Worker- and Firm-Level Effects of an Outsourcing Ban*

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Abstract

In December 2017, the government of Türkiye announced a comprehensive ban on the procurement of outsourced services by public institutions and mandated that all workers providing such services be transitioned into permanent public positions within six months. We study the labor-market consequences of this abrupt and large-scale policy change using an administrative, linked employer–employee dataset. We find that workers who transitioned into public employment experienced higher wages and improved job security. At the firm level, private service providers with greater exposure to the reform faced sharp employment contractions, higher exit rates, and declines in productivity and profitability. In contrast, municipal-owned enterprises that internalized service provision became more productive and more profitable. We also document modest positive spillovers in local labor markets, including wage gains and reduced turnover among low-skilled private-sector workers. Overall, our results highlight that the outsourcing ban triggered a reallocation of rents away from private service providers toward workers and public employers.

Keywords: public employment; outsourcing reform; labor market spillovers; firm dynamics; productivity
JEL Classification: J31, J38, J62, L33

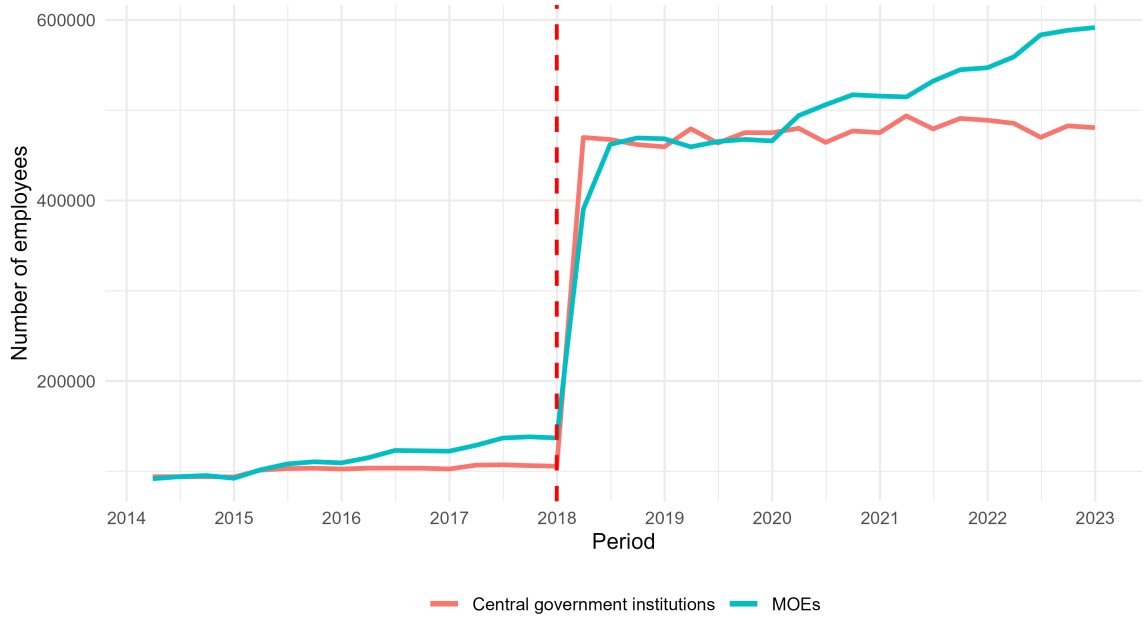
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1 Introduction

In December 2017, six months before a highly anticipated presidential election, the Turkish government announced a ban on public-sector outsourcing. Under the new policy, all outsourced workers providing services to the public sector were transferred into permanent public positions within the following six months. The reform directly affected workers amounting to approximately 3.2% of the total labor force and increased permanent public worker employment (excluding public servants) by almost 200% as seen in Figure 1. Given its scale and abrupt implementation, the reform can be viewed as a significant policy experiment. Its effects are important not only for Türkiye, but also for the general lessons they may provide regarding the implications of domestic outsourcing.

Figure 1: Number of workers in municipal companies and central government institutions



We exploit Türkiye's large-scale, government-led outsourcing reform using administrative linked employer–employee data to study the labor market effects of outsourcing or, equivalently, its prohibition. Our contribution is along several dimensions. First, the scale and abrupt implementation of this policy is unique and allows us to estimate its effects with a high degree of precision, both for the overall population of affected workers and firms, and across a set of demographic subgroups.

Second, in addition to worker-level outcomes, we examine the effects of the policy on firm-level outcomes, including survival, productivity, and profitability, for firms that previously provided outsourced services to the public sector and municipally owned firms which previously received outsourced services. To our knowledge, there is limited evidence on

the implications of domestic outsourcing for firm-level outcomes;[1] this study helps fill this gap by examining a rich set of firm-level variables. Third, we also study indirect effects of the reform, i.e., potential spillover effects on heavily exposed occupations which is also not common in the literature.[2] Taken together, our analysis adds to the literature by offering a comprehensive assessment of the labor market effects of a large-scale policy experiment based on administrative records.

For our empirical analysis, we use the Entrepreneurship Information System (EIS), an employer–employee linked dataset constructed from administrative records and maintained by the Ministry of Industry and Technology of the Republic of Türkiye. The dataset covers all the registered firms and workers and allows us to track employment relationships over time. As part of the 2017 reform, all outsourced workers providing services to municipal-owned enterprises (MOEs) and public institutions affiliated with the central government were granted permanent public positions. However, the EIS data allow us to directly observe MOEs but not central government institutions. Consequently, our analysis focuses on MOEs and the connected set of workers and private firms that previously supplied outsourced services to them.

We study the labor-market consequences of the outsourcing ban along three dimensions: worker outcomes, firm dynamics, and local labor-market spillovers. At the worker level, we analyze wage and turnover responses of workers who transitioned into permanent public positions following the reform. At the firm level, we examine how the ban affected the survival, employment, productivity, and profitability of private firms that had previously supplied outsourced services to the public sector, as well as the performance of municipal-owned enterprises (MOEs) that internalized these activities. Finally, to capture broader equilibrium effects, we study spillovers in local labor markets (LLMs) by relating outcomes of non-treated workers to the local intensity of transitions into permanent public employment across province–occupation cells.

The results point to considerable gains for workers who moved into permanent public employment. Treated workers experienced wage increases on the order of 10–15 percent, with especially pronounced effects among male workers, alongside sharp and sustained declines in turnover. These patterns indicate sizable improvements in earnings and job stability for a predominantly low-skilled workforce.

Firm-level responses, by contrast, reveal a pronounced reallocation of economic activity. Private firms with greater exposure to the reform experienced substantial contractions in employment and significantly higher exit probabilities: for example, a 10 percent employment exposure to MOE contracts increased exit risk by roughly 12 percent and

[1] Some evidence on the firm-level effects of outsourcing can be found in Bilal and Lhuillier (2021) and Estefan et al. (2024).

[2] A notable exception is Felix and Wong (2025).

reduced expected firm size by about 26 percent. Moreover, productivity and profitability of more exposed firms deteriorated markedly after the reform, suggesting that public outsourcing contracts were valuable and difficult to replace. In contrast, MOEs that internalized service provision became more productive and more profitable following the reform.

Beyond directly affected firms and workers, we also find modest but meaningful local labor-market spillovers, including wage gains of around 1–2 percent and reductions in turnover among low-skilled private-sector workers in more exposed province–occupation cells.

Overall, the findings suggest that the reform involved meaningful welfare trade-offs. While higher wages, greater job stability for low-skilled workers, and improved performance of public entities point toward welfare gains in an inequality-averse setting, the contraction of outsourced service firms and potential increases in private-sector input costs highlight offsetting efficiency losses.

Related literature. Recovering causal estimates of the labor market effects of domestic outsourcing is challenging. In most datasets, workers performing outsourced tasks are not observable. Even when such workers are identified based on a set of assumptions, selection into outsourced work complicates the causal interpretation of its labor market effects. To address these challenges, the literature on domestic outsourcing has employed several empirical strategies. These include classifying workers in specific industries or occupations as outsourced, applying pre-determined criteria to worker flows to identify on-site outsourcing events within firms, and exploiting policy-induced variation, as we do.

Accordingly, studies that exploit policy-induced variation (Estefan et al., 2024; Moreno-Contreras and van Gameren, 2025; Jiménez and Rendon, 2025; Felix and Wong, 2025) are the most closely related to the present paper. Jiménez and Rendon (2025) examine the outsourcing restrictions introduced in Peru in 2022 and study their effects on workers' wages and employment status using survey data, finding no significant short-run impacts. Estefan et al. (2024) use Mexican manufacturing survey data covering the period 2013–2023 and exploit Mexico's outsourcing ban of 2021. They find that the ban increased wages, reduced manufacturing firms' investment, and increased their exit probability. Moreno-Contreras and van Gameren (2025) utilize a different survey dataset covering the period 2020–2022 and explore the worker-level effects of Mexico's outsourcing ban. They find that the ban increased wages, formality rates, and labor rights coverage.

Our study differs from Estefan et al. (2024), Moreno-Contreras and van Gameren (2025) and Jiménez and Rendon (2025) along several important dimensions. First, the reform we exploit applies to the public sector and was implemented in a centralized and mandatory manner, leaving little scope for non-compliance. Second, the Turkish reform was enacted in

2017, which allows us to examine not only short-run but also longer-run effects. Third, we use administrative linked employer–employee records, different then these studies which use survey data. Finally, while Estefan et al. (2024) also considers firm-level responses, we complement this evidence by examining a broad set of firm-level outcomes and by explicitly studying spillover effects across occupations.

Felix and Wong (2025) exploit the legalization of outsourcing in Brazil in 1993. Their main empirical strategy relies on comparing labor market outcomes for security guards with those of less affected occupations. They find that outsourcing legalization increased employment among younger security guards, while reducing employment among older guards. The authors also examine spillover effects of outsourcing legalization on firms that did not adopt outsourcing. Our study differs from Felix and Wong (2025) in that we do not focus on a single occupation, but instead examine all occupations that were outsourced prior to the reform. In addition, we analyze firm-level outcomes of the policy.

Another empirical strategy for studying the labor market implications of domestic outsourcing, pioneered by Goldschmidt and Schmieder (2017), focuses on identifying on-site outsourcing events. Dorn et al. (2018) and Gürer and Taymaz (2026) provide further applications of this approach. Both Goldschmidt and Schmieder (2017) and Dorn et al. (2018) find that on-site outsourcing of low-skilled occupations, such as food services, cleaning, security, and logistics, leads to wage reductions for outsourced workers. In Gürer and Taymaz (2026), we use the same administrative data as in the present study but exploit additional information from buyer–supplier transaction records, which allows us to examine high-skilled domestic outsourcing as well. We find that while low-skilled outsourced workers experience wage losses, high-skilled outsourced workers experience wage gains.

This approach relies on strong assumptions regarding worker flows in order to credibly identify on-site outsourcing events. These assumptions often lead to considerable reductions in sample size. Moreover, outsourcing does not necessarily occur on-site. Firms may directly contract with business service providers, or on-site outsourcing may predate the sample period. In either case, the absence of observable worker flows implies that such outsourced workers are not identified by the on-site outsourcing approach.

A number of other studies employ various methods to examine the labor market effects of outsourcing, with worker-level effects as the primary outcome of interest. A brief review of this literature is provided below.

Using U.S. Current Population Survey data, Dube and Kaplan (2010) estimate fixed effects models and document negative wage effects for janitors and security guards employed by business service firms. Katz and Krueger (2019), implementing their own survey to the 2015 Rand American Life Panel, show that workers engaged in alternative work

arrangements, such as temporary help employment, earn lower wages than similar workers in standard employment relationships.

Bilal and Lhuillier (2021) use French data and an AKM framework to study the wage effects of outsourcing, documenting a wage penalty that is consistent with earlier findings in the literature. They also provide evidence that firms engaging in outsourcing experience productivity gains. Leveraging a unique feature of Argentine administrative data, Drenik et al. (2023) are able to distinguish between regular and outsourced workers. Also using an AKM strategy, they find that high-wage firms pay a lower wage premium to outsourced workers than to regular workers.

Spitze (2022) relies on self-reported outsourcing status from the NLSY1979 and applies fixed effects regressions to analyze wage and employment outcomes. The findings point to wage losses among low-skilled workers, statistically insignificant gains for high-skilled workers, and an overall increase in employment linked to the possibility of outsourcing. Guo et al. (2024) use administrative data from Brazil and identify outsourced workers based on industry and occupation codes, focusing on cleaners and security guards. They study the wage and employment security effects of domestic outsourcing and find a small wage penalty associated with outsourcing, alongside, surprisingly, higher employment security for outsourced workers.

Bergeaud et al. (2025) study the relationship between broadband internet access and outsourcing expenditures, showing that improved internet connectivity increases firms' outsourcing expenditures. To examine wage effects, they classify workers moving into business service industries as outsourced and find wage losses for low-skilled workers and wage gains for high-skilled workers.

Evidently, the literature on the labor market effects of domestic outsourcing has primarily focused on worker-level outcomes, such as wages and employment, with a few exceptions. As noted earlier, we contribute to this growing literature by exploiting a large-scale, government-led policy experiment. The scale of the reform provides sufficient statistical power to examine worker-level effects across sub-demographic groups. In addition, we go beyond existing work by analyzing firm-level outcomes and by exploring potential spillover effects of the policy.

The remainder of this paper is organized as follows. Section 2 provides institutional background on public-sector outsourcing in Türkiye and describes the 2018 outsourcing ban. Section 3 introduces the administrative data sources and presents descriptive statistics. Section 4 examines the effects of the reform on worker-level outcomes, focusing on wages and job turnover. Section 5 analyzes firm-level responses, including firm survival, employment, productivity, and profitability, for both outsourcing firms and municipal-owned enterprises. Section 6 investigates spillover effects on wages and turnover in local

labor markets. Section 7 provides a commentary on the welfare effects of the outsourcing ban, discussing the distributional implications of the reform across workers, firms, and local labor markets, and synthesizing the evidence from the preceding analyses. Section 8 concludes.

2 Background

For many years, the use of outsourced workers ("*taşeron*" in Turkish) in the public sector in Türkiye was a source of conflict between the government and labor unions. From the government's perspective, outsourcing offered flexibility and cost savings, particularly for non-core functions such as cleaning and security services, which were widely contracted out through subcontracting arrangements. Outsourced workers, however, argued that both their wages and job security were substantially lower than workers holding permanent public positions performing comparable tasks.

For instance, in public universities, some of the outsourced cleaning staff were reportedly dismissed during the summer recess and re-hired at the beginning of the fall semester, a practice aimed at avoiding the payment of severance pay (a legally mandated compensation paid to workers upon involuntary separation). Against this background, outsourced workers and labor unions increasingly demanded an end to public-sector outsourcing, invoking the principle of equal pay for equal work.

Outsourced workers attained their objective in the period leading up to the June 2018 presidential election, which was widely expected to be highly competitive. Towards the end of December 2017, approximately six months before the elections, the government announced that all outsourced workers providing services to the public sector as of December 4, 2017 would be granted permanent public employment.[3] The regulation covered not only outsourced workers employed by the central government and its affiliated institutions (such as hospitals, schools, universities, ministries, and other public offices) but also those working for municipalities and municipally owned enterprises (MOEs). State-owned enterprises constituted the only exclusion from this arrangement. The transition of outsourced workers into permanent public positions was expected to be gradually completed by June 2018.

Prior to this decision, outsourcing and subcontracting arrangements were widespread in the public sector in Türkiye. According to public employment statistics published by the Republic of Türkiye Presidential Strategy and Budget Directorate, total permanent public worker employment (public servants excluded) was roughly 385 thousand by December 2017, while the overall labor force was approximately 31.5 million individuals, according

[3]Decree Law No. 696, Official Gazette, December 24, 2017, Issue 30280, Article 127. Available in Turkish at: https://www.resmigazete.gov.tr/eskiler/2017/12/20171224-22.htm

to the TurkStat Labor Force Statistics. According to an official infographic released by the Ministry of Labour and Social Security at the time of the reform, it was announced that approximately 1.02 million outsourced workers would be transferred to permanent public positions.

There are no official statistics documenting the exact number of outsourced workers who ultimately transitioned into permanent public employment. However, anecdotal evidence suggests that this figure was approximately 750,000 workers. The remaining workers did not obtain permanent positions for a variety of reasons, including retirement, failure to pass security screenings, or voluntary exit from employment.[4]

Using administrative records, described in the next section, we also attempt to estimate the approximate number of workers who transitioned into permanent public employment. Figure 1 plots employment levels in the central government and MOEs as identified in these administrative data. The sharp increase in employment observed in the first two quarters of 2018 amounts to approximately 750,000 workers, a figure broadly consistent with the available anecdotal evidence.

Evidently, the public outsourcing ban in Türkiye constituted a large-scale government intervention. It directly affected approximately 3.2% of the labor force. The number of workers transitioned into permanent public positions was almost 200% of pre-existing permanent public worker employment.

3 Data and descriptives

3.1 Entrepreneurship Information System (EIS)

Our main dataset is the Entrepreneurship Information System (EIS), an administrative matched employer–employee dataset covering the period 2006-2023 maintained by the Republic of Türkiye Ministry of Industry and Technology and made available to researchers through its secure data center. EIS brings together several administrative sub-datasets collected by different public institutions within the scope of Türkiye's Official Statistics Programme.[5] Owing to its comprehensive coverage and administrative nature, EIS currently constitutes the most reliable data source for analyzing labor market dynamics in Türkiye.

For the purposes of this study, we utilize three sub-datasets contained in EIS. The first is the worker registers, collected by the Social Security Institution. These registers cover

[4] According to information obtained by Confederation of Progressive Trade Unions of Turkey (DİSK) through a formal request submitted to Presidential Communication Center of Turkey (CİMER), Social Security Institute (SGK) records indicate that 744,342 outsourced workers were transferred to permanent public-sector employment under Decree Law No. 696, while approximately 275,000 workers were excluded from the transition. Available in Turkish at: https://www.birgun.net/makale/kamuda-kac-taseron-isci-kadroya-alindi-236728

[5] See https://www.resmiistatistik.gov.tr/ for further information on the programme.

all formally registered workers in Türkiye and are available for March, June, September, and December of each year.[6] The worker-level information includes age, gender, monthly earnings, and the number of days worked. While worker registers are available for the period 2006–2023, unique worker identifiers, which allow us to track individuals and their employers over time, are only available from 2012 onwards.

The second sub-dataset is the firm registers, primarily compiled by the Turkish Statistical Institute. This dataset provides detailed firm-level information, including firm age, headquarters location, and the firm's main economic activity classified at the NACE Rev.2 four-digit level. The firm registers do not directly report whether a given firm is a municipal-owned enterprise (MOE). The data center granted us the opportunity to identify municipal-owned enterprises through their tax identification numbers.[7] We manually compiled a list of tax identifiers corresponding to 1811 municipal-owned enterprises. The data center officials then matched these tax identifiers to firm identifiers in the firm registers and provided us with a binary indicator identifying whether a firm in the dataset is a municipal-owned enterprise.

The third sub-dataset we use is the balance sheet data, collected by the Ministry of Treasury and Finance. This dataset contains detailed information on firms' financial statements, including revenues, costs, capital stocks, and investment expenditures. Using these balance sheet records, we construct firm-level value added. Specifically, value added is calculated as output minus intermediate inputs, where output is defined as total revenues plus changes in inventories of final goods, and intermediate inputs are defined as the cost of goods sold plus changes in input inventories, net of labor costs and depreciation.

We identify the workers who were granted permanent public positions as a result of the reform using the following procedure. As discussed in the Background section, eligibility for the reform required workers to be providing services to public institutions as of December 4, 2017. Accordingly, we classify a worker as a beneficiary of the reform if the following two conditions are satisfied: (i) the worker is observed as employed in the dataset in December 2017 with a reported number of days worked of at least five, and (ii) the worker switches employers and begins working for a public institution no later than June 2018, together with at least five coworkers from the same firm. Using this identification strategy, we identify a total of 260,880 workers transitioning into permanent positions in MOEs (see also Figure 1).

The EIS data do not contain information on central government institutions and their employees. As a result, workers who were granted permanent positions in institutions affiliated with the central government cannot be directly identified within EIS. We therefore

[6] Although worker records are available on a monthly basis starting in 2019, we restrict the sample to these four months to ensure consistency over the full sample period.

[7] We would like to express our gratitude to the officials at the data center for their support.

recover the number of such workers using an alternative data source. The next subsection provides a description of this alternative data source.

3.2 TurkStat Social Security Data

A dataset similar in structure to the EIS is also available at TurkStat. This dataset covers private firms and, unlike the EIS, its worker registers also include workers employed by institutions affiliated with the central government. By matching workers to firms, workers who cannot be linked to the business registry frame dataset can be identified as public-sector workers employed by central government institutions. This feature allows us to identify workers who transitioned into permanent positions in central government institutions (i.e., the central government institutions line in Figure 1) and to construct occupation–region-level measures of these transitions. These measures are used to construct the occupation–province level exposure variables in our spillover analyses.

Our strategy for identifying workers who transitioned into permanent positions in institutions affiliated with the central government closely mirrors the approach used to identify transitions into municipal-owned enterprises (MOEs). Specifically, we require workers to be observed as employed in the December 2017 records with a reported number of days worked of at least five, and to remain in the dataset thereafter while no longer being matched to a private firm by June 2018 at the latest, together with at least five coworkers from the same plant. Using this identification strategy, we classify 359,318 workers as transitioning into permanent positions in institutions affiliated with the central government (see also Figure 1). Combined with the 260,880 workers who transitioned into permanent employment in MOEs, this yields a total of 620,198 workers identified as beneficiaries of the reform.

This figure is below the approximately 750,000 workers who are, based on anecdotal evidence, considered to have benefited from the reform. The discrepancy reflects the deliberately conservative nature of our identification strategy. In practice, eligibility for the reform required workers to be providing services as of December 4, 2017, whereas our approach additionally requires workers to have at least five reported days of employment in December 2017. Moreover, the reform itself did not impose any requirement that workers transition together with coworkers from the same firm. We introduce this additional criterion to minimize the inclusion of regular transitions into public employment that are unrelated to the reform. As a result, some workers who genuinely benefited from the reform may not be captured by our definition.

This dataset has several important limitations. First, it does not contain individual-level information on workers' age or gender. Second, since the receiving entities are public institutions, firm-level balance sheet information is not available, preventing the analysis of firm-level outcomes such as productivity or profitability. Third, because TurkStat does

not provide a matching procedure based on tax identification numbers, as in the EIS data, we cannot identify municipal-owned enterprises that hired substantial numbers of outsourced workers; consequently, these entities remain in the control group in our difference-in-differences analyses.

Using this data, we can study the direct impact of the reform on the outcomes of workers who begin employment with central government institutions after the reform. The results are qualitatively similar to the event-study estimates for workers transitioning into municipal employment (not reported). In principle, the data could also be used to study firm-level outcomes such as survival for private firms affected by the reform. Nevertheless, in order to preserve consistency and coherence across analyses, we conduct our main empirical analysis using the EIS data, focusing on municipal firms and the private firms and workers connected to them throughout the paper.

3.3 Descriptive Statistics

Due to limitations of the alternative dataset described above, we conduct our main analyses using the EIS data. Accordingly, our treatment group consists of 260,880 workers who transitioned into permanent positions in municipal-owned enterprises (MOEs). The potential control group consists of all remaining individuals in the labor force observed in the EIS. In practice, however, the composition of the control group varies across specifications. For example, in gender-specific analyses, the control group is restricted to individuals of the same gender as those in the treatment group. Similarly, in analyses by skill level, the control group is defined within the corresponding high- or low-skilled subsample.

Table 1 presents the gender, skill and age composition of the treated and non-treated samples in the period just before the outsourcing ban. First two rows show that workers who transitioned from outsourced firms to MOEs are predominantly male. While males constitute 73% of the non-treated sample, they represent 87% of the treated group. This gender imbalance becomes more striking when examining male-to-female ratios: while this ratio is 2.64 for non-switchers, it reaches 6.47 for switchers, indicating that the treated group is substantially more male-dominated than the broader workforce.

A similar pattern of differentiation emerges in the skill distribution between treated and non-treated workers (third and fourth rows in Table 1). We classify workers as high-skilled if the first digit of their ISCO code is 1, 2, or 3. Other workers, excluding those with codes 9901 (interns) and 9999 (unknown), are classified as low-skilled. Approximately 89% of the treated group consists of low-skilled workers, compared to 79% in the non-treated sample. Specifically, the majority of treated workers (62%) are employed in cleaning and security occupations.

Finally, rest of rows reveals that the age distribution of treated workers is somewhat older and more concentrated in middle age groups compared to the non-treated sample. While both groups span similar age ranges, the treated group shows a notable concentration in the 30-44 age range, with the modal age group being 35-39 years.

Table 1: # of workers for treated & non-treated workers

	Treated	Non-treated
Gender:		
Male	225,947	9,626,493
Female	34,933	3,641,881
Skill:		
Low	234,719	10,501,336
High	27,691	2,832,180
Age group:		
20-24	9,656	1,585,336
25-29	36,379	2,152,337
30-34	50,391	2,175,940
35-39	54,358	2,206,559
40-44	47,442	1,835,925
45-49	37,857	1,435,185
50-54	16,616	812,810
55-59	6,028	387,056
60-64	1,346	150,747

In the remainder of this section, we present trends in the wages of workers who transitioned into permanent positions in MOEs. We refer to these workers as *Switchers*. For comparison purposes, we construct three additional groups: (i) workers employed in MOEs during the analysis period who did not transition into permanent positions (*Non-switchers in MOEs*); (ii) workers employed in business service firms during the analysis period who did not transition (*Non-switchers in outsourced firms*),[8] and (iii) all remaining workers observed in the dataset (*Non-switchers in other firms*).

We begin by presenting the evolution of average wages relative to the minimum wage for the four groups in Figure 2. Several patterns are worth noting. First, the wages of the treatment group, *Switchers*, exhibit a modest but visible increase immediately following the reform, between March and June 2018. Second, at the beginning of the analysis period, *Switchers* earned approximately 1.5 times the minimum wage while employed

[8]Outsourced firms are identified as the employers immediately preceding workers' transition to MOEs. This classification captures firms that provided outsourced services to municipalities prior to the reform. We identify 2,068 firms meeting this criterion.

in business service firms, whereas by 2023 their wages had risen to around 1.75 times the minimum wage. Notably, *Switchers* are the only group to experience a pronounced upward trend in wages over the analysis period. Third, the wage trajectories of *Switchers* increasingly resemble those of *Non-switchers in MOEs*, particularly after 2020, roughly two years after the reform. Although wage levels for *Switchers* do not fully catch up to those of *Non-switchers in MOEs* within the sample period, there is clear evidence of convergence, with initially lower wages of *Switchers* gradually catching up to the higher wages of incumbent MOE workers.

Overall, the descriptive evidence presented in 2 suggests that *Switchers* benefited from the reform in terms of wages, with the gains becoming more pronounced in the longer run.

Figures 3 replicate the wage trend analysis separately for low- and high-skilled occupations across all four groups. Panel A shows that the wage trajectories of *Switchers* in low-skilled occupations closely mirror those observed for the full sample of *Switchers*. This pattern is unsurprising, given that the majority of *Switchers* are employed in low-skilled occupations, as documented in Table 1. Importantly, the key descriptive findings from the aggregate analysis continue to hold for low-skilled workers: *Switchers* remain the only group exhibiting a pronounced upward trend in wages over the analysis period, and their wage trajectories increasingly converge toward those of *Non-switchers in MOEs*, particularly after 2020.

Panel B indicates that the main conclusions drawn for low-skilled *Switchers* also apply to *Switchers* in high-skilled occupations. High-skilled *Switchers* similarly experience a modest increase in average wages relative to the minimum wage immediately after the reform. Moreover, they remain the only group over the analysis period to exhibit a notable rise in relative wages, increasing from approximately 1.6 times the minimum wage in 2014 to around 2 times the minimum wage by 2023. While the wage trajectories of high-skilled *Switchers* also converge toward those of high-skilled *Non-switchers in MOEs*, the pace and magnitude of convergence are noticeably weaker than those observed among low-skilled occupations.

Figure 4 presents the evolution of average wages relative to the minimum wage for female and male *Switchers*. While average wages for the two groups are broadly similar prior to the reform, male *Switchers* consistently earn higher wages than their female counterparts in the post-reform period, although the magnitude of this difference is not large enough to alter the general interpretations discussed above for female *Switchers*. Our primary focus is on the effects of the outsourcing ban and the transition to permanent public employment, rather than on the presence of a potential gender wage gap among public employees. Nonetheless, the observed post-reform wage differences may reflect heterogeneity in occupational assignments, tenure, and job characteristics within public institutions.

Figure 2: Mean wages for switchers and non-switchers in different firms

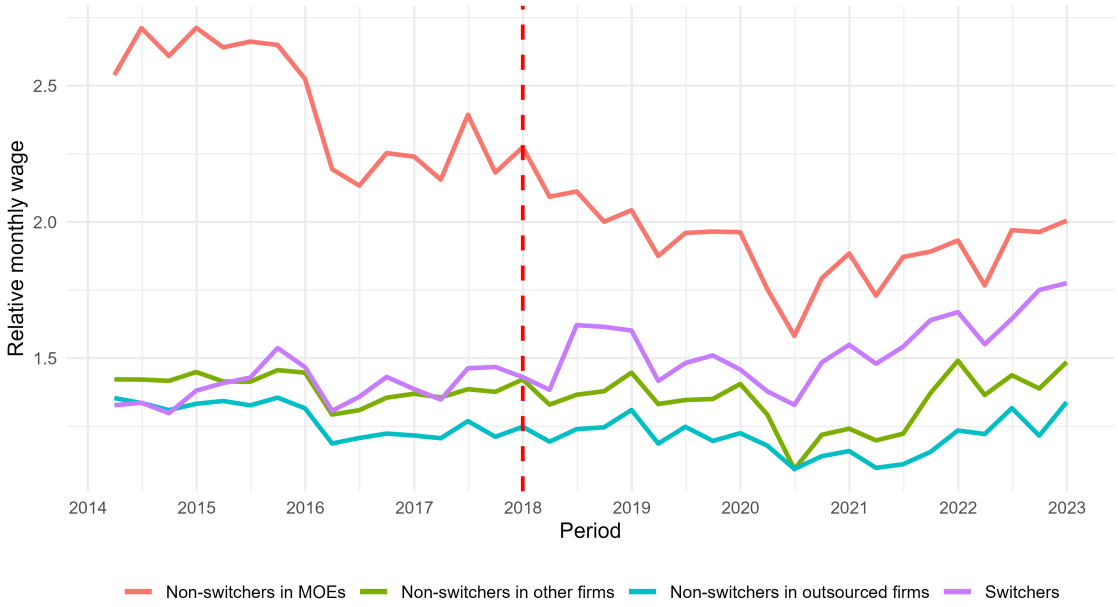


Figure 3: Mean wages for low- and high-skilled switchers and non-switchers in different firms

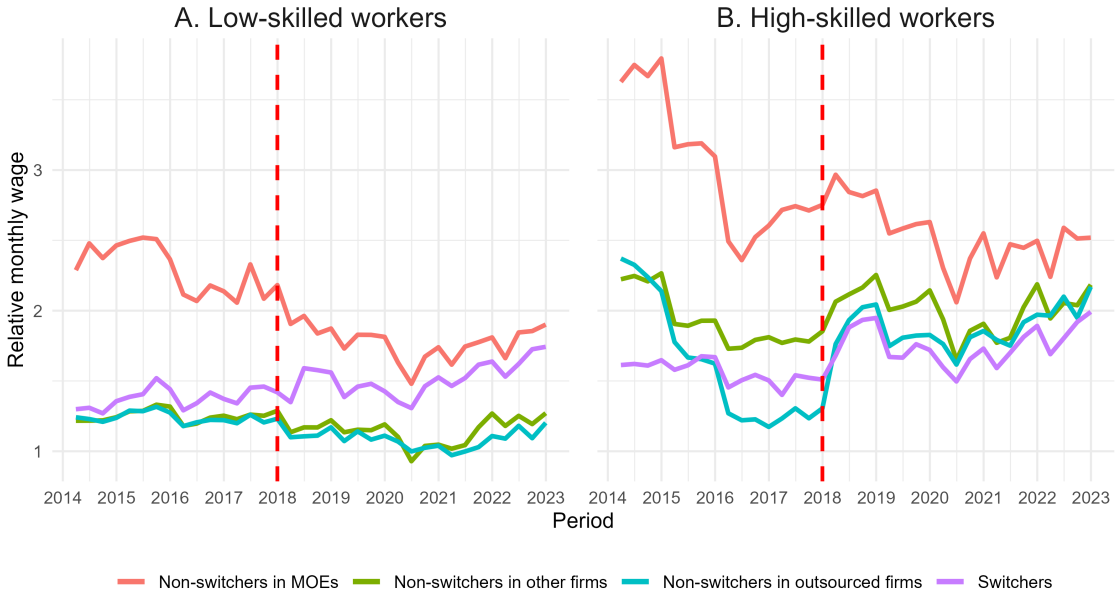
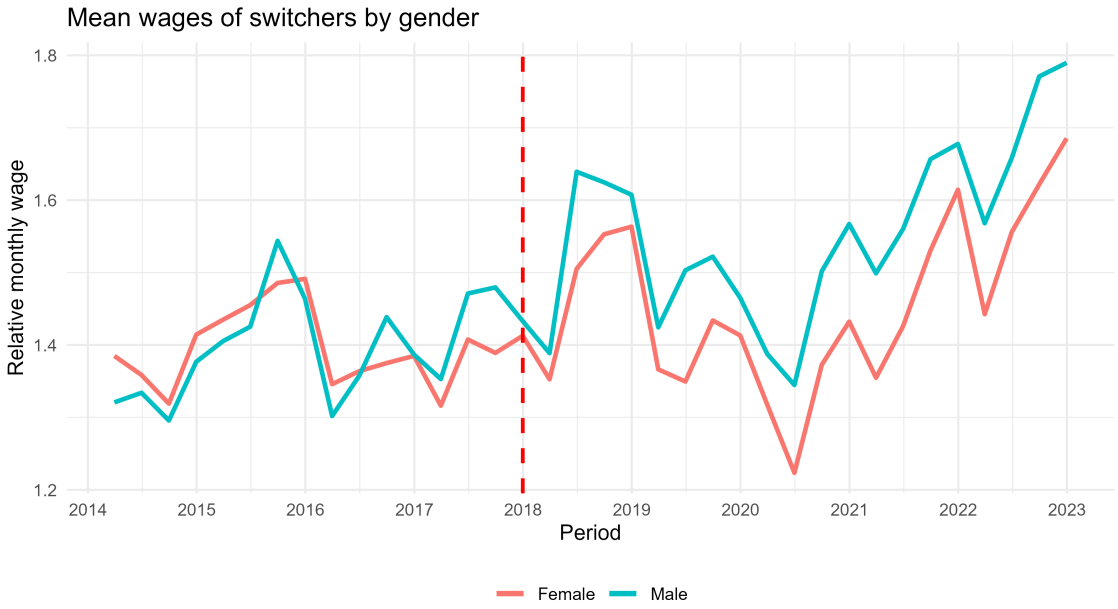
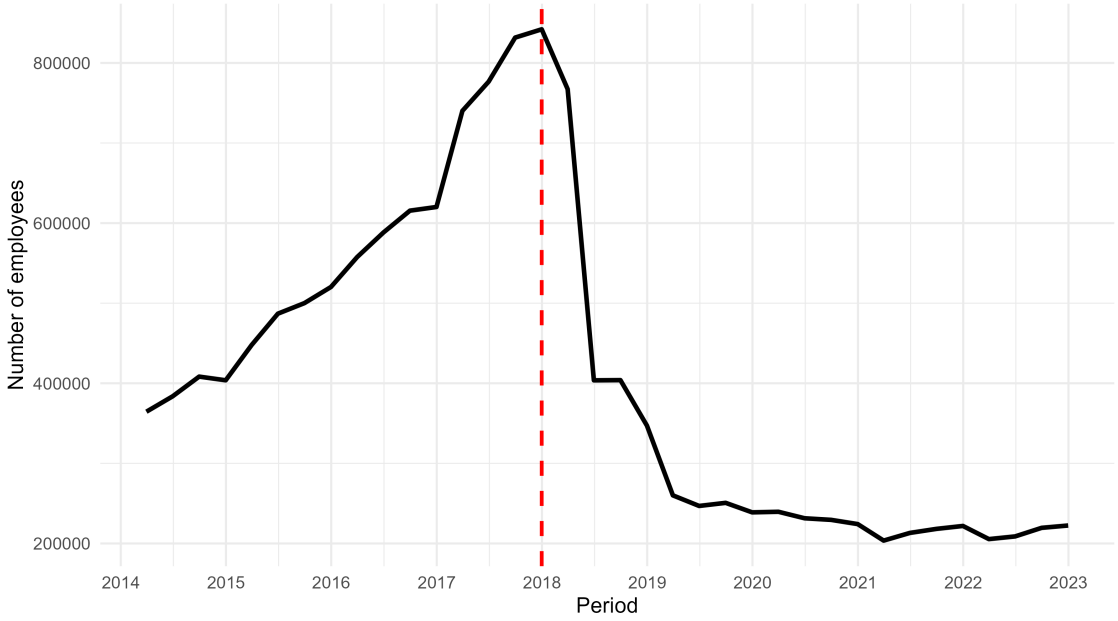


Figure 4: Mean wages of switchers by gender



Finally, Figure 5 displays the evolution of total employment in outsourcing firms that had contracts with municipal-owned enterprises prior to the outsourcing ban. Employment in these firms rises steadily until 2018 and reaches a peak immediately before the reform. Following the implementation of the outsourcing ban, total employment declines sharply and remains at a substantially lower level thereafter. The following sections examine this pattern econometrically.

Figure 5: Total employment in outsourcing firms supplying labor to MOEs



Overall, the descriptive evidence suggests that both female and male workers, as well as low- and high-skilled workers performing outsourced tasks, appears to have benefited from the reform in terms of wages. This naturally raises several questions. Are the observed wage gains statistically significant and causally attributable to the reform? How did the reform affect the performance of business service firms that were required to terminate outsourcing contracts and release workers, as well as municipal-owned enterprises that were obliged to permanently employ these workers? Finally, did the wage gains experienced by *Switchers* spill over to the wages of comparable workers employed in the private sector? The following sections address these questions using a set of carefully designed econometric analyses.

4 Worker-level Results

We begin by presenting the implications of the outsourcing ban for worker-level outcomes. In particular, we examine its effects on the wages of treated workers and on their turnover rates.

4.1 Wage Effects

We employ an event study framework to estimate the dynamic wage effects of the 2018 reform that converted subcontracted workers into permanent public sector employees.

Our primary estimating equation is:

$$\log(wage_{it}) = \sum_{\tau=-8,\tau\neq-1}^{20} \beta_\tau \times switcher_i \times 1[Event\ time_{it} = \tau] + \alpha_i + \gamma_t + \delta_a + \theta_o + \mu_{pt} + \varepsilon_{it} \tag{1}$$

where $\log(wage_{it})$ is the natural logarithm of real monthly wages for worker i in quarter t. The variable $switcher_i$ is an indicator equal to one for workers who transitioned from subcontracted to permanent status in April 2018 (2018q2). The event time variable $Event\ time_{it}$ measures the number of quarters relative to the reform date for each worker, with $Event\ time_{it} = 0$ corresponding to 2018q2. The indicator $1[Event\ time_{it} = \tau]$ equals one when worker i is observed τ quarters relative to their reform date. We normalize the coefficient for $\tau = -1$ (2017q4) to zero, making it the reference period.

The specification includes a comprehensive set of fixed effects. Worker fixed effects (α_i) control for time-invariant individual characteristics such as ability, educational background, and baseline job quality. Time fixed effects (γ_t) absorb aggregate wage trends and macroeconomic shocks common to all workers. Age group fixed effects (δ_a) capture life-cycle wage patterns, while occupation fixed effects (θ_o) account for systematic wage differences across job categories. Finally, province-by-time fixed effects (μ_{pt}) flexibly control for regional labor market shocks and local economic conditions that evolve over time.

The coefficients of interest are $\{\beta_\tau\}_{\tau\neq-1}$, which measure the percentage wage difference between switchers and non-switchers in event period τ relative to the reference quarter. The pre-reform coefficients (β_τ for $\tau < 0$) provide a test of the parallel trends assumption: if switchers and non-switchers had similar wage trajectories before the reform, these coefficients should be statistically insignificant. The post-reform coefficients (β_τ for $\tau \geq 0$) trace out the dynamic treatment effects, showing both the immediate impact and longer-run evolution of the wage response. Standard errors are clustered at the firm level to account for serial correlation in wage outcomes among workers transitioning into permanent positions from the same firm. We estimate equation (1) separately by gender and skill level to explore heterogeneity in treatment effects.

We estimate the regressions on subsamples defined by gender and occupation. In gender-specific specifications, both the treatment and control groups are restricted to individuals of the same gender. Likewise, regressions by skill level are estimated within gender–occupation subsamples; for example, the male high-skilled specification includes only workers who are male and employed in high-skilled occupations immediately prior to the reform. To ensure comparability with the treatment group, individuals in the control group are required to be employed both immediately before the reform (2017q4) and immediately after the reform (2018q1). Apart from this restriction, the panel remains unbalanced.

Figure 6 presents the event study estimates from Equation (1) for male and female workers. The pre-reform coefficients (β_τ for $\tau < 0$) are generally statistically indistinguishable from zero and clustered around the reference period, providing support for the parallel trends assumption. This suggests that switchers and non-switchers experienced similar wage trajectories prior to the reform.

The post-reform estimates show statistically significant wage gains for workers who transitioned to permanent public sector employment. Wages increase sharply following the reform and remain higher throughout the post-period around, indicating that the move from subcontracted to permanent status led to meaningful improvements (around 10% to 15% for male workers) in compensation. An exception is observed during the COVID-19 quarters, when estimated wage gains decline. This drop may be related to the suspension or reduction of bonuses, premia, and other supplementary payments in the public sector during the pandemic.

We also observe a modest decline in wage gains in 2022. This coincides with two large increases in the minimum wage ahead of the 2023 elections, which primarily raised wages among private-sector workers in the control group. Since treated workers are not employed at the minimum wage, these increases narrowed the relative wage gap between the treatment and control groups during this period.

However, the results also reveal considerable gender heterogeneity in treatment effects. Although both male and female workers experienced positive wage gains, the magnitude of these effects is systematically smaller for women. This gender gap in reform-induced wage increases persists throughout most of the post-reform period. The gap widens markedly during the COVID-19 period, suggesting that female workers may have been more vulnerable to pandemic-related wage pressures. While the differential narrows in subsequent quarters as the labor market recovers, female workers continue to experience smaller wage gains than their male counterparts. Table A1 in the Appendix reports the average difference-in-differences estimates, showing that, relative to non-switchers, male workers experience wage increases that are approximately 1.5 times larger than those of female workers.

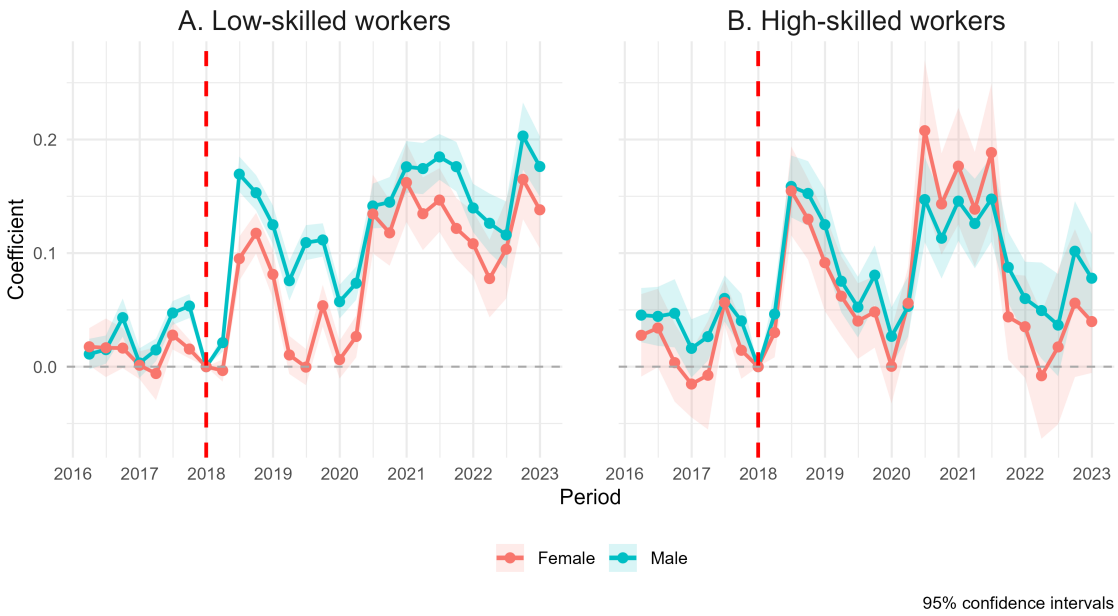
Figure 6: Wage effect of the outsourcing ban



Notes: This figure shows the wage effect of the outsourcing ban on workers who switched from outsourced firms to municipal-owned enterprises, estimated using Equation (1). Individuals in the control group are required to be employed in both 2017q4 and 2018q1 to ensure comparability with the treatment group; the panel is otherwise unbalanced. Standard errors are clustered at the firm level. Regressions for male workers include 200,011,503 observations, while regressions for female workers include 64,458,874 observations. The quarter immediately preceding treatment (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are plotted in Figure 7.

Panel A of Figure 7 shows the dynamic wage effects for low-skilled workers. Both the pre- and post-reform patterns closely resemble those in the baseline results for men and women, indicating that the overall wage gains reported in Figure 6 are largely driven by this group. The results for high-skilled workers differ in several respects. As shown in Panel B, wage differences between men and women are much smaller, and estimated wage gains are more limited than those observed for low-skilled workers. This is not surprising, as high-skilled workers already earn relatively high wages, leaving less room for further increases following the reform. Despite this attenuation, the average wage effects remain positive, as reported in the second and last columns of Table A2.

Figure 7: Wage effect of the outsourcing ban, low- and high-skilled workers



Notes: These figures show the wage effect of the outsourcing ban on workers who switched from outsourced firms to municipal-owned enterprises, estimated using Equation (1) for different subsamples defined by workers' skill levels. Panel A presents the effects for low-skilled workers, while Panel B presents the effects for high-skilled workers. Skill groups are defined using the first-digit ISCO classification: workers with first-digit ISCO codes between 1 and 3 are classified as high-skilled, while all others—excluding interns and individuals with unknown occupations—are classified as low-skilled. Individuals in the control group are required to be employed in both 2017q4 and 2018q1 to ensure comparability with the treatment group; the panel is otherwise unbalanced. Standard errors are clustered at the firm level. Regressions for male workers include 157,259,722 observations, while regressions for female workers include 42,692,812 observations in Panel A. In Panel B, regressions for male workers include 46,403,415 observations, while regressions for female workers include 18,005,078 observations. The quarter immediately preceding treatment (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table A2.

4.2 Turnover Effects

One of the central concerns associated with outsourced work is the lack of job security. As shown in the previous section, workers who switch into municipal-owned enterprises experience wage increases, which may in turn affect their job turnover behavior. In this section, we examine whether workers who transition into permanent public positions exhibit lower job turnover, that is, whether their employment becomes more stable following the treatment.

Our outcome variable, $Turnover_{it}$, is a binary indicator. Specifically, $Turnover_{it} = 1$ if the worker leaves their firm i at t to start working for a different firm in $t + 1$. The indicator also takes the value of one if the worker leaves their firm i at t, is not employed in $t + 1$,

and does not return to the same firm in $t+2$.

To illustrate, $Turnover_{it} = 1$ if a worker leaves their firm at t and either starts working for a new firm in $t+1$ or in $t+2$ following a period of non-employment. Likewise, $Turnover_{it} = 1$ if the worker leaves their firm at t and returns to the same firm only in $t+3$, after not working in both $t+1$ and $t+2$. Permanent exits from the labor force are also coded as $Turnover_{it} = 1$ in the last period the worker is observed. By contrast, $Turnover_{it} = 0$ if the worker experiences a short employment interruption but returns to the same firm in $t+2$, corresponding to a one-quarter leave. Therefore, the following equation presents the empirical specification:

$$Turnover_{it} = \exp\left(\sum_{\tau=-8,\tau\neq-1}^{20} \beta_\tau \times switcher_i \times 1[Event\ time_{it} = \tau] + \alpha\ switcher_i + \Gamma_{employee\ tenure} + \Gamma_{time} + \Gamma_{province}\right) \cdot \varepsilon_{it} \quad (2)$$

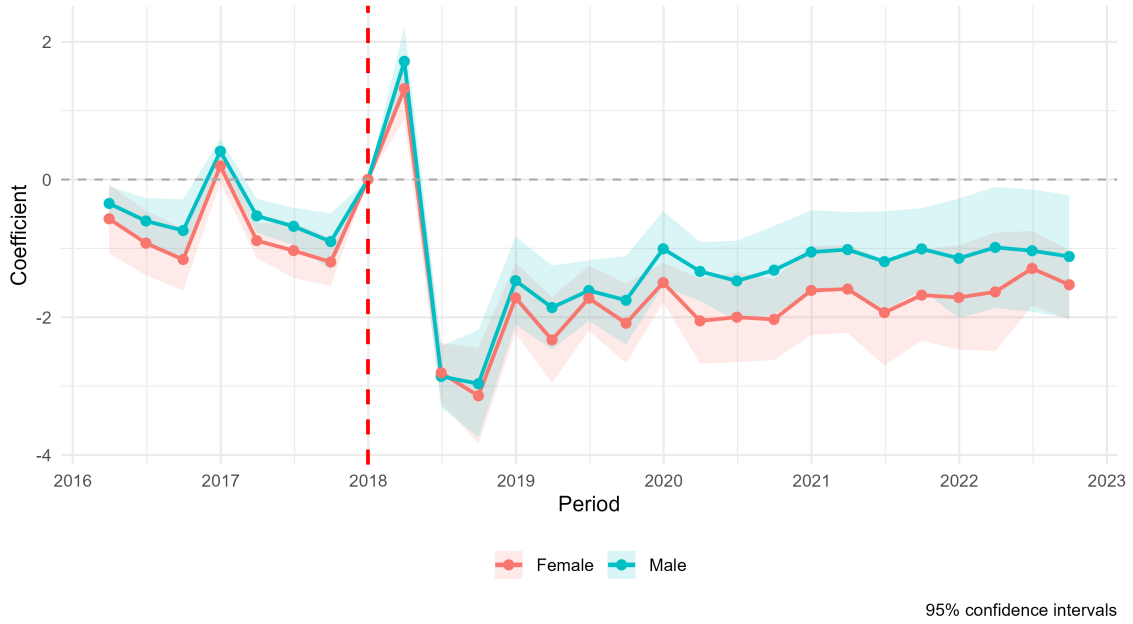
Where outcome variable $Turnover_{it}$ is explained above, the coefficients β_τ capture the dynamic, quarter-by-quarter effects of the outsourcing ban on the job separation (turnover) probability of switcher workers, relative to the omitted baseline period ($\tau = -1$). We control for worker tenure by including employee-tenure fixed effects, denoted by $\Gamma_{employee\ tenure}$. The indicator $switcher_i$ accounts for time-invariant differences between workers who previously worked for outsourced firms and other workers. In addition, we include time fixed effects (Γ_{time}) to absorb common macroeconomic shocks and province fixed effects (Γ_{time}) to control for region-specific factors. Equation (2) is estimated using a Poisson pseudo–maximum likelihood (PPML) estimator and can be interpreted as a discrete-time hazard model for worker–firm separation.

As in the wage regressions, turnover regressions are estimated on subsamples defined by gender and occupation. Figure 8 presents the event study estimates of the reform on worker turnover for the subsamples of make and female workers separately based on equation (2). As before, the last period before treatment 2017q4 (indicated by the vertical red dashed line in event) is considered as the base period. Figure 8 suggests that the first period after treatment, 2018q1 is associated with a hike in turnover probability. This is simply due to the nature of the reform. Recall that individuals are transferred into permanent positions until 2018q2. Many individuals are transferred between 2018q1 and 2018q2, mechanically increasing the turnover probability of treated individuals in 2018q1.

Following the completion of the reform, the conditional expected turnover probability declines sharply for both genders, implying a reduction in the conditional mean of roughly 95% ($= |exp(-3) - 1|$) by 2018q3. Thereafter, the effect stabilizes, with the conditional

probability remaining about 86% lower for female workers and 63% lower for male workers who benefited from the reform, relative to comparable non-treated workers. The larger and more persistent decline among women may reflect greater risk aversion or a stronger preference for job stability after securing a permanent public position.[9]

Figure 8: Worker turnover effect of the outsourcing ban



Notes: This figure shows the turnover effect of the outsourcing ban on workers who switched from outsourced firms to municipal-owned enterprises, estimated using Equation (2). A hazard model is estimated using PPML. All observations in the worker dataset are included to estimate worker–firm separation effects. Standard errors are clustered at the industry & firm-tenure level. Regressions for male workers include 319,481,352 observations, while regressions for female workers include 125,908,857 observations. The quarter immediately preceding treatment (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table **??**.

Panels A and B of Figure 9 replicate the analysis separately for low- and high-skilled workers, respectively. The results indicate that worker retention is slightly higher among low-skilled workers than among their high-skilled counterparts. In other words, low-skilled workers—particularly women—are more likely to remain employed in MOEs after the outsourcing ban. One plausible explanation is the stronger job security and labor protection provided by these institutions. In contrast, high-skilled workers exhibit weaker retention effects, likely reflecting their better outside options in the labor market.

Tables **??** and **??** in the Appendix present the average treatment effects for the gender

[9]To illustrate the magnitude of the estimated effects, consider an individual with a given set of covariates for whom the model predicts a turnover probability of 50% in the absence of treatment. A coefficient of -3 implies that treatment multiplies this conditional expectation is reduced by approximately 95% to 2.5%. Similarly, a coefficient of -2 implies a post-treatment predicted turnover probability of about 7%.

subsamples and for the joint gender–occupation subsamples, respectively.

Figure 9: Worker turnover effect of the outsourcing ban, low- and high-skilled workers



Notes: These figures show the turnover effect of the outsourcing ban on workers who switched from outsourced firms to municipal-owned enterprises, estimated using Equation (2) for different subsamples defined by workers' skill levels. Panel A presents the effects for low-skilled workers, while Panel B presents the effects for high-skilled workers. Skill groups are defined using the first-digit ISCO classification: workers with first-digit ISCO codes between 1 and 3 are classified as high-skilled, while all others—excluding interns and individuals with unknown occupations—are classified as low-skilled. All observations in the worker dataset are included to estimate worker-firm separation effects. Standard errors are clustered at the industry & firm tenure level. Regressions for male workers include 254,033,638 observations, while regressions for female workers include 91,995,276 observations in Panel A. In Panel B, regressions for male workers include 63,491,043 observations, while regressions for female workers include 32,580,754 observations. The quarter immediately preceding treatment (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table **??**.

5 Firm-level Results

The outsourcing ban also has important implications for firms. Two groups of firms are particularly affected by the reform: outsourcing firms and municipal-owned enterprises (MOEs). While outsourcing firms lose a large share of their workforce following the reform, MOEs absorb these workers as regular employees. In this section, we examine the firm-level consequences of the outsourcing ban. Section 5.1 focuses on outsourcing firms that provided services to MOEs prior to the reform and analyzes the effects of the policy on firm survival and firm size. Section 5.2 then examines the impact on other firm outcomes, including productivity and profitability. Finally, Section 5.3 conducts a parallel

analysis for MOEs.

5.1 Survival and Firm Size Results

As we see in Section 3.3, outsourced firms faced sharp employment reductions. This may have caused these firms to exit the market or scale down their size. This section analyzes the survival and firm size effects of the policy reform on outsourcing firms. Therefore, we estimate the following equation to examine whether firm survival has been affected by the policy reform. Let the subscript f denote firms. The equation is presented as follows:

$$y_{ft} = \exp\Big(\beta\ exposure_f \times (T_f \times 1_{t>2018\ March}) + \theta\ exposure_f + \\ \alpha\ log(initial\ employment_f) + \Gamma_{firm\ age} + \Gamma_{time} + \Gamma_{province}\Big) \cdot \varepsilon_{ft} \qquad (3)$$

In the survival analysis, y_{ft} is a binary variable that takes the value one in the last period in which firm f is observed in the data, and zero otherwise. Thus, exit is recorded only once for each firm, corresponding to the final period of observation. The coefficient β shows the effect of the outsourcing ban on the firm survival of outsourced firms. We adopt a continuous treatment measure, $exposure_f$, indicating the degree of exposure of a firm, which is defined as the ratio of switcher workers to total employment of the firm, as shown in the following equation:

$$exposure_f = \frac{\text{number of switchers in firm}f}{\text{total number of employees in firm}f}, \qquad (4)$$

As a robustness check, we also construct an alternative measure of exposure based on wages, defined as the ratio of total wages paid to workers transitioning into permanent public positions to the total wage bill of the firm just before the reform. Our results are robust to this alternative specification (see below).

We also control for firm size by including the initial value of employment of the firm, $(initial\ employment_f)$. Furthermore, firm-age fixed effects, $\Gamma_{firm\ age}$, are included to account for a key determinant of the hazard function. We additionally control for time- and province-related shocks by including Γ_{time} and $\Gamma_{province}$ fixed effects, respectively. We estimate this discrete-time hazard function using PPML, following the same approach applied in the worker-level turnover analysis in Section **??**. Finally, we restrict our sample to firms that share the same NACE codes as the outsourced firms.

Even if outsourced firms survive, their size may decline due to the demand shock induced by the reform. For this reason, we also conduct a firm-size analysis. Firm size is measured

as the number of employees observed at firm f in period t. Unlike the exit outcome discussed above, when estimating firm size we extend the firm panel by one additional period after its last observed period and assign a firm size of zero in that period. This construction allows us to capture the decline in employment associated with firm exit.

We employ the regression specification introduced in Equation (5) to examine the implications of the reform for both the survival (exit) probabilities and the sizes of firms that provided outsourced services to MOEs prior to the law. Accordingly, the outcome variable y_{ft} captures either the exit status or the size of firm f at time t, depending on the specification.

For example, consider a firm that is last observed in $2012q2$. In the exit specification, the exit indicator equals zero in $2012q1$ and one in $2012q2$. In the firm-size specification, the firm employs 30 workers in $2012q1$ and 25 workers in $2012q2$. We then add one additional observation for $2012q3$ and set firm size equal to zero in that period. Therefore, the following equation is estimated:

$$y_{ft} = \exp\left(\sum_{\rho=2016q1, \rho \neq 2017q4}^{2022q4} \beta_\rho \times \ exposure_f \times (T_f \times 1_{t=\rho}) + \theta \ exposure_f + \right.$$
$$\left. \alpha \ log(initial \ employment) + \Gamma_{firm \ age} + \Gamma_{time} + \Gamma_{province} \right) \cdot \varepsilon_{ft} \quad (5)$$

Our main coefficients of interest, β_ρ, capture the quarter-specific effects associated with the interaction between period indicators, $(T_f \times 1_{t=\rho})$, It shows the combined effect of the reform on (i) employment conditional on survival and (ii) firm exit. Firm-level exposure to the reform, denoted by $exposure_f$. All other fixed effects and control variables are identical to those included in Equation (3).

Table 2 presents the effect of the outsourcing ban on firm survival for firms that previously provided labor to MOEs. While the first column reports results using employment exposure as the main explanatory variable, the second column replaces this measure with wage exposure as a robustness check.

The coefficients on initial employment and $exposure_f$ enter with the expected signs, indicating that firms with higher initial employment levels and greater exposure are associated with lower exit probabilities. However, once exposure is interacted with the post-treatment dummy, the estimated coefficient becomes positive and statistically significant. This implies that firms more heavily exposed to the policy reform experience a higher probability of exit following the outsourcing ban.

Quantitatively, if 10% of a firm's workforce switches to a public institution, the firm's exit

probability increases by approximately 12% $(= \exp(1.128 \times 0.1) - 1)$ relative to otherwise similar firms. This finding remains robust when wage exposure is used as the treatment intensity measure, as shown in column (2).

Table 2: Firm survival effect of the outsourcing ban

Model:	Employment exposure (1)	Wage exposure (2)
Variables		
$log(initial\ employment_f)$	-0.3205***	-0.3105***
	(0.0309)	(0.0309)
$expoosure_f$	-0.2073	-0.2384
	(0.2523)	(0.2484)
$expoosure_f \times D_{t>Mar\ 2018}$	1.128***	1.132***
	(0.2612)	(0.2596)
Fixed-effects		
Industry	Yes	Yes
Firm age	Yes	Yes
Province	Yes	Yes
Time	Yes	Yes
Fit statistics		
Observations	34,242,280	34,242,280
Squared Correlation	0.01648	0.016485
Pseudo R^2	0.04194	0.04194
BIC	11,147,188.8	11,147,185.5

Notes: This table reports the firm survival effects of the 2018 outsourcing ban, estimated using Equation (3) with PPML. The dependent variable is a binary indicator equal to one in the last period in which firm f is observed in the data and zero otherwise. Columns (1) and (2) report estimates for the full sample, using employment-based exposure and wage-bill–based exposure, respectively. Both specifications control for initial firm employment, denoted by $\log(initial\ employment_f)$. The variable $exposure_f$ is a continuous treatment measure defined as the ratio of switcher workers to total firm employment prior to the reform, and $D_{t>Mar\ 2018}$ is a post-reform indicator. In column (2), $exposure_f$ is constructed using the wage-bill share of switcher workers as a robustness check. The coefficient on the interaction between $exposure_f$ and the post-reform indicator captures the effect of the reform on firm exit. Both specifications include industry, firm-age, province, and time fixed effects. The sample consists of firms operating in the same NACE industries as outsourced firms. Goodness-of-fit measures (squared correlation, pseudo R^2, and BIC) and the number of observations are reported in the table. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

We next examine whether the effect of the reform on firm survival differs by the skill intensity of the services supplied by firms that previously provided outsourced services to MOEs. We classify firms into two industry groups: (i) providers of low-skilled business services, including food services, cleaning, security, and logistics; and (ii) providers of high-skilled professional business services, such as IT, programming, consultancy, engineering, and architecture. A detailed description of the industry classification is provided in the Appendix D1. Firms whose activities cannot be clearly classified as low- or high-skilled are excluded from this subsample analysis.

The results are reported in Table 3. The first column presents estimates for low-skilled industries, while the second column reports results for high-skilled industries. The coefficients on the control variables are similar in magnitude and sign to those reported in Table 2 and align with expectations.

In contrast, the coefficients of interest differ markedly across the two groups. For low-skilled industries, we estimate a positive and statistically significant coefficient, indicating that firms in this group are more likely to exit the market following the reform. Magnitude is also similar to the one in the baseline results of Table 2. For high-skilled industries, the estimated coefficient is also positive but smaller in magnitude and statistically insignificant, suggesting a more limited impact of the outsourcing ban on firm survival in these sectors.

Table 3: Firm survival effect of the outsourcing ban by skill group of industries

	Low-skill	High-skill
Model:	(1)	(2)
Variables		
$log(initial\ employment_f)$	-0.3083***	-0.3826***
	(0.0935)	(0.0702)
$expoosure_f$	-0.2199	-0.1963
	(0.3248)	(0.5556)
$expoosure_f \times D_{t>Mar\ 2018}$	1.160***	0.9341
	(0.2056)	(0.8644)
Fixed-effects		
Industry	Yes	Yes
Firm age	Yes	Yes
Province	Yes	Yes
Time	Yes	Yes
Fit statistics		
Observations	3,216,468	2,915,937
Squared Correlation	0.01228	0.01349
Pseudo R^2	0.03086	0.04261
BIC	1,094,425.7	798,896.3

Clustered (industry & firm age) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table reports the firm survival effects of the 2018 outsourcing ban, estimated using Equation (3) with PPML. The dependent variable is a binary indicator equal to one in the last period in which firm f is observed in the data and zero otherwise. Column (1) reports estimates for firms operating in low-skilled industries, while column (2) reports estimates for firms operating in high-skilled industries. All specifications control for initial firm employment, denoted by $\log(initial\ employment_f)$. The variable $exposure_f$ is a continuous treatment measure defined as the ratio of switcher workers to total firm employment prior to the reform, and $D_{t>Mar\ 2018}$ is a post-reform indicator. In column (2), $exposure_f$ is constructed using the wage-bill share of switcher workers as a robustness check. The coefficient on the interaction between $exposure_f$ and the post-reform indicator captures the effect of the reform on firm exit. All specifications include industry, firm-age, province, and time fixed effects. Industries are classified into two groups: (i) low-skilled business services, including food services, cleaning, security, and logistics; and (ii) high-skilled professional business services, including IT, programming, consultancy, engineering, and architecture. Goodness-of-fit measures (squared correlation, pseudo R^2, and BIC) and the number of observations are reported in the table. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

We next illustrate the event-study estimates of the effect of employment exposure to the reform on firm size. Figure 10 shows that by 2018q2, the latest date by which the transition of workers into permanent positions in MOEs was expected to be completed, higher employment exposure to the reform is associated with a substantial contraction in firm size. Specifically, long-run coefficients of approximately -3 imply that a 10 percentage point increase in employment exposure reduces the expected firm size by about 26 percent $(= |\exp(-3 \times 0.1) - 1|)$. Importantly, firm size does not recover through the end of 2022, nearly five years after the reform.

The pronounced and persistent decline in firm size may reflect several mechanisms. First, while the reform directly targeted outsourced workers, managerial, supervisory, and administrative staff employed by these firms did not benefit from the legislation and may have been laid off following the loss of outsourced contracts with MOEs. Second, for firms with high exposure to public-sector outsourcing, the reform may have effectively eliminated their primary source of demand, forcing some of these firms to scale down sharply or exit altogether, as documented in the survival analysis.

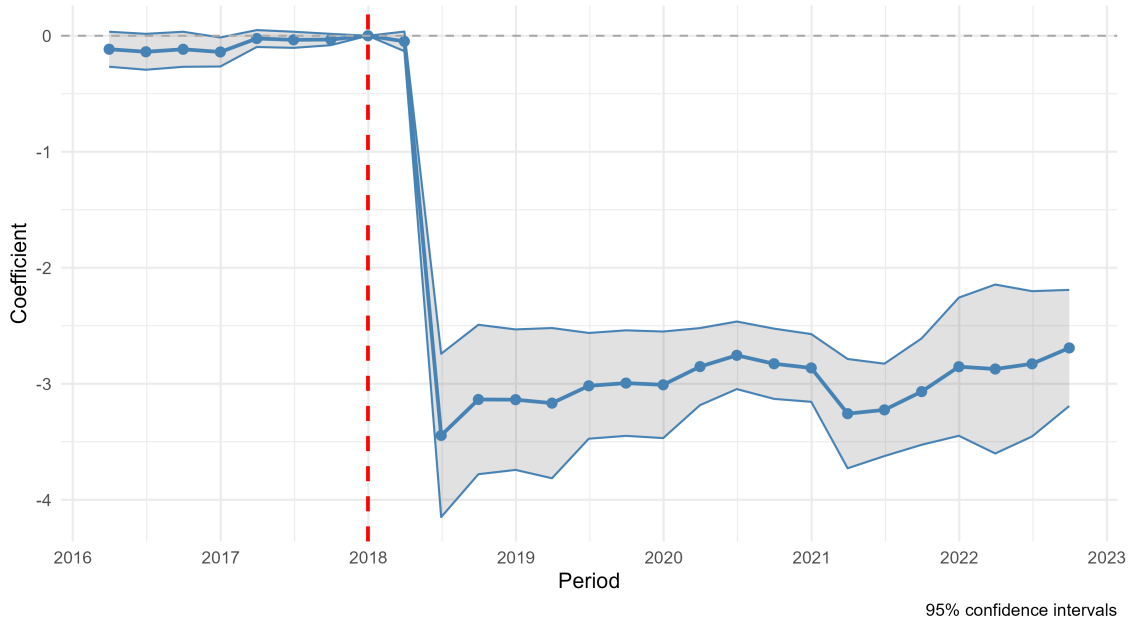
The results by firm skill group are presented in Figure 11. Overall, the estimated effects for firms providing services with different skill intensities are similar in direction and magnitude and closely mirror the aggregate patterns documented above. However, the estimates for low-skilled industries are more precisely estimated, with narrower confidence intervals and higher statistical significance compared to those for high-skilled industries.

We additionally estimate the average effects of the reform using both employment and wage exposure measures. The results are reported jointly in Table A4. The findings are nearly identical across the two measures and closely aligned in magnitude with the event study results discussed above. We further estimate the average effects separately for firms operating in low- and high-skilled service industries using employment exposure; the corresponding results are presented in Table A4.

Overall, our analysis suggests that outsourced firms experience a significant increase in firm exit following the policy reform. These effects vary by firms' activities. Firms providing labor-intensive services to municipal-owned enterprises (MOEs) exhibit higher exposure to the reform compared to firms engaged in high-skilled activities. By contrast, the scale effects are broadly similar across the two groups.[10]

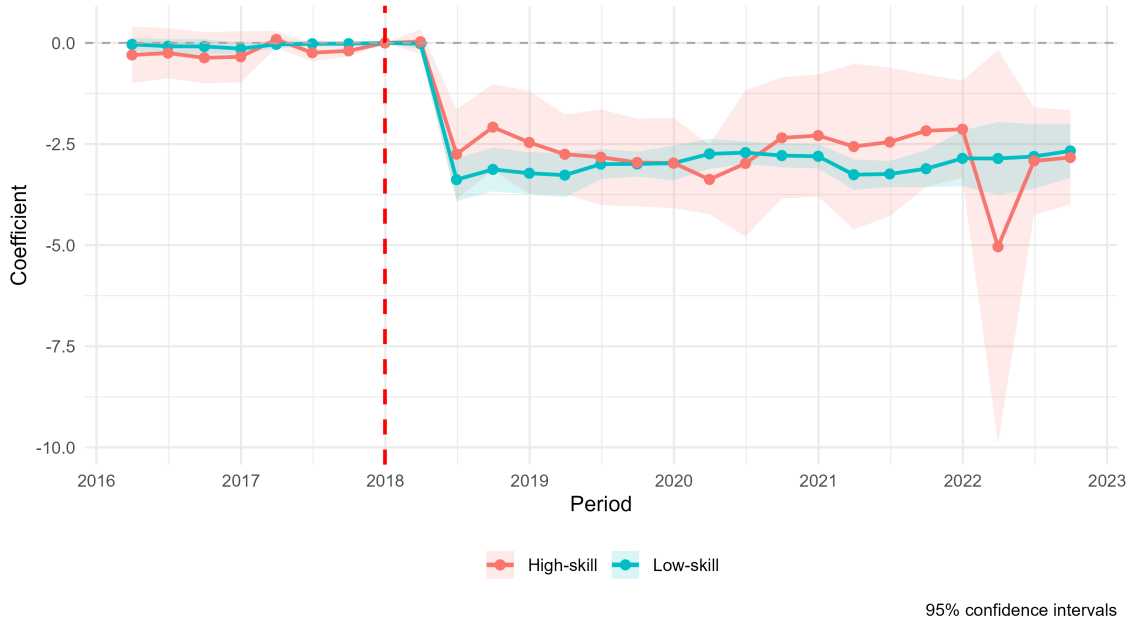
[10]The firm-size specification captures the combined effects of firm exit and employment adjustments. To isolate employment responses among surviving firms, we additionally estimate the same model excluding firms that exit. This allows us to assess the intensive-margin effect. The results remain qualitatively similar, indicating limited employment adjustments among surviving firms.

Figure 10: Firm size effect of the outsourcing ban, all firms



Notes: This figure shows the firm-size effects of the outsourcing ban estimated using Equation (5) with a continuous treatment measure. Exposure is defined as the ratio of switcher workers to total firm employment measured immediately prior to the outsourcing ban. The estimates capture the combined effect of the reform on (i) employment conditional on firm survival and (ii) employment losses due to firm exit. The model is estimated using PPML. The sample includes all firms that share the same four-digit NACE industry with outsourced firms in the dataset. Standard errors are clustered at the industry and firm-age level. The regressions are based on 35,533,481 observations. The quarter immediately preceding the reform (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table A4.

Figure 11: Firm size effect of the outsourcing ban by skill groups of industries



Notes: This figure shows the firm-size effects of the outsourcing ban estimated using Equation (5) with a continuous treatment measure. Exposure is defined as the ratio of switcher workers to total firm employment measured immediately prior to the outsourcing ban. The estimates capture the combined effect of the reform on (i) employment adjustments among surviving firms and (ii) employment losses due to firm exit. The model is estimated using PPML. Low-skilled industries include food services, security, cleaning, and logistics, while high-skilled industries include IT, programming, consultancy, engineering, and architecture. Standard errors are clustered at the industry and firm-age level. The regressions are based on 35,533,481 observations. The quarter immediately preceding the reform (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table A4.

5.2 Productivity and Profitability effects

The previous section showed that business service firms with higher exposure to MOEs, and thus more strongly affected by the reform, were more likely to exit the market after the reform or to experience substantial contractions in size. This section turns to the performance of the surviving firms. Specifically, we ask how productivity and profitability evolved in response to the subcontracting ban.

To address this question, we estimate the following difference-in-differences specification, introduced in equation (6):

$$asinh(y_{ft}) = \sum_{\rho=2014, \rho \neq 2017}^{2022} \beta_t \times \ exposure_f \times (T_f \times 1_{t=\rho}) + D_f + D_t + D_{pt} + \varepsilon_{ft} \quad (6)$$

where y_{ft} denotes firm-level outcomes for firm f in year t. We focus on value added

(introduced in Section 3.1) per employee and profit per workday. As both variables include many negative and zero values, we apply the inverse hyperbolic sine (IHS) transformation, which allows us to retain these observations while preserving an interpretation similar to logarithmic changes for large values. Both outcomes are constructed from the balance sheet information available in the EIS. Since balance sheets are reported on an annual basis, we estimate these regressions using annual data, unlike in the previous sections.

The variable $exposure_f$ measures firms' exposure to MOEs, as defined in the previous section and formalized in equation (4). The interaction term $(T_f \times 1_{t=\rho})$ represents a set of period dummies. Accordingly, the coefficients β_t trace out the dynamic effects of the subcontracting ban, capturing how firms with greater pre-reform exposure to MOEs respond relative to less exposed firms in terms of productivity and profitability.

Firm fixed effects D_f control for time-invariant heterogeneity across firms, while year fixed effects D_t absorb common macroeconomic shocks. Province-by-year fixed effects D_{pt} further account for region-specific time trends. We estimate this specification using the full unbalanced sample of firms. Firms that are not business service firms and have no direct relationship with MOEs naturally have $exposure_f = 0$.

Results for productivity per employee and profit per employee are presented in Figures 12 and 13, respectively. Corresponding average treatment effects are reported in Appendix Table A5.

Figures 12 and 13 display broadly similar patterns. Although some pre-treatment coefficients are negative and statistically significant at the 5% level, their magnitudes are small relative to the post-treatment effects and, more importantly, they do not exhibit a systematic trend. If anything, both productivity and profitability of more highly exposed firms display a mild upward trend relative to less exposed firms prior to the treatment.

The post-treatment coefficients indicate that value added per employee begins to decline approximately one year after the reform, while profit per employee decreases immediately following the reform for firms with higher exposure to the treatment. These declines occur despite a substantial reduction in employment levels. Both effects intensify over time and reach their peak in 2021—three to four years after the reform—with coefficients of roughly -1.6 for productivity and exceeding -1.5 for profitability.

Interpreting the coefficients is not straightforward due to the use of the arc-sin transformation for the outcome variables and the continuous exposure to treatment measure. For sufficiently large and positive values of the underlying variables, the arc-sin transformation can be interpreted as approximately log-linear. Under this approximation, the estimated coefficients can be viewed as semi-elasticities.

Consider first the peak post-treatment coefficient for profitability, which exceeds -1.5. Interpreted as a semi-elasticity, this estimate implies that a fully exposed firm, i.e., a

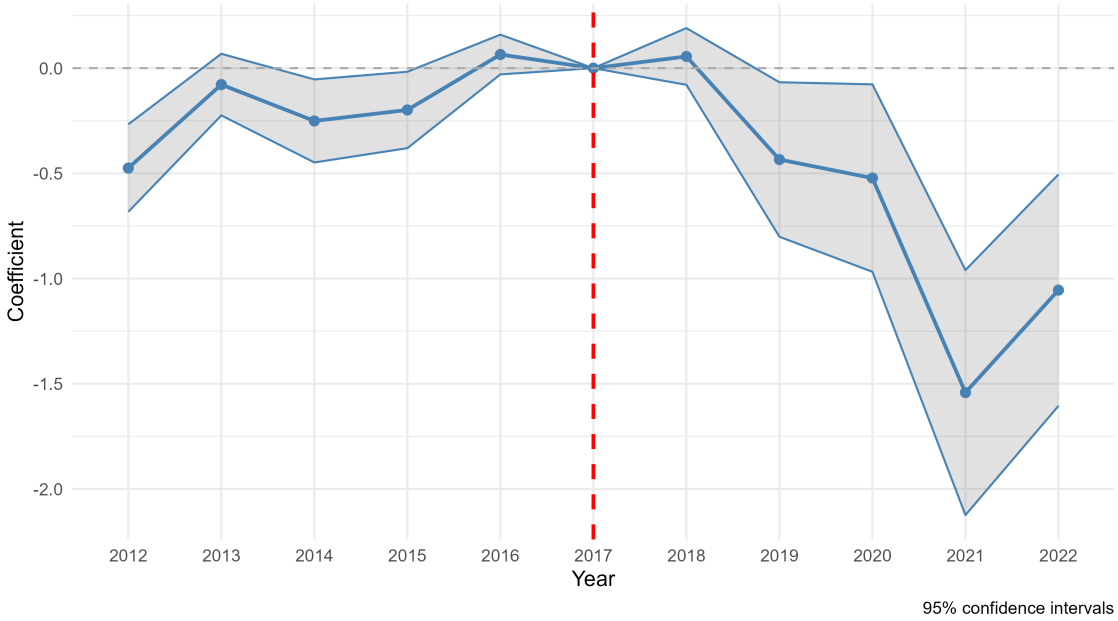
firm with $exposure_f = 1$, experiences a decline in profit per employee of approximately $|exp(-1.5) - 1|$ relative to a firm with zero exposure. This corresponds to a reduction of over 78 percent and indicates an almost complete collapse in per-employee profitability for firms whose workforce was entirely devoted to providing outsourced services to MOEs prior to the reform.

A more economically relevant interpretation focuses on marginal changes in exposure using average effects in Table A5, as most firms exhibit intermediate exposure levels rather than full exposure. For example, relative to a firm with no exposure, a firm with an exposure level of 0.2 would experience an implied change of roughly $-0.7085 \times 0.2 = -0.1321$. Under the same approximation ($|exp(-0.7085) - 1|$), this corresponds to a decline in profit per employee of about 50 percent. Even at moderate exposure levels, therefore, the estimated effects imply economically large and meaningful losses in profitability compared to non-exposed firms following the reform.

A similar interpretation applies to productivity effects. The peak coefficient of approximately -1.5 for value added per employee implies that a fully exposed firm experiences a reduction in productivity of roughly $|exp(-1.542) - 1|$, or about 78 percent, relative to a firm with zero exposure. While this magnitude reflects an extreme comparison between fully exposed and non-exposed firms, marginal exposure effects remain substantial. Relative to a non-exposed firm, a firm with an exposure level of 0.2 would experience an implied change of about $-1.542 \times 0.2 = -0.26$, corresponding to a productivity decline of approximately 26 percent.

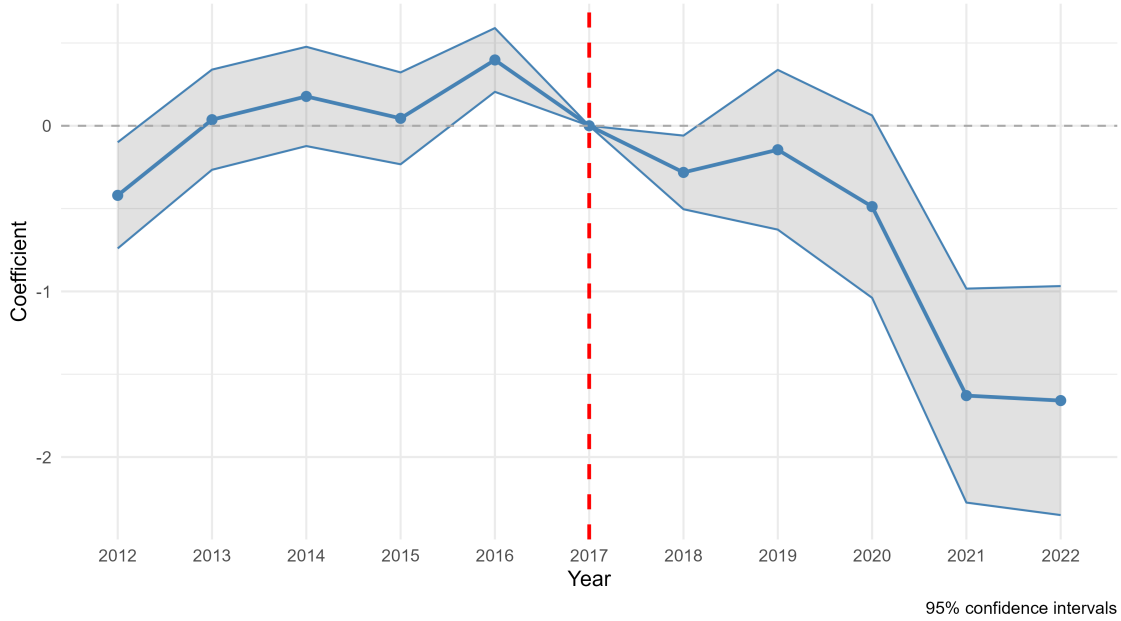
Taken together, these results indicate that the subcontracting ban generated large and persistent negative effects on both productivity and profitability among more exposed firms, even when evaluated at rather small (e.g., 0.2) exposure levels. The magnitude and persistence of these effects suggest that contracts with MOEs constituted highly valuable and profitable business opportunities for these firms. Losing access to MOE-related activities appears to have disproportionately weakened the performance of more exposed firms, indicating that these contracts were not easily replaced by alternative sources of demand even in the long-run, i.e., 4-5 years after the reform.

Figure 12: Productivity effect of the outsourcing ban on outsourced firms



Notes: This figure shows the productivity effects of the outsourcing ban estimated using Equation (6) with a continuous treatment measure. Exposure is defined as the ratio of switcher workers to total firm employment measured immediately prior to the outsourcing ban. The sample includes all firms that share the same four-digit NACE industry with outsourced firms in the dataset. Standard errors are clustered at the firm level. The regressions are based on 6,930,427 observations. The quarter immediately preceding the reform (2017), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table A5.

Figure 13: Profitability effect of the outsourcing ban on outsourced firms



Notes: This figure shows the profitability effects of the outsourcing ban estimated using Equation (6) with a continuous treatment measure. Exposure is defined as the ratio of switcher workers to total firm employment measured immediately prior to the outsourcing ban. The sample includes all firms that share the same four-digit NACE industry with outsourced firms in the dataset. Standard errors are clustered at the firm level. The regressions are based on 7,241,321 observations. The quarter immediately preceding the reform (2017), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table A5.

5.3 Productivity and profitability of MOEs

As documented in the previous section, firms providing outsourced services to MOEs experienced substantial declines in value added per employee and profit per employee following the reform. These patterns indicate that the contracts these firms were forced to relinquish were economically valuable and enabled profitable operation. This section turns to the other side of the market and examines the effects of the reform on MOEs, which absorbed these workers into permanent positions.

As a prior, if MOEs were able to purchase outsourced services under contracts that involved high markups or generated substantial rents for service providers, terminating these contracts and producing the same services in-house should, all else equal, be beneficial for MOEs. The analysis below evaluates this hypothesis empirically.

In particular, we examine whether MOEs improved their performance in terms of value added per employee (productivity) and profit per employee following the reform. Our regression specification is presented in equation (7). The only difference relative to equation (6) is the use of a binary treatment indicator: $MOE_f = 1$ if a firm is an MOE

and 0 otherwise. Aside from this distinction, equations (6) and (7) are identical.

$$asinh(y_{ft}) = \sum_{\rho=2014, \rho \neq 2017}^{2022} \beta_t \times \ MOE_f \times (T_f \times 1_{t=\rho}) + D_f + D_t + D_{pt} + \varepsilon_{ft} \quad (7)$$

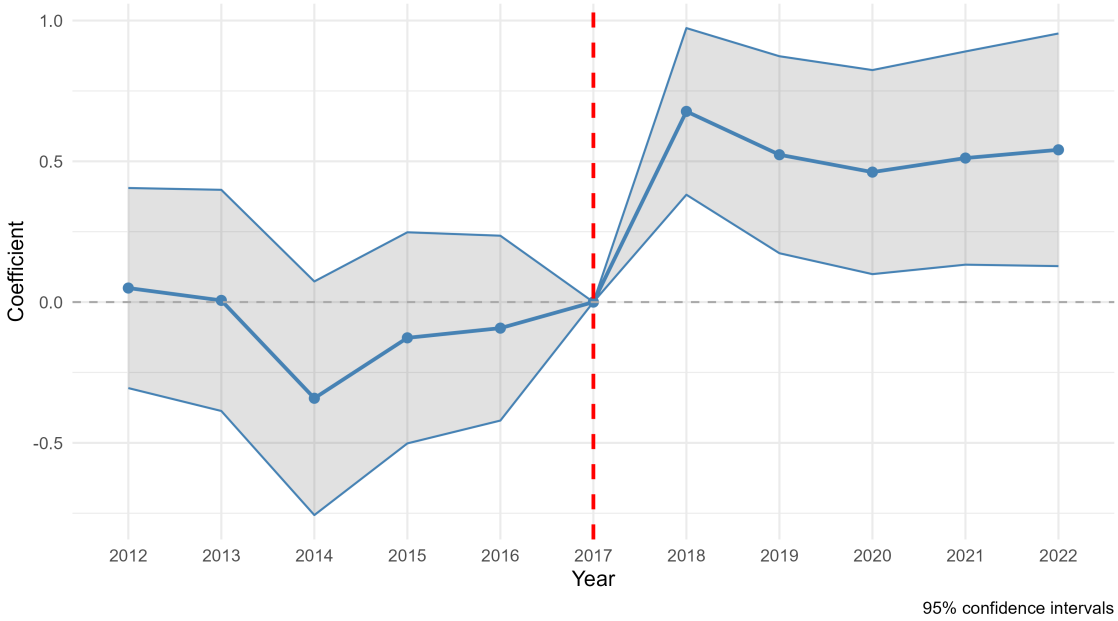
Results are presented figures (14) and (15). Appendix Table A6 presents the corresponding average treatment effects.

As evident from figures (14) and (15), there is no evidence to reject the parallel trends assumption, i.e., pre-treatment coefficients are almost always insignificant at the 5% level. However, positive and (mostly) significant post-treatment coefficients suggest that MOEs benefit from the reform in terms of productivity and profitability.

Post-treatment coefficients for productivity lie between 0.5 and 0.7. Under the semi-elasticity approximation, these estimates imply that value added per employee at MOEs increased by roughly $exp(0.5) - 1 \approx 64\%$ to $exp(0.7) - 1 \approx 101\%$ relative to non-MOEs after the reform. The corresponding coefficients for profit per employee are around 0.5, indicating an increase in profits per employee of approximately 64 percent. These magnitudes suggest substantial performance gains for MOEs following the internalization of previously outsourced services. Only in the final year of the sample, 2022, does the profit per employee coefficient fall below 0 and lose statistical significance at the 5 percent level. This may reflect temporary fluctuation rather than a reversal of the overall effect.

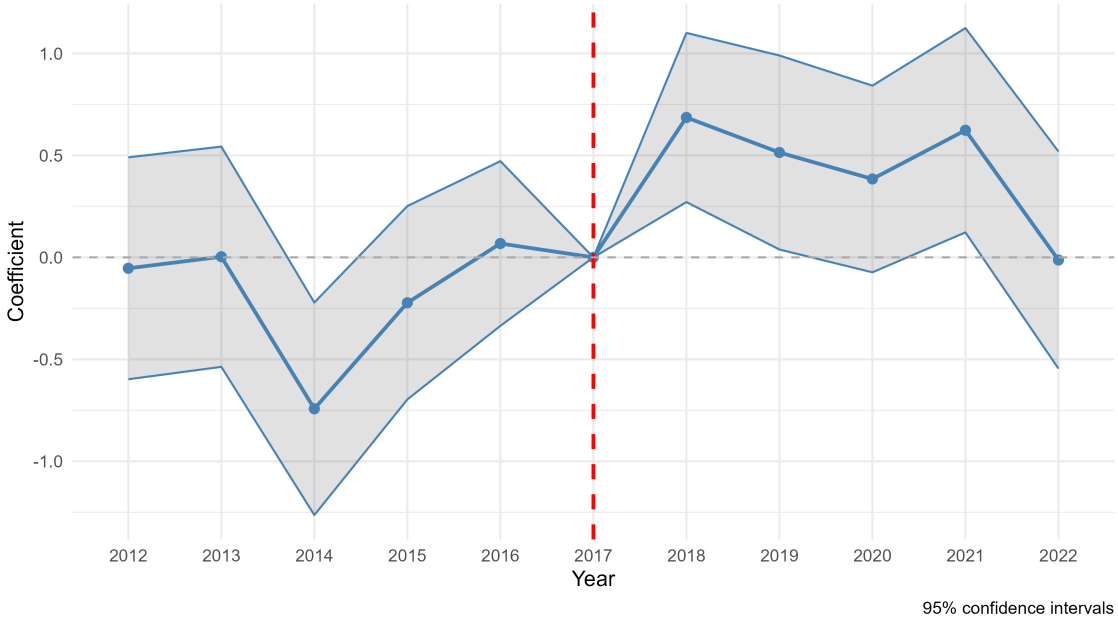
Overall, the results indicate that municipal-owned enterprises (MOEs) benefited from the reform in terms of both productivity and profitability. The post-reform improvements following the internalization of previously outsourced activities suggest that, prior to the reform, these services were procured under contracts that were relatively profitable for service providers. Granting permanent positions to subcontracted workers and producing these services in-house appears to have been advantageous for MOEs, enabling them to reduce input costs or improve operational efficiency while maintaining service provision. We test this mechanism by using the logarithm of input expenditure per worker-day as the outcome variable and estimating Equation (7). Figure 16 shows a sharp decline in input usage among MOEs after the outsourcing ban relative to other firms, consistent with the mechanism described above. The corresponding average effects are reported in the column (3) of Table A6.

Figure 14: Productivity effect of the outsourcing ban on MOEs



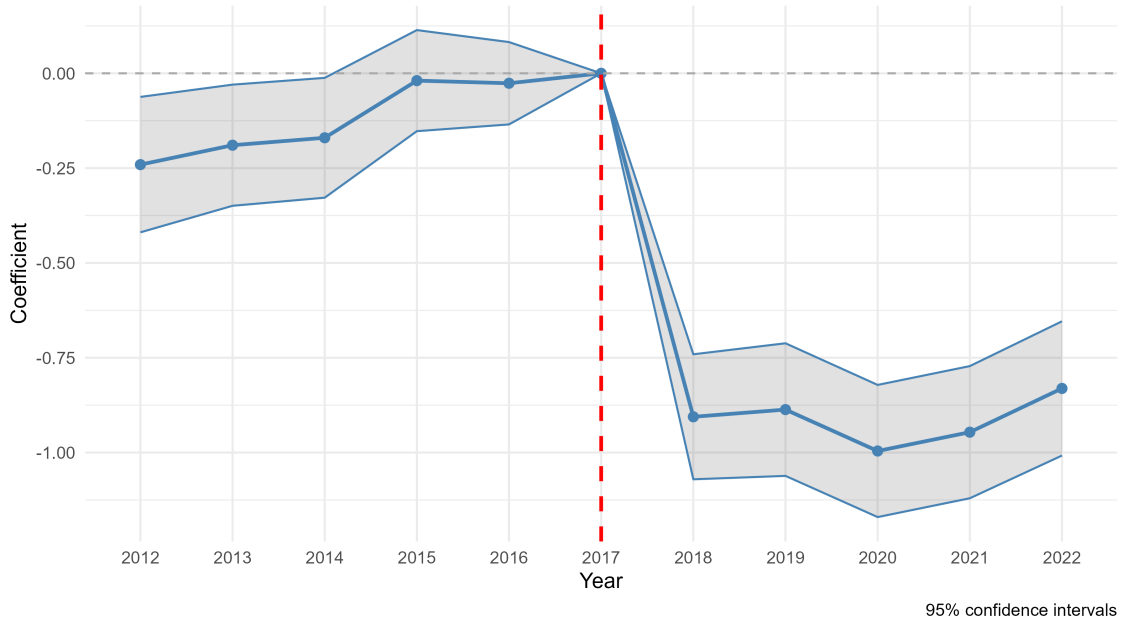
Notes: This figure shows the productivity effects of the outsourcing ban estimated using Equation (7) with a binary treatment measure, which takes value of one if firm is MOE. The sample includes all firms that share the same four-digit NACE industry with MOEs in the dataset. Standard errors are clustered at the firm level. The regressions are based on 4,253,412 observations. The quarter immediately preceding the reform (2017), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table A6.

Figure 15: Profitability effect of the outsourcing ban on MOEs



Notes: This figure shows the profitability effects of the outsourcing ban estimated using Equation (7) with a binary treatment measure, which takes value of one if firm is MOE. The sample includes all firms that share the same four-digit NACE industry with MOEs in the dataset. Standard errors are clustered at the firm level. The regressions are based on 4,481,099 observations. The quarter immediately preceding the reform (2017), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table A6.

Figure 16: Input usage effect of the outsourcing ban on MOEs



Notes: This figure shows the input usage effects of the outsourcing ban estimated using Equation (7) with a binary treatment measure, which takes value of one if firm is MOE. The sample includes all firms that share the same four-digit NACE industry with MOEs in the dataset. Standard errors are clustered at the firm level. The regressions are based on 3,847,796 observations. The quarter immediately preceding the reform (2017), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table A6.

6 Spillover Results

Thus far, we have focused on the direct effects of the outsourcing ban, documenting how treated workers and firms responded to the policy reform. However, such a large-scale reallocation of labor is also likely to affect LLMs more broadly. In particular, public institutions absorbed a substantial number of workers who would otherwise have remained in the private sector. These workers—especially low-skilled workers—typically exhibit high job mobility and turnover over their working life cycle. Their withdrawal from (LLM) may therefore generate spillover effects for non-treated workers through multiple channels.

In particular, there are two channels through which the reform may cause spillover effects. First, the reduction in the pool of available outsourcing contracts may decrease the number of job opportunities for remaining workers, potentially increasing labor market monopsony power and exerting downward pressure on wages. Second, the resulting labor scarcity may tighten labor markets for private-sector employers, increasing competition for workers and raising wages. The net effect on wages is therefore theoretically ambiguous and must be determined empirically. These changes in wage-setting conditions may in turn affect worker turnover behavior.

This section examines both the direction and magnitude of the reform's spillover effects on the wages and turnover behavior of remaining workers. To isolate these spillover effects, we exclude treated workers, i.e., switchers from outsourced firms to MOEs or to central government institutions, and MOE employees from the sample.

Our first outcome variable is wages. We measure LLM wages at the province–occupation level, using province (p) and occupation (o) cells as the unit of analysis. This level of aggregation is appropriate given that commuting across provinces is limited relative to commuting across districts (*ilçeler* in Turkish), which constitute the most disaggregated geographic unit in our data. For each province–occupation–period cell, we compute the logarithm of mean monthly wages.

We then estimate the following event-study specification, which is similar in spirit to the triple-difference approach in Felix and Wong (2025):

$$y_{opt} = \sum_{\rho=2016q1, \rho \neq 2017q4}^{2022q4} \beta_t \times \ exposure_{op} \times (T_{op} \times 1_{t=\rho}) + D_{op} + D_{ot} + D_{pt} + \varepsilon_{opt} \quad (8)$$

The equation (8) where outcome y_{opt} represents the log of average real wage in occupation o and province p for the period t as we explained above. The variable $exposure_{op}$ shows what extent the LLM is affected by the policy shock. Unsurprisingly, not all regions and occupations have been equally affected by the reform. LLMs in provinces with high number of public institutions and MOEs might have been more exposed to the reform. Accordingly, our exposure variable is the intensity of switcher workers in total employment in a given province-occupation pair:

$$exposure_{op} = \frac{\text{number of switchers in occupation } o \text{ and province } p}{\text{total employees in occupation } o \text{ and province } p} \quad (9)$$

Importantly, the numerator in equation (9) includes all workers who switch to MOEs or central government institutions, while the denominator reflects total employment in the corresponding LLM. As in the firm-level analysis, exposure is measured using pre-reform period employment shares to mitigate concerns about endogeneity. As such, identification comes from differential exposure to the outsourcing ban across province–occupation cells.

The regression specification also includes province–occupation fixed effects (D_{op}), which absorb time-invariant differences in wage levels across LLMs, such as persistent productivity differences or occupational composition. Province–time fixed effects (D_{pt}) control for aggregate local shocks, including region-specific economic conditions and policy changes, while occupation–time fixed effects (D_{ot}) account for nationwide occupation-specific trends in wages.

We next examine spillover effects on worker turnover. The outcome variable is a worker-level indicator equal to one if a firm–worker separation occurs for non-switcher worker i in period t. The identification strategy closely follows the direct turnover specification in Equation (2), with the key difference being that treatment intensity varies at the province–occupation level rather than at the firm level.

Specifically, we estimate the following discrete-time hazard model using PPML:

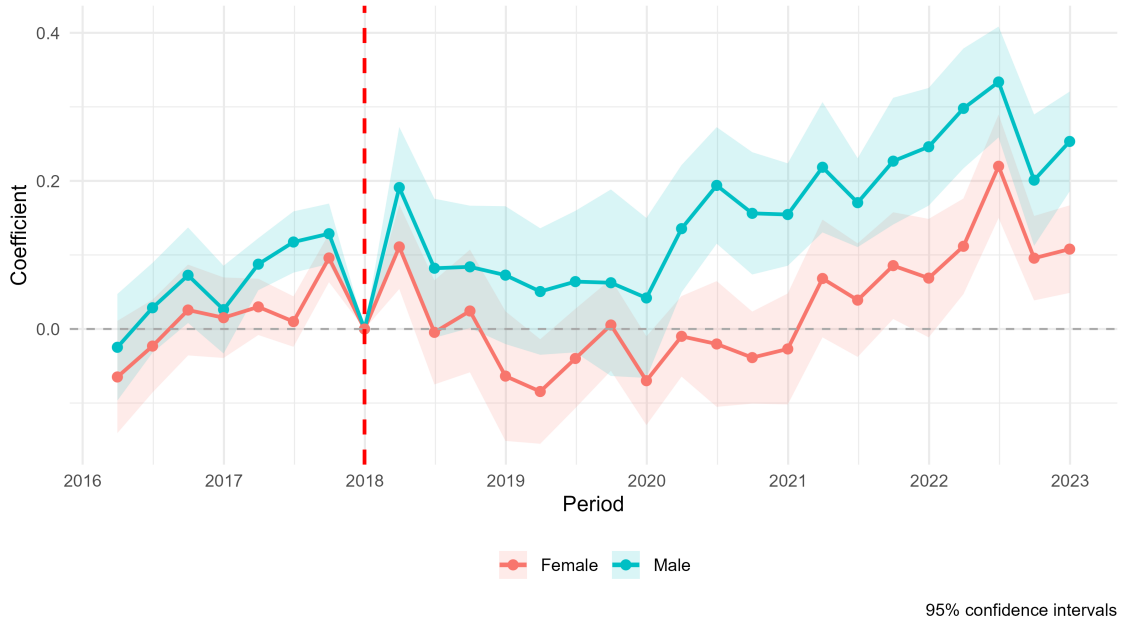
$$Turnover_{it} = \exp\Big(\Gamma_{employee\ tenure} + \Gamma_{time} + \Gamma_{province} + \alpha\ switcher_i + \\ + \sum_{\rho=2016q1, \rho\neq 2017q4}^{2022q4} \beta_\rho \times\ exposure_{op} \times (T_{op} \times 1_{t=\rho})\Big) \cdot \varepsilon_{it} \qquad (10)$$

where $exposure_{op}$ defined as above and coefficients β_ρ trace the dynamic spillover effects of the outsourcing ban on worker turnover.

In Figure 17, we present event-study estimates of wage spillovers obtained from Equation (8), separately for male and female workers. Coefficients in the pre-treatment period (to the left of the red dashed line) are close to zero, indicating no evidence of differential pre-trends. Following the reform, coefficients for male workers turn positive and become statistically significant two to three years after implementation, suggesting that wage spillover effects of the outsourcing ban materialize in the medium run. This pattern is consistent with a tightened LLM mechanism.

Quantitatively, using the average post-treatment estimates reported in the first column of Table A7, a 10% higher intensity of switchers in a given region–occupation cell is associated with at least a 1.2% wage increase for non-switcher male workers. In contrast, the fourth column of the same table shows no statistically significant average effect for female workers, implying a zero wage spillover on average. Nevertheless, the event-study estimates in Figure 17 indicate positive, albeit smaller and delayed, wage spillovers for female workers in the longer run (four-to-five years after the reform), relative to their male counterparts.

Figure 17: Wage spillover effect of the outsourcing ban



Notes: This figure shows the wage spillover effects of the outsourcing ban on workers who did not switch from outsourced firms to municipal-owned enterprises (MOEs), estimated using Equation (8). Switcher workers and MOE employees are excluded from the sample. Exposure is defined within a continuous treatment framework as the share of switcher workers in total employment in a given province and two-digit ISCO occupation, measured immediately prior to the reform. The outcome variable is the log of mean quarterly wages at occupation-province-time level. Standard errors are clustered at the province $\times$ two-digit ISCO and time level. Regressions for male workers include 113,336 observations, while regressions for female workers include 108,619 observations. The quarter immediately preceding the reform (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table A7.

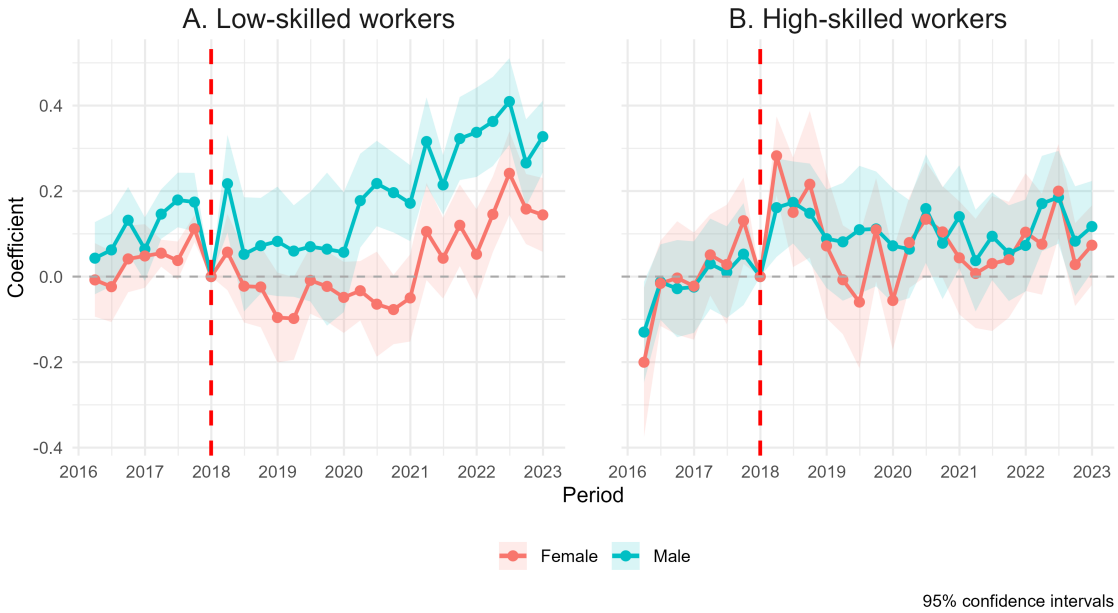
Figure 18 presents wage spillover effects by workers' skill orientation. For low-skilled male and female workers (Panel A), the results closely replicate the main findings from the baseline analysis. The corresponding average post-treatment estimates for low-skilled males and females are reported in Appendix Table A7. In contrast, wage spillover effects for high-skilled workers are considerably weaker. For both men and women, the estimated effects are smaller in magnitude and statistically indistinguishable from zero.

In Appendix B, we further examine wage spillover effects by age group and gender. Figure B2 and Table B2 show that wage spillovers are positive and statistically significant primarily for low-skilled male workers aged 30–39, 40–49, and 50–64.

We also explore in Appendix C the spillover effects of the reform on the age composition of LLMs. Using Equation (8) with average age at the province–occupation level as the outcome variable, we find that the outsourcing ban increases the mean age of male workers by approximately one year in a fully exposed LLM, as reported in Table C1. This pattern

suggests that the outsourcing ban drew older workers, who may have been attracted by higher wages, into employment.

Figure 18: Wage spillover effect of the outsourcing ban, low- and high-skilled workers



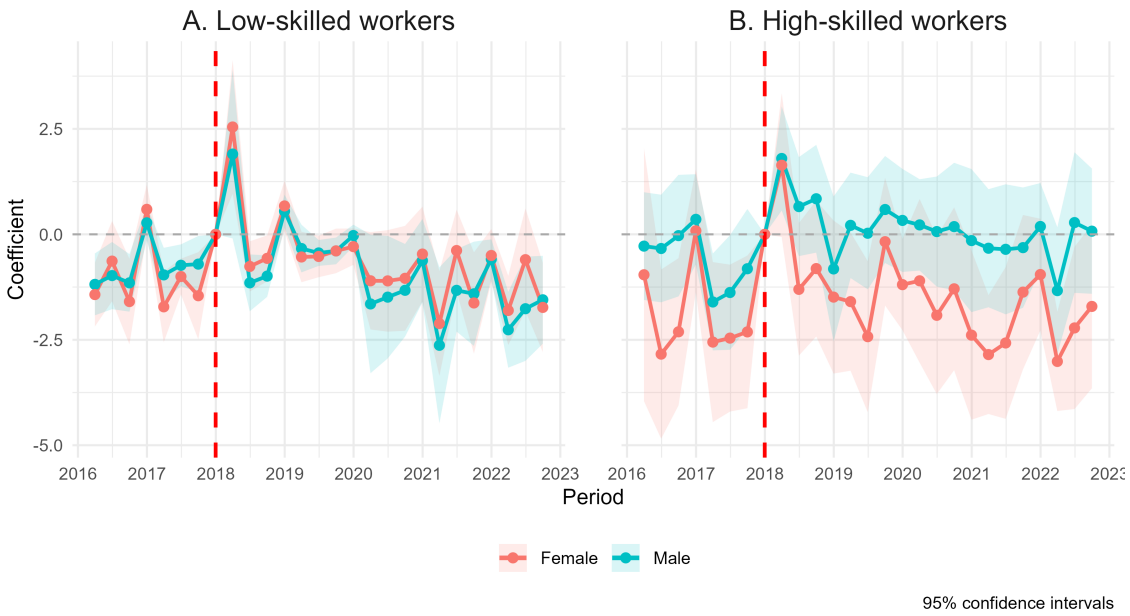
Notes: This figure shows the wage spillover effects of the outsourcing ban on workers who did not switch from outsourced firms to municipal-owned enterprises (MOEs), estimated using Equation (8) for different subsamples defined by workers' skill levels. Panel A presents the effects for low-skilled workers, while Panel B presents the effects for high-skilled workers. Skill groups are defined using the first-digit ISCO classification: workers with first-digit ISCO codes between 1 and 3 are classified as high-skilled, while all others—excluding interns and individuals with unknown occupations—are classified as low-skilled. Switcher workers and MOE employees are excluded from the sample. Exposure is defined within a continuous treatment framework as the share of switcher workers in total employment in a given province and two-digit ISCO occupation, measured immediately prior to the reform. The outcome variable is the log of mean quarterly wages at occupation-province-time level. Standard errors are clustered at the province $\times$ two-digit ISCO and time level. Regressions for male workers include 69,647 observations, while regressions for female workers include 65,501 observations in Panel A. In Panel B, regressions for male workers include 43,627 observations, while regressions for female workers include 42,961 observations. The quarter immediately preceding the reform (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table A7.

Figure 19 presents the worker turnover spillover effects of the outsourcing ban. Panel A shows that turnover among low-skilled male and female workers declines after the reform, indicating that non-switcher workers are more likely to remain with their current employers. The magnitude of the effect is similar across genders and economically modest. Quantitatively, as reported in Table A8, a 10 percentage point increase in exposure reduces the turnover probability by at least 5 percent $(= |exp(-0.5895 \times 0.1) - 1|)$, relative to province–occupation pairs with lower exposure.

In contrast, we find no evidence of turnover spillover effects for high-skilled male workers. For high-skilled female workers, however, turnover declines modestly following the reform, as shown in Figure 19. This corresponds to an approximately 9 percent lower probability of changing firms in highly exposed province–occupation cells compared to those with lower exposure.

Taken together, the results in this section yield two main conclusions. First, the outsourcing ban in the public sector generates spillover effects beyond directly treated workers, affecting non-switcher workers performing similar tasks in LLMs. Second, these spillover effects are heterogeneous by skill level and gender and are primarily concentrated among low-skilled male workers. The observed reductions in turnover among non-switcher workers are consistent with the wage spillover effects documented earlier, suggesting that improved wage conditions may have increased job retention following the reform.

Figure 19: Turnover spillover effect of the outsourcing ban, low- and high-skilled workers



Notes: These figures show the turnover spillover effects of the outsourcing ban estimated using Equation (10) for subsamples defined by workers' skill levels. Switcher workers who transferred from outsourced firms to municipal-owned enterprises (MOEs), as well as MOE workers, are excluded from the analysis. Exposure is defined within a continuous treatment framework as the ratio of switcher workers to total employment in a given two-digit ISCO occupation and province, measured immediately prior to the outsourcing ban. The model is estimated using PPML and captures worker–firm separation risks. Panel A presents the effects for low-skilled workers, while Panel B presents the effects for high-skilled workers. Skill groups are defined using the first-digit ISCO classification: workers with first-digit ISCO codes between 1 and 3 are classified as high-skilled, while all remaining workers—excluding interns and individuals with missing or unknown occupations—are classified as low-skilled. All observations in the worker-level dataset are used to estimate separation effects. Standard errors are clustered at the industry and firm-tenure level. In Panel A, regressions for male workers include 242,608,231 observations, while regressions for female workers include 90,341,645 observations. In Panel B, regressions for male workers include 61,757,102 observations, while regressions for female workers include 31,889,341 observations. The quarter immediately preceding the reform (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table A8.

7 Welfare Effects of the Reform: A Commentary

The outsourcing ban enacted in December 2017 was large in scale and abrupt in implementation. Given its scope, the reform is likely to have non-negligible implications for social welfare. Providing a precise aggregate welfare evaluation is difficult, as the reform affected multiple actors through several channels. Nevertheless, we can discuss the likely directions of welfare changes associated with its main effects.

To fix ideas, consider Türkiye as a small open economy with a utilitarian social welfare

function that places some weight on inequality. Over the period of interest, the economy is characterized by a persistent trade deficit, exchange rate volatility, double-digit inflation, and growth rates around 5–7%. Approximately 47% of the workforce earns the minimum wage, close to the poverty line; unemployment stands at around 10%, and informality at 16%.

The most direct effects of the reform were increases in wages and reductions in turnover for workers who transitioned into permanent public positions. As shown in Table 1, the majority of treated workers are low-skilled and perform tasks such as cleaning and security. Importantly, after the transition, these workers continue performing the same tasks in the same physical locations, but under public employment contracts and with higher wages and greater job security. This suggests little or no loss in output associated with low-skilled labor, while higher earnings and reduced job instability unambiguously raise welfare from a utilitarian perspective.

Potential output losses may arise for a limited group of higher-skilled workers who previously operated within specialized service firms and may no longer benefit from economies of scale or scope after the transition. However, the number of such workers affected by the reform is small, implying that any efficiency loss along this margin is likely to be limited. Overall, the direct wage and job security effects of the reform can therefore be viewed as net positive.

A common concern is that municipalities and municipal-owned enterprises (MOEs) would be negatively affected, as they now have to directly finance higher wages instead of purchasing services through outsourcing contracts. If this were the case, one would expect higher output prices, increased tax burdens, or adverse effects on employment and growth. However, the evidence in Section 5.3 shows that MOEs become more productive and more profitable after the reform. This suggests that pre-reform outsourcing contracts were highly advantageous to service providers and relatively costly for MOEs. While we cannot directly assess whether similar effects hold for central government institutions, the observed improvements for MOEs weaken the argument that the reform imposed substantial fiscal or efficiency costs on public entities.

Outsourced service providers, as documented in Sections 5.1 and 5.2, emerge as the main losers of the reform. For these firms, government contracts were not merely a component of their business but often essential for continued operation. At the same time, Section 6 shows that spillover effects of the reform raised wages and reduced turnover among workers who remained in the private sector, which constitutes an additional welfare gain from a utilitarian perspective. However, the loss of government contracts, combined with rising labor costs (particularly important given the labor-intensive nature of activities such as cleaning and security) placed substantial pressure on outsourced firms. As a result, many firms exited the market or downsized significantly, and those that remained may

have responded by increasing prices in their private-sector contracts.

Nevertheless, the contraction of outsourced service firms likely increased input costs for private-sector clients, either through higher wages for in-house workers or higher prices for remaining service contracts. These cost increases may have reduced competitiveness in parts of the private sector and represent an offsetting channel through which the reform could have generated welfare losses.

Taken together, the welfare effects of the reform operate through opposing channels. On the positive side, the reform raised wages and job security for predominantly low-skilled workers, reduced turnover, and appears to have improved productivity and profitability in public entities, all of which are likely to increase social welfare in an inequality-averse, utilitarian framework. On the negative side, the contraction of outsourced service firms and the associated increase in private-sector input costs may have generated efficiency losses and reduced competitiveness.

8 Conclusion

In December 2017, the Turkish government banned public institutions and municipal-owned enterprises (MOEs) from procuring outsourced services and mandated that all workers providing such services be granted permanent public positions by June 2018, six months after the announcement. This paper exploits the outsourcing ban as a quasi-experimental policy shock and provides a comprehensive assessment of its labor-market implications using administrative employer–employee data.

Our main findings are threefold. First, we document substantial direct effects of the reform on worker-level outcomes. Workers absorbed into municipal and central government positions experienced large and persistent wage gains, with effects being particularly pronounced for male workers, on the order of 10–15 percent. In addition, the transition to permanent public employment led to sharp and sustained declines in worker turnover, indicating improvements in job security.

Second, we examine firm-level consequences of the outsourcing ban. The reform imposed negative effects on private firms that had previously supplied outsourced services to the public sector. Firms with greater pre-reform exposure to MOE contracts experienced pronounced contractions in employment and significantly higher exit probabilities. For example, a 10% employment exposure to MOE contracts increased exit probability by 12% and reduced expected firm size by 26%, relative to firms with no exposure. Moreover, productivity and profitability of more exposed firms deteriorated markedly after the reform, suggesting that public outsourcing contracts were valuable and not easily replaced by alternative sources of demand.

We extend the firm-level analysis by studying the performance of MOEs that internalized previously outsourced services, which constitutes a key contribution of this paper. We find that MOEs experienced increases in both productivity and profitability following the reform. Our findings regarding firm-level outcomes suggest that, prior to the reform, outsourced service provision was relatively costly for MOEs and generated substantial rents for private service providers.

Third, the reform generated non-trivial spillover effects in local labor markets. We show that province–occupation cells with higher concentrations of absorbed workers experienced modest positive wage spillovers for male non-switchers—for example, wage increases of roughly 1–2 percent associated with a 10 percentage point increase in exposure—as well as reductions in worker turnover among low-skilled workers. These spillovers are heterogeneous by skill and gender: effects are concentrated among low-skilled, prime-age male workers.

Overall, the evidence suggests that the outsourcing ban entailed meaningful distributional and efficiency consequences, with welfare effects operating through offsetting channels. The reform improved wages and job stability for a large group of predominantly low-skilled workers, generated positive spillovers in local labor markets, and appears to have enhanced productivity and profitability within public entities that internalized service provision. These gains point to welfare improvements in an inequality-averse, utilitarian framework. At the same time, the contraction and exit of outsourced service firms, together with potential increases in private-sector input costs, highlight the presence of efficiency losses on the firm side. Taken together, our findings indicate that the reform primarily reallocated rents away from private service providers toward workers and public entities, yielding net welfare gains for disadvantaged workers while imposing concentrated costs on outsourced firms and, indirectly, parts of the private sector.

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A Difference-in-Differences estimates

Table A1: Direct wage effect of the outsourcing ban

	Male				Female			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
$switcher_i \times D_{t>Mar\ 2018}$	0.0867***	0.0985***	0.1003***	0.1325***	0.0547***	0.0600***	0.0599***	0.0882***
	(0.0042)	(0.0041)	(0.0042)	(0.0044)	(0.0056)	(0.0056)	(0.0056)	(0.0062)
Fixed-effects								
Worker	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age group		Yes	Yes	Yes		Yes	Yes	Yes
Occupation			Yes	Yes			Yes	Yes
Province × time				Yes				Yes
Fit statistics								
Observations	200,316,676	200,316,676	200,034,088	200,011,503	64,529,703	64,529,703	64,474,549	64,458,874
R^2	0.63681	0.63875	0.63883	0.64101	0.62077	0.62222	0.62258	0.62430
Within R^2	0.00033	0.00042	0.00044	0.00074	5.84×10^{-5}	7.06×10^{-5}	7.03×10^{-5}	0.00015

Notes: This table reports the direct wage effects of the 2018 outsourcing ban on workers who switched from outsourced firms to municipally owned enterprises (MOEs), estimated using Equation (1). Dependent variable is the log of real monthly wage of worker i in period t. Columns (1)–(4) present estimates for male workers, while columns (5)–(8) present estimates for female workers. The variable $switcher_i$ is an indicator equal to one for workers who transferred from an outsourced firm to an MOE, and $D_{t>Mar\ 2018}$ is a post-reform indicator. The coefficient of interest captures the average post-reform wage change for switchers relative to non-switchers. To ensure comparability between treatment and control groups, all individuals are required to be employed in both 2017q4 and 2018q1; the panel is otherwise unbalanced. All specifications include worker and time fixed effects, with additional controls introduced progressively, including age-group fixed effects, occupation fixed effects, and province-by-time fixed effects. The set of fixed effects, goodness-of-fit measures (R^2 and within R^2), and the number of observations are reported in the table. Standard errors are clustered at the firm level and reported in parentheses. Preferred specifications are column (4) for males and column (8) for females. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. The corresponding event-study estimates for the preferred specifications are reported in Figure 6.

Table A2: Direct wage effect of the outsourcing ban by skill group

	Male		Female	
	Low-skill	High-skill	Low-skill	High-skill
Model:	(1)	(2)	(3)	(4)
Variables				
$switcher_i \times D_{t>Mar\ 2018}$	0.1365***	0.0696***	0.0885***	0.0761***
	(0.0044)	(0.0071)	(0.0066)	(0.0112)
Fixed-effects				
Worker	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes
Age group	Yes	Yes	Yes	Yes
Occupation	Yes	Yes	Yes	Yes
Province $\times$ time	Yes	Yes	Yes	Yes
Fit statistics				
Observations	157,259,722	42,692,812	46,403,415	18,005,078
R^2	0.57793	0.77315	0.53631	0.73431
Within R^2	0.00090	5.44×10^{-5}	0.00016	5.66×10^{-5}

Notes: This table reports the direct wage effects of the 2018 outsourcing ban on workers who switched from outsourced firms to municipally owned enterprises (MOEs), estimated using Equation (1) for subsamples defined by workers' skill levels. Dependent variable is the log of real monthly wage of worker i in period t. Columns (1) and (3) report estimates for low-skilled workers, while columns (2) and (4) report estimates for high-skilled workers. Skill groups are defined based on the first-digit ISCO classification: workers with first-digit ISCO codes 1–3 are classified as high-skilled, while all other workers—excluding interns and individuals with unknown occupations—are classified as low-skilled. Columns (1) and (2) present estimates for male workers, and columns (3) and (4) present estimates for female workers. The variable $switcher_i$ is an indicator equal to one for workers who transferred from an outsourced firm to an MOE, and $D_{t>Mar\ 2018}$ is a post-reform indicator. The coefficient of interest captures the average post-reform wage change for switchers relative to non-switchers. To ensure comparability between treatment and control groups, all individuals are required to be employed in both 2017q4 and 2018q1; the panel is otherwise unbalanced. All specifications include worker and time fixed effects, as well as age-group, occupation, and province-by-time fixed effects. The set of fixed effects, goodness-of-fit measures (R^2 and within R^2), and the number of observations are reported in the table. Standard errors are clustered at the firm level and reported in parentheses. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. The corresponding event-study estimates are reported in Figure 7.

Table A3: Worker turnover effect of the outsourcing ban

	Male			Female		
	All workers	Low-skill	High-skill	All workers	Low-skill	High-skill
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
$switcher_i$	0.1328	0.1159	0.2022	0.2877	0.2556	0.4248*
	(0.2226)	(0.2210)	(0.2328)	(0.1956)	(0.1897)	(0.2182)
$switcher_i \times D_{t>Mar\ 2018}$	-1.800***	-1.840***	-1.460***	-1.934***	-2.040***	-1.654***
	(0.1664)	(0.1689)	(0.1900)	(0.1521)	(0.1714)	(0.1706)
Fixed-effects						
Firm tenure	Yes	Yes	Yes	Yes	Yes	Yes
Province	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	319,481,352	254,033,638	63,491,043	125,908,857	91,995,276	32,580,754
Squared Correlation	0.11134	0.11742	0.08421	0.06160	0.06864	0.03695
Pseudo R^2	0.08922	0.09124	0.07684	0.05586	0.06014	0.03945
BIC	285,421,141.5	233,099,843.7	49,390,152.9	104,463,099.2	77,698,738.1	24,550,526.1

Notes: This table reports the effects of the 2018 outsourcing ban on worker turnover (worker–firm separations) among workers who switched from outsourced firms to municipally owned enterprises (MOEs), estimated using Equation (2) with a Poisson pseudo–maximum likelihood (PPML) estimator. Columns (1)–(3) present estimates for male workers, while columns (4)–(6) present estimates for female workers. Columns (1) and (4) report estimates for all workers, columns (2) and (5) for low-skilled workers, and columns (3) and (6) for high-skilled workers. Skill groups are defined based on the first-digit ISCO classification: workers with first-digit ISCO codes 1–3 are classified as high-skilled, while all other workers—excluding interns and individuals with unknown occupations—are classified as low-skilled. The dependent variable is a binary indicator equal to one if a worker leaves firm i in period t and is observed working at a different firm in period $t+1$. Coefficients can therefore be interpreted within a discrete-time hazard framework for worker–firm separation. Additional details on the construction of this variable are provided in Section 4.2. The variable $switcher_i$ is an indicator equal to one for workers who transferred from an outsourced firm to an MOE, and $D_{t>Mar\ 2018}$ is a post-reform indicator. The coefficient of interest captures the post-reform change in the probability of separation for switchers relative to non-switchers. All specifications include firm-tenure, province, and time fixed effects. The set of fixed effects, goodness-of-fit measures (squared correlation, pseudo R^2, and BIC), and the number of observations are reported in the table. Standard errors are clustered at the industry-by–firm-tenure level and reported in parentheses. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. The corresponding event-study estimates are reported in Figures 8 and 9.

Table A4: Firm size effect of the outsourcing ban by skill group of industries

Exposure:	Employment	Wage	Employment	Employment
	All sample	All sample	Low-skill	High-skill
Model:	(1)	(2)	(3)	(4)
Variables				
$log(initial\ employment_f)$	0.8968***	0.8968***	0.8551***	0.9251***
	(0.0286)	(0.0286)	(0.0332)	(0.0231)
$expoosure_f$	0.3009***	0.3042***	0.2223***	0.7275***
	(0.0698)	(0.0691)	(0.0485)	(0.2473)
$expoosure_f \times D_{t>Mar\ 2018}$	-2.893***	-2.754***	-2.928***	-2.522***
	(0.2441)	(0.2538)	(0.1945)	(0.2531)
Fixed-effects				
Industry	Yes	Yes	Yes	Yes
Firm age	Yes	Yes	Yes	Yes
Province	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes
Fit statistics				
Observations	35,533,481	35,533,481	3,343,157	3,002,631
Squared Correlation	0.67267	0.67260	0.68207	0.44928
Pseudo R^2	0.72276	0.72276	0.76710	0.60626
BIC	399,112,013.6	399,116,676.7	54,625,036.7	26,991,592.7

Notes: This table reports the firm-size effects of the 2018 outsourcing ban, estimated using Equation (5) with PPML. The dependent variable is firm employment of firm t in period t. Columns (1) and (2) report estimates for the full sample, using employment-based exposure and wage-bill–based exposure, respectively. Column (3) reports estimates for firms operating in low-skilled industries, while column (4) reports estimates for firms operating in high-skilled industries. All specifications control for initial firm employment, denoted by $\log(initial\ employment_f)$. The variable $exposure_f$ is a continuous treatment measure defined as the ratio of switcher workers to total firm employment prior to the reform, and $D_{t>Mar\ 2018}$ is a post-reform indicator. In column (2), $exposure_f$ is constructed using the wage-bill share of switcher workers as a robustness check. The coefficient on the interaction between $exposure_f$ and the post-reform indicator captures the combined effect of the reform on firm employment, reflecting both employment changes conditional on survival and firm exit. All specifications include industry, firm-age, province, and time fixed effects. The sample consists of firms operating in the same NACE industries as outsourced firms. Industries are classified into two groups: (i) low-skilled business services, including food services, cleaning, security, and logistics; and (ii) high-skilled professional business services, including IT, programming, consultancy, engineering, and architecture. Goodness-of-fit measures (squared correlation, pseudo R^2, and BIC) and the number of observations are reported in the table. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. The corresponding event-study estimates are reported in Figures 10 and 11.

Table A5: Productivity and profitability effects of the outsourcing ban on the outsourced firms

Outcome:	$asinh(v.added/labor)$	$asinh(profit/labor)$
Model:	(1)	(2)
Variables		
$exposure_f \times D_{t>2018}$	-0.3362***	-0.7085***
	(0.1034)	(0.1452)
Fixed-effects		
Firm	Yes	Yes
Year	Yes	Yes
Province × year	Yes	Yes
Fit statistics		
Observations	6,917,598	7,224,876
R^2	0.47627	0.54120
Within R^2	3.36×10^{-6}	1.1×10^{-5}

Notes: This table reports the effects of the 2018 outsourcing ban on productivity and profitability of outsourced firms, estimated using Equation (6). Column (1) uses the inverse hyperbolic sine of value added per workday as the outcome, while column (2) uses the inverse hyperbolic sine of profit per workday. The treatment variable $exposure_f \times D_{t>2018}$ captures firms' exposure to the reform interacted with a post-reform indicator. Firm, year, and province × year fixed effects are included in all specifications. The sample consists of firms operating in the same NACE industries as outsourced firms. Standard errors are clustered at the firm level and reported in parentheses. The number of observations, R^2, and within R^2 are reported in the table. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. The corresponding event-study estimates are reported in Figures 12 and 13.

Table A6: Productivity and profitability effects of the outsourcing ban on the MOEs

Outcome:	$asinh(v.added/labor)$	$asinh(profit/labor)$	$log(input/labor)$
Model:	(1)	(2)	(3)
Variables			
$MOE_f \times D_{t>2018}$	0.6275***	0..5785***	-0.8279***
	(0.1250)	(0.1551)	(0.0669)
Fixed-effects			
Firm	Yes	Yes	Yes
Year	Yes	Yes	Yes
Province × year	Yes	Yes	Yes
Fit statistics			
Observations	4,245,042	4,470,050	3,847,796
R^2	0.44620	0.52388	0.71405
Within R^2	2.41×10^{-5}	7.49×10^{-6}	0.00018

Notes: This table reports the effects of the 2018 outsourcing ban on productivity, profitability, and input intensity of municipally owned enterprises (MOEs), estimated using Equation (7). Column (1) uses the inverse hyperbolic sine of value added per workday as the outcome, column (2) uses the inverse hyperbolic sine of profit per workday, and column (3) uses the logarithm of input per workday. The treatment variable $MOE_f \times D_{t>2018}$ is an interaction between an indicator for MOE firms and a post-reform indicator. Firm, year, and province × year fixed effects are included in all specifications. The sample consists of firms operating in the same NACE industries as MOEs. Standard errors are clustered at the firm level and reported in parentheses. The number of observations, R^2, and within R^2 are reported in the table. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. Corresponding event-study estimates are reported in Figures 14, 15, and 16, in the same order as the columns of this table.

Table A7: Wage spillover effect of the outsourcing ban

	Male			Female		
	All workers	Low-skill	High-skill	All workers	Low-skill	High-skill
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
$exposure_{op} \times D_{t>Mar\ 2018}$	0.1244***	0.1234**	0.1203*	-0.0209	-0.0062	-0.0382
	(0.0404)	(0.0465)	(0.0633)	(0.0379)	(0.0428)	(0.0694)
Fixed-effects						
Province × occupation	Yes	Yes	Yes	Yes	Yes	Yes
Occupation × time	Yes	Yes	Yes	Yes	Yes	Yes
Province × time	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	113,336	69,347	43,627	108,619	65,501	42,961
R^2	0.84983	0.75879	0.84935	0.66853	0.49678	0.72108
Within R^2	0.00138	0.00174	0.00098	2.25×10^{-5}	2.12×10^{-6}	6.65×10^{-5}

Notes: This table reports the wage spillover effects of the 2018 outsourcing ban on workers who did not switch from outsourced firms to municipally owned enterprises (MOEs), estimated using Equation (8). The dependent variable is the logarithm of mean quarterly wages at the occupation–province–time level. Columns (1)–(3) present estimates for male workers, while columns (4)–(6) present estimates for female workers. Columns (1) and (4) report estimates for all workers, columns (2) and (5) for low-skilled workers, and columns (3) and (6) for high-skilled workers. Skill groups are defined using the first-digit ISCO classification: workers with first-digit ISCO codes 1–3 are classified as high-skilled, while all other workers—excluding interns and individuals with unknown occupations—are classified as low-skilled. Switcher workers and MOE employees are excluded from the sample. The variable $exposure_{op}$ is a continuous treatment measure defined as the share of switcher workers in total employment within a given province and two-digit ISCO occupation, measured immediately prior to the reform, and $D_{t>Mar\ 2018}$ is a post-reform indicator. All specifications include province × occupation, occupation × time, and province × time fixed effects. The number of observations, R^2, and within R^2 are reported in the table. Standard errors are clustered at the province × ISCO and time level. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. Corresponding event-study estimates are reported in Figures 17 and 18.

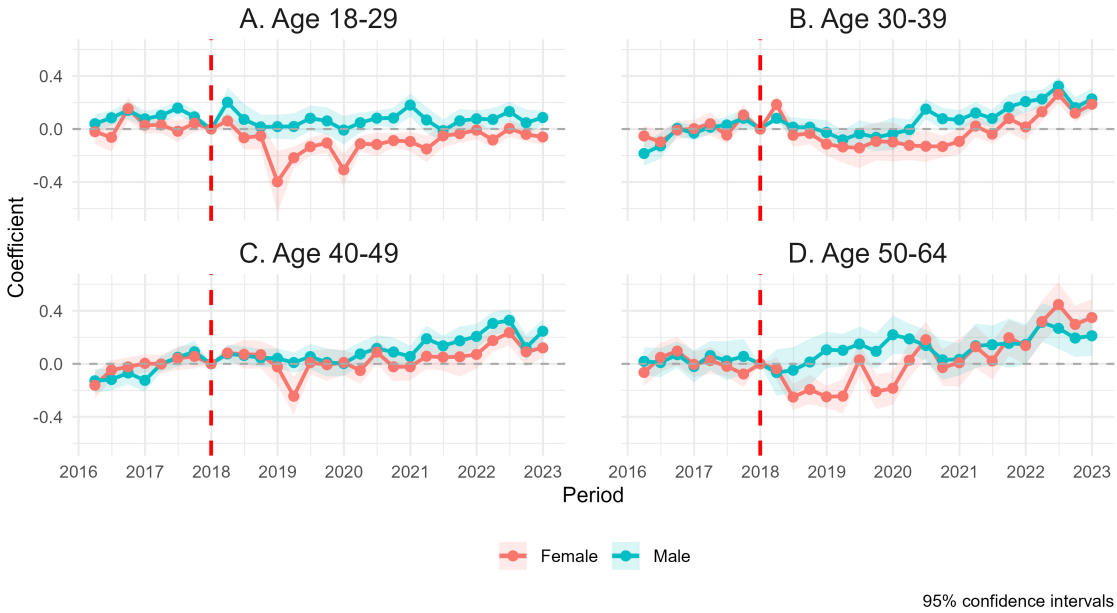
Table A8: Turnover spillover effect of the outsourcing ban

Model:	Male, all (1)	Male, low-skill (2)	Male, high-skill (3)	Female, all (4)	Female, low-skill (5)	Female, high-skill (6)
Variables						
$exposure_{op}$	-0.1972	-0.3053	0.3594	0.3337	0.1579	0.5499*
	(0.5051)	(0.5117)	(0.2521)	(0.3436)	(0.3729)	(0.3054)
$exposure_{op} \times D_{t>Mar\ 2018}$	-0.6242***	-0.7323***	-0.1834	-0.6066*	-0.5895**	-1.024**
	(0.2171)	(0.2092)	(0.2835)	(0.3287)	(0.2737)	(0.4947)
Fixed-effects						
Firm tenure	Yes	Yes	Yes	Yes	Yes	Yes
Province	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	304,367,679	242,608,231	61,757,102	122,231,676	90,341,645	31,889,341
Squared correlation	0.11179	0.11868	0.08442	0.05914	0.06841	0.03662
Pseudo R^2	0.08753	0.08951	0.07606	0.05359	0.05920	0.03869
BIC	275,474,915.9	226,466,137.6	48,487,774.4	101,191,426.2	76,771,041.5	24,208,499.9

Notes: This table reports the turnover spillover effects of the 2018 outsourcing ban on workers who did not switch from outsourced firms to municipally owned enterprises (MOEs), estimated using Equation (10) with a Poisson pseudo–maximum likelihood (PPML) estimator. The dependent variable is a binary indicator equal to one if a worker leaves firm i in period t and is observed working at a different firm in period $t+1$. Coefficients can therefore be interpreted within a discrete-time hazard framework for worker–firm separation. Additional details on the construction of this variable are provided in Section 4.2. Columns (1)–(3) present estimates for male workers, while columns (4)–(6) present estimates for female workers. Columns (1) and (4) report estimates for all workers, columns (2) and (5) for low-skilled workers, and columns (3) and (6) for high-skilled workers. Skill groups are defined using the first-digit ISCO classification: workers with first-digit ISCO codes 1–3 are classified as high-skilled, while all other workers—excluding interns and individuals with unknown occupations—are classified as low-skilled. Switcher workers and MOE employees are excluded from the sample. The variable $exposure_{op}$ is a continuous treatment measure defined as the share of switcher workers in total employment within a given province and two-digit ISCO occupation, measured immediately prior to the reform, and $D_{t>Mar\ 2018}$ is a post-reform indicator. All specifications include firm tenure, province, and time fixed effects. The number of observations, squared correlation, and pseudo R^2 are reported in the table. Standard errors are clustered at the industry and firm tenure level. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. Corresponding event-study estimates are reported in Figures 19.

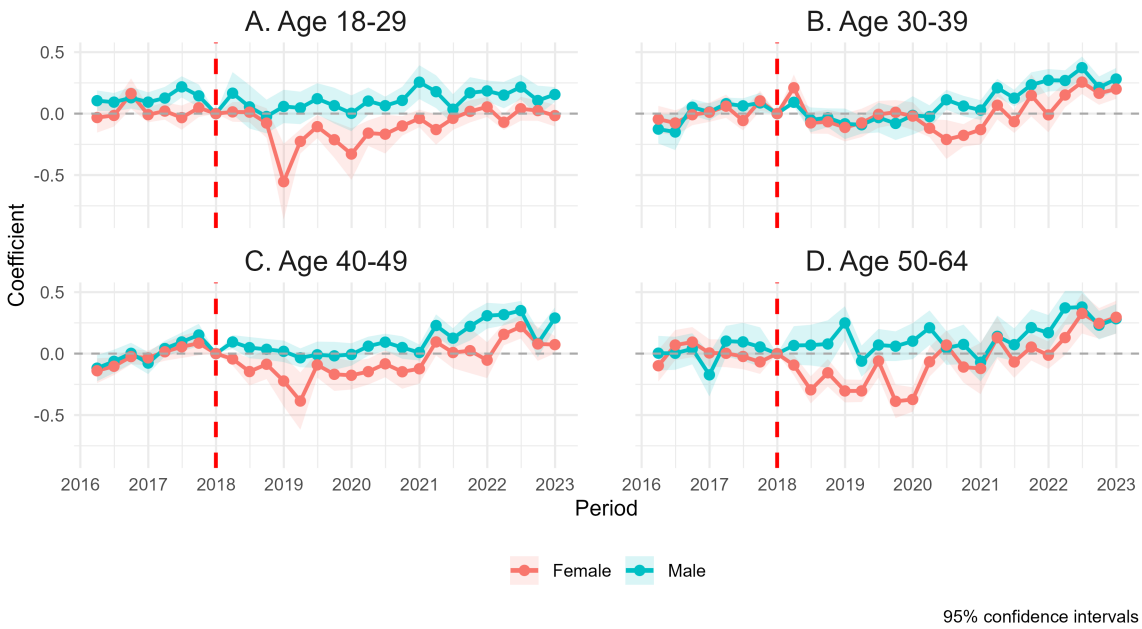
B Wage spillover effects by age group

Figure B1: Wage spillover effect of the outsourcing ban by age group



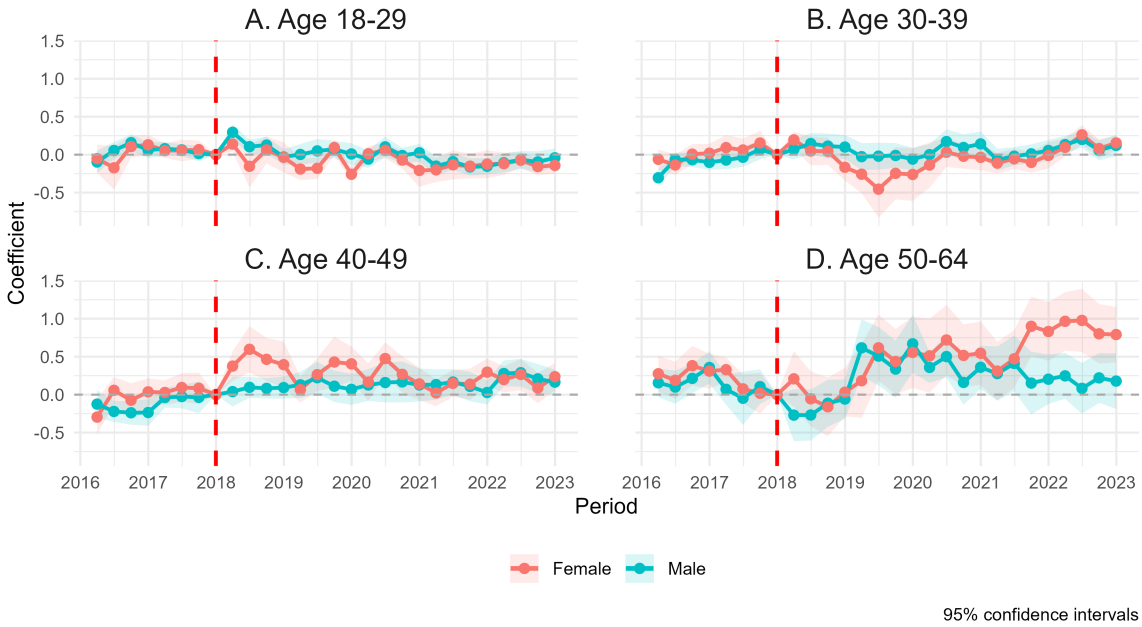
Notes: This figure shows the wage spillover effects of the outsourcing ban on workers who did not switch from outsourced firms to municipal-owned enterprises (MOEs), estimated using Equation (8) for different age groups and gender. Switcher workers and MOE employees are excluded from the sample. Exposure is defined within a continuous treatment framework as the share of switcher workers in total employment in a given province and two-digit ISCO occupation, measured immediately prior to the reform. The outcome variable is the log of mean quarterly wages at occupation-province-time level. Standard errors are clustered at the province $\times$ two-digit ISCO and time level. The quarter immediately preceding the reform (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates, number of observations and other fit-statistics are reported in Table B1.

Figure B2: Wage spillover effect of the outsourcing ban by age group, low-skilled workers



Notes: This figure shows the wage spillover effects of the outsourcing ban on low-skilled workers who did not switch from outsourced firms to municipal-owned enterprises (MOEs), estimated using Equation (8) for different age groups and gender. Switcher workers and MOE employees are excluded from the sample. Exposure is defined within a continuous treatment framework as the share of switcher workers in total employment in a given province and two-digit ISCO occupation, measured immediately prior to the reform. The outcome variable is the log of mean quarterly wages at occupation-province-time level. Standard errors are clustered at the province $\times$ two-digit ISCO and time level. The quarter immediately preceding the reform (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates, number of observations and other fit-statistics are reported in Table B2.

Figure B3: Wage spillover effect of the outsourcing ban by age group, high-skilled workers



Notes: This figure shows the wage spillover effects of the outsourcing ban on high-skilled workers who did not switch from outsourced firms to municipal-owned enterprises (MOEs), estimated using Equation (8) for different age groups and gender. Switcher workers and MOE employees are excluded from the sample. Exposure is defined within a continuous treatment framework as the share of switcher workers in total employment in a given province and two-digit ISCO occupation, measured immediately prior to the reform. The outcome variable is the log of mean quarterly wages at occupation-province-time level. Standard errors are clustered at the province $\times$ two-digit ISCO and time level. The quarter immediately preceding the reform (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates, number of observations and other fit-statistics are reported in Table B3.

Table B1: Wage spillover effect of the outsourcing ban by age group

	Male				Female			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age group:	18-29	30-39	40-49	50-64	18-29	30-39	40-49	50-64
Variables								
$exposure_{op} \times D_{t>Mar\ 2018}$	-0.0225	0.1415**	0.1758***	0.1291**	-0.1297***	-0.0637	0.0243	0.0798
	(0.0326)	(0.0462)	(0.0450)	(0.0570)	(0.0435)	(0.0511)	(0.0530)	(0.0786)
Fixed-effects								
Province × occupation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation × time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province × time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics								
Observations	111,819	112,259	111,196	106,040	103,149	103,591	100,781	86,183
R^2	0.67072	0.81919	0.81532	0.61118	0.48158	0.63451	0.65686	0.48197
Within R^2	3.3×10^{-5}	0.00141	0.00155	0.00014	0.00054	0.00014	1.61×10^{-5}	0 7.49×10^{-5}

Notes: This table reports the wage spillover effects of the 2018 outsourcing ban on workers who did not switch from outsourced firms to municipally owned enterprises (MOEs), estimated using Equation (8). The dependent variable is the logarithm of mean quarterly wages at the occupation–province–time level. Columns (1)–(4) present estimates for male workers, while columns (5)–(8) present estimates for female workers. Columns (1) and (5) report estimates for workers aged 18–29, columns (2) and (6) for workers aged 30–39, columns (3) and (7) for workers aged 40–49, and columns (4) and (8) for workers aged 50–64. Switcher workers and MOE employees are excluded from the sample. The variable $exposure_{op}$ is a continuous treatment measure defined as the share of switcher workers in total employment within a given province and two-digit ISCO occupation, measured immediately prior to the reform, and $D_{t>Mar\ 2018}$ is a post-reform indicator. All specifications include province × occupation, occupation × time, and province × time fixed effects. The number of observations, R^2, and within R^2 are reported in the table. Standard errors are clustered at the province × two-digit ISCO and time level. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. Corresponding event-study estimates are reported in Figure B1.

Table B2: Wage spillover effect of the outsourcing ban by age group, low-skilled workers

	Male				Female			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age group:	18-29	30-39	40-49	50-64	18-29	30-39	40-49	50-64
Variables								
$exposure_{op} \times D_{t>Mar\ 2018}$	0.0041	0.1229**	0.1576***	0.1695***	-0.0851	-0.0332	-0.0085	0.0132
	(0.0357)	(0.0483)	(0.0512)	(0.0617)	(0.0565)	(0.0494)	(0.0636)	(0.0742)
Fixed-effects								
Province × occupation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation × time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province × time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics								
Observations	68,411	68,617	68,145	66,301	60,909	62,078	61,235	54,238
R^2	0.64156	0.73656	0.73690	0.61778	0.41011	0.48832	0.48921	0.41436
Within R^2	1.48×10^{-6}	0.00137	0.00172	0.00085	0.00023	4.38×10^{-5}	2.82×10^{-6}	3.46×10^{-6}

Notes: This table reports the wage spillover effects of the 2018 outsourcing ban on low-skilled workers who did not switch from outsourced firms to municipally owned enterprises (MOEs), estimated using Equation (8). The dependent variable is the logarithm of mean quarterly wages at the occupation–province–time level. Columns (1)–(4) present estimates for male workers, while columns (5)–(8) present estimates for female workers. Workers with first-digit ISCO codes 4–9—excluding interns and individuals with unknown occupations—are classified as low-skilled. Columns (1) and (5) report estimates for workers aged 18–29, columns (2) and (6) for workers aged 30–39, columns (3) and (7) for workers aged 40–49, and columns (4) and (8) for workers aged 50–64. Switcher workers and MOE employees are excluded from the sample. The variable $exposure_{op}$ is a continuous treatment measure defined as the share of switcher workers in total employment within a given province and two-digit ISCO occupation, measured immediately prior to the reform, and $D_{t>Mar\ 2018}$ is a post-reform indicator. All specifications include province × occupation, occupation × time, and province × time fixed effects. The number of observations, R^2, and within R^2 are reported in the table. Standard errors are clustered at the province × two-digit ISCO and time level. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. Corresponding event-study estimates are reported in Figure B2.

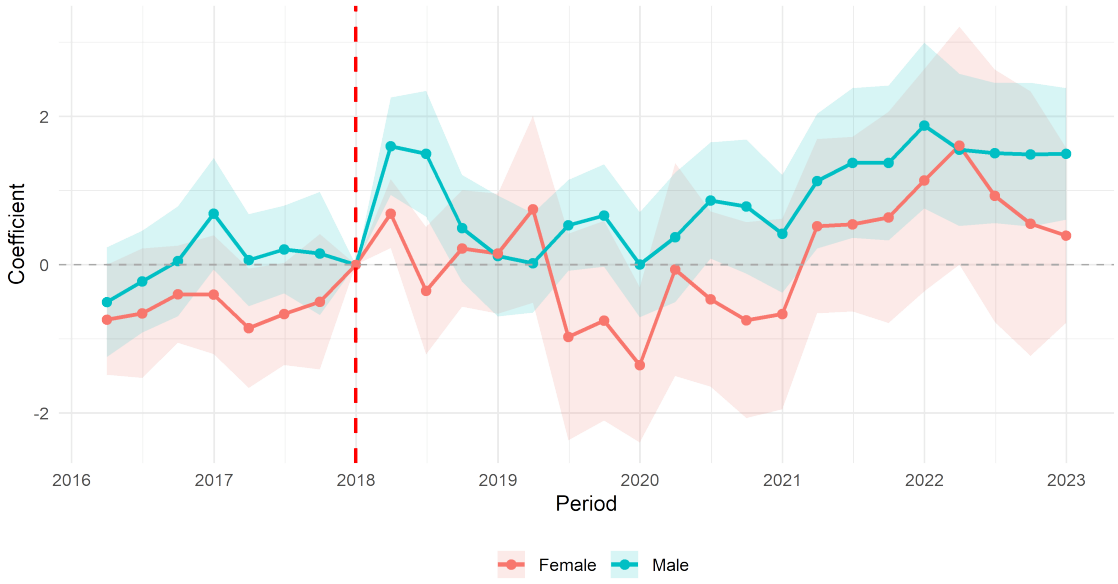
Table B3: Wage spillover effect of the outsourcing ban by age group, high-skilled workers

	Male				Female			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age group:	18-29	30-39	40-49	50-64	18-29	30-39	40-49	50-64
Variables								
$exposure_{op} \times D_{t>Mar\ 2018}$	-0.0651	0.1528*	0.2141**	0.1303	-0.2077***	-0.1507	0.0993	0.2828
	(0.0663)	(0.0780)	(0.0790)	(0.1481)	(0.0660)	(0.1035)	(0.1176)	(0.1763)
Fixed-effects								
Province × occupation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation × time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province × time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics								
Observations	43,239	43,440	42,897	40,248	42,153	41,796	39,461	31,141
R^2	0.59848	0.80073	0.79509	0.65450	0.51086	0.65641	0.68252	0.52831
Within R^2	0.00019	0.00116	0.00153	0.00018	0.00137	0.00060	0.00015	0.00044

Notes: This table reports the wage spillover effects of the 2018 outsourcing ban on high-skilled workers who did not switch from outsourced firms to municipally owned enterprises (MOEs), estimated using Equation (8). The dependent variable is the logarithm of mean quarterly wages at the occupation–province–time level. Columns (1)–(4) present estimates for male workers, while columns (5)–(8) present estimates for female workers. Workers with first-digit ISCO codes 1–3 are classified as high-skilled. Columns (1) and (5) report estimates for workers aged 18–29, columns (2) and (6) for workers aged 30–39, columns (3) and (7) for workers aged 40–49, and columns (4) and (8) for workers aged 50–64. Switcher workers and MOE employees are excluded from the sample. The variable $exposure_{op}$ is a continuous treatment measure defined as the share of switcher workers in total employment within a given province and two-digit ISCO occupation, measured immediately prior to the reform, and $D_{t>Mar\ 2018}$ is a post-reform indicator. All specifications include province × occupation, occupation × time, and province × time fixed effects. The number of observations, R^2, and within R^2 are reported in the table. Standard errors are clustered at the province × two-digit ISCO and time level. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. Corresponding event-study estimates are reported in Figure B3.

C Age spillover effects

Figure C1: Age spillover effect of the outsourcing ban



Notes: This figure shows the age spillover effects of the outsourcing ban on workers who did not switch from outsourced firms to municipal-owned enterprises (MOEs), estimated using Equation (8). Switcher workers and MOE employees are excluded from the sample. Exposure is defined within a continuous treatment framework as the share of switcher workers in total employment in a given province and two-digit ISCO occupation, measured immediately prior to the reform. The outcome variable is the mean age at occupation-province-time level. Standard errors are clustered at the province $\times$ two-digit ISCO and time level. Regressions for male workers include 113,336 observations, while regressions for female workers include 108,619 observations. The quarter immediately preceding the reform (2017q4), indicated by the vertical red dashed line, is used as the base period. The corresponding average difference-in-differences estimates are reported in Table C1.

Table C1: Age spillover effect of the outsourcing ban

Gender:	Male	Female
Model:	(1)	(2)
Variables		
$exposure_{op} \times D_{t>Mar\ 2018}$	1.024**	0.0423
	(0.4333)	(0.6354)
Fixed-effects		
Province × occupation	Yes	Yes
Occupation × time	Yes	Yes
Province × time	Yes	Yes
Fit statistics		
Observations	113,336	108,619
R^2	0.72602	0.73048
Within R^2	0.00072	5.54×10^{-7}

Notes: This table reports the age spillover effects of the 2018 outsourcing ban on workers who did not switch from outsourced firms to municipally owned enterprises (MOEs), estimated using Equation (8). The dependent variable is the mean quarterly ages at the occupation–province–time level. Column (1) presents estimates for male workers, while columns (2) presents estimates for female workers. Switcher workers and MOE employees are excluded from the sample. The variable $exposure_{op}$ is a continuous treatment measure defined as the share of switcher workers in total employment within a given province and two-digit ISCO occupation, measured immediately prior to the reform, and $D_{t>Mar\ 2018}$ is a post-reform indicator. All specifications include province × occupation, occupation × time, and province × time fixed effects. The number of observations, R^2, and within R^2 are reported in the table. Standard errors are clustered at the province × ISCO and time level. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. Corresponding event-study estimates are reported in Figure C1.

D Classification of outsourcing industries

Table D1: Skill classification of outsourcing industries

NACE code	Description
Low skill:	
5629	Other food service activities
8121	General cleaning of buildings
8122	Other building and industrial cleaning activities
8129	Other cleaning activities
8010	Private security activities
4920	Freight rail transport
4941	Freight transport by road
5020	Sea and coastal freight water transport
5040	Inland freight water transport
5121	Freight air transport
5210	Warehousing and storage
High skill:	
62*XX*	Computer programming, consultancy and related activities
63*XX*	Information service activities
69*XX*	Legal and accounting activities
70*XX*	Activities of head offices; management consultancy activities
71*XX*	Architectural and engineering activities
72*XX*	Scientific research and development
73*XX*	Advertising and market research
74*XX*	Other professional, scientific and technical activities

Notes: This table lists the NACE industries classified as low- and high-skilled outsourcing sectors. The classification is based on the skill intensity of core activities and is used in the firm-level analyses of firm size and firm survival (Section 5.1). See Gürer and Taymaz (2026) for detailed information on the classification.