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The impact of the outsourcing ban in Mexico on labor market outcomes. An analysis using worker level data

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Abstract
Mexico's ban on subcontracting or outsourcing practices, implemented in 2021, represents a pivotal development in the nation's labor market policies. This paper presents an analysis of the effects of the prohibition of domestic outsourcing, drawing on individual worker-level data. Employing a difference-in-differences approach with multiple time periods and heterogeneous cohort and time effects, we examine the impact of the ban on various aspects of the workforce, including wages, formality rates, and labor rights coverage. Our analysis reveals increases in wages of about 6% for previously outsourced workers, slightly higher for those who maintained their workplace, and up to 10 to 13% when transitioning shortly after the ban's enactment. Moreover, these workers experienced greater increases in formality rates and labor rights coverage compared to those who had already been hired directly prior to the policy change. We do find some evidence for strategic behavior related to the outsourcing ban's implementation. These findings shed light on the complex dynamics of labor policy reform in Mexico and have significant implications for the country's labor landscape.

Keywords Domestic outsourcing, Labor market outcomes, Wages, Impact analysis, Difference-in-differences, Worker-level data, Mexico

JEL classification J38, J53, K31, L24, M55, O15

1 Introduction

Outsourcing, in general terms, refers to the practice by which a company subcontracts employees through the payroll of another firm rather than hiring them directly into its own workforce. The use of outsourcing, subcontracting, or, more broadly, alternative labor arrangements has long been a prominent issue in global labor markets (ILO 2016). Outsourcing has increased since the 1990s in countries such as the United States (Katz and Krueger 2019), Germany (Goldschmidt and Schmieder 2017), and France (Bilal and Lhuillier 2022), as well as in many other OECD economies (OECD 2021), where between 5% and 15% of workers are reported as outsourced. However, cross-country comparisons are complicated by definitional discrepancies and measurement limitations. Similar trends are observed across Latin America, including in Brazil (Felix and Wong 2024), Peru (Jiménez and Rendon 2025), and Mexico (Estefan et al. 2024; INEGI 2020). Importantly, in many legal frameworks, workers on the payroll of a subcontracted firm face a different legal position than workers hired directly, with less labor protection and social benefits for the workers involved. Proponents of outsourcing therefore tout its potential for cost savings and efficiency gains for firms, while critics raise concerns about its detrimental effects

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on wages, workers' rights and job security (Abraham and Taylor 1996; Appelbaum 2017; Goldschmidt and Schmieder 2017).

In Mexico outsourcing had become quite widespread, with about 4.7 million workers, 17.3% of the occupied population, being subcontracted in 2019, about 70% more than a decade before (INEGI 2020). In this panorama, the decision of the Mexican government in April 2021 to implement an almost general ban on outsourcing, with the intention of safeguarding labor rights, fostering greater employment stability and avoid tax evasion, represents a pivotal moment in the nation's labor history. Studies that explore the consequences of the policy for labor markets outcomes predominantly rely on firm-level aggregates, offering insights into the overall numbers and averages of outsourced workers and their salaries (INEGI 2020; IMSS 2022; Estefan et al. 2024). Other studies combine longitudinal plant and worker-level social security data, but largely limited to firms that had outsourced their entire workforce (Colonna and Aldeco 2024) or interpreted bulk flows from one establishment to another as driven by subcontracting (Casco et al. 2024). These approaches provide important information, indicating that an important decrease of outsourcing practices with limited overall employment effects occurred, and mixed findings regarding impacts on wages.

This paper contributes to the literature by examining the effects of the outsourcing ban using representative individual-level data of affected workers, rather than relying on firm-level aggregates or focusing solely on the (private) formal sector. First, we assess the impact of the ban on the wages of formerly outsourced workers, exploring whether their earnings changed significantly due to the policy. Second, we analyze the evolution of their wages over time to identify income trends following the ban. We also examine whether there has been an increase in social security coverage. Finally, we investigate changes in the provision of other labor rights, such as paid vacations, bonuses, and profit-sharing. Through these analyses, we aim to provide a comprehensive understanding of how the outsourcing ban has influenced workers' well-being and labor conditions.

Our study is not only relevant for Mexico but contributes to the broader international literature as it is one of the few that explicitly analyzes a policy change that prohibits outsourcing practices on a broad scale. Where many studies compare outsourced with directly hired workers, often in a specific industry, occupation, or even a firm (Goldschmidt and Schmieder 2017; Drenik et al. 2023; Dube and Kaplan 2010; Felix and Wong 2024), we benefit from an exogenous policy shock generated by the implementation of the almost-general outsourcing ban to shed light on the impact of this policy that affected the whole economy. The restrictive policy implemented in Mexico is similar in spirit to a policy restricting outsourcing practices implemented recently in Peru (Jiménez and Rendon 2025), who used household surveys instead of administrative data as in the existing studies for Mexico.

The data utilized in our study come from the National Occupation and Employment Survey (ENOE). This survey provides quarterly data on individuals, facilitating a longitudinal analysis as it operates as a rotating panel, allowing to track respondents over up to five quarters. The study period spans from the third quarter of 2020 to the second quarter of 2022. This time frame, approximately 10 months prior to the reform and nine months subsequent to the deadline provided to firms for staff adjustments, was chosen to capture the phase previous to outsourced workers transitioning to being directly hired ('insourced') and the phase after the transition. The ENOE offers a wealth of information, including indicators that will be used to identify outsourced workers, because, as in the other studies for Mexico, neither in our data one single precise indicator of outsourced workers is available. An advantage of the ENOE data is that, in contrast with social security (administrative) data, it does not only capture private sector formal jobs but also tracks informal and public sectors workers in a setting of outsourcing. As argued by Jiménez and Rendon (2025), such household survey data include all individuals potentially affected by the policy, as it tracks also those entering and exiting the administrative records. This is particularly relevant in a context like Mexico, where it is not uncommon for employers to register only a portion of their workforce for social security purposes (Levy 2018). The legal reform was accompanied by increased inspections and regulatory oversight, creating new challenges and opportunities for both workers and employers. As a result, its effects may extend beyond the directly impacted individuals, implying potential spillover effects similar to those observed following recent increases in the minimum wage (Kaplan and Pérez Arce 2006; Campos-Vázquez and Rodas-Millán 2020).

In the ENOE data, we identify a treatment group composed of outsourced workers who transitioned to direct employment during the adjustment period mandated by Mexican authorities for workforce restructuring. The control group consists of workers with similar characteristics and circumstances who were already directly hired before the ban was enacted. In further analyses, we divide the treated group into two subgroups: those who remained in the same sector (and possibly at the same employer), and those who were directly hired into a different sector, and thus definitely changing their workplace. The adjustment period, which lasted approximately nine months, allows us to distinguish between early, middle, and late transitioners. This classification enables us to conduct a difference-in-differences (DID) analysis with

multiple time periods and heterogeneous cohort and time effects (Callaway and Sant'Anna 2021) to determine the impact of the ban.

Our main findings suggest that the implementation of the outsourcing ban led to an average wage increase of approximately 6%, with a larger effect of 8.6% for workers who remained in the same sector. In contrast, the effects were smaller or statistically insignificant for those who were directly hired into a different sector. We also find heterogeneity based on the timing of the transition; early and middle transitioners experienced wage gains of 10–13%, while those who transitioned late during the adjustment period saw negligible effects. Notably, wage increases occurred even before the legal implementation of the ban, indicating that employers may have strategically adapted to the new regulations in an effort to retain high-performing workers. Beyond the impacts on wages, we also find evidence that the ban contributed to greater formalization among previously outsourced workers, including increased access to benefits such as profit-sharing. In general, our findings point in the same direction as the national and international literature, with perhaps slightly stronger wage impacts than in other research for Mexico, something that may be related to the variations in data sets including the identification of outsourcing.

The paper is structured as follows. Section 2 offers a background on the theoretical and empirical aspects of outsourcing, including a description of the policy implementation in Mexico and a review of previous research on the topic. Section 3 addresses the methodological aspects, detailing the data source, delineation of outsourced workers, and econometric framework used in the analysis. Section 4 presents the results of the study. In Sect. 5, the implications and interpretations of the results are discussed, while Sect. 6 provides some concluding remarks.

2 Background

The term "outsourcing" encompasses a spectrum of practices with diverse implications, ranging from offshoring (moving activities to other countries) to off-site operations and in-house arrangements. This study focuses on a setting of domestic outsourcing, wherein companies have contracted a portion of their labor force through independent subcontractors instead of directly into their own workforce, while workers continued to operate from the same physical location (Goldschmidt and Schmieder 2017).

2.1 Theoretical framework and international evidence

Abraham and Taylor (1996) and Goldschmidt and Schmieder (2017) delineate three primary motivations behind employers' decisions to outsource staff. Firstly, outsourcing allows high-wage companies to capitalize on lower labor costs in the market. Secondly, it enables the smoothing of workload fluctuations by supplementing regular workers with outsourced labor during peak periods. Finally, firms may leverage the economies of scale offered by specialized service providers. In the United States, the prevalence of outsourcing has surged as firms seek to reduce costs by outsourcing work to non-union providers or offshore locations, driven by competitive pressures or shareholder demands (Bernhardt et al. 2016). Similarly, in Germany, outsourcing witnessed an uptick following the abandonment of collective agreements in the early 1990 s (Goldschmidt and Schmieder 2017) and the deregulation of temporary agencies (Vitols 2004).

A key rationale behind the surge in outsourced personnel globally (ILO 2016; OECD 2021) is the potential cost saving. Firms often subcontract with providers that offer lower wages and fewer benefits, particularly for peripheral tasks that are easily monitored or replaceable (Abraham and Taylor 1996; Kogut and Zander 1992). Additionally, Appelbaum (2017) suggests that firms' pursuit of cost containment strategies, such as outsourcing, reflects a diminishing emphasis on the labor process and reduced investment in retaining employees. Weil (2014) argues that outsourcing is also driven by firms' efforts to circumvent legal constraints, unions, and equity norms, enabling them to offer disparate wages to similar workers.

Furthermore, outsourcing has been associated with adverse consequences for job quality. Outsourced activities, often characterized by low profit margins, can lead to work intensification and diminished bargaining power for workers (Ji and Weil 2015). Contractors, engaged in fierce competition, may be compelled to lower bid prices, resulting in deficient remunrations for outsourced workers (Weil 2014). The outsourcing of low-skilled workers can further exacerbate inequities, as they are removed from internal equity norms and compensated closer to market rates, particularly in high-wage firms. This variation in job quality and wages contributes to widening inequality (Bernhardt et al. 2016), with outsourced workers facing diminished job security and increased vulnerability to labor law violations (Weil 2014).

Outsourcing disrupts the organic relationship among workers, such as shared facilities, which traditionally foster solidarity and equity (Appelbaum 2017). Consequently, the expected effect of banning outsourcing is a transition of previously outsourced workers to direct employment by their actual employer, with the expectation of receiving wages on par with their colleagues. This shift is driven by enhanced bargaining power with the firm and the perceived fairness of equitable wages among workers. The theoretical underpinning of this mechanism draws from Adams' (1963) model, that posits that the perceived value of labor input should equate

to payment. In other words, workers supply effort (e) proportional to the ratio of their actual wage (w) to the perceived fair wage (w^*), i.e., $e = w/w^*$. Workers receiving perceived unfair wages ($w < w^*$) are likely to adjust their effort accordingly ($e < 1$). Experimental evidence supporting this notion is provided by Valenzi and Andrews (1971), who found that 20% of workers quit immediately when informed of unequal pay arrangements. Similarly, Breza et al. (2018) discovered that workers are willing to sacrifice earnings up to 9.3% to avoid wage disparities, with this aversion persisting and increasing over time. Moreover, pay disparities can lead to increased absenteeism and undermine cohesion and cooperation among team members. Even in non-unionized sectors, Blau (1977) and Groshen (1991) demonstrate that workers have an intrinsic sense of equity, expecting parity in compensation. Thus, firms accustomed to outsourcing, in order to incentivize attendance, effort, and cooperation need to raise wages to uphold perceptions of equity within the organization.

More recently, Bilal and Lhuillier (2022) have investigated the equity–efficiency trade-off underlying domestic outsourcing decisions. Their theoretical model highlights three key implications regarding outsourcing. It shows that, in order to save on labor costs, the more productive firms are more likely to outsource, while moreover generating a productivity effect for the outsourcing firm, raising output and (overall) labor demand. That is counteracted, however, by a wage penalty for the outsourced workers: the model suggests that: contractor firms pay lower wages. Using longitudinal administrative data of the French labor market, they find evidence for their theoretical framework, noting that, between 1997 and 2016, outsourcing practices reduced low-skilled earnings by 1.5%, increased sorting on the labor market, and raised output by 6%.

Several studies have examined the impact of outsourcing on workers' wages across different countries. For instance, Drenik et al. (2023) observed a 5.9% lower wage for outsourced staff in Argentina, while Goldschmidt and Schmieder (2017) found that outsourced workers in Germany earned approximately 10% less than their non-outsourced counterparts. In the United States, Dube and Kaplan (2010) reported wage reductions ranging from 4.1 to 24.4%. Furthermore, the legalization of outsourcing in Brazil is found have had adverse effects on employment and income of older security guards, while younger new entrants obtained more labor opportunities (Felix and Wong 2024). It is important to note that these studies often only encompass specific occupations, some unaffected by the Mexican outsourcing ban, such as janitors, guards, gardeners, and cleaning and catering staff. In a more general setting, a recent analysis for Türkiye, Gürer and Taymaz (2025) identified outsourcing practices by observing bulk flows and buyer–supplier transactions between the outsourcing company and the subcontractor, allowing them to determine that domestic outsourcing carried a wage penalty of about 10% for the low-skilled outsourced, while for high-skilled workers outsourcing came with a similarly-sized wage premium, findings in line with Bergeaud et al. (forthcoming).

Particularly interesting for our analysis are the restrictions on outsourcing implemented in Peru in 2022, a policy comparable in scope and intentions to the regulation in Mexico. Using household survey data, Jiménez and Rendon (2025) found that, although the policy effectively reduced outsourcing practices, its impacts on employment and labor force participation were only slightly positive while no significant impacts on wages nor formality are encountered. The fact that—in sharp contrast with Mexico—the Peruvian law is still facing legal challenges, uncertainties regarding prolonged enforcement may be behind the limited immediate effects.

2.2 Outsourcing in Mexico

Globally, outsourcing practices are subject to various limitations and regulations. For instance, in the construction sector of several European countries, outsourcing is either restricted or prohibited, and some nations impose joint liability on firms that benefit from outsourced labor (Díaz Pérez et al. 2020). With the notable exception of Peru (Jiménez and Rendon 2025), Mexico's 2021 reform stands out by imposing an almost total ban on outsourcing—going significantly further than most regulations elsewhere.

This legal reform can be understood as a response to specific features of the Mexican labor market, where outsourcing had become a widespread strategy. It offered employers advantages such as specialization in core activities, risk mitigation, cost reduction, increased competitiveness, and greater labor flexibility. After the 2008 economic crisis, Mexico's recovery was marked by job insecurity, diminished social labor protections, and growing use of precarious employment arrangements, including subcontracting and freelance contracts (Samaniego 2018). The prevalence of outsourcing expanded rapidly during this period. According to Economic Census data, the number of outsourced workers in Mexico increased by 70% between 2008 and 2019—from 2,732,036 in 2008 to 3,578,247 in 2014, and 4,685,356 in 2019 (INEGI 2020).

Similar to other economies, outsourcing in Mexico has led to disparities in remuneration among workers. Outsourced personnel in Mexico typically receive lower wages and fewer benefits than directly hired workers. Contractors often distribute profits unevenly, favoring core staff over outsourced employees. In 2019, audits by the Secretary of Labor and Social Welfare (STPS) revealed widespread irregularities: many firms

outsourced nearly their entire workforce, underreported wages, misclassified job roles, and engaged in frequent contract renewals (Levy 2018; Brito Laredo et al. 2022). As noted by Ramirez et al. (2021), outsourcing in Mexico has primarily functioned as a cost-saving strategy rather than a legitimate employment structure. However, it brings significant downsides, including increased informality, labor regulation evasion, and systematic underreporting of wages (Díaz Pérez et al. 2020).

In light of the growing relevance of outsourcing in the Mexican labor market, a legal reform was introduced in November 2012 through amendments to the Federal Labor Law (*Ley Federal del Trabajo*). This reform explicitly permitted outsourcing, provided that certain conditions were met. However, it proved insufficient to protect workers, as it lacked provisions for joint liability and adequate oversight mechanisms. As a result, some companies began transferring their workforce to secondary subcontracting firms as a strategy to evade tax and labor obligations (Bensusán 2013). In that context, Rojas García and van Gameren (2021) explored the wage effects of outsourcing in Mexico using the ENOE, identifying outsourced personnel based on criteria such as receiving remuneration based on commissions, having temporary contracts, or working in other firms' facilities, and compared them with non-outsourced workers with otherwise similar characteristics. Their results reveal that subcontracted workers tend to be younger, unmarried, less educated, and earn between 2.0% and 16.4% less than directly hired counterparts, with a greater wage penalty for the lowest deciles.

In April 2021, the Mexican government implemented a new outsourcing reform aimed at addressing the issues that remained unresolved by the amendments of 2012, improving labor conditions for directly hired workers, and reducing reliance on outsourcing companies (Muñoz-González and Paredes-Arriaga, 2022). The reform introduced major changes, including amendments, additions, and repeals to several laws, most notably the Federal Labor Law and the Social Security Law (*Ley del Seguro Social*). One of the most significant changes is the near-total prohibition of subcontracted personnel. Employment agencies may still assist with recruitment, selection, and training, but they are no longer considered employers. Subcontracting is now limited to services that are not part of a company's primary business activity. Temporary or non-core services must be governed by a written contract specifying the scope of work and an estimated number of workers. Contractors must be registered with the STPS and renew their registration every three years. The law imposes penalties for unauthorized outsourcing, avoiding labor inspections, or withholding information. Notably, roles such as security, cleaning, and gardening remain exceptions and can still be legally outsourced.

2.2.1 Empirical evidence on Mexico's outsourcing ban

The Mexican Social Security Institute (*Instituto Mexicano del Seguro Social*, IMSS) conducted an aggregate-level analysis, identifying approximately 5 million workers employed at outsourcing companies (IMSS 2022). Of these, 2.9 million workers transitioned from subcontractors to genuine employers. This may suggest successful implementation of the reform, as employers adapted their workforce accordingly in a timely manner. Over the period between November 2020 and January 2022, the base salary increased by 19.9%, with a higher increase of 22% for women. Notably, the reform did not lead to any distortions in the formal labor market, as evidenced by the absence of an increase in terminations (IMSS 2022). However, these results are primarily descriptive, but, while informative and useful as benchmark, are not based on an impact analysis that would allow for causal interpretations. More generally, the results do not provide insight into the timing of these changes, nor a comparison of individual workers, while moreover the base salary (on which social security contributions are based) may differ from the actual wage that firms pay (Levy 2018; Díaz Pérez et al. 2020).

In one of the first attempts for a causal analysis of the outsourcing ban, Estefan et al. (2024) focus on manufacturing, about one-fifth of the country's GDP, utilizing the (establishment-level) Annual and Monthly Manufacturing Surveys (EAIM, EMIM). Their analysis reveals a drastic decrease in the number of outsourced workers with limited to no adverse effects for output, employment levels, or other inputs. Using the monthly data, using an establishment's outsourcing rate as treatment indicator, a noteworthy increase in a firm's average wages, amounting to a 35% rise relative to pre-reform levels of mean total wage compensation across all establishments is found. The annual data however suggest that the wage bill in establishments that outsourced all their workforce increased by 10% on average. The monthly data also allow to analyze impacts on wage bill components, in particular on salaries, benefits, social security payments, and profit-sharing, reporting even larger positive impacts, for fully exposed firms relative to pre-reform total wage compensations—suggesting a rise of formality as well as profit-sharing.

Using plant-level data from the EMIM, Colonna and Aldeco (2024) focused on firms that had outsourced nearly all their operations—so-called "empty establishments". This model of outsourcing was used by about two thirds of the firms engaged in subcontracting. In line with the reform's goals, the authors find a sharp decline in outsourcing practices. However, they observe no

significant changes in total employment or total hours worked. Interestingly, profit-sharing payments increased following the reform. Worker-level social security (IMSS) data from firms that had outsourced nearly their entire workforce show that wages among previously outsourced employees fell by about 1.6% relative to unaffected workers, with stronger effects for women and employees in medium-sized firms. The authors interpret this modest wage reduction as a trade-off for the newly enforced profit-sharing obligations, which had previously been avoided. Despite the slight wage drop, total labor compensation in firms that had fully outsourced their staff rose by approximately 2%.

Casco et al. (2024) use the social security (IMSS) data to construct the employment histories of individual workers and establishments, and identify outsourced workers by assuming that workers who, in July 2021, moved in bulk (in groups of 20 or more) between establishments were previously outsourced, arguing that regularization of previously outsourced employees is the prime reason for such bulk flows, an identification strategy alike Gürer and Taymaz (2025). They find that regularized workers are 6 pp more likely to remain in a formal job than otherwise similar but non-outsourced workers, while wages are found to increase by about 3 to 4% after the outsourcing ban. Heterogeneity across industries is reported, with strongest effects in manufacturing and business services, but the impact varies over time—with trade showing longer-lasting impacts—while transport and social services saw small wage reductions. Furthermore, the wage effects were concentrated in larger firms. To a lesser extent, also heterogeneity by gender—women seeing slightly stronger wage increases but leveling out over time— and by age, with most workers observing an immediate benefit but the youngest workers enjoying higher wage increases in later months.

3 Methodology

The data set used in this research is derived from the National Occupation and Employment Survey (ENOE), conducted quarterly by the Mexican National Statistics Institute (INEGI 2024) employing a representative sample of the population aged 15 and above. The data relevant for our analysis covers the period from mid-2020 to mid-2022, that is, the quarters directly before and after the implementation of the outsourcing ban.[1] The data encompass sociodemographic, household, and work-related variables. Importantly, the survey's rotative panel structure enables longitudinal tracking of individuals over time.

The analysis focuses on initially, before the ban, outsourced workers who transitioned to being directly hired during the law's adjustment period. These workers are affected in a direct way by the ban and therefore constitute the treatment group; once they change their hiring status, they are considered treated and remain as such in subsequent periods. As the comparison or baseline group, we selected otherwise similar workers who were insourced—directly hired—already before the legal change. By observing how the income of previously outsourced workers deviates from this baseline following the implementation of the ban, we can identify the effects attributable to the policy change.

The dependent variables in our analysis, in addition to the worker's monthly income. are whether there is a change in the post-ban job's formality status, and if workers received secondary benefits such as an annual bonus, paid vacations, and profit-sharing. To assess the percentage change in income attributable to the outsourcing ban, we transform this variable into a logarithmic form. Job formality is represented by an indicator variable that takes a value of 1 if the worker is registered with one of the social security institutes in Mexico, primarily at the IMSS (private sector workers) or the ISSSTE (most of the public sector employees). Similarly, end-of-year bonus receipt is indicated by a value of 1 for workers that receive an annual gratification (*aguinaldo*) equivalent to 15 days of wage in December as stipulated by the labor law. Vacation rights is indicated by a value of 1 if workers obtained paid vacations, while profit-sharing is indicated by a value of 1 if workers receive their mandatory share of the firm's profits (*reparto de utilidades*) distributed proportionally based on the number of workers and their respective wages.

The independent variable of interest is an indicator variable that is equal to 1 if the worker belongs to the treatment cohort—outsourced prior to the implementation of the ban—and 0 otherwise. Additionally, we incorporate control variables that are known to influence income, including sociodemographic factors such as age, gender, education level, marital status, and region, as well as job-related variables such as business size, sector, and daily labor hours (Arceo-Gómez and Campos-Vázquez 2014), while several interaction variables are also included to account for potential moderating effects.

[1] Due to COVID-19 restrictions, the survey methodology shifted from face-to-face interviews to telephone surveys, resulting in changes in sample size, but further the 'COVID-waves' were fully comparable to the standard ENOE. Note that the pandemic hit before our observational period starts, and remained relevant by the end of our observations. Hence, we do not consider that our findings are particularly sensitive for the pandemic, in line with the arguments of Jiménez and Rendon (2025) for a similar exercise for Peru. Moreover, lockdown measures in Mexico were less stringent and public (financial) government support was less generous than in many other countries, and despite heterogeneity between sectors, labor demand has not structurally changed (Hoehn-Velasco et al. 2021; Campos-Vázquez et al. 2021).

3.1 Data in detail: definition of outsourcing

The definition of outsourcing utilized in this study, aimed at identifying workers affected by the reforms, aligns with the criteria outlined by INEGI for "employed personnel not dependent on the firm". According to this definition, such workers are working at the establishment of a firm but are contracted through a different entity. They engage in various activities related to production, commercialization, administration, and accountability, among others, while personnel employed in surveillance, maintenance, cleaning services, and gardening should be excluded because the outsourcing ban does not apply to these activities.

Given that the ENOE lacks specific indicators for outsourced personnel, we consider as outsourced those workers categorized as "staff hired and provided by another firm", which denote subordinated workers operating within the facilities or premises of other companies. Also, individuals classified as "staff working for fees or commissions without basic salary" are considered outsourced personnel. This is done because they perform core work for the firm's primary business activities, even though they are self-employed and paid solely through commissions, tips, or fees; as noted before, companies did not always register all their workers for social security (Levy 2018). On the other hand, self-employed who provide professional services like lawyers, doctors, or accountants are excluded from the analysis, because they constitute (small) businesses that generally sell their services to more than one firm.[2]

These categories establish the treatment group comprising workers outsourced before the ban who transitioned to direct employment during the stipulated adjustment period granted to firms, between the second and fourth quarter of 2021. Meanwhile, the control group consists of workers consistently employed directly by their respective firms, identified as those operating "within the facilities or vehicles of the firm" that hired them.

3.1.1 Timing of transition from outsourcing

The time frame under consideration spans from the third quarter of 2020 to the second quarter of 2022, encompassing a total of eight quarters. Given that firms face no restrictions on incorporating workers into their in-house workforce, this action can be executed at any time. However, as depicted in Fig. 1, there is a notable increase in the direct hiring of outsourced workers, particularly evident in the second quarter of 2021 coinciding with the publication of the outsourcing reforms. This trend persists in subsequent periods, indicating a sustained shift in employment practices. The observed amount in the first quarter of 2022, subsequent to the deadline for staff adjustments, may be attributed to firms who reacted too late to the legal changes.

Crucially, the time frame of eight quarters can be segmented into three distinct sections: (1) the 'pre-ban period', comprising the third and fourth quarters of 2020 and the first quarter of 2021; (2) the 'adjustment period', spanning from the second to the fourth quarter of 2021; and (3) the 'post-ban period', covering the first and second quarters of 2022. Additionally, within the second segment, the adjustment period, the treated group is further subdivided into three cohorts whose transitions may be governed by different decision-making mechanisms: (a) the 'early cohort', consisting of outsourced workers who transitioned to direct employment in the second quarter of 2021, around the time the law was implemented; (b) the 'middle cohort', comprising workers who transitioned in the third quarter of 2021; and (c) the 'late cohort', that consists of workers who transitioned in the final quarter of 2021, that is, at the end of the legally predicated adjustment period.

Table 1 provides a comprehensive overview of the three cohorts. Across all treatment cohorts, the majority is male, constituting approximately 80% of each group. Notably, the early cohort comprises younger individuals with comparatively fewer years of education. This cohort exhibits a higher proportion of full-time employment and is more prevalent in medium-sized businesses and the construction sector. Conversely, the middle cohort skews the oldest and boasts the highest average years of education. Members of this group are predominantly affiliated with micro and small businesses, with a notable concentration in the services sector. In comparison with the early cohort, the middle cohort's representation in formal employment and labor rights entitlement is slightly lower. In contrast, the late cohort emerges as the second oldest group, with a notable presence in large firms and the highest average wages. While this cohort demonstrates a similar proportion of male participants, its composition differs significantly in terms of education, sectoral participation, and business size affiliation compared to the early and middle cohorts.

3.1.2 Retained job or hired elsewhere

In addition to dividing the treated individuals into cohorts based on the timing of their transition, the full

[2] Using the NAICS (North American Industry Classification System) codes available in the ENOE, we exclude respondents classified as "Professional, scientific, and technical services". Furthermore, we exclude respondents in "Business support services and waste management", capturing, as good as we can, guards, clearing and catering staff. An analysis exclusively on these occupations not directly affected by the ban could serve as a placebo as no effects have to be expected. The small number of observations hampers strong conclusions, but are supportive for our argument that the ban potentially had effects for broader groups of workers who were only indirectly affected by the legal changes and increased inspections of compliance with labor regulations.

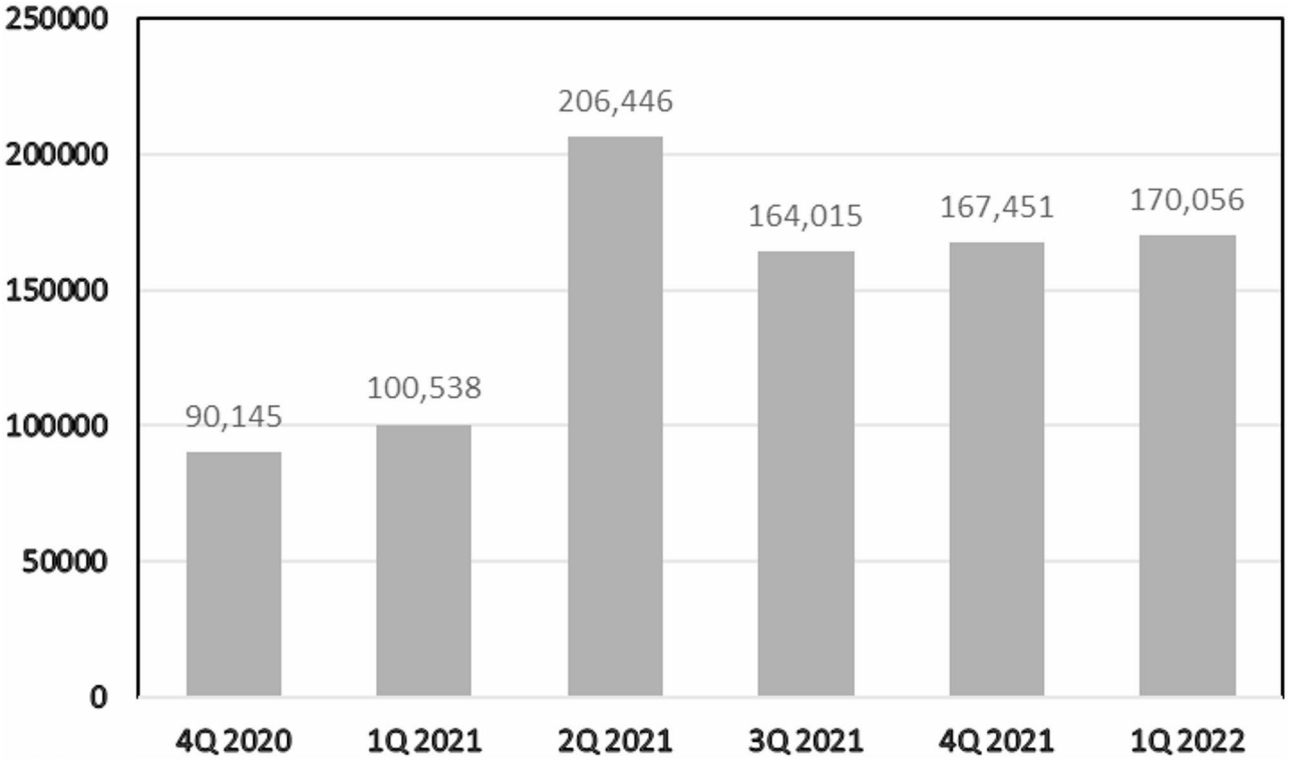

Fig. 1 Transition of Outsourced Personnel to Directly Hired, 2020Q3-2022Q1. Note: Number of workers identified as outsourced in a specific quarter who transitioned to being directly hired in the subsequent quarter (the quarter on the *x*-axis). Source: INEGI (2024)

sample is further segmented into two groups to examine differences between workers who retained their jobs and those who moved to new ones. We define the 'job kept' group as workers who remained employed in the same sector after the outsourcing ban, what can suggest job continuity.[3] In contrast, the 'job lost' group includes those who reported being directly hired after the reform but in a different sector—what certainly indicates a change of job. Changing sectors implies that these workers were not direct beneficiaries of the reform's goal to promote regular hiring within the same workplace. However, being hired directly in another sector suggests that nonetheless they may have benefited from improved labor market conditions and opportunities. As shown in Table 2 (first row), 66.8% of formerly outsourced workers who were later directly hired stayed within their original sector. This retention rate is similar to the 70% figure reported by IMSS (2022).

Both subsamples exhibit comparable levels of male participation and educational attainment (Table 2). Additionally, individuals in the 'job kept' sample tend to be married more often and are more likely to be employed by medium to large-sized businesses. Notably, the proportion of workers engaged in the manufacturing sector is higher among those who retained their positions, while the converse holds true for the services sector. Furthermore, a higher percentage of workers that retained employment are engaged in full-time positions and command a superior wage compared to their counterparts who experienced job displacement, while moreover they exhibit a higher incidence of formal employment status and enjoy a greater array of labor rights protections.

3.2 Estimation technique

The outsourcing ban's impact on workers' wages and other labor outcomes is evaluated by the difference-in-differences (DID) approach with multiple time periods based on outcome regressions developed by Callaway and Sant'Anna (2021). This method allows us to focus on the cohort-time effects, which measure the average treatment effect for a specific cohort at a particular time, where a cohort is defined by the time period when units are first treated, i.e., transitioned from outsourced to direct hire.

An advantage of the cohort-time treatment effect parameters is their flexibility in accommodating heterogeneity with respect to observed covariates, the timing of treatment initiation, and the evolution of treatment effects over time. However, several key assumptions underlie this method. First, the irreversibility of

[3] Although the ENOE contains a question about when a worker started their current job, most respondents do not provide this information; perhaps because they have been in their jobs for a long time and do not recall when they began.

Table 1 Descriptive statistics by cohort

	Control group	Early cohort	Middle cohort	Late cohort
Dependent variables				
log(Wage)	8.82	9.04	8.98	9.05
	(0.55)	(0.55)	(0.58)	(0.50)
Formal	76.8%	89.9%	82.9%	83.9%
	(0.325)	(0.249)	(0.315)	(0.305)
Bonus	73.3%	80.8%	78.3%	78.2%
	(0.354)	(0.325)	(0.336)	(0.342)
Vacations	67.8%	76.1%	72.7%	73.9%
	(0.380)	(0.361)	(0.387)	(0.363)
Profit sharing	35.1%	44.7%	32.4%	40.9%
	(0.404)	(0.418)	(0.383)	(0.397)
Independent variables				
Male	55.6%	80.8%	80.6%	78.3%
	(0.474)	(0.392)	(0.385)	(0.394)
Age	36.0	37.3	38.6	38.1
	(13.0)	(10.1)	(11.3)	(11.7)
Years of schooling	11.3	10.9	11.8	11.4
	(4.16)	(3.66)	(5.88)	(5.85)
Full time	56.1%	61.8%	51.3%	54.9%
	(0.406)	(0.418)	(0.401)	(0.403)
Marital status				
Single	38.7%	25.6%	22.9%	22.7%
	(0.471)	(0.434)	(0.416)	(0.408)
Married	33.3%	46.5%	40.8%	44.5%
	(0.447)	(0.495)	(0.484)	(0.475)
Common law marriage	19.3%	22.2%	30.4%	23.7%
	(0.374)	(0.413)	(0.455)	(0.406)
Divorced/Widowed	8.7%	5.5%	6.0%	9.1%
	(0.260)	(0.226)	(0.226)	(0.269)
Business size				
Micro	31.2%	32.2%	33.7%	31.7%
	(0.375)	(0.402)	(0.360)	(0.383)
Small	24.0%	23.1%	27.1%	25.7%
	(0.361)	(0.331)	(0.327)	(0.347)
Medium	21.1%	28.8%	27.6%	23.3%
	(0.341)	(0.377)	(0.328)	(0.322)
Large	20.8%	15.4%	11.3%	17.7%
	(0.357)	(0.287)	(0.257)	(0.303)
Government	0.1%	0.2%	0.1%	0.5%
	(0.069)	(0.035)	(0.022)	(0.053)
Sector				
Construction	6.8%	18.9%	17.6%	15.7%
	(0.217)	(0.348)	(0.340)	(0.324)
Manufacturing	29.7%	19.7%	17.5%	21.7%
	(0.405)	(0.353)	(0.335)	(0.352)
Commerce	22.3%	17.5%	19.5%	20.5%
	(0.364)	(0.342)	(0.344)	(0.359)
Services	35.7%	36.8%	42.4%	37.9%
	(0.423)	(0.420)	(0.446)	(0.418)
Agriculture	4.5%	1.2%	0.4%	0.6%
	(0.171)	(0.104)	(0.065)	(0.062)

Table 1 (continued)

	Control group	Early cohort	Middle cohort	Late cohort
Number of observations	15,487	251	384	889
Weighted number of observations	4,317,847	66,118	96,180	209,171

Source: INEGI (2024)

Note: Summary statistics before the transition from outsourcing; for the Early, Middle, and Late cohort: workers who ceased being outsourced in the second, third, and fourth quarter of 2021, respectively. Control group contains those who had been directly hired already before the policy change. Standard deviations in parentheses

treatment assumption implies that once a unit becomes treated, it remains treated in all the subsequent periods. This holds in our data because previously outsourced workers cannot return to this modality, as it is no longer legal. Second, random sampling assumes that observations are independent and identically distributed, allowing to treat all potential outcomes as random variables without imposing restrictions on their relationship with treatment allocation or the intertemporal dependence of observed variables. The ENOE sampling framework, together with the nondiscriminatory implementation of the outsourcing ban, ensure that this assumption holds. The third assumption restricts anticipation of treatment for all eventually treated cohorts. For this study we assume no anticipation ($\delta = 0$), considering that workers could not choose their treatment status, i.e. when they were integrated into the firm's staff.

The final two assumptions are common in any DID analysis. Regarding the parallel trends assumption, Fig. 2 shows that the logarithm of wages had generally similar trends in the period before the ban's enactment. Finally, the overlap assumption, in this specification with multiple groups, ensures that a relevant portion of the population initiates treatment in each cohort and that the propensity score does not approach one for any group at any time, preventing scenarios where one cohort dominates the treatment or control group entirely.

Under these assumptions, the commonly used potential outcomes framework can be applied to identify the impact of the outsourcing ban on labor market outcomes. Following Callaway and Sant'Anna (2021), the observed ($Y_{i,t}$) and potential ($Y_{i,t}(g)$) outcomes for each worker i are linked by:

$$Y_{i,t} = Y_{i,t}(0) + \sum_{g=2}^{T} \left(Y_{i,t}(g) - Y_{i,t}(0)\right) G_{i,g}$$

where $G_{i,g} = 1$ if worker i belongs to group or cohort g, indicating the period when the (irreversible) treatment—the transition from outsourcing to direct hiring—occurred, and zero otherwise. In other words, each unit is associated with a singular potential outcome trajectory. For units receiving treatment, the observed outcomes correspond to the unit-specific potential outcomes aligned with the particular period of treatment initiation. In the empirical part, we analyze the heterogeneity of impact for different treatment initiation periods during the law's adjustment period, that is, the early, middle and late cohorts addressed above, while we also address two quarters pre- and post-ban to see if effects occurred while the law was under discussion or with a deferment.

The Average Treatment Effect on the Treated (ATET) for cohort g at time t with a lag of δ periods (that is assumed to be zero, see above) is then given by:

$$ATET(g,t;\delta) = E\left[\frac{G_g}{E[G_g]}\left(Y_t - Y_{g-\delta-1} - m_{g,t,\delta}(X)\right)\right]$$

where $E[G_g]$, the expected value of G_g, is the average proportion of workers belonging to cohort g in the population; Y_t is the outcome variable of interest (logarithm of wage, formality, end-of-year bonus, paid vacations, and profit-sharing) at time t; and $Y_{g-\delta-1}$ the lagged outcome variable in cohort g, $\delta + 1$ periods ago, while

$$m_{g,t,\delta}(X) = E\left[\left(Y_t - Y_{g-\delta-1} \mid X, C = 1\right)\right]$$

represents the conditional expectation of the outcome variable Y_t, given the covariates X that may influence the outcome, for cohort g at time t with a lag of δ periods. It captures the relationship between the outcome variable and the covariates for the treated cohort at time t after adjusting for lagged outcomes and covariates, in the hypothetical case that they had not been treated, while $C = 1$ indicates workers who do not participate in the treatment in any time period but were directly hired before the ban was enacted, and thus constitute the control group.

Hence, $ATET(g, t, \delta)$ measures the average difference in outcomes between the treatment group g and the control group at time t after accounting for any differences in the pre-treatment period while controlling for covariates X (summarized in Tables 1 and 2). Their aggregation by time, cohort, and time of exposure to treatment (for example the effect experienced a quarter after being hired directly) entails calculating a weighted average based on the proportion of workers within each level of aggregation, in general of the form

$$ATET = \sum_{g \in G} \sum_{t=2}^{T} w(g,t)\, ATET(g,t)$$

Table 2 Descriptive statistics by treatment sample

	Full sample	Job kept sample	Job lost sample
Sample shares:	100.0%	66.8%	33.2%
Dependent variables			
log(Wage)	9.03	9.051	8.99
	(0.50)	(0.50)	(0.57)
Formal	84.7%	86.7%	81.2%
	(0.300)	(0.267)	(0.340)
Bonus	78.7%	79.0%	75.8%
	(0.338)	(0.329)	(0.365)
Vacations	74.0%	73.9%	71.2%
	(0.369)	(0.361)	(0.390)
Profit sharing	39.4%	42.3%	33.9%
	(0.399)	(0.406)	(0.380)
Independent variables			
Male	79.4%	78.2%	78.0%
	(0.392)	(0.396)	(0.403)
Age	38.1	38.5	37.6
	(11.32)	(11.43)	(11.14)
Years of schooling	11.4	11.4	11.4
	(5.54)	(6.25)	(4.26)
Full time	55.2%	56.7%	53.2%
	(0.406)	(0.401)	(0.411)
Marital status			
Single	23.3%	21.6%	25.8%
	(0.415)	(0.403)	(0.431)
Married	43.9%	45.6%	41.0%
	(0.481)	(0.479)	(0.480)
Common law marriage	25.1%	25.4%	25.4%
	(0.422)	(0.420)	(0.425)
Divorced/Widowed	7.7%	7.5%	7.8%
	(0.252)	(0.245)	(0.256)
Business size			
Micro	32.3%	31.6%	34.0%
	(0.381)	(0.381)	(0.386)
Small	25.6%	23.1%	28.6%
	(0.339)	(0.321)	(0.357)
Medium	25.4%	27.2%	23.2%
	(0.335)	(0.342)	(0.322)
Large	15.7%	17.2%	12.8%
	(0.290)	(0.300)	(0.266)
Government	0.4%	0.3%	0.6%
	(0.044)	(0.037)	(0.066)
Sector			
Construction	16.7%	17.6%	15.9%
	(0.333)	(0.348)	(0.315)
Manufacturing	20.3%	23.4%	15.5%
	(0.349)	(0.369)	(0.303)
Commerce	19.7%	20.8%	20.7%
	(0.351)	(0.356)	(0.361)
Services	38.9%	34.0%	43.3%
	(0.426)	(0.419)	(0.422)
Agriculture	0.7%	0.4%	0.9%
	(0.072)	(0.050)	(0.081)

Table 2 (continued)

	Full sample	Job kept sample	Job lost sample
Number of observations	1,524	1,043	481
Weighted number of observations	371,469	248,273	123,196

Source: INEGI (2024)

Note: Summary statistics before the transition from outsourcing. Full sample comprises all formerly outsourced workers (treated observations); 'Job kept' sample consists of workers who remain employed in the same activity, 'Job lost' sample consists of the workers who got hired in another sector. Standard deviations in parentheses

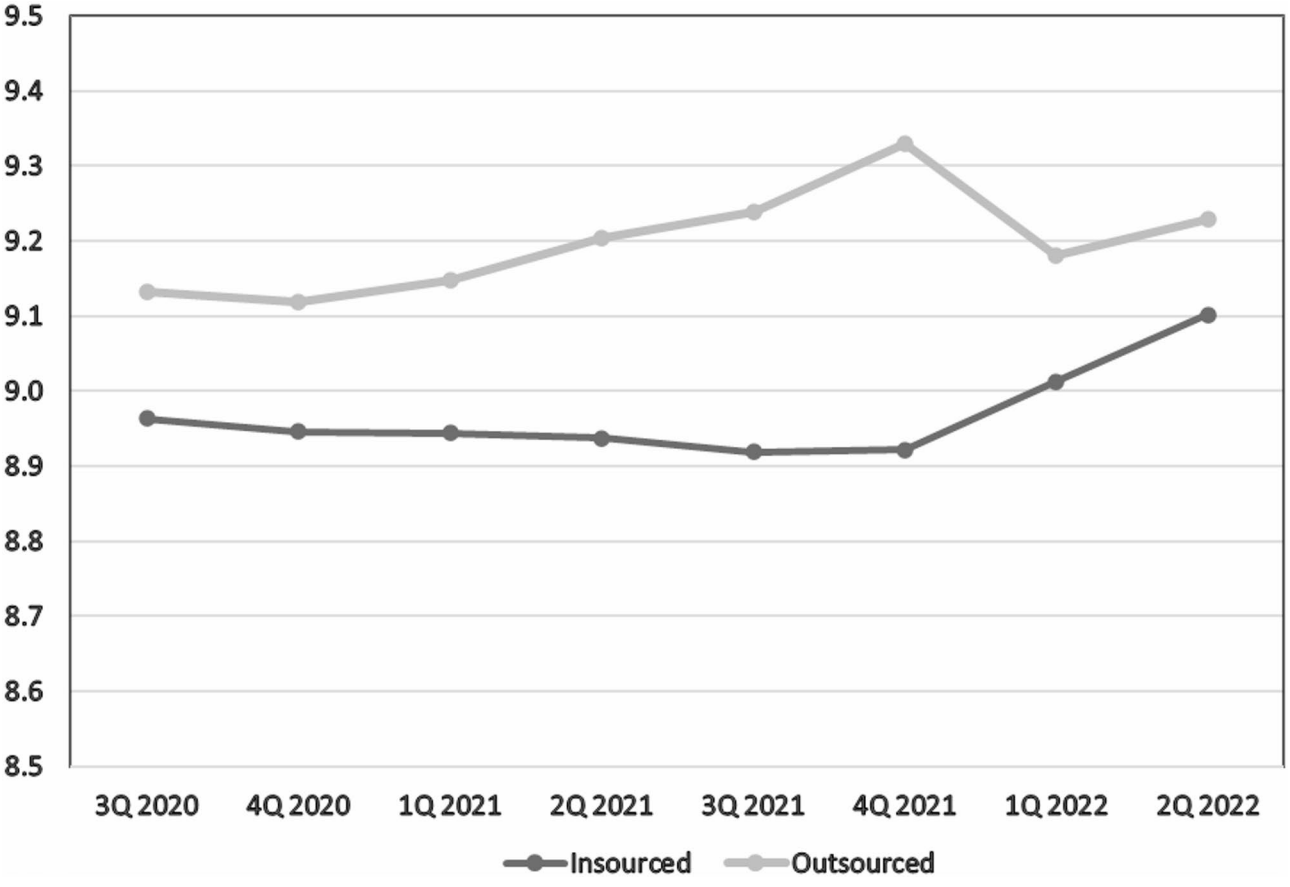

Fig. 2 Conditional wage tendencies of outsourced and insourced workers. Note: Trends in the logarithm of wages for outsourced (treated) and insourced (control group) workers, adjusted for the covariates, before (3Q2020-1Q2021) and after the ban was enacted. Source: INEGI (2024)

where $ATET(g, t)$ is the impact for cohort g at time t as defined above (assuming $\delta = 0$), and $w(g, t)$ represents the weight of cohort or group g at time t, defined by the proportion of observations that belong to that cohort.

4 Results

The difference-in-differences (DID) analysis with multiple time periods and heterogeneous cohort and time effects sheds light on how the outsourcing ban affected labor outcomes. Specifically, it reveals the average differences in wages (Sect. 4.1) and other labor market outcomes (Sect. 4.2) between the previously outsourced workers and those who were directly hired already before the ban. These observed differences can be interpreted as the impact of the outsourcing reform implemented in April 2021.

4.1 Impact on wages

Using the full sample of all workers identified as outsourced when the ban was implemented and transitioned to direct employment during the adjustment period from the second to fourth quarter of 2021, the analysis yielded the impacts on wages reported in Table 3. Significant effects are found across all cohorts. For the early cohort, wages increased by 9.2% in the first quarter of 2021, previous to being incorporated into a firm, followed by 7.4% in the next quarter—the first of the adjustment period—when they joined firm's staff, 17.5% in the third quarter of 2021 and 28.2% in the last quarter. The middle cohort

Table 3 DID coefficients, impacts on wages; full sample

	Early cohort	Middle cohort	Late cohort	All cohorts
Pre-ban period				
4Q 2020	0.081	−0.267	0.121**	
	(0.053)	(0.414)	(0.061)	
1Q 2021	0.092*	0.090	0.071	
	(0.049)	(0.169)	(0.047)	
Adjustment period				
2Q 2021	0.074*	0.169**	0.082**	0.074*
	(0.040)	(0.055)	(0.036)	(0.040)
3Q 2021	0.175**	0.191**	0.186**	0.188**
	(0.083)	(0.053)	(0.049)	(0.047)
4Q 2021	0.282*	0.176**	0.029	0.061**
	(0.146)	(0.082)	(0.030)	(0.029)
Post-ban period				
1Q 2022	0.058	−0.007	0.060	0.048
	(0.132)	(0.074)	(0.060)	(0.048)
2Q 2022	−0.091	0.017	−0.015	−0.015
	(0.152)	(0.081)	(0.045)	(0.039)
All periods	0.100**	0.126**	0.028	0.063**
	(0.049)	(0.043)	(0.029)	(0.022)

Source: INEGI (2024)

Note: ATET over time by cohort in the full sample. Standard deviations in parentheses. 'All periods' represents the ATET across the entire time horizon, while 'All cohorts' depicts the ATET for each quarter with all three cohorts combined

**, *: statistically significant at 5 and 10%, respectively

Table 4 DID coefficients, impacts on wages; 'job kept' sample

	Early cohort	Middle cohort	Late cohort	All cohorts
Pre-ban period				
4Q 2020	−0.120	−0.241	0.083	
	(0.222)	(0.448)	(0.083)	
1Q 2021	0.102**	0.049	0.065	
	(0.051)	(0.258)	(0.057)	
Adjustment period				
2Q 2021	0.076*	0.135**	0.046	0.076*
	(0.041)	(0.064)	(0.041)	(0.041)
3Q 2021	0.108	0.164**	0.213**	0.150**
	(0.076)	(0.057)	(0.055)	(0.048)
4Q 2021	0.368**	0.187**	0.043	0.101**
	(0.129)	(0.076)	(0.034)	(0.032)
Post-ban period				
1Q 2022	0.119	0.059	0.092	0.088
	(0.115)	(0.082)	(0.086)	(0.062)
2Q 2022	−0.025	0.018	−0.005	−0.003
	(0.123)	(0.093)	(0.053)	(0.044)
All periods	0.119**	0.131**	0.046	0.086**
	(0.048)	(0.046)	(0.035)	(0.024)

Source: INEGI (2024)

Note: ATET over time by cohort in the 'job kept' sample. Standard deviations in parentheses. 'All periods' represents the ATET across the entire time horizon, while 'All cohorts' depicts the ATET for each quarter with all three cohorts combined

**, *: statistically significant at 5 and 10%, respectively

witnessed a (statistically insignificant) rise of 9.0% previous to the adjustment period, while they experienced larger and highly significant increases of 16.9%, 19.1%, and 17.6%, respectively, during the adjustment period. For the late cohort there is a significant 12.1% increment in the last quarter of 2020, a year prior to their direct hiring. During the adjustment period, wages rose by 8.2% and 18.6% in the second and third quarters of 2021, the two quarters prior to their incorporation into firm staff in the last quarter of 2021 (also shown in Figure A.1).

The average effect of the ban among all previously outsourced workers is presented in the last column. During the adjustment period it shows a 7.4% increase in the second quarter of 2021, 18.8% in the following quarter, and a 6.1% increment in the final quarter of 2021. After the adjustment period, there are no significant effects. Aggregating the effects over all periods (Table 3, last row), there is an overall wage increase of 10.0% for the early cohort and 12.6% for the middle cohort. However, no significant wage change was observed for the late cohort compared to the control group. Overall, the effect for all workers over the complete time frame is a wage increase of 6.3%.

The analysis for the 'job kept' sample, reported in Table 4, focuses on workers who were employed in the same sector both before and after the policy reform. This provides insights into the impact of the outsourcing ban on workers who, with a certian probability, continued to perform the same job for the same employer. Similar to the full sample analysis, significant wage increases are observed across different cohorts primarily during the adjustment period, but not necessarily when the actual transition was made. In particular, for the early cohort there is a noTable 36.8% increase in wages in the fourth quarter of 2021. Additionally, there is a 10.2% increase in the quarter before they were insourced, followed by a 7.6% augmentation at the time they were hired directly. The middle cohort shows significant wage increases primarily during the adjustment period, with gains of 13.5%, 16.4%, and 18.7%. The late cohort only demonstrates a significant increase of 21.3% in the third quarter of 2021, halfway through the adjustment period (see also Figure A.2).

The average effect for all workers in the 'job kept' sample is concentrated in the adjustment period, with increases of 7.6%, 15.5% and 10.1% from the second to the fourth quarter of 2021, respectively. Aggregating the effects over time reveals larger coefficients compared to the full sample; an 11.9% increase for the early cohort, a 13.1% increase for the middle cohort, and again no significant change for the late cohort. Overall, for all workers directly hired in the same sector where they were outsourced in the pre-ban period, wage increases of 8.6% are found.

Table 5 DID coefficients, impacts on wages; 'job lost' sample

	Early cohort	Middle cohort	Late cohort	All cohorts
Pre-ban period				
4Q 2020	0.157*	−0.082	0.186**	
	(0.080)	(0.245)	(0.079)	
1Q 2021	0.060	0.063	0.077	
	(0.063)	(0.055)	(0.068)	
Adjustment period				
2Q 2021	0.058	0.188**	0.110**	0.058
	(0.051)	(0.052)	(0.053)	(0.051)
3Q 2021	−0.767	0.169**	0.153**	−0.071
	(0.985)	(0.062)	(0.065)	(0.259)
4Q 2021	0.298**	0.099	0.012	0.069*
	(0.116)	(0.069)	(0.042)	(0.035)
Post-ban period				
1Q 2022	0.167	0.042	0.012	0.039
	(0.111)	(0.079)	(0.055)	(0.043)
2Q 2022	0.232	0.020	−0.023	0.025
	(0.889)	(0.084)	(0.066)	(0.125)
All periods	−0.049	0.104**	0.004	0.027
	(0.211)	(0.050)	(0.038)	(0.055)

Source: INEGI (2024)

Note: ATET over time by cohort in the 'job lost' sample. Standard deviations in parentheses. 'All periods' represents the ATET across the entire time horizon, while 'All cohorts' depicts the ATET for each quarter with all three cohorts combined

**, *: statistically significant at 5 and 10%, respectively

Complementing the 'job kept' sample, the 'job lost' group includes individuals who reported working in a different sector after exiting outsourced employment. Also in this group, significant wage effects also appear mainly during the adjustment period (Table 5). For the early cohort, wages rose by 15.7% in the last quarter of 2020 and by 29.8% in the fourth quarter of 2021. The middle cohort experienced significant gains of 18.8% and 16.9% in the second and third quarters of 2021, respectively. The late cohort saw increases of 11.0% and 15.3% in those same periods (see also Figure A.3). Aggregating all cohorts, a significant wage increase of 6.9% is observed in the fourth quarter of 2021. Over the entire observation period, only the middle cohort experienced a significant cumulative wage increase (10.4%). In contrast with the full sample and the 'job kept' group, there was no consistent overall wage gain for the 'job lost' group when considered as a whole. This suggests that the observed increases were concentrated in certain cohorts and timeframes, rather than being broadly experienced across all workers who changed sector.

We terminate the analysis of wage impacts by examining how wages evolved over time relative to the moment when workers were hired directly—capturing the effect of exposure duration. In the full sample, wages increased significantly in the lead-up to direct hiring: by 12.1% four quarters before, and by 16.5% in the quarter immediately prior. At the moment of insourcing, wages rose by 7.5%. No significant effects were observed thereafter (see Fig. 3 and Appendix Table A.1). It is important to recall that this sample consists of workers who stayed in the same sector and others who found employment in another sector. Disaggregating, we find different patterns. In the 'job kept' sample, workers retained by the same employer saw wage increases of 16.8% one quarter before insourcing, 8.2% at the moment of absorption, and a further 11.7% in the following quarter. In contrast, in the 'job lost' sample,

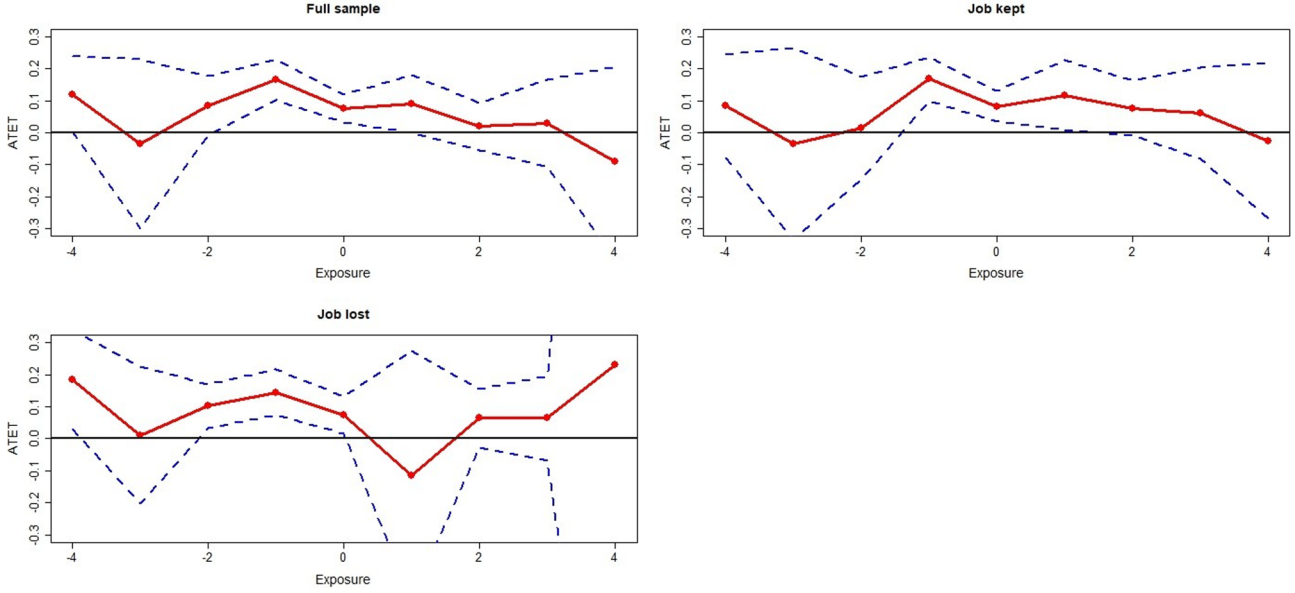

Fig. 3 DID coefficients: Dynamic effect. Note: ATET by duration of exposure, with 95% confidence interval, in each sample. Exposure time 0 is when the outsourced worker is directly hired. Source: INEGI (2024)

Table 6 DID coefficients, impacts on formality and other labor rights' coverage; full sample

	Formality	Bonus	Vacations	Profit-sharing
Adjustment period				
2Q 2021	0.159**	0.166**	0.155**	0.092**
	(0.027)	(0.033)	(0.038)	(0.046)
3Q 2021	0.235**	0.235**	0.203**	0.113**
	(0.037)	(0.038)	(0.044)	(0.046)
4Q 2021	0.097**	0.069**	0.081**	0.048
	(0.023)	(0.024)	(0.026)	(0.030)
Post-ban period				
1Q 2022	0.001	0.009	−0.015	0.026
	(0.034)	(0.034)	(0.039)	(0.038)
2Q 2022	−0.081**	−0.084**	−0.107**	−0.057
	(0.032)	(0.032)	(0.035)	(0.037)
All periods	0.067**	0.060**	0.048**	0.037*
	(0.017)	(0.017)	(0.019)	(0.021)

Note: ATET over time in the full sample. Standard deviations in parentheses. 'All periods' represents the ATET across the entire time horizon

**, *: statistically significant at 5 and 10%, respectively

Table 7 DID coefficients, impacts on labor rights; middle cohort of the 'job kept' sample

	Formality	Bonus	Vacations	Profit-sharing
Pre-ban period				
4Q 2020	−0.058	0.191	0.162	−0.219
	(0.284)	(0.309)	(0.357)	(0.395)
1Q 2021	−0.124	0.003	0.049	0.033
	(0.197)	(0.197)	(0.195)	(0.221)
Adjustment period				
2Q 2021	0.152**	0.070	0.153**	0.057
	(0.053)	(0.053)	(0.055)	(0.068)
3Q 2021	0.246**	0.234**	0.202**	0.155**
	(0.046)	(0.050)	(0.057)	(0.059)
4Q 2021	0.026	−0.037	0.030	−0.008
	(0.058)	(0.061)	(0.060)	(0.071)
Post-ban period				
1Q 2022	−0.061	−0.062	−0.078	−0.054
	(0.066)	(0.069)	(0.072)	(0.070)
2Q 2022	−0.212*	−0.117	−0.080	0.005
	(0.110)	(0.081)	(0.078)	(0.082)
All periods	0.079**	0.072*	0.075*	0.061
	(0.036)	(0.040)	(0.041)	(0.045)

Source: INEGI (2024)

Note: ATET over time for the middle cohort of the 'job kept' sample. Standard deviations in parentheses. 'All periods' represents the ATET across the entire time horizon

**, *: statistically significant at 5 and 10%, respectively

wage increases occurred earlier and were more spread out: 18.6% four quarters before direct hiring, 10.2% two quarters before, 14.5% one quarter before, and 7.3% at the time of insourcing. As with the full sample, no significant changes followed after direct hiring. These results suggest that wage gains for outsourced workers were concentrated around the transition into direct employment, with short-lived effects that varied in timing and intensity depending on whether workers stayed with the same employer or found work elsewhere.

4.2 Impacts on labor rights

Although the earned wages are a core labor market outcome, in the Mexican setting with high levels of informality and incompliance with legal dispositions, it is also relevant to analyze whether the outsourcing ban has had positive effects on other labor rights. In particular, it is important to examine if the newly directly hired workers have acquired formal-sector jobs that provide social security rights and accompanying benefits.

When examining the impact of the outsourcing ban on formality using the full sample (Table 6, first column; see also Figure A.4), a significant rise of formality is observed for previously outsourced workers during the adjustment period, amounting to a 15.9% increase in the second quarter of 2021, 23.5% in the third quarter, and 9.7% in the fourth quarter, followed by an 8.1% reduction in the second quarter of 2022. Overall, formality among previously outsourced workers increased by 6.7% by the end of the period.

Similar trends are observed for the annual bonus and vacation benefits, as expected given that these secondary benefits are more common in a formal employment setting. The percentage of formerly outsourced workers reporting they received the end-of-year bonus increased by 16.6% in the second quarter of 2021, 23.5% in the third quarter, and 6.9% in the fourth quarter, with a subsequent 8.4% reduction, while the overall increase was 6.0% (Table 6, col. 2). For paid vacations, the increases were 15.5%, 20.3%, and 8.1%, respectively, in each quarter during the adjustment period, with a 10.7% reduction in the second quarter of 2022, giving rise to an overall increase equal to 4.8% (Table 6, col. 3). Regarding profit sharing, significant effects are found in the second and third quarters of 2021, with increases of 9.2% and 11.3%, respectively, without subsequent reductions and no significant effect over the entire study period (Table 6, col. 4).

As noted above, the full sample includes workers who changed their sector of employment and thus their workplace, which may explain the unexpected negative effects observed for formality, bonuses, and paid vacations. To isolate these effects, Table 7 presents the changes in coverage for the middle cohort of the 'job kept' sample, that is, workers who were probably insourced by the same employer for whom they were working before. Among these workers, significantly positive effects are observed primarily during the adjustment period around the time they got hired directly. Formal employment increased by 15.2% and 24.6% in the second and third quarters of 2021, respectively, although the negative effect remains (now only significant at 10%) in the second quarter of

2022. Vacation coverage rose by 15.3% and 20.2% in the same periods, without significant effects afterwards. For bonuses and profit sharing, there were increases of 23.4% and 15.5% in the third quarter, during their transition to direct employment. The overall effect on formality, bonus, and vacation coverage is higher in this cohort (Table 7, last row), highlighting that the changes were more substantial than what the full sample suggests.

5 Discussion

Our results provide strong evidence that the ban on outsourcing implemented in Mexico in April 2021 had significantly positive effects on previously outsourced workers. In particular, we found that it led to wage increases ranging from 10.0 to 12.6% for the early and middle transition cohorts compared to their counterparts who were already hired directly before the ban. This effect was even slightly stronger for those who remained in the same workplace, experiencing wage increases of 11.9–13.1%. For those who got hired in-house only late during the adjustment or transition period, the wage benefits were smaller and even insignificant, resulting in overall impacts of 6.3% in the full sample, and 8.6% for those who stayed in the same sector. No overall significant effect is found for those who got hired directly in another sector of the economy, but also in this group the middle cohort managed to gain on average a 10.4% wage increase, comparable to those who got hired in the same workplace as before.

The finding that wage earnings increased after outsourcing was declared unlawful is consistent with theoretical predictions and empirical findings of a wage penalty for outsourced workers as documented—though often for specific sectors or occupations—in the international literature. Regarding Mexico's outsourcing ban, our findings align with the descriptive analysis reported by IMSS (2022), indicating a similar proportion of workers retaining their positions and experiencing wage increases. Moreover, our findings are consistent with Estefan et al. (2024) who analyze the ban's impacts for the manufacturing sector using firm-level data instead of worker-level observations, reporting wage growth between 10 and 35% for different time horizons. The larger impacts are found when analyzing short-term data, but similar to our case, the effects seem to level out in the longer run. On the other hand, our wage impacts are greater than Casco et al. (2024)'s, who find a wage rise of 3 to 4% for previously outsourced workers moving in bulk to a regular position. For 'empty establishments' that had outsourced all their workforce, Colonna and Aldeco (2024) report small negative wage effects—but an improved total remuneration due to better compliance with profit-sharing regulations.

A potential explanation for the varied impacts reported in the literature is the difficulty in correctly identifying outsourced workers. Due to the lack of explicit information on outsourcing status—both in our worker-level data and in the firm-level data sets used by Estefan et al. (2024), Colonna and Aldeco (2024), and Casco et al. (2024)—all studies rely on assumptions, making some degree of misclassification inevitable. It is not evident which approximation method yields the most accurate classification. One advantage of using representative worker-level data is that we are not restricted to particular occupations (as is often the case in studies for other countries). In addition, this approach allows us to examine the impacts not only on those rehired by their previous employer, but also on workers who transitioned to direct employment in a different sector—capturing broader labor market dynamics triggered by the reform. To approximate whether a worker retained their job, we used continued employment in the same sector as a proxy. While this may not be perfectly accurate, firm-level analyses face a similar limitation, as there is no guarantee that the same individuals remain employed within observed firms across time. A potential limitation of our approach is that it includes only outsourced workers who transitioned to formal direct employment; those who dropped out of the formal labor market are not part of our sample. We consider this a minor issue, as there is no evidence of increased unemployment or informality during the period under study.

Regarding the heterogeneity of impacts by transition timing, we consider that the middle cohort—which showed the strongest wage increases—best represents the 'true' or 'pure' effect of the outsourcing ban. Unlike early or late transitioners, workers in this group were likely insourced as a direct response to the policy, rather than due to other (strategic) motives. This cohort tends to be older and more educated, suggesting that they are experienced, valuable workers. At the same time, their characteristics imply they are less likely to switch jobs swiftly, making them attractive candidates for regularization by employers. Additionally, they more often worked in smaller firms, where keeping a high-performing worker may be especially important, and where workers themselves may feel a stronger connection to the business. The early cohort also exhibited strong wage gains, though for different reasons. These workers were, on average, younger, more frequently single, and less educated —but often employed full-time. Their profiles suggest they may have been more likely to consider offers from competing firms if not regularized quickly. As such, employers may have opted for early direct hiring to retain the most promising among them. Many of these workers were already operating in formal settings through

subcontractors, and their employment status was rapidly adjusted following the reform.

In contrast, the late cohort showed weaker impacts. Their average characteristics fall in between the other two groups, suggesting less urgency for employers to act. They were more commonly employed in large firms and in the manufacturing sector, where decision-making processes might be slower and follow more standardized procedures. Interestingly, this group experienced wage increases well before formal insourcing and again during the adjustment period, possibly reflecting company-wide compensation practices not directly tied to individual-level regularization. This pattern supports the idea that large enterprises implemented the reform more mechanically, with less/targeted responses to individual-specific worker profiles.

Generally lower to no significant effects were observed for workers who transitioned to other activities, except, interestingly, precisely for the middle cohort, with characteristics of which we alleged they are valuable for their pre-ban employers. We not only found that this cohort showed the largest wage gains when maintaining their workplace, but also that they are best able to capitalize on their capacities by acquiring higher wages when moving to another sector. In contrast, the early cohort, with characteristics indicating more flexibility to develop other activities, did not immediately earn more when moving to another sector. We interpret this as, in case workers had shown promising performance in their pre-ban jobs, they were more likely to get rewarded with an in-house contract, but if not, they had to leave and find a job elsewhere without clear prospects to demand a better payment.

For the early cohort we find some evidence, especially in the sample that maintained their job, that shortly before the formal enactment and implementation of the new policies regarding outsourcing—that is, while the law was presented and discussed in parliament—firms already began increasing wages. Our interpretation is that this occurred because firms were conscious of potential workforce losses in case workers—aware of their increased bargaining power—would seek alternative job opportunities if not rewarded with a good proposal by their contractor. We consider that strategic behavior aimed at finding the best opportunities and the best matches was an important factor for both employers and employees, especially regarding the most flexible and promising outsourced workers.

Summarizing, it is evident that the outsourcing ban did not impact the wages of all previously outsourced workers equally, and that those who retained their positions, particularly among the earlier cohorts, experienced the greatest benefits. Although difficult to compare directly due to differences in data and identification strategies, the relevance of heterogenous impacts is also noted by Casco et al. (2024), who find that employees who stayed with the same employer observed wage rises earlier than those who were reported to work at another firm after the reform. Heterogeneity by level of education, suggesting that companies put more effort to directly hire or pay more their better-qualified workers, is in line with the finding of larger wage penalties among the low-skilled by Gürer and Taymaz (2025) for Türkiye and Rojas García and Van Gameren (2021) for Mexico in the pre-ban environment. Our findings however are strikingly different from Jiménez and Rendon (2025), who provide evidence that a similar (but still legally challenged) restriction on outsourcing in Peru did not lead to meaningful enhancements of the labor market conditions of affected workers.

In addition to the impacts on wages, there was an increase in the proportion of formally employed workers and more in general of compliance with other legally established labor rights. This could have been expected, due to the amplification of inspection and revision activities mandated for firms engaged in outsourcing services. These findings are in line with Colonna and Aldeco (2024), who find that (mandatory) profit sharing increased after outsourcing was banned, a finding they connect with a reduced prevalence of avoidance of legal obligations that outsourcing had enabled. Casco et al. (2024) find evidence that previously outsourced workers remain longer in formal employment than workers who were not affected by the ban. The negative impacts that we find for some labor rights in the full sample after the termination of the adjustment period may be due to the control group catching up and claiming rights—often not complied before in the broader context of the Mexican labor market (Levy 2018)—that the previously outsourced workers acquired in large numbers during the adjustment period. Moreover, especially the early cohort started from an already relatively favorable pre-ban formalization of their (outsourced) jobs. Our interpretation is that the decisions whom to offer an in-house contract after the outsourcing ban involved many considerations related not only to maintaining the best-qualified previously outsourced workers but also to keeping content the earlier-insourced staff regarding compliance with labor regulations.

In general, the stronger effects on various outcomes found in the middle cohort consisting of workers with the most valuable characteristics provide evidence for strategic behavior related to the outsourcing ban's implementation. That is also our interpretation for the wage increases in some groups immediately before the actual legal implementation. Furthermore, the analysis of the impacts related to the duration of exposure, where particularly those who maintained their jobs witnessed wage increases not only at the moment of insourcing

but also in subsequent quarters, suggest that the best-qualified workers benefitted most. In contrast, those who moved to another sector observed rising wages before the policy implementation, perhaps making them relatively expensive to be insourced by the firm where they were employed, but also resulting in a relatively small and unrepeated increase when finding a job elsewhere. Taken together, it all suggests that employers and employees strategically adapted to the new legal requirements, ensuring that the most valuable workers would be convinced to stay while workers with a less-appreciable background only made the transition late and with less pronounced improvements of their labor conditions.

Despite the limitations of our study, it provides a straightforward visualization of how and when the outsourcing ban affected workers. It also distinguishes groups with different characteristics, and differentiates from previous studies by focusing on representative individual worker-level data rather than firm averages, offering more detailed insights into worker outcomes for a broad share of the labor market. For future research, it would be great to have access to more detailed panel data that would enable to more precisely identify outsourced workers, track the exact timing of transition to direct hiring, and determine their precise activities. In addition, it would be interesting to have information regarding the order of compliance with the regulations stipulated by the Federal Labor Law and with the social security obligations, such as the registered wages (that count towards retirement pensions, among other things) and the actual remunerations, and the stability of the formal labor relations. Further research, moreover, is needed to explore other dimensions of the reform—such as its influence on incentives for productivity-enhancing investments and its longer-term impacts on labor market outcomes and economic growth.

6 Conclusion

The analysis presented in this paper provides robust evidence that the 2021 outsourcing ban in Mexico led to substantial wage increases for workers who were previously outsourced, particularly those who transitioned to direct employment with their original employers. Wage gains ranged from 10 to 13% for workers who made this transition in the first two quarters of the adjustment period mandated by the reform, while those who transitioned later experienced negligible effects—resulting in an overall average increase of 6%. Among workers who retained their jobs, the average wage increase was slightly higher, at 8.6%. In addition to wage gains, there was also an increase in access to formal employment benefits mandated by law, such as end-of-year bonuses, paid vacations, and profit-sharing. These findings suggest that the reform had a broadly positive impact on the labor conditions of the affected workforce.

The timing of the wage increases—mostly concentrated around the transition from outsourcing to direct hiring—suggests that workers' proven performance and their potential value to other employers, as well as considerations of internal equity, played a key role in driving these changes. Anticipating potential staff losses due to the reform, firms proactively raised wages to retain their most valuable employees. Once integrated into the firm's workforce, these workers often received further wage adjustments to ensure parity with existing staff and avoid that they would seek to capitalize their capacities elsewhere. However, wage increases and improvements in labor rights did not always coincide precisely with the moment of direct hiring. Instead, they may have been linked to predetermined evaluation or contract renewal periods within firms.

Taken together, the findings provide clear evidence of the outsourcing ban's positive impact on the labor outcomes of previously subcontracted workers in Mexico. Overall, the findings underscore the importance of policy interventions focused on worker protection as a means to influence labor market dynamics and promote more equitable outcomes. By examining the effects of the outsourcing ban, this study enhances our understanding of how regulatory changes can affect wage trajectories and employment patterns in the modern economy, while highlighting the complexities of the dynamics underlying employer–employee decision-making. Further research on these short-term outcomes and their implications for the long-term development of the labor market and the economy in general is needed.

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Data availability
The data used for the analysis are available at Mexico's National Statistics Institute (INEGI), and can be freely downloaded from https://www.inegi.org.mx/programas/enoe/15ymas/#microdatos.

Declarations

Competing interests
The authors declare that they have no competing interests.

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