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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 on-site 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 higher exit rates and, if they survived, declines in employment, productivity, and profitability. In contrast, municipal-owned enterprises that internalized service provision became more productive and profitable. We also document modest positive wage spillovers in local labor markets. Overall, our results suggest that the outsourcing ban reallocated 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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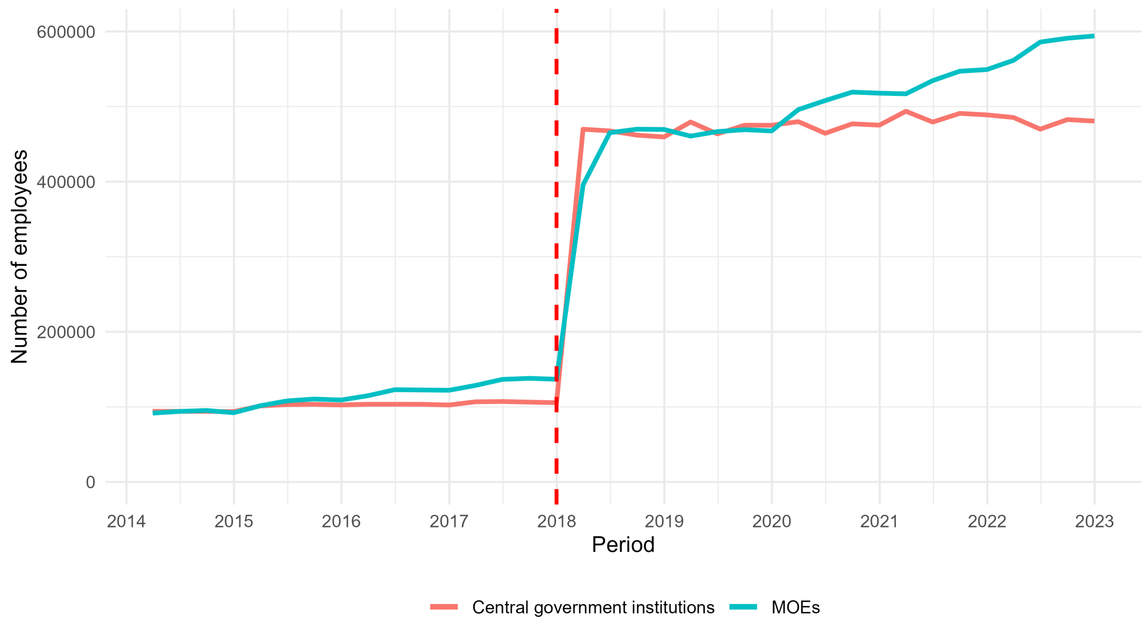
1 Introduction

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 on-site 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 wage 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, size, 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] Overall, 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). We tailor our identification strategy to each case and use event study estimates as our baseline.

The results show clear gains for workers who moved into permanent public employment. These workers experienced higher wages and lower turnover, with gains that are more pronounced among low-skilled workers and, in particular, among men. Overall, the reform improved earnings and job stability for a predominantly low-skilled workforce.

At the firm level, the reform led to a reallocation of rents. Private firms that were more exposed to the reform were more likely to exit, and those that survived became smaller. Their productivity and profitability also declined, suggesting that public outsourcing contracts were valuable for service providers and not easily replaced. In contrast, public entities that internalized these services improved their performance measured by productivity and profitability after the reform.

[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).

We also find modest spillovers in local labor markets, with small wage increases among low-skilled private-sector workers in more exposed areas.

To sum up, workers who moved into public employment benefited through higher pay and greater job stability, while public entities improved their performance after bringing services in-house. These gains came at a cost for outsourced firms, which were more likely to exit and performed worse if they survived. Accordingly, the reform redistributed gains toward public entities and a broad group of relatively low-income workers, while imposing losses on parts of the private sector.

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 data 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 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), Gürer and Taymaz (2026) and Godechot and Lojkine (2026) provide further applications of this approach. These studies generally find that on-site outsourcing of low-skilled occupations, such as cleaning, security, and food services, leads to wage losses for affected workers, while in Gürer and Taymaz (2026) we additionally document wage gains for high-skilled outsourced workers.

While the Goldschmidt and Schmieder (2017) approach has proven influential, it relies on strong assumptions regarding worker flows to identify outsourcing events, often leading to substantial sample restrictions. Moreover, outsourcing does not necessarily occur on-site: firms may directly contract with service providers, or outsourcing relationships may predate the observation period. In such cases, the absence of observable worker flows implies that outsourced workers are not identified.

A broader set of studies examines the labor market effects of outsourcing using alternative identification strategies, including fixed effects models, AKM decompositions, and occupation- or industry-based classifications of outsourced work. Using U.S. survey data, Dube and Kaplan (2010) and Katz and Krueger (2019) document wage penalties for workers in outsourced or alternative work arrangements. Using French survey data, Fana et al. (2024) document an outsourcing penalty for the workers at the lower end of the wage distribution. Similarly, Bilal and Lhuillier (2021) and Drenik et al. (2023), employing AKM frameworks with French and Argentine data, find that outsourced workers receive lower wage premia, particularly at high-wage firms.

Other contributions rely on self-reported outsourcing status or proxy measures based on worker transitions and occupational classifications. Spitze (2022) and Guo et al. (2025) find modest wage penalties for outsourced workers, with heterogeneous effects across skill groups. Bergeaud et al. (2025) link outsourcing to technological change, showing that improved internet access increases outsourcing and leads to wage losses for low-skilled workers but gains for high-skilled workers.

Overall, the literature has primarily focused on worker-level outcomes, such as wages and employment. We contribute to this literature by exploiting a large-scale policy reform and by examining not only worker-level effects but also firm-level responses and spillover effects across occupations.

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 of the reform on wages in local labor markets. Section 7 concludes along with a discussion on the welfare implications of the reform.

2 Background

For many years, the use of outsourced workers ("*taşeron*" in Turkish) in the public sector in Türkiye[3] 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 on-site services to the public sector as of December 4, 2017 would be granted permanent public employment.[4] 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

[3]The first documented examples of outsourcing in the Turkish public sector date back to 1985 in the Beyoğlu District Municipality, followed by similar practices in the Afyon and Adana municipalities in 1987 and 1989, respectively (Sayan, 2018).

[4]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

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 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 wage employment (public servants excluded) was roughly 385 thousand by December 2017, of which approximately 100 thousand were employed in municipal economic enterprises. For the same period, the overall labor force was approximately 31.5 million individuals, according to 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.[5]

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.

[5]According to information obtained by Confederation of Progressive Trade Unions of Türkiye (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

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.[6] Owing to its comprehensive coverage and administrative nature, EIS currently constitutes the most reliable data source for analyzing labor market dynamics in Türkiye and has been widely used in studies on labor and firm dynamics. See, e.g., Akcigit et al. (2020) and Demir et al. (2024).

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 all formally registered workers in Türkiye and are available for March, June, September, and December of each year.[7] 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.[8] 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

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

[7] 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.

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

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 268,907 workers transitioning into permanent positions in MOEs. This number is naturally smaller than the spike observed for MOEs in Figure 1, as our identification imposes the restrictions in conditions (i) and (ii).

The EIS data do not contain information on central government institutions and their employees, and, thus, workers granted permanent positions in such institutions cannot be identified within the EIS. We therefore utilize an alternative dataset[9] to recover this number in Figure 1 and to construct occupation–province-level measures of these transitions, which we use to build the occupation–province-level exposure variables in our spillover analyses.

3.2 Descriptive Statistics

Our treatment group consists of 268,759 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, 2017q4. First two rows show that the treated group is substantially more male-dominated than the broader workforce. While males constitute 73% of the non-treated sample, they represent 87% of the treated group.

[9]A dataset similar in structure to the EIS is available at TurkStat. Both datasets include only workers registered under the 4(a) scheme (workers working for an employer in exchange for wages). They do not cover individuals under 4(b) (self-employed) or 4(c) (civil servants) status. However, unlike the EIS, worker registers of TurkStat data include employees of central government affiliated institutions, allowing us to identify workers who transitioned into permanent positions in such institutions. Applying the same identification criteria used for MOE transitions, we identify 359,318 such workers. Combined with the 268,907 workers who transitioned into MOEs, this yields a total of 620,198 workers identified as reform beneficiaries. This falls below the approximately 750,000 workers commonly cited as having benefited from the reform. The difference reflects the deliberately conservative nature of our identification strategy, which imposes additional requirements beyond the reform's own eligibility criteria.

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.[10] 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, the other rows show that the treated workers are somewhat older and more concentrated in middle age groups compared to the non-treated sample. That is, the treated group shows a notably higher concentration in the 30-44 age range than the nontreated group.

Table 1: # of treated & non-treated workers

	Treated (% share)	Non-treated (% share)
Gender:		
Male	230,968 (86.5)	9,618,949 (72.5)
Female	35,848 (13.4)	3,640,624 (27.4)
Skill:		
Low	240,148 (89.3)	10,493,556 (78.7)
High	28,759 (10.6)	2,830,841 (21.2)
Age group:		
20-24	10,464 (3.9)	1,584,117 (12.4)
25-29	37,621 (14.1)	2,150,531 (16.8)
30-34	51,486 (19.3)	2,174,322 (17.0)
35-39	55,393 (20.8)	2,205,051 (17.3)
40-44	48,194 (18.1)	1,834,819 (14.4)
45-49	38,345 (14.4)	1,434,444 (11.2)
50-54	16,857 (6.3)	812,440 (6.3)
55-59	6,141 (2.3)	386,883 (3.0)
60-64	1,346 (0.5)	150,862 (1.1)

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 outsourced firms during the analysis period who did not

[10] ISCO stands for International Standard Classification of Occupations, a framework developed by the International Labour Organization (ILO) for classifying occupations. We use the 2008 revision (ISCO-08).

transition (*Non-switchers in outsourced firms*), and (iii) all remaining workers observed in the dataset (*Non-switchers in other firms*). Note that 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 2,569 firms meeting this criterion.

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 in outsourced 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.

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

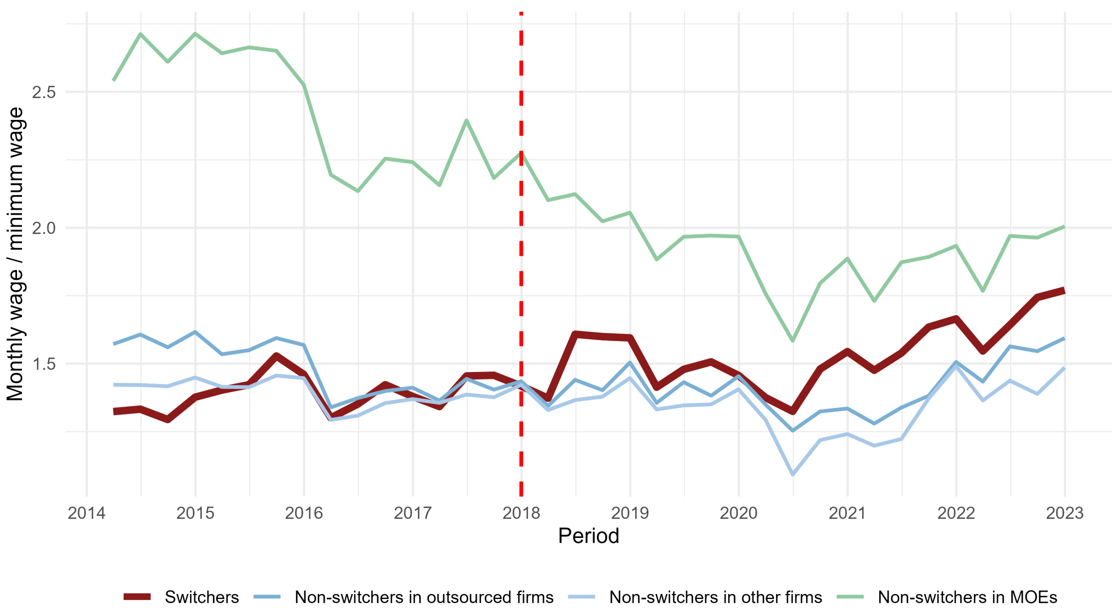
Figure 3 replicates the wage trend analysis separately for low- and high-skilled occupations across all four groups. Panel A shows that the wage trajectories of *Switchers* in low-skilled occupations closely mirror those observed for the full sample of *Switchers*. This pattern is unsurprising, given 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*.

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.[11]

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.

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.

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



[11]The relative wages of high-skilled workers declined sharply at the start of 2016, driven by a roughly 30% increase in the minimum wage in January. High-skilled workers' wages did not appear to rise at a comparable rate.

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

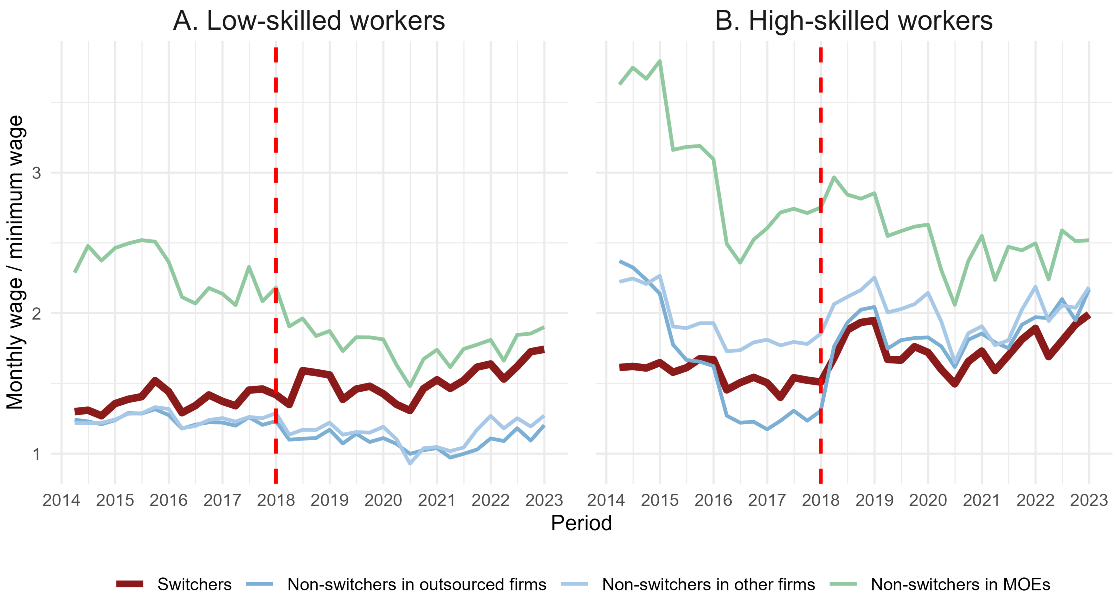
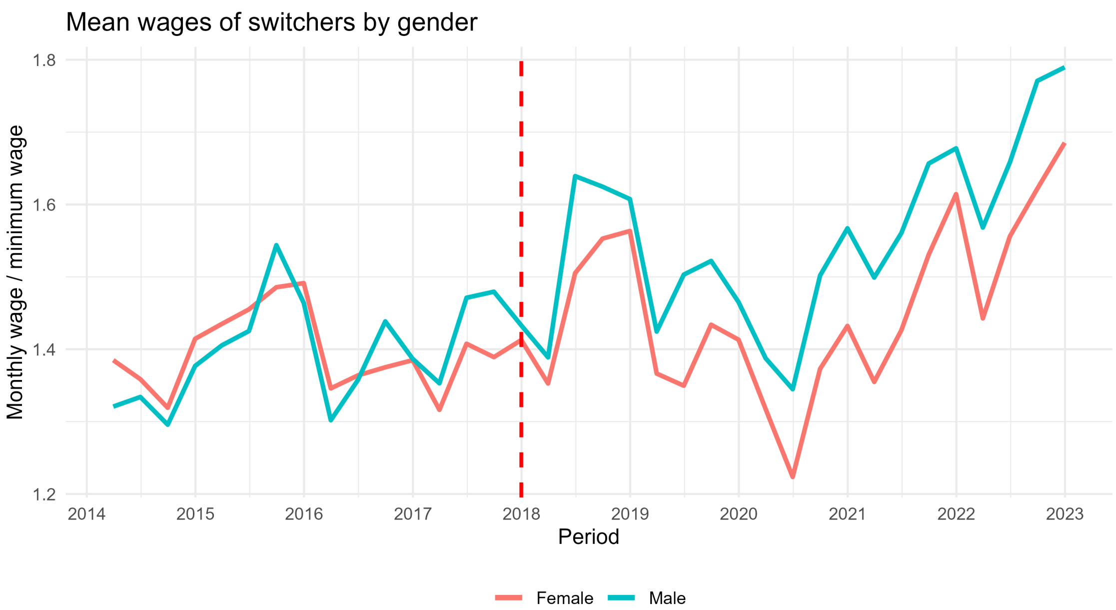
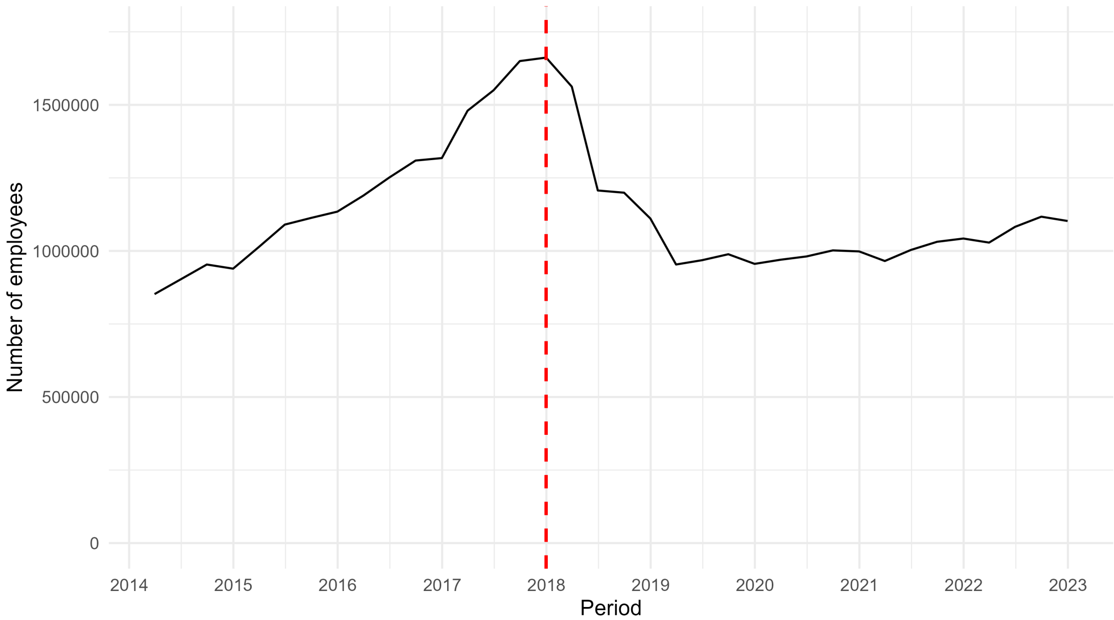


Figure 4: Mean wages of switchers by gender



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 outsourced 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.

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



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 outsourced workers into permanent public sector employees. Our primary estimating equation is:

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

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 outsourced 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 the announcement date of the reform, 2017q4. The indicator $1[Event\ time_{it} = \tau]$ equals one when worker i is observed τ quarters relative to the announcement date. We normalize the coefficient for $\tau = 0$ (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. 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}) control for regional labor market shocks.

The coefficients of interest are $\{\beta_\tau\}_{\tau \neq -1}$, which measure the log 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 $\tau \geq 0$) capture the dynamic treatment effects. 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, that is, both the treatment and control groups are restricted to individuals of the same gender. To further ensure comparability with the treatment group, individuals in the control group are required to be employed both immediately before (2017q4) and after (2018q1) the announcement of the reform. 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 insignificant 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, indicating that the move from outsourced 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, 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 wage gains, the magnitude of these effects is smaller for women. This gender gap persists throughout most of the post-reform period. 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 across specifications.

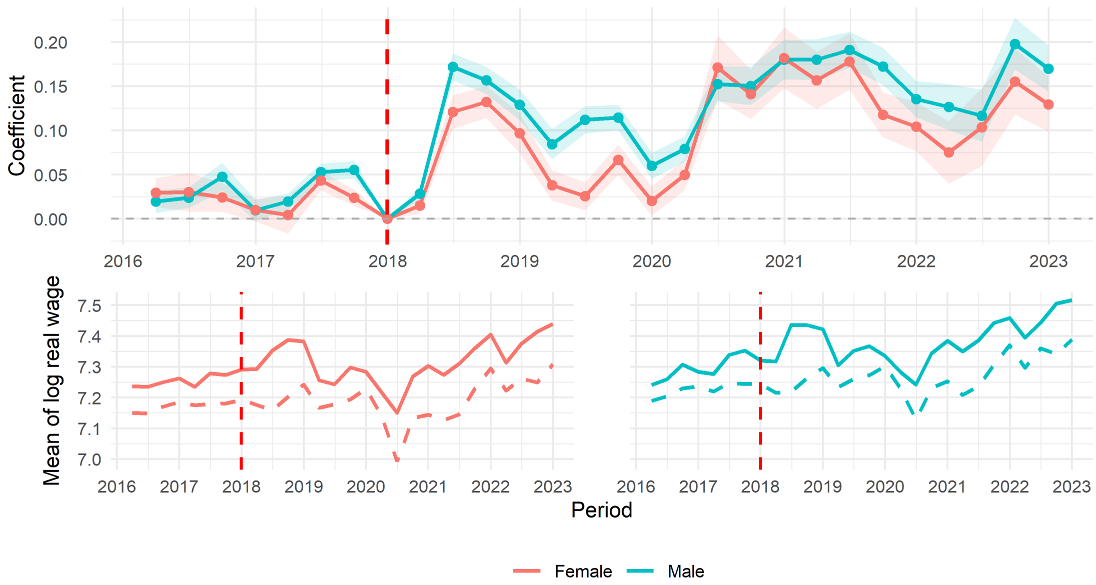
We examine the wage effects of the outsourcing ban across additional subsamples in Appendix Table A2. First, we implement a hybrid matching algorithm[12] to restrict the comparison group to workers with similar observable characteristics, confirming the robustness of our findings. Regression results by skill group indicate that wage gains are more pronounced among workers in low-skilled occupations than among those in high-skilled occupations. We further find that the documented gains persist in the MOEs of the municipalities governed by both the ruling party (Justice and Development Party, *AKP*) and the main opposition party (Republican People's Party, *CHP*). While Appendix Table A2 reports average post-treatment effects, the corresponding event-study estimates are presented in figures D1 to D4.

4.2 Turnover Effects

As established in the previous section, workers who transition into municipal-owned enterprises experience wage gains. Such improvements in compensation may, in turn, shape workers' voluntary separation decisions. Permanent public sector positions are generally associated with greater employment protection, which is expected to reduce the likelihood of involuntary separations as well. In this section, we investigate whether transitioning into a permanent public position translates into lower job turnover.

[12]We follow a two-stage matching methodology as in Gürer and Taymaz (2026). Further details can be found in the table notes.

Figure 6: Wage effect of the outsourcing ban



Notes: The top panel plots the wage effects of the outsourcing ban on workers who transitioned 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. The regressions include 200,064,871 observations for male workers and 64,470,794 observations for female workers. The quarter immediately preceding treatment (2017q4), indicated by the vertical red dashed line, is used as the base period. The figures report point estimates with 95% confidence intervals. The corresponding difference-in-differences estimates are reported in Table A1. The bottom panels show mean real wages of treated (solid line) and untreated (dashed line) workers over time. The left panel corresponds to female workers, while the right panel corresponds to male workers.

Our outcome variable, $Turnover_{it}$, is a binary indicator defined as follows. $Turnover_{it} = 0$ if the worker remains employed at the same firm at time $t+1$, or, not observed in the dataset at $t+1$ but employed in the same firm at $t+2$ (temporary separation). Conversely, $Turnover_{it} = 1$ if the worker separates from the firm at $t+1$ to join a different firm, or fails to return to the original firm following a quarter of nonemployment.

$$\begin{aligned} Turnover_{it} = \exp\Bigg(& \sum_{\tau=-8,\tau\neq-1}^{20} \beta_\tau \times switcher_i \times 1[Event\ time_{it} = \tau] + \\ & \alpha\ switcher_i + \Gamma_{worker\ tenure} + \Gamma_{time} + \Gamma_{province} \Bigg) \times \varepsilon_{it}. \end{aligned} \tag{2}$$

The coefficients β_τ capture the dynamic 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 at the current firm by including tenure fixed effects, denoted by $\Gamma_{worker\ tenure}$. 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}) and province fixed effects ($\Gamma_{province}$). 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 where worker-tenure is a proxy for survival duration.[13]

As in the wage regressions, turnover regressions are estimated on subsamples defined by gender. Figure 7 presents the event study estimates and mean turnover rates of treated and untreated workers. It should be noted that the raw means exhibit pronounced seasonality in turnover rates prior to 2018, among outsourced workers. This is due to the prevalence of one-year contracts in outsourcing arrangements.

In the event study estimates of Figure 7, the last period before the announcement of the reform (2017q4) (indicated by the vertical red dashed line in event) is considered as the base period. Figure 7 suggests that the first period after the announcement, 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 turnover probability declines sharply for both genders, implying a reduction of roughly 95% ($= |exp(-3) - 1|$) by 2018q3. Thereafter, the effect stabilizes; the probability of turnover remains about 86% ($= |exp(-2) - 1|$) lower for female-treated workers and 63% ($= |exp(-1) - 1|$) lower for male-treated workers compared to 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.[14]

Appendix Table A3 reports average treatment effects on turnover probability. As a robustness check, we first present estimates using the matched samples as in the wage analyses. Our baseline results remain qualitatively unchanged. The table further disaggregates the effects by gender, skill group, and the political affiliation of the municipality whose MOE employs the worker after the reform. The reduction in turnover probability is larger for low-skilled workers regardless of gender, likely reflecting better outside options available to high-skilled workers.

We also find that the decline in turnover is stronger in municipal enterprises governed

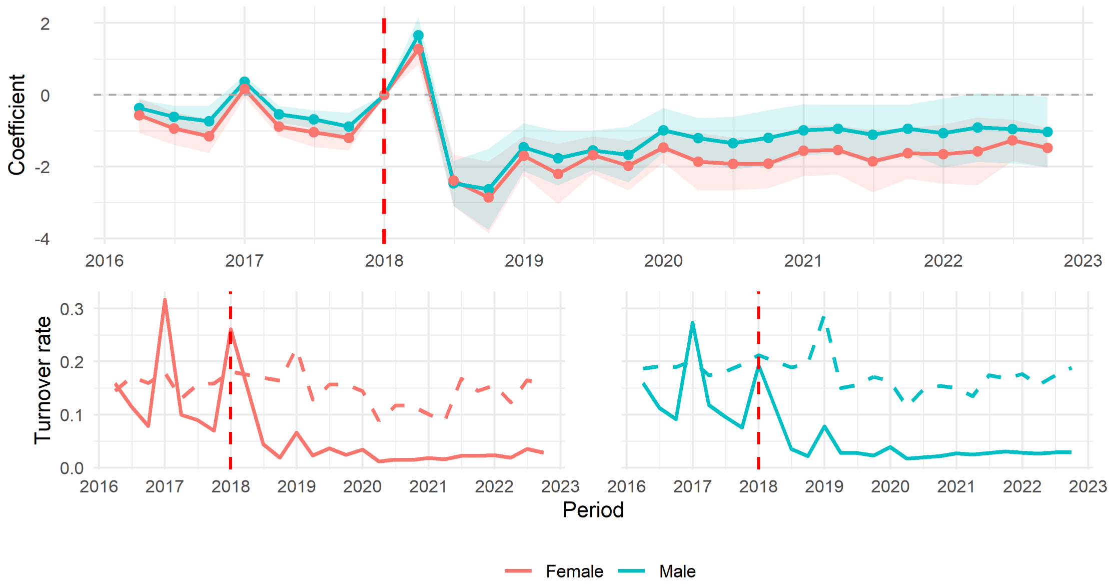
[13] We top-code worker tenure at 8 years.

[14] 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 the predicted turnover probability for a treated individual is $\exp(-3) \approx 0.05$ times the untreated baseline, reducing it by approximately 95%, from 50% to 2.5%

by the main opposition (Republican People's Party, CHP) than in those governed by the ruling party (Justice and Development Party, AKP). This pattern is not straightforward to interpret and warrants further investigation. One possible explanation is that opposition-run municipalities are generally located in more developed areas and may offer better working conditions, job stability, or workplace environment, which could reduce workers' incentives to leave their positions.

Corresponding event-study estimates for these subsample analyses are reported in Figures D5–D8.

Figure 7: Worker turnover effect of the outsourcing ban



Notes: The top panel plots 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 319,484,064 observations, while regressions for female workers include 125,909,807 observations. The quarter immediately preceding treatment (2017q4), indicated by the vertical red dashed line, is used as the base period. The figures report point estimates with 95% confidence intervals. The corresponding average difference-in-differences estimates are reported in Table A3. The bottom panels show mean turnover rates of treated (solid line) and untreated (dashed line) workers over time. The left panel corresponds to female workers, while the right panel corresponds to male workers.

5 Firm-level Results

The outsourcing ban also has important implications for firms. Two groups are particularly affected: outsourced firms and municipal-owned enterprises (MOEs). While outsourced firms lose a substantial share of their workforce after the reform (see Figure 5), 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 outsourced firms that provided services to MOEs prior to the reform and analyzes the effects on firm survival and size. Section 5.2 then examines the impact on the productivity and profitability of outsourced firms. Finally, Section 5.3 investigates the reform's effects on the productivity and profitability of MOEs.

5.1 Survival and Firm Size Results

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:

$$y_{ft} = \exp\left(\sum_{\tau=1}^{19} \beta_\tau \times exposure_f \times 1[Event\ time_{it} = \tau] + \theta\ exposure_f + \right.$$
$$\left. \alpha\ log(initial\ employment_f) + \Gamma_{firm\ age} + \Gamma_{time} + \Gamma_{province} \right) \times \varepsilon_{ft} \qquad (3)$$

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, i.e., the final period of observation. The coefficient β_τ shows the effect of the outsourcing ban on the survival of outsourced firms. We use a continuous treatment measure, $exposure_f$, which indicates the degree of exposure of a firm. This exposure variable is defined as the ratio of switcher workers to total employment of the firm:

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

We control for firm size by including the initial value of employment of the firm. Firm-age fixed effects, $\Gamma_{firm\ age}$, a key component of the hazard function, are included. 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. Finally, we restrict our sample to firms that share the same NACE codes as the outsourced firms.

The upper panel of Figure 8 shows that the exit probability increases sharply in the first period after treatment for firms with greater exposure to the reform. Quantitatively, for a firm where 10% of the workforce consists of switchers (treated workers), the exit probability in the first post-treatment period is about 35% ($= \exp(3 \times 0.1) - 1$) higher than that of a firm with zero exposure.

In the long run, firms with higher exposure continue to exhibit higher exit probabilities. For instance, a firm with 10% exposure has an exit probability that is approximately 10.5% $(= \exp(1 \times 0.1) - 1)$ higher than a firm with zero exposure.

The bottom panel of Figure 8 plots raw exit rates for firms with any positive exposure to the reform against those with zero exposure. Exit rates are substantially higher for exposed firms immediately after the reform. In the long run, however, their exit rates fall slightly below those of non-exposed firms. Note that these patterns are not directly comparable to the event-study estimates above, since the latter exploit continuous variation in exposure, whereas the bottom panel relies on a binary treatment definition.

Overall, the results suggest that firms with any exposure are more likely to exit just after the reform. Exit rates decline thereafter, but still increase with the degree of exposure, as shown via the event-study estimates.

Appendix Table A4 reports average treatment effects under alternative specifications.. First, as a robustness check, we construct an alternative exposure measure based on wages, defined as the share of total wages paid to switcher workers in the firm's pre-reform wage bill. The results are very similar to those obtained using the employment-based exposure measure.

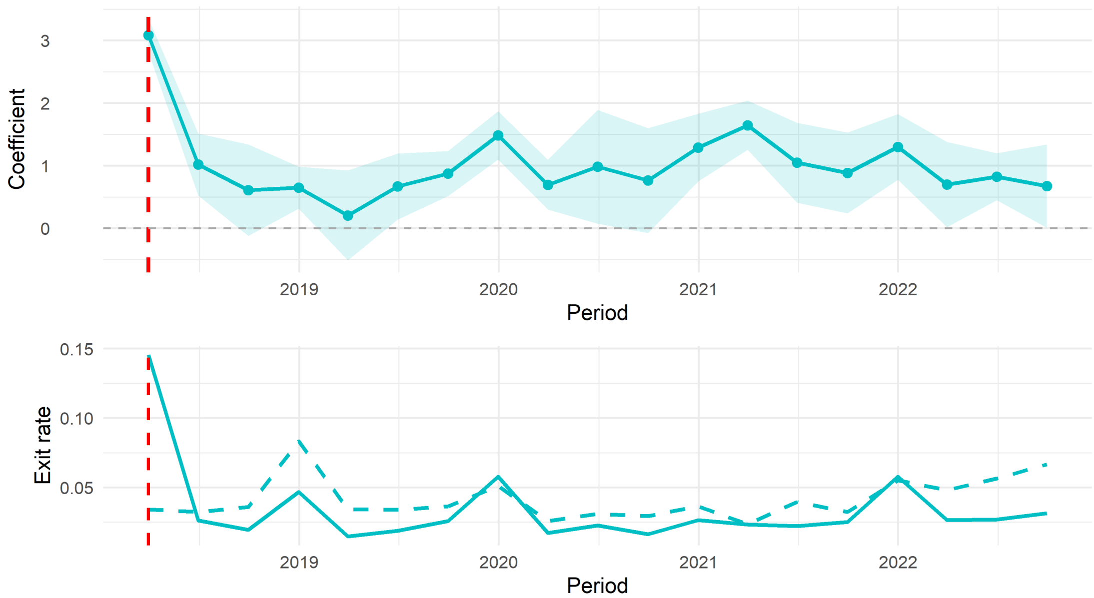
We then examine heterogeneity by the skill intensity of services provided by firms. Firms are grouped into (i) low-skilled service providers (e.g., food, cleaning, security, logistics) and (ii) high-skilled professional service providers (e.g., IT, consultancy, engineering, architecture), as detailed in Appendix B1. Firms that cannot be clearly classified are excluded from this analysis. The results differ considerably across groups. For low-skilled industries, the estimated effect is positive and statistically significant. For high-skilled industries, the coefficient is smaller and statistically insignificant, suggesting a more limited impact.

Finally, we re-estimate the average treatment effects by the political affiliation of the municipalities connected to MOEs (which the outsourced firms previously served). The effect of exposure on firm exit is consistently positive and statistically significant across all groups. However, the magnitude is smaller in municipalities with close electoral races, especially where the ruling party narrowly won. This pattern suggests that municipalities and their MOEs may have made greater efforts to keep outsourcing firms afloat after the reform in more politically competitive areas.

Even if outsourced firms survive, their size may decline due to the demand shock induced by the reform. We therefore examine changes in firm size conditional on survival using a log-employment specification presented in equation (5). The regression sample consists of firms that are observed over the full sample period, resulting in a balanced panel. The dependent variable is the logarithm of firm employment, and the coefficients of interest capture how firm size evolves over time as a function of exposure to the reform. The specification includes firm fixed effects, α_f, and province-by-time fixed effects, μ_{pt}, to control for time-invariant firm characteristics and local shocks.

$$\log(employment_{ft}) = \sum_{\tau=-8,\tau\neq-1}^{20} \beta_\tau \times exposure_f \times 1[Event\ time_{it} = \tau] + \alpha_f + \mu_{pt} + \varepsilon_{ft} \quad (5)$$

Figure 8: Firm survival effect of the outsourcing ban, all firms



Notes: The top panel plots the firm survival effects of the outsourcing ban estimated using Equation (3) 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 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. 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 36,500,949 observations. The quarter immediately preceding the reform (2017q4), indicated by the vertical red dashed line, is used as the base period. The figures report point estimates with 95% confidence intervals. The corresponding average difference-in-differences estimates are reported in Table A4. The bottom panel shows mean exit rates of treated (solid line) and untreated (dashed line) firms over time. Treated firms are those with strictly positive exposure.

The event-study estimates are presented in Figure 9. The results indicate that firms more exposed to the reform experience a sharp decline in size. This reduction persists in the long run, i.e., five years after the reform. Quantitatively, firms with 10% of their employment exposed to the reform experience approximately 32% reduction in their workforce in the long run ($= |\exp(-3.8 \times 0.1) - 1|$), relative to firms with no exposure. The bottom panel of Figure 9 shows that firms with positive employment exposure experience, on average, a

78% decline in total employment $(= (\exp(6) - \exp(4.5))/\exp(6))$. The disproportionate decline in firm size relative to employment exposure suggests that, while the reform directly targeted outsourced workers, managerial, supervisory, and administrative staff employed by these firms may have been laid off following the loss of outsourcing contracts with MOEs.

As in the survival analyses, we define firm-level exposure based on the total wages paid to workers affected by the reform. The resulting treatment effects for employment and wage-based exposure are reported in Appendix Table A5 and are highly similar.

Appendix Table A5 further indicates that the negative effects of exposure are more pronounced in industries providing low-skilled services than in those providing high-skilled services (see Appendix B1 for the classification). Finally, we examine how the effect of exposure on firm size varies by the political affiliation of the MOE with which the outsourced firm contracts. The results, reported also in Appendix Table A5, show that the negative and statistically significant effect of exposure on firm size persists in both ruling-party and opposition municipalities.

Overall, firms exposed to the reform are less likely to survive, and those that do survive become smaller. These effects persist in the long run.

5.2 Productivity and Profitability of Outsourced Firms

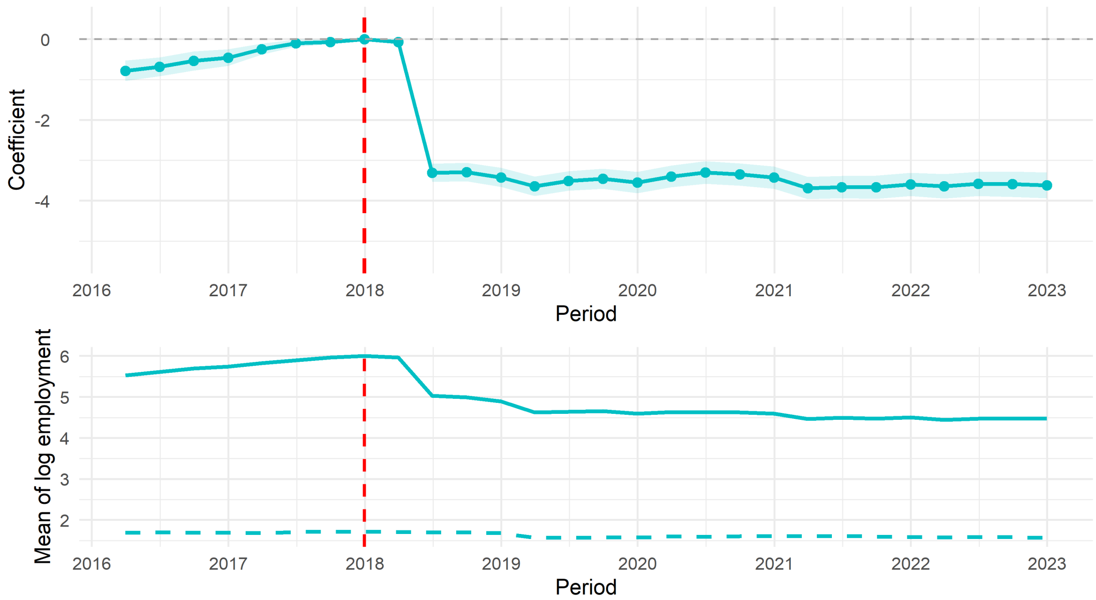
We continue investigating the performance of the surviving outsourced firms. Specifically, we ask how their productivity and profitability evolved after the reform. To address this question, we estimate the following specification:

$$y_{ft} = \sum_{\rho=2012, \rho\neq 2017}^{2022} \beta_t \times \ exposure_f \times (T_f \times 1_{t=\rho}) + \alpha_f + \mu_{pt} + \varepsilon_{ft} \tag{6}$$

where y_{ft} denotes firm-level outcomes for firm f in year t. We focus on value added (introduced in Section 3.1) per workday (productivity) and operating profit divided by output (profitability) as our dependent variables. Since value added variable includes many negative and zero values, we apply the inverse hyperbolic sine transformation. 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 equation (4). The interaction term $(T_f \times 1_{t=\rho})$ represents a set of period dummies. Accordingly, our coefficients of interest are β_t. The specification includes firm, α_f, and province-by-year fixed effects, μ_{pt}. We estimate this specification using a fully balanced sample of firms as in the analyses of firm size above in order to focus on the outcomes of surviving firms only. Firms that are not outsourced firms and have no direct relationship with MOEs naturally have $exposure_f = 0$.

Figure 9: Firm size effect of the outsourcing ban, all firms



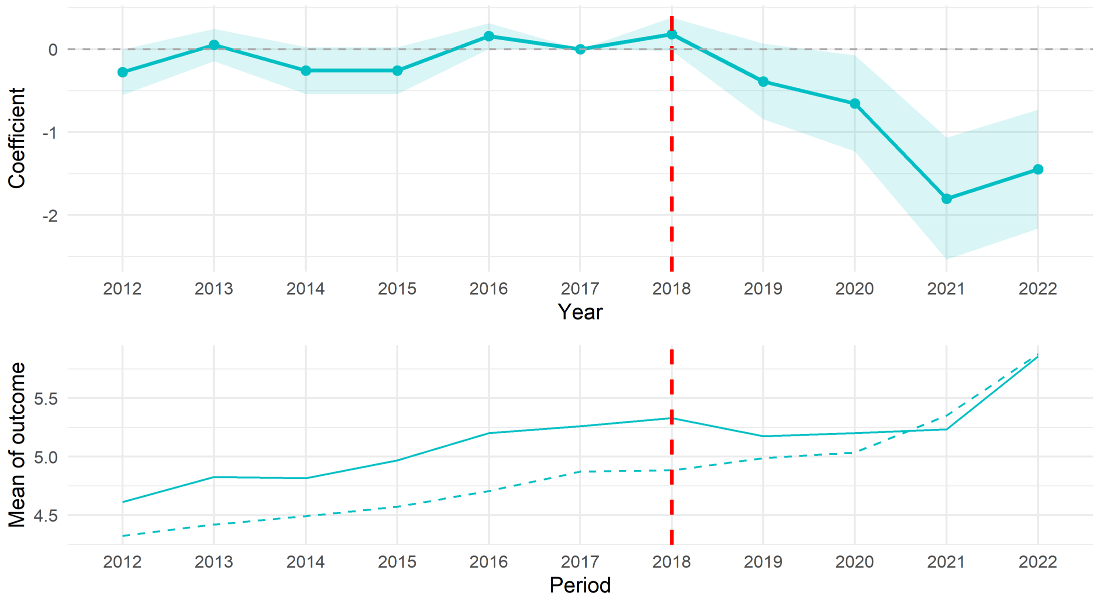
Notes: The top panel plots the firm-size effects of the outsourcing ban estimated using Equation (5) with a continuous treatment measure. Exposure is defined as the ratio of switcher workers to total firm employment measured immediately prior to the outsourcing ban. Outcome is the log of firm employment. 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 9,671,868 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 A5. The bottom panel shows mean exit rates of treated (solid line) and untreated (dashed line) firms over time. Treated firms are those with strictly positive exposure.

Results are presented in Figure 10 and Figure 11, respectively. Corresponding average treatment effects are reported in Appendix Table A6.

The figures display broadly similar patterns. The post-treatment coefficients indicate that both productivity and profitability begin to decline the year after the reform for firms with higher exposure to the treatment. Both effects intensify over time and reach their peak in 2021 (about three years after the reform) with coefficients of roughly -1.75 for productivity and -0.125 for profitability.

Interpreting the productivity coefficients is not straightforward due to the use of the inverse hyperbolic sine transformation. For sufficiently large values, this transformation approximates the natural logarithm, allowing for a log-like interpretation. Under this approximation, the estimates imply that firms with 10% exposure to the reform experience a 16% ($|\exp(-1.75 * 0.1) - 1|$) decline in value added per workday[15] and about a 1.25% decline in profitability relative to firms with no exposure.

Figure 10: Productivity effect of the outsourcing ban on outsourced firms

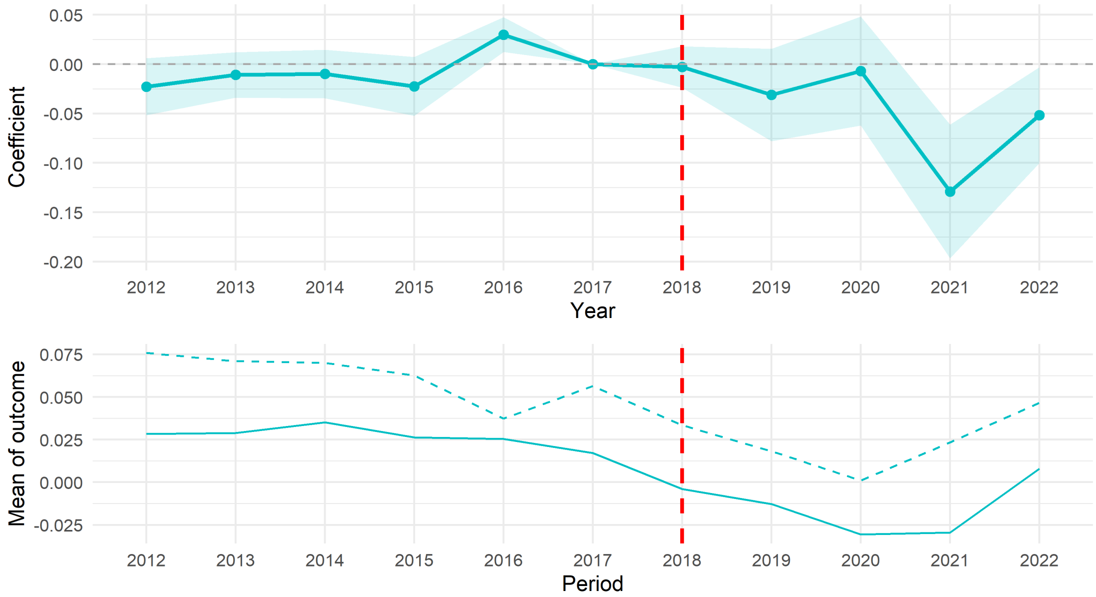


Notes: The top panel plots the productivity effects of the outsourcing ban estimated using Equation (6) with a continuous treatment measure. Exposure is defined as the ratio of switcher workers to total firm employment measured immediately prior to the outsourcing ban. The sample includes all firms that 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 3,216,032 observations. The reform year (2018) is indicated by the red vertical dashed line. 2017 is the base period. The corresponding average difference-in-differences estimates are reported in Table A6. The bottom panel shows mean productivity value of treated (solid line) and untreated (dashed line) firms over time. Treated firms are those with strictly positive exposure.

Taken together, these results indicate that the outsourcing ban generated negative effects on both productivity and profitability among firms whose labor force is more exposed to the reform. This suggests that contracts with MOEs constituted a profitable business opportunity for these firms. Losing access to MOE-related activities appears to have weakened their performance. These contracts could not be replaced by alternative sources of demand even in the long-run, i.e., 4-5 years after the reform.

[15]We also examine the effect of exposure on value added per output and find that it declines after the reform as exposure increases. The results are presented in Figure C1.

Figure 11: Profitability effect of the outsourcing ban on outsourced firms



Notes: The top panel plots 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 3,604,187 observations. The reform year (2018) is indicated by the red vertical dashed line. 2017 is the base period. The corresponding average difference-in-differences estimates are reported in Table A6. The bottom panel shows mean profit rate value of treated (solid line) and untreated (dashed line) firms over time. Treated firms are those with strictly positive exposure.

5.3 Productivity and profitability of MOEs

This section turns to the other side of the market and examines the effects of the reform on the productivity and profitability of municipal-owned enterprises (MOEs), which absorbed these workers into permanent positions.

As a prior, if MOEs previously purchased outsourced services under contracts with high markups, terminating these contracts and producing the same services in-house should, all else equal, improve their performance. The analysis below evaluates this hypothesis empirically.

In particular, we examine whether MOEs improved their performance in terms of value added per workday (productivity) and profit per unit of output (profitability) following the reform. We use the inverse hyperbolic sine transformation of value added per employee, as this variable contains many zero and negative values.

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.

$$y_{ft} = \sum_{\rho=2014,\rho\neq 2017}^{2022} \beta_t \times \ MOE_f \times (T_f \times 1_{t=\rho}) + \alpha_f + \mu_{pt} + \varepsilon_{ft} \tag{7}$$

Results are presented figures (12) and (13). Appendix Table A7 presents the corresponding average treatment effects.

Post-treatment coefficients for productivity lie around 0.5. Since the arcsinh transformation approximates the natural logarithm for high values, this estimate implies that value added per employee at MOEs increased by around $exp(0.5) - 1 \approx 64\%$ relative to non-MOEs after the reform. The corresponding coefficients for profit per output are around 0.07, that is, profit rates of MOEs increased by, on average, 7 percentage points after the reform. These magnitudes suggest considerable performance gains for MOEs following the internalization of previously outsourced services.

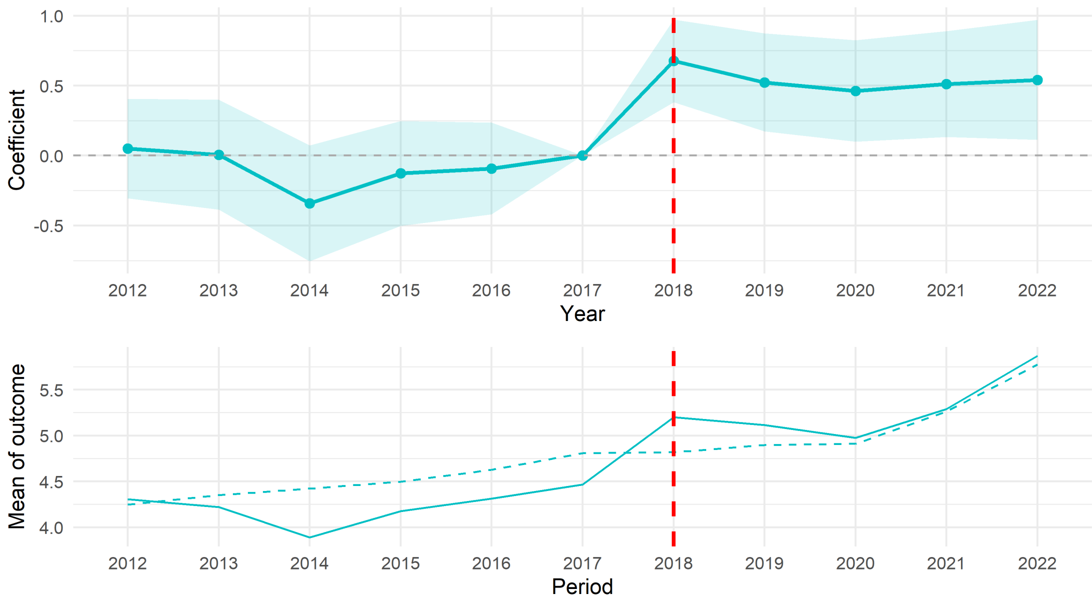
Overall, the results indicate that MOEs benefited from the reform in terms of both productivity and profitability. The post-reform improvements support the hypothesis that, prior to the reform, outsourced services were procured under contracts that were costly for MOEs and profitable for outside service providers. Bringing these services in-house by granting permanent positions to previously outsourced workers appears to have reduced input costs and improved productivity. We further examine the reform's implications for value added per output. The corresponding event study estimates, presented in Figure C2, suggest that value added per output increases for MOEs after the reform.

6 Wage Spillover Results

Thus far, we have focused on the direct effects of the outsourcing ban on workers and firms. However, such a large-scale reallocation of labor is also likely to affect local labor markets (LLMs) more broadly. Public institutions absorbed a substantial number of workers who would otherwise have remained in the private sector. Withdrawal of such a large number of workers from LLMs may 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 ambiguous.

Figure 12: Productivity effect of the outsourcing ban on MOEs



Notes: The top panel plots 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 4,245,042 observations. The reform year (2018) is indicated by the red vertical dashed line. 2017 is the base period. The corresponding average difference-in-differences estimates are reported in Table A7. The bottom panel shows mean productivity value of treated (solid line) and untreated (dashed line) firms over time.

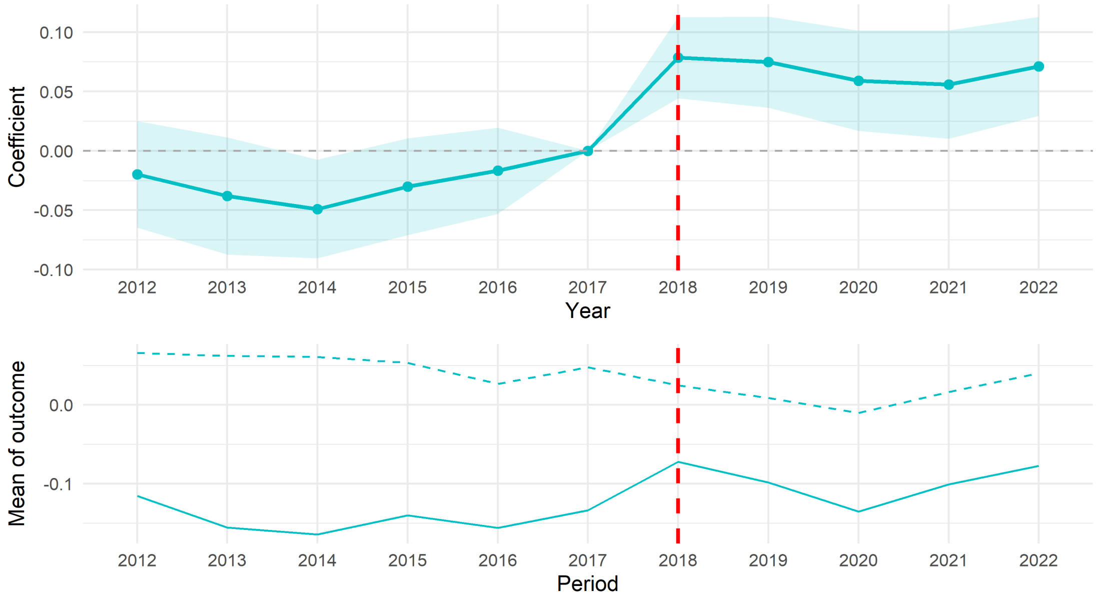
Our outcome variable is wages. We measure wages at the province–occupation level, using province (p) and occupation (o) cells as the unit of analysis. For each province–occupation–period cell, we compute the logarithm of mean monthly wages. To isolate spillover effects, we exclude treated workers, i.e., switchers from outsourced firms to MOEs or central government institutions, as well as non-switcher MOE employees from these averages.

We then estimate the following event-study specification:

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

The outcome y_{opt} represents the log of average real wage in occupation o and province p for the period t. Unsurprisingly, not all regions and occupations have been equally affected by the reform. LLMs in provinces with high number of public institutions and MOEs are more exposed to the reform. Accordingly, our exposure variable, $exposure_{op}$, is defined as:

Figure 13: Profitability effect of the outsourcing ban on MOEs



Notes: The top panel plots 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 5,164,637 observations. The reform year (2018) is indicated by the red vertical dashed line. 2017 is the base period. The corresponding average difference-in-differences estimates are reported in Table A7. The bottom panel shows mean profit rate value of treated (solid line) and untreated (dashed line) firms over time.

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

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. 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. Province–time fixed effects (D_{pt}) control for local shocks, while occupation–time fixed effects (D_{ot}) account for nationwide occupation-specific trends in wages.

In Figure 14, we present event-study estimates, separately for male and female workers. Coefficients in the pre-treatment period are close to zero. 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. The estimates for female workers indicate positive, albeit smaller and delayed (four-to-five years after the reform) wage spillovers for female workers relative to their male counterparts. At its peak around mid-2022, a 10% higher intensity of switchers in a given province–occupation cell is associated with a 3% wage increase for non-switcher male workers and a 2% wage increase for non-switcher female workers.

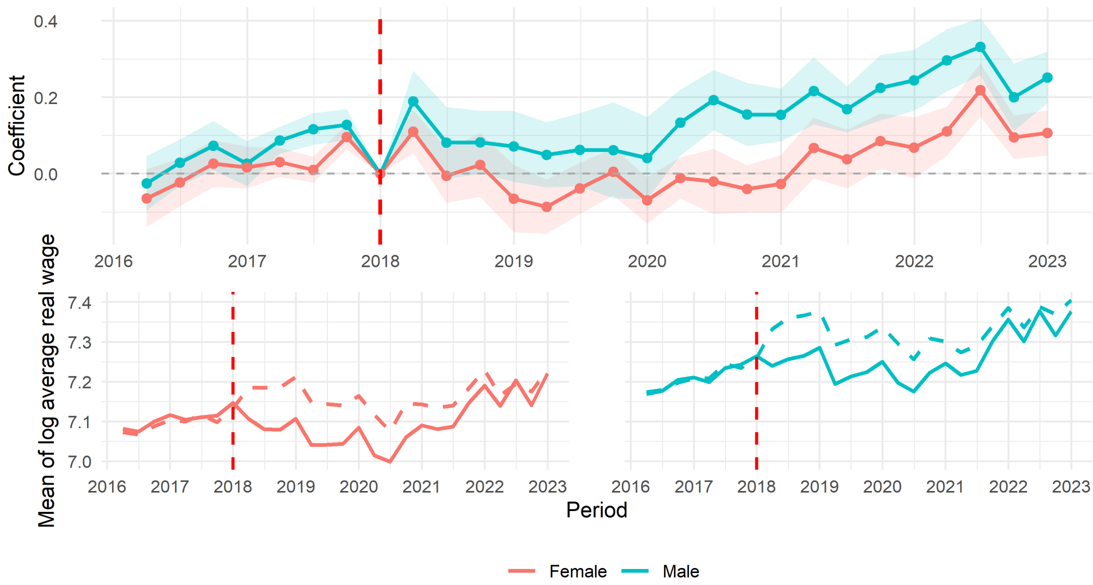
We estimate the average spillover effect across further subsamples and present the results in Appendix Table A8. Event-study estimates corresponding to the subsample analyses in this section are reported in Figure D9 to D12. Columns (2) and (6) of Appendix Table A8 show the average spillover effects after excluding the three largest metropolitan areas (İstanbul, Ankara, and İzmir) for male and female workers, respectively. The estimates are virtually unchanged, indicating that the spillover effect is not driven by labor market conditions specific to the largest cities.

We next examine heterogeneity by skill intensity. Columns (3) and (7) display estimates for low-skilled workers, while columns (4) and (8) turn to high-skilled workers. For male workers, the low-skilled estimate (column 3) is nearly identical to the baseline in column (1), indicating that the aggregate effect is driven by the low-skilled segment of the workforce. The high-skilled estimate (column 4) is of comparable magnitude but only weakly significant. For female workers, neither the low-skilled nor the high-skilled subsample produces a statistically significant effect, as the impact on female workers materializes with a considerable delay, as shown in Figure 14.

Following Felix and Wong (2025), we further examine heterogeneity across age groups to assess whether the policy shock altered wage differentials over the age distribution. Table A9 reports estimates for low-skilled workers by age group. Columns (1)–(4) show that wage spillovers for male workers rise monotonically with age: the effect becomes positive and significant in the 30–39 group, and increases further in the 40–49 and 50–64 groups. Columns (5)–(8) reveal no spillover effect (on average across the post treatment period) for female workers in any age group. For high-skilled workers, reported in Table A10, we observe a significant positive spillover only among workers aged 40–49.

Overall, the results in this section document two main patterns. First, the 2018 outsourcing ban generated positive wage spillovers onto non-switcher workers in more-exposed local labor markets. Second, these gains are unevenly distributed, i.e., they are concentrated among low-skilled male workers and rise with age.

Figure 14: Wage spillover effect of the outsourcing ban



Notes: The top panel 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 A8. The bottom panels show mean real wages of treated (solid line) and untreated (dashed line) workers over time. The left panel corresponds to female workers, while the right panel corresponds to male workers. Treated occupation-province pairs are those with strictly positive exposure.

7 Conclusion and Discussion

In December 2017, the Turkish government banned public institutions and municipal-owned enterprises (MOEs) from procuring outsourced services and required that all workers providing such services on-site be transferred into permanent public positions within six months. This paper uses the reform as a policy shock and studies its effects on workers, firms, and local labor markets using administrative employer–employee data.

We document three main findings. First, workers who moved into public employment experienced higher wages and lower turnover, indicating improved job stability. These gains are concentrated among low-skilled workers, who make up the majority of those affected by the reform.

Second, the reform had negative effects on private firms that previously provided outsourced services to the public sector. More exposed firms were more likely to exit. Those that survived experienced declines in size, productivity and profitability. At the same time, MOEs that internalized these services experienced increases in productivity and profitability. This suggests that, before the reform, outsourcing contracts were costly for public entities and generated rents for private providers.

Third, the reform generated spillover effects in local labor markets. Province–occupation cells with higher concentrations of absorbed workers experienced positive wage spillovers, particularly among low-skilled, prime-age male workers.

The reform is large in scale and abrupt in its implementation, so it is likely to have non-trivial implications for social welfare. We cannot provide a precise aggregate welfare evaluation, as the reform affected multiple actors through several channels. We can, however, discuss the likely directions of these effects.

The most direct welfare gains came from higher wages and greater job security 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. After the transition, these workers continue performing the same tasks in the same locations, but under public contracts with higher pay and greater job stability. This implies little or no loss in output for low-skilled labor, while higher earnings and reduced job instability unambiguously raise welfare from a utilitarian point of view. In addition, Section 6 shows that the reform generated positive spillovers in local labor markets, increasing wages and reducing turnover among low-skilled workers who remained in the private sector, which constitutes an additional welfare gain.

The evidence also indicates that MOEs became more productive and more profitable after the reform, weakening the argument that the in-house production of outsourced services would impose fiscal or efficiency costs on public entities.

Outsourced service providers emerge as the main losers. For these firms, government contracts were often essential for continued operation. Many firms exited or downsized significantly. The contraction of the outsourced service sector likely raised input costs for private-sector clients, either through higher wages for in-house workers or higher prices for service contracts.

Overall, the evidence indicates that the reform reallocated rents away from private service providers toward workers and public entities. This reallocation benefited a large group of relatively low-income workers, while imposing costs on outsourced firms and 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								
$switcher_i \times D_{t>Mar\ 2018}$	0.0890***	0.1004***	0.1023***	0.1341***	0.0597***	0.0648***	0.0646***	0.0927***
	(0.0042)	(0.0041)	(0.0042)	(0.0043)	(0.0056)	(0.0056)	(0.0056)	(0.0061)
Fixed-effects								
Worker	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Age group		Yes	Yes	Yes		Yes	Yes	Yes
Occupation			Yes	Yes			Yes	Yes
Province $\times$ time				Yes				Yes
Fit statistics								
Observations	200,370,062	200,370,062	200,087,456	200,064,871	64,541,643	64,541,643	64,486,469	64,470,794
R^2	0.63675	0.63870	0.63877	0.64095	0.62073	0.62218	0.62255	0.62426
Within R^2	0.00035	0.00045	0.00047	0.00077	7.14×10^{-5}	8.45×10^{-5}	8.42×10^{-5}	0.00017
Number of clusters	2,051,637	2,051,637	2,050,738	2,048,608	1,097,175	1,097,175	1,096,547	1,094,823

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 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 plotted in Figure 6.

Table A2: Direct wage effects of the outsourcing ban by worker skill and the ruling party of the receiving MOE

	Male								Female							
	All workers	Matched sample	Low-skill	High-skill	AKP-ruled MOES	CHP-ruled MOES	Close-race AKP MOEs	Close-race CHP MOEs	All workers	Matched sample	Low-skill	High-skill	AKP-ruled MOES	CHP-ruled MOES	Close-race AKP MOEs	Close-race CHP MOEs
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Variables																
$switcher_i \times D_{t>Mar\ 2018}$	0.1341***	0.1087***	0.1365***	0.0696***	0.1282***	0.1391***	0.1308***	0.1688***	0.0927***	0.0327***	0.0885***	0.0761***	0.1013***	0.0754***	0.1069***	0.1040***
	(0.0043)	(0.0041)	(0.0044)	(0.0071)	(0.0055)	(0.0081)	(0.0093)	(0.0201)	(0.0061)	(0.0067)	(0.0066)	(0.0112)	(0.0081)	(0.0111)	(0.0192)	(0.0271)
Fixed-effects																
Worker	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age group	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province $\times$ time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics																
Observations	200,064,871	11,652,223	157,259,722	42,692,812	197,557,867	195,037,269	194,242,075	193,807,221	64,470,794	1,561,519	46,403,415	18,005,078	64,064,097	63,859,949	63,696,103	63,632,201
R^2	0.64095	0.51381	0.57793	0.77315	0.64201	0.64282	0.64307	0.64327	0.62426	0.49147	0.53631	0.73431	0.62484	0.62521	0.62531	0.62545
Within R^2	0.00077	0.00463	0.00090	5.44×10^{-5}	0.00045	0.00020	8.75×10^{-5}	1.34×10^{-5}	0.00017	0.00034	0.00016	5.66×10^{-5}	0.00011	3.51×10^{-5}	3.12×10^{-5}	1.34×10^{-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). The dependent variable is the log of the real monthly wage of worker i in period t. Columns (1)–(8) present estimates for male workers, and columns (9)–(16) present estimates for female workers. Columns (2) and (10) present matching sample results. We implement a two-stage matching procedure combining exact matching and propensity score matching (PSM). In the first stage, control observations are exactly matched to treated workers based on gender, province, and one-digit ISCO occupation category prior to treatment. In the second stage, we estimate a propensity score model using pre-treatment characteristics, including four-quarter average wages, worker age, firm age, and firm size. Columns (1) and (9) report estimates for the full sample and correspond to columns (4) and (8) of Table A1, respectively. Columns (3) and (11) restrict the sample to low-skilled workers, while columns (4) and (12) restrict the sample to high-skilled workers. Skill groups are defined based on the first-digit ISCO classification: workers with ISCO codes 1–3 are classified as high-skilled, while all others—excluding interns and individuals with unknown occupations—are classified as low-skilled. Columns (5) and (13) report estimates for switchers employed in MOEs governed by the AKP, while columns (6) and (14) report estimates for those employed in MOEs governed by the CHP. Columns (7) and (15) restrict the sample to municipalities where the AKP won by a margin of less than 5%, and columns (8) and (16) restrict the sample to municipalities where the CHP won by a margin of less than 5%. The variable $switcher_i$ is an indicator equal to one for workers who moved 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, individuals are required to be employed in both 2017q4 and 2018q1; the panel is otherwise unbalanced. All specifications include worker fixed effects, as well as age-group, occupation, and province-by-time fixed effects. 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 number of observations, R^2, and within-R^2 are reported in the table. Corresponding event-study estimates are presented in Figures D1–D4.

Table A3: Worker turnover effect of the outsourcing ban

	Male								Female							
	All workers	Matched sample	Low-skill	High-skill	AKP-ruled MOES	CHP-ruled MOES	Close-race AKP MOEs	Close-race CHP MOEs	All workers	Matched sample	Low-skill	High-skill	AKP-ruled MOES	CHP-ruled MOES	Close-race AKP MOEs	Close-race CHP MOEs
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Variables																
$switcher_i$	0.1385	0.3909***	0.1209	0.2127	0.1249	0.1282	0.0936	0.2164	0.2882	0.5887***	0.2575	0.4199*	0.2533	0.3059	0.2394	0.3964*
	(0.2167)	(0.1702)	(0.2157)	(0.2157)	(0.2268)	(0.2183)	(0.2280)	(0.2398)	(0.1922)	(0.1242)	(0.1861)	(0.2150)	(0.2051)	(0.1977)	(0.1986)	(0.2115)
$switcher_i \times D_{t>Mar\ 2018}$	-1.667***	-1.848***	-1.703***	-1.370***	-1.596***	-1.986***	-1.388***	-2.230***	-1.813***	-1.945***	-1.907***	-1.565***	-1.701***	-2.156***	-1.523***	-2.565***
	(0.2244)	(0.1542)	(0.2278)	(0.2211)	(0.2360)	(0.1991)	(0.1928)	(0.2219)	(0.1873)	(0.1335)	(0.2100)	(0.1953)	(0.1695)	(0.2070)	(0.2085)	(0.2196)
Fixed-effects																
Firm tenure	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics																
Observations	319,484,064	11,433,587	254,033,638	63,491,043	316,741,305	314,129,532	313,238,182	312,806,446	125,909,807	1,553,188	91,995,276	32,580,754	125,420,062	125,216,077	125,008,802	124,942,360
Pseudo R^2	0.08915	0.1388	0.09116	0.07682	0.08871	0.08853	0.08820	0.08830	0.05585	0.1571	0.06013	0.03945	0.05549	0.05546	0.05520	0.05520
BIC	285,447,508.0	7,502,664.6	233,121,221.7	49,391,288.3	283,580,253.8	281,801,204.7	281,275,947.6	280.947,321.5	104,464,441.0	926,939.7	77,699,527.2	24,550,443.6	104,148,191.5	104,014,285.8	103,892,417.3	103,833,471.1

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. 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)–(8) present estimates for male workers, and columns (9)–(16) present estimates for female workers. Columns (2) and (10) present matching sample results. We implement a two-stage matching procedure combining exact matching and propensity score matching (PSM). In the first stage, control observations are exactly matched to treated workers based on gender, province, and one-digit ISCO occupation category prior to treatment. In the second stage, we estimate a propensity score model using pre-treatment characteristics, including four-quarter average wages, worker age, firm age, and firm size. Columns (1) and (9) presents whole sample estimations and their corresponding event-study results are plotted in Figure 7. Columns (3) and (11) restrict the sample to low-skilled workers, while columns (4) and (12) restrict the sample to high-skilled workers. Skill groups are defined based on the first-digit ISCO classification: workers with ISCO codes 1–3 are classified as high-skilled, while all others—excluding interns and individuals with unknown occupations—are classified as low-skilled. Columns (5) and (13) report estimates for switchers employed in MOEs governed by the AKP, while columns (6) and (14) report estimates for those employed in MOEs governed by the CHP. Columns (7) and (15) restrict the sample to municipalities where the AKP won by a margin of less than 5%, and columns (8) and (16) restrict the sample to municipalities where the CHP won by a margin of less than 5%. The variable $switcher_i$ is an indicator equal to one for workers who moved 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 plotted in Figures D5 and D8.

Table A4: Firm survival effect of the outsourcing ban by skill group and municipal political environment

Exposure:	Employment	Wage	Employment	Employment	Employment	Employment	Employment	Employment
	All sample	All sample	Low-skill	High-skill	AKP-contracted	CHP-contracted	Close-race AKP contracted	Close-race CHP contracted
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
$log(initial\ employment_f)$	-0.3226***	-0.3226***	-0.3083***	-0.3826***	-0.3229***	-0.3232***	-0.3233***	-0.3233***
	(0.0294)	(0.0294)	(0.0935)	(0.0702)	(0.0295)	(0.0297)	(0.0298)	(0.0297)
$expoosure_f$	-0.2555	-0.2869	-0.2433	-0.2685	-0.3365	-0.0391	-0.2435	-0.0868
	(0.2515)	(0.2469)	(0.2712)	(0.5743)	(0.2432)	(0.3220)	(0.2237)	(0.2728)
$expoosure_f \times D_{t>Mar\ 2018}$	1.165***	1.169***	1.182***	1.106	1.283***	1.343***	0.7804***	1.005***
	(0.2603)	(0.2583)	(0.2014)	(0.8893)	(0.2193)	(0.3949)	(0.2609)	(0.2707)
Fixed-effects								
Industry	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics								
Observations	36,500,949	36,500,949	3,216,468	2,915,937	36,477,422	36,461,343	36,451,212	36,448,272
Pseudo R^2	0.04180	0.04180	0.03087	0.04261	0.04179	0.04178	0.04176	0.04176
BIC	11,820,730.1	11,820,726.3	1,094,422.5	798,894.3	11,816,328.3	11,813,546.0	11,811,726.1	11,811,239.9

Notes: This table reports the effects of the 2018 outsourcing ban on firm survival, 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. Corresponding event-study results of Column (1) is plotted in Figure 8. Column (3) presents results for firms operating in low-skilled outsourcing industries, while Column (4) presents results for firms in high-skilled outsourcing industries. Outsourcing industries are classified into two groups: (i) low-skilled business services, including food services, cleaning, security, and logistics; and (ii) high-skilled professional services, including IT, programming, consultancy, engineering, and architecture. Columns (5)–(8) restrict the sample based on the political affiliation of the municipal-owned enterprises (MOEs) with which firms have contracts. Column (5) includes outsourcing firms contracting with AKP-governed MOEs, while Column (6) includes outsourcing firms contracting with CHP-governed MOEs. Column (7) further restricts the treated sample to firms contracting with AKP-governed MOEs in municipalities where the winning margin is less than 5%, and Column (8) applies the same restriction for CHP-governed MOEs. All specifications control for initial firm size, 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 alternatively 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. Goodness-of-fit measures (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 * $p < 0.1$.

Table A5: Firm size effect of the outsourcing ban by skill group of industries and political heterogeneity

Exposure:	Employment All sample	Wage All sample	Employment Low-skill	Employment High-skill	Employment AKP contracted	Employment CHP contracted	Employment Close-race AKP contracted	Employment Close-race CHP contracted
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
$expoosure_f \times D_{t>Mar\ 2018}$	-2.885***	-2.743***	-3.308***	-2.752***	-2.851***	-2.799***	-2.824***	-3.349***
	(0.1390)	(0.1364)	(0.1837)	(0.2150)	(0.1839)	(0.2712)	(0.3777)	(0.3680)
Fixed-effects								
Firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province $\times$ time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics								
Observations	9,671,868	9,671,868	750,492	496,476	9,660,960	9,653,796	9,648,972	9,647,496
R^2	0.86801	0.86800	0.82540	0.83094	0.86708	0.86672	0.86599	0.86584
Number of clusters	268,663	268,663	20,847	13,791	268,360	268,161	268,027	267,986

Notes: This table reports the firm-size effects of the 2018 outsourcing ban, estimated using Equation (5). The dependent variable is the log of 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. Corresponding event-study results of Column (1) is plotted in Figure 9. Column (3) presents results for firms operating in low-skilled outsourcing industries, while Column (4) presents results for firms in high-skilled outsourcing industries. Outsourcing industries are classified into two groups: (i) low-skilled business services, including food services, cleaning, security, and logistics; and (ii) high-skilled professional services, including IT, programming, consultancy, engineering, and architecture. Columns (5)–(8) restrict the sample based on the political affiliation of the municipal-owned enterprises (MOEs) with which firms have contracts. Column (5) includes outsourcing firms contracting with AKP-governed MOEs, while Column (6) includes outsourcing firms contracting with CHP-governed MOEs. Column (7) further restricts the treated sample to firms contracting with AKP-governed MOEs in municipalities where the winning margin is less than 5%, and Column (8) applies the same restriction for CHP-governed MOEs. 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. The coefficient on the interaction between $exposure_f$ and the post-reform indicator captures the effect of the reform on firm employment. All specifications include firm and province-by-time fixed effects. The sample consists of firms operating in the same NACE industries as outsourced firms. Goodness-of-fit measures (R^2 and number of clusters) and the number of observations are reported in the table. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

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

Outcome:	$asinh(v.added/labor)$	$profit\ rate$
Model:	(1)	(2)
Variables		
$exposure_f \times D_{t\,>\,2018}$	-0.5644***	-0.0335**
	(0.1544)	(0.0145)
Fixed-effects		
Firm	Yes	Yes
Province $\times$ year	Yes	Yes
Fit statistics		
Observations	3,216,032	3,604,187
R^2	0.38984	0.42819
Within R^2	1.34×10^{-5}	2.62×10^{-6}
Number of clusters	355,063	378,228

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 profit rate (operating profit/output). The treatment variable $exposure_f \times D_{t>2018}$ captures firms' exposure to the reform interacted with a post-reform indicator. Firm and province $\times$ 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 10 and 11.

Table A7: Productivity and profitability effects of the outsourcing ban on the MOEs

Outcome:	$asinh(v.added/labor)$	$profit\ rate$
Model:	(1)	(2)
Variables		
$MOE_f \times D_{t\,>\,2018}$	0.6275***	0.0906***
	(0.1250)	(0.0147)
Fixed-effects		
Firm	Yes	Yes
Province $\times$ year	Yes	Yes
Fit statistics		
Observations	4,245,042	5,164,637
R^2	0.47317	0.49990
Within R^2	1.85×10^{-5}	1.92×10^{-5}
Number of clusters	908,442	1,102,878

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 profit rate (operation profit/output). The treatment variable $MOE_f \times D_{t>2018}$ is an interaction between an indicator for MOE firms and a post-reform indicator. Firm and province $\times$ 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, within R^2 and number of clusters are reported in the table. Corresponding event-study estimates are reported in Figures 12 and 13. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table A8: Wage spillover effect of the outsourcing ban

	Male				Female			
	All workers	Exc. big cities	Low-skill	High-skill	All workers	Exc. big cities	Low-skill	High-skill
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
$exposure_{op} \times D_{t>Mar\ 2018}$	0.1232***	0.1230***	0.1230**	0.1177*	-0.0217	-0.0246	-0.0066	-0.0401
	(0.0404)	(0.0403)	(0.0465)	(0.0634)	(0.0379)	(0.0378)	(0.0430)	(0.0333)
Fixed-effects								
Province $\times$ occupation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation $\times$ time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province $\times$ time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics								
Observations	113,336	108,882	69,347	43,627	108,619	104,215	65,501	42,961
R^2	0.84982	0.7245	0.75882	0.84935	0.66854	0.72614	0.49679	0.69550
Within R^2	0.00136	0.00137	0.00173	0.00094	2.43×10^{-5}	3.13×10^{-5}	2.45×10^{-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)–(4) present estimates for male workers, while columns (5)–(8) present estimates for female workers. Columns (1) and (4) report estimates for all workers, columns (3) and (7) for low-skilled workers, and columns (4) and (8) for high-skilled workers. As robustness check, Equation (8) is re-estimated by excluding big cities and results are reported in columns (2) and (6). 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 $\times$ occupation, occupation $\times$ time, and province $\times$ 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 $\times$ 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 14 and D9.

Table A9: 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 $\times$ occupation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation $\times$ time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province $\times$ 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 $\times$ occupation, occupation $\times$ time, and province $\times$ 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 $\times$ 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 D11.

Table A10: 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 D12.

B Classification of outsourcing industries

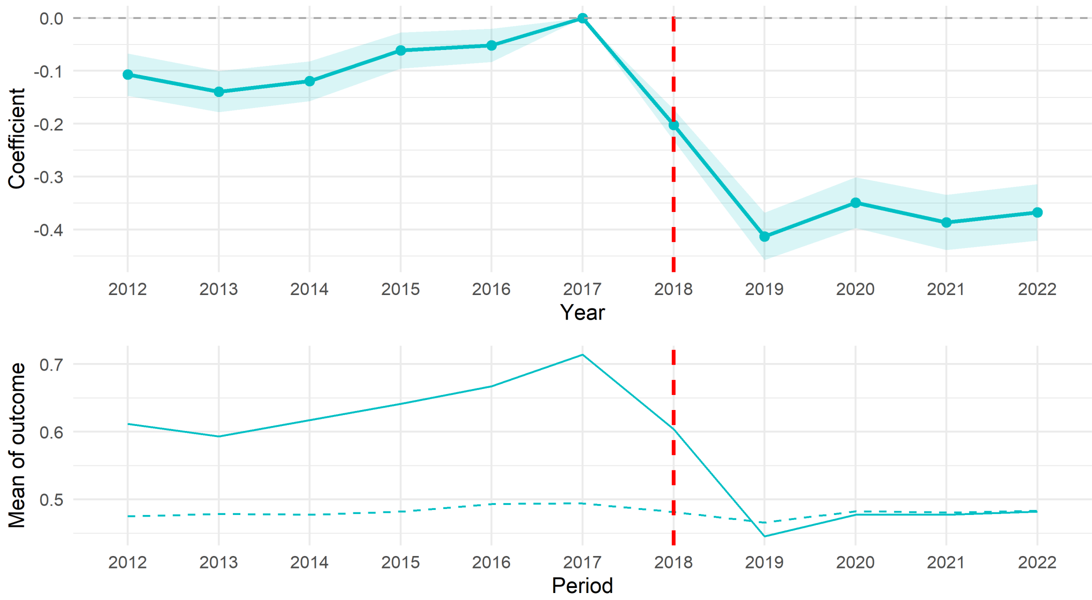
Table B1: Skill classification of outsourcing industries

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.

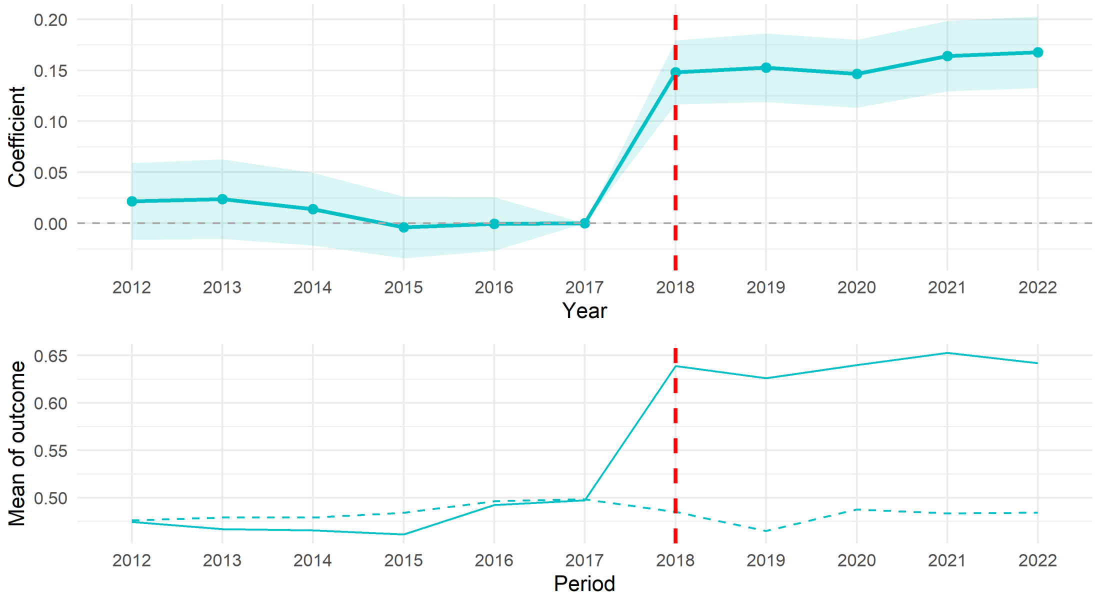
C Value added share effect of the outsourcing ban

Figure C1: value added share effect of the outsourcing ban on outsourced firms



Notes: The top panel plots the value added share 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 3,733,382 observations. The reform year (2018) is indicated by the red vertical dashed line. 2017 is the base period. The bottom panel shows mean value added / output of treated (solid line) and untreated (dashed line) firms over time. Treated firms are those with strictly positive exposure.

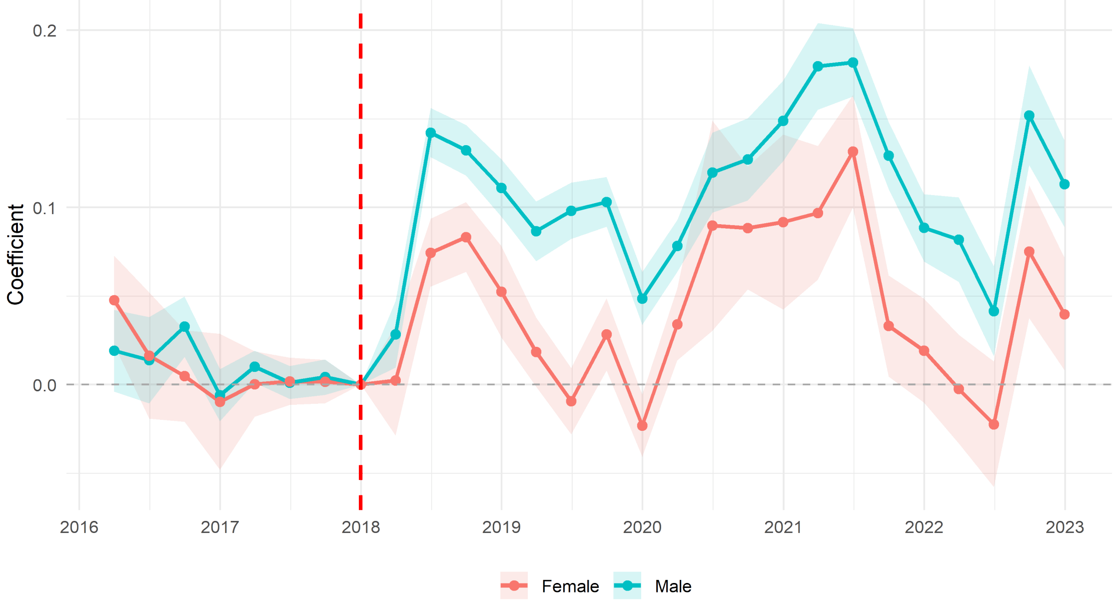
Figure C2: Value added share effect of the outsourcing ban on MOEs



Notes: The top panel plots the value added share 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 5,489,425 observations. The reform year (2018) is indicated by the red vertical dashed line. 2017 is the base period. The bottom panel shows mean value added / output of treated (solid line) and untreated (dashed line) firms over time.

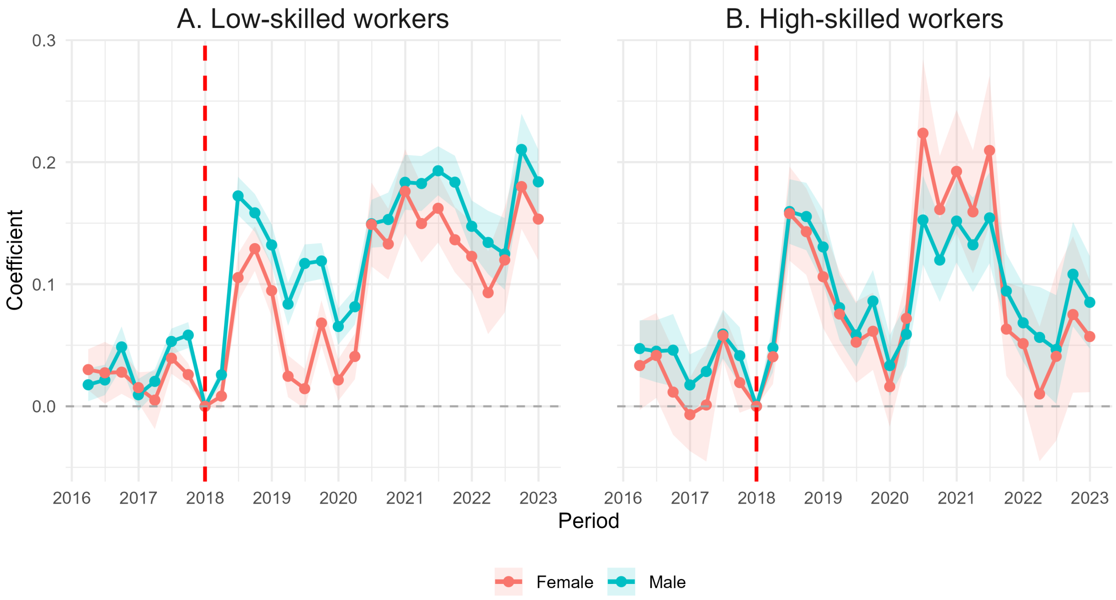
D Event Study Results by Subsample

Figure D1: Wage effect of the outsourcing ban (matched sample)



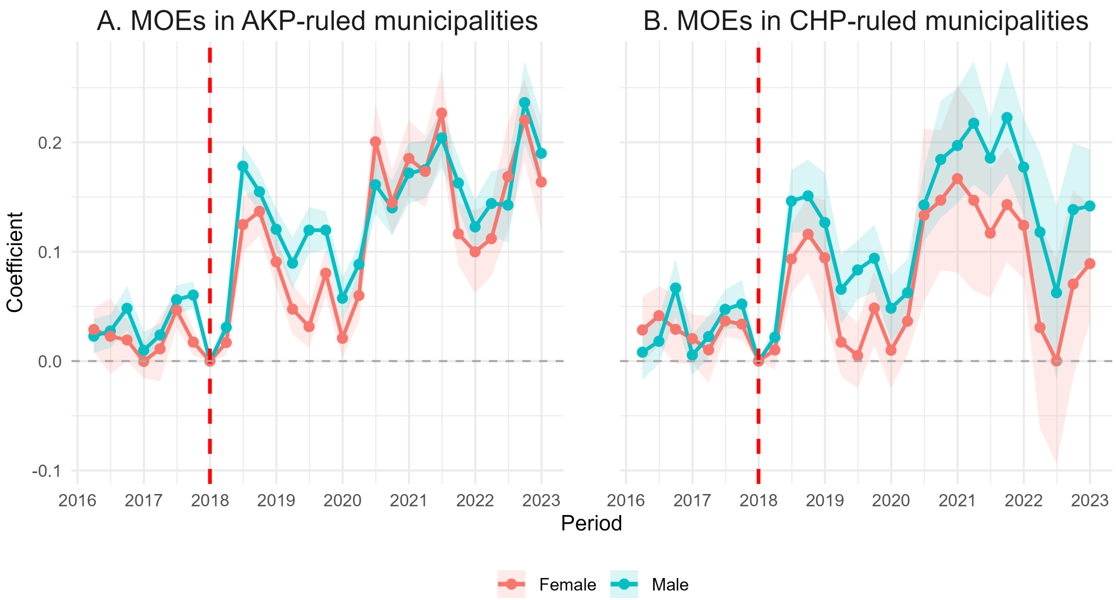
Notes: This figure plots the wage effects of the outsourcing ban on workers who transitioned from outsourced firms to municipal-owned enterprises, estimated using Equation (1). The results are based on a matched sample constructed using a two-stage procedure that combines exact matching and propensity score matching (PSM), and are reported separately for male and female workers. In the first stage, control observations are exactly matched to treated workers based on gender, province, and one-digit ISCO occupation category prior to treatment. In the second stage, propensity scores are estimated using pre-treatment characteristics, including four-quarter average wages, worker age, firm age, and firm size. Standard errors are clustered at the firm level. The regressions include 11,652,223 observations for male workers and 1,561,519 observations for female workers. The quarter immediately preceding treatment (2017q4), indicated by the vertical red dashed line, serves as the reference period. The figure reports point estimates with 95% confidence intervals. The corresponding difference-in-differences estimates are reported in Table A2.

Figure D2: Wage effect of the outsourcing ban, low- and high-skilled workers



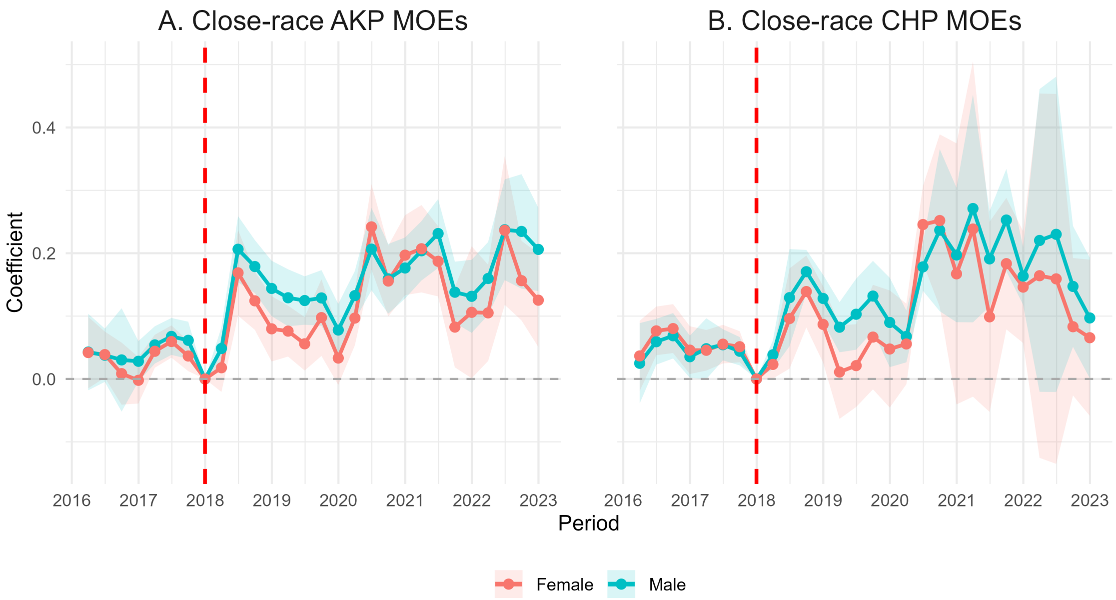
Notes: These figures show the wage effect of the outsourcing ban on workers who switched from outsourced firms to municipal-owned enterprises, estimated using Equation (1) for different subsamples defined by workers' skill levels. Panel A presents the effects for low-skilled workers, while Panel B presents 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 46,403,415 observations in Panel A. In Panel B, regressions for male workers include 42,692,812 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.

Figure D3: Direct wage effects of the outsourcing ban by the ruling party of the receiving MOE



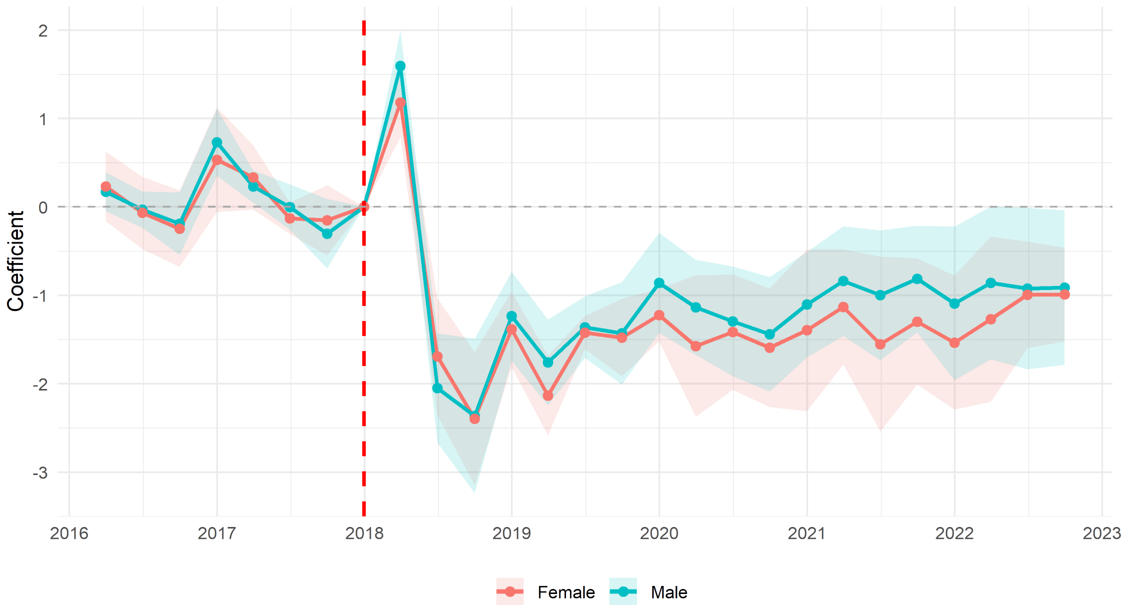
Notes: These figures present the wage effects of the outsourcing ban on workers who transitioned from outsourced firms to municipal-owned enterprises (MOEs), estimated using Equation (1) across subsamples defined by the ruling party of the destination MOE. Panel A reports the effects for workers employed in AKP-governed MOEs, while Panel B reports the effects for workers employed in CHP-governed MOEs. To ensure comparability with the treatment group, individuals in the control group are required to be employed in both 2017q4 and 2018q1; the panel is otherwise unbalanced. Standard errors are clustered at the firm level. In Panel A, regressions for male workers include 197,557,867 observations, while those for female workers include 64,064,097 observations. In Panel B, regressions for male workers include 195,037,269 observations, and those for female workers include 63,859,949 observations. The quarter immediately preceding treatment (2017q4), indicated by the vertical red dashed line, serves as the reference period. The corresponding average difference-in-differences estimates are reported in Table A2.

Figure D4: Direct wage effects of the outsourcing ban by the ruling close-race party of the receiving MOE



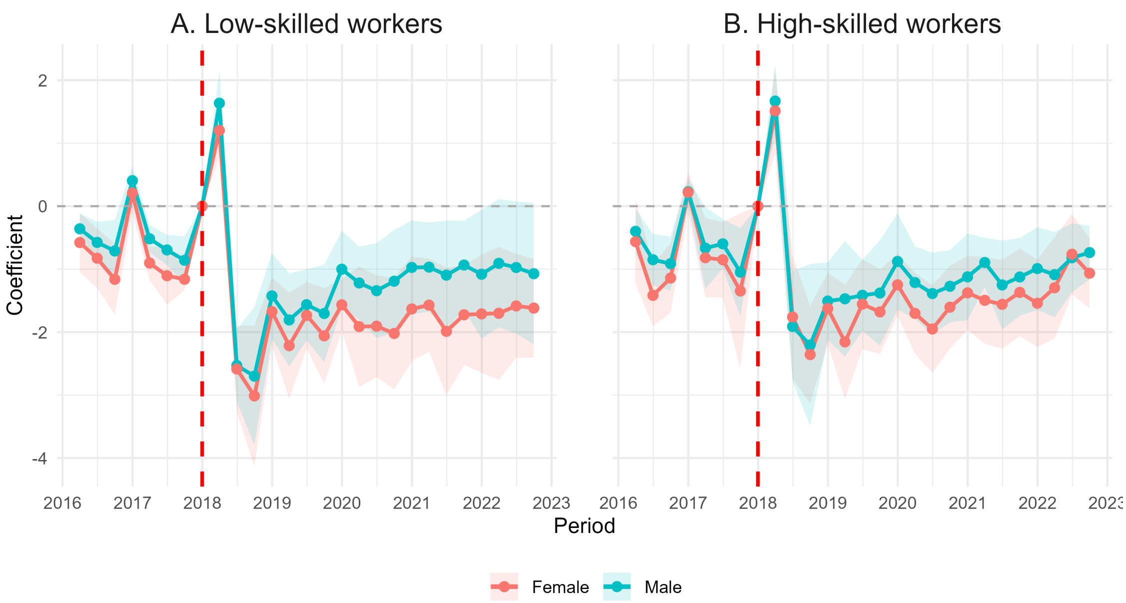
Notes: These figures present the wage effects of the outsourcing ban on workers who transitioned from outsourced firms to municipal-owned enterprises (MOEs), estimated using Equation (1) across subsamples defined by the ruling party in close municipal elections. The treated sample is restricted to switcher workers employed in municipalities where the winning party secured victory by a margin of less than 5%. Panel A reports the effects for workers employed in AKP-governed MOEs, while Panel B reports the effects for workers employed in CHP-governed MOEs. To ensure comparability, individuals in the control group are required to be employed in both 2017q4 and 2018q1; the panel is otherwise unbalanced. Standard errors are clustered at the firm level. In Panel A, regressions for male workers include 194,242,075 observations, while those for female workers include 63,696,103 observations. In Panel B, regressions for male workers include 193,807,221 observations, and those for female workers include 63,696,103 observations. The quarter immediately preceding treatment (2017q4), indicated by the vertical red dashed line, serves as the reference period. The corresponding average difference-in-differences estimates are reported in Table A2.

Figure D5: Worker turnover effect of the outsourcing ban (matched sample)



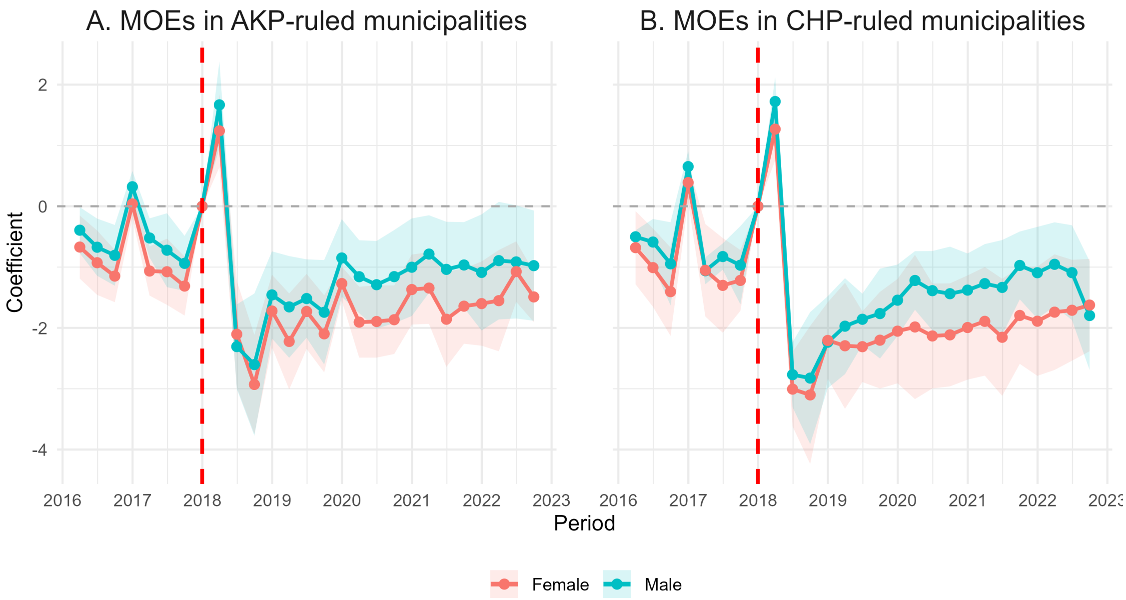
Notes: This figure shows the turnover effect of the outsourcing ban on workers who switched from outsourced firms to municipal-owned enterprises, estimated using Equation (2). The results are based on a matched sample constructed using a two-stage procedure that combines exact matching and propensity score matching (PSM), and are reported separately for male and female workers. In the first stage, control observations are exactly matched to treated workers based on gender, province, and one-digit ISCO occupation category prior to treatment. In the second stage, propensity scores are estimated using pre-treatment characteristics, including four-quarter average wages, worker age, firm age, and firm size. 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 319,484,064 observations, while regressions for female workers include 125,909,807 observations. The quarter immediately preceding treatment (2017q4), indicated by the vertical red dashed line, is used as the base period.

Figure D6: Worker turnover effect of the outsourcing ban, low- and high-skilled workers



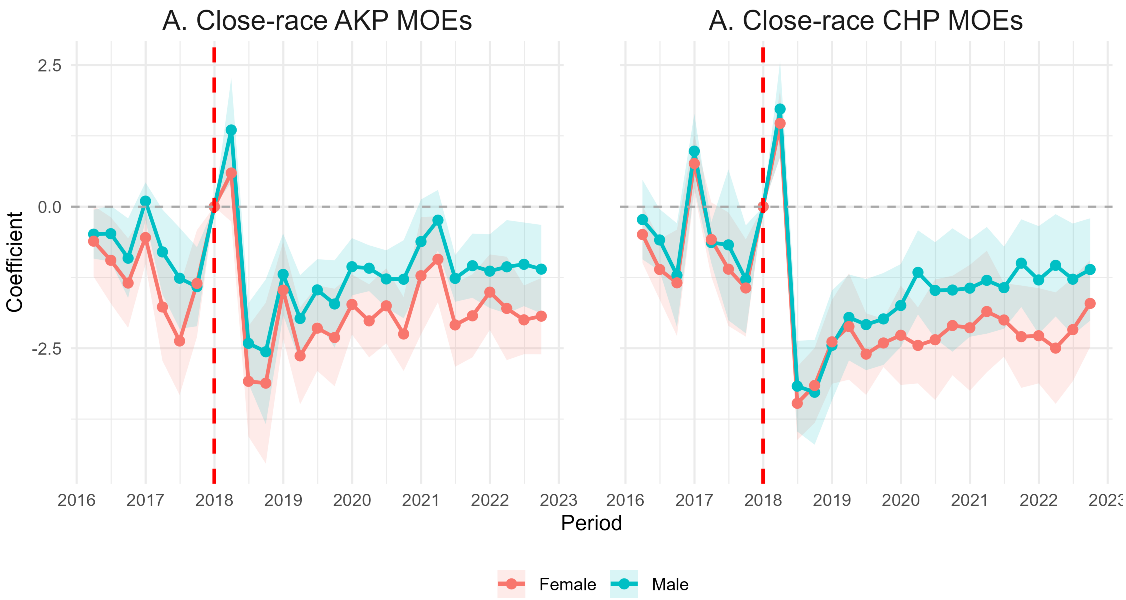
Notes: These figures show the turnover effect of the outsourcing ban on workers who switched from outsourced firms to municipal-owned enterprises, estimated using Equation (2) for different subsamples defined by workers' skill levels. Panel A presents the effects for low-skilled workers, while Panel 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 A3.

Figure D7: Worker turnover effects of the outsourcing ban bythe ruling party of the receiving MOE



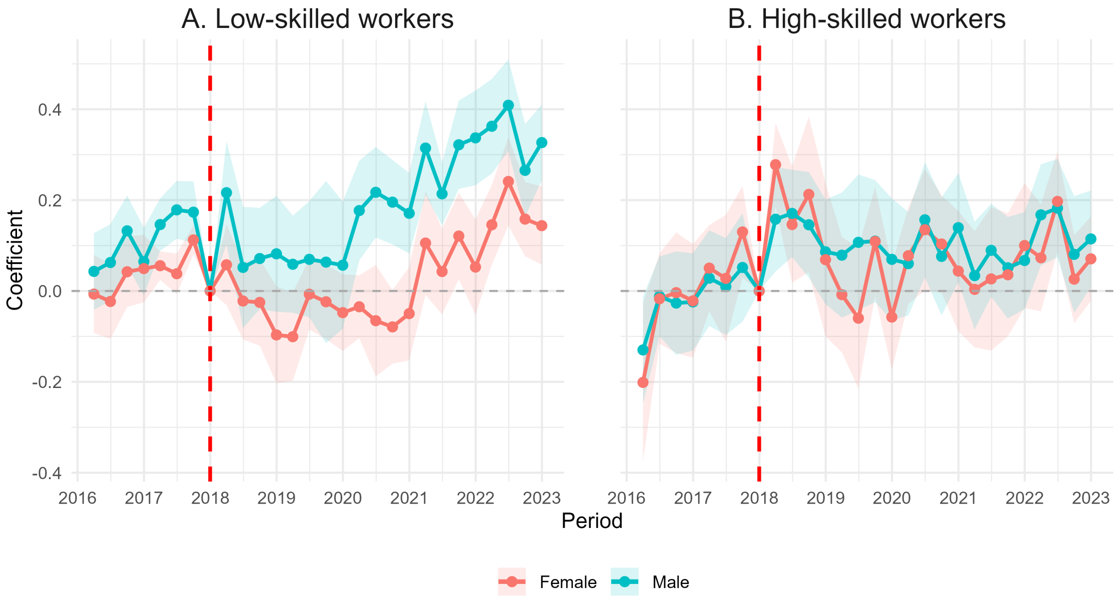
Notes: These figures show the turnover effect of the outsourcing ban on workers who switched from outsourced firms to municipal-owned enterprises, estimated using Equation (2) across subsamples defined by the ruling party of the destination MOE. Panel A reports the effects for workers employed in AKP-governed MOEs, while Panel B reports the effects for workers employed in CHP-governed MOEs. 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 316,741,305 observations, while regressions for female workers include 125,420,062 observations in Panel A. In Panel B, regressions for male workers include 314,129,532 observations, while regressions for female workers include 125,216,077 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 A3.

Figure D8: Worker turnover effects of the outsourcing ban bythe ruling party of the receiving MOE



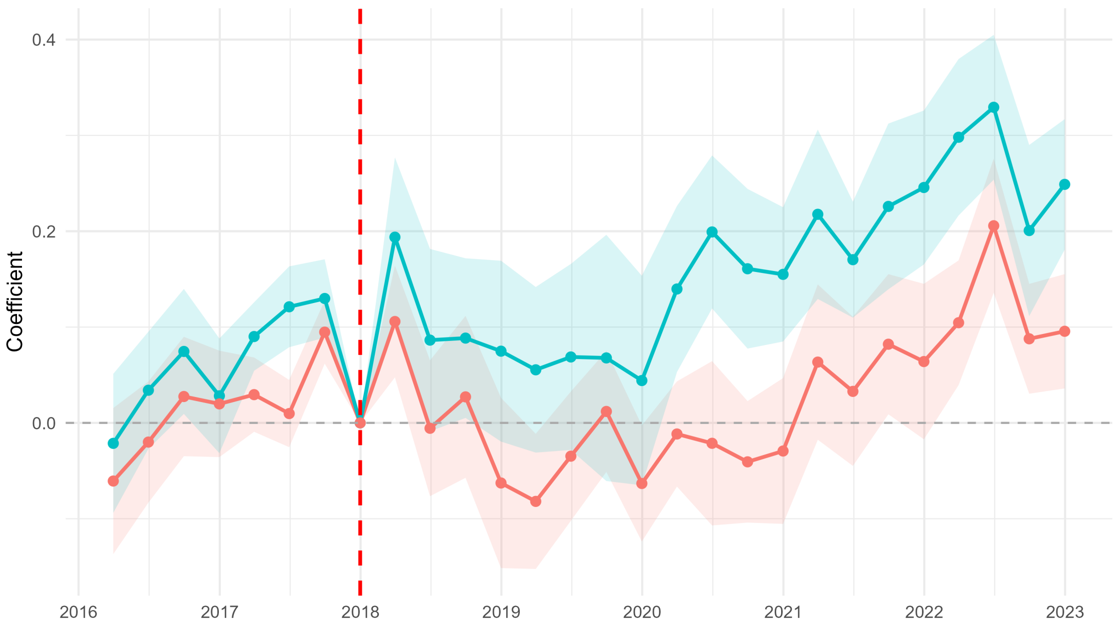
Notes: These figures show the turnover effect of the outsourcing ban on workers who switched from outsourced firms to municipal-owned enterprises, estimated using Equation (2) across subsamples defined by the ruling party in close municipal elections. The treated sample is restricted to switcher workers employed in municipalities where the winning party secured victory by a margin of less than 5%. Panel A reports the effects for workers employed in AKP-governed MOEs, while Panel B reports the effects for workers employed in CHP-governed MOEs. 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 313,238,182 observations, while regressions for female workers include 125,008,802 observations in Panel A. In Panel B, regressions for male workers include 312,806,446 observations, while regressions for female workers include 124,942,360 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 A3.

Figure D9: Wage spillover effect of the outsourcing ban, low- and high-skilled workers



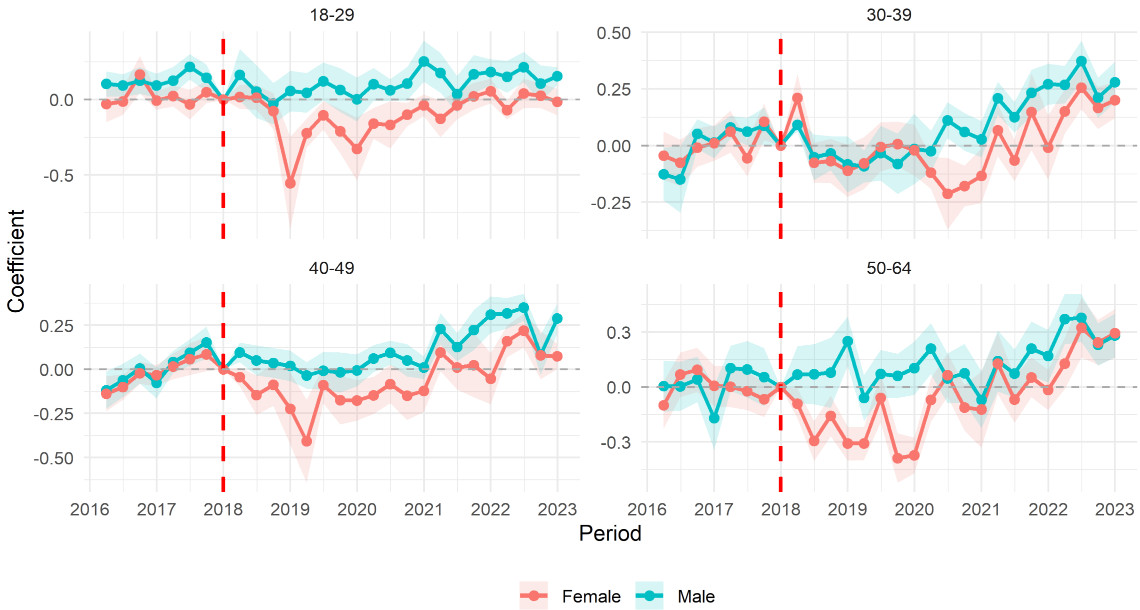
Notes: These figures show the wage spillover effects of the outsourcing ban on workers who did not switch from outsourced firms to municipal-owned enterprises (MOEs), estimated using Equation (8) for different subsamples defined by workers' skill levels. Panel A presents the effects for low-skilled workers, 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 A8.

Figure D10: Wage spillover effect of the outsourcing ban excl. big three metropolitan cities



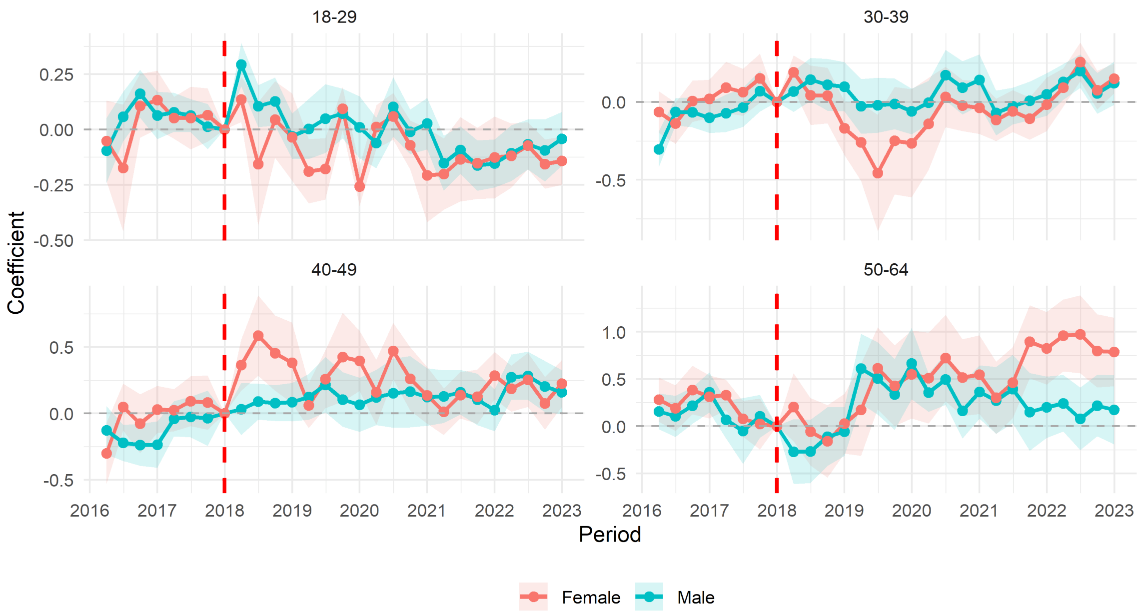
Notes: This figure shows the wage spillover effects of the outsourcing ban on workers who did not switch from outsourced firms to municipal-owned enterprises (MOEs), estimated using Equation (8) excluding Istanbul, Ankara and Izmir. Switcher workers and MOE employees are excluded from the sample. Exposure 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 A8.

Figure D11: Wage spillover effect of the outsourcing ban by age group, low-skilled workers



Notes: These figures show 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 A9.

Figure D12: Wage spillover effect of the outsourcing ban by age group, high-skilled workers



Notes: These figures show 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 A10.