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Just After Minimum Wage Hikes:

Short-Run Labor-Demand Response and Reallocation

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Abstract

How labor markets adjust immediately after minimum wage hikes remains an open, policy-relevant question. This paper studies short-run minimum-wage effects in Japan's spot labor market using Timee data and a wage-bin difference-in-differences design. We find a 2% employment decline in affected bins, driven by reduced vacancy creation rather than worker supply. Effects are more negative where the minimum-wage bite is higher and in low-wage occupations. Using job descriptions and amenity information, we document reallocation across job types: postings shift toward greater amenity provision and experienced-worker targeting, while female-targeted descriptions become less common, suggesting short-run labor-demand adjustments may foreshadow longer-run reallocation.

Keywords: minimum wage, employment, job amenities, gig workers, spot work, difference-in-differences

JEL codes: J21, J23, J38, J88

1 Introduction

The gig economy—comprising temporary, contract-based, and freelance online work—has expanded rapidly (Boeri *et al.* 2020). An increasing share of freelance workers is now matched with customers through online platforms.[1] Because gigs range from on-demand shifts to project-based contracts, gig work represents one of the most adjustable forms of labor demand. Yet, evidence on minimum-wage effects in this setting remains limited, with a few notable exceptions (Glasner 2023). In particular, for alternative work arrangements (Mas and Pallais 2020)—and especially for spot labor markets in which posted jobs require minimal skills and worker turnover is high—we still know little about the extent of employment declines, how quickly they occur, and which jobs contract in the short run. Clarifying the magnitude, timing, and composition of these short-run employment changes and the resulting reallocation is essential for designing timely, well-targeted policy support and for linking immediate adjustments to longer-run reallocation (Dustmann *et al.* 2022).

We address these questions using confidential job-posting and contract-level data from *Timee*, a leading Japanese on-site spot-work platform. Spot work platforms have expanded worldwide, reflecting a broader shift toward flexible, on-demand labor markets.[2] We exploit the platform's data to disentangle labor demand from employment by studying vacancies and matches side by side and to trace short-run dynamics around minimum-wage changes at daily, weekly, and monthly frequencies. We further use non-wage amenities and job-description content to characterize job types, allowing us to document reallocation across them. These features provide a high-frequency view of how labor demand and job composition adjust in the immediate aftermath of a wage-floor increase.

Spot labor markets exhibit three features that make them ideal for studying short-run minimum-wage effects: limited labor heterogeneity, short-term employment contracts, and wage posting. First, they attract workers seeking jobs that require minimal skills and are therefore especially exposed to minimum-wage changes. The resulting matches are relatively homogeneous, making these markets close to the textbook homogeneous-labor benchmark. Second, short-term contracts allow labor demand to adjust rapidly. Firms hire for discrete, temporary tasks rather than permanent positions, so demand in the spot market is likely to be more elastic than in standard labor markets. Our spot-work data capture an extreme case of short-term labor-demand adjustment, a phenomenon rarely observed in the empirical literature, which often focuses instead on the labor

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[1]For instance, Kässi and Lehdonvirta (2018) report that the demand for online gig work grew by approximately 21% between 2016 and 2018, based on an analysis of open projects and tasks on various platforms.

[2]Examples include Taskmo (India), TROOPERS (Singapore), Geupgu (South Korea), QuikShift (Australia), Coople (Switzerland/UK), and Instawork (United States). Related on-demand platforms also include TaskRabbit (United States; task-based on-site services), Wonolo (United States), and GigSmart (United States).

supply of taxi and Uber drivers (Angrist *et al.* 2021, Buchholz *et al.* 2023, Kuroda and Onishi 2023). Finally, wage posting precludes bargaining—a central element in traditional models of long-term employment contracts—allowing us to focus on models grounded in monopsonistic or oligopsonistic labor-market structures. Because adjustment costs are exceptionally low in this setting, our estimated demand elasticities should be interpreted as an upper bound for standard employment elasticities in traditional labor markets. These short-run responses are complementary to, rather than in conflict with, longer-run evidence on minimum-wage elasticities, reallocation, and firm exit, and they clarify the earliest and most flexible margin through which minimum-wage shocks propagate (Dube and Lindner 2024).

Our data offer three advantages for studying short-run adjustments in spot labor markets. First, the data are recorded at the event level with associated click logs, allowing us to track employers' job-posting activity and workers' application activity immediately before and after the implementation of the new minimum wage. This granularity is substantially finer than the month–by-state level used in Sabia (2009a,b), Brochu *et al.* (2025) and Melo *et al.* (2025) or the quarter–by-state level in Addison *et al.* (2009) and Cengiz *et al.* (2019), enabling a more detailed view of temporal adjustments. Second, because establishments can readily modify job descriptions at the time of posting and the data provide clearly observable information on fixed-amount transportation reimbursements as adjustable non-wage amenities, we can use this amenity and description information to characterize job types and trace detailed reallocation across job types in the short run. Third, since spot labor demand is not tied to long-term employment relationships, it is less affected by reputation concerns or switching costs, making this an ideal setting for identifying immediate behavioral responses to policy changes.

From a macro perspective, Timee's role in part-time spot employment has grown markedly in recent years. While the number of active users has increased gradually, the number of vacancies has grown sharply, especially since 2022, leading to a rise in labor-market tightness on the platform. Hiring counts show a substantial upward trend beginning in 2020, reflecting a pronounced increase in successful matches. The job-finding rate has risen above 1.0, indicating that each active worker matches with multiple spot vacancies per month. Meanwhile, the worker-finding rate has remained stable at approximately 0.8, suggesting that about 80% of posted jobs are filled. These trends highlight the platform's growing role in facilitating spot employment matching.

As an illustrative example, we first use granular platform data from Tokyo to examine how the 2023 statutory minimum-wage increase reshaped employment patterns at the intensive margin of job characteristics. This uniquely detailed dataset allows us to track daily, weekly, and monthly shifts in the employment distribution across wage levels, posted working hours, and transportation reimbursements. Descriptive evidence reveals a clear upward shift in the wage distribution around the new minimum-wage threshold, consistent with firms adjusting wages to comply with the policy.

By contrast, working hours and transportation reimbursements remain largely stable, suggesting limited adjustment in other job characteristics. These fine-grained patterns highlight the value of high-frequency, contract-level data in capturing immediate and heterogeneous responses to labor-market regulation, and they motivate a causal analysis that isolates the impact of the minimum-wage change from broader trends.

Given these patterns, we employ a difference-in-differences (DID) framework inspired by Cengiz *et al.* (2019) and Dustmann *et al.* (2022), which leverages variation in exposure to the minimum wage increase across wage bins. Rather than identifying treated workers based on demographic or job characteristics, we exploit the wage distribution itself, comparing outcomes in bins near the new minimum wage threshold with those in the upper tail of the distribution, which are assumed to be unaffected. We define treatment and control groups based on their relative position to the minimum wage in each prefecture, enabling a consistent bin-level comparison over time. This granular approach is well suited to our setting, as it fully utilizes the spot-work platform data aggregated at the prefecture-by-month-by-wage-bin level. By estimating dynamic effects through interactions of relative time and exposure group indicators, we trace the evolution of treatment effects before and after the policy change. To summarize these effects across the distribution, we implement the "excess jobs minus missing jobs" decomposition following Cengiz *et al.* (2019) and compute employment elasticities and changes in the average wage for affected workers.

Our findings show that the minimum wage increase reduces employment at the lower end of the wage distribution. The increase yields an approximately 2% decline in total employment among wage bins directly affected by the policy, with a 3% reduction in jobs below the new minimum wage and a 1% increase just above it. Consistent with this pattern, the employment elasticity with respect to the minimum wage, computed following Cengiz *et al.* (2019), is -0.388. Given that spot work represents the most flexible adjustment margin, we interpret this estimate as an upper bound for standard employment elasticities, complementing rather than contradicting longer-run evidence. These effects materialize immediately after the policy takes effect and remain stable throughout the post-minimum-wage-hike period. Unlike evidence from broader labor markets where excess jobs offset missing jobs (Cengiz *et al.* 2019), our setting shows that missing jobs consistently exceed excess jobs—at least in the very short run, when adjustment through capital deepening or substitution toward long-term employment relationships is difficult—resulting in a net negative employment effect. These results indicate that the spot labor market adjusts quickly to minimum-wage hikes, primarily through employment reductions.

We further show that these employment effects operate primarily through labor demand rather than labor supply. Vacancy postings decline sharply below the new minimum wage, while modest gains appear just above it, indicating that firms adjust mainly by reducing vacancy creation. By contrast, total working hours decline much less than vacancy counts, as missing and excess hours

largely offset each other in the months following the reform. Because total hours fall much less than vacancy counts, hours per vacancy rise modestly: firms post fewer jobs but offer slightly longer shifts. On the worker side, the high-frequency platform data reveal no evidence of a contraction in labor supply around the policy change or toward the end of the fiscal year, and employment trends remain similar across gender and worker-status groups (e.g., students and stay-at-home spouses). This evidence points to labor-demand responses as the primary driver of the observed employment decline. Using weekly and daily data, we also find that these effects appear immediately after implementation and stabilize quickly.

Next, we use our distributional DID approach to characterize heterogeneity in minimum-wage effects across prefectures, occupations, time-of-day categories, and job attributes. Exploiting prefecture-level variation in the minimum-wage bite, we show that employment losses are more pronounced in prefectures with higher Kaitz indices, indicating stronger contractions where the wage floor is more binding. We then document substantial occupational heterogeneity: the largest reductions in low-wage employment are concentrated in restaurants, retail, and event staffing, while logistics, customer service, and light work display more balanced patterns with partial offsetting gains above the new minimum. Modest increases in higher wage bins appear in some occupations, including office work. Across time-of-day categories, effects are similar, indicating little within-day substitution.

Moving beyond broad occupational shifts, we demonstrate that minimum-wage hikes also trigger significant changes in the internal composition of job postings within the same categories. Specifically, employment reweights toward postings that offer transportation reimbursement and toward descriptions targeting experienced workers, while female-targeted descriptions become less common. Taken together, these results indicate that minimum-wage increases generate not only net employment effects but also systematic short-run reallocation in job characteristics and recruitment language. Consistent with the long-run reallocation mechanisms highlighted by Dustmann et al. (2022), our evidence suggests that such reallocation manifests immediately along the most flexible margin—spot labor demand—where adjustments occur first through the entry, exit, and reshaping of postings before propagating to longer-run changes in firm composition and employment structure.

1.1 Related Literature

Our paper contributes to three related literatures: minimum wage effects in platform-mediated spot labor markets, the reallocation of labor across job types in response to minimum-wage hikes, and the short-run dynamics of job destruction and creation around the wage floor.

First, we contribute to the emerging literature on alternative work arrangements and online

labor markets (Mas and Pallais 2020, Katz and Krueger 2019) by studying the employment effects of minimum wage increases in spot labor markets. Existing studies on platform-mediated work largely focus on remote gig work involving relatively high-skill tasks, such as programming or design (Glasner 2023). In contrast, we study on-site spot work—short-term, low-skill jobs that require physical presence and are accessible to a broad segment of the workforce—which has expanded rapidly worldwide as part of a broader shift toward flexible, on-demand labor arrangements.

These features imply that spot labor markets differ fundamentally in their exposure to minimum-wage policies. Jobs are frequently posted at or near the minimum wage, and both workers and employers are directly affected by statutory wage floors, leaving limited scope for spillover effects to higher wages. Using comprehensive platform data, we find clear negative employment effects following minimum-wage increases, accompanied by minimal spillovers across the wage distribution.[3] In this respect, our finding of a clear negative employment effect contrasts with the limited employment effects documented for the broader labor market in Lee (1999), Autor *et al.* (2016), Cengiz *et al.* (2019), and highlights how the nature of work—on-site, low-skill, and short-term—shapes the incidence of minimum-wage policies.[4]

Second, our paper contributes to the literature on minimum-wage-induced reallocation by documenting how a wage floor reshapes the composition of jobs within a rapidly adjusting spot labor market (Cengiz *et al.* 2019, Dustmann *et al.* 2022). While much of the minimum-wage literature emphasizes net employment effects, an equally important question is *which* jobs contract, *which* expand, and how employment is reallocated across locations, occupations, and job types. Using a distributional DID design on prefecture-by-month-by-wage-bin cells, we quantify missing and excess jobs and decompose these changes in three ways: by prefecture exposure to the minimum wage hike (measured by the Kaitz index), by occupation, and by posting-based job characteristics. The results reveal systematic reallocation patterns: employment losses are concentrated in heavily exposed regions and low-wage occupations, while the composition of surviving postings shifts toward certain job types.

In this reallocation perspective, posting characteristics such as transportation reimbursement (non-wage amenities) and description-based targeting (gender and skill) serve as markers of job

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[3]A large literature examines the employment effects of minimum wages, documenting substantial heterogeneity across settings, demographic groups, and sectors. While many studies focus on specific age groups, such as teenagers or young workers (Card 1992, Neumark and Wascher 1992, Allegretto *et al.* 2011, Neumark *et al.* 2014, Allegretto *et al.* 2017), others examine employment effects within particular industries, most notably low-wage sectors such as restaurants (Katz and Krueger 1992, Card and Krueger 1994, Dube *et al.* 2010). More recent work emphasizes cases in which employment effects are small or statistically insignificant (Clemens 2021, Manning 2021, Neumark and Shirley 2022). Methodological debates and evidence on sectoral heterogeneity and spillovers are surveyed in Dube and Lindner (2024).

[4]Related platform-based studies examine minimum-wage effects on vacancies rather than employment outcomes, including evidence from online and flexible labor platforms (Adams-Prassl *et al.* 2020, Melo *et al.* 2025).

type rather than merely adjustment margins.[5] We show that minimum wage hikes are followed by compositional shifts away from low-amenity postings and toward postings targeting more experienced workers, indicating that reallocation occurs not only across wage bins but also across economically meaningful job types. These results complement findings on reallocation in standard employment settings (Dustmann *et al.* 2022) by highlighting that, in on-site spot markets, reallocation occurs more rapidly through the entry and exit of narrowly defined postings.

Third, we provide evidence on the very short-run *speed* and *composition* of job losses and job creation following minimum wage hikes. Building on the distributional approach of Cengiz *et al.* (2019) and the wage-bin design in Dustmann *et al.* (2022), we track month-by-month (and, in robustness checks, higher-frequency) movements in missing and excess jobs around the new wage floor.[6] This design allows us to identify which postings disappear immediately below the new minimum, which reappear just above it, and how these patterns vary across regions, occupations, and advertised job characteristics.

Studying the short run is policy-relevant because firms may face liquidity constraints and adjustment frictions at the time of implementation; consequently, immediate job destruction may reflect temporary financial stress rather than long-run inefficiency (Doh *et al.* 2024). Despite the significance of these immediate responses, there is limited evidence on firm adjustments at daily, weekly, or monthly frequencies, with only a few exceptions (Sabia 2009a,b, Brochu *et al.* 2025, Melo *et al.* 2025). By revealing the near-immediate sorting of postings around the new wage floor—where compliance is rapid and spillovers to higher wage bins are limited—we clarify how minimum wage policy reshapes the spot job distribution in the months following a reform. This perspective complements the long-run literature on capital–labor substitution, firm exit, and reallocation (Neumark and Wascher 2008, Dustmann *et al.* 2022) by identifying the initial margins and job types through which longer-run adjustments unfold.

The rest of the paper is organized as follows. Section 2 outlines the spot-work context and the Japanese minimum-wage system. Section 3 introduces the data and presents motivating patterns. Section 4 describes the identification strategy. Section 5 reports the main results. Section 6 examines heterogeneity across prefectures, occupations, and posting attributes. Section 7 concludes.

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[5]Recently, a growing body of literature has investigated various margins of adjustment for employers, such as price pass-through (Aaronson and French 2007, Harasztosi and Lindner 2019, Leung 2021), reductions in non-wage compensation (Simon and Kaestner 2004, Clemens *et al.* 2018), decreases in firm revenue (Draca *et al.* 2011, Bell and Machin 2018), and firm entry and exit (Draca *et al.* 2011, Mayneris *et al.* 2018, Aaronson *et al.* 2018, Harasztosi and Lindner 2019, Luca and Luca 2019, Dustmann *et al.* 2022, Chava *et al.* 2023). See Clemens (2021) and Dube and Lindner (2024) for comprehensive reviews. Job advertisement content has also been shown to shape labor-market segmentation and self-selection: titles and descriptions predict wages and applications (Marinescu and Wolthoff 2020) and often carry explicit or implicit gender or skill signals (Brenčič 2010, Kuhn and Shen 2013, Kuhn *et al.* 2020, Helleseter *et al.* 2020).

[6]Related applications of this wage-bin approach include Gopalan *et al.* (2021) and Brochu *et al.* (2025).

2 Background

2.1 Non-regular Work and Spot Work in Japan

Figure 1 shows that both non-regular work and the importance of spot labor markets have grown substantially in Japan between 2013 and 2024. Panel (a) documents the rising prevalence of non-regular workers. Using platform data, Panel (b) shows that spot-work hiring spans a wide range of job categories, with Entertainment, Food Industry, and Office Work accounting for large shares, and Event Staff, Light Work, and Professional jobs also contributing meaningfully. Together, these patterns suggest a growing reliance on spot-work platforms for flexible employment opportunities and broader acceptance of on-demand work arrangements in Japan.
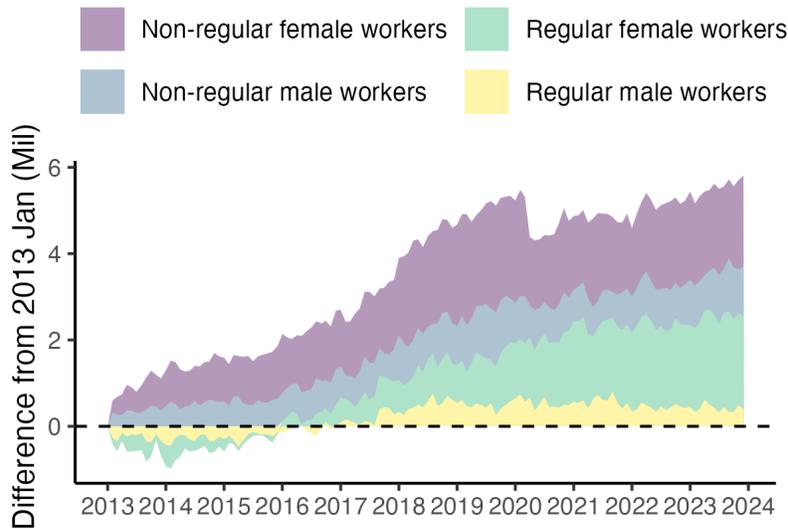
2.2 Spot Work Platform

We use the term "spot work platform" to refer to the non-regular labor market provided by Timee.[7] Timee is the leading online platform in Japan's spot-work market, given its scale relative to comparable platforms.[8] As of 2024, more than 7 million workers were registered on the platform—more than 10% of Japan's labor force of roughly 70 million—illustrating its significant reach in the country's labor market. The platform connects businesses with temporary workers for short-term jobs through a mobile app.

Labor Demand On the labor-demand side, businesses must first undergo a screening and verification process before being allowed to post jobs. Once registered, they can easily create postings through a user-friendly interface by specifying job details such as tasks, wages, and shift times. The platform makes these listings immediately visible to workers and facilitates the matching and contracting process.

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[7]According to the Japan Spot Work Association (JASWA), "spot work" refers to employment contracts for short-hour, one-off work engagements that are facilitated through digital platforms. These platforms serve as intermediaries that match job offers with available workers—often on a same-day or near-term basis—and rely on digital technologies to streamline matching, contracting, and compensation. JASWA plays a central role in promoting sound practices in the sector by issuing guidelines for legal compliance, offering interpretations of labor laws, and supporting proper labor management. In particular, it emphasizes the use of digital infrastructure to reduce the administrative burden traditionally associated with employment, including time tracking, wage calculation, and performance evaluation. This description draws on information provided by the association's official overview and published guidelines (https://www.jaswa.or.jp/about/; https://jaswa.or.jp/pdf/spotwork_gudeline_v2.pdf).

[8]As of 2024, major spot-work platforms exhibit the following scale: Timee (approximately 230,000 registered establishments; PR TIMES https://prtimes.jp/main/html/rd/p/000000222.000036375.html), Sharefull (approximately 55,000 corporate registrations; Sharefull Info https://sharefull.com/information/6602/), and Mercari Hallo (approximately 50,000 registered stores; HRog https://hrog.net/knowledge/122159/).

(a) Hires, Labor force survey
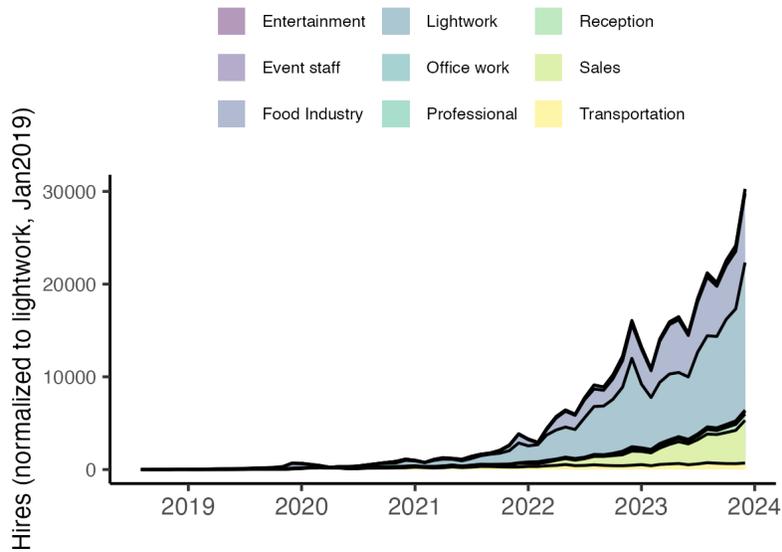
(b) Hires, Spot work platform

Figure 1: Employment Changes: National vs. Spot Work Platform

Note: Panel (a) depicts the time series of job changes in Japan, standardized to January 2013. The colored areas indicate changes within specific worker groups. This graph is based on data from the Economic Survey by the Japanese Cabinet Office (see details at https://www5.cao.go.jp/keizai3/2024/0802wp-keizai/setsumei-e2024.pdf, accessed September 19, 2024). Panel (b) reports the platform-side hires by job category over time; the exact unit and normalization follow the axis label in the figure panel. See the survey of Miyamoto (2025) for an overview of labor markets in Japan.

Labor Supply On the labor-supply side, the primary users of the platform are individuals seeking flexible, task-based employment rather than long-term positions, while establishments

use the platform to seek short-term workers. Workers can register on the platform for free and immediately gain access to job offers from various companies, with the platform streamlining the process of matching workers with available shifts across industries such as food, retail, and logistics, without the need for a traditional hiring process such as formal applications, interviews, or long-term employment contracts.

Certain qualifications (nurses, caregivers, childcare workers, etc.) are required for some jobs, but these jobs are a minority of the total, and most are jobs that any worker can do. Workers in this market can apply for jobs and perform their duties without having any special human capital. Therefore, we treat this labor market as one requiring minimal skills.

Platform Objective The platform's objective is to maximize revenue from successful matches between workers and businesses. Unlike digital platforms that rely on advertising, registration fees, or subscriptions, Timee does not charge either side for access. Instead, it collects a 30% commission on each contracted payment between a business and a worker. Payments are remitted only when the worker actually completes the shift, so platform revenue is tied to realized matches rather than postings alone. This design aligns the platform's incentives with the volume of completed matches. This arrangement benefits both companies and workers by offering flexible, short-term employment opportunities while avoiding the formalities and commitments of long-term contracts.

At the same time, the platform allows workers to take repeated jobs from the same establishment, and businesses may extend longer-term employment offers if they wish. In this sense, the platform does not preclude transitions into regular part-time employment and can serve as a trial channel through which businesses and workers explore longer-term arrangements. Its simplicity and immediacy are especially attractive to workers seeking to fit employment around their schedules.

Transfers from Companies to Workers On this platform, workers may receive not only wage income based on hourly pay and hours worked but also transportation reimbursement. This reimbursement is intended to cover travel to and from the workplace, but firms are not legally required to provide it. Because the amount must be set when a job is posted, firms often lack precise information about workers' realized commuting costs, and workers are not required to reconcile differences ex post. Transportation reimbursement is therefore a salient non-wage margin along which firms could, in principle, adjust to higher labor costs following a minimum-wage increase.
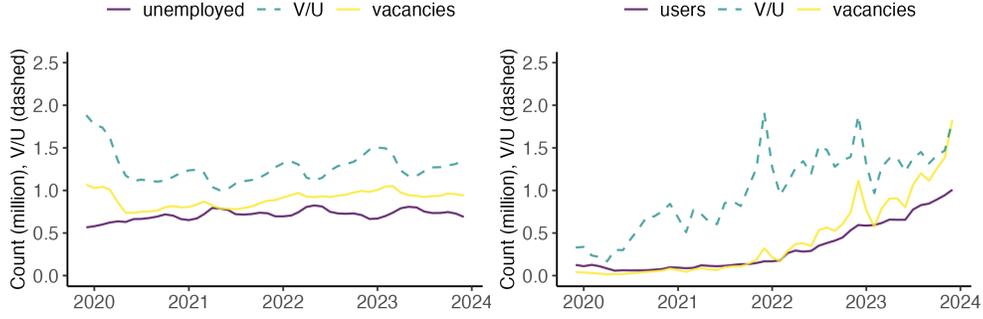
(a) User U, Vacancy V, and Tightness $(\frac{V}{U})$
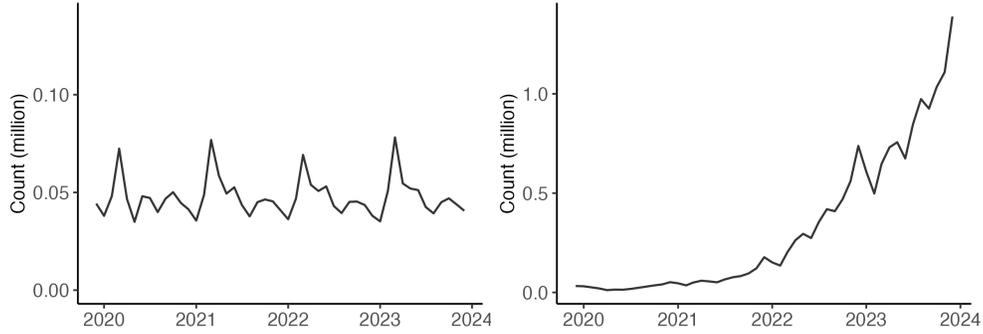
(b) Hire H
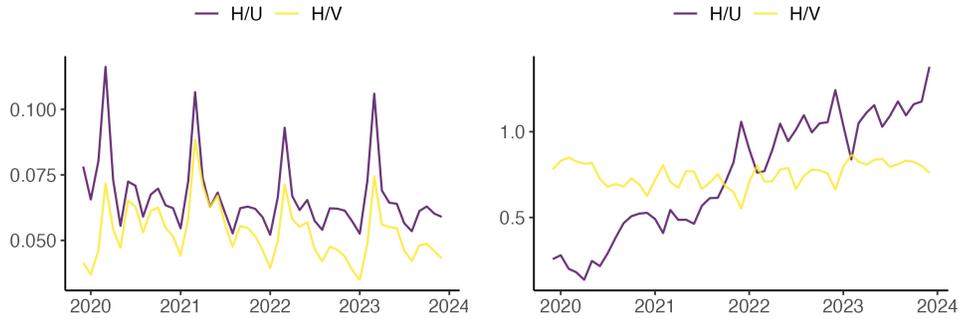
(c) Job and Worker Finding Rate $(\frac{H}{U}, \frac{H}{V})$

Figure 2: Trends of Macro Variables: Hello Work Part-time (left) vs. Platform (right), 2019–2023

Note: Details are in Kanayama and Otani (2024). For the left panels, we use the Report on Employment Service (*Shokugyo Antei Gyomu Tokei*) for the month-level aggregate data from December 2019 to December 2023 to capture trends in matching between unemployed workers and vacancies via a conventional platform. These datasets include the number of job openings, job seekers, and successful job placements, primarily sourced from the Ministry of Health, Labour and Welfare (MHLW) of Japan, which publishes monthly reports and statistical data on the Public Employment Security Office, commonly known as Hello Work. Hello Work is a government-operated institution in Japan and a conventional platform that provides job seekers with employment counseling, job placement services, and vocational training, playing a critical role in Japan's labor market.

2.3 Institutional vs. Platform-Based Matching: Hello Work and Spot Work

Figure 2 provides a comparative analysis of labor market dynamics between the Hello Work public employment platform (left panel) and a private spot work platform (right panel) from December 2019 to December 2023. Hello Work is a government-operated institution in Japan and a conventional platform that provides job seekers with part-time and full-time employment counseling, job placement services, and vocational training, playing a critical role in Japan's labor market.

First, in Figure 2, we discuss the left panels for Hello Work, which cover part-time workers and jobs.[9] In Panel (a), the number of unemployed individuals remains stable over the observed period, hovering around 0.8 million. Vacancies and tightness (V/U) show stable trends. Panel (b) shows that the hiring count through Hello Work remains low, with a small oscillating pattern throughout the period. This stable but limited hiring activity suggests that the platform may face constraints or inefficiencies in increasing the job match rate for part-time workers. In Panel (c), the job and worker finding rates (H/U and H/V) exhibit a gradual decline over time, suggesting reduced effectiveness in matching job seekers with available vacancies via Hello Work. Here, H/V is interpreted as hires per vacancy posting in aggregate data. This decline might reflect a growing preference for alternative job search methods such as the spot-work platform.

In contrast, the right panels in Figure 2 reflect the dynamics of the private platform, where a distinctive pattern emerges. Unlike Hello Work's part-time flow from unemployed to employed, workers on this platform do not automatically leave after securing a match, which retains a larger pool of active users. The number of registered users increases gradually, while the number of vacancies rises sharply, especially post-2022, leading to a notable rise in labor market tightness (V/U). This reflects an expanding demand for spot employment opportunities. The hiring count in Panel (b) shows a rapid upward trajectory starting around 2022, underscoring a marked increase in successful matches.

In Figure 2, Panel (c) illustrates the job and worker finding rates (H/U and H/V), highlighting significant differences compared to Hello Work. The job finding rate (H/U) continues to rise, reaching values close to 1.3, indicating that each worker matches with multiple vacancies per month. On the other hand, the worker finding ratio (H/V) remains stable around 0.8. Because V is the number of postings, this reflects hires per posting rather than a position-level fill probability. This pattern demonstrates the platform's increasing efficiency in matching workers to vacancies. It indicates a maturing spot labor market that still exhibits strong growth.

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[9]For full-time jobs on the public and private job matching platforms, Otani (2025) estimates trends of matching efficiency and elasticity between 2014 and 2024. Otani (2024) also provides the long-term trends of full-time and part-time job matching in Hello Work between 1972 and 2024.
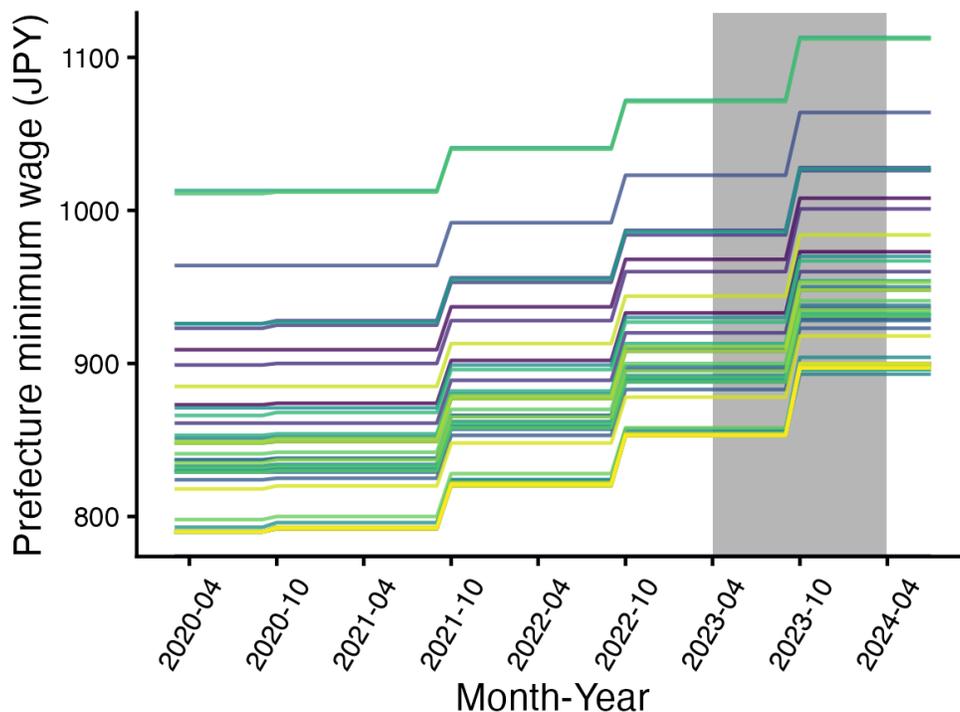
Figure 3: Dynamics of Regional Minimum Wage Changes

Note: The figure plots monthly regional minimum wages across prefectures. Until 2022, each prefecture was assigned to one of four categories based on workers' average income, and each category was given a guideline minimum wage. Many prefectures set their regional minimum wage close to that guideline. The shaded period is our focus.

2.4 Minimum Wages in Japan

This section introduces the regional minimum wages set in each of the 47 prefectures in Japan, which we define as "regional minimum wages." The introduction of regional minimum wages follows the framework described by Tamada (2009), Tamada (2011), and Kawaguchi and Mori (2021). The Ministry of Health, Labor and Welfare established the Central Minimum Wage Council, which operates under the tripartite principle and consists of representatives from labor, management, and public interest groups.

The Council divides the 47 prefectures into three regional blocks based on regional welfare levels. It sets guidelines ("Meyasu") for the recommended increase in the hourly minimum wage for each group.[10] These recommendations primarily reference the average annual wage increase rate for workers at workplaces with fewer than 30 regular employees, as reported in the Minimum Wage Survey. The Council typically holds its first meeting in June and finalizes the guidelines by the end of July. Based on these guidelines, the Local Minimum Wage Councils in each prefecture determine the specific minimum wage increases in August, with the revised wages coming into

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[10]The Council had divided the 47 prefectures into four regional blocks before 2022.

effect during the first week of October. This two-stage process has been implemented annually since 1978, with a few exceptions, ensuring regular increases in the local minimum wage.

The Regional Minimum Wage Act applies to nearly all workers in Japan, regardless of industry, age, or gender. However, exceptions exist where the director of the labor bureau in each prefecture deems it appropriate to pay less than the regional minimum wage.[11]

Figure 3 illustrates the changes in regional minimum wages over time, showing minimum wage variations across prefectures from 2020 to 2023. In October 2020, due to the negative employment shock caused by COVID-19, the minimum wage increased only slightly, by an average of 0.2%. However, from 2021 onwards, the minimum wage increased consistently each year, with an average increase of 4.7% in 2023. Notably, except for the period of economic stagnation during the COVID-19 pandemic, substantial minimum wage increases have occurred in October across all prefectures.

While minimum wage hikes in Japan are effective at raising wages (Aoyagi *et al.* 2016), they are also associated with negative employment effects. Numerous studies have examined the impact of minimum wage policies on employment in Japan (Yugami 2005, Tachibanaki and Urakawa 2006, Kawaguchi and Mori 2009, Higuchi 2013, Kambayashi *et al.* 2013, Akesaka *et al.* 2017, Okudaira *et al.* 2019, Izumi *et al.* 2020, Kawaguchi and Mori 2021, Izumi *et al.* 2022). Many of these studies report negative employment effects for certain demographic groups (Kawaguchi and Yamada 2007, Kawaguchi and Mori 2009, Kambayashi *et al.* 2013, Akesaka *et al.* 2017, Okudaira *et al.* 2019, Kawaguchi and Mori 2021, Izumi *et al.* 2022, Mori and Okudaira 2025). For example, Kawaguchi and Mori (2021) examines the employment effects of minimum wages by leveraging the 2007 reform of Japan's Minimum Wage Act.[12] They find that the employment elasticity of the minimum wage for less-educated young men is -1.2. This implies that although the minimum wage in Japan affects only certain demographic groups, the magnitude of its impact can be larger than what is observed in other countries.

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[11]Examples where it is considered appropriate to pay less than the minimum wage include: 1) individuals with significant mental or physical disabilities, 2) employees in trial periods, 3) participants in certified basic vocational training programs, 4) workers engaged in light work, and 5) those performing intermittent work.

[12]This reform mandated that monthly minimum wage earnings must meet or exceed monthly public assistance levels. Prefectures where minimum wages fell below public assistance thresholds experienced substantial increases. Using the difference between public assistance levels and monthly minimum wage earnings in 2006 as an instrumental variable, they estimate the causal effect of the minimum wage on employment during the years following the reform. This approach has since been applied in several studies (Okudaira *et al.* 2019, Mizushima and Noguchi 2021, Yamagishi 2021, Izumi *et al.* 2022, Mori and Okudaira 2025, Yamanouchi *et al.* 2025).

3 Data

3.1 Data Source and Construction

We base our analysis on contract- and posting-level records from Timee, a privately operated Japanese platform that matches spot workers and employers. As the oldest and largest player in Japan's spot labor market, Timee provides highly representative data for this setting. We focus on April 2023 through March 2024 in order to study the October 2023 minimum-wage revision, the most recent reform for which a full post-treatment window is available in our data.

We aggregate the contract-level data to calculate key metrics at the prefecture-month-wage-bin and prefecture-month levels, including the number of job postings (vacancies, measured at the posting level) and matches (employment). Unlike Dustmann *et al.* (2022), there is no difference between actual and contractual working hours, which is an advantage of our data. The dataset also includes not only detailed employer characteristics (industry, occupation, and location—prefecture, municipality, longitude, latitude) but also precise timestamps for vacancy posting and filling (and shift start), as well as the full job-description text. Measuring vacancy duration as the elapsed time a posting remains advertised—from appearance on the platform until it is filled, rather than until work begins—has the advantage of focusing on the time to find candidates, as in Bassier *et al.* (2025). Job descriptions set by employers provide rich textual signals—implicitly conveying skill requirements (e.g., "experience required," heavy-lifting, certification cues), task intensity and shift constraints, and preferred worker attributes. These signals influence applicant self-selection, as shown in Kuhn *et al.* (2020), even when they are not binding screening criteria. This rich dataset enables comprehensive heterogeneity analysis of how minimum wage changes impact the short-term dynamics of spot labor markets.

Table 1 reports summary statistics at the contract, prefecture–month–wage-bin, and prefecture–month levels. At the contract level, hourly pay averages 1,086 JPY (median 1,100), with typical shifts of 4.79 hours. Employers commonly offer transportation reimbursement (mean 405 JPY; median 500), but the wide range underscores substantial heterogeneity by location and policy. About 81% of contracts include any reimbursement. For timing, vacancy duration is measured as the elapsed time from posting availability to fill; the posting-to-start lag (start lead time) is the elapsed time from posting availability to the shift start. Among fully filled vacancies, the mean duration is 3.28 days and the mean posting-to-start lag is 9.07 days. Job-description targeting flags indicate that beginner-targeted, experienced-worker-targeted, female-targeted, and male-targeted postings account for roughly 1%, 30%, 3%, and 3% of contracts, respectively.

At the prefecture–month–wage-bin level, employment and vacancies average 191.50 and 235.06 per bin, respectively; the mean transportation reimbursement is 111.72 JPY per contract in the cell, with a 21% reimbursement incidence and about 1% staff-meal incidence. At the prefecture–month

Table 1: Summary Statistics

(a) Contract Level

	N	mean	median	sd	min	max
Hourly wage	8673942	1086.01	1100.00	123.12	853.00	2000.00
Working hour	8673942	4.79	4.50	1.84	1.00	16.00
Num of positions	8673942	1.67	1.00	4.60	1.00	8961.00
Transportation reimbursement (JPY)	8673942	404.65	500.00	255.19	0.00	1500.00
Transportation reimbursement dummy	8673942	0.81	1.00	0.39	0.00	1.00
Vacancy duration (Day)	6710362	3.26	0.25	6.73	0.00	99.34
Posting-to-start lag (Day)	6710362	9.04	5.85	9.69	0.00	99.99
Beginer-targeted job	8673942	0.01	0.00	0.10	0.00	1.00
Experienced-targeted job	8673942	0.30	0.00	0.46	0.00	1.00
Female-targeted job	8673942	0.03	0.00	0.17	0.00	1.00
Male-targeted job	8673942	0.03	0.00	0.17	0.00	1.00

(b) Prefecture-month-wage-bin Level

	N	mean	median	sd	min	max
Employment	61584	191.45	0.00	1789.29	0.00	83124.00
Vacancy	61584	234.98	0.00	2171.39	0.00	111887.00
Mean transportation reimbursement (JPY)	61584	111.73	0.00	222.09	0.00	1500.00
Mean transportation reimbursement dummy	61584	0.21	0.00	0.38	0.00	1.00

(c) Prefecture-month Level

	N	mean	median	sd	min	max
Employment	564	20904.55	7150.00	34822.35	505.00	262937.00
Vacancy	564	25657.84	9240.50	42700.28	633.00	350673.00

Note: This table reports the mean, median, standard deviation, and minimum and maximum values of each outcome variable during the period of analysis, April 2023 through March 2024. 150 JPY is about 1 USD at the exchange rate in 2025. Vacancy duration and posting-to-start lag are defined only for vacancies that are filled.

level, spot employment averages 20,910 and vacancies 25,666, revealing substantial time-series and cross-prefecture variation and a persistent gap consistent with unfilled positions.

3.2   Preliminary Evidence Before and After the Minimum Wage Change

Leveraging the strengths of granular spot work platform data, we focus on Tokyo to illustrate how the minimum wage adjustment reshaped employment patterns, allowing for a detailed examination of wage shifts while revealing the stability of working hours and transportation reimbursement.

The Hourly Wage Distribution   Figure 4 illustrates the transition in employment distribution across hourly wage bins at both the daily and weekly levels before and after the minimum wage adjustment. The ability to observe such granular, high-frequency adjustments is a unique strength of our spot labor platform data, enabling us to capture firms' real-time responses to policy changes with day-to-day resolution. Panel (a) shows the daily evolution of the employment distribution in Tokyo, revealing a sharp and immediate decline in employment below the new minimum wage threshold (1,113 JPY) just one day after the policy took effect. Jobs previously offered at lower wages either disappeared or were adjusted upward, with a clear clustering of employment around the new minimum wage floor. Complementarily, Panel (b) aggregates these patterns at the weekly level, confirming the persistence of this shift over the course of a month. Panel (c) shows that the effect persists over a six-month period.
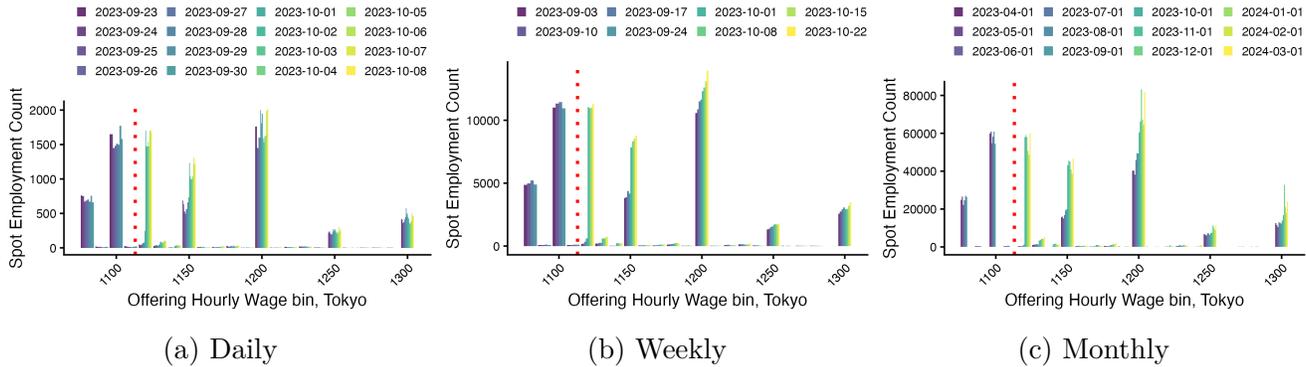

(a) Daily   (b) Weekly   (c) Monthly

Figure 4: Transition of Employment Distribution by Week and Day Before and After the Minimum Wage Change

Note: The x-axis represents the wage bins, while the y-axis indicates the total count of spot employment. The colored bars represent different months, allowing for a clear comparison of employment pattern shifts over time.

Adjustments in Working Hours and Transportation Reimbursement   Panel (a) in Figure 5 shows that the employment distribution across working-hour bins remains broadly stable.

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Common shift lengths, such as four and eight hours, remain prominent, with at most modest changes in shorter shifts. Panel (b) shows a similarly stable distribution of transportation reimbursement, concentrated in the 500 JPY bin. Overall, the descriptive evidence suggests that firms mainly adjusted posted wages rather than other job attributes.

(a) Posted Working Hour

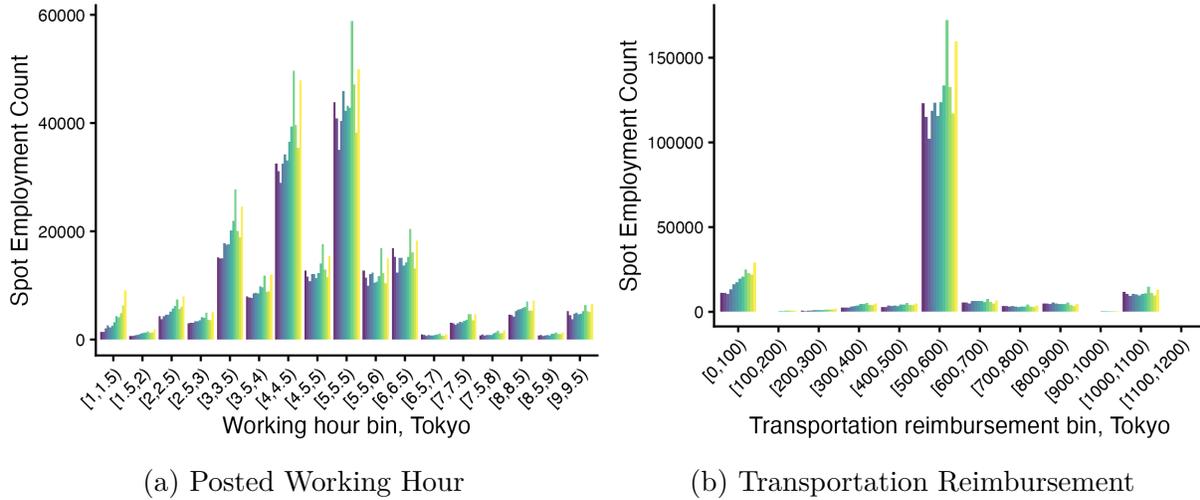(b) Transportation Reimbursement

Figure 5: Transition of Employment Distribution Before and After the Minimum Wage Change

Note: The x-axis represents working-hour bins (Panel (a)) and transportation reimbursement bins (Panel (b)), while the y-axis indicates the total count of spot employment. The colored bars represent different months, allowing for a clear comparison of employment pattern shifts over time.

A natural question is whether the wage increase resulting from the minimum wage adjustment led to adjustments in working hours and transportation reimbursement, or whether firms primarily confined their response to wage adjustments. Figure 6 demonstrates that the minimum wage adjustment primarily impacted the distribution of employment across wage bins, while working hours and transportation reimbursement remained largely unaffected. In Panel (a), employment previously concentrated in wage bins below the new minimum wage threshold shifted upward, clustering around the new minimum wage level, indicating compliance-driven wage adjustments. However, changes in the distribution of working hours are minimal, suggesting that firms maintained their established job structures despite increased labor costs. Similarly, transportation reimbursement patterns in Panel (b) exhibit stability, with no significant redistribution, reinforcing the notion that firms adjusted wages in response to the policy change without altering other compensation components. This pattern is further supported by Panel (c), which shows that even among jobs with short working hours—where transportation costs make up a larger share of compensation—there was little adjustment in reimbursement levels. These patterns highlight that the primary response to the minimum wage hike was wage compliance, rather than broader changes in job design or additional benefits.

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(a) Working Hour and Wage (b) Transportation Reimburse-(c) Transportation Reimbursement
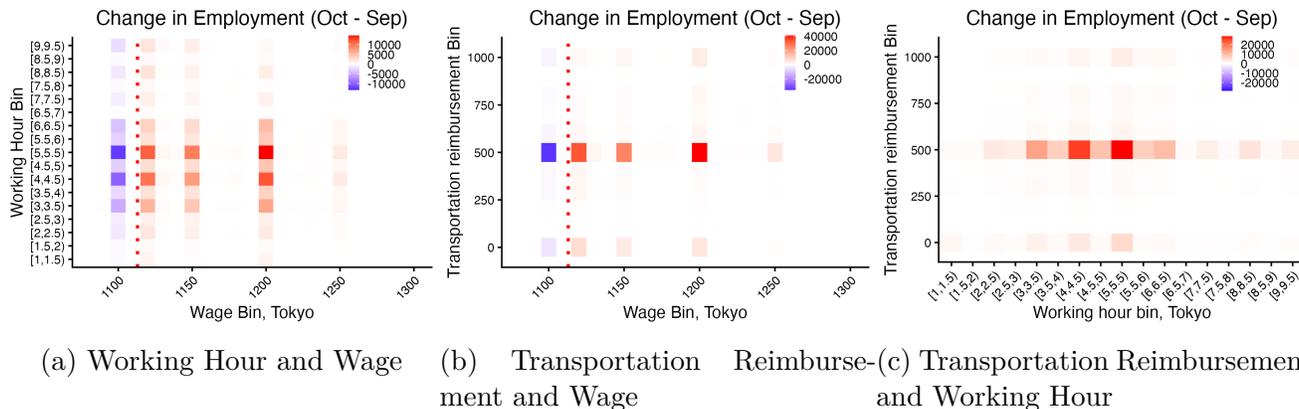ment and Wage and Working Hour

Figure 6: Change in Employment Distribution Before and After the Minimum Wage Change

Note: The color intensity reflects the change in spot employment between September and October, with red indicating an increase and blue indicating a decrease. The red dashed line denotes Tokyo's 2023 October minimum wage, i.e., 1,113 JPY.

Summary These illustrative data patterns motivate us to move beyond descriptive analysis and investigate the causal impact of the minimum wage adjustment on employment outcomes. While the observed shifts in wage distribution suggest strong compliance effects and limited adjustment on other job attributes, such as working hours and transportation reimbursement, these patterns alone do not establish causality. In particular, they cannot account for concurrent platform growth or heterogeneous trends across prefectures, which may confound observed changes. To rigorously evaluate whether and to what extent the policy change affected employment dynamics—particularly in terms of spot hiring behavior—we turn to a difference-in-differences (DID) framework. This approach allows us to isolate the effect of the minimum wage hike by comparing changes in outcomes between treated and control groups over time, accounting for underlying trends and potential confounders.

4 Empirical Strategy

4.1 Prefecture-month–wage-bin DID Estimates

The key challenge in estimating the effect of a minimum wage increase on the outcome is that we cannot observe the counterfactual wage distribution in the absence of the policy shock. To address this issue, following Dustmann *et al.* (2022), we employ a difference-in-differences strategy that compares outcomes before and after the minimum wage increase between observations near the minimum wage threshold and those in the upper part of the wage distribution, which are unlikely to be affected by the policy change.

Specifically, we first decompose the wage distribution in each prefecture into wage bins, where each bin j has a width of 10 yen. We then partition each wage bin j in prefecture p into mutually exclusive groups $E_j(p) = e$ for $e \in \{-1, \ldots, 3, \infty\}$, based on its relative distance from the increased minimum wage MW_p[13]. Specifically, if wage bin j in prefecture p falls within the interval $[MW_p + e \times 100, MW_p + e \times 100 + 99]$, it is assigned to group e. We define the threshold $\bar{W}_p = MW_p + 400$, and assign $E_{j(p)} = \infty$ if wage bin j falls within $[\bar{W}_p, \infty]$. We assume that wage bins in group ∞ are unaffected by the minimum wage increase, an assumption we refer to as the limited spillover effect assumption. Under this assumption, we refer to group ∞ as the control group. Finally, for each group e, we compare the evolution of the outcome between group e and group ∞ before and after the minimum wage increase.

We implement this DID identification strategy by running the following regression:

$$\frac{Y_{j(p),t}}{N_{p,t}} = \sum_{l \neq -1} \sum_{e \neq \infty} \mu_{l,e} \mathbf{1}\{t - 7 = l\} \mathbf{1}\{E_{j(p)} = e\} + \alpha_e + \lambda_t + \epsilon_{j,t}, \tag{1}$$

where $t \in \{1, \ldots, 12\}$ denotes the calendar month, and $l \in \{-6, \ldots, 5\}$ denotes the relative time period before or after the minimum wage increase at time $t = 7$.[14] $Y_{j(p),t}$ is employment for each bin j in prefecture p at time t, $N_{p,t}$ is employment in prefecture p at time t, α_e is the group fixed effect, and λ_t is the time fixed effect.[15] In additional analyses, we replace the left-hand-side outcome with corresponding measures such as vacancies or total hours. Note that this regression is saturated and each coefficient $\mu_{l,e}$ is the difference-in-differences estimand:

$$\mu_{l,e} = E\left[\frac{Y_{j(p),7+l}}{N_{p,7+l}} - \frac{Y_{j(p),6}}{N_{p,6}} \,\bigg|\, E_{j(p)} = e\right] - E\left[\frac{Y_{j(p),7+l}}{N_{p,7+l}} - \frac{Y_{j(p),6}}{N_{p,6}} \,\bigg|\, E_{j(p)} = \infty\right],$$

where the pre-treatment period is $t = 6$ and the control group is group ∞.

Under the DID identification strategy, each coefficient $\mu_{l,e}$ captures the dynamic effect of the minimum wage increase on the outcome in group e at a given relative period l. Formally, we assume the three identifying assumptions below:

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[13]In all prefectures, the increase in the minimum wage from 2022 to 2023 is less than 100 JPY. Therefore, any nominal wage bin that is 200 JPY or more below the new 2023 minimum wage would necessarily lie at least 100 JPY below the pre-revision (2022) minimum wage. In our setting, such bins are essentially never observed in posted wages and, in any case, lie outside our analysis window. Accordingly, we define "missing jobs" using the 100 JPY window immediately below the new minimum wage (the $e = -1$ group) and do not include bins further below (which would correspond to $e < -1$).

[14]$t = 1$ corresponds to April 2023, and $t = 12$ corresponds to March 2024.

[15]The notation $j(p)$ indicates that the wage bin j belongs to prefecture p. We adopt this notation because the regression in (1) exploits variation only at the group e and time t levels.

Limited Spillover Effect Assumption This assumption requires that the effect of the minimum wage increase on the outcome vanishes at a certain point $\bar{W}_p$ in the wage distribution in each prefecture p. It ensures that group ∞ (the upper part of the wage distribution) serves as a valid control group. We will assess the validity of this assumption by examining the impact of the minimum wage increase on groups near $\bar{W}_p$ such as $e = 2, 3$.[16]

Parallel Trends Assumption This assumption requires that for each group e, the outcome evolves similarly over time between group e and the control group ∞. This assumption is also not directly testable, but we assess the plausibility of this assumption by checking the pre-trend differences for the outcome. To assess the sensitivity of our estimates to potential heterogeneity in underlying trends between treated and control wage bins, we additionally estimate (i) an augmented version of our main specification that includes wage-bin fixed effects and wage-bin-by-time fixed effects, and (ii) a difference-in-difference-in-differences specification that removes wage-bin-specific seasonal trends, and report the corresponding results in the Appendix A.4.

No Anticipation Assumption This assumption rules out the anticipation effect of the minimum wage increase on the outcome at a given period $l = -1$, i.e., before exposure to the policy shock.

4.2 Excess Jobs–Missing Jobs Approach

Next, following Cengiz *et al.* (2019), we estimate the effect of the minimum wage increase on the outcome throughout the wage distribution by aggregating each $\mu_{l,e}$ from the prefecture-month-wage-bin DID estimates. Let K denote the pre-treatment outcome of the treated wage-bin group in September 2023. We denote $\Delta a_l = \frac{\sum_{e=0}^{3} \mu_{l,e}}{K}$ and $\Delta b_l = \frac{\mu_{l,-1}}{K}$ as the excess jobs and the missing jobs at a given relative period l, respectively, which normalizes the excess and missing jobs by the pre-treatment outcome.[17] Let $\Delta a = \frac{1}{6}\sum_{l=0}^{5} \Delta a_l$ and $\Delta b = \frac{1}{6}\sum_{l=0}^{5} \Delta b_l$ denote the total excess jobs and missing jobs after the minimum wage increase, respectively. Also, let $\Delta a_e = \frac{1}{6}\sum_{l=0}^{5} \mu_{l,e}$ and $\Delta b_{-1} = \frac{1}{6}\sum_{l=0}^{5} \mu_{l,-1}$ define the excess jobs and the missing jobs at a given relative group e, respectively. We compute the overall employment effect at a given relative period l, denoted as Δe_l, by summing the excess and missing jobs at that period: $\Delta e_l = \Delta a_l + \Delta b_l$.[18] Furthermore,

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[16]As an additional robustness check, we report in the Appendix A.4 the results obtained when redefining the control group as wage bins at least 300 yen above the new minimum wage ($MW_p + 300$) and, alternatively, at least 500 yen above the new minimum wage ($MW_p + 500$).

[17]In the same spirit, Cengiz *et al.* (2019) use the sample-average employment-to-population ratio in treated states during the year (four quarters) prior to treatment, denoted by $EPOP_{-1}$, as a normalization benchmark.

[18]Following Cengiz *et al.* (2019), we normalize the excess and missing jobs by the pretreatment total employment.

we calculate the total employment effect after the minimum wage increase, denoted as Δe, by summing Δe_l over all relevant periods: $\Delta e = \sum_{l=0}^{5} \Delta e_l$.

5 Results

5.1 Prefecture-month–wage-bin DID Estimates on Employment

We begin by presenting the impact of minimum wage increases on wage bins below and above the minimum wage in the wage distribution. Panel (a) in Figure 7 presents the estimated excess jobs and missing jobs for each month before and after the minimum wage increase. Panel (b) in Figure 7 presents the estimated impact of the minimum wage increase on employment for each group e in the wage distribution, reflecting the cumulative effect across all months following the increase.

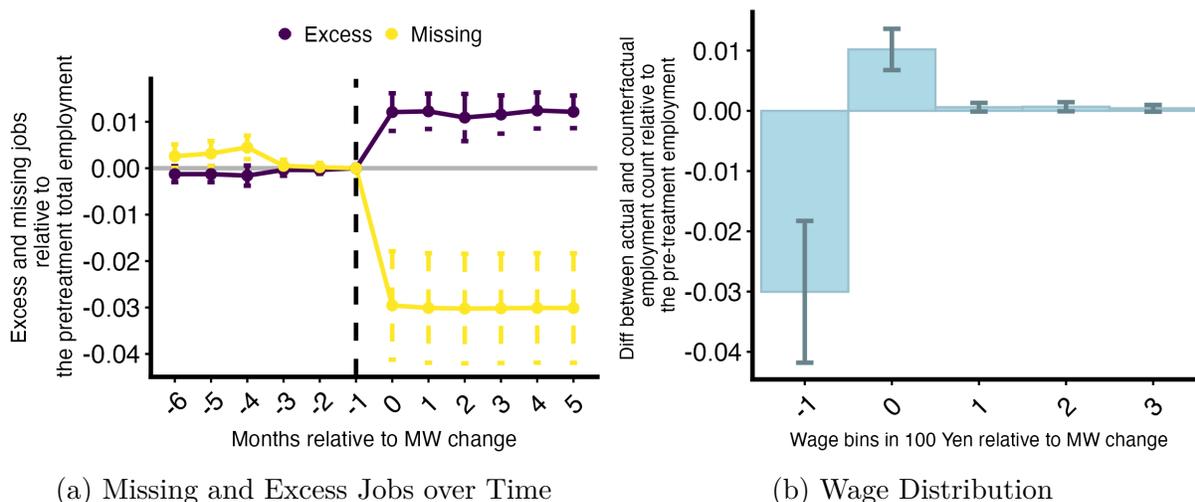(a) Missing and Excess Jobs over Time   (b) Wage Distribution

Figure 7: Impact of Minimum Wages on Employment

Note: Panel (a) shows the estimated excess jobs and missing jobs for each month before and after the minimum wage increase. Panel (b) shows the estimated impact of the minimum wage increase on employment for each group e in the wage distribution.

Three key findings emerge from Figure 7. First, immediately after the minimum wage increase, we observe clear job losses (missing jobs) below the minimum wage and job creation (excess jobs) above it. Second, these missing and excess jobs remain stable across the post-treatment months in our sample. Consistent with the longer-run persistence documented by Cengiz *et al.* (2019), this pattern suggests that distributional effects need not dissipate quickly. Finally, whereas Cengiz *et al.* (2019) find that excess jobs and missing jobs are approximately offsetting in the broader U.S. labor market, our spot-market analysis shows that missing jobs (-3.0%) exceed excess jobs (1.2%).[19]

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[19]The elasticity with respect to minimum wages computed from the formula of Cengiz *et al.* (2019) is -0.388

This pattern is consistent with employers' greater ability to reduce spot hiring in response to wage increases than to adjust standard employment relationships.

Note that Panels (a) and (b) in Figure 7 also confirm the validity of the assumptions underlying our identification strategy. From Panel (a), we observe that the estimated excess jobs and missing jobs are close to zero in the period before the minimum wage increase. This supports the plausibility of the parallel trends assumption. From Panel (b), we observe that excess jobs are hardly generated in the upper distribution, far from the minimum wage. This supports the plausibility of the limited spillover effect assumption.[20]

Shorter Time Window   We exploit the granularity of our platform data to probe the time dimension more directly than in the baseline monthly analysis. Specifically, we re-estimate the wage-bin DID at weekly and daily frequencies for three purposes: (i) to assess robustness to the choice of the time window, (ii) to provide a stricter pre-trend check by examining higher-frequency dynamics immediately before the minimum-wage implementation, and (iii) to measure the speed of adjustment by tracing how quickly missing and excess jobs emerge and stabilize after the policy takes effect. Because spot work can adjust rapidly, these higher-frequency estimates may reveal short-run dynamics that are smoothed out at the monthly level.
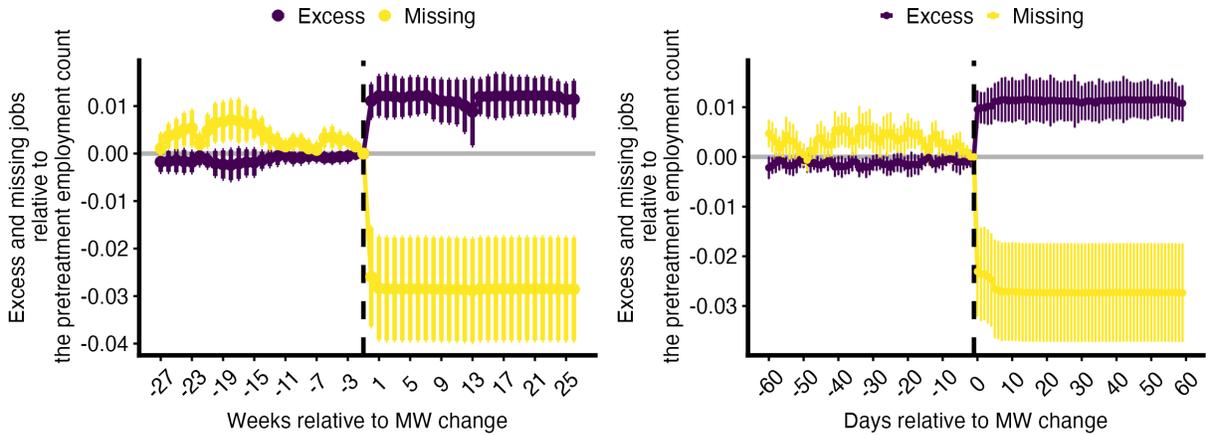
(a) Missing and Excess Jobs over Time per Week (b) Missing and Excess Jobs over Time per Day

Figure 8: Impact of Minimum Wages on Employment

Note: Panel (a) shows the estimated excess jobs and missing jobs for each week before and after the minimum wage increase. Panel (b) shows the estimated excess jobs and missing jobs for each day before and after the minimum wage increase. In Panel (b), we restrict the analysis to the period from 60 days before to 60 days after the treatment in order to focus on changes around the treatment, and report the results.


(0.132), where the parenthetical value is the standard error. See Appendix A.1 for details.

[20]Similar to our findings, Autor *et al.* (2016) and Cengiz *et al.* (2019) also show that the spillover effects of the minimum wage on higher wages are limited.

Panel (a) in Figure 8 reports weekly estimates of missing and excess jobs. The pre-treatment coefficients fluctuate but remain close to zero, reinforcing the plausibility of parallel trends. Immediately after implementation, missing and excess jobs appear and then remain stable, with magnitudes comparable to the baseline monthly estimates in Figure 7. Panel (b) reports daily estimates, restricting attention to a symmetric window of 60 days before and after the policy change to focus on near-term dynamics. As in the weekly analysis, the pre-treatment series stays near zero and the post-treatment effects materialize quickly and persist, indicating rapid adjustment in this market. Overall, the weekly and daily results corroborate the baseline findings, strengthen the pre-trend evidence, and show that most of the adjustment occurs immediately after the minimum-wage change rather than gradually over subsequent months.

5.2 Impact on Spot Labor Demand: Vacancy Counts and Total Working Hours

The employment effects documented above naturally raise the question of whether adjustments to minimum wage increases operate primarily through labor demand or labor supply in spot labor markets. Unlike standard employment settings, spot work allows firms to adjust hiring margins flexibly along multiple dimensions, including the number of vacancies posted and the total hours offered. To disentangle these channels, we separately examine the responses of vacancy counts and total working hours using the same DID framework.
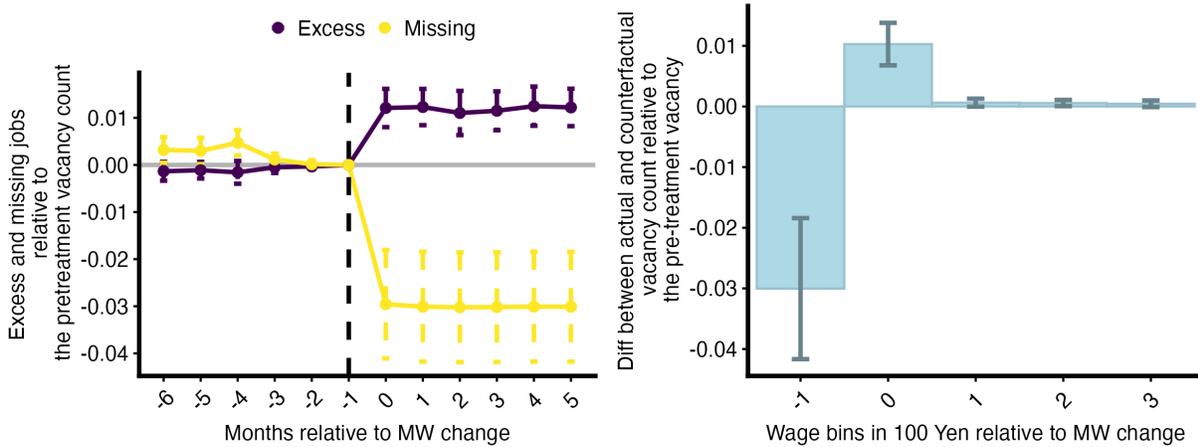
(a) Missing and Excess Posted Vacancies over Time

(b) Wage Distribution

Figure 9: Impact of Minimum Wages on Vacancies

Note: Panel (a) shows the estimated excess and missing posted vacancies for each month before and after the minimum wage increase. Panel (b) shows the estimated impact of the minimum wage increase on vacancies for each group e in the wage distribution.

Vacancy Counts Figure 9 shows that employment changes are largely driven by adjustments in vacancy postings, that is, spot labor demand. Panel (a) documents clear missing posted vacancies below the minimum wage and excess vacancies just above it, with effects that persist over time. Panel (b) confirms that these changes are concentrated around the minimum wage threshold, with little evidence of spillovers to higher wage bins. These patterns indicate that the decline in employment below the minimum wage primarily reflects reduced vacancy creation rather than increased job-filling frictions, while modest gains above the threshold arise from expanded postings at slightly higher wages. Vacancy creation thus emerges as the central margin of adjustment on the demand side.

(a) Missing and Excess Working Hours over Time   (b) Wage Distribution
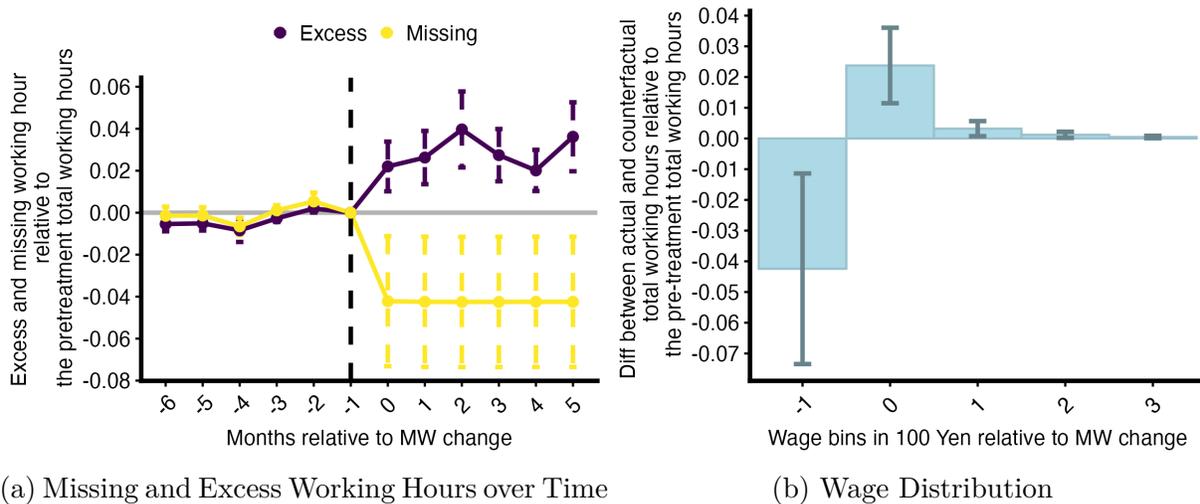
Figure 10: Impact of Minimum Wages on Total Working Hours

Note: Panel (a) shows the estimated excess and missing working hours for each month before and after the minimum wage increase. Panel (b) shows the estimated impact of the minimum wage increase on total working hours for each group e in the wage distribution.

Total Working Hours Figure 10 examines adjustments in total working hours following the minimum wage increase. Panels (a) and (b) show that, unlike vacancy counts, the net change in total working hours is relatively small, as excess and missing working hours largely offset each other in the months after the reform. This pattern implies that while the number of posted vacancies declines, the reduction in aggregate working hours is limited. Taken together with the vacancy results, these findings suggest a modest increase in hours per vacancy: firms post fewer jobs but offer slightly longer hours per posting. Thus, minimum wage increases primarily affect spot labor demand through the extensive margin of vacancy creation, accompanied by a limited compensating adjustment along the intensive margin.[21]

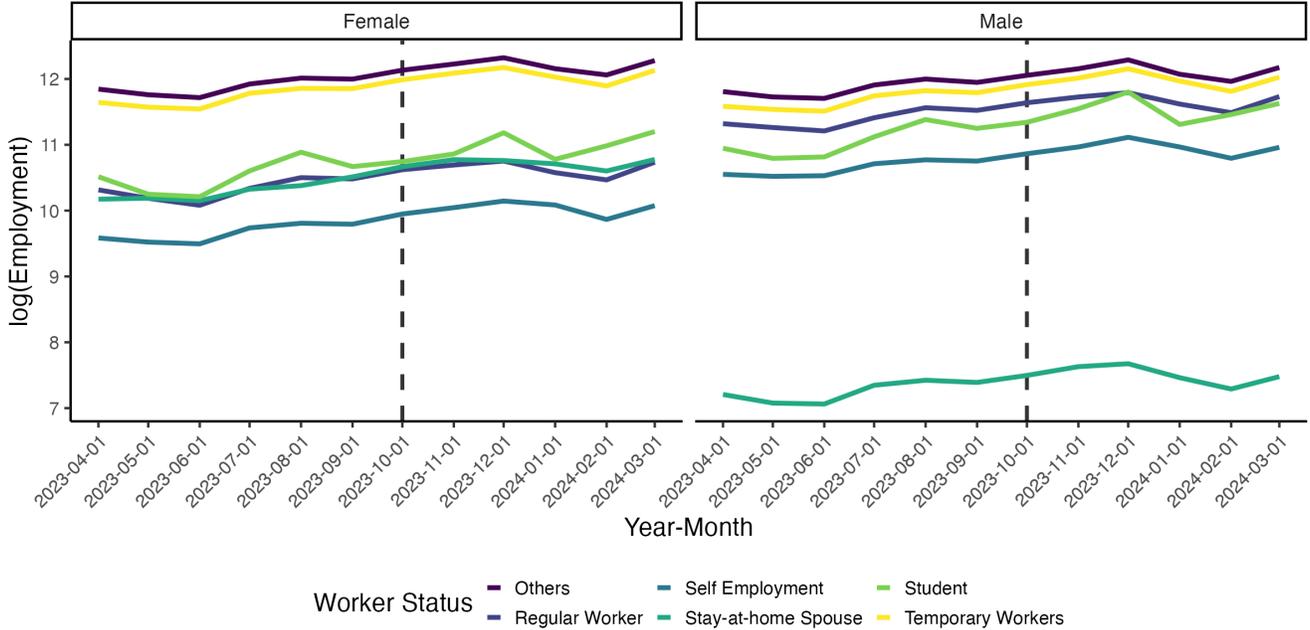[21]In Appendix A.3, we examine the platform revenue.

Figure 11: Employment Dynamics by Worker Status

Note: The figure plots the temporal dynamics of the log number of workers by worker status.

5.3 Discussion of the Impact on Spot Labor Supply

A natural concern is whether minimum wage increases affect spot labor supply (Godøy *et al.* 2024), particularly in the Japanese context where labor supply often exhibits seasonal adjustment toward the end of the fiscal year (Akabayashi 2006, Takahashi *et al.* 2009, Bessho and Hayashi 2014, Yokoyama and Kodama 2016, Yokoyama 2018). In principle, workers may respond to higher minimum wages by reducing participation or reallocating effort across jobs (Kodama and Momota 2024, Mori and Okudaira 2025). In standard employment relationships, however, tracking short-run labor supply adjustments at daily or weekly frequencies is challenging, as hours and participation are typically observed only at coarse intervals. In contrast, while spot work platforms capture worker behavior only within the platform, they offer a unique advantage: high-frequency observation of worker-side actions throughout the fiscal year. This allows us to examine whether workers reduce engagement with spot work as the year-end approaches.

The left panel of Figure 11 reports the log number of employed female workers by worker status, while the right panel reports the corresponding series for males. Across all gender–status groups, employment displays a similar upward trend, with no evidence of a differential slowdown for groups that are more likely to face tax-related work disincentives toward the end of the fiscal year. Although temporary female workers and stay-at-home spouses are more likely to qualify for spousal tax deductions in Japan, employment continues to rise through November and December

2023 for both women and men. The absence of gender- or status-specific divergence indirectly suggests that the observed decline in spot employment following the minimum wage increase is unlikely to be driven by labor supply adjustments. Instead, the evidence points to labor demand responses by firms as the primary driver. Moreover, the steady growth in spot employment toward year-end indicates that spot work contributes to income gains outside the immediate vicinity of institutional thresholds such as the spousal tax deduction, highlighting its role in expanding earning opportunities rather than amplifying year-end labor supply constraints.

6 Heterogeneous Job-Posting Responses to the Minimum Wage

This section investigates heterogeneity in firms' responses to minimum wage increases, motivated by the idea that adjustment costs and bindingness vary systematically across locations, occupations, and job attributes, leading employers to reallocate employment and modify postings along margins that are cheapest to adjust in the short run.[22]

6.1 Prefecture-month and Occupation-month Level DID Estimates

In Section 5.1, we estimate the effect of the minimum wage increase on the outcome by aggregating policy events across prefectures. However, the minimum-wage bite varies across prefectures, and the relationship between the employment effect and the Kaitz index (the ratio of the minimum wage to the median wage) is of independent interest. We therefore implement the DID strategy separately for each prefecture p and show that estimated employment effects are more negative in prefectures with a higher Kaitz index.

Specifically, we first construct a separate dataset for each prefecture p and wage bin j and run the following regression within each dataset:

$$Y_{j,t}^p = \sum_{l \neq -1} \sum_{e \neq \infty} \mu_{l,e}^p \mathbf{1}\{t - 7 = l\} \mathbf{1}\{E_j^p = e\} + \alpha_e^p + \lambda_t^p + \epsilon_{j,t}^p. \tag{2}$$

Here, the superscript p indicates that this regression is estimated separately for each prefecture p. As in Equation (1), each coefficient $\mu_{l,e}^p$ is the DID estimand, capturing the dynamic effect of the minimum wage increase on the outcome at a given period l in prefecture p under the DID

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[22]Additional heterogeneity analysis is provided in the Appendix.

identifying assumptions:

$$\mu_{l,e}^p = E[Y_{j,7+l}^p - Y_{j,6}^p | E_j^p = e] - E[Y_{j,7+l}^p - Y_{j,6}^p | E_j^p = \infty],$$

where the pre-treatment period is $l = -1$ and the control group is group ∞ in prefecture p.

Next, as in Section 4.2, we compute the missing jobs, excess jobs, and the overall employment effect for each prefecture p by aggregating $\mu_{l,e}^p$ in the first step:

$$\Delta a^p = \sum_{l=0}^{5} \Delta a_l^p, \ \ \Delta b^p = \sum_{l=0}^{5} \Delta b_l^p, \ \ \Delta e^p = \sum_{l=0}^{5} \Delta e_l^p,$$

where the superscript p indicates that each quantity is defined within prefecture p.

6.2   Prefecture-month Level Results

Figure 12 illustrates the relationship between the Kaitz index and employment outcomes across prefectures. The Kaitz index is calculated for each prefecture as the ratio of the October 2023 minimum wage to the median wage. Panel (a) plots the estimated numbers of missing and excess jobs against the Kaitz index. It shows a positive association between the Kaitz index and excess jobs, and a negative association with missing jobs. When the minimum wage is high relative to the median wage, minimum wage hikes tend to have a stronger impact. Panel (b) presents the relationship between the Kaitz index and the overall employment effect, revealing a negative correlation. These findings suggest that, in Japan's spot work market, minimum wage increases have already begun to produce modest negative employment effects, in contrast to the United States, where such effects remain limited (Cengiz *et al.* 2019).

6.3   Occupation-month Level Results

While prior studies have often concentrated on sectors like restaurants—where the minimum wage is most binding and effects are more easily detected—we adopt a distributional approach that captures wage and employment responses even in industries with limited direct exposure. This enables a more comprehensive evaluation of the policy's impact across a broader range of occupations.[23]

Figure 13 illustrates heterogeneous occupation-level responses to minimum wage hikes, revealing distinct patterns across job categories. The most pronounced employment losses below the

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[23]Lordan and Neumark (2018) and Aaronson and Phelan (2019) find that the minimum wage reduces the employment of the lowest-wage workers in routine automatable occupations. Dustmann *et al.* (2022) find that minimum wages could also affect productivity by improving the composition of jobs in the economy; in response to minimum wage increases, small, inefficient firms exited the market, and workers at these firms found employment at more productive firms. Also, Forsythe (2023) finds that there is no heterogeneity across occupations.

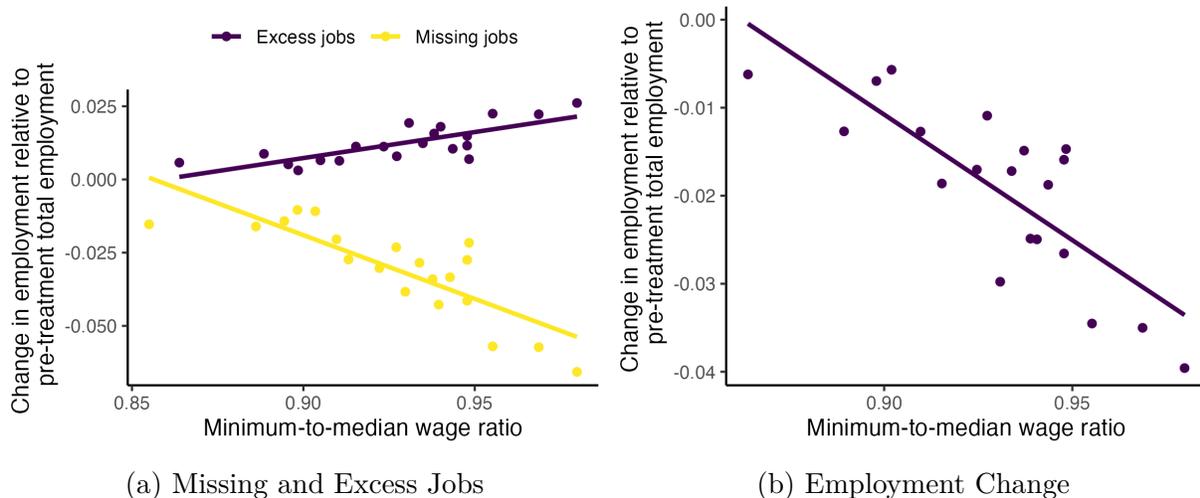(a) Missing and Excess Jobs   (b) Employment Change

Figure 12: Relationship between Excess Jobs, Missing Jobs, Employment Change, and the Kaitz Index (Minimum-to-Median Wage Ratio) across Prefectures

Note: Figure 12 shows the binned scatter plots for missing and excess jobs in Panel (a), and total employment changes in Panel (b) by the value of the minimum-to-median wage ratio (Kaitz index) for the 47 prefecture-specific estimates. The Kaitz index is calculated for each prefecture as the ratio of the October 2023 minimum wage to the median wage.

new minimum wage occur in Restaurant, Retail, and Event Staff, where a substantial number of low-wage jobs disappear. In contrast, occupations such as Logistics, Customer Service, and Light Work exhibit relatively balanced patterns, with moderate excess job creation at or just above the new minimum that partially offsets missing jobs. Professional and Entertainment roles show smaller overall impacts, suggesting limited exposure to the minimum wage threshold. We also observe modest increases in higher wage bins for some occupations, including office work. Overall, these findings suggest that minimum wage increases lead not only to occupational reallocation but also to shifts across the wage distribution within occupations.

6.4 Heterogeneity Across Job Attributes

Beyond aggregate employment and vacancy responses, minimum wage increases may alter the composition of jobs posted on the platform. We therefore examine heterogeneity across time slot and job attributes inferred from posting descriptions, which capture salient characteristics such as amenity provision and gender or skill targeting. These attributes allow us to classify postings into economically meaningful groups and to assess which types of jobs expand or contract following the minimum wage increase. By tracking changes in employment across these description-based categories, we study how the composition of spot jobs adjusts in the short run, complementing evidence on longer-run reallocation documented in Dustmann *et al.* (2022).

(a) Restaurant   (b) Light Work   (c) Retail

(d) Customer Service   (e) Professional   (f) Logistics

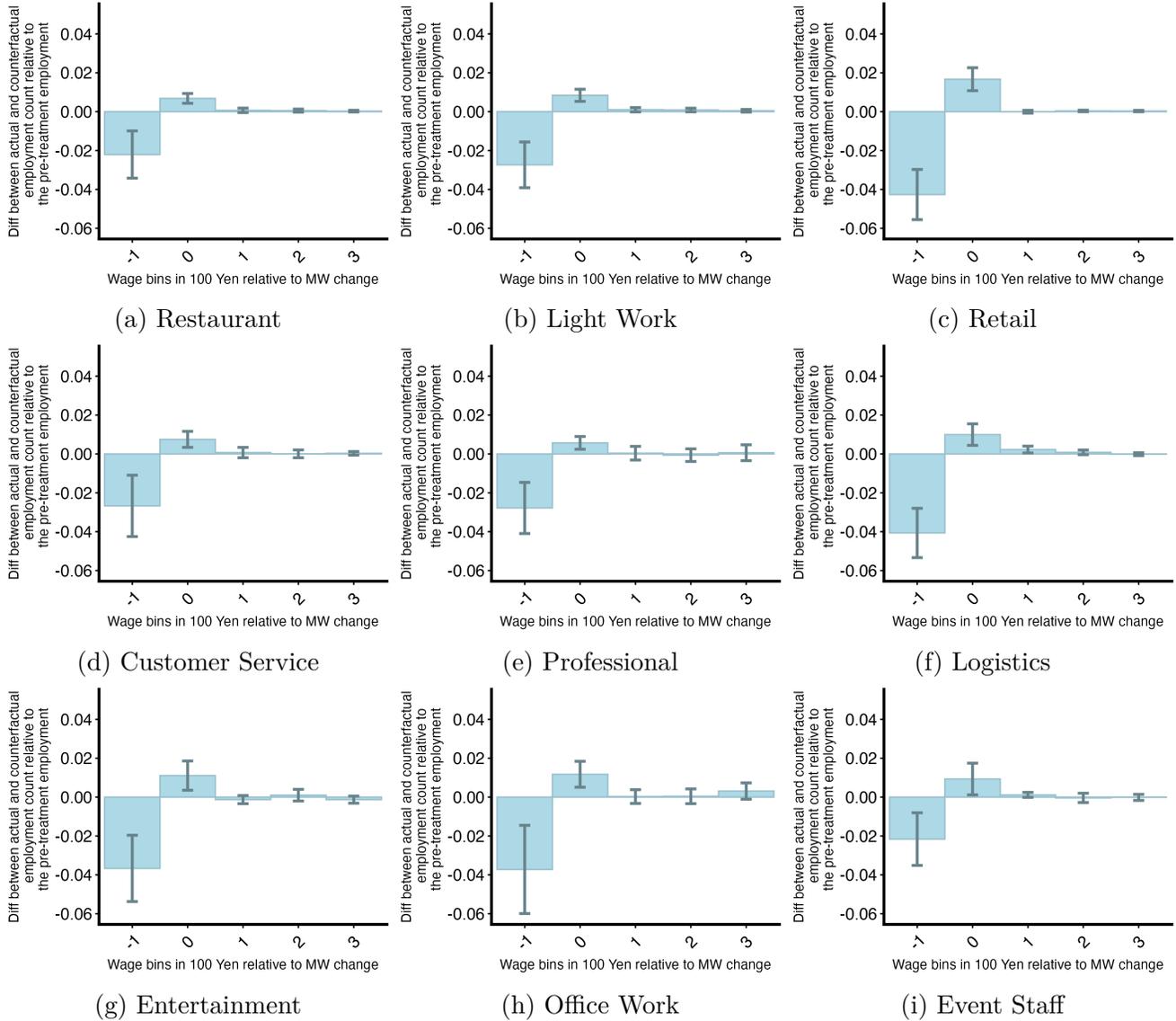(g) Entertainment   (h) Office Work   (i) Event Staff

Figure 13: Impact of Minimum Wages on Employment by Occupation

Note: Figure 13 shows the estimated impact of the minimum wage increase on employment for each group e in the wage distribution for each occupation in the platform.

Time-of-Day Reallocation We examine whether minimum wage hikes induce within-day reallocation by shift timing, a salient non-wage attribute of spot jobs. Specifically, we classify postings into four time-of-day categories—morning (6:00–12:00), evening (12:00–18:00), night (18:00–24:00), and midnight (24:00–6:00)—and estimate the distributional DID effects separately for each category. This time-of-day granularity allows us to test whether establishments shift demand toward particular (potentially less desirable) hours when labor costs rise. Figure 14 shows that the employment effects are broadly similar across time-of-day categories, suggesting little within-day substitution across shift times.
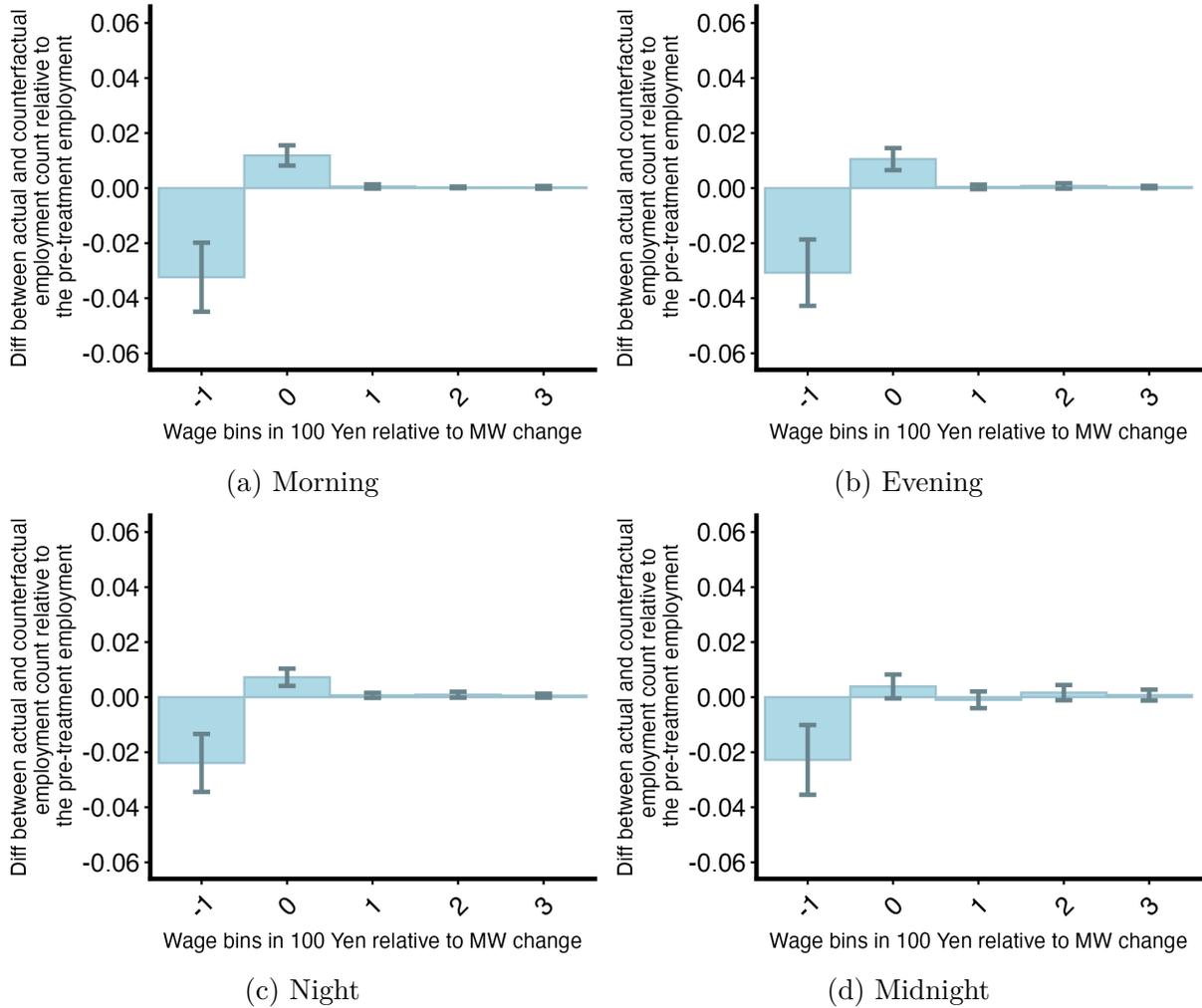
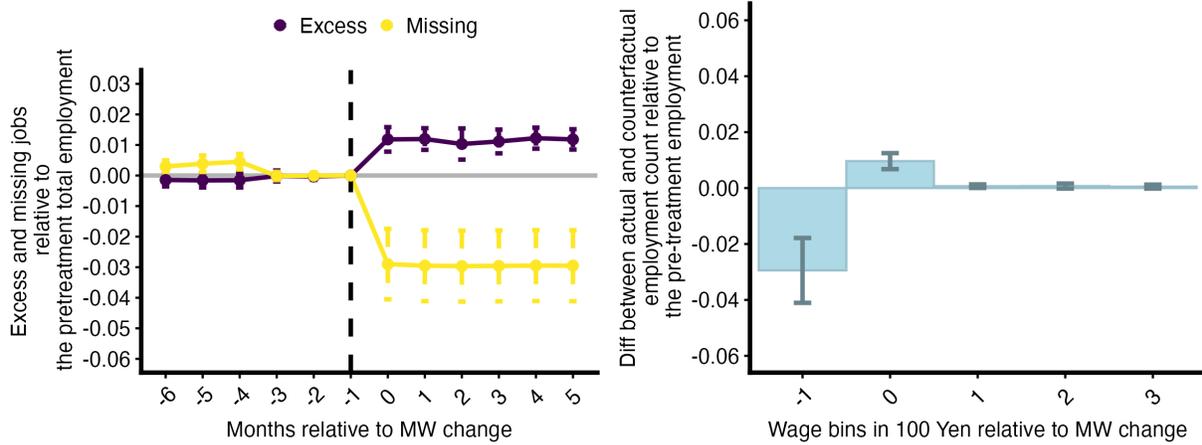Figure 14: Impact of Minimum Wages on Employment by Time-of-Day Category

Note: We divide the 24-hour day into four periods—morning (6:00–12:00), evening (12:00–18:00), night (18:00–24:00), and midnight (24:00–6:00).

Transportation Reimbursement Provision Figure 15 examines how minimum wage increases affect employment dynamics by whether a job posting offers transportation reimbursement. We find that postings that do not offer transportation reimbursement experience a modest but systematic decline after the reform, indicating that these jobs are more likely to disappear when labor costs rise. In contrast, postings that provide transportation reimbursement are relatively more resilient, with a small increase in excess employment.

This pattern should be interpreted as a compositional change rather than as evidence that firms actively raise reimbursement levels in response to the policy.[24] Jobs lacking transportation

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[24]Conditioning on surviving postings, average transportation reimbursement increases slightly after the reform, shown in Figure 18 in Appendix A.2.

benefits are more likely to exit, mechanically increasing the average amenity level among remaining postings. Overall, these results suggest that minimum wage increases are accompanied by a reallocation of spot jobs toward positions offering higher non-wage amenities.

(a) Amenity Provision: Missing and Excess Jobs over Time
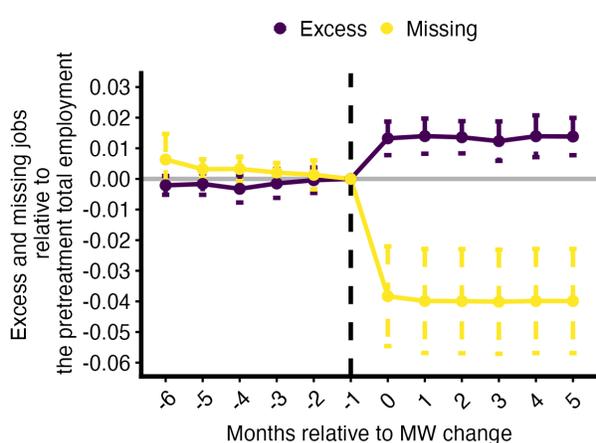
(b) Amenity Provision: Wage Distribution
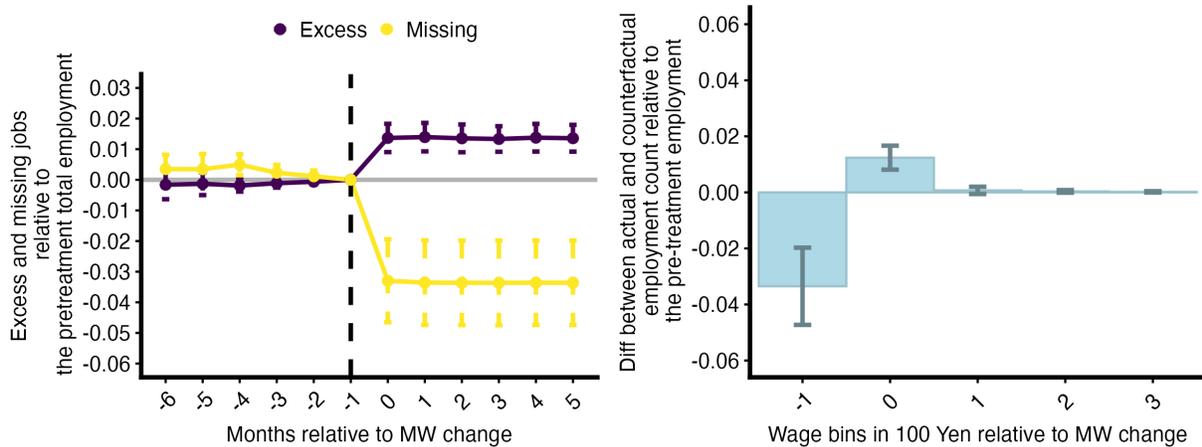
(c) No Amenity Provision: Missing and Excess Jobs over Time

(d) No Amenity Provision: Wage Distribution

Figure 15: Impact of Minimum Wages on Employment Separated by Transportation Reimbursement Provision

Note: Panels (a) and (c) show the estimated excess and missing jobs before and after the minimum wage increase for postings with and without transportation reimbursement, respectively. Panels (b) and (d) show the corresponding wage-distribution effects for each group e.

Job Description and Composition: Gender and Skill Targeting   We next examine how the composition of job postings changes along dimensions related to worker targeting, as revealed by job descriptions. Although firms cannot screen or reject applicants on this platform, descriptions
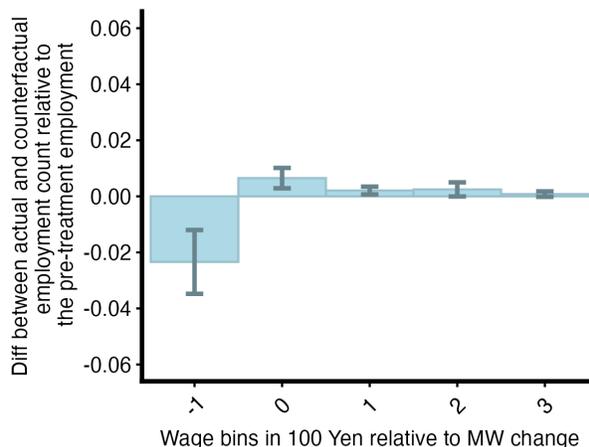
(a) Female-Targeted: Missing and Excess Jobs over Time
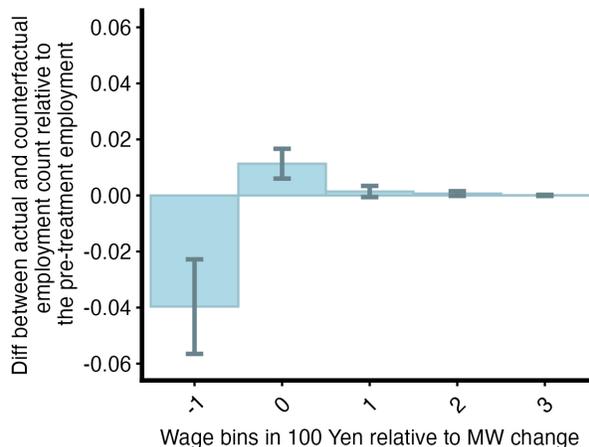
(b) Female-Targeted: Wage Distribution
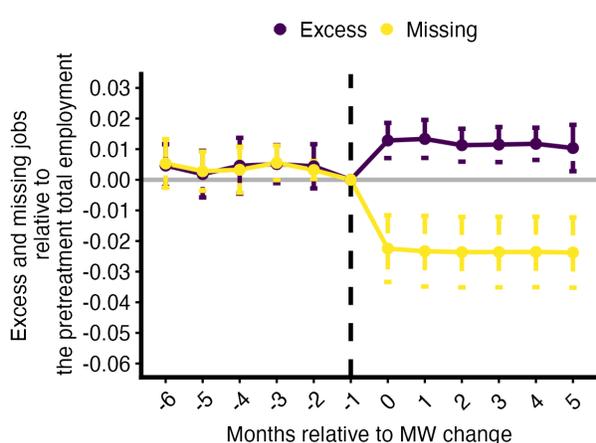
(c) Male-Targeted: Missing and Excess Jobs over Time

(d) Male-Targeted: Wage Distribution

Figure 16: Impact of Minimum Wages on Employment Separated by Gender

Note: Panel (a) plots the changes in female-targeted jobs before and after the minimum wage increase. Panel (b) plots the estimated effects of minimum wage increases on female-targeted jobs for each group $e \in \{0, \dots, 3\}$. Panels (c) and (d) provide the corresponding results for male-targeted jobs.

convey information about desired worker attributes and task requirements, shaping which workers are more likely to apply (Kuhn *et al.* 2020). We therefore interpret changes across description-based categories as reflecting shifts in the types of jobs being posted, rather than changes in hiring

standards per se.[25][26]

Figure 16 shows that postings explicitly targeting female applicants decline more sharply after the minimum wage increase than those targeting male applicants. This asymmetric contraction leads to a post-hike reweighting of employment toward male-targeted postings, indicating a change in posting composition along the gender-targeting dimension. We interpret this pattern as evidence of compositional reallocation in recruitment language, not as evidence about workers' underlying productivity.

Figure 17 documents a similar compositional shift along the skill-targeting dimension. While the volume of missing jobs is comparable across beginner- and experienced-worker-targeted postings, excess job creation is concentrated among experienced-targeted postings and remains positive after the minimum wage increase. As a result, the composition of posted jobs tilts toward positions targeting more experienced workers. Taken together, these findings indicate that minimum wage increases are associated with systematic changes in job composition, favoring postings that target worker attributes more closely matched to task-specific productivity.

6.5 Limitations

While our contract-level data allow us to examine the immediate responses of employers to minimum wage hikes in spot labor markets, several limitations remain.

First, our data are limited to labor demand and supply within a single job-matching platform, which restricts the external validity of our findings. This limitation is shared by many studies relying on platform data (Horton 2025, Melo *et al.* 2025, Stanton and Thomas 2021). Spot work platforms are rapidly expanding worldwide, so future research using alternative data from such platforms will be valuable for assessing external validity and mapping how minimum-wage adjustments generalize across settings.

Second, firm-level exposure designs are informative in other settings (Card and Krueger 1994, Machin *et al.* 2003, Draca *et al.* 2011, Harasztosi and Lindner 2019) and are useful for understanding firms' margins of adjustment, but they are difficult to implement here because the set of affected firms cannot be tracked consistently over time. Platform usage is highly seasonal, many firms do

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[25]Our targeting measures are based on keyword mentions in the job description. Specifically, we set a female-targeted (male-targeted) dummy equal to one if the description contains an explicit reference to women (men)—for example, language indicating that women are active in the workplace (e.g., "many women are actively employed")—and equal to zero otherwise. These indicators are not mutually exclusive: postings that mention both women and men are coded as one for both dummies. Importantly, a "female-targeted" posting in our definition does *not* imply that the job is restricted to women or that male applicants are excluded; rather, it captures descriptive language that may influence worker self-selection.

[26]For text variables, we observe the recorded description for each posting instance but do not observe complete within-ID version histories. Hence, we cannot separately identify within-posting text edits from compositional entry/exit of postings.
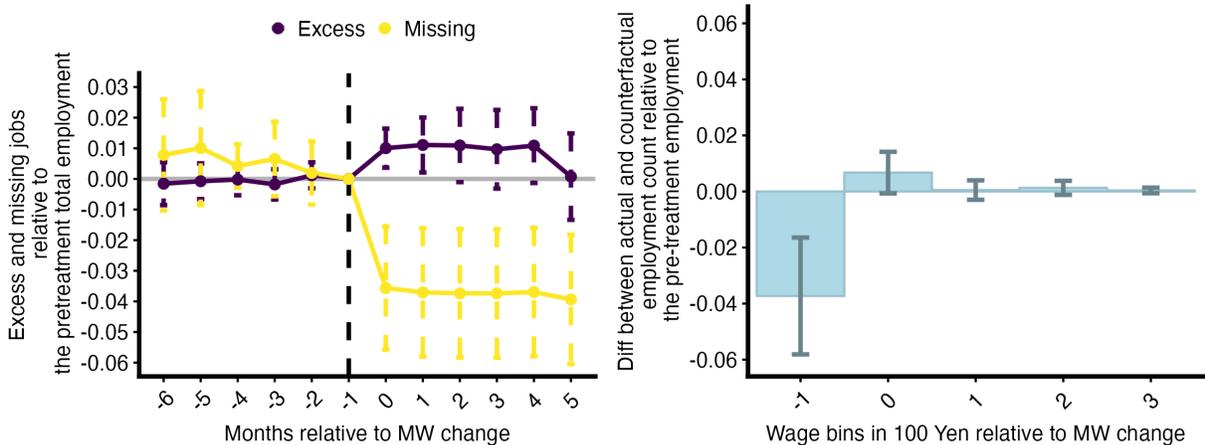
(a) Beginner-Targeted: Missing and Excess Jobs over Time

(b) Beginner-Targeted: Wage Distribution
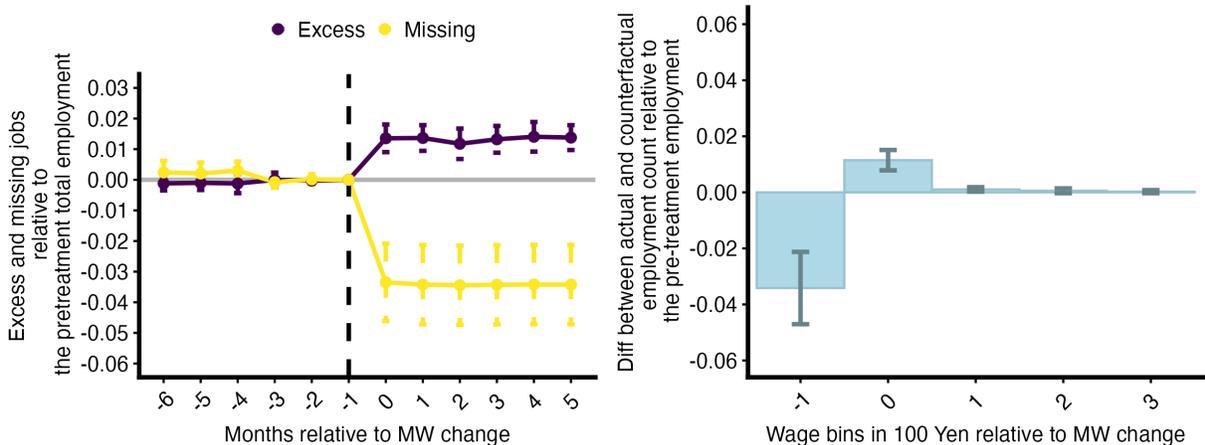
(c) Experienced-Worker-Targeted: Missing and Excess Jobs over Time

(d) Experienced-Worker-Targeted: Wage Distribution

Figure 17: Impact of Minimum Wages on Employment Separated by Skill Targeting

Note: Panel (a) plots the changes in beginner-targeted jobs before and after the minimum wage increase. Panel (b) plots the estimated effects of minimum wage increases on beginner-targeted jobs for each group $e \in \{0, \ldots, 3\}$. Panels (c) and (d) provide the corresponding results for experienced-worker-targeted jobs.

not post continuously, and it is hard to distinguish permanent exit from temporary inactivity. As a result, firm-month panels constructed from continuous posters are likely to be non-representative, making firm-level analysis difficult to interpret.

Third, our data are well suited to measuring short-run adjustments *within* the spot market—such as vacancy creation, wages, and selected posting attributes—but they do not allow us to trace substitution across broader production margins or to quantify the *timing* and *sequence* of such substitution. In particular, we cannot observe whether establishments substitute away from spot hiring toward capital investment or toward long-term contract workers, nor can we determine

how rapidly these substitutions occur. Prior work suggests that capital–labor substitution may arise following minimum wage hikes (Harasztosi and Lindner 2019), but such investment responses typically unfold over longer horizons. Labor–labor substitution may operate more quickly—for example, establishments may reduce spot postings and reallocate tasks to incumbent workers, potentially alongside shifts in skill requirements (Clemens *et al.* 2021).[27] More broadly, reallocating long-term workers can act as a form of wage retrenchment (Dube and Lindner 2024). Identifying *which* margin adjusts first and *how fast* substitution propagates across inputs requires linked data that connect spot hiring to establishments' broader staffing and investment decisions.

Taken together, our findings provide valuable evidence on short-term spot-market adjustments to minimum wage hikes, but a fuller account of firm behavior—especially substitution across inputs and the dynamics of adjustment—remains an important direction for future research using richer linked employer–worker and investment data.

7 Conclusion

This paper studies short-run minimum-wage effects in a spot labor market using high-frequency posting- and contract-level data from Timee. Using a wage-bin DID design, we find that missing jobs below the new minimum wage exceed excess jobs just above it, yielding a net employment decline of about 2% in affected bins and an elasticity of about -0.388. These effects appear immediately after implementation and remain stable over the post-reform window. Comparing vacancies and matches shows that the decline is driven primarily by reduced vacancy creation rather than by worker-side contraction in platform activity. Because spot work is an unusually flexible adjustment margin, we interpret these short-run elasticities as upper bounds in absolute value for standard employment elasticities in traditional labor markets.

We also document systematic reallocation across prefectures, occupations, and posting types. Employment losses are larger in prefectures with a higher minimum-wage bite and in low-wage occupations such as restaurants and retail. At the same time, surviving postings reweight toward jobs offering transportation reimbursement and toward descriptions targeting more experienced workers, while female-targeted descriptions become less common. Taken together, our results provide a high-frequency picture of how minimum-wage hikes reshape employment and vacancy distributions in the short run and offer a foundation for linking immediate adjustments to longer-run reallocation dynamics.

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[27]In Appendix B, we examine substitution across hiring channels (Timee vs. Hello Work) using staggered platform entry. We do not find prefecture-month evidence that Timee expansion reduces public-office vacancies. This does not directly test substitution toward incumbent workers within firms.

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A Additional Analysis

A.1 Elasticities Related to Minimum Wages

Following the method of Cengiz *et al.* (2019), we estimate (i) the elasticity of employment with respect to minimum wages and (ii) the employment elasticity with respect to the affected wages.

Table 2: Impact of Minimum Wages on Employment and Wages

	Estimate (SE)
Missing jobs below new MW	-0.03
	(0.006)
Excess jobs above new MW	0.012
	(0.002)
Affected wages	0.366
	(0.171)
Affected employment	-0.268
	(0.091)
Employment elasticity w.r.t MW	-0.388
	(0.132)
Emp. elasticity w.r.t affected wage	-0.732
	(0.091)
Job below new MW	0.068
% MW changes	0.047

Note: The table reports the effects of a minimum wage increase based on the event study analysis exploiting 47 prefecture-level minimum wage changes in 2023 (i.e., about 4% minimum wage increase). The table reports six-month averaged post-treatment estimates on missing jobs up to 100 JPY below the new minimum wage, excess jobs at and up to 300 JPY above it, employment, and wages. Robust standard errors in parentheses are clustered by wage bins separated by 10 JPY.

Table 2 reports six-month–averaged post-treatment effects for the baseline specification depicted in Figure 7. The magnitude of missing jobs below new minimum wages exceeds the magnitude of excess jobs above new minimum wages in the six-month window. The estimated employment elasticity with respect to the minimum wage is -0.388, which is consistent with Figure 7. Unlike the findings of Cengiz *et al.* (2019) and Dube and Zipperer (2024), our own-wage employment elasticity is -0.732, indicating a larger (absolute) employment response to affected wages than the median estimate (-0.13) reported by Dube and Lindner (2024).

A.2 Other Measured Outcome

Amenity as Outcome   We estimate the impact of minimum wage increases on transportation reimbursement offered by firms as an amenity. The estimation method follows that of Section 4,

with the outcome variable being transportation reimbursement linked to employment (set to zero if no transportation reimbursement is provided) and the probability of providing any transportation reimbursement. Since employment in group $e = -1$ disappears after the minimum wage revision, we focus on estimating and aggregating the effects on transportation reimbursement for groups with $e \geq 0$.

(a) Amenity Amount over Time

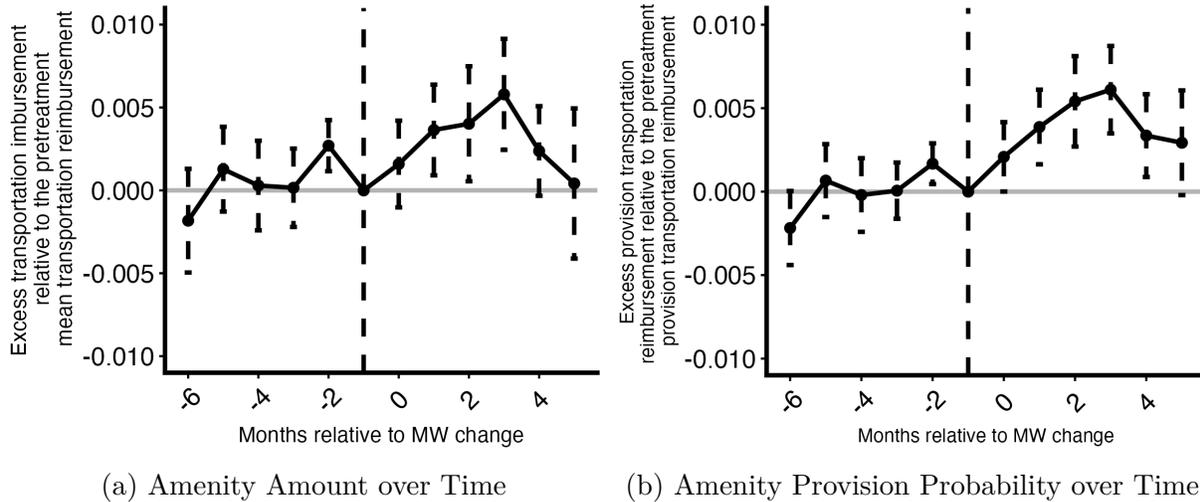(b) Amenity Provision Probability over Time

Figure 18: Impact of Minimum Wages on Amount and Provision of Transportation Reimbursement as Amenities

Note: Panel (a) plots changes in transportation reimbursement across excess wage bins before and after the minimum wage increase. Panel (b) provides the corresponding probabilities of providing transportation reimbursement.

Panel (a) in Figure 18 displays the estimated changes in transportation reimbursement, measured relative to the timing of the minimum wage increase. The estimates fluctuate around zero in the pre-treatment period and show no systematic trend following the policy change, suggesting that transportation reimbursement as an amenity remained largely stable over time. While some estimates are statistically significant at conventional levels—peaking around a 0.6% increase—they are economically negligible in the post-treatment period. This indicates that the minimum-wage impact via the intensive margin did not meaningfully extend to changes in transportation reimbursement practices.

Panel (b) in Figure 18 presents the dynamic effects of the minimum wage increase on the provision probability of transportation reimbursement. The estimates gradually increase in the post-treatment period, peaking around 0.7%, suggesting a statistically significant but economically modest increase in the likelihood that firms offer this amenity. This pattern suggests that although there is some statistically detectable response at the extensive margin—whether or not the amenity is offered—the degree of adjustment remains limited. Thus, even when considering the amenity provision decision itself, the minimum-wage effect on amenities appears minimal.
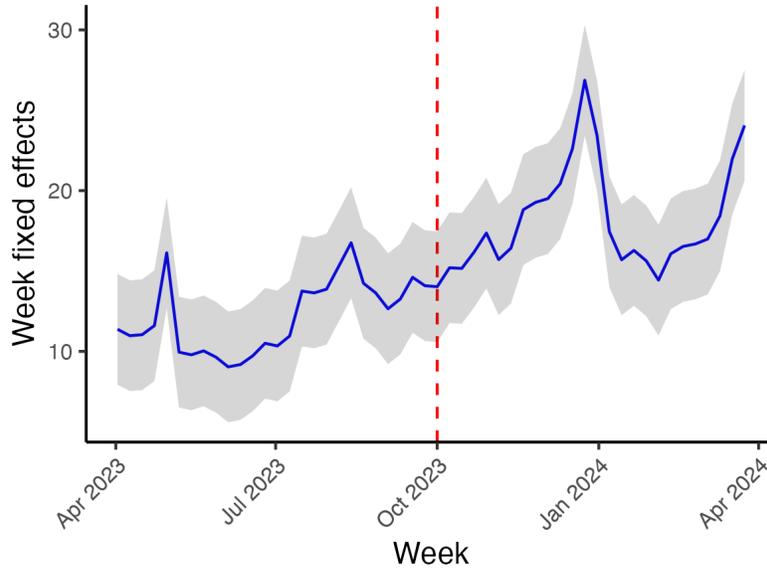
Figure 19: Estimated Week Fixed Effects in Contracted Total Earnings

Note: The figure plots week fixed effects with confidence intervals from a regression of total earnings at the prefecture-week level on prefecture and week indicators. Total earnings are calculated as hourly wages multiplied by working hours.

A.3 Platform Revenue

A key advantage of using platform data is the ability to observe not only posted wages but also actual matching outcomes—namely, the contracted payments—on a daily basis. Even if employment volume declined as shown above, rising wages due to the minimum wage hike would still raise total earnings. Since 30% of contracted payments are paid by businesses to the platform as revenue, this indicator is directly linked to platform earnings.

Figure 19 illustrates the temporal dynamics of weekly fixed effects from a regression of total earnings at the prefecture-week level on prefecture and week dummy variables. We observe no decline in total earnings after the October revision; if anything, there is a modest level increase. Given the platform's proportional commission rule, this implies that platform revenue is at least stable in the short run. Combined with the employment and hours results, the evidence is consistent with compositional adjustment toward higher hourly wages with limited movement in total transaction volume.

A.4 Addressing Potential Issues

The Definition of Control Group Our baseline event-study DID design defines treatment status based on each wage bin's distance to the new prefecture-level minimum wage and uses

(a) Missing and Excess Jobs over Time

(b) Wage Distribution

(c) Missing and Excess Jobs over Time
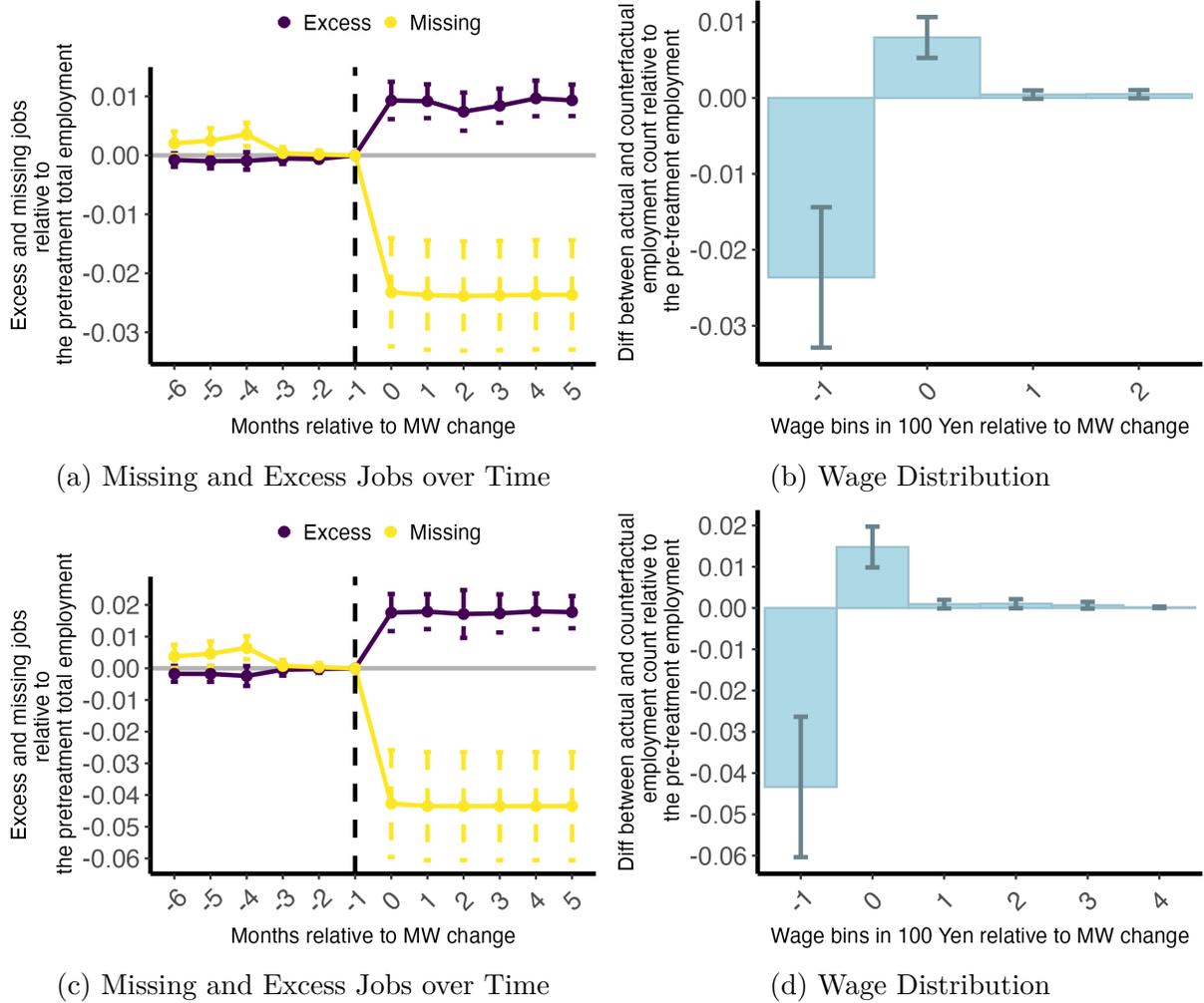
(d) Wage Distribution

Figure 20: Impact of Minimum Wages on Employment: Alternative Definition of the Control Group

Note: This figure presents robustness checks using alternative definitions of the control wage bin in the event-study DID design. Panels (a) and (b) report the results when the control group is defined as wage bins with wages $\geq MW_p + 300$. Panels (c) and (d) report the results when the control group is defined as wage bins with wages $\geq MW_p + 500$. Panels (a) and (c) plot the event-study coefficients for missing jobs (employment below the new minimum wage), while Panels (b) and (d) plot the event-study coefficients for excess jobs (employment at and above the new minimum wage within the treated range).

the upper part of the wage distribution as the control group. Concretely, we set a threshold $\overline{W}_p = MW_p + 400$ and assign all wage bins at or above $\overline{W}_p$ to the control group (group ∞). This choice is motivated by the "limited spillover" assumption: minimum-wage effects should vanish sufficiently far above the new wage floor so that higher-wage bins provide a valid counterfactual trend for bins near the threshold. Under this assumption, the identifying variation comes from comparing changes in outcomes for bins near the minimum wage to changes for bins in group ∞ around the timing of the reform.

A natural concern is that the estimated effects could be sensitive to where the control region begins. If spillovers extend further up the wage distribution than assumed, defining the control group too close to the minimum wage (e.g., $MW_p + 300$) could contaminate the control bins and mechanically attenuate the estimated treatment effects. Conversely, defining the control group farther away (e.g., $MW_p + 500$) reduces the risk of contamination but may change precision by shifting the composition of the control region. To assess this sensitivity, we re-estimate our baseline specification under two alternative thresholds for $\overline{W}_p$: $MW_p + 300$ and $MW_p + 500$.

Figure 20 reports the corresponding event-study paths for missing and excess jobs and the associated wage-distribution patterns under alternative control-threshold definitions. Across both alternative control definitions, the main qualitative patterns remain intact. First, pre-treatment coefficients stay close to zero, supporting the plausibility of the parallel-trends assumption under each control threshold. Second, the post-reform dynamics continue to show a sharp contraction of employment below the new minimum wage (missing jobs) and an expansion just above it (excess jobs), with the net effect remaining negative because missing jobs exceed excess jobs. Finally, the distributional profile of the effects remains concentrated near the wage floor, consistent with limited spillovers to higher wage bins—the key condition needed for group ∞ to serve as a valid control. Overall, our conclusions about immediate job losses below the new minimum wage and incomplete offsetting gains above it are not driven by the baseline choice of $\overline{W}_p = MW_p + 400$.

Main Specification with Wage-bin Specific Trends A potential concern with our baseline event-study DID design is that wage bins far above the minimum wage (which form the control group) may follow different underlying time patterns than wage bins close to the minimum wage. In particular, if high-wage and low-wage bins exhibit systematically different trends even absent the policy change, the standard parallel-trends assumption may be violated, and our baseline estimates could partly reflect differential underlying dynamics rather than the minimum-wage revision.

To address this concern, we estimate an augmented specification that flexibly absorbs wage-bin-specific heterogeneity. Specifically, we re-estimate our main event-study regression while including wage-bin fixed effects, J_j, and wage-bin-by-time fixed effects, $\Omega_{j,t}$, in addition to the baseline set of controls:

$$\frac{Y_{j(p),t}}{N_{p,t}} = \sum_{l \neq -1} \sum_{e \neq \infty} \mu_{l,e} \mathbf{1}\{t - 7 = l\} \mathbf{1}\{E_{j(p)} = e\} + \alpha_e + \lambda_t + J_j + \Omega_{j,t} + \epsilon_{j,t}. \tag{3}$$

The inclusion of J_j controls for time-invariant differences across wage bins, while $\Omega_{j,t}$ allows each wage bin to have its own time trends, thereby relaxing the requirement that high-wage and low-wage bins must share common trends.

Because the mapping from a nominal wage bin j to an exposure group $E_{j(p)}$ depends on prefecture-specific minimum wages, the same bin j can belong to different exposure groups across prefectures. As a result, even after absorbing wage-bin-by-time shocks via $\Omega_{j,t}$, there remains within-(j,t) cross-prefecture variation in $\mathbf{1}\{E_{j(p)} = e\}$. Hence the event-study interaction $\mathbf{1}\{t-7 = l\}\mathbf{1}\{E_{j(p)} = e\}$ is not perfectly collinear with $\Omega_{j,t}$, and the coefficients $\mu_{l,e}$ are identified from this residual variation.

This specification compares changes around the minimum-wage revision within wage bins after netting out wage-bin-specific time variation, and thus provides a stringent robustness check against confounding differential trends across the wage distribution.

(a) Missing and Excess Jobs over Time
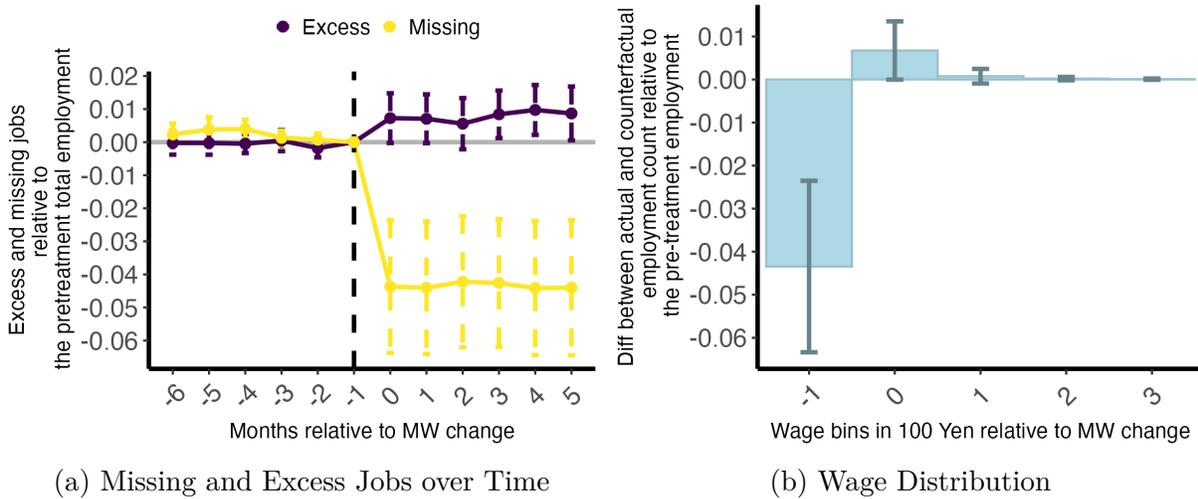
(b) Wage Distribution

Figure 21: Impact of Minimum Wages on Employment: Controlling Wage-bin Specific Trends

Note: Panel (a) shows the estimated excess jobs and missing jobs for each month before and after the minimum wage increase. Panel (b) shows the estimated impact of the minimum wage increase on employment for each group e in the wage distribution.

Figure 21 reports the resulting estimates. Reassuringly, the qualitative patterns remain consistent with our baseline findings: the estimated effects remain concentrated near the new minimum wage, with the most pronounced adjustments occurring in wage bins at and below the wage floor and substantially smaller changes farther above it. Moreover, the absence of meaningful movements in the pre-revision period is preserved, suggesting that our main conclusions are not driven by spurious differences in underlying trends between low- and high-wage bins. Overall, allowing for wage-bin fixed effects J_j and wage-bin-by-time fixed effects $\Omega_{j,t}$ does not materially alter the core result that the minimum-wage increase induces an immediate redistribution of activity away from the lowest wage bins.

Difference-in-Difference-in-Differences Approach   We introduce a difference-in-difference-in-differences (DDD) approach as a sensitivity check for our identification strategy. Our baseline difference-in-differences (DID) specification relies on the assumption that, absent the policy change, the treated and control wage bins would have followed parallel trends. However, in practice, it is plausible that employment proportions in wage bins close to the minimum wage threshold and those in higher-wage bins exhibit different underlying trends even before the policy change. Such wage-bin-specific trends may arise from differential demand conditions, compositional changes, or other factors that are correlated with wage levels.

To mitigate concerns about differential wage-bin trends, we propose a DDD specification that uses two-years differences in outcomes—i.e., the difference between outcomes in 2023 and those in the corresponding month in 2021. The key idea is to net out wage-bin-specific seasonal and persistent components by comparing each month's outcome to the same calendar month two years earlier. We then estimate an event-study regression identical to our main specification, except that the outcome is the two-year change in the employment rate in each wage bin. The DDD regression is given by:

$$\frac{Y_{j(p),t}}{N_{p,t}} - \frac{Y_{j(p),t-24}}{N_{p,t-24}} = \sum_{l \neq -1} \sum_{e \neq \infty} \widetilde{\mu}_{l,e} \mathbf{1}\{t - 7 = l\}\mathbf{1}\{E_{j(p)} = e\} + \alpha_e + \lambda_t + \epsilon_{j,t}, \qquad (4)$$

This specification is identical to our main event-study DID regression, except that the outcome is defined as a two-year difference. Specifically, the outcome is the change in the employment rate in wage bin j of prefecture p between month t and the corresponding month two years earlier $(t - 24)$.

The parameters of interest are the set of $\widetilde{\mu}_{l,e}$:

$$\begin{aligned}
\widetilde{\mu}_{l,e} &= E\left[\left(\frac{Y_{j(p),7+l}}{N_{p,7+l}} - \frac{Y_{j(p),7+l-24}}{N_{p,7+l-24}}\right) - \left(\frac{Y_{j(p),6}}{N_{p,6}} - \frac{Y_{j(p),6-24}}{N_{p,6-24}}\right) \middle| E_{j(p)} = e\right] \\
&\quad - E\left[\left(\frac{Y_{j(p),7+l}}{N_{p,7+l}} - \frac{Y_{j(p),7+l-24}}{N_{p,7+l-24}}\right) - \left(\frac{Y_{j(p),6}}{N_{p,6}} - \frac{Y_{j(p),6-24}}{N_{p,6-24}}\right) \middle| E_{j(p)} = \infty\right] \\
&= \mu_{l,e} - \mu_{l,e}^{2021},
\end{aligned}$$

where $\mu_{l,e}^{2021}$ denotes $E\left[\frac{Y_{j(p),7+l-24}}{N_{p,7+l-24}} - \frac{Y_{j(p),6-24}}{N_{p,6-24}} \middle| E_{j(p)} = e\right] - E\left[\frac{Y_{j(p),7+l-24}}{N_{p,7+l-24}} - \frac{Y_{j(p),6-24}}{N_{p,6-24}} \middle| E_{j(p)} = \infty\right]$. The term $\mu_{l,e}^{2021}$ can be interpreted as a placebo DID parameter evaluated two years earlier: it captures the treated–control differential change between months $6 - 24$ and $7 + l - 24$, i.e., in the pre-policy year (e.g., in 2021 when the policy change of interest occurs in 2023). In an ideal setting with no anticipation and no spurious differential dynamics, this placebo effect should be close to

zero. More importantly, if treated and control groups are subject to different seasonal patterns, $\mu_{l,e}^{2021}$ may be non-zero even in the absence of the policy. In that case, subtracting this placebo component from the baseline DID estimand yields $\widetilde{\mu}_{l,e} = \mu_{l,e} - \mu_{l,e}^{2021}$, which removes differential seasonality and provides a more robust estimate of the event-time treatment effect.

Note that this identification strategy implicitly requires that the 2021 minimum-wage revision does not affect postings that fall into wage bin j. In practice, however, this assumption cannot be satisfied. In our data, wage bins j are constructed based on the wage distribution of postings (or realized matches) observed between April 2023 and March 2024. Consequently, over the period April 2023–September 2023, postings can mechanically fall into the wage range above the 2021 minimum wage but below the higher minimum wage introduced in 2023. Therefore, if the 2021 minimum-wage increase affected postings in this wage range, the parameter identified by our DDD specification, $\widetilde{\mu}_{l,e}^{2021}$, should be interpreted as removing not a "clean" placebo effect but a contaminated placebo term, $\mu_{l,e}^{2021}$, which captures the impact of the 2021 minimum-wage revision.[28]

This contamination potentially matters for our interpretation. Because the DDD holds nominal wage bins j fixed across years, the bins just below MW_p in 2023—which define the 2023 "missing-jobs" region—may overlap with the 2021 "excess" interval in nominal terms: these bins can be at or above MW_p in 2021 and thus fall into our broad excess range relative to MW_p in 2021 (up to $MW_p + 300$ in 2021). This is an overlap in nominal-bin definitions, not a statement that the 2023 missing range coincides with the peak bunching bin in 2021. If the 2021 minimum-wage hike generated excess postings in that range, the DDD procedure would net out those excess jobs when forming the placebo difference, thereby mechanically boosting the estimated missing-job effect. As a result, the coefficient estimates for missing jobs may be biased to be more negative, even under an otherwise valid DDD design.

Panel (a) in Figure 22 shows the estimated excess jobs and missing jobs for each month before and after the minimum wage increase in our DDD specification. Panel (b) presents the estimated impact of the minimum wage increase on employment for each group e in the wage distribution.

Three important findings emerge from Panels (a) and (b) of Figure 22. First, the estimated excess and missing jobs are close to zero in the pre-treatment period, prior to the minimum-wage increase. Second, although the magnitude of the estimates is larger than in our baseline DID specification (Equation 1), the time patterns of the point estimates are similar. Finally, we find no evidence of spillover effects to higher wage tiers for $e \geq 1$. Taken together, these findings suggest that our baseline results are potentially robust to allowing for heterogeneous time trends across wage bins.

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[28]We do not use the 2022 data as the comparison period when constructing the differenced outcome because the resulting bias could be substantial.

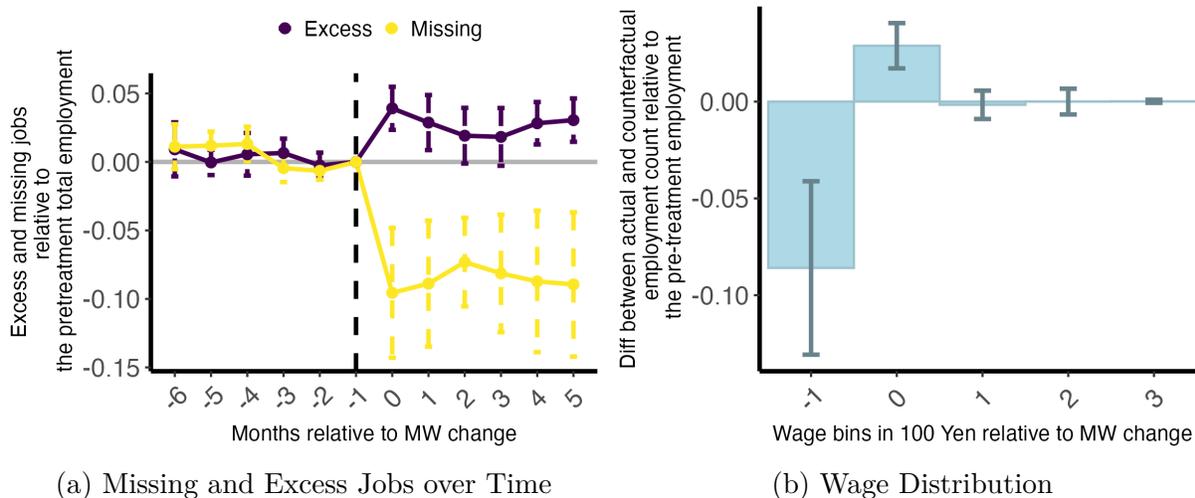(a) Missing and Excess Jobs over Time   (b) Wage Distribution

Figure 22: Impact of Minimum Wages on Employment: DDD Specification

Note: Panel (a) shows the estimated excess jobs and missing jobs for each month before and after the minimum wage increase. Panel (b) shows the estimated impact of the minimum wage increase on employment for each group e in the wage distribution.

B   Hiring Channel Substitution and Timee Rollout

In this section, we examine whether app-based spot work platforms substitute for or complement traditional hiring channels such as public employment offices. Using confidential contract-level data from Timee and public vacancy records, we exploit the platform's staggered regional rollout from 2018 to 2024. We conduct difference-in-differences analysis to assess whether Timee's expansion affected vacancy postings on the public system. We find flat pre-trends and no post-entry effect. The evidence suggests that spot work provides rapid surge capacity to absorb temporary labor demand, rather than displacing conventional, public-channel hiring.

Understanding how employers and job seekers choose between standard hiring channels (e.g., public employment offices) and digital labor platforms is central to modern labor-market design (Kroft and Pope 2014, Kuhn and Mansour 2014, Goldfarb and Tucker 2019). As platform adoption accelerates while conventional systems persist, questions about hiring–channel substitution—between app-based and public-office vacancies—have become increasingly salient. Existing studies show that digital postings on platforms like Craigslist, CareerBuilder, and Indeed can shift local outcomes: unemployment, vacancy rates, and rental markets (Kroft and Pope 2014), crime (Cunningham *et al.* 2024), media and political behavior (Djourelova *et al.* 2025), substitution across job boards (Brenčič 2016), and migration (Maria Balgova 2024). In contrast, little is known about substitution between spot gig work and standard employment. At its core, this raises a fundamental choice for employers: whether to fill labor needs via standard hiring (stable contracts) or via app-based spot work (short-term, demand-responsive).

We fill the gap with a 2018–2024 prefecture–month panel covering Japan, combining (i) confidential *Timee* data for short, on-site spot shifts and (ii) public *Hello Work* records on conventional vacancies, registered unemployed, and successful matches. Timee rolled out heterogeneously across prefectures and expanded rapidly. We exploit this staggered timing and intensity variation to test whether platform entry and growth reduced public-office vacancies.

Two features of the Timee data are central. First, we observe confidential contract-level matches linking each vacancy to a realized hire with full job detail. This labor-demand granularity allows us to study a substitution margin distinct from prior Craigslist-type studies (Kroft and Pope 2014, Brenčič 2016): whether firms meet needs via fast, flexible spot contracts or slower standard hires, with an explicit cost–flexibility tradeoff (e.g., Timee charges 30% commission per contract). These logs also allow us to construct a prefecture-specific Platform Popularity Index (posts per 1,000 residents) a la Kroft and Pope (2014), and track substitution on both the posting (vacancies) and matching (employment) margins over time. Second, unlike household-service apps (e.g., TaskRabbit in Cullen and Farronato (2021)), Timee spans a wide range of on-site, shift-based jobs—retail, food service, logistics, events, and light manufacturing—providing broad labor-market coverage.

Using the prefecture–month panel and staggered rollout, we estimate dynamic difference-in-differences models testing whether Timee entry and growth affect Hello Work vacancies. Results show flat pre-trends and no post-entry effect on public vacancies. This indicates that conventional jobs are not replaced by spot work. Instead, Timee acts as a buffer for temporary labor demand, absorbing workload spikes that would otherwise trigger overtime or leave tasks unfilled. It operates alongside, not in place of, public systems—offering short-notice hiring capacity without reducing standard vacancies. We also confirm robustness via a two-way fixed effects model using Platform Popularity as a continuous regressor.

B.1 Prefecture-month-level Timee Introduction and Expansion

To exploit prefecture-month variation in Timee's expansion, we focus on the calendar month in which job postings first appear in a given prefecture (the rollout month). This is defined as the first month that Timee postings are recorded in platform logs for that prefecture and serves as the binary treatment in our difference-in-differences design.

Figure 23 summarizes Timee's spatial rollout and intensity patterns. Panel (a) shows that the platform first launched in major metropolitan prefectures like Tokyo, Osaka, and Aichi, then expanded to less urban areas—reflecting a core-to-periphery diffusion strategy typical of digital platforms. Panel (b) orders prefectures by 2018 population and shows that higher-population areas generally adopted earlier. While this pattern holds overall, some lower-ranked prefectures, such as Nagano, received the rollout earlier than more populous regions, suggesting that other factors

also shaped the rollout sequence.

B.2 Empirical Exercise: Staggered Rollout of Timee

B.2.1 Binary Treatment Staggered DID

Our main identification strategy for estimating the impact of Timee's rollout on Hello Work vacancy counts exploits the expansion of Timee across Japanese prefectures during our sample period. As discussed in Section B.1, the timing of Timee's entry varies across prefectures and is not systematically related to stronger past or predicted economic conditions at the prefecture level. Accordingly, we identify the treatment effect on traditional vacancies using difference-in-differences by comparing prefecture–month outcomes between treated and not-yet-treated prefectures while accounting for the staggered timing of Timee's entry.

To align notation with the staggered-adoption framework, we write the event-time parameterization below, but estimation is conducted using the Callaway and Sant'Anna (2021) group-time ATT procedure rather than OLS on this equation:

$$Y_{p(g),t} = \alpha_{p(g)} + \beta_t + \sum_{g \in G} \sum_{j \neq g-1} \delta_{g,j} D_{p(g)}^{g,j} + \varepsilon_{p(g),t}, \tag{5}$$

where $Y_{p(g),t}$ is the log of the Hello Work vacancy outcome for prefecture $p(g)$ in month t; $\alpha_{p(g)}$ and β_t denote prefecture and month fixed effects; and $D_{p(g)}^{g,j}$ is an event–time indicator equal to one for prefecture $p(g)$ when t is j months relative to its first Timee entry month g (with $j = g - 1$ as the reference period).

The parameters of interest are event-time average treatment effects across adoption cohorts. We estimate group-time $\text{ATT}(g,t)$ using not-yet-treated prefectures as comparisons and then aggregate to event time following Callaway and Sant'Anna (2021).

Figure 24 plots event-time average treatment effects on the treated (ATT) for log Hello Work vacancies around Timee entry. The pre-entry coefficients are tightly centered near zero, supporting parallel trends. Following entry, point estimates remain close to zero for roughly the first year and, while some later coefficients turn mildly negative, their confidence intervals overlap zero throughout. Overall, we find no statistically meaningful reduction in public-office vacancies after Timee's rollout. This suggests that spot gig jobs are not substituting for conventional employment but rather serving as a buffer for temporary labor-demand shocks. The spot work platform absorbs sudden increases in workload that might otherwise lead to overtime for existing staff or unfilled tasks. In this sense, Timee complements rather than displaces the public vacancy channel, offering firms rapid, short-notice staffing capacity without diminishing the pool of standard job openings.
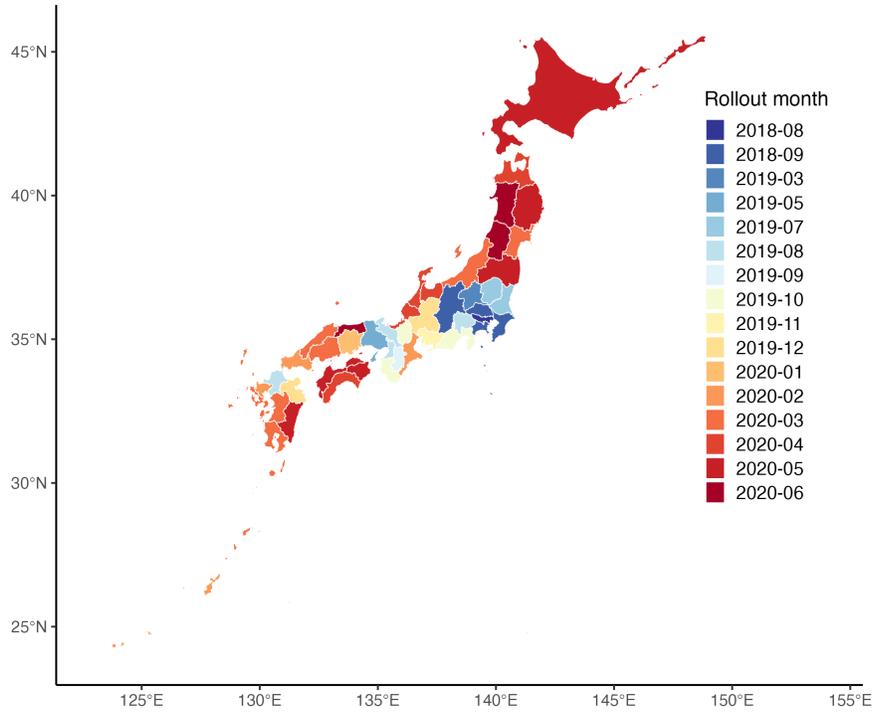
(a) Geographical Variation of Timee Rollout
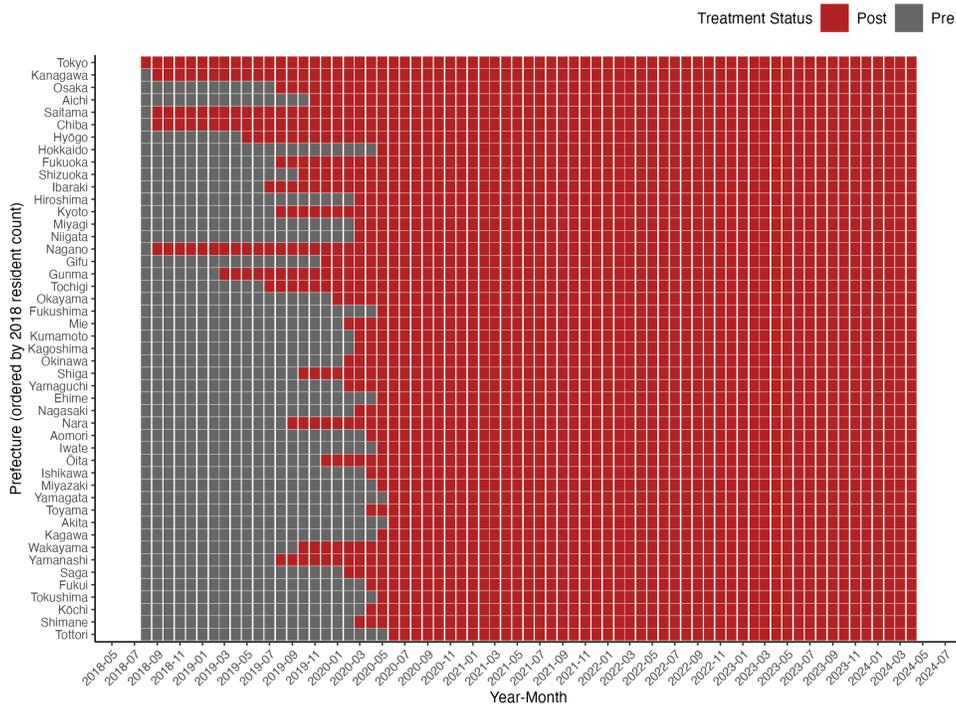
(b) Timee Rollout Timing (ordered by Resident Counts (2018))
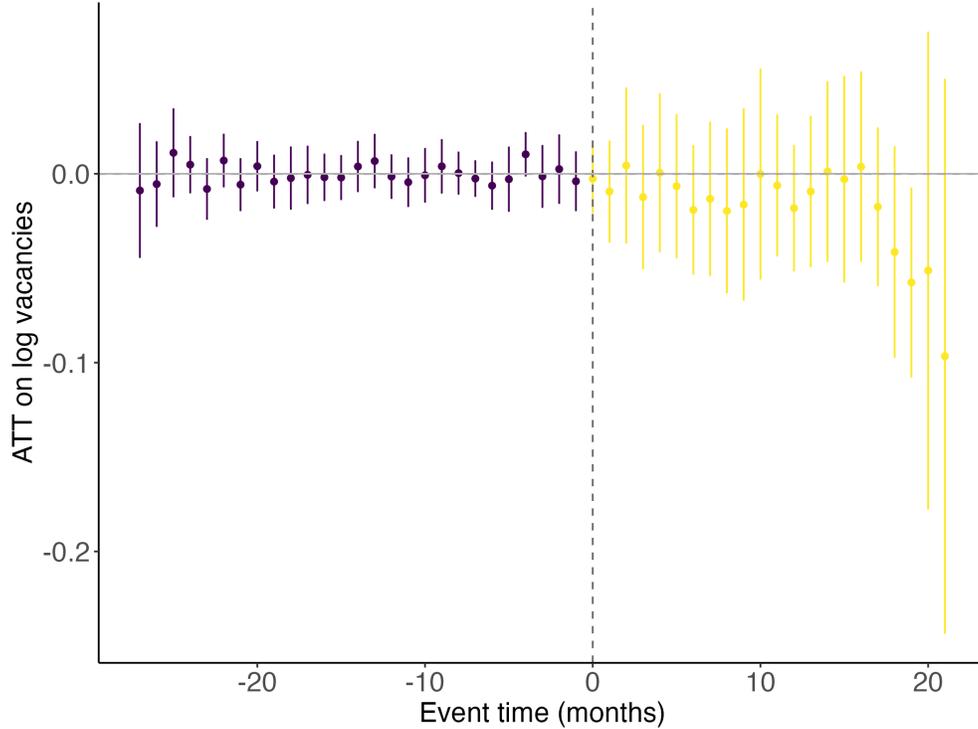
Figure 23: Timee Rollout Across Prefectures

Figure 24: Staggered Timee Entry and log of Hello Work Vacancy

Notes: The figure reports $\text{ATT}(g, t)$ from a difference-in-differences design with staggered adoption. Not-yet-treated prefectures form the comparison set. Vertical lines denote 95% confidence intervals (standard errors are calculated using the multiplier bootstrap in Callaway and Sant'Anna (2021)).

B.3 Timee Popularity Index and Continuous-Intensity Estimates

To complement the binary rollout treatment, we construct a time-varying prefecture-month Timee Popularity Index that captures relative platform penetration. Specifically, Pop_{pt} is the number of posted spot jobs in prefecture p and month t per 1,000 residents, where the denominator is fixed at prefecture population from the 2018 Basic Resident Registration. This construction mirrors the Craigslist penetration measure used by Kroft and Pope (2014), who define platform popularity as the number of online job advertisements per 1,000 people in each Metropolitan Statistical Area (MSA). In their study, this index is used to exploit cross-sectional variation in online job search intensity and to study substitution between online and offline job ads. Similarly, our index captures cross-prefecture and over-time variation in Timee adoption. For robustness, we also construct an alternative version based on realized contracts (spot employment matches per 1,000 residents), using detailed contract-level data linked to administrative identifiers.

Figure 25 summarizes the spatial rollout and intensity patterns. Panel (a) illustrates heterogeneous popularity trajectories, with several prefectures experiencing pronounced surges after 2022, while Panel (b) plots a modest, noisy correlation between 2024 popularity and population size.
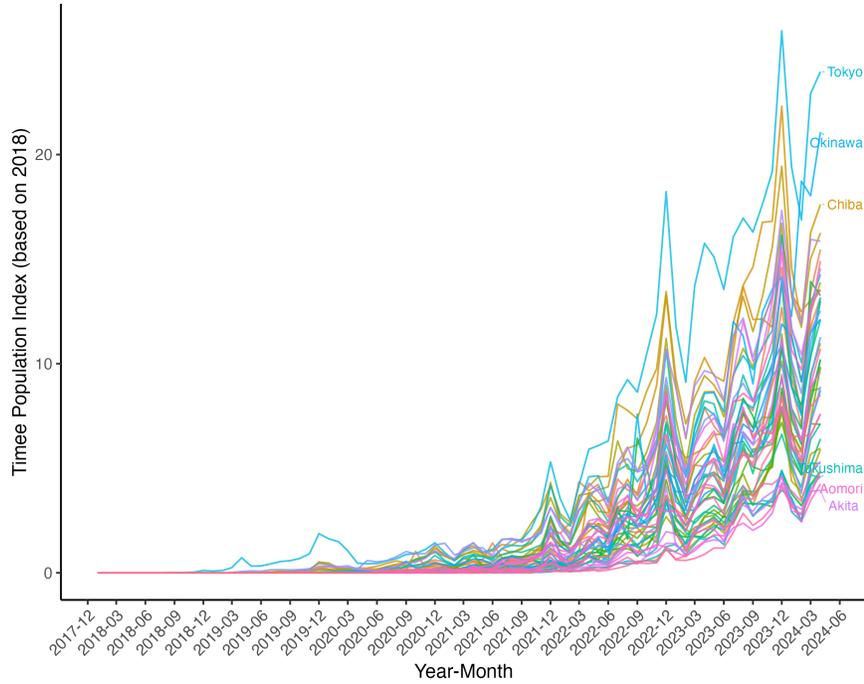
(a) Timee Popularity
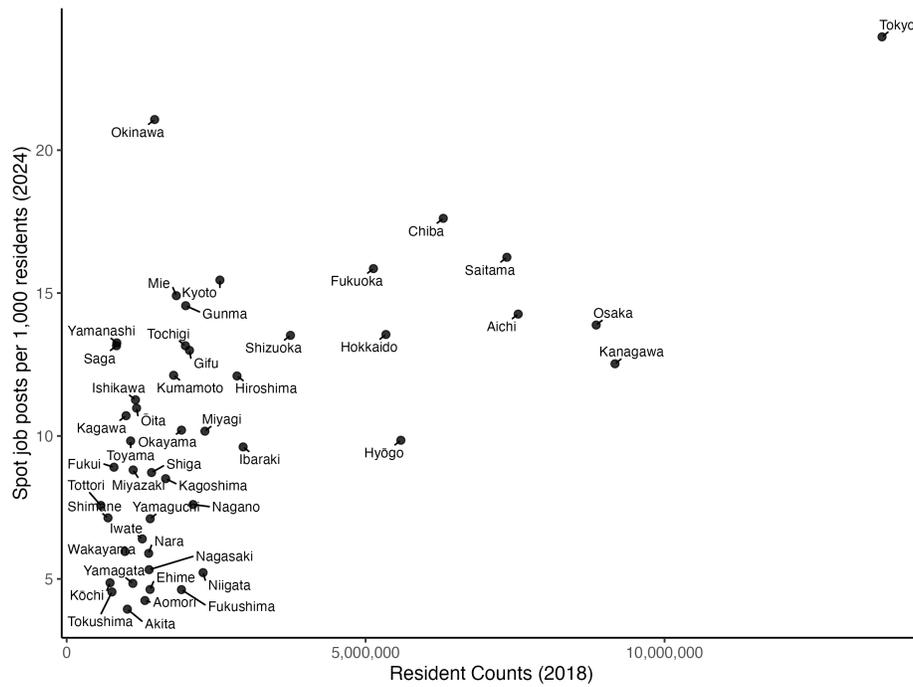
(b) Timee Popularity (2024) vs Resident Counts

Figure 25: Timee Popularity Trajectories and Scale

Note: Timee's popularity index is defined monthly as the number of spot-job posts per 1,000 residents, with 2018 resident counts used as the fixed denominator.

As an intensity-based robustness check, we estimate a two–way fixed effects model that treats Timee popularity as a continuous regressor:

$$Y_{pt} = \alpha_p + \beta_t + \gamma \operatorname{Pop}_{pt} + \varepsilon_{pt}, \tag{6}$$

where Pop_{pt} denotes the Timee Popularity Index in prefecture p and month t, and γ captures the within-prefecture association between popularity and Hello Work vacancies after accounting for prefecture and month shocks. Because popularity and vacancies are observed contemporaneously at the prefecture-month level, we interpret γ as a descriptive association rather than a causal parameter. As robustness checks, we (i) include month fixed effects interacted with baseline covariates, (ii) examine alternative normalizations of Pop_{pt}, and (iii) verify that popularity is only weakly related to event time within rollout cohorts.

Table 3 shows that Timee popularity has no detectable association with Hello Work vacancy counts. Overall, we find no statistically meaningful reduction in Hello Work vacancies after Timee's rollout.

Table 3: Impact of Timee Popularity Index on Hello Work Vacancy Counts

	(1)	(2)
Timee vacancy counts per 1000 residents	0.002	
	(0.003)	
Timee matching counts per 1000 residents		0.002
		(0.004)
FE (Month)	Y	Y
FE (Prefecture)	Y	Y
Num.Obs.	3572	3572
R2	0.996	0.996
R2 Adj.	0.996	0.996

Note: Standard errors are in parentheses, clustered at prefecture level.

In conclusion, this section evaluates whether app-based spot-work platforms crowd out the stock of standard vacancies recorded by public employment offices or instead operate alongside them. Using a prefecture–month panel and the platform's staggered rollout, dynamic DID estimates show flat pre-trends and no post-entry effect of Timee on Hello Work vacancy counts. Thus, Timee's expansion does not crowd out standard vacancies; rather, the spot market functions as a rapid surge capacity that buffers transitory labor-demand shocks without replacing conventional hiring.