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OUTSOURCING POLICY AND WORKER OUTCOMES:
CAUSAL EVIDENCE FROM A MEXICAN BAN

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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. Then, leveraging data from a manufacturing panel survey from 2013 to 2023 and a natural experiment featuring a ban on domestic outsourcing in 2021, we show that the ban drastically reduced outsourcing, increased wages, and reduced measured markdowns without lowering output or employment. Consistent with the presence of monopsony power, we observe large markdowns for the largest firms, with the decline in markdowns in response to the ban concentrated among high-markdown firms. However, we also find that the reform reduced capital investment and increased the probability of market exit.

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A data appendix is available at http://www.nber.org/data-appendix/w32024

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 subcontracting schemes, which have been shown to reduce wages (Goldschmidt and Schmieder, 2017) and undermine worker protections (Autor, 2003). In theory, however, these 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 that combined a ban on outsourcing with provisions to monitor and enforce the ban. Using a differences-in-differences strategy with longitudinal establishment-level data, we find that the policy did indeed reduce 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. We rationalize these results as reflecting a reduction in firm monopsony power, consistent with the decline in measured markdowns that we observe. While our findings indicate that the reform was effective in improving worker outcomes, we also find that it reduced capital investment and increased the probability of market exit.

Although institutional arrangements featuring prevalent outsourcing, mandated social security, and mandated profit sharing are common in many countries, Mexico is a particularly interesting and informative case to study. First, Mexico showed high and persistent use of outsourcing prior to the reform. We document a rising prevalence of outsourcing in the manufacturing sector, with outsourcing as a share of manufacturing workers tripling from 7 percent to 21 percent between 2000 and 2021, the year of the reform. Second, the context of the

[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.

reform offers quasi-experimental variation: the policy led to a precipitous drop in 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 enabling us to document pre-existing patterns, estimate establishment markdowns, and examine establishment responses. Given prior legislation, the data contain explicit measures of outsourced labor, defined as on-site workers for whom the employing establishment sets employment responsibilities but no formal employer–employee relationship exists. 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.

We leverage data from a panel manufacturing survey from 2013 to 2023 and the 2021 policy prohibiting outsourcing to quantify the causal impacts of the policy. The absence of pretrends and the presence of clearly identifiable seasonal payments mandated by policy in the monthly data give us confidence that our differences-in-differences strategy is appropriate for identification of the causal effect.[2] Owing to an effect of 10 percent on wages, we find that the reform reduced markdowns by 27 percent, as measured by our preferred empirical specification. Moreover, we find no impacts on the establishment's employment, use of other productive inputs, or output.

Finally, the relative stability in employment, other inputs, and output accompanied by an increase in wages implies a decrease in the ratio of the marginal product of labor to the wage, i.e., the labor markdown. We indeed find a drop in measured labor markdowns, as measured by standard approaches from the literature (Brooks et al., 2021*b*; Yeh, Macaluso and Hershbein, 2022), among previously outsourcing firms. We conjecture that the increased wages 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. We start by documenting that markdowns were sizable and stable before the reform and highest among large firms and those that are sizable

[2]Specifically, we find a 70 percent 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 of 62 percent precisely in May of each year, the month in which Mexican legislation mandates dividend dispersal.

relative to their labor market, consistent with their size reflecting monopsony power. Returning to the impact of the reform, 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. However, we report an increase of 1 percentage point in the probability of market exit and a reduction in capital investment of 2.8 percent, but no reduction in dividends paid. These negative impacts are quantitatively small relative to the reduction in outsourcing and wage gains, however.

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 and Mongey, 2022*a*,*b*; Berger et al., 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, implying that labor market interventions intended to protect workers' rights can have unintended negative consequences.[3] Our empirical analysis advances the counterargument: while they can have unintended consequences, worker protections can also have the *intended* consequence of reducing exploitation. This finding resonates 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 (Ap-

[3] In a similar vein, Felix and Wong (2021) show favorable effects of a reform legalizing outsourcing on the employment of guards in Brazil.

pelbaum, 2017), and increases in wage inequality (Bilal and Lhuillier, 2021; Goldschmidt and Schmieder, 2017). Similar to Bilal and Lhuillier's (2021) results for France, our evidence is consistent with outsourcing having negative impacts on workers. Our use of a legal ban as a source of quasi-experimental variation is unique, as is our focus on a developing economy, where the consequences for workers may be more dire since wages and labor protections are lower than in developed economies.

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 4 outlines the differences-in-differences strategy used to measure the causal impacts of the outsourcing ban and reports its effects on employment, wages, markdowns, factor substitution, investment, and market exit. Section 5 presents stylized empirical facts regarding the correlation between establishment size, markdowns, and outsourcing. Section 6 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.

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.[4] In this system, a formal firm

[4] 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,

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.[5] 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.[6] Although commonplace, outsourcing is usually difficult to measure and therefore study directly. Given the 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 core economic

France, Japan, Switzerland, and Israel (Tulchinsky, 2018). For a detailed description of other Latin American social insurance models, see Frölich et al. (2014).

[5]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.

[6]On Mexico, see Brito Laredo et al. (2022), Franco et al. (2020), and Velarde, Mueller and García (2021). Figure 1 in OECD (2021) shows that long-run outsourcing growth over the past 20+ years has been common across all OECD countries.

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 core economic activities, as defined by the establishment's NAICS code, such as workers engaged in cleaning, catering, security, and gardening. For clarity, we refer to firms supplying the workers who conduct these noncore activities as specialized subcontractors, and we exclude them from our analyses.

Domestic outsourcing manifested in two main practices before the reform: insourcing and third-party outsourcing. 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[7] 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 tax burden. The employing firm contracts a third party to hire core workers and manage the employing firm's payroll, with the aim of deducting payroll costs from its value-added tax (VAT) bill and minimize the tax burden associated with giving workers access to public healthcare. To do so, 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 increasing function of the workers' registered average daily wage, not their total income. Workers do not receive the mandated employment benefits (e.g., retirement pensions) under third-party outsourcing, however. 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 com-

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

pany 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 show that domestic outsourcing helped employing firms avoid profit sharing and evade social security contributions and benefit payments.[8] 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 describing the main characteristics of staffing establishments in Figure 1. Panel A shows that the employee count distribution of staffing establishments is bimodal, with substantial mass at its right tail, implying that staffing establishments tend to employ large amounts of workers.[9] Panel B then shows that revenue in staffing establishments is almost entirely distributed between profits and labor payments, with a negligible fraction going to other productive inputs, indicating that these establishments indeed corresponded to shell companies that owned very few assets.

Next, in Table 1, we compare the mean labor payment shares of nonsalary payments for three establishment types: staffing establishments, 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,

[8]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.

[9]The corresponding distribution for manufacturing, depicted in Panel A of Figure B.1, has less mass at the right.

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.[10]

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 2. The figure combines data from 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.[11] The reform com-

[10]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).

[11]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*.

prised 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 for the transfer of outsourced workers employed on firms' premises within a 3-month grace period, stipulating that the regularization of workers should conclude by August 2021.

3 Data Sources and Measurement

In this section, we describe our data sources, definitions, and constructed measures (especially the process for constructing markdowns). 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.

3.1 Data Sources

To maximize completeness of coverage, length of the time series, data richness,[12] and data frequency, we utilize confidential establishment-level data from multiple sources: economic censuses, annual manufacturing surveys, and monthly manufacturing surveys. 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.[13] 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 pri-

[12] A key advantage of our data over data from the social security authority (the IMSS) is its comprehensiveness in registering all employment and labor payments at the establishment level. The IMSS data omit employment through shell staffing companies and register only the average monthly wages reported to the social security authority while excluding all remuneration types not subject to social security contributions. Both shortcomings make IMSS data inadequate for measuring the ban's effects on employment or wages correctly, although they remain suitable for measuring the effects on formal employment and social security payments. We use matched hirer–employee data from the IMSS in Appendix A.2 to test the reform's impacts on both of these outcomes.

[13] 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.

marily 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 for staffing establishments 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 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 come from Mexico's annual manufacturing survey. We analyze the period from 2013 to 2023. 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. An important caveat of this survey is that 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 by payment category.

Monthly Manufacturing Survey. To measure the causal impacts of the outsourcing reform by payment category, we complement the aforementioned annual survey data with data from Mexico's monthly manufacturing survey. This survey gathers information from the same panel of 10,447 establishments as the annual survey, spanning the same 239 six-digit NAICS industry codes. As with the annual survey, we analyze the period from 2013 to 2023. 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.

3.2 Measuring Markdowns

Markdowns are defined as the ratio of the value of the marginal product of labor to the wage. Since the former is not measured explicitly, standard methods for obtaining markdown estimates are indirect. Since these estimators are not our innovation, 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*[14]:

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

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 knowledge 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–

[14]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.

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.

4 Causal Impact Estimates of the Outsourcing Reform

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 postreform indicator, reflecting the subsequent collapse in outsourcing (recall Figure 2).[15]

We apply 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, employment, wages, markdowns, other input usage, and output in Section 4.2. Finally, we present the reform's impacts on each component of wages for insourced employees in Section 4.3.

[15]We illustrate both sources of variation in more detail in Appendix A.1. Figure A.3 shows that, conditional on outsourcing, there is substantial variation in the share of outsourced workers at the establishment level at baseline. Figure A.4 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.

4.1 Empirical Strategy

To recover the causal impact of the outsourcing reform on the outcome of interest 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)[16]:

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

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 preshock period available in the data; B is the last postshock 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 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 pretrends 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.

[16] We run our estimations on data that have been detrended by outsourcing-specific, pretreatment time trends. Concretely, we use preintervention data to regress the outcome of interest on the establishment's outsourcing employment share corresponding to the period prior to the reform, interacted with a linear time trend, controlling for time dummies and the uninteracted outsourcing employment share. We predict postintervention outcomes using the resulting coefficients from this regression and subtract the prediction from the outcome of interest.

We implicitly assume that establishments did not respond to the news of a potential change in regulation before the passing and enactment of the outsourcing reform. In Appendix D.1, we test the robustness of our results to our relaxing this assumption by constructing an exposure variable that leverages cross-sectional variation in the outsourced employment share in 2018, the year in which the new government came into power.

Recent contributions advancing the differences-in-differences literature cautioned authors against trusting parameters associated with popular two-way fixed-effect specifications when the treatment status is continuous, as the parameters can be hard to interpret (Callaway, Goodman-Bacon and Sant'Anna, 2021). Instead, these works propose that average causal responses be identified through comparisons of the evolution of the outcome variable between lower-dose and higher-dose units.[17] Thus, as a robustness check of our main results, Appendix D.2 quantifies the reform's impacts using a design that relies on a discrete dose measure. Specifically, we compare the before–after change in the outcomes of interest between establishments with a prereform outsourcing employment share above the cross-sectional median against the change for those with an outsourcing employment share below the median.

4.2 Markdown Impact Estimates

Figure 3 plots the coefficient estimates for Equation (1), calculated from annual manufacturing survey data. Visual inspection of the coefficients leads us to soundly reject the presence of differential pretrends for all of the outcome variables of interest, lending support to the parallel trends assumption necessary for the identification of causal impacts.

Table 2 reports estimates of the causal impact of the outsourcing reform. Panel A begins by presenting the impacts on the prevalence of outsourcing practices. For a fully exposed establishment, the reform lowered the probability of outsourcing all employment in the establishment by 73 percentage points (p=0.000). This reduction corresponds to increases of 57 (p=0.000) and 16 (p=0.000) percentage points in the probability of using outsourcing for some but not all employees and the probability of not using outsourcing for any employees, respec-

[17] The validity of the resulting estimates depends on a stronger version of the parallel trends assumption requiring that the average change in outcomes across all units, had they been assigned a particular dose, would be the same as the average change in the outcome of interest for the units that effectively experienced that dose.

tively. These findings indicate that the reform was effective in reducing outsourcing.

Next, Panel B reports the impacts of the reform on employment. The table reveals a strongly significant impact on insourced employment, amounting to an average increase of 62 percent for an establishment that outsourced all of its workers prior to the reform, relative to the total employment mean across all establishments in 2020 (p=0.000). This increase corresponds to an average drop of 53 percent in the count of outsourced workers relative to the prereform mean of total employment across all establishments (p=0.000). The sum of these two effects gives rise to a point estimate impact of 9 percent on overall employment, but the estimate is only marginally significant (p=0.083).

Next, Panel C turns to investigating the impacts of the reform on the usage of other productive inputs and output. We fail to find evidence of an effect on the establishment's usage of capital, raw materials, and energy, or evidence of an impact on output. Thus, there is no evidence of a substitution effect operating against labor or a reduction in production as a whole as a consequence of the reform. Thus, outsourced workers moved into direct employment without a reduction in overall employment or output or any substitution toward other inputs.

We examine the impacts on average wages at the establishment level in Panel D by looking at the type of compensation per worker. The average annual wage increases by 10 percent on average for establishments that outsourced all of their workers prior to the reform (p=0.000) relative to the mean wage across all establishments the year prior to the reform. To dissect the mechanism underlying this increase, we decompose it into two components: increases in the wage bill of insourced employees and decreases in the wage bill of outsourced employees. We find that the reform raised the ratio of the wage bill of insourced employees to the total employee count by 51 percent on average relative to the prereform mean wage across all establishments (p=0.000). In contrast, the reform lowered the ratio of the wage bill of outsourced employees to the total employment count by 41 percent relative to the prereform mean wage (p=0.000). From the standpoint of competitive price-taking labor demand, the increase in wages with no reduction (and perhaps an increase) in employment is a puzzle.

We therefore turn in Panel E to examining the impacts on wage markdowns. Our estimation sample includes only observations for which a data lag is available, which is necessary for markdown estimation. Nevertheless, we report a strongly significant reduction of 27 percent relative to the mean wage markdown at baseline (p=0.000), estimated with a translog assumption, for establishments that outsourced all their workers prior to the reform. This markdown reduction is equivalent to a 21 percent increase in the wage share of the marginal revenue product of labor relative to its prereform mean.

The table shows estimates of similar magnitude and statistical significance for markdowns estimated under the alternative assumptions that the production function is translog with constant returns to scale, that the production function is Cobb–Douglas, and that impacts on the log of the ratio of the revenue share of materials to the revenue share of labor capture markdown impacts. Based on the standard interpretation of these measures of markdowns, the findings indicate that the reform was successful in reducing monopsony power and increasing workers' rents.

4.3 Impacts by Wage Component

This section examines the impacts on wages in more depth by applying our differences-in-differences strategy to monthly manufacturing survey data. Beyond allowing us to obtain higher-frequency impact estimates and benefit from longer time series, this analysis of high-frequency impacts allows us to leverage the information available in the monthly survey but not in the annual survey. In particular, the monthly survey data decompose the wages of in-sourced workers into four components: salaries, benefits, social security payments, and profit sharing. These disaggregated data elucidate the channels underlying the positive impacts of the reform on average wages, which not only strengthens our confidence in the measurement but also can provide information that matters for worker welfare. If the wage increase caused by the reform is solely explained by a rise in salaries, the rent 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, the rent gains for workers likely include an improvement in insurance values.[18] Moreover, if the wage increase is partly explained by profit sharing, the rent gains for workers include a higher option value of employment with the establishment.

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

Before presenting the reform's impacts on the wage components of insourced employment, we present the estimates with respect to the establishment's outsourcing practices, employment, and wages. This exercise confirms that the impacts calculated from the monthly data are commensurate with those estimated from the annual data in the previous section. Furthermore, this exercise sheds light on the time it takes for impacts to materialize. As before, we begin by plotting the differential effect of exposure to the reform on the outcomes of interest in Figure 4. We find no evidence of differential trends in any of the outcomes of interest prior to the reform, lending support to the parallel trends assumption necessary for the identification of causal impacts. Moreover, we find that the impact magnitudes estimated from the monthly data are consistent with the estimates from the previous section across all variables and that the bulk of the impacts take less than one year to materialize. Finally, in Panel C, we see the periodic spikes in monthly payments to insourced labor (and total labor) postreform. 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), respectively.[19] The corresponding spikes in Panel D indicate an increase in these periodic payments and constitute smoking-gun evidence that these series reflect the impact of the reform itself.

Table 3 presents our quantitative estimates of the reform's impacts for fully exposed establishments (i.e., those outsourcing all of their employment one month prior to the reform, in March 2021) after 12 months. Panel A shows that the reform increased the probability of not employing any workers through outsourcing by 81 percentage points (p=0.000). This increase is explained by a 75-percentage-point reduction in the probability of employing all workers in the establishment through outsourcing (p=0.000) and a smaller reduction of 6 percentage points in the probability of employing at least one worker but not all workers through outsourcing (p=0.000). These findings reveal that our usage of annual data to measure the impacts on outsourcing practices obscures the within-year impacts of the reform. The difference between the annual and within-year impacts is starkest for outsourcing practices since some establishments employed outsourced workers in the three months prior to the reform but stopped using outsourcing schemes to hire employees after the reform went into effect.

[19] Profit-sharing payments occur in May because the law mandates dividend dispersal in May of each year. The Christmas payment is termed the *aguinaldo*, an additional salary equivalent to 15 days' pay distributed by law by all firms to their directly hired employees.

Panel B reports the average employment impacts. The reform caused the number of insourced workers to increase by 81 percent for fully exposed establishments in the year preceding the reform relative to the mean employment level across all establishments at baseline (p=0.000). This increase closely matches the reduction of 78 percent in the number of outsourced workers relative to the prereform mean of employment across all establishments at baseline (p=0.000). Correspondingly, we report an increase of only 3 percent in overall employment, which is somewhat commensurate with the 9 percent increase in the annual data and is statistically insignificant (p=0.482). However, both data series confirm no reduction in employment.

Panel C of the figure examines the reform's impacts on wages. In line with our findings in the previous section, the table shows that the reform increased the average wage, defined as the ratio of the total wage bill to the number of workers, by 35 percent relative to the prereform mean total wage compensation across all establishments (p=0.000). (This larger estimate reflects the longer postreform period in the monthly data series, while the 10 percent estimate from the annual data captures only the first year of the transition.) This increase is explained by a surge of 117 percent in the ratio of wage payments for insourced employees to the total employee count (p=0.000), which overshadows the reduction of 82 percent (p=0.000) in the corresponding ratio for outsourced workers.

Finally, Panel D presents the reform's impacts on salaries, benefits, social security, and profit sharing after 12 months. We find that the reform increased average salaries by 87 percent relative to the prereform total wage compensation across all establishments (p=0.000). This impact estimate omits the additional boost to salaries from the Christmas payment occurring in December of each year.

Similarly, the reform increased social security payments and benefits by 30 percent relative to the prereform mean total wage compensation across all establishments (p=0.000). Finally, the reform increased profit sharing by 62 percent relative to the prereform mean total wage compensation 13 months after the reform (p=0.000).

In sum, we find clearly attributed increases in labor payments without corresponding reductions in employment and measured decreases in markdowns, which we interpret as increased rents for workers. These gains from the reform stem from improvements in take-home pay and in the insurance and option value of employment. While the impact of a reduction in outsourcing on average markdowns on its own may be viewed as suggestive, in the next section, we provide additional evidence for our interpretation of this as a reduction in monopsony power and a shift in rents from firms to workers.

5 Additional Evidence of Firm Monopsony Power

This section presents more evidence that monopsony power was high and pervasive prior to the reform—especially among large firms and firms that outsourced—and that the reform disproportionately impacted firms with the highest baseline monopsony power. The analysis focuses on the annual economic census data.

5.1 Pervasive Baseline Monopsony Power

Table 4 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.2 presents heatmaps of markdowns by state, demonstrating these stronger regional markdowns at a disaggregated geographical level.

5.2 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.[20] 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.[21]

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 5 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. [22]

5.3 Markdowns and Outsourcing

We next show that markdowns and the use of outsourcing are closely correlated at the establishment level.

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

[20] 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.

[21] 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 robustness, in Appendix D.3, 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.

[22] 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.4, 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.5 presents a graph similar to Figure 5 but partitioning the range of establishment shares of total revenue in the local labor market into equally sized intervals in the $(0, 1)$ range.

establishment fixed effects and year dummies. Table 5 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.[23] 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 materials to the revenue share of labor, as suggested in Bond et al. (2021).

5.4 Heterogeneous Impacts of the Ban

Having established the patterns in markdowns in the baseline prereform data, we now analyze the differential effects of the reform on the establishments in the annual manufacturing survey by their prereform markdown levels. If the reform lowered markdowns at the bottom of the baseline distribution, our findings from the previous sections would be consistent with a perverse effect whereby the labor costs of establishments that paid fair wages before the policy change were exacerbated. Conversely, if the impacts occurred at the top of the baseline distribution, our findings would be consistent with the policy change having successfully reduced labor exploitation. We first examine the effect heterogeneity by partitioning our estimation sample into quartiles based on the establishment markdown distribution for 2020 and estimate a fully saturated version of our differences-in-differences specification, interacting each regression term with a full set of categorical dummies for establishment quartiles. Figure 6 reports the reform's impact on markdowns in 2021 for each baseline markdown quartile and for our four alternative markdown measures. Markdowns drop in the establishments with the highest ini-

[23]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 4.

tial markdowns (those above the cross-sectional 75th baseline percentile), indicating that the policy change reduced labor exploitation.

Next, Table 6 examines the effect heterogeneity 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 *all* of the impact of markdowns is concentrated among the top-markdown quartile of firms at baseline. We find marginally significant effect heterogeneity among establishments operating in high-markdown industries and those in the central and south regions, which had somewhat higher markdowns. In contrast, we find no statistically significant effects operating adversely against foreign-owned establishments or those in urban locations.

5.5 Establishment Responses to the Drop in Markdowns

Given that we observe no impacts on input usage or revenue, competitive firms making zero economic profits could respond to the increase in labor costs by exiting the market, while profitable establishments could respond by altering the allocation of profits, after deducting the share dispersed to workers, into investment and dividend payments. A drop in dividend payments would indicate that the reduction in labor exploitation occurred at the expense of capital owners. In contrast, an investment drop would correspond to a slowdown in capital accumulation. Alternatively, although we find no impacts on employment in the short run, higher labor costs could lead to increased investment, with a desired substitution away from labor and perhaps longer-run decreases in employment.

To formally test these mechanisms, we apply our differences-in-differences strategy to three additional outcomes constructed with data from the annual manufacturing survey: establishment exit, as captured by a panel attrition indicator; capital investment, calculated as the difference between the capital stock in the current period and the undepreciated capital stock in the previous period; and dividend payments, estimated as the remaining value added after all expenses, including the profits shared with workers. To ensure that our identifying assumptions hold, Figure 7 demonstrates the absence of differential pretrends in these outcomes. Table 7 then reports our results. Column (1) shows a significant impact of 1 percentage point on the probability of exiting the market in the year after the reform (p=0.000). Column (2) reports an investment drop of 2.8 percent relative to its mean in the year prior to the reform (p=0.000). In contrast, Column (3) reports no statistically significant impact on dividend payments. Thus, establishments responded to the increase in labor cost by exiting the market or reducing capital investment instead of tapping into dividend payments to cover additional labor expenses.[24] Again, with outsourcing concentrated among firms with high markdowns and market power, the impacts on exit and investment are quantitatively small relative to the earlier impacts on outsourcing and wages.

6 Conclusion

This paper examined the causal impact of a reform prohibiting domestic outsourcing on employment, 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, 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. 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

[24] In Appendix A.3, 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.

from changes in the organization of production after liberalizing trade. One caveat is that our study focused exclusively on the short-term impacts of the reform that we study. Future research should examine the long-term effects of labor policy and the welfare and distributional effects implied by labor policy designed to protect workers' rights.

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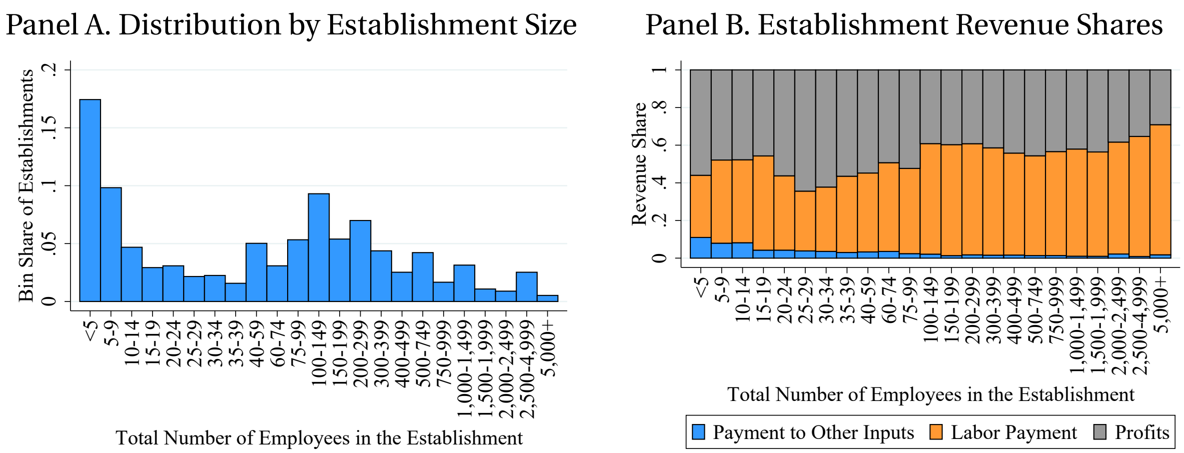
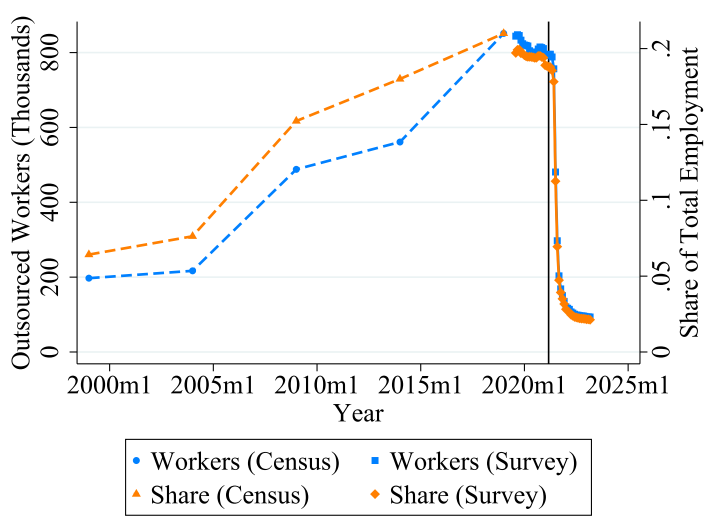
Figures

Figure 1: Characteristics of Staffing Establishments, 2019



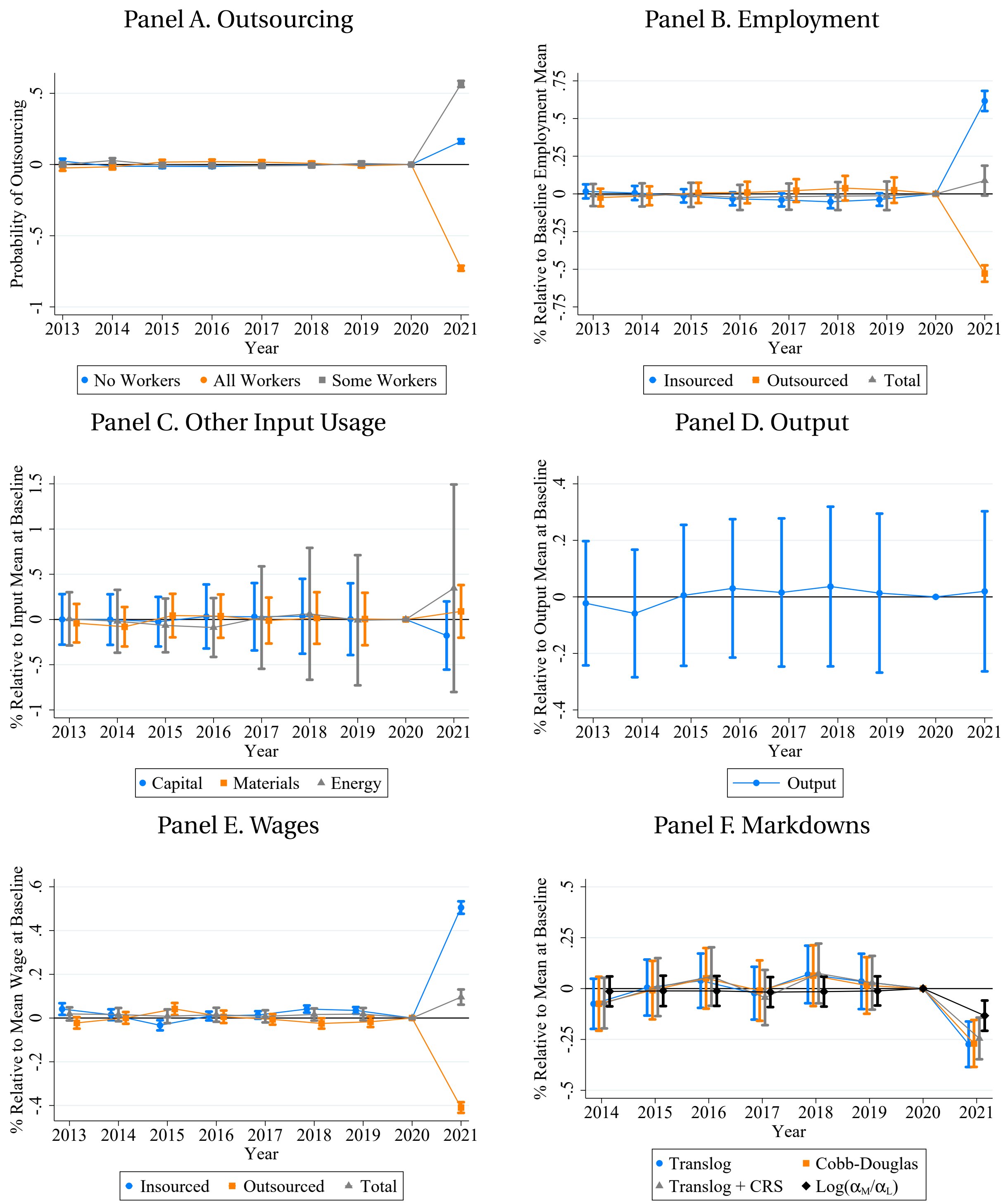
Notes: This figure presents the size distribution and revenue shares for the universe of staffing establishments identified in the economic census. Panel B constructs profits using value-added and excludes expenses unrelated to capital, energy, materials, and labor from the calculation of revenue shares. Staffing establishments are identified as those supplying nonspecialized workers (i.e., workers providing services other than gardening, catering, security, cleaning, and other specialized services) to other establishments.
Source: Authors' elaboration using data from the 2019 wave of the Mexican economic census.

Figure 2: Outsourcing Growth in the Manufacturing Sector and Regulatory Clampdown



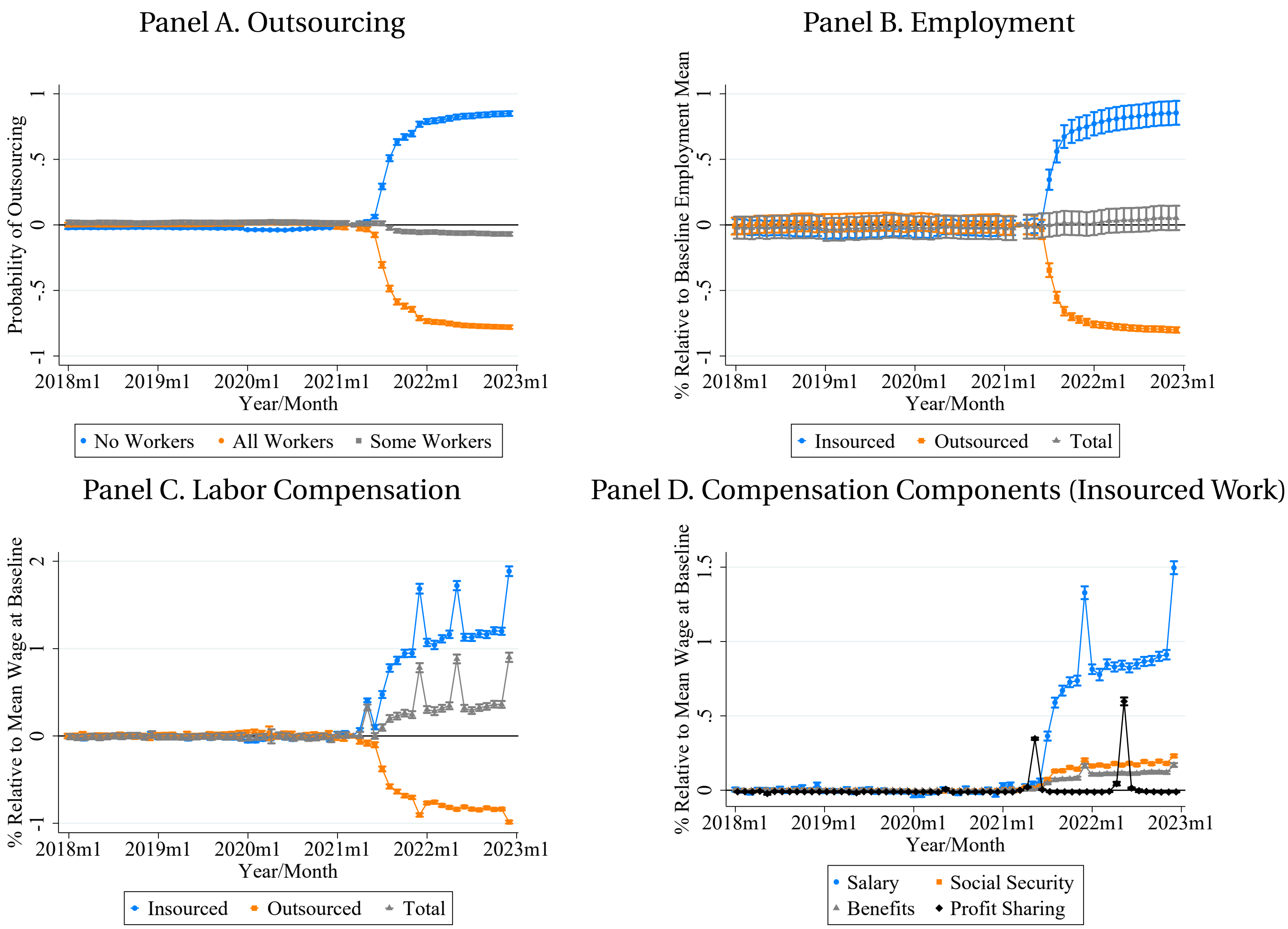
Notes: This figure presents the trend in outsourcing from 1999 to 2023 for the group of establishments in the manufacturing sector that keep employment records and declare positive employment, capital, raw materials, and energy usage. The vertical black line represents the enactment of the outsourcing reform in April 2021.
Source: Authors' elaboration using data from the Mexican economic census and the manufacturing panel of the National Institute of Statistics.

Figure 3: Tests for Differential Pretrends in Establishment-Level Outcomes



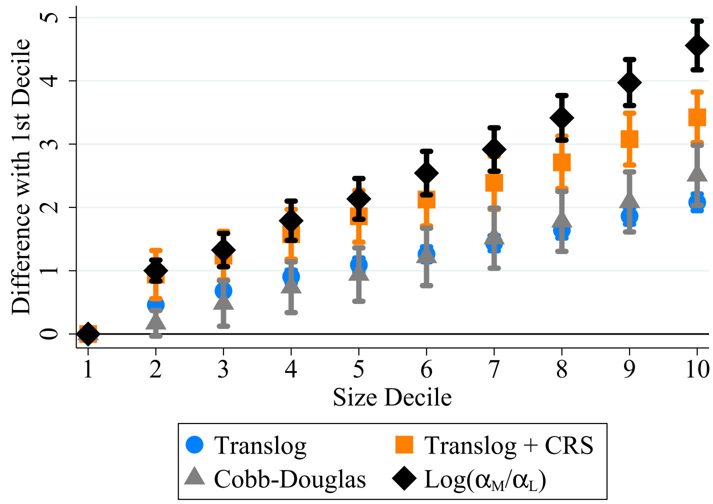
Notes: Each panel in this figure presents the regression coefficients and 95 percent confidence intervals of year dummies interacted with the establishment's outsourced employment share in 2020, controlling for year dummies and the outsourced employment share in 2020. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for 2020 is excluded from each regression, so the effects can be interpreted as deviations from the outcome mean of the group with zero exposure the year prior to the 2021 reform. All monetary amounts are deflated to July 2019 with Mexico's GDP deflator, the *índice nacional de precios al productor* (INPP), and its subindexes.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2021.

Figure 4: Tests for Pretrends in Monthly Establishment-Level Outcomes



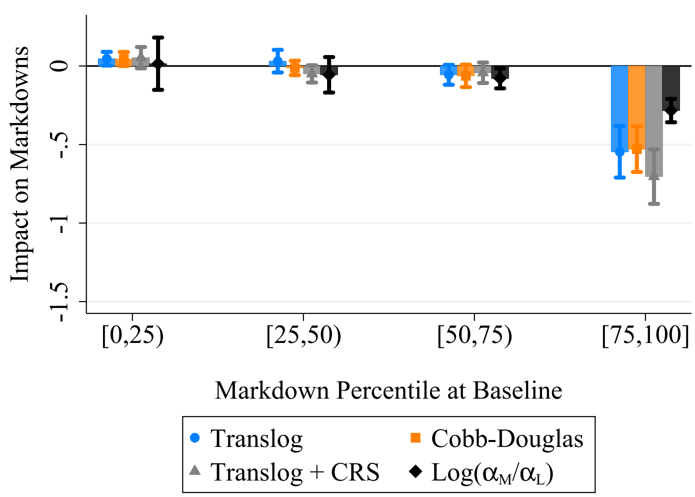
Notes: This figure presents the regression coefficients and 95 percent confidence intervals of date dummies interacted with the share of workers hired through outsourcing by the establishment at June 2020, controlling for date dummies and the share of outsourced workers in June 2020. Each panel presents results for a different set of outcomes. The effects in Panel B are expressed relative to the mean employment level across all establishments in the year prior to the reform (i.e., March 2020–March 2021). The effects in Panels C and D are expressed relative to the mean total wage compensation across all establishments in the year prior to the reform. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The interaction for March 2021 is excluded from each regression, so the effects can be interpreted as deviations from the outcome mean of the comparison group in the month prior to the 2021 reform.
Source: Authors' elaboration using data from the Mexican monthly manufacturing survey from 2018 to 2023.

Figure 5: Markdown Gradient with Establishment Size



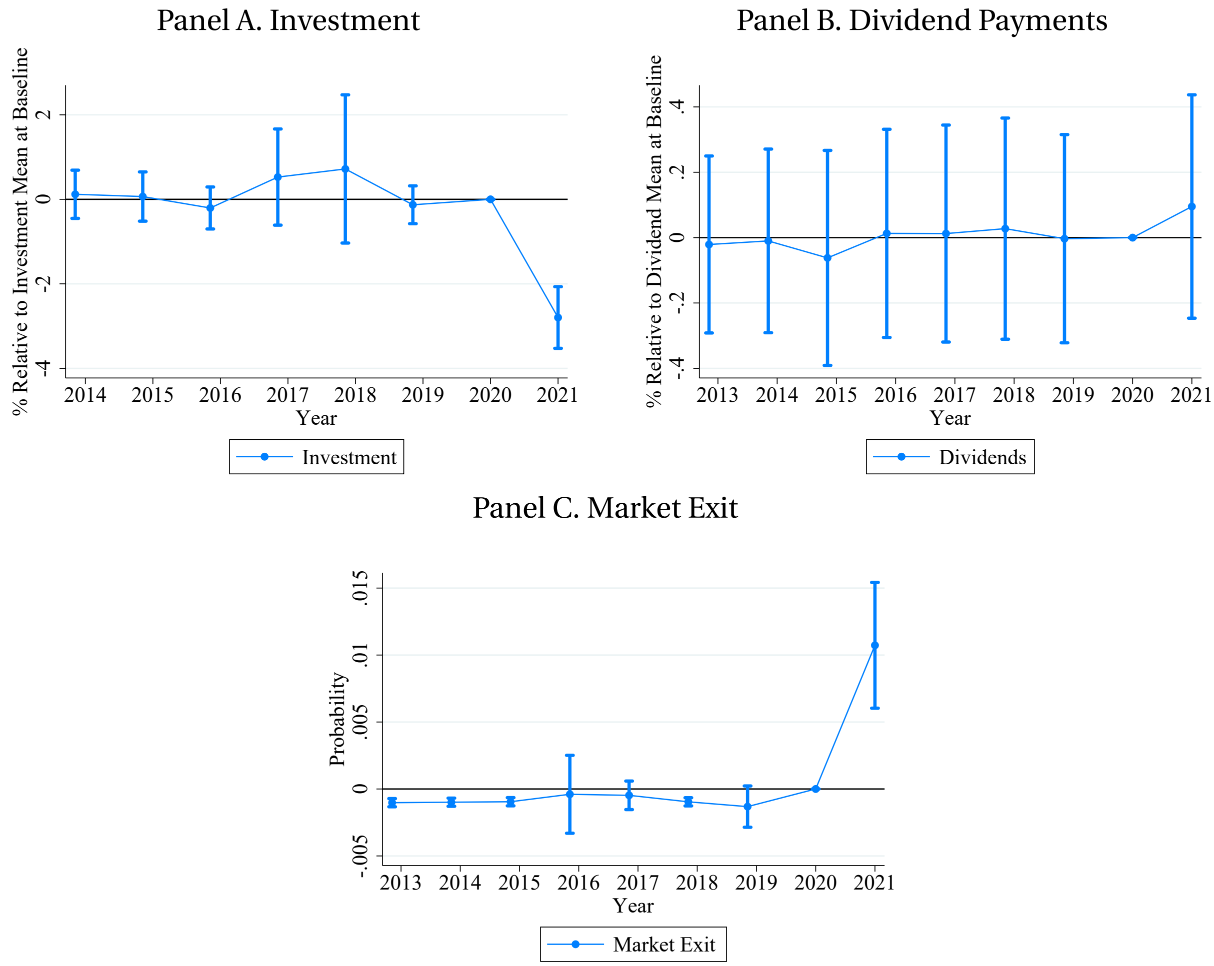
Notes: This figure reports the coefficients and 95 percent confidence intervals of establishment size decile dummies, where the deciles are taken with respect to the national distribution of the establishments' shares of total revenue in their respective local labor markets, in a regression of wage markdowns 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 6: 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 2021 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.

Figure 7: Establishment Responses to the Drop in Markdowns



Notes: For each outcome, this figure presents the regression coefficients and 95 percent confidence intervals of year dummies interacted with the outsourced employment share of the establishment in 2020, controlling for year and the outsourced employment share in 2020. 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 the effects can be interpreted as deviations from the outcome mean of the group with zero exposure in the year prior to the 2021 reform. All monetary amounts are deflated to July 2019 with Mexico's GDP deflator, the *índice nacional de precios al productor* (INPP), and its subindexes.
Source: Authors' elaboration using data from the annual manufacturing survey.

Tables

Table 1: Mean Labor Pay Shares in Manufacturing and Staffing Establishments, 2019

Payment Type	Staffing	Manufacturing		Difference (Direct Hiring-Staffing)	
		Direct Hiring	Outsourcing	Percentage Points	p-value
	(1)	(2)	(3)	(4)	(5)
Social Security	0.05	0.12	0	0.07	0.000
Profit Sharing	0.02	0.05	0	0.03	0.000
Other Benefits	0.02	0.04	0	0.02	0.000
Total Non-Salary	0.09	0.21	0	0.12	0.000

Notes: This table presents the employment-weighted means and standard errors of the labor pay shares of non-salary payments across all establishments in the staffing sector and the establishments in the manufacturing sector that either hire all their workers directly or hire all of them through outsourcing. The p-value in Column (5) corresponds to a Wald test of the difference in means between Columns (1) and (2). Staffing establishments are identified as those supplying nonspecialized workers (i.e., those providing services other than 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 2: Causal Impacts of the Reform on Annual Establishment-Level Outcomes

		Panel A. Outsourcing		
Regressor	All Workers	Some Workers	No Workers	
	(1)	(2)	(3)	
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	-0.73***	0.57***	0.16***	
	(0.01)	(0.01)	(0.01)	
N	82,873	82,873	82,873	
R^2	0.223	0.063	0.007	
		Panel B. Employment		
Regressor	Insourced	Outsourced	Total	
	(4)	(5)	(6)	
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	0.62***	-0.53***	0.09*	
	(0.03)	(0.03)	(0.05)	
N	82,873	82,873	82,873	
R^2	0.003	0.01	0.00006	
		Panel C. Usage of Other Inputs and Output		
Regressor	Capital Stock	Raw Materials	Energy	Output
	(7)	(8)	(9)	(10)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	-0.18	0.09	0.35	0.02
	(0.19)	(0.15)	(0.59)	(0.14)
N	82,873	82,873	82,873	82,873
R^2	0.00002	0.000008	0.00005	0.000003
		Panel D. Wages		
Regressor	Total Pay/Total Workers	Insourced Pay/Total Workers	Outsourced Pay/Total Workers	
	(11)	(12)	(13)	
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	0.10***	0.51***	-0.41***	
	(0.02)	(0.01)	(0.01)	
N	82,873	82,873	82,873	
R^2	0.0005	0.014	0.039	
		Panel E. Markdowns		
Regressor	Translog	Cobb-Douglas	Translog+CRS	$\text{Log}(\frac{\alpha_M}{\alpha_L})$
	(14)	(15)	(16)	(17)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	-0.27***	-0.27***	-0.24***	-0.13***
	(0.06)	(0.06)	(0.05)	(0.04)
N	19,167	19,167	19,167	19,167
R^2	0.002	0.002	0.002	0.0007

Notes: The effects shown correspond to impacts 1 year after the reform. The measure of cross-sectional exposure to the reform is the share of outsourced workers in 2020. All monetary amounts are deflated to July 2019 with Mexico's GDP deflator, the *índice nacional de precios al productor* (INPP), and its subindexes. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. The effects in Panel B are expressed relative to the mean employment level across all establishments in 2020, the year prior to the reform. The effects in Panel C are expressed relative to each outcome variable's mean in the year prior to the reform. The effects in Panel D are expressed relative to the mean total wage compensation across all establishments in the year prior to the reform. The estimation sample in Panel E includes only observations for which a lag of the input variables is available. * p<0.1, ***p<0.01.
Source: Authors' elaboration using data from the Mexican annual manufacturing survey from 2013 to 2021.

Table 3: Causal Impacts of the Reform on Monthly Establishment-Level Outcomes

	Panel A. Outsourcing		
Regressor	All Workers	Some Workers	No Workers
	(1)	(2)	(3)
$\text{Outsourcing}_{i,\text{June 2020}} \times \text{Post}_t$	-0.75***	-0.06***	0.81***
	(0.01)	(0.01)	(0.01)
N	577,501	577,501	577,501
R^2	0.444	0.003	0.304
	Panel B. Employment		
Regressor	Insourced	Outsourced	Total
	(4)	(5)	(6)
$\text{Outsourcing}_{i,\text{June 2020}} \times \text{Post}_t$	0.81***	-0.78***	0.03
	(0.05)	(0.01)	(0.05)
N	577,501	577,501	577,501
R^2	0.007	0.074	0.00002
	Panel C. Wages		
Regressor	Insourced Pay/Total Workers	Outsourced Pay/Total Workers	Total Pay/Total Workers
	(7)	(8)	(9)
$\text{Outsourcing}_{i,\text{June 2020}} \times \text{Post}_t$	1.17***	-0.82***	0.35***
	(0.02)	(0.01)	(0.02)
N	577,501	577,501	577,501
R^2	0.103	0.27	0.014
	Panel D. Insourced Wage Components		
	Salaries	Social Security and Benefits	Profit Sharing
	(10)	(11)	(12)
$\text{Outsourcing}_{i,\text{June 2020}} \times \text{Post}_t$	0.87***	0.3***	0.62***
	(0.02)	(0.01)	(0.01)
N	577,501	577,501	577,501
R^2	0.124	0.089	0.01

Notes: The effects shown correspond to the impacts 12 months after the reform. The measure of cross-sectional exposure to the reform is the share of outsourced workers in June 2020. The effects in Panel B are expressed relative to the mean employment level across all establishments in the year prior to the reform (i.e., March 2020–March 2021). The effects in Panels C and D are expressed relative to the mean total wage compensation across all establishments in the year prior to the reform. All monetary amounts are deflated with the intermediate inputs subindex of Mexico's GDP deflator, the *índice nacional de precios al productor* (INPP). 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 2023.

Table 4: 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 5: Markdowns and Outsourcing
Outcome Variable: Establishment-Level Markdowns

Regressor	Translog	Translog + CRS	Cobb-Douglas	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 6: Impact Heterogeneity
Outcome Variable: Wage Markdowns

Regressor	(1)	(2)	(3)	(4)	(5)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	-0.002	-0.22***	-0.18**	-0.29***	-0.21
	(0.03)	(0.07)	(0.08)	(0.06)	(0.21)
Interacted with:					
Markdown>75th Percentile in 2020	-0.53***				
	(0.1)				
Markdown Industry>75th Percentile in 2019		-0.18*			
		(0.11)			
Central or South Region			-0.21*		
			(0.11)		
Foreign Ownership				0.1	
				(0.15)	
Metropolitan Area					-0.06
					(0.22)
N	19,167	19,167	19,167	19,167	19,167
R^2	0.006	0.002	0.002	0.002	0.002

Notes: The estimation sample for the regressions in this table includes only observations for which a lag of the input variables is available. The effects shown correspond to impacts 1 year after the reform. The measure of cross-sectional exposure to the reform is the share of outsourced workers in 2020. Standard errors are robust to heteroskedasticity of unknown form and are clustered at the establishment level. All regressions control for the interacted variables. * p<0.1, ***p<0.01.
Source: Authors' elaboration using data from the annual manufacturing survey from 2013 to 2021.

Table 7: Establishment Responses to the Drop in Markdowns

Regressor	Market Exit	Investment	Dividend Payments
	(1)	(2)	(3)
$\text{Outsourcing}_{i,2020} \times \text{Post}_t$	0.01***	-2.80***	0.1
	(0.002)	(0.37)	(0.17)
N	84,546	73,479	82,873
R^2	0.002	0.001	0.00005

Notes: Each column reports the effect of the reform for a different outcome. The effects shown correspond to impacts 1 year after the reform. The measure of cross-sectional exposure to the reform is the share of outsourced workers in 2020. The estimation sample in Column (1) is a balanced panel constructed by coding an exit dummy for all establishments ever observed in the manufacturing survey for all years from 2013 to 2021. In Column (2), the estimation sample excludes observations from 2013, as investment is calculated as the difference between capital in the current period and the undepreciated capital from the previous period. The estimation sample in Column (3) includes all observations in the annual manufacturing survey. The effects in Columns (2) and (3) are expressed relative to their respective mean outcomes across all establishments in 2020. All monetary amounts are deflated to July 2019 with Mexico's GDP deflator, the *índice nacional de precios al productor* (INPP), and its subindexes. 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 2021.