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Outsourcing Policy and Worker Outcomes: Causal Evidence from a Mexican Ban

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Abstract

Using Mexican economic census data from 1994 to 2019, we document a rising trend in domestic outsourcing, particularly among large firms, and a negative association between outsourcing and labor compensation, including profit sharing and social security. We leverage higher-frequency data from a manufacturing panel survey, matched employer–employee data, and a ban on domestic outsourcing in 2021 to show that the ban reduced outsourcing, increased labor's share, and reduced markdowns without raising total labor costs or affecting employment, output, or productivity. We propose a theoretical model in which corporate fiscal incentives drive outsourcing and account for the observed empirical patterns.

JEL classification

J38, J42, J81, M55, O15

Keywords

markdowns, monopsony, outsourcing, developing countries

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1 Introduction

The global downward trend in labor's share of income, together with growing evidence of monopsony power in labor markets in both advanced and developing economies, has sparked interest in the causes of these phenomena and in the potential of policy to remedy them.[1] One potential cause is the increased use of domestic outsourcing (Caldwell, Dube and Naidu, 2023; Stansbury and Summers, 2020), which has been shown to reduce wages (Goldschmidt and Schmieder, 2017) and undermine worker protections (Autor, 2003). In theory, however, outsourcing arrangements may be no different from the use of other intermediate services that reduce costs and increase aggregate productivity, and in a competitive labor market, a reduction in labor costs could increase employment.[2] Thus, whether policy can limit outsourcing and improve labor market outcomes for workers remains an open empirical question.

This paper evaluates a policy effort to improve worker conditions in Mexico, using a 2021 reform banning domestic outsourcing of *core* workers, defined as on-site workers for whom the employing firm sets employment responsibilities that are paramount to its primary economic activities but with whom no formal employer–employee relationship exists.[3] Implementing a difference-in-differences (DID) strategy with longitudinal establishment-level data and matched employer–employee data, we find that the policy did indeed reduce outsourcing of core workers (hereafter, *core outsourcing*) and, in so doing, increased labor compensation and reduced markdowns without affecting employment, as reflected in increases in mandated social security payments and profit sharing, which we observe directly.

This increase in total labor compensation was not accompanied by an increase in total labor costs because payments to staffing companies exceeded actual worker compensation. Moreover, the removal of outsourcing as a hiring modality had no effects on output, productivity, labor flexibility, local firm dynamics, other input utilization, or taxable profits. This last finding,

[1]With regards to labor's share, Karabarbounis and Neiman (2014) document a global decline, while Grossman and Oberfield (2022) provide a broad review of the potential causes of its decline in the United States. Examples of the importance of monopsony power in the United States are provided in Berger, Herkenhoff and Mongey (2022*a*) and Yeh, Macaluso and Hershbein (2022), and Brooks et al. (2021*a*,*b*) present evidence for India and China.

[2]See, for example, recent important contributions by Bilal and Lhuillier (2021) and Bertrand, Hsieh and Tsivanidis (2021).

[3]This definition excludes workers providing specialized services to the firm, such as cleaning, catering, security, and gardening staff.

coupled with the close correspondence between the wedge that outsourcing drove between labor remuneration and labor costs, on the one hand, and the tax, profit sharing, and social security obligations firms had avoided through these arrangements, on the other, indicates that outsourcing served not a productive function but a fiscal one, enabling firms to reduce their overall fiscal burden through tax planning, at the expense of worker remuneration.

Mexico is a particularly interesting and informative case for studying core outsourcing, especially from the standpoint of its measurement and identification. First, although institutional arrangements featuring prevalent core outsourcing, mandatory social security, and mandatory profit sharing are common to many countries, Mexico's use of core outsourcing was high and persistent before the reform. We document a rising prevalence of core outsourcing in manufacturing, where the employment share of outsourced workers tripled from 7 to 21 percent between 2000 and 2021, the year of the reform. Second, the context of the reform offers quasi-experimental variation: The policy led to a precipitous drop in core outsourcing, so we can compare establishments that had previously outsourced workers to those that did not in a DID specification. Third, we have comprehensive, longitudinal, establishment-level data and matched employer–employee data, which enable us to document preexisting patterns, estimate establishment markdowns and responses, and quantify the effects on employment status and registered wages at the employee level. Per legislative provision, most of the data contain explicit measures of core outsourced labor, an advantage that is unique in the literature. Finally, the stakes of controlling core outsourcing are higher for developing countries such as Mexico than for developed ones: In the former, worker wages are already lower, the subject of worker abuse is more salient because most workers receive no labor benefits (Ronconi, 2019), and changes to employment and social security regulations are proposed frequently.

We leverage data from a panel manufacturing survey recording monthly and annual information at the establishment level from 2013 to 2025 to quantify the causal impacts of the 2021 ban. By 2024, total labor compensation had risen sizably by 16.4 percent, including an increase in salaries and benefits of 7.1 percent, while firing costs rose relatively little, and total labor costs remained constant because of the steep reduction in the fees paid to staffing firms for management services. The clear presence in the monthly data of legislatively mandated seasonal payments lends credibility to our identification strategy.[4] According to our baseline specification, because of this increase in compensation, the reform raised the labor share at the establishment level by 3 percentage points. Moreover, we use the manufacturing panel to test for effects on other establishment-level outcomes and find no impacts on employment, labor flexibility, use of other productive inputs, capital investment, output, total factor productivity (TFP), or market exit. Finally, using 3 waves of 5-year economic census data and a Bartik-style identification strategy, we verify the absence of general equilibrium (GE) effects on local economic dynamism, including firm entry and exit rates.

Next, using matched employer–employee data from the social security authority, we estimate the reform's effects on wages at the worker level. Following Goldschmidt and Schmieder (2017), we identify a particular type of outsourcing using large cluster flows of workers from professional services firms to manufacturing. The reform increased wages for transferred workers by 10.4 percent on average by 2023; these estimates match the ones we obtained from the establishment-level data. Moreover, in line with the literature attributing wage losses after outsourcing episodes to worker moves from high-paying client firms to lower-paying staffing firms and their resulting loss of access to firm-specific rents (Dube and Kaplan, 2010; Goldschmidt and Schmieder, 2017; Drenik et al., 2020), the wage increases are largely accounted for by increases in firm premia, as estimated by AKM decomposition (Abowd, Kramarz and Margolis, 1999), consistent with the transferred workers regaining access to firm-specific rents. The largest wage increases accrued to workers whose pre-transfer firms' fixed effect falls in the lower half of the distribution, which implies that the reform equalized employment conditions across workers.

Furthermore, we use matched employer–employee data to test the reform's effects on unemployment and job-to-job separation rates. The reform temporarily protected against unemployment in the 12 months after the transition to direct hiring, consistent with contractual relationships being reset. We find no detectable effect on job-to-job separation rates, which reinforces findings from survey and census data on employment flexibility and local firm dynamics.

[4]Specifically, we find a spike in salaries paid in December, corresponding to the legislatively mandated disbursement of a thirteenth month's pay at Christmas for directly hired workers, and an increase in profit sharing in May of each year, the mandated month for dividend disbursement.

The observed wage increase with no employment increase may be explained by a shift in rents from firms to workers. Indeed, the reform's statement of purpose argued that core outsourcing enables worker exploitation (Gaceta Parlamentaria, 2020). Accordingly, we examine the role of monopsony power. We first demonstrate the extent of markdowns[5] pre-reform and their correlation with core outsourcing, using several waves of 5-year economic census data for the universe of Mexican manufacturing establishments. Markdowns were high and pervasive before the reform, particularly among firms that outsourced. Moreover, consistent with the presence of labor market power, markdowns increased with firm size, and outsourcing prevalence was higher among large firms.

Returning to the manufacturing survey data, we find that the reform reduced markdowns by 28 log points. This drop is concentrated among the quartile of establishments with the highest markdowns, consistent with a reduction in monopsony power.

Following our empirical analysis, we propose a theoretical model in which firms jointly exploit payroll overreporting, VAT fraud, and inflated corporate deductions to reduce their overall tax burden. The key insight is that these are not independent margins of evasion: Overreporting of labor costs necessitates registration of fictitious input costs that simultaneously increase creditable VAT and deductible expenses. In other words, payroll fraud is linked to corporate tax evasion through the firm's cost structure.

Our paper contributes to a rapidly growing literature on labor market power, methods to estimate it, and its impacts on wages, markdowns, and employment in the US (Benmelech, Bergman and Kim, 2022; Berger, Herkenhoff and Mongey, 2022*a*,*b*; Berger et al., 2023; Dodini, Stansbury and Willén, 2023; Dube et al., 2020; Lamadon, Mogstad and Setzler, 2022; Manning, 2013; Yeh, Macaluso and Hershbein, 2022) and developing countries (Amodio, Medina and Morlacco, 2022; Brooks et al., 2021*a*; Bassier, 2023; Brooks et al., 2021*b*; Amodio and Roux, 2022; Felix, 2021; Naidu, Nyarko and Wang, 2016; Zavala, 2022), with the most recent contributions focusing on quantifying the extent of monopsony power and its impact on firm rents. Our empirical analysis complements this line of research by using policy variation to confirm the presence of monopsony power *ex post*, showing that establishments' labor demand does not simply

[5] This measure of exploitation is standard in the literature (Brooks et al., 2021*b*; Yeh, Macaluso and Hershbein, 2022).

move along a downward-sloping curve. Our results therefore validate the standard markdown measures that use observational data.

We also contribute to a second literature on domestic outsourcing pioneered by Autor (2003), who uses an event study to show that US state courts' decisions to protect workers against unjust dismissal in the 1980s fostered the growth of temporary help employment,[6] ultimately having the unintended consequence of reducing productivity and distorting production choices (Autor, Kerr and Kugler, 2007). Our empirical analysis advances the opposite argument: While worker protections can have unintended consequences, they can also have the *intended* consequence of reducing exploitation. In this regard, our results resonate with the findings of associations between domestic outsourcing and lower wages and benefits (Dube and Kaplan, 2010; Drenik et al., 2020; Weil, 2014), expansions in firm rents (Appelbaum, 2017), and increases in wage inequality (Bilal and Lhuillier, 2021; Goldschmidt and Schmieder, 2017) and bring a new insight to the literature: namely, that outsourcing helps firms bypass profit-sharing regulations, which disproportionately benefit lower-skill workers (Nimier-David, Sraer and Thesmar, 2023). We see our findings as complementary to those of Felix and Wong (2024) and Guo, Li and Wong (2024), who show favorable employment effects from a reform legalizing outsourcing of noncore employees in Brazil, and Bertrand, Hsieh and Tsivanidis (2021), who report a favorable impact of contract labor on firm growth in India. Our results emphasize that differences in regulatory environments could lead to widely different impacts of regulation. For example, while high firing costs make contract labor necessary to enable firm growth, in Mexico, the differential enforcement of worker provisions across producing and staffing firms enabled the latter to grow and accumulate labor market power.

Furthermore, we address a significant gap in this literature about how to identify outsourced workers in the data. As Bernhardt et al. (2016) highlight, nonsystematic reporting of the identity of outsourced workers in US firm-level surveys and government data has hindered researchers' understanding of the extent, growth, and implications of outsourcing for workers, job quality, and policy. By leveraging data from Mexico—where reporting of outsourcing is mandatory for most data sources—and incorporating a policy experiment, we overcome this limitation and

[6] Relatedly, staffing services added 9.2 percent to manufacturing employment in the US in 2006, compared to 2.3 percent in 1989 (Dey, Houseman and Polivka, 2012).

provide a comprehensive, economy-wide assessment of the prevalence, growth, and effects of this hiring practice on firms and workers.

Finally, we contribute to the literature on statutory labor protections in developing countries, pioneered by Kugler (1999, 2004, 2005) and Kugler and Kugler (2009) for Colombia and Maloney (1999, 2002, 2004) for Mexico.[7] In these settings, labor informality is a defining feature of the labor market (see Ulyssea, 2020),[8] and social security benefits apply only to employees of formal firms that make statutory contributions proportional to registered wages. We show that outsourcing enables large formal firms to depress these registered wages and thereby curtail their social security contributions, which erodes the protections that formal employment is designed to guarantee. We further show that overhauling labor regulations and improving enforcement shrinks the scope for wage depression without negatively impacting employment. Moreover, our model provides a concrete microfoundation for how institutions can be arranged to reallocate income from labor to profits through tax evasion. This represents the domestic counterpart of international profit shifting, whereby multinationals concentrate income in low-tax jurisdictions through related-party transfer pricing (Clausing, 2003; Hines Jr and Rice, 1994). Our findings complement the literatures on the declining labor share (Karabarbounis and Neiman, 2014) and corporate tax avoidance (Saez and Zucman, 2019; Zucman, 2014) by identifying the firm-level mechanics that connect the two.

The rest of the paper is structured as follows. Section 2 provides context on domestic outsourcing practices in Mexico and their blanket ban by the Mexican government in 2021. Section 3 describes the data sources for our empirical analysis. Section 4 outlines our methodology to estimate markdowns and quantifies the correlation of markdowns with the use of outsourcing before the reform. Section 5 outlines the DID strategy for measuring the ban's causal impacts at the establishment level and reports its effects on employment, labor flexibility, wages, the labor share, factor substitution, investment, output, TFP, and market exit. Section 6 quantifies the causal impact at the worker level using matched employer–employee data. Section 7 discusses the theoretical interpretation of our findings. Section 8 concludes.

[7] A related literature documents the effect of minimum wage increases and enforcement of safety provisions for workers of multinationals in Bangladesh (Bossavie, Cho and Heath, 2023; Boudreau, 2024).

[8] The wide prevalence of labor informality in Mexico has been extensively documented by Azuara and Marinescu (2013), Bosch and Campos-Vazquez (2014), Busso, Fazio and Levy (2012), Conover, Khamis and Pearlman (2022), Levy (2008), and Samaniego de la Parra and Fernández Bujanda (2024).

2 Institutional Context

This section gives context on domestic outsourcing in Mexico and its banning in 2021. Section 2.1 describes the legal framework governing contractual employment relations in Mexico, much of which is common to many other countries, and firms' strategic use of domestic outsourcing to bypass employment regulations. Section 2.2 reports key empirical regularities pertaining to domestic outsourcing. Finally, Section 2.3 summarizes the ban's legal provisions.

2.1 Domestic Outsourcing Within the Mexican Legal Framework

Since 1943, Mexico's formal insurance system has followed an earnings-related approach, the so-called Bismarckian model used in many other countries.[9] In this system, a formal firm contractually hiring a worker registers the worker's average daily wage with the social security authority, the Instituto Mexicano del Seguro Social (IMSS). The hiring firm must pay the government an earmarked tax or contribution proportional to the registered wage each month. This contribution gives the worker access to public health- and childcare facilities. It also funds a bundle of public wage-dependent benefits, including life and critical illness insurance and a retirement pension.

Beyond social insurance, Mexican legislation offers other protections of workers' rights. According to the constitution, employees have the right to a share of their employers' profits, referred to as the *participación de los trabajadores en las utilidades* (PTU). Although Mexico's statutory PTU share of 10 percent is relatively high, profit-sharing provisions themselves are common to many countries. For example, all countries in the Organisation for Economic Cooperation and Development (OECD) except the US have similar provisions.[10] Federal legislation also stipulates a universal right of directly hired workers in a firm to unionize and sets severe financial penalties for firms that terminate a worker for reasons not involving contract breach, including three months' severance pay and up to a year's wages. Again, these provi-

[9] Political scientists classify social protection systems by the relation between contributions and benefits: Beveridgean systems feature a flat-rate benefit rule and Bismarckian systems an earnings-related rule (Cremer and Pestieau, 2003). Bismarckian social insurance is not unique to Latin America; such models appear in several advanced economies, among them Germany, France, Japan, Switzerland, and Israel (Tulchinsky, 2018). For a detailed description of other Latin American social insurance models, see Frölich et al. (2014).

[10] For a review, see Estrin et al. (1997). Appendix A Figure A.1 presents the prevalence of profit-sharing schemes for selected advanced countries in 2019.

sions are not unique to Mexico: The right to form trade unions is stipulated in Article 23 of the Universal Declaration of Human Rights, and wrongful termination legislation exists in virtually every country, including the US.

As in many other countries, given the sizable labor-related costs imposed by legislation, domestic outsourcing was rising in Mexico before the ban.[11] Although common, outsourcing is usually difficult to measure and therefore study directly. Given its policy significance, however, Mexico collects detailed data according to well-established definitions. We refer to domestic outsourcing as a legal scheme whereby one firm contracts a staffing firm to hire *core* workers formally and pay their wages and social security contributions on the focal firm's behalf. Core workers are those physically employed in primary economic activities within an establishment of the focal firm. For clarity, we refer to the first firm as the *employing firm* and the second as the *staffing firm*. Note that this definition of core workers excludes workers employed on the establishment premises who do not carry out primary economic activities, as defined by the establishment's North American Industrial Classification System (NAICS) code, such as workers engaged in cleaning, catering, security, and gardening. We refer to firms supplying the workers who conduct these noncore activities as *specialized subcontractors* and exclude them from our analyses.

The theoretical literature in economics has highlighted efficiency gains as the primary motive for outsourcing (e.g., Bilal and Lhuillier, 2021), but domestic outsourcing of *core* workers before the reform occurred mainly through two schemes—insourcing and third-party outsourcing—primarily associated in the legal literature with tax and profit-sharing evasion (see Brito Laredo et al., 2022; Franco et al., 2020; Velarde, Mueller and García, 2021). Insourcing is a practice designed to lower profit-sharing payouts whereby a firm sets up a dual organizational structure, parking most of the profits generated by its productive establishments in a company[12] with no employees while hiring employees through a shell company that supplies personnel to the former and retains minimal profits. Panel A of Figure A.2 in Appendix A diagrams this practice.

Third-party outsourcing is a practice designed to lower a firm's payroll, value-added tax

[11]Figure 1 in OECD (2021) shows that outsourcing has been growing over the past 20+ years across all OECD countries.

[12]The term *company* refers to an artificial person, created by law, that has a separate legal entity.

(VAT), and corporate tax burden. To minimize the payroll burden, the third party creates a shell company with fake owners; this company, in turn, minimizes its social security contributions to the government by registering workers as earning the minimum average daily wage. It then "tops up" workers' wages with supplementary pay, including bonuses, grocery vouchers, and vacation pay, none of which are subject to social security contributions. This reduces the tax burden of direct hires, but also the social security benefits of workers,[13] which are an increasing function of the workers' registered average daily wage, not their total income. While workers continue to enjoy access to public healthcare, they do not receive the mandated employment benefits in the same amount (e.g., retirement pensions) under third-party outsourcing. Panel B of Figure A.2 diagrams this practice. To evade the VAT, the shell staffing company fabricates fake invoices and claims tax deductions, then redistributes a fraction of the evaded liabilities to the employing firm through a cash kickback. Finally, since labor services are tax deductible, this practice enables the employing firm to evade corporate taxes by colluding with the staffing company to inflate invoiced amounts, with excess amounts being redistributed back to the firm through cash kickbacks, as in the case of VAT evasion. While this type of domestic outsourcing fell squarely into the category of tax evasion before the reform, shell companies faced limited legal punishment because they had no assets or real owners.

Both outsourcing practices, which could be combined, as well, shifted the legal burden of battling unions and individual workers to the shell staffing company. Per Mexican legislation prior to the reform, the actual employing firms were neither responsible for meeting union demands nor liable for wrongful termination of workers, even if the shell staffing company declared bankruptcy or insolvency.

The use of core outsourcing to simulate employment relationships, circumvent statutory labor costs, and evade taxation is not unique to Mexico. Comparable practices have been documented across multiple jurisdictions and have prompted regulatory and judicial responses aimed at curbing their use. In Brazil, the Federal Supreme Court (STF) ruled in 2024 that outsourcing cannot be used to disguise employment relationships or circumvent statutory labor rights, including vacation benefits, Christmas pay, and the FGTS (severance indemnity fund;

[13] Previous empirical evidence shows that the value workers place on social security benefits exceeds the employer's cost of providing them (Samaniego and Sharma, 2023).

de Barros Penteado, 2023). In Peru, regulations enacted in 2022 restrict outsourcing to noncore business activities, effectively prohibiting the use of external labor in a firm's principal income-generating operations (Jiménez and Rendon, 2025). In Colombia, recent reform proposals seek to limit outsourcing arrangements that reduce subcontracted workers' benefits relative to direct employees' (Willis Towers Watson, 2023). In India, the Contract Labour (Regulation and Abolition) Act of 1970 allows banning of outsourcing arrangements based on "sham contracts" that disguise direct employment relationships (Shyam Sundar, 2018). Across the European Union, several countries impose joint liability on firms that benefit from outsourced labor, limiting the scope for evasion of labor obligations (European Foundation for the Improvement of Living and Working Conditions, 2008).

2.2 Domestic Outsourcing in the Data

We use prereform data for staffing establishments from the 2019 economic census wave to document key empirical regularities pertaining to domestic outsourcing.[14] We identify staffing establishments in the data as those supplying nonspecialized workers (i.e., workers other than specialized subcontractors) to other establishments.

We begin by characterizing the revenue structure and size distribution of staffing establishments and comparing them with those of manufacturing establishments of similar size in Appendix A Figure A.3. Staffing establishments employ more workers than manufacturing establishments, and their revenue is distributed almost entirely between labor and profits. Conditional on size, they pay lower social security contributions to the government, offer lower employment benefits, and share less of their profits with workers. Finally, they entirely absorb the litigation costs from worker terminations by employing firms.

In Table 1, we summarize the appendix information on mean labor payment shares of nonsalary payments across three establishment types: staffing establishments, manufacturing establishments that hire workers directly, and manufacturing establishments that rely on outsourced workers. As a share of labor payments, social security contributions, profits shared, and

[14]Ideally, we could use data on payments received by staffing establishments from each manufacturing establishment to link hiring and employing establishments. Unfortunately, these data do not exist; indeed, a key provision of the reform, as described below, was the creation of a mandatory registry with contractual and employment information for all specialized contractors.

other benefits are on average 7, 3, and 2 percentage points lower, respectively, in staffing than in the manufacturing establishments hiring workers directly, and they are zero in the manufacturing establishments that rely on outsourced workers. In total, nonsalary payments in staffing establishments are less than half, or 12 percentage points lower than, those in the manufacturing establishments hiring workers directly.

Table 1: Comparison of Staffing and Manufacturing Establishments, 2019

Variable	Staffing	Manufacturing by Hiring Modality		Direct Hiring vs. Staffing		Direct Hiring vs. Outsourcing	
		Direct	Outsourcing	Difference	p-value	Difference	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log(Workers)	7.97	6.17	6.55	-1.8	0.000	-1.42	0.000
Labor Share	0.98	0.47	0.22	-0.51	0.000	-0.25	0.000
Labor Cost Shares							
Salary	0.86	0.78	0	-0.08	0.000	0.78	0.000
Total Nonsalary	0.08	0.2	0	0.12	0.000	0.2	0.000
Social Security	0.05	0.12	0	0.07	0.000	0.12	0.000
Benefits	0.02	0.04	0	0.02	0.000	0.04	0.000
Profit Sharing	0.01	0.04	0	0.03	0.000	0.04	0.000
Firing Costs	0.04	0.02	0	-0.02	0.000	0.02	0.000
Staffing Fee	0.02	0	1	-0.02	0.000	-1	0.000

Notes: This table presents the employment-weighted means across all staffing establishments and manufacturing establishments that either hire all their workers directly or outsource all of them. The p-value in Column (5) corresponds to a Wald test of the difference in means between Columns (1) and (2). The p-value in Column (7) corresponds to a Wald test of the difference in means between Columns (2) and (3). Staffing establishments are those supplying nonspecialized workers (i.e., excluding gardening, catering, security, cleaning, and other specialized services) to other establishments.
Source: Authors' elaboration using data from the 2019 wave of the Mexican economic census.

What types of establishments outsource? Table 2 shows the main correlates of outsourcing for the universe of manufacturing establishments from 1994 to 2019. Column (1) shows that a 1 percent increase in the total employee count is associated with an increase in the establishment's outsourced share of workers of 1 percentage point (p=0.000). Columns (2)–(4) show strongly significant correlations between the share of outsourced workers in the establishment and the establishment's local labor market employment share, log establishment revenue, and the establishment's local labor market revenue share, respectively. Column (5) shows that the share of outsourced employees is 5 percentage points higher among foreign-owned than among domestic establishments. Moreover, we construct an indicator for *maquiladora* establishments, which host the manufacturing operations of American firms, typically importing their inputs and exporting their output for final consumption in the US.[15] We interact the foreign ownership indicator with the maquiladora dummy to measure whether establishments of this type are disproportionately likely to domestically outsource employees (i.e., employ Mexican workers formally hired by a third party in Mexico) than other foreign-owned establishments. As expected, Column (6) shows that the share of outsourced employees in maquiladora establishments is 2 percentage points higher than that in other foreign-owned establishments (p=0.000).

Table 2: Outsourcing and Establishment Size
Outcome Variable: Share of Outsourced Employees

Regressor	By Establishment Size				By Foreign Ownership	
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Total Employee Count)	0.01***					
	(0.0006)					
Employment Share of Local Labor Market		0.07***				
		(0.005)				
Log(Total Revenue)			0.01***			
			(0.0007)			
Revenue Share of Local Labor Market				0.07***		
				(0.004)		
Foreign Ownership					0.05***	0.05***
					(0.003)	(0.003)
Foreign Ownership × Maquiladora						0.02***
						(0.005)
Maquiladora						0.0003
						(0.001)
N	230,185	230,185	230,185	230,185	230,185	230,185
R^2	0.109	0.09	0.122	0.091	0.124	0.124

Notes: All regressions include market fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the market level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican economic census waves from 1994 to 2019.

Appendix B gives visual evidence that large firms use outsourcing disproportionately. This fact is consistent with the pattern reported by Goldschmidt and Schmieder (2017) for Germany and Bilal and Lhuillier (2021) for France. In addition, establishments hit with revenue shocks are likelier to outsource, consistent with the evidence for the United States in Atencio De Leon (2023) and Atencio De Leon, Macaluso and Yeh (2023) and with the idea that outsourcing increases establishments' flexibility in responding to shocks. In sum, Mexican outsourcing patterns look quite similar to other countries'.

[15] See Estefan (2026) for a thorough description of this program.

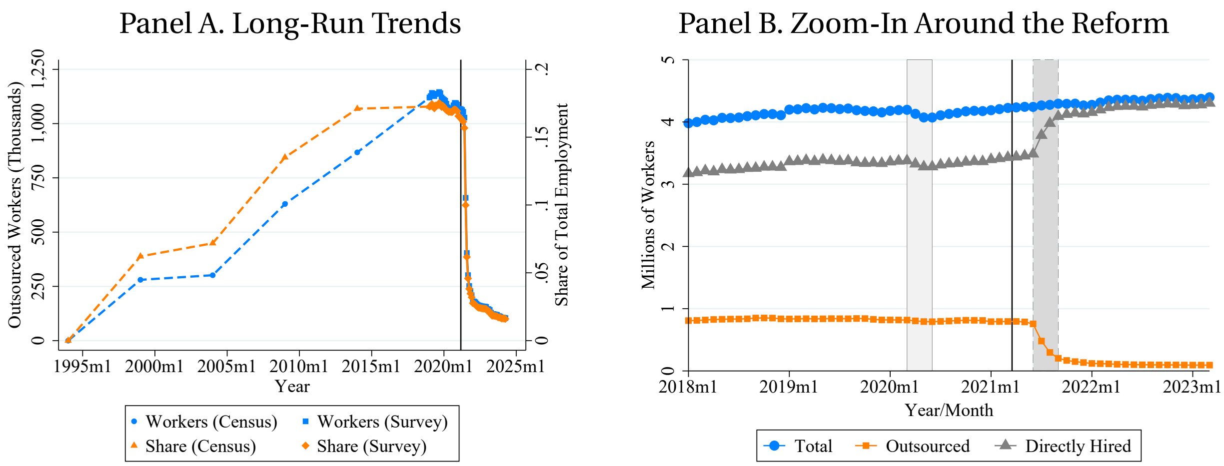
2.3 Mexico's April 2021 Outsourcing Ban

Though domestic outsourcing has been growing in popularity since the 1980s in the US (Davis-Blake and Broschak, 2009), its expansion in Mexico began only after the signing of the North American Free Trade Agreement (NAFTA) in 1994, when US firms started subcontracting manufacturing processes to Mexico (Bergin, Feenstra and Hanson, 2009). Thereafter, to contain the growth of domestic outsourcing amid concerns of uncontrolled expansion, Mexican legislators started passing regulatory changes, including reforms to federal laws in 2009, 2012, 2015, and 2017.[16]

These changes proved to be of no avail: Domestic outsourcing grew uninterruptedly in absolute and relative terms from 1994 to 2019, as shown in Figure 1. The figure combines data from two sources: Mexico's economic census and the National Institute of Statistics and Geography's (INEGI's) survey panel of manufacturing, which track each other well in overlapping years. Outsourcing leveled off with the 2018 election, and the observed collapse in 2021 corresponds to the government ban, which we now describe.

[16]The legislative changes passed included defining domestic outsourcing as a special employment regime with narrow applicability, transferring responsibility to employing firms for keeping all documentation related to the hiring company's tax and social security obligations, and requiring employing firms to allow government inspection visits (Covarrubias, Belaunzarán et al., 2020; Morales Ramírez, 2022).

Figure 1: Outsourcing Growth in the Manufacturing Sector and Regulatory Clampdown



Notes: This figure presents trends in outsourcing in Mexico's manufacturing sector from 1999 to 2023. Panel A uses data for the universe of manufacturing establishments, covering six waves of the economic census from 1994 to 2019, coupled with post-2019 data from the monthly manufacturing survey, reweighted to align with the economic census. The vertical black line represents the enactment of the outsourcing reform in April 2021. Panel B shows raw data for the panel of establishments in the monthly manufacturing survey. The light gray area depicts the period of the most restrictive COVID-19 lockdown in Mexico, April–June 2020. The dark gray area outlined with a dashed black line represents the grace period stipulated by the reform for the transfer of previously outsourced employees to the payroll of their employing firms.
Source: Authors' elaboration using data from the Mexican economic census and INEGI's monthly manufacturing survey.

In 2018, the newly elected government adopted a hard-line stance against outsourcing. Aided by a qualified congressional majority, on April 23, 2021, the government enacted a reform of the entire body of legislation governing labor relationships in Mexico.[17] The reform comprised three main provisions. First, it prohibited outsourcing, substituting it with a new subcontracting scheme limited to specialized services, such as cleaning, catering, gardening, and security services, falling outside the scope of the employing firm's core economic activities. Second, for the monitoring of specialized subcontractors, the reform mandated the creation of a universal registry. To register, specialized contractors must pay taxes and social security contributions to the government, share profits with workers, and renew their registration every three years. Registered specialized subcontractors must also share their payroll information and contracts with employing firms with the government. Finally, the reform toughened enforcement measures against violations of the outsourcing legislation. Specifically, it made employing firms and shell staffing companies *equally* liable for subcontracted workers' payroll

[17]This legislation includes the *Ley del Seguro Social*, the *Ley del Instituto del Fondo Nacional de Vivienda para los Trabajadores*, the *Código Fiscal de la Federación*, the *Ley del Impuesto sobre la Renta*, and the *Ley del Impuesto al Valor Agregado*.

taxes and social security contributions, required firms to comply with inspection mechanisms, set tougher financial sanctions for ordinance violations, and strengthened enforcement efforts by aligning the provisions of several pieces of legislation and initiating agreements between government departments to prevent firms from exploiting loopholes.

As a practical matter, the reform mandated that all outsourced workers who performed the firm's core activities be transferred to the employing firm's payroll. The government published regularization instructions and oversaw the transfer of outsourced workers employed on firms' premises within a 3-month grace period concluding in August 2021.

Importantly, the definition of outsourcing was carefully drafted in the reform to avoid negatively impacting other hiring practices often confounded with it in the policy discourse. First, the reform did not ban temporary employment. Mexican legislation allows formal firms to hire workers temporarily with no additional tax burden. In fact, temporary employment played a quantitatively important role in direct hiring before the reform: 14 percent of all directly hired workers in manufacturing were temporary in March 2021. Second, the reform did not ban job search boards or recruitment agencies, which could continue operating as long as their activities centered on search and recruitment, not staffing. Third, the reform did not target contract labor, which remains legal and is registered separately in the data.[18] However, the reform's bylaws do stipulate that simulating a contracting relationship, whereby workers are falsely presented as contractors, is fraud, punishable by imprisonment.

3 Data Sources

In this section, we describe our data sources. We focus overwhelmingly on manufacturing for three reasons: The data coverage is most complete and consistent for this sector since the criterion that the business have a fixed location is met more consistently in manufacturing than in sectors such as construction, services, and retail. Second, manufacturers use processing of materials, which enables us to use standard methods to construct their markdowns. Third, manufacturing goods are highly traded spatially, so the local impacts of the reform on labor

[18]According to Mexican legislation, contractors are independent workers (i.e., not hired through staffing companies) who provide services to firms without entering a subordinate relationship in which the firm dictates working hours, employment location, and task instructions.

markets are less likely to have spilled over to firm-level demand.

To maximize completeness of coverage, length of the time series, data richness, and data frequency, we use confidential data from multiple sources: establishment-level data from economic censuses, annual manufacturing surveys, and monthly manufacturing surveys, as well as matched employer–employee data from the social security authority. The detailed information about outsourced labor reported in almost all these sources make the Mexican data especially informative.

Economic Census. The data for this paper come from the 7 most recent waves of the Mexican economic census, which is conducted every five years by INEGI. The census covers all establishments (formal and informal) but excludes ambulant street vendors with no fixed location. We analyze the period 1994–2024 for the manufacturing sector. Manufacturing comprised 21 percent of Mexico's GDP in the first quarter of 2024 (Instituto Nacional de Estadística y Geografía, 2024). We harmonize industry codes across census waves and assign each establishment a six-digit industry code based on the 1997 NAICS classification to end up with 302 industries surveyed across the 7 census waves. For each establishment, the census reports total employment, annual payroll, total revenues, value added, intermediate input consumption, and productive capital.

There are two main employment categories: directly hired employment and outsourced employment. The former comprises all nonremunerated workers, including primary owners and family members, and remunerated workers hired directly by the establishment to work on its premises. The latter type of worker may be formally or informally hired. For directly hired workers who are formally employed, the establishment pays base salary, supplementary pay, benefits, social security contributions, and profit sharing. Importantly, INEGI reports base salary, supplementary pay, and benefits in two categories: salaries and benefits. The former includes most monetary remuneration for work, such as salaries, commissions, bonuses, Christmas pay, and vacation premia; the latter captures payments to private institutions or in-kind transfers, including private medical and pension plans, childcare, and groceries and meals. Because outsourcing channels part of workers' remuneration through items recorded under the benefits category that are not subject to social security contributions, neither category in isolation fully captures workers' ordinary remuneration. Accordingly, we combine salaries and benefits when analyzing the impacts of the reform on individual wage components to better approximate workers' regular earnings net of payroll contributions but before income taxes.

Outsourced workers are employed on the establishment's premises but are formally hired through a different company. This employment category excludes contract labor and specialized subcontractors, whose services, such as cleaning and security, enter the census estimations as a separate category within intermediate consumption. The establishment employing the outsourced workers reports only the total payment made to the staffing firm, not the amount ultimately paid to workers. To estimate these payments, we examine the labor payment data of the staffing establishments themselves. Lacking a direct mapping between employing firms and staffing firms, we use the employment-weighted cross-sectional mean of the revenue share of labor across all establishments in the staffing sector to impute the labor cost of outsourced workers.

Based on these employment categories and their respective labor payments, the annual payroll reported by the census is the sum of all payments to workers (in both categories). Annual payroll data are reported in thousands of current Mexican pesos.

In addition to employment and the annual payroll, the census data report total revenues and value added for each establishment. The total revenues measure recorded in the census captures total sales of goods and services and all other sources of revenue for the establishment. The value added measure results from subtracting intermediate consumption (which includes the total cost of raw materials; energy provision, including electricity, gas and fuels; contracting and subcontracting expenses for services such as gardening and security; and repair and maintenance expenses) from total output. Finally, for each establishment, the census also reports the value of capital and its depreciation. Capital is defined as the value of all fixed assets owned by the establishment with a lifespan greater than one year and used in the production of its goods and services. Thus, we can calculate the labor, capital, raw materials, energy usage, and total output for each economic establishment in the country.[19]

Finally, for a subset of establishments that do not keep labor, capital, raw materials, or energy expense accounts, the economic census reports only their revenues, employment, and

[19]Appendix C.1 provides further details about the construction of our output and input measures.

economic sector. For example, for labor, the census reports the employment level of these establishments but does not keep track of their wages, social security payments, or profit sharing. Such establishments should not be confused with self-employment, as it is not necessarily true that they employ just one individual. These establishments constitute 37 percent of all employment, but we exclude them from our empirical analysis, as their inclusion would introduce measurement error to our computation of input revenue shares.

The census reports a unique firm and establishment identifier for the 2009, 2014, 2019, and 2024 census waves, and we use the concordance tables in Busso, Fentanes and Levy (2018) to identify establishments in the 1994, 1999, and 2004 waves. In this way, we can link establishments across all 7 waves for longitudinal data analysis.

Annual Manufacturing Survey. The data that we use to measure the causal impacts of the outsourcing reform on nonlabor outcomes come from Mexico's annual manufacturing survey by INEGI. We analyze the period 2013–2024.[20] The survey gathers data from 10,447 establishments, which can be linked across survey waves with a unique identifier, and its sample spans 239 six-digit 2013 NAICS industry codes. For each establishment, the survey reports directly hired and outsourced employment, annual payroll, total revenues, intermediate input consumption, productive capital, and other production costs. We use these data to calculate labor, capital, raw materials, energy usage, output, and taxable profits for each establishment.

There are two important caveats to consider with respect to this survey. First, it does not separately report salaries and supplementary pay, social security payments, benefits, and profit sharing, rendering us reliant on a different data source for our measurement of impacts on employment and labor payments. Second, the panel rotated at the end of 2021, with 5,583 establishments dropped from and 3,955 new establishments added to the 2022 sample. In estimation, we present the results corresponding to the subsample of 4,864 establishments that did not drop out of the panel because of the rotation in 2021, but we report the results for the panel of 10,447 establishments running up to 2021 as a robustness check.

[20] INEGI will publish the data from the 2025 survey wave on December 31, 2026.

Monthly Manufacturing Survey. To measure the causal impacts of the outsourcing reform on employment and labor payments by category, we complement the annual survey data with data from Mexico's monthly manufacturing survey, also levied by INEGI. This survey gathers information from a panel of 8,819 establishments overlapping with the annual survey panel and spanning the same 239 six-digit NAICS industry codes. Because the data from this survey are released faster than the annual survey, we can extend the analysis through May 2025, covering 2018–2025. The key difference between the two surveys is that the monthly survey gathers information only on directly hired and outsourced employment, annual payroll, and total revenues. While limited in scope, the monthly survey is high frequency and offers information on salaries and supplementary pay, social security payments, benefits, and profit sharing, which allows us to measure the timing of the impacts on remuneration by source at the establishment level.

Matched Employer–Employee Data. To measure the causal impact of the reform on the probability of transitioning into direct employment for previously outsourced workers and on these workers' registered mean wage before the social security authority, we use IMSS matched employer–employee data from 2019 to 2023. These data report the hiring company (not employing company), economic sector, employment status (temporary or permanent), and registered mean wage at monthly frequency and individual level for the universe of formal workers in manufacturing and professional services. The registered wage corresponds to the official measure used for social security contributions and, as such, excludes forms of remuneration not subject to such contributions, making it a narrower measure of wages than that used in our analysis of INEGI data.

4 Outsourcing and Wage Markdowns

The statement of purpose of the outsourcing reform bill, submitted to the federal legislature in November 2020, justified the ban by arguing that outsourcing of core workers enabled firms to mark wages downward through simulation of employment relationships and tax evasion (Gaceta Parlamentaria, 2020). This section presents evidence that markdowns were indeed high and pervasive before the reform—especially among the firms that outsourced. The analysis focuses on the economic census data.

4.1 Measuring Markdowns

The typical indicator of labor market power is the markdown, defined as the ratio of the marginal product of labor to the wage. The former is not measured explicitly,[21] so standard methods for estimating markdowns are indirect. Because markdowns estimators are not our innovation, we simply summarize them here and detail our estimation and price-deflation strategies in Appendix C.3. Specifically, Brooks et al. (2021*a*,*b*) and Yeh, Macaluso and Hershbein (2022) apply cost minimization to derive the labor markdown of establishment i at time t, ν_{it}, as the ratio of the output elasticity of labor, θ_{it}^{L}, to its cost share, α_{it}^{L}, divided by the establishment's markup, μ_{it}, which can itself be calculated by means of De Loecker and Warzynski's (2012) analogous ratio estimator: the ratio of the output elasticity, θ_{it}^{M}, to the cost share, α_{it}^{M}, of any price-taking, flexibly chosen input, M:[22]

$$\nu_{it} = \frac{\frac{\theta_{it}^{L}}{\alpha_{it}^{L}}}{\mu_{it}} = \frac{\frac{\theta_{it}^{L}}{\alpha_{it}^{L}}}{\frac{\theta_{it}^{M}}{\alpha_{it}^{M}}}.$$

Following the literature, we use the raw materials (M) as that flexibly chosen, price-taking input. We do not use energy, E, because substantial market power exists in this public market. The intuition is that both markups and markdowns create a wedge between output elasticities and cost shares. Flexible, price-taking inputs have no markdowns, and so their gap captures the pure markup, and cost minimization further implies that markups apply across all inputs uniformly. Hence, any remaining wedge for labor is the markdown. Importantly, the empirical ratio $\mu_{it} = \theta_{it}^{M}/\alpha_{it}^{M}$ need not equal one in practice, even in the absence of markups. The reason is

[21] Appendix C.2 details the production function estimation methods used to estimate the marginal product of labor at the establishment level.

[22] This approach to markdown estimation does not take any specific stance regarding the sources of market power in output or labor markets. De Loecker and Warzynski (2012) show that the ratio approach to estimating markups is compatible with a variety of cases of imperfect competition, including Cournot, Bertrand, and monopolistic competition. Similarly, Yeh, Macaluso and Hershbein (2022) show that the ratio approach to estimating markdowns nests several theoretical frameworks, including wage-posting, additive random utility, and monopsonistic competition models.

that firms may face market imperfections, such as contracting frictions and credit constraints, that distort the revenue shares measured in the data. Dividing by μ_{it} therefore nets out all potential firm-specific wedges that affect labor and raw materials symmetrically.

Calculating the output elasticity of labor and the establishment's markup requires knowledge of its production function, which can be obtained in various ways, depending on the assumptions about the function. For the sake of robustness, we consider four different approaches. The first approach (which we call translog) is the most general and assumes a second-order translog production function, $F(K, L, E, M)$, using the proxy method of Ackerberg, Caves and Frazer (2015) to estimate a unique production function for each industry that is time invariant except for a Hicks-neutral productivity term. Our second approach (Cobb–Douglas) uses the same methods but estimates a more restrictive Cobb–Douglas production function. Assuming a Cobb–Douglas function amounts to assuming that output elasticities do not vary across establishments within the same industry, which implies that the markdown trajectories within an industry mirror those of the ratios of the expenditure share of raw materials to the expenditure share of labor. The third approach (translog+CRS) addresses the critique of Gandhi, Navarro and Rivers (2020) that standard proxy methods are insufficiently identified without further restrictions. We reestimate the same translog production function with the additional assumption of constant returns to scale, as suggested by Flynn, Traina and Gandhi (2019). Our final approach ($\log(\alpha_M/\alpha_L)$) turns on that fact that, if the production function is Cobb–Douglas, differences in revenue shares between groups of establishments within the same industry over time reflect differences in markdowns across groups and over time. Such an approach is recommended by Bond et al. (2021) and is used by Brooks et al. (2021*a*). Fortunately, similarly to Brooks et al. (2021*a*), who use a slightly different variant of this approach, we find that the four different approaches yield results that differ somewhat quantitatively but are comparable both qualitatively and in their orders of magnitude.

4.2 Baseline Markdowns

In Appendix D.1, we show that our markdown estimates exhibit several of the statistical regularities documented in the literature. First, average markdowns over the 20-year prereform period were sizable and comparable to those reported for the United States by Yeh, Macaluso and Hershbein (2022), as shown in Table D.1. Second, while markdowns are pervasive across all regions and industries, they vary substantially, with the highest values observed for transport equipment and machinery, as shown in Table D.2, and the central and southern parts of the country, as shown in Table D.3 and Figure D.1. Finally, markdowns increase monotonically with establishment size, measured by the establishment's revenue share in its local labor market defined as a 3-digit industry×municipality combination, as shown in Figure D.2.[23]

4.3 Markdowns and Outsourcing

We next show that markdowns and the use of outsourcing are closely correlated at the establishment level even after establishment size is controlled for.

First, to estimate the correlation between markdowns and outsourcing, we regress the markdown on the share of outsourced employees at the establishment level via ordinary least squares (OLS) with establishment fixed effects, year dummies, and a control for the (log) number of workers. Table D.4 reports the results from this regression, with each column in the table reporting the results for a different markdown measure. Column (1) shows that a one-percentage-point increase in the share of outsourced employees raises the establishment's markdown, as estimated with the translog assumption for the production function, by 0.0034 on average (p=0.000). This increase is equivalent to a reduction in the wage share of the marginal revenue product of labor of 0.23 percentage points.[24] Columns (2)–(4) report impact estimates of similar magnitude and significance for markdowns estimated under the alternative assumptions that the production function is translog and exhibits constant returns to scale, as suggested in Flynn, Traina and Gandhi (2019); that the production function is Cobb–Douglas; and that differences in markdowns within an industry are reflected in the log of the ratio of the revenue share of materials to the revenue share of labor, as suggested in Bond et al. (2021).

[23] This relationship remains robust when we define local labor markets as 3-digit industry×commuting zone combinations, when we assume that outsourcing decisions are made at the firm level, and when we partition the establishment size distribution in alternative ways; see Appendices D.2, D.3, and D.4.

[24] Intuitively, the markdown is the reciprocal of the ratio of the wage to the marginal revenue product of labor. Thus, we back out the percentage-point change in the wage share of the marginal revenue product of labor by dividing 0.0034 over the mean markdown of 1.49 from Table D.1.

5 Causal Impacts of the Reform on Firms

To recover the reform's causal impacts, we propose a DID strategy that leverages two sources of variation: cross-sectional variation in exposure to the reform, as measured by an indicator for whether the establishment outsourced any employees before the reform, and time variation in the legality of outsourcing, as measured by a post-reform indicator, reflecting the subsequent collapse in outsourcing (recall Figure 1).

We explain this DID strategy in Section 5.1 and detail how we address identification threats in Section 5.2. We then report the reform's effects on outsourcing prevalence and employment in Section 5.3, mean wages and firing costs in Section 5.4, labor costs and profits in Section 5.5, other input usage and investment in Section 5.6, output and TFP in Section 5.7, market exit in Section 5.8, and the labor share and markdowns in Section 5.9. Finally, we analyze the results from a battery of robustness checks in Section 5.10.

5.1 Empirical Strategy

To recover the reform's impact on outcome of interest Y_{it} for establishment i after j periods, we estimate the parameter β_j in the following linear regression model via OLS:

$$Y_{it} = \sum_{j=A}^{B} [\mathbb{1}_{\{t=t_0+j\}} \times \text{Outsourcing}_{i,t_0}]\beta_j + \gamma_i + \delta_t + \mathbf{X}_{it}\boldsymbol{\lambda} + \varepsilon_{it}, \tag{1}$$

where $\text{Outsourcing}_{i,t_0}$ is an indicator for whether establishment i outsourced any worker at t_0, the period immediately before the reform; A is the first pre-shock and B the last post-shock period in the data; γ_i is an establishment fixed effect, which controls for all time-invariant outcome differences between establishments, including industry and location; δ_t is a time dummy, which absorbs all aggregate shocks that affect outcomes equally across all establishments; $\mathbf{X}_{it}$ is a vector of flexible controls, including the establishment's initial revenue and productivity and 3-digit industry indicators interacted with time dummies, which control for differences in size and revenue growth between establishments and for industry-specific outcome trends, respectively; and ε_{it} is an idiosyncratic unobserved shock to the outcome of interest. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level.

Importantly, although there is substantial variation in the share of outsourced workers at establishment level at baseline (see Appendix A Figure A.4), our specification leverages only variation in whether an establishment outsourced *at all* before the reform. Following Callaway, Goodman-Bacon and Sant'Anna (2024), our binarized specification recovers the average dose-specific treatment effects on treated establishments after j periods, weighted by the observed distribution of outsourcing intensity. Identification requires the standard parallel trends assumption: that outcomes would have followed the same trend in outsourcing and nonoutsourcing establishments in the absence of the reform.[25]

While the parallel trends assumption is fundamentally untestable, we follow the literature in interpreting an absence of differential pretrends in the outcomes of interest as evidence consistent with its validity. Following standard practice, we omit the interaction between outsourcing exposure and the dummy for the immediate pre-reform period so that all the coefficient estimates capture the evolution of the outcome gap between outsourcing and nonoutsourcing establishments relative to this omitted period.

5.2 Identification Threats

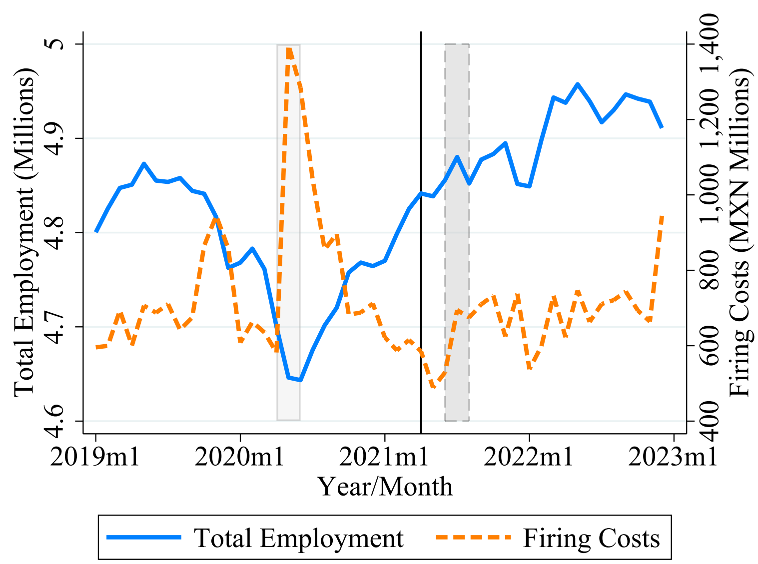
We address two threats to the parallel trends assumption that are particularly salient in our context. First, the prevalence of outsourcing in Mexico grew uninterruptedly since 1994, implying differential pretrends, for example, in the prevalence of direct hiring between treated and untreated establishments. Thus, rather than assuming that parallel trends hold exactly in the post-period, we construct robust confidence sets for each outcome following Rambachan and Roth (2023), under the assumption that post-treatment violations of the assumption are no larger than the maximum pre-treatment violation. Specifically, we report confidence sets under $\bar{M} = 1$, which is considerably more conservative than the values adopted in the example applications of this framework (see Rambachan and Roth, 2023).

Second, the reform was implemented in the wake of the COVID-19 pandemic, amid macroeconomic instability. The most plausible threat is that establishments that outsourced before the

[25] To build credibility, we verify that our binarized specification does not mask meaningful heterogeneity in dose-response by reporting dose-specific treatment effects on employment and wages in Appendix E. We also show that the effects on wages rise approximately linearly with exposure but are close to zero for employment across all exposure levels.

reform may have experienced different pandemic recovery paths than nonoutsourcing establishments.[26] For outcomes measured with the monthly manufacturing survey, this concern is mitigated by timing: The reform was enacted in April 2021 and its implementation concluded in September, over 18 months after the pandemic's onset. By then, Mexico had lifted all lockdowns, reopened nonessential industries, and rolled out vaccination campaigns. Aggregate data on hours worked and firing costs presented in Figure 2 confirm that the economic recovery was well underway by early 2021, suggesting that the worst of the pandemic shock had passed before the reform took effect. This timing allows us to observe any pandemic-driven differential trends in the pre-reform period and separate them from the reform's effects in estimation.

Figure 2: Total Employment and Firing Costs in the Manufacturing Sector



Notes: This figure presents total employment and firing costs in Mexico's manufacturing sector from 2019 to 2023. The vertical black line represents the enactment of the outsourcing reform in April 2021. The light gray area depicts the period of Mexico's most restrictive COVID-19 lockdown, April–June 2020. The dark gray area outlined with a dashed black line represents the 3-month grace period stipulated by the reform for the transfer of previously outsourced employees to the payroll of their employing firms.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2019 to 2023.

For outcomes measured with the annual manufacturing survey, however, the pandemic may have altered pre-reform differences between outsourcing and nonoutsourcing establishments in ways harder to disentangle, as fewer pre-reform observations are available at annual frequency. We address this using the framework of Rambachan and Roth (2023). Specifically,

[26]For instance, the pandemic had a disproportionately negative impact on small Latin American firms (Guerrero-Amezaga et al., 2022), which were also less likely to outsource. Slower recovery among such firms would confound our estimates of the reform's effects.

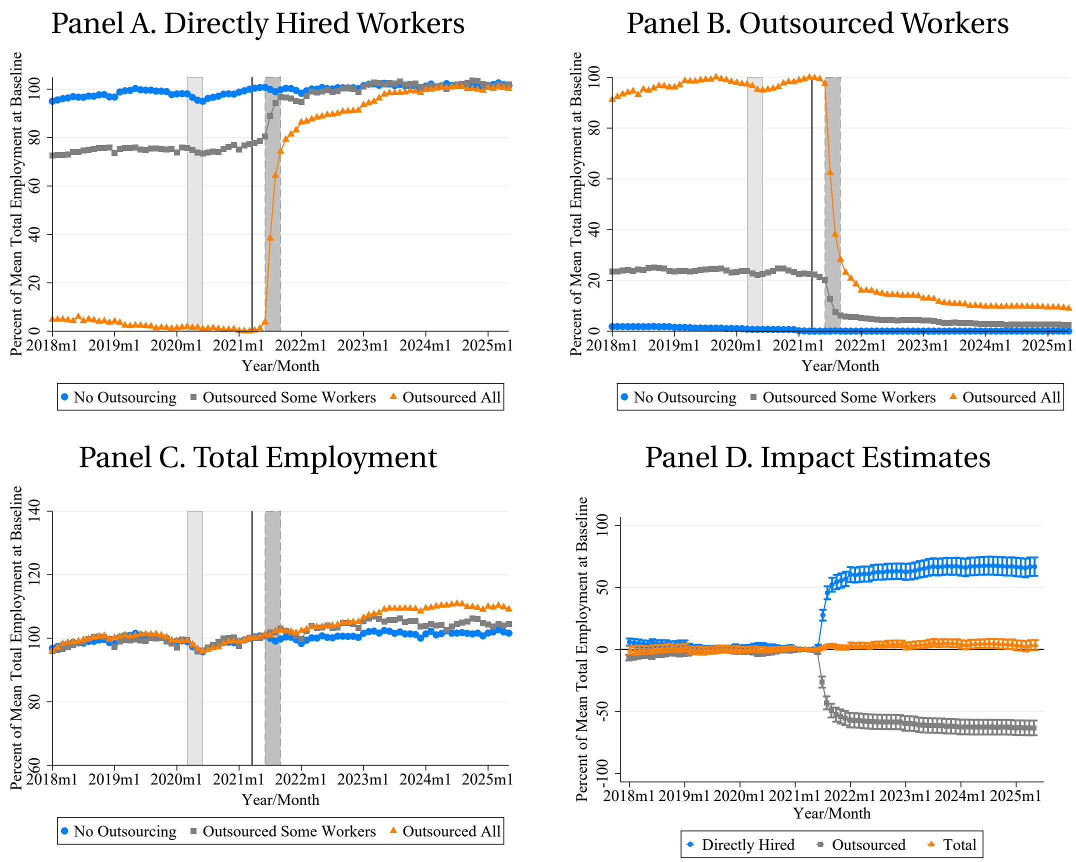
we normalize our event-study estimates by omitting the interaction of outsourcing exposure with the 2019 dummy rather than the 2020 dummy. This allows us to directly quantify the pandemic-induced divergence between the two groups in 2020, which we then use to bound the maximum plausible deviation from parallel trends induced by the pandemic in the post-reform period, under the assumption that differential recovery effects cannot exceed the initial pandemic impact. Under this approach, for the reform to be confounded by the pandemic, differential recovery in the post-reform period would have had to exceed the divergence caused by the pandemic itself.

5.3 Outsourcing Prevalence and Employment

We begin our analysis by estimating the reform's impacts on employment. Because outsourcing and direct hiring are directly observed in the data, we estimate the impact on total employment and decompose this effect across employment types. To facilitate the decomposition, we express all employment figures relative to the total employment mean of each treatment group in March 2021.[27] Figure 3 presents raw trends alongside our DID estimates for these outcomes.

[27] Appendix F shows our results do not depend on this decomposition. We report quantitatively similar impacts on log total employment at establishment level in Figure F.1 and Column (1) of Table F.1 and when we use the annual survey (Figure F.2 and Column (1) of Table F.2).

Figure 3: Reform Effects on Employment



Notes: Panels A, B, and C each present the mean of a different outcome for three groups of establishments: (i) establishments that did not outsource any workers in the month before the reform, (ii) establishments that outsourced at least one but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. The first gray shaded area denotes the strictest COVID-19 lockdown imposed by the Mexican federal government. The second gray shaded area, outlined with a dashed line, denotes the grace period mandated by the reform, during which staffing companies were allowed to transfer previously outsourced workers to their client firms. Panel D presents the difference-in-differences estimates for the three outcomes.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from January 2018 to May 2025.

Table 3 quantifies the observed impacts at December 2024, approximately 3 years after the reform. For exposed establishments, the reform increased the number of directly hired workers by 60.1 percentage points relative to mean total employment at baseline (p=0.000), owing to a reduction of 60.5 percentage points in the number of outsourced workers (p=0.000). Since outsourcing represented 68.5 percent of mean total employment, this reduction amounts to an 88 percent reduction in outsourced employment. The net effect is a reduction of 0.4 percentage points in overall employment, although this estimate is not significant (p=0.868).[28]

[28]We report quantitatively similar effects corresponding to the annual manufacturing survey in Figure F.3 and Table F.3. Moreover, our findings are robust to our accounting for market exit by estimating effects on a balanced

Table 3: Reform Impacts on Establishment-Level Employment

Regressor	Directly Hired	Outsourced	Total
	(1)	(2)	(3)
		Panel A. No Controls	
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t = \text{December 2024}\}$	67.0***	-62.8***	4.2**
	(3.8)	(3.0)	(2.0)
Robust Confidence Set	[48.3,86.7]	[-77,-49.2]	[-6.76,15.7]
N	721,806	721,806	721,806
R^2	0.061	0.174	0.001
		Panel B. With Flexible Controls	
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t = \text{December 2024}\}$	60.1***	-60.5***	-0.35
	(3.7)	(2.9)	(2.1)
Robust Confidence Set	[38.9,82.8]	[-79.3,-43.5]	[-15,15.6]
N	721,806	721,806	721,806
R^2	0.076	0.194	0.011
Mean for the Treaded in March 2021	31.5	68.5	100

Notes: Outcomes are expressed relative to the mean total employment of each group in March 2021. The set of flexible controls includes indicators for 3-digit industry and the establishment's initial revenue interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. **p<0.05, ***p<0.01.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2024.

Additionally, to estimate the reform's impacts on outsourcing prevalence at establishment level, we categorize establishments by their degree of outsourcing. Specifically, we create three mutually exclusive dummies indicating whether the establishment outsources all its employees, outsources only some employees, or directly hires all its employees. We present the raw trends of these indicators along with the corresponding DID estimates using data from our monthly manufacturing survey in Appendix F Figure F.5.

Table 4 reports our estimates of the reform's 3-year impacts for the establishments outsourcing at least one worker before the reform. The reform increased these establishments' probability of not outsourcing any workers by 80 percentage points (p=0.000). This increase corresponds to a reduction of 53 percentage points in the probability of outsourcing all the workers in the establishment (p=0.000) and a smaller reduction of 27 percentage points in the probability of outsourcing some but not all workers (p=0.000). Thus, the reform did indeed reduce outsourcing.[29]

panel in which employment is set to zero for exiting establishments (Panel A of Figure F.4 and Columns (1)–(3) of Table F.4).

[29] As in the case of employment, we report quantitatively similar effects corresponding to the annual manufacturing survey in Figure F.6 and Table F.5.

Table 4: Reform Impacts on Establishment-Level Outsourcing

Regressor	All Workers (1)	Some Workers (2)	No Workers (3)
		Panel A. No Controls	
Outsourcing$_{i,\text{March 2021}} \times \mathbb{1}\{t = \text{December 2024}\}$	-0.54***	-0.26***	0.80***
	(0.01)	(0.01)	(0.01)
Robust Confidence Set	[-.65,-.45]	[-.41,-.11]	[.62,.99]
N	721,806	721,806	721,806
R^2	0.428	0.085	0.522
		Panel A. With Flexible Controls	
Outsourcing$_{i,\text{March 2021}} \times \mathbb{1}\{t = \text{December 2024}\}$	-0.53***	-0.27***	0.80***
	(0.01)	(0.01)	(0.01)
Robust Confidence Set	[-.64,-.43]	[-.43,-.11]	[.59,1.01]
N	721,806	721,806	721,806
R^2	0.441	0.094	0.526
Mean for the Treated in March 2021	0.59	0.41	0

Notes: The set of flexible controls includes indicators for 3-digit industry and the establishment's initial revenue interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2024.

5.4 Mean Wages and Firing Costs

Next, we move on to estimate the reform's effects on mean wages and their composition at establishment level. We quantify the effects on four mutually exclusive components of wages: salaries, benefits, social security payments, and profit sharing.[30] For the analysis, however, we pool salaries and benefits into a single category, as together they capture all forms of compensation through which firms can deliver regular earnings to workers.

With this decomposition, we can determine whether the reform increased worker earnings or merely reconfigured their composition. It also helps elucidate the channels underlying changes in the wage bill, defined as the sum of these four components. For instance, if a wage increase caused by the reform is explained solely by a hike in the sum of salaries and benefits, the gains for workers likely result from a rise in take-home pay. In contrast, if the increase is also attributable to an increase in social security payments, their gains likely include an improvement in insurance value.[31] Likewise, if the increase comes partly from profit sharing, workers' gains include a higher option value of employment with the firm.

[30]We describe the methodology for this decomposition and the construction of the average wage and the labor share at establishment level in Appendix C.4.

[31]In Mexico, the cash-out value of public old-age and disability insurance policies guaranteed by formal employment is a function of social security payments.

Figure F.7 plots raw trends in mean wages by treatment group and the corresponding DID estimates, calculated from the monthly manufacturing survey data. As in the previous section, we express all figures relative to the outcome mean of each group in March 2021 to facilitate the subsequent decomposition of the total wage effect. The mean wage difference between treatment groups exhibits biannual negative spikes in the pre-reform period, which disappear after the reform. These biannual spikes correspond to the legally mandated dates for profit sharing (May) and payment of a thirteenth month's salary at Christmas (December),[32] indicating that establishments that outsourced workers before the reform paid lower wages in those months than establishments that hired workers directly. To corroborate this finding, we run 12 month-specific DID regressions, one for each calendar month, rather than a single regression pooling the data from all months. We present the coefficient estimates from these regressions in Panel A of Figure F.9. The results confirm spikes in the estimates in both calendar months.

Next, we present the raw trends and DID estimates for each component of wages in Figure F.10. Our estimates confirm that the pre-reform differences between treatment groups reflect lower take-home pay in December and lower profit sharing disbursal in May, followed by a post-reform flattening of differences across months for both outcomes. We again corroborate this finding with 12 month-specific regressions for each component. The estimates display the expected seasonal patterns: The reform's impacts on take-home pay spike in December, and those on profit sharing peak in May, as shown in Panel B of Figure F.9. These patterns provide compelling evidence that the reform strengthened compliance with labor regulations.[33]

Furthermore, we present raw trends by treatment group and DID estimates for firing costs relative to total workforce compensation in Figure F.12. The raw trends reveal a post-reform increase in firing costs among establishments that outsourced all workers before the reform. However, they also show a spike in firing costs during the COVID-19 lockdown. Therefore, despite being positive for the post-period, the estimates exhibit evidence of differential pre-trends around the time of the reform.

[32] We find quantitatively similar impacts on log total labor cost in Figure F.8 and Column (2) of Table F.1. Moreover, we report quantitatively similar effects estimated from the annual manufacturing survey data in Panel B of Figure F.2 and Column (2) of Table F.2.

[33] Consistent evidence emerges when we plot the log mean wage differences between treatment groups by calendar month over the full post-reform period (Figure F.11). Relative to the pre-period, differential spikes for the treatment group appear in May and December.

In Table 5, we quantify these impacts at 2024, three years after the reform.[34] The estimated impact is a 16.4 percentage point increase in mean wages ($p = 0.000$), as reported in Column (1). The decomposition in Columns (2)–(4) reveals an increase in take-home pay, captured by a 7.1-percentage-point rise in salaries and benefits ($p = 0.000$), and increases in the insurance and option values of employment, reflected in significant rises in social security payments and profit sharing of 7.4 ($p = 0.000$) and 3 ($p = 0.000$) percentage points, respectively. These increases in labor compensation are accompanied by a 0.9-percentage-point rise in firing costs relative to total workforce compensation ($p = 0.000$).[35]

Table 5: Reform Impacts on Wage Composition and Firing Costs

Regressor	Mean Wages				Firing Costs
	Total	Salaries & Benefits	Social Security	Profit Sharing	
	(1)	(2)	(3)	(4)	(5)
Panel A. No Controls					
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t \in 2024\}$	18.5***	8.8***	8.1***	2.9***	0.8***
	(1.8)	(1.6)	(0.3)	(0.3)	(0.2)
Robust Confidence Set	[-2.2,39.7]	[-8.3,26.5]	[3.3,12.7]	[1.0,5.0]	[-2.9,4.4]
N	721,806	721,806	721,806	721,806	721,806
R^2	0.070	0.061	0.105	0.049	0.002
Panel B. With Flexible Controls					
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t \in 2024\}$	16.4***	7.1***	7.4***	3.0***	0.9***
	(1.9)	(1.7)	(0.4)	(0.3)	(0.2)
Robust Confidence Set	[-6.1,39.6]	[-12,27.3]	[2.5,12.3]	[-0.7,6.8]	[-3.5,5.4]
N	721,806	721,806	721,806	721,806	721,806
R^2	0.073	0.066	0.11	0.05	0.005
Mean for the Treated in March 2021	100	90.8	7.8	1.4	0.305

Notes: Outcomes in Columns (1)–(4) are expressed relative to the mean wage of each group in March 2021. Firing costs in Column (5) are expressed relative to the mean wage bill of each group in March 2021. The set of flexible controls includes indicators for 3-digit industry and the establishment's initial revenue interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2024. All monetary amounts are deflated with Mexico's GDP deflator, the *índice nacional de precios al productor* (INPP).

5.5 Total Labor Cost and Profits

Above, we showed that the reform increased the wage bill and firing costs at establishment level. But total labor costs may not have increased if these changes were offset by a reduction

[34]We derive these estimates by replacing the month dummies with year dummies in the interaction terms in our baseline regression specification.

[35]We find similar impacts but stronger support for the parallel trends assumption, as captured by the robust confidence sets of Rambachan and Roth (2023), when aggregating monthly data on labor costs to annual level in Table F.6.

in payments to staffing agencies. Thus, the overall effect of the reform on total labor costs and, therefore, on profits, is *ex ante* ambiguous. Here, we estimate the reform's effects on total labor costs and taxable profits. If outsourcing generated efficiency gains, an increase in total labor costs following the reform would necessarily imply a reduction in profits. In contrast, if total labor costs do not increase and taxable profits remain unchanged, this would be consistent with outsourcing generating gains through evasion of corporate income taxes, social security contributions, and VAT, with under-the-table kickback payments from staffing firms being eliminated by the reform.

To test this mechanism, we turn to the annual manufacturing survey, which contains detailed information on firms' operating costs, allowing us to compute taxable profits. We use these data to estimate the reform's effects on total labor costs (expressed relative to each group's mean in 2019), decompose them into total labor compensation and fees paid to staffing firms, and estimate the effects on the firm's taxable profit margin.

Figures F.13 and F.14 present raw trends and DID estimates for total labor costs and profits, respectively. The results reveal a compositional shift away from payments to staffing firms and toward labor compensation, with no evidence of an increase in total labor costs and no reduction in taxable profits.[36] We quantify these effects in Table 6. The 22.1-percentage-point increase in total labor compensation ($p = 0.000$) is fully offset by a 24.6-percentage-point reduction in fees paid to staffing firms ($p = 0.000$), which results in a statistically nonsignificant 2.5-percentage-point decline in total labor costs ($p = 0.806$). Consistent with this, the estimated effect on the firm's taxable profit margin is a zero ($p = 0.617$), indicating that outsourcing generated no efficiency gains but instead reduced the taxable burden of employment through evasion of corporate, social security, and VAT liabilities.

[36]We report similar findings when estimating the effects on log total labor costs in Panel C of Figure F.2 and Column (3) of Table F.2.

Table 6: Reform Impacts on Establishments' Total Labor Costs and Profits

Regressor	Labor Cost			Profit Margin
	Total	Workforce Compensation	Staffing Fee	
	(1)	(2)	(3)	(4)
		Panel A. No Controls		
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	0.55	26.7***	-26.2***	0.00
	(3.2)	(3.4)	(1.6)	(0.01)
Robust Confidence Set	[-23,27]	[6.7,48]	[-43,-11]	[-.082,.094]
N	42,826	42,826	42,826	42,826
R^2	0.02	0.044	0.132	0.007
		Panel B. With Flexible Controls		
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	-2.5	22.1***	-24.6***	0.00
	(3.1)	(3.4)	(1.4)	(0.01)
Robust Confidence Set	[-25,22]	[-3.7,49]	[-41,-9.2]	[-.061,.072]
N	42,826	42,826	42,826	42,826
R^2	0.139	0.161	0.162	0.021
Mean for the Treated in 2019	100	71.8	28.2	0.326

Notes: Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 with Mexico's GDP deflator, the *índice nacional de precios al productor* (INPP).

5.6 Other Input Utilization

An outsourcing ban could trigger substitution between labor and other productive inputs, depending on the substitution elasticity. To examine this possibility, we estimate the impacts of the reform on capital accumulation, energy consumption, and raw material utilization. We rely on data from the annual manufacturing survey, as information on input utilization is reported only annually. Figures F.15 and F.16 begin by showing raw trends and DID estimates for capital, energy, raw materials, and investment. Next, Table F.7 reports the estimated impacts on each of these variables at three years after the reform.[37] Columns (1)–(4) offer no evidence of an effect on establishment usage of capital, raw materials, energy, or investment.[38] Thus, there is no evidence of substitution away from labor or a reduction in capital accumulation.

[37] Recall from Footnote 20 that INEGI will publish the 2025 wave of the annual manufacturing survey only in December 31, 2026, so we cannot currently estimate the impacts at 4 years after the reform.

[38] In Appendix L, we complement the analysis of investment among existing establishments by examining new investment prospects with a monthly survey of private-sector analysts from the central bank. Our findings are also robust to our accounting for market exit by estimating effects on a balanced panel in which investment is set to zero for exiting establishments (Panel B of Figure F.4 and Column (4) of Table F.4).

5.7 Output and TFP

Next, we investigate the reform's effects on output and TFP. We would expect a negative effect on both if outsourcing entailed managerial efficiency gains in production. Using the methodology described in Section C.2, we estimate TFP under three alternative assumptions about the form of the production function: translog, translog with constant returns to scale, and Cobb–Douglas. We also compute a fourth TFP measure that relies on a Cobb–Douglas assumption but imputes the coefficients of the production function on the basis of the cross-sectional mean input shares for each 4-digit industry. Our first three productivity measures require the lagged input variables to be used as instruments in our estimating the production function, so we can use only observations corresponding to establishments that did not fall out of the annual manufacturing panel after its rotation in 2021 to estimate productivity impacts at 2022. This issue limits our sample size, but Figure F.17 Panel A and Table F.8 provide reassuring evidence that the productivity impacts are of similar direction, magnitude, and significance for the initial panel of establishments running up to 2021.

Figures F.18 and F.19 present raw trends and DID estimates for output and productivity, respectively. We fail to find significantly differential pretrends between exposed and nonexposed establishments before the reform. Next, Table F.9 reports our estimates of the impact on output and TFP at three years after the reform. We find no significant impact on either variable under any of our four alternative assumptions about the form of the production function. Thus, we conclude that outsourced workers moved into direct employment with no reduction in output, which indicates that there were no efficiency losses in production.

5.8 Market Exit

Because the reform may have affected firm profitability through changes in labor cost structure, we might expect impacts on market exit, particularly among marginal firms. To investigate this outcome, we construct an establishment-level indicator of market exit at time t, defined as the inverse of the firm's survival probability. We apply our DID strategy to estimate this outcome in a balanced panel spanning 2013–2024 and comprising all manufacturing establishments that did not rotate out of the annual manufacturing survey in 2021 and had not exited the market prior to the reform.

We report our estimates of the probability of market exit in Figure F.20 and Table F.10. Importantly, the pre-reform survival probabilities in the figure are mechanically equal to one, as the estimation sample conditions on establishment survival in 2020. The regression estimates indicate that, by three years after its enactment, the reform had exerted no statistically significant effect on market exit. We corroborate this finding in Appendix M, where we analyze the reform's effects on local firm dynamics—including entry and exit rates and establishment counts—with a Bartik instrument and data from the universe of manufacturing establishments in the economic census, rather than the annual manufacturing survey.

5.9 Labor Share and Markdowns

We now examine whether the reform successfully achieved its primary policy goals of reducing labor exploitation and steering economic rents toward workers. We estimate impacts on two key variables capturing this intended outcome: the labor share and markdowns.

As we did for our TFP impact estimation above, we compute markdowns under alternative assumptions on functional form, with the sample used for estimation including only establishments that did not rotate out of the annual manufacturing survey in 2021. In addition, our sample includes only observations for which lagged data are available, as we need these for markdown estimation.[39] We report raw trends and DID estimates for markdowns using this sample in Figure F.21 and for the labor share in Figure F.22.

Table 7 reports our impact estimates. We find a strongly significant increase in the labor share amounting to 3 percentage points ($p = 0.000$) for establishments that had outsourced any workers pre-reform. Consistent with this finding, we report a strongly significant reduction of 26 log points ($p = 0.000$) in wage markdowns estimated by means of the translog assumption and a statistically significant reduction of 27 log points for the markdowns estimated under the added assumption that the production function displays constant returns to scale. The reduction is slightly larger at 28 log points ($p = 0.000$) when the markdowns are estimated under a Cobb–Douglas assumption and of similar magnitude when we use the log of the ratio of the

[39] Panel B of Figure F.17 and Table F.11 show impacts on markdowns similar to those discussed in the main text but for the initial panel of establishments running up to 2021.

revenue share of materials to the revenue share of labor to capture markdown impacts, as suggested in Bond et al. (2021). These findings indicate that the reform was successful in reducing exploitation and transferring rents to workers.

Table 7: Reform Impacts on Establishment-Level Labor Share and Markdowns

Regressor	Labor Share	Log Markdowns			
		Translog	Translog+CRS	Cobb-Douglas	$\log(\alpha_M/\alpha_L)$
	(1)	(2)	(3)	(4)	(5)
		Panel A. No Controls			
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t=2024\}$	0.02***	-0.24***	-0.26***	-0.25***	-0.26***
	(0.00)	(0.03)	(0.03)	(0.04)	(0.03)
Robust Confidence Set	[.004,.05]	[-.44,-.05]	[-.46,-.08]	[-.48,.01]	[-.46,-.09]
N	26,919	26,919	26,919	26,919	26,919
R^2	0.103	0.098	0.113	0.058	0.113
		Panel B. With Flexible Controls			
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t=2024\}$	0.03***	-0.26***	-0.27***	-0.28***	-0.28***
	(0.00)	(0.03)	(0.04)	(0.03)	(0.03)
Robust Confidence Set	[.007,.06]	[-.5,-.03]	[-.5,.008]	[-.52,-.06]	[-.52,-.06]
N	26,919	26,919	26,919	26,919	26,919
R^2	0.135	0.121	0.092	0.132	0.131
Mean for the Treated in 2019	0.158				

Notes: The sample used for estimation includes only observations of establishments that did not rotate out of the annual manufacturing panel in 2021 and for which a lag of the input variables is available. We exclude all expenses other than capital, raw materials, energy, and labor in the calculation of the labor share. Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 with Mexico's GDP deflator, the *índice nacional de precios al productor* (INPP).

Finally, we estimate heterogeneity in the markdown impacts. If the impacts correspond to the bottom of the markdown distribution, our findings would be consistent with the reform having the perverse effect of exacerbating labor costs for establishments that paid fair wages to begin with. Conversely, if the impacts are concentrated at the top of the distribution, our findings would be consistent with the reform's having successfully reduced labor exploitation.

Table 8 examines impact heterogeneity across five dichotomous establishment-level characteristics at baseline: whether the establishment markdown was above the 75th percentile in 2019, whether its industry's average markdown was above the 75th percentile in 2019, whether the establishment's operations are based in Mexico's central or southern regions, whether the establishment is foreign owned, and whether the establishment operates in a metropolitan area. The first column indicates that the impact on markdowns is concentrated among the firms in the top quartile of baseline markdowns.[40] In contrast, we find no statistically significant effects operating adversely against establishments in high-markdown industries or the central and southern regions or in foreign-owned or urban establishments.

Table 8: Impact Heterogeneity
Outcome Variable: Log(Markdowns)

Regressor	(1)	(2)	(3)	(4)	(5)
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	-0.12***	-0.24***	-0.28***	-0.26***	-0.31***
	(0.04)	(0.04)	(0.03)	(0.04)	(0.06)
Interacted with:					
Markdown>75th Percentile in 2019	-0.39***				
	(0.05)				
Top-5 Markdown Industry in 2019		-0.05			
		(0.06)			
Central or South Region			0.06		
			(0.06)		
Foreign Ownership				-0.01	
				(0.05)	
Metropolitan Area					0.06
					(0.07)
N	26,919	26,919	26,919	26,919	26,919
R^2	0.128	0.122	0.122	0.121	0.122

Notes: The estimation sample includes only observations of establishments that did not rotate out of the annual manufacturing panel in 2021 and for which a lag of the input variables is available. All regressions include establishment fixed effects, time dummies, 3-digit industry indicators, and the initial revenue and productivity of the establishment interacted with time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. All regressions control for the interacted variables. ***p<0.01.
Source: Authors' elaboration with data from the Mexican economic census waves from 2014 to 2019, which is used to rank industries by baseline markdown, and the annual manufacturing survey from 2013 to 2024.

5.10 Robustness Checks

We conduct a battery of checks to test the robustness of our findings. First, our estimates may be confounded by the effect of COVID-19 on firms and workers. To rule this concern out, in Appendix G, we test for differential effects on the average wage and employment in the industries deemed essential at pandemic onset. Establishments in these industries were allowed to resume operations immediately after the most restrictive phase of the lockdown, April–June 2020. Figure G.1 shows that, in essential industries, employment returned to normal three months after the strictest lockdown but took 12 months to return to its prepandemic level in nonessential industries. Were our empirical strategy indeed confounded by the effect of COVID-19, we would

[40] To assess whether this finding is explained by mean reversion, we classify establishments by their markdown quartile in the pooled distribution of establishments' markdowns in their initial year of appearance in the annual manufacturing survey and test for heterogeneity in Table F.12, finding a similar pattern.

expect to observe differential impacts on employment or wages by industry type. However, Figure G.2 and Table G.1 show that the reform's effects on employment and wages in essential industries is statistically indistinguishable from those in nonessential industries.

Second, in Appendix H, we test whether the reform reduced employment *flexibility* rather than employment *levels*. Novel empirical evidence for the US is consistent with outsourcing providing flexibility for businesses (Atencio De Leon, Macaluso and Yeh, 2023). As we mentioned above, this possibility is limited in our context because the reform did not ban temporary employment. Nevertheless, we test whether the reform impacted flexibility, as measured by the standard deviation of employment at establishment level. Figure H.1 shows that, generally, the volatility of outsourced employment is higher than that of directly hired employment but that the volatility of both employment types temporarily *rose*, rather than dropped, after the reform. This makes sense: Transferring outsourced workers to the employing firm's payroll shifted employment counts from one employment category to the other. Comparing employment volatility at exposed and unexposed establishments in Figure H.2, we observe that it increased for the former but not the latter. Finally, in Figure H.3 and Table H.1, we test whether the reform had a differential effect on employment levels and wages for exposed establishments with high volatility at baseline but find no statistically significant evidence that this was the case.

Third, in Appendix I, we address the important concern that firms may have adjusted their hiring practices immediately after the new government was elected because they correctly foresaw that the reform would ensue. Outsourcing would in that case have declined long before it was banned. Then, our empirical strategy—instead of capturing the true causal impacts of the reform—would simply be comparing the outcomes of establishments that had anticipated the reform with those of establishments that hadn't. Were this the case, we would expect to observe a structural shift in aggregate outsourcing prevalence or change in trend around the time of the election. We do not. Figure I.1 shows that the cross-sectional mean share of outsourced workers at establishment level does not jump or change slope around the election. Furthermore, we would observe different estimates of the reform's impact on outsourcing prevalence and employment if we used only establishments that had directly hired all their workers before the election as controls (i.e., excluding from the control group any establishments that had ever outsourced employees). Figure I.2 and Tables I.1 and I.2 show that the impacts estimated when we use this restricted control group are quantitatively similar to our baseline estimates.

Fourth, our headline findings might arise from mean reversion, similarly to the spillover effects from internal wage increases in large US firms such as Amazon, Target, and Walmart on the wages paid by other local employers (Derenoncourt et al., 2022; Derenoncourt and Weil, 2024).[41] In Appendix J Tables J.1– J.5, we conduct time-shifted placebo analyses using pre-reform data for the passing of fictitious reforms in 2016, 2017, 2018, and 2019 for the annual manufacturing survey data and a placebo reform in January 2019 for the monthly data, given that the latter data are available for only 2018–2025. Across outcomes, we find small, statistically nonsignificant, and nonrobust placebo effects, as measured by the confidence sets proposed by Rambachan and Roth (2023). Importantly, for direct hiring, the largest placebo estimate is less than 5 percent of our headline effect, consistent with the high persistence of outsourcing practices, which implies limited scope for spurious reversion-driven changes in direct hiring in the absence of the reform.

Finally, to ensure that our finding that the reform reduced markdowns does not hinge on our modeling choices, we probe two concerns about markdown estimation in Appendix K. First, the results might be sensitive to our normalizing the ratio of the output elasticity of labor to the labor revenue share by the corresponding object for raw materials. To address this possibility, we omit this normalization and reestimate the reform's effects using the labor ratio alone in Figure K.1 and Table K.1. Second, while our main analysis estimates the production function under alternative functional-form assumptions, it always instruments for current input choices with lagged own inputs, abstracting from the possibility that current input choices may also partly respond to shifts in competitive conditions. To assess whether imperfect competition matters, in Figure K.2 and Table K.2, we implement the methodology of Ackerberg and De Loecker (2024) to identify the production function under Cournot competition, which augments the set of excluded instruments with competitors' lagged output. Our results remain statistically significant and stable in magnitude across both robustness checks.

[41] In contrast, Dustmann et al. (2022) investigate Germany's introduction of a national minimum wage in 2015 and use placebo tests based on an earlier period (2012–2014), finding that the estimated wage changes during the placebo period are small compared to the sharp increase observed after the policy was implemented.

6 Reform Impacts on Workers

In this section, we estimate the reform's effects on the labor market outcomes of previously outsourced workers. We center our attention on whether, after the reform, such workers were *less* likely to remain formally employed and, in particular, employed in manufacturing. Although our main empirical analysis showed the reform had null employment impacts at establishment level, testing for divergent post-reform employment trajectories of formerly outsourced workers is crucial because our establishment-level data do not let us rule out that the outsourced workers were terminated and replaced with other workers after the reform. If so, we would expect a widening gap in the likelihood of formal employment between the previously outsourced and other workers. Findings to the contrary, in turn, would constitute compelling evidence that the reform caused previously outsourced workers to gain insider status within their employing firms.

We also take particular interest in the reform's effect on the worker wages registered with the social security authority. Had firms responded to the reform by passing through the cost of the increase in social security contributions, profit sharing, and other benefits to workers, we would expect a reduction in registered wages. Evidence to the contrary would confirm our finding that the reform increased all components of labor compensation, not only wages, because, in Mexico's Bismarckian social security system, social security contributions and other benefits are an increasing function of the wage registered with the social security authority. Moreover, if the wage increases were concentrated at the bottom of the wage distribution, we could conclude the reform reduced inequality across workers.

6.1 Empirical Method

Our analysis relies on matched employer–employee data for the universe of Mexican formal firms and workers from the social security authority (the IMSS). Although these data are comprehensive, the IMSS had no reliable registry of staffing companies before the reform, so we cannot back out the identities of employees hired through a staffing firm but employed on the premises of a different firm. To surmount this challenge, we replicate as closely as possible the empirical strategy of Goldschmidt and Schmieder (2017) to identify a particular type of outsourcing from large cluster flows of workers from professional services to manufacturing firms, excluding smaller flows and flows within manufacturing.

We define a clustered flow of workers as a group of workers all employed in establishment A, which we refer to as the predecessor, and then in the following month all employed in establishment B, the successor. Such clustered flows are deemed a result of the reform mandate if all the following conditions hold: First, we eliminate small flows that may be part of regular month-to-month worker movements by requiring the flow to consist of 10 employees or more. Second, the flow must have occurred during one of the three months of the mandated transfer period (July–September 2021). Third, the flow must have occurred from professional services to manufacturing.

We construct a comparison group of directly hired workers with a matching algorithm in three steps. First, we select a sample of potential controls. For each outsourced worker, we take the set of directly hired workers employed in any 4-digit manufacturing industry in June 2021, a month before the beginning of the transfer period, as our potential control group. To avoid including staffing firms operating in the manufacturing sector as controls, we require the firms in this group to have not downsized by more than 50 percent during the 3-month transfer period.

Second, we estimate a probit regression of whether a worker is outsourced for each transfer month, using worker tenure and establishment size in June 2021 and monthly wages for January–June 2021 as predictors. The inclusion of pre-transfer wages as predictive variables in the probit effectively limits the estimation sample to workers with tenure of at least six months before the month of the transfer. Under this criterion, we exclude workers who regained employment sometime in the first half of 2021 after an unemployment spell associated with the COVID-19 pandemic, so that our analysis does not capture wage or employment impacts attributable to changes in workforce composition in the post-pandemic months. Additionally, to ensure common support at the matching stage, we winsorize the estimation sample by excluding workers whose wages in the pre-transfer months of 2021 display month-to-month fluctuations that exceed 95 percent of the cross-sectional distribution for the universe of workers in professional services and manufacturing.

Finally, using nearest-neighbor propensity score matching without replacement, we match outsourced workers successively by cohort with workers directly hired in manufacturing from the probit estimation sample, with the condition that the selected control workers must have remained employed in the month of the transfer. Outsourced workers in the September 2021 cohort are matched first, those in the August 2021 cohort second, and those in the July 2021 cohort last. In Appendix N Table N.1, we compare the characteristics of matched workers between each cohort of outsourced workers and the corresponding group of matched comparison workers. For all the transfer cohorts, the matched workers are similar, as measured by the size of the standardized differences in the means of the available variables in the dataset. Notably, though our selection criteria are rather strict, the number of flows we identify is comparable to the number identified by Goldschmidt and Schmieder (2017) for a 30-year period in Germany.

Once each outsourced worker is matched with a directly hired worker in manufacturing, we construct our estimation sample, which includes only the employment spells in the predecessor and successor establishments for each worker so that we can avoid capturing wage changes related to firm moves. Similarly, for the control group, only the employment spell in the firm where the worker was employed in June 2021 is retained. In addition to wages, we construct an unemployment indicator that takes value 1 if the worker dropped out of the formal sector and a separation indicator that takes 1 if the worker changed employing firm but remained employed in the post-period.

We use the matched sample to estimate the reform's effect in an event-study design with staggered treatment timing. The recent DID literature has developed a range of estimation methods for this setting (for a comprehensive review, see Roth et al., 2023), as conventional two-way fixed effects (TWFE) specifications can yield biased estimates when treatment adoption is staggered.[42] We opt for Sun and Abraham's (2021) interaction-weighted (IW) estimator, which has the advantage of being cast as a familiar regression specification and is particularly well suited to our application for two reasons: First, because our sample holds the firm of employment constant in the pre-reform period for both treated and control workers, worker and

[42] In particular, TWFE estimators may rely on comparisons between already treated and newly treated units (Goodman-Bacon, 2021), which can generate negative weights (De Chaisemartin and d'Haultfoeuille, 2020) and contaminate the interpretation of dynamic treatment effects (Sun and Abraham, 2021). As alternatives, several estimators have been proposed to recover cohort-specific treatment effects and aggregate them appropriately across cohorts and event times. Their implementations range from semiparametric estimation and aggregation of group–time average treatment effects (Callaway and Sant'Anna, 2021) to imputation- (Borusyak, Jaravel and Spiess, 2024) and regression-based estimators (Sun and Abraham, 2021).

firm effects are not separately identified in the pre-treatment data. This precludes our using Borusyak, Jaravel and Spiess's (2024) imputation estimator, which requires formulation of a counterfactual outcome for workers had they stayed in the same firm. Second, because workers are matched before estimation and the regression includes no time-varying covariates, Callaway and Sant'Anna's (2021) doubly robust approach provides no additional adjustment for covariate imbalance. Moreover, when the comparison group consists exclusively of never-treated workers, both approaches identify the same cohort-specific average treatment effects on the treated (CATTs). Here, the choice between estimators is largely practical. The IW estimator conveniently accommodates the unbalanced post-treatment panel structure that arises as workers exit destination firms, whereas the Callaway and Sant'Anna (2021) implementation typically requires balancing the panel or dropping worker-period observations to construct one.

We estimate treatment effects separately for each treatment cohort instead of pooling the three cohorts in one specification. This reflects the data structure imposed by our matching procedure, which requires control workers to remain employed at their predecessor firm through the month preceding the transfer of their matched treated counterpart. Hence, the date at which control workers first enter unemployment risk differs across cohorts.[43] While this issue is innocuous for wages, a single specification pooling the three cohorts would mechanically yield negative unemployment effects for the last two pre-treatment months. Estimating each cohort separately ensures that each treated worker is compared only to control workers who face the same unemployment risk window.

We first estimate the CATTs for each cohort e separately with an interacted TWFE regression:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{\ell \neq -1} \delta_{e,\ell} \mathbb{1}\{t - e = \ell\} + \varepsilon_{it}, \tag{2}$$

where Y_{it} is the outcome of interest for worker i in month t; α_i and γ_t are worker and time fixed effects; E_i denotes the month of the transfer for worker i (i.e., the treatment cohort); $\mathcal{G} = \{\text{Jul}, \text{Aug}, \text{Sep}\}$ is the set of treatment cohorts; $\ell = t - e$ is event time, measuring months relative to the transfer; and $\delta_{e,\ell} \equiv \text{CATT}(e, \ell)$ is the CATT for cohort e at relative time ℓ. Standard errors are clustered at the level of the predecessor firm.

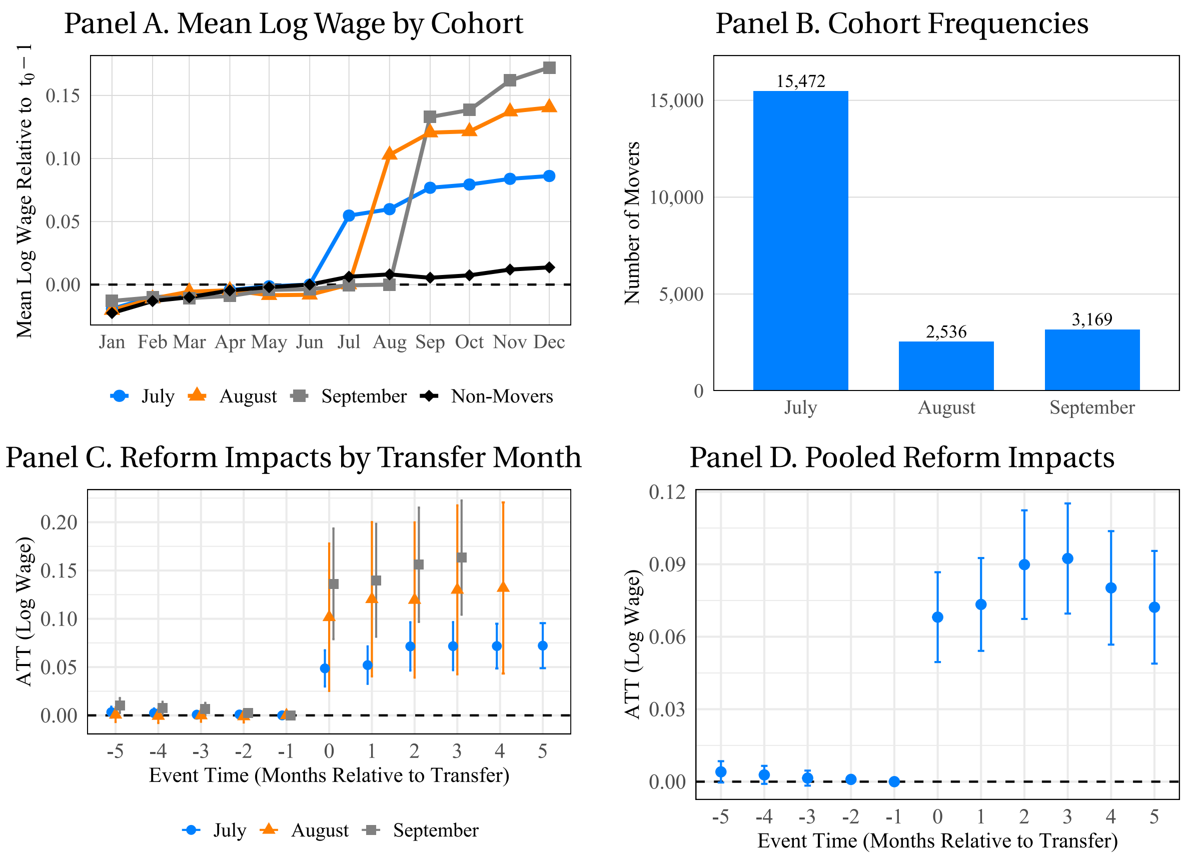
[43] For example, control workers matched to the July cohort become at risk of unemployment one month before those matched to the August cohort and two months before those matched to the September cohort.

For each cohort, we aggregate the CATTs across post-treatment event times $\mathcal{L}_e = \{\ell > 0 : \ell \text{ observed for cohort } e\}$ into a cohort-level average, $\nu_e = \sum_{\ell \in \mathcal{L}_e} \delta_{e,\ell} \cdot \Pr(\ell_i = \ell \mid \ell_i \in \mathcal{L}_e)$. We also construct pooled dynamic effects across cohorts for each ℓ as $\nu_\ell = \sum_{e \in \mathcal{G}_\ell} \delta_{e,\ell} \cdot \Pr(E_i = e \mid E_i \in \mathcal{G}_\ell)$, where $\mathcal{G}_\ell \subseteq \mathcal{G}$ is the set of cohorts observed at event time ℓ. Finally, we combine the cohort-level averages into an overall treatment effect, $\text{ATT} = \sum_{e \in \mathcal{G}} \nu_e \cdot \Pr(E_i = e \mid E_i \in \mathcal{G})$.

6.2 Wages

We begin by examining the raw trends in mean wages by treatment cohort together with the corresponding treatment effect estimates in Figure 4. Panel A displays mean log wages that we normalize by subtracting the cohort-specific mean log wage in the treatment month and, for the pooled sample of controls, by subtracting the pooled control group mean log wage in June. We focus on the year of the reform to highlight the discrete jumps in mean wages occurring exactly in the month of the move for all treatment cohorts. However, the post-treatment wage differences persisted beyond 2023, as shown in Appendix N Figure N.1. The staggered timing of these jumps aligns closely with the timing of worker transfers, a pattern that would be difficult to reconcile with alternative explanations unrelated to the reform. For example, if idiosyncratic wage shocks confounded these trends, we would not expect to see once-and-for-all jumps exactly at the month of the move. Similarly, if stigma effects were present, we would expect workers to command *lower* wages after transitioning to direct hiring.

Figure 4: Wage Effects of the Reform at Worker Level



Notes: This figure illustrates the impact of the outsourcing ban on worker-level wages. Panel A displays mean log wages by month, which we normalize by subtracting the cohort-specific mean log wage in the treatment month; control group means correspond to the pooled sample of cohort-specific controls and are normalized relative to June. Panel B shows the number of workers transitioning each month. Panel C presents the interaction-weighted average treatment effect of the outsourcing ban for each cohort, estimated with the interaction-weighted (IW) estimator. Panel D reports the average reform effect, which we calculate by averaging the cohort-specific event-study estimates.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021.

Panel B reports the number of workers transitioning each month, showing that the bulk of worker transfers occurred in July, which had approximately five times as many moves as August and September. Panel C reports the CATTs of the outsourcing ban by time relative to the move, estimated with the IW estimator. The magnitude of the estimated impacts closely matches the observed jumps in mean wages at the time of the move for all cohorts. Finally, Panel D reports the average reform effect, calculated as the worker-frequency-weighted average of the cohort-specific event-study estimates.

We quantify the CATTs for each cohort and the average effect using progressively longer post-reform estimation windows in Table 9. In the estimation sample restricted to 2021, the transition from outsourcing to direct hiring increased mean wages by 6.4 percent ($p < 0.001$), 12.0 percent ($p = 0.004$), and 14.8 percent ($p < 0.001$) for workers transferred in July, August, and September, respectively, yielding an average effect of 8.3 percent ($p < 0.001$). These effects increase slightly in magnitude as the estimation window expands to include 2021–2022 and 2021–2023. By 2023, the wage gains had reached 8.2 percent ($p < 0.001$), 12.9 percent ($p = 0.009$), and 19.1 percent ($p < 0.001$) for workers transferred in July, August, and September, respectively, corresponding to an average effect of 10.4 percent ($p < 0.001$).

Table 9: Reform Impacts on Worker-Level Wages by Post-Treatment Horizon

	July	August	September	Pooled
	(1)	(2)	(3)	(4)
ATT (2021)	0.064***	0.120***	0.148***	0.083***
	(0.011)	(0.042)	(0.030)	(0.010)
Observations	351,249	58,213	73,584	483,046
R^2	0.9904	0.9860	0.9751	0.9876
ATT (2021–2022)	0.073***	0.133***	0.174***	0.096***
	(0.012)	(0.046)	(0.030)	(0.012)
Observations	891,036	144,945	179,178	1,215,159
R^2	0.9870	0.9827	0.9685	0.9837
ATT (2021–2023)	0.082***	0.129***	0.191***	0.104***
	(0.013)	(0.049)	(0.029)	(0.012)
Observations	1,288,902	207,830	247,059	1,743,791
R^2	0.9846	0.9808	0.9642	0.9811
Treated Workers	15,472	2,536	3,169	21,177

Notes: Columns (1)–(3) report the cohort average treatment effects (CATT) by treatment cohort, estimated with the interaction-weighted (IW) estimator over progressively longer post-treatment sample periods. Standard errors are clustered at the predecessor firm level. Column (4) presents, for each specification, the equally weighted average across cohorts of the estimated CATTs. The total number of workers in each regression equals twice the number of treated workers. ***$p < 0.01$.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021 through 2023.

6.3 Unemployment

Despite the outsourcing ban's positive effects on worker-level wages, the reform could in principle have increased the probability of unemployment for the workers affected by it. To assess this possibility, we construct an outcome variable capturing the cumulative risk of unemployment following the transfer. This variable equals zero before the transfer month and switches to one at the onset of the worker's first unemployment spell after the transfer.

Appendix O Figure O.1 presents raw trends in this outcome by treatment cohort together with the corresponding treatment effect estimates. Panel A plots the mean unemployment risk by cohort. The raw trends show that unemployment risk initially rose more quickly for the control workers before stabilizing and evolving in parallel with that of the treated workers. Panel B reports the number of worker transitions by month of move. Panels C and D present the CATTs and pooled event-study estimates, respectively. These indicate that the reform reduced unemployment risk, with the effect becoming increasingly negative during the first 12 months after the transfer and flattening thereafter. In other words, the transition to direct hiring temporarily reduced unemployment risk for previously outsourced workers, with the effect stabilizing after approximately one year.

Appendix O Table O.1 quantifies these impacts across post-reform horizons. The reform reduced unemployment risk by 3.8 percentage points at 2021 ($p < 0.001$). By the end of 2022, previously outsourced workers' unemployment risk was 7 percentage points lower than control workers' ($p < 0.001$), stabilizing at roughly 7.5 percentage points lower in 2023 ($p < 0.001$). This pattern is consistent with institutional features of the reform: The transition to direct hiring effectively reset employment relationships, which typically involve 12-month contracts.

6.4 Job-to-Job Separations

A related question is whether the reform affected the propensity of previously outsourced workers to separate from their employers. Separation behavior provides information about the degree of monopsony power exercised by firms. In particular, low separation rates despite low wages suggest a low elasticity of labor supply to the firm (Bassier, Dube and Naidu, 2022). This issue is particularly relevant in developing countries, where limited availability of formal jobs may discourage workers from quitting even when wages are low (Bassier, 2023).

In our context, what direction we should expect for the reform's effect on separation rates is not immediately obvious. On the one hand, the reform increased the attractiveness of the current job for previously outsourced workers by raising wages and improving employment benefits, thereby increasing the value of the current match. On the other hand, the reform raised the value of alternative formal jobs, improving all workers' outside options. To assess this empirically, we construct a separation risk variable defined analogously to the unemployment risk variable above. The variable equals zero as long as the worker remains employed at the successor firm and switches to one when the worker separates from the successor firm to take a different formal job, remaining equal to one thereafter. We further restrict the group of potential controls for each treated cohort to workers who had not separated from their June 2021 employer by the month of the move. Importantly, all our analyses omit separation from the predecessor firm, which would be equal to one at the month of the move by construction.

We present the results of our analysis of the reform's impacts on job-to-job separation rates in Appendix P. Table P.1 reports the results of our matching procedure with the additional requirement that control workers remain employed at their June 2021 employer until the month of the move of their matched treated counterpart. Figure P.1 plots the raw trends in separation risk by treatment cohort together with the corresponding effect estimates. Panel A shows no meaningful differences in separation rates between treated and control workers. Panel B reports the number of workers by cohort, which is identical to the distribution observed in our analyses of wage and unemployment outcomes. Panels C and D report the CATTs and pooled estimates, respectively. Consistent with the patterns in Panel A, these provide no evidence of a statistically significant effect on job-to-job separation rates. Table P.2 confirms this conclusion. Across all estimation windows, including the full sample through December 2023, we find no statistically significant impact on separation behavior.

6.5 Firm Premia

The literature attributes the wage penalties from outsourcing to differences in firm wage premia, as workers typically move from high-paying client firms to lower-paying staffing firms, losing access to firm-specific rents (Dube and Kaplan, 2010; Goldschmidt and Schmieder, 2017; Drenik et al., 2020). We examine whether the wage effects we estimate in our setting are consistent with this mechanism operating in reverse—that is, whether the reform induced worker transitions from lower-premium outsourcing firms to higher-premium direct employers.

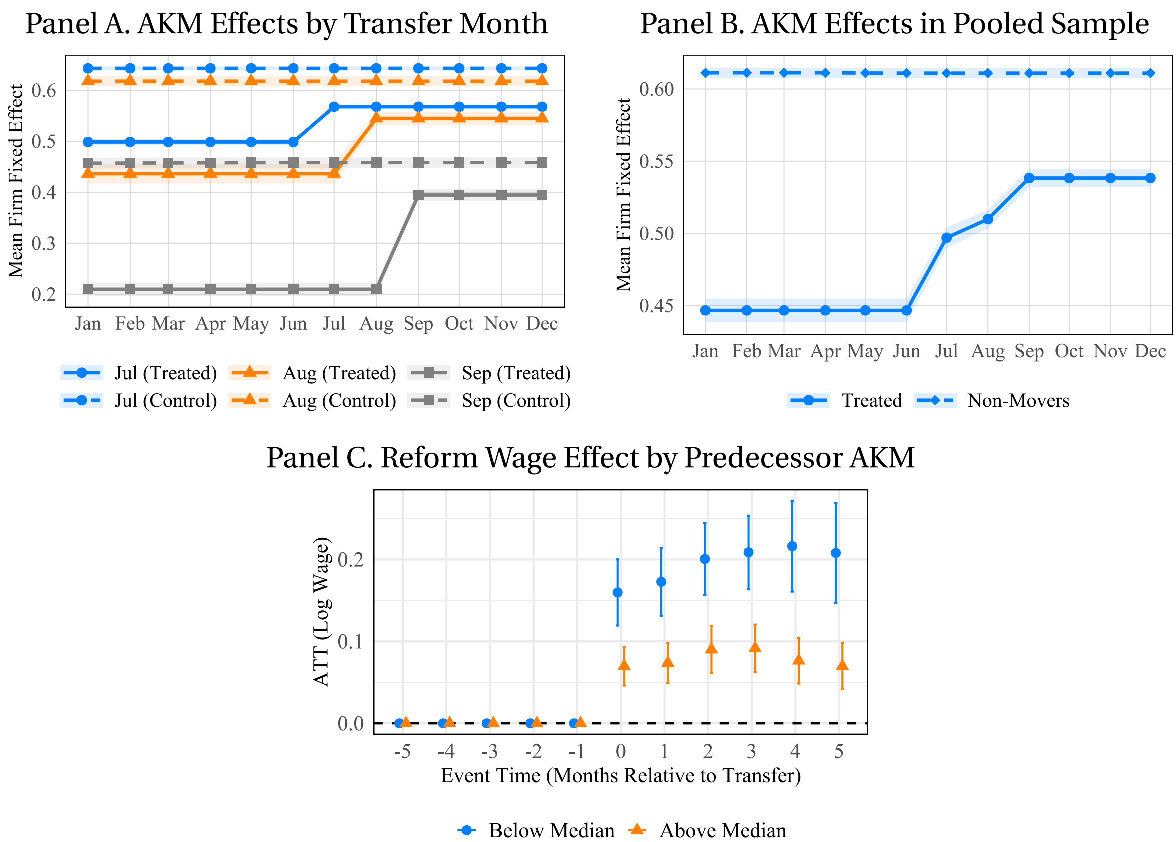
Following Abowd, Kramarz and Margolis (1999), we estimate a TWFE wage equation:

$$\ln w_{it} = \alpha_i + \psi_{J(i,t)} + \gamma_t + x_{it}\beta + \varepsilon_{it}, \tag{3}$$

where α_i is a worker fixed effect, $\psi_{J(i,t)}$ is a firm fixed effect for the firm J employing worker i in period t, γ_t is time fixed effects, and x_{it} is a vector of time-varying observables. For estimation, we pool the universe of formal workers in professional services and manufacturing from 2019 to 2023, which covers around 102.5 million observations. While the worker and firm fixed effects are separately identified only within connected sets of firms linked by worker mobility, this set covers around 99 percent of the observations in our matched sample of workers.

Below, we refer to the estimated firm fixed effect as the AKM effect, a measure of the wage premium paid by each establishment. As a first test of whether the reform's wage effects reflect gains in firm-specific rents, Figure 5 plots the mean AKM effect of the employing firm for treated workers. Panel A reports cohort-specific means, while Panel B reports the corresponding mean for the pooled sample of treated workers. To isolate shifts in means from composition effects, we restrict the sample used to compute these means to the subset of matched workers from our headline matching procedure who remained employed at the same post-move firm through December 2021 and who were continuously employed at the same pre-move firm since January 2021. We find evidence of increases in firm premia of 6.9, 10.8, and 18.5 percent for workers treated in July, August, and September, respectively, and an increase of 9.2 percent in the pooled sample. The magnitude of these increases is remarkably similar to that of the wage estimates above, suggesting that the reform-induced wage gains largely reflect workers moving to firms with higher wage premia.

Figure 5: Outsourcing Reform and Firm AKM Effects



Notes: Panel A shows the mean firm AKM effect for each cohort of treated workers. To isolate shifts in means from composition effects, we restrict the sample to the subset of matched workers from our headline matching procedure who remained employed at the same post-move firm through December 2021 and who were continuously employed at the same pre-move firm since January 2021. Panel B shows the mean firm AKM effect for the pooled sample of treated and matched control workers across all cohorts. Panel C presents the reform's effect on log wages, estimated as the equally weighted average across cohorts of the estimated cohort-specific average treatment effects on the treated (CATTs), separately for workers whose predecessor firm's AKM effect is below the cross-sectional median and those with a predecessor AKM effect above the median.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021.

If gains in firm rents indeed drive the wage gains for the outsourced workers, we would expect larger increases for clustered flows of workers whose predecessor firms had low AKM effects than for flows from firms with high AKM effects. To assess this possibility, Panel C reports the wage effects of the reform separately for clustered flows involving predecessor firms with AKM effects below the cross-sectional median and those with predecessor AKM effects above it. The wage effects for the former group are three times as large as those for the latter, amounting to 7 and 21 percent, respectively. Taken together, these findings provide further evidence that gains in firm-specific rents substantially explain the observed wage effects.[44]

6.6 Robustness Checks

We conduct several robustness checks to confirm our results on wages. First, because the order of matching could potentially introduce bias, we match all outsourced workers simultaneously and relax the requirement that control units remain employed past June 2021. The results, reported in Appendix R, closely resemble our headline estimates.

Second, differential selection into employment after the reform could mechanically affect mean wages. For example, the temporary reduction in unemployment risk documented above could raise average wages among treated workers if workers whose jobs were preserved tended to earn higher wages or if control workers who lost their jobs earlier tended to be lower-wage workers. To assess this possibility, in Appendix S, we follow Goldschmidt and Schmieder (2017) and estimate the reform's impacts on a series of balanced panels of workers. Specifically, we restrict the sample used in estimation to the subset of matched workers from our headline matching procedure who remain employed at the successor firm throughout each post-reform horizon. These estimates closely track our baseline results.

Third, in Appendix T, we implement three alternative strategies to identify staffing firms in the IMSS data and again obtain similar effects on employment and wages. The first strategy classifies staffing firms as establishments registered in the IMSS's professional services category that permanently exited the market in July 2021. The second strategy exploits the universal registry of specialized service providers created by the reform. Under the relevant provision, firms wishing to continue operating after the reform were required to apply for registration and demonstrate that they provided specialized services (e.g., cleaning, catering, security, or gardening) rather than staffing services. Application rejections indicate that the firm had been providing staffing services, and the reform required that the firm's workers be transferred to their actual employer. Accordingly, we identify staffing firms indirectly as those with applications rejected because the firms provided staffing rather than specialized services. The third

[44] In Appendix Q, we show that the reform did not have a detectable impact on firm AKM effects. Since these capture firm-level wage premia, a decline would suggest that integrating previously outsourced workers reduced compensation for incumbent workers. We find no evidence of such an effect.

strategy is a mix of the first two. All three strategies yield results in the same direction as our headline estimates.

7 Theoretical Model

Our findings pose a puzzle for standard theoretical frameworks. First, the outsourcing ban generated wage increases for previously outsourced workers, yet left employment unchanged. This finding is inconsistent with the theory of competitive labor markets, which predicts that employment falls when wages rise, and with the classical view of monopsony, which predicts employment *rises* when wages approach marginal productivity. Second, despite the mandate that compensation packages in the formal sector be uniform, total labor costs remained constant. Firms paid workers more while spending the same, which reveals that reported labor costs under outsourcing exceeded actual compensation.

We explain both facts using a model of the type proposed by Kline (2025), augmented to incorporate an outsourcing decision motivated by tax and benefit evasion. We describe the model in detail in Appendix U, but here we provide an overview of why outsourcing increases markdowns in this framework, as well as the key mechanisms that allow it to account for both empirical findings.

We model firms that differ in productivity and workers that differ in their outside option. There is a continuous distribution of firms, where a firm of type $p > \underline{p}$ can hire workers to perform a task that generates value added p, either directly or through outsourcing. Workers search for jobs and differ only in their unobserved outside option b, which is continuously distributed with minimum $\underline{b}$. Search frictions and the distribution of outside options together imply an isoelastic labor supply function to the firm under direct hiring, or to the staffing firm under outsourcing, with inverse elasticity β in surplus compensation over the minimum outside option, $\underline{b}$.

Firms operate in a tax environment with mandatory benefits, profit sharing, corporate profit taxation, and a VAT. Specifically, corporate profits are taxed at rate τ_π, and firms remit VAT on their sales while crediting VAT on production inputs. Workers receive mandatory social security benefits, $B(w)$, as a function of the wage w, and firms must pay a payroll tax $\tau_\ell(w)$ to finance those benefits. Firms must also distribute a fraction δ of annual profits to workers. Outsourcing can reduce the employing firm's total labor costs by lowering these tax and benefit burdens, since the staffing firm can evade these obligations. In particular, through outsourcing, the employing firm shifts an amount e ("evasion") of corporate profits to the staffing firm, which pays no taxes, remits a share $\gamma > \tau_\pi$ of those profits back to the firm, and keeps the remainder. Evasion e also reduces the employing firm's effective VAT burden: Because outsourced labor services are subject to VAT, inflating the invoiced amount increases the firm's creditable VAT and reduces its net VAT liability. In addition, the staffing firm pays benefits only on the minimum wage, $\underline{w}$, and strips workers of profit sharing.

7.1 Higher Markdowns Under Outsourcing

The first key empirical finding that the model reconciles is the presence of higher markdowns under outsourcing when evasion is a motive. Under direct hiring, the profit-maximizing regular earnings rule takes the form:

$$w_D^* + B(w_D^*) = \frac{\beta}{1+\beta} p + \frac{1}{1+\beta} \underline{b}, \tag{4}$$

where $B(w)$ denotes social security benefits, and $\underline{b}$ is the minimum outside option. This optimal wage condition is analogous to that implied by traditional rent-sharing models, with $\beta/(1+\beta)$ playing the role of the worker's bargaining weight and $\underline{b}$ the worker's outside option. In contrast, under outsourcing through a staffing agency, the firm pays a management fee κ and exploits tax evasion opportunities e. The optimal regular earnings become:

$$w_O^* + B(\underline{w}) = \frac{\beta}{1+\beta} \left[p - \kappa - e\left(1 - \frac{\gamma}{1-\tau_\pi}\right)\right] + \frac{1}{1+\beta} \underline{b}. \tag{5}$$

This expression shows that outsourcing depresses wages by diverting surplus away from workers to the staffing firm, creating a wedge between labor costs and worker remuneration while still offering a wage above the worker's outside option. In the language of the rent-sharing model, outsourcing does not reduce the worker's bargaining power but the surplus available to be shared, redirecting it from the worker–firm pair to the staffing company.

Furthermore, outsourcing eliminates workers' access to profit sharing, which further increases markdowns by reducing total compensation.

7.2 Improvement in Outside Options

The second key empirical finding that the model reconciles is the absence of a significant employment response to the outsourcing ban. Wages rise when outsourcing is eliminated, but the central insight of the model is that an economy-wide reform improves *everyone's* outside option through the imposition of a minimum compensation package in the formal sector. This offsets the employment gains that any individual firm would experience if the wage increase were unique to that firm.

This general equilibrium effect operates through $\underline{b}$. When minimum wages bind, the pre-ban minimum outside option is $\underline{b} = \underline{p}$, the lowest productivity level in the support of the productivity distribution. Post-ban, it becomes $\underline{b} = (1+\tau_\ell(\underline{w}))\underline{w}$, the minimum statutory compensation under direct hiring. This shift in $\underline{b}$ is *not* firm-specific, as the reform affects the entire distribution of reservation values.

In the model, which features a shifted power form for labor supply to the firm (i.e., $N(c) \propto (c - \underline{b})^\beta$, where c is total compensation), the number of matches depends on total compensation *minus* $\underline{b}$. Raising $\underline{b}$ from $\underline{p}$ to $(1+\tau_\ell(\underline{w}))\underline{w}$ mechanically reduces the attractiveness of the firm. At transitioning firms, wages rise but the employment response is muted because the improvements to outside options offset the wage increase in the supply equation. Markdowns are compressed since the wedge $\kappa + e(1-\gamma/(1-\tau_\pi))$ disappears, yet aggregate employment remains flat because universal improvements in $\underline{b}$ dampen labor supply elasticities everywhere.

This channel is unavailable in the classical monopsony setting, where employment responds mechanically to wage-setting power, and in the standard rent-sharing model, which treats outside options as exogenous.

7.3 Labor Cost Inflation and Tax Evasion

The third result reconciled by the model is that, while wages rose, total labor costs remained unchanged. Coupled with the absence of any other change in real outcomes, this pattern suggests systematic cost inflation under outsourcing. The model therefore does not merely show that evasion affects markdowns and employment; it also clarifies *how* outsourcing facilitates tax evasion and why the ban closes this channel without generating adverse real effects.

Specifically, we note that firms' total payments to staffing companies, $F \equiv (1+VAT)(w+\tau_\ell(\underline{w})\underline{w}+\kappa+e)$, are treated as intermediate inputs generating creditable VAT. However, staffing companies bear legal responsibility for VAT, which creates organizational separation that enables noncompliance: In contexts of weak state capacity, staffing companies can use well-known evasion methods (see Carrillo et al., 2023) to avoid remitting VAT on e, retaining $(1-\gamma)e$ for evasion costs while redistributing γe to the firm.

This creates powerful incentives to inflate e. Higher reported labor costs simultaneously (i) increase creditable VAT,[45] (ii) reduce taxable profits, and (iii) lower social security and profit-sharing payments—all without reducing employment, since monopsony power insulates firms from competitive forces. The ban eliminates this incentive since, as shown in Equation (4), direct-hire wages depend only on p and $\underline{b}$, with no scope for artificial inflation via e. Reported labor costs remain constant even as wages rise because the firm's ability to introduce a wedge between reported labor costs and actual labor compensation vanishes.

7.4 Comparison with Alternatives

Additionally, in Appendix V, we compare the predictions of our model with those of the classical monopsony and rent-sharing frameworks. We develop a common economic environment for both theories in Appendix V.1. In Appendix V.2, we show that in the classical monopsony model, where the firm is modeled as a block within a GE framework (e.g., Berger, Herkenhoff and Mongey, 2022*a*), staffing can be modeled as a cartel of firms that pools the individual firms' market power, lowering the Frisch elasticity of labor supply faced by each firm. In Appendix V.3, we show that in the rent-sharing framework (e.g., Card, Devicienti and Maida, 2014; Card et al., 2018), where wages are determined by Nash bargaining, outsourcing can be modeled as a reduction in worker bargaining power. While these models share many of the predictions of

[45] Using data from the initial annual manufacturing survey (discontinued in 2021), Appendix F Figure F.23 documents an increase in net VAT remitted (debited minus credited) among exposed establishments following the reform, consistent with increased fiscal compliance.

our own, neither jointly explains both puzzles: classical monopsony predicts that employment rises when markdowns fall, and rent-sharing treats outside options as exogenous, missing the GE channel.

8 Conclusion

This paper examined the causal impact of a reform prohibiting domestic outsourcing on a broad set of firm and worker outcomes, including employment, labor costs, wages, markdowns, output, productivity, input substitution, capital investment, market exit, local firm dynamics, unemployment, and job-to-job separations. Using a DID strategy that combines cross-sectional variation across establishments in exposure to the reform at baseline, as measured by whether the establishment outsourced workers, and a before–after reform comparison, we find that the reform increased total labor compensation by approximately 16 percent and raised wages for directly affected workers by about 7–10 percent, while leaving employment, flexibility, total labor costs, input use, output, productivity, and job-to-job separation rates unchanged. These effects imply substantial redistribution toward workers, reflected in a 3-percentage-point increase in the labor share and a 28-log-point reduction in markdowns.

We rationalize these findings with a model in which outsourcing allows firms to inflate their reported labor costs and shift compensation into noncontributory components, thereby lowering corporate, VAT, and social security liabilities while depressing workers' effective remuneration. By curbing this margin, the reform increased take-home pay and compressed markdowns without generating detectable real-side distortions.

The central policy implication is that labor regulation, particularly restrictions on domestic outsourcing, can improve worker outcomes by limiting firms' ability to exploit institutional and fiscal loopholes. This insight is especially relevant in light of the global expansion of temporary employment arrangements since the 1980s and the rise of firms reliant on gig employment (e.g., Uber). Although our analysis focuses on short- and medium-term effects, the evidence suggests that a strengthening of labor protections need not come at the cost of employment or firm performance. Understanding the long-run equilibrium adjustments to such policies remains an important direction for future research.

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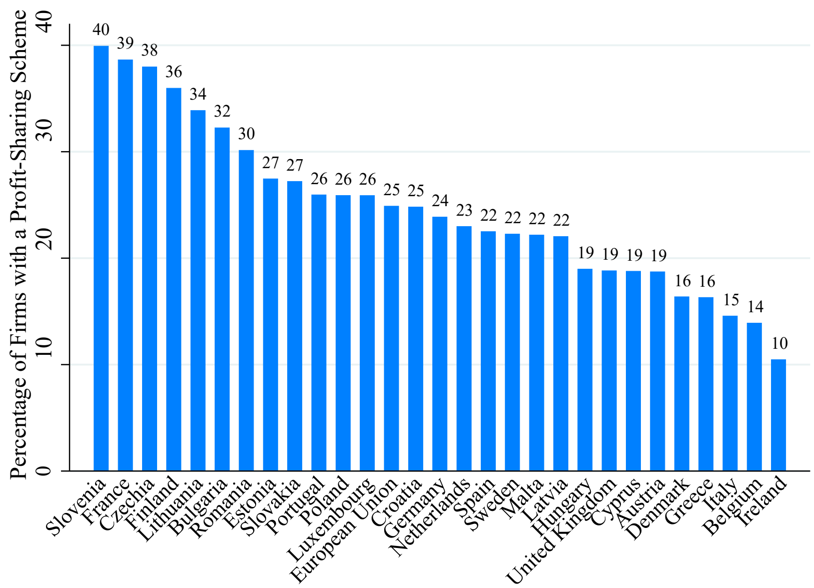
Supplementary Appendix: For Online Publication Only

A Key Supplementary Figures and Tables

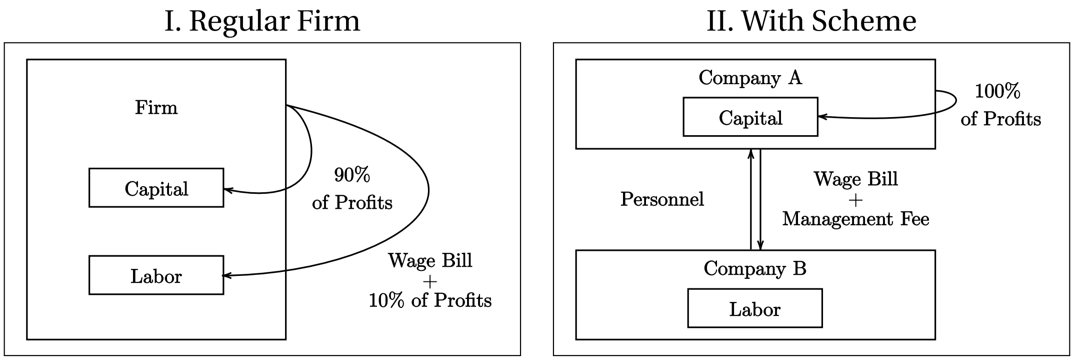
Figure A.1: Prevalence of Profit-Sharing Schemes in Advanced Economies, 2019



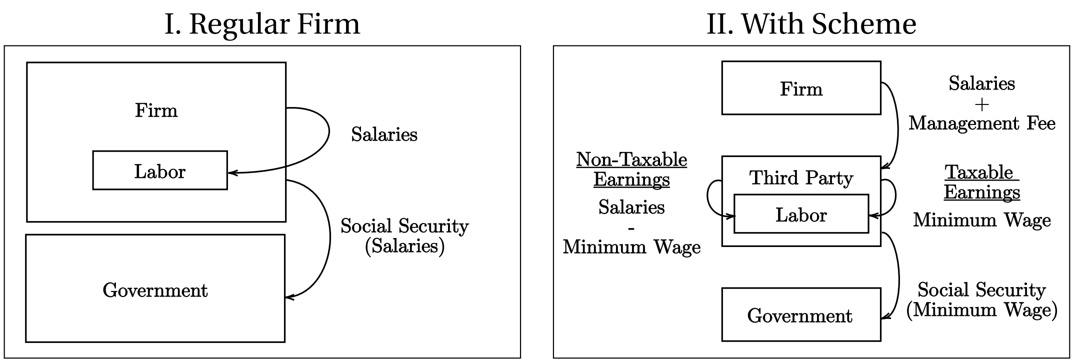
Notes: This figure reports the prevalence of profit-sharing schemes for companies in the European Union and the United Kingdom.
Source: Authors' elaboration using data from the European Company Survey, 2019.

Figure A.2: Diagrammatic Representation of Outsourcing Schemes

Panel A. Insourcing

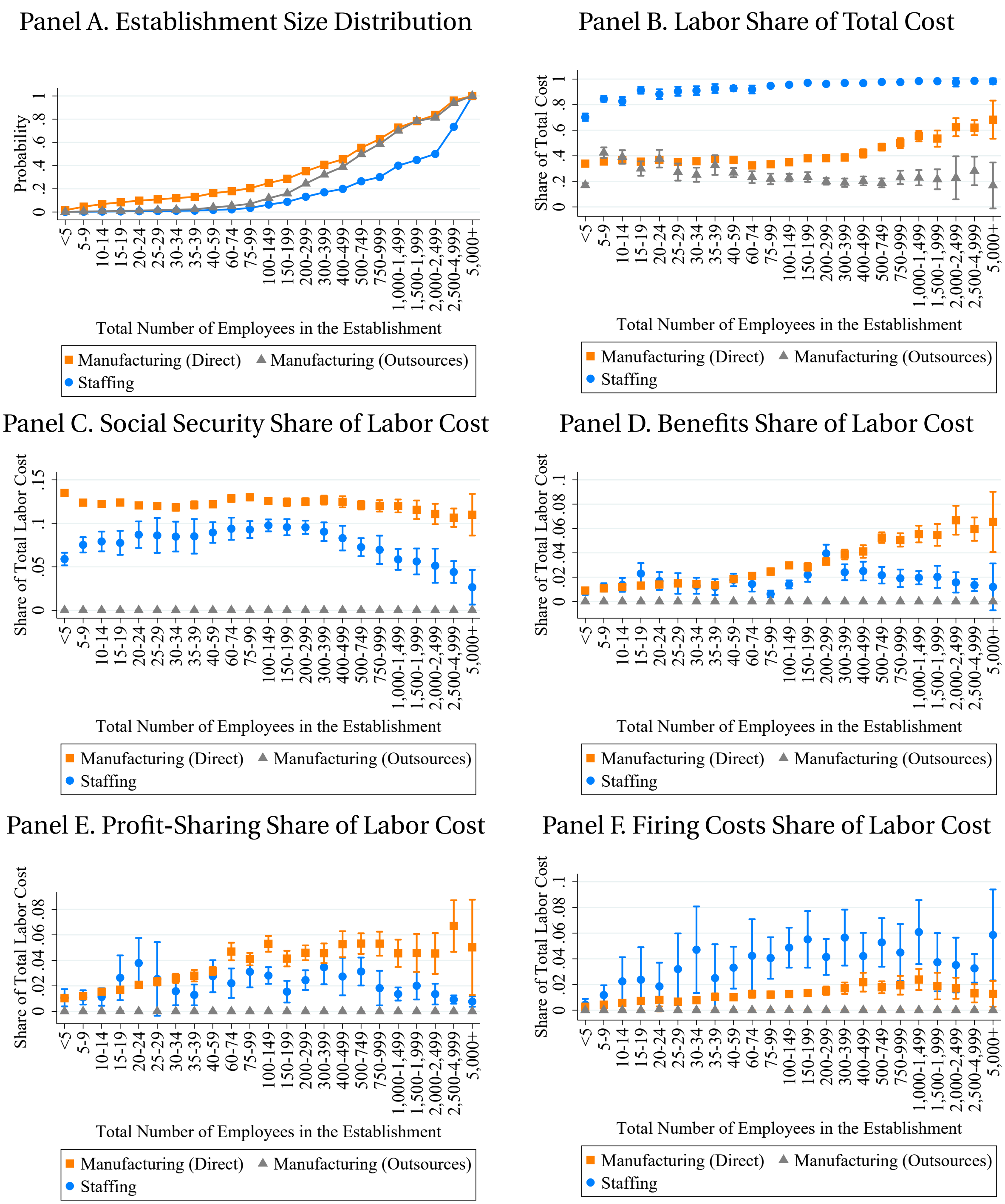


Panel B. Third-Party Outsourcing



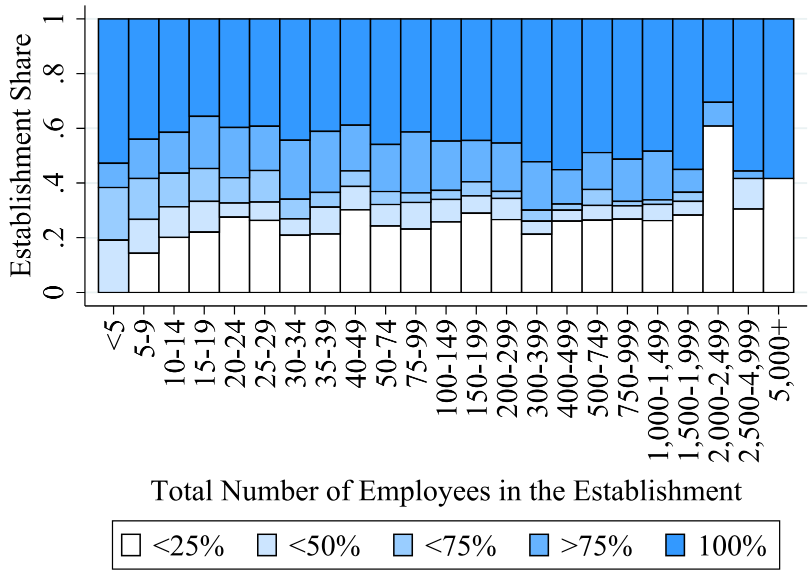
Notes: Each figure depicts a different legal arrangement. Boxes represent legal entities or productive inputs, while arrows represent financial flows or services rendered in exchange for payment. In Panel A, we illustrate the use of insourcing to lower profit-sharing payouts, so we omit social security payments from the depiction of the firm's legal arrangement. In Panel B, we illustrate the use of third-party outsourcing to lower a firm's social security payments (payroll taxes), so capital and profit sharing are omitted from the depiction of the firm.
Source: Authors' elaboration.

Figure A.3: Comparison of Staffing and Manufacturing Establishments, 2019



Notes: This figure compares the size distribution and cost structure of manufacturing establishments that hired all their workers directly, manufacturing establishments that outsourced all their workers, and staffing establishments. Staffing establishments are identified as those supplying nonspecialized workers (i.e., excluding gardening, catering, security, cleaning, and other specialized services) to other establishments.
Source: Authors' elaboration using data from the 2019 wave of the Mexican economic census.

Figure A.4: Distribution of Outsourced Employee Shares by Establishment Size, 2019



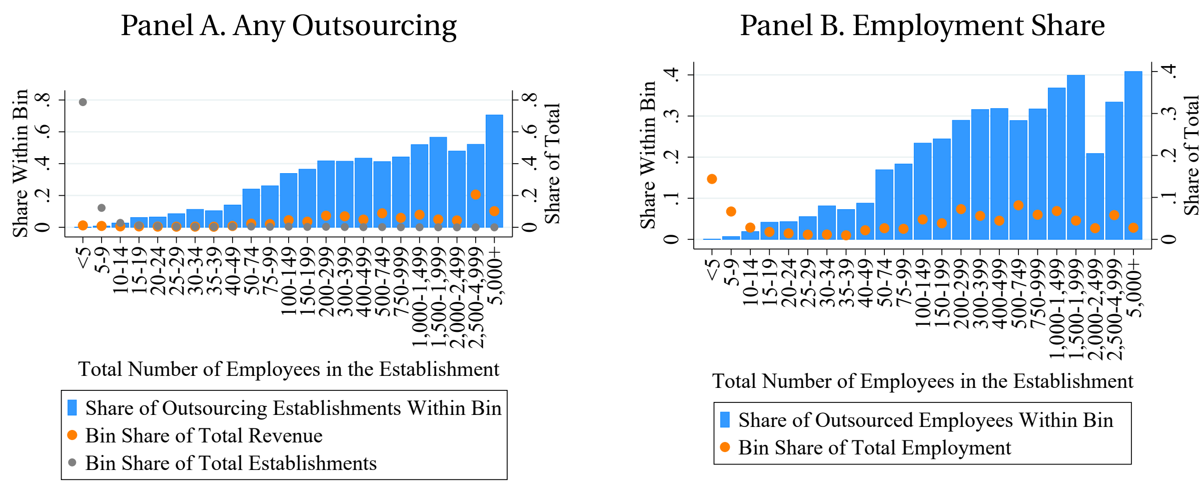
Notes: This figure presents the distribution of outsourced employee shares by establishment size, conditional on at least one employee being hired through outsourcing, for the universe of manufacturing establishments that keep employment and payroll accounts and report positive labor, capital, raw materials, and energy usage in 2019.
Source: Authors' elaboration using data from the Mexican economic census.

B Outsourcing Patterns by Firm Type

B.1 Outsourcing by Establishment Size and Revenue Growth

In this section, we present visual evidence that the prevalence of outsourcing is higher for large establishments than for small establishments. First, Figure B.1 shows that outsourcing increases with the total number of employees in the establishment. Specifically, Panel A shows that the share of establishments that employ at least one outsourced employee on their premises increases monotonically with total employment. Similarly, Panel B reports a positive gradient in the share of outsourced employees with total employment.

Figure B.1: Outsourcing Prevalence by Establishment Size, 2019



Notes: This figure presents the prevalence of outsourcing by establishment size for the universe of manufacturing establishments that keep employment and payroll accounts and report positive labor, capital, raw materials, and energy usage in 2019. Panel A reports the share of establishments that hire at least one of their employees through outsourcing by establishment size bin, as well as each bin's share of the total number of establishments and the bin's share of establishment revenue in the manufacturing sector. Panel B reports the share of outsourced employees by establishment size bin, as well as each bin's share of total employment in the manufacturing sector. *Source:* Authors' elaboration using data from the Mexican economic census.

B.2 Revenue Growth and Outsourcing

Since outsourcing shifts the burden of legal battles against workers to the staffing shell company, outsourced employment may respond more flexibly than direct hiring to idiosyncratic shocks in establishment revenue. To examine the response of outsourcing to idiosyncratic revenue shocks, we regress outsourcing on revenue at the establishment level for three alternative outsourcing measures, along with establishment fixed effects and year dummies. The first measure of outsourcing is a dummy indicating that the establishment hires at least one worker through outsourcing, which captures outsourcing on the extensive margin; the second measure is the inverse sine transformation of the number of outsourced workers, which captures outsourcing on the intensive margin; and the third measure is the outsourced share of total employment, which captures the adjustment of outsourced employment relative to that of directly hired employment. Table B.1 reports results from this exercise. Across the three measures, we find that outsourcing is higher for establishments experiencing positive revenue shocks, supporting the hypothesis that outsourcing enables employing establishments to flexibly adjust their labor costs. On average, a 1 percent shock to revenue increases the probability of outsourcing on the extensive margin by 0.6 percentage points (p=0.000), the number of outsourced workers by 3.2 percent (p=0.000), and the outsourced employment share by 0.3 percentage points (p=0.000). These results echo findings for the U.S. showing that outsourced employment responds faster to idiosyncratic productivity shocks than directly hired employment (Atencio De Leon, 2023).

Table B.1: Outsourcing and Revenue Shocks at the Establishment Level

Regressor	$\mathbb{1}_{\{\text{Outsourced Workers}>0\}}$	IHS(Outsourced Workers)	Employment Share of Outsourcing
	(1)	(2)	(3)
Log(Total Revenue)	0.006***	0.032***	0.003***
	(0.001)	(0.003)	(0.0005)
N	226,784	226,784	226,784
R^2	0.02	0.006	0.004

Notes: All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican economic census waves from 1994 to 2019.

C Estimation Details

C.1 Measurement of Output and Productive Inputs

We measure output and productive inputs following, to the extent possible, the standard procedures used in the U.S. (see Syverson, 2004; Kehrig, 2015). The paragraphs below provide details about the construction of our output and input measures.

Output. We construct a deflated measure of output for establishment i operating in industry j and period t, which captures the goods produced and sold in the same year and the produced goods stored in inventories, as follows:

$$Y_{it}^{j} = \frac{\text{Production}_{it}}{\text{Final Goods Price Deflator}_{t}^{j(i)}},$$

where $j(i)$ is a mapping from establishment to industry, Production_{it} is the value of the establishment's production, including the change in inventories from the beginning to the end of the calendar year, and $\text{Final Goods Price Deflator}_{t}^{j}$ is a price deflator for final goods at the 3-digit industry level from the Mexican producer price index, the *índice nacional de precios al productor*

(INPP). The base period for this price index is July 2019. Since, for some industries, this price index is not available for years prior to 2010, we roll back the industry-specific price indexes using broad sector (i.e., primary, secondary, and tertiary) price index growth rates. We follow a similar procedure to impute price index values elsewhere in our estimation of productive inputs.

Labor and Wages. To measure total labor input, we calculate the number of workers in the establishment. The census reports worker counts separately for three types of employment arrangements: directly hired workers who are remunerated, directly hired workers who are not paid, and outsourced workers. Importantly, all worker counts include only workers hired to work on the establishment's premises, excluding contractors and subcontractors performing tasks not part of the establishment's economic activities, such as security, cleaning, and gardening. We compute the establishment's total worker count as follows:

$$L_{it} = \mathrm{L}_{it}^{\text{Directly Hired \& Remunerated}} + \mathrm{L}_{it}^{\text{Outsourced}},$$

where L_{it}^{j} denotes the number of workers in category j in establishment i and year t.

Our labor input measure excludes directly hired workers who are not paid. Including these workers would lead to systematic bias in the estimated labor income shares, as labor compensation metrics in the census omit the labor income of the self-employed, counting it instead as capital income. The exclusion of nonpaid insourced workers from our labor input measure is essential because previous literature shows that rates of self-employment are greater in developing countries than in rich countries (e.g., Gollin, 2008). Since most directly hired workers who are not paid are also family members, the main caveat associated with excluding these workers is that our markdown measures do not account for labor exploitation among establishment owners and their family members.

Given our labor input definition, the labor compensation of establishment i in year t is

$$w_{it}L_{it} = \frac{\text{Total Workforce Compensation}_{it}}{\text{Intermediate Inputs Price Deflator}_{t}},$$

where Total Workforce Compensation$_{it}$ is the sum of the total labor compensation to directly hired workers and outsourced workers, and the term Intermediate Inputs Price Deflator$_t$ denotes the price deflator for intermediate inputs at time t within the INPP.

Capital. In the absence of reliable data on capital utilization rates for Mexico, we measure the capital stock as the unadjusted sum of all reported fixed assets owned by the establishment at the end of the period, which include buildings, machinery, vehicles, and computers. The capital stock of the establishment is therefore

$$K_{it} = \frac{\text{Fixed Assets Owned by the Establishment}_{it}}{\text{Capital Formation Price Deflator}_t},$$

where Capital Formation Price Deflator$_t$ is the INPP price deflator for capital at time t.

To measure capital expenditures, we simply multiply the capital stock by a rental rate of $r = 0.072$, which we obtain from Instituto Nacional de Estadística y Geografía (2022), as follows:

$$r_{it}K_{it} = \frac{\text{Fixed Assets Owned by the Establishment}_{it} \times 0.072}{\text{Capital Formation Price Deflator}_t}.$$

Materials and Energy. Mexican data sources separately report raw materials used in production and resales. To construct our material input measure, we first exclude resales because, by definition, resales are products bought and then resold without any change to the product. Then, we deflate raw materials using the same price deflator that we use for intermediate inputs. Therefore, our materials input measure is

$$p^M_{it}M_{it} = \frac{\text{Raw Materials Used in Production}_{it}}{\text{Intermediate Inputs Price Deflator}_t}.$$

Finally, to construct our energy input measure, we add up the establishment's properly deflated usage of fuels for production and electricity consumption, as follows:

$$p^E_{it}E_{it} = \frac{\text{Fuels Used in Production}_{it}}{\text{Fuels Price Deflator}_t} + \frac{\text{Electricity Consumption}_{it}}{\text{Electricity Price Deflator}_t},$$

where Fuels Price Deflator$_t$ and Electricity Price Deflator$_t$ are the INPP fuel and electricity price deflators for period t, respectively.

Cost Shares. The total cost of the establishment is calculated as

$$TC_{it} = w_{it}L_{it} + r_{it}K_{it} + p_{it}^{M}M_{it} + p_{it}^{E}E_{it}.$$

Hence, the input cost shares are calculated as

$$s_{it}^{L} = \frac{w_{it}L_{it}}{TC_{it}},$$

$$s_{it}^{K} = \frac{r_{it}K_{it}}{TC_{it}},$$

$$s_{it}^{M} = \frac{p_{it}^{M}M_{it}}{TC_{it}},$$

and

$$s_{it}^{E} = \frac{p_{it}^{E}E_{it}}{TC_{it}}.$$

Note that the ratio of any two cost shares is equal to the ratio of any two error-free revenue shares (e.g., $\hat{\alpha}_{it}^{M}/\hat{\alpha}_{it}^{L} = s_{it}^{M}/s_{it}^{L}$).

C.2 Production Function Estimation

The estimation of production functions is one of the oldest problems in econometrics. The key challenge for their empirical estimation is that firms optimally choose their inputs as a function of their productivity, which is unobservable to the econometrician. This simultaneity problem has been called *transmission bias* in the industrial organization literature, dating back to Marschak and Andrews (1944). As ignoring this source of endogeneity could lead to severe overestimation of output elasticities for flexible inputs relative to predetermined inputs, sophisticated methods have been devised to address this issue in the estimation of production functions. These include dynamic panel methods (Blundell and Bond, 2000), which propose using lagged first-differences and lagged levels of productive inputs as instruments for production function equations in levels, and "proxy" methods (Olley and Pakes, 1996; Levinsohn and Petrin, 2003; Wooldridge, 2009; Ackerberg, Caves and Frazer, 2015), which assume the existence of a flexible input with invertible demand in terms of productivity to "control" for productivity. These methods are particularly suitable in our context and will serve in the estimation of establishment-level markdowns.

We assume that logged output satisfies $y_{it} = \log(Q_{it}) + \varepsilon_{it}$, where ε_{it} denotes measurement error that enters the production estimate in a multiplicative fashion and satisfies $E[\varepsilon_{it}|Q_{it}] = 0$. This measurement error is assumed to be unobservable for the establishment. Furthermore, we assume that productivity is multiplicative in production, or $Q_{it} = \Omega_{it}F(L_{it}, K_{it}, M_{it}, E_{it})$.

Therefore, we can write

$$y_{it} = f(l_{it}, k_{it}, m_{it}, e_{it}) + \omega_{it} + \varepsilon_{it}, \tag{C1}$$

where $f(l_{it}, k_{it}, m_{it}, e_{it}) = \log(F(L_{it}, K_{it}, M_{it}, E_{it}))$ and l_{it}, k_{it}, m_{it}, e_{it}, and ω_{it} denote the log transformations of labor, capital, materials, energy, and productivity, respectively.

Crucially, productivity ω_{it} is observed by the establishment before it chooses its flexible inputs, but it is not observable to the econometrician. The so-called proxy method deals with this source of endogeneity by first assuming that the establishment's demand for raw materials is an invertible function of the period's productivity realization, or $m_{it} = m_t(\omega_{it}; l_{it}, k_{it}, e_{it})$. Under this assumption, there exists some function $h_t(; k_{it}, l_{it}, e_{it}) = m_t^{-1}(; k_{it}, l_{it}, e_{it})$ such that $\omega_{it} = h_t(m_{it}; k_{it}, l_{it}, e_{it})$.

This assumption is then supplemented with another assumption regarding the stochastic process that governs productivity. For our application, we assume that productivity ω_{it} is a Markovian stochastic process with a conditional expectation function denoted by $E[\omega_{it}|\omega_{i,t-1}] = g_t(\omega_{i,t-1})$, so we have

$$\omega_{it} = g_t(\omega_{i,t-1}) + \zeta_{it}, \tag{C2}$$

where ζ_{it} is period t's productivity innovation, satisfying $E[\zeta_{it}|\omega_{i,t-1}] = 0$.

Substituting Equation (C2) into Equation (C1), we have

$$y_{it} = f(l_{it}, k_{it}, m_{it}, e_{it}) + g_t(\omega_{i,t-1}) + \zeta_{it} + \varepsilon_{it}. \tag{C3}$$

Note that, by Equation (C2), $\omega_{i,t-1}$ is mean independent from period t's input choices, so the only problematic source of endogeneity in the estimation of f in Equation (C3) is the productivity innovation ζ_{it}. However, the usual timing assumptions made regarding the input choices by the establishment provide a natural instrumental variable (IV) strategy to circumvent this estimation hurdle. Namely, all flexible input choices from period $t-1$, the capital input choice in period t, their interactions, and their squares are mean independent from ζ_{it} by construction, or:

$$E[\zeta_{it} \times \mathbf{Z}_{it}] = \mathbf{0}, \tag{C4}$$

where $\mathbf{Z}_{it}$ contains all the elements in $(l_{i,t-1}, k_{it}, m_{i,t-1}, e_{i,t-1})$, their two-way interactions, and their squares.

The moments in Equation (C4) identify the production function parameters provided that the functional dependence of f on the productive input vector can be summarized with a sufficiently small number of parameters and that the candidate instruments meet the so-called relevance condition. This condition requires that the establishment's input choices be autocorrelated. A sufficient condition for this assumption to hold is for input prices to be persistent over time.

Having laid out the theoretical framework for identification, we describe in detail our three-stage estimation procedure, which follows directly from Ackerberg, Caves and Frazer (2015). For specificity, we assume that the production function is translog and can be reasonably approximated using a quadratic polynomial in $(l_{it}, k_{it}, m_{it}, e_{it})$ with a coefficient vector $\boldsymbol{\beta}$.

The first step in the estimation procedure leverages the fact that output can be written as a function of observables and measurement error, as follows:

$$\begin{aligned} y_{it} &= f(l_{it}, k_{it}, m_{it}, e_{it}; \boldsymbol{\beta}) + h_t(m_{it}; k_{it}, l_{it}, e_{it}) + \varepsilon_{it} \\ &= \phi_t(l_{it}, k_{it}, m_{it}, e_{it}) + \varepsilon_{it}, \end{aligned}$$

where $\phi_t(l_{it}, k_{it}, m_{it}, e_{it}) \equiv f(l_{it}, k_{it}, m_{it}, e_{it}; \boldsymbol{\beta}) + h_t(m_{it}; k_{it}, l_{it}, e_{it})$. We can estimate ϕ_t using a third-degree polynomial in $(l_{it}, k_{it}, m_{it}, e_{it})$. Let $\hat{\phi}_t$ denote the ordinary least squares (OLS) estimate of ϕ_t.

In the second step, for a hypothetical guess of $\boldsymbol{\beta}$, we construct estimates of ω_{it}, as follows:

$$\hat{\omega}_{it}(\boldsymbol{\beta}) = \hat{\phi}_t(l_{it}, k_{it}, m_{it}, e_{it}) - \mathbf{X}'_{it}\boldsymbol{\beta},$$

where $\mathbf{X}_{it}$ is a vector containing the terms of the quadratic polynomial in $(l_{it}, k_{it}, m_{it}, e_{it})$. Then, we regress $\hat{\omega}_{it}(\boldsymbol{\beta})$ on a cubic polynomial in $\hat{\omega}_{i,t-1}(\boldsymbol{\beta})$ with coefficient vector $\boldsymbol{\rho}$. The residuals from this regression are the implied values of ζ_{it}, denoted as $\hat{\zeta}_{it}(\boldsymbol{\beta})$.

In the final step, we then search over the $\boldsymbol{\beta}$ space using standard generalized method of moments (GMM) techniques to minimize the following moment conditions:

$$E\left[\zeta_{it}(\boldsymbol{\beta}) \times \mathbf{Z}_{it}\right] = 0. \tag{C5}$$

Our estimate of $\boldsymbol{\rho}$ is given by the coefficient estimate of an OLS regression of $\hat{\omega}_{it}(\boldsymbol{\beta})$ on a third-order polynomial of $\hat{\omega}_{it}(\boldsymbol{\beta})$, evaluated at the parameter vector estimate $\boldsymbol{\beta}$ that solves the GMM minimization problem.

The GMM estimator of the parameter vector $\boldsymbol{\beta}$, denoted by $\hat{\boldsymbol{\beta}}$, not only pins down the value of TFP but also enables the calculation of output elasticities. For example, under our assumption that the production function is translog, the output elasticities with respect to raw materials and employment depend on the establishment's input choices and are given by

$$\hat{\theta}^m_{it} = \hat{\beta}_m + 2\hat{\beta}_{mm} m_{it} + \hat{\beta}_{mk} k_{it} + \hat{\beta}_{me} e_{it} + \hat{\beta}_{ml} l_{it} \text{ and}$$

$$\hat{\theta}^l_{it} = \hat{\beta}_l + 2\hat{\beta}_{ll} l_{it} + \hat{\beta}_{lk} k_{it} + \hat{\beta}_{lm} m_{it} + \hat{\beta}_{le} e_{it}.$$

If instead we assume that the production function is Cobb–Douglas, output elasticities are constant and are given by

$$\hat{\theta}^m_{it} = \hat{\beta}_m \text{ and } \hat{\theta}^l_{it} = \hat{\beta}_l.$$

Thus, assuming a Cobb–Douglas production function amounts to assuming that output elasticities do not vary across establishments within the same industry.

Finally, Gandhi, Navarro and Rivers (2020) show that the moment conditions implied by the choice of instruments in the "proxy" method are insufficient for the identification of $\boldsymbol{\beta}$. This identification problem amounts to having insufficient information about the shape of the production function in the moment conditions implied by our IV strategy. As suggested in (Flynn, Traina and Gandhi, 2019), we resolve this issue by adding an assumption of constant returns to scale[46] to the moment conditions in Equation (C4) for estimation, as follows:

$$E\left[\sum_{I\in\{l,k,m,e\}} \frac{\partial f(l_{it},k_{it},m_{it},e_{it})}{\partial I_{it}}\right] - 1 = 0. \tag{C6}$$

C.3 Markdown Estimation Details

This section provides further details of our markdown estimation procedures. Section C.3.1 follows the standard cost minimization procedure to derive the formulas to construct markdowns using revenue elasticities and revenue shares. Section C.3.2 presents the procedure we use to estimate revenue shares from the data. Section C.3.3 describes an alternative markdown estimation approach, which we use as a robustness check.

C.3.1 Deriving an Expression for Markdowns

As described above, the labor markdown is identified by the ratio of the output elasticity of labor to its revenue share, divided by the establishment's markup. We begin our exposition by deriving the identifying equation for the establishment's markup and then show that the labor markdown is indeed identified as the ratio of the output elasticity of labor to its revenue share, divided by the markup.

We consider an active establishment i that produces output Q_{it} at time t and sells it in the market at a unitary price of P_{it}, using the production technology

$$Q_{it} = F(L_{it}, K_{it}, M_{it}, E_{it}; \Omega_{it}),$$

where L_{it}, K_{it}, M_{it}, E_{it}, and Ω_{it} denote labor, capital, materials, energy, and productivity, respectively. We assume that the production function F is continuous and twice differentiable with respect to its arguments. Furthermore, capital is assumed to be a predetermined input, meaning that it is chosen one period in advance, and a dynamic input, meaning that the optimal choice of capital depends on its previous values. On the other hand, the labor, materials, and energy used by the establishment are assumed to be flexible inputs, or inputs chosen

[46]This assumption seems to be a good approximation for the U.S. manufacturing sector (Basu and Fernald, 1997; Foster, Haltiwanger and Syverson, 2008; Syverson, 2004).

each period by the establishment after it observes its productivity realization, and static inputs, which satisfy static first-order conditions. Additionally, the establishment is assumed to have some level of power in the final good market and the markets for labor and energy, allowing it to influence prices, but it is assumed that the establishment has no market power in the capital and raw materials markets. Finally, we assume that the establishment faces a downward-sloping demand curve for its final good.

The establishment solves the following intratemporal cost minimization problem, conditional on its productivity realization and optimal output and capital choices:

$$\mathscr{C}(Q_{it}, K_{it}, w_{it}, r_{it}, p^M_{it}, p^E_{it}, \Omega_{it}) = \min_{\{L_{it}, M_{it}, E_{it}\}} w_{it}(L_{it})L_{it} + r_{it}K_{it} + p^M_{it}M_{it} + p^E_{it}(E_{it})E_{it}$$
$$\text{s.t. } Q_{it} = F(L_{it}, K_{it}, M_{it}, E_{it}; \Omega_{it}),$$

with the associated Lagrangian function

$$\mathscr{L}^{\min} = w_{it}(L_{it})L_{it} + r_{it}K_{it} + p^M_{it}M_{it} + p^E_{it}(E_{it})E_{it} + \lambda_{it}(Q_{it} - F(L_{it}, K_{it}, M_{it}, E_{it}; \Omega_{it})),$$

where w_{it}, r_{it}, p^M_{it}, and p^E_{it} denote the establishment's price for labor, capital, materials, and energy, respectively.

The first-order conditions of this cost minimization problem offer crucial insights for the identification of the establishment's markup, defined as the ratio of output price to marginal cost, or $\mu_{it} \equiv \frac{P_{it}}{\frac{\partial \mathscr{C}(Q_{it}, K_{it}, w_{it}, r_{it}, p^M_{it}, p^E_{it}, \Omega_{it})}{\partial Q_{it}}}$.[47] First, by the envelope theorem, we have that the Lagrangian multiplier is the marginal cost of production, or $\lambda_{it} = \frac{\partial \mathscr{C}(Q_{it}, K_{it}, w_{it}, r_{it}, p^M_{it}, p^E_{it}, \Omega_{it})}{\partial Q_{it}}$. Second, the first-order condition for raw materials is

$$\frac{\partial \mathscr{L}^{\min}}{\partial M_{it}} = p^M_{it} - \lambda_{it}\frac{\partial F(L_{it}, K_{it}, M_{it}, E_{it}; \Omega_{it})}{\partial M_{it}} = 0.$$

Rearranging terms in the last equality, multiplying both sides of the equation by $\frac{M_{it}}{Q_{it}}$, substituting the marginal cost of production for λ_{it}, and using the markup definition, we find that

[47] The ratio on the right-hand side of the equation is equal to 1 only when there is perfect competition and the establishment has no influence over the market price of output, and it is greater than 1 whenever there is imperfect competition and the establishment has price-setting power.

the markup of establishment i is identified by the ratio on the right-hand side of the following equation:

$$\mu_{it} = \frac{\theta_{it}^{M}}{\alpha_{it}^{M}}, \tag{C7}$$

where $\theta_{it}^{M} \equiv \frac{\partial \log F()}{\partial \log M_{it}}$ is the output elasticity with respect to raw materials and $\alpha_{it}^{M} \equiv \frac{p_{it}^{M} M_{it}}{P_{it} Q_{it}}$ is its revenue share.

We then derive an equation that identifies the labor markdown, defined as the ratio of the marginal revenue product of labor to the wage rate, or $\upsilon_{it} \equiv \frac{\frac{\partial P_{it} Q_{it}}{\partial L_{it}}}{w_{it}}$.[48] The first-order condition of the cost minimization problem with respect to labor is

$$\frac{\partial \mathscr{L}^{\min}}{\partial L_{it}} = w'_{it}(L_{it}) L_{it} + w_{it}(L_{it}) - \lambda_{it} \frac{\partial F(L_{it}, K_{it}, M_{it}, E_{it}; \Omega_{it})}{\partial L_{it}} = 0.$$

Rearranging terms, multiplying both sides of the equation by $\frac{L_{it}}{Q_{it}}$, and substituting the labor supply elasticity definition $\varepsilon_{L,w} \equiv \frac{\partial \log L_{it}}{\partial \log w_{it}}$ into the resulting equation, we obtain

$$\left(1 + \frac{1}{\varepsilon_{it}^{L,w}}\right) = \frac{\frac{\theta_{it}^{L}}{\alpha_{it}^{L}}}{\mu_{it}}. \tag{C8}$$

Thus, a sufficient condition for the desired result to hold is the equality of the establishment's labor markdown and the reciprocal of the labor supply elasticity. If the establishment is profit maximizing, this condition holds. To see why, consider the profit maximization problem of the establishment:

$$\Pi(w_{it}, r_{it}, p_{it}^{M}, p_{it}^{E}, \omega_{it}) = \max_{\{L_{it}, M_{it}, E_{it}\}} P_{it} Q_{it} - w_{it}(L_{it}) L_{it} - r_{it} K_{it} - p_{it}^{M} M_{it} - p_{it}^{E}(E_{it}) E_{it}$$
$$\text{s.t.} \quad Q_{it} = F(L_{it}, K_{it}, M_{it}, E_{it}; \Omega_{it}).$$

Since labor is assumed to be a flexible input, we have that the first-order condition of the

[48]The ratio on the right-hand side of the equation is equal to 1 when the marginal worker is paid exactly her marginal contribution to the revenues of the establishment, and it is greater than 1 when the worker is paid less than her marginal contribution to the establishment's revenues. Put differently, the reciprocal of the markdown is the fraction of the revenues generated by the marginal worker for which she is effectively paid.

profit maximization problem depends only on labor at t. Specifically, we have

$$\frac{\partial \mathscr{L}^{\max}}{\partial L_{it}} = \frac{\partial P_{it} Q_{it}}{\partial L_{it}} - w'_{it}(L_{it}) L_{it} - w_{it}(L_{it}) = 0,$$

where $\mathscr{L}^{\max}$ is the Lagrangian associated with the dynamic profit maximization problem.

Rearranging terms and substituting the definition of the labor supply elasticity into the resulting equation, we obtain

$$\frac{\frac{\partial P_{it} Q_{it}}{\partial L_{it}}}{w_{it}} = \left(1 + \frac{1}{\varepsilon^L_{it}}\right) = \upsilon_{it}, \tag{C9}$$

where the last equality follows from the markdown definition.

Substituting the last equality in Equation (C9) into Equation (C8), we finally obtain

$$\upsilon_{it} = \frac{\frac{\theta^L_{it}}{\alpha^L_{it}}}{\mu_{it}}. \tag{C10}$$

The right-hand side of this equation identifies the labor markdown, as Equation (C7) identifies the markup of the establishment. The labor markdown of the establishment can therefore be estimated as a ratio of ratios: the ratio of (1) the ratio of the output elasticity of labor to its revenue share to (2) the ratio of the output elasticity of raw materials to their revenue share.

C.3.2 Revenue Share Estimation

Computing the ratio on the right-hand side of Equation (C10) involves estimating the revenue shares of labor and raw materials. As in De Loecker and Warzynski (2012), we use the estimated residual from the first step in our estimation procedure for the production function described in Section C.2, denoted by $\hat{\varepsilon}_{it}$, to correct these shares for measurement error in the revenue measure. Specifically, since we observe only $Y_{it} \equiv Q_{it} \exp(\varepsilon_{it})$, we compute the error-free expenditure shares $\hat{\alpha}^M_{it} = \frac{p^M_{it} M_{it}}{P_{it} \frac{Y_{it}}{\exp\{\hat{\varepsilon}_{it}\}}}$ and $\hat{\alpha}^L_{it} = \frac{w_{it} L_{it}}{P_{it} \frac{Y_{it}}{\exp\{\hat{\varepsilon}_{it}\}}}$ for raw materials and labor, respectively. This correction isolates the revenue variation that correlates with the productive inputs $(l_{it}, m_{it}, k_{it}, e_{it})$ and removes all other sources of variation in revenues.

C.3.3 An Alternative Estimation Approach

Bond et al. (2021) highlights additional identification and estimation issues pertaining to the ratio estimator of the markup, which arise when the econometrician uses the revenue elasticity for a flexible input in place of its output elasticity, as in our case. In particular, if the establishment maximizes profits and minimizes production costs, the markup ratio estimator that relies on the revenue elasticity of the flexible input equals one and thus is uninformative about actual markups.

This result follows because, if an establishment with market power in the final good market maximizes profits, it internalizes the effect of its output choices on prices, so the markup equals one plus the reciprocal of the price elasticity of demand. Specifically, the ratio of the marginal revenue of raw materials, denoted by $\theta_{it}^{M,\text{revenues}}$, to their revenue share is

$$
\begin{aligned}
\frac{\theta_{it}^{M,\text{revenue}}}{\alpha_{it}^{M}} &= \frac{\frac{\partial p_{it}(Q_{it})Q_{it}}{\partial M_{it}} \frac{M_{it}}{p_{it}(Q_{it})Q_{it}}}{\alpha_{it}^{M}} \\
&= \frac{\left(\frac{\partial p_{it}(Q_{it})}{\partial Q_{it}} \frac{\partial Q_{it}}{\partial M_{it}} Q_{it} + \frac{\partial Q_{it}}{\partial M_{it}} P_{it}(Q_{it})\right) \frac{M_{it}}{P_{it}(Q_{it})Q_{it}}}{\alpha_{it}^{M}} \\
&= \frac{\theta_{it}^{M,\text{output}} \times \left(1 + \frac{1}{\epsilon_{it}^{P,Q}}\right)}{\alpha_{it}^{M}} \\
&= \mu_{it}\left(1 + \frac{1}{\epsilon_{it}^{P,Q}}\right) \\
&= 1,
\end{aligned}
$$

where $\theta_{it}^{M,\text{output}}$ is the output elasticity of raw materials and $\epsilon_{it}^{P,Q}$ is the price elasticity of demand. The last equality above follows from Lerner's monopoly pricing rule.

Fortunately, the ratio estimator for markdowns is immune to the criticism of Bond et al. (2021) because markdowns are estimated as a ratio of ratios. By a line of reasoning analogous to that for markups, we have that our markdown measure satisfies the following:

$$\frac{\frac{\theta_{it}^{L,\text{revenue}}}{\alpha_{it}^{L}}}{\frac{\theta_{it}^{M,\text{revenue}}}{\alpha_{it}^{M}}} = \frac{\frac{\theta_{it}^{L,\text{output}}\left(1+\frac{1}{\epsilon_{it}^{P,Q}}\right)}{\alpha_{it}^{L}}}{\mu_{it}\left(1+\frac{1}{\epsilon_{it}^{P,Q}}\right)} = \upsilon_{it},$$

where the last equality follows from Equation (C10).

However, while the ratio estimator for markdowns is immune to this criticism, as a robustness check, we follow the recommendation in Bond et al. (2021) and use the differences in revenue shares between groups of establishments within the same industry to infer how markdowns differ across groups.

For simplicity, we assume that all establishments within an industry have the same Cobb–Douglas production function. Taking logs in Equation (C10), we have $\log\upsilon_{it} = \log\theta_{it}^{L} - \log\alpha_{it}^{L} - \mu_{it}$. Substituting Equation (C7) into this equation and rearranging terms, we have

$$\log\left(\frac{\alpha_{it}^{M}}{\alpha_{it}^{L}}\right) = \log\left(\frac{\beta^{M}}{\beta^{L}}\right) + \log\upsilon_{it}, \tag{C11}$$

where the input elasticities $\beta^{M} = \theta_{it}^{M}$ and $\beta^{L} = \theta_{it}^{L}$ are constant terms.

To study whether a binary characteristic of the establishment D_{it} impacts markdowns, we can specify a linear relationship between log markdowns and this characteristic, as follows:

$$\log\upsilon_{it} = \delta_0 + D_{it}\delta_1 + \eta_{it}, \tag{C12}$$

where $E[\eta_{it}|D_{it}] = 0$.

Substituting Equation (C11) into Equation (C12), we have the linear specification

$$\log\left(\frac{\alpha_{it}^{M}}{\alpha_{it}^{L}}\right) = \delta_0 + \log\left(\frac{\beta^{M}}{\beta^{L}}\right) + D_{it}\delta_1 + \eta_{it}. \tag{C13}$$

From this equation, we can learn about the association between log markdowns and the binary variable D_{it}. Thus, we estimate Equation (C13) via OLS for each characteristic of interest.

C.4 Methodology for the Labor Cost Decomposition

We define the total labor cost of the establishment as follows:

$$\text{Total Labor Cost}_{it} = \text{Wage Bill}^{D}_{it} + \text{Wage Bill}^{O}_{it} + \text{Staffing Fee}^{O}_{it} + \text{Firing Costs}_{it}, \tag{C14}$$

where $\text{Wage Bill}^{D}_{it}$ denotes the wage bill of directly hired employees in establishment i at time t, $\text{Wage Bill}^{O}_{it}$ denotes the wage bill of outsourced employees, $\text{Staffing Fee}^{O}_{it}$ denotes the payment made by i to the staffing company that hires its workers in return for the staffing firm's services, and Firing Costs_{it} denotes severance payments, litigation, and other costs associated with terminating workers. In turn, the wage bill for each type of worker $j \in \{D, O\}$ is the sum of four components

$$\text{Wage Bill}^{j}_{it} = \text{Salaries}^{j}_{it} + \text{Benefits}^{j}_{it} + \text{Social Security}^{j}_{it} + \text{Profit Sharing}^{j}_{it}. \tag{C15}$$

While we observe total firing costs and the wage bill components for directly hired workers in the establishment-level data, we do not observe the components of the wage bill for outsourced workers, as the determination of these payments corresponds to the staffing company. Neither do we observe the fee paid to the staffing firm for managing outsourced workers. Therefore, we impute their values following a two-step procedure that relies on the total payment made by i to the staffing company, $\text{Total Payment}_{it}$, which we observe directly in the establishment-level data.

First, we impute the wage bill of outsourced employees and the management fee using the employment-weighted mean revenue share of labor across all staffing establishments in the census data, denoted by $\bar{s}_{L}^{\text{Staffing}}$, as follows:

$$\widehat{\text{Wage Bill}}^{O}_{it} = \bar{s}_{L}^{\text{Staffing}} \text{Total Payment}_{it}, \text{ and} \tag{C16}$$

$$\widehat{\text{Staffing Fee}}^{O}_{it} = (1 - \bar{s}_{L}^{\text{Staffing}}) \text{Total Payment}_{it}. \tag{C17}$$

Second, using the employment-weighted mean of the wage bill shares for the 4 wage components from the census data across all staffing establishments, we impute salaries, benefits, social security payments, and profit sharing for outsourced workers. For example, we impute salaries as follows:

$$\text{Salaries}_{it}^{O} = \bar{s}_{\text{Salaries}}^{\text{Staffing}} \widehat{\text{Wage Bill}}_{it}^{O}, \tag{C18}$$

where $\bar{s}_{\text{Salaries}}^{\text{Staffing}}$ is the employment-weighted mean of the wage bill share of salaries across all staffing establishments in the census data.

Finally, our imputation of the wage bill for outsourced workers also enables the construction of the average wage and the labor share of the total cost at the establishment level, defined as

$$\widehat{\text{Average Wage}}_{it} = \frac{\text{Wage Bill}_{it}^{D} + \widehat{\text{Wage Bill}}_{it}^{O}}{\text{L}_{it}^{D} + \text{L}_{it}^{O}}, \text{ and} \tag{C19}$$

$$\widehat{s}_{it}^{L} = \frac{\text{Wage Bill}_{it}^{D} + \widehat{\text{Wage Bill}}_{it}^{O}}{\text{Total Cost}_{it}}. \tag{C20}$$

D Markdown Estimation Results

D.1 Key Descriptive Statistics

Table D.1: Summary Statistics of the Establishment-Level Labor Markdown Distribution

Census Wave	Mean	Median	Standard Deviation	Interquartile Range	Observations
	(1)	(2)	(3)	(4)	(5)
1999	1.8	1.54	1.16	1.53	28,624
2004	1.47	1.21	0.98	1.13	40,718
2009	1.37	1.09	0.97	1.09	44,077
2014	1.4	1.13	0.97	1.07	48,336
2019	1.5	1.21	1.05	1.23	68,430
Total	1.49	1.2	1.03	1.2	230,185

Notes: This table presents a selected set of summary statistics of the labor markdown distribution for the universe of manufacturing establishments in the economic census. We estimate markdowns assuming that the production function is translog with parameters that vary at the 3-digit industry level. The dashed horizontal line between 1999 and 2004 marks a change in the economic census questionnaire occurring in 2004. Statistics for 1994 are not shown because markdown estimation requires lagged data and our dataset begins in that year.
Source: Authors' elaboration using data from the Mexican economic census from 1994 to 2019.

Table D.2: Average Labor Markdown by Industry and Census Wave

Industry	1999	2004	2009	2014	2019
	(1)	(2)	(3)	(4)	(5)
Transportation equipment	1.97	1.7	2.02	1.83	1.92
	(0.06)	(0.05)	(0.06)	(0.06)	(0.06)
Machinery	1.91	1.67	1.61	1.67	1.89
	(0.07)	(0.05)	(0.06)	(0.07)	(0.06)
Food	2.16	1.68	1.56	1.66	1.74
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Chemical	1.7	1.63	1.73	1.64	1.67
	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)
Nonmetallic mineral products	1.39	1.21	1.16	1.22	1.58
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Petroleum and coal products	1.62	1.64	1.7	1.6	1.54
	(0.07)	(0.05)	(0.07)	(0.08)	(0.09)
Miscellaneous	1.6	1.43	1.37	1.23	1.5
	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)
Plastics and rubber products	1.56	1.37	1.52	1.35	1.49
	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)
Electrical equipment, appliances, and components	1.75	1.57	1.7	1.4	1.47
	(0.06)	(0.05)	(0.06)	(0.05)	(0.05)
Fabricated metal products	1.48	1.32	1.19	1.15	1.34
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Paper	1.2	1.18	1.22	1.18	1.23
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Apparel	1.41	1.36	1.19	1.21	1.19
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Primary metal	1.22	1.21	1.01	1.01	1.18
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Wood products	1.15	1.08	0.98	0.93	1.05
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Leather and allied products	1.02	1.16	0.84	0.89	0.97
	(0.12)	(0.13)	(0.08)	(0.06)	(0.07)
Printing and related support activities	0.9	0.86	0.8	0.75	0.94
	(0.06)	(0.01)	(0.01)	(0.01)	(0.01)
Beverage and tobacco products	0.9	0.89	0.83	0.86	0.74
	(0.01)	(0.01)	(0.004)	(0.004)	(0.003)
Computer and electronic products	0.59	0.52	0.55	0.51	0.62
	(0.05)	(0.04)	(0.05)	(0.03)	(0.04)
Furniture and related products	0.48	0.46	0.46	0.43	0.5
	(0.01)	(0.01)	(0.01)	(0.01)	(0.004)
Total	1.8	1.47	1.37	1.4	1.5
	(0.007)	(0.005)	(0.005)	(0.004)	(0.004)

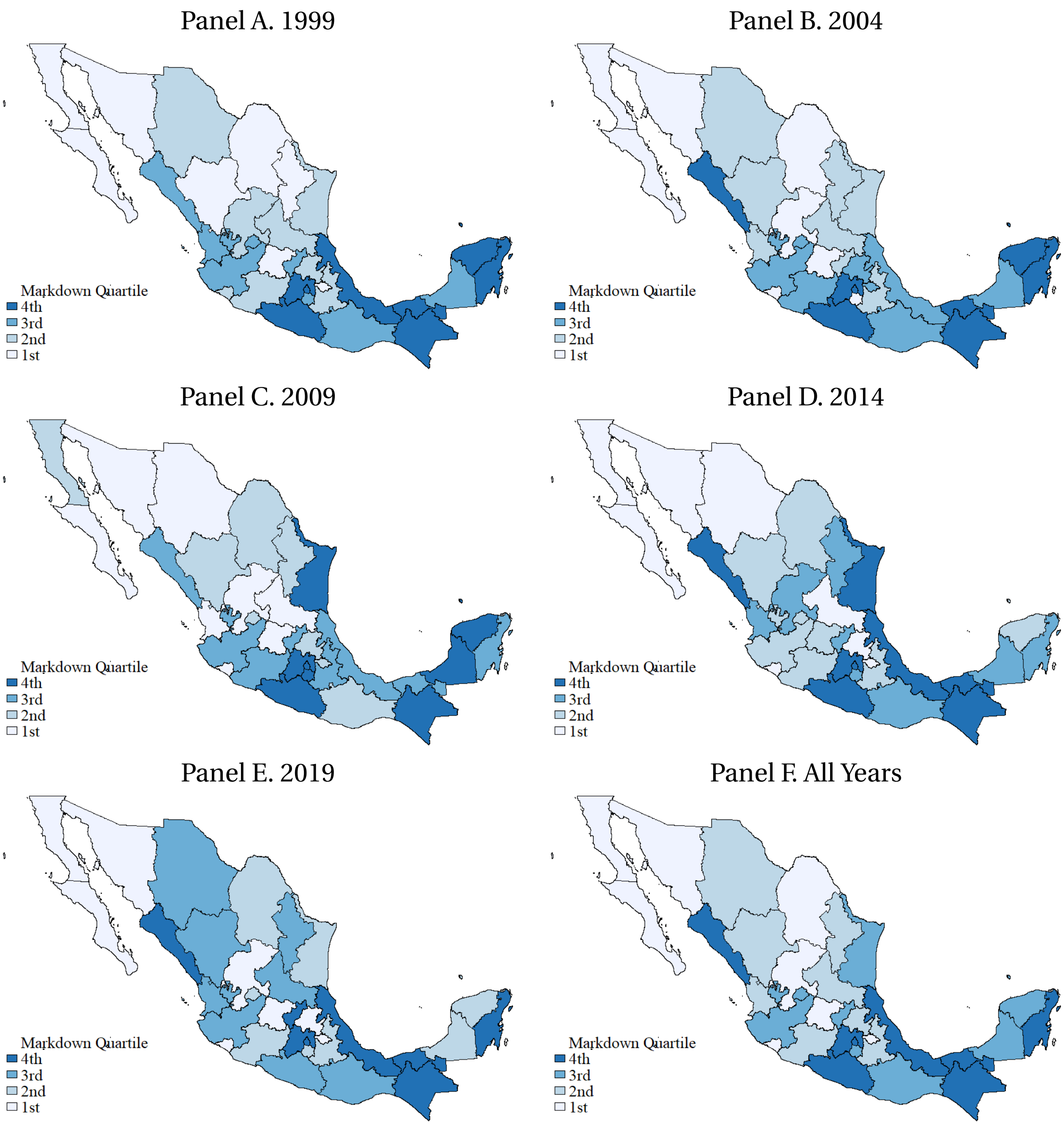
Notes: We estimate markdowns assuming that the production function is translog with parameters that vary at the industry-group level. Industry groups are defined by 3-digit 1997 North American Industrial Classification System (NAICS) codes for manufacturing industries. Industries are sorted in descending order according to their average markdown in 2019. Standard errors are in parentheses.
Source: Authors' elaboration using data from the Mexican economic census.

Table D.3: Average Labor Markdown by Country Region and Census Wave

Region	1999	2004	2009	2014	2019	Total
	(1)	(2)	(3)	(4)	(5)	(6)
Central	1.89	1.55	1.44	1.49	1.57	1.57
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
South	1.9	1.56	1.43	1.46	1.55	1.54
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
North	1.67	1.36	1.3	1.33	1.45	1.41
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Bajío	1.75	1.41	1.31	1.32	1.44	1.42
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Total	1.8	1.47	1.37	1.4	1.5	1.49
	(0.007)	(0.005)	(0.005)	(0.004)	(0.004)	(0.002)

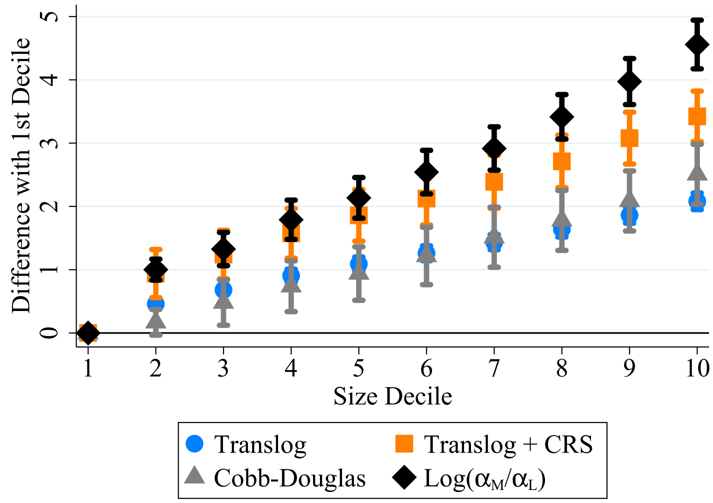
Notes: The North region includes Baja California, Baja California Sur, Coahuila, Chihuahua, Durango, Nuevo León, Sinaloa, Sonora, and Tamaulipas. The Bajío region includes Aguascalientes, Colima, Guanajuato, Jalisco, Michoacán, Nayarit, Querétaro, San Luis Potosí, and Zacatecas. The Center region includes Mexico City, Hidalgo, Estado de México, Morelos, Puebla, Tlaxcala, and Veracruz. The South region includes Campeche, Chiapas, Guerrero, Oaxaca, Quintana Roo, Tabasco, and Yucatán. Regions are ranked according to their average labor markdown in 2019. We estimate markdowns assuming that the production function is translog with parameters that vary at the 3-digit industry level. Industry groups are defined by 3-digit 1997 North American Industrial Classification System (NAICS) codes for manufacturing industries. Standard errors are in parentheses.
Source: Authors' elaboration using data from the Mexican economic census.

Figure D.1: Average Markdown in Manufacturing by State and Census Wave



Notes: Each shade in the figure denotes a different quartile of the average markdown distribution, with lighter shades representing lower quartiles and darker shades representing higher quartiles. Quartiles in Panels A through E are taken with respect to the cross-sectional distribution of average markdowns at the state level by year, whereas Panel F depicts quartiles with respect to the distribution of average markdowns taken over all establishments and years at the state level. We estimate markdowns assuming that the production function is translog with parameters that vary at the 3-digit 1997 North American Industrial Classification System (NAICS) industry code level.
Source: Authors' elaboration using data from the Mexican economic census.

Figure D.2: Markdown Gradient with Establishment Size



Notes: This figure reports the coefficients and 95 percent confidence intervals of establishment size decile dummies, where the deciles are taken with respect to the national distribution of the establishments' shares of total revenue in their respective local labor markets, in a regression of wage markdowns on these dummies, local labor market fixed effects, and year indicators. Each marker type represents a different markdown measure. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the market level. Markets are 3-digit NAICS industry codes × metropolitan area/municipality pairs. The reference group for the coefficient estimates are the establishments in the first size bin. The regression pools data from the economic census waves from 1999 to 2019. N=230,185.
Source: Authors' elaboration using data from the Mexican economic census.

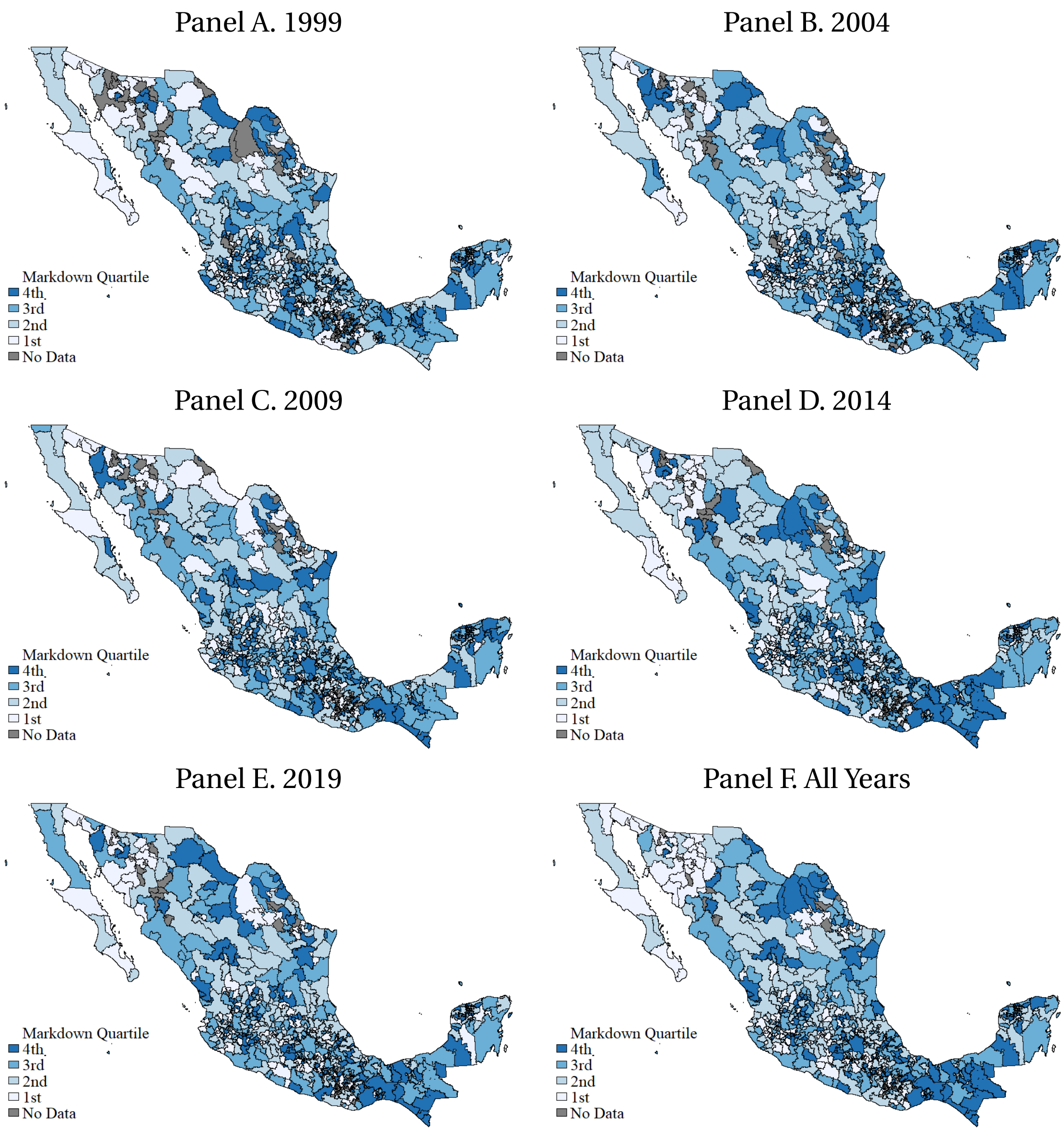
Table D.4: Markdowns and Outsourcing
Outcome Variable: Establishment-Level Markdowns

Regressor	Translog	Translog + CRS	Cobb–Douglas	$\text{Log}\left(\frac{\alpha^M}{\alpha^L}\right)$
	(1)	(2)	(3)	(4)
Share of Outsourced Employees	0.34***	0.30***	1.39***	1.53***
	(0.04)	(0.05)	(0.12)	(0.15)
N	230,185	230,185	230,185	230,185
R^2	0.0818	0.0843	0.0486	0.139

Notes: All regressions include a control for the log employment count, establishment fixed effects, and year dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. Markets are 3-digit North American Industrial Classification System (NAICS) industry codes × metropolitan area/municipality pairs. ***p<0.01.
Source: Authors' elaboration using data from the Mexican economic census waves from 1994 to 2019.

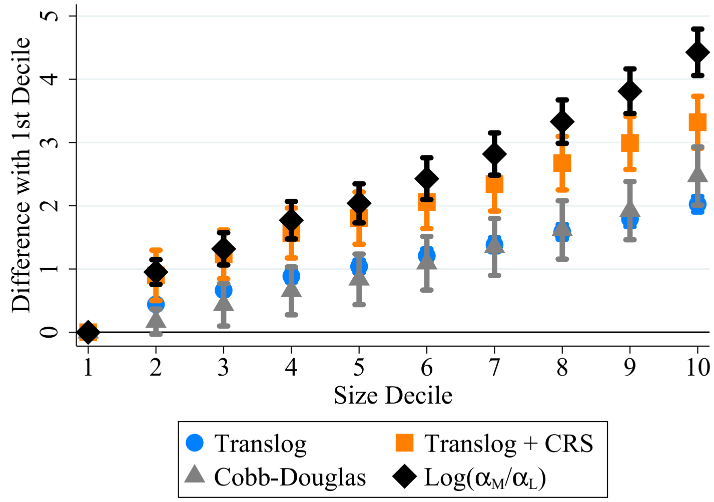
D.2 Commuting Zones as Local Labor Market Definition

Figure D.3: Average Markdown in Manufacturing by Commuting Zone and Census Wave



Notes: Each shade in the figure denotes a different quartile of the average markdown distribution, with lighter shades representing lower quartiles and darker shades representing higher quartiles. Quartiles in Panels A through E are taken with respect to the cross-sectional distribution of average markdowns at the commuting zone level by year, whereas Panel F depicts quartiles with respect to the distribution of average markdowns taken over all establishments and years at the commuting zone level. We estimate markdowns assuming that the production function is translog with parameters that vary at the 3-digit 1997 North American Industrial Classification System (NAICS) industry code level.
Source: Authors' elaboration using data from the Mexican economic census.

Figure D.4: Commuting Zones as Markets – Markdown Gradient with Establishment Size



Notes: This figure reports the coefficients and 95 percent confidence intervals of establishment size decile dummies, where the deciles are taken with respect to the national distribution of establishment shares of total revenue in their respective local labor markets, in a regression of wage markdowns on these dummies, local labor market fixed effects, and year indicators. Each marker type represents a different markdown measure. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the market level. Markets are 3-digit North American Industrial Classification System (NAICS) industry code × commuting zone pairs. The reference group for the coefficient estimates are the establishments in the first size bin. Regressions pool data from the economic census waves from 1999 to 2019. N=230,185.
Source: Authors' elaboration using data from the Mexican economic census.

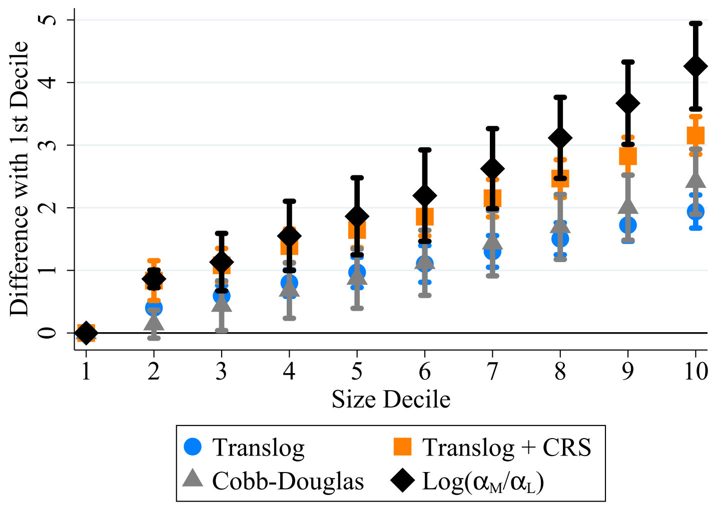
Table D.5: Commuting Zones as Local Markets – Outsourcing and Establishment Size
Outcome Variable: Share of Outsourced Employees

Regressor	(1)	(2)
Employment Share of Local Labor Market	0.08***	
	(0.006)	
Revenue Share of Local Labor Market		0.08***
		(0.005)
N	230,132	230,132
R^2	0.084	0.0856

Notes: All regressions include market fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. Markets are 3-digit North American Industrial Classification System (NAICS) industry code × commuting zone pairs. ***p<0.01.
Source: Authors' elaboration using data from the Mexican economic census waves from 1994 to 2019.

D.3 Markdowns and Outsourcing Gradients with Firm-Level Revenue

Figure D.5: Markdown Gradient with Firm Size



Notes: This figure reports the coefficients and 95 percent confidence intervals of firm size decile dummies, where the deciles are taken with respect to the national distribution of firm shares of total revenue in their respective local labor markets, in a regression of establishment-level wage markdowns on these dummies, local labor market fixed effects, and year indicators. Each marker type represents a different markdown measure. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the market level. Markets are 3-digit North American Industrial Classification System (NAICS) industry code × metropolitan area/municipality pairs. The reference group for the coefficient estimates are the firms in the first firm size bin. The regression pools data from the economic census waves from 1999 to 2019. N=229,717.
Source: Authors' elaboration using data from the Mexican economic census.

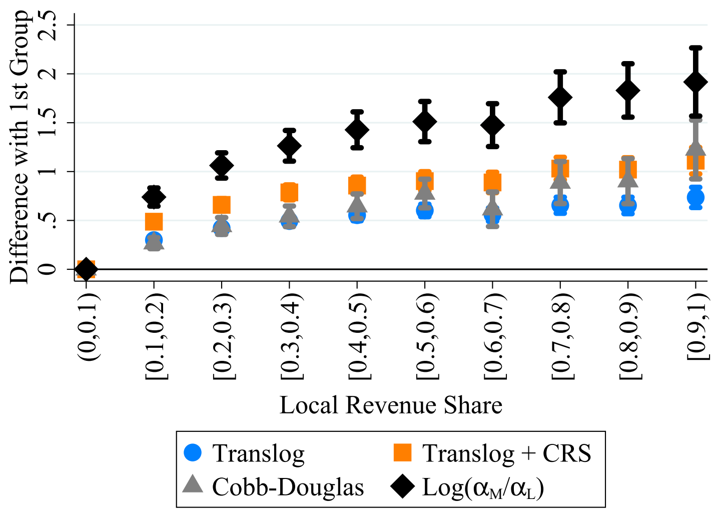
Table D.6: Outsourcing and Firm Size
Outcome Variable: Firm Share of Outsourced Employees

Regressor	(1)	(2)
Firm Employment Share of Local Labor Market	0.07***	
	(0.005)	
Firm Revenue Share of Local Labor Market		0.06***
		(0.004)
N	228,717	228,717
R^2	0.089	0.09

Notes: Firms are the unit of observation. All regressions include firm fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the firm level. Markets are 3-digit North American Industrial Classification System (NAICS) industry code × metropolitan area/municipality pairs. ***p<0.01.
Source: Authors' elaboration using data from the Mexican economic census waves from 1994 to 2019.

D.4 Markdown Gradient under an Alternative Partition of the Size Range

Figure D.6: Markdown Gradient under an Alternative Partition of the Size Range



Notes: This figure reports the coefficients and 95 percent confidence intervals of establishment size category dummies in a regression of wage markdowns on these dummies, establishment fixed effects, and year indicators. Each marker type represents a different markdown measure. Establishment size is defined as the establishment share of total revenue in its local labor market. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. Markets are 3-digit North American Industrial Classification System (NAICS) industry code × metropolitan area/municipality pairs. The reference group for the coefficient estimates are the establishments in the first size category. The regression pools data from the economic census waves from 1999 to 2019. N=226,784.
Source: Authors' elaboration using data from the Mexican economic census.

E Dose-Specific Average Treatment Effects

In this section, we show that our binarized specification does not mask meaningful heterogeneity in dose-response. To that end, we estimate the average treatment effect of dose d at time t on the establishment-level outcome Y, denoted by $ATE(d,t) \equiv E[Y_t^d - Y_t^0]$, focusing on employment and wages, our key regression outcomes. Before turning to the formal estimation, we present a binned scatterplot for each outcome of interest, showing the relative outcome change two years after the reform, denoted as $\Delta Y_t - E[\Delta Y_t | d = 0]$, as a function of the establishment's outsourcing share of employment in March 2021, denoted as d. Under the so-called strong parallel trends assumption,[49] it can be shown that $\Delta Y_t - E[\Delta Y_t | d = 0] = ATE(d,t) + u_t$, where u_t is

[49] Strong parallel trends requires that the observed outcome path of each dose group reflects what the entire population would have experienced under that dose, i.e., $E[Y_t(d) - Y_{t-1}(0)] = E[Y_t(d) - Y_{t-1}(0) | D = d]$ for all d. This is stronger than the standard parallel trends assumption invoked in our main specification, as it restricts treatment effect heterogeneity across dose groups. See Callaway, Goodman-Bacon and Sant'Anna (2024) for a

an error term. Thus, we should expect the cloud of dots in such a scatterplot to lie above zero on the y-axis and increase with the dose if higher average treatment effects result from higher exposure to the reform. Conversely, we should expect the cloud of points to lie close to zero everywhere on the y-axis if the average treatment effect is zero for every dose level. Indeed, Figure E.1 shows that the points in the scatterplot for employment lie close to zero regardless of the exposure level to the reform, whereas the points in the scatterplot for wages lie everywhere above zero on the y-axis, tracing a clear upward-sloping curve.

Next, we turn to formally estimating the average treatment effect of dose d at time t, following a two-step procedure. First, we regress

$$\Delta Y_{it} = \alpha_t + \sum_{k=1}^{K} \psi_k(d_i)\beta_{kt} + \varepsilon_{it}, \tag{E1}$$

where $\Delta Y_{it} = Y_{it} - Y_{it_0}$ is the log difference in the outcome of establishment i from March 2021 to month t, d_i is the outsourcing share of employment in establishment i in March 2021, $\boldsymbol{\psi}_K(d_i) = (\psi_1(d_i), \psi_2(d_i), ..., \psi_K(d_i))'$ is a vector of cubic B-splines in d_i with K knots, and ε_{it} is an error term.

Second, we construct a nonparametric estimator of $ATE(d,t)$, given by

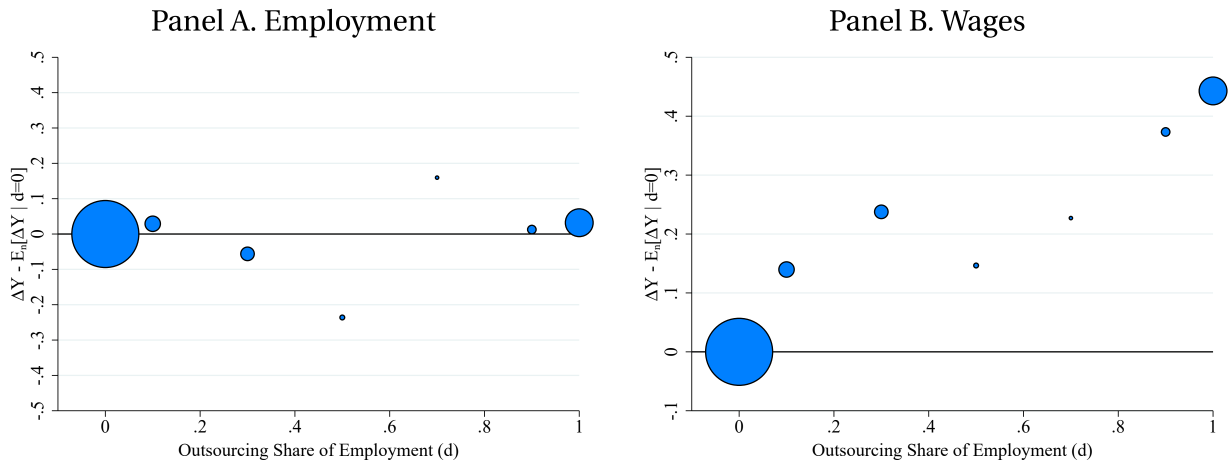
$$\widehat{ATE}(d,t) = \boldsymbol{\psi}_K(d)'\hat{\boldsymbol{\beta}}_{Kt}, \tag{E2}$$

where $\hat{\boldsymbol{\beta}}_{Kt} = (\hat{\beta}_{1t}, \hat{\beta}_{2t}, ...\hat{\beta}_{Kt})$ is the K-dimensional vector of OLS estimates for the coefficients in Equation (E1). This estimator has the desirable property of yielding consistent estimates under the strong parallel trends assumption. Furthermore, standard errors for these estimates can easily be obtained by means of the Delta method.

In Figure E.2, we report our estimated $\widehat{ATE}(d)$ functions for employment and wages 3 years after the enactment of the reform. We note a monotonically increasing average treatment effect of the reform on wages, while no such trend is present for employment.

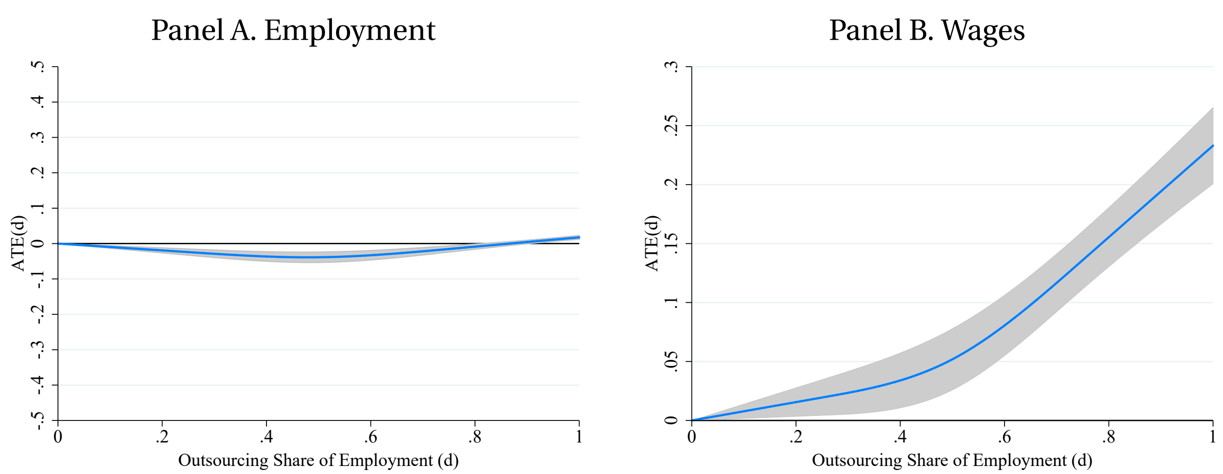
detailed discussion.

Figure E.1: Outcome Changes Two Years After the Reform by the Outsourcing Share of Employment



Notes: This figure presents a bin scatterplot of the establishment-level outcome changes two years after the reform relative to the mean outcome changes experienced by zero-dose establishments. Each bin represents the average across all establishments within a given treatment dose range. The bin range size is 0.1 everywhere in the [0,1] interval with the exception of 0 and 1, with each of these two doses classified in a separate bin. The size of each point in the scatterplot represents the number of establishments in each bin. Our dose exposure measure is the outsourcing share of employment in March 2021. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for March 2021 is excluded from each regression, so the effects can be interpreted as deviations from the outcome mean of the comparison group in the month prior to the 2021 reform.
Source: Authors' elaboration using data from Mexican monthly manufacturing survey. Wages are deflated to July 2019 using the intermediate goods subindex of Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

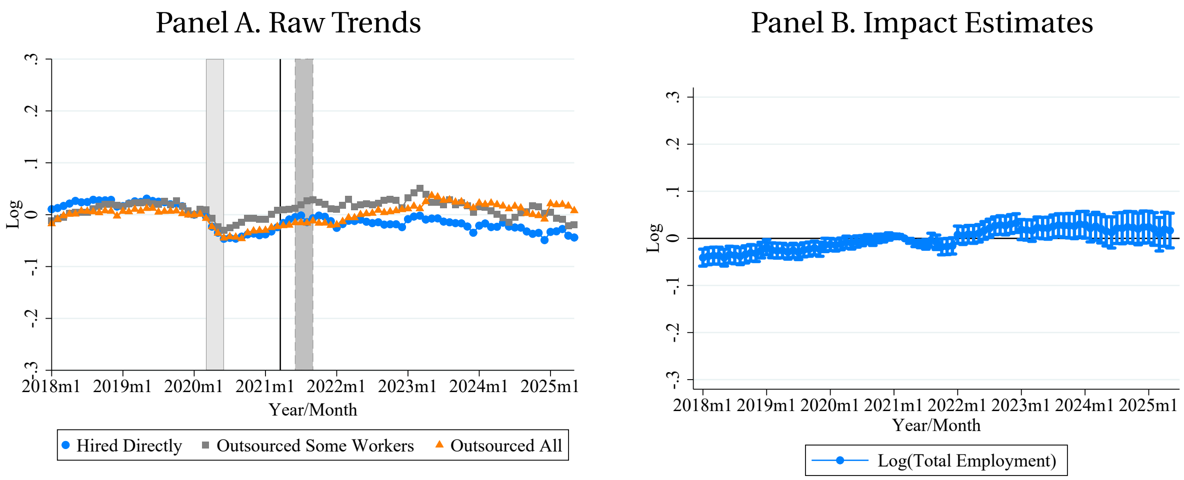
Figure E.2: Nonparametric Estimates of $ATE(d)$



Notes: Each panel in this figure presents the nonparametric estimates and 95 percent confidence intervals of $ATE(d)$ at the establishment level for a different outcome variable. The dose measure is the outsourcing share of employment in March 2021, a month prior to the enactment of the reform. Standard errors are robust to heteroskedasticity of unknown form.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey. Wages are deflated to July 2019 using the intermediate goods subindex of Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

F Additional Establishment-Level Figures and Tables

Figure F.1: Reform Impacts on Log Employment



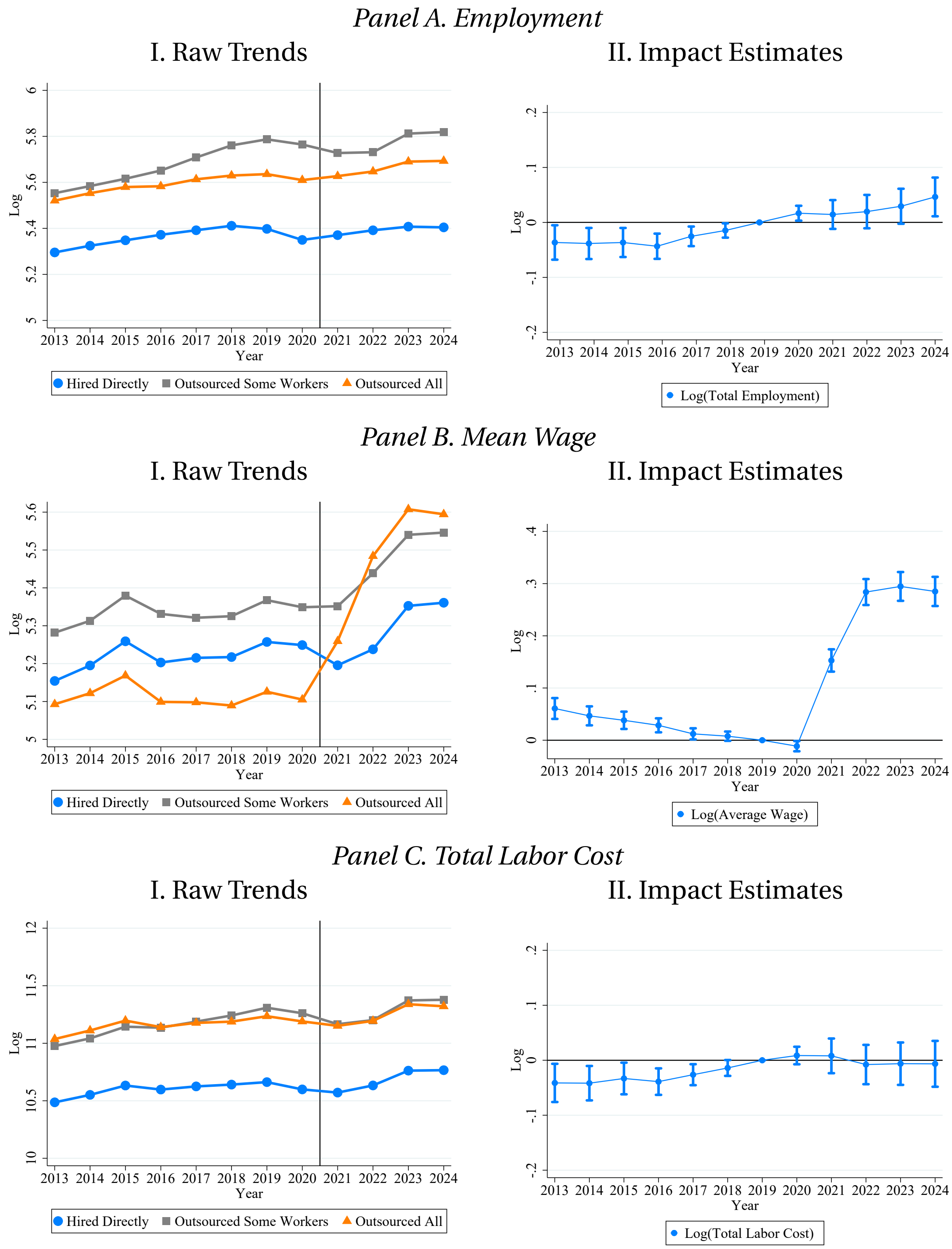
Notes: This figure illustrates the impact of the outsourcing ban on log employment at the establishment level. In Panel A, we compare the mean log employment of three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. The first gray shaded area, outlined with a solid line, denotes the strictest COVID-19 lockdown imposed by the Mexican federal government. The second gray shaded area, outlined with a dashed line, denotes the grace period during which staffing companies were allowed to transfer previously outsourced workers to their client firms, as mandated by the reform. Panel B presents the corresponding difference-in-differences estimates. The interaction for March 2021 is omitted, so the estimated effects are interpreted relative to the mean outcome of the each group the month prior to the reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025.

Table F.1: Reform Impacts on Log Employment and the Log Mean Wage

Regressor	Employment (1)	Mean Wage (2)
	Panel A. No Controls	
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t \in 2024\}$	0.03*	0.11***
	(0.02)	(0.01)
Robust Confidence Set	[-.09,.14]	[-.04,.25]
N	721,806	721,806
R^2	0.008	0.243
	Panel B. With Flexible Controls	
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t \in 2024\}$	-0.02	0.10***
	(0.02)	(0.01)
Robust Confidence Set	[-.10,.06]	[-.04,.24]
N	721,806	721,806
R^2	0.033	0.253

Notes: The set of flexible controls includes indicators for 3-digit industry and the establishment's initial revenue interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. *p<0.1, ***p<0.01.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2024. All monetary amounts are deflated using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.2: Reform Impacts on Log Outcomes: Annual Manufacturing Survey



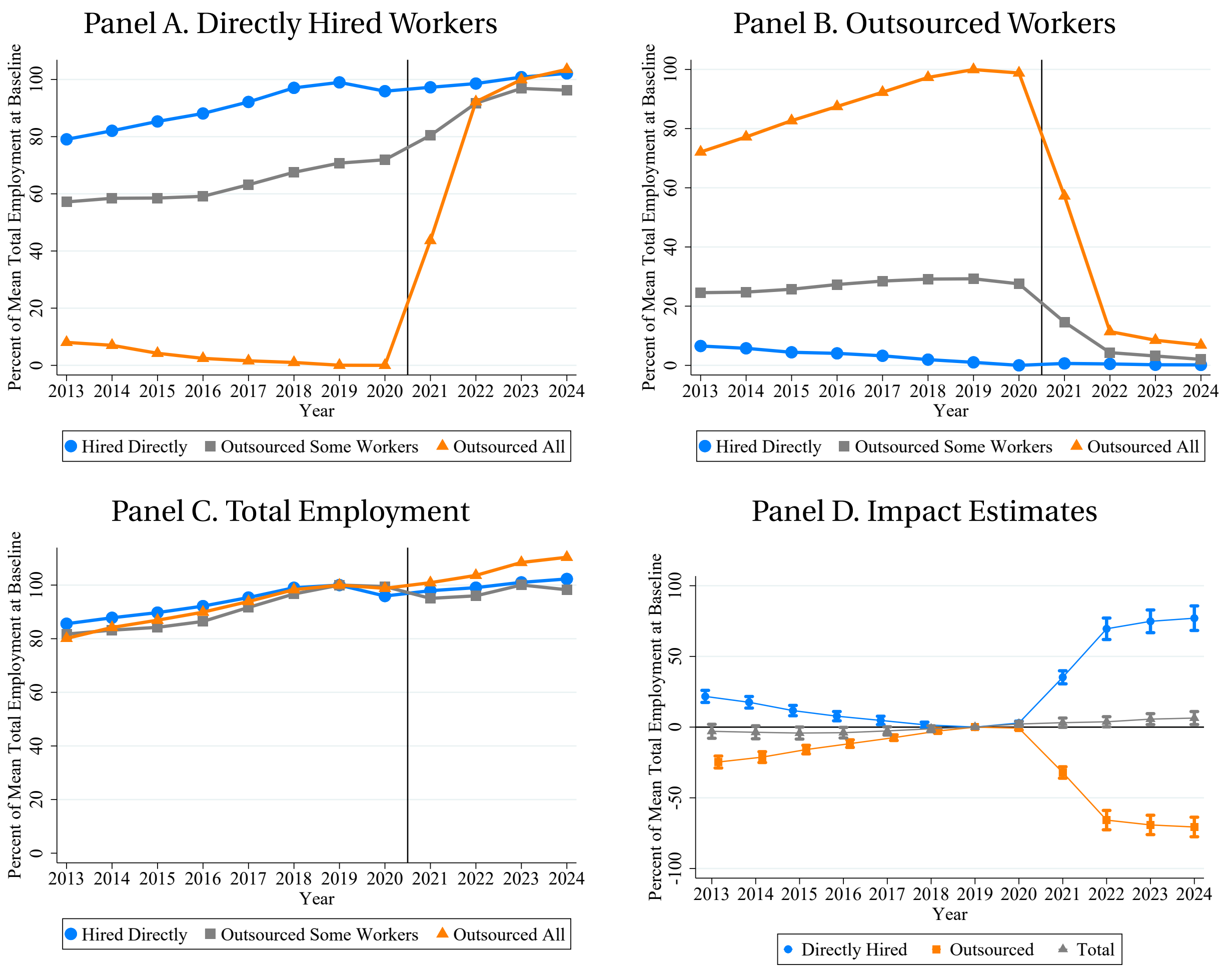
Notes: For each panel, the first subpanel presents raw trends for three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. For each panel, the second subpanel presents the corresponding difference-in-differences estimates. In all regressions, the interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Table F.2: Reform Impacts on Log Outcomes: Annual Manufacturing Survey

Regressor	Employment (1)	Mean Wage (2)	Total Labor Cost (3)
		Panel A. No Controls	
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	0.05***	0.28***	-0.01
	(0.02)	(0.01)	(0.02)
Robust Confidence Set	[-.14,.24]	[.16,.41]	[-.17,.17]
N	42,826	42,826	42,826
R^2	0.02	0.203	0.046
		Panel B. With Flexible Controls	
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	0.02	0.28***	-0.03
	(0.02)	(0.01)	(0.02)
Robust Confidence Set	[-.12,.17]	[.14,.42]	[-.16,.098]
N	42,826	42,826	42,826
R^2	0.051	0.227	0.081

Notes: The set of flexible controls consists of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.3: Reform Impacts on Employment: Annual Manufacturing Survey



Notes: This figure illustrates the impact of the outsourcing ban on employment at the establishment level. Panels A, B, and C present raw trends for three different outcomes expressed in percent relative to each group's mean total employment at baseline. In each panel, we compare three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel D presents the difference-in-differences estimates for the three outcomes. In all regressions, the interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024.

Table F.3: Impacts of the Reform on Establishment-Level Employment: Annual Manufacturing Survey

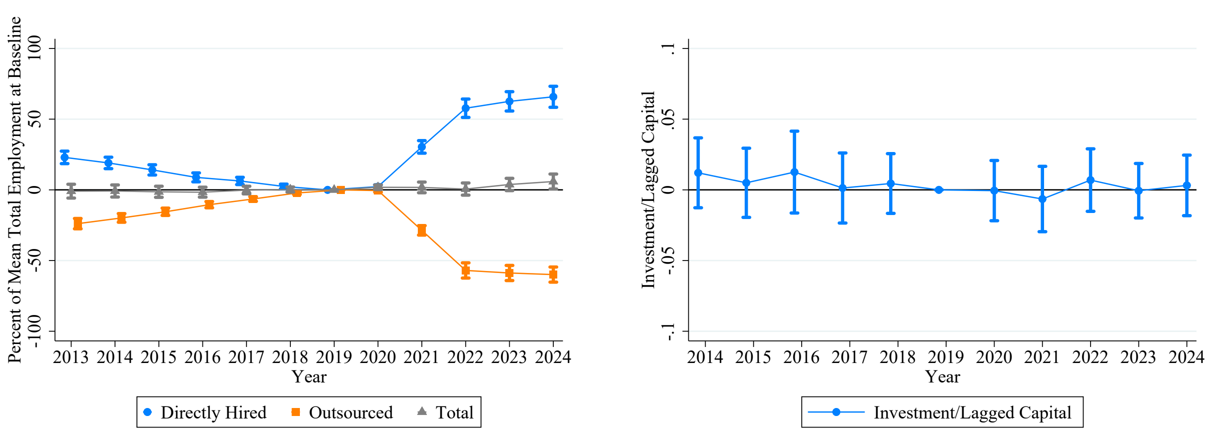
Regressor	Directly Hired	Outsourced	Total
	(1)	(2)	(3)
		Panel A. No Controls	
Outsourcing$_{i,2020}$ $\times$ $\mathbb{1}\{t=2024\}$	77***	-70.6***	6.4***
	(4.4)	(3.5)	(2.4)
Robust Confidence Set	[45,110]	[-101,-43]	[-15,29]
N	42,826	42,826	42,826
R^2	0.138	0.175	0.025
		Panel B. With Flexible Controls	
Outsourcing$_{i,2020}$ $\times$ $\mathbb{1}\{t=2024\}$	72***	-67***	5.0**
	(4.3)	(3.2)	(2.4)
Robust Confidence Set	[36,109]	[-98,-38]	[-15,26]
N	42,826	42,826	42,826
R^2	0.176	0.205	0.059
Mean for the Treaded in 2019	23.9	76.1	100

Notes: The set of flexible controls consists of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. **p<0.05, ***p<0.01.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024.

Figure F.4: Reform Impacts on Employment and Investment Unconditional on Firm Exit

Panel A. Employment

Panel B. Capital Investment



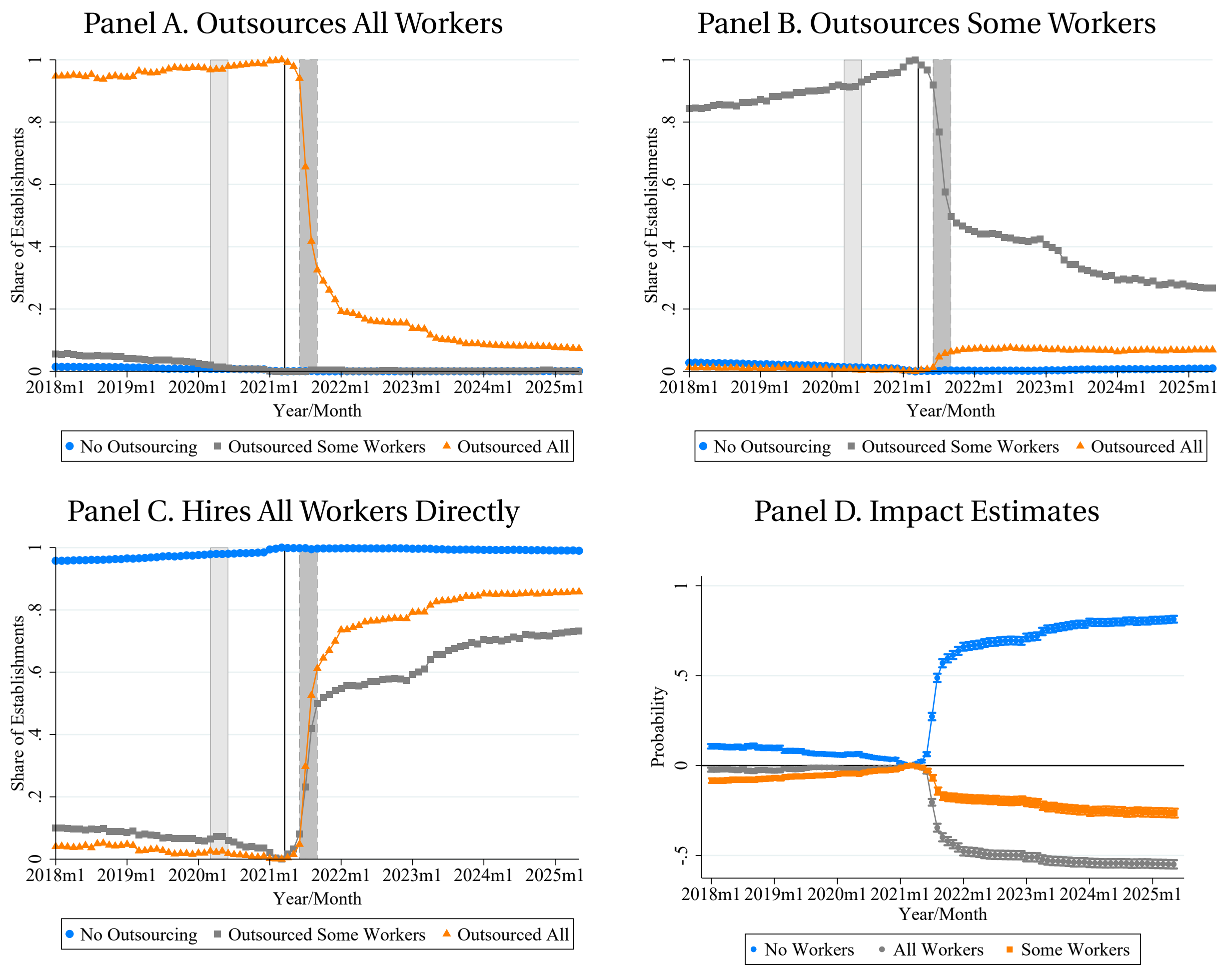
Notes: This figure presents our differences-in-differences coefficient estimates and 95 percent confidence intervals for employment by hiring modality and capital investment, estimated on a balanced panel of establishments by setting to zero the employment and capital investment of establishments that exit the market. The interaction for 2019 is excluded from each regression, so the effects can be interpreted as deviations from the outcome mean of the comparison group the year prior to the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Table F.4: Reform Impacts on Employment and Investment Unconditional on Firm Exit

Regressor	Employment			Investment
	Directly Hired	Outsourced	Total	
	(1)	(2)	(3)	(4)
		Panel A. No Controls		
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	65.8***	-59.9***	5.9**	0.00
	(3.8)	(2.7)	(2.7)	(0.01)
Robust Confidence Set	[36,96]	[-88,-34]	[-8.3,22]	[-.13,.14]
N	56,712	56,712	56,712	50,759
R^2	0.091	0.153	0.024	0.032
		Panel A. With Flexible Controls		
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	61.7***	-57.5***	4.2	0.00
	(3.7)	(2.6)	(2.8)	(0.01)
Robust Confidence Set	[32,93]	[-86,-30]	[-10,20]	[-.14,.15]
N	56,712	56,712	56,712	50,759
R^2	0.133	0.176	0.068	0.038
Mean for the Treated in 2019	76	24	100	0.172

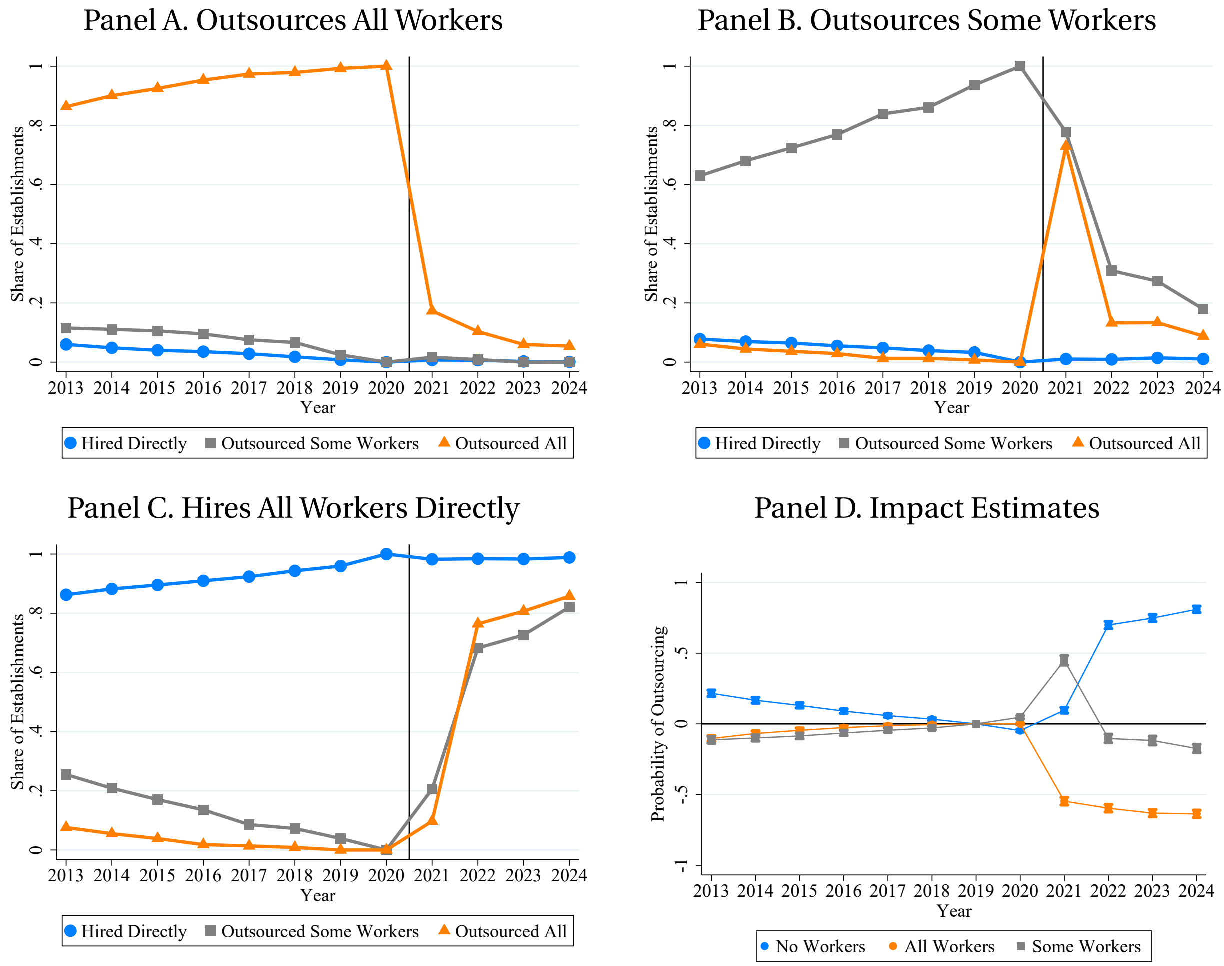
Notes: The sample used for estimation is a balanced panel running from 2013 to 2024, which includes all manufacturing establishments that did not rotate out of the annual manufacturing survey in 2021 and that had not exited the market before the enactment of the reform. Column (4) shows the effect of the reform on the ratio of investment to the lagged capital stock of the establishment, such that the estimation sample is limited to observations for which the first-order lag of capital stock is not zero. Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024.

Figure F.5: Reform Effects on Outsourcing Prevalence



Notes: This figure illustrates the impact of the outsourcing ban on the prevalence of outsourcing at the establishment level. Panels A, B, and C present raw trends for three different outcomes. In each panel, we compare three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. The first gray shaded area, outlined with a solid line, denotes the strictest COVID-19 lockdown imposed by the Mexican federal government. The second gray shaded area, outlined with a dashed line, denotes the grace period during which staffing companies were allowed to transfer previously outsourced workers to their client firms, as mandated by the reform. Panel D presents the difference-in-differences estimates for the three outcomes. In each regression, the interaction for March 2021 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the month prior to the reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from January 2018 to May 2025.

Figure F.6: Reform Impacts on the Prevalence of Outsourcing: Annual Manufacturing Survey



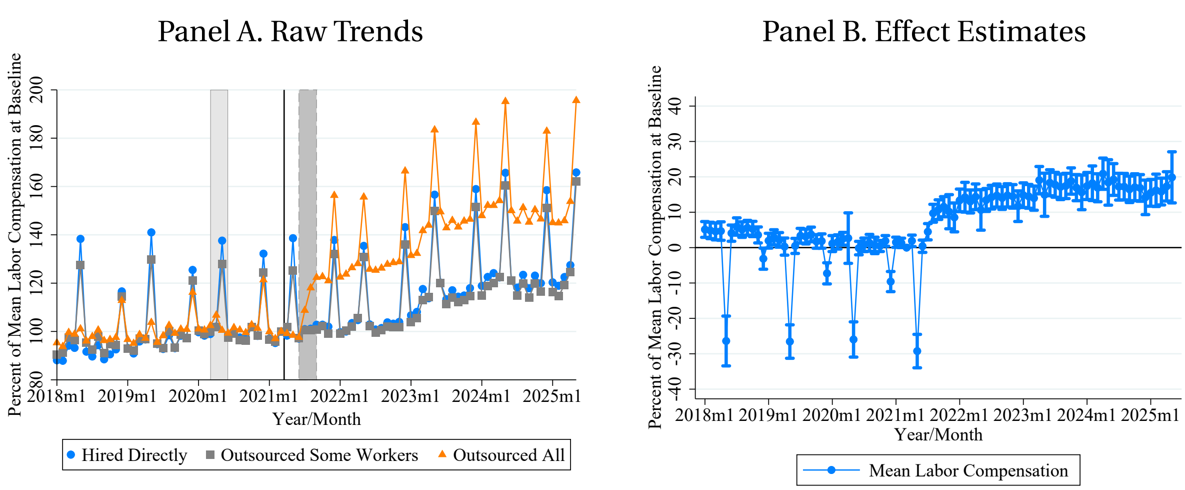
Notes: This figure illustrates the impact of the outsourcing ban on the prevalence of outsourcing at the establishment level. Panels A, B, and C present raw trends for three different outcomes. In each panel, we compare three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel D presents the difference-in-differences estimates for the three outcomes. In all regressions, the interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024.

Table F.5: Impacts of the Reform on Establishment-Level Outsourcing:
Annual Manufacturing Survey

Regressor	All Workers (1)	Some Workers (2)	No Workers (3)
		Panel A. No Controls	
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	-0.64***	-0.17***	0.81***
	(0.01)	(0.01)	(0.01)
Robust Confidence Set	[-.82,-.45]	[-.53,.18]	[.44,1.2]
N	42,826	42,826	42,826
R^2	0.478	0.156	0.482
		Panel A. With Flexible Controls	
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	-0.63***	-0.18***	0.81***
	(0.01)	(0.02)	(0.01)
Robust Confidence Set	[-.83,-.43]	[-.55,.19]	[.42,1.2]
N	42,826	42,826	42,826
R^2	0.487	0.169	0.489
Mean for the Treated in 2019	0.667	0.321	0.013

Notes: The set of flexible controls consists of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024.

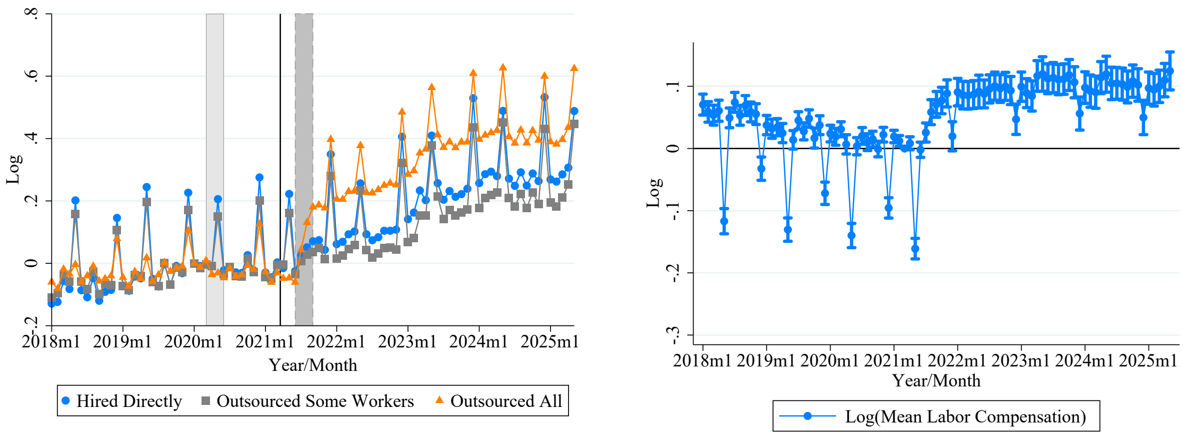
Figure F.7: Reform Effects on the Mean Wage



Notes: This figure illustrates the impact of the outsourcing ban on the mean wage at the establishment level. In Panel A, we compare the mean trends of three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. Trends are expressed relative to the mean of each group in March 2021. The vertical solid line marks the enactment of the reform. The first gray shaded area, outlined with a solid line, denotes the strictest COVID-19 lockdown imposed by the Mexican federal government. The second gray shaded area, outlined with a dashed line, denotes the grace period during which staffing companies were allowed to transfer previously outsourced workers to their client firms, as mandated by the reform. Panel B presents the corresponding difference-in-differences estimates. The interaction for March 2021 is omitted, so the estimated effects are interpreted relative to the mean outcome of the each group the month prior to the reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.8: Reform Impacts on the Log Mean Wage at the Establishment Level

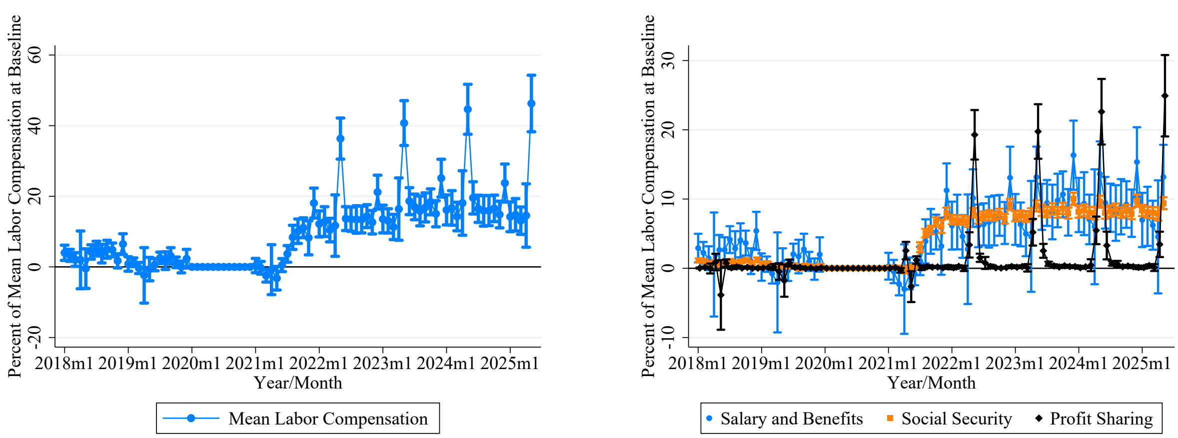
Panel A. Raw Trends | Panel B. Impact Estimates



Notes: This figure illustrates the impact of the outsourcing ban on the log mean wage at the establishment level. In Panel A, we compare the mean outcome of three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. The first gray shaded area, outlined with a solid line, denotes the strictest COVID-19 lockdown imposed by the Mexican federal government. The second gray shaded area, outlined with a dashed line, denotes the grace period during which staffing companies were allowed to transfer previously outsourced workers to their client firms, as mandated by the reform. Panel B presents the corresponding difference-in-differences estimates. The interaction for March 2021 is omitted, so the estimated effects are interpreted relative to the mean outcome of the each group the month prior to the reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

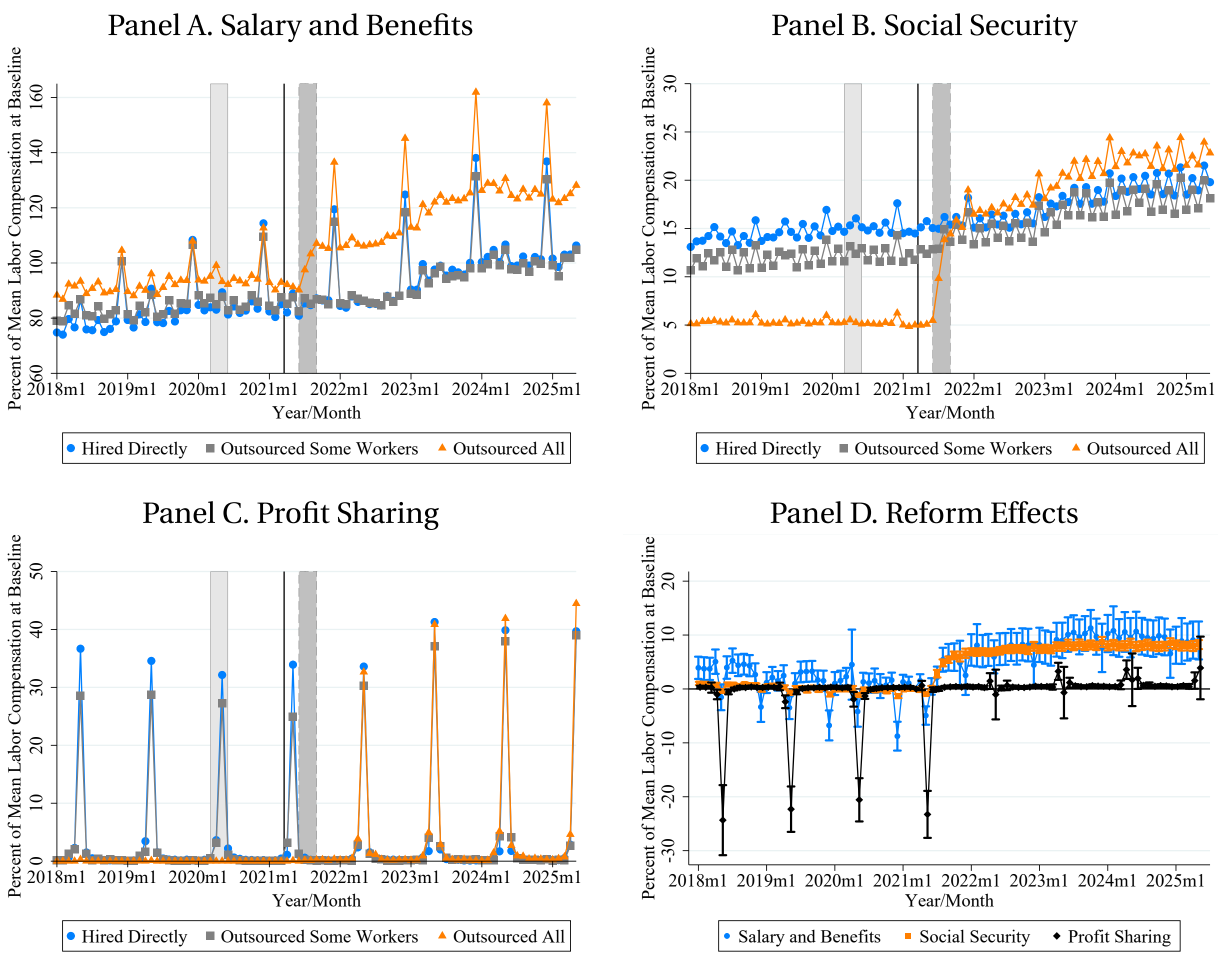
Figure F.9: Reform Impacts on the Mean Wage and Its Composition by Calendar Month

Panel A. Mean Wage | Panel B. Wage Composition



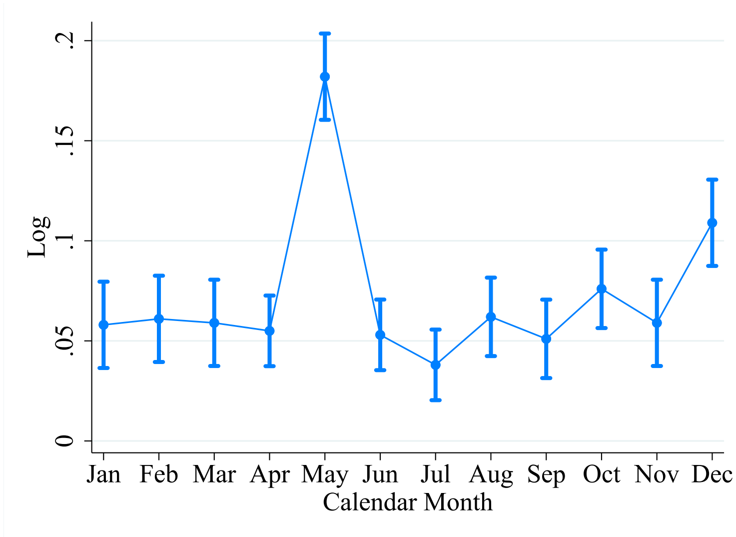
Notes: This figure presents differences-in-differences estimates for the mean wage and its components, obtained by running 12 month-specific regressions, one for each calendar month. All regressions include establishment fixed effects and year dummies, and exclude the interaction between the 2020 indicator and the outsourcing indicator, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year prior to the reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.10: Reform Effects on Mean Wage Composition



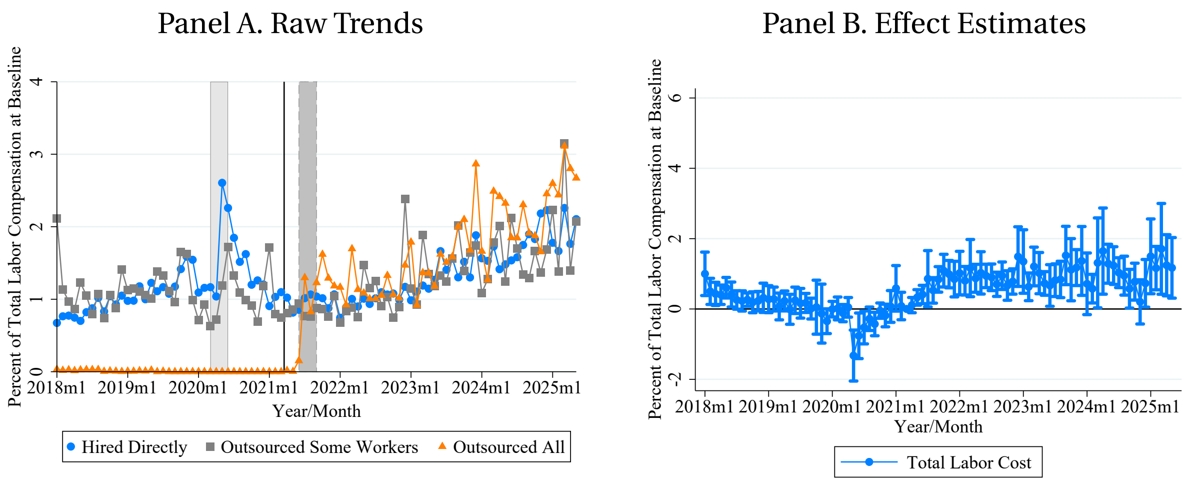
Notes: This figure illustrates the impact of the outsourcing ban on the composition of the mean wage at the establishment level. Panels A, B, and C present raw trends for three different outcomes, expressed relative to the mean wage for each group in March 2021. In each panel, we compare three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. The first gray shaded area, outlined with a solid line, denotes the strictest COVID-19 lockdown imposed by the Mexican federal government. The second gray shaded area, outlined with a dashed line, denotes the grace period during which staffing companies were allowed to transfer previously outsourced workers to their client firms, as mandated by the reform. Panel D presents the difference-in-differences estimates for the three outcomes. In each regression, the interaction for March 2021 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the month prior to the reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.11: Reform Impacts on Log Mean Wage by Calendar Month



Notes: This figure presents differences-in-differences estimates for the log mean wage by calendar month, estimated running 12 month-specific regressions, one for each calendar month. All regressions include establishment fixed effects and year dummies, and exclude the interaction between the 2020 indicator and the outsourcing indicator, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year prior to the reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.12: Reform Effects on Total Firing Costs



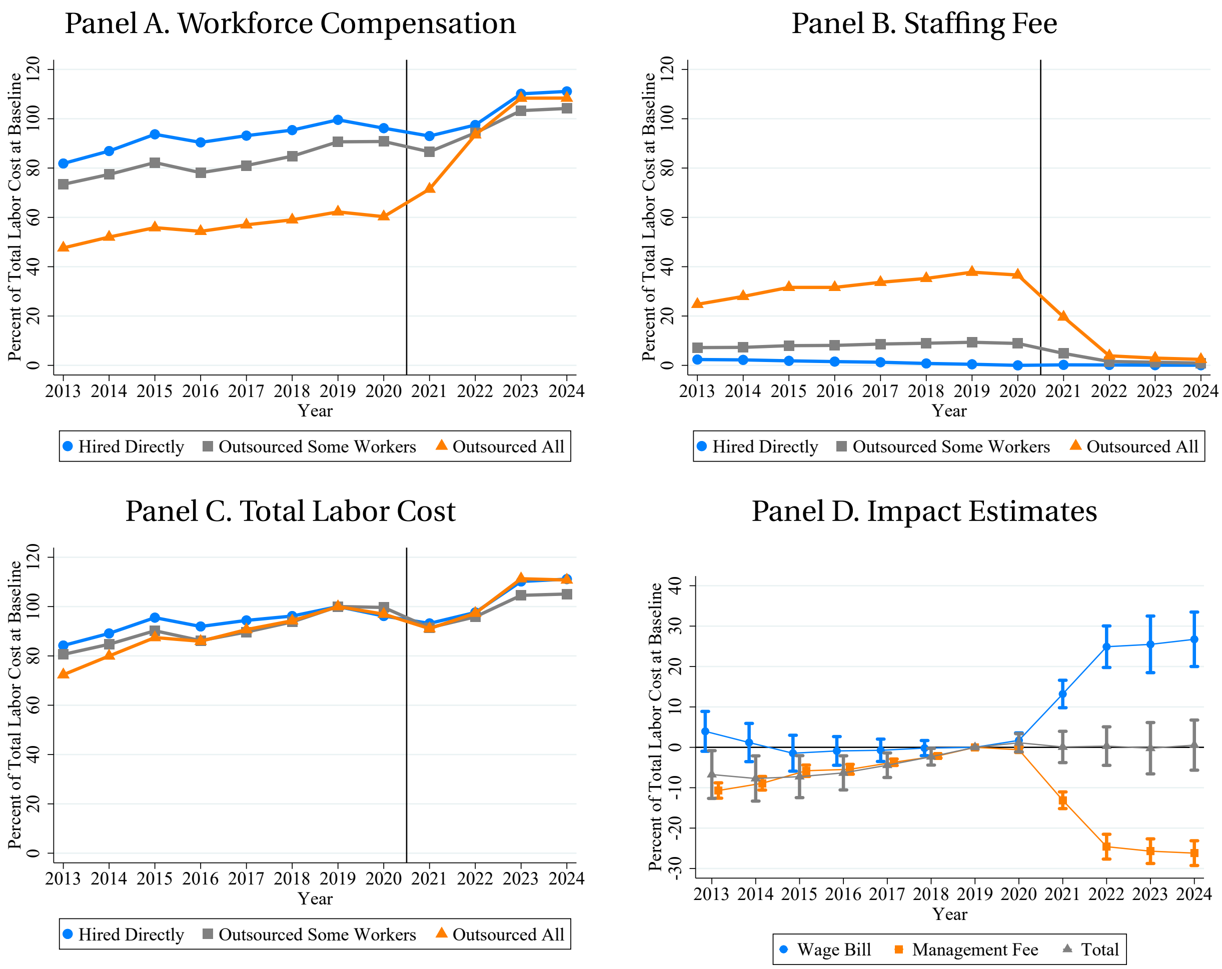
Notes: This figure illustrates the impact of the outsourcing ban on total firing costs at the establishment level. In Panel A, we compare the mean firing costs of three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. Trends are expressed relative to the mean total workforce compensation of each group in March 2021. The vertical solid line marks the enactment of the reform. The first gray shaded area, outlined with a solid line, denotes the strictest COVID-19 lockdown imposed by the Mexican federal government. The second gray shaded area, outlined with a dashed line, denotes the grace period during which staffing companies were allowed to transfer previously outsourced workers to their client firms, as mandated by the reform. Panel B presents the corresponding difference-in-differences estimates. The interaction for March 2021 is omitted, so the estimated effects are interpreted relative to the mean outcome of the each group the month prior to the reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Table F.6: Reform Impacts on Mean Wage Composition:
Monthly Data Aggregated at the Annual Level

Regressor	Mean Wages				Firing Costs
	Total	Salaries & Benefits	Social Security	Profit Sharing	
	(1)	(2)	(3)	(4)	(5)
			Panel A. No Controls		
Outsourcing$_{i,\text{March 2021}} \times \mathbb{1}\{t = 2024\}$	19.8***	9.9***	8.3***	2.8***	1.218***
	(2.0)	(1.8)	(0.4)	(0.2)	(0.2)
Robust Confidence Set	[5.5,35]	[-1.9,22]	[5.4,11]	[1.2,4.3]	[-1.2,3.8]
N	65,177	65,177	65,177	65,177	65,177
R^2	0.206	0.176	0.325	0.009	0.008
			Panel B. With Flexible Controls		
Outsourcing$_{i,\text{March 2021}} \times \mathbb{1}\{t = 2024\}$	17.5***	8.2***	7.6***	2.8***	1.3***
	(2.1)	(1.9)	(0.4)	(0.3)	(0.2)
Robust Confidence Set	[2.7,33]	[-4.3,21]	[4.5,11]	[-.18,5.8]	[-1.2,4]
N	65,177	65,177	65,177	65,177	65,177
R^2	0.229	0.197	0.345	0.017	0.021
Mean for the Treated in 2020	100	89.6	7.9	2.5	0.406

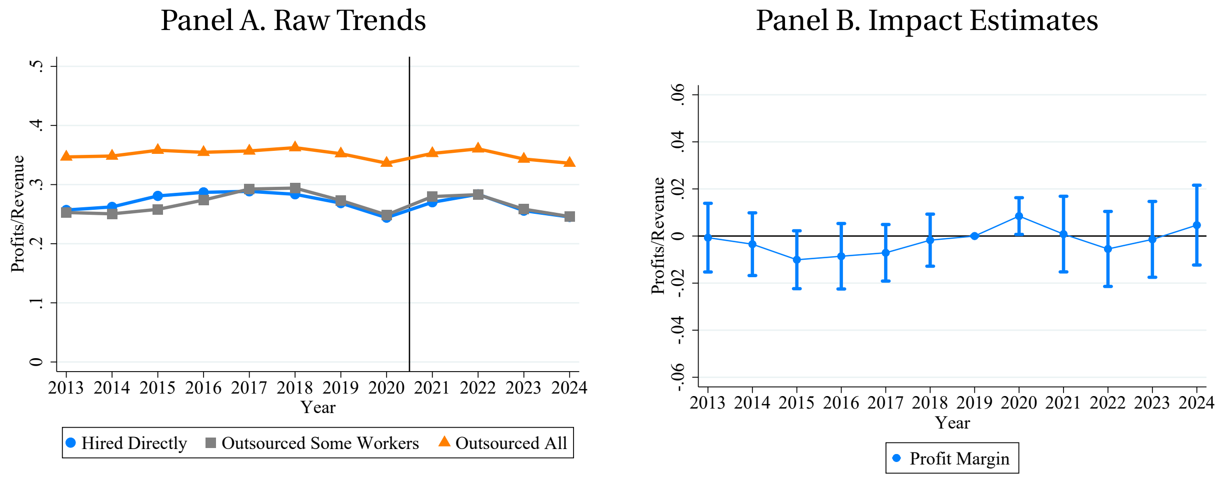
Notes: Outcomes in Columns (1) through (4) are expressed relative to the mean wage of each group in 2020. Firing costs in Column (5) are expressed relative to the mean total workforce compensation of each group in 2020. The set of flexible controls includes indicators for 3-digit industry and the establishment's initial revenue interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2024. All monetary amounts are deflated using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.13: Reform Impacts on the Composition of Labor Cost: Annual Manufacturing Survey



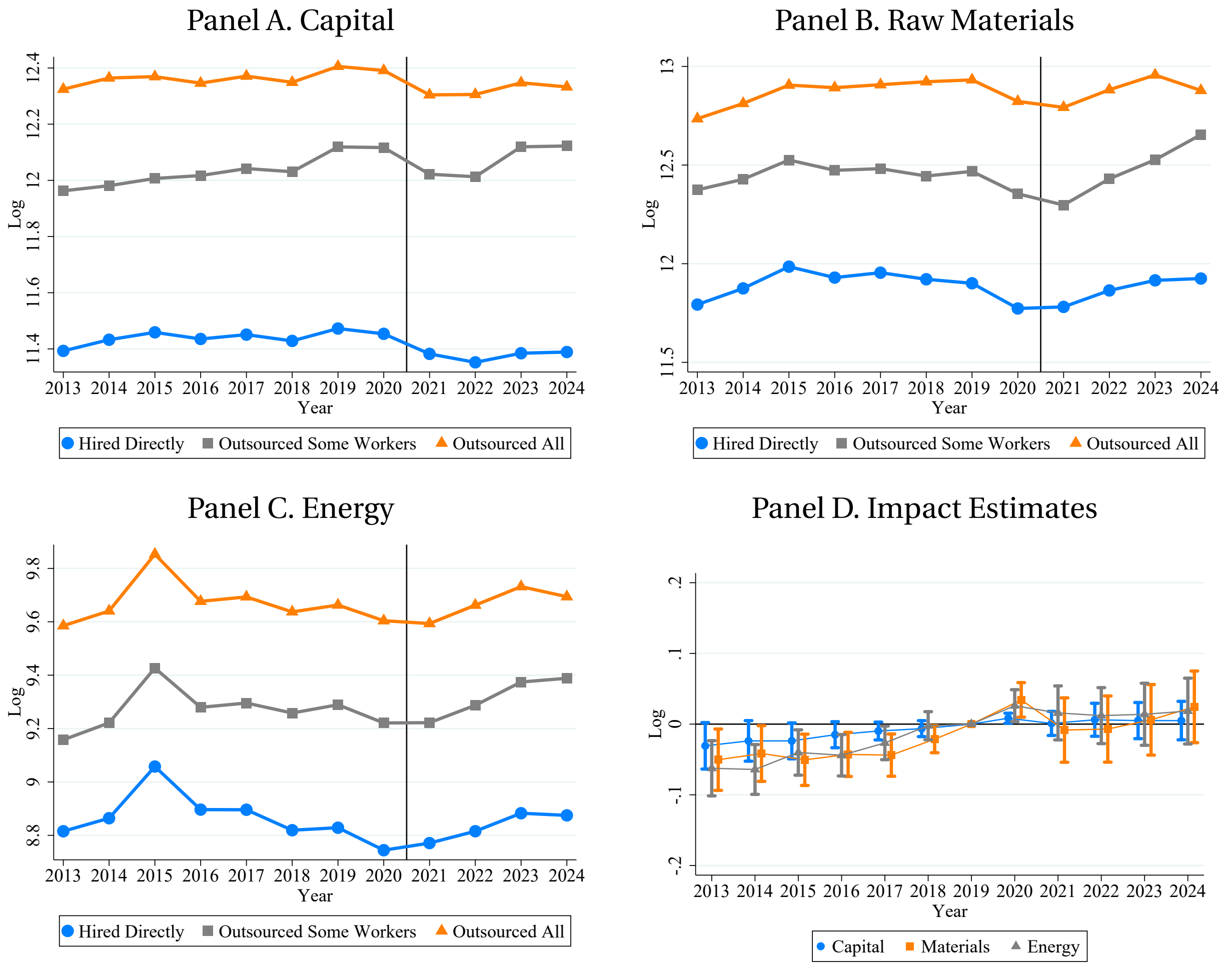
Notes: This figure illustrates the impact of the outsourcing ban on the composition of total labor cost at the establishment level. Panels A, B, and C present raw trends for three different outcomes expressed in percent relative to each group's mean total labor cost at baseline. In each panel, we compare three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel D presents the difference-in-differences estimates for the three outcomes. In all regressions, the interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.14: Reform Impacts on Profits



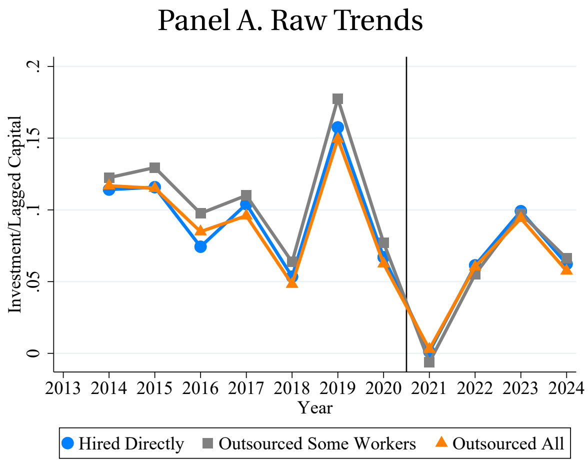
Notes: This figure illustrates the impact of the outsourcing ban on the profit margin of the establishment, defined as the ratio of taxable profits to total revenue. In Panel A, we compare the mean profit margins of three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel B presents the corresponding difference-in-differences estimates. The interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

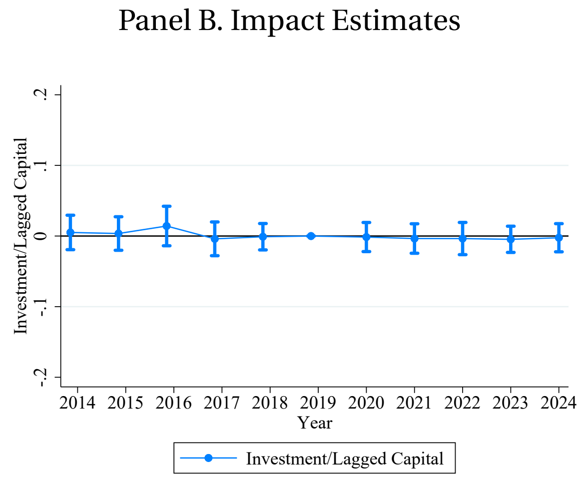
Figure F.15: Reform Impacts on Input Utilization



Notes: This figure illustrates the impact of the outsourcing ban on the use of productive inputs other than labor at the establishment level. Panels A, B, and C present raw trends for three different inputs. In each panel, we compare three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel D presents the difference-in-differences estimates for the three outcomes. In all regressions, the interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.16: Reform Impacts on Capital Investment





Notes: This figure illustrates the impact of the outsourcing ban on establishment-level capital investment as a share of the capital stock lagged by one year. In Panel A, we compare the mean investment levels of three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel B presents the corresponding difference-in-differences estimates. The interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Table F.7: Impacts of the Reform on Establishment-Level Input Utilization

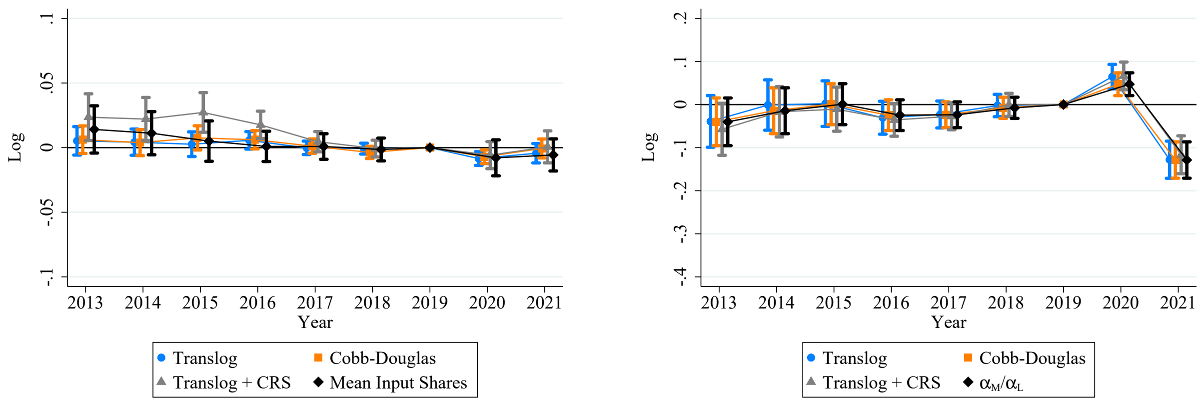
Regressor	Capital Stock	Raw Materials	Energy Consumption	Investment
	(1)	(2)	(3)	(4)
		Panel A. No Controls		
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t = 2024\}$	0.00	0.02	0.02	0.00
	(0.01)	(0.03)	(0.02)	(0.01)
Robust Confidence Set	[-.1,.12]	[-.3,.37]	[-.19,.24]	[-.16,.16]
N	42,826	42,826	42,826	38,981
R^2	0.019	0.0191	0.0312	0.0425
		Panel B. With Flexible Controls		
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t = 2024\}$	0.00	-0.01	0.00	0.00
	(0.01)	(0.03)	(0.02)	(0.01)
Robust Confidence Set	[-.08,.09]	[-.18,.17]	[-.16,.14]	[-.17,.17]
N	42,826	42,826	42,826	38,981
R^2	0.046	0.06	0.066	0.05
Mean at Baseline for the Treated				0.159

Notes: Columns (1) through (3) present the average annual effect of the reform on the log of each outcome variable. Column (4) shows the effect of the reform on the ratio of investment to the lagged capital stock of the establishment, such that the estimation sample is limited to observations for which the first-order lag of capital stock is observed. Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.17: Reform Impacts on Productivity and Markdowns:
Initial Panel of the Annual Manufacturing Survey

Panel A. Productivity

Panel B. Markdowns



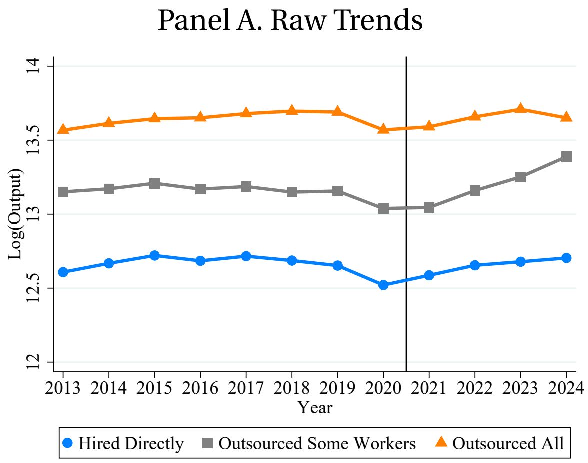
Notes: This figure presents our differences-in-differences coefficient estimates and 95 percent confidence intervals for productivity and markdowns estimated using the initial panel of the annual manufacturing survey. The interaction for 2019 is excluded from each regression, so the effects can be interpreted as deviations from the outcome mean of the comparison group the year prior to the COVID-19 pandemic.
Source: Authors' elaboration using data from the initial panel of the Mexican annual manufacturing survey from 2013 to 2021. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

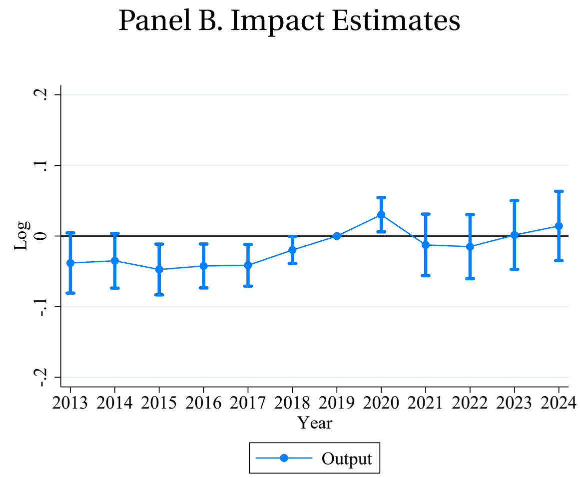
Table F.8: Impacts of the Reform on Establishment-Level Productivity:
Initial Panel of the Annual Manufacturing Survey

Regressor	Translog	Translog+CRS	Cobb-Douglas	Mean Input Shares
	(1)	(2)	(3)	(4)
		Panel A. No Controls		
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2021\}$	-0.004	0.001	-0.001	-0.006
	(0.00)	(0.01)	(0.00)	(0.01)
Robust Confidence Set	[-.02,.01]	[-.03,.03]	[-.02,.01]	[-.03,.02]
N	21,980	21,980	21,980	21,980
R^2	0.145	0.044	0.145	0.054
		Panel B. With Flexible Controls		
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2021\}$	0.003	0.01	0.008**	-0.012*
	(0.00)	(0.01)	(0.00)	(0.01)
Robust Confidence Set	[-.01,.02]	[-.01,.03]	[-.006,.02]	[-.04,.01]
N	21,980	21,980	21,980	21,980
R^2	0.271	0.11	0.238	0.141

Notes: Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. *p<0.1, **p<0.05.
Source: Authors' elaboration using data from the initial panel of the Mexican annual manufacturing survey from 2013 to 2021. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

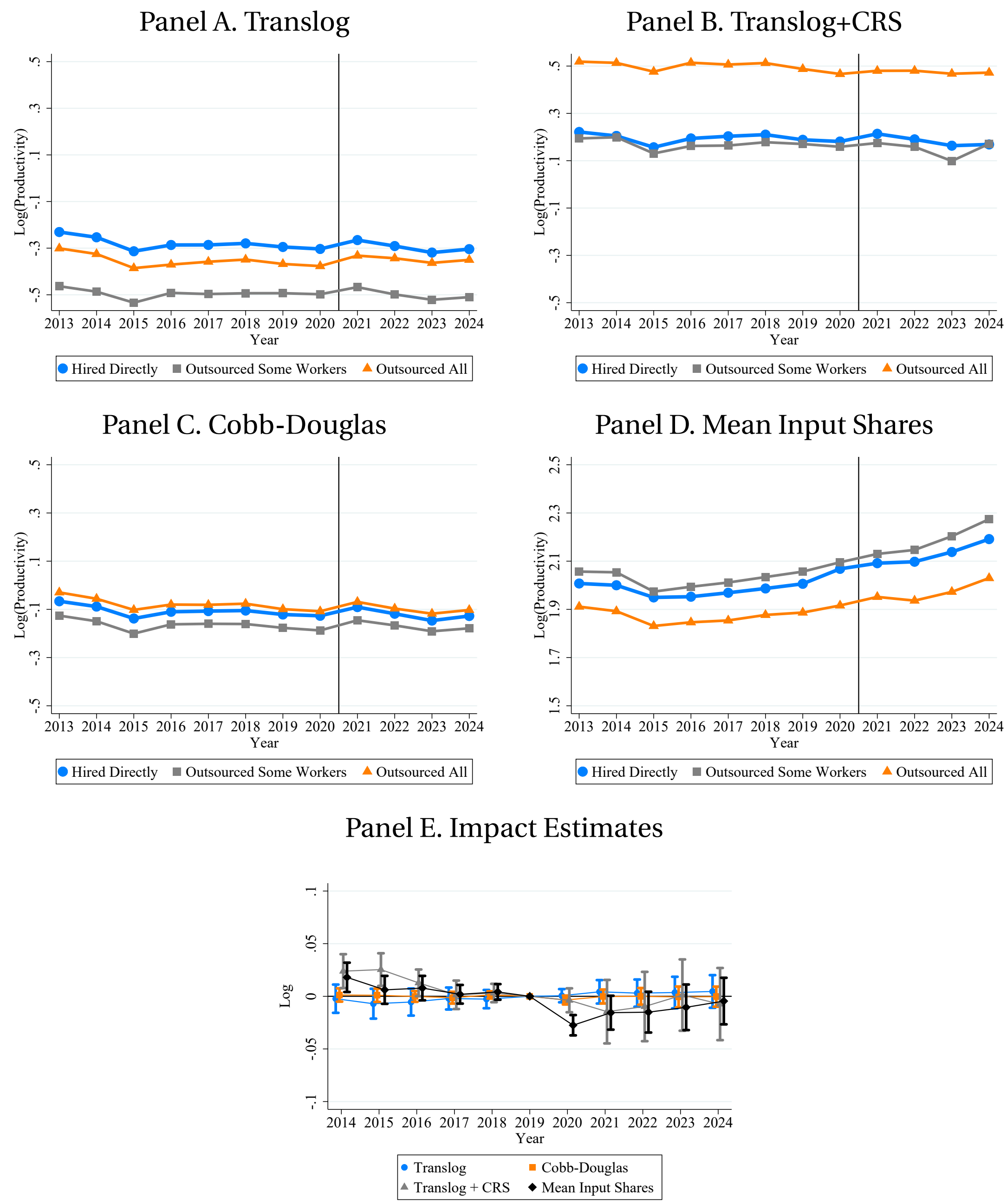
Figure F.18: Reform Impacts on Output





Notes: This figure illustrates the impact of the outsourcing ban on log output at the establishment level. In Panel A, we compare the mean log output of three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel B presents the corresponding difference-in-differences estimates. The interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.19: Reform Impacts on Productivity



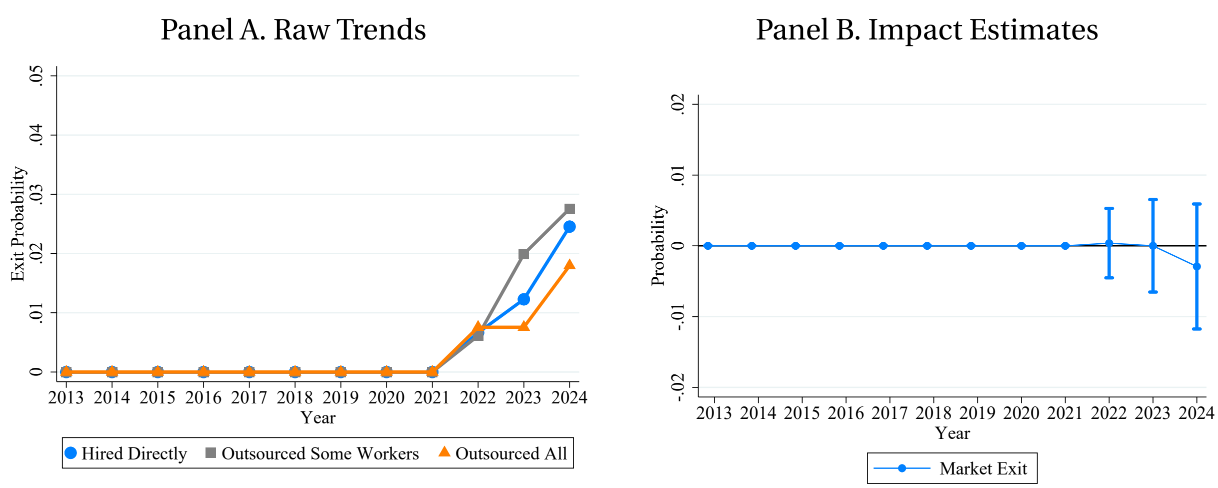
Notes: This figure illustrates the impact on productivity at the establishment level. Panels A, B, C, and D present raw trends in this outcome under four alternative assumptions about the production function. In each panel, we compare three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel E presents the difference-in-differences estimates for the four outcomes. In all regressions, the interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Table F.9: Impacts of the Reform on Establishment-Level Output and Productivity

Regressor	log(Output)	Productivity			
		Translog	Translog+CRS	Cobb-Douglas	Mean Input Shares
	(1)	(2)	(3)	(4)	(5)
			Panel A. No Controls		
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	0.02	0.00	-0.01	0.00	0.00
	(0.03)	(0.01)	(0.02)	(0.00)	(0.01)
Robust Confidence Set	[-.25,.27]	[-.05,.06]	[-.09,.10]	[-.03,.03]	[-.09,.07]
N	26,919	26,919	26,919	26,919	26,919
R^2	0.018	0.036	0.01	0.109	0.213
			Panel B. With Flexible Controls		
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	-0.01	0.01	-0.01	0.00	0.01
	(0.03)	(0.01)	(0.02)	(0.01)	(0.01)
Robust Confidence Set	[-.28,.26]	[-.05,.06]	[-.09,.10]	[-.02,.03]	[-.07,.08]
N	26,919	26,919	26,919	26,919	26,919
R^2	0.051	0.106	0.063	0.14	0.388

Notes: The sample used for estimation includes only observations of establishments that did not rotate out of the annual manufacturing panel in 2021 and for which a lag of the input variables is available. Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.20: Reform Impacts on the Probability of Market Exit



Notes: This figure illustrates the impact of the outsourcing ban on the probability of market exit at the establishment level. In Panel A, we compare the exit rates of three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel B presents the corresponding difference-in-differences estimates. The interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024.

Table F.10: Impacts of the Reform on the Probability of Market Exit at the Establishment Level

Regressor	Exit
	(1)
	Panel A. No Controls
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	-0.003
	(0.005)
Robust Confidence Set	[-.016,.011]
N	56,712
R^2	0.0154
	Panel B. With Flexible Controls
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	0.002
	(0.005)
Robust Confidence Set	[-.012,.016]
N	56,712
R^2	0.03

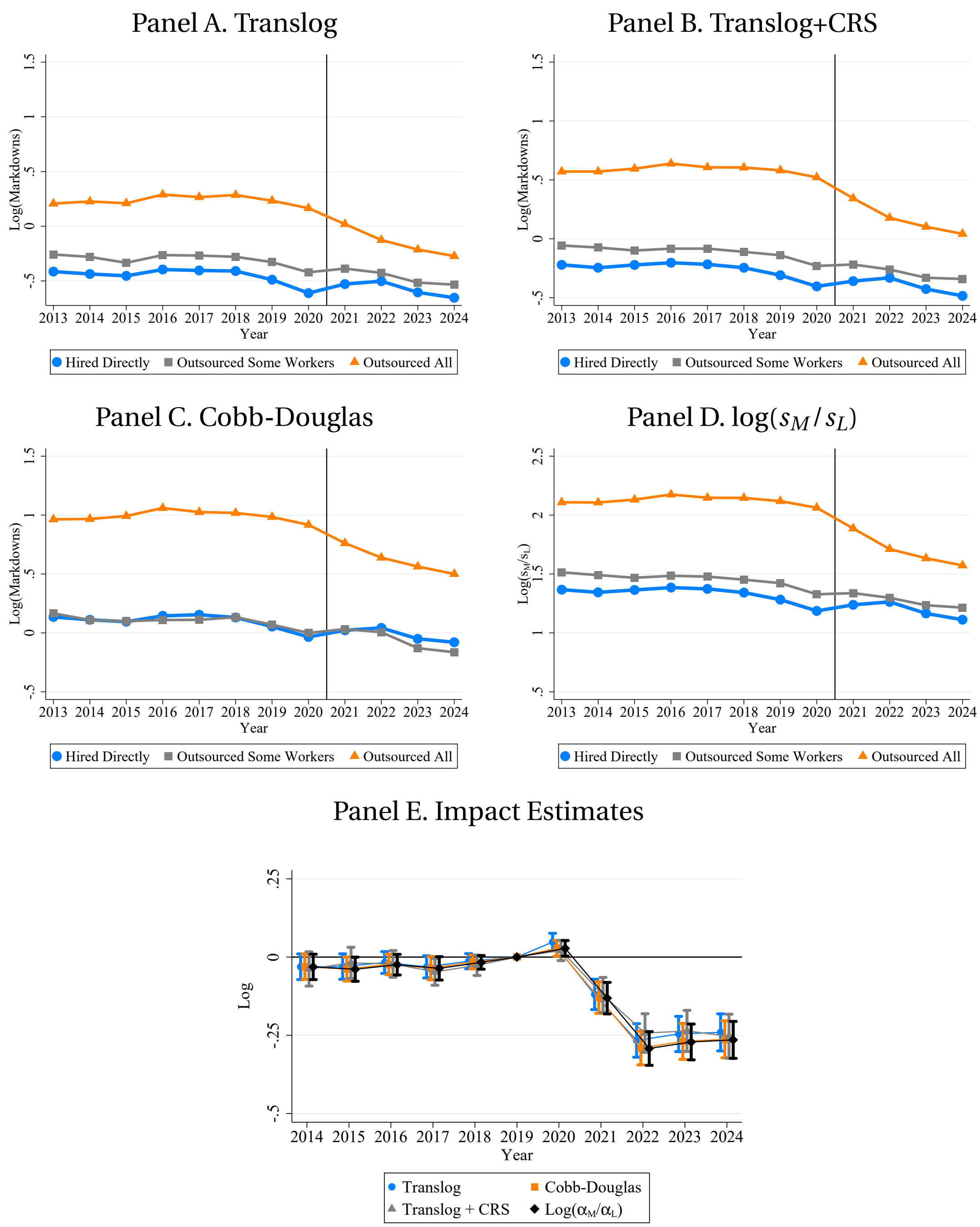
Notes: The sample used for estimation is a balanced panel running from 2013 to 2024, which includes all manufacturing establishments that did not rotate out of the annual manufacturing survey in 2021 and that had not exited the market before the enactment of the reform. Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024.

Table F.11: Impacts of the Reform on Establishment-Level Markdowns: Initial Panel of the Annual Manufacturing Survey

Regressor	Translog	Translog+CRS	Cobb-Douglas	$\log(\alpha_M/\alpha_L)$
	(1)	(2)	(3)	(4)
		Panel A. No Controls		
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2021\}$	-0.13***	-0.12***	-0.13***	-0.13***
	(0.02)	(0.02)	(0.02)	(0.02)
Robust Confidence Set	[-.24,-.008]	[-.23,.008]	[-.22,-.02]	[-.22,-.02]
N	21,980	21,980	21,980	21,980
R^2	0.026	0.028	0.031	0.031
		Panel B. With Flexible Controls		
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2021\}$	-0.13***	-0.14***	-0.13***	-0.13***
	(0.02)	(0.02)	(0.02)	(0.02)
Robust Confidence Set	[-.24,-.03]	[-.24,-.03]	[-.21,-.05]	[-.21,-.05]
N	21,980	21,980	21,980	21,980
R^2	0.055	0.06	0.047	0.047

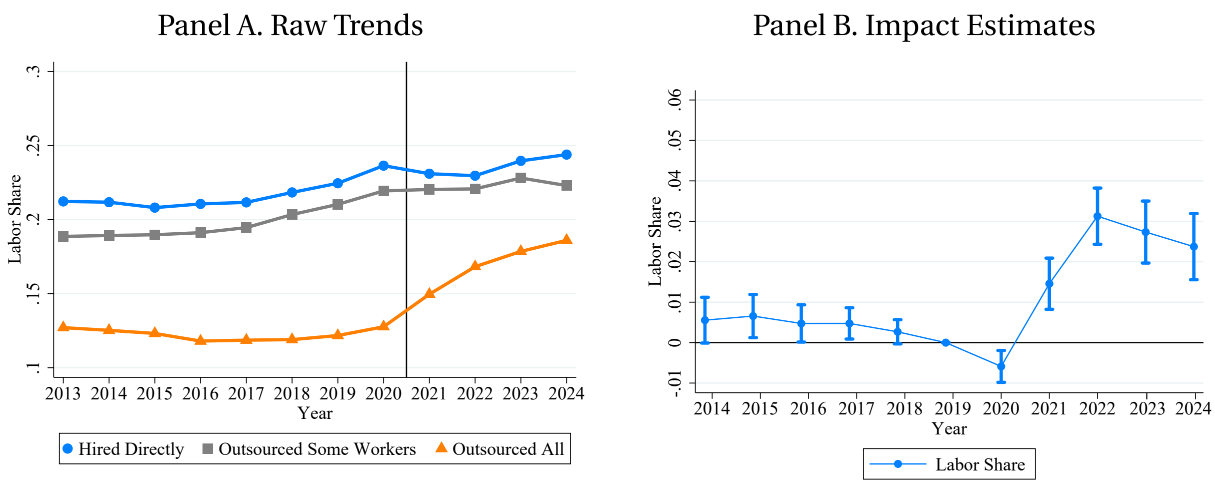
Notes: Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the initial panel of the Mexican annual manufacturing survey from 2013 to 2021. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.21: Reform Impacts on Wage Markdowns



Notes: This figure illustrates the impact on markdowns at the establishment level. Panels A, B, C, and D present raw trends in this outcome under four alternative assumptions about the production function. In each panel, we compare three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel E presents the difference-in-differences estimates for the four outcomes. In all regressions, the interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure F.22: Reform Impacts on the Labor Share



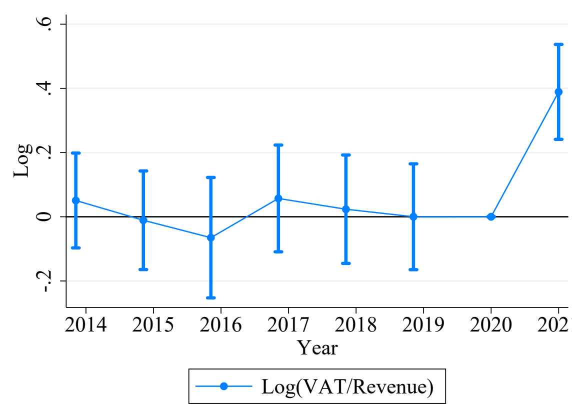
Notes: This figure illustrates the impact of the outsourcing ban on the labor share at the establishment level. In Panel A, we compare the mean labor share of three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel B presents the corresponding difference-in-differences estimates. The interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Table F.12: Impact Heterogeneity by Initial Firm-Level Markdown Percentile
Outcome Variable: Log(Markdowns)

Regressor	(1)	(2)	(3)	(4)	(5)
Outsourcing$_{i,2020} \times \mathbb{1}\{t = 2024\}$	-0.15***	-0.24***	-0.28***	-0.26***	-0.31***
	(0.03)	(0.04)	(0.03)	(0.04)	(0.06)
Interacted with:					
Initial Markdown>75th Percentile	-0.34***				
	(0.06)				
Top-5 Markdown Industry in 2019		-0.05			
		(0.06)			
Central or South Region			0.06		
			(0.06)		
Foreign Ownership				-0.01	
				(0.05)	
Metropolitan Area					0.06
					(0.07)
N	26,919	26,919	26919	26,919	26,919
R^2	0.135	0.122	0.122	0.121	0.122

Notes: Markdown percentiles are defined relative to the pooled distribution of establishments' markdowns in their initial year of appearance in the panel. The estimation sample for the regressions in this table includes only observations of establishments that did not rotate out of the annual manufacturing panel in 2021 and for which a lag of the input variables is available. All regressions control for establishment fixed effects and time dummies, as well as 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. All regressions control for the interacted variables. ***p<0.01.
Source: Authors' elaboration with data from the Mexican economic census waves from 2014 to 2019, which is used to rank industries according to the baseline markdown, and the annual manufacturing survey from 2013 to 2024.

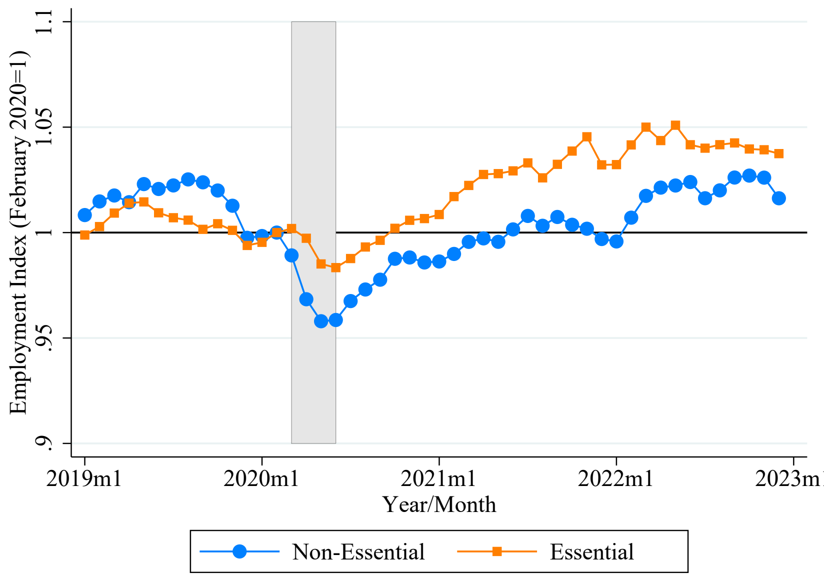
Figure F.23: Reform Impacts on VAT Remitted:
Initial Panel of the Annual Manufacturing Survey



Notes: This figure presents our differences-in-differences coefficient estimates and 95 percent confidence intervals for VAT remitted (debited minus credited) as a share of revenue using the initial panel of the annual manufacturing survey. The interaction for 2020 is excluded from each regression, so the effects can be interpreted as deviations from the outcome mean of the comparison group the year prior to reform.
Source: Authors' elaboration using data from the initial panel of the Mexican annual manufacturing survey from 2014 to 2021. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

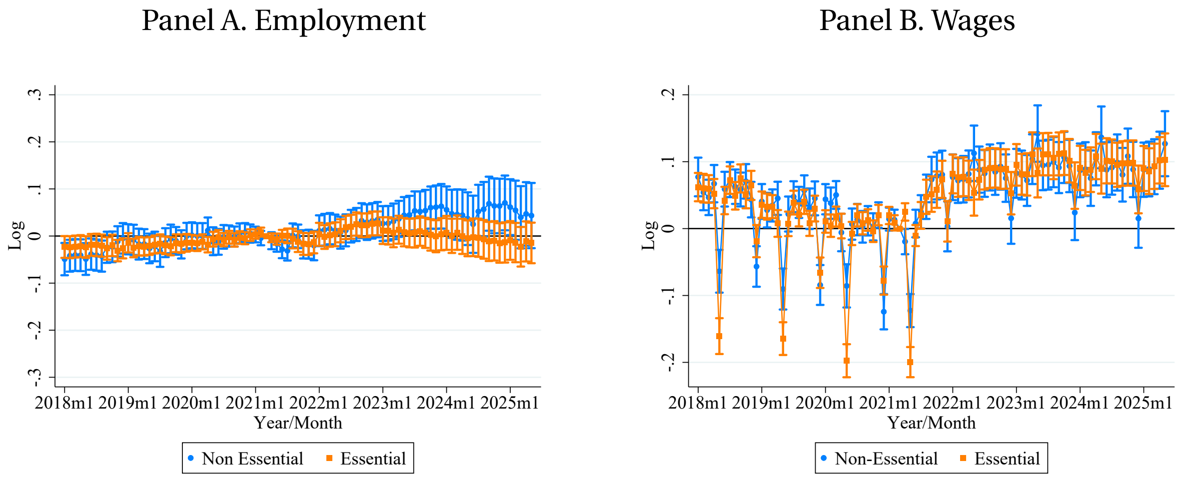
G Differential Impacts by Essential Industry Status During COVID-19

Figure G.1: Employment Recovery by Industry Status During COVID-19



Notes: This figure presents trends in the cross-sectional mean of log employment in the manufacturing sector from 2019 to 2022, by date of return to work following the onset of the COVID-19 pandemic. The indicator "essential" takes the value of 1 if, by government mandate, the economic activities of the establishment were deemed essential to the economy and were therefore allowed to resume in June 2020 and 0 otherwise. The gray area represents the period of the most restrictive lockdown implemented by the federal authorities in Mexico following the onset of the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2019 to 2022.

Figure G.2: Reform Impacts on Log Employment and Log Mean Wage by Industry Status



Notes: This figure presents the results from fully interacting our difference-in-differences specification with an indicator for "essential" establishments, which takes the value of 1 if, by government mandate, the economic activities of the establishment were deemed essential to the economy and were allowed to resume in June 2020 and 0 otherwise. The interaction for March 2021 is excluded from each regression, so the effects can be interpreted as deviations from the outcome mean of the comparison group in the month prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025. Wages are deflated to July 2019 using the intermediate goods subindex of Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

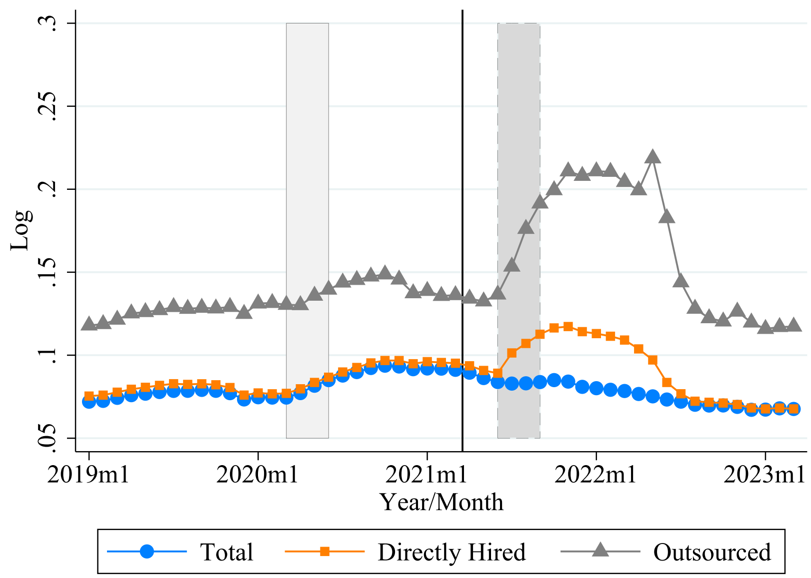
Table G.1: Reform Impacts on Log Employment and Log Mean Wage by Industry Status

Regressor	Employment (1)	Mean Wage (2)
	Panel A. No Controls	
$\text{Nonessential}_i \times \text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t \in 2024\}$	0.05*	0.09***
	(0.03)	(0.02)
$\text{Essential}_i \times \text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t \in 2024\}$	0.00	0.11***
	(0.02)	(0.02)
p-value (H_0: Nonessential=Essential)	0.148	0.472
N	721,806	721,806
R^2	0.013	0.247
	Panel B. With Flexible Controls	
$\text{Nonessential}_i \times \text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t \in 2024\}$	-0.04	0.08***
	(0.03)	(0.02)
$\text{Essential}_i \times \text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t \in 2024\}$	-0.01	0.11***
	(0.02)	(0.02)
p-value (H_0: Nonessential=Essential)	0.445	0.304
N	721,806	721,806
R^2	0.033	0.254

Notes: This table presents the results from fully interacting our difference-in-differences specification with an indicator for "essential" establishments, which takes the value of 1 if, by government mandate, the economic activities of the establishment were deemed essential to the economy and were allowed to resume in June 2020 and 0 otherwise. The set of flexible controls includes indicators for 3-digit industry and the establishment's initial revenue interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025. Wages are deflated to July 2019 using the intermediate goods subindex of Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

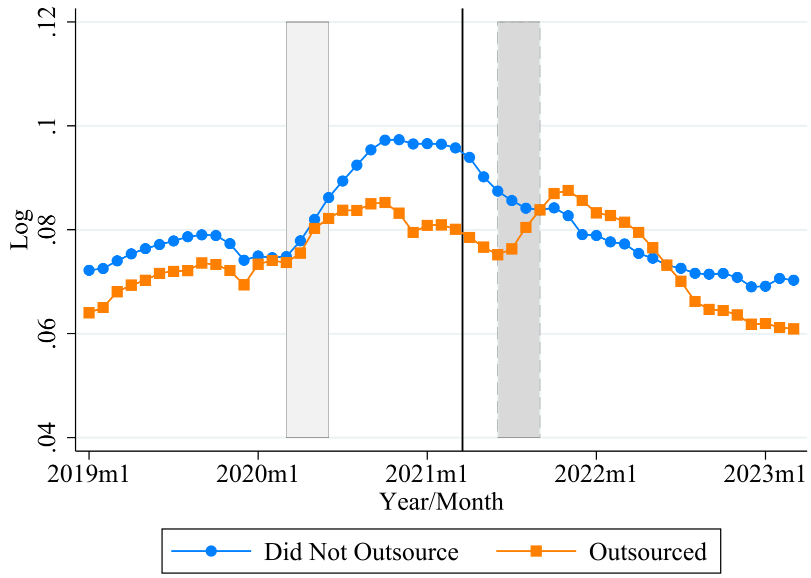
H Reform Impacts on Employment Volatility

Figure H.1: Standard Deviation of Log Employment by Employment Type



Notes: This figure presents trends in the mean volatility of employment in the manufacturing sector by employment type. Our measure of employment volatility is the standard deviation of log employment at the establishment level, which we calculate for each month using data from a 12-month rolling window. The vertical solid line depicts the enactment of the reform, while the vertical dashed line depicts the cutoff date for the transfer of previously outsourced workers to the payroll of the establishment. The first gray area, outlined by a solid line, represents the period of the strictest COVID-19 lockdown implemented by the federal authorities in Mexico. The second gray area, outlined by a dashed line, represents the grace period stipulated by the reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2019 to 2023.

Figure H.2: Trends in the Employment Volatility of Exposed and Nonexposed Establishments

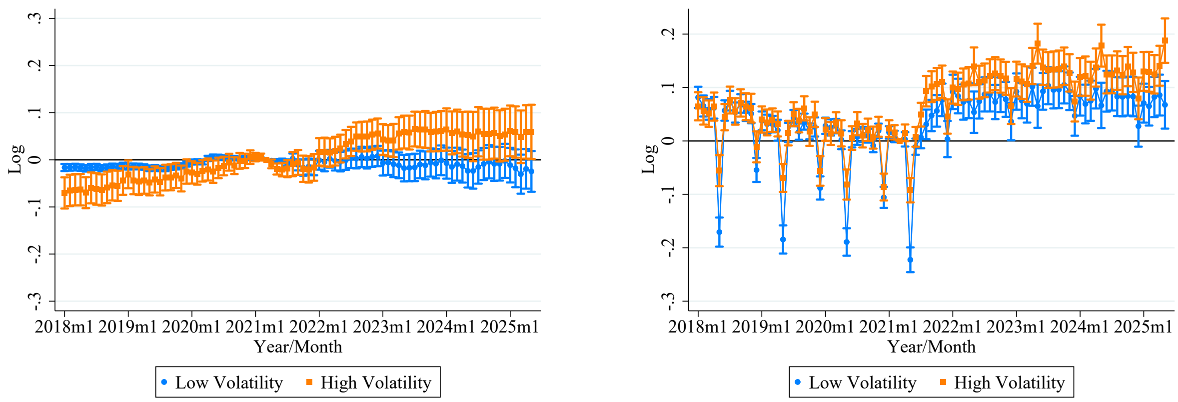


Notes: This figure presents the mean volatility of employment in establishments that outsourced at least one worker and establishments that hired all their workers directly in February 2020, the month prior to the onset of the most restrictive COVID-19 lockdown in Mexico. Our measure of employment volatility is the standard deviation of log employment at the establishment level, which we calculate for each month using data from a 12-month rolling window. The vertical solid line depicts the enactment of the reform, while the vertical dashed line depicts the cutoff date for its enactment. The first gray area, outlined by a solid line, represents the period of the strictest COVID-19 lockdown implemented by the federal authorities in Mexico. The second gray area, outlined by a dashed line, represents the grace period stipulated by the reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2019 to 2023.

Figure H.3: Reform Impacts on Log Employment and Log Mean Wage, by Employment Volatility at Baseline

Panel A. Employment

Panel B. Wages



Notes: This figure presents the results from fully interacting our difference-in-differences strategy with an indicator for high employment volatility, which takes the value of 1 if the pre-reform standard deviation of the establishment's employment is greater than the cross sectional mean and 0 otherwise. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for March 2021 is excluded from each regression, so the effects can be interpreted as deviations from the outcome mean of the comparison group in the month prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025. Wages are deflated to July 2019 using the intermediate goods subindex of Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

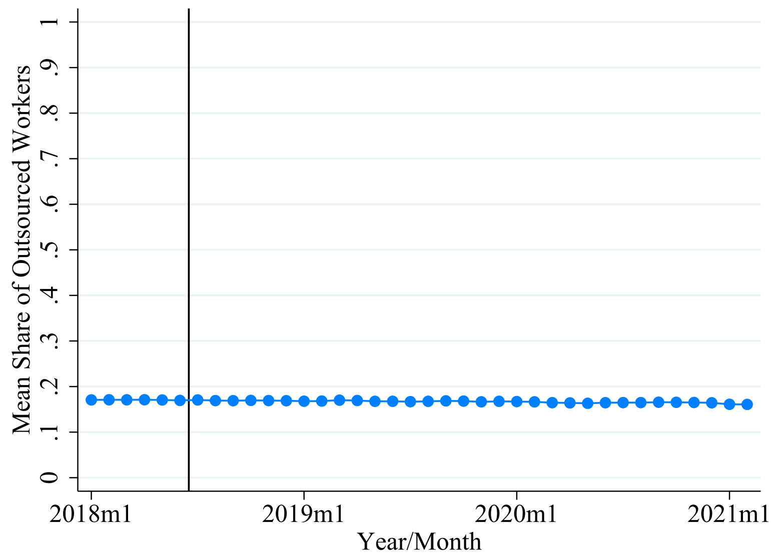
Table H.1: Reform Impacts on Log Employment and Log Mean Wage, by Employment Volatility at Baseline

Regressor	Employment (1)	Mean Wage (2)
	Panel A. No Controls	
Low Volatility$_i$ × Outsourcing$_{i,\text{March 2021}}$ × $\mathbb{1}\{t \in 2024\}$	0.02	0.13***
	(0.01)	(0.02)
High Volatility$_i$ × Outsourcing$_{i,\text{March 2021}}$ × $\mathbb{1}\{t \in 2024\}$	0.06**	0.13***
	(0.03)	(0.02)
p-value (H_0: Low Volatility=High Volatility)	0.149	0.952
N	712,134	712,134
R^2	0.0129	0.253
	Panel B. With Flexible Controls	
Low Volatility$_i$ × Outsourcing$_{i,\text{March 2021}}$ × $\mathbb{1}\{t \in 2024\}$	0.00	0.13***
	(0.02)	(0.02)
High Volatility$_i$ × Outsourcing$_{i,\text{March 2021}}$ × $\mathbb{1}\{t \in 2024\}$	0.01	0.14***
	(0.03)	(0.02)
p-value (H_0: Low Volatility=High Volatility)	0.596	0.739
N	712,134	712,134
R^2	0.035	0.261

Notes: This table presents the results from fully interacting our difference-in-differences specification with an indicator for high employment volatility, which takes the value of 1 if the standard deviation of the pre-reform time series of employment of the establishment is greater than the cross-sectional mean and 0 otherwise. The set of flexible controls includes indicators for 3-digit industry and the establishment's initial revenue interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025. Wages are deflated to July 2019 using the intermediate goods subindex of Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

I Anticipation Effects

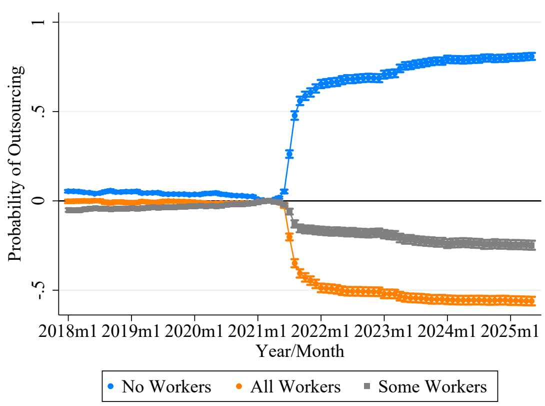
Figure I.1: Mean Share of Outsourced Workers Before and After the Election



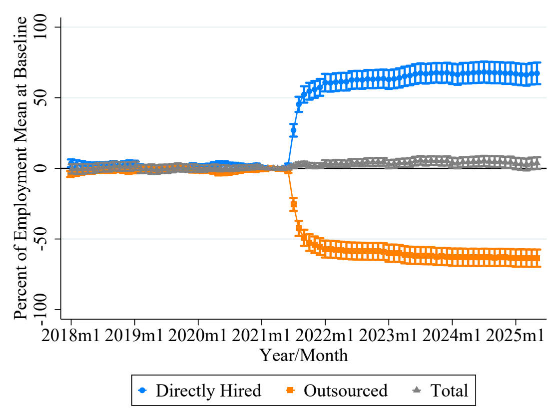
Notes: This figure presents the cross-sectional mean share of outsourced workers at the establishment level. The vertical black line depicts the election of the new government in July 2018.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2021.

Figure I.2: Reform Impacts on Outsourcing Prevalence and Employment
(Excluding the Ever Treated from the Control Group)

Panel A. Outsourcing Prevalence



Panel B. Employment



Notes: This figure presents the results from our differences-in-differences specification for the prevalence of outsourcing and employment by hiring modality. The sample used in estimation uses only the establishments that directly hired all their workers before the election as control group. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for March 2021 is excluded from each regression, so the effects can be interpreted as deviations from the outcome mean of the comparison group in the month prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025.

Table I.1: Reform Impacts on Establishment-Level Outsourcing
(Excluding the Ever Treated from the Control Group)

Regressor	All Workers	Some Workers	No Workers
	(1)	(2)	(3)
		Panel A. No Controls	
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t = \text{December 2024}\}$	-0.56***	-0.24***	0.80***
	(0.01)	(0.01)	(0.01)
Robust Confidence Set	[-.62,-.49]	[-.34,-.15]	[.67,.93]
N	702,659	702,659	702,659
R^2	0.453	0.087	0.552
		Panel B. With Flexible Controls	
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t = \text{December 2024}\}$	-0.55***	-0.25***	0.80***
	(0.01)	(0.01)	(0.01)
Robust Confidence Set	[-.65,-.45]	[-.35,-.16]	[.63,.96]
N	702,659	702,659	702,659
R^2	0.463	0.096	0.556
Mean for the Treated in March 2021	0.13	0.08	0.79

Notes: The sample used in estimation uses only establishments that directly hired all their workers before the election as control group. The set of flexible controls includes indicators for 3-digit industry and the establishment's initial revenue interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025.

Table I.2: Reform Impacts on Establishment-Level Employment
(Excluding the Ever Treated from the Control Group)

Regressor	Directly Hired	Outsourced	Total
	(1)	(2)	(3)
		Panel A. No Controls	
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t = \text{December 2024}\}$	67.5***	-63.1***	4.3**
	(3.9)	(3.1)	(2.1)
Robust Confidence Set	[52.7,82.9]	[-74.6,-53]	[-7.3,16.5]
N	702,659	702,659	702,659
R^2	0.062	0.182	0.001
		Panel B. With Flexible Controls	
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t = \text{December 2024}\}$	60.5***	-60.9***	-0.4
	(3.8)	(3.0)	(2.2)
Robust Confidence Set	[42.9,79.1]	[-78.7,-45]	[-15.8,16.3]
N	702,659	702,659	702,659
R^2	0.076	0.2	0.011
Mean for the Treated in March 2021	85.1	14.6	99.7

Notes: The sample used in estimation uses only establishments that directly hired all their workers before the election as control group. Outcomes are expressed relative to the mean total employment of each group in March 2021. The set of flexible controls includes indicators for 3-digit industry and the establishment's initial revenue interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. **p<0.05, ***p<0.01.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2025.

J Placebo Reforms

Table J.1: The "Causal Impacts" of Placebo Reforms on Employment

Regressor	Directly Hired	Outsourced	Total
	(1)	(2)	(3)
		Panel A. Original Estimates	
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	72***	-67***	5.0**
	(4.3)	(3.2)	(2.4)
Robust Confidence Set	[36,109]	[-98,-38]	[-15,26]
N	42,826	42,826	42,826
R^2	.176	.205	.059
		Panel B. Placebo 2016	
$\text{Outsourcing}_{i,2015} \times \mathbb{1}\{t = 2019\}$	-0.93	3.0	2.1
	(2.3)	(2.0)	(2.5)
Robust Confidence Set	[-35,33]	[-28,36]	[-8,12]
N	24,691	24,691	24,691
R^2	.05	.037	.078
		Panel C. Placebo 2017	
$\text{Outsourcing}_{i,2016} \times \mathbb{1}\{t = 2019\}$	1.8	2.1	3.8
	(2.3)	(1.8)	(2.4)
Robust Confidence Set	[-27,31]	[-25,30]	[-5,13]
N	24,768	24,768	24,768
R^2	.056	.048	.079
		Panel D. Placebo 2018	
$\text{Outsourcing}_{i,2017} \times \mathbb{1}\{t = 2019\}$	2.0	0.7	2.6
	(2.2)	(1.3)	(1.9)
Robust Confidence Set	[-18,22]	[-17,19]	[-3,9]
N	24,855	24,855	24,855
R^2	.064	.056	.081
		Panel E. Placebo 2019	
$\text{Outsourcing}_{i,2018} \times \mathbb{1}\{t = 2019\}$	-0.8	-0.2	-1.0
	(1.1)	(0.8)	(1.2)
Robust Confidence Set	[-11,9]	[-9,9]	[-5,3]
N	24,806	24,806	24,806
R^2	.072	.064	.081

Notes: Placebo regressions are estimated using only pre-reform data, restricting the sample to the period up to 2019. The set of flexible controls consists of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ** p<0.05, ***p<0.01.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024.

Table J.2: The "Causal Impacts" of Placebo Reforms on Log Employment and Log Mean Wage

Regressor	Total Emploment	Mean Wage
	(1)	(2)
	Panel A. Original Estimates	
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	0.02	0.28***
	(0.02)	(0.01)
Robust Confidence Set	[-.12,.17]	[.14,.42]
N	42,826	42,826
R^2	.051	.227
	Panel B. Placebo 2016	
$\text{Outsourcing}_{i,2015} \times \mathbb{1}\{t = 2019\}$	-0.02	0.05***
	(0.01)	(0.01)
Robust Confidence Set	[-.074,.04]	[-.08,.18]
N	24,691	24,691
R^2	.075	.067
	Panel C. Placebo 2017	
$\text{Outsourcing}_{i,2016} \times \mathbb{1}\{t = 2019\}$	-0.00	0.03***
	(0.01)	(0.01)
Robust Confidence Set	[-.09,.08]	[-.06,.12]
N	24,768	24,768
R^2	.077	.067
	Panel D. Placebo 2018	
$\text{Outsourcing}_{i,2017} \times \mathbb{1}\{t = 2019\}$	-0.01	0.02***
	(0.01)	(0.01)
Robust Confidence Set	[-.06,.05]	[-.04,.08]
N	24,855	24,855
R^2	.076	.069
	Panel E. Placebo 2019	
$\text{Outsourcing}_{i,2018} \times \mathbb{1}\{t = 2019\}$	-0.01	0.01
	(0.01)	(0.00)
Robust Confidence Set	[-.05,.03]	[-.02,.04]
N	24,806	24,806
R^2	.077	.071

Notes: Placebo regressions are estimated using only pre-reform data, restricting the sample to the period up to 2019. The set of flexible controls consists of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. * p<0.1, ** p<0.05, ***p<0.01.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using their corresponding sub-index of Mexico's GDP deflator, or Índice Nacional de Precios al Productor (INPP).

Table J.3: The "Causal Impacts" of Placebo Reforms on Labor Costs and Profits

Regressor	Labor Cost			Profit Margin
	Total	Wage Bill	Staffing Fee	
	(1)	(2)	(3)	(4)
	Panel A. Original Estimates			
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	-2.5	22.1***	-24.6***	0.0
	(3.1)	(3.4)	(1.4)	(0.01)
Robust Confidence Set	[-25,23]	[-3.7,49]	[-41,-9.2]	[-.06,.07]
N	42,826	42,826	42,826	42,826
R^2	.139	.161	.162	.021
	Panel B. Placebo 2016			
$\text{Outsourcing}_{i,2015} \times \mathbb{1}\{t = 2019\}$	5.6*	4.2*	1.3	0.00
	(2.9)	(2.3)	(1.0)	(0.01)
Robust Confidence Set	[-14,25]	[-25,33]	[-19,21]	[-.05,.06]
N	24,691	24,691	24,691	24,691
R^2	.144	.167	.036	.024
	Panel C. Placebo 2017			
$\text{Outsourcing}_{i,2016} \times \mathbb{1}\{t = 2019\}$	4.4*	1.9	2.5***	0.01
	(2.5)	(2.1)	(0.8)	(0.01)
Robust Confidence Set	[-11,20]	[-20,24]	[-13,18]	[-.03,.06]
N	24,768	24,768	24,768	24,768
R^2	.146	.169	.044	.024
	Panel D. Placebo 2018			
$\text{Outsourcing}_{i,2017} \times \mathbb{1}\{t = 2019\}$	2.0	0.6	1.4**	0.01**
	(1.8)	(1.6)	(0.6)	(0.01)
Robust Confidence Set	[-6.9,11]	[-13,13]	[-8.4,11]	[-.02,.05]
N	24,855	24,855	24,855	24,855
R^2	.148	.173	.049	.024
	Panel D. Placebo 2019			
$\text{Outsourcing}_{i,2018} \times \mathbb{1}\{t = 2019\}$	-0.5	-1.9*	1.4***	0.01
	(1.1)	(1.0)	(0.3)	(0.00)
Robust Confidence Set	[-5.5,4.1]	[-8.9,4.9]	[-3.3,6.3]	[-.008,.03]
N	24,806	24,806	24,806	24,806
R^2	.149	.174	.055	.025

Notes: Placebo regressions are estimated using only pre-reform data, restricting the sample to the period up to 2019. Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. * p<0.1, ** p<0.05, ***p<0.01.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using their corresponding sub-index of Mexico's GDP deflator, or Índice Nacional de Precios al Productor (INPP).

Table J.4: The "Causal Impacts" of Placebo Outsourcing Reforms on Markdowns

Regressor	Labor Share	Translog	Translog+RTS	Cobb-Douglas	Log(s_M/s_L)
	(1)	(2)	(3)	(4)	
			Panel A. Original Estimates		
Outsourcing$_{i,2020} \times \mathbb{1}\{t=2024\}$	0.03***	-0.26***	-0.27***	-0.28***	-0.28***
	(0.00)	(0.03)	(0.04)	(0.03)	(0.03)
Robust Confidence Set	[.004,.06]	[-.49,-.02]	[-.52,-.01]	[-.52,-.05]	[-.51,-.05]
N	26919	26,919	26,919	26,919	26,919
R^2	.135	.121	0.092	.132	.131
			Panel B. Placebo 2016		
Outsourcing$_{i,2015} \times \mathbb{1}\{t=2019\}$	0.01**	-0.08***	-0.07***	-0.06***	-0.06***
	(0.00)	(0.02)	(0.03)	(0.02)	(0.02)
Robust Confidence Set	[-.02,.04]	[-.32,.17]	[-.30,.15]	[-.30,.17]	[-.30,.17]
N	15,333	15,333	15,333	15,333	15,333
R^2	0.039	0.053	0.052	0.035	0.035
			Panel C. Placebo 2017		
Outsourcing$_{i,2016} \times \mathbb{1}\{t=2019\}$	0.00	-0.04**	-0.04*	-0.04**	-0.04**
	(0.00)	(0.02)	(0.02)	(0.02)	(0.02)
Robust Confidence Set	[-.02,.03]	[-.24,.15]	[-.26,.16]	[-.23,.15]	[-.23,.15]
N	15,484	15,484	15,484	15,484	15,484
R^2	.0386	.0499	.0523	.0349	.0348
			Panel D. Placebo 2018		
Outsourcing$_{i,2017} \times \mathbb{1}\{t=2019\}$	0.00	-0.01	0.00	-0.00	-0.00
	(0.00)	(0.02)	(0.02)	(0.02)	(0.02)
Robust Confidence Set	[-.02,.02]	[-.15,.13]	[-.13,.13]	[-.13,.13]	[-.13,.13]
N	15,534	15,534	15,534	15,534	15,534
R^2	.04	0.05	0.052	0.036	0.036
			Panel E. Placebo 2019		
Outsourcing$_{i,2018} \times \mathbb{1}\{t=2019\}$	0.00	-0.03***	-0.01	-0.02*	-0.02*
	(0.00)	(0.01)	(0.02)	(0.01)	(0.01)
Robust Confidence Set	[-.007,.01]	[-.10,.04]	[-.08,.07]	[-.09,.04]	[-.09,.04]
N	15,553	15,553	15,553	15,553	15,553
R^2	0.041	.055	0.055	0.041	0.041

Notes: Placebo regressions are estimated using only pre-reform data, restricting the sample to the period up to 2019. The sample used for estimation includes only observations of establishments that did not rotate out of the annual manufacturing panel in 2021 and for which a lag of the input variables is available. We exclude all expenses other than capital, raw materials, energy, and labor in the calculation of the labor share. Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. * p<0.1, ** p<0.05, ***p<0.01.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using their corresponding sub-index of Mexico's GDP deflator, or Índice Nacional de Precios al Productor (INPP).

Table J.5: The "Causal Impacts" of a Placebo Reform on Wage Composition

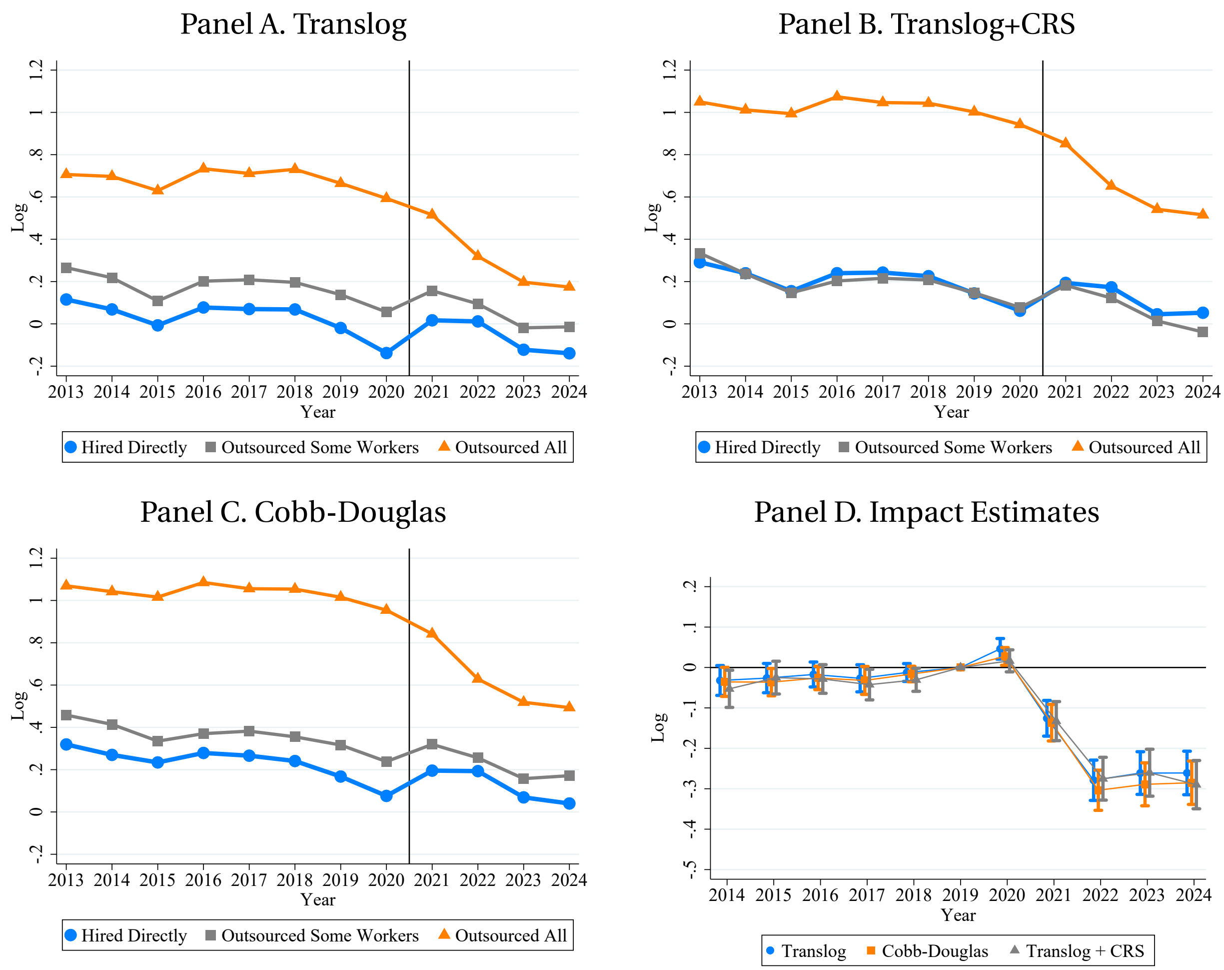
Regressor	Mean Wages				Firing Costs
	Total	Salaries & Benefits	Social Security	Profit Sharing	
	(1)	(2)	(3)	(4)	(5)
	Panel A. Original Estimates				
$\text{Outsourcing}_{i,\text{March 2021}} \times \mathbb{1}\{t \in 2024\}$	16.4***	7.1***	7.4***	3.0***	0.9***
	(1.9)	(1.7)	(0.4)	(0.3)	(0.2)
Robust Confidence Set	[-6.1,39.6]	[-12,27.3]	[2.5,12.3]	[-0.7,6.8]	[-3.5,5.4]
N	721,806	721,806	721,806	721,806	721,806
R^2	0.073	0.066	0.11	0.05	0.005
	Panel B. Placebo Reform in January 2019				
$\text{Outsourcing}_{i,\text{December 2018}} \times \mathbb{1}\{t \in 2019\}$	-0.9	-1.2**	-0.1	0.4	-0.4***
	(0.8)	(0.5)	(0.1)	(0.4)	(0.1)
N	197,594	197,594	197,594	197,594	197,594
R^2	0.049	0.083	0.022	0.026	0.002

Notes: Placebo regressions are estimated using only pre-reform data, restricting the sample to the period up to December 2019. Outcomes in Columns (1) through (4) are expressed relative to the mean wage of each group in March 2021. Firing costs in Column (5) are expressed relative to the mean wage bill of each group in March 2021. The set of flexible controls includes indicators for 3-digit industry and the establishment's initial revenue interacted with time dummies. All regressions control for establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ** p<0.05, ***p<0.01.

Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from January 2018 to May 2025. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or Índice Nacional de Precios al Productor (INPP).

K Alternative Markdown Measures

Figure K.1: Reform Impacts on the Ratio of Labor Elasticity to Labor Share



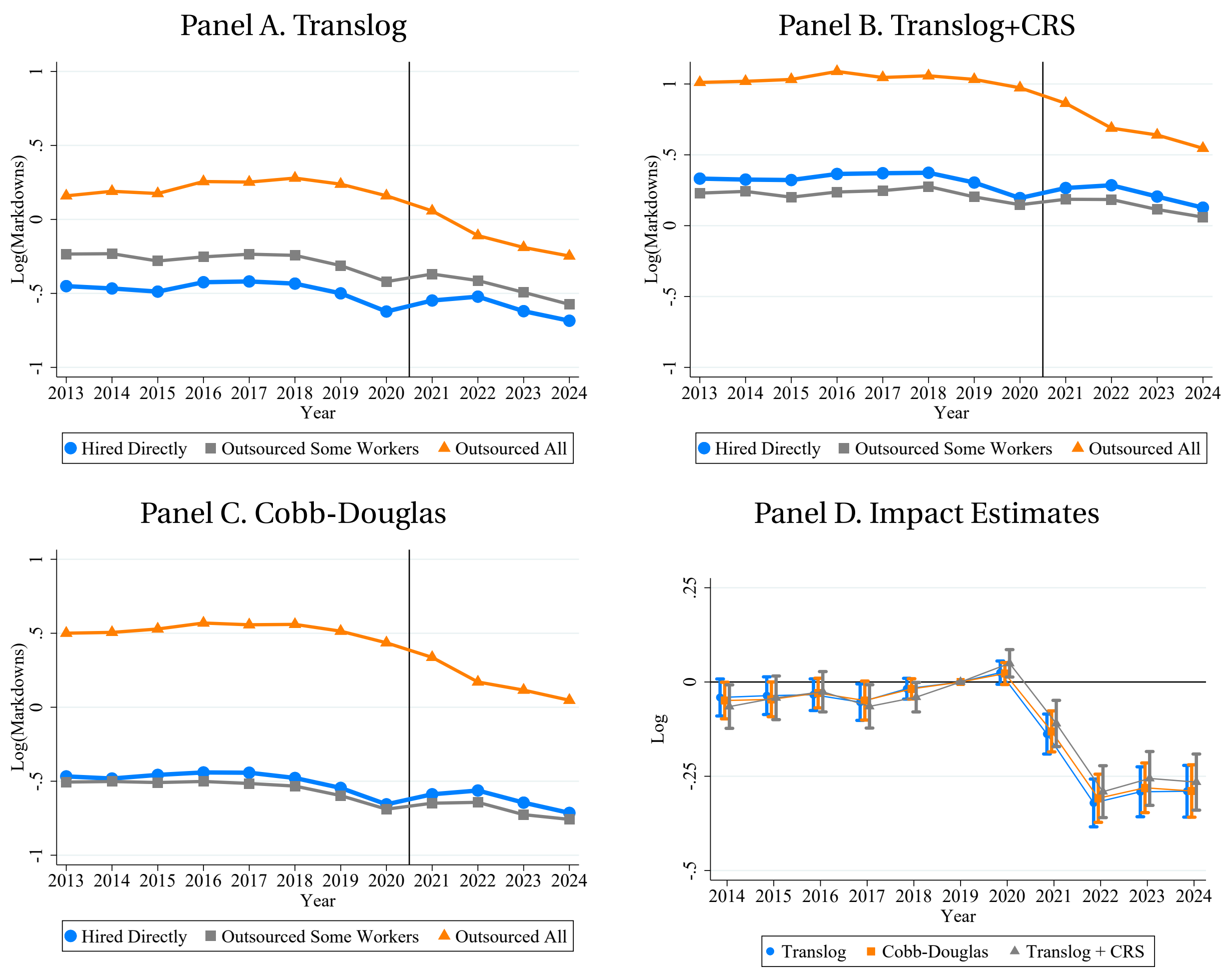
Notes: This figure illustrates the impact of the outsourcing ban on the ratio of labor elasticity to the labor share at the establishment level. Panels A, B, and C present raw trends in this ratio under three alternative assumptions about the production function. In each panel, we compare three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel D presents the difference-in-differences estimates for the three outcomes. In all regressions, the interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Table K.1: Effects of the Reform on the Log Ratio of Labor Elasticity to Labor Share at the Establishment Level

Regressor	Translog (1)	Translog+CRS (2)	Cobb–Douglas (3)
		Panel A. No Flexible Controls	
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	-0.23***	-0.26***	-0.26***
	‘(0.03)	‘(0.03)	‘(0.03)
Robust Confidence Set	[-.63,.18]	[-.58,.068]	[-.56,.054]
N	26,919	26,919	26,919
R^2	0.113	0.084	0.13
		Panel B. With Flexible Controls	
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	-0.26***	-0.29***	-0.29***
	‘(0.03)	‘(0.03)	‘(0.03)
Robust Confidence Set	[-.43,-.085]	[-.49,-.032]	[-.46,-.12]
N	26,919	26,919	26,919
R^2	0.113	0.084	0.13

Notes: The sample used for estimation includes only observations of establishments that did not rotate out of the annual manufacturing panel in 2021 and for which a lag of the input variables is available. We exclude all expenses other than capital, raw materials, energy, and labor in the calculation of the labor share. Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Figure K.2: Reform Impacts on Markdowns Estimated Under the Assumption of Cournot Competition: Annual Manufacturing Survey



Notes: This figure illustrates the impact on markdowns at the establishment level estimated under the assumption of Cournot competition. Panels A, B, and C present raw trends in this outcome under three alternative assumptions about the production function. In each panel, we compare three groups of establishments: (i) establishments that did not outsource any workers in the month prior to the reform, (ii) establishments that outsourced at least one worker, but not all workers, and (iii) establishments that outsourced all workers. The vertical solid line marks the enactment of the reform. Panel D presents the difference-in-differences estimates for the three outcomes. In all regressions, the interaction for 2019 is omitted, so the estimated effects are interpreted relative to the mean outcome of the comparison group in the year before the COVID-19 pandemic.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

Table K.2: Reform Impacts on Establishment-Level Log Markdowns in Cournot Competition

Regressor	Translog (1)	Translog+CRS (2)	Cobb–Douglas (3)
		Panel A. No Controls	
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	-0.29***	-0.27***	-0.29***
	(0.03)	(0.04)	(0.04)
Robust Confidence Set	[-.57,-.01]	[-.6,.09]	[-.56,-.007]
N	20,665	20,665	20,665
R^2	0.096	0.07	0.103
		Panel B. With Flexible Controls	
$\text{Outsourcing}_{i,2020} \times \mathbb{1}\{t = 2024\}$	-0.31***	-0.31***	-0.32***
	(0.04)	(0.04)	(0.04)
Robust Confidence Set	[-.65,.02]	[-.66,.07]	[-.63,.005]
N	20,665	20,665	20,665
R^2	0.132	0.124	0.128

Notes: The sample used for estimation includes only observations of establishments that did not rotate out of the annual manufacturing panel in 2021 and for which a lag of the input variables is available. We exclude all expenses other than capital, raw materials, energy, and labor in the calculation of the labor share. Flexible controls consist of 3-digit industry indicators and the initial revenue and productivity of the establishment interacted with time dummies. All regressions include establishment fixed effects and time dummies. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. ***p<0.01.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *índice nacional de precios al productor* (INPP).

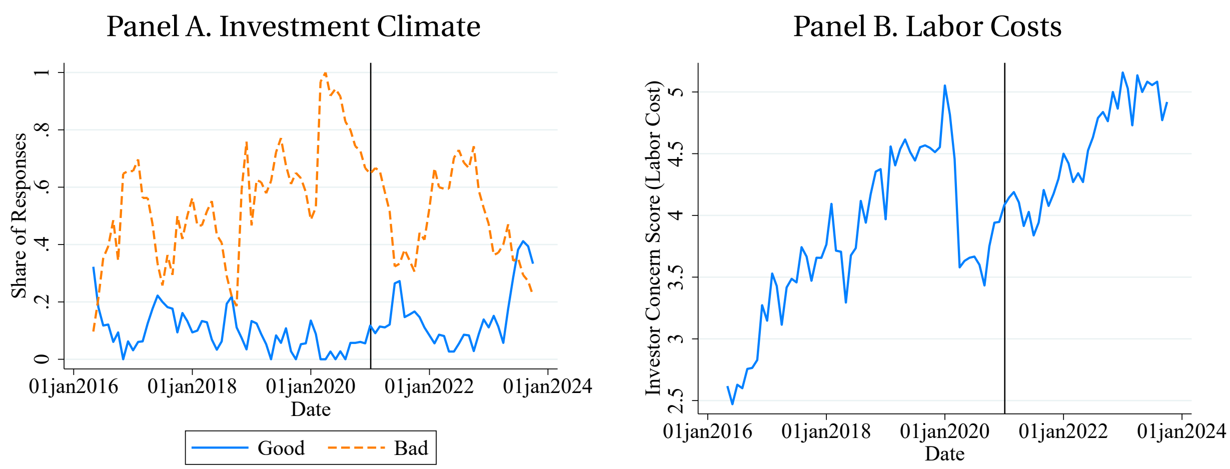
L Reform Effects on Investment Perspectives

To formally examine the reform's impacts on investors' appetite for investing in Mexico, we utilize monthly data from the central bank's private-sector perceptions survey, the *Encuesta sobre las expectativas de los especialistas en economía del sector privado* (EEEESP) from 2016 to 2023. This survey interviews between 30 and 97 private-sector analysts from national and international commercial banks and economic consulting groups every month about the business outlook for Mexico, including their expectations for future inflation, GDP growth, interest and exchange rates, the balance of payments, and the general investment environment. We test whether the reform led to a structural break in interviewees' answers to the following questions: "How would you rate the current business environment for investors to make new investments in Mexico (Good, Bad, Not Sure)?" and "In the next six months, how much of a constraint do labor costs present on the growth of economic activity in Mexico on a scale of 1 to 7?" Figure L.1 depicts the time series of the interviewees' mean responses to these questions. While both time series are nonstationary, we cannot visually detect a structural break in any of them. Nonetheless, we formally test for a structural break after the reform, which was enacted in April 2021, by estimating the parameter β of the following regression model via OLS:

$$\Delta Y_t = \alpha + \mathbb{1}_{\{t>\text{April 2021}\}}\beta + \varepsilon_t, \tag{L1}$$

where Y_t is the interviewees' mean response to the question of interest at time t and ε_t is an error term. Standard errors are robust to heteroskedasticity of unknown form and to autocorrelation of up to one lag, estimated by means of the methodology in Newey and West (1987). Table L.1 shows the results of this exercise. We find no evidence of structural breaks in interviewees' perceptions about the investment environment or the burden of labor costs after the reform.

Figure L.1: Investor Perceptions of Investment Climate in Mexico



Notes: This figure presents the average responses of national and international economic analysts and consultants from the private sector to two monthly questions regarding the investment climate and labor costs in Mexico from 2016 to 2023. Panel A depicts the share of respondents who answered "Good" or "Bad" to the question "How would you rate the current business environment for investors to make new investments in Mexico (Good, Bad, Not Sure)?" Panel B depicts the average response to the question "In the next six months, how much of a constraint do labor costs present on the growth of economic activity in Mexico on a scale of 1 to 7?" The vertical black line in each panel represents April 24, 2021, the enactment date of the domestic outsourcing reform.
Source: Authors' elaboration using data from the Mexican central bank's private-sector perceptions survey.

Table L.1: Tests for Structural Breaks in Survey Responses after the Outsourcing Reform

Regressor	Investment Climate		Labor Cost
	Good	Bad	
	(1)	(2)	(3)
$\mathbb{1}_{\{t>\text{April 2021}\}}$	0.011	-0.020	0.002
	(0.013)	(0.022)	(0.039)
N	89	89	89
Outcome Mean	0.113	0.535	4.068

Notes: Standard errors are robust to heteroskedasticity of unknown form and autocorrelation of up to one lag.
Source: Authors' elaboration using data from the Mexican central bank's private-sector perceptions survey from May 2016 to October 2023.

M Reform Effects on Local Firm Dynamics

While our headline results indicate that the reform did not reduce employment in exposed establishments, the concern remains that it may have had perverse general equilibrium effects, for example, by reducing local economic dynamism. Concretely, the reform may have affected aggregate employment through a reduction in market entry. To test this possibility, we use data from the three most recent waves of the Economic Census, which comprise the universe of manufacturing establishments in Mexico. The 2019 wave provides an accurate snapshot of the period preceding the enactment of the reform, while the 2024 wave provides an accurate snapshot of the medium-term effects of the reform, three years after its implementation. The 2009 wave enables the construction of lagged control variables. For the analysis, we collapse the data at the commuting-zone level using the commuting zones defined by Busso, Fentanes and Levy (2018). Our sample is limited to 1,969 commuting zones that had manufacturing establishments in 2014, 2019, and 2024.

For each census wave, we construct three variables that are standard in the study of firm dynamics: the market entry rate, defined as the ratio of the number of new establishments to the total establishment count; the exit rate, defined as the ratio of the number of establishments that exited the market in the current wave to the number of establishments operating in the previous wave; and the total establishment count in each wave. If the reform reduced total employment through a general equilibrium effect on market dynamism, we would expect local labor markets where firms relied more heavily on outsourcing to experience a decline in entry, an increase in exit, and a reduction in the total establishment count.

We regress the quinquennial change in the outcome of commuting zone ℓ from year $t-1$ to year t, denoted by $\Delta Y_{\ell t}$, on the change in the outsourcing share of employment over the same period, defined as $\Delta S_{\ell t} = (\text{Outsourced}_{\ell t} - \text{Outsourced}_{\ell,t-1})/\text{Employed}_{\ell,t-1}$, as follows:

$$\Delta Y_{lt} = \alpha + \beta \Delta S_{lt} + X_{lt}\delta + \varepsilon_{lt}. \tag{M1}$$

where X_{lt} is a vector of controls that includes the lagged outcome. Standard errors are robust to heteroskedasticity of unknown form. This is the standard shift-share specification used in leading studies investigating local labor market outcomes in the labor economics literature (Autor, Dorn and Hanson, 2013).

While the national decline in outsourced employment between 2019 and 2024 is a direct consequence of the ban, changes in local economic conditions may confound the magnitude of this decline at the commuting-zone level. As a result, OLS estimates may be contaminated by omitted variable bias. To address this concern, we construct a standard shift-share instrument that interacts the baseline industrial composition of the local labor market with nationwide changes in outsourced employment by industry, which are driven by the reform, as follows:

$$\Delta R_{\ell t} = \sum_j \left(\frac{\text{Outsourced}_{\ell,j,t-1}}{\text{Outsourced}_{j,t-1}} \right) \times \left(\frac{\Delta\text{Outsourced}_{-\ell,j,t}}{\text{Employed}_{\ell,t-1}} \right), \tag{M2}$$

where $\text{Outsourced}_{\ell,j,t-1}$ is the number of outsourced workers in three-digit manufacturing industry j in commuting zone ℓ at time $t-1$, $\text{Outsourced}_{j,t-1}$ is the total number of outsourced workers in industry j at time $t-1$, and $\Delta\text{Outsourced}_{-\ell,j,t}$ is the leave-one-out quinquennial change in outsourced employment in industry j between $t-1$ and t.

We present the first-stage regression results in Table M.1. The coefficient in Column (1) indicates that a predicted reduction of 1 percentage point in the outsourcing share of employment is associated with an almost equally sized reduction of 1.05 percentage points, consistent with effective enforcement of the reform. Moreover, this relationship is highly statistically significant ($t = 105$), ruling out concerns about weak instruments. Relatedly, the high R^2 suggests that the majority of the variation in outsourcing between 2019 and 2024 is driven by national conditions captured by the instrument rather than by local economic shocks. Finally, the coefficient estimate remains stable with the progressive inclusion of control variables, as shown in Columns (2) through (5).

We then present the second-stage regression results and compare them to ordinary least squares estimates in Table M.2. We find no statistically significant reduction in either the probability of market entry or the total establishment count at the commuting-zone level, indicating that general equilibrium effects—while theoretically plausible—did not generate detectable declines in aggregate employment through reduced economic dynamism.

Table M.1: Reform Impacts on Firm Dynamics at the Commuting-Zone Level: First Stage

Regressor	(1)	(2)	(3)	(4)	(5)
$\Delta R_{\ell t}$	1.05***	1.05***	1.05***	1.05***	1.05***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
State Dummies and Urban Area Indicator	No	Yes	Yes	Yes	Yes
Local Establishment Count and Total Revenue	No	No	Yes	Yes	Yes
Informality Share	No	No	No	Yes	Yes
Firm Entry and Exit Rates	No	No	No	No	Yes
N	1,969	1,969	1,969	1,969	1,969
R^2	0.963	0.963	0.964	0.964	0.964

Notes: The outcome variable is the change in the outsourcing share of employment from 2019 to 2024 at the commuting-zone level. Time-varying controls are lagged first-differences from 2014 to 2019. Standard errors are robust to heteroskedasticity of unknown form. Effects are expressed in percentage points. ***p<0.01.
Source: Authors' elaboration using data from economic census waves of 2014, 2019 and 2024.

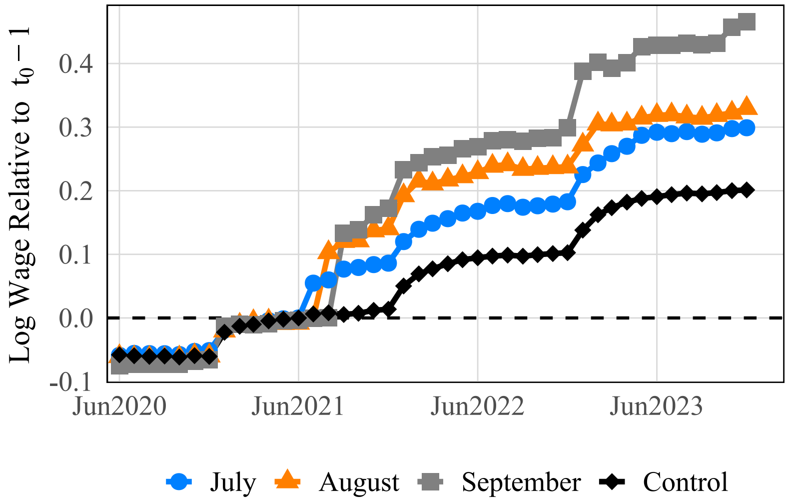
Table M.2: Reform Impacts on Firm Dynamics at the Commuting-Zone Level: Second Stage

Regressor	Entry Rate	Exit Rate	Log Establishment Count
	(1)	(2)	(3)
		Panel A. Ordinary Least Squares	
$\Delta S_{\ell t}$	0.06	0.01	0.05
	(0.04)	(0.02)	(0.06)
N	1969	1969	1969
R^2	0.347	0.255	0.134
		Panel B. Two-Stages Least Squares	
$\Delta S_{\ell t}$	0.04	0.02	0.03
	(0.04)	(0.02)	(0.05)
N	1,969	1,969	1,969
R^2	0.346	0.255	0.13

Notes: All time-varying controls are first-differences from 2014 to 2019. Standard errors are robust to heteroskedasticity of unknown form. Effects are expressed in percentage points.
Source: Authors' elaboration using data from economic census waves of 2014, 2019 and 2024.

N Additional Worker-Level Figures and Tables

Figure N.1: Long-Term Raw Wage Trends by Treatment Cohort



Notes: This figure shows the raw trend in mean log wages for each treatment cohort, with values normalized by subtracting the cohort-specific mean log wage in the month of transition. The control group mean corresponds to the pooled sample of cohort-specific controls, with means normalized relative to June 2021.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) from 2020 to 2023.

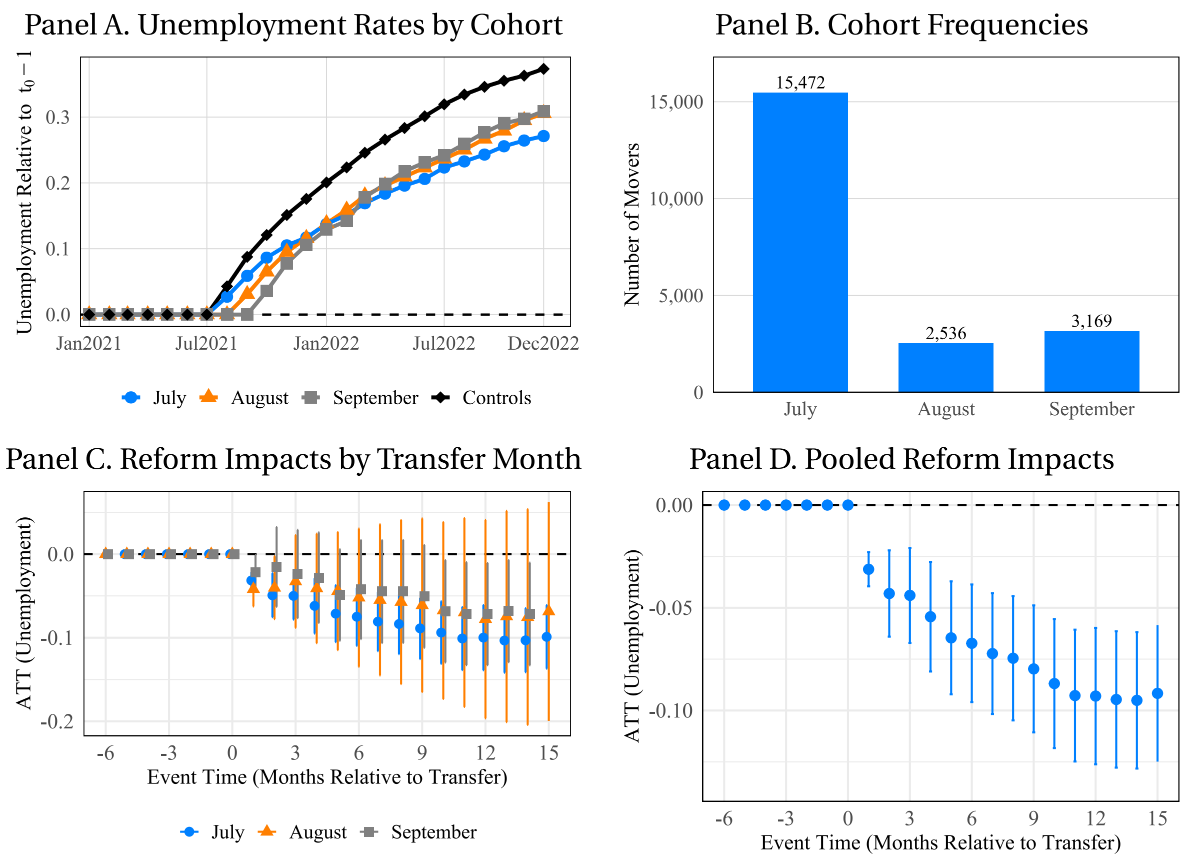
Table N.1: Balance of Key Employment Indicators by Month of Transition

	July			August			September		
	Treated (1)	Control (2)	Std.Diff (3)	Treated (4)	Control (5)	Std.Diff (6)	Treated (7)	Control (8)	Std.Diff (9)
Tenure (Months)	21.9	21.9	0.00	21.6	21.3	0.03	19.6	19.8	-0.02
Wages in 2021									
January	959.1	1,014.5	-0.07	927.5	896.2	0.04	475.1	487.4	-0.02
February	973.3	1,028.1	-0.07	945.6	912.2	0.04	479.2	491.1	-0.02
March	972.7	1,028.3	-0.07	954.5	922.5	0.04	479.1	490.3	-0.02
April	977.5	1,033.8	-0.07	955.3	923.6	0.04	480.3	491.7	-0.02
May	981.3	1,038.0	-0.07	949.6	917.0	0.04	483.2	495.0	-0.02
June	982.4	1,039.6	-0.07	949.9	917.6	0.04	483.5	495.7	-0.02
Firm Size (Workers)	226.0	256.5	-0.11	181.4	178.3	0.02	63.4	62.1	0.02
Workers	15,472	15,472		2,536	2,536		3,169	3,169	

Notes: This table reports mean values of the matching covariates for workers transitioning from the professional services sector to manufacturing and for their matched counterparts employed in manufacturing at the month of transition. Tenure and firm size are measured as of June 2021. Within each panel, the standardized difference is defined as the difference in group means divided by the pooled standard deviation; values below 0.10 are conventionally interpreted as indicating satisfactory covariate balance.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021.

O Unemployment Impacts

Figure O.1: Cumulative Unemployment Effects of the Reform at the Worker Level



Notes: This figure illustrates the cumulative impact of the outsourcing ban on the probability of unemployment at the worker level. Panel A displays unemployment rates by month. Pre-transition unemployment rates in this panel are zero by construction, since the matched sample consists of workers with observed wages from January to June 2021. The control group mean corresponds to the pooled sample of cohort-specific controls. Panel B shows the number of workers transitioning from the professional services sector to manufacturing each month. Panel C presents the interaction-weighted average treatment effect of the outsourcing ban for each cohort, estimated using the interaction-weighted (IW) estimator. Panel D reports the average reform effect, computed as the average of the cohort-specific event study estimates.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) from 2021 to 2022.

Table O.1: Cumulative Impacts of the Reform on Unemployment by Post-Treatment Horizon

	July	August	September	Pooled
	(1)	(2)	(3)	(4)
ATT (2021)	-0.044***	-0.031*	-0.015	-0.038***
	(0.011)	(0.018)	(0.014)	(0.008)
Observations	371,328	60,864	76,056	508,248
R^2	0.3769	0.3222	0.2581	0.3526
ATT (2021–2022)	-0.077***	-0.055	-0.047*	-0.070***
	(0.016)	(0.044)	(0.025)	(0.013)
Observations	1,113,984	182,592	228,168	1,524,744
R^2	0.4173	0.4309	0.3883	0.4146
ATT (2021–2023)	-0.085***	-0.050	-0.050*	-0.075***
	(0.017)	(0.052)	(0.026)	(0.014)
Observations	1,856,640	304,320	380,280	2,541,240
R^2	0.4549	0.4794	0.4456	0.4565
Treated Workers	15,472	2,536	3,169	21,177

Notes: Columns (1) through (3) report the cohort average treatment effects (CATTs) on the cumulative probability of unemployment by treatment cohort, estimated using the interaction-weighted (IW) estimator over progressively longer post-treatment sample periods. Standard errors are clustered at the predecessor firm level. Column (4) reports the equally weighted average of the cohort-specific CATTs. The total number of workers in each regression equals twice the number of treated workers. **p<0.05, ***p<0.01.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021 through 2023.

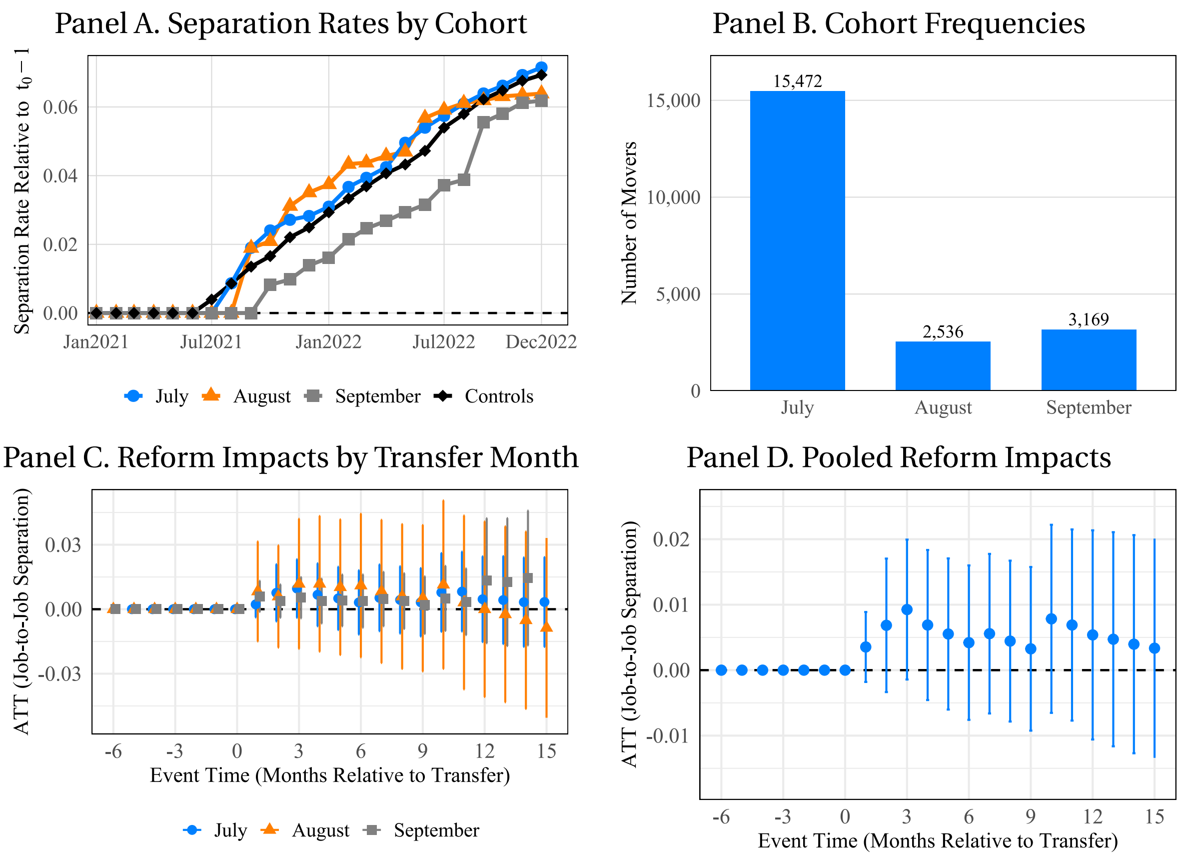
P Impacts on Job-to-Job Separations

Table P.1: Balance of Key Employment Indicators by Month of Transition - Separation Panel

	July			August			September		
	Treated	Control	Std.Diff	Treated	Control	Std.Diff	Treated	Control	Std.Diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tenure (Months)	21.9	21.8	0.01	21.6	21.4	0.01	19.6	19.5	0.01
Wages in 2021									
January	959.1	1,003.3	-0.06	927.5	944.1	-0.02	475.1	482.4	-0.01
February	973.3	1,016.6	-0.05	945.6	961.9	-0.02	479.2	486.3	-0.01
March	972.7	1,017.2	-0.06	954.5	971.3	-0.02	479.1	485.9	-0.01
April	977.5	1,022.7	-0.06	955.3	972.5	-0.02	480.3	487.5	-0.01
May	981.3	1,027.1	-0.06	949.6	965.2	-0.02	483.2	489.8	-0.01
June	982.4	1,028.4	-0.06	949.9	965.7	-0.02	483.5	490.5	-0.01
Firm Size (Workers)	226.0	258.5	-0.11	181.4	183.0	-0.01	63.4	62.9	0.01
Workers	15,472	15,472		2,536	2,536		3,169	3,169	

Notes: This table reports the means of the matching covariates for workers transitioning from the professional services sector to manufacturing and for their matched counterparts who were employed in the manufacturing sector at the month of the transition. Because the sectoral move mechanically entails an employer change for treated workers, we restrict the control pool to workers who do not change employers in the transition month, so that subsequent job transitions are measured relative to a common baseline: the new manufacturing position for treated workers and the existing position for controls. Tenure and firm size are measured as of June 2021. Within each panel, the standardized difference is defined as the difference in group means divided by the pooled standard deviation; values below 0.10 are conventionally interpreted as indicating satisfactory covariate balance.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021.

Figure P.1: Cumulative Effects of the Reform on Job-to-Job Separations at the Worker Level



Notes: This figure illustrates the impact of the outsourcing ban on job-to-job separation rates at the worker level. Panel A displays mean separation rates by month, normalized by subtracting the cohort-specific mean separation rate in the pre-treatment month; control group means correspond to the pooled sample of cohort-specific controls and are normalized relative to June. Panel B shows the number of workers transitioning each month. Panel C presents the interaction-weighted average treatment effect of the outsourcing ban for each cohort, estimated using the interaction-weighted (IW) estimator. Panel D reports the average reform effect, calculated by averaging cohort-specific event study estimates. In Panels A, C, and D, we omit the month of the initial transition, as separation rates in the treatment group are mechanically equal to one by construction, given that treatment is defined by a sectoral transition from professional services to manufacturing.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021 through 2023.

Table P.2: Cumulative Impacts of the Reform on Worker-Level Job-to-Job Separations by Post-Treatment Horizon

	July (1)	August (2)	September (3)	Pooled (4)
ATT (2021)	0.007	0.010	0.005	0.007
	(0.007)	(0.014)	(0.004)	(0.005)
Observations	371,328	60,864	76,056	508,248
R^2	0.3431	0.2967	0.4403	0.3521
ATT (2021–2022)	0.005	0.004	0.007	0.005
	(0.008)	(0.017)	(0.007)	(0.006)
Observations	1,113,984	182,592	228,168	1,524,744
R^2	0.3666	0.3653	0.3775	0.3681
ATT (2021–2023)	0.003	-0.009	0.019	0.004
	(0.010)	(0.019)	(0.012)	(0.008)
Observations	1,856,640	304,320	380,280	2,541,240
R^2	0.3802	0.3873	0.3757	0.3804
Treated Workers	15,472	2,536	3,169	21,177

Notes: Columns (1) through (3) report the cohort average treatment effects (CATTs) on the probability of job-to-job separation by treatment cohort, estimated using the interaction-weighted (IW) estimator over progressively longer post-treatment sample periods. Standard errors are clustered at the predecessor firm level. Column (4) reports the equally weighted average of the cohort-specific CATTs. In all columns, we omit the month of the initial transition from estimation, as separation rates in the treatment group are mechanically equal to one by construction, given that treatment is defined by a sectoral transition from professional services to manufacturing. The total number of workers in each regression equals twice the number of treated workers. *p<0.1, **p<0.05.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021 through 2023.

Q Effects on AKM Firm Fixed Effects

In this section, we estimate the effects of the reform on firm fixed effects using the methodology of Abowd, Kramarz and Margolis (1999). If the reform increased compliance with labor regulation and equalized employment conditions within firms, we should observe increases in firm wage premia after the reform.

To assess this possibility, we estimate AKM models separately by semester (i.e., rolling AKM) to trace the evolution of firm effects. As shown by Lachowska et al. (2023), firm effects are identified within period and should therefore be interpreted in relative terms, rather than as directly comparable levels across periods. In our application, AKM effects spike in the second semester of 2021, when workers were transferred from the professional services sector to the manufacturing sector and several firms report paying wages close to zero (Panel A of Figure Q.1).

Our empirical strategy compares the evolution of the AKM effects of firms receiving previously outsourced workers (Panel B of Figure Q.1) with those of firms employing matched controls in the manufacturing sector, identified from our headline estimation strategy. To recover the causal impact of the outsourcing reform on the AKM effect ψ_{it} of firm i after j semesters, we estimate the parameter β_j in the following linear regression model via OLS:

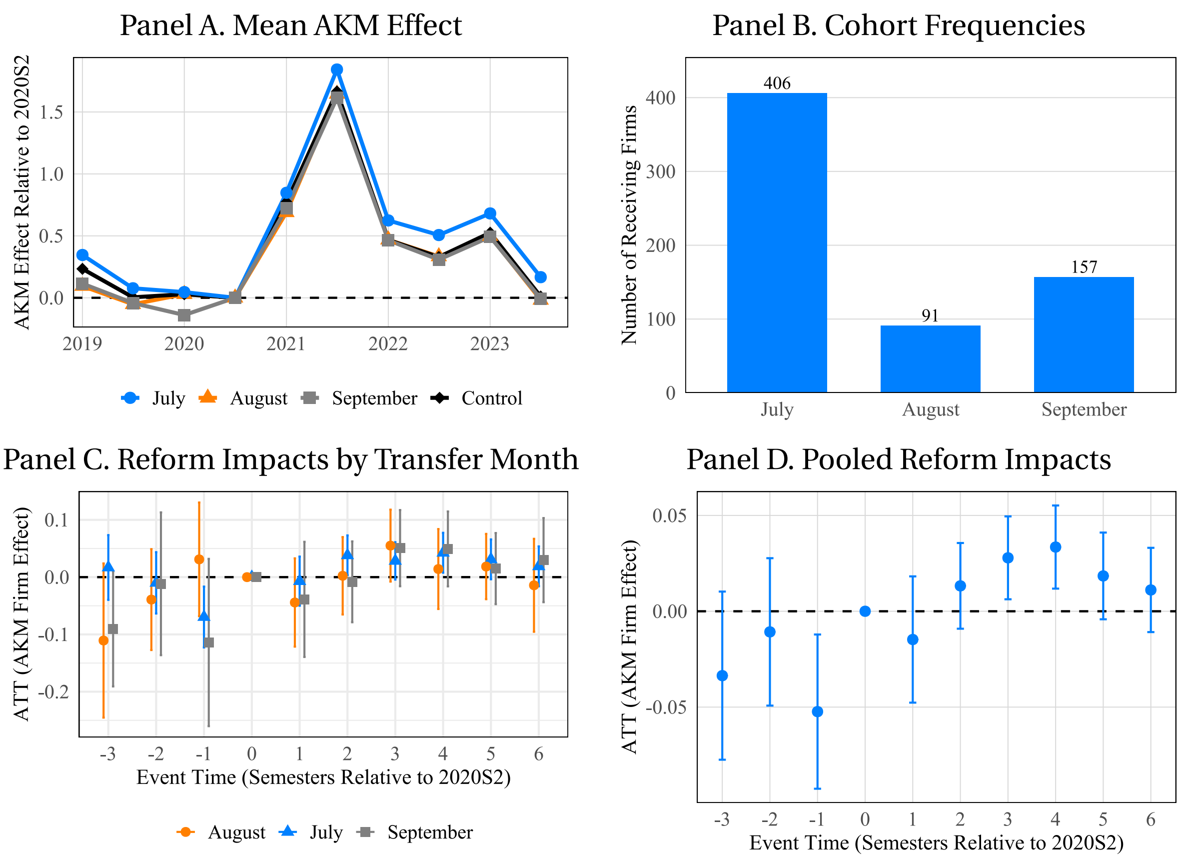
$$\psi_{it} = \alpha_i + \gamma_t + \sum_{j=A}^{B} [\mathbb{1}_{\{t=t_0+j\}} \times \text{Outsourcing}_{i,t_0}]\beta_j + \varepsilon_{it}, \tag{Q1}$$

where $\text{Outsourcing}_{i,t_0}$ is an indicator for whether firm i operating in the manufacturing sector received outsourced workers during the three-month period stipulated in the reform or is a matched control firm; A is the first pre-reform period available in the data; B is the last post-shock period; α_i is a firm fixed effect; γ_t is a semester dummy; and ε_{it} is an idiosyncratic error term. Standard errors are robust to heteroskedasticity and clustered at the firm level. Identification relies on a parallel trends assumption: in the absence of the reform, firm premia would have evolved similarly in treated and control firms.

We present cohort-specific and pooled estimates in Panels C and D of Figure Q.1, respectively. We document a differentially negative response of AKM effects for treated firms in the first semester of 2020, corresponding to the strictest COVID-19 lockdown period. All other pre-reform coefficients are consistent with parallel trends.

Table Q.1 reports the corresponding regression estimates. Columns (1) through (3) present results for firms receiving outsourced workers in July, August, and September, respectively. We find a positive and statistically significant increase of 2.5 percent in the mean AKM effect for firms receiving outsourced workers in July 2021 ($p = 0.023$). Estimates for August and September are positive but not statistically significant (0.5 percent, $p = 0.769$; 1.6 percent, $p = 0.488$). The pooled estimate corroborates these patterns: the reform increased AKM effects for treated establishments by 2 percent ($p = 0.026$). However, these estimates lose statistical significance under the robust confidence sets of Rambachan and Roth (2023) already when $\bar{M} = 0.25$, indicating that the increase in firm premia is only suggestive.

Figure Q.1: Reform Effects on AKM Firm Effects



Notes: This figure illustrates the impact of the outsourcing ban on AKM firm fixed effects of receiving firms. AKM effects are estimated separately by semester using different connected sets. Panel A displays mean AKM effects by semester, normalized by subtracting the cohort-specific mean AKM effect in the second semester of 2020; control group means correspond to the pooled sample of cohort-specific control firms in the manufacturing sector. Panel B shows the number of receiving firms by month. Panel C presents cohort-specific average treatment effects estimated using two-way fixed effects (TWFE) regressions. Panel D reports the pooled reform effect from a TWFE specification.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) from 2019 to 2023.

Table Q.1: Reform Impacts on AKM Firm Fixed Effects

	July	August	September	Pooled
	(1)	(2)	(3)	(4)
ATT	0.025**	0.005	0.016	0.020**
	(0.011)	(0.017)	(0.023)	(0.009)
Robust Confidence Set ($\bar{M} = 0.25$)	[-0.076, 0.138]	[-0.178, 0.193]	[-0.217, 0.257]	[-0.060, 0.097]
Observations	6,073	3,569	4,115	13,757
R^2	0.88	0.873	0.848	0.871

Notes: This table reports reform impacts on AKM firm fixed effects for firms receiving outsourced workers, by month of transfer. Columns (1) through (3) present average treatment effects on the treated (ATT) by transfer month, estimated using two-way fixed effects (TWFE) regressions. Standard errors are clustered at the firm level. Column (4) reports the pooled ATT across receiving firms. The total number of firms in each regression equals twice the number of treated firms. **p<0.05.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) from 2019 to 2023.

R Results from Matching All Workers Simultaneously

In this appendix, we drop the matching requirement for control workers to remain employed until the month of the move and match all workers simultaneously. We then implement the interaction-weighted (IW) estimator of Sun and Abraham (2021) to estimate the cohort-specific average treatment effects on the treated (CATTs) via an interacted TWFE regression:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{e \in \mathcal{G}} \sum_{\ell \neq -1} \delta_{e,\ell} \left(\mathbb{1}\{E_i = e\} \cdot \mathbb{1}\{t - e = \ell\} \right) + \varepsilon_{it}, \tag{R1}$$

where all terms are as defined in Equation (2). Standard errors are clustered at the level of the predecessor firm.

Because all three cohorts and their matched controls now enter the same regression, the cross-cohort aggregation is handled internally by the estimator rather than computed ex post as in the baseline specification. The event-study figures report the IW estimates at each event time, $\nu_\ell = \sum_{e \in \mathcal{G}_\ell} \delta_{e,\ell} \cdot \Pr(E_i = e \mid E_i \in \mathcal{G}_\ell)$, while the overall ATT further aggregates across post-treatment periods as $\text{ATT} = \sum_{e \in \mathcal{G}} \sum_{\ell \in \mathcal{L}_e} \delta_{e,\ell} \cdot \Pr(E_i = e, \ell_i = \ell \mid E_i \in \mathcal{G}, \ell_i > 0)$.

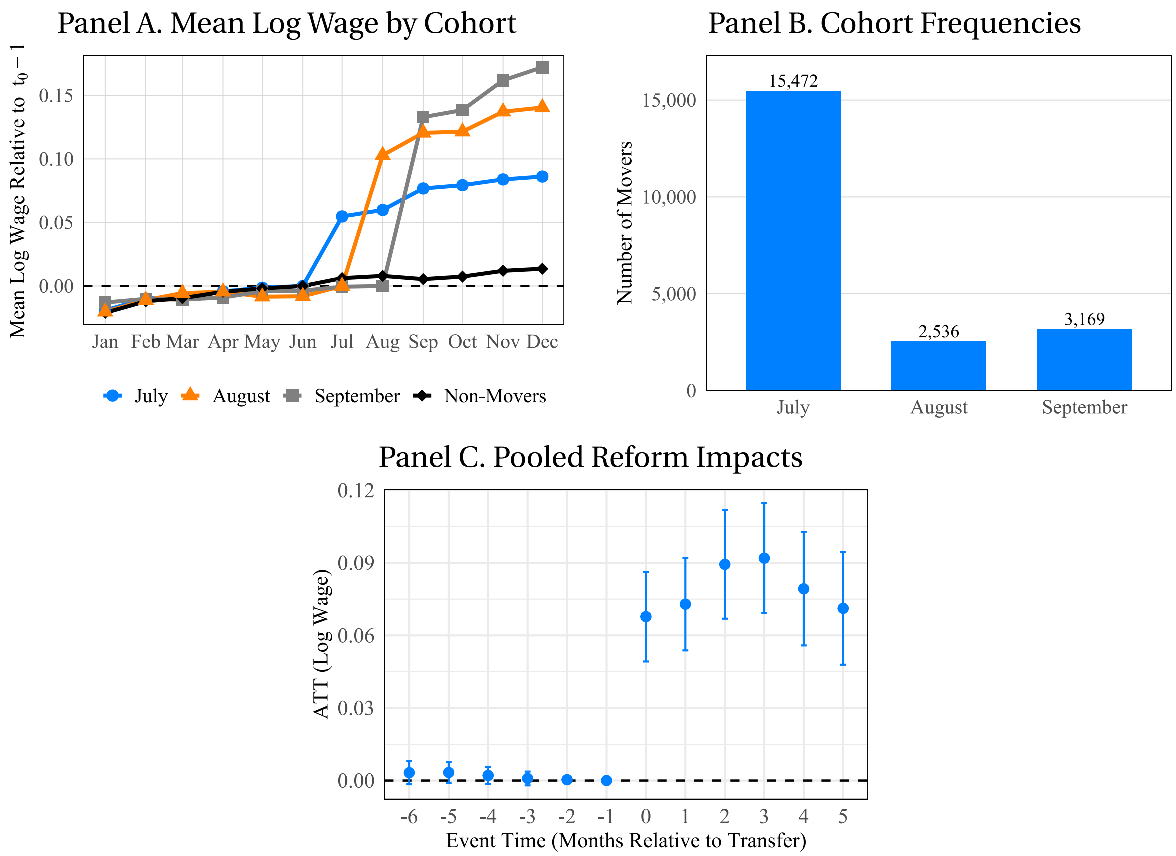
Table R.1 reports the covariate balance achieved under this alternative matching procedure. The balance statistics are quantitatively similar to those obtained under the cohort-specific matching approach, indicating that matching all outsourced workers simultaneously does not materially alter the comparability of treated and control workers. Figure R.1 plots the raw wage trends by treated cohort together with the single group of matched controls, as well as the distribution of treated workers across cohorts and the corresponding reform impacts. The patterns across panels in this figure closely resemble those observed in our baseline analysis. Long-run wage trends under this alternative matching procedure, reported in Figure R.2, are likewise very similar to those obtained under the cohort-specific matching design. Finally, Table R.2 reports the pooled ATT estimates by post-treatment horizon. These estimates are nearly identical to those we obtained when matching workers separately by cohort across all reported horizons.

Table R.1: Balance of Key Employment Indicators — Single Match

	Treated (1)	Control (2)	Std.Diff (3)
Tenure (Months)	21.5	21.4	0.02
Wages in 2021			
January	882.9	906.0	-0.03
February	896.1	918.1	-0.03
March	896.6	919.1	-0.03
April	900.4	923.2	-0.03
May	902.9	925.7	-0.03
June	903.9	926.8	-0.03
Firm Size (Workers)	196.3	211.9	-0.06
Workers	21,177	21,177	

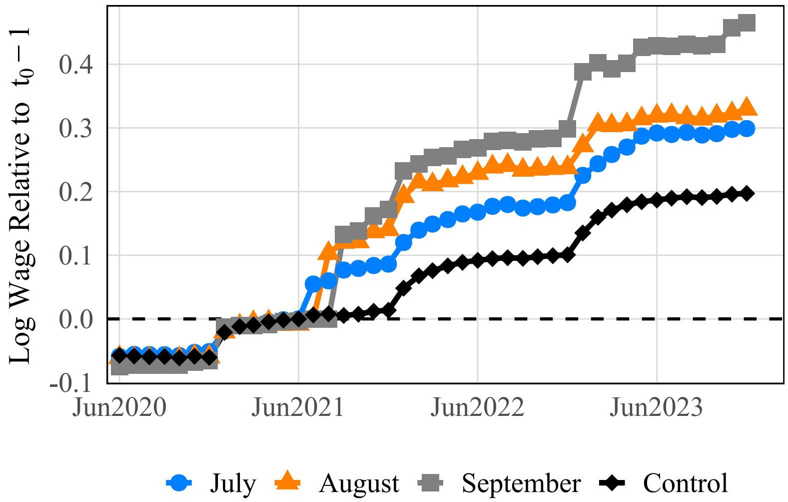
Notes: This table reports the mean values of the matching covariates for workers transitioning from the professional services sector to manufacturing and for their matched counterparts employed in manufacturing in the month of transition. The matching procedure pools all cohorts and applies nearest-neighbor matching without replacement, rather than matching separately by transition month. Tenure and firm size are measured as of June 2021. Within each panel, the standardized difference is defined as the difference in group means divided by the pooled standard deviation; values below 0.10 are conventionally interpreted as indicating satisfactory covariate balance.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021.

Figure R.1: Wage Effects of the Reform at the Worker Level — Single Match



Notes: This figure illustrates the impact estimates of the outsourcing ban on worker-level wages resulting from a matching procedure that pools the workers across all treated cohorts and matches each of them with a suitable control using a nearest-neighbor matching without replacement, rather than matching separately by transition month. Panel A displays mean log wages by month, normalized by subtracting the cohort-specific mean log wage in the treatment month; control group means correspond to the pool of matched controls and are normalized relative to June. Panel B shows the number of workers transitioning each month. Panel C presents the interaction-weighted average treatment effect across cohorts, estimated using the interaction-weighted (IW) estimator.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021.

Figure R.2: Long-Term Raw Wage Trends by Treatment Cohort — Single Match



Notes: This figure shows the raw trend in mean log wages for each treatment cohort, with values normalized by subtracting the cohort-specific mean log wage in the month of transition. The control group mean is constructed from a matching procedure that pools workers across all treated cohorts and matches each treated worker to a suitable control using nearest-neighbor matching without replacement, rather than matching separately by transition month. The control series is normalized relative to June 2021.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) from 2020 to 2023.

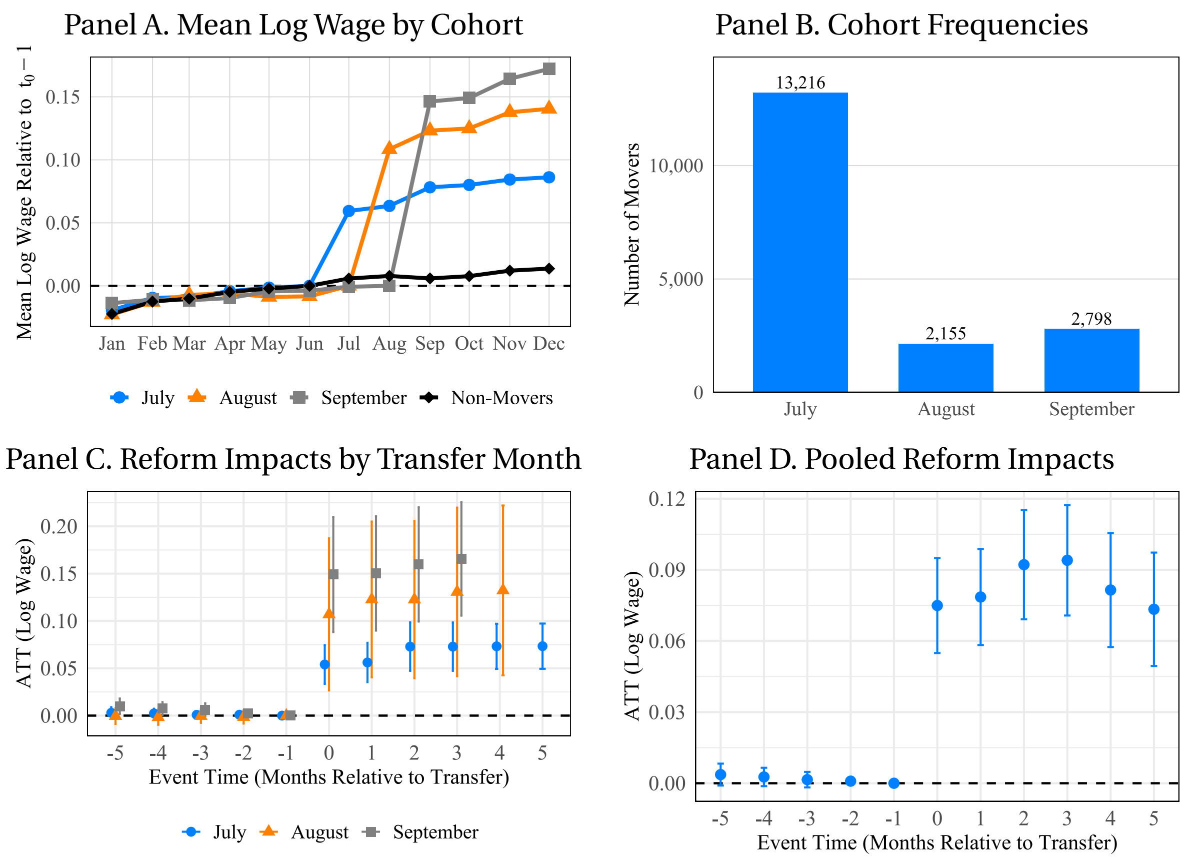
Table R.2: Reform Impacts on Worker-Level Wages by Post-Treatment Horizon — Single Match

	2021	2021–2022	2021–2023
	(1)	(2)	(3)
ATT	0.079***	0.095***	0.104***
	(0.010)	(0.011)	(0.012)
Observations	475,041	1,194,511	1,713,404
Treated Workers	21,177	21,177	21,177
R^2	0.9888	0.9853	0.9830

Notes: This table presents the interaction-weighted average treatment effect of the outsourcing ban across cohorts, estimated using the interaction-weighted (IW) estimator, over progressively longer post-treatment sample periods. The control group mean is constructed from a matching procedure that pools workers across all treated cohorts and matches each treated worker to a suitable control using nearest-neighbor matching without replacement, rather than matching separately by transition month. Standard errors are clustered at the predecessor firm level. The total number of workers in each regression equals twice the number of treated workers. ***p<0.01.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) from 2021 to 2023.

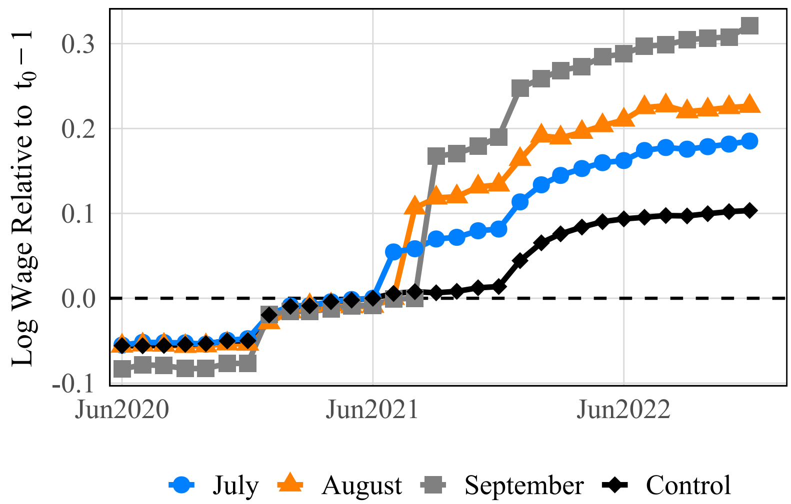
S Wage Impacts Estimated Using a Balanced Panel

Figure S.1: Wage Effects of the Reform at the Worker Level — Balanced Panel



Notes: This figure illustrates the impact of the outsourcing ban on worker-level wages using a balanced panel of workers. The estimation sample retains movers who remain employed in the same post-move firm through December 2021 and who were continuously employed in the same pre-move firm since January 2021. The pool of matched controls is restricted to workers in the manufacturing sector who remain employed by the same firm from January to December 2021. Panel A displays mean log wages by month, normalized by subtracting the cohort-specific mean log wage in the treatment month; control group means correspond to the pooled sample of cohort-specific controls and are normalized relative to June 2021. Panel B shows the number of workers transitioning each month. Panel C presents the interaction-weighted average treatment effect of the outsourcing ban for each cohort, estimated using the interaction-weighted (IW) estimator. Panel D reports the average reform effect, computed as the average of the cohort-specific event study estimates.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021.

Figure S.2: Long-Term Raw Wage Trends by Treatment Cohort – Balanced Panel



Notes: This figure shows the raw trend in mean log wages for each treatment cohort, with values normalized by subtracting the cohort-specific mean log wage in the month of transition. The control group mean corresponds to the pooled sample of cohort-specific controls, with means normalized relative to June 2021. The estimation sample consists of the subset of matched workers from our headline matching procedure who remain employed at the same post-move firm through December 2023 and who were continuously employed at the same pre-move firm since January 2021.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) from 2020 to 2023.

Table S.1: Reform Impacts on Worker-Level Wages – Balanced Panel

	July (1)	August (2)	September (3)	Pooled (4)
ATT (2021)	0.067***	0.123***	0.156***	0.087***
	(0.011)	(0.043)	(0.031)	(0.011)
Observations	303,466	50,407	65,019	418,892
Treated Workers	13,216	2,155	2,798	18,169
R^2	0.9893	0.9848	0.9735	0.9863
ATT (2021–2022)	0.072***	0.121**	0.193***	0.093***
	(0.013)	(0.051)	(0.034)	(0.012)
Observations	518,989	81,241	91,079	691,309
Treated Workers	8,219	1,285	2,798	10,907
R^2	0.9850	0.9791	0.9648	0.9817
ATT (2021–2023)	0.084***	0.120**	0.212***	0.104***
	(0.014)	(0.054)	(0.033)	(0.013)
Observations	819,651	128,691	140,844	1,089,186
Treated Workers	8,219	1,285	1,403	10,907
R^2	0.9812	0.9765	0.9583	0.9777

Notes: This table presents the reform impacts on the log wages of the subset of matched workers from our headline matching procedure who remain employed at the same post-move firm through the end of a progressively longer post-treatment sample period and who were continuously employed at the same pre-move firm since January 2021. Columns (1) through (3) report the cohort average treatment effects (CATT) by treatment cohort, estimated using the interaction-weighted (IW) estimator. Standard errors are clustered at the predecessor firm level. Column (4) presents, for each post-treatment sample period, the equally weighted average across cohorts of the estimated CATTs. The total number of workers in each regression equals twice the number of treated workers. **p<0.05, ***p<0.01.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) from 2021 to 2023.

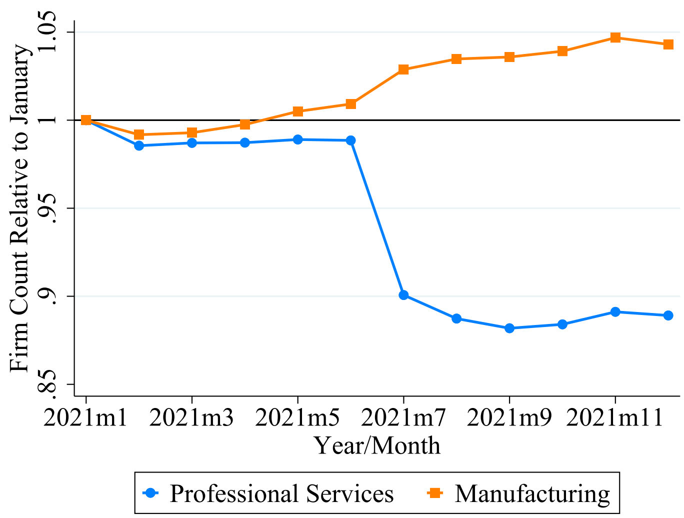
T Alternative Strategies to Identify Staffed Workers

In this appendix, we devise three alternative strategies to identify staffed workers in the IMSS data and estimate the reform's effect on their employment status and wages. All three strategies use canonical DID designs to retrieve causal impacts, but each of them compares the treated workers to a different group.

T.1 Firm Exit from the Professional Services Sector

The first strategy identifies staffing companies as those meeting two conditions: (1) they were registered in the "provision of professional services to other firms" sector in the IMSS economic sector classification, and (2) they permanently exited the market in July 2021. The first condition captures the fact that IMSS officials tended to register staffing companies in the specified sector, alongside firms providing other types of professional services, such as accounting, consulting, and law. The second condition captures that July 2021 was the deadline set by the reform for these firms to exit the market. Consistent with this stipulation, Figure T.1 shows that the number of firms registered in the "provision of professional services to other firms" sector abruptly dropped by 9 percent in July 2021 but did not do so in the manufacturing sector, which we use as a comparison sector in our DID design.

Figure T.1: Drop in the Number of Firms in the Professional Services Sector of IMSS



Notes: This figure presents the number of active firms registered with the Instituto Mexicano del Seguro Social (IMSS) in the "professional services to other firms" and manufacturing sectors relative to their levels in January for each month of 2021.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021.

Our first strategy leverages the transfer of workers during the transition period between the reform's enactment in April 2021 and the July 2021 deadline, during which early-complying staffing firms reassigned workers to their actual employing firms prior to their formal exit from the market. Specifically, our design compares the outcomes of workers employed from January to March 2021 by any of the firms identified by the two criteria above with those of workers employed in the manufacturing sector in the same months, before and after the reform. Concretely, we estimate the following regression model for the outcome of interest Y_{it} of worker i at period t via OLS:

$$Y_{it} = \sum_{j=\text{January 2021}}^{\text{December 2021}} [\mathbb{1}_{t=t_0+j} \times \widehat{\text{Staffing}}_{i,t_0}]\beta_j + \widehat{\text{Staffing}}_{i,t_0}\gamma + \delta_t + \varepsilon_{it}, \tag{T1}$$

where $\widehat{\text{Staffing}}_{i,t_0}$ is an indicator for the event of worker i being employed at any of the identified staffing firms in March 2021, the period immediately prior to the reform; β_j is the effect of the reform after j periods; γ is a group fixed effect, which absorbs all time-invariant variation in the outcome of interest for workers hired by staffing firms at t_0; δ_t is a time dummy, which absorbs all aggregate shocks that affect outcomes equally across all workers; and ε_{it} is an idiosyncratic unobserved shock to the outcome of interest. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the level of the hiring firm in March 2021. In estimation, we exclude the interaction between our staffing indicator and the dummy for March 2021, allowing us to interpret our coefficient estimates as deviations in the outcome of interest relative to the level observed for the group of directly hired workers in manufacturing before the reform. Importantly, the hat over $\widehat{\text{Staffing}}_{i,t_0}$ is due to the fact that we are inferring staffing status based on the two conditions discussed above.

T.2 Universal Registry of Specialized Service Providers

Our second strategy to identify previously outsourced workers leverages one of the reform's key provisions: the creation of a universal registry of specialized service providers that tracks payroll information and contracts of staffing companies with employing firms. As per the reform's provisions, all service providers intending to continue operating after its enactment had to apply to register and demonstrate, by the end of the grace period in September 2021, that they truly provided specialized services (e.g., cleaning, catering, security, gardening), as opposed to staffing services. If an applicant was rejected, the reform mandated that its workers be transferred to their actual employer. The main statutory reason for rejection was evidence that the applicant offered staffing services rather than specialized services.[50]

We use the list of registry applicants in 2021 to compare the labor market outcomes of two groups of workers: employees of unsuccessful applicants who were transferred out of staffing after rejection and employees of successful applicants who were allowed to remain employed in the services sector. To compare like with like, we limit the control sample to only workers who remained continuously employed in the services sector until December 2021, as the social

[50]The other two main reasons were evidence of tax evasion or reduced social security payment and evidence of false declarations in legal documents.

security authority oversaw the transfer of workers from rejected applicants to their employing firms with no unemployment spells. We estimate via OLS the following model on the sample comprised of both groups of workers:

$$Y_{it} = \sum_{j=\text{January 2021}}^{\text{December 2021}} [\mathbb{1}_{t=t_0+j} \times \text{Unsuccessful}_{i,t_0}]\beta_j + \text{Unsuccessful}_{i,t_0}\gamma + \delta_t + \varepsilon_{it}, \qquad \text{(T2)}$$

where $\text{Unsuccessful}_{i,t_0}$ is a dummy taking the value of 1 if worker i was employed in March 2021 at any of the firms that applied for registration but were rejected and 0 if he was employed at any of the successful firms in the same month. The meaning of all other terms is the same as in the previous section. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the level of the hiring firm in March 2021. As before, we exclude the interaction between our success indicator and the dummy for March 2021.

T.3 Mixed Identification Strategy

Our third strategy compares the outsourced workers identified through our first strategy, which relies on the exit of firms from the professional services sector in July 2021, with the comparison group of workers from our second strategy, which consists of employees of successful applicants who were allowed to remain in the services sector following the creation and enforcement of the universal registry of specialized service providers. Our regression model is identical to that described in Equation (T1), with the sole difference being the definition of the comparison group.

T.4 Results

Figure T.2 presents the OLS estimates of the β_j parameters of Equations (T1) and (T2), as well as our mixed identification strategy. All three strategies indicate the reform increased the probability of employment in the manufacturing sector and raised the wages of the treated workers.

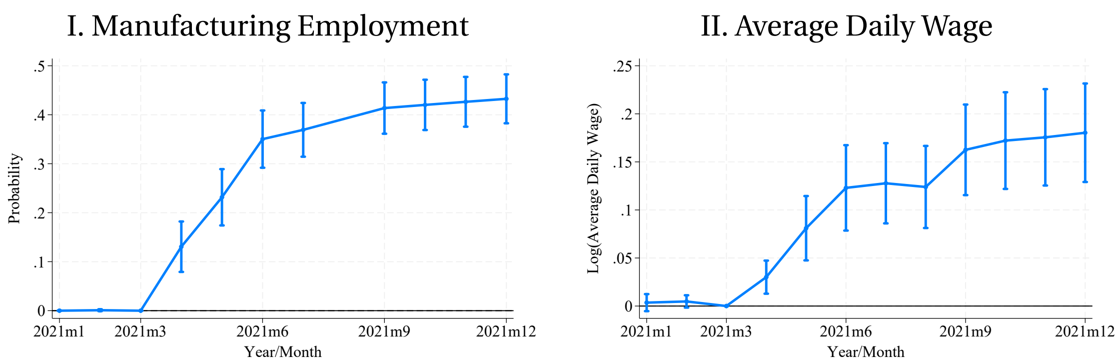
Table T.1 reports the OLS estimates of the impact of the reform on the probability of employment in the manufacturing sector and wages by the end of 2021. Importantly, not all contracts of staffing firms involved employing firms in the manufacturing sector before the reform. Therefore, we do not expect the impact of the reform on the probability of employment in manufacturing to be equal to 100 percent. Rather, we expect it to be broadly consistent with the manufacturing share of GDP, which, as mentioned above, is 21 percent (Instituto Nacional de Estadística y Geografía, 2024). Estimates above this benchmark would be consistent with a disproportionate use of outsourcing arrangements in the manufacturing sector relative to other sectors.

Our estimates from the first identification strategy indicate that the reform increased the probability of being directly hired by a manufacturing firm by 43 percentage points (p=0.000) for employees of firms in the "provision of professional services to other firms" sector that exited the market in July 2021. This magnitude is consistent with a higher prevalence of outsourcing in the manufacturing sector than implied by its 21 percent share of GDP. We also find that their registered monthly wage increased by 18 percent (p=0.000), which exceeds the wage impact estimated under our headline strategy. Similarly, estimates from our second strategy indicate that the reform increased the probability of direct employment in manufacturing by 40 percentage points (p=0.000) and raised registered wages by 22 percent (p=0.000) for employees of unsuccessful applicants who were transferred out of staffing. Our mixed identification strategy yields comparable results: the reform increased the probability of transitioning into manufacturing by 40 percentage points (p=0.000) and registered wages by 23 percent (p=0.000).

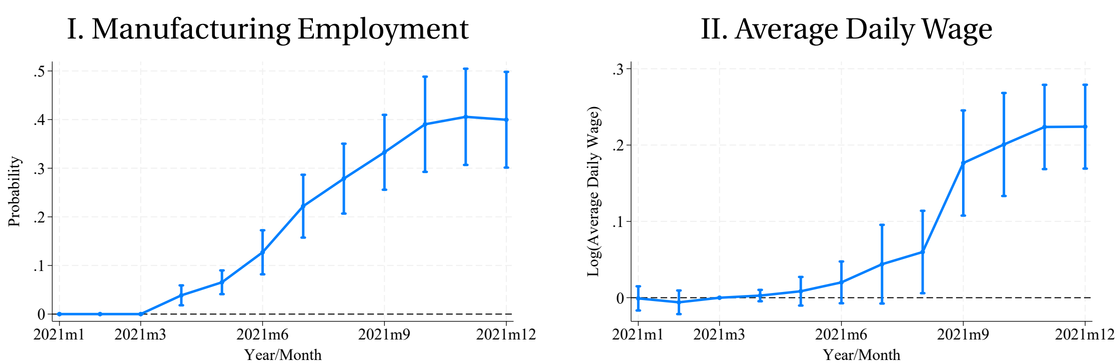
Finally, we present the estimates of the reform's impact on wage inequality from all three identification strategies. We divide our estimation sample into four bins according to the percentile rank of workers in the wage distribution of March 2021 and estimate fully saturated versions of all three DID specifications with categorical quartile dummies. If the reform reduced inequality, we would expect the positive wage impacts to be concentrated among low earners. In Figure T.3, we show that the wage gains from the reform are indeed concentrated at the bottom of the wage distribution.

Figure T.2: Reform Impacts on Worker Outcomes by Identification Strategy

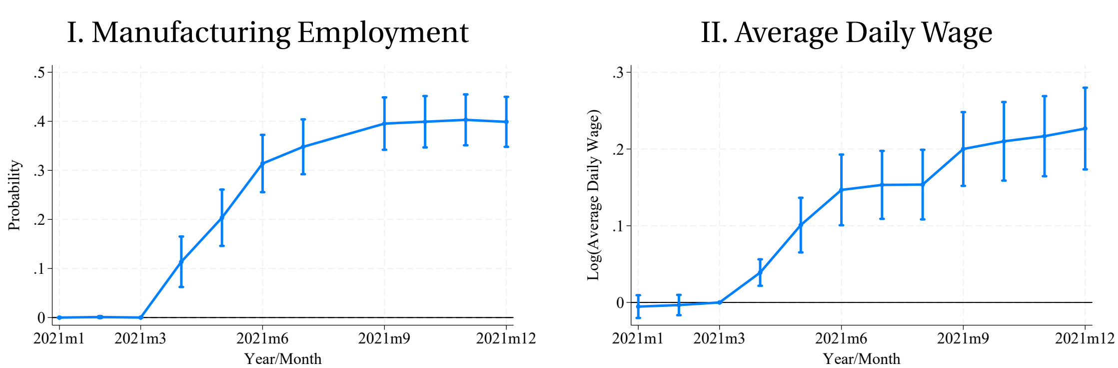
Panel A. Firm Exit from the Professional Services Sector



Panel B. Universal Registry of Specialized Service Providers



Panel C. Mixed Identification Strategy



Notes: In Panel A, the treatment group consists of workers employed from January to March 2021 at any company operating in the "provision of professional services to other firms" sector that subsequently exited the market in July 2021, and the comparison group comprises all workers directly hired in the manufacturing sector in March 2021. In Panel B, the treatment group consists of workers employed in March 2021 at any firm that was rejected from the universal registry of specialized service providers and was required to transfer all its employees out of the services sector, and the comparison group comprises all employees of successful applicant firms that were allowed to remain operating in the services sector. In Panel C, the treatment group is the same as in Panel A, and the comparison group is the same as in Panel B. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the level of the hiring firm in March 2021.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021.

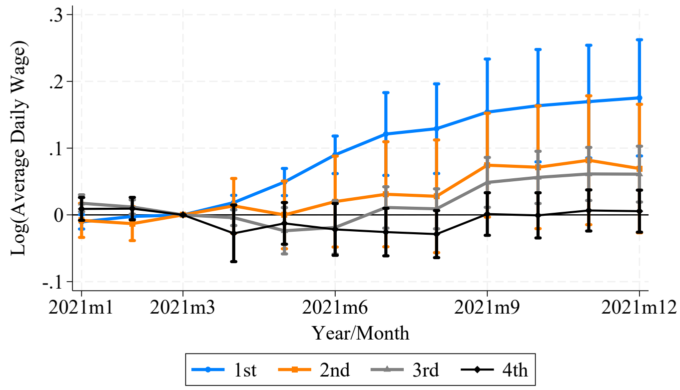
Table T.1: Reform Impacts on Worker Outcomes

Regressor	Employment in Manufacturing (1)	Log(Average Monthly Wage) (2)
	Panel A. Firm Exit from the Professional Services Sector	
$\widehat{\text{Staffing}}_{i,\text{March 2021}} \times \text{December}_t$	0.43***	0.18***
	(0.025)	(0.026)
N	69,544,486	71,326,898
R^2	0.009	0.002
	Panel B. Universal Registry of Specialized Service Providers	
$\text{Unsuccessful}_{i,\text{March 2021}} \times \text{December}_t$	.40***	.22***
	(0.05)	(.028)
N	6,816,852	6,816,319
R^2	0.269	0.983
	Panel C. Mixed Identification Strategy	
$\widehat{\text{Staffing}}_{i,\text{March 2021}} \times \text{December}_t$	.40***	.23***
	(0.026)	(.027)
N	7,086,511	7,048,295
R^2	0.022	0.011

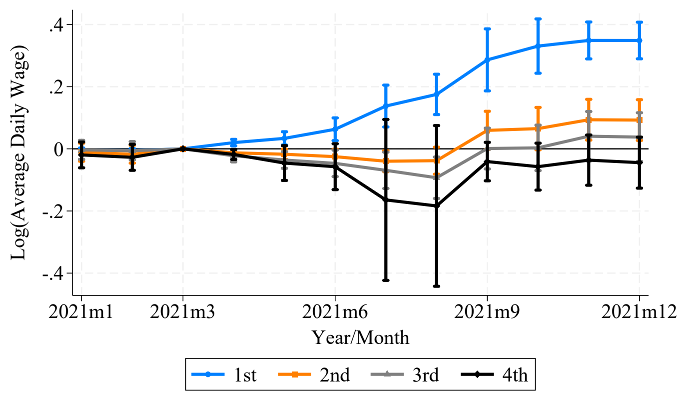
Notes: This table reports the end-of-year effects in 2021 of the outsourcing ban on wages and the probability of employment in the manufacturing sector for previously outsourced workers. In Panel A, the treatment group consists of workers employed in March 2021 at any company operating in the "provision of professional services to other firms" sector that subsequently exited the market in July 2021, and the comparison group comprises all workers directly hired in the manufacturing sector in March 2021. In Panel B, the treatment group consists of workers employed in March 2021 at any firm that was rejected from the universal registry of specialized service providers and was required to transfer all its employees out of the services sector, and the comparison group comprises all employees of successful applicant firms that were allowed to remain operating in the services sector. In Panel C, the treatment group is the same as in Panel A, and the comparison group is the same as in Panel B. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the level of the hiring firm in March 2021. ***p<0.01.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021.

Figure T.3: Impact Heterogeneity in Wages by Identification Strategy and Wage Quartile

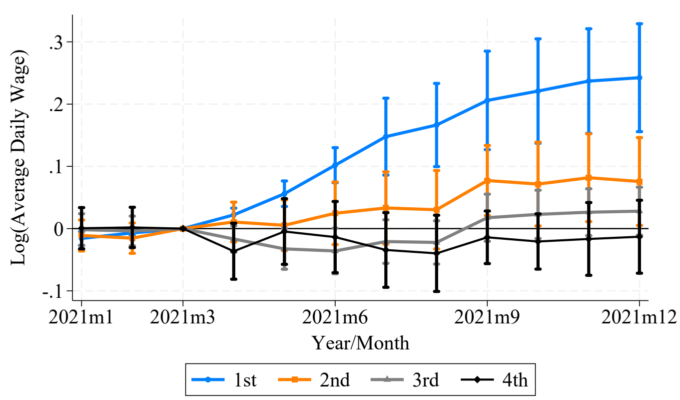
Panel A. Firm Exit from the Professional Services Sector



Panel B. Universal Registry of Specialized Service Providers



Panel C. Mixed Identification Strategy



Notes: In Panel A, the treatment group consists of workers employed in March 2021 at any company operating in the "provision of professional services to other firms" sector that subsequently exited the market in July 2021, and the comparison group comprises all workers directly hired in the manufacturing sector in March 2021. In Panel B, the treatment group consists of workers employed in March 2021 at any firm that was rejected from the universal registry of specialized service providers and was required to transfer all its employees out of the services sector, and the comparison group comprises all employees of successful applicant firms that were allowed to remain operating in the services sector. In Panel C, the treatment group is the same as in Panel A, and the comparison group is the same as in Panel B. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the level of the hiring firm in March 2021.
Source: Authors' elaboration using matched employer–employee data from the Instituto Mexicano del Seguro Social (IMSS) for 2021.

U Theoretical Framework

To explain our findings, we adopt a job-search-based monopsony model of the type proposed by Kline (2025), in which markdowns arise from workers' limited outside options. In contrast to the classical monopsony framework, which predicts an increase in employment and output following a reduction in market power (e.g., Berger, Herkenhoff and Mongey, 2022), this family of models does not need to generate employment or output responses, because monopsony power reflects surplus division induced by search frictions rather than firms' control over labor demand. Simply put, workers accept wages below their marginal revenue product because rejecting an offer entails the risk of a worse alternative.

U.1 The Firm's Problem

There is a unit continuum of workers capable of performing a task with value added p for the firm. Workers differ only in their outside options b, which are distributed on the interval $[\underline{b}, \bar{b}]$ according to the distribution function $(b - \underline{b})/(\bar{b} - \underline{b}) \sim \text{Beta}(\phi, 1)$. The firm cannot observe any worker's outside option but knows how these options are distributed. For simplicity, we assume that the firm chooses to hire all workers for the task either directly or through a staffing company, with differences in hiring modalities arising only across tasks with different value added. If the firm hires workers directly, it can be easily shown that the labor supply function, which we denote by F_D, takes the shifted power form, such that:

$$F_D(w) \propto (w + B(w) + PTU - \underline{b})^{\beta}, \tag{U1}$$

where w is the worker's regular earnings, excluding social security benefits and profit sharing, B is the mapping of registered wages to social security benefits, and PTU denotes profit sharing, with this formulation implicitly assuming that social security benefits and profit sharing are valued by workers one-for-one relative to wages. The functional form for the labor supply curve has an intuitive economic interpretation: labor supply is increasing in the difference between total labor compensation and the outside option of the worker most eager to work at the firm. Importantly, note that the labor supply function represents the supply curve that an individual firm faces by virtue of heterogeneity in the workers' outside options, not the market-level supply curve, which implies that workers not hired by the firm do not automatically become unemployed.

The problem of the firm is to post a wage that maximizes its profits. If workers are hired directly, the firm's problem is given by

$$\pi_D = \max_w F_D(w)(1-\tau_\pi)(p-(1+\tau_\ell(w))w - PTU), \tag{U2}$$

where τ_π is the corporate tax rate and $\tau_\ell : [\underline{w}, \infty] \to [0,1]$ is the social security contribution rate. We assume that $\tau_\ell(\cdot)$ is increasing in registered wages, with $\underline{w}$ denoting the minimum wage. Importantly, we assume that social security contributions are fully rebated to workers, so that they satisfy $\tau_\ell(w)w = B(w)$.

The profit function of the firm can be simplified if we assume a specific functional form for profit sharing. For example, mandatory profit sharing is a function of the firm's excess returns on equity in France (Nimier-David, Sraer and Thesmar, 2023). In Mexico, legislation mandates firms to share a 10 percent of taxable profits with workers, irrespective of a firm's capital structure, with taxable profits defined as the firm's revenues minus deductions, which include ordinary wages and social security contributions. Thus, we assume $PTU = \delta(p-(1+\tau_\ell(w))w)$, where δ is the mandated share of profits to be shared with workers, such that firm's profits become $F_D(w)(1-\tau_\pi)(1-\delta)(p-(1+\tau_\ell(w))w)$.

If workers are outsourced instead, the labor supply function, denoted by F_O, satisfies

$$F_O(w) \propto (w + B(\underline{w}) - \underline{b})^\beta. \tag{U3}$$

Then, the firm's problem is given by

$$\pi_O = \max_w F_O(w)[(1-\tau_\pi)(p - w - \tau_\ell(\underline{w})\underline{w} - \kappa - e) + \gamma e], \tag{U4}$$

where κ is the fee paid by the firm to the staffing company for its services, e is corporate profit shifting, and γ the share of shifted profits that the staffing company redistributes to the firm free of taxes. Note that outsourcing offers three advantages for the firm: it frees the firm from profit sharing regulations, lowers the payment of social security contributions to the minimum, and enables profit shifting (i.e., corporate tax evasion).

To understand how outsourcing may enable profit shifting, note that the total payment made by the firm to the staffing company, defined as $F \equiv (1+VAT)\big(w+\tau_\ell(\underline{w})\,\underline{w}+\kappa+e\big)$, is treated as an intermediate input for fiscal purposes and therefore includes VAT, which is creditable for the firm. However, the legal responsibility for remitting VAT to the government lies with the staffing company rather than the firm.[51] As a result, outsourcing transactions create an organizational separation that can facilitate VAT non-compliance upstream. In particular, the staffing company may use documented VAT evasion methods—such as offsetting VAT liabilities using fictitious input invoices (see Carrillo et al., 2023)—to avoid remitting VAT associated with e. We capture this channel in reduced form by assuming that the staffing company retains a fraction $(1-\gamma)e$ as compensation for evasion-related costs, which could be financial and reputational, and enforcement risks, while redistributing the remaining fraction γe to the firm. In Appendix U.6, we provide a simple microfoundation for the determination of e for the interested reader, formally relating it to enforcement risk and evasion costs.

Two clarifications are in order. First, profit shifting is not assumed to be the primary motivation for outsourcing; its inclusion simply demonstrates that, in the presence of corporate taxation, an outsourcing ban need not raise—and may even reduce—total labor costs, as it provides an incentive to overreport labor costs in contexts of weak tax enforcement. Second, although domestic outsourcing for profit shifting is particularly salient in such weak enforcement environments, manipulating intermediate input prices to reduce tax burdens is not unique to developing countries and parallels transfer pricing by US multinationals, whereby high-tax entities purchase inputs from low-tax foreign affiliates at inflated prices to shift taxable income, ultimately reducing global corporate tax liabilities (Clausing, 2003).

[51] Note that VAT is not deductible for corporate taxation purposes, as reflected by the formulation of π_0 above.

U.2 Optimal Wages and Markdowns

U.2.1 Wages

Solving the problem of the firm when it chooses to hire workers directly yields the following expression for the optimal wage of workers, inclusive of social security benefits:

$$w_D^* + B(w_D^*) = \left(\frac{\beta}{1+\beta}\right) p + \left(\frac{1}{1+\beta}\right) \underline{b}. \tag{U5}$$

Two features of this result are worth noting. First, this wage rule mirrors the surplus rule delivered by the standard rent-sharing model, with $\beta/(1+\beta)$ playing the role of the worker's bargaining weight, and $\underline{b}$ playing the role of the worker's outside option. Second, profit sharing is nondistorting, as it does not appear anywhere in this expression. This result is well-known in the taxation literature. Profit taxes, such as corporate taxes and profit sharing, here captured by τ_π and PTU, do not distort investment decisions of established firms that do not actively issue equity, since realized profits are proportionately lower than without taxes in every period (see Korinek and Stiglitz, 2009).[52]

Solving the problem of the firm when it chooses to outsource workers yields:

$$w_O^* + B(\underline{w}) = \left(\frac{\beta}{1+\beta}\right) \left[p - \kappa - e\left(1 - \frac{\gamma}{1-\tau_\pi}\right)\right] + \left(\frac{1}{1+\beta}\right) \underline{b}. \tag{U6}$$

Here again, two features of this expression are worth highlighting. First, the management fee κ reduces the total compensation of the workers by diverting surplus away from the workers to the staffing firm. Second, corporate tax evasion can further depress worker compensation whenever avoided taxes are less than the payment made to the staffing company to cover evasion-related costs and enforcement risks, that is, whenever $\tau_\pi < 1-\gamma$. In this case, avoidance aimed at lowering corporate tax liabilities ultimately reduces the surplus available to workers.

[52]This theoretical result breaks down if investment can only be funded through realized profits, which is the case for firms without access to formal credit (see Buera, Kaboski and Shin, 2015). In this scenario, the reform would have a particularly detrimental effect on the survival and growth of firms that lack access to formal credit.

U.2.2 Markdowns

We derive expressions for wage markdowns and firm profits in each scenario. Consistent with our empirical analysis, we define markdowns as $\upsilon \equiv MRPL/w$, with markdowns greater than 1 signifying worker exploitation in the sense of Robinson (1933). When the firm hires workers directly, markdowns are given by

$$\upsilon_D(w_D^*) = \frac{1}{\frac{\beta}{1+\beta} + \frac{1}{1+\beta}\frac{\underline{b}}{p}}. \tag{U7}$$

This expression implies that the markdown ratio converges to 1 as labor supply becomes perfectly elastic, or as worker bargaining power approaches 1. Conversely, the markdown ratio converges to $p/\underline{b}$ as labor supply becomes perfectly inelastic, or as worker bargaining power approaches 0.

When the firm outsources workers, markdowns are given by

$$\upsilon_O(w_O^*) = \frac{1}{\frac{\beta}{1+\beta}\frac{p-\kappa-e\left(1-\frac{\gamma}{1-\tau\pi}\right)}{p} + \frac{1}{1+\beta}\frac{\underline{b}}{p}}. \tag{U8}$$

This expression shows that markdowns do not tend to 1 as labor supply becomes perfectly elastic, or as worker bargaining power approaches 1. Rather, markdowns attain a minimum, given by the ratio of the marginal revenue product of productivity adjusted by the amount of resources diverted to the staffing company. From both expressions above, it is straightforward to see that $\upsilon_O(w_O^*) > \upsilon_D(w_D^*)$.

U.3 Profits and the Decision to Outsource

In this model, the decision to outsource workers can be divided into cases. If the minimum wage is not binding, the firm will outsource its workers for sufficiently high productivity levels, since the gains from bypassing profit sharing regulations are convex in productivity. To see why, consider first the optimal profits of the firm when it hires workers directly, which we derive by substituting Equation (U5) into the profit function from Equation (U2) and the labor supply function from Equation (U1):

$$\pi_D^*(p) = (1-\tau_\pi)\left(\frac{\beta+\delta}{1+\beta}\right)^\beta \frac{1-\delta}{1+\beta}(p-\underline{b})^{1+\beta}. \tag{U9}$$

This function can be compared against optimal profits under outsourcing, which we derive analogously, by substituting Equation (U6) into the profit function from Equation (U4) and the labor supply function from Equation (U3), to obtain:

$$\pi_O^*(p) = (1-\tau_\pi)\left(\frac{\beta}{1+\beta}\right)^\beta \frac{1}{1+\beta}\left(p-\kappa-e\left(1-\frac{\gamma}{1-\tau_\pi}\right)-\underline{b}\right)^{1+\beta}. \tag{U10}$$

From Equations (U9) and (U10), it is straightforward to see that profits are higher under outsourcing than under direct hiring for sufficiently high productivity levels, a result which we formalize in Proposition 1.

Proposition 1. *For every $\delta \in (0,1)$, there exists a $\hat{p} > 0$ such that $\pi_O^*(p) > \pi_D^*(p)$ for all $p > \hat{p}$.*

Proof. From Equations (U9) and (U10), it can be shown that $\pi_O^*(p) > \pi_D^*(p)$ if and only if $\frac{p-\kappa-e(1-\frac{\gamma}{1-\tau_\pi})-\underline{b}}{p-\underline{b}} > \left[\frac{\beta+\delta}{\beta}\right]^\beta(1-\delta)$ using some algebra. Taking the limit of this inequality as $p \to \bar{b}$ and $\bar{b} \to \infty$, we obtain $1 > \left[\frac{\beta+\delta}{\beta}\right]^\beta(1-\delta)$. However, taking logs, we obtain that this inequality holds true if and only if $0 > \beta\log\left(1+\frac{\delta}{\beta}\right)+\log(1-\delta)$. Using the standard inequality $\log(1+x) \leq x$ for $x > -1$, we get $\beta\log\left(1+\frac{\delta}{\beta}\right)+\log(1-\delta) < \delta+\log(1-\delta)$. But for $0 < \delta < 1$, we have $\log(1-\delta) < -\delta$, which yields the desired result. □

In addition to outsourcing workers when the productivity of the match is sufficiently high, the firm will choose to outsource workers for lower levels of productivity when the minimum wage is binding. In particular, if the productivity of the match is lower than the minimum statutory labor compensation but exceeds the fee payment to the staffing company, or if productivity exceeds the minimum statutory compensation but hiring directly under the minimum wage is more costly than outsourcing workers, the firm will choose to outsource. We formalize this result in Proposition 2 below.

Proposition 2. *Suppose $w_D^* < \underline{w}$. If $\underline{w}(1+\tau_\ell(\underline{w})) > p > \kappa+e(1-\frac{\gamma}{1-\tau_\pi})+\underline{b}$, the firm will choose to outsource, as outsourcing yields strictly positive profits while direct hiring is infeasible. Furthermore, if $p > (1+\tau_\ell(\underline{w}))\underline{w} > \kappa+e(1-\frac{\gamma}{1-\tau_\pi})+\underline{b}$, such that direct hiring is feasible, but $p < \tilde{p}$, where*

$\tilde{p} = (\frac{1+\beta}{\beta})(1+\tau_\ell(\underline{w}))\underline{w} - \frac{1}{\beta}\underline{b}$ is the value of p for which $w_D^* = \underline{w}$, such that the minimum wage is still binding, there exists a nonempty interval within this binding region for which the firm also outsources.

Proof. For the first part of the proposition, from Equation (U2) and the formula for profit sharing, note that $\pi_D(\underline{w}) = F_D(\underline{w})(1-\tau_\pi)(1-\delta)(p-(1+\tau_\ell(\underline{w}))\underline{w})$. Then, $\pi_D(\underline{w}) < 0$ follows from the assumption that $\underline{w}(1+\tau_\ell(\underline{w})) > p$. In contrast, from Equation (U10) and the inequality $p > \kappa + e(1-\frac{\gamma}{1-\tau_\pi}) + \underline{b}$, it follows that $\pi_O^*(p) > 0$. For the second part of the proposition, Equation (U2) and the formula for profit sharing, coupled with the profit function in Equation (U4), imply that $\pi_D(\underline{w}; p) > \pi_O^*(p)$ if and only if $(1-\delta)(p-(1+\tau_\ell(\underline{w}))\underline{w})\Big((1+\tau_\ell(\underline{w}))\underline{w} + \delta\big(p-(1+\tau_\ell(\underline{w}))\underline{w}\big) - \underline{b}\Big)^\beta > \left(\frac{\beta}{1+\beta}\right)^\beta \left(\frac{1}{1+\beta}\right)\left(p-\kappa-e\left(1-\frac{\gamma}{1-\tau_\pi}\right)-\underline{b}\right)^{\beta+1}$. Define $S: [(1+\tau_\ell(\underline{w}))\underline{w}, \tilde{p}] \to \mathbb{R}^+$ as the ratio of the left-hand side to the right-hand side of this inequality. $S(p)$ is continuous because it is the ratio of continuous and positive functions defined over its domain. Note that $S((1+\tau_\ell(\underline{w}))\underline{w}) = 0$. We are now able to proceed by cases. If $\tilde{p} < \hat{p}$, it must be the case that $S(\tilde{p}) > 1$, as $\pi_D^*(p) > \pi_O^*(p)$ for all $p < \hat{p}$ by Proposition 1. Thus, there exists a $\check{p} < \tilde{p}$ such that $S(\check{p}) = 1$. Furthermore, the threshold $\check{p}$ is unique. To see this,note that differentiating $\log S(p)$ yields

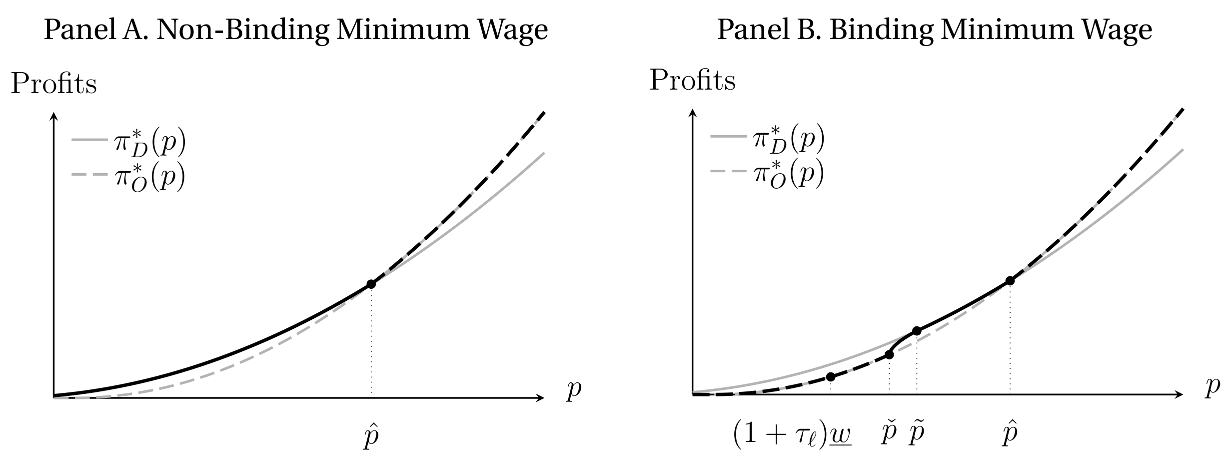
$$\frac{d}{dp}\log S(p) = \frac{1}{p-(1+\tau_\ell(\underline{w}))\underline{w}} + \frac{\beta\delta}{(1+\tau_\ell(\underline{w}))\underline{w} + \delta\big(p-(1+\tau_\ell(\underline{w}))\underline{w}\big) - \underline{b}} - \frac{\beta+1}{p-\kappa-e\left(1-\frac{\gamma}{1-\tau_\pi}\right)-\underline{b}}.$$

Putting this expression over a common denominator and simplifying shows that the numerator is affine in $p-(1+\tau_\ell(\underline{w}))\underline{w}$, while the denominator is strictly positive over the entire interval. Hence $\frac{d}{dp}\log S(p)$, and therefore $S'(p)$, can change sign at most once on $[(1+\tau_\ell(\underline{w}))\underline{w}, \tilde{p}]$. It follows that $S(p)$ is either monotone or single-peaked on this interval, so it can cross the level 1 at most once. Therefore, the threshold $\check{p}$ is unique. On the other hand, if $\tilde{p} \geq \hat{p}$, then $1 > \pi_D^*(p)/\pi_O^*(p) > S(p)$ for all values of $p \geq \tilde{p}$, so the firm always chooses to outsource by Proposition 1. □

Figure U.1 illustrates these results. Panel A plots firm profits as a function of productivity under direct hiring and outsourcing. The firm's optimal profit function is given by the upper envelope of the two profit schedules, which intersect at productivity level $\hat{p}$. For $p > \hat{p}$, profits under outsourcing increase more steeply than under direct hiring, reflecting the convex gains from avoiding profit-sharing regulations as productivity rises. For $p < \hat{p}$, by contrast, the fixed management fee and evasion costs associated with outsourcing initially dominate its benefits, making direct hiring the preferred regime.

Panel B considers the case in which the statutory minimum wage is binding. In this case, direct hiring yields negative profits over a range of intermediate productivity levels, as labor costs implied by the minimum wage exceed revenues net of profit sharing. By contrast, outsourcing can remain profitable over this range, since it allows the firm to pay a management fee below the statutory labor compensation.

Figure U.1: Profit Functions of the Firm by Hiring Regime



U.4 Impacts on Market Exit

When match productivity exceeds the minimum wage plus the associated statutory social security contributions, an outsourcing ban does not induce exit among incumbent firms. Firms that previously relied on outsourcing can continue operating profitably under direct hiring: even if their unconstrained optimal wage lies below the minimum wage, they can instead pay the minimum wage and still earn positive profits. By contrast, when match productivity falls below the minimum statutory worker compensation, the outsourcing ban removes the only hiring arrangement under which such low-productivity firms could operate profitably. As a result, these firms are no longer viable and are forced to exit the market. We formalize this mechanism in Proposition 3.

Proposition 3. *Suppose* $(1+\tau_\ell(\underline{w}))\underline{w} > \kappa + e(1-\frac{\gamma}{1-\tau_\pi}) + \underline{b}$*, such that the cost of outsourcing is lower than that of directly hiring workers at the minimum wage. An outsourcing ban will have no impact on market exit if p satisfies* $p > (1+\tau_\ell(\underline{w}))\underline{w}$*, but it will lead to market exit if p satisfies* $\underline{w}(1+\tau_\ell(\underline{w})) > p > \kappa + e(1-\frac{\gamma}{1-\tau_\pi}) + \underline{b}$.

Proof. We proceed by cases. Consider first the case where $p > (1+\tau_\ell(\underline{w}))\underline{w}$. Since $p > \kappa + e(1-\frac{\gamma}{1-\tau_\pi}) + \underline{b}$, it follows from Equation (U10) that $\pi_O^*(p) > 0$, so the firm could operate profitably under outsourcing before the ban. Moreover, from Equation (U2) and the profit-sharing rule, we have $\pi_D(\underline{w}; p) > 0$. Therefore, after the ban the firm can hire workers directly at $w = \underline{w}$ and earn positive profits, implying that the ban does not induce market exit. Consider next the case where $\kappa + e(1-\frac{\gamma}{1-\tau_\pi}) + \underline{b} < p < (1+\tau_\ell(\underline{w}))\underline{w}$. Then, $\pi_O^*(p) > 0$, so the firm could operate profitably under outsourcing prior to the ban. However, by Equation (U2) and the profit-sharing rule, we have $\pi_D(\underline{w}; p) < 0$. Therefore, the firm exits the market following the outsourcing ban. □

U.5 Welfare Effects of an Outsourcing Ban

We now turn to investigate the effect of an outsourcing ban on social welfare, which is derived from the number of matches between workers and firms. Before proceeding to analyze aggregate welfare effects, we present the welfare effects of the ban in a partial equilibrium setting involving the matches of a firm with productivity p, ignoring changes in workers' outside options. We then incorporate these effects into the analysis, also allowing for heterogeneity in firm productivity. Throughout, we highlight how the enforcement of a minimum wage, comprising take-home pay, social security benefits, and profit sharing, modifies monopsony distortions and, in turn, the efficiency of the ban.

U.5.1 Partial Equilibrium Setting

We define social welfare generated by a firm with productivity p as the sum of worker rents, firm profits, and government spending, evaluated at the optimal wage and given workers' minimum outside option $\underline{b}$. We denote this object by $W^D(p; \underline{b})$ under direct hiring. Under outsourcing, social welfare additionally includes the profits of the staffing company, which arise from the intermediation margin between firms and workers, and we denote this by $W^O(p; \underline{b})$. In both cases, we assume that government spending equals corporate tax revenue.

When the minimum wage is nonbinding, $W^D(p;\underline{b}) > W^O(p;\underline{b})$, since that the classical monopsony intuition applies: outsourcing depresses total compensation below the level that firms would offer under direct hiring, reducing the number of realized matches. By eliminating this wedge, the ban raises total compensation and expands employment at affected establishments. Proposition 4 proves this result formally.

Proposition 4. *When* $\min\{w_D^*, w_O^*\} > \underline{w}$ *(nonbinding minimum wage), an outsourcing ban will improve efficiency.*

Proof. Substituting Equation (U5) and the definition of profit sharing into the equation for worker rents yields $R_D(w_D^*) = [\frac{\beta+\delta}{1+\beta}p + \frac{1-\delta}{1+\beta}\underline{b}]F_D(w_D^*) - \int_{\underline{b}}^{w_D^*} b dF_D(b)$. Substituting Equation (U5) and the definition of profit sharing into the profit function from Equation (U2) yields $\pi_D(w_D^*) = F_D(w_D^*)(1-\tau_\pi)(1-\delta)[\frac{1}{1+\beta}p - \frac{1}{1+\beta}\underline{b}]$. Hence, $W^D(p;\underline{b}) = R_D(w_D^*) + \pi_D(w_D^*) + G = \int_{\underline{b}}^{w_D^*}(p-b)dF_D(b)$. Next, substituting Equation (U6) into the equation for worker rents yields $R_O(w_O^*) = [\frac{\beta}{1+\beta}(p - \kappa - e + \frac{e\gamma}{1-\tau_\pi}) + \frac{1}{1+\beta}\underline{b}]F_O(w_O^*) - \int_{\underline{b}}^{w_O^*} b dF_O(b)$, while substituting Equation (U6) into the profit function from Equation (U4) yields $\pi_O(w_O^*) = F_O(w_O^*)\frac{1}{1+\beta}((1-\tau_\pi)(p-\kappa-e-\underline{b}) + \gamma e)$. Hence, $W^O(p;\underline{b}) = R_O(w_O^*) + \pi_O(w_O^*) + \Lambda(w_O^*) + G = \int_{\underline{b}}^{w_O^*}(p-b)dF_O(b)$. Therefore, social welfare under direct hiring is greater than under outsourcing if and only if $F_D(w_D^*) > F_O(w_O^*)$, which, by the definitions of F_D and F_O, holds if and only if $w_D^* + B(w_D^*) + PTU > w_O^* + B(\underline{w})$. However, $w_D^* + B(w_D^*) + PTU > w_O^* + B(\underline{w})$ if and only if $\frac{\beta+\delta}{1+\beta}p + \frac{1-\delta}{1+\beta}\underline{b} > \frac{\beta}{1+\beta}(p - \kappa - e(1 - \frac{\gamma}{1-\tau_\pi})) + \frac{1}{1+\beta}\underline{b}$, which is true since $\kappa + e(1 - \frac{\gamma}{1-\tau_\pi}) > 0$. □

On the other hand, when the minimum wage is binding, two cases arise depending on whether match productivity exceeds the statutory minimum worker compensation. If productivity is sufficiently high, so that matches remain profitable, an outsourcing ban can reduce social welfare, such that $W^D(p;\underline{p}) < W^O(p;\underline{p})$, if and only if the additional cash payments received by workers due to lower social security contributions under outsourcing exceed the minimum compensation required under direct hiring, namely the minimum wage and profit-sharing payments. If productivity is too low, the ban prevents matches from forming altogether and thus unambiguously reduces social welfare. Proposition 5 formalizes this result.

Proposition 5. *Suppose $w_D^* < \underline{w}$. If $p > (1+\tau_\ell(\underline{w}))\underline{w}$, an outsourcing ban will reduce welfare if and only if $w_O^* > PTU + \underline{w}$. If $p < (1+\tau_\ell(\underline{w}))\underline{w}$, an outsourcing ban will reduce social welfare.*

Proof. We proceed by cases. Suppose first $p > (1+\tau_\ell(\underline{w}))\underline{w}$. Substituting $\underline{w} + B(\underline{w}) = \underline{w}(1+\tau_\ell(\underline{w}))$ and the definition of profit sharing into the equation for worker rents yields $R_D(\underline{w}) = [(1-\delta)(1+\tau_\ell(\underline{w}))\underline{w} + \delta p]F_D(\underline{w}) - \int_{\underline{b}}^{\underline{w}} b\, dF_D(b)$. Substituting $\underline{w} + B(\underline{w}) = \underline{w}(1+\tau_\ell(\underline{w}))$ and the definition of profit sharing into the profit function from Equation (U2) yields $\pi_D(\underline{w}) = F_D(\underline{w})(1-\tau_\pi)(1-\delta)[p-(1+\tau_\ell(\underline{w}))\underline{w}]$. Hence, $R_D(\underline{w})+\pi_D(\underline{w})+G = \int_{\underline{b}}^{\underline{w}}(p-b)\,dF_D(b)$. Therefore, the ban will reduce social welfare if and only if $F_D(\underline{w}) < F_O(w_O^*)$, which holds if and only if $\underline{w} + PTU < w_O^*$ by the definitions of F_D and F_O. Next, suppose $p < (1+\tau_\ell(\underline{w}))\underline{w}$. By Proposition 3, the firm will exit the market, so the ban eliminates the match and therefore reduces welfare. □

U.5.2 General Equilibrium

When the minimum wage is nonbinding, nothing precludes the minimum outside option $\underline{b}$ from being arbitrarily low. The value of such an arbitrarily low outside option is bounded by below only by $\underline{p}$. To see why, note that the minimum outside option of the worker is equal to the minimum total labor compensation that the worker can draw in the market. Therefore, $\underline{b} = \frac{\beta+\delta}{1+\beta}\underline{p} + \frac{1-\delta}{1+\beta}\underline{b}$, which implies $\underline{b} = \underline{p}$. From this result, it follows that the ban does not affect the minimum outside option of the worker.

The main implication of this result is that the effect of the reform for transitioning firms in partial equilibrium coincides with its effect in general equilibrium, as the reform does not alter search behavior, which is pinned down by the minimum outside option. Proposition 6 formalizes this result.

Proposition 6. *When $\underline{p} > (1+\tau_\ell(\underline{w}))\underline{w}$ (nonbinding minimum wage for all firms), an outsourcing ban increases aggregate welfare.*

Proof. Define the aggregate welfare change from the reform as the integral of the welfare change across the firm productivity distribution:

$$\Delta W = \int_{\hat{p}}^{\bar{p}} (W^D(p;\underline{p}) - W^O(p;\underline{p}))\,dH(p) = \int_{\hat{p}}^{\bar{p}} \int_{w_O^*(p)}^{w_D^*(p)} (p-b)\,dF(b) > 0, \tag{U11}$$

where the first equality follows from the absence of a change in hiring modality for firms with $p < \hat{p}$ and from the invariance of the minimum outside option, $\underline{b} = \underline{p}$, while the second equality and the final inequality follow from the proof of Proposition 4. $\square$

In contrast, when the minimum wage is binding, the minimum compensation that the worker can draw in the market is given by $\underline{w}(1+\tau_\ell(\underline{w}))+\delta(p-\underline{w}(1+\tau_\ell(\underline{w})))$, and the minimum level of productivity under which the firm may remain profitable is $\underline{w}(1+\tau_\ell(\underline{w}))$, such that the minimum outside option $\underline{b}$ equals $\underline{w}(1+\tau_\ell(\underline{w}))$. Therefore, the minimum outside option before the ban, $\underline{p}$, differs from the minimum outside option after the ban, $\underline{w}(1+\tau_\ell(\underline{w}))$.

In this case, the employment gains that would otherwise be observed may not be realized. In particular, these gains may be undone by (i) the loss in employment for firms that are no longer profitable and therefore exit the market, (ii) a potential loss in employment at low-productivity firms where workers were better off being paid in cash rather than being paid the minimum compensation package, and (iii) the generalized improvement in the outside options of employed workers, which lowers the attractiveness of their current match. Proposition 7 formalizes this result.

Proposition 7. *When $\underline{p} < \underline{w}(1+\tau_\ell(\underline{w}))$ (minimum wage binding for a subset of firms), the reduction in monopsony distortions induced by the ban may be offset by (i) the exit of low-productivity firms, (ii) potential efficiency losses among low-productivity firms that continue operating, and (iii) an upward shift in workers' outside options, rendering the aggregate welfare effect ambiguous.*

Proof. Define the aggregate welfare change from the reform as the integral of the welfare change across the firm productivity distribution:

$$
\begin{aligned}
\Delta W &= -\int_{\underline{p}}^{(1+\tau_\ell(\underline{w}))\underline{w}} W^O(p;\underline{p})\,dH(p) \\
&\quad + \int_{(1+\tau_\ell)\underline{w}}^{\check{p}} \left(W^D(p;(1+\tau_\ell(\underline{w}))\underline{w}) - W^O(p;\underline{p})\right) dH(p) \\
&\quad + \int_{\check{p}}^{\tilde{p}} \left(W^D(p;(1+\tau_\ell(\underline{w}))\underline{w}) - W^D(p;\underline{p})\right) dH(p) \\
&\quad + \int_{\tilde{p}}^{\hat{p}} \left(W^D(p;(1+\tau_\ell(\underline{w}))\underline{w}) - W^D(p;\underline{p})\right) dH(p) \\
&\quad + \int_{\hat{p}}^{\bar{p}} \left(W^D(p;(1+\tau_\ell(\underline{w}))\underline{w}) - W^O(p;\underline{p})\right) dH(p) \\
&= \underbrace{-\int_{\underline{p}}^{(1+\tau_\ell(\underline{w}))\underline{w}} W^O(p;\underline{p})\,dH(p)}_{\text{Market exit of low-productivity firms}<0} \\
&\quad + \underbrace{\int_{(1+\tau_\ell)\underline{w}}^{\check{p}} \left(W^D(p;\underline{p}) - W^O(p;\underline{p})\right) dH(p)}_{\text{Potential efficiency loss for low-productivity outsourcing firms continuing operations}\lesseqqgtr 0} \\
&\quad + \underbrace{\int_{(1+\tau_\ell)\underline{w}}^{\bar{p}} \left(W^D(p;(1+\tau_\ell(\underline{w}))\underline{w}) - W^D(p;\underline{p})\right) dH(p)}_{\text{Improvement in the outside options of employed workers}<0} \\
&\quad + \underbrace{\int_{\hat{p}}^{\bar{p}} \left(W^D(p;\underline{p}) - W^O(p;\underline{p})\right) dH(p)}_{\text{Efficiency gain for high-productivity firms}>0},
\end{aligned}
\tag{U12}
$$

where the second equality follows from adding and subtracting $W^D(p;\underline{p})$ from all terms in the left-hand side of the equation, subsequently grouping terms, and applying the results in Propositions 4 and 5. □

U.6 Microfoundation for Corporate Tax Evasion

We assume that the fraction of evaded corporate taxes redistributed to the firm by the staffing company is determined via Nash bargaining, with the firm's bargaining weight given by γ. The staffing company's ability to inflate the invoice F for the employing firm is limited by the detection algorithm of the fiscal authority, which is generally designed to ramp up the probability of an audit when an observable proxy for evasion—such as the ratio of revenues F to the declared payroll $\underline{w}$—rises. For simplicity, we capture this detection algorithm by assuming that the detection probability is an increasing function of e. The overreporting amount is therefore set by the staffing company to maximize its profits:

$$\max_e F - w - \tau_\ell(\underline{w})\underline{w} - \kappa - \gamma e - P(e)F,$$

where P denotes the probability of detection, which is assumed to be an increasing and continuously differentiable function, and F denotes a penalty for fraud detection. The solution of this problem gives the optimal corporate tax evasion amount

$$e = P'^{-1}\left(\frac{1-\gamma}{F}\right).$$

This expression reveals two key comparative statics. First, evasion is increasing in the staffing company's bargaining power $1-\gamma$, which determines the share of evaded taxes it appropriates. Second, evasion is decreasing in the expected penalty for detection, with stricter enforcement (i.e., higher F or steeper P') reducing equilibrium evasion.

V Alternative Theories

This section presents theoretical predictions about the effects of an outsourcing ban on employment and wages under two alternative models of wage determination: classical monopsony and rent sharing. Section V.1 outlines a common economic environment for both scenarios, featuring directly hired and outsourced workers. Section V.2 details the key theoretical predictions of the classical monopsony model. Section V.3 explores the key theoretical predictions of the rent sharing model.

V.1 Environment

We consider a static economic environment with two sectors in which a consumption good is produced by monopsonistic firms, and where staffing services with a comparative advantage in personnel management rent outsourced labor to the producing firm. All payments in the economy are made in terms of the consumption good, which is the numeraire. For simplicity, we assume that a single constant-returns-to-scale staffing firm provides staffing services, while

N symmetric producing firms with diminishing returns to scale exist.

V.1.1 Producing Firm

The producing firm operates a production function f that uses directly hired labor l_i, outsourced labor l_o, capital k, and raw materials x as inputs. The wage for directly hired workers is denoted by w_i. We consider two alternative scenarios for the way in which this wage is determined. However, in both scenarios, the firm is a price taker in the market for other inputs, including outsourced labor, which it rents at price w_o. We view the wage as encompassing total compensation, inclusive of wages and benefits, implicitly assuming that workers value wages and benefits one-for-one.[53] Capital is rented at a rate r in the capital market, and raw materials are purchased at a price of q.

We assume that directly hired labor and outsourced labor are perfect substitutes but acknowledge that insourced workers may carry an additional cost associated with tax compliance and human resource (HR) administration, which we denote by $a_i < 1$.[54] The assumption that $a_i < 1$ implies that HR costs are less than the direct cost of paying the worker.

Under these assumptions, the profit function of the producing firm is

$$\pi = f(k, x, l_i + l_o) - w_i(1 + a_i)l_i - w_o l_o - rk - qx.$$

V.1.2 Staffing Services

Staffing services rent outsourced labor, n, at a price of w_o to the producing firm. They are assumed to be price-takers in a competitive output market. Furthermore, as with producing firms, staffing services are assumed to face a per dollar HR cost for managing the wage bill, denoted by $a_o < 1$. Note that $a_i > a_o$ would indicate a relative efficiency advantage of staffing

[53] Although benefits (e.g., profit-sharing, pensions, healthcare insurance) are a key margin through which monopsonistic firms exercise market power, we abstract from this adjustment margin in our exposition to focus on the role of monopsony power on wage determination.

[54] In principle, one could allow this to vary across firms in order to yield heterogeneity in the use of outsourcing across firms.

firms in handling personnel.[55] The profit function of the staffing company is therefore

$$\pi_o = w_o n - (1 + a_o)\,\tilde{w}_o n.$$

Given these technologies, we consider alternative profit maximization and wage determination scenarios for both the producing firms and the staffing company.

V.2 Classical Monopsony

Consider the case of an upward-sloping supply curve which the producing firms internalize. Specifically, the wage for directly hired workers is denoted by $w_i(l_i; L_{-i})$, where external labor demand from other sources, L^{-}, is taken as given. In equilibrium, given symmetry, $L^{-} = (N-1)\,l_i + N l_o$. The producing firm's profit maximization problem is therefore

$$\max_{k,x,l_i,l_o} f\,(k,x,l_i+l_o) - w_i\,(l_i;L^{-})\,(1+a_i)\,l_i - w_o l_o - rk - qx.$$

The first-order condition for profit maximization with respect to capital is:

$$\frac{\partial f}{\partial k} = r. \tag{V1}$$

The firm will choose the type and amount of labor that offers the highest marginal product per dollar spent in labor payments. Specifically, since the marginal product of both types of workers is equalized, the firm will hire directly if

$$\frac{1}{(1+a_i)\left(w_i(l_i;L_{-i}) + w_i'(l_i;L_{-i})\,l_i\right)} \geq \frac{1}{w_o}, \tag{V2}$$

and it will rent outsourced labor if the converse inequality holds. The firm will hire a mix of both types of labor only if the inequality above holds as an exact equality at the margin. In such a case, that equality will be reached by the decision of the firm itself, making the mix of directly

[55] The technology for staffing services exhibits constant returns to scale, meaning that the optimal firm size is potentially determined solely by the aggregate supply of labor, as in the case of classical monopsony power. To avoid this equilibrium outcome, one could introduce a convex cost of providing staffing services, $a_o(n)$, which might include probabilistic penalties for tax avoidance. However, for simplicity, we abstract from this consideration.

hired and outsourced labor determinate.

Further assuming that producing firms face an isoelastic aggregate supply curve for total labor $L = w^{\eta}$, where η denotes the Frisch elasticity of labor supply, we can rewrite Equation (V2) as follows:

$$\frac{1}{(1+a_i)\,\mu_i w_i} \geq \frac{1}{w_o},$$

where $\mu_i = 1 + \frac{1}{\eta}\frac{l_i^*}{L} \geq 1$ is the markdown, which decreases with the elasticity of aggregate labor supply, η, and increases with the size of the firm, l_i^*, relative to the total labor market, L.

Likewise, the staffing company faces the total labor supply curve, $L = w^{\eta}$, and internalizes only its own contribution to it. That is, it realizes that $L = L_{-n} + n$ but takes L_{-n} as given. From the staffing company's profit-maximization problem, the first-order condition with respect to outsourced labor can be written as a Lerner condition for the wage as a markdown on the marginal product of labor,

$$w_o = \tilde{w}_o \mu_o (1+a_o)\,, \tag{V3}$$

where $\mu_o = \left(1 + \frac{1}{\eta}\frac{n^*}{L}\right) \geq 1$ is the markdown.

We assume parameter values ensuring that $n^* > l_i^*$ (i.e., sufficiently large a_i and N, and sufficiently small a_0, r, and q), from which it follows that $\mu_o > \mu_i$. This assumption reflects the empirical observation that staffing companies are typically larger than producing firms and therefore face a more inelastic labor supply curve, enabling them to exert more market power.

We examine the impact of the outsourcing ban on total employment, output, wages, and the labor share of the producing firms. To simplify the exposition, we exclude raw materials and assume the production function is Cobb-Douglas in capital and composite labor, as follows:

$$f(k, l_i, l_o) = k^{\alpha}(l_i + l_o)^{\tilde{\beta}},$$

where $\alpha + \tilde{\beta} < 1$.

For illustrative purposes, we assume the firm relied solely on outsourced labor before the ban, making it fully exposed to the regulatory change, and ensuring that all labor was hired by a single monopsonistic staffing company before the ban. For the following derivations, it will be useful to substitute the firm's demand for capital as a function of the interest rate from Equation

(V1) into the production function to get

$$f(l_o, l_i; r) = A(r)\,(l_i + l_o)^{\beta},$$

where $A(r) \equiv (\alpha/r)^{\frac{\alpha}{1-\alpha}} > 0$ and $0 < \beta \equiv \tilde{\beta}/(1-\alpha) < 1$. From this substitution, it is evident that output and employment impacts at the firm level will always run in the same direction. Moreover, by spanning the positive range of r, we can attain any positive value of A. Therefore, we can consider A as a parameter instead of r without loss of generality.

To keep track of total employment in the producing firm, we define $l \equiv l_i + l_o$. Finally, we denote the employment gain after the ban as $\Delta_l = l^{post} - l^{pre}$, the wage gain as $\Delta_w = w^{post} - w^{pre}$, and the change in the labor share of revenue as $\Delta_{s_L} = s_L^{post} - s_L^{pre}$.

Proposition 8. *For all values of A, a_i, and a_o, such that $n = L$ before the ban, there exists a small enough value η^* of the Frisch elasticity of labor such that $\Delta_l > 0$, $\Delta_w > 0$, and $\Delta_{s_L} > 0$.*

Proof. Under the assumed functional form for the production function, the firm's first-order condition with respect to outsourced labor before the ban is

$$\beta A l_o^{\beta-1} = w_o.$$

By substituting Equation (V3) into the first-order condition, applying the labor supply equation for outsourced labor, and rearranging the terms, we derive the pre-reform employment level

$$l^{pre} = l_o^* = \left[\frac{\beta A}{N^{1/\eta}(1+a_o)\mu_o}\right]^{\frac{\eta}{\eta(1-\beta)+1}}, \tag{V4}$$

with $n^* = L$, such that $\mu_o = 1 + \frac{1}{\eta}$.

After the ban, the firm's first-order condition with respect to directly hired labor is

$$\beta A l_i^{\beta-1} = (1+a_i)\,w_i\mu_i.$$

By substituting the labor supply equation into this first-order condition and rearranging terms, we derive the post-reform employment level

$$l^{post} = l_i^* = \left[\frac{\beta A}{N^{1/\eta}(1+a_i)\mu_i} \right]^{\frac{\eta}{\eta(1-\beta)+1}}, \tag{V5}$$

where $\mu_i = 1 + \frac{1}{\eta}\frac{1}{N}$.

Now, we can use Equations (V4) and (V5) to clearly show

$$\frac{l^{post}}{l^{pre}} = \left[\frac{(1+a_o)\mu_o}{(1+a_i)\mu_i} \right]^{\frac{\eta}{\eta(1-\beta)+1}}.$$

By substituting the markdown definitions into the right-hand side of this equation, we derive the following necessary and sufficient condition for the ratio on the left-hand side to be greater than 1:

$$\eta < \frac{1}{a_i - a_o}\left(1 + a_o - \frac{1+a_i}{N}\right) = \eta^*,$$

where $\eta^* > 0$, provided that $N > 1$ and $a_i, a_o < 1$. This condition states that, for employment to rise with the outsourcing ban, the reduction in monopsony power resulting from dismantling the staffing company, governed by η and N, must exceed the reduction in HR costs associated with outsourcing, governed by a_i and a_o.

Furthermore, from the labor supply equation, it is clear that the wage is increasing in the labor demand ratio:

$$\frac{w^{post}}{w^{pre}} = \left[\frac{l^{post}}{l^{pre}} \right]^{\frac{1}{\eta}},$$

implying that wages increase under the same conditions as labor.

Finally, we show that $\Delta_{s_L} > 0$ for all values of η. Assuming that the government uses payroll taxes to fund the social security benefits of workers, we have

$$s_L^{pre} = s_{l_o} = \frac{\tilde{w}_o l_o}{k^\alpha l_o^{1-\alpha}} = \frac{1-\alpha}{(1+a_o)\mu_o}, \text{ and}$$

$$s_L^{post} = s_{l_i} = \frac{w_i l_i}{k^\alpha l_i^{1-\alpha}} = \frac{1-\alpha}{(1+a_i)\mu_i}.$$

Therefore, the ratio of labor shares follows the same direction as the wage ratio:

$$\frac{s_L^{post}}{s_L^{pre}} = \left[\frac{(1+a_i)\mu_i}{(1+a_o)\mu_o} \right],$$

so labor shares move in the same direction as wages. □

Proposition 8 posits that an outsourcing ban will increase employment and, consequently, output, as well as wages and the labor share, when the reduction in monopsony power resulting from dismantling staffing companies outweighs the efficiency gains and cost savings achieved through outsourcing.

V.2.1 Extensions

We explore three extensions to the classical monopsony framework: (i) allowing directly hired and outsourced labor to face different supply elasticities, (ii) allowing the producing firm to partially outsource labor, and (iii) allowing for multiple staffing firms.

Different Labor Supply Elasticities. In the baseline model, both directly hired and outsourced labor are drawn from the same aggregate labor supply curve $L = w^{\eta}$. We now allow the supply elasticities to differ across hiring channels. Specifically, suppose there are two separate labor pools: directly hired workers are supplied according to $L_i = w_i^{\eta_i}$ and outsourced workers according to $L_o = \tilde{w}_o^{\eta_o}$. The producing firm hires l_i directly from the first pool and rents l_o outsourced workers from the staffing company, which draws from the second pool. Intuitively, a lower η_i than η_0 is plausible if workers value the amenities associated with direct hiring—such as social security coverage, profit sharing, and greater job stability—more highly than outsourced employment, making them less responsive to wage differences across firms once directly hired, with the opposite holding true if workers value flexibility.

Before the ban, at an interior solution where the firm uses both types of labor, the first-order conditions require the marginal product of composite labor to equal the marginal cost through each channel simultaneously:

$$\beta A(l_i + l_o)^{\beta-1} = (1+a_i)\mu_i (N l_i)^{1/\eta_i}, \tag{V6}$$

$$\beta A(l_i + l_o)^{\beta-1} = (1+a_o)\mu_o (N l_o)^{1/\eta_o}, \tag{V7}$$

where $\mu_i = 1 + \frac{1}{\eta_i N}$ and $\mu_o = 1 + \frac{1}{\eta_o}$ as before. Equating the right-hand sides of Equations (V6) and (V7) pins down the optimal ratio of directly hired to outsourced labor as a function of the parameters a_i, a_o, η_i, η_o, and N. The system of Equations (V6)–(V7) then jointly determines pre-ban employment levels l_i^{pre} and l_o^{pre}, with total pre-ban employment $l^{pre} = l_i^{pre} + l_o^{pre}$.

After the ban, the outsourced channel is shut down, and the firm hires exclusively from the directly hired pool. Post-ban employment is determined solely by the first-order condition for directly hired labor:

$$l^{post} = \left[\frac{\beta A}{(1+a_i)\mu_i N^{1/\eta_i}}\right]^{\frac{\eta_i}{\eta_i(1-\beta)+1}}. \tag{V8}$$

When $\eta_i = \eta_o = \eta$, the two labor pools are symmetric and the model collapses to the baseline, in which case Proposition 8 applies directly. When $\eta_i \neq \eta_o$, the pre-ban system does not admit a closed-form solution in general, but the qualitative predictions of the model can still be characterized.

The effect of the ban on total employment depends on the relative magnitude of η_i and η_o through two channels. First, a smaller η_i increases the post-ban markdown μ_i, increasing the monopsony distortion that directly hired workers face after the reform. Second, a larger η_o reduces the pre-ban markdown μ_o of the staffing company, lowering the monopsony distortion on outsourced workers before the reform. Both forces make the ban less likely to raise total employment. Conversely, if η_i is large relative to η_o (i.e., direct hiring faces a more elastic supply than outsourcing), the post-ban monopsony distortion is lower than the pre-ban distortion, and the ban is more likely to increase total employment.

Partial Outsourcing. Our benchmark model assumes the producing firm relies solely on outsourced labor before the ban. We relax this by introducing an augmented Cobb-Douglas production function in which only a fraction of the labor force is substitutable between directly hired and outsourced workers:

$$f(k, l_c, l_s + l_o) = k^{\alpha} l_c^{\gamma} (l_s + l_o)^{\tilde{\beta}},$$

where $\alpha + \gamma + \tilde{\beta} < 1$, l_c denotes "core" directly hired workers who perform tasks that cannot be outsourced, and l_s denotes directly hired workers performing tasks that are perfectly substitutable with outsourced labor l_o.

Before the ban, the producing firm optimally hires l_c directly while sourcing substitutable labor through the staffing company whenever outsourcing is cost-effective (i.e., whenever the inequality analogous to Equation (V2) holds for the substitutable component). Assuming that core and substitutable workers are drawn from separate labor markets, the reform operates exclusively through the substitutable margin. That is, the ban forces $l_o = 0$ and replaces it with directly hired substitutable labor l_s, leaving l_c and the capital choice unchanged at the margin.[56]

The predictions of Proposition 8 carry through for the substitutable component of employment: l_s^{post} exceeds l_o^{pre} under the same threshold condition on η. However, since core employment l_c is unaffected by the reform, the aggregate employment effect is attenuated in proportion to the share of substitutable labor in total employment. Formally, the total employment gain is

$$\tilde{\Delta}_l = l_s^{post} - l_o^{pre},$$

while the proportional change in total firm employment is $\frac{\tilde{\Delta}_l}{l_c + l_o^{pre}} < \frac{\Delta_l}{l^{pre}}$.

This extension implies that empirically, the effects of the outsourcing ban should be larger for firms with a higher share of outsourced (substitutable) workers at baseline, which is consistent with heterogeneous treatment effects across firms.

Multiple Staffing Firms. The baseline model assumes a single staffing firm ($n^* = L$). We now consider $N_s > 1$ symmetric staffing firms, each hiring $n_j = L/N_s$ workers and internalizing only its own contribution to the aggregate supply curve.

[56]More precisely, the ban may affect l_c and k through general equilibrium effects on factor prices. However, conditional on factor prices, the ban operates only through the substitutable margin.

The markdown of each staffing firm is

$$\mu_o = 1 + \frac{1}{\eta N_s}.$$

After the ban, each of the N producing firms faces the markdown $\mu_i = 1 + \frac{1}{\eta N}$ as before. The employment ratio generalizes to

$$\frac{l^{post}}{l^{pre}} = \left[\frac{(1+a_o)\left(1+\frac{1}{\eta N_s}\right)}{(1+a_i)\left(1+\frac{1}{\eta N}\right)} \right]^{\frac{\eta}{\eta(1-\beta)+1}}. \tag{V9}$$

Proposition 9. *The ratio in Equation (V9) exceeds unity if and only if*

$$\eta < \frac{\frac{1+a_o}{N_s} - \frac{1+a_i}{N}}{a_i - a_o} \equiv \eta^{**},$$

where $\eta^{**} > 0$ if and only if $N_s < N\frac{1+a_o}{1+a_i}$. When $N_s = 1$, $\eta^{**} = \eta^*$ as defined in Proposition 8.

Proof. From Equation (V9), $l^{post}/l^{pre} > 1$ if and only if $(1+a_o)\mu_o > (1+a_i)\mu_i$. Expanding the markdowns and rearranging:

$$(1+a_o) + \frac{1+a_o}{\eta N_s} > (1+a_i) + \frac{1+a_i}{\eta N}$$
$$\frac{1}{\eta}\left(\frac{1+a_o}{N_s} - \frac{1+a_i}{N}\right) > a_i - a_o.$$

The left-hand side is positive if and only if $N_s < N\frac{1+a_o}{1+a_i}$, and the inequality holds if and only if $\eta < \eta^{**}$. When $N_s = 1$, the threshold simplifies to $\eta^{**} = \frac{1+a_o-(1+a_i)/N}{a_i-a_o} = \eta^*$. □

The threshold η^{**} is decreasing in N_s: as the staffing sector becomes more fragmented, each staffing firm exercises less monopsony power, so the efficiency gains from dismantling staffing companies are smaller, and the ban is less likely to raise employment. The critical condition $N_s < N\frac{1+a_o}{1+a_i}$ confirms that the assumption of a single staffing firm is not crucial for the qualitative predictions of Proposition 8. What matters is that the staffing sector is sufficiently concentrated relative to the producing sector, so that staffing firms exert meaningfully more monopsony power than individual producing firms would.

V.3 Rent Sharing

For simplicity, we consider the same economic environment as in the previous section, but we incorporate standard modeling assumptions from the wage bargaining literature. Specifically, wages are determined via Nash bargaining over the firm's quasi-rents. If wage bargaining is unsuccessful, each party receives their respective outside option, and the firm liquidates its assets. The firm leases its capital stock on a period-by-period basis but faces a one-period delay between the decision to acquire capital and its availability for use. As is standard in this literature, we assume that workers value monetary payoffs, either wage, w, or an outside option, denoted by b.

V.3.1 Model Setup Under Internal Hiring

To simplify our exposition, we again drop raw materials from the production function. Consequently, the profits of the producing firm are equal to

$$\Pi(w_i) = f(k, l_i) - w_i(1+a_i)l_i - rk. \tag{V10}$$

If a wage agreement with workers is not reached, the firm is able to liquidate a fraction δ of its installed capital. Therefore, profits evaluated at the firm's outside option equal $\Pi^0 = -(1-\delta)rk$.

Let the quasi-rent of reaching an agreement be denoted by $S = w_i - b + \Pi(w_i) - \Pi^0$. We assume it is shared according to the Nash product

$$\max_{w_i - b, \Pi(w_i) - \Pi^0} (w_i - b)^{\phi_i} (\Pi(w_i) - \Pi^0)^{1-\phi_i},$$
$$\text{s.t. } S = w_i - b + \Pi(w_i) - \Pi^0,$$

with solution

$$w_i - b = \phi_i S \text{ and } \Pi(w_i) - \Pi^0 = (1-\phi_i)S. \tag{V11}$$

V.3.2 The Holdup Problem

To obtain the firm's demand for capital, we first substitute Equation (V10) and the definition of Π^0 into the definition of S to get

$$S = f(k, l_i) - b(1 + a_i)l_i - \delta r k. \tag{V12}$$

Then, we substitute Equations (V11) and (V12), and the definition of Π^0 into Equation (V10) to obtain the following expression for the profit function of the firm under direct hiring:

$$\Pi^{\text{Direct Hiring}} = (1-\phi_i)[f(k, l_i) - bl_i] - (1-\phi_i\delta)rk. \tag{V13}$$

Proposition 10. *When contracts are complete and capital is fully liquid (i.e., $\delta = 1$), investment is optimal (i.e., $f_k = r$). When contracts are incomplete and the firm can only liquidate part of its capital if negotiations fail (i.e., $\delta < 1$), investment is suboptimal (i.e., $f_k = \theta r$, where $\theta > 1$).*

Proof. By backward induction, Equation (V13) implies the capital choice of the firm must satisfy the following first-order condition:

$$\frac{\partial \Pi^{\text{Direct Hiring}}}{\partial k} = (1-\phi_i)\left[f_k - \theta r\right] = 0,$$

where

$$\theta = 1 + \frac{\phi_i}{1-\phi_i}(1-\delta) \geq 1.$$

□

Proposition 10 states that, if the producing firm and workers bargain over the surplus remaining after deducting the cost of capital, the so-called holdup problem will not arise, and the firm will invest (and hire labor) optimally.[57] However, if they bargain over the surplus before deducting the cost of capital, the holdup problem will lead to underinvestment since the firm is not the full residual claimant of the additional returns it generates through investment, as in Grout (1984). To see why, note that the wage expression in Equation (V11) is increasing in the firm's capital stock.

[57]The same result would carry over to raw materials.

This holdup problem gives a theoretical justification for outsourcing. Namely, outsourcing can restore optimality in capital investment decisions, as it allows firms to set wages that do not depend on the firm's capital stock, as described below.

V.3.3 Outsourcing

We assume that outsourcing interferes with the wage bargaining process by reducing the bargaining weight of workers, ϕ_o. There are several potential microfoundations to justify such reduction. For example, outsourcing could reduce the bargaining weight of workers by making them outsiders to the producing firm (see Lindbeck and Snower, 1988), who cannot unionize or threaten to take legal action against it.

Alternatively, outsourcing could reduce the bargaining weight of workers because staffing companies are larger than producing firms, and so they have a higher outside option if the employees of any one firm do not agree. Outside options can directly impact bargaining weights in sequential bargaining setups that yield Nash results when payoffs are concave because effective impatience is impacted (for a proof, see Binmore, Rubinstein and Wolinsky, 1986).

Irrespective of the microfoundation for the bargaining weight reduction, to simplify the derivations that follow, we assume without loss of generality that outsourced workers are completely stripped from bargaining power (i.e., $\phi_0 = 0$) and are therefore offered a wage equal to their outside option by the staffing company, so

$$\tilde{w}_o = b.$$

Consequently, if the producing firm employs only outsourced labor, its profits become

$$\Pi^{\text{Outsourcing}} = f(k, l_o) - b(1 + a_o)l_o - rk. \tag{V14}$$

Proposition 11. *Outsourcing leads to optimal capital investment (i.e., $f_k = r$).*

Proof. By backward induction, Equation (V14) implies the capital choice of the employing firm must satisfy the following first-order condition:

$$\frac{\partial \Pi^{\text{Outsourcing}}}{\partial k} = f_k - r = 0.$$

□

Proposition 11 posits that outsourcing eliminates the holdup problem by stripping workers of bargaining power. Outsourcing obliges workers to accept constant wages, making the firm the sole residual claimant of the additional returns it generates through capital investment.

Taken together, Propositions 10 and 11 imply that if firms and workers bargain over the surplus before deducting the cost of capital, an outsourcing ban will increase wages and the labor share without immediately affecting employment or output, while reducing capital investment.

As noted at the beginning of this section, we have assumed that workers value payroll benefits equally with wages. If they instead value payroll benefits less than wages, and staffing companies avoid paying these benefits, then the ban on staffing companies would lead to a loss in total surplus from employment. In either model, we conjecture this would lead to lower employment relative to what we observe in our empirical analyses.

V.3.4 Heterogeneous Productivity

We introduce heterogeneity in match productivity to examine whether the rent-sharing model generates differential effects of the ban across firms. Consider producing firms indexed by j, with production functions $f_j(k,l)$ ordered by productivity so that $f_1 < f_2 < \cdots < f_J$ for all (k,l).

Before the ban, outsourcing strips workers of bargaining power ($\phi_o = 0$), so all workers receive $\tilde{w}_o = b$ regardless of the productivity of the firm at which they are employed. After the ban, workers at firm j bargain over the quasi-rent S_j with bargaining weight $\phi_i > 0$. From the Nash bargaining solution in Equation (V11), the wage gain for workers at firm j is

$$\Delta w_j = w_j - b = \phi_i S_j.$$

Under general production technologies (e.g., CES with elasticity of substitution $\sigma < 1$), the surplus of the match S_j is increasing in firm productivity. Therefore, wage gains from the ban are largest at the most productive firms. Moreover, because all firms pay $\tilde{w}_o = b$ before the ban, the counterfactual labor markdown is also increasing in productivity: higher-productivity firms are more "distorted" in the sense that their workers forgo a larger wage premium under outsourcing. The ban compresses the cross-firm distribution of markdowns, with the largest effects concentrated at the most distorted firms, consistent with the empirical evidence.

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