non-random assignment could introduce selection bias if the treated workers differ systematically from the rest of the population in ways that also affect the outcomes of interest. To mitigate this concern, we employ a nearest-neighbor matching technique to construct a control group that closely resembles the treatment group in terms of observable characteristics.

Using nearest-neighbor matching, we identify control workers who did not experience an employment change but share similar pre-treatment characteristics with the treated workers. These characteristics include demographics (e.g., sex and age), job-related attributes (e.g., tenure and pre-reform wage percentile), and firm characteristics (e.g., industry, firm size, and region). By matching treated and control units based on these covariates, we aim to balance the two groups, ensuring that any differences in outcomes post-reform are more likely attributable to the reform itself rather than pre-existing differences.[14]

We matched the treatment group workers with a random sample of 1.5 million workers unaffected by the reform because implementing this matching with 16 million workers was computationally demanding. In practice, we classify the sample of workers affected by the reform in wage percentile-industry-firm size cells. Then, we take all the non-outsourced workers in the sample in the same cell in April 2021 as the potential control group. Using this sample, we take the non-affected worker with the closest value of the Mahalanobis distance function as the control. We impose no other condition on the control group regarding their working histories. That is, the working history of a worker in the treatment group would look similar to that of a worker in the control group, including their potential movement across establishments and flows outside formality.

In Table A2 we present descriptive statistics for a set of variables for the treated and the non-treated groups in order to assess the balance of the sample. By comparing the two groups across various dimensions, we can determine if they are sufficiently similar, to ensure the validity of our subsequent findings. Most of the variables exhibit only minor discrepancies between the treated and non-treated groups, indicating a generally balanced sample. Following the standard methodology in event studies, individuals who have not been part of significant simultaneous flows work as the comparison group for individuals who have been part of this event. The identification of this model comes from the exogenous timing of the event produced by enacting a new outsourcing policy. The administrative data of formal workers is a monthly panel that allows for exploring the

[14] We include wage information as a matching characteristic because we believe that the proper contrafactual for the outsourced workers is to work in jobs with wages similar to the ones offered by outsourcing firms.

differences in the periods before the event and looking if pre-trends drive the effects we estimate. We will provide evidence that the pre-trends do not drive the effects.

The matched control group created using this technique serves as a counterfactual to the treated group, allowing us to estimate the reform's causal impact under the assumption that, conditional on observable characteristics, the only systematic difference between the two groups is the exposure to the reform. This helps mitigate concerns of selection bias and provides more credible estimates of the reform's effects.

While matching addresses observable differences between treated and control groups, there remains the possibility of omitted variable bias due to unobservable factors that could influence both the likelihood of treatment (i.e., changing employers in groups) and the outcome. To assess the robustness of our estimated effects against such potential biases, in the robustness section, we employ the methodology of Oster (2019) to provide a test of the sensitivity of our results to omitted variable bias. The method involves calculating how much the estimate of the treatment effect would need to change if unobserved factors were as important as observed ones in explaining the outcome. If our estimated treatment effects remain robust under reasonable assumptions about the degree of selection on unobservables relative to observables, we can be more confident that our findings are not driven by omitted variable bias.

5 Results

In this section, we use IMSS administrative records to analyze the effect of the outsourcing reform on two dimensions of the labor market. First, we aim to investigate outsourcing reform's potential influence on formal employment permanence. This step entails exploring whether individuals who have undergone the regularization process exhibit a higher likelihood of retaining their status as workers with private formal employment compared to non-treated workers. This analysis seeks to unveil any discernible patterns or disparities in job stability among various workforce segments following the implementation of the outsourcing reform.

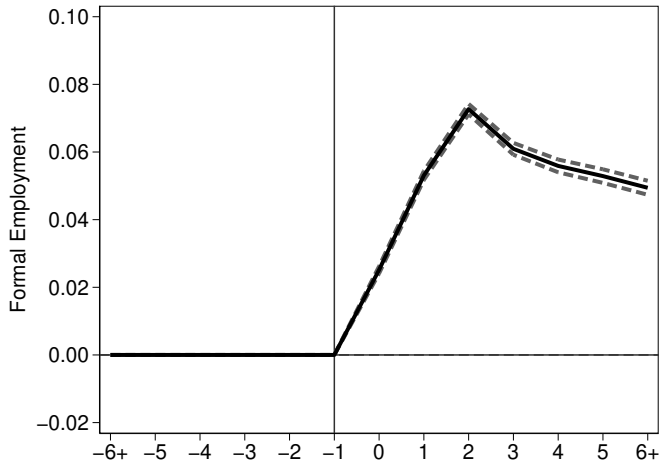
Second, we will examine the effects of the outsourcing reform on wage levels. This step involves inspecting whether there are notable changes in the wage level of employees due to the reform. By examining wage dynamics, we aim to ascertain any shifts in income patterns or inequalities that may have arisen after the outsourcing reform. This dual-pronged approach aims to provide an understanding of the impact of outsourcing reform on employment and wages within the labor market.

5.1 Private Sector Formal Employment

First, we use a linear probability model to examine whether the outsourcing reform impacts workers' likelihood of remaining in the formal sector. In practice, we compare the employment trajectories of the treatment and non-treated groups as described in the previous section. We focus on workers who maintained their employment status six months before the reform, irrespective of whether they stayed with the same employer as in June 2021. The reason for doing this is to keep the sample as balanced as possible.

Our analysis employs the regression model described in Eq. (18), with the dependent variable being a binary indicator (1 for formal employment and 0 for other statuses). It is important to note that when the dependent variable is 0, we lack information on the employment status of the workers; they could be informally employed, unemployed, or out of the labor force. We cluster standard errors on the level of the individual worker. Although most of the regularization of employers took place in July 2021, we chose April 2021 as the base period, the date when the reform was enacted.

Figure 2. Survival in the Private Sector Formal Employment



Notes: The figure shows the coefficients from Eq. (18) when the dummy of being employed in the private sector formal job is used as the dependent variable. The periods before -1 include 2020m11–2021m3. The omitted period represents 2021m4, represented by the vertical line. The post periods represent 2021m5–2021m12. The intervals are pointwise 95 percent confidence intervals for the corresponding elements. Robust standard errors are clustered at the individual-level. Individual-level data from the IMSS database.

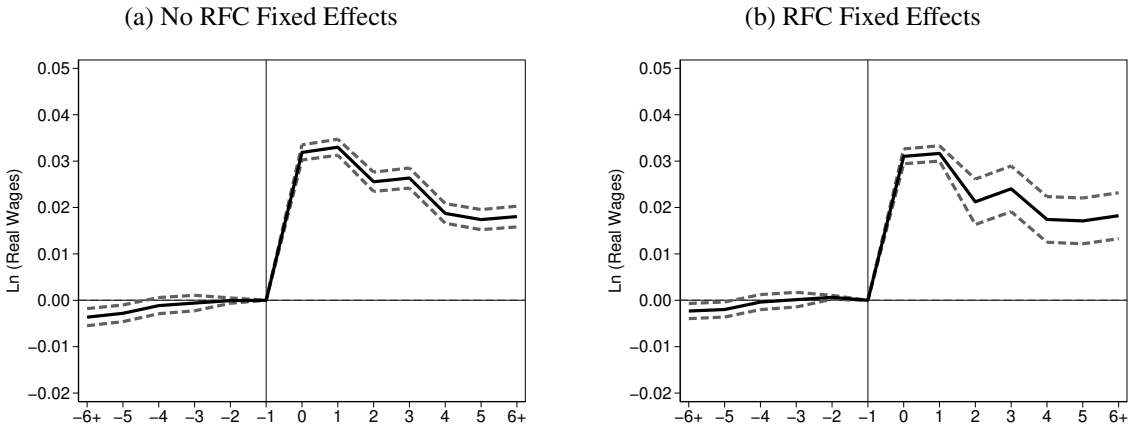
Figure 2 shows the β_k coefficients obtained from estimating Eq. (18) with a dependent variable indicating employment in a private formal job. The regression analysis confirms that regularized workers remain in formal employment longer than workers in the control group. This difference is considerable, highlighting a clear divergence among the groups at the time of the reform. Within six months after the reform, the likelihood of remaining formal is around six percentage points

greater for regularized workers relative to non-treated workers. In other words, regularized workers are more likely to retain their positions in private formal jobs compared to workers who stayed with their outsourcing employers and those who were not part of the regularization process seven months after the enactment of the reform. In sum, the outsourcing reform implied a negligible loss of formal employment for the regularized workers relative to the control group in the aftermath of the reform.

5.2 Wages

The next step in the analysis is to compare the wage trajectories of regularized workers and the corresponding control group in the months before and after the reform. Although most of the regularization of employers took place in July 2021, we choose April 2021 as the base period, the date when the reform was enacted. The results from the event study analysis using log wages as the dependent variable are reported in Figure 3. In both sets of estimations, we add industry, region, firm size, and individual-fixed effects. The individual fixed effects are used to account for time-invariant worker-specific wage differences. Each panel illustrates the impact of the outsourcing reform on the earnings of formal workers, considering two distinct specifications: one without firms-fixed effects and the other with firms-fixed effects. Each point illustrates the magnitude of the estimated cumulative effect of the reform.

Figure 3. Event Study: Year 2021

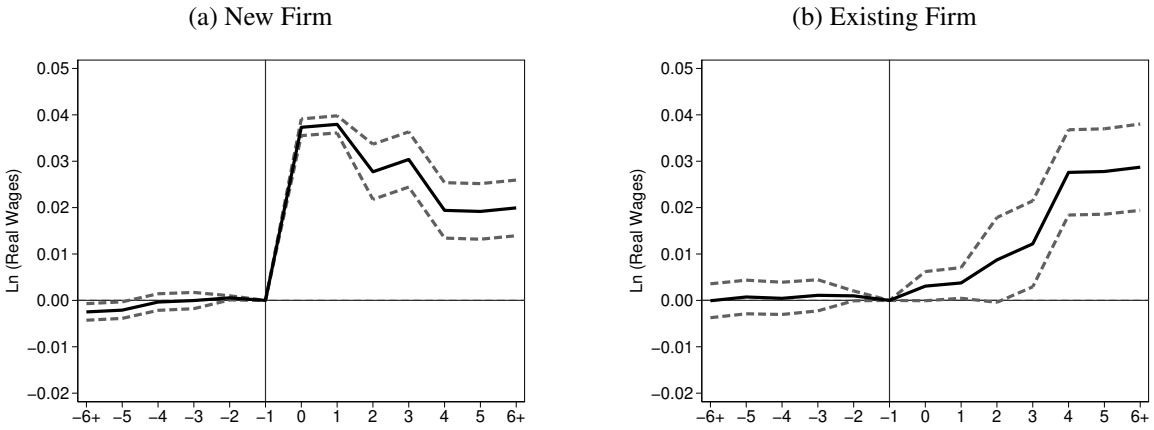


Notes: The figure shows the coefficients from Eq. (18) for several specifications of the model. Baseline fixed effects include individual-level fixed effects, and we also estimate a model that includes firm and individual-level fixed effects. The periods before -1 include 2020m11–2021m3. The omitted period represents 2021m4, represented by the vertical line. The post periods represent 2021m5–2021m12. The intervals are pointwise 95 percent confidence intervals for the corresponding elements. Robust standard errors are clustered at the individual-level. Individual-level data from the IMSS database.

The results suggest that there is no economically meaningful pre-trend in wages. Moreover, we

observe that the results are consistent, and after the implementation of the outsourcing regulation, there is an increase in formal job wages.[15] Panel (a) shows that, in the months leading up to the law's implementation, the plotted points hover around zero and then increase following the passage of the new regulation. Specifically, we observe an increase in wages following the adoption of the new regulation. This wage increase persists and does not fade in the subsequent six months. For instance, the estimated coefficient at $t = 6$ in Panel (a) of Figure A3 suggests that the projected cumulative effect of the reform on worker's earnings is around 2% higher relative to the wage earned the month before the enactment of the reform. This result is not affected by adding firm fixed effects to the specification as shown in Panel (b) of Figure A3.

Figure 4. Event Study: Year 2021



Notes: The figure shows the coefficients from Eq. (18) for several specifications of the model. Baseline fixed effects include firm and individual-level fixed effects. The periods before -1 include 2020m11–2021m3. The omitted period represents 2021m4, represented by the vertical line. The post periods represent 2021m5–2021m12. The intervals are pointwise 95 percent confidence intervals for the corresponding elements. Robust standard errors are clustered at the individual-level. Individual-level data from the IMSS database.

The regularization of outsourced workers adopted two organizational forms. Some workers were transferred to new establishments, employers that started reporting workers in July 2021; others were transferred to existing establishments, employers that reported workers before July 2021. We explore how these two ways of responding to the reform by employers affected wages. Figure 4 reports the results of these two subgroups from the event study analysis.

Even though the time pattern looks different among both subgroups, we find that the outsourc-

[15] As a robustness, we use a specification approach that combines a unit-specific linear trend with fixed effects as in Jacobson et al. (1993). Polynomial options that are often used include unit-specific linear trend, $f(t) = t$, or unit-specific quadratic trends, $f(t) = (t, t^2)$. We allow for employee-specific differences in wage trends by taking $f(t) = t$, thus assuming that the outsourcing law change is exogenous with respect to unobserved determinants of wages, conditional on the worker-fixed effect and the worker-specific time trend. Results are similar to the benchmark specification.

ing reform positively impacts outsourced workers' wages in general. The wage increase took longer in existing firms to materialize, but the final growth of wages was similar in levels after six months across groups.

5.3 Difference-in-differences Results

To complement the analysis, we use a difference-in-differences approach. However, it is important to note that our main specification for studying the impact of the outsourcing regulatory change on workers' labor market outcomes is the event study design. We chose this framework because it allows us to capture the wage dynamics on a monthly basis throughout the post-reform period. This dynamic variation is not accounted for by the difference-in-differences methodology, which provides an average effect over the entire post-reform period but overlooks the changing treatment effects over time. Moreover, the event study is advantageous because it allows us to examine pre-reform trends.

While certain constraints are associated with employing a difference-in-differences framework, using a grouped post-period approach can still offer valuable insights into quantifying the overall impact on wages during the immediate aftermath of the 2021 outsourcing reform. As a result, we adjust our benchmark model described in Eq. (18) by replacing the monthly event-study dummy variables with a grouped post-period dummy variable. This modified difference-in-differences strategy is described as follows:

$$Y_{ijt} = \beta_0 + \alpha_i + \psi_j + \delta_t + \beta_{PR} PR_{it} + \theta_i t + \varepsilon_{ijt} \tag{19}$$

where Y_{ijt} is the outcome of interest for individual i at firm j in month t. PR_{it} is a dummy variable that takes the value of one in April 2021 through the next six months for the individuals affected by the reform. We also add $\theta_i t$, which are monthly individual-level linear time trends. These trends account for linear growth in wages over time. All other features of Eq. (19) are similar to Eq. (18).

The difference-in-differences (DiD) analysis results are presented in Table 4. In this table, odd-numbered columns display estimates without linear trends, while even-numbered columns include estimates with linear trends. The DiD findings are consistent with the main event study results, showing that wages increase following the implementation of the outsourcing reform. The essential advantage of the DiD approach is that the estimated coefficients represent the average change in wages during the post-reform period. This provides a more explicit interpretation of the reform's overall impact on wage levels. Specifically, we find that wages increased by approximately 2.7% to 3.2% compared to the pre-reform period. This range provides a concise summary of the wage

effect and underscores the reform's positive impact on wage growth, reinforcing the conclusions drawn from the event study analysis.

Table 4. Difference-in-differences Specification

	$\ln(w)$ (1)	$\ln(w)$ (2)	$\ln(w)$ (3)	$\ln(w)$ (4)
1(Post-Outsourcing Reform)	0.032***	0.027***	0.031***	0.029***
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	2,920,347	2,920,347	2,920,347	2,920,347
Adjusted R^2	0.940	0.940	0.958	0.958
Baseline FE	✓	✓	✓	✓
Firm FE			✓	✓
Time Trends		✓		✓

Notes: Difference-in-differences estimates from Eq. (19). Estimates show the grouped post-periods (after the outsourcing reform) relative to the pre-reform (previous five months). Individual-level monthly linear time trends are included in primary results. Baseline fixed effects are included at the individual level and month. Additionally, RFC fixed effect are included in primary results. Robust standard errors are clustered at the individual level. Significance levels: *p<0.01, **p<0.05, ***p<0.01.

5.4 Robustness

A series of robustness analyses are conducted to test the validity of the results. First, we perform a placebo test using data from years before the reform. Second, we conduct a robustness check to assess the sensitivity of the assumption of parallel trends following Rambachan and Roth (2023). Finally, we apply Oster (2019) bounding methodology to examine the impact of potential omitted variable bias.

Placebo. We perform a placebo test to validate the robustness of our results. To do this, we construct a sample using the same methodology as before but with data from 2018 and 2019. We do not include data from 2020 to avoid confounding effects of the COVID-19 shock. Specifically, we randomly selected a sample of workers who switched employers in groups of 20 or more in July 2018 and 2019. A control group is then created through matching. Using this dataset, we replicate the event study analysis with wages as the dependent variable, applying the same treatment month—May of each year—as in our main analysis. The purpose is to assess whether this group of workers exhibits a similar pattern of wage growth to the one observed around the reform period.

The coefficients of interest are plotted in Figure A4. As expected, the evidence shows no immediate effect on wages during treatment. The coefficients are very economically small and statistically insignificant during the six months after the imputed implementation of the reform.

The fact that we cannot identify any significant differential effects resulting from the imputed legislation change in both robustness checks supports the idea that the divergence trends in wages across the different worker groups are specific to the outsourcing regulation in 2021.

Parallel Trends. The sensitivity analysis of parallel trends in our paper uses the methodology developed by Rambachan and Roth (2023) to assess whether the assumption of parallel trends holds, which is essential for the credibility of our estimates. The pre-event coefficients of our benchmark specification were not statistically significant or very close to zero, which in the event study literature is often considered suggestive evidence supporting the parallel trends assumptionn. However, Rambachan and Roth (2023) caution that the non-significance of pre-event coefficients alone cannot confirm the assumption of parallel trends. They argue that such evidence indicates that omitted variables have the same effect before the event without ensuring that this effect (parallel trends) will continue post-event. This is because omitted variables might behave differently after the event.

Using the methodology proposed by Rambachan and Roth (2023), we evaluate the sensitivity to potential violations of the assumption of parallel trends. This approach generates a series of bounds to identify the point at which the parallel trends assumption is violated post-event. It involves estimating the bounds of the treatment effect while allowing for potential violations of the parallel trends assumption. Expressly, we either assume that the post-treatment violation of parallel trends is no more than a constant $\overline{M}$ greater than the maximum violation in the pre-treatment period or that these violations do not deviate significantly—by no more than M—from a linear extrapolation of the differences in trends in the pre-treatment period. In particular, we find that the causal effect of the outsourcing reform on wages goes between 1.5 and 3.5 percentage points, ruling out a null effect when M = 1. At the same time, the "breakdown value" for a null effect is around M= 2 when the post-treatment violations of parallel trends are assumed to be twice as large as the maximal pre-treatment violation (see Figure A5).

Our findings show that the treatment effects remain stable and robust across the different sensitivity scenarios, supporting the validity of our parallel trends assumption. This consistency across alternative trend specifications suggests that the observed impacts are indeed attributable to the reform rather than to potential pre-existing differences in trends between the treated and control groups.

Post-Treatment Omitted Variable Bias. To assess the robustness of our estimated effects against potential omitted variable bias arising, for example, from selection into treatment in our event study, we apply the Oster (2019) methodology to test the stability of our results. Quantifying bias arising from unobserved factors inherently requires making assumptions about unknown quantities. In particular, applying the Oster (2019) method to account for such bias involves selecting two parameter values: (i) the relative importance of unobserved covariates for selection into

treatment compared to observed covariates (denoted as δ in Oster, 2019), and (ii) the proportion of variation in the outcome that could be explained if all covariates were observed (denoted as R_{max} in Oster, 2019). We calculate bounds for cases where the degree of proportionality of selection on observables to selection on unobservables is either set to 1 (unobservable selection goes into the same direction) or -1 (unobservable selection goes into the adverse direction). Following Oster (2019) we set $R_{max} = \min\{1.3\tilde{R}, 1\}$ to determine the maximum explanatory power that the model could achieve if all relevant covariates were included.

By comparing the sensitivity of the estimated treatment effects, Oster (2019)'s method provides a way to estimate the extent of bias from unobserved confounders needed to invalidate our results. If these bounds exclude zero, the results from the controlled regression can be considered robust to omitted variable bias. Our findings, presented in Table A5 in the Appendix, show no evidence of potential omitted variable bias for the post-treatment period. This suggests that the impact of the outsourcing reform on wages is not substantially driven by selection bias, lending credibility to our conclusions.

5.5 Heterogeneity

In our benchmark analysis, we find that the change in the outsourcing regulation significantly increases the wages of formal workers. Our estimates suggest that the effect is around 3%. However, these average effects may mask significant heterogeneity by demographic groups and firm characteristics. To further analyze the effect of outsourcing on worker wages, we document heterogeneous effects by gender (Figure A6), age (Figure A7), industry (Figure A8), firm size (Figure A9), and region (Figure A10). These different dimensions allow us to understand better the potential mechanisms driving wage changes. Also, it will provide a clearer picture of the underlying groups affected by the law's passage.

Gender. The analysis of gender-based effects reveals that women may experience a greater wage increase than men due to outsourcing regulation changes during the first months of the reform. Specifically, regularized women received an average wage increase of about 4%, while men saw a more minor increase of around 2.5% during the first month. While the immediate post-reform period saw a divergence in wage trends between women and men, it is noteworthy that this effect gradually diminished over time. Three months after the enactment of the outsourcing legislation, a convergence in the impact on women's and men's wages was observed. This finding suggests that the initial gender disparity in wage increases may have been a temporary effect, emphasizing the dynamic nature of the reform's consequences.

Age. Next, we analyze the results for the event-study specification by age. The findings suggest that the effect of the law change on earnings follows a similar pattern as the overall effect for

several age groups. Specifically, all the workers received a wage raise of around 3% regardless of age immediately after the reform. However, six months after the reform, we can observe a straightforward ordering based on age; the younger, the higher the wage rise. In particular, for workers aged 25-34, wages rose by roughly 2.5% after six months after April 2021, whereas this rise was around 2% to 1.5% for workers aged 35-44, 45-54, and 55-64, respectively.

A possible explanation for this result is that while the immediate impact of the law reform led to a uniform wage increase of around 3% across all age groups, adjustments over time in wages may reflect differences in labor market dynamics and worker productivity by age. Younger workers, particularly those aged 25-34, may have greater upward mobility, adaptability, and potential for productivity growth, making them more likely to benefit from the reform over time. Additionally, younger workers might be in positions with greater potential for wage progression compared to older workers, who may already be closer to their peak earnings.

For older workers, such as those in the 35-64 age range, wage growth might be more limited over time due to factors like reduced flexibility, being closer to retirement, or possibly occupying more stable but slower-growing positions. As a result, the wage increases six months after the reform are more modest for older age groups, with a clear pattern of smaller wage growth as age increases. This age-related wage progression suggests that younger workers are more responsive to the opportunities created by the reform.

Industry. Following the reform, we observed significant wage increases in the extractive, trade, processing (manufacturing), and business services sectors, indicating that the reform positively affected earnings. The extractive industry showed the most substantial wage growth in the first three months, with a sharp wage rise. However, this initial surge in earnings was temporary, as the wage increases in this sector gradually faded over time. A similar trend occurred in the processing sector, though the wage growth was smaller compared to the extractive industry. Notably, the trade industry stands out as the one industry where the wage increase is sustained over time, indicating a lasting positive effect on earnings beyond the initial months after the reform.

On the other hand, the transport and social services sectors experienced negative wage effects following the reform. In these industries, wages stagnated or declined, suggesting that the reform may have had unintended negative consequences for workers in these fields. This could be due to structural challenges or shifts in demand that the reform did not adequately address.

Through the lens of our conceptual framework, differences in several factors, such as the level of outsourcing, labor productivity, and the elasticities of supply and demand in each industry, can potentially explain the varied impact of the reform across industries. When outsourcing is restricted, industries that previously relied heavily on it may experience an increase in in-house labor demand, leading to wage growth. Similarly, industries with higher labor productivity will likely

see wages rise because workers in those sectors generate more value for employers. The price of intermediate services can also play a role. Industries that depend heavily on these inputs may experience short-term wage increases if production costs shift. However, these effects might fade over time as markets adjust, supply chains stabilize, or the initial benefits of reduced outsourcing diminish. Additionally, the responsiveness of labor supply and the ease of substitution between labor and other inputs affect how wages adjust to changes. Industries with more flexible labor supply or higher substitution rates tend to have more volatile wage effects. Overall, the reform's impact varied across industries, benefiting some industries while having unfavorable outcomes in others, which is consistent with our conceptual framework.

Firm Size. The findings suggest that the wage effects of the reform were concentrated primarily in large establishments, specifically those with over a thousand employees. Workers in these large firms experienced significant salary increases, with average wages rising by more than 5%. In contrast, workers in smaller establishments also saw wage increases, but these were smaller in magnitude and statistically insignificant beyond the fourth month after the reform. This result underscores the importance of considering firm size when evaluating the impact of regulatory changes.

Large firms with greater financial resilience, higher profit margins, and higher costs associated with losing employees were likely better positioned to absorb the increased costs brought by the reform and pass on benefits through wage increases to their workers. Conversely, smaller firms may have found it more challenging to adapt to the new regulatory environment, limiting their ability to offer sustained wage increases. These results align with our conceptual framework, as well as earlier findings across industries, where some industries were better equipped to handle the reform's effects.

Region. Finally, we investigate the variation in effects across regions by adopting the classification the Bank of Mexico utilizes in its regional report. The results reveal a clear pattern of geographical heterogeneity regarding the impact of outsourcing regulation on salaries.

Our findings reveal that the outsourcing regulation has led to varying changes in salary levels depending on the location. Specifically, workers in the northern and central northern regions experienced more significant wage increases compared to other areas after the introduction of the regulation. This geographic variation highlights the importance of considering regional factors when assessing the effects of regulatory changes on wages. It suggests that the impact of the reform is not uniform across the country, with some areas benefiting more than others.

In each region, these differences could be attributed to several factors, including the specific industries or sectors that dominate the local economy. For example, regions with a higher concentration of industries that rely more on outsourcing may experience greater wage shifts due to

increased in-house labor demand. Additionally, regional variations in labor productivity, the structure of the local economy, and the elasticities of supply and demand can all influence how the reform impacts wages. Considering these regional dynamics is crucial for evaluating the broader effects of the reform on employment conditions across the country.

6 Conclusion

Outsourcing labor services has become a prominent feature of the Mexican labor market, much like in other countries worldwide. However, concerns have arisen about the potential misuse of outsourcing and its impact on earnings inequality within firms. Research has shown that workers' wages tend to decrease when they switch employers from the contracting firm to the outsourcing firm. Additionally, regulators are concerned that outsourcing can be misused to undermine workers' conditions, particularly when employees perform permanent tasks central to a firm's main activities but are still classified as outsourced.

In response to these concerns, the Mexican Congress passed a labor law reform that restricts outsourcing to specialized tasks that are not part of a company's core business activities. The reform also required companies to hire outsourced workers directly if they were engaged in tasks that should be considered part of the firm's primary functions. Our estimates showed that as a result of the reform, around two million formally employed workers aged 25 to 64 switched employers, representing approximately 10% of all formal private-sector jobs in Mexico.

Then, we examined the effect of the labor outsourcing reform on formal workers' employment and wages. Using administrative data covering Mexico's formal labor market in the private sector and an event-study framework, we found that implementing the subcontracting reform in Mexico affects labor mobility and has positively impacted the wages of formal workers. In particular, after the enactment of the outsourcing regulation, regularized workers are more likely to retain their positions in private formal jobs compared to workers who stayed with their outsourcing employers and those who were not part of the regularization process. In addition, workers who changed employers due to the reform increased their wages by 3% to 4%.

This wage increase was not uniform across all workers, with differences observed by gender, age, industry, firm size, and region. These results mirrored the findings in the literature that estimated the impact when workers go in the opposite direction, i.e., outsourced workers. In this respect, this literature has found that outsourced workers' wages reduce when they switch from the main employer to the outsourcing firm. We find that workers earn higher wages when they go in the opposite direction. In other words, these findings provide important insights into the effects of labor outsourcing regulations on the wages of formal workers, suggesting that policy changes

aimed at regulating subcontracting practices can positively affect employees' earnings. Future research can build on these findings by exploring the long-term impacts of the reform on workers' wages and its effects on employment and job security.

One limitation of our paper is that it does not explore the effects of restricting outsourcing on firm-level outcomes, such as profitability, productivity, and competitiveness. Limiting outsourcing may reduce firms' operational flexibility, making it harder to adapt to market changes or streamline production processes, potentially leading to higher costs and inefficiencies. Outsourcing also allows firms to focus on core activities by delegating specialized tasks, and restricting this could hinder their ability to innovate or scale. Further research is needed to understand how these reforms impact firms and whether certain sectors are more affected than others.

Overall, our study underscores the importance of careful consideration of the effects of labor outsourcing regulations on workers and contributes to the ongoing discussions surrounding labor outsourcing and its impact on the welfare of employees.

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A Appendix

A.1 Descriptive statistics

Appendix Table A1. Sample Check

	Total Sample		Sub-sample		Rest		Difference	t-value
	Mean	SD	Mean	SD	Mean	SD		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wage	528.14	449.52	529.65	450.17	528.01	449.46	-1.644	(-1.357)
Age	38.29	9.34	38.31	9.38	38.29	9.34	-0.022	(-0.859)
Men	0.62	0.49	0.62	0.49	0.62	0.49	0.000	(0.267)
Women	0.38	0.49	0.38	0.49	0.38	0.49	-0.000	(-0.267)
Industry								
Agriculture	0.02	0.13	0.02	0.13	0.02	0.13	-0.001	(-1.413)
Extractive	0.01	0.11	0.01	0.11	0.01	0.11	-0.000	(-1.525)
Processing	0.31	0.46	0.31	0.46	0.31	0.46	-0.001	(-1.101)
Construction	0.02	0.15	0.02	0.15	0.02	0.15	0.000	(1.144)
Electricity	0.00	0.04	0.00	0.04	0.00	0.04	0.000	(0.385)
Trade	0.34	0.47	0.34	0.47	0.34	0.47	0.003*	(1.996)
Transportation	0.06	0.24	0.06	0.24	0.06	0.24	-0.000	(-0.407)
Business Services	0.22	0.42	0.23	0.42	0.22	0.42	-0.000	(-0.414)
Social Services	0.01	0.11	0.01	0.11	0.01	0.11	-0.000	(-0.007)
Region								
North	0.23	0.42	0.23	0.42	0.23	0.42	0.001	(0.487)
Center-North	0.17	0.38	0.18	0.38	0.17	0.38	-0.003**	(-2.772)
Center	0.48	0.50	0.48	0.50	0.48	0.50	0.002	(1.653)
South	0.11	0.31	0.11	0.31	0.11	0.31	0.000	(0.060)
Firm Size								
1 worker	0.00	0.01	0.00	0.01	0.00	0.01	0.000	(1.673)
2 to 5	0.00	0.02	0.00	0.02	0.00	0.02	0.000	(1.644)
6 to 50	0.02	0.13	0.02	0.13	0.02	0.13	0.000	(1.144)
51 to 250	0.28	0.45	0.28	0.45	0.28	0.45	0.001	(0.572)
251 to 500	0.19	0.39	0.19	0.39	0.19	0.39	0.002	(1.549)
501 to 1,000	0.18	0.38	0.18	0.38	0.18	0.38	-0.002*	(-2.292)
More than 1,000	0.34	0.47	0.34	0.47	0.34	0.47	-0.001	(-0.406)
Observations	1,834,930		150,000		1,684,930		1,834,930	

Notes: The table shows a set of worker characteristics for the sample of 150 thousand outsourced workers, compared to the outsourced workers not in sample. This table includes only workers with wage information for the 6 months previous to the change in the law.

Appendix Table A2. Balance Table: 2021

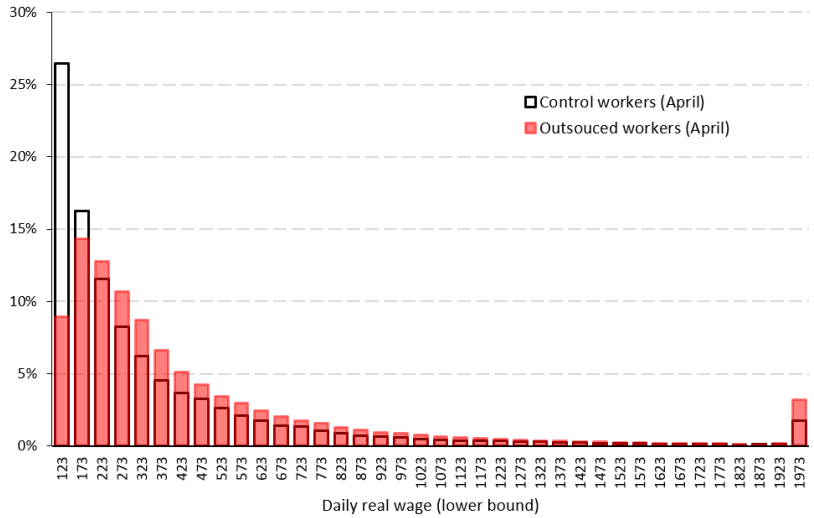
	Total Sample		Treated		Non-treated		Difference	t-value
	Mean	SD	Mean	SD	Mean	SD		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wage	504.54	413.43	506.08	413.72	503.00	413.15	-3.084	(-1.796)
Age	38.17	9.31	38.19	9.36	38.15	9.26	-0.047	(-1.214)
Men	0.62	0.48	0.62	0.48	0.62	0.48	0.001	(0.270)
Women	0.38	0.48	0.38	0.48	0.38	0.48	-0.001	(-0.270)
Industry								
Agriculture	0.02	0.13	0.02	0.13	0.02	0.13	0.000	(0.032)
Extractive	0.01	0.10	0.01	0.10	0.01	0.10	0.000	(0.000)
Processing	0.34	0.47	0.34	0.47	0.34	0.47	0.000	(0.092)
Construction	0.02	0.15	0.02	0.15	0.02	0.15	-0.000	(-0.013)
Electricity	0.00	0.03	0.00	0.03	0.00	0.03	0.000	(0.000)
Trade	0.30	0.46	0.30	0.46	0.30	0.46	-0.000	(-0.104)
Transportation	0.06	0.24	0.06	0.24	0.06	0.24	-0.000	(-0.009)
Business Services	0.23	0.42	0.23	0.42	0.23	0.42	-0.000	(-0.020)
Social Services	0.02	0.12	0.02	0.12	0.02	0.12	0.000	(0.102)
Region								
North	0.25	0.43	0.25	0.43	0.25	0.43	0.000	(0.043)
Center-North	0.18	0.39	0.18	0.39	0.18	0.39	-0.000	(-0.032)
Center	0.47	0.50	0.47	0.50	0.47	0.50	0.000	(0.125)
South	0.11	0.31	0.11	0.31	0.11	0.31	-0.000	(-0.223)
Firm Size								
1 worker	0.00	0.01	0.00	0.01	0.00	0.01	0.000	(0.258)
2 to 5	0.00	0.02	0.00	0.02	0.00	0.03	0.000	(1.497)
6 to 50	0.01	0.10	0.01	0.09	0.01	0.11	0.005***	(10.887)
51 to 250	0.27	0.45	0.27	0.45	0.27	0.44	-0.004*	(-2.002)
251 to 500	0.19	0.39	0.19	0.39	0.19	0.39	-0.002	(-1.136)
501 to 1,000	0.18	0.39	0.18	0.39	0.19	0.39	0.003*	(2.129)
More than 1,000	0.34	0.48	0.35	0.48	0.34	0.47	-0.003	(-1.364)
Observations	231,790		115,895		115,895		231,790	

Notes: The table shows a set of worker characteristics for the sample of outsourced workers in the treatment group, compared to the outsourced workers in the control group.

A.2 Wage Distribution

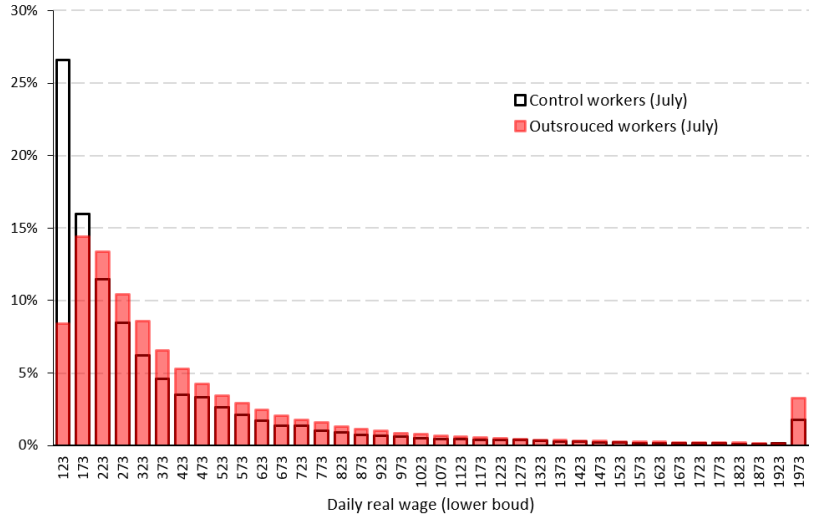
In Figure A1, we can see how the outsourced workers' wage distribution compares to that of the other IMSS workers. We can see that they have, on average, higher wages. In particular, the percentage of outsourced workers that in April 2021 earned between $123 and $173 (category with smallest wages) is around 9%, compared to 27% for the rest of the workers. In Figure A2, we can see that the differences between both groups (outsourced and the rest) increased in July 2021, where that category, in particular, decreased for the outsourced workers and the percentage of workers earning between $223 and $273 increased (which is not the case for the rest of the workers, in either category).

Appendix Figure A1. Daily real wage for Outsourced workers and Control workers in April 2021 (pesos)



Source: Own elaboration based on IMSS data.

Appendix Figure A2. Daily real wage for Outsourced workers and Control workers in July 2021 (pesos)



Source: Own elaboration based on IMSS data.

A.3 Additional Results

Appendix Table A3. Main Results

	$\ln(w)$ (1)	$\ln(w)$ (2)	$\ln(w)$ (3)	$\ln(w)$ (4)	Wage (5)	Wage (6)
Periods						
$k=-6$	-0.004***	-0.002***	-0.006*	-0.005	-0.940	-1.033*
	(0.001)	(0.001)	(0.003)	(0.003)	(0.574)	(0.547)
$k=-5$	-0.003***	-0.002**	-0.005*	-0.004*	-0.561	-0.779
	(0.001)	(0.001)	(0.002)	(0.002)	(0.565)	(0.543)
$k=-4$	-0.001	-0.000	-0.002	-0.002	-1.131*	-1.257**
	(0.001)	(0.001)	(0.002)	(0.001)	(0.603)	(0.586)
$k=-3$	-0.001	0.000	-0.001	-0.001	-0.841	-0.854
	(0.001)	(0.001)	(0.001)	(0.001)	(0.593)	(0.582)
$k=-2$	-0.000	0.001***	0.000	0.000	0.437***	0.520***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.154)	(0.122)
$k=0$	0.032***	0.031***	0.032***	0.032***	16.469***	16.346***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.624)	(0.623)
$k=1$	0.033***	0.032***	0.033***	0.033***	16.673***	16.584***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.637)	(0.632)
$k=2$	0.026***	0.021***	0.023***	0.023***	14.076***	16.316***
	(0.001)	(0.002)	(0.004)	(0.004)	(0.679)	(1.572)
$k=3$	0.026***	0.024***	0.027***	0.026***	14.697***	17.659***
	(0.001)	(0.003)	(0.004)	(0.004)	(0.694)	(1.574)
$k=4$	0.019***	0.017***	0.021***	0.020***	5.035***	8.310***
	(0.001)	(0.003)	(0.005)	(0.005)	(0.653)	(1.551)
$k=5$	0.017***	0.017***	0.021***	0.020***	4.227***	7.827***
	(0.001)	(0.003)	(0.006)	(0.005)	(0.662)	(1.555)
$k=6$	0.018***	0.018***	0.023***	0.022***	4.612***	8.382***
	(0.001)	(0.003)	(0.007)	(0.006)	(0.683)	(1.561)
Constant	6.005***	6.006***	6.007***	6.007***	523.912***	523.447***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.443)	(0.515)
N	2,606,123	2,604,918	2,604,918	2,604,918	2,606,123	2,604,918
R^2	0.938	0.956	0.956	0.956	0.937	0.948
Baseline FE	✓	✓	✓	✓	✓	✓
Firm FE		✓	✓	✓		✓
Trend 2 periods			✓			
Trend 3 periods				✓		

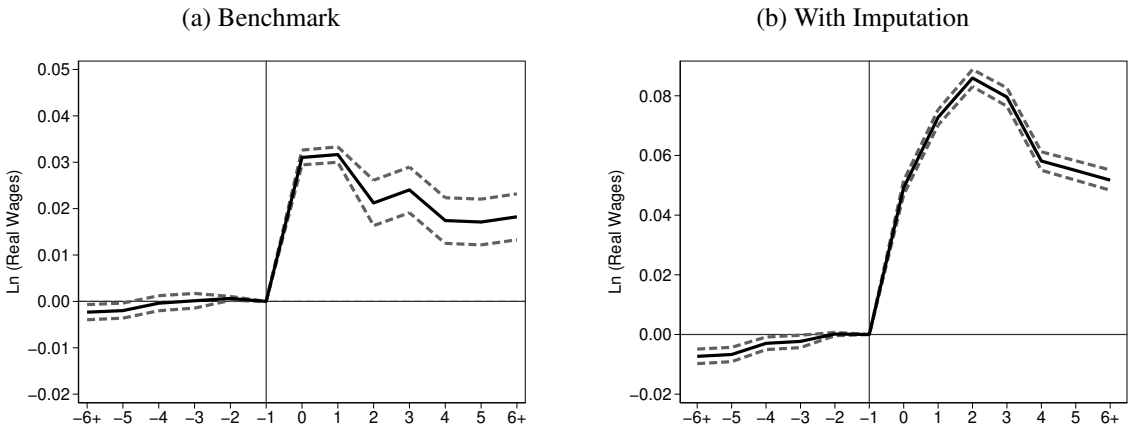
Notes: Coefficients are event-study dummy variables, β_k, from the estimation of Eq. (18). The period before the reform (-1) is the excluded period. Each period (k) represents a month. The baseline fixed effects include both month and individual-level fixed effects. Additionally, some specifications also incorporate firm level fixed effects. Robust standard errors are clustered at the individual level. Significance levels: *p<0.01, **p<0.05, ***p<0.01.

Appendix Table A4. Main Results Static Model

	$\ln(w)$ (1)	$\ln(w)$ (2)	Wage (3)	Wage (4)
z	0.043***	0.045***	20.271***	25.925***
	(0.001)	(0.001)	(0.338)	(0.429)
N	2,606,123	2,604,918	2,606,123	2,606,123
R^2	0.959	0.943	0.952	0.941
Baseline FE	✓	✓	✓	✓
Firm FE		✓		✓

Notes: The table presents the results of a panel data model which assumes a static policy effect, meaning that the current value of the policy influences only the current value of the outcome. Baseline fixed effects are included at the individual level, and month. Additionally, RFC fixed effect are included in primary results. Robust standard errors are clustered at the individual level. Significance levels: *p<0.01, **p<0.05, ***p<0.01..

Appendix Figure A3. Event Study: Year 2021 with Imputation

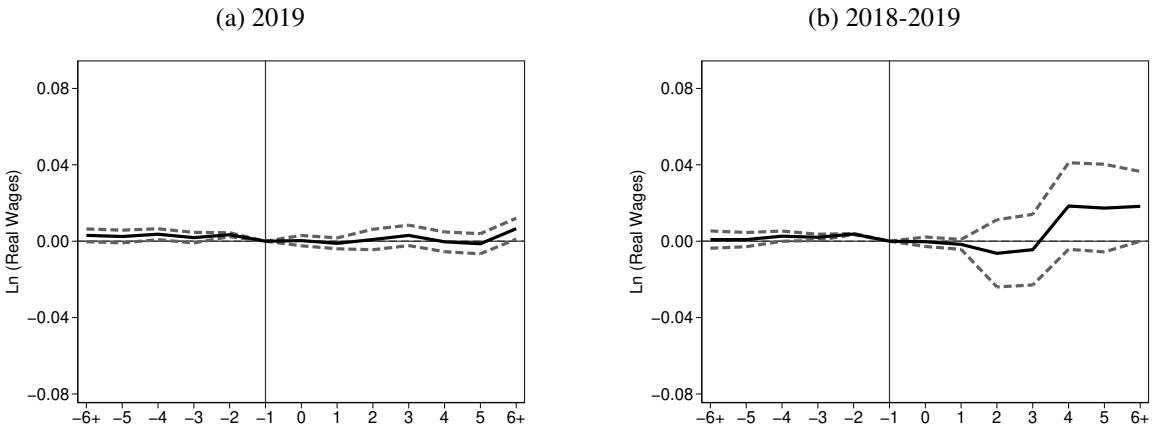


Notes: The figure shows the coefficients from Eq. (18) for several the benchmark specification and for an alternative specification in which we impute zeroes to wages for workers outside of the formal labor market. We conducted a Poisson regression with an imputation of zero for wages of workers who exited the formal sector. This method acknowledges the impact of the reform on formal sector retention while addressing potential differences in the probability of remaining employed formally. Baseline fixed effects were included. The periods before -1 include 2020m11–2021m3. The omitted period represents 2021m4, represented by the vertical line. The post periods represent 2021m5–2021m12. The intervals are pointwise 95 percent confidence intervals for the corresponding elements. Robust standard errors are clustered at the individual-level. Individual-level data from the IMSS database.

A.4 Robustness

A.4.1 Placebo

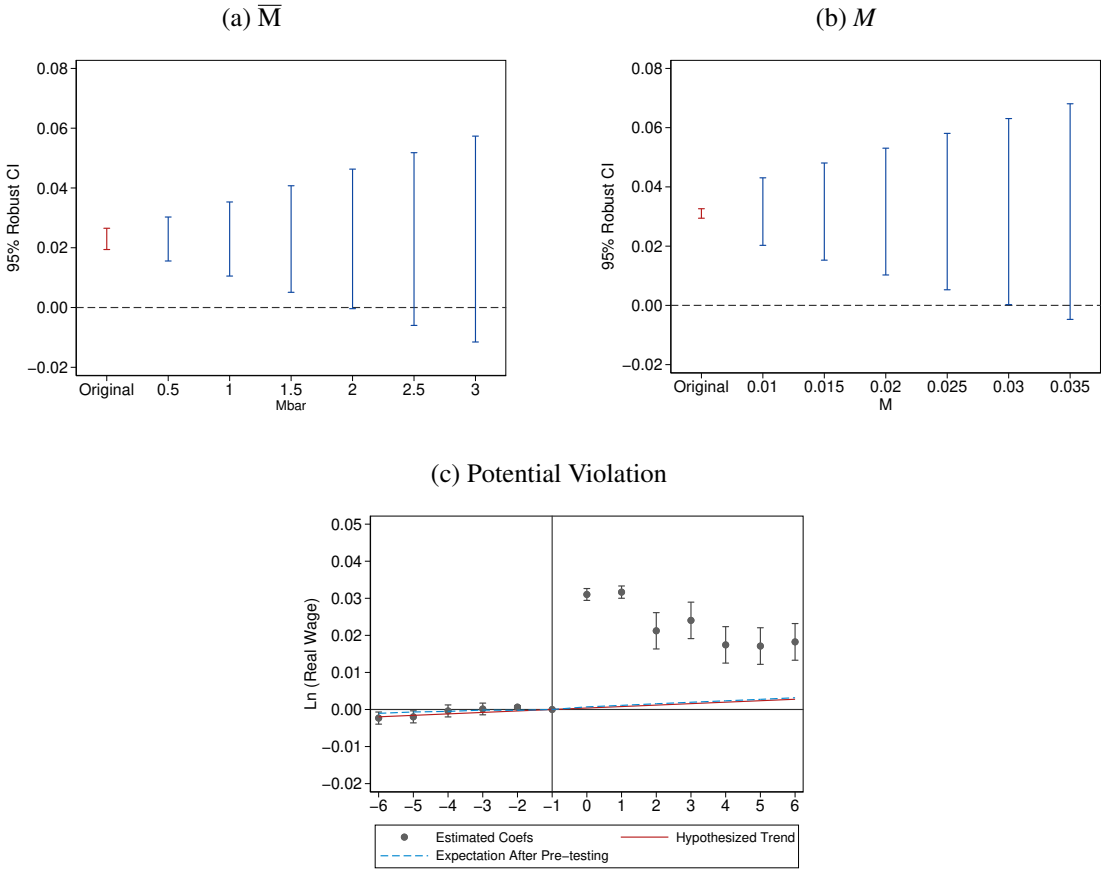
Appendix Figure A4. Event Study: Years 2018-2019



Notes: The figure shows the coefficients from Eq. (18) for several specifications of the model. Baseline fixed effects include industry, region, firm size, and individual-level fixed effects. The periods before -1 include the average of 2017m11–2018m3 and 2018m11–2019m3. The omitted period represents the average of 2018m4, 2019m4 and 2020m4, represented by the vertical line. The post periods represent the average of 2018m5–2018m12 and 2019m5–2019m12. The intervals are pointwise 95 percent confidence intervals for the corresponding elements. Robust standard errors are clustered at the individual-level. Individual-level data from the IMSS database.

A.4.2 Parallel Trends

Appendix Figure A5. Parallel Trends Sensitivity Analysis



Notes: The figure displays the results of the sensitivity analysis of parallel trends following the methodology of Rambachan and Roth (2023).

A.4.3 Omitted Variable Bias

Appendix Table A5. Sensitivity of Main Results of the Event Study using Oster Methodology

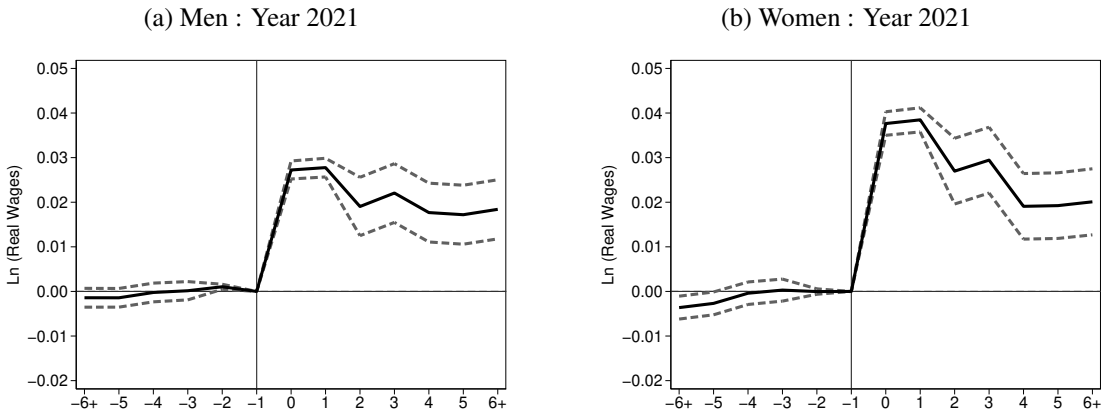
	Periods						
	(0)	(1)	(2)	(3)	(4)	(5)	(6)
1(Outsourced)	0.0310 [0.0307, 0.0314]	0.0317 [0.0310, 0.0325]	0.0212 [0.0181, 0.0229]	0.0240 [0.0213, 0.0255]	0.0174 [0.0148, 0.0222]	0.0171 [0.0133, 0.0241]	0.0182 [0.0143, 0.0254]
N	2,604,918	2,604,918	2,604,918	2,604,918	2,604,918	2,604,918	2,604,918
R^2	0.96	0.96	0.96	0.96	0.96	0.96	0.96
Baseline FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓

Notes: The intervals in squares shows the Oster (2019) bounds around the periods zero, one, two, three, four, five, and six regarding the event study estimations of the main results. In each column of the table, we show the identified set $[\gamma(\delta = -1), \gamma(\delta = 1)]$ under an $R_{max} = \min\{1.3\tilde{R}, 1\}$.

A.5 Heterogeneity

A.5.1 Gender

Appendix Figure A6. Event Study: By Sex

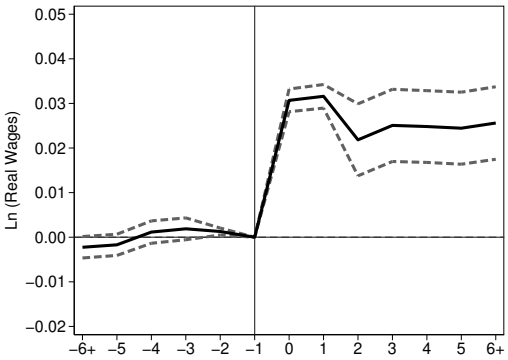


Notes: The figure shows the coefficients from Eq. (18) for several specifications of the model. Baseline fixed effects include industry, region, firm size, and individual-level fixed effects. The periods before -1 include 2020m11–2021m3. The omitted period represents 2021m4, represented by the vertical line. The post periods represent 2021m5–2021m12. The intervals are pointwise 95 percent confidence intervals for the corresponding elements. Robust standard errors are clustered at the individual-level. Individual-level data from the IMSS database.

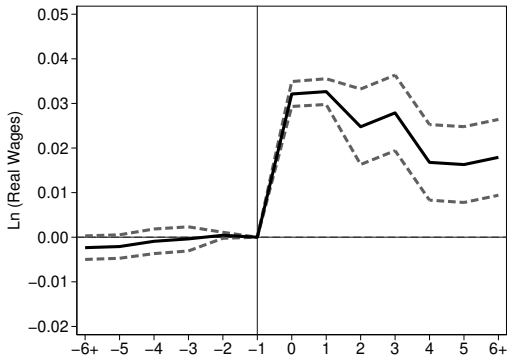
A.5.2 Age

Appendix Figure A7. Event Study: Year 2021 By Age Group (Fixed Effects)

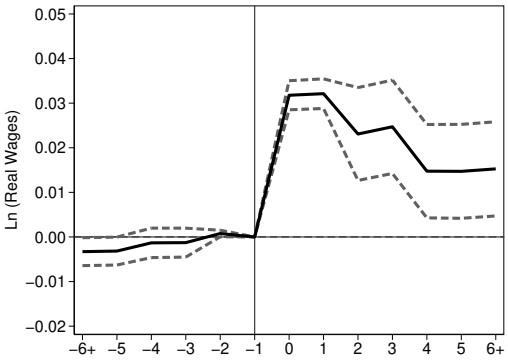
(a) 25 to 34 years old



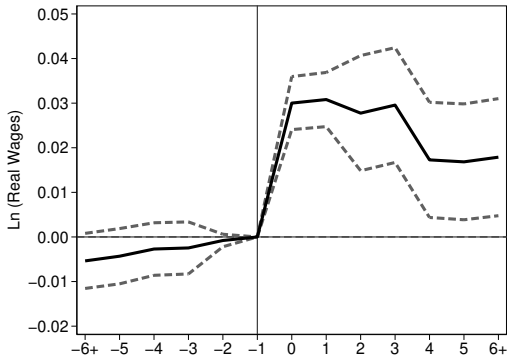
(b) 35 to 44 years old



(c) 45 to 54 years old



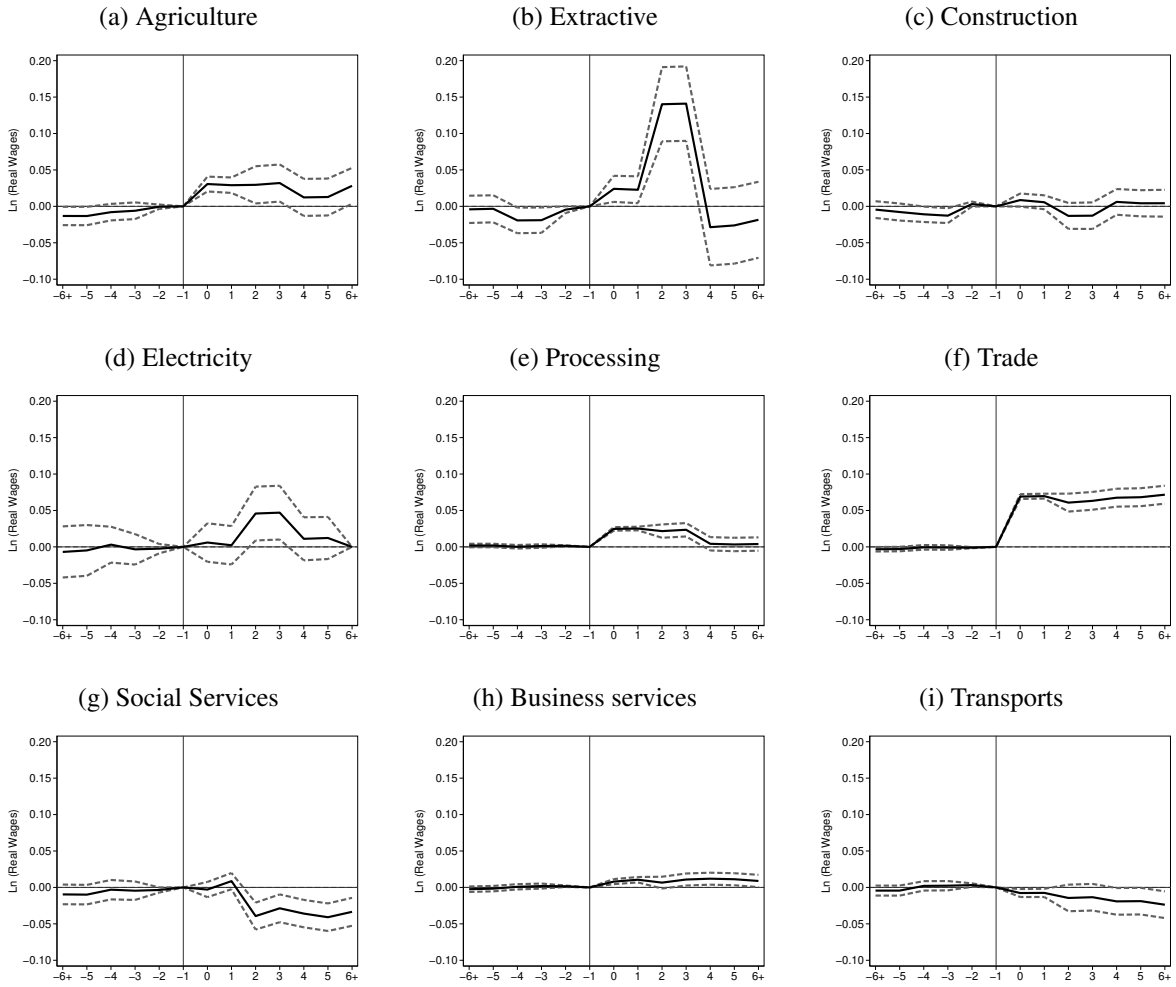
(d) 55 to 64 years old



Notes: The figure shows the coefficients from Eq. (18) for several specifications of the model. Baseline fixed effects include industry, region, firm size, and individual-level fixed effects. The periods before -1 include 2020m11–2021m3. The omitted period represents 2021m4, represented by the vertical line. The post periods represent 2021m5–2021m12. The intervals are pointwise 95 percent confidence intervals for the corresponding elements. Robust standard errors are clustered at the individual-level. Individual-level data from the IMSS database.

A.5.3 Industry

Appendix Figure A8. Event Study: Year 2021 By Industry (Fixed Effects)

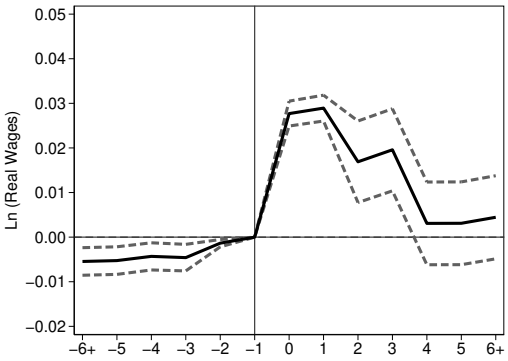


Notes: The figure shows the coefficients from Eq. (18) for several specifications of the model. Baseline fixed effects include region, firm size, and individual-level fixed effects. The periods before -1 include 2020m11–2021m3. The omitted period represents 2021m4, represented by the vertical line. The post periods represent 2021m5–2021m12. The intervals are pointwise 95 percent confidence intervals for the corresponding elements. Robust standard errors are clustered at the individual level. Individual-level data from the IMSS database.

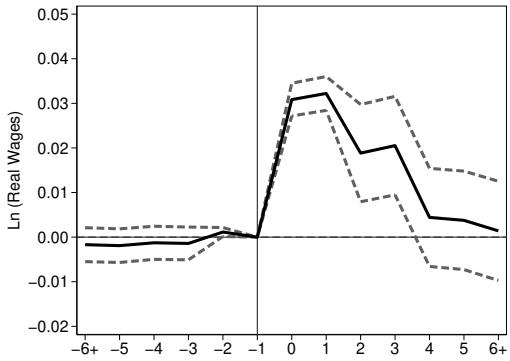
A.5.4 Firm Size

Appendix Figure A9. Event Study: Year 2021 By Firm Size (Fixed Effects)

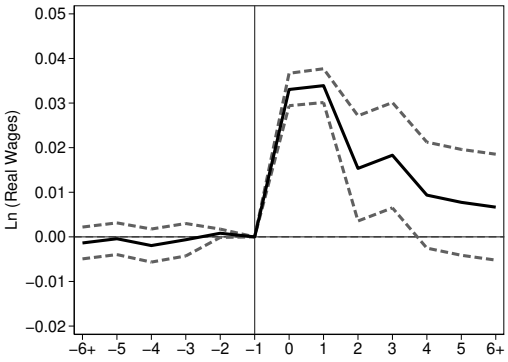
(a) 51 to 250 workers



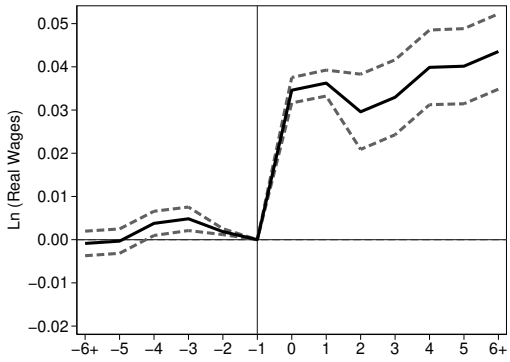
(b) 251 to 500 workers



(c) 501 to 1000 workers



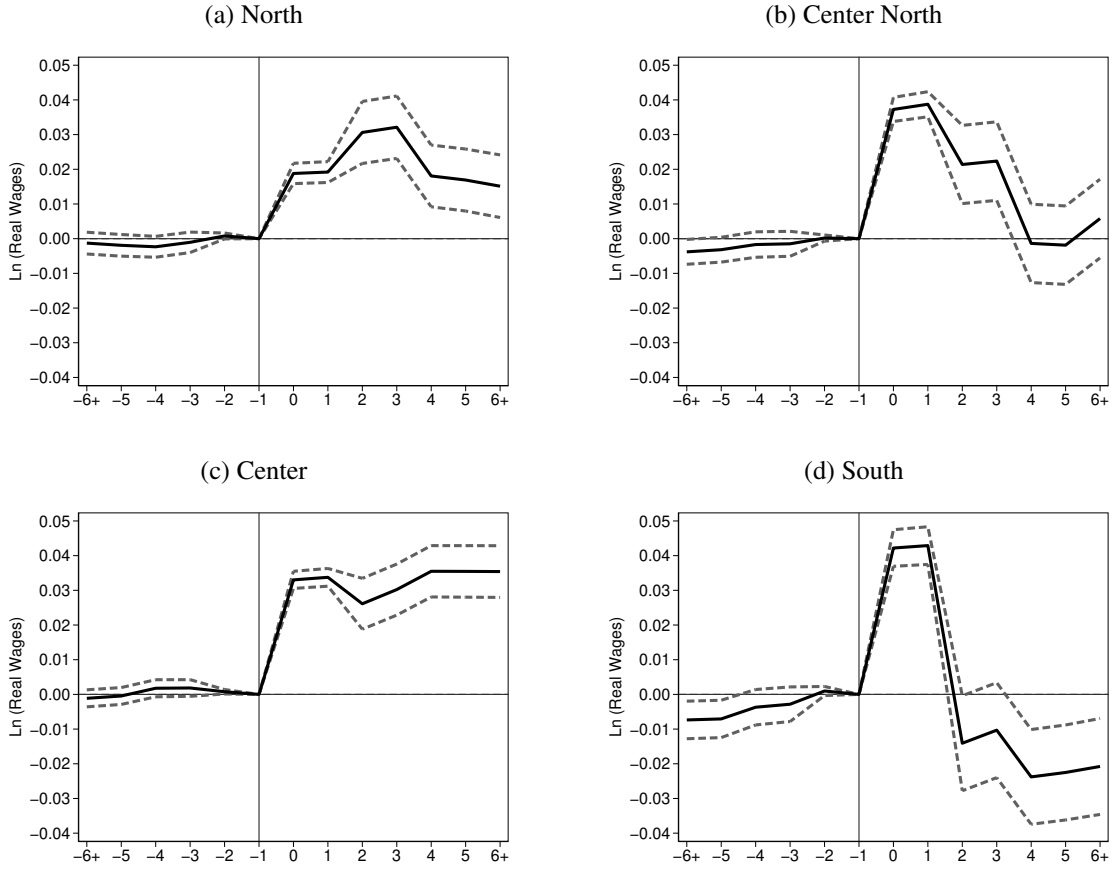
(d) More than 1000 workers



Notes: The figure shows the coefficients from Eq. (18) for several specifications of the model. Baseline fixed effects include industry, region, and individual-level fixed effects. The periods before -1 include 2020m11–2021m3. The omitted period represents 2021m4, represented by the vertical line. The post periods represent 2021m5–2021m12. The intervals are pointwise 95 percent confidence intervals for the corresponding elements. Robust standard errors are clustered at the individual level. Individual-level data from the IMSS database.

A.5.5 Region

Appendix Figure A10. Event Study: Year 2021 By Region (Fixed Effects)



Notes: The figure shows the coefficients from Eq. (18) for several specifications of the model. Baseline fixed effects include industry, firm size, and individual-level fixed effects. The periods before -1 include 2020m11–2021m3. The omitted period represents 2021m4, represented by the vertical line. The post periods represent 2021m5–2021m12. The intervals are pointwise 95 percent confidence intervals for the corresponding elements. Robust standard errors are clustered at the individual level. Individual-level data from the IMSS database.

B Online Appendix: Model Derivations

B.1 Comparative Statics of Labor Demand with Respect to I

To find the effect of I on LD_1, we need to differentiate $LD_1(w_1)$ with respect to I:

$$\frac{dLD_1}{dI} = \frac{d}{dI}\left[(1-I)\kappa\left[IP_m + (1-I)\frac{w_1}{A_1}\right]^{-\sigma}\right]$$

Using the product rule:

$$\frac{dLD_1}{dI} = \frac{d(1-I)}{dI} \cdot \kappa \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma} + (1-I) \cdot \frac{d}{dI} \left[\kappa \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma} \right]$$

Now, compute each part. In the first term, we have that $\frac{d(1-I)}{dI} = -1$. Therefore, the first term becomes:

$$-\kappa \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma}$$

For the second term, lets focus on $\frac{d}{dI} \left[\kappa \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma} \right]$. Using the chain rule, we get:

$$\kappa \cdot \frac{d}{dI} \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma} = \kappa \cdot -\sigma \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma-1} \cdot \frac{d}{dI} \left[IP_m + (1-I)\frac{w_1}{A_1} \right]$$

Differentiate inside the term:

$$\frac{d}{dI} \left[IP_m + (1-I)\frac{w_1}{A_1} \right] = P_m - \frac{w_1}{A_1}$$

So:

$$\kappa \cdot -\sigma \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma-1} \cdot (P_m - \frac{w_1}{A_1})$$

Now, combine the two terms:

$$\frac{dLD_1}{dI} = -\kappa \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma} + (1-I) \cdot \left[\kappa \cdot -\sigma \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma-1} \cdot (P_m - \frac{w_1}{A_1}) \right]$$

Factor out common terms:

$$\frac{dLD_1}{dI} = \kappa \left[IP_m + (1-I)\frac{w_1}{A_1} \right]^{-\sigma} \left[-1 - \sigma(1-I)\frac{P_m - \frac{w_1}{A_1}}{IP_m + (1-I)\frac{w_1}{A_1}} \right]$$

Simplifying, we get:

$$\frac{dLD_1}{dI} = -\kappa \left(IP_m + (1-I)\frac{w_1}{A_1} \right)^{-\sigma} \left[1 + \sigma \frac{(1-I)(P_m - \frac{w_1}{A_1})}{IP_m + (1-I)\frac{w_1}{A_1}} \right]$$

Derivation of Conditions for the Sign of: $\frac{dLD_1(w_1)}{dI}$

To provide a condition on σ when $\frac{dLD_1}{dI} \lessgtr 0$, we need to analyze the term inside the brackets

more carefully. Recall that the partial derivative of labor demand with respect to I is:

$$\frac{dLD_1(w_1)}{dI} = -\kappa \left(IP_m + (1-I)\frac{w_1}{A_1} \right)^{-\sigma} \left[1 + \sigma \frac{(1-I)(P_m - \frac{w_1}{A_1})}{IP_m + (1-I)\frac{w_1}{A_1}} \right]$$

For this expression to be positive or negative, the sign of the term inside the brackets $\left[1 + \sigma \frac{(1-I)(P_m - \frac{w_1}{A_1})}{IP_m + (1-I)\frac{w_1}{A_1}} \right]$ plays a critical role. Let's denote this term as T:

$$T = 1 + \sigma \frac{(1-I)(P_m - \frac{w_1}{A_1})}{IP_m + (1-I)\frac{w_1}{A_1}}$$

Let's start with the condition for $\frac{dLD_1}{dI} < 0$, as the other conditions will follow symmetrically. The derivative $\frac{dLD_1}{dI}$ is negative if $T > 0$:

$$T = 1 + \sigma \frac{(1-I)(P_m - \frac{w_1}{A_1})}{IP_m + (1-I)\frac{w_1}{A_1}} > 0$$

Given that $P_m < \frac{w_1}{A_1}$, then $P_m - \frac{w_1}{A_1} < 0$. This means that the term $\sigma \frac{(1-I)(P_m - \frac{w_1}{A_1})}{IP_m + (1-I)\frac{w_1}{A_1}}$ is negative. For T to remain positive, σ must be such that the absolute value of the negative term is less than 1. Mathematically:

$$\sigma \frac{(1-I)|P_m - \frac{w_1}{A_1}|}{IP_m + (1-I)\frac{w_1}{A_1}} < 1$$

Simplifying, we get:

$$\sigma < \frac{IP_m + (1-I)\frac{w_1}{A_1}}{(1-I)(\frac{w_1}{A_1} - P_m)}$$

Therefore, $\frac{dLD_1}{dI} \lesseqgtr 0$ if $\sigma \lesseqgtr \frac{IP_m + (1-I)\frac{w_1}{A_1}}{(1-I)(\frac{w_1}{A_1} - P_m)}$. These conditions provide a clear criterion based on σ for determining whether an increase in I (the extent of outsourcing) will increase or decrease labor demand in sector 1.

B.2 Comparative Statics of $\log LD_1$ with Respect to I

We start by taking the natural logarithm of the labor demand:

$$\log LD_1(w_1) = \log(1-I) + \log \kappa - \sigma \log \left[IP_m + (1-I)\frac{w_1}{A_1} \right]$$

Looking at the first term $\log(1-I)$, we have that:

$$\frac{d\log(1-I)}{dI} = \frac{-1}{1-I}$$

The second term is constant with respect to I, so:

$$\frac{d\log\kappa}{dI} = 0$$

Now, for the third term, $-\sigma\log[IP_m + (1-I)\frac{w_1}{A_1}]$, we use the chain rule to get:

$$\frac{d}{dI}\left[-\sigma\log(IP_m + (1-I)\frac{w_1}{A_1})\right] = -\sigma\frac{1}{IP_m + (1-I)\frac{w_1}{A_1}} \cdot \frac{d(IP_m + (1-I)\frac{w_1}{A_1})}{dI}$$

Differentiating inside the expression we get:

$$\frac{d(IP_m + (1-I)\frac{w_1}{A_1})}{dI} = P_m - \frac{w_1}{A_1}$$

So the third term becomes:

$$-\sigma\frac{1}{IP_m + (1-I)\frac{w_1}{A_1}}(P_m - \frac{w_1}{A_1})$$

Combining all the terms, we get:

$$\frac{d\log LD_1}{dI} = \frac{-1}{1-I} - \sigma\frac{P_m - \frac{w_1}{A_1}}{IP_m + (1-I)\frac{w_1}{A_1}}$$

B.3 Comparative Statics of w_1 with Respect to I

To analyze the effect of outsourcing on wages, we need to differentiate the equilibrium wage w_1 with respect to the extent of outsourcing I. Recall the equilibrium wage w_1 is implicitly defined by the following equation:

$$w_1^\beta = (1-I)\kappa\left[IP_m + (1-I)\frac{w_1}{A_1}\right]^{-\sigma}\left(w_1^\beta + A_2^\beta + (P_m A_m)^\beta\right)$$

Let´s differentiate the equation with respect to I on both sides:

$$\frac{d}{dI}\left(w_1^\beta\right) = \frac{d}{dI}\left[(1-I)\kappa\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}\left(w_1^\beta + A_2^\beta + (P_m A_m)^\beta\right)\right]$$

Differentiate the left-hand side with respect to I give us:

$$\frac{d}{dI}\left(w_1^{\beta}\right) = \beta w_1^{\beta-1}\frac{dw_1}{dI}$$

Now, to differentiate the right-hand side, we need to apply the product and chain rules. This side is a product of three functions: $(1-I)$, $\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}$, and $\left(w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}\right)$.

Lets start by Differentiating $(1-I)\kappa$, which give us:

$$\frac{d(1-I)}{dI} = -\kappa$$

Next, we differentiate $\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}$, to obtain:

$$\frac{d}{dI}\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma} = -\sigma\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma-1}\left(P_m - \frac{w_1}{A_1} + (1-I)\frac{1}{A_1}\frac{dw_1}{dI}\right)$$

Finally, we we differentiate the expression $\left(w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}\right)$ to get:

$$\frac{d}{dI}\left(w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}\right) = \beta w_1^{\beta-1}\frac{dw_1}{dI}$$

Putting all these terms together, we obtain the following:

$$\frac{d}{dI}\left[(1-I)\kappa\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}\left(w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}\right)\right]$$

$$= -\kappa\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}\left(w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}\right)$$

$$+(1-I)\kappa\left[-\sigma\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma-1}\left(P_m - \frac{w_1}{A_1} + (1-I)\frac{1}{A_1}\frac{dw_1}{dI}\right)\right]\left(w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}\right)$$

$$+(1-I)\kappa\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}\beta w_1^{\beta-1}\frac{dw_1}{dI}$$

Now, we group all terms that involve $\frac{dw_1}{dI}$ on the left-hand side:

$$\beta w_1^{\beta-1}\frac{dw_1}{dI} + (1-I)\kappa\beta w_1^{\beta-1}\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}\frac{dw_1}{dI}$$

$$-(1-I)\kappa\sigma\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma-1}\frac{1}{A_1}\left(w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}\right)\frac{dw_1}{dI}$$

Then, we isolate them:

$$\left[\beta w_1^{\beta-1} + (1-I)\kappa\beta w_1^{\beta-1}\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma} - (1-I)\kappa\sigma\frac{1}{A_1}\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma-1}\right]\frac{dw_1}{dI}$$

And we obtain an expression for $\frac{dw_1}{dI}$:

$$\frac{dw_1}{dI} = \frac{-\kappa\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}\left(w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}\right)\left[1 + \sigma\frac{(1-I)\left(P_m - \frac{w_1}{A_1}\right)}{IP_m + (1-I)\frac{w_1}{A_1}}\right]}{\beta w_1^{\beta-1} + (1-I)\kappa\beta w_1^{\beta-1}\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma} - (1-I)\kappa\sigma\frac{1}{A_1}\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma-1}}$$

Finally, we simplify this expression to get:

$$\frac{dw_1}{dI} = \frac{-\kappa\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}\left[1 + \sigma\frac{(1-I)\left(P_m - \frac{w_1}{A_1}\right)}{IP_m + (1-I)\frac{w_1}{A_1}}\right]}{\frac{\beta w_1^{\beta-1}}{\left(w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}\right)} - \frac{\kappa\sigma(1-I)^2\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma-1}}{A_1}}$$

Derivation of Conditions for the Sign of: $\frac{dw_1}{dI}$

To determine the sign of $\frac{dw_1}{dI}$, we analyze the expression for $\frac{dw_1}{dI}$ under the assumption that $P_m < \frac{w_1}{A_1}$. Let's start analyzing the numerator:

$$-\kappa\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}\left[1 + \sigma\frac{(1-I)\left(P_m - \frac{w_1}{A_1}\right)}{IP_m + (1-I)\frac{w_1}{A_1}}\right]$$

- The term $\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}$ is positive because it is a cost raised to a negative power $-\sigma$.
- The term $\left(P_m - \frac{w_1}{A_1}\right)$ is negative under the assumption that $P_m < \frac{w_1}{A_1}$.

Thus, the expression $1 + \sigma\frac{(1-I)\left(P_m - \frac{w_1}{A_1}\right)}{IP_m + (1-I)\frac{w_1}{A_1}}$ depends on the value of σ and the relative magnitudes of the other terms. Specifically:

- If σ is small, the term inside the brackets could remain positive, but it could become negative if σ is large enough because $\frac{(1-I)\left(P_m - \frac{w_1}{A_1}\right)}{IP_m + (1-I)\frac{w_1}{A_1}}$ is negative.

- Therefore, the sign of the numerator could be positive or negative, depending on whether σ is large enough to dominate the positive term (which would make the numerator positive) or not (which would make the numerator negative).

Now, let's analyze the denominator:

$$\frac{\beta w_1^{\beta-1}}{w_1^{\beta}+A_2^{\beta}+(P_m A_m)^{\beta}} - \frac{\kappa\sigma(1-I)^2\left(IP_m+(1-I)\frac{w_1}{A_1}\right)^{-\sigma-1}}{A_1}$$

- The first term $\frac{\beta w_1^{\beta-1}}{w_1^{\beta}+A_2^{\beta}+(P_m A_m)^{\beta}}$ is positive.
- The second term $-\frac{\kappa\sigma(1-I)^2\left(IP_m+(1-I)\frac{w_1}{A_1}\right)^{-\sigma-1}}{A_1}$ is negative

The sign of the denominator depends on whether the positive term is larger than the negative term. Therefore, the sign of $\frac{dw_1}{dI}$ is ambiguous and depends on the relative magnitudes of the parameters σ, κ, β, and the relationship between P_m and $\frac{w_1}{A_1}$.

- **Condition for $\frac{dw_1}{dI} < 0$ (wage decreases with outsourcing):** This is more likely if the numerator is negative and the denominator is positive, which can occur if σ is small enough that the negative term in the numerator does not dominate, and if the positive term in the denominator dominates the negative term.
- **Condition for $\frac{dw_1}{dI} > 0$ (wage increases with outsourcing):** This is more likely if the numerator is positive (which requires a large σ) and the denominator is negative, or if the numerator is negative but the denominator is also negative.

Thus, the sign of $\frac{dw_1}{dI}$ is ambiguous and depends on the specific values of the parameters and the cost structure in the economy.

B.4 Comparative Statics of $\log w_1$ with Respect to I

To analyze how changes in I affect w_1, we need to find the partial derivative of $\log w_1$ with respect to I: First, we simplify the wage equation in logarithmic form. The equilibrium condition for the wage w_1 is given by:

$$w_1^{\beta} = (1-I)\kappa\left[IP_m+(1-I)\frac{w_1}{A_1}\right]^{-\sigma}\left[w_1^{\beta}+A_2^{\beta}+(P_m A_m)^{\beta}\right]$$

To differentiate $\log(w_1)$ with respect to I, we first take the logarithm of both sides of the equilibrium condition:

$$\log(w_1^\beta) = \log\left[(1-I)\kappa\left(IP_m + (1-I)\frac{w_1}{A_1}\right)^{-\sigma}\left(w_1^\beta + A_2^\beta + (P_m A_m)^\beta\right)\right]$$

Using the logarithm properties, this simplifies to:

$$\beta\log(w_1) = \log(1-I) + \log(\kappa) - \sigma\log\left(IP_m + (1-I)\frac{w_1}{A_1}\right) + \log\left(w_1^\beta + A_2^\beta + (P_m A_m)^\beta\right)$$

Now, we differentiate both sides with respect to I:

$$\frac{d}{dI}\left(\beta\log(w_1)\right) = \frac{d}{dI}\left[\log(1-I) + \log(\kappa) - \sigma\log\left(IP_m + (1-I)\frac{w_1}{A_1}\right) + \log\left(w_1^\beta + A_2^\beta + (P_m A_m)^\beta\right)\right]$$

Let's differentiate each term:

We start differentiate¿ing the left-hand side:

$$\frac{d}{dI}\left(\beta\log(w_1)\right) = \beta\frac{1}{w_1}\frac{dw_1}{dI}$$

So,

$$\frac{d}{dI}\left(\log(w_1)\right) = \frac{1}{w_1}\frac{dw_1}{dI}$$

Then, we differentiate the right-hand side:

The fist term $\log(1-I)$:

$$\frac{d}{dI}\left(\log(1-I)\right) = -\frac{1}{1-I}$$

The second term $-\sigma\log\left(IP_m + (1-I)\frac{w_1}{A_1}\right)$:

$$\frac{d}{dI}\left[-\sigma\log\left(IP_m + (1-I)\frac{w_1}{A_1}\right)\right] = -\sigma\frac{P_m - \frac{w_1}{A_1} + (1-I)\frac{1}{A_1}\frac{dw_1}{dI}}{IP_m + (1-I)\frac{w_1}{A_1}}$$

The third term $\log\left(w_1^\beta + A_2^\beta + (P_m A_m)^\beta\right)$:

$$\frac{d}{dI}\left[\log\left(w_1^\beta + A_2^\beta + (P_m A_m)^\beta\right)\right] = \frac{\beta w_1^{\beta-1}\frac{dw_1}{dI}}{w_1^\beta + A_2^\beta + (P_m A_m)^\beta}$$

Now, combining all the differentiated terms, we get:

$$\beta \frac{1}{w_1} \frac{dw_1}{dI} = -\frac{1}{1-I} - \sigma \frac{P_m - \frac{w_1}{A_1} + (1-I)\frac{1}{A_1}\frac{dw_1}{dI}}{IP_m + (1-I)\frac{w_1}{A_1}} + \frac{\beta w_1^{\beta-1}\frac{dw_1}{dI}}{w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}}$$

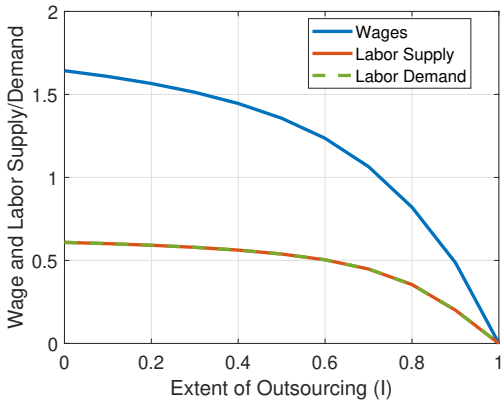
To solve for $\frac{d\log(w_1)}{dI}$, we rearrange the terms:

$$\frac{d\log(w_1)}{dI} = \frac{-\frac{1}{1-I} - \sigma \frac{P_m - \frac{w_1}{A_1}}{IP_m + (1-I)\frac{w_1}{A_1}}}{\beta \left(\frac{1}{w_1} - \frac{\sigma(1-I)}{IP_m + (1-I)\frac{w_1}{A_1}} \right) + \frac{\beta w_1^{\beta-1}}{w_1^{\beta} + A_2^{\beta} + (P_m A_m)^{\beta}}}$$

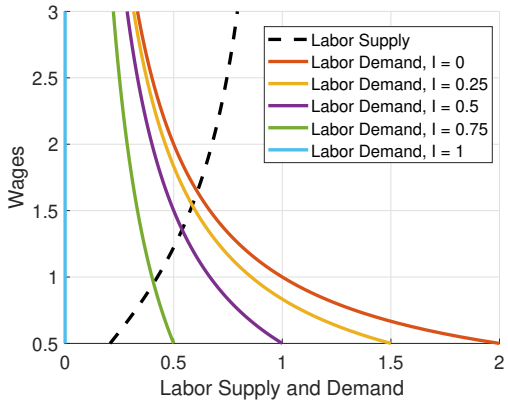
B.5 Model Simulation

Appendix Figure A11. Model Simulations

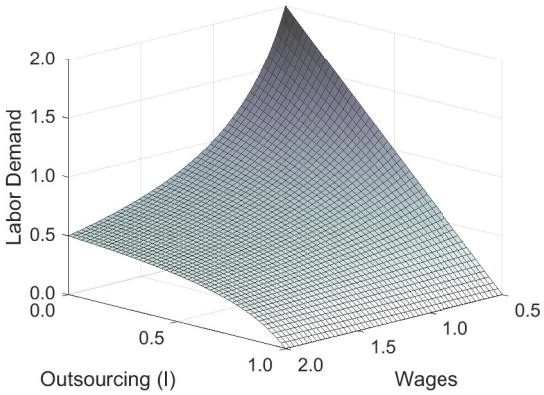
(a) w, LS, and LD for different I



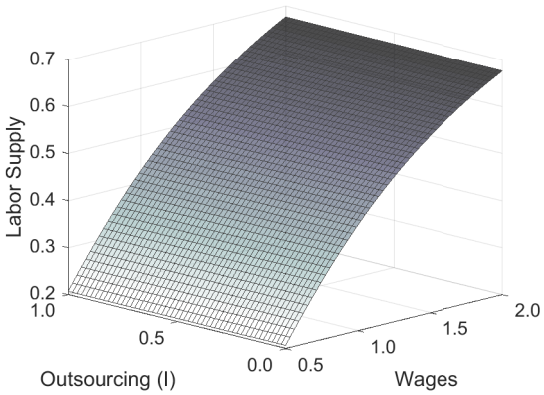
(b) w vs. LS and LD for different I



(c) Labor Demand



(d) Labor Supply



Notes: The figure provides a visual representation of how wages as well as labor supply and demand vary under different scenarios of outsourcing. The parameters are set to $\beta = 1.5$, $\kappa = 1$, $\rho = 1$,$\sigma = 1$, $A_m = 1$, $A_2 = 1$, and $P_m = 0.5$. When simulating, we create grids for wage values and I values (extent of outsourcing). Labor supply is calculated once for each wage value and assigned to the corresponding column in the grid. Then, for each combination of wage values and I values, the corresponding labor demand is calculated using the formulas from the model.