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

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Abstract

A weakening of labor protection policies is often invoked as one cause of observed monopsony power and the decline in labor's share of income, but little evidence exists on the causal impact of labor policies on wage markdowns. Using confidential Mexican economic census data from 1994 to 2019, we document a rising trend over this period in on-site outsourcing, particularly among large firms, and a negative association between outsourcing and labor compensation, including profit sharing, employment benefits, and mandated 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 drastically reduced outsourcing, increased wages at the bottom of the distribution, increased labor's share, and reduced measured markdowns among high-markdown firms without lowering output or productivity or affecting employment or its composition. However, we also find that the reform reduced capital investment and increased the probability of market exit among smaller firms.

Keywords: Markdowns, Monopsony, Staffing Companies, Developing Countries.

JEL codes: J38, J42, J81, M55, O15.

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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 (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 than the use of other intermediate services that reduce costs and increase aggregate productivity (Bilal and Lhuillier, 2021), and in a competitive labor market, a reduction in labor costs could lead to increased employment. 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 labor reform consisting of a ban on the 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 no formal employer–employee relationship exists.[2] We exploit longitudinal establishment-level data and matched employer-employee data, unique within the literature on outsourcing in that it is comprehensive, nationally representative, and includes explicit data on firm outsourcing decisions. Using a difference-in-difference strategy, we find that the policy did indeed reduce core outsourcing and, in so doing, increased labor compensation and reduced markdowns without affecting employment or output, reflecting increases in mandated social security payments and profit sharing, which we observe directly. While our findings indicate that the reform was effective in improving worker outcomes, we also discover increases in total labor cost, as well as evidence of reduced capital investment and an increase in the probability of market exit.

Mexico is a particularly interesting and informative case to study core outsourcing, especially from the standpoint of its measurement and identification. First, although its institutional

[1] With regards to labor's share, Karabarbounis and Neiman (2014) document a global decline, while Grossman and Oberfield (2022) provide a broad review on the potential causes 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), while Brooks et al. (2021*a*,*b*) present evidence for India and China.

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

arrangements featuring prevalent core outsourcing, mandated social security, and mandated profit sharing are common in many countries, Mexico showed high and persistent use of core outsourcing prior to the reform. We document a rising prevalence of core outsourcing in the manufacturing sector, with the employment share of outsourced workers tripling from 7 percent 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, allowing us to compare establishments that previously outsourced workers to those that did not in a differences-in-differences specification. Third, we have comprehensive, longitudinal, establishment-level data and matched employer-employee data enabling us to document pre-existing patterns, estimate establishment markdowns, examine establishment responses, and quantify 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. Finally, the stakes are higher in developing countries such as Mexico, where worker wages are already lower than in developed countries and the subject of worker abuse more salient because the majority of workers do not receive labor benefits (Ronconi, 2019) and where employment and social security regulations are common policies under consideration.

Our empirical analysis starts by investigating the extent of labor exploitation before the reform, as measured by markdowns,[3] and its correlation with core outsourcing using several waves of quinquennial economic census data for the universe of establishments in Mexico's manufacturing sector. In line with the reform's statement of purpose (Gaceta Parlamentaria, 2020), which argued that core outsourcing enabled worker exploitation, we find that markdowns were high and pervasive before the reform, particularly among firms that outsourced. Moreover, consistent with the presence of monopsony power, we find that markdowns increased with firm size and that outsourcing prevalence was higher among large firms.

We then leverage data from a panel manufacturing survey recording monthly and annual information at the establishment level from 2013 to 2024 to quantify the causal impacts of the 2021 prohibition. We find evidence of a sizable rise in labor cost of 21 percent by 2023, explained by a 56 percent increase in mean wages, coupled with a relatively small rise in firing costs and

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

a steep reduction in the fees paid to staffing firms for their management services. The absence of pre-trends and the presence of clearly identifiable seasonal payments mandated by policy in the monthly data give us confidence that our empirical strategy is appropriate for identification of causal effects on labor costs.[4]

Owing to the increase in wages, we find that the reform increased the labor share at the establishment level by 6 percentage points and reduced markdowns by 23 percent, as measured by our preferred empirical specification. Moreover, we find no impacts on the establishment's employment, use of other productive inputs, output, or total factor productivity (TFP). However, we report an increase of 1 percentage point in the probability of market exit and a reduction in capital investment of 9 percent. These negative impacts are quantitatively small relative to the reduction in outsourcing and wage gains, however.

Next, using matched employer-employee data from the social security authority, we show that wage gains concentrate at the bottom of the distribution, implying that the reform reduced inequality between workers. We also use this data to show that the reform increased the probability of direct employment in the manufacturing sector for previously outsourced workers rather than leading to their firing and substitution.

We conjecture that the increased labor cost with no drop in employment could be explained by a shift in rents from monopsony power, and so we examine the role of monopsony power further. Returning to the manufacturing survey data, we show that the entire drop in markdowns is concentrated among the quartile of establishments with the highest markdowns, consistent with a reduction in market power. We also conjecture that the the negative impacts on investment and market exit reflect a reduction in misallocation. To validate our conjecture, we show that these negative impacts concentrate among low-revenue firms, consistent with the reform pushing out of the market the group of firms that remained in operation solely because of the cost advantage associated with worker exploitation.

Our paper contributes to a rapidly growing literature on labor market power, which includes studies that propose methods to estimate labor market power and its impacts on wages, markdowns, and employment in the U.S. (Benmelech, Bergman and Kim, 2022; Berger, Herkenhoff

[4] Specifically, we find a spike in salaries paid in December, corresponding to the dispersal at Christmas of the thirteenth month of pay for directly hired workers as mandated by Mexican legislation and an increase in profit sharing in May of each year, the month in which Mexican legislation mandates dividend dispersal.

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*,*b*; Amodio and Roux, 2022; Felix, 2021; Naidu, Nyarko and Wang, 2016; Zavala, 2022), with the focus of the most recent contributions being 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* since establishments do not simply move along a downward-sloping labor demand curve. Our results therefore validate the standard markdown measures that utilize observational data.

Our specific focus on outsourcing contributes to a second literature on domestic outsourcing, pioneered by Autor (2003), who uses an event study to show that state courts' decisions in the U.S. to protect workers against unjust dismissal in the 1980s fostered the growth of temporary help employment,[5] ultimately having the unintended consequence of reducing productivity and distorting production choices (Autor, Kerr and Kugler, 2007). Our empirical analysis advances the counterargument: while they can have unintended consequences, worker protections can also have the *intended* consequence of reducing exploitation. In this regard, our findings resonate with previous research results pointing to an association 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 the new insight to the literature that outsourcing helps firms bypass profit sharing regulations, previously shown to be disproportionately beneficial to lower-skill workers (Nimier-David, Sraer and Thesmar, 2023). In closely related work, Felix and Wong (2024) and Guo, Li and Wong (2024) show favorable impacts of outsourcing in the context of Brazil, the former also assessing a policy in Latin America: a reform legalizing the outsourcing of non-core employees. Although our findings differ, we see them as complimentary but instead insightful into domestic outsourcing of *core* workers.

Finally, our paper contributes to the literature on labor informality (see Ulyssea, 2020),[6]

[5] Relatedly, staffing services grew to add 9.2% to core manufacturing employment in the U.S. in 2006, compared to 2.3% in 1989 (Dey, Houseman and Polivka, 2012).

[6] Notable contributions to this literature for the Mexican case include Azuara and Marinescu (2013), Bosch and Campos-Vazquez (2014), Busso, Fazio and Levy (2012), Conover, Khamis and Pearlman (2022), Maloney (1999, 2004), and Samaniego de la Parra and Fernández Bujanda (2024).

which shows that hiring workers "off the books" is a common practice among small formal firms aiming to evade burdensome social security schemes (payroll taxes) to reduce costs and remain competitive.[7] We provide novel evidence that domestic outsourcing operates as a mechanism that enables large formal firms to evade payroll taxes on formally hired workers, analogous to how informality on the intensive margin benefits small firms. In this regard, the closest paper to ours is Best (2014), who uses data from Pakistan to show that salaried workers' taxable earnings are underreported by formal firms in response to taxation.

The remainder of the paper is structured as follows. Section 2 provides contextual information regarding domestic outsourcing practices in Mexico and the blanket prohibition on outsourcing enacted by the Mexican government in 2021. Section 3 describes the data sources for our empirical analysis. Section 6.3 documents the baseline correlation between markdowns and outsourcing, which motivated the reform. Section 4 outlines the differences-in-differences strategy used to measure the causal impacts of the outsourcing ban at the establishment level and reports its effects on employment, wages, the labor share, markdowns, factor substitution, investment, output, TFP, and market exit. Section 5 quantifies the causal impact of the reform on employment status and wages at the worker level using matched employer-employee data. Section 7 concludes.

2 Institutional Context

In this section, we provide contextual information on domestic outsourcing in Mexico and the blanket prohibition enacted 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' use of domestic outsourcing as a strategy to bypass employment regulations. Section 2.2 reports key empirical regularities pertaining to domestic outsourcing. Finally, Section 2.3 summarizes the legal provisions of the outsourcing ban.

[7] For more on the "parasite" view of informality, which posits that informality allows firms to earn higher profits from the cost advantages of not complying with taxes and regulations, see Levy (2008).

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 also used in many other countries.[8] In this system, a formal firm contractually hiring a worker registers the average daily wage of the worker 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 on a monthly basis. This contribution gives the worker access to public healthcare and childcare facilities. It also funds a bundle of 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 in many countries. For example, all countries in the Organisation for Economic Cooperation and Development (OECD) except the U.S. have similar provisions.[9] 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 a three-month severance payment and up to one year of wage payments. Again, these legal provisions 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 U.S.

As in many other countries, given the sizable labor-related costs imposed by legislation, domestic outsourcing was increasingly prevalent in Mexico prior to the ban.[10] Although commonplace, outsourcing is usually difficult to measure and therefore study directly. Given the

[8] Political scientists classify social protection systems according to the relation between contributions and benefits: Beveridgean systems are characterized by a flat-rate benefit rule, whereas Bismarckian systems follow an earnings-related rule (Cremer and Pestieau, 2003). The use of Bismarckian social insurance is not unique to Latin America; several advanced economies implement Bismarckian social insurance models, 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).

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

[10] Figure 1 in OECD (2021) shows that long-run outsourcing growth over the past 20+ years has been common across all OECD countries.

policy significance of outsourcing, however, Mexico collects detailed data according to well-established and accepted definitions.

To fix terms, 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 excludes workers employed on the establishment premises who do not carry out primary economic activities, as defined by the establishment's NAICS code, such as workers engaged in cleaning, catering, security, and gardening. Also for clarity, we refer to firms supplying the workers who conduct these noncore activities as specialized subcontractors, and we exclude them from our analyses.

While the theoretical literature in economics has highlighted efficiency gains as the primary motive for outsourcing (e.g., Bilal and Lhuillier, 2021; Felix and Wong, 2024), domestic outsourcing of *core* workers before the reform was mainly carried out through two schemes primarily associated with tax evasion and profit-sharing avoidance in the legal literature, known as insourcing and third-party outsourcing (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 to workers whereby a firm sets up a dual organizational structure, parking most of the profits generated by its productive establishments in a company[11] with no employees while hiring employees through a shell company that supplies personnel to the former and retains minimal profits.

Third-party outsourcing is a practice designed to lower a firm's payroll and value-added tax (VAT) 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 an average daily wage equivalent to the minimum wage. It then pays workers their remaining wages in the form of extraordinary labor income, such as bonus payments, grocery vouchers, and per diem travel allowances, all of which are not subject to social security contributions. This reduces the tax burden of direct hires, which is an increas-

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

ing 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 (e.g., retirement pensions) under third-party outsourcing. To evade the VAT, the shell staffing company fabricates fake invoices and claims tax deductions, then redistributing a fraction of the evaded liabilities to the employing firm through a cash kickback. While this type of domestic outsourcing fell squarely into the category of tax evasion prior to 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, also shifted the legal burden involved in battling unions and individual workers to the staffing shell 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 staffing shell company declared bankruptcy or insolvency.

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.[12] 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 manufacturing establishments of similar size in appendix Figure A.2. 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 cost of legal battles against terminated workers from employing firms.

In Table 3, we summarize the information presented in the appendix, comparing the mean labor payment shares of nonsalary payments for three establishment types: staffing establish-

[12] In the ideal case, we could use data on payments received by staffing establishments from each manufacturing establishment to link hiring and employing establishments. Unfortunately, these data are nonexistent; indeed, one of the key provisions of the reform, as described in subsequent sections, was the creation of a mandatory registry with contractual and employment information for all specialized contractors.

ments, manufacturing establishments that hire workers directly, and manufacturing establishments that rely on outsourced workers. Social security contributions, profits shared, and other benefits, expressed as a share of labor payments, are, respectively, 7, 3, and 2 percentage points lower on average in staffing establishments than in manufacturing establishments hiring workers directly, while they are zero in manufacturing establishments that rely on outsourced workers. In total, nonsalary payments are less than half, or 12 percentage points lower, in staffing establishments than in manufacturing establishments hiring workers directly.

What types of establishments outsource? We show in Appendix B that large firms, establishments that are large relative their labor markets in particular, foreign-owned establishments, and especially foreign-owned *maquiladoras* all utilize outsourcing disproportionately. The fact that large firms disproportionately use outsourced labor is consistent with the pattern reported by Goldschmidt and Schmieder (2017) for Germany and Bilal and Lhuillier (2021) for France. We also find that establishments hit with revenue shocks are more likely to outsource, consistent with 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, the patterns of outsourcing look quite similar to those in other countries.

2.3 Mexico's April 2021 Outsourcing Ban

While domestic outsourcing has grown in popularity since the 1980s in the U.S. (Davis-Blake and Broschak, 2009), its expansion in Mexico began only after the signing of NAFTA in 1994, when U.S. firms started subcontracting manufacturing processes to Mexico (Bergin, Feenstra and Hanson, 2009). After this point, Mexican legislators started passing regulatory changes to contain the growth of domestic outsourcing amid concerns of uncontrolled expansion, including reforms to federal laws in 2009, 2012, 2015, and 2017.[13]

These changes proved to be of no avail: domestic outsourcing grew uninterruptedly in absolute and relative terms from 1999 to 2019, as shown in Figure 1. The figure combines data from

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

two sources: Mexico's economic census and the National Institute of Statistics' survey panel of manufacturing, which track each other well in overlapping years. A leveling-off in outsourcing occurred with the election of the new government in 2018, and the observed collapse in 2021 corresponds to the government ban, which we now describe.

In 2018, a newly elected government adopted a hard-line stance against outsourcing. Aided by a qualified congressional majority, on April 23, 2021, the government passed and enacted a reform of the entire legislation that governs labor relationships in Mexico.[14] The reform comprised three main provisions. First, it prohibited outsourcing, substituting it with a new subcontracting scheme limited to the provision of specialized services, such as cleaning, catering, gardening, and security services, falling outside the core of the employing firm's 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 staffing shell companies *equally* liable for paying subcontracted workers' payroll taxes and social security contributions, it required firms to comply with inspection mechanisms while setting tougher financial sanctions for ordinance violations, and it strengthened enforcement efforts by aligning the provisions of several pieces of legislation and initiating agreements between government departments to prevent loophole exploitation by firms.

As a practical matter, the reform mandated the transfer of previously outsourced workers who performed the employing firm's core activities to its payroll, obliging the employing firms to directly hire them. 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,

[14]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*.

the reform did not ban temporary employment. Mexican legislation allows formal firms to hire workers temporarily without any additional tax burden. In fact, temporary employment played a quantitatively important role in direct hiring before the reform, with 14 percent of all directly hired workers in the manufacturing sector being temporary workers in March 2021. Second, the reform did not ban job search boards and recruitment agencies as long as their activities centered on search and recruitment, not staffing.

3 Data Sources

In this section, we describe our data sources and definitions. We focus overwhelmingly on manufacturing for two reasons: the data coverage is most complete and consistent for this sector since the criterion that the business has a fixed location is more consistently met 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.

To maximize completeness of coverage, length of the time series, data richness, and data frequency, we utilize confidential data from multiple sources: esatablishment-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 details on labor types make the Mexican data especially informative.

Economic Census. The data for this paper come from the 6 most recent waves of the Mexican economic census, which is conducted every five years. The census covers all establishments in the economy but excludes ambulant vendors operating in the streets without a fixed location. We analyze the period from 1994 to 2019 for the manufacturing sector. The selected sector comprised 21 percent of Mexico's GDP in the first quarter of 2023 (Instituto Nacional de Estadística y Geografía, 2023). We harmonize industry codes across census waves and assign each establishment a six-digit industry code based on the 1997 North American Industrial Classification System (NAICS) classification, to end up with 302 industries surveyed across the 6 census waves. For each establishment, the census reports total employment, annual payroll, total output, revenues, value added, intermediate input consumption, and productive capital.

There are two main employment categories: insourced employment and outsourced employment.[15] Insourced employment includes all nonremunerated and remunerated workers hired directly by the establishment to work on its premises. This type of worker may be formally or informally hired. For the insourced remunerated workers who are formally employed, the establishment pays wages and commissions, social security contributions, profit sharing, and other benefits, such as pension plans. Nonremunerated insourced workers include primarily owners and family members.

Outsourced workers are employed on the establishment's premises but are formally hired through a different company. This employment category excludes specialized subcontractors, whose services, such as cleaning and security, enter the census estimations as a separate category within intermediate consumption. The employing establishment of 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 all 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 output and value added for each establishment. The total output measure recorded in the census captures the total sales of goods and services, as well as all other sources of revenue for the establishment. We calculate value added by subtracting intermediate consumption (which includes the total cost of raw materials; energy provision, including electricity, gas and fuels; contracting expenses for services such as gardening and security; and repair and maintenance expenses) from

[15]A third category, contracted labor, consists of workers temporarily employed by the establishment for the provision of specific services, e.g., repair services, that are limited in time and scope. Such workers may be formally or informally employed and therefore may not enjoy access to social security or benefits. In practice, excessive use of contracting may be more harmful to workers, but it is legal, and so we do not focus on it. Unlike the use of contracted *services*, e.g., security, payments to contractor *workers* are included in labor payments, however, and we impute these ultimate payments to labor in an analogous manner to that described for outsourced labor below.

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, total output, revenues, and value added for each economic establishment in the country.

Finally, for a subset of establishments that do not keep labor, capital, raw materials, or energy expense accounts, the economic census reports their revenues, employment, and economic sector. For example, for labor, the census reports the employment level of these establishments but does not keep track of wages, social security payments, or any other labor expenses. Such establishments should not be confused with self-employment, as they do not necessarily employ a single 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 the computation of input revenue shares.

The census reports a unique firm and establishment identifier for the 2009, 2014, and 2019 census waves, and we utilize the concordance tables in Busso, Fentanes and Levy (2018) to identify establishments in the 1994, 1999, and 2004 census waves. Thus, we link establishments across the 6 census waves and conduct longitudinal data analysis.

Annual Manufacturing Survey. The data that we use to measure the causal impacts of the outsourcing reform on non-labor outcomes come from Mexico's annual manufacturing survey. We analyze the period from 2013 to 2022.[16] 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 total insourced and outsourced employment, annual payroll, total output, revenues, intermediate input consumption, and productive capital. We use these data to calculate labor, average wages, capital, raw materials, energy usage, and revenues for each establishment. There are two important caveats to this survey. First, it does not separately report salaries, social security payments, benefits, and profit sharing, rendering us reliant on a different data source for our measurement of impacts on labor outcomes. Second, the panel rotated at the end of 2021, dropping

[16] INEGI will publish the data from the 2023 wave of the survey on December 31, 2024.

5,583 establishments and adding 3,955 new establishments to the 2022 sample. In estimation, we present the results corresponding to the sub-sample of 4,864 establishments that did not attrit the panel because of its rotation in 2021, but we report the results for the panel of 10,447 establishments running up to 2021 as a robustness check in Appendix A.1.

Monthly Manufacturing Survey. To measure the causal impacts of the outsourcing reform on employment and labor payments by category, we complement the aforementioned annual survey data with data from Mexico's monthly manufacturing survey. This survey gathers information from a panel of 8,819 establishments overlapping with the annual survey and spanning the same 239 six-digit NAICS industry codes. As with the annual survey, we analyze the period from 2013 to 2024. The key difference between the two surveys is that the monthly survey gathers information only on insourced and outsourced employment, annual payroll, and total output and revenues. While limited in scope, the monthly survey is high frequency and has the advantage of offering information on salaries, social security payments, benefits, and profit sharing, which allows measurement of the timing of remuneration impacts 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, as well as on the registered mean wage of these workers before the social security authority, we use IMSS matched employer–employee data from 2021. These data report hiring company (not the employing company), economic sector, employment status (temporary or permanent), and the registered mean wage with a monthly frequency at the individual level for the universe of formal workers in Mexico.

4 The Causal Impacts of the Reform on Firms

To recover the causal impacts of the reform, we propose a differences-in-differences strategy that leverages two sources of variation: cross-sectional variation in exposure to the reform, as measured by the establishment's share of outsourced employees prior to 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).[17] We implement our strategy after clearing pre-reform time trends and pre-reform seasonal effects, which are present in raw outcome trends, such as those presented in Figure A.6 of Appendix A.1.

We explain the differences-in-differences strategy used to estimate the causal impacts of the reform in Section 4.1. We then report the effects of the reform on outsourcing prevalence and employment in Section 4.2, labor cost in Section 4.3, other input usage in Section 4.4, output and TFP in Section 4.5, market exit in Section 4.6, and the labor share and markdowns in Section 6.3.3. Finally, we present the reform's heterogeneous effects in Section 6.4 and the results from a battery of robustness checks in Section 4.7.

4.1 Empirical Strategy

To recover the causal impact of the outsourcing reform on the detrended outcome of interest $\tilde{Y}_{it}$ for establishment i after j periods, we estimate the parameter β_j in the following linear regression model via ordinary least squares (OLS):

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

where $\text{Outsourcing}_{i,t_0}$ is the outsourced employment share for establishment i at t_0, the period immediately prior to the reform; A is the first pre-shock period available in the data; B is the last post-hock period; γ is a group fixed effect, which absorbs all time-invariant variation in the outcome of interest for establishments with the same outsourced employment share in the period immediately prior the reform; δ_t is a time dummy, which absorbs all aggregate shocks that affect outcomes equally across all establishments; 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.

The identifying assumption that must hold for β_j to recover the causal impact of the reform

[17] We illustrate both sources of variation in more detail in Appendix A.1. Figure A.4 shows that, conditional on outsourcing, there is substantial variation in the share of outsourced workers at the establishment level at baseline. Figure A.5 shows evidence of a drop in the probability that an establishment outsources all or some of its workers after the reform and a commensurate rise in the probability that it directly hires all of its employees.

after j periods is that the establishments that did not rely on any outsourced workers prior to the reform would have experienced the same trend in the outcome of interest as outsourcing establishments in the absence of the reform—that is, the so-called parallel trends assumption. While the validity of this assumption is impossible to verify, an absence of differential pre-trends in the outcome of interest is generally interpreted as evidence that the parallel trends assumption holds in practice. Following standard practice in the literature, we exclude the interaction between our outsourcing indicator and the dummy for the period immediately prior to the enactment of the reform from the regression specification, allowing us to interpret coefficient estimates as deviations in the outcome of interest relative to the level observed by the group with zero exposure to outsourcing before the reform. Accordingly, we test the significance of the β_j parameters for $j < t_0$ to rule out differential trends in the outcome of interest.

Importantly, the regression outcome above, $\tilde{Y}_{it}$, is the detrended outcome of interest, Y_{it}. It is obtained from the following regression estimated using pre-intervention data:

$$Y_{it} = \alpha + \delta t + \mu_{\text{Month}(t)} + \text{Outsourcing}_{i,t_0} \times [\gamma + \beta t + \lambda_{\text{Month}(t)}] + \varepsilon_{it},$$

where δ and β are exposure-specific time trends, and $\mu_{\text{Month}(t)}$ and $\lambda_{\text{Month}(t)}$ are exposure-specific calendar month effects. We predict post-reform outcomes using the resulting coefficients from this regression and subtracting the prediction $\hat{Y}_{it}$ from Y_{it} to get $\tilde{Y}_{it}$. Thus, coefficients from the differences-in-differences model can be interpreted as deviations in excess of pre-existing differences between groups.

4.2 Outsourcing Prevalence and Employment

We begin our analysis by estimating the causal impacts of the reform on the prevalence of outsourcing as a hiring practice. We estimate impacts on total employment at the establishment level, which we define as the sum of the number of directly hired employees and the number of outsourced employees. As both employment types are directly observed in the data, we categorize establishments by their outsourcing prevalence. 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 first apply our differences-in-differences estimation strategy to the three indicators of outsourcing prevalence at the establishment level. Then, we estimate the impact of the reform on total employment and decompose this effect into effects by employment type. To facilitate the employment decomposition, we express all employment figures relative to the cross-sectional employment mean in March 2021. However, we report quantitatively similar impacts on the log of total employment at the establishment level in Panel A of Figure A.7 and Column (1) of Table A.3 in Appendix A.1.[18]

Figure 3 presents evidence in support of the parallel trends assumption necessary for the identification of causal impacts. Specifically, it plots the coefficient estimates for time dummies interacted with the outsourcing share of employment from our baseline specification for each outcome of interest, calculated using monthly manufacturing survey data.[19] Visual inspection of the coefficients demonstrates the absence of differential pre-trends for all of the outcome variables of interest.

Table 4 reports our estimates of the reform impacts after 3 years for a fully exposed establishment outsourcing all its workers prior to the reform. Panel A shows that the reform increased the probability of not outsourcing any worker by 91 percentage points (p=0.000). This increase corresponds to a reduction of 81 percentage points in the probability of outsourcing all the workers the establishment (p=0.000), and a smaller reduction of 9 percentage points in the probability of outsourcing some but not all workers (p=0.000). These findings indicate that the reformwas effective in reducing outsourcing.

Next, Panel B reports the impacts of the reform on employment. The reform increased the number of directly hired workers by 95 percent for fully exposed establishments, relative to the mean employment level across all establishments at baseline (p=0.000). This increase corresponds to a reduction of 87 percent in the average number of outsourced workers relative to the pre-reform mean employment across all establishments at baseline (p=0.000). The sum of these two effects gives rise to a point estimate impact of 9 percent in overall employment, but the estimate is only close to marginally significant (p=0.101).

[18]We also find quantitatively similar effects on log employment when using the annual survey, as reported in Panel A of Figure A.8 and Column (1) of Table A.4 in Appendix A.1.

[19]We report quantitatively similar effects corresponding to the annual manufacturing survey in Table A.5 and Figure A.9 of Appendix A.1.

4.3 Labor Cost

Next, we move on to estimate the effect of the reform on the total labor cost of the establishment and decompose it into effects on six mutually exclusive components: salaries, social security payments, profit sharing, other benefits, the management fee paid to the staffing firm for its services, and firing costs.[20] This decomposition allows us to determine if the reform results in higher labor costs, or if it solely leads to a reconfiguration of labor cost structure. Moreover, this decomposition enables us to elucidate the channels underlying impacts on the wage bill, defined as the sum of the first four components mentioned above. For instance, if a wage increase caused by the reform is solely explained by a hike in salaries, gains for workers are likely the result of an increase in take-home pay. In contrast, if the wage increase is also explained by an improvement in benefits, which typically include retirement pensions, or an increase in social security payments, gains for workers likely include an improvement in insurance values.[21] Moreover, if the wage increase is partly explained by profit sharing, the gains for workers include a higher option value of employment with the firm.

Figure 4 plots the coefficient estimates for time dummies interacted with the outsourcing share of employment from our baseline regression specification for total labor cost and all its components, calculated using monthly manufacturing survey data. As in the previous section, we express all figures relative to the cross-sectional mean of total labor cost in March 2021 to facilitate the decomposition of the total effect. However, we present evidence of quantitatively similar impacts on the log of total labor cost in Panel B of Figure A.7 and Column (2) of Table A.3 in Appendix A.1.[22]

We find no evidence of differential trends in any outcome prior to the reform, lending support to the parallel trends assumption necessary for the identification of causal impacts. Panel A displays biannual spikes in the impact of the reform on total labor cost. These biannual spikes occur on dates that correspond with the legally mandated dates for profit sharing (May payments) and thirteenth-month salary payments at Christmas (December payments), respec-

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

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

[22]Moreover, we report quantitatively similar effects corresponding to the annual manufacturing survey in Panel B of Figure A.8 and Column (2) of Table A.4 in Appendix A.1.

tively. Panel B indicate an increase in these periodic payments and constitute smoking-gun evidence that these series reflect the impact of the reform itself.

In Table 5, we present the impact of the reform on labor cost for fully exposed establishments (i.e., those outsourcing all of their employment one month prior to the reform, in March 2021) in 2023, two years after the reform.[23] The estimated impact of the reform is an increase of 21.1 percent in total labor cost ($p = 0.016$), as reported in Column (1). The decomposition of this effect into mutually exclusive components in Columns (2) through (7) reveals a rise in take-home pay, captured by an increase of 37.7 percentage points in salary payments ($p = 0.016$); a rise in the insurance and option values of employment, captured by significant increases in social security payments, profit sharing, and benefits of 17.6 ($p = 0.000$), 12.5 ($p = 0.000$), and 3.4 percentage points ($p = 0.000$), respectively; and a rise in management cost for the establishment, as captured by an increase of 2.2 percentage points in firing costs ($p = 0.000$). These increases were partially offset by a 52.3-percentage-point reduction in the fee paid to the staffing firm ($p = 0.000$), giving rise to our impact estimate for total labor costs.

Importantly, the added increases in the 4 components of the wage bill, coupled with the non-significant effect on total employment described in the previous section, led to an increase of 56 percent in the average wage of fully exposed establishments by 2023 ($p-$value=0.000), as shown in Panel C of Figure A.7 and Column (3) of Table A.3 in Appendix A.1.[24]

4.4 Other Input Utilization

Economic theory predicts that the outsourcing ban could trigger a substitution effect against labor and into other productive inputs, depending on the elasticity of substitution between labor and other inputs. 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 only reported annually. Figure 5 begins by showing evidence in support of the parallel trends assumption necessary for the identification of causal impacts for capital, energy, raw materials, and investment.

[23]This is achieved by replacing the year dummies for month dummies in the interacted terms of our baseline regression specification.

[24]We report quantitatively similar effects corresponding to the annual manufacturing survey in Panel C of Figure A.8 and Column (3) of Table A.4 in Appendix A.1.

Then, Table 6 reports the corresponding impact estimates for each of these variables two years after the reform.[25] As reported in Columns (1) through (3), we find no evidence of an effect on the establishment's usage of capital, raw materials, and energy consumption. However, in Column (4), we report a statistically significant reduction of 9 percent ($p = 0.05$) in investment.[26] Thus, there is no evidence of a substitution effect operating against labor, but rather a reduction in capital accumulation.

4.5 Output and Total Factor Productivity

Next, we investigate the effects of the reform on output and TFP. We would expect to see a negative effect on both variables if outsourcing entailed managerial efficiency gains in production. As described in Section 6.2, we estimate TFP under three alternative functional form assumptions for the production function: translog, translog with constant returns to scale, and Cobb-Douglas. Furthermore, we compute a fourth TFP measure which also relies on a Cobb-Douglas assumption but imputes the coefficients of the production function using the cross-sectional mean input shares for each four-digit industry. Since the estimation strategy for our first three productivity measures requires the lagged input variables to be used as instruments when estimating the production function, we can only use observations corresponding to establishments that did not attrit from the annual manufacturing panel after its rotation in 2021 to estimate impacts on productivity in 2022. While this issue limits our sample size, Panel A of Figure A.10 and Panel A of Table A.6 in Appendix A.1 provide reassuring evidence that impacts on productivity follow a similar direction, magnitude, and significance for the initial panel of establishments running up to 2021.

Figure 6 presents evidence in support of the parallel trends assumption necessary to interpret our differences-in-differences estimates on output and productivity as causal. We fail to find significant differential pre-trends between exposed and non-exposed establishments prior to the reform. Next, Table 7 reports our impact estimates for output and TFP two years after the

[25]As mentioned in Footnote 16, we cannot currently estimate impacts 3 years after the reform because INEGI will publish the 2023 wave of the annual manufacturing survey on December 31, 2024.

[26]In Appendix A.2, we complement the analysis of the investment impacts for existing establishments with an examination of the impacts on new investment perspectives using a survey about business perspectives administered monthly by the central bank to private sector analysts.

reform. We find no evidence of a significant impact on output or TFP under any of our four alternative assumptions about the functional form of the production function. Thus, outsourced workers moved into direct employment without a reduction in output, indicating no efficiency losses in production.

4.6 Market Exit

Since the reform led to losses in firm profitability through the increase in labor cost, we would expect to see impacts on market exit, particularly for marginal firms. To investigate impacts on market exit, we construct an indicator at the establishment level for market exit at time t. We apply our differences-in-differences strategy to this outcome using as estimation sample a balanced panel running from 2013 to 2022. This balanced panel comprises all manufacturing establishments that did not attrit from the annual manufacturing survey because of its rotation in 2021 and that had not exited the market before the enactment of the reform.

We present evidence for the parallel trends assumption in Figure 7 and report regression estimates for fully exposed firms in Table 8. Two years after its enactment, the reform increased the probability of exiting the market by 1.1 percentage points on average for establishments previously outsourcing all their employees ($p = 0.000$).

4.7 Robustness Checks

We conduct a battery of robustness checks to test the solidity of our findings. First, we address the possibility that our estimates may confound the effect of COVID-19 on firms and workers. To this end, we test for the differential effect of the reform on average wage and employment for the industries deemed essential by the government in the aftermath of the onset of the pandemic. Establishments in these industries were allowed to resume operations immediately after the most restrictive lockdown period, which lasted from April to June 2020. Figure D.5 in Appendix D.4 shows that, in essential industries, employment returned to normal three months after the strictest lockdown, while it took 12 months for employment to return to its pre-pandemic level in non-essential industries. If our empirical strategy indeed confounded the effect of COVID-19, we would expect to observe differential impacts on employment or wages by industry type. However, Figure D.6 shows that the effect of the reform on employment and wages in essential industries is statistically indistinguishable from its effect in non-essential industries.

Second, we test whether the reform reduced employment flexibility rather than employment levels. Novel empirical evidence for the U.S. is consistent with outsourcing providing flexibility for businesses (Atencio De Leon, Macaluso and Yeh, 2023). As mentioned above, this possibility is limited in our particular 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 the establishment level. Figure D.7 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*, not dropped, in the aftermath of the reform. This makes sense: transferring previously outsourced workers to the employing firm's payroll shifted employment counts from one employment type to another. When comparing the volatility of employment at establishments exposed to the reform against that of unexposed establishments in Figure D.8, we observe that the volatility of employment increased for the former group but not the latter. We quantify the size of this increase in Figure D.9 and Table D.4 and find that it amounted to a 1.1 percent hike (p=0.000) by the end of 2022. Finally, we test whether the reform had a differential effect on employment levels and wages for exposed establishments with high volatility at baseline in Figure D.10 and Table D.5. We find statistically significant evidence (p=0.027) that the reform increased the wages of establishments with low volatility at baseline disproportionately more than in high-volatility establishments. This also makes sense: turnover should theoretically limit the scope for modifying the wages of previously outsourced workers.

Third, we address the important concern that firms may have adjusted hiring practices in anticipation of the reform immediately after the election of the new government, correctly foreseeing that the reform would ensue. This would have led to an outsourcing drop long before its prohibition, in which case our empirical strategy would only retrieve a comparison of outcomes between establishments that anticipated the reform and establishments that failed to foresee it, not the true causal impacts of the reform. If this were the case, we would expect aggregate outsourcing prevalence to undergo a structural shift or a change in trend around the time of the election. This is not what we observe. Figure D.11 in Appendix D.6 shows that the cross-sectional mean share of outsourced workers at the establishment level does not jump or change slope around the time of the election. Furthermore, we would observe different estimates of the reform's impact on outsourcing prevalence and employment if we used only the establishments that hired directly all their workers before the election as a control group (i.e., excluding from the control group the establishments that had ever outsourced their employees). Figure D.12 and Table D.6 show that the impact estimates resulting from restricting the control group are quantitatively similar to our baseline estimates.

Finally, recent methodological advancements in the differences-in-differences literature (for an excellent review, see Roth et al., 2023) show that the parameters associated with linear two-way fixed-effect (TWFE) specifications can be hard to interpret, particularly when treatment is absolutely continuous, as treatment effects may vary with treatment dose. Callaway, Goodman-Bacon and Sant'Anna (2021) introduce alternative estimation procedures that do not suffer from the same drawbacks as TWFE specifications in continuous differences-in-differences settings. In Section D.7, we use their proposed procedures to estimate dose-specific treatment effects and assess the plausibility of the parallel trends assumption for employment and wages.

5 The Causal Impacts of the Reform 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, previously outsourced workers were *less* likely to remain formally employed and, in particular, employed in manufacturing. While we have shown that the reform had null impacts on employment at the establishment level in our main empirical analysis, testing this possibility is crucial because establishment-level data do not allow us to reject the possibility that outsourced workers were terminated and replaced with other workers after the reform. If this proved to be the case, we would expect to see a widening gap in the likelihood of being formally employed between the previously outsourced and other workers. Evidence to the contrary would constitute the smoking gun that the reform caused previously outsourced workers to gain insider status within their employing firms.

We also take particular interest in the effect of the reform on worker wages registered before the social security authority. If firms responded to the reform by passing the cost of the increase in social security contributions, profit sharing, and other benefits through to workers, we would expect to see a reduction in registered wages. Evidence to the contrary would confirm our previous 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 before the social security authority. Moreover, if wage increases concentrated at the bottom of the wage distribution, the reform would have had the effect of reducing inequality across workers.

Our analysis relies on matched employer-employee data for the universe of formal firms and workers in Mexico from the social security authority (the IMSS). While this data is comprehensive, the social security authority had no reliable registry of staffing companies prior to the reform, making it impossible to accurately back out the identity of the employees who were hired through staffing but were employed within the premises of a different firm. To surmount this challenge, we devise two alternative strategies to identify staffed workers in the IMSS data and estimate the reform's effect on their employment status and wages. Both strategies use canonical differences-in-differences designs to retrieve causal impacts, but each of them uses a different comparison group for treated workers.

5.1 Firm Exit from the Professional Services Sector

The first strategy identifies staffing companies as those fulfilling two conditions: (1) being registered in the "provision of professional services to other firms" sector within the IMSS economic sector classifier and (2) permanently exiting the market in July 2021. The first condition captures the fact that IMSS officials tended to register staffing companies in the specified sector, along with firms providing other types of professional services, such as accounting, consulting, and law. The second condition captures the last month of the reform's grace period, after which staffing firms were required to exit the market. Consistent with the reform's provision, Figure 11 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 differences-in-differences design.

Our design compares the outcomes of workers who were employed in March 2021 by any of the firms identified by applying the two criteria described above against those of workers who were employed in the manufacturing sector in the same month, 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:

$$Y_{it} = \sum_{j=\text{January 2021}}^{\text{December 2021}} [\mathbb{1}_{t=t_0+j} \times \widehat{\text{Staffing}}_{i,t_0}]\beta_j + \widehat{\text{Staffing}}_{i,t_0}\gamma + \delta_t + \varepsilon_{it}, \tag{2}$$

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.

5.2 Universal Registry of Specialized Service Providers

The 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 that concluded in July 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 by November 2021. The main statutory reason for rejection was evidence that the applicant offered staffing services rather than specialized services.[27]

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 include only workers who remained continuously employed in the services sector until November 2021, as the social security authority oversaw the transfer of workers from rejected applicants to their employing firms without going through an unemployment spell. We estimate via OLS the following model on the sample comprised of both groups of workers:

$$Y_{it} = \sum_{j=\text{January 2021}}^{\text{November 2021}} [\mathbb{1}_{t=t_0+j} \times \text{Unsuccessful}_{i,t_0}]\beta_j + \text{Unsuccessful}_{i,t_0}\gamma + \delta_t + \varepsilon_{it}, \quad (3)$$

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.

5.3 Results

The parallel trends assumption required to lend a causal interpretation to the OLS estimates of the β_j parameters of Equations (2) and (3) is that the outcomes of interest would have followed the same trend for treated and comparison workers in the absence of the reform. Figure 12 provides evidence supporting the validity of this assumption.

Table 11 reports the OLS impact estimates 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. As mentioned above, Mexico's manufacturing sector comprises only 21 percent of GDP (Instituto Nacional de

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

Estadística y Geografía, 2023). 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 consistent with the participation of manufacturing in GDP.

Our estimates from the first identification strategy indicate the reform increased the probability of being directly hired by a manufacturing firm by 22 percentage points (p=0.000) for employees of the firms in the "provision of professional services to other firms" sector that exited the market in July 2021, which is consistent with the participation of the manufacturing sector in GDP, and that their registered monthly wage increased by 22 percent (p=0.000), consistent with the one-year impact estimate for average wage obtained using establishment-level data. Similarly, estimates from our second strategy indicate that the reform increased the probability of being directly hired in the manufacturing sector by 28 percentage points (p=0.000) and registered wage by 17 percent (p=0.000) for the employees of unsuccessful applicants who were transferred out of staffing.

Finally, we present the estimates of the reform's impact on wage inequality from both 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 both differences-in-differences specifications with categorical quartile dummies. If the reform reduced inequality, we would expect the positive wage impacts to concentrate among low earners. Indeed, in Figure 13, we show that the wage gains from the reform concentrated at the bottom of the wage distribution.

6 Outsourcing and Monopsony Power

In this section we delve into further depth as to the role of monopsony power in the Mexican outsourcing industry. We discuss in turn theory, measurement, baseline evidence, and policy experiment evidence.

6.1 Theory

The absence of a detectable drop in employment or output despite a substantial increase in labor cost is broadly incompatible with competitive price theory, as an exogenous increase in labor cost would predict a drop in the quantity of labor demanded by the firm, coupled with factor substitution toward other productive inputs, ultimately leading to a decrease in output and a price increase. Hence, to explain our empirical findings, we move beyond competitive price theory toward a conceptual framework allowing some degree of labor market power. Following Van Reenen (2024), we discuss the impacts of the ban through the lens of two classes of models where labor market power plays a paramount role: the classical monopsony model and the rent-sharing model, both of which predict a wage increase but with starkly different implications for other outcomes.

In the classical monopsony model, the predicted effect of the ban is an increase in employment and output whenever the reduction in monopsony power resulting from dismantling staffing companies, which are typically larger than producing firms, outweighs the efficiency gains and cost savings achieved through outsourcing. In Appendix E.1.2, we develop a classical monopsony model, inspired by Berger, Herkenhoff and Mongey (2022*a*), with a staffing sector to formalize these statements. The predictions from this model are compatible with our empirical findings, except that the increases in employment and output we observe are not statistically significant.

In the rent-sharing model, outsourcing reduces workers' bargaining weight if staffing firms make workers outsiders to the producing firm, as in Lindbeck and Snower (1988), or if outsourcing limits workers' outside option by virtue of being larger than producing firms.[28] In Appendix E.1.3, we develop a rent-sharing model (see Card, Devicienti and Maida, 2014; Card et al., 2018), where workers can be hired directly or through outsourcing, to illustrate the impacts of the outsourcing ban under complete and incomplete contracting, assuming that outsourcing involves a reduction in bargaining weight. When firms and workers specify complete contracts, whereby they first make surplus-maximizing input choices and then bargain over the split of the surplus rather than merely bargaining over wages in a way that distorts other margins, the ban leaves output, employment, and other inputs unchanged, only leading to a shift in the surplus paid to workers. In contrast, when bargaining occurs before deducting the cost of capital, the firm is not the full residual claimant of the additional returns it generates through capital investment,

[28] Outside options can impact bargaining weights in sequential bargaining setups that yield Nash bargaining results when payoffs are concave (for a proof, see Binmore, Rubinstein and Wolinsky, 1986).

giving rise to the holdup problem and underinvestment, as in Grout (1984). The predictions from this model are particularly helpful in explaining the observed drop in capital investment.

6.2 Measuring Monopsony

Armed with theory, we seek to evaluate the role of monopsony power empirically. The above-referenced ratio of the value of the marginal product of labor to the labor payment is referred to as the markdown and is the standard indicator of monopsony power. Since the value of the marginal product of labor is not measured explicitly, standard methods for obtaining markdown estimates are indirect. These estimators are not our innovation, however, so we simply summarize them here and provide details in Appendix C. Specifically, Brooks et al. (2021*a*,*b*) and Yeh, Macaluso and Hershbein (2022) apply cost minimization to derive the wage markdown of establishment i at time t, ν_{it}, as the ratio of the output elasticity of labor, θ^L_{it}, to its cost share, α^L_{it}, divided by the establishment's markup, μ_{it}, which can itself be calculated using de Loecker and Warzynski's (2012) analogous ratio estimator: the ratio of the output elasticity, θ^M_{it}, to the cost share, α^M_{it}, *of any price-taking, flexibly chosen input, M*[29]:

$$\nu_{it} = \frac{\frac{\theta^L_{it}}{\alpha^L_{it}}}{\mu_{it}} = \frac{\frac{\theta^L_{it}}{\alpha^L_{it}}}{\frac{\theta^M_{it}}{\alpha^M_{it}}}$$

Following the literature, we use the raw materials (M) as that flexibly chosen, price-taking input. We do not use energy, E, since 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.

Calculating the output elasticity of labor and the establishment's markup requires knowl-

[29]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.

edge of its production function, which can be obtained in various ways, depending on the assumptions on the production 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 production function amounts to assuming that output elasticities do not vary across establishments within the same industry, thereby implying that 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 re-estimate 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 utilized by Brooks et al. (2021*a*). Fortunately, similarly to Brooks et al. (2021*a*), who utilize a slightly different variant of markdown approaches, we find that although the different approaches yield results that that differ somewhat quantitatively, they are comparable both qualitatively and in their orders of magnitude.

6.3 Pervasive Baseline Markdowns

The statement of purpose of the outsourcing reform bill, submitted to the federal legislature in November 2020, motivated the ban by arguing that outsourcing core workers enabled worker exploitation through the simulation of employment relationships and tax evasion (Gaceta Parlamentaria, 2020). This section presents evidence that worker exploitation, as measured by markdowns, was indeed high and pervasive before the reform—especially among large firms and firms that outsourced, consistent with the presence of monopsony power. The analysis focuses on the economic census data.

Table 1 presents summary statistics over 20 years on the presence of markdowns, which are high and variable across firms. Here, we present the translog markdowns. The mean markdown across all years of 1.49 is sizable and comparable to what Yeh, Macaluso and Hershbein (2022) report for the United States (1.52), but the variation in markups is larger in Mexico, with a standard deviation of 1.03 (relative to 0.62 in the U.S.). The median of 1.2 implies that labor earns only 80 percent of its marginal revenue product in the median firm, as the markdown is the reciprocal of the ratio of the wage to the marginal revenue product of labor.

Markdowns vary widely but are pervasive in all regions of Mexico and across most manufacturing industries, but they are nonetheless larger in some regions and industries. In Appendix A.1, Table A.1 examines average labor markdowns by census wave and broad industry group, defined with 3-digit NAICS codes. The industries with the highest labor markdowns are industries with large-scale establishments such as transport equipment and machinery, with no substantial change in the industry ranking from 1999 to 2019. Table A.2 presents average markdowns by census wave and country region. Markups are sizable in all regions, but establishments in the central and southern regions of the country have the highest labor markdowns. Additionally, Figure A.3 presents heat maps of markdowns by state, demonstrating these stronger regional markdowns at a disaggregated geographical level.

6.3.1 Markdowns and Establishment Size

If labor markdowns represent monopsony, they should be higher among firms with market power in the labor market. We indeed find that markdowns are increasing in establishment size, measured as the total revenue share of the establishment in its local labor market.[30] We define a local labor market as an industry–geographic area combination. Industries are defined with 3-digit NAICS codes. Geographical demarcations are 2,040 rural municipalities and 74 metropolitan areas. The metropolitan areas comprise 417 urban municipalities.[31]

[30]An alternative measure of establishment size is the total labor share of the establishment in its local labor market; however, with this measure, wage payments would show up on both sides of the regression. Since the correlation between both establishment size measures in the economic census data is 0.97, for parsimony, we present results only for our preferred size measure.

[31]The national population authority of Mexico, called the *Consejo Nacional de Población* (CONAPO), defines a metropolitan area as an urban area spanning 2 or more municipalities with 100,000 inhabitants or more, an urban area spanning a single municipality with 500,000 inhabitants or more, an urban area in border or coastal municipalities with 200,000 inhabitants or more, and the 32 state capitals, regardless of their population. For

To investigate the relationship of interest, we regress the wage markdowns on categorical dummies for establishment deciles of the establishment share of total revenue at the market level via OLS, including market fixed effects and year controls, with heteroskedasticity-robust standard errors clustered at the market level. Figure 2 reports the coefficient estimates and 95 percent confidence intervals resulting from this regression. We observe a positive gradient in markdowns with establishment size, consistent with larger establishments exerting more monopsony power in local labor markets than smaller establishments.[32]

6.3.2 Markdowns and Outsourcing

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

First, to estimate the correlation between markdowns and the use of outsourcing, we regress the markdown on the share of outsourced employees at the establishment level via OLS with establishment fixed effects, year dummies, and a control for (log) number of workers. Table 2 reports the results from this regression, with each column in the table reporting results for a different markdown measure. Column (1) shows that a one-percentage-point increase in the share of outsourced employees raises the markdown of the establishment, 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.[33] Columns (2) through (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

robustness, in Appendix D.1, we present similar findings resulting from our using the definition of commuting zones from Blyde, Busso and Romero-Fonseca (2020) to define local labor markets.

[32]An important concern is that labor and outsourcing decisions could be made at the firm rather than the establishment level. As a robustness check, in Appendix D.2, we report the establishment markdown gradient with firm size and show the correlation between outsourcing prevalence and size at the firm level. As another robustness check, Appendix D.3 presents a graph similar to Figure 2 but partitioning the range of establishment shares of total revenue in the local labor market into equally sized intervals in the $(0, 1)$ range.

[33]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 1.

materials to the revenue share of labor, as suggested in Bond et al. (2021).

6.3.3 Impact of Ban on Labor's Share and Markdowns

We then examine if the reform successfully achieved its primary policy goal: to reduce labor exploitation and steer economic rents toward workers. We estimate the reform's causal impacts on two key variables that capture the intended outcome: the labor share and markdowns.

As we do for TPF impact estimation, we compute markdowns under alternative functional form assumptions, with the sample used for estimation including only establishments that did not attrit from the annual manufacturing survey because of its rotation in 2021. In addition, our sample includes only observations for which a data lag is available, which is necessary for markdown estimation.[34] Visual inspection of the coefficient estimates in Figure 7 supports parallel trends assumption.

Table 9 reports our impact estimates. We find a strongly significant increase in the labor share amounting to 6 percentage points ($p = 0.000$) for establishments that outsourced all their workers prior to the reform. Consistent with this finding, we report a strongly significant reduction of 23 percent ($p = 0.000$) in wage markdowns estimated using a translog assumption and a reduction of similar magnitude and statistical significance for markdowns estimated under the added assumption that the production function displays constant returns to scale. We report a slightly larger reduction of 32 percent ($p = 0.000$) when markdowns are estimated under a Cobb-Douglas assumption and a similar reduction when using the log of the ratio of the 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.

6.4 Heterogeneity

Our findings from previous sections show that the reform increased the labor share and successfully reduced markdowns, while having no statistically distinguishable impact on employment, other input usage, or output, despite increasing labor cost, while reducing capital in-

[34] Panel B of Figure A.10 and Panel B of Table A.6 in Appendix A.1 show similar impacts on markdowns to those discussed in the main body of the paper, but for the initial panel of establishments running up to 2021.

vestment and increasing the probability of market exit. We now examine the heterogeneous impacts of the reform on markdowns, as well as on investment and market exit, both of which convey crucial information about the welfare effects of the reform.

First, we present impacts on markdowns by markdown severity at baseline. To this end, we divide our estimating sample into bins according to the percentile rank of establishments in the baseline markdown distribution, and we estimate a fully saturated version our differences-in-differences specification with categorical quartile dummies. If impacts occurred at the bottom of the baseline 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 impacts occurred at the top of the baseline markdown distribution, our findings would be consistent with the reform successfully reducing labor exploitation.

Figure 9 reports the impact of the reform on all our markdown measures by baseline markdown percentile. We find evidence that the effect of the reform concentrated on the establishments with baseline markdowns above the cross-sectional 75th percentile, indicating that the reform did have the intended effects instead of having perverse effects.

Next, Table 10 examines heterogenous impacts across five dichotomous establishment-level characteristics at baseline: whether the establishment markdown was above the 75th percentile in 2020, whether the establishment industry's average markdown was above the 75th percentile in 2019, whether the establishment's operations are based in the central or south regions of the country, whether the establishment has foreign ownership, and whether the establishment's operations are based in a metropolitan area. The first column indicates that the impact of markdowns is concentrated among the top-markdown quartile of firms at baseline. The second column also reports strongly significant impacts among establishments operating in high-markdown industries. In contrast, we find no statistically significant effects operating adversely against establishments in the central and south regions, foreign-owned establishments, or those in urban locations.

Second, we consider heterogeneity in the unintended consequences of the reform along the dimension of establishment size in Figure 10. We divide our estimation sample into four bins according to the percentile rank of establishments in the revenue distribution at baseline and estimate a fully saturated version of our differences-in-differences specification with categorical quartile dummies. If the reform reduced misallocation by pushing out of the market the group of firms that remained in operation solely because of the cost advantage associated with worker exploitation, we would expect the negative impacts of the reform to concentrate at the bottom of the distribution. Conversely, if the reform increased misallocation by distorting the production decisions of profitable firms, we would expect negative impacts to concentrate at the top of the distribution. We find that the effects of the reform on capital investment and market exit concentrate in establishments with baseline revenues below the cross-sectional 25th percentile, indicating that the reform negatively impacted non-profitable establishments, reducing misallocation.

7 Conclusion

This paper examined the causal impact of a reform prohibiting domestic outsourcing on employment, labor cost, wages, markdowns, input substitution, capital investment, and market exit decisions. Using a differences-in-differences strategy that combines cross-sectional variation across establishments in exposure to the reform at baseline, as measured by the share of outsourced workers, and time variation leveraging a before–after reform comparison, we find that the legislative change to labor regulation did not significantly impact employment, usage of other productive inputs, or output but increased wages, particularly among low earners, ultimately lowering markdowns. While the reform was successful in reducing labor exploitation, it led to a comparatively mild reduction in investment and a similarly mild increase in the market exit rates of marginal establishments.

The key policy implication of our finding is that labor legislation, particularly domestic outsourcing regulations, can protect workers from exploitation but may do so at the expense of investment and the firm's ability to remain in operation. This is a highly relevant finding given the fast growth rate of temporary employment schemes worldwide after the 1980s and hints at the answer to the question of why governments in developing countries have largely failed to promote regulations protecting workers from changes in the organization of production after liberalizing trade. Our findings may also be relevant to hiring practices in the gig economy (e.g., Uber) or contexts where workers are hired as independent contractors. One caveat is that our study focused exclusively on the short- and medium-term impacts of the reform that we study. Future research should examine the long-term effects of labor policy.

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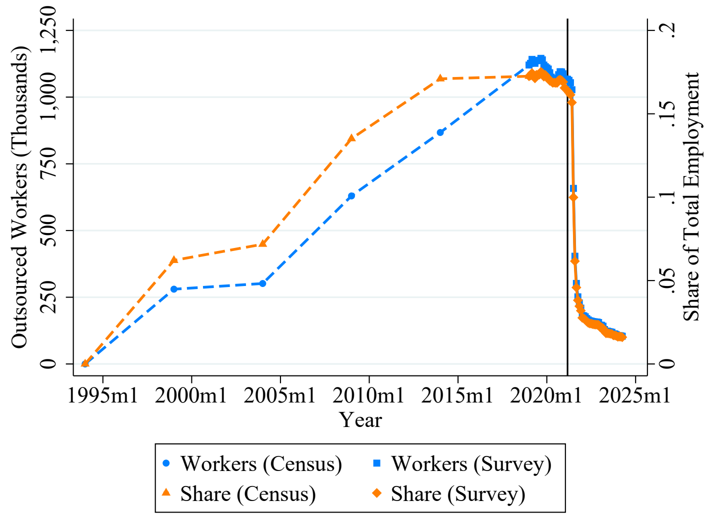
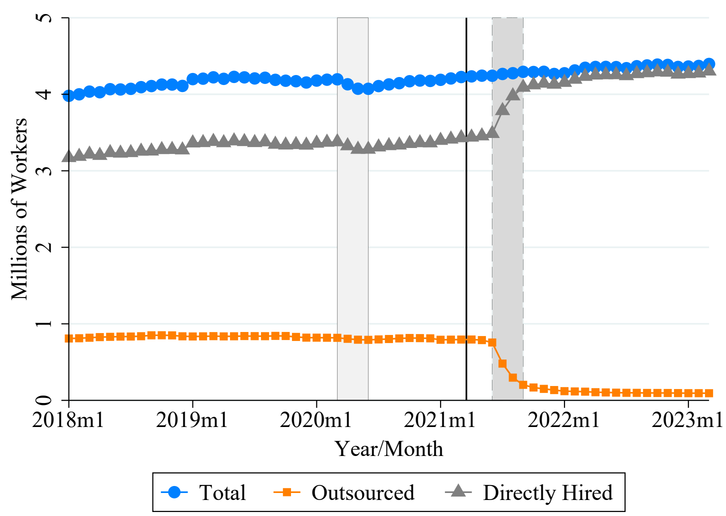
Figures

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

Panel A. Long-Run Trends for the Universe of Manufacturing Establishments

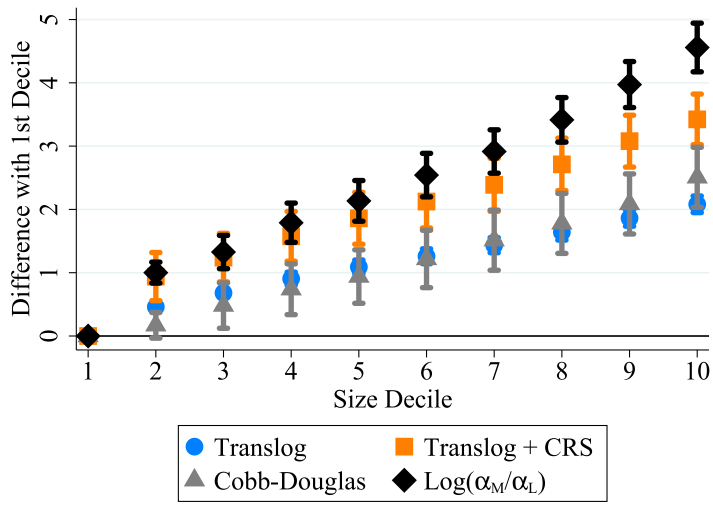


Panel B. Zoom-In Around the Reform for the Establishments in the Monthly Survey



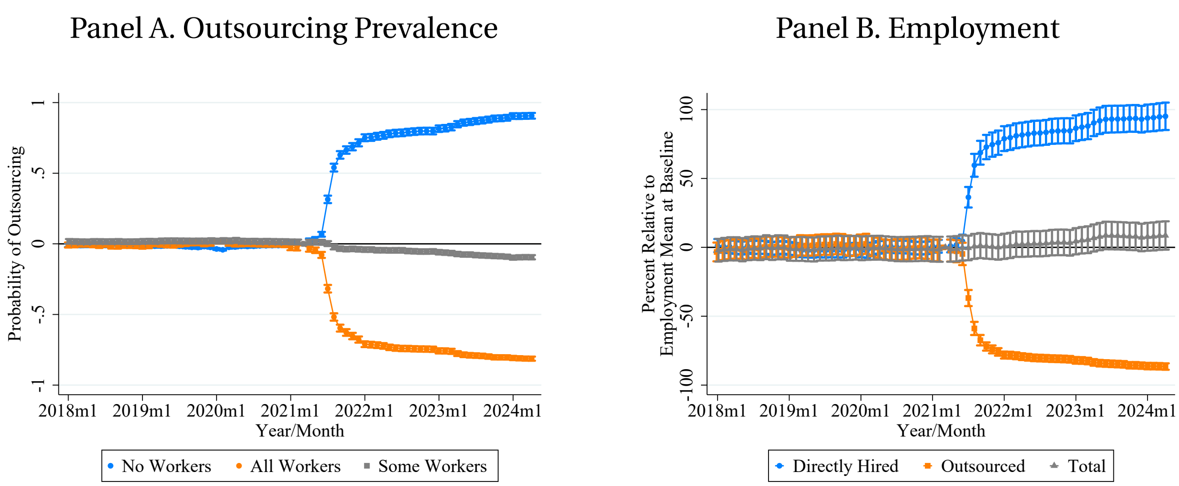
Notes: This figure presents trends in outsourcing in the manufacturing sector of Mexico from 1999 to 2023. Panel A uses data for the universe of manufacturing establishments, covering six waves of the economic census from 1994 to 2019, coupled with post-2019 data from the monthly manufacturing survey, 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 most restrictive COVID-19 lockdown in Mexico, which lasted from April to June 2020. The dark gray area outlined with a dashed black line represents the grace period provisioned 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 the monthly manufacturing survey of the National Institute of Statistics.

Figure 2: Markdown Gradient with Establishment Size



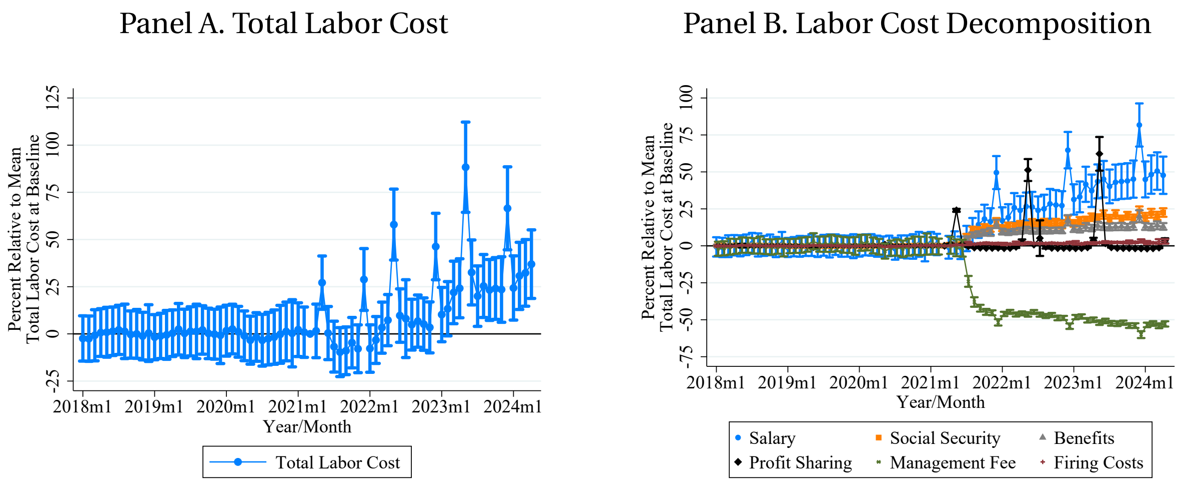
Notes: This figure reports the coefficients and 95 percent confidence intervals of establishment size decile dummies, where the deciles are taken with respect to the national distribution of the establishments' shares of total revenue in their respective local labor markets, in a regression of wage markdowns 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.

Figure 3: Tests for Pre-Trends in Establishment-Level Outsourcing Prevalence and Employment



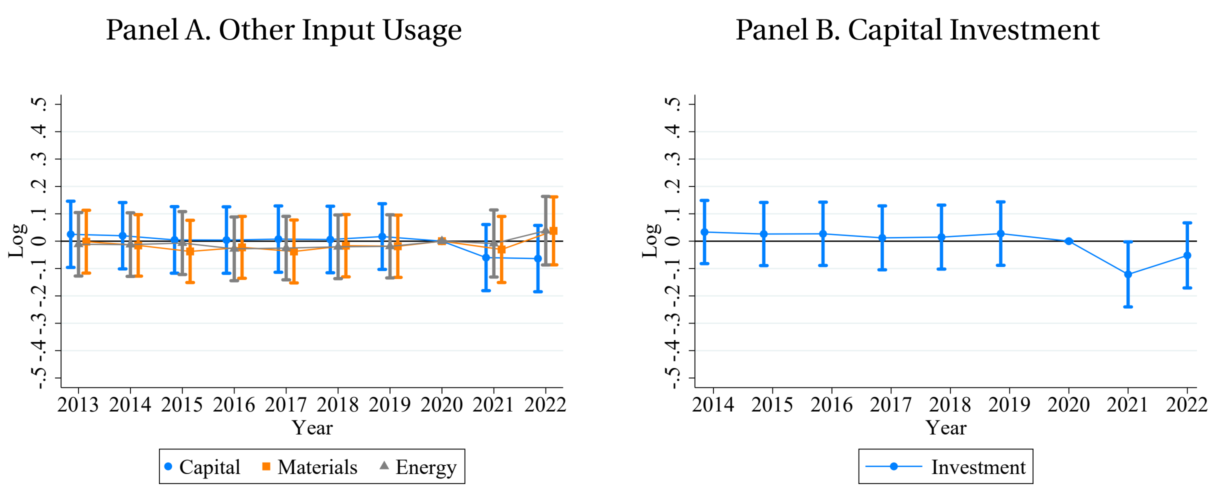
Notes: Each panel in this figure presents the regression coefficients and 95% confidence intervals of month dummies interacted with the establishment's pre-COVID (February 2020) share of outsourced workers, controlling for date and calendar month dummies and the pre-COVID share of outsourced workers. Outcomes are detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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 effects can be interpreted as deviations away from the outcome mean of the comparison group the month prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2024.

Figure 4: Tests for Pre-Trends in Establishment-Level Labor Cost



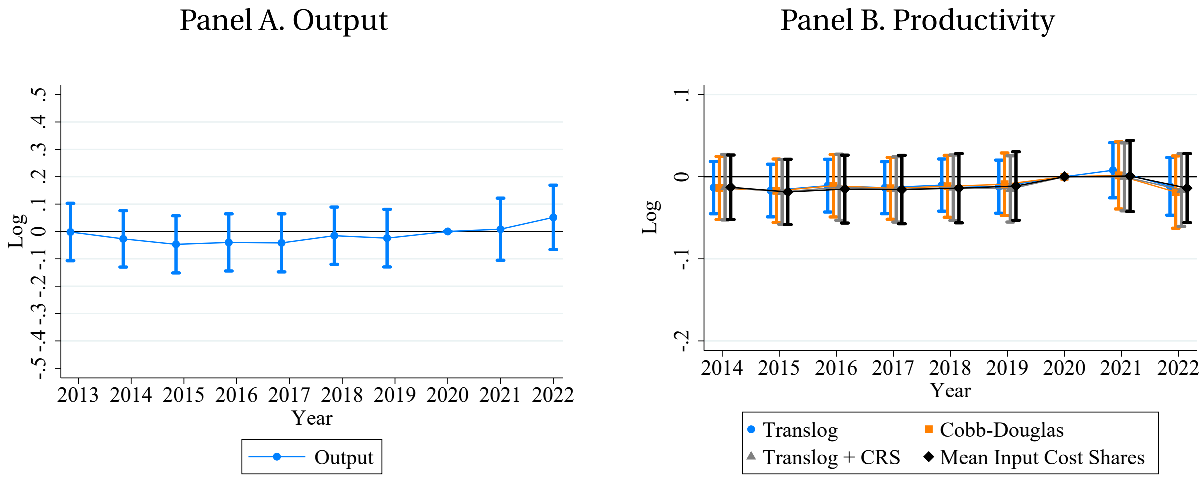
Notes: Each panel in this figure presents the regression coefficients and 95% confidence intervals of month dummies interacted with the pre-COVID (February 2020) share of workers outsourced by the establishment, controlling for date and calendar month dummies and the pre-COVID share of outsourced workers. Outcomes are detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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 effects can be interpreted as deviations away from the outcome mean of the comparison group the month prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Figure 5: Tests for Pre-Trends in Establishment-Level Usage of Other Inputs



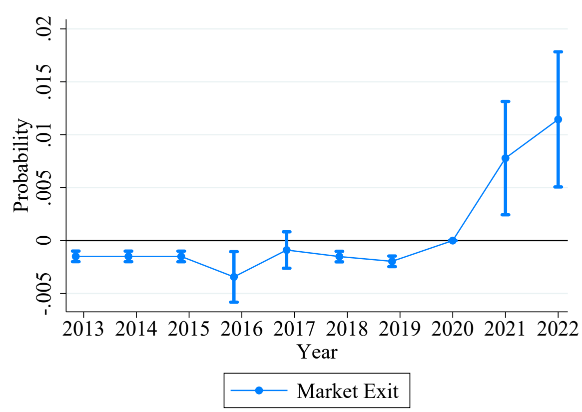
Notes: Each panel in this figure presents the regression coefficients and 95% confidence intervals of year dummies interacted with the employment share of outsourcing of the establishment in 2020, controlling for year and the employment share of outsourcing in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for 2020 is excluded from each regression, so effects can be interpreted as deviations away from the outcome mean of the group with zero exposure the year prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2022. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Figure 6: Tests for Pre-Trends in Establishment-Level Output and Productivity



Notes: Each panel in this figure presents the regression coefficients and 95% confidence intervals of year dummies interacted with the employment share of outsourcing of the establishment in 2020, controlling for year and the employment share of outsourcing in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for 2020 is excluded from each regression, so effects can be interpreted as deviations away from the outcome mean of the group with zero exposure the year prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2022. 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).

Figure 7: Test for Pre-Trends in Establishment-Level Market Exit

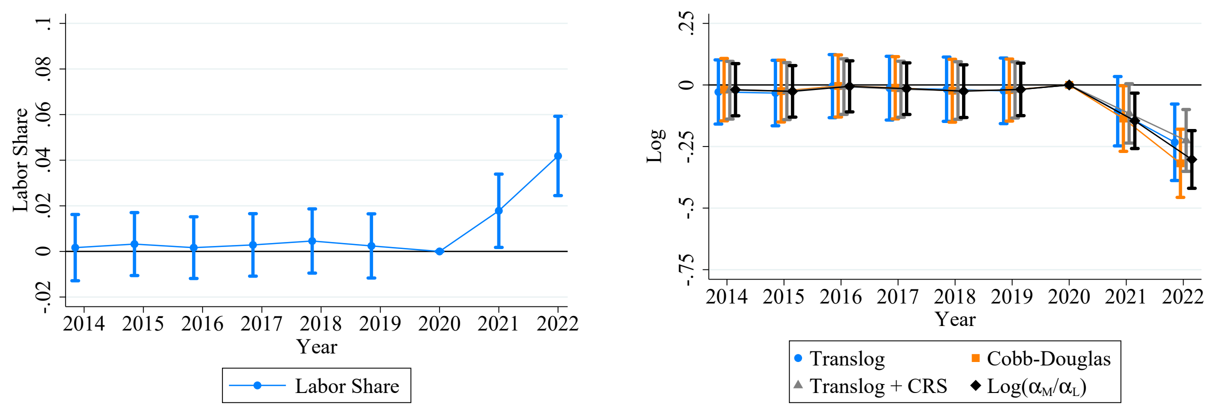


Notes: This figure presents the regression coefficients and 95% confidence intervals of year dummies interacted with the employment share of outsourcing of the establishment in 2020, controlling for year and the employment share of outsourcing in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for 2020 is excluded from each regression, so effects can be interpreted as deviations away from the outcome mean of the group with zero exposure the year prior to the 2021 reform. Standard errors for 2013, 2014, 2015, 2018, and 2019 are estimated precisely at zero because no manufacturing establishment that outsourced workers exited the panel in those years.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2022.

Figure 8: Tests for Pre-Trends in Establishment-Level Labor Share and Markdowns

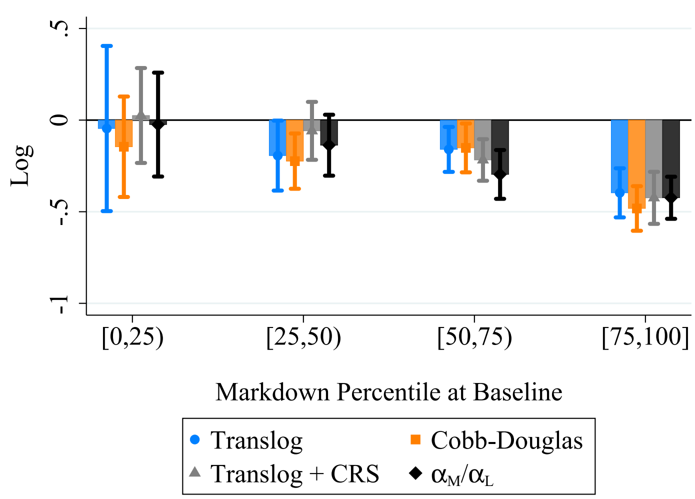
Panel A. Labor Share

Panel B. Markdowns



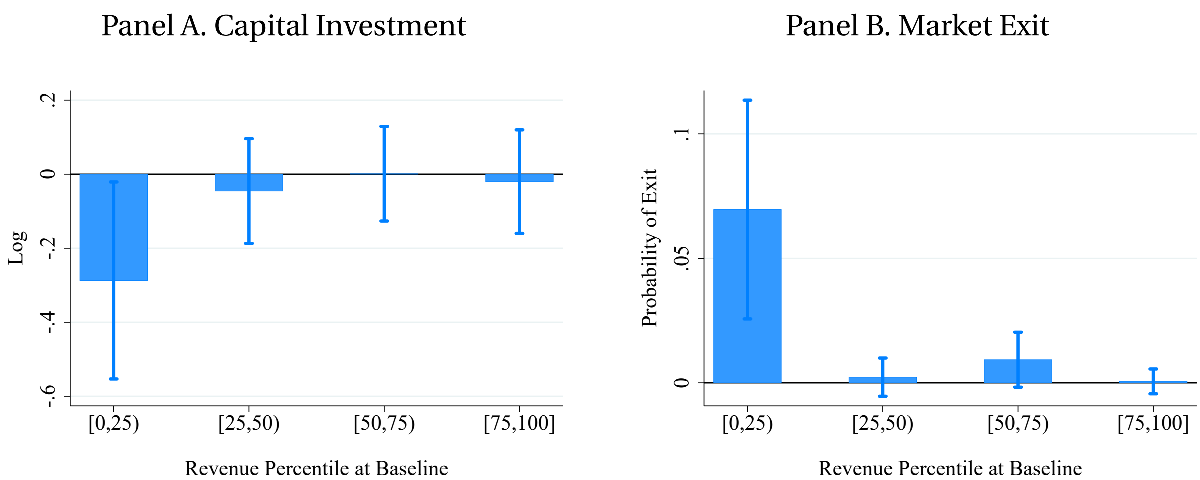
Notes: Each panel in this figure presents the regression coefficients and 95% confidence intervals of year dummies interacted with the employment share of outsourcing of the establishment in 2020, controlling for year and the employment share of outsourcing in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. We exclude all expenses other than capital, raw materials, energy, and labor in the calculation of the labor share. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for 2020 is excluded from each regression, so effects can be interpreted as deviations away from the outcome mean of the group with zero exposure the year prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2022. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Figure 9: Impact Heterogeneity in Markdowns by Markdown Percentile at Baseline



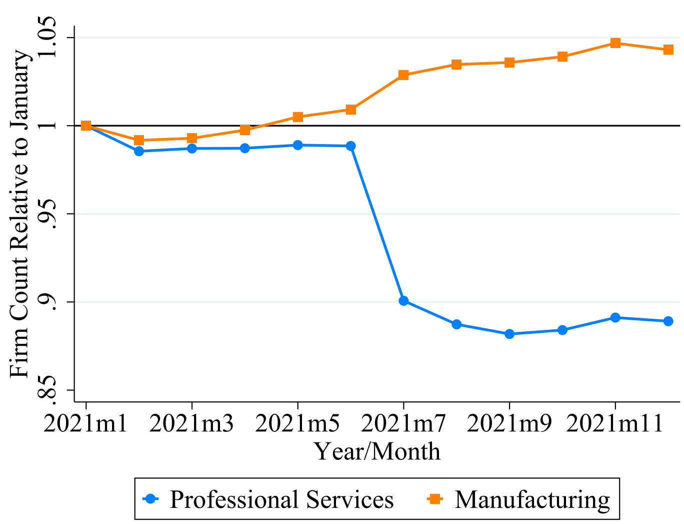
Notes: This figure reports the regression coefficients and 95 percent confidence intervals for the reform's impact on markdowns in 2022 by establishment markdown quartile in 2020. We obtain the impact estimates by fully interacting our differences-in-differences specification with categorical dummies for establishment markdown quartiles for 2020. Standard errors are robust to heteroskedasticity and are clustered at the establishment level.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2022.

Figure 10: Impact Heterogeneity in Investment and Market Exit by Revenue Percentile at Baseline



Notes: This figure reports the regression coefficients and 95% confidence of the impact of the 2021 reform on capital investment and the probability of market exit one year after its enactment by establishment revenue percentile rank in 2020, the year prior to the reform. Impact estimates are obtained by fully interacting our differences-in-differences specification with categorical dummies for each subsample of establishments. Standard errors are robust to heteroskedasticity and are clustered at the establishment level. The estimation sample in Panel A includes all observations in the annual manufacturing survey for which a lag of capital is available, whereas the estimation sample in Panel B is a balanced panel including observations of establishments that did not attrit the annual manufacturing panel because of its rotation in 2021 and that had not exited the market prior to the reform's enactment. In both cases, the estimation sample includes only establishments that report generating positive revenues.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey.

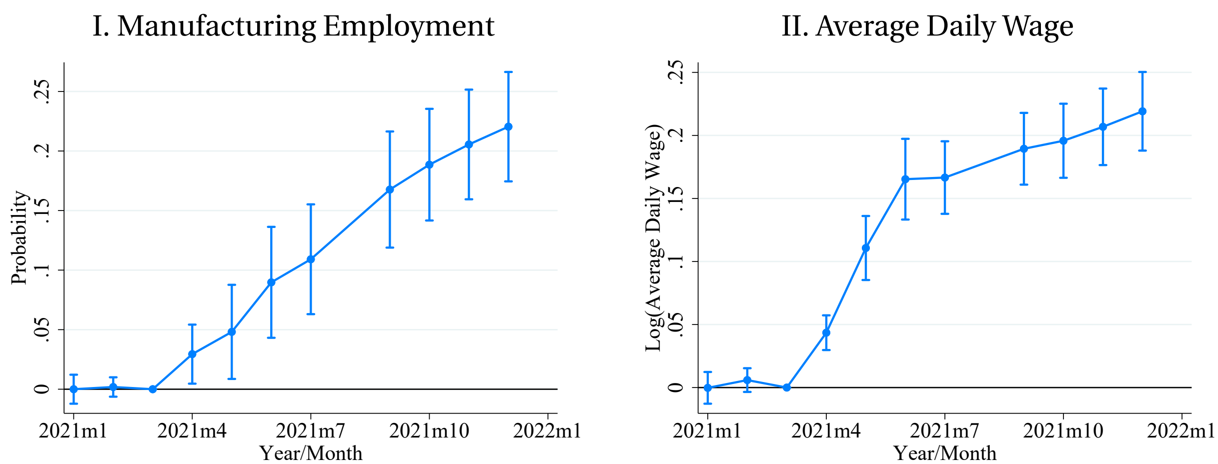
Figure 11: Drop in the Number of Firms in the Professional Services Sector of IMSS



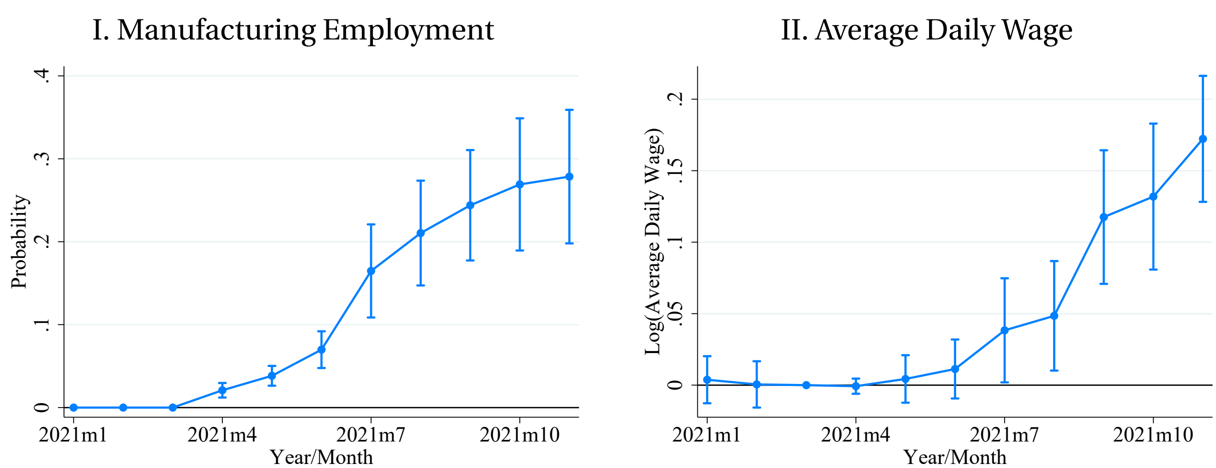
Notes: This figure presents the number of active firms registered with the IMSS in the "professional services to other firms" and manufacturing sectors relative to their levels in January for each month of 2021.
Source: Authors' elaboration using matched employer–employee data from the IMSS for 2021.

Figure 12: Testing for Pretrends in Worker Outcomes by Identification Strategy

Panel A. Firm Exit from the Professional Services Sector



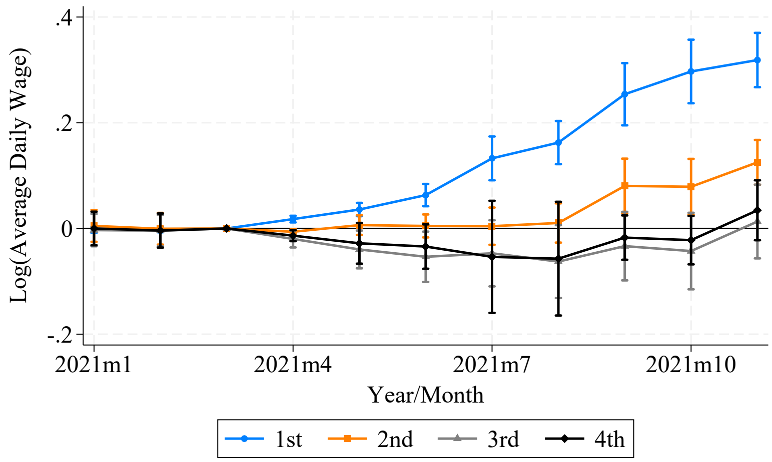
Panel B. Universal Registry of Specialized Service Providers



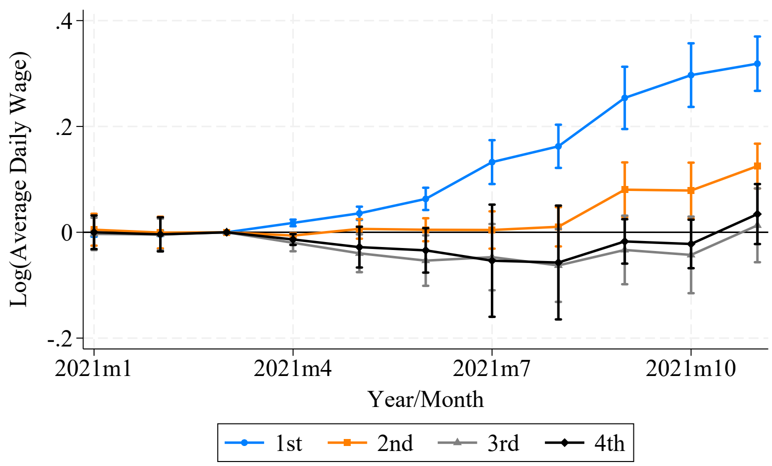
Notes: This figure presents the regression coefficients and 95 percent confidence intervals of month dummies interacted with a treatment indicator, controlling for the uninteracted month dummies and the treatment indicator itself. In Panel A, the treatment group consists of workers employed in March 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. We report effects for November 2021 in Panel B because the grace period to transfer workers of rejected companies ended on that month. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the level of the hiring firm in March 2021. 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 matched employer–employee data from the IMSS for 2021.

Figure 13: 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



Notes: This figure presents presents the regression coefficients and 95 percent confidence intervals from interacting wage quartile dummies, month dummies, and a treatment indicator, controlling for the month dummies and the treatment indicator separately interacted with the wage quartile dummies. Wage quartiles are relative to the cross-sectional distribution in March 2021. 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. We report effects for November 2021 in Panel B because the grace period to transfer workers of rejected companies ended on that month. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the level of the hiring firm in March 2021. 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 matched employer–employee data from the IMSS for 2021.

Tables

Table 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 2: Markdowns and Outsourcing
Outcome Variable: Establishment-Level Markdowns

Regressor	Translog	Translog + CRS	Cobb-Douglas	$\text{Log}(\frac{\alpha^M}{\alpha^L})$
	(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 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 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.

Table 3: Comparison of Staffing and Manufacturing Establishments, 2019

Variable	Staffing	Manufacturing		Directly-Staffing		Directly-Outsources	
		Directly	Outsources	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 Non-Salary	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 establishments in the staffing sector and the establishments in the manufacturing sector that either hire all their workers directly or outsource all of them. The p-value corresponds in Column (5) to a Wald test of difference in means between Columns (1) and (2). The p-value corresponds in Column (7) to a Wald test of difference in means between Columns (2) and (3). Staffing establishments are identified as those supplying non-specialized 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.

Table 4: The Impacts of the Reform on Establishment-Level Outsourcing and Employment

	Panel A. Firm Outsourcing		
Regressor	All Workers	Some Workers	No Workers
	(1)	(2)	(3)
$\text{Outsourcing}_{i,\text{February 2020}} \times \text{Post}_t$	-0.81***	-0.09***	0.91***
	(0.01)	(0.01)	(0.01)
N	635,755	635,755	635,755
R^2	0.616	0.004	0.395
	Panel B. Employment		
Regressor	Directly Hired	Outsourced	Total
	(1)	(2)	(3)
$\text{Outsourcing}_{i,\text{February 2020}} \times \text{Post}_t$	0.95***	-0.87***	0.09
	(0.05)	(0.01)	(0.05)
N	635,755	635,755	635,755
R^2	0.013	0.175	0.00006

Notes: Effects shown correspond to impacts in April 2024. The measure of cross-sectional exposure to the reform is the pre-COVID (February 2020) share of outsourced workers. Effects in Panel B are expressed relative to the cross-sectional employment mean in March 2021, one month prior to the enactment of the reform. Outcomes are detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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.

Table 5: Decomposition of The Impact of the Reform on Establishment-Level Labor Cost

Regressor	Total	Salaries	Social Security	Profit Sharing	Benefits	Staffing Fee	Firing Costs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{Outsourcing}_{i,\text{February 2020}} \times \text{Post}_t$	21.1**	37.7***	17.6***	12.5***	3.41***	-52.3***	2.16***
	(8.8)	(6.1)	(1.3)	(1.2)	(0.53)	(0.78)	(0.25)
N	635,735	635,735	635,735	635,735	635,735	635,735	635,735
R^2	0.0004	0.003	0.013	0.004	0.0004	0.16	0.003

Notes: Effects shown correspond to average monthly impacts in 2023. Outcome variables are expressed relative to the cross-sectional mean of total labor cost in March 2021. The measure of cross-sectional exposure to the reform is the pre-COVID (February 2020) share of outsourced workers. Outcomes are detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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. All monetary amounts are deflated using Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Table 6: The Impacts of the Reform on Establishment-Level Input Utilization

Regressor	Capital Stock	Raw Materials	Energy Consumption	Investment
	(1)	(2)	(3)	(4)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	-0.06	0.01	0.02	-0.09*
	(0.05)	(0.05)	(0.05)	(0.05)
N	35,512	35,512	35,512	31,883
R^2	.00007	0.000001	0.000006	.0002

Notes: Effects shown correspond to the average annual impact of the reform 2 years after the reform on the log of each variable. The measure of cross-sectional exposure to the reform is the share of outsourced workers in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The sample used for estimation in Column (4) only includes observations for which the first-order lag of the capital stock is available. * p<0.1.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2022. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Table 7: The 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)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	0.06	-0.02	-0.02	-0.01	-0.01
	(0.06)	(0.02)	(0.02)	(0.02)	(0.02)
N	35,511	8,675	8,675	8,675	8,675
R^2	0.0001	0.0003	0.0004	0.0004	0.0004

Notes: Regressions in Columns (2) through (5) only include observations of establishments that did not attrit the annual manufacturing panel after its rotation in 2021 for which, in addition, a lag of the input variables is available. Effects shown correspond to impacts 2 years after the reform. The measure of cross-sectional exposure to the reform is the share of outsourced workers in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. 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 2022. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Table 8: The Impacts of the Reform on Market Exit at the Establishment Level

Regressor	Exit
	(1)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	0.011***
	(0.003)
N	47,120
R^2	0.002

Notes: The effect shown corresponds to 2022. The cross-sectional measure of exposure to the reform is the share of outsourced workers in 2020. The sample used for estimation is a balanced panel running from 2013 to 2022, which includes all manufacturing establishments that did not attrit from the annual manufacturing survey because of its rotation in 2021 and that had not exited the market before the enactment of the reform. Outcomes are detrended to account for group-specific pre-trends prior to estimation. 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.

Table 9: The Impacts of the Reform on Establishment-Level Labor Share and Markdowns

Regressor	Labor Share	Log Markdowns			
		Translog	Translog+CRS	Cobb-Douglas	$\frac{\alpha_M}{\alpha_L}$
	(1)	(2)	(3)	(4)	(5)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	0.06***	-0.23***	-0.23***	-0.32***	-0.30***
	(0.01)	(0.08)	(0.06)	(0.07)	(0.06)
N	35,512	8,675	8,675	8,675	8,675
R^2	.0034	.002	.002	.003	.004

Notes: Regressions in Columns (2) through (5) only include observations of establishments that did not attrit the annual manufacturing panel after its rotation in 2021 for which, in addition, a lag of the input variables is available. Effects shown correspond to impacts 2 years after the reform. The measure of cross-sectional exposure to the reform is the share of outsourced workers in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. We exclude all expenses other than capital, raw materials, energy, and labor in the calculation of the labor share. 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 2022. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Table 10: Impact Heterogeneity

Outcome Variable: Wage Markdowns

Regressor	(1)	(2)	(3)	(4)	(5)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	-0.01	-0.21**	-0.37***	-0.30***	-0.22
	(0.05)	(0.09)	(0.10)	(0.10)	(0.17)
Interacted with:					
Markdown>75th Percentile at Baseline	-0.65***				
	(0.15)				
Top 5 Markdown Industry in 2019		-0.63***			
		(0.13)			
Central or South Region			0.17		
			(0.16)		
Foreign Ownership				-0.03	
				(0.16)	
Metropolitan Area					-0.11
					(0.19)
N	8,675	8,675	8,675	8,675	8,675
R^2	.01	.006	.003	.003	.003

Notes: The estimation sample for the regressions in this table includes only observations of establishments that did not attrit the annual manufacturing panel after its rotation in 2021 for which, in addition, a lag of the input variables is available. The effects shown correspond to impacts in 2022. The measure of cross-sectional exposure to the reform is the share of outsourced workers in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. All regressions control for the interacted variables. ** p<0.05, ***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 2022.

Table 11: Effects of the Reform on Worker Outcomes

Regressor	Employment in Manufacturing (1)	Log(Average Monthly Wage) (2)
	Panel A. Firm Exit from the Professional Services Sector	
$\widehat{\text{Staffing}}_{i,\text{March 2021}} \times \text{December}_t$	0.22***	0.22***
	(0.023)	(0.016)
N	78,935,384	72,496,441
R^2	0.041	0.008
	Panel B. Universal Registry of Specialized Service Providers	
$\text{Unsuccessful}_{i,\text{March 2021}} \times \text{November}_t$	.28***	.17***
	(0.041)	(.022)
N	13,111,945	13,111,197
R^2	0.05	0.982

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. The reported coefficients correspond to regressions of the outcomes of interest on month dummies interacted with a treatment indicator, controlling for the uninteracted month dummies and the treatment indicator itself. 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. We report effects for November 2021 in Panel B because the grace period to transfer workers of rejected companies ended on that month. 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 IMSS for 2021.

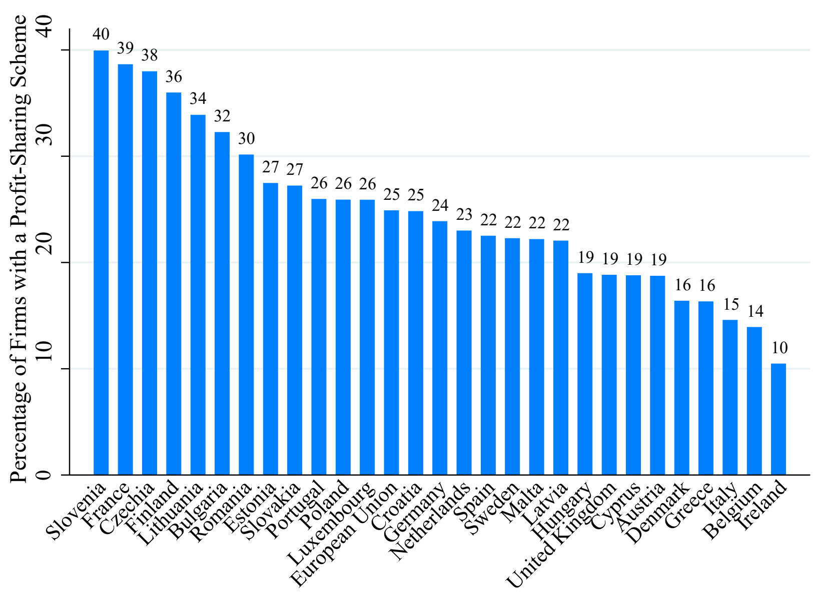
Supplementary Appendix: For Online Publication Only

This appendix contains additional information and analyses. Appendix A reports the results of secondary analyses, including additional figures and tables. In Appendix B, we present evidence of patterns of outsourcing across different types of establishments. Appendix C details our markdown estimation procedure. Appendix D reports the results of a battery of robustness checks for key empirical results in the main body of the paper.

A Supplementary Analyses

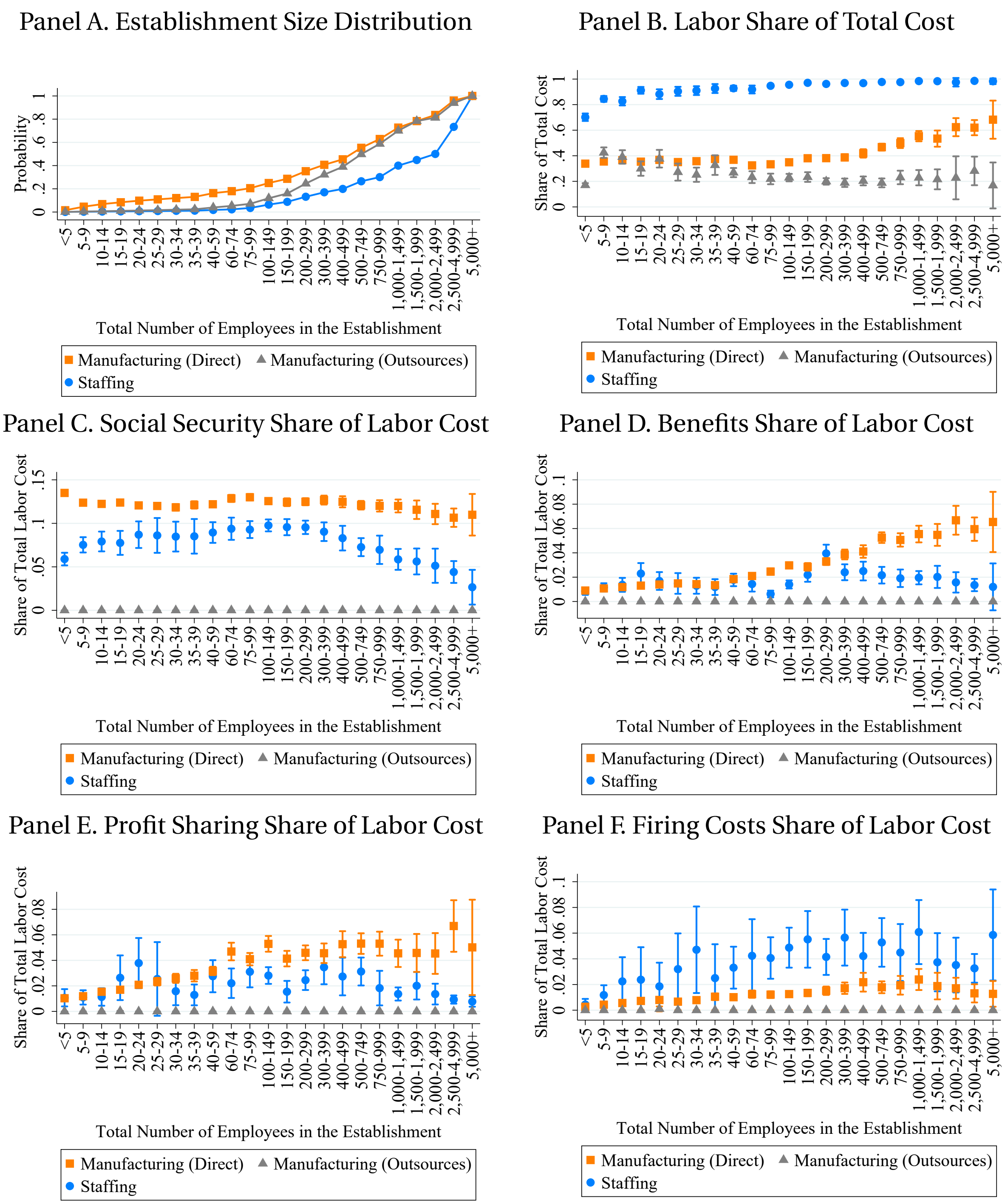
A.1 Additional Figures and Tables

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: Comparison of Staffing and Manufacturing Establishments, 2019



Notes: This figure compares the size distribution and cost structure of manufacturing establishments that hire all their workers directly, manufacturing establishments that outsource all their workers, and staffing establishments. Staffing establishments are identified as those supplying non-specialized 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.

Table A.1: 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 product	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)

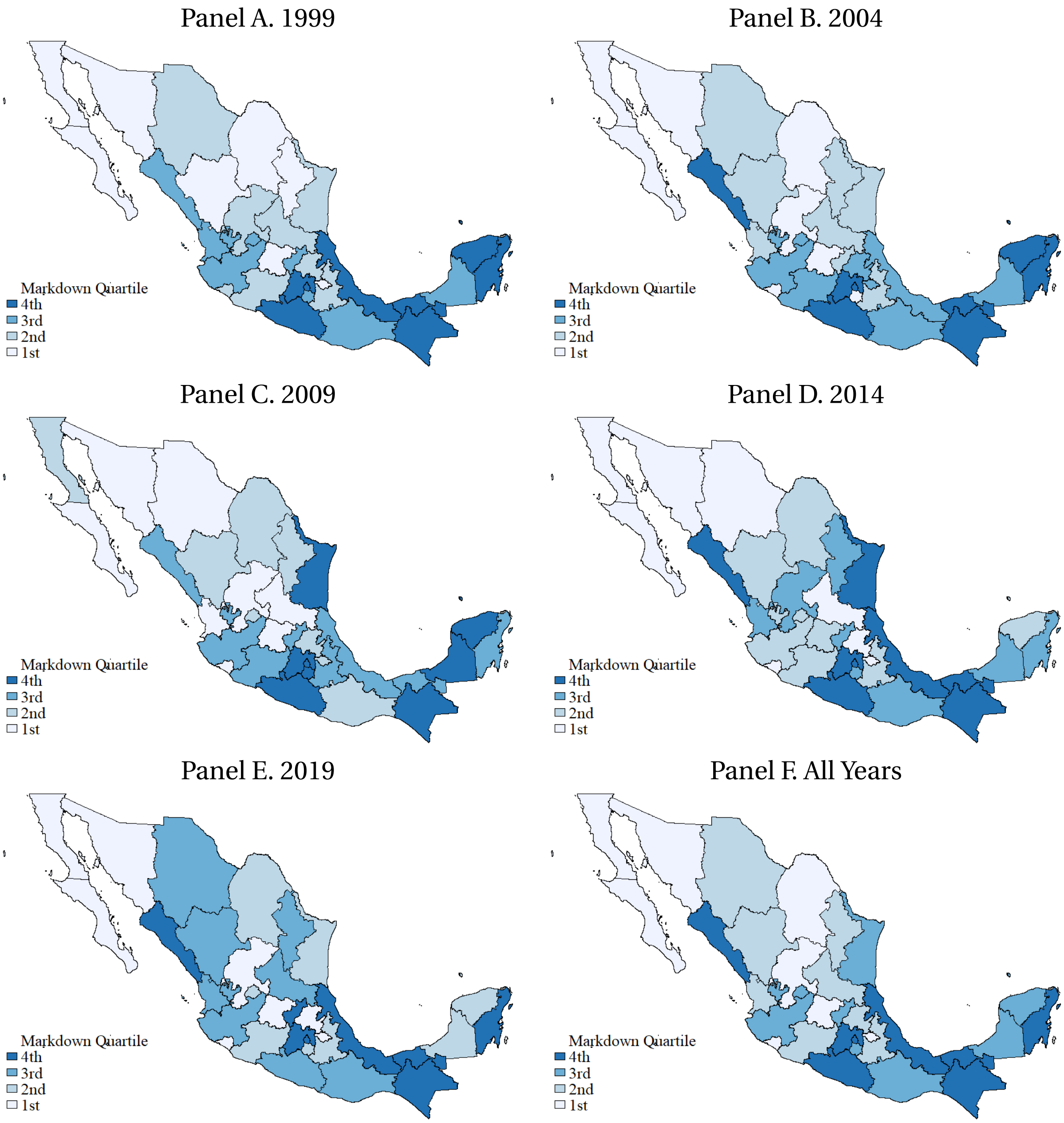
Notes: We estimate markdowns assuming that the production function is translog with parameters that vary at the industry group level. Industry groups are defined by 3-digit 1997 NAICS codes for manufacturing industries. Industries are sorted in descending order according to their average markdown in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

Table A.2: 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)

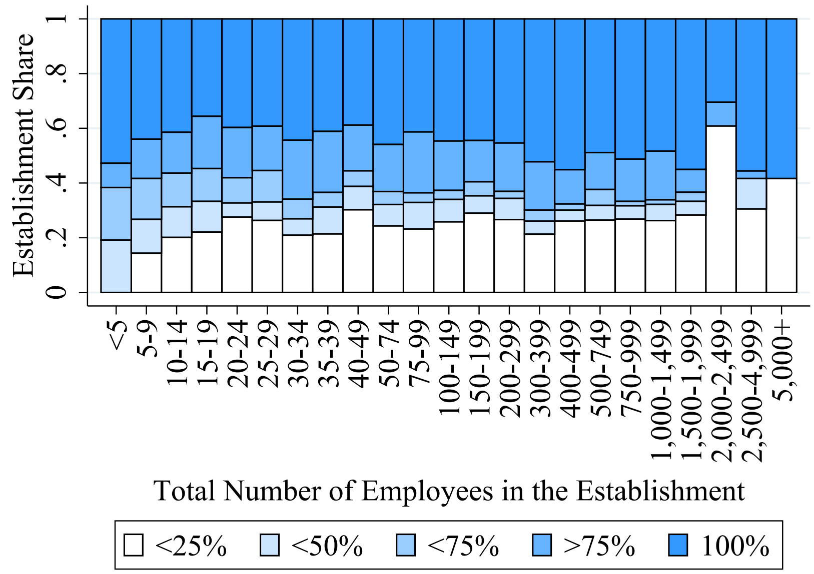
Notes: The North region includes Baja California, Baja California Sur, Coahuila, Chihuahua, Durango, Nuevo León, Sinaloa, Sonora, and Tamaulipas. The *Bajío* region includes Aguascalientes, Colima, Guanajuato, Jalisco Michoacán, Nayarit, Querétaro, San Luis Potosí, and Zacatecas. The Center region includes 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 NAICS codes for manufacturing industries. Standard errors are in parentheses.
Source: Authors' elaboration using data from the Mexican economic census.

Figure A.3: Average Markdown in Manufacturing by State and Census Wave



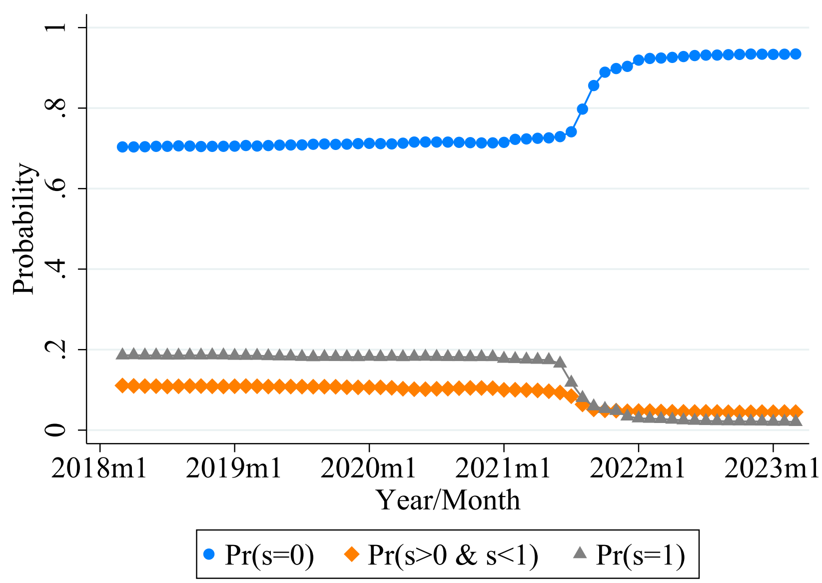
Notes: Each shade in the figure denotes a different quartile of the average markdown distribution, with lighter shades representing lower quartiles and darker shades representing higher quartiles. Quartiles in Panels A through E are taken with respect to the cross-sectional distribution of average markdowns 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 NAICS industry code level.
Source: Authors' elaboration using data from the Mexican economic census.

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



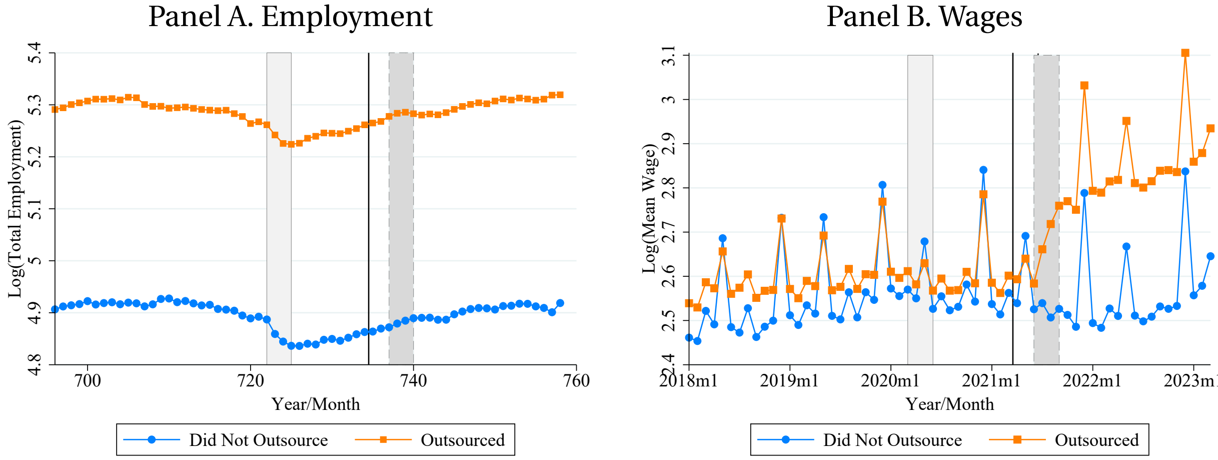
Notes: This figure presents the distribution of outsourced employee percentages by establishment size, conditional on hiring at least one employee through outsourcing, for the universe of manufacturing establishments that keep employment and wage accounts and report positive labor, capital, raw materials,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

Figure A.5: Outsourcing Probabilities at the Establishment Level, 2018–2023



Notes: This figure presents the probability that an establishment hires a share $s \in [0,1]$ of its employees through outsourcing from 2018 to 2023.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2023.

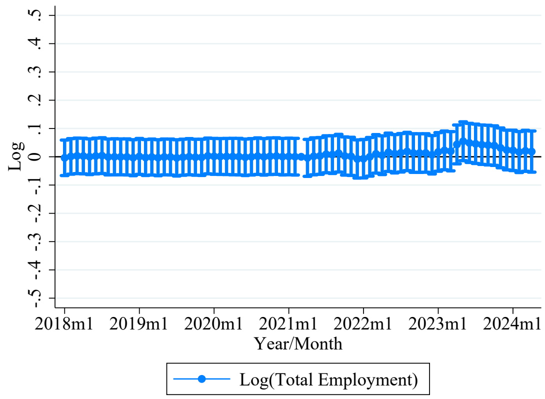
Figure A.6: Raw Trends in Employment and Mean Wage at the Establishment Level



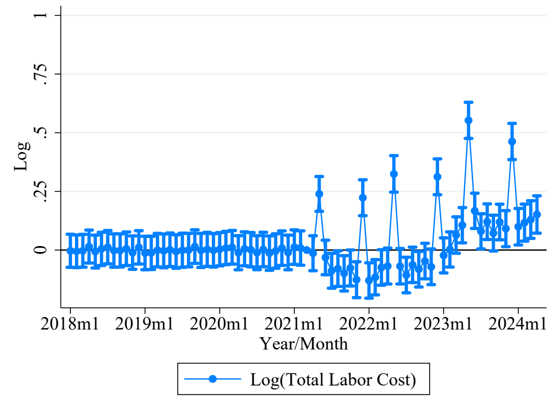
Notes: This figure presents mean employment and wages from 2018 to 2023 in establishments that outsourced at least some worker and establishments that hired all their workers directly in February 2020, the month prior to the onset of COVID-19. The vertical solid line depicts the enactment of the reform, while the vertical dashed line depicts the limit date for its enactment. The first gray area outlined with a solid line represents the strictest COVID-19 lockdown prescribed by federal authorities in Mexico. The second gray area outlined with a dashed line represents the grace period to transfer previously outsourced workers to their employing companies, mandated by the reform.
Source: Authors' elaboration using data from the. Mexican monthly manufacturing survey from 2018 to 2022. Wages are deflated to July 2019 using the intermediate consumption sub-index of Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Figure A.7: Tests for Pre-Trends in Establishment-Level Logged Outcomes

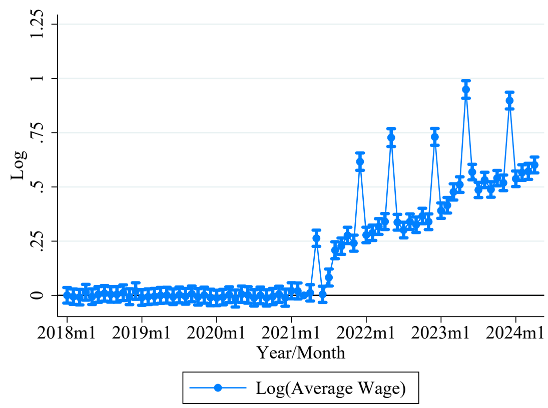
Panel A. Employment



Panel B. Total Labor Cost



Panel C. Mean Wage



Notes: Each panel in this figure presents the regression coefficients and 95% confidence intervals of month dummies interacted with the pre-COVID (February 2020) share of workers outsourced by the establishment, controlling for date and calendar month dummies and the pre-COVID share of outsourced workers. Outcomes are detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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 effects can be interpreted as deviations away from the outcome mean of the comparison group the month prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2024. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Table A.3: The Impacts of the Reform on Establishment-Level Logged Outcomes

Regressor	Employment	Total Labor Cost	Average Wage
	(1)	(2)	(3)
$\text{Outsourcing}_{i,\text{February 2020}} \times \text{Post}_t$	0.04	0.15***	0.56***
	(0.03)	(0.04)	(0.02)
N	615,375	615,375	615,375
R^2	0.00002	0.0002	0.024

Notes: Effects shown correspond to average monthly impacts in 2023. The measure of cross-sectional exposure to the reform is the pre-COVID (February 2020) share of outsourced workers. Outcomes are detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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 to July 2019 using Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Figure A.8: Tests for Pre-Trends in Establishment-Level Logged Outcomes: Annual Manufacturing Survey

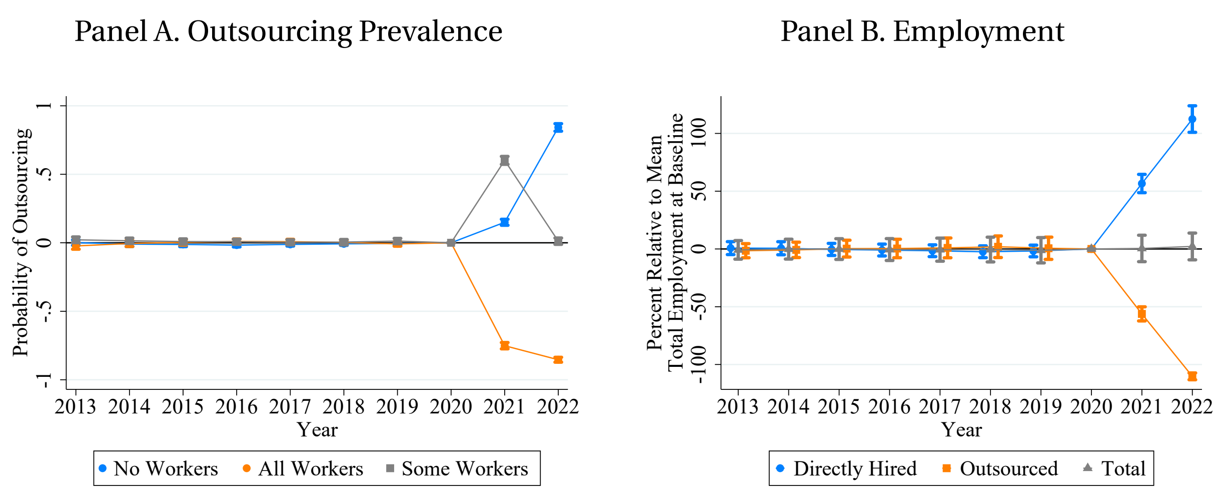
Notes: Each panel in this figure presents the regression coefficients and 95% confidence intervals of year dummies interacted with the employment share of outsourcing of the establishment in 2020, controlling for year and the employment share of outsourcing in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for 2020 is excluded from each regression, so effects can be interpreted as deviations away from the outcome mean of the group with zero exposure the year prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2022. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Table A.4: The Impacts of the Reform on Establishment-Level Logged Outcomes: Annual Manufacturing Survey

Regressor	Employment	Total Labor Cost	Average Wage
	(1)	(2)	(3)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	0.04	-0.01	0.44***
	(0.04)	(0.05)	(0.02)
N	35,512	35,512	35,512
R^2	0.00006	0.00005	0.017

Notes: Effects shown correspond to impacts in 2022. The measure of cross-sectional exposure to the reform is the share of outsourced workers in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. 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 2022. All monetary amounts are deflated to July 2019 using Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Figure A.9: Tests for Pre-Trends in Establishment-Level Outsourcing and Employment: Annual Manufacturing Survey



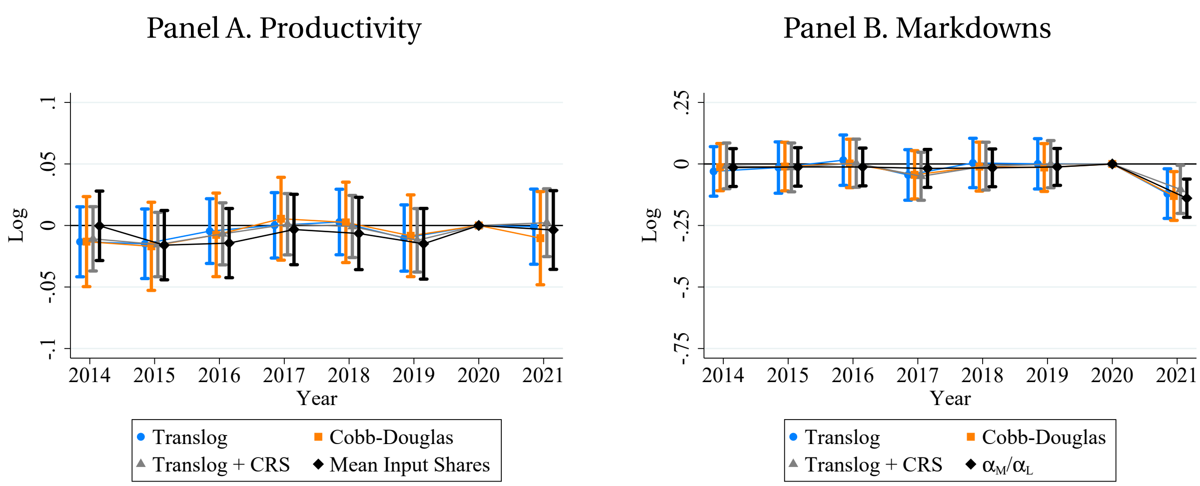
Notes: Each panel in this figure presents the regression coefficients and 95% confidence intervals of year dummies interacted with the employment share of outsourcing of the establishment in 2020, controlling for year and the employment share of outsourcing in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for 2020 is excluded from each regression, so effects can be interpreted as deviations away from the outcome mean of the group with zero exposure the year prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2022.

Table A.5: The Impacts of the Reform on Establishment-Level Outsourcing and Employment: Annual Manufacturing Survey

Regressor	*Panel A. Firm Outsourcing*		
	All Workers	Some Workers	No Workers
	(1)	(2)	(3)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	0.84***	0.01	-0.85***
	(0.01)	(0.01)	(0.01)
N	35,512	35,512	35,512
R^2	0.178	0.082	0.426
	Panel B. Employment		
Regressor	Directly Hired	Outsourced	Total
	(1)	(2)	(3)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	112.4***	-110.3***	2.2
	(5.8)	(1.5)	(5.9)
N	35,512	35,512	35,512
R^2	0.027	0.068	0.00001

Notes: Effects shown correspond to impacts in 2022. The measure of cross-sectional exposure to the reform is the share of outsourced workers in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. Effects in Panel A are expressed in percentage points. Effects in Panel B are expressed in percent relative to the mean employment level across all establishments in 2020, the year prior to the reform. ***p<0.01.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2022.

Figure A.10: Tests for Pre-Trends in Establishment-Level Productivity and Markdowns: Initial Panel of the Annual Manufacturing Survey



Notes: Each panel in this figure presents the regression coefficients and 95% confidence intervals of year dummies interacted with the employment share of outsourcing of the establishment in 2020, controlling for year and the employment share of outsourcing in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for 2020 is excluded from each regression, so effects can be interpreted as deviations away from the outcome mean of the group with zero exposure the year prior to the 2021 reform.
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 A.6: The Impacts of the Reform on Establishment-Level Productivity and Markdowns: Initial Panel of the Annual Manufacturing Survey

		Panel A. Productivity		
Regressor	Translog	Translog+CRS	Cobb-Douglas	Mean Input Shares
	(1)	(2)	(3)	(4)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	-0.01	-0.001	0.002	-0.004
	(0.02)	(0.02)	(0.01)	(0.02)
N	19,167	19,167	19,167	19,167
R^2	.0001	.0001	.0002	.0002
		Panel B. Log Markdowns		
Regressor	Translog	Translog+CRS	Cobb-Douglas	$\frac{\alpha_M}{\alpha_L}$
	(1)	(2)	(3)	(4)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	-0.12**	-0.13**	-0.10**	-0.14***
	(0.05)	(0.05)	(0.05)	(0.04)
N	19,167	19,167	19,167	19,167
R^2	.0004	.0004	.0003	.0007

Notes: Effects shown correspond to impacts in 2021. The measure of cross-sectional exposure to the reform is the share of outsourced workers in 2020. Outcomes are detrended to account for group-specific pre-trends prior to estimation. 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 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).

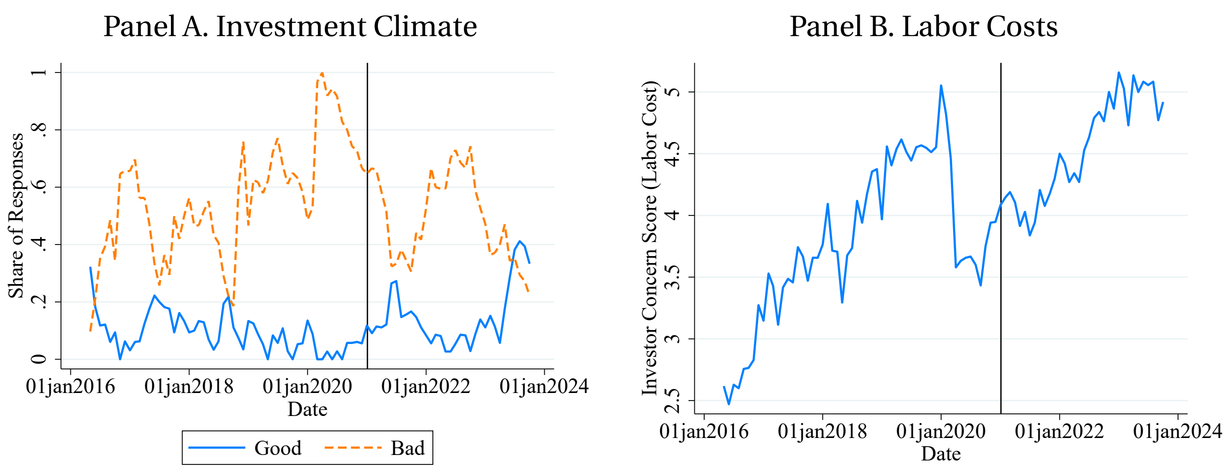
A.2 Reform's Impacts on Investment Perspectives

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 A.11 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:

$$\Delta Y_t = \alpha + \mathbb{1}_{\{t > \text{April 2021}\}} \beta + \varepsilon_t, \tag{A1}$$

where Y_t is the mean response of the interviewees 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 using the methodology in Newey and West (1987). Table A.7 shows the results of this exercise. We find no evidence of structural breaks in interviewees' perceptions about the investment environment and the burden of labor costs after the reform.

Figure A.11: 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 A.7: Tests for Structural Breaks in Survey Responses after the Outsourcing Reform

Regressor	Investment Climate		Labor Cost
	Good	Bad	
	(1)	(2)	(3)
$\mathbb{1}$_{t > 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

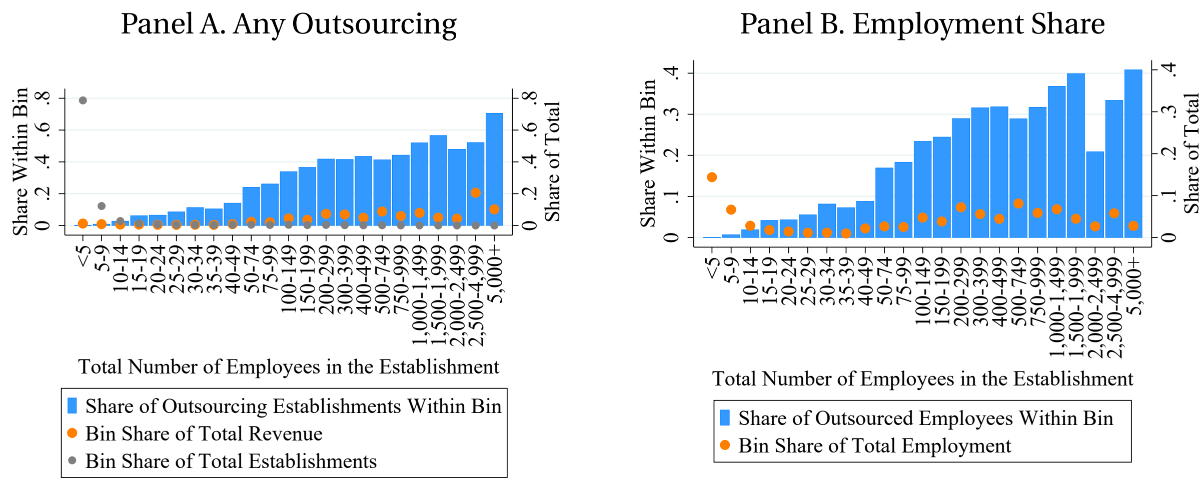
Notes: Standard errors are robust to heteroskedasticity of unknown form and autocorrelation of up to one lag. *Source:* Authors' elaboration using data from the Mexican central bank's private sector perceptions survey from May 2016 to October 2023.

B Outsourcing Patterns by Firm Type

B.1 Large Firms', Foreign Firms', and *Maquiladoras*' Outsourcing

We present visual and regression 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 wage 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.

The regression results reported in Table B.1 show that this relationship also exists at the establishment level, under the same and alternative establishment size measures, even after we control for industry and year dummies. Column (1) shows that a 1 percent increase in the total employee count is associated with an increase in the establishment's share of outsourced workers of 1 percentage point (p=0.000). Columns (2) through (4) show strongly significant correlations between the share of outsourced workers in the establishment and the establishment's local labor market employment share, the log of establishment revenue, and the local labor market revenue share of the establishment, respectively.

The table also shows that foreign-owned manufacturing establishments, which proliferated with the rise in FDI after the enactment of NAFTA (Cuevas, Messmacher and Werner, 2005), display a higher share of outsourced employees than domestic establishments. The correlation between foreign ownership and outsourcing is of interest because a well-documented regularity in the trade literature is that firms receiving FDI are larger and more productive than other firms (see Helpman, 2006). Besides their size and propensity to receive FDI, foreign-owned firms may have greater bargaining power vis-à-vis local workers than other establishments because of the credible threat that they might relocate their operations if labor costs rise.

Table B.1: 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.

Column (5) of the table shows that the share of outsourced employees is 5 percentage points higher among foreign-owned establishments 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 U.S.[35] 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

[35] See Estefan (2023) for a thorough description of this program.

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).

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.2 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.2: 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 four types of employment arrangements: remunerated insourced workers, nonpaid insourced workers, outsourced workers, and workers hired as contractors. Importantly, all worker counts include only workers hired to work on the establishment's premises, excluding those 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:

$$L_{it} = \mathrm{L}_{it}^{\text{Insourced Paid}} + \mathrm{L}_{it}^{\text{Outsourced}} + \mathrm{L}_{it}^{\text{Contractors}},$$

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

Our labor input measure excludes nonpaid insourced workers. Including these workers would lead to systemic 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 nonpaid insourced workers are 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 remunerated insourced workers, outsourced workers, and workers hired as contractors, 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

$$TC_{it} = w_{it}L_{it} + r_{it}K_{it} + p^M_{it}M_{it} + p^E_{it}E_{it}.$$

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 Robustness Checks and Alternative Estimation Approaches

We conduct two checks to verify the robustness of our estimates. First, 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 our having insufficient information about the shape of the production function in the moment conditions implied by our IV strategy. To resolve this issue, we reestimate the markdowns assuming constant returns to scale, as suggested in (Flynn, Traina and Gandhi, 2019). Thus, in addition to the moment conditions in Equation (E22), we add a constant-returns-to-scale assumption,[36] which implies the following moment condition:

$$E\left[\sum_{I\in\{l,k,m,e\}} \frac{\partial f(l_{it}, k_{it}, m_{it}, e_{it})}{\partial I_{it}}\right] - 1 = 0. \tag{C1}$$

Second, 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

[36] 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).

that relies on the revenue elasticity of the flexible input equals one and thus is uninformative about actual markups.[37] 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 (E18), we have $\log \nu_{it} = \log \theta^L_{it} - \log \alpha^L_{it} - \mu_{it}$. Substituting Equation (E15) into this equation and rearranging terms, we have

$$\log\left(\frac{\alpha^M_{it}}{\alpha^L_{it}}\right) = \log\left(\frac{\beta^M}{\beta^L}\right) + \log \nu_{it}, \tag{C2}$$

where the input elasticities $\beta^M = \theta^M_{it}$ and $\beta^L = \theta^L_{it}$ 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:

$$\log \nu_{it} = \delta_0 + D_{it}\delta_1 + \eta_{it}, \tag{C3}$$

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

Substituting Equation (C2) into Equation (C3), we have the linear specification

$$\log\left(\frac{\alpha^M_{it}}{\alpha^L_{it}}\right) = \delta_0 + \log\left(\frac{\beta^M}{\beta^L}\right) + D_{it}\delta_1 + \eta_{it}. \tag{C4}$$

From this equation, we can learn about the association between log markdowns and the binary variable D_{it}. Thus, we estimate Equation (C4) via OLS for each characteristic of interest.

C.3 Proofs

C.3.1 Proof: The Ratio Estimator of the Markup Equals 1

This result follows because, if a 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

[37] See Appendix C.3 for a proof of this and the following statement.

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.

C.3.2 Proof: The Ratio Estimator of the Markdown Recovers Actual Markdowns

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 (E18).

C.4 Methodology for the Labor Cost Decomposition

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

$$\text{Total Labor Cost}_{it} = \text{Wage Bill}^{D}_{it} + \text{Wage Bill}^{O}_{it} + \text{Staffing Fee}^{O}_{it} + \text{Firing Costs}_{it}, \tag{C5}$$

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 its 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

$$\text{Wage Bill}^{j}_{it} = \text{Salaries}^{j}_{it} + \text{Social Security}^{j}_{it} + \text{Profit Sharing}^{j}_{it} + \text{Other Benefits}^{j}_{it}. \tag{C6}$$

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:

$$\widehat{\text{Wage Bill}}^{O}_{it} = \bar{s}_L^{\text{Staffing}} \text{Total Payment}_{it}, \text{ and} \tag{C7}$$

$$\widehat{\text{Staffing Fee}}^{O}_{it} = (1 - \bar{s}_L^{\text{Staffing}}) \text{Total Payment}_{it}. \tag{C8}$$

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, social security payments, profit sharing, and other benefits for outsourced workers. For example, we impute salaries as follows:

$$\text{Salaries}_{it}^{O} = \bar{s}_{\text{Salaries}}^{\text{Staffing}} \widehat{\text{Wage Bill}}_{it}^{O}, \tag{C9}$$

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 total cost at the establishment level, defined as

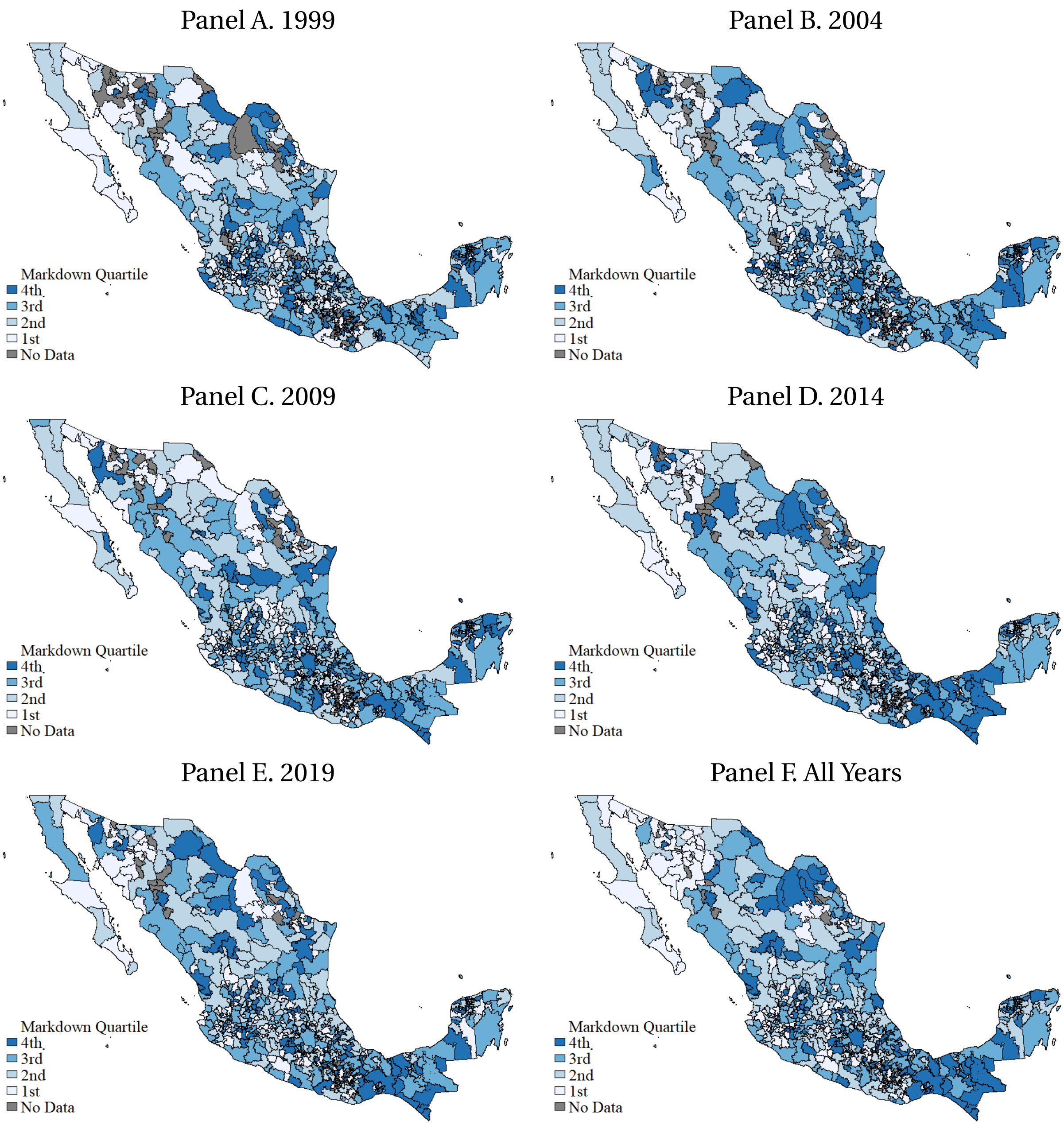
$$\widehat{\text{Average Wage}}_{it} = \frac{\text{Wage Bill}_{it}^{D} + \widehat{\text{Wage Bill}}_{it}^{O}}{\text{L}_{it}^{D} + \text{L}_{it}^{O}}, \text{ and} \tag{C10}$$

$$\widehat{s}_{it}^{L} = \frac{\text{Wage Bill}_{it}^{D} + \widehat{\text{Wage Bill}}_{it}^{O}}{\text{Total Cost}_{it}}. \tag{C11}$$

D Robustness Checks

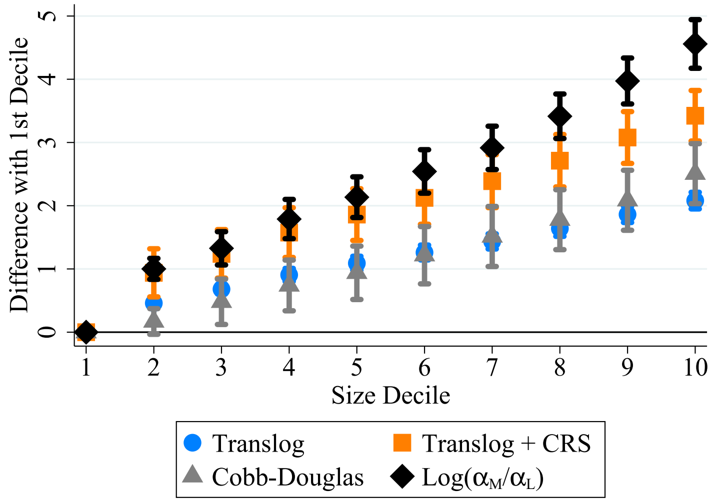
D.1 Commuting Zones as Local Labor Market Definition

Figure D.1: 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 NAICS industry code level.
Source: Authors' elaboration using data from the Mexican economic census.

Figure D.2: 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 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.

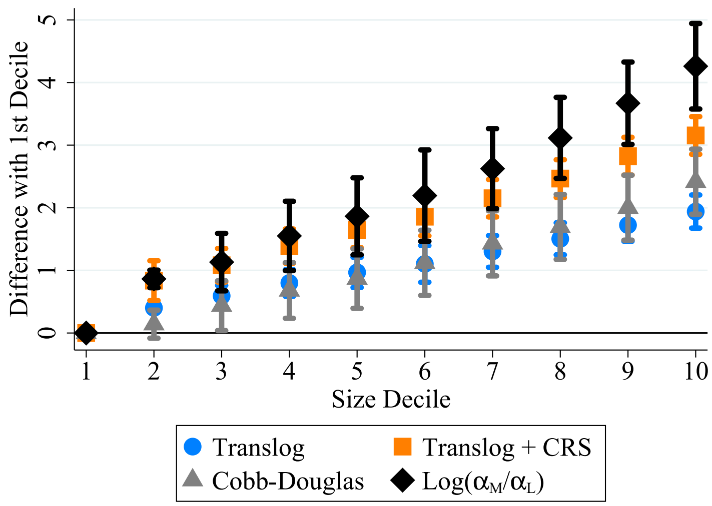
Table D.1: Commuting Zones as Local Markets – Outsourcing and Establishment Size
Outcome Variable: Share of Outsourced Employees

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 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.2 Markdowns and Outsourcing Gradients with Firm-Level Revenue

Figure D.3: 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 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.

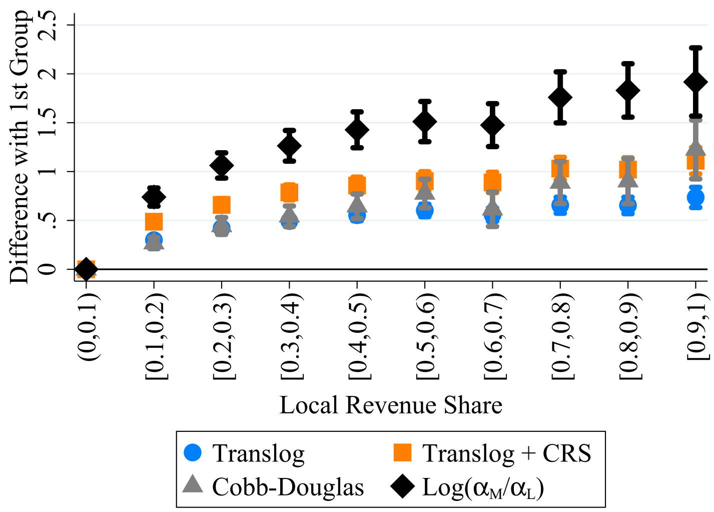
Table D.2: Outsourcing and Firm Size
Outcome Variable: Firm Share of Outsourced Employees

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 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.3 Markdown Gradient under an Alternative Partition of the Size Range

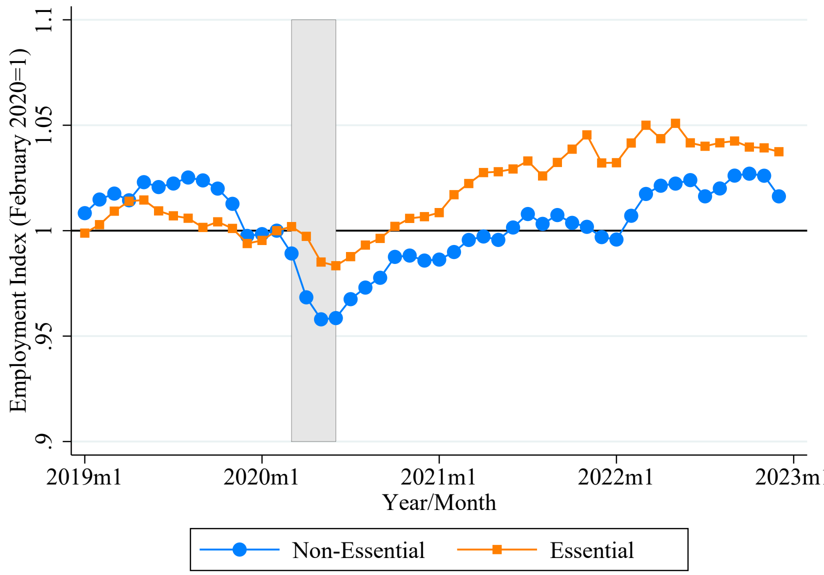
Figure D.4: Markdown Gradient under an Alternative Partition of the Size Range



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 NAICS industry codes × 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.

D.4 Differential Impacts by Essential Industry Status During COVID-19

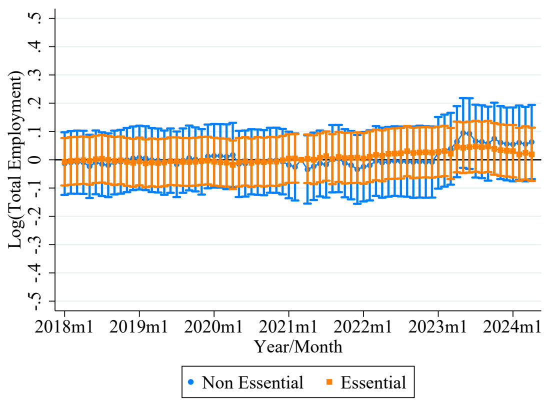
Figure D.5: Employment Recovery by Industry Status During COVID-19



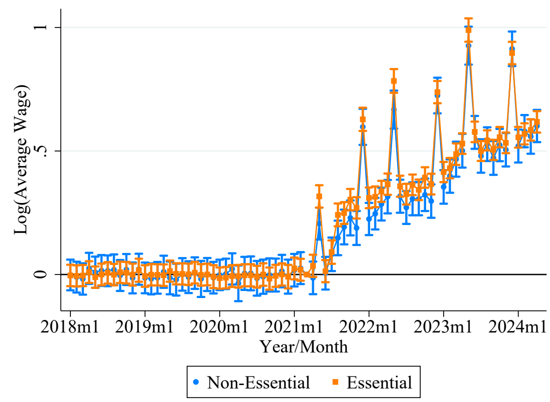
Notes: This figure presents trends in the cross-sectional mean of log employment in the manufacturing sector from 2019 to 2022, by date of return to work following the onset of the COVID-19 pandemic. The indicator "essential" takes the value of 1 if, by government mandate, the economic activities of the establishment were deemed as essential to the economy and were therefore allowed to resume in June 2020, and 0 otherwise. The gray area represents the most restrictive lockdown prescribed by 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 D.6: Tests for Pretrends in Employment and Wages by Industry Status

Panel A. Employment



Panel B. Wages



Notes: This figure presents the results from fully interacting our differences-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 as essential to the economy and were allowed to resume in June 2020, and 0 otherwise. Each panel in this figure presents the regression coefficients and 95% confidence intervals of month dummies interacted with the pre-COVID (February 2020) share of workers outsourced by the establishment, controlling for date and calendar month dummies and the pre-COVID share of outsourced workers in June 2020. Outcomes are detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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 effects can be interpreted as deviations away from the outcome mean of the comparison group the month prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2024. Wages are deflated to July 2019 using the intermediate goods sub-index of Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

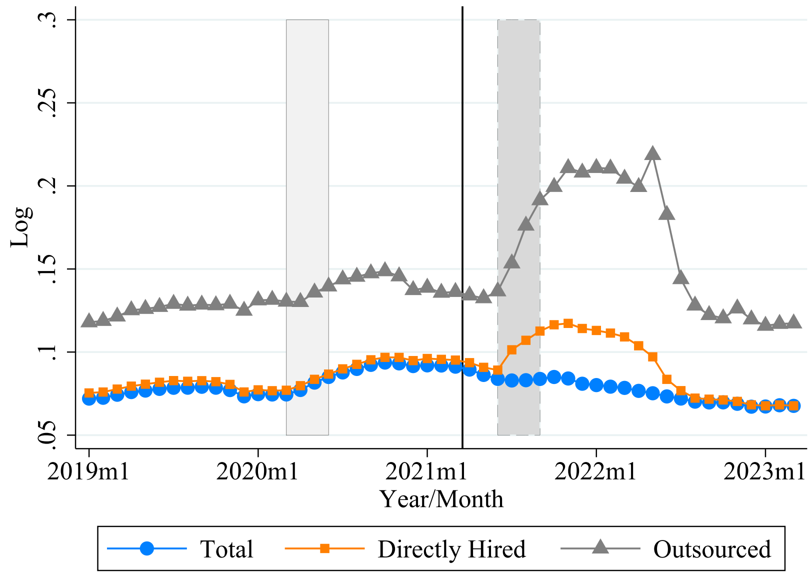
Table D.3: The Impacts of the Reform on Establishment-Level Employment and Wages by Industry Status

Regressor	Employment (1)	Average Wage (2)
Non-Essential$_i$ × Outsourcing$_{i,\text{February 2020}}$ × Post$_t$	0.02	0.40***
	(0.06)	(0.03)
Essential$_i$ × Outsourcing$_{i,\text{February 2020}}$ × Post$_t$	0.03	0.44***
	(0.04)	(0.02)
p-value (H_0: Non-Essential=Essential)	.944	.223
N	615,375	615,375
R^2	.00003	.023

Notes: This figure presents the results from fully interacting our differences-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 as essential to the economy and were allowed to resume in June 2020, and 0 otherwise. Effects shown correspond to average impacts for the entire post-reform period. The measure of cross-sectional exposure to the reform is the pre-COVID (February 2020) share of workers outsourced by the establishment. Outcomes are detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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. Wages are deflated to July 2019 using the intermediate goods sub-index of Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

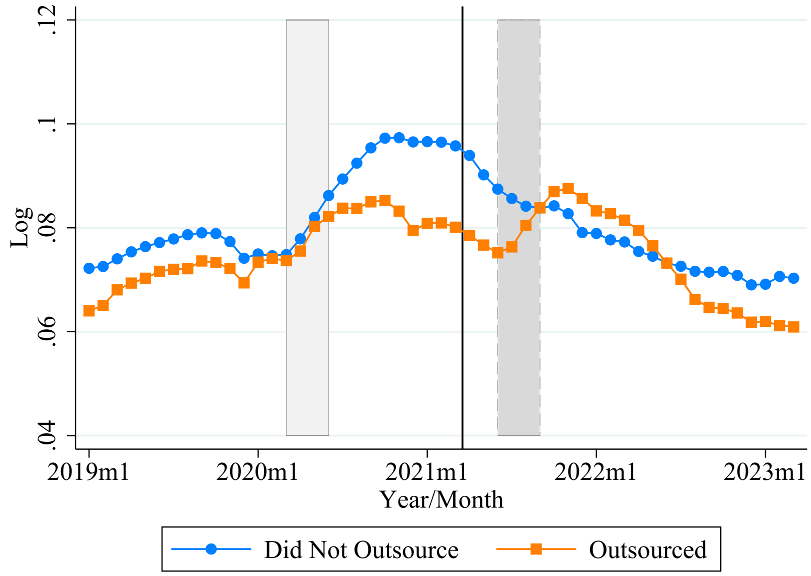
D.5 Reform Impacts on Employment Volatility

Figure D.7: Standard Deviation of Log Employment by Employment Type



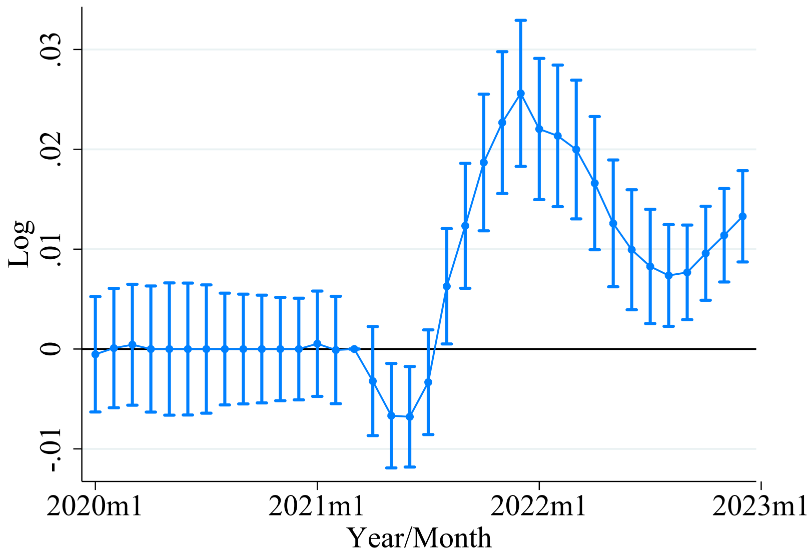
Notes: This figure presents trends in the mean volatility of employment in the manufacturing sector by employment type. Our measure of employment volatility is the standard deviation of log employment at the establishment level, which we calculate for each month using data from a 12-month rolling window.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

Figure D.8: Trends in the Employment Volatility of Exposed and Non-Exposed 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 an 12-month rolling window. The vertical solid line depicts the enactment of the reform, while the vertical dashed line depicts the limit date for its enactment. The first gray area, outlined by a solid line, represents the strictest COVID-19 lockdown prescribed by federal authorities in Mexico. The second gray area, outlined by a dashed line, represents the grace period mandated by the reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2019 to 2023.

Figure D.9: Test for Pretrends in the Establishment-Level Standard Deviation of Employment



Notes: This figure presents the presents the regression coefficients and 95% confidence intervals of month dummies interacted with the pre-COVID (February 2020) share of workers outsourced by the establishment, controlling for date and calendar month dummies and the pre-COVID share of outsourced workers. 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 outcome is detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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 effects can be interpreted as deviations away from the outcome mean of the comparison group the month prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2020 to 2023.

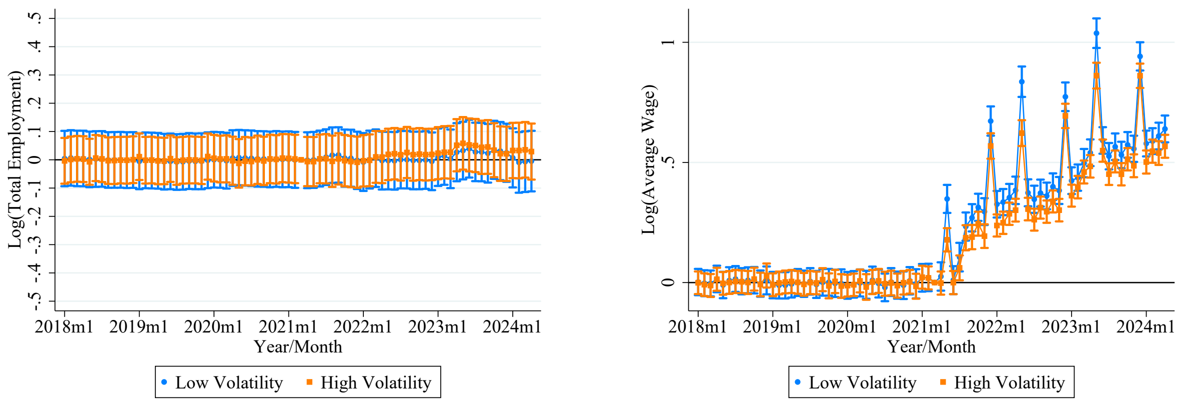
Table D.4: The Impacts of the Reform on Establishment-Level Employment Volatility

Regressor	Standard Deviation of Log Employment
	(1)
$\text{Outsourcing}_{i,\text{February 2020}} \times \text{Post}_t$	0.01***
	(0.002)
N	328,547
R^2	.0008

Notes: The outcome variable 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. Effects shown correspond to average impacts for the post-reform period ending in December 2022. The measure of cross-sectional exposure to the reform is the pre-COVID (February 2020) share of workers outsourced by the establishment. The outcome is detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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 2020 to 2022.

Figure D.10: Tests for Pretrends in Establishment-Level Employment and Wages by Employment Volatility at Baseline

Panel A. Employment | Panel B. Wages



Notes: This figure presents the results from fully interacting our differences-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. Each panel in this figure presents the regression coefficients and 95% confidence intervals of month dummies interacted with the pre-COVID (February 2020) share of workers outsourced by the establishment and the indicator for high employment volatility, controlling for date and calendar month dummies and the pre-COVID share of outsourced workers. Outcomes are detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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 effects can be interpreted as deviations away from the outcome mean of the comparison group the month prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2024. Wages are deflated to July 2019 using the intermediate goods sub-index of Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

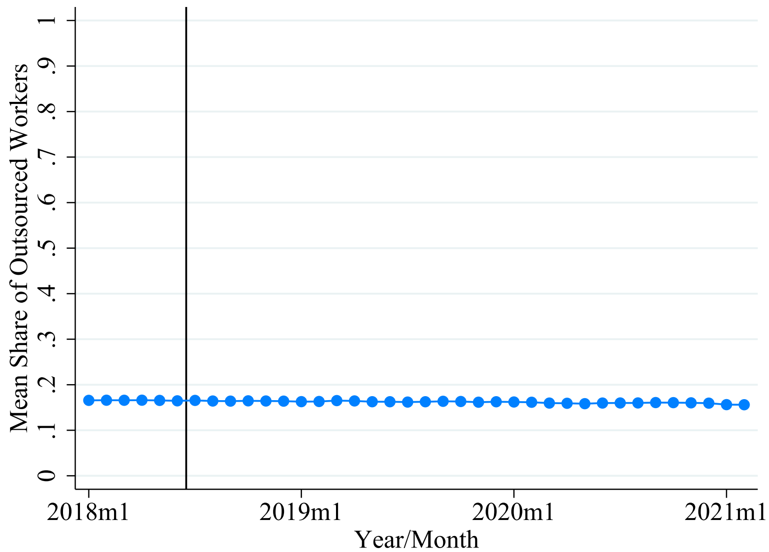
Table D.5: The Impacts of the Reform on Establishment-Level Employment and Wages by Employment Volatility at Baseline

Regressor	Employment (1)	Average Wage (2)
Low Volatility$_i$ × Outsourcing$_{i,\text{February 2020}}$ × Post$_t$	0.01	0.46***
	(0.05)	(0.03)
High Volatility$_i$ × Outsourcing$_{i,\text{February 2020}}$ × Post$_t$	0.02	0.38***
	(0.05)	(0.02)
p-value (H_0: Low Volatility=High Volatility)	.842	.027
N	615,375	615,375
R^2	0.00001	.022

Notes: This figure presents the results from fully interacting our differences-in-differences strategy 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. Effects shown correspond to average impacts for the entire post-reform period. The measure of cross-sectional exposure to the reform is the pre-COVID (February 2020) share of workers outsourced by the establishment. 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. Wages are deflated to July 2019 using the intermediate goods sub-index of Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

D.6 Anticipation Effects

Figure D.11: Mean Share of Outsourced Workers Before and After the Election

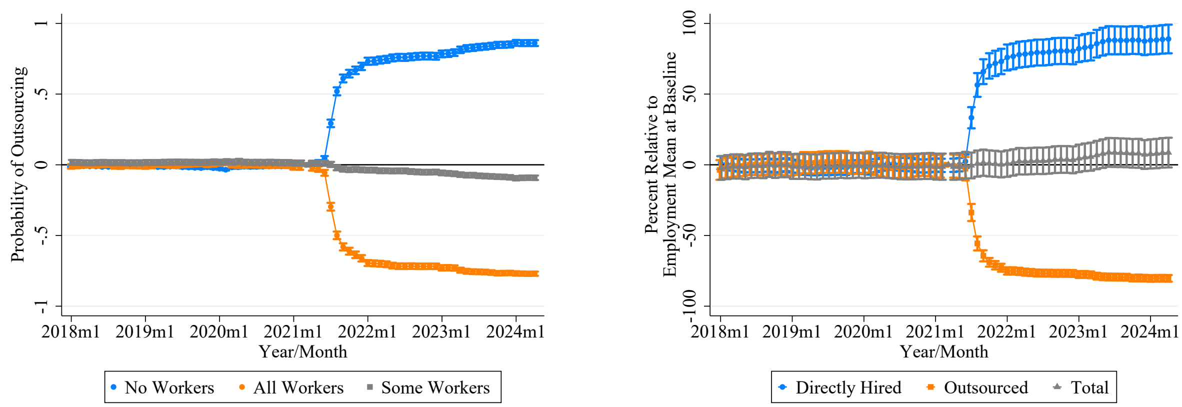


Notes: This figure presents the cross-sectional mean share of outsourced workers at the establishment level. The vertical black line depicts the election of the new government in July 2018.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2021.

Figure D.12: Tests for Pre-Trends in Establishment-Level Outsourcing Prevalence and Employment (Excluding the Ever Treated from the Control Group)

Panel A. Outsourcing Prevalence

Panel B. Employment



Notes: Each panel in this figure presents the regression coefficients and 95% confidence intervals of month dummies interacted with the establishment's pre-COVID (February 2020) share of outsourced workers, controlling for date and calendar month dummies and the pre-COVID share of outsourced workers. The sample used in estimation uses only the establishments that hired directly all their workers before the election as a control group. Outcomes are detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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 effects can be interpreted as deviations away from the outcome mean of the comparison group the month prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2024.

Table D.6: The Impacts of the Reform on Establishment-Level Outsourcing Prevalence and Employment (Excluding the Ever Treated from the Control Group)

		Panel A. Firm Outsourcing	
Regressor	All Workers	Some Workers	No Workers
	(1)	(2)	(3)
$\text{Outsourcing}_{i,\text{February 2020}} \times \text{Post}_t$	-0.77***	-0.09***	0.86***
	(0.01)	(0.01)	(0.01)
N	627,926	627,926	627,926
R^2	.607	.004	.381
		Panel B. Employment	
Regressor	Directly Hired	Outsourced	Total
	(1)	(2)	(3)
$\text{Outsourcing}_{i,\text{February 2020}} \times \text{Post}_t$	0.89***	-0.80***	0.09
	(0.05)	(0.01)	(0.05)
N	627,926	627,926	627,926
R^2	.012	.16	.00006

Notes: Effects shown correspond to impacts in April 2024. The measure of cross-sectional exposure to the reform is the pre-COVID (February 2020) share of outsourced workers. The sample used in estimation uses only establishments that hired directly all their workers before the election as a control group. Effects in Panel B are expressed relative to the cross-sectional employment mean in March 2021, one month prior to the enactment of the reform. Outcomes are detrended to account for group-specific pre-trends and seasonal effects prior to estimation. 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.

D.7 Dose-Specific Average Treatment Effects

In what follows, we focus our attention on estimating the average treatment effect of dose d at time t on the establishment-level outcome Y, denoted by $ATE(d,t) \equiv E[Y_t^d - Y_t^0]$. Before turning 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,[38] it can be shown that $\Delta Y_t - E[\Delta Y_t | d = 0] = ATE(d,t) + u_t$, where u_t is 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 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 D.13 shows that the points in the scatterplot for employment lie close to zero regardless of the exposure level to the reform, whereas 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

$$\Delta Y_{it} = \alpha_t + \sum_{k=1}^{K} \psi_k(d_i)\beta_{kt} + \epsilon_{it}, \tag{D1}$$

where $\Delta Y_{it} = Y_{it} - Y_{it_0}$ is the long 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 non-parametric estimator of $ATE(d,t)$, given by

$$\widehat{ATE}(d,t) = \boldsymbol{\psi}_K(d)'\hat{\boldsymbol{\beta}}_{Kt}, \tag{D2}$$

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

[38] This assumption says that the path of outcomes for lower-dose units must reflect how higher-dose units' outcomes would have changed had they instead experienced the lower dose.

in Equation (D1). This estimator has the desirable property of yielding consistent estimates under the so-called "strong" parallel trends assumption. Furthermore, standard errors for these estimates can easily be obtained using the Delta method.

In Figure D.14, 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.

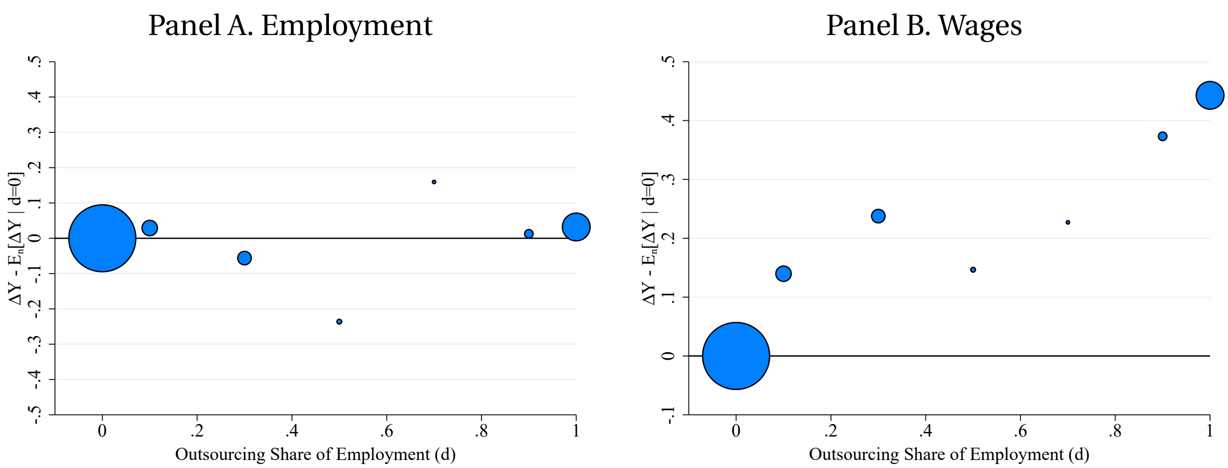
Finally, for each period t, we compute a summary measure of dose-specific average treatment effects using the so-called "binarized" differences-in-differences estimator, which is obtained from the following linear regression specification:

$$\Delta Y_{it} = \alpha_t + \mathbb{1}_{\{d_i>0\}}\beta_t + \varepsilon_{it}, \tag{D3}$$

where ΔY_{it} is defined as before, $\mathbb{1}_{\{d_i>0\}}$ is an indicator for a non-zero outsourcing employment share in March 2021, and ϵ_{it} is an error term. Under the "strong" parallel trends assumption, it can be shown that the OLS estimate $\hat{\beta}_t$ for the slope coefficient of Equation (D3) is a consistent estimate of the average treatment effect of the reform over dose levels, $ATE(t) = E[ATE(d,t)|d>0]$.

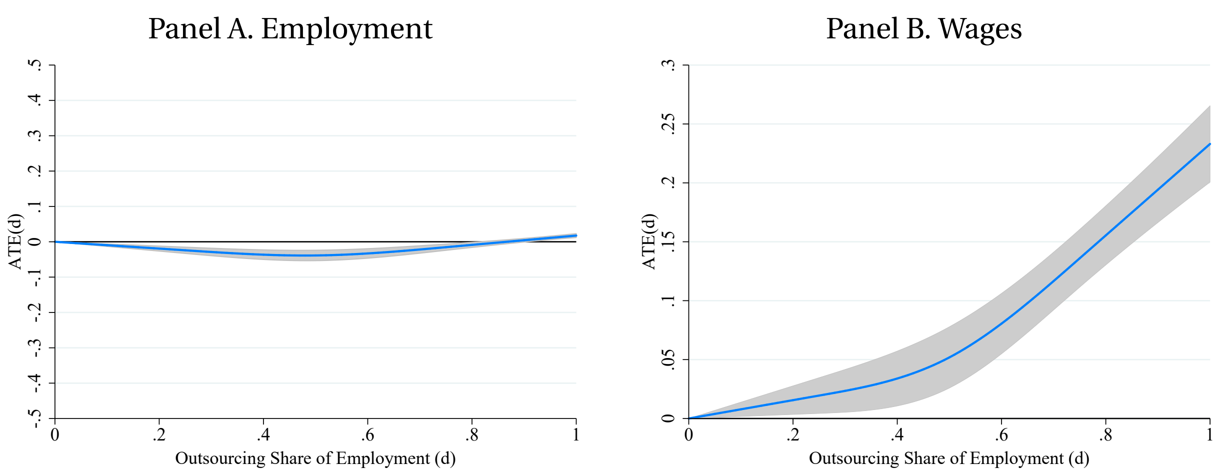
In Figure D.15, we present this summary statistic for all periods in the data to check the plausibility of the parallel trends assumption for employment and wages. We report similar pre- and post-treatment trends in the ATE for both variables as those outlined in the pre-trends checks in the main body of the paper.

Figure D.13: Outcome Changes Two Years After the Reform by the Outsourcing Share of Employment



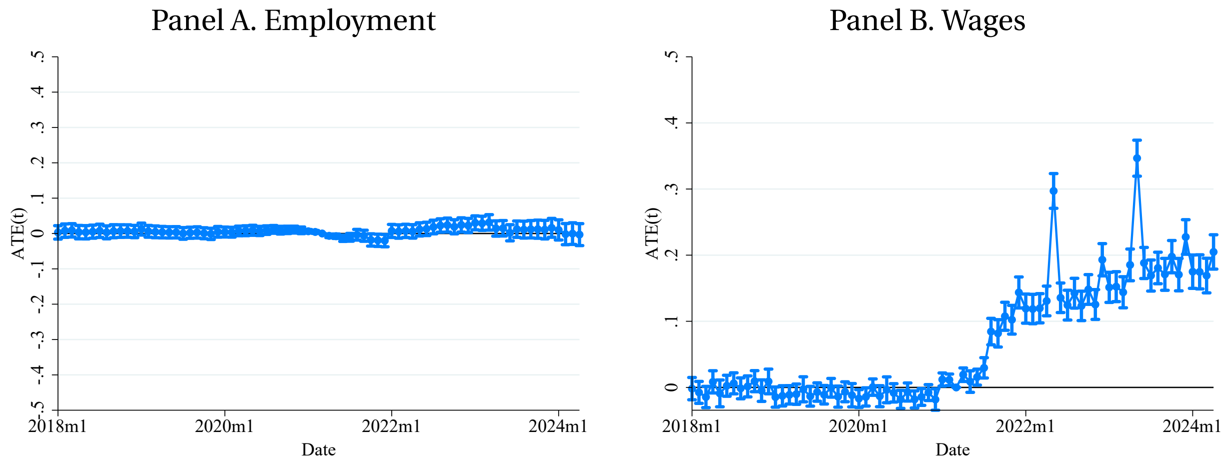
Notes: This figure presents a bin scatterplot of establishment-level outcome changes two years after the reform relative to mean outcome changes experienced by zero-dose establishments. Each bin represents the average across all establishments within a given treatment dose range. Bin range size is 0.1 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. Pre-reform trends are stripped from outcomes prior to estimation, and stripped outcome changes are calculated using May 2023 as end period. 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 effects can be interpreted as deviations away from the outcome mean of the comparison group 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 sub-index of Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Figure D.14: Non-Parametric Estimates of $ATE(d)$



Notes: Each panel in this figure presents the non-parametric estimates and 95% 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. Pre-reform trends are stripped from outcomes prior to estimation. 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 sub-index of Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

Figure D.15: Event-Study Estimates of $ATE(t)$



Notes: Each panel in this figure presents the regression coefficient estimates and 95% confidence intervals of monthly regressions for a different outcome variable. Pre-reform trends are stripped from outcomes prior to estimation. 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 sub-index of Mexico's GDP deflator, or *Índice Nacional de Precios al Productor* (INPP).

E Monopsony Model and Markdown Estimation

This section presents our illustrative models of monopsony through which we interpret results and also the details of our empirical estimation of markdowns.

E.1 Theoretical Framework

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 E.1.1 outlines a common economic environment for both scenarios, featuring directly hired and outsourced workers. Section E.1.2 details the key theoretical predictions of the classical monopsony model. Section E.1.3 explores the key theoretical predictions of the rent sharing model.

E.1.1 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 will assume that a single constant returns to scale staffing firm provides staffing services, while N symmetric diminishing returns to scale producing firms exist.

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 therefore view the wage as encompassing total compensation, inclusive of wages and benefits with the implicity assumption that workers value benefits one-for-one with wages.[39] 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 because of human resource services, which we denote by $a_i < 1$.[40] The assumption that $a_i < 1$ implies that human resource costs are less than the direct cost of paying the worker.

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

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

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

[39] Although benefits (e.g. profit-sharing and pension, insurance funded through payroll taxes) are a key way in which monopsony power is exercised, we abstract from them here in order to focus on the role of monopsony power per se.

[40] 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.[41] 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 and staffing company.

E.1.2 Classical Monopsony

Consider the case of an upward sloping supply curve which the firm internalizes. 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 and given symmetry, $L^- = (N-1)l_i + Nl_o$, of course.) The firm's profit maximization 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:

$$\frac{\partial f}{\partial k} = r. \tag{E1}$$

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

$$\frac{1}{(1 + a_i)\left(w_i(l_i; L_{-i}) + w_i'(l_i; L_{-i}) l_i\right)} \geq \frac{1}{w_o}, \tag{E2}$$

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

[41] The technology for staffing services is constant returns to scale and so optimal firm size is potentially determined only by the aggregate supply of labor with a single employer of labor, e.g., in the case of classical monopsony power. One could add a convex cost of providing staffing services, $a_o(n)$, which might include probabilistic penalties for tax avoidance, for example, in order to avoid this equilibrium outcome, but for simplicity we abstract from that.

such a case, that equality will be reached by the decision of the firm itself, and so that mix of insourced and outsourced labor will be determinate.

Further assuming that the firm faces an isoelastic aggregate supply curve for total labor $L = w^{\eta}$, where η denotes the Frisch elasticity of labor supply, we can rewrite Equation (E2) 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 and is larger, the lower is the elasticity of aggregate labor supply, η, and the greater the size of the firm, l_i^* relative to the total labor market, L.

Likewise, staffing firms also face the same total labor supply, $L = w^{\eta}$, but internalizing only their own contribution to it. That is, the realize $L = L_{-n} + n$ but take L_{-n} as given. Again, their HR cost per unit of labor can differ.[42]

From the profit-maximization problem, the first-order condition with respect to outsourced labor can also be rewritten as a Lerner condition for the wage as a markdown on the marginal product of labor,

$$w_o = \tilde{w}_o \mu_o (1+a_o), \tag{E3}$$

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

We will assume parameter values (i.e., sufficiently large r, x, a_i, a_o and N) ensuring that $n^* > l_i^*$, 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 effectively inelastic labor supply curve, enabling them to exert more market power.

Outsourcing Ban under Classical Monopsony

We examine the impact of the outsourcing ban on total employment, output, wages, and the labor share of the monopsonistic firm. To simplify the exposition, we exclude raw materials and

[42]The technology for staffing services is constant returns to scale and so optimal firm size is determined only by the upward sloping supply of labor. Given the profitability of monopsony power, and the fact that markdowns increase in firm size, the set up would imply a single staffing firm would be the lowest cost. Again, one could easily add a convex cost of providing staffing services, $m(n)$, which might include probabilistic penalties for tax avoidance, for example, in order to avoid this equilibrium outcome, but for simplicity we abstract from that.

assume the production function is Cobb-Douglas in capital and composite labor, as follows:

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

with $\alpha + \tilde{\beta} < 1$. For illustrative purposes, we assume the firm relied solely on outsourced labor before the ban, making it fully exposed to the regulatory change and indeed that all labor was hired by a single monopsonist staffing company. For the following derivations, it will be useful to substitute the firm's capital demand condition as a function of the interest rate and labor demand from Equation (E1) 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, since by spanning the positive range of r, we can attain any positive value of A, we can consider A as a parameter instead of r without loss of generality.

To keep track of total employment at the producing company, 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 1. *For all values of A, a, and m, such that $n = L$ before the ban, there exists a small enough value η^* of the Frisch elasticity of directly supplied 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 (E3) into the first-order condition, applying the labor supply equation for outsourced labor, and rearranging the terms, we derive the pre-reform employment level

$$l_i^{pre} = l_o^* = \left[\frac{\beta A}{N^{1/\eta}(1+a_o)\mu_o} \right]^{\frac{\eta}{\eta(1-\beta)+1}}. \tag{E4}$$

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

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

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

f

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

$$l_i^{post} = l_i^* = \left[\frac{\beta A}{N^{1/\eta}(1+a_i)\mu_i} \right]^{\frac{\eta}{\eta(1-\beta)+1}}. \tag{E5}$$

with $\mu_i = 1 + \frac{1}{\eta}\frac{1}{N}$. Now we can clearly show

$$\frac{l^{post}}{l^{pre}} = \left[\frac{(1+a_i)\mu_i}{(1+a_o)\mu_o} \right]^{\frac{\eta}{\eta(1-\beta)+1}}. \tag{E6}$$

Given $a_i, a_o < 1$ this ratio is bounded below by $1/2$ as $\eta \to \infty$y, but exceeds $N/2 > 1$ as $eta \to 0$. Essentially, the gains from the extra monopsony power of $N > 1$, which depend on both the labor supply elasticity and the relative size of the staffing firm, N, must exceed any efficiency gains in human resource costs.

From the labor supply equation, however, it is clear that the wage is increasing the labor demand ratio:

$$\frac{w^{post}}{w^{pre}} = \left[\frac{N l^{post}}{N l^{post}} \right]^{\frac{1}{\eta}}. \tag{E7}$$

so that wages increase under the same conditions that labor does.

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

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

$$s_L^{post} = s_{l_i} = \frac{\tilde{w}_i l_i}{k^\alpha l_i^{1-\alpha}} = \frac{1-\alpha}{(1+a_i)\mu_i},$$

Again, the ratio follows the same ratio as the wage ratio:

$$\frac{s_L^{post}}{s_L^{pre}} = \left[\frac{(1+a_i)\mu_i}{(1+a_o)\mu_o}\right]. \tag{E8}$$

Hence, shares move the same direction as wages. □

Proposition 1 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.

E.1.3 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.

Model Setup Under Internal Hiring

As is standard in this literature, we assume that workers value monetary payoffs, either wage, w, or an outside option, denoted by b.

To simplify our exposition, we again drop raw materials from the production function. Consequently, the profits of the producing firm are equal to

$$\Pi(w_i) = f(k, l_i) - w_i(1+a_i)l_i - rk. \tag{E9}$$

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 $S = u(w_i) - u^0 + \Pi(w_i) - \Pi^0$. We assume it is shared according to the Nash product

$$\max_{w_i - b, \Pi(w_i)-\Pi^0} (w_i - b)_i^{\phi}(\Pi(w_i) - \Pi^0)^{1-\phi_i},$$
$$\text{s.t. } S = w_i - b + \Pi(w_i) - \Pi^0,$$

with solution

$$w_i - b = \phi_i S \text{ and } \Pi(w_i) - \Pi^0 = (1-\phi_i)S. \tag{E10}$$

The Holdup Problem

To obtain the firm's demand for capital, we first substitute(E9) and the definition of Π^0 into the definition of S to get

$$S = f(k, l_i) - b(1+a_o)l_i - \delta rk. \tag{E11}$$

Then, we use (E10) to obtain the following expression for the wage:

$$w_i = b + \phi_i \frac{S}{l_i}. \tag{E12}$$

Finally, we substitute Equations (E9), (E11), and (E12) and the definition of Π^0 into Equation (E10) to obtain the following expression for the profit function of the firm under direct hiring:

$$\Pi^{\text{Direct Hiring}} = (1-\phi_i)[f(k,l_i) - bl_i] - (1-\phi_i\delta)rk. \tag{E13}$$

Proposition 2. *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 (E13) 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 2 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. (The same result would carry over to materials.) However, if they bargain over the surplus before deducting the cost of capital, the holdup problem will lead to under-investment since the firm is not the full residual claimant of the additional returns it generates through investment. To see why, note that the wage expression in Equation (E12) is increasing in the firm's capital stock.

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.

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 companies, 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, $phi_0 = 0$ 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

$$\Pi^{\text{Outsourcing}} = f(k, l_o) - b(1 + a_o)l_o - rk. \tag{E14}$$

Proposition 3. *Outsourcing leads to optimal capital investment (i.e., $f_k = r$).*

Proof. By backward induction, Equation (E14) 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 3 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 2 and 3 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 in the beginning, 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 our analyses.

E.2 Markdown Estimation Details

This section provides further details of our markdown estimation procedures. Section E.2.1 follows the standard cost minimization procedure to derive the formulas to construct markdowns using revenue elasticities and revenue shares. Section E.2.2 provides the details of the production function estimation.

E.2.1 Deriving an Expression for Markdowns

As described above, the wage 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 wage 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 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:

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

with the associated Lagrangian function

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

where w_{it}, r_{it}, p_{it}^M, and p_{it}^E 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 $\mu_{it} \equiv \frac{P_{it}}{\frac{\partial \mathscr{C}(Q_{it}, K_{it}, w_{it}, r_{it}, p_{it}^M, p_{it}^E, \Omega_{it})}{\partial Q_{it}}}$.[43] First, by the envelope theorem, we have that the Lagrangian multiplier is the marginal cost of production, or $\lambda_{it} = \frac{\partial \mathscr{C}(Q_{it}, K_{it}, w_{it}, r_{it}, p_{it}^M, p_{it}^E, \Omega_{it})}{\partial Q_{it}}$. Second, the first-order condition for raw materials is

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

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

$$\mu_{it} = \frac{\theta_{it}^M}{\alpha_{it}^M}, \tag{E15}$$

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 wage markdown, defined as the ratio of the

[43] 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.

marginal revenue product of labor to the wage rate, or $\upsilon_{it} \equiv \frac{\frac{\partial P_{it}Q_{it}}{\partial L_{it}}}{w_{it}}$.[44] 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

$$\left(1 + \frac{1}{\varepsilon_{it}^{L,w}}\right) = \frac{\frac{\theta_{it}^{L}}{\alpha_{it}^{L}}}{\mu_{it}}. \tag{E16}$$

Thus, a sufficient condition for the desired result to hold is the equality of the establishment's wage 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 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

$$\frac{\frac{\partial P_{it}Q_{it}}{\partial L_{it}}}{w_{it}} = \left(1 + \frac{1}{\varepsilon_{it}^{L}}\right) = \upsilon_{it}, \tag{E17}$$

[44] 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 wage rate is 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.

where the last equality follows from the markdown definition.

Substituting the last equality in Equation (E17) into Equation (E16), we finally obtain

$$\upsilon_{it} = \frac{\frac{\theta^L_{it}}{\alpha^L_{it}}}{\mu_{it}}. \tag{E18}$$

The right-hand side of this equation identifies the labor markdown, as Equation (E15) identifies the markup of the establishment. The wage 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.

E.2.2 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

$$y_{it} = f(l_{it}, k_{it}, m_{it}, e_{it}) + \omega_{it} + \varepsilon_{it}, \tag{E19}$$

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

$$\omega_{it} = g_t(\omega_{i,t-1}) + \zeta_{it}, \tag{E20}$$

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

Substituting Equation (E20) into Equation (E19), we have

$$y_{it} = f(l_{it}, k_{it}, m_{it}, e_{it}) + g_t(\omega_{i,t-1}) + \zeta_{it} + \varepsilon_{it}. \tag{E21}$$

Note that, by Equation (E20), $\omega_{i,t-1}$ is mean independent from period t's input choices, so the only problematic source of endogeneity in the estimation of f in Equation (E21) is the productivity innovation ζ_{it}. However, the 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:

$$E[\zeta_{it} \times \mathbf{Z}_{it}] = \mathbf{0}, \tag{E22}$$

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 (E22) 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 provided that the candidate instruments meet the so-called relevance condition. This condition requires that the establishment's input choices are auto-correlated. 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 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 $\hat{\omega}_{i,t-1}(\boldsymbol{\beta})$. 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:

$$E\left[\zeta_{it}(\boldsymbol{\beta}) \times \mathbf{Z}_{it}\right] = 0. \tag{E23}$$

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.

E.2.3 Markdown Estimation

The GMM estimator of the parameter vector $\boldsymbol{\beta}$, denoted by $\hat{\boldsymbol{\beta}}$, allows us to calculate the output elasticities with respect to raw materials and labor. If 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, thereby implying that markdown trajectories within an industry mirror those of the ratios of the expenditure share of raw materials to the expenditure share of labor.

In contrast, under our baseline assumption that the production function is translog, output elasticities depend on the establishment's input choices and are given by

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

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

Finally, we compute revenue shares of raw materials and labor. As in de Loecker and Warzynski (2012), we use the estimated residual from the first step in our estimation procedure, 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.